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THE
HISTORY
AND
PRESENT STATE
OF
ELECTRICITY,
WITH
ORIGINAL EXPERIMENTS,

By JOSEPH PRIESTLEY, LL.D. F.R.S.

• THE SECOND EDITION, CORRECTED AND ENLARGED.

Causa latet, vis est notissima.
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L O N D O N,
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T O
THE RIGHT HONOURABLE
JAMES Earl of MORTON,
PRESIDENT OF THE ROYAL SOCIETY,
This HISTORY, &c.

IS,
WITH THE GREATEST RESPECT,
INSCRIBED,

By his LORDSHIP'S

MOST OBEDIENT,

AND MOST HUMBLE

SERVANT,

JOSEPH PRIESTLEY.

T O
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JAMES EARL OF MORTON,
President of the Royal Society,
The HISTORY, &c.

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WITH THE GREATEST RESPECT
INSCRIBED,
D. M. LONDON
Mort G...
And Mort...
SERVANT,
JOSEPH WILKINSON.

T H E

P R E F A C E.

IN writing the *history and present state of electricity*, I flatter myself that I shall give pleasure, as well to persons who have a taste for Natural Philosophy in general, as to electricians in particular; and I hope the work will be of some advantage to the science itself. Both these ends would certainly be answered in a considerable degree, were the execution at all answerable to the design.

THE history of electricity is a field full of pleasing objects, according to all the genuine and universal principles of taste, deduced from a knowledge of human nature. Scenes like these, in which we see a gradual rise and progress in things, always exhibit a pleasing spectacle to the human mind. Nature, in all her delightful walks, abounds with such views, and they are in a more especial manner connected with every thing that relates to human life and happiness; things, in their own nature, the most interesting to us. Hence it is, that the power of association has annexed crouds of pleasing sensations to the contemplation of every object, in which this property is apparent.

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THIS pleasure, likewise, bears a considerable resemblance to that of the sublime, which is one of the most exquisite of all those that affect the human imagination. For an object in which we see a perpetual progress and improvement is, as it were, continually rising in its magnitude; and moreover, when we see an actual increase, in a long period of time past, we cannot help forming an idea of an unlimited increase in futurity; which is a prospect really boundless, and sublime.

THE pleasures arising from views exhibited in *civil, natural,* and *philosophical* history are, in certain respects, different from one another. Each has its advantages, and each its defects: and both their advantages and defects contribute to adapt them to different classes of readers.

CIVIL history presents us with views of the strongest passions and sentiments of the human mind, into which every man can easily and perfectly enter; and with such incidents, respecting happiness and misery, as we cannot help feeling would alarm and affect us in a very sensible manner; and therefore, we are at present alarmed and affected by them to a considerable degree. Hence the pleasure we receive from civil history arises, chiefly, from the exercise it affords our passions. The imagination is only entertained with scenes which occasionally start up, like interludes, or episodes, in the great drama, to which we are principally attentive. We are presented, indeed, with the prospect of gradual improvement during the rise of great empires; but, as we read on, we are obliged to contemplate the disagreeable reverse. And the history of most states presents nothing but a tedious uniformity, without any striking events, to diversify and embellish the prospect. Besides, if a man have any sentiment of virtue and benevolence, he cannot help being shocked with a view of the vices and miseries of mankind; which, though they
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be not all, are certainly the most glaring and striking objects in the history of human affairs. An attention, indeed, to the conduct of divine providence, which is ever bringing good out of evil, and gradually conducting things to a more perfect and glorious state, tends to throw a more agreeable light on the more gloomy parts of history, but it requires great strength of mind to comprehend those views; and, after all, the feelings of the heart too often over power the conclusions of the head.

NATURAL history exhibits a boundless variety of scenes, and yet infinitely analogous to one another. A naturalist has, consequently, all the pleasure which the contemplation of uniformity and variety can give the mind; and this is one of the most copious sources of our intellectual pleasures. He is likewise entertained with a prospect of gradual improvement, while he sees every object in nature rising by due degrees to its maturity and perfection. And while new plants, new animals, and new fossils are perpetually pouring in upon him, the most pleasing views of the unbounded power, wisdom, and goodness of God are constantly present to his mind. But he has no direct view of human sentiments and human actions; which, by means of their endless associations, greatly heighten and improve all the pleasures of taste.

THE history of philosophy enjoys, in some measure, the advantages both of civil and natural history, whereby it is relieved from what is most tedious and disgusting in both. Philosophy exhibits the powers of nature, discovered and directed by human art. It has, therefore, in some measure, the boundless variety with the amazing uniformity of the one, and likewise every thing that is pleasing and interesting in the other. And the idea of continual rise and improvement is conspicuous in the whole

study, whether we be attentive to the part which nature, or that which men are acting in the great scene.

It is here that we see the human understanding to its greatest advantage, grasping at the noblest objects, and increasing its own powers, by acquiring to itself the powers of nature, and directing them to the accomplishment of its own views; whereby the security, and happiness of mankind are daily improved. Human abilities are chiefly conspicuous in adapting means to ends, and in deducing one thing from another by the method of analogy; and where shall we find instances of greater sagacity, than in philosophers diversifying the situations of things, in order to give them an opportunity of showing their mutual relations, affections, and influences; deducing one truth and one discovery from another, and applying them all to the useful purposes of human life.

If the exertion of human abilities, which cannot but form a delightful spectacle for the human imagination, give us pleasure, we enjoy it here in a higher degree than while we are contemplating the schemes of warriors, and the stratagems of their bloody art. Besides, the object of philosophical pursuits throws a pleasing idea upon the scenes they exhibit; whereas a reflection upon the real objects and views of most statesmen and conquerors cannot but take from the pleasure, which the idea of their sagacity, foresight, and comprehension would otherwise give to the virtuous and benevolent mind. Lastly, the investigation of the powers of nature, like the study of Natural History, is perpetually suggesting to us views of the divine perfections and providence, which are both pleasing to the imagination, and improving to the heart.

BUT though other kinds of history may, in some respects, vie with that of philosophy, nothing that comes under the denomination

nomination of history can exhibit instances of so fine a rise and improvement in things, as we see in the progress of the human mind, in philosophical investigations. To whatever height we have arrived in natural science, our beginnings were very low, and our advances have been exceeding gradual. And to look down from the eminence, and to see, and compare all those gradual advances in the ascent, cannot but give the greatest pleasure to those who are seated on the eminence, and who feel all the advantages of their elevated situation. And considering that we ourselves are, by no means, at the top of human science; that the mountain still ascends beyond our sight, and that we are, in fact, not much above the foot of it, a view of the manner in which the ascent has been made, cannot but animate us in our attempts to advance still higher, and suggest methods and expedients to assist us in our further progress.

GREAT conquerors, we read, have been both animated, and also, in a great measure, formed by reading the exploits of former conquerors. Why may not the same effect be expected from the history of philosophy to philosophers? May not even more be expected in this case? The wars of many of those conquerors, who received this advantage from history, had no proper connection with former wars: They were only analogous to them. Whereas the whole business of philosophy, diversified as it is, is but one; it being one and the same great scheme, that all philosophers, of all ages and nations have been conducting, from the beginning of the world; so that the work being the same, the labours of one are not only analogous to those of another, but in an immediate manner subservient to them; and one philosopher succeeds another in the same field; as one Roman proconsul succeeded another, in carrying on the same war, and pursuing the same conquests, in the same country. In this case,

case, an intimate knowledge of what has been done before us cannot but greatly facilitate our future progress, if it be not absolutely necessary to it.

THESE histories are evidently much more necessary in an advanced state of science, than in the infancy of it. At present philosophical discoveries are so many, and the accounts of them are so dispersed, that it is not in the power of any man to come at the knowledge of all that has been done, as a foundation for his own inquiries. And this circumstance appears to me to have very much retarded the progress of discoveries.

NOT that I think philosophical discoveries are now at a stand. On the other hand, as quick advances seem to have been made of late years, as in any equal period of time past whatever. Nay, it appears to me, that the progress is really accelerated. But the increase of knowledge is like the increase of a city. The building of some of the first streets makes a great figure, is much talked of, and known to every body; whereas the addition of, perhaps, twice as much building, after it has been swelled to a considerable size, is not so much as taken notice of, and may be really unknown to many of the inhabitants. If the additions which have been made to the buildings of the city of London, in any single year of late, had been made two or three centuries ago, it could not have escaped the observation of historians; whereas, now, they are so scattered, and the proportion they bear to the whole city is so small, that they are hardly noticed. For the same reason, the improvements that boys make at school, or that young gentlemen make at an academy, or the university, are more taken notice of than all the knowledge they acquire afterwards, though they continue their studies with the same assiduity and success.

THE history of experimental philosophy, in the manner in
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which it ought to be written, to be of much use, would be an immense work; but it were much to be wished, that persons who have leisure, and sufficient abilities, would undertake it in separate parts. I have executed it, in the best manner I have been able, for that branch which has been my own favourite amusement; and I shall think myself happy, if the attempt excite other persons to do the like for theirs.

I CANNOT help thinking myself to have been peculiarly fortunate, in undertaking the history of electricity, at the most proper time for writing it, when the materials were neither too few, nor too many to make a history; and when they were so scattered, as to make the undertaking highly desirable, and the work peculiarly useful to Englishmen.

I LIKEWISE think myself peculiarly happy in my subject itself. Few branches of Natural Philosophy would, I think, make so good a subject for a history. Few can boast such a number of discoveries, disposed in so fine a series, all comprised in so short a space of time, and all so recent, the principal actors in the scene being still living.

WITH several of these principal actors it has been my singular honour and happiness to be acquainted; and it was their approbation of my plan, and their generous encouragement that induced me to undertake the work. With gratitude I acknowledge my obligations to Dr. Watson, Dr. Franklin, and Mr. Canton, for the books, and other materials with which they have supplied me, and for the readiness with which they have given me any information in their power to procure. In a more especial manner am I obliged to Mr. Canton, for those original communications of his, which will be found in this work, and which cannot fail to give a value to it, in the esteem of all the lovers of electricity. My grateful acknowledgments are also due to the
Rev.

Rev. Mr. Price F. R. S. and to the Rev. Mr. Holt, our professor of Natural Philosophy at Warrington, for the attention they have given to the work, and for the many important services they have rendered me with respect to it.

To the gentlemen above-mentioned the public is, likewise, indebted for whatever they may think of value in the *original experiments* which I have related of my own. It was from conversing with them that I was first led to entertain the thought of attempting any thing new in this way, and it was their example, and favourable attention to my experiments, that animated me in the pursuit of them. In short, without them, neither my experiments, nor this work would have had any existence.

THE historical part of this work, the reader, I hope, will find to be full and circumstantial, and at the same time succinct. Every new fact, or important circumstance, I have noted as it arose; but I have abridged all long details, and have carefully avoided all digressions and repetitions. For this purpose, I have perused every original author, to which I could have recourse; and every quotation in the margin points to the authority that I myself consulted, and from which the account in the text was actually taken. Where I could not procure the original authors, I was obliged to quote them at second hand, but the reference will always show where that has been done. That I might not misrepresent any writer, I have generally given the reader his own words, or the plainest translation I could make of them; and this I have done, not only in direct quotations, but where, by a change of person, I have made the language my own.

I MADE it a rule to myself, and I think I have constantly adhered to it, to take no notice of the mistakes, misapprehensions,

sions, and altercations of electricians; except so far as, I apprehended, a knowledge of them might be useful to their successors. All the disputes which have no way contributed to the discovery of truth, I would gladly consign to eternal oblivion. Did it depend upon me, it should never be known to posterity, that there had ever been any such thing as envy, jealousy, or cavilling among the admirers of my favourite study. I have, as far as my best judgment could direct me, been just to the merits of all persons concerned. If any have made unjust claims, by arrogating to themselves the discoveries of others, I have silently restored them to the right owner, and generally without so much as giving a hint that any injustice had ever been committed. If I have, in any case, given a hint, I hope it will be thought, by the offending parties themselves, to be a very gentle one; and that it will be a *memento*, which will not be without its use.

I THINK I have kept clear of any mean partiality towards my own countrymen, and even my own acquaintance. If English authors are oftner quoted than foreign, it is because they were more easily procured: and I have found a difficulty I could not have expected, in procuring foreign publications upon this subject.

I FIND it impossible to write a preface to this work, without discovering a little of the enthusiasm which I have contracted from an attention to it, by expressing my wishes, that more persons, of a studious and retired life, would admit this part of experimental philosophy into their studies. They would find it agreeably to diversify a course of study, by mixing something of action with speculation, and giving some employment to the hands and arms, as well as to the head. Electrical experiments are, of all others, the cleanest, and the most elegant, that the compass of philosophy exhibits. They are performed with the least trou-

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ble, there is an amazing variety in them, they furnish the most pleasing and surprising appearances for the entertainment of one's friends, and the expence of instruments may well be supplied, by a proportionable deduction from the purchase of books, which are generally read and laid aside, without yielding half the entertainment.

THE instruction we are able to get from books is, comparatively, soon exhausted; but philosophical instruments are an endless fund of knowledge. By philosophical instruments, however, I do not here mean the globes, the orrery, and others, which are only the means which ingenious men have hit upon, to explain their own conceptions of things to others; and which, therefore, like books, have no uses more extensive than the views of human ingenuity; but such as the air pump, condensing engine, pyrometer, &c. (with which electrical machines are to be ranked) and which exhibit the operations of nature, that is of the God of nature himself, which are infinitely various. By the help of these machines, we are able to put an endless variety of things into an endless variety of situations, while nature herself is the agent that shows the result. Hereby the laws of her action are observed, and the most important discoveries may be made; such as those who first contrived the instrument could have no idea of.

IN electricity, in particular, there is the greatest room to make new discoveries. It is a field but just opened, and requires no great stock of particular preparatory knowledge: so that any person who is tolerably well versed in experimental philosophy, may presently be upon a level with the most experienced electricians. Nay, this history shows, that several raw adventurers have made themselves as considerable, as some who have been, in other respects, the greatest philosophers. I need not tell my
reader

reader of how great weight this consideration is, to induce him to provide himself with an electrical apparatus. The pleasure arising from the most trifling discoveries of one's own, far exceeds what we receive from understanding the much more important discoveries of others; and a mere reader has no chance of finding new truths, in comparison of him who now and then amuses himself with philosophical experiments.

HUMAN happiness depends chiefly upon having some object to pursue, and upon the vigour with which our faculties are exerted in the pursuit. And, certainly, we must be much more interested in pursuits wholly our own, than when we are merely following the track of others. Besides, this pleasure has reinforcements from a variety of sources, which I shall not here undertake to trace; but which contribute to heighten the sensation, far beyond any thing else of this kind that can be experienced by a person of a speculative turn of mind.

IT is a great recommendation of the study of electricity, that it now appears to be, by no means, a small object. The electric fluid is no local, or occasional agent in the theatre of the world. Late discoveries show that its presence and effects are every where, and that it acts a principal part in the grandest and most interesting scenes of nature. It is not, like magnetism, confined to one kind of bodies, but every thing we know is a conductor or non-conductor of electricity. These are properties as essential and important as any they are possessed of, and can hardly fail to show themselves wherever the bodies are concerned.

HITHERTO philosophy has been chiefly conversant about the more sensible properties of bodies; electricity, together with chymistry, and the doctrine of light and colours, seems to be giving us an inlet into their internal structure, on which all their sensible properties depend. By pursuing this new light, there-

therefore, the bounds of natural science may possibly be extended, beyond what we can now form an idea of. New worlds may open to our view, and the glory of the great Sir Isaac Newton himself, and all his contemporaries, be eclipsed, by a new set of philosophers, in quite a new field of speculation. Could that great man revisit the earth, and view the experiments of the present race of electricians, he would be no less amazed than Roger Bacon, or Sir Francis would have been at his. The electric shock itself, if it be considered attentively, will appear almost as surprising, as any discovery that he made; and the man who could have made that discovery, by any reasoning *a priori*, would have been reckoned a very great genius: but electrical discoveries have been made so much by accident, that it is more the powers of nature, than of human genius, that excite our wonder with respect to them. But if the simple electric shock would have appeared so extraordinary to Sir Isaac Newton, what would he have said upon seeing the effects of a modern electrical battery, and an apparatus for drawing lightning from the clouds! What inexpressible pleasure would it give a modern electrician, were the thing possible, to entertain such a man as Sir Isaac for a few hours with his principal experiments!

To return from this excursion to the business of a preface: Besides relating the history of electrical discoveries, in the order in which they were made, I thought it necessary, in order to make the work more useful, especially to young electricians, to subjoin a methodical treatise on the subject, containing the substance of the history in another form, with observations and instructions of my own. The particular uses of these parts of the work are expressed at large in the introductions to them.

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And, in the last place, I have given an account of such original experiments as I have been so fortunate as to hit upon myself.

I INTITLED the work the *history and present state of electricity*; and whether there be any more editions of the whole work or not, care will be taken to preserve the propriety of the title, by occasionally printing ADDITIONS, in the same size, as new discoveries are made; which will always be sold at a reasonable price to the purchasers of the book; or given *gratis*, if the bulk be inconsiderable.

CONSIDERING what respectable persons have already honoured this work with their valuable communications, I hope it will not be deemed arrogance in me, if I here advertise, that if any person shall make discoveries in electricity, which he would chuse to see recorded in this history, he will oblige me by a communication of them; and if they be really original, a proper place shall certainly be assigned to them in the next edition, or paper of additions. And I hope that, if electricians in general would fall into this method, and make either a periodical, or occasional, but joint communication of their discoveries to the public, the greatest advantage would thence accrue to the science.

THE business of philosophy is so multiplied, that all the books of general philosophical transactions cannot be purchased by many persons, or read by any person. It is high time to *subdivide* the business, that every man may have an opportunity of seeing every thing that relates to his own favourite pursuit; and all the various branches of philosophy would find their account in this amicable separation. Thus the numerous branches of a large overgrown family, in the patriarchal ages, found it necessary to separate; and the convenience of the whole, and the strength, and increase of each branch were promoted by the separation. Let the youngest daughter of the sciences set
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the example to the rest, and show that she thinks herself considerable enough to make her appearance in the world without the company of her sisters.

BUT before this general separation, let each collect together every thing that belongs to her, and march off with her whole stock. To drop the allusion: let histories be written of all that has been done in every particular branch of science, and let the whole be seen at one view. And when once the entire progress, and present state of every science shall be fully and fairly exhibited, I doubt not but we shall see a new and capital *era* commence in the history of all the sciences. Such an easy, full, and comprehensive view of what has been done hitherto could not fail to give new life to philosophical inquiries. It would suggest an infinity of new experiments, and would undoubtedly greatly accelerate the progress of knowledge; which is at present retarded, as it were, by its own weight, and the mutual entanglement of its several parts.

I WILL just throw out a farther hint, of what, I think, might be favourable to the increase of philosophical knowledge. At present there are, in different countries in Europe, large incorporated societies, with funds for promoting philosophical knowledge in general. Let philosophers now begin to subdivide themselves, and enter into smaller combinations. Let the several companies make small funds, and appoint a director of experiments. Let every member have a right to appoint the trial of experiments in some proportion to the sum he subscribes, and let a periodical account be published of the result of them all, successful or unsuccessful. In this manner, the powers of all the members would be united, and increased. Nothing would be left untried, which could be compassed at a moderate expence, and it being *one person's business* to attend to these experiments,

periments, they would be made, and reported without loss of time. Moreover, as all incorporations in these smaller societies should be avoided, they would be encouraged only in proportion as they were found to be useful; and success in smaller things would excite them to attempt greater.

I BY no means disapprove of large, general and incorporated societies. They have their peculiar uses too; but we see by experience, that they are apt to grow too large, and their forms are too slow for the dispatch of the *minutiae* of business, in the present multifarious state of philosophy. Let recourse be had to rich incorporated societies, to defray the expence of experiments, to which the funds of smaller societies shall be unequal. Let their transactions contain a summary of the more important discoveries, collected from the smaller periodical publications. Let them, by rewards, and other methods, encourage those who distinguish themselves in the inferior societies; and thus give a general attention to the whole business of philosophy.

I WISH all the incorporated philosophical societies in Europe would join their funds (and I wish they were sufficient for the purpose) to fit out ships for the complete discovery of the face of the earth, and for many capital experiments which can only be made in such extensive voyages.

PRINCES will never do this great business to any purpose. The spirit of adventure seems to be totally extinct in the present race of merchants. This discovery is a grand desideratum in science; and where may this pure and noble enthusiasm for such discoveries be expected but among philosophers, men uninfluenced by motives either of policy or gain? Let us think ourselves happy if princes give no obstruction to such designs. Let them fight for the countries when they are discovered, and let merchants scramble for the advantage that may be made of them. It will

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be an acquisition to philosophers if the seat of war be removed so far from the seat of science; and fresh room will be given to the exertion of genius in trade, when the old beaten track is deserted, when the old system of traffick is unhinged, and when new and more extensive plans of commerce take place. I congratulate the present race of philosophers on what is doing by the English court in this way; for with whatever view expedients into the South seas are made, they cannot but be favourable to philosophy.

NATURAL Philosophy is a science which more especially requires the aid of wealth. Many others require nothing but what a man's own reflection may furnish him with. They who cultivate them find within themselves every thing they want. But experimental philosophy is not so independent. Nature will not be put out of her way, and suffer her materials to be thrown into all that variety of situations, which philosophy requires, in order to discover her wonderful powers, without trouble and expence. Hence the patronage of the great is essential to the flourishing state of this science. Others may project great improvements, but they only have the power of carrying them into execution.

BESIDES, they are the higher classes of men which are most interested in the extension of all kinds of natural knowledge; as they are most able to avail themselves of any discoveries, which lead to the felicity and embellishment of human life. Almost all the elegancies of life are the produce of those polite arts, which could have had no existence without natural science, and which receive daily improvements from the same source. From the great and the opulent, therefore, these sciences have a natural claim for protection; and it is evidently their interest, not to

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suffer promising inquiries to be suspended, for want of the means of prosecuting them.

BUT other motives, besides this selfish one, may reasonably be supposed to attach persons in the higher ranks of life to the sciences; motives more exalted, and flowing from the most extensive benevolence. From Natural Philosophy have flowed all those great inventions, by means of which mankind in general are able to subsist with more ease, and in greater numbers upon the face of the earth. Hence arise the capital advantages of men above brutes, and of civilization above barbarity. And by these sciences also it is, that the views of the human mind itself are enlarged, and our common nature improved and enobled. It is for the honour of the species, therefore, that these sciences should be cultivated with the utmost attention.

AND of whom may these enlarged views, comprehensive of such great objects, be expected, but of those whom divine providence has raised above the rest of mankind. Being free from most of the cares peculiar to individuals, they may embrace the interests of the whole species, feel for the wants of mankind, and be concerned to support the dignity of human nature.

GLADLY would I indulge the hope, that we shall soon see these motives operating in a more extensive manner than they have hitherto done; that by the illustrious example of a few, a taste for natural science will be excited in many, in whom it will operate the most effectually to the advantage of science and of the world; and that all kinds of philosophical inquiries will, henceforward, be conducted with more spirit, and with more success than ever.

WERE I to pursue this subject, it would carry me far beyond the reasonable bounds of a preface. I shall therefore conclude

with mentioning that sentiment, which ought to be uppermost in the mind of every philosopher, whatever be the immediate object of his pursuit; that speculation is only of use as it leads to *practice*, that the immediate use of natural science is the power it gives us over nature, by means of the knowledge we acquire of its laws; whereby human life is, in its present state, made more comfortable and happy; but that the greatest, and noblest use of philosophical speculation is the discipline of the heart, and the opportunity it affords of inculcating benevolent and pious sentiments upon the mind.

A PHILOSOPHER ought to be something greater, and better than another man. The contemplation of the works of God should give a sublimity to his virtue, should expand his benevolence, extinguish every thing mean, base, and selfish in his nature, give a dignity to all his sentiments, and teach him to aspire to the moral perfections of the great author of all things. What great and exalted beings would philosophers be, would they but let the objects about which they are conversant have their proper moral effect upon their minds! A life spent in the contemplation of the productions of divine power, wisdom, and goodness, would be a life of devotion. The more we see of the wonderful structure of the world, and of the laws of nature, the more clearly do we comprehend their admirable uses, to make all the percipient creation happy: a sentiment, which cannot but fill the heart with unbounded love, gratitude, and joy.

EVEN every thing painful and disagreeable in the world appears to a philosopher, upon a more attentive examination, to be excellently provided, as a remedy of some greater inconvenience, or a necessary means of a much greater happiness; so that, from this elevated point of view, he sees all temporary evils

evils and inconveniences to vanish, in the glorious prospect of the greater good to which they are subservient. Hence he is able to venerate and rejoice in God, not only in the bright sun shine, but also in the darkest shades of nature, whereas vulgar minds are apt to be disconcerted with the appearance of evil.

NOR is the cultivation of piety useful to us only as *men*, it is even useful to us as *philosophers*: and as true philosophy tends to promote piety, so a generous and manly piety is, reciprocally, subservient to the purposes of philosophy; and this both in a direct, and indirect manner. While we keep in view the great final cause of all the parts and the laws of nature, we have some clue, by which to trace the efficient cause. This is most of all obvious in that part of philosophy which respects the animal creation. As the great and excellent Dr. Hartley observes. " Since this world is a system of benevolence, and consequently
 " its author the object of unbounded love and adoration, bene-
 " volence and piety are our only true guides in our inquiries
 " into it; the only keys which will unlock the mysteries of
 " nature, and clues which lead through her labyrinths. Of
 " this all branches of natural history, and natural philosophy
 " afford abundant instances. In all these inquiries, let the in-
 " quirer take it for granted previously, that every thing is right,
 " and the best that can be *ceteris manentibus*; that is, let him,
 " with a pious confidence, seek for benevolent purposes, and
 " he will be always directed to the right road; and after a due
 " continuance in it, attain to some new and valuable truth:
 " whereas every other principle and motive of examination,
 " being foreign to the great plan on which the universe is con-
 " structed, must lead into endless mazes, errors, and per-
 " plexities. *

* Hartley's Observations on Man, Vol. 2. p. 245.

WITH respect to the indirect use of piety, it must be observed, that the tranquility, and cheerfulness of mind, which results from devotion forms an excellent temper for conducting philosophical inquiries; tending to make them both more pleasant, and more successful. The sentiments of religion and piety tend to cure the mind of envy, jealousy, conceit, and every other mean passion, which both disgrace the lovers of science, and retard the progress of it, by laying an undue bias upon the mind, and diverting it from the calm pursuit of truth.

LASTLY, let it be remembered, that a taste for science, pleasing, and even honourable as it is, is not one of the highest passions of our nature, that the pleasures it furnishes are even but one degree, above those of sense, and therefore that temperance is requisite in all scientific pursuits. Besides the duties of every man's proper station in life, which ought ever to be held sacred and inviolate, the calls of piety, common friendship, and many other avocations ought generally to be heard before that of study. It is, therefore, only a small share of their leisure, that most men can be justified in giving to the pursuit of science; though this share is more or less, in proportion to a man's situation in life, his natural abilities, and the opportunity he has for conducting his inquiries.

I SHALL conclude with another passage from Dr. Hartley to this purpose. " Though the pursuit of truth be an entertain-
 " ment and employment suitable to our rational natures, and a
 " duty to him who is the fountain of all knowledge and truth,
 " yet we must make frequent intervals and interruptions; else
 " the study of science, without a view to God and our duty, and
 " from a vain desire of applause, will get possession of our
 " hearts, engross them wholly, and by taking deeper root than
 " the pursuit of vain amusements, become, in the end, a much
 " more

“ more dangerous, and obstinate evil than that. Nothing can
 “ easily exceed the vain-glory, self-conceit, arrogance, emula-
 “ tion, and envy, that are found in the eminent professors of the
 “ sciences, Mathematics, Natural Philosophy, and even Divi-
 “ nity itself. Temperance in these studies is, therefore, evi-
 “ dently required, both in order to check the rise of such ill
 “ passions, and to give room for the cultivation of other essen-
 “ tial parts of our natures. It is with these pleasures as with
 “ the sensible ones; our appetites must not be made the mea-
 “ sure of our indulgence, but we ought to refer all to a higher
 “ rule.

“ BUT when the pursuit of truth is directed by this higher
 “ rule, and entered upon with a view to the glory of God, and
 “ the good of mankind, there is no employment more worthy
 “ of our natures, or more conducive to their purification and
 “ perfection. *

* *Hartley's Observations on Man*, Vol. 2. p. 255, &c.

WARRINGTON,
 MARCH 1767.

ADVER-

Advertisement

Concerning the second Edition ; and some
Additions to it.

THE method I took to distinguish the books I had seen from those I had not seen, in the catalogue of electric authors, subjoined to the first edition of this work, has been attended with the advantage I promised myself from it ; several persons, who were in possession of the books I had not seen, having communicated them to me ; and I have carefully perused them, and digested their contents into this second edition. Far the greater part of these new authors, the reader will perceive by the catalogue, were German, and wrote in high Dutch, a language with which, I believe, the literati of this country are but little acquainted, which might be the reason why neither myself nor my friends had ever heard of them before. Though the new materials they have supplied cannot be said to be of the first importance, many of the articles are very curious, and I hope the reader, as well as myself, will think that they have well repaid me for my trouble in learning the language.

It is certainly much to be regretted that philosophers have not one common language ; but neither the theory of language in general, nor the nature and analogies of things to be expressed by it are yet sufficiently understood, to enable us to contrive a new and philosophical one, which might be easily learnt, and would be completely adequate to all the purposes of science ; and Latin is a language which persons of a philosophical turn of
mind

mind have seldom leisure to make themselves so much masters of, as to write in it with that elegance which the taste of the age requires. Besides books written in Latin are but little read, at least, in England; and therefore could have no sale with us. These circumstances make it the more necessary, that there should be, in every country, persons possessed of a competent knowledge of foreign languages, who should be attentive to the progress of science abroad, and communicate to their countrymen all useful discoveries as they are made. Besides the improvements in the history, and other parts of this work, the reader will find, in this edition, an addition of three intire sections of original experiments. All that are of the least consequence are printed, and sold separately, for the benefit of those who purchased the first edition.

As I did not receive the new edition of Dr. Franklin's Letters, till near one half of this work was printed off, I am obliged to add, in this place, a few things, which I could have wished to have inserted in the period which relates to his discoveries.

THE power of points to throw off the electric fire was communicated to Dr. Franklin by his friend Mr. Thomas Hopkinson, who electrified an iron ball of three or four inches diameter with a needle fastened to it, expecting to draw a stronger spark from the point, as from a kind of focus, but was surprised to find little or none, p. 5.

THE manner in which Dr. Franklin first conceived the practicability of drawing lightning from the clouds may be seen from an extract which he has given us from his memorandums. November 7th. 1749. After enumerating all the known points of resemblance between lightning and electricity, he concludes with saying, the electric fluid is attracted by points. We do

not

not know whether this property is in lightning, but since they agree in all the particulars wherein we can already compare them, is it not probable that they agree likewise in this? Let the experiment be made, p. 323.

PROFESSOR WINTHROP observed his apparatus to be strongly electrified for several hours, while the snow, which fell the day before (and which had not electrified his apparatus while it was falling) was driven about by a high wind. This he had observed twice before, p. 434.

DR. FRANKLIN advises persons who are apprehensive of danger from lightning to sit in the middle of a room (provided it be not under a metal lustre suspended by a chain) sitting on one chair, and laying their feet upon another. It is still safer, he says, to bring two or three mattresses, or beds, into the middle of the room, and, folding them up double, to place the chairs upon them, for they, not being so good conductors as the walls, the lightning will not chuse to pass through them; but the safest place of all is in a hammock hung in silken cords, at an equal distance from all the sides of the room, p. 484. I would add, that the place of most absolute safety must be the cellar, and especially the middle of it; for when a person is lower than the surface of the earth, the lightning must strike it before it can possibly reach him.

THE Doctor thinks that, possibly, the mutual repulsion of the opposite sides of the *electrified cup* (see p. 709) may prevent the accumulation of an electric atmosphere upon them, and occasion it to stand, chiefly, on the outside. But he recommends it to the farther examination of the curious, p. 326.

LEEDS,
JANUARY 1769.

T H E
C O N T E N T S.

P A R T I.

THE HISTORY OF ELECTRICITY.

P E R I O D I.

E X P E R I M E N T S and discoveries in electricity prior to those
of Mr. Hawkeſbee. page 1

P E R I O D II.

The experiments and discoveries of Mr. Hawkeſbee. p. 15

P E R I O D III.

*The experiments and discoveries of Mr. Stephen Grey, which were
made prior to those of Mr. Du Faye, and which bring the history
of electricity to the year 1733.* p. 25

P E R I O D IV.

The experiments and discoveries of Mr. Du Faye. p. 43

P E R I O D V.

The continuation, and conclusion of Mr. Grey's experiments. p. 53

P E R I O D VI.

The experiments of Dr. Defaguliers. p. 62

d

P E R I O D

PERIOD VII.

Experiments of the Germans, and of Dr. Watſon, before the diſcovery of the Leyden phial in the year 1746. page 68

PERIOD VIII.

The hiſtory of electricity, from the diſcovery of the Leyden phial in the years 1745 and 1746, till Dr. Franklin's diſcoveries. p. 80

SEC. I. *The hiſtory of the Leyden phial itſelf, till Dr. Franklin's diſcoveries relating to it.* *ib.*

SEC. II. *The methods uſed by the French and Engliſh philoſophers, to meaſure the diſtance to which the electric ſtock can be carried, and the velocity with which it paſſes.* p. 100

SEC. III. *Miſcellaneous diſcoveries of Dr. Watſon, and others, till the time of Dr. Franklin.* p. 110

SEC. IV. *Experiments on animal and other organized bodies in this period, and other experiments connected with them, made chiefly by the Abbé Nollet.* p. 131

SEC. V. *The hiſtory of the medicated tubes, and other communications of medicinal virtues by electricity, with their various reſultations.* p. 141

PERIOD IX.

The experiments and diſcoveries of Dr. Franklin. p. 151

SEC. I. *Dr. Franklin's diſcoveries concerning the Leyden phial, and others connected with them.* *ib.*

SEC. II. *Dr. Franklin's diſcoveries concerning the ſimilarity of lightning and electricity.* p. 162

SEC. III. *Miſcellaneous diſcoveries of Dr. Franklin and his friends in America during the ſame period.* p. 176

PERIOD

PERIOD X.

- The history of electricity, from the time that Dr. Franklin made his experiments in America, till the year 1766.* page 183
- SEC. I. *Improvements in the electrical apparatus, with experiments and observations relating to it.* p. 185
- SEC. II. *Observations on the conducting power of various substances, and particularly Mr. Canton's experiments on air; and Signior Beccaria's on air, and water.* p. 193
- SEC. III. *Mr. Canton's experiments and discoveries relating to the surfaces of electric bodies, and others made in pursuance of them, or relating to the same subject; all tending to ascertain the distinction between the two electricities.* p. 208
- SEC. IV. *Mr. Delaval's experiments relating to the two electricities, and his controversy with Mr. Canton upon that subject.* p. 223
- SEC. V. *Mr. Canton's experiments and discoveries relating to bodies immersed in electric atmospheres, with the discoveries of others, made by pursuing them.* p. 231
- SEC. VI. *Mr. Symmer's experiments relating to the two electricities, and those made by Johannes Franciscus Cigna in pursuance of them.* p. 249
- SEC. VII. *The history of the Leyden Phial continued.* p. 270
- SEC. VIII. *Experiments and observations concerning electric light.* p. 281
- SEC. IX. *The electricity of the Tourmalin.* p. 297
- SEC. X. *Discoveries that have been made since those of Dr. Franklin, with respect to the sameness of lightning and electricity.* p. 309

- SEC. XI. *Observations on the general state of electricity in the atmosphere, and its more usual effects.* page 341
- SEC. XII. *The attempts that have been made to explain some of the more unusual appearances in the earth and heavens by electricity.* P. 351
- SEC. XIII. *Observations on the use of metallic conductors to secure buildings, &c. from the effects of lightning.* P. 374
- SEC. XIV. *Of medical electricity.* P. 383
- SEC. XV. *Miscellaneous experiments and discoveries made within this period.* 397

PART II.

- A SERIES OF PROPOSITIONS, COMPRISING ALL THE GENERAL PROPERTIES OF ELECTRICITY. P. 409

PART III.

- THEORIES OF ELECTRICITY. P. 417
- SEC. I. *Of philosophical theories in general, and the theories of electricity preceding that of Dr. Franklin.* ib.
- SEC. II. *The theory of positive and negative electricity.* p. 429
- SEC. III. *Of the theory of two electric fluids.* p. 441

PART IV.

- DESIDERATA IN THE SCIENCE OF ELECTRICITY, AND HINTS FOR THE FURTHER EXTENSION OF IT. p. 451
- SEC. I. *General observations on the present state of electricity.* ib.
- SEC. II. *Queries and hints calculated to promote further discoveries in electricity.* p. 458
- I. *Queries and hints concerning the electric fluid.* ib.
- II. _____ electrics and conductors. p. 461
- III.

III.	<i>Queries and hints concerning excitation.</i>	page 462
IV.	_____ electrification.	p. 464
V.	_____ <i>the power of charging electrics. ib.</i>	
VI.	_____ <i>the electricity of glais.</i>	p. 466
VII.	_____ <i>the effect of electricity on animal bodies.</i>	p. 468
VIII.	_____ <i>the electricity of the atmosphere.</i>	p. 469
SEC. III.	<i>Branches of knowledge peculiarly useful to an electrician.</i>	P. 471

PART V.

OF THE CONSTRUCTION OF ELECTRICAL MACHINES,
AND THE PRINCIPAL PARTS OF AN ELECTRICAL APPARATUS. P. 479

SEC. I. *General observations on the construction of an electrical apparatus.* ib.

SEC. II. *A description of some particular electrical machines, with observations on their principal advantages and defects.* p. 495

PART VI.

PRACTICAL MAXIMS FOR THE USE OF YOUNG ELECTRICIANS. p. 506

PART VII.

A DESCRIPTION OF THE MOST ENTERTAINING EXPERIMENTS PERFORMED BY ELECTRICITY. p. 518

SEC. I. *Entertaining experiments in which the Leyden phial is not used.* p. 521

SEC. II. *Entertaining experiments performed by means of the Leyden phial.* p. 531

SEC.

- SEC. III. *Entertaining experiments made by a combination of philosophical instruments.* page 539

PART VIII.

NEW EXPERIMENTS IN ELECTRICITY, MADE CHIEFLY
IN THE YEAR 1766. P. 543

- SEC. I. *Experiments on excitation, particularly of tubes in which air is condensed, and of large glass globes.* P. 550
- SEC. II. *Experiments which prove a current of air from the points of bodies electrified either positively or negatively.* P. 559
- SEC. III. *Experiments on mephitic air, and charcoal.* p. 566
- SEC. IV. *Experiments on the conducting power of various substances.* p. 576
- SEC. V. *Experiments on the diffusion of electricity over the surfaces of glass tubes, containing a new method of giving the electric shock.* p. 588
- SEC. VI. *Experiments to verify several particulars of Signior Beccaria's theory of electricity; particularly concerning the electric matter carrying into its path light substances to assist its passage.* p. 601
- SEC. VII. *Various experiments relating to charging and discharging glass jars and batteries.* p. 608
- SEC. VIII. *Experiments on animals.* p. 618
- SEC. IX. *Experiments on the circular spots made on pieces of metal by large electrical explosions.* p. 623
- SEC. X. *Experiments on the effects of the electrical explosion discharged through a brass chain, and other metallic substances.* p. 636
- SEC. XI. *Experiments on the passage of the electrical explosion over*

- over the surface of *some condensing substances, without entering them.* page 648
- SEC. XII. *Experiments on the Tourmalin.* p. 659
- SEC. XIII. *Experiments in which rings, consisting of all the prismatic colours, were made by electrical explosions on the surfaces of metals.* p. 675
- SEC. XIV. *Experiments on the lateral force of electrical explosions.* p. 681
- SEC. XV. *Various experiments on the force of electrical explosions.* p. 687
- SEC. XVI. *Miscellaneous Experiments.* p. 694
- I. *Observations on the electric spark taken through several pieces of metal.* ib.
- II. *A deception relating to the direction of the electric spark.* p. 695
- III. *An experiment intended to ascertain whether electric substances, in their natural state, contain more of the electric fluid than conductors.* p. 696
- IV. *The musical tone of various discharges ascertained.* ib.
- V. *Experiments on the effects of giving a metallic tinge to the surface of glass.* p. 698
- VI. *An experiment intended to ascertain whether fermentation contributes to the production of electricity.* p. 700
- VII. *An experiment intended to ascertain whether evaporation contributes to the production of electricity.* ib.
- VIII. *An experiment intended to ascertain whether freezing be accelerated or retarded by electrification.* ib.
- IX. *The examination of a glass tube, which had been a long time charged and hermetically sealed.* p. 701
- X.

- X. *The weight requisite to bring some bodies into contact ascertained by the electrical explosion.* page 702
- XI. *The effect of the electrical explosion transmitted through various liquors.* p. 703
- XII. *Observations on the colours of electric light.* p. 704
- XIII. *Observations on the small wires that collect electricity from the excited globe.* p. 706
- XIV. *Experiments intended to ascertain the difference in the conducting power of different metals.* p. 707
- XV. *Experiments with an electrified cup.* p. 709

T H E
H I S T O R Y
AND PRESENT STATE OF
E L E C T R I C I T Y .

P A R T I .
THE HISTORY OF ELECTRICITY.

P E R I O D I .

EXPERIMENTS AND DISCOVERIES IN ELECTRICITY PRIOR TO
THOSE OF MR. HAWKESBEE.

THE history of philosophy contains nothing earlier than the observation, that yellow amber, when rubbed, has the power of attracting light bodies. Thales of Miletus, the father of the Ionic philosophy, who flourished about six hundred years before Christ, was so much struck with this property of amber, that he imagined it was animated. But the first writer, who expressly mentions this substance, is Theophrastus, who flourished about the year 300 before Christ. He says, in his book concerning precious stones, sect. 53, that amber (which he supposes to be a native fossil) has the same property of attracting light bodies with the lycnurium; which,

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he observes, attracts not only straws, and small pieces of sticks, but even thin pieces of copper and iron. What he says farther of the lyncurium will be related under the article of the *tourmalin*, which Dr. Watson has, in a manner, proved to be the same substance.

FROM *ἰσχυρὸν*, the Greek name for amber, is derived the term ELECTRICITY, which is now extended to signify not only the power of attracting light bodies inherent in amber, but other powers connected with it, in whatever bodies they are supposed to reside, or to whatever bodies they may be communicated.

THE attractive nature of amber is occasionally mentioned by Pliny, and other later naturalists; particularly by Gassendus, Kenelm Digby, and Sir Thomas Brown; but excepting the electricity of the substance called *jet*,* the discovery of which was very late (though I have not been able to find its author) no advances were made in electricity till the subject was undertaken by William Gilbert, a native of Colchester, and a physician at London; who, in his excellent Latin treatise *de magnete*, published in the year 1600, relates a great variety of electrical experiments. Considering the time in which this author wrote, and how little was known of the subject before him, his discoveries may be justly deemed considerable, though they appear trifling when compared with those which have been made since his time.

To him we owe a great augmentation of the list of electric bodies, as also of the bodies on which electrics can act; and he has carefully noted several capital circumstances relating to the manner of their action, though his theory of electricity was very imperfect, as might be expected.

* Mr. Bose is said to have shown, that the *agate* was very early known to have electric powers. *Geschichte*, Vol. 1. p. 179.

AMBER and jet were, as I observed before, the only substances which, before his time, were known to have the property of attracting light bodies when rubbed; but he found the same property in the *diamond, sapphire, carbuncle, iris, amethyst, opal, vincentina, Bristol stone, beryl, and chrystal*. He also observes that *glass*, especially that which is clear and transparent, has the same property; likewise all *falsitious gems*, made of glass or chrystal; *glass of antimony*, most *sparry substances*, and *belemnites*. Lastly, he concludes his catalogue of electric substances with *sulphur, mastic, sealing wax* made of gum lac tinged with various colours, *hard resin, sal gem, talck*, and *rocbe alum*. Rosin, he said, possessed this property but in a small degree, and the three last mentioned substances, only when the air was clear and free from moisture.

ALL these substances, he observes, attracted not only straws, but all metals, all kinds of wood, stones, earth, water, oil; in short, whatever is solid, and the object of our senses. But he imagined that air, flame, bodies ignited, and all matter which was extremely rare was not subject to this attraction, Gross smoke, he found, was attracted very sensibly, but that which was attenuated very little.

FRICTION, he says, is, in general, necessary to excite the virtue of these substances; though he had one large and smooth piece of amber which would act without friction. But with respect to this he probably deceived himself. The most effectual friction, he observed to be that which was light and quick; and he found that electrical appearances were strongest when the air was dry, and the wind north or east, at which time electric substances would act ten minutes after excitation. But he says, that a moist air, or a southerly wind almost annihilates the elec-

tric virtue. The same effect he also observed from the interposition of moisture of any kind, as from the breath, and many other substances, but not always from the interposition of sarsnet. He says that light and pure oil, sprinkled upon electrics, after excitation, did not obstruct their virtue; but that brandy, or spirit of wine did. He also says, that chrystal, talck, glafs, and all other electrics lost their virtue after being burned or roasted. But this was, in some measure, a mistake. The heat of the sun, collected by a burning glafs, he says, is so far from exciting amber, and other electrics, that it impairs the virtue of them all; though, when electrics have been excited, they will retain their virtue longer in the sun-shine than in the shade.

MOST of the experiments of this author were made with long thin pieces of metal, and other substances, suspended freely on their centers, to the extremities of which he presented the electrics he had excited. His experiments on water were made by presenting a round drop of it upon a dry substance to the excited electric; and it is remarkable, that he observed the same conical figure of the electrified drops which Mr. Grey afterwards discovered, and which will be related more at large in its proper place. Gilbert concluded, that air was not affected by electrical attraction, because the flame of a candle was not: for the flame, he says, would be disturbed if the air had the least motion given to it.

GILBERT imagined that electrical attraction was performed in the same manner as the attraction of cohesion. Two drops of water, he observed, rush together when they are brought into contact; and electrics, he says, are virtually brought into contact with the bodies they act upon, by means of their effluvia, excited by friction.

AMONG

AMONG other differences between electric and magnetic attraction, some of which are very just, and others whimsical enough, he says, that magnetic bodies rush together mutually; whereas in electrical attraction it is only the electric that exerts any power. He observes also particularly, that in magnetism there is both attraction and repulsion, but in electricity only the former, and never the latter.*

SUCH were the discoveries of our countryman Gilbert, who may justly be called the father of modern electricity, though it be true that he left his child in its very infancy.

SIR FRANCIS BACON, in his *Physiological Remains*, gives a catalogue of bodies attractive and not attractive; but it differs in nothing worth mentioning from that of Gilbert, and he does not seem to have made any observations of his own relating to the subject.

ABOUT 30 years after Gilbert, NICOLAUS CABEUS, a Jesuit at Ferrara, repeated his experiments; and found that *white wax*, almost all the *gums*, and *crude gypsum* were to be ranked among electric bodies.†

THESE remarkable phenomena relating to amber, and other electric substances did not escape the attention of the inquisitive and sagacious Mr. Boyle, who flourished about the year 1670.‡ He made some addition to the catalogue of electric substances, and attended to some circumstances relating to electrical attraction, which had escaped the observation of philosophers who lived before him.

* Gilbert de magnete, Lib. 2. Cap. 2. † Geschichte, Vol. 1. p. 186.

‡ It is said that Mr. Boyle followed Otto Guericke in his electrical experiments, and was excited to make them by his example. However as they were cotemporary writers, and their claims to discovery do not interfere with one another, there is no occasion to transpose the paragraphs in this work relating to them.

He found that the hard cake which remains after evaporating good turpentine was electrical, as also the hard mass which remains after distilling petroleum and spirit of nitre, glass of lead, the caput mortuum of amber, and the cornelian; but he could not find that property in the emerald, and he thought that glass possessed it but in a very low degree.

He found, that the electricity of all bodies capable of having it excited in them was increased by wiping, and warming them, previous to their being rubbed. By this means he made an electric body, no bigger than a pea, move a steel needle, which was freely poised, three minutes after he had left off rubbing it. He also found, that it was useful to have the surfaces of electric bodies made very smooth, except in the case of one diamond, on which he tried some experiments; which, though it was rough, was, he says, possessed of a stronger electric virtue than any polished one he had met with.

He observed that excited electrics would attract all kinds of bodies promiscuously, whether electric or not; that excited amber, for instance, would attract both powder of amber, and small pieces of it; differing, as he takes notice, from the property of the load-stone, which acts only on one kind of matter. He found, that his electrics would attract smoke very easily, and takes some pains to account for their not sensibly attracting flame, which Gilbert excepted from the bodies attracted by electricity.

THESE attractions, he found, did not depend upon the air: for he observed that they took place in vacuo. He suspended a piece of excited amber over a light body in a glass receiver, and saw, that when a vacuum was made, and the amber let down near the light body, it was attracted, as if it had been in the open

open air.* But S. Beccaria asserts, that there is no electrical attraction in a perfect vacuum.

MR. BOYLE made an experiment to try whether an excited electric was acted upon by other bodies, as strongly as it acted upon them, and it succeeded: for, suspending his excited electric, he saw that it was sensibly moved by the approach of any other body. We should now be surpris'd, that any person should not have concluded *a priori*, that if an electric body attracted other bodies, it must, in return, be attracted by them, action and reaction being universally equal to one another. But it must be considered, that this axiom was not so well understood in Mr. Boyle's time, nor until it was afterwards explained in its full latitude by Sir Isaac Newton.†

MR. BOYLE got a glimpse, as we may say, of the electric light: for he found, that a curious diamond, which Mr. Clayton brought from Italy, gave light in the dark, when it was rubbed against any kind of stuff; and he found that, by the same treatment, it became electrical. He also observed the same property in several other diamonds.‡

THESE few experiments of Mr. Boyle's, we see, relate only to a few circumstances attending the simple property of electrical attraction. The nearest approach that he made to the discovery of electrical repulsion was his observing, that light bodies, as feathers, &c. would cling to his fingers, and other substances, after they had been attracted by his electrics. He had seen but little of the electric light, and little imagined what astonishing effects would be afterwards produced by the same wonderful

* Histoire de l'électricité, p. 6. † Boyle's Mechanical production of electricity.

‡ Secondat's history of electricity, p. 141.

power,

power, and how large a field he was opening for philosophical speculation in future time.

MR. BOYLE'S theory of electrical attraction was, that the electric emitted a glutinous effluvia, which laid hold of small bodies in its way, and, in its return to the body which emitted it, carried them back with it. One James Hartman, whose account of amber is published in the Philosophical Transactions,* pretends to prove by experiment, that electrical attraction was really owing to the emission of glutinous particles. He took two electric substances, viz. pieces of colophonia, and from one of them made a distillation of a black balsam, and thereby deprived it of its attractive power. He says, that the electric, which was not distilled, retained its fatty substance, whereas the other was, by distillation, reduced to a mere *caput mortuum*, and retained no degree of its bituminous fat. In consequence of this hypothesis, he gives it as his opinion, that amber attracts light bodies more powerfully than other substances, because it emits oily and tenacious effluvia more copiously than they do.

CONTEMPORARY with Mr. Boyle was Otto Guericke, Burgo-master of Magdebourgh, and the celebrated inventor of the air-pump, who is likewise intitled to a distinguished place among the first improvers of electricity.

THIS philosopher made his experiments with a globe of sulphur, made by melting that substance in a hollow globe of glass, and afterwards breaking the glass from off it. He little imagined that the glass globe itself, with or without the sulphur, would have answered his purpose as well. This globe of sulphur he mounted upon an axis, and whirled it in a wooden frame, rubbing

* Abridgment, Vol. 2. p. 473.

it at the same time with his hand; and by this means he performed all the electrical experiments which were known before his time.

HIS was the discovery, that a body once attracted by an excited electric was repelled by it, and not attracted again till it had been touched by some other body. In this manner he kept a feather a long time suspended in the air above his sulphur globe; but he observed, that if he drove it near a linen thread, or the flame of a candle, it instantly retreated to the globe, without having been in contact with any sensible body.

NEITHER the sound, nor the light produced by the excitation of his globe escaped the notice of this accurate philosopher, though he seems not to have observed them in a very great degree: for he was obliged to hold his ear near the globe to perceive the hissing sound of the electric fire; and he compares the light which it gave in the same circumstances to that which is seen when sugar is pounded in the dark.

BUT the most remarkable experiments of this philosopher were two, which depend upon a property of the electric fluid that has not been illustrated till within these late years; viz. that bodies immersed in electric atmospheres are themselves electrified, and with an electricity opposite to that of the atmosphere. Threads suspended within a small distance of his excited globe, he observed to be often repelled by his finger brought near them, and that a feather repelled by the globe always turned the same face towards it, like the moon with respect to the earth. This last experiment seems to have been wholly overlooked by later electricians, though it is a very curious one, and may be made with so much ease.*

* *Experimenta Magdeburgica*, Lib. 4. Cap. 15.

THE members of the Academy at Florence, about the time of Otto Guericke, made some electrical experiments with amber and gems. They observed that *flame* was not affected by electricity; but that it attracted all fluid bodies, and among the rest quicksilver; and that electric bodies showed no power when they were rubbed by smooth bodies, as glass, crystal, or ivory.*

A MUCH finer appearance of electric light than that which Otto Guericke's sulphur globe exhibited was observed by Dr. Wall. The account of it is published in the philosophical transactions.†

MAKING experiments upon artificial phosphorus, which he took to be an animal oil coagulated with a mineral acid, he was led to conjecture that amber, which he supposed to be a mineral oil coagulated with a mineral volatile acid, might be a natural phosphorus; and with this view he began to make experiments upon it, the result of which, being very curious and surprising, it will be most agreeable to my readers to see in the very words of the observer himself.

“ I FOUND,” says he, “ by gently rubbing a well polished piece of amber with my hand, in the dark, that it produced a light: whereupon I got a pretty large piece of amber, which I caused to be made long and taper, and drawing it gently through my hand, being very dry, it afforded a considerable light.

“ I THEN used many kinds of soft animal substances, and found that none did so well as wool. And now new phenomena offered themselves: for, upon drawing the piece of amber swiftly through the wollen cloth, and squeezing it

* Geschichte, Vol. 1. p. 182.

† Phil. Trans. abridged, Vol. 4. p. 275.

“ pretty

“ pretty hard with my hand, a prodigious number of little
“ cracklings were heard, and every one of these produced a little
“ flash of light; but when the amber was drawn gently and
“ slightly through the cloth, it produced only a light but no
“ crackling; but by holding ones finger at a little distance from
“ the amber, a large crackling is produced, with a great flash
“ of light succeeding it. And, what to me is very surprizing,
“ upon its eruption, it strikes the finger very sensibly, wherefo-
“ ever applied, with a push or a puff, like wind. The crackling
“ is full as loud as charcoal on fire, and five or six cracklings,
“ or more, according to the quickness of placing the finger,
“ have been produced from one single friction, light always
“ succeeding each of them.

“ Now I make no question, but upon using a longer and
“ larger piece of amber, both the cracklings and light would
“ be much greater, because I never yet found any crackling
“ from the head of my cane, though it is a pretty large one.
“ This light and crackling seems in some degree, to represent
“ thunder and lightning.”

AFTER reciting this experiment, he gives it as his opinion,
that all, or most bodies, which have electricity, give light, and
that it is the light which is the cause of their being electrical.
He found that light could also be produced by rubbing jet, red
sealing wax, made with gum lac and cinnabar, and the diamond.
He even imagined he could distinguish true from false diamonds
by this test.

NOTWITHSTANDING Dr. Wall made this beautiful discovery,
as he imagined (for he seems not to have seen what Otto Guericke
had written) of light proceeding from amber and other electric
bodies, we see that he laboured under a great deal of confusion

and misapprehension with respect to it. He says, that one thing appeared strange to him in the course of these experiments, which was, that though, upon friction with wool in the day time, the cracklings seemed to be full as many and as large, yet that by all the trials which he made, very little light appeared, though in the darkest room. He says that the best time for making these experiments is when the sun is 18° below the horizon, and that when the sun was so low, though the moon shone ever so bright, the light was the same as in the darkest room, which made him chuse to call it a noctiluca.

IT is remarkable that Dr. Wall compares the light and the crackling of his amber to thunder and lightning: so early was a similitude between the effects of electricity and lightning observed. But little was it imagined that the resemblance between them extended any farther than to appearances and effects. That the cause was the same in both, was a discovery reserved for Dr. Franklin, in a much later period.

THE great Sir Isaac Newton, though he by no means makes a principal figure in the history of electricity, yet made some electrical observations which engaged the attention of his philosophical friends; and which, independent of their being made by him, do well deserve to be transmitted to posterity. They seem to shew, that he was the first who observed, that excited glass attracted light bodies on the side opposite to that on which it was rubbed.

HAVING laid upon the table a round piece of glass, about two inches broad, in a brass ring, so that the glass might be one eighth of an inch from the table, and there rubbing the glass briskly, little fragments of paper, laid on the table, under the glass, began to be attracted, and move nimbly to and fro. After
he

he had done rubbing the glass, the papers would continue a considerable time in various motions; sometimes leaping up to the glass, and resting there a while, then leaping down and resting there, and then leaping up and down again; and this sometimes in lines seemingly perpendicular to the table, sometimes in oblique ones; sometimes also leaping up in one arch, and leaping down in another, divers times together, without sensibly resting between; sometimes skipping in a bow from one part of the glass to another, without touching the table, and sometimes hanging by a corner, and turning often very nimbly, as if they had been carried about in the midst of a whirlwind, and being otherwise variously moved, every paper with a different motion. Upon his sliding his finger on the upper side of the glass, though neither the glass, nor the inclosed air below, were moved, yet he observed, that the papers, as they hung under the glass, would receive some new motion, inclining this way or that, according as he moved his finger.

SOME of the motions, as that of hanging by a corner and twirling about, and that of leaping from one part of the glass to another without touching the table happened but seldom; but, he says, it made him take the more notice of them.*

AN account of this experiment Sir Isaac sent to the members of the R. Society in the year 1675, desiring it might be tried by them, and after some ineffectual attempts, and receiving further instructions, how to make it, they at length succeeded, and the thanks of the society were formally returned to him.†

UPON repeating the experiment with some variety of circumstances, Sir Isaac observes, that rubbing variously, or with

* Birch's Hist. of the R. Society, Vol. 3. p. 260, &c.

† Ib. 271.

various things, altered the case. At one time he rubbed a glass four inches broad, and one fourth thick with a napkin, twice as much as he used to do with his gown, and nothing would stir, and yet presently after, rubbing it with something else, the motion soon began. After the glass had been much rubbed, he thought the motions were not so lasting, and the day following he found the motions fainter, and more difficult to be excited than at first.*

SIR ISAAC also mentions electricity in two queries annexed to his treatise on Optics, from which we learn, that he imagined electric bodies when excited emitted an elastic fluid, which freely penetrated glass, and that the emission was performed by the vibratory motions of the parts of the excited bodies.†

* Birch's Hist. of the R. Society, Vol. 3. p. 270.

† Newton's Optics, octavo, p. 314, and 327.

PERIOD

PERIOD II. OF MR. HAWKESBEE.

THE EXPERIMENTS AND DISCOVERIES OF MR. HAWKESBEE.

AFTER Gilbert, Mr. Boyle, and Otto Guericke, Mr. Hawkesbee, who wrote in the year 1709, distinguished himself by experiments and discoveries in electricity. He first observed the great electric power of glass, the light proceeding from it, and the noise occasioned by it, together with a variety of phenomena relating to electrical attraction and repulsion. He was indefatigable in making experiments, and there are few persons to whom we are more indebted for a real advancement of this branch of knowledge. This will appear by the following succinct account of his experiments, related not exactly in the order in which he has published them, but according to their connection. This method I have chosen as best adapted to give the most distinct view of the whole.

I SHALL first relate the experiments he made concerning electrical attraction and repulsion, in many of which we shall see reason to admire his ingenious contrivances, and shall see that little was added to his observations, till the capital discovery of a *plus* and *minus* electricity by Dr. Watson and Dr. Franklin, and the farther illustration of that doctrine by Mr. Canton.

THE most curious of his experiments concerning electrical attraction and repulsion are those which shew the direction in which those powers are exerted.

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HAVING tied threads round a wire hoop, and brought it near to an excited globe or cylinder, he observed, that the threads kept a constant direction towards the center of the globe, or towards some point in the axis of the cylinder, in every position of the hoop; that this effect would continue for about four minutes after the whirling of the globe ceased, and that the effect was the same whether the wire was held above, or under the glass; or whether the glass was placed with its axis parallel, or perpendicular to the horizon.

He observed, that the threads pointing towards the center of the globe were attracted and repelled by a finger presented to them; that if the finger, or any other body, was brought very near the threads, they would be attracted; but that if it were brought to the distance of about an inch, they would be repelled, the reason of which difference he did not seem to understand.*

He tied threads to the axis of a globe and cylinder, and found that they diverged every way in straight lines from the place where they were tied, when the globe was whirled and rubbed. In both cases, he says, the threads would be repelled by the finger held on the opposite side of the glass, even without touching the glass; though they would sometimes suddenly jump towards it.† He observed further, that by blowing with his mouth towards the glass, at three or four feet distance, the threads would have a very considerable motion to them.

He found that threads, hanging freely in an unexcited globe, at rest, would be moved by the approach of any excited electric at a considerable distance, except in moist weather; which failure he accounts for, by supposing the moisture on the surface

* Physico-Mechanical Experiments, p. 75.

† Ib. p. 78.

of the glass prevented the free passage of the electric effluvia through it.*

THE varieties he observed on the appearances and properties of the electric light, are even more curious and surprising than his discoveries concerning electric attraction and repulsion. It is something remarkable that Mr. Hawkesbee's transition to electric light was like that of Dr. Wall, viz. from the light of phosphorus.

MR. HAWKESBEE first produced a considerable quantity of light by shaking quick-silver in a glass vessel, out of which the air was exhausted. Sometimes what he calls strange flashes of pale light were seen darting in a variety of directions, when the mercury was put in motion within an exhausted receiver.† But the discovery was probably accidental, and he did not seem, at that time, to know the reason of the appearance. He called this light the *mercurial phosphorus*, and did not consider the glass as any way concerned in producing it.

HE also found, that this appearance of electric light (which he still calls the *mercurial phosphorus*) did not require a very perfect vacuum, nor even a near approach to it.‡ Nay he sometimes produced that appearance of light by shaking mercury in a vessel, in which the air was of the same density with the external atmosphere, but still he had no idea of the glass contributing to the phenomenon. ||

HE observed a strong light in vacuo, and a small one in the open air, from rubbing amber upon woollen, but seems to have considered it as any hard body rubbing against a soft one. § He

* *Physico-Mechanical Experiments*, p. 160.

† *Ib.* 12.

‡ *Ib.* 14.

§ *Ib.* p. 18.

¶ *Ib.* 26.

also observed a vivid purple, and afterwards a pale light produced by rubbing glass upon woollen in vacuo.* He says that every fresh glass first gave a purple, and then a pale light, and that woollen tinged with salts or spirits produced a new, strong, and fulgurating light.†

IN the following experiments we find his ideas of electric light much more distinct, and the appearances the same that are usually exhibited by our present electrical machines, the structure of which, we shall find to be nearly the same with those which he used.

HE provided himself with a machine in which he could whirl a glass globe; and he observed, when the air was extracted out of it, that, upon applying his hand to the globe, a strong light appeared on the inside, and, upon letting in the air, he observed the light on the outside also; but with some very considerable differences in its appearances, sticking upon his fingers, and other bodies held near the globe. He also observed, upon this occasion, that one fourth of an atmosphere in the globe did very little diminish the light within. It is pleasing to observe, that the similar appearance in this experiment, and that with mercury in vacuo before-mentioned, made him suspect, though only suspect, that the light produced in the former case proceeded not from the mercury, but from the glass.

THE next experiment is of a very delicate and curious nature. It is not to be wondered at, that Mr. Hawkesbee did not understand the circumstances which contributed to it, as the explication of it depends upon principles which were not discovered till a much later period by Mr. Canton.

* *Physico-Mechanical Experiments*, p. 32.

† *Ib.* 34.

HOLDING an exhausted globe within the effluvia of an excited one, he observed a light in the exhausted globe, which presently died away, if it was kept at rest; but which was revived, and continued very strong, if the exhausted globe was kept in motion. Presenting an exhausted tube to the effluvia of an excited globe, it produced what he calls an interrupted flashing light. He imagined that the exhausted globe was excited by the attraction of the effluvia from the other globe, so little did he understand the true cause of this curious experiment.* When he says that light is producible by the effluvia of one glass falling upon another, he adds, that electric, (by which he means attractive) matter, is not to be brought forth by any such feeble strokes. He had before observed that, upon rubbing an exhausted tube, it discovered no attractive power, nor gave any light outwards, but only inwards.

HE found that when the friction was performed in vacuo, no electricity (that is attraction) could be produced;† but that though the *attractive quality* required the presence both of the external and internal air, in order to its shewing itself, yet the *light* requires the presence of only one of them in order to its appearance; for that either a glass globe full of air rubbed *in vacuo*, or with its air exhausted and rubbed *in pleno*, would produce a very considerable light.‡

HE says also, that those lights are less sensibly affected by the return of air, which are produced by the attrition of exhausted glass in pleno, than those which are produced by the attrition of glass full of air in vacuo, for that, in the former case, no great

* *Physico-Mechanical Experiments*, p. 82. † *Ib.* 242. ‡ *Ib.* 243.

alteration was found in the light or colour, until a certain quantity of air was let into the inside of the exhausted glafs; but that, in the latter case, both light and colour were sensibly changed, upon every admission of air to the outside of the full glafs.*

THE greatest electric light Mr. Hawkesbee produced, was when he enclosed one exhausted cylinder within another not exhausted, and excited the outermost of them, putting them both in motion. Whether their motions conspired or not, he observed, made no difference. When the outer cylinder only was in motion, he says, the light was very considerable, and spread itself over the surface of the inner glafs. What surpris'd him most was, that after both glasses had been in motion some time, during which the hand had been applied to the surface of the outer glafs, the motion of both ceasing, and no light at all appearing; if he did but bring his hand again near the surface of the outer glafs, there would be flashes of light, like lightning, produced in the inner glafs; as if, he says, the effluvia from the outer glafs had been pushed with more force upon it by means of the approaching hand.† This experiment was similar to those which he made with the excited and exhausted globe, and with the exhausted tube; and his reasoning upon it shews, that he was still far from being fully apprized of all the circumstances attending this fact.

THE next experiments which I shall relate of Mr. Hawkesbee's, are those which shew the great copiousness, and extreme subtilty of electric light. They are really amazing, and have not yet been pursued in the manner they deserve.

* *Physico-Mechanical Experiments*, p. 248. † *Ib.* 87.

HE lined more than half of the inside of a glass globe with sealing wax, and having exhausted the globe, he put it in motion; when, applying his hand to excite it, he saw the shape and figure of all the parts of his hand distinctly and perfectly, on the concave superficies of the wax within. It was as if there had only been pure glass, and no wax interposed between his eye and his hand. The lining of wax, where it was spread the thinnest, would but just allow the sight of a candle through it in the dark; but in some places the wax was, at least, one eighth of an inch thick; yet even in those places, the light and the figure of his hand were as distinguishable through it, as any where else. Nay though, in some places, the sealing wax did not adhere so closely to the glass as in others, yet the light on these appeared just as on the rest.*

THESE experiments succeeded equally with pitch instead of sealing wax. And he observed, that when the air was let into the glass, every part of it, the lined part and the unlined, seemed to attract with equal vigour.† Melted flowers of sulphur had no such effect, but common sulphur answered as well as sealing wax, or pitch. In both these last experiments the sulphur was found to have been separated from the glass.‡

USING a large quantity of common sulphur in the same manner, the light in the inside was four times as great, but the figure of his fingers was not so distinguishable as in the former cases. He likewise observed, that on the part near the axis, where the substance of the sulphur was the greatest, no light was produced; which he attributed, chiefly, to the slowness of the motion in that place.§

* Ib. 168. † Ib. 269. ‡ Ib. 274. § Ib. 275.

UPON the admission of a small quantity of air into the globe, thus partially lined with sealing wax, the light wholly disappeared on the part covered with the wax, but not on the other.

HE also observed, that when all the air was let in, and the hoop of threads before-mentioned held over the glass, the threads were attracted at greater distances by the-part which was coated with the wax than by the other: when all the air was exhausted, he says, the wax would attract bodies placed near the out-side of the glass; that even in this case, the threads preserved their central direction, though not so vigorously as when the air was let in; but that they would not be attracted at all, when there was no wax on the inside of the exhausted globe.

MR. HAWKESBEE was not unattentive to the *sound* made by the emission of the electric effluvia, or to the manner in which it effected the sense of feeling. He observed, that when an excited tube of glass attracted various bodies, and threw light upon them, as they were held near it, a noise, which he calls a *snapping*, was likewise heard. He also says, that the rubbed tube, held near the face, gave a feeling, as if fine hairs had been drawn over it; and when he repeated the experiment of whirling and rubbing the glass globe, he observed the light to proceed from it with some noise, and to make a kind of pressure upon the finger, when it was held within half an inch of it.*

NOR was Mr. Hawkesbee's attention confined to the electric power of *glass*. He made experiments with a globe of *sealing wax*, in the center of which was a globe of wood; from which he concluded, that the electricity of sealing wax was, in general the same with that of glass, but different from it in degree. He could not make any light adhere to his finger when presented to

* *Physico-Mechanical Experiments*, p. 65.

the excited sealing wax, any more than when it was presented to an exhausted and excited globe of glass.

He provided himself, in like manner, with a globe of sulphur, and another of rosin with a mixture of brick dust, but the sulphur could hardly be excited at all; whereas the rosin acted more powerfully than the sealing wax had done. This he ascribed to its being used while it was warm: for, in the same warm state, it attracted leaf brads without any attrition at all.*

He says, that the excited rosin gave no light in the dark, and the sulphur but little.†

WITH respect to the power of electricity in general, he observed, that a slight friction was sufficient to excite it, and that a greater pressure, or a more violent motion did not considerably increase it. ‡ He says, that all the phenomena of electricity were improved by warmth; and diminished by moisture; which he attributed to the resistance that the aqueous particles gave to the effluvia; and like Mr. Boyle, and others before him, he was confirmed in this hypothesis, by finding, that the interposition of linen cloth prevented any effects from being observed beyond it.

He also observed, that when the tube was filled with other matter than air, as with dry writing sand (which he actually tried) the attractive power of the effluvia was considerably abated, but he did not know what kind of bodies would produce that effect. He himself observes, that he found the electric virtue of a solid cylinder of glass to be, not indeed quite so strong, as that of a hollow tube, but more permanent. ||

THAT Mr. Hawkesbee, after all, had no clear idea of the distinction of bodies into electric and non-electrics, appears

* Ib. 154. † Ib. 156. ‡ Ib. 52. || Ib. 64.

from some of his last experiments, in which he attempted to produce electric appearances from metals, and from the reasons he gives for his want of success in those attempts. "From these experiments," he says, "I may safely conclude, that if there be any such quality as light to be excited from a brass body, under the forementioned circumstances," viz. of whirling and rubbing, "all the attrition of the several bodies I have used for that purpose, have been too weak to force it from it. And indeed, considering the closeness of the parts of metals, and with what firmness they adhere, entangle, and attract one another, a small degree of attrition is not sufficient to put their parts into such a motion as to produce an electrical quality, which quality, under the forementioned circumstances, I take to be the appearance of light in such a medium.

CONSIDERING what great success Mr. Hawkesbee had with his globe of glass, and his machine to give motion to it, it is surprising that the use of it should be so long discontinued after his death. To this circumstance we may perhaps, in a great measure, ascribe the slow progress that was afterwards made in electrical discoveries. Mr. Hawkesbee's successors confined themselves to the use of tubes. I suppose because they were lighter, more portable, and more easily managed in the experiments which they chiefly attended to: but the use of the globe would certainly have put them much sooner in the way of making the capital discoveries, which were afterwards made in electricity.

PERIOD

PERIOD III.

THE EXPERIMENTS AND DISCOVERIES OF MR. STEPHEN GREY, WHICH WERE MADE PRIOR TO THOSE OF MONSIEUR DU FAYE, AND WHICH BRING THE HISTORY OF ELECTRICITY TO THE YEAR 1733.

NOTWITHSTANDING the important discoveries of Mr. Hawkesbee, and the promising appearance they made, as an opening to further discoveries, we find, after him, a great chasm in the history of electricity, an interruption of discoveries, and, as far as we can learn, of experiments too, for the space of near twenty years; and at a time when philosophical knowledge of every other kind was making the most rapid progress, under the auspices of the great Sir Isaac Newton. But the attention of this great man happened to be engaged by other subjects, and this very circumstance might be the reason why the attention of other philosophers was also diverted from electricity.

AFTER this long interval, commences a new æra in the history of electricity; in which we shall have the works of another labourer in this new field of philosophy to contemplate, viz. Mr. Stephen Grey, a pensioner at the Charter House. No person who ever applied to this study was more assiduous in making experiments, or had his heart more entirely in the work. This will appear by the prodigious number of experiments he made,

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and some considerable discoveries with which his perseverance was crowned; as well as by the self deceptions, which his passionate fondness for new discoveries exposed him to.

BEFORE the year 1728, Mr. Stephen Grey had often observed, in electrical experiments made with a glass tube, and a down feather tied to the end of a small stick, that, after its fibres had been drawn toward the tube, they would, upon the tube's being withdrawn, cling to the stick, as if it had been an electric body, or as if there had been some electricity communicated to the stick, or to the feather. This put him upon thinking, whether, if the feather were drawn through his fingers, it might not produce the same effect, by acquiring some degree of electricity. This experiment succeeded accordingly, upon his first trial; the small downy fibres of the feather being attracted by his finger, when held near it; and sometimes the upper part of the feather with its stems would be attracted also.

IT will be obvious to every electrician, that the success of this experiment depended upon other principles, than those to which he had a view in making it. Proceeding, however, in the same manner, he found the following substances to be all electric; *hair, silk, linen, woollen, paper, leather, wood, parchment,* and *ox gut* in which leaf gold had been beaten. He made all these substances very warm, and some of them quite hot before he rubbed them. He found light emitted in the dark by the silk and the linen, but more especially by a piece of white pressing paper, which is of the same nature with card paper. Not only did this substance, when made as hot as his fingers could bear, yield a light; but, when his fingers were held near it, a light issued from them also attended with a crackling noise,
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like that produced by a glass tube, though not at so great a distance from the fingers. *

THE preceding experiments bring us to the eve of a very considerable discovery in electricity, viz. the communication of that power from native electrics, to bodies, in which it is not capable of being excited; and also to a more accurate distinction of electrics from non-electrics. I shall relate the manner in which these important discoveries were made pretty fully, but, at the same time, as succinctly as possible.

IN the month of February 1729, Mr. Grey, after some fruitless attempts to make metals attractive, by heating, rubbing, and hammering, recollected a suspicion which he had some years entertained; that, as a tube communicated its light to various bodies when it was rubbed in the dark, it might possibly, at the same time, communicate an electricity to them, by which had hitherto been understood only the power of attracting light bodies. For this purpose he provided himself with a tube three feet five inches long, and near one inch and two tenths in diameter; and to each end was fitted a cork, to keep the dust out when the tube was not in use.

THE first experiments he made upon this occasion were intended to try, if he could find any difference in its attraction when the tube was stopped at both ends by the corks, and when left entirely open; but he could perceive no sensible difference. It was, however, in the course of this experiment that, holding a down feather over against the upper end of the tube, he found that it would fly to the cork, being attracted and repelled by it, as well as by the tube itself. He then held the feather over

* Philosophical Transactions abridged, Vol. 8, p. 9.

against the flat end of the cork, and observed, that it was attracted and repelled many times together; at which, he says, he was much surpris'd, and concluded, that there was certainly an attractive virtue communicated to the cork by the excited tube.

He then fixed an ivory ball upon a stick of fir, about four inches long; when, thrusting the other end into the cork, he found, that the ball attracted and repelled the feather, even with more vigour than the cork had done, repeating its attractions and repulsions many times successively. He afterwards fixed the ball upon long sticks, and upon pieces of brass and iron wire, with the same success; but he observed, that the feather was never so strongly attracted by the wire, though it were held very near the tube, as by the ball at the end of it.

WHEN a wire of any considerable length was used, its vibrations, caused by the action of rubbing the tube, made it troublesome to manage. This put Mr. Grey upon thinking whether, if the ball were hung to a packthread, and suspended by a loop on the tube, the electricity would not be carried down the line to the ball; and he found it to succeed according to his expectation. In this manner he suspended various bodies to his tube, and found all of them to be capable of receiving electricity in the same manner.

AFTER trying these experiments with the longest light canes and reeds that he could conveniently use, he ascended a balcony twenty six feet high; and, fastening a string to his tube, he found, that the ball at the end of it would attract light bodies in the court below.

He then ascended to greater heights, and by putting his long canes in the end of his tube, and fastening a long string to the
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end of the canes, he contrived to convey the electricity to much greater distances than he had done before; till, being able to carry it no further perpendicularly, he next attempted to carry it horizontally; and from these attempts arose a discovery, of which he was not in the least aware when he began them.

IN his first trial he made a loop at each end of a packthread, by means of which he suspended it, at one end, on a nail driven into a beam, the other end hanging downwards. Through the loop which hung down, he put the line to which his ivory ball was fastened, fixing the other end of it by a loop on his tube; so that one part of the line, along which the electricity was to be conveyed, viz. that to which the ball was fastened, hung perpendicular, the rest of the line lay horizontal. After this preparation, he put the leaf brass under the ivory ball, and rubbed the tube, but not the least sign of attraction was perceived. Upon this he concluded, that when the electric virtue came to the loop of the packthread, which was suspended on the beam, it went up the same to the beam; so that none, or very little, of it went down to the ball; and he could not, at that time, think of any method to prevent it.

ON June the 30th 1729, Mr. Grey paid a visit to Mr. Wheeler, to give him a specimen of his experiments; when, after having made them from the greatest heights which the house would admit, Mr. Wheeler was desirous of trying whether they could not carry the electric virtue to a greater distance horizontally. Mr. Grey then told him of the fruitless attempt he had made to convey it in that direction: upon which Mr. Wheeler proposed to suspend the line to be electrified by another of silk, instead of packthread; and Mr. Grey told him, it might do better, on account of its smallness; as less of the virtue would probably
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pass off by it than had done by the thick hempen line, which he had used before. With this expedient, they succeeded far beyond their expectations.

THE first experiment they made after this expedient occurred to them, was in a matted gallery at Mr. Wheeler's house July 2d. 1729, about ten o'clock in the morning, as Mr. Grey, after his useful manner, has minutely recorded it. About four feet from the end of the gallery, they fastened a line across the place. The middle part of this line was silk, the rest pack-thread. They then laid the line to which the ivory ball was hung, by which the electric virtue was to be conveyed to it from the tube, and which was eighty feet and a half in length, across this silk line, so that the ball hung about nine feet below it. The other end of the line was, by a loop, fastened to the tube, which they excited at the other end of the room. After this preparation, they put the leaf brass under the ivory ball, and, upon rubbing the tube, it was attracted, and kept suspended for some time.

THE gallery not permitting them to go any greater lengths with a single line of communication, they contrived to return the line, making the whole length of it almost twice that of the gallery, or about one hundred and forty seven feet, which answered very well. But, suspecting that the attraction would be stronger without doubling or returning the line, they made use of a line one hundred and twenty four feet long, running in one direction in the barn; and, as they expected, they found the attraction stronger than when the line had been returned in the gallery.

JULY the 3d. proceeding to make more returns of the line, the silk which supported it happened to break, not being able
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to bear the weight of it, when shaken with the motion that was given to it by rubbing the tube. Upon this they endeavoured to support it by a small iron wire, instead of the silk string; but this also breaking, they made use of a brass wire a little thicker. But this brass wire, though it supported the line of communication very well, did not answer the purpose of these young electricians: for, upon rubbing the tube, no electricity was perceived at the end of the line. It had all gone off by the brass wire which supported it. They had recourse to brass wires, as being stronger than their silk lines, and no thicker; for the same reason that they had before used silk lines in preference to hempen strings; because they could have them stronger, and at the same time smaller. But the result of this experiment convinced them, that the success of it depended upon their supporting lines being *silk*, and not, as they had imagined, upon their being *small*. For the electric virtue went off as effectually by the small brass wire, as it had done by the thick hempen cord.

BEING obliged, therefore, to return to their silk lines, they contrived them to support very great lengths of the hempen line of communication; and actually conveyed the electric virtue seven hundred and sixty five feet, nor did they perceive that the effect was sensibly diminished by the distance.*

IN the same manner in which *silk* was found to be a non-conductor, it is probable that, about the same time, *hair, rosin, glass,* and perhaps some other electric substances were found to have the same property, though the discovery be no where particularly mentioned: for we shall presently find Mr. Grey making use of them, to insulate the bodies that he electrified.

* Phil. Trans. abridged, Vol. 7. p. 15.

AFTER

AFTER this, they amused themselves with trying how large surfaces might be impregnated with the electric effluvia; electrifying a large map, table cloth, &c. They also carried the electric virtue several ways at the same time, and to a considerable distance each way.

THE magnetic effluvia, they found did not in the least interfere with the electric; for when they had electrified the load stone, with a key hanging to it, they both attracted leaf brads like other substances.

SOME time after this, Mr. Wheeler, in the absence of Mr. Grey, electrified a red hot poker, and found the attraction to be the same as when it was cold. He also suspended a live chicken upon the tube by the legs, and found the breast of it strongly electrical.*

IN August 1729, Mr. Grey advanced one step further in his electrical operations. He found that he could convey the electric virtue from the tube to the line of communication without touching it, and that holding the excited tube near it was sufficient. Repeating his former experiments with this variation, in conjunction with Mr. Wheeler, and among others, carrying the electric virtue several ways at the same time without touching the line, they always observed, that the attraction was strongest at the place which was most remote from the tube; a fact which they might have observed, if they had attended to it, in their former experiments. †

SOME time in the same month, Mr. Wheeler and Mr. Grey in conjunction made some experiments, in order to try whether the electric attraction was in proportion to the quantity of matter

* Phil. Transf. abridged, Vol. 7. p. 16.

† Ib. p. 17.

in bodies; and with this view they electrified a solid cube of oak, and another of the same dimensions which was hollow; but they could not perceive any difference in their attractive power; though it was Mr. Grey's opinion, that the electric effluvia passed through all the parts of the solid cube.*

On the 13th. of August in the same year, Mr. Grey made another improvement in his electrical apparatus, by finding that he could electrify a *rod*, as well as a *thread*, without inserting any part of it into his excited tube. He took a large pole twenty seven feet long, two inches and one half diameter at one end, and one inch and one half at the other. It was a sort of wood which is called horse beach, and had its rind on. This pole he suspended horizontally by hair lines, and at the small end of the pole he hung a cork, by means of a packthread about one foot long, and put a small leaden ball upon the cork, to keep the packthread extended. Then the leaf brass being put under the cork, the tube rubbed, and held near the larger end of the pole, the cork ball at the opposite end attracted the leaf brass strongly, to the height of an inch or more. Mr. Grey also observed, that though the leaf brass was attracted by any part of the pole, it was not near so strongly as by the cork. †

ABOUT the beginning of September, Mr. Grey made experiments, to shew that the electric effluvia might be carried in a circle, as well as along lines, and be communicated from one circle to another; and also that it might be done whether the circles were vertical, or horizontal.

ABOUT the latter end of autumn, or the beginning of winter 1729, Mr. Grey resumed his inquiries after other electrical bo-

* Phil. Transf. abridged, Vol. 7. p. 17. † Ib. p. 18.

dies, and found many more to have the same property, but he mentions only the dry leaves of several trees; from whence he concluded, that the leaves of all vegetables had that attractive virtue. *

WE are now advanced to a new scene of Mr. Grey's electrical experiments, viz. upon *fluids*, and upon *animal bodies*. Having no other method of trying whether any substances could have the electric virtue communicated to them, but by making them raise light bodies placed upon a stand under them; it may easily be imagined, that they could not well contrive to put a fluid body into that situation. The only thing that Mr. Grey could do in this way, was to make use of a bubble, in which form a fluid is capable of being held in a state of suspension. Accordingly on March 23d. and 25th. 1730, he dissolved soap in Thames water, and suspending a tobacco pipe, he blew a bubble at the head of it; and, bringing the excited tube near the small end, he found the bubble to attract leaf brads to the height of two, and of four inches. †

APRIL the 8th. 1730, Mr. Grey suspended a boy on hair lines in a horizontal position, just as all electricians had, before, been used to suspend their hempen lines of communication, and their wooden rods; then, bringing the excited tube near his feet, he found that the leaf brads was attracted by his head with much vigour, so as to rise to the height of eight, and sometimes of ten inches. When the leaf brads was put under his feet, and the tube brought near his head, the attraction was small; and when the leaf brads was brought under his head, and the tube held over it, there was no attraction at all. Mr. Grey does not attempt

* Phil. Trans. abridged, Vol. 7. p. 19.

† Ib.

to assign any reason for these appearances. It was not till many years after this time, that the influence of points in receiving and emitting the electric effluvia was observed. While the boy was suspended, Mr. Grey, amused himself with making the electricity operate in several parts of his body, at the same time; and at the ends of long rods, which he made him hold in his hands, and in diversifying the experiment several other ways.

It is curious to observe the inference which Mr. Grey makes from these experiments. By them, says he, we see, that animals receive a greater quantity of electric fluid than other bodies; and that it may be conveyed from them several ways at the same time, to considerable distances. He had no idea that the bodies of animals receive electricity only by means of the moisture that is in them, and that this hampen line of communication, and his wooden rods could not have been electrified at all, if they had been perfectly dry.

In all these experiments Mr. Grey observed, that the leaf brass was attracted to a much greater height from the top of a narrow stand than from the table; and, at least, three times higher than when it was laid on the floor of the room.

ABOUT this time Mr. Grey communicated to the Royal Society his suspicion, that bodies attracted more or less according to their colour, though the substance was the same, and the weight and size equal. He says, he found red, orange, and yellow attracted at least three or four times stronger than green, blue, or purple; but he forbore communicating a more particular account of them, till he had tried a more accurate method which, he says he had thought of, to make the experiments. The communica-

* Phil. Trans. abridged, Vol. 7. p. 20.

tion, however, was never made. The thing itself was only a deception, as will be shewn in some subsequent experiments made by Monsieur Du Faye. *

MR. GREY, having found that he could communicate electricity to a bubble of soap and water, was encouraged to attempt communicating it to water itself. In order to this, he electrified a wooden dish full of water, placed on a cake of rosin, or a pane of glass, and observed, that if a small piece of thread, a narrow slip of thin paper, or a piece of sheet glass was held over the water, in an horizontal position, at the distance of an inch or something more, they would be attracted to the surface of the water and then repelled; but he imagined that these attractions and repulsions were not repeated so often as they would have been, if the body had been solid.

BUT he afterwards contrived to shew the effect of electricity upon water in another and more effectual manner. As this experiment was very curious and exhibited an appearance which was quite new to the electricians of those times, I shall relate the particulars of it very fully, and generally in Mr. Grey's own words. †

He filled a small cup with water higher than the brim, and when he had held an excited tube over it, at the distance of about an inch or more, he says, that if it were a large tube, there would first arise a little mountain of water from the top of it, of a conical form; from the vertex of which there proceeded a light, very visible when the experiment was performed in a dark room, and a snapping noise, almost like that which was made when the finger was held near the tube, but not quite so loud, and of a more

* Phil. Transf. abridged, Vol. 7. p. 22.

† Ib. p. 23.

flat sound. Upon this, says he, immediately the mountain, if I may so call it, falls into the rest of the water, and puts it into a tremulous and waving motion.

WHEN he repeated this experiment in the sun shine, he perceived that very small particles of water were thrown from the top of the mountain; and that, sometimes, there would arise a fine stream of water from the vertex of the conc, in the manner of a fountain, from which there issued a fine stream or vapour, whose particles were so small as not to be seen; yet, he says, he was certain it must be so; since the under side of the tube was wet, as he found when he came to rub it afterwards. He adds, that he had since found, that though there does not always arise that cylinder of water, yet that there is always a stream of invisible particles thrown on the tube, and sometimes to that degree as to be visible on it.

WHEN some of the larger cups were used (his sizes were from three fourths to one tenth of an inch in diameter) which, he says were to be filled as high as could be done without running over; the middle part of the surface, which was flat, would be depressed, upon the approach of the tube, into a concave, and the parts towards the edge be raised; and that when the tube was held over against the side of the water, little conical protuberances of water issued out from it horizontally; and, after the crackling noise, returned to the rest of the water; and that sometimes small particles would be thrown off from it, as from the small portions of it above mentioned.

THIS last experiment he repeated with hot water, and found that it was attracted much more strongly, and at a much greater distance than before. The steam arising from the vertex was in this case visible, and the tube was sprinkled with large drops of water.

He

HE tried these experiments, in the same manner, with quick silver, which was likewise raised up, but, by reason of its gravity, not to so great a height as the water: but, he says, that the snapping noise was louder, and lasted much longer than it did with the water.*

IT is not easy to know what to make of the next set of experiments which engaged the attention of Mr. Grey, or how far he deceived himself in the results of them. He fancied that he had discovered a perpetual attractive power in all electric bodies which did not require heating, rubbing, or any kind of attrition to be excited. The following experiments, he imagined proved the discovery.

HE took nineteen different substances, which were either rosin, gum lac, shell lac, bees wax, sulphur, pitch, or two or three of these differently compounded. These he melted in a spherical iron ladle; except the sulphur, which was best done in a glass vessel. When these were taken out of the ladle, and their spherical surfaces hardened, he says, they would not attract until the heat was abated, or until they came to a certain degree of warmth; that then there was a small attraction, which increased until the substance was cold, when it was very considerable. †

THE manner in which he preserved these substances in a state of attraction, was by wrapping them in any thing which would keep them from the external air. At first, for the smaller bodies, he used white paper, and for the larger ones, white flannel; but he afterwards found that black worsted stockings would do as well. Being thus clothed, he put them into a large firm box; where they remained, till he had occasion to make use of them.

* Phil. Trans. abridged, Vol. 7. p. 24.

† Ib. Vol. 7. p. 24.

HE observed these bodies for thirty days, and found that they continued to act as vigorously as on the first or second day; and that they retained their power until the time of his writing, when some of them had been prepared above four months.

HE makes the most particular mention of a large cone of stone sulphur, covered with a drinking glass in which it was made; and says, that whenever the glass was taken off, it would attract as strongly as the sulphur, which was kept covered in the box. In fair weather, the glass would attract also, but not so strongly as the sulphur, which never failed to attract, let the wind or the weather be ever so variable; as would all the other bodies, only in wet weather, the attraction was not so great, as in fair weather.

HE also mentions a cake of melted sulphur, which he kept without any cover, in the same place with the body above-mentioned, and where the sun did not shine upon them; and says, that it continued to attract till the time of his writing; but that its attraction was not one tenth part of that of the cone of sulphur which was covered.

THESE attractions he tried by a fine thread hanging from the end of a stick. He held the electric body in one hand, and the stick in the other; and could perceive the attraction at as great a distance as he could hold them.

AT the time of his writing, he was upon the subject of permanent electricity in glass, but had not then completed his experiments.*

GREAT light will be thrown upon these experiments of Mr. Grey by some that will be hereafter related of Mr. Wilcke.

* Phil. Trans. abridged, Vol. 6. p. 27.

It is probable that the glass vessel in these experiments was possessed of one electricity, and the sulphur, &c. of the other. But the two electricities were not discovered till afterwards.

WE are now come to a different set of electrical experiments, made by Mr. Grey and Mr. Wheeler in conjunction, similar to some of Mr. Hawkesbee's.

IN the first place, Mr. Grey made some experiments, which, probably unknown to him, had been made before by Mr. Boyle, on excited glass, and several other bodies in vacuo, and found that they would attract at very near the same distance as in pleno. To determine this, he suspended the excited substance in a receiver of an air pump, and when it was exhausted, he let the electric down to a proper distance from some light bodies, placed on a stand below. The event was, as near as could be judged, the same in vacuo as in the open air, if the experiment was made in the same receiver, and if the electric was brought to the light bodies at the same distance of time from the act of excitation.*

ABOUT the latter end of August 1732, Mr. Grey and Mr. Wheeler suspended, from the top of a receiver, a white thread, which hung down to the middle of it. Then exhausting the receiver, and rubbing it, the thread was attracted vigorously. When it was at rest, and hung perpendicularly, the excited tube attracted it; and when the tube was taken away slowly, the thread returned to its perpendicular situation; but the tube being removed hastily, the thread jumped to the opposite side of the receiver. This last effect followed, if the hand was hastily removed from the receiver; and at first it appeared, in both

* Phil. Trans, abridged, Vol. 6. p. 27.

cases, unaccountable to them; but upon further consideration, they concluded, that it proceeded from the motion of the air, made by the tube, or the hand, which took off the attraction on that side, and not on the other.* They also found, that an excited tube would attract the thread through another receiver, which was put over that in which it was suspended. And, sometime after, Mr. Wheeler found that the thread was attracted through five receivers put one over another, and all exhausted: He even thought that, in this case, the attraction was rather stronger than when a single receiver was used. N. B. The more effectually to keep any thing of moisture out of the receivers, which would have been of bad consequence in this experiment, instead of wet leather, he made use of a cement made of wax and turpentine, which Mr. Boyle used in his experiments. †

THESE two gentlemen, about the same time, made a curious experiment which, they say, shewed that attraction is communicated through opaque, as well as transparent bodies, not in vacuo. But a little knowledge of metal, as a conductor of electricity, would have saved them the trouble they gave themselves. They took a large hand-bell, and taking out the clapper, they suspended a cork, besmeared with honey, from the top of it; and set it on a piece of glass, on which they had put some leaf brass. The excited tube was then brought near several parts of the bell; and, upon taking it up, several pieces of leaf brass were found sticking to the cork, and others were removed from the places in which they had been left, having, probably, been attracted by the bell. ‡

* Phil. Trans. abridged, Vol. 7. p. 56.

† Ib. p. 97.

‡ Ib. 96.

WE see by how small steps advances were made in this science by some experiments made by Mr. Grey, 16th June 1731, and which he has thought worth recording; though they contain hardly any thing which we should think new, notwithstanding the discoveries appeared pretty considerable to him.

HE electrified a boy standing on cakes of rosin, as strongly as he had before electrified him when suspended on hair lines. He afterwards electrified a boy suspended on hair lines, by means of a line of communication from another boy who was electrified, at some feet distance from him. He varied this experiment with rods and boys several ways; and concluded from it, that the electric virtue might not only be carried from the tube, by a rod, or line, to distant bodies; but that the same rod, or line, would communicate that virtue to another rod, or line, at a distance from it; and that, by this other rod, or line, the attractive force might be carried to still more distant bodies. This experiment shews that Mr. Grey had not properly considered the line of communication, and the body electrified by it, as one and the same thing, in an electrical view, differing only in form, as they were both alike conductors of electricity.

IN December following, Mr. Grey carried this experiment something further, by conveying electricity to bodies which did not touch the line of communication, making it pass through the center of hoops standing on glass. One of his hoops was twenty, another forty inches in diameter.*

* Phil. Transf. abridged, Vol. 7. p. 100.

PERIOD IV.

THE EXPERIMENTS AND DISCOVERIES OF MR. DU FAYE.

HITHERTO the spirit of electricity seems to have been confined to England; but, about this time, we find that it had passed the seas, and that ingenious foreigners were ambitious of distinguishing themselves, and acquiring reputation in this new field of glory. Mr. Du Faye, intendant of the French king's gardens, and member of the academy of sciences at Paris, assiduously repeated the experiments above-mentioned of Mr. Grey, and likewise added to the common stock many new ones of his own. To him we are also indebted for the observation of several general properties of electricity, or rules concerning the method of its action, which had not been taken notice of before, and which reduced to fewer propositions what had been discovered concerning it. These experiments of his were comprised in eight long memoirs, inserted in the history of the academy of sciences for the years 1733, 1734 and 1737. An account of some of them also makes an article in the Philosophical Transactions, dated December 27th 1733. The first of his memoirs contain a history of electricity, brought down to the year 1732.*

He found that all bodies, except metallic, soft, and fluid ones, might be made electric, by first heating them, more or less, and then rubbing them on any sort of cloth. He also

* Geschichte, Vol. 1. p. 195.

excepts those substances which grow soft by heat, as gum; or which dissolve in water, as glue. He also remarked, that the hardest stone and marble required more chafing and heating than other bodies, and that the same rule obtains with regard to woods; so that box, lignum vitæ, and other kinds of very hard wood must be chafed almost to a degree of burning; whereas fir, lime tree, and cork require but a moderate heat.*

He found that not only damp air, but also great heat was prejudicial to electricity, and that his experiments often failed in the warmest hours of a moderately hot day. †

He says, that, pursuing Mr. Grey's experiments, to make water receive electricity, he found; that all bodies, without exception, whether solid or fluid, were capable of it, when they were placed on glass slightly warmed, or only dried, and the excited tube was brought near them. He particularly mentions his having made the experiments with ice, lighted wood coal, and every thing that happened to be at hand at the time; and constantly remarked, that such bodies as were of themselves the least electric, had the greatest degree of electricity communicated to them by the approach of the excited tube.

He refutes Mr. Grey's assertion concerning the different electricity of differently coloured bodies, and shews that it proceeded not from the colour as a colour, but from the substance which was employed in dying it.

In order to determine (as he imagined) whether some bodies did not contain more electric matter than others, he hung upon an iron bar, suspended on silken strings, threads of silk, wool, cotton, and linen; and observed, that when the excited tube

* Phil. Transf. abridged, Vol. 8. p. 393.

† Geschichte, Vol. 1. p. 211.

was brought near the bar, the linen threads separated to the greatest distance, and so were most affected; the cotton next, then the silk, and the wool, least of all, the threads hanging almost close together. From this he concluded, that, of all these substances linen contained the most electricity in it.*

HAVING communicated the electricity of the tube, by means of a packthread, after Mr. Grey's manner, he observed, that the experiment succeeded better for wetting the line; and though he made the experiment at the distance of one thousand two hundred and fifty six feet, when the wind was high, the line making eight returns, and passing through two different walks of a garden, that the electric virtue was still communicated.†

THE electric spark from a living body, which makes a principal part of the diversion of gentlemen and ladies, who come to see experiments in electricity, was first observed by Mr. du Faye, accompanied at that time, as in most of his experiments, by the Abbe Nollet, who, afterwards, we shall find, did himself obtain a distinguished name among electricians.

MR. DU FAYE, having got himself suspended on silk lines, as Mr. Grey had done the child mentioned above, observed, that, as soon as he was electrified, if another person approached him, and brought his hand within an inch, or thereabouts, of his face, legs, hands, or cloaths, there immediately issued from his body one or more pricking shoots, attended with a crackling noise. He says this experiment occasioned to the person who brought his hand near him, as well as to himself, a little pain, resembling that of the sudden prick of a pin, or the burning from a spark of fire; and that it was felt as sensibly through his cloaths,

* Geschichte, Vol. 1. p. 231.

† Phil. Trans. abridged, Vol. 8. p. 395.

as on his bare face, or hands. He also observes, that, in the dark, those snappings were so many sparks of fire. *

THE Abbe Nollet says he shall never forget the surprize, which the first electrical spark which was ever drawn from the human body excited, both in Mr. Du Faye, and in himself. †

HE says, that those snappings and sparks were not excited, if a bit of wood, of cloth, or of any other substance than a living human body, was brought near him; except metal, which produced very nearly the same effect as the human body. He was not aware, that it was owing to points, or partial dryness, in the substances which he mentions, that they did not take a full and strong spark. He seems also to have been under some deception, when he imagined, that the flesh of dead animals gave only an uniform light, without any snapping, or sparks. ‡

FROM this circumstance, however, he, at that time, concluded, that the bodies of living animals, (and also metals) were surrounded with an atmosphere of vapour, which was actually set on fire by electric light. ||

HE observed, that a cat emitted an electric spark, which evidently gave her pain, when the finger was brought to any part of her body, after she had been stroked, while she was sitting on a silk cushion. This must have appeared very extraordinary, before it was known, that the electric matter passed from the hand to the cat, in the act of rubbing. §

WITH the electric spark, he imagined he could have fired inflammable substances; and he made several attempts with tinder and gunpowder, but without success: He found no appearance

* Phil. Trans. abridged, Vol. 8. p. 395.

† Leçons de Physique, Vol. 6. p. 408.

‡ Phil. Trans. abridged, Vol. 8. p. 395.

§ Geschichte, Vol. 1. p. 215. 230.

¶ Ib. p. 216.

of real fire. This was a capital discovery reserved for the Germans. *

THE two next capital observations of Mr. Du Faye I shall repeat in his own words, because they are important and curious; and yet the former of them is little more than what Otto Guericke had observed before him. "I discovered," he says, "a very simple principle, which accounts for a great part of the irregularities, and, if I may use the term, of the caprices, that seem to accompany most of the experiments in electricity. This principle is, that electric bodies attract all those which are not so, and repel them as soon as they are become electric, by the vicinity or contact of the electric body. Thus leaf gold is first attracted by the tube; acquires electricity by approaching it, and, consequently, is immediately repelled by it; nor is re-attracted, while it retains its electric quality. But if, while it is thus sustained in the air, it chance to light on some other body, it straightway loses its electricity, and consequently is re-attracted by the tube; which, after having given it a new electricity, repels it a second time; and this repulsion continues as long as the tube keeps its power. Upon applying this principle to various experiments of electricity, one will be surpris'd at the number of obscure and puzzling facts which it clears up." By the help of this principle, he, particularly, endeavours to explain several of Mr. Hawkesbee's experiments. †

"CHANCE, he says, has thrown in my way another principle more universal and remarkable than the preceding one; and which casts a new light upon the subject of electricity. The

* Geschichte, Vol. 1. p. 229.

† Phil. Trans. abridged, Vol. 8. p. 396.

principle

“ principle is, that there are two distinct kinds of electricity,
 “ very different from one another; one of which I call *vitreous*,
 “ the other *resinous* electricity. The first is that of glass, rock-
 “ crystal, precious stones, hair of animals, wool, and many
 “ other bodies. The second is that of amber, copal, gum lac,
 “ silk, thread, paper, and a vast number of other substances.
 “ The characteristics of these two electricities is, that they repel
 “ themselves, and attract each other. Thus a body of the vitre-
 “ ous electricity repels all other bodies possessed of the vitreous,
 “ and on the contrary, attracts all those of the resinous electrici-
 “ ty. The resinous, also, repels the resinous, and attracts the
 “ vitreous. From this principle, one may easily deduce the ex-
 “ planation of a great number of other phenomena; and it is
 “ probable, that this truth will lead us to the discovery of many
 “ other things.”

In order to know, immediately, to which of the two classes of electricity any body belonged, he made a silk thread electrical, and brought it to the body, when it was excited. If it repelled the thread, he concluded it was of the same electricity with it, viz. resinous; if it attracted it, he concluded it was vitreous.*

He also observed, that communicated electricity had the same property as the excited. For having electrified, by the glass tubes, balls of wood or ivory; he found them to repel the bodies which the tube repelled, and to attract those which the tube attracted. If they had the resinous electricity communicated to them, they observed the same rule, by attracting those bodies which had the vitreous electricity communicated to them, and

* Phil. Trans. abridged, Vol. 8. p. 397.

repelling

repelling those which had received the resinous. But, he observes, the experiment would not succeed, if the bodies were not made equally electrical; for, if one of them was weakly electrical, it would be attracted by that which was much more strongly electrical, of whatever quality it was.

THIS discovery of the two electricities was certainly a capital one, but was, notwithstanding, left very imperfect by Mr. Du Faye. We shall see that Dr. Franklin found, that, in all probability, the vitreous electricity was positive, or a redundancy of electric matter; and the resinous, negative, or a want of it; and that Mr. Canton has discovered, that it depends upon the surface of the electric bodies, and of the rubber, whether the electricity be positive or negative.

THE doctrine of two different electricities, produced by exciting different substances, considerable as the discovery of it was, seems to have been dropped after Mr. Du Faye, and those effects ascribed to other causes; which is an instance that science sometimes goes backwards.

MR. DU FAYE himself seems, at last, to have adopted the opinion, which generally prevailed to the time of Dr. Franklin; that the two electricities differed only in degree, and that the stronger attracted the weaker: not considering that, upon this principle, bodies possessed of the two electricities ought to attract one another less forcibly, than if one of them had not been electrified at all, which is contrary to fact.

IT will be seen that, many years after, Mr. Kinnerley of Philadelphia, a friend of Dr. Franklin's, being at Boston in New England, made some experiments which again shewed that dif-

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ference of the two electricities. He communicated those experiments to Dr. Franklin, who repeated and explained them. *

IT must be added to the experiments of Mr. Du Faye, that he communicated electricity from one body to another, through the interval of ten or twelve inches, in the middle of which there was a lighted candle. † He also found, that red hot iron might be electrified very well. ‡

MR. DU FAYE was the first person who endeavoured to excite a tube in which air was condensed, and found the attempt ineffectual. Suspecting this might be owing to moisture, which he might force into the tube, in using his condensing instrument, he cemented a large copper colipile to his tube, and compressed the air in it, by putting the colipile upon the fire. After this, he turned a cock, which he had placed to prevent the return of the compressed air, and disengaged the tube from the colipile; but he still found the excitation to be impossible. The Abbe Nollet, who assisted at most of this gentleman's experiments, declares himself not satisfied even with this precaution; thinking that the non-excitation of the tube might still be owing to the moisture, which always exists in the air, and the particles of which must be drawn nearer together by condensation. || In answer to this objection, Mr. Boulanger says, that a small glass full of water poured into a tube, and immediately thrown out again, will not destroy the excitability of the glass near so much as the condensed air. §

MR. DU FAYE was the first who observed that electric substances attract the dew more than conductors. He observed

* See his Letters.

† Nollet's Recherches, p. 203.

‡ Ib. p. 212.

|| Ib. p. 258.

§ Boulanger, p. 132.

that

that a glass vessel, placed on a metal cup, and set in the open air all day, will often be wet when the metal is dry. S. Beccaria accounts for this fact, by supposing that alterations in the electricity of the air easily produce correspondent alterations in the electricity of metals, in which the electric fluid moves with the utmost ease, but not in glass. Whenever, therefore, the state of the electric fluid in the air is altered, the glass is electrified *plus* or *minus*, and therefore attracts the vapours in the air.*

It must be observed, that Mr. Granville Wheeler, in the autumn of the year 1732, made several curious experiments, relating to the repulsive force of electricity. These he repeated to Mr. Grey in the summer following, and designed to communicate them, through his hands, to the Royal Society; but, deferring the execution of it from time to time, he was informed, that Mr. Du Faye had taken notice of the same solution of the repulsive force. Upon this he laid aside all thoughts of communicating his discovery to the public: but, finding that his experiments were different from those of Mr. Du Faye, he was persuaded to publish them in the Philosophical Transactions for the year 1739.

THE experiments were made by threads of various kinds, and other substances, hanging down from silk lines, and generally made to repel one another by the approach of an excited tube. The result of them all he comprised in the three following propositions. 1st. That bodies made electrical, by communication with an excited electric, are in a state of repulsion with respect to such excited bodies. 2dly. That two, or more bodies, made electrical by communicating with an excited electric, are in a

* Beccaria dell' elettricismo naturale et artificiale, p. 179.

state of repulsion with respect to one another. 3dly. Excited electrics do themselves repel one another. * *וְהַחֲבִירִים יִרְדָּוּ אֶחָד אֶת־אֶחָד*

ONE of his experiments, to prove the second of these propositions, deserves to be mentioned for its curiosity. He tied a number of silk threads together, by a knot at each extremity; when, upon electrifying them, the threads repelled one another, and the whole bundle was swelled out into a beautiful spherical figure; so that he could with pleasure, he says, observe the knot at the bottom rising upwards, as the electricity and mutual repulsion of the threads increased; and he could not help imagining his bundle of silks to resemble a bundle of muscular fibres.

By way of corollary to the same proposition, he observes, that it suggests, more plainly than any other known experiment, a reason for the dissolution of bodies in menstrua; viz. that the particles of the solvend, having imbibed particles of the menstrua, so as to be saturated with them, the saturated particles become repulsive of one another, separate, and fly to pieces. † *וְהַחֲבִירִים יִרְדָּוּ אֶחָד אֶת־אֶחָד*

* Phil. Trans. abridged, Vol. 8. p. 411. † Ib. p. 410.

PERIOD V.

THE CONTINUATION, AND CONCLUSION OF MR. GREY'S
EXPERIMENTS.

MR. GREY, upon resuming his experiments, expresses great satisfaction, that his former observations had been confirmed by so judicious a philosopher as Mr. Du Faye; who, he acknowledges, had made several new ones of his own, particularly that important luciferous one, as he calls it; which, put him upon making the experiments which follow, and which were made in the months of July and August 1734.

As Mr. Du Faye had said, that the snappings and the sparks, he had mentioned, were strongly excited by a piece of metal, presented to the person suspended on silk lines; Mr. Grey concluded, that if the person and the metal should change places, the effect would be the same. He, accordingly, suspended several pieces of metal on silk lines, beginning with the common utensils, which were at hand, as the iron poker, tongs, fire shovel, &c. and found, that, when they were electrified; they gave sparks, in the same manner as the human body had done in like circumstances. This was the origin of *metallic conductors*, which are in use to this day. †

* Phil. Transf. abridged, Vol. 8. p. 397.

† Ib. p. 398.

MR.

MR. GREY did not, at that time, think of making his experiments in the dark, in order to see the light proceeding from the iron; not imagining, that electricity communicated to metals, would have produced such surprising phenomena, as, he says, he afterwards found it to do.

CONTINUING his experiments at Mr. Wheeler's, they found, that the flesh of dead animals exhibited, very nearly, the same appearances as that of living animals, contrary to the assertion of Mr. Du Faye.

BUT what most surprised Mr. Grey, and the gentlemen then present, in the experiments he made upon that occasion, was the phenomenon above referred to, and what he now calls *a cone*, or *pencil of electric light*; such as is commonly seen to issue from an electrified point. As this was the first time that this phenomenon, which is now so common, was distinctly seen, I shall relate the experiment, of which it was the result, at large.

MR. GREY, and his friends, provided themselves with an iron rod four feet long, and half an inch in diameter, pointed at each end, but not sharp. Suspending this iron rod upon silk lines in the night; and, applying the excited tube to one end of it, they perceived, not only a light upon that end, but another issuing from the opposite end, at the same time. This light extended itself, in the form of a cone, whose vertex was at the end of the rod: and Mr. Grey says, that he and his company could plainly see, that it consisted of separate threads, or rays of light, diverging from the point of the rod, the exterior rays being incurvated. This light appeared at every stroke they gave the tube.

THEY likewise observed, that this light was always attended with a small hissing noise, which, they imagined, began at the end next the tube, increasing in loudness till it came to the opposite

posite end. He says, however, that this noise could not be heard, but by persons who stood near the rod, and attended to it.*

MR. GREY, repeating these experiments in the September following, after his return to London, observed an appearance, which he says, surpris'd him very much. After the tube had been applied to the iron rod, as before, when the light, which had been seen at both ends, had disappeared; it was visible again upon bringing his hand near the end of the rod; and, upon repeating this motion of his hand, the same phenomenon appeared for five or six times successively; only the rays were, at each time, shorter than the other. He also observed, that these lights, which were emitted by the tube upon the approach of his hand, were, like the others, attended with a hissing noise.

HE took notice, that the light which appeared on the end next the tube, when it was held oblique to the axis of the rod, had its rays tending towards it; and that, all the time he was rubbing the tube, those flashes of light appeared upon every motion of his hand up or down the tube, but that the largest flashes were produced by the motion of his hand downwards. †

WHEN he used two or three rods, laying them, either in a right line, or so as to make any angle with each other, and applied the tube to any one of their ends; he observed, that the farthest end of the farthest rod exhibited the same phenomena as one single rod. ‡

USING a rod pointed only at one end, he observed, that the other end gave but one single snap, but that it was much louder than the greatest of those which were given by the point of the

* Phil. Trans. abridged, Vol. 8. p. 398.

† Ib. p. 399.

‡ Ib. p. 400.

rod,

rod; also that the pain, resembling pricking or burning, was more strongly felt, and that the light was brighter and more contracted.

CONNECTING a pewter plate with the iron rod, and filling the plate with water, he observed the same light, the same pushing of the finger, as he calls it, and the same snapping, as when the experiment was made with the empty plate. And when the experiment was made with water, in the day light, it appeared to rise in a little hill, under the finger which was presented to it; and, after the snapping noise, fell down again, putting the water into a waving motion near the place where it had risen.

THESE effects were the same which he had before observed to proceed from the immediate action of the tube, but by these experiments, he says, that he found (what, no doubt, appeared a real advance in the science to him) that an actual flame of fire, together with an explosion, and ebullition of cold water, might be produced by communicative electricity. What he adds is so remarkable, that I shall repeat his own words, "And although these effects are at present but *in minimis*, it is probable, in time, there may be found out a way to collect a greater quantity of the electric fire, and consequently to increase the force of that power; which, by several of these experiments, *si licet magnis componere parva*; seems to be of the same nature with that of thunder and lightning." *

How exactly has this prophecy been fulfilled in the discoveries of the Leyden electricians, and Dr. Franklin; the former having discovered the amazing accumulation of the electric power, in what is called the Leyden phial; and the latter having proved

* Phil. Trans. abridged, Vol. 8. p. 401.

the matter of lightning to be the very same with that of electricity; though Mr. Grey might possibly mention thunder and lightning only by way of common comparison.

ON February the 18th. 1735, Mr. Grey, repeating the experiments of the iron rods with wooden ones, found all the effects to be similar, but much weaker; as it is now well known must have been the case; wood being so imperfect a conductor, and only in proportion to the moisture it contains.

AT the same time, he relates, that repeating the electrification of water, he found, that the phenomena before mentioned were produced, not only by holding the tube near the water, but when it was removed, and the finger afterwards brought near it.*

MAY the 6th. of the same year, he again suspended a boy on silk, and found that this boy was able to communicate the electric fire, first to one, and then to several persons standing upon electric bodies.

MR. GREY seems still to have imagined, that electricity depended, in some measure, upon colour. The boy suspended on blue lines, he says, retained his power of attraction fifty minutes; on scarlet lines, twenty five minutes; and on orange coloured lines, twenty one minutes. By these experiments, he says, we see the efficacy of electricity on bodies suspended upon lines of the same substance, but of different colour. †

BUT the greatest deception which this ingenious gentleman seems to have lain under, was occasioned by the experiments which he made with balls of iron, to observe the revolution of light bodies about them. The paragraph relating to these experiments, being the last which Mr. Grey wrote, I shall give it at length as a curiosity.

* Phil. Trans. abridged, Vol. 8. p. 402. † Ib. p. 405.

“ I HAVE lately made,” says he, “ several new experiments upon the projectile and pendulous motion of small bodies by electricity; by which small bodies may be made to move about larger ones, either in circles, or in ellipses; and those either concentric, or eccentric to the center of the larger body, about which they move, so as to make many revolutions about them. And this motion will constantly be the same way that the planets move about the sun, viz. from the right hand to the left, or from west to east. But these little planets, if I may so call them, move much faster in their *apogee*, than in the *perigee* parts of their orbits; which is directly contrary to the motion of the planets about the sun. *

THESE experiments Mr. Grey had thought of but a very little while before his last illness, and had not time to complete them; but the progress he had made in them he revealed, on the day before his death, to Dr. Mortimer, then secretary to the Royal Society. He said they struck him with new surprise every time he repeated them, and hoped that, if God would spare his life a little longer, he should, from what these phenomena pointed out, bring his electrical experiments to the greatest perfection. He did not doubt but, in a short time, he should be able to astonish the world with a new sort of planetarium, never before thought of; and that, from these experiments, might be established a certain theory, to account for the motions of the grand planetarium of the universe. These experiments, fallacious as they are, deserve to be briefly recited, together with those which were made in consequence of them after Mr. Grey's death. I shall relate them in Mr. Grey's own words, as they were delivered to Dr. Mortimer, on his death bed.

* Phil. Transf. abridged, Vol. 8. p. 402.

PLACE a small iron globe, said he, of an inch, or an inch and a half in diameter, on the middle of a circular cake of rosin, seven or eight inches in diameter, gently excited; and then a light body suspended by a very fine thread, five or six inches long, held in the hand over the center of the table, will, of itself, begin to move in a circle round the iron globe, and constantly from west to east. If the globe be placed at any distance from the center of the circular cake, it will describe an ellipse, which will have the same eccentricity, as the distance of the globe from the center of the cake.

IF the cake of rosin be of an elliptical form, and the iron globe be placed in the center of it, the light body will describe an elliptical orbit, of the same eccentricity with the form of the cake.

IF the iron globe be placed in, or near, one of the foci of the elliptical cake, the light body will move much swifter in the apogee, than in the perigee of its orbit.

IF the iron globe be fixed on a pedestal, an inch from the table, and a glass hoop, or a portion of a hollow glass cylinder, excited, be placed round it; the light body will move as in the circumstances mentioned above, and with the same varieties.

HE said, moreover, that the light body would make the same revolutions, only smaller, round the iron globe placed on the bare table, without any electrical body to support it; but he acknowledged he had not found the experiment succeed, if the thread was supported by any thing but a human hand; though, he fancied, it would have succeeded, if it had been supported by any animal substance, living or dead.*

MR. GREY went on to recite to Dr. Mortimer other experi-

* Phil. Trans. abridged, Vol. 8. p. 404, 405.

ments still more fallacious; which, out of regard to his memory, I shall forbear to quote. Let the chimeras of this great electrician teach his followers, in the same, and still but newly opened field of philosophy, a proper degree of caution in their reasonings from induction. Let not the example, however, discourage any person from trying what may appear improbable; but let it induce a man to delay the publication of his discoveries, till they have been perfectly ascertained, and performed in the presence of others. In experiments of great delicacy, a strong imagination will have great influence even upon the external senses; of which we shall have frequent instances in the course of this history.

DR. MORTIMER himself seems to have been deceived by these experiments of Mr. Grey. He says, that, in trying them after his death, he found, that the light body would make revolutions round bodies of various shapes and substances, as well as round the iron globe; and that he had actually tried the experiment, with a globe of black marble, a silver standish, a small chip box, and a large cork.*

THESE experiments of Mr. Grey were tried by Mr. Wheeler, and other gentlemen, at the Royal Society's house, and with a great variety of circumstances; but no conclusion could be drawn from what they at that time observed. Mr. Wheeler himself took a great deal of pains to verify them, with various success; and at last he give it as his opinion, that a desire to produce the motion from west to east was the secret cause, that determined the pendulous body to move in that direction, by means of some impression from Mr. Grey's hand, as well as his own; though

* Phil. Transf. abridged, Vol. 8. p. 405.

he was, at the same time, persuaded, that he was not sensible of giving any motion to his hand himself. *

MR. DU FAYE, in the memoirs of the academy of sciences for the year 1737, acknowledges that these experiments of Mr. Grey and Mr. Mortimer did not succeed with him. But says, they were the only ones of Mr. Grey that had not; and, with a temper becoming a philosopher, adds, that he doth not, therefore, say, that they never had succeeded; but seems rather willing to attribute the failure with himself, to his omitting some circumstance, not mentioned by those gentlemen, though, unknown to them, it might be of principal consequence to the experiment. †

* Phil. Trans. abridged, Vol. 8. p. 418.

† Geschichte, Vol. 4. p. 226.

PERIOD VI.

THE EXPERIMENTS OF DR. DESAGULIERS.

WE are now come to the labours of that indefatigable experimental philosopher, DR. DESAGULIERS, in this new field of science. The reason which he gives why he had avoided entertaining the Royal Society upon this subject before, and why he had not pursued it so far as he might have done; considering, as he says, that he could excite as strong an electricity in glass, by rubbing with his hand, as any body could, is worth mentioning for its curiosity, and for the light that it throws upon the temper and manner of Mr. Grey. He says, that he was unwilling to interfere with the late Mr. Stephen Grey, who had wholly turned his thoughts to electricity; but was of a temper to give it entirely over, if he imagined that any thing was done in opposition to him.*

DR. DESAGULIERS begins with observing very sensibly (and the observation is still true) that the phenomena of electricity are so odd, that, though we have a great many experiments upon that subject, we have not yet been able, from their comparison, to settle such a theory as will lead us to the cause of that property in bodies, or even to judge of all its effects, or find out what useful influence electricity has in nature; though certainly, from

* Phil. Trans. abridged, Vol. 8. p. 419.

what

what we have seen of it, we may conjecture that it must be of great use, because it is so extensive.

His first experiments, of which an account is given in the Philosophical Transactions, dated July, 1739, were made with a hempen string, extended upon cat-gut. To the end of the hempen string, he suspended various substances; and says, that all those which he tried, amongst which were several *electrics per se*, as sulphur, glass, &c. without exception, received electricity.*

He changed one of the cat-gut strings, on which his hempen line of communication was extended, and put various other substances in its place, to try what bodies would transmit electricity to the suspended body, and what would not; and from the result of his experiments, partly concluded, that bodies in which electricity could not be excited intercepted the electric effluvia; and that those in which electricity could be excited, did not intercept it, but permitted it to go on to the extremity of the hempen string. But still he had no just idea, that, except metals, it was the moisture in the bodies he tried which intercepted the electric effluvia; and his ideas of the manner in which they were intercepted were very imperfect.

To Dr. Desaguliers we are indebted for some technical terms which have been extremely useful to all electricians to this day, and which will probably remain in use as long as the subject is studied. He first applied the term *conductor* to that body to which the excited tube conveys its electricity; which term has since been extended to all bodies that are capable of receiving that virtue. And he calls those bodies in which electricity may be excited by heating or rubbing *electrics per se*.

* Phil. Trans. abridged, Vol. 8. p. 420.

In the writings of this author we find many axioms relating to electrical experiments, some of which are expressed in a more clear, and distinct manner than they had been before; but the real improvements which he made, were very few and immaterial.

On several occasions, and particularly in a paper delivered to the Royal Society, in the month of January 1741, he lays down, among others, the following general rules, which seem to be more accurate than any which had been delivered before upon the subject.*

“An electric per se will not receive electricity from another electric per se, in which it has been excited, so as to run along its whole length: but will only receive it a little way, being, as it were, saturated with it.

“An electric per se will not lose all its electricity at once, but only the electricity of those parts near which a non-electric has been brought. It, consequently, loses its electricity the sooner, the more of those bodies are near it. Thus, in moist weather, the excited tube holds its virtue but a little while, because it acts upon the moist vapours which float in the air. And if the excited tube be applied to leaf gold laid upon a stand, it will act upon it much longer, and more strongly, than if the same quantity of leaf gold be laid upon a table, which has more non-electric surface than the stand.”† This, however, seems not to be the whole reason; for if the leaf gold were laid upon a broad surface of glass, it would not be acted upon so powerfully; as if it were placed upon a narrow stand of any kind of matter.

“A NON-ELECTRIC, when it has received electricity, loses it

* Phil. Trans. abridged, Vol. 8. p. 430. † Id. p. 427.

“ all

“ all at once, upon the approach of another non-electric. ”
 This however could only be the case when the approaching electric was not insulated, but had a communication with the earth. It must also be brought into contact with the electrified body.

“ ANIMAL substances are non-electrics by reason of the fluids they contain. *

“ EXCITED electricity exerts itself in a sphere round the electric *per se*, or rather in a cylinder, if the body be cylindrical. †

FEW of the many experiments which were made by Dr. Desaguliers (accounts of which were published in the Philosophical Transactions) had, as I observed before, any thing new in them. Those which were the most so are the following.

ENDEAVOURING to communicate electricity to a burning tallow candle, he observed, that the candle attracted the thread of trial, but not within two or three inches of the flame; but that, as soon as the candle was blown out, the thread was attracted by every part of it, and even by the wick, when the fire was quite extinguished. He electrified a wax candle in the same manner, and the experiment succeeded as well, only the electricity came not so near the flame in the wax, as in the tallow candle.

HE says, that only warming a glass receiver, without any rubbing, would cause the threads of a down feather, tied to an upright skewer to extend themselves, as soon as it was put over the feather; and that sometimes rosin and wax would exert their electricity by being only exposed to the open air.

HE observed that if a hollow glass tube, supporting the line of communication, were moistened by blowing through it, it would intercept the electricity.

* Phil. Trans. abridged, Vol. 8. p. 429.

† Ib. p. 431.

HE says, that when an excited tube has repelled a feather, it will attract it again, after being suddenly dipped into water, but in fair weather it will not attract it unless it hath been dipt pretty deep into the water, a foot of its length at least; whereas, in moist weather, an inch or two will suffice.*

HE shewed the attraction of water by an excited tube, in a better manner than it had been shewn before, viz. by bringing the tube to a stream issuing from a condensing fountain; which, thereupon, was evidently bent towards it.

DR. DESAGULIERS seems to have been the first who expressly said, that pure air might be ranked amongst electrics per se, and that cold air in frosty weather, when vapours rise least of all, is preferable, for electrical purposes, to warm air in summer, when the heat raises the vapours. † He also supposed that the electricity of the air was of the vitreous kind; and he accounted for the electricity appearing on the inside only of an exhausted glass vessel, by its going where it met with the least resistance from so electrical a body as the air. ‡

HE endeavoured to account for the fixing of air by the steams of sulphur, according to the experiment of Dr. Hales; by supposing that the particles of sulphur, and those of air, being possessed of different kinds of electricity, attracted one another, whereby their repulsive power was destroyed. He also proposed the following conjecture concerning the rise of vapour. The air at the surface of water being electrical, particles of water, he thought, jumped to it, then, becoming themselves electrical, they repelled both the air and one another, and consequently ascended into the higher regions of the atmosphere. ||

THE last paper of Dr. Desaguliers in the Philosophical Tran-

* Phil. Trans. abridged, Vol. 8. p. 429. † Ib. p. 437. ‡ Ib. p. 438. || Ib. p. 437.
sactions,

sactions, upon the subject of electricity is dated, June 24th. 1742, in which year he published a dissertation on electricity, by which he gained the prize of the academy at Bourdeaux. This prize was a medal of the value of 300 livres, proposed, at the request of Monsieur Harpez de la Force, for the best essay on electricity, and shews how much this subject engaged the attention of philosophers at that time. * The dissertation is excellently drawn up, and comprises all that was known of the subject till that time.

* Geschichte, Vol. 1. p. 261.

PERIOD VII.

EXPERIMENTS OF THE GERMANS, AND OF DR. WATSON,
BEFORE THE DISCOVERY OF THE LEYDEN PHIAL IN THE
YEAR 1746.

ABOUT the time that Dr. Desaguliers had concluded his experiments in England, viz. 1742, several ingenious Germans began to apply themselves to the same studies with great assiduity, and their labours were crowned with considerable success.

To the Germans we are indebted for many capital improvements in our electrical apparatus within this period, without which, the business would have gone on very slowly and heavily; but, by the help of their contrivances, we shall see astonishing effects were soon produced.

MR. BOZE, a professor of philosophy at Wittemburgh, substituted the *globe* for the tube, which had been used ever since the time of Hawkesbee.* He likewise added a *prime conductor*, which consisted of a tube of iron or tin, at first supported by a man standing upon cakes of rosin, and afterwards suspended on silk horizontally before the globe. †

* According to other accounts, Christian Augustus Hausen, professor of mathematics at Leipzig, was the first who revived the use of Hawkesbee's glass globe, and Mr. Boze, who was excited to make experiments in electricity by the example of Mr. Hausen, borrowed this capital improvement from him. *Geschichte*, Vol. 1. p. 278, 279.

† *Histoire de l'électricité*, p. 27.

To prevent the tube from doing any harm to the globe, he put a bundle of thread into the end which was next to it, and which was left open for that purpose. This expedient, besides occasioning various pleasant phenomena, was observed to make the force of the conductor much stronger.*

THE use of the globe was immediately adopted in the university of Leipzig, where Mr. Winckler, the professor of languages, substituted a *cushion* instead of the hand, which had before been employed to excite the globe. But the best rubber for the globe, as well as the tube, was, long after this, still thought, by all electricians, to be the human hand, dry, and free from moisture. †

MR. P. GORDON, a Scotch Benedictine monk; and professor of Philosophy at Erford, was the first who used a *cylinder* instead of a globe. His cylinders were eight inches long, and four inches in diameter. They were made to turn with a bow, and the whole instrument was portable. Instead of a cake of rosin, he insulated by means of a frame, furnished with net work of silk. ‡

THE apparatus, likewise, of many of the German electricians was very *various*, and expensive. Mr. Winckler, in a paper read at the Royal Society, March 21st 1745, § describes a machine for rubbing tubes, and another for rubbing globes, and compares the effects of them both. He observes, that the sparks which are produced from glass vessels drawn to and fro were larger, and more vehemently pungent, provided that those vessels were of the same magnitude with the globes; but that the flux of effluvia was not so constant as from the globes. Mr. Winekler also invented a machine, which he describes at large

* Phil. Trans. abridged, Vol. 10. p. 271. † lb. p. 272. ‡ Histoire, p. 31.

§ Phil. Trans. abridged, Vol. 10. p. 273.

in his works, by means of which he could give his globe six hundred and eighty turns in a minute.* This gentleman likewise contrived to rub glass, and china vessels, in the inside; and he says they acted as strongly on bodies placed on the outside of them, as when they were rubbed on the outside. †

THE German electricians generally used more globes than one at a time, and imagined they found the effects proportionable, though this fact was called in question by Dr. Watson, and others; and Mr. Nollet preferred globes made blue with zaffre, which were carefully tried, and rejected by Dr. Watson afterwards. ‡

SUCH a prodigious power of electricity could they excite from these globes, whirled by a large wheel, and rubbed with woolen cloth, or a dry hand (for we find both these methods were in use among them about this time) that, if we may credit their own accounts, the blood could be drawn from the finger by an electric spark; the skin would burst, and a wound appear, as if made by a caustic. They say, that if several globes or tubes were used, the motion of the heart and arteries of the electrified person would be very sensibly increased; and that, if a vein were opened under the operation, the blood, issuing from it, would appear like lucid phosphorus, and run out faster than when the man was not electrified. Analogous to this last experiment, Mr. Gordon observed, that water, running from an artificial fountain electrified, was scattered in luminous drops, that a larger quantity of water was thrown out in a given time than when the fountain was not electrified. || And that electrified water evaporated faster than water not electrified, when exposed in similar glass vessels. § Part of this account we know might

* Histoire, p. 32. † Geschichte, Vol. 1. p. 400. ‡ Phil. Transf. abridged, Vol. 10. p. 277.

§ Phil. Transf. abridged, Vol. 10. p. 277. || Geschichte, Vol. 2. p. 357.

be true, but some part must have been exaggerated. It is certain that Mr. Gordon increased the electric sparks to such a degree, that they were felt from a man's head to his foot, so that a person could hardly take them without falling down with giddiness,* and small birds were killed by them. † This he effected by conveying electricity, with iron wires, to the distance of 200 ells from the place of excitation. He also found that the sparks were stronger when the wire was thick than when it was small. ‡

MR. WAITZ made his glass tubes act stronger by rubbing them with a waxed cloth and a little oil. He also found, that glass made with very little pot ash, acted much better than that in the composition of which much of it was used; but it required longer time, and more heat to vitrify it. He got some glass made on purpose to ascertain the fact. §

By various experiments of attraction and repulsion, which Mr. Waitz made in rubbing a dog (which he had made thoroughly dry for that purpose) he proved that the flashes of light, which sometimes appear when animals are stroked, are electrical. This had been supposed, but was not accurately ascertained before. § It was this gentleman who gained the prize of 50 ducats, proposed, in the year 1744, by the academy of sciences at Berlin, for the best dissertation on the subject of electricity. It was published along with three others, which were offered at the same time, and thought worthy of that honour. *

THE thing that strikes us most in their experiments, performed by these machines, is their setting fire to inflammable substances. This they were, probably, led to attempt, from observing

* Geschichte, Vol. 2. p. 358.

† Nollet's Recherches, p. 172.

‡ Geschichte, Vol. 2. p. 359.

§ Ib. p. 381.

§ Ib. p. 385.

* Ib. p. 380.

the

the vivid appearance of electric light, the burning pain that was felt by a smart stroke from the conductor, and the many analogies the electric fluid evidently bore to phosphorus and common fire.

THE first person who succeeded in this attempt was Dr. Ludolf of Berlin, towards the beginning of the year 1744; who kindled, with sparks excited by the friction of a glass tube, the ethereal spirits of Frobenius. This he did at the opening of the Royal Academy, and in the presence of some hundreds of persons. He performed the experiment by electric sparks proceeding from an iron conductor. John Henry Winckler, Greek and Latin professor at Leipsick, did the same in the May following, by a spark from his own finger; and kindled, not only the highly rectified spirit above-mentioned, but French brandy, corn spirits, and other spirits still weaker, by previously heating them. He also says, that oil, pitch, and sealing wax might be lighted by electric sparks, provided those substances were first heated to a degree next to kindling. * To these it must be added, that Mr. Galath fired the smoke of a candle just blown out, and lighted it again; † and that Mr. Boze fired gunpowder melting it in a spoon, and first the vapour that rose from it.

THE German electricians, likewise, constructed a machine, by which they could give friction to a glass cylinder in vacuo. By these means they contrived to electrify a wire which terminated in the open air, and there shewed a considerable electric power. They also electrified that end which was in the open air, and made the other end which was in vacuo exert its electricity. ‡

THE same Germans also mention an experiment, which, if pursued, would have led them to discover, that the friction of the

* Phil. Trans. abridged, Vol. 10. p. 275.

† Geschichte, Vol. 2. p. 438.

‡ Phil. Trans. abridged, Vol. 10. p. 275.

glass

glass globe did not produce, but only collect the electric matter. But that was a discovery reserved, as we shall find, for Dr. Watfon. It seems that both Mr. Boze and Mr. Allamand had suspended the machine, and the man who worked it, upon silk; and observed, that, not only the conductor, but also the man, and the machine gave signs of electricity; though they did not attend accurately to all the circumstances of that curious fact, which did not at all answer their expectations. For, imagining that part of the electric power was continually going off to the ground by the machine, they supposed that the effect of insulating it, would have been a stronger electricity.

In this period it was that Mr. Ludolf the younger demonstrated, that the luminous barometer is made perfectly electrical by the motion of the quicksilver; first attracting, and then repelling bits of paper, &c. suspended by the side of the tube, when it was enclosed in another out of which the air was extracted. † Before this experiment, those effects had been ascribed to the air. ‡ Professor Hamberger and Mr. Waitz had discovered that the motion of quicksilver in a glass vessel, out of which the air was extracted, had the power of moving light bodies; and Mr. Allamand likewise found, that it made no difference whether the vessel had air in it or not.

ABOUT the same time also, Mr. Boze took a great deal of pains to determine, whether the weight of bodies would be affected by electricity, but he could not find that it was.

THE electrical stars, made by turning swiftly round an electrified piece of tin, cut with points equidistant from the center; and also the electrical bells, which will be described hereafter

* Wilson's Essay, preface, p. 14. Watfon's Sequel, p. 34. † Geschichte, Vol. 3. p. 495.

‡ Histoire, p. 89. || Geschichte, Vol. 2. p. 426.

among the surprising and diverting experiments performed by the help of electricity, were of German invention. * The star was contrived by Mr. Gordon; and by turning the points a little obliquely, he was surprised to find it began to move of itself. † Lastly, to these it may be added, that Mr. Winckler contrived a wheel to move by electricity; that Mr. Boze conveyed electricity from one man to another by a jet of water, when they were both placed upon cakes of rosin, at the distance of six paces; and that Mr. Gordon even fired spirits by a jet of water. ‡

Mr. Gottfried Heinrick Grummert, of Biala in Poland, made a curious experiment upon electric light, which as we shall see, was afterwards made and pursued to great advantage by Dr. Watson and Mr. Canton. In order to observe whether an exhausted tube would give light when it was electrified, as well as when it was excited he presented one, eight inches long, and a third of an inch wide, to the electrified conductor, and was surprised to find the light dart very vividly the whole length of the tube. He also observed, that, sometime after the tube had been presented to the conductor, and exposed to nothing but the air, it gave light again, without being brought to any electrified body. This light in *vacuo* Mr. Grummert proposed to make use of in mines and places, where common fires, and other lights cannot be had, and for this purpose, he mentions several methods of increasing this light. ||

I SHALL conclude this account of the discoveries of the German Philosophers in this period with a very curious one of professor Kruger, concerning the change made in the colour of bodies by the electric effluvia. In order to try, whether there was any

* Nollet's Recherches, p. 187. † Geschichte, Vol. 2. p. 317, 358.

‡ Phil. Transf. abridged, Vol. 10. p. 276. § Ib. p. 417.

thing

thing of sulphur in these effluvia, he exposed the red leaves of wild poppies to the electric spark, and found that they were presently changed to white. He was not able to produce any change in yellow colours, nor in blue immediately; but found that when they had lain a day or two, after being exposed to this operation they became white. In these experiments the leaves were fastened with white wax to plates of tin. *

SUCH a general attention was excited to electricity by these curious discoveries, that, in the year 1745, electrical experiments were exhibited, in Germany and Holland, for money, as a show; and public advertisements appeared in the news papers for that purpose. †

THE firing of the effluvia of bodies, which was first done in Germany, was soon after repeated in England, and among others by Dr. Miles; who, as appears by a paper of his, read at the Royal Society, March the 7th. 1745, kindled phosphorus by the application of the excited tube itself to it, without the intervention of any conductor. ‡

THIS gentleman's tube happening to be in excellent order upon this occasion, he observed, and was perhaps the first who observed, *pencils of rays*, which he calls *coruscations*, darting from the tube, without the aid of any conductor approaching it. Of these coruscations he gave a drawing, which answers pretty exactly to the appearance of such pencils as are now very common, particularly since Mr. Canton has taught us the use of the amalgama, by which a tube may be excited much more strongly than it could have been before. ||

BUT the most distinguished name in this period of the history of electricity, is that of DR. WATSON. He was one of the first

* Geschichte, Vol. 3. p. 493. † Ib. Vol. 2. p. 399.

‡ Phil. Trans. abridged, Vol. 10. p. 272. || Ib. p. 272.

among the English who took up, and improved upon, the discoveries made by the Germans; and to his ingenuity, and intense application, we owe many curious improvements and discoveries in electricity. His first letters to the Royal Society on this subject are dated between March 28th. and October 24th.

1745.

DR. WATSON'S attention to the subject of electricity seems first, or principally, to have been engaged by the accounts of the Germans having fired spirit of wine by it. In this experiment he succeeded; and, moreover, found that he was able to fire, not only the phlogiston of Frobenius, and rectified spirit of wine, but even common proof spirit. He also fired air made inflammable by a chymical process. * He even fired both spirit of wine, and inflammable air, by a drop of cold water, thickened with a mucilage made with the seed of flea wort, and even with ice. † He also fired these substances with a hot poker electrified, when it would not fire them in any other state. ‡ He fired gunpowder and discharged a musket by the power of electricity, when the gunpowder had been ground with a little camphor, or a few drops of some inflammable chymical oil. || Lastly, it was a discovery of Dr. Watson's, that these substances were capable of being fired by what he calls *the repulsive power of electricity*; which was performed by the electrified person holding the spoon which containeth the substance to be fired, and another person, not electrified, bringing his finger to it. § Before this time, the substance to be fired had always been held by a person not electrified.

In his attempts to fire electrics per se, as turpentine, and balsam of capivi, by this repulsive power, he thought he confuted

* Phil. Trans. abridged, Vol. 10. p. 286. † Ib. p. 290. ‡ Ib. p. 288. || Ib. p. 289.

§ Ib. p. 281.

an opinion which had prevailed amongst many persons, that electricity only floated on the surfaces of bodies, for he found that the fume of these substances could not be fired by a spark fetched from the spoon which contained them. This spark must therefore pass through the electric, from the surface of the spoon below, which was in contact with the electrified conductor.

ELECTRIFYING a number of pieces of fine spun glass, and other pieces of wire, of the same length and thickness, he was agreeably amused by observing, that the threads of glass jumped to the electrified body, and adhered to it without any snapping; whereas the wires jumped up and down very fast, giving a snap, and a small flame, every time.*

In a paper read at the Royal Society, February 6th 1746, he observed, that electric sparks appeared different in colour and form, according to the substances from which they proceeded; that the fire appeared much redder from rough bodies, as rusty iron, &c. than from polished bodies, though they were ever so sharp, as from polished scissars, &c. He judged that the different appearance was owing, rather to the different reflection of the electric light from the surface of the bodies from which it was emitted, than to any difference in the fire itself. †

He also observed, that electricity suffered no refraction in pervading glass; having found, by exact observations, that its direction was always in right lines, even through glasses of different forms, included one within another, and large spaces left between each glass; ‡ that if books or other non-electrics were laid upon glass and interposed between the excited electric and light bodies, the direction of the virtue was still in right lines, and seemed instantly to pass through both the books and the

* Phil. Transf. abridged, Vol. 10. p. 286.

† Ib. p. 290.

‡ Ib. p. 291.

glass.

glafs. In these experiments he constantly observed, that the electric attraction through glafs was much more powerful, when the glafs was made warm, than when it was cold. * He sometimes found electricity to pervade, though in small quantities, electrics of above four inches thick. †

He says, that in electrifying substances of great extent, the attractive power was first observed, at that part of it which was most remote from the excited electric.

He made some experiments which showed, that the fire of electricity was affected, neither by the presence, nor the absence of other fire. One of his experiments was made with a chymical mixture, thirty degrees below the freezing point of Fahrenheit's thermometer; from which, when electrified, the flashes were as powerful, and the strokes as smart, as from red hot iron. ‡

In a sequel to the above experiments, read the 30th of October 1746, Dr. Watson mentions his having lined a glafs globe to a considerable thickness with a mixture of wax and rosin; but he found no difference between that and the other globes. ||

He also made various experiments with a number of globes, whirled at the same time, and having one common conductor; and concluded from them, that the power of electricity was increased by the number and size of the globes, to a certain degree, but by no means in proportion to their number and size. Yet the Doctor allows a very great increase, in an inference he makes from these very experiments. As bodies to be electrified, he says, will only contain a certain quantity of electricity; when that quantity is acquired, *which is soonest done by a number of globes,* the surcharge is dissipated as fast as it is excited. So that, it is

* Phil. Trans. abridged, Vol. 10. p. 292. † Ib. p. 295. ‡ Ib. p. 293. || Ib. p. 295.

plain,

plain, more fire was collected by the number of globes, though the form of the conductor he made use of was such as could not retain it. The great power of his four globes united is manifest from his own account of them. For, he says, that when two pewter plates were held, one in the hand of an electrified person, and the other in the hand of one who stood upon the floor; the flashes of pure and bright flame were so large, and succeeded each other so fast, that, when the room was darkened, he could distinctly see the faces of thirteen persons who stood round the room.*

LASTLY, the Doctor found, that the smoke of original electrics was a conductor of electricity, and also that flame would conduct the whole of it undiminished; by observing that two persons, standing upon electrics, could communicate the virtue to each other, with nothing interposed but the smoke in the one case, and flame in the other.†

It was in this period that Mr. Du Tour discovered, that flame would destroy electricity; as he informed the Abbé Nollet, in a letter dated 21st August 1745. The same was also discovered by Mr. Waitz.

* Phil. Transf. abridged, Vol. 10. p. 295.

† Ib. p. 296.

PERIOD VIII.

THE HISTORY OF ELECTRICITY, FROM THE DISCOVERY OF
THE LEYDEN PHIAL IN THE YEARS 1745 AND 1746, TILL
DR. FRANKLIN'S DISCOVERIES.

SECTION I.

THE HISTORY OF THE LEYDEN PHIAL ITSELF, TILL
DR. FRANKLIN'S DISCOVERIES RELATING TO IT.

THE end of the year 1745, and the beginning of 1746 were famous for the most surprising discovery that has yet been made in the whole business of electricity, which was the wonderful accumulation of its power in glass, called at first the LEYDEN PHIAL; because made by Mr. Cuneus a native of Leyden, as he was repeating some experiments which he had seen with Messrs. Muschenbroeck, and Allamand, professors in the university of that city. * But the person who first made this

* Delibard's Histoire abrégée. p. 33.

great discovery, was Mr. Von Kleift, dean of the cathedral in Camin; who, on the 4th. of November 1745, sent an account of it to Dr. Lieberkuhn at Berlin. This account, as taken by Mr. Gralath out of the register of an academy at Berlin, to which it had been communicated, is as follows. "When a nail, or a piece of thick brass wire, &c. is put into a small apothecary's phial and electrified, remarkable effects follow: but the phial must be very dry, or warm. I commonly rub it over before-hand with a finger, on which I put some pounded chalk. If a little mercury or a few drops of spirits of wine, be put into it, the experiment succeeds the better. As soon as this phial and nail are removed from the electrifying glass, or the prime conductor, to which it hath been exposed, is taken away, it throws out a pencil of flame so long, that, with this burning machine in my hand, I have taken above sixty steps, in walking about my room. When it is electrified strongly, I can take it into another room, and there fire spirits of wine with it. If while it is electrifying, I put my finger, or a piece of gold, which I hold in my hand, to the nail, I receive a shock which stuns my arms and shoulders.

"A TIN tube, or a man, placed upon electrics, is electrified much stronger by this means than in the common way. When I present this phial and nail to a tin tube, which I have fifteen feet long, nothing but experience can make a person believe how strongly it is electrified. I am persuaded, he adds, that, in this manner, Mr. Boze would not have taken a second electrical kiss. Two thin glasses have been broken by the shock of it. It appears to me extraordinary, that when this phial and nail are in contact with either conducting or non-conducting matter the strong shock does not follow. I have cemented it to wood, metal, glass, sealing wax &c. when I have

M

" electrified

“ electrified without any great effect. The human body, therefore, must contribute something to it. This opinion is confirmed by my observing, that, unless I hold the phial in my hand, I cannot fire spirits of wine with it. *

NOTWITHSTANDING Mr. Kleist immediately communicated an account of this famous experiment (which indeed it is evident he has but imperfectly described) to Mr. Winckler at Leipfick, Mr. Swietniki of Dantzick, Mr. Krugar of Hall, and to the professors of the academy of Lignitz, as well as to Dr. Lieberkuhn of Berlin above-mentioned, they all returned him word, that the experiment did not succeed with them. Mr. Galath of Dantzick, was the first with whom it answered; but this was not till after several fruitless trials, and receiving farther instructions from the inventor. †

THE Abbé Nollet had information of this discovery, and, in consequence of it, says, in a letter to Mr. Samuel Wolfe, of the society of Dantzick, dated March, 9th. 1746, that the experiment at Leyden was upon principles similar to this made with a phial half full of water, and a nail dipped in it; and that this discovery would have been called the Dantzick experiment, if it had not happened to have got the name of that of Leyden. ‡

THE views which led to this discovery in Holland were, as I have been informed, as follows. Professor Muschenbroeck and his friends, observing that electrified bodies, exposed to the common atmosphere, which is always replete with conducting particles of various kinds, soon lost their electricity, and were capable of retaining but a small quantity of it, imagined, that, were the electrified bodies terminated on all sides by original electrics, they might be capable of receiving a stronger power, and

* Geschichte, Vol. 1. p. 497.

† Ib. p. 411.

‡ Ib. p. 409.

retaining

retaining it a longer time. Glass being the most convenient electric for this purpose, and water the most convenient non-electric, they first made these experiments with water, in glass bottles: but no considerable discovery was made, till Mr. Cuneus, happening to hold his glass vessel in one hand, containing water, which had a communication with the prime conductor, by means of a wire; and, with the other hand, disengaging it from the conductor (when he imagined the water had received as much electricity as the machine could give it) was surpris'd by a sudden shock in his arms and breast, which he had not in the least expected from the experiment.

MR. ALLAMAND and Mr. Muschenbrœck were the first who repeated and published an account of this experiment in Holland, the Abbé Nollet and Monsieur Monnier in France, and Messrs. Galath and Rugger in Germany.*

It is extremely curious to observe the descriptions which philosophers, who first felt the electrical shock, give of it; especially as we are sure we can give ourselves the same sensation, and thereby compare their descriptions with the reality. Terror and surprisè certainly contributed not a little to the exaggerated accounts they gave of it; and, could we not have repeated the experiment, we should have formed a very different idea of it from what it really is, even when given in greater strength than those who first felt this electrical shock were able to give it. It will amuse my readers if I give them an example or two.

MR. MUSCHENBRÖECK, who tried the experiment with a very thin glass bowl, says, in a letter to Mr. Reaumur, which he wrote soon after the experiment; that he felt himself struck in his arms, shoulders and breast, so that he lost his breath, and

*Geschichte, Vol. 2. p. 433.

was two days before he recovered from the effects of the blow and the terror. He adds, that he would not take a second shock for the kingdom of France. *

THE first time Mr. Allamand made this experiment (which was only with a common beer glass) he says, that he lost the use of his breath for some moments; and then felt so intense a pain all along his right arm, that he at first apprehended all consequences from it, though it soon after went off without any inconvenience. † But the most remarkable account is that of Mr. Winckler of Leipzig. He says, that the first time he tried the Leyden experiment, he found great convulsions by it in his body; and that it put his blood into great agitation; so that he was afraid of an ardent fever, and was obliged to use refrigerating medicines. He also felt an heaviness in his head, as if a stone lay upon it. Twice, he says it gave him a bleeding at his nose, to which he was not inclined; and that his wife (whose curiosity, it seems, was stronger than her fears) received the shock only twice, and found herself so weak, that she could hardly walk; and that, a week after, upon recovering courage to receive another shock, she bled at the nose after taking it only once. ‡

WE are not, however, to infer from these instances, that all the electricians were struck with this panic. Few, I believe, would have joined with the cowardly professor, who said that he would not take a second for the kingdom of France. Far different from these were the sentiments of the magnanimous Mr. Boze, who with a truly philosophic heroism, worthy of the renowned Empedocles, said he wished he might die by the electric shock, that the account of his death might furnish an article for the me-

* Histoire, de l'électricité, p. 30. † Phil. Transf. abridged, Vol. 10. p. 321.

‡ Ib. p. 327.

moirs of the French academy of sciences. * But it is not given to every electrician to die in so glorious a manner as the justly envied Richman.

IT was this astonishing experiment that gave eclat to electricity. From this time it became the subject of general conversation. Every body was eager to see, and, notwithstanding the terrible account that was reported of it, to *feel* the experiment; and in the same year in which it was discovered, numbers of persons, in almost every country in Europe, got a livelihood by going about and showing it.

WHILE the vulgar of every age, sex, and rank were viewing this prodigy of nature and philosophy with wonder and amazement; we are not surprized to find all the electricians of Europe immediately employed in repeating this great experiment, and attending to the circumstances of it. Mr. Allamand remarked, that, when he first tried it, he stood simply upon the floor, and not upon cakes of rosin. He said, that it did not succeed with all kinds of glass; for that, though he had tried several, he had perfect success with none but those of Bohemia, and that he had tried English glasses without any effect at all. † Professor Muschenbroeck at that time only observed, that the glass must not be all wet on the outside.

IT is no wonder that so few of the properties of glass charged with electrical fire were known at first, notwithstanding the attention that was immediately given to the subject by all the electricians in Europe. The experiment is, to this day, justly viewed with astonishment by the most profound electricians: for, though some remarkable phenomena of it have been excellently accounted for by Dr. Franklin, and others, much remains to be

* Histoire, p. 164.

† Phil. Trans. abridged, Vol. 10. p. 321.

done;

done; and, in many respects, the circumstances attending it are still inexplicable. What will result from more attention being given to it, time only can show.

To begin the further illustration of this discovery with such of the phenomena as were observed in Germany, where it was made, Mr. Galath made the shock much stronger by using a glass vessel five inches in diameter, with a narrow neck, ten inches long; by substituting an iron wire with a knob of tin for the iron nail, and water for spirit of wine. * He first found, that the same shock could be communicated to a number of persons, who took hold of one another's hands; if the person at one extremity of the line they made, touched the outside of the phial, and he at the other touched a wire communicating with the inside. In this manner, on the 10th. of April 1746, he gave a shock to twenty persons; and he says, he did not doubt, but it might be given to a thousand. † When these persons were connected by pieces of metal, and did not hold one another's hands, they found the same shock, but not when they held wood, and other imperfect conductors. ‡ This gentleman, also, gave a shock, by means of long wires, to a person standing in a garden, while he himself, who directed the experiment, had the machine in a part of the house at a considerable distance; § and he was the first who made what we now call an *electrical battery*; for he increased the shock by charging several phials at the same time. ¶ Lastly he observed that if the phial had the least crack in it, it could never give a shock; and also that when a phial was discharged it acquired a small charge by standing, without receiving any thing from the machine, so as to give a small

* Geschichte Vol. 2. p. 411. † Ib. p. 439. ‡ Ib. p. 440. § Ib. p. 552. shock.

shock.* This is what we now call the *residuum* of a charge, and is properly part of the charge that lay on the uncoated part of the phial, which doth not let go all its electricity at once; so that it is, afterwards, gradually diffused to the coating.

MR. WINCKLER found out the method of making the discharge of the phial without feeling the shock himself, by not bringing his own body into the direct circuit. † He also gave the shock when several ells of running water, in his garden, made part of the circuit. This he did on the 28th of July 1746, about the same time that Monsieur Monnier performed the same experiment in France. ‡ And, lastly, he found, that the more globes were used, and the larger they were, the stronger was the shock. § This must be the consequence of increasing the power of excitation.

WHEN Mr. Jallabert made the Leyden experiment with hot water, the phial broke by a spontaneous discharge, and a circular piece, two lines and a half in diameter, was thrown from the place of the rupture, against a wall, which was at the distance of five feet. The vessel, he says, had no crack, or other injury. §

DR. WATSON, who gives an account of this famous experiment in the Philosophical Transactions, observes, that it succeeded best when the phial, which contained the water was of the thinnest glass, and the water warmer than the ambient air. He says he tried the effect of increasing the quantity of water in glass vessels of different sizes, as far as four gallons, without in the least increasing the stroke. He also observed, that the force of the stroke did not increase in proportion to the size of the globe, or the number of globes employed upon the occasion; for that he had been as forcibly struck with a phial charged by means of a

* Dantzick Memoirs, Vol. 1. p. 514 516.

† Geschichte, Vol. 2. p. 459.

‡ Ib. Vol. 3. p. 504.

§ Ib. p. 526.

§ Jallabert's Experiences, p. 128.

globe

globe of seven inches in diameter, as from one of sixteen, or from three of ten; and that, at Hamburgh, a sphere had been employed of a Flemish ell in diameter, without the expected increase of power. But, in both these observations, there must have been some mistake. He found, that if mercury was used, instead of water, the stroke was by no means increased in proportion to its specific gravity. He also first observed, that several men, touching each other, and standing upon electrics, were all shocked, though only one touched the gun barrel; but that no more fire was visible from them all, than if one only had discharged it.

SEVERAL of these observations show how imperfectly this great experiment was understood, for some time after it was first made. Dr. Watson, however, observed a circumstance attending the charging of the phial, which, if pursued, would have led him to the discovery, which was afterwards made by Dr. Franklin. He says, that "when the phial is well electrified, and you apply your hand thereto, you see the fire flash from the outside of the glass, wherever you touch it, and it crackles in your hand".*

He also observed, that when a single wire only was fastened round a phial, properly filled with warm water, and charged; upon the instant of its explosion, the electrical corruscations were seen to dart from the wire, and to illuminate the water contained in the phial.

SEVERAL other very important circumstances, relating to the discharge of the phial, were observed by Dr. Watson. He found that the stroke was, *cæteris paribus*, as the points of contact of the non-electrics on the outside of the glass. And upon showing

* Phil. Transf. abridged, Vol. 10. p. 298.

Dr. Bevis the experiments which proved this assertion, the Doctor suggested a more clear and satisfactory method of proving it, and which has been the means of accumulating and increasing the force of charged glass, far beyond what was expected from the first discovery of it. This method was, *coating* the outside of the phial, very near to the neck, with sheet lead, or tinfoil. When a bottle was prepared in this manner, and nearly filled with water, they observed, that a person who only held in his hand a small wire communicating with that coating, felt as strong a shock as he would have felt, if his hand had been in actual contact with every part of the phial touched by the coating. *

DR. WATSON also discovered, that the electrical power, in the discharge of the phial, darts *rectissimo cursu*, as he styles it, between the gun barrel and the phial; and, though it is not strictly true, that the shock goes the nearest way, yet it does so *ceteris paribus*, which alone was a considerable discovery for that time. He observed, that, in a company joining hands, a person touching two other persons in the circle, who did themselves touch one another, felt nothing of the shock, his body making no necessary part of the circle; and also, if a man, holding a wire, which communicated with the outside of the phial, as it hung upon the conductor, should touch the conductor with it; the explosion was made, but the man felt nothing. †

IN a paper read at the Royal Society, January 21st. 1748. Dr. Watson mentions another discovery relating to the Leyden phial, which Dr. Bevis suggested, and he completed. Having been before fully satisfied, that the shock from the phial was not in proportion to the quantity of matter contained in the glass, but was increased by it, and likewise by the number of points of

* Phil. Trans. abridged, Vol. 10. p. 299. † Ib. p. 301.

non-electric contact on the outside of the glass; he procured three jars, into which he put round leaden shot, and, joining their wires and coating, discharged them all as one jar. Upon this he observed, that the electrical explosion from two or three of those jars was not double or treble to that from one of them; but that the explosion from three was much louder than that from two, and the explosion from two much louder than that from one.*

THIS experiment had induced him to imagine, that the explosion from those jars was owing to the great quantity of non-electric matter contained in them. And whilst he was considering of some certain method of assuring himself whether the fact was so, Dr. Bevis informed him, that he had found the electrical explosion to be as great from covering the sides of a pane of glass, within about an inch of the edge, (which was a curious improvement of Mr. Smeaton's) as it could have been from an half pint phial of water. Upon this Dr. Watson coated large jars with leaf silver, both inside and outside, within an inch of the top, and from the great explosion he produced, when so little non-electric matter was contained in them, he was of opinion, that the effect of the Leyden bottle was greatly increased by, if it was not principally owing to, not so much the quantity of non-electric matter contained in the glass, as the number of points of non-electric contact within the glass, and the density of the matter of which those points consisted; provided the matter was, in its own nature, a ready conductor of electricity. He also observed, that the explosion was greater from hot water inclosed in glasses, than from cold; and from his coated jars warmed, than when cold. †

THE Doctor observed, that when the circle for the discharge

* Phil. Transf. abridged, Vol. 10. p. 374.

† Ib. p. 377.

was not through perfect conductors, the explosion was made slowly, and not all at once. This law, he says, was invariable, but he was not able to account for it. But to prove that the electricity passed with its whole force through the circle of non-electrics, he made a circuit consisting of iron bars, and spoons filled with spirits between each bar, (but at some small distance from them) and, upon the explosion, all the spoons were on fire at once. This was the first time, as he observes, that spirits were ever fired without either the spirits, or the non-electric on which they were placed, being insulated, or put upon original electrics. And yet, he says, though we know, from its effects, that the electricity goes through the whole circuit of non-electrics, with all its vigour; its progress is so quick, as not to affect, by attracting or otherwise, any light bodies disposed very near the non-electrics, through which it must necessarily pass.*

It is curious to observe in what manner Dr. Watson explained the shock of the Leyden phial, about the time that he first made the experiment with it. He had then been led (by a course of experiments which will be mentioned hereafter) to the notion both of the *afflux*, and *efflux* of electric matter in all electrical experiments. To apply this principle to the case in hand, he supposed, that the man who felt the shock parted with as much of the fire from his body, as was accumulated in the water and the gun barrel; and that he felt the effect in both arms, from the fire which was in his body, rushing through one arm to the gun barrel, and through the other to the phial. He imagined also, that as much fire as the man parted with was instantly replaced from the floor of the room, and that with a violence equal to the manner in which he lost it. It also appears, from

* Phil. Trans. abridged, Vol. 10. p. 378.



Dr. Watſon's remarks on ſome ſubſequent experiments of Mr. Monnier, that he then imagined, that though a conſiderable quantity of the electric matter pervaded the glaſs (as he thought was ſeen upon preſenting a non-electric body to it, when it ſtood upon the glaſs ſtand, and without which it could not be charged at all) yet, that the loſs of the electric matter this way was not equal to what came in by the wire; the thinneſs of the glaſs permitting it not wholly, but partially to ſtop the electricity. *

AFTERWARDS, when (from a courſe of experiments which will alſo, be recited in their proper place) Dr. Watſon changed his opinion about this afflux and efflux of electric matter, with a generoſity and frankneſs becoming every inquirer after truth, he retracted this hypotheſis; and, in refutation of it, he further adds, that the charged phial will explode with equal violence, if the hoop of the wire be bent, ſo as to come near the coating of the phial, without any other non-electric body being near, from which ſuch a quantity could be ſupplied. He had alſo obſerved, that if a man ſtood upon glaſs, and diſcharged the phial, he felt the ſame ſhock as if he had ſtood upon the floor. I ſhall ſubjoin a remarkable paragraph of the Doctour himſelf upon this occaſion, as I think it very applicable even to us, in this more advanced ſtate of the ſcience.

“ I TAKE notice of theſe,” ſays the Doctour, “ in as much as, “ notwithſtanding the very great progreſs which has been made “ in our improvements in this part of natural philoſophy, with- “ in theſe few years; poſterity will regard us as only in our “ noviciate; and therefore it behoves us, as far as we can be “ juſtified therein by experiment, to correct any concluſions we “ may have drawn, if others yet more probable preſent them-

* Phil. Tranſ. abridged, Vol. 10. p. 348.

“ ſelves.



“ selves. *” The Doctor has lived to see, not only that *posterity* would consider him and his assistants at that time, as in their noviciate; but he *himself*, already in the course of a few years, looks upon both himself and them in the same light. And, considering the quick advances still making in this science, it is to be hoped he may still live to see, even the electricians of the present year, to have been only in their noviciate.

HAVING seen what was done by Dr. Watson towards explaining the electric shock, before it was undertaken by Dr. Franklin; let us see what obligation we are under to other English electricians, and particularly Mr. Wilson.

MR. WILSON says that, as early as the year 1746, he discovered a method of giving the shock to any particular part of the body without affecting the rest. † He increased the strength of the shock by plunging the phial in water, thereby giving it a coating of water on the outside, as high as it was filled on the inside. ‡

In a letter to Mr. Smeaton, dated Dublin October 6th. 1746, he mentions his having made some experiments, in order to discover the law of accumulation of the electric matter in the Leyden bottle; and found, that it was always in proportion to the thinness of the glass, the surface of the glass, and that of the non-electrics in contact with the inside and outside thereof. The experiments, he says, were made with water a little warmed, which was poured into the bottle, while the outside was immersed in a vessel filled with water, but a little colder; leaving three inches, or thereabouts, uncovered, which was preserved dry and free from dust. An account of this experiment he wrote to Mr. Folkes, and it was read before the Royal Society, Octo-

* Phil. Transf. abridged, Vol. 10, p. 373. † Wilson's Essay, p. 88. ‡ Ib. p. 71.
ber

ber 23d. 1746, as appears by their minutes of that day, though the original was lost or mislaid.

ANOTHER curious experiment Mr. Wilson made, in order to prove an hypothesis, which he conceived very early, of the influence of a subtle medium surrounding all bodies, and resisting the entrance or exit of the electric fluid. To determine this, he made the Leyden experiment with a chain, and considered each link of it as having two surfaces, at least; so that lengthening or shortening the chain, in each experiment, would occasion different resistances; and the event, he says, proved accordingly. When he made the discharge with one wire only, he found the resistance to be less than when a chain was used. But to leave no room for doubt, he caused the chain to be stretched with a weight, that the links might be brought nearer into contact, and the event was the same as when a single wire had been used.*

Two circuits being made, one consisting of the arms of a man, and the other of the links of a chain; he found, that the fire would take the arms of the man; but that if the chain were stretched, it would take the chain. No person, he says, who has not made the experiment, would imagine, with how much force the chain must be stretched before the experiment will answer, and the electric fluid pass through it without producing a spark at any of the links; that is, before the links can be brought into absolute contact with one another, their own weight being by no means sufficient. †

MR. WILSON observed, that if one part of the Leyden phial was ground very thin, and covered with sealing wax till it was charged, and then had the sealing wax taken off, and a conduc-

* Letter to Hoadley. † Wilson and Hoadley, p. 65.

for communicating with the earth touched the thin part, the charge would be dissipated in nearly half the time that it otherwise would have been.*

HE observed that bodies, placed without the electric circuit, would be affected with the shock, if they were only in contact with any part of it, or very near it. To shew this to the most advantage, he set a charged phial upon a glass stand, and placed several pieces of brass upon the stand, one of them in contact with the chain that formed the circuit, and others a twentieth of an inch from it, or from one another; and, upon making the discharge, there was a spark visible between each of them.†

ANALOGOUS, in some respects, to this, was Mr. Wilson's observation, that if the circuit was not made of metals, or other very good conductors, the person who laid hold of them, in order to perform the experiment, felt a considerable shock in that arm which was in contact with the circuit.

HE also observed, that when the phial was coated, within and without, with metals, the first explosion bore the greatest proportion to the subsequent ones, the whole charge, being dissipated almost at once; whereas, when water was used, the subsequent explosions were more in number, and more considerable; and that when the phial was charged with nothing but a wire inserted in it, the first explosion and the subsequent ones were still more nearly equal.

MR. WILSON once happening to break a small wire by the convulsive shock given to his arms by the Leyden phial, he fastened to his hands, well guarded with leather, a larger wire, of the thickness of a slender knitting needle, and placed himself in such a manner, that it would necessarily be stretched, if his

* Wilson's Essay, p. 74.

† *Ib.* p. 90.

arm should be convulsed again. He accordingly discharged the phial, and this wire was broken, like the former.*

MR. GEORGE GRAHAM shewed how several circuits for the discharge of the Leyden phial might be made at the same time, and the fire be made to pass through them all. He made a number of persons take hold of a plate of metal, communicating with the outside of the phial; and all together, likewise, lay hold of a brass rod with which the discharge was made; when they were all shocked at the same time, and in the same degree.†

LASTLY Mr. Canton found, that if a charged phial was placed upon electrics, the wire and the coating would give a spark or two alternately; and that, by continuing this operation, the phial would be discharged.‡ This discovery, which is the first that I find recorded of this excellent philosopher, to whom the science of electricity owes so much, has a near affinity to the great discovery of Dr. Franklin; but he did not then observe, that those alternate sparks proceeded from the two contrary electricities. This history will furnish many more instances of persons being on the eve of great discoveries, without actually making them. Both Mr. Galath and Mr. Richman observed, in several cases, a stronger spark between two bodies, when both of them were electrified, than when only one of them was in that state; but neither of them suspected, that the electricities were of a different kind. ||

WE have seen what observations the English philosophers had made upon the Leyden experiment before the time of Dr. Franklin; let us now take a view of what was done by electricians in other parts of the world, within the same period.

* Wilson's Essay, p. 84. † Ib. p. 128. ‡ Ib. p. 64.

|| Wilcke's Preface to the German Version of Franklin's Letters.

As Mr. Muschenbroeck's letter to Mr. Reaumur, concerning the experiment of the phial, came at a time when many learned men were employed about electricity, the Abbé Nollet, and Mr. De Monnier, gentlemen of the academy, zealous to search into so an extraordinary a phenomenon, divesting themselves of the fear with which the professor's letter might justly have inspired them, made the experiment upon themselves, and, in like manner, said they found the commotion very terrible. The report of it instantly spread through the court and the city, from whence all ranks of men crowded to see this new kind of thunder, and to experience the effect of it. *

THE Abbé Nollet was the first who made experiments upon the phial in France, and the result of many of them was the same with what Dr. Watson had discovered, for which reason I shall not recite them here. They may all be seen at one view in his *Leçons de physique*, p. 481. The circumstances which the English philosophers had not attended to, are the following.

THE Abbé received a shock from a bottle out of which the air had been exhausted, and into which the end of his conductor had been inserted. This was an accidental discovery; for he received the shock as he was holding one hand to the glass vessel, in order to observe the beautiful irradiations of the electric light towards it in vacuo, and putting his other hand to the conductor, in order to adjust something about it. The blow he received was greater, he says, than he ever felt from the Leyden experiment in any other form. †

IN the same place he observes, that he never considered the water in the phial as of any use, but to convey the electric matter into the inside of the glass; and that he ascribed the force of

* Nollet's *Leçons de Physique*, p. 452.

† *Recherches*, p. 426.

the glass in giving a shock, to that property of it, whereby it retained it more strongly than conductors do, and was not so easily divested of it as they are.

MR. MONNIER is said, by Mr. Buffon, to have been the first who discovered that the Leyden phial would retain its electricity a considerable time after it was charged, and to have found it to do so for thirty six hours, in time of frost. He frequently electrified his phial at home, and brought it in his hand, through many streets, from the college of Harcourt to his apartments in the King's garden, without any considerable diminution of its efficacy.*

IN France as well as in Germany experiments were made to try how many persons might feel the shock of the same phial. The Abbé Nollet, whose name is famous in electricity, gave it to one hundred and eighty of the guards, in the King's presence; and at the grand convent of the Carthusians in Paris, the whole community formed a line of nine hundred toises, by means of iron wires between every two persons (which far exceeded the line of one hundred and eighty of the guards) and the whole company, upon the discharge of the phial, gave a sudden spring, at the same instant of time, and all felt the shock equally. †

MR. NOLLET also tried the effect of the electric shock upon two birds, one of which was a sparrow, and the other a chaffinch, which, as far as I can find, were the first brute animals of any kind that ever received it. The consequence was, that upon the first shock, they were both instantaneously struck motionless, and, as it were, lifeless, though for a time only; for they recovered some few minutes after. Upon the second shock, the sparrow was struck dead, and, upon examination, was found

* Phil. Transf. abridged. Vol. 10. p. 333. † Ib. p. 335.

livid without, as if it had been killed with a flash of lightning; most of the blood vessels in the body being burst by the shock. The chaffinch revived as before.* Fishes were also killed with the electric shock, by the Abbé, and others.

THE circumstance of the blood vessels of the sparrow being burst is, I imagine, a mistake. I have seen no such effect, when smaller animals have been killed by a shock fifty times as great as, it is probable, the Abbé used upon this occasion.

THE Abbé Nollet as well as Mr. Jallabert mentions the bursting of glass vessels by the electric explosion. They were pierced, he says, with round holes, three or four lines in diameter. †

It seems that the French philosophers, as well as the English, had observed, that, if the phial stood upon glass, it could not be charged, except a person's hand, or some other non-electric substance were brought near to it. Upon this they imagined the fire streamed out of the hand, and passed through the substance of the phial into the water. ‡ This fact surprised them very much, as it well might. They also observed, that a light body would be attracted by a charged phial, as it stood upon the table, if any person touched the wire; but they say that, if the phial itself were touched, the light body would be repelled with a force equal to its attraction in the former case. § They likewise found, that when the charged phial stood upon glass, it might be handled with all safety. ¶ These experiments seem not to have been made with proper circumspection: for by an attention to these very circumstances, Dr. Franklin was afterwards led to the great discovery of the different quality of the electricity, on different sides of the glass.

* Phil. Trans. abridged, Vol. 10. p. 336.

† Nollet's Lectures, Vol. 1. p. 42.

‡ Phil. Trans. abridged, Vol. 10. p. 334.

§ lb. p. 335. § lb. p. 337.

SECTION II.

THE METHODS USED BY THE FRENCH AND ENGLISH PHILOSOPHERS, TO MEASURE THE DISTANCE TO WHICH THE ELECTRIC SHOCK CAN BE CARRIED, AND THE VELOCITY WITH WHICH IT PASSES.

WE are now come to an ampler field of electrical experiments, in which we shall be spectators, not of what might be exhibited in a private room, and by a few operators; but where we shall find an amazing apparatus necessary, and a great number of assistants in the management of it; as well as the greatest judgment, and the most unwearied patience in the conduct of it.

THE French philosophers were the first to appear in this field, but they did little more than excite the English to go far beyond them in these great undertakings. It has been said already, that a circuit was made of nine hundred toises, consisting of men holding iron wires betwixt each two, through which the electric shock was sensibly felt. At another time, they made the shock pass through a wire two thousand toises in length, that is near a Paris league, or about two English miles and a half; though part of the wires dragged upon wet grass, went over charmil hedges,

hedges, or palifades, and over ground newly ploughed up. Into another chain they took the water of the basin in the Thuilleries, the surface of which was about an acre, and the phial was discharged through it.* Mr. Monnier, who made this experiment, mentioning the quantity of surface in the basin of water, as if it was of consequence to the experiment, and expressly saying it was *electrified*; Mr. Galath made several experiments, which prove, that bodies which form the circuit of the shock are not properly electrified. †

MR. MONNIER the younger, also endeavoured to determine the velocity of the electric matter; and for this purpose, made the shock pass through an iron wire of nine hundred and fifty toises in length, but he could not observe, that it spent a quarter of a second in passing it. He also found, that when a wire of one thousand three hundred and nineteen feet, with its extremities brought near together, was electrified; that the electricity ceased at one end, the moment it was taken off at the other. This fact refuted the opinion of those who maintained, that it was the force of the electrical *shock*, which threw the electric matter with so great velocity.

BUT all these attempts of the French would scarce deserve to have been mentioned, but that they preceded the greater, the more accurate, and the more numerous experiments of the English. The names of the English gentlemen, animated with a truly philosophical spirit, and who were indefatigable in this business, deserve to be transmitted to posterity in every work of this nature.

THE principal agent in this great scene was Dr. Watson. He planned and directed all the operations, and never failed to be

* Phil. Trans. abridged, Vol. 10. p. 336.

† Geschichte, Vol. 3. p. 557.

present

present at every experiment. His chief assistants were Martin Folkes Esq. president of the Royal Society, Lord Charles Cavendish, Dr. Bevis, Mr. Graham, Dr. Birch, Mr. Peter Daval, Mr. Trembley, Mr. Ellicott, Mr. Robins, and Mr. Short. Many other persons, and some of distinction, gave their attendance occasionally.

DR. WATSON, who wrote the history of their proceedings, in order to lay them before the Royal Society, begins with observing (what was verified in all their experiments) that the electric shock is not, strictly speaking, conducted in the shortest manner possible, unless the bodies through which it passes conduct equally well; for that, if they conduct unequally, the circuit is always formed through the best conductors, though the length of it be ever so great.

THE first attempt these gentlemen made, was to convey the electric shock across the river Thames, making use of the water of the river for one part of the chain of communication. This they accomplished on the 14th. and 18th. of July 1747, by fastening a wire all along Westminster bridge, at a considerable height above the water. One end of this wire communicated with the coating of a charged phial, the other being held by an observer, who, in his other hand, held an iron rod, which he dipped into the river. On the opposite side of the river, stood a gentleman, who, likewise, dipped an iron rod in the river, with one hand; and in the other, held a wire, the extremity of which might be brought into contact with the wire of the phial.

UPON making the discharge, the shock was felt by the observers on both sides the river, but more sensibly by those who were stationed on the same side with the machine; part of the electric fire having gone from the wire down the moist stones of the bridge,

bridge, thereby making several shorter circuits to the phial; but still all passing through the gentlemen who were stationed on the same side with the machine. This was, in a manner, demonstrated by some persons feeling a sensible shock in their arms and feet, who only happened to touch the wire, at the time of one of the discharges, when they were standing upon wet steps which led to the river. In one of the discharges made upon this occasion, spirits were kindled by the fire which had gone through the river.*

UPON this, and the subsequent occasions, the gentlemen made use of wires, in preference to chains, for this, among other reasons, that the electricity which was conducted by chains was not so strong, as that which was conducted by wires. This, as they well observed, was occasioned by the junctures of the links not being sufficiently close, as appeared by the snapping and flashing at every juncture, where there was the least separation. These lesser snappings, being numerous in the whole length of a chain, very sensibly lessened the great discharge at the gun barrel.

THEIR next attempt was to force the electrical shock to make a circuit of two miles, at the new river at Stoke Newington. This they performed on the 24th. of July 1747, at two places; at one of which the distance by land was eight hundred feet, and by water two thousand: in the other, the distance by land was two thousand eight hundred feet, and by water eight thousand. The disposition of the apparatus was similar to what they before used at Westminster bridge, and the effect answered their utmost expectations. But, as in both cases, the observers at both extremities of the chain, which terminated in the water, felt

* Phil. Trans. abridged, Vol. 10, p. 349, &c.

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the shock, as well when they stood with their rods fixed into the earth twenty feet from the water, as when they were put into the river; it occasioned a doubt, whether the electric circuit was formed through the windings of the river, or a much shorter way, by the ground of the meadow: for the experiment plainly shewed, that the meadow ground, with the grafs on it, conducted the electricity very well.

By subsequent experiments, they were fully convinced, that the electricity had not, in this case, been conveyed by the water of the river, which was two miles in length, but by land, where the distance was only one mile; in which space, however, the electric matter must necessarily have passed over the new river twice, have gone through several gravel pits, and a large stubble field.*

JULY 28th. they repeated the experiment, at the same place, with the following variation of circumstances. The iron wire was, in its whole length, supported by dry sticks, and the observers stood upon original electrics; the effect of which was, that they felt the shock much more sensibly, than when the conducting wire had lain upon the ground, and when the observers had likewise stood upon the ground, as in the former experiment.

AFTERWARDS, every thing else remaining as before, the observers were directed, instead of dipping their rods into the water, to put them into the ground, each one hundred and fifty feet from the water. They were both smartly struck, though they were distant from each other above five hundred feet. †

THE same gentlemen, pleased with the success of their former experiments, undertook another, the object of which was, to

* Phil. Trans. abridged, Vol. 10. p. 360.

† Ib. p. 357.

determine,

determine, whether the electric virtue could be conveyed through dry ground; and, at the same time, to carry it through water to a greater distance than they had done before. For this purpose, they pitched upon Highbury barn beyond Islington, where they carried it into execution on the 5th. of August 1747. They chose a station for their machine, almost equally distant from two other stations for observers upon the new river; which were somewhat more than a mile asunder by land, and two miles by water. They had found the streets of London, when dry, to conduct very strongly, for about forty yards; and the dry road at Newington about the same distance. The event of this trial answered their expectations. The electric fire made the circuit of the water, when both the wires and the observers were supported upon original electrics, and the rods dipped into the river. They also both felt the shock, when one of the observers was placed in a dry gravelly pit, about three hundred yards nearer the machine than the former station, and one hundred yards distant from the river: from which the gentlemen were satisfied, that the dry gravelly ground had conducted the electricity as strongly as water.

FROM the shocks which the observers received in their bodies, when the electric power was conducted upon dry sticks, they were of opinion; that, from the difference of distance simply considered, the force of the shock, as far as they had yet experienced, was very little, if at all impaired. When the observers stood upon electrics, and touched the water, or the ground, with the iron rods, the shock was always felt in their arms or wrists; when they stood upon the ground with their iron rods, they felt the shock in their elbows, wrists, and ancles; and when they stood upon the ground without rods, the shock was

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always

always felt in the elbow and wrist of that hand which held the conducting wire, and in both ancles. *

THE last attempt of this kind which these gentlemen made, and which required all their sagacity and address in the conduct of it, was to try whether the electric shock was perceptible at twice the distance to which they had before carried it, in ground perfectly dry, and where no water was near; and also to distinguish, if possible; the respective velocity of electricity and sound.

For this purpose, they fixed upon Shooter's hill, and made their first experiment on the 14th. of August 1747, a time, when, as it happened, but one shower of rain had fallen during five preceding weeks. The wire communicating with the iron rod, which made the discharge, was six thousand seven hundred and thirty two feet in length, and was supported all the way upon baked sticks; as was also the wire which communicated with the coating of the phial, which was three thousand eight hundred and sixty eight feet long, and the observers were distant from each other two miles. The result of the explosion demonstrated, to the satisfaction of the gentlemen present, that the circuit performed by the electric matter was four miles, viz. two miles of wire, and two of dry ground, the space between the extremities of the wires. A distance which, without trial, as they justly observed, was too great to be credited. A gun was discharged at the instant of the explosion, and the observers had stop watches in their hands, to note the moment when they felt the shock: but, as far as they could distinguish, the time in which the electric matter performed that vast circuit might have been instantaneous. †

IN all the explosions where the circuit was made of any confi-

* Phil. Trans. abridged, Vol. 10. p. 360.

† Ib. p. 363.

derable

derable length, it was observed, that though the phial was very well charged, yet that the snap at the gun barrel, made by the explosion, was not near so loud as when the circuit was formed in a room; so that a by-stander, says Dr. Watson, though versed in those operations, would not imagine, from seeing the flash, and hearing the report, that the stroke, at the extremity of the conducting wire, could have been considerable; the contrary whereof, when the wires were properly managed, he says, always happened.

STILL the gentlemen, unwearied in these pursuits, were desirous, if possible, to ascertain the absolute velocity of electricity at a certain distance; because though, in the last experiment, the time of it's progress was certainly very small, if any; they were desirous of knowing, small as that time might be, whether it was measurable; and Dr. Watson had contrived an excellent method for that purpose.

ACCORDINGLY, on the 5th. of August 1748, the gentlemen met once more, and the last time, at Shooter's hill; when it was agreed to make an electric circuit of two miles, by several turnings of the wire, in the same field. The middle of this circuit, they contrived to be in the same room with the machine, where an observer took in each hand one of the extremities of the wires, each of which was a mile in length. In this excellent disposition of the apparatus, in which the time between the explosion and the shock might have been observed to the greatest exactness, the phial was discharged several times; but the observer always felt himself shocked at the very instant of making the explosion. Upon this the gentlemen were fully satisfied, that, through the whole length of this wire, which was 12276 feet in length, the velocity of the electric matter was instantaneous.*

* Phil. Trans, abridged, Vol. 10. p. 368.

THESE experiments excited the admiration of all foreign electricians. Professor Muschenbroeck, who was greatly satisfied with the extent and success of them, said, in a letter to Dr. Watson, upon the occasion, *Magnificentissimis tuis experimentis superasti conatus omnium.*

IT is said by some, that the last of these experiments go upon a wrong supposition, and therefore can be of no use; it being supposed that the very same particles of the electric fluid, which were thrown on one side of the charged glass, actually made the whole circuit of the intervening conductors, and arrived at the opposite side: whereas Dr. Franklin's theory only requires that the deficiency on one side of the glass be supplied from the neighbouring conductors; which may, in return, receive as much as they parted with, from the side of the glass that was overcharged. So that, to be a little more particular, the redundancy of electric matter on the charged side of a pane of glass, only passes into the bodies which form that part of the circuit which is contiguous to it, driving forward that part of the fluid which was natural to them; till, at length, the fluid which resided in those conductors which formed the last part of the circuit, passes into the exhausted side of the glass.

BUT should this be the case (though in great discharges it supposes the natural quantity of electricity in bodies to be very considerable) and should Dr. Watson, and other philosophers at that time, have conceived otherwise; it does not follow, that the experiments could possibly determine *nothing*: for there still remains something to be measured, viz. the time required for the successive dislodging the electric fluid in the whole length of the circuit.

WERE the whole mass of the electric matter contained in all the intervening conductors absolutely solid, no motion could be made

made at one extremity, without producing an instantaneous motion at the other; just as if one end of a rod be struck, the motion is instantly communicated to the other end. But this cannot be the case in an elastic medium, the parts of which yield to one another. In this case, the motion is communicated in a real succession, like a vibration, running the whole length of the circuit; which must therefore take up time, and be measurable. The motion of sound may be measured, though no particle of the vibrating air be finally displaced. These great experiments of Dr Watson, therefore, had a real object, only it appeared to be too small to be ascertained by them.

SECTION

SECTION III.

MISCELLANEOUS DISCOVERIES OF DR. WATSON, AND OTHERS,
TILL THE TIME OF DR. FRANKLIN;

THE first of these discoveries in order of time, and in importance second to none (except that of the shock itself, and Dr. Franklin's discovery of the different electricity of the opposite sides of the charged glass) was that of Dr. Watson, proving, that the glass tubes and globes did not contain the electric power in themselves, but only served as first movers, and determiners, as he calls it, of that power.

He was first led to this discovery by observing, that, upon rubbing the glass tube, while he was standing upon cakes of wax (in order, as he expected, to prevent any of the electric power from discharging itself through his body upon the floor) the power was, contrary to his expectation, so much lessened, that no snapping could be observed upon another person's touching any part of his body; but that if a person not electrified held his hand near the tube, while it was rubbed, the snapping was very sensible.*

THE event was the same when the globe was whirled in similar circumstances. For if the man who turned the wheel, and who, together with the machine was suspended upon silk, touched the floor with one foot, the electric fire appeared upon the

* Phil. Trans. abridged, Vol. 10. p. 303.

conductor;

conductor; but if he kept himself free from any communication with the floor, no fire was produced.

DR. WATSON by this, and the following experiments in conjunction, discovered, what he calls, the complete circulation of the electric matter. He observed, that only a spark or two would issue from his hand to the insulated machine, unless he, at the same time, formed a communication between the conductor and the floor; but that then there was a constant and copious flux of the electric matter to the machine.

OBSERVING, that while his hand was in contact with the conductor, the man who turned this insulated machine gave sparks, which would fire inflammable substances, and perform other electrical experiments which were usually performed at the conductor; he naturally imagined, that the fire issued from the man, for the very same reason that all electricians had before imagined that it came from the conductor; and seeing that the man gave no fire unless there was a communication between the floor and the conductor, he concluded that, in this case, the fire was supplied by that communication, so that the course of the electricity was inverted, as he expresses it.*

It was not then suspected, that the eye could not distinguish in what direction an electric spark proceeded. Electricians naturally imagined that all electric powers, and consequently the electric fluid, which they supposed to be the cause of these powers, existed in the excited electric, whatever it was; and that whatever powers were exerted by electrified bodies proceeded from a real communication of electric matter to them. Accordingly, when Dr. Watson found that, by cutting off the communication of the electric with the floor, all electrical operations

* Phil. Trans. abridged, Vol. 10. p. 305.

were

were stopped, he concluded, that the electric fluid was collected from the floor to the rubber, and thence conveyed to the globe. For the same reason, seeing the rubber, or the man who had a communication with it, give no sparks but when the conductor was connected with the floor, he would as naturally conclude that the globe was supplied from the conductor, as he had before concluded that it was supplied from the rubber.

COMPARING both these experiments together Dr. Watson was led to infer, that, in all electrical operations, there was both an *afflux* of electric matter to the globe, and the conductor, and likewise an *efflux* of the same electric matter from them. *

FINDING that a piece of leaf silver was suspended between a plate electrified by the conductor, and another communicating with the floor, he reasons from it in the following manner. "No body can be suspended in equilibrio but by the joint action of two different directions of power: so here, the blast of electric ether from the excited plate blows the silver towards the plate unexcited, and this last, in its turn, by the blast of electric ether from the floor setting through it, drives the silver towards the plate electrified. We find from hence, likewise, that the draught of electric ether from the floor is always in proportion to the quantity thrown by the globe over the gun barrel, or the equilibrium by which the silver is suspended could not be maintained." †

DR. WATSON observes, that the Abbé Nollet, two years before he made this communication, had given it as his opinion (though without any experiment which proved it) that the electric matter did not only proceed from the electrified bodies, but from all others about them, to a certain distance. ‡

* Phil. Trans. abridged, Vol. 10 p. 311. † Ib. p. 310. ‡ Ib. p. 315.

SOMETIME after this, Dr. Watson observes, in a paper read at the Royal Society, January 21st. 1748, that Dr. Bevis had carried his experiment, to prove that rubbing the tube or the globe, only conveyed, and did not produce the electric matter, further than he had done. For he had observed, above a year before, that placing one man upon electrics, to rub the tube or globe, and another also upon electrics to touch them, as the conductor; both the man who rubbed, and the man who touched the excited glass would give a spark; and further, that if they touched one another, the snapping was much greater than if either of them touched a person standing upon the floor. Upon this the Doctor seems to have corrected his former opinion of the afflux and efflux of electric matter: for he accounts for this fact by supposing, that as much electricity as was taken from the person rubbing was given to him who touched the conductor, being conveyed by the globe. By this means the electricity of the former of these persons, he observes, was more rare than it naturally was, and that of the latter more dense; so that the density of electricity between these two persons differed more than that between either of them, and another person standing upon the floor. In this manner did Dr. Watson discover, what Dr. Franklin observed, about the same time, in America, and called the *plus* and *minus* in electricity.*

DR. WATSON observed that the flame at the end of an electrified wire was sensible to the hand, as a cool blast of wind, and that when light substances were attracted and repelled between an electrified plate and one communicating with the floor, the succession of these alternate attractions and repulsions was extremely quick, so that sometimes the eye could hardly keep pace

* Phil. Trans. abridged, Vol. 10. p. 369.

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with it; and that when a glass globe, of about an inch in diameter, very light and finely blown, was put into a plate of metal, and another plate hung on the conductor over it, the strokes from the alternate attractions and repulsions were almost too quick for the ear. From this last experiment he likewise deduced an argument to prove the extreme velocity with which they were attracted and repelled. He says, that if they were let fall from the height of six feet or more upon a wooden floor, or even a plate of metal, they were rarely broken; but that by the attraction and repulsion of them between these plates, though at the distance of no more than one sixth of an inch, they were frequently beaten to pieces.*

THE DOCTOR also proved, that the electric matter passed through the substances of the metal of communication, and not over the surface of it, by covering a wire with a mixture of wax and rosin, and discharging a phial through it.

MR. MONNIER the younger discovered, that electricity is not communicated to homogeneous bodies in proportion to their masses or quantity of matter, but rather in proportion to their surfaces; and yet that all equal surfaces do not receive equal quantities of electricity, but that those receive the most which are most extended in length; that a square sheet of lead, for instance, received a much less quantity of electricity than a small strip of the same metal with a surface equal to that of a square sheet. †

MR. WILSON, whose curious observations on the Leyden phial have been mentioned in a former section, claims no small share of honour in this. As early as the latter end of the year 1746, he made the same discovery that Dr. Watson had done, that the electric fluid did not come from the globe, but from

* Phil. Transf. abridged. Vol. 10. p. 309. † Ib. p. 338.

the earth itself, and from all other non-electric bodies about the apparatus. He suggested a method of proving this in a letter to Mr. Ellicott from Chester; and mentions his having completed the experiment himself soon after, in a letter to Mr. Smeaton, from Dublin.

HAVING conceived that the difference between electric and non-electric bodies was owing to the different resistance which a subtle medium, as he calls it, on the surfaces of all bodies gave to the passage of the electric fluid; and conceiving that heat would rarify this medium, and thereby convert electrics into non-electrics, he made some experiments which confirmed him in that supposition. He found that one person might communicate electricity to another, notwithstanding the intervention of a considerable quantity of red hot glass. He also made other experiments of a similar nature, as discharging phials by means of hot glass, hot amber, and various other heated electrics. These, however, as Mr. Canton afterwards observed, might be owing to the hot air upon the surfaces of those bodies, which he found to transmit electricity very well. But another experiment, which Mr. Wilson made upon melted rosin, does not seem liable to that objection. He poured the melted rosin into a phial, and found that he could give shocks with it; but he observed, that these shocks diminished as the rosin grew cold, and that when it was quite cold, they entirely ceased.*

MR. WILSON mentions a curious experiment (of which, however, he does not say that he was the inventor) which he made with paper vanes stuck in a cork, and suspended by a magnet. These, he says, if they were brought near the point of any body proceeding from the prime conductor, would turn

* Wilson's Essay, p. 143.

round very swiftly, but would not turn at all in vacuo. This blast he thought was occasioned by the issuing of the electric matter out of the point, which caused a current in the air; but he did not try what would be the consequence of presenting the vanes to a point which received the electric fluid.*

LASTLY, Mr. Wilson observed, that if a needle were presented to a piece of down hanging to the conductor, it would cling close to it; but that, upon presenting any thing that was blunt, it would be repelled again; and says that Mr. Canton made several curious experiments of the same kind. †

MR. SMEATON, within this period, observed, that if a man who was insulated pressed against the globe with the flat part of his hand, while another person, standing on the floor, did the same, in order to excite it, the person who was insulated would hardly be electrified at all; but that, if he only laid his fingers lightly on the globe, he would be electrified very strongly. ‡ The same ingenious person also observed, that upon heating the middle of a large bar of iron to a glowing heat, and electrifying it, the electric power of the part that was heated was as strong as that of the cold part. ||

For several curious discoveries relating to electricity, made within this period, we are indebted to the ingenious Dr. Miles. In a paper read at the Royal Society January 25th. 1746, he says that having excited a stick of black sealing wax with white and brown paper, or clean dried flannel, he was able to kindle common lamp spirits with it. Comparing the stick of wax with the glass tube, he observed a remarkable difference between the appearance of fire from both, though he did not understand the reason of it. He says he found the luminous effluvia to proceed

* Wilson's Essay, p. 141. † Ib. p. 153. ‡ Ib. p. 24. || Ib. p. 129.

in a much greater quantity from the top of his finger to the stick of wax, than they did to the glass. He several times observed a small globular spot of fire to appear first on his finger, from which issued regular streams towards the wax, in the form of a comet's tail. This is now well known to be the constant appearance of the electric fire between an unelectrified body and an electric excited negatively. *

DR. MILES found a stick of sulphur to perform very well; but not at all, when he had put an iron rammer in the center of it, to strengthen it. It is remarkable, that after setting this stick upright in a cupboard, it lost all its electric virtue, and could never afterwards be excited in the least degree. This effect the Doctor attributed to its being put up without any cover.

DR. MILES also mentions his having got a tube of green glass, which he could never excite but with great difficulty, and then but to a small degree. †

THE same ingenious gentleman, some time after, made an experiment upon pieces of leaf brass in a bottle hermetically sealed. To these he found he could give motion by the approach of the excited tube, in the same manner as if they had been in the open air; but one appearance struck him of which he by no means gives a satisfactory account. He observed that when he removed the tube from the exhausted glass slowly, no commotion was seen in the leaf brass, but a very brisk one upon removing it suddenly. Indeed this fact could not have been understood but by comparing it with other facts depending upon the same principle, and which were not discovered till some years after. ‡

FROM England, to which, as an Englishman, I would give the preference only in matters of absolute indifference, I pass

* Wilson's Essay, p. 317. † Phil. Trans. abridged, Vol. 10. p. 320. ‡ Ib. p. 326.

over to France, where, next to those made in England, the most important discoveries, and the greatest number of them were made in the period of which I am treating. And, without all dispute, the greatest name in France, in this or any other period, except that of Mr. Du Faye, his friend and associate, is that of the Abbé Nollet.

THE favourite observation of Mr. Nollet, on which he built his darling theory of affluences and effluences was, that bodies not insulated, plunged in electric atmospheres, shewed signs of electricity. He observed a sensible blast from the hand of a person not electrified, in the abovementioned circumstances, also the attraction and repulsion of light bodies by them, the appearance of flame, the diminution of their weight by increased evaporation and perspiration, and almost every other appearance and effect of electricity. Moreover observing that this globe contracted a foulness while it was whirling, even when rubbed with a clean hand, he had the curiosity to collect a quantity of the matter which formed that foulness; and finding that, when it was put into the fire, it had the smell of burnt hair, he concluded that it was an animal substance; and that it had been carried by the affluent electricity from his own body to the globe.*

THE only mistake of this ingenious philosopher in these experiments, and which was the source of many others, which, in the end, greatly bewildered and perplexed him was, that the electricity of the body, which was plunged in the atmosphere of an electrified body, was of the same nature with that of the electrified body. Had he but preserved the distinction, which Mr. Du Faye had discovered, between the two electricities, and imagined that the body electrified, and that which was plunged in its atmosphere were possessed of these two different and oppo-

* Nollet's Recherches, p. 142.

five electricities, he might have been led to the great discoveries made by Mr. Canton, Dr. Franklin, and Mr. Wilcke; which, we shall find, arose from that single observation; and he would have avoided a great deal of debate and contention, which has not ended to his advantage.

THIS partial discovery of Mr. Nollet is by no means the only one of his, that the history of electricity presents in this period. He made several experiments on pointed bodies, and observed, that those which had the smallest points soonest threw out brushes of electric light, but did not show other signs of electricity so strong as bodies that were not pointed.*

He took a great deal of pains in making experiments, in order to determine the degree in which different substances conducted the electric fluid; and found that the smoak of gum lac, turpentine, karabé, and sulphur did not carry away the electricity of an excited tube so soon as the smoak of linen, wood, and more especially the steam of water, and the effluvia of burning tallow, and of other fatty substances. In short, he found, that vapours which were not watery did very little, or no injury to electrical experiments, provided the tube was not exposed to them near the fire which caused them. A smoky room did not prevent his performing experiments, at least in any great degree; nor were odoriferous effluvia at all prejudicial to them. †

SEVERAL curious observations were made by the Abbé upon heat, and heated bodies. He found, that a piece of iron glowing hot, so as to throw off ignited particles, did not leave the smallest trace of electricity in an excited tube, to which it had been brought within five or six inches, and only held there two or three seconds; but it ceased to affect the tube at the same

* Recherches, p. 146. † Ib. p. 194, &c.

distance

distance before it ceased to be red, and had no influence at all long before it was cold. The electricity of the tube, in this instance, was probably conveyed through the air heated by the iron; as it can hardly be supposed, that the iron emitted any effluvia capable of producing that effect. *

He found that the excited tube lost nothing of its electricity in the focus of a burning mirror. That the flame of a candle, or the near approach of it, would destroy electricity had been known before: he observed, that the flame was sensibly disturbed by the approach of the excited tube, and he mentions Mr. Du Tour, and the Abbé Needham's having found, that the interposition of the thinnest piece of glass, or of any other substance, between the candle and the tube prevented the dissipation of the electricity. From this fact it was inferred, that the dissipation was owing to some effluvia proceeding from the candle. †

CONTINUING his observations on what increased or impeded electrical experiments, he found, that a light body, placed on a non-electric stand, moved more briskly upon the approach of an electrified body, than when it was placed upon an electric stand. ‡ Several electrical experiments, he observed, succeeded best when there was a number of spectators present, and when they drew near, and stood close together to see his experiments; provided they did not occasion so great a perspiration as made his glasses moist. § This observation we shall find accounted for hereafter by Mr. Wilcke.

THE Abbé moistened, with water or spirit of wine, a slender and pointed bar of iron, and thought that the blast from the point of it was more sensible than when it was not moistened; which

* Recherches, p. 216. † Ib. p. 219. ‡ Ib. p. 222. § Ib. p. 123.

he attributed to the electric fluid carrying away with it some of the particles of the water, and of the spirit of wine. *

SOME few observations the Abbé made on the difference between excited and communicated electricity, and between the electricity of glass and that of sulphur. He observed, that the electricity of an excited globe or tube, caused an odd sensation upon the face, as if a spider's web were drawn over it; whereas that effect was seldom produced by communicated electricity. Excited electricity, he also says, might be perceived by the smell, at more than a foot distance, when communicated electricity could not. †

HE melted sulphur in a glass globe, by turning it over a chafing dish of burning coals; when he observed, that small pieces of sulphur, before they were melted, were attracted and repelled by the glass within, at the same time that the ashes of the coals were attracted without. ‡ Holding a piece of excited sulphur in one hand, with a piece of down sticking to it, and ready to fly off, the down, he says, would cling fast to the sulphur, upon presenting to it an excited glass tube, which he held in his other hand. ||

I SHALL, in the last place, recite the Abbé Nollet's experiments made in vacuo. He found that glass, and other electrics, might be excited in vacuo, but not so strongly as in the open air. § He observed that there was a remarkable difference between the appearance of the electric light in vacuo, and in the open air; being much more diffuse, and unbroken in vacuo. * Inserting the extremity of his conductor into an exhausted glass vessel, he observed the vessel to be full of light, whenever he brought his hand to it; that the light was considerably increased

* Recherches, p. 140. † Ib. p. 136. ‡ Ib. p. 184. || Ib. p. 124. § Ib. p. 236.

* Ib. p. 243.

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when he spread his hand over it; and that when a spark was taken from the conductor, the whole vessel seemed to be full of light. He also observed, that small pieces of metal, inclosed in the vessel, adhered close to the glass; but detached themselves from it, on the approach of the finger, or of any conductor on the outside.

THERE are a few other names of electricians in France, whose experiments and observations, made within this period, deserve to be mentioned. Of these is Mr. Boulanger. He took great pains to determine the degree in which different substances are capable of being excited. The experiments, he says, were made with the greatest care: and though the state of the science did not admit of this business being determined with great accuracy, it may not be disagreeable to see the result of them; which he has comprised in the following table, beginning with those that are least excitable in every column.

FIRST COLUMN.

Ebony.

Guaiacum.

Box wood.

Sandal wood.

Oak.

Elm.

Ash.

Linden tree.

Rose.

Willow.

Ozier.

Cork.

Dry wood of all kinds.

All dry plants.

SECOND COLUMN.

Shells of all kinds.

Whalebone.

Bones.

Ivory.

Horn.

Scales.

Parchment.

Hair

Hair.	Marble of all colours.
Wool.	Slate.
Feathers.	Free stone.
Cotton.	Granite.
Silk.	Porphyry.

THIRD COLUMN.

Allum.	Varnished earth.
Sugar Candy.	Cornelians.
The Phosphorus of Berne.	Agates.
Yellow and white wax.	All opaque precious stones.
Japan varnish.	Porcelaine.

FIFTH COLUMN.

Sandarac.	Hyacinth.
Mastic.	Opal.
Amber.	Emerald.
Jet.	Amethyst.
Pitch.	Topaz.
Gum copal.	Ruby.
Gum lac.	Sapphire.
Colophonia.	Cat's eye.
Sulphur.	Peridot.
Sealing wax.	Granite.
All salts which have sufficient consistence.	Rock chrystal.
All resins.	Venice and Muscovy talk.
	Coloured diamonds, especially yellow.

FOURTH COLUMN.

Loadstone.	White diamonds, especially the brilliant.
Hand stone.	

All transparent precious stones, without excepting those
 Glafs, and all vitrifications, of metals.

THE inference which this author draws from this catalogue is, that the most brittle, and the most transparent substances, are always the most electric; and he has recourse to an awkward hypothesis to account for the marcasites not being excitable at all, notwithstanding they are both brittle and transparent. He says it is owing to condensed air contained in those substances, which is known to prevent excitation.*

THE same author says, that mineral waters are much more sensibly affected with electricity than common water; that black ribbons are much sooner attracted than those of other colours; and, next to them, the brown, and deep red. †

MR. LE CAT, a physician at Rouen, who has distinguished himself by several performances in the learned world, suspended several pieces of leaf gold at his conductor, and observed that they hung at different distances, according to their sizes, the smaller pieces placing themselves nearer the conductor, and the larger receding farther from it. This he compares to the distances at which the planets make their revolutions round the sun, and he supposed the cause to be the same in both. The same author very particularly compares the electric shock, which had just been discovered, to thunder. ‡

GERMANY affords but few articles for the electrical history of this period; one of them, however, is curious, and well deserves to be transmitted to posterity. Mr. Gordon of Erford excited

* Boulanger, p. 74. † Ib. p. 124. ‡ Histoire. p. 84--85.

the electricity of a cat so strongly, that, when it was communicated by iron chains, it fired spirits of wine. *

It has been mentioned before, that several gentlemen in Germany, as well as in England, had found, that if the man who rubbed the globe stood upon electrics, sparks were perceived upon touching him; but Messrs. Klingstierna and Stroems, two German professors, were the first who properly electrified by the rubber; and their experiments were published in the acts of the Royal Academy of sciences at Stockholm for the year 1747. †

Mr. JALLABERT, professor of philosophy at Geneva, found that a coating of pitch did not prevent the conductor from being electrified, which proved that the electric fluid enters the substance of metals. He also proved, that ice was a conductor of electricity, by making the Leyden experiment with a bottle in which water was frozen. ‡

THE amazing and extensive effects of electricity now began to make philosophers look for it where it had not been suspected before. The first account that is given of wollen garments being observed to exhibit signs of electricity, when they were put off, after the flashes of light they gave were known to be owing to electricity, was sent to the Royal Society by Mr. Coke of the Isle of Wight, who says, that a lady of his acquaintance observed it; and that it was also at last found, that it was only new flannel, and after some time wearing, which gave that appearance, and that this property was lost when it was washed. ||

THE same appearance, he observes, upon another occasion, was most conspicuous in frosty weather; in which season he takes notice, that there is generally, not only a greater purity of the

* Nollet's Recherches, p. 58. † Wilcke, p. 112. ‡ Histoire, p. 95, 96.

§ Phil. Trans. abridged, Vol. 10. p. 343.

air, and absence of moisture, but that all hairy and horny substances (for hairs, as he says, are only small horns) are more elastic, and consequently susceptible of, and more capable of exciting strong vibrations. He says, that the flannel being rendered damp with sea water, and afterwards dry, would heighten the electric appearances.*

BUT though this was the first appearance of the kind that was observed, after it was known to arise from electricity; similar appearances had been several times noted before. Bartholin, who flourished in 1650, wrote a book *De luce animalium*, in which he supposes, that unctuous effluvia had a great share in those appearances. The same writer says, that Theodore Beza might be seen by a light proceeding from his eye brows; and that sparks would flash from the body of Charles Gonzaga, Duke of Mantua, upon being gently rubbed. But he does not say whether he had any particular hairy, or scaly superficies to his skin. †

DR. SIMPSON, who published a philosophical discourse on fermentation, dedicated to the Royal Society in 1675, also takes notice of the light proceeding from animals on friction, or peccation as he calls it, and instances in the combing of a woman's head, the currying of a horse, and the stroaking of a cat's back. ‡

MR. CLAYTON also, in a letter to Mr. Boyle, dated June 23d. 1684, at James town in Virginia, gives him an account of a strange accident, as he calls it, which happened to one Mrs. Sewall, whose wearing apparel emitted a flashing of sparks, which were seen by several persons. The like happened to Lady Baltimore her mother in law. ||

I SHALL conclude this section with what I can find, in this

* Phil. Trans. abridged, Vol. 10. p. 344. † Ib. p. 344. ‡ Ib. p. 279. || Ib. p. 272.
period,

period, about increasing the power of electricity, and measuring its effects.

MR. MONNIER the younger, whose name has been frequently mentioned in the course of this history, used glass spheroids instead of globes, and endeavoured to increase his electrical power by using several of these spheroids at a time; but he found, upon trial, that they did not answer his expectations; and was thence disposed to conclude, that there might be a *ne plus ultra* in the intensity of electricity, as well as in the heat communicated to boiling water. *

THE power of glass in electrifying being found to be so great, it is no wonder that philosophers should endeavour to find what kind of glass was capable of being excited to the greatest degree. Among other proposals we find a very memorable one communicated to the Royal Society, April 6th. 1749, by Mr. George Mathias Bose, of Wittemburgh. He says, that a glass ball which has often been employed in violent distillations, and other chymical operations sends forth electricity incomparably more strong, than any glass which had never been exposed to so violent a fire. This article is the more curious, as it shows us how much philosophers at this time piqued themselves upon discoveries in electricity. He asserts his being the first person who ever mentioned this notable circumstance, as he calls it, and desires Dr. Watson, to whom he communicated it, to let him have the honour of that improvement in the Philosophical Transactions. †

IT was within this period that Dr. Watson contrived to improve the strength of electricity by moistening the rubber of his globe, though he was not aware of all the reasons for it. He

* Phil. Transf. abridged, Vol. 10. p. 330. † Ib. 329.

observed

observed that the man who stood on the floor, to excite the globe by his hand, did it more strongly than a cushion. This, he says, he could not conceive to be owing to any other difference, than to his hand being more moist, and consequently more readily conducting the electricity from the floor; wherefore he ordered his machine, and even his cushion, to be made damp; and then found that the electricity was as strong as when the globe was rubbed by the hand.*

A GENTLEMAN at Chartres in France, greatly increased the effects of electricity by means of moisture, for asserting which he is very much ridiculed by the author of *Histoire de l'Electricite*.

MR. WILSON says, that if the cushion (which he made of leather) was gilt with silver, brass, or copper, it would do very well; and that the silk line on which the conductor hung should be red or yellow. † The table, he says, should stand on moist ground, or a wire pass from the machine to the moist ground. ‡

DR. WATSON also found, that though no electricity could be produced by rubbing the globe with original electrics perfectly dry, yet that they answered very well when they had been made moist; the water imbibed by those substances serving as a canal of communication to the electricity between the hand, or the cushion, and the globe; in the same manner as the air, replete with vapours in damp weather, prevents the accumulation of the electric matter in any considerable degree, by conducting it as fast as excited to the nearest non-electrics. He observed on the contrary, that most vegetable substances, though made as dry as possible, furnished electricity, though in small quantities. He excited electricity not only from linen, cotton, &c. but even from sheet lead and a deal board. ||

* Phil. Trans. abridged, Vol. 10. p. 312. † Wilson's Essay, p. 5, 6. ‡ Ib. p. 8.
 || Ib. p. 380.

THE Abbé Nollet says, that he found oil of turpentine upon a piece of woollen cloth excited glass very powerfully, but that the least water mixed with it prevented the excitation. *

MR. BOULANGER says, that if two cylinders be made of the same kind of glass, and of the same fashion, one of them transparent, and the other tinged with any colour, the transparent cylinder will be excited more easily than the coloured one. † He acknowledges, however, that sometimes the most transparent, and the most brittle glass is capable of acquiring but little electricity. ‡ In another place he says, that a cylinder of three or four lines in thickness will acquire a stronger, and a more lasting electricity than a cylinder of one line thick. || He also says, that one person's two hands, or one cushion is better than more. §

ABOUT the same time that Dr. Watson made his first experiments upon the Leyden phial, Mr. Canton discovered a method by which the quantity of electricity accumulated in the phial might be measured to a good degree of exactness. He took the charged phial in his hand, and made it give a spark to an insulated conductor, which spark he took off with his other hand. This operation he repeated till the whole was discharged, and he estimated the height of the charge by the number of the sparks. This is a pretty certain and exact method of knowing how high a phial *has been* charged: but what electricians chiefly want is a method of ascertaining how high a phial *is* charged, or the exact force of the charge while it is contained in the glass.

SOMETHING of this kind was done by Mr. Ellicott, in the same year 1746. He proposed to estimate the strength of common electrification, by its power to raise a weight in one scale of a balance, while the other should be held over the electrified bo-

* Recherches, p. 186. † Boulanger, p. 64. ‡ Ib. p. 64. || Ib. p. 135. § Ib. p. 136.
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dy, and pulled to it by its attractive power.* Mr. Galath also constructed an electrometer upon the same principle. †

THE Abbé Nollet applied the threads that Mr. Grey and Du Faye had used in electrical experiments, to show the degree of electricity. He hung two of them together, and observed the angle of their divergence, by means of the rays of the sun, or the light of a candle, and their shadow upon a board placed behind them. Mr. Waitz also thought of the same kind of electrometer, with this improvement, that he loaded the ends of the threads with small weights. ‡

* Boulanger, p. 324. † Dantzick Memoirs, Vol. 1. p. 525. ‡ Histoire, p. 58.

SECTION IV.

EXPERIMENTS ON ANIMAL, AND OTHER ORGANIZED BODIES
IN THIS PERIOD; AND OTHER EXPERIMENTS CONNECTED
WITH THEM, MADE CHIEFLY BY THE ABBE NOLLET.

HITHERTO the effect of electricity upon human bodies had not been attended to, farther than the mere shock of the Leyden phial. But we shall now see a curious set of experiments on this subject exhibited by the Abbé Nollet. The English philosophers, who led the way in almost every other application of electricity, were among the last to try its effects upon animals, and other organized bodies. The only article that I can find upon this subject, before the discoveries of the Abbé Nollet, is one of Mr. Trembley's; who says that several persons had observed, that while they were electrified, their pulse beat a little faster than before. He says, that he himself, after having been electrified a long time together, had felt an odd sensation all over his body, and that some persons had felt very sharp pains after being electrified.*

THE ingenious Abbé Nollet begins his experiments with the evaporation of fluids by electricity. They were made with the

* Phil. Trans. abridged, Vol. 10. p. 321.

greatest attention, and the following observations were the result of them.

“ 1. ELECTRICITY augments the natural evaporation of fluids; since, excepting mercury, which is too heavy; and the oil of olives, which is too viscous, all the others which were tried suffered a diminution which could not be ascribed to any other cause than electricity.

“ 2. ELECTRICITY augments the evaporation of those fluids the most, which are most subject to evaporate of themselves. For the volatile spirit of sal ammoniac suffered a greater loss than spirit of wine, or turpentine; these two more than common water; and water more than vinegar, or the solution of nitre.

“ 3. ELECTRICITY has a greater effect upon fluids when the vessels which contain them are non-electrics; the effects always seeming to be a little greater when the vessels were of metal, than when they were of glass.

“ 4. THIS increased evaporation was more considerable when the vessel which contained the liquor was more open, but the effects did not increase in proportion to their apertures. For when these liquors were electrified in vessels, whose aperture was four inches in diameter, though they presented to the air a surface sixteen times larger than when they were contained in vessels whose aperture was one inch in diameter, they were, nevertheless, far from suffering a diminution proportioned to that difference.

“ 5. ELECTRIFICATION does not make any liquors evaporate through the pores, either of metal, or of glass; since, after experiments which were continued ten hours, there was found

“ no

“no diminution of their weight, when the vessels in which they were contained were well stopped.”*

AFTER having made experiments on fluids, he began another course on solids of various kinds, the result of which was, that they lost weight only in proportion, to the moisture they contained, and the openness of their pores. †

THE Abbé also extended his experiments to other sensible qualities of bodies, as their smell, their taste, and chymical properties; but found no change in any of them, after a strong and continued electrification of a variety of substances. Electrification did not affect the power of the magnet, and neither retarded nor accelerated the heating or cooling of bodies. ‡

HE then proceeded to the electrification of capillary tubes, full of water; it having been observed by Mr. Boze, who communicated the observation to Mr. Nollet, § that the water would issue in a constant stream when they were electrified; whereas it would only drop very slowly without that operation. Every body, at first sight, would judge that the stream was accelerated, and that the electrified vessel would soon be empty: but this accurate philosopher was unwilling to rely on first appearances, and therefore resolved to ascertain the fact, by measuring the time, and the quantity of liquor running out. And, in order to know if the acceleration, supposing there were any, was uniform, during the whole time of the running out, he made use of vessels of different capacities, terminating in pipes of different bores, from three lines in diameter, to the smallest capillaries.

As the Abbé did not find it so easy a matter to draw a safe

* Nollet's Recherches, p. 327. † Ib. p. 335. ‡ Ib. p. 341. § Ib. p. 345.
conclusion

conclusion in this case as might at first be imagined, he gives us in gross the following result of above an hundred experiments. *

“ 1. THE electrified stream, though it divides, and carries the liquid farther, is neither sensibly accelerated nor retarded, when the pipe through which it issues is not less than a line in diameter.

“ 2. UNDER this diameter, if the tube is wide enough to let the liquid run in a continued stream, electricity accelerates it a little; but less than a person would imagine, if he judged by the number of jets which are formed, and by the distance to which they go.

“ 3. IF the tube be a capillary one, from which the water only drops naturally, the electrified jet not only becomes a continued stream, and even divided into several streams, but is also considerably accelerated; and the smaller the capillary tube is, the greater, in proportion, is this acceleration.

“ 4. So great, is the effect of the electric virtue, that it drives the water in a constant stream out of a very small capillary tube, out of which it had not before been able even to drop.”

THE most unaccountable of these experiments, as the ingenious Abbé acknowledges, are those which suppose a retardation of the electrified current, and he long doubted the fact; but a great number of experiments, carefully noted in his journal, obliged him to admit it, though still with hesitation, and to account for it in the best manner he could; which, indeed, was not very satisfactory. †

THE beautiful appearance of these streams of electrified water, when the experiment was exhibited in the dark, is particularly

* Recherches, p. 327. Phil. Trans. abridged, Vol. 10. p. 382. † Recherches, p. 351. described

described by this author, after Messrs. Boze and Gordon, who first observed it.*

THESE last experiments served as a basis to the Abbe's future enquiries. He considered all organized bodies as assemblages of capillary tubes, filled with a fluid that tends to run through them, and often to issue out of them. In consequence of this idea, he imagined, that the electric virtue might possibly communicate some motion to the sap of vegetables, and also augment the insensible perspiration of animals. He began with the following experiments, the result of which confirmed his supposition. †

He electrified for four or five hours together, fruits, green plants, and sponges, dipped in water which he had carefully weighed; and found that, after the experiment, all those bodies were remarkably lighter than others of the same kind, weighed with them, both before and after the experiment, and kept in the same place and temper. ‡

THE electrification of growing vegetables was first begun in Britain. Mr. Maimbray at Edinburgh electrified two myrtle trees, during the whole month of October 1746; when they put forth small branches and blossoms sooner than other shrubs of the same kind, which had not been electrified. Mr. Nollet, hearing of this experiment, was encouraged to try it himself. ||

He took two garden pots, filled with the same earth, and sowed with the same seeds. He kept them constantly in the same place, and took the same care of them; except that one of the two was electrified fifteen days together, for two or three, and sometimes four hours a day. The consequence was, that the electrified pot always shewed the sprouts of its seeds two or

* Recherches, p. 354. † Ib. p. 355. ‡ Phil. Trans. abridged, Vol. 10. p. 385.

|| Recherches, p. 356.

three days sooner than the other. It also threw out a greater number of shoots, and those longer in a given time; which made him believe, that the electric virtue helped to open and display the germs, and thereby to facilitate the growth of plants. This, however, our cautious philosopher only calls a conjecture, which required further confirmation. The season, he says, was then too far advanced to allow him to make as many experiments as he could have wished, but he says the next course of experiments had greater certainty, and they are not less interesting.*

THE same experiments were carrying on about the same time by Mr. Jallabert, Mr. Boze, and the Abbé Menon, principal of the college of Bueil at Angers, who all drew the same conclusions from them. †

THE Abbé chose several pairs of animals of different kinds, cats, pigeons, chaffinches, sparrows, &c. All these he put into separate wooden cages, and weighed them. One of each pair he electrified for five or six hours together, and then weighed them again. The result was, that the electrified cat was commonly sixty five or seventy grains lighter than the other, the pigeon from thirty five to thirty eight grains, the chaffinch or sparrow six or seven grains. In order to have nothing to charge upon the difference that might arise from the temperament of the individuals he happened to pitch upon, he repeated the same experiments, by electrifying that animal of each pair which had not been electrified before; and, notwithstanding some small varieties which happened, the electrified animal was constantly lighter than the other in proportion ‡

* Recherches, p. 358, &c. Phil. Transf. abridged, Vol. 10. p. 383.

† Recherches, p. 357. ‡ Ib. p. 366, &c.

AFTER

AFTER these experiments, he had no doubt but that electricity increased the insensible perspiration of animals, but it was not certain whether this increase was in the ratio of their bulks, or in that of their surfaces. The Abbé's opinion was, that it was neither in the one, nor the other, strictly speaking, but in a ratio much more nearly approaching the latter than the former; so that he imagined, there was no room to apprehend, that a human person electrified would lose near a fiftieth part of his weight, as it appeared to him that it had happened to one sort of bird; nor 140th. part, as to the pigeon, &c. All that he had then observed upon that head was, that a young man or woman, between the ages of twenty and thirty, from being electrified five hours together, had lost several ounces of their weight, more than they were wont to lose when they were not electrified.*

THE Abbé observes, that no inconvenience whatever was felt by the persons who submitted to be electrified in this manner. They only found themselves a little exhausted, and had got a better appetite. He adds, that none of them found themselves sensibly warmer, and that he could not perceive that their pulse was increased. †

THESE last experiments on human bodies, he justly observes, are difficult to pursue with exactness, because the cloathing, which cannot strictly be compared to the hairs or feathers of animals, retains a considerable share of the perspired matter, and prevents our forming a good judgment of the whole effect of the electric virtue.

THE foregoing experiments, he says, convinced him of the reality of the *effluent* matter, carrying away with it the perspira-

* Phil. Transf. abridged, Vol. 10. p. 384. Recherches, p. 382. † Ib. p. 389.

ble parts of bodies, and what could be evaporated from their surfaces. And he was convinced of the *affluent* matter, by observing all those effects produced, if, instead of electrifying bodies themselves, they were only brought near a large body which was electrified. He moistened a thick sponge in water, and cut it into two pieces, and then weighed the parts separately, and placing the whole near a large electrified body; he found that, after an electrification of five or six hours, that part of the sponge which was nearer to the electrified body had lost more weight than the other. From this fact he concluded, that if any part of an animal body was presented to a large electrified substance, it would perspire more than the other, and that perhaps obstructions might by this means be removed from the pores of it.*

THE experiments above recited of Mr. Nollet by no means satisfied the English philosophers, and particularly Mr. Ellicott, who made experiments to refute the theory which the author had deduced from them. He observed that the syphon, though electrified, would only deliver the water by drops, if the bason in which the water was contained was electrified too. But this does not invalidate Mr. Nollet's curious experiments upon the subject of evaporation and perspiration. For when an animal body is electrified, there is always non-electric matter enough in the atmosphere, to answer the purpose of the unelectrified bason, in the experiment of the capillary tube; thereby to cause a continual exhalation of the perspirable matter from the pores of the skin. Besides, the capillary tube will, in fact, unite the water in a constant stream, when it has only the open air to throw it into. In all debates upon subjects in natural philoso-

* Phil. Trans. abridged. Vol. 10. p. 385.

phy,

phy, facts ought only to be opposed to facts. The veracity of the Abbé Nollet is not to be called in question; though it must be acknowledged, that, in his later writings, at a time when his favourite system was in danger, he makes many mistakes with respect to the facts that nearly affect it.

To account for the appearance of light, which seems, in some cases, to issue from a non-electric body presented to an excited electric, and which Mr. Nollet thought to be the affluent matter, Mr. Ellicott supposes that it was the light which had come from the electric. In accounting for the suspension of leaf gold between an electrified and an unelectrified plate, Mr. Ellicott's theory made it necessary to suppose (what Dr. Franklin afterwards found not to be fact) that the leaf gold will always be suspended nearer the unelectrified than the electrified plate.

In his answer to Mr. Nollet, Mr. Ellicott also endeavours to account for the electric matter issuing from a point at the extremity of the conductor, more sensibly than if it had terminated round or flat. He says that the effluvia, in rushing from the globe along the conductor, as they approached the point, were brought nearer together, and therefore were denser there than in any part of the rod. Consequently, he says, if the light be owing to the density and velocity of the effluvia, it will be visible at the point, and no where else. This, as far as I can find, was the first attempt to account for this phenomenon; but it by no means accounts for the whole virtue of the conductor being dissipated from such points. Indeed, it is no wonder that the influence of points, which are but imperfectly understood even at this day, furnished too difficult a problem so many years ago.*

* Phil. Trans. abridged, Vol. 10. p. 393.

It will, now, be universally acknowledged, that there was very great merit in these experiments of the Abbé Nollet, made upon animal and other organized bodies. He opened a new and noble field of electrical discoveries, and he pursued them with great attention, perseverance and expence. This last circumstance, I suppose, may have been the reason why his experiments have not, as far as I can find, been resumed and pursued by any electrician since his time, though there seems to be great room to improve upon what he began. The only method in which they can be conducted to any purpose, would be by the help of a machine for perpetual electrification, to go by wind or water; which would, likewise, serve for many other capital experiments in electricity. This application of electricity, in particular, may perhaps be of more use in medicine, than any other mode in which it has hitherto been administered.

MR. JALLABERT of Geneva carried the experiments on plants further than the Abbé Nollet had done; and, by electrifying bottles in which the plants were growing in water, and placing in the same exposure other bottles, containing plants of the same kind; proved, in the clearest manner, that the electrified plants always grew faster, and had finer stems, leaves, and flowers than those which were not electrified, and consumed more of their water.*

*Beccaria dell' elettricismo naturale et artificiale, p. 125.

SECTION V.

THE HISTORY OF THE MEDICATED TUBES, AND OTHER COMMUNICATIONS OF MEDICINAL VIRTUES BY ELECTRICITY, WITH THEIR VARIOUS REPUTATIONS.

IN the course of this history we have seen frequent instances of self deception, for want of persons attending to all the essential circumstances of facts; but nothing we have yet seen equals what was exhibited in the years 1747 and 1748. Mr. Grey's deceptions were chiefly owing to his mistaking the cause of real appearances; but in this case we can hardly help thinking; that, not only the imagination and judgment, but even all the external senses of philosophers must have been imposed upon. It was asserted by Signior Pivati at Venice (who has all the merit of these extraordinary discoveries) and, after him, by Mr. Verati at Bologna, Mr. Bianchi at Turin, and Mr. Winekler at Leipfick, that if odorous substances were confined in glass vessels, and the vessels excited, the odours and other medicinal virtues would transpire through the glass, infect the atmosphere of the conductor, and communicate the virtue to all persons in contact with it; also that those substances, held in the hands of persons, elec-
trified

trified, would communicate their virtues to them; so that medicines might be made to operate without being taken into the stomach. They even pretended to have wrought many cures by the help of electricity applied this way. Some of the more curious of these pretended experiments deserve to be recorded, for the entertainment and instruction of posterity.

THE forementioned Signior Johannes Francisco Pivati, a person of eminence at Venice, says, in an Italian epistle, printed at Venice with all the usual licences, in the year 1747, that a manifest example of the virtue of electricity was shown in the balsam of Peru, which was so concealed in a glass cylinder, that, before the excitation of it, not the least smell could by any means be discovered. A man who, having a pain in his side, had applied hyssop to it by the advice of a physician, approached the cylinder thus prepared, and was electrified by it. The consequence was, that when he went home, and fell asleep, he sweated, and the power of the balsam was so dispersed, that even his cloaths, the bed, and chamber, all smelled of it. When he had refreshed himself by this sleep, he combed his head, and found the balsam to have penetrated his hair; so that the very comb was perfumed.*

THE next day, Signior Pivati says, he electrified a man in health in the same manner, who knew nothing of what had been done before. On his going into company half an hour afterwards, he found a gradual warmth diffusing itself through his whole body, and he grew more lively and chearful than usual. His companion was surpris'd at an odour, and could not imagine whence it proceeded, but he himself perceived that the fume arose from his own body, at which he also was much sur-

*Phil. Transf. abridged, Vol. 10. p. 400.

prized,

prized, not having the least suspicion that it was owing to the operation which had been performed upon him by Signior Pivati.*

MR. WINCKLER of Leipfick, being struck with fo extraordinary a relation, fays, that he was defirous of trying the power of electricity on certain fubftances in the fame manner, and that he found the event to confirm what had been related. †

HE put fome beaten fulphur into a glafs fphere, fo well covered and ftopped, that, on turning it over the fire, there was not the leaft fmell of fulphur perceived. When the fphere was cold he electrified it; when, immediately, a fulphureous vapour iffued from it, and, on continuing the electricity, filled the air, fo as to be fmelled at the diftance of more than ten feet. He called in a friend well verfed in electricity, profeffor Haubold, and feveral others, as witneffes and judges of this fact; but they were prefently driven away by the ftench of the fulphur. He ftaid a little longer himfelf in this fulphureous atmofphere, and was fo impregnated thereby, that his body, cloaths, and breath retained the odour, even the next day. On repeating this experiment in the prefence of a perfon who was converfant with the effects of fulphur, the figns of an inflamed blood were vifible in his mouth on the third day. ‡

AFTER this he tried the effect of a more agreeable fmell, and filled the fphere with cinnamon. When he had heated this as before, the fmell of cinnamon was foon perceived by the company, and the whole room was in a fhort time fo perfumed by it, that it immediately affected the nofes of all who came in, and the odour remained the next day.

HE tried the balfam of Peru with the like fuccefs, when his

* Phil. Tranf. abridged, Vol. 10. p. 401. † Ib. ‡ Ib.

above

above-mentioned friend (whose testimony, he says, he did not care to be without) after he had received the power of the balsam, smelled so strong of it, that, going abroad to supper, he was often asked by the company what perfume he had about him. The next day, when Mr. Winckler was drinking tea, he says, he found an unusual sweet taste, owing to the fumes of the balsam that still remained in his mouth. *

IN a few days, when the sphere had lost all the scent of the balsam, they let a chain out of the chamber window, and extended it through the open air, into another room detached from the former. Here they suspended the chain on silken lines, and gave it into the hand of a man, who also stood on extended silken lines, and knew nothing of their purpose. When the electricity had been excited for some time, the man was asked whether he smelled any thing; and, on snuffing up his nose, he said he did. Being asked again what smell it was, he said he did not know. When the electrification had been continued for about a quarter of an hour, the room smelled so strong of it, that the man, who knew nothing of the balsam, said his nose was filled with a sweet smell, like that of some sort of balsam. After sleeping in a house, a considerable distance from the room where the experiment was tried, he rose very chearful in the morning, and found a more pleasant taste than ordinary in his tea. †

I SHALL only give an account of two instances of the effect of medicine applied in this manner. The assistance of Signior Pivati, the celebrated inventor of this improvement in electricity, was implored by a young gentleman, who was miserably afflicted by a quantity of corrupted matter collected in his foot, that eluded all the attempts of the physicians. Signior Pivati filled a

* Phil. Transf. abridged, Vol. 10. p. 401.

† Ib.

glass cylinder with proper materials; and, having electrified it, drew sparks from the part affected, and continued the operation for some minutes. When the patient went to bed, he had a good night, and a mitigation of his pain. When he awaked in the morning, he found a small red tubercle on his foot, which only itched, as if a cold humour had flowed through the inner part of his foot. He sweated every night for eight days together, and at the end of that time was perfectly well.

AFTER this, Signior Donadoni, bishop of Sebenico, came to Signior Pivati, attended by his physician and some friends. His lordship was at that time seventy five years old, and had been afflicted with pains in his hands and feet for several years. The gout had so affected his fingers, that he was not able to move them; and his legs, so that he could not bend his knees. In this deplorable situation, the poor old bishop intreated Signior Pivati to try the effects of electricity on him. The electrician undertook it, and proceeded after the following manner. He filled a glass cylinder with discutient medicines, and managed it so, that the electric virtue might enter into the patient, who presently felt some unusual commotions in his fingers; and the action of electricity had been continued but two minutes, when his lordship opened and shut both his hands, gave a hearty squeeze to one of his attendants, got up, walked, smote his hands together, helped himself to a chair, and sat down, wondering at his own strength, and hardly knowing whether it was not a dream. At length he walked out of the chamber down stairs, without any assistance, and with all the alacrity of a young man.*

A VARIETY of facts of this nature being published, and seem-

* Phil. Trans. abridged, Vol. 10. p. 403.

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ingly well attested, engaged all the electricians of Europe to repeat these experiments; but none of them could succeed but those mentioned above. An excellent remark of Mr. Baker's, who advised trying all these experiments, notwithstanding their seeming very improbable, deserves to be quoted here. "Romatic as these things may seem, they should not be absolutely condemned without a fair trial, since we all, I believe remember the time, when those phenomena in electricity, which are now the most common and familiar to us, would have been thought deserving as little credit as the cases under consideration may seem to do, had accounts of them been sent to us from Rome, Venice, or Bologna, and had we never experienced them ourselves. *

To see these wonders, and to be assured of their truth or fallacy, Mr. Nollet, who was deeply interested in every thing that related to his favourite study, and who set no bounds to his labour or expences, in the pursuit of truth, even passed the Alps, and travelled into Italy, where he visited all the gentlemen who had published any account of these experiments. But though he engaged them to repeat their experiments in his presence, and upon himself; and though he made it his business to get all the best information he could concerning them, he returned, convinced that the accounts of cures had been much exaggerated; that in no one instance had odours been found to transpire through the pores of excited glass; and that no drugs had ever communicated their virtues to persons who only held them in their hands while electrified.

He had no doubt, however, but that, by continued electrification, without drugs, several persons had found considerable

* Phil. Trans. abridged, Vol. 10. p. 406.

relief

relief in various disorders; particularly, that a paralytic person had been cured at Geneva, and that a person deaf of one ear, a footman who had a violent pain in his head, and a woman who had a disorder in her eyes were cured at Bologna.*

THE English philosophers showed no less attention to this subject than the Abbé Noller. The Royal Society had received an account from Mr. Winckler of his experiments, to prove the transudation of odoriferous matter through the pores of excited glass; and none of them succeeding here, the secretary was desired to write to Mr. Winckler, in the name of the society, desiring him to transmit to them, not only a circumstantial account of his manner of making the experiments, but likewise some globes and tubes, fitted up by himself, for that purpose.

THESE vessels, and directions how to use them, Mr. Winckler actually sent, and the experiments were made with every possible precaution, at the house of Dr. Watson (the most interested and active person in the kingdom in every thing relating to electricity) on the 12th of June 1751. There were present Martin Folkes president of the Royal Society, Nicholas Mann Esq. vice president, Dr. Mortimer, and Peter Daval Esq. secretaries, Mr. Canton a fellow, and Mr. Shroder, a gentleman of distinction, well known to, and corresponding with Mr. Winckler. But, notwithstanding all the pains these gentlemen took, pursuing, with the utmost exactness, the directions of Mr. Winckler, and also using methods of their own, which they thought still better adapted to force the effluvia through the glass, they were unsuccessful. They were not able to verify Mr. Winckler's experiments even in one single instance.†

BUT perhaps the most satisfactory refutation, both of this

* Phil. Transf. abridged, Vol. 10. p. 413, &c. † Ib. Vol. 47. p. 231.

pretended transfusion of odours, and the medicinal effects of electricity above-mentioned, was made at Venice, the very place where this medical electricity took its rise. The experiments were made by Dr. Bianchini, professor of medicine, in the presence of a great number of witnesses, many of them prejudiced in favour of the pretended discoveries; but they were all forced to be convinced of their futility, by the evidence of facts; and by experiments made with the greatest care and accuracy.*

AFTER the publication of these accounts properly attested, every unprejudiced person was satisfied, that the pretended discoveries from Italy and Leipfick, which had raised the expectation of all the electricians in Europe, had no foundation in fact; and that no method had yet been discovered whereby the power of medicine could by electricity be made to infuse itself into the human body. † Dr. Franklin also showed by several experiments the impossibility of mixing the effluvia or virtue of medicines with the electric fluid. ‡

In some respects similar to the experiments with the medicated tubes (as those mentioned above were usually called) was that of professor Boze of Wittemburgh, which he termed the *beatification*; and which, for a long time, employed other electricians to repeat after him, but to no purpose. His description of this famous experiment was, that if, in electrifying, large globes were employed, and the electrified person were placed upon large cakes of pitch, a lambent flame would by degrees arise from the pitch, and spread itself round his feet; and that from thence it would be propagated to his knees and body, till, at last, it ascended to his head; that then, by continuing the electrification, the person's head would be surrounded by a glory,

* Phil. Trans. abridged, Vol. 48. p. 399. † Ib. p. 406. ‡ Franklin's Letters, p. 82.

such a one, in some measure, as is represented by painters in their ornamenting the heads of saints.*

THIS experiment, as well as that of the medicated tubes, set all the electricians in Europe to work, and put them to a great deal of expence; but none of them could succeed, so as to produce an appearance any thing like that described by Mr. Boze. No person took more pains in this business than Dr. Watfarr. He himself underwent the operation several times, supported by solid electrics three feet high. Upon being electrified very strongly, he found, as he says, several other persons also did, a tingling upon the skin of his head, and in many parts of his body, or such a sensation as would be felt from a vast number of insects crawling over him at the same time; and he constantly observed the sensation to be the greatest in those parts of his body which were nearest to any non-electric, but still no light appeared upon his head, though the experiment was several times made in the dark, and with some continuance †

AT length the Doctor, wearied with these fruitless attempts, wrote to the professor, and his answer showed that the whole had been a mere trick. He candidly acknowledged, that he had made use of a suit of armour, which was decked with many bulions of steel, some pointed like nails, some like wedges, and some pyramidal; and that when the electrization was very vigorous, the edges of the helmet would dart forth rays, something like those which are painted on the heads of saints. And this was all his boasted beatification. ‡

THIS same Mr. Boze, who seems to have had a singular affectation of something mysterious and marvellous in his experiments, in a letter to the Royal Society at London, said that he had been

* Phil. Trans. abridged, Vol. 10. p. 411. † Ib. ‡ Ib. p. 413

able, by electricity only, to invert the poles of a natural magnet, to destroy their virtue, and restore it again, but he did not describe his method.* Considering that no person in England could succeed in this attempt, and that we are now able to do it but imperfectly, it is hardly probable that he did it at all.

THERE seems to have been some deception in an experiment which the worthy and excellent Dr. Hales communicated to the Royal Society this year, when he says, that he observed the electric spark from warm iron to be of a bright light colour; from warm copper, green; and from a warm egg, of a yellowish flame colour. These experiments, he said, seemed to argue, that some particles of those different bodies were carried off in the electric flashes, whence those different colours were exhibited. †

I SHALL conclude this section, which might justly be intitled the *marvellous*, with mentioning the surprizing effect of an electric spark in setting fire to a fustian frock, on a son of Mr. Robert Roche, when he was electrified for some disorder. † I do not question the fact. The experiment was repeated, and it answered again as well as at the first time, when it was merely accidental. The paper containing this account was read at the Royal Society on the 29th of May 1748. ‡

* Wilson's Essay, p. 219. † Phil. Transf. abridged, Vol. 10. p. 406 ‡ Ib.

PERIOD IX.

THE EXPERIMENTS AND DISCOVERIES OF DR. FRANKLIN.

SECTION I.

DR. FRANKLIN'S DISCOVERIES CONCERNING THE LEYDEN
PHIAL, AND OTHERS CONNECTED WITH THEM.

WE have hitherto seen what had been done in electricity by the English philosophers, and those on the continent of Europe, till about the year 1750; but our attention is now strongly called to what was doing on the continent of America; where Dr. Franklin and his friends were as assiduous in trying experiments, and as successful in making discoveries, as any of their brethren in Europe. For this purpose, we must look back a few years. As Dr. Franklin's discoveries were made intirely independent of any in Europe, I was unwilling to interrupt the former general account, by introducing them in their proper year. For the same reason, I imagine it will be generally more agreeable,

able, to see, at one view, what was done in America for some considerable space of time, without interrupting this account with what was doing, in the mean time, in Europe. I shall, therefore, digest, in the best manner I can, the three first publications of Dr. Franklin, entitled *New Experiments and Observations on Electricity, made at Philadelphia in America*, communicated in several letters to Peter Collinson Esq. of London, fellow of the Royal Society; the first of which is dated July 28th. 1747, and the last April 18th. 1754.

NOTHING was ever written upon the subject of electricity which was more generally read, and admired in all parts of Europe than these letters. There is hardly any European language into which they have not been translated; and, as if this were not sufficient to make them properly known, a translation of them has lately been made into Latin. It is not easy to say, whether we are most pleased with the simplicity and perspicuity with which these letters are written, the modesty with which the author proposes every hypothesis of his own, or the noble frankness with which he relates his mistakes, when they were corrected by subsequent experiments.

THOUGH the English have not been backward in acknowledging the great merit of this philosopher, he has had the singular good fortune to be, perhaps, even more celebrated abroad than at home; so that to form a just idea of the great and deserved reputation of Dr. Franklin, we must read the foreign publications on the subject of electricity; in many of which the terms *Franklinism*, *Franklinist*, and the *Franklinian system* occur in almost every page. In consequence of this, Dr. Franklin's principles bid fair to be handed down to posterity, as equally expressive of the true principles of electricity, as the Newtonian philosophy is of the true system of nature in general.

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THE zeal of Dr. Franklin's Friends, and his reputation, were considerably increased by the opposition which the Abbé Nollet made to his theory. The Abbé, however, never had any considerable seconds in the controversy, and those he had, I am informed, have all deserted him.

THE rise of Dr. Franklin's fame in France was first occasioned by a bad translation of his letters falling into the hands of Mr. Buffon, intendant of the French King's gardens, and author of the Natural History for which he is famous. This gentleman, having successfully repeated Dr. Franklin's experiments, engaged a friend of his, Mr. Dalibard, to revise the translation; which was afterwards published, with a short history of electricity prefixed to it, and met with a very favourable reception from all ranks of people. What contributed not a little to the success of this publication, and to bring Dr. Franklin's principles into vogue in France, was a friend of Mr. Dalibard's exhibiting Dr. Franklin's experiments for money. All the world, in a manner, flocked to see these new experiments, and all returned full of admiration for the inventor of them. *

DR. FRANKLIN had discovered, as well as Dr. Watson, that the electric matter was not created, but collected by friction, from the neighbouring non-electric bodies. He had observed, that it was impossible for a man to electrify himself, though he stood upon glass or wax; for that the tube could communicate to him no more electricity than it had received from him in the act of excitation. He had observed, that if two persons stood upon wax, one of which rubbed the tube, and the other took the fire from it, they would both appear to be electrified; that if they touched one another after that operation, a

* Nollet's Letters, Vol. 1. p. 4.

stronger spark would be perceived between them, than when any other person touched either of them; and that such a spark would take away the electricity of both. *

THESE experiments led the Doctor to think, that the electric fluid was conveyed from the person who rubbed the tube to him who touched it; which introduced some terms in electricity that had not been used before, but have continued in use ever since. The person who touched the tube was said, by Dr. Franklin, to be electrified *positively*, or *plus*; being supposed to receive an additional quantity of electric fire: whereas the person who rubbed the tube was said to be electrified *negatively*, or *minus*; being supposed to have lost a part of his natural quantity of the electric fluid. †

THIS observation was necessary to explain the capital discovery which Dr. Franklin made with respect to the manner of charging the Leyden phial; which is, that when one side of the glass was electrified positively or *plus*, the other side was electrified negatively or *minus*; so that whatever quantity of fire is thrown upon one side of the glass, the same is thrown out of the other; and that there is really no more electric fire in the phial after it is charged than before; all that can be done by charging, being to take from one side, and convey to the other. Dr. Franklin also observed, that glass was impervious to electricity, and that, therefore, since the equilibrium could not be restored to the charged phial by any internal communication, it must be done by conductors externally, joining the inside and the outside. ‡

THESE capital discoveries he made by observing, that when a phial was charged, a cork ball suspended on silk would be at-

* Franklin's Letters, p. 14. † Ib. p. 15. ‡ Ib. p. 3.

tracted by the outside coating, when it was repelled by a wire communicating with the inside; and that it would be repelled by the outside, when it was attracted by the inside.* But the truth of this maxim appeared more evident when he brought the knob of the wire communicating with the outside coating within a few inches of the wire communicating with the inside coating, and suspended a cork ball between them; for, in that case, the ball was attracted by them alternately, till the phial was discharged. †

THE European electricians had observed, that a phial could not be charged unless some conductor was in contact with the outside; but Dr. Franklin made the observation more general, and also was able, by the principle above-mentioned, to give a better account of it. As no more electric fire, he says, can be thrown into the inside of a phial when all is driven from the outside; so, in a phial not yet charged, none can be thrown into the inside when none can be got from the outside. He also showed, by a beautiful experiment, that, when the phial was charged, one side lost exactly as much as the other gained, in restoring the equilibrium. Hanging a small linen thread near the coating of an electrified phial, he observed, that every time he brought his finger near the wire, the thread was attracted by the coating. For as the fire was taken from the inside by touching the wire, the outside drew in an equal quantity by the thread. ‡

He proved that, in discharging the phial, the giving from one side was exactly equal to the receiving by the other, by placing a person upon electrics, and making him discharge the phial through his body; when he observed, that no electricity re-

* Franklin's Letters, p. 4. † *Ib.* p. 5. ‡ *Ib.*

mained in him after the discharge. * He also hung cork balls upon an insulated conductor at the time of the discharge of a phial hanging to it; and observed, that if they did not repel before the explosion, they did not repel at the time, nor after. † But the experiment which most completely proved, that the coating on one side received just as much as was emitted from the discharge of the other, was the following.

He insulated his rubber, then, hanging a phial to his conductor, he found it could not be charged, even though his hand was held constantly to it; because, though the electric fire might leave the outside of the phial, there was none collected by the rubber to be conveyed into the inside. He then took away his hand from the phial, and forming a communication, by a wire from the outside coating to the insulated rubber, he found that it was charged with ease. In this case, it was plain, that the very same fire which left the outside coating was conveyed by the way of the rubber, the globe, the conductor, and the wire of the phial, into the inside. ‡

DR. FRANKLIN'S new theory of charging the Leyden phial led him to observe a greater variety of facts, relating both to charging and discharging it, than other philosophers had attended to. He found that the phial would be electrified as strongly if it were held by the hook, and the coating applied to the globe or tube, as if it were held by the coating, and the hook applied; and, consequently, that there would be the same explosion, and shock, if the electrified phial were held in one hand by the hook, and the coating touched with the other, as when held by the coating, and touched at the hook. To take the charged phi-

* Franklin's Letters, p. 8.

† *Ib.* p. 84.‡ *Ib.* p. 83.

al by the hook with safety, and not diminish its force, he observes, that it must first be set down upon electrics per se. *

DR. FRANKLIN observed, that if a man held in his hand two phials, the one fully electrified, and the other not at all, and brought their hooks together, he would have but half a shock: for the phials would both remain only half electrified, the one being half charged, and the other half discharged. †

If two phials were charged both through their hooks, a cork ball suspended on silk, and hanging between them, would first be attracted, and then repelled by both; but if they were electrified, the one through the wire, and the other through the coating, the ball would play vigorously between them both, till they were nearly discharged. ‡ The Doctor did not, at that time, take notice, that if the phials were both charged through their coatings (by which both the hooks would have been electrified minus) the ball would be repelled by them both, as when they were electrified plus. And when he, afterwards, observed that two bodies electrified minus repelled one another, he seems to have been surpris'd at the appearance, and acknowledged that he could not satisfactorily account for it. ||

It was known to every electrician, that a globe or tube wet on the inside would afford little or no fire; but no good reason was given for it, before Dr. Franklin attempted its explanation by the help of his general maxim. He says, that when a tube lined with any non-electric is rubbed, what is collected from the hand by the downward stroke enters the pores of the glass, driving an equal quantity out of the inner surface, into the non-electric lining; and that the hand, in passing up to take a second stroke, takes out again what had been thrown into the outward

* Franklin's Letters, p. 19. † Ib. p. 21. ‡ Ib. || Ib. p. 34.

surface,

surface, the inner surface at the same time receiving back again what it had given to the non-electric lining; so that the particles of the electric fluid went in and out of their pores, upon every stroke given to the tube. *

IF, in these circumstances, a wire was put into the tube, he observed, that, if one person touched the wire, while another was rubbing the tube, and took care to withdraw his finger as soon as he had taken the spark, which had been made to fly from the inside, it would be charged. †

IF the tube was exhausted of air, he observes, that a non-electric lining in contact with the wire was not necessary; for that, in *vacuo*, the electric fire would fly freely from the inner surface, without a non-electric conductor. ‡

UPON the same principle he accounts for the effects of an excited electric being perceived through the glass in the vacuum beyond it. The tube and its excited atmosphere, being brought near a glass vessel, repels the electric fluid from the inner surface, of the glass; and this fluid, issuing from the inner surface acts upon light bodies in the vacuum, both in its passage from the glass, and likewise in its return to it, when the excited electric on the outside is withdrawn. ||

THIS maxim, that whatever the phial takes in at one surface it loses at the other, led Dr. Franklin to think of charging several phials together with the same trouble, by connecting the outside of one with the inside of another; whereby the fluid that was driven out by the first would be received by the second, and what was driven out of the second would be received by the third, &c. By this means he found, that a great number of bottles might be charged with the same labour as one only; and that they might be charged equally high, were it not that every bot-

* Franklin's Letters, p. 76. † *Ib.* p. 77. ‡ *Ib.* || *Ib.* p. 78.

tle receives the new fire, and loses its old with some reluctance, or rather gives some small resistance to the charging. This circumstance, he says, in a number of bottles, becomes more equal to the charging power, and so repels the fire back again on the globe sooner than a single bottle would do.*

UPON this principle Dr. Franklin constructed an *electrical battery*, consisting of eleven panes of large sash glass, coated on each side, and so connected, that charging one of them would charge them all. Then having a contrivance to bring the giving sides in contact with one wire, and all the receiving sides with another, he united the force of all the plates, and discharged them all at once.†

WHEN Dr. Franklin first began his experiments upon the Leyden phial, he imagined that the electric fire was all crowded into the substance of the non-electric in contact with the glass; but he afterwards found, that its power of giving a shock lay in the glass itself, and not in the coating, by the following ingenious analysis of the bottle.

IN order to find where the strength of the charged bottle lay, he placed it upon glass; then first took out the cork and the wire, and finding the virtue was not in them; he touched the outside coating with one hand, and put the finger of the other into the mouth of the bottle; when the shock was felt quite as strong as if the cork and wire had been in it. He then charged the phial again, and pouring out the water into an empty bottle insulated, expected that if the force resided in the water it would give the shock, but he found it gave none. He then judged that the electric fire must either have been lost in decanting, or must remain in the bottle; and the latter he found to be true;

* Franklin's Letters, p. 12.

† Ib. p. 26.

for, filling the charged bottle with fresh water, he found the shock, and was satisfied that the power of giving it resided in the glass itself. *

THE DOCTOR made the same experiment with panes of glass, laying the coating on lightly, and changing it, as he had before changed the water in the bottle, and the result was the same in both. † This experiment is more satisfactory than the former; because, when the water is poured out of the phial, there still remains a thin coating of the fluid, which might be thought to contain the power of giving a shock.

THAT the electric fire resided in the glass, was also further evident from this consideration, that when glass was gilt, the discharging of it would make a round hole, tearing off a part of the gilding, which, the Doctor thought, could only have been done by the fire coming out of the glass through the gilding. He also says, that when the gilding was varnished even with turpentine, this varnish, though dry and hard, would be burnt by the spark driven through it, yielding a strong smell, and a visible smoke. Also, that when a spark was driven through paper, it would be blackened by the smoke, which sometimes penetrated several of the leaves, and that part of the gilding which had been torn off was found forcibly driven into the hole made in the paper by the stroke. He also observed, that when a thin bottle was broken by a charge, the glass was broken inwards, at the same time that the gilding was broken outwards. ‡

LASTLY, Dr. Franklin discovered, that several substances which would conduct electricity in general, would not conduct the shock of a charged phial. A wet packthread, for instance,

* Franklin's Letters, p. 24.

† Ib. p. 25.

‡ Ib. p. 32.

though

though it transmitted electricity very well, sometimes failed to conduct a shock; as also did a cake of ice. Dry earth too rammed into a glass tube intirely failed to conduct a shock, and indeed would convey electricity but very imperfectly. *

* Franklin's Letters, p. 33.

DR. FRANKLIN'S DISCOVERIES CONCERNING THE SIMILARITY OF LIGHTNING AND ELECTRICITY.

THE greatest discovery which Dr. Franklin made concerning electricity, and which has been of the greatest practical use to mankind, was that of the perfect identity between electricity and lightning. The analogy between these two powers had not been wisely understood by philosophers, and especially by electricians, before the publication of Dr. Franklin's discovery. It was so obvious, that it had struck several persons, and had given one instance, in the lightning Abbe Nollet. The Abbe says, "It is not possible to take upon him to prove, from a well connected comparison of phenomena, that lightning is, in the front of nature, what electricity is in the laboratory; which we now explain at our pleasure, and into minutiae of their great effects, which lightning is to that the whole depends upon the laws of mechanics. It is to be demonstrated, that a cloud, formed by the action of the water of sea, by a mixture of electrical matter, is a globe

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SECTION II.

DR. FRANKLIN'S DISCOVERIES CONCERNING THE SIMILARITY OF LIGHTNING AND ELECTRICITY.

THE greatest discovery which Dr. Franklin made concerning electricity, and which has been of the greatest practical use to mankind, was that of the perfect similarity between electricity and lightning. The analogy between these two powers had not been wholly unobserved by philosophers, and especially by electricians, before the publication of Dr. Franklin's discovery. It was so obvious, that it had struck several persons. I shall give one instance, in the sagacious Abbé Nollet.

THE Abbé says, * “ If any one should take upon him to
 “ prove, from a well connected comparison of phenomena, that
 “ thunder is, in the hands of nature, what electricity is in ours,
 “ that the wonders which we now exhibit at our pleasure are
 “ little imitations of those great effects which frighten us, and
 “ that the whole depends upon the same mechanism; if it is to
 “ be demonstrated, that a cloud, prepared by the action of the
 “ winds, by heat, by a mixture of exhalations, &c. is opposite

* Leçons de Physique, Vol. 4. p. 34.

“ to

“ to a terrestrial object; that this is the electrized body, and, at
 “ a certain proximity from that which is not; I avow that this
 “ idea, if it was well supported, would give me a great deal
 “ of pleasure; and in support of it, how many specious reasons
 “ present themselves to a man who is well acquainted with elec-
 “ tricity. The universality of the electric matter, the readiness
 “ of its action, its inflammability, and its activity in giving fire
 “ to other bodies, its property of striking bodies externally and
 “ internally, even to their smallest parts, the remarkable exam-
 “ ple we have of this effect in the experiment of Leyden, the idea
 “ which we might truly adopt in supposing a greater degree of
 “ electric power, &c. all these points of analogy, which I have
 “ been some time meditating, begin to make me believe, that
 “ one might, by taking electricity for the model, form to ones
 “ self, in relation to thunder and lightning, more perfect and
 “ more probable ideas than what have been offered hitherto,
 “ &c.”

MR. WINCKLER also enumerated many particulars, in which electricity and lightning resemble one another.*

BUT though the Abbé, and others, had been struck with the obvious analogy between lightning and electricity, they went no farther than these arguments *a priori*. It was Dr. Franklin who first proposed a method of verifying this hypothesis, entertaining the bold thought, as the Abbé Nollet expresses it, of bringing lightning from the heavens, of thinking that pointed iron rods, fixed in the air, when the atmosphere was loaded with lightning, might draw from it the matter of the thunderbolt, and discharge it without noise or danger into the immense body of the earth, where it would remain as it were absorbed.

* Geschichte, Vol. 3. p. 528.

MOREOVER, though Dr. Franklin's directions were first begun to be put in execution in France, he himself completed the demonstration of his own problem, before he heard of what had been done elsewhere: and he extended his experiments so far as actually to imitate all the known effects of lightning by electricity, and to perform every electrical experiment by lightning.

BUT before I relate any of Dr. Franklin's experiments concerning lightning, I must take notice of what he observed concerning the power of pointed bodies, by means of which he was enabled to carry his great designs into execution. He was properly the first who observed the intire and wonderful effect of pointed bodies, both in drawing, and throwing off the electric fire.

IT was a small step towards discovering the effect of pointed bodies, that Carolus Augustus van Bergen, professor of medicine at Frankfort on the Oder, observed that sparks taken from a polished body were stronger than those from a rough one. He could fire spirit easily with a polished conductor, but with difficulty by means of one not polished.*

MR. JALLABERT was perhaps the first who observed that a body, pointed at one end, and round at another, produced different appearances upon the same body, according as the pointed, or round end was presented to it. But as Mr. Nollet, in whose presence he made the experiment, says, the effect was not constant, and nothing was inferred from it.† And the Abbé acknowledges, that Dr. Franklin was the first who showed the property of pointed bodies, in drawing off electricity more effectually, and at greater distances than other bodies could do it.‡

HE electrified an iron shot, three or four inches in diameter, and observed, that it would not attract a thread, when the point

* *Geschichte*, Vol. 2. p. 378. † *Lettres*, Vol. 1. p. 130. ‡ *Recherches*, p. 132.

of a needle was presented to it; but that this was not the case, unless the pointed body had a communication with the earth; for, presenting the same pointed body, stuck on a piece of sealing wax, it had not that effect; though the moment the pointed body was touched with his finger, the electricity of the ball to which it was suspended was discharged. The converse of this he proved, by finding it impossible to electrify the iron shot when a sharp needle lay upon it. *

By observing points of different degrees of acuteness, Dr. Franklin corrected the observation of Mr. Ellicott, and other English electricians, that a pointed body, as a piece of leaf gold, would always be suspended nearer to the plate which was unelectricified than that which was electricified, if it were put between them. For the Doctor observed, that it always removed farthest from that plate to which its sharpest point was presented, whether it was electricified or not; and if one of the points was very blunt, and the other very sharp, it would be suspended in the air by its blunt end, near the electricified body, without any unelectricified plate being held below it at all. †

Dr. FRANKLIN endeavoured to account for this effect of pointed bodies, by supposing that the base on which the electric fluid at the point of an electricified body rested, being small, the attraction by which the fluid was held to the body was slight; and that, for the same reason, the resistance to the entrance of the fluid was proportionably weaker in that place than where the surface was flat. ‡ But he himself candidly owns, that he was not quite satisfied with this hypothesis. Whatever we think of Dr. Franklin's theory of the influence of pointed conductors in drawing

* Franklin's Letters, p. 56, &c.

† Ib. p. 67.

‡ Ib. p. 56.

and

and throwing off the electric fluid, the world is greatly indebted to him for the practical use he made of this doctrine.*

DR. FRANKLIN begins his account of the similarity of the electric fluid and lightning by cautioning his readers not to be staggered at the great difference of the effects in point of degree; since that was no argument of any disparity in their nature. It is no wonder, says he, if the effects of the one should be so much greater than those of the other. For if two gun barrels electrified will strike at two inches distance, and make a loud report, at how great a distance will 10,000 acres of electrified cloud strike, and give its fire, and how loud must be that crack!†

I SHALL digest all Dr. Franklin's observations concerning lightning under the several points of resemblance which he observed between it and electricity, mentioning these points of similarity in the order in which he himself remarked them; only bringing into one place the observations which may happen to lie in different parts of his letters, when they relate to the same subject.

1. FLASHES of lightning, he begins with observing, are generally seen crooked, and waving in the air. The same, says he, is the electric spark always, when it is drawn from an irregular body at some distance.‡ He might have added, when it is drawn by an irregular body, or through a space in which the best conductors are disposed in an irregular manner, which is always the case in the heterogenous atmosphere of our globe.

2. LIGHTNING strikes the highest and most pointed objects in its way preferably to others, as high hills, and trees, towers, spires, masts of ships, points of spears, &c. In like manner, all

* Franklin's Letters, p. 62. † Ib. p. 44. ‡ Ib. p. 46.

pointed

pointed conductors receive or throw off the electric fluid more readily than those which are terminated by flat surfaces.*

3. LIGHTNING is observed to take the readiest and best conductor. So does electricity in the discharge of the Leyden phial. For this reason, the Doctor supposes that it would be safer, during a thunder storm, to have ones cloaths wet than dry, as the lightning might then, in a great measure, be transmitted to the ground, by the water, on the outside of the body. It is found, says he, that a wet rat cannot be killed by the explosion of the electrical bottle, but that a dry rat may. †

4. LIGHTNING burns. So does electricity. Dr. Franklin says, that he could kindle with it hard dry rosin, spirits unwarmed, and even wood. He says, that he fired gunpowder, by only ramming it hard in a cartridge, into each end of which pointed wires were introduced, and brought within half an inch of one another, and discharging a shock through them. ‡

5. LIGHTNING sometimes dissolves metals. So does electricity, though the Doctor was mistaken when he imagined it was by a cold fusion, as will appear in its proper place. The method in which Dr. Franklin made electricity melt metals was by putting thin pieces of them between two panes of glass bound fast together, and sending an electric shock through them. Sometimes the piece of glass, by which they were confined, would be shattered to pieces by the discharge, and be broken into a kind of coarse sand, which once happened with pieces of thick looking glass; but if they remained whole, the piece of metal would be missing in several places where it had lain between them, and instead of it, a metallic stain would be seen on both

* Franklin's Letters, p. 47. † Ib. ‡ Ib. p. 48, 92.

the glasses, the stains on the under and upper glass being exactly similar in the minutest stroke.*

A PIECE of leaf gold used in this manner appeared not only to have been melted, but, as the Doctor thought, even vitrified, or otherwise so driven into the pores of the glass, as to be protected by it from the action of the strongest aquaregis. Sometimes he observed that the metallic stains would spread a little wider than the breadth of the thin pieces of metal. True gold, he observed, made a darker stain, somewhat reddish, and silver a greenish stain. †

MR. WILSON supposes that, in this experiment, the gold was not driven into the pores of the glass, but only into so near a contact with the surface of the glass, as to be held there by an exceeding great force; such an one, he says, as is exerted at the surface of all bodies whatever. ‡

6. LIGHTNING rends some bodies. The same does electricity. || The Doctor observes, that the electric spark would strike a hole through a quire of paper. When wood, bricks, stone, &c. are rent by lightning, he takes notice, that the splinters will fly off on that side where there is the least resistance. In like manner, he says, when a hole is struck through a piece of pasteboard by an electrified jar, if the surfaces of the pasteboard are not confined and compressed, there will be a bur raised all round the hole on both sides of the pasteboard; but that if one side be confined, so that the bur cannot be raised on that side, it will all be raised on the other side, which way soever the fluid was directed. For the bur round the outside of the hole is the effect

* Franklin's Letters, p. 48, 65. † Ib. p. 68. ‡ Hoadley and Wilson, p. 68.

|| Franklin's Letters, p. 49.

of the explosion, which is made every way from the center of the electric stream, and not an effect of its direction.*

7. LIGHTNING has often been known to strike people blind. And a pigeon, after a violent shock of electricity, by which the Doctor intended to have killed it, was observed to have been struck blind likewise. †

8. IN a thunder storm at Stretham, described by Dr. Miles, ‡ the lightning stripped off some paint which had covered a gilded molding of a pannel of wainscot, without hurting the rest of the paint. Dr. Franklin imitated this, by pasting a slip of paper over the filleting of gold on the cover of a book, and sending an electric flash through it. The paper was torn off from end to end, with such force, that it was broken in several places; and in others there was brought away part of the grain of the Turkey leather in which the book was bound. This convinced the Doctor, that if it had been paint, it would have been stripped off in the same manner with that on the wainscot at Stretham. §

9. LIGHTNING destroys animal life. Animals have likewise been killed by the shock of electricity. The largest animals which Dr. Franklin and his friends had been able to kill were a hen, and a turkey which weighed about ten pounds. §

10. MAGNETS have been observed to lose their virtue, or to have their poles reversed by lightning. The same did Dr. Franklin by electricity. By electricity he frequently gave polarity to needles, and reversed them at pleasure. A shock from four large jars, sent through a fine sewing needle, he says, gave it polarity, so that it would traverse when laid on water. What is most re-

* Franklin's Letters, p. 124. † Ib. p. 63. ‡ Phil. Transf. Vol. 45. p. 387.

§ Ib. Vol. 45. p. 64. § Franklin's Letters, p. 86, 153.

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markable

markable in these electrical experiments upon magnets is, that if the needle, when it was struck, lay East and West, the end which was entered by the electric blast pointed North, but that if it lay North and South, the end which lay towards the North, would continue to point North, whether the fire entered at that end or the contrary; though he imagined, that a stronger stroke would have reversed the poles even in that situation, an effect which had been known to have been produced by lightning. He also observed, that the polarity was strongest when the needle was struck lying North and South, and weakest when it lay East and West. He takes notice that, in these experiments, the needle, in some cases, would be finely blued, like the spring of a watch, by the electric flame; in which case the colour given by a flash from two jars only might be wiped off, but that a flash from four jars fixed it, and frequently melted the needles. The jars which the Doctor used held seven or eight gallons, and were coated and lined with tinfoil.*

To demonstrate, in the compleatest manner possible, the sameness of the electric fluid with the matter of lightning, Dr. Franklin, astonishing as it must have appeared, contrived actually to bring lightning from the heavens, by means of an electrical kite, which he raised when a storm of thunder was perceived to be coming on. This kite had a pointed wire fixed upon it, by which it drew the lightning from the clouds. This lightning descended by the hempen string, and was received by a key tied to the extremity of it; that part of the string which was held in the hand being of silk, that the electric virtue might stop when it came to the key. He found that the string would conduct electricity even when nearly dry, but that when it was

* Franklin's Letters, p. 90, &c.

wet, it would conduct it quite freely; so that it would stream out plentifully from the key, at the approach of a person's finger. *

AT this key he charged phials, and from electric fire thus obtained, he kindled spirits, and performed all other electrical experiments which are usually exhibited by an excited globe or tube.

As every circumstance relating to so capital a discovery as this (the greatest, perhaps, that has been made in the whole compass of philosophy, since the time of Sir Isaac Newton) cannot but give pleasure to all my readers, I shall endeavour to gratify them with the communication of a few particulars which I have from the best authority.

THE Doctor, after having published his method of verifying his hypothesis concerning the sameness of electricity with the matter of lightning, was waiting for the erection of a spire in Philadelphia to carry his views into execution; not imagining that a pointed rod, of a moderate height, could answer the purpose; when it occurred to him, that, by means of a common kite, he could have a readier and better access to the regions of thunder than by any spire whatever. Preparing, therefore, a large silk handkerchief, and two cross sticks, of a proper length, on which to extend it; he took the opportunity of the first approaching thunder storm to take a walk into a field, in which there was a shed convenient for his purpose. But dreading the ridicule which too commonly attends unsuccessful attempts in science, he communicated his intended experiment to no body but his son, who assisted him in raising the kite.

THE kite being raised, a considerable time elapsed before there

* Franklin's Letters, p. 106

was any appearance of its being electrified. One very promising cloud had passed over it without any effect; when, at length, just as he was beginning to despair of his contrivance, he observed some loose threads of the hempen string to stand erect, and to avoid one another, just as if they had been suspended on a common conductor. Struck with this promising appearance, he immediately presented his knuckle to the key, and (let the reader judge of the exquisite pleasure he must have felt at that moment) the discovery was complete. He perceived a very evident electric spark. Others succeeded, even before the string was wet, so as to put the matter past all dispute, and when the rain had wet the string, he collected electric fire very copiously. This happened in June 1752, a month after the electricians in France had verified the same theory, but before he heard of any thing they had done.

BESIDES this kite, Dr. Franklin had afterwards an insulated iron rod to draw the lightning into his house, in order to make experiments whenever there should be a considerable quantity of it in the atmosphere; and that he might not lose any opportunity of that nature, he connected two bells with this apparatus, which gave him notice, by their ringing, whenever his rod was electrified.*

THE DOCTOR being able, in this manner, to draw the lightning into his house, and make experiments with it at his leisure; and being certain that it was in all respects of the same nature with electricity, he was desirous to know if it was of the positive or negative kind. The first time he succeeded in making an experiment for this purpose was the 12th of April 1753, when it appeared that the lightning was negative. Having found that

* Franklin's Letters, p. 112.

the clouds electrified negatively in eight successive thunder gusts, he concluded they were always electrified negatively, and formed a theory to account for it. But he afterwards found he had concluded too soon. For, on the sixth of June following, he met with one cloud which was electrified positively; upon which he corrected his former theory, but did not seem able perfectly to satisfy himself with any other. The Doctor sometimes found the clouds would change from positive to negative electricity several times in the course of one thunder gust, and he once observed the air to be strongly electrified during a fall of snow, when there was no thunder at all. *

BUT the grand practical use which Dr. Franklin made of his discovery of the sameness of electricity and lightning, was to secure buildings from being damaged by lightning, a thing of vast consequence in all parts of the world, but more especially in several parts of North America, where thunder storms are more frequent, and their effects, in that dry air, more dreadful, than they are ever known to be with us.

THIS great end Dr. Franklin accomplished by so easy a method, and by so cheap, and seemingly trifling apparatus, as fixing a pointed metalline rod higher than any part of the building, and communicating with the ground, or rather the nearest water. This wire the lightning was sure to seize upon, preferable to any other part of the building; whereby this dangerous power would be safely conducted to the earth, and dissipated, without doing any harm to the building. †

DR. FRANKLIN was of opinion, that a wire of a quarter of an inch of thickness would be sufficient to conduct a greater quantity of lightning than was ever actually discharged from the clouds

* Franklin's Letters, p. 112, &c. † Ib. p. 62, 124.

in one stroke. He found, that the gilding of a book was sufficient to conduct the charge of five large jars, and thought that it would probably have conducted the charge of many more. He also found by experiment, that if a wire was destroyed by an explosion, it was yet sufficient to conduct that particular stroke, though it was thereby rendered incapable of conducting another.*

THE Doctor also supposed, that pointed rods erected on edifices might likewise often prevent a stroke of lightning in the following manner. He says, that an eye so situated as to view horizontally the underside of a thunder cloud, will see it very ragged, with a number of separate fragments, or petty clouds, one under another, the lowest sometimes not far from the earth. These, as so many stepping stones, assist in conducting a stroke between a cloud and a building. To represent these by an experiment, he directs us to take two or three locks of fine loose cotton and connect one of them with the prime conductor, by a fine thread of two inches (which may be spun out of the same lock) another to that, and a third to the second, by like threads. He then bids us to turn the globe, and says we shall see these locks extending themselves towards the table (as the lower small clouds do towards the earth) but, that, on presenting a sharp point, erect under the lowest, it will shrink up to the second, the second to the first, and all together to the prime conductor, where they will continue as long as the point continues under them. A most ingenious and beautiful experiment! May not, he adds, in like manner, the small electrified clouds, whose equilibrium with the earth is soon restored by the point, rise up to the main body, and by that

* Franklin's Letters, p. 124, 125.

means occasion so large a vacancy, as that the grand cloud cannot strike in that place.*

MR. WILCKE, in his remarks on Dr. Franklin's letters, says, that on the 20th of August 1758, he saw this supposition verified; as he was viewing a large fringed cloud, strongly electrified, passing over a forest of tall fir trees. The ragged and depending parts of the large cloud were first attracted lower, and then suddenly rose higher, and joined the large cloud. †

He was also an eye witness of two clouds lying one over the other, approaching, and flashing into one another. The lightning spread itself over all the parts of the blacker cloud, which was negative, and which immediately began to dissolve in rain. ‡

* Franklin's Letters, p. 121, &c.

† Ib. p. 351.

‡ Ib. p. 259.

SECTION

SECTION III.

MISCELLANEOUS DISCOVERIES OF DR. FRANKLIN AND HIS FRIENDS IN AMERICA DURING THE SAME PERIOD.

DR. FRANKLIN, retaining the common opinion, that electrified bodies have real atmospheres of the electric fluid (consisting of particles at some distance from the surface of the body, but always going along with it) observed that these atmospheres and the air did not seem to exclude one another; though, he says, this be difficult to conceive, considering that they are generally supposed to repel one another.

AN electric atmosphere, he says, raised round a thick wire, inserted into a phial, drives out none of the air it contained; nor on withdrawing that atmosphere, will any air rush in, as he found by a very curious experiment, accurately made; whence he also concluded, that the elasticity of the air was not affected by it.*

THE experiment, as the Doctor informs me, was made with a small glass syphon, one leg passing through the cork into the bottle. The other leg had in it a drop of red ink, which rea-

* Franklin's Letters, p. 98.

dily moved on the least change of heat or cold in the air contained in the phial; but not at all on the air's being electrified.

He also made an experiment which would seem to prove the immobility, as we may say, of these atmospheres by any external force, if they have any existence at all; but others may think it is rather an argument against their existence. He electrified a large cork ball fastened to the end of a silk string three feet long; and, taking the other end in his hand, he whirled it round, like a sling, a hundred times in the open air, with the swiftest motion he could possibly give it; and observed, that it still retained its electric atmosphere, though it must have passed through eight hundred yards of air.*

To show that a body, in different circumstances of dilatation and contraction, is capable of receiving, or retaining more or less of the electric fluid on its surface, he made the following curious experiment. He electrified a silver can, in which was about three yards of brass chain, one end of which he could raise to what height he pleased, by means of a pulley and a silk cord. He suspended a lock of cotton by a silk string from the ceiling of the room, making it to hang near the cup; and observed, that every time he drew up the chain, the cotton approached nearer to the cup, and as constantly receded from it when the chain was let down. From this experiment it was evident, he says, that the atmosphere about the cup was diminished by raising the chain, and increased by lowering it; and that the atmosphere of the chain must have been drawn from that of the cup when it was raised, and have returned to it again when it was let down. †

To make electric atmospheres in some measure visible, the

* Franklin's Letters, p. 97.

† *Id.* p. 121.

Doctor used to drop rosin on hot iron plates held under bodies electrified; and, in a still room, the smoke would ascend, and form visible atmospheres round the bodies, making them look very beautiful. In trying in what circumstances, the repellency between an electrified iron ball, and a small cork ball would be altered, he observed, that the smoke of rosin did not destroy their repellency, but was attracted both by the iron and the cork.*

THE Doctor observed, that silver exposed to the electric spark would acquire a blue stain, that iron would seem corroded by it; but he could never perceive any impression made on gold, brass, or tin. The spots on the silver or iron were always the same, whether they received the spark from lead, brass, gold, or silver; and the smell of the electric fire was the same, through whatever bodies it was conveyed. †

WHILE we are attending to what was done by Dr. Franklin at Philadelphia, we must by no means overlook what was done by Mr. Kinnerley, the Doctor's friend, while at Boston in New England. Some of his observations, of which an account is given in the Doctor's letters, are very curious; and some later accounts, which he himself has transmitted to England, seem to promise, that, if he continue his electrical inquiries, his name, after that of his friend, will be second to few in the history of electricity.

HE first distinguished himself by re-discovering Mr. Du Faye's two contrary electricities of glass and sulphur, with which both he and Dr. Franklin were at that time wholly unacquainted. But Mr. Kinnerley had a great advantage over Mr. Du Faye; for, making his experiments in a more advanced state of the

* Franklin's Letters, p. 55. † Ib. p. 31, 98.

science, he saw immediately, that the two contrary electricities of glass and sulphur were the very same positive and negative electricities, which had just been discovered by Dr. Watson and Dr. Franklin.

HE observed, that a cork ball, electrified by a conductor from excited glass, would be attracted by excited amber and sulphur, and repelled by excited glass and china; that electrifying the ball with the wire of a charged phial, it would be repelled by excited glass, but attracted by excited sulphur; and that when he electrified it by sulphur or amber, till it became repelled by them, it would be attracted by the wire of the phial, and repelled by its coating. These experiments surprized him very much, but by analogy he was led to infer, *a priori*, the following paradoxes, as he calls them, which were afterwards verified by Dr. Franklin at his request.*

1. "If a glass globe be placed at one end of a prime conductor, and a sulphur one at the other, both being equally in good order, and in equal motion, not a spark of fire can be obtained from the conductor, but one globe will draw out as fast as the other gives in.

2. "If a phial be suspended on the conductor with a chain from its coating to the table, and only one of the globes be made use of at a time, twenty turns of the wheel, for instance, will charge it; after which, as many turns of the other wheel will discharge it; and as many more will charge it again.

3. "THE globes being both in motion, each having a separate conductor, with a phial suspended on one of them, and the chain fastened to the other; the phial will become charged, one globe charging positively, and the other negatively.

4. "THE phial being thus charged, hang it in like manner,

* Franklin's Letters, p. 99.

“ on the other conductor. Set both wheels a going again, and
 “ the same number of turns that charged it before will now
 “ discharge it, and the same number repeated will charge it
 “ again.

5. “ WHEN each globe communicates with the same prime
 “ conductor, having a chain hanging from it to the table, one
 “ of them, when in motion (but which I cannot say) will draw
 “ fire up through the cushion, and discharge it, through the
 “ chain; and the other will draw it up through the chain, and
 “ discharge it through the cushion.”*

WHEN Mr. Kinnerley was advising his friend to try the experiments with the sulphur globe, he cautions him not to make use of chalk on the cushion, telling him that some fine powdered sulphur would do better. And he expresses his hope that if the Doctor should find the two globes to charge the prime conductor differently, he would be able to discover some method of determining which it was that charged positively.

DR. FRANKLIN, when these experiments and conjectures were proposed to him, had no idea of their having any real foundation; but imagined, that the different attractions and repulsions observed by Mr. Kinnerley proceeded rather from the greater or smaller quantities of the electric fire, obtained from different bodies, than from its being either of a different kind, or having a different direction. But finding, upon trial, that the principal of Mr. Kinnerley's suppositions were verified by fact, he had no doubt of the rest. †

IN answer to the doubt of Mr. Kinnerley, whether the glass, or the sulphur electrified positively, the Doctor gave it as his opi-

* Franklin's Letters, p. 100.

† *Ib.* p. 102, 103.

nion,

nion, that the glass globe charged positively, and the sulphur negatively, for the following reasons.

1. BECAUSE, though the sulphur globe seemed to work equally well with the glass one, yet it could never occasion so large, and so distant a spark between his finger and conductor as when the glass globe was used. But what he adds to confirm this proof does not seem to be satisfactory. He supposes that bodies of a certain bigness cannot so easily part with the quantity of electric fluid which they have, and hold attracted within their substance, as they can receive an additional quantity upon their surface, by way of atmosphere; and that therefore so much could not be drawn out of the conductor, as might be thrown on it. *

2. HE observed that the stream or brush of fire, appearing at the end of the wire connected with the conductor, was long, large, and much diverging when the glass globe was used, and made a snapping or rattling noise; but that when the sulphur globe was used, it was short, small, and made a hissing noise. He also observed, that just the reverse of both these cases happened when he held the same wire in his hand, and the globes were worked alternately. The brush was large, long, diverging, and snapping or rattling, when the sulphur globe was turned; but short, small, and hissing, when the glass globe was turned. When the brush was long, large, and much diverging, it seemed to the Doctor, that the body to which it joined was throwing the fire out, and when the contrary appeared, it seemed to be drinking in. †

3. HE observed, that when he held his knuckle before the sulphur globe, while it was turning, the stream of fire between

* Franklin's Letters, p. 104. † Ib.

his

his knuckle and the globe seemed to spread on its surface, as if it flowed from the finger, but before the glass globe it was otherwise.

4. He observed that the cool wind (or what was called so) which is felt as coming from an electrified point, was much more sensible when the glass globe, than when the sulphur one was used. But these, though the best arguments which the senses can furnish, of the course of the electric fluid, the Doctor acknowledges were but hasty thoughts. Indeed, considering that the velocity of the electric fluid has been found, by experiment, to be, nearly, instantaneous in a circuit of many miles, it cannot be supposed that the eye should be able to distinguish which way it goes in the space of one or two inches.*

I SHALL conclude this article with observing that the experiments, which the Doctor made with globes of glass and sulphur, are much more easily exhibited by the conductor and insulated rubber of either of them, all the effects being the reverse of each other.

I MUST now, for the present, take leave of this ingenious writer and his friends, after having brought the history of their labours to the year 1754, and must return to see what was doing on the continent of Europe for two or three years preceding this date, while we left it to go over to America.

* Franklin's Letters, p. 105.

PERIOD X.

THE HISTORY OF ELECTRICITY, FROM THE TIME THAT DR. FRANKLIN MADE HIS EXPERIMENTS IN AMERICA, TILL THE YEAR 1766.

WE are now entering upon the last period into which the history of electricity divides itself, in which the great variety of matter presented to our view must oblige an historian to have recourse to the strictest method; for, otherwise, the narration would be extremely perplexed and disgusting. As this period contains the events of a larger space of time than most of the others, yet without any convenient resting place; as the business of electricity has been considerably multiplied in it, and a greater number of labourers have been employed in gathering in the harvest of discoveries, the seeds of which were sown by Dr. Watson, Dr. Franklin, and others, in the preceding periods; I am obliged to subdivide this into more distinct parts, but I hope they will not be found to be more than were necessary, in order to prevent confusion.

HOWEVER, this circumstance, of the great quantity and variety of materials furnished in this period, in proportion as it tends to embarrass an historian, and exercise his talent for proper distribution

bution and arrangement, is a striking demonstration of a truth, which must give the greatest pleasure to all the lovers of electricity and Natural Philosophy. If the progress continue the same in another period, of equal length, if the harvest of discoveries continue to be more plentiful, and the labourers proportionably more numerous; what a glorious scene shall we see unfolded, what a fund of entertainment is there in store for us, and what important benefits may be derived to mankind!

W HEN we consider the progress of electricity in the last century upon the half period into which the history of electricity divides itself, in which the greater part of matter presented to our view must oblige an historian to have recourse to the strictest method; for, otherwise, the narration would be extremely perplexed and distressing. As this period contains the events of a larger space of time than most of the others, yet without any convenient telling space; as the balance of electricity has been considerably multiplied in it, and a great number of labourers have been employed in gathering in the harvest of discoveries, the facts of which were told by Dr. Franklin, Dr. Franklin, and others, in the preceding periods; I am obliged to labours this task more distasteful, but I hope they will not be found to be more than were necessary, in order to prevent confusion.

However, this circumstance, of the great quantity and variety of materials furnished in this period, in proportion as it tends to embarrass an historian, and excite his talent for proper distribution

SECTION

SECTION I.

IMPROVEMENTS IN THE ELECTRICAL APPARATUS, WITH EXPERIMENTS AND OBSERVATIONS RELATING TO IT.

AS our electrical apparatus has been much improved within this period, I shall first recite what has occurred to me upon this subject; particularly the methods which have, from time to time, been communicated of increasing the power of electricity, by different circumstances of excitation.

So early as the year 1751, upon occasion of trying Mr. Winckler's experiments, notice is taken of Mr. Canton's method of rubbing tubes with silk prepared with linseed oil. These he had found, by the experience of some considerable time, to produce the greatest effect upon tubes, but he had not found that they were proportionably useful in rubbing globes.*

UPON another occasion, Mr. Canton observes, that by means of this rubber, a solid cylinder of glass, which had been set before the fire till it was quite dry, might be excited as easily as a glass tube, so as to act like one in every respect; that even the first stroke would make it strongly electrical. †

* Phil. Transf. Vol. 47. p. 239. † Ib. Vol. 48. pt. 2. p. 784.

B b BUT

BUT the greatest improvement which Mr. Canton discovered for increasing the power of electricity, was by rubbing on the cushion of the globe, or on the oiled silk rubber of the tube, a small quantity of an amalgam of mercury and tin, with a very little chalk or whiting. By this means, a globe or tube may be excited to a very great degree with very little friction, especially if the rubber be made more damp or dry as occasion may require.*

MR. WILCKE says, that a glass tube excited with a woollen cloth, on which some white wax or oil has been put, will throw out flames with a great noise in the dark. † These flames, he says, he never knew to be thrown from a globe, except sometimes when they were first used. ‡

OUR electrical apparatus has been much augmented within this period by the discovery of father Windelinus Ammerfin of Switzerland, who, in a Latin treatise, published at Lucern, in the year 1754, has shewn us, that wood properly dried, till it becomes very brown, is a non-conductor of electricity. He recommends boiling the wood in linseed oil, or covering it over with varnish, after being dried, to prevent any return of moisture into its pores; and adds, that wood, so treated, seems to afford stronger appearances of electricity than even glass. He himself made use of common wooden measures, such as are usually found in granaries, first boiled in oil, and afterwards mounted, so as to be turned by a wheel. ||

IT appears from the Philosophical Transactions, says, Mr. Wilson, so early as the year 1747, that Dr. Watson having occasion to support a long wire, in an experiment made near Shooter's hill, with a view to determine the velocity of the elec-

* Phil. Transf. Vol. 52. pt. 2. p. 461. † Wilcke, p. 124. ‡ Ib. p. 126.
|| Phil. Transf. Vol. 52. pt. 1. p. 342.

tric fluid, used stakes of dry wood, which he told him, were baked, to prevent the electric fluid from escaping into the ground.*

A MORE extraordinary method of procuring electricity than by baked wood, was one that Signior Beccaria made use of. He put a dry and warm cat's skin upon his glass globe, and rubbing it with his hand, excited a very powerful electricity. †

THESE wooden cylinders electrify positively or negatively as the rubber is silk or flannel, but much more powerfully when negative than when positive, owing to the roughness which there generally is upon their surfaces, and therefore make an agreeable variety in an electrical apparatus. But the oldest and most usual method of procuring negative electricity was by globes of sulphur. These Mr. Le Roi made by putting a coating of sulphur upon a globe of glass, and then smoothing it with an hot iron; but Mr. Nollet preferred melting the sulphur in the inside of the glass globe, and then breaking the glass from off it, because this method made a much finer polish. ‡

ONE globe he made of a mixture of sulphur and pounded glass, but he found that it had the same effect as if it had been all of sulphur. He says that, when one part of this globe was excited, the whole surface became electrical. §

BUT since Mr. Canton's discovery of the negative power of rough glass, some philosophers have made use of glass globes made rough by emery; and the usual method of taking off their polish was by rubbing them as they turned upon their axis; but Mr. Speedler, a mathematical instrument maker at Copenhagen, observes, in his letters upon the subject of electricity, that glass

* *Ib.* Vol. 51. pt. 2. p. 896. † *Lettere dell' Eletticismo*, p. 58.

‡ *Nollet's Letters*, Vol. 2. p. 121. § *Ib.* p. 125, 127.

globes, made rough by drawing the stone, or emery, from pole to pole, have a much greater virtue; this method of taking off the polish giving them a greater roughness with respect to the rubber.*

BUT a better, and a readier method than all these of producing negative electricity, is by insulating the rubber of a smooth globe, and connecting it with an insulated prime conductor, while the common conductor hath a communication with the ground. The rubber, if well insulated, is sure to produce a negative electricity, equal in power to the positive of the same globe. Mr. Dalibard directs a great number of precautions, in order to electrify well at the rubber, and to prevent it from receiving any electric fire in its state of insulation. †

MR. BERGMAN of Upsal, says, that very often, when his glass globes could not be excited to a sufficient degree of strength, he lined them with a thin coating of sulphur, and that then they gave a much stronger positive electricity than before. ‡

IN Italy, and other Places, Mr. Nollet informs us, it is the custom of electricians to put a coating of pitch, or other resinous matter on the inside of their globes, which they pretend, makes them always work well. §

WE are obliged to the Abbé Nollet for some observations on the electrical powers of different kinds of glass, in the sixth volume of his *Leçons de physique* printed in the year 1764.

IT is not every sort of glass, says he, that is equally electrifiable. There are some sorts which are not so at all, or hardly at all; such, for example, is that of which they make plates of glass at St. Gobin in Picardy. I have tried it, says he, an hundred times, in the form of plates, tubes, and globes, and in all

* Wilcke, p. 57. † Dalibard's Franklin, p. 110. ‡ Phil. Trans. Vol. 52. pt. 2. p. 485.
 § Lettres, Vol. 2. p. 122.

kinds

kinds of weather, but have scarce been ever able to draw from it the least sensible signs of electricity.

THE glass of which panes for windows are made, and which is also used for drinking glasses, when it is newly manufactured, is excited with great difficulty. I have often, says he, repeatedly rubbed tubes, and other pieces, even in the glass house where they were made, but without success; and it has not been till after some months, and sometimes years, that I could bring them to act.

It is certain, and he says he has constantly observed, that glass becomes more fit for electrical experiments by force of rubbing, and that sometimes it has required some months to bring globes and tubes to act well.

HE did not think that these facts could be accounted for either by the different degrees of transparency, or the different colours of glass. This, indeed, was evident from some globes acquiring electricity from use which had it not originally. The glass of which bottles are made at Severs served him very well, whereas globes of white glass did not become tolerable till after having been used a certain time.

HE could not tell positively why certain kinds of glass were electrizable or not by rubbing, but he suspected, that it was principally owing to the degree of its hardness and vitrification. He was induced to think so, because he found the glass at the French manufactory at St. Gobin, and at Cherbourg (the hardest, the most compact, and the best vitrified of all the kinds of glass in France) was the most difficult to be electrized; whereas the chrystal glass of England, that of Bohemia, &c. which are much softer, were the best of all for experiments in electricity. He says, moreover, that he had procured imperfect glasses, which had not been long enough in the furnace to be clear; and
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that, though they were of the same composition as plates of glass, which, he observed before, was not easily electrized, yet that these were excited very sensibly.

HE says that a globe of ten or twelve inches diameter, and which makes about four revolutions in a second of time, will receive a convenient rubbing; but that we must not expect that if the globe be one half, or one fourth part greater or less, that the effects will be increased or diminished in proportion. *

UPON the subject of insulating bodies, he observes, that when cakes of sulphur, rosin, sealing wax, and bees wax are made use of for this purpose, they ought to be well cooled before they are used: for, he says, he has constantly observed, that when they are newly made, they are not so proper to insulate bodies, as they generally are at the end of some months. †

IT will be proper, under this head, to acquaint young electricians, that globes have been several times known to burst during the act of excitation, and that the fragments have been thrown with great violence in every direction, so as to be very dangerous for the bystanders. This accident happened to Mr. Sabatelli in Italy, Mr. Nollet in France, Mr. Beraud at Lyons, Mr. Boze at Wittemburgh, Mr. Le Cat at Rouen, and Mr. Robein at Rennes.

THE air in the inside of Mr. Sabatelli's globe had no communication with the external air, but that of the Abbé Nollet had. This last, which was of English flint, which had been used two years, and which was more than a line thick, burst like a bomb in the hands of a servant who was rubbing it; and the fragments (the largest of which were not more than an inch in diameter) were dispersed on all sides, to a considerable distance. The

* *Leçons de Physique*. Vol. 6. p. 273---276.

† *Ib.* p. 299.

Abbé says, that all the globes which were burst in that manner exploded after five or six turns of the wheel; and he ascribes this effect to the action of the electric matter, making the particles of the glass vibrate in a manner he could not conceive.*

WHEN Mr. Beraud's globe burst (and he was the first to whom this accident was ever known to happen) he was making some experiments in the dark, on the 8th of February 1750; when a noise was first heard, as of something rending to pieces; then followed the explosion, and when the lights were brought in, it was observed, that those places of the floor which were opposite to the equatorial diameter of the globe were strewed with smaller pieces, and in greater numbers than those which were opposite to other parts of it. This globe had been cracked, but it had been in constant use in that state above a year, and the crack had extended itself from the pole to the equator. The proprietor ascribed the accident to the vibration of the particles of glass, and thought that the crack had some way impeded those vibrations. †

WHEN Mr. Boze's globe broke, he says that the whole of it appeared in the act of breaking, like a flaming coal; a circumstance which we shall see accounted for hereafter by Mr. Wilcke. ‡

MR. BOULANGER says, that glass globes have sometimes burst like bombs, and have wounded many persons, and that their fragments have even penetrated several inches into a wall. || He also says, that if globes burst in whirling by the gun barrel's touching them, they burst with the same violence, the splinters often entering into the wall. §

THE Abbé Nollet had a globe of sulphur which also burst, as

* Nollet's Letters, Vol. 1. p. 19. † Histoire, p. 87. ‡ Wilcke, p. 124.

|| Boulanger, p. 23. § Ib. p. 144.

he

he was rubbing it with his naked hands, after two or three turns of the wheel, having first cracked inwardly. It broke into very small pieces, which flew to a great distance; and into a fine dust, of which part flew against his naked breast; where it entered the skin so deep, that it could not be got off without the edge of a knife. *

* Noller's Letters, Vol. 2. p. 220.

SECTION

SECTION II.

OBSERVATIONS ON THE CONDUCTING POWER OF VARIOUS SUBSTANCES, AND PARTICULARLY MR. CANTON'S EXPERIMENTS ON AIR; AND SIGNIOR BECCARIA'S ON AIR, AND WATER.

ONE of the principal *defiderata* in the science of electricity, is to ascertain wherein consists the distinction between those bodies which are conductors, and those which are non-conductors of the electric fluid. All that has been done relating to this question, till the present time, amounts to nothing more than observations, how near these two classes of bodies approach one another; and before the period of which I am now treating, these observations were few, general, and superficial. But I shall now present my reader with several very curious and accurate experiments, which, though they do not give us satisfaction with respect to the great *defideratum* above-mentioned; yet throw some light upon the subject. They show that substances which had been considered as perfect conductors, or non-conductors, are so only to a certain degree; and that, probably, all the known parts of nature have, in some measure, the properties of both.

THESE experiments were made by two persons, whom, in the style of history, I may justly call two of the greatest heroes of

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this part of my work; viz. MR. CANTON, whose discoveries in electricity are far more numerous, and more considerable than those of any other person, within this period, in England; and SIGNIOR BECCARIA, one of the most eminent of all the electricians abroad.

THAT air was capable of receiving electricity by communication, and of retaining it when received, had not been discovered by any person before Mr. Canton; but, by the help of one of his exquisite contrivances, he was able to ascertain that delicate circumstance, and even measure the degree of it, if it was in the least considerable.

HE got a pair of balls, turned in a lathe, out of the dry pith of elder. These he put into a narrow box, with a sliding cover, so disposed that the threads (which were of the finest linen) were kept straight in the box. Holding this box by the extremity of the cover, the balls would hang freely from a pin in the inside. These balls, hung at a sufficient distance from buildings, trees, &c. easily show the electricity of the atmosphere. They also determine whether the electricity of the clouds and the air be positive, by the decrease; or negative, by the increase of their repulsion, at the approach of excited amber or sealing wax.

By the help of this instrument, he observed, that it was possible to electrify the air of a room near the apparatus; and even the air of the whole room in which it was, to a considerable degree, and he was able to do it both positively and negatively.

IN a paper read at the Royal Society, December the 6th. 1753, he observes, that the common air of a room might be electrified to a considerable degree, so as not to part with its electricity for some time. Having rendered the air of his room very dry, by means of a fire, he electrified a tin tube (with a pair of balls suspended at one of its extremities) to a great degree; when it appeared, that the
neigh-

neighbouring air was likewise electrified. For, having touched the tube with his finger, or other conductor, the balls, notwithstanding, continued to repel one another, though not at so great a distance as before. * But he observes that their repulsion would decrease as they were moved towards the floor, wainscot, or any of the furniture; and that they would touch each other when brought within a small distance of any conductor. Some degree of this electric power, he has known to continue in the air above an hour after the rubbing of the tube, when the weather had been very dry.

To electrify the air, or the moisture contained in it, negatively, Mr. Canton supported, by silk stretched between two chairs (placed back to back, at the distance of about three feet) a tin tube with a fine sewing needle at one end of it; and rubbed sulphur, sealing wax, or a rough glass tube as near as he could to the other end, for three or four minutes; after which he found the air to be negatively electrical, and that it would continue so a considerable time after the apparatus was removed into another room †.

In a paper dated November the 11th. 1754, he says, that dry air, at a great distance from the earth, if in an electric state, will continue so till it meets with some conductor, is probable from the following experiment. An excited glass tube, with its natural polish, being placed upright in the middle of a room (by putting one end of it in an hole, made for that purpose, in a block of wood) would, generally, lose its electricity in less than five minutes, by attracting to it a sufficient quantity of moisture, to conduct the electric fluid from all parts of its surface to the floor; but if, immediately after it was excited, it was placed, in the same manner, before a good fire, at the distance of about two feet, where no moisture would adhere to its surface, it would

* Phil. Transf. Vol. 49. pt. 1. p. 300.

† Ib. Vol. 48. pt. 2. p. 784.

continue electrical a whole day, and how much longer he knew not. *

SINCE the publication of the first edition of this work, Mr. Canton has hit upon another, much readier, and more powerful method of communicating electricity to the air than that described above. This he gives me leave to publish, and it appears to me to be of such a nature, as that it may very possibly lead to farther discoveries concerning the electricity of the atmosphere, and the phenomena depending upon it. "Take," says he, "a charged phial in one hand, and a lighted candle, insulated in the other; and, going into any room, bring the wire of the phial very near to the flame of the candle, and hold it there about half a minute: then carry the phial and candle out of the room, and return with the pith balls, suspended, and held at arms length. The balls will begin to separate on entering the room, and will stand an inch and half, or two inches a part, when brought near the middle of it."

SIGNIOR BECCARIA, who had no knowledge of what Mr. Canton had done, made the same discovery of the communication of electricity to the air, and diversified the experiment in a much more pleasing and satisfactory manner. He proves, that the air, which is contiguous to an electrified body, acquires, by degrees, the same electricity; that this electricity of the air counteracts that of the body, and lessens its effects; and that as the air acquires, so it also parts with this electricity very slowly.

He began his experiments by hanging linen threads upon an electrified chain, and observing, that they diverged the most after a few turns of his globe. After that, they came nearer to-

* Phil. Trans. Vol. 48. pt. 2. p. 784.

gether,

gether, notwithstanding he kept turning the globe and the excitation was as powerful as ever. *

WHEN he had kept the chain electrified a considerable time, and then discontinued the friction, the threads collapsed by degrees, till they hung parallel. After this, they began to diverge again, without any fresh electrification; and, if the air was still, this second divergence would continue an hour, or more.

THIS divergence was lessened by the electrification of the chain. For if the globe was turned again, the threads would first become parallel, and then begin to diverge again as before. Thus the second divergence of the threads took place, when the chain was deprived of its electricity, and when that which the air had acquired began to show itself.

WHILE the threads were beginning to diverge with the electricity of the air, if he touched the chain, and thereby took off what remained of its electricity, the threads would separate farther. Thus the more the electricity of the chain was lessened, the more did the electricity of the air appear.

WHILE the threads were in their second divergence, he hung two other threads, shorter than the former, by another silk thread to the chain; and when all the electricity of the chain was taken quite away, they would separate, like the former threads.

If he presented other threads to the former, in their second divergence, they would all avoid one another. †

IN this complete and elegant manner did Signior Beccaria demonstrate, that air actually receives electricity by communication, and loses it by degrees; and that the electricity of the air counteracts that of the body which conveys electricity to it.

* Lettere dell' Eletticismo, p. 87. † Ib. p. 90.

SIGNIOR BECCARIA also made a variety of other experiments, which demonstrate other mutual affections of the air and the electric fluid; particularly some that prove their mutual repulsion; and that the electric fluid, in passing through any portion of air, makes a temporary vacuum.

He brought the ends of two wires within a small distance of one another, in a glass tube, one end of which was closed, and the other immersed in water; and observed, that the water sunk in the tube, every time that a spark passed from the one to the other, the electric fluid having repelled the air. *

He made the electric explosion a great number of times, in the same air, inclosed in a glass tube, in order to ascertain whether the elasticity of the air was affected by it; but he could not find any alteration. After the operation, he broke the tube under water, but neither did any air make its escape, nor any water force its way into the tube. The experiment was made with all the precaution, with respect to heat and cold, that the nature of the case required. †

SIGNIOR BECCARIA'S experiments on *water*, showing its imperfection as a conductor, are more surprizing than those he made upon air, showing its imperfection in the contrary respect. They prove that water conducts electricity according to its quantity, and that a small quantity of water makes a very great resistance to the passage of the electric fluid.

He made tubes, full of water, part of the electric circuit, and observed, that when they were very small, they would not transmit a shock, but that the shock increased as wider tubes were used. ‡

BUT what astonishes us most in Signior Beccaria's experi-

* *Elettricità artificiale e naturale*, p. 116. † *Ib.* p. 91. ‡ *Ib.* p. 113.

ments with water, is his making the electric spark visible in it, notwithstanding its being a real conductor of electricity. Nothing, however, can prove more clearly how imperfect a conductor it is.

HE inserted wires, so as nearly to meet, in small tubes filled with water; and, discharging shocks through them, the electric spark was visible between their points, as if no water had been in the place. The tubes were generally broken to pieces, and the fragments driven to a considerable distance. This was evidently occasioned by the repulsion of the water, and its incompressibility, it not being able to give way far enough within itself, and the force with which it was repelled being very great.*

THE force with which small quantities of water are thus repelled by the electric fluid, he says, is prodigious. By means of a charge of four hundred square inches, he broke a glass tube two lines thick, when the pieces were driven to the distance of twenty feet. Nay he sometimes broke tubes eight, or ten lines thick, and the fragments were driven to greater distances in proportion. †

HE found the effect of the electric spark upon water greater than the effect of a spark of common fire on gunpowder; and says he does not doubt, but that, if a method could be found of managing them equally well, a cannon charged with water would be more dreadful than one charged with gunpowder. He actually charged a glass tube with water, and put a small ball into it, when it was discharged with great force, so as to bury itself in some clay he placed to receive it. ‡

* *Elettricismo artificiale e naturale*, p. 114. † *Lettere dell'elettricismo*, p. 74. ‡ *Ib.* p. 75, 76. MR. LULLIN says, he produced much greater effects than these, by making the electric spark visible in oil instead of water. Oil being a much worse conductor, the spark in it would be larger. *Dissertatio Physica*, p. 26.

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THIS resistance which small quantities of water make to the electric matter, he imagined, was greater than the resistance made to it by air. * And yet he thought it was possible, that, in this case, the electric matter might not act upon the water immediately, but upon the fixed air that was in it. For when the tubes were not broken, he observed that a great number of air bubbles were let loose, through the whole mass of the water, rose to the top, and mixed with the common atmosphere. †

HE also imagined that the electric fluid acted upon the fixed air in all bodies, though no experiment could make it sensible. ‡

ON the contrary, he supposed that the action of the electric matter tended to fix elastic air, by exciting a sulphureous matter, which Dr. Hales shows to have that property. || But the experiment above-mentioned, of the electric spark taken in a closed tube, doth not favour this supposition.

WHEN a small drop of water was put between the points of two wires, and a large shock passed through them, the water was equally dispersed on the inside of a glass sphere, in which they were all inclosed. In the same manner, he conjectures, that the action of the electric matter promotes the evaporation of water. §

DISCHARGING a shock through a quantity of water, poured on a flat surface, where some parts of the circuit were purposely left almost dry; those parts became quite dry sooner than they would have been, if no shock had passed through them. *

UPON this principle he accounts for the supposed bursting of the blood vessels in small birds killed by the electric shock. † And when a muscle contracts by the shock, he supposes it is ow-

* *Elettricismo artificiale*, &c. p. 115. † *Ib.* p. 116. ‡ *Ib.* p. 83. || *Ib.*

§ *Ib.* p. 117. * *Ib.* p. 121. † *Ib.* p. 128.

ing to the dilatation of the fluids their fibres contain, as the electric matter passes through them.

So imperfect a conductor of electricity is mere water, that, he thought, a green leaf conducted a shock better than an equal thickness of water. * If this be true, and vegetable fluids conduct electricity better than water, it will confirm a conjecture which Dr. Franklin told me, he had drawn from some experiments that he had not properly pursued, viz. that animal fluids conducted electricity better than water. He tried milk many years ago; and Mr. Kinnerley, and others in America, have since tried blood and urine, and also the sinews of animals newly killed; and they were all found to be exceeding good conductors, remarkably better than water.

SIGNIOR BECCARIA also found, that even *metal* was not a perfect conductor of electricity, but made some resistance to the passage of the electric fluid. This he even ascertained, by measuring the time that it was retarded, in its passing through long and small wires, notwithstanding the experiments which had been made before, that seemed to prove the contrary.

HE suspended a wire of 500 Paris feet, in a large building, and, by means of a pendulum which vibrated half seconds, observed, that light bodies placed under a ball of gilt paper, at one end, did not move, till, at least, one vibration of this pendulum, after he had applied the wire of a charged phial to the other.

TRYING the same with a hempen cord, he could count six, or more vibrations before they would stir; but when he had wetted the cord, they were moved after two or three vibrations. † He does not, however, absolutely say that, the elec-

* *Elettricismo artificiale, &c.* p. 135 † *Ib.* p. 51.

tric fluid must have taken up all this time in its progress, as it might require a certain quantity of the fluid, before it could raise the light bodies. But he did imagine, that it moved with more velocity, in proportion as the bodies into which it passed had more or less of the fluid before.* And he was confirmed in this opinion by several phenomena of the atmosphere, which will be related in their proper place; particularly by seeing, very evidently, the progress of a quantity of electric matter in the air, as it advanced to strike his kite.

To these experiments of Signior Beccaria on the conducting powers of air and water, I shall subjoin another curious set of the same author, showing the manner in which the smoke of rosin and of colophonia is affected by the approach of an electrified body, as they have a very near affinity to this subject.

REPEATING Dr. Franklin's experiments to make electric atmospheres visible with the fume of colophonia, which he preferred, for this purpose, to rosin; he observed several curious circumstances, which had escaped the notice of that ingenious philosopher.

HE heated the colophonia on a coal, which he held in a spoon under an electrified cube of metal; and observed, that when part of the smoke ascended to the cube, another part covered the handle of the spoon, and spread to his hand. †

THE smoke lay higher on the flat parts of the cube than on the edges, and corners.

IF a spark was taken from the conductor, the smoke was thrown into an agitation, but presently resumed its former situation.

* *Elettricismo artificiale*, p. 51. † *Ib.* p. 72.

THE cube with its atmosphere gave larger, and longer sparks, than a cube not surrounded with one.

A LARGER spark might be taken from it by the spoon, than by any other body.

HAVING insulated the spoon, he observed, that hardly any of the smoke went to the cube; and that what happened to go near it was not affected by it, any more than it would have been by any other body. He put his finger to the spoon, and the former phenomena returned. Taking it off again, the smoke that had settled on the cube soon dispersed. *

UNDER this head of the electricity of various substances, it will not be improper to mention an experiment made by Mr. Henry Eeles of Lismore in Ireland, which, he thought, proved that steam, and exhalations of all kinds, are electrical. The paper containing this account was read at the Royal Society, April the 23d. 1755.

He electrified a piece of down, suspended on the middle of a long silk string, and made steam and smoke of several kinds pass under it, and through it; and observed, that its electricity was not in the least diminished, as he thought it would have been, if the vapour had been non-electric, and consequently had taken away with it part of the electric matter with which the down was loaded. He observed that the effect was the same, whether the down was electrified with glass or wax, which he thought was not easy to be accounted for. †

To this experiment Dr. Darwin of Litchfield, in a letter addressed to the Royal Society, and read May the 5th. 1757, answers; that many electrified bodies, and particularly all light, dry, animal and vegetable substances, will not easily part

* Elettricità artificiale, p. 73, 74. † Phil. Transf. Vol. 49. pt. 1. p. 153.

with their electricity, though they be touched, for a considerable time, with conductors. He touched a feather, electrified like that of Mr. Eccles, nine times with his finger, and still found it electrified. A cork ball was touched seven times in ten seconds before it was exhausted. *

MR. KINNERSLEY of Philadelphia, in a letter dated March 1761, informs his friend and correspondent Dr. Franklin, then in England, that he could not electrify any thing by means of steam from electrified boiling water; from whence he concluded, that, contrary to what had been before supposed by himself and his friend, steam was so far from rising electrified, that it left its share of common electricity behind. †

To try the effects of electricity upon air, Mr. Kinnersley contrived an excellent instrument, which he calls an *electrical air thermometer*. It consisted of a glass tube, about eleven inches long, and one inch in diameter, made air tight, closed with brass caps at each end, and a small tube, open at both ends, let down through the upper plate, into some water at the bottom of the wider tube. Within this vessel he placed two wires, one descending from the brass cap at the upper end, and the other ascending from the brass cap at the lower end; through which he could discharge a jar, or transmit a spark, &c. and at the same time see the expansion of the air in the vessel, by the rise of the water, in the small tube. With this instrument he made the following experiments, related in a letter to Dr. Franklin, dated March the 12th. 1761.

He set the thermometer on an electric stand, with the chain fixed to the prime conductor, and kept it well electrized a considerable time; but this produced no considerable effect:

* Phil. Transf. Vol. 50. pt. 1. p. 252. † Ib. Vol. 53. pt. 1. p. 84.

from whence he inferred, that the electric fire, when in a state of rest, had no more heat than the air, and other matter wherein it resides.

WHEN the two wires within the vessel were in contact, a large charge of electricity, from above thirty square feet of coated glass, produced no rarefaction in the air; which showed, that the wires were not heated by the fire passing through them.

WHEN the wires were about two inches asunder, the charge of a three pint bottle, darting from one to the other, rarefied the air very evidently; which shewed, that the electric fire produced heat in itself, as Mr. Kinnerley says, as well as in the air, by its rapid motion.

THE charge of a jar which contained about five gallons and a half, darting from wire to wire, would cause a prodigious expansion in the air; and the charge of his battery of thirty square feet of coated glass would raise the water in the small tube quite to the top. Upon the coalescing of the air, the column of water, by its gravity, instantly subsided, till it was in equilibrium with the rarified air. It then gradually descended, as the air cooled, and settled where it stood before. By carefully observing at what height the descending water first stopped, the degree of rarefaction, he says, might be discovered, which, in great explosions, was very considerable.

IT is obvious to remark, that the first sudden rise of the water of Mr. Kinnerley's thermometer, upon an explosion being made in the vessel which contained it, is not to be ascribed to the rarefaction of the air by heat, but to the quantity of air actually displaced by the electrical flash. It is only when that first sudden rise is subsided, as Mr Kinnerley himself observes, that

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the degree of its rarefaction by the heat can be estimated, viz. by the height at which the water then stands above the common level.

DR. FRANKLIN had said, that *ice* failed to conduct a shock of electricity; and Mr. Bergman, in a letter to Mr. Wilson, read at the Royal Society November the 20th. 1760, shows (what Signior Beccaria had done before) that a small quantity of water failed as much as the ice had done with Dr. Franklin, who seems to have made use of an icicle which, Mr. Bergman thought, was not large enough for the purpose. From hence he suspected that large quantities of ice would transmit a shock of electricity as perfectly as a large quantity of water. *

HOWEVER, he seems, afterwards, to have changed his sentiments with respect to ice: for, in a subsequent paper, read at the Royal Society March the 18th. 1762, when he had remarked that snow would not conduct the electric shock; he says, he believes, if he could procure plates of ice of a proper thickness, he could charge them in the same manner as glass. †

JOHANNES FRANCISCUS CIGNA was so fully persuaded of the non-conducting power of ice, that he made use of it in an experiment, designed to ascertain whether electric substances did, according to Dr. Franklin's hypothesis, contain more of the electric matter than other bodies. He inclosed a quantity of ice in a glass vessel, and when he thought he had converted it from an electric to a non-electric by melting; he tried whether it was electrified; but, though he found no appearance of its having acquired any more of the fluid than it ought to have in its new state, he does not seem to have given up his opinion. ‡

* Phil. Transf. Vol. 51. pt. 2. p. 908. † Ib. Vol. 52. pt. 2. p. 485.

‡ Memoirs of the Academy at Turin, for the year 1765. p. 47.

In the last part of this work the reader will find some experiments, which, it is imagined, will ascertain the class of bodies in which ice ought to be ranked, by proving its conducting power to be, at least, nearly equal to that of water.

SECTION

SECTION III.

MR. CANTON'S EXPERIMENTS AND DISCOVERIES RELATING TO THE SURFACES OF ELECTRIC BODIES, AND OTHERS MADE IN PURSUANCE OF THEM, OR RELATING TO THE SAME SUBJECT; ALL TENDING TO ASCERTAIN THE DISTINCTION BETWEEN THE TWO ELECTRICITIES.

TILL this last period of the history, the same electricity had always been produced by the same electric. The friction of glass had always produced a positive, and the friction of sealing wax, &c. had always produced a negative electricity. These were thought to be essential, and unchangeable properties of those substances; and hence the one was by many called the vitreous, and the other the resinous electricity; and to electrify negatively, or produce a resinous electricity by means of glass; or to electrify positively, that is, produce a vitreous electricity, by means of sealing wax, &c. would have been thought as great a paradox, as to electrify at all by the friction of brass or iron. For though it was not known why the electric matter should flow from the rubber to the excited glass, or to the rubber from excited sealing wax, the fact had been invariable; and nothing is even mention-
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ed to have happened, in the course of any experiments, that could lead a person to suspect the possibility of the contrary.

WHAT then must have been the surprise of electricians, to find that these different powers of glass and sulphur were so far from being invariable, that they were even interchangeable; and that the same glass tube could be made to assume the powers of both! And what must have been their satisfaction to find the circumstance on which the convertibility of those opposite powers depended, completely ascertained. This surprise and pleasure was given them by Mr. Canton, who showed that it depended only on the rubber, and the surface of the glass, whether it electrified positively or negatively.

IN what manner, by what train of thought, or by what accident he was led to this discovery, this excellent philosopher has not been pleased to inform us; but it is certainly a discovery which, in an eminent manner, distinguishes this period of my history. It throws great light upon the doctrine of positive and negative electricity, and led the way to other discoveries which throw still more light upon it.

THIS subject of the two electricities seems to have engaged the attention of electricians in a more particular manner, in the whole course of this period, and ever since the discovery of Dr. Franklin, that the electricity of the two surfaces of charged glass are always contrary to one another. Accordingly, the reader will find several sections in this period of the history relating to it; but he will find that though much has been done, much yet remains to be done; and that we are still far from thoroughly understanding the nature of the two electricities, with their dependence upon and relation to one another.

PREVIOUS to the communication of the discovery itself, Mr. Canton observes, that sealing wax might have positive electricity

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superinduced

superinduced upon it. He excited a stick of sealing wax about two feet and a half in length, and an inch in diameter; and, holding it by the middle, he drew an excited glass tube several times over one part of it, without touching the other. The consequence was, that half which had been exposed to the action of the excited glass was positive, and the other half negative: for the former half destroyed the repelling power of balls electrified by glass, while the other half increased it.*

THE experiments, which prove that the appearances of positive and negative electricity depend upon the surface of the electrics, and that of the rubber, were made in the latter end of December 1753.

HAVING rubbed a glass tube with a piece of thin sheet lead, and flour of emery mixed with water, till its transparency was intirely destroyed, he excited it (when it was made perfectly clean and dry) with new flannel, and found it act in all respects like excited sulphur or sealing wax. The electric fire seemed to issue from the knuckle, or end of the finger, and to spread itself on the surface of the tube, in a very beautiful manner.

IF this rough or unpolished tube was excited by a piece of dry oiled silk, especially when rubbed over with a little chalk or whiting, it would act like a glass tube with its natural polish. In this case the electric fire appeared only at the knuckle, or the end of the finger, where it seemed to be very much condensed, before it entered.

BUT if the rough tube was greased all over with tallow from a candle, and as much as possible of it wiped off with a napkin, then the oiled silk would receive a kind of polish by rubbing it;

* Phil. Transf. Vol. 48. pt. 1. p. 356.

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and; after a few strokes, would make the tube act in the same manner as when excited at first by flannel.

THE oiled silk, when covered with chalk or whiting, would make the greased rough tube act again like a polished one; but if the friction was continued till the rubber became smooth, the electric power would be changed to that of sulphur, sealing wax, &c.

THUS, says he, may the positive and negative powers of electricity be produced at pleasure, by altering the surfaces of the tube and rubber, according as the one or the other is most affected by the friction between them. For if the polish be taken off one half of a tube, the different powers may be excited with the same rubber at a single stroke; and, he adds, the rubber is found to move much easier over the rough, than over the polished part of it.

THAT polished glass electrified positively, and rough glass rubbed with flannel negatively, seemed plain from the appearance of the light between the knuckle, or end of the finger, and the respective tubes. But this, Mr. Canton thought, was farther confirmed by observing, that a polished glass tube, when excited by smooth oiled silk, if the hand was kept three inches, at least, from the top of the rubber, would, at every stroke, appear to throw out a great number of diverging pencils of electric fire; but that none were ever seen to accompany the rubbing of sulphur, sealing wax, &c. nor was he ever able to make any sensible alteration in the air of a room merely by the friction of those bodies; whereas the glass tube, when excited so as to emit pencils, would, in a few minutes, electrify the air, to such a degree, that, after the tube was carried away, a pair of balls, about the size of the smallest peas, turned out of cork, or the pith of elder, and hung to a wire by linen threads of six inches long, would

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repel each other to the distance of an inch and an half, when held at arms length in the middle of the room. *

AFTER these experiments of Mr. Canton, Mr. Wilson made several, which throw a little more light upon this curious subject; but it is difficult to draw any general conclusion from them, and his own is not sufficiently determinate. It is, that two electrics being rubbed together, the body whose substance is hardest, and electric power strongest, will always be *plus*, and the softest and weakest *minus*. † Rubbing the tourmalin and amber together, he produced a *plus* electricity on both sides the stone, and a *minus* on the amber; but rubbing the tourmalin and diamond together, both sides of the tourmalin were electrified *minus*, and the diamond *plus*.

THESE experiments, which, he thought, proved this proposition, encouraged him to try what would be the effect of rubbing or forcing air against different electrics, and the effects were very considerable. In these experiments he only made use of a common pair of bellows, and his first experiment was upon the tourmalin. This substance he brought near the end of the pipe, and found, that after it had received about twenty blasts, it was electrified *plus* on both sides. Air, therefore, seemed to be less electric than the tourmalin.

INTO the place of the tourmalin, he brought a pane of glass, and blew against it the same number of times as in the former experiment; and when he had examined both sides, he found that they were electrified *plus* also, but less than the tourmalin.

AMBER, treated in the same manner, was electrified less than the glass.

* Phil. Transf. Vol. 43. pt. 2. p. 782. † Ib. Vol. 51. pt. 1. p. 331.

HE next had recourse to a smith's bellows. The difference which these occasioned was only a much stronger electricity in the tourmalin. Amber was still weaker than the glass; and the glass weaker than the tourmalin.

HAVING in view the medium (which, I have observed, he laid great stress upon, as constituting the difference between electrics and non-electrics) he considered that heat would rarify it on the surfaces of the particles of air; by which means, air, having its resistance lessened, would more readily part with the electric fluid, and, of consequence, electrify more powerfully.

THE pipe of the bellows being made red hot, he blew against the tourmalin, twelve times only, which was eight times less than in the former experiment with cold air. In this experiment the tourmalin was electrified *plus* on both sides, but to a considerable degree more than was done in the former experiments. The hot air had the same effect upon glass, but electrified it less than the tourmalin: and amber; though, like the other bodies, it received an increase of power by the same treatment, was electrified the least of all.

FROM the air electrifying more powerfully when it was hot than when it was cold, and the tourmalin being electrified more than glass, and glass more than amber, as appeared by the last experiments; we seem, says Mr. Wilson, to have obtained a proof, that the whole atmosphere is constantly promoting a flow of the electric fluid, by the alternate changes of heat and cold; and further, that air is not only less electric than the tourmalin, but less than glass, or even amber.*

IN another paper, read at the Royal Society November the

* Phil. Transf. Vol. 51. pt. 1. p. 332. &c.

13th. 1760, Mr. Wilson recites some curious experiments, which, he says, shew that a *plus* electricity may be produced by means of a *minus* electricity.

HAVING electrified the inside of a large Leyden bottle *plus*, by means of a conducting wire from an excited glass globe; he set it on a stand of prepared wood, and took away the conducting wire, after which the mouth of the bottle was closed with a stopple of glass. Then the pointed end of an ivory conductor was brought opposite to the middle of the bottle, and about two inches from it. Upon doing this, the balls were electrified *minus*; and the more so as the ivory was moved nearer the bottle, in an horizontal direction.

BUT, on removing the ivory to a greater distance, the *minus* electricity decreased; and, at a certain distance, there was not any sign of it remaining; but when the distance was increased to about eighteen inches from the bottle, a *plus* electricity appeared, which, continued even after the ivory was removed entirely away.*

WITH a cylinder of baked wood he electrified the balls hanging to the ivory *minus*, at the distance of four feet or more, by holding the cylinder over the middle of the ivory, and continuing it there; and, on moving it nearer, they were more strongly electrified *minus*, but the same cylinder, on removing it back again to the distance of two or three feet, or more, electrified the balls *plus*.

WHEN another conductor of metal, without edges or points, was used, instead of the ivory, and without any thing hanging from it, the same cylinder held over the metal (as was done in the last experiment over the ivory, at the distance of two feet)

* Phil. Transf. Vol. 51. pt. 2. p. 899, &c.

produced

produced a *plus* electricity; and this was rendered weaker as the cylinder was moved nearer; but by lessening the distance to about one foot, the *minus* electricity took place. In these cases Mr. Wilson thought, that the *plus* appearance arose from the earth, air, or other neighbouring bodies.

WHEN the preceding experiments were first made, he was a little embarrassed, by the uncertain appearances of a *plus* electricity at one time, and a *minus* at another, in the same experiment; but, by repeated trials and observations, he found, that a *plus* or *minus* electricity may be produced at pleasure, by carefully attending to the three following circumstances; viz. the form of the bodies, their sudden or gradual removal, and the degrees of electrifying.

MR. WILSON, after this, proceeds to mention some other circumstances of a very nice nature, where, the slightest and almost imperceptible differences in the position or in the course of the friction of two bodies produce, in either of them, the *plus* electricity at one time, and the *minus* at another. Such, says he, are the effects of this subtle and active fluid, when the experiments are carefully made; and therefore they require the most scrupulous attention to trace out the causes which occasion them.

SEALING WAX and silver were the bodies used in the two first experiments, but many other substances seemed to perform as well. The sealing wax was clean, and undisturbed by any friction whatever, but that of the air surrounding it, and had been so for some hours. The silver was fixed to a piece of prepared wood, which was also preserved from friction for the same length of time. Then, taking one of those substances in each hand, the silver being at the end of the wood the farthest from the hand, he laid the smoothest part of the silver upon the sealing wax, and moved it along the surface gently, once only, and
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with a very slight pressure, after which the silver was electrified *plus*, and the wax *minus*.

ON repeating the experiment with equal care, and in the same manner, except that the smooth side of the silver was a little inclined, so that the edge of it pressed against the wax; the silver, after moving it as before, was electrified *minus*, and the wax *plus*, contrary to what was observed in the last experiment.

THESE opposite effects, occasioned by the different applications of the *flat* or *edge* of the silver, seemed to arise from an alteration made in the surface of the wax, by destroying the polish in one case, and not in the other; and in this respect resembled the polished and rough glass mentioned before.

UPON making use of prepared wood instead of wax, and employing different degrees of pressure in the friction, with the same edge of the silver, he produced the like appearances; the least pressure causing a *plus*, and the greatest pressure a *minus* appearance in the silver.

A FLAT piece of steel well polished, and the edges rounded off, afforded the same appearances, by only applying the flat surface to the wood, but it required more pressing to produce the *minus* effect in this case than it did in the former, where the edge was concerned.

WHETHER the reason offered above for explaining these last curious appearances be true or not, Mr. Wilson did not venture to affirm, for want of farther experiments; but thus much he thought might be safely advanced, that we have learned to produce at pleasure a *plus* or *minus* electricity from the same bodies, by attending to the manner of their application and friction.*

* Phil. Transf. Vol. 51. pt. 2. p. 899, &c.

MR. BERGMAN, in a letter to Mr. Wilfon, read at the Royal Society February the 23d. 1764, gives an account of some curious experiments of his, which, in conjunction with those of Mr. Canton above-mentioned, concerning surfaces, may throw considerable light upon the doctrine of positive and negative electricity.

THE experiments were made with two pieces or skains of silk, one of which was extended in a frame, while Mr. Bergman held the other in his hand. He observed, that if the two skains of silk were the same with respect to texture, colour, superficies, and in every thing else, as far as could be judged; and if he drew the whole length of the piece which he held in his hand over one part of that which was extended in the frame, the piece in his hand contracted the positive electricity, and the piece in the frame a negative. If he drew one part of that which he held in his hand over the whole length of the other the effects were reversed.

If the piece in his hand was of a different colour from that in the frame (provided it was not black) the event was the same.

If the piece in his hand was black, it was always negative, which ever way it was rubbed, except the piece in the frame was black too; for then, if the whole length of it was rubbed, it was electrified positively.

IN endeavouring to account for these effects, he observes, that the piece which was most rubbed was made *smoother*, and *warmer* than the other; and was of opinion, that though smoothness did dispose bodies to be excited positively, yet that other circumstances were also to be taken into consideration; having found that when he held in his hand a skain of silk, which, by much friction, was made very smooth, and drew it over one part of another skain, which was rough, and had never been used be-

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fore, that the rough skain was, nevertheless, positive. From this experiment he concluded, that this effect was, in some measure, owing to the colour; and, in pursuing this thought farther, he was led to the following experiments.

If the piece in his hand was well-warmed, though it was drawn over one part of the piece in the frame, it became electrified negatively, and the piece in the frame positively. He made these experiments with the same success upon skains of silk of various colours, blue, green, red, white, &c.

If the piece in the frame was black, it never contracted a positive electricity, although the piece in his hand had been much heated, except this were black too. From these experiments, he thought he might safely conclude, that heat did dispose some substances, at least, to a negative state; and thought that the want of attention to this circumstance might have occasioned mistakes in the event of some experiments, especially those concerning island chrystal.

From the whole he concludes, that there is a certain fixed order with respect to negative and positive electricity, in which all bodies may be placed, while other circumstances remain the same. Let A, B, C, D, E be certain substances, each of which, when rubbed with one which is antecedent to it, is negative, but with a subsequent positive. In this case, the less distance there is between the bodies that are rubbed, the weaker, *cet. par.* will be the electricity; wherefore it will be stronger between A and E, than it will be between A and B. Heat, he says, disposes bodies to a negative electricity, but if the distance above-mentioned be considerable, it may not be able to overcome, though it may weaken that electricity, as is evident from the skains of black silk. When a glass globe grows warm in whirling, we are sensible that its electric power is diminished. Is it not owing,
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says he, to this circumstance, that by heat it is more disposed to negative electricity, by which means the distance above-mentioned between the glass and the rubber is lessened?*

UPON the subject of this section, I must introduce to the acquaintance of my reader two eminent electricians, whose discoveries will give him the greatest satisfaction; I mean Mr. WILCKE, and ÆPINUS, the former of Rostock in Lower Saxony, and the latter of Peterburgh: a circumstance which gives me an occasion of congratulating all the lovers of the sciences, and particularly of electricity, on the extensive spread of their favourite studies. What joy would it have given Mr. Hawkesbee, or Mr. Grey, to have foreseen that two such admirable treatises on the subject of electricity, as those of the persons above-mentioned, would come from countries so remote from the place of its rise!

MR. WILCKE relates many curious experiments concerning the generation of what he calls *spontaneous electricity*, produced by the liquifaction of electric substances, which, compared with those of Mr. Canton, throw great light upon the doctrine of positive and negative electricity.

He melted sulphur in an earthen vessel, which he placed upon conductors; then, letting them cool, he took out the sulphur, and found it strongly electrical; but it was not so when it had stood to cool upon electric substances.

He melted sulphur in glass vessels, whereby they both acquired a strong electricity in the circumstances above-mentioned, whether they were placed upon electrics or not; but a stronger in the former case than in the latter; and they acquired a stronger virtue still, if the glass vessel into which they were poured

* Phil. Transf. Vol. 54. p. 86.

was coated with metal. In these cases, the glass was always positive, and the sulphur negative. It was particularly remarkable, that the sulphur acquired no electricity till it began to cool, and contract, and was the strongest when in the state of greatest contraction; whereas the electricity of the glass was, at the same time, the weakest; and was the strongest of all when the sulphur was shaken out, before it began to contract, and acquired any negative electricity.

PURSuing these experiments, he found that melted sealing wax, poured into glass, acquired a negative electricity, but poured into sulphur it acquired a positive electricity, and left the sulphur negative. Sulphur poured into baked wood became negative. Sealing wax also poured into wood was negative, and the wood consequently positive; but sulphur poured into sulphur, or into rough glass, acquired no electricity at all.*

EXPERIMENTS similar to these were also made by *Æpinus*. He poured melted sulphur into metal cups, and observed that when the sulphur was cool, the cup and the sulphur together showed no signs of electricity, but showed very strong signs of it the moment they were separated. The electricity always disappeared when the sulphur was replaced in the cup, and revived upon being taken out again. The cup had acquired a negative, and the sulphur a positive electricity; but if the electricity of either of them had been taken off while they were separate, they would both, when united, show signs of that electricity which had not been taken off. This electricity, he observes, was only on the surface of the sulphur. †

MR. WILCKE has, likewise, recited several curious experiments, which he made on the friction of various substances, which likewise throw considerable light on the same subject.

* Wilcke, p. 44, &c. † *Æpini Tentamen*, p. 66, 70.

SULPHUR and glass rubbed together produced a strong electricity, positive in the glass, and negative in the sulphur.

SULPHUR and sealing wax being rubbed together, the wax became positive, and the sulphur negative.

WOOD rubbed with cloth was always negative.

WOOD rubbed against smooth glass became negative, but against rough glass positive.

SULPHUR rubbed against metals was always positive, and this was the only case in which it was so; but being rubbed against lead it became negative, and the metal positive; lead appearing, thereby, to be not so good a conductor as the other metals.

AFTER these experiments, Mr. Wilcke gives the following catalogue of the principal substances with which electrical experiments are made, in the order in which they are disposed to acquire positive or negative electricity; any of the substances becoming positively electric when rubbed with any that follow it in the list, and negative when rubbed with any that precede it.

Smooth glass.	White wax.
Woollen cloth.	Rough glass.
Quills.	Lead.
Wood.	Sulphur.
Paper.	Other metals. *
Sealing wax.	

IN all experiments made to determine the order of these substances, Mr. Wilcke says, that great care is necessary, to distinguish original electricity from that which is communicated, or the consequence of friction. †

* Wilcke, p. 54, &c. † Ib. p. 69.

MR. WILCKE says that smooth glass is in all cases positive, and thence infers that it attracts the electric fluid the most of all known substances; but Mr. Canton tells me he has found, that the smoothest glass will acquire a negative electricity by being drawn over the back of a cat.

OF the same nature with these experiments of Mr. Wilcke are the following of *Æpinus*. He pressed close together two pieces of looking glass, each containing some square inches; and observed, that when they were separated, and not suffered to communicate with any conductor, they each acquired a strong electricity, the one positive, and the other negative. When they were put together again, the electricity of both disappeared, but not if either of them had been deprived of their electricity when they were asunder; for in that case, the two, when united, had the electricity of the other. The same experiment, he says, may be made with glass, and sulphur, or with any other electrics, or with any electric and a piece of metal. *

* *Æpini Tentamen*, p. 65.

SECTION

SECTION IV.

MR. DELAVAL'S EXPERIMENTS RELATING TO THE TWO ELECTRICITIES, AND HIS CONTROVERSY WITH MR. CANTON UPON THAT SUBJECT.

MR. CANTON, in the course of experiments related in the preceding section, clearly proved, that the production of either of the two electricities depends intirely upon the surface of the excited electric with respect to the rubber, and showed, that the very same glafs tube would produce either of them at pleasure; yet, notwithstanding this demonstration, Mr. Delaval, several years afterwards, proposed another theory of the two electricities, which seems to be more ingenious than solid; as it goes upon the old supposition of the different powers depending intirely upon the different substances themselves. The account of this theory was read at the Royal Society March the 22d. 1759. It necessarily occasioned some controversy with Mr Canton, in the course of which some new experiments were made, and some new facts discovered; on account of which I shall, with the utmost impartiality, report all that was advanced on both sides.

MR. DELAVAL observed, that there were two of the pure
 chymical

chymical principles of bodies, viz. *earth* and *fulphur*, which were each possessed of a different kind of electricity; one of which we might call a *plus* electricity, the other a *minus*; and thought that it might be expected, that, in a body compounded of both, the opposite powers of those ingredients would counter-balance, and destroy the effect of each other; and therefore, that bodies in which the negative and positive powers were equal, would be neutral, or non-electrics. Such a substance he took metal to be, consisting of calx and sulphur; metals not being calcinable without a degree of heat sufficient to dissipate all their sulphur; as is evident from their not being reducible again to their metallic form, without the admixture of some unctuous matter. The same dissipation of sulphur, he says, must take place in animal and vegetable substances, before they become white ashes. Transparent stones he considered as little more than pure earth, free from the least mixture of oil; judging of others by the chymical resolution of chrysol.

To confirm this theory, Mr. Delaval made experiments with dry powders of calcined metals, viz. cerufs, lead ashes, minium, calx of antimony, &c. inclosing them in long glass tubes, and endeavouring to transmit the electric virtue through them, and always finding it impossible. Animal and vegetable substances, when reduced to ashes, were alike impermeable to electricity, as also the rust of metals.

He was first led to these experiments, and to this hypothesis, by finding that dry mould would not conduct electricity. This he also tried with dry Portland stone, some of which he had cut into plates nearly as thin as window glass. These he heated to a proper degree, and coated them on both sides with metal; in order to make the Leyden experiment. When the stone was hot enough to singe paper, it conducted as perfectly as when cold; but

but on cooling a little, it began not to conduct, and afforded small shocks; which gradually increased in strength for about ten minutes, at which time it was about its most perfect state, and remained so near a quarter of an hour. After that time, the shocks gradually decreased, as the stone grew cooler; till, at last, they ceased, and the stone returned to its conducting state again, but this state appeared before the stone was quite cold.

EXPERIMENTS of this kind succeeded with all bodies abounding with calx, or earth, as stones, earth, dry clay, wood when rotten, or burned in the fire, till the surface becomes black. Among other substances he tried a common tobacco pipe, part of which, near the middle, he heated to a proper degree, and then applied one end of it to an electrical bar, while the other was held in the hand; and he observed, that the electric fluid passed no farther along the pipe than to the heated part.*

FROM these experiments Mr. Delaval inferred, that stones and other earthy substances were convertible, by several methods, and particularly by different degrees of heat, from non-electrics to electrics. But finding, afterwards, that it was the opinion of some persons (Mr. Canton was the person chiefly hinted at) that this change did not immediately, but only consequentially depend on heat, by evaporating the moisture, which would return again when the substance cooled; he observes, in a paper read at the Royal Society December the 17th. 1761, that the tobacco pipe lost its electricity before it was cold, and therefore before it could have imbibed moisture sufficient to destroy its electricity; and besides, that the substance employed in the experiment was not of that kind of bodies which is apt suddenly to draw moisture from the air.

* Phil. Trans. Vol. 51, pt. 1, p. 85.

To account for Mr. Delaval's experiments, Mr. Canton supposes, in a paper read to the Royal Society February 4th. 1762, that stone, tobacco pipe, wood, &c. will conduct when cold by the moisture they contain in that state; that when their moisture is evaporated by heat they become non-conductors; and that when they are made very hot, the hot air at, or near their surfaces will conduct, and the bodies will appear to be conductors again. Hot air, he says, may easily be proved to be a conductor of electricity, by bringing a red hot iron poker, but for a moment, within three or four inches of a small electrified body; when it would be perceived, that its electric power would be almost, if not intirely destroyed; and by bringing excited amber within an inch of the flame of a candle, when it would lose its electricity before it had acquired any sensible degree of heat.

To confirm this, he mentions his having observed, that the tourmalin, Brasil topaz, and Brasil emerald, would give much stronger signs of electricity when cooling, after they had been held about a minute within two inches of an almost surrounding fire, where the air is a conductor, then they ever will after heating them in boiling water. He adds, that if both sides of those stones be equally heated, in a less degree, than will make the surrounding air a conductor, the electricity of each side, whether *plus* or *minus*, would continue so all the time the stone was both heating and cooling, but would increase while it was heating, and decrease while it was cooling; whereas, if the heat was sufficient to make the surrounding air conduct the electric fluid from the positive side of the stone to the negative side of it, while it was heating, the electricity of each side would increase while the stone was cooling, and be contrary to what it was while the stone was heating.

As to the tobacco pipe, Mr. Canton says, that it not only attracts the moisture of the air, but absorbs it. Hence a tobacco pipe, after it begins to cool, will become a conductor again sooner than wood. And that it imbibes moisture faster than wood, he says, is evident, because when wetted, it will not continue wet so long as wood, imbibing the moisture presently.

THAT tobacco pipe does not become a conductor by a particular degree of heat, without evaporating its moisture, is evident, he says, from the following experiments.

If three or four inches of one end of a tobacco pipe, of more than a foot in length, be made red hot, without sensibly heating the other end, this pipe will prove a ready conductor; through the hot air surrounding one part of it, and the moisture contained in the other, although some part of it must have the degree of heat of a non-conductor. But if the whole pipe be made red hot, and suffered to cool till it has only superficial moisture enough to make it a good conductor, and then three or four inches of one end be again made hot, it will become a non-conductor.

If a nail be placed at, or near each end of a longish solid piece of any of the absorbent bodies above-mentioned, so as the point of each nail may be about half the thickness of the body within its surface; this body, by heat, may be made a non-conductor externally or superficially, while it remains a good conductor internally. For the electric fluid will readily pass from one nail to the other, through the middle of the body, when it will not pass on its surface, and even when the internal parts of the body are in an equal degree of heat with the external, as they must soon be after it begins to cool. But if the same body be exposed, for a short time, to a greater degree of heat than before, or if it be

kept longer in the same heat, it will become a non-conductor intirely. *

MR. DELAVAL, in confirmation of particular bodies requiring particular degrees of heat to render them electric or non-electric, independant of moisture, mentions a substance which, he says, is affected by heat in an opposite manner to the former instances; since the degrees of heat, necessary to render the other substances electric, makes this non-electric.

THE substance was *island chrystal* (which is well known for its singular property of a double refraction) on a piece of which he made the following observations. 1. After this piece of chrystal had been rubbed, when the heat of the air was moderate, it showed signs of electricity, though not very strong ones. 2. If the heat was increased, so as to be a little greater than that of the hand, it destroyed its electric power intirely. 3. By cooling the stone again, the electric power was restored.

HE immerfed this piece of chrystal into a vessel filled with quicksilver, and surrounded with ice, where it remained near two hours, when the weather was very cold; and observed, that, upon taking it out with a pair of tongs (that it might not be altered by the heat of his hands) and rubbing it again, it was more strongly electric then he had at any other time experienced; but that, on placing it a few minutes upon the hearth, at some distance from the fire, its electric property was again destroyed, for that rubbing would not occasion any signs of it.

THUS, says he, we see two different kinds of fixed bodies, the one of which acquires an electric property with the same heat with which another loses it; while a third set of substances, as glass, &c. retain their electricity through both the degrees of heat necessary to the other two.

* Phil. Transf. Vol. 52. pt. 2. p. 459.

SOME pieces of island chrystal, which he had procured from different places, had not the property of losing their electricity by a moderate heat. He had, in particular, a piece of that chrystal, one part of which, when greatly heated, became non-electric; while the other part, with the same heat, or even with a much greater one, remained perfectly electric.

HE found several other earthy substances, whose electricity was destroyed by different degrees of heat.

FROM considering that the degree of heat, at which the island chrystal first mentioned was in its most perfect electric state, was less than the usual heat of the air, and that a small increase of that heat rendered it non-electric; he did not think it improbable, that many substances, which are not known to be electric, might prove so, if exposed to a greater degree of cold than they have been hitherto examined in.*

To these observations Mr. Canton replies, that having formerly observed that the friction between mercury and glass in vacuo would not only produce the light of electricity, as in the luminous barometer, or within an evacuated glass ball, but would also electrify the glass on the outside, he immersed a piece of dry glass in a basin of mercury; and found, that by taking it out, the mercury was electrified *minus*, and the glass electrified *plus*, to a considerable degree. He also found, that amber, sealing wax, and island chrystal, when taken out of mercury, were all electrified positively. How then, says he, does it appear, that the electricity which was observed in rubbing the last mentioned substance, after it was taken out of mercury surrounded by ice, was owing to cold, and not to the friction between it and the mercury in taking it out. Island chrystal when warm is

* Phil. Trans. Vol. 52. pt. 1. p. 354, &c. † Ib. Vol. 52. pt. 2. p. 461.

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a non-conductor, and all non-conductors may be excited with proper rubbers. *

MR. BERGMAN of Upsal, in a letter to Mr. Wilfon, read at the Royal Society April the 14th. 1761, says, that he had tried the experiments of Mr. Delaval with island chrystal, but that the event had always been contrary to what Mr. Delaval had reported. Trying different pieces of chrystal, he found one which instead of having its virtue increased by cooling, was sensibly increased by heating. Afterwards trying all the rest which he had by him, whether Swedish chrystal, or island, he found the effect to be the same. From this he inferred, that the chrystals which he had were of a quite different kind from that of Mr. Delaval. †

* Phil. Transf. Vol. 52. pt. 2. p. 461. † Ib. Vol. 53. pt. 1. p. 98.

SECTION V.

MR. CANTON'S EXPERIMENTS AND DISCOVERIES RELATING
TO BODIES IMMERGED IN ELECTRIC ATMOSPHERES, WITH
THE DISCOVERIES OF OTHERS, MADE BY PURSUING THEM.

IN this section I shall present my reader with the finest series of experiments that the whole history of electricity can exhibit, and in which we shall see displayed the genius and address of four of the most eminent electricians in this whole period; viz. Mr. Canton, and Dr. Franklin, Englishmen; and Mr. Wilcke, and Æpinus, foreigners. Mr. Canton had the honour to take the lead, and he made all the essential experiments. Doctor Franklin professedly pursued them; and though *all his strength be put not forth* on this occasion, he diversified the experiments, and made some improvement in the method of accounting for them. But Mr. Wilcke and Æpinus in conjunction carried the experiments vastly farther, and completed the discovery; which is, certainly, one of the greatest that has been made since the time of Dr. Franklin. I say the time of Dr. Franklin, though he himself be one of the persons concerned; for by *the time of Dr. Franklin* will always be understood the time in which he made his capital discoveries in America. This will always be a distinguished epocha in the history of electricity, from which all his own future discoveries will be dated.

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THE original experiments in this section, when Mr. Canton first published them, in his usual concise, though perspicuous manner, without any preamble, to inform us how he was led to them, exhibit such a variety of attractions and repulsions of electrified bodies in different circumstances, as looked like the power of magic; and were they conducted with a little art, I do not know any electrical experiments (made without light, or noise) more proper for a deception of this kind. But when they are attentively considered, they demonstrate a remarkable property of all electrified bodies, which has often been referred to in the course of this history, but which had not been attended to before; nor indeed do I apprehend it was fully understood, till it was explained in all its extent by Mr. Wilcke and Æpinus. It is, that the electric fluid, when there is a redundancy of it in any body, repels the electric fluid, in any other body, when they are brought within the sphere of each other's influence, and drives it into the remote parts of the body; or quite out of it, if there be any outlet for that purpose. In other words, bodies immersed in electric atmospheres always become possessed of the electricity, contrary to that of the body, in whose atmosphere they are immersed. This principle pursued led them to the method of charging a plate of air, like a plate of glass, and to make the most perfect imitation of the phenomena of thunder and lightning.

THE paper containing an account of Mr. Canton's experiments was read at the Royal Society December the 6th. 1753.

MR. CANTON suspended cork balls, one pair by linen threads, and another pair by silk; then holding the excited tube at a considerable distance from the balls with the linen thread, they separated; and, upon drawing it away, they immediately came together: but he was obliged to bring the excited tube much nearer to the balls hanging by silk threads, before they would separate;

separate; though when the tube was withdrawn, they continued separate for some time.

As the balls in the former of these experiments were not insulated, Mr. Canton observes, that they could not properly be said to be electrified; but that when they hung within the atmosphere of the excited tube, they might attract and condense the electric fluid round about them, and be separated by the repulsion of its particles. He conjectures also, that the balls, at this time, contain less than their common share of the electric fluid, on account of the repelling power of that which surrounds them, though some may be continually entering and passing through the threads. And if that be the case, he says, the reason is plain why the balls hung by silk in the second experiment must be in a much more dense part of the atmosphere of the tube before they will repel each other. He adds, that at the approach of an excited stick of wax to the balls, in the first experiment, the electric fire is supposed to come through the threads into the balls, and to be condensed there, in its passage towards the wax; since, according to Dr. Franklin, excited glass emits the electric fluid, and excited wax receives it.

When two balls, suspended by linen threads upon an insulated tin tube, were electrified positively, and had separated; he observed, that the approach of the excited tube would make them come nearer together; if brought to a certain distance, they would touch; and if brought nearer, they would separate again.

In the return of the tube, they would approach each other, till they touched, and then repel as at first. If the tin tube was electrified by wax, or the wire of a charged phial; the balls would be affected in the same manner at the approach of excited

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wax, or the wire of the phial. If the cork balls were electrified by glass, their repulsion would be increased at the approach of an excited stick of wax. And the effect would be the same, if the excited glass was brought towards them, when they had been electrified by wax.

THE bringing the excited glass to the end, or edge of the tin tube, in the former of these experiments, is by Mr. Canton supposed to electrify it positively, or to add to the electric fire it before contained; and therefore some will be running off through the balls, and they will repel each other. But at the approach of excited glass, which likewise emits the electric fluid, the discharge of it from the balls will be diminished, or part will be driven back, by a force acting in a contrary direction, and they will come nearer together. If the tube be held at such a distance from the balls, that the excess of the density of the fluid round about them above the common quantity in air, be equal to the excess of the density of that within them above the common quantity contained in cork, their repulsion will be quite destroyed. But if the tube be brought nearer, the fluid without being more dense than that within the balls, it will be attracted by them, and they will recede from each other again.

MR. CANTON farther observes, that when the apparatus has lost part of its natural store of this fluid, by the approach of excited wax to one end of it, or is electrified negatively, the electric fire is attracted and imbibed by the balls, to supply the deficiency; and that more plentifully at the approach of excited glass, or a body positively electrified, than before; whence the distance between the balls will be increased, as the fluid surrounding them is augmented. And, in general, whether by the approach or recess of any body, if the difference between the density of the internal and external fluid be increased, or diminished;

diminished; the repulsion of the balls will be increased, or diminished accordingly.

He observed, that when the insulated tin tube was not electrified, if the excited glass was brought towards the middle of it, the balls hanging at the end would repel each other, and the more so as the excited tube was brought nearer. When it had been held a few seconds, at the distance of about six inches, and withdrawn, the balls would approach each other till they touched; and, separating again, as the tube was removed farther, would continue to repel when the tube was taken quite away. This last repulsion would be increased by the approach of excited glass, and diminished by that of excited wax; just as if the apparatus had been electrified by wax, after the manner described in the last experiment.

He insulated two tin tubes, which may be distinguished by calling them A and B, so as to be in a line with each other, and half an inch asunder, and at the remote end of each suspended a pair of cork balls. Then, upon bringing the excited glass tube towards the middle of A, and holding it a short time at the distance of a few inches, he observed each pair of balls to separate. Upon withdrawing the tube, the balls of A would come together, and then repel each other again, but those of B would hardly be affected. By the approach of excited glass the repulsion of the balls of A would be increased, and those of B diminished.*

In the former of these experiments, Mr. Canton supposes the common stock of electric matter in the tin tube to be attenuated about the middle, and to be condensed at the ends, by the repelling power of the atmosphere of the excited glass tube, when

* Phil. Transf. Vol. 48. pt. 1. p. 350.

held near it. And perhaps, he says, the tin tube may lose some of its natural quantity of the electric fluid before it receives any from the glass, as that fluid will more readily run off from the ends or edges of it than enter at the middle; and accordingly, when the glass tube is withdrawn, and the fluid is again equally diffused through the apparatus, it is found to be electrified negatively; since excited glass brought under the balls will increase their repulsion.

In the latter of the experiments, Mr. Canton supposes that part of the fluid driven out of one tin tube enters the other, which is found to be electrified positively, by the decreasing of the repulsion of its balls at the approach of excited glass.

It will readily be seen that, at the time these experiments were made, Mr. Canton retained the common idea of electric atmospheres; whereas it will appear by the experiments of Mr. Wilcke and Æpinus (which in fact contain nothing more than those of Mr. Canton) that they tend to refute the common opinion, and are much easier explained upon the supposition, that the portion of fluid belonging to any electrified body is constantly held in contact, or very nearly in contact, with the body; but acts upon the electricity of other bodies at a certain distance.

DR. FRANKLIN pursued, or rather diversified the experiments of Mr. Canton, but retaining, likewise, the common opinion of electric atmospheres, he thought that the phenomena were more easily explained upon the supposition, that these atmospheres, being brought near each other, did not easily mix, and unite into one atmosphere, but remained separate, and repelled each other; and moreover, that an electric atmosphere, would not only repel another electric atmosphere, but also the electric fluid contained in the substance of a body approaching it, and, without

without joining or mixing with it, force it into the other parts of the body that contained it.

THOUGH it must be difficult to assign a reason why the particles of one atmosphere should repel the particles of another atmosphere, or of the fluid contained in another body with more force than they repel one another, or the particles of the fluid contained in the body to which they belong; since the matter is the same in both: yet this idea of the mutual repulsion of electric atmospheres, could it once be supposed, will certainly and clearly account for all the facts; and the theory pleases on account of its simplicity. But the same appearances will be accounted for, in a manner as simple and intelligible, upon the supposition, that the portion of electric fluid belonging to each body, being strongly attracted by the body, is held in close contact with it; but that it acts by repulsion upon the electric fluid belonging to other bodies, at a distance from them; and that the electric fluid doth not actually pass out of one body into another, till it have first repelled the fluid out of the other body, and then be more strongly attracted by the other body, than by its own; which has already got more than its natural share.

THE paper containing an account of these experiments of Dr. Franklin was read at the Royal Society December the 18th. 1755. His apparatus, was different from that of Mr. Canton, but still he exhibited the same effects proceeding from the same cause. He fixed a tassel of fifteen or twenty threads, each three inches long, at one end of his prime conductor, which was five feet long and four inches in diameter, supported by silk lines. The threads were a little damp, but not wet.

IN these circumstances, an excited tube brought near the end of the prime conductor, opposite to the threads, so as to give it
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some sparks, made the threads diverge, each thread having thereby acquired its separate electric atmosphere.

In this state, the approach of the excited tube, without giving any sparks, made the threads diverge more; but, being withdrawn, they closed as much; the atmosphere of the conductor being driven by that of the tube into the threads, and returning again upon withdrawing the tube, which had then left no part of its atmosphere behind it.

THE excited tube brought under the diverging threads made them close a little, having driven part of their atmospheres into the conductor. Upon being withdrawn, they diverged as much; that portion of their atmospheres which they had lost returning again from the conductor, and the tube having left no part of its own.

THE excited tube, held at the distance of five or six inches from the end of the conductor opposite to the threads, made them separate, and, upon being withdrawn, they came together again: but if, in their state of separation, a spark was taken from the conductor near them, they would close; and, upon removing the tube, would separate. The tube, in both cases, left no part of its atmosphere behind it. It only drove the natural quantity of electricity contained in the conductor towards the threads; and part of that being taken away by the spark, the tube would leave the conductor and threads negative, in which case, they would repel one another, as if they had been electrified positively.

In this situation, if the excited tube was brought near the conductor, they would close again; the atmosphere of the tube forcing that of the conductor into the threads, to supply the place of what they had lost: but, upon withdrawing the tube, they would open again; the tube, as before, taking its whole atmosphere away with it. When the excited tube was brought

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under the threads, diverging with negative electricity, they diverged more; the atmosphere of the tube driving away more of the atmospheres of the threads, and giving them none in its place.

LASTLY the Doctor brought the excited tube near the prime conductor, when it was not electrified; and when the threads were, thereby, made to diverge, he brought his finger near them; and observed, that they receded from it. This appearance had been taken notice of by Mr. Hawkesbee, and others. Dr. Franklin accounts for it by supposing, that when his finger was plunged into the atmosphere of the glass tube, part of its natural electricity was driven back, through his hand and body, so as to leave the finger negatively electrified, as well as the threads; in which case they must necessarily repel one another. To confirm this hypothesis, he held a slender lock of cotton, two or three inches long, near the prime conductor, electrified by excited glass, which made the cotton stretch itself towards the conductor; and observed, that, in this state, it would recede from the finger of his other hand, at the same time that it was attracted by a wire of a bottle charged positively.*

THESE experiments of Dr. Franklin, made in pursuance of those of Mr. Canton, were confirmed, as I observed before, and carried much farther by Mr. Wilcke and Æpinus.

MR. WILCKE observes, that a small body immersed in any electric atmosphere, if it be touched by no other body, and be withdrawn before it be repelled, scarce ever shows any sign of electricity; if any, it is of the same kind with that of the body into whose atmosphere it was plunged. † If any body, communicating with the ground, be brought to this light body,

* Phil. Transf. Vol. 49. pt. 1. p. 300. † Wilcke, p. 73.

while

while it remains immersed in the atmosphere of the electrified body, it is first attracted, and then repelled by it. If a point be presented to this light body, and afterwards withdrawn, it will be found to have acquired an electricity opposite to that of the electrified body. From this he concludes, that parts of non-electric bodies, plunged in electric atmospheres, acquire an electricity opposite to that of the atmosphere in which they were plunged.*

He placed two large insulated conductors with their ends opposite to one another, and a cork ball suspended on silk between them; and observed, that, upon the application of the excited glass tube to one end of either of them, the cork ball would play between them very fast; and if the tube were held a while at the same distance, would be at rest. Upon withdrawing the tube, the motion of the cork ball began again, and, at length, ceased gradually as before. If the conductors were removed from one another, while they were within the atmosphere of the tube, they would, upon being brought together again, give a spark. This experiment confirmed the demonstration, that the part of a body which is immersed in the atmosphere of an electrified body acquires the contrary electricity.†

But the most complete demonstration of this general maxim is an experiment of Æpinus. He placed a small weight upon one end of a large metallic conductor, and, by means of a silk string, removed it from the conductor, while the end on which it rested was immersed in the atmosphere of an electrified body; and found that it had actually acquired a different electricity from that of the atmosphere. If the end of the conductor, opposite to that on which the moveable weight was placed, was

* Wilcke, p. 77.

† Ib. p. 78.

made to communicate with the earth, still that part of it which was near the excited electric was affected with the opposite electricity. Placing the moveable weight on the opposite end of the conductor, when it was insulated, he found that it had sometimes acquired an electricity contrary to that of the excited electric, sometimes the same electricity, though weak, and sometimes no electricity at all. *

THE same ingenious philosopher considered that the same principle must extend to glass, and all other electrics; since they, as well as conductors, contain a certain quantity of the electric fluid, in their natural state. To verify this, he took a glass tube, and electrified one end of it positively. The consequence was, that four or five inches of that end were positive; but beyond that there were two inches negative, and beyond that the tube was again positive, though weakly so. This experiment he repeated very often with the same success; as also when, instead of glass, he used a solid stick of sulphur. To account for this fact, he supposed, that the electricity communicated to the end of the tube repelled the natural quantity of the fluid in the glass to some distance. This natural quantity retiring from its former situation, he supposes to become condensed, and consequently to repel another quantity of the fluid natural to the glass from its place; and that thus the whole rod would be alternately positive and negative. The author asserts, that it was from theory only that he was led to this curious experiment, the fact exactly corresponding to what he had before deduced, as the necessary consequence of Dr. Franklin's principles of negative and positive electricity. †

THE hint of these experiments Æpinus received from those

* Æpini Testament, p. 129. † Ib. p. 192.

above-mentioned of Mr. Wilcke; and these gentlemen, residing at the same time at Berlin, pursued these curious experiments jointly, till they were led by them to discover a method of charging a plate of air in the same manner as plates of glass had usually been charged, and to throw still more light upon the theory of the famous Leyden experiment.

In the above-mentioned experiments, these gentlemen observed, that the negative state of one of the bodies depended on the opposite state of the other, which was known to be exactly the case of the two sides of a charged pane of glass; and the reason of the non-communication of the same electricity was evidently the impermeability of the glass to the electric fluid in the one case, and the impermeability of the air in the other. Upon this hint, they made several attempts to give the electric shock by means of air; and at length succeeded, by suspending large boards of wood covered with tin, with the flat sides parallel to one another, and at some inches asunder. For they found, that upon electrifying one of the boards positively, the other was always negative, agreeable to the former experiment; but the discovery was made complete and indisputable by a person's touching one of the plates with one hand, and bringing his other hand to the other plate; for he then received a shock through his body, exactly like that of the Leyden experiment.*

With this plate of air, as we may call it, they made a variety of curious experiments. The two metal plates, being in opposite states, strongly attracted one another, and would have rushed together, if they had not been kept asunder by strings. Sometimes the electricity of both would be discharged by a strong spark between them, as when a pane of glass bursts with too

* Wilcke, p. 97.

great a charge. A finger put between them promoted the discharge, and felt the shock. If an eminence was made on either of the plates, the self discharge would always be made through it, and a pointed body fixed upon either of them prevented their being charged at all.

THE state of these two plates, they excellently observe, justly represents the state of the clouds and the earth during a thunder storm; the clouds being always in one state, and the earth in the opposite; while the body of air between them answers the same purpose as the small plate of air between the boards, or the plate of glass between the two metal coatings in the Leyden experiment. The phenomenon of lightning is the bursting of the plate of air by a spontaneous discharge, which is always made through eminences, and the bodies through which the discharge is made are violently shocked.*

THIS principle, they likewise thought, would explain an observation of the Abbé Nollet, that electricity was often observed to be peculiarly strong, when the room was full of company, and more particularly, when numbers of them drew near together, to see the experiments. The conductor was then in one state, and the company in another; so that, constituting a large surface, when any of them took a spark, as he thereby discharged the electricity of all the company, he would feel it more sensibly than if he had stood single.†

THIS discovery, of the method of giving the electric shock by means of a plate of air, may be reckoned one of the greatest discoveries in the science of electricity since those of Dr. Franklin. It is beautiful to observe how this fine discovery took its rise from the experiments of Mr. Canton. Mr. Canton's experiments

* Wilcke, p. 101. † Ib. p. 96, &c.

were pursued by Dr. Franklin, and those of Dr. Franklin pursued by these gentlemen produced the discovery. It is one and the same principle that, in different circumstances, accounts for this beautiful series of experiments.

THIS experiment of charging a plate of air is likewise related by Æpinus, who says that he was led to the discovery, by reasoning from the consequences of Dr. Franklin's theory.

FROM these experiments he was also led to form a more distinct idea of the impermeability of glass to the electric fluid. For since a plate of air might be charged as well as a plate of glass, that property, whatever it be, must be common to them both; and could not, as Dr. Franklin once supposed, be any thing peculiar to the internal structure of glass. Impermeability, he, therefore, infers, must be common to all electrics; and since they can all receive electricity by communication to a certain degree, it must consist in the difficulty and slowness with which the electric fluid moves in their pores; whereas, in perfect conductors, it meets with no obstruction at all.*

IT was chiefly this course of experiments, also, that led Mr. Æpinus to deny the existence of electric atmospheres, consisting of effluvia from electrified bodies.

HE seems, however, to consider this as a bold opinion; since he herein differs, as he says, from all the electricians who had written before him, and even from Dr. Franklin himself. Though the common opinion, he says, is by no means countenanced by the general principles of his theory, which suppose the electric fluid to move with difficulty through every electric substance like the air.

To those who might say, that an electric atmosphere is a

* Æpini Tentamen, p. 82.

thing

thing obvious to the senses, and no matter of theory; since it may be felt like a spider's web upon the hands or face; he replies, that this feeling, together with the sulphureous smell of electrified bodies, are only sensations excited by the action of the fluid in the electrified bodies upon the electric fluid in the nostrils, or the hand; or upon those parts of the body themselves in an unelectrified state; and that they are not felt by a person who is not possessed of the same kind and degree of electricity.

HE, therefore, thinks there never was any sufficient reason to admit those atmospheres; and declares, that whenever he uses the word, he means no more by it than the sphere of action of the electricity belonging to any body. Or, he says, the neighbouring air, which is electrified by it, may be so called.

BUT that these atmospheres have little effect in electrical experiments, he says, is evident from this circumstance; that if it be blown upon with a pair of bellows, the electricity of the body which it surrounds is not sensibly diminished. The electric fluid, he supposes, to reside wholly in the electrified body, and from thence to exert its attraction or repulsion to a certain distance.*

THE subject of electric atmospheres had not escaped the attention of the accurate Signior Beccaria, who was probably prior to *Æpinus* in supposing, that electrified bodies have no other atmosphere than the electricity communicated to the neighbouring air, and which goes with the air, and not with the electrified bodies, agreeable to that curious discovery of his mentioned above.

HE also mentions an experiment, which, he thinks, directly

* *Æpini Tentamen*, p. 257.

proves,

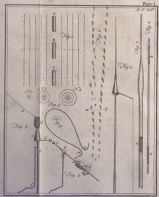
proves, that all the electricity communicated to any body adheres to its surface, and does not spread into the air. He electrified a large conductor of gilt paper, in which the gilding was, in several places, taken off quite round; and observed that whenever he discharged it, by taking a spark at the end, other sparks were visible at all the interruptions; the charge of the more remote parts having come off through the substance of the metal, and not along the air; as the greatest part of it, at least, might have done, if it had lodged there.*

It is now also Mr. Canton's opinion, that electric atmospheres are not made of effluvia from excited or electrified bodies, but that they are only an alteration of the state of the electric fluid contained in, or belonging to the air surrounding them, to a certain distance; that excited glass, for instance, repels the electric fluid from it, and consequently, beyond that distance makes it more dense; whereas excited wax attracts the electric fluid existing in the air nearer to it, making it rarer than it was before.

THIS will be best understood by a figure. Let A [Pl. 1. fig. 1.] represent unexcited glass or wax, B excited glass, and C excited wax; and let the dots on each side of A represent a line of particles of the electric fluid at their proper distance in a natural state.

LET B and C be carried about where you will in the air, B will make an atmosphere equally dense, and C an atmosphere equally rare, while the quantity of the electric fluid each of them contains is the same as at first. When any part of a conductor comes within the atmosphere of B, the electric fluid it naturally contains will be repelled by the dense atmosphere, and will re-

* *Elettricità artificiale*, p. 54.



UVA.BHSC



VVA.BHSC

cede from it. But if any part of a conductor be brought within the atmosphere of C, the electric fluid it naturally contains will be attracted by the rare atmosphere, and move towards it. And thus may the electric fluid contained in any body be condensed or rarified; and if the body be a conductor, it may be condensed or rarified in any part of it, and some may be easily drawn out of, or an additional quantity put into it.

IT was observed before, that an experiment of Dr. Franklin which he thought proves that electric atmospheres did not exclude the air, might justly make us suspect the existence of those atmospheres, since the electric matter is known to repel the air. Another experiment of the same nature was made by Dr. Darwin of Litchfield, who sent an account of it to the Royal Society, which was read May the 5th. 1757. He got a glass tube, open at one end, and having a ball at the other. This ball and half of the tube he coated; and when he had inverted it, and dipped a considerable part of it in a vessel containing oil of turpentine, he introduced a wire into it, and charged it; and observed, that the oil did not at all appear to subside. From this he concluded, that the electric atmosphere, flowing round the wire and the coating of the tube, above the oil, did not displace the air, but existed in its pores. *

AN experiment similar to that of Dr. Franklin and that of Dr. Darwin was made by Signior Beccaria. He took a coated phial, and when he had inserted into it a small glass tube, bent horizontally when it came out of the phial; he closed it with cement, and presented light ashes to the extremity of the tube, the orifice of which was very fine; and always found, that the ashes were blown off, when a spark was taken into the phial, but they re-

* Phil. Transf. Vol. 50. pt. 1. p. 352.

turned

turned towards the end of the tube afterwards.* It is probable, that the metal not being sufficiently in contact with the inside coating, a spark was made in the inside, which expelled the air, and caused the motion in the ashes. The fairest method of trying it would be with a phial, in which the metal that received the fire from the conductor, should be a production of the inward coating.

* Lettere dell' elettricismo, p. 79.

SECTION VI.

MR. SYMMER'S EXPERIMENTS RELATING TO THE TWO ELECTRICITIES, AND THOSE MADE BY JOHANNES FRANCISCUS CIGNA IN PURSUANCE OF THEM.

IT had hitherto been universally supposed, that all the phenomena of electricity were produced by the action of one electric fluid. Even Mr. Du Faye, at the time that he imagined he had discovered another electric fluid, distinct from that of glass, and peculiar to rosin, &c. thought, however, that it was quite independent of the other, and that their operations were never combined. Dr. Watson, and Dr. Franklin thought it was very evident, that the difference between the two electricities consisted in the one being a redundancy, and the other a deficiency of the same matter. And all the experiments that had been made concerning the two electricities seemed to confirm this hypothesis. At length, however, Mr. Symmer produces a great number of curious experiments, relating to the same subject; and infers from them the probable existence of *two electric fluids*, not independent, but always coexistent, and counteracting one another.

K k THE

THE first set of his experiments are very remarkable, but he does little more than relate naked facts. They were diversified, and pursued much farther by Johannes Franciscus Cigna, of Turin, who has also explained them upon the principles of Dr. Franklin's theory; though he was of opinion, that no experiments that had yet been made were decisive, in favour of either of the two hypotheses. Few histories of experiments are more entertaining than the first of these of Mr. Symmer; the subsequent experiments are less satisfactory. The papers relating to them all were read at the Royal Society in the year 1759.*

THIS gentleman had for some time observed, that upon putting off his stockings, in an evening, they made a crackling or snapping noise, and that, in the dark, he could perceive them to emit sparks of fire. He had no doubt but that this proceeded from the principle of electricity, and, after a great number of observations, to determine on what circumstances those strong electrical appearances depended; he found, at length, that it was the combination of white and black that produced the electricity; and that the appearances were the strongest when he wore a white and black silk stocking upon the same leg. † These, however, discovered no sign of electricity while they were upon the leg, or hand (for he found that his hand was sufficient) though they were drawn backwards and forwards upon it several times. Nor when taken from the hand, and presented to an electrometer (i. e. Mr. Canton's balls) did they appear to have acquired any more than a very small degree of

* Phil. Transf. Vol. 51. pt. 1. p. 340.

† The Abbé Nollet, in repeating these experiments of Mr. Symmer, found, that it was not absolutely necessary, that one of the stockings should be black, for that, if one of them was only dipped in a decoction of gall-nuts, which doth not dye them black, but is only a preparative to it, it would have the same effect. Nollet's Letters, Vol. 3. p. 42.

electricity;

electricity; but the moment they were separated, they were found, both of them, to be highly electrified, the white positively, and the black negatively.

BOTH the stockings, when held at a distance from one another, appeared inflated to such a degree, that, when highly electrified, they exhibited the intire shape of the leg; and when two black, or two white stockings were held together, they would repel one another, so as to form an angle, seemingly, of thirty or thirty five degrees.

WHEN a white or black stocking were presented to each other; they would be mutually attracted; and, if permitted, would rush together with surprizing violence. In their approach their inflation gradually subsided, and their attraction of foreign objects diminished, but their attraction of one another increased. When they actually met, they grew flat, and joined as close together, as if they had been so many folds of silk. When they were separated, their electricity did not seem to have been in the least impaired by the shock of meeting; for they would be again inflated, attract, repel, and rush together as before.

WHEN this experiment was performed with two black stockings in one hand, and two white ones in the other, it exhibited a curious spectacle. The repulsion of those of the same colour and the attraction of those of different colours, threw them into an agitation which was not unentertaining, and made them catch each at the opposite colour, at a greater distance than could have been expected.

WHEN the stockings were separated from one another, they would lose their power very soon, much like the excited tube; but when they were together, they would retain it an hour or two, or longer, if the air was favourable to electricity. The sharpest metallic point could not deprive them of it; and when

they were one within the other, no means he could think of could procure the least perceivable discharge of the electricity. In this respect, Mr. Symmer thought there was a considerable resemblance between the black and the white stocking, when put within one another, and the Leyden phial.

WHAT was still more remarkable in these experiments with the white and black stockings, was the power of electrical cohesion which they exhibited. Mr. Symmer perceived that the white and black stockings, when electrified, and allowed to come together, not only joined extremely close, but actually stuck to each other. By means of a balance, he found, that in order to separate them, it required from one to twelve ounces. Another time they raised seventeen ounces which, was twenty times the weight of that stocking which supported them, and this in a direction parallel to its surface.

WHEN one of the stockings was turned inside out, and put within the other, it required twenty ounces to separate them, though when they were applied to each other externally, ten ounces were sufficient.

GETTING the black stockings new dyed, and the white ones washed, and whitened in the fumes of sulphur; and then putting them one within the other, with their rough sides together, it required three pounds three ounces to separate them. And he had reason to think that the sulphur contributed nothing to the experiment.

TRYING this experiment with stockings of a more substantial make, he found the effects more considerable. When the white stocking was put within the black one, so that the outside of the white was contiguous to the inside of the black, they raised nine pounds wanting a few ounces, which was fifty times the weight of the stocking. When the white stocking was turned inside

out,

out, and put within the black one, so that their rough surfaces were contiguous, they raised fifteen pounds one pennyweight and a half, which was ninety two times the weight of the stocking.

HAVING cut off the ends of the thread, and the tufts of silk, which had been left in the inside of the stockings, the cohesion was considerably diminished. Pressing them together between his hands contributed much to strengthen it.*

WHEN the white and black stocking were in cohesion, and another pair, more highly electrified, were separated from one another, and presented to the former, their cohesion would be dissolved; and each stocking of the second pair would catch hold of, and carry away with it, that of its opposite colour. If the degree of electricity of both pairs were equal, the cohesion of the former pair would be weakened, but not dissolved; and all the four would cohere, forming one mass. If the second pair were but weakly electrified, the cohesion of the first pair would be but little impaired, and the cohesion of the whole mass would be small in proportion.

MR. SYMMER also observed, that white and black silk, when electrified, not only cohered with each other, but would also adhere to bodies with broad, and even with polished surfaces, though those bodies were not electrified. This he discovered accidentally, having, without design, thrown a stocking out of his hand, which stuck to the paper hangings of the room. He repeated the experiment, and found it would continue hanging near an hour.

HAVING stuck up the black and white stockings in this manner, he came with another pair of stockings highly electrified; and applying the white to the black, and the black to the white,

* Phil. Transf. Vol. 51. pt. 1. p. 395.

he carried them off from the wall, each of them hanging to that which had been brought to it.

THE same experiments held with the painted boards of the room, and likewise with the looking glass, to the smooth surface of which both the white and the black silk appeared to adhere more tenaciously than to either of the former. *

A FEW observations, similar to some of these of Mr. Symmer, were made by Signior *Alessandro Amadeo Vaudonia*, a friend of Signior Beccaria. He put a beaver shirt between two others, which he wore in extreme cold weather; and whenever he put off the uppermost shirt, which he did every day, he found it, adhered to the beaver shirt, and, on the separation, electric sparks were visible between them. Whenever he put off the beaver shirt, it adhered still more to the under shirt, and when held at a considerable distance from it, would rush to it. These attractions would be repeated many times, but they grew more languid by degrees, till they intirely ceased. Signior Beccaria, upon hearing of this experiment, repeated it with some variation, and found it to answer on himself. †

THE cohesion of the two stockings induced Mr. Symmer to try the force of electrical cohesion in electrified panes of glass. For this purpose, he got two panes of common window glass, the thinnest and the smoothest that he could find, and coated one of the sides of each with tinfoil, leaving a space uncovered near the edges. He then put the uncovered sides together, and charging them both as one pane, he found, as he expected, that their cohesion was considerably strong: but he had no apparatus to measure the strength of it. He then turned the plates upside down, and found that the same operation which had before

* Phil. Transf. Vol. 51. pt. 1. p. 366. † Dell' elettricismo artificiale, p. 197.

charged

charged them, did now uncharge them, according to the analogy of the Leyden phial.

PLACING two panes of glass, each of them coated on both sides, one upon the other, he found that they were both charged separately, and that there was no cohesion between them.

IN pursuance of these last mentioned experiments of Mr. Symmer, and another made at Pekin (which will be recited presently) Signior Beccaria made the following, which are very curious; but which, after the example of the author, I shall relate without attempting an explanation.

HAVING charged a coated plate of glass, he slipped the coating from off the negative side, and applied another, uncoated and uncharged plate of glass close to it. After this, putting a coating upon the uncharged glass (so that the whole resembled one coated plate, consisting of two *laminee*) he formed a communication between the coatings. The consequence was an explosion, and a cohesion of the plates.

If he separated the plates before the explosion, after they had been in conjunction some time, the charged plate was positive on both sides, and the uncharged plate negative on both sides. But if he separated them after the explosion, the charged plate was negative on both sides, and the uncharged plate positive, on both sides.

If after the explosion he separated and joined them again alternately, a small circle of paper, placed under the uncharged plate, adhered to it upon every separation, and was thrown off again upon every conjunction. This he could repeat even five hundred times, with once charging the plate. This was the experiment that he says, was made by some Jesuits at Pekin, in the year 1755, and being sent to the academy of sciences at Petersburgh, was published in their memoirs, Vol. 8. p. 276.

If,

IF, in these experiments, the charged plate, was inverted, and the positive side applied to the uncharged plate, all the effects were exactly the reverse of the former. If it was inverted ever so often, after remaining some time in contact with the uncharged glass, it would produce a change in the electricity. In the dark, a light was always seen upon the separation of these plates.

LAYING the two plates together, like one plate, and coating the outsides of them, he charged them both together, and, at the distance of about four feet, he distinguished six of the *coloured rings*, which Newton describes in his book of Optics, all parallel to one another, and nearly parallel to the edge of the coating. At the angles of the coatings the rings spread to a greater distance; where the coatings did not quite touch the glass, the rings bent inwards; and where the coatings adhered very close, they retired farther from them. Upon discharging these two plates, the coloured rings vanished, and the electric cohesion (which Mr. Symmer had observed in this case) ceased with them.

UPON separating these plates before the explosion, that which had received the positive electricity was positive on both sides, and the other negative on both sides. If they were separated after the explosion, each of them (as in the former experiment) was affected in a manner just the reverse of this. Upon inverting these plates, that which was the thinner appeared to be possessed of the stronger electricity, and (like the charged plate in the former experiment) brought the other to correspond to it.

CHARGING the two plates separately, and taking off two of the coatings, so as to place the two positive or the two negative sides together, there was no cohesion or explosion. But joining a positive and a negative side, they cohered, and a communication

tion being formed on the outside, there was an explosion, which increased the cohesion. Making the above-mentioned experiments with these plates, he says, they acted just as the two that were charged at the same time.*

MR. SYMMER concludes his account of these experiments with declaring it to be his opinion, that there were two electric fluids, or emanations of two distinct electric powers, essentially different from each other; that electricity does not consist in the afflux and efflux of these fluids, but in the accumulation of the one or the other of them in bodies electrified; or, in other words, it consists in the possession of a larger portion of one or the other power, than is requisite to maintain an even balance within the body; and lastly, that, according as the one or the other power prevails, the body is electrified in the one or the other manner. Nor will this principle, says he, of two distinct electrical powers be found, upon due consideration, to disagree with the general system of nature. It is one of the fundamental laws of nature, that action and reaction are inseparable and equal; and, when we look round, we find that every power which is exerted in the material world meets with a counteracting power, which controuls and regulates its effects, so as to answer the wise purposes of providence. †

MR. SYMMER also alledges, in proof of his two distinct powers of electricity, the experiment which Dr. Franklin has related, of piercing a quire of paper with an electric shock. He thought that the bur which was raised on both sides of the paper was produced by two fluids, moving in two different directions. To show the manner in which this stroke was made more evidently,

* Phil. Transf. Vol. 57. p. 458. † Ib. Vol. 51. pt. 1. p. 380.

he mentions two other similar experiments, in which the circumstances of the stroke were a little varied.

A PIECE of paper, covered on one side with Dutch gilding, and which had been left accidentally between two leaves, in a quire of paper in which the former experiment had been made, was found to have the impression of two strokes upon it, about a quarter of an inch from each other; the gilding being stripped off, and the paper left bare for a little space in both places. In the center of one of these places was a little round hole, in the other only an indenture or impression, such as might have been made with the point of a bodkin.

THESE observations Mr. Symmer communicated to Dr. Franklin, who, notwithstanding Mr. Symmer was endeavouring to establish a theory of electricity contrary to his own, with the generosity natural to him, assisted him with his apparatus in making another experiment in pursuance of that mentioned above.

In the middle of a paper book, of the thickness of a quire, Mr. Symmer put a slip of tinfoil; and in another, of the same thickness, he put two slips of the same sort of foil, including the two middle leaves of the book between them. Upon striking the two different books, the effects were answerable to what he expected. In the first, the leaves on each side of the foil were pierced, while the foil itself remained unpierced; but, at the same time, he could perceive an impression had been made on each of its surfaces, at a little distance from one another; and such impressions were still more visible on the paper, and might be traced, as pointing different ways. In the second, all the leaves of the book were pierced, excepting the two that were between the slips of foil; and in these two, instead of holes, the two impressions in contrary directions were visible.

MR.

MR. SYMMER afterwards got an electrical apparatus of his own, formed on the model of that of Dr. Franklin, with which he frequently repeated the experiments above-mentioned, the result of all which he comprises in the three following observations.

1. WHEN a quire of paper, without any thing between the leaves, is pierced with a stroke of electricity, the two different powers keep in the same track, and make but one hole in their passage through the paper: not but that the power from above, or that from below, sometimes darts into the paper at two or more different points, making so many holes, which, however, generally unite before they go through the paper. They seem to pass each other about the middle of the quire, for there the edges are most visibly bent different ways; whereas, in the leaves near the outside of the quire, the holes very often carry more the appearance of the passage of a power issuing out, and exploding into the air, than of one darting into the paper.

2. WHEN any thin metallic substance, such as gilt leaf, or tinfoil, is put between the leaves of the quire, and the whole is struck; in that case, the counteracting powers deviate from the direct track, and leaving the path which they would in common have taken through the paper, only make their way in different lines to the metallic body, and strike it in two different points, distant from one another about a quarter of an inch, more or less; the distance appearing to be the least when the power is greatest: and whether they pierce, or only make impressions upon it, in either case, they leave evident marks of motion from two different parts, and in two contrary directions. It is this deviation from a common course, and the separation of the lines of direction consequent upon it, says he, that affords a proof of the exertion of two distinct and counteracting powers.

3. WHEN two slips of tinfoil are put into the middle of the

quire, including two or more leaves between them; if the electricity be moderately strong, the counteracting powers only strike against the slips, and leave their impression there. When it is stronger, one of the slips is generally pierced, but seldom both; and from what he had observed in such cases, he says it should seem, as if the power which issued from the outside of the phial acted more strongly than that which proceeded from within, for the lower slip was most commonly pierced. But this, he adds, may be owing to the greater space which the power from within has to move through before it strikes the paper.*

In the same paper, Mr. Symmer furnishes a remarkable instance of the power of an hypothesis in drawing facts to itself, in making proofs out of facts which are very ambiguous, and in making a person overlook those circumstances in an experiment which are unfavourable to his views.

When a phial is electrified but a little, Mr. Symmer says, if we touch the coating of it with a finger of one hand, and, at the same time, bring a finger of the other hand to the wire, we shall receive a pretty smart blow upon the tip of each of the fingers, the sensation of which reaches no farther. If the phial be electrified a degree higher, we shall feel a stronger blow, reaching to the wrists, but no farther. When, again, it is electrified to a still higher degree, a severer blow will be received, but will not be felt beyond the elbows. Lastly, when the phial is strongly charged, the stroke may be perceived in the wrists, and elbows; but the principal shock is felt in the breast, as if a blow from each side met there. This plain and simple experiment, says Mr. Symmer, seems obviously to suggest to observation the

* Phil. Transf. Vol. 51. pt. 1. p. 377. &c.

existence

existence of two distinct powers, acting in contrary directions; and, I believe, says he, it would be held as a sufficient proof by any person, who should try the experiment, with a view to determine the question simply from his own perceptions.*

IT is a sufficient answer to this remark of Mr. Symmer's, that if twenty people join hands, they may all be made to feel the shock in their wrists, or their elbows, without having their breasts affected in the least. And can it be supposed, that the two currents of electric fire could come at all their wrists or elbows, without passing through their breasts. According to Mr. Symmer's hypothesis, it should seem, that, in a large circle, those persons only who stood near the phial, on either hand, should feel a *small shock*; that a few persons more, at each extremity of the circle, should feel one something stronger; and that it could only be a very strong shock, which could at all affect the person who stood in the middle; and that then he should be affected the least of any person in the company. But all these consequences are contrary to fact.

THIS hypothesis of Mr. Symmer, notwithstanding he has failed in his application of it to the experiments above-mentioned, has attracted the notice of several electricians, both at home and abroad; and some persons seem inclined to adopt it, in preference to Dr. Franklin's theory. I shall therefore consider it more at large, when I come to treat of *theories* professedly; till which time, I take leave of this ingenious philosopher, and his two electric fluids.

THE experiments of Mr. Symmer excited the attention of Mr. Cigna, and led him to a course of experiments, which throws still more light, both upon the doctrine of the two electricities,

* Phil. Trans. Vol. 51. pt. 1. p. 373. &c.

and

and the Leyden phial. They are also a further illustration of the discovery of Mr. Canton, improved by Mr. Wilcke and *Æpinus*, of the mutual repelency of similar electric atmospheres.

HE took two white silk ribbons, just dried at the fire, and having extended them upon a smooth plain, either a conductor or a non-conductor, he drew over them the sharp edge of an ivory ruler, and found, that both the ribbons had acquired electricity enough to adhere to the plain; though, while they remained upon the plain, they shewed no other sign of it. If they were both taken off from the plain together, they attracted one another, the upper having acquired the resinous, and more powerful; and the lower the vitreous, and weaker electricity. If they were taken up separately, they repelled one another, having both acquired the resinous electricity.*

IN this separation of both the ribbons from the plain, as also in their separation, afterwards, from one another, electric sparks were visible between them; but if they were again put upon the plain, or joined together, no light appeared upon their second separation, without another friction of the ruler. Also, when, by being taken off separately, they had been made to repel one another, if they were laid on the plain again, and taken off together, they would not attract; and if, by being taken off together, they had first been made to attract one another, and were laid on the plain a second time, and then taken off separately, they would not repel, without another friction.

WHEN, by the operation above-mentioned, they had acquired the same electricity, if they were placed, not upon the smooth body on which they had been rubbed, but on a rough one, and a conductor, as hemp or cotton, not very dry; they would, upon

* *Memoirs of the Academy at Turin for the year 1765, p. 31.*

being

being separated, show contrary electricities; which, when they were joined together, would disappear as before.*

If they had been made to repel one another, and were afterwards placed one upon the other, on the rough surface above-mentioned, they would, in a few minutes, attract one another; the lower of the two ribbons having changed its resinous into a vitreous electricity.

If the two white ribbons received their friction upon the rough surface, they always acquired contrary electricities; the upper of the two having the resinous, and the lower the vitreous, in whatever manner they were taken off.

THE same thing that was done by a rough surface was done by any pointed conductor. If two ribbons, for instance, were made to repel, and hang parallel to one another; and the point of a needle were drawn opposite to one of them, along its whole length, they would presently rush together; the electricity of that ribbon to which the needle was presented being changed into the contrary. †

IN the same manner in which one of the ribbons changed its electricity, a ribbon not electrified would acquire electricity; viz. by putting it upon a rough surface, and laying an electrified ribbon upon it; or by holding it parallel to an electrified ribbon, and presenting a pointed conductor to it.

HE placed a ribbon not quite dry under another that was well dried at the fire, upon a smooth plain; and when he had given them the usual friction with his ruler, he found that, in what manner soever they were removed from the plain, the upper of them had acquired the resinous, and the lower the vitreous electricity. ‡

* *Ib.* p. 33.† *Ib.* p. 34.‡ *Ib.* p. 35.

IF both the ribbons were black, all the above-mentioned experiments succeeded, in the same manner as if they had been white. *

IF, instead of his ivory ruler, he made use of any skin, or of a piece of smooth glass, the event was the same; but if he made use of a stick of sulphur, the electricities were, in all cases, the reverse of what they were before; the ribbon which was rubbed having always acquired the vitreous electricity.

WHEN he made use of paper, either gilt or not gilt, the events were uncertain.

WHEN the ribbons were wrapped in paper, gilt or not gilt, and the friction was made upon the paper laid upon the plain above-mentioned, the ribbons acquired, both of them, the resinous electricity. †

IF the ribbons were one black, and the other white, which ever of them was laid uppermost, and in whatever manner the friction was made, the black generally acquired the resinous, and the white the vitreous electricity. ‡

He observed, however, the following constant event; that whenever the texture of the upper piece of silk was loose, yielding, and retiform, like that of a stocking, so that it could move, and be rubbed against the lower, and the rubber was of such a nature as to impart but little electricity to glass; the electricity which the upper piece of silk acquired, did not depend upon the rubber, but upon the body it was laid upon; in which case the black was always resinous, and the white vitreous. But when the silk was of a close texture, hard and rigid, and when the rubber was such as imparted a great degree of electricity to glass; the electricity of the upper piece did not depend upon the

* *Memoirs of the Academy at Turin for the year 1765*, p. 35. † *Ib.* p. 36. ‡ *Ib.* p. 38.
lower,

lower, but upon the rubber. Thus a white silk stocking, rubbed with gilt paper upon glass, became resinous, and the glass vitreous; but if a piece of silk, of a firmer texture, was laid upon a plate of glass, it always acquired the vitreous electricity, and the glass the resinous, if it was rubbed with sulphur; and for the most part, if it was rubbed with gilt paper.* So that the silk that was rubbed received its electricity, sometimes from the rubber, and sometimes from the substance placed under it; according as it received greater friction from the one or the other, or in proportion as one or the other was more proper to give electricity to glass.

ANOTHER set of experiments, which the same Johannes Franciscus Cigna made, illustrate the adhesion of Mr. Symmer's electrical stockings to bodies with smooth surfaces. He insulated a plate of lead, and bringing an electrified ribbon near it, observed that it was attracted very feebly. Bringing his finger to the lead, a spark issued out of it, upon which it attracted the ribbon vigorously, and both together showed no signs of electricity. Upon the separation of the ribbon, they again both appeared to be electrified, and a spark was perceived between the plate and the finger.†

LAYING two plates of glass upon a smooth conductor communicating with the ground, and rubbing them in the same manner as the ribbons had been rubbed, they likewise acquired electricity, and adhered firmly, both to one another, and to the conductor. If it were a plate of lead, not very thick, it would be supported by the attraction. When they were together, they showed no other signs of electricity. ‡

WHEN the two plates of glass were separated from the con-

* *Ib.* p. 40. † *Ib.* p. 43. ‡ *Ib.* p. 52.

ductor, and kept together, they showed, on both sides, a vitreous electricity; and the conductor, if it had been insulated, was seen to have contracted a resinous electricity.

THE two plates of glass themselves, when separated, were possessed of the two electricities; the upper of the vitreous, and stronger; and the lower of the resinous, and weaker.

WITH a rough conductor, whether they were originally rubbed upon it, or brought to it, after they had been rubbed upon a smooth one, they scarce contracted any electricity; though, when they were separated from one another, they were affected as before.

UPON this principle, he endeavours to account for the non-excitation of a globe or tube from which the air is exhausted, or which is lined with conducting substances. In this case, he says, the vitreous electricity on the external surface of the glass is balanced by the resinous in the inward coating, or in the vacuum which serves instead of a coating; and therefore it is in the situation of the plates of glass while they lie upon the conductor above-mentioned: but when the inward coating is taken away, the electricity appears on the outside, without any fresh excitation; as when the plates were removed from the conductor.*

WHEN he laid a number of ribbons of the same colour upon the smooth conductor, and drew his ruler over them; he found, that when he took them up singly, they all gave sparks, at the place where they were separated, as the last ribbon did with the smooth plate, and had all acquired the resinous electricity. †

If they were all taken from the plate together, they cohered in one mass, which, on both sides, appeared to be resinous. If they were laid upon the rough conductor, in the same order

* *Memoirs of the Academy at Turin, for the year 1765, p. 54.*

† *Ib. p. 61.*

(whereby

(whereby the opposite electricities were brought to an equilibrium) and they were all separated singly, beginning with the lowest, sparks appeared as before; but all the ribbons had acquired the vitreous electricity; except the uppermost, which retained the resinous electricity it had received from the friction.*

If they received the friction upon the rough conductor, and were all taken up at once (in order to have a bundle in which the opposite electricities were balanced) all the intermediate ribbons acquired the electricity, either of the highest or the lowest ribbon, according as the separation was begun with the highest, or the lowest.

If two ribbons were separated from the bundle at the same time, they clung together; and, in that state, showed no sign of electricity, as one of them alone would have done. When they were separated, and the different electricities were manifest, the electricity was observed to reside in the outermost, and was opposite to that by which they had both adhered to the bundle, but much weaker. †

He placed a number of ribbons upon a plate of metal, which received electricity from the globe, while he held a pointed body to the other side of the ribbons. The consequence was, that all the ribbons became possessed of the electricity opposite to that of the plate, or of the same, according as they were taken off; except the most remote, which always kept an electricity opposite to that of the plate.

From these experiments he infers, that as electricity is propagated from the outermost ribbon to those underneath it, or else from the plate below to those next above it, when they are separated; when the coating is separated from a charged pane of

* *Ib.* p. 61.† *Ib.*

glafs, it likewife deposits its electricity upon the superficies of the glafs, the phenomena being the same in both. For when he put metal coatings on the side of a plate of glafs, without any cement, they adhered firmly to the glafs when it was charged, and a light appeared upon their being separated from it, as in the case of the ribbons.*

WHEN he coated a number of ribbons in the same manner, and charged them, the coatings adhered firmly to the ribbons; but he could never separate one of them, but (in consequence of the loose texture of the silk) a spark would go to the opposite coating, which immediately fell off, the whole being then discharged. †

BUT he thought the coatings did not deposit all their electricity on the plate, when they were taken off; for though, when both were taken off, the electricities of the two sides still balanced one another (because each retained the same diminished quantity) yet, when one superficies of the glafs, or of the ribbons, received its electricity from friction, and the other only from the opposite coating; he observed, that the electricities which balanced one another while the coating was on, were no longer balanced when it was taken off; the electricity of the surface which was rubbed then prevailing, because the conducting coating had, upon its separation, taken part of its electricity along with it. ‡

To confirm this, he adds another experiment. He charged a pane of glafs, coated on one side, while the other received electricity by a pointed conductor from the machine: He likewise inverted the plate, and made the coated side communicate with the prime conductor, while a pointed piece of metal was

* *Memoirs of the Academy at Turin for the year 1765*, p. 63. † *Ib.* p. 64. ‡ *Ib.* p. 65.
presented

presented to the opposite side; and, in both cases, found, that while the coating remained, the two electricities balanced one another; but that when the coating was slipped off, the electricity of the opposite side prevailed, so as to be apparent on both sides of the plate. *

* *Ib.* p. 65.

SECTION

SECTION VII.

THE HISTORY OF THE LEYDEN PHIAL CONTINUED.

GREAT as were the discoveries of Dr. Franklin concerning the Leyden phial, he left some curious particulars for this period of the history of electricity; and the subject is by no means exhausted. Many of the properties of this wonderful bottle, as the Doctor calls it, are still unexplained. But as more and more light is perpetually thrown upon it, let us hope that, at length, we shall thoroughly understand this great experiment. The greatest discovery concerning the properties of the Leyden phial, in this period, hath already been related in the account of Mr. Wilcke's and Æpinus's method of giving the shock by means of a plate of air; and other observations have, likewise, been occasionally mentioned, in places where their connection required them to be inserted. This section, however, will contain several experiments of a miscellaneous nature, which are well worth notice.

IMMEDIATELY upon the discovery of the shock given by glass, all electricians attempted to charge other electric substances; but none of them succeeded before Signior Beccaria. He found that a very smooth plate of sealing wax, made by pouring that substance,

stance, when melted, upon an oiled marble table, would receive a considerable charge. *

AFTER trying several other electrics, in the same manner, he found that a mixture of pitch and colophonia was charged less than sealing wax, but more than sulphur, and a great deal more than pitch alone. †

BUT the most curious experiment of this philosopher, relating to this subject, was made with a view to ascertain the real direction of the electric fluid in a discharge. He suspended a coated plate of glass by a silk thread, and having charged it, and kept it perfectly still; he observed that no motion was given to it, when the discharge was made by a crooked wire approaching both the sides at the same time. The experiment, in fact, proved the reaction of the glass upon the electric matter; whereby the plate were kept still, notwithstanding the fluid rushed with great violence from one side to the other. He compares the glass to an ivory ball placed between two others, which keeps its place, when, by an impulse given to one of them, the opposite ball flies off. ‡

MR. HARTMAN of Hanover has published an account of a curious experiment, which seems to show the progressive motion of the electrical explosion. He passes a shock through a great number of cannon balls, sometimes to the number of forty, placed near one another, upon small drinking glasses; when all the sparks are seen at the same moment of time, and all the snappings make but one report. But when he substitutes eggs instead of the balls of metal, the progress of the explosion is visible, every two giving a snap and a flash separately. This experiment requires weather very favourable to electricity, and, he says has generally succeeded

* *Lectures dell' elettricismo*, p. 64. † *Ib.* p. 66. ‡ *Ib.* p. 72.

the best with only ten or twelve eggs.* This author has not expressly said in which direction the sparks ran; but as he adopts Dr. Franklin's hypothesis, it may be presumed, that he imagined it to go from the positive to the negative side of the charged glass.

A VERY ingenious experiment has also been made by Amadeus Lullin of Geneva, in order to ascertain the direction of the electric fluid in explosions, and which he thinks comes nearer to the *experimentum crucis* than any other. He placed a common card in the circuit of the electrical explosion, while the wire which communicated with the positive side of the jar lay on one side of it, and that which communicated with the negative side lay on the other, their extremities not being placed opposite to one another, but at some distance. Things being thus circumstanced, he observed, that upon making the discharge, the card was constantly pierced close to the extremity of the wire which communicated with the negative side of the jar, as if the electric fluid, rushing from the positive of the jar into the wire which communicated with it, and issuing from the extremity of it, was driven by its own impulse along the surface of the card, and did not pierce it, till it came opposite to the extremity of the other wire, which communicated with the negative side, by which it was strongly attracted. †

A VERY curious and elegant experiment on the Leyden phial was made by professor Richman of Petersburg, whose unfortunate death will be related in this history.

HE coated both sides of a pane of glass, within two or three inches of the edge, and fastened linen threads to the upper part of the coating, on both sides; which, when the plate was not

* Abhandlung, p. 58, &c.

† Dissertatio Physica, p. 24.

charged

charged, hung down in contact with the coating; but setting the plate up right, and charging it, he observed, that when neither of the sides was touched by his finger, or any other conductor communicating with the earth, both the threads were repelled from the coating, and stood at an equal distance from it; but when he brought his finger, or any other conductor, to one of the sides, the thread hanging to that side fell nearer to the coating, while the thread on the opposite side receded as much; and that when his finger was brought into contact with one of the sides, the thread on that side fell into contact with it likewise, while the thread on the opposite side receded to twice the distance at which it hung originally; so that the two threads always hung so as to make the same angle with one another.*

ÆPINUS shows, that it is not strictly true, that an insulated person, discharging the Leyden phial through his own body, contracts no electricity. Electrifying a large plate of air, he observed, that if the nearer plate, (by which I suppose he means that which he first touched) was electrified positively, he acquired a positive electricity by the discharge; but if it were negative, he acquired a negative electricity. He supposes that the reason why the experiment did not succeed with Dr. Franklin, was, that the surfaces with which he tried the experiment were not large enough to make the effect sensible; and that the distance of the metal plates was, likewise, too small, as it necessarily must be in charging of glass. †

MR. CIGNA has invented a new method of charging a phial, upon the principle discovered by Mr. Canton and Mr. Wilcke; viz. that the electricity of one body repels that of another,

* Æpini Tentamen, p. 335. † Ib. p. 27.

especially if it have a flat surface, and gives it the contrary electricity.

HE insulates a smooth plate of lead, and while he brings an electrified body, as a stocking, to it, he takes a spark with the wire of a phial from the opposite side; and removing the stocking, he takes another spark with his finger, or any conductor communicating with the ground. Bringing the stocking nearer the plate a second time, he takes a second spark, with the wire of the phial, as before; and, removing it again, takes another, in the same manner, with his finger. This operation he continues, till the phial is charged; which, in favourable weather, may be done with very little diminution of the electricity of the stocking. *

IF, instead of taking a second spark with his finger, he had taken it with the wire of another phial, that would have been charged likewise, with no additional labour, and with an electricity opposite to that of the other phial. If the second spark were taken with the coating of the same phial, the charging would be accelerated, but the operation would be troublesome to manage.

THE theory of this new method of charging a phial is very easy, upon the principle referred to above. The approach of the electricity in the stocking, not being able to enter the broad smooth surface of the metal, drives the electric fluid out of that part of the plate which is opposite to it, to the other side, which, being thereby overcharged, will part with its superfluity to the wire of the phial. The stocking being taken away, the plate will have less than its natural share of the electric fluid, and will therefore readily take a spark, either from the finger, or the wire of another phial. †

* *Memoirs of the Academy at Turin for the year 1765*, p. 49.

† *Ib.* p. 51.

THE same ingenious philosopher makes a considerable difference between the electric fluid which gives the shock, and that on which some other phenomena of coated glass depend. The former, which is far the greatest quantity, he supposes to reside, either in the coating itself, or on the surface of the glass; whereas the other, he imagines, to have entered the pores, and affected the substance of the glass itself.]

HE laid two plates of glass, well dried, one upon the other, as one piece; the lower of them being coated on the outside; and, when they were insulated, he alternately rubbed the uppermost plate with one hand, and took a spark from the coating of the lower with the other, till they were charged; when the coating, and both the plates adhered firmly together. Giving a coating to the other side, and making a communication between that and the other coating, the usual explosion was made. But the plates, though thus discharged, still cohered; and though, while they were in this state, they showed no other sign of electricity; yet, when they were separated, they were each of them found to be possessed of electricity opposite to that of the other.

IF the two plates were separated before they were discharged, and the coating of each were touched, a spark came from each; and when put together, they would cohere as before, but were incapacitated for giving a shock.*

HE, therefore, compares the electricity which gives the shock to the electricity of the metal plate in the former experiment; which is lost with taking one spark, as the silk is removed from it, and is different from the electricity by which the two plates of glass cohere. The one is dispersed at once, but the other slowly; the one existing, as he supposes, in the conductors, or

* *Ib.* p. 55.

upon the surfaces of the electrics, and the other in the substance itself.*

AMONG experiments relating to the electric shock, we ought to mention what has been observed within this period of its amazing force in melting wires, and producing other surprising effects.

THAT even artificial electricity, says Dr. Watson, in a paper read at the Royal Society June 28th. 1764, when in two great a quantity, and hurried on too fast, through a fine iron wire, has a remarkable effect upon the wire, appears from a very curious experiment of Mr. Kinnersley. This gentleman, in the presence of Dr. Franklin, made a large case of bottles explode at once through a fine iron wire. The wire at first appeared red hot, and then fell into drops, which burned themselves into the surface of his table or floor. These drops cooled in a spherical figure like very small shot, of which Dr. Franklin transmitted some to Mr. Canton, who repeated the experiment. This proves the fusion to have been very complete; as nothing less than the most perfect fluidity could give this figure to melted iron.

MR. CANTON, in a note subscribed to the same paper, observes, that the diameter of a piece of Mr. Kinnersley's wire, which he received from Dr. Franklin, was one part in 182 of an inch. He adds, that artificial lightning, from a case of thirty five bottles, would entirely destroy brass wire, of one part in 330 of an inch. At the time of the stroke, he says, a great number of sparks, like those from a flint and steel, would fly upwards, and laterally, from the place where the wire was laid, and lose their light, in the day time, at the distance of about two or three inches. After the explosion, a mark appeared on

* Memoirs of the Academy at Turin for the year 1765, p. 56.

the table, the whole length of the wire, and some very round particles of brafs were discovered by a magnifier near the mark, but no part of the wire itself could be found. *

SIGNIOR BECCARIA was able to melt small strips of metal, without inclosing, or covering them with pieces of glafs. But he thought that the same colour was impressed upon glafs by all the metals; and imagined that this circumstance was a trace of the fundamental principles being the same in them all. †

MR. DALIBARD observed, that, when a large pane of glafs discharged itself, the polish was taken off at the place of the discharge, and that the track it left behind it was usually, as he expresses it, in the zig zag form. With the piece of glafs with which he made these discharges he pierced 160 leaves of paper. It contained 1200 square inches. ‡

MR. WILCKE says, that, if a small piece of metal be hung in a silken string, opposite to a part of a glafs jar, made thin for the purpose; upon the spontaneous discharge through that place, the piece of metal will be driven off to the distance of five or six inches. ||

MR. WINKLER fired the seeds of clubmoss [*lycopodium*] by discharging a phial through a quantity of them. He also fired the *aurum fulminans* placed upon a piece of parchment, which was torn to pieces by the explosion. §

By the electric shock, Signior Beccaria could melt borax, and glafs. But the most remarkable of his experiments with the electric shock are those by which he *revivified metals*. This he did by making the explosion between two pieces of the calces.

* Phil. Transf. Vol. 54. p. 208. † *Elettricismo artificiale*, &c. p. 154, 155.

‡ *Histoire abrégée*, p. 84. § Remarks on Franklin's Letters, p. 266.

§ Phil. Transf. Vol. 38. pt. 2. p. 773.

In this manner he revived several of the metals, and among others, zink. He even produced real quicksilver from cinnabar.* In this case of revivifications, he always observed streaks of black beyond the coloured metallic stains, owing, as he imagined, to the phlogiston driven thither from the parts that were vitrified, when the other part revived the calx. † Probably, the phlogiston which revived the calces was in that black dust, which the electric shock will throw from metals, as will be related in its proper place.

ANOTHER curious experiment he made with the electric shock, by discharging it through some brass dust, sprinkled between two plates of sealing wax. The whole was perfectly luminous, and transparent. ‡ An experiment which throws some light upon one of Mr. Hawkesbee's.

WITH the electric shock he also made that capital experiment, on which he lays so much stress in his theory of thunder storms, and by which he proves, that the electric matter forces into its passage all light conducting substances; by means of which it is enabled to pass through a quantity of resisting medium, which it could not otherwise do. He put a narrow piece of leaf silver between two plates of wax, laying it across the plates, but so as not quite to reach one of the sides. The discharge being made through this strip of metal, by bringing a wire opposite to the silver, at the place where it was discontinued; the silver was found melted, and part of it dispersed all along the track that the electric matter took, between the plates of wax, from the silver to the wire. || An accident gave him occasion to observe another fact of a similar nature. He once, inadvertently, received the charge of a small jar, through some smoak of spirit of

* Lettere dell' elettricismo, p. 282. † Ib. p. 255. ‡ Ib. p. 257. || Ib. p. 248.
nitre

nitre; when a hole was made in his thumb, where the fire entered; and which he thought could only have been made by the nitre, which was carried along with the electric fluid.*

VIOLENTLY as the electric explosion generally affects the human body, we have accounts of some persons who could not be made to feel it; particularly three or four mentioned by Mr. Muschenbroeck, among whom was a young woman. † I have also been told, that this was the case with a person near Leeds, who was at the same time a little paralytic.

I SHALL close the history of the Leyden phial for this period with the accounts of some extremely curious facts, which Mr. Canton gives me leave to publish relating to this subject. They certainly deserve the utmost attention of philosophers, and may probably throw some light upon the electricity of the tourmalin.

He procured some thin glass balls, of about an inch and a half diameter, with stems or tubes of eight or nine inches in length, and electrified them, some positively on the inside, and others negatively, after the manner of charging the Leyden phial, and then sealed them hermetically. Soon after, he applied the naked balls to his electrometer, and could not discover the least sign of their being electrical: but holding them at the fire, at the distance of five or six inches, they became strongly electrical, in a very short time; and more so when they were cooling. These balls would, every time they were heated, give the electric fire to, or take it from other bodies, according to the *plus* or *minus* state of it within them. Heating them frequently, he found, would sensibly diminish their power, but keeping one of them under water a week did not appear in the least to impair it. That which he kept under water was charged the 22d. of Sep-

* Ib. p. 249.

† Monthly Review October 1767, p. 250.

tember 1760, was several times heated before it was kept in water, and had been heated frequently afterwards; and yet it still retained its virtue to a considerable degree, on the 31st. of October following, when he sent an account of it to Dr. Franklin. The breaking two of his balls accidentally gave him an opportunity of measuring their thickness, which he found to be between seven and eight parts in 1000 of an inch.

THE balls mentioned in the account above, which was written six years ago, still retain their virtue, but in a less degree.

MR. LULLIN also found, that a glass tube charged, and hermetically sealed, would show signs of electricity when it was heated. *

* *Dissertatio Physica*, p. 324

SECTION

SECTION VIII.

EXPERIMENTS AND OBSERVATIONS CONCERNING
ELECTRIC LIGHT.

MY reader has been informed of the necessity I was under of dividing the business of this period of my history into several parts. He has already seen titles which he could not have expected from the divisions of the preceding periods, but he would perhaps least of all expect a distinct section upon electric light; and yet the experiments and observations which have been made, immediately relating to this subject, are so many, that they deserve a place by themselves. And I would rather err by making too many subdivisions, than too few; because, above all things, I would wish to preserve perspicuity, which is chiefly injured by crowding together things dissimilar.

MANY experiments had been made very early, by Mr. Hawkebee and others, on electricity, and particularly electric light, in vacuo; but so little was, at that time, known of the nature of electricity in general, that, comparatively, little use could be made of these experiments. Very fortunately, Dr. Watson happened to turn his thoughts that way, after the great discovery of the accumulation of electricity in the Leyden phial; and by

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this means he discovered, that our atmosphere, when dry, is the agent by which, with the assistance of other electrics per se, we were enabled to accumulate electricity upon non-electrics (he might have added electrics too) that is, to communicate to them a greater quantity of electricity than those bodies naturally have. That upon the removal of the air, the electric fluid pervaded the vacuum to a considerable distance, and manifested its effects upon any non-electric substances by which it was terminated.

THIS he demonstrated by one of the most beautiful experiments which the whole compass of electricity yet exhibits. He exhausted a glass cylinder, three feet in length, and three inches in diameter, with a contrivance to let down a brass plate, as far as he pleased, into it; in order to make it approach another plate, fixed near the bottom of the vessel.

THIS cylinder, thus prepared, he insulated, and observed, that when the upper plate was electrified, the electric matter would pass from one plate, to another, at the greatest distance to which the brass plates could be drawn; and that the brass plate at the bottom of the cylinder was strongly electrified, as if a wire had connected it with the prime conductor. It was a most delightful spectacle, he says, when the room was darkened, to see the electric matter in its passage through this vacuum; to observe, not as in the open air, small brushes or pencils of rays, an inch or two in length, but coruscations of the whole length of the tube, and of a bright silver hue. These did not immediately diverge, as in the open air, but frequently, from a base apparently flat, divided themselves into less and less ramifications, and resembled very much the most lively coruscations of the aurora borealis.

SOMETIMES he observed, that when the tube had been exhausted in the most perfect manner, the electric fluid was seen

to pass between the brass plates in one continued stream, of the same dimensions throughout its whole length; which, he thought demonstrated, that the cause of that very powerful mutual repulsion of the particles of electric fire, which is seen in the open air, is more owing to the resistance of the air, than to any natural tendency of the electricity itself. For, in the open air we observe that these brushes, when the electricity is strong, diverge so much, as to form almost a spherical figure.*

He made this vacuum part of a circuit necessary to make the discharge of a phial; and, at the instant of the explosion, there was seen a mass of very bright embodied fire, jumping from one of the brass plates in the tube to the other. But this did not take place when one of the plates was farther distant from the other than ten inches. If the distance was greater, the fire began to diverge, and lose part of its force: and this force diminished in proportion to its divergency, which was nearly as the distance of the two plates.

To find a more perfect vacuum for the passage of the electric fluid, he had recourse to an excellent invention of Lord Charles Cavendish; who, by means of a long bent tube of glass, filled with mercury, and inverted, made all the bended part of it (which was above the mercury) the most perfect vacuum that can be made by man. This vacuum Dr. Watson insulated, and one of the basons of the mercury being made to communicate with the conductor, when some non-electric substance touched the other, the electric matter pervaded the vacuum in a continued arch of lambent flame, and, as far as the eye could follow it, without the least divergency.

CONNECTING one of the basons with the machine, which was

* Phil. Trans. Vol. 47. p. 367.

insulated, the fire was seen pervading the vacuum in a contrary direction. And this he considered as the *experimentum crucis* of two principles which he had advanced before; viz. that electricity is furnished to the conductor, not by the excited electric, but from the non-electrics in contact with the rubber; and that we are able to take from, or add to that quantity of electricity, which is naturally inherent in bodies.

HE also observed, that if, in the forementioned circumstances, the hand of a person standing upon the floor was brought near the side of the glass, the coruscations would dart themselves that way in a great variety of forms, extremely curious to behold.

BUT the Doctor found, that even this vacuum, did not conduct so perfectly as metals, or water; because a person standing upon the floor, and applying his finger to the upper brass plate, received a smart stroke. This he conceived to arise from the electricity of the brass being so much more rarified than that of the body of the man who applied his finger.*

MR. WILSON engaged Mr. Smeaton, the inventor of a new and more perfect kind of air pump, to make some electrical experiments in vacuo. The following is the account of them that he transmitted to Mr. Wilson. They are, in several respects, similar to those made by Dr. Watson, and yet are attended with a considerable variety of circumstances.

A GLASS vessel, about one foot in length, and eight inches in its greatest diameter, open at both ends, had one of its ends closed by a brass ferrule, which constituted one of the centers on which it turned; the other end was closed with a metal plate. In the center of this plate was a square stem, which was applied to the arbor of a lath, by which the glass was turned round. On one

* Phil. Transf. Vol. 47. p. 373.

side of this last plate was fixed a cork, by means of which the glass was screwed upon the air pump.

UPON rarefying the air within the glass about 500 times, and afterwards turning the glass in the lath, whilst, at the same time, it was rubbed with his hand; a considerable quantity of lambent flame, variegated with all the colours of the rainbow, appeared within the glass, under the hand. This light was pretty steady in every respect, except that every part of it was perpetually changing colour.

WHEN a little air was let into the glass, the light appeared more vivid, and in a greater quantity; but was not so steady; for it would frequently break out into a kind of coruscations, like lightning, and fly all about within the glass. When a little more air was let in, the flashing was continual, and streams of bluish light seemed to issue from under his hand, within the glass, in a thousand forms, with great rapidity; and appeared like a cascade of fire. Sometimes it seemed to shoot out into the forms of trees, moss, &c.

WHEN more air was let in, the quantity of light was diminished, and the streams composing the flashes narrower. The glass now required a greater velocity, and harder friction. These circumstances increased as more air was let in; so that, by such time as the glass was one third full of air, these coruscations quite vanished, and a much smaller quantity of light appeared partly within, and partly without the glass. And when all the air was let in, the light appeared wholly without the glass, and much less in quantity than when the glass was in part exhausted.*

MR. CANTON, in repeating Dr. Watson's experiment with the Torricellian vacuum, observed one circumstance attending

* Wilson's Essay, p. 216.

it,

it, which throws great light upon the Leyden phial. He observed, that when the excited tube was brought near one of the basons of this machine (insulated) a light was seen through more than half of the vacuum; which soon vanished, if the tube was not brought nearer, but which appeared again as it moved farther off; and that this appearance might be repeated several times, without exciting the tube afresh.

THIS experiment he considered as a kind of ocular demonstration of the truth of Dr. Franklin's hypothesis, that when the electric fluid was condensed on one side of the glass, it was repelled from the other, if it met with no resistance. Thus, at the approach of the excited tube, he supposed the fire to be repelled from the inside of the glass surrounding the vacuum, and to be carried off through the columns of mercury, but to return again as the tube was withdrawn.*

THIS curious experiment Mr. Canton showed, and explained to Mr. Wilson; who afterwards expatiated upon it, in a book published by him and Dr. Hoadly in conjunction, intitled *Observations on a series of electrical experiments*; in a note of which, p. 28, he says, *Mr. Canton has taken notice of this vanishing and returning of the light.*

MR. CANTON has since diversified this beautiful experiment, by bringing the excited tube to another glass tube, exhausted, and hermetically sealed; by which means, he exhibits the perfect appearance of an aurora borealis. The flame from one of its extremities, which is in a manner coated by the hand which

* Phil. Transf. Vol. 48. pt. 1. p. 356. It has been seen p. that this observation, of the return of the electric light in *vacuo*, was made before the discovery of the Leyden phial by Mr. Grumert of Biala in Poland, but this was unknown both to Dr. Watson and Mr. Canton.

holds

holds it, will dart to the other extremity, at uncertain intervals of time, for near a quarter of an hour together, without repeating the application of the excited tube.

WHEN it was generally agreed among electricians, that what had been called vitreous and resinous electricity were in reality a redundancy of the electric fluid in one case, and a deficiency in the other; and when, in consequence of this supposition, the one was called positive, and the other negative electricity; there still remained some doubt which of the two was positive, and which negative. Mr. Wilson, in a paper read at the Royal Society, December the 6th. 1759, recites an experiment which, he thought, put the matter beyond all dispute, and absolutely determined, that what had been called vitreous was really positive, and what had been called resinous was negative; as, indeed, had generally been supposed, though, as Mr. Wilson thought, without sufficient reason, notwithstanding what had been advanced by Dr. Franklin, and Mr. Canton upon that subject

REPEATING the beautiful experiment mentioned before, as first contrived by Lord Charles Cavendish, he says he attended to a circumstance which seemed to have been overlooked by Dr. Watson, who published the account of it. This was a singular appearance of light upon one of the surfaces of the quicksilver. To observe this remarkable appearance to more advantage, Mr. Wilson let a small quantity of air into the tube, by which means four columns of quicksilver were obtained, and consequently six visible surfaces, in one of the legs of the inverted tube. He then electrified the mercury in the other leg, while the mercury on the opposite side had a communication with the earth, when, the room being dark, the stream of electric light was visible through the whole length of the vacuum, and its general appearance

ance was of a seeming uniform density; except at the upper surfaces of each column, where about one tenth of an inch above the surface, the light was always considerably brighter; whereas the under surfaces exhibited no such an appearance, the light being rather less bright in those places than in the general appearance of the whole illuminated vacuum.

THIS luminous appearance Mr. Wilson ascribed to the resistance the fluid met with at the upper surface of the quicksilver, in endeavouring to get into it. He therefore inferred that excited glass electrified bodies positively, or gave them a greater quantity of the electric fluid than they had.

ELECTRIFYING, in the same place, with a cylinder of rosin, instead of glass, the luminous appearances were all on the under surfaces of the columns of quicksilver; from which he inferred, that rosin electrified bodies negatively, depriving them of part of the electric fluid which they naturally had; or, as he expresses it, occasioning a current of electric fluid to set the contrary way.

THESE luminous appearances, Mr. Wilson also considered, as a strong confirmation of the existence of a *medium*, at or near the surfaces of bodies, which hindered the entrance or exit of the electric fluid. A doctrine which Mr. Wilson had advanced, and laid great stress upon on several other occasions.*

THE arguments which to Mr. Wilson appeared conclusive, in proof of what is commonly supposed, that glass electrifies *plus*, and sulphur, &c. *minus*, did not appear so to Æpinus; though he acknowledges that the knobs of light in the vacuum did, in common with many other appearances, prove a real difference between the two electricities; and thought that it was very easy to conceive, that when an elastic fluid issues from a body, it should

* Phil. Transf. Vol. 51. pt. 1. p. 308.

be denser near the surface from whence it issues, than where it finds more liberty to expand itself. He might have added, that this might have been expected, from the mutual attraction which is supposed to subsist between the electric fluid, and other bodies. But *Æpinus* did not expressly mention this circumstance. Mr. *Wilson*, therefore, makes light of the objection; and adds, that when he related the experiment with the bent tube, in his letter to Dr. *Heberden*, he omitted some phenomena attending the fact; which greatly favoured the doctrine he advanced. If, says he, when glass is electrified, and applied to the first column, we suffer the electric fluid to pass along the tube in small quantities only, and at short intervals, little luminous streams will be seen to move from the first to the second column of quicksilver, and consequently from the glass. The like appearances happen, but in a contrary direction, when resin or amber is made use of, and applied to the same column. Glass, therefore, he concludes, electrifies *plus*, or fills bodies with more of this fluid than belongs to them naturally, and resin, &c. *minus*. *

It will excite no disagreeable sensation in the less grave and saturnine of my readers to be informed, that I have been amusing them with a controversy which took its rise from a deception: for Mr. *Canton* informs me, and gives me leave to inform the public, that the light which Mr. *Wilson* takes notice of, as appearing on one surface of the mercury in the double barometer of Lord *Charles Cavendish*, and which Mr. *Wilson* takes to be a proof of the existence of a medium on the surfaces of bodies, which hinders the entrance or exit of the electric fluid to some degree, he found to be caused by nothing but common air. For if the *Torricellian* vacuum be properly made, no difference of

* *Phil. Trans.* Vol. 55. pt. 438. p. 441.

light can be seen on the surfaces of the columns of mercury; but if as much air be let into the vacuum, as will make each column of mercury a quarter of an inch shorter than that of a good barometer, the light will appear much as Mr. Wilson has described it. When Mr. Wilson supposed that Dr. Watson, when he made the experiment of the Torricellian vacuum, did not attend to the singular appearance of light on one of the surfaces of the mercury, he little suspected that if the vacuum Dr. Watson made was free from air, there was no such singular appearance of light to be attended to. Air, Mr. Canton adds, must be condensed near the surface of all bodies that attract it; and will, therefore, be some hindrance to the exit, or entrance of the electric fluid, except the bodies be very sharply pointed.

SOME curious observations relating to electric light were made by Mr. Wilcke. Rubbing two pieces of glass together in the dark, he observed a vivid phosphoreal light: which, however, threw out no rays, but adhered to the place where it was excited. It was attended with a strong phosphoreal smell, but with no attraction, or repulsion. From this experiment he inferred, that friction alone would not excite electricity, so as to be accumulated upon any body; and that to produce this effect, the bodies rubbed together must be of different natures, with respect to their attracting the electric fluid. He, moreover, imagined, that all examples of phosphoreal light, without attraction, were owing to the same excitation of electricity, without the accumulation of it. Such he imagined to be the case of light emitted by the Bolognian stone, cadmea fornacum, rotten wood, pounded sugar, and glass of all kinds. *

A TUBE excited with a woollen cloth, on which white wax

* Wilcke, p. 123, 124.

or oil had been put, he says, threw out flames; each of which when examined, appeared to rise out of a little protuberance of fire. The flame was one, and very narrow at the bottom, but farther from the tube it divided into several ramifications; which always leaned to those parts of the tube which were the least excited, or to conductors in the neighbourhood.*

He says that, upon presenting a finger or other non-electric to an excited negative electric, a cone of light is formed; the base of which is at the finger, or other non-electric, and the apex at the electric, on the surface of which it spreads to a considerable distance all round. †

SOMETIMES, he had seen fiery particles thrown laterally from an irregular electric spark, which shone like stars, and were very like those which are produced by the collision of flint and steel. ‡

SUSPENDING various balls from his conductor, and presenting others to them, which were sometimes of glass and sometimes of metal, and varying them in every manner possible, he always found (except when two metal balls were used) that the light between them formed a cone, the base of which was always on the body which was positive, and the apex on that which was negative. He says that this criterion is sufficient to distinguish the two electricities from one another.

He observes that, at the apex of a cone issuing from pointed bodies, electrified positively, there is a cylindrical spark, out of which lucid rays, like a river, are darted. These rays, he says, form a lucid cone, the apex of which is turned towards the point from which the fire proceeds. Sometimes from the apex, or at some distance from it, there is a lucid point, which, he

* Wilcke, p. 125.

† Ib. p. 127.

‡ Ib. p. 130.

says, Haufenius calls *the fire of the second kind*, out of which flew streams of fire. The streams never issue from the electrified body itself, but always from this lucid point. He says, moreover, that this lucid point at the extremity of an electrified body, and which throws out lucid rays, forms the distinctive character of the positive cone.*

A NEGATIVE cone, he says, is small, consisting of very slender filaments, which immediately adhere to the point at which the light enters, or to its sides; and, if accurately examined, seems to form little cones, the bases of which rest upon the body.

WHEN he afterwards comes to consider the cause of negative cones of light, he owns himself to be at a great loss how to do it.

MR. WILCKE put English phosphorous upon a pointed body, which, in the dark, rendered the whole visible; and when he suspended this pointed body perpendicularly, the phosphoreal vapours were seen to ascend; but upon electrifying it, as it hung in the same direction, the vapours were carried downwards, and formed a very long cone, extending out of the middle of the cone of electric light, which was seen perfectly distinct from it. When the electrification was discontinued, the phosphoreal vapour ascended as at first. From this depression of the phosphoreal effluvia Mr. Wilcke infers the efflux of the electric fluid from the point, and upon the surface, and not only through the substance of the pointed body. It is pity that he did not try this curious experiment with pointed bodies electrified negatively. He would certainly have found the same depression

* Wilcke, p. 132.

of the phosphoreal effluvia, and would, probably, have retracted his conclusion concerning this proof of the efflux.*

MR. WILCKE also thought it to be a proof, that the electric matter did not only flow out of the substance of electrified bodies, but upon the surface of them, that a metallic ring, projecting ever so little beyond the point of a wire on which it had been put, prevents the appearance of the lucid point.

THE last observation which I shall recite of Mr. Wilcke concerning electric light is, that if a point not electrified be opposed to a point electrified positively, the cones of light, which, in other circumstances, would appear upon both of them, disappear; but that if a positive cone be opposed to a negative cone they both preserve their own characteristic properties. †

SIGNIOR BECCARIA was of opinion, that the direction of the electric fluid may be determined from the phenomena of pointed bodies. The *pencil* (by which he means the electric fire at a point electrified positively) he says, contracts as it approaches a flat piece of metal not electrified; whereas the *star* (by which he means the electric fire at a point electrified negatively) expands in the same circumstances, and has a small cavity near the point towards the large superficies. The pencil is attended with a snapping noise, the star makes little or no noise. He hardly gives any reason for the first of these phenomena; he only says, that such is the necessary consequence of a fluid issuing out of, or entering into a point. But the greater noise made by the pencil, he thought was made by the impulse given by the electric matter to the air, causing it to vibrate: and this must be greater when the fluid is thrown from the point into the air,

* Wilcke, p. 134. † Ib. p. 140.

than

than when it comes through different portions of the air, and meets in one point.*

WHEN two points are opposed to one another, he says, the phenomena are much the same in both. †

SIGNIOR BECCARIA observed that hollow glass vessels, of a certain thinness, exhausted of air, gave a light when they were broken in the dark. By a beautiful train of experiments, he found, at length, that the luminous appearance was not occasioned by the breaking of the glass, but by the dashing of the external air against the inside, when it was broke. He covered one of these exhausted vessels with a receiver, and letting the air suddenly on the outside of it, observed the very same light. This he calls his *new invented phosphorus*. ‡

THIS excellent philosopher produced a most beautiful appearance of electric light in the following manner. He conveyed positive electricity to a brass ball, suspended by a wire, within an exhausted receiver, when, upon presenting to it another ball in *vacuo*, the lower hemisphere of the former was most beautifully illuminated, with a visible electric atmosphere. When he conveyed negative electricity to the ball, the same beautiful illumination was observed on the ball presented to it. This experiment, he says, is a very delicate one, requiring great patience and dexterity, in adjusting the distances, &c. in order to make it succeed perfectly. ||

THAT electric light is more subtle and penetrating, if one may say so, than light produced in any other way is manifest from several experiments, particularly the remarkable one of Mr. Hawkebee; but none prove it so clearly as some made by the ingenious Mr. Lane, who gives me leave to mention these.

* *Elettricismo artificiale*, p. 63. † *Ib.* ‡ *Lettere dell' elettricismo*, p. 365, &c.
|| *Phil. Transf.* Vol. 56. p. 107.

WHEN he had, for some different purpose, made the electric shock pass over the surface of a piece of marble, in the dark; he observed, that the part over which the fire had passed was luminous, and retained that appearance for some time. No such effect of the electric shock having ever been observed before, he repeated the experiment with a great variety of circumstances, and found it always answered with all calcareous substances, whether animal or mineral, and especially if they had been burnt into a lime. And, as far as he had tried, many more substances would retain this light than would not do it; among others several vegetable substances would do it, particularly white paper. Tiles, and brick were luminous, but not tobacco-pipe clay, though well burnt.

THAT gypseous substances, when calcined, were luminous, appeared from bits of images made of plaster of Paris; and of this class, he says, is the famous Bolognian stone. But many bodies, he found, were luminous after the electric stroke, which were not apparently so, when exposed to the rays of the sun.

HE made these curious experiments by placing the chains, or wires that led from the conductor to the outward coating of his jar, within one, two, or three inches (according to the strength of the charge) from one another, on the surface of the body to be tried, and discharging a shock through them. If the stone was thin, he found, that if one chain was placed at the top, and the other at the bottom, it would appear luminous on both sides after the explosion.

MR. CANTON, to whom these experiments were communicated, clearly proved, that it was the *light* only that the substances retained, and nothing peculiar to electricity; and, moreover, after frequent trials, discovered a composition, which retains both common light, and that of electricity, much more strongly

strongly than either the Bolognian stone, or any other known substance whatever. With this new phosphorus he makes a great number of most beautiful experiments. The flash made by the discharge of a common jar, within an inch of a circular piece of it, of about two inches and a half in diameter, will illuminate it so much, that the figures on a watch plate may be easily distinguished by it in a darkened room, and it will retain the light half an hour.

I SHALL close this section of experiments and observations on electric light, with an account of a remarkable appearance which occurred to Mr. Hartman. When he had been making experiments four or five hours together, in a small room, and after going out of it, returned soon, with a lighted candle in his hand, walking pretty swiftly; he perceived a small flame to follow him, at the distance of about a step and a half, but it vanished when he stopped to examine it. He was a good deal alarmed at the appearance at first, but afterwards imagined it to be occasioned by the accension of the sulphur, which had been thrown into the air by the violent continued electrification. *

* Abhandlung, p. 135.

SECTION IX.

THE ELECTRICITY OF THE TOURMALIN.

THIS period of my history furnishes an entirely new subject of electrical inquiries; which, if properly pursued, may throw great light upon the most general properties of electricity. This is the *Tourmalin*: though, it must be acknowledged, the experiments which have hitherto been made upon this fossil stand like exceptions to all that was before known of the subject.

THE tourmalin, as Dr. Watson has in a manner demonstrated, was known to the antients under the name of the *lyncurium*. All that Theophrastus says concerning the *lyncurium* agrees with the tourmalin, and with no other fossil that we are acquainted with. He says, that it was used for seals, that it was very hard, that it was endued with an attracting power like amber, and that it was said, particularly by Diocles, to attract not only straws, and small pieces of wood, but also copper and iron, if beaten very thin; that it was pellucid, of a deep red colour, and required no small labour to polish it. The account which passed current among the ancients concerning the origin of this stone was

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fabulous,

fabulous, which made Pliny think that all that was said of it was fabulous too.

THIS stone, though not much attended to by European philosophers, till very lately, is common in several parts of the East Indies, and more particularly in the island of Ceylon, where it is called by the natives *tourmalin*. In this island the Dutch became acquainted with it, and by them it is called *afcentrikker*, from its property of attracting ashes, when it is thrown into the fire.

THE first account we have had, of late years, concerning this extraordinary stone is in the History of the Royal Academy of sciences at Paris for the year 1717; where we are told, that Mr. Lemery exhibited a stone, which was not common; and came from Ceylon. This stone, he said, attracted and repelled small light bodies, such as ashes, filings of iron, bits of paper, &c.

LINNÆUS, in his *Flora Zeylonica*, mentions this stone under the name of *lapis electricus*, and takes notice of Mr. Lemery's experiments.

NOTWITHSTANDING this, no further mention was made of this stone and its effects till some years after; when the Duc De Noya, in his letter to Mr. Buffon, presented to the Royal Society, informed us, that when he was at Naples, in the year 1743, the Count Pichetti, secretary to the king, assured him, that, during his stay at Constantinople, he had seen a small stone called *tourmalin*, which attracted and repelled ashes. This account the Duc De Noya had quite forgotten, but being in Holland in the year 1758, he saw, and purchased two of those stones. With these, in company with Messrs. Dauben-

ton and Adamson, he made a great number of experiments, of which he favoured the public with a particular account.*

BUT prior to the Duc De Noya's experiments, Mr. Lechman had acquainted Æpinus with the attractive power of the tourmalin, and furnished him with two of them, on which he made many experiments; the result of which he published in the History of the Academy of sciences and belles lettres at Berlin for the year 1756. The substance of the memoir is as follows.

THE tourmalin has always, at the same time, a positive and a negative electricity; one of its sides being in one state, and the other in the opposite; and this does not depend on the external form of the stone. These electricities he could excite in the strongest degree, by plunging the stone in boiling water.

If one side of the tourmalin be heated more than the other (as if it be laid upon a hot cake of metal) each of the sides acquire an electricity opposite to that which is natural to it; but if left to itself, it will return to its natural state.

If one of the sides of the tourmalin be rubbed, while the other is in contact with some conductor communicating with the ground; the rubbed side is always positive, and the other negative. If neither side be in contact with a conductor, both become positive. If, in the former of these cases, the tourmalin be rubbed, so as to acquire a sensible heat, and the side which is naturally positive be made negative, it will upon standing to cool, return to its natural state; but if it have acquired no sensible heat, it will not return to its natural state while any kind of electricity remains. If it be heated, even when it is rubbed and

* Phil. Transf. Vol. 51. pt. 1. p. 396.

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insulated,

insulated, (in which case both sides become positive) it will still return to its natural state upon cooling.

THE Duc de Noya mentions these experiments of *Æpinus*, but does not admit of a *plus* and *minus* electricity belonging to the tourmalin when heated. On the contrary, he says, that both the sides are electrified plus, but one of them more than the other, and that it was the difference between those degrees which led *Æpinus* into his mistake.*

THE tourmalin was introduced to the notice of the English philosophers by Dr. Heberden, who fortunately recollecting to have seen one of them, many years before, in the possession of Dr. Sharpe at Cambridge (and it was the only one known in England at that time) procured it for Mr. Wilson, who, though it was but a small one, repeated with it most of the experiments of *Æpinus*, so far as to satisfy himself that his opinion of its positive and negative power was well founded.

AFTERWARDS, Dr. Heberden, ever desirous of extending the bounds of science, procured some of these stones from Holland, and put them into the hands of those persons who were likely to make the best use of them, particularly Mr. Wilson and Mr. Canton; in whose hands they were not lodged in vain, as will appear by the brief account I shall subjoin of their experiments upon them.

MR. WILSON'S observations are too many, and too particular to be all inserted in this work. The result of them was, in the main, the same with that of *Æpinus*, establishing the opinion of the two different powers of this stone; but, contrary to *Æpinus*, he asserts, that when the sides of the tourmalin are unequally heated, it exhibits that species of electricity which is

* Phil. Transf. Vol. 57. pt. 1. p. 315.

natural

natural to the hotter side, that is, the tourmalin is *plus* on both sides, when the *plus* side is the hotter; and *minus* on both sides, when the *minus* side is the hotter.

UPON this Æpinus repeated all his former experiments, and still found the result of them agreeable to his former conclusion, and contrary to that of Mr. Wilson. Mr. Wilson also repeated his, without any variation in the event, and imagined the difference between him and Æpinus might arise from the different sizes of the tourmalins they made use of, or from their different manner of making the experiments. And it is evident, from the description of both their apparatuses, that of Mr. Wilson was much better calculated for the purpose of accurate experiments than that of Æpinus. Mr. Wilson also used a greater variety of methods of communicating heat to his tourmalins. He both plunged them in boiling water, held them to the flame of a candle, and exposed them to heated insulated electrics.*

THOUGH the detail of all Mr. Wilson's experiments would be, as I observed before, much too long for my purpose, I cannot help relating one of them, which was made with the last mentioned method of treating. He heated one end of a glass tube red hot, and when he had exposed what both he and Æpinus call the negative side of the tourmalin to it; he observed that about three inches of the heated part of the glass were electrified *minus*, though the glass beyond that was electrified *plus*, and continued so even after the glass was cold, the electric fluid having passed from the tourmalin to the glass; since these were the same appearances that were produced by presenting an excited tube to the heated glass.

* Phil. Transf. Vol. 55. p. 436. &c.

HE then applied the *plus* side of the tourmalin to the same heated glass, and found that the tube was electrified *minus*, above a foot in length, without the least appearance of a *plus* electricity beyond the *minus* one, as in the other experiment; and this *minus* electricity appeared when the tube was nearly cold. In this case he judged that the electric fluid had passed from the glass to the tourmalin.

MR. WILSON imagined that the tourmalin, as well as glass, was permeable to the electric fluid, and that the resistance to its entering the substance of it was less on what he calls the negative than on the positive side. These conclusions he drew from the two following experiments. Rubbing the positive side of the stone slightly, he found both sides electrified *plus*, but rubbing the negative side in the same manner, both sides were electrified *plus* more strongly than before.*

SEVERAL experiments led Mr. Wilson to conclude that the tourmalin resisted the exit and entrance of the electric fluid considerably less than glass, or even than amber; and upon the whole he infers, that the tourmalin differs in nothing from other electric bodies but in acquiring electricity by heat. †

EXAMINING a great number of tourmalins, he found that a line drawn from the *plus* part, through the center of the stone, would always pass through the *minus* part.

ALMOST all these tourmalins he greased over, and whilst they were warm enough to preserve the grease liquid, he tried each tourmalin separately, but found no alteration in the virtue of the stone, except that it was a little weakened; though it is well known, he says, that moisture of any sort readily conducts the electric fluid. If, therefore, the tourmalin had not a fixed

* Phil. Transf. Vol. 51. pt. 1. p. 327.

† Ib. Vol. 51. pt. 1. p. 329.

kind

kind of electricity, the *plus* and *minus* electricity, observable on the two sides of the stone, must, by this treatment, have united, and destroyed each other. From this circumstance Mr. Wilson concluded, that the tourmalin suffered the electric fluid to pass through it only in one direction, and that in this it bore some analogy to the loadstone; having, as it were, two electric poles, which are not easily destroyed or altered.* But he was not aware how imperfect a conductor oil, or grease of any kind is.

This induced him to try whether, like the loadstone, the tourmalin would lose its virtue after being made red hot; but, though he kept two of them in a strong fire for half an hour, he could not perceive the least alteration in them: but plunging one in water, while it was red hot entirely destroyed its virtue, and gave it the appearance of having been shivered in many parts without breaking. † He observes that when a tourmalin, which he had from Dr. Morton, was held between the eye and the light, and viewed in the direction through which the electric fluid is found to pass, it appears of a darker colour considerably than when it is viewed at right angles to the former direction. This appearance, he says, obtains in many other tourmalins, especially when they happen to be as conveniently shaped. ‡

NOTWITHSTANDING the attention given to this subject by Epinus and Mr. Wilson, the most important discovery relating to the electricity of the tourmalin was reserved for Mr. Canton; who, in a paper read at the Royal Society in the same month with that above-mentioned of Mr. Wilson, viz. December 1759, observes, that the tourmalin emits and absorbs the electric fluid

* Ib. p. 337, 338. † Ib. p. 338. ‡ Ib. Vol. 53. p. 448.

only

only by the increase or diminution of its heat. For if the tourmalin, he says, be placed on a plain piece of heated glass or metal, so that each side of it, by being perpendicular to the surface of the heating body, may be equally heated; it will, while heating, have the electricity of one of its sides positive, and that of the other negative. This will, likewise, be the case when it is taken out of boiling water and suffered to cool; but the side which was positive while it was heating will be negative while it is cooling, and the side which was negative will be positive.

IN this paper Mr. Canton refers to the Gentleman's Magazine for the month of September before, in which he had published the result of some experiments he had made on a tourmalin which he had procured from Holland. The propositions he there lays down are so few, and comprise the principal part of what is known upon this subject in so concise and elegant a manner, that I shall recite them all in this place.

1. WHEN the tourmalin is not electrical or attractive, heating it, without friction, will make it so; and the electricity of one side of it (distinguished by A) will be positive, and that of the other side (B) will be negative.

2. THE tourmalin not being electrical will become so by cooling; but with this difference, that the side A will be negative, and the side B positive.

3. IF the tourmalin, in a non-electrical state, be heated, and suffered to cool again, without either of its sides being touched; A will be positive, and B negative, the whole time of the increase and decrease of its heat.

4. EITHER side of the tourmalin will be positive by friction, and both may be made so at the same time.

THESE, says he, are the principal laws of the electricity of this

this wonderful stone; and he adds, if air be supposed to be endued with similar properties, i. e. of becoming electrical by the increase or diminution of its heat (as is probable, if its state before and after a thunder storm be attended to) thunder clouds both positive and negative, as well as thunder gusts, may easily be accounted for.

THESE capital discoveries were made before Mr. Canton had received of Dr. Heberden the tourmalins above-mentioned. When those came to his hands he was enabled to make several new and curious experiments, which I have leave to publish.

HE put one of the tourmalins, which was of the common colour into the flame from a blow-pipe and burnt it white; when he found that its electrical property was intirely destroyed. The electricity of another was only in part destroyed by the fire. Two other tourmalins he joined together, when they were made soft by fire, without destroying the electrical property. The virtue of another was improved by its being melted at one end; and he found (contrary to what Mr. Wilson had observed of another tourmalin, which he treated in the same manner) that one tourmalin retained its electrical property after it had been frequently made red hot, and, in that state, put into cold water.

BUT the most curious of his experiments were made upon a large irregular tourmalin, about half an inch in length, which he cut in three pieces; taking one part from the positive, and another from the negative end. Trying these pieces separately, he found the outer side of the piece which he cut from the end that was negative when cooling was likewise negative when cooling, and that the outer side of that piece which was cut from the end that was positive when cooling was likewise positive when cooling; the opposite sides of both pieces being, agree-

able to the general law of the electricity of the tourmalin, in a contrary state.

THE middle part of the same tourmalin was affected just as it had been when it was intire; the positive end remained positive, and the negative end continued negative. The same he had also observed of two other tourmalins, each about the size of this, which were also cut out of a large one.

ON January the 8th. 1762, Mr. Canton took the Doctor's large tourmalin (which Mr. Wilson has given a description of in the 51st. vol. of the Philosophical Transactions, p. 316) and having placed a small tin cup of boiling water on one end of his electrometer, which was supported by warm glass, while the pith balls were suspended at the other end; he dropped it into the water, and observed, that during the whole time of its being heated, and also while it was cooling in the water, the balls were not at all electrified.

THE result of this experiment was contrary to one of a similar nature made by Mr. Wilson on the same tourmalin; who says, as has been related above, that when the tourmalin was covered over with liquid grease, it still retained its electric virtue.

TILL the year 1760 it had been supposed, that, of all electric substances, the tourmalin alone, possessed the property of being excited by heating and cooling, but in the beginning of that year, Mr. Canton having had an opportunity of examining a variety of gems, by the favour of Mr. Nicolas Crisp a jeweller in Bow church yard, first found the *Brazil topaz* to have the electrical properties of the tourmalin. The largest he met with he put into the hands of Dr. Heberden, who returned it November the 27th. 1760, and sent with it the tourmalins above-mentioned.

IN September 1761, Mr. Wilson (who had been informed of
Mr.

Mr. Canton's discovery) met with several other gems, of different sizes and colours, that resembled the tourmalin with respect to electrical experiments. The most beautiful of them were something like the ruby, others were more pale, and one inclining to an orange colour. In point of hardness and lustre, they were nearly the same with the topaz.

From all his experiments upon these gems he thought it was abundantly evident, that the direction of the fluid did not depend upon the external figure of the gem, but upon some particular internal make and constitution thereof. And that there is some such natural disposition in all gems affording these appearances may be collected, he says, from another curious specimen of the tourmalin kind, which is green, and formed of long slender crystals with several sides; many of which are found sticking together, and are brought from South America.

These gems, numbers of which were furnished him by Mr. Emanuel Mendes Da Costa, he found not only, like the tourmalin, in regard to electric appearances; but that the direction of the electric fluid moving therein was always along the grain or shootings of the crystals, one end thereof being electrified *plus*, and the other end *minus*. And that the fluid is more disposed to pass in that direction than in another, he thought, might be farther collected from what has been observed on the grain of the loadstone by Dr. Knight; who found that though the magnetic poles of the natural loadstone might be varied in any direction, yet that the same loadstone admitted of being made much more magnetical along the grain than across it.

From these experiments and observations Mr. Wilson inferred by analogy, that the electric fluid, flowing through all these stones and gems, moves in that direction in which the grain happens to lie; and that the reason of this is, that the resistance

the fluid meets with in passing through the gem is less in that direction than in any other. *

In a subsequent paper of Mr. Wilson's, read at the Royal Society December the 23d. 1763, and March 1764, he recites several curious experiments on the effects of removing the tourmalin from one room to another, in which there was some difference of heat; the result of which exactly confirms Mr. Canton's discovery, that the side which is positive when heating is negative when cooling, and *vice versa*. Upon a very nice examination, and during some favourable circumstances, Mr. Wilson says he has observed the tourmalin to be feebly electrified, when the thermometer varied up and down only one degree. †

* Phil. Transf. Vol. 52. pt. 2. P. 443.

† Ib. p. 457.

SECTION X.

DISCOVERIES THAT HAVE BEEN MADE SINCE THOSE OF DR. FRANKLIN, WITH RESPECT TO THE SAMENESS OF LIGHTNING AND ELECTRICITY.

THE year 1752 makes an æra in electricity no less famous than the year 1746, in which the Leyden phial was discovered. In the year 1752, was verified the hypothesis of Dr. Franklin, of the identity of the matter of lightning and of the electric fluid; and his great project of trying the experiment, by real lightning actually brought down from the clouds, was carried into execution.

THE French philosophers were the first to distinguish themselves upon this memorable occasion, and the most active persons in the scene were Mr. Dalibard and Delor, both zealous partisans (as Mr. Nollet calls them) of Dr. Franklin. The former prepared his apparatus at Marly La Ville, situated five or six leagues from Paris, the other at his own house, on some of the highest ground in that capital. Mr. Dalibard's machine consisted of an iron rod forty feet long, the lower extremity of which was brought into a centry box, where the rain could not come; while, on the outside,

outside, it was fastened to three wooden posts by long silk strings defended from the rain. This machine happened to be the first that was favoured with a visit from this ethereal fire. The philosopher himself happened not to be at home at that time; but, in his absence, he entrusted the care of his apparatus to one Coiffier a joiner, a man who had served fourteen years among the dragoons, and on whose understanding and courage he could depend. This artisan had all proper instructions given him, both how to make observations, and also to guard himself from any harm there might be from them; besides being expressly ordered to get some of his neighbours to be present, and particularly to send for the curate of Marly, whenever there should be any appearance of the approach of a thunder storm. At length the long expected event arrived.

ON wednesday, the 10th. of May 1752, between two and three o'clock in the afternoon, Coiffier heard a pretty loud clap of thunder. Immediately he flies to the machine, takes a phial furnished with a brass wire, and presenting one end of the wire to the rod, he sees a small bright spark issue from it, and hears the snapping that it made. Taking a second spark stronger than the former, and attended with a louder report, he calls his neighbours, and sends for the curate. The curate runs with all his might, and the parishioners, seeing the precipitation of their spiritual guide, imagine that poor Coiffier had been killed with lightning. The alarm spreads through the village, and the hail which came on did not prevent the flock from following their shepherd. The honest ecclesiastic arriving at the machine, and seeing there was no danger, took the wire into his own hand, and immediately drew several strong sparks, which were most evidently of an electrical nature, and compleated the discovery for which the machine was erected. The thunder cloud was not more than a quarter of an hour

hour in passing over the zenith of the machine, and there was no thunder heard besides that single clap. As soon as the storm was over, and no more sparks could be drawn from the bar, the curate wrote a letter to Mr. Dalibard, containing an account of this remarkable experiment, and sent it immediately by the hands of Coiffier himself.

HE says, that he drew sparks from the bar of a blue colour, an inch and a half in length, and which smelt strong of sulphur. He repeated the experiment at least six times in the space of about four minutes, in the presence of many persons, each experiment taking up the time, as he, in the stile of a priest expresseth himself, of a *pater* and an *ave*. In the course of these experiments he received a stroke on his arm, a little above the elbow, but he could not tell whether it came from the brass wire inserted into the phial, or from the iron bar. He did not attend to the stroke at the time he received it, but, the pain continuing, he uncovered his arm when he went home, in the presence of Coiffier; and a mark was perceived round his arm, such as might have been made by a blow with the wire on his naked skin; and afterwards several persons, who knew nothing of what had happened, said that they perceived a smell of sulphur when he came near them.

COIFFIER told Mr. Dalibard, that for about a quarter of an hour before the curate arrived, he had, in the presence of five or six persons, taken much stronger sparks than those which the curate mentioned. *

EIGHT days after, Mr. Delor saw the same thing at his own house, although only a cloud passed over, without either thunder or lightning. †

* Dalibard's Franklin, Vol. 2. p. 109, &c.

† Nolle's Letters, Vol. 1. p. 9.

THE same experiments were afterwards repeated by Mr. Delor, at the request of the king of France, who, it is said, saw them with the greatest satisfaction, and expressed a just sense of the merit of Dr. Franklin. These applauses of the king excited in Mr. De Buffon, Dalibard, and Delor, a desire of verifying Dr. Franklin's hypothesis more completely, and of pursuing his speculations upon the subject.

MR. DELOR's apparatus in Paris consisted of a bar of iron ninety nine feet high, and answered rather better than that of Mr. Dalibard, which, as was observed before, was only forty feet high. But as the quantity of electricity which they could procure from the clouds, in these first experiments, was but small, they added to this apparatus what they called a *magazine* of electricity, consisting of many bars of iron insulated, and communicating with the pointed iron rod. This magazine contained more of the electric matter, and gave a more sensible spark, upon the approach of the finger, than the pointed bar.

A MAGAZINE of this kind Mr. Mazcas had in an upper room of his house, into which he brought the lightning, by means of a wooden pole projecting out of his window, at the extremity of which a glass tube, filled with rosin, received a pointed iron rod, twelve feet long. But all this while the electrics, which they made use of to support these iron rods, were too much exposed to the open air, and consequently were liable to be wet, which would infallibly defeat their experiments.

THE most accurate experiments, made with these imperfect instruments, were those of Mr. Monnier. He was convinced that the high situation in which the bar of iron had commonly been placed was not absolutely necessary for this purpose: for he observed a common speaking trumpet, suspended upon silk five or six feet from the ground, to exhibit very evident signs of electricity.

tricity. He also found that a man placed upon cakes of rosin, and holding in his hand a wooden pole, about eighteen feet long, about which an iron wire was twisted, was so well electrified when it thundered, that very lively sparks were drawn from him; and that another man, standing upon non-electrics, in the middle of a garden, and only holding up one of his hands in the air, attracted, with the other hand, shavings of wood which were held to him.

HE says, that he observed a continual diminution of the electricity when the rain came on, though the thunder was still very loud, and though the cake of rosin which supported his conductor was not wet. But he afterwards found, that this was not universally true.

HE once observed, that when the conducting wire was surrounded with drops of rain, only some of them were electrified; as was evident from the conic figure they had, while the rest remained round as before. It was also perceived, that the electrified and non-electrified drops generally succeeded each other alternately; which made Mr. Monnier call to mind a singular phenomenon, which happened some years before to five peasants who were passing through a corn field, near Frankfort upon the Oder, during a thunder storm; when the lightning killed the first, the third, and the fifth of them, without injuring the second, or the fourth.*

It was not owing to any want of attention to this subject that the English philosophers were not the first to verify the theory of Dr. Franklin. They happened to have few opportunities of trying experiments. In the few they had, they were disappointed by the rain wetting their apparatus, which was not better constructed than that of the French.

* Phil. Trans. Vol. 47. p. 551.

At length success crowned the assiduity and happier contrivance of Mr. CANTON, who, to the lower end of his conducting wire, had had the precaution to fasten a tin cover, to keep the rain from the glass tube which supported it. By this means, on the 20th. of July 1752, he got sparks at the distance of half an inch, but the whole appearance ceased in the space of two minutes. *

MR. WILSON, who took great pains in these pursuits, as he did in every thing else relating to electricity, perceived several electrical snaps, on the 12th. of August following, from no other apparatus than an iron curtain rod; one end of which he put into the neck of a glass phial, which he held in his hand, while the other was made to point into the air.

DR. BEVIS also, at this time, viz. on the 12th. of August, observed nearly the same appearances that Mr. CANTON had done before. †

MR. CANTON afterwards, resuming his observations on lightning, with the assiduity and accuracy with which he observes every thing, found, by a great number of experiments, that some clouds were in a positive, and some in a negative state of electricity. And that, by this means, the electricity of his conductor would sometimes change from one state to the other five or six times in less than half an hour. ‡

THIS observation of Mr. CANTON, on the different electricity of the clouds was made, and the account of it published in England, before it was known that Dr. FRANKLIN had made the same discovery in America.

WHEN the air was dry, he observed, that the apparatus would continue electrified for ten minutes, or a quarter of an hour after the clouds had passed the zenith, and sometimes till

* Phil. Transf. Vol. 47. p. 568. † Ib. p. 569. ‡ Ib. Vol. 48. pt. 1. p. 356.

they

they were more than half way towards the horizon; that rain, especially when the drops were large, generally brought down the electric fire; and hail in summer, he believed, never failed. The last observation he had made before the time of his writing this paper, his apparatus had been electrified by a fall of thawing snow. This was on the 12th. of November 1753, which, he says, was the 26th. day, and the sixty first time, it had been electrified since it was first set up viz. about the middle of the preceding May.

ONLY two thunder storms had happened at London during that whole summer, and his apparatus was so strongly electrified by one of them, that the ringing of the bells (which he suspended to his apparatus, to signify when the electrification was begun, and which were frequently rung so loud as to be heard in every room in the house) was stifled by the almost constant stream of dense electric fire between each bell and the brass ball, which would not suffer it to strike.

UPON a farther occasion, he observes, that in the succeeding months of January, February, and March, his apparatus was electrified no less than twenty five times, both positively and negatively, by snow as well as by hail and rain; and almost to as great a degree when Fahrenheit's thermometer was between twenty eight and thirty four, as he had ever known it in summer, except in a thunder storm.*

MR. CANTON concludes his paper with proposing the two following queries. 1. May not air suddenly rarefied give electric fire to, and air suddenly condensed receive electric fire from, clouds and vapours passing through it? 2. Is not the aurora borealis the flashing of electric fire from positive towards negative

* Phil. Transf. Vol. 48. pt. 2. p. 785.

clouds at a great distance, through the upper part of the atmosphere, where the resistance is least? *

MR. CANTON not only observed the different states of positive and negative electricity in the clouds, but also noted the proportion that the one bore to the other for a considerable time. In the first period he had observed the clouds had been positively electrical 83 times, and negative 101. In this period he had punctually set down how often the powers had shifted, and the whole time that the apparatus continued to be electrified, but he had intirely neglected to note the time that each power lasted. But this last circumstance he afterwards carefully attended to, for about two months, viz. from the 28th. of June to the 23d. of August 1754; and found the apparatus to be electrified positively thirty one times, which taken together lasted three hours thirty five minutes; and negatively forty five times, the whole duration of which was ten hours thirty nine minutes. He also observed that the positive power was generally the strongest. This account he wrote the 31st. of August 1754.

THESE observations which Mr. Canton gives me leave to make public are extremely curious, and must have required great attention; but they are hardly sufficient to authorize any general conclusion.

ONE of the effects of lightning and electricity is the melting of metals. This was first thought to be a cold fusion; but that opinion is refuted, in a very sensible manner, by Dr. Knight, in a paper read at the Royal Society November 22d. 1759. He observes, that the instances most generally given of cold fusion are two; viz. that of a sword being melted in its scabbard, and that

* Phil. Transf. Vol. 48. pt. 1. p. 358.

of money being melted in a bag, both the scabbard and the bag remaining unhurt.

A GREAT number of authors, he says, have mentioned both the facts, but without giving either their own testimony, or that of any one else for the truth of them, or describing any of the other concomitant circumstances. And it seemed to him very possible, that lightning might produce effects similar to those above-mentioned, without our being obliged to have recourse to a cold fusion to account for them.

IF, says he, the edge or external surface of a sword had been melted, whilst the main part of the blade remained entire, it would have afforded sufficient ground to assert, in general terms, that the sword was melted, and yet the scabbard might have remained unhurt; because either the edge or surface of a sword might be instantly melted by lightning and cooled so suddenly, as to make no impression of burning upon the scabbard. Metals, as well as other bodies, he observes, will both heat and cool sooner in proportion as they are thin and slender; that very small wire will instantly become red hot, and even melt, and run into a round globule in the flame of a common candle; though it is no sooner removed out of the flame, but it is instantly cold. He therefore concludes, that the edge of a sword, or even its surface, might be instantly melted by lightning; and being in contact with, or rather still united to the rest of the blade, which might be cold, it would part with its heat too suddenly to produce any appearance of burning.

HE was confirmed in this reasoning, by examining some fragments and particles of wire melted by lightning, which were sent him by Mr. Mountaine. Amongst them appeared globules of various sizes, which had undergone very different degrees of fusion. The largest of them had not been fluid enough to put on a spherical

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rical figure, but they approached nearer to it, in proportion as they were smaller; so that, in the smallest granulae, the fusion was most perfect, the globules being very round and smooth. Their sizes continued diminishing till they became invisible to the naked eye, and some of them, when viewed with a microscope, required a third or fourth magnifier to see them distinctly.

SOME of the bits of wire were rough and scaly, like burnt iron, and were swelled in those places where they were beginning to melt. Others continued straight, and of an equable thickness; but their outward surface seemed to have undergone a perfect fusion, so that there were two or more pieces adhering together, as if joined by a thin folder.

IN the Philosophical Transactions, Dr. Knight says, there are two or three relations which seem at first to favour a cold fusion, but when duly considered prove nothing conclusive.*

BUT that there is really no such thing as cold fusion, either by electricity or lightning, was most clearly demonstrated by Mr. Kinnerley, in a letter to Dr. Franklin, dated Philadelphia March the 12th. 1761.

HE suspended a piece of small brass wire, about twenty four inches long, with a pound weight at the lower end; and, by sending through it the charge of a case of bottles, containing above thirty feet of coated glass, he discovered what he calls a new method of wire drawing. The wire was red hot, the whole length well annealed, and above an inch longer than before. A second charge melted it so that it parted near the middle, and measured, when the ends were put together, four inches longer than at first.

THIS experiment, he says, was proposed to him by Dr.

* Phil Trans. Vol. 51. pt. 1. p. 294, &c.

Franklin,

Franklin, in order to find whether the electricity, in passing through the wire, would so relax the cohesion of its constituent particles, as that the weight might produce a separation; but neither of them had the least suspicion that any heat would be produced.

THAT he might have no doubt of the wire being *hot*, as well as *red*, he repeated the experiment on another piece of the same wire, encompassed with a goose quill, filled with loose grains of gun-powder; which took fire, as readily as if it had been touched with a red hot poker. Also tinder, tied to another piece of the wire, kindled by it; but when he tried a wire about twice as big, he could produce no such effects.

HENCE, says he, it appears, that the electric fire, though it has no sensible heat when in a state of rest, will, by its violent motion, and the resistance it meets with, produce heat in other bodies, when passing through them, provided they be small enough. A great quantity will pass through a large wire, without producing any sensible heat; when the same quantity, passing through a very small one, being there confined to a narrower passage, the particles crowding closer together, and meeting with a greater resistance, will make it red hot, and even melt it.

HENCE, he concludes, that lightning does not melt metal by a cold fusion, as Dr. Franklin and himself had formerly supposed; but that, when it passed through the blade of a sword, if the quantity was not very great, it might heat the point so as to melt it, while the broadest and the thickest part might not be sensibly warmer than before.

WHEN trees and houses are set on fire by the dreadful quantity, which a cloud, or sometimes the earth discharges, must not the heat, says he, by which the wood is first kindled, be generated
by

by the lightning's violent motion through the resisting combustible matter?

If lightning, by its rapid motion, produced heat in itself as well as in other bodies (which Mr. Kinnerley imagined was evident from some experiments made with his electrical thermometer, mentioned before) he thought that its sometimes singeing the hair of animals killed by it might easily be accounted for; and that the reason of its not always doing so might be, that the quantity, though sufficient to kill a large animal, might not be great enough, or not have met with resistance enough, to become by its motion burning hot.

WE find, says he, that dwelling houses, struck with lightning, are seldom set on fire by it; but that when it passes through barns, with hay or straw in them, or store houses containing large quantities of hemp, or such like matter, they seldom, if ever, escape a conflagration. This, he thought, might be owing to such combustibles being apt to kindle with a less degree of heat than was necessary to kindle wood.*

ALL that was done by the French and English electricians, with respect to lightning and electricity, fell far short of what was done by SIGNIOR BECCARIA at Turin. His attention to the various states of the atmosphere, his assiduity in making experiments, his apparatus for making them, the extent of his views in making them, the minute exactness with which he has recorded them, and his judgment in applying them to a general theory, far exceeded every thing that had been done by philosophers before him, or that has been done by any person since. And though I shall give considerable scope to my account of his experiments and observations, I shall be able to give

* Phil. Transf. Vol. 53. p. 92, &c.

my reader but a faint idea of the extent, variety, and value of his labours in this great field.

HE made use both of kites, and pointed rods, and of a great variety of both, at the same time, and in different places. Some of the strings of his kites had wires in them, and others had none. Some of them flew to a prodigious height, and others but low; and he had a great number of assistants to note the nature, time, and degree of appearances, according as his views required.

To keep his kites constantly insulated, and at the same time to give them more or less string, and for many other purposes, he had the string rolled upon a reel, which was supported by pillars of glass; and his conductor had a communication with the axis of the reel. *

To distinguish the positive and negative state of the clouds, when the electricity was vigorous, with more certainty, and with more safety than it could be done by presenting an excited stick of glass, or sealing wax to threads diverging from his conductor; he inclosed a pointed wire and a flat piece of lead opposite to it within a cylindrical glass vessel, wrapped in pasteboard, so that the inside could have no communication with the external light. Into this cover, and opposite to the point of the wire, he inserted a very long tube of pasteboard; through which he could look from a considerable distance, and see the form of the electric light at the end of the wire; which is the surest indication of its quality. †

FROM Signior Beccaria's extremely exact and circumstantial account of the external appearances of thunder clouds, which he prefixes to his observations on their probable causes, I shall

* Lettere dell' elettricismo, p. 112.

† *ib.* p. 107.

draw a general outline of the most remarkable particulars, in the usual progress of a thunder storm.

THE first appearance of a thunder storm (which generally happens when there is little or no wind) is one dense cloud, or more, increasing very fast in size, and rising into the higher regions of the air. The lower surface is black, and nearly level; but the upper finely arched, and well defined. Many of these clouds often seem piled one upon another, all arched in the same manner; but they keep continually uniting, swelling, and extending their arches.

AT the time of the rising of this cloud, the atmosphere is generally full of a great number of separate clouds, motionless, and of odd and whimsical shapes. All these, upon the appearance of the thunder cloud, draw towards it, and become more uniform in their shapes as they approach; till, coming very near the thunder cloud, their limbs mutually stretch towards one another; they immediately coalesce, and together make one uniform mass. These he calls *adscititious* clouds, from their coming in, to enlarge the size of the thunder cloud. But, sometimes, the thunder cloud will swell, and increase very fast without the conjunction of any adscititious clouds, the vapours in the atmosphere forming themselves into clouds wherever it passes. Some of the adscititious clouds appear like white fringes, at the skirts of the thunder cloud, or under the body of it, but they keep continually growing darker and darker, as they approach to unite with it.

WHEN the thunder cloud is grown to a great size, its lower surface is often ragged, particular parts being detached towards the earth, but still connected with the rest. Sometimes the lower surface swells into various large protuberances bending uniformly towards the earth. And sometimes one whole side of the cloud will have an inclination to the earth, and the extremity

mity of it will nearly touch the earth. * When the eye is under the thunder cloud, after it is grown large, and well formed, it is seen to sink lower, and to darken prodigiously; at the same time that a number of small adscitious clouds (the origin of which can never be perceived) are seen in a rapid motion, driving about in very uncertain directions under it. While these clouds are agitated with the most rapid motions, the rain generally falls in the greatest plenty, and if the agitation be exceeding great, it commonly hails. †

WHILE the thunder cloud is swelling, and extending its branches over a large tract of country, the lightning is seen to dart from one part of it to another, and often to illuminate its whole mass. When the cloud has acquired a sufficient extent, the lightning strikes between the cloud and the earth, in two opposite places, the path of the lightning lying through the whole body of the cloud and its branches. The longer this lightning continues, the rarer does the cloud grow, and the less dark is its appearance; till, at length, it breaks in different places, and shows a clear sky. When the thunder cloud is thus dispersed, those parts which occupy the upper regions of the atmosphere are equally spread, and very thin; and those that are underneath are black, but thin too; and they vanish gradually, without being driven away by any wind. ‡

HAVING seen what this philosopher observed abroad, and in the air, let us see what he took notice of at his apparatus within doors. This never failed to be electrified upon every approach of a thunder cloud, or any of its branches; and the stream of fire from it was generally perpetual, while it was directly over the apparatus. ||

* Lettere dell' elettricismo, p. 151. † Ib. p. 155. ‡ Ib. p. 146, 176. || Ib. p. 167.

THAT thunder clouds were sometimes in a positive as well as negative state of electricity, Signior Beccaria had discovered, before he heard of its having been observed by Dr. Franklin, or any other person.* The same cloud, in passing over his observatory, electrified his apparatus, sometimes positively, and sometimes negatively. † The electricity continued longer of the same kind, in proportion as the thunder cloud was simple, and uniform in its direction; but when the lightning changed its place, there commonly happened a change in the electricity of his apparatus. It would change suddenly after a very violent flash of lightning, but the change would be gradual when the lightning was moderate, and the progress of the thunder cloud slow.

It was an immediate inference from his observations of the lightning abroad, and his apparatus within, that the quantity of electric matter, in an usual storm of thunder, is almost inconceivably great; considering how many pointed bodies, as trees, spires, &c. are perpetually drawing it off, and what a prodigious quantity is repeatedly discharged to, or from the earth. ‡

AFTER this summary view of appearances, I shall, in the same succinct manner, represent the manner in which this excellent philosopher accounts for them, and some other principal and well known phenomena of thunder storms.

CONSIDERING the vast quantity of electric fire that appears in the most simple thunder storms, he thinks it impossible that any cloud, or number of clouds should ever contain it all, so as either to discharge or receive it. Besides, during the progress and increase of the storm, though the lightning frequently struck to

* *Lettere dell' elettricismo*, p. 133. † *Ib.* p. 172. ‡ *Ib.* p. 180.

the earth, the same clouds were the next moment ready to make a still greater discharge, and his apparatus continued to be as much affected as ever. The clouds must, consequently, have received at one place, the moment that a discharge was made from them in another. * In many cases, the electricity of his apparatus, and consequently of the clouds, would instantly change from one kind to another several times; an effect which cannot be accounted for by any simple discharge, or recruit. Both must have taken place in a very quick succession. †

THE extent of the clouds doth not lessen this difficulty: for, be it ever so great, still the quantity ought to be lessened by every discharge: and, besides, the points, by which the silent discharges are made, are in proportion to the extent of the clouds. ‡ Nor is the difficulty lessened by supposing that fresh clouds bring recruits; for besides that the clouds are not ripe for the principal storm, till all the clouds, to a great distance, have actually coalesced, and formed one uniform mass, those recruits bear no sort of proportion to the discharge, and whatever it was, it would soon be exhausted.

THE fact, therefore, must be, that the electric matter is continually darting from the clouds in one place, at the same time that it is discharged from the earth in another. And it is a necessary consequence from the whole, that the clouds serve as conductors to convey the electric fluid from those places of the earth which are overloaded with it, to those which are exhausted of it. ||

To ascertain this fact in the most complete manner, he proposes that two observatories be fixed, about two leagues asunder, in the usual path of the thunder clouds; and that observations be

* *Lectere dell' elettricismo*, p. 183, 188. † *Ib.* p. 220. ‡ *Ib.* 185. || *Ib.* p. 193. made,

made, whether the apparatus be not often positive at one place, when it is negative at the other. *

THAT great quantities of electric matter do sometimes rush out of particular parts of the earth, and rise through the air, into the higher regions of the atmosphere, he thinks is evident from the great quantities of sand, ashes, and other light substances, which have often been carried up into the air, and scattered uniformly over a large tract of country. † No other known efficient cause of this phenomenon can be assigned, except the wind; and it has been observed when there was no wind stirring; and the light bodies have even been carried against the wind. ‡ He supposes, therefore, that these light bodies are raised by a large quantity of electric matter, issuing out of the earth, where it was overcharged with it, and (by that property of it which he had demonstrated) attracting, and carrying with it every substance that could serve as a conductor in its passage. All these bodies, being possessed of an equal quantity of the electric fluid, will be dispersed equally in the air, and consequently over that part of the earth where the fluid was wanting, and whither they serve to convey it. || Had these bodies been raised by the wind, they would have been dispersed at random, and in heaps.

THIS comparatively rare phenomenon (but of which he had been more than once a spectator) he thinks exhibits both a perfect image, and demonstration, of the manner in which the vapours of the atmosphere are raised to form thunder clouds. The same electric matter, wherever it issues, attracts to it, and carries up into the higher regions of the air, the watery particles that are dispersed in the atmosphere. The electric matter ascends to the higher regions of the atmosphere, being solicited by the less re-

* *Lettere dell' elettricismo*, p. 194. † *Ib.* p. 199. ‡ *Ib.* p. 225. || *Ib.* p. 207.
siflance

stance it finds there than in the common mass of the earth; which, at those times, is generally very dry, and consequently highly electric. The uniformity with which thunder clouds spread themselves, and swell into arches, must be owing to their being affected by some cause which, like the electric matter, diffuses itself uniformly wherever it acts, and to the resistance they meet with in ascending through the air. * As a proof of this, steam, rising from an electrified colipile, diffuses itself with the same uniformity, and in similar arches, extending itself towards any conducting substance. †

THE same cause which first raised a cloud, from vapours dispersed in the atmosphere, draws to it those that are already formed, and continues to form new ones; till the whole collected mass extends so far as to reach a part of the earth where there is a deficiency of the electric fluid. ‡ Thither too, will those clouds, replete with electricity, be strongly attracted, and there will the electric matter discharge itself upon the earth. A channel of communication being, in this manner, found, a fresh supply of electric matter will be raised from the overloaded part, and will continue to be conveyed by the medium of the clouds, till the equilibrium of the fluid between the two places of the earth be restored. When the clouds are attracted in their passage by those parts of the earth where there is a deficiency of the fluid, those detached fragments are formed, and also those uniform depending protuberances, which will be shown to be, in some cases, the cause of water-spouts, and hurricanes. ||

THAT the electric matter, which forms and animates the thunder clouds, issues from places far below the surface of the earth, and that it buries itself there, is probable from the deep

* Ib. p. 205. † Ib. p. 206. ‡ Ib. p. 212. || Ib. p. 214.

holes that have, in many places, been made by lightning. * Flashes of lightning have, also, been seen to arise from subterraneous cavities, and from wells. † Violent inundations have accompanied thunder storms, not occasioned by rain, but by water bursting from the bowels of the earth, from which it must have been dislodged by some internal concussion. Deep wells have been known to fill faster in thunder storms, ‡ and others have constantly grown turbid at the approach of thunder. §

THIS very rise, as well as the whole progress of thunder clouds, has sometimes been, in a manner, visible. Exhalations have been frequently seen to rise from particular caverns, attended with a rumbling noise, and to ascend into the higher regions of the air, with all the phenomena of thunder storms described above, according to the description of persons who lived long before the connection between electricity and lightning was suspected. §

THE greatest difficulty attending this theory of the origin of thunder storms relates to the collection, and insulation of electric matter within the body of the earth. With respect to the former, he has nothing particular to say. Some operations in nature are certainly attended with a loss of the equilibrium in the electric fluid, but no person has yet assigned a more probable cause of the redundancy of the electric matter which, in fact, often abounds in the clouds, than what we may suppose possible to take place in the bowels of the earth. And supposing the loss of the equilibrium possible, the same cause that produced the effect would prevent the restoring of it; so that not being

* *Lettere dell' elettricismo*, p. 227. † *Ib.* p. 228. ‡ *Ib.* p. 233. § *Ib.* p. 362.
§ *Ib.* p. 231.

able to force a way, at least one sufficiently ready, through the body of the earth, it would issue at the most convenient vent into the higher regions of the air, as the better passage. His electrical apparatus, though communicating with the earth, has frequently, in violent thunder storms, given evident sparks to his finger.*

IN the enumeration of the effects of thunder storms, he observes that a wind always blows from the place from which the thunder cloud proceeds; that this is agreeable to the observations of all mariners, and that the wind is more or less violent in proportion to the suddenness of the appearance of the thunder cloud, the rapidity of its expansion, and the velocity with which the adscititious clouds join it. The sudden condensation of such a prodigious quantity of vapours must displace the air, and repel it on all sides. †

HE, in some measure, imitated even this effect of thunder, at least produced a circulation of all the air in his room, by the continued electrification of his chain. ‡

AMONG other effects of lightning, he mentions the case of a man rendered exceeding stiff, presently after he was struck dead in a storm of thunder. But the most remarkable circumstance, in this case, was the lightning (choosing the best conductor) having struck one particular vein, near his neck, and followed it through its minutest ramifications; so that the figure of it appeared through the skin, finer than any pencil could have drawn it. ||

HE cautions persons not to depend upon the neighbourhood of a higher, or, in all cases, a better conductor than their own body; since, according to his repeated observations, the light-

* Ib. p. 236. † Ib. p. 339, 340. ‡ Ib. p. 343. || Ib. p. 242.

ning by no means descends in one undivided track; but bodies of various kinds conduct their share of it, at the same time, in proportion to their quantity and conducting power. *

A GREAT number of observations, relating to the descent of lightning, confirm his theory of the manner of its ascent: for, in many cases, it throws before it the parts of conducting bodies, and distributes them along the resisting medium through which it must force its passage. †

UPON this principle it is, that the longest flashes of lightning seem to be made, by its forcing into its way part of the vapours in the air. ‡ One of the principal reasons why those flashes make so long a rumbling, is their being occasioned by the vast length of a vacuum, made by the passage of the electric matter. For though the air collapses the moment after it has passed, and the vibration (on which the sound depends) commences at the same moment, through the whole length of the track; yet, if the flash was directed towards the person who hears the report, the vibrations excited at the nearer end of the track will reach his ear much sooner than those excited at the more remote end; and the sound will, without any repercussion or echo, continue till all the vibrations have successively reached him. ||

I MUST introduce in this place a very curious experiment and observation of Mr. Lullin, concerning the production of electricity in the clouds. He made a long insulated pole project from the side of one of the Alps; and, on the 29th. of June 1766, observed, that when small clouds of vapour, raised by the heat of the sun, rose near the foot of the mountain, and ascended along the side of it; if they touched the extremity of the pole only, it was electrified; but if the whole pole, and con-

* *Lettere dell' elettricismo*, p. 246. † *Ib.* p. 247. ‡ *Ib.* p. 851. § *Ib.* p. 252.
sequently

frequently part of the hill on which it stood, was likewise involved, it was not electrified. From this he concludes, that the electricity of the clouds is produced by their passing through the air while the sun shines upon them. But to which of these two circumstances, namely the motion through the air, or the action of the sun's rays this was owing he could not determine, though he made several experiments for that purpose.*

ONE of the most remarkable effects of lightning is that it gives polarity to the magnetic needle, and to all bodies that have any thing of iron in them, as bricks, &c; and by observing which way the poles of these bodies lye, it may be known, with the utmost certainty, in what direction the stroke passed. † In one case he actually ascertained the direction of the lightning in this manner. ‡

SINCE a sudden stroke of lightning gives polarity to magnets, he conjectures that a regular and constant circulation of the whole mass of the fluid, from North to South, may be the original cause of magnetism in general. || This is a truly great thought; and, if just, will introduce greater simplicity into our conceptions of the laws of nature.

THAT this ethereal current is insensible to us, is no proof of its non-existence, since we ourselves are involved in it. He had seen birds fly so near a thunder cloud, as he was sure they would not have done, if they had been affected by its atmosphere. §

THIS current he would not suppose to arise from one source, but from several, in the northern hemisphere of the earth. The aberration of the common center of all these currents from

* *Dissertatio physica*, p. 42. † *Lettere dell' elettricismo*, p. 262. ‡ *Ib.* p. 263.
 || *Ib.* p. 263. § *Ib.*

the North point may be the cause of the variation of the needle, the period of this declination of the center of the currents may be the period of the variation, and the obliquity with which the currents strike into the earth may be the cause of the dipping of the needle, and also why bars of iron more easily receive the magnetic virtue in one particular direction. *

He thinks that the *Aurora Borealis* may be this electric matter performing its circulation, in such a state of the atmosphere as renders it visible, or approaching nearer to the earth than usual. Accordingly very vivid appearances of this kind have been observed to occasion a fluctuation in the magnetic needle. †

STONES and bricks struck by lightning are often vitrified. He supposes that some stones in the earth, having been struck in this manner, first gave occasion to the vulgar opinion of the thunder bolt ‡

SIGNIOR BECCARIA was very sensible that heat contributes much to the phenomena of thunder, lightning, and rain; but he could not find, by any experiment, that it tended to promote electricity. He, therefore, rather thought that heat operated, in this case, by exhaling the moisture of the air, and thereby cutting off the communication of the electric fluid between one place and another, particularly between the earth and the higher regions of the air, whereby its effects were more visible. ||

HAVING entertained my reader with the observations of this great Italian genius, I must once more conduct him to France, where he will see several experiments well worth his notice. In this country we have seen that Dr. Franklin's theory of the identity of electricity and the matter of lightning was first verified,

* Lettere dell' elettricismo, p. 269. † Ib. p. 272. ‡ Ib. p. 263. || Ib. p. 359. and

and we shall now see it verified in the grandest and most conspicuous manner.

THE greatest quantity of electricity that was ever brought from the clouds, by any apparatus prepared for that purpose, was by Mr. De Romas, assessor to the presideal of Nerac. This gentleman was the first who made use of a wire interwoven in the hempen cord of an electrical kite, which he made seven feet and a half high, and three feet wide, so as to have eighteen square feet of surface. This cord was found to conduct the electricity of the clouds more powerfully than an hempen cord would do, even though it was wetted; and, being terminated by a cord of dry silk it enabled the observer (by a proper management of his apparatus) to make whatever experiments he thought proper, without danger to himself.

By the help of this kite, on the 7th. of June 1753, about one in the afternoon, when it was raised 550 feet from the ground, and had taken 780 feet of string, making an angle of near forty five degrees with the horizon; he drew sparks from his conductor three inches long and a quarter of an inch thick, the snapping of which was heard about 200 paces. Whilst he was taking these sparks, he felt, as it were, a cobweb on his face, though he was above three feet from the string of the kite; after which he did not think it safe to stand so near, and called aloud to all the company to retire, as he did himself about two feet.

THINKING himself now secure enough, and not being incommoded by any body very near him, he took notice of what passed among the clouds which were immediately over the kite; but could perceive no lightning, either there, or any where else, nor scarce the least noise of thunder, and there was no rain at all. The wind was West, and pretty strong, which raised the kite 100 feet higher, at least, than in the other experiments.

AFTERWARDS,

AFTERWARDS, casting his eyes on the tin tube, which was fastened to the string of the kite, and about three feet from the ground, he saw three straws, one of which was about one foot long, a second four or five inches, and the third three or four inches, all standing erect, and performing a circular dance, like puppets, under the tin tube, without touching one another.

THIS little spectacle, which much delighted several of the company, lasted about a quarter of an hour; after which, some drops of rain falling, he again perceived the sensation of the cobweb on his face, and at the same time heard a continual rustling noise, like that of a small forge bellows. This was a further warning of the increase of electricity; and from the first instant that Mr. De Romas perceived the dancing straws, he thought it not advisable to take any more sparks even with all his precautions; and he again intreated the company to spread themselves to a still greater distance.

IMMEDIATELY after this came on the last act of the entertainment, which Mr. De Romas acknowledged made him tremble. The longest straw was attracted by the tin tube, upon which followed three explosions, the noise of which greatly resembled that of thunder. Some of the company compared it to the explosion of rockets, and others to the violent crashing of large earthen jars against a pavement. It is certain that it was heard into the heart of the city, notwithstanding the various noises there.

THE fire that was seen at the instant of the explosion had the shape of a spindle eight inches long and five lines in diameter. But the most astonishing and diverting circumstance was produced by the straw, which had occasioned the explosion, following the string of the kite. Some of the company saw it at forty five or fifty fathoms distance, attracted and repelled alternately, with this remarkable circumstance, that every time it was attracted

tracted by the string, flashes of fire were seen, and cracks were heard, though not so loud as at the time of the former explosion.

IT is remarkable, that, from the time of the explosion to the end of the experiments, no lightning at all was seen, nor scarce any thunder heard. A smell of sulphur was perceived, much like that of the luminous electric effluvia issuing out of the end of an electrified bar of metal. Round the string appeared a luminous cylinder of light, three or four inches in diameter; and this being in the day time Mr. De Romas did not question but that, if it had been in the night, that electric atmosphere would have appeared to be four or five feet in diameter. Lastly, after the experiments were over, a hole was discovered in the ground, perpendicularly under the tin tube, an inch deep, and half an inch wide, which was probably made by the large flashes that accompanied the explosions.

AN end was put to these remarkable experiments by the falling of the kite, the wind being shifted into the East, and rain mixed with hail coming on in great plenty. Whilst the kite was falling, the string came foul of a penthouse; and it was no sooner disengaged, than the person who held it felt such a stroke in his hands, and such a commotion through his whole body, as obliged him instantly to let it go; and the string, falling on the feet of some other persons, gave them a shock also, though much more tolerable.*

THE quantity of electric matter brought by this kite from the clouds at another time is really astonishing. On the 26th. of August 1756, the streams of fire issuing from it were observed to be an inch thick, and ten feet long. This amazing flash of

*-Gent. Magaz. for August 1756, p. 37³.

lightning,

lightning, the effect of which on buildings or animal bodies, would perhaps have been equally destructive with any that are mentioned in history, was safely conducted by the cord of the kite to a non-electric body placed near it, and the report was equal to that of a pistol.

MR. ROMAS had the curiosity to place a pigeon in a cage of glass, in a little edifice, which he had purposely placed, so as that it should be demolished by the lightning brought down by his kite. The edifice was, accordingly, shattered to pieces, but the cage and the pigeon were not struck.*

THE Abbé Nollet, who gives this account, adds, that if a stroke of this kind had gone through the body of Mr. De Romas, the unfortunate professor Richman had not probably been the only martyr to electricity, and advises, that great caution be used in conducting such dangerous experiments. †

WHEN we consider how many severe shocks the most cautious and judicious electricians often receive through inadvertence, we shall not be surpris'd, that when philosophers first began to collect and make experiments upon real lightning, it should sometimes have proved a little untractable in their hands, and that they were obliged to give one another frequent cautions how to proceed with it.

THE Abbé Nollet, as early as the year 1752, advises that these experiments be made with circumspection; as he had been informed, by letters from Florence and Bologna, that those who had made them there had had their curiosity more than satisfied by the violent shocks they had sustained in drawing sparks from an iron bar electrified by thunder. One of his correspondents informed him, that once, as he was endeavouring to fasten a

* Nollet's Letters, Vol. 2. p. 239.

† Phil. Transf. Vol. 52. pt. 1. p. 342.

small chain, with a copper ball at one of its extremities, to a great chain, which communicated with the bar at the top of the building (in order to draw off the electric sparks by means of the oscillations of this ball) there came a flash of lightning, which he did not see, but which affected the chain with a noise like wild fire. At that instant, the electricity communicated itself to the chain of the copper ball, and gave the observer so violent a commotion, that the ball fell out of his hands, and he was struck backwards four or five paces. He had never been so much shocked by the experiment of Leyden. *

MR. ROMAS received a severe stroke when he first raised his kite: and Mr. Dalibard says, that Mr. Monnier, a physician of St. Germain en Laye, member of the academy of sciences at Paris; and Mr. Bertier of the oratory at Montmorency, a correspondent of the academy, were both struck down by strokes of lightning, as they were taking sparks from their apparatus. †

BUT the greatest sufferer by experiments with lightning, since mankind have introduced so dangerous a subject of their inquiries, was professor Richman of Petersburg before mentioned. He was struck dead, on the 6th. of August 1753, by a flash of lightning drawn by his apparatus into his own room, as he was attending to an experiment he was making with it. There were two accounts of this fatal accident communicated to the Royal Society, one by Dr. Watson who had it from the best authority; ‡ and the other translated from the High Dutch. § From both these the following is extracted.

THE professor had provided himself with an instrument which he called an electrical gnomon, the use of which was to measure the strength of electricity. It consisted of a rod of metal termi-

* Phil. Transf. Vol. 48. pt. 1. p. 205.

† Dalibard's Franklin, Vol. 2. p. 129.

‡ Phil. Transf. Vol. 48. pt. 2. p. 765.

§ Ib. Vol. 49. pt. 1. p. 61.

nating in a small glass vessel, into which (for what reason I do not know) he put some brass filings. At the top of this rod, a thread was fastened, which hung down by the side of the rod when it was not electrified; but when it was, it avoided the rod, and stood at a distance from it, making an angle at the place where it was fastened. To measure this angle, he had the arch of a quadrant fastened to the bottom of the iron rod.

HE was observing the effect of the electricity of the clouds, at the approach of a thunder storm, upon this gnomon; and, of course, standing with his head inclined towards it, accompanied by Mr. Solokow (an engraver, whom he frequently took with him, to be a joint observer of his electrical experiments, in order to represent them the better in cuts) when this gentleman, who was standing close to his elbow, observed a globe of blue fire, as he called it, as big as his fist, jump from the rod of the gnomon towards the head of the professor, which was, at that instant, at about a foot distance from the rod. This flash killed Mr. Richman, but Mr. Solokow could give no account of the particular manner in which he was immediately affected by it: for, at the same time that the professor was struck, there arose a sort of steam, or vapour, which intirely benumbed him, and made him sink down upon the ground; so that he could not remember even to have heard the clap of thunder, which was very loud.

THE globe of fire was attended with a report as loud as that of a pistol: a wire, which brought the electricity to his metal rod, was broken to pieces, and its fragments thrown upon Mr. Solokow's cloaths. Half of the glass vessel in which the rod of the gnomon stood was broken off, and the filings of metal that were in it were thrown about the room.

UPON examining the effects of the lightning in the professor's chamber, they found the door case half split through, the door
torn

torn off, and thrown into the room. * They opened a vein of the breathless body twice, but no blood followed, and endeavoured to recover sensation by violent chafing, but in vain. Upon turning the corps with the face downwards, during the rubbing, an inconsiderable quantity of blood ran out of the mouth. There appeared a red spot on the forehead, from which spirted some drops of blood through the pores, without wounding the skin. The shoe belonging to the left foot was burst open, and, uncovering the foot at that place, they found a blue mark; from which it was concluded, that the electrical force of the thunder, having entered the head, made its way out again at that foot.

UPON the body, particularly on the left side, were several red and blue spots, resembling leather shrunk by being burnt. Many more blue spots were afterwards visible over the whole body, and in particular over the back. That upon the forehead changed to a brownish red, but the hair of the head was not singed, notwithstanding the spot touched some of it. In the place where the shoe was unripped, the stocking was intire; as was the coat every where, the waistcoat only being singed on the foreflap, where it joined the hinder; but there appeared on the back of Mr. Solokow's coat long narrow streaks, as if red hot wires had burned off the nap, and which could not be well accounted for.

WHEN the body was opened the next day, twenty four hours after he was struck, the cranium was very intire, having no fissure, nor cross opening; the brain as found as it possibly could be, but the transparent pellicles of the windpipe were excessively tender, gave way, and easily rent. There was some

* Phil. Transf. Vol. 48. pt. 2. p. 763.

extravasated blood in it, as likewise in the cavities below the lungs; those of the breast being quite sound, but those towards the back of a brownish black colour, and filled with more of the above-mentioned blood: otherwise, none of the entrails were touched; but the throat, the glands, and the thin intestines were all inflamed. The singed leather-coloured spots penetrated the skin only. Twice twenty four hours being elapsed, the body was so far corrupted that it was with difficulty they got it into a coffin. *

* Phil. Trans. Vol. 49. pt. 1. p. 674

SECTION

SECTION XI.

OBSERVATIONS ON THE GENERAL STATE OF ELECTRICITY
IN THE ATMOSPHERE, AND ITS MORE USUAL EFFECTS.

ELECTRICIANS, after observing the great quantity of electric matter with which the clouds are charged during a thunder storm, began to attend to the lesser quantities of it which might be contained in the common state of the atmosphere, and the more usual effects of this great and general agent in nature. Mr. Monnier, whose observations of the electricity of the air during a thunder storm have been already mentioned, was the first who found that there was very often, and perhaps always, a quantity of electric matter in the atmosphere, when there was no appearance of thunder. This he confirmed by decisive experiments, made at St. Germain en Laye, and published in a memoir read at the Royal Academy of sciences at Paris November the 15th. 1752.*

BUT more accurate experiments upon the electricity of the air were made by the Abbé Mazeas, at Chateau de Maintenon,

* Phil. Transf. Vol. 48. pt. 1. p. 203.

during

during the months of June, July, and October 1753, and communicated to the Royal Society, in a letter to Dr. Stephen Hales.

THE Abbé's apparatus consisted of an iron rod 370 feet long, raised ninety feet above the horizon. It came down from a very high room in the castle, where it was fastened to a silken cord six feet long; and was carried from thence to the steeple of the town, where it was likewise fastened to another silken cord of eight feet long, and sheltered from rain. And a large key was suspended, by the end of this wire, in order to receive the electric fluid.

ON the 17th. of June, when he began his experiments, the electricity of the air was sensibly felt every day, from sun rise till seven or eight in the evening, except in moist weather, when he could perceive no signs of electricity. In dry weather, the wire attracted minute bodies at no greater distance than three or four lines. He repeated the experiment carefully every day, and constantly observed, that, in weather void of storms, the electricity of a piece of sealing wax of two inches long was above twice as strong as that of the air. This observation inclined him to conclude, that in weather of equal driness the electricity of the air was always equal.

It did not appear to him that hurricanes and tempests increased the electricity of the air, when they were not accompanied with thunder; for that, during three days of a very violent continual wind, in the month of July, he was obliged to put some dust within four or five lines of the conductor, before any sensible attraction could be perceived.

THE direction of the winds, whether East, West, North, or South, made no sensible alteration in the electricity of the air, except when they were moist.

IN the driest nights of that summer, he could discover no signs of electricity in the air; but it returned in the morning when the sun began to appear above the horizon, and vanished again in the evening, about half an hour after sun set.

THE strongest common electricity of the atmosphere, during that summer, was perceived in the month of July, on a very dry day, the heavens being very clear, and the sun extremely hot. The distance of ten or twelve lines was then sufficient for the approach of the dust to the conductor, in order to see the particles rise in a vertical direction, like the filings of iron on the approach of a magnet.

ON the 27th. of June, at two in the afternoon, he perceived some stormy clouds rising above the horizon, and immediately went up to his apparatus; and, having applied the dust to the key, it was attracted with a force which increased in proportion as the clouds reached the zenith. When they had come nearly over the wire, the dust was so impetuously repelled, as to be entirely scattered from the paper. He drew considerable sparks from it, though there was neither thunder nor lightning. When the stormy clouds were in the zenith of his wire, he observed that the electricity was increased to such a degree, that even the filken thread attracted light bodies at the distance of seven or eight inches.

THESE stormy clouds remained about two hours above the horizon, without either thunder or lightning; nor did a very heavy rain diminish the electricity, except about the end, when the clouds began to be dissipated. *

MR. KINNERSLEY observed, that when the air was in its driest state, there was always a considerable share of electricity in

* Phil. Trans. Vol. 48. pt. 1. p. 377. &c.

it,

it, and which might be easily drawn from it. Let a person, he says, in a negative state, standing out of doors, in the dark, when the air is dry, hold, with his arm extended, a long sharp needle, pointing upwards, and he will soon be convinced that electricity may be drawn out of the air; not indeed very plentifully, for, being a bad conductor, it seems loth to part with it, yet some will evidently be collected. The air near the person's body, having less than the natural quantity, will have none to spare; but his arm being extended, as above, some will be collected from the remoter air, and will appear luminous as it converges to the point of the needle.

LET a person electrified negatively, he says, present the point of a needle horizontally, to a cork ball suspended by silk, and the ball will be attracted towards the point, till it has parted with so much of its natural quantity of electricity, as to be in a negative state, in the same degree with the person who holds the needle; then it will recede from the point, being, as he supposes, attracted the contrary way by the electricity of greater density in the air behind it. But as this opinion, he pleasantly says, seems to deviate from *electrical orthodoxy*, he would be glad to see these phenomena better accounted for by the superior and more penetrating genius of his friend Dr. Franklin, to whom he is writing.

WHETHER the electricity in the air, in clear dry weather, be of the same density at the height of 200 or 300 yards, as near the surface of the earth, he thought might be satisfactorily determined by Dr. Franklin's old experiment of the kite.

THE twine, he says, should have throughout a very small wire in it, and the ends of the wire, where the several lengths are united,

united, ought to be tied down with a waxed thread, to prevent their acting in the manner of points.

WHEN he wrote this letter, he had tried the experiment twice, when the air was as dry as it ever is in that country, and so clear that not a cloud had been seen, and found the twine each time in a small degree electrified positively.*

THE preceding observations of Mr. Monnier, Mr. Mazeas, and Mr. Kinnersley, fall far short of the extent and accuracy of those of Signior Beccaria; whose observations on the general state of electricity in the atmosphere I have reserved for the last place of the section, because they are the most considerable, though they were all made independent of, and, many of them, prior to those mentioned before.

HE observed that, during very high winds, his apparatus gave no signs of being electrified. † Indeed he found that in three different states of the atmosphere, he could find no electricity in the air. 1. In windy and clear weather. 2. When the sky was covered with distinct and black clouds, that had a slow motion. 3. In most moist weather, not actually raining. ‡ In a clear sky, when the weather was calm, he always perceived signs of a moderate electricity, but interrupted. In rainy weather, without lightning, his apparatus was always electrified a little time before the rain fell, and during the time of the rain, but it ceased to be affected a little before the rain was over.

THE higher his rods reached, or his kites flew, the stronger signs they gave of their being electrified. || Also longer strings or cords, extended and insulated in the open air, acquired electricity sooner than those which were shorter. A cord 1500 Paris feet long, stretched over the river Po, was as strongly

* Phil. Transf. Vol. 53. pt. 1. p. 87.

† Lettere dell' elettricismo, p. 106.

‡ Ib. p. 166.

|| Ib. p. 114.

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electrified

electrified during a shower, without thunder, as a metallic rod, to bring lightning into his house, had been in any thunder storm.*

HAVING two rods for bringing the lightning into his house, 140 feet asunder, he observed, that if he took a spark from the higher of them, the spark from the other, which was thirty feet lower, was at that instant lessened; but, what is remarkable, is that its power revived again, though he kept his hand upon the former. †

HE imagined that the electricity communicated to the air might sometimes furnish small sparks to his apparatus; since the air parts with the electricity it has received very slowly, and therefore the equilibrium of the electric matter in the air will not be restored so soon as in the earth and clouds. ‡

AMONG the effects of a moderate electricity in the atmosphere, Signior Beccaria reckons *rain, hail and snow*.

CLOUDS that bring rain, he thought, were produced in the same manner as thunder clouds, only by a more moderate electricity. He describes them at large, and the resemblance which all their phenomena bear to those of thunder clouds is indeed very striking. ||

HE notes several circumstances attending rain without lightning, which make it very probable, that it is produced by the same cause as when it is accompanied with lightning. Light has been seen among the clouds by night in rainy weather; and even by day rainy clouds are sometimes seen to have a brightness evidently independent of the sun. § The uniformity with which the clouds are spread, and with which the rain falls, he thought were evidences of an uniform cause, like that of elec-

* Lettere dell' elettricismo, p. 165. † Ib. p. 176. ‡ Ib. p. 347. || Ib. p. 284.
§ Ib. p. 288.

tricity.

tricity.* The intensity of electricity in his apparatus generally corresponded very nearly, to the quantity of rain that fell in the same time. † Nor is any thing to be inferred to the contrary of this supposition from the apparatus not being always electrified during rain. It has sometimes failed during thunder. Indeed it follows from his general theory, that the electricity of his apparatus could not always correspond to the electricity of the clouds; since it must in some measure depend upon the situation of the observatory, with respect to those parts of the earth or clouds which are giving or taking electric fire. This was confirmed by an observation which he made upon one thunder cloud, which passed over his observatory. At its approach his apparatus was electrified positively, when it was directly over him all signs of electricity ceased, and when it was passed, his apparatus was electrified negatively. ‡ This observation very much favours his general theory of thunder clouds.

SOMETIMES all the phenomena of thunder, lightning, hail, rain, snow, and wind, have been observed at one time; which shows the connection they all have with some common cause. ||

SIGNIOR BECCARIA, therefore, supposes that, previous to rain, a quantity of electric matter escapes out of the earth, in some place where there was a redundancy of it; and, in its ascent to the higher regions of the air, collects and conducts into its path a great quantity of vapours. The same cause that collects, will condense them more and more: till, in the places of the nearest intervals, they come almost into contact, so as to form small drops; which uniting with others as they fall, come down, in rain. The rain will be heavier in proportion as the electricity is more vi-

* Ib. p. 299. † Ib. p. 307. ‡ Ib. p. 310. § Ib. p. 290, 345.

gorous, and the cloud approaches more nearly to a thunder cloud. *

HE imitated the appearance of clouds that bring rain by insulating himself between the rubber and conductor of his electrical machine, and with one hand dropping *colopbonia* into a spoon fastened to the conductor, and holding a burning coal, while his other hand communicated with the rubber. In these circumstances, the smোক spread along his arm, and, by degrees, all over his body; till it came to the other hand that communicated with the rubber. The lower surface of this smোক was every where parallel to his cloaths, and the upper surface was swelled and arched like clouds replete with thunder and rain. † In this manner, he supposes, the clouds that bring rain diffuse themselves from over those parts of the earth which abound with electric fire, to those parts that are exhausted of it; and, by letting fall their rain, restore the equilibrium between them.

SIGNIOR BECCARIA thought that the electricity communicated to the air, which both receives and parts with it slowly, would account for the retention of vapours in a clear sky; for small disjoined clouds, not dispersed into rain; for the smaller and lighter clouds in the higher regions of the air, which are but little affected by electricity; and also for the darker, heavy, and sluggish clouds in the lower regions, which retain more of it. ‡ The degree of electricity which he could communicate to the air of his room, notwithstanding its being in contact with the floor, the walls, &c. made this appear to him both possible, and probable. ||

He even imagined, that some alteration in the weight of the air might be made by this electricity of it. § He observed his

* *Lettere dell' elettricismo*, p. 305. † *Ib.* p. 294. ‡ *Ib.* p. 348, 349. || *Ib.* p. 350. § *Ib.* barometer

barometer to fall a little immediately upon a flash of lightning; but he acknowledges that this circumstance is no sufficient foundation to suppose that electricity will account for *much* variation of the height of the barometer. * But he thought that the phenomena of rain favoured the supposition, that the electric matter in the air did, in some measure, lessen its pressure. For when the electric matter is actually in the air, collecting and condensing the vapours, the barometer is lowest. When the communication is made between the earth and the clouds by the rain, the quicksilver begins to rise; the electric matter, which supported part of the pressure, being discharged. And this, he shows, will be the case whether the electricity in the air be positive or negative. †

HAIL, this ingenious philosopher supposes to be formed in the higher regions of the air, where the cold is intense, and where the electric matter is very copious. In these circumstances, a great number of particles of water are brought near together, where they are frozen, and in their descent collect other particles: so that the density of the substance of the hail stone grows less and less from the center; this being formed first, in the higher regions, and the surface being collected in the lower. Agreeable to this, it is observed, that, in mountains, hail-stones, as well as drops of rain, are very small; there being but small space through which they can fall, and thereby increase their bulk. Drops of rain and hail agree also in this circumstance, that the more intense is the electricity that forms them, the larger they are. ‡ Motion is known to promote freezing, and so the rapid motion of the electrified clouds may promote that effect in the air. ||

* *Ib.* p. 353. † *Ib.* p. 354. ‡ *Ib.* p. 314. § *Ib.* p. 318.

CLOUDS of snow differ in nothing from clouds of rain, but in the circumstance of cold, which freezes them. Both the regular diffusion of snow, and the regularity in the structure of the parts of which it consists (particularly some figures of snow or hail, which he calls *rosette*, and which fall about Turin) show the clouds of snow to be actuated by some uniform cause like electricity.* He even endeavours, very particularly, to show in what manner certain configurations of snow are made, by the uniform action of electricity †. All these conjectures about the cause of hail and snow were confirmed by observing, that his apparatus never failed to be electrified by snow, as well as by rain.

A MORE intense electricity unites the particles of hail more closely than the more moderate electricity does those of snow. In like manner, we see thunder clouds more dense than those which merely bring rain, and the drops of rain are larger in proportion, though they often fall not from so great a height. ‡

* Lettere dell' elettricismo, p. 320, 322, 325. † Ib. p. 325, 331, 333. ‡ Ib. p. 318.

SECTION XII.

THE ATTEMPTS THAT HAVE BEEN MADE TO EXPLAIN SOME OF
THE MORE UNUSUAL APPEARANCES IN THE EARTH
AND HEAVENS BY ELECTRICITY.

IN the two preceding sections of this period, relating to the electricity of the atmosphere, the experiments and observations of Signior Beccaria have made a principal figure; and the materials I have collected from him make a no less considerable part of this. They who may have thought he indulged too much to imagination before, will think him absolutely extravagant here; but his extravagancies, if they be such, are those of a great genius; and had he a thousand more such extravagancies, I should, with pleasure, have followed him through them all.

THE meteor usually called a *falling star* has hitherto puzzled all philosophers. Signior Beccaria makes it pretty evident, that it is an electrical appearance; and the fact which he relates as a proof of it, is exceeding curious and remarkable.

As he was one time sitting with a friend in the open air, an hour after sun set, they saw what is called a falling star directing
its

its course towards them, and apparently growing larger and larger, till it disappeared not far from them; when it left their faces, hands, and cloaths, with the earth, and all the neighbouring objects, suddenly illuminated, with a diffused and lambent light, attended with no noise at all. While they were starting up, standing, and looking at one another, surpris'd at the appearance, a servant came running to them out of a neighbouring garden, and asked them if they had seen nothing; for that he had seen a light shine suddenly in the garden, and especially upon the streams which he was throwing to water it.*

ALL these appearances were evidently electrical; and Signior Beccaria was confirm'd in his conjecture, that electricity was the cause of them, by the quantity of electric matter which, as was mentioned before, he had seen gradually advancing towards his kite; for that, he says, had very much the appearance of a falling star. Sometimes also he saw a kind of *glory* round the kite, which followed it when it changed its place, but left some light, for a small space of time, in the place which it had quitted. †

THAT appearances, which bear evident marks of electricity, have a very sensible progressive motion, is demonstrat'd from a variety of meteorological observations. I shall relate one made by Mr. Chalmers, when he was on board the *Montague* under the command of Admiral Chambers. The account of it was read at the Royal Society March the 22d. 1749.

On the 4th. of November 1749, in lat. 42°. 48', long. 9°. 3', he was taking an observation on the quarter deck, about ten minutes before twelve, when one of the quarter-masters desired he would look to the windward; upon which he observed a large ball of blue fire rolling on the surface of the water, at about

* *Lettere dell' elettricismo*, p. 111.

† *Ib.* p. 130.

three miles distance from them. They immediately lowered their top sails, &c. but it came down upon them so fast, that before they could raise the main tack, they observed the ball to rise almost perpendicular, and not above forty or fifty yards from the main chains; when it went off with an explosion as if hundreds of cannon had been fired at one time, and left so great a smell of brimstone, that the ship seemed to be nothing but sulphur. After the noise was over, which, he believed, did not last longer than half a second, they found their main top-mast shattered into above a hundred pieces, and the main mast rent quite down to the heel. There were some of the spikes which nail the fish of the main mast drawn with such force out of the mast, and they stuck so fast in the main deck, that the carpenter was obliged to take an iron crow to get them out. There were five men knocked down, and one of them greatly burnt by the explosion. They believed, that when the ball, which appeared to them to be of the bigness of a large mill-stone, rose, it took the middle of the main top-mast, as the head of the mast above the hounds was not splintered. They had a hard gale of wind from the N. by W. to the N. N. E. for two days before the accident, with a great deal of rain and hail, and a large sea. From the northward they had no thunder or lightning, neither before nor after the explosion. The ball came down from the North East, and went to the South West.

THAT the *Aurora Borealis* is an electrical phenomenon was, I believe, never disputed, from the time that lightning was proved to be one. To the circumstances of resemblance which had before been taken notice of between this phenomenon and electricity; Signior Beccaria adds, that when the aurora borealis has

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extended lower than usual into the atmosphere, various sounds, as of rumbling, and hissing, have been heard. *

MR. BERGMAN says, he has often observed the magnetic needle to be disturbed by a high aurora borealis, but that he could never procure any electricity from them, either with pointed metallic rods, or by means of a kite. †

MR. CANTON (besides his conjecture, mentioned before, p. 334, that the aurora borealis may be the flashing of electric fire from positive towards negative clouds at a great distance, through the upper part of the atmosphere, where the resistance is least) supposes that the aurora borealis, which happens at the time that the needle is disturbed by the heat of the earth, is the electricity of the heated air above it; and this, he says, will appear chiefly in the northern regions, as the alteration in the heat of the air in those parts will be the greatest. This hypothesis, he adds, will not seem improbable, if it be considered, that electricity is now known to be the cause of thunder and lightning, that it has been extracted from the air at the time of an aurora borealis; that the inhabitants of the northern countries observe the aurora to be remarkably strong, when a sudden thaw happens after severe cold weather; and that the curious in these matters are now acquainted with a substance that will, without friction, both emit and absorb the electric fluid, only by the increase or diminution of its heat; meaning the tourmalin, in which he had discovered that property. ‡

In a paper dated November the 11th. 1754, he says he has sometimes known the air to be electrical in clear weather, but never at night, except when there has appeared an aurora borealis, and then but to a small degree, which he had several opportuni-

* *Elettricità artificiale e naturale*, p. 221. † *Phil. Trans.* Vol. 52. pt. 2. p. 435-
1 lb. Vol. 51. pt. 1. p. 403.

ties of observing that year. How far positive and negative electricity in the air, with a proper quantity of moisture between, to serve as a conductor, will account for this, and other meteors, sometimes seen in a serene sky, he leaves to be inquired into.*

SIGNIOR BECCARIA takes some pains to show that *water spouts* have an electrical origin. To make this more evident, he first describes the circumstances attending their appearance, which are the following.

THEY generally appear in calm weather. The sea seems to boil, and send up a smok under them, rising in a hill towards the spout. At the same time, persons who have been near them have heard a rumbling noise. The form of a water spout is that of a speaking trumpet, the wider end being in the clouds, and the narrower end towards the sea. The size is various, even in the same spout. The colour is sometimes inclining to white, and sometimes to black. Their position is sometimes perpendicular to the sea, sometimes oblique; and sometimes the spout itself is in the form of a curve. Their continuance is very various, some disappearing as soon as formed, and some continuing a considerable time. One that he had heard of continued a whole hour. But they often vanish, and presently appear again in the same place. †

THE very same things that water spouts are at sea are some kinds of *whirlwinds* and *burricanes* by land. They have been known to tear up trees, to throw down buildings, make caverns in the earth; and, in all these cases, to scatter earth, bricks, stones, timber, &c. to a great distance in every direction. ‡ Great quantities of water have been left, or raised by them, §

* Phil. Transf. Vol. 48. pt. 2. p. 784. † Eletticismo artificiale e naturale, p. 206, &c.

‡ Ib. p. 210.

as to make a kind of deluge; and they have always been attended with a prodigious rumbling noise.

THAT these phenomena depend upon electricity cannot but appear very probable from the nature of several of them; but the conjecture is made more probable from the following additional circumstances. They generally appear in months peculiarly subject to thunder storms, and are commonly preceded, accompanied, or followed by lightning, rain, or hail; the previous state of the air being similar. Whitish or yellowish flashes of light have sometimes been seen moving with prodigious swiftness about them. And, lastly, the manner in which they terminate exactly resembles what might be expected from the prolongation of one of the uniform protuberances of electrified clouds, mentioned before, towards the sea; the water and the cloud mutually attracting one another: for they suddenly contract themselves, and disperse almost at once; the cloud rising, and the water of the sea under it falling to its level. But the most remarkable circumstance, and the most favourable to the supposition of their depending upon electricity is, that they have been dispersed by presenting to them sharp pointed knives or swords. This, at least, is the constant practice of mariners, in many parts of the world where these water spouts abound; and he was assured by several of them, that the method has often been undoubtedly effectual.*

THE analogy between the phenomena of water spouts and electricity, he says, may be made visible, by hanging a drop of water to a wire communicating with the prime conductor, and placing a vessel of water under it. In these circumstances, the drop assumes all the various appearances of a water spout,

* *Elettricismo artificiale e naturale*, p. 213.

both in its rise, form and manner of disappearing. Nothing is wanting but the smoak, which may require a great force of electricity to become visible.

MR WILCKE also considers the water spout as a kind of great electrical cone, raised between the cloud strongly electrified, and the sea or the earth,* and he relates a very remarkable appearance which occurred to himself, and which strongly confirms his supposition. On the twentieth of July 1758, at three o'clock in the afternoon, he observed a great quantity of dust rising from the ground, and covering a field, and part of the town in which he then was. There was no wind, and the dust moved gently towards the East, where there appeared a great black cloud, which, when it was near his zenith, electrified his apparatus positively, and to as great a degree as ever he had observed it to be done by natural electricity. This cloud passed his zenith, and went gradually towards the West, the dust then following it, and continuing to rise higher and higher till it composed a thick pillar, in the form of a sugar loaf, and at length seemed to be in contact with the cloud. At some distance from this, there came, in the same path, another great cloud, together with a long stream of smaller clouds, moving faster than the preceding. These clouds electrified his apparatus negatively, and when they came near the positive cloud, a flash of lightning was seen to dart through the cloud of dust, the positive cloud, the large negative cloud, and as far as the eye could distinguish, the whole train of smaller negative clouds which followed it. Upon this, the negative clouds spread very much, and dissolved in rain, and the air was presently clear of all the dust. The whole appearance lasted not above half an hour. †

* Wilcke, p. 142.

† Remarks on Dr. Franklin's letters. p. 148.

To Signior Beccaria's theory of water spouts and hurricanes, I shall add a description of a hurricane in the West Indies, from the *Account of the European settlements in America*, part of which is transcribed from the *Philosophical Transactions*. Both were evidently written without the most distant view to any philosophical theory, and least of all that of electricity; and yet those who are disposed to favour this hypothesis may perceive several circumstances, which tend to strengthen it. I need not point them out.

“ It is in the rainy season, principally, in the month of August, more rarely in July and September, that they are assailed by *burrricanes*, the most terrible calamity to which they are subject from the climate. This destroys, at one stroke, the labours of many years, and frustrates the most exalted hopes of the planter; and often just at the moment when he thinks himself out of the reach of fortune. It is a sudden and violent storm of wind, rain, thunder, and lightning; attended with a furious swelling of the sea, and sometimes with an earthquake; in short, with every circumstance which the elements can assemble that is terrible and destructive.

“ FIRST they see, as a prelude to the ensuing havock, whole fields of sugar canes whirled into the air, and scattered over the face of the country. The strongest trees of the forest are torn up by the roots, and driven about like stubble. Their wind-mills are swept away in a moment. Their works, the fixtures, the ponderous copper boilers, and stills, of several hundred weight, are wrenched from the ground, and battered to pieces. Their houses are no protection. The roofs are torn off at one blast, whilst the rain, which in an hour rises five feet, rushes in upon them with an irresistible violence.

“ THERE are signs, which the Indians of these islands taught
“ our

" our planters, by which they can prognosticate the approach
 " of a hurricane. It comes on either in the quarters, or at the
 " full or change of the moon. If it will come on at the full
 " moon, you being at the change, observe these signs. That day
 " you will see the sky very turbulent. You will observe the sun
 " more red than at other times. You will perceive a dead calm,
 " and the hills clear of all those clouds and mists which usually
 " hover about them. In the clefts of the earth, and in the
 " wells, you will hear a hollow rumbling sound, like the rush-
 " ing of a great wind. At night the stars seem much larger
 " than usual, and surrounded with a sort of bars. The North
 " West sky has a black and menacing look, and the sea emits a
 " strong smell, and rises into vast waves, often without any wind.
 " The wind itself now forsakes its usual steady Easterly stream,
 " and shifts about to the West; from whence it sometimes
 " blows, with intermissions, violently and irregularly, for about
 " two hours at a time. You have the same signs at the full of
 " the moon. The moon itself is surrounded with a great bar,
 " and sometimes the sun has the same appearance."*

THE first person who advanced that earthquakes were probably
 caused by electricity, was DR. STUKELY, upon occasion of
 the earthquakes at London, on February the 8th. and on March
 the 8th. 1749; and another which affected various other parts
 of England, the center being about Daventry in Northampton-
 shire, on the 30th. of September 1750. The papers which the
 Doctor delivered to the Royal Society on these occasions, and
 which were read March the 22d. 1749, and December the 6th.
 1750, well deserve the attention of all philosophers and electri-
 cians. I shall here give the substance of both; only abridging,
 and differently arranging the materials of them.

* [Account of the European Settlements in America, Vol. 2. p. 96, &c.
 Phil. Trans. abridged, Vol. 2. p. 106.

THAT earthquakes are not owing to subterraneous winds, fires, vapours, or any thing that occasions an explosion, and heaves up the ground, he thought might easily be concluded from a variety of circumstances. In the first place, he thought there was no evidence of any remarkable cavernous structure of the earth; but that, on the contrary, there is rather reason to presume, that it is, in a great measure, solid; so as to leave little room for internal changes, and fermentations within its substance; nor do coal-pits, he says, when on fire, ever produce any thing resembling an earthquake.

In the second earthquake at London, there was no such thing as fire, vapour, smoke, smell, or an eruption of any kind observed, though the shock affected a circuit of thirty miles in diameter. This consideration alone, of the extent of surface shaken by an earthquake, he thought was sufficient to overthrow the supposition of its being owing to the expansion of any subterraneous vapours. For it could not possibly be imagined, that so immense a force, as could act upon that compass of ground instantaneously should never break the surface of it, so as to be discoverable to the sight or smell; when small fire balls, bursting in the air, have instantly propagated a sulphureous smell all around them, to the distance of several miles.

BESIDES, the operation of this great fermentation, and production of elastic vapours, &c. ought to be many days in continuance, and not instantaneous; and the evaporation of such a quantity of inflammable matter would require a long space of time.

He thought that if vapours and subterraneous fermentations, explosions, and eruptions were the cause of earthquakes, they would absolutely ruin the whole system of springs and fountains, wherever they had once been: which is quite contrary to fact, even

even where they have been frequently repeated. Mentioning the great earthquake which happened A. D. 17, when no less than thirteen great cities of Asia Minor were destroyed in one night, and which may be reckoned to have shaken a mass of earth 300 miles in diameter, he asks; How can we possibly conceive the action of any subterraneous vapours to produce such an effect so instantaneously? How came it to pass, that the whole country of Asia Minor was not at the same time destroyed, its mountains reversed, its fountains and springs broken up, and ruined for ever, and the course of its rivers quite changed? Whereas, nothing suffered but the cities. There was no kind of alteration in the surface of the country, which, indeed, remains the same to this day.

To make the hypothesis of subterraneous vapours, &c. being the cause of earthquakes the more improbable, he observes, that any subterraneous power, sufficient to move a surface of earth thirty miles in diameter, as in the earthquakes which happened at London, must be lodged at least fifteen or twenty miles below the surface of the earth, and therefore must move an inverted cone of solid earth, whose basis is thirty miles in diameter, and axis fifteen or twenty miles; an effect which, he says, no natural power could produce.

UPON the same principle, the subterraneous cause of the earthquake in Asia Minor must have moved a cone of earth of 300 miles in base and 200 in the axis; which, he says, all the gun-powder which has ever been made since the invention of it would not have been able to stir, much less any vapours, which could be supposed to be generated so far below the surface.

IT is not upon the principles of any subterraneous explosion that we can, in the least, account for the manner in which

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ships, far from any land, are affected during an earthquake; which seem as if they struck upon a rock, or as if something thumped against their bottoms. Even the fishes are affected by an earthquake. The stroke, therefore, must be occasioned by something that could communicate motion with unspeakable greater velocity than any heaving of the earth under the sea, by the elasticity of generated vapours. This could only produce a gradual swell, and could never give such an impulse to the water, as would make it feel like a stone.

COMPARING all these circumstances, he says, he had always thought, that an earthquake was an electrical shock, of the same nature with those which are now become familiar in electrical experiments. And this hypothesis he thought was confirmed by the phenomena preceding and attending earthquakes, particularly those which occasioned this publication.

THE weather, for five or six months before the first of these earthquakes, had been dry and warm to an extraordinary degree, the wind generally South and South West, and that without rain; so that the earth must have been in a state of electricity ready for that particular vibration in which electrification consists. On this account, he observes, that the Northern regions of the world are but little subject to earthquakes in comparison with the Southern, where the warmth and dryness of the air, so necessary to electricity, are common. All the flat country of Lincolnshire before the earthquake in September, though, underneath it is a watery bog, yet, through the whole preceding summer and autumn, (as they can have no natural springs in such a level) the drought had been so great on the surface of the earth, that the inhabitants were obliged to drive their cattle several miles to water. This, he says, shows how fit the dry surface was for an electrical

electrical vibration; and also, which is of great importance, that earthquakes reach but very little below the surface of the earth.

BEFORE the earthquake at London, all vegetables had been uncommonly forward. At the end of February, in that year, all sorts of garden stuff, fruits, flowers, and trees were observed to be as forward as, in other years, about the middle of April; and electricity is well known to quicken vegetation.

THE Aurora Borealis had been very frequent about the same time, and had been twice repeated just before the earthquake, of such colours as had never been seen before. It had also removed to the South, contrary to what is common in England; so that some Italians, and people from other places where earthquakes are frequent, observing these lights, and the peculiar temperature of the air, did actually foretell the earthquake. For a fortnight before the earthquake in September, the weather was serene, mild, and calm; and, one evening, there was a deep red Aurora Borealis, covering the cope of heaven, very terrible to behold.

THE whole year had been exceedingly remarkable for fire balls, thunder, lightning, and coruscations, almost throughout all England. Fire balls were more than once seen in Ireland, and Lincolnshire, and particularly observed. And all these kinds of meteors, the Doctor says, are rightly judged to proceed from the electrical state of the atmosphere.

IN these previous circumstances of the state of the earth and air, nothing, he says, is wanting to produce the wonderful effect of an earthquake, but the touch of some non-electric body; which must necessarily be had *ab extra*, from the region of the air, or atmosphere. Hence, he infers that, if a non-electric cloud discharge its contents upon any part of the earth, in that highly electrical state, an earthquake must necessarily ensue. As the discharge from an excited tube produces a commotion in the

human body, so the discharge of electric matter from the compass of many miles of solid earth must needs be an earthquake, and the snap from the contact be the horrid uncouth noise attending it.

THE Doctor had been informed, by those who were up and abroad the night preceding the earthquake, and early in the morning, that convulsions in the air were extremely frequent; and that, a little before the earthquake, a large and black cloud suddenly covered the atmosphere, which probably occasioned the shock, by the discharge of a shower. Dr. Childrey, he says, observes, that earthquakes are always preceded by rain, and sudden tempests of rain in times of great draught.

A SOUND was observed to roll from the river Thames towards Temple-Bar, before the houses ceased to nod, just as the electrical snap precedes the shock. This noise, an observer said, was much greater than any he had ever heard. Others, who write upon earthquakes, commonly observe, that the noise precedes the shock: whereas it must have been quite the contrary, if the concussion had depended upon a subterraneous eruption. This noise attending earthquakes, the Doctor thought, could not be accounted for, but upon the principles of electricity. The earthquake in September was attended with a rushing noise, as if houses were falling, and people, in some places, were so universally frightened, as to run out of their houses, imagining that their own, and those of their neighbours were tumbling on their heads. In some villages, the people, being at divine service, were much alarmed with the noise; which they said, beyond all comparison, exceeded all the thunder they had ever heard.

THE flames and sulphureous smells, which are sometimes observed during earthquakes, the Doctor thought were more easily accounted for, from the supposition of their being electrical phenomena.

nomena, than from their being occasioned by the eruption of any thing from the bowels of the earth.

THE impressiion made by an earthquake upon land and water, to the greatest distances is, as was observed before, instantaneous; which could only be effected by electricity. In the earthquake in September, the concussion was felt through a space of 100 miles in length, and forty in breadth; and, as far as could be judged, at the same instant of time. That this tract of ground, which amounted to 4000 square miles in surface, should be thrown into such agitation in a moment, is such a prodigy, the Doctor says, as we could never believe, or conceive, did we not know it to be fact from our senses. But if we seek the solution of it, we cannot think any natural power equal to it, but that of electricity; which acknowledges no sensible transi-tion of time, no bounds.

THE little damage generally done by earthquakes, the Doc-tor thought to be an argument of their being occasioned by a simple vibration, or tremulous motion of the surface of the earth, by an electrical snap. This vibration, he says, impressed on the water, meeting with the solid bottoms of ships, and lighters, occasions that thump which is said to be felt by them: yet, of the millions of ordinary houses, over which it passed, not one fell. A consideration which sufficiently points out what sort of a motion this was not; also what sort of a motion it was, and whence derived; not a convulsion in the bowels of the earth, but an uniform vibration along its surface, like that of a musical string, or what we put a drinking glass into, by rubbing one's finger on the edge; which yet, being brought to a certain pitch, breaks the glass; undoubtedly, he adds, an electrical repulsion of its parts.

THAT earthquakes are electrical phenomena, is further evi-
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dent, he says, from their chiefly affecting the sea coast, places along rivers, and, I may add, eminences. The earthquake in September spread mostly to the North and South; which the Doctor says is the direction of the Spalding river, whereby it was conveyed to the sea shore, where it was particularly sensible; thence up Boston channel, and so up Boston river to Lincoln. The greatest part of this earthquake displayed its effects along, and between the two rivers Welland, and Avon, and that from their sources down to their mouths. It likewise reached the river Witham, which directed the electrical stream that way also to Lincoln; for which reason, meeting the same coming from Boston, it was most sensibly felt there. It reached, likewise, to the Trent at Nottingham, which conveyed it to Newark.

THE first electrical stroke in this earthquake seemed to the Doctor to have been made on the high ground about Daventry in Northamptonshire. From thence it descended chiefly Eastward, and along the river Welland, from Harborough, to Stamford, Spalding, and the sea; and along the river Avon and Nen to Northampton, Peterborough, Wisbich, and the sea. It spread itself all over the vast level of the isle of Ely, promoted by a great number of canals; natural and artificial, made for draining that country. It was still conducted Eastward, by Mildenhall river in Suffolk, to Bury, and the parts adjacent. All these circumstances duly considered were to him a confirmation of the doctrine he advanced on this subject.

LASTLY, the Doctor adds, as a further argument in favour of his hypothesis, that pains in the back, rheumatic, hysteric, and nervous cases; head aches, cholics, &c. were felt by many people of weak constitutions, for a day or two after the earthquake;

quake; just as they would after electrification; and, to some, these disorders proved fatal.

IN what manner the earth and atmosphere are put into that electrical and vibratory state, which prepares them to give or receive that snap and shock, which we call an earthquake, and whence it is that this electric matter comes, the Doctor does not pretend to say; but thinks it as difficult to account for as magnetism, gravitation, muscular motion, and many other secrets in nature.*

To these observations of Dr. Stukeley, I shall add some circumstances which were observed by Dr. Hales, in the earthquake at London, on March the 8th. 1749; as tending to strengthen the hypothesis of its being caused by electricity; though the Doctor, who relates them, thought that the electric appearance were only occasioned by the great agitation which the electric fluid was put into, by the shock of so great a mass of the earth.

AT the time of the earthquake, about twenty minutes before six in the morning, the Doctor, being awake in bed, on a ground floor, at a house near the church of St. Martin's in the fields, very sensibly felt his bed heave, and heard an obscure rushing noise in the house, which ended in a loud explosion up in the air, like that of a small cannon. The whole duration, from the beginning to the end, seeming to be about four seconds.

THIS great noise, the Doctor conjectured, was owing to the rushing, or sudden expansion of the electric fluid at the top of St. Martin's spire, where all the electric effluvia, which ascended along the large body of the tower, being strongly condensed,

* Phil. Trans. abridged, Vol. 10. p. 526, 535, and p. 541., 551.

and accelerated at the point of the weathercock, as they rushed off, made so much the louder expansive explosion.

THE DOCTOR further says, that the soldiers, who were upon duty in St. James's park, and other persons who were then up, saw a blackish cloud, and a considerable lightning, just before the earthquake began. *

MR. HARTMAN is of opinion that electricity is the cause of earthquakes, and gives a succinct enumeration of all the circumstances which favour this hypothesis. †

MY reader, who has seen to how great an extent Signior Beccaria has already carried the principles of electricity, will have no doubt but that he supposes earthquakes to be derived from that cause. And indeed, without any knowledge of what Dr. Stukely had done, he did suppose them to be electrical phenomena; but, contrary to the Doctor, imagined the electric matter which occasioned them to be lodged deep in the bowels of the earth, agreeable to his hypothesis concerning the origin of lightning.

It is certain that if Signior Beccaria's account of the origin of thunder clouds be admitted, there will be little difficulty in admitting further, that earthquakes are to be reckoned among the effects of electricity. For if the equilibrium of the electric matter can, by any means, be lost in the bowels of the earth; so that the best method of restoring it shall be by the fluid bursting its way into the air, and traversing several miles of the atmosphere, to come to the place where it is wanted; it may easily be imagined, that violent concussions may be given to the earth, by the sudden passage of this powerful agent. And several circumstances attending earthquakes he thought rendered this hypothesis by no means improbable.

VOLCANOS are known to have a near connection with earth-

* Phil. Transf. abridged, Vol. 10. p. 540, 541. † Abhandlung, p. 148.

quakes;

quakes; and flashes of light, exactly resembling lightning, have frequently been seen to rush from the top of mount Vesuvius, at the time that ashes and other light matter have been carried out of it into the air, and been dispersed uniformly over a large tract of country. Of these be produces a great number of instances, from the best authority.*

A RUMBLING noise, like thunder, is generally heard during an earthquake. At such times, also, flashes of light have been seen rising out of the ground, and darting up into the air. Real lightning hath sometimes occasioned small shakings of the earth, at least has been attended by them. But the strongest circumstance of resemblance which he observed is the same that Dr. Stukeley lays so much stress on, viz. the amazing swiftness with which the earth is shaken in earthquakes. An earthquake, says he, is by no means a gradual heaving, as we might have expected from other causes, but an instantaneous concussion, so that the fluidity of the water is no security against the blow. The very ships, many leagues off the coast, feel as if they struck against a rock.

THIS admirable philosopher, having imitated all the great phenomena of natural electricity in his own room, doth not let the earthquake escape him. He says, that if two pieces of glass, inclosing a thin piece of metal, be held in the hand, while a large shock is sent through them, a strong vibration, or concussion will be felt; which sometimes, as in Dr. Franklin's experiments, breaks them to pieces.

SIGNIOR BECCARIA thinks, that there are traces of electrical operations in the earthquake, that happened at Julian's attempt to rebuild the temple of Jerusalem. †

* Lettere dell' elettricismo, p. 226, 362, &c. † Ib. p. 365.

THAT the electric fluid is sometimes collected in the bowels of the earth, he thought was probable from the appearance of *ignes fatui* in mines, which sometimes happens; and is very probably an electrical phenomenon.*

WHICH of these two philosophers have advanced the more probable opinion concerning the seat of the electric matter which occasions earthquakes, I shall not pretend to decide. I shall only observe that, perhaps, a more probable general hypothesis than either of them may be formed out of them both. Suppose the electric matter to be, some way or other, accumulated on one part of the surface of the earth, and, on account of the dryness of the season, not easily to diffuse itself; it may, as Signior Beccaria supposes, force itself a way into the higher regions of the air, forming clouds in its passage out of the vapours which float in the atmosphere, and occasion a sudden shower, which may further promote the passage of the fluid. The whole surface, thus unloaded, will receive a concussion, like any other conducting substance on parting with, or receiving a quantity of the electric fluid. The rushing noise will, likewise, sweep over the whole extent of the country. And, upon this supposition, also, the fluid, in its discharge from the country, will naturally follow the course of the rivers, and also take the advantage of any eminences, to facilitate its ascent into the higher regions of the air.

I SHALL close this account of the theory of lightning, and other phenomena of the atmosphere, with an enumeration of the principal appearances of natural electricity observed by the ancients, and which were never understood before the discovery of

* Dell' elettricismo artificiale e naturale, p. 223.

Dr. Franklin. It will be very easy for me to do this, as I find them already collected to my hands by Dr. Watson.*

A LUMINOUS appearance, which must have been of an electrical nature, is mentioned by Plutarch in his life of Lyfander. He considered it as a meteor.

PLINY, in his second book of natural history, calls these appearances *stars*, and tells us, that they settled not only upon the masts, and other parts of ships; but also upon men's heads. *Exsistunt*, says that historian, *stellæ et in mari terrisque. Vidi nocturnis militum vigiliis inhaerere pilis pro vallo fulgorem effigie eae et antennis navigantium, aliisque navium partibus, ceu vocali quodam sono insistunt, ut volucres, sedem ex sede mutantes.*—*Geminæ autem salutare et prosperi cursus prænunciæ; quarum adventu, fugari diram illam ac minacem appellatamque Helenam ferunt. Et ob id Polluci et Castori id numen assignant, eosque in mari deos invocant. Hominum quoque capiti vespertinis horis, magno præfagio circumfulgent.* But, adds he, these things are *incerta ratione et in naturæ majestate abdita.*

“STARS make their appearance both at land and sea. I have
“seen a light in that form on the spears of soldiers, keeping
“watch by night upon the ramparts. They are seen also on the
“sailyards, and other parts of ships, making an audible sound,
“and frequently changing their places. Two of these lights
“forebode good weather, and a prosperous voyage; and drive
“away one that appears single, and wears a threatening aspect.
“This the sailors call Helen, but the *two* they call Castor and
“Pollux, and invoke them as Gods. These lights do sometimes,
“about the evening, rest on men's heads, and are a great and

* Phil. Transf. Vol. 48. pt. 1. p. 210.

“ good omen. But these are among the awful mysteries of nature.”

SENECA in his *Natural Questions*, chap. I. takes notice of the same phenomenon. *Gylippo, Syracusas petenti visa est stella supra ipsam lanceam constitisse. In Romanorum castris visa sunt ardere pila, ignibus scilicet in illis delapsis.*

“ A STAR settled on the lance of Gylippus, as he was sailing to Syracuse: and spears have seemed to be on fire in the Roman camp.”

IN Cæsar de Bello Africano, cap. 6. edit. Amstel. 1686. We find them attending a violent storm. *Per id tempus fere Cæsaris exercitui res accidit incredibilis auditu; nempe Virgiliarum signo confecto, circiter vigilia secunda noctis, nimbus cum saxea grandine subito est coortus ingens. Eadem nocte legionis V. pilorum cacumina sua sponte arserunt.*

“ About that time, there was a very extraordinary appearance in the army of Cæsar. In the month of February, about the second watch of the night, there suddenly arose a thick cloud, followed by a shower of stones; and the same night, the points of the spears belonging to the fifth legion seemed to take fire.”

LIVY, cap. 32. mentions two similar facts. *In Sicilia militibus aliquot spicula, in Sardinia muro circumeunti vigiliis equiti, scipionem, quem in manu tenuerat, arsisse; et litora crebris ignibus fulsisse.*

“ THE spears of some soldiers in Sicily, and a walking stick, which a horseman in Sardinia was holding in his hand, seemed to be on fire. The shores were also luminous with frequent fires.”

THESE appearances are called, both by the French and Spaniards, inhabiting the coasts of the Mediterranean, St. Helme's, or St. Telme's fires; by the Italians the fires of St. Peter, and St.

St.

St. Nicholas; and are frequently taken notice of by the writers of voyages.

If some late accounts from France, adds the Doctor, are to be depended upon, this phenomenon, has been observed at Plauzet for time immemorial, and Mr. Binon, the curé of the place, says, that for twenty seven years, which he has resided there, in great storms accompanied with black clouds, and frequent lightnings, the three pointed extremities of the cross of the steeple of that place appeared surrounded with a body of flame; and that when this phenomenon has been seen, the storm was no longer to be dreaded, and calm weather returned soon after.

MODERN history furnishes a great number of examples of flames appearing at the extremities of pointed metallic bodies, projecting into the air. Little notice was taken of these, while the cause of them was unknown; but since their near affinity with lightning has been discovered, they have been more attended to, and collected.

SECTION

SECTION XIII.

OBSERVATIONS ON THE USE OF METALLIC CONDUCTORS TO SECURE BUILDINGS, &c. FROM THE EFFECTS OF LIGHTNING.

THE former sections of this period relate chiefly to the theory of electricity. In the two next I shall consider what has been done towards reducing this science into practice. And, in the first place, I shall recite the observations that have been made respecting the use of metallic conductors, to secure buildings from lightning, as having the nearest connection with the subject of the sections immediately preceding.

DR. FRANKLIN'S proposal to preserve buildings from the dreadful effects of lightning was by no means a matter of mere theory. Several striking facts, which occurred within the period of which I am treating, demonstrate its utility.

INNUMERABLE observations show how readily metallic rods actually conduct lightning, and how small a substance of metal is sufficient to discharge great quantities of it. Mr. Calendrini, who afterwards applied to Dr. Watson, to be informed of the best methods of securing powder magazines, says that he himself

self was an eye witness of the effect of a flash of lightning, where he observed it had struck the wire of a bell, and had been completely conducted by it, from one room of a house to another, through a very small hole in the partition. This observation was prior to the discoveries of Dr. Franklin, but was recollected and recorded afterwards.*

DR. FRANKLIN himself, in a letter to Mr. Dalibard, dated Philadelphia, June the 29th. 1755, relating what had been shown him of the effects of lightning on the church of Newbury in New England, observes; that a wire, not bigger than a common knitting needle, did in fact conduct a flash of lightning, without injuring any part of the building as far as it went; though the force of it was so great that, from the termination of the wire down to the ground, the steeple was exceedingly rent and damaged, some of the stones, even in the foundation, being torn out, and thrown to the distance of twenty or thirty feet. No part of the wire, however, could be found, except about two inches at each extremity, the rest being exploded, and its particles dissipated in smoke and air, as the Doctor says, like gunpowder by a common fire. It had only left a black smutty track upon the plaister of the wall along which it ran, three or four inches broad, darkest in the middle, and fainter towards the edges. From the circumstances of this fact it was very evident, that, had the wire been continued to the foot of the building, the whole shock would have been conducted without the least injury to it, though the wire would have been destroyed.†

BUT the most complete demonstration of the real use of Dr. Franklin's method of securing buildings from the effects of lightning, is Mr. Kinnerley's account of what happened to the house

* Phil. Transf. Vol. 54. pt. 1. p. 203.

† Ib. Vol. 49. pt. 1. p. 309.

of Mr. West, a merchant of Philadelphia in Pennsylvania, which was guarded by an apparatus constructed according to the directions of Dr. Franklin. It consisted of an iron rod, which extended about nine feet and an half above a stack of chimnies, to which it was fixed. It was more than half an inch in diameter in the thickest part, and went tapering to the upper end, in which there was an hole that received a brass wire about three lines thick and ten inches long, terminating in a very acute point. The lower part of the apparatus joined to an iron stake, driven four or five feet into the ground.

MR. WEST judging, by the dreadful flash of lightning and instant crack of thunder, that the conductor had ben struck, got it examined; when it appeared, that the top of the pointed rod was melted, and the small brass wire reduced to seven inches and a half in length, with its top very blunt. The slenderest part of the wire he suspected, had been dissipated in smoke; but some of it, where the wire was a little thicker, being only melted by the lightning, sunk down (while in a fluid state) and formed a rough irregular cap, lower on one side than on the other, round the upper end of what remained, and became intimately united with it. It is remarkable, that, notwithstanding the iron stake, in which the apparatus terminated, was driven three or four feet into the ground, yet the earth did not conduct the lightning so fast, but that, in thunder storms the lightning would be seen diffused near the stake two or three yards over the pavement, though at that time very wet with rain.*

In order to secure ships from sustaining damage by lightning, Dr. Watson, in a letter to Lord Anson, read at the Royal Society, December the 16th. 1762, advises, that a rod of copper,

Phil. Transf. Vol. 53. pt. 1. p. 96.

about the thickness of a goose quill, be connected with the spindles and iron work of the mast; and, being continued down to the deck, be from thence, in any convenient direction, so disposed, as always to touch the sea water. *

WITH respect to powder magazines, Dr. Watson advised Mr. Calandrini above-mentioned, that the apparatus to conduct the lightning from them be detached from the buildings themselves, and conveyed to the next water.

WHAT lately happened to St. Bride's church in London is a sufficient proof of the utility of metallic conductors for lightning. Dr. Watson, who published an account of this fact in the Philosophical Transactions, observes, that the lightning first took a weathercock, which was fixed at the top of the steeple; and was conducted without injuring the metal, or any thing else, as low as where the large iron bar or spindle which supported it (and which came down several feet into the top of the steeple) terminated. There, the metallic communication ceasing, part of the lightning exploded, cracked, and shattered the obelisk, which terminated the spire of the steeple, in its whole diameter, and threw off at that place several large pieces of Portland stone, of which the steeple was built. Here it likewise removed a stone from its place, but not far enough to be thrown down. From thence the lightning seemed to have rushed upon two horizontal iron bars, which were placed within the building cross each other, to give additional strength to the obelisk, almost at the base of it, and not much above the upper story. At the end of one of these iron bars, on the East and North East side, it exploded again, and threw off a considerable quantity of stone. Almost all the damage done to the steeple, except near the top, was

* *Ib.* Vol. 52. pt. 2. p. 633.

confined to the East and North East side, and generally, where the ends of the iron bars had been inserted into the stone, or placed under it; and, in some places, by its violence in the stone, its passage might be traced from one iron bar to another.

It is very remarkable, that, to lessen the quantity of stone in this beautiful steeple, cramps of iron had been employed in several parts of it; and upon these, stones of no great thickness had been placed, both by way of ornament, and to cover the cramped joint. In several places these stones had, on account of their covering the iron, been quite blown off, and thrown away. A great number of stones, some of them large ones, were thrown from the steeple, three of which fell upon the roof of the church, and did great damage to it; and one of them broke through the large timbers which formed the roof, and lodged in the gallery.

Upon the whole, the steeple was found, on a survey, to be so much damaged in several of its parts, that eighty five feet were taken down, in order to restore it substantially; and the manner in which this steeple was damaged completely indicated, as Dr. Watson observes, the great danger of insulated masses of metal from lightning; and, on the contrary, evinced the utility and importance of masses of metal continued, and properly conducted, in defending them from its direful effects. The iron and lead employed in this steeple, in order to strengthen and preserve it, did almost occasion its destruction; though, after it was struck by the lightning, had it not been for these materials keeping the remaining parts together, a great part of the steeple must have fallen.

This building suffered the more, on account of the thunder storm having been preceded by several very warm days. The nights had scarce furnished any dew, the air was quite dry, and in

in a state perfectly unfit to part with its highly accumulated electricity, without violent efforts. This great dryness had made the stones of St. Bride's steeple, and all other buildings under the like circumstances, far less fit, than if they had been in a moist state, to conduct the lightning, and prevent the mischief. For, though the thunder storm ended in a heavy shower of rain, none, except a few very large drops, fell till after the church was struck. And Dr. Watson had no doubt, but that the succeeding rain prevented many accidents of a similar kind, by bringing down, with every drop of it, part of the electric matter, and thereby restoring the equilibrium between the earth and clouds.

It is frequently observed, he says, that, in attending to the apparatus for collecting the electricity of the clouds, though the sky is much darkened, and there have been several claps of thunder, at no great distance, yet the apparatus will scarcely be affected by it; but that, as soon as the rain begins and falls upon so much of the apparatus as is placed in the open air, the bells belonging to it will ring, and the electrical snaps succeed each other in a very extraordinary manner. This, as he observes, demonstrates, that every drop of rain brings down part of the electric matter of a thunder cloud and dissipates it in the earth and water, thereby preventing the mischiefs of its violent and sudden explosion. Hence when the heavens have a menacing appearance, a shower of rain is much to be wished for.

From all these considerations, Dr. Watson had no doubt, but that the mischief done to St. Bride's steeple was owing to the efforts of the lightning, after it had possessed the apparatus of the weathercock, endeavouring to force itself a passage from thence to the iron work employed in the steeple. As this must be done *per saltum*, as he expresses it, there being no regular metallic communication, it was no wonder, when its force was vehement, that it rent every thing which was not metallic that ob-

fructed its easy passage; and that, in this particular instance, the ravages increased, as the lightning, to a certain distance, came down the steeple.

THE DOCTOR advises that, in order to have ocular demonstration when these metallic conductors do really discharge the lightning, that they be discontinued for an inch or two, in some place convenient for observation; in which case the fire will be seen to jump from one extremity of the wire to the other. If any danger be apprehended from this discontinuance of the metallic conductor, he says that a loose chain may be ready to hang on, and complete the communication.*

MR. DELAVAL, who also gives an account of the same accident, observed, that, in every part of the building that was damaged, the lightning had acted as an elastic fluid, endeavouring to expand itself where it was accumulated in the metal; and that the effects were exactly similar to those which would have been produced by gunpowder pent up in the same places, and exploded.

IN the same paper Mr. Delaval gives it as his opinion, that a wire, or very small rod of metal, did not seem to have been a canal sufficiently large to conduct so great a quantity of lightning as struck this steeple; especially if any part of it, or of the metal communicating with it; was inclosed in the stone work; in which case, he thought, the application of it would tend to increase its bad effects, by conducting it to parts of the building which it might otherwise not have reached.

UPON the whole, he thought that a conductor of metal, less than six or eight inches in breadth, and a quarter of an inch in thickness (or an equal quantity of metal in any other form that

* Phil. Transf. Vol. 54. p. 201, &c.

might

might be found more convenient) cannot with safety be depended on, where buildings are exposed to the reception of a great quantity of lightning. *

MR. WILSON, in a paper written upon the same occasion, advises, that pointed bars or rods of metal be avoided in all conductors of lightning.

As the lightning, he says, must visit us some way or other, from necessity, there can be no reason to invite it at all; but, on the contrary, when it happens to attack our buildings, we ought only so to contrive our apparatus, as to be able to carry the lightning away again, by such suitable conductors, as will very little, if at all, promote any increase of its quantity.

To attain this desirable end, in some degree at least, he proposes, that the several buildings remain as they are at the top; that is, without having any metal above them, either pointed or not, by way of conductor; but that on the inside of the highest part of the building, within a foot or two of the top, a rounded bar of metal be fixed, and continued down along the side of the wall, to any kind of moisture in the ground. †

SIGNIOR BECCARIA whose observations and experience with respect to lightning give a weight to his opinion superior to that of any other man whatever, seems to think very differently from Mr. Wilson on this subject. He says that no metallic apparatus can attract more lightning than it can conduct. And so far is he from thinking one conductor, rounded at the top, and a foot or two under the roof sufficient; that if the building be of any extent, he advises to have several of the usual form; that is, pointed, and higher than the building. One conductor he thought sufficient for one tower, steeple, or ship; but he thought, two

* Phil. Transf. Vol. 54. p. 234. † Ib. p. 249.

necessary.

necessary for the wing of a building 200 feet long, one at each extremity; three for two such wings, the third being fixed in the middle; and four for a square palace of the same front, one at each corner.*

My readers at a distance from London will hardly believe me, when I inform them, that the elegant spire which has been the subject of a great part of this section, and which has been twice damaged by lightning (for it is now very probable, that a damage it received in the year 1750, was owing to the same cause) is now repaired, without any metallic conductor, to guard it in case of a third stroke.

* Lettere dell' elettricismo, p. 278.

SECTION

SECTION XIV.

OF MEDICAL ELECTRICITY.

THE subject of medical electricity falls almost wholly within the period of which I am now treating. For, though some effects of electricity upon animal bodies had been noted by the Abbé Nollet, and a few diseased persons had said they had received benefit from being electrified; yet very little had been done this way, and physicians had scarcely attended to it, till within this period; whereas electricity is now become a considerable article in the *materia medica*.

THE first account I have met with of the application of electricity to medical purposes is of Mr. C. Kratzenstein, professor of medicine at Halle; who, in the year 1744, cured a woman of a contracted little finger in a quarter of an hour. He also so far relieved a person who had two lame fingers, by once electrifying them, that he could play upon the harpsicord, which he had before been disabled from doing. He also observed, that a man's pulse, which had beat eighty in a second before he was electrified,

fied, immediately after beat eighty eight, and was presently increased to ninety six. *

THERE is, another celebrated instance of the cure of a palsy before this period; which is that performed by Mr. Jallabert, professor of Philosophy and Mathematics at Geneva, on a locksmith of the age of fifty two, † whose right arm had been paralytic fifteen years, occasioned by a blow of a hammer. He was brought to Mr. Jallabert on the 26th. of December 1747, and was almost completely cured by the 28th. of February 1748. In this interval he was frequently electrified, sparks being taken from the arm, and sometimes the electric shock sent through it. ‡ Mr. Jallabert's own account of this cure is very circumstantial.

THE report of this cure performed at Geneva engaged Mr. Sauvages of the Academy in Montpelier to attempt the cure of paralytics, in which he had considerable success. In one case it occasioned a salivation, and in another a profuse sweat. Many paralytics, however, were electrified without any success. Indeed the prodigious concourse of patients of all kinds, which the report of these cures brought together, was so great, that few of them could be electrified, except very imperfectly. For two or three months together, twenty different patients were electrified every day. It is not surprising to find, that the neighbouring populace considered these cures as an affair of witchcraft, and that the operators were obliged to have recourse to their priests to undeceive them. || In the course of these experiments it was found, by very accurate observations made with a pendulum, that electrification increases the circulation of the blood about one sixth.

* Geschichte, Vol. 1. p. 294. † Jallabert's experiences, 143. ‡ Histoire, pt. 3. p. 36.

|| Ib. p. 97.

ONE of the first who attended to electricity in a medical way was Dr. Bohadch a Bohemian; who, in a treatise upon medical electricity, communicated to the Royal Society, gave it as his opinion, after the result of much experience, that of all distempers, the *hemiplegia* seemed to be the most proper object of electricity! He also thought it might be of use in intermitting fevers.

THE palsy having happened to be the first disorder in which electricity gave relief, there was a considerable number of cases published pretty early, in which paralytics were said to have found benefit from this new method of treatment. In the year 1757 Mr. Patric Brydone performed a complete cure of a hemiplegia, and, indeed, an almost universal paralytic affection, in about three days. The patient was a woman, aged thirty three, and the palsy was of about two years continuance. † And John Godfrey Teske, very nearly cured a young man, of the age of twenty, of a paralytic arm, of which he had not had the least use from the age of five years. ‡

THE Abbé Nollet's experiments upon paralytics had no permanent good effect. || He observes however that, during fifteen or sixteen years that he had electrified all sorts of persons, he had known no one bad effect to have arisen from it to any of them. §

DR. HART, in a letter to Dr. Watson, dated Salop March the 20th. 1756, mentions a cure performed by electricity upon a woman of twenty three years of age, whose hand and wrist had for some time been rendered useless by a violent contraction of the muscles. She was not sensible of the first shock that was given her; but, as the shocks were repeated, the sensation in-

* Phil. Transf. Vol. 47. p. 351. † Ib. Vol. 50. pt. 1. p. 392. ‡ Ib. Vol. 51. pt. 1. p. 179.

|| Recherches, p. 412. § Ib. p. 416.

creased, till she was perfectly well. She was also cured a second time, after a relapse occasioned by a cold.*

But perhaps the most remarkable case that has yet occurred of the use of electricity in curing a disorder of this kind; and indeed of any that is incident to the human body was of that dreadful disorder, an universal *tetanus*. It is related by Dr. Watson in the Philosophical Transactions; and the account was read at the Royal Society the 10th. of February 1763. The patient was a girl belonging to the Foundling hospital, about seven years of age, who was first seized with a disorder occasioned by the worms, and at length by an universal rigidity of her muscles; so that her whole body felt more like that of a dead animal than a living one. She had continued in this dismal condition above a month, and about the middle of November 1762, after all the usual medicines had failed, Dr. Watson began to electrify her; and continued to do it by intervals, till the end of January following; when every muscle of her body was perfectly flexible, and subservient to her will, so that she could not only stand upright, but could walk, and even run like other children of her age. †

Dr. EDWARD SPRY relates a complete cure which he made of a locked jaw and paralysis, in the case of a girl of eighteen years of age. Small shocks were given to the muscles particularly affected. ‡

That electricity may be hurtful, and even in some cases in which analogy would lead us to promise ourselves it might be of use, is evident from many cases, and particularly from one related by Dr. Hart of Shrewsbury, in a letter to Dr. Watson, which was read at the Royal Society November the 14th. 1754.

* Phil. Transf. Vol. 49. pt. 2. p. 558. † Ib. Vol. 53. p. 10. ‡ Ib. Vol. 57. p. 88.

A YOUNG girl about sixteen, whose right arm was paralytic, and greatly wasted in comparison of the other, on being electrified twice, became universally paralytic, and remained so above a fortnight; when the new palsy was removed by proper medicines, though the first diseased arm remained as before. However Dr. Hart, notwithstanding this bad accident, had a mind to try electricity again. The girl submitted to it; but after having been electrified about three or four days, she became a second time universally paralytic, and even lost her voice, and the use of her tongue; so that it was with great difficulty she could swallow. She was relieved of this additional palsy a second time by a proper course of medicines, continued about four months; but was discharged out of the hospital as incurable of her first palsy. It is said that the Doctor would have tried electricity a third time; but the girl, being more nearly concerned in the experiment than her physician, thought proper to decline it. *

DR. FRANKLIN'S account of the effects of electricity, in the manner in which he applied it, is by no means favourable to its use in such cases. He says, in a letter to Dr. Pringle, read at the Royal Society January the 12th. 1758, that some years before, when the news papers made mention of great cures performed in Italy and Germany by electricity, a number of paralytics were brought to him, from different parts of Pennsylvania and the neighbouring provinces, to be electrified, and that he performed the operation at their request. His method was, first to place the patient in a chair, or upon an electric stool, and draw a number of large strong sparks from all parts of the affected limb or side. He then fully charged two six gallon glass

* Phil. Transf. Vol. 48. pt. 2. p. 735.

jars, and sent the united shock of them through the affected limb or limbs, repeating the stroke commonly three times each day.

THE first thing he observed was an immediate greater sensible warmth in the affected limbs, which had received the stroke, than in the others; and the next morning the patients usually said, that, in the night, they had felt a prickling sensation in the flesh of the paralytic limbs; and would sometimes show a number of small red spots, which they supposed were occasioned by those prickings. The limbs too were found more capable of voluntary motion, and seemed to receive strength. A man, for instance, who could not, the first day, lift his lame hand from off his knee, would the next day raise it four or five inches, the third day higher; and on the fifth was able, but with a feeble languid motion, to take off his hat. These appearances, the Doctor says, gave great spirits to the patients, and made them hope for a perfect cure; but he did not remember that he ever saw any amendment after the fifth day; which the patients perceiving, and finding the shocks pretty severe, became discouraged, went home; and in a short time relapsed; so that he never knew any permanent advantage from electricity in palsies.

PERHAPS, says he, some permanent advantage might be obtained, if the electric shocks had been accompanied with proper medicine and regimen, under the direction of a skilful physician. He thought too, that many small shocks might have been more proper than the few great ones which he gave; since, in an account from Scotland, a case was mentioned in which 200 shocks from a phial were given daily, and a perfect cure had been made.*

THAT there is an intimate connection between the state of

* Phil. Trans. Vol. 50. pt. 2. p. 481.

electricity

electricity in the air and the human body, is evident from several facts, particularly a very remarkable one related by the Abbé Mazéas, in a letter to Dr. Hales. He was electrifying a person who was subject to epileptic fits, by his apparatus to make observations upon the electricity of the common atmosphere. At first this person bore the sparks very well, but in two or three minutes the Abbé, perceiving his countenance to change, begged he would retire, lest any accident should happen; and he was no sooner returned home, than his senses failed him, and he was seized with a most violent fit. His convulsions were taken off with spirits of hartshorn, but his reason did not return in an hour and an half. He went up and down the stairs like one who walks in his sleep, without speaking to, or knowing any person, settling his papers, taking snuff, and offering chairs to all who came in. When he was spoken to, he pronounced inarticulate words, which had no connection.

WHEN this poor man recovered his reason, he fell into another fit; and his friends told the Abbé, he was more affected with that distemper when it thundered than at any other time; and if it ever happened, which it rarely did, that he then escaped; his eyes, his countenance, and the confusion of his expressions, sufficiently demonstrated the weakness of his reason.

THE next day, the Abbé learned from the person himself, that the fear of thunder was not the cause of his disease; but that, however, he found a fatal connection between that phenomenon and his distemper. He added, that when the fit seized him, he perceived a vapour rising in his breast, with so much rapidity, that he lost all his senses before he could call for help. *

* Phil. Transf. Vol. 48. pt. 1. p. 357.

MR. WILSON cured a woman of a deafness of seventeen years standing. He also observes, that she had a very great cold when she began to be electrified; but that the inflammation ceased the first time, and the cold was quite gone when the operation had been performed again the second day. But he acknowledges, that he had tried the same experiment upon six other deaf persons without any success. *

THE same person observes that one gentleman, near seventy years old, could never be made to receive a shock except in his wrists. He says that he himself could once have borne very great shocks without inconvenience, but that he could not bear them at the time that he wrote.

MEDICAL electricity is very much obliged to the labours and observations of Mr. Lovet, Lay-clerk of the cathedral church at Worcester, who has for many years been indefatigable in the application of electricity to a great variety of diseases. His success has been very considerable, and all the cases he has published seem to be well authenticated.

ACCORDING to Mr. Lovet, electricity is almost a specific in all cases of violent pains, of however long continuance, in every part of the body; as in obstinate head-aches, the sciatica, the cramp, and disorders resembling the gout. He had no trials of the proper gout, but only on those who were slightly attacked, and who received immediate relief.

THE tooth ach, he says, is generally cured instantly, and he scarce ever remembered any one who complained of its raging a minute after the operation. †

IT has seldom failed, he says, to cure rigidities, or a wasting of the muscles, and hysterical disorders, particularly if they be

* Wilson's Essay, p. 207. † Lovet's Essay, p. 112.

attended with coldness in the feet. According to him, it cures inflammations, it has stopped a mortification, cured a fistula lachrymalis, and dispersed extravasated blood.* He also says it has been of excellent use in bringing to a suppuration, or in dispersing without suppuration, obstinate swellings of various kinds, even those that were scrophulous. In his hands it cured the falling sickness, and fits of various kind; though the patients had been subject to them for many years, and one cure he mentions of a hemiplegia.† Lastly, he relates a well attested case, from Mr. Floyer, surgeon at Dorchester, of a complete cure of what seemed to be a *gutta serena*. The same Mr. Floyer, he also says, cured with it two young women of obstructions, one of whom had taken medicines a year to no purpose.‡

In the rheumatism, Mr. Lovet candidly confesses, it has failed; but says it was seldom in the case of young persons, if they were taken in time.

THE manner in which electricity operated in these cures, Mr. Lovet imagined to be, by removing secret obstructions; which are probably the cause of those disorders. In all his practice he never knew an instance of harm being done by it, and thinks that, in all the cases in which it has done harm, the manner of administering it has been injudicious. In general, he thinks the shocks have been made too great. This he imagined to have been the case of the patient before mentioned of Dr. Hart, who was made more paralytic by electric shocks. Mr. Lovet advises to begin, in general, with simple electrification, especially in hysterical cases; then to proceed to taking sparks, and lastly to giving moderate shocks, but hardly ever any that are violent, or painful.

* Ib. p. 76.

† Ib. p. 101.

‡ Ib. p. 119.

THE account of the application of electricity by Dr. Zetzel of Upsal, which may be seen in Mr. Lovet's treatise, agrees in the main with the result of his own practice; and where there is any difference between them, Mr. Lovet thinks there are evident marks of unfairness in the Swedish account. And a subsequent account from Sweden mentions several cures being made in those very cases, in which Dr. Zetzel says that no relief was to be had from electricity.

THE Rev. Mr. J. Wesley has followed Mr. Lovet in the same useful course of medical electricity; and recommends the use of it to his numerous followers, and to all people. Happy it is when an ascendancy over the minds of men is employed to purposes favourable to the increase of knowledge, and to the best interests of mankind. Mr. Wesley's account of cures performed by electricity agrees very well with that of Mr. Lovet, whom he often quotes. He adds, that he has scarce ever known an instance in which shocks all over the body have failed to cure a quotidian or tertian ague.* He mentions cases of blindness cured or relieved by it; and says that he has known hearing given by it to a man that was born deaf.† He mentions cures in cases of bruises, running sores, the dropsy, gravel in the kidneys (causing the patient to part with it) a palsy in the tongue, and lastly in the genuine consumption. But Mr. Boisser says it is of disservice in phthical complaints. ‡

MR. WESLEY candidly says, he has not known any instance of the cure of an hemiplegia; and though many paralytics have been helped by electricity, he scarcely thinks that any palsy of a year's standing has been thoroughly cured by it. He asserts, however, that he has never yet known any person, man, woman,

* Wesley's Desideratum, p. 3. † Ib. p. 48. ‡ Carmichael Tentamen inaugurale medicum de Paralyfi, p. 34. ex Act. Ups.

or child, sick or well, who has found (what Mr. Wilson says, that some persons complained of) an unusual pain some days after the shock. Mr. Wesley had only known that the rheumatic pains, which were afterwards perfectly cured, had increased on the first or second application. *

MR. WESLEY directs the same method of administration with Mr. Lovet. In deep hysterical cases, he advises that the patients be simply electrified, sitting on cakes of rosin, at least half an hour morning and evening; when, after some time, small sparks may be taken from them, and afterwards shocks given to them, more or less strong, as their disorder requires; which, he says, has seldom failed of the desired effect. †

THIS account of the medical use of electricity by Mr. Lovet and Mr. Wesley is certainly liable to an objection, which will always lie against the accounts of those persons who, not being of the faculty, cannot be supposed capable of distinguishing with accuracy either the nature of the disorders, or the consequences of a seeming cure. But, on the other hand, this very circumstance of their ignorance of the nature of disorders, and consequently of the best method of applying electricity to them, supplies the strongest argument in favour of its innocence, at least. If in such unskilful hands it has produced so much good, and so little harm; how much more good, and how much less harm would it probably have produced in more skilful hands.

BUT whatever weight there be in this objection against the last mentioned writers, it certainly cannot be urged against Antonius de Haen, one of the most eminent physicians of the present age; who, after six years uninterrupted use of it, reckons it among the most valuable assistances of the medical art; and

* Wesley's Desideratum, p. 50.

† *Ib.* p. 56.

expressly says, that though it has often been applied in vain, it has often afforded relief where no other application would have been effectual. But I shall recite in a summary manner from his *Ratio Medendi*, the result of all his observations on this subject.

With respect to partial palsies, in particular, he says, it never did the least harm; that one or two persons who had received no benefit from it in six intire months, were yet much relieved by persevering in the use of it. That some persons discontinuing it, after having received some benefit, relapsed again; but afterwards, by recurring to the use of electricity, recovered, though more slowly than before. Some persons, he says, were relieved who had been paralytic one, three, six, nine, and twelve years, and some longer; but that in one or two of these cases, the patients had received less relief, and more slowly than was usual in recent cases. In some cases, he says, a most unexpected benefit had been found by those who had been paralytic in their tongues, eyes, fingers, and other particular limbs. A paralysis and trembling of the limbs, from whatever cause it arose, he says, never failed to be relieved by it; and he relates one instance of a perfect cure being performed in a remarkable case of this nature, after receiving ten shocks.*

DE HAEN's custom was to apply the operation for half an hour together at least. He seems to have used gentle shocks, and he joined to electricity, the use of other remedies, which, however, would not have been effectual without it. †

ST. VITUS's dance, he says, never failed to be cured by electricity. ‡ He always observed it to promote a more copious discharge of the menses, and to relieve in cases of obstruction;

* *Ratio Medendi*, Vol. i. p. 234, 199. † *Ib.* p. 233. ‡ *Ib.* p. 389.

but,

but, for this reason, he advises that it be not administered to women with child. He found it of use in some cases of deafness, but it entirely failed in its application to a gutta serena, and to a strumous neck.*

LASTLY, he relates a remarkable case, communicated to him by Mr. Velfe at the Hague, of the cure of a mucous apoplexy.† To the cases which have been mentioned occasionally, in which harm may be apprehended from electrification, may perhaps be added the venereal disease in which Mr. Veratti advises, that electrification be by all means avoided.‡

I SHALL conclude this account of medical electricity with observing that there are two general effects of electricity on the human body, of which, it should seem, that physicians might greatly avail themselves. These are, that it promotes insensible perspiration, and glandular secretion. The former is effected by simple electrification, and the latter by taking sparks from the glands, or the parts contiguous to them; on which it acts like a stimulus. Of the former, instances have been given in the experiments of the Abbé Nollet, and a few have been given occasionally of the latter.

To these I shall now add, that Linnæus observed, that when electric sparks have been drawn from the ear, it has instantly promoted a more copious secretion of ear-wax; and that it has also been observed, that, when the eye, or the parts about the eye, have been electrified, the tears have flowed copiously. But the most remarkable case that I have met with, is, of its promoting the secretion of that matter which forms the hair; whereby hair has been actually restored to a part that had long been bald. ||

* Ib. Vol. 2. p. 200.

† Ib.

‡ Carmichael Tentamen, p. 34.

|| Ib. p. 33.

HITHERTO electricity has been generally applied to the human body either in the method of drawing sparks, as it is called, or of giving shocks. But these operations are both violent; and though the strong concussion may suit some cases, it may be of disservice in others, where a moderate simple electrification might have been of service.

THE great objection to this method is the tediousness and expence of the application. But an electrical machine might be contrived to go by wind or water, and a convenient room might be annexed to it; in which a floor might be raised upon electrics, and a person might sit down, read, sleep, or even walk about during the electrification. It were to be wished, that some physician of understanding and spirit would provide himself with such a machine and room. No harm could possibly be apprehended from electricity, applied in this gentle and insensible manner, and good effects are, at least, possible, if not highly probable. It would certainly be more for the honour of the faculty, that the practice should be introduced in this manner, than that it be left to some rich Valetudinarian, who may take it into his head, that such an operation may be of service to him.

SECTION

SECTION XV.

MISCELLANEOUS EXPERIMENTS AND DISCOVERIES MADE
WITHIN THIS PERIOD.

HAVING distributed into distinct sections all the subjects, under which I had collected materials enow to form a separate account; I have reserved for the last place, those smaller articles, which could neither with propriety be introduced under the former heads, nor were large enough to make a section themselves.

It has been a great controversy among electricians, whether glass be permeable to the electric fluid. Mr. Wilson appeared in favour of the permeability, and, in a paper read at the Royal Society December the 6th. 1759, produced the following experiments to support his opinion; notwithstanding he, even afterwards, acknowledged, in a paper read at the Royal Society November the 13th. 1760; that, in the Leyden experiment, Dr. Franklin had proved that the fluid did not go through the glass.*

He took a very large pane of glass, a little warmed; and holding it upright by one edge, while the opposite edge rested

* Phil. Trans. Vol. 51. pt. 2. p. 896.

upon

upon wax, he rubbed the middle part of the surface with his finger, and found both sides electrified *plus*. *

UPON this I cannot help observing that it ought to be so on Dr. Franklin's principles. If one side be rubbed by the finger, it acquires from the finger some of the electric fluid. This, being spread on the glass as far as the rubbing extended, repels an equal quantity of that contained in the other side of the glass, and drives it out on that side; where it stands as an atmosphere, so that both sides are found *plus*. If the unrubbed side were in contact with a conductor communicating with the earth, the electric fluid would be carried away, and then that side would be left *apparently* in the natural state. If the electric fluid found on the unrubbed side was really part of that which had been communicated by and from the finger, and so had actually *permeated* the glass, it might, when conducted away, be continually replaced by fresh permeating fluid communicated in the same manner: But if the effect is continually diminishing, while the supposed cause, repeated, continues the same, there seems reason to doubt the supposed relation between that cause and the effect. For it appears difficult to conceive how some electric fluid, having passed through a permeable body, should make it more difficult for other particles of the same electric fluid to follow, till, at length, none could pass at all.

MR. WILSON also says, that, holding the same pane of glass within two feet of the prime conductor, which was electrified *plus*, that part of the glass which was opposite to the conductor became electrified *minus* on both sides; but, in a few minutes, the *minus* electricity disappeared, and the *plus* continuing, diffused itself into the place of the other, so that now the whole was electrified *plus*.

* Phil. Transf. Vol. 51, pt. 1, p. 314.

THE experiment so far succeeding, induced him to make use of a less piece of glass, that the whole might be electrified *minus*. These advances, he says, led him to observe the power of electrifying that small piece of glass at different distances.

HE exposed the same small piece of glass to the prime conductor, at the distance of two feet, and observed a *minus* electricity at both surfaces.

As he moved the glass nearer, to a certain distance, it was more sensibly electrified *minus*; and after that, on moving it still nearer, the *minus* appearance was less and less sensible; till it came within the distance of about one inch, and then it was electrified *plus* on both sides.

THIS *plus* electricity in the glass, he found, might be changed to a *minus* again, by removing the glass, and holding it for a time at a greater distance; which he thought to be a proof of the repulsive power of that fluid. *

HAVING by him a pane of glass, one side of which was rough and the other smooth, he rubbed it slightly on one side; upon doing which, both sides were electrified *minus*.

ON this I must also take the liberty to observe that, as the electric fluid contained in glass in its natural state, is kept equal in both sides by the common repulsion; if the quantity in one side is diminished, the fluid in the other side, being less repelled, retires inward, and leaves that surface also *minus*.

SLIGHT changes, *plus* or *minus*, may be made in either surface, that have not strength to act on the other side, by repulsion, or by abating repulsion, through the glass; and so *plus* electricity may be given to one surface, and *minus* to the other in some degree. Both sides may also be made *plus*, and both *minus*, by

* Ib. Vol. 51. pt. 1. p. 128.

rubbing,

rubbing, or by communication, without any necessity of supposing the glass permeable.

AND yet it is probable that some glass, from having a greater mixture of non-electric matters in its composition, may be permeable, when cold, in some small degree, as all glass is found to be when warmed.

MR. WILSON treated the other side of this pane of glass in the same manner, after which the *minus* electricity was changed into a *plus* on both sides.

THOUGH Dr. Franklin was of opinion, that glass when cold is not permeable to electricity, he had made no experiments upon it when hot; but Mr. Kinnersley, a friend of his, made one, which seemed to prove, that it was very differently affected in this respect, in the different states of hot and cold. He found, that a coated Florence flask (made of very thin glass, and full of air bubbles) containing boiling water, could not be electrified. The electricity, he says, passed as readily through it as through metal. The charge of a three pint bottle went freely through it, without injuring the flask in the least. When it became cold, he could charge it as before. This effect he attributed to the dilatation of the pores of glass by heat. *

ALL Mr. Wilson's experiments to prove the permeability of glass were repeated by Mr. Bergman of Upsal; and, as he says, with success. †

ÆPINUS, however, was by no means satisfied with Mr. Wilson's experiments concerning the permeability of glass; and yet he brings no other fact in answer to his arguments, but a very common one, which shows that a glass tube both receives and loses its electricity very slowly; so that he only asserts a *difficulty*,

* Phil. Trans. Vol. 53. pt. 1. p. 85.

† Ib. Vol. 52. pt. 2. p. 485.

and

and a *slowness* in the electric fluid passing through electric substances, as was mentioned before; and consequently Mr. Wilfon seems to have an advantage in the controversy: for, as he says, passing through, though ever so slowly, is a real passing through. *

ÆPINUS has shown, by a curious experiment, that if a metallic conductor and a cork ball be both electrified positively, so as to repel one another; yet, that, if the ball be forcibly brought within two, three, or four lines of the conductor, it will be attracted by it; and be repelled again, if it be forcibly pushed beyond that limit of attraction. If the ball be confined to move within the same small distance, a moderate electrification of the conductor will repel the ball to its utmost limit; but a stronger electrification of the conductor will cause it to be attracted. He, therefore, limits the general maxim, that bodies possessing the same kind of electricity repel one another; and asserts, that this will be the case, only when the quantity of electric fluid belonging to them both, as one body, is greater, or less than that which is natural to them. † This experiment deserves particular attention.

SIGNIOR BECCARIA, who has contributed so largely to several of the sections in this period, furnishes a few articles which well deserve a place in this.

He thought it was evident, that the electric fluid tended to move in a right line, because a longer spark may be taken in a direct line, from the end of a long conductor, than can be taken from the same place in any other direction. But he thought it

* Phil. Trans. Vol. 53. p. 443.

† Æpini Testamon, p. 146.

was still more evident, from observing, both in the air, and in vacuo; that, presenting the finger, or a brass ball, at a proper distance, and in a certain angle with the conductor (which experience will soon find) the electric spark will make an exact curve, to which the conductor produced will be a tangent: as if the electric matter was acted upon by two different forces, one its own acquired velocity, urging it forward in a right line; the other the attraction of the body presented to it, which throws it out of the right line.*

IN his observations on pointed bodies, he says, that two pointed bodies, equally sharp, in their approach to an electrified conductor, will appear luminous only at half the distance at which one of them would have done. †

THE same ingenious philosopher reports a curious, but cruel experiment which he made on a live cock. He detached the belly of one of the muscles from the thigh of the animal, leaving the extremities in their proper insertions, and then discharged a shock through it. At the instant of the stroke, the leg was violently distended, and the muscle greatly inflated; the motion beginning at the tendon, and the extension of it resembling the opening of a lady's fan. No pricking with a pin could make it act so strongly. ‡

I MUST not omit to mention, in this chapter of miscellaneous experiments, what the Dutch writers have reported concerning the *Gymnotus*, a fish peculiar to Surinam, which very much resembles what naturalists relate concerning the *Torpedo*. Mr. Muschenbroeck says, the *gymnotus* is possessed of a kind of natural electricity, but different from the common electricity, in that persons who touch it in water are shocked, and stunned by

* *Elettricità artificiale*, p. 56. † *Ib.* p. 67. ‡ *Lettere dell' elettricismo*, p. 129-
it,

it, so as to be in danger of drowning. The fish has been taken, and put into a vessel; when experiments were made upon it at leisure; and it was found, that it might be touched with all safety with a stick of sealing wax; but if it was touched with the naked finger, or with a piece of metal, and especially a gold ring, held in the fingers, the arm was shocked as high as the elbow. If it was touched with the foot, the sensation reached as high as the knee, and the pain was as great, as if the part had been struck with something hard. This kind of electricity is the same by night or by day, when the wind is in every direction, when the fish was put in vessels of any materials, and whether it was in water or out of water. Every part of the body of the fish is capable of giving this shock, but more especially the tail. The sensation is the strongest when the fish is in motion, and it is transmitted to a great distance; so that if persons in a ship happen to dip their fingers or feet in the sea, when the fish is swimming at the distance of fifteen feet from them they are affected by it. Other fishes, put into the same vessel with it, presently die; but it is itself killed by the lobster. The gymnotus is found in the upper part of the river of Surinam, particularly the rocky part of it. It feeds upon all kinds of fish, and will even eat bread. This author proposes as a query whether the sensation communicated by the torpedo does not depend upon a similar electricity; since Monsieur Reaumer says, that when it is touched, the hand, arm, and shoulder are seized with a sudden stupor, which lasts for some time, and is unlike any other sensation.*

This gymnotus, I suppose, is a different fish from the *Anguil-*

* Muschenbroeck's *Introd. ad philosoph. naturalem*, No. 901—909.

le tremblante, the trembling eel, which is also a native of Surinam, and lives in marshy places, from whence it cannot be drawn, except when it is intoxicated. It cannot be touched with the hand, or with a stick, without feeling a terrible stun, which reaches as high as the shoulder. If it be trod upon with shoes, the legs and thighs are affected in a similar manner. Fourteen persons joining hands, and the first of them touching with a stick, they were all shocked violently. It is conjectured that this power of giving a shock resides in two muscles, which are particularly prominent and conspicuous. *

It is to be regretted, that none of the persons who have made experiments on these fishes should have endeavoured to ascertain whether they were capable of exhibiting the phenomena of attraction and repulsion, or the appearance of electric light, as experiments of this kind are of principal consequence, and must have been very easy to make.

MR. HAMILTON, professor of Philosophy at the university of Dublin, made a curious experiment with a wire five or six inches long, finely pointed at each end. To the middle of this wire he fitted a brass cap, which rested on the point of a needle communicating with the conductor. Half an inch at each extremity of this wire he bent, in opposite directions, perpendicular to the rest of the wire, and in the plane of the horizon. The consequence of electrifying this apparatus was, that the wire would turn round with very great velocity; moving, as he says, always in a direction contrary to that in which the electric fluid issues from its point, without having any conducting

* P. Fermin's Nat. Hist. of Surinam, p. 59.

substance

substance near it, except the air. He also observes, that if this wire were made to turn the contrary way, it would stop, and turn as before.*

THE same experiment was also made by Mr. Kinnerley of Boston, with this addition, that he electrified the wire negatively; and observed, to his great surprize, that it still turned the same way. This he endeavoured to account for by supposing that, in the former case, the points, having more electricity than the air, were attracted by it; in the latter case, the air, having more than the points, was attracted by them. †

IT may, by some, be thought that this pointed wire turning the same way, whether it be electrified negatively or positively, is a proof that the electric fluid issues out at the points in both cases alike, and by the reaction of the air is, together with the points, driven backwards; contrary to what ought to have been the case if the electric fluid had really issued out of the point in one case, and entered it in the other. But it will be found by experiment, that an eolipile, with its stem bent like the wire above-mentioned, and suspended on its center of gravity by a fine thread, will move in the same direction, whether it be throwing steam out at the orifice; or, after it is exhausted, and cooling, it be drawing the air, or water in.

WITH respect to the power of points, it has been observed by Mr. Villette of Liege, that a needle, concealed in a glass tube, which projected an inch beyond it, took a stronger spark from a prime conductor than a man's finger; also that when the points of needles are covered with tallow, bees wax, sulphur, &c. they take peculiarly strong sparks. He adds, that, when sulphur is

* Phil. Transf. Vol. 51. pt. 2. p. 905.

† Phil. Transf. Vol. 53. pt. 1. p. 86.

used.

used, and the sparks are taken obliquely, they are sometimes of a beautiful citron colour. *

MR. LULLIN has made an additional observation to Monsieur du Faye's, concerning the different manner in which conducting and non-conducting substances are affected, when they are exposed to the dew. He says, that if a plate of glass be extended upon silken threads, and exposed to the open air all night, and a plate of metal, less than the glass, be laid upon it, in the morning, the metal will be dry; and likewise the glass, on both sides, exactly under the metal; but that the edges of the glass, where the metal did not reach, will be wet on both sides. †

I SHALL close this section of miscellaneous articles, and the whole history of electricity, with a succinct account of some of the chief particulars in which the analogy between electricity and magnetism consists; very nearly as it was drawn up, in an abridgment of Æpinus, and communicated to me for this purpose, by Mr. Price.

1. As a rod of iron held near a magnet will have several successive poles; so will a glass tube touched by an excited tube have a succession of positive and negative parts.

2. BODIES positively and negatively electrical, when in contact, will unite to one another; as will magnets, when they are laid with their opposite poles to one another.

3. GLASS is a substance of a nature similar to hardened steel. The positive and negative sides of the former answer to the attracting and repelling ends of the latter, when magnetical.

4. As it is difficult to move the electric fluid in the pores of

* Nollet's Letters, Vol. 3. p. 212. † Ib. p. 54.

the former; so, likewise, it is difficult to move the magnetic fluid in the pores of the latter.

5. As there can be no condensation of the electric fluid in the former, without a rarefaction; so, in the latter, if there be a condensation, or positive magnetism in one end of a bar, there must be an evacuation, or negative magnetism in the other end.

6. STEEL corresponds to electrics per se, and iron in some measure, to conductors of electricity.

7. STEEL is less susceptible of the magnetic virtue, but when it has acquired it, it retains it more strongly than iron; just as electrics per se will not so easily receive the electric fluid, but when it is forced into them, will retain it more strongly than conductors.

8. ÆPINUS adds, and reckons it one of his discoveries, that an electrified body does not act on other bodies, except they are themselves electrified; just as a magnet will not act on any other substances, except they are themselves possessed of the magnetic virtue. So that an electrified body attracts and repels another body, only in consequence of rendering it first of all electrical; as a magnet attracts iron, only in consequence of, first of all, making it a magnet.

9. MR. CANTON has also found, that if the tourmalin be cut into several pieces, each piece will have a positive and negative side, just as the pieces of a broken magnet would have.

Thus far, says Mr. Price, there is an analogy, and, in some instances, a striking one, between magnetism and electricity, upon the supposition that the cause of magnetism is a fluid. But there is no magnetic substance which answers perfectly to the
conductors

conductors of electricity. There is no afflux and efflux of the magnet fluid ever visible. The equilibrium in a magnet cannot be instantaneously restored, by forming a communication between the opposite ends with iron, as it may in charged glass. Nor are there any substances positively or negatively magnetical only, as there are bodies which are positively or negatively electrical only.

PART

PART II.

A

SERIES OF PROPOSITIONS,

COMPRISING ALL THE

GENERAL PROPERTIES OF ELECTRICITY.

AFTER tracing, at large, the progress of all the discoveries relating to electricity, and giving an historical account of them, in the order in which they were made; it will probably, be no disagreeable repetition, if I give, at the close of it, a SERIES OF PROPOSITIONS, comprising all the general properties of electricity, drawn up in as succinct a manner as possible. And, notwithstanding the large detail which has been made, it will be found, that a few propositions are sufficient to comprise almost all that we know of the subject.

THIS circumstance may be regarded as a demonstration of the real progress that has been made in this science. And as this progress advances, and the history enlarges, paradoxical as the assertion may seem, this part may be expected to contract itself in the same proportion. For the more we know of any
G g g science,

science, the greater number of particular propositions are we able to resolve into general ones; and, consequently, within narrower bounds shall we be able to reduce its principles.

I MIGHT have made this part of my work much shorter, even in the present state of the science, if I would have admitted into it any thing theoretical; but I have carefully avoided the principles of any theory, even the most probable, and the nearest to being perfectly ascertained, in this series of propositions; in which I propose to comprehend only *known facts*; that my younger readers may carefully distinguish between *fact* and *theory*; things which are too often confounded.

I HAVE not, in this part of my work, descended to any *minutiae*, in the description of electrical appearances, because they have been entered into before, and a repetition of them would have been tedious. At the same time, I think it will be found, upon examination, that I have not omitted to take notice of any discovery of importance. I have also introduced into it the definition of all the most necessary *technical terms*; that this part of the work might serve as a methodical, and sufficient introduction, to those who are beginning the study of electricity; and desire a general knowledge of the first elements of the science, before they enter upon the detail of particulars, which will be best learned afterwards, from the history.*

By *electricity*, in the following propositions, I would be understood to mean, only those *effects* which will be called electrical; or else the *unknown cause* of those effects, using the term, as we use the letters (*x*) and (*y*) in Algebra.

* Lest this part of the work should not prove a sufficient *introduction to the study of electricity*, I have since published a small piece with this title. It contains a more familiar explication of the fundamental principles of electricity, mixing theory with facts, and illustrating, chiefly, those experiments which are the most entertaining.

ALL known substances are distributed by electricians into two sorts. Those of one sort are termed *electrics*, or *non-conductors*; and those of the other *non-electrics*, or *conductors of electricity*.

METALS of all kinds, together with semimetals, and water are conductors. So also is charcoal, and other substances of a similar nature, as will be shown at large in the last part of this work. All other substances, whether mineral, vegetable, or animal, are non-conductors. But many of these when they are made very hot, as glass, rosin, baked wood; and, perhaps, all the rest on which the experiment can be made in this state, are conductors of electricity.

ALL bodies, however, though in the same state of heat and cold, are not equally perfect electrics, or perfect conductors. Vegetable and animal substances, for instance, in their natural state, are seldom perfect electrics, on account of the moisture that is contained in them. And, independent of moisture, there is probably a gradation in all substances, from the most perfect conductors to the most perfect non-conductors of electricity.

IT is the property of all kinds of electrics, that when they are rubbed by bodies differing from themselves (in roughness or smoothness chiefly) to attract light bodies of all kinds which are presented to them; to exhibit an appearance of light (which is very visible in the dark) attended with a snapping noise, upon the approach of any conductor; and, if the nostrils be presented, they are affected with a smell like that of phosphorus.

An electric substance exhibiting these appearances, is said to be *excited*, and some of them, particularly the tourmalin, are excited by heating and cooling, as well as by rubbing.

IT is necessary, however, to a considerable excitation of any electric, that the substance against which it is rubbed (usually

called the *rubber*) have a communication with the earth, or bodies abounding with electricity, by means of conductors; for if the rubber be *insulated*, that is, if it be cut off from all communication with the earth by means of electrics, the friction has but little effect.

WHEN insulated bodies have been attracted by, and brought into contact with any excited electric, they begin to be repelled by it, and also to repel one another: nor will they be attracted again, till they have been in contact with some conductor communicating with the earth; but, after this, they will be attracted as at first.

IF conductors be insulated, electric powers may be *communicated* to them by the approach of excited electrics. They will then attract light bodies, and give sparks, attended with a snapping noise, like the electrics themselves. But there is this difference between excited and communicated electricity, that a conductor to which electricity has been communicated parts with its whole power at once, on the contact of a conductor communicating with the earth; whereas an excited electric, in the same circumstances, loses its electricity only partially; it being discharged only from the part which was actually touched by the conductor, or those in the neighbourhood of it; so that the spark of electric fire is not so dense, nor the explosion made by parting with it so loud, from excited, as from communicated electricity.

ELECTRIC substances, brought into contact with excited electrics, will not destroy their electricity; whence it is that they are called non-conductors, because they will not convey or *conduct* away whatever is the cause of electric appearances in bodies.

WHEN electricity is strongly communicated to insulated animal

mal bodies, the pulse is quickened, and perspiration increased; and, if they receive, or part with their electricity on a sudden, a painful sensation is felt at the place of communication.

THE growth of vegetables is quickened by electricity.

No electric can be excited without producing electric appearances in the body with which it is excited, provided that body be insulated. For this insulated rubber will attract light bodies, give sparks, and make a snapping noise upon the approach of a conductor, as well as the excited electric.

IF an insulated conductor be pointed, or if a pointed conductor communicating with the earth be held pretty near it, little or no electric appearance will be exhibited; only a light will appear at each of the points, during the act of excitation, and a current of air will be sensible from off them both.

THESE two electricities, viz. that of the electric itself, and that of the rubber, though similar to, are the reverse of one another. A body attracted by the one will be repelled by the other, and they will attract, and in all respects act upon one another more sensibly than upon other bodies; so that two pieces of glass or silk, possessed of contrary electricities, will cohere firmly together, and require a considerable force to separate them.

THESE two electricities, having been first discovered by producing one of them from glass, and the other from amber, sealing wax, sulphur, rosin, &c. first obtained the names of *vitreous* and *resinous electricity*; and it being afterwards imagined that one of them was a redundancy, and the other a deficiency of a supposed electric fluid, the former (viz. that which is produced from the friction of smooth glass tubes or globes by the human hand, or a common leathern rubber) obtained the name of *positive*; and the latter (viz. that which is produced from the friction of sticks or globes of sulphur, &c. or collected from the rubber of a glass globe

globe above-mentioned) that of *negative* electricity: and these terms are now principally in use.

If a conductor, not insulated, be brought within the *atmosphere*, that is, the sphere of action, of any electrified body, it acquires the electricity opposite to that of the electrified body; and the nearer it is brought, the stronger opposite electricity doth it acquire; till the one receive a spark from the other, and then the electricity of both will be discharged.

THE electric substance which separates the two conductors, possessing these two opposite kinds of electricity, is said to be *charged*. Plates of glass are the most convenient for this purpose, and the thinner the plate, the greater charge it is capable of holding. The conductors contiguous to each side of the glass are called their *coating*.

AGREEABLE to the above-mentioned general principle, it is necessary, that one side of the charged glass have a communication with the rubber, while the other receives the electricity from the conductor; or with the conductor, while the other receives from the rubber.

IT follows also, that the two sides of the plate thus charged are always possessed of the two opposite electricities; that side which communicates with the excited electric having the electricity of the electric, and that which communicates with the rubber, that of the rubber.

THERE is, consequently, a very eager attraction between these two electricities with which the different sides of the plate are charged; and, when a proper communication is made by means of conductors, a flash of electric light, attended with a report (which is greater or less in proportion to the quantity of electricity communicated to them, and the goodness of the conductors)

conductors) is perceived between them, and the electricity of both sides is thereby discharged.

THE substance of the glass itself, in or upon which these electricities exist, is impervious to electricity, and does not permit them to unite; but if they be very strong, and the plate of glass very thin, they will force a passage through the glass. This, however, always breaks the glass, and renders it incapable of another charge.

THE flash of light, together with the explosion between the two opposite sides of a charged electric, is generally called the *electric shock*, on account of the disagreeable sensation it gives any animal, whose body is made use of to form the communication between them.

THIS electric shock is always found to perform the circuit from one side of the charged glass to the other by the shortest passage, through the best conductors. Common communicated electricity also observes the same rule, in its transmission from one body to another.

IT has not been found that the electric shock, takes up the least sensible space of time in being transmitted to the greatest distances.

THE electric shock, as also the common electric spark, displaces the air through which it passes; and if its passage from conductor to conductor be interrupted by non-conductors of a moderate thickness, it will rend and tear them in its passage; and in such a manner as to exhibit the appearance of a sudden expansion of the air about the center of the shock.

IF a strong shock be sent through a small animal body, it will often deprive it instantly of life.

WHEN the electric shock is very strong, it will give polarity

rity to magnetic needles, and sometimes it reverses their poles.

GREAT shocks, by which animals are killed, are said to hasten putrefaction.

ELECTRICITY and lightning are, in all respects, the same thing. Every effect of lightning may be imitated by electricity, and every experiment in electricity may be made with lightning, brought down from the clouds, by means of insulated pointed rods of metal.

PART III.

THEORIES OF ELECTRICITY.

SECTION I.

OF PHILOSOPHICAL THEORIES IN GENERAL, AND THE THEORIES OF ELECTRICITY PRECEDING THAT OF DR. FRANKLIN.

ONE of the most intimate of all associations in the human mind is that of *cause* and *effect*. They suggest one another with the utmost readiness upon all occasions; so that it is almost impossible to contemplate the one, without having some idea of, or forming some conjecture about the other. In viewing the works of nature, we necessarily become first acquainted with appearances, or effects. We naturally attend to the circumstances in which such appearances always arise, and cannot help

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considering them as the causes of those appearances. Then, considering these circumstances themselves as new appearances, we are desirous of tracing out other circumstances that gave birth to them. Thus, constantly ascending in this chain of causes and effects, we are led, at last, to the first cause of all: and then we consider all secondary, and inferior causes, as nothing more than the various methods in which the supreme cause acts, in order to bring about his great designs.

IN all science, we first ascend from particular to general. For nature exhibits nothing but particulars; and all general propositions, as well as general terms, are artificial things; being contrived for the ease of our conception and memory; in order to comprehend things clearly, and to comprise as much knowledge as possible in the smallest compass. It is no wonder then that we take pleasure in this process. Besides, we actually see in nature a vast variety of effects proceeding from the same general principles, operating in different circumstances; so that judging from appearances, that nature is every where uniform with itself, we are led, by analogy, to expect the same in all cases; and think it an argument in favour of any system, if it exhibit a variety of effects springing from a few causes. For such variety in effects, and such simplicity in causes, we generally see in nature.

HAVING discovered the cause of any appearance, it is the business of philosophy to trace it in all its effects, and to predict other similar appearances from similar previous situations of things. By this means, the true philosopher, knowing what will be the result of putting every thing, which the present system exhibits, into every variety of circumstances, is master of all the powers of nature, and can apply them to all the useful purposes of life. Thus does *knowledge*, as Lord Bacon observes, become:

become *powers*; and thus is the philosopher capable of providing, in a more effectual manner, both for his own happiness and for that of others; and thereby of approving himself a good citizen, and an useful member of society.

IT is obvious, from this general view of the business of philosophy, that, in order to trace those circumstances in which any appearance in nature is certainly and invariably produced, it is chiefly useful to observe what there is *in common* in the circumstances attending similar appearances: for on those common circumstances, all that is common in the appearances must depend. And the easiest possible method, by which we can trace out the connection of causes and effects in nature, is to begin with comparing those appearances which are most similar, where the difference consists in a single circumstance; the whole effect of which, in different appearances, is thereby perfectly known. And when we have, by this means, noted the whole effect of all the separate circumstances and situations of things, we can easily judge of their effect in all possible combinations.

HENCE *analogy* is our best guide in all philosophical investigations; and all discoveries, which were not made by mere accident, have been made by the help of it. We observe a complex appearance, attended with a proportionable variety of circumstances and situations. We also see another appearance, in some respects similar, in others dissimilar; the circumstances, likewise, similar and dissimilar in the same proportion; or we purposely vary the circumstances of the former appearance, and observe what difference it occasions. But, unless there be a very great analogy, or similarity, between them, so that the influence of a single circumstance, or of a few circumstances, can be traced separately, no probable judgment can be formed of their real operation.

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BUT in all this process, a man who acts from design, and not absolutely at random, would never think of trying the influence of any circumstance in an appearance, unless, from some other analogies in nature, more or less perfect, he had formed some idea what its influence would probably be; at least, he must, from analogies in nature previously observed, have formed an idea of several possible consequences, and try which of them will really follow. That is, in other words, every experiment, in which there is any design, is made to ascertain some *hypothesis*. For an hypothesis is nothing more than a preconceived idea of an event, as supposed to arise from certain circumstances, which must have been imagined to have produced the same, or a similar effect, upon other occasions. An hypothesis absolutely verified ceases to be termed such, and is considered as a fact; though, when it has long been in an hypothetical state, it may continue to be called, occasionally, by the same name.

THE only danger in the use of hypotheses arises from making this transition too soon. And when an hypothesis is no longer considered as a mere probable supposition, but a real fact; a philosopher not only acquiesces in it, and thereby mistakes the cause of one particular appearance; but, by its analogies, he mistakes the cause of other appearances too, and is led into a whole system of error. A philosopher who has been long attached to a favourite hypothesis, and especially if he have distinguished himself by his ingenuity in discovering or pursuing it, will not, sometimes, be convinced of its falsity by the plainest evidence of fact. Thus both himself, and all his followers, are put upon false pursuits, and seem determined to warp the whole course of nature, to suit their manner of conceiving of its operations.

BUT, provided philosophers can be upon their guard against this species of vanity (which must be owned to be very tempting)

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and against the obstinacy which is the consequence of it; hypotheses, and even a great variety of them, are certainly very promising circumstances to philosophical discoveries. Hypotheses, while they are considered merely as such, lead persons to try a variety of experiments, in order to ascertain them. In these experiments, new facts generally arise. These new facts serve to correct the hypothesis which gave occasion to them. The theory, thus corrected, serves to discover more new facts; which, as before, bring the theory still nearer to the truth. In this progressive state, or method of approximation, things continue; till, by degrees, we may hope that we shall have discovered all the facts, and have formed a perfect theory of them. By this perfect theory, I mean a system of propositions, accurately defining all the circumstances of every appearance, the separate effect of each circumstance, and the manner of its operation.

I HAVE dwelt so long upon this subject, because I apprehend, that electricians have generally been too much attached to their several theories, so as to have retarded the progress of real discoveries. Indeed, no other part of the whole compass of philosophy affords so fine a scene for ingenious speculation. Here the imagination may have full play, in conceiving of the manner in which an invisible agent produces an almost infinite variety of visible effects. As the agent is invisible, every philosopher is at liberty to make it whatever he pleases, and ascribe to it such properties and powers as are most convenient for his purpose. And, indeed, if he can frame his theory so as really to suit all the facts, it has all the evidence of truth that the nature of things can admit.

WITH the first electricians, electrical attraction was performed by means of unctuous effluvia emitted by the excited electric. These were supposed to fasten upon all bodies in their way, and

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to carry back with them all that were not too heavy. For, in that age of philosophy, all effluvia were supposed to return to the bodies from which they were emitted; since no person could, otherwise, account for the substance not being sensibly wasted by the constant emission. When these light bodies, on which the unctuous effluvia had fastened, were arrived at the excited electric, a fresh emission of the effluvia was supposed to carry them back again. But this effect of the effluvia was not thought of, till electrical repulsion had been sufficiently observed.

WHEN the Newtonian philosophy had made some progress, and the extreme subtilty of light, and other effluvia of bodies, was demonstrated; so that philosophers were under no apprehension of bodies being wasted by continual emission, the doctrine of *the return of the effluvia* was universally given up, as no longer necessary; and they were obliged to acquiesce in the unknown principles of attraction and repulsion, as supposed to be properties of certain bodies, communicated to them by the Divine Being, the mechanical cause of which they scarce attempted to explain.

WHEN Mr. Du Faye discovered the two opposite species of electricity, which he termed the *vitreous* and *resinous* electricity, he necessarily formed the idea of *two distinct electric fluids*, repulsive with respect to themselves, and attractive of one another. But he had no idea of both species being actually concerned in every electrical operation, and that glass or rosin alone always produced them both. This theory, therefore, was as simple in its application as the other.

WHILE nothing more was known of electricity but attraction and repulsion, this general theory was sufficient. The general attraction of all bodies to all bodies was called (and by some absurdly

hardly enough supposed to be accounted for by) gravitation, and many superficial philosophers thought they had given a very good account of electricity, cohesion, and magnetism, by calling them particular species of attraction peculiar to certain bodies.

BUT when electricity began to show itself in a greater variety of appearances, and to make itself sensible to the smell, the sight, the touch, and the hearing: when bodies were not only attracted and repelled, but made to emit strong sparks of fire, attended with a considerable noise, a painful sensation, and a strong phosphoreal smell; electricians were obliged to make their systems more complex, in proportion as the facts were so. It was then generally supposed, that the matter of the electric fluid was the same with the chymical principle of fire; though some thought it was a fluid *sui generis*, which very much resembled that of fire; and others, with Mr. Boulanger at their head, thought that the electric fluid was nothing more than the finer parts of the atmosphere, which crowded upon the surfaces of electric bodies, when the grosser parts had been driven away by the friction of the rubber.

THE great difficulty common to all these theories was to ascertain the direction of the electric matter. It is no wonder that, when electrical appearances were first observed, all electric powers were supposed to reside in, and therefore to proceed from the excited electric. Consequently, the electric spark was first imagined to be darted from the electrified body towards any conductor that was presented to it. It was never imagined there could be any difference in this respect whether it was amber, glass, sealing wax, or any thing else that was excited. Nothing was thought to be more evident to the senses than this progress of the electric matter: What then must have been the astonishment of all electricians, when they first observed electric appearances

ances at an insulated rubber; at the same time that it was demonstrated, that the action of the rubber did not produce, but only collect the electric fluid.

IN this case, the current could not have been supposed to flow both from the conductor and the rubber; and yet the first appearances were the same. To provide a supply of the electric matter, they were obliged to suppose that, notwithstanding appearances were nearly the same, the electric fluid was really received by the electrified body in the one case, and emitted by it in the other. But now, being obliged to give up the argument for the manner of its progress from the evidence of sight, they were at a loss whether, in the usual method of electrifying by excited glass, the fluid proceeded from the rubber to the conductor, or from the conductor to the rubber; and nothing was found to obviate these difficulties, till an excellent theory of positive and negative electricity was suggested by Dr. Watson, and digested and illustrated by Dr. Franklin.

It was soon found, that the electricity, at the rubber was the reverse of that at the conductor, and in all respects the same with that which had before been produced by the friction of sealing wax, sulphur, resin, &c. Seeing, therefore, that both the electricities, as they had heretofore been called, were produced at the same time, by one and the same electric, and by the same friction, all electricians and among the rest Mr. Du Faye himself concluded, that they were both modifications of one and the same fluid; and the old doctrine of the different electricities was universally discarded.

THE accidental discovery of the Leyden phial most clearly demonstrated the imperfection of all the theories preceding that of positive and negative electricity, by exhibiting an astonishing appearance, which no electricians, with the help of any theory, could

could have foreseen, and of which they could have formed no idea, *a priori*.

UPON this great event, new theories of electricity multiplied apace, so that it would be to no purpose to enumerate them all. Indeed many of them were no more than the beings of a day. For no sooner were they started, but the authors themselves, upon the appearance of some new fact, saw reason to new model, or entirely reject them. I shall, therefore, content myself with giving the outlines of some of the principal theories of electricity, which have their adherents at present, without considering whether they took their rise before or after this discovery.

WITH some, and particularly Mr. Wilson, the chief agent in all electrical operations is Sir Isaac Newton's ether; which is more or less dense in all bodies in proportion to the smallness of their pores, except that it is much denser in sulphureous and unctuous bodies. * To this ether are ascribed the principal phenomena of attraction and repulsion, whereas the light, the smell, and other sensible qualities of the electric fluid are referred to the grosser particles of bodies, driven from them by the forcible action of this ether. Many phenomena in electricity are also attempted to be explained by means of a subtile medium, at the surface of all bodies, which is the cause of the refraction and reflection of the rays of light, and also resists the entrance and exit of this ether. † This medium, he says, extends to a small distance from the body, and is of the same nature with what is called the electric fluid. On the surface of conductors this medium is rare, and easily admits the passage of the electric fluid; whereas on the surface of electrics it is dense, and resists it. This medium is rarified by heat, which converts non-conduc-

* Wilson's Dissertation, p. 5. † Hoadley and Wilson, p. 55.

tors into conductors.* On this theory I shall make no particular remarks, because I cannot say that I clearly comprehend it.

BUT the far greater number of philosophers suppose, and with the greatest probability, that there is a fluid *sui generis* principally concerned in the business of electricity. They seem, however, though perhaps without reason, entirely to overlook Sir Isaac Newton's ether; or if they do not suppose it to be wholly unconcerned, they allow it only a secondary and subordinate part to act in this drama. And among those who suppose a fluid *sui generis*, there is a great diversity of opinions about the mode of its existence, and the manner of its operation.

THE ingenious Abbé Nollet whose theory has been more the subject of debate than all the other theories before Dr. Franklin's, supposes that, in all electrical operations, the fluid is thrown into two opposite motions; that the *effluence* of this matter drives all light bodies before it, by impulse, upon the electrified body, and its *effluence* carries them back again. But he seems very much embarrassed in accounting for facts where both these currents must be considered, at the same time that he is obliged to find expedients to prevent their impeding the effects of each other. To obviate this great difficulty, he supposes, that every excited electric, and likewise every body to which electricity is communicated, has two orders of pores, one for the emission of the effluvia, and the other for the reception of them. A man of less ingenuity than the Abbé could not have maintained himself in such a theory as this; but, with his fund of invention, he was never at a loss for resources upon all emergencies, and in his last publication appears to be as zealous for this strange hypothesis as at the first.

* Hoadley and Wilson, p. 78.

HE more than once requested a deputation of the members of the Academy of Sciences, to be witnesses of some experiments, in which, he thought, there was a visible effluence of the electrical effluvia from the conductor, both to the globe at one of its extremities, and to any non-electric presented to it at the other; and their testimony was signed and registered in proper form.* But it does not seem to the honour of Mr. Nollet, or those gentlemen of the Academy, to be so very positive in a matter which does not admit of the evidence of sense.

THE Abbé's confidence upon this subject is very remarkable. These effects, says he, well considered, and reviewed a thousand times, in the course of thirty years, in which I have applied to electricity, make me say with confidence, that those pencils of rays are currents of electric matter, which fly from the conductor towards the excited globe. This is so evident, that I would freely appeal to the ocular testimony of any unprejudiced person, who should see the experiments which I have recited. But, says he, the fact in question is contrary to a system of electricity, which some persons persist in maintaining. They have the assurance to tell me, that the matter of the luminous pencil, in my experiment, moves in a direction quite opposite to that which I suppose; that it proceeds from the excited globe, and is from thence thrown upon any non-electric within its reach.† In another place, he says, that the principle of simultaneous effluences and affluences is by no means a *system*, but a *fact* well proved.‡

THE Abbé Nollet proposes an hypothesis to explain the difference between common electricity and the electric shock. All the effects of common electricity, he says, plainly show, that the electric matter is animated with a progressive motion, which

* *Leçons de Physique*, p. 368, 395.

† *Ib* p. 363.

‡ *Lettres sur l'électricité*, p. 98.

really carries it forwards ; whereas the remarkable case of the electric shock appears to be an instantaneous percussive, which the contiguous parts of the same matter communicate to one another, without being displaced. Sound and wind, he says, are motions of the air ; but would a philosopher, be permitted to take the one for the other, in measuring their velocity or extent. But this comparison is by no means just. *

It must be acknowledged, that far the greater part of the Abbé Nollet's arguments in favour of his doctrine of effluences and affluences are very unsatisfactory, and that his method of accounting for electrical attraction and repulsion, with other phenomena in electricity, by means of it, is more ingenious than solid. It is a great pity that this truly excellent philosopher had not spent more time in diversifying facts, and less in refining upon theory. But it is in some measure the natural fault of a disposition to philosophize.

Mr. Du TOUR improves upon this hypothesis of the Abbé Nollet, by supposing that there is a difference between the affluent and effluent current ; and that the particles of the fluid are thrown into vibrations of different qualities, which makes one of these currents more copious than the other, according as sulphur or glass is used. Difficult as it is to form any idea of this hypothesis, the author appears very much attached to it, and has no doubt of its accounting for all electrical appearances.

Leçons de Physique, p. 293.

SECTION

SECTION II.

THE THEORY OF POSITIVE AND NEGATIVE ELECTRICITY.

THE English philosophers, and perhaps the greater part of foreigners too, have now generally adopted the theory of *positive* and *negative* electricity. As this theory has been extended to almost all the phenomena, and is the most probable of any that have been hitherto proposed to the world. I shall give a pretty full account of it, and show how it agrees with all the propositions of the last part, to which it has hitherto been applied.

THIS theory generally goes by the name of Dr. Franklin, and there is no doubt of his right to it; but justice requires that I distinctly mention the equal, and, perhaps, prior claim of Dr. Watfon, to whom I have before said it had occurred. Dr. Watfon showed a series of experiments to confirm the doctrine of *plus* and *minus* electricity to Martin Folkes Esq. then president, and to a great number of fellows of the Royal Society, so early as the beginning of the year 1747, before it was known in England that Dr. Franklin had discovered the same thing in America. See the Philosophical Transactions, Vol. 44, p. 739; and Vol. 45, p.

93—101. Dr. Franklin's paper, containing the same discovery, was dated at Philadelphia, June the 1st. 1747.

ACCORDING to this theory, all the operations of electricity depend upon one fluid *sui generis*, extremely subtle and elastic, dispersed through the pores of all bodies; by which the particles of it are as strongly attracted, as they are repelled by one another.

WHEN the equilibrium of this fluid in any body is not disturbed; that is, when there is in any body neither more nor less of it than its natural share, or than that quantity which it is capable of retaining by its own attraction, it does not discover itself to our senses by any effect. The action of the rubber upon an electric disturbs this equilibrium, occasioning a deficiency of the fluid in one place, and a redundancy of it in another.

THIS equilibrium being forcibly disturbed, the mutual repulsion of the particles of the fluid is necessarily exerted to restore it. If two bodies be both of them overcharged, the electric atmospheres (to adopt the ideas of all the patrons of this hypothesis before *Æpinus*) repel each other, and both the bodies recede from one another to places where the fluid is less dense. For, as there is supposed to be a mutual attraction between all bodies and the electric fluid, electrified bodies go along with their atmospheres. If both the bodies be exhausted of their natural share of this fluid, they are both attracted by the denser fluid, existing either in the atmosphere contiguous to them, or in other neighbouring bodies; which occasions them still to recede from one another, as much as when they were overcharged.

SOME of the patrons of the hypothesis of positive and negative electricity conceive otherwise of the immediate cause of this repulsion. They say that, as the denser electric fluid, surrounding two bodies negatively electrified, acts equally on all sides of those bodies, it cannot occasion their repulsion. Is not the repulsion,
say

say they, owing rather to an accumulation of the electric fluid on the surfaces of the two bodies; which accumulation is produced by the attraction of the bodies, and the difficulty the fluid finds in entering them? This difficulty in entering is supposed to be owing, chiefly, to the *air* on the surface of bodies, which is probably a little condensed there; as may appear from Mr. Canton's experiment above-mentioned on the double barometer.

LASTLY, if one of the bodies have an overplus of the fluid, and the other a deficiency of it, the equilibrium is restored with great violence, and all electrical appearances between them are more striking.

THE influence of points in drawing or throwing off the electric fluid has not been quite satisfactorily accounted for upon any hypothesis, but it is as agreeable to this as any other. As it is evident that every electric atmosphere meets with some resistance, both in entering and quitting any body, whatever be the cause of that resistance, it is natural to suppose, that it must be least at the points of bodies where there are fewer particles of the body (on which the resistance depends) opposed to its passage, than at the flat parts of the surface, where the resisting power of a greater number of particles is united.

THE *light* which is visible in electrical appearances is generally supposed to be part of the composition of the electric fluid, which appears when it is properly agitated. But this supposition concerning electric light is not necessary to the general hypothesis. It may be supposed, upon this as well as Mr. Wilson's theory, that the light, and the phosphoreal smell, in electrical experiments arise from particles of matter much grosser than the proper electric fluid, but which may be driven from bodies by its powerful action.

THE *sound* of an electrical explosion is certainly produced by the

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the air being displaced by the electric fluid, and then suddenly collapsing, so as to occasion a vibration, which diffuses itself every way from the place where the explosion was made. For in such vibrations sound is known to consist.

BUT the chief excellence of this theory of positive and negative electricity, and that which gave it the greatest reputation, is the easy explication which it suggests of all the phenomena of the Leyden phial. This fluid is supposed to move with the greatest ease in bodies which are conductors, but with extreme difficulty in electrics per se; insomuch that glass is absolutely impermeable to it. It is moreover supposed, that all electrics (and particularly glass) on account of the smallness of their pores, do at all times contain an exceeding great, and always an equal quantity of this fluid; so that no more can be thrown into one part of any electric substance, except the same quantity go out at another, and the gain be exactly equal to the loss. These things being previously supposed, the phenomena of charging and discharging a plate of glass admit of an easy solution.

IN the usual manner of electrifying, by a smooth glass globe, all the electric matter is supplied by the rubber from all the bodies which communicate with it. If it be made to communicate with nothing but one of the coatings of a plate of glass, while the conductor communicates with the other, that side of the glass which communicates with the rubber must necessarily be exhausted, in order to supply the conductor, which must convey the whole of it to the side with which it communicates. By this operation, therefore, the electric fluid becomes almost entirely exhausted on one side of the plate, while it is as much accumulated on the other; and the discharge is made by the electric fluid rushing, as soon as an opportunity is given it, by means of proper conductors, from the side which was overloaded to that which was exhausted.

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IT is not necessary, however, to this theory, that the very same individual particles of electric matter which were thrown upon one side of the plate, should make the whole circuit of the intervening conductors, especially in very great distances, so as actually to arrive at the exhausted side. It may be sufficient to suppose, as was observed before, that the additional quantity of fluid displaces and occupies the space of an equal portion of the natural quantity of fluid belonging to those conductors in the circuit, which lay contiguous to the charged side of the glass. This displaced fluid may drive forwards an equal quantity of the same matter in the next conductors; and thus the progress may continue, till the exhausted side of the glass is supplied by the fluid naturally existing in the conductors contiguous to it. In this case the motion of the electric fluid in an explosion will rather resemble the vibration of the air in sounds, than a current of it in winds.

IT will easily be acknowledged, that while the substance of the glass is supposed to contain as much as it can possibly hold of the electric fluid, no part of it can be forced into one of the sides, without obliging an equal quantity to quit the other side; but it may be thought a difficulty upon this hypothesis, that one of the sides of a glass plate cannot be *exhausted*, without the other receiving more than its natural share, particularly as the particles of this fluid are supposed to be repulsive of one another. But it must be considered, that the attraction of the glass is sufficient to retain even the large quantity of the electric fluid which is natural to it, against all attempts to withdraw it, unless that eager attraction can be satisfied by the admission of an equal quantity from some other quarter. When this opportunity of a supply is given, by connecting one of the coatings with the rubber, and the other with the conductor, the two attempts to in-

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produce more of the fluid into one of the sides, and to take from it on the other, are made, in a manner, at the same instant. The action of the rubber tends to disturb the equilibrium of the fluid in the glass, and no sooner has a spark quitted one of the sides, to go to the rubber, but it is supplied by the conductor on the other; and the difficulty with which these additional particles move in the substance of the glass effectually prevents its reaching the opposite exhausted side; near as the two sides are to one another, and eager as is the endeavour of the fluid to go whither it is so strongly attracted.

It is not said, however, but that either side of the glass may give or receive a *small quantity* of the electric fluid, without altering the quantity on the opposite side. It is only a very considerable part of the charge that is meant, when one side is said to be filled, while the other is exhausted.

It is a little remarkable, that the electric fluid, in this, and in every other hypothesis, should so much resemble the ether of Sir Isaac Newton in some respects, and yet differ from it so essentially in others. The electric fluid is supposed to be, like ether, extremely subtle and elastic, that is, repulsive of itself; but, instead of being, like the ether, repelled by all other matter, it is strongly attracted by it; so that, far from being, like the ether, rarer in the small than in the large pores of bodies, rarer within the bodies than at their surfaces, and rarer at their surfaces than at any distance from them; it must be denser in small than in large pores, denser within the substance of bodies than at their surfaces, and denser at their surfaces than at a distance from them. But no other property can account for the extraordinary quantity of this fluid contained within the substance of electrics per se, or for the common atmospheres of all excited and electrified bodies.

To account for the attraction of light bodies, and other electrical appearances, in air of the same density with the common atmosphere, when glass (which is supposed to be impermeable to electricity) is interposed; it is conceived, that the addition or subtraction of the electric fluid, by the action of the excited electric, on one side of the glass, occasions (as in the experiment of the Leyden phial) a subtraction or addition of the fluid on the opposite side. The state of the fluid, therefore, on the opposite side being altered, all light bodies within the sphere of its action must be affected, in the very same manner as if the effluvia of the excited electric had actually penetrated the glass, according to the opinion of all electricians before Dr. Franklin.

THE manner in which clouds acquire their positive or negative electricity is not determined, according to this, or any other theory, with sufficient certainty. Mr. Canton's conjecture is, that the air resembles the tourmalin, and, consequently, acquires its electricity by heating or cooling; but whether it gains or loses the electric fluid in either state must be determined by experiment. Signior Beccaria's theory of the electricity of the clouds has been related at large.

THIS hypothesis of positive and negative electricity has been adopted, and, in some measure, rendered more systematical by Æpinus, in his elaborate treatise entitled, *Tentamen Theoriæ Electricitatis et Magnetismi*.

He has extended the property of impermeability to air and all electrics as well as glass, and defined it in a better manner. He supposes impermeability to consist in the great difficulty with which electric substances admit the electric fluid into their pores, and the slowness with which it moves in them. Moreover, in consequence of this impermeability of air to the electric fluid, he denies the reality of electric atmospheres, and thinks, as was ob-

served before, that Dr. Franklin's theory will do much better without them.

He thinks that all the particles of matter must repel one another: for that, otherwise (since all substances have in them a certain quantity of the electric fluid, the particles of which repel one another, and are attracted by all other matter) it could not happen, that bodies in their natural state, with respect to electricity, should neither attract nor repel one another.

He that reads the first chapter, as well as many other parts of his elaborate treatise above-mentioned, may save a good deal of time and trouble by considering, that the result of many of his reasonings and mathematical calculations cannot be depended upon; because he supposes the repulsion or elasticity of the electric fluid to be in proportion to its condensation; which is not true, unless the particles repel one another in the simple reciprocal ratio of their distances, as Sir Isaac Newton has demonstrated, in the second book of his Principia.

MR. WILCKE, as well as *Æpinus*, adopts all the general principles of Dr. Franklin's theory of positive and negative electricity, but thinks that no experiments which have hitherto been made show which of the electricities is positive and which negative. Supposing, however, what is called positive to be really so, and that smooth glass, for instance, rubbed upon sulphur attracts the electric fluid from it, he would account for it upon the same principles whereby water stands in drops on rough surfaces, but is diffused on smooth ones. The electric fluid, he would suppose, is more strongly attracted by the smooth surface of the glass, and therefore diffuses itself over it, while it retreats from electricities of rougher surfaces.* But this explanation, I imagine, will give little satisfaction to sceptical electricians.

* Wilcke, p. 65.

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MR. WILCKE acknowledges there is great difficulty in accounting for the repulsive power of bodies electrified negatively, and thinks that it obliges us to suppose the mutual repulsion of all homogeneous matter. Mr. Waitz, he says, was of the same opinion. According to him, therefore, bodies which have too great a proportion either of the electric fluid, or of their proper constituent matter, must aid one another. In the former case, by the repulsion of the electric fluid, in the latter, by the repulsion of constituent parts of the bodies. Mr. Wilcke observes upon this subject, that the attraction of light bodies to negative electrics cannot be owing to the repulsive power of the electric fluid in the neighbouring air, driving them, or the electric matter in them to the place where there is a want of it; because the velocity ought to decrease as it recedes from the impulsive power: whereas it is accelerated, as if it were attracted by the negative electric.*

BUT to this it may be replied, that a succession of impulses, though every subsequent one should be weaker than the preceding, will produce an accelerated motion. Besides, the nearer the light body is to the negative electric, the nearer it is to the point where the equilibrium of the fluid is most destroyed; or the less force there is on the side of the electric to balance the force that drives the light body towards it, and therefore the impulses themselves must increase.

MR. WILCKE, whose treatise on the two electricities is admirable, both for its materials, and methodical arrangement of them, distinguishes three causes of excitation, viz. *warming*, *liquefaction*, and *friction*; and he advises, that we carefully distinguish between *spontaneous* and *communicated* electricity. By the former he

* Wilcke, p. 15, and Remarks on Franklin's Letters, p. 270.

means that which is the result of the apposition, or mutual action of two bodies; in consequence of which, one of them is left positively electric and the other negative. Whereas *communicated* electricity is that which is superinduced upon a body, or part of a body, electric or non-electric, without its being previously heated, melted, or rubbed; or without any mutual action between it and any other body. This distinction is, in general, very obvious; but Mr. Wilcke defines it more accurately than it had been done before, and mentions several cases in which they are often confounded.

SIGNIOR BECCARIA admits the theory of positive and negative electricity, though he explains some electrical phenomena in a manner different from the other patrons of that system.

He supposes that electrified bodies move to one another only in the act of giving and receiving the electric fluid: * this effect being produced by the electric matter making a vacuum in its passage, and the contiguous air afterwards collapsing, and thereby pushing the bodies together. † This vacuum, he says, is very observable upon great explosions of thunder, when animals have been struck dead without being touched with the lightning; a vacuum being only suddenly made near them, and the air immediately rushing out of their lungs to fill it, whereby they are left flaccid and empty; whereas when persons are properly killed by lightning, their lungs are found distended. ‡

IN confirmation of this hypothesis, he says, that less motion is given to bodies by electricity, as the air is excluded from them, and that in vacuo no motion at all can be given to them. || He also says, that no electric light is visible in a barometer in which there is a perfect vacuum: whence he infers, that electric light

* Lettere dell' elettricismo, p. 36. † Ib. p. 41. ‡ Ib. p. 42. || Ib. p. 48.

is visible only by means of some vibrations which it excites in the air.*

THIS hypothesis seems to be unworthy of so eminent an electrician, and it is certainly not fact, that there is no electric light, if there be no electrical attraction in vacuo.

To account for the collection or dissipation of electricity by points, he says that the electric fluid appears, from experiments, to move with the greatest violence in the smallest bodies. All electrical appearances will, therefore, be most sensible at the points of bodies; and, consequently, it will be soonest dissipated there. But this does not seem to touch the real difficulty.

DR. FRANKLIN the author of this excellent theory of positive and negative electricity, with a truly philosophical greatness of mind, to which few persons have ever attained, always mentions it with the utmost diffidence. Every appearance, says he, which I have yet seen, in which glass and electricity are concerned, are, I think, explained with ease by this hypothesis. Yet, perhaps, it may not be a true one, and I shall be obliged to him who affords me a better.†

IT is no wonder, indeed, that this excellent philosopher should treat even his own hypothesis with such indifference, when he had so just a sense of the nature, use, and importance of all hypotheses. Nor is it, says he, of much importance to us, to know the manner in which nature executes her laws. It is enough if we know the laws themselves. It is of real use to us to know that china left in the air, unsupported, will fall and break; but how it comes to fall, and why it breaks, are matters of speculation. It is a pleasure indeed to know them, but we can preserve our china without it. ‡

*Ib. p. 59.

† Franklin's Letters, p. 78.

‡ Ib. p. 59.

THE great merit of this writer as an electrician stands independent of all hypotheses, upon the firm basis of the discovery of many new and important facts, and, what is more, applied to the greatest uses. Supposing him, for instance, to have been mistaken in his account, how the clouds come to be possessed of electricity, must not all the world acknowledge themselves indebted to him for the discovery of the sameness of the electric fluid and the matter of lightning; and especially for a certain method of preserving their buildings and persons from the fatal effects of thunder storms.

SECTION

SECTION III.

OF THE THEORY OF TWO ELECTRIC FLUIDS.

CONVINCED, as the reader may have perceived that I am, of the usefulness of various theories, as suggesting a variety of experiments, which lead to the discovery of new facts; he will excuse me, if I recall his attention to the old theory of vitreous and resinous electricity, as it was first suggested by Mr. Du Faye, upon his discovery of the different properties of excited glass, and excited amber, sulphur, rosin, &c. and as it has been new modelled by Mr. Symmer. To show my absolute impartiality, I shall, notwithstanding the preference I had given to Dr. Franklin's theory, endeavour to represent this to as much advantage as possible, and to do it more justice than has yet been done to it, even by Mr. Symmer himself; who, as I observed before, has fallen into some mistakes in his application of it. Indeed, hitherto very little pains has been taken with this theory, nor has it been extended to any great variety of phenomena.

LET us suppose then, that there are two electric fluids, which have a strong chymical affinity with each other, at the same time

that the particles of each are as strongly repulsive of one another. Let us suppose these two fluids, in some measure, equally attracted by all bodies, and existing in intimate union in their pores, and while they continue in this union to exhibit no mark of their existence. Let us suppose that the friction of any electric produces a separation of these two fluids, causing (in the usual method of electrifying) the vitreous electricity of the rubber to be conveyed to the conductor, and the resinous electricity of the conductor to be conveyed to the rubber. The rubber will then have a double share of the resinous electricity, and the conductor a double share of the vitreous; so that, upon this hypothesis, no substance whatever can have a greater or less quantity of electric fluid at different times; the quality of it only can be changed.

THE two electric fluids, being thus separated, will begin to show their respective powers, and their eagerness to rush into reunion with one another. With whichever of these fluids a number of bodies are charged, they will repel one another, they will be attracted by all bodies which have a less share of that particular fluid with which they are loaded, but will be much more strongly attracted by bodies which are wholly destitute of it, and loaded with the other. In this case they will rush together with great violence.

UPON this theory, every electric spark consists of both the fluids rushing contrary ways, and making a double current. When, for instance, I present my finger to a conductor loaded with vitreous electricity, I discharge it of part of the vitreous, and return as much of the resinous, which is supplied to my body from the earth. Thus both the bodies are unelectrified, the balance of the two powers being perfectly restored.

WHEN I present the Leyden phial to be charged, and, consequently

quently, connect the coating of one of its sides with the rubber, and that of the other with the conductor, the vitreous electricity of that side which is connected with the conductor is transmitted to that which is connected with the rubber, which returns an equal quantity of its resinous electricity; so that all the vitreous electricity is conveyed to one of the sides, and all the resinous to the other. These two fluids, being thus separated, attract one another very strongly through the thin substance of the intervening glass, and rush together with great violence, whenever an opportunity is presented, by means of proper conductors. Sometimes they will force a passage through the substance of the glass itself; and, in the mean time, their mutual attraction is stronger than any force that can be applied to draw away either of the fluids separately.

HAVING stated the general principles of this hypothesis of two fluids, I shall now enter into a brief comparison of it with that of a single fluid, as explained by the mode of positive and negative electricity; that we may see which of them will account for the same facts in the easier manner, and more agreeable to the analogy of nature in other respects. For, allowing that no fact can be shown to be absolutely inconsistent with either of them; yet, certainly, that will be judged preferable, which is attended with the least difficulty in conceiving of its mode of operation.

In the first place, the supposition itself, of two fluids, is not quite so easy as that of one, though it is far from being disagreeable to the analogy of nature, which abounds with affinities, and in which we see innumerable cases of substances formed, as it were to unite with and counteract one another. Here, likewise, agreeable to the theory of two electric fluids, while those substances are in union, we see nothing of their separate and peculiar powers, though they be ever so remarkable. What, for in-

stance, do we see of the striking properties of the *acid* and *alkali* while they are united in a neutral salt? What powers in nature are more formidable than the vitreous acid, and phlogiston (which consists principally, if not wholly, of mephitic air) and what more innocent than common sulphur, which is a composition of them both, and from which the action of fire separates them.

THE two fluids being supposed, the double current from the rubber to the conductor and from the conductor to the rubber is an easy and necessary consequence. For if, upon the common supposition, the action of the rubber puts a single fluid into motion in one direction, we might expect that, if there were two fluids, which counteracted each other, they would, by the same operation, be made to move in contrary directions. And a person who has been used to conceive that a single fluid may be made to move either way, viz. from the conductor to the rubber, or from the rubber to the conductor, at pleasure, according as a rough or a smooth globe is used, can have much less objection to this part of the hypothesis.

ADMITTING then this different action of the rubber and the electric upon the two different fluids, the manner of conveying electric atmospheres, or powers to bodies is the same on this as on any other theory; and it is apprehended, that the phenomena of negative electricity are more easily conceived by the help of a real fluid, than by no fluid at all. Indeed Dr. Franklin himself ingenuously acknowledges, that he was a long time puzzled to account for bodies that were negatively electrified repelling one another; whereas Mr. Du Faye, who observed the same fact, had no difficulty about it, supposing that he had discovered another electricity, similar, with respect to the properties of elasticity and repulsion, to the former.

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By this double action of the rubber, the method of charging a plate of glass is exceeding easy to conceive. Upon this hypothesis, all the vitreous electricity quits its union with the resinous on the side communicating with the rubber, and is brought over to the side communicating with the conductor; which, by the same operation, had been made to part with its resinous electricity in return.

ALL the vitreous electricity being thus brought to one side of the plate of glass, and all the resinous to the other, the phenomena of the plate while standing charged, or when discharged, are, perhaps, more free from all difficulty than upon any other hypothesis. When one of the sides of the glass is conceived to be loaded with one kind of electricity, and the other side with the other kind; the strong affinity between them, whereby they attract each other with a force proportioned to their nearness, immediately supplies a satisfactory reason, why so little of either of the fluids can be drawn from one of the sides without communicating as much to the other. Upon this supposition, that consequence is perhaps more obvious than upon the supposition of one half of the glass being crowded with the electric matter, and the other half exhausted. In the former case, every attempt to withdraw the fluid from one of the sides is opposed by the more powerful attraction of the other fluid on the opposite side. On the other hypothesis, it is only opposed by the attraction of the empty pores of the glass.

LASTLY, the explosion upon the discharge of the glass has as much the appearance of two fluids rushing into union, in two opposite directions, as of one fluid, proceeding only in one direction. The same may be said of the appearance of every common electric spark, in which, upon this hypothesis, there is always

always supposed to be two currents, one from the electric, or the electrified body, and the other to it.

I do not say that the bur which is usually seen on both sides of a quire of paper pierced by an electric explosion, and the current of air flowing from the points of all bodies electrified negatively as well as positively, are material objections to the doctrine of a single fluid. I have even shown how they may be explained in a manner consistent with it; but upon the supposition of two fluids, and two currents, the difficulty of accounting for these facts would hardly have occurred.

THE phenomena of discharging a plate of glass, upon the hypothesis of two fluids, are indeed, injudiciously explained by Mr. Symmer; who supposes that the two fluids do not always make the whole circuit of the intervening conductors, but enter them more or less, from each side of the plate, according to the strength of the charge. But upon this supposition, the fire of the smallest charge performs the whole circuit, as well as the fire of the greatest, in order to restore the equilibrium of the two fluids on each side of the glass.

It is almost needless to observe, that the influence of points is attended with exactly the same difficulty upon this theory as upon the other. It is equally easy, or equally difficult, to suppose one fluid to enter and go out at the point of an electrified conductor, at different times, as to suppose that, of two fluids, one goes out, and the other goes in, at the same time.

THAT bodies immersed in electric atmospheres must acquire the contrary electricity, is quite as easy to conceive upon this, as upon any other hypothesis. For, in this case, suppose the electrified body to be possessed of the vitreous electricity, all the vitreous electricity of the body which is brought near it will be driven

driven backwards, to the more distant parts, and all the resinous electricity will be drawn forwards. And when the attraction between the two electricities, in these different bodies, is so great, as to overcome the opposition to their union, occasioned by the attraction of the bodies that contained them, the form of their surfaces, and the resistance of the interposing medium, they will rush together; an electric spark will be visible between them, and the electricity of both will appear to be discharged; the prevailing electricity of each being saturated with an equal quantity of the opposite kind, from the other body.

THIS hypothesis will likewise easily account for the difficulty of charging a very thick plate of glass, and the impossibility of charging it beyond a certain thickness: for these fluids at a greater distance, will attract one another less forcibly; and at a certain still greater distance will not attract at all.

HAVING given the most favourable view that I can of this hypothesis of two electric fluids, I shall, with the same fairness, make the best answer I am able to the principal objection that will probably be made to it.

IF it be asked, why the two fluids, meeting on the surface of the globe, or in the electric explosion, do not unite, by means of their strong affinity, and make no further progress; it may be answered, that the attraction between all other bodies and the particles of both these fluids may be supposed to be, at least, as strong, as the affinity between the fluids themselves; so that the moment that any body is dispossessed of one, it may recruit itself, to its usual point of saturation, from the other.

BESIDES, in whatever manner it be that one of the electric fluids is dislodged from any body (since, upon every theory, the two electricities are always produced at the same time) the opposite electricity will, by the same action, be dislodged from the

the other substance. And (as upon the common theory) whatever it be that dislodges the fluid from any substance, it will be sufficient to prevent its return; consequently, supposing both the substances necessarily to have a certain proportion of electric matter, each may be immediately supplied from that which was dislodged from the other.

THE rubber, therefore, at the time of excitation, gives its vitreous electricity to that part of the smooth glass against which it has been pressed, and takes an equal quantity of the resinous in return. The glass, being a non-conductor, does not allow this additional quantity of vitreous electricity to enter its substance. It is therefore diffused upon the surface, and, in the revolution of the globe, is carried to the prime conductor. There (as in the experiments begun by Mr. Canton, and prosecuted by Mr. Wilcke, &c.) it repels the vitreous, and violently attracts the resinous electricity; and (the points of the conductor favouring the mutual transition) the vitreous, which abounds upon the globe, passes to the conductor; and the resinous, which abounds on the nearest parts of the conductor rushes upon the globe. There it mixes with, and saturates what remained of the vitreous electricity, on the part on which it flows, and thereby reduces it to the same state in which it was before it was first excited. Every part of the surface of the globe performs the same office, first exchanging electricities with the rubber, and then with the conductor.

THE solution of this difficulty will likewise solve that of the electric explosion, in which there is a collision, as it were, of the two fluids, while yet they completely pass one another. For still each surface of the glass may be supposed to require its certain portion of electric matter, and therefore cannot part with one sort without receiving an equal quantity of the other.

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It must be considered also, that the air, through which these fluids pass, has already its natural quantity of electricity, so that being fully saturated, it can contain no more; and that the two fluids only rush to the places from whence they had been forcibly dislodged, and where the greater body of the opposite fluid waits to embrace them.

MR. SYMMER's hypothesis of a double current differed in some respects from that of the Abbé Nollet. The Abbé, however, according to his usual candour, speaks of him with the highest respect; at the same time, he still appears an advocate for his old favourite hypothesis.

MR. CIGNA, who pursued the experiments above recited of Mr. Symmer, observes, with respect to his theory; that it is not contradicted by any phenomena that are yet known, and that it suits some of them in a peculiarly clear and elegant manner; particularly every thing relating to charging and discharging a plate of glass; all the experiments which seem to show a mutual attraction between the two electricities, when they are kept asunder; and that curious experiment above-mentioned of Signior Beccaria, of discharging a plate of glass suspended by a silken string, without either touching or moving the plate. Yet, upon the whole, he declares in favour of Dr. Franklin's theory of positive and negative electricity, on account of its admirable simplicity, and because philosophers ought not to multiply causes without necessity.

DR. FRANKLIN's theory, he says, completely solves all the cases of the two electricities destroying one another when they are mixed; but doth not so clearly account for their attracting, and counteracting one another when they are separate. He concludes with saying, he doth not chuse to say much on so very obscure a question, which has divided the opinions of very great

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men; and that any hypothesis of the two electricities, which will account for the destruction of all the signs of electricity when they are united, and their mutual attraction when they are separate, will equally suit all the phenomena that have yet been discovered.

I HAVE taken a little pains with this theory, because I thought, it had been hitherto, too much overlooked, and that sufficient justice had not been done to it, even by those who proposed it. For the future, I hope it will be seen to more advantage, and appear a little more respectable among its sister hypotheses; and then, *valeat quantum valere potest*. If any electrician will favour me with the communication of any other theory, not obviously contradicted by facts, I shall think myself obliged to him, and shall think I do a piece of real service to the science in the publication of it. If more persons favour me with more different theories, I should think my book, as far as theories are of any use, so much the more valuable.

PART IV.

DESIDERATA IN THE SCIENCE OF ELECTRICITY, AND HINTS
FOR THE FURTHER EXTENSION OF IT.

SECTION I.

GENERAL OBSERVATIONS ON THE PRESENT STATE OF
ELECTRICITY.

THAT real progress has been made in electricity, has, I presume, been sufficiently demonstrated in the course of the preceding history; that a great deal still remains to be done, will, I think, be evident from this part of the work. Those persons who think that nothing has been done to any purpose in Natural Philosophy, or that the advances have been made very slowly, since the time of Sir Isaac Newton, need only read the preceding history, to be convinced, both that a great deal has been done, and that the progress in this kind of knowledge,

instead of being slow, has been amazingly rapid. To quicken the speed of philosophers in pursuing this progress, and at the same time, in some measure, to facilitate it, is the intention of this treatise, and more especially of this part of it. When a traveller imagines he is near his journey's end, he is little solicitous about making dispatch, thinking that, without any haste, the labour of the day will quickly be over; whereas, if he find that, whatever progress he may have made, he has a great deal still to make, he continues, or quickens his speed.

THE principal reason why many ingenious persons have so soon got to their *ne plus ultra* in philosophical discoveries, has evidently been their attachment to favourite theories; which they imagined both accounted for all the phenomena that had been observed, and would likewise account for all that should be observed. Having therefore attained to the great object of a science, and discovered the ultimate and most general principles of it, there was nothing more that was worth their notice; it being beneath men of genius to spend their time in diversifying effects, when there were no new causes to be found. I hope that what has hitherto been said concerning the nature and use of hypotheses, and about the progress and present imperfect state of those which respect electricity, will convince those electricians who may not yet have been convinced of it, that our business is still chiefly with *facts*, and the *analogy of facts*; that far too few of these have been discovered to ascertain a perfect general theory, and that all that the present hypotheses can do for us must consist in suggesting further experiments.

If we look back upon the history of electricity, and consider the state of facts and of hypotheses at any particular period of time past, we shall see that there was always the same apparent reason for acquiescing in what had been done, as at present. The theories

ries of the first electricians, lame and imperfect as they were, were yet sufficient to account for all the facts they were acquainted with; and as for other facts, they could have no idea or apprehension of them, and therefore could not be solicitous about them.

MR. BOYLE, no doubt, was as fully satisfied with his simple hypothesis of the unctuous effluvia, as Mr. Nollet with his theory of affluences and effluences; or the greatest part of the present race of electricians with that of positive and negative electricity. Mr. Hawkesbee, when he made his surprising discoveries concerning the properties of electric light, and many curious circumstances concerning electric attraction and repulsion, might very naturally think that little more was to be done. Indeed, who could have thought otherwise, when the science was actually at a stand for several years after him? All that the indefatigable Mr. Grey (who made the great discovery of the communication of electric powers to bodies not electric per se) imagined to remain undone, were mere chimeras and illusions. Mr. Du Faye, who made the discovery of vitreous and resinous electricity, had no idea of the electric shock; and the German philosophers, who accidentally observed it, knew nothing of its most remarkable properties. Notwithstanding a great number of treatises on the subject of electricity appeared presently after this discovery, and some of them very systematical, comprehending, no doubt, what the authors of them thought to be the whole of the science, yet none of them had the least idea of the amazing discoveries of Dr. Franklin, relating either to the Leyden phial, or to the nature of lightning. And though numbers of Dr. Franklin's admirers thought that he had exhausted the whole subject, he himself was far from thinking so; and the history of electricity, since the

date

date of his capital discoveries, demonstrates that his suspicion was true.

It may be said, that there is a *ne plus ultra* in every thing, and therefore in electricity. It is true: but what reason is there to think that we have arrived at it. Mr. Grey might have used the same language above twenty years ago; but every body will, now, acknowledge, that it would have been above twenty years too soon: and yet, I think, it is evident, that Mr. Grey had really more reason to think he had arrived at the *ne plus ultra* of electricity, than we have to think that we are arrived at it. Time has brought to light a great number of *incomplete*, as well as complete experiments, and perhaps more of the former than of the latter; concerning all which, as he could have no knowledge, so he could have no doubts; so that, though we know much more than he did; we, at the same time, know how much more is unknown better than he could. Hitherto the acquisition of electrical knowledge has been like the acquisition of riches. The more we possess, the more we wish to possess; and, I hope, the more indefatigable we shall be to acquire the possession of it.

ONE thing extremely useful to the progress of further discoveries, is to know what has really been done by others, and where the science stands at present. For want of this knowledge, many a person has lost his time upon experiments which he might have known had either failed or succeeded with others; and which it was, therefore, not worth his while to repeat. But the sources of this kind of information are too much scattered, and too distant for most persons to have access to them. This was the first motive of the present undertaking, intended to exhibit a distinct view of all that has been done in electricity to the present time, and likewise the order and manner in which every thing has been done; that electricians, having a distinct
idea

idea of what the progress of electrical knowledge has been, might see more clearly what remains to be done, and what pursuits best promise to reward their labour.

INDEED it is almost impossible for any person to read the history of electricity without gathering many hints for new experiments. When he has the whole before him at one view, he can better bring the distant parts together; and from the comparison of them, new lights may arise. When he sees what experiments have failed, and what have succeeded; what branches of the science have been most attended to, and what things seem to have been overlooked; what has been discovered by accident, and what by theory; when he sees both the true lights which directed some happy discoverers, and the false lights which misled others, he will have the best preparation for pursuing his own inquiries.

To point out many of the *desiderata* in the science of electricity, I am sensible, will, for this reason, be superfluous to many persons, and probably to most who will have read thus far of this treatise: for sufficient hints of them must have been suggested by the perusal of the history. But if I have been anticipated in this part of my work by some of my readers, it will not displease them to find it; and to others the contents of this chapter will be peculiarly useful.

If, indeed, I had consulted my reputation as a writer, or a philosopher, I should not have attempted this chapter at all. For not only will many of the articles which I shall now put down as *desiderata* in the science be soon no longer so, and even young electricians be able to give satisfactory answers to some difficult queries I am going to propose; but many of them will probably appear idle, frivolous, or extravagant ones; and, in a more advanced state of the science, it will hardly be imagined why I put
them

them down at all. But if this chapter be a means of hastening so desirable an event, and of accelerating the progress of electrical knowledge, I am very willing that it should, ever after, stand as a monument of my present ignorance.

“THESE thoughts,” to adopt the words of Dr. Franklin, with much more propriety than he himself first used them, “are many of them crude and hasty, and if I were merely ambitious of acquiring some reputation in philosophy, I ought to keep them by me, till corrected and improved by time and farther experience. But since even short hints and imperfect experiments, in any new branch of science, being communicated, have oftentimes a good effect, in exciting the attention of the ingenious to the subject, and so become the occasion of more exact disquisitions, and more complete discoveries; you are at liberty,” says he, to Mr. Collinson, “to communicate this paper to whom you please, it being of more importance that knowledge should increase, than that your friend should be thought an accurate philosopher.”

I WOULD not even propose to draw up the following *queries* upon the plan of those of Sir Isaac Newton, at the end of his treatise on Optics. Many of them are such, that I have hardly the most distant expectation of their being verified; but the attempt to verify them may possibly lead to some other discoveries of more importance. They are such random thoughts as led to the new experiments I have made; and not having any more leisure to pursue them myself, I freely impart them to my reader, that he may make as much advantage of them as he can: being determined, upon taking leave of the subject, *to write myself fairly out*, as Mr. Addison says; or, as the Spanish writers say, *to leave nothing in my inkhorn*.

HAPPY would it be for science, if all philosophers who are engaged

engaged in the same pursuits, would make one common chapter of all their hints and queries: and greatly honoured should I think myself if the present chapter in this treatise might be made use of for that purpose, and if, in future editions of the work, it should be looked into as the common receptacle of the *present desiderata* among the whole body of electricians, and of their imperfect hints for new discoveries. With pleasure should I see each of them distinguished by the name of some generous and illustrious contributor. A few, the reader will find, have been added to my own, and are distinguished in this manner.

MANY persons can throw out hints, who either have not leisure, or a proper apparatus for pursuing them: others have leisure, and a proper apparatus for making experiments, but are content with amusing themselves and their friends in diversifying the old appearances, for want of hints and views for finding new ones. By this means, therefore, every man might make the best use of his abilities for the common good. Some might strike out lights, and others pursue them; and philosophers might not only enjoy the pleasure of reflecting upon their own discoveries, but also upon the share they had contributed to the discoveries of others.

SECTION II.

QUERIES AND HINTS CALCULATED TO PROMOTE FURTHER
DISCOVERIES IN ELECTRICITY.

I.

QUERIES AND HINTS CONCERNING THE ELECTRIC
FLUID.

WHAT is the proportion of the several colours in electric light, in different cases, and in different appearances of it?

Is not the electric light a real vapour ignited, similar to that of phosphorus; and may not experiments be, hereafter, made, where we shall have the explosion, the shock, and the other effects of electricity, without the light? Is the electric light ever visible except in vacuo? In the open air the electric fluid makes itself a vacuum in order to its passage.

COLLECT the electric fluid, not from the general mass of the earth but from bodies of particular kinds, and observe if it have any different properties, with respect to light, &c.

Is it exactly the same at sea, as on land; below the surface of the earth, as above it, &c. &c. &c.?

DR.

DR. FRANKLIN observed, that iron was corroded by being exposed to repeated electric sparks. Must not this have been effected by some acid? What other marks are there of an acid in the electric matter? May not its phosphoreal smell be reckoned one?

Is there only one electric fluid, or are there two? Or is there any electric fluid *sui generis* at all, distinct from the ether of Sir Isaac Newton? If there be, in what respect does it differ from the ether?

ARE the particles which affect the organ of smelling, as well as the particles of light, parts of the proper electric fluid, or are they merely adventitious, being, some way or other, brought into action by electricity?

DOES not some particular order of the particles, which Sir Isaac Newton supposes to be continually flying from the surfaces of all bodies, constitute the electric fluid; as others, he imagined, constituted the air, and others the ether, &c.?

Is it probable that there is even any temporary, or growing addition to, or diminution of the whole stock of electricity?

WHENCE arises the elasticity of the electric fluid, and according to what law do its particles repel one another? *Mr. Price.*

Is not the electric matter the same with, or at least, hath it not some near relation to that luminous matter which forms the solar atmosphere, and produces the phenomenon called the *zodiacal light*; which is thrown off principally, and to the greatest distance, from the equatorial parts of the sun, in consequence of his rotation on his axis, extending visibly in the form of a luminous pyramid, as far as the orbit of the earth; and which, according to Monsieur De Mairan's ingenious, and at least, plausible hypothesis, falling into the upper regions of our atmosphere, is collected chiefly towards the polar parts of the earth, in con-

sequence of the diurnal revolution, where it forms the aurora borealis?

MAY not the sun be the fountain of the electric fluid; and the zodiacal light, the tails of comets, the aurora borealis, lightning, and artificial electricity, as its various, and not very dissimilar modifications. *

DID not the sulphureous smell draw our attention towards the vitreolic acid, the peculiar hissing noise accompanying the electric blast, spontaneously issuing, for instance, from the pointed wire of a fully charged phial, appears rather to mimic the explosive action of deflagrating nitre; and may, accordingly, without much violence, be supposed to arise from the nitrous aerial acid, violently commensuating with the phlogiston, which it either meets with in the air, or which is conveyed to it by the electrified body. Or, were we to adopt the hypothesis of two distinct electric fluids, we might, by way of temperament, propose as a query, whether the nitrous acid, &c. may not be the constant concomitant of those explosive pencils of light, which are observed to dart from the points of bodies replete with the vitreous electricity; while the silent and languid luminous specks (resembling the small tip of a lighted match) appearing on the extremities of bodies endued with a resinous electricity, may as probably indicate the accension of a sulphureous matter, and consequently the presence of the vitreolic acid, the electric spark of explosion, appearing on the approach of the two bodies, being considered as the effect, at least, in part, of the menstrual action of these two acids on each other, &c. †

* Monthly Review, October 1767, p. 353.

† *Ib.* December, p. 458.

II.

QUERIES AND HINTS CONCERNING ELECTRICS AND CONDUCTORS.

IN what does the difference between electrics and conductors consist? In other words, what is it that makes some bodies permeable to the electric fluid, and others impermeable to it?

ARE the pores of electric bodies smaller than those of conductors, and do they contain very much, or very little of the electric fluid?

WHAT is it in the internal structure of bodies that makes them break with a polish? Perhaps all solid electrics do so.

HAS elasticity any connection with electricity, some electrics being extremely elastic?

WHAT is the reason why, in some of Mr. Hawkesbee's experiments, the electric light was visible through a considerable thickness of very opaque electrics, as rosin, sulphur, pitch, &c; but not through the thinnest metallic conductors?

WHAT similarity is there in the processes of calcination, vegetation, animalization, and in some measure chrysalization; since all bodies which have gone through any of those processes, and perhaps no others, are found to be electrics?

ARE not both electrics and conductors more perfect in their kind in proportion to their specific gravity?

WILL not water conduct electricity the best in its state of greatest

greatest condensation; and metals the least in their greatest expansion, as shown by a pyrometer?

COMPARE the invisible effluvia of water with the invisible effluvia of a burning candle, and also those proceeding from other bodies, with respect to their power of conducting electricity.

OBSERVE what degree of heat will discharge any given degree of electricity, in order to find in what degree heat makes air a conductor.

III.

QUERIES AND HINTS CONCERNING EXCITATION.

WHAT is the difference, in the internal structure of electrics, that makes some of them excitable by friction, and others by heating and cooling?

WHAT have friction, heating, cooling and the separation after close contact in common to them all? How do any of them contribute to excitation? And in what manner is one, or the other kind of electricity produced by rubbers and electrics of different surfaces?

Is not Æpinus's experiments of pressing two flat pieces of glass together, when one of them contracts a positive and the other a negative electricity similar to the experiments of Mr. Wilcke, concerning the production of electricity by the liquefaction of various substances in others; when the substance which melts and contracts is in one state, and that which contains it is in the

the opposite? And are not both these cases similar to the excitation of the tourmalin, &c. by heating and cooling? In this case may not the tourmalin and the air act upon one another and be in opposite states?

Is not the circumstance common to all these cases, some affection of that space near the surface of the bodies in which the refractive power lies? When bodies which have been pressed together within that space recede from one another, more surface, and consequently more of that space is made, doth not the electric fluid flow into it from that body which has the least power of retaining it, and which it can permeate with the most ease; when not being able to enter the substance of the other, it rests upon its surface?

ARE not the particles of the electric and rubber thrown into a vibration in the act of excitation, which makes frequent recedings of the parts from one another, and thereby promotes the effect above-mentioned?

WHAT is the real effect of putting moisture or amalgam upon the rubber? Do not those substances increase the power of excitation, as conductors more distant from the smooth glass, in the gradation of electrics, than the surface of the leather? Or do they only make the rubber touch in more points, or alter the surface of the rubber?

HAS that difference of surface on which colour depends any influence upon the power of excitation?

THE tourmalin and a vessel of charged glass hermetically sealed are both excited by heating and cooling. What other properties have they in common?

IV.

QUERIES AND HINTS CONCERNING ELECTRIFICATION.

Does electrification increase the exhalation of vapours either from cold or from boiling water? If it do, is the increased exhalation the same in all states of the atmosphere?

Does not the electric matter pass chiefly on the surface of bodies?

Is the action of electrified bodies upon one another more properly an attraction or a repulsion?

Would not continued electrification promote putrefaction?

In what manner is the mutual repulsion of two bodies electrified negatively performed? Is it by the attraction of the denser electric fluid in the neighbourhood, by the quantity of it which may be supposed to be accumulated on the surfaces of such bodies in the manner described p. 430, or to the mutual repulsion of the particles of matter of which the bodies consist?

V.

QUERIES AND HINTS CONCERNING THE POWER OF CHARGING ELECTRICS.

WHAT is the real operation of conductors in coating electric substances?

WHY

WHY may not one phial be charged by connecting it with another (while it is charging) as high as if it were charged at the prime conductor? Or by what rule must the force of those different chargings be estimated? To all appearance, two phials charged together, so as that one of them receives the fire from the other, do not give so large a shock, as only one of them charged in the usual way.

MR. WILCKE supposes, that when one plate of glass is charged by the very same electric matter which has been expelled in charging another plate, only part of that which is expelled from the former can enter the substance of the latter; because part must remain upon the wire, the coating, &c. and some must be lost. For the same reason, only part of that which is expelled from the second can enter the third; so that a very remote jar will not be charged at all.*

WHAT is the *maximum* of charging a glass jar, with respect to the quantity of its surface, covered by the coating? It is evident that some jars will discharge themselves, when only a small part at the bottom of them is coated, and when the explosion is very inconsiderable.

ENDEAVOUR to charge a plate of glass with the coating pressed into actual contact with its surface, by means of heavy weights. Also endeavour to excite a plate of glass in the same manner. It is pretty certain that, in the usual method of exciting and charging, the real substance of the glass is not touched; and though water be attracted by glass; it may only be to a certain distance from it.

* Remarks on Dr. Franklin's Letters, p. 351.

VI.

QUERIES CONCERNING THE ELECTRICITY OF GLASS.

THROUGH what thickness of glass will an excited electric, of any given strength, attract and repel light bodies? Is not the same thickness the limit of charging glass with the electric fluid?

Is not a plate of glass contracted in its dimensions by charging, the two electricities strongly compressing it, so as to increase its specific gravity?

Is the tone of a glass vessel, made in the form of a bell, the same when it is charged as when it is uncharged? Or would the ringing of it make it more liable to break in those circumstances?

Does the electric fluid with which glass is charged reside in the pores of the glass, or only on its surface; or rather within the space that is occupied by the power of refraction, i. e. a small space within, and likewise without the surface?

Is the refractive power of glass the same when it is charged or excited?

How does the different refractive power of glass, or its density (which is probably in the same proportion with its refractive power) affect its property of being excited or charged?

Is there not a considerable difference in glass when it is new made, and when it has been kept a month or two, both with respect to excitation and charging?

LET

LET glass of every different composition be tried both with respect to excitation, and charging. Would it not be found that differences with respect to metallic ingredients, hardness, annealing, continuance in fusion, &c. would influence both the properties; and that, in several cases, the same circumstance that was favourable to one would be unfavourable to the other?

GLASS has hitherto been supposed to be full of the electric fluid, and its impermeability has been accounted for upon the difficulty with which the electric fluid moves in its pores. But may we not suppose the substance of glass to be absolutely impermeable to electricity, that no foreign electric matter ever so much as enters a single pore of it, but lodges wholly on its surface; for instance, between the point of contact and the real surface, or within the limits of the refractive power that is a little way on both sides the surface. This place is, I think, on many accounts, extremely convenient to dispose of the electric matter, whether we make it to consist of two fluids, or of one. Their being kept asunder, if there be two, or its being prevented from getting through, if there be but one, will be much easier to conceive in this case, than upon the supposition that the electric fluid *can* enter and move in the substance of the glass, though it can only enter and move with difficulty, as *Æpinus* expresses it. For, let the motion be ever so difficult, one would think that this circumstance could only make it move so much the slower, and that, give the electricity in the charged plate of glass time enough, and it would at length, without any external communication, perform the journey to the other side, whither it has so strong a tendency to go.

MOREOVER, one would think, that, upon the hypothesis of the admission of the electric fluid within the pores of the glass,

when the discharge of a phial was actually made through the substance of the glass, it might be in a silent manner, without breaking the glass; whereas when the surfaces of the glass are supposed to be violently pressed, and the pores of it not in the least entered by any particle of the fluid, or fluids, the impossibility of the electric charge getting through the glass is evident, as well as the necessity of its breaking the glass, if it do force a passage.

VII.

QUERIES AND HINTS CONCERNING THE EFFECT OF ELECTRICITY ON ANIMAL BODIES.

Is the fluid on which electricity depends at all concerned in any of the functions of an animal body? In what manner is the pulse of a person electrified quickened, and his perspiration increased?

Does not the air, by being heated in the lungs, communicate an electric virtue to the blood? What connection has this circumstance with the mephitic air which is exhaled from the lungs in great quantities, as well as contained in all the other excrements of the animal body?

MAY not the increased perspiration of an animal body be greater in a moist atmosphere than in a dry one, there being then more conducting particles in the atmosphere, to act and react upon the effluvia in the pores of the body; on which

the copious perspiration does, probably, in a great measure, depend?

VIII.

QUERIES AND HINTS CONCERNING THE ELECTRICITY OF THE
ATMOSPHERE.

In what manner do the clouds become possessed of electricity?

Does the wind in any measure contribute to it?

Is it effected by the gradual heating and cooling of the air? If so, whether is it the heating or the cooling that produces positive electricity? Which ever it be, the contrary will probably produce negative electricity. Let the experiment be made by an electrical kite. *Mr. Canton.*

As thunder generally happens in a sultry state of the air, when it seems replenished with some sulphureous vapours; may not the electric matter then in the clouds be generated by the fermentation of sulphureous vapours with mineral or acid vapours in the air. *Mr. Price.*

MR. WILCKE supposes the air to contract its electricity, in the same manner as sulphur and other substances do, when they are heated and cooled in contact with various bodies. Thus the air, being heated or cooled in the neighbourhood of the earth, gives electricity to the earth, or takes from it; and the electrified air, being conveyed upwards, by various means, communicates its electricity to them. *

* Remarks on Franklin's Letters, p. 302.

LET rain, snow, and hail be received in insulated vessels, in different states of the atmosphere, to observe whether they contain any electricity, and in what degree.

MAY not the void space above the clouds be always occupied with an electricity opposite to that of the earth? And may not thunder, earthquakes, &c. be occasioned by the rushing of the electric fluid between them, whenever the redundancy in either is excessive? Is not the aurora borealis, and other electrical meteors, which are remarkably bright and frequent before earthquakes, some evidence of this?

Is not the earth in a constant state of moderate electrification, and is not this the cause of vegetation, exhalation and other the most important processes in nature? These are promoted by increased electrification. And is it not probable that earthquakes, hurricanes, &c. as well as lightning, are the consequence of too powerful electricity in the earth?

SUPPOSING earthquakes to be caused by the discharge of a redundant electricity from the surface of the earth, might they not be prevented, in countries subject to them, by kites constantly flying very high, with wires in the strings, so as to promote an easy communication between the earth and the upper regions of the atmosphere?

SECTION

SECTION III.

BRANCHES OF KNOWLEDGE PECULIARLY USEFUL TO AN ELECTRICIAN.

IN the historical part of this work I have shown what has been done on the subject of electricity, and under the preceding *desiderata*, I have endeavoured to give some idea of what yet remains to be done, with a few hints concerning further experiments. In the close of this part, I would willingly do something more towards enabling my reader to make farther advances in electrical inquiries. However, all that can be done in this way must, in its own nature, be more imperfect than even the account of the *desiderata*: for it is evident, that he who is able to teach others to make discoveries might make them himself. Notwithstanding this, it is possible that some general observations may be of use to this purpose; such for instance as Lord Bacon makes, in his *Novum Organon*; a book which, though it contain few or no philosophical discoveries itself, has contributed not a little to the discoveries contained in others. A few such general observations, confined to the subject of electricity, I shall endeavour to suggest in this place.

IT

It is an observation which the progress of science daily confirms, that all truths are not only consistent, but also connected with one another. The observation has, with no small appearance of justice, been extended even to the arts; there being no two of them so remote, but that some of the methods and processes used in the one have some analogy to some that are used in the other. Hence the knowledge of one art or science is subservient to the knowledge of others; and no person can presume that he is perfectly master of any one, till he has received all the assistance he can from, at least, all its sister arts or sciences.

INDEED the very existence of the various arts and sciences is almost a demonstration of their relation to each other. For it were highly unreasonable to suppose, that the elements of any new art or science were discovered by means independent of the study or practice of those already known. As it is by easy transitions that we pass from one part of any particular science to another, so it is by transitions equally easy that mankind have passed from one distinct science to another. Consequently, to those previously discovered arts and sciences must we have recourse, in order to understand the full evidence, on which the first principles of any new art or science rest.

ELECTRICITY is by no means an exception to this general rule. It has its sister sciences as well as others. In pursuance of them were its own principles first discovered; and the further prosecution of electrical experiments has shown its connection with more sciences than it was at first apprehended to have any relation to. Now the study of all these cannot but, reciprocally, contribute to perfect and extend the knowledge of electricity.

GILBERT, the first of modern electricians, was led to make his

his electrical experiments by their relation to those of magnetism, into which he was professedly inquiring. The study of chymistry seems to have led Mr. Boyle to attend to electricity, as well as to other occult qualities of particular bodies. Electric light was considered by all those who first observed it as a species of phosphorus; and with this view was Mr. Hawkesbee conducted in all the experiments he made upon it.

THESE, and other discoveries in electricity, having been made thus indirectly, excited the attention of philosophers to the subject, and induced them to sit down to the study of it in a direct and professed manner. Upon this it soon appeared, that electricity was no secondary, or occasional, but a principal, and constant agent in the works of nature, even in some of its grandest scenes; and that its agency, far from being confined to bodies of a particular class, extended its influence to all without exception; that the mineral, vegetable, and animal world, with the human frame in a particular manner, were all subject to its power; and that electrical experiments and principles enter into the most interesting arts and sciences which have them for their object. We also see every day, that electricity is extending itself still more into the subjects of other sciences, both by means of the analogy of their operations, and also by their reciprocal influences.

ON these accounts, to be an electrician at present, requires a much more extensive fund of various knowledge than it did but ten years ago; and a man must have a very comprehensive knowledge of nature in all its known operations, before he can reasonably expect to make any further discoveries. For it can only be by applying electricity to various parts of nature, and by combining its operations with other operations, both of nature and art, that any thing new can be found out. Almost all that

can be done by the common electrical machines, and the usual apparatus of them, has been done already; so that we must look farther in quest of new discoveries. I hope, therefore, that I shall be excused, if I endeavour to give a hint of that kind of knowledge which, I apprehend, may be peculiarly subservient to improvements in electricity, and furnish views and materials for new experiments.

NATURAL PHILOSOPHY, cannot but be of the greatest use for this purpose; but, of all its branches, none promises to be of more use to the electrician than CHYMISTRY. Here seems to be the great field for the extension of electrical knowledge: for chymistry and electricity are both conversant about the latent and less obvious properties of bodies; and yet their relation to each other has been but little considered, and their operations hardly ever combined; few of our modern electricians having been either speculative or practical chymists.

AMONG other branches of Natural Philosophy, let the doctrine of LIGHT AND COLOURS be also particularly attended to. It was this that Newton thought would be the key to other, at present, occult properties of bodies.

LET particular attention be also given to every thing that the imperfect state of Natural Philosophy furnishes respecting the ATMOSPHERE, its composition and affections. The phenomena of lightning show the connection of this subject with electricity; and probably, electricity may be our key to a much more extensive knowledge of meteorology than we are yet possessed of.

THE shock of the Leyden phial, the discovery of the sameness of lightning and electricity, together with the cure of several diseases by electrical operations, are sufficient to convince us of the singular importance of the study of ANATOMY, and every

every thing relating to the animal economy to an electrician. And had physicians more generally attended to electricity, as an article of the materia medica, many more important and useful discoveries might, no doubt, have been made. Enow, however, have been made to excite us to farther inquiries.

ÆPINUS has lately given us an excellent specimen of what use MATHEMATICS, and especially algebraical calculations may be of to an electrician, and their use, will probably, in time, be found still more extensive.

As electricity has much to expect from several branches of Natural Philosophy, so it will be ready, in its turn, to lend its assistance to them. It already supplies arguments and proofs of some principles in Natural Philosophy, which strengthen those that are drawn from other quarters. By electricity, as well as by the principles of light and colours, we can demonstrate, that it requires a considerable force to bring bodies which are contiguous to one another, and even lie upon one another, into actual contact; and the moisture of the air may perhaps be shown to more exactness by Mr. Canton's electrical balls than by any other hygrometer whatever. But I do not mean to pursue this subject, and only mention these cases by way of example.

UPON the subject of the proper furniture for an electrician, I think it may be justly added, that a knowledge of MECHANICS will be useful to him; by which I mean, upon this occasion, not only the theory, but in some measure the practice too. For without some mechanical knowledge of his own his electrical machinery will be very often out of order, and but ill answer his purpose.

IF, indeed, a person mean nothing more than to amuse himself and his friends with the experiments that have been made

by others (and this is a method of amusement which I am far from discouraging) the machines he may purchase, ready constructed to his hands, will answer his purpose very well; and the directions which are usually given along with the machines will enable him to perform the common experiments with tolerable certainty: and if any damage should happen to his apparatus, any mathematical instrument maker (if he happen to live in or near a large town) can readily repair it for him. But if a man propose to study the subject of electricity as a philosopher, with a view to extend the knowledge of it, the assistance of others will not be sufficient for him.

THE common electrical machines, and the usual electrical apparatus, will enable a person to do little more than exhibit the common experiments. If he propose to go further, he must diversify his apparatus; he must often alter the construction of his machines, and will find that common workmen cannot execute any thing out of their usual way, without more than general directions. Besides, unless a person be fortunately situated, workmen of every kind cannot always be at hand, to do every little thing he may want in the mechanical way, whenever he may happen to get a hint of a new experiment that requires it.

AN electrician, therefore, ought never to be without the common tools of a *cabinet maker*, *clock*, and *watch maker*, at least, and know, in some measure, how to use them. With respect to glass, he ought, by all means, to learn the use of a *blow pipe*, the method of drawing out and bending glass tubes, and performing, with some degree of dexterity, other operations upon glass, which he will want to use in a great variety of forms. An electrician, thus furnished, will be able, upon any occasion, to serve himself: and the slowness and blunders of mechanics do
but

but ill suit with the ardour of persons engaged in philosophical inquiries.

IT were much to be wished, that philosophers would attend more than they do to the construction of their own machines. We might then expect to see some real and capital improvements in them; whereas little can be expected from mere mathematical instrument makers; who are seldom men of any science, and whose sole aim is to make their goods elegant and portable.

FORMERLY, indeed, philosophers were obliged to construct their own machines. Mr. Boyle, Mr. Hawkesbee, and Dr. Defaguliers would have done nothing by giving tradesmen orders for what they wanted. There were no such things to be had. Necessity therefore drove them to the study and practice of mechanics, and from their contrivances are derived almost all the philosophical instruments which are now in use.

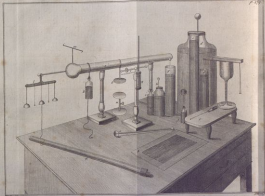
EVERY original genius, like them, must, in this respect, follow their steps. He will extend his views beyond the power of the present machinery, which can only be adapted to the present state of science. And, I think, one principal reason of the imperfect state of several branches of electrical knowledge with us, may be evidently traced to some general imperfections in the structure of all our common machines in England; which render several kinds of experiments very difficult, or almost impossible to be made; as may be shown in the next part of the work; in which I shall treat at large of the construction of machines, and give the best directions I am able for using them.

LASTLY, if an electrician intend that the public should be benefited by his labours, he should, by all means, qualify himself to draw according to the rules of PERSPECTIVE; without which he will often be unable to give an adequate idea of his experiments

ments to others. There is so much beauty in the rules of this ingenious art, and so much pleasure in the application of them, that I cannot help wondering, that all gentlemen of a liberal education do not take the small degree of pains, that is necessary to make themselves masters of it. All the mechanical methods of drawing, especially where a great number of right lines are used, as in drawing machines, &c. are exceedingly imperfect, and insufficient. They admit not of half the variety of perspective drawings. They can hardly ever be near so correct: besides that, I know by experience, they take up much more time, and the operation is exceedingly slavish and troublesome.



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P A R T V.

OF THE CONSTRUCTION OF ELECTRICAL MACHINES,
AND THE PRINCIPAL PARTS OF AN ELECTRICAL APPA-
RATUS.

SECTION I.

GENERAL OBSERVATIONS ON THE CONSTRUCTION OF AN
ELECTRICAL APPARATUS.

IMPROVEMENTS in electrical machines have, as might well be expected, kept pace with improvements in the science of electricity. While nothing more than electrical attraction and repulsion were known, nothing that we should now call an *electrical apparatus* was necessary. Every thing that was known might be exhibited by means of a piece of amber, sealing wax, or glass; which the philosopher rubbed against his coat, and presented

presented to bits of paper, feathers, and other light bodies that came in his way, and cost him nothing.

To give a greater degree of friction to electric substances Otto Guericke and Mr. Hawkesbee contrived to whirl sulphur and glass in a spherical form; but their limited knowledge of electricity did not suggest, or require the more complex structure of a modern electrical machine: Mr. Hawkesbee's contrivances indeed, were excellent, and the apparatus for many of his experiments well adapted to the purposes for which they were intended.

WHEN no further use could be made of globes, philosophers had recourse to the easier and cheaper apparatus of glass tubes, and sticks of sulphur or sealing wax; and the first conductors they made use of were nothing more than hempen cords supported by filken lines. To these, bars of metal were soon substituted. After that, recourse was again had to the globe, as much more convenient to give an uniform supply of electric matter to these insulated conductors; and, in due time, a rubber was used to supply the place of a human hand.

THE discovery of the Leyden phial occasioned still more additions to our electrical apparatus; and the more modern discoveries of Dr. Franklin and others have likewise made proportionable additions highly requisite. No philosopher, for instance, can now be satisfied, if he be not able to supply a conductor from the clouds, as well as from the friction of his glass globes or tubes. But having already marked the progress of improvements in electrical machines, as well as in electrical science, I shall content myself with this brief recapitulation, and proceed to describe what experience (in many cases dear bought) has taught me to think the best method of constructing machines, and to lay down the best rules for conducting electrical operations.

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NOTWITHSTANDING globes or cylinders are now of the most extensive use in electrical experiments, GLASS TUBES are, nevertheless, most convenient for several purposes, and no electrician ought to be without them. They should be made as long as a person can well draw through his hand at one stroke, which is about three feet, or something more; and as wide as he can conveniently grasp. The thickness of the metal is not material, perhaps the thinner they are the better, if they will bear sufficient friction; which, however, needs only to be very gentle, when the tube is in good order. It is most convenient to have the tube closed at one end: for, besides that the electric matter is thereby retained best on its surface, the air may more easily be drawn out of it, or condensed in it, by means of a brass cap fitted to the open end. A tube thus furnished is requisite for various experiments. [*a. Pl. II.*]

THE best rubber that has yet been found for a smooth glass tube is the rough side of black oiled silk, especially when a little amalgam of mercury and any metal, is put upon it.

AN electrician should be furnished with rough glass tubes, *i. e.* tubes with their polish taken off, as well as with smooth ones; but a cylinder of baked wood will do nearly as well. The best rubber for a rough glass tube, or a cylinder of baked wood, as well as for a stick of sulphur or sealing wax, is soft new flannel; or rather skins, such as hare skins, or cat skins, tanned with the hair on, being smoother, and having a more exquisite polish.

ELECTRICIANS are not quite agreed whether the preference is, upon the whole, to be given to GLOBES, or CYLINDERS. In favour of cylinders it is said, that more of their surface may be touched by the rubber. On the other hand, in favour of globes, it is said, that they can more easily be blown true, so as to press the rubber equally: they may also be made larger in diameter,

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and by this means, the axis (if they have any) may be further from the excited surface: for when the *axes* are near the surface, the electric fire will seem to strike them, so that they will sometimes appear luminous in the dark, and, if they be insulated, the extremities of the axes will give sparks; which is certainly a diminution of the electric fire at the conductor.

For this reason, I would advise, that all *axes* be avoided as much as possible, having found by experience, that they are in no case whatever necessary, the largest globes being whirled horizontally, with the greatest ease, and in every respect to more advantage, with one neck than two. This method of fitting up globes also makes electrical machines much less complex, expensive and troublesome.

LET every globe intended to be thus fitted up have its neck inclosed in a pretty deep brass cap, ending in a dilated brim, of about half an inch broad, if the globe be a large one. To this neck let there be fitted a short iron axis, and on that a PULLEY; and let a space of about three quarters of an inch of the axis be left between the pulley and the cap. In this place the axis is to be supported by a strong BRASS ARM, [c. Pl. vii.] proceeding from the pillar into which the extremity of the axis is put, and in which it turns. This brass arm may be made to receive globes of any size whatever, room being left in it for pulleys of any size, that may be wanted for them.

IN this manner globes may be fixed much more truly than they can with two necks, and they are mounted with much more ease, and less expence. The weight of large globes is no objection to this method. The largest need not to weigh above eight or ten pounds, and these have been found to turn with great ease in this manner. The rubber, if it be placed under the globe, will contribute to support the weight of it.

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LET there be a hole made in the brass cap above-mentioned, in order to preserve a communication between the external air, and the air within the globe: for if the air within the globe be either rarer or denser than that without, the excitation is found to be lessened in proportion; and, judging from experience, nothing is to be apprehended from any moisture which might be supposed to insinuate itself into the globe by such a communication.

IT will be found convenient to have the axis project about an inch beyond the pillar in which it turns, [as at *d.* Pl. vii.] that a handle may be fitted to it, and that it may thereby be turned without a wheel, for the greater variety of experiments.

IF an axis be used, let both the extremities of it be carefully turned in a lathe; otherwise it will not turn without a very disagreeable rattling; and let the part within the globe be made round, and smooth, or covered with some electric substance, to prevent its taking off much of the electric virtue of the globe.

ONE of the pillars, in which these globes or cylinders with two axes are turned, should be moveable; for then a globe or cylinder of any size may be used, and they should be made high enough, and have holes at small distances quite to the top, to take two globes upon occasion, one above the other. [Plate vii.]

IT has not yet been determined by electricians what kind of glass is the fittest for electrical purposes, but the best flint is commonly used. I have not made so many experiments, as I could wish, to ascertain this circumstance; as they are both very uncertain, and expensive; but I have some reason to think that common bottle metal is fittest for the purpose of excitation: at least, the best globe I have yet seen is one that I have of that metal. Its virtue is certainly exceeding great, and I at-

tribute it in part to the great hardness of the metal, and in part to its exquisite polish. The blowing of any thing spherical in this metal, and especially the making large globes smooth is very precarious; and they can hardly be made with two necks.

THE globe above-mentioned is about ten inches in diameter, but nothing has been determined about the best size. I have used almost every size from three inches to near eighteen in diameter, without knowing what advice to give. Perhaps *ceteris paribus*, twelve or thirteen inches may be, upon the whole, as convenient as any; but much larger, if they could be whirled with the same ease, would probably do better.

IF a person chuse to have no assistant, but would turn the globe, and manage the apparatus himself, (which is, on many accounts, very desirable) it will be most convenient to have the axis of the WHEEL level with the table at which he sits. But if he chuse rather to stand all the time he is making his experiments, it should be raised proportionably higher. It will, perhaps, be most convenient to make the diameter of the wheel about eighteen inches; and the diameter of the pulleys should be such as will give them, at least, four or five revolutions for one of the wheel. For the globe should generally revolve at least four or five times in a second, which is much swifter than it can be well turned without a wheel.

THE wheel should be made moveable with respect to the frame in which the globes are hung, or the frame should be moveable with respect to the wheel, to suit the alterations which the weather will make in the length of the *string*, particularly if it be made of hemp; but worsted makes an excellent string, and is not so apt to alter with the weather, and a leathern strap is perhaps better than either. If the distance between the wheel and the pulley cannot be altered, the operator must occasionally
moisten

moisten his hempen string, in order to make it tighter, which is, on many accounts, very inconvenient. Several grooves in the same wheel are very useful, and almost necessary, if more than one globe be used at the same time. If a flat leathern strap be not used they should be cut sharp at the bottom; as should also the grooves in the pulleys, that the string may lay faster hold of them, and that strings of different sizes may be used.

THE best RUBBERS for globes or cylinders are made of red basil skins, particularly the neck part of them, where the grain is more open, and the surface rather rough. That the rubber may press the globe equally, it should be put upon a plate of metal bent to the shape of the globe, and be stuffed with any thing that is pretty soft. Bran is good; and if the stuffing be a conductor, as flax, it will be better than if it be a non-conductor, as hair, or wool. It should rest upon a spring, to favour any inequality there may be in the form of the globe or cylinder. The best position of the rubber, for a variety of purposes, is an horizontal one, but it should be capable of being placed in every variety of horizontal position; and the spring which supports the rubber should be made to press more or less at pleasure. The rubber should be made nearly as large one way as the other. If it be made very narrow, some parts of the globe will pass it without a sufficient friction. To remedy that inconvenience, the hand (if it be dry) may be held to the globe, just before the rubber, to add to its breadth; but that posture is very inconvenient.

IT is advisable that there be no sharp edges or angles about the rubber, for that would make the *insulation* of it (which is a matter of great consequence) ineffectual. By the insulation of the rubber every electrical experiment may be performed with the twofold variety of positive and negative, and a conductor be made

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to give or take fire at pleasure. This insulation is best made by means of baked wood, in the form of a plate; five or six inches in diameter, [*g.* Pl. vii.] interposed between the metallic part of the rubber and the steel spring that supports it. When positive electricity is intended to be produced, a chain [*n.* Pl. vii.] must connect the rubber with the floor; but when negative electricity is wanted, the chain must be removed, and hung upon the common conductor, while another prime conductor must be connected with the rubber; which will therefore be electrified negatively.

THE best method of collecting the electric fire from the globe seems to be by three or four pointed wires, [*m.* Pl. vii.] two or three inches long, hanging lightly upon the globe; and neither so light as to be thrown off the globe by electrical repulsion (which would occasion a loss of the electric matter) nor so heavy as to prevent their separating to a proper distance, and being drawn backwards or forwards, as the most effectual discharge of the fire, accumulated on different parts of the globe, may require. For this purpose they are best suspended on an open metallic ring. Needles with fine points do admirably well.

It is requisite, for a variety of uses, that the PRIME CONDUCTOR be fixed very steady. It ought not, therefore, to hang in silken strings, but have a solid support; and, of all others, the preference is to be given to baked wood, being found by experience to make the most perfect insulation, and being the cheapest, and best in all other respects. The only inconvenience attending it is, that it requires to be baked again now and then, especially if it be kept in a damp situation; and a hollow pillar of glass lined with sealing wax will answer exceeding well, and doth not require so much attention.

FOR common purposes a small conductor is most convenient,
but

but where a strong spark is wanted, it is proper to have a large conductor at hand, which may be occasionally placed in contact with the smaller, and be removed from it at pleasure. But whatever be the size of a prime conductor, the extremity of it, or that part which is most remote from the globe, should be made much larger and rounder than the rest: [*A. Pl. vii.*] for the effort of the electric matter to fly off is always the greatest at the greatest distance from the globe.

As the electrician will have frequent occasion to insulate various bodies, I would advise that he make all the stands and stools which he uses for that purpose of baked wood. It may easily be turned into any form, it insulates better than glass, and is not so brittle. But care should be taken that the wood be thoroughly baked, even till it be quite brown. It will not then be found very apt to collect moisture from the air. If it should, a little warming and rubbing will be sufficient to expel the moisture again. At most it can only be necessary to boil it in linseed oil, or give it a slight coating of varnish after it has come hot out of the oven. If this preparation be used, it must be well heated once more, immediately after the boiling.

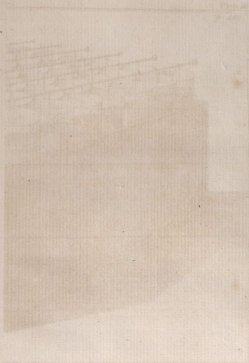
THE electrician, having thus constructed his machine, will want METALLIC RODS, [*s. Pl. ii.*] to take sparks from his conductor for various uses. These should have knobs, larger or smaller in proportion to the curvature of the conductor. If the knob be too small, it will not discharge the conductor at once, but by degrees, and with a less sensible effect; whereas the spark between broad surfaces is thick and strong.

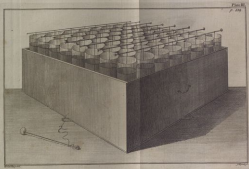
THE more formidable part of an electrical apparatus consists in the COATED GLASS, that is used for the Leyden experiment. The form of the plate is immaterial with respect to the shock;
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and, for different experiments, both plates of glass, and jars, of various forms and sizes, must be used. For common uses, the most commodious form is that of a jar, as wide as a person can conveniently hold by grasping, and as tall as it will stand without any danger of falling; perhaps about three inches and a half in diameter, and eight inches in height. The mouth should be pretty open, that it may be the more conveniently coated on the inside, as well as the outside, with tinfoil: but it will generally be most convenient to have the mouth narrower than the belly; for then it may be more easily kept clean and dry, and the cork, when one is wanted, will be easier to manage. But no electrician would chuse to be without a great number of jars of various sizes and forms. A considerable variety may be seen in plate ii. fig. *c, d, e, f, g, b, i, j, k*. The form of a coated plate of glass is represented at *b*, in the same plate.

THE method of coating is much preferable to that of putting water or brass shavings into the jars, which both makes them very heavy, and likewise incapable of being inverted, which is requisite in many experiments. Brass dust, however, or leaden shot is very convenient for small phials. These serve very well where it is necessary to remove the coating as soon as the jar is charged, but, for this purpose, quicksilver will generally answer the best. The tinfoil may be put on either with paste, gum water, or bees wax. To coat the insides of vessels, which have narrow mouths, moisten the inside with gum water, and then pour some brass dust upon it. Enough will stick on to make an exceeding good coating; and if nothing very hard rub against it, it will not easily come off. This brass dust, which is extremely useful in a great variety of electrical experiments, may be had at the pin-makers.

IN the construction of an ELECTRICAL BATTERY I would not recommend





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recommend very large jars. A number of smaller are preferable on several accounts. If one of these break by an explosion, or be cracked by any accident, the loss is less considerable; besides, by means of narrow jars, a greater force (that is a greater quantity of coated surface) may be contained in less room; and, as narrow jars may be made thinner, they will be capable of being charged higher in proportion to their surface than large jars, which must necessarily be made thick. The largest jars that the glass-men can conveniently make are about seventeen inches in height; and they should not be more than three in diameter, and of the same width throughout. Thus they may be easily coated both within and without, and a box of a moderate size will contain a prodigious force: for the jars being coated within two inches of the top, they will contain a square foot of coated glass a piece.

THE first battery that I constructed for my own use, consisted of forty one jars of this size; but a great number of them bursting by spontaneous discharges, I constructed another, which I much prefer to it, and of which a drawing is given Plate III. It consists of sixty four jars, each ten inches long, and two inches and a half in diameter, coated within an inch and a half of the top. The coated part of each is half a square foot; so that the whole battery contains thirty two square feet. The wire of each jar has a piece of very small wire twisted about the lower end of it, to touch the inside coating in several places; and it is put through a pretty large piece of cork, within the jar, to prevent any part of it touching the side, which would tend to promote a spontaneous discharge. Each wire is turned round, so as to make a hole at the upper end; and through these holes a pretty thick brass rod with knobs is put, one rod serving for one row of the jars.

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THE communication between these rods is made by laying over them all a chain, which is not drawn in the plate, lest the figure should appear too confused. If I chuse to use only part of the battery, I lay the chain over as many rods as I want rows of jars. The bottom of the box, in which all the jars stand, is covered with tinfoil and brass dust; and a *bent wire*, touching this tinfoil, is put through the box, and appears on the outside, as in the plate. To this wire is fastened whatever is intended to communicate with the outside of the battery; as the piece of small wire in the figure; and the discharge is made by bringing the brass knob to any of the knobs of the battery.

THIS is the battery which I have generally used in the experiments related in the last part of this work; though, when I have wanted a very great force, I have joined both the batteries, and even several large jars to them. And it will perhaps be allowed to be some evidence of the goodness of this construction, that after using it so much, I see no reason to wish the least alteration in any part of it. Were I to construct another battery, I should take jars of the very same size, and dispose them in the very same manner.

THE glass of which this battery is made is what the workmen call flint-green; which I think is much better for this purpose than the best flint; as I find jars made of it not so apt to discharge themselves. Besides it is much cheaper than flint.

To discover the kind and degree of electricity, many forms of ELECTROMETERS have been thought of, as the reader may have perceived in the course of the history, but this business is still imperfect. Mr. Canton's balls are of excellent use both to discover small degrees of electricity, to observe the changes of it from positive to negative, and *vice versa*; and to estimate the

the force of a shock before the discharge, so that the operator shall always be able to tell, very nearly, how high he has charged his jars, and what the explosion will be whenever he chuses to make it.

MR. CANTON'S BALLS (represented on a glass, standing on the stool [*c* Plate ii.] are only two pieces of cork, or pith of elder, nicely turned in a lathe, to about the size of a small pea, and suspended on fine linen threads. It is convenient to have these in small boxes for the pocket; the box being the full length of the strings, that they may lie there without being bent.

MR. KINNERSLEY'S electrometer described p. 204, is useful to ascertain how great shocks have been, and for many curious experiments in electricity. A drawing is given of it [*r* Plate ii.] but the glass tube is represented as much shorter than it was made by Mr. Kinnerley. I think it in general more convenient; as the bore of the small tube may easily be proportioned to it. But if a person get one long tube, of the same size throughout, it may be cut into different lengths, and the same brass caps will fit any part of it.

AT the top of the stand of baked wood which supports Mr. Kinnerley's electrometer, I have fixed another, contrived by Mr. Lane, to give a number of shocks, all of precisely the same degree of strength. It consists of a brass rod furnished with a knob, which, by means of a fine screw, may be fixed at any distance from the prime conductor, or any other fixed body communicating with the inside of a jar or battery. In consequence of this, the jar or battery, with which it is connected, can be charged no higher than the distance at which those fixed bodies are placed will permit, for at that height of the charge,

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the explosion will always be made between them. See Mr. Lane's more particular description of this useful instrument.*

To the account of these articles of an electrical apparatus, which must be used within doors, it will not be wholly insignificant to add, that a strong firm table is highly requisite. For if the TABLE on which the apparatus is disposed be apt to shake, a great number of experiments cannot be performed to advantage.

In order to repeat the noble experiment of the sameness of the electric fluid with the matter of lightning, and to make further observations on the electricity of the atmosphere, the electrician must be provided with a MACHINE FOR DRAWING ELECTRICITY FROM THE CLOUDS. For the best construction of such a machine, take the following directions. On the top of any building (which will be the more convenient if it stand upon an eminence) erect a pole, [*a* fig. 2. Pl. 1.] as tall as a man can well manage, having on the top of it a solid piece of glass or baked wood, a foot in length. Let this be covered with a tin or copper vessel [*b*] in the form of a funnel, to prevent its ever being wetted. Above this let there rise a long slender rod [*c*] terminating in a pointed wire, and having a small wire twisted round its whole length, to conduct the electricity the better to the funnel. From the funnel make a wire [*d*] descend along the building, about a foot distance from it, and be conducted through an open sash, into any room which shall be most convenient for managing the experiments. In this room let a proper conductor be insulated, and connected with the wire coming in at the window. This wire and conductor, being completely insulated, will be electrified whenever there is a considerable quantity of electricity in the air; and notice will be given when it is properly charged, either by Mr. Can-

* Phil. Transf. Vol. 57. p. 451.

ton's balls, hung to it, or by such a set of bells as will be described hereafter.

To make these experiments in perfect safety, the electrified wire should be brought within a few inches of a conducting rod, that serves to guard the house, that the redundant electricity may pass off that way, without striking any person that may happen to stand near it. The conductor to guard the house should consist of one rod, between one fourth and one half of an inch thick, if it be of iron, but smaller if it be brass or copper, terminating upwards in a sharp point, about four or five feet above the highest part of the building; and below, it should, if possible, be continued to some well or running water. Otherwise it should be sunk several feet into the ground, at the distance of some yards from the building. It is of no consequence whether this conducting rod be fastened on the inside, or outside of the house, or how many bendings are made in it.

If the electrician be desirous of making experiments upon the electricity of the atmosphere to greater exactness, he must raise a kite, by means of a string in which a small wire is twisted. The extremity of this line must be silk, and the wire must terminate in some metallic conductor, of such a form as shall be thought most convenient. Mr. Romas's experiment will perhaps convince my reader, that it may be dangerous to raise this kite at the approach of a thunder storm; and upon this occasion the common apparatus above described for drawing electricity from the clouds will, probably, answer his purpose well enough.

BUT, with the following apparatus, I should apprehend no great danger in any thunder storm. Let the string of the kite [a. fig. 3. Pl. i.] be wound upon a reel [b] going through a slit in a flat board, fastened at the top of it; by which more or less of

of the string may be let out at pleasure. Let the reel be fixed to the top of a tin or copper funnel [c] such as was described above; and from the funnel let a metallic rod [d] with a large knob be projected, to serve for a conductor. This funnel and reel must be supported by a staff [e] the upper end of which, at least, must be well baked; and the lower end may be made sharp, to thrust into the ground, when the kite is well raised.

THE safety of this apparatus depends upon the chain [f] fastened to the staff, by a hook a little below the funnel, and dragging on the ground: for the redundant lightning will strike from the funnel to the chain, and so be conducted as far as any one chuses, without touching the person who holds the staff.

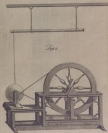
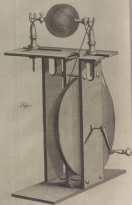
SPARKS may be taken from the conductor belonging to this apparatus with all safety, by means of a small rod of baked wood [a. fig. 4.] furnished with a small funnel [b] and a brass rod [c] and a chain connected with it: for the lightning which strikes the rod, will pass by the funnel and the chain, without touching the person who holds the rod.

MR. HARTMAN, in the construction of his apparatus for observing the electricity of the atmosphere with safety, makes use of long silken strings to support his metallic rod. These, therefore, require a large shed, fastened to the rod above them, to keep them dry. And, lest the rain that falls upon this shed should carry off all the electricity, he makes a channel all round, which receives the rain; and thence he conveys it, under the shed, into an-insulated receptacle. * But I cannot help thinking this complex apparatus unnecessary, especially if a solid stick of glass and small cover be used, instead of the silken strings and large shed.

* Anmerkungen, &c. p. 38.



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SECTION II.

A DESCRIPTION OF SOME PARTICULAR ELECTRICAL MACHINES, WITH OBSERVATIONS ON THEIR PRINCIPAL ADVANTAGES AND DEFECTS.

AFTER this general account of the construction of electrical machines, and the principal parts of an electrical apparatus, my reader may perhaps expect a more particular account of some of the principal varieties with which they are usually made. And though it might be presumed, from what has been advanced upon that head, that any person might judge for himself, I shall endeavour to gratify those who are willing to provide themselves with an electrical machine, by giving drawings and descriptions of some of the best constructions that have fallen under my notice, observing what I apprehended to be their several advantages and defects.

I SHALL begin with Mr. Hawke's machine [Plate 4. fig. 1.] which is an excellent construction considering the state of electricity in his time. The drawings annexed will render a very particular description of this, or the other machine, unnecessary. This has no rubber, no prime conductor, or field for

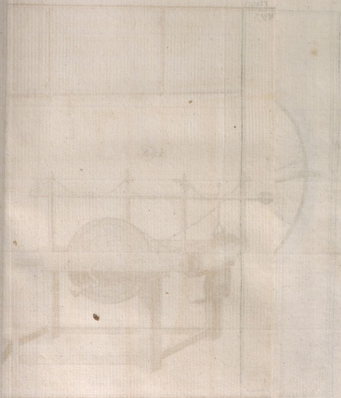
for making experiments; for no such things were wanted in his time: but it may be easily accommodated with them all. A conductor may hang from the ceiling, a rubber may be supported by a spring fixed under the globe; and a table placed near the machine, may receive the apparatus necessary for making experiments. The inconveniences of this construction are, that the operator cannot well turn the wheel himself. A servant is therefore necessary, who must sit to his work. The machine admits only of one globe, or cylinder, which must have two necks; though it admits of a considerable variety of such, and it is by no means portable.

THE Abbé Nollet's machine [Plate iv. fig. 2.] resembles the greatest number of the electrical machines that were used about the time that the Leyden phial was discovered. These were the machines, heavy and unwieldy as they seem, that were generally carried from place to place, when electrical experiments made a gainful business, and would bear the expence of the conveyance.

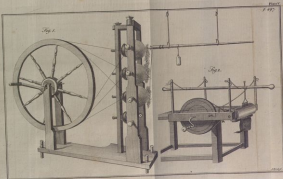
IN those early times, electricians had no idea, that it was possible to make the globe revolve too swiftly. They, therefore, made their wheels exceeding large, and the frame of the machine proportionably strong. The globe was generally rubbed by the hand, the conductor was a bar of iron, or generally a gun barrel, suspended in filken lines from the top of the room, and the apparatus was disposed on an adjoining table.

THESE machines are now universally laid aside, being more fit for a large laboratory than a private study. Besides they necessarily require an assistant, and do not admit of half the variety in the disposition of the principal parts of the construction, which the variety of experiments now demands.

ABOUT the time that Mr. Boze's beatification was talked of, electricians



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electricians were very desirous of exciting a very great power of electricity; though, having no method of accumulating, or preserving it, it was dispersed as soon as raised. The machine represented in Plate v. fig. 1. was a contrivance of Dr. Watson's, to whirl four large globes at a time, and unite the power of them all.

I CANNOT help regretting that no such machines as these are constructed at this day, when, by means of electrical batteries, so great a power might be preserved, and employed to the greatest purposes. I wish the Doctor would refit the machine here described, if it be yet in being, and construct a battery proportioned to it. But I should rejoice more to see a machine moving by wind or water, turning twenty or thirty globes, and charging electrical batteries adequate to them. I make no doubt but that a full charge of two or three thousand square feet of coated glass would give a shock as great as a single common flash of lightning. They are not philosophers who will say, that nothing could be gained, and no new discoveries made by such a power.

PLATE v. fig. 2. exhibits a machine which Mr. Wilson constructed, about the time above-mentioned. It is much more commodious than any that had been contrived before, as all the parts are brought within a moderate compass; so that the same person may turn the wheel, and conduct the experiments. One of the most complete machines, and one that had the greatest power of any that I have seen, except my own, is of this construction.

ITS inconveniences are, that it admits but little variety of globes or cylinders, and both these and the rubber are not sufficiently distant from other bodies. The rubber is not insulated, and the conductor is unsteady. This machine has a frame standing upon the ground, but the general construction may be pre-

served, and the machine be made to screw to a table. Some I have seen which, by this means, were made very portable; and a box was contrived in the inside, to contain the apparatus.

OF the more modern constructions (of which there is an endless variety) the more elegant are those in which the globe is turned by tooth and pinion. This reduces the wheel work, contained in the box [*a* Pl. vi. fig. 1.] to an exceeding small compass, and gives the workmen an opportunity of making the machine all in brass, very elegant, and portable. But I object to them, as liable to accidents, which electricians in general cannot easily repair; and I would wish philosophers to be as independent as possible of all workmen. Of this more elegant kind of machines, is that made by Mr. Nairne, a drawing of which may be seen in this plate. The conductor belonging to machines of this construction is generally hung in silk, supported, either by wooden pillars, in a frame, as in the figure annexed, or by two brass arms extending from the machine.

THESE machines are certainly very commodious for screwing to a table. They require no assistant, and they admit of the experiments being made in a sitting posture; which is a great recommendation of a machine, to those persons who chuse to do things with little trouble, and who are fond of a studious sedentary life. This construction admits of very little variety in the size or number of globes, and hardly of a glass vessel of any other form. But the greatest inconvenience attending it, is the upright position of the globe and rubber, whereby every thing put upon it is apt to slip down; and the rubber is not insulated.

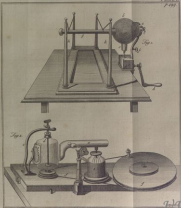
AT Mr. Martin's I have seen a machine, in which a cylinder was made to revolve between two brass pillars, by means of a wheel and pinion, concealed in a neat box. In this construction

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struction the axis of the cylinder, and the rubber, was horizontal; in which respect it is preferable to that mentioned above; but if the machine were placed upon a table, the handle seemed to be too high to admit of its being used in a sitting posture.

In the machine represented in plate vi. fig. 2. and which was invented by Mr. Read, mathematical instrument maker at the quadrant in Knightsbridge, Hyde park; a cylinder stands perpendicular to the horizon, supported by a brass bow, which receives the upper end of the axis; and motion is given to it by means of a pulley, at the lower end of the axis, and a wheel, which lies parallel to the table. The conductor [a] is furnished with points to collect the fire, and it is screwed to the wire of a coated jar, [b] standing in a socket, between the cylinder and the wheel. One machine of this kind I have seen, in which the cylinder and the wheel were not separated by the conductor.

THIS construction is peculiarly useful to physicians and apothecaries; and, with Mr. Lane's electrometer [c] annexed to it, (the figure of which he has given me leave to insert in the drawing annexed, taken from his own machine) as many shocks as are requisite may be given, of precisely the same, and any degree of force, without any change of posture, either in the patient, or the operator; who has nothing to do but turn the wheel, without so much as touching any other part of the apparatus.

WHEN this machine is used for simple electrification, and other purposes where the shock is not required; the coated jar must be taken away, and another jar, without any coating, put in its place. By this means the conductor is fixed, which is a very great advantage, and which few machines are possessed of.

But these machines, besides that they admit no variety of globes or cylinders, and no insulation of the rubber, require a motion of the arm, which I should think not quite easy.

THE ingenious Dr. Ingenhaus has constructed a machine in which friction is not given to any kind of hollow glass vessel whatever; but to a *circular plate of glass*, generally about nine inches in diameter.

THIS plate turns vertically, and rubs against four cushions, each an inch and a half long, placed at the opposite ends of the vertical diameter. The conductor is a brass tube, has two horizontal branches coming from it, reaching within about half an inch of the extremity of the glass; so that each branch, takes off the electricity excited by two of the cushions.

THIS construction is original and ingenious, but the cushions cannot easily be insulated, and a plate of glass is much more liable to injuries than a globe, or even a cylinder.

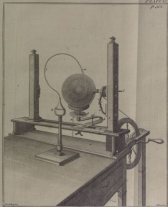
IN the first edition of this work, I mentioned one Mr. Ramsden as the inventor of this construction; a mistake, which he himself led me into.

MR. WESLEY's people, I believe, generally use a machine in which two cylinders are turned by the same wheel: but one that I saw, in the possession of a very intelligent person of that persuasion, had the cylinders and rubbers so confined in a chest, that, though it might do very well for medical uses, it was very ill adapted to the purposes of philosophy.

BUT the machine which I would advise a philosopher to construct for his own use, is that of which a drawing is given Plate vii. This construction is the result of my best attention to this subject. I have used it above six months (how much I leave the reader to imagine) without seeing the least reason to make any alteration of consequence in it; and believe it to have almost all the advantages,

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Fig. 100
p. 100



UVA. BHSC

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advantages, which an electrical machine, designed for the closet, can have. The reader will, therefore, allow me to be a little longer in the description of it than I have been of the rest.

THE FRAME consists of two strong boards of mahogany [*a a*] of the same length, parallel to one another, about four inches asunder; and the lower is an inch on each side broader than the upper. In the upper board is a *groove*, reaching almost its whole length. One of the *pillars* [*b*] which are of baked wood, is immovable, being let through the upper board, and firmly fixed in the lower, while the other pillar slides in the groove above-mentioned, in order to receive globes or cylinders of different sizes; but it is only wanted when an axis is used. Both the pillars are perforated with *boles* at equal distances, from the top to the bottom; by means of which globes may be mounted higher or lower according to their size; and they are tall, to admit the use of two or more globes at a time, one above the other. Four of a moderate size may be used, if two be fixed on one axis: and the wheel has several grooves for that purpose.

IF a globe with only one neck be used, as in the plate, a BRASS ARM with an open socket [*c*] is necessary to support the axis beyond the pulley; and this part is also contrived to be put higher or lower, together with the brass socket in which the axis turns. The axis [*d*] is made to come quite through the pillar, that it may be turned by another handle, without the wheel, if the operator chuses. The frame, being screwed to the table, may be placed nearer to, or farther from the wheel, as the length of the string requires, in different states of the weather. The WHEEL is fixed in a frame by itself, [*e*] by which it may have any situation with respect to the pulley, and be turned to one side,

side, so as to prevent the string from cutting itself. The hinder part of this frame is supported by a foot of its own.

THE RUBBER [*f*] consists of a hollow piece of copper, filled with horse hair, and covered with a basil skin. It is supported by a socket, which receives the cylindrical axis of *a round and flat piece of baked wood*, [*g*] the opposite part of which is inserted into the socket of a bent steel spring. [*h*] These parts are easily separated; so that the rubber, or the piece of wood that serves to insulate it, may be changed at pleasure. The SPRING admits of a twofold alteration of position. It may be either slipped along the groove, or moved in the contrary direction; so as to give it every desirable position with respect to the globe or cylinder; and it is, besides, furnished with a *screw*, [*i*] which makes it press harder or lighter, as the operator chuses.

THE PRIME CONDUCTOR [*k*] is a hollow vessel of polished copper, in the form of a pear, supported by a pillar, and a firm basis of baked wood, and it receives its fire by means of *a long arched wire, or rod of very soft brass*, [*l*] easily bent into any shape, and raised higher or lower, as the globe requires; and it is terminated by an *open ring*, in which are hung *some sharp pointed wires* [*m*] playing lightly on the globe when it is in motion. The body of the conductor is furnished with *holes, and sockets*, for the insertion of metallic rods, to convey the fire wherever it is wanted, and for many other purposes convenient in a course of electrical experiments. The conductor is, by this means, steady, and yet may be easily put into any situation. It collects the fire perfectly well, and (what is of the greatest consequence, though but little attended to) retains it equally every where.

WHEN positive electricity is wanted, a wire, or chain, as is represented in the plate [*n*] connects the rubber with the table, or the floor. When negative electricity is wanted, that wire is
connected

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Fig. 10
p. 100

UVA. BHSC

connected with another conductor such as that represented [Pl. ii.] while the conductor in Plate vii. is connected, by another wire or chain, with the table. If the rubber be made tolerably free from points, the negative power will be as strong as the positive. In this machine I do not know which is the stronger of the two.

In short, the capital advantages of this machine are, that glass vessels, or any other electric body, of any size or form, may be used, with one neck, or two necks, at pleasure; and even several of them at the same time, if required. All the essential parts of the machine, the *globe*, the *frame*, the *wheel*, the *rubber*, and *conductor*, are quite separate; and the position of them to one another may be varied in every manner possible. The rubber has a complete insulation, by which means the operator may command either the negative or the positive power, and may change them in an instant. The conductor is steady, and easily enlarged, by rods inserted into the holes, with which it is furnished, or by the conjunction of other conductors, in order to give larger sparks, &c. The wheel may be used or not at pleasure; so that the operator may either sit, or stand to his work, as he pleases; and he may, with the utmost ease, both manage the wheel and his apparatus.

The machine represented in [Pl. viii.] is constructed on the same general principles with the last. It is inferior to it in one respect, that it admits only of globes or cylinders with one neck, but these are far preferable to any other; and it is much more commodious for use, as it doth not require any strong table like the other. It consists of a pillar of mahogany [a] standing upright on three feet. This pillar divides in two places, to receive a wheel [b] in the lower part of it, and in the upper part a pulley [c] which is turned by a leathern strap [d] tightened by means
of

of a small buckle. In the center of the pulley is a strong iron spindle, turning in two firm brass sockets, fastened to each side of the pillar. In one of these sockets the extremity of the spindle turns upon a center, by means of a piece of iron [c] screwed into it, while the other is held tight by a brass clasp, which may be made to hold it closer, or more loosely, at pleasure, by means of a screw. [f] The iron spindle is made hollow in the form of a parallelepiped, in order to receive a piece of brass or iron, in which the brass cap that holds the globe [g] terminates. These are exactly fitted to one another, and by this means any globe may be taken out, and another put into the machine with very little trouble, if these parts be always made to the same pattern.

THE RUBBER [b] is separated from the spring [i] by a plate of glass [j] which effectually insulates it; but the chain [k] connects them together when positive electricity is wanted, as in the usual method of electrifying. The spring may be made to press more or less, by means of a screw; [l] and it may be raised higher or lower, to suit globes of different sizes, by means of a contrivance which is not represented in the plate.

THE PRIME CONDUCTOR *m, n, o*, is the same as in Pl. vii. From the same board which supports it arises another pillar, at the top of which is Mr. Lane's electrometer; the knob of which [p] may be placed as near to the knob opposite to it on the prime conductor [q] as is desired, by means of the graduated part. [r] But the whole of this may be taken away when it is not wanted.

WHEN negative electricity is desired, the chain [k] must be removed from the rubber, and hung upon the prime conductor, so as to connect it with the table; and a short brass rod, with a knob at the end of it, must be screwed into a small socket, which will

will be found in the rubber above the plate of glass. This brass rod will then serve for a negative prime conductor; for, in this situation, when the wheel of the machine is turned, this rod, being insulated (together with the rubber, through which all the electric fire passes to the globe) will receive sparks from whatever is presented to it, and therefore electrify negatively.

As it requires some dexterity and experience to turn the machine, standing on three feet only, without shaking it; small plates of brass, upon which the edges of heavy weights, made of lead or iron, may be placed, are fastened to two of them; but a large board may be firmly screwed under all the feet, or various other methods may be used, whereby the pillar, which supports the machine, may stand as firm as a person chuses.

P A R T VI.

PRACTICAL MAXIMS FOR THE USE OF YOUNG
ELECTRICIANS.

AS the chapter I am now entering upon is professedly designed for the use of young electricians, it is hoped that the proficient will excuse my inserting a few plain and trite maxims; which, though they be superfluous with respect to him, may not be so to all my readers. The greatest electricians (who are generally those who have had the fewest instructions) may remember the time when the knowledge of a rule or maxim, which they would perhaps smile to see in a book, would have saved them a great deal of trouble and expence; and it is hoped they will not envy others acquiring wisdom cheaper than they did. In a general treatise, every man has an equal right to expect to find what he wants; and it is for the interest of the science in general, that every thing be made as easy and inviting as possible to beginners. It is this circumstance only that can increase the number of electricians, and it is from the increase of this number that we may most reasonably expect improvements in the science.

WHEN

WHEN the air is dry, particularly when the weather is frosty, and when the wind is North, or East, there is hardly any electrical machine but will work very well. If the air be damp, let the room in which the machine is used be well aired with a fire, and let the globe and every thing about it be made very dry, and it may be made to work almost as well as in the best state of the air.

WHEN a tube is used, the hand should be kept two or three inches below the upper part of the rubber; otherwise the electricity will discharge itself upon the hand, and nothing will remain upon the tube for electrical purposes.

A LITTLE bees wax drawn over the surface of a tube will greatly increase its power. When the tube is in very good order, and highly excited, it will, at every stroke, throw off many pencils of rays from its surface, without the approach of any conductor, except what may float in the common atmosphere.

It has been the custom of many electricians to line their globes with sealing wax, or some other electric substance, in order to make them act with more ease and vigour. Mr. C. L. Epinasse gives the following receipt for an electric composition for this purpose. Take four pounds of Venice turpentine, one pound of rosin, and one pound of bees wax. Boil these over a gentle fire, stirring them now and then, for four hours; at the end of which stir in one quarter of a pound of vermilion. Then a little of the mixture being taken out and left to cool, will be hard and brittle, a token that it is fit for use. Having well heated your globe or cylinder, pour the melted mixture into it, and turn it about, so as to spread it evenly over the inside surface,

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face,

face, to the thickness of a sixpence, and let it cool very gradually. *

I MAKE no doubt but that this electric lining is useful in some cases, as it was remarkably in two large globes of my own, the history of which I have given p. 555; but in the case of small globes, I cannot help thinking that Mr. L. Epinasse expects too much from it.

To increase the quantity of electric fire from a globe, moisten the rubber a little from time to time; or rather moisten the under side of a loose piece of leather, which may occasionally be put upon the rubber. But the most powerful exciter of electricity is a little amalgam, which may be made by rubbing together mercury and thin pieces of lead or tinfoil in the palm of the hand. If the rubber should be placed perpendicular to the horizon, it will be necessary to use a little tallow to make it stick. With this excellent resource, almost all states of the weather are equal to an electrician.

A LITTLE time after fresh amalgam has been put upon the globe, and often at other times, if there be any foulness upon the cushion; and sometimes when there is none, there will be formed upon the globe small black spots, of a hard rough substance; which grow continually larger, till a considerable quantity of that matter be accumulated upon the surface. This must be carefully picked off, or it will obstruct the excitation, and in a great measure defeat the electrical operations.

WHEN the amalgam has been used for some time, there will be formed upon the rubber a thick incrustation of the same kind of black substance which is apt to adhere to the globe. This incrustation is a very great improvement of the rubber. For when once a considerable body of this matter is formed, and it is a

* Phil. Transf. Vol. 57. p. 186.

little moistened, or scraped, as much fire will be produced, as if fresh amalgam were used: so that it seems to supersede the further use of the amalgam.

As the electric matter is only collected at the rubber, it is necessary that it have a communication with the common mass of the earth, by means of good conductors. If, therefore, the table on which the machine stands, or the floor of the room in which it is used, be very dry, little or no fire will be got, be the machine ever so good. In this case it will be necessary to connect the rubber, by means of chains or wires, with the floor, the ground, or even the next water, if the neighbouring ground be dry. This Dr Franklin informs me he was frequently obliged to do in Philadelphia.

WHEN the electricity of a globe is very vigorous, the electric fire will seem to dart from the cushion towards the wire of the conductor. I have seen those lucid rays (which are in part visible in day light) make the circuit of half the globe, and reach the wires: and they will frequently come in a considerable number, at the same time, from different parts of the cushion, and reach within an inch or two of the wires. The noise attending this beautiful phenomenon exactly resembles the crackling of bay leaves in the fire. Frequently these lucid arches have *radiant points*, often four or five in different parts of the same arch. These radiant points are intensely bright, and appear very beautiful. It is peculiarly pleasing to observe these circles of fire rise from those parts of the cushion where the amalgam or moisture has been put, or which have been lately scraped. Single points on the rubber will then seem intensely bright, and for a long time together will seem to pour out continual torrents of flame. If one part of the rubber be pressed closer than another, the circles will issue in that place more frequently than in any other.

WHEN

WHEN the conductor is taken quite away, circles of fire will appear on both sides the rubber, which will sometimes meet, and completely encircle the globe. If a finger be brought within half an inch of the globe, in that state, it is sure to be struck very smartly, and there will often be a complete arch of fire from it to the rubber, though it be almost quite round the globe.

THE smaller the conductor is made, the more fire may be collected from it: for there is less surface from which the fire may escape. But in charging a phial, if the wire be placed close to the conductor, the difference will be inconsiderable, whether a smaller or larger conductor be used, till it begin to be charged pretty high: for, till that time, the conductor will not have acquired any considerable atmosphere.

If the conductor be made perfectly well, and the air be dry, there will never be any loss of fire from any part of it. For when the whole surface has received as high a charge as the machine can give to it, it will, in all places alike, perfectly resist all further efforts to throw more upon it, and the circulation of the fluid by the rubber will be stopped, being balanced, as it were, by equal forces. Or if it lose in all places alike, the dissipation must be invisible. This maxim almost admits of ocular demonstration. For when the rubber is perfectly insulated, and the conductor has an opportunity of discharging itself, the rubber will take sparks from a wire placed near it very fast; but when the conductor has little opportunity of emptying itself, it will take fewer of those sparks.

To form a just estimate of the electrical power of any machine, and to compare different machines in this respect, take two wires with knobs of any size, and fix one of them at the conductor, and the other at some certain distance from it, about an inch, or an inch and a half; and when the wheel is turned,

count

count the number of sparks that pass between them during any given time. Fix the same wires to any other conductor, belonging to any other machine (but the same conductor would be more exact) and the difference between the number of strokes in any given time will ascertain the difference between the strength of the two machines.

THE larger the conductor is made, the stronger spark it will give: for the more extended the electrified surface is, the greater quantity of the electric atmosphere it contains, and the more sensible will be its effect when it is all discharged at once. The conductor, however, may be made so large, that the necessary dissipation of the electric matter from its surface into the air will be equal to the supply from the machine, which will constitute the MAXIMUM of the power of that machine, and will be different in different states of the air.

A CERTAIN degree of friction is necessary to give a globe its greatest power. A number of globes increases the power, but the increase of friction will make it more difficult for a man to excite their power. A few trials with a number of globes would enable any man to judge of the *maximum* of his strength in exciting electricity. I should imagine, from my own experience, that no person could excite much more electricity from any number of globes, than he could from one; supposing him to continue the operation an hour, or even only half an hour together.

WHEN a long conductor is used, the longest and the strongest spark may be drawn from the extremity of it, or from that part which is the most remote from the globe.

THE largest and most pungent sparks are drawn from any conductor along an electric substance. Thus if the conductor be supported by pillars of glass or of baked wood, the longest sparks will be taken close to the pillar.

IF

If the conductor bend inwards in any place, so as to make the surface concave, a peculiarly large, strong, and undivided spark may be drawn from that place. Where the surface is convex, the spark is more apt to be divided and weakened.

If a smooth cork ball be hung in a long silken string, and electrified positively, it will always be repelled by positive, and attracted by negative electricity. But the strongest repulsion will be changed into attraction at a certain distance.

If two pith balls hung by linen threads, and diverging with positive electricity, be insulated, though in connection with conductors of considerable length, the approach of a body electrified positively will first make them separate, and then (if the electricity of the balls be small, and that of the approaching body great) it will, at a certain distance, make them approach, and at length come into contact with it. Sometimes the divergence previous to the convergence is very slight, and, without great attention, is apt to be overlooked.

If the balls have a free communication with the earth, for instance, if they be held in the hand of a person standing on the ground, and (as in the former case) they be made to diverge with positive electricity, in consequence of being held within the influence of a body electrified negatively, the approach of positive electricity will make them converge, and negative electricity will make them diverge: the electric matter of the approaching body, in the former case, repelling that of the balls, and thereby, as it were, unelectrifying them; whereas, in the latter case, the negative electricity of an approaching body draws it more powerfully into the threads, and makes them diverge more. This method of judging is, therefore, excellently adapted to ascertain the kind of electricity in the atmosphere.

phere, or of a charged jar or battery, the balls being held in the hand of a person standing on the earth or the floor.

To discover smaller degrees of electricity than the balls can show to advantage, use a very fine thread, or two of them. If insulation be necessary, fasten it to a stick of baked wood. But the most accurate measure of electricity I have yet hit upon, is a single thread of silk as it comes from the worm. When the end of this has received a small degree of electricity, it will retain it a considerable time, and the slightest electric force will give it motion. Before any experiments be made, let it be carefully observed how long, in any particular situation, it will retain the degree of electricity that is intended to be given to it; and let allowance be made for that in the course of the experiments. It will retain electricity much longer, if a small piece of down from a feather be fastened to it, but it will not acquire the virtue so soon. And it will be most easy to manage, if two or three threads of silk be used, and the piece of down be so adjusted to them, that it shall but just prefer a perpendicular situation, and not absolutely float in the air at random. This electrometer is not liable to the inaccuracies of those that have a sensible weight: for as there is always a sphere of attraction within a sphere of repulsion, the weight of the electrified body will allow another to pass the boundary of those two spheres, without a sensible obstruction; but the body I am describing immediately retires, with all its spheres of attraction and repulsion about it.

THE force of the ELECTRIC SHOCK is in proportion to the quantity of surface coated, the thinness of the glass, and the power of the machine. That this last circumstance ought to be taken into consideration is evident: for different machines will charge the same jar very differently. With one machine, for

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instance,

instance, it may be made to discharge itself, when it cannot with another.

THE globe which charges the fastest, or whose electrical power is greatest, charges also highest. Therefore a number of globes will not only charge faster, but also, probably, higher than a single globe.

THE most effectual method of charging a jar is to connect the outside by means of wires, with the rubber, while the wire proceeding from the inside is in contact with the conductor. In this manner the inside of the jar will be supplied with the very same fire that left the outside. In this case also the jar will receive as high a charge as it is capable of receiving, though the rubber be insulated, and have no communication but with the outside coating; so that, in the case of charging, there can be no occasion for the directions given above, when the table, the floor of the room, or the ground are very dry.

THE greatest quantity of fire that a jar will hold is not always the quantity it will contain when it is coated just so low as not to discharge itself. In this case, indeed, the part that is coated is charged as high as it can be, but then a considerable part of the surface is not charged at all, or very imperfectly. On the other hand, if the jar be coated very high, it may be made to discharge itself with as small an explosion as one chuses. The exact *maximum* of the charge of any jar is not easy to ascertain.

THE greatest effort in a jar to make a discharge seems to be about half a minute, or a minute, after it is removed from the conductor; owing, perhaps, to non-electric dust or moisture attracted by and adhering to the glass, between the outside and inside coating; so that if there be any apprehension of its

discharging

discharging itself, it is advisable to discharge it before it has stood charged at all.

WHEN a thin jar is discharged, it is advisable not to do it by placing the discharging rod opposite to the thinnest part. It will endanger the bursting of the jar in that place.

THE more persons join hands to take a shock the weaker it is.

If two jars, of the same thickness, be used together, the stronger of them will receive no higher a charge than the weaker. If one of them, for instance, be coated so high as that it will discharge itself, either with or without bursting, after a few turns of the wheel; the other will always be discharged along with it, though it was capable of being charged ever so high by itself. The method, therefore, of estimating the force of a number of jars, is to consider each of them as capable of containing no more fire than the weakest in the company. It follows from hence, that if a single jar in a large battery have the smallest crack in the coated part of it, not one of them is capable of being charged in conjunction with it.

IN large batteries, it is advisable to coat the jars pretty high, the dissipation of the electric matter from so great a surface when the charge is high being very considerable. The battery might be made so large, as that, after a very moderate charge, the machine would be able to throw no more fire in than was exhaled, as we may say, from the surface. This would be the
MAXIMUM OF THE POWER OF THAT MACHINE IN CHARGING.

IN order to judge of the strength of a charge (which, in large batteries, is a thing of considerable consequence) present Mr. Canton's balls to the wires, from time to time. A comparison of the degree of their divergence, compared with the actual explosion,

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plosion, will soon enable the operator to tell how high his battery is charged, and what will be the force of the explosion.

IN comparing different explosions by their power to melt wires, let it be observed, that, in wires of the same thickness, the forces that melt them will be in the proportion of the lengths; and in wires of the same length, in the proportion of the squares of their diameters.

Do not expect that the explosion of a battery will pierce a number of leaves of paper in proportion to its force in other respects. That depends upon the height of the charge much more than the quantity of coated surface. I have known an explosion which would have melted a pretty thick wire not able to pierce the cover of a book, which a small common jar would have done with ease. If it had been pierced with the explosion of the battery, the hole would have been larger in proportion.

LET no person imagine that, because he can handle the wires of a large battery without feeling any thing, that therefore he may safely touch the outside coating with one hand, while the other is upon them. I have more than once received shocks that I should not like to receive again, when the wires showed no sign of a charge; even two days after the discharge, and when papers, books, my hat and many other things had lain upon them the greatest part of the time. If the box be tolerably dry, the residuum of the charge will not disperse very soon. I have known even the *residuum of a residuum* in my batteries to remain in them several days. For presently after an explosion, I seldom fail to discharge the residuum, which, in some cases, is very considerable, for fear of a disagreeable accident.

A SMALL shock passing through the body gives a sensation much more acute and pungent than a large one. I cannot boast, like Dr. Franklin, of being twice struck senseless by the electric shock;

shock; but I once, inadvertently, received the full charge of two jars, each containing three square feet of coated glass. The stroke could not be called painful, but, though it passed through my arms and breast only, it seemed to affect every part of my body alike. The only inconvenience I felt from it was a lassitude, which went off in about two hours.

MR. WILCKE was struck down senseless, by accidentally receiving, from his head to his feet, the charge of a large chymical receiver. He thought, that if he had received a shock five times as large, he should never have written the account of it. *

* Remarks on Franklin's Letters, p. 362.

PART VII.

A DESCRIPTION OF THE MOST ENTERTAINING EXPERI-
MENTS PERFORMED BY ELECTRICITY.

ELECTRICITY has one considerable advantage over most other branches of science, as it both furnishes matter of speculation for philosophers, and of entertainment for all persons promiscuously. Neither the air pump, nor the orrery; neither experiments in hydrostatics, optics, or magnetism; nor those in all other branches of Natural Philosophy ever brought together so many, or so great concourses of people, as those of electricity have done singly. Electrical experiments have, in almost every country in Europe, occasionally furnished the means of subsistence to numbers of ingenious and industrious persons, whose circumstances have not been affluent, and who have had the address to turn to their own advantage that passion for the marvellous, which they saw to be so strong in all their fellow creatures. An emperor need not desire a greater revenue than the sums which, have been received in shillings, sixpences, threepences, and twopences, for exhibiting the Leyden experiment.

AND if we only consider what it is in objects that makes them capable of exciting that pleasing astonishment, which has such charms

charms for all mankind; we shall not wonder at the eagerness with which persons of both sexes, and of every age and condition, run to see electrical experiments. Here we see the course of nature, to all appearance, entirely reversed, in its most fundamental laws, and by causes seemingly the slightest imaginable. And not only are the greatest effects produced by causes which seem to be inconsiderable, but by those with which they seem to have no connection. Here, contrary to the principles of gravitation, we see bodies attracted, repelled, and held suspended by others, which are seen to have acquired that power by nothing but a very slight friction; while another body, with the very same friction, reverses all its effects. Here we see a piece of cold metal, or even water, or ice, emitting strong sparks of fire, so as to kindle many inflammable substances; and in *vacuo* its light is prodigiously diffused, and copious; so as exactly to resemble, what it really is, the lightning of heaven. Again, what can seem more miraculous than to find, that a common glass phial or jar, should, after a little preparation (which, however, leaves no visible effect, whereby it could be distinguished from other phials or jars) be capable of giving a person such a violent sensation, as nothing else in nature can give, and even of destroying animal life; and this shock attended with an explosion like thunder, and a flash like that of lightning? Lastly, what would the antient philosophers, what would Newton himself have said, to see the present race of electricians imitating in miniature all the known effects of that tremendous power, nay disarming the thunder of its power of doing mischief, and, without any apprehension of danger to themselves, drawing lightning from the clouds into a private room, and amusing themselves at their leisure, by performing with it all the experiments that are exhibited by electrical machines.

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So far are philosophers from laughing to see the astonishment of the vulgar at these experiments, that they cannot help viewing them with equal, if not greater astonishment themselves. Indeed, all the electricians of the present age can well remember the time, when, with respect to these things, they themselves would have ranked among the same ignorant staring vulgar.

BESIDES, so imperfectly are these strange appearances understood, that philosophers themselves cannot be too well acquainted with them; and therefore should not avoid frequent opportunities of seeing the same things, and viewing them in every light. It is possible that, in the most common appearances, some circumstance or other, which had not been attended to, may strike them; and that from thence light may be reflected upon many other electrical appearances.

WHETHER philosophers may think this consideration worth attending to or not, I shall, for the sake of those electricians who are young enough, and, as it may be thought, childish enough, to divert themselves and their friends with electrical experiments, describe a number of the most beautiful and surprising appearances in electricity; that the young operator may not be at a loss what to exhibit when a company of gentlemen or ladies wait upon him, and that he may be able to perform the experiments to the most advantage, without disappointing his friends, or fretting himself.

To make this business the easier to the young operator, I shall consult his convenience in the order in which I shall relate the experiments, beginning with those which only require simple electrification, then proceeding to those in which the Leyden experiment is used, and concluding with those in which recourse must be had to other philosophical instruments in conjunction with the electrical machine.

SECTION

SECTION I.

ENTERTAINING EXPERIMENTS IN WHICH THE LEYDEN PHIAL
IS NOT USED.

THE phenomena of electrical attraction are shown in as pleasing a manner by the tube, as they can be by any methods that have been found out since the later improvements in electricity. It is really surprising to see a feather, or a piece of leaf gold first attracted by a glass tube excited by a slight friction, then repelled by it, and held suspended in the air, or driven about the room wherever the operator pleases; and the surprise is increased by seeing the feather, which was repelled by the smooth glass tube, attracted by an excited rough tube, or a stick of sealing wax, &c. and jumping from the one to the other, till the electricity of both be discharged. Nor is the observation of Otto Guericke the least pleasing circumstance, viz. that in turning the tube round the feather, the same side of the feather is always presented towards it.

BUT since electrical substances part with their electricity but slowly, the more rapid alternate attractions and repulsions are shown to the best advantage at the prime conductor. Thus pre-

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sent a number of seeds of any kind, grains of sand, a quantity of brass dust, or other light substances in a metal dish (or rather in a glass cylindrical vessel standing on a metal plate) to another plate hanging from the conductor [as at *n* and *o* Pl. ii.] and the light substances will be attracted and repelled with inconceivable rapidity, so as to exhibit a perfect shower, which, in the dark will be all luminous.

SUSPEND one plate of metal to the conductor, and place a metal stand, of the same size, at the distance of a few inches exactly under it, and upon the stand put the figures of men, animals, or whatever else shall be imagined, cut in paper or leaf gold, and pretty sharply pointed at both extremities; and then, upon electrifying the upper plate, they will perform a dance, with amazing rapidity of motion, and to the great diversion of the spectators.

IF a downy feather, or a piece of thistle down be used in this manner, it will be attracted and repelled with such astonishing celerity, that both its form and motion will disappear; all that is to be discerned being its colour only, which will uniformly fill the whole space in which it vibrates.*

IF a piece of leaf gold be cut with a pretty large angle at one extremity, and a very acute one at the other, it will need no lower plate, but will hang by its larger angle at a small distance from the conductor, and by the continual waving motion of its lower extremities, will have the appearance of something animated, biting or nibbling at the conductor. It is therefore called by Dr. Franklin the *golden fish*.

TO the dancing figures above-mentioned, it is very amusing to add a set of ELECTRICAL BELLS. These consist of three small

* Lovet, p. 28.

bells,

bells, the two outermost of which are suspended from the conductor by chains, and that in the middle by a silken string, while a chain connects it with the floor; and two small knobs of brass, to serve instead of clappers, hang by silken strings, one between each two bells. In consequence of this disposition, when the two outermost bells, communicating with the conductor, are electrified, they will attract the clappers, and be struck by them. The clappers, being thus loaded with electricity, will be repelled, and fly to discharge themselves upon the middle bell. After this, they will be again attracted by the outermost bells; and thus by striking the bells alternately, a continual ringing may be kept up as long as the operator pleases. In the dark, a continual flashing of light will be seen between the clappers, and the bells; and when the electrification is very strong, these flashes of light will be so large, that they will be transmitted by the clapper from one bell to the other, without its ever coming into actual contact with either of them, and the ringing will, consequently, cease. When these two experiments of the bells and the figures are exhibited at the same time, they have the appearance of men or animals dancing to the music of the bells; which, if well conducted, may be very diverting.

If a piece of burnt cork, about the bigness of a pea, cut into the form of a spider, with legs of linen thread, and a grain or two of lead put in it, to give it more weight, be suspended by a fine silken thread, it will, like a clapper between the two bells, jump from an electrified to an unelectrified body and back again, or between two bodies possessed of different electricities, moving its legs as if it were alive, to the great surprize of persons unacquainted with the construction of it. This is an American invention, and is described by Dr. Franklin. *

* Letters, p. 17

SEVERAL very beautiful experiments, which depend on electrical repulsion, may be shown to great advantage by bundles of thread, or of hair, suspended from the conductor, or presented to it. They will suddenly start up, and separate upon being electrified, and instantly collapse when the electricity is taken off. If the operator can manage this experiment with any degree of dexterity, the hair will seem to the company to rise and fall at the word of command.

If a large plummy feather be fixed upright on an electrified stand, or held in the hand of a person electrified, it is very pleasing to observe how it becomes turgid, its fibres extending themselves in all directions from the rib; and how it shrinks, like the sensitive plant, when any unelectrified body touches it, the point of a pin or needle is presented to it, or the prime conductor with which it is connected.

BUT the effects of electrical repulsion are shown in a more surprising manner by means of water issuing out of a capillary tube. If a vessel of water be suspended from the conductor, and a capillary syphon be put into it, the water will issue slowly, and in the form of large drops from the lower leg of the syphon; but, upon electrifying this little apparatus, instead of drops, there will be one continued stream of water; and if the electrification be strong, a number of streams, in the form of a cone, the apex of which will be at the extremity of the tube; and this beautiful shower will be luminous in the dark.

LASTLY, Mr. Rackitrow's experiment (as it is generally called) is a striking instance of electrical attraction and repulsion, and, at the same time, exhibits a very pleasing spectacle. Electrify a hoop of metal, suspended from the prime conductor (or supported with small pieces of sealing wax, &c.) about half an inch above a plate of metal, and parallel to it. Then place a
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round glass bubble, blown very light, upon the plate, near the hoop, and it will be immediately attracted to it. In consequence of this, the part of the bubble which touched the hoop will acquire some electric virtue, and be repelled; and, the electricity not being diffused over the whole surface of the glass, another part of the surface will be attracted, while the former goes to discharge its electricity upon the plate. This will produce a revolution of the bubble quite round the hoop, as long as the electrification is continued, and will be either way, just as it happens to set out, or as it is driven by the operator. If the room be darkened the glass ball will be beautifully illuminated. Two bubbles may be made to revolve about the same hoop, one on the inside, and the other on the outside; and either in the same, or contrary directions. If more hoops be used, a greater number of bubbles may be made to revolve, and thus a kind of *planetarium* or *orrery* might be constructed, and a ball hung over the center of all the hoops would serve to represent the sun in the center of the system. Or the hoops might be made elliptical, and the sun be placed in one of the *foci*. N. B. A bell or any metallic vessel inverted would serve instead of a single hoop.

ALL the motions above-mentioned are the immediate effect of electrical attraction and repulsion. The following amusing experiments are performed by giving motion to bodies through the medium of air, i. e. by first putting the air in motion. Let the electrician provide himself with a set of vanes, made of gilt paper or tinsel, each about two inches in length and one in breadth. Let these be stuck in a cork, which may be suspended from a magnet by means of a needle; and then, if they be held at a small distance on one side of the end of a pointed wire proceeding from the conductor, they will be turned round with great rapidity by the current of air which flows from the point. If
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the vanes be removed to the other side of the point, the motion will presently stop, and begin again with the same rapidity in a contrary direction; and thus the motion may be changed at pleasure. This experiment may be diversified by vanes cut in the form of those of a smoke jack; when, being held over the end of a pointed wire, turned upwards, and electrified, they will be turned round very swiftly, by the current of air flowing upwards. If they be held under a point projecting downwards, they will be turned the contrary way.

ON the top of a finely pointed wire, rising perpendicularly from the conductor, let another wire sharpened at each end be made to move freely as on a center. If it be well balanced, and the points be bent horizontally, in opposite directions, it will, when electrified, turn very swiftly round, by the reaction of the air against the current which flows off the points. These points may be nearly concealed, and horses or other figures placed upon the wires, so as to turn round with them, and look as if the one pursued the other. This experiment Mr. Kinnerley calls the ELECTRICAL HORSE RACE. If the number of wires proceeding from the same center be increased, and different figures be put upon them, the race will be more complicated and diverting. If this wire which supports the figures have another wire finely pointed rising from its center, another set of wires, furnished with other figures, may be made to revolve above the former, and either in the same, or in a contrary direction, as the operator pleases.

IF such a wire, pointed at each end, and the ends bent in opposite directions, be furnished like a dipping needle with a small axis fixed in its middle, at right angles with the bending of the points, and the same be placed between two insulated wire strings, near and parallel to each other, so that it may turn on

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its axis freely upon and between them; it will, when electrified, have a progressive as well as circular motion, from one end of the wires that support it to the other, and this even up a considerable ascent.

A VARIETY of beautiful appearances may be exhibited by means of electrical LIGHT, even in the open air, if the room be dark. Brushes of light from points electrified positively, and not made very sharp, or from the edges of metallic plates, diverge in a very beautiful manner, and may be excited to a great length, by presenting to them a finger, or the palm of the hand, to which they feel like soft lambent flames, which have not the least pungency, nor give a disagreeable sensation of any kind. It is also amusing to observe the difference there is between brushes of light from pointed bodies electrified positively, or negatively.

In the electrical horse race above-mentioned, a small flame will be seen in the dark at every point of the bent wires; so that, if the operator can contrive to make the wire terminate in the horse's tail, it will seem to be all on fire. And if a circular plate of metal be cut into the shape of a star, so that every point may be at the same distance from the center, and the center be made to turn freely on a point, like the wires in the preceding experiment, a small flame will be seen at every point; and if the star be turned round, it will exhibit the appearance of a lucid circle, without any discontinuance of the light.

If the electric sparks be taken from a brass ball, at the extremity of a long brass rod, inserted into the prime conductor, they will often be several inches long, and issue in a great variety of crooked directions, exactly resembling the course of lightning, and exhibiting a very amusing spectacle. A friend of Dr. Franklin's supposes that the spark is thrown out of a strait course by the density

density of the air increased by the action and reaction of the two fluids, which are repulsive of one another.*

As the motion of electric matter is, to the senses, instantaneous, a variety of beautiful appearances may be exhibited by a number of small electric sparks, disposed in various forms. This may be done by means of a board and a number of wires, in the following manner. Let two holes be made through the board, about a quarter of an inch on each side of the spot where a spark is desired. Let the extremities of the wires neatly rounded, come through these holes, and be brought near together, exactly over the place; and let the wires on the back side of the board be so disposed, as that an electric spark must take them all in the same circuit. When they are thus prepared, all the points will appear luminous at once, whenever a spark is taken by them at the prime conductor. In this manner may beautiful representations be made of any of the constellations, as of the great bear, Orion, &c; and in this manner, also, may the outlines of any drawing, as of figures in tapestry be exhibited.

The Abbé Nollet has taken a great deal of pains to make the appearance of letters, and other figures, by means of electric sparks, and as it is impossible to make the sparks follow one another in a complete circle, on the same side of any flat surface, he makes use of plates of glass, and places one half of the circle, &c. on one side of the plate, and the other half on the other side, connecting the pieces that are nearest one another, but on different sides of the glass, by wires brought round it.† The description would be too tedious for this place, but the execution

* Franklin's Letters, new edition, p. 167. † Lettres, Vol. 3. p. 281.

will be very easy, to any person who has but a little knowledge of electricity.

THE force of an electric spark in setting fire to various substances was one of the first experiments that gave an *eclat* to electricity, and it is still repeated with pleasure. Spirits of wine a little warmed are commonly made use of for this purpose. The experiment will not fail to succeed, if a pretty strong spark be drawn, in any manner, or direction whatever, through any part of it; and this may easily be done many ways, if it be contained in a metal spoon with a pretty wide mouth. A candle newly blown out may be lighted again by the electric spark passing through the gross part of the smoak, within half an inch of the snuff; though it is perhaps blown in again by the motion given to the air by the force of the explosion. Also air produced by the effervescence of steel filings with oil of vitrol diluted with water, and many other substances, which throw out an inflammable vapour, may be kindled by it.

THE strong phosphoreal or sulphureous smell, which may be perceived by presenting the nostrils within an inch or two of any electrified point, makes a curious experiment, but it does not give a pleasing sensation.

LASTLY, the most entertaining experiment that can be performed by simple electrification, is when one or more of the company stand upon an insulated stool, holding a chain from the prime conductor. In this case, the whole body is, in reality, a part of the prime conductor, and will exhibit all the same appearances, emitting sparks wherever it is touched by any person standing on the floor. If the prime conductor be very large, the sparks may be too painful to be agreeable, but if the conductor be small, the electrification moderate, and none of the company present touch the eyes, or the more tender parts of the face of the

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person

perfon electrified, the experiment is diverting enough to all parties.

MOST of the experiments above-mentioned may also be performed to the moft advantage by the perfon standing upon the stool, if he hold in his hand whatever was directed to be faftened to the prime conductor. Spirits of wine may be fired by a spark from a perfon's finger as effectually as in any other way. Care muft be taken that the floor on which the stool is placed be free from duft, but it is moft advifable to have a large fmooth board for the purpofe.

SECTION II.

ENTERTAINING EXPERIMENTS PERFORMED BY MEANS
OF THE LEYDEN PHIAL.

NO electrical experiments answer the joint purpose of pleasure and surprize in any manner comparable to those that are made by means of the Leyden phial. All the varieties of electrical attraction and repulsion may be exhibited, either by the wire, or the coating of it; and if the knobs of two wires, one communicating with the inside, and the other with the outside of the phial, be brought within four or five inches of one another, the electrical spider above-mentioned will dart from the one to the other in a very surprizing manner, till the phial be discharged. But the peculiar advantage of the Leyden experiment is, that, by this means, the electrical flash, report, and sensation, with all their effects, may be increased to almost any degree that is desired.

WHEN the phial, or the jar, is charged, the shock is given through a person's arms and breast, by directing him to hold a chain communicating with the outside in one hand, and to touch the wire of the phial, or any conductor communicating

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with it, with the other hand. Or the shock may be made to pass through any particular part of the body without much affecting the rest, if that part, and no other, be brought into the circuit through which the fire must pass from one side of the phial to the other.

A GREAT deal of diversion is often occasioned by giving a person a shock when he does not expect it; which may be done by concealing the wire that comes from the outside of the phial under the carpet, and placing the wire which comes from the inside in such a manner in a person's way; that he can suspect no harm from putting his hand upon it, at the same time that his feet are upon the other wire. This, and many other methods of giving a shock by surprise, may easily be executed by a little contrivance; but great care should be taken that these shocks be not strong, and that they be not given to all persons promiscuously.

WHEN a single person receives the shock, the company is diverted at his sole expence; but all contribute their share to the entertainment, and all partake of it alike, when the whole company forms a circuit, by joining their hands; and when the operator directs the person who is at one extremity of the circuit to hold a chain which communicates with the coating, while the person who is at the other extremity of the circuit touches the wire. As all the persons who form this circuit are struck at the same time, and with the same degree of force, it is often very pleasant to see them start at the same moment, to hear them compare their sensations, and observe the very different accounts they give of it.

THIS experiment may be agreeably varied, if the operator, instead of making the company join hands, direct them to tread upon each others toes, or lay their hands upon each others heads; and

and if, in the latter case, the whole company should be struck to the ground, as it happened when Dr. Franklin once gave the shock to six very stout robust men, the inconvenience arising from it will be very inconsiderable. The company which the Doctor struck in this manner neither heard nor felt the stroke, and immediately got up again, without knowing what had happened. This was done with two of his large jars (each containing about six gallons) not fully charged. *

THE most pleasing of all the surprises that are given by the Leyden phial is that which Mr. Kinnerley† contrived and called the MAGIC PICTURE, which he describes in the following manner. Having a large metzotinto, with a frame and glass (suppose of the king) take out the print, and cut a pannel out of it, near two inches distant from the frame all round. If the cut be through the picture, it is not the worse. With thin paste, or gum water, fix the board that is cut off on the inside of the glass, pressing it smooth and close, then fill up the vacancy, by gilding the glass well with leaf gold, or brass. Gild likewise the inner edge of the back of the frame all round, except the top part, and form a communication between that gilding and the gilding behind the glass; then put in the board, and that side is finished. Turn up the glass, and gild the fore-side exactly over the back gilding; and when it is dry, cover it, by pasting on the pannel of the picture that has been cut out, observing to bring the correspondent parts of the board and picture together, by which the picture will appear of a piece as at first, only part is behind the glass, and part before. Lastly, hold the picture horizontally by the top, and place a little moveable gilt crown on the king's head.

* Franklin's Letters, new edition, p. 324.

† *Ib.* p. 29.

If now the picture be moderately electrified, and another person take hold of the frame with one hand, so that his fingers touch its inside gilding, and with the other hand endeavour to take off the crown, he will receive a terrible blow, and fail in the attempt. The operator who holds the picture by the upper end, where the inside of the frame is not gilt, to prevent its falling, feels nothing of the shock, and may touch the face of the picture without danger, which he pretends to be a test of his loyalty. If a ring of persons take a shock among them, the experiment is called the CONSPIRATORS.

As the electric fire may be made to take whatever circuit the operator shall please to direct, it may be thrown into a great variety of beautiful forms. Thus, if a charged phial be placed at one extremity of the gilding of a book, and the discharge be made by a wire which touches the other extremity, the whole gilding will be rendered luminous. But if several pretty strong shocks be sent through the same gilding, they will soon render it incapable of transmitting any more, by breaking and separating the parts too far asunder. Also the electric constellations and figures, mentioned above, may be lighted up much more strongly by a charged phial than by sparks from the conductor; only, they cannot be lighted up so often in this way.

On the same principle that the wires of phials charged differently will attract and repel differently, is made an ELECTRICAL WHEEL, which Dr. Franklin says, turns with considerable strength, and of which he gives the following description. A small upright shaft of wood passes at right angles through a thin round board, of about twelve inches diameter, and turns on a sharp point of iron, fixed in the lower end; while a strong wire in the upper end, passing through a small hole in a thin brass plate, keeps the shaft truly vertical. About thirty *radii*, of equal length,

length, made of sash glass, cut in narrow slips, issue horizontally from the circumference of the board; the ends most distant from the center being about four inches apart. On the end of every one a brass thimble is fixed.

IF now the wire of a bottle, electrified in the common way, be brought near the circumference of this wheel, it will attract the nearest thimble, and so put the wheel in motion. That thimble, in passing by, receives a spark, and thereby being electrified, is repelled, and so driven forwards, while a second being attracted approaches the wire, receives a spark, and is driven after the first; and so on, till the wheel has gone once round; when the thimbles before electrified approaching the wire, instead of being attracted, as they were at first, are repelled, and the motion presently ceases.

BUT if another bottle, which had been charged through the coating, be placed near the same wheel, its wire will attract the thimble repelled by the first, and thereby double the force that carries the wheel round; and not only taking out the fire that had been communicated by the thimbles to the first bottle, but even robbing them of their natural quantity, instead of being repelled when they come again towards the first bottle, they are more strongly attracted; so that the wheel mends its pace, till it goes with great rapidity, twelve or fifteen rounds in a minute, and with such strength, that the weight of 100 Spanish dollars, with which we once loaded it, did not in the least seem to retard its motion. This is called an ELECTRICAL JACK, and if a large fowl was spitted on the upper shaft, it would be carried round before a fire, with a motion fit for roasting.

BUT this wheel, continues the Doctor, like those driven by wind, moves by a foreign force, to wit that of the bottles.

THE SELF MOVING WHEEL, though constructed on the same principles

principles, appears more surprising. It is made of a thin round plate of window glass, seventeen inches diameter, well gilt on both sides, all but two inches next the edge. Two small hemispheres of wood are then fixed with cement to the middle of the upper and under sides, centrally opposite; and in each of them a thick strong wire, eight or ten inches long, which together makes the axis of the wheel. It turns horizontally, on a point at the lower end of its axis, which rests on a bit of brass, cemented within a glass salt cellar. The upper end of its axis passes through a hole in a thin brass plate, cemented to a long and strong piece of glass; which keeps it six or eight inches distant from any non-electric, and has a small ball of wax or metal on its top, to keep in the fire.

In a circle on the table which supports the wheel, are fixed twelve small pillars of glass, at about eleven inches distance, with a thimble on the top of each. On the edge of the wheel is a small leaden bullet, communicating by a wire with the gilding of the upper surface of the wheel; and about six inches from it, is another bullet, communicating, in like manner, with the under surface. When the wheel is to be charged by the upper surface, a communication must be made from the under surface to the table.

When it is well charged, it begins to move. The bullet nearest to a pillar moves towards the thimble on that pillar, and, passing by, electrifies it, and then pushes itself from it. The succeeding bullet, which communicates with the other surface of the glass, more strongly attracts that thimble, on account of its being electrified before by the other bullet, and thus the wheel increases its motion, till the resistance of the air regulates it. It will go half an hour, and make, one minute with another, twenty turns in a minute, which is 600 turns in the whole,

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the bullet of the upper surface giving in each turn twelve sparks to the thimbles, which makes 7200 sparks, and the bullet of the under surface receiving as many from the thimble, those bullets moving in the time near 2500 feet. The thimbles are well fixed, and in so exact a circle, that the bullets may pass within a very small distance of each of them.

IF instead of two bullets, you put eight, four communicating with the upper surface, and four with the under surface, placed alternately (which eight, at about six inches distance, complete the circumference) the force and swiftness will be greatly increased, the wheel making fifty turns in a minute, but then it will not continue moving so long.

THESE wheels, the Doctor adds, may be applied perhaps to the ringing of chimes, and moving light made orreries.*

A PHIAL makes the most beautiful appearance when it is charged without any coating on the outside, by putting the hand, or any conductor, to it, for then, at whatever part of the jar the discharge is made, the fire will be seen to branch from it in most beautiful ramifications all over the jar, and the light will be so intense, that the minutest of the branches may be seen in open day light.

THE discharge of a large electrical battery is rather an awful than a pleasing experiment, and the effects of it, in rending various bodies, in firing gun powder, in melting wires, and in imitating all the effects of lightning, never fail to be viewed with astonishment. In order to fire gun powder, it must be made up into a small cartridge, with blunt wires inserted at each end, and brought within half an inch of each other, through which the shock must pass: or a very small wire may be drawn

* Franklin's Letters, p. 28, &c.

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through

through the center of it, and the explosion will be made by its melting. A common jar will easily strike a hole through a thick cover of a book, or many leaves of paper, and it is curious to observe the bur raised on both sides, as if the fire had darted both ways from the center.

A CONSIDERABLE number of experiments with an electrical battery, some of which exhibit fine appearances, will be particularly described in the last part of the work.

SECTION

SECTION III.

ENTERTAINING EXPERIMENTS MADE BY A COMBINATION
OF PHILOSOPHICAL INSTRUMENTS.

IN order to exhibit some of the finest electrical experiments, the operator must call to his aid other philosophical instruments, particularly the condensing machine, and the air pump.

If the fountain made by condensed air be insulated, and be made to emit one stream, that stream will be broken into a thousand, and equally dispersed over a great space of ground, when the fountain is electrified; and by only laying a finger upon the conductor, and taking it off again, the operator may command either the single stream, or the divided stream at pleasure. In the dark, the electrified stream appears quite luminous.

THE greatest quantity of electric light is seen in *vacuo*. Take a tall receiver very dry, and in the top of it insert with cement a wire not very acutely pointed. Then exhaust the receiver, and present the knob of the wire to the conductor, and every spark will pass through the vacuum in a broad stream of light, visible through the whole length of the receiver, be it ever so tall. This stream often divides itself into a variety of beautiful rivulets,

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which are continually changing their course, uniting and dividing again, in a most pleasing manner. If a jar be discharged through this vacuum, it gives the appearance of a very dense body of fire, darting directly through the center of the vacuum, without ever touching the sides; whereas, when a single spark passes through, it generally goes more or less to the side, and a finger put on the outside of the glass will draw it wherever a person pleases. If the vessel be grasped by both hands, every spark is felt like the pulsation of a great artery, and all the fire makes towards the hands. This pulsation is felt at some distance from the receiver; and in the dark, a light is seen betwixt the hands and the glass.

ALL this while the pointed wire is supposed to be electrified positively; if it be electrified negatively, the appearance is remarkably different. Instead of streams of fire, nothing is seen but one uniform luminous appearance, like a white cloud, or the milky way in a clear star-light night. It seldom reaches the whole length of the vessel, but is generally only like a lucid ball at the end of the wire.

A VERY beautiful appearance of electric light in a darkened room may also be produced by inserting a small phial into the neck of a tall receiver, so that the external surface of the glass may be exposed to the vacuum. The phial must be coated on the inside, and while it is charging, at every spark taken from the conductor into the inside, a flash of light is seen to dart, at the same time, from every part of the external surface of the jar, so as quite to fill the receiver. Upon making the discharge, the light is seen to return in a much closer body, the whole coming at once.

BUT the most beautiful of all the experiments that can be exhibited by the electric light is Mr. Canton's AURORA BOREALIS,
of

of which the following is but an imperfect description. Make a torricellian vacuum in a glass tube, about three feet long, and seal it hermetically, whereby it will be always ready for use. Let one end of this tube be held in the hand, and the other applied to the conductor, and immediately the whole tube will be illuminated, from end to end; and when taken from the conductor, will continue luminous without interruption for a considerable time, very often above a quarter of an hour. If, after this, it be drawn through the hand either way, the light will be uncommonly intense, and without the least interruption from one hand to the other, even to its whole length. After this operation, which discharges it in a great measure, it will still flash at intervals, though it be held only at one extremity, and quite still; but if it be grasped by the other hand, at the same time, in a different place, strong flashes of light will hardly ever fail to dart from one end to the other; and this will continue twenty four hours, and perhaps much longer, without fresh excitation. Small and long glass tubes exhausted of air, and bent in many irregular crooks and angles, will, when properly electrified in the dark, beautifully represent flashes of lightning.

I SHALL conclude this description of entertaining experiments with an account of the manner in which Dr. Franklin and his friends closed the year 1748. The hot weather coming on, when electrical experiments were not so agreeable, they put an end to them for that season, as the Doctor says, somewhat humourously in a party of pleasure on the banks of the Skuykil. First, spirits were fired by a spark sent from side to side through the river, without any other conductor than the water. A turkey was killed for their dinner by the electrical shock, and roasted by the electrical jack, before a fire kindled by the electrified bottle, when the healths of all the famous electricians in England, Holland,

land, France, and Germany, were drunk in electrified bumpers, under a discharge of guns from the electrical battery. *

HAPPY would the author of this treatise be to see all the great electricians of Europe, or even those in England, upon such an occasion, and especially after having made discoveries in electricity of equal importance with those made in Philadelphia in the year referred to. With pleasure would he obey a summons to such a rendezvous, though it were to serve the illustrious company in the capacity of operator, or even in the more humble office of waiter. Cheerfulness and social intercourse do, both of them, admirably suit, and promote the true spirit of philosophy.

* Franklin's Letters, p. 35.

P A R T VIII.

NEW EXPERIMENTS IN ELECTRICITY, MADE CHIEFLY
IN THE YEAR 1766.

I SHALL, in the last part of this work, present my reader with an account of such new experiments in electricity as this undertaking has led me to make. I hope the perusal of this work may suggest many more, and more considerable ones to my readers, and then I shall not think that I have written in vain.

To make this account the more useful to such persons as may be willing to enter into philosophical investigations, I shall not fail to report the real views with which every experiment was made, false and imperfect as they often were. I was always greatly pleased with the extreme exactness and simplicity of Mr. Grey, and shall, therefore, imitate his artless manner. And though an account of experiments drawn up on this plan be less calculated to do an author honour as a philosopher; it will, probably, contribute more to make other persons philosophers, which is a thing of much more consequence to the public.

MANY modest and ingenious persons may be engaged to attempt

tempt philosophical investigations, when they see, that it requires no more sagacity to find new truths, than they themselves are masters of; and when they see that many discoveries have been made by mere accident, which may prove as favourable to them as to others. Whereas it is a great discouragement to young and enterprising geniuses, to see philosophers proposing that first, which they themselves attained to last; first laying down the propositions which were the result of all their experiments, and then relating the facts, as if every thing had been done to verify a true preconceived theory.

THIS synthetic method is, certainly, the most expeditious way of making a person understand a branch of science; but the analytic method, in which discoveries were actually made, is most favourable to the progress of knowledge.

I HAVE, indeed, endeavoured to make the whole preceding history of electricity useful in this view, by not contenting myself with informing the reader what discoveries have been made; but, wherever it could be done, acquainting him *how* they were made, and what the authors of them had in view when they made them. In general, this has not been difficult to do, the facts being recent, and most of the persons concerned now living. And perhaps, in no branch of science has there been less owing to genius, and more to accident; so that no person, who will give a little attention to the subject, need be without hopes of adding something to the common stock of electrical discoveries. Nay it would be extraordinary, if, in a great number of experiments, in which things were put into a variety of new situations, no new fact, worth communicating to the public, should arise.

THE method I propose will, likewise, give the most pleasure to those persons, who delight in tracing the real progress of the
human

human mind, in the investigation of truth, and the acquisition of knowledge; as I hope it will carry with it sufficient evidence of its own authenticity. For this progress, we may assure ourselves; has, in all cases, been by easy steps, even when it has been the most rapid. Were it possible to trace the succession of ideas in the mind of Sir Isaac Newton, during the time that he made his greatest discoveries, I make no doubt but our amazement at the extent of his genius would a little subside. But if, when a man publishes discoveries, he, either through design, or through habit, omit the intermediate steps by which he himself arrived at them; it is no wonder that his speculations confound others, and that the generality of mankind stand amazed at his reach of thought. If a man ascend to the top of a building by the help of a common ladder, but cut away most of the steps after he has done with them, leaving only every ninth or tenth step; the view of the ladder, in the condition in which he has been pleased to exhibit it, gives us a prodigious, but an unjust idea of the man who could have made use of it. But if he had intended that any body should follow him, he should have left the ladder as he constructed it, or perhaps as he found it, for it might have been a mere accident that threw it in his way. It is possible he had even better have destroyed it entirely; as, in some cases, a person would more easily make a new ladder of his own, than repair an old and damaged one.

THAT Sir Isaac Newton himself owed something to a casual turn of thought, the history of his astronomical discoveries informs us; and where we see him most in the character of an experimental philosopher, as in his optical inquiries (though the method of his treatise on that subject is by no means purely analytical) we may easily conceive that many persons, of equal patience and industry (which are not called qualities of the under-

standing) might have done what he did. And were it possible to see in what manner he was first led to those speculations, the very steps by which he pursued them, the time that he spent in making experiments, and all the unsuccessful and insignificant ones that he made in the course of them; as our pleasure of one kind would be increased, our admiration would probably decrease. Indeed he himself used candidly to acknowledge, that if he had done more than other men, it was owing rather to a habit of *patient thinking*, than to any thing else.

I do not say these things to detract from the merit of the great Sir Isaac Newton; but I think that the interests of science have suffered by the excessive admiration and wonder, with which several first rate philosophers are considered; and that an opinion of the greater equality of mankind, in point of genius, and powers of understanding, would be of real service in the present age. It would bring more labourers into the common field; and something more, at least, would certainly be done in consequence of it. For though I by no means think that philosophical discoveries are at a stand, I think the progress might be quickened, if studious and modest persons, instead of confining themselves to the discoveries of others, could be brought to entertain the idea, that it was possible to make discoveries themselves. And, perhaps, nothing would tend more effectually to introduce that idea, which is at present very remote from the minds of many, in which it ought to have a place, than a faithful history of the manner in which philosophical discoveries have actually been made by others.

THAT this fidelity has been preserved in the following narrative, I make no doubt of its being its own voucher. Its imperfections will be a sufficient evidence. The same fidelity will also oblige me to relate several facts as appearing new to myself, which

which the course of the preceding history will show to have been discovered by others, though I was not then aware of it. Of such after discoveries, however, I have mentioned only those which, it will be seen, I have pursued something farther than the original authors, having attended to circumstances overlooked by them; or, at least, having made the experiments with much more exactness; so that the reader may expect something really new under every article. And the experiments which prove the same thing will be found considerably different from those of others, and to furnish additional arguments of the same general propositions. This repetition of old discoveries, and this variety in the experiments by which they were made, were both occasioned by a situation which is more or less common to every electrician in England; whereby we are ignorant of a great deal of what has been done by others.

In the following narrative will also be found an account not only of experiments which are complete, which exhibit some new fact, and from which something relating to the general theory of electricity may be deduced; but also some that are incomplete, which produced no new appearance, and from which nothing positive could be concluded. If electricians in general had done this, they would have saved one another a great deal of useless labour, and would have had more time for making experiments really new, and which might have terminated in considerable discoveries. Besides, if things be really put into new situations, though nothing *positive* can be inferred from the experiment, at least something *negative* may; and this cannot be said to be of no importance in science; nor strictly speaking to be no new truth. A sufficient number of these experiments may, in many cases, lay a foundation for probable and positive conclusions.

I MAKE no apology for leaving so many of these experiments imperfect, and for publishing this account of them before they have been pursued so far as it may, perhaps, be thought they deserve. I rather think the generality of philosophers ought to make an apology to the public, for delaying the communication of their experiments and discoveries so long as they have done. It is possible I may never have any more leisure or opportunity to pursue them, and others may better command both; whereby the discoveries will be sooner brought to their maturity, and the progress of this branch of philosophy accelerated. The genuine spirit of philosophy is, surely, not that of mechanicks, who make the most of every little improvement in their arts, and never divulge them, till they can make no more advantage of them themselves. If I could this day communicate to any fellow-labourer a hint, which it was more probable he could immediately pursue to advantage than myself, I would not defer it till to-morrow. Nor do I think it is any great boast of a philosophical indifference to fame to make this declaration. The great Sir Isaac Newton seems to have had no idea of the pursuit of fame. He deferred the communication of his important discoveries through real modesty, thinking it impertinent to trouble the public with any thing imperfect. I make no pretensions to that kind of modesty; whether it be of a true or a false kind, I think it manifestly injurious to the progress of knowledge. Like those who contended in one of the games of ancient Greece, I shall immediately deliver my torch to any person who can carry it with more dexterity. If others do the same, it may come into my hands again, several times, before we reach the goal.

It may be said, that I ought, at least, to have waited till I had seen the connection of my new experiments with those

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that were made before, and have shown that they were agreeable to some general theory of electricity. But when the facts are before the public, others are as capable of showing that connection, and of deducing a general theory from them as myself. If but the most inconsiderable part, of the temple of science be well laid out, or a single stone proper for, and belonging to it be collected; though at present it be ever so much detached from the rest of the building, its connection and relative importance will appear in due time, when the intermediate parts shall be completed. Every *fact* has a real, though unseen connection with every other fact: and when all the facts belonging to any branch of science are collected, the system will form itself. In the mean time, our guessing at the system may be some guide to us in the discovery of the facts; but, at present, let us pay no attention to the system in any other view; and let us mutually communicate every new fact we discover, without troubling ourselves about the system to which it may be reduced.

I THINK I shall give the most distinct view of the few things that I have observed, in the short course of my electrical experiments, if I relate them pretty nearly in the order in which they occurred, only taking care not to intermix things of a very different nature. The earliest date in my experiments is the beginning of the year 1766; when, in consequence of forming an acquaintance with some gentlemen who have distinguished themselves for their discoveries in electricity, and of undertaking to write the preceding history, my attention was first turned towards making some original experiments in this part of Natural Philosophy, which had served for my occasional amusement some time before.

SECTION

SECTION I.

EXPERIMENTS ON EXCITATION, PARTICULARLY OF TUBES
IN WHICH AIR IS CONDENSED, AND OF LARGE GLASS
GLOBES.

FINDING by my own experiments, and those of others, that a glass tube, out of which the air was exhausted, discovered no sign of electricity outwards, but that all its effects were observed on the inside; I imagined that, if the air was condensed in the tube, it would operate more strongly on the outside; so that an additional atmosphere would give it a double virtue. But the result was the very reverse of my expectations.

SOMETIME in the month of January, when the weather was dry and frosty, I took a glass tube, such as is generally used for electrifying, about two feet and a half in length, and an inch in diameter. It was closed at one end, and by means of a brass cap at the other, I fitted a condensing engine to it; and when the tube was very dry, and in excellent order for making experiments, I began to throw in more air. At every stroke of the piston I endeavoured to excite the tube, but found its virtue diminished. It was obliged to be brought nearer than be-

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fore to attract light bodies, and gave less light when rubbed in the dark; till, as near as I could judge, I had got one additional atmosphere into the tube; when its power was scarce discernible. Letting out the air by degrees, I observed it gradually recovered its power. It attracted light bodies at a greater distance; it gave louder snappings and more light in the dark; and when the additional air was wholly let out, its power was immediately as great as it had been before any air was thrown in. This I tried several times with the same success.

COMMUNICATING these experiments to Dr. Franklin and Dr. Watson, they suggested to me, that the non-excitation of the tube above-mentioned might be owing to moisture introduced along with the air, and adhering to the inside of the tube. This conjecture was rendered more probable by another experiment I had made in the mean time.

REPEATING my attempts to excite the tube above-mentioned, I found that, after very hard rubbing, it began to act a little; and that its virtue increased with the labour. Thinking it might be the warmth which produced this effect, I held the tube to the fire, and found that when it was pretty hot, it would act almost as well as when it contained no more than its usual quantity of air. I conjectured that the warmth might expel the moisture from the sides of the glass, or make the enclosed air capable of holding a greater quantity of water in a state of perfect solution.

WILLING to determine whether the additional quantity of air with the moisture it occasioned, acted in all respects like a non-electric coating, I tried the experiment with condensed air, that Dr. Defaguliers did with sand. After condensing the air, and finding the excitation of it impossible, as usual, I let the air suddenly

suddenly out, to see whether the tube would then show any effect of the preceding friction; but it had not acquired the smallest degree of electricity; though the first stroke of the rubber, immediately afterwards, made it give sparks to the finger at the distance of two and three inches. Perhaps the degree of moisture it had contracted was very slight, and expelled by the act of excitation.

Uron being desired to repeat this experiment with a particular view to the moisture; I observed that not the least cloudiness could be perceived to adhere to the glass, at the time that it was absolutely incapable of excitation. When one part of the tube was made warm, and the other left cold, the same stroke of the rubber would excite the warm part, without in the least affecting the rest. But still the cold part of the tube appeared not in the least more cloudy than that which was warm; and the moment the air was let out, the first stroke of the rubber made the whole strongly electrical.

WILLING to ascertain whether the condensing of air necessarily introduced more moisture in a glass vessel than the air could hold in perfect solution, I constructed a glass condenser in such a manner, that I could charge and discharge small phials in the inside of it; concluding, that if the additional air brought more additional moisture, it would be impossible to charge a phial at all in those circumstances; whereas, if the air was free from moisture, it would make the phial hold a greater charge, double in two atmospheres, treble in three, &c. Accordingly, I charged a tube about three quarters of an inch in diameter, and coated about eight inches, in the glass vessel, containing about two atmospheres; and it received a much greater charge than it could be made to take in the open air, and as near as could be judged, by the report and flash, twice as great. At last the
tube

tube burst by a spontaneous discharge, after being charged and discharged three or four times, in the condensed air. It is not at all probable, that it could have been broke by any charge it could have held in the open air. This experiment seemed to determine, that there was no very great degree of moisture introduced into the glass vessel by the condensation of air.

I AFTERWARDS found that experiments on condensed air had been made by Mr. Du Faye and others, but not with all the circumstances above-mentioned.

SOME of my electrical friends are of opinion, that the reason why a tube with condensed air in it cannot be excited is, that the dense air within prevents the electric fluid from being forced out of the inside of the tube, without which none can be forced into the outside; and that heating the tube makes the air within less electrical, and the tube also; in consequence of which, it more easily parts with the fluid on one side, and admits it on the other. But upon this principle how can a solid stick of glass be excited?

IMAGINING that a greater quantity of electric fire would be produced from the friction of larger globes than those of the usual size, I provided myself on the 24th of April 1766, with a globe seventeen inches and a half in diameter. It had only one neck, and was made exceeding well; only being rather too large for the mouth of the furnace, a small coal had stuck to its equatorial diameter, which, when it was struck off, made a small hole in it. This, in some measure, disfigured the globe, but I never imagined it could prevent its excitation in any great degree; so that I still indulged hopes of acquiring, by its means, a prodigious power of electricity. But what was my surprize when, after I had got it mounted in the best manner possible, and after

trying, for hours together, every method of friction, in the most favourable circumstances for excitation, I could scarce get the appearance of fire from it; the sparks from the prime conductor being barely visible.

ACQUAINTING Dr. Franklin with my disappointment, he advised me to get the first coat of the globe taken off with emery; as it had often been observed, that many globes would not work well, till after a considerable time, when the glass-house coat, as it may be called, is worn off. This operation I accordingly performed upon it, and incredibly laborious it proved; which greatly increased my disappointment, when I found that it had all been labour in vain, for the globe had no more electrical power than before.

DISPAIRING of making any thing of this globe, I laid it aside, and, on the 22d of May, got another, about fourteen inches in diameter. In blowing this globe, every circumstance that I could imagine had, in the least, contributed to my ill success with the former, was carefully avoided. The former was made late in the week, when the metal had been long in fusion; because I had been told, that globes made in that state of the metal were always the best for electrical purposes. This was blown early in the week, when the workmen say the metal is most transparent, and freest from all kinds of imperfection. The former was warmed, in the course of making it, in a place in which wood and coals were frequently thrown, to keep up the heat. This was kept free from the fumes of any fuel whatever. Nothing could be finer than the metal of this globe, nothing more perfect in its form. It was also very well mounted, and I did not doubt of success. But, after all, this globe, if possible, gave less fire than the former. I had recourse to every method of excitation that I had ever heard of, or could myself

myself imagine, but all in vain. The thing looked like enchantment.

WHILST I was thinking over every thing that I could imagine might possibly be the cause of my ill success with these globes, I recollected, that another globe, which I had got made for a friend, in the same state of the metal with my last, and only an inch and half less in diameter, acted exceeding well; and that there was no other apparent difference between them, but that his had two necks, and an axis quite through it; whereas mine had only one neck, and no axis at all. Willing to try every thing, I resolved to get the brass cap of my globe perforated, and a small wire introduced, to serve instead of an axis. This was done; but, in making the perforation, it happened, unfortunately, as I then thought, but the most fortunately in the world as it proved, that a lump of hard cement, about the bigness of a small walnut was pushed into the inside of the globe. Vexatious as this circumstance was, I was impatient to try my new experiment, and immediately began to whirl the globe, with this succedanium of an axis, though the cement was all the while rattling, and fouling the inside.

I HAD not whirled the globe long, in these circumstances, before I plainly perceived that its power increased. After some time, it was pretty considerable, and I did not doubt but it was owing to the axis; nay I had formed a pretty plausible theory, to account for an axis being necessary to a globe of such a size. Willing, however, to verify the fact, and ascertain my new hypothesis, I took out the wire; but, to my surprisè, found the virtue of the globe not at all diminished. On the contrary, it continued increasing, and by the time that the cement was well broken, and dispersed, so as to have given a kind of lining to the globe, its power was exceeding strong, and it acted as well

as any globe I had ever seen. In this state, I observed, that after exciting any part of the surface, the small pieces of cement in the inside, to the distance of about two inches, would jump from the finger, or any conductor, presented on the outside.

HAVING, in this unexpected manner, made a perfect cure of this smaller globe, I remounted the larger, and considering, that the cement could probably act only as any other electric lining, I introduced into it some pounded sulphur, mixed with some flower of brimstone; and found that, as soon as there was enough to render it semi-opaque, it acted very well.

In this state, the appearance of the globe was, in several respects, very remarkable. The part that was rubbed had none of the sulphur upon it, except those places where the polish had been, in some measure, taken off by the emery, in the first operation. These being circular, the brimstone lay upon them, like the belts of Jupiter. The hemisphere opposite to the neck had twice as much sulphur upon it as the other; and, in both hemispheres, the sulphur lay thicker, as it receded from the equatorial diameter.

I AFTERWARDS put as much more sulphur into it, which doubled the lining equably every where; but left two or three great heaps, in particular parts of the equatorial diameter, where it was rubbed, and where I could perceive no defect of polish. Whirling the globe, upon this, I found the virtue almost quite gone, and even the amalgam could not revive it. Endeavouring to take the sulphur out of the globe, I broke a great hole into it; and also the new globe was broken the same day, by a lump of hard cement, in the inside, falling from the top to the bottom. These accidents rendered my experiments incomplete.

I THEN proposed to get another large globe, with one neck, and a large hole in the opposite side; by means of which I could easily

easily put different substances into it, and take them out again, in order to find the cause of the appearances above-mentioned. But apprehending this course of experiments might prove a little too expensive, and after all, terminate in nothing, I unwillingly desisted.

I HAD afterwards, however, an opportunity of trying a few experiments upon a glass spheroid, about twelve inches in diameter, in one end of which I had got a pretty large hole made, through which I could introduce my hand into the inside, and which, when it was whirled, I could leave open or closed at pleasure. The other end was furnished with a brass cap and pulley, to adapt it to the machine. The experiments I made with it could not ascertain what I wanted to discover, namely the reason of the non-excitation of the large globes in the circumstances above-mentioned; because this spheroid was excited without any electric lining. But, however, it is possible, that the reader may think them, on other accounts, worth communicating.

I FIRST put my handkerchief, and various other non-electric substances, into the inside; and observed, that while they kept in one piece, and did not disperse to different sides of the globe at the same time, the excitation was very little impeded; but when my handkerchief, &c. opened, and spread over the surface of the spheroid, the excitation was nearly stopped.

I PUT a quantity of quicksilver into it; and while the motion of the spheroid was moderate, and allowed the quicksilver to remain at the bottom, or to ascend but a little height on one side, the excitation was much impeded; and still more if it spread further, over a considerable part of the inside surface, though without partaking of its motion. But when the motion of the spheroid was rapid, and entirely communicated to the quicksilver

quicksilver, so that they had no motion with respect to one another, the virtue always revived a little; but still the excitation was inconsiderable.

POUNDED glass, and other electrics had no sensible effect; neither had any number of quills, or a great quantity of downy feathers; though they instantly dispersed themselves, and covered all the equatorial parts of the spheroid.

I SHALL add to this section, on the subject of excitation, that I once whirled a very thin globe, about six or seven inches in diameter, which was made to weigh air, and not one fourth part so thick as a common-Florence flask. It was excited very powerfully by a piece of leather which had been soaked in a mixture of tallow and bees wax, and into which a quantity of amalgam had been worked. With this globe I could make my common jar discharge itself over more than five inches of the external surface, which I reckon to be a considerable proof of its power. It seems to follow from this experiment, that the thinness of glass globes, or tubes, is by no means any obstruction to their electric power.

IN the course of these experiments I had read Mr. Bergman's account of his curing a globe by a lining of melted sulphur, and had proposed to try that in the last place, on account of the disagreeable operation; but found it superseded in the manner described above.

SECTION

SECTION II.

EXPERIMENTS WHICH PROVE A CURRENT OF AIR FROM THE POINTS OF BODIES ELECTRIFIED EITHER POSITIVELY OR NEGATIVELY.

DURING a course of electrical experiments, made to divert some of my friends, one of the company happened to present a pointed wire to my hand, as I was standing upon the stool; when I was surpris'd to perceive a cool blast proceeding from it; though, according to Dr. Franklin's theory, the current of the fluid went from my hand to the point. I then presented my nostrils to the point, and perceived the same strong phosphoreal smell, as if the point had been electrified positively. These facts made me entertain some doubts about the direction of the current, and the principles of Dr. Franklin's theory, and led me to the following course of experiments; which prove nothing against that theory, but establish a real current of air from the points of all electrified bodies.

To ascertain the course of the electric fluid, I began with discharging large flocks through a quantity of water, in which dust was floating; and I electrified pointed wires, inserted into close

close receivers full of smoke; &c. but these determining nothing, I, at last, recollected, that, of all things, flame is the least sensibly affected with electrical attraction or repulsion, but most easily with the least breath of air; and did not then doubt, but that the current of air would be in the direction of the fluid, being, as it were, impelled by it.

WITH these views, on the 25th. of February, I presented the flame of a candle to a pointed wire, electrified negatively, as well as positively. The blast was so strong (in both cases alike) as to lay bare the greatest part of the wick, the flame being driven from the point; and sometimes a pretty large candle would be actually blown out by the blast. But, in all cases, the effect was the same whether the electric fluid issued out of the point, or entered it.

PLACING the flame between two points, one of which communicated with the prime conductor electrified positively, and the other with the floor, the flame was blown from that which communicated with the conductor upon the other, but not to so great a distance as if the other had been away. Changing the points, the effect was still the same, whether that which communicated with the conductor was the more sharp, or the more blunt of the two, the flame always receding from it.

REVERSING this experiment, and making one of the points communicate with the rubber, and the other with the floor, the former always blew the flame towards the latter. It was evident, however, that the point which communicated with the floor blew likewise; for it counteracted the other, and would, when brought near the flame, raise it almost perpendicular, when it had been blown quite aside by the other.

PLACING the flame between two points, one of which communicated with the rubber, and the other with the conductor,

it

it was equally affected by both, being always blown from the point which was nearest to it.

IT was very observable, that, notwithstanding the current of air from the points affected the flame so remarkably; yet a small portion of it, when it was brought very near the point, would be strongly attracted by it, at the same time that the greatest part of the flame was, by the current of air, blown the contrary way. This effect was always the same, whether the point was electrified positively or negatively; though, I fancied that the negative point attracted the flame more sensibly than the other.

To make the points blow more strongly, in a direct line, I sometimes inclosed them in small tubes of glass.

AFTERWARDS I diversified this experiment in the following manner. I charged the inside of a small jar positively; then setting it upon a glass stand, in contact with a pointed wire, I placed the flame of a candle within an inch of the point, and touched the wire of the jar, with a brass rod which I held in my hand. At every touch the flame was blown strongly from the point. Sometimes it would be blown out; but another point, being held opposite to it, would support the flame; and more strongly, if that point was joined with the rod with which I touched the wire of the jar. Charging the jar negatively in the inside, all the effects were the very same. Discharging the jar through the points, with the flame in a right line between them, it was disturbed, but not blown to one side more than the other.

To take off all the effect of electrical attraction and repulsion, and leave the current of air to act singly, I interposed pieces of brass wire communicating with the earth, between the points of the wire and the flame; and found the blast to be rather increased than diminished thereby.

HAVING communicated these experiments to Dr. Franklin, he

advised me to try the force of this current upon *paper vanes*, such as he has described in his letters: for, with him, they seemed to turn one way or the other indifferently, just as they happened to set out. Accordingly I took a cork, and stuck into the sides of it thirteen vanes, each being half a card, well dried, and each proceeding from the center of the cork. Into the cork I stuck a needle, by which I suspended the whole on a magnet.

THESE VANES I held two or three inches from the point of a wire, communicating with the outside coating of the jar, placed upon an electric stand, in the manner described above; and observed, that whenever I took a spark from the wire communicating with the inside, the vanes were strongly blown upon, and made to turn, as if the current of air had flowed from the point; at the same time that, according to Dr. Franklin's theory, the electric fluid was entering it. If they were made to turn the contrary way, the current soon stopped them, and never failed to bring them back, and make them move as before.

WHEN wires communicating with the floor were placed between the vanes and the point, to take of all the electrical attraction and repulsion, the vanes still moved as briskly as ever.

WHEN the jar was charged pretty high, the motion might be made so swift, that the separate vanes could hardly be distinguished, as the whole set turned round.

I MOREOVER observed, that the vanes were turned very briskly, not only when held near the point, but also when held any where within the distance of six or seven inches from the sides of the wire, which I made sometimes of a considerable length. The stream would turn the vanes one way on one side of the wire, and the contrary way on the other; and being removed quickly to the different sides, the direction of their course might be changed several times, in the discharge of one small jar.

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I MADE points to project two ways at the same time, and observed, that the stream was the same from both, and also when the points were made to project at right angles from one another. In this position of the wires, it was amusing to observe, that the vanes would move one way, when held near one of the wires; and immediately turn about and move the contrary way, if removed near the other.

HITHERTO I had made my vanes of exceeding dry paper, in order to make them less affected by electrical attraction and repulsion, that so the current of air might be the more indisputable; but Mr. Canton desiring me to try vanes that were conductors, I first dipped my paper vanes in water, and afterwards made a set of tinsel, of the same form with the other. These vanes, being conductors of electricity, promoted a freer current of the electric matter, and consequently, occasioning a greater motion to be given to the air, they whirled about with more rapidity than the former. When they were insulated, they were affected just as the dry paper vanes had been.

WITH these vanes, I diversified the experiment in a manner which showed the sameness of the current, notwithstanding the change of electricity, in a clearer manner than before. I insulated a jar, with a wire projecting from the coating, and held the tinsel vanes near the extremity of it. All the time the jar was charging, the vanes turned with great rapidity, as if by a blast from the point. Keeping the jar, the pointed wire, and the vanes in the same situation, the gradual discharge of the jar, made by now and then touching the wire which communicated with the inside, made the vanes still turn the same way, and, as far as could be perceived, with the same force.

To diversify this experiment, I placed a charged jar upon a stool which had glass feet, a pointed wire projecting from the coating,

coating, a quantity of brass dust before the point, and a brass chain communicating with the ground on the other side of the dust. In this situation, every attempt to discharge the jar threw a considerable quantity of the dust from the point, being raised about seven or eight inches, and blown to a considerable distance. Removing the pointed wire from the coating of the jar, and connecting it with the chain, the same attempt to discharge it blew the dust upon the jar. Using two points, one at the jar, and the other at the chain, the dust was disturbed, and raised up, but not blown one way more than the other. Fine flour answered nearly as well.

LASTLY, I made the experiment of the current with vanes in the form of a smoke jack, which answered as well as the others. They were moved when held more than a foot above the point, and likewise at a considerable distance below it, when it was turned downwards.

AFTER these experiments, I read in Mr. Wilson's treatise on electricity, that the vanes would not turn in vacuo. This I tried, and found it to be true, and at the same time I found, they would not turn in a close receiver, not exhausted, where the air was confined, and had not a free circulation.

THE current of air from the points of bodies, electrified *plus* or *minus*, is not more difficult to be accounted for on Dr. Franklin's hypothesis of positive and negative electricity, than any other case of electrical repulsion. The particles of the atmosphere, near the points of electrified bodies, having, by their means, become possessed of more or less than their natural share of the electric fluid, must, according to the rule above-mentioned, retire to places where they can discharge or replenish themselves, as occasion may require. If it be asked why the particles of the atmosphere do not, in the same manner, recede from

from all the parts of the electrified body, as well as from the points; it is answered, that, as the pressure of the atmosphere will prevent a vacuum, and as electrical attraction and repulsion are most powerful at the points of bodies, on account of the easier entrance or exit of the fluid at the points (upon whatever principle that effect depends) the electrified atmosphere (whether negative or positive makes no difference) must fly off at the points preferably to any other places, and the weight of the atmosphere will force the air of the neighbouring places upon the flatter parts of the electrified conductor, notwithstanding the real endeavour it may have to recede from it.

SECTION

SECTION III.

EXPERIMENTS ON MEPHITIC AIR, AND CHARCOAL.

I HAVE related several instances of self-deception in other persons: to show that I do not mean to spare myself, I shall now relate one of my own. However it is a mistake that I should not have troubled the reader with, had it not terminated in a real discovery.

HAVING read, and finding by my own experiments, that a candle would not burn in air that had passed through a charcoal fire, or through the lungs of animals, or in any of that air which the chymists call *mephitic*; I was considering what kind of change it underwent, by passing through the fire, or through the lungs, &c. and whether it was not possible to restore it to its original state, by some operation or mixture. For this purpose, I gave a great degree of intestine motion to it; I threw a quantity of electric matter from the point of a conductor into it, and performed various other operations upon it, but without any effect.

AMONG other random experiments, I dipped a charged phial into

into it; but, though I could not perceive that it produced any effect upon the air, I was surpris'd to find, upon taking out the phial, that it was wholly discharged. I imagin'd, however, at that time, that the discharge was owing to some imperfection in the manner of making the experiment, which I could not easily remedy.

AFTERWARDS, concluding, from some experiments of my own, and others of Dr. Macbride that this mephitic air was not any thing that had ever been common air, but a *fluid sui generis*, that had several properties very different from those of common air; I thought of resuming my experiments, to ascertain whether it was not different from common air with respect to electricity; mephitic air being perhaps a conductor, as common air was a non-conductor.

ACCORDINGLY, some time in the month of January 1766, I fill'd a receiver, which was open at the top, and which held about three pints, with air from my lungs; and, with every precaution I could think of, dipped a small charged phial into it; and, upon taking it out after continuing there about two seconds, found it quite discharged, just as might have been expected from dipping it into water. I repeated the experiment several times, and always with the same success. Lest I should have discharged the phial, some way or other, in passing the neck of the receiver; I chang'd the air, and then introduced the charged phial as before, several times; but never found it in the least discharged. Hence I concluded, that, in the former case, it must have been discharged by the quality of the air in the receiver.

As there still remained some suspicion, that the phial might have been discharged by the *moisture* which was mix'd with the air as expired from my lungs (though I did not doubt but that
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what moisture there was in it was quickly attracted by the sides of the glass vessel, so as to leave the center free from it, and imagined that no quantity of moisture which the air could be supposed to contain could be able to discharge the phial so quickly) I immediately repeated the experiment, with air which I thought could not be supposed to contain any moisture.

I FILLED the same receiver with air from the center of a charcoal fire, and dipping the charged phial into it, as I had done before, found it as effectually discharged as in the former case. I repeated this experiment also a great number of times, and always with the same success. Upon changing the air also and then introducing the phial, I found it not discharged. There seemed, therefore, to be no doubt, but that air in which flame would not subsist was a conductor of electricity.

I WAS confirmed in this opinion by placing a coated phial in a receiver filled with mephitic air, and finding that it was absolutely impossible to charge it in the smallest degree, in that situation; though it was charged very well, the moment I took it out of that air, without any wiping; which seemed to show, that the incapacity of charging had not been owing to any moist vapours that adhered to the glass.

I WAS also confirmed in my opinion of the conducting power of mephitic air, by considering, that all metallic bodies, which are the most perfect conductors we know, consist of a vitrifiable earth, and what the chymists call *phlogiston*, or probably as I then thought nothing more than this same mephitic air in a fixed state.

In this confidence of having made a complete discovery, and one of some importance, after having repeated the above-mentioned experiments, and many others to the same purpose, a great number of times; I once happened to change the mephitic
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for common air in my receiver, without wiping the inside of the glass, which had been my usual method, though I had done it with no other view than more effectually to change the air; but now, though the air was sufficiently changed, by moving the receiver several times up and down, I had no sooner dipped the charged phial into it, than I found it discharged, in the same manner as it had been done in the mephitic air. The discharge had, after all, been made by the moisture on the inside. To ascertain this, I moistened the inside of the receiver with a sponge, and found that no phial could remain charged in it for the least space of time. This showed, that the experiments above recited were ambiguous.

DR. FRANKLIN, to whom I had communicated these experiments, recollected them when he was this last summer at Pyrmont, where a large body of mephitic air always lies upon the surface of the medicinal spring (for this air is evidently specifically heavier than common air, and does not easily mix with it) but not having a proper apparatus, and the company there making experiments inconvenient, he did nothing that was decisive; though, from the little that he had an opportunity of doing, he imagined it was not a conductor: and I have since found that this supposition was just. A charged phial may be dipped into a body of mephitic air, resting on the surface of fermenting vat, without being discharged. If two equal phials, however, be equally charged at the same time, and one of them be plunged into the mephitic air, and the other kept out of it; the latter will always retain the charge longer than the former, which will sometimes retain it but a very short time; owing, as I suppose, to the *moisture*, which is readily absorbed by the mephitic air. This circumstance accounts for the antiquity of my former experiments. I also found *inflammable* air to be the

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same as common, or mephitic air, with respect to the power of conducting electricity.

THESE experiments on mephitic air, deceitful as they were, led, however, to a discovery, which may possibly throw some new light upon some of the most fundamental principles of electricity.

FINDING that I could make nothing of mephitic air itself (for I had endeavoured to procure it in a variety of ways, but without success) I considered that it was from *charcoal* that I procured it in the greatest quantity, and thought I would try charcoal itself in substance. Accordingly, on May the 4th. 1766, I tried the charcoal, in a variety of ways and states; and found it to be, what I had suspected, an excellent conductor of electricity.

PRESENTING a piece of charcoal to the prime conductor, together with my finger, or a piece of brass wire, I constantly observed, that the electric spark struck the charcoal before either of the other conductors, if it happened to be advanced ever so little before them. Having a very rough surface, the charcoal did not take a dense spark from the conductor, till it was made a little smooth, and brought within about half an inch; when, to all appearance, it did quite as well as any piece of metal, there being a constant stream of dense, and white electric fire between the conductor and it. I tried the charcoal in every state of heat or cold, and found no alteration of its conducting power.

I PLACED a great number of pieces of charcoal, not less than twelve or twenty, of various sizes, in a circuit, and discharged a common jar through them; when, to all appearance, the discharge was as perfect, as if so many pieces of metal had been placed in the same manner. Two of the pieces, about the middle of the circuit, I placed about an inch and a half from one another; but, upon the discharge, the spark passed the interval very full and strong. A piece of charcoal also made the discharge at the wire with one spark, but the report was not so loud as when

when the discharge was made with a piece of metal. It was observable, that a black gross smoke rose from between each of the pieces of charcoal, at the moment of the discharge; but the ignition was momentary, and the fire could not be perceived on the charcoal.

To make the experiment of the conducting power of charcoal in the most indisputable manner, I took a piece of baked wood, which I had often used for the purpose of insulation, being an excellent non-conductor, and putting it into a long glass tube, I thrust it into the fire, and converted it into charcoal. In this operation, a very great quantity of gross smoke rose from it, so that, seeming to part with more of its moisture, one would have expected it would have come out a better non-conductor; but, upon trial, its electric property was quite gone, and it was become a very good conductor.

THE experiments above-mentioned were first made with *wood charcoal*, of which I found pieces of very different degrees of conducting power; but the most perfect conductors I have found of this kind are some pieces of *pit charcoal*. These seem to be, in all respects, as perfect conductors as metals. They receive a strong bright spark from the prime conductor, though seldom at above an inch distance, on account of the roughness on their surface, which cannot be taken off; and in discharging a jar through them, or with them, no person can imagine any difference between them and metal, either in the colour of the electric spark, or the sound made by the explosion. When they are broken, they exhibit an appearance which very much resembles that of broken steel. There is however a great variety in the electrical properties of different pieces of this kind of charcoal; and for want of proper opportunity I have not yet succeeded in ascertaining, with sufficient certainty, the circumstances, in the preparation, &c. on which this variety depends.

I WOULD have preferred the examination of wood charcoal on many accounts; particularly, as the same substance is, in this case, converted from a perfect electric to a perfect conductor; and all the degrees of conducting power may be found in different specimens of it; whereas pit coal is itself a conductor, though an imperfect one: but not having any opportunity, I procured specimens of all the varieties I could imagine in the same heap of pit charcoal, with respect to their nearness or distance from the surface, &c; but though I examined them with all the care and attention that I could apply, and in every method that I could think of, the differences were so exceedingly small, if any, that I could not fix upon any circumstance that I could depend upon for the cause of them.

EVEN common cinders from an open fire, of the kind of coals which we generally burn, I find to be very little inferior to charcoal; which is suffered to flame, but covered very close as soon as it is well burnt, and before any ashes are formed. Coals and cinders from a common fire, being a very commodious subject for experiments, I did not fail to make as many upon them as I could imagine would be of any use; except that I had no opportunity of trying a sufficient variety of coals. I took several out of the fire after they had done blazing, some of which I covered with ashes, some I quenched in water, and some I left to cool in the open air. I also reduced some of the coals to cinders in a glass vessel, without suffering them to flame; and I treated in the very same manner various pieces of oak, cut from the same plank; but when I examined them, I found their differences, with respect to their power of conducting electricity, very inconsiderable, if any. I thought the cinder of a coal which we call kennel, and which is remarkable for flaming much while it burns, to be a better conductor than a cinder from

a common coal: but the difference might be owing to its more uniform texture, and smoother surface. Charcoal made of coals which yield a strong sulphureous smell when they are burnt, and of which the charcoal itself is not quite divested, was, to all appearance, as good a conductor as that of the other kind, which is more esteemed.

IN this course of experiments I found myself much at a loss for a sufficiently accurate method of ascertaining the difference of conducting substances, and I wish that electricians would endeavour to find such a measure. One of the best that I am acquainted with, and which I applied among others on this occasion, is by the residuum of discharges, measured by Mr. Lane's electrometer. It is well known, that the worse the conductors are that form the circuit, the greater residuum will be left in a jar after a discharge; and Mr. Lane's electrometer, which measures an explosion, will likewise measure the residuum. To apply this method with accuracy, I put pieces of charcoal, &c. of the same length into the circuit, I used the very same chain in every experiment, and the same disposition of every part of the apparatus; I also made the explosions exactly equal, and after every discharge compleated the circuit by the chain before I took the residuum; and lastly, I was careful to take up the same time in each operation, which I repeated very often. This method of measuring the conducting power of substances I learned of Mr. Lane.

IN the prosecution of these experiments on charcoal, I burned a piece which I had found to be a most excellent conductor, first between two crucibles, and then in the open fire, and tried it at different times till it was almost burned a way; but contrary, to my expectations, I found its property very little diminished. I was, likewise, surprized to find that *scit* whether of wood coal,

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or pit coal hardly conducted at all. I made five or six inches of the foot of pit coal part of the electric circuit, which completed the communication between the inside and outside of a charged jar for several seconds; and yet found the charge not much diminished. A piece of wood foot, which is a firm shining substance, which does not soil the fingers, and which seems to break in a polish in several places, would hardly conduct any part of a charge in the least sensible degree. When rubbed against my hand, or my waistcoat in frosty weather (though it was difficult to find any part of it that was large and smooth enough for the purpose) I more than once thought it attracted the thread of trial. The snuff of a candle would not conduct a shock, though it was placed in the middle of the circuit, and it was easily set on fire by the explosion of a small jar.

BUT notwithstanding my want of success, I make no doubt, but that any person of tolerable sagacity, who has an opportunity of making experiments in a laboratory, where he could reduce to a coal all kinds of substances, in every variety of method, might very soon ascertain what it is that makes charcoal a conductor of electricity. In all the methods in which I could make charcoal, the fume of the bodies was suffered to escape; but let trials be made of substances, reduced to a coal without any communication with the open air, or where the vapours emitted from them shall meet with different degrees of resistance to their escape, ascertained by actual pressure.

CHARCOAL, besides its property of conducting electricity, is, on many other accounts, a very remarkable substance; being indestructible by any method, besides burning in the open air; and yet it seems not to have been sufficiently studied by any chymist. A proper examination of it promises very fair, not only to ascertain the cause of its conducting, and, perhaps, of all
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conducting powers; but to be an opening to various other important discoveries in chymistry and Natural Philosophy; and the subject seems to be fairly within our reach.

PIT COAL, and probably all other substances, at the same time that they lose much of their weight, increase considerably in their bulk in the operation of charring. Does it not seem to follow from hence, that its conducting power may possibly be owing to the largeness of its pores, agreeable to the hypothesis of Dr. Franklin, that electric substances have exceeding small pores, which dispose them to break with a polish.

OR, since the calces of metals, which are electric bodies, become metals, and conductors, by being fused in contact with charcoal; are not metals themselves conductors of electricity, in consequence of something they get from the charcoal?

THIS course of experiments, however, evidently overturns one of the earliest, and, hitherto, universally received maxims in electricity, viz. that *water* and *metals* are conductors, and all other bodies non-conductors: for we have here a substance, which is clearly neither water, nor a metal, and yet a good conductor.

SECTION

SECTION IV.

EXPERIMENTS ON THE CONDUCTING POWER OF VARIOUS
SUBSTANCES.

FINDING some contrariety of opinion among electricians about the nature of ICE, some saying it was a conductor of electricity, and others a non-conductor, so as even to be capable of being charged like glass, I took the opportunity of a pretty severe frost, in the month of February, to assure myself of the fact.

IN order to this, I took a large piece of ice, washed it very clean, and scraped off all the sharp points about it. After this, when it was again perfectly frozen, I insulated it, at night, in the open air, whither I had carried my machine on purpose, at the same time that it was freezing intensely.

WHEN, by drawing a feather over its surface, I found it to be perfectly dry, I electrified it, and fetched large sparks, not less than an inch in length, from all parts of it. I charged a jar at it, almost as well as at the prime conductor; I also discharged the jar through it, and along the surface of it, in several places; so that I had no doubt, but that ice was, nearly, as good a conductor

ductor of electricity as water. To try the same to more advantage, I took a charged jar into the open fields; and, by means of a great length of chain, discharged it along a large surface of ice on a pond, whilst the surface was very dry, and the frost continued very intense. But the ice being not so good a conductor as metal, if the chain communicating with the outside of the jar happened to lie five or six inches from the knob of the wire communicating with the inside, the fire would strike to the chain, along the surface of the ice, without entering it.

SNOW is evidently not so good a conductor as ice; probably because its parts do not lie in contact with one another, as those of ice.

FINDING also that electricians were not perfectly agreed about the conducting power of *hot glass*, and that the methods which had been used to prove it were liable to objection; since, when the electricity was communicated along the outside of the glass, it might be said that the hot air, and not the hot glass was the conductor; it occurred to me, that the following experiment would determine this affair, in a more satisfactory manner than it had hitherto been done.

I PROCURED a glass tube, about four feet long; and, by means of mercury in the inside, and tin-foil on the outside, I charged about nine inches of the lower part of it. Then carefully slipping off the tin-foil, and pouring out the mercury, I heated the charged part of the glass red hot; and found, upon replacing the coating, that it was discharged.

I MADE the experiment a second time, with the same success; so that I had no doubt, but that glass, when red hot, was pervious to the electric fluid. It could not have gone round from

the inside to the outside, without going over a surface of six feet of glass, the greatest part of which was kept very cold, and all exceeding dry.

THAT the charge had not been lost by changing the quick-silver was evident: for when I repeated that part of the experiment, without heating the glass, the charge was found to be very little diminished.

SOME time after, when I was preparing some baked wood for the purposes of insulation, I found, that if I used them soon after they were taken out of the oven, they would not answer my purpose at all. The electricity went off by them to the floor. But when they had stood, in the very same situation, till they were cold, they insulated very well.

UPON this, I made a piece of baked wood, which I had formerly used for insulation, pretty hot; and when it was so hot, that I could hardly hold it in my hand, it took a slender spark from the conductor, about an inch long; but it would not discharge a jar at once. It did it however silently, pretty much like moist wood.

THE consideration of the conducting power of charcoal, and the manner in which it is made, namely, by burning inflammable substances in a close place, and generally without flaming, led me to make a few experiments on the conducting power of the effluvia of flaming bodies, at the very time of their emission: for whatever those effluvia be, they seemed in some measure to contain the conducting principle. That substance, or principle, or whatever it be called which can only be exhaled in flame, and not in smoke, does in this case make a body a conductor; whereas if it be suffered to escape, the conducting power is diminished.

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THE conducting power of the flame of a candle was observed very early; but it was not compared with that of other things, and it had by some been supposed to be nothing more than the heat communicated to the neighbouring air. The experiments I am going to recite seem to overturn this hypothesis, and to establish that mentioned above.

MARCH the 14th. a small charged phial held not longer than a second within two or three inches of the flame of a candle, either above or below it, where the heat was altogether inconsiderable, and the rarefaction of the air in a manner nothing, was totally discharged. The event was the same when I used the flame of a wax candle, or the flame of spirits of wine. When it was held much nearer to a red hot poker, it was not discharged near so soon; and when it was held exceeding near to a piece of red hot glass, it was not discharged at all, except by one explosion, seemingly conducted by the hot glass. Similar experiments were made by placing the candle, the poker, and the hot glass near the prime conductor. It was also found, that the small phial above-mentioned could not be discharged in the focus of a concave mirror.

BUT the small jar above-mentioned was discharged in these experiments silently; and though they seemed to be clearly in favour of the conducting power of the effluvia, which pass off in flame, there was nothing very striking in them; but afterwards, when I had constructed an electrical battery, I repeated the experiments in a much more striking and convincing manner.

DECEMBER the 15th. I brought the flame of a candle between two brass knobs, one communicating with the inside, and the other with the outside of the battery; and observed, that as the flame advanced towards them, it began to be put into a quiver-

ing motion, exceeding quick, and was strongly drawn both ways towards each knob, leaving the wick bare at the top; and as soon as the flame was quite between the rods, the battery discharged at once, at the distance of three inches and an half. This is a very fine experiment. The interposition of the flame between the two brass rods is like putting fire to a train of gunpowder, which explodes immediately.

WHEN I advanced the ignited wick of a candle, just blown out, towards the rods, it was ventilated exceeding briskly; and when it was put between them, when separated to about the distance of an inch, the discharge was made, and the candle blown in again.

To compare the conducting power of flame with that of other bodies, which had more heat but less effluvia, I put a red hot poker between the two rods, but it did not promote the discharge of the battery till they were brought within about an inch and an half of one another; so that the explosion was made at about twice the usual distance, allowing for the space occupied by the poker itself; and yet the air in the neighbourhood of the poker was more than ten times hotter than in the neighbourhood of the candle, considering the distance at which they were held from the rods. Both sides of the hot poker were marked with an imperfect circle, like those that were impressed on each of the knobs; an account of which will be given hereafter.

I THEN interposed a piece of red hot glass, which has as great a heat as the iron, but emits less effluvia; but it did not promote the discharge till the brass rods were brought within an inch of one another, which was so near, that the glass almost touched them both.

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As I was diversifying the experiments concerning the passage of the electric explosion over the surfaces of various bodies, as will be mentioned hereafter, I accidentally discovered how exceedingly poor a conductor is *oil* of every kind; inasmuch that I think it ought rather to be classed among electric substances; though before that time, I imagined that oil did not differ very much from water, with respect to its conducting power. I had been led into the mistake by some experiments of Mr. Willson, who has some where advanced the proposition above-mentioned; and argues that the tourmalin is possessed of a fixed kind of electricity, incapable of being conducted away, because it retains the separate power of each of its sides, though surrounded with melted grease; whereas I find, that nothing of an oily nature will conduct electricity.

LAYING a chain, which communicated with the outside of my battery, in a dish of melted tallow, I brought a brass rod communicating with the inside towards it, in order to make the discharge, by transmitting the explosion over the surface without entering it; when I was surprised to find, not only that the electric matter would not take the surface, but that, though it attracted a column of tallow at the distance of about three quarters of an inch (which was thicker in proportion as the rod was brought near the surface) and though I continued amusing myself with this column of tallow a considerable time; in which state it formed a complete communication between both sides of the battery, yet the charge was very little dissipated. I repeated this experiment, with the same event, with oil of olives, the thinnest oil of turpentine, and even ether. A plate of common oil of olives connected the inside and outside of the battery for near ten minutes, without my being able to perceive that the charge was more dissipated, than it would have been without that communication.

cation. Ether is the lightest fluid in nature next to air; yet, being properly an oil, it proved no better a conductor than the most tenacious. I was most surpris'd that the ether did not take fire by this treatment, as nothing is more inflammable; and if the electric matter can pass through it, nothing fires so soon.

FROM these experiments, and those above-mentioned, on ice, I concluded, that fluidity, as such, contributes nothing to the conducting power of substances, separate from the heat which makes them fluid. To complete my experiments on oils, I filled phials with all kinds of oils, according to their chymical distinctions, including the finest *essential oils*, the strongly *empyreumatic*, and those that are termed mineral, as *oil of amber*; and found them all incapable of giving a shock. But I found that this method of trying the conducting power of substances, viz. by inclosing them in phials, and endeavouring to give shocks by them is very inaccurate, showing them to be better conductors than they really are. Pounded glass, flour of brimstone, and other electric substances gave a considerable shock; but a bottle containing nothing but air gave a greater shock than any of them; though the wire inserted into it was very blunt, and was kept in the center of the bottle. Finding, by these experiments, that oil plainly conducted much less than air, I endeavoured to charge a plate of oil like a plate of glass; and for this purpose I perforated a glass salver, and thereby gave a coating of tin-foil to both sides of a quantity of oil poured into it; but the brim of the salver would not contain enough to give it a sufficient thickness; otherwise, I make no doubt, but that a shock might be given by it better than by air.

I SHALL just mention upon this subject, what I lately observ'd, and do not know whether it has been noticed by any writer, that

that *ice of oil*, contrary to ice of water, is specifically heavier than the fluid substance, and sinks in it. *

FINDING so great an agreement, with respect to electric properties, in this whole chymical class of bodies, I began a kind of course of chymical electricity; but had not leisure, or opportunity to pursue it as it deserved. The few hints that I collected may possibly be of service to future inquirers; and for this reason I shall note them just as they occurred, though they contain little that is remarkable.

ALL *saline substances* that I examined proved, in general, pretty good conductors. I tried most of them by making the discharge of the battery through them when insulated; which appears to me to be a very good method, indeed the only one that can well be depended upon. In discharging the battery with a piece of *alum*, the explosion was attended with a peculiar hissing noise, like that of a squib. *Rock salt* conducted pretty well, but not quite so well as the alum. The electric spark upon it was peculiarly red. *Sal ammoniac* exceeded them both in its conducting powers, but it would not take the least sensible spark; so that it seemed made up of an infinite number of the finest points. *Volatile sal ammoniac* I only tried in a phial, when it gave a small

* OTHER persons, I find, have made experiments, which show how imperfect a conductor oil is. The following proofs of this are exceeding curious and pleasing. J. F. Cigna, Doctor of Medicine in Turin, observed electrical attraction and repulsion between conducting substances plunged in oil. Noller's Letters, Vol. 3. p. 168.

MONSIEUR VILLETTE, optician at Liege, filled a dish of metal with oil, and when he had electrified the dish, he plunged a needle into the oil, and received a very strong spark as soon as the point of it came within a small distance of the dish. A small cork ball being made to swim in this oil, upon the approach of the thicker end of the stalk of a lime, plunged to the bottom, and immediately rose again to the top. Noller's Letters, Vol. 3. p. 312.

shock.

shock. *Salt petre* did not conduct so well as sal ammoniac. Endeavouring to make the electric explosion pass over its surface, it was dispersed into a great number of fragments in all directions with considerable violence, some flying against my face. *Selenitic salt* conducted a shock but poorly. *Vitriolated tartar* gave a small shock. *White Sugar* seems to be an exception to this rule: for it may be fairly said to be no conductor; as the charge of the battery would hardly pass through it in the least degree.

THE *metallic salts* in general conducted better than other neutrals: *blue and green vitriol* conducted very well, though they would not transmit a shock.

THAT ores in which the metal is really in a metalline state should be very good conductors might naturally be expected. Thus a piece of gold ore from Mexico was hardly to be distinguished, in this respect, from the metal itself; and a piece of silver ore from Potosi, though mixed with pyrites, conducted very well. But even ores in which the metal is mineralized with sulphur and arsenic, as the ores of lead and tin, and *cinnabar* the ore of quicksilver were little, if at all inferior to them. The *cinnabar* that I tried was factitious; but there can be no doubt of its being the same as the native. When I made the explosion of the battery pass through it, it was rent into many pieces, and the fragments dispersed in all directions. Ores, however, that contain nothing but the earth of the metal conduct electricity but little better than other stones; though I thought that all the specimens of iron ore that I tried conducted better than marble.*

* I find that Mr. Boze, very early, thought it was easy to distinguish the ores of metals from other earthy substances, by means of their greater conducting power. *Geschichte*, Vol. 1. p. 293.

I EXAMINED some *black sand* that came from the coast of Africa, which is a good iron ore, and part of which is affected by the magnet as much as steel filings; and found it to conduct electricity, but not a shock. Separating with a magnet all that would be easily attracted by it (about one sixth of the whole) it conducted a shock very well. The rest would hardly conduct at all.

THOUGH I think I may venture to say, that the true and proper ores of the more valuable metals might be known by their property of conducting electricity, I cannot say that electricity will furnish any rule to ascertain the value of the different ores of the same metal. I tried two pieces of copper ore, one the most valuable that is known, and another of only about half the value; but they were hardly to be distinguished from one another in their conducting power.

BLACK lead in a pencil conducted a shock seemingly like metal or charcoal. A small lump of it took as full and strong a spark from the prime conductor as a brass knob.

ALL the *stony substances* that I tried conducted very well, though dry and warm. Even a piece of polished *agate*, though semi-transparent, received the electric spark into its substance; though it would pass over about three quarters of an inch of its surface to reach the finger that held it, and it discharged the battery but slowly. *Limestone*, and *lime* just burnt were equally imperfect conductors, hardly to be distinguished from one another. *Lapis hæmatites*, and *touchstone* both conducted pretty well; as did a piece of *gypsum*, and *plaster of Paris*, only the latter, having a smoother surface, took a stronger spark. A piece of *slate*, such as is commonly used to write on, was a much better conductor than a piece of *free stone*, which conducted very poorly. Marbles also conducted considerably better than free stone. I

found very little difference among any of the specimens of marble that I tried, in which was a piece of Egyptian granite. A piece of Spanish chalk, which is a *talc*, conducted pretty much like marble.

A LARGE piece of white *spar*, with a tinge of blue, and semi-transparent, would hardly conduct in the least degree. I took pretty strong sparks from the prime conductor while it was in contact with it.

A PIECE of *pyrites* of a black colour took sparks at a considerable distance from the prime conductor, like some of the inferior pieces of charcoal. Another piece of *pyrites*, which had been part of a regular sphere, consisting of a shining metallic matter, did not conduct near so well, though much better than any other stony substance. It was a kind of medium betwixt a stone and an ore.

A PIECE of *asbestos* from Scotland, just as it is taken from its bed, would not conduct. It was in contact with the conductor, while I took sparks at the distance of half an inch with a moderate electrification.

OF *liquid substances*, oil of *vitriol* conducted pretty well, and the most highly rectified *spirit of wine* gave a shock much like water, but perhaps not quite so well.

THIS course of experiments on the conducting power of substances, according to their chymical classes, would, probably, be very useful, if pursued with care. Those mentioned above were generally single experiments, which are not so much to be depended upon.

THERE are some other *mixed substances* whose conducting power I have tried, and because I think it would not be easy to say, *a priori*, to which of the two classes they belong, I shall just mention

mention the result of my experiments upon them, nearly in the order in which they were made.

DRY glue, which is an animal substance, is a conductor of electricity, but does not conduct a shock.

POUNDED glass mixed with the white of an egg, and which had stood till it was perfectly dry, was a conductor. I had put it upon some broken jars, thinking that the composition would be an electric substance, and that it would make the jars hold a charge again.

PAINT, made of white lead and oil, very old, and dry, proved a conductor. I tried it in a china vessel which had been firmly pieced with it. A part of the vessel, through which there was no crack, would receive a charge very well; but a piece in which there was a crack, and which had been filled with this cement, could not be charged at all.

SECTION V.

EXPERIMENTS ON THE DIFFUSION OF ELECTRICITY
OVER THE SURFACES OF GLASS TUBES, CONTAINING A NEW
METHOD OF GIVING THE ELECTRIC SHOCK.

IT had been observed by many electricians, that new globes are often difficult to excite; but I have made some experiments, which prove this fact, and other differences between new and old glass, in a more distinct manner than any thing else I have yet met with; but they leave the cause still unexplained.

THE most remarkable property of new flint glass is the easy diffusion of electricity over its surface. I have several times got tubes made two or three yards long, terminating in solid rods. These I have taken almost warm from the furnace, in the finest weather possible, having immediately insulated them, and hanging pith balls at one extremity, have always found, that they would separate the moment that the wire of a charged phial was applied to the other end. This I had reason to think would be the case at almost any distance at which the experiment could be made. I have even charged a phial very sensibly, when it was held close to the glass, at the distance of a yard from the wire of a charged phial, held close to another part of it, the coatings of
both

both phials being held in my hands. When the same tubes were a few months older, I found that the electric virtue could not be diffused along their surfaces farther than about half a yard.

SOME tubes, which I have tried the day they were made, I have found impossible to be excited in the least degree, even with the use of oiled silk and amalgam, for an hour together; when a single stroke of the same rubber has rendered other tubes highly electrical, and two or three have made them to emit spontaneous pencils. The same new tubes, upon being much rubbed, have begun to be excited, and in a few days have acted pretty well.

BUT that the first coat of new glass is, in some measure, a conductor of electricity, was most evident from some experiments which I made with long and very thin tubes, which were blown some time in the month of March. These, to amuse myself, I coated in different places, and the diffusion of electricity, from the coated part to that which was not coated, appeared to me very extraordinary. I think my reader will not be displeased if I relate a few of the particulars.

I PROCURED a tube, open at both ends, about a yard in length, but of very unequal width. About three inches of the middle part of it I coated on both sides; and charging it, by means of a wire introduced at one of the ends; I perceived, not only that the part through which I had introduced the wire was strongly electrical on the outside, but that at the opposite end, where there was neither coating nor wire, the fire crackled under my fingers, as I drew the tube through them, and a flame seemed to issue continually out at both the ends, while it was at rest and charged. N. B. One end of this tube was broken, and rough, the other was smooth.

I PROCURED another tube, about an inch in diameter, and very

very thin. It was about three feet and a half in length, and closed at one end. About nine inches below the mouth, I coated three inches of it, both on the inside and outside. This part I charged, and then observed the whole tube, to the very extremity of it, to be strongly electrical, crackling very loud when I drew my hand along it, and giving sparks, as from an excited tube, at about the distance of an inch, all the way.

To give the reader a better idea of these experiments, I have given a drawing [Pl. i. fig. 7.] of one of the tubes with which they were made. It is open at one end, and the part [a] is coated.

AFTER drawing the whole tube through my hand, all the electricity on the outside was discharged; but, upon putting my finger within the mouth of the tube, an effort to discharge itself seemed to be produced, which showed itself by a light streaming visibly from the coating, both towards the finger and likewise as vigorously towards the opposite end of the tube. After this I found all the outside of the tube loaded with electricity as before, which might be taken off, and revived again many times, with the same original charge; only it was weaker every time.

HOLDING this tube by the coated part, and presenting the uncoated outside, near the close end of the tube, to the prime conductor, the inside became charged as well as the outside; and, upon introducing a wire, a considerable explosion was made.

THE discharge made the outside strongly electrical, and by taking this electricity off, the tube was charged again very sensibly.

HOLDING it by the uncoated part, and presenting the coated part to the conductor, the inside became charged as before.

HAVING first perfectly discharged this tube, I closed the open
end

end with cement, made of bees wax and turpentine, an inch or more in thickness; but still, by applying the outside of the tube (either the coated or the uncoated part) to the conductor, I found it manifestly charged, but not quite so high as when the end was left open, though the difference was not great.

I PROVIDED myself with another tube, about an inch and a quarter wide, and three feet long; but it was drawn out one foot more very small; and another foot at the extremity was solid, so that it was in all five feet long. I coated about four inches of this tube, two feet below the mouth of it. The balls being hung at the extremity of this tube, or rather of the solid rod in which it terminated, they separated the moment I began to charge the coated part. The discharge brought them together, though not immediately, but a second discharge would generally do it.

THE residuums of any of these tubes, of which so small a part was coated, were very considerable. I thought that all of them might be equal to the first discharge. In the last mentioned tube, there was a residuum after a great number of discharges, I believe twenty or thirty.

IMAGINING that the diffusion on the surfaces of the tubes above-mentioned depended upon the newness of the glass, I preserved them six or seven months; having observed by examining them at proper intervals in the mean time, that this property, and others depending upon it gradually lessened; and before this time it was quite gone. There was no diffusion of electricity over their surfaces, and they were as easily excited as other tubes, at the same time that they received a very good charge.

AT length, by some accident or other, all the tubes on which I had made these experiments were broken, except one, which was closed at one end, and which, indeed, was the most remark-

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able of them all. Upon this tube, in the month of November, I began to renew my experiments, comparing it with others which I got made at that time, in order to ascertain on what circumstances this diffusion of electricity depended. These I shall distinctly relate, noting the time when each experiment was made, and every other circumstance which I can imagine could possibly have any influence in the case.

NOVEMBER the 13th. I once more endeavoured to repeat the experiments above-mentioned with the old thin tube, with as much care and precaution as possible, but without the least success. At the same time I charged two other thin tubes, one closed, and the other open, after they had been made about six weeks, but without being used in the mean time, and they answered exactly as the former tube had done, when it was new. The charge from a small coated part diffused itself all over the tube; so that at the distance of a yard from the coating, it gave sparks to the finger of an inch in length, and in all respects exhibited the appearance of a tube fresh excited. On this occasion I first observed, what afterwards drew my attention in a more particular manner, that when my finger was brought to the tube about two inches above the coating [as at *b*. Pl. i. fig. 7.] it discharged a great quantity of that diffused electricity; and my whole arm was violently shocked.

NOVEMBER the 19th. After heating the old tube, and endeavouring to repeat the former experiments, both while very warm, and after it was cold again, but to as little purpose as before; I took it to the glass house, and got it made red hot all over, so that it would easily bend any way; and as soon as it was cold, I tried the old experiments, and found that it had completely recovered its former property. Charging a small coated part, the electricity was diffused to the end of the tube, over
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three feet of dry glass, and it gave sparks at the distance of an inch in any part of it, exactly as if it had been excited with the best rubber. When it was drawn through my hand, whereby that diffusion was taken off, it presently returned again; and the extremity of the tube would get loaded while its communication with the coating had been cut off, by my hand being constantly held on the middle of it.

I ALSO observed, that the middle part of this tube, which had been ofteneft heated, in melting the whole over again, one half at a time, had a much stronger diffusion than the other parts. It was no sooner taken off, than it appeared again, so that it gave a continual stream of fire.

THE quantity of *residuum* after a discharge of this tube was prodigious, so that the outside coating would, immediately after, give almost a constant stream of fire to any conductor presented to it, for a considerable time.

THIS tube was now, as it had been at the first, absolutely incapable of being excited with the best rubber.

JANUARY the 6th. 1767. Examining all the tubes with which I had made the experiments of the diffusion, I found that property either quite, or very nearly gone. One of them I restored by heating it red hot. Another I heated only at the end most remote from the coating; but there was no diffusion upon it, when the coated part was charged; the part which had not been made red hot intercepting it.

NOVEMBER the 24th. In order to determine whether this property of diffusion depended in any measure upon the smoothness of the surface, I made a circular part of one of the thin tubes, about half a yard beyond the coated part, quite rough with emery, about three inches in length; but this did not prevent the diffusion in the least; both that rough part, and the

smooth glass beyond it were as much loaded with the electricity as the rest.

I THEN took the polish off a line the whole length of the tube, from the coating to the extremity of it; but still the effect was the same: and I make no doubt would have been so if I had made all the surface rough.

IN order to ascertain whether this property depended upon the thinness of the tubes, I got one made of a twelfth of an inch thick, and used it immediately; the diffusion was very sensible, and it was incapable of being excited. This, however, was not always the case with tubes of so great a thickness.

NOVEMBER the 25th. Willing to carry this experiment a little further, I got another tube four feet long, and of the eighth of an inch thick. I coated a small part of it in the same manner as I had done the others, namely about three inches, at the distance of nine inches below the orifice, and observed the diffusion to be very remarkable in proportion to the charge it received, which was very moderate. It could not be sensibly excited in the least degree; except that, in the dark, an exceeding small light was visible near the finger, when it touched any part of it, immediately after excitation; but not the least snapping could be perceived, nor any thing felt with the finger.

To find whether this property depended upon the kind, as well as the newness of the glass, I afterwards, coated a part of a very thin glass of the common bottle metal, but I found no diffusion upon it at all. It was what is commonly called a singing glass. I would have pursued this experiment by trying the same glass in other forms, and by trying other kinds of glass, but I had no opportunity.

I OBSERVED, in all the tubes which had the diffusion, that

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in drawing my hand from the extremity of them towards the coating, after they were charged, so as to take off the diffusion, there was a considerable noise at the orifice, as if the tube had been gradually discharging itself, and this operation did apparently lessen the charge.

IN the dark the electric fire seemed to pour perpetually from the open end, or both the ends, if they were both open; and whenever I drew my hand over it, the fire streamed from the coating towards my hand in a very beautiful manner.

IT was very remarkable, that, the first time I charged any of these tubes after they had stood a while, the diffusion was the most considerable, and that it lessened every successive charge; till, at last, it was exceeding small; but after the tube had stood a few hours uncharged, it was as vigorous as ever.

DECEMBER the 1st. I, for the first time, took particular notice, that, in charging a thin tube, and afterwards holding the coating in one hand, and drawing my other hand, so as to grasp the tube; beginning at that end which was most remote from the coating; that sometimes, when my hand came near the coating [as at *b*. Pl. i. fig. 7.] I received a very considerable shock through both my arms and in my breast, exactly like what is felt from the Leyden phial.

THE same day, I felt a similar shock from another thin tube; and what was more remarkable, I did not receive it till the third time of drawing my hand over the tube, having missed the stroke the two first times; though I moved my hand, as near as I could judge, in the same manner. This shock was not very great, but sensible in both arms.

DECEMBER the 3d. I received another shock, the third time of drawing my hand over the tube, and much more violent than the last; it affecting both my arms and breast. At this time I

observed very exactly, that my hand was near two inches and an half from the coating, and that a strong light was visible under my hand, and extended to the coating. The diffusion at this time had not been very great, and the tube seemed to be about half discharged after the shock.

AT that time I could not think of any plausible theory to account for this shock; but presently after I accidentally received another shock, in some respects similar to this, the theory of which I have been so happy as to investigate, and which may throw some light upon this.

DECEMBER the 21st. I made a torrecellian vacuum in a tube about a yard in length [Pl. i. fig. 8.] and holding one end of it in my hand, I presented a part near the other end to the prime conductor; and observed, that, while the electric fire was pouring along the whole length of it, I felt some peculiarly smart twitches every now and then in my hand, just such as are felt when a thin uncoated phial is held in the hand, while it is charged at the prime conductor, but more pungent. On removing the tube from the prime conductor, it threw out spontaneous sparks from the place where it had touched the conductor, exactly like those which issue from the wire of an overcharged phial; but they were longer, and much more beautiful. Then, bringing my other hand near the place where the tube had touched the conductor, I received a very considerable shock in both my arms and breast, exactly like that which I had received before from the thin tubes; and, as with them, the shock was rather stronger in the hand which was brought to the tube than in that which held it. If, without bringing my other hand to the tube, I only presented it to the table, or any other conductor, it would throw out from the same place several strong sparks, attended with a flash of light, which filled the whole length

length of the tube. These sparks resembled those which issue from the wire of a charged phial, when it is presented to the like imperfect conductors, and at the same time held in the hand.

I AFTERWARDS observed, that the strongest shock which this tube could give was felt when one hand continued in the place where it held the tube, in order to charge it, and the other was made to touch the tube, an inch or two above it [as at *c* Pl. i. fig. 8.] and at the instant of the stroke a very dense spark of electric fire was seen darting the whole length of the tube. When three persons besides myself joined hands, it shook all our arms greatly.

THE tube could not be discharged by putting one hand so near the other, unless that part of the tube had been brought to the conductor in charging it; and if any particular part of the tube only, had been brought to the conductor, the discharge could not be made without touching that part.

WHEN the tube had given a shock from any one place, it would give one or two more smaller shocks, from other places.

THE experiments I made with this tube being certain and invariable, and the shock I received from the other tubes precarious, I gave more particular attention to this, in order to ascertain the nature of this shock; thinking that, if I could accomplish this, it would assist me in the investigation of the other. Accordingly, I coated about six inches, near each end of the tube, [*a* and *b* Pl. i. fig. 8.] leaving the space of about half a yard of uncoated glass between the coatings; and observed, that when I held one of these coatings in my hand, and presented the other to the prime conductor, it always received a considerable charge, and was discharged in one bright spark at the distance

distance of above an inch, and sometimes two inches, if, besides the coated part, I had likewise presented the uncoated part to the prime conductor; and sometimes the uncoated part would discharge itself by a bright flash to the lower coating, leaving the coated part charged as before. If I held the tube by the middle, where there was no coating, and presented one of the coatings to the conductor, it received a pretty good charge.

I THEN stood upon an insulated stool, and presenting one of the coatings to be charged, while I held the other; I observed, that it received not more than one fourth part of the charge it had before; upon which I immediately concluded, that the lower coating must have been charged negatively, whilst the upper was charged positively. This was quite confirmed by observing, that sparks could be drawn from my body, while I stood upon the stool presenting the tube to be charged, but no longer than till the tube had received its full charge; and that then the explosion was as great as it had been when I stood upon the floor.

WHEN I insulated the tube, by placing it in a glass vessel, it was still less capable of taking a charge than when I stood upon a stool and held it, this method making a more perfect insulation. If any conductor was presented to the lower coating while the other was held to the prime conductor, sparks issued from it very plentifully, till it had got a considerable charge; when those sparks entirely ceased, and the tube, upon trial, gave a very great explosion.

THESE experiments make the theory of this new method of giving the electric shock pretty obvious. The electric matter thrown upon the upper coating repels an equal quantity from the inside of the tube opposite to it; which, passing freely through the vacuum, (as is visible in the dark) is accumulated
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on the inside of the other extremity of the tube, and thereby repels a quantity from the lower coating: so that the two coatings being in opposite states, though on the same side of the tube, the same kind of shock is given by them, as if they had been on opposite sides.

BEING fully satisfied with the experiments made by these two coatings, and the theory of them, I amused myself with coating the middle part of the tube in various ways.

WHEN the three coatings were about the same size, and placed at equal intervals; which ever of them was held in the hand, the other two were charged and discharged separately. If the coating of one of the ends was held in the hand, and the other two were charged, the greatest explosion was from that which was discharged first. If those two coatings were placed near one another, they were both discharged by the attempt to discharge either of them, and a flash of light was seen betwixt them both. In this case the explosion was sometimes made at the distance of two inches and a half.

WHEN the middle coating was made very large, and placed contiguous to the upper, the explosion was less; a spontaneous discharge being soon made to the lower coating.

WHEN the middle coating was taken away, it often happened that, in drawing the whole tube over the prime conductor, beginning at the upper coating; when it came to the lower, by which I held it, a spark would dart to it from all the uncoated part of the tube, which discharged the electricity of that part, while the upper coating still retained its proper charge.

WHEN this spontaneous discharge was not made, the explosion might be made at twice, once at the naked glass, near the lower coating, and again at the upper coating. If the discharge was first made at the upper coating, there remained very little for the
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the lower part of the tube. And if the explosion was made about the middle of the tube, the whole was discharged at once, and in a very beautiful manner.

I MUST leave my reader to compare the theory of this shock with that given by the long and open tubes, as I am not able to do it to my entire satisfaction without more experiments; which, as I observed, are precarious, and which I had not leisure to attend to.

As this course of experiments was begun by an accidental observation of the different electric properties of new and old glafs, I shall (after this long excursion, which I little foresaw) conclude with an experiment or two relating more immediately to the original subject of them.

IMAGINING that the difference between new and old glafs might be owing to the larger superficial pores of the former, which made it approach to the nature of a conductor, and which contracted with time; I thought it might possibly be determined by the experiment of the metallic tinge, the wider pores receiving it better than the smaller; and I was not disappointed in my expectations. November the 19th. I several times laid two glafs tubes, one a very old one, and the other quite new, close together, with a piece of leaf gold or copper between them; and though I varied the disposition of them in every way that I could think of, and changed the tubes for others; I always found the new glafs to receive a much fairer, more beautiful, and indelible impression than the old glafs. Twice the quantity of the metal was in all the cases struck into it.

SECTION

SECTION VI.

EXPERIMENTS TO VERIFY SEVERAL PARTICULARS OF SIGNIOR BECCARIA'S THEORY OF ELECTRICITY; PARTICULARLY CONCERNING THE ELECTRIC MATTER CARRYING INTO ITS PATH LIGHT SUBSTANCES TO ASSIST ITS PASSAGE.

BEING greatly struck with Signior Beccaria's theory concerning the passage of the electric matter from the earth to the clouds, previous to a thunder storm, and thinking his experiments to prove the power of electricity to conduct into its path light substances that could assist its passage not quite satisfactory, I endeavoured to ascertain the fact in a better manner, and shall lay before my readers the result of my experiments.

NOVEMBER the 9th. I discharged frequent shocks, both of a common jar, and another of three square feet, through trains of brass dust, laid on a stool of baked wood, making interruptions in various parts of the train; and always found the brass dust scattered in the intervals, so as to connect the two disjointed ends of the train; but then it was likewise scattered nearly as much from almost all other parts of the train, and in all directi-

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ons. The scattering from the train itself was, probably occasioned by small electric sparks between the particles of the dust; which, causing a vacuum in the air, drove all that light matter to a considerable distance. But the particles of the dust which were strowed in the intervals of the train, some of which were, at least, three inches, could hardly be conveyed in that manner.

WHEN small trains were laid, the dispersion was the most considerable, and a light was very visible in the dark, illuminating the whole circuit. It made no difference, in any of these experiments, which way the shock was discharged.

WHEN I laid a considerable quantity of the dust at the ends of two pieces of chain, through which the shock passed, at the distance of about three inches from one another, the dust was always dispersed over the whole interval, but chiefly laterally; so that the greatest quantity of it lay in arches, extending both ways, and leaving very little of it in the middle of the path. It is probable, that the electric power would have spread it equally, but that the vacuum made in the air, by the passage of the fluid from one heap of dust to the other, dispersed it from the middle part.

I THEN insulated a jar of three square feet, and upon an adjoining glass stand laid a heap of brass dust; and at the distance of seven or eight inches a brass rod communicating with the outside of the jar. Upon bringing another rod, communicating with the inside, upon the heap of dust, it was dispersed in a beautiful manner, but not one way more than another. However, it presently reached the rod communicating with the outside.

MAKING two heaps, about eight inches asunder, I brought one rod communicating with the inside upon one of them, and another rod communicating with the outside upon the other. Both the heaps were dispersed in all directions, and soon met; presently

presently after which the jar was discharged, by means of this dispersed dust, in one full explosion. When the two heaps were too far asunder to promote a full discharge at once, a gradual discharge was made through the scattered particles of the dust.

WHEN one heap of dust was laid in the center of the stand, and the two rods were made to approach on each side of it, they each attracted the dust from the side of the heap next to them, and repelled it again in all directions. When they came very near the heap, the discharge was made through it, without giving it any particular motion.

ALL these experiments show that light bodies, possessed of a considerable share of electricity, disperse in all directions, carrying the electric matter to places not abounding with it; and that they sometimes promote a sudden discharge of great quantities of that matter from places where it was lodged, to places where there was a defect of it. But an accident led me to a much more beautiful, and perhaps a more satisfactory manner of demonstrating the last part of this proposition, than any that I hit upon while I was pursuing my experiments with that design.

DECEMBER the 11th. Hanging a drop of water upon the knob of a brass rod communicating with the inside of my battery, in order to observe what variety it might occasion in the circular spots, which will be mentioned hereafter; I was greatly surpris'd to find the explosion made all at once, at the distance of two inches.

I, AFTERWARDS, put some brass dust upon a plate of metal communicating with the inside of the battery, and making the discharge through the dust, it exploded at the distance of an inch

and a half. The dust rose towards the discharging rod, and from thence was dispersed in all directions.

THESE experiments are the more remarkable, as they demonstrate so great a difference between the distance at which the battery may be made to discharge at once, by the help of these light bodies, and without them. The discharge of the battery by the knobs of brass rods, in the open air, is at the distance of about half an inch; but, by this means, we see it made at three or four times that distance.

UNLESS a person try the following experiment, he will hardly conceive the extreme probability of the clouds and the rain being possessed of an electric virtue, in order to their uniform dispersion, according to Signior Beccaria's theory. Put a quantity of brass dust into a coated jar, and when it is charged, invert it, and throw some of the dust out. It is very pleasing to see with what exact uniformity it will be spread over any flat surface, and fall just like rain or snow. In no other method can it be spread so equably.

It is taken for granted by Signior Beccaria and others, that persons are sometimes killed by lightning without being really touched by it, a vacuum of air only being suddenly made near them, and the air rushing out of their lungs to supply it; and with so much violence, that they could never recover their breath. As a proof of this, he says, that the lungs of such persons are found flaccid; whereas, when they are properly killed by the electric shock, the lungs are found inflated. This account always appeared to me highly improbable. It determined me, however, to make a few experiments, in order, if possible, to ascertain the fact with some degree of exactness. The result was as follows.

DECEMBER

DECEMBER the 18th. I placed that part of an egg shell in which is a bladder of air within an inch of the place where I made the explosion of the battery, on the surface of some quicksilver; when the bladder was instantly burst, and the greatest part of it torn quite away. The shell was quite dry; so that the bladder could not stretch in the least.

IT is evident from this experiment, that there is a sensible expansion of the neighbouring air, to fill a vacuum made by the electric explosion; but that this is so considerable as to occasion the suffocation and death of any animal, is, I think, very improbable from the following facts.

I PUT a cork as slightly as possible in the mouth of a small phial; but, though I held it exceeding near the place of the explosion, it was not drawn out.

I MADE the explosion pass over the surface of a moist bladder, stretched on the mouth of a galley pot; but it produced no sensible effect upon it. I also held at one time the bill of a robin red breast, and at another time the nose of a mouse near the electric explosion, but they did not seem to be at all affected by it. In order to examine the state of the lungs, I killed small animals by shocks discharged both through the brain, and through the lungs; but when they were dissected there appeared no difference. The lungs were in the very same state as when they were killed in another manner.

To these miscellaneous experiments, intended to verify several particulars of Signior Beccaria's theory of electricity, I shall add a small set, which, though they were begun before I had seen that author, are in some respects similar to his curious experiment of discharging a plate of glass hanging by a silken string without giving motion to it; his being designed to ascertain the effect

effect of the discharge upon the glass, and mine respecting the conducting substances that formed the circuit.

OCTOBER the 7th. To determine, if possible, the direction of the electric fluid in an electric explosion, I hung several brass balls by silken strings, and discharged shocks through them, when they were as much at rest as I could make them; but I could not perceive that any motion was given to them by the stroke. Afterwards, I discharged a jar a great number of times through small globules of quicksilver, laid on a smooth piece of glass; but could not perceive that they were driven one way more than another, though they were often thrown into disorder; probably by the repulsion of the air, occasioned by the vacuum of the explosion.

I THEN placed four cork or pith balls, at equal distances, upon a stool of baked wood, with a piece of chain at the same distance from the outermost balls; and observed, that, upon every attempt to make a discharge, the two middle balls were driven close together, while the two outermost were each of them attracted to the piece of chain that was next to it. Then, laying a great number of bits of threads in the same manner upon the stool, several of the pieces that lay near the chain stuck to them, and a great number of those that lay in the middle were driven together in a heap.

THE attraction to the chains I attribute to the electricity given to them by their connection with the jar, which would be greatly increased by the attempt to make an explosion; and the crowding together of the pieces in the middle of the circuit, I attribute to the current of air blowing them together from both the extremities of the chain. Thus part of the flame of a candle next to an electrified point will be attracted by the power of
electricity,

electricity, while the rest of the flame will be repelled from it by the current of air.

THESE experiments led me to make a discharge through an insulated bell, in order to observe in what manner it would be affected by the electric shock only, when it was not touched by any thing else. Accordingly, I made the discharge of the battery through it several times; and by each explosion it was made to ring, as loud as it could be made to do with a pretty smart stroke of ones finger nail.

I ALSO made a discharge of the battery through the external coating of a glass jar, but without touching it with the discharging rods; and it plainly produced the same tone, as when it was rung by percussion.

SECTION VII.

VARIOUS EXPERIMENTS RELATING TO CHARGING AND DISCHARGING GLASS JARS AND BATTERIES.

AS several things have occurred, in the course of my experiments, relating to charging and discharging both common jars and large electrical batteries, which I have not seen in the writings of any electricians; and as some of the facts are not easily accounted for, I shall mention a few of the more remarkable of them, just as they happened.

APRIL the 28th. As I was amusing myself with charging three jars of the ordinary size, while they stood upon a metal plate on the table, with their wires at different distances from the same prime conductor, which was fixed on pillars of baked wood; I observed, that whenever one of the jars, which stood next to the conductor, discharged itself, the others would discharge themselves too; though they were far from having received their full charge, being placed at a greater distance from the common conductor, and consequently having taken but few sparks, in comparison of that which stood the nearest.

A VARIETY of experiments seem to show, that, while a jar continues charged, the electric matter is continually insinuating itself

itself farther and farther into the substance of the glass, so that the hazard of its bursting is the greatest some time after the charging is over.

MAY the 26th. After having charged forty one jars together, each containing about a square foot of coated glass, I let them stand about a minute and a half, while I was adjusting some part of the apparatus, in order to make the discharge; when they exploded by the bursting of one of the number. I observed also, that the jar which was burst was at a considerable distance from the place where I saw the flash at the wires. It was also broke through in two different places.

FOR the same reason, there is no being sure that jars, which have stood one discharge, will bear another equally high. I am confident that several of mine have burst with a much less charge than they had actually held before.

JUNE the 29th. A jar of an ordinary size, which had been in constant use for several months, and which had discharged itself more than a hundred times without any injury, at length burst, as I was discharging it at the prime conductor. The hole was at a different place from that at which the discharge was made, but this does not always happen. The tip of my little finger happened to lie very lightly on the place, and I felt it was burst by a small pricking, as of a pin, though the explosion at the conductor was nearly equal to that of any other discharge. The coating of a jar contiguous to one that is burst is always melted by the explosion.

JUNE the 25th. A small thin phial, which had been charged singly as high as it could bear, so as to have discharged itself, and also in conjunction with four others of its own size, burst by a spontaneous explosion, when it was charged in conjunction with a battery.

I HAD never heard of a jar bursting in more than one place, or more than one jar in a battery bursting at the same time; but I have often found, to my cost, that this event is very possible. In this case, there must be a discharge at more places than one at the same time: and, besides, it seems to follow from it, that whenever there is a solicitation, as we may say, to discharge at one place, the effort to discharge at every other place is encreased at the same time.

IT has frequently happened with me, that jars have been burst at the instant that I was making the discharge in the common way; and when I have come to charge them again, they have appeared to be burst, in some place of the battery where I never expected it. Two instances of this kind happened in the explosion mentioned above; but the most remarkable fact of this kind happened the 31st. of May, when a battery of about forty jars, each containing a square foot of coated glass discharged itself.

UPON examination, I found that six of the jars were burst, one had the tinfoil coating on the outside quite melted, in a circular spot about half an inch in diameter; and in the inside it was burned quite black, near an inch and an half. A second was melted, on the outside, about three quarters of an inch in diameter, and the black spot in the inside was two inches. A third had one hole made in the form of a star, more small cracks like *radii* proceeding from a center than could be counted. And there was hardly one of the jars that was burst with a single hole. Some were burst in seven or eight different places, of which some were very remote from others; but generally there was one principal hole, and several smaller, but independent ones, in the neighbourhood of it, as within half an inch, an inch, or two inches from it.

JUNE

JUNE the 14th. The above-mentioned battery discharged itself once more; when three jars were burst, and one of them, besides its principal hole, had a circular row of fractures, quite round the hole, at the distance of about half an inch. This appearance struck me as something very remarkable, but some light may perhaps be thrown upon it by a subsequent course of experiments. Each of the smaller fractures was about a tenth of an inch in length.

NOVEMBER the 17th. Having charged both my batteries, one of them, at that time, of thirty one square feet, and the other of thirty two, I made the explosion; when one jar in each battery was afterwards found to be broken. They broke at the instant of the discharge, so that I did not suspect what had happened. Both the batteries had frequently borne a much higher charge. In one of the smaller jars, the coating, besides being burst opposite to the hole, was rent about an inch and an half along a crack that was made from it.

WHEN jars disposed in batteries have been burst in this manner, I have never failed to observe one circumstance which appears to me truly remarkable. It is, that though, in this case, several passages be opened for restoring the equilibrium of the electric fluid, yet the whole seems to pass in the circuit that is formed for it externally. At least, the effect of the explosion is not sensibly diminished, upon any substances that are exposed to it. This I had a fair opportunity of observing when I was transmitting the explosion of the battery through wires of different metals. I found that the utmost force of the battery would do little more than melt a piece of silver wire on which I was trying it, and yet it was, at one time, totally dispersed by an explosion, in making which three jars were broke in different parts of the battery.

THE most remarkable fracture I remember, was of a jar an eighth of an inch thick, and which was therefore, for a long time, thought to be too thick for use. This jar, however, which had never held but a moderate charge, burst spontaneously; when there was found in it one hole like what is commonly observed, from which extended two cracks that met on the opposite side of the jar, so that it came in two parts: but, besides this, there were two other holes, barely visible to the naked eye, at some inches distance from the principal hole, and considerably distant from one another. Yet these holes, when examined with a microscope, were plainly fractures, like others made in the same manner, having a white speck in the middle. One of them was above the external coating, but not above the internal.

I HAD frequently been much surpris'd at the great distance at which several of my jars would discharge themselves, one of five inches being very common. This induced me to try at what distance I could make that spontaneous discharge.

FEBRUARY the 21st. I got a jar, eight inches and a half in depth, and three in diameter. Finding it discharged itself very easily, when coated in the usual manner, that is, about four inches from the top; I cut the coating away, till I had brought it within two inches and a quarter from the bottom; when it still retained the same property; and, at length, it burst by a discharge through a white speck of unvitriified matter, an inch and three quarters above the top of the coating.

I THEN procured a jar made blue with zaffre, seven inches and a half high, and two inches and an half in diameter. I coated of it only one inch and a quarter from the bottom, and yet it discharged itself very readily. I afterwards, by degrees, cut the coating down to little more than half an inch from the bottom, and still the discharge continued to be made as before. This
property

property it retained till the month of October following, when it was broke by an accident.

I HAVE another blue jar, of nearly the same size with the former, which is almost full of brass dust, but has no coating at all on the outside. Yet, if I set this jar upon the table, in contact with a single piece of brass chain, going quite round it, and lying upon the table, it will discharge itself the whole length of the glass. N. B. The manner in which the uncoated part of these jars becomes charged exhibits an exceeding beautiful appearance, especially in the dark; the fire flashing from the top of the coating, in the form of branches of trees, first on one side of the glass, and then on the other, and growing larger and larger till they go over the top.

I HAVE made some experiments, to try how thick a plate of glass may be charged, but I have not been able to ascertain this circumstance with any degree of exactness: I only found, that I was not able to give the least charge to a plate of glass half an inch thick, when it was not warmed. It was the bottom of a large glass tumbler; but meeting with it only upon a journey at the house of an ingenious electrician, I had no opportunity of making many experiments upon it. I imagine that warming it would have made it capable of being charged. Glass of a quarter of an inch thick will hold a pretty good charge.

MR. KINNERSLEY'S experiments, p. 400, leaving me no reason to doubt, but that Florence flasks were capable of receiving a charge like any other thin glass, which might be made a conductor by heat, I imagined I could soon construct a very strong and very cheap electrical battery out of them. Accordingly I procured a few, for a specimen, but was greatly surprized to find that the electricity went through them, when quite cold, like water through a sieve, without making any fracture in them:

them: for they continued to hold the same small charge, which was different in different flasks. Mentioning this disappointment to Mr. Canton, he informed me, that he had met with the same, and that the permeability of this kind of glass to the electric fluid was owing to small unvitricified parts which may be seen in them. I thought it might be of use to publish this fact, as it may prevent other persons from being disappointed in the same expectations.

As glass had generally been charged when it was smooth, and electrics which had the property of rough glass, when excited, were exceeding difficult to charge; I had the curiosity to try what might be done with *rough glass* itself. Accordingly, I first made part of a jar rough, connecting the inside with the outside coating; thinking that the roughness might possibly promote a spontaneous discharge; but I found it was not made in that place preferably to any other. I afterwards took the polish from all that part of the outside of a jar that was above the coating, but it was charged and discharged exactly as before. Lastly I made a plate of glass rough on both sides, taking off all its polish, and found that it received a charge as well as a smooth plate.

THE manner in which tubes and plates of glass have broken, when I have failed to strike a metallic tinge into them, by the discharge of an electrical battery, have sometimes been attended with circumstances which I cannot easily account for. The following are the facts.

DECEMBER the 3d. Endeavouring to fix a metallic tinge upon a flat piece of glass, it was broken by the explosion, parallel to the line along which the metal lay, at about an inch distance, but not where the tinge itself was.

IN attempting to give a metallic tinge to a part of a long glass tube, it broke, though not in the place where the tinge was made

made, but on the opposite side, which was shattered all to pieces. The leaf gold had been bound tight to the glass, under a piece of pasteboard, which covered the gold, but not all the tube. Another tube also was broken in large fragments where the metal had been put on, but into small splinters on the opposite side; and for the space of six or seven inches further, it was not broken at all on the side of the metal, but very much on the other side.

AT another time, in attempting the same thing with another glass tube; the end of it, which was near a foot distant from the place where the metal was laid, and which was a little cracked in an oblique direction, broke off in a round piece.

As few experiments have been published about *melting wires*, and procuring globules of metal by electrical discharges, and as several things have occurred in my attempts that way, which perhaps have not occurred to other persons, I shall mention a few of the most material circumstances. They will, at least, serve as a direction to those who may be disposed to attempt the same thing.

I HAD frequently attempted to procure those beautiful *globules of metal*, mentioned, p. 276, some of which I had seen with Mr. Canton; and for that purpose had made the discharge through small wires laid in the bottom of china bowls, &c. but always without success. At length, I thought of inclosing the wires in small tubes; and this expedient fully answered my purpose: for, November the 12th. discharging a battery of thirty two square feet through an iron wire inclosed in a small glass tube, I found innumerable globules of the metal, of very different sizes. The whole piece melted was about two inches. Breaking the glass tube, I found the inside surface uniformly covered with those globules, and a black dust, both fixed into the glass; so that they

they could not be separated, without tearing away part of the glass.

THINKING to avoid this inconvenience, I fixed the small wire in the center of a glass tube, of, at least, a quarter of an inch in diameter; but, upon the discharge, this tube, though much wider than the former, was uniformly covered with the globules, and the black dust, which stuck very fast to it, though the metal did not seem to have penetrated into the substance of the glass. When the tube was broken, I scraped off part of the black lining, and the part next the glass looked like a thin plate of the metal.

IMAGINING that the melted metal would not adhere so closely to a conducting substance, as it had to the glass, I inclosed the wire in a paper tube a quarter of an inch wide. Upon opening it after the discharge, it was found uniformly covered with that black dust, and the stain was every where indelible. Sparks of fire had been seen three feet from the place of discharge, but no part of the metal could be found.

I THEN confined the wire closer, wrapping it tight in paper. Upon the discharge, a great number of sparks were seen, for about a second of time, a quarter of a yard from the paper, which was burned through in several places. Very few pieces could be found; but those were pretty large and irregular. I now found, that, in order to produce these globules, the charge must be moderate, that when the charge was very high, the whole substance of the wire was dispersed in particles too small to be found; and, on the other hand, when the charge was not sufficient, the metal was melted into fragments too large to form themselves into regular globules.

WITH the same battery I once melted a piece of iron wire one seventieth of an inch in diameter, when a piece of it was thrown quite

quite across the table, to the distance of about six feet; where it fell upon a bureau, then tumbled down to the ground, and continued glowing hot all the time. At other times, sparks from melted iron have been thrown three yards, in opposite directions, from the place of the fusion, and continued a sensible space of time red hot upon the floor.

AT another time I had a very fine opportunity of observing what part of the conductors which form an electric circuit are most affected by the explosion: for, upon discharging a battery of fifty one square feet through an iron wire nine inches long, the whole of it was glowing hot and continued so for some seconds; the middle part growing cool first, while both the extremities were sensibly red. Upon examining it afterwards, both the extremities were found quite melted; an inch or two of the part next to them were exceeding brittle, and crumbled into small pieces upon being handled; while the middle part remained pretty firm, but had quite lost its polish, so that it looked darker than before.

SECTION VIII.

EXPERIMENTS ON ANIMALS.

AS I have constructed an electrical battery of considerably greater force than any other that I have yet heard of, and as I have sometimes exposed animals to the shock of it, and have particularly attended to several circumstances, which have been overlooked, or misapprehended by others; it may not be improper to relate a few of the cases, in which the facts were, in any respect, new, or worth notice.

JUNE the 4th. I killed a rat with the discharge of two jars, each containing three square feet of coated glass. The animal died immediately, after being universally convulsed, at the instant of the stroke. After some time, it was carefully dissected; but there was no internal injury perceived, particularly no extravasation, either in the abdomen, thorax, or brain.

JUNE the 19th. I killed a pretty large kitten with the discharge of a battery of thirty three square feet; but no other effect was observed, except that a red spot was found on the pericranium, where the fire entered. I endeavoured to bring it to life, by distending the lungs, blowing with a quill into the trachea, but to no purpose. The heart beat a short time after the stroke, but respiration ceased immediately.

JUNE

JUNE the 21st. I killed a small shrew with the discharge of a battery of thirty six square feet, but no other effect was perceived, except that the hair of the forehead was singed, and in part torn off. There was no extravasation any where, though the animal was so small, and the force with which it was killed so great. This fact, and many others of a similar nature, make me suspect some mistake, in cases where larger animals are said to have had all their blood vessels burst by a much inferior force.

IN all the accounts that I have met with of animals killed by the electric shock, the victims were either small quadrupeds, or fowls; and they are all represented as killed so suddenly, that it could not be seen how they were affected previous to their expiration. In some of my experiments, the great force of my battery has afforded me a pretty fair opportunity of observing in what manner the animal system is affected by the electric shock, the animals which I have exposed to it being pretty large; so that a better judgement may be formed of their sensations, and consequently of the immediate cause of their death, by external signs. I do not pretend to draw any conclusion myself from the following facts. I have only noted them as carefully as I could for the use of physicians and anatomists.

JUNE the 26th. I discharged a battery of thirty eight square feet of coated glass, through the head, and out at the tail of a *full grown cat*, three or four years old. At that instant, she was violently convulsed all over. After a short respite, there came on smaller convulsions, in various muscles, particularly on the sides; which terminated in a violent convulsive respiration, attended with a rattling in the throat. This continued five minutes, without any motion that could be called breathing, but was succeeded by an exceeding quick respiration, which continued near half an hour. Towards the end of this time, she was

able to move her head, and fore feet, so as to push herself backwards on the floor: but she was not able to move her hind feet in the least, notwithstanding the shock had not passed through them. While she continued in this condition, I gave her a second stroke, which was attended, as before, with the violent convulsion, the short respite, and the convulsive respiration; in which, after continuing about a minute, she died.

BEING willing to try, for once, the effect of a much greater shock than that which killed the cat upon a large animal, I gave an explosion of sixty two square feet of coated glass to a dog of the size of a common cur. The moment he was struck, which was on the head (but, not having a very good light, I could not tell exactly where) all his limbs were extended, he fell backwards, and lay without any motion, or sign of life, for about a minute. Then followed convulsions, but not very violent, in all his limbs; and after that a convulsive respiration, attended with a small rattling in the throat. In about four minutes from the time that he was struck, he was able to move, though he did not offer to walk till about half an hour after; in all which time, he kept discharging a great quantity of saliva; and there was also a great flux of rheum from his eyes, on which he kept putting his feet; though in other respects he lay perfectly listless. He never opened his eyes all the evening in which he was struck, and the next morning he appeared to be quite blind, though seemingly well in every other respect.

HAVING dispatched the dog, by shooting him through the hinder part of his head, I examined one of his eyes (both of which had an uniform bluish cast, like a film over the pupil) and found all the three humours perfectly transparent, and, as far as could be judged, in their right state; but the *cornea* was throughout

throughout white and opaque, like a bit of gristle, and remarkably thick.

BEFORE this experiment, I had imagined, that animals struck blind by lightning had probably a *gutta serena*, on account of the concussion which is seemingly given to the nervous system by the electric shock; whereas this case was evidently an inflammation, occasioned by the explosion being made so near the eyes, terminating in a species of the *albugo*; but which I suppose would have been incurable. One of the eyes of this dog was affected a little more than the other; owing, probably, to the stroke being made a little nearer to one eye than the other. I intended to give the stroke about an inch above the eyes.

IN order to ascertain the effects of electricity on an animal body, I, after this, began a course of experiments on the conducting power of its constituent parts; and for some time imagined that a piece of spinal marrow of an ox conducted sensibly worse than the muscular flesh; but after a great number of trials with pieces of spinal marrow from various animals, and pieces of muscular flesh, of the same size and form, and in various states of moisture and dryness, I gave up that opinion as fallacious; but I cannot help wishing the experiments were resumed with some more accurate measure of conducting power than hath yet been contrived.

BEING willing to observe, if possible, the immediate effect of the electric shock on the heart and lungs of animals, I gave, June the 5th. a shock from six square feet to a frog, in which the thorax had been previously laid open, so that the pulsation of the heart might be seen. Upon receiving the stroke, the lungs were instantly inflated; and, together with the other contents of the thorax, thrown quite out of the body. The heart, however, continued to beat, though very languidly, and
there

there was no other sign of life for about ten minutes. After that, a motion was first perceived under its jaws; which was propagated, by degrees, to the muscles of the sides; and at last the creature seemed as if it would have come to life, if it had not been so much mangled. The stroke entered the head, and went out at the hind feet.

JUNE the 6th. I discharged a battery of thirty three square feet through the head and whole extended body of another frog. Immediately upon receiving the stroke, there was, as it were, a momentary distention of all the muscles of the body, and it remained shrivelled up in a most surprising manner. For about five minutes there appeared no sign of life, and the pulsation of the heart could not be felt with the finger. But afterwards, there first appeared a motion under the jaws, then all along the sides, attended with convulsive motions of the other parts, and in about an hour, it became, to all appearance, as well as ever.

THE same day, I gave the same stroke to two other frogs. They were affected in the same manner, and perfectly recovered in less than three hours.

THESE facts surprized me very much. I attribute the recovery of the frogs partly to the moisture, which always seems to cover their body, and which might transmit a good part of the shock; and partly to that provision in their constitution, whereby they can subsist a long time without breathing. To ascertain this, I would have given the shock to toads, serpents, fishes, &c. and various other exanguious animals, but I had not an opportunity. Besides, it is paying dear for philosophical discoveries, to purchase them at the expence of humanity.

SECTION

SECTION IX.

EXPERIMENTS ON THE CIRCULAR SPOTS MADE ON PIECES
OF METAL BY LARGE ELECTRICAL EXPLOSIONS.

IN the courses of experiments with which I shall present the reader in this and the two following sections, I can pretend to no sort of merit. I was unavoidably led to them in the use of a very great force of electricity. The first new appearance was, in all the cases, perfectly accidental, and engaged me to pursue the train; and the results are so far from favouring any particular theory or hypothesis of my own, that I cannot perfectly reconcile many of the various phenomena to any hypothesis.

FROM the first of my giving any particular attention to electrical experiments, I entertained a confused notion, that a person would stand the best chance for hitting upon some new discovery, by applying a greater force than had hitherto been used. Considering the prodigious number of electrical machines that were in the hands of so many ingenious men, in different parts of the world, I imagined that all that could be done *in little* had been tried; and that the usual experiments had been diversified and combined in almost every method possible; where-

as, since electrical machines, I observed, had, of late years, been gradually reduced into less and less compass, a great power of electricity would be almost a new thing, and might therefore supply the means of new experiments. Even Dr. Franklin's force, I considered, was small, in comparison of what might easily be raised, and without a very great expence.

WITH these general and random expectations, I kept gradually increasing my quantity of coated glass, till I got a battery of thirty, forty, sixty, and at length near eighty square feet; and the reader will, in some measure, have seen already, that I was not wholly disappointed in it. The following courses of experiments are more remarkable instances of the advantage I derived from this power of electricity.

THE first remarkable fact that I was by this means led to discover, is that of the circles with which pieces of metal that receive electrical explosions are marked. I shall faithfully relate all the circumstances, and varieties in which it has been exhibited, and the observations I have made upon it; and this I cannot do better than by writing the narrative, in the order in which the appearances occurred.

JUNE the 13th. 1766. After discharging a battery, of about forty square feet, with a smooth brass knob, I accidentally observed upon it a *pretty large circular spot*, the center of which seemed to be superficially melted, in a great number of dots, larger near the center, and smaller at a distance from it. Beyond this central spot was a circle of black dust, which was easily wiped off; but, what I was most struck with was, that, after an interruption of melted places, there was an entire and exact circle of shining dots, consisting of places superficially melted, like those at the center. The appearance of the whole, exclusive of the black dust, is represented, Plate i. fig. 5. No. 1.

JUNE

JUNE the 14th. I took the spot upon smooth pieces of lead, and silver. It was, in both cases, like that on the brass knob, only the central spot on the silver consisted of dots disposed with the utmost exactness, like *radii* from the center of a circle, each of which terminated a little short of the external circle.

EXAMINING the spots with a microscope, both the shining dots that formed the central spot, and those which formed the external circle, appeared evidently to consist of *cavities*, resembling those of the moon, as they appear through a telescope, the edges projecting shadows into them, when they were held in the sun.

THE most beautiful appearance of this kind was exhibited by a spot, which I took on a gold watch case. Besides the cavities, there were, in several places of the spot, hollow *bubbles* of the metal, which must have been raised when it was in a state of fusion. These looked very beautiful when examined with a microscope in the sun, and were easily distinguished from the cavities, by having their radiant points (which were very remarkable, and dazzling to the eye) on opposite sides to those of the cavities, with respect to the sun. The whole progress seems to have been first a fusion, then an attraction of the liquid metal, which helped to form the bubbles; and lastly the bursting of the bubbles, which left the cavities. N. B. By this explosion half an inch of a steel wire, one seventieth of an inch in diameter, was melted, and entirely dispersed. In the dispersion, sparks of it were seen red hot, above half a yard from the place where the wire had lain. This circumstance I have frequently observed since. I have also seen such sparks fly from large brass knobs, at the instant of several explosions.

I TOOK the spot upon polished pieces of several metals, with the charge of the same battery, and observed that the cavities in

4 L

them

them were some of them deeper than others, as I thought, in the following order, beginning with the deepest, *tin, lead, brass, gold, steel, iron, copper, silver.*

I WILL not be very positive as to the order of some of the metals, but silver was evidently not affected a fourth part so much as gold, and much less than any of the others. The circles were marked as plain, but the impression was more superficial. *Qu.* Is this owing to the heat being sooner diffused equably through a piece of silver, than through the substance of any other metal?

I THOUGHT there might possibly be some difference in the circles on metals which had been a long time in a solid form, and those which had been lately fluid; and to ascertain it, I made the explosion between a piece of lead just solid after melting, and another smooth piece, that I had kept a considerable time.

The piece of fresh lead was melted more than the other, but there was no other difference between them.

THE semi-metals, as *bismuth* and *zink*, received the same impression as the proper metals; being melted about as much as iron.

I MADE three discharges between a piece of highly polished steel, and a piece of very smooth iron; and in all the cases thought the steel was more deeply melted than the iron. I mention this experiment more particularly, on account of the singular, and beautiful appearance of the circular spot upon the steel in two of the discharges. A circular spot, of about an eighth of an inch in diameter, was uniformly melted, and pretty well defined; and there was a space round this central spot, of the same breadth, uniformly filled with small melted places; but in one of them twice as large as in the other. They exhibited the exact appearance of a planet surrounded by a dense atmosphere; such as, I think, I remember seeing the figures of, in the plates
of

of *Barnet's* theory of the earth. The other circle upon the steel was a common one.

WHEN the kitten above-mentioned was killed, there was no circular spot, or any fusion on the brass knob. I have always found it the most perfect, when the circuit has been composed of the best conductors, and had the fewest intervals.

JUNE the 19th. Hanging the case of one watch upon the brass knobs communicating with the inside of the battery, and receiving the explosion from it upon the case of another watch, which was sometimes of the same, and sometimes of a different metal, and measuring the circles afterwards; I found them to be very nearly of the same diameter. The small varieties seemed to be accidental; or at least did not depend, either upon the metal, or the direction of the electric fluid. But I thought it pretty evident, from a great number of experiments, that the metal which communicated with the outside of the battery, and which I held in my hand to take the explosion, was marked the more distinctly of the two.

IT seemed that, when the battery was charged very high, the central spot was the most irregular, many of the dots which composed it spreading into the outer circle, and some dots appearing beyond the outer circle, and very much effacing it; so that the best way to procure a distinct circle, is to take a moderate charge of a very large battery. This may be the reason why the outer circle cannot be perceived, when only small jars are used; the circumference of the circle being very small, and the charge generally too high. In a very weak charge, it is too faint to be perceived. I have sometimes, however, seen a very distinct circle made by only two jars, each containing half a square foot of coated glass.

THE diameter of the spot seems to depend upon the quantity

$$4 L^2$$

of coated glass; but in what proportion, I have not yet accurately ascertained.

I HAVE observed a good deal of variety in the external circles. Sometimes they have consisted of pretty large dots, disposed at nearly equal distances, in an exact circle; which, in the spaces betwixt each large dot, was completed by smaller dots, visible only by a microscope. But, generally, the external circle consists of a space full of dots, placed irregularly, but so that a line drawn through the midst of them makes a pretty exact circle round the central spot.

PRESENTLY after I had observed the single circle, I imagined that, whatever was the cause of the appearance, it was not improbable, but that two or more concentric circles might be procured, if a greater quantity of coated glass was used, or perhaps if the explosion was received upon metals that were more easily fused than brass. Accordingly, June the 27th. taking the moderate charge of a battery, consisting of about thirty eight square feet, upon a piece of tin, I first observed a second outer circle, at the same distance from the first, as the first was from the central spot. It consisted of very fine points hardly visible, except when held in an advantageous light; but the appearance of the whole was very beautiful, such as is represented, Plate i. fig. 5. No. 2.

JUNE the 28th. I got another double circle, on a flat pewter standish, much plainer than the former, the outer being about the same distance from the inner, as the inner was from the outside of the central spot.

HAVING hitherto found the circles the most distinct on metals that melt with the least degree of heat, I soon after procured a piece of that composition which melts in boiling water; and having charged sixty square feet of coated glass, I received the explosion with it, and found, what I was endeavouring to get,

three

three concentric circles; the outermost of which was not quite so far from the next to it, as that was from the innermost. All the space within the first circle was melted; but the space was very well defined, and by no means like a central spot, which in this case was quite obliterated. The appearance of these three concentric circles is represented, Plate i. fig. 5. No. 3.

I HAVE several times since found parts of three concentric circles upon brass knobs, when I have used no more than thirty square feet of coated glass. They seem to be more easily perceived, when the knobs are a little tarnished: for then the small dots, in which the metal is melted, are more easily distinguished, especially when they are held in a proper light with respect to the sun.

I MADE many attempts to make these circles larger than I had usually found them upon pieces of metal, chiefly by means of worse conductors; thinking that the electric matter not being so well conducted, and passing with less rapidity, would spread wider. This was probably the case, but then it is likewise probable, that I wanted force to make such an impression visible. For this purpose, however, I received the explosion between two pieces of raw flesh, two potatoes, two moist bladders, and things of a similar nature, but without any effect whatever; no mark at all, or, at least, nothing regular remaining upon them. When I took the explosion upon a piece of wood charcoal, it seemed to be melted, and run in small heaps, within the space of about the usual diameter of a circular spot; and when I took it upon a piece of pit charcoal, a piece seemed to be struck out of it, and a hole was left in it; but there was no regular circle upon either of them; nor was there any sensible ignition in either case.

AT one time I laid a piece of *lead ore* scraped very smooth upon the wires of the battery, and took the explosion with a
 piece

piece of tin ore scraped in the same manner; but though I examined the places with a microscope, I could not be sure that there was any part melted, much less any regular circular spot; but there lay on both of them a yellow matter, like sulphur, round the place where the explosion was taken, and a very disagreeable smell was excited. This probably arose from a mixture of the sulphur of the lead ore, and the arsenic of the tin ore.

I RECEIVED the explosion in *vacuo*, at the distance of about three inches; but found no regular circular spot, owing, probably, to the two interruptions I was obliged, in this case, to make in the circuit, one in the air, and the other within the receiver; by means of which the effect of both would be weakened, the whole force being, as it were, divided between them: for in all such cases, though both the explosions were made in the open air, I found the circles less perfect.

AFTERWARDS, I contrived to make the explosion in one additional atmosphere of condensed air, but the circles were smaller, and less distinct than the other two circles, which I was obliged, at the same time, to make at the other interruption of the circuit, in the open air. The denser air would probably confine the electric matter within a narrower compass; in the same manner as the common air prevents that diffusion of it which is remarkable in *vacuo*.

THE distance at which the discharge was made occasioned no difference in the diameter of these circular spots. When, by putting a drop of water upon the brass rod communicating with the inside of the battery, I made the discharge at the distance of two inches, the spot was just the same as if it had been received, as usual, at the distance of half an inch, i. e. about a quarter of an inch in diameter.

I ALWAYS

I ALWAYS found that if the explosion was obliged to pass through any bad conductor before it reached the metal, the impression it made upon it was contracted, and deeper than if it had been received immediately by the metal. This was evident when paper, a piece of bladder, or varnish were put upon the brass rods with which the discharges were made; though a very thin coating of varnish or moisture did not entirely prevent the appearance of the circles.

IN making a course of experiments with bad conductors, and in using various methods to promote the discharge of the battery at greater distances than usual, I was peculiarly struck with some phenomena which occurred in the use of water.

I PUT a drop of water, about a quarter of an inch in diameter, upon the brass rod communicating with the inside of the battery, and took the explosion directly over it. The discharge was made at the distance of about an inch, and the extremity of the drop was marked with a most beautiful circle, exceedingly well defined on the inside, and vanishing gradually outwards, like a fine shade in drawing. But what struck me most in the appearance was, that, in this circle, there was no central spot.

NOT knowing what this new circumstance was owing to, I wetted a piece of smooth copper, which lay upon the wires of the battery, and taking the explosion upon it, I only found a long streak at the edge of the wetted place, well defined on the side of the water, but vanishing gradually on the opposite side, as in the former case. In this, and other similar experiments, I observed that the electric matter avoided the water, and would go a greater way in the air, in order to come at the metal.

I THEN laid more water upon the copper, but so as only to moisten it; for the surface, being convex, would not allow it to lie in any great quantity; and upon taking the explosion, I found

no

no circle, but several beautiful circular spots melted very deep, one of which was much larger than the rest. These experiments seem to show, that the electric matter meets with a considerable resistance in passing through water, which confines its excursion more than the air; and that, by such a condensation, its force is greatly increased, so as to leave deeper impressions upon the metal than when it had passed only through the air, in like manner, if two pieces of metal be placed, nearly in contact, or if they be light, and one of them lie upon the other; the impression made upon both of them, by the discharge of the battery passing through them, will be considerably deeper, than it would have been if the electric matter had not been confined to so small a compass as the points in contact.

To account for the formation of these concentric circles, nothing seems to be necessary, but the supposition of the elasticity of the electric fluid, whereby its particles repel one another. For then, supposing a quantity of electric matter to issue from one piece of metal to another, through the air, it will endeavour to spread, but will be confined in its passage by the surrounding electric medium, and the strong attraction of the opposite metal. If this piece of metal have a flat surface, or one that is nearly so, the fluid will be attracted by it pretty equally, within a certain space; so that the mutual repulsion of its particles will have room to exert itself, and produce a division of the whole quantity; and as this repulsion is the same in all directions, the effect must be its throwing itself into a circle, or several concentric circles, on its entering the opposite piece of metal, and consequently melting it, in that form. For the same reason the circles themselves will consist of separate dots, each of which might have been caused by the fluid in another hollow circle, but

but being so small, the fusion of the metal could not show that circumstance.

OF the circles being formed in this manner, I have been in a manner an eye witness, when I have presented a flat piece of metal to a large prime conductor strongly electrified, and have seen the large sparks, five or six inches long, divide about the middle, and strike the metal in a circle, about an inch in diameter; generally with a central spot, but sometimes without one.

THE manner in which several of the jars, mentioned in a former section, were broken seems to be analogous to the formation of these circles. I mean those that were pierced with a number of small holes in the neighbourhood of the principal one; but more especially that which was broke with an intire circle of small and independent fractures round the principal hole.

THE remarkable story of the five peasants of whom the first, third, and fifth were killed by lightning, as they were walking in a right line; and which was mentioned before, as analogous to a fact observed by Mr. Monnier, will perhaps be thought more analogous to this. For supposing the diameter of the concentric circles formed by lightning to be sufficiently great, and the central spot to fall upon the third person, the two on each side of him would escape, by being in the first interval round the central spot; while the two who walked first and last would fall into the circumference of the first circle. But other facts lead me to think, that all these effects may have been produced by a constant stream of electric matter, in a progressive motion, the first part of which being discharged upon any object, the stream is weakened when it meets with the second, but is accumulated again when the third comes in its way, and so alternately, as long as the stream and the line of objects continues. This account seems to be rendered more probable by the number of ob-

jects that have been struck in this manner, particularly in a case which is related by Dr. Wallace in his account of the Orkney islands, p. 78. "In the year 1680" he says, "the lightning entered a gentleman's cow-stall where were twelve cows standing side by side, as they used to be, and killed every other one; that is, it killed the first, and missed the second; it killed the third, and missed the fourth; and so of the rest, so that six were killed, and six remained alive and untouched."

COMMUNICATING this experiment to Mr. Price, he suggested to me, that the circles called *fairy rings*, which consist of grass of deeper green in pasture fields, and which have by some been imagined to be occasioned by lightning might be analogous to the circles above-mentioned, but that they want a central spot. I have since examined one of these rings. It was about a yard in diameter, the ring itself about a quarter of a yard broad, and equally so in the whole circumference; but there was no appearance of any thing to correspond to the central spot.

I HAVE since met with a curious article in the Philosophical Transactions, relating to those fairy circles, communicated by Mr. Jessop, which confirms the supposition of their being occasioned by lightning, and with which I shall therefore conclude this section.

"I HAVE often been puzzled to give an account of those phenomena, which are commonly called fairy-circles. I have seen many of them, and those of two sorts; one sort bare, of seven or eight yards diameter, making a round path something more than a foot broad, with green grass in the middle; the others like them, but of several bignesses, and encompassed with a circumference of grass, about the same breadth, much fresher and greener than that in the middle. But my worthy friend Mr. Walker, gave me full satisfaction from his own experience;

" it

“ it was his chance one day, to walk out among some mowing
 “ grafs (in which he had been but a little while before) after a
 “ great storm of thunder and lightning; which seemed by the
 “ noise and flashes to have been very near him: he presently
 “ observed a round circle, of about four or five yards diameter,
 “ the rim whereof was about a foot broad, newly burnt bare, as
 “ the colour and brittleness of the grafs roots did plainly testify.
 “ He knew not what to ascribe it unto but the lightning, which,
 “ besides the odd capricious remarkable in that fire in particular,
 “ might without any wonder, like all other fires, move round,
 “ and burn more in the extremities than the middle. After the
 “ grafs was mowed, the next year it came up more fresh and
 “ green in the place burnt, than in the middle, and at mowing-
 “ time was much taller and ranker.” *

* Phil. Trans. abridged, Vol. 2. p. 182.

SECTION X.

EXPERIMENTS ON THE EFFECTS OF THE ELECTRICAL
EXPLOSION DISCHARGED THROUGH A BRASS CHAIN,
AND OTHER METALLIC SUBSTANCES.

FROM the very first use of my battery, I had observed a very *black smoke* or *dust* to arise upon every discharge, even when no wire was melted, and the brass chain I made use of was of a considerable thickness. Of this circumstance, however, I only made a slight memorandum, as what I could not then account for, and paid no particular attention to it; till on the 13th of June 1766, I was struck with another casual appearance, as I was intent upon the experiments relating to the circles above-mentioned.

I OBSERVED, that a piece of white paper, on which lay the chain I was using to make the discharge, was marked with a *black stain*, as if it had been burnt, wherever the links had touched it. Yet I could not then think that it could be burnt by so thick a chain. I imagined the chain must have been dirty, and the dirt have been shaken off by the stroke. Still however I neglected the experiment till, observing a very striking appearance of the same kind, on the 1st. of September following,

I was determined to attend to the circumstances of it a little more particularly than I had done.

I MADE my chain very clean, and wrapping it in white paper, I made a discharge of about forty square feet through it, and found the stain wherever it had touched the paper.

SOME time after, I wrapped the paper, in the same manner, round a piece of brass wire; but, making a discharge through it, saw no stain. To ascertain whether this appearance depended upon the discontinuity of the metallic circuit; on the 13th. of the same month, I stretched the chain with a considerable weight and found the paper, on which it lay as the shock passed through it, hardly marked at all.

FINDING that it depended upon the discontinuity, I laid the chain upon white paper, making each extremity fast with pins stuck through the links; and when I had made the discharge, observed that the black stains were opposite to the *body of the wire* that formed the chain, and not to the *intervals*, as I had sometimes suspected.

SEPTEMBER the 18th. Observing that a pretty considerable quantity of black matter was left upon the paper, on every discharge with the same chain; I imagined it must have lost weight by the operation, and to ascertain this circumstance, I took another chain not so thick as that I had used before. It was five feet four inches long, and weighed exactly one ounce, seventeen penny weights, four grains. After the discharge, I found it had lost exactly half a grain of its weight. The shock had only passed through a part of it, the rest lying on a heap. I then discharged the same shock through its whole length, and weighed it, found it had lost just another half grain. By repeated experiments I found, afterwards, that the surest way to strike off part of its weight, was to make the shock pass through
a small

a small part of its length, and that when a considerable length was used the event was uncertain.

N. B. THESE and all the following experiments, except where the contrary is expressed, were made with a battery of *thirty two square feet*, that force appearing to be sufficient, and the charging of it not taking up much time. At the time of both the above-mentioned discharges, an iron wire of one seventieth of an inch in diameter was made red hot, but, was not melted.

OBSERVING how deep a stain was made by the links of a thick brass chain, I had the curiosity to try what would be the consequence of sending a shock through a piece of charcoal. Accordingly I took a small piece, about half an inch in length, and found that, in the discharge, it was all blown to dust. The pasteboard on which it lay was torn, the charcoal being forced into it, so that the impression appeared on the other side. The blackness was spread to a great distance, and the tinge every where indelible.

SEPTEMBER the 21st. In making the mark above-mentioned, on part of the sheet of paper, on which I had written an account of the experiment to Dr. Franklin, I happened to lay the chain so as to make it return at a sharp angle, in order to impress the form of a letter on the paper; and observed that, upon the discharge, the part of the chain that had been doubled was displaced, and pulled about two inches towards the rest of the chain. At this I was surpris'd, as I thought it lay so, as that it could not slide by its own weight. Upon this I repeated the experiment with more accuracy. I stretched the whole chain along a table, laying it double all the way, and making it return by a very sharp angle. The consequence always was, that the chain was shortened about two inches, and sometimes more; as if a sudden pull had been given to it by both the ends.

CONSIDERING

CONSIDERING that this pull must have been given to it by the several links suddenly repelling one another, at the instant of the explosion, I compared the links with the black marks that were made by them upon the table, and found that each link had been pulled from the place on which it had lain, and most of all, at the greatest distance from the place of the explosion.

CONVINCED that the chain had been shortened by the mutual repulsion of the links, I endeavoured to measure with exactness how much the shortening was, in a given length of chain. To do this, I measured two feet four inches of the chain, as it lay upon the table, in one straight line, without any return, one end being fixed and the other moveable; and found that, upon discharging sixty four square feet through it, it was shortened a quarter of an inch in its whole length. I had contrived that the suddenness of the motion should not throw one part of the chain upon the other.

SUSPECTING that the black smoke, which rose at every discharge, might come, not from the chain, but from the paper, or the table on which it lay, and which was probably burnt by the contact of it, I let the chain hang freely in the air; but, upon making the discharge, I observed the same black gross smoke that had before risen from the paper or the table. It was therefore part of the metal itself, which had been converted into that black dust.

To give my reader a better idea of the mark made upon white paper by a chain, through which the electric shock is transmitted, I laid a chain upon the original drawing of Plate I. for the engraver to copy as exactly as he could; and he has succeeded pretty well. The breadth of the spots are about the mean thickness of the wire of the chain, and [a, b] marks the place to which

which that part of the chain which was returned was thrown back, by the sudden repulsion of the links.

I HAD before observed the electric sparks betwixt each link to be most intensely bright, so as, sometimes, to make the whole chain appear like one flame in the dark; but the appearance of the chain, at the instant of the shock, as it hung freely in the air, was exceedingly beautiful; the sparks being the largest and brightest at the bottom, and smaller, by degrees, towards the top, where they were scarcely visible; the weight of the lower links having brought them so much nearer together.

SEPTEMBER the 26th. Being still in some doubt whether the blackness that was left on the paper came from the burning of the paper, or some thing that was thrown from the chain; I once more hung the chain freely in air, and put under it, but so as not to touch it, a piece of white paper, on which I also laid a few pieces of down, to observe whether they would be affected by any electric attraction or repulsion. On making the discharge, the down was all dispersed, and the paper was marked with a black stain, near the length of an inch; which was the distance at which the two parts of the chain hung from one another, a little above the paper. Some parts of the stain were deeper than others, the whole mark consisting of four different spots of a deeper black, joined by fainter streaks, answering to four links of the chain, which hung nearly parallel to the paper. The stain could not be wiped off with a handkerchief, though it was not so deep as when the chain had touched the paper. Thus I was satisfied, that a considerable part, at least, of the blackness had come from the chain.

SEPTEMBER the 27th. Willing to ascertain more exactly what part of the chain, the solid links, or the intervals, was most affected by the shock; I dipped it in water, and laying it quite
wet

wet upon a piece of white paper, discharged a shock through it. Part of the water was thrown into my face, being scattered in all directions, and all the chain left instantly and perfectly dry. The paper was very much stained for the space of an inch broad, wherever the chain lay; not equably, but as if it had been handled with dirty fingers. The stain was indelible, and where the chain was returned, a hole was struck quite through the paper.

To determine whether the paper, in the above-mentioned instances, had really been *burnt*, as well as *stained*. I laid a part of the chain, at the time of the last discharge, upon three half crowns; and found they were all melted, in the places where the chain had touched them. The marks made by the fusion were about the breadth of the chain, and so deep that nothing but a tool could efface them.

To determine, if possible, more sensibly what it was that made the black tinge, I laid the chain upon my hand, when I had a moderate charge; and it was marked just like the paper. I felt a kind of pricking or burning at the instant of the explosion, and the painful sensation continued a small space of time.

I MADE no doubt but that with a heat that melted metals, I could easily contrive to fire gunpowder; but, though I laid the chain upon the grains, and rammed the powder about the chain put through a quill, I could not succeed. In the first case, the powder was dispersed; and in the second, the quill was burst, and there was a smell, as after an explosion of gunpowder, but no actual firing of it.

HITHERTO I had always put the chain in contact with bodies that were conductors. I was now willing to try what would be the consequence of laying it in contact with electrics. Accordingly, I dipped the chain in melted rosin, till it had got a coat-

ing of a considerable thickness. When it was quite stiff, I laid it carefully, without bending, upon white paper, and made the discharge through it. The rosin was instantly dispersed from all the outside of the chain, it being left as clean as if none had ever been put on. That with which the holes in the chain had been filled, having been impelled in almost all directions, was beaten to powder; which, however, hung together, but was perfectly opaque; whereas it had been quite transparent, before this stroke. I felt some of the rosin fly in my face. The stain upon the paper was very deep, containing a good deal of rosin, and several holes were struck through the paper on which it was laid. A half crown, on which part of the chain had lain, was melted, and so deeply stained with the rosin, that it could not easily be cleaned.

I NEXT laid the chain upon a *piece of glass*; and considering how both the half crown and the rosin had been affected, expected it would have been broken to pieces; but instead of that, the glass was marked in the most beautiful manner, wherever the chain had touched it; every spot the width and colour of the link. The metal might be scraped off the glass at the outside of the marks; but in the middle part it was forced within the pores of the glass; at least nothing I could do would force it off. On the outside of this metallic tinge was the black dust, which was easily wiped off.

I HAVE since given the same tinge to glass with a silver chain, and small pieces of other metals; but could not do it with large pieces. They were melted where they touched one another, but the glass was not tinged.

OCTOBER the 7th. I had the curiosity to try, whether I could not give a tinge to glass with quicksilver. In order to this, I laid some globules in a right line, and laid a thin piece of glass upon them, to flatten them, and bring them nearer into contact with
one

one another. Both the slips of glass were shattered in a thousand pieces, and dispersed all over the room, several of them flying in my face; though no part of the quicksilver could be found, except what adhered to some fragments of the glass, to which it had given a kind of uniform whiteness; but no distinct globules could be seen, and it was easily wiped off, so that no part of it was fixed in the glass. My head ached all the remainder of the day, which I attributed to the fumes of the mercury.

SEPTEMBER the 28th. Having dipped the chain in water, and found it instantly dispersed, I wished to see what would be the effect of discharging a shock through a chain quite covered with water. Accordingly, little imagining the consequence, I laid the chain upon a piece of white paper, in the bottom of a china dish, and poured in water just sufficient to cover it. Also, under one part of the chain, and in the water, I put a half crown. Upon the explosion, the water was blown about the room, to a great distance, the half crown was melted in two places, the dish broken into many pieces, and the part that lay immediately under the chain into very small fragments. The paper was a little stained, and the water, I could perceive, had been a little fouled by the black dust.

BEING certain that the dish must have been broken by the concussion given to the water by the electric spark under it, in the manner in which Signior Beccaria's tubes were broken (though I had not seen his work at that time, but had seen the experiment at Mr. Lane's *) I had the curiosity to try what would be the effect of making a discharge through the chain hanging freely in water. I therefore got a tin vessel, holding a

* See his account of it, Phil. Transf. Vol. 57. p. 458.

quart, and letting the chain hang three inches and a half below the surface of the water, made the discharge. The electric sparks appeared intensely bright in the water, all along the chain; some of the water was thrown out, and the vessel appeared to have been pressed with some force upon a book, which I had put under it, a visible impression being made upon it. The vessel must have received a great concussion: for the dust had been shaken from the bottom upon the book, though I had carried the vessel up and down the room, without perceiving that any dust adhered to it.

I WAS willing to repeat this experiment with some variation of circumstances, and fastened a piece of small silver wire to two pieces of strong brass wire, and plunged the whole an inch or two under the surface of the water. Upon the discharge, the silver wire was melted, at least snapped asunder, the vessel had been pressed downwards more violently than before, a considerable quantity of the water was thrown about the table, and some was dashed perpendicularly upwards, against the top of the room; where there were five wet places, each about the bigness of a half crown. I have since frequently melted wires under water.

SEPTEMBER the 29th. I made a discharge through three pieces of the same chain, each being a different circuit. They all left their impression upon the paper, and nearly equal. Also three out of four pieces made pretty equal marks, but the fourth failed intirely.

AT another time, a chain, which communicated with the outside of the battery, but which made no part of the circuit, made the black stain on a piece of white paper on which it accidentally lay, almost as deep as the chain that formed the circuit. I was then melting a piece of wire, which had the same effect

as using a bad conductor. The same thing has frequently happened since.

NOVEMBER the 12th. I put a chain through a glass tube, so wide as that it could only touch one side; and upon the discharge, observed four sets of marks, made by the metal being driven into the glass; as if four chains had been in the tube, and all had received the shock. Two of the rows, on one of which I imagine the chain had lain, were better marked than the other two, but all were very plain.

THE last thing that engaged my attention with respect to this course of experiments, was that *black dust* which I have observed to be discharged from the brass chain, and other pieces of metal. As it was so extremely light as to rise like a cloud in the air, so as sometimes to be visible near the top of the room; I concluded that it could not be the metal itself, but probably the *calx*, or the calx and *phlogiston*, in another kind of union than that which constitutes a metal; and that the electric explosion reduced metals to their constituent principles as effectually as any operation by fire could do it, and in much less time. I was confirmed in this opinion by finding, in the first place, that this black dust collected from a brass chain would not conduct electricity, which is known to be a property of the calces of metals, and also by the result of some of the following experiments.

CONSIDERING this black dust as a proof of calcination, and observing it to be produced when I made the explosions for the circular spots between gold and silver watch cases, as was related above; I began to think I had made a calcination of those metals, which all the chymists say is impossible: but the following experiments convinced me, that it could only be the alloy that was in them which had yielded the black dust or calx.

SENSIBLE that my experiments with these metals would conclude

clude nothing, unless I got the specimens quite pure, I first procured a small quantity of *grain gold*, which I was informed was the purest that the goldsmiths know, and discharged an explosion of the battery through a train of the pieces, an inch and a half in length, laid on a piece of white paper. Only two of the larger grains could be found after the explosion. Two leaves of paper were burnt, or torn through in several places, and more would probably have been torn in the same manner, if I had used more. But what I principally attended to was the tinge that was given to the paper, with a view to which I had made the experiment. The paper was stained near an inch on each side of the train, with black intermixed with red, making an odd motley appearance.

WITH the same view, I laid a similar train of bits cut with a knife from a piece of as pure silver as I could procure. They were dispersed, and the paper burnt through, in the same manner as with the gold; and the space of about an inch on each side of the train was stained with black intermixed with a deep yellow, which was considerably different from the tinge made by the fusion of the gold.

THE blackness in these tinges convinced me, that there had been a calcination of some part of the metal; but I was convinced it must have been some alloy, by an experiment I presently after made with a piece of leaf gold; which, I believe, is generally the purest that can be got. A small slip of this I put through a quill, letting a part hang out at each end; and when I had made the discharge through it, I found the quill tinged with a beautiful *vermillion* red, without the least intermixture of black. When I dispersed a slip of leaf brass in the same manner, the greater part of the tinge was black, with a little brown mixed with it in a few places.

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IN order to ascertain whether the black dust was a pure calx, or contained a portion of the metal, I procured a small quantity of it, by sending an explosion through some pieces of iron wire, sometimes put into a quill, sometimes laid upon white paper, and sometimes upon glass, or inclosed in glass tubes; but could never be quite sure that there was any part of it that was not affected with the magnet, which the mere calx would not have been. But the black dust could not be the pure metal, for then it would either have been bright immediately after fusion, or have given a blue colour, which is the proper production of iron.

SOME of the experiments with the brass chain, related in this section, are similar to one of Mr. Wilson's, mentioned p. 95, concerning bodies placed without the electric circuit being affected with the explosion. As to the cause of this, and the other appearances above-mentioned, I have no conjecture worth communicating to the public. I have only pursued the analogy of facts, and that not very far. Others may compare them, pursue them farther, and ascertain their causes.

MR. CANTON has since clearly proved the calcination of pure gold and silver by the heat of electrical explosions, producing numberless most beautiful globules of transparent glass, and also others tinged with all the varieties of colour from those metals. He has also made it probable, that the *black dust* mentioned in this section, is the calx, or glass of the metal, reduced to smaller particles than the laws of optics require to produce colour.

SECTION

SECTION XI.

EXPERIMENTS ON THE PASSAGE OF THE ELECTRICAL
EXPLOSION OVER THE SURFACE OF SOME CONDUCT-
ING SUBSTANCES, WITHOUT ENTERING THEM.

I OBSERVED, in relating the experiments on ice, that, in my attempts to ascertain its conducting power, I sometimes saw the flash of the electrical explosion strike directly to the chain, along the surface of the ice. But as this passage on the surface was produced only by a common jar, it was not much greater than the distance at which the discharge was usually made, and the appearance did not strike me. But afterwards the same phenomenon occurred in the use of my battery, where the passage over the surface so far exceeded the usual distance of a common discharge, that it engaged my attention in a very particular manner, and produced some pleasing experiments; which I shall recite in the manner, and nearly in the order in which they happened.

DECEMBER the 11th. Thinking to make a circular spot on a piece of raw flesh, I took a leg of mutton, and laying the chain that communicated with the outside of the battery over the flank
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of it, took the explosion on the outward membrane, about seven inches from the chain; but was greatly surpris'd to observe the electric fire not to enter the flesh, but to pass, in a body, along the surface of it, to come to the chain.

THINKING that this effect might be occasioned by the fatty membrane on which the explosion was taken, I again laid the chain, in the same manner, over the shank, and took the explosion upon the fibres of the muscles, where they had been cut from the rest of the body; but still the fire avoided entering the flesh, made a circuit of near an inch round the edge of the joint, and pass'd along the surface, to come to the chain as before, though the distance was near eleven inches.

IMAGINING this effect was promoted by the chain lying lightly on the surface of the flesh, and therefore not really in contact with it; I took another explosion, when the hook of the chain was thrust into the flesh; on which the fire entered the mutton, and, as I held it in my hands, both my arms were violently shocked up to my shoulders; whereas, in the cases of the electric fire passing over the surface of the flesh, my fingers, happening to touch the chain, were only affected with a slight pricking, or superficial burning, which has been explained before.

THIS phenomenon being so remarkable, and the battery by this means discharging at a distance about twenty times greater than it could usually be made to do, I thought to try other substances, of a conducting power similar to that of raw flesh; and of these, *water* was the most obvious. Accordingly, the next day, I laid a brass rod communicating with the outside of the battery very near the surface of a quantity of water (to resemble the chain lying upon the surface of the flesh, without being in contact with it) and, by means of another rod furnished with knobs, made a discharge on the surface of the water, at the dis-

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tance of several inches from any part of the rod; when the electric fire struck down to the water, and, without entering it, passed visibly over its surface, till it arrived at that part of the rod which was nearest to the water, and the explosion was exceeding loud. If the distance at which I made the discharge exceeded seven or eight inches, the electric fire entered the water, making a beautiful star upon its surface, and yielding a very dull sound.

THE resemblance between this passage of the electric matter over the surface of the water, and that which Dr. Stukeley supposed to sweep the surface of the earth, when a considerable quantity of it is discharged to the clouds during an earthquake, immediately suggested to me, that the water over which it passed, and which was visibly thrown into a tremulous motion, must receive a concussion, resembling that which is given to the waters of the sea on such an occasion.

To try this, myself, and other persons who were present, put our hands into the water, at the time that the electrical flash above-mentioned passed over its surface; and we felt a sudden concussion given to them, exactly like that which is supposed to affect ships at sea during an earthquake. This percussion was felt in various parts of the water, but was strongest near the place where the explosion was made.

AFTERWARDS, I made the explosion of a jar, containing three square feet of coated glass, at some distance below the surface of the water, so as to be visible in the water, and we felt the same concussion that we had done before, when the fire of the battery passed over the surface, only much weaker. The flash of electric fire in the water does certainly displace some of it, and thereby give a sudden concussion to the rest; and the similarity

larity of the effect is a considerable evidence of a similarity in the cause.

I AFTERWARDS made the fire of a jar pass through the water, making a space of about a foot part of the circuit; when, putting our hands in its passage, they were affected, but in a very different manner from what they were before: for this evidently affected the nerves and muscles of the hand internally, and occasioned a small degree of the same kind of convulsion which is felt by the electric shock itself; whereas the other was a mere percussion, affecting the surface of the hand. Both sensations were, indeed, felt most sensibly at the surface of the water, though our hands were, in some measure, affected by both as low as we could put them.

BEING willing to experience what kind of a sensation this passage over the surface would occasion, I laid a chain in contact with the outside of a jar lightly on my finger, and sometimes kept it at a small distance, by means of a thin piece of glass; and, if I made the discharge at the distance of about three inches, the electric fire was visible on the surface of the finger, giving it a sudden concussion, which seemed to make it vibrate to the very bone; and when it happened to pass on that side of the finger which was opposite to the eye, the whole seemed perfectly transparent in the dark. If I took the distance much larger, the fire entered the finger, occasioning a very different sensation from the former. The one was like a blow, but of a very peculiar kind, whereas the other is well known to be a convulsion.

I THEN ventured to put my fingers upon a piece of the spinal marrow of an ox, while the explosion of the battery was passing over it, when I felt only a slight pricking, or percussion on each side of my finger; and the sensation continued for some time. This sensation did not extend at all beyond the place of percussion;

but afterwards, putting two of my fingers on the same piece of spinal marrow, when the charge of the battery was considerably stronger, I received a concussion which affected my whole hand, but it was with a kind of a vibratory motion.

PLEASED with this resemblance of the earthquake, I endeavoured to imitate that great natural phenomenon in other respects; and it being frosty weather, I took a plate of ice, and placed two sticks, about three inches high, on their ends, so that they would just stand with ease; and upon another part of the ice I placed a bottle, from the cork of which was suspended a brass ball by a fine thread. Then, making the electric flash pass over the surface of the ice, which it did with a very loud report, the nearer pillar fell down, while the more remote stood; and the ball, which had hung nearly still, immediately began to make vibrations about an inch in length, and nearly in a right line from the place of the flash.

I AFTERWARDS diversified this apparatus, erecting more pillars, and suspending more pendulums, &c. sometimes upon bladders stretched on the mouth of open vessels; and at other times, on wet boards swimming in a vessel of water. This last method seemed to answer the best of any; for the board representing the earth, and the water the sea, the phenomena of them both during an earthquake may be imitated at the same time; pillars, &c. being erected upon the board, and the electric flash being made to pass either over the board, over the water, or over them both. This makes an exceeding fine experiment.

WHEN I first made this experiment of the electric flash passing over the surface of the water, I thought it necessary, that neither the piece of metal communicating with the outside, nor that communicating with the inside of the jars should touch the water immediately before the discharge. But I afterwards found, that
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the experiment would answer, though either, or even both of them were dipped in the water: for in this case the explosion would still prefer the surface to the water itself, if the distance was not very great; and would even pass at a greater distance along the surface, when there was a nearer passage from one rod to the other in the water.

JUST before the discharge, both the rods were observed to attract the water very strongly. It was thrown upon the rod communicating with the outside when it was laid near half an inch above the surface. When I put a drop of water on the rod communicating with the inside, the discharge was made at the distance of about two inches from the surface of the water, the fire first descending perpendicularly, and then passing along the surface; and if the rod communicating with the outside had a drop of water upon it, it might be placed higher over the water than if it had not. At the time of the explosion, this drop was elongated, and promoted the discharge very considerably.

MY attention was next drawn to the kind of impression which was made upon the water by the passage of the electric fluid in this manner. To ascertain this, I first placed a shilling level with the water, to receive the explosion before it passed along the surface; and observed that it was melted, but only about half as much as I imagined it would have been in the common way. There was no regular circular spot. And I could never perceive that the brass rod which communicated with the outside of the battery was at all melted by the explosion.

JUDGING from the concussion given to the whole body of the water over which the shock passed, I thought that the trace of it might possibly be preserved on the surface of soft paste; and accordingly I made the explosion pass over the surface of some,

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and plainly observed, that the part under the passage was depressed; the electric matter having repelled it. The impression was not deeper where the explosion first fell than in any other part of the track.

To distinguish more accurately between the effect of the electric matter when it probably enters the water, and when it only passes over the surface, I spread a little water, exceeding thin, upon the surface of a smooth piece of slate; but, though the explosion passed over the surface, with its usual violence, I could not perceive that it had occasioned the least degree of evaporation; which Signior Beccaria found to be the consequence of making the electrical explosion through water in such circumstances.

WHEN the explosion passed over the surface of the plate of ice, in the experiment of the earthquake above-mentioned, the ice seemed to be melted, both where the chain had been laid, and also along the tract over which the explosion had passed. But this melting, if it was such, was not uniform; but looked as if a chain with small links had been laid hot upon it; and the impression was not at all deeper where the explosion was first received.

WHEN the explosion passed over the surface of a green leaf, the leaf was rent in two directions; the longer in the track of the explosion, and the other at right angles to it.

I SEVERAL times made the explosion on the surface of snow, when it always dispersed a considerable quantity of it, making a hole near two inches deep, and almost as broad as long; for it could not be made to pass at a greater distance than about three inches.

I WAS not a little surprised to find that I could not make this electrical explosion pass equally over the surface of substances which were conductors in nearly the same degree; and for a long

long time imagined, that this property was peculiar to water, or to bodies that conducted by means of the water they contained. I could never make it pass the surface of any kind of charcoal; though all the degrees of conducting power may be found in different pieces of it: and I was the more confirmed in my opinion, by observing, that, though the explosion passed perfectly well over the surface of a smooth board, that had been just wetted, and immediately wiped as clean as possible; yet two hours after, when the board was quite dry, it would not pass at all in the same place. It also passed with great violence over the surface of a bladder which had been moistened about a quarter of an hour before, and then seemed to be quite dry; but would not pass in the least degree two or three hours after. In the former case, the explosion had left a mark where it had passed over, darker than the rest of the surface, a kind of polish which was on it being taken off: in the latter case, as the dry bladder conducted very imperfectly, the fire of the charge spread in a beautiful manner, covering a space of about an inch in diameter.

THIS electrical explosion would not pass in the least degree, over the surface of new glass, notwithstanding its property of diffusion above-mentioned seemed to promise that it might. Neither would it pass at all over the surface of alum, rock salt, sal ammoniac, blue or green vitriol, or a piece of polished agate; though these are all conductors of a middle kind, like water; and several of them had very smooth surfaces. It also refused the surface of dry wood, and dry leather, even the smoothest cover of a book.

BUT I found that I had concluded too soon, that this passage of the electrical explosion was peculiar to the surface of water, by finding, first, that it passed over the surface of a touch-stone, and then over a piece of the best kind of iron ore, exceeding
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smooth on some of its sides. This piece is about an inch thick, and about three inches in its other dimensions. The full charge of a jar of three square feet would not enter it. It was diverting to observe how the electrical explosion would make a circuit, round its angles, when it was made in a place remote from the jar. It looked like a thing invulnerable.

THIS electrical explosion passed over the surface of oil of vitriol with a dull sound, and a red colour, which was the only appearance of the kind that I have yet met with. In all other cases, if it passed at all, it was in a bright flame, and with a report peculiarly loud. It passed over the surface of the most highly rectified spirit of wine without firing it; but when I took too great a distance, the electric fire entered the spirit, and the whole plate was in a blaze in a moment.

I ONCE fancied that the fluidity of water was in a great measure the cause of this phenomenon; but I found I could not make it pass over the surface of quicksilver or melted lead; though neither of the rods with which the discharge was made touched the metals. A dark impression was made on the surfaces of both the quicksilver and the lead, of the usual size of the circular spot; and remained very visible, notwithstanding the state of fusion in which the metals were.

So far was the electrical explosion from passing over the surface of any metal, that I observed, if the distance through the air, in order to a passage through the metal, was ever so little nearer than the distance along the surfaces, it never failed to enter the metal; so that its entering the surface of the metal, and its coming out again seemed to be made without the least obstruction. If as much water was laid on a smooth piece of brass as could lie upon it, it would not go over the surface of the water, but always struck through the water into the metal.

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But if the metal lay at any considerable depth under the water, it would prefer the surface. It even passed over three or four inches of the surface of water as it was boiling in a brass pot over the fire, in the midst of the steam and the bubbles, which seemed to be no hinderance to it.

ANIMAL fluids, of all kinds that I have tried, seemed in a peculiar manner to favour the passage of the electrical explosion over their surfaces, and the report of those explosions was manifestly louder than when water was used in the experiment. This I remarked more particularly when I made use of milk, the white and yolk of an egg, both fresh broken, and after it had stood a day or two, and had contracted a hard pellicle. In all the experiments with the egg, it was observed, that no peculiar impression was made in the place where the electric matter first came upon the surface.

IT was very remarkable, that the report made by all these explosions, in which the electric matter passed over the surfaces, was considerably louder than when the discharge was made between two pieces of metal; and they were observed by persons at some distance out of the house, and in a neighbouring house, very much to resemble the smart cracking of a whip; and indeed it would not be very easy to distinguish them. But the sound made by these explosions, though by far the loudest that I ever heard of the kind, fell much short of the report made by a single jar, of no very great size, of Mr. Rackstraws; who says that it was as loud as that of a pistol.

IT was pretty evident, that the distance at which the fire passed over animal substances was greater than it could be made on the surface of water; particularly in the first experiment of the leg of mutton. It also passed about ten inches over the surface of a piece of spinal marrow taken from an ox.

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I WAS much struck with a beautiful appearance which occurred in the course of these experiments, though it was of a different nature from them. When the electrical explosion does not pass over the surface of water, but enters the fluid, it makes a regular star upon it, consisting of ten or a dozen rays; and, what is most remarkable, those rays which stretch towards the brass rod that communicates with the outside of the battery are always longer than the rest; and if the explosion be made at such a distance, as to be very near taking the surface, those rays will be four or five times longer than the rest; and a line bounding the whole appearance will be a beautiful ellipsis, one of whose *foci* is perpendicularly under the brass knob with which the discharge is made.

It will be in vain to attempt these experiments without a considerable force. Nothing at all, to any purpose, can be done with a common jar; since the explosion of it will hardly pass over the surface of any conductor farther than it will discharge through the air. The charge of a jar containing three square feet of coated glass will not make any considerable appearance upon the water; and as far as I can judge, the distance at which the explosion will pass along any surface is in proportion to the strength of the charge. For this reason I make no doubt but that I could have performed all the experiments above-mentioned to much greater advantage, if I had applied a greater force, but that would have required more time, and a moderate force was sufficient to ascertain the facts.

N. B. In these experiments, I put the discharging rod through a handle of baked wood; by which means, I could with safety lay one end of it upon the wires of the battery, and make the explosion with the other, on what substances I pleased.

SECTION XII.

EXPERIMENTS ON THE TOURMALIN.

FATIGUED with the incessant charging of the electrical battery, and stunned with the frequent report of its explosion; I was desirous of some respite from those labours, and with pleasure took up the gentle and silent TOURMALIN. And I make no doubt but that my readers, who must have sympathized with me, will be equally pleased with the change.

It was in the month of August 1766, that, being in London, I received from Dr. Heberden, who is glad to encourage every attempt in philosophical inquiries, his set of tourmalins; among which was that fine one which had passed through the hands of Mr. Wilson and Mr. Canton, and of which a description is given in the 31st. vol. of the Philosophical Transactions, p. 316. But notwithstanding I had this valuable stone so long in my possession, it was not till the latter end of December that I began to make any experiments with it, having, in the mean time, been engaged in other electrical pursuits. At length, however, having brought my other experiments to the state in which the read-

er hath seen them, I was desirous of being an eye witness of the wonderful properties of this stone, and of pursuing a few hints which had occurred to me with respect to it. The result of my experiments I shall lay before the reader, after having informed him in what manner, and with what precautions they were made.

THE methods I used to apply heat to the tourmalin were various, but they will be sufficiently explained in the particular experiments. To ascertain the kind of electricity, I always had near me a *stand of baked wood*, from the top of which projected various arms for different purposes. Three of them were of glass, to two of which were fastened threads of silk, as it comes from the worm, supporting light pieces of down; from the other hung a fine thread, about nine or ten inches long; while a brass arm supported a pair of Mr. Canton's pith balls. At the other extremity of this arm, which was pointed, I could place a charged jar, to keep the balls constantly and equably diverging, with positive or negative electricity. Sometimes I suspended the balls, not insulated, within the influence of large charged jars. And lastly, I had always at hand a *fine thread of trial* not insulated, and hanging freely, to observe whether the stone was electrical or not when I began any experiments, and sometimes to measure the strength of the power which it had acquired.

BEFORE I began any experiments, I never failed to try how long my electrometers would retain electricity, and in what degree. If the thread would retain the virtue for a few minutes, I generally preferred it, when I wanted to communicate the electricity of the tourmalin; because it would catch it in a moment. If the thread would not retain the virtue long enough, or if I wanted a less variable degree of electricity than the thread could retain, I had recourse to the *feathers*, which never failed

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to retain the virtue that was communicated to them for several hours together. I have often found them pretty strongly electrified, after remaining untouched a whole night, though there had been no fire in the room. They might be touched without any sensible loss of their electricity; but they received the virtue very slowly.

THE reader must observe, that by the *positive* or *negative* side of a tourmalin, in the following experiments, I always mean the side which is positive or negative while the stone is cooling. Also, when I mention *the tourmalin* without any distinction, I always mean Dr. Heberden's large one, the convex side of which is positive in cooling, and the flat side negative.

THE consideration of Mr. Wilcke's experiments on the production of spontaneous electricity, by melting one substance within another, first made me conjecture, that the tourmalin might collect its electricity from the neighbouring air. To ascertain this circumstance I made the following experiments, which seem to prove that my conjecture was just. It was with a view to this experiment that I first expressed a desire to have a tourmalin in my possession. I afterwards found that Mr. Wilson had made an experiment mentioned p. 301, which is, in part favourable to this hypothesis, though he supposed the electricity to permeate the stone, so that one side might have been supplied from the other. But the following experiments will show, that the supposition of the permeability of the tourmalin to the electric fluid is altogether unnecessary to account for any of the appearances it exhibits.

ON the standard bar of a most excellent pyrometer made by Mr. Ellicott, I laid a part of a pane of glass, and upon the glass Dr. Heberden's large tourmalin. The bar was heated by a spirit lamp placed underneath it; and I treated the tourmalin in this manner,

manner, to ascertain with exactness when the heat was increasing, decreasing, or stationary. In this disposition of my apparatus I observed, that, whenever I examined the tourmalin, the glass had acquired an electricity opposite to that of the side of the stone which had lain upon it, and equally strong. If, for instance, I presented the flat side of the stone to a feather electrified positively, as the heat was increasing, it would repel it at the distance of about two inches, and the glass would attract it at the same, or a greater distance; and when the heat was decreasing, the stone would attract it, and the glass repel it at the distance of four or five inches. It made no difference which side of the glass I presented; both sides attracting or repelling the same feather with equal strength. When I fastened a shilling with sealing wax upon the glass, the events were always the same. The electricity of both the shilling and the glass was always opposite to that of the stone. I was surprised to observe how soon the electricity, both of the stone and the glass, would change when it came to the turn; for in less than a minute I have sometimes found them the reverse of what they were before.

THERE was, however, in the cases in which I laid the convex side of the tourmalin upon the flat surface of the glass, or shilling, one exception to the rule above-mentioned; viz. that, in cooling, the glass and shilling were positive, as well as the stone. This I imagined to be owing to the stone touching the surface on which it lay in so few points, that it collected its electricity from the air, and imparted it to the body on which it lay; and this supposition was confirmed by experiment. For getting a mold made for the convex side of the stone in plaister of Paris, and heating the tourmalin in the mold, fastened to a slip of glass, I always found the mold and the glass possessed of the electricity contrary to that of the stone, and equally strong. When they

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were cooling, the mold seemed sometimes to be more strongly negative than the stone was positive; for, at one time, when the stone repelled the thread at the distance of about three inches, the mold attracted it at the distance of near six.

HAVING made the experiments above-mentioned with the tourmalin placed upon glass, or conducting substances, laid upon the glass, I had the curiosity to try what would be the consequence of heating and cooling the stone in contact with other substances, both electrics and conductors. And these experiments brought me gradually to the discovery of a method of reversing all the experiments that have hitherto been made upon the tourmalin, making that side which is positive in heating or cooling to be negative, and that which is negative to be positive; so that the kind of electricity shall be just what the operator shall direct, by the application of proper substances to the stone.

I BEGAN these experiments with substituting another tourmalin instead of the piece of glass above-mentioned; and when only one of the tourmalins was heated, they were both affected just as the tourmalin and glass had been. If, for instance, the negative side of a hot tourmalin was laid upon the negative side of a cold one, this latter became positive, as a piece of glass would have been in the same circumstances.

WHEN I heated both the tourmalins, though they were fastened together with cement, they both acquired the same power that they would have done in the open air. In these cases, as the stones could not be made to touch one another in a sufficient number of points, nothing could be concluded from the experiments. The same objection lay against heating or cooling the tourmalin upon rough glass; when I always found them both to be affected as they would have been if the glass had been smooth.

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THIS consideration made me think of cooling the tourmalin in contact with *sealing wax*, which might be made to fit the stone as exactly as possible, though it were ever so irregular. Accordingly I half buried the negative side of a tourmalin in hot sealing wax; and when it was cold, turning it out of its waxen cell, found it positive (contrary to what it would have been in the open air) and the wax negative. The other side of the tourmalin, which was exposed to the open air, was affected in the same manner as it would have been if the opposite side had been exposed to the air too, so that both sides were positive in cooling. As the negative side of the tourmalin became positive by cooling in wax, I had no doubt but that the positive side would be so, as I actually found it.

I WOULD have ascertained the state of the different sides of the tourmalin when it was *beating* in wax, but I found it extremely difficult to do it with sufficient certainty. It cannot be known exactly when the stone begins to cool in these circumstances: besides, in this method of treatment, it must necessarily be some time in the open air before it can be presented to the electrometer; and the electricity of the sides in heating is by no means so remarkable as it is in cooling. In the attempts I did make with the positive side of the tourmalin buried in wax, I generally found it negative, but once or twice it seemed to be positive.

WHEN I cooled the tourmalin in *quicksilver*, contained in a china cup, it always came out positive, and left the quicksilver negative; but this effect could not be concluded to be the consequence of the application of the one to the other, because it is almost impossible to touch quicksilver with the tourmalin without some degree of friction; which never fails to make both sides strongly positive, though it be quite cold, and especially if the stone be dipped deep into it.

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IT then occurred to me, that the tourmalin would not be apt to receive any friction from simple pressure against the palm of my hand; and this being a conducting substance communicating with the earth, the circumstances of the experiment would be new, and might possibly produce new appearances. The event more than answered my expectations: for in heating or cooling the tourmalin in contact with the palm of my hand, each side of the stone was affected exactly in a manner contrary to what it would have been if exposed to the open air. In this case, though the positive appearances may be suspected to be ambiguous, on account of the difficulty of avoiding some small degree of friction, in removing the stone from the hand; yet the negative appearances are, by that very circumstance, rendered the more indisputable, and therefore remove the objection from the positive ones. For the greater satisfaction of my reader, I shall relate these experiments exactly as they were made.

I FASTENED the convex side of Dr. Heberden's large tourmalin to the end of a stick of sealing wax, and when it was quite cold, I pressed the flat side of it pretty hard against the softest part of the palm of my hand. Immediately upon this, presenting it to an electrified feather, it appeared to be strongly negative, contrary to what it would have been if exposed to the open air; and it continued negative till it had acquired all the heat it could get from my hand, when its power decreased, though it was sensibly negative to the last. Perceiving no alteration, I let the stone cool in the open air; when, according to Mr. Canton's rule, it grew more strongly negative, till it was quite cold. Thus the same side of the stone was made negative both in heating and cooling.

HEATING the same flat side, by holding it near a red hot poker, and then just touching it with the palm of my hand

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(when I could not bear it to rest a moment) it became positive. Letting it cool in the air, it was negative, and touching it again with my hand it became positive. Thus I made the same side of the stone alternately positive and negative for a considerable time; and at length, when I could bear to keep it upon my hand, it acquired a strong positive electricity, which continued till it was brought to the heat of my hand.

To complete these experiments, I removed the wax from the convex side, and fastened it to the flat side of the stone. Then warming the convex side, by pressing it against the palm of my hand, it became pretty strongly positive, contrary to what it would have been if heated in the open air, and continued positive in a small degree after it had got all the heat it could from my hand. Letting it cool in the open air, it grew, according to Mr. Canton's rule, more strongly positive, and continued so till it was quite cold. Thus the same side of the stone was made positive both in heating and cooling.

I THEN heated the convex side, by holding it near a red hot poker, and pressing it against the palm of my hand, as soon as I could bear it, it became (contrary to what it would have been in the open air) pretty strongly negative; though it be extremely difficult to get a negative appearance from this side. It cannot always be caught when it is heating in the open air. Care, however, must be taken, lest a slight attraction of the electrified feather, by a body not electrified, be mistaken for negative electricity.

HAVING made the above-mentioned experiments, to see how the tourmalin would be affected by being heated or cooled in contact with various substances, to which only one of its sides was exposed at once; I made others in which the stone was entirely surrounded by them. It appeared very evident, from

Mr.

Mr. Canton's experiment, mentioned p. 305, that it could answer no purpose to inclose it in substances that were conductors: for though the two electricities should be generated, the equilibrium would instantly be restored between them. I therefore made use of electric substances only, and began with *oil* and *tallow*, both covering the tourmalin with them when it was hot, and also heating it in boiling oil. But this treatment produced no new appearance, the electricity of the stone being only a little lessened. The event was the same when a tourmalin was covered with *cement* made of bees wax and turpentine.

AT last I made a small tourmalin very hot, and dropping melted sealing wax upon it, covered the stone all over, to the thickness of about a crown piece; and found it to act nearly, if not quite as well through this coating of wax, as if it had been exposed to the open air. I take it for granted, that the inside of the case of wax next to the stone was possessed of the electricity opposite to that of the stone, at the same time that the outside was the same with it. A pretty deception may be made by means of this experiment: for if a tourmalin be concealed in a stick of sealing wax, the wax will seem to have acquired the properties of the tourmalin.

HEATING the stone, or letting it cool in *vacuo* might easily be imagined to have the same effect as heating or cooling it in contact with conducting substances; I had the curiosity, however, to try the experiment, by letting it cool in an exhausted receiver, in which I had a contrivance to bring a thread of trial near it, or withdraw it at pleasure. The stone was set upright on its edge, by means of bits of glass which it touched but in a few points. The consequence was, that the virtue of the stone seemed to be diminished about one half; owing, perhaps, to the vacuum not being sufficiently perfect. For the same reason,

the tourmalin has but little virtue immediately upon being taken out of boiling water, or after being heated in flame.

ONE time I fixed a thin piece of glass, with a small coating upon it, opposite and parallel to the flat side of the tourmalin, and at about a quarter of an inch distance from it, in an exhausted receiver; to observe whether the electricity would be transmitted from the glass to the stone through the vacuum: but though the glass was electrified, it was so slightly, that I could not be certain of what kind it was.

IN order to ascertain the circumstances relating to the change of the electricity of the tourmalin with more exactness than could be done by heating and cooling the stone in any of the usual methods, I laid it upon the standard bar of the pyrometer, and communicated heat to it by a spirit lamp placed underneath it. The result of these experiments was in general agreeable to Mr. Canton's rules; but a few circumstances occurred in this method of treating it, which could not be determined in any other; and therefore it may be worth while just to mention them. I generally heated the bar, which is of iron, eight inches long, till the index moved seventy degrees, each of which corresponds to one 7200th. part of an inch; and observed, that whichever side of the stone lay uppermost, it was extremely difficult to ascertain the nature of its electricity all the time the heat was increasing; though, in order to do it, I held over it an electrified thread, about two inches in length, fastened to a stick of sealing wax, which just supported it in an horizontal situation. It was evident, however, that it was electrified, by its attracting a thread of silk at the distance of about a quarter of an inch; but if I took the stone off the bar, and immediately presented the side that had lain upon it to an electrified thread or feather, I always perceived the convex side to be negative, and the flat
side

side positive in the same circumstances; but not half so much as they were in the contrary state by cooling. In this case, the two powers were very distinguishable by the small thread above-mentioned, as the stone lay upon the bar; and also by bits of down fastened to silk threads. One of these, which had touched the convex side of the stone, as it lay uppermost upon the bar, could not be made to touch it again in less than five hours and a half.

To see what would be the effect of keeping the tourmalin in the very same degree of heat a considerable time together, I laid it upon the middle part of the bar, heated by two spirit lamps, one at each extremity, and making the index move forty five degrees, I kept it in the same degree of heat, without the least sensible variation, for above half an hour together; and observed, that the upper side, which was the convex one, was always electrified to a small degree, attracting a fine thread at the distance of about a quarter of an inch. If in that time I took it off the bar ever so quick, and presented it to an electrified feather; the flat side, which lay upon the bar, was negative, and the upper side very slightly positive; as appeared by its only not attracting the feather. When I put a piece of glass betwixt the standard bar and the tourmalin, and kept them likewise in the same degree of heat, for the same space of time, the result was the same as before, and the glass was slightly electrified, in a kind opposite to that of the stone.

In heating the tourmalin upon the pyrometer, one of its sides was necessarily made much hotter than the other. This inconvenience I avoided in the following method of treatment, which, though not so accurate in some respects, has peculiar advantages in others. By means of two rough places in the stone, I tied it in a silk thread, which only touched the extreme edge of it on
both

both sides. Being in this manner perfectly insulated, I contrived to make it hang in the air, at any distance from a fire, or candle, &c; and by twisting the string, I could make it present both its sides alternately, so as to heat it very equally.

WHEN, in this manner, I had made it so hot, that I could hardly bear to handle it, I let it remain in the same situation a quarter of an hour, in order to be sure that it was heated equally throughout. Then, with a bundle of fine thread, held some time before in the same degree of heat, I took off the electricity which the stone had acquired in heating, and continuing it in the same situation, I found it acquired extremely little, if any electricity. Sometimes, when I thought it had acquired a little (which might be occasioned by the variation of heat in the fire) it was so small, that I could not determine of what kind it was. This fully satisfied me of the justness of Mr. Canton's observation that it is not heat, but the circumstance of changing its degree of heat that gives electricity to this stone.

IF the stone be heated pretty suddenly, I have sometimes found that it may be handled, and pressed with the fingers several times before the electricity it acquires in heating will be changed, though it begins to cool the moment it is removed from the fire.

IN this same method of treatment, I verified Mr. Canton's observation, that when the tourmalin is heated, and suffered to cool again, without either of its sides being touched, the same side will be positive or negative the whole time of the increase and decrease of the heat. But, as he observes, p. 226, in his experiments on hot air, the stone must, in this case, be heated only to a small degree. I also proved the converse of this proposition: for, beginning where I left the stone in the last experiment, and removing it farther from the fire, both sides acquired

a strong electricity, as usual; and bringing it again nearer to the fire, I observed that both the sides not only retained the electricity they had acquired in cooling, all the time it was heating, but a considerable time after it had remained in the same degree of heat.

I CANNOT, however, entirely acquiesce in the reason that Mr. Canton gives for this appearance: for if the surrounding air would conduct the electric fluid from the positive side of the stone to the negative, I should think it would be in the same situation as in the experiment Mr. Canton made upon it surrounded with water, p. 306, and that neither side would discover any electricity at all. When the heat is three or four times greater than is sufficient to change the electricity of the two sides, the virtue of the stone is the strongest, and appears to be so when it is tried in the very neighbourhood of the fire. In the very center of the fire, the stone never fails to cover itself with ashes, attracted to it from all sides, and from this property it acquired its name in Dutch.

It requires, indeed, some time for the electricity of the sides to change from one state to the other; and therefore the time of the sensible change is not always at the time of its beginning to cool, but these two circumstances will be brought nearer together the hotter the stone is made, because then the efforts (of whatever kind they are) to acquire any particular species of electricity will be the most vigorous, and sooner produce their effect; so as to be more able to overcome obstacles to it, such as must arise from the contrary electricity with which the stone is possessed. Thus, if either side of the stone be in a state to acquire either kind of electricity, and a quantity of the contrary electricity be communicated to it by friction, or *ab extra*, that foreign electricity will be either only weakened, or lost, or changed; and

and these in a longer or a shorter space of time, according to the vigour, as we may say, with which the stone is made to exert itself to counteract that influence. But I have great reason to suspect my own opinion, when it is different from that of so accurate and excellent a judge of this subject as Mr. Canton.

It is a fact, however, that the stone often changes its electricity very slowly; and the electricity it acquires in cooling never fails to remain many hours upon it, with very little diminution. It is even possible that, in some cases, the electricity acquired by heating may be so strong, as to overpower that which is acquired by cooling; so that both sides may show the same power in the whole operation. And I am very certain that, in my hands, both the sides of Dr. Heberden's large tourmalin have frequently been positive for several hours together, without any appearance of either of them having been negative at all. Perhaps the flat side of this stone, which is positive in heating, might continue so according to Mr. Canton's observation; and the electricity of the convex side might have changed, as it very often does, too soon for me to observe it. This fact, however, has happened so often with me, and is so very remarkable, that I think I ought not to omit the mention of it, let the cause be what it will.

This appearance happened so constantly when I first began to make experiments with the tourmalin, that I had concluded the Duc de Noya had reason to assert, contrary to *Æpinus*, that both sides of the tourmalin in all cases acquired positive electricity; and I should have acquiesced in that opinion, had it not been for the friendly remonstrances of Dr. Franklin and Mr. Canton; in consequence of which I renewed my experiments, and at length found other appearances. At the time above-mentioned, I generally heated the tourmalin by presenting each side alternately to a red hot poker, or a piece of hot glass held at the distance

distance of about half an inch; and sometimes I held it in the focus of a burning mirror; but I have since found the same appearance when I have heated it in the middle of an iron hoop made red hot. The stone, in all these cases, was fastened by its edge to a stick of sealing wax. This appearance I have observed to happen the ofteneft when the iron hoop has been exceeding hot, so that the outside of the stone must have been heated some time before the inside; and I also think there is the greatest chance of producing this appearance when the convex side of the stone is made the hotter of the two. When I heat the large tourmalin in this manner, I seldom fail to make both sides positive till the stone be about blood warm. I then generally observe a ragged part of the flat side, towards one end of the stone, will become negative first, and by degrees the rest of the flat side; but very often one part of the flat side will, in this method of treatment, be strongly positive half an hour after the other part is become negative.

THIS account of the appearance is made the more probable by the manner in which the stone was affected when only one of its sides was heated at one time. For when the convex side only was heated, the stone often continued a long time with both its sides positive, generally till it was not sensibly warm. But, in this case, before the convex side became positive, it would sometimes be negative two or three minutes. On the other hand, when the flat side only was heated, it would be positive a long while, and the convex side negative; but the flat side becoming negative a considerable time before the convex side ceased to be so, both sides would continue negative till the stone was nearly cold.

EXTREMELY sorry I am for the article with which I must close this section. In the first of the above-mentioned courses of ex-

periments, that fine tourmalin, which has been so often mentioned in the course of this work, slipped out of my hands; and though it fell only from the height of my breast, upon a boarded floor, two pieces were broke off from one of its ends. The stone, however, is more disfigured than injured by the accident: for the larger of the fragments weighs but ten grains, and the smaller only one, while the rest of it weighs four penny weights sixteen grains. I cannot perceive that its virtue is at all lessened. Mr. Wilson observes, that there were several cracks in it; and for that reason I had been careful never to expose it to any great degree of heat.

It is broke with eight or ten different faces, each of which hath a most exquisite polish; but there is no appearance of any *strata* or *laminae* in the internal structure of the stone. A piece of glass or pitch might be supposed to break in the same manner. The larger of the fragments has considerable power, and the two sides have the same different powers that they had when they were part of the entire stone.

SECTION XIII.

EXPERIMENTS IN WHICH RINGS, CONSISTING OF ALL THE PRISMATIC COLOURS, WERE MADE BY ELECTRICAL EXPLOSIONS ON THE SURFACES OF METALS.

IT was a discovery of Sir Isaac Newton, that the colours of bodies depend upon the thickness of the fine plates which compose their surfaces. He hath shown that a change of the thickness of these plates occasions a change in the colour of the body, rays of a different colour being thereby disposed to be transmitted through it, and, consequently, rays of a different colour reflected at the same place, so as to present an image of a different colour to the eye. A variation in the density occasions a variation in the colour, but still a medium of any density will exhibit all the colours, according to the thickness of it. These observations he confirmed by experiments on plates of air, water, and glass. He likewise mentions the colours which arise on polished steel by heating it, as likewise on bell metal, and some other metalline substances, when melted and poured on the ground, where they may cool in the open air, and he ascribes them to the *scorie*, or vitrified parts of the metal,

which he says most metals, when heated or melted, do continually protrude, and send out to their surfaces, covering them in the form of a thin glassy skin.

THIS great discovery concerning the colours of bodies, depending upon the thickness of the fine plates which compose their surfaces, of whatever density those plates may be, I have been so happy as to hit upon a method of illustrating and confirming by means of electrical explosions. A number of these being received on the surface of any piece of metal, change the colour of it to a considerable distance from the spot on which they were discharged, so that the whole circular space is divided into a number of concentric rings, each of which consists of all the prismatic colours, and perhaps as vivid as they can be produced in any method whatever.

IT was not by any reasoning *a priori*, but by mere accident, that I was led to the discovery of these colours. Having occasion to take a great number of explosions, in order to ascertain the lateral force of them; I observed that a piece of brass, through which they were transmitted, was not only melted, and marked with a circle by a fusion round the central spot, but likewise tinged beyond the circular spot with a greenish colour, which I could not easily wipe out with my finger. Struck with this new appearance, I replaced the apparatus, and continued the explosions, till, examining with a microscope, I plainly perceived all the prismatic colours, in the order of the rainbow. The diameter of the red, in this instance, was one third of an inch, and of the purple near one fourth. The diameter of the whole coloured space in the subsequent experiments, in which I generally used thirty or forty explosions, was near an inch.

PLEASED with the first experiment, I presently diversified in

a great variety of ways, the result of which I shall comprise in the following observations.

1. WHEN a pointed body is fixed opposite to a plain surface, the nearer it is placed, the sooner the colours appear, the closer do they succeed one another, and the less space they occupy. It seems, however, that when the point is at such a distance, that the electric matter has room to expand, and form as large a circular spot as the battery will admit, this coloured space is as large as it is capable of being made; but still the colours appear later, in proportion to the distance beyond that. When the point is fixed exceeding near, or made to touch the surface, the colours appear at the first explosion, but they spread very irregularly, and make no distinct rings.

2. THE more acutely pointed is the wire, from which the electric fire issues, or at which it enters, the greater is the number of rings. A blunt point makes the rings larger, but fewer. It is likewise much later before they make their appearance at a given distance.

3. IN making these rings, the first appearance is a dusky red about the edge of the central spot; presently after which (generally after four or five strokes) there appears a circular space, visible only in an oblique position to the light, and looking like a shade on the metal. This expands very little during the whole course of the explosions. It seems to be an attempt, as it were, at the first red; for, by degrees, as the other colours fill the greater part of that space, the extreme edge of it becomes a deeper brown.

4. AFTER a few more explosions, a second circular space is marked, by another shade beyond the first, being one eighth or one tenth of an inch in breadth, which I have never observed to change its appearance, after ever so many explosions. This
shade,

shade, by succeeding the first, which becomes gradually of a brown or light red colour, seems to be an attempt at the fainter colours, which intervene between the reds.

5 ALL the colours make their first appearance about the edges of the circular spot. More explosions make them expand towards the extremity of the space first marked out; while others succeed in their place; till, after thirty or forty explosions, three distinct rings appear, each consisting of all the colours. If the explosions be continued farther, the colours become less beautiful and distinct, the red generally prevailing, and suffusing the rest.

6. THE innermost, *i. e.* the last formed colours, are always the most vivid, and those rings are likewise closer to one another than the rest.

7. THESE colours may be brushed with a feather, or a finger may be drawn over them, without injury; but they are easily peeled off, with ones nail, or any thing that is sharp. The innermost are the most difficult to erase.

8. THE first rings are sometimes covered with a quantity of black dust, part of which, however, may be wiped off, with a feather, and the colours will appear under it.

9. IT makes no difference whether the electric matter issue from the pointed body upon the plate, or from the plate upon the pointed body, the surface opposite to the point being marked exactly alike in both cases; also the points themselves, from which the fire issues, or at which it enters, are coloured to a considerable degree, about half an inch. The colours, also, return here as upon the plate.

10. THE more circles are made at the same time, the more delicate, I think, the colours will be, whereas the surface is torn, as it were, by violent explosions, and the colours appear rough and coarse. But this roughness is only perceived on steel. On silver,

silver, tin, and polished brass, the colours were always equally free from that coarseness.

11. A POLISHED surface is not necessary for these colours, for they appear very well, though they do not make so beautiful an appearance on the rough surfaces.

12. THESE coloured rings appear equally well on all the metals that I have tried, viz. gold, silver, copper, brass, iron, steel, lead, and tin. I have not tried any of the semi-metals, but I have no doubt of their answering as well as the proper metals.

13. WHEN the pointed wire was made to incline to the plane on which the colours were made, the circular spot was quite round, and the center of it in a perpendicular let fall from the point upon the plain surface; but the colours were projected in an oblong form, the center being in the pointed wire continued.

UPON shewing these coloured rings to Mr. Canton, I was agreeably surprized to find, that he had likewise produced all the prismatic colours from all the metals, but by a different operation. He extended fine wires, of all the metals, along the surface of pieces of glass, ivory, wood, &c. and when the wire was exploded, he always found them tinged with all the colours. They are not disposed in so regular and beautiful a manner as in the rings I produced, but they equally demonstrate, that none of the metals, thus exploded, discovers the least preference to one colour more than to another. A variety of other very extraordinary appearances occurred in the course of Mr. Canton's experiments in melting wires, but I forbear to mention them, as I hope he will soon favour the public with a communication of them himself.

IN what manner these colours are formed, it may not be easy to conjecture. In Mr. Canton's method of producing them, the metal, or the calcined and vitrified parts of it, seem to be dispersed

perfed in all directions from the place of explosion in the form of spheres, of a very great variety of sizes, tinged with all the variety of colours, and some of them smaller than can be distinctly seen by any magnifier. In my method of making these colours, they seem to be produced in a manner similar to the production of colours on steel, and other metals by heat, *i. e.* the surface is affected, without the parts of it being removed from their places, certain plates or *laminæ* being formed, of a thickness proper to exhibit the respective colours.

SECTION

SECTION XIV.

EXPERIMENTS ON THE LATERAL FORCE OF ELECTRICAL EXPLOSIONS.

BEING informed, in accounts of damages done by lightning, of persons and things being removed to a considerable distance, without receiving any hurt; I was excited to try whether I could produce similar effects by electricity. All the other known effects of lightning had been frequently imitated by the application of this power, but I do not know that this effect has ever been so much as taken notice of by any electricians. The experiments I presently found to be very easy, and I think it not difficult to ascertain the cause, and the manner in which this striking effect is produced.

IF pieces of cork, powder of any kind, or any light bodies whatever, be placed near the explosion of a jar or battery, they will not fail to be moved out of their places, upon the instant of the discharge. If the explosion of a large battery be made to pass over the surface of animal or vegetable substances, in the manner described above, and large corks be strewed along or near the part intended for it, it is surprizing to observe with what violence

lence they will be driven about the room. This dispersion is in all directions from the center of the explosion, and it makes no difference whether the rods, between which it is made, be sharp pointed or otherwise.

THE effect of this lateral force is very remarkable in attempts to fire gun powder in electrical explosions. If the gun powder be confined ever so close in quills or cartridges, and they be held fast in vices; yet, when the explosion is made in the center of them, it will sometimes happen (even when a wire has been melted in the midst of the powder, and the fragments have been seen red hot, for some time, in different parts of the room) that the powder has not been fired, or only a few grains of it, the rest being dispersed with great violence, part of it flying against the faces of persons who assisted in making the experiments. This circumstance, together with the charcoal being a conductor of electricity, makes it so extremely difficult to fire gun powder by electrical explosions; and it is evidently owing to this lateral force, that parts of the melted wire fly so many ways, and to so great a distance from the place of explosion.

THIS lateral force is exerted, not only in the neighbourhood of an explosion, when it is made between pieces of metal in the open air, but also when it is transmitted through wires that are not thick enough to conduct it perfectly; and the smaller the wire, and the more complete the fusion, the greater is the dispersion of light bodies placed near it. At one time, when the wire was not melted, but turned blue by the explosion (in which case it generally assumes a dusky red, which lasts but for a moment) there was a small dispersion from every part of the wire, but by no means so great as it would have been if it had been melted, or only heated to a greater degree.

BY a considerable number of trials, I found, that a greater
force

force of explosion would move light bodies at a greater distance, but the smaller the bodies were, the less was this difference; so that I imagined, that if they had no weight at all, they would, probably, be moved at the same distance, by the explosion from any quantity of coated surface, charged equally high: but there was a great difference in the weights removed by different forces at the same distance. Placing the same piece of cork at the same distance from the place of explosion, I found that the discharge of one jar removed it one fourth of an inch, two jars one inch and one fourth, three jars one inch and three fourths, and four about two inches, so that I do not wonder at very heavy bodies being moved from their places, and to considerable distances, by strong flashes of lightning.

THAT the immediate cause of this dispersion of bodies in the neighbourhood of electrical explosions is not their being suddenly charged with a quantity of electric matter, and therefore flying from others that are equally charged with it is, I think, evident from the following experiments and observations. I never observed the least sensible attraction of these light bodies to the brass rods through which the explosion passed, or to the electric matter passing between them, previous to this repulsion, though I used several methods, which could not have failed to show it, if there had been any such thing. Sometimes I suspended them in fine silken strings, and observed that they had contracted no electricity after they had been agitated in the manner described above. Sometimes I dipped them in turpentine, and observed that no part of it was found sticking, either to the brass rods themselves, or to any part of the table betwixt them and the place where the light bodies had been laid. I even found that the explosion of a battery, made ever so near to a brass rod, did not so much as disturb the equilibrium of the electric fluid in the body itself; for when I had insulated the rod, and hung a pair of pith balls on the end

opposite to that near which the explosion passed, I found that the balls were not in the least moved at the time of explosion; which they would have been, if part of the electric fluid natural to the body had been driven, though but for a moment, towards the opposite end. I also observed that the effect was the same when the explosion was made to pass through one of the knobs of the insulated rod. This lateral force was evident through thin substances of various kinds, interposed between the explosion and the bodies removed by it; as paper, tin foil, and even glass; for when some grains of gun powder were put into a thin phial close stopped, and held near the explosion of a battery, they were thrown into manifest agitation.

I THEREFORE think it most probable, that this lateral force is produced by the expulsion of the air from the place where the explosion is made. For the electric matter makes a vacuum of air in its passage; and this air, being displaced suddenly, gives a concussion to all the bodies that happen to be near it. Hence the removal of the light bodies, and the agitation communicated to the thin substances, and to the air, and the light bodies placed beyond them.

THE only objection to this hypothesis is, that this lateral force is not so much less in vacuo as might be expected, when the air is supposed to receive the concussion first, and to communicate it to other bodies; but it must be considered, that the most perfect vacuum we can make with a pump is not free from air. I have tried to make this experiment in a torricellian vacuum, but could not succeed at that time. Besides, as the electric matter of which an explosion consists must take a wider path in vacuo, if not equally fill the whole space, it may affect a body in its passage, without the intervention of any air. In condensed

air,

air, this lateral force was not, as far as I could perceive, much encreased.

WILLING to feel what kind of an impulse it was that acted upon bodies, when they were driven away by this lateral force of electricity, I held my finger near the path of an explosion of the battery, passing over the surface of a green leaf, when I felt a stroke, as of something pushing against my finger. Several corks, placed in the same situation, were driven to a considerable distance by the same explosion.

RECOLLECTING that this power, which I now call the *lateral force of electrical explosions*, must be the same with that which gives the concussion to water, mentioned in my experiments to imitate an earthquake, and to vegetable and animal substances, over the surface of which it passes; and being determined to make a more satisfactory trial of it then I had ventured to do before, I laid a green leaf upon the palm of my hand, intending to make the explosion pass over the leaf; but the leaf was burst, and torn to pieces, and the explosion passing over my hand gave it a violent jar, the effect of which remained in a kind of tingling for some time.

LASTLY, in order to judge the more perfectly of this force, I laid a chain communicating with the outside of the battery upon my bare arm above the wrist, and bringing the discharging rod near the flesh, within about two inches and an half of the chain, I made the explosion pass over that quantity of the surface of the skin. Had I taken a greater distance, I was aware that the explosion would have entered the flesh, which, I was sensible, would have given a painful convulsion to the muscles through which it passed. In this case, the sensible effect was very different from that, being the same external *concussion* as before, and I have sometimes thought, that the sensation is not disagreeable. How-

ever

ever the hairs upon the skin were all singed, and curled up along the whole path of the explosion, and for the space of about half an inch on each side of it also the *papille pyramidales* of the skin were raised, as when a person is shivering with cold. This was also the case in every part of the arm which the chain touched, and even that part of it which was not in the circuit. Both the path of the explosion, and the place on which the chain had lain, had a redness, which remained till the next day. Sometimes the flesh has contracted a blackness by this experiment, which has remained for a few hours.

... of the explosion, must be continued with that which gives the conclusion to what, recorded in my experiments, in my experiments, that a vegetable and animal substance, over the first part of which it passed, and being detained to make a more lasting stay, till after that I had returned to do so. I laid a green leaf upon the palm of my hand, intending to make the explosion pass over the leaf, but the leaf was burnt, and burnt so severely, that the explosion passing over my hand gave it a worse fate, the effect of which remained in a kind of ring, for some time.

I desired, in order to judge the more perfectly of this force, I laid a chain communicating with the outside of the battery upon my bare arm above the wrist, and bringing the discharging rod near the foot, within about two inches and in half of the chain, I made the explosion pass over that quantity of the surface of the skin. This I took a great pleasure, I was aware that the explosion would have injured the skin, which I was sensible, would have been a great disadvantage to the experiment, which is not the case, the effect of which was very different from that being the same as that of a chain, and I have sometimes thought, that the nature is not dissimilar. How-

SECTION

SECTION XV.

VARIOUS EXPERIMENTS ON THE FORCE OF ELECTRICAL
EXPLOSIONS.

MAKING the explosion of a battery pass over the surface of a green cabbage leaf, I observed that it left a track, near a quarter of an inch in breadth, exceedingly well defined, and distinguishable by a difference of colour from the rest of the leaf. Along this path also the firmness of texture in the leaf was entirely destroyed, that part becoming quite flexible, like a piece of cloth. Presently after it turned yellow, grew withered, and became perfectly brittle.

WILLING to try the effect of this explosion passing along the surface of other substances, I laid a piece of common window glass on the path, pressed by a weight of six ounces; but it was shattered to pieces, and totally dispersed, together with the leaf on which it lay. Placing the black side of a piece of cork wood upon it, pressed by a weight of half a pound, the leaf was not rent, but the cork was furrowed all the way, a trench being made in it, about half an inch in breadth, and a quarter of an inch in depth. Laying the smooth cut surface of the piece of
cork,

cork, it was furrowed all the way as if it had been cut with a file, but not near so deep as before. Many of the small pieces which had been rubbed off in the explosion, remained in the furrow. Also the substance of the cork seemed to be shattered, and it was easily rubbed off, a little way into it.

I MADE this explosion on the surface of some red wine in a small dish, and kept a part of the same quantity exposed in a similar manner, but I could perceive no difference between them after several days.

THE track of an electrical explosion on the surface of the cabbage leaf being so well defined, suggested an experiment to ascertain whether there was any sensible *momentum* in the electric fluid, when it is rushing with violence from one side of a battery to the other. For this purpose I made the explosion pass over the leaves when they were cut in right and acute angles; so that the shortest path, from the inside to the outside of the battery, was to turn close at the angle; and observed, that it was not diverted from its course in the least degree by the rapidity of its own motion, but that it had turned exactly at the angle; and kept as close to the opposite side, as if the motion had begun at the angle. The electric matter had however been evidently attracted by the veins of the cabbage leaf, having pursued them a little way, at least having sensibly affected them, wherever it met with them in its passage.

THIS experiment suggested another, intended to determine whether the force of an explosion was at all diminished by being diverted from a right lined course, and made to turn in a great number of angles. To do this, I first found, by a great number of trials, what length of a small iron wire I was able to melt with a battery of about twenty square feet, in the middle of a circuit of about three yards of brass wire, considerably thicker than

than the iron, and stretched in two right lines, suspended on silken strings. The length of the iron wire melted in these circumstances was about three inches. I then took the same brass wire, and, fixing pins into a board of baked wood, twisted it about them, making it turn in a very great number of acute angles; and I put three inches of the same iron wire in the middle of this crooked circuit, that I had done in the straight one, so that the electric matter in the explosion was obliged to make a great number of turns at acute angles, before it could come to the iron wire; but I always found that the same length of iron wire was melted in these circumstances, as in the other, and not the least difference was perceived in the force.

BUT though the *form* of the wire through which an explosion passed made no difference in its force, I found a very remarkable difference occasioned by the *length of the circuit*, in wires of the same thickness, and which, I own surprized me very much.

IN order to ascertain the practicability of firing mines by electrical explosions, I took twenty two yards of small brass wire (but so thick, however that I could not have melted the least part of it, by the force of any battery I have ever constructed) and extending it along a dry boarded floor, with a small piece of iron wire, and a cartridge of gun powder about it, in the place that was most remote from the battery; I found that, upon the discharge, the wire was not melted, nor the gun powder exploded; also the report was very faint. In other circumstances a charge of the same battery was able to melt more than nine inches of this iron wire, and this same cartridge was easily fired near the battery, connected with shorter pieces of the same brass wire; so that the diminution of force must have been owing to the length of the circuit.

IN the place of this small brass wire, I substituted an iron

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wire,

wire, one fifth of an inch thick; when about half an inch of the small iron wire was exploded; so that the force was not lessened so much in a circuit of the thick iron wire, as it had been in one of the small brass wire. In order to judge how much of the force might be lost by nearer circuits, consisting of less perfect conductors, I joined the middle of the circuit made by the iron wire with water, in which both the wires were immersed. The effect was, that the small iron wire was only made red hot, but not exploded as before.

BEING sensible how much depended upon avoiding all lesser circuits, whereby part of the fire of an explosion might return to the battery, without reaching the extremity of the circuit, where I intended the whole of its force to be exerted; in the remaining experiments, I insulated half the circuit of iron wire. There was no occasion for insulating the whole circuit: for if there was but one passage *to* or *from* the middle of it, there could be put one *from* or *to* it. In this method it was easy to ascertain what loss of force was occasioned by the length of the circuit, as every other circumstance was carefully excluded. And it presently appeared to be very considerable; for though I could melt nine inches of the small iron wire at the distance of fifteen yards from the battery, when I tried twenty yards, I found that I was just able to make six inches of it red hot. The battery in these experiments was in the house, and the wires of which the circuit consisted were conveyed by silken strings into a garden adjoining to the house.

MENTIONING this loss of force, occasioned by the length of the circuit, in electrical explosions to Dr. Franklin, he told me that the same observation had occurred to him, and that he also had been disappointed in an attempt to fire gun powder at a distance from his battery.

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STRUCK with this appearance, I endeavoured to ascertain the quantity of this obstruction, by trying what other courses the electric fire would chuse, preferably to a long metallic circuit. In the first place, taking about a yard of the small brass wire mentioned above, I disposed it in the manner described [fig. 9. Pl. i.] connecting one of the ends with the outside of the battery and the other with the inside. In the first place I brought the parts [a] and [b] (near the two extremities) into contact, and, upon the discharge, found there had been a fusion in that place, and that a great part of the fire had taken the shorter circuit, though it had been obliged to quit the wire in one place, and enter in again in another. Afterwards I removed the parts [a] and [b] to a small distance from one another, and, upon the explosion, observed a strong spark pass between them. Removing them to greater and greater distances, I found the explosion chose to pass above one third of an inch in the air, rather than make the circuit of the continued wire. Using a longer and smaller iron wire, the passage through the air exceeded half an inch. I then took four or five yards of iron wire, one tenth of an inch thick; when the passage through the air was still half an inch; and taking three yards and a half of a wire that was one fifth of an inch thick, the spark in the air was half an inch, and sometimes near three quarters of an inch. Making use of only half the length of this wire, the passage through the air was only half that distance, or one fourth of an inch. When I kept the place of near contact about the middle of this wire, and made the explosion at the extremities of the whole wire, I was obliged to bring them about as near again, i. e. to little more than one eighth of an inch before the passage would be through the air; so that the force of the whole explosion must have been greatly weakened by its passing through so

much of the wire. Lastly, I took a pair of kitchen tongs, the legs of which were two feet in length, and the smallest part of them above half an inch in diameter; when the circuit was made about one sixth of an inch in the air (for at that distance from one another the ends of the tongs had been fixed) rather than through four feet of that thick iron.

NOTWITHSTANDING this passage of the explosion through the air, at the same time that a metallic circuit was open for it, it was evident that the whole of the force did not pass this way, nor indeed the greatest part of it. For when I extended a small iron wire between [*a*] and [*b*,] I could only make about half an inch of it red hot, whereas, when there was no other metallic circuit, I was able, with the same battery, to explode more than two inches of it.

As the electric fire meets with so much obstruction in passing through a circuit of iron of this thickness, I make no doubt, but that it is considerably obstructed in passing through metallic circuits of any thickness whatever; and that it would prefer a passage through the air, if they were made even of no great length. In this method, the different degrees of conducting power in different metals may be tried, using metallic circuits of the same length and thickness, and observing the difference of the passage through the air in each. N. B. A common jar answers as well in these experiments as a large battery.

It is evident, from many experiments, that the whole fire of an explosion does not pass in the shortest and best circuit, but that if inferior circuits be open, part will pass in them at the same time. Of this I made the following satisfactory trial. I took an iron chain, and laid it upon a table, in contact with a charged jar; so that the parts of it made two circuits for the discharge, which I could vary at pleasure; and I observed that,
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when one of the circuits was but half an inch, and the other more than half a yard; yet, if the charge was high, it always went in them both, there being considerable flashes between the links of the remotest part of the chain. If the charge was weak, it passed in the shorter circuit only.

It is evident that when the wires of a battery are not in close contact, there must be some loss of force in the discharge; but this never appeared to me to be so considerable, as Mr. L. Epinaffe seems to have imagined.* In order to ascertain this by experiment, I first found, by repeated trials, what length of an iron wire, of a certain thickness, I was able to melt with a battery consisting of twenty jars, constructed in the manner described above. It was about two inches and a half. I then foldered all the wires together, and also foldered one rod to them all, instead of a chain which I had used before, so that I avoided near a hundred sparks, in each of which some force had been lost; but I did not find, after many trials, that the power of the battery was sensibly diminished. I still could not melt three inches of the same wire.

* Phil. Trans. Vol. 57. pt. 1. p. 186.

SECTION XVI.

MISCELLANEOUS EXPERIMENTS.

I. OBSERVATIONS ON THE ELECTRIC SPARK TAKEN THROUGH SEVERAL PIECES OF METAL.

MARCH the 24th. 1766. I observed that an electric spark taken from the prime conductor itself was not near so strong and pungent, as one taken through a piece of metal interposed, and interposed between my finger and the conductor.

THE effect was the same whatever was the form of the interposed piece of metal. And, in this manner, whatever was presented received a full and strong spark: whereas a great part is commonly dissipated, in pencils or stars, even when pretty large brass knobs are presented to the prime conductor itself, if the excitation be very powerful; unless both the conductor and knob have one precise degree of convexity, adapted to one another.

ONE single brass ball made the spark as strong as the interposition of a long piece of metal, or of many pieces.

WHETHER one, a few, or a great number of pieces were used, it seemed that the intervals taken together must be equal.

BUT

BUT these intervals taken together will be larger when the pieces are placed in a right line, than when they are laid in a curve.

WHETHER one body, or a number of them be interposed; if a spark be solicited it will not strike the first, unless it can, at the same time, strike all the rest.

ALL these experiments succeed, in the same manner, with the explosion of a charged jar.

SOME of the above-mentioned circumstances, I afterwards found, had been taken notice of by Signior Beccaria.

II. A DECEPTION RELATING TO THE DIRECTION OF THE ELECTRIC SPARK.

As I was once amusing myself with taking long sparks from a large prime conductor of polished copper, and considering the deceptions that electricians had fallen into with respect to the direction of the electric matter; I could not help being struck with one deception, which the evidences of my senses would never have rectified; and which showed very clearly, how little the evidences of the senses is to be depended upon in such cases. I observed, that, whether I made this large conductor give, or take the electric fire (for I could make it do either at pleasure, and with the same force) I still fancied that a spark taken with a brass ball above the conductor descended to it, and that a spark taken below it descended from it; but sparks taken laterally seemed to have no one certain direction.

III.

III. AN EXPERIMENT INTENDED TO ASCERTAIN WHETHER ELECTRICAL SUBSTANCES, IN THEIR NATURAL STATE, CONTAIN MORE OF THE ELECTRIC FLUID THAN CONDUCTORS.

THINKING to ascertain Dr. Franklin's hypothesis, concerning the essential difference between conductors and non-conductors, I made a pretty large piece of glass red hot (in which state I had proved it to be a real conductor of electricity) and placed it upon a smooth piece of copper, insulated; supposing that, if electric substances had naturally a much greater share of the electric fluid than conductors, this piece of glass, in passing from a conducting to a non-conducting state, must exhaust the copper of its natural share of the electric fluid, and leave it electrified negatively. But I could perceive no kind of electricity, either in the copper, or the glass, during the whole time of its cooling.

SOME time after, I found that Mr. Cigna had endeavoured to ascertain the same thing, by reducing ice into water; but ice and water are both conductors of electricity.

IV. THE MUSICAL TONE OF VARIOUS DISCHARGES ASCERTAINED.

As the course of my experiments has required a great variety of electrical explosions I could not help observing a great variety in the musical tone made by the reports. This excited my curiosity to attempt to reduce this variation to some measure. Accordingly, November the 17th. by the help of a couple of spinets, and two persons who had good ears for music, I endeavoured

voured to ascertain the tone of some electric explosions; and observed, that every discharge made several strings, particularly those that were chords to one another, to vibrate: but one note was always predominant, and sounded after the rest. As every explosion was repeated several times, and three of us separately took the same note, there remained no doubt but that the tone we fixed upon was, at least, very near the true one. The result was as follows.

A JAR containing half a square foot of coated glass sounded F sharp, concert pitch. Another jar of a different form, but equal surface, sounded the same.

A JAR of three square feet sounded C, below F sharp. A battery, consisting of sixty four jars, each containing half a square foot, sounded F below the C.

THE same battery, in conjunction with another of thirty one jars, each containing a square foot, sounded C sharp. So that a greater quantity of coated glass, always gave a deeper note.

DIFFERENCES in the degree of a charge in the same jar made little or no difference in the tone of the explosion; if any, a higher charge gave rather a deeper note.

FROM these experiments it will be easy for any person to compare the quantity of square feet of coated glass, with the lengths of musical strings giving the same note. For this purpose, I could easily have found more terms of the series; but I am afraid philosophers in general will think it trifling enough to have found so many. I do not expect that electrical explosions will ever be introduced into concerts of music; or that these experiments will be of any use to measure the extent of the clouds from which a clap of thunder proceeds. But true philosophers will not absolutely despise any new fact or observation, though it have no immediate, or apparent use.

V. EXPERIMENTS ON THE EFFECTS OF GIVING A METALLIC
TINGE TO THE SURFACE OF GLASS.

It has long been a question among electricians, where the electric matter that constitutes the charge of a plate of glass lies; whether within the pores of the glass, or only upon the surface; and some experiments I have made will perhaps be thought to throw some light on this difficult subject.

I CONSIDERED that the common coating of a jar is not in actual contact with the glass, but that the metallic tinge, which is given to glass by an electric explosion of the metal upon its surface, is probably in contact with it, if not lodged in its pores. I therefore gave a coating of this kind to both sides of a plate of glass; and at first imagined that the glass coated in this manner did receive a charge, as well as if it had been coated in the common way; for it gave a real shock; but I very well remember, at that time, being a little surprised to see the electric fire run over the surface of that coating, a thing not possible in the common way. However, not sufficiently attending to that circumstance, I was pursuing the experiment, and trying whether, by combining this piece of glass with a large battery, and making the discharge of both upon this metallic tinge, I could not melt part of it, and thereby fetch it out of the glass; as that method would have melted, and absolutely dispersed a considerable part of the coating of a common jar. But I was prodigiously surprised to find, that, though the connection of this metallic tinge with the battery was complete, the discharge could not be made by bringing the discharging rod upon it; though within three quarters of an inch of another brass rod, that formed the communication between this plate of glass and the battery. This convinced

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ced me that the metallic tinge did not answer the purpose of a coating; and I presently satisfied myself, that a piece of uncoated glass would receive just such a charge as the tinged glass had done.

To ascertain this matter still further, I struck a tinge of this kind along two opposite sides of a glass tube, about half a yard in length; and holding it with my hand in contact with a part of this metallic tinge, found that it was excited just like another tube: for when I discharged the electricity of any part of the tube where the tinge was struck, it did not at all discharge other parts of the tube, whither the same tinge extended. Also the electric snapping from the tinged part of the glass could not be distinguished from the snapping at other places; except that, sometimes, where the gold lay thicker than ordinary, a denser stream of electric matter was visible on its surface, and ran in several small streaks, in different directions, from the place where the spark was taken.

This experiment seems to show, that a coating of metal exceedingly near the surface of the glass is not at all affected either by the excitation or charging of it; and seems to confirm the hypothesis of the electric fluid not entering the pores of the glass.

As the giving this metallic tinge to both sides of a plate of glass is not very easy, the reader will not, perhaps, be displeased to be informed in what manner I succeeded in it. After fatiguing myself a long time in endeavouring to strike a piece of leaf brass into the two sides of a plate of glass, to serve instead of a coating (having always broken the glass in fixing either the first or second coating) I at length put two other pieces of glass, one on each side of that to which I intended to give the tinge, with pieces of leaf brass between them both; and making one explosion through both of them at the same time, the upper and the lower piece of glass were shattered to pieces, but the middle piece

(being equally affected on both sides) remained whole, and the coatings were nearly as I wished them.

VI. AN EXPERIMENT INTENDED TO ASCERTAIN WHETHER FERMENTATION CONTRIBUTES TO THE PRODUCTION OF ELECTRICITY.

SEPTEMBER the 3d. In order to determine whether any of the electric fluid was discharged from, or acquired by bodies in a state of *fermentation*; I hung a pair of pith balls at the extremity of a piece of wire communicating with a quantity of steel filings, fermenting with oil of vitriol, inclosed in a glass vessel. But they never separated in the least.

VII. AN EXPERIMENT INTENDED TO ASCERTAIN WHETHER EVAPORATION CONTRIBUTES TO THE PRODUCTION OF ELECTRICITY.

DECEMBER the 26th. I put a small quantity of water upon a thin piece of glass, and made it all suddenly evaporate by a red hot iron held under it; but the glass had acquired no degree of electricity. The weather was frosty.

VIII. AN EXPERIMENT INTENDED TO ASCERTAIN WHETHER FREEZING BE ACCELERATED OR RETARDED BY ELECTRIFICATION.

JANUARY the 6th. 1767. I exposed two dishes of water in the open air, while it was freezing intensely, and electrified one of them

them pretty strongly; but could perceive no difference in the time, either when it began to freeze, which was in about three minutes, or in the thickness of the ice when both had been frozen some time.

HAPPENING to cast my eyes into the fields, out of the window, through which I had put the board which I used for the purpose of this experiment, I observed, on each side of the electrified wire, the same *dancing vapour*, which is seen near the surface of the earth in a hot summer's day, or near any heated body that occasions an exhalation of vapours.

IX. THE EXAMINATION OF A GLASS TUBE, WHICH HAD BEEN A LONG TIME CHARGED AND HERMETICALLY SEALED.

DECEMBER the 30th. I examined a glass tube, about three feet in length, one half of which I had charged in the month of March preceding, and then sealed hermetically; but could not perceive that it was excited in the least degree, either by heating or cooling. The difference in the result of this experiment from several of Mr. Canton's, related p. 280, I attribute to the thickness of the glass of my tube. Mr. Canton charged small balls exceeding thin. I also observed that there was no perceivable difference in the excitation of the charged or uncharged part of this tube, and that both parts acted exceeding well.

I AFTERWARDS opened this tube, and pouring a quantity of leaden shot into it, found it to contain a very good charge. It gave me one considerable shock, and several small ones; as I made no use of an outward coating, but only discharged it by grasping it in several places by my hand.

X.

X. THE WEIGHT REQUISITE TO BRING SOME BODIES INTO CONTACT ASCERTAINED BY THE ELECTRICAL EXPLOSION.

It is plain from optical experiments, and also from a variety of other considerations, that bodies of no great weight lying upon one another, are not in actual contact. As the same thing is demonstrated by an electric spark being visible between pieces of metal lying upon one another, and other effects of electricity (particularly the fusion of the parts through which it goes out of one body and enters another, not actually in contact with it) I was desirous to determine, by this criterion, what weight was sufficient to bring bodies into actual contact. With these views, I began with laying twenty smooth shillings upon one another, and making the discharge of the battery through them; thinking that the fusion would disappear, when the weight was sufficient to press them into contact. But I found that the whole column was not sufficient; for every piece was melted on both its sides, so that every two contiguous sides had spots exactly corresponding to one another. The deepest impressions were made near the top of the column, but they did not diminish with exact regularity. Perhaps small particles of dust might prevent some of them from coming sufficiently near one another.

AFTERWARDS, I gradually increased my weights, till I found that about six pounds was sufficient for my purpose. The fusion was visible under that weight, but never under above half a pound more, though I repeated the experiment several times.

I HAD some suspicion, that the largeness of the explosion might have occasioned a momentary repulsion, separation, and consequent

quent fusion of these pieces of metal, though pressed by such a weight, but I found I was not able to produce any fusion; under a greater weight than that above-mentioned, though, instead of thirty two square feet of coated glass, I used above sixty.

XI. THE EFFECT OF THE ELECTRICAL EXPLOSION TRANSMITTED THROUGH VARIOUS LIQUORS.

I BELIEVE it is generally supposed, that ale, and other liquors are turned sour by lightning, and I was desirous of ascertaining whether that fact (if it be one) was owing to the liquors being properly struck with the lightning, or to the state of the air, &c. during the thunder storm. In order to this, I provided myself with a glass tube, nine inches long, and about a quarter of an inch in diameter, and by inserting a wire into one end of it, which was stopped with sealing wax, could easily transmit an electrical shock through any substances contained in it.

By this means, November the 13th. I began with discharging the explosion of the battery through this tube, filled with *fresh small beer*, and observed a considerable quantity of fixed air, or something in the form of bubbles, to ascend in it; but when I tasted it, I could perceive no difference between it and that out of which it was taken. No doubt the escape of so much air would tend to make it grow stale something sooner.

I THEN discharged several large shocks through a tube filled with *red wine*, but, after two or three days, could perceive no alteration in its taste, or other sensible qualities. In this discharge, the electric matter did not immediately strike the wine,

wine, but a metal rod, which just touched its surface; but I afterwards gave it two or three more shocks, in which the wine itself was made to receive the explosion, but there was no variation in the effects.

I PASSED the shock through a tube filled with *milk*, in both the methods above-mentioned; but it was sweet three days after. Also a tube filled with *fresh ale* received several large shocks without undergoing any sensible change of properties.

IN all these explosions I held the tube in my hand, without feeling any thing of the shock.

I ALSO made the electric spark visible a great number of times in a small quantity of *syrup of violets*, without producing any change of colour, or other sensible qualities.

XII. OBSERVATIONS ON THE COLOURS OF ELECTRIC LIGHT.

FINDING it advanced in the writings of several electricians (who must have copied it from one another, without ever repeating the experiment, though it may be done so soon) that electric light contained no prismatic colours; I had the curiosity to try so extraordinary a fact, and immediately saw both the fallacy of the experiment when it was first made, and the cause of it. Holding a prism before my eyes, while the electric sparks were taken at the prime conductor, I observed as beautiful prismatic colours as any that are exhibited by the image of the sun; but when the light was a little diffused, as in those red or purple parts of a long spark, as it is called, the colours were not so vivid, and less easily distinguished from one another; and when the light was still more diffused, through a vacuum, the

the prism made no sensible alteration in the appearance of it. Thus the middle part of any large object appears of its natural colour through a prism: for though the rays be really separated, they are immediately confounded with others from different parts of the same object; so that its natural colour must necessarily be the result.

As the flames of different bodies yield very different proportions of the prismatic colours, I have often thought of attempting to ascertain the proportion of these colours in electric light, and compare it with the proportion of colours from light procured in various other ways. This may possibly determine what that heterogeneous matter is, that is roused into action by the rapid passage of the electric fluid, and which is the cause of the light, the smell, and perhaps other sensible qualities of electricity; but I have not had leisure to pursue the inquiry.

THE electric spark, taken in the middle of a phial filled with inflammable air, is always of a red or purple colour, and cannot be made to look white; but the larger the explosion is, the nearer it approaches to white.

I SHALL close this article with just mentioning another deception, which some persons may possibly lie under, with respect to what is called the *length of the electric spark*. When a jar is discharged, it may be imagined, that a body of fire is seen extending from the inside to the outside; whereas it is pretty certain, that that appearance is occasioned by the very rapid motion of a single ball of fire; in the same manner as a lighted torch, with no greater motion than a man's arm can give to it, will seem to make an entire circle of fire. That the fire of an electrical explosion consists of a ball, or cylinder, of no great length, seems pretty evident from one of the experiments with the circles, p. 630, in which the diameter of the circle was the same,

whether the explosion was taken at the distance of half an inch, or of two inches; and also from the experiments of its passage over surfaces, in which it was sometimes made twenty times longer than usual, without any sensible diminution of its thickness.

XIII. OBSERVATIONS ON THE SMALL WIRES THAT COLLECT ELECTRICITY FROM THE EXCITED GLOBE.

HAVING made use of several brass wires, about two inches and a half long, to collect the electric matter from my globe; I observed, after a month or two, that about half an inch of the ends of them, which touched the globe, had contracted a blackness, particularly on the side which lay next the globe. I then took them off the ring to which they had hung, and rubbing them carefully, observed, that the same friction which made the rest of the wire quite bright, made but little alteration in this acquired blackness. Recollecting, at the same time, S. Beccaria's theory of magnetism, instead of replacing the wires, I hung two very fine needles in their place; and December the 20th. after about two months, in which I had made the most use of the machine, I examined them, and found that blackness at their points, but could not be sure that they had acquired any degree of magnetism. They had, indeed, a very small degree; but I had not examined them so very accurately before I hung them on, as I did afterwards. The experiment deserves to be repeated with more care, but it requires a longer and more constant use of an electrical machine than, it is probable, I shall ever have an opportunity of employing.

XIV

XIV. EXPERIMENTS INTENDED TO ASCERTAIN THE DIFFERENCE IN THE CONDUCTING POWER OF DIFFERENT METALS.

IN a conversation I once had with Dr. Franklin, Mr. Canton, and Mr. Price, I remember asking whether it was probable that there was any difference in the conducting power of different metals; and if there was, whether it was possible to ascertain that difference? I have since endeavoured to carry the scheme into execution, by transmitting the same explosion of the battery through two wires at a time, of two different metals, and of the same thickness. They were hooked one to the other, and held fast in hand vices, after they were measured with a pair of compasses to exactly the same length. The experiments were much more pleasing and satisfactory than I expected, but the result by no means corresponded to my ideas *a priori*.

I FIRST joined a piece of iron wire and a piece of copper wire. The explosion totally dispersed the iron, and left the copper untouched. The brass likewise disappeared when joined with the copper, and the iron when joined with the brass.

So far the experiments were extremely easy; a single charge of the battery sufficing to determine the difference between any two; but when I came to compare the more perfect metals, I found much more difficulty, and was obliged to try four or five charges of the battery upon every two: for, their conducting powers being nearly the same, I either made the charge too high, and dispersed them both; or too low, and touched neither of them. At length, I happened to hit upon such charges, that the copper vanished, and left both the silver and the gold;

and the gold remained when the silver was dispersed. The hook, however, of the silver was melted off when the copper was dispersed, and the hook of the gold when the silver was dispersed: for the heat is always the greatest where the electric fire passes from one body to another. Before the dispersion both of the copper and the silver, I had made explosions of such a strength, as though too small to melt them, gave them a bluish tinge.

FROM these experiments it is easy to settle the order in which the metals above-mentioned are to be ranked, with respect to the power of electricity to melt them. It is as follows. *Iron, brass, copper, silver, gold.*

NOT being able to get *lead* or *tin* drawn into wires, I got pieces of those metals rolled into plates equally thin, and taking small slips, of equal length and breadth, I transmitted the explosion through them; when the lead gave way the first. I intended to have compared these plates with others of iron, brass, &c. but had not an opportunity. I have little doubt but that tin would melt before iron; though indeed I had expected that tin would have melted before lead, and gold before silver. But according to Mr. Wilcke's experiments, p. 316, lead is a worse conductor than any of the other metals. My own experiments on the circular spots, p. 626 made me expect that gold would have melted before silver.

IT is very remarkable, that when iron wire is melted by the electric explosion, *bright sparks* are generally dispersed about the room, in all directions; but that they are seldom, or never seen when wire of any other metal is used. If but a small residuum of a battery be taken between two iron rods, when the explosion is extremely little, a great number of small sparks will fly in all directions from the iron, to the distance of about an inch, and exhibit

exhibit a beautiful appearance. Fewer of these sparks will be seen if one of the rods be brass, and, I think, none, in these small discharges, if they both be brass.

BEFORE any use can be made of these experiments, to determine the relative conducting power of the several metals, the order in which they melt with common heat should be compared with the order in which they melt with the electrical explosion; and I could not readily find a table of that kind. I think, however, that iron is said to require more heat to bring it into a state of fusion than any other metal, and it requires but a small force of electricity to do it, so that I think these two orders are not the same. Before this matter can be settled, it should likewise be found, how much more easily any of the metals will be melted before another, by transmitting shocks through wires of different lengths and thicknesses, which would be a very tedious business. I make no doubt but that an explosion which melts a copper wire of any given diameter would disperse an iron wire of twice the diameter, so that copper would be a much greater security, as a conductor to guard a building from lightning than iron, besides its being less liable to rust; but then it is more expensive.

XV. EXPERIMENTS WITH AN ELECTRIFIED CUP.

I SHALL close the account of my experiments with a small set, in which, as well as in the last, I have little to boast besides the honour of following the instructions of Dr. Franklin. He informed me, that he had found cork balls to be wholly unaffected by the electricity of a metal cup, within which they were held; and he desired me to repeat and ascertain the fact, giving me leave to make it public.

ACCORD-

ACCORDINGLY, December the 21st. I electrified a tin quart vessel, standing upon a stool of baked wood; and observed, that a pair of pith balls, insulated by being fastened to the end of a stick of glass, and hanging entirely within the cup, so that no part of the threads were above the mouth of it, remained just where they were placed, without being in the least affected by the electricity; but that, if a finger, or any conducting substance communicating with the earth, touched them, or was even presented towards them, near the mouth of the cup, they immediately separated, being attracted to the sides; as they also were in raising them up, the moment that the threads appeared above the mouth of the cup.

If the balls had hung in the cup a considerable time without touching it, and they were taken out immediately after the electricity of the cup was discharged, they were found to have acquired no degree of electricity.

If they had touched any part of the cup, though they showed no electricity while they were within it; yet, upon being taken out, they appeared to have acquired some; which was more if they had touched a part near the edge of the cup, less if they had touched any part more remote from the edge, and least of all if they had touched the bottom only. If they had first touched the side near the top, and then the bottom, they came out with that small degree of electricity which they would have acquired, if they had touched the bottom only.

In any case, if the balls were taken out while the cup remained electrified, they necessarily acquired some degree of electricity, in passing the mouth of the cup.

To pursue this experiment a little further, I took a small coated phial, such as is represented upon the stool [c Pl. ii.] and observed, that when I held it by the wire, within the elec-

trified

trified cup, it acquired no charge, the electricity of the cup affecting both the inside and outside coating alike. If the external coating touched the bottom of the cup, the phial received a very small charge. If it was made to touch the side, it acquired a greater charge; and the nearer to the top it was held, the higher charge it received; the wire of the phial, which communicated with the inside coating, being further removed from the influence of the electricity of the cup.

MAY we not infer from this experiment, that the attraction of electricity is subject to the same laws with that of gravitation, and is therefore according to the squares of the distances; since it is easily demonstrated, that were the earth in the form of a shell, a body in the inside of it would not be attracted to one side more than other.

DO TH it not follow from the experiments of the balls, compared with those with the phial, that no body can receive electricity in one place, unless an opportunity be given for its parting with it in another; at least, that a quantity must be repelled from any particular part before any more can enter; since a small body can no more receive electricity when all its sides are equally exposed to the action of an electrified body, than a phial can be charged when both its coatings are equally exposed to the same electricity?

Do not these experiments, likewise, favour the hypothesis of S. Beccaria, that there is no electrical attraction without a communication of electricity?

MR. LULLIN also made several of these experiments with an electrified hollow vessel. He observed, that if an electrified cork ball, suspended by a silken string, was let down into the vessel, and touched the bottom, it left all its electricity behind it. He also made the experiment with a glass vessel, coated at the bot-
tom

tom and charged, with the same event. It also made no difference whatever was the form, or size of the body let down into it, provided it was one third less than the depth of the vessel; but if, when it touched the bottom, it, at the same time, reached the top, or came near the top of the vessel, it acquired electricity; and a considerable degree, if it exceeded the top. The form of the vessel made no difference in his experiments, nor did it make any whether the vessel was entire or perforated. A wire net answered perfectly well. These experiments, Mr. Lullin imagines, clearly prove Nollet's doctrine of the constant motion of electric atmospheres: for he thinks, that this free motion on which electrification depends, is prevented from taking place within the vessel, by the contrary tendency of the opposite sides. *

* *Dissertatio physica*, p. 38.

A CATALOGUE OF BOOKS WRITTEN ON THE SUBJECT OF ELECTRICITY, EXCLUSIVE OF PAPERS IN BOOKS OF PHILOSOPHICAL TRANSACTIONS, AND OTHER MISCELLANEOUS WORKS; DISTINGUISHING [BY ASTERISMS] THOSE WHICH THE AUTHOR HAD SEEN, AND MADE USE OF IN COMPILING THIS WORK.

- * **GILBERT** de Magnete Magneticisque corporibus, 1600, London, folio.
 * *Oris de Garrick's* experimenta nova Magdeburgica, 1672, Amsterdam, folio.
 * *Herschel's* physico mechanical experiments, 1709, 1710, London, octavo.
 G. M. *Boni's* oratio inauguralis de electricitate, 2 parts, 1738, Wittenburg, Gralath's bibliothek.
 J. *Mortonia's* dissertatio de electricitate, 1740, 1742, Upsal, quarto, Gralath's electriche bibliothek.
 * *Defagulier's* dissertation concerning electricity, 1742, London.
 C. A. *Hauseni* novi prosectus in historia electricitatis, Leipzig, 1743.
 C. G. *Krautzenstein's* abhandlung von der nutzen der electricitat in der artzney wissenschaft, 2d. edi. 1745, Halle. Gralath's bibliothek.
 A. G. *Rosenberg's* versuch einer erklärung von den ursachen der electricitat, 1745, Breslau Gralath's bibliothek.
Nallet's conjectures sur les causes de l'electricité des corps, Paris, 1745. Wilson, p. 12.
 ----- essai sur l'electricité des corps, 1746, 1754. Paris.
 * *Walze's* abhandlung von der electricitat und deren ursachen, together with two other essays in high Dutch, and one in French, all written for the prize proposed by the academy at Berlin, 1745, Berlin, quarto.
 * *Wilson's* essay towards an explication of the phenomena of electricity deduced from the ether of Sir Isaac Newton, 1746, London, octavo.
 * *Watson's* experiments and observations, tending to illustrate the nature and properties of electricity, 3d. edi. 1746, London, octavo.
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 * *Frick's* essay to show the cause of electricity, 1746, London, octavo.
Krautzenstein's theoria electricitatis, more geometrico explicata, Hall, 1746.
 J. *Piderit's* dissertatio inauguralis de electricitate, 1745, Marburgh. Gralath's bibliothek.
 * *Martin's* essay on electricity, 1748, Bath, octavo.
Boni's recherches sur la cause, et sur la véritable theorie de l'electricité 1746.
 Congettura sifiche intorno i fenomeni della machina elettrica, Rome, 1746, octavo, Hist. p. 168.
Franzisco Pisoni's lettere della elettricità medica, Venice, 1747.
Boni's tentamina electrica, Wittenburg, 1747.
 * *Giambattista Favri's* congettura sifiche intorno alle cagioni de' feromeni osservati in Rome nella machina elettrica, 1747, Rome, quarto.
 * *Rackstrow's* miscellaneous observations, together with a collection of experiments on electricity, 1748, London, octavo.
 * *Watson's* account of experiments, made to discover whether the electric power would be sensible at great distances, &c. 1748, London, octavo.
 Recueil des traités sur l'electricité, traduits de l'alemand et de l'anglois, Paris, 1748, octavo, 3 vols.
 * *Nallet's* recherches sur les causes particulieres des phenomenes electriques, 1749, Paris, twelves.
 * *Jallabert's* experiences sur l'electricité avec quelques conjectures sur la cause de ses effets. 1749. Paris, twelves.
 A latin dissertation on electricity by P. *Francois Plata*, a jesuit of Palermo, 1749. Hilloise.
 * *Wilson's* treatise on electricity, 1750, 1752, London, octavo. N. B. This book is quoted in this work by the title of *Wilson's* Essay.
 * *Secander's* histoire de l'electricité in his Observations Physiques, 1750, Paris, twelves.
 * *Boulanger's*

- * *Boulanger's traité de la cause et des phénomènes de l'électricité*, 1750, Paris, twelves.
N. B. *I have seen no more than one part of this treatise, which I had of Dr. Watson. How many more parts there are of the whole work, I have not been informed.*
- Jes. Veratti observations sur l'électricité, aux quelles on a joint les expériences faites à Montpellier pour guérir les paralytiques au moyen de l'électricité*, à la Haye, 1750, twelves.
- Electricorum effectuum explicatio*, by *Father Binn*, 1751.
- * *Histoire générale et particulière de l'électricité*, 1752, twelves.
- J. H. Winckler's progr. de avertendi fulminis artificio ex doctrina electricitatis*, Leipzig, 1753.
- * *Giambattista Beccaria dell' elettricismo artificiale e naturale*, 1753, Turin, quarto.
- S. H. Quelmaix dissertatio de viribus electricis medicis*, Leipzig, 1753, quarto.
- * *Franklin's new experiments and observations on electricity, made at Philadelphia in America*, part. 1. 3d. ed. 1760.
----- part. 2. 2d. ed. 1754.
----- part. 3. 1754. London, quarto.
----- New edition with additions 1769.
- Brevis relatio de electricitate propria lignorum*, auctore *P. Wundelino Ammersia De Lucerne Helvetiorum*. Ord. Minim. St. Francisci de P. Conventual 1755. Noller's letters, vol. 2. p. 235.
- J. B. Landriani dissertatio de nova electricitatis theoria*. Milan. 1755. Wilcke, 12.
- J. A. Euleri, disquisitio de causa physica electricitatis*. ab academia scientiarum imperiali Petropolitana premio coronata, Petersburg, 1755. Wilcke, 12.
- * *Dalibard's histoire abrégée de l'électricité*, and French version of Franklin's letters, 1766, Paris, 2 vols. twelves.
- * *Levet's subtile medium proved*, 1756. London, octavo.
- * *David Gralath's geschichte der electricität in the memoirs of the society at Dantzick*, 1747, 1754, and 1756. Danzig, quarto. N. B. Vol. 1st. 2d. and 3d. of *this work*, in the marginal references mean the volumes of the work in which it is contained.
- * *Elektrische bibliothek*. in the Dantzick memoirs for 1754, and 1756.
- * *Johannes Carolus Wilcke's disputatio physica experimentalis de electricitatus contrariis*, 1757, Rostock, quarto. N. B. *This admirable treatise consists of four parts, but, to my great regret, the copy which I had contained only the three first of them. It was that which the author sent to Dr. Franklin, before the remainder was printed.*
- Busebian dissertatio de fulgure et tonitru ex phenomenis electricis* Göttingen, 1757, quarto.
- * *Giambattista Beccaria's lettere dell' elettricismo*, 1758. Bologna, folio.
- * *J. C. Wilcke's hernn Franklin's briefe von der electricität nebst anmerkungen*, 1758, Leipzig, twelves.
- * *Wilson and Hoadley's observations on a series of electrical experiments*, 2d. ed. 1759. London, quarto.
- * *Apian's tentamen theorie electricitatis et magnetismi*, 1759. Petersburg, quarto.
- * *J. F. Hartmann's abhandlung von der verwandtschaft and ähnlichkeit der elektrischen kraft mit der erforschelichen luft erscheinungen*, von 1759, Hanover, octavo.
- * *Noller's lettres sur l'électricité*, tome 1. 1749, tome 2. 1760, tome 3. 1767, Paris, twelves.
- * *De Tour's recherches sur les differens mouvemens de la matiere électrique*, 1760, Paris, twelves.
- * *Wolff's desideratum, or electricity made plain and useful*, 1760, London, twelves.
- Apian's on the tourmalin*, 1762.
- * *Noller's Leçons de physique*, tome, 6th. 1764, Paris, twelves.
- * *J. F. Hartmann's anmerkungen über die nothigte achtsamkeit bey erforschung der gewetter electricitatz*, &c. 1764. Hanover, quarto.
- * *Johannes Franciscus Cigna de novis quibusdam experimentis electricis*, from the memoirs of the academy of Turin for the year 1765, quarto.
- * *Levet's philosophical essays in three parts*, 1765. London, octavo.

* J. F.

- *J. F. Hartmann's* electriche experimente in lustleeren raume, 1766, Hanover, twelves.
- *Anadeus Lullia's* dissertatio physica de electricitate, 1766, Geneva, octavo.

OTHER BOOKS, THE DATES OF WHICH I HAVE NOT FOUND.

- Winkler's* essay sur la nature, la cause, et les effects de l'electricité. Histoire, p. 33.
Boze's poem on electricity. Histoire.
 Observations sur l'electricité par Mr.—chirurgien de la salpêtre. Hist. p. 98.
 Nouvelle dissertation sur l'electricité par un physicien de Chartres. Histoire.
Mr. Krager's meditations on electricity. Hall.
 Tentamen de vi electrica ejusque phenomenis, auctore Nic. Baumgarten.
Laurentii Berand S. J. theoria electricitatis, Petersburg. Wilcke, p. 12.
Watkins on electricity.

As the reader will see by the asterisks what books I have had an opportunity of perusing, he will see in what parts my history is most likely to be defective. And I shall think myself greatly obliged to any person, who will favour me with the use of any treatise which contains a discovery of importance. But I do not apprehend that any thing very material can have escaped me.

A N

INDEX of THINGS.

- A**FFLUX, and efflux of electric matter, 91, 112, 137.
- Air*, condensed, obstructing excitation, 50, 550. ranked among electrics per se, 66. current of, from electrified points, 113, 115, 559. electrified, 194. displaced by the electric spark, 204. charged and giving a shock, 242. effects of its admission into a vacuum with respect to light, 290. mephitic, experiments concerning it, 566. inflammable, no conductor, 569.
- Amalgam*, promotes excitation, 508.
- Amber*, known to be electrical, 1.
- Anatomy*, the knowledge of it useful to an electrician, 474.
- Animals*, their hair excited by rubbing, 46, 71, 124, 126. killed by electric explosions, 98, 169. effects of electrification on them, 136. their fluids conduct better than water, 201. their muscles how affected by the electric spark, 402. various experiments on them, 618.
- Apparatus*, electrical, improvements in it, 185.
- Atmosphere*, electricity in it, 341. antient electrical appearances in it, 371.
- *electrical*, affections of bodies immersed in them, 9, 118, 232, &c. not excluding the air, 176, 247. their immobility by external force, 177. made visible, 177, 22. their existence denied, 244.
- Attraction, electrical*, experiments made on it, 124, 522.
- *and repulsion* one sphere within another, 401.
- *entertaining* experiments with them, 522, 524.
- Aurora borealis*, an electrical phenomenon, 332, 353.
- B**ALLS, Mr. Canton's, 491.
- Barometer*, luminous, 73.
- Battery*, electrical, 86, 159. directions concerning them, 488.
- Bearfication*, an experiment so called, 148.
- Bells*, electrical, 73, 523.
- Blindness*, occasioned by electricity, 169.
- Bridel*, St. steeple, demolished by lightning, 377.
- C**ALCES, of metals, non-conductors, 224.
- Capillary tubes*, the electrification of water flowing through them, 133.
- Car*, electrified by friction, 46, 187.
- Cabin*, experiments with explosions through one, 636.
- Charcoal*, proved to be a conductor, 570.
- Charge*, electrical, retained a considerable time, 98. measured, 129. given to the outside of a phial, 156. shown to lie in the glass itself, 159, 160. given to various electrical substances, 270. given by means of a plate of lead, 273.
- Charging*, and discharging, various experiments concerning them, 608.
- Crystal*, island, how excited, 228, &c.
- Chymistry*, useful to an electrician, 474.
- Circuit*, for the electrical explosion, a number of them made, 96. whether it retain any electricity after a discharge, 273. curvature in it not retarding the force of the explosion, 688. of great length do retard it, 689. effects of interceptions in them, 695.
- Circular spots*, made by electrical explosions, 623.
- Clouds*, how they acquire electricity, 310.
- Coating* of phials invented, 89. directions concerning it, 487.
- Cobolton*, electrical, its power, 252, 265, 275.
- Colour*, its influence in electricity, 35, 44, 57. of red substances changed into white by the electric

INDEX OF THINGS.

- electric spark, 75. of the electric spark itself, 77, 150, 406, 704. occasioning a change of the two electricities, 217, 250. prismatic, made by explosions on metal, 675.
- Conductors* first used in a technical sense, 63. their different conducting power, 119, 583.
- to secure buildings, &c. from lightning, their utility and construction, 370, 380.
- *prime* (see afterwards)
- Conspirators*, an experiment so called, 534.
- Contacts*, bodies brought into it not without a considerable weight, 64, 702.
- Cups*, metallic, electrified, 709.
- Cylinders*, used instead of globes, 69, 481. of solid glass excited, 23. of wood used instead of glass 187.
- D**ESIDERATA in electricity, 451.
- Dew* attracted by electric substances, 50.
- Diffusion* of electricity on the surface of new glass, 588.
- Discharges*, spontaneous, its phenomena, 277.
- Diseases* cured by electricity, 381. palsy, 384, 394. tetanus, 386. deafness, 390. various pains, 390. gutta serena, 391. ague, 392. hysterical cases, 393. St. Viter's dance, 394. in what cases hurtful, 395.
- Days*, black, made by explosions through metals, 636.
- E**ARTH, dry, a non-conductor, 224.
- Earthquake*, its connection with electricity, 359. imitated, 650.
- Eel*, trembling, of Guiana, its electric properties, 404.
- Effluvia* of various bodies, their conducting power, 578.
- Electricity*, derivation of the term, 2. acting through glass, 12, 40, 77. the direction in which it is exerted, 15. the communication of it to non-electric bodies, 27. communicated to large surfaces, 32. to animal bodies, 32, 42. in proportion to the surfaces of bodies, 33. conveyed to great distances, 28, 45. thought to be perpetual in some bodies, 38. made very strong, 70. collected, not produced by friction, 73, 110, 113, 115, 153. conveyed through the substance of conductors, 114, 125. communicated to hot iron, 116. difference between excited and communicated, 121. increase of its force, 127. spontaneous, 219. how produced in the clouds, 330.
- Electricity*, the two, discovered, 47, 154, 178. Mr. Canton's experiments concerning them, 208. Mr. Wilson's, 212. Mr. Bergman's, 217. Mr. Symmer's, 250. Mr. Cigna's, 262. repelling one another, ib. distinguished, which positive and which negative, 272, 287, 512.
- Electrics* distinguished from non-electrics, 23. various substances found to be so, 3, 5, 25. classed according to their powers, 122. whether they contain much of the electric fluid, 696.
- Electrification*, effects of it on various properties of bodies, 133. constant, proposed in some diseases, 396. of persons, amusing, 539.
- Electrometers*, several described, 129, 194. Mr. Kinnersley's, 204, 491. Mr. Lane's, 491. Mr. Canton's, ib.
- Evaporation* of fluids promoted by electrification, 131. whether it contribute to the generation of electricity, 700.
- Excitation* promoted by waxed cloth and oil, 71. by moisture, 127, 128. by oil of turpentine and a woollen cloth, 129. by silk prepared with linseed oil, 184. by amalgam, 186. obstructed by wetting the globe or tube on the inside, 157. directions concerning it, 507. original experiments concerning it, 550.
- Experiments* succeeding remarkably well in a room crowded with company, 120. entertaining ones, 518. original, of the author, 543.
- F**AIRY RINGS, made by lightning, 634.
- Fermentation*, whether it contributes to produce electricity, 700.
- Fire*, not affecting electricity, 78.
- Fish*, electrical, 522.
- Flame* destroying electricity, 79.
- Flourish* flashes, experiments with them, 613.
- Fluid*, electric, conducting substances into its path, 278, 601. endeavouring to move in a right line, 401.
- Freezing*, whether affected by electricity, 700.
- Frisians*, in what degree useful, 3, 23, 581. of bodies producing electricity, 220.
- G**ARMENTS, observed to give electric light, 125.
- Gems* found to be electric, 3. similar to the tourmalin, 307.
- Glass* found to be electric, 3, 6. different in the electric power of different kinds, 117, 127, 129, 188. whether permeable to electricity, 244, 397. charged, and hermetically sealed, 229, 701. red hot, a conductor, 577. rough used to electrify negatively, 187, 209.
- Globes*, of sulphur, 8, 23. of glass, 18. of seal-

- ing wax, 22. made blue with zaffre, 70. a number of them whirled at the same time, 78. lined with electrics, 507. burbling during excitation, 190. large ones, difficult to excite, 535. directions concerning them, 481.
- Gymnasia*, its electrical properties, 402.
- H** ALL, its relation to electricity, 349.
- Heat*, hurtful to electricity, 44, 119. makes glass, and other electrics conductors, 115, 226.
- Horse-race*, electrical, 526.
- Hurricane*, an electrical phenomenon, 355.
- I** CE, whether it be a conductor, 125, 206. 576.
- Ignis fatuus*, an electrical phenomenon, 370.
- Jack*, electrical, 535.
- Jars*, directions concerning them, 488.
- Jet*, known to be electric, 2.
- K** ITE, to draw electricity from the clouds, 493.
- L** EYDEN phial discovered, 80. its wire and coating giving sparks alternately, 96. the outside giving sparks, 99. the different electricities of its two sides, 155, 272. several of them charged with the same fire, 158. analyzed, 159. entertaining experiments with it, 531.
- Light*, electric, observed, 7, 9, 10, 17, &c. 74. visible through sealing wax, 21, 278. a cone, or pencil of it, 54. visible in vacuo, 158, 282. its return upon withdrawing an excited tube, 286. produced by the friction of pieces of glass, 290. in the form of a cone between two glass balls, 291. produced by the dashing of air against glass, 294. forming a beautiful atmosphere round brass balls in vacuo, *ib.* extremely penetrating, *ib.* entertaining experiments with it, 527. imbibed by bodies, 295.
- Lightning* and electricity compared, 12, 56, 162, 166, &c. drawn from the clouds by pointed bodies, 170, 309, &c. method of securing buildings from its effects, 173. positive and negative, 314. their proportion to one another, 316. brought down in great quantities by a kite, 323. fatal to Mr. Richman, 337. how it kills animals, 604. killing persons alternately, 633.
- Liquefaction* producing electricity, 219.
- Liquors*, effects of explosions transmitted through them, 703.
- Lycopodium*, probably, the tourmalin, 2, 297.
- M** ACHINES, electrical, their construction, 497. Mr. Hawkebee's, 495. Nollet's, 496. Mr. Wilson's, 497. one made by Mr. Nairne, 498. Mr. Read's 499. Dr. Ingenhous's, 500. two, of the authors construction, *ib.* for rubbing tubes, 69. for drawing electricity from the clouds, 492.
- Mechanics* useful to an electrician, 475.
- Medicated tubes*, experiments with them, 141. refuted, 146.
- Medium subtilis*, surrounding all bodies, 94.
- Magnetism* and electricity compared, 5, 6, 406. given by electrical explosions, 169. how communicated to iron, 331, 706.
- Metals* melted by electrical explosions, 167, 276. tinged by the electric spark, 78. not perfect conductors, 201, 691. revived by electricity, 277. their fusion by lightning, not a cold one, 316. small globules of them made by electrical explosions, 615, calcined by electricity, 647. their different conducting powers, 707.
- Momentary* of an explosion endeavoured to be ascertained, 688. not diminished by angles in the circuit, *ib.*
- Musical tone of explosions*, 696.
- N** EGATIVE electricity, produced by insulating the rubber, 198.
- O** IL, no conductor, 199, 580.
- P** APER, a quire pierced with the electric explosion, 257, &c.
- Phial*, broken by a spontaneous discharge, 87, 99.
- Pencil*, of light, thrown out by an excited tube, 291. and *flar* distinguished, 293. of rays, 75.
- Perspectives*, the practice of it recommended, 477.
- Perspiration*, insensible, promoted by electrification, 137.
- Phosphorus* kindled by electricity, 75. its effects when put upon pointed bodies, 292.
- Picture* magical, 533.
- Planetarium* electrical, 525.
- Plants* electrified, 135, 140.
- Plate of glass* charged, 90.
- Points*, electrification more sensible at them, 139. their power in drawing and throwing off electric fire, 164. entertaining experiments with them, 404. experiments with them when concealed, 405.
- Pressure* of, bodies together, and their separation producing electricity, 405.

INDEX OF THINGS.

Prime conductors, origin of metallic ones, 53. first used with globes, 68. directions concerning them, 486, 510.

Propositors comprizing electrical knowledge, 64, 409.

Pulse quickened by electrification, 131, 137.

QUERIES and hints for promoting discoveries in electricity, 458.

RAIN, its relation to electricity, 346.

Repulsive, electrical, observed, 7, 9, 47.

Refluxum of a charge, 87.

Revolution of bodies thought to be produced by electricity, 58, &c.

Ribbons, Mr. Cigna's experiments with them, 262.

Rings, coloured, observed between two charged plates of glass, 236. made by explosions, 675.

Rods found to be electric, 3.

Rubber, or cushion, introduced, 69. directions concerning it, 485.

SHOCK, or explosion, the first effects of it, 83. given to a number of persons, 86, 98. transmitted through ponds and rivers, 87, 102.

stronger when the glass is thin and warm, 87. in proportion to the points of contact with a non-electric coating, 88. goes *rediffusis carnis*, 89. imperfectly conducted, 91, 160.

given to particular parts of the body, 93. law of its increase, ib. affecting bodies without the circuit, 95. convulsing the muscles, ib. given by an exhausted phial, 97. transmitted to a great distance, 100, &c. its velocity attempted to be ascertained, 101, 106, &c. its progressive motion, 271. its direction, 272. not affecting some persons, 279.

rules concerning it, 513. a new method of giving it, 594. passing over the surface of bodies, 648. its lateral force, 681. various experiments of its force, 687.

Silk shown to be electric, 26. and a non-conductor, 31.

Smoothness, or roughness, occasioning a change in the electricity, 209, 219.

Snow, its connection with electricity, 350.

Sound made by electrical experiments, 22.

Spark, electric, first observed to come from a

human body, 45. firing inflammable substances, 71, 76, 116. entertaining experiments with it, 529. taken through pieces of metal, 694. a deception with respect to it, 695.

Spider, electrical, 523.

Scar, electrical, 73. a falling one, an electrical phenomenon, 351.

Steam, no conductor of electricity, 204.

Stockings, Mr Symmer's experiments with them, 250.

Stout Portland, the difference of its conducting powers depending upon heat, 224.

Sulphur, found to be electric, 3. used in the form of a globe, 8, 180.

THEORIES, Mr. Boyle's, 8. Newton's, 14. Mr. Delaval's, 224. Mr. Symmer's, 257. a general account of them, 417. Mr. Du Fay's, 422. Mr. Boulanger's, 422. Mr. Wilson's, 425. Nollet's, 426. Du Tours, 429. of positive and negative electricity, 429. of two electric fluids, 441.

Tings, metallic, given to the surface of glass, 698.

Teumalin, the lycuicism of the ancients, 297. its electric property discovered, 298. the Duc de Noya's experiments on it, 298.

Epinus's, 299. Mr. Wilson's, 300, 307. Mr. Canton's, 303. the authors, 659.

Texas, brasilian, its electric properties, 306.

Tubes, of glass, directions concerning them, 481.

VACUO, effects of electricity in it, 6, 19, 40. a cylinder rubbed in it, 72, 171. entertaining experiments in it, 539.

Velocity, what degree is proper to be given to a globe, 190.

WARMTH, favourable to electrical experiments, 6, 26, 43.

Water electrified, 4, 36, 57, 66. an imperfect conductor, 198.

Water-spout, an electrical phenomenon, 355.

Wax, white, found to be electric, 5.

Weather proper for electrical experiments, 3.

Weight of bodies not affected by electrification, 73.

Wheel, electrical, 534. a self moving one, 535.

Wood, baked, excited, and whirled in a machine instead of glass, 186.

INDEX of NAMES.

ÆPINUS, 222, 240, 273, 299, 400,
401, 406, 435.
Alhmand, 73, 83.
Ammerlin, Windelinus, 186.

BACON, Sir Francis, 5.
Bartholin, 126.
Boyle, 5, &c.
Beccaria Signior, 7, 187, 196, 254, 270, 277,
293, 320, &c. 345, &c. 354, 355, 368, 381,
401, 601.
Beraud, 191.
Bergen, Carolus Augustus Van, 164.
Bergman, Thorbern, 188, 206, 217, 230, 354,
400.
Bevis, Dr. 89, 113, 314.
Bianchi, 141.
Bianchini, 148.
Binon, 173.
Bohadrch, 385.
Boulanger, 120, 129, 191, 423.
Boze, 2, 68, 72, 84, 127, 135, 148, 191, 584.
Brown, Sir Thomas, 2.
Brydome, Mr. 385.

CABÆUS, Nicolaus, 5.
Canton, Mr. 15, 49, 96, 115, 129, 185,
194, 209, 227, &c. 232, 246, 276, 279, 285,
289, 295, 303, 314, 354, 469, 491, 540,
563, 647, 679.
Cat, Mr. Lc. 124.
Chalmers, Mr. 352.
Cigna, Johannes Franciscus, 216, 261, 273,
449, 583.
Clayton, Mr. 126.
Coke, Mr. 125.
Cunzeus, 80.

DALIBARD, 153, 183, 277, 309.
Darwin, Dr. 203, 247.
Delaval, Mr. 223, &c. 380.
Delor, Mr. 309.
Defagoliers, Dr. 625, &c.
Digby, Kenelm, 2.

ELLICOTT, Mr. 129, 138.
Epinaffe, Mr. L', 507, 693.

FAYE, Mr. Du, 43, &c. 422.
Franklin, Dr. 15, 49, 151, &c. 276, 375,
387, 429, 541, 551, 561, 569.

GASSENDUS, 2.
Gilbert, 2.
Gordon, 69, 124, 135.
Graham, Mr. George, 96.
Gralath, Mr. 81, 88, 130.
Grey, Stephen, 25, &c. 53, &c. 62.
Grammert, 74.
Guericke, Otto, 8, &c.

HAEN Antonius de, 394.
Hales, Dr. 150, 367.
Hamilton, Mr. 404.
Hart, Dr. 385.
Hartman, 18, 271, 296, 368, 494.
Haufen, 68.
Hawkebee, 15, &c. 495.
Heberden, Dr. 300, 659.

JALLABERT, 87, 99, 125, 140, 164, 384.
Jellop, Mr. 634.
Jugenhaus, Dr. 500.

KINNERSLEY,

I N D E X O F N A M E S .

- K**INNERSLEY, Mr. 49, 178, 204, 276, 318, 375, 405, 491, 533.
 Kleist, Von, 81.
 Klingfierna, 125.
 Knight, Dr. 316.
 Kratzenstein, 383.
 Krugar, 74.
- L**ANE, Mr. 294, 499, 573, 642.
 Lechman, 299.
 Lemery, 298.
 Lieberkuhn, 81.
 Linnæus, 395.
 Lovet, Mr. 390.
 Ludolf, 72, 73.
 Lullen, 199, 272, 280, 330, 406, 711.
- M**AIMBRAY, Mr. 135.
 Mazeas, 312, 341, 389.
 Miles, Dr. 75, 117, 169.
 Monnier, 83, 97, 101, 114, 127, 312, 341.
 Mortimer, Dr. 58.
 Maschenbroeck, 80, 118, 403.
- N**EEDHAM the Abbé, 120.
 Newton, Sir Isaac, 12, 675.
 Nollet the Abbé, 46, 50, 82, 97, 112, 118, 121, 129, 131, &c. 146, 162, 188, 336, 385, 426, 449, 496, 528.
 Noya, Duc De, 298.
- P**IVATI, 142, 144.
 Pliny, 2.
 Price, Mr. 406, 469, 634.
- R**ACKSTROW, Mr. 524.
 Ramfden, Mr. 500.
 Reaumur, 83.
 Read, Mr. 499.
- Richman, 66, 272, 337, &c.
 Roi, Mr. de, 187.
 Roman, Mr. 333.
- S**ABATELLI, 190.
 Sauvages, 384.
 Simpson, Dr. 126.
 Smeaton, Mr. 98, 116, 285.
 Solokow, 338.
 Speedler, 187.
 Spry, Dr. 386.
 Stroemz, 125.
 Stukeley, Dr. 359, &c.
 Symmer, Mr. 250, &c. 441.
- T**HALES, 1.
 Theophrastus, 1.
 Tour, Du, 79, 120, 428.
 Trembley, 131.
- V**AUDONIA, Alessandro Amadeo, 254.
 Verati, 141.
 Vilette, 405, 583.
- W**AITZ, 7, 79.
 Wall, Dr. 10.
 Watson, Dr. 2, 15, 75, 87, 101, &c. 110, &c. 127, 147, 186, 281, 371, 374, 376, 386, 429.
 Wesley, Mr. 392.
 Wheeler, Mr. Granville, 29, &c. 51.
 Wilcke, 175, 186, 219, 239, 277, 290, 357, 436, 465, 517.
 Willson, Mr. 93, &c. 114, 128, 168, 212, 287, 300, 314, 381, 395, 425, 497.
 Winckler, 69, 72, 84, 87, 143, 162, 277.
- Z**ETZEL, 392.

F I N I S .

DIRECTIONS TO THE BINDER FOR PLACING THE CUTS.

Plate 1. to face page 246.

- 2. _____ 479.
- 3. _____ 489.
- 4. _____ 495.
- 5. _____ 497.
- 6. _____ 499.
- 7. _____ 501.
- 8. _____ 503.

The Specimen of the CHART, to follow the Account of it at
the End of the Book.

UVA.BHSC





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