Antipoverty transfers and school attendance: Panama's *Red de Oportunidades*

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Purpose: The paper seeks to gauge the impact of the *Red de Oportunidades* programme on the school attendance of children from households that participate in the programme.

Design/methodology/approach: In order to measure the impact of the programme, we apply propensity score matching, a quasi-experimental technique that allows us to find an appropriate control group to compare with the treatment group.

Findings: Results show that the programme does not always manage to bring into line school attendance of children from families involved in the programme with that of children from families who are not. Nevertheless, differences are still evident in terms of age, gender and geographical area.

Practical implications: Conditional cash transfer programmes should be designed carefully, taking into account a great variety of factors such as geographical characteristics, educational resources and infrastructure, not only to replicate programmes that have proved to be effective in other countries. In this sense, it seems that the impact of cash transfers on primary school attendance can be wholly attributed to the programme, implying that it is better to allocate more resources to groups in terms of age and gender where education is still not universal.

Originality value: To the best of the authors' knowledge, this is the first time the impact of conditional cash transfers (CCTs) on school attendance has been examined in a country that still displays major geographical differences in terms of poverty, namely, Panama. The *Red de Oportunidades* programme has barely been studied.

Keywords: conditional cash transfers; school attendance; propensity score matching; *Red de Oportunidades*; Panama

1. Introduction

Education is included in the Universal Declaration of Human Rights. It is one of the three components of the Human Development Index, together with life expectancy and having a decent level of income (UNDP, 1990), and is key to improving people's quality of life. It is also the fourth Sustainable Development Goal (SDG), and proves crucial to achieving many other objectives. Education helps people escape the cycle of poverty (World Bank, 1990), and contributes to reducing inequalities as well as achieving gender equality. It is also vital in terms of fostering tolerance and creating societies that are more peaceful.

Although major strides have been made in recent years with regard to improving access to education at all levels, especially for girls (United Nations, 2019), progress has by no means been homogeneous, either between countries or between territories within the same country. Education policies and conditional cash transfer programmes could be behind some of the improvements to have taken place in this regard.

In Latin American and Caribbean countries, one of the most popular tools aimed at breaking the intergenerational transmission of poverty is conditional cash transfer (CCT) programmes, which were set up in the mid-1990s. The main advantage of CCTs is the ability to reduce both the level and impact of poverty (Fiszbein and Schady, 2009; Paes-Sousa *et al*., 2013), to promote education and to improve preventive health care in poor households (Rawlings and Rubio, 2003).

As regards education, CCTs programmes do not always guarantee the expected effect in all circumstances, due to several reasons. Some of these are associated with the programme parameters (focalization, benefit structure, conditionality or co-

responsibility, exit rules and recertification), others with institutional aspects (such as insufficient organizational capacity and public management systems), and in other cases with the existence of drawbacks and bottlenecks in their application (Handa and Davis, 2006; Schubert and Slater, 2006; De Brauw and Hoddinott, 2011).

With regard to the programme parameters, it is yet to be proven whether or not conditional transfers to households are sufficient to ensure an increase in school enrolment, although it does seem unlikely that a small cash transfer would encourage beneficiaries to send the children to school and thus invest in education (Handa and Davis, 2006; Bastagli *et al*., 2019). Moreover, conditionality on school attendance might not make much sense in middle-income countries, where education indicators are already satisfactory (García and Saavedra, 2017; Bastagli *et al*., 2019). As a result, conditioning transfers to obtaining better performance and enhanced learning outcomes might be considered as one possible option (Villatoro, 2008).

In relation to institutional aspects, it should be noted that CCT programmes influence demand but ignore supply-side issues. The unequal distribution and quality of access to education services, between rural and urban areas, is a constraint and a bottleneck to the success of these programmes. Without investment to ensure the provision of school services, conditionality is impractical. A further issue to be taken into account concerns problems of corruption and clientelism that can emerge as a result of cash transfers (Schubert and Slater, 2006). Finally, significant changes in beneficiaries' behaviour are only likely to occur if there are sufficient investments.

In order to gain an insight into how this kind of programme might improve education in poor households, this paper seeks to analyse the effect of the *Red de Oportunidades* programme (*RdO*) on school attendance in Panama.

The literature exploring the effects of CCTs on education has yielded mixed results,

partly because the outcome variable for measuring impact varies considerably (García and Saavedra, 2017). Whereas certain studies focus on school enrolment rates (Schultz, 2004; Attanasio *et al*., 2005; Oosterbeek *et al*., 2008; Schady and Araujo, 2008; Arraiz and Rozo, 2011; Behrman *et al*., 2012; Glewwe and Kassouf, 2012; Schaffland, 2012; De Brauw *et al*., 2015), others consider school attendance (Skoufias and Parker, 2001; Cardoso and Portela, 2004; Attanasio *et al*., 2005; Hermeto, 2009; Borraz and González, 2009; Filmer and Schady, 2011; Behrman *et al*., 2012; Martínez, 2012; Schaffland, 2012). Other studies have centred on more long-term effects, focusing on indicators such as school performance or educational paths which, although desirable, are not among these programmes' specific objectives. Broadly speaking, CCTs have been shown to have a positive, albeit varying degree of success, on school enrolment depending on the particular programme (Rawlings and Rubio, 2005). Given that beyond mere schooling, what may really have a positive influence vis-à-vis improving human capital is children from the more disadvantaged families regularly attending school, it would seem that gaining an understanding of how CCT programmes affect school attendance is of great interest. Yet here the findings prove less conclusive. Although a close relation between the two effects might be expected, studies jointly exploring the impact of school enrolment and attendance fail to provide evidence to support this. Such was the case of two of the most closely studied experiences in the first generation of CCT programmes: *Bolsa Escola* in Brazil and *Progresa/Oportunidades* in Mexico.

The *Bolsa Escola* programme in Brazil boosted school attendance by approximately three percentage points among both boys and girls from poor families (Cardoso and Portela, 2004). Schaffland (2012) concluded that the programme was successful in reducing the number of days children missed school. Hermeto (2009) found no

significant differences in attendance among children from families who were beneficiaries of the programme compared to those who were not. In the case of the *Progresa/Oportunidades* programme in Mexico, various studies report a positive effect both in enrolment and attendance (Skoufias and Parker, 2001; Behrman *et al*., 2005). In contrast, Martínez (2012) found that the programme failed to have any impact on school absenteeism since, prior to intervention, school indicators were already high in the areas where the study was carried out. Finally, other studies such as those by Attanasio *et al*. (2005) focusing on smaller-scale programmes at least in terms of the number of beneficiaries, report a positive effect on attendance of the *Familias en Acción* programme in Colombia, although only in the case of children aged between 12 and 17 (not so for younger children aged 8 to 11). Likewise, Borraz and González (2009) concluded that the *Ingreso Ciudadano* programme had no significant effect on attendance, either for children aged 8 to 11 or those aged between 12 and 14, in urban areas of Uruguay.

The literature has led to the conclusion that early results, which are mostly positive, need to be nuanced when it is possible to break down the impact into geographical areas or age groups. In particular, studies which take into consideration different age groups or educational levels support the idea that the programme helps to improve (or at least, to maintain) school attendance among younger children, although no appreciable changes are in evidence in school attendance for children aged between 6 and 11 who are in compulsory primary education (Parker *et al*., 2006).

Panama is characterised by the historical duality of the economy. Despite being one of the Latin American and Caribbean countries with the highest per capita gross domestic product (GDP), certain marginal areas that suffer appalling human conditions provide a stark contrast to the more dynamic areas. Over the last decade, Panama has reached

the highest mean economic growth in the region and has striven to improve institutions and to implement public policies (Him, 2017), yet the impact of the *RdO* programme has scarcely been subject to evaluation.

To the best of our knowledge, Arraiz and Rozo's (2011) contribution is the only study to explore the effect of CCTs on enrolment and child labour in rural and indigenous areas in Panama, based on the *Encuesta de Niveles de Vida* (Living Standard Survey, 2008), only two years after the programme was instigated. As a result, it seems appropriate to draw on more recent studies that span a greater number of years in which the programme has been operating.

This paper examines the impact of the *RdO* programme on school attendance among children aged 6 to 17, using the *Encuesta Continua de Hogares* (Continuous Household Survey, ECH), which has been conducted by the National Institute of Statistics and Census since 1963. Specifically, the work draws on data corresponding to 2010 and 2013, thereby affording a clearer insight into the programme's impact, given that it has been in force over a longer period and has been able to bring about changes in beneficiaries' behaviour. During this period, as can be seen from the analysis of the qualitative data, the programme's monitoring and evaluation system evidenced certain problems in coordinating with schools to verify compliance with co-responsibility in education (Him *et al*., 2019).

The rest of the paper is divided into six sections. The following section describes the education system in Panama. Section 3 sets out the main features of the *RdO* programme. Section 4 provides a brief description of the methodology. The data and how the technique is applied (propensity score matching) are explained in Section 5. Finally, the main results and the most relevant conclusions are presented in Sections 6 and 7, respectively.

2. Education in Panama

In order to contextualize the research, this section briefly describes the education system in Panama. The system is divided into three levels: primary, secondary, and higher education. Compulsory education lasts a total of 11 years, and includes preschool education from four to five years of age (pre-kindergarten and kindergarten), six years of primary school (grades 1-6), and three years of middle school (grades 7-9). Optional secondary school for students aged 15 to 18 follows and is divided into academic and vocational tracks. Primary school, which is mandatory and free, is characterized by high enrolment ratios and low student/teacher ratios.

As already pointed out, Panama has a high per capita GDP coupled with a high poverty rate caused by the historical duality of its economy. In this context, education is considered a critical issue for the country's future growth and development. Nonetheless, investment in education is still quite modest. Specifically, public expenditure on education as a percentage of GDP remains constant at around 3.5%, below that of other similar countries.

In 2006, when the *RdO* programme was first implemented, school attendance in primary and secondary education stood at 98.2% and 74.1%, respectively, indicating that increased enrolment was already being observed when the programme was being implemented (Economic Commission for Latin America and the Caribbean [ECLAC] database). Yet Panama is a country with substantial inequalities, above all due to the relatively large size of the indigenous population, and education still depends to a great degree on the particular area. Differences between urban and rural areas in school attendance are striking. While 99.1% of children between 7 and 12 years of age attend school in urban areas, only 96.9% of children in rural areas do so. The respective figures for 13-19 year olds are even more conspicuous; 81.2% for urban and 62.5% for rural

areas (ECLAC database). School attendance rates in Panama are thus close to universal in primary education, but are at some distance in high school. Difficult communications are considered one reason for these differences, given that some students must walk long distances to get to school (Whetten *et al*., 2018). Opportunities for attending school beyond sixth grade are more limited because high schools are mostly located in large towns. Nevertheless, poverty appears to be the main reason. Favoured by the lax child labour laws, parents in rural areas often pull their children out of school and send them to work as coffee pickers.

Ultimately, the success of such programmes will depend not only on attending to the demand side but also on improving the supply side. On the supply side, some indicators related to access to drinking water, sanitation and electricity provide us with an idea of the conditions in which the programme is applied and to what extent educational facilities are appropriately equipped. The percentage of schools with access to drinking water allows the conditions in which the population attends school to be assessed. In 2010, four out of ten primary schools lacked drinking water and a third of secondary schools evidenced deficiencies in the provision of this basic service (World Bank, database). In addition, an electrical power supply is essential for almost any activity. In the educational context, it plays an important role in learning and in the use of information and communication technologies. Again, schools located in *comarcas indígenas* (indigenous areas) have difficulties with the supply of electricity. In order to achieve the desired objective, it is clearly necessary to provide schools in areas that suffer from limited resources with the appropriate infrastructure and facilities.

3. *Red de Oportunidades* programme

The *RdO* programme came into force in 2006 with the aim of alleviating the basic needs of households that were enduring extreme poverty. The programme sets out several

incentives (cash transfers) awarded to these households and which are aimed at encouraging them to invest in their children's education and to make greater use of educational as well as mother-child health facilities. In exchange, beneficiaries agree to take part in training programmes designed to foster their skills in order to increase their future revenues and also to enrol their children at school.

Initially, the programme was funded through discretionary allocations made by the Presidency of the Republic of Panama. However, since 2009, the World Bank and the Inter-American Development Bank (IDB) began to contribute jointly. The total amount for the period 2009-2014 came to 46.94 million dollars, with 20.17 million being provided by the IDB and 24.0 million by the World Bank. This was not aimed at paying CCTs but at funding a series of complementary actions. The remaining 2.77 million dollars, and the *Fondo Especial para la Pobreza Extrema* (Special Fund for Extreme Poverty) whose income derives from the revenue generated by the *Canal de Panamá* (Panama Canal), and which amounts to 161 million, is used to pay the cash transfers. Since 2010, the *RdO* programme has been included in the *Plan Estratégico de Gobierno de Panamá* (Strategic Government Plan of Panama).

The programme parameters provide the basis for defining the organizational structure and procedures to be followed until the transfer reaches the beneficiary. The main ones are: (i) the eligibility criteria or focalization; (ii) the benefit structure; (iii) conditionality or co-responsibility; (iv) penalties; and (v) the system of recertification and exit strategies.

The *RdO* uses two subsequent methods to select potential beneficiaries. The first, geographical focalization, identifies marginality at the smallest political-administrative division in Panama (*corregimiento*). The programme uses a composite marginality index (CMI) which varies between 0 and 100 (the closer to zero, the greater

the marginality) and includes two dimensions: (i) extreme poverty measured in terms of consumption, and (ii) basic unmet needs in terms of health, accommodation, water and sanitation, and education. The second method declares households to be eligible provided they meet two requirements: (i) extreme poverty, according to the definition stipulated in the programme (geographical focalization), established at 312 dollars per month per household and 45 dollars per month per person (Ministerio de Desarrollo Social, 2009); (ii) requirements in terms of demographic composition, distinguishing between urban and rural areas, and indigenous districts, based on the proxy means test (PMT). The PMT estimates the probability that a household suffers extreme poverty using data from the *Encuesta de Vulnerabilidad Social* (Social Vulnerability Survey, EVS), which reflects households' socioeconomic, demographic, physical and human characteristics. The process concludes with an eligibility report based on different cut-off points that depend on the area of intervention (indigenous, rural, and urban) and has varied on different occasions to include more households in extreme poverty as beneficiaries. The information provided by the Programme Management Information System reveals that 48 percent of beneficiaries are concentrated in rural areas, and 36 percent in indigenous areas.

The PMT procedure aims to minimize exclusion and inclusion errors. Nevertheless, all conditional cash transfer programmes suffer inclusion and exclusion errors to some degree (Cecchini and Atuesta, 2017; Amarante and Brun, 2018). As regards the eligibility report, there was found to be an inclusion error of 13.7 percent, meaning that more than 58,000 people who took part in the programme should not have been helped as they did not comply with the thresholds established by the focalization criterion. There was also found to be an exclusion error of 9.6 percent, meaning that more than 43,000 people who should have received help from the programme were excluded (Him

et al., 2016).

With regard to the benefit structure, the *RdO* programme only envisages cash transfers (regular and fixed per household), without this meaning that beneficiaries are forced to renounce other transfers in kind. Initially, the amount was set at 35 dollars per month (two-monthly payments). In 2008, this figure was increased to 50 dollars a month in order to offset the impact of the global crisis. The amount aimed to achieve a 30 percent reduction in the gap between consumption in poor families and the poverty line (Arraiz and Rozo, 2011).

By way of an exception, indigenous areas are paid per family and not per household. As with other Latin American and Caribbean programmes, the person responsible for receiving the transfer is the mother or, when this is not possible (there is no mother, she is unable to take charge or she has no identity card), it is the person in charge of the household. Another aspect of the benefit structure is the mode of payment, which is basically channelled through branches of the Panama National Bank. For hard to reach areas, payment is made through a mechanism known as mobile banks.

The transfer is conditioned on households having children aged between 4 and 17 years old who are in formal education or in non-formal education that has been approved by the Ministry of Education and who attend school regularly. The programme establishes a series of exceptions to those co-responsibilities in education for children who, when enrolling, can prove that: they have some disability or chronic illness; they live far from the nearest available school (over a two-hour trip or an additional cost of 10 dollars per month); safety problems; the available school not having the capacity to accept children who are beneficiaries or where there are no educational facilities available in the area of influence. Children are allowed up to 10 percent non-attendance of actual school days (Ministerio de Desarrollo Social, 2009).

Finally, another of the programme's parameters relates to criteria concerning beneficiary exit and recertification. The *RdO* programme establishes a period of three years (36 months), which can be extended up to a maximum of four. Once this period has concluded, beneficiaries graduate and leave the programme. However, they may be subject to a process of recertification for a similar or shorter period should the programme manager deem it appropriate. The definition of this parameter evidences certain ambiguities which might be open to discretionary decisions. It is unclear whether a beneficiary is leaving a programme due to their having overcome the situation of poverty, budgetary adjustments or whether it is the result of political favouritism or patronage.

The scope of the *RdO* programme may be measured either in terms of the financial resources made available to fund it or in terms of the extent of the programme's coverage (people living in households that benefit from transfers, which may be measured in absolute terms or as a percentage of the total population). The programme was initially endowed with funds equivalent to 0.09 percent of national GDP, a figure which reached its peak in 2009 and 2010 (0.22 percent). Since then, this percentage has gradually fallen, reaching 0.07 percent in 2016, compared to the average of 0.33 percent dedicated by Latin American and Caribbean countries (ECLAC database). Together with Haiti, Guatemala, and Belize, Panama is thus one of the countries to devote least in terms of its budget to this type of programme. In the case of Panama, this result is the combination of a slight drop in spending in the *RdO* over the last two years and of high GDP growth rates (Cecchini and Atuesta, 2017). When it began, the programme was intended to reach 15 percent of Panama's population (some 75,000 households). However, the highest coverage achieved was 12.0 percent in 2009, a figure which fell to 9.6 percent in 2016, well below the average for the region (20.2 percent).

One of the *RdO* programme's main shortcomings is its failure to verify compliance of conditionalities[1], which leads to two major problems. On the one hand, no information is available concerning the coverage of the services provided. On the other, co-responsibilities and penalties are not enforced (Gómez Hermosillo, 2009), even though the *RdO* stipulates a maximum penalty of 10 dollars each two months for failure to comply. In addition, a number of challenges remain which might help to improve the programme's effectiveness. These include: the need to design appropriate mechanisms to optimize supply and the quality of health and education services, fostering a policy to enhance road networks in hard to reach areas (indigenous areas) and promoting the development of local firms (Waters, 2010).

4. Methodology

In order to estimate the impact of the *RdO* programme on school attendance the quasi-experimental propensity score matching (PSM) technique is used, since the programme has not been implemented in an experimental context, and beneficiary families were not chosen at random. This technique is chosen from among the different methods based on the counterfactual analysis (experimental or quasi-experimental) available for evaluating programmes and public policies (Khandker *et al*., 2010).

Following the traditional terminology (Heckman *et al.*, 1997), Y represents our variable of interest, *school attendance*. In particular, Y_{1i} denotes school attendance status when the child belongs to a family that takes part in the *RdO* programme, and Y_{0i} denotes the school attendance status if the child had belonged to a non-beneficiary family. As regards the variable identifying participation in the programme, it is represented through a dummy variable D, which equals 1 if the individual forms part of a family that participates in the programme (treatment group) and 0 if the individual forms part of a non-beneficiary family (control group). We are interested in comparing the school

attendance of children from families that form part of the *RdO* programme to what would have happened with their attendance had the family not benefitted from the programme; that is, we are interested in the expected difference between the two outcomes over the population with D =1. In the literature, this measure is called the average treatment effect on the treated (ATT). The ATT could be written as $E(Y_{1i} - Y_{0i} | D_i =1) = E(Y_{1i} | D_i =1) - E(Y_{0i} | D_i =1)$, or similarly, when considering that participation occurs in accordance with certain observable characteristics X, which may in turn impact the outcome variable, as $E(Y_{1i} - Y_{0i} | D_i =1, X) = E(Y_{1i} | D_i =1, X) - E(Y_{0i} | D_i =0, X)$.

The problem is that the counterfactual outcome, $E(Y_{0i} | D_i =1)$, cannot be observed as individuals can only be in one of the two situations at the same time. The proposed method seeks to solve this problem by substituting the expected outcome of the person who participated, assuming that they had not participated $E(Y_{0i} | D_i=1)$, for the expected outcome of those who actually did not participate $E(Y_{0i} | D_i=0)$. However, the decision to take part in the programme is not always taken randomly, such that substituting will prove appropriate under certain circumstances, specifically, when fulfilling the conditional independence assumption and the common support condition.

In this sense, the technique requires finding an ideal comparison group (control group) in relation to the treatment group, based on a sample of non-participants and measuring the proximity between the two groups in terms of the observable characteristics. Once the treatment and control groups are defined, matching is carried out. This involves finding individuals within the control group who are similar or identical to those in the treatment group, bearing in mind the set of observable characteristics (X). However, in terms of observable characteristics, matching may in practice prove to be a difficult task when there are many characteristics. As a result, Rosenbaum and Rubin (1983)

proposed using the propensity score to solve this problem of multidimensional matching. This technique reduces the set of observable characteristics to a scalar one. Specifically, the propensity score, or p(X), is obtained by estimating the probability of participation, conditional on a set of characteristics. ATT would thus be expressed as $E(Y_{1i} - Y_{0i} \mid D = 1, p(X)) = E(Y_{1i} \mid D = 1, p(X)) - E(Y_{0i} \mid D = 0, p(X))$.

As pointed out, the PSM technique requires the conditional independence assumption to be fulfilled. It is assumed that people who display identical characteristics have the same chance of belonging to the treatment group and that, once the observable characteristics have been controlled, the possible result is irrespective of participation in the programme. The distribution of the characteristics that determine the propensity score is the same in the two samples: the treatment and the control groups. The existence of common support ensures that for each treated individual there is another individual with similar characteristics X in the control group to perform the matching.

The literature offers a number of methods for matching which differ in the way the set of observations in the control group is determined. The most common are: (i) nearest neighbour matching; (ii) the radius method, and (iii) the Kernel method. As there is no evidence in the literature that one of these criteria is better than any other (there is more of a trade-off vis-à-vis efficiency/bias), several are normally applied as a measure of robustness.

5. Data and implementation of the propensity score

In cases where no specific follow-up method for beneficiary families has been established to evaluate a programme, household surveys provide an appropriate source of basic information for this task (Bonal and Tarabini, 2006). One advantage of this kind of survey is that it also offers information on families who do not participate in the programme but who do fulfil the requirements for being in the programme, such that a

comparison may be found in the group. In our case, the data used are taken from the *Encuesta Continua de Hogares* (Continuous Household Survey, ECH) conducted by the National Institute of Statistics and Census which, as of 2010, included a question allowing us to identify households who participate in the *RdO* programme. The analysis was carried out for two years (2010 and 2013). The 2010 sample comprises 48,881 individuals who belong to 13,391 households. In 2013, the sample was slightly smaller, and consisted of 44,237 individuals grouped in a total of 11,853 households.

In order to apply the methodology described above, children between the ages of 6 and 17 who belong to poor or extremely poor families were chosen. This age group was selected since it corresponds to the levels of primary and secondary education and because it falls within the time window in which the programme should have been able to enforce co-responsibility in education. To determine each family's situation of poverty (or extreme poverty), per capita income based on the ECH was built. This income was then compared with the poverty lines (general and extreme) officially established by the Ministry of Economics and Finance. In order to construct income, we worked with the notion of total current income, which includes: income from salaried work (monetary and in kind); income generated from independent work (patrons and self-employed workers), including self-consumption and self-supply; revenue from property; retirement and pensions; and other public and/or private transfers that enter the household, following the CEDLAS method and the World Bank (2014). As a result of this selection, the final sample is composed of 5,794 and 4,879 children in 2010 and 2013, respectively, of whom 1,523 and 1,737 participate in the programme in 2010 and 2013, respectively (Table 1).

[Table 1]

Given that the aim is to measure the programme's impact on school attendance, our variable of interest is built based on the survey question: *Do you attend school regularly*? It is a dichotomous variable that takes the value 1 if individuals answer that they do, and 0 otherwise[2]. Another dichotomous variable was constructed which identifies for each individual whether their family participates in the *RdO* programme (treatment group) or does not (control group). The questionnaire contains various questions aimed at finding out what support families are receiving. They were directly asked: *Did you receive any money last month from: Conditional Cash Transfers? (Red de Oportunidades)*

To construct the propensity score, a probit model was estimated in which different characteristics were included: related to the child (gender); geographical (area of residence and province); the head of the family (marital status and occupation of the head of the household); related to health (registered with the social security office); and basic questions related to the household (number of children). Finally, other aspects related to the dwelling were also taken into account such as whether there was overcrowding. The procedure was repeated, separating the total sample into ages: on the one hand, the group of children between 6 and 11, corresponding to primary education and, on the other, the group of children between 12 and 17, ages that cover secondary education. For each sub-sample resulting from selecting an age group, its corresponding propensity score was estimated, such that in each case the balancing property is fulfilled. As a result, the variables included in the probit might vary. Most of the variables proved statistically significant and displayed the expected sign in the probit models estimated.

One basic assumption of the methodology is that observations with the same propensity score will have the same distribution in the observable characteristics, regardless of

whether or not they have received cash transfers. In other words, given the same propensity score, allocation or not to the treatment group is random. As a result, in order to evaluate the quality of the matching, various statistics were used that compare the samples both before and after matching (Rosenbaum and Rubin, 1983; Sianesi, 2004). The results to emerge from this analysis proved satisfactory overall. The null hypothesis that the means of the observed characteristics are equal across beneficiary and control group is not rejected for virtually all the variables included in the probit models (balancing propensity test) and significant improvements were obtained in other statistics (for example, both the mean and median bias fell, as did the pseudo-R^2). In all instances, a balanced sample was achieved and very few observations were found outside the region of common support.

6. Results

The results of the effect of the *RdO* programme on school attendance for the whole sample as well as for each separate age group are shown in Table 2 (The PSMATCH2 programme by Leuven and Sianesi (2003) was used[3]). As can be seen, the value of the ATT is negative in almost all cases, although it is not always significant (z-stat)[4]. This means that no positive effect of participation in the *RdO* programme on school attendance in families who are beneficiaries of the programme has been found. The same result was also obtained when performing the estimation separately for each age group. When dividing into groups, the ATT in the youngest group (6 to 11 years old) failed to prove significant in any of the cases, leading us to conclude that there are no differences in attendance rates amongst the two groups (treatment and control). Nevertheless, for the 12 to 17 year-old group, the effect does prove significant, particularly for 2013, indicating that attendance rates of those who participate are below those of the control group (by almost five points). For the 12 to 17 year-old group, the

RdO programme even failed to bring attendance rates into line with each other. Some possible explanations may stem from institutional factors and programme governance. Exclusion and inclusion errors when identifying programme beneficiaries, as well as other deficiencies in governance, have been detected (Him *et al.*, 2019), such as the need to establish more effective protocols in order to guarantee the monitoring of co-responsibilities in education. In addition, some results indicate that the cash transfer provided to households is not a sufficient incentive to make families change their mind about placing their youngest children in the labour market (Hoops & Rosati, 2014). Therefore, the protective effect of the programme is not decisive in reducing child labour in the older age group.

[Table 2]

One possible explanation for the null effect found for primary education might be the fact that attendance rates were already high. This finding concurs with the argument of Arraiz and Rozo (2011) who point out that, in a context in which school participation rates before the programme was implemented were relatively low, the programmes tend to exert a positive and greater effect than in situations in which rates were already high. During the years covered in the study, Panama was close to universal coverage in primary education, such that the result obtained is as expected: the *RdO* programme has no statistically significant effect. In secondary education, however, there is a gap which not only does not seem to be disappearing but would seem to be worsening in 2013.

The results shown in Table 2 provide a comparison of how the effect changed between the two years (2010 and 2013). In 2010, the attendance rate prior to matching was 91.4 whereas by 2013 it had fallen slightly (91.0). When comparing these attendance rates after matching (final column of the table), the reduction proves significant, regardless of which matching method is used. During the period analysed, when the *RdO*

programme was in force, attendance rates fell slightly, mainly as a result of the drop in the younger age group (6-11 year olds).

Since the programme is implemented in areas which differ enormously with regard to access to social services (basically, education and health), the programme's uneven success over the various regions would seem to make sense. As a result, the impact was also evaluated by areas. The literature provides a range of studies indicating that the rural or urban environment may be a relevant factor in terms of the impact of conditional cash transfer programmes. For example, Martínez (2012) examines the impact of the *Oportunidades* programme only in urban areas of Mexico and finds that it fails to have any effect on attendance. In a similar vein, Schaffland (2012) finds no significant differences in certain regions when evaluating the *Bolsa Familia* (former *Bolsa Escola*) programme in Brazil. In contrast, other authors such as Behrman *et al*. (2012) report that the effect is similar in rural and urban areas during the initial stages of the programme. Attanasio *et al*. (2010) find that the marginal effect is more intense in certain urban areas than in rural areas in Colombia.

Table 3 shows the results broken down into geographical areas (rural, indigenous and urban). It should first be pointed out that there is only an increase in attendance rates in 2013, compared to 2010, in rural and indigenous areas, bringing them closer to urban attendance rates, although they still remained below. In any case, once the technique has been applied, a positive although not statistically significant effect in indigenous areas emerges, with these being the areas where the programme would be expected to compensate to a greater degree for marginalisation and poverty. Looking at the attendance rates, those for the treatment group are slightly higher than for the control group, although the difference is small and does not prove to be significant. For rural areas, the effect is always negative, but is only significant in 2013. The widespread

trend of reaching universal coverage increases attendance rates in the control group, although the increase has not been sufficient for the treatment group. It would seem that the *RdO* programme fails to exert a positive effect in areas where there is a greater concentration of poverty. One further aspect concerns a possible time delay for urban areas. The effect is seen to be negative and significant in 2010, when the programme is still its infancy, whereas there are barely any differences between the two groups in terms of attendance rates in 2013 when the programme is already well established. Similar conclusions may be drawn from Arraiz and Rozo (2011) since, in their case, the programme's effect on enrolment fails to reach all education levels in rural areas, with the authors failing to find any effect on enrolment in secondary education in indigenous areas.

[Table 3]

Finally, a difference is seen to emerge in terms of the gender effect (Table 4). Various studies point to the impact of this kind of programme differing between boys and girls. In general terms, these programmes seem to favour girls more than boys, due to greater income elasticity in the case of girls (Schultz 2004; De Brauw *et al*. 2015). The progression of attendance rates between the two years considered in the study differs for boys and girls. Whereas attendance falls in the case of girls (-1.6 points), it increases slightly for boys (0.4 points), although the rates continue to be higher amongst girls. Once again, negative mean effect values are obtained (ATT), although these only prove significant amongst girls. Girls in the *RdO* programme continue to exhibit lower attendance rates than those in the control group, with the difference being some three points lower.

[Table 4]

7. Conclusions

This work seeks to gauge the impact of Panama's Red de Oportunidades programme on school attendance. Families taking part in the programme receive cash transfers (fixed and homogeneous) and agree to have their children attend school regularly. In order to estimate the programme's effect, a quasi-experimental impact evaluation technique (propensity score matching) was applied.

Results show that the programme does not always manage to bring into line the school attendance of children from families involved in the programme with that of similar families not in the programme. In particular, attendance levels continue to remain lower for those taking secondary education (12-17 year-olds) and in the case of girls. It should be pointed out that attendance rates in rural and indigenous areas are slightly higher, although the effect is not statistically significant.

There are several possible reasons for these results. First, the literature shows that when the school attendance rate is already high and close to universal level, any improvement is barely noticeable (Martínez, 2012). Certain studies go a step further, and report that conditionalities on school attendance might not make much sense in middle-income countries where school attendance is already satisfactory (Villatoro, 2008). In Panama, the proportion of children in primary school is already high, as over 98.3% of children in households were reported to be in school at baseline, which explains why the effect for primary education is not significant in any case. In sum, CCT programmes would only make sense when the level to which the poor invest in education is below the optimum level from a social standpoint. Recent research suggests that cash transfers should be conditional upon educational achievement as a means of improving learning outcomes (Villatoro, 2008). Nevertheless, conditional cash transfers might work as preventive mechanisms that avoid non-desirable situations such as dropping out of

school early. In this regard, participation in CCT programmes of families whose children already attend school regularly would also prove justifiable.

Second, it should also be pointed out that implementing measures on the demand side is not enough when access to social services is not guaranteed. In Panama, access to public services in many cases is limited by geographical characteristics (Waters, 2010). This can explain our result in the case of secondary education. Given that high schools are located in large towns –which implies additional costs in attending school- students have fewer opportunities to attend beyond sixth grade. Social programmes thus need to take into account the supply side of schooling by providing other material means to those students living in areas that lack sufficient infrastructure (roads, paths, electricity, running water or sanitation). In this case, non-financial access barriers can result in school attendance being negatively impacted.

Third, although education has increased in value among parents who consider making sacrifices so that their children can study, girls still play an important role in the home. The fixed and limited amount of cash transfers, which do not depend on the number of children, implies that families must decide from amongst their children which of them will be allowed to study outside the community.

The results to emerge have direct implications for the design of conditional cash transfer programmes. In particular, the question arises as to whether it makes sense to follow a standard application of such programmes or whether, by contrast, to take into account the singularities of each geographical area. In a similar vein, there is a need to refocus resources towards those educational levels which remain some way off universal coverage. In sum, public authorities should undertake a thorough and deep-rooted analysis of the main parameters of the CCT component of the *RdO* programme so as to better adjust to the country's particular features. Some possible proposals would be to

introduce a non-fixed cash transfer or to increase the amount thereof. For instance, the latest reform of the *Bono de Desarrollo Humano* in Ecuador has incorporated an additional variable component that depends on the number of children and their ages (Lucero and Burbano, 2018). Both proposals would significantly decrease the opportunity costs involved in educating children and would encourage parents to re-consider the need for child labour. A clear and appropriate system for monitoring compliance with co-responsibility also needs to be established since families might have become lax when it comes to fulfilling obligations, with this being one of the main weaknesses of the *Red de Oportunidades* programme.

[1] Him *et al*. (2019) analysed qualitative data from the Management Information System and from interviews with different stakeholders (senior technical officers from various administrative units of the programme management, directors of schools with children whose families are beneficiaries of the programme, etc.) in order to detect deviations in the programme execution. They found that they had problems in adequately controlling school attendance.

[2] When the outcome is dichotomous, the (average) effect of treatment is estimated as the difference between the proportion of subjects experiencing the event in each of the two groups (treated vs. control) in the matched sample. As a result, the average treatment effect is interpreted as a difference in probability (Austin, 2011; Austin and Stuart, 2015).

[3] The bootstrap option was used to obtain bootstrapped standard errors.

[4] Given that our outcome variable is dichotomous, a z-test is used to assess the statistical significance of the effect.

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Table 1. Descriptive statistics for the control variables included in the probit model (sample 6-17 years old).

Variable	2010			2013		
	Total	Treated	Control	Total	Treated	Control
Gender						
Men	49.52	48.92	49.73	48.72	48.13	49.05
Women	50.48	51.08	50.27	51.28	51.87	50.95
Area						
Rural	63.53	87.33	55.05	68.42	92.34	55.19
Urban	36.47	12.67	44.95	31.58	7.66	44.81
Regions[1]						
Western	30.01	29.15	30.32	34.52	49.13	31.13
Eastern	7.02	14.51	4.35	16.40	42.86	12.06
Metropolitan	32.48	22.59	36.01	24.60	29.17	33.00
Central	30.48	33.75	29.31	24.49	43.73	23.81
Marital status of the household head						
Together	55.09	61.65	52.75	61.20	46.46	57.15
Separated or divorced	15.19	7.88	17.79	15.63	31.51	18.09
Married	22.73	23.44	22.48	16.37	36.30	16.79
Widow(er)	4.94	6.63	4.33	5.35	19.94	6.02
Single	2.05	0.39	2.65	1.46	7.57	1.94
Occupation of the household head[2]						
Executive, management, professional	1.69	1.45	1.78	2.22	0.83	3.08
(Senior) office staff	3.98	1.69	4.75	2.07	1.14	2.65
Salesperson, labourers, artisans, operators	94.33	96.86	93.47	95.72	98.03	94.28
Registered with the social security office						
No	70.92	85.75	65.63	77.02	90.21	69.73
Yes	29.08	14.25	34.37	22.98	9.79	30.27
Number of children in the household (mean)	3.21	3.51	3.10	3.18	3.44	2.29
Overcrowding						
Yes	76.75	89.17	72.32	79.75	88.67	74.86
No	23.30	10.83	27.68	20.25	11.39	25.14
Number of observations	5794	1523	4271	4879	1737	3143

Source: Authors' own based on ECH (2010; 2013). Notes: [1] *Western*: Bocas del Toro and Chiriquí and the district of Ngäbe Buglé, *Eastern*: Darién, district of Worgandí, Emberá, Madugandí, Emberá Wonnan and Kuna Yala, *Metropolitan area*: Panamá and Colón, *Central*: Los Santos, Coclé, Veraguas and Herrera. [2] Fully comprehensive information is available on all variables except the employment situation of the household head.

Table 2. Effect of the *Red de Oportunidades* programme on school attendance (ATT).

Age group: 6-17 years old

	2010				2013				2013/2010	
	91.4				91.0				-0.004	
Matching method	School attendance (%) Treated	Control	ATT	z-stat	School attendance (%) Treated	Control	ATT	z-stat	diff	t-stat
NN	88.0	89.1	-0.011	-0.97	88.8	90.1	-0.013	-1.18	-0.002	-4.95***
Radius (0.1)	88.0	90.5	-0.025	-2.52**	88.8	91.3	-0.026	-2.62**	-0.001	-4.26***
Kernel	88.0	90.0	-0.019	-1.98	88.8	91.4	-0.026	-2.73**	-0.007	-27.05***

Age group: 6-11 years old

	2010				2013				2013/2010	
	98.2				97.5				-0.007	
Matching method	School attendance (%) Treated	Control	ATT	z-stat	School attendance (%) Treated	Control	ATT	z-stat	diff	t-stat
NN	97.4	96.6	0.008	0.92	96.7	94.7	0.020	1.95	0.012	23.96***
Radius (0.1)	97.4	97.7	-0.004	-0.44	96.7	96.9	-0.002	-0.26	0.002	9.12***
Kernel	97.4	97.5	-0.001	-0.14	96.7	96.9	-0.001	-0.26	0.000	0.00

Age group: 12-17 years old

	2010				2013				2013/2010	
	83.3				83.4				0.001	
Matching method	School attendance (%) Treated	Control	ATT	z-stat	School attendance (%) Treated	Control	ATT	z-stat	diff	t-stat
NN	76.7	77.7	-0.010	-0.43	80.0	83.1	-0.031	-1.45	-0.021	-21.84***
Radius (0.1)	76.7	82.0	-0.053	-2.75**	80.0	84.6	-0.050	-2.55**	0.006	8.35***
Kernel	76.7	80.4	-0.037	-1.88	80.0	84.9	-0.050	-2.73**	-0.012	-16.81***

Source: authors' own based on the ECH (2010, 2013).

Note: *** p< 0.01, ** p< 0.05, *p< 0.10. NN = nearest neighbour.

Table 3. Effect of the *Red de Oportunidades* programme on school attendance (ATT) by area.

Area: Rural

	2010				2013				2013/2010	
	90.0				90.9				0.009	
Matching method	School attendance (%) Treated	Control	ATT	z-stat	School attendance (%) Treated	Control	ATT	z-stat	diff	t-stat
NN	88.5	90.9	-0.024	-1.83	89.3	93.3	-0.040	-2.94**	-0.016	-32.6***
Radius (0.1)	88.5	90.3	-0.018	-1.39	89.3	92.1	-0.028	-2.05**	-0.010	-22.8***
Kernel	88.5	90.6	-0.021	-1.64	89.3	92.0	-0.027	-1.97	-0.006	-13.3***

Area: Indigenous

	2010				2013				2013/2010	
	85.4				88.0				0.026	
Matching method	School attendance (%) Treated	Control	ATT	z-stat	School attendance (%) Treated	Control	ATT	z-stat	diff	t-stat
NN	86.4	80.3	0.062	1.76	87.6	84.6	0.030	1.37	0.004	2.44**
Radius (0.1)	86.4	84.6	0.019	0.56	87.6	88.3	-0.007	-0.34	0.033	20.73***
Kernel	86.4	84.1	0.024	0.71	87.6	87.7	0.000	-0.05	0.001	0.63

Area: Urban

	2010				2013				2013/2010	
	95.6				94.3				-0.013	
Matching method	School attendance (%) Treated	Control	ATT	z-stat	School attendance (%) Treated	Control	ATT	z-stat	diff	t-stat
NN	89.3	96.3	-0.070	-3.41**	92.7	91.9	0.008	0.28	-0.023	-15.19***
Radius (0.1)	89.3	96.0	-0.067	-3.44**	92.7	94.2	-0.015	-0.63	-0.03	-30.90***
Kernel	89.3	95.9	-0.066	-3.36**	92.7	94.2	-0.015	-0.63	-0.03	-37.74***

Source: authors' own based on the ECH (2010, 2013).

Note: *** p< 0.01, ** p< 0.05, *p< 0.10. NN = nearest neighbour.

Table 4. Effect of the *Red de Oportunidades* programme on school attendance (ATT) by gender.

Girls

	2010				2013				2013/2010	
	92.4				90.8				-0.016	
Matching method	School attendance (%) Treated	Control	ATT	z-stat	School attendance (%) Treated	Control	ATT	z-stat	diff	t-stat
NN	88.47	90.63	-0.022	-1.33	87.93	90.83	-0.029	-1.79	-.007	-8.70***
Radius (0.1)	88.47	91.96	-0.035	-2.53**	87.93	91.31	-0.033	-2.42**	.003	4.08***
Kernel	88.47	91.73	-0.033	-2.36**	87.93	91.36	-0.034	-2.50**	-.001	-21.04***

Boys

	2010				2013				2013/2010	
	90.4				90.8				0.004	
Matching method	School attendance (%) Treated	Control	ATT	z-stat	School attendance (%) Treated	Control	ATT	z-stat	diff	t-stat
NN	87.58	86.35	0.012	0.70	89.52	89.98	-0.005	-0.26	-0.017	-22.51***
Radius (0.1)	87.58	88.94	-0.013	-0.89	89.52	90.75	-0.012	-0.97	0.002	4.16***
Kernel	87.58	87.90	-0.014	-0.20	89.52	90.76	-0.012	-0.97	-0.009	-18.08***

Source: authors' own based on the ECH (2010, 2013).

Note: *** p< 0.01, ** p< 0.05, *p< 0.10. NN = nearest neighbour.