THE IMPACT OF CONDITIONAL CASH TRANSFERS ON EDUCATIONAL INEQUALITY OF OPPORTUNITY

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Abstract: Most conditional cash transfer evaluations have focused on estimating program effects on schooling, consumption, and labor supply. Fewer studies have addressed these outcomes using a distributive lens. This article uses data from three programs in Latin America to obtain evidence of their impact on educational inequality of opportunity, measured using primary enrollment. The main results indicate that groups considered vulnerable gain more in terms of access to education and that these interventions help level the playing field. They do not eliminate inequality of opportunity but are certainly a useful complement to equity-enhancing policies.

Conditional cash transfers (CCTs) have rapidly become a mainstream policy instrument in developing countries around the world. For instance, by 2008 almost thirty countries had some type of CCT program in implementation (Fiszbein and Schady 2009). Among the reasons leading to this widespread adoption we may include their targeted approach toward the poor, short- and long-term objectives, clearly defined benefit structures, and randomized design.[1]

This context has led to a substantial literature estimating the effects of these interventions on various outcomes. Most of the available evidence has quantified program impact on consumption, education, health, nutrition, infant mortality, and other socioeconomic variables.[2] Considerably less attention has been given to

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1. There are some nonrandomized CCTs, which include Argentina's Asignación Universal por Hijo and Brazil's Bolsa Escola (later Bolsa Família). However, the current standard design involves random assignment of transfers. See Fiszbein and Schady (2009) for more on the conceptual design of these programs.

2. Some of the main studies that assess short-run impact in Latin America include Skoufias and Parker (2001), Gertler (2004), Schultz (2004), Behrman, Sengupta, and Todd (2005), and Behrman, Parker, and Todd (2011) for Mexico; Cruces et al. (2008) and Cruces and Gasparini (2008) for Argentina; Bourguignon Ferreira, and Leite (2003) and Soares, Ribas, and Osorio (2010) for Brazil; Attanasio et al. (2010)

the distributive effects of CCTs, with the exceptions of Handa et al. (2001), Soares et al. (2009), and Skoufias, Lindert, and Shapiro (2010). One rational explanation for the limited evidence on this front is that it is due to one of the program's defining characteristics: it is targeted at the poor. This particular feature restricts findings from any distributive analysis on CCTs to the lower end of the income (or consumption) distribution, which hinders their external validity.[3] However, there may be valuable lessons in studying the distributive effects of CCT programs on a particular component of inequality: inequality of opportunity.

Inequality of opportunity is concerned with outcome disparities that arise from factors considered unfair, such as exogenous circumstances over which individuals have no control (Roemer 1998). Consequently, these circumstances generate a natural classification of individuals into social groups that represent a situation of "advantage" or "disadvantage." This sorting implies that inequality of opportunity has a clearly defined horizontal perspective where group membership has considerable relevance to a person's life chances (Stewart 2009). On this view, equality of opportunity is achieved when the opportunity sets between social groups are equally distributed (Aaberge, Mogstad, and Peragine 2011).[4]

This study's main objective is to provide evidence on whether CCT programs have contributed to equalizing educational opportunities in primary schooling. I focus on education since it is one of the main components of all CCT programs, is directly linked to upward mobility, and is to date considered one of the main pathways to escape the vicious cycle of poverty (Breen and Jonsson 2005; Peragine and Serlenga 2008). Since the definition of inequality of opportunities I propose deals with differences between groups, the selected circumstance types are chosen to depict advantaged and disadvantaged individuals in terms of plausibly exogenous characteristics. These include ethnicity, gender, socioeconomic background (using parental education level as a proxy), and whether a child is born into a unified or disintegrated household. While this is far from an exhaustive list, these groups represent relevant and observable circumstances for analysis.

The empirical assessment is carried out on three CCTs implemented in rural areas: Honduras's Programa de Asignación Familiar (PRAF), Mexico's Programa de Educación, Salud y Alimentación (PROGRESA), and Nicaragua's Red de Protección Social (RPS). I first rely on impact-evaluation methods to estimate program effects on advantaged and disadvantaged types to determine whether there is evidence of closing enrollment gaps. Second, I also quantify the changes

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for Colombia; Carrillo and Ponce (2008) for Ecuador; Larrañaga, Contreras, and Ruiz Tagle (2012) for Chile; Jones, Vargas, and Villar (2008) and Copestake (2008) for Peru; Glewwe and Olinto (2004) and Moore (2008) for Honduras; Maluccio and Flores (2005) for Nicaragua; and Levy and Ohls (2010) for Jamaica. Fiszbein and Schady (2009) present a comprehensive review of other evaluations in Africa and other developing countries.

3. This is not the only reason why distributive analysis cannot be completely applied in these contexts. See Djebbari and Smith (2008) for a list of the required assumptions for analyzing the distributional consequences of CCT programs.

4. This view is referred to as the ex ante view of equality of opportunity (see Aaberge, Mogstad, and Peragine 2011). Note that this conception focuses solely on inequality between groups and is neutral with respect to inequality within the selected groups, making this view consistent with the analysis undertaken here but limited because it does not capture inequality within each group.

in between-group inequality in attendance, framing the results using an opportunity perspective.

The findings from this analysis provide several contributions to the literature on conditional cash transfers. The first is to provide further evidence on CCT impact but using a distributive lens.[5] The second is to quantify the magnitude to which these programs affect between-group inequalities in education. Finally, an additional contribution is that all estimates are obtained from homogenized data, which allows comparison of program performance.

INEQUALITY OF OPPORTUNITY AND CONDITIONAL CASH TRANSFERS

Defining inequality of opportunity

Inequality, like deprivation, is a multidimensional concept (Savaglio 2006; Duclos, Sahn, and Younger 2011). The trend in recent distributive studies has been to decompose inequality into two sources: factors controlled by individuals (e.g., effort) and exogenous circumstances. The seminal contribution in this literature is Roemer (1998), which argues that inequalities surfacing from factors beyond individual control are unfair and that in an equal opportunity society, disparities should arise solely from variation in the allocation of effort consistent with a meritocracy.[6]

On this view, circumstances generate a natural classification of individuals into types: social groups that represent a situation of advantage or disadvantage. These types may be defined using a single attribute (e.g., race) or a combination of these (e.g., race, gender, and socioeconomic background). For example, consider a simple definition of individuals by race. Ethnic minority groups are usually considered disadvantaged in numerous socioeconomic outcomes when compared to majority groups (Busso, Cicowiez, and Gasparini 2005). In this example, the minority race would usually constitute the disadvantaged group while the majority represents the advantaged type.[7] In a more general case, these groups may be identified in similar fashion depending on different combinations of circumstances.

This sorting implies that inequality of opportunity has a clearly defined horizontal perspective where group membership has considerable relevance to a person's life chances (Stewart 2009). Mainly, the advantaged group or type has higher well-being in one or more dimensions due to segregation, social stigmas, or other potential factors affecting the outcome under study (Bowles, Alden, and Borgerhoff 2010). This perspective is consistent with one of the two main approaches to

5. The available evidence on the effect of CCTs on opportunities has few empirical contributions. Among them, Wendelspiess (2010) analyzes the effect of PROGRESA on inequality of opportunity, although the author defines equality of opportunity using Sen's capability perspective, which differs from Roemer's (1998) approach used here in the manner in which effort and circumstances are modeled (see Aaberge, Mogstad, and Peragine 2011 for more on these conceptual differences).

6. An intense philosophical debate exists with respect to fairness and equality, which lies beyond the objectives of this article. See Fleurbaey (2008) for a general overview.

7. Bourguignon, Ferreira, and Walton (2007) suggest that social stigmas may (erroneously) generate a feeling of inferiority for certain groups such as racial or ethnic groups, which drives them to lower outcomes.

equality of opportunity, known as the ex ante approach (Aaberge, Mogstad, and Peragine 2011). This particular conception suggests that equality of opportunity is achieved when the opportunity sets between types are identical, regardless of their circumstances. Hence, inequality of opportunity falls if between-group disparities decrease. Consequently, adopting this view of equality of opportunity implies quantifying between-type inequalities, since the approach is neutral to differences within these groups.[8]

To further exemplify the above definition, consider its application to educational outcomes. This framework suggests that achieving equality of opportunity requires no educational disparities between individuals who differ solely by circumstances such as ethnicity, gender, or other factors beyond their control. Hence a simple test of inequality of opportunity would be to compare the educational distributions for advantaged and disadvantaged groups and observe if the conditional outcomes are different. The existing literature has already analyzed educational distributions in Latin America by a number of socioeconomic characteristics and found significant educational disparities (Barros et al., 2009; Gasparini, Cruces, and Tornarolli 2011).

Improving the distribution of educational opportunities acquires additional relevance because of its widely acknowledged correlation to upward mobility (Breen and Jonsson 2005). Education is considered one of the main pathways to escape the vicious cycle of poverty (Peragine and Serlenga 2008). Hence, opportunity-enhancing educational policies are expected to lead to a higher average education of the population and a more egalitarian distribution of schooling. It is this shift that has the potential to increase earnings and lower income inequality, subsequently improving overall well-being (Behrman 2011).

This article frames CCTs as policies able to reduce inequality of opportunity in education using the previously defined view as its underlying conceptual framework (and as suggested by Keane and Roemer 2009). The findings from this article aim to provide evidence on the ability of these programs to benefit the disadvantaged more than the advantaged. If this situation is observed, then the interventions should equalize the opportunity sets between types and reduce inequality of opportunity, according to the above definition. However, it is important to note that CCTs are not the only way to achieve equality of opportunity and constitute one policy among other social policies that directly intend to improve opportunities.

CONDITIONAL CASH TRANSFERS AS OPPORTUNITY-ENHANCING POLICIES

There has been an increasing trend in the implementation of CCT programs in developing countries. Primarily, these interventions aim to improve current welfare and promote investment in human capital to prevent future deprivation by

8. The other perspective used to study inequality of opportunity is the ex post (or tranches) approach, which uses a within-group perspective. In this conception, there is equality of opportunity when all individuals who exert the same level of effort attain identical outcomes.

providing income transfers to the poor through the household demand approach (Rawlings and Rubio 2005).[9]

In addition to their shared objectives, CCT programs also encompass other similar defining characteristics (Fiszbein and Schady 2009). First, they are targeted at the poor. Second, CCTs are designed to have a clearly defined benefit structure based on the number of children in beneficiary households, their ages, and their current grade in school. Third, as their name indicates, these interventions transfer an amount of income to households conditional on fulfillment of certain requirements. In most cases, households must send school-age children to educational centers and health check-ups at local clinics. Fourth, implementation of a CCT requires that a specific monitoring and evaluation framework be set up to measure the effects of the program. Finally, implementing a CCT implies that there needs to be a high level of efficiency and coordination among a number of sectors and across government levels.

In particular, it is these programs' long-term view that may be interpreted as improving equality of opportunity. This may be best exemplified by focusing on one of their main components: the accumulation of human capital. There is by now widespread evidence that CCTs stimulate human capital accumulation by increasing enrollment, since income constraints for the poor are relaxed and allow these families to send their children to school (see Filmer and Schady 2011 and the references therein). Higher attendance will eventually increase average years of education for the poor and consequently lead to higher average education and a more equal distribution of schooling (Schultz 2004; Mejía and St-Pierre 2008). Ultimately this more equitable distribution is also expected to generate lower income inequality and raise overall well-being (Behrman 2011).[10]

A reasonable assumption is that part of the reduction in inequality expected from this process may be attributable to the reduction in inequality between certain groups. In terms of the opportunity perspective followed here, this would imply that disparities among circumstance types fall. In fact, CCT programs have been found to improve the relative position of certain circumstance types (e.g., African Americans in the United States) compared to traditionally advantaged groups in society (O'Gorman 2010). This result may be explained due to the initially worse conditions that usually characterize these disadvantaged groups. Therefore, providing a program that generates educational incentives may have a higher impact on the disadvantaged types considering their low initial endowments, closing the preprogram gap between the groups and reducing inequality of opportunity.

Hence, CCT programs have an implicit equity-enhancing goal. Moreover, it is reasonable to assume that part of the expected equalization surfaces from re-

9. The converse policy promotes more traditional supply-side incentives such as school construction and teacher incentives, which do not necessarily address equity concerns.

10. However, while inequality may be expected not to increase (De Janvry and Sadoulet 2006), there have been some findings that suggest that the narrowness of CCT programs may not affect deeply rooted or structural inequality (Copestake 2008).

ductions in between-group disparities. This last statement implies that CCT programs may have an equalizing effect on opportunities between groups, which is precisely the definition of inequality of opportunity stated beforehand. Nevertheless, few studies have assessed CCT effects using this horizontal perspective; and except for some research by Handa et al. (2001), Soares et al. (2009), and Skoufias, Lindert, and Shapiro (2010), there remains a gap in the assessment of the distributional consequences of conditional cash transfers.

Therefore, these programs provide an ideal framework to test their effect on educational inequality of opportunities, since by promoting human capital investment CCTs have a long-term goal of equalizing opportunities. Moreover, another substantial advantage lies in their randomized design, which generates unique conditions to isolate the effect of the interventions from confounding factors and quantify the causal effect of these programs on inequality of opportunity.[11]

Note, however, that CCTs are only one in a series of social policies that either directly or indirectly contemplate an equal opportunity perspective. This article focuses solely on these interventions due to their growth throughout developing countries, especially in Latin America. Therefore, the analysis here presents only a partial picture. For a more comprehensive overview of other channels by which education may affect equality of opportunity, see Keane and Roemer (2009).

PROGRAMS, DATA, AND DEFINITIONS

The case studies I employ to assess the distributive effect of CCT programs on educational opportunities include Honduras's Programa de Asignación Familiar (PRAF), Mexico's Programa de Educación, Alimentación y Salud (PROGRESA), and Nicaragua's Red de Protección Social (RPS). All share the common characteristics of conditional cash transfers described in the previous section. Additionally, they were all rural interventions randomized at the village level and were relatively short-term interventions (one to three years) deployed around the turn of the past decade.[12]

In particular, the second phase of Honduras's PRAF began in 2000 and was designed to reach households in the poorest rural regions of the country.[13] The program incorporated both supply and demand incentives in its original design. Nevertheless, only the demand side was finally implemented in 40 villages, of which half received the transfer.[14] Mexico's PROGRESA was first deployed in rural areas in 1997. Since then, the intervention has quickly become the benchmark

11. See Duflo, Glennerster, and Kremer (2008) for additional benefits from randomized social experiments such as the ones used here.

12. See Alzúa, Cruces, and Ripani (2012) for a more thorough discussion on the similarities and differences between these programs.

13. The PRAF program began implementation in the early 1990s, mostly as a measure to mitigate the effects of macroeconomic adjustment policies on the extreme poor.

14. Glewwe and Olinto (2004) report that this failure was mostly due to administrative factors, among other issues.

CCT in Latin America and the largest social program in Mexico.[15] The analysis carried out here draws from its initial rural phase, which geographically targeted 506 villages, of which 320 were selected to receive the transfer and 186 served as control villages. Nicaragua's RPS conditional cash transfer program began in 2000. Its first phase consisted of a three-year pilot in the two poorest rural areas in Nicaragua. The program was deployed in 42 villages, half of which were randomly assigned to the treatment group.

All three programs encouraged the accumulation of human capital by providing cash transfers to households in treatment villages. However, there are two fundamental differences across interventions. First, only RPS provided additional supply-side incentives, since these were not implemented in PRAF and not contemplated during PROGRESA's initial phase. Second, PRAF and RPS focused on primary school attendance, while PROGRESA included incentives for children in secondary school. Therefore, the analysis below focuses on primary enrollment in an effort to maximize comparability, although service delivery and other particularities remain a distinguishing factor between the programs and their resulting effects (see Alzúa, Cruces, and Ripani 2012 for more on this matter).

The available data for these programs correspond to baseline and follow-up surveys in the targeted communities.[16] Each survey constitutes a representative sample of individuals in the selected villages, except in Mexico, where the information constitutes a census. Hence, in what follows, all estimates and statistics presented are calculated using the sampling weights provided with the data. All data sources include detailed information on a number of socioeconomic variables, circumstances, and educational outcomes for children of primary school age (defined as between ages six and twelve).[17]

Unfortunately, there are several limitations with the data. Certainly, to capture the entire educational distribution, it is necessary to study both access and quality. However, while information on school attendance is collected for each of the three programs, there is no common assessment in terms of other educational outcomes. Hence, the results are limited in that they capture only how CCTs change access to education and are unable to address other important factors in the educational debate, such as quality. However, with richer data this will certainly be an interesting direction for future research.

The raw data sets are processed using a predefined criterion in order to maximize comparability across the programs, in similar fashion to the procedure used

15. The program was renamed Oportunidades after nationwide expansion. See Handa and Davis (2006) for details on this expansion and the evaluation of the program after the rural phase.

16. The data for two of these programs are publicly available. Mexico's Secretaría de Desarrollo Social (SEDESOL) provides electronic data for PROGRESA's first phase online (http://www.oportunidades.gob.mx/EVALUACION/index.php). The International Food Policy Research Institute (IFPRI) provides the data for Nicaragua's RPS program (http://www.ifpri.org/dataset/nicaragua). Finally, the data for Honduras's PRAF is not publicly available but was obtained and used by permission of the IDB in the context of a joint project with CEDLAS (Alzúa, Cruces, and Ripani 2012).

17. A review of each country's educational system indicates that this age bracket is the standard length of primary schooling. (See the methodological guide in the Documents section of the Socio-Economic Database for Latin America and the Caribbean, http://sedlac.econo.unlp.edu.ar/eng/.)

by the Socio-Economic Database for Latin America and the Caribbean (SEDLAC) for national household surveys and that employed by Alzúa, Cruces, and Ripani (2012) for these same programs. In particular, the homogenization procedure begins by constructing longitudinal data sets for each intervention from the original data. Then the relevant variables are defined identically in each survey and for the same sample population, which makes results across interventions comparable since measurement is standardized across all data sets and the studied sample is also the same.

Once homogenized, the PRAF survey contains data for 3,227 children with complete enrollment information before program implementation in 2000 and again two years later in 2002. The survey for PROGRESA contains baseline information (1997–1998) for approximately 24,885 young children across three follow-ups six months apart (November 1998, March 1999, and November 1999). Finally, the data for Nicaragua's RPS contain a baseline (collected in 2000) and two follow-ups in 2001 and 2002, providing information for 2,038 children.[18]

Defining circumstance groups

The selected circumstance types are defined using several attributes considered plausibly exogenous. While debate continues regarding what exactly constitutes a circumstance (for discussion see Barros et al. 2009, chapter 1), I propose dividing the population by a series of attributes that seem as close as possible to being out of a child's control. Specifically, the selected circumstances are ethnicity, gender, parental education (defined as the maximum attainment of either the mother or father), and whether the child is born into a single-parent (or disintegrated) household.[19] These four characteristics are by no means exhaustive but constitute a relevant subset of all potential circumstances available in the surveys. Some elements in this set of circumstances are hard to object to, like gender or ethnicity, and the remaining characteristics also seem intuitively sound. While the inclusion of disintegrated backgrounds may seem somewhat dubious, there is substantial evidence that single-headed households are more vulnerable to poverty (Gindling and Oviedo 2008). In particular, Chant (1985) states that these households are thought to be worse off socially and economically, whether this is temporary or permanent.

Naturally, this selection implies leaving out other potential groupings. For instance, while previous studies have considered household income, number of siblings, and parental occupation as circumstances (Barros et al. 2009; Ferreira and Gignoux 2011), I focus on those that may be considered as most completely independent of the child. For different reasons, these three aspects do not fulfill these requirements. Income (or consumption) for example, may be modified during the prenatal period to account for an additional child. Fertility decisions are

18. The reported number of observations corresponds to unbalanced panels. Further refining the data to balanced panels reduces these numbers but does not significantly affect the estimates.

19. Ethnicity is only available in PROGRESA data and corresponds to classification by mother tongue.

Table 1 Distribution of children by circumstance type

Circumstance types	PRAF	PROGRESA	RPS
Ethnic group			
Indigenous	—	29.7	—
White/mestizo	—	70.3	—
Gender			
Girls	50.5	49.1	49.6
Boys	49.5	50.9	50.4
Parental education			
Less than primary	71.9	58.0	84.2
Primary complete	28.1	42.0	15.8
Household type			
Both parents present	82.3	90.4	86.1
Single parent	17.7	9.6	13.9

Sources: Author's calculations based on program surveys. The data for two of these programs are publicly available. Mexico's Secretaría de Desarrollo Social (SEDESOL) provides electronic data for PROGRESA's first phase online (http://www.oportunidades.gob.mx/EVALUACION/index.php). The International Food Policy Research Institute (IFPRI) provides the data for Nicaragua's RPS program (http://www.ifpri.org/dataset/nicaragua). The data for Honduras's PRAF is not publicly available but was obtained and used by permission of the IDB in the context of a joint project with CEDLAS (Alzúa, Cruces, and Ripani 2012).
Note: Estimates weighted using sampling weights provided with the data.

problematic since children tend to help as unemployed family workers from a young age (Schultz 2004). Finally, parental occupation is not considered because the majority of the adult workforce in these villages is employed in the agricultural sector, reducing variability.

In what follows, the circumstance variables are defined as binary indicators, with the value 1 identifying a child belonging to the disadvantaged group and 0 the advantaged group. Table 1 summarizes the empirical definitions for each circumstance type and the percentage of children who belong to each category.

In general, besides the somewhat equal gender distribution, the remaining groups are less balanced. For instance, more than half the children live in households where parents have low education. Additionally, only a small number of children live in a single-parent household (between 10 to 17 percent). From this distribution of children into circumstance types, it is possible to obtain insight on the targeting of each program, since the coverage level and beneficiary population were somewhat different in each case. For example, out of the three programs, RPS was targeted at individuals with less-advantaged circumstances, followed by PRAF and PROGRESA; the latter seems more balanced since it was the intervention with the largest beneficiary pool and the only one with nationwide coverage.

The changes in educational opportunities between these groups will be assessed before and after the implementation of the programs using impact evaluation methods that exploit the randomized assignment of the interventions. This framework will provide measures of mean impact by group and changes in between-type inequality. While there are some scalar measures of inequality of opportunity available in the literature, they have several disadvantages and do not provide additional benefits in this context, as I discuss below.

EMPIRICAL STRATEGY

The estimates of program effects on enrollment for each circumstance type will be obtained by difference in differences (DD), considered the best-suited estimation technique in the case of random assignment. This framework controls for preexisting differences among treatment and control groups that are not necessarily eliminated due to randomization. The DD models used here will take the following general form, where E_{ivt} is a binary variable that denotes if child i living in village v is attending school at time t, α_v captures differences between treatment and control villages, and λ_t controls for aggregate time trends:

$$E_{ivt} = \alpha_v + \lambda_t + \beta x_{vt} + \theta Z_{ivt} + u_{ivt} \tag{1}$$

The policy variables are the interaction of the treatment and time effects (x_{vt}) whose coefficient vector β provides the estimate of program impact in each period after exposure. Finally, Z_{ivt} is a matrix of individual-specific covariates and u_{ivt} is an individual-specific error assumed to be uncorrelated with all right-hand-side variables.

In this setup the estimates of vector β capture the effect of the program on school attendance. However, it is important to note that since assignment (and not participation) is random; the estimated parameters will actually capture the Intention to Treat (ITT) effect on the population of compliers as defined in Angrist, Imbens, and Rubin (1996).[20]

This regression framework will be used for two purposes. First, equation (1) will be estimated separately for advantaged (A) and disadvantaged (D) children in each group defined in table 1. This will provide evidence of whether the improvement due to the programs was higher for a particular type in each grouping, and provide an initial notion of changes in between-type inequality. Further, I look at the interaction of the group identifier for each type with the policy variables in a more traditional heterogeneous effects analysis (Djebbari and Smith 2008; Dammert 2009). This will help determine whether the difference in the parameter estimates between advantaged and disadvantaged groups is significant, providing a statistical test for the null hypothesis that the program affects both groups similarly.

20. However, in these programs there is indication that the differences between the ITT and the average treatment effect (ATE) are not large. For instance PROGRESA had a 97 percent compliance rate. While the other programs have lower compliance rates, these are not significantly lower than for the Mexican program. Therefore, in this case ITT estimates may approximate the ATE relatively well.

Estimation is carried out by ordinary least squares (OLS) and controls for individual fixed effects. Even while the main dependent variable is binary, linear models provide results at the conditional mean that do not differ substantially from marginal effects computed from binary regressions and require less restrictive assumptions (Angrist and Pischke 2009). Moreover, the equivalence between linear and nonlinear marginal effects on binary outcomes is expected to be even closer when using randomized data, attested by its use in most available studies using data from these programs. The standard errors are corrected to account for the program assignment at the village level (Donald and Lang 2007).[21] In this case, OLS also presents more advantages than traditional binary outcome models, since the variance estimation becomes more complex and does not necessarily provide efficiency gains.

It is important to mention that while there are some measures of inequality of opportunity available in the literature, they do not provide additional benefits to the estimates presented here. Moreover, their application in CCT settings is not straightforward. For instance, inequality of opportunity is usually measured on continuous variables such as income (Peragine 2004a, 2004b; Le Franc, Pistolesi, and Trannoy 2008, 2009; Ferreira and Gignoux 2011). Currently, there are fewer indicators for discrete ordinal variables such as enrollment. Among these measures we have the human opportunity index (HOI) in Barros et al. (2009) and a recent multidimensional dissimilarity measure in Yalonetzky (2012). However, while innovative, their use would not significantly contribute additional information to the proposed analysis.

Barros et al.'s (2009) HOI is usually estimated on cross-sectional data for binary outcomes such as enrollment and access to basic services. While the index is intuitive and relatively straightforward to implement, it has been subject to scrutiny due to its inability to fulfill certain desirable properties (Peragine 2011). In addition, the index quantifies both between and within inequality of opportunities without capacity for distinction, which is not compatible with the conceptual definition of equality of opportunity used here.

In contrast, the dissimilarity index (*D*) proposed by Yalonetzky (2012) overcomes many of the issues with the HOI. In particular, *D* is axiomatic and thus fulfills several desirable properties. Moreover, it was designed to quantify inequality of opportunity between groups, which makes it a proper fit with the conceptual framework. However, despite these benefits, there seem to be no outstanding gains from its use for a number of reasons. First, the values from the index are not interpretable. Therefore, the observed change would indicate the direction of program effect on inequality of opportunity but not what that change means or if it is economically significant. Second, the measure is biased toward zero unless there is substantial variation between the groups, which suggests that its computation provides substantially low values when inequalities between groups are small and the population mean is high (as is usually the case with primary school

21. Standard errors were also estimated using block bootstrap with 250 replications as proposed by Bertrand, Duflo, and Mullainathan (2004) but were omitted since the results did not vary significantly. These results are available on request.

attendance).[22] Finally, inference for this index is usually carried out using bootstrap methods, which requires making ad hoc distributional assumptions.

Therefore, the regressions will provide the main estimates for assessing CCT impact on educational opportunities. The estimates will provide the average gain in attendance for each circumstance group and help determine enrollment changes between groups due to the interventions, with their corresponding statistical tests of significance. Complemented with the initial and final distributions of enrollment, the findings will paint a picture of whether CCTs reduced inequality of opportunity by lowering between-type inequalities.

FINDINGS

Inequality of opportunity at the baseline

Table 2 presents mean enrollment rates for children aged six to twelve to characterize preprogram inequalities. Furthermore, the table also tests the hypothesis that baseline attendance rates between groups were equal.[23]

First, primary enrollment is relatively widespread in the villages, with an average attendance rate above 90 percent in PRAF and PROGRESA villages but only 70 percent in RPS localities. This is due to the latter's targeting of the two poorest areas in Nicaragua, which are worse off in all outcomes compared to the villages in the other two programs. Second, the overall enrollment distribution shows no significant differences between treatment and control groups, as expected due to the randomized nature of the programs.[24] However, there are some statistically significant differences when we consider advantaged and disadvantaged types, even in villages that have a high overall attendance rate. This implies that simple regressions by advantaged and disadvantaged groups would not be sufficient to determine the changes in attendance, since we would also need to control for group membership.[25]

Of the selected groupings, parental education seems the most relevant source of disparities in enrollment. The estimated differences between children in high and low education environments range from 2 percentage points in PROGRESA to more than 11 percentage points in RPS villages. The remaining circumstances also present some disparities, with boys having significantly higher enrollment in PROGRESA (around 1 percentage point higher).

These descriptive statistics reflect that even though primary enrollment is high, there is a visible level of educational inequality of opportunity in these poor areas, since enrollment levels reflect some group differences. Now the main ques-

22. The author acknowledges this fact and performs a monotonic transformation of the index by taking its square root (see Yalonetzky 2009, 2012). Nevertheless, this procedure is only useful when variation is substantial.

23. The mean tests are conducted by weighted regressions and correcting the variances to account for program assignment at the village level.

24. Some differences between treatment and control children may be found in the gender distribution in RPS, although the remaining partitions seem well balanced.

25. I would like to acknowledge an anonymous reviewer for pointing out this limitation with the approach.

Table 2 Between-type inequality in primary enrollment before program implementation

	PRAF		*PROGRESA*		*RPS*	
	Treatment	Control	Treatment	Control	Treatment	Control
All	92.5	90.5	96.5	96.2	69.4	70.8
Ethnic Group						
Indigenous	—	—	96.3	96.3	—	—
White/mestizo	—	—	96.6	96.2	—	—
Difference			−0.4	0.1		
Gender						
Girls	92.5	91.3	96.0	95.9	67.2	72.9
Boys	92.5	89.8	97.0	96.6	71.4	68.7
Difference	−0.1	1.5	−1.0***	−0.8*	−4.2	4.2*
Parental education						
Less than primary	90.6	89.3	96.1	95.7	66.5	67.9
Primary complete	97.2	94.5	98.1	98.0	82.8	79.8
Difference	−6.6***	−5.2***	−2.0***	−2.3***	−16.2**	−11.9**
Household type						
Both parents present	90.4	90.3	95.8	95.8	66.8	66.3
Single parent	92.9	90.6	96.6	96.3	69.8	71.6
Difference	−2.5	−0.3	−0.8	−0.5	−3.0	−5.3

Sources: Author's calculations based on program surveys. The data for two of these programs are publicly available. Mexico's Secretaría de Desarrollo Social (SEDESOL) provides electronic data for PROGRESA's first phase online (http://www.oportunidades.gob.mx/EVALUACION/index.php). The International Food Policy Research Institute (IFPRI) provides the data for Nicaragua's RPS program (http://www.ifpri.org/dataset/nicaragua). The data for Honduras's PRAF is not publicly available, but was obtained and used by permission of the IDB in the context of a joint project with CEDLAS (Alzúa, Cruces, and Ripani 2012).
Notes: Means tests carried out by regression with cluster robust standard errors at village level. Estimates weighted using sampling weights provided with the data.
*Significant at 10%; **Significant at 5%; ***Significant at 1%

tion becomes whether the programs improved this initial distribution toward a more equitable state where circumstances are less determinant of educational access.

Did the programs benefit disadvantaged groups?

As mentioned beforehand, all regressions are estimated by OLS controlling for individual fixed effects and using the sample weights included in the data. The base specification includes the following covariates: age of the household head, the number of young children aged 0–2 and 3–5 in the household, and the number of adult members aged 13–25, 26–39, 40–55, 56–69, and older than 70.

In general, all programs except PRAF show a statistically significant increase in overall attendance rates (table 3, column 1) and roughly coincide with the existing estimates in the literature for each of the interventions.[26]

The results by circumstance type categories (table 3, columns 2–5) also reflect a general picture of increasing enrollment. These regressions provide suggestive evidence that the disadvantaged groups identified in table 2—girls, ethnic minorities, and children from low educated backgrounds—seemed to benefit more from the interventions than the advantaged group. Some explanations behind the larger comparative gains for some of these groups are likely attributable to the worse initial conditions depicted earlier. For instance, some of these groups had lower attendance before the program than the corresponding advantaged groups; and therefore the estimates are a preliminary indication that perhaps between-group differences might be falling due to the observed larger improvement.

However, these regressions by group provide only suggestive evidence of falling inequality of opportunity. For instance, they omit the preexisting differences between treatment and control groups that may bias the estimated impact. Moreover, they do not capture whether the observed growth is statistically significant, and they have less power to capture differential gains because the number of observations falls rapidly by partitioning the sample. Therefore, the regressions are reestimated using all available observations and including an interaction between an identifier variable (which is unity when the child belongs to the disadvantaged group) and the policy variables to statistically test whether the higher observed improvement indicates that the program benefits disadvantaged groups, and to account for the baseline differences found beforehand. These results are presented in table 4.

The estimates confirm most of the suggestions derived beforehand. The exception is PRAF, where there is no evidence that disadvantaged children benefit more than advantaged children for any of the selected groupings. However, the overall estimates were also not significant, implying that the results capture the general inability of the program to affect enrollment. In particular, previous evaluations have acknowledged that this poor performance is associated with the low incentives granted to the beneficiaries in PRAF (Glewwe and Olinto 2004).

In PROGRESA, there is evidence of a more than average gain in enrollment for the ethnic minority (more than 1 percentage point every six months). Additionally, there does seem to be a higher relative improvement for children whose parents are less educated and for girls, although the effects are not immediate and suggest that the improvement of educational opportunities may take some time. There seem to be no differential effects by household type, implying that both groups benefit similarly from this CCT.

Finally, and not surprisingly, RPS represents the standout case, since all disad-

26. For instance, see Glewwe and Olinto (2004) and Moore (2008) for PRAF; Skoufias and Parker (2001), Schultz (2004), and Behrman, Parker, and Todd (2011) for PROGRESA; and Maluccio and Flores (2005) for RPS.

Table 3 Program effects on primary enrollment by circumstance group

		All	*Ethnicity*		*Gender*		*Parental education*		*Household type*	
			White/ mestizo	Indigenous	Boys	Girls	Primary complete	Less than primary	Both parents	Single parent
PRAF Baseline: Aug–Dec. 2000	ITT (May–Aug. 2002)	0.028	—	—	0.043	0.015	0.013	0.037	0.027	0.022
		(0.018)			(0.025)*	(0.019)	(0.033)	(0.023)	(0.019)	(0.036)
	Observations	6,484			3,214	3,270	1,548	4,280	5,269	1,215
	Groups	4,348			2,154	2,205	1,104	2,983	3,598	868
PROGRESA Baseline: Sept. 1997–Mar. 1998	ITT (Nov. 1998)	0.012	0.008	0.013	0.009	0.011	0.002	0.015	0.014	−0.002
		(0.004)***	(0.005)	(0.009)	(0.005)*	(0.006)*	(0.007)	(0.008)*	(0.004)***	(0.012)
	ITT (Mar. 1999)	0.015	0.017	0.019	0.013	0.012	0.002	0.012	0.016	0.004
		(0.005)***	(0.006)***	(0.008)**	(0.006)**	(0.006)*	(0.007)	(0.009)	(0.005)***	(0.014)
	ITT (Nov. 1999)	0.019	0.014	0.011	0.018	0.018	0.005	0.021	0.020	0.003
		(0.005)***	(0.006)**	(0.010)	(0.006)***	(0.007)***	(0.007)	(0.009)**	(0.004)***	(0.014)
	Observations	96,266	56,274	25,089	47,763	46,816	27,849	30,077	87,515	8,751
	Groups	35,065	29,096	18,096	27,448	27,254	19,925	21,580	31,684	3,577

(continued)

Table 3 (continued)

		All	*Ethnicity*		*Gender*		*Parental education*		*Household type*	
			White/ mestizo	Indigenous	Boys	Girls	Primary complete	Less than primary	Both parents	Single parent
RPS Baseline: Aug.–Sept. 2000	ITT (Oct. 2001)	0.209 (0.045)***	—	—	0.199 (0.050)***	0.221 (0.051)***	0.150 (0.067)**	0.209 (0.053)***	0.209 (0.047)***	0.217 (0.068)***
	ITT (Oct. 2002)	0.143 (0.056)**	—	—	0.137 (0.065)**	0.146 (0.060)**	0.068 (0.077)	0.146 (0.067)**	0.153 (0.057)**	0.079 (0.081)
	Observations	5,650			2,830	2,820	782	4,564	4,972	678
	Groups	2,585			1,298	1,287	387	2,094	2,267	318

Sources: Author's calculations based on program surveys. The data for two of these programs are publicly available. Mexico's Secretaría de Desarrollo Social (SEDESOL) provides electronic data for PROGRESA's first phase online (http://www.oportunidades.gob.mx/EVALUACION/index.php). The International Food Policy Research Institute (IFPRI) provides the data for Nicaragua's RPS program (http://www.ifpri.org/dataset/nicaragua). The data for Honduras's PRAF is not publicly available, but was obtained and used by permission of the IDB in the context of a joint project with CEDLAS (Alzúa, Cruces, and Ripani 2012).
Notes: Estimates weighted using sampling weights provided with the data.
Standard errors (in parentheses) clustered at village level.
*Significant at 10%; **Significant at 5%; ***Significant at 1%

Table 4 Changes in between-type inequality in primary enrollment

		Ethnic minority	Girls	Less-educated parents	Single parent
PRAF	ITT (May–Aug. 2002)	—	−0.006	0.016	0.002
Baseline: Aug.–Dec. 2000			(0.012)	(0.019)	(0.016)
	Observations		6,484	5,828	6,484
	Groups		4,348	3,960	4,348
PROGRESA	ITT (Nov. 1998)	0.013	0.004	0.005	−0.006
Baseline: Sept. 1997–Mar. 1998		(0.006)**	(0.004)	(0.005)	(0.006)
	ITT (Mar. 1999)	0.016	0.004	0.008	−0.010
		(0.005)***	(0.004)	(0.005)	(0.007)
	ITT (Nov. 1999)	0.016	0.011	0.010	−0.007
		(0.006)***	(0.004)***	(0.005)*	(0.008)
	Observations	81,363	94,579	57,926	96,266
	Groups	32,860	35,049	30,348	35,065
RPS	ITT (Oct. 2001)	—	0.144	0.191	0.133
Baseline: Aug.–Sept. 2000			(0.040)***	(0.051)***	(0.061)**
	ITT (Oct. 2002)	—	0.097	0.137	0.036
			(0.048)**	(0.056)**	(0.065)
	Observations		5,650	5,346	5,650
	Groups		2,585	2,440	2,585

Sources: Author's calculations based on program surveys. The data for two of these programs are publicly available. Mexico's Secretaría de Desarrollo Social (SEDESOL) provides electronic data for PROGRESA's first phase online (http://www.oportunidades.gob.mx/EVALUACION/index.php). The International Food Policy Research Institute (IFPRI) provides the data for Nicaragua's RPS program (http://www.ifpri.org/dataset/nicaragua). The data for Honduras's PRAF is not publicly available, but was obtained and used by permission of the IDB in the context of a joint project with CEDLAS (Alzúa, Cruces, and Ripani 2012).
Notes: Estimates weighted using sampling weights provided with the data.
Standard errors (in parentheses) clustered at village level.
Notes: The reported estimates correspond to the interaction between a binary variable that identifies the disadvantaged group (see definitions in Table 1) and the treatment variable.
*Significant at 10%; **Significant at 5%; ***Significant at 1%

vantaged groups benefit substantially more in terms of enrollment. Girls increase their enrollment by 14 percentage points in the first year and almost 10 in the second when compared to boys. Children of less-educated parents increase their attendance by similar values, and the picture is also completed by a higher relative improvement for children from single-parent households. However, we must recall that the initial conditions of children in RPS villages left much more room for improvement vis-à-vis the remaining interventions.

Therefore, these estimates indicate that CCT programs seem to benefit groups considered to be more disadvantaged in terms of primary enrollment. Combined with the descriptive results, which showed that most of these less-favored groups also had lower initial attendance rates, this would seem to support that inequality of opportunity in primary attendance fell in these villages.

Changes in between-type inequality

However, to reduce inequality of opportunity, these programs must not only benefit disadvantaged groups more but also create a more equal distribution. Therefore, to complete the analysis, it is essential to observe the changes in between-group inequality due to the programs. In table 5 I present the observed enrollment distribution at the last available follow-up for each of the programs. I select the final period mainly for simplicity and to obtain a view of the distribution at the end of program implementation.

In comparison to the baseline, there are significant changes in the treated population and virtually no change in the controls, an additional test for the validity of the randomization.[27] Overall, the enrollment distribution seems to have shifted to a more equitable state. For instance, while treated children with more highly educated parents had 6.6 percentage points higher attendance in PRAF at the baseline, this difference fell to 3.4 points at the end of the program. This is compatible with a reduction in the between-type enrollment gap of almost 48 percent for the treated. Moreover, the corresponding change in the control group is small (4 percent). Subtracting the latter, this suggests that between-group inequalities in enrollment fell by more than 40 percent, even in a program with limited effects such as the Honduran CCT.

In PROGRESA, enrollment differences between advantaged and disadvantaged groups become quite close to zero in the final period. RPS presents a similar case as the Honduran program, with between-type inequality falling by more than 50 percent. However, while there is a reduction in the level of group inequality, in most programs the difference between advantaged and disadvantaged types remains significant, implying that while the programs reduce inequality of opportunity, they do not eliminate it.

The effects on enrollment distribution for the other groupings present similar findings. In general, the difference in attendance rates between the types fell for children living in treatment villages compared to control children. There is some

27. There are some findings consistent with spillovers, although in general, the enrollment distribution seems unaffected for those who resided in control villages.

Table 5 Between-type inequality in primary enrollment at final follow-up

	PRAF		*PROGRESA*		*RPS*	
	Treatment	Control	Treatment	Control	Treatment	Control
All	96.5	92.3	97.6	96.2	90.2	79.9
Ethnic group						
Indigenous	—	—	97.3	96.0	—	—
White/mestizo	—	—	97.6	96.4	—	—
Difference	—	—	−0.3	−0.4	—	—
Gender						
Girls	96.1	92.1	97.8	96.2	89.1	82.6
Boys	96.9	92.4	97.3	96.2	91.3	77.2
Difference	−0.8	−0.3	0.5**	0.0	−2.1	5.4**
Parental education						
Less than primary	95.6	90.6	97.9	96.5	89.1	77.8
Primary complete	98.9	95.5	97.2	96.1	95.6	90.3
Difference	−3.4***	−4.8***	0.6**	0.4	−6.5**	−12.5***
Household type						
Both parents	95.0	93.5	96.0	94.5	91.3	79.1
Single parent	96.8	92.0	97.7	96.3	90.0	80.0
Difference	−1.8	1.5	−1.7**	−1.8***	1.3	−0.9

Sources: Author's calculations based on program surveys. The data for two of these programs are publicly available. Mexico's Secretaría de Desarrollo Social (SEDESOL) provides electronic data for PROGRESA's first phase online (http://www.oportunidades.gob.mx/EVALUACION/index.php). The International Food Policy Research Institute (IFPRI) provides the data for Nicaragua's RPS program (http://www.ifpri.org/dataset/nicaragua). The data for Honduras's PRAF is not publicly available, but was obtained and used by permission of the IDB in the context of a joint project with CEDLAS (Alzúa, Cruces, and Ripani 2012).
Notes: Means tests carried out by regression with cluster robust standard errors at village level. Estimates weighted using sampling weights provided with the data.
*Significant at 10%; **Significant at 5%; ***Significant at 1%

minor evidence of spillovers, although a full assessment of this point lies beyond the analysis here.

Therefore, while the econometric estimates showed some differences in program impact across advantaged and disadvantaged groups, there is clear evidence that the enrollment distribution between these types became more equal due to the programs. Hence, there seems to be equalization in educational opportunities for the beneficiary population, since between-group inequality falls and therefore, so does inequality of opportunity.

DISCUSSION

This article has studied the effect of CCT programs on primary educational opportunities for three programs in Latin America. The findings contribute to the development literature by providing further evidence of these programs' impact by measuring changes in between-group inequalities that capture a horizontal dimension of inequality of opportunity.

The findings indicate that CCT programs seem to differentially favor disadvantaged groups. These results are further reinforced by an observed improvement in the enrollment distribution between groups. However, while there is evidence that inequality of opportunity is decreasing, it is not eliminated. Nevertheless, in addition to their widely documented gains, these programs may be useful tools to reduce vulnerability for future generations and perhaps even address structural inequalities and the proliferation of inequality traps (Bourguignon, Ferreira, and Walton 2007).

To conclude, some caveats are in order. While these findings are illustrative, they also present some limitations and pose new research questions. The approach used here is neutral to inequality of opportunity within groups, which leaves room for additional assessment of CCT impact on this type of inequality. Additionally, further work may focus on the changes in between-type inequality in other outcomes such as secondary enrollment, health and nutrition, and labor supply, which may grant a more comprehensive overview of the distributive effects of these programs. Moreover, the analysis here only looks at one aspect of educational distribution: access to education. It remains myopic to other important concerns in education such as quality. Finally, data for more time periods might show whether the reduction in group inequalities has continued to drop and whether the findings presented here translate into a more equal distribution of income, which is perhaps the ultimate objective of equalizing opportunities.

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