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The Effect of Conditional Cash Transfers on Educational Opportunities: Experimental Evidence from Latin America

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Abstract

Conditional Cash Transfers (CCTs) provide income to the poor in an effort to improve current welfare and promote investment in human and social capital to prevent future deprivation. So far, the impact evaluation literature has focused on estimating current effects on outcomes such as school attendance, consumption and labor supply. However, these studies overlook potential redistributive effects, mainly via the equalization of opportunities. The ensuing analysis draws from recent contributions in the literature on opportunities and incorporates these with impact evaluation methods. The main findings indicate a remarkable redistributive effect of CCTs and a positive initial impact on opportunities. However, while mean outcomes improve markedly, the evidence suggests that the distribution of opportunities readjusts to the positive gains, perhaps indicating deeply rooted inequities. These results are expected to encourage discussion on program impact beyond those evaluated and addressing the programs' long-term consequences.

JEL Classification: D30, D63, I38, J22

Key words: poverty, opportunities, education, children, impact evaluation, conditional cash transfers

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1. Introduction

Conditional Cash Transfers (CCTs) have become a standard for poverty alleviation in developing countries. These programs are two-tiered, since they provide income transfers which lift households above the poverty line coupled with incentives to invest in human and social capital to break the cycle of poverty. In particular, CCTs are appealing due to their targeted approach, short- and long-run objectives, simple benefit structure and randomized design which facilitates measuring their impact.

The availability of data from CCT programs has led to extensive work on estimating the current (or short-run) effects of these interventions on outcomes such as consumption, education, health, infant mortality and time allocation[1]. The approaches (to name a few) have ranged from simple estimation of average treatment effects, addressed heterogeneous responses and spillovers to the non-eligible population. However, while the existing body of work has provided exhaustive evidence on the short-run gains from the programs, it has not evaluated its structural consequences. For instance, do CCTs improve opportunities for its beneficiaries? Moreover, is there hope for equal opportunities for future generations?

Intuitively, if these programs improve opportunities, CCTs would contribute to creating a fairer society; since all differences in outcomes which stem from circumstances would disappear. In this scenario, the gains from the program would have mainly positive effects. However, persistent inequality is not easily eliminated, especially if these inequities are inherited and deeply rooted. The research presented here draws from this theoretical underpinning to evaluate program effects on opportunities, seeking to capture any evidence of structural effects of CCTs, and any other behavioral aspects associated with their impact.

Nevertheless, to fully answer the above questions it is imperative to measure opportunities. Therefore, this paper uses recent contributions in the measurement of the distribution of opportunities by Ferreira and Gignoux (2008) and Barros, Molina and Saavedra (2008). This framework allows obtaining estimates of cross-sectional inequality of opportunities for any given outcome. In particular, the discussion focuses on *educational* opportunities due to their documented relationship with mobility as a channel to improve welfare (Breen and Jonsson, 2005). The main

[1] For instance in Latin America, evaluations of CCT programs include Parker and Skoufias (2001), Gertler (2004), Schultz (2004) for Mexico; Cruces *et al.* (2008) and Cruces and Gasparini (2008) for Argentina; Bourguignon *et al.* (2003) for Brazil; Attanasio *et al.* (2005) for Colombia; Carrillo and Ponce (2008) for Ecuador; Larrañaga *et al.* (2008) for Chile; Jones *et al.* (2008) for Perú, Glewwe and Olinto (2004) for Honduras, Maluccio and Flores (2004) for Nicaragua; and Levy and Ohls (2007) for Jamaica.

outcome variable in the study is the probability of completing primary school on time for children aged 6-16 years. This measure is a useful proxy of schooling quality, since in an acceptable educational system children are able to promote each grade without difficulty or extremely low marks. Also, it is quite feasible to conceive that in an equal opportunity setting all children would have a similar probability of completing primary.

The findings from the analysis provide insight into two aspects. The first is whether these policies induce an improvement in the distribution of opportunities for children. These estimates are analogous to existing short-run effects since they focus on the mean, although an important difference is that they are defined in the opportunity space instead of the outcome space. The second consists in evaluating whether there is equalization in the distribution of those opportunities or if inequality persists. However, it is important to note that while the ensuing analysis provides evidence on the direction and magnitude of these effects, it does not seek to explore the determinants of such behavior. Notwithstanding, this would present a promising line for future research.

The empirical estimates are drawn from three CCT programs in Latin America[2]. These are: Mexico's *Programa de Educación, Alimentación y Salud*, PROGRESA; Honduras' second phase of the *Programa de Asignación Familiar*, PRAF-II; and Nicaragua's *Red de Protección Social*, RPS. Longitudinal data is available for all cases, and each survey includes extensive information on family background and socioeconomic information about the respondents. All surveys are homogenized to ensure comparability, which highlights (dis)advantages of each particular case, and constitutes an additional empirical contribution of this study.

The rest of this paper is organized as follows. The next section begins by reviewing the literature on inequality of opportunities, highlighting its policy relevance and explaining why CCTs provide a unique framework to evaluate the impact of these policies on the opportunity distribution. Section 3 tackles the measurement issue, by presenting the estimation framework for opportunities and the impact evaluation methods used here. Finally, Section 4 presents the empirical results and Section 5 concludes.

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[2] Latin America is one of the regions with the most countries providing CCTs in 2008 with 17 implemented programs (Fizbein and Schady, 2009), a coverage rate of 57 per cent.

2. Literature Review

Inequality, contrary to deprivation, has been more controversial when it comes to designing policy. Nevertheless, a number of potential gains may be drawn from reducing inequality, especially that which derives from unfair sources such as gender, birthplace or negative outcomes from past generations. This last statement makes the direct assumption that observed inequality in outcomes may emanate from a variety of sources, and hence, it is essential to identify them. This section briefly reviews the normative literature which decomposes inequality into meritocratic and circumstance components. The discussion is policy-oriented, emphasizing on how a targeted egalitarian policy may generate a fairer society and induce a virtuous cycle of development.

2.1 Inequality in outcomes and Inequality of opportunities

Inequality, like welfare and deprivation, has also been studied as a multidimensional concept (see Savaglio, 2002, for a survey), with perhaps the most well-known strand of studies being that which decomposes inequality into two components: factors controlled by the individual (or effort) and exogenous circumstances, an approach known as *(in)equality of opportunities*. The seminal contribution in this literature is Roemer (1998), who argues that differences which derive from factors beyond individual control are unfair and that all differences should arise solely from differences in efforts[3]. In fact, in his definition of a totally equitable world, inequality still exists; however it surfaces from the effort allocation for each individual.

The equality of opportunity approach argues that individuals should face a "level playing field" before deciding the amount of effort which maximizes their well-being. However, it may also be that effort depends on circumstances, which may explain in part why some groups are consistently worse-off. Hence, this approach argues that the sources of these inequalities matter, especially for policymaking since government efforts are directed at improving the living conditions of its population. Therefore, reducing inequality of opportunities seems more in line with this notion of social justice and fairness; while reducing meritocratic inequality does not. A simple example may help understand this point.

[3] An intense philosophical debate exists with respect to fairness and inequality, which lies beyond the objectives of this paper and precedes purely economic evidence. See Rawls (1974), Dworkin (1981a and 1981b), Arneson (1989), LeGrand (1991), Thomson (1994), Phelan (2002), Sen (1992, 2000a and 2000b) and Fleurbaey (2008).

2.2 Equality of what? Potential policy implications of equal opportunity policies

Imagine two scenarios of high inequality: on the one hand, consider a country where differences are due to family origin, gender, ethnicity or other factors which are beyond individual control. On the other hand, picture a country where disparities are due exclusively to varying levels of effort.

In this example both countries have a high level of inequality, but the source of these disparities is vastly different. The first case represents a society with inequality of opportunities, where individuals face a certain choice set based on the circumstances they are born into and outcomes are solely dependent on these inherited characteristics. The second case corresponds to a society where inequality is merit-based, in which circumstances are not relevant and all differences in outcome distributions are due to differing levels of effort.

For the society with meritocratic inequality, an egalitarian policy would reduce inequality, but it would eliminate differences from individual effort. In lieu with the above discussion, this would eliminate disparities considered fair, contradicting the view of justice on which the discussion here is based. For the society with unequal opportunities, equity-enhancing policies would have several effects. On the one hand, there is a direct effect that equalizing opportunities today by definition improves the outcomes of the next generation, since their outcomes are less dependent on the circumstances they will be born into. On the other hand, improving the distribution of opportunities may also serve as a catalyst for development by fostering a virtuous circle. Analog to the virtuous circles described in Perry *et al.* (2006), equal opportunities may lead to enhanced mobility, where efforts are encouraged since all individuals have the same opportunity set. These conditions may in turn enhance growth due to an increase in output and productivity. In turn, the potential gains to growth now have higher potential to raise individuals out of poverty and improve well-being since there is a more egalitarian opportunity and outcome distribution.

This example may be more useful if framed from a starting point in which educational opportunities are equalized. Intuitively, if some policy improves opportunities in this dimension, this intervention would contribute to creating a fairer society; since all differences in schooling outcomes which stem from circumstances would disappear. This wider availability of opportunities is expected to generate a higher average level of human capital (Mejía and St. Pierre, 2008), which facilitates mobility (Breen and Jonsson, 2005). Finally, since education is related positively with welfare, this has a growth effect via wages and labor income and ultimately, on aggregate growth.

Both examples indicate that the main difference between egalitarian policies which equalize outcomes and those which equalize opportunities is mainly temporal. For instance, while the first may yield important short-run benefits, there is less encouragement to adopt such a policy where efforts are the source of differences in outcomes. In the case of an equal opportunity policy, the effects are expected to show in the long-run, and may have more beneficial effects on future generations and less so in the immediate future.

2.3 Conditional cash transfers: Accidental equal opportunity policies?

In recent years, there has been an increasing trend in creating development policies which jointly consider improving current outcomes and eradicating structural aspects of deprivation with a strong egalitarian component. These interventions are known as conditional cash transfers, social programs which provide income transfers to the poor in an effort to improve current welfare and promote investment in human and social capital to prevent future deprivation. This coincides quite well with the type of policies discussed above as equal opportunity policies. As an additional advantage, these programs are usually devised as randomized trials, which facilitate evaluation of program impact on many dimensions. In the present case, they provide a unique framework to assess the impact of such policies on equality of opportunities.

In particular, a CCT program is defined by 5 aspects (Fizbein and Schady, 2009); first, they are targeted programs, i.e. they have a distinct beneficiary population in mind. The usual scheme involves geographic targeting, focusing on the poorest communities. Second, CCTs are generally designed to have a simple benefit structure, based on the number of children in beneficiary households and their age. Third, as their name indicates, these interventions transfer an amount of income to households conditional to fulfilling certain requirements. In most cases, these correspond to sending school-age children to educational centers and periodical 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 –health, education, finance, auditing- and also across government levels –national, municipal and communal-.

Their main objectives are: (i) reducing current poverty and (ii) promoting investment in human and social capital to prevent future deprivation. The first objective is clearly directed at short-term effects as an aid to lift individuals above the poverty line. However, the second objective seeks to prevent future poverty by fostering human capital accumulation; mainly in education, health and nutrition. Therefore, the second objective of CCTs could be reinterpreted as indirectly aiming to improve opportunities in an effort to reduce vulnerability to future deprivation.

In addition to the incorporation of opportunity concerns in their objectives, CCTs also provide an ideal framework to empirically assess how opportunities affect their beneficiaries. The randomized nature of these programs identifies treatment and control groups which are similar in observable and unobservable characteristics. Hence, comparison of the opportunity distributions for both groups simplifies estimating the effect attributable to the program.

3. Methodology and estimation framework

This section describes the methodology used to measure opportunities and how these techniques are employed jointly with impact evaluation methods to obtain the estimates of program effects on opportunities presented below.

3.1 The Measurement of Inequality of Opportunities

Inequality of opportunities was defined as the source of unequal outcomes which stems from circumstances beyond individual control. Formally, let C denote circumstances and E represent efforts; then a general population model for any given outcome may be written as:

$$y = f(C, E, u) \tag{1}$$

where u represents all unobservable factors.

Roemer (1998) suggests that the vector E should include some measure of individual effort applicable to the outcome of interest (e.g. hours of study for education) and the vector C should contain variables which are completely out of individual control (e.g. gender). Both determinants should be uncorrelated with u. However, Ferreira and Gignoux (2008) point out that this exogeneity assumption allows efforts to be endogenous to circumstances. In such a case, someone's race or their family background may affect the allocation of effort across the individual's life, leading circumstances to affect outcomes through both direct and indirect channels. Rewriting (1) to incorporate this fact, then outcomes are determined by the following general model:

$$y = f[C, E(C, v), u] \tag{2}$$

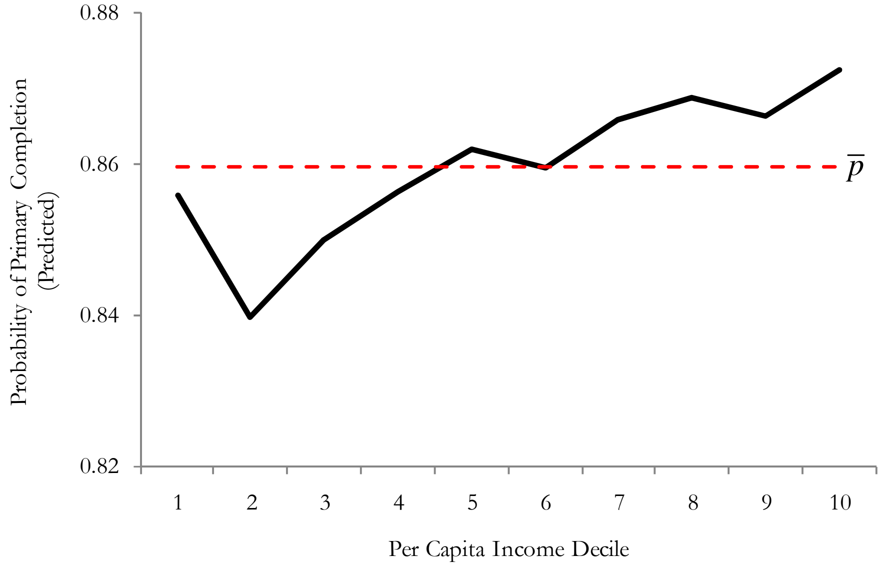
However, since inequality is a distributional characteristic, the entire distribution needs to be evaluated. Denote $F(\cdot)$ as a standard and well-behaved cumulative distribution function. Then, equality of opportunities exists when $F(y \mid C) = F(y)$[4], i.e. when circumstances are not relevant for the outcome of interest. From the above derivation, this implies that in a case of perfect equality any element of C has no direct effect on outcomes and no indirect effect via the decisions individuals make about their level of effort. Hence, all inequality comes from its meritocratic component, which is Roemer's ideal case.

So, a natural way to assess the existence of inequality of opportunity is to quantify the length to which $F(y \mid C) \neq F(y)$. The contributions in this direction may be classified into two strains: those which evaluate the entire distribution and those who compute scalar-based indexes. The first focuses on measuring stochastic dominance in the distribution of outcomes (O'Neill, 2000; Goux and Maurin, 2002; and LeFranc *et al.*, 2005, 2006), through non-parametric estimates of outcome distributions. This is usually implemented when the distribution being evaluated is continuous, such as income or consumption. However, the main caveat of this methodology is that it does not allow quantification of distances between groups. The second group of studies estimates inequality of opportunities with scalar-based indices for both continuous and dichotomous outcomes. In particular, this last attribute is pivotal since the outcome studied in this paper is a probability (dichotomous). Hence, this last approach is used in the empirical estimates.

Inequality of opportunity for discrete outcomes may be measured by observing dissimilarity in access rates and measuring distances from the population mean. A visual example is useful to capture this notion. Figure 1 presents the estimated probability of children 13-16 of finishing primary school on time in PROGRESA communities by income deciles[5] (baseline estimates).

[4] This condition implies three additional corollaries, which are described by Ferreira and Gignoux (2008) in detail.
[5] This figure is analog to Figure 2 in Barros *et al.* (2009). The age cut-off of 13 years is selected since it coincides with the expected age by the Mexican government by which a primary degree should be achieved.

Figure 1. Probability of completing primary schooling on time for children 13-16 in PROGRESA communities



Source: Own calculations on PROGRESA baseline survey

As may be seen in the Figure, probabilities vary across income groups, i.e. they are unequally distributed. Children in households with low income are below the population mean (denoted by $\overline{p}$), while those in the upper part of the income distribution have higher probabilities than the mean. For the sake of argument, assume that in this example household income is exogenous to the child. In order for equality of opportunity to exist, these differences should not be present. In fact, in a totally equitable world, all children would have the same probability of completing primary on time. In this case, educational opportunities would have a degenerate distribution at $\overline{p}$.

Thus, a natural measure of inequality of opportunity is to quantify the gaps in Figure 1 in order to achieve this equal distribution. In a sense, this measures how segregated a particular outcome is with respect to a set of exogenous characteristics. The Dissimilarity Index - a widely used tool in sociology- quantifies these distances by adding the probability gaps in outcomes with respect to the mean. Therefore, it summarizes the total distance of all groups with respect to the population mean. Formally, the D-Index takes the following form[6]:

$$D = \frac{1}{2\overline{p}} \sum_{i=1}^{m} \beta_i \left| p_i - \overline{p} \right| \qquad (3)$$

[6] A complete formal derivation is beyond the scope of this paper. However, the complete analytical proof may be found in Barros, Molina and Saavedra (2008).

where the term $\left|p_i - \overline{p}\right|$, captures the distances with respect to the mean and β_i are population weights (usually $1/N$). For ease of interpretation, the distances are normalized to range in a closed $[0,1]$ interval. The D-Index may be interpreted as the percentage of all available opportunities which need to be distributed from better-off groups to those that are worse-off to achieve an equal distribution of that particular outcome. In a situation of perfect equality of opportunities, *D=0*. In terms of Figure 1, the result indicates what fraction of opportunities must be reallocated in order to achieve a distribution which coincides with the horizontal line. This measure allows cross-sectional estimates of inequality of opportunity and through time if information is available, as well as computing standard errors for *D*[7].

Nevertheless, it is important to note that this indicator only accounts for dispersion in the opportunities in any given outcome. To fully assess the impact on opportunities (and account as much as possible for general equilibrium effects) it is also imperative to look at the mean. For instance, in Figure 1, the mean probability may improve due to a policy. However, it may be that the gaps remain unchanged or in the direst case, grow larger. Therefore, in order to assert a positive effect of programs on equality of opportunity in education requires that both the mean and the D-Index improve. Therefore in the estimates presented in Section 4, program effects are estimated both on the average probability of primary completion as well as on its variability to fully comprehend the effects of the programs on educational opportunities.

3.2 Impact Evaluation Methods

For the estimation of program effects, Difference in Differences (DD) appears as the most well-suited estimation technique. This is due to several reasons. First, the programs were devised as social experiments, with randomly assigned participation offers at the village level. This research design guarantees that the control group (no transfer) is comparable to the treatment group (transfer recipients) in both observable and unobservable characteristics. For distributional characteristics, program effects may be calculated by subtracting the gains of the treatment group from the comparable control group, a simple DD exercise. For individual-level outcomes regression approaches are more common, since they allow controlling for otherwise unobserved individual-level heterogeneity using linear fixed effects models and ensure that any flaw in randomization may be controlled if sufficient information is available.

[7] The variance estimator for *D* is described in the Appendix.

The regression DD models used here take the following form, with Y_{ivt} denoting the outcome variable of interest for individual *i* who lives in village *v* during time *t*. I_{sv} is a binary variable which denotes if the individual resides in a treatment village.

$$Y_{ivt} = A_s + B_t + cX_{ivt} + \beta I_{vt} + u_{ivt} \quad (4)$$

A_s and B_t are group and time effects, respectively, X_{ivt} is a matrix of individual covariates and u_{ivt} is an error term not correlated with any explanatory variable. The estimated effect of the program is β, which represents the average treatment effect (ATE). However, since program assignment (and not participation) is randomly allocated then the causal effect estimated in (4) is only valid for compliers. Hence, estimates of β in this study are actually the Intention to Treat (ITT) effect, as defined by Angrist, Imbens and Rubin (1996)[8].

The estimation is carried out by OLS with individual fixed effects even though all the dependent variables are binary outcomes (enrollment, employment, and the probability of primary completion). As pointed out by Angrist and Pischke (2009), these linear probability models generate results that do not differ substantially from probit or logit regressions. Moreover, β in a linear model has a straightforward causal interpretation, unlike the same parameter in a non-linear specification.

Finally, the standard errors in the estimations account for the structure of the program assignment. In particular, since randomization occurs at village level (*v*) instead of individual or household level (*i*), DD standard errors should account for potential intra-cluster correlation or the causal effects obtained from (4) could be potentially biased (see Bertrand, Duflo and Mullainathan, 2004; and Donald and Lang, 2007). Omission of these correlations would lead to inconsistent standard errors and ultimately, erroneous conclusions with respect to the effects of the program. In what follows, two corrections are applied (as in Alzúa, Cruces and Ripani, 2009). First, serial correlation of outcomes is addressed by estimating cluster-robust errors, CRVE; and second, a non-parametric alternative is also employed by block-bootstrapping CRVE errors.

[8] In some cases, the difference between both parameters is negligible, e.g. PROGRESA had a 97% compliance rate. In this example, the local effect is almost identical to the ATE. The other programs have lower compliance rates, but not significantly lower than for the Mexican program. Therefore, in what follows we assume that ITT estimates approximate ATE relatively well.

4. Empirical Application

4.1 Program description

While most conditional cash transfer programs share many characteristics, several aspects remain country-specific. Therefore, before any estimates are presented, this sub-section briefly describes each program and highlights certain specificities in their educational components[9].

The *Programa de Asignación Familiar* was created by the Government of Honduras in the early 1990s to mitigate the impact of macroeconomic adjustments on the poor and alleviate structural poverty. Its second phase (PRAF-II) was reorganized as a CCT designed to reach approximately 47,000 households in the poorest regions of the country. The program incorporated both supply and demand incentives in its educational component. Nevertheless, only the demand side was finally implemented[10]. Hence, the estimates below correspond to the 40 randomly selected municipalities in which demand incentives were deployed.

The Mexican *Programa de Educación, Alimentación y Salud* (PROGRESA) was first implemented in rural areas during 1997. Since then, the program has quickly become the benchmark CCT program in Latin America and the largest poverty alleviation program in Mexico[11]. The analysis here uses data from the initial rural pilot, which geographically targeted 506 villages of which 320 were selected to receive PROGRESA and 186 to serve as control villages. The educational component included demand incentives to increase primary and secondary enrollment and no supply incentives.

The Nicaraguan *Red de Protección Social* (RPS) conditional cash transfer began in 2000. A first phase consisted of a three-year pilot in two rural areas of the central region of Nicaragua (Madriz and Matagalpa) who had the highest poverty rates in the country. The program's educational component provides demand and supply efforts to improve education in the 42 villages which were selected for the pilot. One half of those localities were randomly assigned to the treatment group, and the other half did not receive the transfer.

In general, all three programs have a strong educational component. However, there are two fundamental differences. First, only RPS contains supply-side incentives since these were not implemented in PRAF-II and not considered in PROGRESA. Second, PRAF-II and RPS only

[9] A more general (and extended) description of all three programs may be found in Alzúa, Cruces and Ham (2010).
[10] Glewwe and Olinto (2004) reported that this failure was due to administrative factors and other issues.
[11] 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 initial rural deployment.

encourage primary schooling while PROGRESA also focuses on secondary schooling. These particularities should be considered when drawing conclusions from the estimates below.

4.2 Data

The surveys used in this study correspond to the baseline and follow-up surveys for each program. The data contained in each survey is representative of the targeted communities (except for Mexico where it covers all villages) and includes detailed information on socioeconomic characteristics, circumstances and outcomes for children. The dataset is structured into panels at the individual level, which were processed to achieve maximum comparability between programs and originate in Alzúa, Cruces and Ripani (2009).

Specifically, Honduras' PRAF-II survey gathers data for 9,592 children before program implementation in 2000 and again two years later (2002). The survey for Mexico's PROGRESA contains baseline information (1997-1998) for approximately 38,625 children across three follow-up time periods (Nov. 1998, Mar. 1999 and Nov. 1999). Finally, the data for Nicaragua's RPS comprises a baseline (2000) and two follow-ups in 2001 and 2002, providing information for 3,131 children.

4.3 Defining Circumstances and Outcomes

The formal framework developed in Section 3 formally defined the vector of circumstance variables, denoted by $\boldsymbol{C}$. Nonetheless, what measurable elements exist in the data that may be included in this set of "uncontrollable" givens? Moreover, how may we guarantee that they are actually exogenous?

The first question is still subject to much debate (see Chapter 1 of Barros *et al.*, 2009, for a thorough discussion), and is the first that will be answered. Since the population of interest is children in primary attendance age (6-16), this study defines as circumstances: (i) Ethnicity[12], (ii) Gender, (iii) Mother's Level of Education, (iv) Father's Level of Education, and (v) whether the child is born into a single-parent household. These five characteristics are by no means exhaustive, but constitute a relevant subset of all potential circumstances and respond to the available information in all surveys.

Previous studies have considered additional characteristics (e.g. household income, number of siblings and parental occupation) as exogenous. However, focus here lies on those which are considered as close as possible to completely beyond control of the child. For different reasons,

[12] Ethnicity is only available in PROGRESA.

these three conditions may not be entirely exogenous. Income (or consumption) for example, may be modified during the pre-natal period to account for an additional child. Fertility decisions are also not considered completely given since children tend to serve as unemployed family workers from a young age as Schultz (2004) argues. Finally, parental occupation is not considered because on average, more than 90% of adults are occupied in the agricultural sector due to the targeting of each program.

The answer to the second question is less evident, since there is no formal test for exogeneity. Some elements in this set of circumstances are hard to object, like gender or ethnicity; while the remaining characteristics also seem intuitively sound. It is plausible that educational outcomes of a child's parents and household composition are independent from the child at birth, but once again this statement is not testable.

In what follows, the circumstance variables are defined as binary indicators, with the value 1 identifying whether the child belongs to the "least advantaged" group and 0 otherwise. For instance, ethnicity is zero for those belonging to the major ethnic group, and unity for the minority. Table 1 summarizes the empirical definitions for each circumstance type and presents the percentage of children who belong to each category.

The outcome variable of interest is the probability of completing primary school on time. This is the same as used in Barros, Molina and Saavedra (2008). This measure is a useful proxy of schooling quality, since in an acceptable educational system; children are able to promote each grade without difficulty or extremely low marks. Also, it is quite feasible to conceive that in an equal opportunity setting all children would have a similar probability of completing primary. Information on the outcome variable is available for all surveys and time periods except for PROGRESA's second follow-up survey (Mar. 1999), which does not enquire about the variables needed to construct the estimated probabilities and is thus left out.

The rest of the section employs the methodology outlined in Section 3 to measure the impact of CCTs on educational opportunities. To begin, comparable results are presented for program effects on enrollment and child labor by circumstance type. This analysis is included for two purposes. On the one hand, it provides a descriptive analysis of the programs' effect on the schooling-labor choice of children. These estimates are a first approximation to assess the effect on opportunities by observing how this decision is affected. On the other hand, these results provide comparable estimates of treatment effects by circumstance type, allowing to draw conclusions on whether the program equalizes outcomes (i.e. favors the least advantaged group). The section concludes by analyzing the initial state of inequality of opportunities and then presenting estimates of program impact using DD approaches.

4.4 A baseline: program effects on the schooling-labor choice of children

Table 2 presents baseline statistics for enrollment and child labor in each country. Means tests show that outcomes are significantly different when comparing circumstance groups,[13] especially with respect to child labor. Before the programs, all five circumstances seem affect the schooling-labor choice of children. For instance, boys work significantly more than girls, parental education seems to be correlated positively with school enrollment and inversely with labor status, and children in single-parent households are less prone to be enrolled in school.

How did the program change this initial distribution? Program effects are estimated accounting for individual fixed effects on unbalanced data for each program as outlined in the methodological section. The models include children's demographic characteristics such as age (and its square); as well as household composition variables including the number of children 0-2 and 3-5 in the household and adults members aged 17-25, 26-39, 40-55, 56-69 and older than 70. Finally, the regressions also include the age of the household head. Table 3 presents DD estimates for school enrollment for children aged 6-16. In general, all programs show a statistically significant increase in overall attendance rates. Looking at results by circumstance type, enrollment increased significantly for disadvantaged groups (e.g. girls and children in low educated environments). Program effects on child labor are presented in Table 4 and show a decrease, except in Honduras where there is no effect. These findings coincide quite accurately with the existing empirical literature for the three programs, where estimates of program impact are similar or identical[14].

This evidence indicates that most programs affected the schooling-labor choice of children by managing to keep children in school and out of the labor market. An interesting additional fact is CCTs seem to have a large redistributive effect, since the evidence shows that improvements are more substantial for vulnerable segments of the population. Hence, these estimates indicate that enrollment is up and child employment is down, and more so for the least advantaged. However, does this shift in the schooling-labor choice of young children also impact on the distribution of educational opportunities (as measured by the defined outcome variable)?

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[13] Means tests are conducted via regression, with clustered standard errors at the locality level.
[14] For instance, see Glewwe and Olinto (2004) for PRAF-II; Behrman, Sengupta and Todd (2001), Parker and Skoufias (2001) and Schultz (2004) for PROGRESA; and Maluccio and Flores (2004) for RPS.

4.5 Inequality of Opportunities at the baseline

As an initial approach, it would be ideal to observe how each circumstance affects opportunities before program implementation. Tables 5-7 presents the probability of primary school completion for children aged 13-16 at the baseline[15]. These estimates make no distinction between children in control or treatment villages since they correspond to pre-intervention states which, due to the randomized nature of each program, provide similar results[16]. This age bracket is selected since the lower bound (13 years) roughly coincides with the expected age at which children are assumed to have completed primary education in each country. The first column presents the average probability of primary completion for a child with all "favorable" circumstances, which constitutes the base scenario for comparison for each circumstance type. The estimates in the succeeding columns may be interpreted as the marginal effect in average probability from belonging to one of the disadvantaged groups.

In general, the probability of primary completion varies among countries. The outstanding case is Mexico, where approximately 84% of children 13-16 would complete this educational level if they belonged to the most advantaged groups. However, the average base scenario is far less optimistic in Honduras (62% average probability of completion) and Nicaragua (45%). As would be expected, the average probability of primary completion increases as children grow older. However, the most important results from the tables are those that indicate the marginal effects of group membership.

For instance, there is a large negative effect for children born to parents with low education in Honduras, signaling that educational outcomes are transmitted across generations. In particular, children aged 13 whose father and mother have low education show a significantly lower probability of completing primary (33 percentage points less). This means that for this particular child, there is only a 25 per cent chance of completing the basic educational level. In Mexico, the circumstances with significant effects are ethnicity, gender and mother's level of education. Compared to the base scenario, an indigenous boy born to a low educated mother is 12 percentage points less likely to finish primary. Finally, the relevant circumstances in Nicaragua include gender and parental education. Once again, a worst-case scenario sees a 13-year old boy born into a low education environment with a pessimistic 6 percent probability of finishing his primary studies on time.

[15] Results for Logit models are presented. LPM estimates were also calculated and are available from the author upon request. The differences between both estimations are negligible, with the Logit specification being favored because it allows the descriptive simulations in Tables 5-7 by age of the child.
[16] These results are available upon request.

These average probabilities are complemented with baseline estimates of the D-Index in Table 8. In the baseline period, to equate educational opportunities, 62% of opportunities needed to be redistributed in PRAF-II villages. This figure is 51% in PROGRESA and 69 percent in RPS. This evidence indicates that before the start of the program, inequality of opportunity exists and seems to be highly important. Moreover, the findings show the grim state in the targeted villages in each country which is not surprising since these localities were selected due to their high poverty rate. However this leads to the natural question, how does a CCT program affect this initial level of inequity?

4.6 Program effects on Inequality of Opportunities

A first approach to this answer is also suggested in Table 8, which shows trends in both the mean and the variability of opportunities. The evidence indicates heterogeneous behavior when comparing each program. For instance, after exposure to PRAF-II there is a mild increase in access, but virtually no change in inequality of opportunities. PROGRESA and RPS villages show more pronounced improvements, although their behavior is somewhat different. For instance, PROGRESA seems to improve access and opportunities only during the first period. However, the table indicates that during the second year, there is no improvement and in fact shows a worsening of the distribution of opportunities. In turn, RPS continually improves access rates; and the D-Index behaves in a similar fashion than the Mexican program. However, while illustrative, these calculations do not isolate the effect attributable to the programs. For this purpose, impact evaluation methods are required.

First, estimates for the effect on the mean are presented in Table 9. The estimation procedure is the same as for the enrollment and child labor results, with program effects estimated using fixed-effects linear regressions. The results confirm the trends from the prior table. On the one hand, there is no evidence of effects on mean opportunities in PRAF-II villages and positive effects in PROGRESA and RPS villages. On the other hand, there are clear differences between these two cases where opportunities improve. In particular, there is evidence of a continual increase due to the Nicaraguan program, while the increase in PROGRESA is only found in the first period and is small in magnitude. However, this is only one of the two conditions required to assert whether the distribution of opportunities is improved since this greater access needs to be fairly distributed.

Table 10 presents a difference in differences exercise for the D-Index which isolates the effect of the program on the variance of opportunities by comparing the opportunity distribution of children exposed to the program and those residing in control villages. Once again, there is no evidence of an effect in PRAF-II villages. In this case however, RPS shows no indication of an improvement in the distribution of opportunities. Hence, while the access increases, inequality in educational opportunities remains. However, the most interesting finding comes from the estimates for PROGRESA children. The change in the D-Index attributable to PROGRESA shows that the first period reduction was highly significant and negative (denoting an immediate and overall improvement in opportunities since mean outcomes also improved). However, this initial gain does not hold according to the evidence. In fact, after the first year, opportunities stagnate and their distribution seems to worsen. This shift in the distribution may indicate that the program has an impact on opportunities, but that there is another effect which compensates this initial redistribution.

Therefore, these findings provide a first insight into the redistributive consequences of CCT programs on opportunities by showing that there are intricate mechanisms at work after interventions. On the one hand, the average level of opportunities seems to be affected positively by the programs demand-centered incentives (and more so when supply-side factors are also included). However, with respect to the distribution of those opportunities, the estimates indicate that positive gains are likely but temporary. The evidence for PROGRESA, the gold standard of CCTs shows that there is evidence of a readjustment of opportunity distributions to counter these gains. This may be a first indication of mechanisms such as inequality traps in action, where sources of persistent inequality maintain an inequitable distribution despite improved average outcomes. These mechanisms and their implications for social policies are surely to be an interesting topic for future research and public policy debate; since they may shed light on important mechanisms to foster virtuous cycles of growth-equality and poverty reduction.

5. Conclusion

This paper studied the effect of CCT programs on educational opportunities. Until now, the existing body of work has provided exhaustive evidence on the short-run gains from the programs, and has omitted evaluation of more structural consequences, such as the effect on opportunities. This paper uses recent contributions in the measurement of the distribution of opportunities to overcome this gap in the literature and in order to encourage discussion on the structural effects of such interventions, especially with respect to opportunities.

In summary, there are three main findings in this paper: first, there are important differences in program effects between circumstance types. CCT programs seem to have a strong effect on mean outcomes which favors disadvantaged groups. This is especially true for child labor and less so in enrollment, where the effect was relatively widespread across groups. Second, there is evidence of a positive effect of conditional cash transfers on educational opportunities in the short-run. However, this leads to the third finding, that although the effect on the mean is positive, there is no indication of an improvement in the distribution of those opportunities. In fact, in the case of PROGRESA, the initial positive gains are compensated by another effect, which restores the initial unequal setting and may be associated with structural inequality.

Certainly, there are a number of reasons which may explain the behavior found here. Distributional changes in opportunities are much more complex than for standard outcomes, and while this paper takes a first step in analyzing the structural consequences of CCT programs by quantifying program effects, there is still much work to be carried out. In particular, a detailed analysis of the channels which cause the positive impact and their long-run implications on beneficiaries is most welcome, as well as other contributions in both theoretical and empirical fields to seek explanation for these results. Also, this paper looks at medium term outcomes, which may limit the capabilities of the above methodology to capture effects further down the line. Nevertheless, as a first exercise, the work presented here outlines a possible starting point these extensions and hopefully greater interest in the structural consequences of conditional cash transfers, and on concepts such as opportunities, where it is necessary to observe both mean outcomes and their variance.

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Tables

Table 1
Proportion of children 6-16 in each circumstance type

Circumstance Types	Defined as	PRAF-II	PROGRESA	RPS
All Population				
Ethnic Group				
Majority	White/Mestizo	n.a.	70.2	n.a.
Minority	Indigenous/Native	n.a.	29.8	n.a.
Gender				
Male	Male	50.9	51.2	50.1
Female	Female	49.1	48.8	49.9
Mother's Education				
High	Primary Complete or Higher	16.9	32.3	10.7
Low	Less than Primary Complete	83.1	67.7	89.3
Father's Education				
High	Primary Complete or Higher	17.9	30.7	8.2
Low	Less than Primary Complete	82.1	69.3	91.8
Household Type				
Both parents	If both father and mother are present	81.8	89.5	85.3
Single-Parent	If only one of the parents is present	18.2	10.5	14.7

Source: Own calculations on Program Surveys
n.a.-Not available

Table 2
Descriptive Statistics by Circumstance Type (children 6-16)

	PRAF-II		PROGRESA		RPS	
	Enrolled	Working	Enrolled	Working	Enrolled	Working
All	74.4	60.5	82.8	14.0	62.5	17.1
By Circumstance						
Ethnic Group						
Minority	n.a.	n.a.	85.9	12.4	n.a.	n.a.
Majority	n.a.	n.a.	81.5	14.6	n.a.	n.a.
Difference			**4.4*** **	**-2.2***		
Gender						
Female	75.7	33.8	81.3	7.4	63.4	5.7
Male	73.1	73.4	84.2	20.1	61.5	28.4
Difference	**2.6****	**-39.6*****	**-2.9*****	**-12.7*****	**1.9**	**-22.7*****
Mother's Education						
Low	72.7	61.2	82.9	13.9	60.4	17.7
High	85.0	60.1	90.3	9.2	81.2	14.7
Difference	**-12.3*****	**1.1**	**-7.5*****	**4.7*****	**-20.8*****	**3.0**
Father's Education						
Low	72.1	61.3	82.5	13.9	62.5	17.1
High	90.1	42.0	90.7	8.5	77.9	11.5
Difference	**-18.0*****	**19.4*****	**-8.2*****	**5.4*****	**-15.5*****	**5.6****
Household Type						
Both parents	71.4	63.5	79.5	17.0	58.6	20.5
Single-Parent	75.0	59.8	83.2	13.6	63.1	16.6
Difference	**-3.6****	**3.7**	**-3.6*****	**3.4*****	**-4.5***	**3.9****

Source: Own calculations on Program Surveys
n.a.-Not available
* Significant at 10%; ** Significant at 5%; *** Significant at 1%

Table 3
Program effects on enrollment (children 6-16), by circumstance type

		All	Ethnicity		Gender		Mother's Education		Father's Education		Household Type	
			Majority	Minority	Boys	Girls	High	Low	High	Low	Both Parents	Single Parent
	ITT (May-Aug. 2002)	**0.024**	**n.a.**	**n.a.**	**-0.004**	**0.056**	**0.080**	**0.012**	**-0.025**	**0.020**	**0.020**	**0.037**
PRAF-II	Clustered	(0.017)			(0.026)	(0.018)***	(0.037)**	(0.021)	(0.044)	(0.022)	(0.018)	(0.037)
Baseline:	Bootstrapped	(0.016)			(0.029)	(0.018)***	(0.036)**	(0.021)	(0.046)	(0.024)	(0.019)	(0.037)
Aug-Dec. 2000												
	Observations	9,620			4,861	4,759	1,342	7,039	1,250	5,758	7,750	1,870
	Groups	6,004			3,018	3,001	941	4,518	849	3,703	4,930	1,279
	ITT (Nov. 1998)	**0.028**	**0.028**	**0.047**	**0.032**	**0.030**	**0.022**	**0.034**	**0.020**	**0.027**	**0.028**	**0.030**
	Clustered	(0.005)***	(0.007)***	(0.015)***	(0.007)***	(0.008)***	(0.012)*	(0.010)***	(0.014)	(0.011)**	(0.005)***	(0.013)**
	Bootstrapped	(0.005)***	(0.007)***	(0.016)***	(0.008)***	(0.008)***	(0.012)*	(0.010)***	(0.013)	(0.011)**	(0.005)***	(0.012)**
	ITT (Mar. 1999)	**0.030**	**0.029**	**0.042**	**0.029**	**0.034**	**0.009**	**0.033**	**0.028**	**0.020**	**0.030**	**0.028**
PROGRESA	Clustered	(0.006)***	(0.008)***	(0.013)***	(0.008)***	(0.008)***	(0.013)	(0.011)***	(0.015)*	(0.011)*	(0.006)***	(0.016)*
Baseline:	Bootstrapped	(0.006)***	(0.008)***	(0.013)***	(0.009)***	(0.008)***	(0.013)	(0.011)***	(0.014)**	(0.011)*	(0.006)***	(0.017)*
Sept.1997-Mar. 1998												
	ITT (Nov. 1999)	**0.033**	**0.030**	**0.028**	**0.031**	**0.041**	**0.021**	**0.035**	**0.023**	**0.043**	**0.034**	**0.029**
	Clustered	(0.007)***	(0.008)***	(0.016)*	(0.009)***	(0.009)***	(0.015)	(0.012)***	(0.016)	(0.012)***	(0.007)***	(0.016)*
	Bootstrapped	(0.007)***	(0.008)***	(0.017)*	(0.010)***	(0.010)***	(0.016)	(0.012)***	(0.016)	(0.012)***	(0.007)***	(0.016)*
	Observations	146,059	85,612	37,984	72,485	70,958	29,639	45,491	25,245	43,584	131,663	14,396
	Groups	49,427	42,070	26,810	39,972	39,715	22,890	31,388	20,348	30,816	44,275	5,450
	ITT (Oct. 2001)	**0.180**	**n.a.**	**n.a.**	**0.186**	**0.174**	**0.055**	**0.188**	**0.062**	**0.167**	**0.170**	**0.267**
	Clustered	(0.042)***			(0.047)***	(0.044)***	(0.072)	(0.045)***	(0.056)	(0.045)***	(0.042)***	(0.065)***
	Bootstrapped	(0.044)***			(0.047)***	(0.046)***	(0.077)	(0.044)***	(0.066)	(0.047)***	(0.039)***	(0.067)***
RPS												
Baseline:	**ITT (Oct. 2002)**	**0.144**	**n.a.**	**n.a.**	**0.148**	**0.138**	**0.034**	**0.155**	**0.006**	**0.126**	**0.136**	**0.205**
Aug.-Sept. 2000	Clustered	(0.047)***			(0.053)***	(0.049)***	(0.077)	(0.050)***	(0.091)	(0.049)**	(0.047)***	(0.079)**
	Bootstrapped	(0.050)***			(0.054)***	(0.048)***	(0.082)	(0.051)***	(0.094)	(0.051)**	(0.043)***	(0.080)**
	Observations	8,318			4,202	4,116	779	6,885	506	6,139	7,268	1,050
	Groups	3,542			1,768	1,774	363	2,947	236	2,594	3,089	453

Source: Own calculations on Program Surveys
n.a.-Not available
Standard errors (in parentheses) clustered at village level.
250 replications for bootstrapped errors
* Significant at 10%; ** Significant at 5%; *** Significant at 1%

Table 4
Program effects on child labor (children 6-16), by circumstance type

		All	**Ethnicity**		**Gender**		**Mother's Education**		**Father's Education**		**Household Type**	
			Majority	Minority	Boys	Girls	High	Low	High	Low	Both Parents	Single Parent
	ITT (May-Aug. 2002)	**0.091**	**n.a.**	**n.a.**	**0.067**	**0.205**	**0.024**	**0.090**	**-0.157**	**0.111**	**0.091**	**0.137**
PRAF-II	Clustered	(0.057)			(0.062)	(0.113)*	(0.130)	(0.065)	(0.115)	(0.073)	(0.062)	(0.091)
Baseline:	Bootstrapped	(0.059)			(0.067)	(0.126)	(0.190)	(0.069)	(0.135)	(0.074)	(0.062)	(0.106)
Aug-Dec. 2000												
	Observations	4,248			2,799	1,449	416	3,289	423	2,647	3,393	855
	Groups	3,258			2,028	1,231	372	2,512	361	2,027	2,623	688
	ITT (Nov. 1998)	**-0.029**	**-0.032**	**-0.014**	**-0.032**	**-0.011**	**0.013**	**-0.028**	**0.033**	**-0.042**	**-0.029**	**-0.026**
	Clustered	(0.010)***	(0.012)***	(0.023)	(0.014)**	(0.010)	(0.022)	(0.015)*	(0.023)	(0.016)***	(0.010)***	(0.021)
	Bootstrapped	(0.010)***	(0.013)**	(0.023)	(0.013)**	(0.011)	(0.021)	(0.014)**	(0.023)	(0.016)***	(0.010)***	(0.021)
	ITT (Mar. 1999)	**-0.028**	**-0.034**	**-0.018**	**-0.042**	**-0.008**	**-0.007**	**-0.037**	**0.010**	**-0.047**	**-0.026**	**-0.048**
PROGRESA	Clustered	(0.009)***	(0.011)***	(0.024)	(0.014)***	(0.009)	(0.018)	(0.015)**	(0.021)	(0.015)***	(0.009)***	(0.022)**
Baseline:	Bootstrapped	(0.008)***	(0.011)***	(0.024)	(0.014)***	(0.009)	(0.018)	(0.014)***	(0.023)	(0.015)***	(0.009)***	(0.021)**
Sept.1997-Mar. 1998												
	ITT (Nov. 1999)	**-0.030**	**-0.035**	**-0.002**	**-0.042**	**-0.008**	**-0.028**	**-0.027**	**-0.025**	**-0.048**	**-0.032**	**-0.018**
	Clustered	(0.011)***	(0.012)***	(0.031)	(0.015)***	(0.011)	(0.018)	(0.017)	(0.024)	(0.015)***	(0.011)***	(0.022)
	Bootstrapped	(0.011)***	(0.013)***	(0.032)	(0.014)***	(0.011)	(0.019)	(0.017)	(0.024)	(0.014)***	(0.011)***	(0.022)
	Observations	120,492	70,528	31,511	59,906	58,380	23,975	37,336	20,221	35,839	108,101	12,391
	Groups	41,882	35,393	22,399	33,556	33,215	18,685	26,097	16,442	25,676	37,365	4,750
	ITT (Oct. 2001)	**-0.068**	**n.a.**	**n.a.**	**-0.093**	**-0.043**	**-0.031**	**-0.071**	**0.023**	**-0.062**	**-0.063**	**-0.092**
	Clustered	(0.024)***			(0.036)**	(0.022)*	(0.050)	(0.028)**	(0.067)	(0.029)**	(0.024)**	(0.043)**
	Bootstrapped	(0.024)***			(0.034)***	(0.023)*	(0.049)	(0.030)**	(0.066)	(0.027)**	(0.023)***	(0.045)**
RPS												
Baseline:	**ITT (Oct. 2002)**	**-0.081**	**n.a.**	**n.a.**	**-0.102**	**-0.062**	**-0.051**	**-0.085**	**0.013**	**-0.075**	**-0.081**	**-0.067**
Aug.-Sept. 2000	Clustered	(0.032)**			(0.043)**	(0.029)**	(0.050)	(0.037)**	(0.073)	(0.035)**	(0.032)**	(0.063)
	Bootstrapped	(0.033)**			(0.045)**	(0.029)**	(0.052)	(0.037)**	(0.077)	(0.034)**	(0.031)***	(0.067)
	Observations	8,205			4,160	4,045	769	6,789	502	6,052	7,166	1,039
	Groups	3,506			1,746	1,760	360	2,916	236	2,563	3,055	451

Source: Own calculations on Program Surveys
n.a.-Not available
Standard errors (in parentheses) clustered at village level.
250 replications for bootstrapped errors
* Significant at 10%; ** Significant at 5%; *** Significant at 1%

Table 5
Predicted probabilities of completing primary on time (children 13-16), PRAF-II

	PRAF-II					
	Base Scenario	**Marginal Effect by Circumstance Type**				
Age		Ethnic Minority	Female	Mother's Education Low	Father's Education Low	Household Type
13	0.586	n.a.	0.036	-0.148	-0.182	-0.323
			(0.030)	(0.040)***	(0.052)***	(0.228)
14	0.572	n.a.	0.036	-0.148	-0.181	-0.320
			(0.030)	(0.040)***	(0.053)***	(0.225)
15	0.693	n.a.	0.031	-0.139	-0.173	-0.331
			(0.027)	(0.040)***	(0.049)***	(0.271)
16	0.643	n.a.	0.034	-0.145	-0.180	-0.331
			(0.028)	(0.039)***	(0.052)***	(0.252)

Source: Own calculations on Program Surveys
n.a.-Not Available
Notes: Predicted probabilities are estimated by Logit pooling treated and control children. Controls include 4 age levels (13-16), log household consumption, number of children in the household and the circumstance vector. Heteroskedasticity robust CRVE standard errors in parentheses.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%

Table 6
Predicted probabilities of completing primary on time (children 13-16), PROGRESA

	PROGRESA					
	Base Scenario	**Marginal Effect by Circumstance Type**				
Age		Ethnic Minority	Female	Mother's Education Low	Father's Education Low	Household Type
13	0.740	-0.043	0.044	-0.036	-0.018	0.027
		(0.018)***	(0.009)***	(0.012)***	(0.012)	(0.030)
14	0.834	-0.031	0.031	-0.026	-0.013	0.020
		(0.013)***	(0.006)***	(0.009)***	(0.008)	(0.021)
15	0.883	-0.024	0.023	-0.020	-0.010	0.014
		(0.010)***	(0.005)***	(0.007)***	(0.006)	(0.016)
16	0.893	-0.022	0.021	-0.019	-0.009	0.013
		(0.009)***	(0.005)***	(0.006)***	(0.006)	(0.014)

Source: Own calculations on Program Surveys
n.a.-Not Available
Notes: Predicted probabilities are estimated by Logit pooling treated and control children. Controls include 4 age levels (13-16), log household consumption, number of children in the household and the circumstance vector. Heteroskedasticity robust CRVE standard errors in parentheses.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%

Table 7
Predicted probabilities of completing primary on time (children 13-16), RPS

	RPS					
	Base Scenario	**Marginal Effect by Circumstance Type**				
Age		Ethnic Minority	Female	Mother's Education Low	Father's Education Low	Household Type
13	0.253	n.a.	0.090	-0.154	-0.125	-0.045
			(0.045)**	(0.069)***	(0.058)***	(0.159)
14	0.362	n.a.	0.105	-0.207	-0.165	-0.056
			(0.054)**	(0.090)***	(0.070)***	(0.207)
15	0.554	n.a.	0.103	-0.267	-0.205	-0.063
			(0.056)**	(0.099)***	(0.078)***	(0.246)
16	0.629	n.a.	0.094	-0.274	-0.206	-0.060
			(0.054)**	(0.101)***	(0.077)***	(0.242)

Source: Own calculations on Program Surveys
n.a.-Not Available
Notes: Predicted probabilities are estimated by Logit pooling treated and control children. Controls include 4 age levels (13-16), log household consumption, number of children in the household and the circumstance vector. Heteroskedasticity robust CRVE standard errors in parentheses.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%

Table 8
Trends in inequality of opportunity (children 6-16)

	PRAF-II		**PROGRESA**		**RPS**	
	Coverage	*D-Index*	*Coverage*	*D-Index*	*Coverage*	*D-Index*
t=0	0.126	0.607	0.311	0.509	0.059	0.666
	(0.005)	(0.075)	(0.002)	(0.021)	(0.005)	(0.094)
t=1	0.131	0.606	0.339	0.437	0.050	0.609
	(0.005)	(0.079)	(0.004)	(0.028)	(0.005)	(0.148)
t=2			0.339	0.468	0.073	0.619
			(0.003)	(0.026)	(0.005)	(0.088)

Source: Own calculations on Program Surveys
Clustered Standard Errors in Parentheses

Table 9
Program effects on the probability of completing primary on time

		All Children	**Children 13-16**
	ITT (May-Aug. 2002)	**0.002**	**-0.002**
PRAF-II	Clustered	(0.005)	(0.009)
Baseline:	Bootstrapped	(0.006)	(0.009)
Aug-Dec. 2000	*Observations*	7,422	2,222
	ITT (Nov. 1998)	**0.006**	**0.010**
	Clustered	(0.006)	(0.004)**
	Bootstrapped	(0.006)	(0.004)**
PROGRESA			
Baseline:	**ITT (Nov. 1999)**	**0.010**	**0.004**
Sept.1997-Mar. 1998	Clustered	(0.007)	(0.005)
	Bootstrapped	(0.007)	(0.005)
	Observations	37,507	12,549
	ITT (Oct. 2001)	**0.014**	**0.037**
	Clustered	(0.004)***	(0.010)***
RPS	Bootstrapped	(0.004)***	(0.011)***
Baseline:			
Aug.-Sept. 2000	**ITT (Oct. 2002)**	**0.016**	**0.041**
	Clustered	(0.008)*	(0.018)**
	Bootstrapped	(0.009)*	(0.018)**
	Observations	6,273	2,086

Source: Own calculations on Program Surveys
Standard errors (in parentheses) clustered at village level.
250 replications for bootstrapped errors.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%

Table 10
Program effects on inequality of educational opportunities (children 6-16)

		PRAF-II			PROGRESA			RPS	
		Treatment	Control	**Difference**	Treatment	Control	**Difference**	Treatment	Control	**Difference**
t=0		0.614	0.627	**-0.013**	0.509	0.509	**0.000**	0.677	0.766	**-0.089**
		(0.053)	(0.044)	(0.069)	(0.003)	(0.004)	(0.005)	(0.071)	(0.128)	(0.146)
t=1		0.608	0.645	**-0.036**	0.427	0.455	**-0.028**	0.639	0.689	**-0.049**
		(0.029)	(0.035)	(0.046)	(0.005)	(0.008)	(0.009)	(0.120)	(0.139)	(0.184)
t=2					0.473	0.458	**0.015**	0.601	0.671	**-0.070**
					(0.004)	(0.006)	(0.007)	(0.104)	(0.100)	(0.144)
Diff-In-Diff	Period 1			**-0.023**			**-0.028**			**0.040**
				(0.083)			(0.011)***			(0.235)
	Period 2						**0.043**			**-0.021**
							(0.012)***			(0.234)

Source: Own calculations on Program Surveys
n.a.-Not Available
* Significant at 10%; ** Significant at 5%; *** Significant at 1%

Appendix

Variance Estimator for the D-Index

The D-Index is actually a ratio of two linear functions of the predicted probabilities ($\hat{p}_i$), for those below mean access, $L = \{i : \hat{p}_i \leq \overline{p}\}$, and those who are above the population mean, $U = \{i : \hat{p}_i > \overline{p}\}$.

The variance estimator of $\hat{D}$ used in this paper is a sandwich-type estimator, $\sigma^2 = \Gamma \Omega_\beta \Gamma'$, calculated using the following formula:

$$\Gamma = \frac{1}{\overline{p}^2}\left(\left(\sum_{i \in L} w_i \hat{p}_i\right) \cdot \left(\sum_{i \in U} w_i \hat{p}_i (1-\hat{p}_i) x_i\right)\right) - \left(\left(\sum_{i \in U} w_i \hat{p}_i\right) \cdot \left(\sum_{i \in L} w_i \hat{p}_i (1-\hat{p}_i) x_i\right)\right)$$