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Did Progresá Reduce Inequality of Opportunity for School Re-enrollment?

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ABSTRACT

The school re-enrollment probability conditional on a child's circumstances (gender, indigenous origin, and family background) is our measure of the child's school re-enrollment opportunity. For children who completed grades 1 to 6, we use Generalized Lorenz and Lorenz dominance to show that Progresá improved aggregate opportunities and reduced inequality of opportunity. Moreover, using the Human Opportunity Index—or alternatively, a Gini Opportunity Aggregator—the decrease in inequality of opportunity accounts for about 15% to 40% of the total effect of the program on aggregate opportunities.

Keywords: School Re-enrollment; Inequality of Opportunity, Impact Evaluation; Progresá.

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1. INTRODUCTION

A key concern in developing countries is the provision of access to basic opportunities for all (see, e.g., [Molinas et al. 2012](#)), but such universal access is usually not achieved. One such opportunity is access to education. As powerfully advocated by both economists and political philosophers (see, e.g., [Roemer 1993 and 1998](#); [Fleurbaey 2008](#); [Cohen 1989](#)), access to education should not be influenced by one's circumstances, that is, characteristics over which one has no control, such as family background, race, or gender. Hence, not only the level of children's opportunities, but also the way these opportunities are distributed among children with different circumstances is highly relevant from a normative perspective. Both attitude survey research ([Schokkaert and Devooght 2003](#); [Gaertner and Schwettmann; 2007](#)) and experimental evidence ([Cappelen et al. 2007, 2010 and 2013](#); [Mollerstrom et al. 2015](#)) have shown that many endorse the normative stance that uncontrollable characteristics should not influence an individual's outcomes (income, health, or education). In addition, [Marrero and Rodríguez \(2013\)](#) show that circumstance-related inequalities negatively correlate with economic growth. Hence, reducing inequality of opportunity might be instrumental in increasing economic growth.

Many social interventions, including Progresas, are motivated by a concern to provide ample opportunities for all. The question regarding whether or not these interventions influence the level of children's opportunities and their distribution is key. Mexico's Progresas was founded in 1997. It was later renamed Oportunidades, and in 2014, changed into Prospera. It aims to expand children's educational opportunities. In fact, the conditional cash transfer program is explicitly designed to give poor parents incentives to keep their children in school. This is done by increasing school subsidies when the children are in a higher grade (to make schooling more attractive compared with child labor) and by providing larger grants for girls at ages when they are more likely to drop out.

There is widespread agreement in the evaluation literature that Progresas increased average school enrollment, with large effects for children aged 12 years and above (see, e.g., [Schultz 2004](#); [Behrman et al. 2005](#); [Todd and Wolpin 2006](#); [Attanasio et al. 2012](#); [Dubois et al. 2012](#)). Some studies go further and identify the average effects for children from different circumstances. [Schultz \(2004\)](#) and [Todd and Wolpin \(2006\)](#), for instance, find larger positive treatment effects on school enrollment for girls than for boys. [Figueroa \(2014\)](#) identifies the effect on the entire distribution of child cognitive and non-cognitive skills for girls, for boys, and for children from indigenous and non-indigenous origin. There are two applications of the recent literature on inequality of opportunity to project evaluation; both are inspired by [Roemer \(1993 and 1998\)](#). [Van de gaer et al. \(2014\)](#) determines the effect of Oportunidades on the distribution of children's health outcomes, conditional on their circumstances. They use tests for restricted stochastic dominance developed by [Davidson and Duclos \(2013\)](#) to identify which children gain but provide no overall assessment of the effect of the program on inequality of opportunity. The other application is by [Andreoli et al. \(2014\)](#), who determine the effect of child care expansion in Norway on the distribution of their earnings 30 years later, conditional on their parents' earnings decile, using techniques of (inverse) stochastic dominance. By comparing the effect of child care expansion on all possible pairwise conditional distributions, they establish that childcare expansion leads to a decrease of inequality of opportunity. Both approaches rely on a comparison of non-parametrically estimated conditional distribution functions of children sharing the same circumstances, which makes them much harder to implement than the test presented in our study and limits the number of different values that circumstances can take: four in the case of [Van de gaer et al. \(2014\)](#) and ten in the case of [Andreoli et al. \(2014\)](#). The parametric approach followed in the present paper allows the researcher to consider more circumstances; it is closer to what has become the dominant approach in the measurement of inequality of opportunity.

This study provides an assessment of the effect of Progresas on inequality of school re-enrollment opportunities. The latter are defined as the probabilities of children being re-enrolled in school after having completed a particular grade, conditional on their circumstances (gender, indigenous origin, and parental education level). The higher this estimated probability, the better the re-enrollment opportunities available to the child. The estimated probabilities are collected in a "counterfactual" distribution. They can be aggregated by an opportunity aggregator function to obtain an overall assessment of the opportunities offered to all children. All inequalities in this counterfactual distribution are due to children facing different circumstances. Hence, the inequality in that distribution measures the inequality that is correlated with circumstances, and is, therefore, a valid measure of inequality of opportunity.¹

¹ We draw on recent contributions to the literature on the measurement of inequality of opportunity. In this literature, the estimated value of an individual's outcome, such as her or his income or school re-enrollment probability, conditional on her or his circumstances, is taken as a measure of her or his opportunities. This is a measurement statement, not a causal statement. Suppose that children facing particular circumstances are more motivated to re-enroll at school. As they cannot control any motivations that correlate with their circumstances, the effect of these circumstances on their motivation and, thereby, on their re-enrollment probability will be attributed to

We construct two counterfactual distributions: one for the case wherein the children are treated, and one wherein they are not. To this end, we first estimate a logistic regression to obtain the effect of the program on school re-enrollment for children facing different circumstances. Next, we use this regression to predict the probability of each child being re-enrolled in school if a child lives in a household that participates in the program and if she or he lives in a non-participating household, given her or his circumstances. This gives us two vectors of predicted re-enrollment probabilities: one with and one without the program.

To determine the effect of the program on inequality of opportunity, we provide Generalized Lorenz and Lorenz comparisons of the vectors of predicted re-enrollment probabilities with and without the program. If the former Generalized Lorenz dominates the latter, the program leads to an improvement for a large class of opportunity aggregators that are increasing in individuals' opportunities and are inequality averse with respect to the distribution of these opportunities. If the vector of the predicted re-enrollment probabilities with the program Lorenz dominates the vector without the program, the program leads to a decrease of inequality of opportunity for a large class of inequality measures. We also verify the importance of the effect on inequality of opportunity to the total effect on the opportunity aggregator. To this end, we use the Human Opportunity Index proposed by [de Barros et al. \(2008 and 2009\)](#) and, as an alternative, the Gini Opportunity Aggregator. Both can be decomposed into changes that, in turn, arise from changes in average re-enrollment and in an index that measures the inequality of re-enrollment distribution among children facing different circumstances; the latter measures the changes in inequality of opportunity.

We find that for children that successfully completed one to six years of schooling, Progresá leads to a distribution of school re-enrollment opportunities that both Generalized Lorenz and Lorenz dominates the distribution of school re-enrollment opportunities in the absence of the program. For children that completed primary school, the results are particularly strong. Using the Human Opportunity Index or the Gini Opportunity Aggregator, the decrease in inequality of opportunity is shown to be responsible for about 15% to 40% of the increase in aggregated opportunities.

The next section describes Progresá and the sample selection procedure. Section 3 explains the methodology. The empirical results are presented in section 4. Section 5 discusses the results. Section 6 concludes the study.

2. DESCRIPTION OF PROGRESA AND SAMPLE SELECTION

Progresá (Programa de Educación, Salud y Alimentación) was implemented by the Mexican government in 1997 to aid poor rural households. Its goal was to alleviate poverty and break the intergenerational transmission of poverty through the development of human capital. The program consists of two components: cash transfers, and in-kind health and nutritional benefits. Educational grants are conditional on child school enrollment and regular school attendance (an attendance rate of 85% is required).² Grants for consumption of food are conditional on regular medical check-ups in health clinics, and attendance in health and nutrition discussions. In-kind health benefits and nutritional supplements are provided for children up to age five, and for pregnant and lactating women.

Primary education in Mexico consists of six grades, followed by three years of secondary education and three years of high school. Educational grants begin in the third grade in primary school, increase through each grade, and finally end after secondary school. At the secondary level, girls receive higher grants than boys do (see [Skoufias 2005](#), Table 1.1 for the exact amounts). The total monthly monetary payments a household can receive (in the form of educational grants and grants for food) is capped. Between November 1998 and October 1999, households with school-aged children received an average of 101 pesos per month in food transfers, and 139 pesos per month in school transfers. In total, between November 1998 and October 1999, these transfers represented more than 18% of the total average value of household consumption (see [Skoufias 2005](#), Table 1.5).

Based on data from national censuses, highly deprived rural localities with access to a primary school and a health clinic were identified by the program. In these localities, households that experienced extreme poverty (identified by an index based on socioeconomic data of the household; see [Skoufias et al. 1999](#)) were included into the program.

For logistical reasons, not all localities eligible for participating in the program were enrolled at the same time. A random procedure assigned some localities to receive immediate treatment in November 1997 and monetary payments from May 1998; the others began receiving treatment in December 2000.

Data on children's education were collected for children aged between 6 and 17 years in October 1998 for both the 320 immediate treatment localities and 186 delayed treatment localities. We follow a large corpus of literature (see,

their circumstances. [Roemer \(1993\)](#) powerfully defends this position. For an overview of the rapidly expanding literature on inequality of opportunity, see [Ferreira and Peragine \(2016\)](#), [Ramos and Van de gaer \(2016\)](#) or [Roemer and Trannoy \(2015\)](#).

² The evidence reported in [de Brauw and Hoddinott \(2011\)](#) shows that the conditionality of Progresá is important in increasing enrollment, particularly at entry into secondary school.

e.g., Gertler 2004; Schultz 2004; Behrman et al. 2005; Todd and Wolpin 2006; de Janvry et al. 2006; Attanasio et al. 2012; Dubois et al. 2012) and use the delayed treatment localities sample as a control group to identify the short-run effects of the program.³ In both the treatment and control samples, we only consider households that were eligible for the program, and they comprise the localities' poor households. Our sample contains all children from poor households in 1998 aged between 6 and 17 years for which data on education were available. These data were available for 90% of all children in this age group. In addition, we use baseline data collected in 1997 to obtain information on the children's background characteristics, such as parental education, gender of the head of the household, as well as dwelling characteristics and demographic composition of the household. We use the 1997 survey because of a lack of background information in the 1998 survey. We retrieved background information for 94.6% of the children in our sample and created dummy variables for missing background information in case a particular background characteristic was missing.

For both the treatment and control samples, Figure 1 provides the percentage of children that reached grades 0 to 9 as their highest completed grade, and were re-enrolled two months after academic year 1998–99 commenced in August 1998.

Figure 1. Re-enrollment rates in October 1998 per grade for the treatment and control samples.

School re-enrollment is high across all grades, except for grade 6, which is when primary education ends, and grade 9, which is when secondary education ends.⁴ We pay special attention to grade 6 because the transition from primary

³ Children that belong to treated households have been receiving treatment only for the last eight months. Moreover, the sample is restricted to school-aged children, none of whom could have benefited directly from the nutritional supplements (only given to children up to age 5). Hence, the long-run effects of the program are greater, as shown in Behrman et al. (2009).

⁴ This pattern is well known. It was, in the context of Progresa, described and analyzed by Schultz (2004), one of the few authors to look at re-enrollment rates per grade rather than enrollment rates per age. The latter rates follow a smoother inverted U-shape than the former; see, Behrman et al. (2005).

to secondary school is an important bottleneck, and re-enrollment in the treatment sample is much higher than in the control sample. For children that completed grades 3 to 5, there are clear differences between these re-enrollment rates.

Two remarks must be made at this point. First, a simple comparison of the treatment and control samples might be misleading if these samples differ in terms of observable or unobservable characteristics. Suppose the treatment sample contains more children of higher-educated parents who keep their children longer in school. Then, some of the difference in average re-enrollment between the treatment and control samples is due to the higher frequency of educated parents in the treatment sample. Thus, the causal effect of the program would be unidentified. However, according to consensus, the differences between the treatment and control samples in terms of characteristics are limited (see, [Behrman and Todd \(1999\)](#)).⁵ Second, the average re-enrollment per grade itself does not inform us about the effect the program has on the distribution of re-enrollment among children facing different circumstances. For example, an increase in the average re-enrollment rate is compatible with an above average increase of re-enrollment rates for children with favorable circumstances (for instance, those with highly educated parents) and a decrease of re-enrollment rates for children with unfavorable circumstances (those having poorly educated parents). Since our goal is to evaluate the effect of Progresá on inequality of re-enrollment opportunities, we must go beyond average treatment effects. The next section describes the procedure we follow, where we also consider the program's effect on inequality of opportunity.

3. METHODOLOGY

3.1. Theoretical framework

We first have a fixed set, $\{1, \dots, n\}$, of children. For each child, i , we determine p_i , that is, the probability that she or he re-enrolls given her or his circumstances, where the circumstances are the characteristics of the child (or the environment) over which the child has no control. The higher a child's p_i , the more opportunities the child's circumstances offer. Therefore, in the literature on the measurement of inequality of opportunity, p_i measures the value of the child's opportunities. Vector $p = (p_1, \dots, p_n) \in [0, 1]^n$ records the value of all children's opportunities in our set. To evaluate their opportunities, we define an opportunity aggregator function: $O : [0, 1]^n \rightarrow \mathbb{R}$.

We impose the following standard properties on this aggregator function. First, $O(p)$ satisfies anonymity: any permutation of vector p yields the same value for the opportunity aggregator function as with the original p . Second, $O(p)$ is non-decreasing in every p_i : whenever there is one child whose opportunities increase, without decreasing another's opportunities, aggregate opportunities do not decrease. Third, $O(p)$ satisfies the weak transfer principle: increasing the value of opportunities for a child with low opportunities by a given amount and decreasing the value of opportunities for a child with higher opportunities by the same amount, without giving the former more opportunities than the latter, cannot decrease the value of $O(p)$. Let p^N be the vector of opportunities in the absence of the program, and p^T be the vector of opportunities with the program. We now have the following well-known result ([Shorrocks 1983](#)).

Proposition 1: For all non-decreasing opportunity aggregator functions, $O(p)$, that satisfy anonymity and the weak transfer principle, aggregate opportunities in vector p^T will not be smaller than in vector p^N if and only if the Generalized Lorenz curve of p^T lies nowhere below the Generalized Lorenz curve for p^N .

The Generalized Lorenz dominance is a condition under which we can unambiguously establish how the program affects children's aggregate opportunities for all opportunity aggregator functions satisfying the three basic properties.

We are also interested in what happens to inequality in the distribution of children's opportunities. We define an inequality measure as a function $I : [0, 1]^n \rightarrow \mathbb{R}$. We impose standard properties on this inequality measure. First, $I(p)$ satisfies anonymity: any permutation of vector p yields the same value for the inequality measure as with the original p . Second, $I(p)$ satisfies the weak transfer principle: increasing the value of opportunities for a child with low opportunities by a given amount and decreasing the value of opportunities for a child with higher opportunities by the same amount, without giving the former more opportunities than the latter, cannot increase inequality. Third,

⁵ Formal balance tests are given in Online Appendix D. In Tables D.1 and D.2 we test the balance of our 1998 sample for each grade using baseline data collected in 1997. In addition, using a logistic model, we estimate the probability of participating in the program conditional on the same rich set of preprogram characteristics in Table D.3. The results show that grade 7 is the only grade for which there might be significant differences in composition between the treatment and control samples in terms of the preprogram characteristics considered.

$I(p)$ is relative: multiplying all children's opportunities by the same positive constant does not affect the value of the inequality measure. We can now use the following standard result (Foster and Shorrocks 1988).

Proposition 2: For all inequality of opportunity measures, $I(p)$, that satisfy anonymity, the weak transfer principle, and relativity, inequality of opportunity in vector p^T will not be larger than inequality of opportunity in vector p^N if the Lorenz curve of p^T lies nowhere below the Lorenz curve of p^N .

Finally, we are interested in decomposing the effect of ProgresA on aggregate opportunities into an effect on average opportunities and on inequality of opportunity. This kind of question can be analyzed using “abbreviated” opportunity aggregator functions (analogous to “abbreviated social welfare functions”; see Lambert 2001). Particularly, by defining average opportunities, $C = (1/n) \sum_{i=1}^n p_i$, we use abbreviated opportunity aggregator functions that can be written as

$$O(p) = C [1 - I(p)]. \quad (1)$$

Observe that the numerical value of these abbreviated opportunity aggregator functions lies between 0 (when all $p_i = 0$) and 1 (when all $p_i = 1$). Following a procedure similar to the one suggested by de Barros et al. (2008), the change in the values of abbreviated opportunity aggregator functions can be decomposed as follows:

$$\begin{aligned} \Delta_O &= O(p^T) - O(p^N) = C^T(1 - I(p^T)) - C^N(1 - I(p^N)) \\ &= C^T(1 - I(p^T)) - C^N(1 - I(p^N)) + C^N(1 - I(p^T)) - C^N(1 - I(p^T)) \\ &= (C^T - C^N)(1 - I(p^T)) + C^N(I(p^N) - I(p^T)). \end{aligned} \quad (2)$$

Alternatively,

$$\begin{aligned} \Delta_O &= O(p^T) - O(p^N) = C^T(1 - I(p^T)) - C^N(1 - I(p^N)) \\ &= C^T(1 - I(p^T)) - C^N(1 - I(p^N)) + C^T(1 - I(p^N)) - C^T(1 - I(p^N)) \\ &= (C^T - C^N)(1 - I(p^N)) + C^T(I(p^N) - I(p^T)). \end{aligned} \quad (3)$$

As there is no reason to prefer one kind of decomposition to the other, we compute the Shapley value of the decomposition and take the average of (2) and (3):

$$\Delta_O = \underbrace{[C^T - C^N] \left[1 - \frac{1}{2}(I(p^T) + I(p^N)) \right]}_{\Delta_C} + \underbrace{\frac{1}{2} [C^T + C^N] [I(p^N) - I(p^T)]}_{\Delta_I}. \quad (4)$$

Equation (4) shows how the change in opportunity aggregator Δ_O can be decomposed into two components: Δ_C , the contribution of the change in average school re-enrollment; and Δ_I , the contribution of the change in inequality of opportunity.

The Human Opportunity Index, the abbreviated opportunity aggregator function proposed by de Barros et al. (2008), takes as inequality measure the dissimilarity index

$$D = \frac{1}{2C} \frac{1}{n} \sum_{i=1}^n |p_i - C|, \quad (5)$$

which equals the percentage of average school re-enrollment that has to be taken from those with a school re-enrollment above average and given to those with a school re-enrollment below average, such that everyone, independent of her of his circumstances, ends up with average school re-enrollment (all $p_i = C$). We define vector $\hat{p}$ as the vector obtained after permuting the elements of p , such that $\hat{p}_1 \leq \hat{p}_2 \leq \dots \leq \hat{p}_n$, and m , such that $\hat{p}_m \leq C < \hat{p}_{m+1}$. Thus, the value of the Human Opportunity Index, the opportunity aggregator function corresponding to (1) and (5), can be written as

$$H = \frac{1}{n} \left[\left(2 - \frac{m}{n}\right) \sum_{i=1}^m \hat{p}_i + \left(1 - \frac{m}{n}\right) \sum_{i=m+1}^n \hat{p}_i \right]. \quad (6)$$

This aggregator function is strictly increasing in all $\hat{p}_i$. It also satisfies the weak transfer principle. However, observe that transfers among children that have lower than average opportunities or transfers among children that have higher

than average opportunities do not affect the value of H . The function only values transfers from children with above average opportunities to those with below average opportunities. This is a disadvantage for those who adhere to the strong transfer principle. The latter requires that increasing the value of opportunities for a child with low opportunities by a given amount and decreasing the value of opportunities for a child with higher opportunities by the same amount, without giving the former more opportunities than the latter, should always decreases inequality.

There are many ways of constructing an abbreviated opportunity aggregator function satisfying the strong transfer principle. We thus propose one based on the familiar Gini index of inequality:

$$G = 1 + \frac{1}{n} - 2 \frac{\hat{p}_n + 2\hat{p}_{n-1} + 3\hat{p}_{n-2} + \dots + n\hat{p}_1}{n^2 C}. \quad (7)$$

The opportunity aggregator function corresponding to (1) and (7), the value of the Gini Opportunity Aggregator, can be easily written as

$$S = \frac{1}{n^2} \sum_{i=1}^n (2i-1) \hat{p}_{n+1-i}, \quad (8)$$

which indicates the familiar pattern of weights that are a linearly decreasing function of the rank order of the $\hat{p}_i$.

3.2. Selection of circumstances

To determine the extent of inequality of opportunity, we need, in principle, a complete description of each child's circumstances. Our data contain a rich set of circumstances owing to the information that was collected as part of the evaluation of the program in October 1998 and September 1997. The set of circumstances we use include personal and background characteristics: gender; race (indigenous background); parental background (parents' level of education); gender of the head of the household; whether both spouses live in the same house; and whether there was a secondary school in the locality near the child's residence. We outline the reasons for selecting these circumstances below.

Disparities in access to and performance at school are known to still exist in Mexico. Some of these disparities are correlated with gender. By the time Progresá was implemented, Mexico had no significant gender disparities in years of schooling attained, but school attendance of girls dropped in the transition to secondary school.⁶ To capture the effect of gender, we use a dummy indicating whether the child is a boy or a girl. Having an indigenous background is traditionally believed to diminish the possibilities that a child will receive education because of this population's social relegation. However, Mier et al. (2003) suggest that, conditional on primary school completion, indigenous boys have a higher probability of being enrolled in secondary education. Speaking an indigenous language is perhaps the best indicator for indigenous background since this trait is not common among non-indigenous populations. Hence, we follow standard practice and measure indigenous background by a variable that indicates if the child belongs to a family wherein either the head of the household or the spouse of the head speaks an indigenous language.

The effect of parental characteristics on children's schooling has been widely documented (see, e.g., Parker and Pederzini 2001; Mier et al. 2003); particularly important is the level of parental education. Molinas et al. (2012) show that parental education is the most important circumstance that explains school enrollment of children aged 10 to 14 years in most Latin American countries, including Mexico. In our sample, there are only few household heads with more than secondary education (grades 7–9); therefore, educational background of the children is measured by dummy variables to indicate if the head of the household has no education, some education (without having completed primary school), or completed at least primary school. The same variables were constructed for the spouse of the head. We used dummies to indicate the gender of the head of the household and whether the head and the spouse live in the same household, where the latter helps in identifying single parent families. The presence of a secondary school in the locality where the child resides is potentially important, as it could affect the decision of the child to go to primary or secondary school. Finally, we must address the many missing values for some of the circumstances. To enable the use of these observations in the estimation of the effects of the circumstances with non-missing values, we include additional dummies as circumstances to indicate the presence of missing values.

Table 1 presents the composition of the total sample (all grades) and grade 6 in terms of circumstances and missing values. Except for the parent's educational level, there are few differences in composition between the sample for all grades and grade 6. Almost half of the children in the sample are male, and about 30% have an indigenous background.

⁶ Parker and Pederzini (2001) and Mier et al. (2003) point out that these findings are consistent, as boys were more likely to repeat grades.

Table 1. Composition of and missing values in samples (in percentage)

	All grades	Grade 6
(a) Composition		
Child gender (male)	47.5	43.8
Indigenous background	29.4	27.2
Household head gender (male)	86.9	86.0
Household head living with spouse	90.7	89.4
Secondary school in the locality	29.1	24.8
Household head no education	25.9	26.8
Household head incomplete primary	45.7	50.0
Household head at least complete primary	16.8	12.1
Spouse no education	28.4	27.9
Spouse incomplete primary	37.7	41.3
Spouse at least complete primary	14.2	10.0
(b) Missing values		
Child gender (male)	7.8	8.0
Indigenous background	15.5	13.8
Household head gender	5.8	5.9
Household head living with partner	1.0	0.8
Secondary school in the locality	0.4	0.4
Household head education	11.7	11.2
Spouse education	19.8	20.8

NOTE—In part (a), columns 2 and 3 show the fraction of children that have the characteristic mentioned in the first column, as a percentage of all the children in the sample. In part (b), columns 2 and 3 show the fraction of children for which the characteristic mentioned in the first column is missing, as a percentage of all the children in the sample.

Ninety percent of the sample lives in a household whose head is male and cohabitates with his spouse. One-quarter of the children in the sample have access to a secondary school in their locality of residence. The parental education levels are low, particularly for children that completed grade 6.

3.3. Empirical methodology

We now estimate the probability of re-enrollment for children in the sample. For each grade completed before the start of the school year, we perform a logistic regression. In the regression, school re-enrollment is a binary dependent variable ($Y_i = 1$ if child i is enrolled in the next grade and $Y_i = 0$ otherwise). The K dimensional vector, X_i , comprises the circumstances (X_{ik} is child i 's value for circumstance k). Finally, the dummy treatment variable, T_i , indicates whether the child participated in the program ($T_i = 1$) or not ($T_i = 0$). We also include interaction terms between circumstances and the treatment dummy variable. These interaction effects allow the treatment to have different effects for children facing different circumstances (e.g., girls versus boys or indigenous versus non-indigenous children). The specification is standard:

$$Prob(Y_i = 1) = \frac{\exp(\beta_0 + \sum_{k=1}^K \beta_k X_{ik} + \gamma_0 T_i + \sum_{k=1}^K \gamma_k T_i X_{ik})}{1 + \exp(\beta_0 + \sum_{k=1}^K \beta_k X_{ik} + \gamma_0 T_i + \sum_{k=1}^K \gamma_k T_i X_{ik})}. \quad (9)$$

The estimated values of the coefficients $\beta_0, \beta_1, \dots, \beta_K$ and $\gamma_0, \gamma_1, \dots, \gamma_K$ are used to generate two estimated probabilities for every child: p_i^T , the predicted probability of re-enrollment if the child is “treated”; and p_i^N , the corresponding

probability if the child is “not treated.” Observe that, if two children, say i and j , have a different predicted probability ($p_i^Z \neq p_j^Z$, $Z = T$ or N), we can attribute this to differences in circumstances. Hence, the inequality in the vector of predicted probabilities, p^T (or p^N), is entirely due to children facing different circumstances, and is, for that reason, a measure of inequality of opportunity amongst children participating (not participating) in the program. Then, the average re-enrollment can be computed easily, with $Z=T$ or N :

$$C^Z = \frac{1}{n} \sum_{i=1}^n p_i^Z. \quad (10)$$

The (Generalized) Lorenz curves can be computed in the usual manner (see Appendix), and the decomposition of the Human Opportunity Index and the Gini Opportunity Aggregator is based on Equation (4).

4. EMPIRICAL RESULTS

4.1. Logistic regression

We estimate a logistic regression for each grade, where we estimate the probability that a child is re-enrolled as a function of her or his circumstances.⁷ The results show that treated children, boys and indigenous children, have a higher probability of re-enrollment. Moreover, parental education plays an important role: living in a household where the head has a higher level of education (a proxy for education of the father) and, specially, the spouse of the household head has a higher level of education (a proxy for education of the mother) correlates with a higher probability of school re-enrollment. Living in a locality where a secondary school is present also correlates positively with school re-enrollment, particularly after completion of primary school (grade 6). Indigenous children have a higher probability of re-enrollment, but we must note that we controlled for other features of the household in the estimation, such as the level of education of the household head and the spouse. The impression that indigenous children would have lower rates of re-enrollment than non-indigenous children might be because such children normally live in households with lower-educated parents. No firm conclusions can be drawn about the interaction effects between treatment and circumstances: only few of them are statistically significant.

4.2. Average effect

We use the estimated coefficients to predict the probability of re-enrollment for each child for the treatment case (p_i^T) and for the non-treatment case (p_i^N). We compare the average of these two probabilities per grade and determine which percentage of the children (would) gain from receiving Progresa benefits (i.e., for which $p_i^T > p_i^N$). The results are reported in Table 2.

Column I shows the grade the children completed before the start of the school year. Columns II and III show the sample sizes of the treatment (TS) and control (CS) samples, respectively. The average re-enrollment rates for these samples are shown in columns IV and V. These numbers are also depicted in Figure 1; given the take-up rate of 97% (Gertler 2004), if the treatment and control samples are balanced, their difference is almost an estimate of the average treatment effect. For children that live in a control locality, column VI shows the predicted average probability of re-enrollment in case they were to receive treatment. For children living in a treatment locality, this predicted probability equals the average re-enrollment probability in column IV. For children that live in a treatment locality, column VII shows the predicted average probability of re-enrollment if they did not receive treatment. For children living in a control locality, this predicted probability equals the average re-enrollment probability in column V. Column VIII shows the difference between the average predicted probabilities for the treatment and the non-treatment cases for the entire sample. In view of the high take-up rate (97%), this is almost equal to the conditional average treatment effect. Column IX shows the percentage of children for whom the predicted re-enrollment probability with treatment is larger than without the treatment. The final column, X, shows the conditional average treatment effect estimated by Schultz (2004). They can be compared with the estimates in column VIII. Although the sample selection, the variables included in the regression, and the specification are different from ours, the results are remarkably similar, particularly for the grades for which Schultz’ estimates are significant at 1%, namely, grades 1 to 6.⁸

The following observations can be made based on the aforementioned results. First, the average re-enrollment probabilities in the treatment case (columns IV and VI) as well as in the non-treatment case (columns VII and V)

⁷ We focus on the main thrusts indicated by the coefficients when statistical significance exists. The regression coefficients are reported in Table A.1 in Online Appendix A.

⁸ For the other grades, the significance levels of his conditional average treatment effects are between 0.14 and 0.85.

Table 2. Samples sizes and average (predicted) re-enrollment rates per grade in 1998

(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)	(IX)	(X)
	Sample size		Av. Re-enroll.		Av. p_i^T	Av. p_i^N	Av. $(p_i^T - p_i^N)$	% Gains	Schultz (2004)
Grade	TS	CS	TS	CS	CS	TS	TS $\cup$ CS	TS $\cup$ CS	CATE
0	2,803	1,773	0.786	0.774	0.777	0.781	0.004	0.505	-0.002
1	2,139	1,333	0.973	0.947	0.973	0.947	0.026	0.790	0.022
2	2,116	1,316	0.943	0.920	0.940	0.924	0.019	0.700	0.020
3	2,011	1,202	0.928	0.889	0.926	0.890	0.038	0.814	0.032
4	1,773	1,078	0.925	0.880	0.923	0.885	0.041	0.709	0.041
5	1,640	979	0.914	0.870	0.913	0.872	0.043	0.847	0.047
6	2,555	1,519	0.520	0.397	0.518	0.400	0.120	0.985	0.111
7	803	474	0.929	0.930	0.929	0.929	-0.070	0.542	0.013
8	645	345	0.912	0.890	0.913	0.884	0.026	0.631	0.001
9	449	254	0.454	0.433	0.462	0.439	0.021	0.560	0.066

NOTE— $TS(CS)$ stands for the sample of children that lived in a treatment (control) locality. $TS \cup CS$ indicates that the union of both samples is used. The numbers in the last column are the conditional average treatment effects (CATE) for children from poor households estimated by Schultz (2004).

for TS and CS are very close to each other. This suggests that the differences in re-enrollment rates between the two samples are not due to differences in composition of the circumstances we incorporated. Second, the differences between the average predicted re-enrollment probabilities, p_i^T and p_i^N , for both TS (column IV versus VII) and CS (column VI versus V) are very small for grades 0 and 7. Here, the treatment has little effect on the school re-enrollment probabilities. The differences are largest for grade 6, that is, for children who completed primary education. This, in turn, translates to a greater conditional average treatment effect for this grade in column VIII. Hence, Progresá induces children to stay in school after completion of primary education. Finally, the last column confirms that for most children, the predicted probability of re-enrollment is greater with treatment than without it. The percentage that gains is relatively lower (below 56%) for grades 0, 7, and 9, but high (above 70%) for children who completed grades 1 to 6. Overall, more children seem to gain, than lose, from the program. Yet, the questions regarding the program's influence on aggregated opportunities when we consider inequality of opportunity, or the program's effect on inequality of opportunities, remain unanswered.

4.3. Dominance

We note that the results for grade 6 are more salient. Further, children are more prone to drop out in this grade. Thus, we first focus on grade 6. In Figure 2, panel (a) depicts the Generalized Lorenz curves and panel (b) depicts the Lorenz curves with and without treatment.

At 1% level of significance, the Generalized Lorenz curve when participating in Progresá lies above the curve for not participating in it for all deciles. This suggests that Progresá improves aggregate opportunities.⁹ Panel (b) shows that the Lorenz curve when participating in Progresá is never below the curve for not participating in it. Moreover, for the bottom decile, the difference between the coordinates is statistically significant at 10%; for the next decile, at 5%; and for higher deciles, at 1%. Hence, we can infer that Progresá reduced inequality of opportunity in grade 6.

The results for the other grades can be found in Online Appendix C. Table C.1 and Table C.2 report the Generalized Lorenz and Lorenz dominance results, respectively. The differences between the Generalized Lorenz and Lorenz curves are not significant for grades 0, 7, 8, and 9, but highly significant for grades 1 to 6.

4.4. Decomposing the effect

⁹ All tests for significance in Figure 2, Tables 3, 4, 5, C.1, and C.2 are based on the Bootstrap procedure described in Online Appendix B.

Figure 2. Generalized Lorenz and Lorenz curves for grade 6

(a) Generalized Lorenz curves

(b) Lorenz curves

NOTE—*** indicates that the null hypothesis—that the upper curve coincides with the lower curve—has to be rejected against the alternative hypothesis—that it lies above the lower curve—at 1% significance; **, at 5%; and *, at 10%. Significance is based on confidence intervals constructed using the Bootstrap procedure described in Online Appendix B.

Table 3. The Human Opportunity Index (all entries multiplied by 100)

(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)	(IX)
Grade	H^N	ΔH	C^N	ΔC	D^N	ΔD	Δ_{CH}	Δ_D
0	73.7	0.328	77.8	0.444	5.232	0.119	0.409	-0.093
1	93.3	3.128 ***	94.7	2.604 ***	1.449	-0.578 **	2.555 ***	0.555 **
2	90.2	2.337 **	92.2	1.941 **	2.246	-0.467	1.880 **	0.435
3	87.0	4.438 ***	89.0	3.762 ***	2.270	-0.821 **	3.650 ***	0.746 **
4	85.3	5.993 ***	88.3	4.135 ***	3.418	-2.163 ***	3.968 ***	1.955 ***
5	84.9	4.419 ***	87.1	4.254 ***	2.568	-0.300	4.097 ***	0.267
6	34.2	12.824 ***	39.9	11.992 ***	14.283	-4.903 ***	9.717 ***	2.251 ***
7	91.6	-0.229	93.0	-0.067	1.422	0.175	-0.066	-0.163
8	86.1	3.445 *	88.6	2.614	2.807	-0.991	2.517	0.891
9	37.4	4.003	43.7	2.067	14.347	-4.883	1.673	2.182

NOTE—Significance at 1%, 5%, or 10% is indicated by ***, **, and *, respectively, and is based on confidence intervals constructed using the Bootstrap procedure described in Online Appendix B.

The next step in our analysis is to decompose the effect of Progresa on the Human Opportunity Index and the Gini Opportunity Aggregator into an effect on average opportunities and an effect on inequality of opportunity. Table 3 reports the results for the Human Opportunity Index, defined in (6).

Column I shows the grade completed before the start of the school year. Column II shows the value of the Human Opportunity Index; column IV, the average re-enrollment rate; and column VI, the value of the dissimilarity index. All values are based on the estimated probability under the non-treatment case (p^N). The change in the Human Opportunity Index, the average re-enrollment rate, and the dissimilarity index due to treatment are shown in columns III, V, and VII, respectively. The last two columns decompose the change in the Human Opportunity Index. Column VIII shows the part of the change in the Human Opportunity Index that can be attributed to an increase in average re-enrollment, and column IX, the part of the change that can be attributed to changes in the dissimilarity index. As the values of all level statistics are determined very precisely (columns II, IV, and VI), we only report whether or not the differences are statistically significant (columns III, V, VII, VIII, and IX).

The following conclusions can be drawn based on the results in the table. First, the values of the Human Opportunity Index are very high for all grades, except for grades 6 and 9, which follow primary and secondary school completion, respectively. Second, there are gains from treatment in terms of the Human Opportunity Index for almost all grades, except grade 7. However, the decrease in the Human Opportunity Index is marginal and not statistically significant (column III). Third, the positive effect observed in grades 1 to 6 is due to an increase in average re-enrollment (column V) and to a decrease in the dissimilarity index (column VII), although the latter is not statistically significant for grades 2 and 5. In fact, it contributed between 16% and 32% to the increase in the Human Opportunity Index, except in grade 5, where it only contributed 6%.

Table 4. The Gini Social Welfare Function (S) (all entries multiplied by 100)

(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
Grade	S^N	ΔS	DGN	ΔG	Δ_{CG}	Δ_G
0	72.1	0.324	7.34	0.113	0.460	-0.088
1	92.8	3.389 ***	2.04	-0.862 **	2.641 ***	0.827 **
2	89.3	2.621 **	3.14	-0.788	1.979 **	0.734 *
3	86.1	4.753 ***	3.24	-1.199 **	3.846 ***	1.090 **
4	84.1	6.657 ***	4.73	-2.940 ***	4.294 ***	2.657 ***
5	84.1	4.401 ***	3.43	-0.320	4.334 ***	0.286
6	32.4	12.893 ***	18.72	-6.061 ***	13.478 ***	2.782 ***
7	91.1	-0.198	1.97	0.141	-0.068	-0.131
8	85.1	3.749	3.93	-1.357	2.683	1.220
9	35.0	4.647	19.73	-6.534	2.338	2.920 *

NOTE—Significance at 1%, 5%, or 10% is indicated by ***, **, and *, respectively, and is based on confidence intervals constructed using the Bootstrap procedure described in Online Appendix B.

Table 4 reports the results for the Gini Opportunity Aggregator defined in (8). We follow the format in Table 3, but do not repeat the results for average re-enrollment, because they are identical to the ones reported in Table 3. The only difference between the results for the Gini Opportunity Aggregator and the Human Opportunity Index is that the effects on the former are, by and large, marginally greater. The results tell the same story: Progresá not only improved average re-enrollment rates of children, but also decreased inequality of opportunity. The effects are statistically significant for grades 1 to 6 and are particularly strong for children that completed primary education (grade 6). In the grades for which the Gini Opportunity Aggregator increased significantly at 5% (i.e., grades 1 to 6), the decrease in inequality of opportunity, Δ_G , is not statistically significant for grades 2 and 5. It contributed between 22% and 39% to the improvement in the value of the Gini Opportunity Aggregator, except for grade 5, where it only contributed 6%.

4.5. Role of individual circumstances

The results for the Human Opportunity Index and the Gini Opportunity Aggregator are completely in line with each other and with the (Generalized) Lorenz dominance results. To explain these results, we return to the estimated regressions for school re-enrollment.

In the context of a linear regression model, Morabito et al. (2018) argue that a program equalizes for a circumstance if the circumstance's impact without program participation is greater than its impact with participation. This occurs when the absolute value of the regression coefficient associated with this characteristic is greater than the absolute value of the sum of the regression coefficient and the coefficient associated with the interaction of the characteristic and

the program participation dummy. When the opposite occurs, the program is “disequalizing” for that characteristic. To understand the presence and size of the (dis)equalizing effects, we work with a linear probability model in this section. The estimates of this model are reported in Online Appendix E and are entirely in line with the estimates of the logistic model.

In Table 5, we summarize the results for all grades. Part (a) describes the effects of individual circumstances on re-enrollment probabilities for each grade, estimated by the linear probability model. The first row shows the estimated effect of the treatment dummy. Program participation has strong, positive effects in grades 0, 4, and 6. A positive (negative) coefficient for the treatment dummy leads to a higher percentage increase (decrease) in the estimated probability of school re-enrollment for children with a lower re-enrollment probability. Hence, a positive (negative) treatment dummy has a progressive (regressive) effect on the distribution of estimated school re-enrollment probabilities. The estimates indicate that this effect is equalizing for seven out of ten grades. The rows show the circumstances’ effect on re-enrollment probabilities in case the child did not participate (N) and did participate (T). For instance, in grade 0, program participation reduced the effect of gender on the re-enrollment probability from 4.1% to 1.1%. For 90% of the households, the values of the dummies for the gender of the household head and for whether the household head lived with a spouse are identical. Hence, we aggregated them to one variable, “Household head characteristic,” whose coefficients are defined as the sum of the coefficients of the gender of the household head and whether the household head is married. If the absolute value of the effect of a circumstance on re-enrollment is smaller under treatment, than under no treatment, the treatment diminishes the effect of this circumstance on school re-enrollment. Therefore, the treatment is equalizing for this circumstance. This occurs for 50 out of the 80 estimates reported in the Table. The program equalizes most often for the effects of gender (for eight out of ten grades), the presence of a secondary locality, and spouses having completed primary education (seven out of ten grades). In combination with the positive effects in the first row, this result indicates that, overall, the equalizing forces outnumber the disequalizing forces.

Part (b) summarizes the Generalized Lorenz and Lorenz dominance results. The asterisks indicate the maximal level of significance found for any of the deciles, and the numbers below the asterisks indicate the deciles for which this maximal significant difference was found. The dominance results are not entirely driven by the treatment dummy. For grade 0, the estimated dummy was significantly positive at 10%, but no (Generalized) Lorenz dominance could be found. For grades 2 and 9, the estimated treatment dummy was insignificant and negative, yet we found (Generalized) Lorenz dominance in favor of the program. In grades 3 and 5, the estimated treatment dummy was insignificantly positive, and we found strong (Generalized) dominance results. In all these instances, the interaction effects of the circumstances and the treatment dummy are crucial for establishing the (Generalized) Lorenz dominance results.

Analyzing part (a) of Table 5 allows us to further examine why no dominance can be established for some grades. In grades 0 and 7, we find a mixture of equalizing and disequalizing effects. In grades 8 and 9, the equalizing effects occur more frequently; they are often greater than the disequalizing effects. However, our sample sizes are less than one-third of the sample sizes for grades 0–6 (see columns II and III in Table 2). This sample size decreases the precision of the estimated parameters and predicted probabilities. Hence, only a Lorenz dominance relationship can be found for deciles 2–4 in grade 9 at a 10% level of significance.

Now, let us focus on grades 1–6, that is, the grades with clearly established dominance relationships. Dominance in grade 1 can be attributed to the equalizing effect of the program for both parents’ education level. In grade 2, equalization can be attributed to the program’s effect on the influence of the father’s education level. In grade 3, the main equalizing force is on the mother’s education level. While the latter is important for grade 4, the treatment dummy is important too. The re-enforcing effect of treatment on spouses with a low education level explains why we found no dominance at the bottom of the distribution for grade 5. In grade 6, we observe serious disequalizing effects for spouse’s education, which, however, prevents us from establishing dominance because of the positive treatment dummy and the equalization for the presence of a secondary locality.

Interestingly, at the entry into education (grade 0) and into higher levels of education (grades 6 and 9), our estimated models find a higher school re-enrollment probability for boys. Progresá diminishes male advantage at these three points. Simultaneously, the program does not seem to equalize for parental education at these entry points. In grades 6 and 9, without the program, the circumstance with the greatest effect on school re-enrollment is the presence of a secondary school in the locality. This effect is substantially reduced by the program. Observe that without participation in Progresá, the presence of a secondary school is an advantage in grade 6, but a disadvantage in grade 9. This stands to reason: after completing primary school, it is less costly to go to secondary school in localities where

Table 5. Summary of the results

	Grade									
	0	1	2	3	4	5	6	7	8	9
(a) Effect of circumstances on re-enrollment probabilities										
Treatment	10.4	1.1	-1.1	3.3	18.0	4.9	16.1	-2.5	2.3	-5.7
Ch gen N	4.1	-1.7	1.7	-2.1	1.7	0.8	6.6	2.8	-1.5	13.2
Ch gen T	1.1	2.0	0.9	2.6	-0.7	-0.3	4.4	1.6	-1.2	5.3
Ind N	0.7	0.0	-1.8	4.4	2.2	6.6	8.2	3.1	5.3	16.0
Ind T	-6.0	1.2	-0.6	0.3	2.0	4.6	9.1	-1.5	6.4	14.3
Hh char N	6.7	6.9	-1.2	3.0	6.6	-2.3	3.3	0.3	4.7	-2.1
Hh char T	-0.9	7.8	5.4	4.1	-4.0	-3.9	-1.4	0.4	1.6	0.7
Sec loc N	2.4	0.7	2.3	-0.1	4.3	3.9	24.8	3.7	-3.0	-18.9
Sec loc T	4.4	1.5	0.3	-0.8	2.2	3.1	17.9	0.8	0.3	-3.7
Hh inc p N	13.1	2.6	5.2	2.7	0.2	1.9	1.7	-1.7	-8.9	-8.3
Hh inc p T	9.7	2.1	1.9	3.0	1.6	1.0	4.4	3.7	-3.5	-3.7
Hh com p N	12.8	4.3	5.3	6.6	6.9	3.8	14.6	-0.9	-13.7	-7.4
Hh com p T	19.0	1.9	4.5	6.5	4.0	3.7	13.1	4.6	-14.5	8.4
Sp inc p N	4.1	2.8	4.1	2.1	7.5	0.4	2.6	-0.1	5.8	14.1
Sp inc p T	6.5	1.7	4.1	0.2	2.9	7.0	3.9	3.5	3.3	10.0
Sp com p N	15.0	5.6	9.2	8.2	11.0	8.6	11.9	3.4	13.8	9.9
Sp com p T	10.4	3.2	13.0	3.1	3.7	8.3	16.4	5.0	-1.9	7.3
(b) Lorenz dominance results										
GLD		***	**	***	***	***	***			
		3-10	4-10	2-10	1-10	5-10	1-10			
LD		***	**	**	***	**	***			*
		6-9	7-9	4-9	1-9	9	3-9			2-4

NOTE—In part (a), the first row shows the effect of program participation. The following rows show the effect of circumstances in the absence of program participation (N), and then with program participation (T). In part (b), the asterisks indicate the highest level of significance found for the differences between the Generalized Lorenz and Lorenz curves, respectively. *** denotes this difference is significant at 1%; **, at 5%; and *, at 10%. The numbers below the asterisks identify the decile range for which this dominance is found.

there is a secondary school. In these communities, more children for whom the costs of schooling are important finish secondary school. When they seek re-enrollment, they are required to join a high school that may not be situated within their locality.

For grades 1–4, overall, the program equalizes for parental schooling levels. However, for higher grades, the program's impact on the effect of parental education is ambiguous. For children who completed primary education (grade 6), the program even increases the effect of the spouse's schooling level on school re-enrollment.

5. CONCLUSION

Many social programs aim to improve opportunities for children, particularly for those who have access to insufficient opportunities due to their circumstances. From this perspective, it is surprising that most program evaluation studies only identify an average treatment effect. Some studies focus on children with particular circumstances (girls versus boys or indigenous versus non-indigenous) to identify which groups benefit most from the program. However, to the best of our knowledge, no study has attempted to obtain an overall assessment of the effect of a social program such as

Progresa on the distribution of school re-enrollment among children facing different circumstances. Our study is the first attempt at assessing Progresa's impact by considering its effect on the distribution of opportunities for children.

We found inspiration in extant literature that focuses on quantifying the extent to which opportunities are unequally distributed. A child's probability of being re-enrolled, conditional on her or his circumstances, is seen as a measure of the opportunities available to her or him. Inequalities in these probabilities can be attributed exclusively to children facing different circumstances. Hence, a social planner who wants to evaluate the distribution of opportunities should favor the redistribution (i.e., transfer) of opportunities from those with higher to those with lower opportunities. The evaluation framework we developed incorporates this concern.

Progresa provides conditional cash transfers for children from grade 3 in primary school onward. It seems understandable that we can only identify a statistically significant effect on children who completed at least grade 2, such that they are enrolled in grade 3 or higher. Yet, we also found statistically significant effects for children who only completed grade 1. This result could be attributed to anticipation effects, or perhaps such children benefit indirectly from the transfers the family receives for older siblings.¹⁰ Moreover, the sample sizes become smaller for children who completed grade 7 or higher, thereby increasing the difficulty in identifying statistically significant effects for these grades. For the other grades, overall, the distribution of re-enrollment probabilities conditional on circumstances for the treatment case Generalized Lorenz and Lorenz dominates the distribution for the non-treatment case. Generalized Lorenz dominance implies that any opportunity aggregator that is non-decreasing in children's opportunities, aggregates them in an anonymous manner and is inequality averse with respect to the distribution of these opportunities must find that Progresa increases aggregate opportunities. Lorenz dominance implies that whenever inequality of opportunity is measured by a relative inequality measure, Progresa decreases inequality of opportunities. Finally, when the Human Opportunity Index or the Gini Opportunity Aggregator is chosen to aggregate children's opportunities, except for grade 5, the effect of the reduction in inequality of opportunity by the program accounts for 15% to 40% of the effect of the program on aggregate opportunities. Hence, we found clear evidence that Progresa not only improved school re-enrollment opportunities in the aggregate and on average, but also significantly and substantially reduced inequality of school re-enrollment opportunities among children from households eligible to receive support from it.

The results we presented leave several questions unanswered. First, whether or not Progresa equalized school re-enrollment opportunities for all children in Mexico depends on the way it is financed and the effect thereof on the opportunities of children of non-participating families. Second, we evaluated the effects on school re-enrollment for the first year Progresa was implemented. The methodology can be readily extended to evaluate longer-run effects of the program. However, such an examination is not straightforward in the case of Progresa, because the households in the control group started receiving treatment a few months after the first school year ended. We can readily apply the procedures developed herein using the techniques in Behrman et al. (2005) and (2011) in order to estimate the long-run effects of the program and construct the expected outcomes thereof. Third, we only examined re-enrollment opportunities, but did not consider other outcomes, such as grade progression. Even if the program had no effects on re-enrollment it could affect grade advancement. The methods we developed can be used to study the effect on these alternative outcomes. Fourth, we abstracted from the general equilibrium effects of the program. Again, the tools we propose can accommodate this, but only if the expected outcomes are constructed to incorporate general equilibrium effects. The approach used in Debowicz and Golan (2014) links micro- and macro-simulation models and can, in principle, be used.

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¹⁰ According to Ferreira et al. (2017), the effects of transfers received for older siblings not necessarily run in the same direction as anticipation effects.

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APPENDIX

COMPUTATION OF THE (GENERALIZED) LORENZ CURVES

Rearranging the elements in vectors p^T and p^N increasingly results in ordered vectors $\hat{p}^T$ and $\hat{p}^N$, respectively. To construct the Generalized Lorenz curves, we define each decile d with $d = 1, \dots, 10$, M_d^T as the value of the coordinate of the Generalized Lorenz curve for the treatment case and M_d^N for the non-treatment case. The value of the coordinate at decile d is given by:

$$M_d^Z = \sum_{i=1}^{\tilde{d}(d)} \hat{p}_i^Z, \quad (1)$$

where $Z=T$ with treatment, but N without treatment; $\tilde{d}(d)$ is the integer that is closest to $[n/10] * d$.¹¹ The difference between the coordinates of the Generalized Lorenz curve for the treatment and non-treatment cases is $M_d^D = M_d^T - M_d^N$. Similarly, for decile d , we define L_d^T as the value of the coordinate of the Lorenz curve for the treatment case, and L_d^N for the non-treatment case, with their difference being $L_d^D = L_d^T - L_d^N$. The value of the coordinate of the Lorenz curve at decile d is given by:

$$L_d^Z = \frac{1}{nC^Z} \sum_{i=1}^{\tilde{d}(d)} \hat{p}_i^Z, \quad (2)$$

where $Z=T$ with treatment, but N without treatment; C^Z is the value of the average predicted probability p_i^Z , as shown in (10). $\tilde{d}(d)$ is as above.

¹¹ Given the computational burden that arises with the Bootstrap procedure explained in Online Appendix B, we only examine the values at each decile.

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Online Appendix
Did Progresá Reduce Inequality of Opportunity for School Re-enrollment?

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APPENDIX

A. LOGISTIC REGRESSION FOR SCHOOL RE-ENROLLMENT PER GRADE

Table A.1. Estimated coefficients of the logistic model for school re-enrollment per grade

	Grade achieved before beginning school year 1998									
	0	1	2	3	4	5	6	7	8	9
T	0.554*	-0.0979	-0.196	0.156	1.607***	0.417	0.719***	-0.350	0.112	-0.231
	(0.312)	(0.755)	(0.429)	(0.507)	(0.579)	(0.482)	(0.259)	(0.715)	(0.811)	(0.686)
Ch gender	0.246**	-0.351	0.228	-0.213	0.173	0.0798	0.294***	0.435	-0.192	0.582**
	(0.106)	(0.289)	(0.206)	(0.208)	(0.208)	(0.195)	(0.112)	(0.345)	(0.363)	(0.268)
Ind	0.0468	0.0668	-0.182	0.494*	0.255	0.663**	0.367**	0.551	0.582	0.694*
	(0.200)	(0.308)	(0.264)	(0.292)	(0.236)	(0.295)	(0.165)	(0.456)	(0.497)	(0.356)
Hh gender	-0.125	-0.421	-0.755**	-0.428	-0.132	-0.0917	0.131	0.610	0.487	0.297
	(0.247)	(0.547)	(0.363)	(0.385)	(0.446)	(0.290)	(0.183)	(0.649)	(0.863)	(0.593)
Hh married	0.451*	0.838*	0.505	0.627*	0.590	-0.100	0.0185	-0.628	-0.0668	-0.408
	(0.256)	(0.487)	(0.395)	(0.368)	(0.378)	(0.378)	(0.207)	(0.790)	(1.006)	(0.600)
Sec loc	0.166	0.170	0.371	0.0204	0.556**	0.394	1.055***	0.619	-0.357	-0.825**
	(0.219)	(0.413)	(0.243)	(0.210)	(0.248)	(0.363)	(0.143)	(0.432)	(0.393)	(0.348)
Hh inc prim	0.705***	0.410	0.618**	0.234	-0.0433	0.143	0.0766	-0.277	-1.024**	-0.371
	(0.169)	(0.455)	(0.276)	(0.214)	(0.257)	(0.229)	(0.138)	(0.406)	(0.443)	(0.389)
Hh com prim	0.709***	1.085*	0.676	0.772**	1.156**	0.381	0.632***	-0.142	-1.508**	-0.321
	(0.217)	(0.577)	(0.461)	(0.356)	(0.466)	(0.385)	(0.228)	(0.678)	(0.588)	(0.390)
Sp inc prim	0.220	0.514	0.525**	0.234	0.781**	0.0429	0.117	-0.00734	0.586	0.629*
	(0.213)	(0.371)	(0.227)	(0.232)	(0.319)	(0.208)	(0.163)	(0.417)	(0.410)	(0.380)
Sp com prim	1.231***	1.975***	2.978***	1.340***	1.637***	1.219**	0.520**	0.634	1.568**	0.434
	(0.326)	(0.741)	(0.957)	(0.504)	(0.574)	(0.529)	(0.222)	(0.698)	(0.661)	(0.464)
Ch gen * T	-0.178	1.167***	-0.0556	0.610**	-0.285	-0.121	-0.103	-0.185	0.0267	-0.361
	(0.139)	(0.384)	(0.270)	(0.250)	(0.273)	(0.307)	(0.146)	(0.440)	(0.480)	(0.322)
Ind * T	-0.394*	0.391	0.0576	-0.451	0.0651	0.0323	0.0215	-0.768	0.402	-0.103
	(0.226)	(0.453)	(0.376)	(0.351)	(0.387)	(0.406)	(0.224)	(0.523)	(0.662)	(0.464)
Hh gen * T	0.130	0.123	0.282	0.379	-0.0955	-0.426	-0.465**	-0.600	-0.627	-0.668
	(0.308)	(0.663)	(0.460)	(0.569)	(0.574)	(0.430)	(0.227)	(0.767)	(0.896)	(0.680)
Hh mar * T	-0.514	-0.0379	0.551	-0.134	-0.898*	0.187	0.251	0.670	0.345	0.813
	(0.316)	(0.665)	(0.549)	(0.518)	(0.514)	(0.494)	(0.255)	(0.922)	(1.043)	(0.696)
Sec loc * T	0.116	0.548	-0.312	-0.125	-0.206	0.0875	-0.293	-0.500	0.381	0.668
	(0.274)	(0.539)	(0.388)	(0.302)	(0.378)	(0.458)	(0.214)	(0.529)	(0.468)	(0.408)
Hh inc prim * T	-0.222	0.314	-0.398	0.107	0.243	-0.0983	0.106	0.784	0.515	0.212
	(0.223)	(0.558)	(0.388)	(0.265)	(0.322)	(0.303)	(0.169)	(0.517)	(0.523)	(0.464)
Hh com prim * T	0.586*	-0.274	0.511	0.350	-0.462	0.214	-0.0645	0.825	0.871	0.671
	(0.312)	(0.743)	(0.750)	(0.495)	(0.636)	(0.441)	(0.285)	(0.836)	(0.727)	(0.486)
Sp inc prim * T	0.158	-0.143	0.196	-0.209	-0.384	0.856***	0.0482	0.536	-0.172	-0.209
	(0.226)	(0.495)	(0.385)	(0.273)	(0.411)	(0.326)	(0.184)	(0.543)	(0.554)	(0.439)
Sp com prim * T	-0.417	-0.571	-1.562	-0.565	-1.046	0.112	0.211	0.277	-1.733**	-0.129
	(0.387)	(0.940)	(1.122)	(0.659)	(0.701)	(0.666)	(0.258)	(0.904)	(0.825)	(0.583)
Ch gen m	-0.298*			0.239					0.708	
	(0.154)			(0.278)					(0.582)	

Table A.1 continued on next page

Table A.1 (*continued*)

Grade achieved before beginning school year 1998										
	0	1	2	3	4	5	6	7	8	9
Ind m									-0.534 (0.529)	
Hh gen m			-1.506*** (0.533)						-1.187** (0.560)	
Hh mar m	1.332*** (0.410)				-1.313** (0.549)					
Sec loc m	0.457*** (0.0982)									
Hh ed m	0.491*** (0.166)		1.141*** (0.400)	0.610** (0.279)						
Sp ed m		0.932*** (0.283)							0.285 (0.481)	
Constant	0.155 (0.263)	1.797*** (0.567)	1.818*** (0.427)	1.388*** (0.379)	0.879** (0.368)	1.538*** (0.303)	-1.265*** (0.203)	2.176*** (0.649)	2.252*** (0.862)	-0.440 (0.623)
# obs	4,576	3,472	3,432	3,213	2,851	2,619	4,074	1,277	990	703

NOTE—All variables in the regression are dummy variables: they are either 1 or 0. We indicate when they are equal to 1. Variable T: equal to 1 if the child is treated; Ch gender: equal to 1 if the child is a boy; Ind: equal to 1 for indigenous children; Hh gender: equal to 1 if the household head is male; Hh married: equal to 1 if the household head is married; Sec loc: equal to 1 if there is a secondary school in the locality where the child resides; Hh inc prim (Sp inc prim): equal to 1 if the (spouse of the) household head has some schooling, but did not complete primary education; Hh com prim (Sp comp prim): equal to 1 if the (spouse of the) household head completed at least primary schooling; Ch gen m: equal to 1 if the child's gender is missing; Ind m: equal to 1 if the indigenous background of the child is missing; Hh gen m: equal to 1 if the household head's gender is missing; Hh mar m: equal to 1 if the household head's marital status is missing; Sec loc m: equal to 1 if we do not know whether there is a secondary school in the locality where the child resides; Hh ed m (Sp ed m): equal to 1 if the (spouse of the) household head's education is missing. An empty cell for a missing circumstance dummy indicates that the dummy was perfectly collinear with (a subset of) the other dummies or that its coefficient was not significant in the original specification (one that included a dummy for each missing characteristic). We retain only the missing dummies that are significant (in the original specification) in order to avoid situations wherein the dependent variables perfectly predict school re-enrollment. This was important for the Bootstrap procedure described in Appendix B. In some cases, variables that were originally significant are no longer significant in the final specification, as is the case for "Ch gen m" in grade 3. Standard errors in parenthesis are adjusted for clustering at the locality level. *** denotes significance at 1%; **, at 5%; and *, at 10%.

B. BOOTSTRAP PROCEDURE

The bootstrap procedure is based on the following steps.

Step 0: $k = 0$;

Step 1: Increase the value of k by 1. For a grade with a sample of size n , take a random sample of n elements from this sample with replacement, and estimate the logistic specification described in section 3.3 in the main text. Define the coefficient vector associated with circumstances as β_c , the one associated with missing circumstances as β_m , and the one associated with interactions between circumstances and treatment as β_{ct} .

Step 2: for $i = 1$ to n compute the predicted values:

- p_i^T is the probability of re-enrollment if treated;
- p_i^N is the probability of re-enrollment if not treated.

Step 3: From step 2, for some observations, one or more coefficients perfectly predict school re-enrollment. To these observations, we assign a predicted school re-enrollment equal to 0 or 1 as follows:

- If β_c or β_m perfectly predicts being re-enrolled in school, $p_i^T = p_i^N = 1$. If β_c or β_m perfectly predicts not-being re-enrolled, $p_i^T = p_i^N = 0$.
- If β_{ct} perfectly predicts being re-enrolled in school, $p_i^T = 1$. If β_{ct} perfectly predicts not-being re-enrolled, $p_i^N = 0$.

Step 4: Use the re-enrollment probabilities obtained after step 3 to compute the following:

- (i) All the statistics mentioned in Tables 2, 3, and 4 in the main text;
- (ii) The Lorenz and Generalized Lorenz coordinates at each decile for each grade.

Store the results of (i) and (ii) in the $k - th$ row of a matrix A .

Step 5: Repeat steps 1–4 2,999 times. We end up with a matrix A of dimension $[3000, B]$, with B being the total number of statistics computed in each loop.

Step 6: Use the empirical distribution obtained for each statistic to construct its two-sided 99%, 95%, and 90% confidence intervals.

Step 7: Perform significance tests for each statistic computed. The two-sided test at a significance level α verifies whether 0 is included in the $1 - \alpha$ % confidence interval.

Step 8: Finally, compute a statistical test for the joint significance of the difference in (Generalized) Lorenz curves at all deciles. We compute the fraction f of the empirical distribution for which the difference is positive at all deciles and interpret $1 - f$ as the level of significance at which we can reject the null hypothesis of absence of dominance.

C. GENERALIZED LORENZ AND LORENZ DOMINANCE RESULTS FOR ALL GRADES

Table C.1. Generalized Lorenz coordinates per grade

Decile	Grade									
	0	1	2	3	4	5	6	7	8	9
1 (T)	268.96	319.31	292.32	276.56	246.25	213.99	145.74	110.06	82.06	20.40
1 (N)	268.15	300.11	279.92	255.83	209.62	209.26	98.25	110.52	74.34	14.16
1 (T-N)	0.81	19.20**	12.40	20.73**	36.63***	4.73	47.49***	-0.46	7.72	6.24
2 (T)	572.95	650.12	602.48	564.21	500.88	434.21	308.55	222.39	167.66	44.76
2 (N)	569.74	612.76	576.39	525.08	438.23	422.77	209.07	223.57	155.43	31.95
2 (T-N)	3.21	37.34**	26.09*	39.13***	62.65***	11.44	99.48***	-1.18	12.23	12.81
3 (T)	898.91	985.90	917.42	856.44	759.44	666.13	485.37	338.02	255.63	71.33
3 (N)	893.70	932.86	883.29	801.85	674.56	638.65	326.36	340.12	239.47	54.49
3 (T-N)	5.19	53.04***	34.13*	54.59***	84.88***	27.48*	159.01***	-2.10	16.16	16.84
4 (T)	1,244.86	1,322.40	1,235.53	1,150.13	1,020.97	903.35	667.02	455.79	345.08	99.70
4 (N)	1,238.06	1,257.43	1,195.42	1,079.94	920.56	857.43	454.86	458.23	326.20	80.74
4 (T-N)	6.80	64.97***	40.11**	70.19***	100.41***	45.92**	212.16***	-2.44	18.88	18.96
5 (T)	1,597.93	1,661.83	1,561.99	1,446.29	1,284.30	1,144.23	863.28	574.35	435.03	130.08
5 (N)	1,596.59	1,584.11	1,513.32	1,364.06	1,173.90	1,082.53	593.63	576.35	414.08	109.42
5 (T-N)	1.34	77.72***	48.67**	82.23***	110.40***	61.70***	269.65***	-2.00	20.95	20.65
6 (T)	1,967.78	2,002.89	1,891.05	1,747.06	1,550.07	1,386.05	1,072.43	695.18	526.00	162.43
6 (N)	1,965.12	1,914.73	1,835.03	1,651.15	1,432.00	1,311.39	743.48	696.61	503.60	140.24
6 (T-N)	2.66	88.16***	56.02**	95.91***	118.07***	74.66***	328.95***	-1.434	22.39	22.19
7 (T)	2,344.82	2,344.77	2,221.74	2,048.79	1,817.68	1,633.54	1,299.45	816.91	618.15	197.34
7 (N)	2,340.22	2,247.38	2,159.39	1,942.94	1,694.26	1,545.27	917.02	817.98	594.66	176.49
7 (T-N)	4.60	97.39***	62.35**	105.85***	123.42***	88.27***	382.43***	-1.07	23.49	20.85
8 (T)	2,739.02	2,688.99	2,555.57	2,354.09	2,087.23	1,883.02	1,543.87	939.51	711.66	234.63
8 (N)	2,724.94	2,584.91	2,487.51	2,239.61	1,962.44	1,783.83	1,121.27	940.22	687.26	215.68
8 (T-N)	14.08	104.08***	68.06**	114.48***	124.79***	99.19***	422.60***	-0.71	24.40	18.96
9 (T)	3,149.05	3,033.39	2,892.28	2,664.76	2,359.13	2,135.97	1,809.61	1,061.91	806.70	274.84
9 (N)	3,130.18	2,926.53	2,823.96	2,545.44	2,236.56	2,028.53	1,351.68	1,062.36	781.23	257.09
9 (T-N)	18.87	106.86***	68.32**	119.32***	122.57***	107.44***	457.94***	-0.45	25.48	17.75
10 (T)	3,579.13	3,378.93	3,232.10	2,979.50	2,635.26	2,392.71	2,114.51	1,186.32	902.83	321.43
10 (N)	3,558.83	3,271.24	3,165.49	2,858.62	2,517.37	2,281.29	1,625.95	1,187.18	876.95	306.90
10 (T-N)	20.30	107.69***	66.60**	120.88***	117.89***	111.42***	488.56***	-0.86	25.88	14.53
Joint		**		**	***		***			

NOTE—The table shows, at each decile, the value of the Generalized Lorenz curve for treatment (T) and for no treatment (N), as well as the difference between the two (T-N). Significance of this difference at 1%, 5%, and 10% is indicated by ***, **, and *, respectively. A joint test for dominance can be found in the last row of the table; it indicates whether the difference between the treated and non-treated Generalized Lorenz curve is jointly positive at all deciles. The level of significance for all tests is computed using Bootstrap confidence intervals, as explained in Appendix B.

Table C.2. Lorenz coordinates per grade

Decile	Grade									
	0	1	2	3	4	5	6	7	8	9
1 (T)	0.075	0.095	0.090	0.093	0.093	0.089	0.069	0.093	0.092	0.064
1 (N)	0.075	0.092	0.088	0.090	0.083	0.092	0.060	0.093	0.086	0.046
1 (T-N)	-0.000	0.002	0.002	0.003	0.010***	-0.002	0.009*	-0.000	0.006	0.018
2 (T)	0.160	0.192	0.186	0.189	0.190	0.182	0.146	0.188	0.187	0.140
2 (N)	0.160	0.188	0.182	0.184	0.174	0.185	0.129	0.189	0.179	0.104
2 (T-N)	0.000	0.004*	0.004	0.006*	0.016***	-0.004	0.017**	-0.001	0.008	0.035*
3 (T)	0.251	0.292	0.284	0.287	0.288	0.278	0.230	0.285	0.286	0.222
3 (N)	0.251	0.286	0.279	0.280	0.268	0.280	0.201	0.287	0.276	0.178
3 (T-N)	0.000	0.005**	0.005	0.007*	0.020***	-0.002	0.029***	-0.002	0.010	0.044*
4 (T)	0.348	0.391	0.382	0.386	0.387	0.378	0.315	0.384	0.386	0.311
4 (N)	0.348	0.386	0.378	0.378	0.366	0.376	0.280	0.386	0.375	0.264
4 (T-N)	-0.000	0.006**	0.005	0.008**	0.022***	0.002	0.036***	-0.002	0.010	0.047*
5 (T)	0.447	0.492	0.483	0.485	0.487	0.478	0.408	0.484	0.486	0.405
5 (N)	0.449	0.486	0.478	0.477	0.466	0.475	0.365	0.486	0.477	0.357
5 (T-N)	-0.002	0.006**	0.005	0.008**	0.021***	0.004	0.043***	-0.002	0.010	0.048
6 (T)	0.550	0.593	0.585	0.586	0.588	0.579	0.507	0.586	0.588	0.506
6 (N)	0.552	0.587	0.580	0.578	0.569	0.575	0.457	0.587	0.580	0.458
6 (T-N)	-0.002	0.006***	0.005*	0.009**	0.019***	0.004	0.050***	-0.001	0.008	0.048
7 (T)	0.655	0.694	0.687	0.688	0.690	0.683	0.615	0.689	0.691	0.615
7 (NT)	0.654	0.688	0.682	0.680	0.673	0.677	0.564	0.690	0.684	0.576
7 (T-N)	0.001	0.006***	0.005**	0.008**	0.017***	0.005	0.051***	-0.001	0.007	0.039
8 (T)	0.765	0.796	0.791	0.790	0.792	0.787	0.730	0.792	0.796	0.731
8 (N)	0.766	0.791	0.786	0.784	0.780	0.782	0.690	0.793	0.791	0.704
8 (T-N)	-0.001	0.005***	0.005**	0.007**	0.012***	0.005*	0.040***	-0.001	0.005	0.027
9 (T)	0.880	0.898	0.895	0.894	0.895	0.893	0.856	0.895	0.902	0.856
9 (N)	0.880	0.895	0.892	0.890	0.889	0.889	0.831	0.895	0.899	0.839
9 (T-N)	0.000	0.003***	0.003**	0.004**	0.007***	0.004**	0.025***	-0.000	0.003	0.017
joint		*		*	***		**			

NOTE—The table shows, at each decile, the value of the Generalized Lorenz curve for treatment (T) and for no treatment (N), as well as the difference between the two (T-N). Significance of this difference at 1%, 5%, and 10% level is indicated by ***, **, and *, respectively. A joint test for dominance can be found in the last row of the table; it indicates if the difference between the treated and non-treated Generalized Lorenz curve is jointly positive at all deciles. The level of significance for all tests is computed using Bootstrap confidence intervals, as explained in Appendix B.

D. COMPOSITION OF TREATMENT AND CONTROL SAMPLES

We now test whether the treatment and control groups differ using four sets of variables.

The first set of variables in the first column refers to dummy variables that are the circumstances of the children: they are either 1 or 0. We indicate when they are equal to 1. Variable T: equal to 1 if the child is treated; Ch gender: equal to 1 if the child is a boy; Ind: equal to 1 for indigenous children; Hh gender: equal to 1 if the household head is male; Hh married: equal to 1 if the household head is married; Sec loc: equal to 1 if there is a secondary school in the locality where the child resides; Hh inc prim (Sp inc prim): equal to 1 if the (spouse of the) household head has some schooling, but did not complete primary education; Hh com prim (Sp comp prim): equal to 1 if the (spouse of the) household head completed at least primary schooling; Ch gen m: equal to 1 if the child's gender is missing; Ind m: equal to 1 if the indigenous background of the child is missing; Hh gen m: equal to 1 if the household head's gender is missing; Hh mar m: equal to 1 if the household head's marital status is missing; Sec loc m: equal to 1 if we do not know whether there is a secondary school in the locality where the child resides; Hh ed m (Sp ed m): equal to 1 if the (spouse of the) household head's education is missing.

The second set of variables refers to the possession of assets by the household. Dirtfloor equals 1 if the house has a dirt floor; Poor roof equals 1 if the roof of the house is made of poor quality material; Poor wall equals 1 if the wall of the house is made of poor quality material; N rooms is the number of rooms in the house; Water land equals 1 if there is running water on the property; Water house equals 1 if there is running water in the house; Toilet equals 1 if the house has a toilet; Electricity equals 1 if there is electricity in the house; Blender equals 1 if the family owns a blender; Fridge equals 1 if the family owns a fridge; Gas stove equals 1 if the family owns a gas stove; Gas heater equals 1 if the family owns a gas heater; Radio equals 1 if the family owns a radio; DVD equals 1 if the family owns a DVD player; TV equals 1 if the family owns a TV; Video equals 1 if the family owns a video player; Wash mach equals 1 if the family owns a washing machine; Fan equals 1 if the family owns a fan; Car equals 1 if the family owns a car; Truck equals 1 if the family owns a truck; Land own equals 1 if the family owns land; Animals own equals 1 if the family owns animals.

The third set of variables refers to socio-demographic characteristics of the household. Age head is the age of the household head; Age spouse is the age of the spouse; Workhead equals 1 if the household head currently works; Workspouse equals 1 if the spouse works; Children 0-5 is the number of children younger than 5 in the household; Children 06-12 is the number of children aged 6-12 in the household; Children 13-15 is the number of children aged 13-15 in the household; Children 16-20 is the number of children aged 16-20 in the household; Women 20-39 is the number of women aged 20-39 in the household; Women 40-59 is the number of women aged 40-59 in the household; Women 60 is the number of women aged above 59 in the household; Men 20-39 is the number of men aged 20-39 in the household; Men 40-59 is the number of men aged 40-59 in the household; Men 60 is the number of men aged above 59 in the household.

The fourth set of characteristics refers to missing values for some of the other variables. House char m equals 1 if some information on the house's characteristics is missing; Assets m equals 1 if information on one of the family's assets is missing; Hh agework m equals 1 if the household head's age or work is missing; Sp agework m equals 1 if the spouse's age or work is missing; Demograph m equals 1 if some demographic information is missing.

Table D.1. Composition of treatment and control sample (preprogram characteristics), grades 0–4

	Grade 0			Grade 1			Grade 2			Grade 3			Grade 4		
	T	C	z/t	T	C	z/t	T	C	z/t	T	C	z/t	T	C	z/t
Ch gender	0.47	0.47	0.16	0.48	0.48	0.20	0.48	0.47	0.46	0.49	0.48	0.23	0.47	0.47	0.05
Ind	0.29	0.30	-0.29	0.29	0.29	-0.09	0.28	0.30	-0.29	0.29	0.31	-0.44	0.31	0.33	-0.48
Hh gender	0.86	0.88	-2.13	0.88	0.87	0.38	0.87	0.88	-0.59	0.88	0.89	-0.50	0.86	0.87	-0.36
Hh married	0.91	0.91	0.02	0.92	0.92	0.17	0.92	0.92	-0.08	0.91	0.91	0.32	0.90	0.90	-0.11
Sec loc	0.28	0.28	0.11	0.29	0.29	-0.03	0.28	0.28	-0.07	0.30	0.25	0.85	0.31	0.28	0.40
Hh inc prim	0.41	0.41	0.00	0.44	0.43	0.22	0.46	0.45	0.45	0.46	0.46	0.06	0.45	0.46	-0.08
Hh comp prim	0.19	0.17	0.95	0.19	0.18	0.77	0.18	0.16	0.83	0.19	0.16	1.42	0.17	0.15	1.06
Sp inc prim	0.34	0.33	0.31	0.37	0.34	1.56	0.38	0.33	2.06	0.37	0.38	-0.45	0.39	0.36	1.40
Sp comp prim	0.16	0.15	0.42	0.17	0.17	0.17	0.16	0.15	0.53	0.15	0.13	1.12	0.14	0.12	0.83
Ch gender m	0.07	0.05	-1.89	0.06	0.05	-0.79	0.06	0.06	-0.26	0.05	0.05	0.17	0.07	0.06	-0.62
Ind m	0.23	0.22	0.16	0.16	0.17	-0.37	0.13	0.16	-1.80	0.13	0.16	-2.00	0.13	0.16	-1.47
Hh gen m	0.07	0.05	1.50	0.06	0.05	0.78	0.06	0.06	0.21	0.05	0.05	-0.18	0.07	0.06	0.57
Hh mar m	0.01	0.01	1.98	0.01	0.01	2.01	0.01	0.01	2.46	0.01	0.01	0.10	0.01	0.01	0.99
Hh ed m	0.13	0.13	0.26	0.12	0.12	0.03	0.11	0.13	-1.20	0.10	0.12	-1.92	0.12	0.14	-1.35
Sp ed m	0.20	0.20	-0.16	0.18	0.21	-1.38	0.18	0.22	-2.35	0.19	0.22	-1.75	0.19	0.21	-1.62
Sec loc m	0.01	0.00	1.38	0.01	0.00	1.16	0.01	0.00	1.38	0.01	0.00	1.27	0.01	0.00	1.22
Dirtfloor	0.71	0.70	0.33	0.66	0.68	-0.42	0.68	0.67	0.29	0.67	0.68	-0.23	0.67	0.68	-0.47
Poor roof	0.74	0.71	0.73	0.72	0.72	-0.17	0.73	0.70	0.93	0.73	0.72	0.37	0.72	0.74	-0.50
Poor wall	0.15	0.22	-2.02	0.14	0.21	-2.26	0.14	0.18	-1.31	0.14	0.19	-1.53	0.15	0.20	-1.65
No rooms	1.46	1.45	0.12	1.51	1.48	0.70	1.57	1.52	0.96	1.56	1.54	0.28	1.58	1.56	0.39
Water land	0.30	0.24	1.12	0.31	0.25	1.33	0.33	0.26	1.47	0.33	0.26	1.35	0.32	0.24	1.70
Water house	0.05	0.04	1.01	0.05	0.04	1.23	0.05	0.04	0.85	0.06	0.04	1.51	0.05	0.04	1.33
Toilet	0.47	0.46	0.19	0.50	0.48	0.53	0.51	0.51	0.04	0.51	0.50	0.27	0.53	0.54	-0.12
Electricity	0.57	0.60	-0.62	0.60	0.61	-0.44	0.61	0.63	-0.57	0.62	0.64	-0.39	0.61	0.63	-0.46
Blender	0.14	0.19	-2.03	0.18	0.21	-1.15	0.18	0.22	-1.36	0.18	0.22	-1.55	0.18	0.22	-1.99
Fridge	0.04	0.03	0.33	0.05	0.05	0.61	0.05	0.05	0.11	0.05	0.07	-1.08	0.05	0.04	0.77
Gas stove	0.15	0.15	-0.18	0.16	0.19	-0.81	0.17	0.18	-0.39	0.17	0.18	-0.30	0.17	0.18	-0.43
Gas heater	0.02	0.01	0.48	0.02	0.02	0.09	0.02	0.01	1.77	0.02	0.02	-0.01	0.02	0.02	0.53
Radio	0.48	0.51	-1.22	0.53	0.53	0.21	0.54	0.54	-0.06	0.52	0.55	-1.08	0.55	0.54	0.35
DVD	0.03	0.03	-0.38	0.04	0.03	0.83	0.03	0.03	1.11	0.04	0.03	0.86	0.04	0.03	0.59
TV	0.28	0.31	-0.96	0.33	0.36	-0.95	0.32	0.38	-2.11	0.33	0.39	-1.64	0.34	0.37	-1.07
Video	0.01	0.01	0.42	0.02	0.01	0.48	0.01	0.01	-0.36	0.02	0.01	1.31	0.02	0.02	-0.36
Wash mach	0.01	0.01	0.74	0.02	0.01	1.03	0.02	0.02	0.59	0.02	0.02	0.17	0.02	0.02	-0.49
Fan	0.02	0.04	-1.77	0.03	0.04	-0.88	0.03	0.04	-1.06	0.02	0.04	-1.34	0.02	0.04	-1.74
Car	0.00	0.00	0.79	0.01	0.00	1.89	0.01	0.00	0.93	0.01	0.00	1.28	0.01	0.00	0.96
Truck	0.01	0.01	-0.01	0.02	0.02	-0.26	0.02	0.02	-0.70	0.02	0.03	-0.84	0.03	0.03	-0.49
Land own	0.58	0.55	0.77	0.59	0.58	0.11	0.61	0.59	0.67	0.63	0.59	1.10	0.62	0.60	0.33
Animals own	0.35	0.32	0.95	0.38	0.33	1.63	0.38	0.35	0.84	0.40	0.37	1.19	0.37	0.37	0.06
Hh age	34.67	35.91	-1.51	35.29	36.12	-1.14	36.63	36.66	-0.05	37.71	37.39	0.41	36.90	37.16	-0.30
Sp age	27.63	28.44	-1.29	28.89	28.40	0.71	29.77	28.38	2.05	30.10	29.29	1.17	30.11	30.17	-0.08
Work head	0.75	0.79	-1.45	0.77	0.77	0.19	0.79	0.79	0.01	0.80	0.77	1.23	0.77	0.77	-0.04
Work spouse	0.07	0.06	0.32	0.06	0.06	0.17	0.07	0.05	1.05	0.07	0.05	0.89	0.07	0.06	0.80
Ch 0-5	1.57	1.53	0.92	1.28	1.20	1.61	1.30	1.22	1.61	1.18	1.13	1.09	1.10	1.07	0.80

Table D.1 continued on next page

Table D.1 (*continued*)

	Grade 0			Grade 1			Grade 2			Grade 3			Grade 4		
	T	C	z/t	T	C	z/t	T	C	z/t	T	C	z/t	T	C	z/t
Ch 6-12	1.70	1.72	-0.22	2.11	2.08	0.48	2.16	2.15	0.12	2.23	2.13	1.89	2.18	2.10	1.79
Ch 13-15	0.54	0.54	-0.08	0.53	0.51	0.62	0.59	0.60	-0.36	0.68	0.67	0.46	0.69	0.76	-2.09
Ch 16-20	0.41	0.43	-0.71	0.39	0.41	-0.78	0.46	0.43	1.05	0.54	0.49	1.80	0.51	0.53	-0.56
Women 20-39	0.76	0.75	0.59	0.76	0.74	0.67	0.75	0.73	0.77	0.71	0.73	-0.61	0.69	0.68	0.59
Women 40-59	0.22	0.26	-2.82	0.24	0.25	-0.11	0.26	0.24	1.24	0.29	0.29	0.28	0.31	0.31	-0.51
Women 60	0.08	0.10	-1.09	0.08	0.09	-0.79	0.10	0.09	0.29	0.08	0.10	-1.53	0.08	0.09	-1.23
Men 20-39	0.60	0.62	-0.87	0.61	0.60	0.49	0.59	0.58	0.52	0.57	0.58	-0.79	0.55	0.54	0.07
Men 40-59	0.32	0.33	-0.32	0.33	0.31	0.88	0.36	0.32	1.89	0.40	0.37	1.25	0.40	0.38	1.10
Men 60	0.07	0.09	-1.30	0.08	0.09	-0.89	0.09	0.09	-0.06	0.08	0.09	-0.99	0.07	0.08	-1.25
Hh Char m	0.07	0.08	-1.02	0.07	0.08	-0.67	0.06	0.07	-1.45	0.06	0.08	-1.92	0.06	0.08	-1.69
Assets m	0.71	0.69	0.77	0.73	0.71	0.55	0.73	0.71	0.61	0.76	0.73	0.96	0.74	0.74	0.08
Demog m	0.06	0.07	-0.80	0.06	0.07	-0.80	0.05	0.06	-1.51	0.05	0.07	-2.19	0.05	0.07	-1.47

NOTE—The table reports the average value of the variable in the first column for each grade's treatment (T) and control (C) samples, as well as the value of the z-test for whether these values are statistically and significantly different from each other.

As children's circumstances are used to estimate the probability of school re-enrollment, we only need to consider the test for the remaining preprogram characteristics, that is, from "Dirtfloor" onward; there are 39 of these, such that, in case the groups are similar in composition, using a level of significance of 5%, we expect the treatment and control samples to differ in terms of composition for about two characteristics per grade. In grade 0, we find three significant differences in composition; in grades 2 and 4, we find two; and in grades 1 and 3, we find only one. Hence, we conclude that the samples are comparable in terms of the preprogram characteristics considered.

Table D.2. Composition of treatment and control sample (preprogram characteristics), grades 5–9

	Grade 5			Grade 6			Grade 7			Grade 8			Grade 9		
	T	C	z/t	T	C	z/t	T	C	z/t	T	C	z/t	T	C	z/t
Ch gender	0.50	0.45	2.30	0.45	0.42	1.20	0.52	0.54	-0.78	0.54	0.44	2.92	0.51	0.48	0.69
Ind	0.29	0.30	-0.24	0.27	0.28	-0.39	0.29	0.30	-0.23	0.29	0.31	-0.47	0.31	0.32	-0.12
Hh gender	0.88	0.87	0.90	0.86	0.85	0.66	0.86	0.87	-0.43	0.85	0.87	-0.58	0.86	0.89	-1.36
Hh married	0.90	0.90	0.08	0.90	0.89	1.23	0.90	0.91	-0.54	0.90	0.92	-0.76	0.89	0.91	-0.76
Sec loc	0.30	0.30	-0.06	0.25	0.24	0.13	0.38	0.38	-0.13	0.38	0.37	0.10	0.39	0.44	-0.70
Hh inc prim	0.49	0.48	0.48	0.50	0.50	-0.14	0.46	0.48	-0.53	0.49	0.48	0.37	0.53	0.52	0.29
Hh comp prim	0.17	0.15	0.80	0.12	0.12	0.43	0.17	0.18	-0.22	0.18	0.19	-0.18	0.18	0.22	-1.20
Sp inc prim	0.40	0.38	0.76	0.42	0.40	0.54	0.42	0.38	0.97	0.46	0.38	2.14	0.43	0.48	-1.10
Sp comp prim	0.14	0.12	0.68	0.10	0.10	0.20	0.15	0.15	-0.12	0.13	0.16	-0.91	0.14	0.17	-0.93
Ch gender m	0.05	0.06	0.64	0.06	0.06	0.57	0.05	0.05	-0.30	0.07	0.06	-0.80	0.06	0.04	-1.40
Ind m	0.12	0.12	0.08	0.13	0.15	-1.42	0.15	0.14	0.50	0.11	0.15	-1.63	0.14	0.12	0.67
Hh gen m	0.05	0.06	-0.60	0.06	0.06	-0.49	0.05	0.05	0.31	0.07	0.06	0.84	0.07	0.04	1.47
Hh mar m	0.01	0.01	0.28	0.01	0.01	1.27	0.01	0.00	0.50	0.01	0.01	0.70	0.02	0.01	0.44
Hh ed m	0.10	0.11	-0.20	0.11	0.12	-1.17	0.12	0.11	0.67	0.11	0.11	0.06	0.13	0.09	1.69
Sp ed m	0.19	0.19	-0.33	0.20	0.23	-1.43	0.21	0.22	-0.25	0.18	0.19	-0.52	0.20	0.17	1.03
Sec loc m	0.00	0.00	1.28	0.01	0.00	1.40	0.01	0.00	1.00	0.01	0.00	0.99	0.00	0.00	0.99
Dirtfloor	0.67	0.68	-0.45	0.66	0.66	-0.06	0.64	0.65	-0.28	0.59	0.66	-1.50	0.59	0.63	-0.67
Poor roof	0.74	0.74	-0.02	0.71	0.73	-0.55	0.70	0.72	-0.32	0.73	0.73	0.06	0.74	0.69	0.95
Poor wall	0.15	0.22	-1.80	0.13	0.21	-2.21	0.18	0.26	-1.72	0.21	0.26	-1.15	0.16	0.27	-1.92
No rooms	1.63	1.65	-0.31	1.68	1.67	0.14	1.69	1.68	0.19	1.78	1.74	0.66	1.85	1.86	-0.06
Water land	0.33	0.27	1.12	0.33	0.26	1.73	0.37	0.28	1.71	0.40	0.25	2.54	0.41	0.35	0.85
Water house	0.05	0.05	0.53	0.05	0.04	0.62	0.06	0.04	1.44	0.07	0.05	1.21	0.09	0.06	1.31
Toilet	0.56	0.57	-0.13	0.54	0.56	-0.68	0.60	0.64	-1.01	0.65	0.64	0.30	0.63	0.70	-1.33
Electricity	0.63	0.68	-1.16	0.62	0.64	-0.56	0.69	0.70	-0.35	0.69	0.73	-0.69	0.68	0.78	-1.98
Blender	0.18	0.25	-2.56	0.19	0.25	-2.07	0.23	0.30	-2.13	0.25	0.31	-1.86	0.25	0.31	-1.58
Fridge	0.06	0.05	0.10	0.05	0.05	-0.34	0.06	0.06	0.05	0.07	0.06	0.58	0.08	0.05	1.48
Gas stove	0.16	0.20	-1.02	0.18	0.20	-0.88	0.20	0.19	0.23	0.23	0.23	-0.17	0.20	0.21	-0.43
Gas heater	0.02	0.02	0.34	0.02	0.02	0.62	0.02	0.01	1.36	0.02	0.01	0.59	0.02	0.02	-0.29
Radio	0.57	0.57	-0.18	0.58	0.58	-0.12	0.61	0.63	-0.53	0.60	0.67	-1.76	0.59	0.68	-1.99
DVD	0.04	0.03	0.39	0.03	0.03	-0.08	0.04	0.02	2.57	0.05	0.02	1.94	0.04	0.04	-0.40
TV	0.35	0.40	-1.52	0.34	0.39	-1.46	0.39	0.47	-2.01	0.43	0.46	-0.62	0.41	0.49	-1.50
Video	0.02	0.01	1.41	0.02	0.01	0.28	0.02	0.01	1.90	0.02	0.01	1.07	0.03	0.02	1.22
Wash mach	0.02	0.02	0.71	0.01	0.02	-0.69	0.03	0.02	1.08	0.03	0.04	-0.47	0.04	0.01	2.04
Fan	0.03	0.05	-1.10	0.03	0.04	-1.53	0.04	0.06	-0.89	0.05	0.05	-0.21	0.07	0.06	0.34
Car	0.01	0.00	1.58	0.01	0.00	1.03	0.01	0.00	2.30	0.01	0.00	1.66	0.01	0.01	-0.18
Truck	0.02	0.03	-0.78	0.02	0.03	-0.79	0.02	0.03	-0.15	0.03	0.02	0.43	0.04	0.04	-0.20
Land own	0.64	0.63	0.42	0.66	0.62	1.30	0.62	0.60	0.44	0.67	0.60	1.59	0.66	0.65	0.25
Animals own	0.40	0.40	-0.28	0.43	0.42	0.16	0.39	0.32	1.87	0.40	0.38	0.68	0.39	0.38	0.28
Hh age	38.99	39.83	-0.94	39.98	39.49	0.64	39.65	39.25	0.35	39.84	40.04	-0.15	40.05	41.45	-1.17
Sp age	30.96	31.70	-0.93	31.62	30.68	1.19	31.19	30.31	0.67	32.49	32.06	0.37	32.66	33.00	-0.30
Work head	0.81	0.78	0.93	0.78	0.76	1.13	0.77	0.78	-0.37	0.77	0.78	-0.31	0.75	0.81	-1.57
Work spouse	0.07	0.05	1.28	0.05	0.06	-0.98	0.05	0.04	1.12	0.06	0.04	1.57	0.03	0.04	-0.28
Ch 0-5	1.07	1.10	-0.54	0.97	0.97	0.06	0.92	0.88	0.60	0.84	0.87	-0.53	0.83	0.84	-0.18

Table D.2 continued on next page

Table D.2 (*continued*)

	Grade 5			Grade 6			Grade 7			Grade 8			Grade 9		
	T	C	z/t	T	C	z/t	T	C	z/t	T	C	z/t	T	C	z/t
Ch 6-12	2.20	2.18	0.37	2.02	1.96	1.15	1.92	1.93	-0.08	1.81	1.77	0.49	1.70	1.75	-0.57
Ch 13-15	0.77	0.84	-1.74	0.98	1.00	-0.78	1.01	0.94	1.35	1.21	1.17	0.83	1.20	1.21	-0.17
Ch 16-20	0.57	0.56	0.18	0.71	0.66	1.57	0.66	0.61	1.08	0.74	0.61	2.36	0.76	0.69	1.05
Women 20-39	0.69	0.72	-1.04	0.65	0.63	0.67	0.62	0.66	-0.90	0.68	0.66	0.41	0.65	0.71	-0.96
Women 40-59	0.31	0.35	-1.60	0.38	0.38	-0.10	0.36	0.35	0.19	0.38	0.34	1.34	0.39	0.38	0.26
Women 60	0.08	0.09	-0.89	0.08	0.10	-1.36	0.09	0.11	-0.66	0.10	0.12	-0.68	0.10	0.10	0.08
Men 20-39	0.52	0.54	-0.70	0.47	0.51	-1.55	0.43	0.49	-1.68	0.46	0.48	-0.53	0.40	0.43	-0.81
Men 40-59	0.44	0.42	0.87	0.48	0.45	1.45	0.49	0.49	0.00	0.52	0.48	1.18	0.54	0.57	-0.73
Men 60	0.10	0.10	-0.30	0.10	0.09	0.95	0.11	0.07	2.00	0.09	0.09	-0.10	0.10	0.07	1.29
Hh Char m	0.06	0.05	0.71	0.06	0.07	-1.44	0.08	0.06	1.19	0.05	0.06	-0.05	0.07	0.05	1.37
Assets m	0.77	0.75	0.69	0.79	0.76	1.26	0.76	0.72	0.94	0.80	0.73	1.80	0.79	0.75	0.90
Demog m	0.05	0.04	0.44	0.04	0.06	-1.01	0.06	0.06	0.37	0.04	0.05	-0.38	0.05	0.04	0.39

NOTE—The table reports the average value of the variable in the first column for each grade’s treatment (T) and control (C) samples, as well as the value of the z-test for whether these values are statistically and significantly different from each other.

As children’s circumstances are used to estimate the probability of school re-enrollment, we only need to consider the test for the remaining preprogram characteristics, that is, from “Dirtfloor” onward; there 39 of these, such that, in case the groups are similar in composition, using a level of significance of 5%, we expect the treatment and control samples to differ in terms of composition for about two characteristics per grade. In grade 7, we find five significant differences in composition; in grade 9, we find three, in grades 6 and 8, we find two; and in grade 5, we find only one. Hence, we conclude that the samples, with grade 7 as a possible exception, are comparable in terms of the preprogram characteristics considered.

Table D.3. Logistic regression estimates for being in the treatment sample

	Grade achieved before beginning school year 1998									
	0	1	2	3	4	5	6	7	8	9
Ch gender	0.014 (0.079)	0.028 (0.079)	0.029 (0.081)	0.027 (0.077)	0.007 (0.075)	0.229** (0.092)	0.062 (0.090)	-0.126 (0.129)	0.313** (0.143)	0.203 (0.167)
Ind	-0.034 (0.223)	0.046 (0.223)	-0.092 (0.249)	-0.201 (0.228)	-0.140 (0.236)	-0.022 (0.259)	-0.062 (0.240)	0.163 (0.264)	-0.026 (0.316)	0.127 (0.367)
Hh gender	-0.497** (0.209)	-0.039 (0.269)	-0.758*** (0.278)	-0.581** (0.259)	-0.283 (0.295)	-0.140 (0.282)	-0.470* (0.248)	-0.401 (0.424)	0.075 (0.431)	0.058 (0.484)
Hh married	0.464** (0.195)	-0.050 (0.232)	0.262 (0.202)	0.066 (0.225)	0.044 (0.252)	-0.132 (0.252)	0.233 (0.198)	-0.210 (0.389)	-0.457 (0.410)	-0.128 (0.445)
Sec loc	0.062 (0.335)	-0.016 (0.296)	0.003 (0.300)	0.255 (0.295)	0.149 (0.297)	-0.009 (0.297)	0.100 (0.265)	-0.008 (0.296)	-0.044 (0.306)	-0.306 (0.308)
Hh inc prim	0.148 (0.121)	0.060 (0.123)	0.067 (0.121)	0.032 (0.117)	-0.047 (0.121)	0.094 (0.158)	-0.086 (0.115)	0.040 (0.175)	-0.007 (0.283)	0.272 (0.337)
Hh com prim	0.348** (0.177)	0.191 (0.174)	0.210 (0.164)	0.287 (0.187)	0.140 (0.196)	0.227 (0.211)	0.070 (0.180)	0.196 (0.285)	0.124 (0.328)	0.037 (0.419)
Sp inc prim	0.046 (0.139)	0.186 (0.136)	0.237* (0.124)	-0.089 (0.155)	0.167 (0.134)	0.150 (0.158)	0.098 (0.130)	0.296 (0.208)	0.366 (0.238)	-0.254 (0.293)
Sp com prim	0.029 (0.178)	0.096 (0.202)	0.171 (0.185)	0.057 (0.191)	0.111 (0.188)	0.153 (0.198)	0.140 (0.173)	0.202 (0.274)	0.119 (0.314)	-0.226 (0.395)
Ch gender m	-0.213 (0.165)	-0.073 (0.264)	0.176 (0.316)	0.550* (0.309)	-0.068 (0.411)	0.287 (0.497)	-0.604* (0.360)	-0.699 (0.611)	1.281 (0.808)	
Ind m	0.201 (0.149)	0.129 (0.245)	-0.344 (0.334)	-0.560* (0.310)	-0.077 (0.407)	-0.237 (0.470)	0.399 (0.369)	0.739 (0.591)	-1.378* (0.787)	0.445 (0.363)
Hh gen m	-0.884 (0.646)	-0.843 (0.649)	-1.010* (0.594)	-1.369** (0.640)	0.728 (0.789)	-0.448 (0.540)	-0.350 (0.543)	-1.117 (1.184)	1.152 (1.194)	-2.276 (1.432)
Hh mar m	1.067** (0.450)	0.787* (0.466)	1.309*** (0.451)	0.133 (0.518)	0.242 (0.573)	0.204 (0.478)	0.767 (0.478)	0.198 (0.986)	0.536 (0.851)	-0.114 (0.784)
Hh ed m	0.107 (0.621)	0.519 (0.477)	-0.164 (0.488)	0.383 (0.615)	-1.229* (0.718)	0.124 (0.554)	-0.372 (0.450)	1.007 (0.928)	-1.069 (1.054)	1.349 (1.157)
Sp ed m	-0.001 (0.275)	0.171 (0.335)	0.182 (0.275)	-0.204 (0.310)	-0.341 (0.320)	-0.392 (0.345)	-0.055 (0.325)	0.010 (0.617)	-0.180 (0.689)	0.511 (0.901)
Dirtfloor	0.095 (0.150)	-0.040 (0.164)	0.026 (0.146)	-0.129 (0.172)	-0.116 (0.166)	-0.032 (0.148)	0.041 (0.152)	0.036 (0.213)	-0.359 (0.226)	-0.079 (0.237)
Poor roof	0.135 (0.207)	-0.046 (0.213)	0.126 (0.177)	-0.037 (0.201)	-0.192 (0.175)	0.048 (0.217)	-0.162 (0.185)	0.032 (0.228)	0.031 (0.255)	0.494* (0.271)
Poor wall	-0.442** (0.204)	-0.494** (0.195)	-0.328 (0.213)	-0.312 (0.226)	-0.297 (0.207)	-0.481** (0.235)	-0.524** (0.214)	-0.478* (0.282)	-0.171 (0.294)	-0.732** (0.336)
No rooms	0.039 (0.053)	0.026 (0.058)	0.069 (0.066)	-0.011 (0.061)	0.019 (0.055)	0.038 (0.065)	0.010 (0.057)	0.004 (0.089)	-0.016 (0.085)	0.100 (0.106)
Water land	0.294 (0.256)	0.312 (0.272)	0.406 (0.260)	0.203 (0.250)	0.380 (0.253)	0.341 (0.254)	0.423* (0.250)	0.398 (0.277)	0.746** (0.310)	0.331 (0.369)
Water house	0.047 (0.296)	0.099 (0.264)	-0.088 (0.259)	0.264 (0.279)	0.149 (0.315)	-0.039 (0.262)	-0.146 (0.287)	0.174 (0.334)	-0.020 (0.381)	0.122 (0.490)
Toilet	0.086	0.098	-0.039	0.047	-0.026	0.012	-0.100	-0.131	0.030	-0.290

Table D.3 continued on next page

Table D.3 (*continued*)

Grade achieved before beginning school year 1998										
	0	1	2	3	4	5	6	7	8	9
Electricity	(0.152)	(0.145)	(0.154)	(0.155)	(0.141)	(0.161)	(0.146)	(0.200)	(0.238)	(0.258)
	-0.100	-0.112	-0.092	-0.096	-0.102	-0.152	-0.054	0.095	-0.265	-0.471
	(0.205)	(0.209)	(0.207)	(0.213)	(0.208)	(0.195)	(0.205)	(0.284)	(0.273)	(0.356)
Blender	-0.336***	-0.214	-0.190	-0.240*	-0.417***	-0.481***	-0.399***	-0.517***	-0.475**	-0.258
	(0.129)	(0.147)	(0.122)	(0.127)	(0.140)	(0.149)	(0.126)	(0.172)	(0.205)	(0.222)
Fridge	0.304	0.255	0.213	-0.304	0.475	0.221	0.179	-0.001	0.427	0.982**
	(0.325)	(0.300)	(0.279)	(0.294)	(0.302)	(0.258)	(0.247)	(0.450)	(0.327)	(0.432)
Gas stove	0.008	-0.280	-0.100	-0.011	-0.168	-0.242	-0.164	0.038	-0.204	-0.195
	(0.222)	(0.184)	(0.210)	(0.204)	(0.204)	(0.233)	(0.172)	(0.241)	(0.283)	(0.284)
Gas heater	0.088	-0.211	0.533	-0.263	0.095	-0.046	0.161	0.609	-0.236	-0.295
	(0.344)	(0.369)	(0.378)	(0.307)	(0.379)	(0.384)	(0.320)	(0.508)	(0.478)	(0.726)
Radio	-0.143	0.070	0.044	-0.096	0.122	0.160	0.065	0.158	-0.165	-0.329
	(0.122)	(0.105)	(0.098)	(0.106)	(0.105)	(0.112)	(0.117)	(0.162)	(0.165)	(0.224)
DVD	-0.121	0.143	0.162	0.178	0.185	0.224	0.012	1.011**	0.718*	-0.415
	(0.255)	(0.229)	(0.264)	(0.242)	(0.233)	(0.266)	(0.254)	(0.406)	(0.437)	(0.572)
TV	-0.097	-0.172	-0.372***	-0.277**	-0.176	-0.124	-0.171	-0.366**	0.103	-0.026
	(0.109)	(0.120)	(0.105)	(0.129)	(0.125)	(0.150)	(0.143)	(0.171)	(0.206)	(0.227)
Video	0.200	0.120	-0.157	0.699*	-0.124	0.576	0.351	0.769	0.628	0.540
	(0.358)	(0.342)	(0.446)	(0.411)	(0.302)	(0.354)	(0.453)	(0.496)	(0.697)	(0.490)
Wash mach	0.514	0.392	0.364	0.176	-0.067	0.379	-0.080	0.373	-0.703	1.295
	(0.408)	(0.455)	(0.400)	(0.370)	(0.408)	(0.459)	(0.369)	(0.462)	(0.551)	(0.912)
Fan	-0.556	-0.165	-0.202	-0.301	-0.554	-0.099	-0.432	0.026	0.011	0.605
	(0.346)	(0.339)	(0.318)	(0.328)	(0.368)	(0.346)	(0.378)	(0.386)	(0.456)	(0.574)
Car	0.507	1.211*	0.657	0.979	0.543	0.974	0.873	1.645*		-0.777
	(0.680)	(0.652)	(0.552)	(0.709)	(0.567)	(0.727)	(0.801)	(0.992)		(0.794)
Truck	-0.112	-0.245	-0.437	-0.323	-0.271	-0.425	-0.279	-0.397	0.164	-0.798
	(0.561)	(0.418)	(0.385)	(0.389)	(0.389)	(0.400)	(0.404)	(0.395)	(0.451)	(0.628)
Land own	-0.017	-0.264	-0.147	-0.109	-0.126	-0.007	0.072	0.172	-0.007	-0.420
	(0.257)	(0.238)	(0.245)	(0.223)	(0.250)	(0.220)	(0.207)	(0.309)	(0.290)	(0.410)
Animals own	0.127	0.208	0.068	0.041	-0.083	-0.055	-0.067	0.431**	0.036	-0.017
	(0.160)	(0.147)	(0.175)	(0.135)	(0.168)	(0.147)	(0.150)	(0.205)	(0.223)	(0.236)
Hh age	-0.009*	-0.015**	-0.016***	-0.013**	-0.008	-0.009	-0.007	0.002	-0.008	-0.031*
	(0.005)	(0.006)	(0.006)	(0.006)	(0.008)	(0.008)	(0.007)	(0.011)	(0.013)	(0.017)
Sp age	-0.003	0.003	0.008	0.002	-0.008	-0.009	0.003	0.006	-0.011	0.020
	(0.006)	(0.008)	(0.007)	(0.007)	(0.009)	(0.007)	(0.007)	(0.013)	(0.013)	(0.017)
Work head	-0.423*	-0.111	-0.155	-0.020	-0.218	0.096	0.113	0.007	0.147	-0.475
	(0.232)	(0.225)	(0.223)	(0.207)	(0.209)	(0.206)	(0.165)	(0.255)	(0.272)	(0.338)
Work spouse	0.092	-0.018	0.285	0.149	0.185	0.359	-0.288	0.279	0.619	-0.316
	(0.259)	(0.268)	(0.328)	(0.341)	(0.240)	(0.274)	(0.229)	(0.315)	(0.390)	(0.445)
Ch 0-5	-0.003	0.049	0.059	0.033	0.007	-0.025	-0.023	0.099	-0.084	0.064
	(0.042)	(0.056)	(0.045)	(0.049)	(0.053)	(0.050)	(0.046)	(0.075)	(0.087)	(0.106)
Ch 06-12	-0.028	-0.014	-0.047	0.044	0.031	0.003	0.015	0.014	0.010	-0.061
	(0.043)	(0.051)	(0.050)	(0.042)	(0.043)	(0.062)	(0.041)	(0.075)	(0.072)	(0.086)
Ch 13-15	0.020	0.045	-0.063	-0.015	-0.157**	-0.122	-0.089	0.180	-0.022	0.041
	(0.062)	(0.064)	(0.058)	(0.066)	(0.066)	(0.077)	(0.056)	(0.118)	(0.121)	(0.155)
Ch 16-20	-0.032	-0.079	0.017	0.109*	-0.023	0.075	0.065	0.067	0.141	0.058

Table D.3 continued on next page

Table D.3 (continued)

Grade achieved before beginning school year 1998										
	0	1	2	3	4	5	6	7	8	9
Women 20-39	(0.061) -0.057 (0.113)	(0.061) -0.013 (0.101)	(0.060) -0.026 (0.107)	(0.061) -0.297*** (0.112)	(0.064) -0.129 (0.112)	(0.061) -0.252** (0.103)	(0.055) -0.009 (0.089)	(0.067) -0.267* (0.154)	(0.120) 0.161 (0.165)	(0.119) -0.342 (0.225)
Women 40-59	-0.208* (0.115)	0.029 (0.137)	0.038 (0.142)	-0.182 (0.122)	-0.098 (0.181)	-0.233 (0.142)	-0.147 (0.130)	-0.328* (0.192)	0.337 (0.229)	-0.321 (0.266)
Women 60	-0.125 (0.139)	-0.056 (0.166)	0.059 (0.148)	-0.206 (0.143)	-0.177 (0.168)	-0.087 (0.171)	-0.310* (0.170)	-0.295 (0.253)	-0.166 (0.242)	0.091 (0.316)
Men 20-39	-0.116 (0.106)	0.075 (0.109)	0.118 (0.112)	-0.091 (0.111)	0.018 (0.114)	-0.043 (0.106)	-0.164 (0.103)	-0.129 (0.143)	0.020 (0.198)	-0.029 (0.187)
Men 40-59	0.123 (0.115)	0.312** (0.158)	0.401*** (0.133)	0.143 (0.146)	0.341* (0.177)	0.250 (0.164)	0.153 (0.143)	0.017 (0.235)	0.242 (0.257)	0.120 (0.280)
Men 60	0.187 (0.171)	0.216 (0.170)	0.388* (0.202)	0.196 (0.182)	0.214 (0.214)	0.378* (0.200)	0.436** (0.170)	0.533* (0.320)	0.414 (0.360)	0.578 (0.447)
House Char m	-0.403 (0.450)	-0.071 (0.401)	-0.041 (0.444)	0.176 (0.522)	-0.385 (0.440)	0.348 (0.567)	-0.689* (0.378)	2.163** (1.070)	0.641 (0.556)	1.844** (0.825)
Assets m	0.152 (0.239)	0.180 (0.228)	0.194 (0.262)	0.281 (0.215)	0.188 (0.252)	0.132 (0.218)	0.151 (0.213)	-0.180 (0.372)	0.331 (0.349)	0.514 (0.487)
Demog m	-0.937 (0.761)	-1.378 (0.857)	-0.364 (1.023)	-1.486* (0.857)	0.351 (1.004)	-0.607 (0.958)	-0.175 (0.852)	-3.009* (1.789)	1.301 (1.339)	-5.886*** (1.650)
Constant	1.093* (0.594)	0.840 (0.599)	0.980 (0.599)	1.785*** (0.578)	1.815*** (0.622)	1.338** (0.669)	1.140** (0.577)	0.409 (0.874)	0.971 (1.013)	2.037* (1.089)
# obs	4569	3461	3422	3206	2846	2614	4065	1275	985	703
# clusters	150	148	148	148	148	147	150	134	127	119

NOTE—For the definition of the variables, see the discussion at the beginning of Appendix D. Standard errors in parenthesis are adjusted for clustering at the locality level. *** denotes significance at 1%; **, at 5%; and *, at 10%.

To test whether the composition of the treatment and control group is similar, we only need to consider those variables that are not present in the estimation of the probability of school re-enrollment; we only consider the second, third, and fourth set of variables. We have 39 of these, such that, in case the groups are similar in composition, using a level of significance of 5%, we would expect about two coefficients to be statistically significant at 5% for each grade. We find this number to be exactly equal to two for grades 0, 4, and 8; three for grades 1, 2, 3, 5, and 6; four for grade 9; and five for grade 7. Hence, one could question the similarity in terms of composition for grades 7 and 9. We could not find statistically significant differences for these grades between the statistics for with and without treatment. For grades with statistically established results that have significance (grades 1 to 6), we can maintain the hypothesis that the treatment and control samples are similar in terms of the observables listed in the table.

E. LINEAR PROBABILITY MODEL FOR SCHOOL RE-ENROLLMENT PER GRADE

Table E.1. Estimated coefficients of the linear probability model for school re-enrollment per grade

Grade achieved before beginning school year 1998										
	0	1	2	3	4	5	6	7	8	9
T	0.104*	0.011	-0.011	0.034	0.180***	0.049	0.161***	-0.025	0.023	-0.057
	(0.061)	(0.034)	(0.043)	(0.055)	(0.068)	(0.049)	(0.057)	(0.056)	(0.078)	(0.158)
Ch gender	0.041**	-0.017	0.017	-0.021	0.017	0.008	0.066***	0.028	-0.015	0.132**
	(0.017)	(0.015)	(0.014)	(0.020)	(0.020)	(0.022)	(0.025)	(0.022)	(0.034)	(0.061)
Ind	0.007	0.000	-0.018	0.044*	0.022	0.066**	0.082**	0.031	0.053	0.160*
	(0.033)	(0.016)	(0.022)	(0.025)	(0.023)	(0.026)	(0.037)	(0.026)	(0.040)	(0.083)
Hh gender	-0.018	0.029	-0.062**	-0.041	-0.017	-0.013	0.029	0.044	0.054	0.069
	(0.045)	(0.031)	(0.030)	(0.036)	(0.050)	(0.034)	(0.040)	(0.056)	(0.088)	(0.131)
Hh married	0.085*	0.040*	0.050	0.071	0.083	-0.010	0.004	-0.041	-0.007	-0.090
	(0.052)	(0.032)	(0.043)	(0.045)	(0.053)	(0.044)	(0.045)	(0.054)	(0.112)	(0.137)
Sec loc	0.024	0.007	0.023	-0.001	0.043**	0.039	0.248***	0.037	-0.030	-0.189**
	(0.034)	(0.017)	(0.015)	(0.020)	(0.021)	(0.032)	(0.033)	(0.025)	(0.039)	(0.076)
Hh inc prim	0.131***	0.026	0.052*	0.027	0.002	0.019	0.017	-0.017	-0.089**	-0.083
	(0.029)	(0.027)	(0.027)	(0.024)	(0.031)	(0.027)	(0.030)	(0.026)	(0.037)	(0.087)
Hh com prim	0.128***	0.043*	0.053*	0.066***	0.069**	0.038	0.146***	-0.009	-0.137**	-0.074
	(0.032)	(0.024)	(0.031)	(0.023)	(0.031)	(0.035)	(0.053)	(0.040)	(0.055)	(0.089)
Sp inc prim	0.041	0.028	0.041**	0.021	0.075**	0.004	0.026	-0.001	0.058	0.141*
	(0.037)	(0.021)	(0.018)	(0.024)	(0.031)	(0.025)	(0.036)	(0.029)	(0.044)	(0.084)
Sp com prim	0.150***	0.056***	0.092***	0.082***	0.110***	0.086**	0.119**	0.034	0.138**	0.099
	(0.039)	(0.020)	(0.016)	(0.029)	(0.028)	(0.034)	(0.051)	(0.034)	(0.050)	(0.104)
Ch gen * T	-0.030	0.037***	-0.008	0.047**	-0.024	-0.011	-0.021	-0.012	0.003	-0.079
	(0.023)	(0.015)	(0.017)	(0.021)	(0.024)	(0.029)	(0.034)	(0.028)	(0.042)	(0.075)
Ind * T	-0.067*	0.012	0.012	-0.041	-0.002	-0.020	0.009	-0.046	0.011	-0.017
	(0.038)	(0.018)	(0.028)	(0.028)	(0.030)	(0.032)	(0.051)	(0.031)	(0.044)	(0.110)
Hh gen * T	0.018	0.018	0.029	0.043	0.000	-0.029	-0.108**	-0.046	-0.067	-0.156
	(0.053)	(0.033)	(0.036)	(0.044)	(0.057)	(0.041)	(0.051)	(0.065)	(0.091)	(0.153)
Hh mar * T	-0.094	-0.011	0.037	-0.032	-0.106*	0.013	0.061	0.047	0.036	0.184
	(0.058)	(0.037)	(0.054)	(0.053)	(0.061)	(0.054)	(0.057)	(0.066)	(0.111)	(0.159)
Sec loc * T	0.020	0.008	-0.020	-0.007	-0.021	-0.008	-0.069	-0.029	0.033	0.152
	(0.042)	(0.018)	(0.021)	(0.025)	(0.027)	(0.037)	(0.049)	(0.032)	(0.043)	(0.092)
Hh inc prim * T	-0.034	-0.005	-0.033	0.003	0.014	-0.009	0.027	0.054	0.054	0.046
	(0.040)	(0.028)	(0.032)	(0.026)	(0.034)	(0.032)	(0.038)	(0.035)	(0.043)	(0.106)
Hh com prim * T	0.062	-0.024	-0.008	-0.001	-0.029	-0.001	-0.015	0.055	0.092	0.158
	(0.045)	(0.025)	(0.036)	(0.026)	(0.039)	(0.036)	(0.066)	(0.049)	(0.067)	(0.114)
Sp inc prim * T	0.024	-0.011	-0.000	-0.019	-0.046	0.066**	0.013	0.036	-0.025	-0.041
	(0.038)	(0.021)	(0.024)	(0.025)	(0.036)	(0.032)	(0.042)	(0.037)	(0.053)	(0.099)
Sp com prim * T	-0.046	-0.024	0.038*	-0.051*	-0.073*	-0.003	0.045	0.016	-0.157**	-0.026
	(0.043)	(0.019)	(0.022)	(0.031)	(0.035)	(0.038)	(0.059)	(0.044)	(0.066)	(0.134)
Ch gen m	-0.053*			0.018					0.060	
	(0.028)			(0.021)					(0.059)	
Ind m									-0.042	
									(0.055)	

Table E.1 continued on next page

Table E.1 (*continued*)

	Grade achieved before beginning school year 1998									
	0	1	2	3	4	5	6	7	8	9
Hh gen m			-0.097*** (0.033)						-0.103* (0.054)	
Hh mar m	0.185*** (0.043)				-0.153* (0.089)					
Sec loc m	0.072*** (0.019)									
Hh ed m	0.099*** (0.030)		0.072*** (0.022)	0.052** (0.024)						
Sp ed m		0.037*** (0.012)							0.027 (0.046)	
Constant	0.564*** (0.054)	0.896*** (0.031)	0.858*** (0.043)	0.809*** (0.045)	0.742*** (0.059)	0.828*** (0.035)	0.210*** (0.042)	0.895*** (0.048)	0.877*** (0.088)	0.397*** (0.143)
# obs	4,576	3,472	3,432	3,213	2,851	2,619	4,074	1,277	990	703
R-squared	0.059	0.024	0.035	0.024	0.035	0.032	0.076	0.017	0.027	0.063

NOTE—All variables in the regression are dummy variables: they are either 1 or 0. We indicate when they are equal to 1. Variable T: equal to 1 if the child is treated; Ch gender: equal to 1 if the child is a boy; Ind: equal to 1 for indigenous children; Hh gender: equal to 1 if the household head is male; Hh married: equal to 1 if the household head is married; Sec loc: equal to 1 if there is a secondary school in the locality where the child resides; Hh inc prim (Sp inc prim): equal to 1 if the (spouse of the) household head has some schooling, but did not complete primary education; Hh com prim (Sp comp prim): equal to 1 if the (spouse of the) household head completed at least primary schooling; Ch gen m: equal to 1 if the child's gender is missing; Ind m: equal to 1 if the indigenous background of the child is missing; Hh gen m: equal to 1 if the household head's gender is missing; Hh mar m: equal to 1 if the household head's marital status is missing; Sec loc m: equal to 1 if we do not know whether there is a secondary school in the locality where the child resides; Hh ed m (Sp ed m): equal to 1 if the (spouse of the) household head's education is missing. An empty cell for a missing circumstance dummy indicates that the dummy was perfectly collinear with (a subset of) the other dummies or that its coefficient was not significant in the original specification (one that included a dummy for each missing characteristic). We keep only missing dummies that are significant (in the original specification) to avoid any situations wherein the dependent variables perfectly predict school re-enrollment. This was important for the Bootstrap procedure described in Appendix B. In some cases, variables that were originally significant are no longer significant in the final specification, as is the case for "Ch gen m" in grade 3. Standard errors in parenthesis are adjusted for clustering at the locality level. *** denotes significance at 1%; **, at 5%; and *, at 10%.