Did Progresa Reduce Inequality of Opportunity for school re-enrollment? Preliminary version *

José Luis Figueroa † Dirk Van de gaer ‡
Sherppa Sherppa and CORE

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Abstract

The way school enrollment is distributed over children facing different circumstances (like gender, indigenous origin, parental education or place of residence) should be an important concern in the evaluation of social programs like Progresa. The literature has shown that Progresa increases average school enrollment for children aged 12 and above, especially during the transition from primary to secondary school, and some authors found evidence that the program has larger effects for some groups, for instance for girls than for boys. This paper finds inspiration in the recent literature on the measurement of inequality of opportunity to evaluate the effect of the program, taking its effect on inequality of opportunity in a systematic way into account. Focussing on school re-enrollment opportunities for each grade attained, our findings are that Progresa improved aggregate opportunities, average school re-enrollment and reduced inequality of opportunity for school re-enrollment. Moreover, using the Human Opportunity Index, proposed by de Barros, Vega, and Saavedra (2008) and de Barros, Ferreira, Vega, and Chanduvi (2009), and used extensively in Molinas, de Barros, Saavedra, and Guigale (2012), 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.

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†SHERPPA, Vakgroep Sociale Economie, F.E.B., Ghent University, Sint-Pietersplein 6, B-9000 Gent, Belgium.

‡SHERPPA, Vakgroep Sociale Economie, F.E.B., Ghent University, Sint-Pietersplein 6, B-9000 Gent, Belgium. and CORE, Université Catholique de Louvain, Voie du Roman Pays 34, B-1348 Louvain-la-Neuve, Belgium

1 Introduction

A key concern in developing countries is the provision of access to basic opportunities for all (see, e.g., Molinas, de Barros, Saavedra, and Guigale (2012)). One such opportunity is access to education. When universal access is not achieved, the next best thing is that all children have equal access at a highest possible level. As powerfully advocated by both economists and political philosophers (see, e.g. Roemer (1993), Roemer (1998), Fleurbaey (2008) or Cohen (1989)), a child's access to education should not be influenced by his circumstances, defined as characteristics over which he has no control, which include his family background, race, gender and place of residence. Hence not only the level of children's opportunities, but also the way these opportunities are distributed between children with different circumstances is highly relevant.

Many social interventions, including Progresa, are motivated by the concern to provide ample opportunities for all. The question whether such social interventions influence both the level of children's opportunities and their distribution is key. Mexico's Progresa, in 2002 extended in reach and renamed Oportunidades, and in 2014 changed into Prospera, aims at expanding children's educational opportunities. In fact, the program is explicitly designed to give parents incentives to keep their children in school by increasing school subsidies when the children are in a higher grade (to make schooling more attractive compared to child labor) and by providing larger grants for girls than for boys at ages when otherwise girls are more likely to drop out.

Most evaluation studies of social programs identify average treatment effects: they identify, for instance, the effect of the program on average school enrollment of children of a particular age. There is widespread agreement in the evaluation literature that Progresa increased average school enrollment, with large effects for children aged 12 and above (see, e.g., Schultz (2004), Behrman, Sengupta, and Todd (2005), Todd and Wolpin (2006), Attanasio, Meghir, and Santiago (2012) and Dubois, Janvry, and Sadoulet (2012)). Important conclusions follow from these studies. Schultz (2004), on page 22, for instance, reports that: "the impact ... is to increase the educational attainment of a cohort of poor youth by 0.66 years of schooling ..., for which the youth earn a 12% higher wage per year of schooling over their adult working lifetimes (age 18-65) based on the 1996 urban wage structure."

Some studies go further and identify average effects for children having 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) goes beyond average treatment effects for different groups and identifies the effect on the entire distribution of child cognitive and non-cognitive skills for girls, for boys, for children from indigenous and non-indigenous origin. Van de gaer, Vandenbossche, and Figueroa (2014) explicitly focus on children's circumstances, measured by the four possible combinations based on whether they had at least one indigenous parent or not and whether they had at least one parent that completed primary education or not. They establish whether children's health improves or not, as a function of their circumstances and their place in the distribution of the health outcome. While such studies identify which types of children gain from the program and which ones don't, their non-parametric methodology limits the number of different circumstances they can consider and they do not provide a systematic assessment of the consequences of the program on inequality of opportunity.

This paper provides such an assessment. We draw on recent contributions in the literature on inequality of opportunity that propose to measure the inequality in a conterfactual distributions that only reflect differences in circumstances (see for example, Pistolesi (2009) and Ferreira and Gignoux (2011)) [1]. In our case, we construct two conterfactuals: one in case children are treated and one in case 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 for each child, given his circumstances, the probability of being re-enrolled in school if living in a household that participates in the program, and the probability in case of living in a non-participant household. This gives us two vectors of predicted re-enrollment probabilities, one with and one without the program, and we compare the inequality contained in both vectors.

To determine the effect of the program on inequality of opportunity, we provide, in first place, results for Generalized Lorenz and Lorenz dominance. One can interpret Generalized Lorenz dominance as an improvement for a large class of opportunity aggregators. Similarly, one can interpret Lorenz dominance as a decrease for a large class of inequality of opportunity measures. We also want to see how important the effect on inequality of opportunity is for the total effect on the aggregator. To this end, we use the Human Opportunity Index (de Barros, Vega, and Saavedra (2008) and de Barros, Ferreira, Vega, and Chanduvi (2009)) and, as an alternative, the Gini Opportunity Aggregator. Both can be decomposed into changes that are due to changes in average re-enrollment and changes in an index that measures the inequality of the distribution of re-enrollment between children with different circumstances, which therefore measures changes in inequality of opportunity.

We find that, generally, Progresa leads to a distribution of school re-enrollment probabilities (conditional on circumstances) that both Generalized and Lorenz dominates the distribution of school re-enrollment 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 responsible for about 15 to 40 % of the increase in aggregated opportunities.

2 Description of PROGRESA and sample selection

Progresa (Programa de Educación, Salud y Alimentación) was implemented by the Mexican government in 1997 for 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. First, households receive cash transfers. Educational grants are conditional on children's school enrollment and on regular school attendance (an attendance rate of 85 per cent is required) and grants for consumption of food are conditional on regular medical check-ups in health clinics and

[1]For an overview of this rapidly expanding literature, see Ferreira and Peragine (2015), Ramos and Van de gaer (2015) or Roemer and Trannoy (2015)

attendance of health and nutrition talks. Second, in-kind health benefits and nutritional supplements are provided for children up to age five, and for pregnant and lactating women. The analysis presented here refers to the first year after the program began. The reason, as explained in more detail below, is that during the first two years of the program, experimental data was available to evaluate the effects of participation. After 2 years, the group that was randomly selected as a control was incorporated in the program, and the experimental design of the program was lost.

Primary education in Mexico consists out of 6 grades, followed by 3 years of secondary education and 3 years of high school. The educational grants started at the third grade in primary school, increased through grade levels and ended after secondary school. At the secondary level, girls received higher grants than boys, 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 per month on average 101 pesos through food related transfers and 139 through school related transfers. In total, between November 1998 and October 1999, these transfers represented more than 18 per cent 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. In these localities households that experienced extreme poverty (measured on the basis of household income, characteristics of the head of household, and variables related to dwelling conditions) were included in 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.

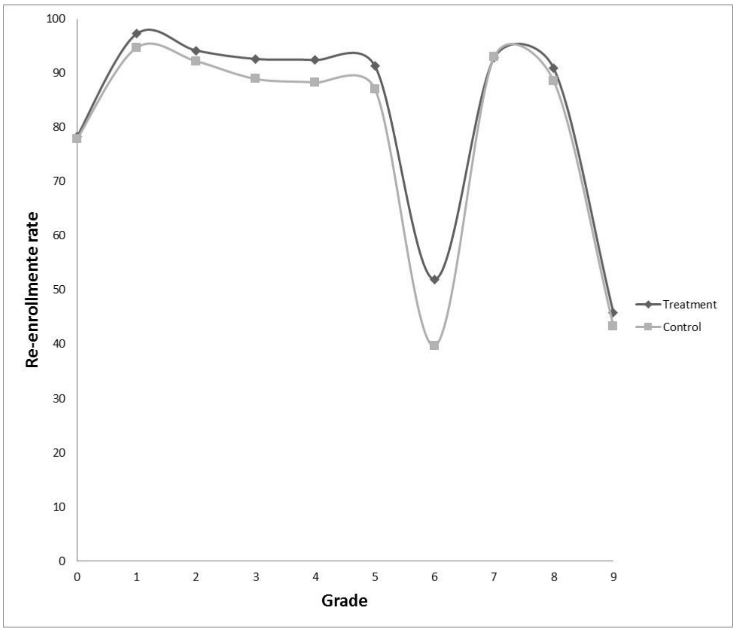
Collecting data to make a rigorous evaluation of Progresa possible was an important concern of the program designers from the outset, and the delayed incorporation of some localities was instrumental to obtain these data. The data on children's education we use were collected in October 1998, when a first group of localites, hereafter referred as to inmediate treatment group, already received treatment, but not a second groups of localities which was incorporated later (referred to as delayed treatment group). Data were collected in 320 immediate treatment localities and 186 delayed treatment localities. We follow much of the literature (see, e.g., Gertler (2004); Schultz (2004); Behrman, Sengupta, and Todd (2005); Todd and Wolpin (2006); Attanasio, Meghir, and Santiago (2012); Dubois, Janvry, and Sadoulet (2012)) and use the delayed treatment sample as a control group in order to identify the short run effects of the program [2]. In both treatment and control samples, we only consider the households that were eligible for the program; these are the localities' poor households. In addition, we use baseline data collected in 1997 to obtain some of the information on children's background characteristics such as parental education, gender of the head of the household, as well as dwelling characteristics

[2] Children that belong to treated households have been receiving treatment only for the last 8 months. Moreover, the sample is restricted to school-aged children, none of which can have benefitted directly from the nutritional supplements (only given to children up to age 5). Hence the long run effects of the program are probably larger.

and demographic composition of the household. The reason for using the 1997 survey is the lack of background information in the 1998 sample. A caveat of such a procedure, however, is the difficulty to merge information from both years for some of the children observed in 1998. Our sample, thus, contains children whose background information in 1997 could be successfully traced with their information on education from 1998[3].

Figure 1 below provides, for both treatment and control samples, the percentage of the children that attained grade 0 to 9 as their highest completed grade and are re-enrolled two months after the academic year 1998-99 started in August 1998.

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



Source: Author's calculation.

School re-enrollment is high across all grades, except for grade 6, which is when primary education is finished, and grade 9, which is when secondary school is finished. There are clear differences between the treatment and control sample for children that completed grades 3 to 6. Especially for the latter, re-enrollment in the treatment sample is higher than in the control sample.

Two remarks must be made at this point. First, a simple comparison of treatment and control samples might be misleading if these samples differ in terms of observable or unobservable characteristics. Suppose, for instance that the treatment sample contains more children from higher educated parents and that higher educated parents keep their children longer in school. If this is the case, some of the difference in average re-enrollment between the treatment and control sample would be due to this higher

[3]Information on education was collected only for children between 6 and 17 years old in 1998. In total, 84% of these children have also information in 1997.

frequency of educated parents in the treatment sample and not only be a consequence of the treatment, and the causal effect of the program would be unidentified. The consensus is, however, that the differences between the treatment and control samples in terms of characteristics are limited (see, Behrman and Todd (1999)).[4] Second, looking at average re-enrollment per grade is not informative about the effect the program has on the distribution of re-enrollment between children confronted with different circumstances. For example, an increase in the average re-enrollment rate could result from some children having advantaged circumstances (for instance those with highly educated parents) seeing their re-enrollment rate increase much more than average, and those with disadvantaged circumstances (those having poorly educated parents), seeing their re-enrollment rate decrease. Since the ultimate goal of this paper is to evaluate the effect of Progresa on inequality of re-enrollment opportunities, we have to look at distributional effects. The next section describes in detail the procedure we follow to account for the effect of the program on inequality of opportunity.

3 Methodology

3.1 Theoretical framework

We have a fixed set $N = \{1, \ldots, n\}$ of children. For each child i we have determined p_i, the probablity that he or she re-enrolls given his or her circumstances, where circumstances are characteristics of the child (or its environment) over which it has no control, such that we do not want to hold the child responsible for these characteristics. The higher a child's p_i, the more advantage the child's circumstances offer [5]. Therefore, in the literature on the measurement of inequality of opportunity, p_i is considered to measure the value of the child's opportunities. The vector $p = (p_1, \ldots, p_n) \in [0, 1]^n$ records the value of the opportunities of the children in our set N. To evaluate the opportunites of our set of children, we define an opportunity aggregator function $O : [0, 1]^n \longrightarrow \mathbb{R}$.

We impose the following standard properties on this aggregator function. First, $O(p)$ satisfies anonimity: any permutation of the verctor p yields the same value for the opportunity aggregator function as the original p. Second, $O(p)$ satisfies non-decreasingness 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 the vector of opportunities with the program. We now have the following well-known result (Shorrocks (1983)).

[4] In addition, we test for each grade, the balance of our sample using baseline data collected in 1997. Using a logistic model, we estimate the probability of participating in the program conditional on a rich set of pre-program characteristics. The results, shown in the Appendix (table A4) suggest that there are no substantial differences between treatment and controls.

[5] This is a measurement statement, not a causal statement. Suppose that children facing some circumstances are more motivated to re-enroll at school. In that case, the effect of circumstances on their motivation and thereby on their re-enrollment probability will be attributed to their circumstances.

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

Generalized Lorenz dominance is a condition under which we can unambiguously compare, for all opportunity aggregator functions satisfying the three properties, what happens to the aggregate opportunities of the children.

We are, however, also interested in what happens to the inequality in the distribution of childrens' opportunities. Define an inequality measure as a functions $I : [0,1]^n \longrightarrow \mathbb{R}$. We impose standard properties on this inequality measure. First, $I(p)$ satisfies anonimity: any permutation of the vector p yields the same value for the inequality index as 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, $I(p)$ 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 anonimity, the weak transfer principle and relativity, inequality of opportunity in the vector p^T will not be larger than inequality of opportunity in the vector p^N if the Lorenz curve of p^T lies nowhere below the Lorenz curve of p^N.

Finally, we are interested to decompose the effect of Progresa on aggregate opportunities into an effect on average opportunities and an effect on inequality of opportunity. This kind of question can be analyzed using "abbreviated" opportunity aggregator functions (by analogy with "abbreviated social welfare functions" - see Lambert (2001)). More in particular, defining average opportunities $C = (1/n)\sum_{i=1}^{n} p_i$, use abbreviated opportunity aggregator functions that can be written as

$$O(p) = C\left[1 - I(p)\right]. \tag{1}$$

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 the procedure suggested by de Barros, Vega, and Saavedra (2008), the change in the values of abbreviated opportunity aggregator functions brought about by Progresa can be decomposed as follows:

$$\begin{aligned} \underbrace{O(p^T) - O(p^N)}_{\Delta_O} &= C^T(1 - I(p^T)) - C^N(1 - I(p^N)) \\ = C^T(1 - I(p^T)) - C^N(1 - I(p^T)) &+ C^N(1 - I(p^T)) - C^N(1 - I(p^N)) \\ &= \underbrace{(C^T - C^N)(1 - I(P^T))}_{\Delta_C} + \underbrace{C^N(I(p^N) - I(p^T))}_{\Delta_I}. \end{aligned} \tag{2}$$

Equation (2) shows how the change in the Opportunity aggregator, Δ_O can be decomposed in 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, proposed by de Barros, Vega, and Saavedra (2008), takes as inequality measure the dissimilatiy index

$$D = \frac{1}{2C}\frac{1}{n}\sum_{i=1}^{n}|p_i - C|, \tag{3}$$

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 his circumstances, ends up with average school re-enrollment (all $p_i = C$). With the vector $\hat{p}$ as the vector obtained after permutating the elements of p such that $\hat{p}_1 \leq \hat{p}_2 \leq \ldots \leq \hat{p}_n$ and m such that $\hat{p}_m \leq C < \hat{p}_{m+1}$, it is easy to show that the opportunity aggregator function corresponding to (1) and (3), the value of the Human Opportunity Index, can be written as

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

This aggregator function is strictly increasing in all $\hat{p}_i$. It also satisfies the weak transfer principle, but observe that transfers between children that have lower than average opportunities or transfers between children that have higher than average opportunites do not affect the value of H. It only values transfers from children with above average opportunities to children with below average opportunities. This might be considered a disadvantage for those who adhere to the strong transfer principle, which 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, always decreases inequality.

There exist many ways of constructing an abbreviated opportunity aggregator function satifying the strong transfer principle. We propose one, based on the familiar Gini index of inequality,

$$G = 1 + \frac{1}{n} - 2\frac{\hat{p}_n + 2\hat{p}_{n-1} + 3\hat{p}_{n-2} + ... + n\hat{p}_1}{n^2 C}. \tag{4}$$

It is easy to show that the opportunity aggregator function corresponding to (1) and (4), the value of the Gini Opportunity Aggregator, can be written as

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

showing 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

In principle, to determine the extent of inequality of opportunity, we need a complete description of each child's circumstances. Our data contain a rich set of circumstances thanks to the information that was collected as part of the evaluation of the program in October 1998 and September 1997. Descriptive statistics of these variables are shown in Table A.1 in the Appendix. The set of circumstances we used include personal and background characteristics and are gender, race (indigenous background), parental background (level of education of the parents), gender of the head of the household, whether his or her spouse lives in the same home and whether there was a secondary school in the locality where the child lives. These circumstances have been selected for the following reasons.

It is well established that large disparities still exist in Mexico in terms of access to and performance at school and part of such disparities are correlated with gender, girls being more disadvantaged than boys. To capture the effect of gender, we use a dummy indicating whether the child is a boy or a girl. Similarly, having an indigenous background is traditionally believed to diminish the possibilities that a child receives education given the social relegation of this part of the population. Indigenous background is measured by a variable that indicates if the child belongs to a family where either the head of the household or the spouse of the head speaks an indigenous language. Speaking an indigenous language is perhaps the best indicator for indigenous background since it is not common that non-indigenous people in Mexico speak an indigenous language.

The effect of parental characteristics on children's schooling has been widely documented. Particularly important is the education of the parents. Molinas, de Barros, Saavedra, and Guigale (2012) show for example that parents' education is the most important circumstance to explain school enrollment of children aged 10 to 14 in most Latin American countries, including Mexico. In our sample, there are few household heads with more than secondary education (grades 7-9); therefore, educational background of the children is measured by dummy variables that 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. Gender of the head of the household and whether the head and the spouse live in the same household are also indicated by dummies. The later helps to identify single parent families. The presence of a secondary school in the locality where the child lives is potentially important, as it could affect the decision of the child to go to primary or secondary school. Finally, as we have many missing values for some of the circumstances (see Part (b) of Table A.1) and to be able to use these observations in the estimation of the effects of the circumstances for which their values are not missing, additional dummies that indicate the presence of missing values are included as circumstances.

3.3 Empirical methodology

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

$$Prob(Y_i = 1) = \frac{exp(\beta_0 + \sum_{k=1}^{K} \beta_k X_{ik} + \gamma_0 T_i + \sum_{k=1}^{K} \gamma_k T_i X_{ik})}{1 + exp(\beta_0 + \sum_{k=1}^{K} \beta_k X_{ik} + \gamma_0 T_i + \sum_{k=1}^{k} \gamma_k T_i X_{ik})}. \tag{5}$$

The estimated values of the coefficients β_0, β_1, ..., β_k, and γ_0, γ_1, ..., γ_k are used to generate, for every child, two estimated probabilities: 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), this must be due to differences in circumstances. Hence, the inequality in the vector of predicted probabilities p_i^T (or p_i^N) is entirely due to children having different circumstances, and is, for that reason, a measure of inequality of opportunity amongst children participating (not participating) in the program[6]. Average re-enrollment can be computed easily: with Z=T or N,

$$C^Z = \frac{1}{n} \sum_{i=1}^{n} p_i^Z. \tag{6}$$

Having obtained the predicted values p_i^N and p_i^T, we rearrange them increasingly, into the vectors p^N and p^T, respectively. These vectors form the input in our evaluation exercise.

To construct the Generalized Lorenz curves, we define M_d^T as the value of the coordinate of the Generalized Lorenz curve in case of being treated and M_d^N as that in case of not being treated. The value of these coordinate at each decile d for $d = 1, ..., 10$ [7] is given by:

$$M_d^Z = \sum_{i=1}^{\tilde{d}(d)} p_i^Z, \tag{7}$$

where Z=T if treated or N otherwise and $\tilde{d}(d)$ is the integer that is closest to $[n/10] * d$. For each $d = 1, ..., 10$, the difference between the coordinates of the Generalized Lorenz curve in case of being treated and not being treated is $M_d^D = M_d^T - M_d^N$. Similarly, define, for each decile $d = 1, ..., 10$, L_d^T as the value of the coordinate of the Lorenz curve in case of being treated, L_d^N as that in case of not being treated and the difference between both as $L_d^D = L_d^T - L_d^N$. The value of the coordinates of the Lorenz curve at decile d is given by:

$$L_d^Z = \frac{1}{\tilde{d}(d) C^Z} \sum_{i=1}^{\tilde{d}(d)} p_i^Z, \tag{8}$$

where Z=T if treated or N otherwise, C^Z is the respective value of the average predicted probability p_i^Z as shown in (6) and $\tilde{d}(d)$ is as above. $M_d^D > 0$ for all d implies that the Generalized Lorenz curve of treated children dominates that of non-treated children, and

[6]This is the basic idea of the direct approach to the measurement of inequality of opportunity see, e.g., Pistolesi (2009) and Ferreira and Gignoux (2011). For a comparison with other measures, see Ramos and Van de gaer (2015).

[7]Given the computational burden that arises with the Bootstrap procedure explained in the Appendix, we only look at values at each decile.

therefore, that Progressa increases aggregate opportunities for all aggregator functions satisfying anonimity, the weak transfer principle and non-decreasingness (see Proposition 1). Similarly, $L_d^D > 0$ for all d indicates Lorenz dominance such that Progresa reduces inequality of opportunity for all inequality indices satisfying anonimity, relativity and the weak transfer principle (see Proposition 2).

We can easily compute D^T and H^T by replacing p by p^T in (3) and (6) and inserting the results in (1). Similarly replacing p by p^N results in D^N, C^N and H^N. Following the decomposition procedure described in (2), the change in the Human Opportunity Index that is due to Progresa can be decomposed as follows:

$$\underbrace{H^T - H^N}_{\Delta_H} = \underbrace{(C^T - C^N)(1 - D^T)}_{\Delta_{C^H}} + \underbrace{C^N(D^N - D^T)}_{\Delta_D},$$

such that the change in the Human Opportunity Index can be decomposed in two components: Δ_{C^H}, the contribution of the change in average school re-enrollment, and Δ_D, the contribution of the change in inequality of opportunity. After computing G^T, S^T, G^N and S^N, following the decomposition procedure described in (2), the change in the Gini Opportunity Aggregator can be decomposed similarly:

$$\underbrace{S^T - S^N}_{\Delta_S} = \underbrace{(C^T - C^N)(1 - G^T)}_{\Delta_{C^G}} + \underbrace{C^N(G^N - G^T)}_{\Delta_G},$$

where we see that the change in the Gini Opportunity Aggregator can be decomposed in two components: Δ_{C^G}, the contribution of the change in average school re-enrollment, and Δ_G, the contribution of the change in inequality of opportunity.

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 his circumstances (the regression coefficients are reported in Table A.2 in the Appendix). The results suggest that treated children, boys and indigenous children have a higher probability of re-enrollment. Parental education plays an important role: living in a household where the head has a higher level of education (a proxy we use for education of the father) and, especially where the wife of the household head has a higher level of education (a proxy for maternal education) both correlate 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, especially after completion of primary school (grade 6). The only surprising result is that indigenous children have a higher probability of re-enrollment, but remember that we control for other features of the household in the estimation, such as the level of education of the household head and his wife. Some of the popular impression that indigenous children have lower rates of re-enrollment than non-indigenous children might be due to the fact that they live in households with lower educated parents. No firm conclusions can be drawn about the interaction effects between treatment and circumstances: only very few of them are statistically significant[8]. It is striking, though, that, when the interaction effects are statistically significant, they usually have the opposite sign of the direct effect of the circumstance. This suggests that the treatment might compensate the effect of the circumstance on school re-enrollment, which can be expected to diminish inequality of opportunity.

4.2 Average effect

We use the estimated coefficients to predict for each child the probability of re-enrollment in case he is treated (p_i^T), and in case he is not treated (p_i^N)[9]. We compare the average of these two probabilities per grade group 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 1.

Table 1: 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		% gains ($p_t > p_n$)
Grade	TS	CS	TS	CS	TS	CS	TS	CS	TS ∪ CS
0	2803	1773	0.786	0.774	0.786	0.776	0.780	0.774	0.522
1	2139	1333	0.973	0.947	0.973	0.973	0.947	0.947	0.808
2	2116	1316	0.943	0.920	0.943	0.940	0.923	0.920	0.735
3	2011	1202	0.928	0.889	0.928	0.926	0.890	0.889	0.850
4	1773	1078	0.925	0.880	0.925	0.923	0.884	0.880	0.749
5	1640	979	0.914	0.870	0.914	0.913	0.871	0.870	0.922
6	2555	1519	0.520	0.397	0.520	0.517	0.400	0.397	0.994
7	803	474	0.929	0.930	0.929	0.929	0.929	0.930	0.534
8	645	345	0.912	0.890	0.912	0.912	0.883	0.890	0.758
9	449	254	0.454	0.433	0.454	0.462	0.438	0.433	0.560

Notes: TS(CS) stand for the sample of children that lived in a treatment (control) locality. TS∪CN indicates that the union of both samples is used. Source: Author's calculations.

The first column (I) gives the grade the children completed before the start of the school year. The second column (II) gives the sample sizes of the treatment (TS) and the third (III) that of the control sample (CS). The fourth and fifth columns show the re-enrollment rates for these samples; these numbers are the same as those depicted in Figure 1. Columns (VI)-(IX) give the predicted average probabilities of re-enrollment in case the child receives treatment (p^T) and in case he does not receive treatment (p^N), for the children in both the treatment and control sample. The final column, (X), gives the percentage of children for which the predicted probability in case of treatment is larger than the predicted probability of enrollment without treatment.

[8]We don't discuss the coefficients of the missing value dummies, as we only included these dummies to keep our sample as large as possible - see also final comment in Section 3.2.

[9]In the regressions some variables perfectly predicted school enrollment. They were then left out of the estimation (see Table A.2). In that case we deleted one of the co-linear variables, resulting in empty cells in Table A.2. See footnote in the table for further details.

The following observations can be made. First, the numbers in column (IV) and (VI) are equal, as are the numbers in columns (V) and (IX). This is a result of the estimation procedure we followed. Second, the average probabilities in case of receiving treatment for the TS and CS at the one hand (columns VI and VII) and the average probabilities in case of not receiving treatment for the TS and CS on the other hand (columns VIII and IX), are very close to each other. This suggests that differences in re-enrollment rates between the two samples are not due to differences in composition in terms of the circumstances we incorporated. Third, differences between the average predicted re-enrollment probabilities $\hat{p}^T$ and $\hat{p}^N$ for both TS (column VI versus VIII) and CS (column VII versus IX) are very small for grades 0 and 7. Except for grade 5, the differences from grade 1 onwards become more pronounced, and they are particularly large for grade 6, i.e., for those that completed primary education. This suggests that Progresa induces children to stay in school, especially after completion of primary education. Finally, the last column confirms that for most children the predicted probability of re-enrollment is larger when they receive treatment than when they don't. The percentage that gains is very large for children that completed grades 1 to 6. So, overall, more children seem to gain from the program than loose. Yet, the question what the program does to the evaluation of aggregated opportunities when inequality of opportunity is taken into account, or what the program does to inequality of opportunities remains unanswered so far.

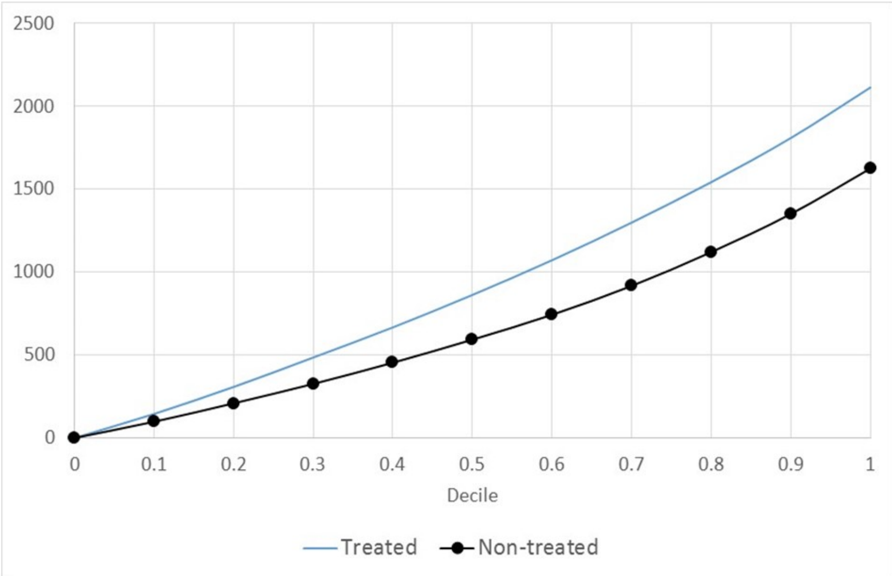
4.3 Dominance

We discuss in first place the results for Generalized Lorenz dominance. As the results for grade 6 are more outspoken and this is the grade where most children drop from school, we depict in Figure 2 the Generalized Lorenz Curve for this grade only. Results for the other grades are presented in the Appendix, in Table A.4. As can be observed in Figure 2, the curve for treated children lies, for all deciles, above the one for non-treated children, suggesting that Progresa improves aggregate opportunites for the participants. That is, for all opportunity aggregator functions satisfying the three properties mentioned in Proposition 1, the distribution of opportunities in case of treament is unambiguously preferred to the distribution in the absence of treatment. Furthermore, the difference between both curves is substantial and statistically significant.

We now use Proposition 2 to look into the effects of the program on inequality of opportunity; we compare the Lorenz curve when children are treated and not treated. Again, the results for grade 6 are more interesting, so we show only results for this grade in Figure 3, but the reader can find in Table A.4. the coordinates for all the grades. Looking at the figure, it is clear that the Lorenz curve when treated is never below when not treated. Moreover, at each decile, the difference of the value of the coordinates of the Lorenz Curve between M_d^T and M_d^N is always statistically significant. Hence we can infer that Progese reduced inequality of opportunity in grade 6 for all inequality measures satisfying the three properties mentioned in Proposition 2.

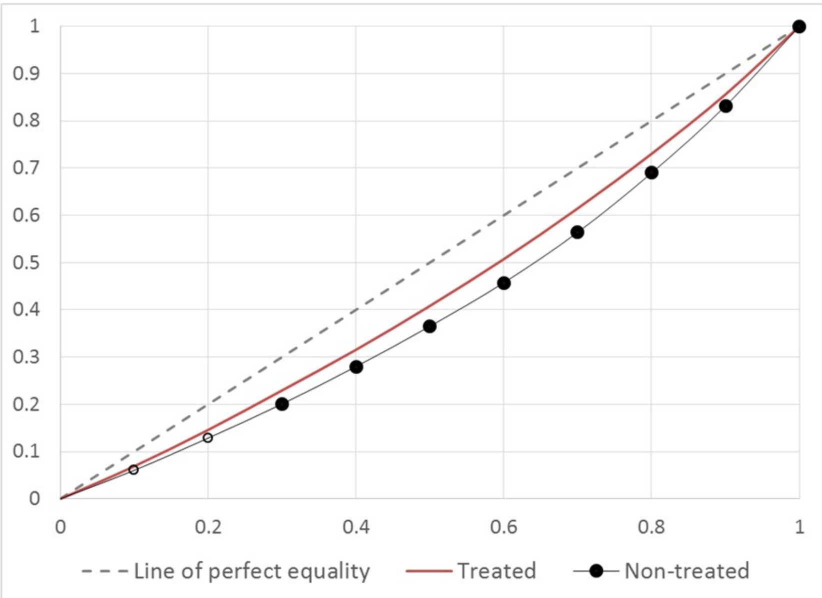
The results for the other grades are, overall also positive. For most grades, the differences (M_d^D and L_d^D) are always positive, suggesting that the program reduces inequality of opportunities for those grades too. Clearly, for grades 0, 5 and 7, some values for M_d^D or L_d^D are negative, but these negative differences are small and never statistically significant.

Figure 2: Generalized Lorenz Curve grade 6



Notes: A solid-black circle (●) indicates the difference between treated and non-treated Generalized Lorenz curves, at a particular decile, is significant at 1% level of significance, while a white-unfilled circle (❍) indicates that this difference is significant at 5%.

Figure 3: Lorenz Curve grade 6



Notes: A solid-black circle (●) indicates the difference between treated and non-treated Lorenz curves, at a particular decile, is significant at 1% level of significance, while a white-unfilled circle (❍) indicates that this difference is significant at 5%.

4.4 Decomposing the effect

The final step in our analysis is to decompose the effect of Progresa on our two selected aggregator functions, the Human Opportunity Index and the Gini Opportunity Aggregator, into an effect on average opportunities and inequality of opportunity. In Table 2 we report the results for the Human Opportunity Index.

Table 2: The Human Opportunity Index (all entries multiplied by 100)

(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)	(IX)	(X)	(XI)	(XII)
	Human Opp. Index			Re-enrollment rate			Dissimilarity Index				
Grade	T	N	T-N	T	N	T-N	T	C	T-N	Δ_{C^H}	Δ_D
0	74.025	73.701	0.324	78.215	77.772	0.444	5.357	5.233	0.123	0.420	-0.096
1	96.472	92.723	3.749	97.319	94.218	3.102	0.871	1.587	-0.715	3.075	0.674
2	92.500	90.163	2.337**	94.175	92.235	1.941**	1.779	2.246	-0.467	1.906**	0.430
3	91.388	86.950	4.438***	92.733	88.970	3.762***	1.450	2.270	-0.821**	3.708***	0.730**
4	91.273	85.279	5.993***	92.433	88.298	4.135***	1.255	3.418	-2.163***	4.083***	1.910***
5	89.288	89.434	-0.146	91.360	91.043	0.317	2.268	1.768	0.500	0.309	-0.456
6	47.034	34.210	12.824***	51.903	39.910	11.992***	9.380	14.283	-4.903***	10.867***	1.957***
7	91.415	91.645	-0.229	92.899	92.967	-0.067	1.597	1.422	0.175	-0.066	-0.163
8	89.538	86.094	3.445*	91.195	88.581	2.614	1.816	2.807	-0.991	2.567	0.878
9	41.396	37.393	4.003	45.723	43.656	2.067	9.464	14.347	-4.883	1.871	2.132

*Notes: *denotes significance at 10%, ** at 5%, and *** at 1%. Confidence intervals were determined using a Bootstrap procedure. Source: Author's calculations.*

The first column in the table gives the grade attained before the start of the school year. Column (II) and (III) give the value of the Human Opportunity Index based on the estimated probability of re-enrollment when treated (p_i^T) and when non-treated (p_i^N), respectively. Similarly, columns (V) and (VI) show the average re-enrollment rate, and columns (VIII) and (IX) the dissimilarity index. Column (IV) gives the change in the Human Opportunity Index, and equals the difference between column (II) and (III). Similarly, columns (VII) and (X) give the change in the average re-enrollment rate and dissimilarity index and equal the difference between columns (V) and (VI), and (VIII) and (IX), respectively. Column (XI) gives the part of the change in the Human Opportunity Index that can be attributed to an increase in average re-enrollment, and column (XII) that of the 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, III, V, VI, VIII, and IX), we only report whether the differences are statistically significant (columns IV, VII, IX and XII).

The following conclusions can be drawn. First, the values of the Human Opportunity Index are very high for all grades, except for grades 6 and 9, irrespective of whether children receive treatment or not. Second, there exist gains from treatment in terms of the Human Opportunity Index for almost all grades, the exceptions being grades 5 and 7, but these losses are rather small and not statistically significant(column IV). Third, the positive effect observed in grades 2, 3, 4, and 6, is due to an increase in average re-enrollment (column VII), and to a decrease in the dissimilarity index (column X). Finally, in those grades where the Human Opportunity Index increased significantly at 5 %, the decrease in inequality of opportunity, Δ_D, contributed between 16% and 32% to the increase in the Human Opportunity Index.

Table 3: The Gini Social Welfare Function (S) (all entries multiplied by 100)

(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)	(IX)
	Gini Welfare				Gini			
Grade	T	N	T-N	T	N	T-N	Δ_{C^G}	Δ_G
0	72.381	72.062	0.319	7.459	7.341	0.117	0.410	-0.091
1	96.175	92.115	4.059	1.176	2.231	-1.055	3.065	0.994
2	91.957	89.336	2.621 **	2.355	3.143	-0.787	1.895 **	0.726 *
3	90.840	86.087	4.752 ***	2.041	3.240	-1.199 **	3.685 ***	1.067 **
4	90.778	84.121	6.657 ***	1.790	4.730	-2.939 ***	4.061 ***	2.595 ***
5	88.522	88.899	-0.376	3.106	2.355	0.750	0.306	-0.683
6	45.332	32.439	12.892 ***	12.660	18.721	-6.061 ***	10.473 ***	2.419 ***
7	90.934	91.132	-0.1975	2.115	1.974	0.141	-0.065	-0.131
8	88.846	85.097	3.748 *	2.575	3.932	-1.356	2.546	1.202
9	39.687	35.041	4.646	13.201	19.735	-6.534	1.794	2.852

*Notes: *denotes significance at 10%, ** at 5%, and *** at 1%. Confidence intervals were determined using a Bootstrap procedure. Source: Author's calculations.*

Table 3 reports the results for the Gini Opportunity Aggregator (S), using the same format as in Table 2, but without repeating the results for average re-enrollment, as they are identical to the ones reported in Table 2. The results are entirely in line with those reported in and discussed after Table 2. The only difference is that the effects on the Gini Opportunity Aggregator are usually slightly larger. Clearly, the results tell the same story: Progresa not only improved average re-enrollment rates of children, but it also decreased inequality of opportunity. The effects are statistically significant for grades

2, 3, 4 and 6, and are especially large for those children that completed primary education (grade 6) and grade 4. In those grades where the Gini Opportunity Aggregator increased significantly at %5 (i.e. grades 2, 3, 4 and 6), the decrease in inequality of opportunity, Δ_G, contributed between 19% and 39% to the improvement in the value of the Gini Opportunity Aggregator.

5 Conclusion

Many social programs aim to improve children's opportunities, especially for those that, due to their circumstances, would not get sufficient opportunities. 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), and identify for which of these children the program works better. However, so far, no study has tried to obtain an overall assessment of the effect of a social program like Progresa on the distribution of school re-enrollment between children with different circumstances. This paper is a first attempt to provide an assessment of the effects of Progresa that takes into account its effect on the distribution of children's opportunities.

To do so, we find inspiration in the recent literature that tries to quantify the extent to which opportunities are unequally distributed. A child's probability of being re-enrolled, conditional on its circumstances is seen a a measure of the opportunities available to the child. Inequalities in these probabilities are exclusively due to children having different circumstances and are therefor considered offensive. Hence a social planner that wants to evaluate the distribution of opportunites should favor a redistribution (i.e. a transfer) of opportunities from those with higher to those with lower opportunities. The evaluation framework we develop here incorporates this concern.

We have seen that, overall, the distribution of re-enrollment probabilities conditional on circumstances when treated both Generalized Lorenz and Lorenz dominates the distribution of re-enrollment probabilities when not treaed. The former indicates that any social evaluator that aggregates children's opportunities in an anonymous way and is inequality averse with respect to the distribution of opportunities must find that Progresa increases aggregate opportunities. The latter indicates that, whenever inequality of opportunity is measured by a relative measure, progresa decreases inequality of opportunities. Finally, when the Human Opportunity Index or the Gini Opportunity Aggregator is chosen to aggregate children's opportunities, the effect of the reduction in inequality of opportunity by the program accounts for between 15 and 40 % of the effect of the program on the aggregate opportunities. Hence, we found clear evidence that Progresa not only improved school re-enrollment opportunities in the aggregate and on average, but it also significantly and substantially reduced inequality of school re-enrollment opportunities.

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6 Appendix

Table A.1: Composition of and missing values in treatment and control samples in percentage

	TS	NS	T∪N
(a)Composition			
Gender child (male)	48.01	46.61	47.48
Indigenous background	28.84	30.32	29.4
Gender household head (male)	86.71	87.18	86.89
Household head living with partner	90.72	90.63	90.69
Secondary school in the locality	29.46	28.61	29.14
Household head no education	25.47	26.48	25.85
Household head incomplete primary	45.83	45.45	45.69
Household head at least complete primary	17.26	15.9	16.74
Spouse no education	28	29.98	28.37
Spouse incomplete primary	38.54	36.3	37.69
Spouse at least complete primary	14.4	13.76	14.16
(b) Missing values			
Gender child (male)	7.67	8.07	7.82
Indigenous background	14.99	16.25	15.47
Gender household head	5.97	5.59	5.83
Household head living with partner	1.2	0.73	1.02
Secondary school in the locality	0.57	0	0.36
Education of household head	11.44	12.18	11.72
Education of spouse	19.07	20.96	19.79

Notes: TS(NS) stands for the sample of children that lived in a treatment(control) locality; TS∪CN indicates that the union of both samples is used In part (a), columns 2-4 give the fraction of the children that have the characteristics mentioned in the first column, as a percentage of the children in that sample for which data on that characteristic were reported In part (b), columns 2-4 give 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. Source: Author's calculations.

Table A.2: Estimated coeffcients of the logistic model for school enrollment per grade

	Grade in 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 sex	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 sex	-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)
Secondary 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 Comp 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 Comp 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 Sex * 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 Sex * 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 married * 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)

Continued on next page

Table A.2 – continued from previous page

	Grade in 1998									
	0	1	2	3	4	5	6	7	8	9
Secondary 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 comp 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 comp 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 sex m	-0.298*			0.239					0.708	
	(0.154)			(0.278)					(0.582)	
Hh educ m	0.491***		1.141***	0.610**						
	(0.166)		(0.400)	(0.279)						
Sp educ m		0.932***							0.285	
		(0.283)							(0.481)	
Secondary loc m	0.457***									
	(0.0982)									
Hh married m	1.332***				-1.313**					
	(0.410)				(0.549)					
Hh sex m			-1.506***						-1.187**	
			(0.533)						(0.560)	
Ind m									-0.534	
									(0.529)	
Constant	0.155	1.797***	1.818***	1.388***	0.879**	1.538***	-1.265***	2.176***	2.252***	-0.440
	(0.263)	(0.567)	(0.427)	(0.379)	(0.368)	(0.303)	(0.203)	(0.649)	(0.862)	(0.623)
Observations	4,576	3,472	3,432	3,213	2,851	2,619	4,074	1,277	990	703

Notes:

a) Robust standard errors in parentheses. The stars behind the coefficients indicate their significance: *** (**) [*] means significant at 1 (5) [10] percent.

b) Empty cells indicate either that the corresponding dependent variable was perfectly collinear with (a subset of) the other variables or that the coefficient was not significant in the original specification (one that included a dummy for each missing characteristic).

c) Keeping only missing dummies that are significant (in the original specification) obeys to the fact of avoiding as much as possible dependent variables that perfectly predict school enrollment. This point turned especially important when implementing the bootstrap procedure described in Appendix 6.1. In some cases, variables that were originally significant are not any more in the final specification, as is the case for "Ch sex m" in grade 3.

d) All variables in the regression are dummy variables: they are either 1 or zero. We indicate when they are equal to 1. Variable T: equal to 1 if the child is treated; Ch sex: equal to 1 if the child is a boy; Ind: equal to 1 for indigenous children; Hh sex: equal to 1 if the household head is male; Hh married: equal to 1 if the household head is married; Secondary loc: equal to 1 if there is a Secondary school in the locality where the child lives; 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 Comp prim (Sp Comp prim) : equal to 1 if the (spouse of the) household head completed at least primary schooling; Ch Sex m: equal to 1 if the child's gender is mising; Ind m: equal to 1 if the indigenous background of the child is missing; Hh Sex m: equal to 1 if the household head's gender is missing; Hh married m: equal to 1 if the household head's marital status is missing; Secondary loc m: equal to 1 if we don't know whether there is a secondary school in the locality where the child lives; Hh educ m (Sp educ m): equal to 1 if the (spouse of the) household head's education is missing. Source: Authors' calculations.

6.1 Appendix: Bootstrap procedure

Step 0: k=1;

Step 1: For each sample of size n, take a random sample of n elements with replacement and estimate the logistic specification described in Section 3.3 in the main text.

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

- $\beta_k' X_i^T$, the index value if treated;
- $\beta_k' X_i^N$, the index value if not treated;
- p_i^T the probability of enrollment if treated;
- p_i^N the probability of enrollment if not treated;

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_{cm}.

Step 3: From step 2, some observations for which one or more coefficients β_k predict perfectly school enrollment were dropped, and therefore the corresponding predicted values $\hat{p}_i^T$ and $\hat{p}_i^N$ are not estimated. For these observations, we replace the estimated predicted values according to the following criteria:

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

Step 4: Use the values presented in step 3 to compute the following:

(i) All the statistics mentioned in Tables 2 and 3 in the main text of the paper.

(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 3000 times, and each time, store the results in the $k-th$ row of matrix A so that we end up with matrix A of dimension [3000, B], with B being the total number of statistics computed in each loop.

Step 6: Use the respective empirical distribution obtained for each statistic to construct the 99%, 95% and 90% confidence intervals for each of them.

Step 7: Finally, perform a test for Lorenz and Generalized Lorenz dominance on the basis of the vectors of ordinates. The test consist in posing the null as "nondominance" of the treated by the non-treated respective curve, such that, in case of rejection of the null, all that is left is dominance. To carry out the test, we compute the fraction (f) of jointly positive values for the difference of the Lorenz and Generalized Lorenz coordinates, and interpret $1 - f$ as the level of significance to reject the null.

Table A.3: Generalized Lorenz coordinates per grade for treatment and controls

Grade in 1998	0	1	2	3	4	5	6	7	8	9
Decile 1 (T)	268.955	319.307	292.321	276.556	246.252	213.992	145.735	110.059	82.062	20.395
Decile 1 (NT)	268.147	300.111	279.923	255.830	209.624	224.687	98.247	110.520	74.343	14.162
Difference (T-N)	0.809	19.197	12.398	20.725	36.628	-10.696	47.488***	-0.461	7.719*	6.234*
Decile 2 (T)	572.950	650.106	602.475	564.211	500.877	434.210	308.548	222.386	167.658	44.763
Decile 2 (NT)	569.735	612.759	576.386	525.082	438.231	452.578	209.072	223.571	155.427	31.953
Difference (T-N)	3.215	37.347	26.089*	39.129***	62.646***	-18.368	99.476***	-1.185	12.231*	12.810*
Decile 3 (T)	898.905	985.904	917.421	856.441	759.435	666.126	485.372	338.022	255.626	71.330
Decile 3 (NT)	893.714	932.861	883.294	801.845	674.562	682.248	326.355	340.123	239.466	54.487
Difference (T-N)	5.191	53.043	34.127*	54.595***	84.873***	-16.123	159.017***	-2.101	16.160*	16.843*
Decile 4 (T)	1,244.863	1,322.397	1,235.532	1,150.133	1,020.969	903.354	667.020	455.793	345.082	99.697
Decile 4 (NT)	1,238.062	1,257.432	1,195.421	1,079.936	920.561	914.076	454.864	458.228	326.202	80.744
Difference (T-N)	6.801	64.965	40.111*	70.197***	100.408***	-10.722	212.156***	-2.435	18.880*	18.952*
Decile 5 (T)	1,597.925	1,661.827	1,561.993	1,446.290	1,284.304	1,144.231	863.277	574.346	435.030	130.075
Decile 5 (NT)	1,596.589	1,584.112	1,513.322	1,364.062	1,173.895	1150.528	593.631	576.349	414.079	109.423
Difference (T-N)	1.336	77.715	48.671*	82.228***	110.409***	-6.297	269.647***	-2.002	20.951*	20.652*
Decile 6 (T)	1,967.781	2,002.885	1,891.052	1,747.058	1,550.066	1,386.048	1072.427	695.176	525.995	162.425
Decile 6 (NT)	1,965.117	1,914.730	1,835.031	1,651.149	1,432.000	1389.420	743.475	696.612	503.602	140.237
Difference (T-N)	2.664	88.155	56.021*	95.909***	118.066***	-3.372	328.951***	-1.435	22.393*	22.188
Decile 7 (T)	2,344.820	2,344.770	2,221.739	2,048.790	1,817.676	1,633.540	1299.446	816.906	618.154	197.340
Decile 7 (NT)	2,340.224	2,247.375	2,159.389	1,942.944	1,694.257	1632.185	917.015	817.978	594.660	176.493
Difference (T-N)	4.596	97.395	62.350*	105.846***	123.419***	1.355	382.431***	-1.072	23.494*	20.847
Decile 8 (T)	2,739.024	2,688.985	2,555.567	2,354.094	2,087.232	1,883.024	1543.873	939.507	711.656	234.634
Decile 8 (NT)	2,724.944	2,584.907	2,487.511	2,239.610	1,962.440	1878.256	1121.268	940.219	687.255	215.676
Difference (T-N)	14.080	104.078	68.056*	114.484***	124.792***	4.768	422.605***	-0.712	24.401*	18.957
Decile 9 (T)	3,149.050	3,033.391	2,892.280	2,664.757	2,359.131	2135.973	1809.614	1061.914	806.701	274.838
Decile 9 (NT)	3,130.184	2,926.530	2,823.956	2,545.436	2,236.558	2128.589	1351.678	1062.361	781.225	257.085
Difference (T-N)	18.866	106.861	68.324*	119.321***	122.573***	7.384	457.936***	-0.447	25.476*	17.753
Decile 10 (T)	3,579.128	3,378.930	3,232.098	2,979.497	2,635.260	2392.714	2114.512	1186.323	902.830	321.432
Decile 10 (NT)	3,558.825	3,271.238	3,165.490	2,858.615	2,517.366	2384.422	1625.953	1187.184	876.949	306.902
Difference (T-N)	20.303	107.692	66.608*	120.882***	117.894***	8.292	488.559***	-0.861	25.880*	14.530
Positive treatment effect (joint test)				**	***		***			

*Notes: The stars behind the coefficients indicate the difference between treated and non-treated is statiscally significant. *** (**) [*] means significant at 1 (5) [10] percent. Level of significance based on Bootstrap confidence intervals. (T) stands for treated while (NT) for non-treated. A joint test for dominance at the end of the table indicates if the difference between treated and non-treated is jointly positive for all deciles. Source: Authors' calculations.*

Table A.4: Lorenz coordinates per grade for treatment and controls

Grade in 1998	0	1	2	3	4	5	6	7	8	9
Decile 1 (T)	0.0751	0.0945	0.0904	0.0928	0.0934	0.0894	0.0689	0.0928	0.0917	0.0635
Decile 1 (NT)	0.0753	0.0917	0.0884	0.0895	0.0833	0.0942	0.0604	0.0932	0.0856	0.0462
Difference (T-N)	-0.0002	0.0028	0.0020	0.0033	0.0102***	-0.0048	0.0085**	-0.0004	0.0062*	0.0173*
Decile 2 (T)	0.1601	0.1924	0.1864	0.1894	0.1901	0.1815	0.1459	0.1875	0.1874	0.1395
Decile 2 (NT)	0.1601	0.1873	0.1821	0.1837	0.1741	0.1898	0.1286	0.1885	0.1789	0.1043
Difference (T-N)	0.0000	0.0051	0.0043	0.0057**	0.0160***	-0.0083	0.0173**	-0.0010	0.0085*	0.0352**
Decile 3 (T)	0.2512	0.2918	0.2838	0.2874	0.2882	0.2784	0.2295	0.2849	0.2857	0.2222
Decile 3 (NT)	0.2511	0.2852	0.2790	0.2805	0.2680	0.2861	0.2007	0.2867	0.2756	0.1778
Difference (T-N)	0.0000	0.0066	0.0048	0.0069**	0.0202***	-0.0077	0.0288***	-0.0018	0.0102*	0.0444**
Decile 4 (T)	0.3478	0.3914	0.3823	0.3860	0.3874	0.3775	0.3154	0.3842	0.3857	0.3106
Decile 4 (NT)	0.3479	0.3844	0.3776	0.3778	0.3657	0.3834	0.2798	0.3863	0.3754	0.2635
Difference (T-N)	-0.0001	0.0070	0.0046	0.0082**	0.0217***	-0.0058	0.0357***	-0.0021	0.0103*	0.0471**
Decile 5 (T)	0.4465	0.4918	0.4833	0.4854	0.4874	0.4782	0.4083	0.4841	0.4863	0.4053
Decile 5 (NT)	0.4486	0.4843	0.4781	0.4772	0.4663	0.4825	0.3651	0.4858	0.4765	0.3570
Difference (T-N)	-0.0022	0.0076	0.0052*	0.0082**	0.0210***	-0.0043	0.0432***	-0.0017	0.0098*	0.0482*
Decile 6 (T)	0.5498	0.5928	0.5851	0.5864	0.5882	0.5793	0.5072	0.5860	0.5880	0.5060
Decile 6 (NT)	0.5522	0.5853	0.5797	0.5776	0.5688	0.5827	0.4573	0.5872	0.5795	0.4576
Difference (T-N)	-0.0024	0.0074	0.0054*	0.0088**	0.0194***	-0.0034	0.0499***	-0.0012	0.0084*	0.0484*
Decile 7 (T)	0.6551	0.6939	0.6874	0.6876	0.6898	0.6827	0.6145	0.6886	0.6910	0.6148
Decile 7 (NT)	0.6539	0.6870	0.6822	0.6797	0.6730	0.6845	0.5640	0.6895	0.6843	0.5759
Difference (T-N)	0.0013	0.0069	0.0052**	0.0079**	0.0167***	-0.0018	0.0506***	-0.0009	0.0066*	0.0389*
Decile 8 (T)	0.7653	0.7958	0.7907	0.7901	0.7920	0.7870	0.7301	0.7919	0.7955	0.7310
Decile 8 (NT)	0.7657	0.7902	0.7858	0.7835	0.7796	0.7877	0.6896	0.7925	0.7909	0.7038
Difference (T-N)	-0.0004	0.0056	0.0049**	0.0066**	0.0125***	-0.0007	0.0405***	-0.0006	0.0046*	0.0272*
Decile 9 (T)	0.8798	0.8977	0.8949	0.8944	0.8952	0.8927	0.8558	0.8951	0.9017	0.8563
Decile 9 (NT)	0.8796	0.8946	0.8921	0.8904	0.8885	0.8927	0.8313	0.8955	0.8990	0.8389
Difference (T-N)	0.0003	0.0031	0.0028**	0.0039***	0.0068***	0.0000	0.0245***	-0.0004	0.0027*	0.0174*
Positive treatment effect (joint test) (joint test)				*	***		**			

*Notes: The stars behind the coefficients indicate the difference between treated and non-treated is statiscally significant. *** (**) [*] means significant at 1 (5) [10] percent. Level of significance based on Bootstrap confidence intervals. (T) stands for treated while (NT) for non-treated. A joint test for dominance at the end of the table indicates if the difference between treated and non-treated is jointly positive for all deciles. Source: Authors' calculations.*

Table A.5: Logistic regression estimates being in treatment sample

	Grade achieved before beginning school year 1998									
	0	1	2	3	4	5	6	7	8	9
Gender child	0.014	0.028	0.029	0.027	0.007	0.229**	0.062	-0.126	0.313**	0.203
	(0.079)	(0.079)	(0.081)	(0.077)	(0.075)	(0.092)	(0.090)	(0.129)	(0.143)	(0.167)
Ind	-0.034	0.046	-0.092	-0.201	-0.140	-0.022	-0.062	0.163	-0.026	0.127
	(0.223)	(0.223)	(0.249)	(0.228)	(0.236)	(0.259)	(0.240)	(0.264)	(0.316)	(0.367)
Hh sex	-0.497**	-0.039	-0.758***	-0.581**	-0.283	-0.140	-0.470*	-0.401	0.075	0.058
	(0.209)	(0.269)	(0.278)	(0.259)	(0.295)	(0.282)	(0.248)	(0.424)	(0.431)	(0.484)
Hh married	0.464**	-0.050	0.262	0.066	0.044	-0.132	0.233	-0.210	-0.457	-0.128
	(0.195)	(0.232)	(0.202)	(0.225)	(0.252)	(0.252)	(0.198)	(0.389)	(0.410)	(0.445)
Secondary Loc	0.062	-0.016	0.003	0.255	0.149	-0.009	0.100	-0.008	-0.044	-0.306
	(0.335)	(0.296)	(0.300)	(0.295)	(0.297)	(0.297)	(0.265)	(0.296)	(0.306)	(0.308)
Hh Inc prim	0.148	0.060	0.067	0.032	-0.047	0.094	-0.086	0.040	-0.007	0.272
	(0.121)	(0.123)	(0.121)	(0.117)	(0.121)	(0.158)	(0.115)	(0.175)	(0.283)	(0.337)
Hh Comp prim	0.348**	0.191	0.210	0.287	0.140	0.227	0.070	0.196	0.124	0.037
	(0.177)	(0.174)	(0.164)	(0.187)	(0.196)	(0.211)	(0.180)	(0.285)	(0.328)	(0.419)
Sp Inc prim	0.046	0.186	0.237*	-0.089	0.167	0.150	0.098	0.296	0.366	-0.254
	(0.139)	(0.136)	(0.124)	(0.155)	(0.134)	(0.158)	(0.130)	(0.208)	(0.238)	(0.293)
Sp Comp prim	0.029	0.096	0.171	0.057	0.111	0.153	0.140	0.202	0.119	-0.226
	(0.178)	(0.202)	(0.185)	(0.191)	(0.188)	(0.198)	(0.173)	(0.274)	(0.314)	(0.395)
Gender child m	-0.213	-0.073	0.176	0.550*	-0.068	0.287	-0.604*	-0.699	1.281	
	(0.165)	(0.264)	(0.316)	(0.309)	(0.411)	(0.497)	(0.360)	(0.611)	(0.808)	
Ind m	0.201	0.129	-0.344	-0.560*	-0.077	-0.237	0.399	0.739	-1.378*	0.445
	(0.149)	(0.245)	(0.334)	(0.310)	(0.407)	(0.470)	(0.369)	(0.591)	(0.787)	(0.363)
Sp Educ m	-0.001	0.171	0.182	-0.204	-0.341	-0.392	-0.055	0.010	-0.180	0.511
	(0.275)	(0.335)	(0.275)	(0.310)	(0.320)	(0.345)	(0.325)	(0.617)	(0.689)	(0.901)
Hh Educ m	0.107	0.519	-0.164	0.383	-1.229*	0.124	-0.372	1.007	-1.069	1.349
	(0.621)	(0.477)	(0.488)	(0.615)	(0.718)	(0.554)	(0.450)	(0.928)	(1.054)	(1.157)

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Table A.5 – continued from previous page

	Grade achieved before beginning school year 1998									
	0	1	2	3	4	5	6	7	8	9
Hh Married m	1.067**	0.787*	1.309***	0.133	0.242	0.204	0.767	0.198	0.536	-0.114
	(0.450)	(0.466)	(0.451)	(0.518)	(0.573)	(0.478)	(0.478)	(0.986)	(0.851)	(0.784)
Hh Sex m	-0.884	-0.843	-1.010*	-1.369**	0.728	-0.448	-0.350	-1.117	1.152	-2.276
	(0.646)	(0.649)	(0.594)	(0.640)	(0.789)	(0.540)	(0.543)	(1.184)	(1.194)	(1.432)
Dirtfloor	0.095	-0.040	0.026	-0.129	-0.116	-0.032	0.041	0.036	-0.359	-0.079
	(0.150)	(0.164)	(0.146)	(0.172)	(0.166)	(0.148)	(0.152)	(0.213)	(0.226)	(0.237)
Poor roof	0.135	-0.046	0.126	-0.037	-0.192	0.048	-0.162	0.032	0.031	0.494*
	(0.207)	(0.213)	(0.177)	(0.201)	(0.175)	(0.217)	(0.185)	(0.228)	(0.255)	(0.271)
Poor wall	-0.442**	-0.494**	-0.328	-0.312	-0.297	-0.481**	-0.524**	-0.478*	-0.171	-0.732**
	(0.204)	(0.195)	(0.213)	(0.226)	(0.207)	(0.235)	(0.214)	(0.282)	(0.294)	(0.336)
No rooms	0.039	0.026	0.069	-0.011	0.019	0.038	0.010	0.004	-0.016	0.100
	(0.053)	(0.058)	(0.066)	(0.061)	(0.055)	(0.065)	(0.057)	(0.089)	(0.085)	(0.106)
Water land	0.294	0.312	0.406	0.203	0.380	0.341	0.423*	0.398	0.746**	0.331
	(0.256)	(0.272)	(0.260)	(0.250)	(0.253)	(0.254)	(0.250)	(0.277)	(0.310)	(0.369)
Water house	0.047	0.099	-0.088	0.264	0.149	-0.039	-0.146	0.174	-0.020	0.122
	(0.296)	(0.264)	(0.259)	(0.279)	(0.315)	(0.262)	(0.287)	(0.334)	(0.381)	(0.490)
Toilet	0.086	0.098	-0.039	0.047	-0.026	0.012	-0.100	-0.131	0.030	-0.290
	(0.152)	(0.145)	(0.154)	(0.155)	(0.141)	(0.161)	(0.146)	(0.200)	(0.238)	(0.258)
Electricity	-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

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Table A.5 – continued from previous page

	Grade achieved before beginning school year 1998									
	0	1	2	3	4	5	6	7	8	9
	(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 machine	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)

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Table A.5 – continued from previous page

	Grade achieved before beginning school year 1998									
	0	1	2	3	4	5	6	7	8	9
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)
Children 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)
Children 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)
Children 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)
Children 16-20	-0.032	-0.079	0.017	0.109*	-0.023	0.075	0.065	0.067	0.141	0.058
	(0.061)	(0.061)	(0.060)	(0.061)	(0.064)	(0.061)	(0.055)	(0.067)	(0.120)	(0.119)
Women 20-39	-0.057	-0.013	-0.026	-0.297***	-0.129	-0.252**	-0.009	-0.267*	0.161	-0.342
	(0.113)	(0.101)	(0.107)	(0.112)	(0.112)	(0.103)	(0.089)	(0.154)	(0.165)	(0.225)
Women 40-59	-0.208*	0.029	0.038	-0.182	-0.098	-0.233	-0.147	-0.328*	0.337	-0.321
	(0.115)	(0.137)	(0.142)	(0.122)	(0.181)	(0.142)	(0.130)	(0.192)	(0.229)	(0.266)
Women 60	-0.125	-0.056	0.059	-0.206	-0.177	-0.087	-0.310*	-0.295	-0.166	0.091
	(0.139)	(0.166)	(0.148)	(0.143)	(0.168)	(0.171)	(0.170)	(0.253)	(0.242)	(0.316)
Men 20-39	-0.116	0.075	0.118	-0.091	0.018	-0.043	-0.164	-0.129	0.020	-0.029
	(0.106)	(0.109)	(0.112)	(0.111)	(0.114)	(0.106)	(0.103)	(0.143)	(0.198)	(0.187)
Men 40-59	0.123	0.312**	0.401***	0.143	0.341*	0.250	0.153	0.017	0.242	0.120
	(0.115)	(0.158)	(0.133)	(0.146)	(0.177)	(0.164)	(0.143)	(0.235)	(0.257)	(0.280)
Men 60	0.187	0.216	0.388*	0.196	0.214	0.378*	0.436**	0.533*	0.414	0.578
	(0.171)	(0.170)	(0.202)	(0.182)	(0.214)	(0.200)	(0.170)	(0.320)	(0.360)	(0.447)
House Charac m	-0.403	-0.071	-0.041	0.176	-0.385	0.348	-0.689*	2.163**	0.641	1.844**
	(0.450)	(0.401)	(0.444)	(0.522)	(0.440)	(0.567)	(0.378)	(1.070)	(0.556)	(0.825)
Assets m	0.152	0.180	0.194	0.281	0.188	0.132	0.151	-0.180	0.331	0.514
	(0.239)	(0.228)	(0.262)	(0.215)	(0.252)	(0.218)	(0.213)	(0.372)	(0.349)	(0.487)
Demographics m	-0.937	-1.378	-0.364	-1.486*	0.351	-0.607	-0.175	-3.009*	1.301	-5.886***

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Table A.5 – continued from previous page

	Grade achieved before beginning school year 1998									
	0	1	2	3	4	5	6	7	8	9
	(0.761)	(0.857)	(1.023)	(0.857)	(1.004)	(0.958)	(0.852)	(1.789)	(1.339)	(1.650)
Constant	1.093*	0.840	0.980	1.785***	1.815***	1.338**	1.140**	0.409	0.971	2.037*
	(0.594)	(0.599)	(0.599)	(0.578)	(0.622)	(0.669)	(0.577)	(0.874)	(1.013)	(1.089)
Observations	4569	3461	3422	3206	2846	2614	4065	1275	985	703
Number of clusters	150	148	148	148	148	147	150	134	127	119

*Notes: standard errors in parentheses are adjusted for clustering at the locality level and *** means that the corresponding coefficient is significantly different from zero at a 1% level of significance; ** at 5% and * at 10%.*