Local governance and efficiency of conditional cash transfers: Bolsa Escola in Brazil

by

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September 2007

Abstract

Conditional cash transfer programs, initially managed centrally, are increasingly relying on municipal roles for beneficiary selection and program implementation. We use a rigorous identification of program efficiency at the municipal level to establish the municipal correlates that matter for performance. Brazil's Bolsa Escola program offered transfers to poor mothers in exchange for regular school attendance, with municipalities in charge of beneficiary selection and the enforcement of conditionalities. On average, the program reduced the drop-out rate by 7.8% points (from a drop out rate of 17% without the program) but increased grade failure by 0.8% points (from a grade failure of 13% without the program), resulting in a net decline of 6.2% in grade retention. Performance, however, varied widely across municipalities. Municipal characteristics associated with greater program impact on reduced drop-out show the positive effects of more competitive democratic processes, political rewards for incumbent mayors, greater transparency in beneficiary selection, and stricter enforcement of conditionalities. Based on differential interpretation of rules across municipalities, we find that decentralized selection of beneficiaries fared no worse in program efficiency than centralized selection.

I. Introduction and overview

Many governments have actively pursued decentralization to locally elected governments in order to improve the provision of public services (Bardhan, 2002). The expectation is that information is available at the local level that does not reach central governments. Local governments can use this information to allocate or target public budgets more efficiently (Faguet, 2004). In addition, incentives to make more effective use of public

[*]We are indebted to Kathy Lindert, Bénédicte de la Brière, Donald Nelson, Lucia Chaves, Simone Cabral Marinho dos Santos, Irles Mayorga, and Julia Morim in helping us implement this research project, and to The World Bank, UNESCO, and DIFID for financial assistance in covering the cost of the survey.
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resources may be better aligned at the local level: local authorities can be held more accountable to intended beneficiaries as the latter have better information on local providers and more direct access to the instruments that enable them to reward and punish providers for their performance (Seabright, 1996). Local accountability can also be mobilized immediately through administrative mechanisms that involve stakeholder participation as well as through the less immediate channel of the electoral cycle for incumbent politicians or political parties (World Bank, 2003).

There are, however, drawbacks as local information remains imperfect and unequally accessible, and local accountability mechanisms are quite incomplete. As a result, the informational/incentive advantage can turn into an accountability disadvantage if local inequality is high and institutions for local accountability are weak. The net effect of centralization vs. decentralization in public service delivery remains controversial, with a huge deficit in solid empirical evidence. Alderman (2002) has for instance shown that more information is indeed available at the local level than at the centralized level and that it has been used by local authorities in Albania to better target the poor, particularly in poorer jurisdictions. Galasso and Ravallion (2005) find that local pro-poorness in the targeting of public food-for-school transfers in Bangladesh is greater with larger programs, poorer villages, less local inequality in asset ownership, greater proximity to cities, and less private transfers to the poor. Bardhan and Mookherjee (2006) analyze the decentralized targeting of credit and agricultural input kits in West Bengal villages, finding little evidence of elite capture, but biases in the targeting of infrastructure projects under employment generation programs. However, while evidence is becoming available on social biases in decentralized targeting, the jury is still out in identifying the local conditions that are associated with greater efficiency in decentralized service provision. This is because local level measures of program efficiency are difficult to identify. In this paper, we rigorously measure program efficiency at the municipal level and use it to establish correlates between efficiency and municipal features.

Whether programs should be managed in a centralized or decentralized fashion has also been an issue for the targeting and monitoring of conditional cash transfer (CCT) programs, where monthly transfers to poor mothers are conditional on regular school attendance by their children. The Mexican Oportunidades program, and the numerous other country programs derived from that experience, have used a centralized approach (Skoufias, 2005). By contrast, the Brazilian Bolsa Escola program was decentralized at the municipal level, entrusting the municipality with roles in the selection of beneficiaries and implementation of the transfers. Expectedly, efficiency in the municipal management of block grants to meet program objectives – reducing child drop out and grade failure rates – varies considerably across municipalities according to municipal conditions. Conditions that matter can be structural (size of the municipality, average income level, income inequality), programmatic (transparency in beneficiary selection, strict enforcement of conditionalities), and political (more competitive democratic processes, incumbency in local mayor elections). Identifying correlates between conditions and performance can provide important guidelines for program design.

To establish these correlates, we measure efficiency in CCT program implementation at the municipal level using an extensive data set combining administrative and survey data collected in 261 municipalities across the Northeast of Brazil. The analysis is based on administrative records for some 300,000 students from one or two schools in these municipalities over the period 1999 to 2003. The first two years are prior to initiation of the Bolsa Escola program and the last three during implementation. Records provide information on dropout and grade failure for each child, allowing to use child and year fixed effects in the identification of municipal performance in program implementation. A detailed municipal survey allows us to look at the role of municipal factors as they relate to differential levels of program impact, in particular municipal and mayor characteristics, and program implementation practices for beneficiary selection and the enforcement of conditionalities.

We find that Bolsa Escola had a strong impact in reducing child dropout during the school year, securing a 7.8 percentage points improvement in complete year attendance. It, however, did not decrease the grade failure rate, which in fact increased by 0.8 percentage point, expectedly because the transfers helped maintain at school children less able or less motivated in studying that might otherwise have dropped out. The net gain is a 6.2 percentage points decrease in grade retention which, compared to an average grade retention of 26.4% is an important achievement. We find that a number of municipal features and program implementation practices are strongly correlated to differences in levels of impact of the program on the drop out rate. We obtain in particular clear evidence that a more transparent beneficiary identification and selection process, and stricter enforcement of conditionalities are associated with higher impacts. We also find that expected electoral rewards for incumbent mayors are associated with larger impacts on the drop out rate. Because the program rules were somewhat confusing, some municipalities believed that the Federal Government in Brasilia was the one selecting beneficiaries. We see no difference in drop out rates between municipalities that followed a decentralized versus a centralized beneficiary selection.

The paper is organized as follows. In section II, we introduce features of the Bolsa Escola program and review results obtained in previous evaluations. In section III, we explain how data were collected on the school performance of children and on municipal characteristics. In Section IV, we give descriptive statistics on the drop out rate and the failure rate across municipalities. We explain the empirical methodology used for impact identification and measurement in section V. We then report in section VI results on the impact of Bolsa Escola on dropout rates and grade promotion, measured at the municipal level. In section VI, we characterize how differences in municipal characteristics and in program implementation procedures relate to differences in program efficiency measured by the program's impact on municipal dropout rates. We conclude in section VII.

II. The Bolsa Escola program and previous evaluations

Bolsa Escola was a conditional cash transfer (CCT) program that offered mothers in poor households a monthly stipend if their children ages 6 to 15 attended school on a regular basis. The program was implemented across all of Brazil between the years 2001 and 2003, until it was folded into the broader Bolsa Familia program (Lindert, Linder, Hobbs, and de la Brière, 2006).

Bolsa Escola was first implemented in the Federal District and extended to cities like Recife, before being scaled up into a national program. Two studies have analyzed these earlier forms of the program. For the Federal District, Abramovay et al. (1998) find that grade promotion rates were eight percentage points higher for beneficiary children than for children of non-beneficiary families. For the city of Recife, Aguiar and Araújo

(2002) find that drop out rates were 0.4 percent among beneficiaries in 1996 compared to 5.6 percent among non-beneficiary children, a gain of 5.2 percentage points. The studies are, however, not based on comparable treatment and control groups. In both cases, the control group is the population of children based on information from the School Census (World Bank, 2001). While results are of the same order of magnitude as those which we report here, these early programs were somewhat different from the federal program as transfers were higher and conditionalities weakly enforced. As far as we know, there are no studies that have evaluated the Federal program.

Using a simulation model based on observed child schooling responses to wage based on the PNAD data, Bourguignon, Ferreira, and Leite (2003) provide an ex-ante evaluation of Bolsa Ecola. Their findings suggest that over 50 percent of the children of poor households will respond to the incentives of the program. This implies halving the pre-program drop out rate, again a result not far from our own estimates

By contrast to these studies, the present impact evaluation study of Bolsa Escola is the first to use a rigorous identification of impact based on observed responses to the incentives provided by the CCT. We then go beyond impact characterization in relating municipal achievements to municipal features in order to understand what matters locally in relating to differential levels of program efficiency.

III. Survey design and implementation

Data collection took place between October and December of 2004 in 261 municipalities randomly selected across the states of Ceará, Pernambuco, Paraíba, and Rio Grande do Norte in the Brazilian Northeast. The municipalities of these four states were stratified according to their land inequality, size of public sector, and quota of program beneficiaries; and were randomly sampled from 8 strata. The sample was stratified to capture sufficient variation along variables that may be correlated with governance and importance of the program. Our sample is representative only for these four states and not necessarily for the Northeast as a whole. In each of the 261 municipalities analyzed, two data collection instruments were applied: (1) compilation of school records and (2) a municipal survey.

School records

To properly measure the effect of Bolsa Escola on school attendance and student achievement, we collected in each municipality children's school records for approximately 500 eligible children during the period 1999-2003. To gather these records, one or two schools were randomly drawn proportionately to the number of Bolsa Escola recipients (data which were obtained from the payments records of the Ministry of Education) within each selected municipality. Information on the grades, enrollment, and grade promotion for each child in the school was compiled. In total, we collected administrative records for approximately 293,800 children in primary and secondary school over five years, giving us some 624,059 complete data points.

Dropout and grade failure are recorded in the teachers' annual reports. In these reports, teachers provide the full list of students who started the year, and then indicate, by the end of the year, if a child has passed the grade, failed the grade, transferred to another school, dropped out of school, or died.

Although administrative records are presumably more accurate than self-reported information on attendance and grade promotion, not having conducted household interviews resulted in at least two shortcomings. First, we do not have information on children and household characteristics. And while the use of child fixed-effects eliminates any biases associated with our inability to control for time invariant characteristic of the child and his family, it does prevent us from exploring how the impacts vary according to these characteristics. Moreover, we cannot investigate whether the program was targeted according to certain observable characteristics of children, other than their prior attendance or achievement status. Secondly, we cannot follow children who transfer out of the school. However, we can observe if the child transferred to another school (as opposed to being reported as missing school), and we fortunately observe that less than four percent of the children had transferred. In the analysis that follows, we simply remove these children from the sample.

Municipal survey

The municipal survey consisted of several parts designed to gather general information on municipal governance, public administration, and implementation of the

Bolsa Escola program. Respondents on the various sections of the questionnaire were mostly public administrators, but also included politicians and key members of civil society, such as the local priest or president of the labor union.

For questions on Bolsa Escola, we interviewed the respective program coordinator about how the municipality identified and selected beneficiaries, and imposed and monitored the conditionalities. We also gathered information to assess how transparent the program was in its implementation.

In sum, we assembled a unique database comprised of municipal information on 261 municipalities and comprehensive school records for over 293,800 eligible children spanning the years 1999-2003.

IV. School records information on drop out and failure rates

In this section, we provide some basic descriptive statistics on the administrative data that were collected. Absent standardized test scores, the focus of this study is on dropout rates and failure rates.

Dropout rates

Figure 1 depicts the distribution of dropout rates across municipalities, by beneficiary status and year. The plots in the first row show the dropout rates among children who did not receive Bolsa Escola, and the plots in the second row are the dropout rates among children who received Bolsa Escola. The last row computes the difference in drop out rates between program recipients and non-recipients.

Figure 1 suggests that municipalities on average targeted the program to children who were less likely to drop out of school based on their pre-program performance. Before the start the program in 1999-2000, dropout rates among beneficiary children are on average only 4.5 percent compared to 17 percent among children who did not participate in the program (see Appendix Table). In fact, 95 percent of the municipalities have lower dropout rates among program recipients than non-program recipients during both the treatment years (2001-2003) and the pretreatment years (1999-2000), and only 13 percent of the municipalities had differences in pre-program dropout rates between treatment and control groups that were not statistically different from one another.

Failure rates

Figure 2 presents a similar set of distributions as those depicted in Figure 1, but for failure rates. These distributions are based on the sample of children that did not dropout of school, since this would automatically result in a failure.

Unlike with dropout rates, failure rates are higher among beneficiaries (14 percent) than among non-beneficiaries (12.1 percent) (see Appendix Table). However, while significant, this difference is small. In fact, whereas pre-program dropout rates were similar between treatment and control groups in only 13 percent of the municipalities, pre-program failure rates are similar in over 67 percent of the municipalities. This figure suggests that, conditional on attending school, the program was targeted toward children with slightly higher failure rates.

Implications for targeting

We conclude this inspection of descriptive statistics by noting that the program was not targeted toward the more problematic children in terms of prior histories of dropping out. To the contrary, the program was targeted at the better performing students in terms of propensity of dropping out. This has two implications for the subsequent analysis. One is that we need to proceed by double difference as opposed to a simple difference analysis as there was clear selection between treated and non-treated groups. This is precisely what our panel data with pre- and post-program observations allow us to do. The other is that the magnitude of program impact is expectedly diminished by positive selection, since the program differentially targeted children that were already more likely to meet the enrollment continuity objective. This is different from targeting CCT for maximum effect of the conditionality as has been explored with the Progresa experience (de Janvry and Sadoulet, 2006).

V. Empirical Methodology

We analyze two aspects of the Bolsa Escola program. Our first objective is to measure the impact of Bolsa Escola on two dimensions of educational achievement – dropout and grade retention rates – by municipality. Given the non-experimental design of the program, credible estimates of the program's impact will depend on the internal validity of the research design. In this section, we propose an identification strategy to measure the effects of Bolsa Escola. This method relies on the analysis of schools' administrative records, which provide panel data on children before and after the start of the program to estimate the program's impact for each municipality.

Having estimated the impact of the program by municipality, our second objective is to measure how municipal-level characteristics and municipality behavior toward program implementation relate to the program's impact. Because the municipality is responsible for selecting program beneficiaries, the program becomes an instrument to maximize the municipality's own socio-political objectives, which could lead to substantial variation in the program's impact across municipalities. In this second stage, we will consequently associate municipal-level characteristics and behavior – such as poverty and inequality levels, quality of governance, administrative structure, transparency of the selection process, and electoral cycle – with the impact of the program.

First-stage Estimation

The empirical approach used to estimate the impact of Bolsa Escola on schooling outcomes (dropout and grade retention rates) can be formalized in the following regression model. For an individual child *i* in municipality *j*, let S_{ijt}^{0} denote a schooling outcome in period *t* without a program (superscript 0). The individual *i* is observed in time period *t* and has characteristics that make him belong to a group $B_{ij} \in \{0,1\}$, where a one denotes being selected as a beneficiary of the Bolsa Escola program. Our universe consists of all the children that were deemed eligible according to the general criteria of the program. Eligibility characteristics are children ages 6-15 years old, currently enrolled in primary or secondary school, and whose family's monthly income is no more than Reais$90 per capita. Among eligible children, the municipality selected which children will receive the transfer, given its fixed allotment of stipends. The children who were eligible for the program but were not selected by the municipality serve as our control group. In the absence of the Bolsa Escola program, the schooling outcome for child i, is

$$S^0_{ijt} = \alpha + \beta_t + \eta B_{ij} + \varepsilon_{ijt}\,, \qquad (1)$$

where β_t represents a time fixed effect, η is the effect of program selection as a beneficiary, and ε_{ijt} is a random error term.

With the program (superscript 1), the schooling outcome becomes:

$$S^1_{ijt} = \alpha + \beta_t + \eta B_{ij} + \theta_{jt} + \varepsilon_{ijt}\,,$$

where θ_{jt} measures the program impact on beneficiary children in municipality j at time t.

Combining these two expressions gives schooling outcomes as:

$$\begin{aligned} S_{ijt} &= P_{ijt}S^1_{ijt} + (1 - P_{ijt})S^0_{ijt} \\ &= \alpha + \beta_t + \eta B_{ij} + \theta_{jt}P_{ijt} + \varepsilon_{ijt}, \end{aligned} \qquad (2)$$

where $P_{ijt} = B_{ij}T_t$ is an indicator for if the child did in fact participate in the program, $T_t = 1$ for $t \geq t_0$, and t_0 is the date of introduction of the program.[1] The last equality follows from the common assumption that the effect of the program $\theta_{jt} = S^1_{ijt} - S^0_{ijt}$ is constant across individuals, measuring the average municipal program impact. Note that with repeated observations per child, we can generalize this model to allow for individual-specific fixed effects that are potentially correlated with whether the child participates in the Bolsa Escola program or not. The equation to be estimated thus becomes:

$$S_{ijt} = \alpha + \beta_t + \phi_{ij} + \theta_{jt}P_{ijt} + \varepsilon_{ijt}, \qquad (2')$$

where ϕ_{ij} is a child fixed effect.

It is important to emphasize that the parameter θ_{jt} is identified by comparing changes in the schooling outcomes of beneficiaries to non-beneficiaries over time, among the children that were all eligible. With this approach, we estimate not only an impact for each municipality but also by year. Another key aspect of this approach is the fact that we

[1] There is virtually no distinction between participating in the program and being offered the program because take-up rates are 100 percent.

now have repeated observations on a single child. This will allow us to control for the non-random selection of children who received the program (as confirmed by inspection of the descriptive statistics), which may otherwise bias the treatment effect parameter. This method requires a fairly reasonable counterfactual assumption, namely $E[\varepsilon_{ijt} \mid P_{ijt}] = 0$, i.e., that in the absence of the program differences in schooling outcomes over time would be the same for those who have received a transfer and those who qualified for the program but did not receive one. Employing individual fixed-effects increases the plausibility of this assumption.

Second-stage estimation

Given the decentralized design of the program, one can expect considerable variation in the impact of the program across municipalities. To understand features of contexts under which the program performs more efficiently, the second stage of the analysis will consist in associating various municipal-level characteristics z_j to the program's impact. Our empirical strategy can be formalized as follows. Assuming that μ_j is a random disturbance term, we can estimate the following model:

$$\bar{\theta}_j = \lambda + \delta z_j + \mu_j, \qquad (3)$$

where $\bar{\theta}_j$ is the average impact of the program in municipality *j*. The coefficients δ indicate how municipal characteristics relate to the impact of the program. These characteristics may include measures such as per capita income, human development, income inequality, the degree of electoral competition, and characteristics of the mayor and the municipal administration. Contrary to equations 2 and 2', these equations (3) are not causal: they measure partial correlations between program impact and municipal features.

VI. The impact of Bosla Escola on dropout rates and grade promotion

The average impact of Bolsa Escola on dropout rates

Table 1 presents regression results from estimating several variants to the difference-in-difference model in equations 2 and 2'. The dependent variable in each of these regressions is a binary variable, which assumes a value of one if the child drops out of school during the school year. Column (1) presents the basic OLS estimates of the impact of Bolsa Escola on dropout rates, without controlling for individual fixed effects (equation 2). A causal interpretation of these estimates would suggest that Bolsa Escola actually *increased* dropout among beneficiary children by 1.7 percentage points. However, given the 12.6 percentage difference in pretreatment dropout behavior between treatment and control children, there are several reasons why this naïve estimate might be biased. If, for instance, program officials targeted the program to children relatively more at risk of dropping out of school, then this would make the measurement less negative or even positive. Similarly, if the program rewarded children who were already attending school (for having received some positive shock), then reversion to the mean would suggest a less negative or even a positive impact.

Column (2) presents the estimate of the treatment effect, after controlling for the child's initial dropout status the year before introduction of the Bolsa Escola program. The estimated treatment effect suggests in fact that Bolsa Escola did *reduce* dropout rates by at least 3.4 percentage points. Column (3) presents estimates of the treatment effect that includes individual fixed-effects (equation 2'). This model not only controls for the pre-treatment dropout status of the child (thus incorporating the previous specification), but also for individual characteristics that are time invariant such as gender, and certain household and community characteristics. Under this specification, Bolsa Escola reduced dropout rates among beneficiary children by 5.6 percentage points.

Our most general specification is presented in column (4). This specification extends the fixed-effects model to allow for children with different pre-treatment dropout status to have different year effects. This accounts for a possibly fundamentally different pattern of behavior over time for these two types of children. Identification is then based upon comparison of the change in dropout level between the treated children and their counterparts of the same type. The estimate presented in this model suggests that Bolsa Escola reduced dropout rates by 7.8 percentage points.

Even though the model presented in column (4) is quite flexible, the substantial difference in pretreatment dropout status between beneficiaries and non-beneficiaries is still cause for concern. One possible robustness test is to estimate the impact of the program only among the 13 percent of the municipalities where the pre-treatment dropout status was not statistically different between program beneficiaries and non-beneficiaries. Columns (5) and (6) present the estimation results for this restricted sample of municipalities. The estimates for both the OLS model and the fixed-effects model are highly consistent with those presented in column (4).

Table 2 explores whether the program had a differential impact according to the type of class, the grade level, and over time. As seen in columns 1 to 3, the program had a larger impact on beneficiary children enrolled during the night classes – classes which tend to be for older and less able children –, reducing their dropout rates by 13.3 percentage points. Program impacts were equally large both when comparing primary versus secondary school children, and over the years of program implementation. The difference in the program's impact between 2001 and 2002 is only 1.2 percentage points and 0.5 percentage points for 2002 and 2003.

The average impact of Bolsa Escola on grade failure rates

Table 3 reports the estimates of the impact of Bolsa Escola on the likelihood that a child fails the grade. The estimates reported are based on specifications similar to those presented in Table 2. The estimation sample has been restricted to the set of children that have not dropped out (since this is an automatic failure). The results should thus properly be read as probability of failure, conditional on not having dropped out of school. Because the sample of children that are in school is itself affected by the program, one should be careful in interpreting the impact of Bolsa Escola on the failure rate. It combines the direct effect of Bolsa Escola on the probability that a particular child would fail his grade and the indirect effect through the composition of the children population that does not drop out. Hence, even in absence of any direct effect, one could observe an impact on the average failure rate, strictly from the compositional effect.

The results presented in Table 3 suggest that Bolsa Escola may have actually increased the failure rate. The estimates for our most general specification (see column 4) indicate that Bolsa Escola increased the likelihood of failing a grade by 0.8 percentage points, which represents 5.7 percent over the average value of 13.1 percentage points.

Table 4 examines how the effect of Bolsa Escola on failure rates differs according to the type of class, grade level, and over time. This perverse effect of Bolsa Escola appears to be the result of beneficiaries in the night class. Although the estimate is not statistically significant, the magnitude is relatively large. And as seen in columns 4 and 5, most of the effect is concentrated in the primary school, and in the second and third years of the program.

This result on failure rates is perhaps not surprising of the attendance-achievement trade-off that can be induced by a CCT program. The CCT effect induced parents to keep at school children that would otherwise have dropped out. The increase in grade failure is consistent with the idea that Bolsa Escola may have brought in or maintained at school children with lower academic aptitude or that were less motivated to succeed that those who stayed in school without a transfer.

The overall impact of Bolsa Escola on grade promotion

Ultimately, the objective of the program is to increase the human capital of the beneficiary population. Thus, the success of the program depends not only on keeping children in school but also on insuring that they successfully pass their grade. In Table 5, we analyze the impact of the Bolsa Escola program on reducing grade retention. This is done with a reduced-form regression of the overall grade retention, which is defined as either dropping out or failing the grade for those that did not drop out. We find (column 4) that this joint effect is a decrease of 6 percentage points in the grade retention rate, compared to the average value of 23 percentage points. While this is a substantial achievement, there is still a long way to go to a well-performing school system. The increase in failure rate is thus responsible for a loss of 1.6 of the 7.8 percent gain in keeping children in school.

A quick calculation using pre-program drop out and failure rates in the Appendix Table, confirms this large gain in grade promotion. Without the program the combination of an attendance rate of 83% (dropout rate of 17%; see Appendix Table, average for 1999 and 2000) and a conditional success rate of 88% (failure rate of 12%; see Appendix Table) gave a grade promotion rate of 73%. With the program, the attendance rate is higher, rising by 7.8 percentage points to 90.8%, but the conditional success rate is slightly lower, at 87.2%, leading to a grade promotion rate of 79.2%. This is the 6 percentage points increase estimated in the combined regression reported in Table 5.

Results can be summarized in the following Figure 3 where we start from a pre-program group of 100 eligible children. In the end, the effect of the program on attendance is 7.8% while the net effect of the program on grade promotion is 6.2%.

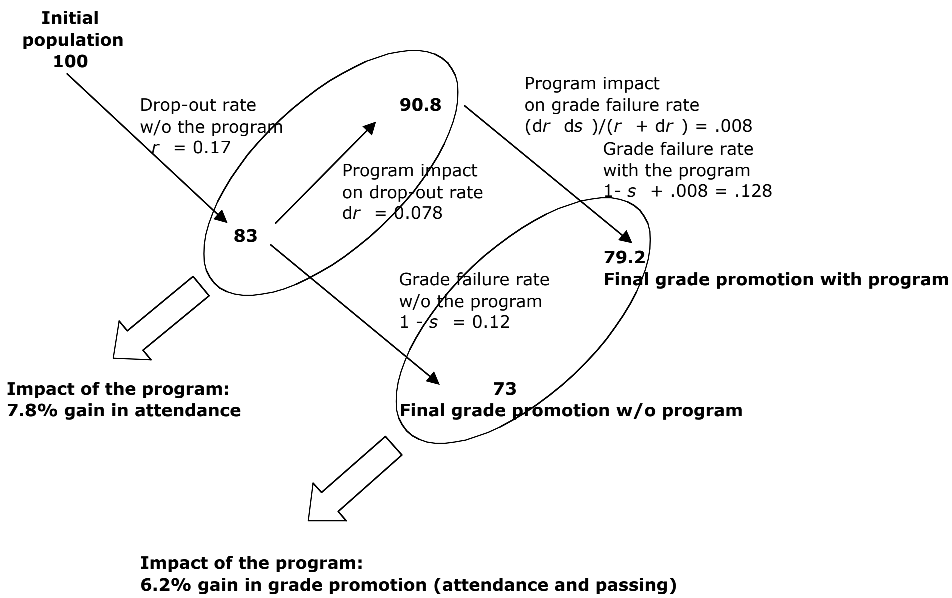

Figure 3. Impact of the program on school attendance and grade promotion

A hypothetical but interesting exercise consists in computing what the failure rate of the children that are kept in school by the program would have to be if the program had no direct effect on the failure rate of the other children. Let r denote the attendance rate without the program. We assume that these children have a conditional success rate s, independent of whether there is or not a program. The program increases the attendance rate by dr, with children that we assume have a success rate of $s - ds$. The average success rate with the program is thus:

$$\frac{rs + dr(s - ds)}{r + dr} = s - \frac{drds}{r + dr}.$$

We estimated the impact on the attendance rate to be $dr = .078$ (Table 2, col. 4) and on the conditional failure rate to be $\frac{drds}{r + dr} = .008$ (Table 3, col. 4). Hence, for the children that are kept in school by the program, the differential failure rate is:

$$ds = .008\frac{r + dr}{dr} = 0.094.$$

This means that, if the failure rate in the student population that comes to school even without the program remained at the pre-program value of 13%, it is as high as 22.4% for the children that are kept in school by the program. If, however, the presence of these children implies a worsening of school conditions for all and an increase in failure rate of the other children, then the difference between the two groups of children could be lower. By the same token, if the Bolsa Escola conditionality on attendance improves the passing rate of the children that would go to school (but possibly with less regularity) without program, then it implies that the failing rate of the children retained in school by the program would be even higher. In any case, this suggests a need for complementary actions that would ensure a successful grade passing performance for the children that are maintained in school by the program.

The distribution of impacts of Bolsa Escola on dropout and failure rates

As discussed above, the Bolsa Escola program reduced dropout rates by 7.8 percentage points on average. However, given the program's decentralized design there is expectedly considerable variation in the program's impact across municipalities. Fortunately, our survey design allows us to estimate an impact of the program on both dropout and failure rates for each municipality in the sample. Figure 4 presents the distribution of estimated impacts of the program on dropout rates using the econometric specification presented in column 4 of Table 1. The distribution of impacts is skewed towards negative values with a median impact of 6.7 percentage points. While the impacts range from -25.5 to 10.7 percentage points, over 95 percent of the impacts are in fact negative and few positive impacts are measured precisely.[2] In addition to the distribution of unbiased estimates of the impact, Figure 4 also plots the absolute value of corresponding t-statistic. Over 55 percent of the estimates are significantly negative at a 95 percent confidence level and 65 percent at a 90 percent confidence level.

Figure 5 presents the distribution of impacts on failure rates using the econometric specification in column 4 of Table 3. The distribution is only slightly skewed towards the positive values, but otherwise fairly symmetric. The median impact is –0.2 percentage

[2] As can be seen in Figure 4, only one is measured at 90 percent confidence level and the rest have an average t-statistics of 0.80.

points, and the range of the impacts goes from –14.4 to 20.5 percentage points. Unlike the previous distribution, only 10 percent of these impacts are significantly different from zero, and they lie mostly at the positive tail of the distribution.

VII. Are the program's impact on dropout rates related to municipal characteristics and to municipal program implementation procedures?

There are at least three reasons why the effects of the program might vary across municipalities (Hotz, Imbens, and Mortimer, 2005). First, the distribution of municipal and mayor characteristics associated with program efficacy may differ across municipalities. For example, if children with more highly educated parents respond better to the program than children with less educated parents, then a municipality with a larger proportion of school-aged children with higher educated parents may achieve a greater impact. Thus, even if each municipality targeted and implemented the program in the same manner, differences in contextual factors would create differential effects of the program.

Second, even if municipal and mayor characteristics were identical across municipalities (or we could condition perfectly on these differences in contextual variables), municipalities may want to target different subpopulations according to their own socio-economic and political objectives or constraints, thus creating differential effects. How a municipality identifies and selects beneficiaries will ultimately affect which subgroups receive treatment and consequently the impact of the program.

Third, the municipalities, while offering the same treatment, may differ in the manner in which they operate the program. Because Bolsa Escola is a conditional cash transfer program, differences in how a municipality monitors and enforces the program requirements may have important consequences for the effects of the program.

In this section, we examine how differences in the way a municipality identifies and selects it beneficiaries, and enforces and monitors the program requirements, are correlated with the program's effect on reducing dropout rates. However, before doing so, we investigate in Table 6 the extent to which contextual characteristics of a municipality are associated with program impacts.

Table 6 presents the estimation results of equation 3 that regresses Bolsa Escola's impact in a particular municipality on various characteristics of the municipality and the mayor. Column 1 presents the base specification, which includes the basic socio-economic characteristics of the municipality such as population density, share of rural households, share of literate population, per capita income, and income inequality. Various sets of mayor characteristics and other municipal characteristics are sequentially added in each column, such that column 5 presents the most general specification which includes all the municipal and mayor characteristics, as well as state fixed-effects.

Despite the variation in impacts across municipalities, the base specification explains only 9 percent of the variation. Population density is a consistently strong predictor of impact. Per capita income also appears to be a strong predictor of positive impact, but much of this variation is driven by state-level variation. Controlling for state fixed-effects, the negative correlation between income and impacts diminishes substantially and become no longer statistically significant.

The gender of the mayor and the mayor's term are robust to the inclusion of state fixed effects. The program performed 3.2 percentage points better in municipalities with a male mayor, but 2.2 percentage points worst among second-term mayors, both of which are large effects. These second term mayors cannot be re-elected, and electoral rewards are consequently not operative on them, possibly explaining this deterioration in performance. First term mayors are conversely associated with a better program impact performance, lending support to the effectiveness of a long route of social accountability via electoral rewards (World Bank, 2003). The role of political economy forces in influencing program impact is also reflected in the share of public employees related to the mayor and the share of the municipal secretariat that are politicians as opposed to technicians, both associated with a worst performance. By contrast, a larger municipal quota of Bolsas as a share of all children enrolled in primary and secondary school improves performance, suggesting that lesser room for clientelistic rent allocation favors performance. These results support the proposition that more open and competitive local democratic practices and lesser room for clientelistic behavior improve performance in the decentralized implementation of transfer programs.

Beneficiary identification

Public administrators (in 82% of the municipalities) and school teachers (in 70%) were mostly responsible for identifying the beneficiary population. Health agents and members of civil society – such as NGOs, municipal councils, and civil society volunteers – played a much more limited role, participating in identification in only 32 and 11 percent of the municipalities, respectively.

Most of the registration process took place in public locations, with either schools (85 percent) or the mayor's office (55 percent) providing the natural setting. Only 28 percent of all municipalities in the sample registered individuals at their home, and among these municipalities, the median percentage of households interviewed was 20 percent. Moreover, only approximately 38 percent of the municipalities used some form of geographical targeting to decide in which areas to begin the registration. Among those municipalities that did prioritize specific areas, 62 percent targeted the poorest neighborhoods, although other considerations that mattered included the number of schools in an area, ease of access to the region, and the rural nature of a community.

Schools also performed the function of announcing the program to the community, with 94 percent of the municipalities using schools to notify individuals about the program. Schools, however, were not the only source of information about the program, as it was also advertised on the radio (66 percent) and in public announcements (53 percent).

In Table 7, we investigate the extent to which these different approaches to registering program beneficiaries are correlated to program's impact on dropout rates. The estimation results presented in each column are slight variants of the model specified in equation 3, and each specification controls for the full set of mayor and municipal characteristics introduced in Table 6, along with state fixed-effects.

Column 1 presents the association between who registered the pool of eligible children and program impacts. The use of teachers, health agents, or members of civil society are not correlated with program impacts, but municipalities that relied on public administrators from the mayor's office to register its beneficiary population did experience a sizeable reduction in program performance. The use of public administrators is associated with a 2.3 percentage point increase in program impact, which implies a 33 percent reduction in the program's impact on dropout rates. Registration by members of the mayor's office as opposed to teachers, health agents, and civilian volunteers can be associated with less transparency in enlisting beneficiaries, and hence in a lower level of program impact on school attendance.

While the use of teachers may not have affected the effectiveness of the program, registering the beneficiaries at the school did have a beneficial effect, correlating with a 2.1 percent points higher impact (column 2). Municipalities that registered beneficiaries in the communities also had a slightly larger impact of 1.2 percentage points. We find no evidence that the municipalities that targeted the registration process geographically or conducted home visits improved the program's effectiveness (column 3). However, the program's impact on dropout rates is 1.5 percentage points better in municipalities that verified the information provided in the registration process.

Column 4 investigates whether differences in how beneficiaries were notified about the registration process is related to the program's effect on dropout rates. With 93% of the municipalities notifying parents through the schools, it appears that the registration process was well advertised, which would explain why the different approaches to announcing the registration process is not associated with program impacts. Column 5 presents a regression where these different decisions are estimated jointly, and we find that the results are robust.

Beneficiary selection

With introduction of a new federal program, there was considerable confusion in municipalities about their role in the selection process. Sixty-three percent of the municipalities believed that their role was to identify eligible children and that the Federal Program in Brasilia would select beneficiaries from the list of reported children. Twenty three of 63 prioritized the list before sending it in, which means that they might de-facto have selected beneficiaries if the Federal Program proceeded down the list. However, even after accounting for this, at least 40 percent of the municipalities did not knowingly select their program recipients. Whether this misunderstanding of the selection process is correlated with program impacts is investigated in Figure 6.

The figure depicts the distribution of impacts on drop out rates, by whether or not the municipalities selected their beneficiaries. Compared to the distribution of impacts for municipalities that did select their beneficiaries, the distribution of impacts for municipalities that did not selected their beneficiaries is shifted slightly to the left, but with a fatter tail on positive impacts. When the comparison is made in a regression framework to control for mayor and municipal characteristics, we find (result not reported) that on average there is only a 0.4 percentage point difference, and that it is not statistically different from zero.

That the program's impact was not different with decentralized vs. centralized beneficiary selection suggests that decentralized targeting is not less effective than centralized targeting. The advantages and disadvantages of decentralized targeting relative to centralized targeting (i.e., better information but greater risk of capture) may be in this case canceling out.

Monitoring and enforcement of conditionalities

Participants to the Bolsa Escola program received monthly payments conditional on attending at least 85 percent of their classes. When asked whether this condition was enforced, 99% of the municipal program coordinators answered positively. However, how the condition was enforced varied widely across municipalities.

This is because they have little incentive to monitor participation requirements in a strict manner or are using the transfers to achieve other goals than educational gains for the children of recipient households. When asked what consequences students would incur for not complying with the school attendance requirement for 3 consecutive months, 71 percent of the municipalities responded that nothing would happen if the family could justify the absences. In 72 percent of the municipalities, program coordinators indicated that the household would have their payments suspended for an average of 2 months, but only 27 percent claimed that the child would be cut from the program.

Table 8 examines whether the way municipalities dealt with non-compliance is associated with program impacts. When we examine the types of penalties that the municipality imposes we do see that those who do enforce non-attendance by loss of the

CCT have better impacts, suggesting that the conditionality may have an important role in the impact. Hence, in the debate on whether to condition transfers or not in securing larger program impacts on child human capital (Schady and Araujo, 2006), results obtained here support the proposition that threat of loss of benefits can indeed be effective in enhancing program impacts. de Braw and Hoddinott (2007) find similarly that conditioning the Progresa/Oportunidades transfers in Mexico improved school enrollment, particularly in the transition between primary and secondary school when most of the dropout occurs.

VIII. Conclusions

Bolsa Escola was one of the largest conditional cash transfer (CCT) programs in the world, only second in rank to Mexico's Oportunidades program (Rawlings and Rubio, 2005). It had the unique characteristic of being decentralized at the municipal level, with local authorities in charge of beneficiary identification and selection and of program implementation. Yet, while many impact evaluations have been done for Oportunidades and other centralized programs, no similar evaluation is available for Bolsa Escola and other decentralized programs, in particular to find out what makes a difference at the municipality level for program efficiency. This paper uses a large survey conducted in 261 municipalities of the Northeast and school records collected for 293,500 children over a five year period bracketing introduction of the Bolsa Escola program to provide a rigorous impact evaluation of the program. Specifically, we analyze, in a first stage, the impact of the transfers on school drop out, grade failures, and grade retention at the child level. In a second stage, we analyze how municipal characteristics and municipal performance in program implementation are associated with the level of program impact in each municipality.

Results show that the program was not targeted at children with lower pre-program performance in school attendance. The possibility of better targeting the CCT on children at risk of dropping out of school thus may leave room for increasing the impact of the program on schooling. In spite of this, we find that the CCT had a very strong impact on school attendance, inducing a 7.8 percentage points decline in drop out. This impact was equally large in primary as in secondary school, and across the three years of program implementation, and was larger in the more difficult night shift, typically with older and less able students, than in the morning and afternoon classes. Not surprisingly, we find that the program worsened the grade failure rate, which increased by 0.8 percentage point. This effect was larger in primary school and in the last two years of program implementation. This can be attributed to the fact that the cash incentive helped retain at school less able and motivated children that would otherwise have dropped out. The large positive school attendance effect was thus accompanied by a small negative grade failure effect. The net effect of the program in reducing drop out or grade failure is an important 6 percentage points gain in grade promotion. While a sign of success, this achievement still leaves 21% of the children selected for Bolsa Escola either dropping out of school or failing the grade. Hence, in spite of this success, room for improvement remains, in particular on the supply side of schooling in helping retain more children and helping those who were retained by the transfer be more successful in passing the grade.

Partial correlations between municipal features and magnitude of program impact on drop out rates are revealing of municipal characteristics and management practices that favor a larger impact. Most importantly, we repeatedly found that features that favor transparency in program implementation (procedures followed for the identification of beneficiaries) are associated with larger program impact. This includes the registration of beneficiaries by teachers, health agents, and civilian volunteers as opposed to by members of the mayor's staff, registration in more public places such as the school, and verification of the information provided in the registration process. We also found that enforcement of the school attendance conditionality via threat of loss of benefits is strongly associated with a larger impact on continuity of enrollment. Conditioning the cash transfers thus appears to be efficient for child schooling. Finally, we observed that electoral rewards are effective for social accountability as first term mayors have a superior performance with the program compared to second term mayors that cannot run for re-election. More open and competitive municipal democratic practices, with less room for clientelistic allocation of rents, also relate to better program impact. Decentralized selection of beneficiaries did not lead to program impact any different than centralized selection, but did not perform worst either, suggesting a wash between the information gains and the costs of capture typically associated with decentralization.

These results on early steps toward decentralized implementation of CCT programs are encouraging. They show positive impact on the main goal pursued by the CCT, namely greater continuity in school attendance. They suggest the importance of conditioning transfers in achieving impact. And they also suggest the key role of municipal features such as transparency and democratic rewards in enhancing the magnitude of impacts. They, however, also show that progress remains to be made in targeting transfers for maximum impact of the conditionality, and in capitalizing on the potential gains from decentralization for greater program efficiency.

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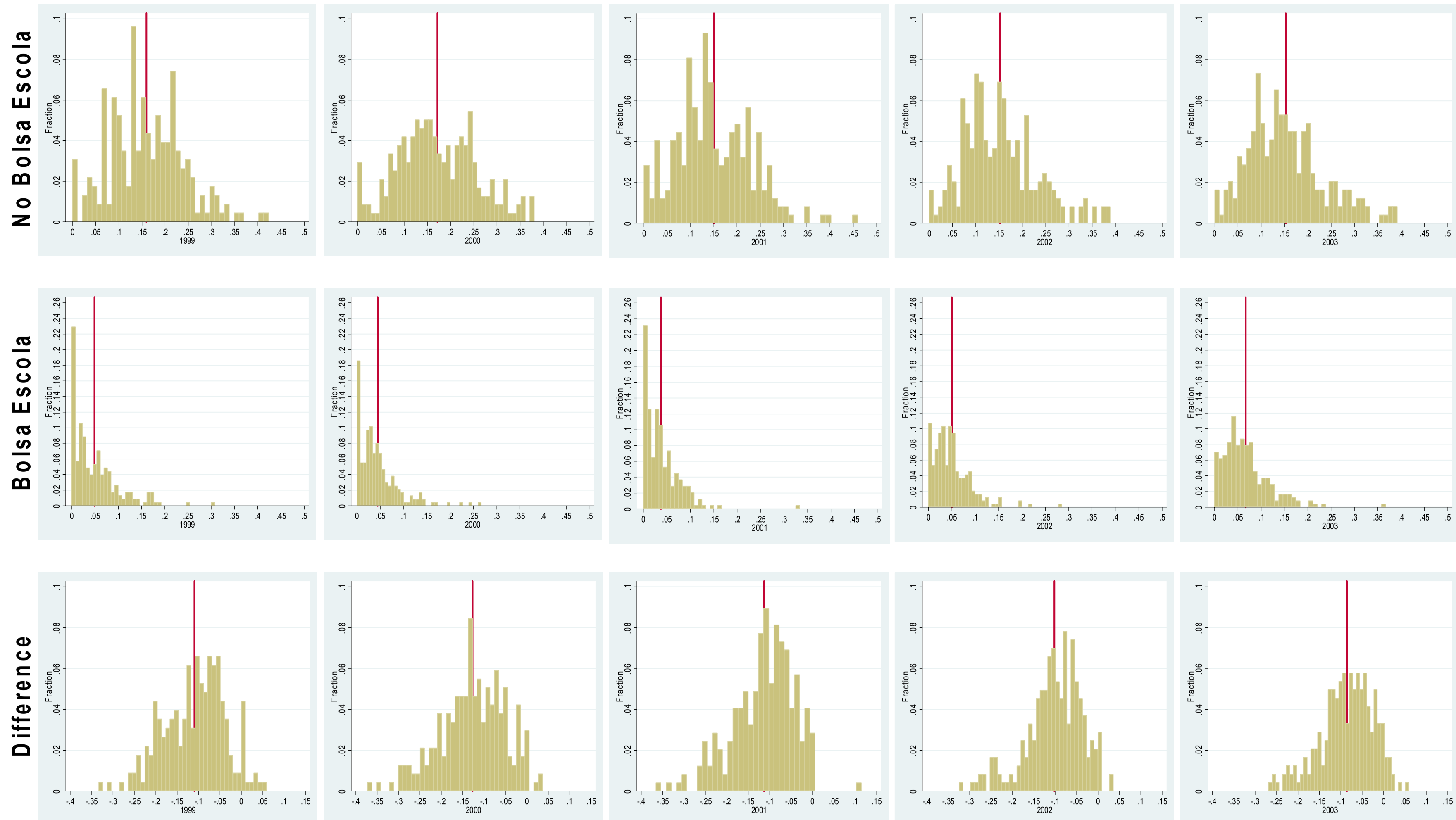

Figure 1: Distribution of dropout rates across municipalities by beneficiary status and year

1999-2000: Before Bolsa Escola. 2001-03: During Bolsa Escola. Vertical line is the average across municipalities (see Appendix Table)

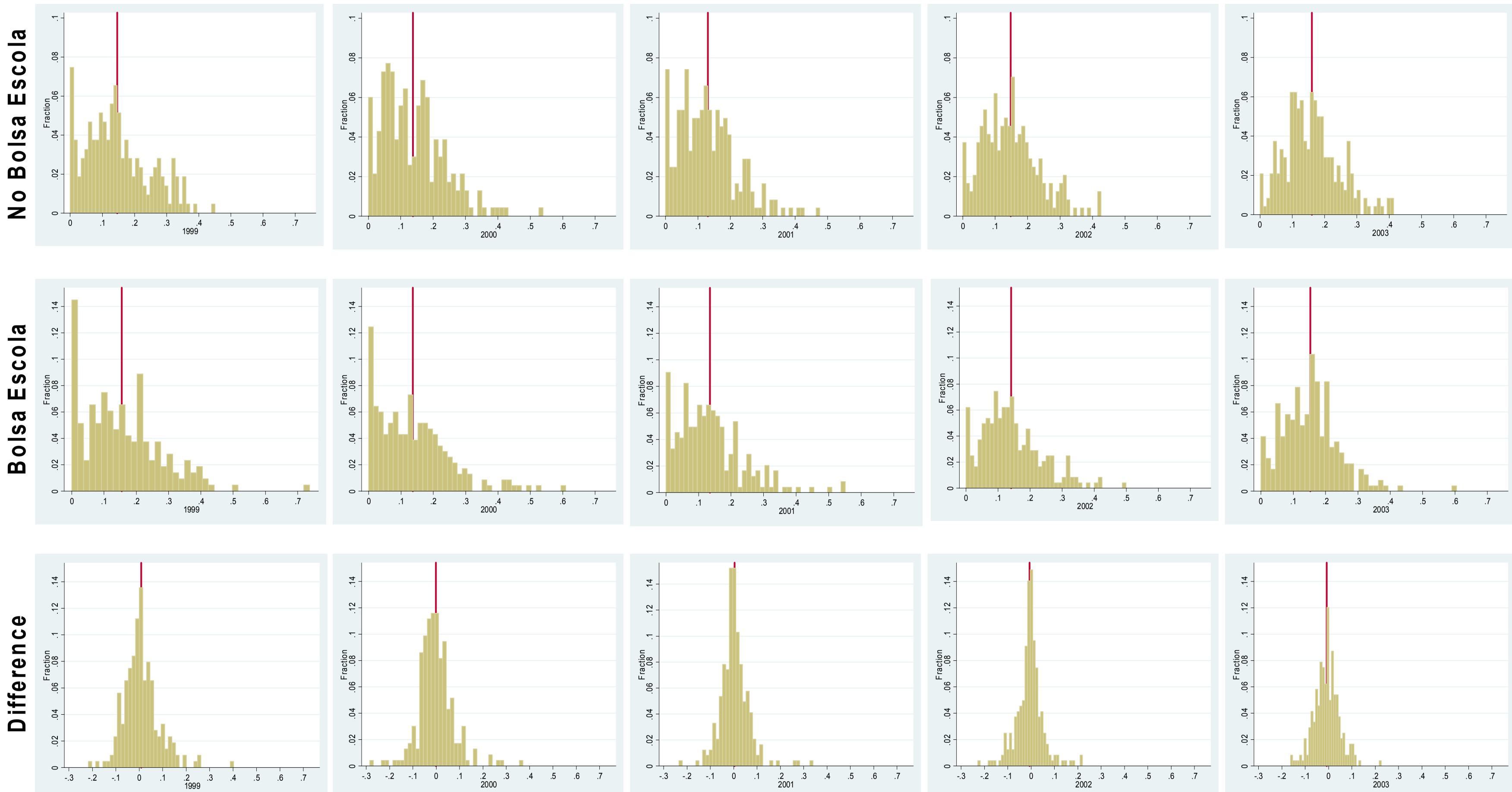

Figure 2: Distribution of failure rates across municipalities by beneficiary status and year

1999-2000: before Bolsa Escola; 2001-03: during Bolsa Escola

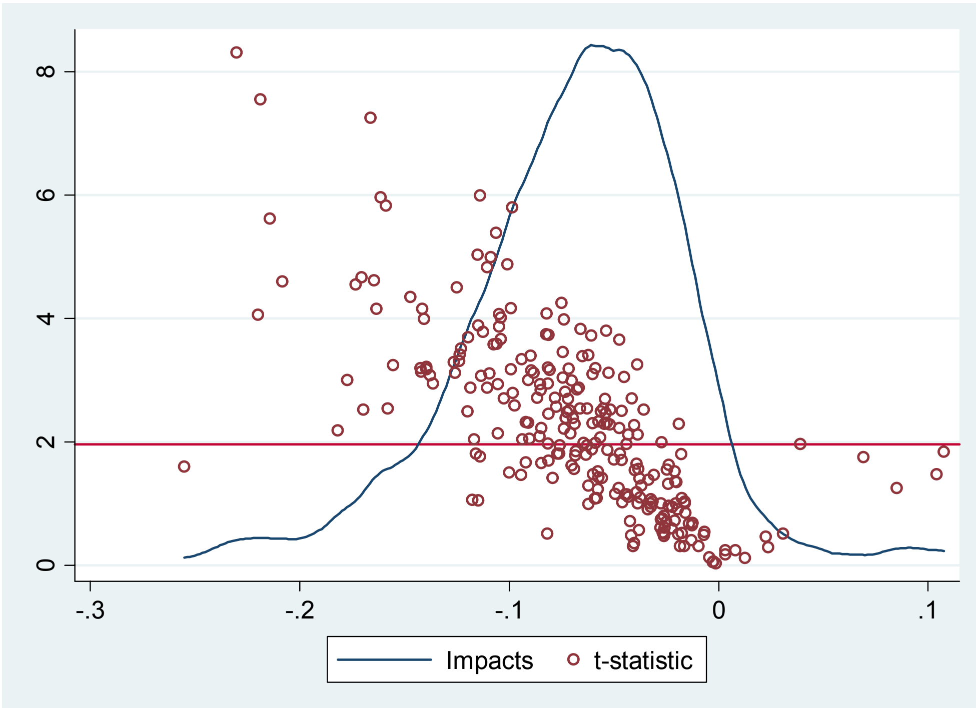

Figure 4: Distribution of estimated impacts of the program on dropout rates by municipality

Horizontal line = 90% confidence level

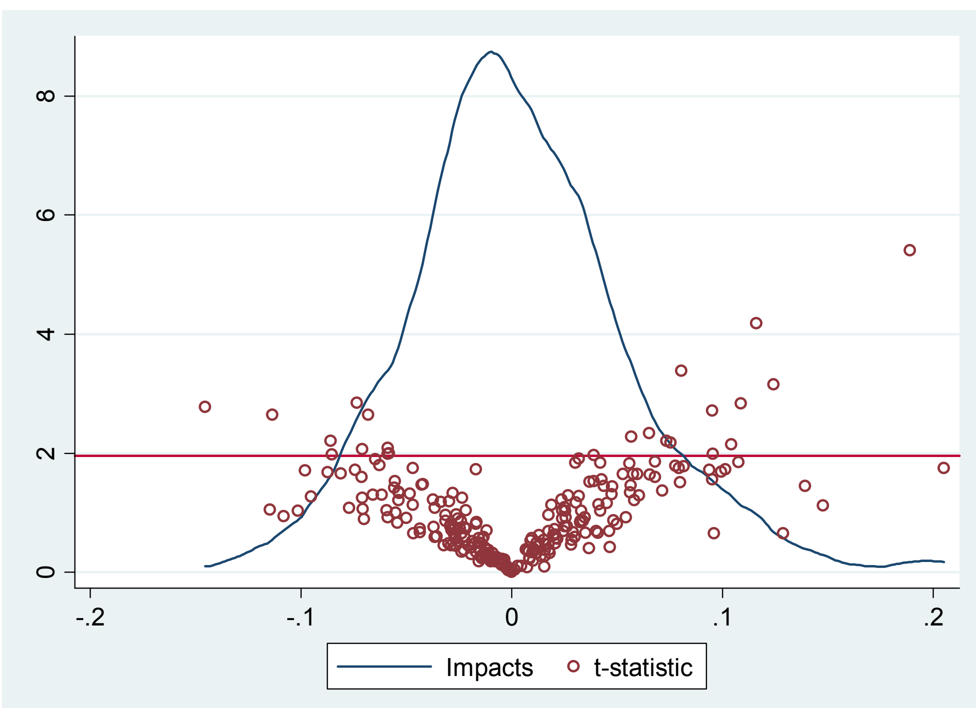

Figure 5: Distribution of estimated impacts of the program on failure rates by municipality

Horizontal line = 90% confidence level

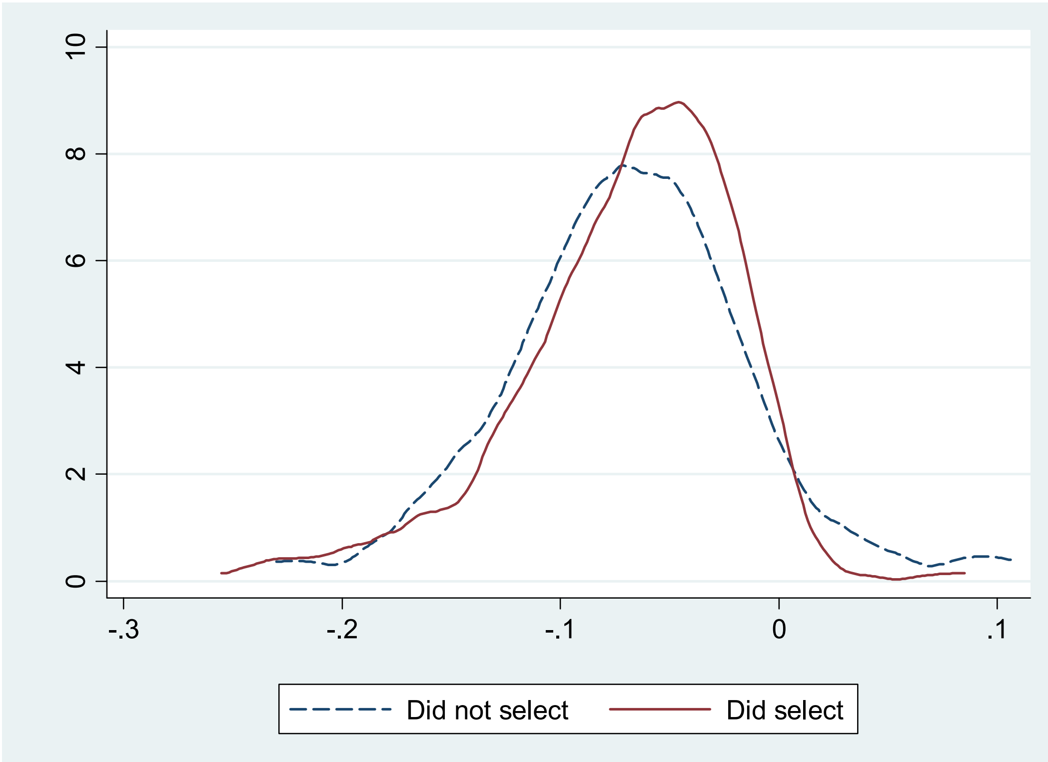

Figure 6: Distribution of impacts of the program on dropout rates, by whether the municipality selected its beneficiaries or not

Table 1. Average impact of the Bolsa Escola program on dropout rates

Dependent variable: Dropout (1/0)	OLS (1)	OLS (2)	FE (3)	FE (4)	OLS (5)	FE (6)
Treatment effect	0.017	-0.034	-0.056	-0.078	-0.067	-0.074
	[0.004]	[0.003]	[0.002]	[0.002]	[0.013]	[0.007]
Bolsa Escola beneficiary	-0.128	-0.031			-0.009	
	[0.005]	[0.002]			[0.004]	
Year intercepts	Y	Y	Y	Y	Y	Y
Child intercepts	N	N	Y	Y	N	Y
Dropout status in 2000	N	Y	N	N	Y	N
Dropout status in 2000 X Year effects	N	N	N	Y	Y	Y
Mean of dependent variable	0.136	0.115	0.136	0.115	0.094	0.094
Number of children			293481	121003		10170
Observations	624059	362429	624077	362442	30168	30173
R-squared	0.02	0.35	0.02	0.21	0.42	0.2

Robust standard errors in bracket
Columns (5) and (6): Sample restricted to the 13 municipalities where pre-treatment drop-out rates between recipients and non-recipients are not statistically different.

Table 2. Differential impacts of the Bolsa Escola program on dropout rates

Dependent variable: Dropout (1/0)	Morning (1)	Afternoon (2)	Night (3)	Primary in 2000 (4)	Secondary in 2000 (5)	(6)
Treatment effect	-0.055	-0.068	-0.133	-0.083	-0.074	
	[0.003]	[0.004]	[0.020]	[0.003]	[0.002]	
Treatment effect in 2001						-0.07
						[0.002]
Treatment effect in 2002						-0.082
						[0.003]
Treatment effect in 2003						-0.087
						[0.003]
Year intercepts	Y	Y	Y	Y	Y	Y
Child intercepts	Y	Y	Y	Y	Y	Y
Dropout status in 2000	N	N	N	N	N	N
Dropout status in 2000 X Year effects	Y	Y	Y	Y	Y	Y
Mean of dependent variable	0.081	0.103	0.280	0.119	0.111	0.115
Number of children	66530	59427	22206	69801	52229	121000
Observations	145900	118528	38993	219561	148487	362429
R-squared	0.22	0.21	0.23	0.18	0.25	0.21

Robust standard errors in bracket

Table 3. Average impact of the Bolsa Escola program on failure rates

Dependent variable: Failure (1/0)	OLS (1)	OLS (2)	FE (3)	FE (4)	OLS (5)	FE (6)
Treatment effect	-0.004	0.012	0.008	0.008	0.012	0.012
	[0.004]	[0.005]	[0.003]	[0.002]	[0.006]	[0.003]
Bolsa Escola recipient	0.000	-0.009			-0.009	
	[0.005]	[0.003]			[0.002]	
Year intercepts	Y	Y	Y	Y	Y	Y
Child intercepts	N	N	Y	Y	N	Y
Failure status in 2000	N	Y	N	N	Y	N
Failure status in 2000 X Year effects	N	N	N	Y	Y	Y
Mean dependent variable	0.147	0.131	0.147	0.131	0.135	0.135
Number of children			254996	111100		72616
Observations	538789	320562	538805	320575	209701	209709
R-squared	0.00	0.18	0.00	0.23	0.35	0.24

Robust standard errors in bracket
Columns (5) and (6): Sample restricted to the 13 municipalities where pre-treatment drop-out rates between recipients and non-recipients are not statistically different.

Table 4. Differential impacts of the Bolsa Escola program on failure rates

Dependent variable: Failure (1/0)	Morning (1)	Afternoon (2)	Night (3)	Primary in 2000 (4)	Secondary in 2000 (5)	(6)
Treatment effect	0.000	0.000	0.011	0.008	0.000	
	[0.004]	[0.005]	[0.017]	[0.003]	[0.003]	
Treatment effect in 2001						0.000
						[0.003]
Treatment effect in 2002						0.013
						[0.003]
Treatment effect in 2003						0.013
						[0.004]
Year intercepts	Y	Y	Y	Y	Y	Y
Child intercepts	Y	Y	Y	Y	Y	Y
Failure status in 2000	N	N	N	N	N	N
Failure status in 2000 X Year effects	Y	Y	Y	Y	Y	Y
Mean dependent variable	0.131	0.137	0.119	0.157	0.092	0.131
Number of children	62144	54147	17176	63497	48609	111097
Observations	134087	106316	28038	193419	131948	320562
R-squared	0.24	0.24	0.27	0.22	0.27	0.23

Robust standard errors in bracket

Table 5. Average and differential impacts of the Bolsa Escola program on grade retention

Dependent variable: Dropout or Failure (1/0)	OLS (1)	OLS (2)	FE (3)	FE (4)	Morning (1)	Afternoon (2)	Night (3)	Primary in 2000 (4)	Secondary in 2000 (5)
Treatment effect	0.011	-0.009	-0.034	-0.06	-0.045	-0.057	-0.108	-0.055	-0.07
	[0.005]**	[0.005]*	[0.003]***	[0.003]***	[0.004]***	[0.005]***	[0.022]***	[0.003]***	[0.004]***
Bolsa Escola beneficiary	-0.11	-0.041							
	[0.006]***	[0.003]***							
Year intercepts	Y	Y	Y	Y	Y	Y	Y	Y	Y
Child intercepts	N	N	Y	Y	Y	Y	Y	Y	Y
Dropout or Failure status in 2000	N	Y	N	N	N	N	N	N	N
Dropout or Failure status in 2000 X Year effects	N	N	N	Y	Y	Y	Y	Y	Y
Mean of dependent variable	0.264	0.231	0.261	0.232	0.201	0.226	0.366	0.257	0.193
Number of children			293481	121003	66530	59427	22206	69801	52229
Observations	624059	362429	624077	362442	145900	118528	38993	219561	148487
R-squared	0.01	0.27	0.01	0.21	0.23	0.21	0.22	0.19	0.25

Robust standard errors in bracket

Table 6. Partial correlations of Bolsa Escola's impact on dropout rates with municipal and mayor characteristics

Dependent variable Program's impact on dropout rate	Mean	(1)	(2)	(3)	(4)
Municipal characteristics					
Population density (persons/km)/1000	0.001	-3.831	-4.467	-4.671	-4.844
		[0.978]**	[0.972]**	[1.044]**	[1.343]**
Number of districts/100	0.030	0.079	0.042	-0.015	0.035
		[0.098]	[0.089]	[0.086]	[0.099]
Share of rural households	0.466	-0.035	-0.031	-0.035	-0.02
		[0.027]	[0.027]	[0.028]	[0.028]
Share of literate population	0.668	0.145	0.118	0.114	0.048
		[0.081]+	[0.078]	[0.078]	[0.075]
Log per capita income	4.190	-0.081	-0.069	-0.079	-0.026
		[0.024]**	[0.024]**	[0.025]**	[0.030]
Income inequality (Gini coefficient)	0.520	-0.033	-0.015	-0.022	-0.07
		[0.083]	[0.080]	[0.083]	[0.084]
Mayor characteristics					
Age/100	0.483		-0.014	-0.021	-0.047
			[0.038]	[0.039]	[0.036]
Education	6.335		0.001	0.001	0
			[0.002]	[0.002]	[0.002]
Gender (male=1)	0.915		-0.027	-0.031	-0.032
			[0.013]*	[0.014]*	[0.013]*
Second-term	0.585		0.02	0.023	0.022
			[0.008]**	[0.008]**	[0.007]**
Share of legislative branch that supports the mayor	0.655		0.032	0.03	0.023
			[0.020]	[0.020]	[0.019]
Owns the local media (1/0)	0.117		-0.013	-0.018	-0.017
			[0.010]	[0.011]+	[0.011]
Wife is a politician	0.131		0.025	0.022	0.016
			[0.011]*	[0.011]*	[0.010]
Years of political experience			0.003	0.002	0.003
			[0.003]	[0.003]	[0.003]
Other municipal characteristics					
Share of public employees related to the mayor	0.016			0.106	0.156
				[0.062]+	[0.053]**
Share of secretariat that are politicians (vs. technicians)	0.473			0.005	0.023
				[0.011]	[0.011]*
Number of public employees/10000	0.062			0.15	0.062
				[0.080]+	[0.078]
Number of secretaries/100	0.171			-0.031	-0.013
				[0.030]	[0.026]
Has an NGO	0.331				0.001
					[0.008]
Share of public sector	7.652				0.124
					[0.196]
Number of newspapers	0.29				-0.004
					[0.003]
Number of radio stations	1.200				0.004
					[0.004]
Judiciary district	0.5755328				-0.013
					[0.009]
Share of the quota	0.4831601				-0.003
					[0.001]**
Received training	0.215716				0.005
					[0.009]
Bosa Escola council	0.8048374				0.002
					[0.010]
Constant		0.21	0.153	0.208	0.063
		[0.102]*	[0.115]	[0.121]+	[0.133]
Mean of dependent variable	-0.067				
State intercepts		N	N	N	Y
Observations		232	232	232	232
R-squared		0.09	0.19	0.21	0.32

Table 7. Partial correlations of Bolsa Escola's impact on dropout rates with features of beneficiary identification

	Mean	(1)	(2)	(3)	(4)	(5)
Who registered the beneficiaries?						
Professors	0.706	-0.002				0.001
		[0.008]				[0.009]
Health agents	0.311	0.003				0.004
		[0.007]				[0.009]
Mayor's office	0.816	0.021				0.023
		[0.009]*				[0.011]*
Members of civil society	0.105	-0.001				0.005
		[0.011]				[0.012]
Where were the beneficiaries registered?						
Schools	0.851		-0.021			-0.023
			[0.012]+			[0.014]+
Health centers	0.088		0.011			0.01
			[0.016]			[0.015]
Mayor's office	0.544		0.005			0
			[0.008]			[0.008]
Communities	0.364		-0.012			-0.014
			[0.007]+			[0.008]+
Other aspects of registration						
Geographic targeting	0.364			-0.005		-0.002
				[0.007]		[0.007]
Home visits	0.250			0.003		0.003
				[0.008]		[0.010]
Verified information	0.632			-0.015		-0.02
				[0.007]*		[0.008]*
How were individuals notified about the registration?						
Radio	0.662				0.001	-0.004
					[0.008]	[0.008]
Pamphlets	0.184				-0.004	0
					[0.008]	[0.009]
Community leaders	0.601				-0.007	-0.006
					[0.007]	[0.007]
Schools	0.930				0.006	0.012
					[0.011]	[0.011]
Public announcements	0.548				0.001	0.003
					[0.007]	[0.007]
All controls from Table 6		Y	Y	Y	Y	Y
State intercepts		Y	Y	Y	Y	Y
Mean dependent variable		-0.067	-0.067	-0.067	-0.067	-0.067
Observations		235	232	233	236	228
R-squared		0.34	0.35	0.34	0.33	0.4

Robust standard errors in brackets. + significant at 10%; * significant at 5%; ** significant at 1%

Table 8. Partial correlations of Bolsa Escola's impact on dropout rates with monitoring and enforcement of program requirements

	Mean	
What happened when a child violated the conditionality?		
Lose the transfer	0.734	-0.017
		[0.009]+
Cut from the program	0.279	-0.006
		[0.010]
Received a visit from a program official	0.442	0.011
		[0.008]
Nothing	0.717	0.008
		[0.008]
All controls from Table 6		Y
State intercepts		Y
Mean dependent variable		-0.067
Observations		233
R-squared		0.35

Robust standard errors in brackets. + significant at 10%; * significant at 5%; ** significant at 1%

Appendix Table. Averages in Figures 1 and 2

	Dropout rates			Failure rates		
Year	Beneficiaries	Non-beneficiaries	Difference	Beneficiaries	Non-beneficiaries	Difference
1999	0.048	0.163	-0.115	0.151	0.126	0.024
	(0.001)	(0.001)	(0.003)	(0.003)	(0.001)	(0.003)
2000	0.041	0.179	-0.138	0.128	0.115	0.013
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)
2001	0.036	0.163	-0.128	0.132	0.114	0.017
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)
2002	0.046	0.156	-0.110	0.140	0.128	0.012
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)
2003	0.063	0.157	-0.095	0.144	0.137	0.007
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)

Standard errors are reported in parentheses. The dotted line separates pre-program and program years.