

Local Electoral Incentives and Decentralized Program Performance¹

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April 2009

Abstract

To take advantage of their superior access to information, local governments are increasingly entrusted with the provision of public goods. Yet, for decentralized programs to be effectively implemented, citizens must be able to hold elected officials accountable for their performance. While recognized theoretically, rigorous estimates of the efficiency gains from local electoral incentives remain largely absent. This paper analyzes how electoral incentives affect the performance of a major decentralized conditional cash transfer program, Bolsa Escola in Brazil. The data allow identification of both program impact at the municipal level and the role of electoral incentives under term limits. We show that the program's reduction of primary school dropout rates was 36 percent higher in municipalities governed by mayors who faced reelection possibilities compared to those with lame-duck mayors. We establish correlates with local governance practices that help understand how a mayor's behavior affects program performance. And we also show that high program performance strongly contributes to the likelihood of re-election of incumbent mayors.

JEL classification: D78, H43, I28, O15

Keywords: Decentralization; electoral incentives; conditional cash transfer; impact evaluation.

I. Introduction

Many countries have decentralized the implementation of public programs to locally-elected governments in seeking efficiency gains in performance (Bardhan, 2002). The expectation is that local governments can use information not available to central authorities in allocating or targeting public resources (Faguet, 2004). Moreover, stakeholders can use electoral incentives to reward good program performance by locally-elected officials (Seabright, 1996; World Bank, 2003).

¹ We are indebted to Kathy Lindert, Bénédicte de la Brière, Donald Nelson, Lucia Chaves, Simone Cabral Marinho dos Santos, Irlés 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. Authors' addresses: Alain de Janvry, alain@are.berkeley.edu; Frederico Finan, ffinan@econ.ucla.edu; Elisabeth Sadoulet, sadoulet@are.berkeley.edu.

In practice, however, decentralized program implementation may not meet these expectations. Although the informational advantages associated with decentralization have been shown to exist (Alderman, 2002), use of this information for efficient program implementation depends on whether local governments can in fact be held accountable to their citizens. This remains an open question. The accountability of local governments to program stakeholders can be curtailed by the power of opportunistic elites (Rose-Ackerman, 1999) or by political institutions that do not give citizens the ability to discipline elected officials (Persson and Tabellini, 2002). For instance, term limits may curtail incentives for politicians to provide high quality local public goods when they can no longer seek re-election (Besley, 2006). As such, local politicians' performance in program implementation depends on the electoral incentives given to them to make good use of the informational advantages they possess.

In this paper, we investigate the extent to which local electoral incentives and governance practices affect the level of impact of a major decentralized conditional cash transfer (CCT) program in Brazil – Bolsa Escola – designed to reduce school dropout among the children of the poor. Although Bolsa Escola, now part of a broader program called Bolsa Familia, was a federal program, municipal governments were responsible for identifying beneficiaries and enforcing conditionalities. This not only created substantial variation in the manner in which the program was implemented, but also in its impact. Using an extensive dataset that combines a municipal survey with school records for 290,517 children over the period 1999 to 2003 (with two years before the program and three years under the program), we estimate the program's impact on primary and secondary school dropout rates for each of 261 municipalities in the Northeast of Brazil. We then identify the impact of municipal electoral incentives on local politicians' program performance by measuring the difference between first and second-term mayors. While many studies in constitutional economics have posited the importance of electoral incentives for effective decentralized service delivery, this study is among the first to support this empirically.

We find that while, on average across municipalities, the program reduced dropout rates during the school year by 8 percentage points, there was also a considerable amount of variation in program impacts across municipalities. Municipalities governed by a first-term mayor who was eligible for re-election had an estimated 36 percent higher program performance compared to municipalities governed by a second-term mayor who is, by constitutional law, not eligible for re-election. This difference persists when comparing second-term mayors to first-term mayors that got re-elected in the subsequent election, thus controlling for revealed ability, and to mayors with the same level of political experience. In any case, since ability and experience induce a better performance of second term mayors, incomplete control for these characteristics would lead to

underestimation of electoral incentives. We show that these findings are consistent with a simple model of political agency where electoral incentives induce greater effort in program management by first term mayors.

We provide insights into the processes that underlie this difference in performance between first and second-term mayors. We find that public denouncements of program mismanagement, such as inclusion of non-poor children among beneficiaries and political manipulations in program implementation, are more frequent under second than first-term mayors. A number of poor management practices relating to lack of transparency that affect program performance are clearly more frequently associated with second than first-term mayors, indicating how lower performance actually comes about.

Finally, we show that first-term mayors have reason to care about program performance because it helps them get re-elected. This is particularly true for the 25 percent mayors who achieve the largest program impacts: for them, the probability of re-election is 28 percent higher. Mayors with no public denouncements of inclusion errors are also rewarded by a 26 percent higher probability of re-election.

Our findings contribute to a growing empirical literature in constitutional economics that emphasizes the importance of electoral accountability in aligning politicians' actions with voters' preferences. Following the political economy agency models of Barro (1970), Ferejohn (1986), and Banks and Sundaram (1993), Besley and Case (1995) show that re-election incentives affect the fiscal policy of U.S. governors while List and Sturm (2006) provide evidence that term limits influence environmental policy. Using the outcomes of random audits of municipal finances, Ferraz and Finan (2008) show that mayors that face re-election incentives are significantly less corrupt than mayors without such incentives. While both their and the present paper analyze the role of local electoral incentives on politicians' performance, the two papers differ in a complementary fashion: the Ferraz-Finan paper focuses on rents extracted via corruption practices while the present paper focuses on effort made in achieving higher program performance.

Much of the empirical work on the effectiveness of decentralized service delivery has used the quality of targeting as an indicator of program performance instead of a measure of program impact. For instance, Alderman (2002) has shown that more information is available at the local level than at the centralized level and that this informational advantage has been used by local authorities in Albania to better target the poor. Galasso and Ravallion (2005) for public food-for-school transfers in Bangladesh and Bardhan and Mookherjee (2006) for credit and agricultural input kits in West Bengal villages find that the degree of local pro-pooriness in

decentralized targeting is affected by program and village features. Because, as noted by Ravallion (2007), targeting may not be a good indicator of program performance, our paper starts by developing a local-level impact indicator for the program analyzed.

The paper is organized as follows. In section II, we present the features of the Bolsa Escola program and review results obtained in previous evaluations. In section III, we propose a model of local electoral incentives that highlights the differences in effort in program implementation between first-term and second-term mayors. In section IV, we explain the data collection and give descriptive statistics on dropout rates across municipalities. We explain the method for measuring program impact at the municipality level in section V. We then report in section VI results on the impact of Bolsa Escola on dropout rates. In section VII, we give estimates of the gains in performance due to electoral incentives. We also explore the association of local governance practices with program performance and the electoral rewards from good performance. We conclude in section VIII on the benefits of electoral incentives for decentralized program performance.

II. The Bolsa Escola program

Primary and secondary education are compulsory in Brazil for children ages 6 to 15 and, as a consequence, enrollment at the beginning of the school year is almost universal (Souza, 2005). But high enrollment rates hide a major problem in that a large number of children dropout of school during the school year, only to re-enroll in the following year as required by law. This induced the Brazilian government to introduce an educational CCT program, Bolsa Escola, that offered mothers in poor households a monthly stipend conditional on their children ages 6 to 15 attending school on a regular basis. Households with a monthly per capita income of less than 90 Reais (around \$40) were eligible. The transfer was R15 per child with a maximum of R45 per household.² While the eligibility and conditionality rules were similar to those of other educational CCT programs such as Mexico's Oportunidades, a distinguishing feature of the Bolsa Escola program was that it was decentralized at the municipal level. Each municipality received a quota of stipends it could provide to its population. Municipalities were responsible for identifying all households below the poverty line with children that met the program's requirements. Because the number of qualified households generally exceeded the quota, it was also the responsibility of the municipality to then select program beneficiaries.³ Municipalities

² As a comparison, the minimum wage was R240 per month.

³ For every municipality in our sample, the number of qualified households exceeded its quota.

were also entrusted with enforcing the school attendance conditionality. The program was implemented across all of Brazil between the years 2001 and 2003, and subsequently incorporated into the broader Bolsa Familia program with little substantive change (Lindert, Linder, Hobbs, and de la Brière, 2006).

Bolsa Escola was first conceived in the Federal District of Brasilia 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, Andrade, and Waiselfisz (1998) found that dropout rates were 7 percentage points lower for beneficiary children than for children of non-beneficiary families. For the city of Recife, Aguiar and Araújo (2002) found that dropout 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. While these 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 the program's requirements were weakly enforced. Using municipal-level data, Glewwe and Kassouf (2008) find that Bolsa Escola reduced dropout rates by about 8 percentage points for children in grades 1-4 but did not impact the dropout rates of children in grades 5-8.⁴

The present evaluation of Bolsa Escola uses a rigorous identification of impact based on observed individual child responses to the incentives provided by the CCT. Measurements are obtained both overall and for each municipality. We then use this measure of municipal performance to analyze the importance of electoral incentives and of local governance practices on observed outcomes.

III. A model of local electoral incentives

In this section, we present a simple political economy model that will help interpret our main empirical finding that the program impacts were higher in municipalities where the mayor faced re-election incentives relative to those where the mayor did not. The model is an adaptation of the standard political agency model found in Barro (1970), Ferejohn (1986), and recently Alesina and Tabellini (2007).⁵ The basic insight from this class of models is that in a political context in which elections reward competent or able politicians, incumbents with reelection possibilities

⁴ Bourguignon, Ferreira, and Leite (2003) provided an ex-ante evaluation of Bolsa Escola. Using a simulation model based on observed child schooling responses to labor market wages estimated from the PNAD data, they find that over 50 percent of the children of poor households would respond to the incentives of the program. This implies halving the pre-program dropout rate, again a result not far from our own estimates.

⁵ Persson and Tabellini (2002) and Besley (2006) provide reviews of this class of models.

have the incentive to exert more effort in the implementation of the program in order to increase their re-election chances.

To see how electoral accountability at the local level affects the impact of the program, consider a decentralized CCT program where a municipality is responsible for the selection of program beneficiaries. The local government must decide whether a child i with characteristics z_i is selected or not ($P_i \in \{0,1\}$), given the number of stipends B allocated to the municipality. The impact $\theta_i = \theta(z_i)$ that the program is expected to have on the school performance of child i depends on his characteristics. The average program impact in the municipality is $\bar{\theta} = \sum_i P_i \theta(z_i)$.

From the perspective of the program itself, maximum impact is obtained by giving beneficiary status to children with the highest θ_i , in descending order, until the municipal quota is reached. However, this first best outcome may be difficult to achieve for at least two reasons. First, the objectives of the program and of the mayor may not be entirely aligned. Second, even if the mayor was concerned with maximizing program effectiveness in reducing dropout, to correctly anticipate a child's response to the program requires levels of administrative ability and effort that the mayor may not have or provide.

To formalize these possibilities, consider a two-period model where citizens choose at the end of the first period whether to re-elect the incumbent mayor or not. Voters care about the program's impact $\bar{\theta}(a, e, z, B)$, which is influenced by both the mayor's ability a and effort e , with first derivatives $\bar{\theta}_a$ and $\bar{\theta}_e$ positive, respectively. The mayor's own utility is also function of his direct reward R from beneficiary selection that can be written as $R(a, e, z, B)$, decreasing in effort $R_e < 0$. To simplify further the derivation of the model, we assume that the program impact is $\bar{\theta} = g(z, B) + a + e$, and the politician's utility is $R - \psi(e)$, with $\psi_e > 0, \psi_{ee} > 0$. As defined here, e represents the effort exerted by the mayor in both the selection of beneficiaries and/or the administration of the program. Although more effort does increase the program's impact, the mayor experiences a disutility from exerting effort.

The timing of events is as follows. First the mayor selects the beneficiaries. His ability to select beneficiaries is a random variable $a_1 \sim N(\bar{a}, \sigma_a^2)$, unknown even to him in the first period. He chooses the amount of effort to exert in increasing the impact of the program. At the end of the first period, voters observe the outcome $\bar{\theta}$, but not its decomposition between ability and effort. Based on this observation, they decide on whether to re-elect the incumbent or not. If the incumbent is re-elected, his ability is maintained. Otherwise, a challenger comes in with an

ability randomly drawn from the distribution of abilities. In period 2, the mayor chooses effort thus determining the impact of the program.

Because the game ends in period 2, there is no incentive for mayors to exert any effort in the second period. So program impact in the second period is $g(z, B) + a_1$ if the incumbent is reelected, and expected impact is $g(z, B) + \bar{a}$ otherwise. Unable to induce politicians to exert effort, voters will seek to elect the most able mayor. The incumbent is reelected if the impact exceeds a threshold W corresponding to the outcome expected from the average politician.

The first-term mayor's inter-temporal objective is:

$$\max_e R - \psi(e) + \Pr(\bar{\theta} \geq W)R.$$

The optimal effort e^* is the solution to $\psi_{e^*} = R\varphi(W - g(z, B) - e^*)$, where φ is the density function of ability. The mayor sets his effort to equate the current marginal cost of not using the program for his own benefit to the future marginal benefit of being re-elected.

Rational voters know that they should expect this optimal level of effort e^* and corresponding outcome $W = g(z, B) + \bar{a} + e^*$ from the average politician. They will vote the incumbent in if the observed outcome is higher than the expected outcome, $\bar{\theta} \geq W$. Substituting the expression for W , the optimal effort of the first term mayor is the solution to:

$$\psi_{e^*} = R\varphi(\bar{a}).$$

The corresponding program impact of a first term mayor is:

$$\bar{\theta}_1 = g(z, B) + a_1 + e^*,$$

greater than the second term impact, if he is re-elected:

$$\bar{\theta}_2 = g(z, B) + a_1.$$

The intuition for this result is straightforward. When faced with the possibility of re-election, incumbent politicians will exert more effort to increase the program's impact. Thus, given the theory, we would expect the program to perform better in municipalities with mayors who have re-election incentives relative to those with mayors who do not. This is the main testable prediction that we take to the data.

While our model predicts that the program will perform better in municipalities governed by mayors with re-election incentives, there are alternative models that provide the opposite prediction. If, for instance, our model only captured a mayor's ability (and ability was revealed after taking office), then elections would serve as a screening device for high-ability mayors. In this case, the program would perform better in municipalities with second-term mayors, who on

average would be more able than first-term mayors. While this is certainly a possibility, our results suggest that the program performed better in municipalities governed by first-term mayors, perhaps even in spite of the fact that second-term mayors are potentially more able than first-term mayors. This is important as it says that observing a better program performance in the first term has to come from electoral incentives even if elections serve in improving second-term performance via screening for ability.

Implicit in our model is the assumption that voters care about the program's impact. Voters, however, may only care about whether they are the recipients of the program. In this case, the incentive politicians have to target the program might be quite different from those presented in the model. In fact, mayors who face re-election possibilities may prefer to target the non-poor with children not at risk of dropping out of school in order to garner support from this group (Besley and Kanbur, 1993). In this case, the program may perform worse in municipalities with first-term mayors. Here again, observing better first-term program performance would have to come from electoral incentives even if elections induce clientelistic games via incorporation of children not at risk of dropping out of school.

IV. Data and descriptive statistics

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.⁶ In each of the 261 municipalities, two data collection instruments were applied – compilation of school records and a municipal survey – and complemented by secondary data on the municipality regarding Bolsa Escola payment records and electoral outcomes.

School records

To properly measure the effects of Bolsa Escola on school attendance, we collected in each municipality school records for approximately 500 children over the 1999-2003 period. To gather these records, one or two schools were randomly drawn proportionately to the number of Bolsa Escola recipients (this number was obtained from the payments records of the Ministry of Education) within each selected municipality. Information on the enrollment status of each child

⁶ 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.

in the school was compiled from the annual class reports filled in by teachers. Matching records across school years was done manually with children's first and last names, allowing us to match 85 percent of the collected records.⁷ In total, we can follow the school performance for 290,517 children in primary and secondary school over part or all of five years, giving us 604,561 data points.

In the class 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. Table 1 shows the magnitude of the school dropout problem that Bolsa Escola was designed to address. In 2000, before the program started, 15 percent of the children dropped out of school during the course of the year. And even among the non-beneficiaries of Bolsa Escola, 98 percent of them came back to school in some later year.

Although administrative records have the advantage of greater accuracy 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 school attendance and 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 see that less than four percent of the children had transferred. In the analysis that follows, we simply remove these children from the sample.

Bolsa Escola eligibility and beneficiary status

In our school visits, teachers were asked to identify what percentage of children was eligible for Bolsa Escola. In every school, teachers identified more than 97 percent of the children as qualifying for the program.⁸ We therefore decided to consider all the children as eligible. As we will see later, the identification strategy does not depend on strict eligibility of all children.

⁷ The remaining 15 percent include cases where the child either left school without this being recorded; or the child's name was simply changed, incomplete, or unreadable. With no discernable pattern in the occurrence these cases across grades, it is unlikely that any of these reasons could be correlated with being a stipend recipient. Also restricting the estimation to the balanced-panel of children yields similar results.

⁸ That 97 percent of the children were qualified for the program is not surprising given both the public school system in Brazil, which is almost exclusively attended by poor children, and the fact that we

We obtained the complete list of beneficiaries from the Bolsa Escola federal office for the years 2001 and 2002. For each household, records include parents' and children's names, and the school attended by the child. Matching these beneficiaries to the school information gives the beneficiary status of all the children from the school records. Matching was again done manually using children's full names (last names of both parents and first name of the child). Beneficiary status in 2003 was assumed to be the same as in the past two years as no reselection was undertaken in that or prior years.

We have no independent information to confirm the non-beneficiary status of children in the school record that we could not match with the Bolsa Escola list. Matching errors may thus entail beneficiary children misclassified as non-beneficiaries. This would imply that we estimate a lower bound for the impact of the program.

Municipal data

The municipal survey contained several parts designed to gather general information on municipal administrative capacity, political influence and governance practices, and implementation of the Bolsa Escola program. Designated respondents for 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 corresponding program coordinator about how the municipality identified and selected beneficiaries, and monitored and enforced the conditionalities. We also gathered information to assess the extent to which the program was implemented in a transparent manner.

Overall we found considerable variation in the procedures used to identify and register potential beneficiaries. Differences relate to who were the agents responsible for identifying potential beneficiaries, where the registration took place, what criteria were used to prioritize registration (e.g., use of geographical targeting), and whether efforts were made to verify the information collected.⁹ The number of potential beneficiaries largely exceeded the quota of stipends that was allocated to the municipality by the central government. On average, an estimated 49 percent of eligible households were left out of the program, leaving it to the municipality to select the beneficiaries from among the pool of eligible households. We observed a great deal of variation in the criteria used to perform that selection (household income, number

sampled schools with probability proportional to the number of Bolsa recipients. The Northeast of Brazil is also the region with the highest incidence of poverty.

⁹ See de Janvry, Finan, Sadoulet, Nelson, Lindert, de la Brière, and Lanjouw (2005) for details on these procedures.

of children, etc.) and in the agents involved in it (direct intervention of the mayor's office, delegation to a Bolsa Escola Council, etc.). There was also variation in implementing conditionalities across municipalities. While most municipalities reported monitoring the attendance conditionality, in practice data from the Ministry of Education suggest that the actual forwarding of information on compliance to the central level, which directly made payments of benefits to beneficiaries, was only partial. Consequently, the imposition of financial penalties for non-compliance was low. In summary, our survey does reveal a great deal of heterogeneity across municipalities in program implementation despite definition of rules at the federal level.

In addition to information on program implementation, the survey was also designed to document practices that might be associated with poor governance. For instance, in our sample of municipalities, we found that 47 percent of the administrative positions were held by political appointees rather than technocrats.¹⁰ In 12 percent of the municipalities, the mayor's spouse was also an elected politician; and in 25 percent of the municipalities, more than 15 percent of the employees in the mayor's office were related to the mayor.

V. Measuring program performance

In this section, we present the econometric specifications used to estimate the impact of the Bolsa Escola program on dropout rates. Given our data and research design, we estimate the impact of the program for every municipality in the sample. It is this program impact that provides us with a measure of municipal program performance.

With decentralized implementation, beneficiaries are selected by the municipality. There is consequently no reason to believe that beneficiaries are strictly comparable to non-beneficiaries. To illustrate this selection process, the last rows of Table 1 report pre-program performance of the future beneficiaries and non-beneficiaries of Bolsa Escola. Beneficiaries were clearly not selected at random. Their pre-program dropout performance is less than a third that of non-beneficiaries. This in itself suggests that there is room in many municipalities for increasing the impact of the program by selecting children more at risk of dropout.¹¹

¹⁰ Throughout Brazil's political history, there has been a clear distinction between political appointees or "traditional politicians" and technocrats (técnicos) who are appointed individuals with non-political backgrounds (see Hagopian, 1996),

¹¹ The efficiency gains from specifically targeting children at risk of dropping out of school were calculated for Progres/Oportunidades, showing that they can be quite large if programs are budget constrained. See de Janvry and Sadoulet (2006).

We propose an empirical strategy to measure the effect of Bolsa Escola on the selected beneficiaries for each municipality that uses panel data on children before and after the start of the program, and further proceed with a number of tests to verify robustness of the results.

We start with a standard difference-in-difference model, where the schooling outcome S_{ijt} for child i in municipality j in period t is modeled as:

$$S_{ijt} = \phi_{ij} + X_{ijt}\beta + \theta_{ij}P_{ijt} + \varepsilon_{ijt}, \quad (1)$$

where S_{ijt} is schooling outcome (dropout), ϕ_{ij} is an individual fixed effect, X_{ijt} are time varying observables, and P_{ijt} is an indicator for program participation.¹² θ_{ij} is a random coefficient that measures the gain for individual ij from participating in the program, and ε_{ijt} an unobserved shock to schooling, assumed unrelated to program participation, conditional on X . Given that the process of selection of beneficiaries was done only once at the onset of the program in 2001, the concern is not that beneficiaries could be brought into the program in response to unanticipated shocks after the program has started. The main concern is that selected children may have an anticipated trend in schooling different from that of the non-selected children. We control for this by allowing year effects to vary by the pre-program dropout status S_{ij0} of the child, which is perhaps the best predictor of future dropout behavior. Hence, variables X_{ijt} are different sets of year effects: β_{jt} and $\delta_{jt}S_{ij0}$.

With this model for individual behavior, the *treatment on the treated* (TT) effect in municipality j is given by:

$$\bar{\theta}_j = E(\theta_{ij} | B_{ij} = 1), \quad (2)$$

where the conditioning on $B_{ij} = 1$ denotes that the individual ij is a beneficiary. This expression highlights the sources of differentiation in average TT effects across municipalities. Municipalities may differ in the distribution of individual effects θ_{ij} in their eligible population or in their selection of beneficiaries amongst the population.

The difference-in-difference estimator for this parameter can be estimated from the least squares estimator of:

$$S_{ijt} = \phi_{ij} + \beta_{jt} + \gamma_{jt}S_{ij0} + [\bar{\theta}_j + \theta^*(S_{ij0} - \bar{S}_{j0})]P_{ijt} + \mu_{ijt}, \quad (3)$$

¹² There is no distinction between participating in the program and being offered the program because take-up rates are 100 percent.

where $\mu_{ijt} = (\theta_{ij} - \bar{\theta}_j - \theta^*(S_{ij0} - \bar{S}_{j0}))P_{ijt} + \varepsilon_{ijt}$, $\theta^* = E(\theta_{ij} | B_{ij} = 1, S_{ij0} = 1) - E(\theta_{ij} | B_{ij} = 1, S_{ij0} = 0)$, and $\bar{S}_{j0} = E(S_{ij0} | B_{ij} = 1)$.

The estimator is consistent if $E((\theta_{ij} - \bar{\theta}_j - \theta^*(S_{ij0} - \bar{S}_{j0}))P_{ijt} + \varepsilon_{ijt} | \phi_{ij}, S_{ij0}, P_{ijt}) = 0$. The first term $E((\theta_{ij} - \bar{\theta}_j - \theta^*(S_{ij0} - \bar{S}_{j0}))P_{ijt} | \phi_{ij}, S_{ij0}, P_{ijt})$, which accounts for the difference between the child specific effect and the common effect conditional on S_{ij0} , is null by construction. Hence the estimator for the TT effect is consistent if $E(\varepsilon_{ijt} | \phi_{ij}, X_{ijt}, P_{ijt}) = 0$.

This specification thus provides an estimated TT effect when the selection of beneficiaries is based on an individual's fixed characteristics, a time fixed effects specific to the pre-treatment status, or of course any other variable unrelated to school dropout behavior. In this specification, the average impact of the program on the selected children of a municipality is identified by comparison of changes in their dropout rate with that of non-beneficiary children of the municipality with the same pre-program dropout status.¹³ In the empirical analysis, pre-program status is observed in both 1999 and 2000. With this approach, we estimate a TT impact for each municipality.

The validity of this identification strategy relies on the assumption that, conditional on an individual's dropout behavior prior the program, the trend in future dropout behavior of beneficiaries and non-beneficiaries would be the same in the absence of the Bolsa Escola program. To check on the validity of this assumption, we verify it in the pre-program period. With only two years of observations, we thus compare the changes in dropout rates between 1999 and 2000 for (future) beneficiaries and non-beneficiaries. This is given by the difference-in-difference estimator for each municipality as follows:

$$S_{ijt} = \alpha_j + \beta_j B_{ij} + \gamma_j T_{00} + \hat{\theta}_j B_{ij} T_{00} + \mu_{ijt}, \text{ for } t = 1999, 2000, \quad (4)$$

where B_{ij} represents the status of (future) beneficiary and T_{00} is a dummy variable for year 2000. The estimated coefficient is not significantly different from 0 in 194 municipalities. The point estimates, however, vary quite a bit, and are positive (meaning that future beneficiaries have a higher increase in dropout rates than non-beneficiaries) in 30 percent of the municipalities. We will thus perform robustness checks on the estimated impact of the program by restricting the

¹³ This will control for any reversion to the mean if selection was based on the dropout status. Note that in this particular case, where beneficiaries have much lower dropout rate than non-beneficiaries, a reversion to the mean would suggest that we were under-estimating the program's impact.

sample of municipalities to those with difference in trends that are either non-significant or positive.

VI. The impact of Bolsa Escola on dropout rates

In this section, we first report the estimation of the average impact over all municipalities in the sample. The estimated impact is robust to various empirical specifications and robustness checks. We then report the variation in program impacts across municipalities.

Average impact on dropout rates

Table 2 presents regression results from estimating several variants to equation (3), where the dependent variable is a binary variable for whether or not the child drops out of school during the school year. Column (1) presents an estimate of the treatment effect that only includes year and individual child fixed-effects. Under this specification, Bolsa Escola reduced dropout rates among beneficiary children by 5.7 percentage points. In columns (2) and (3), we extend the fixed-effects model to allow for children with different pre-treatment dropout status to experience different year effects (equation (3) above). The estimates suggest that Bolsa Escola reduced dropout rates by around 8 percentage points for its selected beneficiaries. Hence, if it were not for the program, we would have observed a dropout rate of 12 percent instead of the 4 percent currently observed among beneficiaries in 2001-2003. The program thus induced a substantial 66% percent decline in dropout.

Columns (4) and (5) present further robustness checks by restricting the sample to municipalities where pre-program trends verify the assumption underlying the identification. In column (4), the estimation is performed on the 194 municipalities where the pre-program differences in change in dropout rates between (future) beneficiaries and non-beneficiaries are not significantly different. In column (5), we restrict the sample to the 111 municipalities where beneficiaries had a higher increase in dropout rates than non-beneficiaries in the pre-program period. In both cases, the estimated impact on beneficiaries is basically the same as when estimated on the whole sample in column (2).

To further verify that the program's impact is not confounded with possible differential quitting rate from beneficiaries and non-beneficiaries, we estimated the treatment effect on the sub-sample of 16564 children enrolled all 5 years. Note that this sample has a different age structure than the population at large, with no older children in the earlier years and no young

children in the later years. However, the results are very similar with an estimated *TT* effect of -5.6 percentage points.

Estimation of municipal-level impacts on dropout rates

The Bolsa Escola program reduced dropout rates by 8 percentage points on average. Program impact varies, however, considerably across municipalities. Our survey design allows to estimate an impact $\bar{\theta}_j$ of the program for each municipality j . The solid line in Figure 1 presents the frequency distribution of these program impacts $\bar{\theta}_j$ on dropout rates, using the econometric specification presented in column 2 of Table 2 for each of the 261 municipalities. The distribution of impacts is skewed towards negative values with a median impact of -6.7 percentage points. While the estimated impacts range from -25.5 to 10.7 percentage points, over 95 percent of them are in fact negative. In addition to the distribution of unbiased estimates of impact, Figure 1 also plots the absolute values of the corresponding t-statistics. Each small circle represents the estimated impact for one municipality, with the point estimate reported on the horizontal axis and its corresponding t-statistic on the vertical axis. Few positive impacts are measured precisely.¹⁴ Over 55 percent of the estimates are significantly negative at the 5 percent level and 65 percent at the 10 percent level. Given the substantial variation in program performance across municipalities, a natural question to ask is to what extent electoral accountability – an important motive for decentralization – affects program performance across municipalities?

VII. Impact of electoral accountability on municipal program performance

In this section, we provide evidence consistent with theory that the program performed better in municipalities where mayors faced re-election incentives. To estimate these effects of electoral accountability, we compare the program's impact on dropout rates in municipalities where the mayor was serving in his first term (and hence could be re-elected) to those where the mayor was in his second (and thus final) term. Although the difference in program impacts between first and second-term mayors will capture the effects of re-election incentives, it is also potentially confounded by at least two factors. First, given that second-term mayors are a selected group, it is likely that they are more politically able than first-term mayors. If political ability is positively correlated with program performance, then the simple difference will be biased upwards, meaning

¹⁴ As can be seen in Figure 2, only one positive impact is significantly different from 0 at the 10 percent level and the rest have an average t-statistic of 0.80.

we will underestimate the reduction in dropout rate. Second, second-term mayors by construction have more consecutive years of experience in office than first-term. Without controlling for these potential differences in experience, our estimates will again be upwardly biased. To see this more explicitly, consider the following regression model:

$$\bar{\theta}_j = \alpha + \beta^* R_j + X_j \delta + \gamma_1 Ability_j + \gamma_2 Experience_j + \varepsilon_j, \quad (5)$$

where R_j is a dummy variable equal to 1 if the mayor is in his second term and the X_j are municipality or mayor characteristics. The simple difference between first and second-term mayors will be:

$$\beta = E[\bar{\theta}_j | R=1, X_j] - E[\bar{\theta}_j | R=0, X_j] = \beta^* + \gamma_1 \mu_1 + \gamma_2 \mu_2,$$

where $\mu_1 = E[Ability | R=1, X_j] - E[Ability | R=0, X_j]$ and

$$\mu_2 = E[Experience | R=1, X_j] - E[Experience | R=0, X_j].$$

Thus assuming that γ_1 and γ_2 are positive, then $\beta > \beta^*$.

These biases may not be of concern as they affect performance in the opposite direction as electoral incentives. However, to account for these potential biases, we follow Ferraz and Finan (2008) and employ two strategies. First, we compare second-term mayors to the set of first-term mayors who are re-elected in the subsequent election. If the bias from the OLS regression comes from unobserved political ability that positively selects more able politicians into a second-term, this approach controls for a significant portion of this bias by comparing mayors that are as politically able as second-term mayors. Second, in comparing first and second-term mayors, we can account for the effects of political experience by restricting the sample to first-term mayors that have had at least two years of political experience in another office.

The effects of re-election incentives on program performance

Table 3 presents regression results from estimating several specifications based on equation (5).¹⁵

The dependent variable in each regression is the impact of the program on dropout rates for a

¹⁵ We purposefully chose to first estimate the program impact at the municipality level and then estimate the impact of mayors' electoral status on this impact, because this is what best corresponds to the fact that municipalities are independent of each others in their management of the program. The alternative of a joint estimation over all municipalities, such as $S_{ijt} = \phi_{ij} + (\theta_1 + \theta_2 R_j) P_{ijt} + \mu_{ijt}$, implies assuming a distribution for μ_{ijt} that is common to all ijt observations, which is not the same as assuming one distribution per municipality in the model $S_{ijt} = \phi_{ij} + \theta_j P_{ijt} + \mu_{ijt}$. The OLS estimators of these two models are different as they verify $\bar{S} = \bar{\phi} + \hat{\theta}_1 \bar{P} + \hat{\theta}_2 \bar{R} \bar{P}$ and $\bar{S} = \bar{\phi} + (\hat{\theta}_1 + \hat{\theta}_2 \bar{R}) \bar{P}$, respectively.

particular municipality. Column (1) reports the unadjusted relationship between program impact and whether the mayor is in his second term. From this bivariate relationship, we see that the impact of the program is 2 percentage points worse among municipalities governed by a second-term mayor relative to a first-term mayor. At an average impact of -8 percentage points among first-term mayors, this represents a 25 percent decline in performance.

Columns (2) and (3) correspond to specifications that include additional sets of controls. In column (2), we account for basic differences in mayors and municipalities, as well as state intercepts. Column (3) includes other municipal characteristics that although endogenously determined might proxy for some unobserved determinants of program performance. As seen across both columns, the inclusion of these additional controls has virtually no effect on the original estimate. For instance under the most flexible specification (column 3), the point estimate is 0.021 (standard error = 0.007) which is statistically significant at the 1% level. This stability of the parameter to the addition of many mayor and municipality characteristics gives confidence that the estimated effect is likely not due to omitted variable bias.

In columns (4) and (5), we examine the extent to which political ability and experience are biasing our results. Column (4) reports the results from the comparison of second-term mayors to the subset of first-term mayors that are re-elected in the subsequent 2004 elections. Using this sample of mayors, we find that re-election incentives improve program performance by 2.6 percentage points, which is statistically similar to our base specification. To account for any potential differences in experience between first and second-term mayors, we compare in column (5) second-term mayors to the subset of first-term mayors that have had at least 2 terms of political experience as either a local legislator or a state or federal congressman. Although we control for political experience in column (2) (and find that the estimates are robust), this comparison excludes first-term mayors that have less political experience than second-term mayors. The results are consistent with the previous specifications.

Column (6) in Table 3 presents other aspects of local governance that are also correlated with program impact. The program performs worse among municipalities with higher levels of nepotism or cronyism, as measured by whether the mayor's spouse is also a politician, the share of employees in the mayor's office who are related to the mayor, and the share of secretariat members who are politicians rather than technicians. We find that even after accounting for these symptomatic features of bad governance, the effects of re-election incentives are still important.

What do second-term mayors do to perform poorly?

If second-term mayors have a program performance inferior to that of first-term mayors, then this should not go un-noticed with stakeholders. In stakeholder opinions collected in the municipal survey, complaints about program implementation should be more frequent under second-term mayors than first. Results in Table 4 show that this is indeed the case. Mayors in their second term face 21 percent more frequently complaints about inclusion of non-qualifying children (Type II errors in targeting). Although there is no reason to believe that targeting non-poor children is inefficient (maximum efficiency is obtained by targeting the children most likely to respond to the transfer offer, as described in the model of section III), it indicates that the population is aware that the program is not used for its declared purpose of increasing school attendance amongst the poor. They also provoke complaints about non-transparent selection procedures and political manipulation in beneficiary selection. While they may not be aware of program outcomes for their municipality, stakeholders do perceive that program implementation deteriorates with second-term mayors

Mayors can choose the institutional set up to implement the Bolsa Escola program, and there are large variations in practices across municipalities. Some of these practices are clearly associated with poor program performance. The question is whether these correlates of poor performance are more prevalent with second than first-term mayors.

Many of the municipal practices that are associated with a higher program impact have to do with greater transparency in program implementation. As can be seen in panel A of Table 5, this includes registering beneficiaries in community buildings and schools as opposed to the mayor's office. It also involves verifying the information provided by parents about their poverty status. In terms of enforcement of program rules, it involves not sending a program coordinator to visit the household as opposed to strict application of rules. And it implies having a social council composed of citizens and public officials that could effectively deliberate implementation procedures. All these practices bear positively on program performance. Panel B of Table 5 shows that these favorable practices in program implementation are systematically less used by second-term mayors, suggesting the channels through which second-term losses in performance occur.

Political rewards to program delivery

The results thus far correspond to a simple model of political agency. Voters care about the program's impact, and thus mayors with re-election incentives exert more effort into program delivery in order to increase their chances of re-election. A natural implication of this model is

that first-term mayors are more likely to get re-elected in municipalities where the program performed better.

Figure 2 shows a non-parametric estimate of the unconditional relationship between program impact and probability of reelection among incumbent mayors in the 2004 election. It indicates a clear upward trend for mayors who have achieved a program impact below -0.091, which corresponds to the top 25% in the distribution of program impacts. We verify the result in a regression framework in Table 6. The electorate may have difficulty recognizing a mayor's program performance (as can be seen from the non-significant coefficients in columns (1) and (2)), unless performance is outstanding. This would be the case for the top 25 percent with the largest program impacts (columns (3) and (4)). We see that such incumbent mayors have a 28 percent greater chance of being re-elected. The electorate can also see how the program was administered, in particular who was selected as beneficiary, and whether children that did not qualify were included (type II error). Here as well, good performance brings electoral rewards. The results in columns (5) and (6) show that mayors with no public denouncements of inclusion errors have a 26 percent higher chance of re-election. When put together in columns (7) and (8), these results are robust.

Local electoral incentives in perspective

In Brazil, mayors are elected by simple majority for a four year term and cannot serve for more than two consecutive terms. Electoral incentives to act in the public's best interest thus apply directly in the first term if they seek reelection. In many countries, electoral incentives can also apply to lame-duck mayors if political parties have the capacity to offer career rewards to office holders (Ferejohn, 1986). In this case, a lame-duck mayor will be rewarded if his political party remains in power, creating incentives to cater to the public interest in the second term as well. There are two features of Brazilian local politics that matter here in interpreting the results. The first is that mayors have considerable autonomy in influencing the use of large amounts of local resources, with little accountability else than through electoral incentives and eventual random audits (Ferraz and Finan, 2008). A mayor's own objectives can thus make a large difference for decentralized program performance. Lack of electoral incentives is a tempting invitation to engage in rent seeking based on control over local resources. The second feature of Brazilian local politics is that political parties have weak capacity to discipline their members at the local level. There is typically a large number of political parties competing for office in municipal elections, and most of these parties are ad-hoc for the current election and personality-centered. In the 2000 elections, for example, 26 parties won at least one municipality. The consequence is that

party incumbency in a mayor's second term is a weak incentive to cater to public interest as opposed to engaging in rent seeking.

In a study of party incumbency at the municipal level in Brazil, Titiunik (2009) finds that mayors' rent seeking behavior in their second term results in the electorate punishing the mayor's party by voting it out of office. She thus finds the paradoxical regularity that party incumbency is a disadvantage for reelection: voters express their dissatisfaction with lame-duck mayors by voting against the incumbent party.

The Titiunik study combined with ours thus creates a compelling interpretation of local electoral incentives and decentralized program performance in Brazil. First term mayors have an incentive to cater to the public interest as assessed by decentralized program performance, but weak incentives to refrain from rent seeking in their second term. Voter dissatisfaction with second term performance results in a strong negative effect for party incumbency.

VIII. Conclusions

In contrast to most other conditional cash transfer programs, Bolsa Escola was decentralized at the municipal level, with local authorities in charge of beneficiary identification and program implementation. The decentralized design offered service providers with better access to information about stakeholders. It also offered citizens the opportunity of holding local politicians accountable for program performance, thereby creating an incentive for politicians with reelection concerns to use their informational advantage to achieve a larger program impact on school dropout. The Bolsa Escola experience thus provides an important yet rare opportunity to understand empirically how electoral incentives and other government practices can affect decentralized program performance.

We find that, while Bolsa Escola had a strong overall impact on beneficiary school attendance, reducing dropout rates by 8 percentage points, municipalities governed by mayors with re-election incentives fared much better. Compared to mayors in their second and final term, first-term mayors achieved a 36 percent gain in program impact. This finding is robust to the introduction of state fixed effects, municipal and mayor characteristics, and local governance practices. It is also robust to comparing the performance of first-term mayors who will win reelection or of those with political experience to that of second-term mayors. In any case, remaining effects on performance due to ability and experience run opposite to the electoral incentive effect, and would induce a downward bias on the estimate of this effect.

Consistent with these findings on program performance, we show that first-term mayors face fewer complaints about inclusion of non-qualifying children, selection procedures, and political manipulation in beneficiary selection. They are also more likely to adopt specific program implementation practices that are not only more transparent but also associated with superior program performance.

In the end, first-term mayors care for program performance because it helps them get re-elected. Mayors with the 25 percent highest program impacts have a 28 percent greater chance of re-election. Those with no public denouncements of inclusion errors have a 26 percent higher probability of being re-elected.

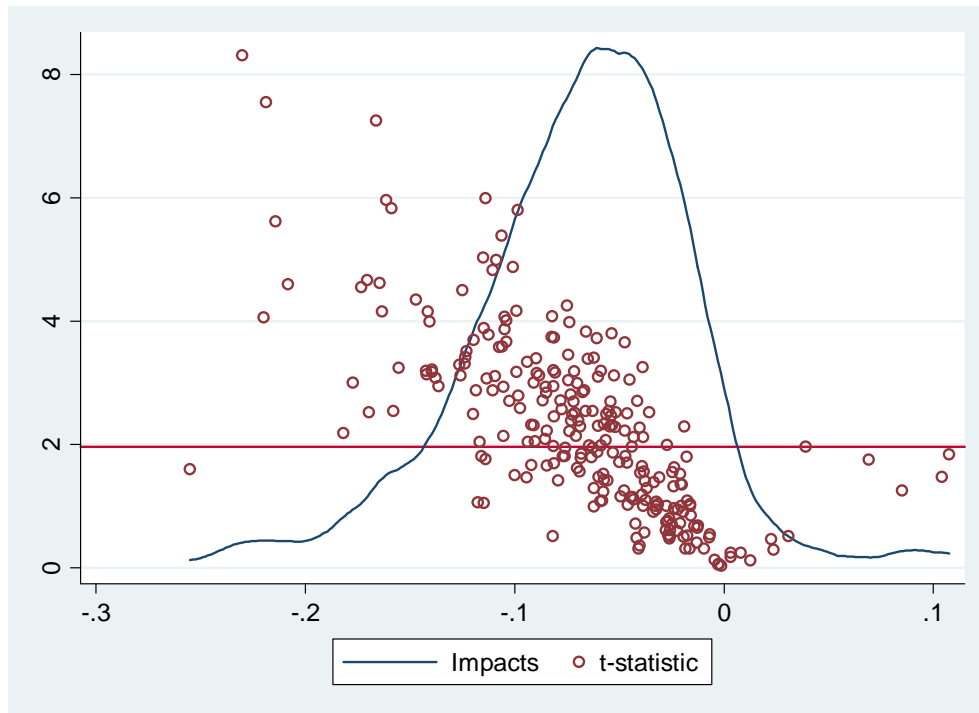
Overall our findings support the proposition that electoral incentives can play a central role in the success of decentralized service delivery. The presence of formal local institutions, particularly electoral rules that enable voters to reward and punish locally-elected officials, is key for reaping the informational gains that decentralization provides. When constitutional rules do not support electoral accountability, introducing other mechanisms of citizen control becomes all the more important.

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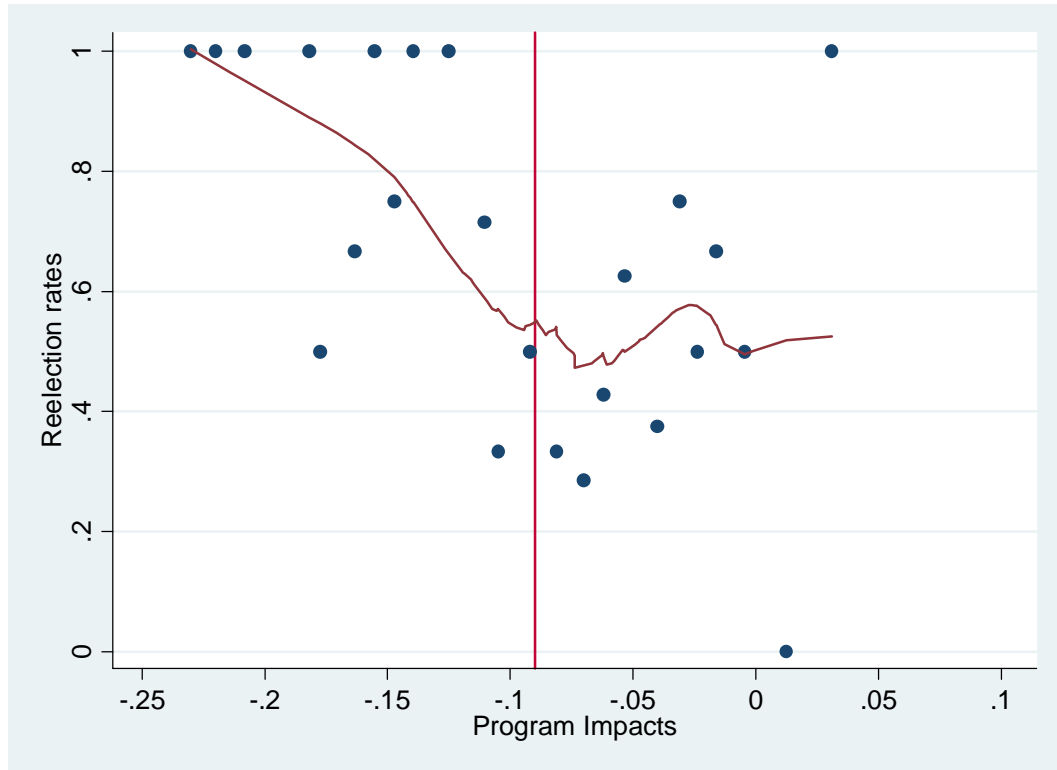
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Figure 1. Distribution of estimated impacts of Bolsa Escola on dropout rates by municipality



Notes: Each circle represents the impact for one municipality, with the point estimate on the horizontal axis and the absolute value of the associated t-statistic on the vertical axis. The horizontal line at $t = 1.96$ delineates the 10 percent significance level. The superposed solid line represents the distribution of the impact point estimates in the sample of municipalities.

Figure 2. Reelection rates by program impact



Notes: The Figure shows reelection rates in 2004 by program impact. The plot presents the proportion of first-term mayors that were re-elected in 2004 for a bin size of 0.01 impact (circles) along with a locally weighted regression calculated with a bandwidth of 0.8. Municipalities to the left of the vertical line were in the top 25 percent in terms of program impact.

Table 1. Dropout rates

Year	Dropout (%)
1999	14.2
2000	15.0
2001	13.2
2002	12.8
2003	13.3
Pre-program (1999 and 2000)	
Bolsa Escola beneficiaries	4.4
Non beneficiaries	17.1

Dropout is defined as dropping out before the end of the school year.

Table 2. Average impact of Bolsa Escola on dropout rates

Dependent variable: Dropout (1/0)	(1)	(2)	(3)	(4)	(5)
Treatment effect	-0.057 [0.003]**	-0.080 [0.004]**	-0.085 [0.004]**	-0.075 [0.004]**	-0.077 [0.008]**
Year intercepts	Yes	Yes	Yes	Yes	Yes
Child intercepts	Yes	Yes	Yes	Yes	Yes
Dropout status in 2000 * Year effects	No	Yes	Yes	Yes	Yes
Dropout status in 1999 * Year effects	No	No	Yes	No	No
Mean of dependent variable	0.137	0.116	0.116	0.114	0.108
Number of children	290517	118234	118234	78737	24630
Observations	604561	344107	344107	229720	67487
R-squared	0.72	0.69	0.73	0.68	0.67

Robust standard errors clustered at the municipality level in brackets. + significant at 10%; * at 5%; ** at 1%

Samples in columns (2) to (5) correspond to the subsamples of children that meet the corresponding specifications, e.g., children in school in 2000, children with at least 2 observations for fixed effect estimation, etc.

Samples in columns (4) and (5) correspond to the municipalities where pre-program trends for beneficiaries and non-beneficiaries are not statistically different (in col. 4) and are greater than -.01 (in col. 5).

Table 3. Effects of electoral incentives on program performance

Dependent variable: Program's impact on dropout rate	(1)	(2)	(3)	(4)	(5)	(6)
Mayor in second-term	0.020 [0.008]*	0.022 [0.007]**	0.021 [0.007]**	0.026 [0.009]**	0.018 [0.010]+	0.020 [0.007]**
Governance practices						
Mayor's spouse is a politician						0.018 [0.010]+
Share of public employees related to the mayor						0.178 [0.062]**
Share of secretariat that are politicians (vs. technicians)						0.020 [0.012]
Municipal characteristics	No	Yes	Yes	Yes	Yes	Yes
Mayor characteristics	No	Yes	Yes	Yes	Yes	Yes
Other municipal characteristics	No	No	Yes	Yes	Yes	Yes
State intercepts	No	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	-0.067	-0.067	-0.067	-0.067	-0.064	-0.067
Observations	236	236	236	193	176	236
R-squared	0.03	0.27	0.31	0.38	0.32	0.34

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

Mayor characteristics include gender, education, number of terms held in a political position, age, and party affiliation dummies. Municipal characteristics include population density (pop/km), number of districts, % rural, % literate population, log per capita income, margin of victory in the previous election, and gini coefficient. Other municipal characteristics include: existence of an NGO, share of children benefited by the program, municipality is a judiciary district, existence of social council, received training, number of radios, number newspapers, public sector employment (as share of population), total number of employees in the mayor's office, and total number of secretariats.

Sample in column (4) restricted to second-term mayors and first-term mayors that will be re-elected in 2004. Sample in column (5) restricted to second-term mayors and first-term mayors with at least 2 terms of political experience in another office.

Table 4. Effects of electoral incentives on public denouncements

Dependent variable:	Complaints about Type II (inclusion) errors	Complaints about the selection process	Complaints of political manipulaiton	Received any of these complaints	Received all of these complaints
	(1)	(2)	(3)	(4)	(5)
Mayor in second-term	0.21 [0.069]**	0.071 [0.067]	0.110 [0.057]+	0.111 [0.057]+	0.097 [0.055]+
Municipal characteristics	Yes	Yes	Yes	Yes	Yes
Mayor characteristics	Yes	Yes	Yes	Yes	Yes
Other municipal characteristics	Yes	Yes	Yes	Yes	Yes
State intercepts	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	0.576	0.788	0.216	0.860	0.165
Observations	236	236	236	236	236
R-squared	0.27	0.16	0.19	0.2	0.16

Robust standard errors clustered at the municipality level in brackets.+ significant at 10%; * at 5%; ** at 1%

Table 5. Effects of program implementation methods on program performance and effects of electoral incentives on choice of program implementation methods

	(1)	(2)	(3)	(4)	(5)
Panel A: Effects of program implementation methods on outcomes					
Dependent variable:	Program performance				
Registered beneficiaries in communities	-0.005 [0.002]*				
Registered beneficiaries in schools		-0.027 [0.012]*			
Receive visit from program coordinator when conditionalities were not met			0.017 [0.008]*		
Verified information				-0.015 [0.007]+	
Proportion of social councils that are deliberative					-0.017 [0.009]+
R-squared	0.29	0.31	0.30	0.30	0.30
Panel B: Effects of electoral incentives on choice of program implementation methods					
Dependent variable:	Registered beneficiaries in communities	Registered beneficiaries in schools	Receive visit from program coordinator	Verified information	Proportion of deliberative social councils
Mayor in second-term	-0.341 [0.213]	-0.041 [0.050]	0.199 [0.072]**	0.02 [0.071]	-0.139 [0.057]*
Mean of dependent variable	0.492	0.849	0.441	0.638	0.645
R-squared	0.2	0.19	0.22	0.19	0.14
Municipal characteristics	Yes	Yes	Yes	Yes	Yes
Mayor characteristics	Yes	Yes	Yes	Yes	Yes
Other municipal characteristics	Yes	Yes	Yes	Yes	Yes
State intercepts	Yes	Yes	Yes	Yes	Yes
Observations	236	232	236	235	236

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

Table 6. Electoral rewards to program performance

Dependent variable: Mayor was reelected in 2004	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Program impact	-1.251 [0.866]	-1.227 [1.296]						
In top 25 percent of program impacts (0/1)			0.234 [0.108]*	0.282 [0.142]+			0.249 [0.107]*	0.248 [0.145]+
Public denouncement for Type II (inclusion) errors					-0.172 [0.106]	-0.261 [0.131]+	-0.191 [0.103]+	-0.224 [0.129]+
Municipal characteristics	No	Yes	No	Yes	No	Yes	No	Yes
Mayor characteristics	No	Yes	No	Yes	No	Yes	No	Yes
State intercepts	No	Yes	No	Yes	No	Yes	No	Yes
Observations	98	98	98	98	98	98	98	98
R-squared	0.02	0.3	0.05	0.33	0.03	0.33	0.09	0.36

Observations are for the 104 municipalities with incumbent mayor in the 2004 elections.

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