Local Electoral Incentives and Decentralized Program Performance

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Abstract

This paper analyzes how electoral incentives affected the performance of a major decentralized conditional cash transfer program intended on reducing school dropout rates among children of poor households in Brazil. We show that while this federal program successfully reduced school dropout by 8 percentage points, the program's impact was 36 percent larger in municipalities governed by mayors who faced reelection possibilities compared to those with lame-duck mayors. First term mayors with good program performance were much more likely to get re-elected. These mayors adopted program implementation practices that were not only more transparent but also associated with better program outcomes.

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 have unique advantages in allocating public resources, such as access to information not available to central authorities (Faguet, 2004).

In practice, however, decentralized program implementation may not meet this expectation. 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 constituents for their efforts (Seabright, 1996; World Bank, 2003). How to achieve this has long been debated (Rose-Ackerman, 1999). The accountability of local governments to program stakeholders requires political institutions that give citizens the ability to discipline elected officials (Persson and Tabellini, 2002). This may be missing. For instance, term limits may curtail incentives for politicians to make efforts in providing high quality local public goods when they can no longer seek re-election (Besley, 2006). Local politicians' performance in program implementation would thus depend on existence of electoral incentives for them to make good use of the informational advantages they possess.

In this paper, we investigate the extent to which local electoral incentives 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 created substantial variation across municipalities in the manner in which the program was implemented and 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 using the constitutional two-term limit for re-election and measuring the difference in achievement between first and second-term mayors. While many studies in constitutional

economics have posited the importance of electoral incentives for effective decentralized services delivery (World Bank, 2003), this study is among the first to support this empirically.

We find that while, on average, the program reduced dropout rates during the school year by 8 percentage points, there was considerable variation across municipalities. Municipalities that happened to be governed by a first-term mayor had an estimated 36 percent higher program performance compared to municipalities governed by a second-term mayor. 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 a comparable level of political experience.

Using municipal level election data, we show that first-term mayors had reason to care about good program performance. The probability of re-election was 28 percent higher for mayors who were in the top quartile of program impacts. Mayors with no public denouncements of inclusion errors were also rewarded with a 26 percent higher probability of re-election. We show that a number of good management practices related to transparency that affect program performance were more frequently associated with first than second-term mayors, indicating how better performance actually came about.

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. Foster and Rosenzweig (2004) show in Indian villages that greater electoral competition in controlling governing bodies and a higher share of poor landless in the voting population increase the allocation of public budgets toward pro-poor local public goods. Besley and Case (1995) found that re-election incentives affect the fiscal policy decisions of U.S. governors. Consistent with a model where incumbents who are willing to run again seek to build their reputations with the electorate through their achievements, governors with re-election possibility make greater efforts to keep taxes and expenditures down relative to those in their final term. In a similar vein, List and Sturm (2006) provide evidence that electoral incentives influence the environmental policy choices of U.S. governors even though these are not frontline policy issues. In states with large groups of green voters, they pursue more environmentally friendly policies when they can be re-elected than when they face binding term limits, and vice-versa in states with small environmental constituencies. Using the outcomes of random audits of municipal finances in Brazil, Ferraz and Finan (2009) have shown that mayors that face re-

election incentives are significantly less corrupt than mayors without such incentives. While both Ferraz and Finan (2009) and this paper analyze the role of local electoral incentives on politicians' performance, the papers differ in a complementary fashion: the first focuses on rents extracted via corruption practices while the second focuses on efforts made in achieving public program performance.

Much of the empirical work on the effectiveness of decentralized services delivery has focused on how decentralized programs are targeted, due in large part to the fact that measures of program performance were unavailable. For instance, Alderman (2002) has shown that the local-level informational advantage was used by local authorities in Albania to better target the poor. Both Galasso and Ravallion (2005), in the case of public food-for-school transfers in Bangladesh, and Bardhan and Mookherjee (2006), in the case of credit and agricultural input kits in West Bengal villages, found that program and village features affected the degree of local pro-poorness in the decentralized targeting. But, ultimately, what we care about is the impact of the program, and targeting may not be a good indicator of program performance (Ravallion, 2007). Our paper, which complements this literature, is the first empirical study to show how the performance of a decentralized program is affected by local institutions, and particularly those that help establish local accountability.

It is fairly well established that politicians can reap electoral rewards from public expenditure programs. Access to government transfers has been shown to increase votes for elected officials who can claim credit for the transfers. Levitt and Snyder (1997) thus demonstrated that public spending is rewarded by voters' electoral support in House of Representatives elections in the US. Manacorda, Miguel, and Vigorito (2009) found that households who benefited from a cash transfer in Uruguay were more likely to give lasting political support to the current relative to the previous government. Our paper extends these results by showing that not only are transfers rewarded, but that the quality of the politician's program performance in terms of its intended effects is rewarded by a higher likelihood of being re-elected. It is because of this link between program performance and votes that political institutions can be such a powerful instrument in enhancing the quality of implementation of decentralized public programs.

The paper is organized as follows. In section II, 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 III, we present the features of the Bolsa Escola program and review results obtained in previous evaluations. In section IV, we explain the data collection and present descriptive statistics on dropout rates across municipalities. We explain the method for measuring program performance 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. In section VIII, we interpret the results first by verifying that electoral rewards do accompany good program performance and second by exploring through which governance practices first term mayors achieve superior program performance. We conclude in section IX on the benefits of electoral incentives for decentralized program performance.

II. A model of local electoral incentives

In this section, we construct a simple political economy model to show how electoral incentives can affect the performance of a decentralized program when its performance depends on efforts made by an elected municipal official. The program is one where municipalities must select beneficiaries among qualifying households, the number of which exceeds the exogenous quota of school stipends that a municipality can allocate. Because qualifying children are heterogeneous in their responses to the offer of a stipend, the impact of the program is largely due to the selection of beneficiaries. The central government's objective is to reduce the school dropout while the local government's objective is re-election with a two-term limit. Voters are informed of and concerned with municipal program performance. The local electorate can thus reward for good performance local politicians who are up for re-election. While improving program performance does require politicians to exert costly effort, it does increase their chances of re-election.[2]

Following the model set up, 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

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[2] The model is an adaptation of the standard political agency model found in Barro (1970), Ferejohn (1986), Banks and Sundaram (1993), and recently Alesina and Tabellini (2007). The basic insight from this class of models is that in a political context in which elections reward politicians' performances, incumbents with reelection possibilities have the incentive to exert effort in the implementation of the program in order to increase their re-election chances.

municipality. The expected program's impact $\theta_i = \theta(z_i)$ on the school performance of child i depends on his characteristics. The program's 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 be willing to 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 $\bar{\theta}(a,t,e,z,B)$, which is influenced by the mayor's ability a, experience t, and effort e, with first derivatives $\bar{\theta}_a$, $\bar{\theta}_t$, and $\bar{\theta}_e$ positive, respectively. The mayor's own utility is also a function of his direct reward R from beneficiary selection that can be written as $R(a,t,e,z,B)$, which is decreasing in effort $R_e < 0$. To simplify further the derivation of the model, we assume that the program impact is $\bar{\theta} = g(z,B) + a + t + e$, 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 the selection of beneficiaries and 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 $a_1 : \Phi(\bar{a}, \sigma_a^2)$, 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 $\bar{\theta}_1$, 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 and he has gained experience t. Otherwise, a challenger comes in with an ability randomly drawn from the distribution of abilities and no experience. In period 2, the mayor chooses again the level of effort to exert, thus determining the impact $\bar{\theta}_2$ of the program for that period.

Because the game ends in period 2, there is no electoral incentive for mayors to exert any effort in the second period. Thus the program's impact in the second period is $g(z,B)+a_1+t$ if the incumbent is reelected, otherwise the expected impact is $g(z,B)+\bar{a}$ otherwise. Unable to induce politicians to exert effort, voters will seek to elect the mayor with the highest combination of ability and experience. The incumbent is reelected if the impact is higher than a threshold W that reveals that he would do better in the future than the average politician.

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

$$\max_e R - \psi(e) + \Pr\left(\bar{\theta}_1 \geq W\right)R.$$

The optimal effort e^* is the solution to $\psi_{e^*} = R\varphi\left(W - g(z,B) - e^*\right)$, where φ is the density function of ability. The mayor sets his effort to equate current marginal cost to the future marginal benefit of being re-elected.

Rational voters know that they should expect this optimal level of effort e^*. They will vote the incumbent in if $\bar{\theta}_1 \geq W = g(z,B)+\bar{a}+e^*-t$, which reveals that $a_1+t \geq \bar{a}$. Substituting the expression for W, the optimal effort of the first term mayor is the solution to:

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

The corresponding program impact of a first-term mayor is:

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

while it is

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

in his second term impact, if he is re-elected. First-term mayors exert effort, while second term mayor have experience.

The average performance across mayors in their first term is:

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

With selection, the average performance of second-term mayors is:

$$\bar{\bar{\theta}}_2 = g(z,B)+t+\int_{\bar{a}-t}^{\infty} a\varphi(a)da.$$

And the difference is:

$$\bar{\bar{\theta}}_1 - \bar{\bar{\theta}}_2 = e^* - \left[\int_{\bar{a}-t}^{\infty} a\varphi(a)da - \bar{a} \right] - t \ .$$

The first term is positive and represents the effort exerted by the mayors in their first term in response to the re-election incentive, while the two other factors are negative, representing the lower average ability and experience of first-term mayors. As this comparison makes explicit, the difference in program performance between first and second-term mayors captures both a selection effect of the elections, as well as an incentive effect from the possibility of re-election. Even though it is difficult to identify each effect separately, a positive difference suggests that the incentive effect induced by the possibility of re-election dominates the selection effect of having second-term mayors with higher ability and more experience. Implicit in our model is the assumption that voters care about the program's impact. Voters, however, may only care about whether they themselves are program beneficiaries. 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 potential swing voters regardless of their children's risk of dropping out of school in order to garner support from this group (Besley and Kanbur, 1993). In this case, the program would perform worse in municipalities with first-term mayors. Here again, observing better first-term program performance has to come from electoral incentives especially if elections induce first-term clientelism games.

III. 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.[3] 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

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[3] In our sample, 98 percent of the children that dropped out at some point during the year registered again in school in some later year.

household.[4] 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 is that it was decentralized at the municipal level. Each municipality received a quota of stipends it could provide to its population. This quota was determined on a formula basis using indicators from the 1996 population census. 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 municipality's responsibility to select program beneficiaries among qualifying households. Transfers to the selected beneficiaries were made directly by Brasilia to their bank accounts at the program's set level with no municipal discretion. Municipalities were entrusted with enforcing the school attendance conditionality.[5] The program was implemented across all of Brazil in 2001 and incorporated in 2004 into the current Bolsa Familia program (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. These results are of the same order of magnitude as those which we report here even though 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/Familia once extended at the national scale 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.[6]

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[4] As a comparison, the minimum wage was R240 per month.

[5] Every three months, teachers were required to compile the monthly attendance of program beneficiaries and send them to the Secretary of Education of the municipality and Brasilia. It is from these attendance sheets that the conditionality was imposed.

[6] Bourguignon, Ferreira, and Leite (2003) provided an ex-ante evaluation of Bolsa Ecola. 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.

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 for the 2001-2003 period. We then use these measures of municipal performance to analyze the importance of electoral incentives and of local governance practices on observed outcomes. Identification of electoral incentives on performance is provided by the fact that introduction of Bolsa Escola in 2001 happened exogenously relative to the first or second term status of the current mayor. Electoral outcomes are observed in the subsequent municipal election held in 2004.

IV. Data and descriptive statistics

Data collection for the project 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.[7] In each of the 261 municipalities, two data collection instruments were applied – compilation of school records and a municipal survey –, 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 dropout, we collected in each municipality school records for at least 500 children over the 1999-2003 period.[8] 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 in the school was compiled from the annual class reports filled in by teachers. Matching records across school years was done manually using children's first and last names, allowing us to match 85

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[7] 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.

[8] The school year in Brazil runs from February to December. We thus observe two complete years before the program (1999 and 2000) and three complete years with the program (2001 to 2003).

percent of the collected records.[9] 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.[10]

In the class reports, which are compiled at the end of each school year, 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.[11] Thus, our measure of dropout indicates any child that did not complete the school year, but neither transferred to another school nor died. It is not related to the attendance records reported to the Bolsa Escola administration.

Although administrative records have the advantage of providing a large data set with greater accuracy than self-reported information on attendance and grade promotion, not having conducted household interviews results 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, lack of information prevents 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 who dropped out had transferred. In the analysis that follows, we simply removed these children from the sample.

Table 1 demonstrates the magnitude of the school dropout problem that Bolsa Escola was designed to address. It presents the school dropout rates of beneficiaries and non-beneficiaries by year for the sample of children who were enrolled both in 1999 and 2000, which represents our

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[9] 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.

[10] The target was to have at least 500 current students per municipality, which led to an average of 1100 students in the school records as many schools are large and we collected information on all students attending school at any point in time during the 1999-2002 period.

[11] The class records are not electronic and not shared with Bolsa Escola. They do not report on daily school attendance to meet Bolsa Escola conditionality, where cheating could occur.

main estimation sample. In 2000, the year before the program started, 9.3 percent of non-beneficiary children dropped out of school during the course of the year (see column 3). This percentage increases over time and by 2003, 15.4 percent of the sample had dropped out at some point during the school year. The steady increase over time reflects the fact that these children are getting older and are much more likely to dropout at higher grades.

The dropout behavior of non-beneficiaries stands in stark contrast to that of program beneficiaries whose dropout rates range from 2 to 6.1 percent during the period. This comparison not only provides some initial insights into the program's impact, but also reveals the extent of selection that occurred in the program's targeting. For instance, only 3.1 percent of the program beneficiaries had dropped out in the year prior to introduction of the program.

Given that the program was targeted to children with lower dropout propensities, the use of child fixed effects in estimating the program's impact becomes critical to account for this initial selection. This can be seen clearly in columns 4 and 5 of Table 1. In column 4, we present the unadjusted difference-in-differences estimate by year. Although the estimates during the post-program years suggest that Bolsa Escola reduced dropout rates among program beneficiaries by about 5 percentage points, we also see a 2.4 percentage points decline even prior to the program which raises concerns for our counterfactual assumption of parallel trends. However, in column 5, we see the importance of including child fixed-effects. Under this specification, there is no difference in the change in dropout rates between beneficiaries and non-beneficiaries prior to introduction of the program. Moreover, the difference is not only statistically insignificant but small in magnitude (point estimate = -0.002; robust standard error = 0.002). Given the tightness of the confidence intervals, we can reject impacts of -0.006. This is an order of magnitude smaller than the estimated impact of the program during the post-program years, which are large and statistically significant, ranging from -0.065 to -0.091.

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.[12] We therefore consider all the children as eligible. As we will see later, the identification strategy does not depend on strict eligibility of all children.

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 in 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 can safely be assumed to be the same as in the previous 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 records that we could not match with the Bolsa Escola list, and we could not match 25% of the Bolsa Escola list into school records. 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 and mayor characteristics and governance practices, and implementation methods for 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 the 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.

Overall we found considerable variation across municipalities in the procedures used to identify and register potential beneficiaries. Differences relate to where the registration of potential beneficiaries took place, whether efforts were made to verify the information given by parents, and whether the municipality had social councils that could engage in deliberating

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[12] 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 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.

program implementation.[13] The number of potential beneficiaries largely exceeded the quota of stipends that was allocated to the municipality by the federal 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. There was also variation in implementing conditionalities across municipalities. While most municipalities reported monitoring the school attendance conditionality, this was done more strictly in some municipalities than others. Overall, our survey revealed 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 governance practices. For instance, in our sample of municipalities, we found that in 25 percent of the municipalities, more than 15 percent of the employees in the mayor's office were related to the mayor indicating nepotism, and in 12 percent of the municipalities, the mayor's spouse was also an elected politician. Across municipalities, 47 percent of the administrative positions were held by political appointees rather than technocrats.[14]

In Table 2, we examine how these governance practices as well as other characteristics of the municipality differ across those municipalities governed by a first-term mayor (column 1) versus a second-term mayor (column 2). Because of the need to assume selection on observables in our second-stage analysis, it is important to understand whether characteristics that are likely to affect program performance also differ by the mayor's term of office. As the table demonstrates, there are few differences in observable characteristics between these municipalities. Out of the 24 controls that we use for our analysis, there are only 3 characteristics which are statistically different at the 10 percent level (see column 3). As expected, second-term mayors have much more terms of experience and a larger margin of victory in the 2000 elections. We also see that second-term mayors exhibit more nepotism as they are more likely to hire their own relatives to work in the mayor's office. Aside from these differences, the other characteristics of both the mayor (e.g., gender, education) and of the municipality (e.g., income per capita, income inequality, and access to media) are all balanced.

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

[14] 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).

In Table 2, we also examine differences in pre-program dropout propensities between beneficiaries and non-beneficiaries and whether these differences differed between first and second-term mayors. To measure a child's propensity to dropout prior to the program, we estimate the following model of school dropout, S_{ijt}, for child i in municipality j in period t:

$$S_{ijt} = \phi_{ij} + \gamma_{1j}T_{00} + \gamma_{2j}\left(S_{i99} \times T_{00}\right) + \varepsilon_{ijt} \text{, for } t\text{=1999 and 2000,}$$

where T_{00} is a time dummy for the year 2000, S_{i99} is child i's 1999 dropout status, and ε_{ijt} is a random error term. We then define the municipal-specific child fixed-effect, ϕ_{ij}, as child i's propensity to drop out of school prior to introduction of the program.

The difference in dropout propensities between beneficiaries and non-beneficiaries is only slightly lower among first-term mayors relative to second-term mayors, and this difference is not statistically significant. We also do not find any difference in pre-program dropout rates between first and second-term mayors.

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 the data and research design, the impact of the program on beneficiaries can be estimated for every municipality in the sample. It is this program impact that provides a measure of municipal program performance for the analysis of electoral incentives.

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

We start with a standard difference-in-differences model where schooling outcome is modeled as:

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

where S_{ijt} is schooling outcome (dropout), ϕ_{ij} is an individual fixed effect, X_{ijt} are time varying observables including year fixed effects, and P_{ijt} is an indicator for program participation.[15] 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_{ijt}.

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

$$\bar{\theta}_j = E\left(\theta_{ij}\middle|B_{ij}=1\right), \tag{2}$$

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. Municipal treatment effects may thus differ due to both their selection of beneficiaries from amongst the population and the distribution of individual effects θ_{ij} in their eligible population.

The validity of this identification strategy relies on the assumption that, conditional on individual fixed-effects, the trend in future dropout behavior of beneficiaries and non-beneficiaries would be the same in the absence of the Bolsa Escola program. 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 had started. The main concern is that the selection decision was based on pre-program schooling outcomes.

To address this concern, we include in X_{ijt} differential time effects by pre-program dropout histories S_{ij0} which are the best predictors of future dropout behavior. The estimation of $\bar{\theta}_j$ can be obtained by rewriting equation (1) as follows:

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

where $\mu_{ijt} = \left(\theta_{ij} - \bar{\theta}_j - \theta^*\left(S_{ij0} - \bar{S}_{j0}\right)\right)P_{ijt} + \varepsilon_{ijt}$, $\theta_j^* = E\left(\theta_{ij}\middle|B_{ij}=1, S_{ij0}=1\right) - E\left(\theta_{ij}\middle|B_{ij}=1, S_{ij0}=0\right)$, $\bar{S}_{j0} = E\left(S_{ij0}\middle|B_{ij}=1\right)$, and β_{jt} and $\gamma_{jt}S_{ij0}$ denote different sets of year effects.

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

The estimator is consistent if $\mathrm{E}\left(\left(\theta_{ij} - \bar{\theta}_j - \theta^*\left(S_{ij0} - \bar{S}_{j0}\right)\right)P_{ijt} + \varepsilon_{ijt}\middle|\phi_{ij},\beta_{jt},\gamma_{jt}S_{ij0},P_{ijt}\right) = 0$.

The first term $\mathrm{E}\left(\left(\theta_{ij} - \bar{\theta}_j - \theta^*\left(S_{ij0} - \bar{S}_{j0}\right)\right)P_{ijt}\middle|\phi_{ij},\beta_{jt},\gamma_{jt}S_{ij0},P_{ijt}\right)$, 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 $\mathrm{E}\left(\varepsilon_{ijt}\middle|\phi_{ij},\beta_{jt},\gamma_{jt}S_{ij0},P_{ijt}\right) = 0$.

This specification thus provides an unbiased estimate of the *TT* effect when the selection of beneficiaries is based on an individual's fixed characteristics, time fixed effects specific to the pre-treatment dropout histories, 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 within-person comparisons of changes in dropout rates with that of non-beneficiary children of the municipality with the same pre-program dropout status.[16] In the empirical analysis, pre-program status is observed in both 1999 and 2000. With this approach, we estimate an average *TT* impact for each municipality.

To further justify our identification assumption, we present two additional robustness tests. First, we re-estimate the model in equation (1) matching on beneficiaries and non-beneficiaries with the exact same pre-program dropout histories (Card and Sullivan, 1988). While similar to equation (3), this model relaxes the functional form assumption and restricts the estimates to areas of common support. Second, with two years of pre-program data, we 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:

$$S_{ijt} = \alpha_{ij} + \gamma_j T_{00} + \theta_j B_{ij}T_{00} + \mu_{ijt} \text{, for } t = 1999,\ 2000\,, \tag{4}$$

where B_{ij} represents the status of future beneficiary and T_{00} is a dummy variable for year 2000. This allows us to not only test for differential trends in pre-program dropout behavior between beneficiaries and non-beneficiaries, but also to test whether the program effects are different when we restrict the estimation sample to those municipalities without differential pre-program trends. Thus, our final specification estimates the program effects conditional on child fixed-

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[16] 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.

effects, and differential time effects by pre-program dropout histories, and for the subset of municipalities where pre-program trends in dropout behavior were not different between beneficiaries and non-beneficiaries.

VI. The impact of Bosla 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 3 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 as reported in the school records. 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. But, as we discussed above, a causal interpretation of this estimate assumes that program beneficiaries were not selected based on pre-program dropout behavior.

In columns (2) to (5), we relax this assumption by extending the fixed-effects model to allow for children with different pre-treatment dropout status to experience different year effects (equation (3) above). In column (2), the model allows for different year effects based on dropout status in 2000 (the year before the program started), whereas the model presented in column (3) allows for time effects to vary with both child 1999 and 2000 dropout status. Column (4) presents a model that allows for separate time effects for each of the four possible combinations of dropout histories in 1999 and 2000. Column (5) presents the most flexible specification. Here we estimate the model presented in column (1) separately for each of the four dropout histories and compute the overall treatment effect by weighting the results for each of the individual dropout histories by the sample fractions.

Overall, the estimates suggest that Bolsa Escola reduced dropout rates by around 8 percentage points for its selected beneficiaries. The point estimates are highly statistically significant and change little across the various specifications. The results imply that if it were not

for the program, we would have observed a dropout rate of 12 percent instead of the 4 percent observed among beneficiaries in 2001-2003. The program thus induced a substantial 66% percent decline in dropout.

Columns (6) and (7) present further robustness checks by restricting the sample to municipalities where pre-program trends verify the assumption underlying the identification. In column (6), 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 (7), 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 columns (2) to (5).

To further verify that the program's impact is not confounded with possible differential quitting rates for beneficiaries and non-beneficiaries, we estimated the treatment effect on the sub-sample of 16564 children enrolled in 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 (not reported in the table) are quite similar, with an estimated *TT* effect of -5.6 percentage points in spite of a very different population sample.

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 us estimate an impact $\bar{\theta}_j$ of the program for each municipality *j*. 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 3 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 are negative. In addition to the distribution of unbiased estimates of impact, Figure 1 plots the absolute values of the corresponding t-statistics. Each 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.[17] 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 incentives affect program performance across municipalities.

VII. Impact of electoral incentives 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 happened to be serving in his first term (and hence could be re-elected) to those where the mayor happened to be serving in his second (and thus final) term at the time when the program was introduced. Although the difference in program impacts between first and second-term mayors will capture the effects of re-election incentives, as we discussed in Section II, it is 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 that we will underestimate the reduction in dropout rate. [18] 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, estimates will again be upwardly biased. To see this more explicitly, consider the following regression model:

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

where R_j is a dummy variable equal to 1 if the mayor is in his first term, the X_j are municipal and mayor characteristics, and β^* is expected to be negative if first-term mayors obtain a higher reduction in dropout. Thus assuming that γ_1 and γ_2 are negative, then $\beta > \beta^*$, where β is the

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[17] As can be seen in Figure 1, 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.

[18] This assumes that elections create a positive selection on high ability mayors. If, on the other hand, elections select the most populist candidates and populism was negatively correlated with ability then second-term mayors would be on average less able than first-term mayors. In this case, we would be over-estimating the effects of re-election incentives on program performance. While this is a possibility, the results that we present both in this section and in the following one are inconsistent with this assumption.

first-term effect on performance without controlling for ability and experience. Not controlling (or controlling incompletely) for ability and experience would thus yield an upward bias. The favorable effect of first-term on the decline in dropout rate would be under-estimated.

These biases may not be of concern in identifying the role of electoral incentives on performance as they affect performance in the opposite direction. However, to account for these potential biases, we follow Ferraz and Finan (2009) and employ two strategies. First, we compare second-term mayors to the set of first-term mayors who were 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 terms of political experience in another office.

There is another confounding factor for which we cannot control, but it would again increase second term performance, running opposite to our electoral incentives model. Mayors may control other resources that they can use to compensate qualifying households who did not receive a Bolsa Escola stipend in a way that induces them to stay at school. This would lower the estimated impact of Bolsa Escola on drop out. If first-term mayors are more likely to compensate non-beneficiaries because of re-election incentives, we would be under-estimating the impact of electoral incentives on performance.

Table 4 presents regression results from estimating several specifications based on equation (5), and where the sample is restricted to the set of municipalities for which we have a full set of covariates.[19] The dependent variable in each regression is the program's impact on

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[19] We purposefully chose to first estimate the program impacts at the municipality level and then estimate the impact of mayors' electoral status on these impacts, 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 $S_{ijt} = \phi_{ij} + \left(\theta_1 + \theta_2 R_j\right)P_{ijt} + \mu_{ijt}$, 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 $S_{ijt} = \phi_{ij} + \theta_j P_{ijt} + \mu_{ijt}$. Although the OLS estimators of these two models need not yield the same estimates, as they verify $\bar{S} = \bar{\phi} + \hat{\theta}_1 \bar{P} + \hat{\theta}_2 \overline{RP}$ and $\bar{S} = \bar{\phi} + \left(\hat{\theta}_1 + \hat{\theta}_2 \bar{R}\right)\bar{P}_j$, respectively, our results are similar using either approach. Moreover, because this two-step estimator will induce heteroskedasticity in the second-stage estimation, we allow for an arbitrary form of heteroskedasticity in computing our standard errors (Hanushek 1974).

dropout rate for a particular municipality, which we estimated based on the model presented in equation (3).[20] Column (1) reports the unadjusted relationship between program impact and whether the mayor happens to be in his first term which shows a 2 percentage points gain in reducing dropout. Columns (2) and (3) include additional sets of controls. In column (2), we account for differences in municipal and mayor characteristics and introduce state fixed effects. 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 which is statistically significant at the 1% level. This stability of the parameter to the addition of many mayor and municipal characteristics gives confidence that the estimated effect is likely not due to omitted variable bias. At an average impact of -6.7 percentage points across all municipalities – with -7.9 percentage points in municipalities with first term mayors and -5.9 percentage points in municipalities with second term mayors – this 2.1 percentage points gain in program performance corresponds to a 36 percent better performance for first term mayors.

In columns (4) and (5), we examine the extent to which political ability and experience may be biasing our results. Column (4) reports the results from the comparison of first-term mayors that were re-elected in the subsequent 2004 elections to the subset of second-term mayors. 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) 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 to second-term mayors. 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 4 introduces governance practices that are correlated with program impact. The program performs worse in municipalities with higher levels of nepotism or

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[20] Specifically, we estimate the program's impact separately for each municipality, controlling for time and child fixed-effects and the child's 1999 and 2000 dropout status interacted with time dummies. The average impact of the program using this specification, which is more flexible than those presented in Table 3, and for this sample of municipalities is -0.067.

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 (positive but not significant). Even though these governance practices are slightly more common among second-term mayors (see Table 2), we find that even after accounting for these symptomatic features of bad governance the effects of re-election incentives remains unaltered.

As an additional test of robustness, we also compare first-term mayors to the set of second-term mayors that became candidates in the subsequent 2006 and 2008 elections.[21] If re-election incentives explain the difference in program performance between first and second-term mayors, then we also would expect second-term mayors who were anticipating on running for other political offices to behave similarly to first-term mayors. Although we unfortunately do not have a measure of a mayor's ex-ante desire to run for other political offices, we do find that second-term mayors who did run for other political offices (ex-post) had similar program performances to that of first-term mayors. For instance, conditional on running in the 2006 and 2008 elections, the difference between first-term and second-second mayors is only -0.003 (compared to -0.020 in our main specification). Although this result should be interpreted with caution given that the program's impact may have also induced some second-term mayors to run for office, it provides further evidence consistent with a re-election incentive interpretation.

VIII. Interpreting the results

In interpreting the results, there are two issues that need to be verified. The first is that there are indeed electoral rewards to good program performance, suggesting that the electorate is informed about performance and cares about it in casting their votes. The second is to explore what do first-term mayors do to achieve a higher performance.

Electoral rewards to good program performance

The results thus far correspond to a simple model of political agency. It requires that voters are informed of and care about the program's impact, and that mayors with re-election

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[21] Ten percent of mayors in our sample ran in the 2006 elections for state and federal deputies, senators, and governors. Twenty-five percent of the mayors were candidates in the 2008 elections for mayor and local councilmen.

incentives exert effort on program delivery in order to increase their chances of re-election. A prediction of this model is that first-term mayors are more likely to get re-elected in municipalities where the program performed better. Clearly, re-election responds to the quality of performance in a broader set of public functions which are presumably correlated with effective management of the Bolsa Escola program (Ferraz and Finan, 2008).

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 -0.09, which corresponds to the top 25 percent in the distribution of program impacts.

We verify this result in a regression framework in Table 5. 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 mayors with the largest program impacts (columns (3) and (4)). We see that these incumbent mayors have a 28 percent greater chance of being re-elected. The electorate can also see how the program was administered, in particular whether children that did not qualify were included (type II error).[22] 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. We can thus conclude that re-election is associated with good performance in managing Bolsa Escola in accordance with federal program objectives, likely symptomatic of good performance in broader public functions as well. The electorate thus appears to be informed about program performance and concerned with it in casting their votes in local elections.

What do first-term mayors do to achieve a higher performance?

Mayors have considerable leeway in choosing 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 better and worse program performance. The question is whether the positive correlates of performance are more prevalent with first than second-term mayors, and correspondingly the negative correlates less prevalent.

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[22] Our measure of public denouncement is an indicator for whether or not a member of the community had complained to the secretary of education that a household above the income threshold is benefitting from the program. Fifty-eight percent of the municipalities in our sample experienced a complaint.

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 6, this includes registering beneficiaries in schools as opposed to less neutral sites such as the mayor's office. It also involves verifying the information given by parents about their self-declared poverty status. And it implies having social councils composed of citizens and public officials that can effectively deliberate implementation procedures. All these practices bear positively on program performance, reducing the dropout rate. In terms of enforcement of program rules, the practice associated with lower dropout rates involves not sending a program coordinator to visit the household when conditionalities are not met as opposed to strict application of rules. Panel B of Table 6 shows that these favorable practices in program implementation (e.g., a deliberative social council) are used more frequently by first-term mayors, and unfavorable practices (coordinator visits) less frequently, suggesting the channels through which first-term gains in performance occur.

IX. Conclusion

Bolsa Escola, subsequently incorporated in the current Bolsa Familia, was a decentralized conditional cash transfer program aimed at reducing school drop-out rates among the children of the poor in Brazil, with municipal authorities in charge of program implementation. The federal government thus sought to achieve higher program performance by entrusting local politicians with the selection of beneficiaries and the enforcement of rules. The expectation is that this gives politicians the opportunity of using local information and citizens the opportunity of holding local politicians accountable for program performance, thereby creating an incentive for politicians with re-election concerns to increase effort in achieving high program performance. The Bolsa Escola experience is thus symptomatic of a broad trend toward increasing decentralization in the provision of local public goods. Because the program was introduced exogenously relative to whether the local mayor was in his first or terminal second term, it provides a rare opportunity to understand empirically how local electoral incentives offered by re-election 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 greater program impact. This finding is robust to the introduction of state fixed effects, municipal and mayor characteristics, and indicators of local governance practices. It is also robust to comparing the performance of first-term mayors who will win re-election, or of those with similar political experience, to that of second-term mayors. In addition, 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. The same applies to other factors that can affect mayors' performance such as compensating non-beneficiaries with other resources and seeking first-term clientelistic gains in selecting children.

Consistent with model predictions, we see that superior performance is rewarded in elections, indicating that the electorate is informed of and concerned with program performance in casting their votes at re-election time. Mayors with the 25 percent highest program impacts have a 28 percent greater chance of re-election. Those with no public denouncements of illegitimate inclusion of beneficiaries have a 26 percent higher probability of being re-elected. First-term mayors are more likely to adopt specific program implementation practices that are not only more transparent but also associated with superior program performance.

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

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Figure 1. Frequency distribution and t-statistics of estimated impacts of Bolsa Escola on dropout rates by municipality
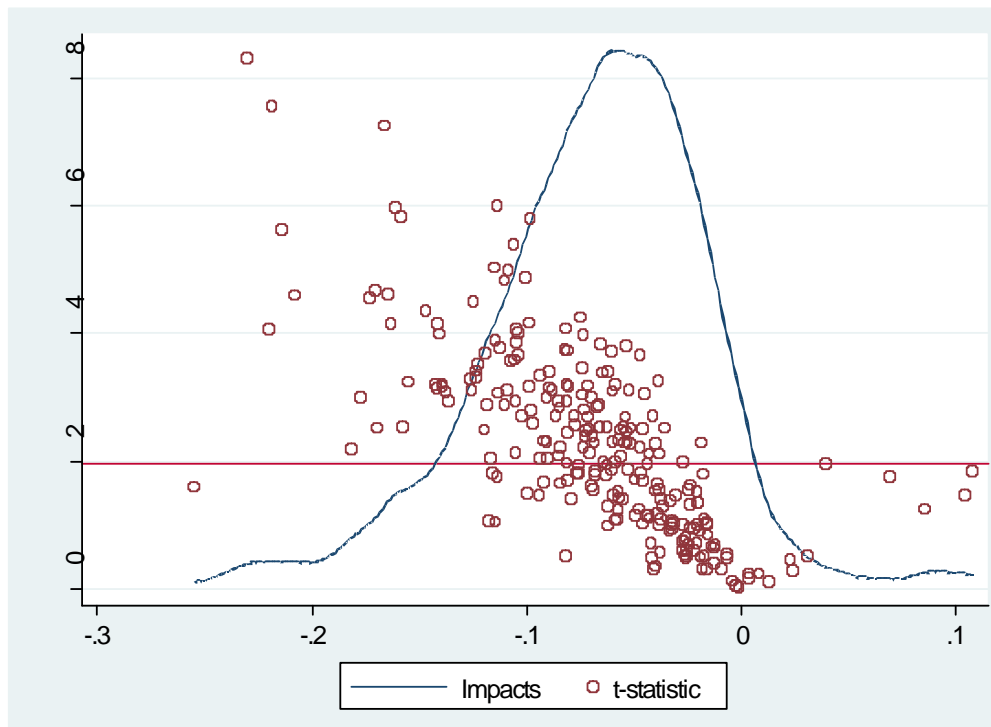
Notes: Each circle represents the impact for one municipality, with the point estimate on the horizontal axis and the absolute value of the associated t-statistic on the vertical axis. The horizontal line at t =1.96 delineates the 5 percent significance level. The frequency distribution is of the impact point estimates in the sample of municipalities.

Figure 2. Reelection rates by program impact
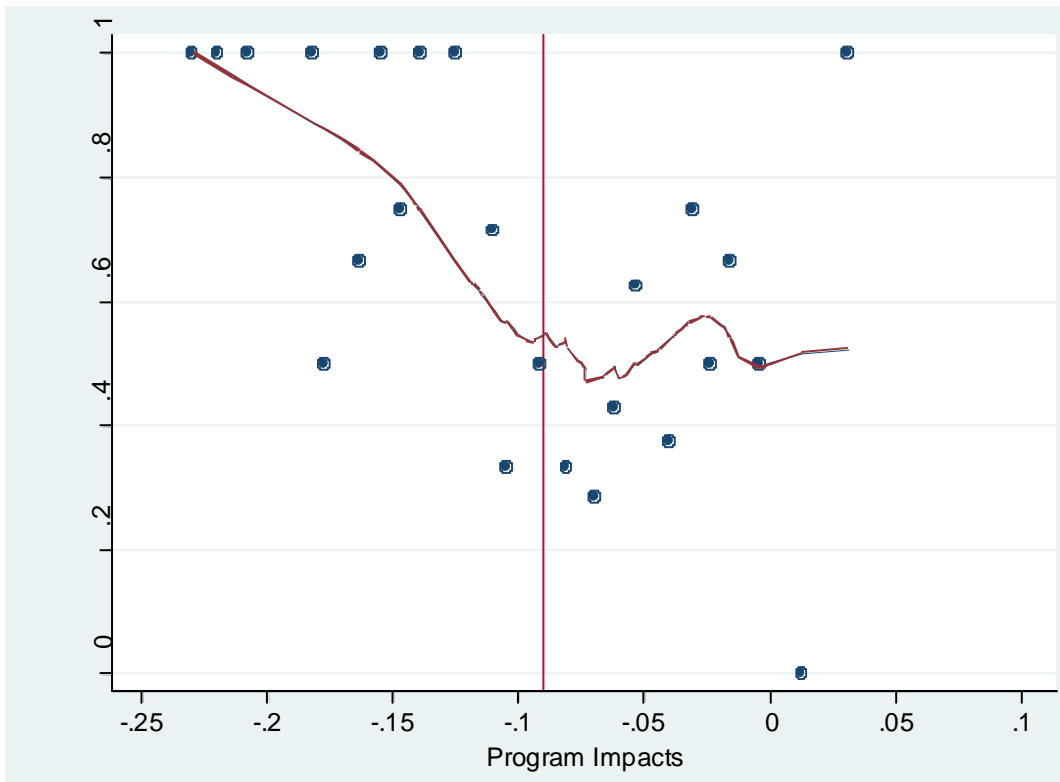
Notes: The figure shows reelection rates in 2004 by program impact. The plot presents the proportion of first-term mayors that were re-elected in 2004 for a bin size of 0.01 impact (circles) along with a locally weighted regression calculated with a bandwidth of 0.8. Municipalities to the left of the vertical line were in the top 25 percent in terms of program impact.

Table 1. Summary statistics on dropout rates

Year	Proportion o beneficiaries dropout (1)	Dropout rates Beneficiary (2)	Non-beneficiary (3)	Difference-in-differen (base year = 1999) OLS (4)	Difference-in-differen (base year = 1999) Fixed-effects (5)
1999	0.182	0.020	0.058		
2000	0.228	0.031	0.093	-0.024 [0.003]	-0.002 [0.002]
2001	0.236	0.026	0.112	-0.048 [0.003]	-0.065 [0.002]
2002	0.246	0.040	0.133	-0.055 [0.003]	-0.084 [0.002]
2003	0.256	0.061	0.154	-0.055 [0.004]	-0.091 [0.003]

Notes: Dropout is defined as dropping out before the end of the school year. The sample includes 118,234 children who were enrolled both in 1999 and 2000. "Beneficiaries" refers to children who benefited from the program in 2001-03. Column 4 reports the difference-in-differences estimates by year using 1999 as a base year. Column 5 reports the difference-in-differences estimates by year using 1999 as a base year with child fixed-effects.

Table 2. Summary statistics of mayors and municipalities

	First-term mayor (1)	Second-term mayor (2)	P-value of difference (3)
Mayor characteristics			
Male	0.882	0.939	0.17
Has a primary education	0.848	0.815	0.55
Has a high school education	0.818	0.745	0.22
Has a college education	0.603	0.505	0.18
Age of mayor	0.482	0.490	0.46
Terms of experience	2.295	2.705	0.02
Margin of victory in 2000 elections	0.146	0.221	0.00
Municipal characteristics			
Population density	0.001	0.001	0.61
Number of districts	0.029	0.032	0.27
Share of rural population	0.444	0.475	0.23
Literacy rate	0.676	0.668	0.34
Income per capita (logs)	4.226	4.185	0.15
Gini coefficient	0.526	0.518	0.48
An NGO exists	0.315	0.350	0.65
Share of population employed by public sector	0.043	0.043	0.53
Judiciary district	0.618	0.553	0.20
Number of radio stations	0.680	0.645	0.85
Has local newspapers	0.209	0.130	0.54
Administrative characteristics			
Received Bolsa Escola training	0.205	0.218	0.91
Bolsa Escola council exists	0.790	0.819	0.78
Wife is politician	0.158	0.099	0.27
Degree of nepotism	0.011	0.021	0.09
Share of administrative positions held by a political appointee	0.441	0.503	0.24
Number of secretaries	0.159	0.177	0.32
Program implementation			
Average dropout rates prior to the program	0.146	0.136	0.41
Difference in pre-program dropout propensities	-0.114	-0.106	0.18
Registered beneficiaries in schools	0.861	0.832	0.78
Received visit from program coordinator	0.286	0.533	0.00
Verified information	0.608	0.657	0.00
Proportion of deliberative social councils	0.700	0.609	0.00
Number of municipalities	98	138	

Notes: This table presents a comparison of the mean political, mayor, and socio-economic characteristics of municipalities with first and second-term mayors. Column 1 reports the means for the 98 municipalities with a first term mayor. Column 2 reports the means for the 138 municipalities with a second term mayor. Column 3 reports the p-value of the test of difference in means using state fixed effects.

Table 3. Average impact of Bolsa Escola on dropout rates

Dependent variable: Dropout (1/0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment effect	-0.057	-0.080	-0.085	-0.085	-0.096	-0.074	-0.092
	[0.003]**	[0.004]**	[0.004]**	[0.005]**	[0.004]**	[0.008]**	[0.016]**
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Child fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dropout status in 2000 * Year effects	No	Yes	Yes	n/a	n/a	n/a	n/a
Dropout status in 1999 * Year effects	No	No	Yes	n/a	n/a	n/a	n/a
Dropout history * Year effects	No	No	No	Yes	n/a	Yes	Yes
Mean of dependent variable	0.137	0.116	0.116	0.116	0.116	0.114	0.150
Number of children	290517	118234	118234	118234	118234	78737	16437
Observations	604561	344107	344107	344107	344107	229720	33308
R-squared	0.72	0.69	0.73	0.73	n/a	0.68	0.77

Notes: This table reports the effects of Bolsa Escola on dropout rates. The sample in columns (2) to (5) is restricted to children that were enrolled in school in 1999 and 2000. Samples in columns (6) and (7) correspond to municipalities where differences in pre-program dropout trends for beneficiaries and non-beneficiaries were not statistically different (in col. 6) and greater than 0 (in col. 7). Robust standard errors clustered at the municipality level in brackets. + significant at 10%; * at 5%; ** at 1%.

Table 4. Effects of electoral incentives on program performance

Dependent variable: Program's impact on dropout rate	(1)	(2)	(3)	(4)	(5)	(6)
Mayor in first term	-0.020	-0.022	-0.021	-0.026	-0.018	-0.020
	[0.008]*	[0.007]**	[0.007]**	[0.009]**	[0.010]+	[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 fixed effects	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

Notes: This table reports the effects of re-election incentives on program performance. 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 a 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 5. Electoral rewards and program performance

Dependent variable: Mayor was reelected in 2004	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Program impact	-1.251	-1.227						
	[0.866]	[1.296]						
In top 25 percent of program impacts (0/1)			0.234	0.282			0.249	0.248
			[0.108]*	[0.142]+			[0.107]*	[0.145]+
No public denouncement of Type II (inclusion) errors					0.172	0.261	0.191	0.224
					[0.106]	[0.131]+	[0.103]+	[0.129]+
Municipal characteristics	No	Yes	No	Yes	No	Yes	No	Yes
Mayor characteristics	No	Yes	No	Yes	No	Yes	No	Yes
State fixed effects	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

Notes: This table reports the effects of program performance on re-election outcomes. Observations are for the municipalities with incumbent mayor in the 2004 elections. Robust standard errors in brackets. + significant at 10%; * significant at 5%; ** significant at 1%. Municipal and mayor characteristics as defined in footnote of Table 4.

Table 6. Electoral incentives, program implementation, and program performance

	(1)	(2)	(3)	(4)
Panel A: Program implementation and performance				
Dependent variable:		Program performance		
Registered beneficiaries in schools	-0.027 [0.012]*			
Verified information given by parents		-0.015 [0.007]+		
Proportion of social councils that are deliberative			-0.017 [0.009]+	
Received visit from program coordinator when conditionalities were not met				0.017 [0.008]*
R-squared	0.31	0.30	0.30	0.30

Panel B: Electoral incentives and program implementation

Dependent variable:	Registered beneficiaries in schools	Verified information	Proportion of social councils that are deliberative	Received visit from program coordinator
Mayor in first term	0.041 [0.050]	-0.020 [0.071]	0.139 [0.057]*	-0.199 [0.072]**
Mean of dependent variable	0.849	0.638	0.645	0.441
R-squared	0.19	0.19	0.14	0.22
Control for both panel estimations				
Municipal characteristics	Yes	Yes	Yes	Yes
Mayor characteristics	Yes	Yes	Yes	Yes
Other municipal characteristics	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Observations	232	235	236	236

Notes: Panel A reports associations between program implementation methods and program performance. Panel B reports the effects of re-election incentives on choice of program implementation method. Robust standard errors in brackets. + significant at 10%; * significant at 5%; ** significant at 1%. Municipal and mayor characteristics as defined in footnote of Table 4.