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The Impact of the *Bolsa Escola/Familia* Conditional Cash Transfer Program on Enrollment, Drop Out Rates and Grade Promotion in Brazil

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Abstract

This paper examines the impact of Brazil's *Bolsa Escola* (later renamed *Bolsa Familia*) program on children's progress in school in Brazil. The *Bolsa* program, which started in the 1990s and expanded rapidly in 2001 and 2002, provides monthly cash payments to poor households if their children (between the ages of 6 and 15) are enrolled in school. Using eight years of school census data (from 1998 to 2005), our estimation method compares changes in enrollment and in dropout and grade advancement rates across schools that adopted the *Bolsa* program at different times. We estimate that, after accounting for cumulative effects, the *Bolsa* program has increased enrollment in Brazil by about 5.5 percent in grades 1-4 and by about 6.5 percent in grades 5-8. We also estimate that the program has lowered dropout rates by about 0.5 percentage points and raised grade promotion rates by about 0.9 percentage points for children in grades 1-4, and has reduced dropout rates by about 0.4 percentage points and increased grade promotion rates by about 0.3 percentage points for children in grades 5-8. Only about one third of Brazil's children participate in the *Bolsa* program, so the assumption that these results are mainly due to the impact of the program on participants, with no effect on non-participants, implies that the impact of participating in the *Bolsa* program is about three times higher than these estimates. While these impacts cast a favorable light on the program, simple calculations based on the enrollment impacts suggest that the likely benefits in terms of increased wages may not exceed the costs of the program.

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I. Introduction

Many economists agree that higher levels of education increase economic growth (Barro, 1991; Mankiw, Romer and Weil, 1992; Hanushek and Kimko, 2000; Krueger and Lindahl, 2001; Sala-i-Martin, 2004; Hanushek and Woessmann, 2008), raising incomes and, more generally, the quality of life. Economists' support for education is matched by strong support from international development agencies. Two of the eight Millennium Development Goals (MDGs) adopted at the United Nations Millennium Summit in 2000 focus on education: first, all children should complete primary school, and second, gender equality should prevail at all education levels.

The Millennium Goals may not be met due to constraints parents face when making decisions about their children's schooling. The main barriers are the direct costs (school fees, books, uniforms, etc.) and the opportunity cost of time in school – the reduction in the time children spend working or doing other activities if they spend more time studying in school (and at home). Several countries have not only ended fees or provided free meals and uniforms, they also pay families of students who attend school. These programs, called conditional cash transfer (CCT) programs, have two objectives: (1) alleviation of poverty today; and (2) increased investment in the human capital of poor children to increase their well being when they are adults. The first objective is met when poor families receive program payments. The second is achieved by conditioning those payments on certain behaviors, such as immunizing young children, and enrolling older children in school. Such programs are now found in many developing countries, especially in Latin America. The two largest are Mexico's *Progresa* (later renamed *Oportunidades*) program and Brazil's *Bolsa Escola* (renamed *Bolsa Familia*) program.

Latin America has made significant progress in education since 1980. For example, the net primary enrolment rate rose from 70% in 1980 to 94% in 2004, and the net secondary enrolment rate jumped from 16% to 61% (Damon and Glewwe, 2007). But further progress is needed. For example, while enrolment rates in Brazil increased from 86% in 1990 to 97% in 2001 for 8-11 year old children, among children age 15 in 2001 it was only 87%. Indeed, in 2001 40% (nine million) of Brazilian youths from 18 to 25 years old had less than 8 years of education (PNAD, 2001). To encourage all children to complete 8 years of schooling, Brazil launched the *Bolsa Escola* program in 2001.

Bolsa Escola (renamed *Bolsa Familia* in 2004) provides transfers to poor families with school-age children, conditional on those children being enrolled in school. Several studies have shown that CCT programs in Latin America improve student educational outcomes, but almost all examined Mexico's *Progresa* program or similar programs in Central America. These studies are very credible because they exploit the fact that these programs were implemented as randomized trials. In contrast, there are few analyses of the impact of *Bolsa Escola* (which was not randomized) on education outcomes in Brazil. This is unfortunate since Brazil is the largest and most populous nation in Latin America, and the *Bolsa* program is the world's largest CCT program. This paper uses an unusually rich panel data set to evaluate the impact of *Bolsa Escola/Familia* on enrollment and on dropout and grade promotion rates at the primary and lower secondary levels. Estimates are presented at the school level and at the *municipio* (county) level.

The following sections review the literature, describe the *Bolsa Escola/Familia* program and the data, explain the estimation methodology, and present the results. A final section summarizes the paper and provides suggestions for future research.

II. Literature Review

Parker, Rubalcava and Teruel (2008) review many studies that analyze the impact of CCT programs on schooling in Latin American and in other developing countries. Maluccio and Flores (2004) estimate that Nicaragua's *Red de Proteccion Social* program raised enrolment by 17.7 percentage points, increased daily attendance by 11 percentage points, and raised retention rates by 6.5% for children in grades 1 to 4 in that country. In Honduras, the *Programa de Asignacion Familiar* had positive, but smaller, impacts on daily attendance and enrolment, and a small negative effect on dropping out, for children age 6 to 13 (Glewwe and Olinto, 2004). Attanasio, Fitzsimons and Gomez (2005) found that Colombia's *Familias en Accion* increased enrolment for children age 12-17 but had no effect for 8-11 year old children. Schady and Araujo (2006) estimated a positive impact of Ecuador's *Bono de Desarrollo Humano* program on enrolment. Two programs outside of Latin America focus on girls' education. Khandker, Pitt and Fuwa (2003) found that a CCT program in Bangladesh increased enrolment for 11-18 year old girls, while Filmer and Schady (2006), estimated that the *Japan Fund for Poverty Reduction* program in Cambodia increased secondary school girls' enrolment and attendance.

Many, if not most, studies examine Mexico's *Progresa/Oportunidades* program. Behrman, Sengupta and Todd (2000) found that it increased enrolment for 12 to 14 year old girls but had no significant impact for younger children; they attribute the latter to the already high enrolment rates for younger children. They also found, for 11-15 year old children, a significant reduction in the schooling gap (the difference between actual grade attained and the grade a child would have attained had he or she entered school at age six and progressed one grade per year). Schultz (2004) found a strong positive effect of

Progresa on girls' and boys' enrolment, with a stronger effect for girls. Dubois, de Janvry and Sadoulet (2004) estimate that *Progresa* increased children's probability of staying in school, and grade progression and primary completion, but reduced grade progression at the secondary level. Skoufias and Parker (2001) found that *Progresa* significantly increased enrolment and reduced employment among both boys and girls.

Unlike the extensive research on Mexico's *Progresa* program, studies of Brazil's *Bolsa Escola/Familia* are rare, perhaps because it was not implemented as a randomized trial. *Bolsa's* first evaluation, by the World Bank (2001), focused on its operation in the Federal District (which is Brazil's capital, Brasilia) in 1995 and 1996. This study simply compared beneficiaries and non-beneficiaries; *Bolsa* appeared to reduce dropping out by 6 percentage points and increase grade promotion rates by 8-10 percentage points, but had little effect on students' test scores. Yet this study has several shortcomings. First, and most important, it did not account for initial differences across beneficiaries and non-beneficiaries. Second, it was conducted in the capital, a wealthy area that is not representative of Brazil. Finally, *Bolsa* has changed since 1996, as explained below.

Bourguignon, Ferreira and Leite (2003) used Brazilian household survey data from 1999 to estimate a model of household behavior, which they use to simulate the (future) impact of *Bolsa Escola/Familia*. They estimated that it would induce most eligible out of school youths to enroll in school. Yet they caution that their results depend on their technical assumptions and so provide only "orders of magnitude for the likely effects of transfer programs of the *Bolsa Escola* type." This paper, while an interesting research exercise, yields only rough estimates of the impact of the *Bolsa* program. Some assumptions needed to estimate the model, for example that children not in school who work outside the home do no household work (p.237), are doubtful. At best, this paper provides only rough estimates of *Bolsa's* impact.

Cardoso and Souza (2003) and Ferro and Kassouf (2005) both estimate that *Bolsa Escola* has a large positive impact on enrollment. Yet both used cross-sectional data and did little to control for selection into the program and, more generally, omitted variable bias. Also, both used data from 2000 (Demographic Census) and 2001 (PNAD, the National Household Sample Survey), before the program was greatly expanded.

A very recent study by de Janvry, Finan and Sadoulet (2007) finds that *Bolsa Escola/Familia* reduced dropping out by 8 percentage points but did not affect repetition. Yet this analysis is limited to 5 states in Northeast Brazil, and has a much smaller sample of schools than the data used in this paper, which reduces the precision of the estimates. Their data also lack some key variables, such as student race; the estimates presented below often vary by race.

In summary, while the *Bolsa Escola/Familia* program is the world's largest CCT program (see below), there is very little research on it. The research to date suffers from estimation problems, data that cover only a small part of Brazil, and (in most cases) analysis of the earliest version of the program. The analysis in this paper uses 8 years of nationwide data, including 5 years when the program was operating nationwide and develops an estimation procedure that minimizes a wide variety of estimation problems.

III. Description of *Bolsa Escola/Familia* Program

The first two *municipios* (similar to U.S. counties) to implement Brazil's *Bolsa Escola* were the cities of Brasilia (the Federal District) and Campinas (in São Paulo

State). These programs, which began in 1995, provided cash payments to poor families with children from age 6 to 15 conditional on those children enrolling in school and attending at least 85% of school days. By 1998 over 50 *municipios* in seven states (out of 26 plus the Federal District) had similar programs, but this was still only 1% of Brazil's 5,500+ *municipios*.

Given the program's popularity, and positive evaluations of other CCT programs in Latin America, President Fernando Henrique Cardoso's government created the Federal *Bolsa Escola* program in April, 2001. By the end of 2001, nearly 5 million families in over 5,000 *municipios* (out of 5,560) were receiving payments. In 2003, President Luis Inacio Lula da Silva expanded *Bolsa Escola* to other types of households, and renamed it *Bolsa Familia*. Benefits were extended to poor families with children 0 to 5 years old or with a pregnant or breastfeeding woman, and to all "very poor" families (even those without children). By 2007, over 11 million families (about 46 million people, one fourth of Brazil's population) received *Bolsa* payments. The government budget for the program was over 7.5 billion Reais (about 4 billion U.S. $) in 2006, which was 0.35% of GNP. This is larger than Mexico's *Progresa* program, which served about 4 million families and cost U.S. $2.2 billion in 2004 (de Janvry and Sadoulet, 2006).

To qualify for *Bolsa Escola/Familia*, a family's monthly per capita income must be below 120 Reais (about 60 U.S. $), that is below one-half of Brazil's minimum wage. Those with monthly incomes from 60 to 120 Reais are eligible if they have either children under 16 years old or a breastfeeding or pregnant woman.[1] Those with monthly per capita incomes below 60 Reais are classified as very poor and receive payments even in the absence of children or a pregnant or breastfeeding woman. Families with a per

[1] The program was expanded in March 2008 to include 16 and 17 year old children.

capita income from 60 to 120 Reais per month receive 15 Reais per month per beneficiary (either a child below age 16 or a pregnant or breastfeeding woman), up to a maximum of three (to avoid incentives to increase fertility).[2] Families with monthly per capita incomes below 60 Reais receive 50 Reais per month plus another 15 Reais per beneficiary (up to three). To receive the 15 Reais, each child age 6 to 15 must be enrolled in school and attend at least 85% of school days, each pregnant or breastfeeding women must obtain prenatal and postnatal health care services, and children age 0 to 7 must have all recommended vaccinations.

To enroll in the program, families must fill out an application, available at the city hall of their *municipio*, that requests information on income and household composition. The information determines admission to the program, subject to the *municipio's* budget for the program.[3] That budget is set (and financed) by the Federal Government, based on the estimated number of poor families in each *municipio*, as derived from the population census and recent household surveys (*Pesquisa Nacional por Amostra Domicílios*, PNAD). *Bolsa Familia* payments are usually given to a woman in the household, usually the head or the head's wife, through a bank card, because studies show that women are more likely to use additional income to improve their families' wellbeing. Soares, Ribas and Osorio (2007) argue that this process for selecting beneficiaries may allow ineligible families to obtain benefits because the data on the applications are not verified.

Although *Bolsa Escola/Familia* is often viewed as a program operated by schools, in fact schools have no role in funding or implementing the program since funding comes

[2] Payments were raised in 2007 and 2008. The current monthly payment to very poor families is 62 Reais. The monthly payment per child is 20 Reais for children up to age 15 and 30 Reais for children age 16 or 17.
[3] de Janvry et al. (2005) report that in almost all *municipios* the number of potential beneficiaries greatly exceeded the number of beneficiaries they could fund with the budget allocated by the central government.

from the *Ministry of Social Development* and *Bolsa* is implemented by the *municipio* government. Thus schools have no incentive to misreport information on the program (or on anything else) in order to gain some type of program benefit (though schools could misreport information to benefit a student's family).

IV. Data Available

The main data source used in this paper is Brazil's school census. Each year it is administered to over 250,000 public and private schools, from preschools to high schools. These schools enroll 53 million students and employ two million teachers. The main education outcomes in this census are enrolment, dropping out, and grade promotion and repetition. It also collects school characteristic data, including the existence of a library, a computer lab, an internet connection, a science lab, and a gymnasium, as well as school participation in government transport, meal, textbook, and income transfer programs.

The school census data from 1998 to 2005 were used to create a panel of schools. Table 1 shows the number of schools (public and private) in each census. The focus is on children in grades 1 to 8, which includes the age range eligible to receive *Bolsa* benefits. Schools can have grades 1 to 4 or grades 5 to 8, or both, as well as preschool and/or higher levels (high school, vocational classes, etc.). The third column shows the number of schools with grades 1-4 or 5-8 (or both). Over time that number steadily declines, reflecting Brazil's demographic trends (lower fertility) and its policy of merging small schools to create larger ones and closing schools in bad physical condition. Although the number of these schools fell by about 50,000 from 1998 to 2005, the number of preschools and high schools increased, so the total number of schools dropped by only about 20,000.

The last column in Table 1 shows the panel data set for schools with grades 1-4 and/or grades 5-8. In 1998, Brazil had 187,514 schools with those grades. Of these, 174,153 could be matched (by school ID codes) to the 183,475 schools in 1999 census. Non-matches reflect new schools, schools that were closed or merged into larger schools, and school code errors. Each row in Table 1 shows how the panel set becomes smaller as another year is added. In 2005 there are 136,114 schools with grades 1-4 or 5-8, of which 107,243 have data for all years from 1998 to 2005. Poor students rarely enroll in private schools, and *Bolsa's* income limits effectively exclude families wealthy enough to enroll their children in those schools, so the rest of the paper focuses on public schools.

Table 2 shows selected school characteristics from 1998 to 2005 for public schools with grades 1-4. Average total enrollment declined from 135 to 110 over those years, reflecting demographic trends and reduced repetition. The grade promotion rate is the percentage of students who, based on academic performance, advance to the next grade; thus it is an indicator of academic performance. This rate increased from 68% in 1998 to 73% in 2005, which may indicate better academic performance but also reflects a policy of "social promotion"). The dropout rate (the fraction of enrolled students who leave school before the end of the school year) fell sharply, from 14.5% to 8.9%. Class size decreased modestly, from 27.5 to 24.5. Another educational outcome of interest is age-grade distortion. In Brazil, students usually start grade 1 at age 7, so they should finish grade 4 at age 11 and grade 8 at age 15. A student who finishes grade 4 or grade 8 at an age above the "correct" one has age-grade distortion. The percentage of grade 4 students with age-grade distortion also decreased, from 57% in 1998 to 37% in 2005.

Table 2 also presents indicators of school quality and participation in *Bolsa Escola/Familia*. Since 1998 the percentage of teachers with a college degree increased dramatically, from 8.4% to 26.2% in 2005. The availability of computers, printers and computer labs also increased rapidly. The last column shows school participation in the *Bolsa* program (more precisely, the percentage of schools reporting that one or more students participate in *Bolsa*), which is available starting in 2001. In that year, only 23.5% of schools reported student participation in the program. This increased sharply in 2002 to 84.7%, after which participation slowly increased, reaching 90.8% in 2005.

Table 3 (top panel) shows several school characteristics in 2001, for all public schools with grades 1-4 and separately for schools with and without *Bolsa* students. This demonstrates that simple comparisons across schools with and without *Bolsa* students can yield implausible results. The year 2001 is shown because it has the least lopsided proportions of schools with and without students in the program. Total enrollment and grade promotion are lower, and the dropout rate is higher, in *Bolsa* schools, so this simple comparison suggests that the program reduced enrollment and grade promotion and increased dropping out, which is doubtful. The obvious explanation is that *Bolsa* is targeted to poor children, who have lower initial education outcomes. Table 3 also shows that schools with *Bolsa* students have fewer college-educated teachers, computers, printers and computer labs. Only students per classroom is similar for the two groups.

The data in Table 2 suggest that the *Bolsa* program raised enrollment and grade promotion, and reduced dropping out, in schools with grades 1-4. First, average school enrollment dropped by 4.0 students from 1999 to 2000, before the program was expanded to a national scale. In the two years of *Bolsa's* most rapid expansion (from 2000 to 2002) total enrollment dropped by only 3.1 students from 2000 to 2001 and by only 2.6 from 2001 to 2002. From 2002 to 2005, the program expanded slowly, and the annual drop in enrollment increased to about 4 or 5 students per year. Turning to grade promotion, the rate was about 69% or 70% from 1998 to 2000. It then increased to 72.4% in 2001 and 73.0% in 2002, the years of *Bolsa's* rapid expansion. The rate stayed at about 73% from 2002 to 2005 (except for 2004, when it was 71%). Finally, dropout rates were 13% or 14% from 1998 to 2000, and then fell to 10.6 in 2001 and 9.5 in 2002, after which they fluctuated between 9% and 10%. Overall, for all three education outcomes trends suggest improvements during the two years the program expanded most quickly.

Next, consider public schools with grades 5-8. Unlike the trend for schools with grades 1-4, Table 4 shows rising enrollment from 1998 to 2000, followed by a decline. Also, unlike schools with grades 1-4, grade promotion changed very little, with no clear trend. Yet the dropout rate has a pattern similar to that for schools with grades 1-4; it decreased from 13.6% in 1998 to 9.1% in 2005. Students per classroom is higher in schools with grades 5-8 than in schools with grades 1-4, but in both schools it steadily decrease over time. Finally, as in schools with grades 1-4, the age-grade distortion rate steadily decreased from 56% in 1998 to 38% in 2005.

The percentage college-educated teachers in public schools with grades 5-8 is much higher than in those with grades 1-4, and it increased from 62% in 1998 to 80% in 2005. There was also a sharp increase over time in the percentage of those schools with computers, printers and computer labs, as was seen for schools with grades 1-4 (although in any year the percentage of schools with grades 5-8 with these resources is much higher). Finally, the percentage of schools with grades 5-8 reporting one or more students in the *Bolsa* program was only 13% in 2001, but it increased sharply to 76% in 2002, after which it slowly increased, reaching 86% in 2005.

The second panel of Table 3 compares public schools with grades 5-8 in 2001, by whether they have students benefiting from *Bolsa Escola/Familia*. As in schools with grades 1-4, schools with *Bolsa* students have lower enrollment and grade promotion and a higher drop out rate. While their student-teacher ratio is somewhat lower, they have fewer college-educated teachers, computers, printers and computer labs. Again, this likely reflects that *Bolsa* is targeted to poor children, who are disadvantaged not only in terms of family circumstances but also by the quality of the schools they attend.

Finally, for schools with grades 5-8 the changes in education outcomes over time in Table 3 suggest that *Bolsa* improved education outcomes. This is hard to infer from the enrollment data since its trend is quadratic, yet grade promotion jumped by almost two percentage points in 2001, the first year of *Bolsa's* wide expansion. More persuasive is the sharp decline in the dropout rate from 13.3 in 2000 to 11.1 in 2001 and 10.3 in 2002; this occurred precisely for the two years when *Bolsa* expanded most rapidly.

V. Methodology

Let y_{ist} be an educational outcome of interest for child i in school s at time t for a particular set of grades. In general, y_{ist} is a function of child and household characteristics (denoted by the vector $\mathbf{c}_{ist}$), school and teacher characteristics ($\mathbf{s}_{st}$), and whether the *Bolsa* program operates at time t in the community where the school is located (B_{st}, measured in the school census by the school reporting one or more students participating in *Bolsa*):

$$y_{ist} = f(\mathbf{c}_{\mathrm{ist}}, \mathbf{s}_{\mathrm{st}}, B_{st}) \qquad (1)$$

To ease interpretation, redefine the $\mathbf{c}_{\mathrm{ist}}$ and $\mathbf{s}_{\mathrm{st}}$ variables as deviations from their means.[4]

Assume that the $f(\;)$ is linear, which is reasonable as long as sufficient interaction terms between the various sets of variables are included:

$$y_{ist} = \boldsymbol{\alpha}'\mathbf{c}_{\mathrm{ist}} + \boldsymbol{\beta}'\mathbf{s}_{\mathrm{st}} + \gamma B_{st} + \boldsymbol{\delta}'(\mathbf{c}_{\mathrm{ist}} \times B_{st}) + \boldsymbol{\theta}'(\mathbf{s}_{\mathrm{st}} \times B_{st}) + \varepsilon_{ist} \qquad (1')$$

where ε_{ist} denotes idiosyncratic deviations of $f(\;)$ from this linear approximation.

The $\mathbf{c}_{\mathrm{ist}} \times B_{st}$ interaction terms are important, for two reasons. First, the impact of participating in the program could vary by child characteristics. Second, not all children are eligible for *Bolsa*, so $\mathbf{c}_{\mathrm{ist}}$ could include variables that determine program eligibility, such as household income. This would not be necessary if one had individual level data for the *Bolsa* variable, B_{st} (recall that program eligibility varies over students within schools). Unfortunately, the data available are at the school level, so B_{st} must be specified at that level. Yet, in principle, the interaction term $\mathbf{c}_{\mathrm{ist}} \times B_{st}$ captures variation in eligibility at the student level if $\mathbf{c}_{\mathrm{ist}}$ includes student characteristics that determine eligibility.

The interaction term with school characteristics, $\mathbf{s}_{\mathrm{st}} \times B_{st}$, may also be important since (perceived) school quality may make schools more attractive. Yet the impact of *Bolsa* on enrolment may be higher (a better school, combined with a transfer payment,

[4] Community characteristics, such as child wage rates, job prospects for educated adults, and local interest in education, could be added to equation (1). That is not done here to avoid notational clutter, and because our data from Brazil include no community characteristics. However, it is not difficult to include such variables in equation (1); they could be specified in the same way the school variables ($\mathbf{s}_{\mathrm{st}}$) are specified.

persuades parents to keep a child in school) or lower (better schools are already highly valued, so the transfer has little additional impact) for higher quality schools.

A. School level Analysis. For school level estimation, sum equation (1′) over *i*:

$$y_{st} = \boldsymbol{\alpha}'\mathbf{c}_{st} + \boldsymbol{\beta}'\mathbf{s}_{st} + \gamma B_{st} + \boldsymbol{\delta}'(\mathbf{c}_{st}\times B_{st}) + \boldsymbol{\theta}'(\mathbf{s}_{st}\times B_{st}) + \varepsilon_{st} \qquad (2)$$

where $y_{st} = (1/N_{st})\sum_{i=1}^{N_{st}} y_{ist}$, $\mathbf{c}_{st} = (1/N_{st})\sum_{i=1}^{N_{st}} \mathbf{c}_{ist}$, $\varepsilon_{st} = (1/N_{st})\sum_{i=1}^{N_{st}} \varepsilon_{ist}$, and N_{st} is the number of students in school *s* at time *t*. Since $\mathbf{c}_{st}$ and $\mathbf{s}_{st}$ are deviations from their means, γ measures the impact of the *availability* of the *Bolsa* program on the *average* student in an average school, and $\boldsymbol{\delta}$ and $\boldsymbol{\theta}$ measure how this average impact varies by child and school characteristics, respectively. Note that γ does *not* measure the (average) impact of *participating* in the program, and the analogous point holds for $\boldsymbol{\delta}$ and $\boldsymbol{\theta}$.

If data were available for all variables in $\mathbf{c}_{st}$ and $\mathbf{s}_{st}$, OLS estimates of equation (2) would be consistent estimates of γ, $\boldsymbol{\delta}$ and $\boldsymbol{\theta}$. Yet many $\mathbf{c}_{st}$ and $\mathbf{s}_{st}$ variables are unobserved. For example, $\mathbf{c}_{st}$ could include child innate ability and parental tastes for schooling, and $\mathbf{s}_{st}$ could include principal and teacher motivation. To see the implications for estimation, modify (2) to distinguish between observed and unobserved variables in $\mathbf{c}_{st}$ and $\mathbf{s}_{st}$:

$$\begin{aligned} y_{st} &= \boldsymbol{\alpha}'\mathbf{c}_{st}+\boldsymbol{\alpha}^{*\prime}\mathbf{c}_{st}^{*} + \boldsymbol{\beta}'\mathbf{s}_{st}+\boldsymbol{\beta}^{*\prime}\mathbf{s}_{st}^{*} + \gamma B_{st} + \boldsymbol{\delta}'(\mathbf{c}_{st}\times B_{st})+\boldsymbol{\delta}^{*\prime}(\mathbf{c}_{st}^{*}\times B_{st}) + \boldsymbol{\theta}'(\mathbf{s}_{st}\times B_{st})+\boldsymbol{\theta}^{*\prime}(\mathbf{s}_{st}^{*}\times B_{st}) + \varepsilon_{st} \qquad (2')\\ &= \boldsymbol{\alpha}'\mathbf{c}_{st} + \boldsymbol{\beta}'\mathbf{s}_{st} + \gamma B_{st} + \boldsymbol{\delta}'(\mathbf{c}_{st}\times B_{st}) + \boldsymbol{\theta}'(\mathbf{s}_{st}\times B_{st}) + [\boldsymbol{\alpha}^{*\prime}\mathbf{c}_{st}^{*} + \boldsymbol{\beta}^{*\prime}\mathbf{s}_{st}^{*} + \boldsymbol{\delta}^{*\prime}(\mathbf{c}_{st}^{*}\times B_{st}) + \boldsymbol{\theta}^{*\prime}(\mathbf{s}_{st}^{*}\times B_{st}) + \varepsilon_{st}] \end{aligned}$$

Asterisks denote unobserved variables (and their associated parameters), and variables without asterisks now denote observed variables (and their parameters have no asterisks).

The second line of (2′) shows that, for estimation, all unobserved variables become part of the error term.

Consistent estimation of equation (2′) by OLS requires the term in brackets to be uncorrelated with all observed variables, which is unlikely. First, the availability of *Bolsa* at any school may be affected by unobserved child ($\mathbf{c}_{st}$*) and school ($\mathbf{s}_{st}$*) characteristics. For example, community leaders had to make efforts to implement *Bolsa* in their *municipios*, and such leaders may affect unobserved school characteristics. Also, *Bolsa* was implemented more quickly in communities with low education outcomes (see Table 3), outcomes that may reflect unobserved school and child characteristics.

Second, the *Bolsa* variable, B_{st}, increases over time (see Tables 2 and 4). Some elements of observed child and school characteristics, $\mathbf{c}_{st}$ and $\mathbf{s}_{st}$, could also increase (or decrease) over time, and for those variables the same would be true of their interactions with B_{st}. Since *unobserved* child and school characteristics can also change over time, so the term in brackets in equation (2′) could slowly increase (or decrease) over time even if all elements in $\mathbf{c}_{st}$* and $\mathbf{s}_{st}$* have a mean of zero; this leads to correlation between the error term in (2′) and B_{st} (as well as some elements of $\mathbf{c}_{st}$, $\mathbf{s}_{st}$, $\mathbf{c}_{st}\times B_{st}$ and $\mathbf{s}_{st}\times B_{st}$).

To remove bias due to correlation of B_{st} (and other observed variables) with the unobserved determinants of y_{st} one could find instruments for the observed variables, but no credible instruments are in our data. Instead, we approximate all unobserved variables by school and time fixed effects, plus state-specific time trends. That is, assume that:

$$\mathbf{c}_{st}^* = \boldsymbol{\sigma}_{c,s} + \boldsymbol{\tau}_{c,t} + \boldsymbol{\pi}_{c,j}\times t + \boldsymbol{\eta}_{c,st} \qquad (3)$$

$$\mathbf{s}_{st}^* = \boldsymbol{\sigma}_{s,s} + \boldsymbol{\tau}_{s,t} + \boldsymbol{\pi}_{s,j}\times t + \boldsymbol{\eta}_{s,st}$$

Essentially, equation (3) decomposes all variables in $\mathbf{c}_{st}$* and $\mathbf{s}_{st}$* into a (time invariant) school fixed effect (σ), a time fixed effect (τ) that does not vary over schools, a time trend that varies over Brazil's states but not over schools within states ($\boldsymbol{\pi}_j \times t$, where *j* denotes state), and white noise deviations from these fixed effects and trends ($\boldsymbol{\eta}_{c,st}$ and $\boldsymbol{\eta}_{s,st}$).[5]

Inserting the expressions in equation (3) into the unobserved terms (other than ε_{st}) in equation (2′) yields the following:

$$\boldsymbol{\alpha}^{*\prime}\mathbf{c}_{st}^{*} + \boldsymbol{\beta}^{*\prime}\mathbf{s}_{st}^{*} + \boldsymbol{\delta}^{*\prime}(\mathbf{c}_{st}^{*}\times B_{st}) + \boldsymbol{\theta}^{*\prime}(\mathbf{s}_{st}^{*}\times B_{st}) \qquad (4)$$

$$= \sigma_s + \tau_t + \pi_j\times t + \eta_{st} + \boldsymbol{\delta}^{*\prime}((\boldsymbol{\sigma}_{c,s} + \boldsymbol{\tau}_{c,t} + \boldsymbol{\pi}_{c,j}\times t + \boldsymbol{\eta}_{c,st})\times B_{st}) + \boldsymbol{\theta}^{*\prime}((\boldsymbol{\sigma}_{s,s} + \boldsymbol{\tau}_{s,t} + \boldsymbol{\pi}_{s,j}\times t + \boldsymbol{\eta}_{s,st})\times B_{st})$$

$$= \sigma_s + \tau_t + \pi_j\times t + \eta_{st} + \sigma_{s(B)}B_{st} + \tau_{t(B)}B_{st} + \pi_{j(B)}\times t\times B_{st} + \eta_{st(B)}\times B_{st}$$

where $\sigma_s = \boldsymbol{\alpha}^{*\prime}\boldsymbol{\sigma}_{c,s} + \boldsymbol{\beta}^{*\prime}\boldsymbol{\sigma}_{s,s}$ (a school fixed effect), $\tau_t = \boldsymbol{\alpha}^{*\prime}\boldsymbol{\tau}_{c,t} + \boldsymbol{\beta}^{*\prime}\boldsymbol{\tau}_{s,t}$ (a time fixed effect), $\pi_j = (\boldsymbol{\alpha}^{*\prime}\boldsymbol{\pi}_{c,j} + \boldsymbol{\beta}^{*\prime}\boldsymbol{\pi}_{s,j})$, so the π_j terms allow for separate time trends in each of Brazil's 26 states (and the Federal District), and $\eta_{st} = \boldsymbol{\alpha}^{*\prime}\boldsymbol{\eta}_{c,st} + \boldsymbol{\beta}^{*\prime}\boldsymbol{\eta}_{s,st}$. In addition, $\sigma_{s(B)} = \boldsymbol{\delta}^{*\prime}\boldsymbol{\sigma}_{c,s} + \boldsymbol{\theta}^{*\prime}\boldsymbol{\sigma}_{s,s}$, $\tau_{t(B)} = \boldsymbol{\delta}^{*\prime}\boldsymbol{\tau}_{c,t} + \boldsymbol{\theta}^{*\prime}\boldsymbol{\tau}_{s,t}$, $\pi_{j(B)} = \boldsymbol{\delta}^{*\prime}\boldsymbol{\pi}_{c,j} + \boldsymbol{\theta}^{*\prime}\boldsymbol{\pi}_{s,j}$, and $\eta_{st(B)} = \boldsymbol{\delta}^{*\prime}\boldsymbol{\eta}_{c,st} + \boldsymbol{\theta}^{*\prime}\boldsymbol{\eta}_{s,st}$. The term $\sigma_{s(B)}$ is an unobserved school fixed effect that "turns on" only when students participate in the *Bolsa* program; the program's impact could vary in unobserved ways by interactions with (time invariant) unobserved child and school characteristics. The term $\tau_{t(B)}$ allows the time fixed effect to differ for schools with and without *Bolsa* students. The $\pi_{j(B)}$ term is for a time trend that is in effect only when the program is operating; it allows the impact of *Bolsa* to change over time, at different rates in each state, due to

[5] Our estimates use two time trends for each state, one for schools where students began participating in *Bolsa* in 2001 ("early adopters") and one for schools where student participation began in 2002 or later.

changes in unobserved child and school characteristics that influence that impact. Finally, $\eta_{st(B)}$ is random noise.

Inserting (4) into (2′) gives the equation estimated in this paper:

$$y_{st} = \boldsymbol{\alpha}'\mathbf{c}_{st} + \boldsymbol{\beta}'\mathbf{s}_{st} + \gamma B_{st} + \boldsymbol{\delta}'(\mathbf{c}_{st}\times B_{st}) + \boldsymbol{\theta}'(\mathbf{s}_{st}\times B_{st}) + \sigma_s + \tau_t + \pi_j\times t + \eta_{st} \qquad (5)$$

$$+ \sigma_{s(B)}\times B_{st} + \tau_{t(B)}B_{st} + \pi_{j(B)}\times t\times B_{st} + \eta_{st(B)}\times B_{st} + \varepsilon_{st}$$

The intuition behind this estimation method is as follows. Implementation of the *Bolsa* program, B_{st}, may be correlated with child and school characteristics, and with changes in those characteristics over time, but these changes should be very gradual and so are controlled for in the regression by adding school and time fixed effects and state-specific time trends. In other words, once one conditions on these fixed effects and time trends B_{st}, and all other observed variables, are no longer correlated with the remaining random error terms, η_{st}, ε_{st} and $\eta_{st(B)}$, all of which are assumed to be white noise.

There remains, however, one estimation problem: the school fixed effects that interact with the *Bolsa* program, $\sigma_{s(B)}\times B_{st}$, are, in effect, a set of school dummy variables that, when summed, equal B_{st}, so γ, the average impact of *Bolsa*, is not identified. This also occurs for the year fixed effects that interact with B_{st}. To resolve this, recall that the means of $\mathbf{c}_{st}$* and $\mathbf{s}_{st}$* are zero, so one can constrain the means of $\sigma_{s(B)}$ and $\tau_{t(B)}$ to be zero.[6] While this constraint is easy to impose for the five year fixed effects (2001 to 2005), it is extremely difficult to impose for the over 100,000 school fixed effects. Failing to impose this constraint is most likely to cause bias in estimates of $\boldsymbol{\delta}$ and $\boldsymbol{\theta}$. To see why, suppose

[6] This constraint cannot be imposed if some schools never have students who participate in *Bolsa*, but in fact almost 98% of the schools in our sample have *Bolsa* students for at least one year.

there are no interaction effects in equations (2) and (2′), so $\boldsymbol{\delta} = \boldsymbol{\theta} = \boldsymbol{\delta}^* = \boldsymbol{\theta}^* = \mathbf{0}$. Then $\sigma_{s(B)}B_{st} + \tau_{t(B)}B_{st} + \pi_{j(B)}\times t\times B_{st} + \eta_{st(B)}\times B_{st}$ drops out of (4) and (5) and one needs to control only for $\sigma_s + \tau_t + \pi_j\times t + \eta_{st}$, which one can do by standard fixed effects estimation. It is the estimation of interaction effects that generates $\sigma_{s(B)}\times B_{st}$, and the inability to constrain the sum of these effects to be zero could lead to inconsistent estimation of observed interaction effects ($\boldsymbol{\delta}$ and $\boldsymbol{\theta}$). Thus, while estimates of γ are unlikely to have large biases, estimates of $\boldsymbol{\delta}$ and $\boldsymbol{\theta}$ may be seriously biased and so should be treated with caution.

Finally, this approach can be modified to make it more flexible. First, state-level time trends need not be linear; for example, γ is still identified if trends are quadratic. Second, the full impact of the *Bolsa* program may not be felt in its first year. Enrollment, grade promotion and dropping out in any year could also be affected by whether *Bolsa* operated in previous years, since learning accumulates over time and because adding or losing students in one year has implications for future educational outcomes. This can be checked by adding lagged terms, denoted as $B_{s,t-1}$, $B_{s,t-2}$, etc., to equation (5).

B. *Municipio* Level Analysis. Our analysis of the school level data is hampered by the fact that the *Bolsa* variable measures only the existence of that program, so it only estimates the impact of the *availability* of the program. Fortunately, *municipio* level data exist on the number of households participating in *Bolsa*, which allows for estimation at the *municipio* level of the impact of *participating* in the program.

To use *municipio* level data, replace B_{st}, the dummy variable indicating program availability in equation (1′), with B_{ist}, the indicator of student i's participation in *Bolsa*:

$$y_{ist} = \boldsymbol{\alpha}'\mathbf{c}_{\text{ist}} + \boldsymbol{\beta}'\mathbf{s}_{\text{st}} + \gamma B_{ist} + \boldsymbol{\delta}'(\mathbf{c}_{\text{ist}}\times B_{ist}) + \boldsymbol{\theta}'(\mathbf{s}_{\text{st}}\times B_{ist}) + \varepsilon_{ist} \qquad (1'')$$

Sum this over students within each *municipio* to obtain a *municipio* level equation:

$$y_{mt} = \boldsymbol{\alpha}'\mathbf{c}_{\mathrm{mt}} + \boldsymbol{\beta}'\mathbf{s}_{\mathrm{mt}} + \gamma B_{mt} + \boldsymbol{\delta}'(\mathbf{c}_{\mathrm{mt}}\times B_{mt}) + \boldsymbol{\theta}'(\mathbf{s}_{\mathrm{mt}}\times B_{mt}) + \varepsilon_{mt} \qquad (6)$$

where $\mathbf{c}_{\mathrm{mt}}$, $\mathbf{s}_{\mathrm{mt}}$, B_{mt} and ε_{mt} are averages over students in *municipio m* at time *t*.[7] Unlike B_{st} in (2), B_{mt} is not binary; it is the fraction of students in a *municipio* participating in *Bolsa*.[8]

As with the school level regressions, one must account for the many unobserved $\mathbf{c}_{\mathrm{mt}}$ and $\mathbf{s}_{\mathrm{mt}}$ variables. The same estimation method used for the school level regressions, adding school and time fixed effects and state level time trends, can be used here.

VI. Results

This section presents estimates of *Bolsa Escola/Familia*'s impact on enrollment, grade promotion and dropout rates in Brazil. The first subsection presents school level estimates, and the second presents *municipio* level results. Following the methodology of Section V, all regressions include year fixed effects, school or *municipio* fixed effects, and state level time trends. For the school level regressions, each state has two time trends, one for schools where students began participating in *Bolsa* in 2001 and another for schools where students first participated in 2002 or later. This allows "early" adopters and "late" adopters to have different time trends. Since almost all *municipios* had students who started participating in 2001, the *municipio* level regressions have only one state

[7] In fact, $\mathbf{c}_{\mathrm{mt}}\times B_{mt}$ and $\mathbf{s}_{\mathrm{mt}}\times B_{mt}$ only approximate the *municipio* sums of $\mathbf{c}_{\mathrm{ist}}\times B_{ist}$ and $\mathbf{s}_{\mathrm{ist}}\times B_{ist}$; this should have little effect on estimates of $\boldsymbol{\delta}$ and $\boldsymbol{\theta}$. As explained above estimates of $\boldsymbol{\delta}$ and $\boldsymbol{\theta}$ must be interpreted cautiously.

[8] More precisely, the *municipio* data are the number of households that participate in the programs, and the variable in the *municipio* level analysis is the proportion of households that participate in the *Bolsa* program. This is highly correlated with, but not exactly equal to, the proportion of students who participate.

level time trend. Finally, eight additional national time trends are added, based on school enrollment in 1998. This controls for Brazil's policy of merging small schools to create larger ones, which causes smaller schools' enrollment to increase more rapidly over time.

A. School Level Regressions. Table 5 shows basic estimates of the impact of the *Bolsa* program on (log) enrollment, dropping out and grade promotion of children in grades 1-4. The top panel has the simplest specification. The estimated impact for all three outcomes is highly significant, with the expected sign. Schools with students enrolled in the *Bolsa* program have 2.8 percent higher enrollment, a lower dropout rate (by 0.31 percentage points), and a higher grade promotion rate (by 0.53 percentage points). The estimated impacts are slightly smaller, but still highly significant, if the state level time trends are specified as quadratic, rather than linear (not shown in Table 5).

The *Bolsa* variable equals one if at least one child in a school participates in the program, so those estimates are average effects over *all* children in schools where at least some participate; thus they estimate the impact of the *availability* of the program, *not* the impact of *participating* in the program. Only about one third of children in Brazil participate in *Bolsa*,[9] so the assumption that non-participants do not benefit implies that the impact on participants is about three times higher than the estimates in Table 5.

It is possible that other, unobserved changes occurred in schools around the time the *Bolsa* program was implemented that affect these three education outcomes and are not adequately captured by the control variables used in Table 5, which can lead to biased estimates. To check this possibility, consider the first three years of data, from 1998 to 2000. If there are unobserved changes that are highly correlated with student participation

[9] According to the 2004 PNAD, Brazil had about 34.6 million children age 6-15 in 2004. Approximately 11.1 million were in families who participated in the *Bolsa* program.

in *Bolsa*, in some schools where *Bolsa* participation began in 2001 these unobserved changes occurred in 2000, while in others they occurred in 2001 and in still others in 2002. This implies that, using only the first three years of data, regressing these three variables on a "fake" variable that equals zero for all schools in 1998 and 1999 but equals one in 2000 for the schools that had *Bolsa* students in 2001 (and equals zero in 2000 for schools without *Bolsa* students in 2001) would lead to a significant impact of the "fake" variable. This is done in the second panel of Table 5. The coefficients are much smaller than the *Bolsa* coefficients in the first panel (an order of magnitude smaller for two of the three), and all are statistically insignificant. This suggests that the *Bolsa* program itself, and not some unobserved school or community variable correlated with *Bolsa*, is causing these changes in school enrollment, dropping out and grade promotion.

Another robustness check is to discard the data for 2001. The school census data used here do not match the Ministry of Social Development data on the extent of program participation in that year. Some school principals may have under-reported student participation in *Bolsa* in 2001 because that was the first year the school census asked about *Bolsa*, and in that year (unlike in later years) the question did not contain the word *Bolsa*.[10] If the principals understood the question better in 2002 and later years, dropping the 2001 data may yield more accurate results. This is examined in the third panel of Table 5. The estimated impacts of *Bolsa* are somewhat larger (in absolute value) than in the first panel. While this suggests that school principals made errors when filling out the 2001 census form (random errors would induce attenuation bias when the 2001 data are retained), another possibility is that the program has cumulative effects; for schools

[10] The school census has a question asking prinicipals to mark what programs exist at their schools. There are 15 choices, each with a box for the principal to mark. In 2001, the box for the *Bolsa* program had the label "minimum income program". In 2002 and later it was "minimum income program/*Bolsa Escola*".

where *Bolsa* began in 2001, the *Bolsa* variable for 2002 reflects two years of operation of the program, and if the program has cumulative effects the estimated impacts will increase when the 2001 data are excluded. Indeed, there is clear evidence below that *Bolsa* does have cumulative effects for all three education outcomes.

Another potential problem is omitted variable bias. Despite using school and year fixed effects, state level time trends, and time trends by initial level of enrollment, student participation in the *Bolsa* program may be correlated with trends in school characteristics that directly affect education outcomes. The fourth (last) panel of Table 5 examines this. Adding eight school characteristics, plus the proportion of female students, yields estimated program impacts almost identical to those in the first panel. In general, these school variables are highly significant and have the expected signs. The main unexpected finding is that a program that provides computers seems to worsen outcomes; this may reflect the targeting of this program to poorly performing schools.

The estimates in Table 5 are averages over *all* students in schools where one or more participate in *Bolsa*. These effects are likely to vary over children since only poor children are eligible (though this is not strictly enforced; see Soares et al., 2007) and because the program's impact may vary over participants. Overall, one would expect *Bolsa* to have a strong effect on children from disadvantaged families. To check for such heterogeneity in effects one can interact the *Bolsa* variable with student characteristics.

Unfortunately, the school census data have only three student variables: sex, race and age. The first two indicate the number of female, black, mulatto, East Asian ("yellow") and indigenous students (white is the omitted category) in each. The race data are available only for 2005, yet the racial composition of schools is likely to change very slowly over time. While school fixed effects preclude using race variables as regressors, but they can still be interacted with the *Bolsa* program variable.

The age variable can be used to calculate the average age of students entering grade 1, which is an indirect measure of households' economic status; households with limited resources and/or parents with lower tastes for education tend to delay enrolling their children in school.[11] This variable was constructed by calculating the average age of students in grade 1 and subtracting the grade 1 repetition rate. Since grade promotion is one of the dependent variables, this age variable must be "purged" of any effect of its opposite, repetition, on the average age of grade 1 students. Because the *Bolsa* program could affect this variable, all interactions use its value in 1998, before *Bolsa* began.

Table 6 presents results that interact these child characteristics, and the school characteristics in Table 5 (since the program impact may vary by school characteristics), with the *Bolsa* variable. Overall, the school variable interactions indicate that the *Bolsa* program has stronger effects on enrollment for better schools, amplifying inequities in observed school quality indicators.

Turning to the child variables, schools with more girls have higher enrollment, and the program is more effective at promoting enrollment in schools with more girls; this implies that *Bolsa* has a larger impact on female students. The presence of more "delayed enrollment" in grade 1 significantly reduces *Bolsa's* impact on enrollment. To the extent that this variable indicates families with low income or other disadvantages (such as less educated parents or malnourishment in early childhood), the program is less effective at inducing disadvantaged children to enroll in school. This negative interaction

[11] In the 2004 PNAD, among poor families (<120 reais per capita per month) 21.8% of grade 1 students were 9 years old or older, while in non-poor families this figure was only 12.8%.

could also reflect that children who enroll at later ages are older and so have a greater opportunity cost of time in school that in turn reduces the impact of *Bolsa* on enrollment.

Brazil's many ethnic groups differ in many ways, including education outcomes. The 2004 PNAD reports that white children age 7-15 have school enrollment rates of 97.3%. The rates for blacks, mulattos and indigenous children of that age are lower: 93.6%, 95.2% and 89.6%, respectively. The rate for Asian children, 97.6%, is slightly above that of white children. Table 6 indicates that *Bolsa* is more effective at increasing the enrollment of blacks, mulattos and indigenous children than it is for whites, so it appears to equalize enrollment by race. Surprisingly, it also increases Asian student enrollment, again relative to whites, even though Asian enrollment rates are not below those of whites (although among the poor, the rate for Asians, 93.5%, is lower than for whites, 96.0%). These impacts are large; while *Bolsa*'s average impact is to increase enrollment by about 2.6 percentage points, the increase in enrollment for schools where all students are black is about 13 points (about 10% of the students are black, so the black variable when all students are black, measured as a deviation from the mean, is 90, and so the overall impact is 0.026 + 0.0011*90 = 0.125). Similarly, the impacts on schools with all mulattos and all indigenous are about 4 and 15 percentage points, respectively (mulatto and indigenous students constitute 50% and 2% of all students, respectively).

Finally, *Bolsa*'s impact on enrollment is smaller in relatively large schools (as measured by enrollment in 1998). This is not surprising because larger schools tend to be in urban areas, where a larger percentage of children are already enrolled.

Turning to dropout rates, the impact of the *Bolsa* program varies little of school quality indicators. Regarding child characteristics, while girls are less likely to drop out of school, *Bolsa* is less effective at reducing their dropping out, perhaps because they already have relatively low rates. Yet it seems more effective at keeping children from disadvantaged backgrounds (measured by the average age when starting school) in school even though, as discussed above, it seems less effective at inducing such children to enroll. The negative impact of *Bolsa* on dropout rates is weaker for blacks but stronger for mulattos. The parameter estimate of 0.007 for blacks implies that *Bolsa* slightly increases the dropout rate by 0.1 percentage points for a school with all black students (-0.524 + 0.007*90 = 0.104). Perhaps increased enrollment for relatively weak students leads, in later years, to increased dropping out.

Finally, consider grade promotion. Overall, the positive impact of the program is weaker in schools with better school quality indicators. Girls tend to learn more in school, as measured by the grade promotion rate, but the program's impact on their promotion rates is smaller than it is for boys. The positive impact of the program on grade promotion is somewhat smaller for students from disadvantaged backgrounds (as measured by average age at enrollment), blacks, indigenous students, and Asians. One possible explanation for the smaller effects for these students is that increased enrollment for these groups brought in relatively weak students, who are more likely to repeat.

The estimates thus far implicitly assume that the impact of the *Bolsa* program does not depend on how long it has been in place. Yet impacts may accumulate over time as students are "treated" for many years. Table 7 investigates cumulative effects by lagging the *Bolsa* variable as far back as three years. For all three dependent variables, the impacts accumulate over time, peaking after 2-3 years. More specifically, the impact on enrollment is a 2.8 percentage point increase after one year, a 4.3 point increase after two years, and 5.5 points after three years. Assuming that *Bolsa* affects only students who actually participate in it (about a third of all students), these results indicate that, over the long-run, *Bolsa* raises participants enrollment by about 17% percentage points.

For dropping out and grade promotion, the estimates indicate that *Bolsa* reduces the dropout rate by 0.30 percentage points after one year and by about 0.54 points after two years. If the entire impact is concentrated on participants, the program reduces the dropout rate among participants by 1.6 percentage points. Finally, the estimates show that *Bolsa* raises the grade promotion rate by about 0.5 percentage points after one year and by nearly 1.0 points after two years, and assuming that only participants are affected implies that, in the long-run, the program raises participants' grade promotion rates by about 3 percentage points.

The estimated impacts of the *Bolsa* program on grade 5-8 students are shown in Tables 8, 9 and 10. Table 8 repeats, for these students, the regressions in Table 5. The sample is only about a third as large, since there are fewer (but larger) schools at higher levels of education, but it is still very large: about 182,000 (nearly 23,000 schools over eight years). In the simplest specification (top panel of Table 8), the *Bolsa* program appears to raise enrollment by 3.2 percentage points, which (assuming all the impact is concentrated on the third of the students who participate) implies an (average) enrollment increase of about 10 percentage points for participating students. The results also indicate that *Bolsa* reduces dropout rates, and raises grade promotion rates, by about 0.3 percentage points (average over all students), and by about 0.8 percentage points among participating students. The enrollment and dropout estimates are very similar to those in Table 5, but the impact on grade promotion is only half as large. This may reflect the fact that promotion rates in grades 5-8, at 90%, are higher than the grade 1-4 rate (about 82%), leaving less room for improvement.

The second, third and fourth panels in Table 8 check the robustness of these results. As in Table 5, the second panel uses a "fake" *Bolsa* variable to check whether the estimated impacts in the first panel are due to something else. Again, there is no evidence that the first panel results are biased, although the dropout and grade promotion estimates are not very precise.[12] The third panel drops the 2001 data. In two of three cases (the exception being enrollment), the absolute value of the coefficient increases somewhat, as in Table 5. The log enrollment estimate decreases by a small amount. In any case, the first panel results are clearly not driven by the 2001 data. Finally, the last panel in Table 8 adds several school variables to see whether doing so affects estimates of *Bolsa's* impact. Again, the estimated impacts are unaffected by adding these variables.

Table 9 examines whether the *Bolsa* program impacts vary by school and student characteristics. For enrollment, three school level interactions have significantly positive effects, and two are significantly negative; in contrast to grades 1-4, for enrollment *Bolsa* does not amplify existing inequalities in observable indicators of school quality. Yet for the dropping out and promotion regressions it does appear to amplify existing inequities.

Turning to student variable interactions, *Bolsa's* positive impact on enrollment is even stronger for both girls and children from disadvantaged backgrounds (measured by delayed enrollment). All three ethnic groups with relatively low enrollment (black, mulatto and indigenous) also had larger than average enrollment impacts. Finally, as for

[12] The coefficient estimate for enrollment in the top panel would still be highly significant in terms of the standard error in the second panel, but this is not the case for the dropping out and promotion regressions.

grades 1-4 the program impact is smaller for larger schools, again probably indicating that urban schools already have high enrollment.

For dropping out, the negative program impact is stronger for children from disadvantaged backgrounds (measured by delayed enrollment) but weaker for black, mulatto and indigenous students, which occurred only for blacks in grades 1-4. Perhaps higher enrollment for these groups in grades 1-4 causes more dropping out in grades 5-8. For grade promotion, there is no difference in *Bolsa's* impact by sex, but disadvantaged students seem to benefit more. The three disadvantaged ethnic minorities (black, mulatto and indigenous) seem to benefit less in terms of their learning, which is consistent with a smaller program impact on their dropping out and may again reflect weaker students.

As in grades 1-4, the full impact of the *Bolsa* program for grade 5-8 students may not be felt during its first year. This is examined in Table 10. As in grades 1-4, the impact on enrollment accumulates over three years. In the first year, enrollment rises by 3.0 percentage points, but after three years it rises by 6.5 points. The impact on dropping out also accumulates over three years, but the impacts are less precisely estimated. In the first year the dropout rate falls by about 0.3 percentage points, and after three years it falls by 0.4 or 0.5 percentage points. In contrast, for promotion there is no accumulative effect, unlike the pattern estimated for grades 1-4.

B. *Municipio* Level Regressions. As Section V explained, alternative estimates can be obtained at the *municipio* level. We averaged student and school characteristics in each *municipio* to create a *municipio* panel from 1998 to 2005. It was merged with Ministry of Social Development data on the percentage of families receiving *Bolsa* transfers in each *municipio*. When new *municipios* were created after 1998 by splitting an existing *municipio*, these *municipios* are recombined in the data to maintain a balanced panel. Similarly, if neighboring *municipios* were combined after 1998 into a single *municipio*, those *municipios* are combined for all years in the data.

Tables 11 and 12 show the basic estimates of the *municipio* regressions for grades 1-4 and 5-8, respectively. No results are shown that disaggregate the impact by school or student characteristics; those estimates were usually insignificant and displayed no clear patterns. Lagged results are also excluded, since they were either insignificant or had irregular patterns (perhaps due to very high correlation). In these regressions the *Bolsa* variable is the percentage of families (a proxy for the percent of students) participating in the program. Thus these are estimates of the impact of participating in the program.

The top panel in Table 11, the simplest specification, shows that a one percentage point increase in participation in *Bolsa* increases grade 1-4 enrollment by 0.13 percentage points, which implies that enrollment among participating students increases by 13 percentage points. This is close to, albeit somewhat larger than, the estimate derived from Table 5 that the program impact on participants is 8.4% (2.8%×3). The top panel of Table 11 also indicates that program participation reduces dropping out by 2.6 percentage points and raises grade promotion by 2.6 points. These are also somewhat higher than the estimates inferred from Table 5 (1.6 and 0.9 percentage points, respectively).

The second, third and fourth panels of Table 11 present the same robustness checks done in Table 5. Creating a “fake” *Bolsa* variable for the year 2000 and estimating its impact using 1998-2000 data yields insignificant, or at most marginally significant, program impacts. The impact is much smaller in magnitude for enrollment, and for all three education outcomes the coefficient changes sign. Thus it seems unlikely that the first panel results are generated by an unobserved variable that is highly correlated with *Bolsa*. Dropping the year 2001 (third panel) has little effect on the estimated program impact, and the same holds after adding several school variables (last panel).

For grades 5-8, all the estimated impacts in Table 12 have the expected sign but only one, that for grade promotion, is significant. The imprecise estimate for enrollment suggests that program participation raises participants' enrollment by 5.8%. This is lower than the 9.6% (3.2×3) effect inferred from the school level results in Table 8. The insignificant estimate of *Bolsa's* impact on the dropout rate suggests that program participation reduces that rate by 0.6 percentage points, slightly below the 0.81 percentage point (0.27×3) effect inferred from Table 8. Finally, the sole significant effect in Table 12, that participation in *Bolsa* raises grade promotion by 1.8 percentage points, is about double the inferred impact of 0.84 (0.28×3) from Table 8. Overall, these results, while imprecisely estimated, are fairly similar to those in Table 8.

Turning to the remaining panels in Table 12, the robustness checks in the third and fourth panels reveal no problems with the estimates. This same is also true for the enrollment estimate in the second panel, which uses only the 1998-2000 data, but these three years of data produce statistically significant impacts for dropping out and grade promotion that have the opposite (and counterintuitive) sign of the estimated impacts in the first panel. It is unclear what generates these counterintuitive results. In any case, there is no evidence that the imprecisely estimated results in the first panel are caused by some unobserved variable that is positively correlated with the program variable. Perhaps the most prudent conclusion is that the *municipio* level data yield little information about the impact of *Bolsa* on the education outcomes of grade 5-8 students.

VII. Conclusion

Brazil's *Bolsa Escola/Familia* program is the largest program in the world that provide incentives for families to enroll their children in school. The impact of this conditional cash transfer (CCT) program is difficult to estimate because, unlike other CCT programs in Latin America, it was not implemented as part of a randomized trial. Fortunately, school census data can be used to estimate *Bolsa's* impact given assumptions about the nature of unobserved determinants of education outcomes in Brazil.

Our school level estimates indicate that, after accounting for lagged effects, this program increased enrolment by about 5.5 percent in grades 1-4 and by about 6.5 percent in grades 5-8, decreased dropout rates by about 0.5 percentage points in grades 1-4 and by about 0.4 percent in grades 5-8, and raised grade promotion rates by about 0.9 percentage points in grades 1-4 and 0.3 percentage points in grades 5-8. Assuming that the program has little or no impact on non-participants, the impact on participants, who constitute one third of all children in Brazil, is about three times as high. *Municipio* level estimates are broadly similar, although those for grades 5-8 are quite imprecise.

It is not particularly surprising that this program has these effects. The real issue for policymakers is whether *Bolsa Escola/Familia*'s benefits exceed its costs. Simple estimates based on the enrollment impacts suggest that this may not be the case. The long-run effect of the program appears to be to increase enrollment rates among participants by about 18%. For the target population, the same increase in years of schooling implies an increase of about 1.5 years. Assuming that each year of schooling raises wages by 8%, this implies a 12% increase in wages among the poorest third of the population. This amounts to an increase of at most 2% in wages of the whole population, or perhaps about 1% of GDP. Whiles this compares favorably with the program cost of 0.35% of GDP, these costs are incurred today while the benefits accrue over the next 40 years of the working life of beneficiaries. Applying a 3% discount rate implies a net present value of about 0.60% of GDP, and applying a 6% discount rate implies a net present value of 0.40% of GDP. Adding the opportunity cost of time would reduce these figures somewhat, as would accounting for costs in terms of additional teachers and school supplies. Overall, it is not clear whether the benefits outweigh the costs.

The intuition for why the benefits may not exceed the costs is that the increase in school enrollment among participants of about 18% implies that 82% of participants would have enrolled in school even without the program, so the 82% of the funds directed to them has no effect (although this could be interpreted as a benefit solely on distributional grounds). This inefficiency raises the question of whether *Bolsa* could be targeted towards those households that would not enroll their children in the absence of the program. Answering this question is an important task for further research.

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Table 1 – Number of Schools in Brazil's School Census from 1998 to 2005.

School census years (1)	Total number of schools (2)	Schools with 1st to 4th and/or 5th to 8th grade classes (3)	School with panel data from 1998 to current year (4)
1998	267,532	187,514	187,514
1999	266,645	183,475	174,153
2000	261,988	181,532	166,251
2001	264,735	177,808	157,081
2002	256,986	172,529	148,209
2003	253,405	169,096	141,716
2004	248,257	143,262	116,285
2005	248,103	136,114	107,243

Source: school census.

Table 2 – Student and School Characteristics: Public Schools with grades 1-4, 1998-2005.

A. Student Characteristics					
Years	Total Enrollment	Grade Promotion	Drop out rate	Number of students/ classroom	% of grade 4 students with age-grade distortion
1998	135.3	68.4	14.5	27.5	56.6
1999	134.4	70.6	12.8	27.0	53.2
2000	130.4	69.5	13.6	26.4	50.4
2001	127.3	72.4	10.6	25.8	47.3
2002	124.7	73.0	9.5	25.4	43.6
2003	120.5	72.9	9.5	25.1	40.4
2004	115.5	70.9	9.9	24.8	37.5
2005	110.4	72.7	8.9	24.5	37.0

B. Indicators of School Quality					
Years	Percent of teachers with college	% of schools with computers	% of schools with printers	% of schools with computer labs	% of schools with *Bolsa Escola/Familia* Program
1998	8.4	8.7	8.6	1.4	-
1999	9.3	11.4	10.8	1.9	-
2000	9.8	13.6	12.9	2.6	-
2001	11.0	16.1	15.3	3.2	23.5
2002	12.8	18.8	17.9	4.3	84.7
2003	16.7	21.7	20.5	5.5	88.3
2004	20.7	21.8	21.6	12.1	90.3
2005	26.2	23.8	21.6	8.0	90.8

Source: school census

Table 3 – School and Characteristics in 2001: Public schools with and without *Bolsa*.

	Schools with Grades 1-4			*Schools with Grades 5-8*		
	All Schools	*Bolsa* Schools	Non-*Bolsa* Schools	All Schools	*Bolsa* Schools	Non-*Bolsa* Schools
Total Enrollment	127.3	97.5	136.5	406.8	334.0	418.0
Grade Promotion	72.4	70.5	73.0	79.0	78.6	79.1
Dropping out rate	10.6	11.6	10.3	11.1	12.7	10.8
Number students per classroom	25.8	25.0	25.9	31.1	29.4	31.3
% of teachers with college	11.0	6.8	12.2	65.0	51.7	67.0
% students age-grade distortion	47.3	50.8	46.2	49.3	55.0	48.4
% of schools with computer	16.1	8.2	18.5	61.8	46.6	64.1
% of schools with printer	15.3	7.7	17.6	59.2	44.3	61.5
% of schools with computer lab	3.24	1.5	3.8	22.7	15.7	23.8

The grade 1-4 sample size is between 20,548 and 20,569 for treatment schools and 66,779 and 66,843 for control schools, except for students per classroom, which is 5,698 in treatment schools and 25,166 in control schools. The grade 5-8 sample size varies from 3,032 to 3,035 for treatment schools and 19,732 to 19,747 for control schools, except for students per classroom, which is 2,194 in treatment schools and 15,349 in control schools.

Table 4 – Student and School Characteristics: Public Schools with grades 1-4, 1998-2005.

A. Student Characteristics

Years	Total Enrollment	Grade Promotion	Drop out rate	Number of students/ classroom	% of grade 8 students with age-grade distortion
1998	394.9	78.2	13.6	32.4	56.3
1999	408.7	78.2	12.7	32.3	53.8
2000	415.0	77.3	13.3	32.0	52.3
2001	406.8	79.0	11.1	31.1	49.3
2002	397.6	78.7	10.3	30.7	46.4
2003	378.5	78.4	9.9	30.5	43.1
2004	359.4	76.7	10.2	30.2	40.2
2005	348.8	77.4	9.1	30.0	37.8

B. Indicators of School Quality

Years	Percent of teachers with college	% of schools with computers	% of schools with printers	% of schools with computer labs	% of schools with *Bolsa Escola/Familia* Program
1998	62.4	38.4	37.6	9.2	--
1999	62.9	48.5	46.3	16.7	--
2000	63.8	54.6	52.2	19.9	--
2001	65.0	61.8	59.2	22.7	13.3
2002	67.9	69.2	66.4	26.9	76.0
2003	71.1	76.1	72.9	30.8	83.2
2004	75.6	75.7	75.0	48.6	85.8
2005	80.3	79.0	73.4	37.0	86.4

Source: school census

Table 5 – Program Impact on Enrollment, Dropping out and Promotion: Basic Results (public schools with grades 1 to 4)

	Log enrollment		Dropping out		Promotion	
Variables	Coef.	S. E.	Coef.	S. E.	Coef.	S. E.
Basic Model (1998-2005)						
School with *Bolsa Escola/Familia*	.0282***	.0018	-.309 ***	.0582	.533 ***	.0779
Number of observations	699,255		698,229		698,229	
F – test	375.6 ***		350.5 ***		182.3 ***	
Basic Model (1998-2000 only)						
School with *Bolsa Escola/Familia* in 2001, assigned to year 2000	.00136	.0028	-.031	.136	.265	.168
Number of observations	262,220		261,845		261,845	
F – test	71.9 ***		22.3 ***		32.8 ***	
Basic Model (dropping 2001))						
School with *Bolsa Escola/Familia*	.0330 ***	.0025	-.582 ***	.0757	.721 ***	.101
Number of observations	611,848		610,902		610,902	
F – test	374.0 ***		350.0 ***		177.2 ***	
Adding School/Child Variables (1998-2005)						
School with *Bolsa Escola/Familia*	.0274 ***	.0018	-.310 ***	.058	.530 ***	.0780
Computer lab	.0353 ***	.0033	-.094 *	.056	.604 ***	.0776
Computer	.0491 ***	.0031	-.017	.060	.269 ***	.0799
Library	.0203 ***	.0027	.090 *	.053	-.102	.0707
Teacher college	.0001 ***	.0000	-.002 ***	.001	.0002	.0010
Program meal	.0125 ***	.0032	-.316 ***	.108	.208	.1365
Program school TV	.0042 **	.0017	-.008	.044	-.121 **	.0569
Program computer	-.0136 ***	.0021	.174 ***	.045	-.403 ***	.0698
Girl	.0014 ***	.0001	-.013 ***	.002	.031 ***	.0031
Number of observations	699,255		698,229		698,229	
F – test	353.8 ***		314.1 ***		165.3 ***	
Control variables (all regressions)						
Dummies for years 98 – 05	yes		yes		yes	
Trend x enrollment level in 98 (08)	yes		yes		yes	
Trend x states x Bolsa in 2001 (27)	yes		yes		yes	
Trend x states x Bolsa in 2002+ (27)	yes		yes		yes	
Dummies for Schools (87,407)	yes		yes		yes	

Robust standard-errors.
*** significant at 1% level.
** significant at 5% level.
* significant at 10% level.

Table 6 - Program Impact on Enrollment, Dropping out and Promotion: Adding Interaction Terms (public schools with grades 1 to 4, 1998-2005)

	Log enrollment		Dropping out		Promotion	
Variables	Coef.	S. E.	Coef.	S. E.	Coef.	S. E.
School with *Bolsa* Program	0.0261 ***	0.0022	-0.524 ***	0.078	0.292 ***	0.104
Computer lab	0.0194 ***	0.0045	-0.009	0.158	0.188	0.210
Computer	0.0393 ***	0.0030	-0.204 *	0.107	0.430 ***	0.142
Library	0.0114 ***	0.0025	-0.161 *	0.089	0.274 **	0.118
Teacher college	-0.0001 ***	0.0000	-0.005 ***	0.001	0.005 ***	0.002
Program meal	0.0096 ***	0.0027	-0.177 *	0.097	0.077	0.128
Program school TV	0.0036 *	0.0019	-0.059	0.068	0.063	0.090
Program computer	0.0002	0.0029	-0.194 *	0.102	-0.122	0.135
Computer lab x *Bolsa* Program	0.0184 ***	0.0049	-0.186	0.175	0.769 ***	0.232
Computer x *Bolsa* Program	0.0485 ***	0.0036	0.210	0.129	-0.277	0.171
Library x *Bolsa* Program	0.0226 ***	0.0029	0.448 ***	0.104	-0.769 ***	0.138
Teacher college x *Bolsa* Prog	0.0004 ***	0.0000	0.003 *	0.001	-0.006 ***	0.002
Prog school TV x *Bolsa* Prog	0.0002	0.0025	0.132	0.088	-0.393 ***	0.116
Prog computer x *Bolsa* Prog	-0.0147 ***	0.0039	0.669 ***	0.139	-0.720 ***	0.185
Girl	0.0004 ***	0.0001	-0.022 ***	0.002	0.039 ***	0.003
Girl x *Bolsa Escola/Familia*	0.0021 ***	0.0001	0.018 ***	0.003	-0.012 ***	0.004
Age-repetition 98 x *Bolsa*	-0.0049 ***	0.0006	-0.549 ***	0.023	-0.169 ***	0.030
Black 05 x *Bolsa Escola/Fam.*	0.0011 ***	0.0001	0.007 ***	0.002	-0.017 ***	0.003
Mulato 05 x *Bolsa Escola/Fam.*	0.0004 ***	0.0000	-0.005 ***	0.001	0.001	0.002
Indigenous 05 x *Bolsa Escola*	0.0015 ***	0.0001	0.006 *	0.003	-0.025 ***	0.004
Yellow 05 x *Bolsa Escola/Fam.*	0.0012 ***	0.0001	0.007 *	0.004	-0.023 ***	0.005
(Enrollment 98/1000) x *Bolsa*	-0.155 ***	0.0069	-1.801 ***	0.245	1.043 ***	0.325
Number of observations	562,408		561,789		561,789	
F – test	518.4 ***		227.3 ***		116.8 ***	
Control variables:						
Dummies for years 98 – 05	yes		yes		yes	
Trend x enrollment in 98 (08)	yes		yes		yes	
Trend x states x Bolsa in 2001 (27)	yes		yes		yes	
Trend x states x Bolsa in 2002+ (27)	yes		yes		yes	
Dummies for Schools (87,407)	yes		yes		yes	

Robust standard-errors.
*** significant at 1% level.
** significant at 5% level.
* significant at 10% level.

Notes: For all interaction terms, student and school characteristics are rescaled to have a mean of zero, so that the coefficient on the *Bolsa* variable indicates the impact on average student in an average school.

Table 7 - Program Impact on Enrollment, Dropping out and Promotion: Adding Program Lag Terms (public schools with grades 1 to 4, 1998-2005)

	Log enrollment		**Dropping out**		**Promotion**	
Variables	Coef.	S. E.	Coef.	S. E.	Coef.	S. E.
School with *Bolsa* Program	0.0276 ***	0.0018	-0.299 ***	0.059	0.508 ***	0.079
School with *Bolsa* lagged 1 year	0.0153 ***	0.0019	-0.245 ***	0.059	0.440 ***	0.082
School with *Bolsa* lagged 2 year	0.0124 ***	0.0022	-0.023	0.065	0.162 *	0.090
School with *Bolsa* lagged 3 year	0.0012	0.0024	0.046	0.077	-0.196 *	0.105
Computer lab	0.0348 ***	0.0033	-0.086	0.056	0.586 ***	0.078
Computer	0.0488 ***	0.0031	-0.015	0.060	0.266 ***	0.080
Library	0.0202 ***	0.0026	0.091 *	0.053	-0.104	0.071
Teacher college	0.00014 ***	0.00003	-0.002 ***	0.001	0.000	0.001
Program meal	0.0128 ***	0.0032	-0.321 ***	0.108	0.216	0.136
Program school TV	0.0041 **	0.0017	-0.007	0.044	-0.125 **	0.057
Program computer	-0.0131 ***	0.0021	0.168 ***	0.045	-0.390 ***	0.070
Girl	0.0014 ***	0.00009	-0.013 ***	0.002	0.031 ***	0.003
Number of observations	699,255		698,229		698,229	
F – test	340.4 ***		302.2 ***		159.1 ***	
Control variables:						
Trend x enrollment in 98 (08)	yes		yes		yes	
Dummies for years 98 – 05	yes		yes		yes	
Trend x states x Bolsa in 2001 (27)	yes		yes		yes	
Trend x states x Bolsa in 2002+ (27)	yes		yes		yes	
Dummies for Schools (87,407)	yes		yes		yes	

Robust standard-errors.
*** significant at 1% level.
** significant at 5% level.
* significant at 10% level.

Table 8 – Program Impact on Enrollment, Dropping out and Promotion: Basic Results (public schools with grades 5 to 8)

	Log enrollment		Dropping out		Promotion	
Variables	Coef.	S. E.	Coef.	S. E.	Coef.	S. E.
Basic Model (1998-2005)						
School with *Bolsa Escola/Familia*	.032***	.0031	-.273 ***	.075	.282 ***	.0925
Number of observations	182,192		182,007		182,007	
F – test	176.6 ***		119.1 ***		60.9 ***	
Basic Model (1998-2000 only)						
School with *Bolsa Escola/Familia* in 2001, assigned to year 2000	-.00004	.0061	-.157	.230	-.054	.267
Number of observations	68,322		68,204		68,204	
F – test	74.2 ***		9.61 ***		15.5 ***	
Basic Model (dropping 2001)						
School with *Bolsa Escola/Familia*	.0273***	.0038	-.436 ***	.089	.427 ***	.111
Number of observations	129,129		129,043		129,043	
F – test	155.3 ***		112.4 ***		53.2 ***	
Adding School/Child Variables (1998-2005)						
School with *Bolsa Escola/Familia*	.0317 ***	.0031	-.267 ***	.075	.260 ***	.092
Computer lab	.0096 ***	.0033	-.176 **	.069	.030	.089
Computer	.0080 **	.0036	-.060	.082	-.090	.102
Library	-.0079 **	.0031	-.181 ***	.067	-.059	.084
Teacher college	.0000	.0001	.001	.001	-.010 ***	.002
Program meal	-.0036	.0041	-.172	.108	-.341 ***	.129
Program school TV	.0004	.0023	-.105 *	.057	.278 ***	.070
Program computer	-.0144 ***	.0024	-.122 **	.056	.169 **	.071
Girl	-.0014 ***	.0003	-.053 ***	.006	.085 ***	.008
Number of observations	182,191		182,006		182,006	
F – test	158.5 ***		107.9 ***		56.8 ***	
Control variables (all regressions)						
Dummies for years 98 – 05	yes		yes		yes	
Trend x enrollment level in 98 (09)	yes		yes		yes	
Trend x states x Bolsa in 2001 (27)	yes		yes		yes	
Trend x states x Bolsa in 2002+ (27)	yes		yes		yes	
Dummies for Schools (22,774)	yes		yes		yes	

Robust standard-errors.
*** significant at 1% level.
** significant at 5% level.
* significant at 10% level.

Table 9 - Program Impact on Enrollment, Dropping out and Promotion: Adding Interaction Terms (public schools with grades 5 to 8, 1998-2005)

	Log enrollment		**Dropping out**		**Promotion**	
Variables	Coef.	S. E.	Coef.	S. E.	Coef.	S. E.
School with *Bolsa* Program	0.0351 ***	0.0027	-0.194 **	0.089	0.364 ***	0.109
Computer lab	-0.0351 ***	0.0032	-0.067	0.091	-0.202 *	0.112
Computer	0.0042	0.0029	0.045	0.086	-0.061	0.106
Library	-0.0100 ***	0.0024	-0.312 ***	0.070	0.103	0.086
Teacher college	0.0003 ***	0.0000	0.008 ***	0.001	-0.013 ***	0.001
Program meal	-0.0109 ***	0.0033	-0.128	0.097	-0.368 ***	0.118
Program school TV	0.0126 ***	0.0022	0.016	0.063	0.143 *	0.078
Program computer	-0.0080 ***	0.0026	-0.138 *	0.076	0.104	0.093
Computer lab x *Bolsa* Program	0.0788 ***	0.0039	-0.137	0.111	0.277 **	0.136
Computer x *Bolsa* Program	0.0134 ***	0.0040	-0.438 ***	0.116	0.351 **	0.142
Library x *Bolsa* Program	0.0103 ***	0.0031	0.272 ***	0.088	-0.317 ***	0.108
Teacher college x *Bolsa* Prog	-0.0005 ***	0.0000	-0.019 ***	0.001	0.011 ***	0.002
Prog school TV x *Bolsa* Prog	-0.0301 ***	0.0030	-0.195 **	0.086	0.157	0.106
Prog computer x *Bolsa* Prog	0.0000	0.0038	-0.095	0.109	0.188	0.133
Girl	-0.0038 ***	0.0002	-0.062 ***	0.005	0.101 ***	0.006
Girl x *Bolsa Escola/Familia*	0.0046 ***	0.0002	-0.005	0.007	-0.001	0.008
Age-repetition 98 x *Bolsa*	0.0216 ***	0.0015	-1.573 ***	0.044	1.093 ***	0.054
Black 05 x *Bolsa Escola/Fam.*	0.0003 **	0.0001	0.011 ***	0.004	-0.030 ***	0.005
Mulato 05 x *Bolsa Escola/Fam.*	0.0006 ***	0.0001	0.013 ***	0.002	-0.025 ***	0.003
Indigenous 05 x *Bolsa Escola*	0.0008 ***	0.0002	0.040 ***	0.007	-0.053 ***	0.009
Yellow 05 x *Bolsa Escola/Fam.*	0.0004 *	0.0002	0.002	0.007	-0.015 *	0.009
(Enrollment 98/1000) x *Bolsa*	-0.1352 ***	0.0053	0.282 *	0.164	-0.076	0.201
Number of observations	147,575		147,484		147,484	
F – test	557.5 ***		120.4 ***		73.3 ***	
Control variables:						
Dummies for years 98 – 05	yes		yes		yes	
Trend x enrollment in 98 (09)	yes		yes		yes	
Trend x states x Bolsa in 2001 (27)	yes		yes		yes	
Trend x states x Bolsa in 2002+ (27)	yes		yes		yes	
Dummies for Schools (22,774)	yes		yes		yes	

Robust standard-errors.
*** significant at 1% level.
** significant at 5% level.
* significant at 10% level.

Table 10 - Program Impact on Enrollment, Dropping out and Promotion: Adding Program Lag Terms (public schools with grades 5 to 8, 1998-2005)

	Log enrollment		Dropping out		Promotion	
Variables	Coef.	S. E.	Coef.	S. E.	Coef.	S. E.
School with *Bolsa* Program	0.0297 ***	0.0029	-0.264 ***	0.074	0.259 ***	0.092
School with *Bolsa* lagged 1 year	0.0202 ***	0.0030	-0.111	0.072	-0.046	0.093
School with *Bolsa* lagged 2 year	0.0155 ***	0.0036	-0.177 **	0.081	-0.088	0.104
School with *Bolsa* lagged 3 year	-0.0029	0.0042	-0.076	0.097	-0.068	0.126
Computer lab	0.0087 ***	0.0033	-0.169 **	0.069	0.034	0.089
Computer	0.0079 **	0.0036	-0.059	0.082	-0.089	0.102
Library	-0.0078 **	0.0031	-0.180 ***	0.067	-0.058	0.084
Teacher college	0.0000	0.0001	0.001	0.001	-0.010 ***	0.002
Program meal	-0.0037	0.0041	-0.169	0.108	-0.340 ***	0.129
Program school TV	0.0002	0.0023	-0.103 *	0.057	0.278 ***	0.070
Program computer	-0.0140 ***	0.0024	-0.124 **	0.056	0.169 **	0.071
Girl	-0.0014 ***	0.0003	-0.053 ***	0.006	0.085 ***	0.008
Number of observations	182,191		182,006		182,006	
F – test	152.7 ***		104.0 ***		54.7 ***	
Control variables:						
Dummies for years 98 – 05	yes		yes		yes	
Trend x enrollment in 98 (09)	yes		yes		yes	
Trend x states x Bolsa in 2001 (27)	yes		yes		yes	
Trend x states x Bolsa in 2002+ (27)	yes		yes		yes	
Dummies for Schools (22,774)	yes		yes		yes	

Robust standard-errors.
*** significant at 1% level.
** significant at 5% level.
* significant at 10% level.

Table 11 – Program Impact on Enrollment, Dropping out and Promotion: Basic Results (*municipio* level regressions for public schools with grades 1 to 4)

	Log enrollment		**Dropping out**		**Promotion**	
Variables	Coef.	S. E.	Coef.	S. E.	Coef.	S. E.
Basic Model (1998-2005)						
% families w/ *Bolsa Escola/Familia*	.00128***	.00035	-.0260***	.0068	.0262 ***	.0092
Number of observations	35,530		35,523		35,523	
F – test	26.0 ***		174.9 ***		58.9 ***	
Basic Model (1998-2000 only)						
% families w/ *Bolsa Escola/Familia* in 2001, assigned to year 2000	-.000545	.00044	.0333	.020	-.0419 *	.025
Number of observations	13,468		13,463		13,463	
F – test	187.6 ***		42.3 ***		560.5 ***	
Basic Model (dropping 2001))						
% families w/ *Bolsa Escola/Familia*	.00134***	.00038	-.0232***	.0071	.0202 ***	.0097
Number of observations	31,085		31,080		31,080	
F – test	24.5 ***		169.1 ***		63.7 ***	
Adding School/Child Variables (1998-2005)						
% families w/ *Bolsa Escola/Familia*	0.00124***	0.00033	-0.0268***	0.0068	0.0279***	0.0092
Computer lab	-0.01130	0.01394	-0.2695	0.1956	1.342 ***	0.2759
Computer	0.22366***	0.02111	0.1962	0.3184	-0.134	0.4049
Library	0.21441***	0.01553	0.1042	0.2178	-0.341	0.2838
Teacher college	0.00030 *	0.00017	-0.0025	0.0026	-0.0082**	0.0039
Program meal	0.00420	0.01220	-0.5476**	0.2772	-0.3001	0.3606
Program school TV	0.168 ***	0.01133	0.0520	0.1609	-0.0321	0.2312
Program computer	-0.0124 *	0.00703	0.1810 *	0.1039	-0.338 *	0.1941
GNP per capita / 1000	0.00127**	0.00061	-0.0088	0.0092	0.0131	0.0138
Girl	0.00381***	0.00084	0.0216	0.0148	-0.0075	0.0196
Number of observations	35,530		35,523		35,523	
F – test	31.8 ***		123.3 ***		40.1 ***	
Control variables (all regressions)						
Dummies for years 98 – 05	yes		yes		yes	
Trend x enrollment level in 98 (08)	yes		yes		yes	
Trend x states (27)	yes		yes		yes	
Dummies for *municipios* (4,523)	yes		yes		yes	

Robust standard-errors.
*** significant at 1% level.
** significant at 5% level.
* significant at 10% level.

Table 12 – Program Impact on Enrollment, Dropping out and Promotion: Basic Results (*municipio* level regressions for public schools with grades 5 to 8)

	Log enrollment		**Dropping out**		**Promotion**	
Variables	Coef.	S. E.	Coef.	S. E.	Coef.	S. E.
Basic Model (1998-2005)						
% families w/ *Bolsa Escola/Familia*	.000583	.00038	-.0064	.0069	.0180 **	.0086
Number of observations	35,654		35,649		35,649	
F – test	1495.9***		159.2 ***		140.9 ***	
Basic Model (1998-2000 only)						
% families w/ *Bolsa Escola/Familia* in 2001, assigned to year 2000	.000505	.00055	.0684 ***	.023	-.0554 **	.028
Number of observations	13,449		13,445		13,445	
F – test	3028 ***		147.8***		307.0 ***	
Basic Model (dropping 2001))						
% families w/ *Bolsa Escola/Familia*	.000519	.00040	-.00698	.0074	.0134	.00924
Number of observations	31,187		31,182		31,182	
F – test	1305 ***		153.4 ***		161.6 ***	
Adding School/Child Variables (1998-2005)						
% families w/ *Bolsa Escola/Familia*	0.00054	0.00037	-0.0077	0.0069	0.0195 **	0.0086
Computer lab	-0.00289	0.0094	-0.411 **	0.186	0.2193	0.241
Computer	0.0801***	0.0107	0.0648	0.222	-0.4462	0.279
Library	0.0883***	0.0088	-0.2117	0.170	-0.1741	0.218
Teacher college	0.00093***	0.00014	-0.0016	0.0030	-0.0156***	0.0037
Program meal	-0.00448	0.010	-0.0053	0.266	-0.731 **	0.305
Program school TV	0.0725***	0.0076	-0.0726	0.156	0.204	0.198
Program computer	-0.0171***	0.0050	-0.706***	0.123	0.550 ***	0.159
GNP per capita / 1000	0.00159**	0.00066	-0.0074	0.012	0.0093	0.015
Girl	-0.00165**	0.00070	-0.0915***	0.013	0.125 ***	0.016
Number of observations	35,654		35,649		35,649	
F – test	112.9 ***		100.3 ***		44.7 ***	
Control variables (all regressions)						
Dummies for years 98 – 05	yes		yes		yes	
Trend x enrollment level in 98 (08)	yes		yes		yes	
Trend x states (27)	yes		yes		yes	
Dummies for *municipios* (4,523)	yes		yes		yes	

Robust standard-errors.
*** significant at 1% level.
** significant at 5% level.
* significant at 10% level.