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The Impact of *Bolsa Família* on Schooling

Girls' Advantage Increases and Older Children Gain

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ABSTRACT

Brazil's *Bolsa Família* is one of the largest conditional cash transfer programs in the world, providing cash grants to poor households conditional on their fulfilling a series of requirements related to schooling and health. Despite the importance of schooling for future labor market outcomes and the relevance of this large-scale program for other conditional cash transfer programs, there is little evidence on Bolsa Família's impacts on children's education outcomes. We estimate the impact of Bolsa Família on a range of education outcomes, including school participation, grade progression, grade repetition, and dropout rates. Using a large-sample household panel survey from 2005–2009 collected for this evaluation, we develop a statistically balanced comparison group of eligible nonparticipant households and estimate impacts using propensity-score-weighted regression. We estimate that Bolsa Família increased average school participation among all children age 6 to 17 years by (a weakly significant) 4.5 percent. It had no effect on grade promotion, on average. However, within the subsample of girls, Bolsa Família caused substantial improvements in schooling outcomes, including significant increases in school participation (8.2 percent) and rates of grade progression (10.4 percent). We show that the gains in girls' schooling do not derive from catch-up effects, but rather increase girls' existing advantage in schooling attainment. In general, impacts are larger among older children, in rural areas, and in the Northeast.

Keywords: conditional cash transfer programs, education, Bolsa Família, Brazil

JEL Classification: I21, I38, O15

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1. INTRODUCTION

A key feature of conditional cash transfer (CCT) programs is the dual aim to provide assistance to poor households while creating incentives for human capital investments in children to break the intergenerational transmission of poverty. Numerous developing and developed countries use CCT programs to promote childhood education by including a requirement, or *conditionality*, that children enroll in and attend school. An extensive literature documents that in Latin America and elsewhere, CCT programs with schooling conditionalities have caused significant increases in school participation rates (see Fiszbein and Schady [2009] for a review). The findings have spurred the creation of more CCT programs with similar requirements. The justification for CCT programs also depends ultimately on their impacts on education attainment, or, in the short term, school progression. Impacts of CCT programs on school progression are mixed and are often limited to a narrower segment of the school-age population than impacts on school enrollment or attendance.

Also of interest is who benefits from CCT programs and why. Impacts on school enrollment are often larger for children in poorer households (Glewwe and Olinto 2004; Maluccio and Flores 2004; Filmer and Schady 2008) and during years of transition into school or between school levels (Schultz 2004; Schady and Araujo 2008; de Brauw et al. 2012). Several studies report larger impacts on schooling outcomes for girls than for boys (Schultz 2004; Ahmed et al. 2007; de Brauw et al. 2012). In some cases, this reflects the common finding that impacts are larger among groups with low initial outcomes, such as in areas where girls traditionally lag behind boys in school enrollment.[1]

However, the impact of CCT programs on education outcomes may differ between girls and boys due to sex-based differences in opportunity costs of schooling, returns to education, and cost or ease of school attainment and performance. Which of those factors contributes to differences in impacts by sex is not well understood, although identifying the impact pathways would have important implications for program targeting.

In this paper, we study the impacts on school enrollment and school progression (dropout rates, grade progression, and grade repetition) of the *Bolsa Família* program in Brazil, including a detailed exploration of differences in impacts by children's sex, age, and area of residence. Bolsa Família, which began in 2003, is now the largest CCT program in the world, with more than 12 million beneficiary households in 2012. It covers both rural and urban areas across all five regions in Brazil. Initially, the schooling conditionality under Bolsa Família covered children aged 6 to 15 years, but a complementary program introduced in 2008 called *Benefício Variável Jovem* (BVJ) expanded access to transfers with the schooling conditionality to children aged 16 to 17 years. Although the benefits schedule pays larger transfers to older children under BVJ than to children aged 6 to 15, notably the transfer amounts do not differ by sex. This leads to interesting comparisons with the age and sex distribution of impacts of Latin America's other large CCT program, Mexico's *Oportunidades* program (formerly PROGRESA), where the transfer schedule varied by sex and by current grade level.

Our analysis makes use of a rich longitudinal household-level dataset of more than 11,000 households collected specifically to evaluate Bolsa Família, with survey rounds in 2005 and 2009. The data include households' reports of children's school participation and grade levels in current and previous years, allowing analysis of both enrollment and grade transitions over time. The large sample and broad coverage of the program permit disaggregations of analysis by sex, age, and location. We also provide evidence on two dimensions of heterogeneity of impact that are rare for Latin American CCT programs: (1) comparisons between rural and urban areas, and (2) comparisons between children within the mandatory age for schooling (6 to 14) and older children (15 to 17). The key empirical challenge in estimating impacts of Bolsa Família is the nonrandomized assignment of the program. We take advantage of variation in rollout over time and across municipalities for identification. Using propensity-score-

[1] Some programs, such as PROGRESA in Mexico and Turkey's CCT program, provided larger transfers to households with girls as a strategy to reduce the sex gap in school enrollment. In cases, one cannot determine whether the larger impact of the program on girls' enrollment derives from lower initial enrollment rates for girls or from the higher payments made to them.

weighted regression (Hirano, Imbens, and Ridder 2003), we construct a statistically balanced comparison group of eligible nonrecipient households, based on preprogram characteristics of households and municipalities, to estimate impacts on schooling outcomes.

Our main findings relate to differences in impact by sex. Although we find a modest and weakly significant increase in aggregate school participation (4.5 percentage points) over all 6-to-17-year-olds, the impacts of Bolsa Família on schooling outcomes are larger and broader for girls than boys. In particular, Bolsa Família significantly increases school participation by 8.2 percentage points for girls age 6 to 17, but it has no impact on average school participation for boys in this age range. Also, Bolsa Família has no effect on the average probability of progressing to the next grade for children aged 6 to 17, but it has a large impact in grade progression for girls aged 6 to 17 (10.4 percentage points) and even larger impacts among girls aged 15 to 17 (20.9 percent). Interestingly, girls experience these disproportionate gains in schooling outcomes relative to boys, despite having similar or slightly better rates of preprogram enrollment and grade progression than boys. Moreover, because Bolsa Família's payments do not differ for boys and girls of the same age, the stronger impacts for girls cannot be driven by differing direct incentives.

We also find evidence of other sources of considerable heterogeneity. Consistent with evidence for other CCT programs, effects on schooling outcomes are typically larger for older children (15 to 17 years) than younger children (6 to 14 years). We also find that impacts tend to be larger in rural areas than in urban areas, and in the historically disadvantaged Northeast region than elsewhere.

The most closely related work to ours is Glewwe and Kassouf (2012).[2] They use Brazilian school census data from 1998 to 2005, during which time *Bolsa Escola* (a predecessor program to Bolsa Família) and Bolsa Família were gradually rolled out, to estimate program impacts on schooling outcomes. They find that the program increased school enrollment by 5.5 to 6.5 percent, reduced dropout rates by 0.4 to 0.5 percentage points, and increased grade promotion rates by 0.3 to 0.9 percentage points. However, because their results refer to the period through 2005, when the program was smaller and before BVJ was added in 2008, there is potential for very different impacts on schooling outcomes in the later years we study. Moreover, because they have only school-level information available, they are not able to estimate impacts on unconditional participation rates and have limited ability to control for household characteristics.

Although Bolsa Família is the largest CCT program in the world, there is very limited evidence on its impacts. In contrast to Mexico's PROGRESA/*Oportunidades*, which is the subject of a vast literature, due largely to the strength of its initial randomized design and comprehensive data collection (for example, see Skoufias and Parker 2001; Schultz 2004; Behrman, Sengupta, and Todd 2005; Dubois, de Janvry, and Sadulet 2008),[3] no randomized evaluation was designed for Brazil, nor were large longitudinal household survey data available prior to the collection of the data used here. Thus, our work is among the few to attempt a rigorous evaluation of the Bolsa Família program in any dimension.

The paper proceeds as follows. Section 2 provides a description of the Bolsa Família and BVJ program, focusing on the education conditionality and highlighting features that motivate our identification strategy. Section 3 describes the data, provides descriptive statistics on the education indicators we study, and describes our use of the data to construct treatment and comparison groups. Section 4 describes our identification strategy, including the use of propensity score weighting to balance preprogram observables across the treatment and comparison groups. Section 5 presents our results with discussion. Section 6 concludes.

[2] To our knowledge, Glewwe and Kassouf (2012) is the only other paper rigorously estimating schooling impacts of Bolsa Família. Estimates on the schooling impacts of Bolsa Escola, the precursor to Bolsa Família, include Cardoso and Sousa (2003) and Bourguignon, Ferreira, and Leite (2003), who use micro-simulation.

[3] The impact of other Latin American CCT programs on education outcomes has also been studied in Maluccio and Flores (2004) and Glewwe and Olinto (2004).

2. THE BOLSA FAMÍLIA PROGRAM AND THE POLICY CONTEXT

The Bolsa Família Program

Payments associated with Bolsa Família consist of (1) a conditional variable payment per child aged 0 to 15 years, for up to three children, to "poor" households below a per capita income threshold, and (2) an additional unconditional transfer to "extremely poor" households below a lower per capita income threshold. The transfer is conditional on pregnant women receiving timely prenatal and postnatal care visits, all children aged 0 to 5 within the household receiving timely vaccinations and growth-monitoring visits, and all children aged 6 to 15 attending school at least 85 percent of school days. In 2008, the complementary BVJ program was introduced, which added variable payments and a schooling conditionality for children aged 16 and 17, requiring attendance at least 80 percent of school days. After BVJ's introduction, any household in Bolsa Família with children aged 16 or 17 was automatically part of BVJ following the same eligibility thresholds and without any additional enrollment procedure.

The per capita income thresholds and benefit values for Bolsa Família and BVJ have changed over time, roughly to account for inflation. At the program's inception in 2003, the per capita monthly income thresholds were 100 reais (R$) (approximately US$50), for "poor" households and R$50 for "extremely poor" households. The monthly per child conditional variable payments were R$15 for ages 6 to 15, and the monthly unconditional transfer to extremely poor households was R$50. Table 2.1 shows the benefit schedule as of September 2009, near the end of the period we study. Notably, the BVJ per child payments for children age 16 or 17 (R$30) were larger than the per child payments for children age 6 to 15 (R$20), but neither benefit type differed by the sex of the child.[4]

Table 2.1 Benefit schedule for Bolsa Família and BVJ payments (in reais), as of September 2009

Monthly household income per capita is less than . . .	Basic benefit (unconditional transfer)	Variable benefit (transfer per child aged 6–15 years fulfilling conditionality, up to three children)	BVJ benefit (transfer per child aged 16–17 years fulfilling conditionality, up to two children)
R$137 ("poor")	-	R$20	R$30
R$69 ("extremely poor")	R$62	R$20	R$30

Source: Brazilian Ministry of Social Development (2012).

To be eligible for Bolsa Família payments, households must be listed in a registry called the *Cadastro Único*. That registry contains self-reported information on household demographic characteristics, household income, and prior participation in transfer programs. All households are free to register in the Cadastro. However, municipality-level officials are responsible for organizing the registration process, such that there is substantial heterogeneity across municipalities in targeting for Cadastro registration, as well as in registration methods (Lindert et al. 2007).[5]

Moreover, conditional on registration in the Cadastro, the criteria for selection into Bolsa Família also differ by municipality. Beneficiaries are selected at the national level, through the following procedure. Each municipality is assigned a quota for a maximum number of Bolsa Família recipient households based roughly on poverty maps.[6] If the number of households in a particular municipality

[4] This feature differs from several other CCT programs, such as PROGRESA, for example, in which the benefit value depended on the child's grade level and, in secondary school, was higher for girls than for boys.

[5] For example, in some municipalities, officials go door-to-door for registration, but in others, households are asked to register at a central place, such as a school, or to attend a town hall meeting to register.

[6] We could not obtain an algorithm for how the quotas were calculated from poverty map characteristics, suggesting that the mapping may have been rough, rather than derived through a precise calculation. Municipality-level quotas have also gradually increased over time, as program coverage has expanded.

with income per capita below the threshold is lower than the municipality's quota, then all such households are selected for Bolsa Família. If the number of households in a particular municipality with income per capita below the threshold exceeds the municipality's quota, priority is assigned for selecting households into Bolsa Família, roughly according to the following criteria[7]: (1) lower household income per capita, and (2) the number of children aged 0 to 17 in the household. Consequently, while eligibility criteria are similar across municipalities, two households below the income per capita threshold with very similar characteristics may have different recipient status, due to being in municipalities with different quotas and due to differences in prioritization of eligibility criteria across municipalities.

An implication of these program features is that there is likely to be some dimension of self-selection into the program, in the sense that Cadastro registration is voluntary. This feature will motivate our conditioning of impact estimates on Cadastro registration, described in a later subsection, "Descriptive Statistics on Education Indicators." Another implication is that there are likely to be very similar households in similar but distinct municipalities that, due to differences in municipality-level quotas and prioritization, have different Bolsa Família recipient status. This variation will be key to our identification strategy, described in Section 4. We interpret the municipality-level variation in procedures as suggesting that, once a large set of household characteristics and municipality characteristics are accounted for, the probability that a particular household is a recipient of Bolsa Família is uncorrelated with the outcomes we consider.[8]

Monitoring School Attendance

Formal responsibility for monitoring and enforcing the education conditionality lies with the Ministry of Education. Teachers in each school collect daily attendance for students, which is then consolidated by school directors, matched to a list of Bolsa Família beneficiaries given to the school by the municipality, and sent to the municipal education secretariat on a regular basis. Every two months, the municipalities consolidate the attendance information, with calculations of the percentage of absences each month for each student. This information is then sent to the Ministry of Education. Although monitoring is conducted, in practice very few households are dropped from the program due to noncompliance with the education conditionality (Lindert et al. 2007). Households typically receive several warnings from the program, since the goal is to induce households into greater compliance rather than to "punish" households.

Education Policy Context in Brazil

Table 2.2 presents the education structure for primary school (*ensino* fundamental, broken up into two stages) and high school (ensino medio) in Brazil. The schooling structure changed on November 24, 2005, to include mandatory kindergarten.[9] Prior to the change, schooling was mandatory from ages 7 to 14 years; *classe de alfabetização* (or "CA," the equivalent of kindergarten in the United States) was separate from primary school and optional before grades one through eight; and the minimum age for starting grade one was 7 years. After the change, schooling became mandatory from ages 6 to 14 years; CA was included as part of primary school, was renamed "first year," and was made mandatory before the newly renamed "second through ninth" years; and the minimum age for "first year" became 6 years. If

[7] We also could not obtain a precise algorithm for how these criteria were combined and used to prioritize households, suggesting that the prioritization may have been through visual scanning rather than consistent constructions of rankings.

[8] A potential concern may be that political factors affect municipality quotas or prioritization of households within municipalities, introducing another level of selection into the treatment group. However, Lindert et al. (2007) document that in practice, the risks of political manipulation are "minimal in scope, due to the technical design of the program." Three federal control agencies constitute an official "oversights and control network" (*rede de fiscalização*) for Bolsa Família and hold responsibility for routine audits.

[9] The change was made in the law *Projeto de Lei nº. 3.675/04*.

children entered school on time and proceeded on time, these mandatory ages would correspond to the completion of both stages of *ensino* fundamental.[10]

Table 2.2 Education structure in Brazil, before and after November 24, 2005

U.S. equivalent	Before November 24, 2005			After November 24, 2005		
	Age	Course ("curso")	Level ("serie")	Age	Course ("curso")	Level ("serie")
Kindergarten	6	Classe de alfabetização (CA)	CA	6	Ensino fundamental I	1st year
Elementary school	7–10	Ensino fundamental I	Grade 1–4	7–10	Ensino fundamental I	2nd–5th year
Junior high school	11–14	Ensino fundamental II	Grade 5–8	11–14	Ensino fundamental II	6th–9th year
High school	15–17	Ensino médio	Grade 9–11	15–17	Ensino médio	10th–12th year

Source: Brazilian Ministry of Education (2012).
Notes: The "Age" column reflects the official minimum age for entry. Mandatory ages for schooling before and after November 24, 2005, are shaded in grey.

[10] Minimum working age in Brazil is 16 years for paid labor, with the exception of an apprenticeship, for which it is 14 years.

3. THE EVALUATION DATA

Data Collection

In 2005, 15,426 households were interviewed under the supervision of the *Centro de Desenvolvimento e Planejamento Regional*. Commissioned by the federal Ministry of Social Development as part of a baseline study on Bolsa Família referred to as AIBF-1 (*Avaliação de Impacto do Programa Bolsa Família*), this multipurpose survey included household-level questions on demographics, living conditions, assets, income, consumption, anthropometry, health, education, participation in cash transfer and subsidy programs, and women's decisionmaking.

The sampling design included households that in 2005 were already participating in Bolsa Família (Stratum 1), households that in 2005 were registered in Cadastro Único but not participating in Bolsa Família (Stratum 2), and households that in 2005 were not in Cadastro Único and therefore were not participating in Bolsa Família (Stratum 3). The sampling strategy for AIBF-1 was intended to be such that approximately 30 percent of households fell in Stratum 1, 60 percent fell in Stratum 2, and 10 percent fell in Stratum 3. However, based on households' responses to AIBF-1, in fact about 40 percent fell within the definition of Stratum 1, 35 percent fell within the definition of Stratum 2, and 25 percent fell within the definition of Stratum 3. Households in the North, Northeast, and Center-West regions were over-sampled, whereas those in the Southeast and South were under-sampled. Sample weights were constructed to make the data nationally representative.

In 2009, a follow-up survey (AIBF-2) of the same households was fielded. It was possible to trace and re-interview 11,433 of the households in the baseline sample, implying an annual attrition rate of approximately 6.5 percent. The main sources of attrition were field teams being unable to physically locate respondents' recorded addresses and households no longer residing at their recorded addresses (de Brauw et al. 2012). Based on information in the surveys, we constructed attrition weights to be used in conjunction with the original sample weights, to take into account the nonrandom attrition.[11] In both 2005 and 2009, households were asked about their registration in Cadastro Único as well as whether they received transfers. Thus, we are able to observe how households transition over the two rounds across various combinations of being registered in Cadastro Único and receiving Bolsa Família transfers.

Education Variables

Both the 2005 and 2009 rounds of data include information on schooling for each household member. This information includes whether the household member is currently in school and the corresponding course/level, the previous year's enrollment status and course/level, and the highest course/level the household member has completed if not currently in school.[12] Although the schooling structure changed between the 2005 and 2009 rounds, enumerators were instructed to code the 2009 round per the 2005 schooling structure for consistency.

We focus on the following set of indicators based on the following information for children aged 6 to 17 years: an unconditional measure of whether the child currently participates in school ("participation"); conditional on a child participating in school the previous year, whether the child has progressed from the previous grade level ("grade progression"); conditional on a child participating in school the previous year, whether the child has repeated the previous grade level ("grade repetition"); and

[11] See the appendix for a summary of the model used to estimate attrition weights.

[12] The questions on whether the child is in school currently or was in school last year translate literally to "attends currently" (*freqüenta atualmente*) or "attended last year" (*freqüentava no ano passado*), respectively. However, they refer more broadly to school participation rather than to recent school attendance rate.

conditional on a child participating in school the previous year, whether the child has dropped out ("dropout").[13]

There are some important limitations to the information in these data. First, the data are constructed as a panel at the household level, but it is not possible to match individuals within the household across rounds.[14] Thus, although the education indicators can be constructed for children in 2005 and children in 2009, they cannot be linked for the same child across both rounds. However, because each round includes information on both previous and current schooling, we can nonetheless construct measures related to schooling trajectories. Second, although enumerators were instructed to maintain the 2005 schooling structure conventions when coding the 2009 round, inspection of the data suggests that there was inconsistency in doing so, possibly because respondents were confused about their own children's levels and could not accurately report them to the enumerator. Thus, although we can infer whether a particular child has progressed or repeated grades since the previous year from a respondent's responses, since the responses on previous and current levels are likely to be answered by the respondent with the same schooling structure in mind (that is, consistently using the 2005 structure or consistently using the 2009 structure), we cannot construct a reliable measure of a child's absolute level of schooling. As a result, we also cannot construct reliable measures of whether a child has completed a particular course, such as whether the child has completed primary school or secondary school.

Descriptive Statistics on Education Indicators

In order to interpret the impact estimates, it is helpful to observe the patterns in schooling outcomes in the full sample of 6-to-17-year-olds in the 2005 survey round.[15] We first consider unconditional school participation. Figure 3.1, Panel A shows that among both boys and girls, school participation rates tend to be quite high between ages 7 to 14 years, but then steadily decline from age 15 to 17 (corresponding to ages after mandatory schooling). Figure 3.1, Panels B to D show the pattern of children's dropout rates, grade progression rates, and grade repetition rates, conditional on a child being in school the previous year. Panel B shows that dropout rates mirror the pattern in participation rates, with dropout relatively low between ages 7 to 14, then steadily increasing across ages 15 to 17. Panel C shows that grade progression rates slightly increase then slightly decrease between ages 7 to 14, then also fall from age 15 to 17, whereas Panel D shows that grade repetition rates are roughly similar after age 9. Across all four measures (Panels A through D), girls tend to have better outcomes than boys of the same age. Girls' participation rates are similar or slightly higher, particularly at older ages; girls' dropout rates are similar

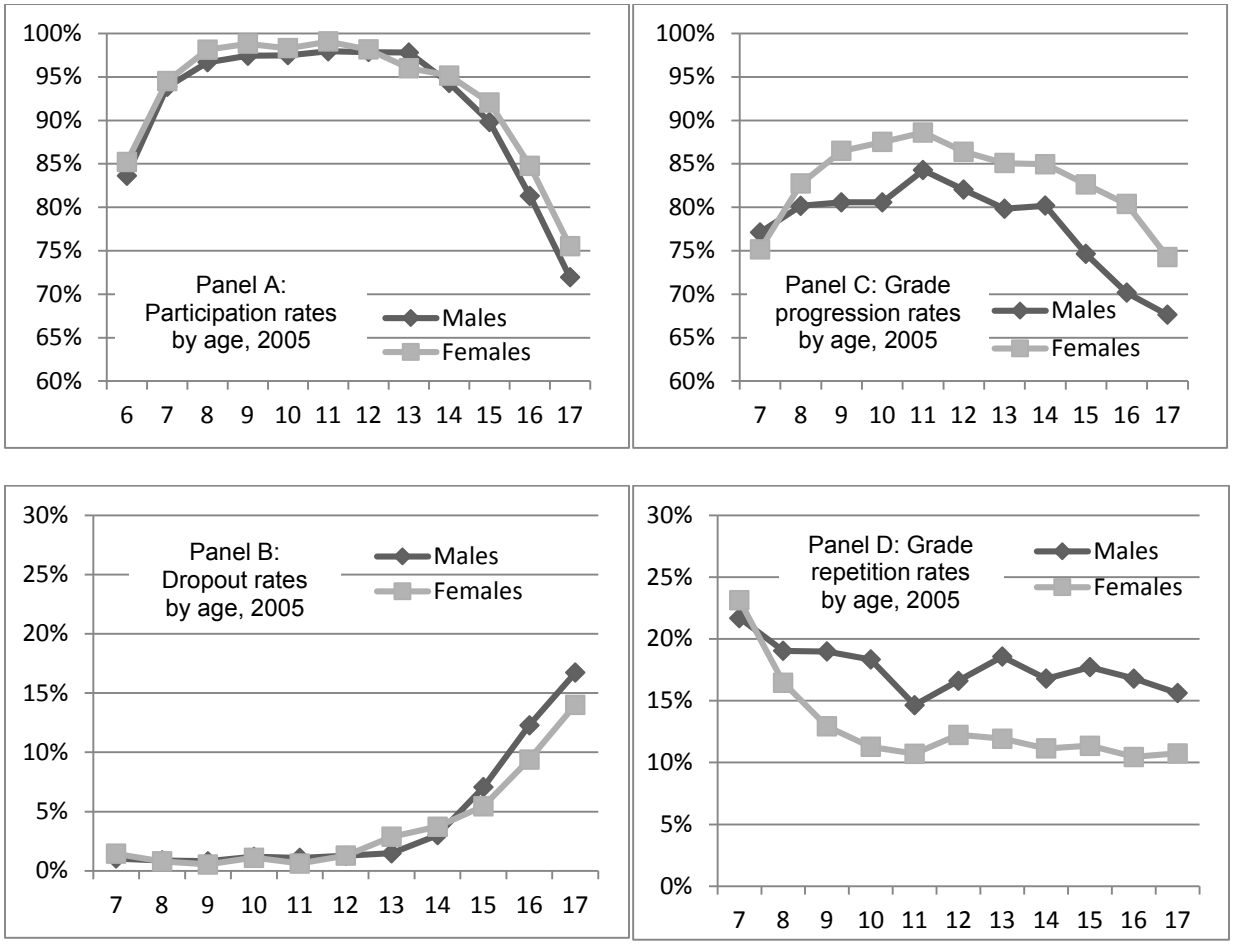
[13] For each child that is reported to currently participate in school, the data also include the household's self-report on how many days in the past seven days the child's school was open and on how many of those days the child attended school. We constructed attendance rates out of this information and found nearly no variation. In particular, nearly all households reported that children who participated in school attended every day that it was open in the last seven days. Given that there is likely considerable upward bias in these responses and there is insufficient variation for meaningful analysis, we do not present an analysis of impacts on the attendance rate.

[14] Individual identification numbers were inadvertently not kept intact across rounds, and attempts to link by name, sex, and age were not successful.

[15] We present descriptive statistics over the entire 2005 sample to uncover broad patterns in the sample. We also examined patterns in 2009 for the comparison group subsample we define later in the paper under "Construction of Treatment and Comparison Groups" to suggest the counterfactual situation of Bolsa Família recipients in the absence of the program. Patterns are very similar to those shown here, but noisier, due to relatively small sample sizes in disaggregations by sex and each age in years, exacerbated by some observations in each category with large sample weights. Similarly, patterns are very similar if we restrict the analysis only to the 2005 sample that does not receive Bolsa Família. Thus, we can be confident that the patterns shown here for 2005 are not driven by the small proportion of households already receiving Bolsa Família benefits.

or slightly lower, particularly at older ages; and girls are consistently more likely to progress in grades and less likely to repeat grades than boys.[16]

Figure 3.1 Participation, dropout, grade progression, and grade repetition rates, by age and sex, 2005



Source: Survey data from "*Avaliação de Impacto do Programa Bolsa Família"* (AIBF-1; 2005 and AIBF-2; 2009).

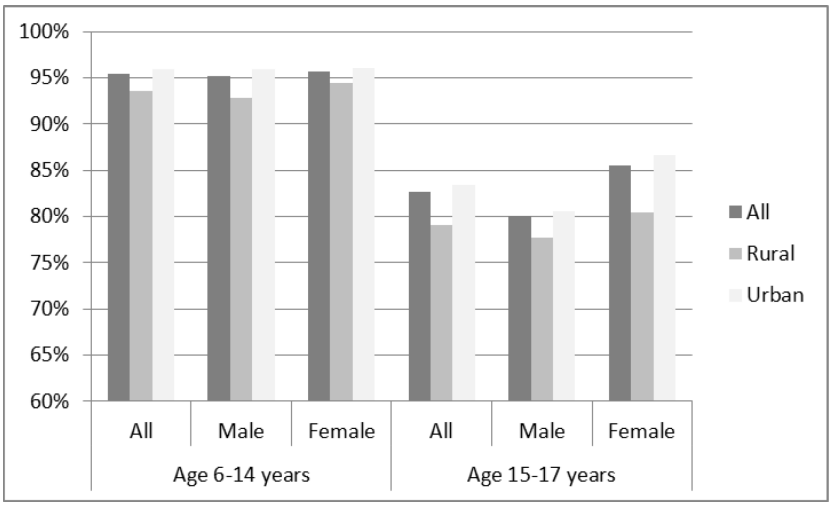
The patterns of outcomes presented in Figure 3.1 suggest that it is important to disaggregate impacts by age subgroups where there is possibly more potential for impact. For example, given that participation rates are very high between ages 7 and 14 (as we would expect, given the mandatory ages for schooling by law), there is limited scope for the program to further increase participation in this age range. However, there is considerable scope to affect participation between ages 15 and 17. Therefore, in addition to estimating impacts over the overall sample of 6-to-17-year-olds, we disaggregate impacts

[16] The observed difference in school participation rates by sex is somewhat unusual among poor countries, including in Latin America. In most developing countries, girls' school participation rates are lower than boys' school participation rates. However, the differences in grade progression rates by sex are consistent with patterns in many other countries. In the context of PROGRESA in rural Mexico, Behrman, Sengupta, and Todd (2005) document that in the control group that does not receive the program, girls progress more quickly than boys through primary school grades. The *Millennium Development Goals Report 2012* also states, "In many countries, gender gaps appear on the first day of school. . . . Once girls gain access to primary education, however, they tend to outperform boys. Data show that in most countries, girls are less likely to repeat grades or leave school early. In 102 out of 129 countries reporting data, girls progress more rapidly to the last grade of primary education than boys" (United Nations 2012, 5764).

between the 6-to-14-year-olds and the 15-to-17-year-olds.[17] Given similar patterns but consistently better outcomes for girls, we also disaggregate all impacts by sex.

Figure 3.2 shows the pattern of participation rates by these age groups and by sex, disaggregated by area of residence. We see that in each category, participation rates are lower in rural areas than in urban areas. Therefore, we also disaggregate impacts between rural and urban areas. Finally, the Northeast is widely regarded to be the most historically disadvantaged region in Brazil. Therefore, we also present impacts for the Northeast region and for all the other regions combined.

Figure 3.2 Participation rates disaggregated by area of residence, by age group and sex, 2005



Source: Survey data from "*Avaliação de Impacto do Programa Bolsa Família"* (AIBF-1; 2005) and (AIBF-2; 2009).

Empirical Issues for Measuring Impact

We seek to estimate average-treatment-on-the-treated (*ATT*) impacts of Bolsa Família on children's schooling outcomes. The key challenge to estimating the *ATT* in any context is*k* that it aims to measure the difference between actual outcomes of a group receiving the program and counterfactual outcomes of the same group had it not received the program. Since counterfactual outcomes cannot be directly observed, we must construct a comparison group of nonrecipients whose outcomes can be considered valid proxies for program recipients' outcomes in the absence of the program.

The AIBF datasets include both Bolsa Família recipient households and non-recipient households. However, the nature of Bolsa Família creates two key challenges in constructing a valid comparison group of nonrecipients. First, the program was not randomly assigned. Because Bolsa Família was targeted to poor households, we expect that recipient households would be different from nonrecipient households in general, even in the program's absence. If we simply compare recipients to nonrecipients without accounting for these factors, we risk attributing all differences in schooling outcomes between recipients and nonrecipients to the program, when other factors correlated with program receipt may also contribute to the differences. We thus use a methodology that accounts for nonrandom program assignment.

Second, there is a voluntary component to receiving Bolsa Família transfers. Even if observable characteristics are similar across groups of recipients and nonrecipients, if unobservable characteristics might lead households to self-select into eligibility and also correlate with outcomes of interest, those

[17] Ideally we would separately estimate impacts on 6-year-olds, since participation is relatively low for them in the overall sample. However, within our smaller estimation sample of treatment and comparison households, there are too few observations to confidently estimate impacts on only that age group, particularly in light of large sample weights potentially leading to a single observation dominating impact estimates in a small subsample. Therefore, we group the 6-year-olds into the "6-to-14" age group.

characteristics must be taken into account to avoid selection bias. In the context of Bolsa Família, registration in the Cadastro is voluntary. All Bolsa Família recipients must take the initiative to register in the Cadastro to be eligible for program receipt, such that comparing recipients of the program to nonrecipients not even registered in the Cadastro might introduce bias. We thus compare recipient households only to nonrecipient households also registered in the Cadastro. Our estimates, for this reason, are all *ATT* impacts conditional on Cadastro registration.[18]

Finally, there could be another level of selection: households that are registered in the Cadastro and deemed eligible for Bolsa Família but that choose not to fulfill the conditions should, in principle, also not be receiving Bolsa Família. However, as Lindert et al. (2007) emphasize and as noted earlier, the primary role of these conditionalities is to promote use of health and education resources. Monitoring of compliance is done largely in the spirit of assisting beneficiaries, not punishing them, and fewer than 2 percent were removed for noncompliance. Therefore, this source of selection bias is of lesser concern.

The key to our evaluation strategy is that, as described earlier, the data include a substantial pool of households that are registered in the Cadastro but do not receive Bolsa Família benefits. We take households participating in Bolsa Família and therefore registered in the Cadastro as our treatment group, and select a pool of similar households not participating in Bolsa Família but registered in the Cadastro as our comparison group.[19] To address the first concern above, we account for other observable differences between the two groups, using propensity score weighting. An identifying assumption of this estimator is that, after controlling for these observable differences between the treatment and comparison groups, remaining correlates of schooling are uncorrelated with treatment status. In the next subsection, we describe how conditioning on Cadastro registration helps account for selection bias, addressing the second concern.

Construction of Treatment and Comparison Groups

Using the aforementioned proposed treatment and comparison definitions, we describe the construction of these groups in the AIBF-1 and AIBF-2 data. The treatment group consists of use sample households that both received Bolsa Família benefits in 2009 and were listed in the Cadastro in 2005 but did not receive transfers from Bolsa Família or its predecessor programs in 2005. The comparison group consists of all households that report being listed in the Cadastro in either 2005 or 2009 and are "comparable" to Bolsa Família recipients on several other dimensions, but received no Bolsa Família transfers (or the predecessor programs) in 2005 or 2009.

In particular, out of the 11,433 households interviewed in 2009, we excluded three groups of sample households from the analysis in order to reduce bias from various forms of selection into the program: 1,512 households do not report being listed in the Cadastro Único, 3,443 households report already receiving Bolsa Família in the 2005 survey round, and 1,064 households received a predecessor social program in 2005 (for example, Bolsa Escola, *Bolsa Alimentacao*, or *Auxilio Gas*). Out of the remaining households, the sample receiving Bolsa Família transfers in 2009 includes 2,828 households—the potential treatment group. The sample not receiving Bolsa Família transfers in 2009 includes 2,586 households—the potential comparison group. The resulting estimation sample is considerably smaller than the original 2005 sample of 15,426 households. The reduction in available sample also derives from sample attrition between rounds and from missing values in outcome variables. We include attrition weights in the impact estimates to account for potential attrition bias. We also find that omitted observations due to missing values are relatively evenly distributed across our treatment and comparison groups, suggesting that they should not bias the resulting estimates.

[18] As described earlier, there is also heterogeneity across municipalities in the extent to which initiative was required to register in the Cadastro. However, it is reasonable to assume that, averaged across all municipalities, the groups are similar.

[19] One might consider an alternative impact evaluation strategy based on regression-discontinuity design—in particular, within this group of Cadastro registrants, looking only at nonrecipient households just above the income threshold and recipient households just below the income threshold. Although this idea is appealing, the official Cadastro data containing income information could be linked to the survey data for only a very small fraction of the sample households, making it unusable.

4. EMPIRICAL METHODOLOGY AND IDENTIFICATION STRATEGY

Propensity Score Weighting

We use a two-step process to adjust for differences in observable and unobservable characteristics between the Bolsa Família recipients and nonrecipients. First, as described in the previous section, we select a comparison group of nonrecipients that is likely to be fairly similar to the treated group of recipients in terms of observable and unobservable characteristics. Second, we use estimated propensity scores to further weight each observation in this selected comparison group according to its similarity on observable characteristics to treated observations. In this subsection, we describe our implementation of propensity score weighting.

Propensity score weighting (Hirano, Imbens, and Ridder 2003) entails estimating and applying weights to statistically balance observable preprogram characteristics between selected treatment and comparison groups. We estimate a propensity score for each household, denoting the predicted probability that the household is in the treatment group rather than the comparison group, based on a range of observable preprogram characteristics. We then use the propensity scores, p, to place weights on the comparison observations: each treatment observation receives a weight of 1, whereas comparison observations receive a weight of $p/(1-p)$. Intuitively, comparison households with very similar observable characteristics to treatment households are assigned high weights, whereas comparison households with relatively dissimilar observable characteristics to treatment households are assigned relatively less weight. By placing these weights on comparison households pre-selected to be similar on some dimensions to treatment households, we further balance observable characteristics between treatment and comparison households, even if they were unbalanced before weighting. Hirano, Imbens, and Ridder (2003) show that under a set of reasonable assumptions, applying propensity score weights leads to unbiased impact estimates of *ATT*. A summary of the theoretical basis for propensity score weighting is presented in Appendix A.

We also include sampling and attrition weights in the estimation. Incorporating these weights allows us to interpret our estimates of *ATT* as representative of the treated population, adjusting for over-sampling of certain types of households in the baseline and selective attrition of certain types of households in the follow-up. The main disadvantage of using propensity score weighting as opposed to matching methods is the higher variance of the estimator (Freedman and Berk 2008). We also describe our approach to dealing with high variance in Appendix A.

Implementation of Propensity Score Weighting

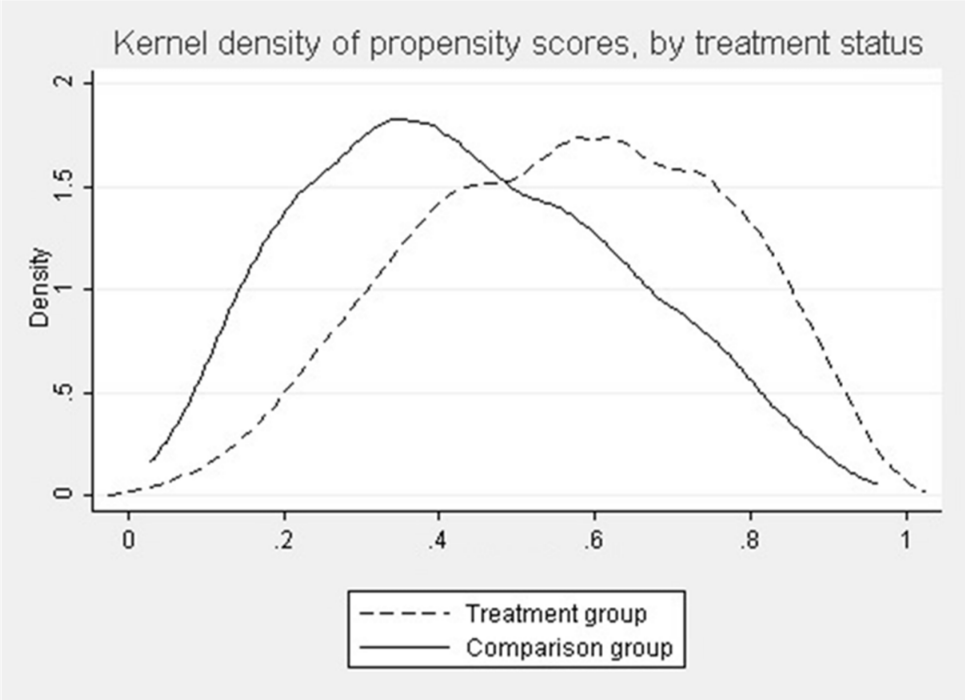
For each household in our estimation sample, we estimate the propensity score for the household to be in the treatment group. We include as covariates observable characteristics correlated both with the probability of a household being in the treatment group and with children's education outcomes conditional on treatment status. Doing so addresses the bias otherwise associated with nonrandom program assignment based on characteristics also correlated with key outcomes. We start by selecting a large set of observable preprogram characteristics we perceive as having potential to correlate with both program receipt and our outcomes of interest conditional on program receipt status. The set of observables chosen includes variables used to determine selection of households into Bolsa Família, as well as key household and municipality characteristics that may shape outcomes conditional on receiving Bolsa Família.[20] We allow a flexible relationship between the probability of treatment and these observable characteristics. Rather than imposing a functional form, we follow a stepwise algorithm for

[20] The covariates are shown in Appendix A and include the following categories: municipality-level characteristics including demographics that might affect the municipality quota, municipality-level characteristics that reflect initial conditions of available health and education infrastructure, household-level characteristics including demographics and poverty indicators that might determine eligibility, given the municipality quota, and household-level characteristics that might reflect initial conditions shaping the impact of receiving Bolsa Família transfers.

including covariates that gives an approximation to nonparametric estimation (Imbens, Newey, and Ridder 2005), which is summarized in Appendix A.

We then compare distributions of estimated propensity scores across treatment and comparison groups. We find considerable overlap (Figure 4.1), suggesting that observables predicting receipt of Bolsa Família are distributed similarly across the two groups. The overlapping distributions suggest that weighting observations according to estimated propensity scores may address imbalances between the two groups. Indeed, as we show in Appendix Table B.3, applying propensity score weights to the estimation sample balances average preprogram characteristics between treatment and comparison groups. Although some large differences exist between treatment and comparison groups (statistically significant at the 5 percent level or better) before application of propensity weights, we find no average differences between groups that are statistically significant after the application of propensity weights. Balancing occurs even among characteristics not directly included in propensity score estimation. We thus conclude that the propensity scores account for significant differences between treatment and comparison groups.

Figure 4.1 Overlap in estimated propensity scores between treatment and comparison groups



Source: Survey data from "*Avaliação de Impacto do Programa Bolsa Família"* (AIBF-1; 2005) and (AIBF-2; 2009).

We further apply the propensity score weights to several schooling indicators and measure the difference in means between treatment and comparison groups in the 2005 survey (Table 4.1).[21] We find no differences in weighted baseline means that are significant at the 10 percent level or lower, and the magnitudes of all of the differences are close to zero. Again, these preprogram outcomes were not actually included in estimating the respective propensity scores. We therefore conclude that propensity scores also balance baseline outcomes between our treatment and comparison groups, giving us confidence to proceed with the propensity-score-weighted estimation of impacts on schooling.

[21] Although Table 4.1 shows balancing in 2005 over mean outcomes aggregating over boys and girls, we also tested and found balanced samples between the treatment and comparison groups on the same variables in separate samples of boys and girls.

Table 4.1 Difference in mean values between treatment and comparison groups, in 2005, using propensity score weights

Variable	All	Rural	Urban	Not NE	NE
Participation rates					
Ages 6 to 14 years	0.036	0.012	0.043	0.004	0.085
	(0.039)	(0.029)	(0.049)	(0.019)	(0.088)
Ages 15 to 17 years	0.044	0.074	0.035	0.029	0.054
	(0.052)	(0.106)	(0.060)	(0.048)	(0.089)
Observations	7,820	1,512	6,186	5,025	2,795
Dropout rates					
Ages 6 to 14 years	-0.003	0.011	-0.007	-0.006	0.003
	(0.005)	(0.010)	(0.006)	(0.008)	(0.004)
Ages 15 to 17 years	-0.024	0.069	-0.047	-0.001	-0.038
	(0.056)	(0.043)	(0.069)	(0.041)	(0.095)
Observations	6,056	1,140	4,814	3,809	2,247
Grade progression rates					
Ages 6 to 14 years	-0.012	0.062	-0.035	-0.003	-0.021
	(0.033)	(0.082)	(0.035)	(0.036)	(0.060)
Ages 15 to 17 years	0.038	0.057	0.029	0.062	0.009
	(0.066)	(0.143)	(0.075)	(0.060)	(0.108)
Observations	6,056	1,140	4,814	3,809	2,247
Grade repetition rates					
Ages 6 to 14 years	0.015	-0.073	0.042	0.009	0.019
	(0.033)	(0.082)	(0.034)	(0.035)	(0.060)
Ages 15 to 17 years	-0.014	-0.126	0.017	-0.061	0.029
	(0.044)	(0.143)	(0.038)	(0.047)	(0.070)
Observations	6,056	1,140	4,814	3,809	2,247

Source: Survey data from "*Avaliação de Impacto do Programa Bolsa Família"* (AIBF-1; 2005) and (AIBF-2; 2009).
Notes: NE = Northeast region. Standard errors of differences in means are in parentheses. All differences in mean values between treatment and comparison groups are insignificant at the 10 percent level.

Impact Estimation

Our results are based on single-difference estimates of individual-level outcomes measured in 2009 (in AIBF-2) using household-level propensity score weights estimated on observables from 2005 (in AIBF-1). Ideally, we would estimate double-difference impacts using the two rounds of schooling information on each individual in our data. However, although households can be linked across the two waves, the data were collected in such a way that individuals cannot be linked. Therefore differenced outcomes for individual-level schooling indicators cannot be constructed. Further, the change in schooling structure between the 2005 and 2009 waves considerably complicates comparing different age cohorts across the two rounds. The key advantage to using double-difference over single-difference is to account for preprogram time-invariant differences between treatment and comparison groups. Preprogram balancing indicates that no significant preprogram differences remain once our propensity score weights are used, implying that although single-difference estimates may have larger standard errors, they are unbiased.

5. RESULTS—IMPACT ESTIMATES ON EDUCATION INDICATORS

Average Impacts on School Participation

Table 5.1 shows the impacts of receiving Bolsa Família on school participation (that is, the unconditional probability of a child participating in school) among all children and disaggregated by sex. From Panel A, we see that across all children aged 6 to 17 years, there is only a weakly significant increase in the overall participation rate, by 4.5 percentage points. However, effects are stronger in rural areas, in the Northeast, and among older children aged 15 to 17. In rural areas, there is a significant increase of 7.4 percentage points among children aged 6 to 17, driven by a 14.9 percentage point increase among the 15-to-17-year-olds. In the Northeast, there is a highly significant increase of 14.3 percentage points, with significant impacts in both the 6-to-14 and the 15-to-17 age ranges. Panel B shows that, among boys, aggregate increases in participation of 6-to-17-year-olds are not significant, except in the Northeast, where the increase is driven by a significant increase of 24.8 percentage points among boys aged 15 to 17 years. There is also a significant increase in participation in rural areas among boys aged 15 to 17 years, by 18.4 percentage points, which is significantly larger than the impact on younger rural boys. Among girls (Panel C), Bolsa Família causes significant increases in aggregate participation of 6-to-17-year-olds by 8.2 percentage points. Increases are larger and highly significant in rural areas (15.7 percentage points) and in the Northeast (19.3 percentage points), and weakly significant in urban areas (6.2 percentage points). In the aggregated and disaggregated estimates, the gains apply to the entire age range of 6 to 17 years, with a slightly but insignificantly higher impact for the 15-to-17-year-olds.[22]

Table 5.1 Impact of Bolsa Família on probability of participating in school, by sex, 2009

Variable	All	Rural	Urban	Non-NE	NE
Panel A: Participation, All					
6–17 years	0.045*	0.074**	0.033	-0.017	0.143***
	(0.026)	(0.037)	(0.030)	(0.030)	(0.040)
6–14 years	0.030	-0.003	0.021	-0.022	0.092**
	(0.029)	(0.034)	(0.034)	(0.034)	(0.038)
15–17 years	0.073*	0.149***	0.064	-0.008	0.166***
	(0.044)	(0.050)	(0.053)	(0.051)	(0.060)
Difference between 6–14 and 15–17	0.044	0.152**	0.043	0.014	0.074
	(0.053)	(0.061)	(0.063)	(0.061)	(0.071)
Observations	6,507	1,249	5,258	4,262	2,245
Panel B: Participation, Boys					
6–17 years	0.012	-0.006	0.003	-0.037	0.084**
	(0.035)	(0.039)	(0.042)	(0.048)	(0.043)
6–14 years	-0.009	-0.054	-0.016	-0.053	0.036
	(0.039)	(0.033)	(0.046)	(0.053)	(0.044)
15–17 years	0.086	0.184***	0.059	0.016	0.248***
	(0.062)	(0.064)	(0.080)	(0.074)	(0.072)
Difference between 6–14 and 15–17	0.095	0.239***	0.075	0.069	0.212**
	(0.073)	(0.072)	(0.092)	(0.091)	(0.084)
Observations	3,374	656	2,718	2,232	1,142

[22] In a small proportion of our estimates, there is an inconsistency between the estimated aggregate impacts and impacts on subgroups, such as urban and rural households, in which the average impact in the full sample lies outside the range of impacts estimated in exhaustive subsamples. These inconsistencies are caused by changes in the propensity score and attrition weights for the comparison observations when the analysis is conducted on subgroups.

Table 5.1 Continued

Variable	All	Rural	Urban	Non-NE	NE
Panel C: Participation, Girls					
6–17 years	0.082**	0.157***	0.062*	0.001	0.193***
	(0.033)	(0.053)	(0.033)	(0.025)	(0.054)
6–14 years	0.073**	0.111**	0.060*	0.008	0.140***
	(0.035)	(0.056)	(0.035)	(0.028)	(0.053)
15–17 years	0.080*	0.177**	0.066	-0.014	0.186***
	(0.046)	(0.077)	(0.048)	(0.037)	(0.072)
Difference between 6–14 and 15–17	0.008	0.066	0.006	-0.022	0.046
	(0.058)	(0.095)	(0.059)	(0.046)	(0.090)
Observations	3,133	593	2,540	2,030	1,103

Source: Survey data from "*Avaliação de Impacto do Programa Bolsa Família"* (AIBF-1; 2005) and (AIBF-2; 2009).
Notes: NE = Northeast region. Impact estimates represent the difference in the probability that a child participating in Bolsa Família currently attends school, compared with the comparison group. Standard errors are in parentheses. Significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Average Impacts on Schooling Transitions

Next, we examine impacts on schooling transitions across years. Tables 5.2 through 5.4 show—for all children, for boys only, and for girls only, respectively—the impacts of receiving Bolsa Família on whether the child progresses a grade level, repeats a grade level, or drops out of school, given that the child was in school the previous year.

Table 5.2 looks at impacts on grade progression, grade repetition, and dropouts among all children aged 6 to 17 years. Panel A shows no impact on grade progression of children 6 to 17 years overall. However, there is a significant increase in grade progression among 6-to-17-year-olds in the Northeast (9.3 percentage points), with no significant differences in impact between the 6-to-14- and 15-to-17-year-olds. There are also significant increases in 15-to-17-year-olds overall, cutting across rural, urban, and non-Northeast subgroups. We find no impacts overall on grade repetition or dropout rates among children 6 to 17 years (Panels B and C). However, the significant increase in grade progression among 6-to-17-year-olds in the Northeast corresponds to a significant decrease in dropouts in the same subgroup, of nearly identical magnitude (about 9 percentage points). This pattern indicates that in the Northeast, reductions in dropout translate to increases in grade progression, rather than increases in grade repetition, suggesting the program not only keeps children in school but causes them to progress in grade levels. Among 15-to-17-year-olds in rural areas, we find that the increase in grade progression corresponds also to a decrease in dropouts in the same subgroup (as well as an insignificant decrease in grade repetition, which is nonetheless significantly different than the impact on younger children). Among 15-to-17-year-olds in the non-Northeast, we find that the increase in grade progression corresponds to a significant reduction in grade repetition in the same subgroup. For 15-to-17-year-olds in urban areas, we find that the weakly significant increase in grade progression corresponds to small insignificant decreases in both grade repetition and dropout.

Table 5.2 Impact of Bolsa Família on grade progression, grade repetition, and dropouts among all children, 2009

Variable	All	Rural	Urban	Non-NE	NE
Panel A: Grade progression, All					
6–17 years	0.039	0.104	0.017	0.004	0.093*
	(0.034)	(0.072)	(0.036)	(0.042)	(0.055)
6–14 years	0.002	-0.027	-0.003	-0.029	0.032
	(0.036)	(0.063)	(0.041)	(0.051)	(0.046)
15–17 years	0.112**	0.201**	0.086*	0.095*	0.127
	(0.048)	(0.084)	(0.052)	(0.051)	(0.084)
Difference between 6–14 and 15–17	0.111*	0.229**	0.089	0.123*	0.096
	(0.060)	(0.106)	(0.066)	(0.072)	(0.096)
Observations	4,638	967	3,671	2,862	1,776
Panel B: Grade repetition, All					
6–17 years	-0.013	-0.023	-0.007	-0.027	-0.001
	(0.031)	(0.072)	(0.031)	(0.038)	(0.049)
6–14 years	0.014	0.064	0.000	0.011	0.013
	(0.032)	(0.056)	(0.037)	(0.047)	(0.037)
15–17 years	-0.058	-0.098	-0.041	-0.118***	<0.001
	(0.043)	(0.075)	(0.044)	(0.046)	(0.073)
Difference between 6–14 and 15–17	-0.073	-0.162*	-0.041	-0.130**	-0.012
	(0.053)	(0.093)	(0.058)	(0.065)	(0.082)
Observations	4,638	967	3,671	2,862	1,776
Panel C: Dropout, All					
6–17 years	-0.027	-0.080*	-0.009	0.023	-0.092***
	(0.021)	(0.043)	(0.022)	(0.021)	(0.036)
6–14 years	-0.016	-0.037	0.003	0.017	-0.044
	(0.022)	(0.039)	(0.023)	(0.025)	(0.033)
15–17 years	-0.054	-0.104**	-0.045	0.024	-0.128**
	(0.034)	(0.044)	(0.039)	(0.028)	(0.055)
Difference between 6–14 and 15–17	-0.038	-0.067	-0.048	0.006	-0.083
	(0.041)	(0.059)	(0.045)	(0.037)	(0.064)
Observations	4,638	967	3,671	2,862	1,776

Source: Survey data from "*Avaliação de Impacto do Programa Bolsa Família"* (AIBF-1; 2005) and (AIBF-2; 2009).
Notes: NE = Northeast region. Impact estimates are estimated conditional on the child being in school in the previous year. Standard errors are in parentheses. * Significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

We present impacts on grade progression, grade repetition, and dropouts for boys only in Table 5.3. Panel A shows no impact on grade progression of boys 6 to 17 years overall, and in fact, it shows no increases within any subgroup. Among 6-to-14-year-old boys in rural areas and in the non-Northeast, we, in fact, see weakly significant *decreases* in grade progression, significantly different from the effects on older boys in those areas. Panels B and C indicate that such decreases in grade progression among 6-to-14-year-old boys in rural areas and the non-Northeast correspond to borderline-insignificant increases in grade repetition among the same subgroups, with little effect on dropouts. These findings suggest that the program may be inducing younger boys in these areas, who, in any case, would be in school, to repeat grades rather than progress through them. We also see, however, that among 15-to-17-year-old boys in the Northeast, the program causes a highly significant increase in grade repetition (26.3 percentage points), corresponding to a highly significant decrease in dropouts in the same subgroup. Taken together, these results suggest that in the Northeast, the program induces older boys to stay in school, but those boys tend to repeat grade levels.

Table 5.3 Impact of Bolsa Família on grade progression, grade repetition, and dropouts among boys, 2009

Variable	All	Rural	Urban	Non-NE	NE
Panel A: Grade progression, boys					
6–17 years	-0.031	-0.097	-0.014	-0.063	-0.010
	(0.036)	(0.065)	(0.039)	(0.043)	(0.056)
6–14 years	-0.038	-0.134*	-0.028	-0.090*	0.005
	(0.041)	(0.075)	(0.045)	(0.050)	(0.056)
15–17 years	0.022	0.116	0.024	0.045	-0.108
	(0.051)	(0.085)	(0.057)	(0.056)	(0.093)
Difference between 6–14 and 15–17	0.060	0.250**	0.052	0.135*	-0.113
	(0.065)	(0.113)	(0.072)	(0.075)	(0.109)
Observations	2,359	501	1,858	1,469	890
Panel B: Grade repetition, boys					
6–17 years	0.054*	0.085	0.038	0.046	0.065*
	(0.032)	(0.070)	(0.035)	(0.042)	(0.039)
6–14 years	0.053	0.105	0.042	0.074	0.014
	(0.036)	(0.075)	(0.039)	(0.049)	(0.040)
15–17 years	0.050	-0.019	0.045	-0.037	0.263***
	(0.049)	(0.072)	(0.054)	(0.055)	(0.079)
Difference between 6–14 and 15–17	-0.003	-0.124	0.002	-0.112	0.249***
	(0.061)	(0.103)	(0.067)	(0.073)	(0.088)
Observations	2,359	501	1,858	1,469	890
Panel C: Dropout, boys					
6–17 years	-0.024	0.012	-0.024	0.017	-0.055
	(0.022)	(0.036)	(0.023)	(0.018)	(0.047)
6–14 years	-0.015	0.029	-0.014	0.016	-0.020
	(0.025)	(0.025)	(0.028)	(0.019)	(0.047)
15–17 years	-0.072**	-0.096	-0.069**	-0.007	-0.156***
	(0.029)	(0.059)	(0.028)	(0.026)	(0.053)
Difference between 6–14 and 15–17	-0.057	-0.125*	-0.055	-0.023	-0.136*
	(0.038)	(0.064)	(0.039)	(0.032)	(0.070)
Observations	2,359	501	1,858	1,469	890

Source: Survey data from "*Avaliação de Impacto do Programa Bolsa Família"* (AIBF-1; 2005) and (AIBF-2; 2009).
Notes: NE = Northeast region. Impact estimates are estimated conditional on the child being in school in the previous year. Standard errors are in parentheses. * Significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level.

Impacts on such measures of progress through school are larger and broader for girls, as shown in Table 5.4. Panel A shows that the program causes significant increases in overall grade progression among girls aged 6 to 17 years (10.4 percentage points), with the largest increases among 15-to-17 year-old girls (20.9 percentage points). Among girls aged 6 to 17 years in rural areas, there is an even larger significant increase in grade progression (27.9 percentage points), cutting across both girls aged 6 to 14 years and girls aged 15 to 17 years. Among girls aged 6 to 17 years in the Northeast, there is a weakly significant increase in grade progression (12.7 percentage points), driven by a large increase among girls aged 15 to 17 years, which is significantly higher than the impact on younger girls. Both in urban areas and in the non-Northeast, there is no significant overall impact on girls aged 6 to 17, but still a significant increase among girls aged 15 to 17 years in those subgroups. Looking across Panels B and C, the increase in grade progression over all girls aged 15 to 17 corresponds to a significant decrease in grade repetition in the same age range, suggesting that overall the increases are driven by girls who would be in school anyway but who now progress across grade levels. Specifically in rural areas, however, we see that the increases in grade progression among girls aged 6 to 14 are accompanied by significant decreases in dropouts, suggesting that the program causes younger rural girls to both stay in school and progress across grades. For older rural girls, we see that the increase in grade progression is accompanied by a

decrease in grade repetition, suggesting that for rural girls aged 15 to 17, conditional on being in school the previous year, the program does not affect their dropouts but does induce them to progress across grades rather than repeat grades. Among girls aged 15 to 17 years in the non-Northeast, we see that the increased grade progression corresponds to decreased grade repetition, suggesting again that the program does not affect dropouts but induces those who stay in school to progress rather than repeat grades. Meanwhile among girls aged 15 to 17 years in the Northeast, we see that the increases in grade progression are accompanied by decreases in dropouts, suggesting that the program keeps rural older girls in the Northeast in school and moreover induces them to progress across grade levels.

Table 5.4 Impact of Bolsa Família on grade progression, grade repetition, and dropouts among girls, 2009

Variable	All	Rural	Urban	Non-NE	NE
Panel A: Grade progression, girls					
6–17 years	0.104**	0.279***	0.069	0.087	0.127*
	(0.047)	(0.075)	(0.053)	(0.063)	(0.065)
6–14 years	0.041	0.146**	0.037	0.045	0.024
	(0.054)	(0.071)	(0.061)	(0.077)	(0.063)
15–17 years	0.209***	0.225**	0.180**	0.203***	0.261***
	(0.064)	(0.106)	(0.077)	(0.076)	(0.096)
Difference between 6–14 and 15–17	0.167**	0.079	0.143	0.158	0.237**
	(0.083)	(0.128)	(0.098)	(0.109)	(0.115)
Observations	2,279	466	1,813	1,393	886
Panel B: Grade repetition, girls					
6–17 years	-0.067	-0.103	-0.058	-0.104*	-0.032
	(0.044)	(0.067)	(0.048)	(0.058)	(0.060)
6–14 years	-0.020	-0.002	-0.046	-0.058	0.020
	(0.048)	(0.056)	(0.055)	(0.069)	(0.050)
15–17 years	-0.170	-0.133	-0.135**	-0.223***	-0.117
	(0.053)	(0.085)	(0.064)	(0.068)	(0.077)
Difference between 6–14 and 15–17	-0.149**	-0.131	-0.089	-0.165*	-0.138
	(0.072)	(0.102)	(0.084)	(0.097)	(0.092)
Observations	2,279	466	1,813	1,393	886
Panel C: Dropout, girls					
6–17 years	-0.036	-0.176***	-0.011	0.018	-0.094**
	(0.042)	(0.056)	(0.031)	(0.028)	(0.046)
6–14 years	-0.021	-0.144**	0.009	0.013	-0.044
	(0.033)	(0.056)	(0.033)	(0.035)	(0.050)
15–17 years	-0.039	-0.092	-0.046	0.020	-0.144**
	(0.046)	(0.061)	(0.053)	(0.037)	(0.071)
Difference between 6–14 and 15–17	-0.018	0.052	-0.054	0.007	-0.099
	(0.057)	(0.083)	(0.062)	(0.050)	(0.087)
Observations	2.279	466	1,813	1,393	886

Source: Survey data from "*Avaliação de Impacto do Programa Bolsa Família"* (AIBF-1; 2005) and (AIBF-2; 2009).
Notes: NE = Northeast region. Impact estimates are estimated conditional on the child being in school in the previous year.
* Significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level. Standard errors are in parentheses.

These results provide weakly significant evidence that Bolsa Família increased school participation overall for children age 6 to 17 and that these impacts were larger and significant among children age 15 to 17, in rural areas, and in the Northeast. However, the effects are not evenly shared. Although boys age 15 to 17 experience substantial gains in school participation as a result of Bolsa Família, those improvements are accompanied by only limited gains for boys in progress through school, in the form of lower dropout rates. Girls, on the other hand, experience broad gains in school participation

and school progression. Both older (15 to 17 years) and younger (6 to 14 years) girls experience large gains, on average, in school participation, improved grade progression, less grade repetition, and reduced dropouts. Bolsa Família is helping girls to stay in school longer and to progress while in school.

This pattern of girls experiencing larger impacts from a CCT program on school participation and grade progression is generally consistent with the findings from other studies of CCT programs (Schultz 2004; Ahmed et al. 2007). However, in those programs, girls typically had lower baseline outcomes, so that the perceived gains could be due to regression to the mean or could be catch-up gains made possible by having low status before the program. Similarly, girls often received larger transfers than boys in those programs. However, such conditions cannot explain the impact of Bolsa Família on girls' schooling outcomes.

6. CONCLUSION

A large literature documents that, in Latin America and elsewhere, CCT programs with education conditionalities have had significant impacts on children's school enrollment. There is less evidence on how CCT programs affect other schooling outcomes, such as grade transitions or on which children benefit most from the conditioned transfers. In this paper, we study impacts of Bolsa Família on a range of schooling outcomes, including a detailed investigation into heterogeneity of impacts. Our analysis makes use of rich longitudinal household-level data to study impacts on school enrollment as well as grade transitions, with a focus on assessing differences in impacts by sex, age, and area of residence.

We find considerable heterogeneity in impacts, most notably by sex. Whereas we find a modest, weakly significant increase in aggregate participation over 6-to-17-year-old children (4.5 percentage points), roughly consistent with previous evidence (Glewwe and Kassouf 2012), we find that positive impacts of Bolsa Família on schooling outcomes are much stronger for girls than boys. This finding is particularly notable, given that we also find girls typically have similar or slightly better enrollment rates and grade transitions than boys in the absence of the program. Thus, whereas our finding that girls (and particularly older girls) experience the largest positive impacts on schooling from a CCT program is consistent with the finding of most other CCT programs, the result is not driven by worse outcomes for girls in the absence of the program, as is the case in many other CCT programs. Moreover, the stronger impacts for girls cannot be driven by differing direct incentives, since Bolsa Família's payment amounts do not differ by sex, and also do not appear to be driven by differential reallocation of labor hours across boys and girls. Thus, our results raise the compelling question of what other mechanisms might drive the stronger schooling impacts for girls than boys. This pattern of effects, in which, conditional on participation, girls progress through school more successfully than boys, was also found in Mexico by Behrman, Sengupta, and Todd (2005) in their study of PROGRESA. In the context of Bolsa Família, this pattern is unlikely to be due to selection effects in which only the most successful girls are able to remain in school, so they progress more rapidly than boys. Rather, our results are suggestive of sex-based differences in cost or ease of succeeding and progressing in school, although the supporting evidence is not sufficient to be conclusive.

We also find evidence of other sources of considerable heterogeneity, which our diverse sample allows us to explore. Effects on schooling are typically larger for older children (consistent with evidence from other CCT programs), larger and more highly significant in rural areas than in urban areas, and larger and more highly significant in the historically disadvantaged Northeast region relative to the average across the other four regions.

We note several caveats to our findings. First, although we would ideally use double-difference to compare changes in the same individuals over time across treatment and comparison groups, we cannot calculate these differences, due to problems linking individuals in the same household across rounds. However, given that the use of propensity score weights gives balancing in our preprogram covariates and preprogram education indicators, our single-difference estimates are unlikely to be driven by preprogram differences. Second, while we would prefer to use objective measures of schooling, our measures of education indicators are self-reported. However, our aggregate results are very similar to aggregate results in Glewwe and Kassouf (2012) that draw on administrative school census data, which suggests bias from self-reporting (which could, in principle, be systematically correlated with program receipt) is not driving results in our key education indicators.

Bearing in mind these considerations, this paper makes several important contributions to the substantial literature on the impact of CCT programs on education outcomes. We present unique evidence on the relative impact of a CCT on girls' and boys' schooling, with supporting evidence on the plausibility of potential mechanisms. We also show considerable heterogeneity in impacts by area of residence, due to our diverse sample across urban and rural areas in all five regions, contrasting with the primarily rural focus of other CCT programs. Finally, because we have large longitudinal household-level survey data collected specifically to assess Bolsa Família, our work is among the few papers to attempt rigorous evaluation of the Bolsa Família program in any dimension.

APPENDIX A: CONSTRUCTION OF ATTRITION WEIGHTS

Defining attrition as households that were found in the 2005 survey but not in the 2009 survey, we follow the correction procedure outlined in Fitzgerald, Gottschalk, and Moffitt (1998a, 1998b). We first estimate a probit model to explain whether or not a household is lost to attrition, conditioning on exogenous characteristics. From that we can construct weights that take into account the likelihood that households did, or did not, attrit from the sample. We calculate the predicted probability that a household does not attrit. The weights are the inverse of those predicted probabilities. Households with low probabilities of being re-interviewed, but who are traced and re-interviewed, receive higher weights. We include these attrition weights in all of the estimates reported in the paper.

We find large differences in attrition rates between regions and rural/urban location in the sample; attrition was highest in the Center-West region and the Southeast, and lowest in the North and Northeast. Smaller households were also more likely to be lost to attrition; attrition rates were quite similar for other important variables, such as Cadastro registration. Correlates of attrition are shown in Appendix Table B.1.

Theoretical Basis for Propensity Score Weighting

We present here a brief overview of the theoretical basis for propensity score weighting, based on Hirano, Imbens, and Ridder (2003).

The aim of our evaluation is to construct, for a range of outcomes, an estimate of the average impact of Bolsa Família on those that receive it—referred to as the average impact of the treatment on the treated (*ATT*). The formalization of this concept is as follows.

Let Y_t^1 be a household's outcome in time period t if it is a recipient of Bolsa Família, let Y_t^0 be that household's outcome in time period t if it does not receive any program benefits, and let D be an indicator variable equal to 1 if the household receives program benefits and 0 if not (that is, an indicator of "treatment"). The impact of the program is just the change in the outcome caused by receiving benefits: $\Delta = Y_t^1 - Y_t^0$. For each household, either only Y_t^1 or only Y_t^0 is observed in any period t.

We wish to estimate the difference between the outcome that treated households would realize if they receive the program and the outcome that treated households would realize if they do not receive the program in period t, given a vector X of observable characteristics of the households:

$$ATT = E(\Delta \mid X,D = 1) = E(Y_t^1 - Y_t^0 \mid X,D = 1) = E(Y_t^1 \mid X,D = 1) - E(Y_t^0 \mid X,D = 1).$$

However, only Y_t^1 and not Y_t^0 is observed for households treated in period t, that is, those with $D = 1$. Because $E(Y_t^0 \mid X,D = 1)$ is not observed, we must construct a statistical comparison group for recipients out of our observations on nonrecipients, that is, households with $D = 0$. In particular, we must construct a group of nonrecipients and then adjust it in such a way that balances any observable characteristics X potentially correlated both with treatment status and the outcome conditional on treatment status.

One way of doing so involves estimating a "propensity score," $P(X) = \Pr(D = 1 \mid X)$. This propensity score is the predicted probability that any household is a program recipient based only on its observable characteristics X. The approach of propensity-weighted regression entails the researcher selecting a set of nonrecipients to use as a comparison group, then using estimated propensity scores for program receipt to more heavily weight the comparison observations with higher propensity scores. The validity of this approach rests, in part, on two assumptions:

$$E(Y_t^0 \mid X,D = 1) = E(Y_t^0 \mid X,D = 0), \tag{A1}$$

and

$$0 < P(X) < 1. \tag{A2}$$

Expression (A1) assumes "conditional mean independence," that is, that conditional on *X,* nonparticipants have the same mean outcomes as participants would have if they did not receive the program. Expression (A2) assumes that, based only on the set of observables *X,* all observations in the comparison group have positive predicted probability of being treated.

We first consider the case without sampling or attrition weights.

Under (A1), (A2), and several other technical assumptions, Hirano, Imbens, and Ridder show that we obtain an unbiased estimate of *ATT* through a weighted regression framework, if the ratio of assigned weights is $\frac{P(X)}{1-P(X)}$:1 for comparison : treatment observations. [23]

Hirano, Imbens, and Ridder also show that the observables *X* used to construct the propensity score can be directly included in this weighted regression to account for additional variation and thereby improve precision. [24]

It is straightforward to extend this methodology to the case where, as in this evaluation, there are also sampling weights and attrition weights. These weights can simply be multiplied to the propensity-score weights to derive an "effective weight" to be used in the weighted regression.

Accounting for High Variance

The main drawback to the propensity-score-weighting method is that the variance associated with the estimator is high relative to other estimation strategies (Freedman and Berk 2008). As a result, one consideration in using propensity score weighting is the potential to make statistical Type II errors—that is, to accept the null hypothesis even though it is not true. In practical terms, Type II errors imply potentially missing significant impacts of Bolsa Família, leading us to take two specific measures to deal with high variance.

First, to the extent possible, we lower the variance of the propensity-score-weighting estimator by including the covariates used to estimate the propensity score directly in all our weighted regressions estimating treatment effects. As mentioned above, these variables should no longer affect the point estimate of the treatment effect after propensity score weighting, but act only to improve its precision.

Second, when we find impact estimates that are borderline significant (that is, statistically significant between the 5 and 10 percent level) even after adding covariates, we attempt to also estimate them via nearest-neighbor matching, also known as "covariate matching" (Abadie and Imbens 2006) and confirm that results are significant. Covariate matching compares outcomes among treated observations with outcomes among selected observations in the comparison group for which a set of explanatory variables is closest to the treated observation, according to a distance measure. The estimated treatment effect is the average difference for the outcome between the treatment and nearest-neighbor comparison observations. An advantage to using covariate matching is that it is a lower-variance estimation strategy relative to propensity score weighting. Covariate matching is also entirely nonparametric and does not rely on the distributional assumptions that underlie the probit or logit model used to estimate propensity scores in propensity score weighting. However, covariate matching does not allow us to readily take into account sample weights and attrition weights, such that we would expect slight differences between the point estimates of impacts based on propensity score weighting versus covariate matching. Moreover, covariate matching is very computationally intensive in large samples and therefore, for a dataset as large as our Bolsa Família study sample, is more suitable to use as a robustness check for borderline-significant results than as our primary estimation strategy.

[23] Note that this approach differs from matching methods, in that for matching, only certain observations out of the eligible comparison group are used—based on some metric of similarity to treated observations, depending on the particular method—but that typically each of those observations is then assigned a weight of 1. In propensity score weighting, all observations in the comparison group selected by the researcher are used, but each is assigned a weight based on its propensity score. This approach is preferable for our context, since incorporating sample weights and attrition weights is then relatively straightforward.

[24] We include these observables as covariates in all of our estimates.

Algorithm for Estimation of Propensity Scores

Our approach to estimating propensity scores follows Hirano and Imbens (2001) and Imbens, Newey, and Ridder (2005). We start by estimating a logit model including only region dummy variables interacted with rural/urban dummies, to ensure that we account for broad differences in market conditions. We weight the regression by sampling weights from the AIBF-1 data multiplied by attrition weights. Next, we start to consider the set of N variables at the household and municipality level as possible covariates for inclusion in the logit model. We estimate N regressions, each sequentially and separately including one variable to the basic logit model. We keep the variable that reduces the log pseudo-likelihood the most. We then take the remaining list of $N - 1$ variables, and sequentially add the remaining $N - 1$ variables to the logit model, again keeping the one that maximizes the reduction of the log pseudo-likelihood. We follow this procedure until the reduction hits a threshold that roughly corresponds to adding a variable to the logit model that has a t-ratio of 1, indicating that the remaining variables in the list have little predictive power.[25]

We next take the K covariates that are chosen in the first step, and in the second step we square all of them and interact them with one another, creating an additional $K(K + 1)/2$ variables. We then add these second-order terms to the model sequentially until no term exceeds a threshold that loosely indicates significant predictive power (for example, a t-ratio that corresponds to a p-value of 0.1). We repeat this procedure for each of the three comparisons, therefore letting the data tell us the relationship between Bolsa Família eligibility and potential explanatory variables.

[25] We use the list of predictive explanatory variables for any regressions using covariate matching, when we run robustness checks. Since covariate matching does not parameterize the relationship between explanatory variables and treatment status, we do not need to include any second-order terms.

APPENDIX B: SUPPLEMENTARY TABLES

Table B.1 Correlates of attrition, Bolsa Família evaluations, AIBF-1 to AIBF-2

Variable	Marginal effect	Standard error
Characteristics of head, 2005		
Female	0.0041	0.00795
Illiterate	-0.03063	0.00939**
Beneficiary status, 2005		
Not registered in Cad Único	0.0375	0.01112**
Registered, not receiving BF	0.0289	0.00925**
Income level, 2005		
Per capita consumption	0.00008	0.00001**
Tracing information		
Number of tracing items	-0.00404	0.00089**
Number of key tracing items	-0.00677	0.00454
Selected locations: Lives in:		
Maranhão	0.4922	0.10698**
Piauí	-0.08534	0.08522
Ceará	0.1413	0.07981*
Rio Grande do Norte	-0.27783	0.00419
Paraíba	0.0702	0.10011
Alagoas	0.1871	0.10781*
Sergipe	-0.2522	0.01026**
Bahia	0.0730	0.06040
Minas Gerais	-0.1134	0.04051**
Espírito Santo	0.1726	0.12725
São Paulo	0.2810	0.08382**
Santa Catarina	0.1929	0.23346
Rio Grande do Sul	0.3270	0.08782**
Mato Grosso do Sul	0.3406	0.09042**
Mato Grosso	0.2605	0.10880**
Goiás	0.4507	0.06901**

Source: Survey data from "*Avaliação de Impacto do Programa Bolsa Família"* (AIBF-1; 2005) and (AIBF-2; 2009).
Notes: Included, but not reported, are other locations and interaction terms of state by located in urban area.
* significant at the 10 percent level; ** significant at the 5 percent level.

Table B.2 Household- and municipality-level characteristics included as possible covariates for propensity scores and selected by algorithm for final specifications of propensity scores

Variable	Year	Covariate selected by algorithm for inclusion in propensity score estimation?
Household-level variables		
Number of children aged 0–15 at baseline	2005	YES
Household size	2005	YES
Number of rooms in house (truncated at 10)	2005	
Number of bedrooms in house	2005	
Number of bathrooms in house	2005	
Housing quality index, from 0 to 11	2005	YES
Whether household owns its house	2005	
Log of per capita monthly expenditure (food + nonfood)	2005	YES
Whether head is illiterate	2005	YES
Head's years of education	2005	
Head's sex	2005	
Municipality-level variables		
Average family size	2000	YES
Child dependency ratio	2000	
Incidence of poverty	2003	
Incidence of extreme poverty	2003	
Percent of population working without card	2000	YES
Percent of population working in agricultural sector	2000	YES
Black population, as percentage of total population	2000	
"Pardo" population, as percentage of total population	2000	
Indigenous population, as percentage of total population	2000	
Percent of households with "adequate housing"	2000	
Percent of households with access to piped water	2000	YES
Percent of households with access to solid-waste collection	2000	YES
Percent of households with access to general sewage network	2000	
Percent of households with access to septic tanks	2000	
Percent of households with access to electricity	2003	
Households with landline phones (per 1,000)	2000	
Households with cell phones (per 1,000)	2000	YES
Average years of education	2000	
School attendance rate: 7–14 years old	2000	YES
Illiteracy rate: 7–14 years old	2003	
Number of public schools per capita	2003	YES
Average number of students per class in elementary school	2003	
Number of clinics ("postos medicos") per thousand inhabitants	2002	

Source: Survey data from "*Avaliação de Impacto do Programa Bolsa Família"* (AIBF-1; 2005) and (AIBF-2; 2009).
Notes: Municipality-level variables are drawn from the 2000 national census, IBGE survey 2002, and IBGE survey 2003. All comparisons include as covariates the interactions of region and rural/urban dummies.

Table B.3 Balancing of preprogram characteristics across comparison and treatment groups, unweighted and weighted

Variable	Included in estimation of p-score?	Unweighted means		Weighted means		p-value on difference in weighted means
		C	T	C	T	
Household-level variables						
Number of children age 0–15 at baseline	Yes	1.42	2.09	1.72	1.68	0.71
Household size	Yes	3.84	4.53	4.03	4.01	0.89
Number of rooms in house		5.18	4.88	5.06	5.07	0.96
Number of bedrooms in house		0.27	0.34	0.25	0.22	0.56
Number of bathrooms in house		0.89	0.77	0.83	0.86	0.54
Housing quality index, from 0 to 11	Yes	8.25	7.55	7.59	7.83	0.22
Whether household owns its house		0.67	0.67	0.68	0.63	0.26
Log of per capita monthly expenditures	Yes	5.20	4.96	5.01	5.10	0.18
Whether head is illiterate	Yes	0.22	0.25	0.24	0.24	0.99
Head's years of education		3.49	3.52	3.51	3.57	0.82
Whether head is female		0.36	0.36	0.37	0.38	0.79
Municipality-level variables						
Average family size	Yes	3.60	3.69	3.58	3.57	0.80
Child dependency ratio		49.49	52.34	49.87	49.55	0.60
Incidence of poverty		39.29	45.74	43.68	42.98	0.66
Incidence of extreme poverty		0.13	0.16	0.14	0.14	0.51
Percent without card	Yes	39.64	43.54	42.99	43.43	0.78
Percent in agricultural sector	Yes	23.69	27.63	28.23	28.64	0.80
Percent black		6.51	6.69	7.59	7.15	0.29
Percent "pardo"		46.95	51.94	48.42	46.67	0.40
Percent indigenous		0.38	0.42	0.43	0.49	0.26
Percent with "adequate housing"		34.98	28.67	33.41	34.03	0.71
Percent with piped water	Yes	69.70	64.79	68.09	68.56	0.71
Percent with solid-waste collection	Yes	71.24	64.64	67.17	67.31	0.94
Percent with general sewage network		38.56	30.15	37.43	38.74	0.53
Percent with septic tanks		14.28	14.71	12.79	12.09	0.56
Percent with electricity		91.09	88.57	90.06	91.13	0.16
Landline phones (per 1,000)		246.47	220.58	240.90	244.13	0.62
Cell phones (per 1,000)	Yes	146.18	131.30	140.03	143.73	0.52
Average years of education		5.57	5.24	5.15	5.17	0.82
School attendance rate: 7–14 years old	Yes	94.51	93.77	93.40	93.57	0.70
Illiteracy rate: 7–14 years old		12.77	15.64	14.22	13.55	0.38
Number of public schools per capita	Yes	0.001	0.001	0.001	0.001	0.94
Number of students per elementary class		28.22	28.00	27.59	27.50	0.68
Clinics (per 1,000)		2.04	1.80	1.66	1.63	0.81

Source: Survey data from "*Avaliação de Impacto do Programa Bolsa Família"* (AIBF-1; 2005) and (AIBF-2; 2009).

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