Improving Learning Outcomes Through Social Assistance: Regression-Discontinuity Evidence from Brazil*

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Abstract

Social assistance benefits are often conditional on school attendance. However, they will only lead to higher human-capital accumulation and increased labor market earnings through learning. We use a regression-discontinuity design to examine whether conditional cash transfers impact test scores in national exams. Identification of causal effects exploits an unexpected change in the eligibility cutoff of Brazils *Bolsa Famlia* that gives rise to exogenous variation in program participation. The analysis draws on detailed administrative data linking individual records from the programs payment sheets, the nations single registry of the poor and vulnerable, and educational outcomes from student censuses and test scores. The results reveal that the program leads to significant improvements in learning of Mathematics and Portuguese language. Investigating the mechanisms through which these effects take place, we find that the program causes parents to monitor student effort more frequently, and students to increase effort in school and decrease engagement in work.

Keywords: Redistribution, school participation, learning, education, development.
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1 Introduction

Learning is a key driver of human capital accumulation and future labor-market outcomes. In many countries safety nets consist predominantly of social assistant benefits conditional of school attendance[1]. It is well recognized that the provision of social assistance reduces poverty in the short-run by providing minimum level of income and generally improves the conditioned-on outcome. In this paper, we examine whether conditional cash transfers impact student learning—the key channel through which they may lead to higher human-capital accumulation and increased labor market earnings. Several recent papers have studied the marginal impact of attaching conditions to social assistance programs. Although their administration is costlier relative to unconditional transfers, they intend to address market failures that lead to underinvestment in education or health by imposing certain behaviors on recipient households (Hanlon, Barrientos, and Hulme 2010). Recent studies found that such schemes pose trade-offs with respect to gains in overall welfare, which can be particularly large in the presence of low quality (or accessibility) of conditioned services (Baird, McIntosh, and Ozler 2011; Attanasio, Veruska, and Marcos 2015; Blattman, Fiala, and Martinez 2015; Benhassine et al. 2013). On the other hand, transfers conditional on educational outcomes provide a valuable mechanism to improve parents monitoring over childrens school attendance (Bursztyn and Coffman 2012).

We use a regression discontinuity design (RDD) to examine the causal impacts on learning outcomes of Brazil's Conditional Cash Transfer (CCT) program Bolsa Família. This is the largest CCT in the world, reaching 14.1 million households (more than 80 million individuals)—about a quarter of Brazil's population.[2] It is a well-established program that started in 2003. We exploit detailed administrative data linking the program's payment sheets, the country's single registry of the poor and vulnerable, and information on educational outcomes from student censuses and Prova Brasil, a nationally representative exam covering 5th and 9th grade students, testing proficiency in Portuguese Language and Mathematics.

An evaluation of the causal impact of social assistance programs on students' learning in participating households requires a comparison of students in households who participate in the program with students in households who do not. Since July 2009, households with monthly per capita income below R$140 have been entitled to receive the Bolsa

[1]Social safety nets (SSNs), also known as social assistance or welfare schemes, are defined as noncontributory transfers targeted to the poor or vulnerable. They include income or in-kind support and can be made conditional on certain behaviors of recipients' households (e.g. conditional cash transfers, CCTs) or provided without any conditions (e.g. unconditional cash transfers, UCTs) (Grosh et al. 2008, World Bank 2009).

[2]Traditional social safety net programs conditional on school attendance —school feeding— are the social assistance the programs' with the greatest coverage in lower-income countries (World Bank, 2015). While their coverage remains stable, condition cash transfers have been expanding. They the main type of safety net in upper-middle-income countries and their coverage has been expanding in both low- and high-income countries. CCTs are now present in 64 countries, a dramatic increase from 2 countries in 1997 and 27 in 2008. They play a key role in expanding social safety net coverage of the poor, covering around 30 percent of all the poor households in middle-income countries (World Bank, 2015).

Família conditional cash transfers.[3] However, the administrative records on monthly income that determine program participation are self-declared by households. This raises a strong potential for manipulation of the assignment variable. We present evidence of precise manipulation around the $R140 cut off, which in turn invalidates the assumptions needed for a valid RDD (Angrist and Lavy 1999, Lee and Lemieux 2010). This margin can thus not be exploited to identify causal effects. Instead, we take advantage of an unexpected change in the program's eligibility cutoff in July 2009 and use information on households' monthly per capita income measured before the R$140 cutoff was announced, when the CCT eligibility cutoff (since April 2006) had been R$120 in monthly per capita income. We identify the impact on learning using the associated exogenous variation in program participation, exploiting the fact that there are students in households that were not in the program before (because their income was too high) that were included in the program due to the unexpected cut off change. This variation allows us to use regression discontinuity design (RDD) estimation. The estimator identifies the average treatment effect for children in households near the eligibility cut-off.

We have two main sets of results. First, the RDD estimates reveal that the program led to better learning outcomes in both subjects tested by Prova Brasil. Portuguese Language test scores improved by 23 percent of a standard deviation and Mathematics test scores improved 31 percent of a standard deviation among the students that were included in the program due to the unexpected cut off change. Second, we provide new evidence on the mechanisms through which the effect on learning takes place. Specifically, we find evidence that the program causes students to exert more effort in school, parents to monitor student effort more frequently, and students to decrease engagement in work.

Our paper builds on and contributes to several strands of the literature. First, our findings speak to the broader question of how benefits should be designed for promoting learning, and ultimately support children in poor households build skills and improve labor market prospects. Much of the existing literature establishes the importance of learning to human capital accumulation and economic development (Hanushek and Kimko 2000, and Hanushek and Woessmann 2008). However, it finds mixed evidence on the effects of interventions such as provision of information, computers/materials or (student or performance) incentives (Kremer, Brannen and Glennerster 2013, Murnane and Ganimian 2014, Kremer, Glewwe, and Moulin 2009., Glewwe, Kremer, Moulin, and Zitzewitz 2004, Kremer, Miguel and Thornton 2009). Many of these studies often rely on small-scale programs (Evans and Popova 2015). We add to this literature by providing rigorous evidence from national-scale program using a regression discontinuity design and assessing specific mechanisms through which the effect on learning occurs.

Second, our new identification method, the analysis of manipulation and robustness

[3] Brazil's single registry of the poor and vulnerable ("Cadastro Unico") all people with income up to half of the minimum wage (R$207), however only those with income up to R$140 since July 2009 and R$120 before that are covered by Bolsa Familia

to composition effects, complement and extend the existing literature on the educational effects of cash transfer programs to an important area that remaining largely unexplored – learning.[4] The literature has shown that CCTs improve school enrolment, attendance and/or dropout. In Nicaragua, the Red de Protección Social program raised enrollment by 18 percentage points, retention rates by 7 percentage points, and daily attendance by 11 percentage points among children in primary school (Maluccio and Flores 2004). In Honduras, the Programa de Asignación Familiar had positive effects on the enrollment and daily attendance of children aged 6–13 years and small negative impacts on dropout rates (Glewwe and Olinto 2004). In Colombia, Familias en Acción increased the enrollment of children aged 12–17 years but not of children aged 8–12 years (Attanasio et al. 2010). Glewwe and Kassouf (2012) examine the impact of Brazil's Bolsa Escola and Bolsa Família programs on dropout rates. Using school-level census data to compare changes in enrollment, dropout rates, and enrollment across schools that adopted the program at different times, they estimate that the program has increased enrollment and lowered dropout rates.[5]

In contrast to evidence on attendance, enrolment and droput, evidence on the effects of CCTs on learning is very limited. Data availability and lack of robust identification methods were important impediment this line of research. Filmer and Schady (2011) provides an important exception. In an experiment in Cambodia, they evaluate the impact of the program using the fact that it made payments of varying magnitude to otherwise comparable households, using a sharp RDD. They find that a modest cash transfer, equivalent to approximately 2 percent of the consumption of the median recipient household, caused a substantial increase (about 25 percentage points) in school attendance and a small, negative effect on learning. However, the program, size of the transfer and duration of the support were limited and this could have led to this result. Other types of studies compare grade retention rates and test scores between CCT beneficiaries and non-beneficiaries (Behrman, et al. 2005, Janvry, et al. 2006, Lavinas, et al. 2001). They show that, for those age groups for which the program has enrollment effects, beneficiaries achievement is lower than non-beneficiaries. However, composition effects (for example, because those who do not drop out because of the program have lower achievement than non-beneficiaries) can be driving these results, even in cases where beneficiary status is randomly assigned.

Third, we contribute to the literature that analyzes the schooling decisions of poor households in developing countries. In an influential paper, Bursztyn and Coffman 2012 show that transfers conditional on educational outcomes usually provide a valuable mechanism to improve parents' monitoring over children's school attendance. Their findings suggest important intergenerational conflicts in schooling decisions, a lack of parental

[4]See Baird et al. (2013) for a systematic review of the educational effects of cash transfer programs.

[5]Parker, Rubacalva, and Teruel (2008) and Saavedra and Garcia (2012) perform a meta-analysis of 42 referenced papers and find a robust effect of CCTs on dropout and attendance rates.

control and observability of school attendance in the absence of the conditionality. Our finding that conditional cash transfers cause students to exert more effort in learning, to be less involved in work-related activities, and parents to monitor student engagement more frequently are additional rationales for conditional cash transfer programs—the indirect effects through the monitoring they provide.

Finally, the causal estimates we offer based one of the flagship social assistance programs in the world contribute to inform pressing policy issues in developing countries, where many governments are deliberating how to promote learning in general and long-run effects of CCTs. In particular, they suggest that the higher attendance rates promoted by CCTs are translated into better leaning outcomes, establishing a clear path to human-capital accumulation and increased earnings.

The remainder of the paper proceeds as follows. Section 2 provides institutional background on the Bolsa Família program. Section 3 describes the sources of administrative data employed. Section 4 presents the identification strategy, and section 5 reports the main results and demonstrates their robustness to varying bandwidth size, definitions of the treatment group and attrition. Section 6 concludes.

2 Beneficiary Selection Process in the Bolsa Família Program

Bolsa Família is the largest CCT program in the world, reaching 14.1 million households—around a quarter of Brazil's population. It has an annual budget of around R$24.7 billion. Created in 2003, Bolsa Família was also one of the world's first national CCTs (following only Mexico's Progresa program, created in 1997), and it changed the face of social assistance in Latin America. Today CCTs are the main form of social assistance in Latin America and have spread to more than 189 countries around the world.

To be a Bolsa Família beneficiary, an individual has to be registered in Cadastro Único, which requires an interview by a certified interviewer. Each of Brazil's 5,560 municipalities is responsible for maintaining its own database, and they take varying approaches to information collection and updating. In some municipalities, the potential beneficiary goes to a social assistance center, requests entrance in the program, and is interviewed there. In other municipalities, teams of interviewers visit poor areas and interview households in their homes.

Households registered in Cadastro Único with monthly income per person below the program cut off are entitled to Bolsa Família, and are included in the program according to municipal vacancies. Once in the program, they receive the following benefits, depending on their characteristics:[6]

[6]If the household has monthly income below R$70 per person, it is classified as extreme poor and gains access to more transfers, which are not described here because they are not relevant for our identification strategy.

- Households with at least one schoolchild (0–15 years old) are entitled to a fixed monthly payment of R$32 per child.
- Households with adolescents (16–17 years old) are entitled to a monthly payment of R$38 per adolescent, limited to two benefits of this kind per family.
- Households including a pregnant woman are entitled to R$32 for 15 months, starting at the beginning of the pregnancy.

Beneficiaries in turn have to comply with with the following education and health requirements: School enrollment and a minimum attendance rate of 85 percent for children aged 6–15 years or 75 percent for adolescents aged 16 and 17 years; Vaccination and nutritional (weight and height) monitoring of children under 7 years of age and prenatal exams for pregnant women. Families in the program also have to update their Cadastro Único information whenever a relevant change occurs, such as a change of address, household composition, or income sources. Even if no relevant change happens, they still have to contact the municipality every two years and resubmit critical data such as income.[7] If they do not contact the municipal authorities, it is common for the local Cadastro Único administrator to search for them . For the purpose of our identification strategy, it is important to emphasize that both registration and updating processes can be requested by a family at any time.

3 Data

We used administrative records compiled by the Ministry of Social Development and the Ministry of Education. From Brazil's single registry of the poor and vulnerable (Cadastro Único) we obtain all the poor and vulnerable in Brazil, houldshol composition and income. Information on program participation is obtained by monthly Bolsa Familia program payment sheets. From the Censo Escolar (a student census), we obtain student level information on educational outcomes and from and Prova Brasil (nationally representative proficiency test) on student level test scores.

Brazil's single registry of the poor and vulnerable (Cadastro Único). National data set that of the registered poor and vulnerable in Brazil, defined as individuals in households with per capita income below half the minimum wage. It includes detailed information on their earnings, living conditions, and demographic and occupational characteristics. Cadastro Único started exclusively as Bolsa Família's administrative database but evolved through the years to be the main federal database on poverty. Currently, more than 20 social programs use it, and it covers virtually all of the poor and vulnerable in Brazil (Campello and Neri 2013). This universality allows us to identify the variables we are interested in by comparing beneficiary and nonbeneficiary families in the data set,

[7]The percentage of households updating their information in each municipality is one of the indicators composing the Cadastro Único quality index, which is closely tracked by the federal government and to which monetary incentives are attached. That is, municipalities with good-quality data are rewarded with higher transfers.

without any serious concern about the possibility of bias due to self-selection into Cadastro Único. Throughout the study we use a Cadastro Único dataset extraction from June 2009.

Bolsa Familia's payment sheets. In Cadastro Único we have access to the main attributes of our target population. But to find out who is a Bolsa Família beneficiary in a given month, we have to use the program's payment sheet. For that purpose, we merged payment sheet data from January 2009 through December 2011 and then linked this database with Cadastro Único.

Censo Escolar. Data on educational outcomes come from two main sources. Censo Escolar is a yearly census of all school pupils in Brazil, produced by the Ministry of Education's National Institute for Educational Studies and Research "Anísio Teixeira" (INEP). We use data from 2009 to 2011 covering child characteristics (such as age, gender, and grade); parents' socioeconomic background (education, declared income, occupation, and housing conditions); and teacher and school attributes.

Prova Brasil. National test taken every two years by 5th- and 9th-grade public school pupils in Brazil. It examines proficiency in Portuguese language and mathematics.

We merged Censo Escolar and Prova Brasil though INEP's common identifier. There is no common identifier between Cadastro Único and INEP's data, so we compared five main variables present in both datasets to find matches: name, mother's name, father's name, date of birth and place of birth. Our sample consists of students who took the Prova Brasil test in 2009 and were registered in Cadastro Único, with information updated in the last two years.

Table 1 presents summary statistics for the main variables used in the study, across both grades (full sample) and for 5th and 9th-grde students separately. Test scores are standardized within each the subject and grade, using the whole sample of Prova Brasil test takers. The table shows that on average the students in household who participated in Bolsa Familia in 2009 have worse test results than the average Prova Brasil test taker, which is consistent with the fact that the program focuses on low-income families.

4 Identification Strategy

In June 2009, the month of our Cadastro Único data, BFP's income threshold was R$ 120. Because program eligibility is based on a fixed threshold of household per capita income, a fuzzy RDD comparing the educational outcomes of program participants just above and just below the eligibility cutoff seems straightforward.

Let Y_i denote the educational outcome of interest for an individual i. We would estimate:

$$Y_i = a + bD_i + f(Income_i) + e_i \tag{1}$$

where a is a constant, D_i indicates program participation of household i in year t,

$f(Income_i)$ is a second [8] degree polynomial interacted with a dummy variable that equals 1 if income is below R$140, and zero otherwise, and e_{it} is the error term.

A natural candidate to instrument for treatment status of a given household is the location of the self-declared household income relative to the eligibility cutoff:

$$D_i = a + bT_i + f(Income_i) + e_i \tag{2}$$

where T_i is a dummy variable that equals 1 if income is below R$140, and zero otherwise. However, monthly household income (the running variable in this design) is *self-declared*. Thus households with income close to the eligibility cutoff have a clear monetary incentive to declare to the interviewer that their income is below the cutoff in order to benefit from the cash transfer. The presence of manipulation of the running variable would violate local randomization, invalidating the use of RDD (Lee and Lemieux 2010). Therefore, before using a RDD to estimate BFP's effect, we investigate income manipulation.

There are two main ways of investigating the manipulation of the running variable. The first is to test the continuity of baseline covariates around the cut off, and the second to investigate the presence of excess bunching around the cut off. To test the null hypothesis of no discontinuity of baseline covariates at the R$120 cutoff we estimate:

$$Y_i = a + bT_i + f(Income_i) + e_i, \tag{3}$$

where the program participation indicator D_i was replaced by T_i, a dummy variable that equals 1 if income is below R$120, and zero otherwise. Equation 3 is the reduced form version of equation (1). Table 2 presents estimates of the coefficient b for each baseline covariate. It shows evidence of manipulation, since family size and geographic location (belonging to Region South East) are significantly related to participation in *Bolsa Famlia*.

The second method to investigate the presence of manipulation is to test for excess bunching, that is, if individuals sort themselves around the cut off in a non-random way. The standard test in the literature for the presence of sorting in an RDD design is that proposed by McCrary (2008). This test has a null hypothesis that there is no discontinuity and hence no manipulation in the running variable. However, the discrete nature of our data with natural accumulation in round numbers renders the use of the McCrary test inadequate [9]. In the appendix we follow Chetty et al. (2011) to propose a method to detect excess bunching that is adequate to the distribution of income per person observed in Cadastro Único. Applying this method we find evidence of manipulation around the R$ 120 cut off.

[8] In all models polynomial degree was chosen by minimizing the adjusted G-Statistics proposed by Lee and Card (2008) for parametric RDD with discrete data.

[9] Frandsen (2013) showed how the test proposed by McCrary (2008) over identifies manipulation if the running variable is discrete and proposed an alternative test for the discrete case. However, Frandsens proposal is not adequate to our case because of the natural accumulation of our running variable in round numbers.

Manipulation of income is consistent with the economic incentives of beneficiaries, and our findings are consistent with the evidence presented by Firpo et al. (2014) for the BFP program and by Camacho and Conover (2011) for Colombian social programs. In sum, the evidence is strong enough to invalidate the simple application of a RDD to identify BFP's effects.

To circumvent this challenge and identify the causal impact of BFP on educational outcomes, we exploit an unanticipated change in the program cut off. In July 2009 an update in the BFP's income cut off was announced, increasing it from R$ 120 to R$ 140 [10], a threshold that remained until April 2014. To make sure candidates could not manipulate their income around the new cut off we use a June 2009 extraction from the *Cadastro Único* database to measure self-declared income per capita. This assures that all our self-declared income data was collected before the R$ 140 cut off was announced.

We then repeat the formal investigation of manipulation, but around the new the new cut off (R$140) instead of (R$120). In Table 3 we present IV estimates of the system of equations (1) and (2). We find no significant relationship between baseline covariates and BFP partipation. In Table 4 we present reduced form estimates and again find no evidence of manipulation, since the relationship between being above or below the cut off and baseline covariates is insignificant for all baseline covariates.

With an appropriate forcing variable we now turn to the precise definition of treatment, D_i . Since we can construct monthly measures of program participation using the program's payroll data, a straightforward definition of program participation is $D_i = 1$, if the family is in payroll in the month that our outcome is measured, and $D_i = 0$ otherwise. However, because of the temporal gap between income measurement - which took place in June 2009 -, and the measurement of outcomes we are interested in - the Prova Brasil test taken in November 2011 -, this simple definition might give rise to bias and to a weak first stage. A weak first stage could arise because every month new households are included or excluded from the program, according to their income per person in the current month. As time evolves, incomes naturally fluctuate, families update their information and flow in and out of the program, June 2009's income variable will become less and less related to current BFP participation. Bias might arise because families can react to the new cut off and start manipulating their incomes around R$ 140.

We avoid these potential problems by defining treatment as entrance in the BFP program between August 2009 and October 2009. Letting three months pass after the cut off change allows time for the change to actually take effect in the field, that is, for families who became eligible only after the cut off change – families with income below the new cut off but above the previous cut off - to be included in the program, despite administrative delays. Figure 1 shows that with families below R$ 140 indeed have a much higher probability of treatment than families above. Table 5 shows that the first stage is indeed significant.

[10] Bolsa Famlia legal framework: Decree n 6.917 from 07/30/2009.

On the other hand, three months is a short enough period that beneficiaries that reacted to the announcement and requested income updates had not yet being included in the program. We observe zero inclusions in the program between August-October 2009 of families who had income above R$ 140 in June 2009, while there are 37,813 new entrants below the cut off in the same period.

Therefore, this forcing variable and treatment definition give rise to a valid RDD, allowing us to identify the causal effect of being included in the BFP between August-October 2009 on Prova Brasil 2011 learning outcomes. To identify Local Average Treatment Effects the system we estimate is:

$$Y_i = a + bD_i + f(Income_i) + +X_i + e_i, \tag{4}$$

where Y_i is the *Prova Brasil* outcome measure in November 2011, a is a constant, D_i indicates program entrance between August-October 2009 of individual i, $f(Income_i)$ is a second degree polynomial on the July 2009 *Cadastro Único* income interacted with T_i, X_i is a set of baseline covariates included only to increase precision, and e_i is the error term. We instrument treatment with the equation:

$$D_{i2009} = a + bT_{i2009} + f(Income_{i2009}) + e_i, \tag{5}$$

where T_{i2009} is a dummy variable that equals 1 if $Income_{i2009}$ is below R$140 and zero otherwise.

5 Main results

We now turn to the question of how *Bolsa Familia* participation impacts learning outcomes, as measured by scores in *Prova Brasil.* In Table 6 we report the effects of *Bolsa Família* on Portuguese Language and Mathematics test scores for the whole sample, as well as for the sample divided by the two grades in which the test is applied - 5^{th} and 9^{th} grades. The estimates reveal that the BFP program improves in 37 percent of a standard deviation both Portuguese Language and Mathematics test scores. When separated by grade the improvement for Portuguese Language in the the 5^{th} grade is of 49 percent of a standard deviation; however, results are not statistically significant for the 9^{th} grade students or Mathematics in the 9^{th} grade.

To interpret results it is important to keep in mind that the average number of months in which students received BFP benefits between August 2009 and November 2011, the month the Prova Brasil test took place, is 18.30 (median 18 months) months among the treatment group and zero in the control group. Figure 3 presents the distribution of months of benefits received among the treated.

These are large, significant effects, and to shed light on the channels through which they arise we construct four variables from question applied to *Prova Brasil* test takers. From questions on the frequency in which students do homework and how frequently they study out of class hours we build a proxy for students effort. From questions on how frequently parents monitor homework and how much parents incentivize students we build a measure of parents engagement. We use a question on how many hours students spend per day on house chores and if they work at of the household to measure work engagement. And finally we use question on frequency of reading different materials to build a measure of reading intensity. All answers are self-reported by students. Table 7 shows that BFP participation increase students effort and parents engagement. It decreases work engagement and has no significant effect on reading intensity.

5.1 Robustness

An important decision in estimating RD models is the size of the interval around the cut off that will be used for estimation. Imbens and Kalyanaraman (2012) suggest a procedure for the non-parametric case. However, the discrete nature of our data invalidates the methods assumptions, and there is no precise method for bandwidth choice in the parametric RDD we use. Parametric RD identification relies on the assumption that the model chosen approximates the true data generating process in the support of the running variable. In trying to keep this assumption as weak as possible, we chose an interval around the cut off that was as small as possible, while still being wide enough to contain a number of observations that gives adequate statistical power. We further accounted for model choice uncertainty by applying the correction in standard errors proposed by Lee and Card (2008). Even with this precautions, it is important to check how our estimates change if we chose different bandwidths. Table 8 shows results for bandwidths of 15 and 20 [11]. In it we see that results are stable to varying bandwidth size.

Polynomial order. We follow Lee and Card and cluster at the income level to allow for model choice uncertainty. We also test in table XX the sensitivity to polynomial degree

Another source of concern in our identification strategy are differential attrition rates between treated and not treated individuals. The forcing variable we use was measured before July 2009, and the outcome variable measured in November 2009. This could create two types of sample imbalance: Since CCTs are expected to affect drop out rates, it is possible that those who entered BFP after the cut off change have significantly higher probability of participating in *Prova Brasil*. And since CTTs are expected to affect grade progression, those who entered BFP in 2009 could have advanced grade significantly faster than those that did not. Table 8 presents the effects of the treatment on the probability of taking the *Prova Brasil* test in 2011, for Group 1 and Group 2 students. Group 1 is composed of all students in fifth and sixth grades in 2009, the potential participants in *Prova Brasil* 2011 as fifth graders, while group 2 is composed all of students in seventh and eighth grades in 2009, the potential participants in *Prova Brasil* 2011 as ninth graders.

[11] We use the next two multiples of five after 10 because of the heaping in multiples of five observed in our data and discussed in the appendix.

We see that entrance in BFP is not significantly related to any change in the probability of *Prova Brasil* participation. Therefore, we can rule out the possibility of differential attrition between treated and not treated.

6 Concluding remarks

In this paper, we have used a regression-discontinuity design to estimate the causal impacts of the *Bolsa Família* conditional cash transfer program on learning in Brazil. Our empirical analysis has exploited detailed administrative data linking the program's payment sheets, the nation's single registry of the poor and vulnerable, and information on educational outcomes from school censuses and national test scores. Identification of program impacts has exploited an unexpected change in the program's eligibility cut off which gives rise to exogenous variation in program participation. The estimation results reveal that the program increases test scores on average 37 percent of a standard deviation, a strong effect when compared to other education interventions. Considering that CCTs have proven positive impacts in areas other than education, and that BFP transfers are relatively small, the evidence suggests that the BFP is highly cost-effective program.

Our findings have implications for our understanding of the long-run effects of CCTs in developing countries. In particular, they confirm the hypothesis that CCTs contribute to human capital accumulation among the poor, thus increasing earnings potential and helping break the transmission of poverty between generations. The large CCTs effects on learning seems to be related to improved student effort, more parents' monitoring and decreased students' engagement in work.

7 Appendix: Validation of the Regression Discontinuity Design through Bunching Tests

The discrete distribution with bunching on round numbers of the forcing variable in our RD design invalidates the use of manipulation tests proposed by McCrary (2008) or Frandsen (2013). Here we develop a test of manipulation around the cut off appropriate for our running variables with bunching in round numbers. To do so, we follow the analysis in Chetty et al. (2011).

Figure 5 plots a histogram of the empirical distribution of income per capita in October 2009. The figure is restricted to individuals with household income per capita within the range $[90, 190]$. To construct this histogram, we group individuals into R\$1 bins, and plot the bin counts.

Figure 5 shows a decreasing empirical income distribution with several spikes, mostly in round numbers (e.g. 90, 100, 110), and with one of those spikes being in the bin R\$140. The figure also plots a counterfactual distribution, that simulates the income per capita distribution in the case of no excess bunching at specific bins (i.e. no spikes in the histogram). We believe bunching occurs as a result of both wages paid at round numbers and individuals rounding even more their responses when declaring income. To construct the counter factual distribution we first fit a seventh-order polynomial to the counts plotted in the figure, "excluding" the data for the spikes, by estimating a regression of the following form:[12]

$$C_j = \alpha + \sum_{i=1}^{7} \beta_i \cdot Z_j^i + \sum_{i \in S} \gamma_i \cdot D_i + \varepsilon_j, \tag{6}$$

where C_j is the number of individuals in income bin j, Z_j is household income per capita in bin j, and $D_j = 1[i \in S]$, where $S = \{i : i \text{ is a spike}\}$. We define an initial estimate of the counterfactual distribution as the predicted values from (6) omitting the contribution of the dummies for the spikes, i.e. $\widehat{C}_j^0 = \hat{\alpha} + \sum_{i=1}^{7} \hat{\beta}_i \cdot Z_j^i$. This calculation, however, does not satisfy the constraint that the area under the counterfactual distribution must equal that of the empirical distribution. To account for this problem, we shift up each predicted value by the factor $\frac{\sum_{j=1}^{K} C_j}{\sum_{j=1}^{K} \widehat{C}_j^0}$, where K is the total number of bins, and get the final counterfactual distribution, $\widehat{C}_j = \frac{\sum_{j=1}^{K} C_j}{\sum_{j=1}^{K} \widehat{C}_j^0} \cdot \widehat{C}_j^0$.

Let B_j denote the estimated excess number of individuals who locate in bin j, that is, $\widehat{B}_j = C_j - \widehat{C}_j$. Let also $\hat{b}_j = \widehat{B}_j / C_j$ be the estimated excess number of individuals relative to the actual count of individuals, for bin j. We focus our attention on bins that are spikes in the histogram. We perform two statistical exercises based on measures of $\hat{b}_j$. First, we test the null hypothesis that $\hat{b}_j = 0$, against the alternative hypothesis that $\hat{b}_j > 0$, for each $j \in S$.[13] This exercise tests the importance of the excess bunching in each spike bin. Second, we test the null hypothesis that $\hat{b}_j - \hat{b}_{140} = 0$, against the alternative hypothesis that $\hat{b}_j - \hat{b}_{140} \neq 0$, for each $j \in S \setminus 140$. This exercise tests the importance of the excess bunching in the R\$140 bin relative to other spike bins. We calculate p-values for the statistics using a residual bootstrap, where we draw from the estimated vector of errors in (6) with replacement to generate a new set of counts and apply the procedure described above to construct the statistics.

Table 14 displays the results from the first statistical exercise. Almost all of the spike bins are statistically different from zero. Only the bins R\$110 and R\$130 are not statistically different from zero at the 95% level. These results are in line with what we observe in Figure 5, and confirm our choice of these bins as spikes.

Table 15 displays the results from the second statistical exercise. The interpretation of the results is as follows. A positive coefficient for $\hat{b}_j - \hat{b}_{140}$ indicates that the relative excess bunching in bin j is more important than the relative excess bunching in bin 140.

[12] We decide whether a bin is a spike or not by visual inspection of the histogram. The set of bins that are spikes is $\{90, 93, 95, 100, 103, 110, 112, 116, 120, 125, 126, 130, 133, 138, 140, 150, 160, 166, 175, 190\}$.

[13] Note that this is a one-sided test, which is appropriate for this case, because $\hat{b}_j$ never takes negative values by definition.

A coefficient of zero indicates the the relative excess bunching in bin j is as important as the relative excess bunching in bin 140. And, a negative coefficient indicates that the relative excess bunching in bin j is less important than the relative excess bunching in bin 140. We perform 19 tests, one for each spike bin different from 140. We find nine coefficients that are positive and statistically significant, seven coefficients that are not statistically different from zero, and three coefficients that are negative and statistically significant. That is, the relative excess bunching in all but three of the spike bins are at least as important as the relative excess bunching found in bin 140. Moreover, the bins for which the test is negative and statistically significant include the two bins (110 and 130) that are shown not to present excess bunching at the 95% level in Table 14. This represents strong evidence supporting the argument that the excess bunching found in bin 140 is not atypical in the distribution, and if anything, is less important than the bunching found in the other spike bins.

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8 Tables

Table 1: Summary Statistics

	Both Grades	5^{th} Grade	9^{th} Grade
Test Score Portuguese	-0.125	-0.137	-0.111
	(0.947)	(0.943)	(0.950)
Test Score Mathematics	-0.120	-0.134	-0.104
	(0.952)	(0.949)	(0.954)
Treated	0.339	0.390	0.282
	(0.473)	(0.488)	(0.450)
Parents Engagement	0.980	0.976	0.986
	(0.139)	(0.153)	(0.119)
Reading	0.734	0.784	0.670
	(0.442)	(0.411)	(0.470)
Student Effort	0.621	0.674	0.555
	(0.485)	(0.469)	(0.497)
Work Engagement	0.314	0.290	0.344
	(0.464)	(0.454)	(0.475)
Gender	0.505	0.478	0.536
	(0.500)	(0.499)	(0.499)
Age	10.946	9.149	12.973
	(2.268)	(1.271)	(1.170)
Race	0.581	0.594	0.566
	(0.493)	(0.491)	(0.496)
Parents Education	3.262	3.276	3.248
	(1.903)	(1.879)	(1.929)
Family Size	4.700	4.714	4.685
	(1.620)	(1.659)	(1.576)
Grade in 2009	4.649	2.907	6.618
	(1.978)	(0.689)	(0.698)
Rural	0.115	0.124	0.105
	(0.320)	(0.329)	(0.307)
Region South East	0.362	0.359	0.365
	(0.481)	(0.480)	(0.481)
Observations	1,575,299	873,657	701,642

Note: The table presents mean and standard deviations of the outcomes and baseline covariates used in the study. The sample is all students who participated in *Prova Brasil* in 2011 and were registered in *Cadastro nico* in July 2009.

Table 2: Testing the continuity of covariates around the R$ 120 cut off: Reduced Form

	Both Grades	$5th$ Grade	$9th$ Grade
Region South East	0.030*	0.057***	0.006
	(0.017)	(0.018)	(0.018)
Gender	0.008	0.014**	0.004
	(0.006)	(0.006)	(0.010)
Age	-0.112*	-0.020	-0.021
	(0.057)	(0.065)	(0.045)
Race	-0.047*	-0.077***	-0.019
	(0.027)	(0.025)	(0.031)
Parents' Education	0.007	0.001	0.010
	(0.025)	(0.030)	(0.025)
Family Size	-0.063	-0.035	-0.088
	(0.382)	(0.401)	(0.366)
Grade in 2009	-0.109**	-0.032	-0.013
	(0.048)	(0.045)	(0.044)
Rural	-0.017***	-0.014	-0.020***
	(0.006)	(0.009)	(0.005)
Observations	76,803	38,218	38,585

Standard errors clustered at the income level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The table presents the coefficients of the *Bolsa Famlia* participation dummy variable, its standard error and the number of observations in an OLS regression where the dependent variable is specified in the row label. Second degree polynomials on income were adjusted above and below cut off, including covariates to increase precision. Bandwidth was set to 10. Standard errors are clustered at the discrete income values to account for model choice uncertainty, following Lee and Card (2008).

Table 3: Testing the continuity of covariates around the R$ 140 cut off: Reduced Form

	Both Grades	$5th$ Grade	$9th$ Grade
Region South East	-0.016	-0.016	-0.016
	(0.026)	(0.038)	(0.019)
Gender	-0.001	-0.013	0.011
	(0.010)	(0.011)	(0.018)
Age	0.002	0.059	0.026
	(0.070)	(0.071)	(0.056)
Brown	0.015	0.032	0.001
	(0.034)	(0.041)	(0.031)
Parents' Education	-0.022	-0.014	-0.030
	(0.025)	(0.032)	(0.023)
Family Size	-0.291	-0.240	-0.333
	(0.298)	(0.302)	(0.298)
Grade in 2009	-0.026	-0.007	0.025
	(0.051)	(0.046)	(0.047)
Rural	0.004	0.017	-0.007
	(0.008)	(0.010)	(0.010)
Observations	36,660	17,785	18,875

Standard errors clustered at the income level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Entries on panel (b) come from reduced form regressions of the outcome indicated in the row on a dummy indicating compliance with the assignment rule of household income per person lower than or equal to R$140, a second-order polynomial on income per person, the interaction of the polynomial with the compliance dummy, and all covariates. Coefficients of the compliance dummy, its standard errors and the number of observations are reported. In all regressions bandwidth was set to 10 and standard errors are clustered at the discrete income values to account for model choice uncertainty.

Table 4: Testing the continuity of covariates around the R$ 140 cut off: LATE

	Both Grades	$5th$ Grade	$9th$ Grade
Region South East	-0.263	-0.202	-0.328
	(0.266)	(0.250)	(0.308)
d region5	0.003	-0.041	0.053
	(0.073)	(0.073)	(0.118)
Gender	-0.029	-0.065	0.004
	(0.114)	(0.117)	(0.177)
Age	0.837	0.396	0.178
	(0.876)	(0.555)	(0.542)
Race	0.173	0.145	0.197
	(0.360)	(0.314)	(0.442)
Parents' Education	-0.241	-0.733	0.384
	(0.893)	(0.937)	(1.014)
Family Size	-0.917	-0.737	-1.135
	(2.025)	(1.584)	(2.584)
Grade in 2009	0.684	-0.084	0.490
	(0.686)	(0.248)	(0.478)
Rural	0.038	0.084	-0.020
	(0.070)	(0.081)	(0.100)
Observations	40,057	18,817	21,240

Standard errors clustered at the income level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Entries come from fuzzy RD regressions of the outcome indicated in the row on a dummy indicating participation in the *Bolsa Familia* program (instrumented with a dummy for complying with the assignment rule of household income per person lower than or equal to R$140), a second-order polynomial on income per person, the interaction of the polynomial with the *Bolsa Familia* dummy, and all covariates. Coefficients of the *Bolsa Familia* participation dummy, its standard errors and the number of observations are reported. In all regressions bandwidth was set to 10 and standard errors are clustered at the discrete income values to account for model choice uncertainty.

Table 5: Months of BFP participation among treated and non-treated

	Treated			Non-treated		
Months	N	Percentage	Accumulated	N	Percentage	Accumulated
0	294	4.96	4.96	31,257	99.77	99.77
1	111	1.87	6.83	45	0.14	99.91
2	34	0.57	7.4	0	0.00	99.91
3	51	0.86	8.26	0	0.00	99.91
4	54	0.91	9.17	0	0.00	99.91
5	37	0.62	9.79	0	0.00	99.91
6	70	1.18	10.97	1	0.00	99.91
7	48	0.81	11.78	3	0.01	99.92
8	260	4.38	16.17	0	0.00	99.92
9	83	1.4	17.57	0	0.00	99.92
10	39	0.66	18.22	0	0.00	99.92
11	223	3.76	21.98	1	0.00	99.93
12	544	9.17	31.15	0	0.00	99.93
13	308	5.19	36.35	0	0.00	99.93
14	165	2.78	39.13	1	0.00	99.93
15	942	15.88	55.01	0	0.00	99.93
16	17	0.29	55.29	0	0.00	99.93
17	39	0.66	55.95	0	0.00	99.93
18	46	0.78	56.73	3	0.01	99.94
19	57	0.96	57.69	0	0.00	99.94
20	44	0.74	58.43	0	0.00	99.94
21	191	3.22	61.65	3	0.01	99.95
22	73	1.23	62.88	0	0.00	99.95
23	90	1.52	64.4	0	0.00	99.95
24	79	1.33	65.73	0	0.00	99.95
25	2,033	34.27	100.00	16	0.05	100.00
Total	5,932	100.00		31,330	100.00	

Notes: The table present the number of treated and non-treated students remained in the BFP program between November 2009 and November 2011.

Table 6: Month of BFP entrance

	Income in [126,140]			Income in [114,155]		
Month	N	Percentage	Accumulated	N	Percentage	Accumulated
May-09	1,583	26.48	26.48	19	15.57	15.57
Jun-09	72	1	27.69	10	8.2	23.77
Jul-09	414	6.93	34.62	9	7.38	31.15
Aug-09	1,490	25	59.54	31	25.41	56.56
Sep-09	450	7.53	67.07	12	9.84	66.39
Oct-09	1,919	32	99.18	17	13.93	80.33
Nov-09	29	0.49	99.67	1	0.82	81.15
Nov-11	20	0	100.00	23	18.85	100.00
Total	5,977	100.00		122	100.00	

Notes: The table presents the number of students entering in the BFP program by month between May 2009 and November 2011. Note that there was no student in our sample entered in the program between December 2009 and November 2011.

Table 7: The Effects of Being Below the R$140 Cut Off on Treatment

	Both Grades	5^{th} Grade	9^{th} Grade
Treated	0.235***	0.295***	0.189***
	(0.025)	(0.026)	(0.027)
Observations	31,039	14,610	16,429

Notes: Entries come from a reduced form regression of the participation in *Bolsa Famlia* on a dummy indicating compliance with the assignment rule of household income per person lower than or equal to R$140, a second-order polynomial on income per person, the interaction of the polynomial with the compliance dummy, and all covariates. Coefficients of the compliance dummy, its standard errors and the number of observations are reported. Bandwidth was set to 10. In all regressions standard errors are clustered at the discrete income values to account for model choice uncertainty.

Table 8: Effects of the *Bolsa Famlia* program on Mathematics and Portuguese Language proficiency

	Portuguese	Mathematics
Panel (a): LATE estimates		
Both Grades	0.181***	0.153*
	(0.067)	(0.079)
	31,039	31,039
5th	0.188**	0.130
	(0.074)	(0.108)
	14,610	14,610
9th	0.174	0.188
	(0.138)	(0.183)
	16,429	16,429
Panel (b): Reduced Form estimates		
Both Grades	0.016	0.022
	(0.016)	(0.020)
	36,654	36,654
5th	0.026	0.016
	(0.020)	(0.031)
	17,784	17,784
9th	0.008	0.029
	(0.022)	(0.030)
	18,870	18,870

Standard errors clustered at the income level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Entries on panel (a) come from fuzzy RD regressions of the outcome indicated in the column on a dummy indicating participation in the *Bolsa Familia* program (instrumented with a dummy for complying with the assignment rule of household income per person lower than or equal to R$140), a second-order polynomial on income per person, the interaction of the polynomial with the *Bolsa Familia* dummy, and all covariates. Coefficients of the *Bolsa Familia* participation dummy, its standard errors and the number of observations are reported. Each entry on panel (b) comes from a reduced form regression of the outcome indicated in the column on a dummy indicating compliance with the assignment rule of household income per person lower than or equal to R$140, a second-order polynomial on income per person, the interaction of the polynomial with the compliance dummy, and all covariates. Coefficients of the compliance dummy, its standard errors and the number of observations are reported. In all regressions bandwidth was set to 10 and standard errors are clustered at the discrete income values to account for model choice uncertainty.

Table 9: Mechanisms Through Which *Bolsa Famlia* Affects Learning

	Student Works	Parents Engagement	Student Effort	Reading
Panel (a): LATE estimates				
Both Grades	-0.038***	0.195***	0.080**	0.018
	(0.015)	(0.062)	(0.038)	(0.057)
	11,978	14,411	13,930	14,411
5th	-0.058***	0.152*	0.021	-0.001
	(0.021)	(0.080)	(0.036)	(0.045)
	5,815	6,705	6,491	6,705
9th	-0.007	0.239**	0.173**	0.055
	(0.026)	(0.101)	(0.086)	(0.109)
	6,163	7,706	7,439	7,706
Panel (b): Reduced Form estimates				
Both Grades	-0.006*	0.035**	0.011	0.001
	(0.003)	(0.014)	(0.010)	(0.012)
	14,001	16,815	16,267	16,815
5th	-0.014***	0.035	0.003	0.000
	(0.005)	(0.022)	(0.012)	(0.012)
	7,031	8,107	7,849	8,107
9th	0.002	0.034*	0.022	0.005
	(0.005)	(0.018)	(0.015)	(0.019)
	6,970	8,708	8,418	8,708

Standard errors clustered at the income level in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: Entries on panel (a) come from fuzzy RD regressions of the outcome indicated in the column on a dummy indicating participation in the *Bolsa Familia* program (instrumented with a dummy for complying with the assignment rule of household income per person lower than or equal to R$140), a second-order polynomial on income per person, the interaction of the polynomial with the *Bolsa Familia* dummy, and all covariates. Coefficients of the *Bolsa Familia* participation dummy, its standard errors and the number of observations are reported. Each entry on panel (b) comes from a reduced form regression of the outcome indicated in the column on a dummy indicating compliance with the assignment rule of household income per person lower than or equal to R$140, a second-order polynomial on income per person, the interaction of the polynomial with the compliance dummy, and all covariates. Coefficients of the compliance dummy, its standard errors and the number of observations are reported. Bandwidth was set to 15 in the two leftmost columns and to 20 in the rightmost columns. In all regressions standard errors are clustered at the discrete income values to account for model choice uncertainty.

Table 10: Robustness of *Bolsa Famlia* Effects to Bandwidth Choice

	Panel (a): LATE estimates			
	Bandwidth 15		Bandwidth 20	
	Portuguese	Mathematics	Portuguese	Mathematics
Both Grades	0.439**	0.428**	0.475***	0.524**
	(0.191)	(0.213)	(0.167)	(0.224)
	47,473	47,473	61,393	61,393
5th	0.408**	0.264	0.308**	0.109
	(0.197)	(0.214)	(0.157)	(0.182)
	22,895	22,895	29,699	29,699
9th	0.412	0.600	0.124	0.256
	(0.351)	(0.413)	(0.219)	(0.281)
	24,578	24,578	31,694	31,694
	Panel (b): Reduced Form estimates			
Both Grades	0.055*	0.053	0.069***	0.076***
	(0.029)	(0.036)	(0.021)	(0.028)
	47,473	47,473	61,393	61,393
5th	0.067*	0.043	0.057*	0.020
	(0.035)	(0.041)	(0.032)	(0.036)
	22,895	22,895	29,699	29,699
9th	0.037	0.054	0.014	0.029
	(0.029)	(0.038)	(0.025)	(0.034)
	24,578	24,578	31,694	31,694

Standard errors clustered at the income level in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: Entries on panel (a) come from fuzzy RD regressions of the outcome indicated in the column on a dummy indicating participation in the *Bolsa Familia* program (instrumented with a dummy for complying with the assignment rule of household income per person lower than or equal to R$140), a second-order polynomial on income per person, the interaction of the polynomial with the *Bolsa Familia* dummy, and all covariates. Coefficients of the *Bolsa Familia* participation dummy, its standard errors and the number of observations are reported. Each entry on panel (b) comes from a reduced form regression of the outcome indicated in the column on a dummy indicating compliance with the assignment rule of household income per person lower than or equal to R$140, a second-order polynomial on income per person, the interaction of the polynomial with the compliance dummy, and all covariates. Coefficients of the compliance dummy, its standard errors and the number of observations are reported. Bandwidth was set to 15 in the two leftmost columns and to 20 in the rightmost columns. In all regressions standard errors are clustered at the discrete income values to account for model choice uncertainty.

Table 11: Robustness of *Bolsa Famlia* Effects to Polynomial Order Choice

	Panel (a): LATE estimates			
	Order 0		Order 2	
	Portuguese	Mathematics	Portuguese	Mathematics
Both Grades	0.158**	0.123	0.151	0.246
	(0.078)	(0.086)	(0.132)	(0.177)
	37,569	37,569	37,569	37,569
5th	0.095	0.041	0.304*	0.342
	(0.107)	(0.142)	(0.159)	(0.218)
	17,568	17,568	17,568	17,568
9th	0.241	0.235	0.107	0.279
	(0.149)	(0.162)	(0.228)	(0.426)
	20,001	20,001	20,001	20,001
	Panel (b): Reduced Form estimates			
Both Grades	-0.014	-0.016	0.021	0.034
	(0.012)	(0.010)	(0.021)	(0.029)
	37,569	37,569	37,569	37,569
5th	-0.015	-0.007	0.059*	0.067
	(0.017)	(0.017)	(0.033)	(0.046)
	17,568	17,568	17,568	17,568
9th	0.000	-0.011	0.011	0.027
	(0.014)	(0.017)	(0.021)	(0.041)
	20,001	20,001	20,001	20,001

Standard errors clustered at the income level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Entries on panel (a) come from fuzzy RD regressions of the outcome indicated in the column on a dummy indicating participation in the *Bolsa Familia* program (instrumented with a dummy for complying with the assignment rule of household income per person lower than or equal to R$140), a second-order polynomial on income per person, the interaction of the polynomial with the *Bolsa Familia* dummy, and all covariates. Coefficients of the *Bolsa Familia* participation dummy, its standard errors and the number of observations are reported. Each entry on panel (b) comes from a reduced form regression of the outcome indicated in the column on a dummy indicating compliance with the assignment rule of household income per person lower than or equal to R$140, a second-order polynomial on income per person, the interaction of the polynomial with the compliance dummy, and all covariates. Coefficients of the compliance dummy, its standard errors and the number of observations are reported. Bandwidth was set to 15 in the two leftmost columns and to 20 in the rightmost columns. In all regressions standard errors are clustered at the discrete income values to account for model choice uncertainty.

Table 12: Robustness of *Bolsa Famlia* Effects to Polynomial Order Choice

	Order 2		Order 3	
	Portuguese	Mathematics	Portuguese	Mathematics
Panel (a): LATE estimates				
Both Grades	0.151	0.246	0.364**	0.528***
	(0.132)	(0.177)	(0.169)	(0.206)
	37,569	37,569	37,569	37,569
5th	0.304*	0.342	0.365***	0.192
	(0.159)	(0.218)	(0.139)	(0.162)
	17,568	17,568	17,568	17,568
9th	0.107	0.279	0.384	0.964**
	(0.228)	(0.426)	(0.274)	(0.413)
	20,001	20,001	20,001	20,001
Panel (b): Reduced Form estimates				
Both Grades	0.021	0.034	0.064**	0.092**
	(0.021)	(0.029)	(0.027)	(0.035)
	37,569	37,569	37,569	37,569
5th	0.059*	0.067	0.082**	0.043
	(0.033)	(0.046)	(0.031)	(0.038)
	17,568	17,568	17,568	17,568
9th	0.011	0.027	0.053	0.133***
	(0.021)	(0.041)	(0.032)	(0.043)
	20,001	20,001	20,001	20,001

Standard errors clustered at the income level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Entries on panel (a) come from fuzzy RD regressions of the outcome indicated in the column on a dummy indicating participation in the *Bolsa Familia* program (instrumented with a dummy for complying with the assignment rule of household income per person lower than or equal to R$140), a second-order polynomial on income per person, the interaction of the polynomial with the *Bolsa Familia* dummy, and all covariates. Coefficients of the *Bolsa Familia* participation dummy, its standard errors and the number of observations are reported. Each entry on panel (b) comes from a reduced form regression of the outcome indicated in the column on a dummy indicating compliance with the assignment rule of household income per person lower than or equal to R$140, a second-order polynomial on income per person, the interaction of the polynomial with the compliance dummy, and all covariates. Coefficients of the compliance dummy, its standard errors and the number of observations are reported. Bandwidth was set to 15 in the two leftmost columns and to 20 in the rightmost columns. In all regressions standard errors are clustered at the discrete income values to account for model choice uncertainty.

Table 13: Effects of the treatment on the Probability of Participating in *Prova Brasil*

	Group 1	Group 2
Probability of Participation in PB	0.030	-0.140
	(0.055)	(0.139)
	105,200	135,152

Notes: Entries come from fuzzy RD regressions of the probability of participation in *Prova Brasil* on a dummy indicating participation in the *Bolsa Familia* program (instrumented with a dummy for complying with the assignment rule of household income per person lower than or equal to R$140), a second-order polynomial on income per person, the interaction of the polynomial with the *Bolsa Familia* dummy, and covariates. Coefficients of the *Bolsa Familia* participation dummy, its standard errors and the number of observations are reported. Group 1 denotes the group students in third and fourth grades in 2009, that is, the group of potential participants in Prova Brasil 2011 as fifth graders. Group 2 denotes the group students in seventh and eight grades in 2009, that is, the group of potential participants in Prova Brasil 2011 as ninth graders. In all regressions standard errors are clustered at the discrete income values to account for model choice uncertainty and bandwidth was set to 10.

Table 14: Bunching Tests - 1

statistic	coefficient	p-value
$\hat{b}_{90}$	0.385***	0.000
$\hat{b}_{93}$	0.174**	0.011
$\hat{b}_{95}$	0.574***	0.000
$\hat{b}_{100}$	0.857***	0.000
$\hat{b}_{103}$	0.700***	0.000
$\hat{b}_{110}$	0.105	0.134
$\hat{b}_{112}$	0.342***	0.000
$\hat{b}_{116}$	0.705***	0.000
$\hat{b}_{120}$	0.639***	0.000
$\hat{b}_{125}$	0.563***	0.000
$\hat{b}_{126}$	0.584***	0.000
$\hat{b}_{130}$	0.193*	0.050
$\hat{b}_{133}$	0.439***	0.000
$\hat{b}_{138}$	0.518***	0.000
$\hat{b}_{140}$	0.435***	0.000
$\hat{b}_{150}$	0.806***	0.000
$\hat{b}_{160}$	0.372***	0.009
$\hat{b}_{166}$	0.535***	0.000
$\hat{b}_{175}$	0.689***	0.000
$\hat{b}_{190}$	0.679**	0.013

Notes: This table displays the results from formal statistical tests for the null hypothesis that $\hat{b}_j$ is equal to zero, for bin j. $\hat{b}_j$ is the estimated excess number of individuals relative to the actual count of individuals, for bin j. p-values were constructed using a residual bootstrap. *** denotes statistically significance at 99% level. ** denotes statistically significance at 95% level. * denotes statistically significance at 90% level.

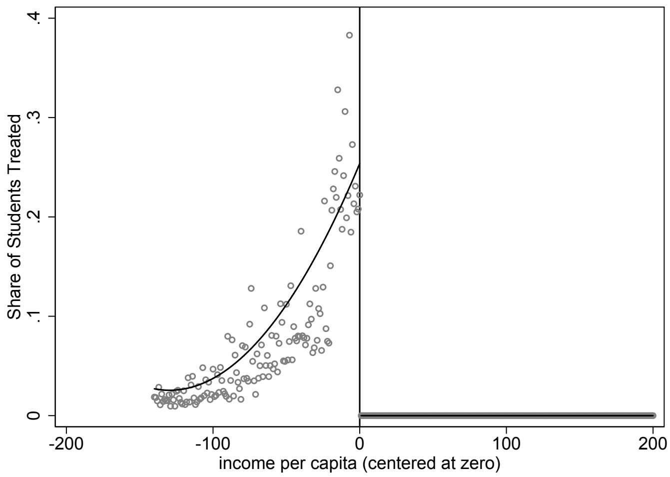
Table 15: Bunching Tests - 2

statistic	coefficient	p-value
$\hat{b}_{90}$ - $\hat{b}_{140}$	-0.051	0.330
$\hat{b}_{93}$ - $\hat{b}_{140}$	-0.261**	0.023
$\hat{b}_{95}$ - $\hat{b}_{140}$	0.139*	0.086
$\hat{b}_{100}$ - $\hat{b}_{140}$	0.422***	0.000
$\hat{b}_{103}$ - $\hat{b}_{140}$	0.264***	0.000
$\hat{b}_{110}$ - $\hat{b}_{140}$	-0.330***	0.009
$\hat{b}_{112}$ - $\hat{b}_{140}$	-0.094	0.253
$\hat{b}_{116}$ - $\hat{b}_{140}$	0.270***	0.001
$\hat{b}_{120}$ - $\hat{b}_{140}$	0.204***	0.007
$\hat{b}_{125}$ - $\hat{b}_{140}$	0.127	0.121
$\hat{b}_{126}$ - $\hat{b}_{140}$	0.149*	0.073
$\hat{b}_{130}$ - $\hat{b}_{140}$	-0.242**	0.032
$\hat{b}_{133}$ - $\hat{b}_{140}$	0.003	0.527
$\hat{b}_{138}$ - $\hat{b}_{140}$	0.083	0.258
$\hat{b}_{150}$ - $\hat{b}_{140}$	0.371***	0.000
$\hat{b}_{160}$ - $\hat{b}_{140}$	-0.063	0.266
$\hat{b}_{166}$ - $\hat{b}_{140}$	0.100	0.211
$\hat{b}_{175}$ - $\hat{b}_{140}$	0.254**	0.011
$\hat{b}_{190}$ - $\hat{b}_{140}$	0.244*	0.080

Notes: This table displays the results from formal statistical tests for the null hypothesis that $\hat{b}_j - \hat{b}_{140}$ is equal to zero, for bin $j \neq 140$. $\hat{b}_j$ is the estimated excess number of individuals relative to the actual count of individuals, for bin j. p-values were constructed using a residual bootstrap. *** denotes statistically significance at 99% level. ** denotes statistically significance at 95% level. * denotes statistically significance at 90% level.

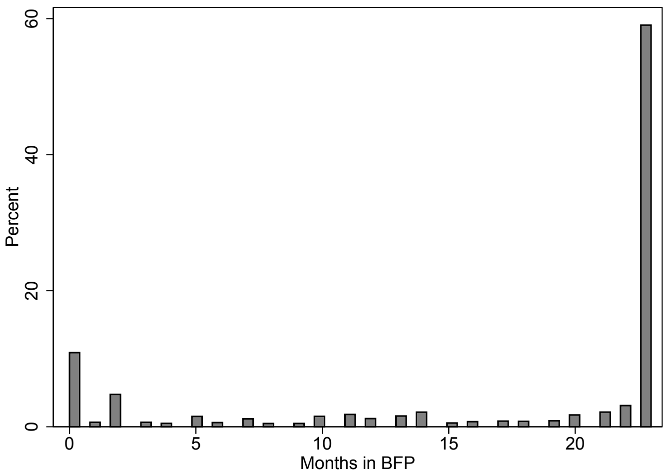
9 Figures

Figure 1: Treatment probability, by household income per capita



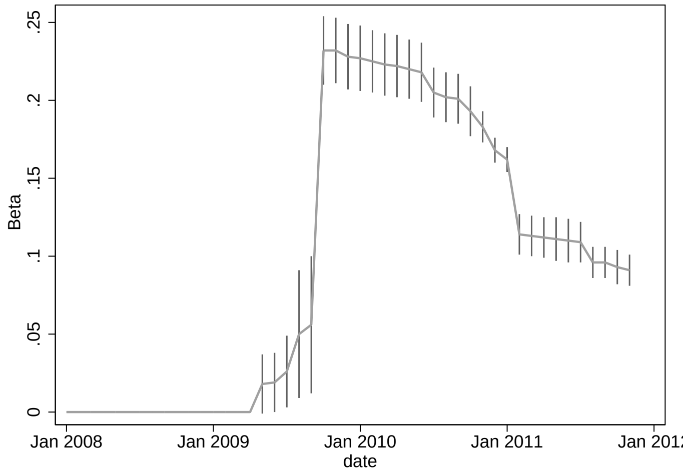
Notes: This figure the average test score in the indicated subject by income per person. Dots represent mean values of the variable of interest within bins of R$ 1. The continuous lines are fitted second degree polynomials.

Figure 2: Permanence in BFP after cut off change



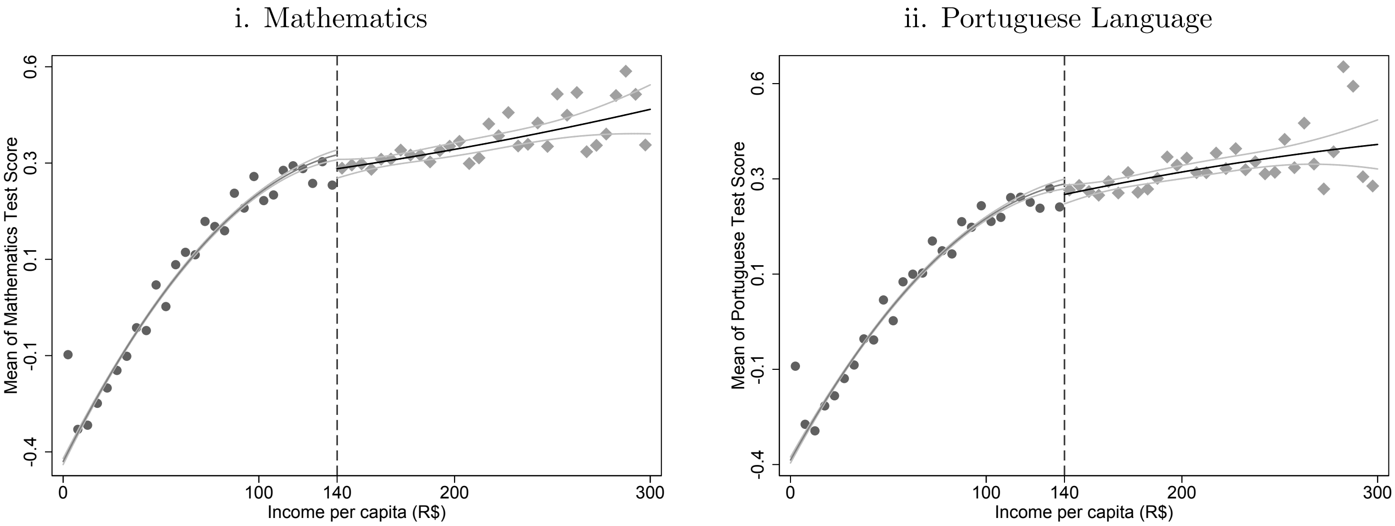
Notes: This figure plots the fraction of individuals in the treatment group by the number of months of *Bolsa Famlia* transfers received between August 2009 and November 2011.

Figure 3: First Stage by Month



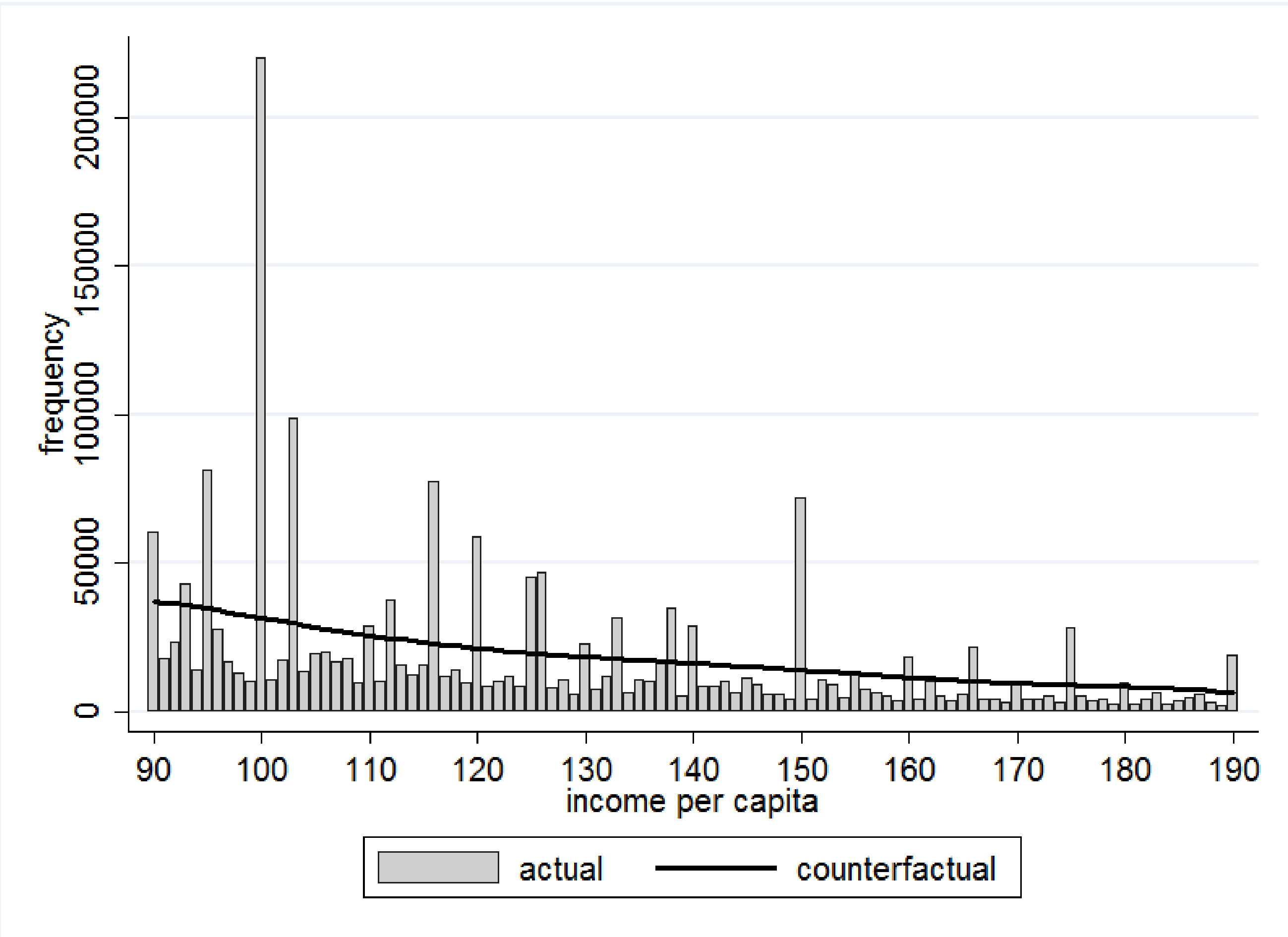
Notes: The graph presents the results of a first stage regression of the probability of participating in *Bolsa Famlia* on a dummy variable that indicates if the student is above or below the program cut-off. The vertical bars represent standard deviations.

Figure 4: Outcomes, by household income per person



Notes: This figure the average test score in the indicated subject by income per person. Dots represent mean values of the variable of interest within bins of R$ 1. The continuous lines are fitted second degree polynomials.

Figure 5: Bunching test: empirical and counterfactual distributions



Notes: This figure plots a histogram of the empirical distribution of household income per person in October 2009, and a counterfactual distribution, that simulates the income per person distribution in the case of no excess bunching at specific bins (i.e. no spikes in the histogram). Each bar represents the actual number of individuals with an amount of income per person given by the bin label. The solid line is a seventh-degree polynomial fitted to the empirical distribution excluding the dummies for the spikes.