

Poverty and the Political Economy of Public Education Spending: Evidence from Brazil

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Abstract

A large literature has emphasized elite capture of democratic institutions as the explanation for the low levels of spending on public education in many low-income democracies. This paper provides an alternative to that longstanding hypothesis. Motivated by new cross-country facts and evidence from Brazilian municipalities, we hypothesize that many democratic developing countries might invest less in public education spending because poor decisive voters prefer the government to allocate resources elsewhere. One possible explanation is that low-income voters could instead favor redistributive programs that increase their incomes in the short run, such as cash transfers. To test for this possibility, we design and implement an experimental survey and an incentivized choice experiment in Brazil. The findings from both interventions support our hypothesis.

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“Let’s be frank: we do not give importance to education because the voters do not give it either. Nobody wins an election talking about education: I, incidentally, am an example. If the people awaken to education, rulers will have to act.”

Cristovam Buarque, Brazilian Senator, former Minister of Education and presidential candidate.¹

1 Introduction

Given the evidence of high returns to education for the poor in developing countries, it is surprising that many of those nations have low levels of public education spending.² A large literature on historical development emphasizes the underprovision of public education *by elites* as an explanation to this puzzle (see, for example, Engerman and Sokoloff, 2000, Mariscal and Sokoloff, 2000, and Acemoglu and Robinson, 2001, 2006). The literature proposes several possible reasons: elites might not want to pay for education; they might want to maintain access to a supply of inexpensive labor; or they might want to avoid empowering the citizenry, which could lead to revolts and the overthrow of the ruling group’s power. According to these views, improving institutions, by implementing *de facto* enfranchisement, should increase the poor’s access to education. As a result, democracy could lead to growth, via increased investment in human capital demanded by poor voters.

However, when one looks at the existing data, a puzzle arises. When examining cross-country evidence, Mulligan, Gil, and Sala-i-Martin (2004) suggest that more democratic political institutions (as measured by the Polity IV project) are not associated with higher levels of public education spending. While we replicate their findings (see Table 1, column 1), a closer look at the data suggests that the puzzle is even more complex. Using their same dataset and specifications, if one analyzes the correlation between democracy levels and public education spending at different levels of country income, one observes that there is in fact a *negative* relationship between these two variables in poor countries, and a positive relationship in rich countries (see Table 1, column 2 for the interaction between democracy and per capita income, and column 3 for the interaction

¹See Guedes, S. (2007) “Presidente da Comissão de Educação, Cristovam defende revolução no ensino,” <http://www12.senado.gov.br/noticias/materias/2007/03/02/presidente-da-comissao-de-educacao-cristovam-defende-revolucao-no-ensino>, March 3).

²See Psacharopoulos, 1985 and 1994, and Duflo, 2001, for evidence on returns to education in developing countries.

with median income)³. Although these facts do not necessarily rule out arguments of elite capture of institutions, it is difficult to reconcile these patterns with most stories of underprovision of education by elites, which would not predict a *negative* effect of democracy on public education spending in poor countries.

This paper proposes a simple, alternative explanation for the low public education expenditures in many poor democratic countries. We argue that public education spending may be low not because the rich oppose it, but because the poor prefer that governments allocate resources elsewhere, such as direct cash/fiscal transfers. Indeed, there are several plausible models that would lead the poor to use the franchise to demand cash transfers rather than greater government educational spending. Poor households have high marginal utility of current income, are more likely to face credit constraints, and may be relatively impatient. Moreover, the poor might also prefer cash transfers to public education spending due to framing, mental accounting (see Tversky and Kahneman, 1981), or salience effects (see Bordalo, Gennaioli and Shleifer, 2013). Indeed, the same income increase is more salient to someone who is departing from a lower income level than it is for someone who is departing from a higher one.⁴

To test our hypothesis, we combine both observational and experimental data from Brazil. We first examine cross-municipality evidence from the country, as municipalities are responsible for public spending in primary education. We observe that for municipalities with low median incomes (and hence a likely poorer decisive voter since voting is compulsory), increases in municipal public education spending are associated with *decreases* in the probability of reelection of incumbent parties, whereas in municipalities with high median incomes, increases in that type of spending are associated with higher probability of incumbent party reelection. These patterns reproduce the cross-country findings, but in a more uniform economic and institutional setting. Nevertheless, the evidence from municipalities still presents some issues: the analysis is correlational and does

³As observed in column 4 of Table 1, the pattern is robust to excluding tertiary education, which is less likely to benefit the poor

⁴More precisely, and following BGS more closely, consider two individuals with similar levels of education, but with one being poorer than the other. Suppose they are both offered a bundle of government spending that could be allocated to either improvements in education or to increases in individuals' income via cash transfer. Relative to education, a given increase in income would be more salient to the poorer individual, who would then attach a disproportionately high weight to that attribute of the bundle of government spending.

not deal with potentially unobserved heterogeneity/omitted variables, or even reverse causality. To tackle these issues, we move to experimental evidence.

We implement two interventions in the *Distrito Federal* (DF) state, surrounding (and including) the capital of Brazil, Brasília. The first intervention is a survey with randomized information shocks to respondents on how their local government allocated resources in previous years. After receiving an information shock (or no shock), respondents are asked to rate their local government. First we look at an information shock reporting increases in public education spending accompanied by decreases in public expenditures associated with cash transfers. In terms of perceived priorities of the government, this shock makes subjects associate the local government more with improvements in public education and less with increases in cash transfers. Next, we show that among *poor respondents* such information shock causes more *negative* ratings of the local government, when compared with poor respondents who receive no information on government spending. Among rich respondents, we find the opposite effect: information on more educational spending and less spending on cash transfers leads to more positive ratings. These results are consistent with a demand-driven interpretation of both the cross-country and cross-municipality results.

However, the findings from the survey intervention have potential limitations. First, people with different incomes may differ in many unobservable dimensions. Second, the poor may not think that public education spending would benefit them as directly as cash transfers would.⁵ To deal with these concerns, we introduce a second experimental intervention, where we exogenously vary income and propose an education treatment in which recipients benefit *directly* from the educational investment.

This second experiment involves eighty parents of fourth and fifth graders enrolled in a public school in a poor district of the DF state. We exogenously increase household income for a subset of the sample for two months (an increase of about 25% of the median monthly household income in the sample). We next assess how this change in income shifts parents' incentivized choices between more cash and free after-school tutoring sessions for their child. We observe that increased

⁵We believe that it is unlikely that our results are driven by this alternative mechanism. As we discuss below, in the setting we study, public schools are mostly attended by poor children. Also, the DF government is responsible for spending on primary and secondary education, which, unlike tertiary education, is more likely to benefit poor households.

household income *causally* moves parents towards preferring more education in an education-versus-cash tradeoff. Regardless of whether this result is coming from an income effect or from differential salience of cash transfers, it corroborates our hypothesis using revealed preference evidence. This second experiment offers a specific type of educational investment, and only a temporary income shock. Furthermore, we cannot rule out the presence of other channels in the more natural settings analyzed in the other parts of the paper. Still, this revealed preference design indicates that lower income can play an important role when individuals face decisions involving tradeoffs similar to the ones faced outside of the experimental setting.

This paper relates to recent work that has analyzed preferences for public spending among poor voters in developing countries.⁶ We add to this literature by designing experiments that directly measure the preferences of low-income voters between two different types of redistributive spending.

Also related to our argument is a growing literature indicating that poor individuals are more likely to make intertemporal choices that appear to be short-run biased, either because of differences in preferences or due to tighter constraints that the poor might face to their optimizing behavior.⁷ We contribute to this literature by isolating the role of very low income levels directly driving short-run bias in policy choice by the poor.

More recently, a few papers have provided evidence consistent with our argument. When assessing the relationship between education and military rivalry, Aghion, Persson, and Rouzet (2012) find that democratic transitions are negatively associated with educational investments. Alesina and Reich (2013) provide historical evidence that public education was not a priority of low-income rioters in 19th century Europe. In the other direction, Stasavage (2005) finds evidence that electoral competition in African countries is associated with increased primary education

⁶See Manacorda et al. (2011) and Fujiwara (2011) for empirical analyses in South American countries. See Chattopadhyay and Duflo (2004) and Beaman et al. (2009) for evidence from India. For U.S. evidence, see Husted and Kenny (1997), Naidu (2010), and Cascio and Washington (2012).

⁷See Dynan, Skinner and Zeldes (2004) on saving rates; Behrman, Birdsall and Szekely (1998) on educational investments; and Aleem (1990), Dreze, Lanjouw and Sharma (1997), and Skiba and Tobacman (2007), on borrowing behavior. Some studies have argued that the poor might have higher discount rates, and more harmful self-control problems than higher-income individuals do (see Hausman, 1979, Lawrance, 1991, and Harrison, Lau and Williams, 2002, Banerjee and Mullainathan, 2009, and Bernheim, Ray and Yeltekin, 2011). Some papers have also assessed the effects of providing the poor with informal saving technologies and/or commitment devices (e.g., Ashraf, Karlan and Yin, 2011, Duflo, Kremer and Robinson, 2011, Dupas and Robinson, 2011, and Brune et al., 2013). Finally, Shah, Mullainathan, and Shafir (2012) argue that scarcity itself can lead to short-run biases.

spending.⁸

Finally, in terms of experimental design, our paper relates to recent work that has also used randomized information shocks (see Jensen, 2009, Card et al., 2012, Banerjee et al., 2010, and Kendall, Nannicini, and Trebbi, 2015). More closely related to our theme of preferences for redistributive spending is the paper by Kuziemko et al. (2013), which develops online survey experiments assessing the effects of information shocks about inequality and taxes on preferences for redistribution.

The remainder of the paper is organized as follows. In Section 2, we present the cross-municipality analysis. Section 3 presents the design and results from our first, and main intervention (i.e., the survey with information shocks). In Section 4, we describe the design and present the results from our second, auxiliary experiment with incentivized choices between cash transfers and free tutoring. Section 5 concludes.

2 Motivating Evidence: Brazilian Municipalities

The Brazilian educational system generally performs poorly in international evaluations of the quality of education, such as the PISA test.⁹ Still, returns to schooling in the country are quite high.¹⁰ According to the 2011 Brazilian National Household Survey (PNAD), 98.4% of children in aged 7 to 14 are enrolled in primary school (due to compulsory education laws). Moreover, the vast majority of students in Brazil are enrolled in public schools (76.8% of all students surveyed). In that survey, the median monthly household per capita income in families with children enrolled in public schools was 36.6% of that income level for families with children enrolled in private schools. As seen in Appendix Figure A.1, the percentage of children aged 7 to 14 enrolled in public schools goes down as household per capita income increases: for the first quintile of the household per capita income distribution it is 97% and for the fifth quintile it is down to 38%. This suggests that

⁸Other work has found evidence that democracies in Latin America spend more than do autocracies on social items such as education and health (e.g., Ames, 1987, Avelino, Brown and Hunter, 2005, and Brown and Hunter, 1999). Acemoglu et al. (2013) theoretically and empirically study the relationship between democracy, redistribution, and inequality.

⁹In the 2009 wave of the PISA test, Brazil was ranked 53rd out of 65 participant countries, and 19th among the 31 non-OECD surveyed countries.

¹⁰The average wage of someone with a high-school degree in Brazil is 101% higher than that of someone with no schooling (PNAD, 2011).

low-income households are more likely than high-income ones to directly benefit from investments in public education.

Municipalities are the lowest level of government in Brazil, below the federal and state governments.¹¹ In 2010, there existed 5,563 municipalities in Brazil. Municipal elections for all municipalities take place simultaneously in Brazil every four years in October and voting is compulsory for individuals aged 18 to 70.¹² Municipalities are responsible for basic (primary) education, which corresponds to elementary and middle school in the US.¹³

We construct a dataset combining demographic, electoral, and public spending data from different official sources for all municipalities in Brazil for which data are available.¹⁴ We build a panel of municipalities for the 2004 and 2008 elections, and look within municipalities at the effect of increases in the log of municipality spending on public education on whether or not the incumbent party was reelected for the mayor’s office.¹⁵

We start by running the following OLS regression, for municipality i in election year t :

$$Reelection_{i,t} = \beta_0 + \beta_1 * \ln(Educ_{i,t}) + \beta_1 * \ln(Budget_{i,t}) + \delta' \mathbf{X}_{i,t} + \eta_i + \gamma_t + \varepsilon_{i,t}. \quad (1)$$

In Equation 1, $Reelection_{i,t}$ is a dummy variable equal to one if the incumbent party is reelected in election year t (2004 or 2008) in municipality i , and zero otherwise. The variable $\ln(Educ_{i,t})$ is the log of the yearly average of the level of public education spending in municipality i during each term $t = 2004, 2008$.¹⁶

Since our main hypothesis is about tradeoffs in public spending, we also control for total mu-

¹¹ The setting of government spending in Brazilian municipalities has been used by other recent papers, such as Ferraz and Finan (2008, 2010), Caselli and Michaels (2013), Brollo et al. (forthcoming), Litschig and Morrison (forthcoming), and more closely related to this paper, Firpo, Pieri, and Souza (2012).

¹²Nationwide turnout was over 85% in the 2004 elections, according to the Superior Electoral Court, *TSE*.

¹³Technical and financial collaboration of federal government entities are encouraged, but not required by the letter of law. In 1996, Law N. 9394, *Lei de Diretrizes e Bases da Educação Nacional*, was passed, requiring municipalities to allocate at least 25% of their budget to education spending.

¹⁴See Appendix A for a description of the variables used and the data sources.

¹⁵We focus on reelection of the incumbent party as our dependent variable due to the existence of term limits for incumbent candidates (two consecutive terms) in Brazil.

¹⁶To calculate our main explanatory variable of interest, we first deflate the yearly levels of municipal spending on public education to end of 2000 prices in Brazilian *Reals*, then we compute the yearly average of that variable for each one of the terms analyzed (2001-2004 and 2005-2008), and finally we calculate the log of each one of these two averages.

municipal budget, measured by $\ln(\text{Budget}_{i,t})$, i.e., the log of yearly average budget for municipality i during each term. For a given budget size, we are interested in the electoral impact of increasing educational spending (and therefore reducing other types of spending). Without controlling for total budget, we would only be capturing the effect of increased educational spending, which could happen together with increases in all other spending categories if the budget is also growing.

We include municipality fixed effects (η) to control for unobservable municipal characteristics that do not vary through time, and a time dummy (γ) to control for a general time trend between the two periods. Since municipality-level, socio-demographic controls were only measured once during the period under consideration, we do not include them. Finally, \mathbf{X}_i is a vector of mayors' characteristics (age, gender, schooling level, and political party). In our regressions, we drop the two municipalities with zero reported median income (the results are unchanged if they are kept).

We are interested in examining whether increasing education spending is associated with a greater or lower probability of reelection, separately for municipalities with lower and higher levels of median income. We first linearly interact $\ln(\text{Educ}_{i,t})$ with $\ln(\text{median income in 2000})$. Then, to better see the overall pattern of effects of increasing education spending across municipality median income levels, we also present results from interacting $\ln(\text{Educ}_{i,t})$ with each quintile of the median income distribution across municipalities.¹⁷

By including municipality fixed effects in our analysis, we know that our results cannot be driven by time-invariant differences across municipalities, and in particular, their degree of capture of institutions by elites. By looking within municipality, we are able to examine the effect of decisions of a mayor, as opposed to capturing the impact of characteristics from a given municipality. The identification is not perfect though since greater investments in education might correlate with an omitted variable that we do not observe. However, it is still valuable to analyze whether the party of a mayor who chooses to spend more on education as opposed to other categories of public spending is more or less likely to be removed from power in the next election.

Appendix Table A.1 reports the summary statistics for the municipal spending and mayors' personal characteristics variables used in the regressions. Table 2 displays our regression results.

¹⁷Our patterns of effects are kept if we use other quintiles to split our samples, such as quartiles or sextiles.

Column 1 of that table shows that increases in public (primary) education have a very slightly positive, yet not significant effect on the probability of party reelection. In column 2, we include the linear interaction of the log of education spending with the log of municipal median income in Brazilian *Reais* at 2000 prices, drawn from the 2000 Brazilian census. In column 3, we add interactions of the log of municipal spending on public education with dummies of the quintiles of the distribution of monthly municipal median income in our sample.¹⁸

We observe that in municipalities with low levels of median income, more education spending is associated with a *lower* probability of party reelection (significantly so for the first quintile). Moreover, as income goes up, the effects of increased public education spending also goes up almost monotonically, becoming significantly positive in the fourth and fifth quintiles.¹⁹ One might argue that looking at the effect of increases in actual public education *quality* might be more meaningful than looking at public education spending, since some of the spending might not translate into improvement in education (due to corruption, leakage, etc.). Thankfully, the Brazilian government collects a municipality-level index of public primary education quality, called IDEB (*Índice de Desenvolvimento da Educação Básica*), which combines information on grade passing rates and scores on a national standardized exam, *Prova Brasil*, separately for grades one through four and five through nine. The IDEB started being computed by the federal government in 2005. In our analysis, we use two waves, 2005 and 2009, corresponding to the levels of public primary education quality right after the 2001-2004 and the 2005-2008 terms.

Table 3 shows that our effects patterns are kept if we look instead at measures of public primary education quality (as measured by the IDEB or by grade passing rates). Across all four measures, for municipalities with low levels of median income, more public primary education quality is associated with a lower probability of party reelection, and the effect of more education quality on the likelihood of party reelection becomes positive as median income goes up.

¹⁸Our results are unchanged when we include a dummy on whether the incumbent mayor was facing a term limit (we lose observations when adding that variable since the information is not available for all municipalities for the term 2001-2004). Our findings are also unchanged when we include the previous election cycle (1997-2001), thus having a panel with three elections. However, personal information was not available for all mayors elected in 1996, so we decide to focus on the two cycles for which we could collect more information.

¹⁹Although the rich might not benefit directly from public education spending, they might benefit indirectly through channels such as crime reduction or increased qualification of the labor force.

The observed patterns are consistent with our argument that low levels of public education spending could be reflecting the voting choices of the poor. However, the evidence we have presented so far is correlational and might be difficult to interpret. Our experimental evidence will allow us to test our hypothesis and rule out potential alternative explanations for the patterns described in this section.²⁰

3 Survey Experiment: Information Shocks and Rating the Local Government

In the previous section, we presented motivating evidence suggesting that poor voters might prefer their government to devote resources to expenditures other than public education. Our argument that the poor prefer the government to devote resources to categories that satisfy their most urgent needs (via increased income) suggests that low-income individuals would prefer the government to spend resources in cash transfers rather than in public education. If the hypothesis is true, the poorer the individual is, the less he or she will support a government that increases spending on public education while reducing spending in cash transfers. To assess our argument, we analyze how respondents to a survey react, in terms of their rating of the local government, to (randomly provided) information shocks reporting past changes in local public spending.

3.1 Experimental Design

3.1.1 Sample

We designed a survey, implemented in August 2009 in the *Distrito Federal*. Although the DF is technically viewed as a state, it is geographically comparable to a municipality (and is in fact composed of only one municipality). The DF is run by one local government which decides and implements the bulk of expenditures on public education (including primary education and excluding higher education). In 2009, the local government was also responsible for the bulk of cash transfer

²⁰Potentially confounding factors include unobserved changes in municipality-level characteristics that could affect how education spending impacts reelection outcomes, or omitted mayor characteristics that correlate with both education spending and reelection prospects, and differentially so for poor and rich municipalities.

spending, under the program *Bolsa Escola, Vida Melhor*, separately from the federal program *Bolsa Familia*.

Our survey was conducted by surveyors hired by a local polling organization, who did not have any knowledge of the purpose of the study. They interviewed a random sample of 2,003 individuals in twelve of the twenty districts that compose the DF. The company used the sampling method they commonly use in political poll, by approaching and surveying individuals in areas of the DF with large levels of foot traffic (markets, bus terminals, shopping centers), during different days of the week.

Participation in the survey was voluntary and started with socio-demographic questions, in which the respondent would report his or her age, gender, marital status, personal and household income levels, years of schooling, total number of children, number of children in school, number of children in public school, and whether the household benefits from the local cash transfer program.²¹

3.1.2 Direct Comparison of Public Education and Cash Transfers

To motivate our analysis, before describing the information shocks intervention, we describe results from directing asking respondents about their views on public education vs. cash transfers. At the end of the survey, after the measurement of the main outcome variable of interest that we discuss below (the rating of the government), each respondent answered the question: *What is more important for the state government to achieve? (a) Improve public education; (b) Increase cash transfers.*

Figure 1 presents our findings, by plotting by quartile of the household income distribution the fraction of subjects pointing out improving public education as a more important goal for the state government than increasing cash transfers.²² From quartile 1 to quartile 2, the fraction of

²¹For an English version of the survey, see Appendix D.

²²One might note that, although the fraction of respondents pointing out public education improvement as the more important goal increases with the level of income of the respondent, the fraction of respondents reporting it is above 50% for all quartiles of the household income distribution in the sample. This could be due to social desirability effects in the survey (respondents might consider that is more socially acceptable to report public education as more important than cash transfers). However, if we assume that those effects do not interact with the level of income of the respondent or the respondent's household, then the presence of those effects would increase the probability of favoring public education spending for all quartiles, but not the difference across quartiles. Finally, it is worth noting that the DF state is also by far the richest state in per capita terms in Brazil. The level of GDP per capita

respondents choosing education over cash transfers significantly increases; the same is true when moving from quartile 2 to quartile 3. In quartiles 3 and 4, that share is approximately unchanged. Appendix Table A.4 replicates these findings by presenting regressions controlling for observables. These findings suggest that as respondents' income goes up respondents are more likely to prefer public education improvements over cash transfer increases.

3.1.3 Information Shocks

After the first set of demographic questions, the survey followed with our “treatments”: randomized information shocks on how the local government allocated resources in the previous years. We randomized at the individual level the specific shock that each subject received. All the information shocks involved actual amounts of public spending changes that we gathered from the Brazilian Ministry of the Economy. After receiving the information shock, each subject was asked to rate the current administration of the local government (which started in 2007) by giving it a grade from 0 to 10. Each individual in the sample was assigned to one of four different treatment groups.

In the first one, the *No information* treatment group, individuals received no information shocks and were simply asked to rate the local (state) government. This group serves as a “control group” for our analysis. In all three other treatment groups, subjects received information shocks phrased similarly, as follows.

In the *More education* treatment group, the surveyor would read the following passage before asking the subject to rate the local government: *Did you know that, compared to 2006, the state government increased in 2007 the share of total public expenditures allocated to public education by 9%?* This treatment therefore provides information that the local government is spending more resources on public education, without revealing any tradeoff in public spending (lower share of public spending in other categories).²³

in the DF state in 2009 was 93% higher than that of the state ranked second (Sao Paulo), and 201% higher than the national level (IBGE). Also, according to the 2009 Brazilian National Household Survey (PNAD), the 25th-percentile monthly family income level in the DF state was 47% higher than the national level in that year, while the median monthly family income level was 67% higher. Based on our results, one could therefore expect that higher shares of the population would favor cash transfers over public education spending in the rest of the country.

²³Across all surveyed subjects, only two answered that they were previously aware of the information provided by the shocks. Therefore, we interpret the treatments as information shocks, rather than drawn attention or priming

In the *More education, less cash* treatment group, the surveyor would read the following passage before asking the individual to evaluate the state government: *Did you know that, compared to 2006, the state government increased in 2007 the share of total public expenditures allocated to public education by 9% but reduced the share allocated to social assistance by 9%?*²⁴ The *More education, less cash* treatment provides information that the local government is spending a higher fraction of resources on public education, but also reveals a possible tradeoff: the fraction of public spending associated with cash transfers has been reduced. It is important to emphasize that this information shock has the exact same wording as the previous one, with only the addition of the tradeoff passage at the end.

Finally, in the *More cash* treatment group, the following passage would be read: *Did you know that compared to the first year in the previous state government, the current state government increased in its first year the share of total public expenditures allocated to social assistance from 1.3% to 3.1%?*²⁵

treatments.

²⁴Since there is no category directly named “cash transfers” in the public spending dataset that we gathered, we use the category “social assistance” as our measure of cash transfer expenditures. To be sure that the association is valid, subjects were asked in the beginning of the survey (after answering demographic questions and before receiving any information shock) to point out which category of spending they believed cash transfers were part of. They were offered four options: *Public education*, *Social assistance*, *Other category/categories*, and *Does not know/no answer*. For 95.8% of the subjects, cash transfers spending is part of the *Social assistance* category, whereas for only 0.2% of them cash transfers are part of the *Public education* category. For 0.6% of the individuals, cash transfers are part of some other category, and 3.4% of respondents reported to not know the answer to the question or did not answer. In the analysis of the treatment effects, we add a dummy on whether the subject did not indicate *Social assistance* as the cash transfer category. Unfortunately, we do not observe their beliefs about which other items are part of “social assistance.” Still, we believe respondents were indeed thinking about cash transfers when told about social assistance. First, the follow-up survey shows that the main information shock updates people’s priors substantially: they are much more likely to report “increasing cash transfers” as the bigger priority of the administration, after they hear about the increase in the share of spending on “social assistance.” It is important to note that the wording on the priors update question refers to *cash transfers* and not to *social assistance*. Furthermore, in the direct question about what respondents think should be the bigger priority, the survey also asked explicitly about “cash transfers” and not “social assistance.” The income gradient in responses to the direct question mirrors our findings from the information shocks.

²⁵This treatment was implemented as a robustness check to address a possible alternative interpretation to the findings regarding the other two treatments, namely that lower-income subjects might just react more negatively to any information shock on how the local government spent resources. Unfortunately, since we were constrained in terms of only reporting true variations in spending patterns, the *More cash* information shock has a slightly different wording and refers to a different period from the previous shocks. However, we do not believe this is a major issue, since our main goal is to assess how low- and high-income subjects react to the same information shock, and given that our main goal is to look at the effects of shocks involving information on public education spending.

3.1.4 Outcome of Interest: Rating the Local Government

As mentioned above, after receiving an information shock (or no information if the respondent was part of the *No information* group), each subject was asked to rate the current administration of the local government (which started in 2007) by giving it a grade from 0 to 10. The wording of the question was: *From 0 to 10, what grade would you give to the Arruda administration until now?* Respondents' 0-10 rating of the government in the survey is our dependent variable in our main analysis.

Finally, after assessing the local government, each respondent ended the participation in the survey by answering the following two questions. First, participants were asked the direct comparison question described above (*What is more important for the state government to achieve?*). Then, they were asked: *Which of the following two numbers do you believe to be greater? (a) The amount actually spent in improving public education for every R\$100 allocated by the local government to public education spending; (b) The amount actually spent in increasing cash transfers for every R\$100 allocated by the local government to cash transfer spending.* This last question provides a measure of the perception of the relative effectiveness (and measure of diversion of funds/corruption) in the two types of public spending.

3.1.5 Information Shocks and Update of Priors about the Government's Priorities

An important question is, do individuals exposed to the "More education, less cash" information shock associate more the state government administration under consideration with improvements in public education and less with increases in cash transfers, when compared to individuals not exposed to any information? We conduct a follow-up survey with a very similar sample and find that individuals across income levels share similar priors about the government's priorities, and that individuals, indeed, update their priors in the direction intended, and similarly across income levels (see Appendix B for a more detailed discussion).

3.1.6 Empirical Specification

In our main analysis, we run the following OLS regression for individual i ,

$$Grade_i = \delta_0 + \sum_{j=1}^3 \delta_j * treat_{j,i} + \psi_0 * low\ income_i + \sum_{j=1}^3 \psi_j * treat_{j,i} * low\ income_i + \phi' \mathbf{X}_i + e_i. \quad (2)$$

In Equation 2, *Grade* is the 0-10 grade given by the subject to the government, *treat_j* refers to one of the three information shocks randomly assigned to respondents (the group that receives no information shock is therefore the omitted group), and *low income* measures how low the income level of the respondent is. We use four different measures of *low income* in our analysis: (i) log of monthly household income; (ii) log of monthly personal income; (iii) a dummy for household income below the median in the sample; (iv) a dummy for household income in the first quartile of the sample. We also present a figure with the treatment effects estimated by quartile of household income. To take into account respondents that reported to have zero income and those that did not report their income level, dummy variables were added for those two sets of respondents throughout, as well as their interactions with the treatment indicators. Finally, \mathbf{X}_i is a vector of individual controls. Standard errors are clustered at the surveyor level (results are unchanged when clustering by district of interview).

3.2 Main Results: Reactions to Information Shocks

Appendix Table A.3 presents summary statistics for our explanatory variables of interest across treatment groups, and suggests that the randomization across treatments was successful. Also, as expected, the mean level of household income of families with children attending public school is significantly lower (at 1%) than the mean level of household income of families with children attending private school (R\$ 1,436, as opposed to R\$ 5,376). In the first two quartiles of the household income distribution in our sample, 92% of families with a least one child attending school have at least one child attending public school; this share reduces to 77% in the third quartile, and 25% in the fourth quartile. These numbers indicate that lower income households are much more likely to directly benefit from public education improvements than higher income households are.

Figure 2 displays our main findings by plotting the estimated treatment effects across quartiles of the household income distribution of the *More education, less cash* information shock (informing the respondent that the local government increased the share of total public spending allocated to public education, but decreased the share allocated to expenditures associated with cash transfers). We observe that the effects start significantly negative for quartile 1 and monotonically increase until displaying a significantly positive magnitude on quartile 4. For example, for individuals with household incomes above the median of the sample, the information shock increases on average the grade given to the government from 5.81 to 6.34, when compared to the subjects above the median in the control group (p-value=0.051). For those with household incomes below the median, the same shock reduces on average their grades from 5.62 to 4.97 relative to the subjects below the median in the control group (p-value=0.06).²⁶

Figures 3 and 4 display respectively the treatment effects from the *More education* and the *More cash* information shocks. Figure 3 indicates that respondents in the first three quartiles on average react very slightly to the information shock in terms of their assessment of the government (and not significantly so), while those in the fourth quartile react positively (and significantly so) to it. Without associating the increase in public education spending with a decrease in cash transfer spending, respondents might not be thinking about a tradeoff in public spending. Finally, Figure 4 indicates that the treatment effects from information shocks about past increases in spending associated with cash transfers are positive for all quartiles, but decline as the household income level goes up. We can therefore be reassured that the previous results were not driven by a general negative reaction of low-income individuals to information shocks about public spending in general.²⁷

²⁶We can also use individuals' answers to the question on what is more important for the state government to achieve (improving public education or increasing cash transfers) to check whether individuals reporting that increasing cash transfers is more important are more likely to react negatively to the *More education, less cash* information shock, as we should expect. Indeed, the average grade given to the local government by respondents in the *More education, less cash* treatment group is significantly (at the 10% level) lower among those that indicate increasing cash transfers to be more important than improving public education (their average grade is 5.10) than among those who indicate improving public education to be more important (5.81). Also, the average grade among those who indicate improving public education to be more important goes up when we compare respondents in this treatment group with those in the control group (from 5.74 to 5.81), while it goes down for those reporting increasing cash transfers to be more important (from 6.2 to 5.10, significant at the 5% level).

²⁷Appendix Figures A.2-A.4 replicate Figures 2-4, without conditioning on controls, and display similar patterns of treatment effects.

Table 4 presents the regression results from the different information shocks and their interactions with different measures of income. The results corroborate the visual evidence: information about more education spending and less cash transfer spending lowers the rating of the government by low-income respondents and raise the rating by those with higher incomes. In Appendix Table A.5 we replicate Table 4 without conditioning on controls. In Appendix C we analyze the robustness of our findings, and present results on heterogeneity of treatment effects that can help understand our findings and rule out alternative stories. In particular, we provide evidence that: (i) our heterogeneity of treatment effects according to household income levels is robust to including interactions of each treatment dummy with the level of schooling of the respondent; and (ii) our results are not driven by a perception by the poor that there is relatively more corruption in public education spending.

4 Auxiliary Experiment: Revealed Preference

4.1 Experimental Design

We designed and implemented an auxiliary experiment to deal with two remaining problems from the main intervention: first, since income differences are not randomly assigned, people with different incomes may differ in many unobservable dimensions; second, the poor may not think that public education spending would directly benefit them. In this second experiment, we exogenously vary income and offer an education opportunity that would directly benefit the recipients.²⁸

The experiment was also conducted in the DF state, on the first week of November 2012. The participants in the experiment were eighty randomly chosen parents who had one child enrolled in either the fourth or fifth grade in a large public school of the district of *Varjão*, in the *Distrito Federal* state, in Brazil.²⁹ Parents were recruited with letters distributed to the child inviting one parent to come to the school at the end of any day of the week the experiment was conducted.

²⁸Furthermore, as we discuss next, the second intervention involves actual, incentivized choices, as opposed to stated preferences, as in the survey experiment.

²⁹We partnered with local NGO *Agência de Notícias dos Direitos da Infância* (ANDI) [News Agency for Children's Rights] to conduct the study. The local government agreed to let us conduct the experiment with eighty parents from the studied school.

Parents were offered R\$5 to attend the study; the show-up rate was 83%.

One surveyor was assigned to each participant to read the survey questions in a school room.³⁰ All questions asked by participants were answered by surveyors. Surveyors were randomly ordered at the beginning of the day and assigned according to availability throughout the rest of the day. No communication across subjects was allowed during the entire experiment. For each subject, total participation took on average around 15-20 minutes.

Parents were randomly assigned to one of two treatments, according to a random number generator. In any treatment, the experiment began with the surveyor offering the parent the opportunity to receive different types of benefits. Parents were first offered an initial monthly payment for November and December 2012 (which they received with probability 1 or 0.25, as discussed below). The amount of this first payment was randomly varied across treatment groups: for the *Low income* treatment it was R\$10 per month, and for the *High income* treatment it was R\$210 per month. All cash transfers offered in the experiment were completely unconditional. The experiment followed with subjects being offered a second benefit, when they were asked to make several choices, as described next.

At the second benefit stage, subjects were asked seventeen questions, each one a choice between: (i) R\$10 + R\$X to be added to their initial monthly payment and (ii) R\$10 to be added to their monthly payment *and* free, individual, weekly, three-hour long, after-school Math and Portuguese tutoring sessions for their child for November and December 2012.³¹ The amount R\$X started at zero and was increased by R\$5 increments question after question, as presented below:

³⁰Surveyors were all undergraduate students from the University of Brasília. See the Supplemental Appendix for a picture from the implementation.

³¹The tutoring sessions would take place at the school and would stop on the week of Monday, December 17, since classes end on December 20. There is no market for tutoring in the area we studied, and the service, outside of the experiment, did not exist. The cost in the project to get an undergraduate student to tutor was a wage of R\$10 per hour and an additional payment R\$5 per week for transportation, for a total of R\$140 per month for each student receiving tutoring. See Appendix D for a picture of a tutoring session.

Which Additional Benefit Would Your Prefer?		
R\$10 per month	OR	R\$10 per month and free tutoring sessions
R\$15 per month	OR	R\$10 per month and free tutoring sessions
⋮		
R\$90 per month	OR	R\$10 per month and free tutoring sessions

Each treatment used these same seventeen questions.³² We added another level of (cross-) randomization, regarding the stakes of each parent’s choices. Half of the parents were informed that 25% of participants in their group would have their first benefit implemented as well as one of their decisions from the seventeen questions, and that decision would be randomly chosen from the seventeen questions. For the other half, instead of 25%, the probability of implementation was 100%. By randomly varying the stakes, we were able to save money implementing the experiment, and we also verified that the results were not statistically different from what they would have been if the probability of implementation were higher. Our choice elicitation is a version of the Becker-DeGroot-Marschak (BDM) elicitation procedure, which incentivizes truthful reporting of willingness to pay. This design therefore allows for (i) the elicitation of the willingness to pay (WTP) for the free tutoring sessions for two months; (ii) how such WTP varies when the household level of income is randomly increased for two months.³³

Finally, it is worth noting that we decided to break the benefits into two parts to make sure the seventeen questions eliciting the WTP for tutoring were identical to participants across treatment conditions. By doing this, we were able to reduce concerns relating to visual reference points affecting the decisions; the two treatments would otherwise display different cash payment values. This in turn could have made the amount that individuals are willing to forgo for the free tutoring sessions seem relatively bigger or smaller depending on the treatment. Another important implication is that with identical questions, there are no differences in the stakes associated with each

³²See the translated version in English of the questionnaires in Appendix D.

³³The research assistants reported only one case of non-monotonic choices: one parent chose the tutoring option one question after she had first switched to cash payments. The surveyor re-read the parent’s choice in the previous question and the parent then corrected her previously non-monotonic choice keeping the preference for cash payment. Also, for only one parent the maximum offer was not enough to induce a switch to cash payments. The results are unchanged if we drop that parent from the sample.

one of the seventeen questions across the two treatments.

Figure 5 summarizes the experimental design.

Experimental outcomes

We focus on one main outcome variable: the parent’s *willingness to pay for tutoring* – equal to the largest amount that the parent is willing to forgo to get free tutoring for the child.³⁴

Empirical specification

To estimate the treatment effects on our outcome variable of interest, we first make mean comparisons across treatments without controls. Although the assignment to treatments was random, we also estimate treatment effects controlling for observables. To that end, we run the following (OLS) regression in our empirical analysis:

$$Y_i = \alpha + \beta I_{High\ income,i} + \gamma' \mathbf{X}_i + \epsilon_i,$$

where Y is the dependent variable, \mathbf{X} is a vector of controls (including a dummy on whether the parent faced a 100% chance of implementation of one of her choices, as opposed to 25%), and $I_{High\ income,i}$ is a dummy variable for whether the parent received the *High income* treatment. Therefore, β measures our treatment effect of interest – the impact of increased income on the willingness to pay for tutoring.

In our complete specification, we include the following additional covariates: log of household income, gender indicators (for the parent and for the child), age (parent and child), employed parent indicator, religion dummies, parent’s marital status dummies, schooling (parent and child), number of children in the household, dummy on whether the household has been receiving conditional cash transfers from the government, parent’s race dummies, number of days the child missed class in the last two months, number of grades the child has already failed, and surveyor dummies. We cluster the standard errors at the surveyor level.

³⁴Note that the willingness to pay could be up to R\$5 greater. We code the willingness to pay the same across all treatments and focus on across-treatment differences. For robustness, we recode the willingness to pay differently for the two treatments. First, we increase the willingness to pay by R\$5 in the *Low income* treatment group and leave it unchanged in the *High income* treatment group; this creates a lower bound on our effects. Second, we leave the willingness to pay unchanged in the *Low income* treatment group and increase it by R\$5 in the *High income* treatment group; thus creating an upper bound on our effects. The results (available upon request) are robust to recoding the willingness to pay variable.

Finally, we also display the treatment effects on the willingness to pay for tutoring by examining directly across treatments the cumulative distribution for that variable.

Caveats and limitations of the design

It is important to highlight some of the limitations of the design. First, the income transfer is only temporary, lasting for two months. Second, some participants might have perceived tutoring as the socially desirable choice, however, as long as social desirability does not interact with payment size, it should not be a source of concern. Third, if participants believed that the goal of the experimenter was to get them to choose tutoring, they could have felt the need to reciprocate towards the experiment, and especially so after receiving a larger money transfer. We believe this is likely not the main driver of our findings, since participants had no reason to believe that investing in education was desired by the experimenter, but it is a potential caveat. It is also important to emphasize that parents do not know the returns to the tutoring investment, so we cannot make any statement on whether they are making the “right” choices in any given treatment.

Another potential caveat relates to the fact that participants might make different inferences about the quality of the tutoring services provided, depending on the size of the initial transfer they receive. In particular, respondents might believe the services are of higher quality after receiving a larger transfer (e.g., by believing that the experimenter is “richer”). Although we find it unlikely that this will drive our findings (since both treatments involved the same setting and an implementation in partnership with the same well-known NGO, thus not leaving room for a substantially different inference about the wealth of the experimenter), we do not have data to fully rule out this potentially confounding mechanism.

Finally, another set of limitations relates to our ability to interpret our treatment effects as “income effects.” A confounding mechanism relates to the idea of mental accounting (Tversky and Kahneman, 1981). Indeed, the same income increase is more salient to someone who is departing from a smaller cash transfer than it is for someone who is departing from a larger cash transfer. However, as we note in our introduction, salience effects are one of many plausible mechanisms that could explain why poorer individuals might prefer cash transfers to public education investments. Therefore, even if we may not interpret our findings as income effects, they would still be consistent

with our main hypothesis.³⁵

4.2 Experimental Results

In Appendix Table A.9 we present summary statistics for the observables used in our regressions in the two treatment groups of interest, indicating that the randomization was successful. Figure 6 shows the cumulative distributions of the willingness to pay for tutoring in the two treatment groups of interest, and illustrates how the exogenous shift in household income makes parents tilt more toward education in the education versus cash choice they face. In particular, the median of the aforementioned willingness to pay goes from \$15 to R\$40 when household income is exogenously shifted by R\$200 (a Mann-Whitney rank-sum test yields a p-value of 0.0027). The average WTP for tutoring goes from R\$23.5 to R\$39.4, once income is exogenously increased from the *Low income* to the *High income* treatment groups (the p-value of the difference is 0.003).³⁶

For any given level of cash transfer offered in the set of choices between cash and free tutoring, the proportion of parents choosing the cash option over education is strictly reduced when income is randomly increased. Moreover, the reduction in that proportion of parents is generally statistically significant (with the exception of the amounts of cash offered that are too low (high) such that very few (almost all) parents in both treatment group choose the cash option). As an example, take the choice between (i) an extra R\$30 per month, and (ii) an extra R\$10 plus free tutoring: the proportion of parents preferring cash to education goes down from 51.3% to 22% when income is randomly increased (the p-value of the difference is 0.0059). This means that if a government were to choose how to allocate resources between these two choices using majority voting, the treatment group with lower income would have chosen the pure cash option while the group with increased income would have preferred the option with less cash and more educational investment. Although

³⁵Furthermore, potentially viewing the experimental results as salience effects can help interpret the large effect sizes, given the small size of the transfers in terms of present discounted lifetime wealth.

³⁶In Appendix Table A.10, we present the treatment effects on the parent's willingness to pay for tutoring, running OLS regressions, including covariates. In Appendix Figure A.5, we also show that our results are robust to running permutation tests with 10,000 repetitions for the comparison of the raw WTP's across the two treatment groups of interest, as an alternative to standard t-tests. As mentioned above, we also randomized the stakes of the experiment: either 25% or 100% probability of implementation. In Appendix Table A.10, we can see that the dummy on whether the stakes were higher has no effect on the willingness to pay for tutoring. In results available upon request, we also observe that our main treatment of interest only has a very small and non-significant differential effect according to the stakes of the experiment, and is significant in both low and high stakes conditions.

we cannot rule out the presence of other channels in the other parts of the paper, the revealed preference experiment suggests that lack of income can play an important role when individuals face decisions involving tradeoffs similar to the ones faced outside of the experimental setting.

5 Conclusion

Diverging from models of elite capture, our findings suggest that public education spending could be low not because the rich oppose it, but because the poor prefer that the government allocates resources elsewhere. In particular, our results are consistent with low-income voters favoring instead redistributive instruments that yield immediate gains in consumption, such as cash transfers. Our findings suggest a new channel of long-term persistence of poverty and inequality, driven not by elites preventing educational investments but rather by the poor not voting for these investments.

Our results do not imply that the poor value education less than the rich do. Our findings instead indicate that (often times constrained) low-income individuals may have more urgent needs and might not be able to afford having fewer resources for present consumption in order to have more education for their children in the future in exchange. Whether our experimental results are driven by credit constraint, income or salience effects is still an open question, and an important avenue for future research, with important policy implications.

Our findings indicate that there could be a low-education trap in democratic countries in which the median voter is relatively poor.³⁷ If one's goal is to stimulate public education spending in such countries, "tying the hands" of governments or establishing long-run education targets might be solutions to overcome electoral incentives against that type of spending. A related point comes from the observation that many developing countries have recently adopted cash transfer programs.³⁸ Our results suggest that governments in those countries might have few electoral incentives in the future to move away from cash transfers and toward other types of spending such as public

³⁷Conversely, our results might help explain historical episodes of fast growth with substantial investment in public education in poorer, non-democratic countries, such as contemporary China for instance.

³⁸As of September 2012, the largest federal cash transfer program in Brazil, *Bolsa Família*, had over 13 million beneficiary families, and provided an average monthly payment per household of about R\$150 (or about 75 US dollars). Source: Brazilian Ministry of Social Development and Fight against Hunger: <http://www.brasil.gov.br/noticias/arquivos/2012/09/28/programa-de-transferencia-de-renda-paga-r-2-bilhoes-no-mes-de-setembro>.

education, since poor decisive voters will generally display high marginal utility of consumption. Indeed, as analyzed in recent work (Manacorda et al., 2011) cash transfers do indeed generate political support. Tying cash transfers to school attendance (like many programs in practice do) can potentially flip the substitutability between transfers and public education and turn them into complements. This might help induce increases in school attendance while still promoting income growth for the poor. As a result, politicians interested in improving schooling outcomes in developing countries, while still having support from poor voters, could potentially use conditional cash transfers program to achieve both goals. Indeed, the anecdotal evidence in Brazil suggests that municipal penetration of the federal conditional cash transfer program strongly correlates with support for the incumbent party in national elections. However, conditional cash transfers might not be enough to generate support for public investments seeking to improve public education *quality*.

We end with directions of future research. Given our research goals, we focus here on the tradeoff between public education and cash transfer spending. An interesting direction would be to extend the analysis to other tradeoffs in public expenditures, in order to understand better voters' preferences over different types of public spending.

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Figures and Tables

Figure 1: **Survey Experiment – Fraction of respondents by quartile of the household income distribution indicating improving public education as more important government goal than increasing cash transfers**

With 95% confidence interval.

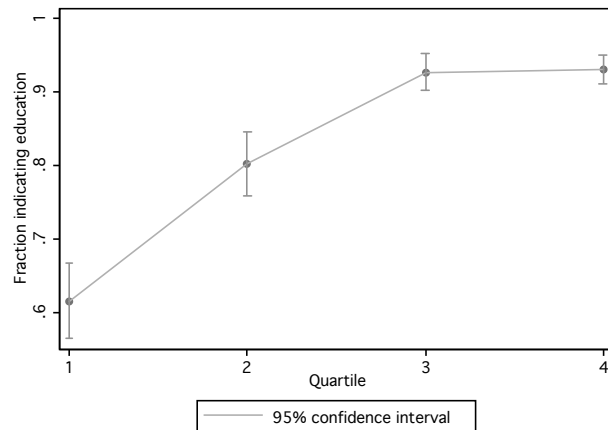
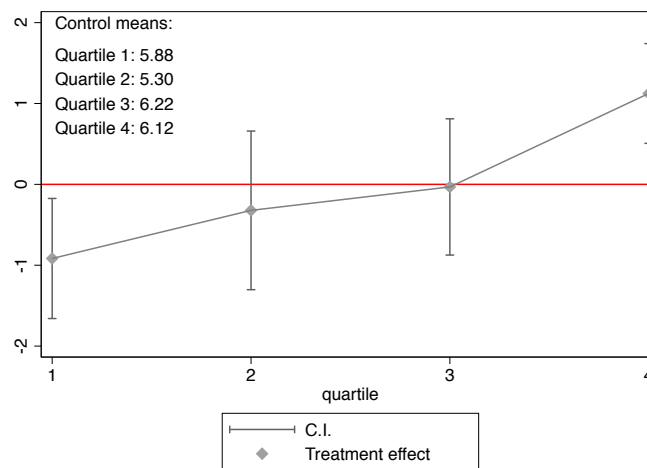


Figure 2: **Survey Experiment – Treatment effects of the *More education, less cash* information shock on rating (0-10 grade) of the local government, by quartiles of the household income distribution**

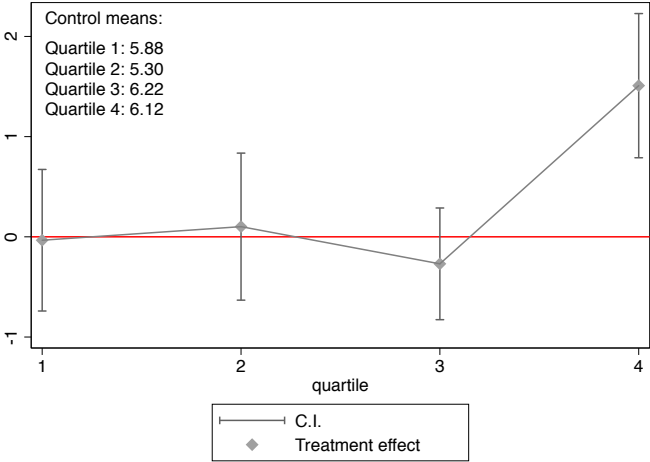
With 95% confidence interval. Treatment effects estimated using specification from Table 4 and means from control group by quartile.



Notes: The wording for the information shock was: *Did you know that, compared to 2006, the state government increased in 2007 the share of total public expenditures allocated to public education by 9% but reduced the share allocated to social assistance by 9%?*

Figure 3: **Survey Experiment – Treatment effects of the *More education* information shock on rating (0-10 grade) of the local government, by quartiles of the household income distribution**

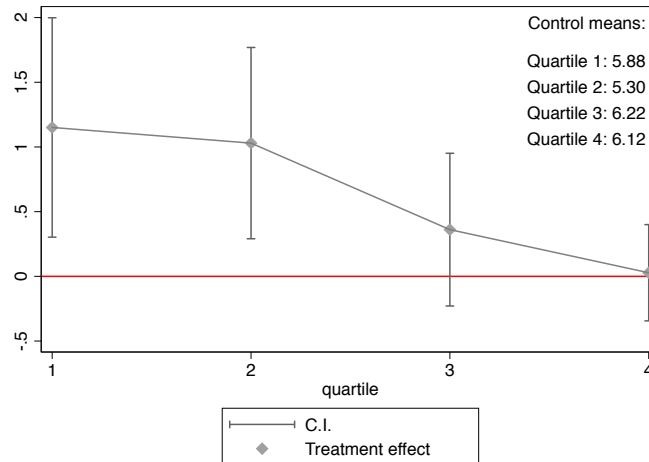
With 95% confidence interval. Treatment effects estimated using specification from Table 4 and means from control group by quartile.



Notes: The wording for the information shock was: *Did you know that, compared to 2006, the state government increased in 2007 the share of total public expenditures allocated to public education by 9%?*

Figure 4: Main Survey Experiment – Treatment effects of the *More cash* information shock on rating (0-10 grade) of the local government, by quartiles of the household income distribution

With 95% confidence interval. Treatment effects estimated using specification from Table 4 and means from control group by quartile.



Notes: The wording for the information shock was: *Did you know that compared to the first year in the previous state government, the current state government increased in its first year the share of total public expenditures allocated to social assistance from 1.3% to 3.1%?*

Figure 5: Auxiliary (Revealed Preference) Experiment - Experimental design

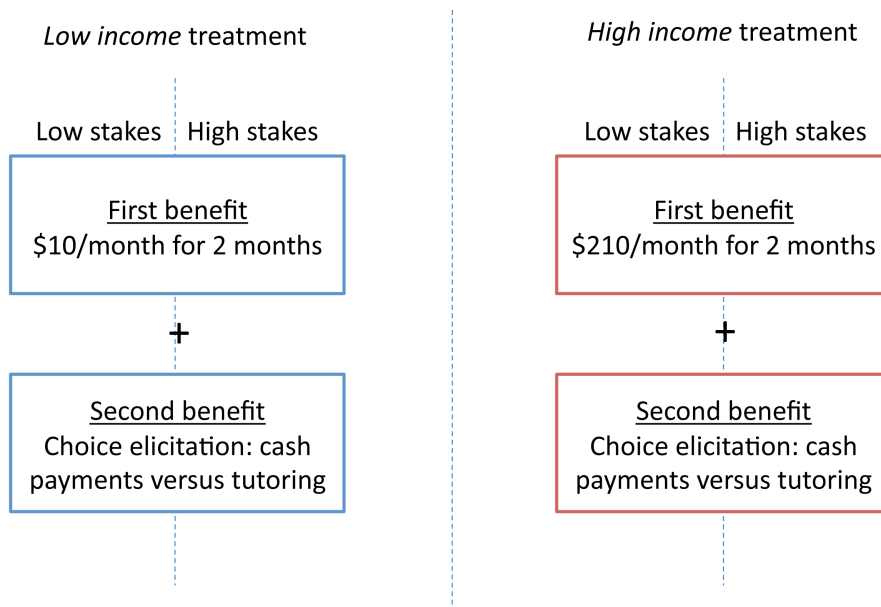
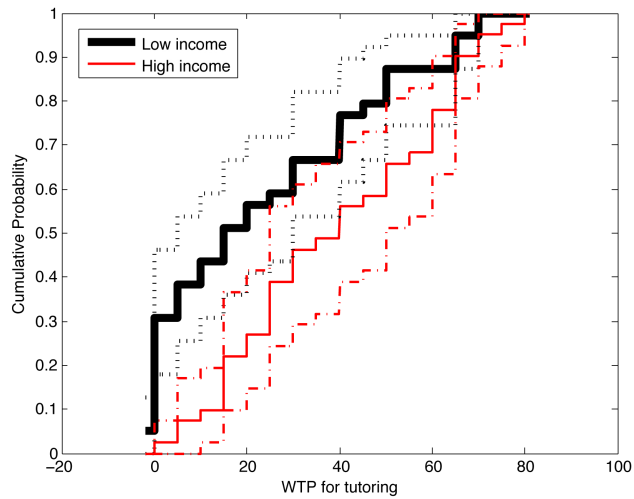


Figure 6: **Cumulative probability for the WTP for tutoring - *Low income* and *High income* treatment groups** (with 95% bootstrap confidence intervals - 1,000 bootstrap samples)



Notes: We re-sampled with replacement from the empirical distribution 1,000 times. From these 1,000 bootstrap samples, we computed the confidence intervals for each point on the cumulative distribution.

Table 1: Mulligan, Gil, and Sala-i-Martin (2004) revisited

	Dependent variable: public education spending as % of GDP (1980-1990)			
	[1]	[2]	[3]	[4]
	Total public education spending			Excluding tertiary education
Democracy index, 1960-1990	0.424	-8.673**	-7.164***	-4.521*
	[0.525]	[3.655]	[2.282]	[2.441]
Communist dummy	1.085**	1.252***	1.261**	2.142*
	[0.452]	[0.446]	[0.606]	[1.188]
British legal origin	0.527*	0.650**	0.612*	1.109***
	[0.305]	[0.301]	[0.342]	[0.390]
Percentage of population aged 65+, 1960-1990	0.059	-0.023	-0.151*	-0.153
	[0.060]	[0.067]	[0.087]	[0.114]
Log(population)/10, 1960-1990	-2.276***	-2.035**	-3.035**	-2.790**
	[0.861]	[0.845]	[1.413]	[1.343]
Real GDP per capita, 1960-1989 average, log	-0.056	-0.264		
	[0.293]	[0.298]		
Share of value added from agriculture, 1960-1990	-3.383**	-3.733**	-2.429	-2.340
	[1.603]	[1.569]	[1.516]	[1.925]
Democracy index×Real GDP per capita (average, log)		1.126**		
		[0.448]		
Log median income			0.267	0.417
			[0.318]	[0.334]
Democracy index×Log median income			0.895***	0.545*
			[0.292]	[0.324]
Observations	110	110	64	62
R-squared	0.30	0.341	0.477	0.423

Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. Log median income approximated using most recent measurements from UNU-WIDER/ World Income Inequality (WDI) Database. We compute the approximation of median income by calculating the level of income per capita in the third quintile of the distribution of income using the last numbers available for each country between 1980 and 1990. The dependent variable in column 4 is constructed combining the UNESCO and WDI databases.

Table 2: **Public education spending and probability of reelection in a panel of Brazilian municipalities** - OLS regressions

	Dependent variable: incumbent party reelected (dummy)		
	[1]	[2]	[3]
Log of municipal budget during term	0.002 [0.094]	0.032 [0.093]	0.025 [0.094]
Log of municipal education spending during term ($\ln(Ed)$)	0.048 [0.070]	-1.103*** [0.211]	
$\ln(Ed) \times$ Log of municipality monthly median income in 2000		0.265*** [0.047]	
$\ln(Ed) \times$ Municipality median income in first quintile			-0.171** [0.083]
$\ln(Ed) \times$ Municipality median income in second quintile			-0.006 [0.083]
$\ln(Ed) \times$ Municipality median income in third quintile			0.141 [0.088]
$\ln(Ed) \times$ Municipality median income in fourth quintile			0.256*** [0.096]
$\ln(Ed) \times$ Municipality median income in fifth quintile			0.200** [0.097]
Observations	9,153	9,153	9,153
R-squared	0.690	0.693	0.692
Mean of dep. variable		0.294	

Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. We use a panel of Brazilian municipalities for the last elections with available data (2004 and 2008). The variable “log of municipal budget during term” is the log of the yearly average budget for a municipality during the incumbent’s term. The variable $\ln(Ed)$ is the log of the yearly average of the level of public (primary) education spending in a municipality during the incumbent’s term. All values are in year 2000 prices. We include municipality fixed effects to control for unobservable municipal characteristics that do not vary through time, and a dummy for the second period to control for a general time trend. Since municipality-level, socio-demographic controls were only measured once during the period under consideration, we do not include them. We include variables representing mayors’ personal characteristics (age, age squared, male dummy, schooling dummies, married dummy) as well as party characteristics (party dummies and a dummy on whether the party was in power in the previous term).

Table 3: Public education quality and probability of reelection in a panel of Brazilian municipalities - OLS regressions

	Dependent variable: incumbent party reelected (dummy)			
	[1]	[2]	[3]	[4]
Log of municipal budget during term	0.097 [0.068]	0.091 [0.077]	0.082 [0.067]	0.084 [0.077]
Education variable×Municipality median income in first quintile	-0.034 [0.027]	-0.068** [0.028]	-0.006*** [0.002]	-0.004* [0.002]
Education variable×Municipality median income in second quintile	-0.009 [0.026]	-0.021 [0.024]	-0.003* [0.002]	-0.003 [0.002]
Education variable×Municipality median income in third quintile	0.041* [0.025]	0.031 [0.023]	0.001 [0.002]	0.004** [0.002]
Education variable×Municipality median income in fourth quintile	0.096*** [0.026]	0.067*** [0.024]	0.004 [0.003]	0.006** [0.002]
Education variable×Municipality median income in fifth quintile	0.109*** [0.027]	0.042 [0.028]	0.003 [0.004]	0.007** [0.003]
Interacted education variable:				
Public Education Quality			Public Education Quality	Public Education Quality
Index (IDEB)			Index (IDEB)	Index (IDEB)
Grades 1-4			Grades 1-4	Grades 1-4
Grades 5-9			Grades 5-9	Grades 5-9
Passing Rate (%)			Passing Rate (%)	Passing Rate (%)
Observations	8,824	8,998	8,909	9,064
R-squared	0.698	0.690	0.697	0.690

Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. We use a panel of Brazilian municipalities for the last elections with available data (2004 and 2008). The variable “log of municipal budget during term” is the log of the yearly average budget for a municipality during the incumbent’s term. In columns 1 and 2, we standardize the Brazilian index of basic (public) education quality (*IDEB*), so the coefficients of interest measure the effect of a standard-deviation increase in the index at each quintile of municipality median income. Column 1 examines the index for grades 1-4 and column 2 examines it for grades 5-9. In columns 3 and 4, we instead look at the grade passing rate (in %) for grades 1-4 (column 3) and grades 5-9 (column 4). In all columns we include municipality fixed effects to control for unobservable municipal characteristics that do not vary through time, and a dummy for the second period to control for a general time trend. Since municipality-level, socio-demographic controls were only measured once during the period under consideration, we do not include them. We include variables representing mayors’ personal characteristics (age, age squared, male dummy, schooling dummies, married dummy) as well as party characteristics (party dummies and a dummy on whether the party was in power in the previous term).

Table 4: Information shocks and rating of the local government (0-10 grade)
(OLS regressions)

Dependent variable: <i>0-10 grade given to state government</i>	Used measure of income:			
	Log of household income [1]	Log of personal income [2]	Dummy: quartile 1 of household income [3]	Dummy: household income below median [4]
More education treatment dummy	-4.027** [1.524]	-3.901** [1.726]	0.426* [0.222]	0.565** [0.221]
More education, less cash treatment dummy	-5.487*** [1.180]	-7.372*** [1.610]	0.230 [0.263]	0.527* [0.243]
More cash treatment dummy	3.558** [1.428]	4.777** [1.643]	0.444** [0.195]	0.207 [0.210]
Log household income	0.202 [0.168]			
Log personal income		-0.118 [0.209]		
Quartile 1 of household income			-0.019 [0.376]	
Household income below median				-0.535** [0.223]
More education×measure of income	0.583** [0.202]	0.586** [0.240]	-0.477 [0.386]	-0.560** [0.237]
More education, less cash×measure of income	0.733*** [0.142]	1.011*** [0.217]	-1.139** [0.432]	-1.176*** [0.306]
More cash×measure of income	-0.403** [0.176]	-0.582** [0.224]	0.713 [0.425]	0.843** [0.372]
Observations	1,875	1,875	1,875	1,875
R-squared	0.109	0.110	0.087	0.101
Mean of dependent variable in control group			5.734	

Robust standard errors (clustered by surveyor) in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%

Additional controls: years of schooling, male indicator, age, age squared, marital status dummies, number of children, child in public school dummy, beneficiary of local cash transfer program, more diversion in public education spending dummy, district and surveyor fixed effects dummy equal to one if respondent believes cash transfers are not part of social assistance spending (all columns); zero household income and missing household income dummies, and their interactions with treatment dummies (column 1); zero personal income and missing personal income dummies, and their interactions with treatment dummies (column 2); missing household income dummy and its interaction with treatment dummies (columns 3 and 4).

Appendix

(For Online Publication)

A Brazilian municipalities analysis

Variables used in the regressions

- **Dependent variable** (drawn from the Brazilian Superior Electoral Court database, *TSE*)
 - *Incumbent party reelected* (dummy)
- **Municipality public finance data** (drawn from Ministry of the Economy - *Ministerio da Fazenda/STN* database)
 - *Log of municipality budget*: log of the yearly average level of municipality budget during each term (each yearly variable is in year 2000 prices).
 - *Log of municipal public education spending* : log of the yearly average level of municipal education spending between during each term (each yearly variable is in year 2000 prices).
- **City-level census data** (drawn from 2000 Brazilian census - *IBGE* database)
 - *Log of median income* (year 2000 prices).
- **Characteristics of the incumbent party and incumbent mayor** (drawn from the Brazilian Superior Electoral Court database, *TSE*)
 - *Dummy on whether the current incumbent party was reelected in the previous elections.*
 - *Party dummies.*
 - *Mayors' characteristics: age, schooling level, married dummy, gender.*

B Main Experiment: Information Shocks and Update of Priors about the Government’s Priorities

An important assumption in our analysis is that information shocks reporting increases (decreases) in one type of spending update upwards (downwards) individuals’ original perceptions (or priors) about the changes that occurred in that type of spending during the period under consideration. More than that, we would want the shocks to affect individuals’ perceptions of the priorities of the state government administration throughout its term as a whole. Specifically, we would want individuals exposed to the *More education, less cash* information shock to associate more the state government administration under consideration with improvements in public education and less with increases in cash transfers, when compared to individuals not exposed to any information shock.

To specifically address these issues in detail, we conducted in 2011 a follow-up survey with 500 subjects.³⁹ Those subjects were not a subset of the sample originally interviewed in the main survey. However, both samples were drawn from the same overall population using the same sampling strategy. Yet, one might worry about whether they are in fact comparable. Reassuringly, our evidence suggests that the samples are indeed very similar. The first two columns of Appendix Table A.2 compare the means of the variables measured in the original and follow-up surveys. Only one variable (age) has significantly different means in the two samples. The similarity of the two samples is also true for variables that relate to respondents’ perceptions of the functioning of the state government during the 2007-2010 term (such as respondents’ perceptions of whether there is more diversion of funds in education spending than in cash transfer spending). To further address the extent to which the samples are comparable, we reproduce the two tables (Appendix Tables A.4 and A.7) for which the dependent variables are present in both surveys, now adding a dummy on whether the respondent was interviewed in the follow-up survey. In both tables, the coefficient on that dummy is always statistically and economically insignificant (results available upon request).

The follow-up survey had two randomized treatment groups: a “control group” (again not exposed to any information shock on changes in public spending) and an information shocks group (exposed sequentially to the three pieces of information composing the three information shocks from the original survey).⁴⁰ They were first exposed to the *More education* information shock, then to the *Less cash* part of the *More education, less cash* information shock, and finally to the *More cash* information shock.⁴¹

The survey asked all subjects the following question: *Which one did you think was a bigger priority of the state government throughout the 2007-2010 term? Improving public education or increasing cash transfers?* In the control group, without any information shocks, 31.7% of subjects indicated improving public education. In the information shock group, after being exposed to the two pieces of information forming the *More education, less cash* information shock (and before being exposed to the *More cash* information shock), 95.2% of subjects reported public education

³⁹For a copy of one of the versions of the questionnaire used in the follow-up survey and its English version, see the Supplemental Appendix.

⁴⁰The last two columns of Appendix Table A.2 compare the means of the variables in the follow-up survey separately for the control (no information shocks) and treatment groups. With no exceptions, the means are not significantly different in the two groups. This indicates that the randomization was successful.

⁴¹All subjects exposed to the shocks reported that they did not previously know the information given to them. For 95.2% of subjects, cash transfers spending was reported to be part of the *Social assistance* category, whereas for only 0.4% of them cash transfers was indicated to be part of the *Public education* category.

improvements to be a bigger priority of the government administration throughout its term. This means that the pieces of information contained together in the *More education, less cash* information shock in the original survey make respondents associate the government more with improving public education and less with increasing cash transfers.

Since we are interested in measuring how information shocks change respondents' rating of the local government by quartile of income, it is important to also establish how people's priors about the government's priorities are shifted by the shocks at different levels of income. It is reassuring to know that respondents from different income quartiles had similar priors about the priorities, and that the update of priors is also very similar across quartiles. The share of respondents whose priors (i.e., based on the answers from the group that received no information shock) were that the bigger priority was improving public education is 32% in quartile 1, 18% in quartile 2, 34% in quartile 3, and 30% in quartile 4. The increase in these shares after receiving the information shock is of 65 p.p. in quartile 1, 73 p.p. in quartile 2, 61 p.p. in quartile 3, and 64 p.p. in quartile 4. It is also important to note that we cannot reject tests of equality of those numbers for all pairwise comparisons across quartiles, both in the control group (i.e., for the beliefs without the information shocks), and for the treatment effects on priors' updates.⁴²

C Main Experiment: Additional Analyses

We now present extensions of the main analysis of the results of experiment #1.

C.1 The effect of income and the effect of schooling

In our survey, we do not explicitly elicit individuals' beliefs about returns to schooling, but we can have a sense of the importance of individuals' schooling separately from that of income in explaining our findings, by adding interaction terms between the level of schooling of the respondents and each of the treatment indicators. The results are reported in Appendix Table A.6. Our main findings are maintained when we add the additional terms, and the level of schooling of the respondents does not seem to significantly affect the treatment effects. This suggests that household income might be more important than personal education in explaining our findings.⁴³

⁴²Respondents in the information shock group were also asked to indicate, after receiving the first two information shocks, which state government administration they believe was responsible for the changes in spending described to them. They were asked to choose between the administration in office between 2003 and 2006 and the one in office between 2007 and 2010. The latter is the one that subjects were asked to rate in the main survey after receiving the shocks. For 97.1% of subjects, the administration in office between 2007 and 2010 was responsible for the changes in spending contained in the information shocks.

⁴³One might also be concerned that the same information shock might be perceived differently by people with different levels of cognitive ability, and that cognitive ability is correlated with income. In particular, low-income and high-income individuals might perceive differently the relative importance of 9% when assessing the meaning of a "9 %-increase" in a type of spending. However, this would imply that the treatment effects would be either amplified or reduced for poor respondents, but not *reversed*. A related concern regards whether respondents with different levels of cognitive abilities make different inferences about the competence of the local government in spending resources efficiently, after they receive an information shock revealing a tradeoff in public spending. To address these issues, the regressions in Appendix Table A.6 are useful, since schooling could be seen as proxy for cognitive ability. Finally, in Appendix Table A.7, which is discussed in the next subsection, we observe that receiving the *More education, less cash* information shock does not affect respondents' perceptions of the relative levels of diversion of funds in public education *versus* cash transfer spending. Moreover, the effects of the information shock on such perceptions do not seem to vary according to the level of income of the respondents; if we add interaction terms between the information

C.2 Additional interaction effects

To further assess the validity of our findings, we present additional linear interaction effects in Appendix Table A.8.

- Perception of amount actually spent on public education versus cash transfers for every R\$ 100 allocated to each category of expenditure

A source of concern in interpreting our results could be if low-income subjects believed that there is more diversion of funds in public education than in cash transfers spending by the local government. In this case, our findings would not be capturing differences in preferences between the two types of spending but instead differences in the perception that one type is more likely to yield gains than the other. Our second experiment shuts down this concern by design, since tutoring the perceived probability that tutoring will provided does not vary with the size of the cash transfer. We can also use the data from the first experiment to suggest that perception of actual spending is not a likely source of concern. We first show, in Appendix Table A.7, that poorer people are not more likely to believe that there is more diversion of funds in public education than in cash transfer spending. If anything, poorer people are less likely to believe so.

In addition to that, in Appendix Table A.8, we interact our treatment dummies with an indicator on whether the respondent believes that there is more diversion of funds in public education than in cash transfers spending. As shown in that table, the negative effect (in terms of assessment of the local government) of the *More education, less cash* information shock is stronger for those who think that there is more diversion of funds in public education spending. However, even for those who think that there is more diversion of funds in cash transfers spending, the information shock would have a positive effect for high-income subjects and a negative effect for low-income individuals.

- Children attending public school

An additional source of concern would arise if low-income subjects were not the beneficiaries of public education spending. That does not seem to be the case: the mean level of household income of families with children attending public school is R\$ 1,436, significantly lower (at 1%) than the mean level of household income of families with children attending private school, R\$ 5,376.

As an additional check, we also interact our treatment dummies with an indicator on whether the respondent has children attending public school. We would expect subjects who have children in public school to react more positively to that information shock, because they are more likely to benefit directly from increased public education spending. As shown in Appendix Table A.8, the effect of the *More education, less cash* treatment is not significantly different for those with children attending public school, although the sign of the interaction coefficient is indeed positive.

- Beneficiaries of the local conditional cash transfer program

We also analyze how our treatment effects vary according to the whether or not the respondent is a beneficiary of the local conditional cash transfer (CCT) program (called *Bolsa Escola, Vida*

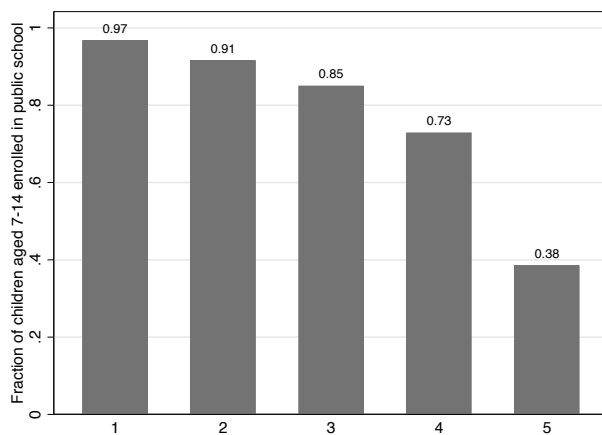
shock and different measures of income, the coefficients are highly insignificant, both statistically and economically (results available upon request).

Melhor).⁴⁴ In our sample, about 11% of the respondents reported to be current beneficiaries of the program. As shown in Appendix Table A.8, the negative effect (in terms of assessment of the local government) of the *More education, less cash* information shock is significantly stronger for beneficiaries of the CCT program. However, even if we restrict the attention to the non-beneficiaries of the program, the information would have a positive effect for high-income subjects and a negative and effect for low-income individuals. The results from this last set of interactions also suggest that on average respondents seem to associate increases in cash transfer spending as more spending for the current beneficiaries of cash transfer programs, rather than expanding the programs to new beneficiaries.

To further assess our argument, in the follow-up survey, we asked the respondents that reported to be beneficiaries of the local CCT program how they would spend the additional money if the local government increased by R\$ 20 (about US\$ 12) the amount they receive as transfers. Six options were given to them. The results are as follows: 17.1% reported that they would use the extra money to help pay their bills; 26.8% reported that they would use it to improve their family's standard of consumption/standard of living; 21.9% reported they would use it to pay debts; 19.5% reported that they would spend it to improve the quality of nutrition of their family; 4.9% reported that they would spend it to improve their children's education; and 9.8% reported they would spend it other, unspecified, ways.

Appendix Figures and Tables

Figure A.1: **Share of children aged 7 to 14 enrolled in public schools by quintile of the household per capita income distribution.** Source: 2011 Brazilian National Household Survey (PNAD)



⁴⁴In the program, families with per capita household incomes below a certain threshold receive cash transfers conditional upon school attendance of their children.

Figure A.2: **Survey Experiment – Treatment effects of the *More education, less cash* information shock by quartiles of the household income distribution**

With 95% confidence interval. Treatment effects estimated without controls (clustering by surveyor).

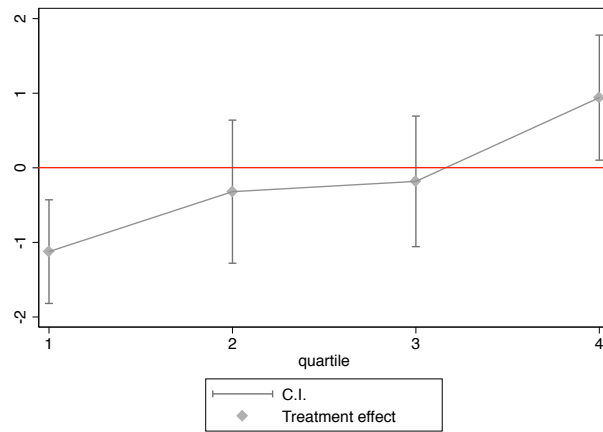


Figure A.3: **Survey Experiment – Treatment effects of the *More education* information shock by quartiles of the household income distribution**

With 95% confidence interval. Treatment effects estimated without controls (clustering by surveyor).

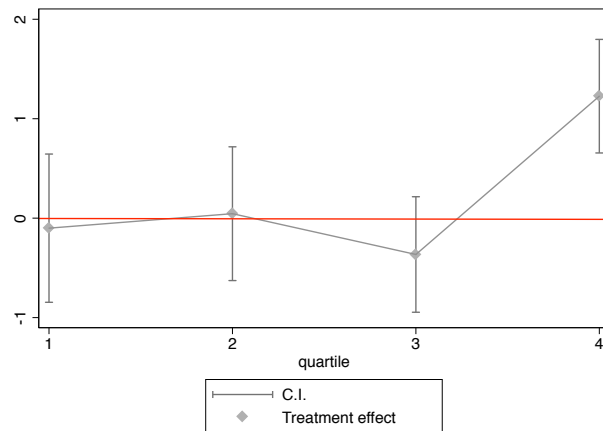


Figure A.4: **Survey Experiment – Treatment effects of the *More cash* information shock by quartiles of the household income distribution**

With 95% confidence interval. Treatment effects estimated without controls (clustering by surveyor).

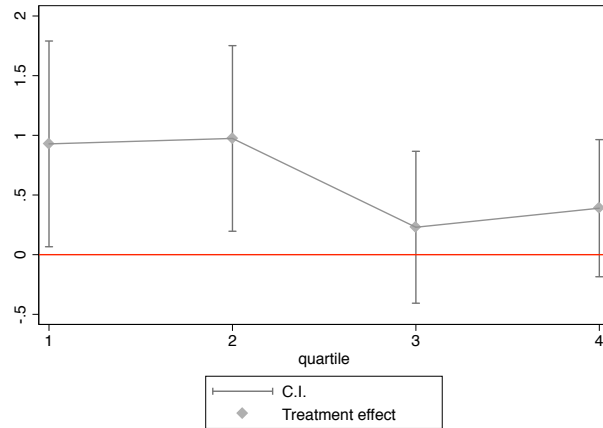
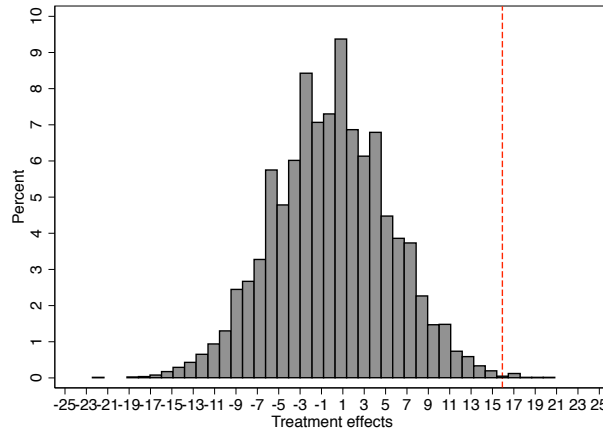


Figure A.5: **Permutation tests for *High income* treatment effects - 10,000 replications**



Notes: The dashed line corresponds to the treatment effects estimated in the paper. We randomly assign “treatment” status to parents in each group of interest, 10,000 times, and calculate a distribution of “treatment effects” based on the random assignment. We then compare the size of the treatment effects we find (using the actual treatment assignment) to the distribution of “treatment effects” when treatment is randomly assigned.

Table A.1: Summary statistics - Brazilian municipalities analysis

Panel A: 2004 elections			
Variable	Obs	Mean	Std. Dev.
<i>Municipal characteristics (all prices in year 2000 R\$)</i>			
Log of yearly average of total municipal spending during 2001-2004 term	4972	15.63	0.99
Log of yearly average of municipal education spending during 2001-2004 term	4732	14.43	0.99
Municipality median monthly income in 2000	5379	99.33	56.59
<i>Mayors' characteristics:</i>			
Male dummy	5431	0.94	0.23
Married dummy	5432	0.83	0.38
Age	5430	47.73	9.43
Schooling: Reads and writes	5432	0.02	0.14
Schooling: Incomplete primary	5432	0.13	0.34
Schooling: Complete primary	5432	0.10	0.30
Schooling: Incomplete secondary	5432	0.05	0.22
Schooling: Complete secondary	5432	0.23	0.42
Schooling: Incomplete tertiary	5432	0.07	0.25
Schooling: Complete tertiary	5432	0.38	0.49
Panel B: 2008 elections			
Variable	Obs	Mean	Std. Dev.
<i>Municipal characteristics (all prices in year 2000 R\$)</i>			
Log of yearly average of total municipal spending during 2005-2008 term	4559	15.97	1.01
Log of yearly average of municipal education spending during 2005-2008 term	4549	14.72	1.05
Municipality median monthly income in 2000 (year 2000 R\$)	5379	99.33	56.59
<i>Mayors' characteristics:</i>			
Male dummy	5394	0.93	0.26
Married dummy	5432	0.80	0.40
Age	5426	47.39	9.69
Schooling: Reads and writes	5395	0.02	0.13
Schooling: Incomplete primary	5395	0.12	0.32
Schooling: Complete primary	5395	0.07	0.26
Schooling: Incomplete secondary	5395	0.04	0.20
Schooling: Complete secondary	5395	0.26	0.44
Schooling: Incomplete tertiary	5395	0.07	0.25
Schooling: Complete tertiary	5395	0.41	0.49

See Appendix A for a description of all variables.

Table A.2: Means of observables in the original survey, the follow-up survey, and in the control and treatment groups of the follow-up survey

	Original survey	Follow-up survey	No-shock group	Info. shocks group
	N=2003	N=500	N=250	N=250
Male respondent	0.502 [0.500]	0.510 [0.500]	0.504 [0.501]	0.516 [0.501]
Age	37.026 [12.767]	34.745*** [13.739]	34.766 [13.777]	34.724 [13.729]
Years of schooling	9.476 [4.035]	9.78 [3.732]	9.712 [3.733]	9.848 [3.737]
Single	0.461 [0.499]	0.478 [0.500]	0.468 [0.500]	0.488 [0.501]
Married	0.426 [0.495]	0.438 [0.497]	0.440 [0.497]	0.436 [0.497]
Divorced	0.052 [0.223]	0.064 [0.245]	0.072 [0.259]	0.056 [0.230]
Personal income	1640.442 [2355.173]	1700.704 [2430.96]	1647.220 [2593.117]	1754.187 [2261.828]
Household income	2799.135 [3962.914]	2960.613 [3620.504]	2932.395 [3982.486]	2989.422 [3219.519]
Number of children	1.737 [1.736]	1.817 [1.502]	1.923 [1.532]	1.711 [1.466]
Has a child in public school?	0.352 [.478]	0.340 [0.474]	0.336 [0.473]	0.344 [.476]
Education priority over cash transfers	0.843 [0.364]	0.826 [0.380]	0.824 [.382]	0.827 [0.379]
More diversion in education spending	0.343 [0.475]	0.317 [0.466]	0.338 [.474]	0.297 [0.458]

We display the means across treatments of the covariates used in the main regressions. We perform t-tests of equality in means, comparing the means of each variable in the original and follow-up samples (columns 1 and 2) and in the control and treatment groups of the follow-up survey (columns 3 and 4). Standard deviations in brackets. *** 1% significant difference (for the mean in the follow-up survey when compared to the mean in the original survey). We converted personal and household income measures in the follow-up survey to August 2009 Reais using the Brazilian official inflation index (IPCA).

Table A.3: **Summary statistics and balance of observables – Survey experiment**

	Control	More education	More education, less cash	More cash	p-value of test (1)=(2)=(3)=(4)
	(1) N=501	(2) N=504	(3) N=497	(4) N=501	
Male respondent	0.492 [0.500]	0.501 [0.500]	0.519 [0.500]	0.495 [0.500]	0.83
Age	36.732 [12.471]	37.396 [13.000]	36.293 [12.556]	37.672 [13.023]	0.33
Years of schooling	9.506 [4.100]	9.347 [4.069]	9.430 [4.013]	9.618 [3.962]	0.74
Single	0.465 [0.499]	0.446 [0.446]	0.451 [0.451]	0.483 [0.483]	0.65
Married	0.397 [0.490]	0.452 [0.499]	0.457 [0.499]	0.399 [0.490]	0.09
Divorced	0.068 [0.252]	0.044 [0.205]	0.046 [0.211]	0.052 [0.222]	0.37
Personal income	1572.848 [2099.589]	1594.168 [2113.016]	1737.778 [2834.269]	1655.317 [2288.894]	0.76
Household income	2723.119 [2723.119]	2872.905 [2872.905]	2758.789 [2758.789]	2842.269 [2842.269]	0.94
Number of children	1.737 [1.699]	1.827 [1.827]	1.636 [1.636]	1.746 [1.746]	0.38
Child in public school?	0.375 [0.485]	0.359 [0.480]	0.340 [0.474]	0.333 [0.472]	0.5

We display the means across treatments of the covariates used in the main regressions. We perform F-tests of equality in means across all four groups. Standard deviations in brackets.

Table A.4: **What is more important for the government to achieve: improving public education or increasing cash transfers? (OLS regressions)**

Dependent variable - dummy variable: <i>improving public education is more important than increasing cash transfers</i>			
	[1]	[2]	[3]
Log household income	0.084*** [0.011]		
Log personal income		0.076*** [0.011]	
Quartile 2 of household income			0.163*** [0.031]
Quartile 3 of household income			0.261*** [0.036]
Quartile 4 of household income			0.254*** [0.033]
Years of schooling	0.005** [0.002]	0.006** [0.002]	0.004** [0.002]
Male indicator	0.019 [0.021]	0.011 [0.019]	0.007 [0.020]
Age	0.006* [0.003]	0.004 [0.003]	0.007** [0.003]
More education treatment dummy	0.000 [0.014]	0.000 [0.016]	-0.002 [0.012]
More education, less cash treatment dummy	0.018 [0.016]	0.010 [0.017]	0.022 [0.015]
More cash treatment dummy	0.026 [0.018]	0.022 [0.019]	0.023 [0.018]
Number of children	-0.008 [0.008]	-0.008 [0.007]	-0.006 [0.008]
Child in public school dummy	0.003 [0.021]	0.003 [0.021]	-0.013 [0.022]
Beneficiary of local CCT program dummy	-0.070 [0.042]	-0.082** [0.037]	-0.057 [0.042]
More diversion in education spending dummy	-0.058*** [0.016]	-0.057*** [0.015]	-0.051*** [0.016]
Observations	1632	1632	1632
R-squared	0.18	0.17	0.20
Mean of dependent variable (Last column refers to mean in quartile 1)	0.832	0.832	0.617

Robust standard errors (clustered by surveyor) in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%

Additional controls: marital status dummies, age squared, district and surveyor fixed effects (all columns); zero household income and missing household income dummies (column 1); zero personal income and missing personal income dummies (column 2); missing household income dummy (columns 3 and 4).

Table A.5: Information shocks and rating of the local government (0-10 grade)
(OLS regressions with no additional covariates)

Dependent variable: <i>0-10 grade given to state government</i>	Used measure of income:			
	Log of household income [1]	Log of personal income [2]	Dummy: quartile 1 of household income [3]	Dummy: household income below median [4]
More education treatment dummy	-4.927** [1.649]	-4.532** [1.832]	0.422* [0.232]	0.588** [0.242]
More education, less cash treatment dummy	-6.497*** [1.087]	-8.172 *** [1.783]	0.245 [0.242]	0.528** [0.226]
More cash treatment dummy	2.567* [1.307]	3.937** [1.749]	0.435* [0.219]	0.231 [0.242]
Log household income	0.179 [0.207]			
Log personal income		-0.012 [0.235]		
Quartile 1 of household income			-0.041 [0.423]	
Household income below median				-0.542* [0.305]
More education*measure of income	0.702*** [0.224]	0.674** [0.260]	-0.523 [0.469]	-0.653** [0.266]
More education, less cash*measure of income	0.864*** [0.128]	1.118*** [0.242]	-1.368*** [0.361]	-1.286*** [0.272]
More cash*measure of income	-0.275 [0.162]	-0.469 [0.241]	0.494 [0.439]	0.675 [0.401]
Observations	1,998	1,998	1,998	1,998
R-squared	0.066	0.052	0.030	0.049
Mean of dependent variable in control group			5.734	

Robust standard errors (clustered by surveyor) in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%
Additional controls: zero household income and missing household income dummies (column 1); zero personal income and missing personal income dummies (column 2); missing household income dummy (columns 3 and 4).

Table A.6: Information shocks and assessment of the local government, adding interaction terms between treatment dummies and respondents' schooling levels (OLS regressions)

Dependent variable: <i>0-10 grade given to state government</i>	Used measure of income:			
	Log of household income [1]	Log of personal income [2]	Dummy: quartile 1 of household income [3]	Dummy: household income below median [4]
More education treatment dummy	-4.398** [1.461]**	-4.141** [1.693]**	0.850 [0.786]	1.139 [0.729]
More education, less cash treatment dummy	-5.315*** [1.241]***	-7.296*** [1.638]***	-0.709 [0.951]	-0.340 [0.811]
More cash treatment dummy	3.604** [1.386]**	4.868*** [1.524]***	0.652 [0.667]	0.254 [0.690]
Log household income	0.120 [0.210]			
Log personal income		-0.117 [0.249]		
Quartile 1 of household income			-0.039 [0.459]	
Household income below median				-0.551* [0.281]*
More education×measure of income	0.756*** [0.214]***	0.7167** [0.259]**	-0.568 [0.474]	-0.698** [0.244]**
More education, less cash×measure of income	0.625*** [0.198]***	0.925*** [0.253]***	-0.872 [0.586]	-0.934** [0.350]**
More cash×measure of income	-0.422** [0.180]**	-0.620** [0.206]**	0.656 [0.461]	0.829* [0.410]*
Years of schooling	0.029 [0.055]	0.039 [0.055]	0.030 [0.055]	0.021 [0.054]
More education×Years of schooling	-0.099 [0.063]	-0.075 [0.063]	-0.044 [0.069]	-0.056 [0.064]
More education, less cash×Years of schooling	0.067 [0.071]	0.057 [0.065]	0.094 [0.075]	0.081 [0.065]
More cash×Years of schooling	0.010 [0.049]	0.018 [0.055]	-0.021 [0.051]	-0.004 [0.050]
Male indicator	-0.150 [0.157]	-0.159 [0.160]	-0.136 [0.154]	-0.176 [0.153]
Age	-0.053* [0.029]*	-0.041 [0.032]	-0.043 [0.030]	-0.050 [0.030]
Number of children	-0.004 [0.059]	-0.017 [0.059]	-0.005 [0.063]	-0.005 [0.059]
Child in public school dummy	0.263 [0.177]	0.221 [0.161]	0.192 [0.156]	0.231 [0.166]
Beneficiary of local CCT program dummy	0.605** [0.217]**	0.529 [0.196]**	0.486 [0.215]**	0.551 [0.220]**
More diversion in education spending dummy	0.172 [0.138]	0.197 [0.145]	0.221 [0.145]	0.195 [0.141]
Observations	1,875	1,875	1,875	1,875
R-squared	0.11	0.11	0.09	0.10
Mean of dependent variable in control group			5.734	

Robust standard errors (clustered by surveyor) in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Additional controls: marital status dummies, age squared, district and surveyor fixed effects, dummy equal to one if respondent believes cash transfers are not part of social assistance spending (all columns); zero household income and missing household income dummies, and their interactions with treatment dummies (column 1); zero personal income and missing personal income dummies, and their interactions with treatment dummies (column 2); missing household income dummy and its interaction with treatment dummies (columns 3 and 4).

Table A.7: **In which category of spending do you believe there is more diversion of funds: public education or cash transfers? (OLS regressions)**

Dependent variable - dummy variable: *there is more diversion of funds in public education than in cash transfer spending*

	[1]	[2]	[3]
Log household income	0.037*** [0.011]		
Log personal income		0.006 [0.007]	
Quartile 2 of household income			-0.010 [0.030]
Quartile 3 of household income			0.011 [0.035]
Quartile 4 of household income			0.092*** [0.022]
Years of schooling	-0.007*** [0.002]	-0.006** [0.002]	-0.007*** [0.002]
Male dummy	-0.011 [0.015]	-0.013 [0.015]	-0.008 [0.016]
Age	-0.003 [0.005]	-0.003 [0.004]	-0.003 [0.005]
More education treatment dummy	0.036 [0.032]	0.036 [0.031]	0.036 [0.032]
More education, less cash treatment dummy	0.003 [0.031]	0.002 [0.032]	0.002 [0.032]
More cash treatment dummy	0.013 [0.028]	0.012 [0.027]	0.015 [0.028]
Number of children	0.001 [0.011]	0.001 [0.011]	0.000 [0.011]
Child in public school dummy	-0.017 [0.028]	-0.021 [0.028]	-0.012 [0.025]
Beneficiary of local CCT program dummy	0.144*** [0.034]	0.136*** [0.033]	0.140*** [0.034]
Observations	1,875	1,875	1,875
R-squared	0.17	0.17	0.17
Mean of dependent variable (Last column refers to mean in quartile 1)	0.343	0.343	0.333

Robust standard errors (clustered by surveyor) in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%

Additional controls: marital status dummies, age squared, district and surveyor fixed effects (all columns); zero household income and missing household income dummies (column 1); zero personal income and missing personal income dummies (column 2); missing household income dummy (column 3). The dependent variable is a dummy variable equal to one if the subject responded that his/her perception of the amount actually spent in improving public education for every R\$100 allocated by the local government to public education spending is less than his/her perception of the amount actually spent in increasing cash transfers for every R\$100 allocated by the local government to cash transfer spending.

Table A.8: Information shocks and assessment of the local government - Additional interaction effects (OLS regressions)

Dependent variable: <i>0-10 grade given to state government</i>	Used measure of income:			
	Log of household income [1]	Log of personal income [2]	Dummy: quartile 1 of household income [3]	Dummy: household income below median [4]
More education treatment dummy	-4.219* [1.956]	-3.707 [2.154]	0.413 [0.378]	0.496 [0.354]
More education, less cash treatment dummy	-4.677** [1.767]	-6.543*** [2.124]	0.429 [0.327]	0.652* [0.304]
More cash treatment dummy	3.561** [1.453]	4.945** [1.733]	0.408* [0.226]	0.196 [0.267]
Log household income	0.220 [0.193]			
Log personal income		-0.079 [0.238]		
Quartile 1 of household income			-0.080 [0.411]	
Household income below median				-0.613** [0.280]
More educ×measure of income	0.594** [0.243]	0.551* [0.286]	-0.360 [0.438]	-0.470* [0.262]
More educ, less cash×measure of income	0.646*** [0.201]	0.915*** [0.272]	-0.876 [0.524]	-0.954** [0.389]
More cash×measure of income	-0.402** [0.178]	-0.599** [0.235]	0.662 [0.428]	0.833** [0.368]
More diversion in education spending dummy	0.329 [0.191]	0.379* [0.200]	0.348 [0.203]	0.329 [0.203]
More education, less cash×more diversion in educ.	-0.381 [0.279]	-0.359 [0.276]	-0.304 [0.287]	-0.366 [0.284]
Child in public school dummy	-0.044 [0.384]	-0.049 [0.354]	-0.021 [0.347]	-0.023 [0.347]
More education, less cash×child in public school	0.380 [0.432]	0.375 [0.379]	0.116 [0.362]	0.258 [0.419]
Beneficiary of local CCT program dummy	1.318** [0.490]	1.251** [0.518]	1.310** [0.503]	1.384** [0.500]
More education, less cash×CCT recipient	-1.663* [0.768]	-1.628* [0.783]	-1.810* [0.841]	-1.797** [0.813]
Observations	1,875	1,875	1,875	1,875
R-squared	0.11	0.11	0.09	0.1
Mean of dependent variable in control group			5.734	

Robust standard errors (clustered by surveyor) in brackets * significant at 10%; ** significant at 5%; *** significant at 1%. Additional controls: marital status dummies, age squared, district and surveyor fixed effects, dummy equal to one if respondent believes cash transfers are not part of social assistance spending (all columns), years of schooling, male dummy, age, number of children, zero household income and missing household income dummies, and their interactions with treatment dummies (column 1), zero personal income and missing personal income dummies, and their interactions with treatment dummies (column 2), missing household income dummy and its interaction with treatment dummies (columns 3 and 4), interaction of *More education* treatment dummy and of *More cash* treatment dummy with *More diversion in education spending* dummy, with *Child in public school* dummy, with and *Beneficiary of local CCT program* dummy.

Table A.9: **Summary statistics and balance of observables – Revealed preference experiment**

	Low income treatment N=39	High income treatment N=41	p-value of test of equality
Child's grade in school	4.28 [0.46]	4.24 [0.43]	0.7
Child's age	11.62 [0.59]	11.83 [0.77]	0.17
Male child dummy	0.59 [0.5]	0.46 [0.5]	0.26
Parent's age	33.33 [6.75]	35.07 [6.94]	0.26
Male parent dummy	0.38 [0.49]	0.44 [0.5]	0.63
Parent's years of schooling	6.56 [0.53]	6.32 [0.47]	0.73
# of children in the HH	3.05 [1.39]	2.95 [1.53]	0.76
# of days of class missed last 2 months	1.87 [2.85]	1.63 [2.61]	0.7
# of grades child has failed	0.31 [0.52]	0.32 [0.52]	0.94
CCT beneficiary dummy	0.92 [0.27]	0.85 [0.36]	0.33
Log HH income	6.73 [0.35]	6.71 [0.37]	0.78
Employed parent	0.59 [0.5]	0.71 [0.46]	0.28
Catholic parent	0.51 [0.51]	0.51 [0.51]	0.99
Protestant parent	0.44 [0.5]	0.41 [0.5]	0.85
Married parent	0.56 [0.5]	0.51 [0.51]	0.65
Single parent	0.38 [0.49]	0.27 [0.45]	0.27
Separated parent	0.03 [0.16]	0.15 [0.36]	0.06
Divorced parent	0.03 [0.16]	0.07 [0.26]	0.34
Black parent	0.26 [0.44]	0.32 [0.47]	0.55
White parent	0.13 [0.34]	0.07 [0.26]	0.86
Mixed race parent	0.59 [0.5]	0.61 [0.49]	0.42

We present the sample means and standard deviations (in brackets) of observables. We perform t-tests of equality in means in the two treatment conditions.

Table A.10: **High income treatment effects on WTP for school tutoring** - OLS regressions

Dependent variable: WTP for tutoring			
	[1]	[2]	[3]
<i>High income</i> treatment dummy	15.929*** [2.909]	16.196*** [2.909]	15.335*** [2.794]
High stakes dummy		-0.067 [2.763]	0.122 [3.095]
Log of HH income		40.653*** [5.714]	41.076*** [6.218]
Individual and household covariates	No	Yes	Yes
Surveyor dummies	No	No	Yes
Observations	80	80	80
R-squared	0.107	0.660	0.689
Mean of dep. variable in <i>Low income</i> group		23.461	

Robust standard errors (clustered by surveyor) in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. The specification in column 2 includes the following covariates: male indicator (parent and child), age (parent and child), employed parent indicator, religion dummies, parent's marital status dummies, schooling (parent and child), number of children in the household, dummy on whether the household has been receiving conditional cash transfers from the government, parent's race dummies, number of days the child missed class in the last two months, number of grades the child has already failed. In column 3, we additionally includes surveyor dummies.

D Experimental Documentation

We enclose here an English version of the questionnaires used by the surveyors for the *More education, less cash* treatment and the follow-up survey (experiment #1), and for the *Low income* treatment group with 25% stakes (experiment #2) Then we we enclose a picture of the implementation of experiment #2 and a picture of a tutoring session.

TRANSLATED VERSION OF QUESTIONNAIRE 2
(*MORE EDUCATION, LESS CASH* INFORMATION SHOCK)

Opinion Survey
Questionnaire 2

Name of surveyor: _____ Date: _____ Time of interview: _____

Good morning! Good afternoon! My name is.... I am a surveyor. We are doing a survey with the population of the Federal District.

First part: Information about respondent:

Complete name of respondent: _____

Respondent's phone numbers: _____

Complete address: _____

A. City of residence of interviewee: _____

B. Sex: 1() Male 2() Female

C. Age: _____

D. What is your level of schooling?

1. Illiterate
2. Just reads and writes
3. Incomplete primary
4. Complete primary
5. Incomplete secondary
6. Complete secondary
7. Incomplete undergraduate
8. Complete undergraduate
0. Does not know/no answer

E. What is your marital status?

1. Single
2. Married
3. Separated/Divorced
4. Widow(er)
5. Does not know/no answer

F. Could you tell me your monthly personal income? _____ In R\$

1. Does not earn personal income
2. Does not know/no answer

G. Could you tell me your monthly family income? (Your household income)
_____ In R\$

1. Does not earn personal income
2. Does not know/no answer

H. Could you tell me your exact schooling level?

0. None

1. First grade/primary
2. Second grade/primary
3. Third grade/primary
4. Fourth grade/primary
5. Fifth grade/primary
6. Sixth grade/primary
7. Seventh grade/primary
8. Eight grade/primary
9. First grade/secondary
10. Second grade/secondary
11. Third grade/secondary
12. Incomplete undergraduate
13. Complete undergraduate
14. Post-graduate
99. Does not know/no answer

Second part:

1. How many children do you have?
 99. Does not know/no answer
2. How many of them go to school?
 99. Does not know/no answer
3. How many of them study at a public school?
 99. Does not know/no answer
4. Does your family receive conditional cash transfers from the Government of the Federal District? (*Renda Minha ou Bolsa-Escola, Vida Melhor* program)
 1. Yes
 2. No
 3. Does not know/no answer
5. If the respondent answered “yes” to the question above, please continue with the next two questions. If not, skip both and go directly to question 7 (and mark “0” on the next two questions).
 Would you switch from your transfer (*Renda Minha ou Bolsa-Escola, Vida Melhor*) to a transfer of equal amount, but which were paid independently of your child attending school?
 1. Yes
 2. No
 - 0.
6. Would you switch from your transfer (*Renda Minha ou Bolsa-Escola, Vida Melhor*) to a transfer that paid R\$ more, but which were paid independently of your child attending school?
 1. Yes
 2. No
 - 0.
7. Which of the following categories of public spending do you believe cash transfer spending is part of?
 1. Public education

2. Social assistance
 3. Other category/categories
 4. Does not know/no answer
8. Did you know that, compared to 2006, the state (Arruda) government increased in 2007 the share of total public expenditures allocated to public education by 9% but reduced the share allocated to social assistance by 9%?
1. Yes
 2. No
9. From 0 to 10, what grade would you give to the Arruda government until now? _____
99. Does not know/no answer
10. *In the next two questions, in case the respondent says he/she does not know the answer or does not want to answer the question, please mark "0".*
- What is more important for the state government to achieve? (Please choose one of them)
1. Improve public education
 2. Increase cash transfers
 - 0.
11. Which of the following two numbers do you believe to be greater?
1. The amount actually spent in improving public education for every R\$ 100 allocated by the local government to public education spending
 2. The amount actually spent in increasing cash transfers for every R\$ 100 allocated by the local government to cash transfer spending.
 - 0.

Thank you for your participation!

TRANSLATED VERSION OF QUESTIONNAIRE 2 FROM FOLLOW-UP SURVEY
(INFORMATION SHOCKS TREATMENT GROUP)
(QUESTIONS IN BOLD WERE NOT ASKED IN THE CONTROL GROUP)

Opinion Survey
Questionnaire 2

Name of surveyor: _____ Date: _____ Time of
interview: _____

Good morning! Good afternoon! My name is.... I am a surveyor. We are doing a survey
with the population of the Federal District.

First part: Information about respondent:

A. Administrative region of residence of interviewee: _____

B. Sex: 1() Male 2() Female

C. Age: _____

D. What is your level of schooling?

1. Illiterate
2. Just reads and writes
3. Incomplete primary
4. Complete primary
5. Incomplete secondary
6. Complete secondary
7. Incomplete undergraduate
8. Complete undergraduate
0. Does not know/no answer

E. What is your marital status?

1. Single
2. Married
3. Separated/Divorced
4. Widow(er)
5. Does not know/no answer

F. Could you tell me your monthly personal income? _____ In R\$

1. Does not earn personal income
2. Does not know/no answer

G. Could you tell me your monthly family income? (Your household income)
_____ In R\$

1. Does not earn personal income
2. Does not know/no answer

H. Could you tell me your exact schooling level?

0. None
1. First grade/primary
2. Second grade/primary
3. Third grade/primary
4. Fourth grade/primary
5. Fifth grade/primary

6. Sixth grade/primary
7. Seventh grade/primary
8. Eight grade/primary
9. First grade/secondary
10. Second grade/secondary
11. Third grade/secondary
12. Incomplete undergraduate
13. Complete undergraduate
14. Post-graduate
99. Does not know/no answer

Second part:

1. How many children do you have?
 99. Does not know/no answer
2. How many of them go to school?
 99. Does not know/no answer
3. How many of them study at a public school?
 99. Does not know/no answer
4. Does your family receive conditional cash transfers from the Government of the Federal District? (*Bolsa-Escola* program)
 1. Yes
 2. No
 3. Does not know/no answer
5. If the respondent answered “yes” to the question above (4), please continue with the next questions. If not, go directly to question 6 (and mark “99” on question 5). If the Distrito Federal government (GDF) increased the your monthly transfer by R\$ 20, which one of these would be your priority to spend the additional amount?
 1. Help pay your family bills
 2. Improve the standard of consumption/standard of living of your family
 3. Pay debts
 4. Improve the nutrition of your family
 5. Invest in the improvement of your child’s education
 6. Other forms of spending
6. Which of the following categories of public spending do you believe cash transfer spending is part of?
 1. Public education
 2. Social assistance
 3. Other category/categories
 4. Does not know/no answer
7. **Did you know that, compared to 2006, the state (Arruda) government increased in 2007 the share of total public expenditures allocated to public education by 9%?**
 1. Yes
 2. No
8. **Before I had read that information, had you ever thought that the increase in the share of total public expenditures allocated to public education during the**

period described above in the Arruda government had been of 9% or more than 9%?

1. Yes
 2. No
9. **Did you know that, compared to 2006, the state (Arruda) government reduced in 2007 the share of total public expenditures allocated to social assistance by 9%?**
1. Yes
 2. No
10. **Before I had read that information, had you ever thought that the reduction in the share of total public expenditures allocated to social assistance during the period described above in the Arruda government had been of 9% or more than 9%?**
1. Yes
 2. No
11. Which one did you think was a bigger priority of the Arruda government throughout its tenure (2007-2010)?
1. Improving public education
 2. Increasing cash transfers
12. **Which state government administration do you think was responsible for the spending changes described above? (Please choose one of the two options below).**
1. **The Arruda administration**
 2. **The state government administration prior to Arruda's**
13. *In the next two questions, in case the respondent says he/she does not know the answer or does not want to answer the question, please mark "0".*
What is more important for the state government to achieve? (Please choose one of them)
1. Improve public education
 2. Increase cash transfers
 - 0.
14. Which of the following two numbers do you believe was greater during Arruda's administration? (Please choose one of the two options below)
1. The amount actually spent in improving public education for every R\$ 100 allocated by the Arruda government administration to public education spending
 2. The amount actually spent in increasing cash transfers for every R\$ 100 allocated by the Arruda government administration to cash transfer spending.
 - 0.
15. **Did you know that compared to the first year in the previous state government, the Arruda state government increased in its first year the share of total public expenditures allocated to social assistance from 1.3% to 3.1%?**
1. Yes
 2. No
16. **Before I had read that information, had you ever thought that the increase in the share of total public expenditures allocated to social assistance during the period described right above in the Arruda government had been equal or greater than the one I just read to you?**

- 1. Yes**
- 2. No**

Thank you for your participation!

ENGLISH VERSION OF THE QUESTIONNAIRE
(LOW INCOME TREATMENT AND LOWER STAKES VERSION)

Welcome

Thank you for participating in our study.

This study is conducted in cooperation between a researcher from the University of California, Los Angeles, and ANDI (News Agency for Children's Rights).

We will ask you questions.

Please do not talk to anyone besides the researchers. We also ask you to turn off your cell phone.

We will have to ask you to leave in case you do not accept these rules.

Thank you for showing up.

Today it will be asked that you answer a few questions. Your participation will last for about 20 minutes. If you have any questions, please raise your hand and one of our researchers will help you.

Your participation is purely voluntary, and you may leave anytime you wish, without any penalties.

I have read the description of this study, my doubts have been cleared, and I want to participate.

Questions about this study should be directed to Leonardo Bursztyn, bursztyn@ucla.edu.

Name of surveyor:

What is your name?

Thanks for participating. We will now offer you the chance to earn different types of benefits.

- 1. The first benefit is a monthly payment to you and your family for the next two months (November and December 2012). Nothing is required to receive the money.**
- 2. The second benefit will depend on what you choose next. We will offer you 17 choices between more money payments and free, after-school tutoring for your child, both for the next two months as well. We will ask you to tell what you prefer for every one of these 17 questions. We will explain the details when we present you with the choices.**

It is important to know that there will be a raffle. 25% of the participants that receive these offers will be randomly selected and will receive the benefits for November and December 2012. If you are one of the winners, you will receive the first benefit (the monthly payment) for the two months. As for the second benefit, we will randomly choose one of the 17 questions and you will receive for the two months what you chose for it. Therefore, it is best for you to tell what you really prefer because there is a chance you will get it.

First benefit:

The first benefit is a payment of R\$10 to you, once this month, and once next month.

Second benefit:

If you could choose a type of second benefit for your family to receive until the end of this year, what would be your preference for each of one the choices below?

As we mentioned before, there will be a raffle. 25% of the families will be randomly selected and we will randomly choose one of the questions and you will receive for November and December 2012 what you chose for it. Therefore, it is best for you to tell what you really prefer because there is a chance you will get it.

Math and Portuguese

(1)

(a) Receive R\$10 per month without any requirement, or

(b) Receive R\$10 per month without any requirement, and also your child will receive, for free, three weekly hours of tutoring provided by a college student to help your child in school. The tutoring is for Math and Portuguese. The times of the sessions would be arranged between your family and the tutor.

(2)

(a) Receive R\$15 per month without any requirement, or

(b) Receive R\$10 per month without any requirement, and also your child will receive for free three weekly hours of tutoring provided by a college student to help your child in school.

(3)

(a) Receive R\$20 per month without any requirement, or

(b) Receive R\$10 per month without any requirement, and also your child will receive for free three weekly hours of tutoring provided by a college student to help your child in school.

(4)

(a) Receive R\$25 per month without any requirement, or

(b) Receive R\$10 per month without any requirement, and also your child will receive for free three weekly hours of tutoring provided by a college student to help your child in school.

(5)

(a) Receive R\$30 per month without any requirement, or

(b) Receive R\$10 per month without any requirement, and also your child will receive for free three weekly hours of tutoring provided by a college student to help your child in school.

(6)

(a) Receive R\$35 per month without any requirement, or

(b) Receive R\$10 per month without any requirement, and also your child will receive for free three weekly hours of tutoring provided by a college student to help your child in school.

(7)

(a) Receive R\$40 per month without any requirement, or

(b) Receive R\$10 per month without any requirement, and also your child will receive for free three weekly hours of tutoring provided by a college student to help your child in school.

(8)

(a) Receive R\$45 per month without any requirement, or

(b) Receive R\$10 per month without any requirement, and also your child will receive for free three weekly hours of tutoring provided by a college student to help your child in school.

(9)

(a) Receive R\$50 per month without any requirement, or

(b) Receive R\$10 per month without any requirement, and also your child will receive for free three weekly hours of tutoring provided by a college student to help your child in school.

(10)

(a) Receive R\$55 per month without any requirement, or

(b) Receive R\$10 per month without any requirement, and also your child will receive for free three weekly hours of tutoring provided by a college student to help your child in school.

(11)

(a) Receive R\$60 per month without any requirement, or

(b) Receive R\$10 per month without any requirement, and also your child will receive for free three weekly hours of tutoring provided by a college student to help your child in school.

(12)

(a) Receive R\$65 per month without any requirement, or

(b) Receive R\$10 per month without any requirement, and also your child will receive for free three weekly hours of tutoring provided by a college student to help your child in school.

(13)

(a) Receive R\$70 per month without any requirement, or

(b) Receive R\$10 per month without any requirement, and also your child will receive for free three weekly hours of tutoring provided by a college student to help your child in school

(14)

(a) Receive R\$75 per month without any requirement, or

(b) Receive R\$10 per month without any requirement, and also your child will receive for free three weekly hours of tutoring provided by a college student to help your child in school.

(15)

(a) Receive R\$80 per month without any requirement, or

(b) Receive R\$10 per month without any requirement, and also your child will receive for free three weekly hours of tutoring provided by a college student to help your child in school.

(16)

(a) Receive R\$85 per month without any requirement, or

(b) Receive R\$10 per month without any requirement, and also your child will receive for free three weekly hours of tutoring provided by a college student to help your child in school.

(17)

(a) Receive R\$90 per month without any requirement, or

(b) Receive R\$10 per month without any requirement, and also your child will receive for free three weekly hours of tutoring provided by a college student to help your child in school.

Personal information:

Are you separated, married, divorced, or single?

-Separated

-Married

-Divorced

-Single

What is your family's total level of income?

What is your gender?

-Male

-Female

How old are you?

What is your religion?

-Catholic

-Protestant

-Spiritualistic

-Other: _____

-None

Are you employed?

- Yes
- No

What is your education level?

- None
- First grade (primary)
- Second grade (primary)
- Third grade (primary)
- Fourth grade (primary)
- Fifth grade (primary)
- Sixth grade (primary)
- Seventh grade (primary)
- Eighth grade (primary)
- First grade (secondary)
- Second grade (secondary)
- Third grade (secondary)
- College – incomplete
- College – complete

What is your race?

- Black
- White
- Mixed race
- Native

How many children do you have?

For how many of them does your family receive government aid to attend school?

What is your child's gender?

- Male
- Female

What is your child's age?

What school grade is your child attending?

- Third
- Fourth

How many days of class did your child miss in the last two months?

Has your child failed any grade?

- Yes
- No

If yes, how many?

How much do you think your child's (monthly) salary would be if he/she finished high school?

Figure A.6: Picture from the implementation



Figure A.7: Picture of a tutoring session

