POVERTY AND THE POLITICAL ECONOMY OF PUBLIC EDUCATION SPENDING: EVIDENCE FROM BRAZIL

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Abstract
A large body of literature has emphasized the 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. (JEL: C90, H52, I25, O15, P16)

> "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]

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1. See 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).

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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 body of 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; 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 data set 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 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).

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

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

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

Notes: Robust standard errors in brackets. 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.
*Significant at 10%; **significant at 5%; ***significant at 1%.

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

4. More precisely, and following Bordalo, Gennaioli, and Shleifer (2013) 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 disproportionally high weight to that attribute of the bundle of government spending.

are associated with *decreases* in the probability of re-election of incumbent parties, whereas in municipalities with high median incomes, increases in that type of spending are associated with higher probability of incumbent party re-election. 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 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 80 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 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

5. We believe that it is unlikely that our results are driven by this alternative mechanism. As we discuss in what follows, 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.

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 body of 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 spending.[8]

Finally, in terms of experimental design, our paper relates to recent work that has also used randomized information shocks (see Banerjee et al. 2010; Jensen 2010; Card et al. 2012; 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,

6. See Manacorda, Miguel, and Vigorito (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 US evidence, see Husted and Kenny (1997), Naidu (2010), and Cascio and Washington (2012).

7. See 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; Harrison, Lau, and Williams 2002; Banerjee and Mullainathan 2009; 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; Brune et al. 2013). Finally, Shah, Mullainathan, and Shafir (2012) argue that scarcity itself can lead to short-run biases.

8. Other 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; Brown and Hunter 1999; and Avelino, Brown, and Hunter 2005). Acemoglu et al. (2014) theoretically and empirically study the relationship between democracy, redistribution, and inequality.

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 aged 7–14 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 Online Appendix Figure A.1, the percentage of children aged 7–14 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 low-income households are more likely than high-income households 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 18–70.[12] Municipalities are responsible for basic (primary) education, which corresponds to elementary and middle school in the United States.[13]

We construct a data set 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

9. In 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.

10. The average wage of someone with a high-school degree in Brazil is 101% higher than that of someone with no schooling (PNAD 2009, 2011).

11. The setting of government spending in Brazilian municipalities has been used by other recent papers, such as Ferraz and Finan (2008, 2011), Caselli and Michaels (2013), Brollo et al. (2013), Litschig and Morrison (2013), and more closely related to this paper, Firpo, Pieri, and Souza (2012).

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

13. Technical 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.

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

on public education on whether or not the incumbent party was re-elected for the mayor's office.[15]

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

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

In equation (1), $Reelection_{i,t}$ is a dummy variable equal to 1 if the incumbent party is re-elected in election year t (2004 or 2008) in municipality i, and 0 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 municipal budget, measured by $\ln(Budget_{i,t})$—namely, 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 re-election, separately for municipalities with lower and higher levels of median income. We first linearly interact $\ln(Educ_{i,t})$ with ln (*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 $\ln(Educ_{i,t})$ 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,

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

16. To 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 reais, 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.

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

TABLE 2. Public education spending and probability of re-election in a panel of Brazilian municipalities—OLS regressions.

Dependent variable: incumbent party re-elected (dummy)			
	(1)	(2)	(3)
Log of municipal budget during term	0.002	0.032	0.025
	[0.094]	[0.093]	[0.094]
Log of municipal education spending during term ($\ln(Ed)$)	0.048	-1.103^{***}	
	[0.070]	[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	

Notes: Robust standard errors in brackets. 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).
*Significant at 10%; **significant at 5%; ***significant at 1%.

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.

Online 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. 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 re-election. 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 re-election (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 1–4 and 5–9. 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 re-election, and the effect of more education quality on the likelihood of party re-election becomes positive as median income goes up.

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]

18. Our 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.

19. Although 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.

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

TABLE 3. Public education quality and probability of re-election in a panel of Brazilian municipalities—OLS regressions.

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

Notes: Robust standard errors in brackets. 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).
*Significant at 10%; **significant at 5%; ***significant at 1%.

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 they 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 DF. 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 that 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 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 12 of the 20 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 their 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 directly asking respondents about their views on public education versus cash transfers. At the end of the survey, after the measurement of the main outcome variable of interest that we discuss in what follows (the rating of the government), each respondent answered the following 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

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

22. One 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

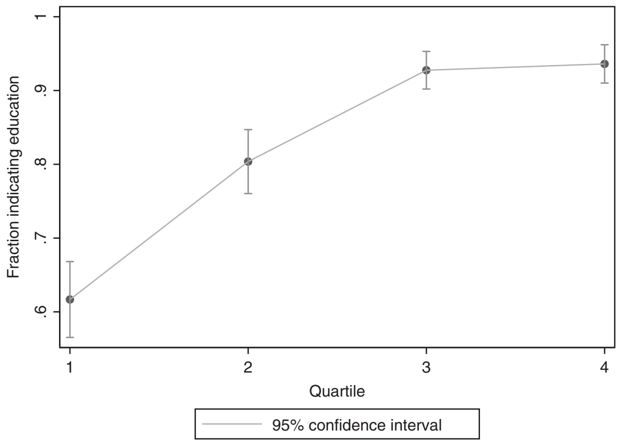

FIGURE 1. Survey experiment. Fraction of respondents by quartile of the household income distribution indicating improving public education as more important government goal than increasing cash transfers. With 95% confidence interval.

1 to quartile 2, the fraction of 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. Online 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, 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 per capita terms in Brazil. The level of GDP per capita 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 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.

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 *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]

23. Across 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 treatments.

24. Since there is no category directly named "cash transfers" in the public spending data set 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 at 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 not to 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.

25. This 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.

3.1.4. Outcome of Interest: Rating the Local Government. As mentioned previously, 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 the following. *From 0 to 10, what grade would you give to the Arruda administration until now?* The 0–10 rating of the government by the respondents 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 previously. *What is more important for the state government to achieve?* Then, they were asked the following question. *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 the state government administration under consideration more 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 Online 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:

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

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.

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 who reported to have zero income and those who 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

Online 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 for 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]

26. We 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

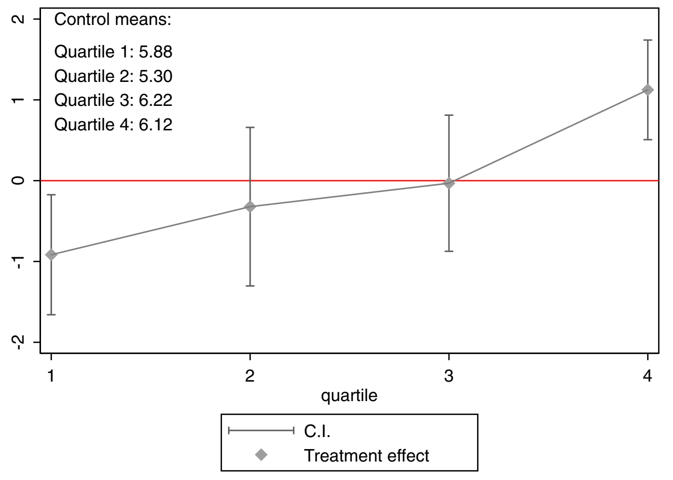

FIGURE 2. Survey experiment. Treatment effects of the *More education, less cash* information shock on rating (0–10 grade) of the local government, by quartiles of the household income distribution. With 95% confidence interval. Treatment effects estimated using specification from Table 4 and means from control group by quartile. The wording for the information shock was as follows. *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%?*

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]

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).

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

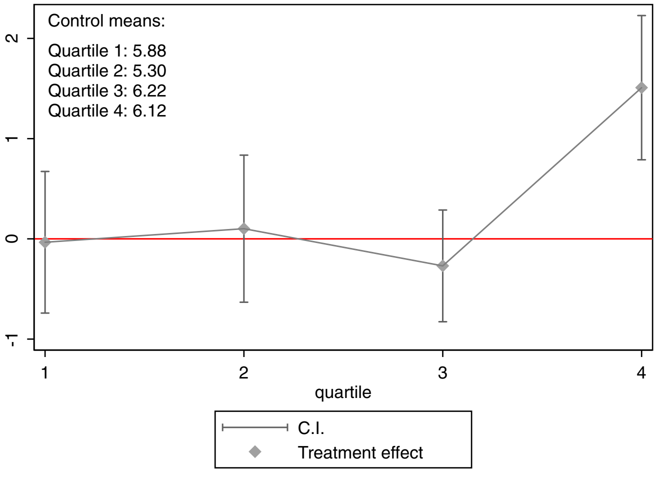

FIGURE 3. Survey Experiment. Treatment effects of the *More education* information shock on rating (0–10 grade) of the local government, by quartiles of the household income distribution. With 95% confidence interval. Treatment effects estimated using specification from Table 4 and means from control group by quartile. The wording for the information shock was as follows. *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%?*

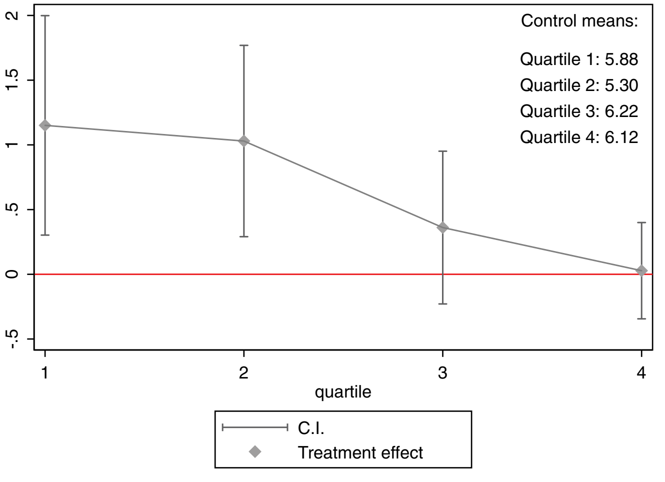

FIGURE 4. Main Survey Experiment. Treatment effects of the *More cash* information shock on rating (0–10 grade) of the local government, by quartiles of the household income distribution. With 95% confidence interval. Treatment effects estimated using specification from Table 4 and means from control group by quartile. The wording for the information shock was as follows. *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%?*

TABLE 4. Information shocks and rating of the local government (0–10 grade)—OLS regressions.

	Used measure of income:			
Dependent variable: 0–10 grade given to state government	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**	−3.901**	0.426*	0.565**
	[1.524]	[1.726]	[0.222]	[0.221]
More education, less cash treatment dummy	−5.487***	−7.372***	0.230	0.527*
	[1.180]	[1.610]	[0.263]	[0.243]
More cash treatment dummy	3.558**	4.777**	0.444**	0.207
	[1.428]	[1.643]	[0.195]	[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**	0.586**	−0.477	−0.560**
	[0.202]	[0.240]	[0.386]	[0.237]
More education, less cash × measure of income	0.733***	1.011***	−1.139**	−1.176***
	[0.142]	[0.217]	[0.432]	[0.306]
More cash × measure of income	−0.403**	−0.582**	0.713	0.843**
	[0.176]	[0.224]	[0.425]	[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			

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

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 raises the rating by those with higher incomes. In Online Appendix Table A.5, we replicate Table 4 without conditioning on controls. In Online 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 80 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 DF 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. 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 in what follows). The amount of this first payment was randomly varied across treatment groups: for the *Low income*

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

29. We 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 80 parents from the studied school.

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

treatment it was R$10 a month, and for the *High income* treatment it was R$210 a 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 17 questions, each one a choice between: (i) R$10 + R$X to be added to their initial monthly payment and (ii) R$10 to be added to their monthly payment *and* free, individual, weekly, three-hour long, after-school Mathematics and Portuguese tutoring sessions for their child for November and December 2012.[31] The amount R$X started at zero and was increased by R$5 increments question after question, as presented in the following table.

	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 17 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 17 questions, and that decision would be randomly chosen from the 17 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 (WTP). This design therefore allows for (i) the elicitation of the WTP for the free tutoring sessions for two months, and (ii) how such WTP varies when the household level of income is randomly increased for two months.[33]

31. The tutoring sessions would take place at the school and would stop in 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 Online Appendix D for a picture of a tutoring session.

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

33. The research assistants reported only one case of nonmonotonic 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 nonmonotonic 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.

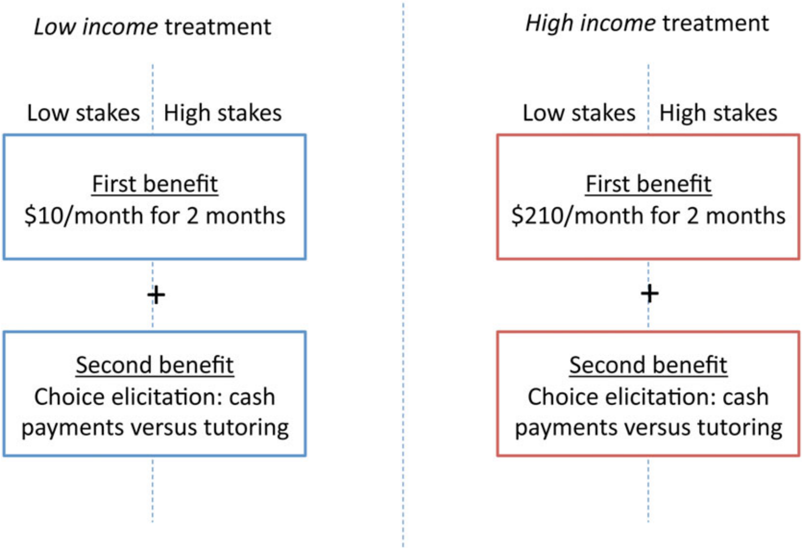

FIGURE 5. Auxiliary (revealed preference) experiment. Experimental design.

Finally, it is worth noting that we decided to break the benefits into two parts to make sure the 17 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 one of the 17 questions across the two treatments.

Figure 5 summarizes the experimental design.

Experimental Outcomes. We focus on one main outcome variable: the parent's WTP for tutoring, which is equal to the largest amount that the parent is willing to forgo to obtain 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

34. Note that WTP could be up to R$5 greater. We code WTP to pay the same across all treatments and focus on across-treatment differences. For robustness, we recode the WTP differently for the two treatments. First, we increase the WTP 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 WTP 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 WTP variable.

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 WTP 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.

Finally, we also display the treatment effects on the WTP 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

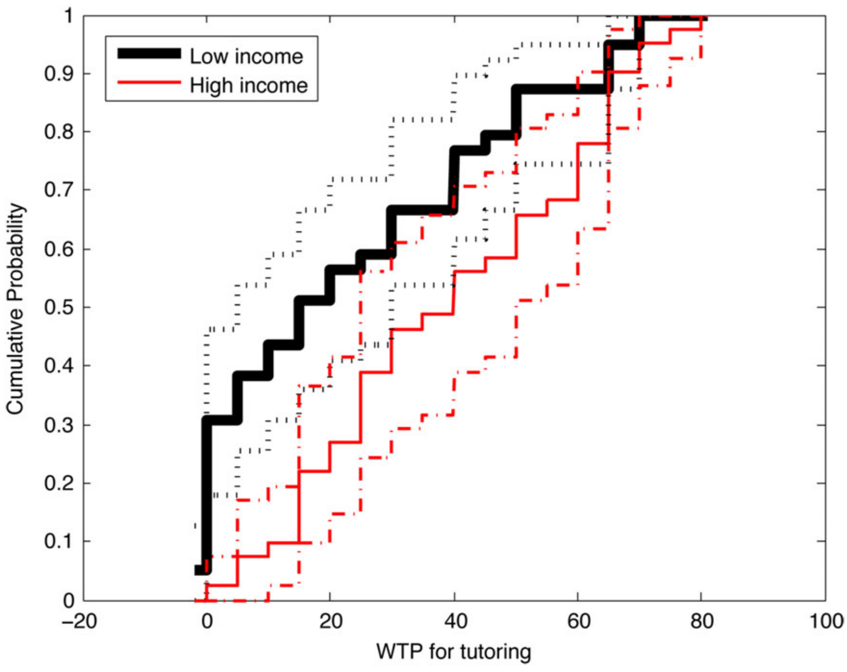

FIGURE 6. Cumulative probability for the WTP for tutoring—*Low income* and *High income* treatment groups (with 95% bootstrap confidence intervals—1,000 bootstrap samples). We resampled with replacement from the empirical distribution 1,000 times. From these 1,000 bootstrap samples, we computed the confidence intervals for each point on the cumulative distribution.

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 Online 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 WTP 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

35. Furthermore, 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.

education versus cash choice they face. In particular, the median of the aforementioned WTP 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 groups 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 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.

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

Our results do not imply that the poor value education less than the rich do. Our findings instead indicate that (often constrained) low-income individuals may have more urgent needs and might not be able to afford to have 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 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.

37. Conversely, 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.

38. As 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 US$75). Source: Brazilian Ministry of Social Development and Fight against Hunger: www.brasil.gov.br/noticias/arquivos/2012/09/28/programa-de-transferencia-de-renda-paga-r-2-bilhoes-no-mes-de-setembro.

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Online Appendix
Replication files