

The Psychological Effects of Poverty on Investments in Children's Human Capital[§]

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ABSTRACT: While accumulating research suggests that poverty imposes a psychological tax on decision-making, there is still no evidence of whether such psychological consequences also affect real economic decisions, in particular those that could generate poverty traps. This paper tests this hypothesis by offering individuals the opportunity to invest in an educational program with large average impacts on children's educational outcomes. Drawing on a survey experiment to emulate the psychological consequences of poverty, we find that poor parents' willingness to invest decreases with the predicted returns of the program – while it increases with returns in the control group. We show that the failure to evaluate long-term returns operates through *mental bandwidth*: experimentally poor subjects display higher *focus* on short-term returns, performing better on incentivized attention and memory tests. While prior experience with the program does not help, making program's returns *top-of-mind* mitigates the psychological effects of poverty on focus and investment decisions.

This version: April 8th, 2018

Keywords: Psychology of poverty; Scarcity; Children's human capital; Poverty traps.

JEL codes:

[§] We would like to thank comments from seminar participants at the University of Stavanger and the University of Zurich. We also thank Guilherme Avelar and Flavio Riva for excellent research assistance. All remaining errors are ours.

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1 Introduction

While there is increasing evidence that poverty impairs the foundations of decision-making, inducing individuals to focus on scarcity at hand at the expense of other dimensions (Mani et al., 2013; Lichand and Mani, 2018), there is still limited research on the psychological consequences of poverty for real economic decisions, in particular those that could generate poverty traps. This paper tests whether the psychology of poverty affects one such decision: parents' demand for an educational program with proven impacts on learning outcomes for their children – including the likelihood of progressing to high-school, linked to significantly higher labor market returns (16.4% higher earnings in Brazil; Barbosa Filho and Pessoa, 2008).

There are reasons to believe that the psychology of poverty may affect parental investments in children's human capital. First, while investments in children may take many years to materialize, subject to a great deal of uncertainty, poverty has been shown to make individuals systematically less prone to undertaking risky investments (Haushofer and Fehr, 2014). Second, while investments trade off costs in the present against returns in the future, poverty induces mental bandwidth to focus on short-term financial outcomes (Mullainathan and Shafir, 2013), potentially compromising the evaluation of long-term returns.

To study the psychological effects of poverty on parental investments in their children's human capital, we undertake a large-scale experiment across public schools in São Paulo, Brazil. We endow over 2,500 parents of high-school freshmen with R\$ 10 (about USD 3) in exchange for answering a phone survey (IVR) that offers them the opportunity to invest this endowment in hiring an SMS educational program over the next 6 months.

The program delivers 2 text messages a week, meant to nudge parents to pay closer attention to their children's school life. All parents have prior experience with the program, as this sample was part of a study that assigned parents to different communication strategies over SMS during the previous year (Cunha et al., 2018a). The SMS program has high impacts: on average, it reduces 5 absences per year, increases standardized test scores by about 0.09 standard deviation (equivalent to one quarter ahead in school), and decreases retention rates by 3 percentage points (Cunha et al., 2018a). While average impacts are very high, different families have very different expected returns from the SMS program. Drawing upon a regression tree, we predict heterogeneous treatment effects by student and family characteristics: for some families, the program has no effects on absences; for other families, it reduces 1, 2 or 3 absences within 6 months. We take advantage of those differences to investigate how parents' willingness to invest in children's human capital varies as a function of returns on investment.

The goal of our experiment is to understand how the *psychology of poverty* affects parents' willingness to invest in the SMS program as a function of its returns. Investment decisions by the poor (families earning less than 1 minimum wage per month) and the rich (or, more accurately, less poor; families whose monthly earnings are above 5 minimum wages) display stark differences. While demand for the program among the rich is increasing in the predicted returns of the program, among the poor – strikingly –, demand decreases with predicted returns.

Just comparing investment decisions across poor and rich families, however, does not necessarily convey the psychological effects of poverty on that decision. There are many other differences between those families, including the extent to which they may expect different labor market returns to attending classes, which may result in different optimal investment schedules.

To deal with that challenge, we randomly assign subjects to the psychological effects of poverty by resorting to a survey experiment (what cognitive psychologists call *priming*). At the beginning of the phone survey, we ask parents what they would do if their child's school started charging a high amount for textbooks and uniforms (R\$ 400, or about USD 130), and that amount were due at the end of that month.¹ Following previous studies (Mullainathan and Shafir, 2013), the control group is asked a variation of the same question, in which the only difference is that schools are said to charge a low amount instead (R\$ 20, or about USD 6.50).

The priming technique is meant to emulate the psychological feeling of having financial worries constantly *top-of-mind* among the poor. For this reason, we call the control group *experimentally rich*, and the treatment group, *experimentally poor*. Manipulation checks confirm that the priming makes experimentally poor parents systematically more worried about coping with household bills (65.1% report to be very worried or desperate about not having money to pay all bills at the end of the month, compared to 56.8% in the control group, a difference statistically significant at the 1% level).

In what comes to willingness to pay across experimentally rich and poor, differences match the patterns that emerge from cross-sectional variation in family income.² Amongst the experimentally rich, a linear approximation implies that about 35% of parents invest in the program even when it has no effect on absences, and that demand increases by about 5 percentage points per predicted reduction in absences coming from the SMS program. Conversely, amongst the experimentally poor, a linear approximation implies that 46% of parents invest when the program has no effect – a substantially *higher* share than that

¹ Textbooks and uniforms are typically provided at no cost for public school students in Brazil.

² In this paragraph, we restrict attention to experimental results within the subsample not informed about returns (see below).

among the rich –, and demand *falls* by about 4.5 percentage points per predicted reduction in absences coming from the SMS program. Results are estimated with statistical precision, all significant at the 5% level.

Since we actually implement parent’s decisions, what we document are real consequences of the psychology of poverty for human capital investments in children. Parents who decide not to invest in the program have no future opportunity of doing so, and the endowment is converted into airtime credit which they cannot transfer or use to make purchases in Brazil. This is the first paper to document how such mechanism translates into real economic decisions, and in particular into one that is likely to affect inter-generational transmission of poverty.

Next, we investigate whether these effects operate through mental bandwidth. To do so, we undertake audio versions of attention, memory and impulse-control tests, following Lichand and Mani (2018). Those tests are incentivized: top-performers earn extra R\$ 2 (about 0.70 USD), creating the opportunity for short-term gains. Consistent with the focus mechanism, experimentally poor subjects perform significantly better in those tests (0.1 standard deviation – about half the effect of having attended school beyond primary education on cognitive performance –, statistically significant at the 5% level). While primed subjects often fare worse in attention and memory tests in prior studies (Mullainathan and Shafir, 2013), what is key in our setting is the combination of priming subjects *about financial worries* and submitting them to *incentivized* cognitive tests. The enhanced focus effect we find is consistent with results in Shah, Mullainathan and Shafir (2012) and Lichand and Mani (2018), and is distinctive about the mental bandwidth mechanism. All in all, the evidence points out that the psychology of poverty induces subjects to focus on short-run returns, at the expense of properly evaluating long-term returns on investment.

A common criticism of Behavioral Economics is that enough market experience would eliminate such failures of rationality. To test this hypothesis, we take advantage of another experiment (Cunha et al., 2018a). In that study – conducted over the course of 20 weeks, in the year before our experiment –, the authors randomly assigned parents to either 1 SMS per week with a child-specific message, or to a control group, which received *at most* 1 school-wide SMS per month with communication about school activities. We call the former group *high-experience* in the context of our experiment. Manipulation checks at the end of the 20 weeks confirm that parents assigned to the treatment group are much more likely to recall receiving text messages (89.8%, versus 46.4% in the control group, a statistically significant difference at the 1% level).

Results are as follows.³ While experience shifts willingness to pay upwards – making parents 10 percentage points more likely to undertake the investment for all return levels –, it does not change the negative slope of willingness to pay with respect to returns among experimentally poor parents. Among high-experience parents, the higher intercept is no longer statistically significant; having said that, a linear approximation indicates that experimentally poor parents’ willingness to invest decreases 5.6 percentage points per absence reduced by the SMS program, an effect significant at the 5% level.

Among high-experience subjects, the experimentally poor are 6.6 percentage points less willing to invest than the experimentally rich, across all return categories. How large is this effect? As a benchmark, poor individuals (those below 1 minimum wage) in our sample invest 5.9 percentage points less than rich ones (above 3 minimum wages), across all return categories. While it is unlikely that the psychology of poverty accounts entirely for the lower propensity to invest in children’s education among the poor, our estimate lies within a reasonable range for cross-sectional differences arising from variation in family profiles. What is more, if priming studies have been criticized for generating effects that are “not robust”⁴, Lichand and Mani (2018) show that the psychological effects of survey experiments along the lines we conduct in this paper match those of real income shocks, such as rainfall or random variation in payday of conditional cash transfers among the poor.

If mental bandwidth is the mechanism behind those effects, then a key implication is that such effects should be mitigated by making long-run returns top-of-mind. We test this hypothesis by cross-randomizing parents to information about the predicted returns of the SMS program. Half of our subjects are informed over SMS about the predicted effect of the program on their child’s absences, 2 days before the phone survey; the other half (the control group) receives a neutral message, which just states the number of families who participated in the program in the previous year.

Consistent with the focus mechanism, within parents informed about predicted returns there are no differences across experimentally rich and poor in what comes to the demand for the program: neither the intercept nor the slope of willingness to pay with respect to returns of the program are systematically affected by the priming. We link this result to mental bandwidth with the help of incentivized cognitive tests: informed subjects fare systematically worse in those tests (about 0.1 standard deviation, which exactly offsets the effects of priming on focus). Our interpretation is that receiving the text message prior to the call

³ In this paragraph, we also restrict attention to experimental results within the subsample not informed about returns (see below).

⁴ <https://www.nature.com/news/nobel-laureate-challenges-psychologists-to-clean-up-their-act-1.11535>

makes long-term returns top-of-mind, preserving parent's ability to evaluate such returns even under higher financial worries.

Can we distinguish salience of long-term returns from other effects information? To do that, we resort to an alternative measure of returns of the SMS program: its predicted effects on the likelihood of the child advancing to high-school. A regression tree based on student and family characteristics predicts that, for some families, the program does not affect the probability of completing 9th grade; for other families, the program increases it by 2, 3 or 4 percentage points. We then repeat the analyses of the effects of priming, experience and information on investment decisions, under this alternative measure of returns.

This strategy has two advantages. First, advancing to high-school is much more consequential than having fewer absences, directly mapping into higher expected earnings in the future. If the psychology of poverty still makes willingness to invest decrease with predicted returns in that case, then it would make for a much stronger case of its connection to potential poverty traps. Second, because we informed (half of) parents only about the predicted effects of the SMS program on *attendance* – but *not* on the likelihood of advancing to high-school –, then the effects of information in that case provide a clean test of salience of long-term returns as opposed to other potential effects of information, since the predicted effects of the programs on each dimension turn out to be uncorrelated.⁵

Using this alternative measure of predicted returns yields the same empirical regularities that we found before. Experimentally poor parents' willingness to invest slopes downward with respect to predicted returns – while it slopes upward amongst the experimentally rich. While experience does not reverse demand's negative slope with respect to returns on investment, information about long-term returns does – even though it refers to a different, uncorrelated dimension. Such patterns confirm that focus is the fundamental mechanism behind those effects, and that the psychology of poverty has the potential to generate poverty traps.

How generalizable are our findings? Shall one always expect a demand function that decreases with returns on investment as a psychological consequence of poverty? While this may be an extreme manifestation of the phenomenon – which may or may not hold in different settings –, the underlying principle we have documented should *not* be context-specific: since we have shown that poverty induces subjects to focus on short-term returns, this should systematically decrease executive functions allocated to evaluating long-term returns, leading to sub-optimal investment decisions.

⁵ The raw correlation between predicted returns on attendance and those on the likelihood of passing 9th grade is -0.0091 .

Our results may help reinterpret important findings from previous economic research. The mechanism we document may at least partly explain the results in Jensen (2010), which finds large effects of an intervention that informs parents about the returns of education in Indonesia on children’s schooling. While the author attributes higher parental investments to belief correction within a Bayesian updating framework, our findings suggest that at least part of that effect could be driven by the fact that the intervention makes long-run returns top-of-mind, regardless of the extent to which beliefs are accurate.

Our findings may also be relevant outside the context of investments in children. In particular, the psychology of poverty may help rationalize the findings of Cohen, Dupas and Schaner (2015), which document systematic misallocation of investments in malaria testing and treatment in Kenya, often undertaken by those who are not sick – with low or even negative returns from doing so –, and not by those who are sick or most at risk – who would benefit the most from such investments. More broadly, poverty-induced focus on short-term cash flows may help explain the puzzling low take-up of preventive health care among the poor as soon as price is above zero (Kremer and Glennerster, 2011).

Last, in what comes to policy implications, our results suggest that popular instruments to boost investments in children among the poor – e.g.: credit lines earmarked for education – may be insufficient to spark such investments. While liquidity constraints were absent from our experimental setup, the poor still failed to undertake profitable investment opportunities. In contrast, adapting the environment to poor individuals’ psychology – either by making decisions automatic, or by making long-term returns top-of-mind – may be key to mitigating the psychological effects of poverty, with the potential to break away from inter-generational poverty traps.

The remainder of the paper is structured as follows. Section 2 provides a brief survey of the evidence on the psychology of poverty, and states its implications for investments in children. Section 3 provides details on the SMS educational program, and summarizes the analysis of its impacts on children’s learning outcomes in Cunha et al. (2018a, 2018b). Section 4 describes the procedure through which we predict individual-level returns of the program on attendance. Section 5 presents our empirical strategy and the results of the survey experiment. Section 6 turns to our analyses of the effects of experience. Next, Section 7 analyzes the effects of making long-term returns top-of-mind, through our information experiment. Section 8 follows by looking back at the previous analyses under an alternative measure of predicted returns of the SMS program: its impacts on students’ likelihood of completing 9th grade. Section 9 concludes the paper.

2 The Psychology of poverty

Accumulating evidence from lab experiments shows that poverty imposes a *psychological tax* on the foundations of decision-making: subjects' attention, working memory and impulse control are systematically lower in the presence of scarcity (Mullainathan and Shafir, 2013). Interestingly, lab experiments also show that such effects are not merely driven by mechanisms such as higher stress (Haushofer and Fehr, 2014), but arise out of mental bandwidth reallocation towards scarcity at hand, generating focus on that dimension at the expense of other dimensions (Shah, Mullainathan and Shafir, 2012).

In contrast, evidence from the field about the psychological effects of poverty is still scarce. Mani et al. (2013) document that, among sugarcane farmers in India, poverty significantly affects cognitive performance. Taking advantage of quasi-random variation in the timing at which farmers sell their harvest to the sugar mill, and exploring within-farmer variation in performance in attention and working memory tests before harvest – when they are cash poor – and after harvest – when they are flush with cash –, the authors find that poverty significantly affects brain's executive functions. Effect sizes are dramatic: farmer's performance in Raven's matrices, widely used to measure intelligence, implies that poverty drops farmers' IQ from normal to *cognitively impaired*. Having said that, how do we know that those effects are driven by mental bandwidth, or alternatively by other mechanisms induced by poverty, such as differential nutrition due to lower skipped meals after harvest?

In turn, Carvalho, Meyer and Wong (2016) do not find significant effects of payday variation in cognitive performance among the unemployed in the United States. Randomly assigning the timing at which subjects get payed in an online survey which involved attention, working memory and impulse-control tests, the authors document that performance before payment is not significantly different from that after payment.

One crucial distinction between the experiments in India and the US is the presence of risk. Being promised a later payment in an online survey may not trigger financial worries to the extent that it does if a monopsonist has not yet purchased one's harvest. And both risk and levels are fundamental dimensions of the lives of the poor.

To test which out of low levels or risk is the fundamental mechanism behind the psychological effects of poverty, Lichand and Mani (2018) combine survey experiments and natural experiments coming from rainfall shocks and payday variation in Brazil's flagship conditional cash transfer, assessing the effects of different drivers of financial worries on family farmers' cognitive performance in lab-in-the-field attention, memory and impulse-control tests. The authors show that while both priming farmers about the risk of

droughts and negative rainfall shocks adversely affect their cognitive performance, conditional cash transfers' payday variation does not. Nevertheless, all shocks affect focus: priming and recent rainfall shocks make enhance farmers' focus on attention and memory tasks related to water, while the lag to payday takes farmers' focus away from those. Together, results suggest that the psychology of poverty actually taxes the foundations of decision-making, but the *fundamental driver of cognitive effects is risk*, rather than just lower availability of resources.

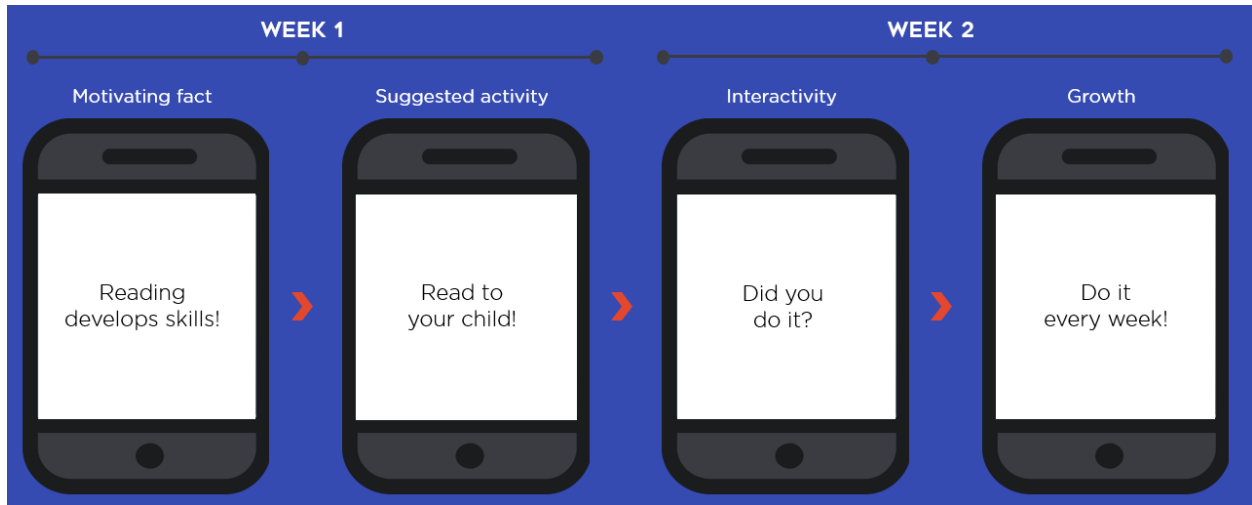
While brain's executive functions are expected to influence all decisions, there is still no causal evidence on whether the psychology of poverty actually impacts real economic decisions, in particular those that could generate poverty traps. Dean (2018) documents the effects on noise on cognitive function and productivity within factories in Kenya. While noise is shown to affect both productivity and performance in attention and memory tests, it is unclear the extent to which this provides clean evidence of economic consequences for the mental bandwidth mechanism induced by poverty. While Lichand and Mani (2018) analyze the psychological effects of poverty on farmers' demand for information about credit and insurance products, their experiment has limited statistical power to detect small effect sizes.

This is the first paper to document how the psychology of poverty translates into real economic decisions, and in particular into one that is likely to affect inter-generational transmission of poverty.

3 The SMS educational program

The educational program we offer parents in the context of our experiment (Eduq+, powered by the Brazilian social impact startup MGov), delivers nudges via text messages (SMS) directly to caregivers' mobile phones. Content is organized in thematic sequences comprised of four messages, with two messages delivered each week. Inspired by READY4K! (York, Loeb and Doss, 2018), sequences start with a motivating fact, followed by a suggested activity – which is always non-curricular, as it is often the case that students' educational achievement is higher than that of their parents. In the following week, caregivers receive an interactive message, posing them a question linked to the activity suggested the week before. The last message (“growth”) is meant to nudge caregivers towards making it a habit to follow students' school life more closely.

Figure – Sample sequence of the SMS educational program



In Cunha et al. (2018b), a differences-in-differences strategy exploiting the absence of differential trends before the onset of the treatment documents that the SMS program we offer parents in the context of this paper increases attendance by 3 percentage points in Math and Language classes (or about 3 extra classes within 6 months), and improves Math test scores (according to teachers’ report cards) by 0.12 standard deviation.

A variation of the program, which delivered 1 SMS per week to caregivers focused on a specific set of suggested activities – namely, nudging parents to pay close attention to students’ attendance in Math classes –, was randomly assigned against a pure control group in Cunha et al. (2018a), allowing the authors to assess its effects on standardized test scores and grade repetition rates. Authors show that the program increases Math attendance by 2 percentage points, standardized test scores by 0.095 standard deviation (equivalent to about one quarter ahead in school performance), and the likelihood of advancing to high-school by 3 percentage points (implying a return rate of over 1,000% on dollar invested by the Government just in what comes to expected savings due to less students failing 9th grade).

4 Predicting heterogeneous treatment effects

Even the 1-message a week version of the SMS program was shown to have large average impacts on students’ educational outcomes. Beyond averages, for the purposes of this paper we are interested in predicting *heterogeneous treatment effects*, such that we can draw upon an objective measure of returns on investment *for each family* in our experiment.

To do that, we follow Athey and Imbens (2017) in implementing regression trees to partition the sample into subgroups driven by impact heterogeneity of Cunha et al. (2018a)’s experimental results. The reason we focus on the 1-message a week version of the SMS program is that only this version was randomly assigned against a pure control group, allowing us to predict heterogeneous treatment effects for grade promotion rates (which are only measured once a year, hence unavailable for the empirical strategy relying on differential trends over time in Cunha et al., 2018b).

The regression tree estimation algorithm trades off *goodness-of-fit* against *over-fitting* to fine-tune the depth of the tree, that is, the complexity of how it partitions the data to predict individual-level treatment effects. We feed the algorithm with Math attendance data and all student and family characteristics from baseline data for the over 10,000 subjects of the experiment in Cunha et al. (2018a).

We estimate family-level predicted impacts of the program, $\hat{\beta}_i$, as a function of student’s race, age and gender, and of caregiver’s race, age, gender, income and schooling:

$$\hat{\beta}_i \sim f(\text{black}_i, \text{caretaker_black}_i, \text{age}_i, \text{caretaker_age}_i, \text{mother}_i, \text{girl}_i, \text{poor}_i, \text{lowS}_i) \quad (1)$$

In equation (1), $\text{poor}_i = 1$ if household income is below 1 minimum wage, and $= 0$ otherwise; and $\text{lowS}_i = 1$ if the primary caregiver is a primary-school drop-out or never went to school, and $= 0$ otherwise.

Figure 1 displays the regression tree estimated following this procedure.⁶ As shown, the algorithm picks caregivers’ age as the main dimension on which to partition the dataset, further allowing impacts to vary by student’s gender in the case of caregivers between 33 and 40 years old (girls benefit more than boys, with 1 extra absence reduced by the program over the course of 6 months).

[Figure 1]

Some of the patterns generated by the regression tree can be attributed intuitive interpretations.⁷ The oldest caregivers – those 48 years old and above – are the ones who most benefit from the SMS program in what comes to its effects on their children’s attendance (about 3 less absences predicted over the course of 6 months), perhaps because they are less connected to technology and allocate large attention to the messages received. At the other end, the youngest caregivers – those younger than 33 years old, with children 14 or above – do not benefit at all from the SMS program in what comes to its effects on attendance (no predicted

⁶ R code available upon request.

⁷ Having said that, there is no reason to expect that predictions from machine learning algorithms provide interpretable results, especially under a richer feature space.

reduction in absences), perhaps because their mental bandwidth available for children is so constrained that they are infra-marginal with respect to the effects of the program.

For our purposes, predicted heterogeneous treatment effects of the SMS program on students' attendance provide an objective measure of returns on investment that we can use to analyze the psychological effects of poverty moving forward.

In Section 8, we draw on the same procedure to generate predicted heterogeneous treatment effects of the SMS program on students' likelihood of advancing to high-school. We briefly summarize the results of the estimated regression tree for this case in that section.

5 Does the psychology of poverty affect parental investments in children's human capital?

This section is structured as follows. Subsection 5.1 introduces the empirical strategy of our survey experiment. Next, subsection 5.2 describes our sample, followed by a discussion of balance and selective attrition tests. Subsection 5.3 follows by showcasing how the investment decision changes with cross-sectional variation in family income, to motivate our experimental analysis. Subsection 5.4 presents manipulation checks to assess whether the priming affects financial worries, as it was designed to do. Next, subsection 5.5 turns to the effects of the priming on the investment decision as a function of the returns of the SMS program, followed by a discussion of the evidence for the mental bandwidth mechanism in subsection 5.6.

5.1 Empirical strategy

To isolate the causal effects of the psychology of poverty on parents' investment decision, we resort to a survey experiment. We endow over 2,500 parents of high-school freshmen with R\$ 10 (about USD 3) in exchange for answering the phone survey that offers them the opportunity to invest this endowment in hiring an SMS educational program over the next 6 months. The provided endowment is meant to rule out liquidity constraints from our experimental setting, allowing us to focus on the psychological consequences of poverty separate from other constraints.

At the beginning of the phone survey, we ask parents what they would do if their child's school started charging a high amount for textbooks and uniforms (R\$ 400, or about USD 130), and that amount were due at the end of that month. Following previous studies (Mullainathan and Shafir, 2013), the control group is asked a variation of the same question, in which the only difference is that schools are said to charge a low

amount instead (R\$ 20, or about USD 6.50). See Appendix B for the full script of treatment and control messages.

This technique (what cognitive psychologists call *priming*) is meant to emulate the psychological feeling of having financial worries constantly *top-of-mind* among the poor. For this reason, we call the control group *experimentally rich*, and the treatment group, *experimentally poor*. While priming studies have been criticized for generating effects that are “not robust”, Lichand and Mani (2018) show that the psychological effects of survey experiments along the lines we conduct in this paper match those of real income shocks, such as rainfall or random variation in payday of conditional cash transfers among the poor.

Taking advantage of experimental variation in the psychology of poverty, we then analyze how it affects parent’s willingness to invest in their children’s education. See Appendix A for the full script of how the investment decision was framed. Since we actually implement parent’s decisions, what we document are real consequences of the psychology of poverty for human capital investments in children. Parents who decide not to invest in the program have no future opportunity of doing so, and the endowment is converted into airtime credit which they cannot transfer or use to make purchases in Brazil.

We estimate the following equations:

$$Y_i = \alpha + \beta T_i + \sum \gamma_k X_{k,i} + \varepsilon_i \quad (2)$$

$$Y_i = \alpha + \beta_0 T_i + \beta_r (T_i \times R_i) + \theta R_i + \sum \gamma_k X_{k,i} + \varepsilon_i \quad (3)$$

In equations (2) and (3), $Y_i = 1$ if caregiver i decides to take up the SMS program, and $= 0$ otherwise; $T_i = 1$ if caregiver i is primed about financial worries, and $= 0$ otherwise; X_i is a vector of student and family characteristics, and ε_i is an error term. In equation (3), R_i is the predicted return of the program in terms of lower absences ($R_i \in \{0,1,2,3\}$), following the procedure described in Section 4.

We are interested in whether the psychology of poverty decreases the average willingness to invest in the program (β), but also in how it changes willingness to invest when the program has no returns (β_0) and for each additional absence the program is predicted to decrease (β_r).⁸

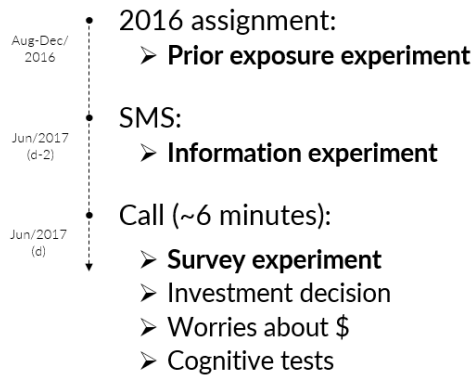
⁸ We have pre-registered an earlier version of our analysis plan at <https://www.socialsciceregistry.org/trials/1380>. While we have abandoned random variation in how we framed the investment decision after piloting the survey, the underlying principle was implemented through the information experiment.

This survey experiment is combined with a prior exposure experiment, to shed light on the effects of experience (see Section 6), and an information experiment, to document the effects of making long-term returns top-of-mind (see Section 7).

We first restrict attention to the sub-sample for whom no information was provided over SMS before the phone survey. To avoid triple interactions – which are hard to interpret –, we investigate the effects of experience and those of making returns top-of-mind later on by estimating equations (2) and (3) separately for different sub-samples.

The timeline for the different experiments is outlined in the figure below. The prior exposure experiment takes place nearly one year before the survey experiment, with communication with caregivers in the context of the SMS program taking place over the course of the following 20 weeks. The information experiment takes place 2 days before the survey experiment. Last, the phone survey that starts with the survey experiment takes about 6 minutes. Following the initial question (see Appendix B), we start by eliciting subjects investment decision (subsection 5.5), followed by manipulation checks to document financial worries (subsection 5.4), and cognitive tests (subsection 5.6).

Figure – Timeline



Among caregivers who decide to exchange the R\$ 10 endowment for the SMS program, communication takes place over the course of 20 weeks, starting in August/2017.

5.2 Sample, balance and selective attrition

Our sample design cross-randomizes subjects to the priming and information treatments, stratifying assignment to each cell by predicted returns of the SMS program on attendance and by treatment condition in Cunha et al. (2018a)’s experiment. We draw from 15,574 subjects, not having included subjects from the

pure control group in the previous experiment so as to ensure that every participant in our experiment has some prior experience with the SMS program.

Figure – Sampling frame

		Survey experiment	
		Not Primed	Primed
Information	No	Rich (3,873)	Poor (3,904)
	Yes	Rich ROI top-of-mind (3,944)	Poor ROI top-of-mind (3,853)

Out of the 15,574 subjects, we successfully re-contacted 2,558 caregivers in 2017, distributed quite symmetrically across assignment cells.

Figure – Sample design

		Survey experiment	
		Not Primed	Primed
Information	No	Rich (663)	Poor (638)
	Yes	Rich ROI top-of-mind (634)	Poor ROI top-of-mind (623)

We have basic baseline information on families’ their children’s characteristics, coming from baseline data collection. 41% of caregivers (and 39.7% of students) are black or brown. Their average age was 39 years old (and that of students, 14.7) at the time of enrollment, in 2016. 50.3% of students are girls, and 80% of caregivers are mothers.

Most of enrolled families are poor: 25.6% of them are primary school dropouts or never went to school; 16.4% of them live in households with income below 1 minimum wage (or about 300 USD), and another 50% make at most 3 minimum wages (which, in São Paulo State, where this study takes place, could apply to some families living in slums). While families making more than 3 minimum wages are by no means rich in our setting, we take advantage of differences in intensity of financial burden these families face for the cross-sectional analysis in the next subsection.

We next undertake balance and selective attrition tests with respect to each of the experiments we draw upon in our analyses. Because take-up of the phone survey is low – only 10.5% of the universe picks up and answers to the call through the end (see Table 2) –, even though treatment conditions were randomly assigned across all subjects, it may be the case that the sample we end up with is selected, or unbalanced with respect to student or family characteristics.

Table 1 shows that no student or family characteristics are statistically different at the 10% level across the treatment and control groups in our survey experiment, consistent with random assignment. For this reason, controlling for student and family characteristics should increase the precision of the estimates of the effects of priming, by reducing residual errors, but should not affect its point estimates. Having said that, Table 1 also shows that, in what comes to the information experiment, there are significantly more girls in the treatment condition (52% vs. 49% in the control group, a difference statistically significant at the 10% level) and a lower share of subjects earning less than 1 minimum wage (15% vs. 18% in the control group, significant at the 5% level). Since we split the sample according to whether subjects received information before the survey experiment, such differences may become relevant when we analyze the effects of information, and for this reason we control for all student and family characteristics in all our estimates.

[Table 1]

Most importantly, Table 2 shows that, despite low take-up of the phone survey, attrition is not selective with respect to any of the treatments. Some student and family characteristics are systematically associated with attrition: mothers are 1.7 percentage points more likely to take up the survey, low-schooling caregivers are 2.8 percentage points less likely to take it up, and younger caregivers are more likely to take it up (a 30 year-old parent is 5.4 percentage points more likely than a 60 year-old). No treatment status leads caregivers to systematically take up the phone call through the end: differences are not only statistically insignificant at the 10% level, but also very small across all experimental conditions.

[Table 2]

5.3 Correlation of investment decisions with family income

To motivate our experimental results in the following subsections, we start by analyzing how the decision to invest in the SMS program as a function of its predicted returns varies across poor and rich families in our sample. Figure 2 contrasts willingness to invest amongst families earning over 3 minimum wages (the *rich*, shown on the left panel) and that amongst those earning less than 1 minimum wage (the *poor*, shown on the right panel).

[Figure 2]

Among the rich, a linear approximation of the demand for the SMS program slopes upward with its predicted returns. According to that approximation, about 42% of caregivers are willing to invest even when the program has no predicted effects on their children’s attendance, and demand increases by 1.5 percentage points per absence reduced by the program.

In contrast, among the poor, not only is demand for the SMS program (also approximated by a linear function) below that of the rich for every return category – what is not surprising, for poverty may decrease demand for costly investments for a variety of reasons –, but also, strikingly, it slopes *downward* with its predicted returns. According to that approximation, about 40% of caregivers are willing to invest even when the program has no predicted effects on their children’s attendance, and demand *decreases* by 1.0 percentage point per absence reduced by the program

Of course, just comparing demand schedules across poor and rich families does not necessarily convey the psychological effects of poverty on investment decisions. There are many other differences between those families, including the extent to which they may expect different labor market returns to attending classes, which may result in different optimal investment schedules. For this reason, we turn to our experimental results in the following sections.

5.4 Financial worries

We start by presenting the results of manipulation checks, aimed at verifying that the priming indeed increases financial worries as intended. See Appendix A for the full script of how the question about financial worries was framed.

We estimate the following equation:

$$Y_i = \alpha + \beta T_i + \sum \gamma_k X_{k,i} + \varepsilon_i \quad (4)$$

In equation (4), $Y_i = 1$ if caregiver i states to be very worried or desperate about household bills due by the end of the month, and $= 0$ otherwise; $T_i = 1$ if caregiver i is primed about financial worries, and $= 0$ otherwise; X_i is a vector of student and family characteristics, and ε_i is an error term.

Figure 3 displays the distribution of very worried or desperate caregivers across conditions of the survey experiment. In the control group, 56.8% of subjects fall in that category, in contrast to 65.1% in the treatment group. Column (1) in Table 3 confirms that the 8.3 percentage-point difference is estimated with precision, statistically significant at the 1% level.

[Figure 3]

[Table 3]

Results are robust to controlling for student and family characteristics, as well as to including indicators for each predicted return category. Since the priming works as expected, we can move on to assess its effects on caregivers' decision to invest in their children's human capital.

5.5 Investment decision

In this subsection, we analyze results restricting attention to the subsample assigned not to receive information about the predicted returns of the SMS program. Column (1) of Table 4 estimates equation (2), assessing the average effect of the psychology of poverty on caregivers' demand for the program. Columns (2) and (3) estimate equation (3), assessing its effects separately on caregivers' demand for the program when it is predicted to have no returns (the *intercept*), and on how demand changes with each additional absence that the program is predicted to reduce (the *slope*).

[Table 4]

Column (1) documents that the experimentally poor are no less willing to invest in the program than the experimentally rich, on average. Columns (2) and (3) reveal, however, that the absence of an average effect is the product of two stark differences between the experimentally poor and rich. First, the experimentally poor are *much more likely to invest* in the program when it is *predicted to have no effects* (35.2% amongst the rich, and 46.2% amongst the poor). Second, while demand increases with predicted returns among the experimentally rich (5 percentage points per absence reduced), it *decreases with predicted returns* among the experimentally poor (4.5 percentage point per absence reduced). All effects are estimated with precision, statistically significant at the 5% level or lower. What is more, estimates are robust to controlling for student and family characteristics and including fixed effects for each return category of the SMS program.

Figure 4 is the graphical counterpart of Table 4's results. While it preserves Figure 2's feature that demand slopes negatively with program's returns among the poor, in Figure 4 the differences in intercepts and slopes across the experimentally rich and poor can be attributed causal interpretation: those must be due to the psychology of poverty.

[Figure 4]

5.6 Mechanism: focus on short-term returns

Next, we investigate whether these effects operate through mental bandwidth. To do so, we undertake audio versions of cognitive tests, following Lichand and Mani (2018). The cognitive outcomes we measure comprise tasks aimed at assessing working memory, attention and impulse control (so called brain's executive functions; Diamond, 2013). The motivation for looking at executive functions is that those are the foundations of decision-making.

We measure working memory through digit span tests, in which subjects must remember as many digits as they can from the numbers they hear (the more digits accurately recalled, the higher the score). We measure attention and impulse control through stroop tests, in which subjects must answer the number of times they heard a particular digit repeated in a sequence. While it is tempting to press the digit that he or she just heard repeated multiple times, the correct answer is never the digit itself. See Appendix A for the full script of the cognitive tests we submit our subjects to.

Such cognitive tests are incentivized in our phone survey: subjects are told they can earn extra R\$ 2 (about 0.70 USD) if they are amongst top-performers in those tests. The goal of setting it up like that is

creating the opportunity for short-term gains, enabling us to test the hypothesis that the priming about financial worries induces enhanced focus on short-term returns.

For each cognitive test j , we estimate the following equation:

$$Y_i^j = \alpha + \beta_j T_i + \sum \gamma_k X_{k,i} + \varepsilon_i \quad (5)$$

In equation (5), in the case of digit span, Y_i^j is given by the share of accurately recalled digits, across all versions of the test; in the case of stroop, Y_i^j is given by the share of correctly identified number of repetitions, across all versions of the test; $T_i = 1$ if caregiver i is primed about financial worries, and $= 0$ otherwise; X_i is a vector of student and family characteristics, and ε_i is an error term.

Since we conduct a multiplicity of tests, estimating separate regressions for each outcome would substantially inflate the probability of false positives above stated significance levels. For this reason, we build summary measures for each set of outcomes and for cognitive load, following Kling, Liebman and Katz (2007). To do that, first we normalize all outcomes to z-scores. Second, following Kling and Liebman (2004), we run seemingly unrelated regressions (SUR) to compute an effect size $\hat{\beta}$ for each summary measure, given by equation (6):

$$\hat{\beta} = \frac{1}{K} \sum_{j=1}^K \frac{\hat{\beta}_j}{\hat{\sigma}_{j_c}} \quad (6)$$

In equation (6), $\hat{\beta}_j$ are the point estimates obtained for ordinary least squares (OLS) regressions of Y^j on a particular treatment variable, $\hat{\sigma}_{j_c}$ is the variance of that outcome for the control group, and K is the number of outcomes in that category. We use bootstrapping to obtain standard errors for $\hat{\beta}$.

Results for the summary measure of cognitive outcomes can be intuitively visualized in Figure 5. The distribution of cognitive performance for experimentally poor subjects (in red) lies to the right of that of experimentally rich (in blue). Results in Table 5 confirm that, consistent with the focus mechanism, experimentally poor subjects perform significantly better in those tests: the priming improves cognitive performance (measured by the summary measure) by 0.1 standard deviation – about half the effect of having attended school beyond primary education on cognitive performance. Effects are precisely estimated, statistically significant at the 5% level, are robust to the inclusion of student and family characteristics and

program's return category fixed effects, and are equally driven by subjects' performance in stroop and digit span tests.⁹

[Figure 5]

[Table 5]

While primed subjects often fare worse in attention and memory tests in prior studies (Mullainathan and Shafir, 2013), what is key in our setting is the combination of priming subjects *about financial worries* and submitting them to *incentivized* cognitive tests. The enhanced focus effect we find is consistent with results in Shah, Mullainathan and Shafir (2012) and Lichand and Mani (2018), and is distinctive about the mental bandwidth mechanism.

All in all, the evidence indicates that the psychology of poverty induces subjects to focus on short-run returns, at the expense of properly evaluating long-term returns on investment.

6 Does experience help?

This section is structured as follows. Subsection 6.1 introduces the empirical strategy of our prior exposure experiment. Next, subsection 6.2 describes the results.

6.1 Empirical strategy

A common criticism of Behavioral Economics is that enough market experience would eliminate such failures of rationality. To test this hypothesis, we take advantage of another experiment (Cunha et al., 2018a). In that study – conducted over the course of 20 weeks, in the year before our experiment –, the authors randomly assigned parents to either 1 SMS per week with a child-specific message, or to a control group, which received *at most* 1 school-wide SMS per month with communication about school activities. We call the former group *high-experience* in the context of our experiment.

⁹ The higher precision of the estimates for the summary measure is a common feature of this setup (also seen in Lichand and Mani, 2018), due to the fact that performance in different tests often displays low correlation (0.18 in our sample).

Manipulation checks at the end of the 20 weeks confirm that parents assigned to the treatment group are much more likely to recall receiving text messages (89.8%, versus 46.4% in the control group, a statistically significant difference at the 1% level; Cunha et. al, 2018a).

In order to test the hypothesis of whether experience mitigates the psychological effects of poverty on investment decisions, the next subsection re-estimates equations (2) and (3) restricting attention to the *high-experience* sub-sample for whom no information was provided over SMS before the phone survey.

6.2 Results

Results are displayed in Table 6. Column (1) documents the average effects of the psychology of poverty on the demand for the SMS program. While for the uninformed subsample as a whole the priming had no average effect on demand, amongst high-experience subjects it significantly *decreases* willingness to invest. The reason becomes clear in columns (2) and (3). Amongst high-experience caregivers, the significantly higher intercept for the experimentally poor is no longer statistically significant; however, experimentally poor parents' willingness to invest still decreases with returns – 5.6 percentage points per absence reduced by the SMS program, an effect significant at the 5% level (even under a sample about half the size of the previous analyses).

[Table 6]

Figure 6 sheds light on why experience may fail to mitigate the psychological consequences of poverty. While experience shifts willingness to pay upwards – making the experimentally poor 10 percentage points more likely to undertake the investment for all return levels –, it does not change the negative slope of willingness to pay with respect to returns among those parents.

[Figure 6]

In sum, we reject the hypothesis that experience mitigates the psychological effects of poverty on investment decisions. The supplementary appendix complements the analyses by showing that experience does not systematically affect focus on short-term returns, what may be key to mitigating the psychological consequences of poverty (see Section 7).

Taking advantage of the fact that the priming now has significant average effects – among high-experience subjects, the experimentally poor are 6.6 percentage points less willing to invest than the experimentally rich, across all return categories – we can now try to benchmark its effect sizes. How large is this effect?

Table 7 displays the average effect of priming on demand for the SMS program, along with raw correlations between willingness to invest and the few family’s socio-economic variables we have from baseline data. As a benchmark, poor individuals (those below 1 minimum wage) in our sample invest 5.9 percentage points less than rich ones (above 3 minimum wages), across all return categories.

[Table 7]

While it is unlikely that the psychology of poverty accounts entirely for the lower propensity to invest in children’s education among the poor, our estimate lies within a reasonable range for cross-sectional differences arising from variation in family profiles. What is more, if priming studies have been criticized for generating effects that are not robust, Lichand and Mani (2018) show that the psychological effects of survey experiments along the lines we conduct in this paper match those of real income shocks, such as rainfall or random variation in payday of conditional cash transfers among the poor.

7 Making long-term returns top-of-mind

This section is structured as follows. Subsection 7.1 introduces the empirical strategy of our information experiment. Next, subsection 7.2 describes the results.

7.1 Empirical strategy

If mental bandwidth is the mechanism behind the psychological effects of poverty, then a key implication is that such effects should be mitigated by making long-run returns top-of-mind. We test this hypothesis by cross-randomizing parents to information about the predicted returns of the SMS program. Half of our subjects are informed over SMS about the predicted effect of the program on their child’s absences, 2 days before the phone survey; the other half (the control group) receives a neutral message, which just states the number of families who participated in the program in the previous year.

In order to test the hypothesis of whether making long-term returns top-of-mind mitigates the psychological effects of poverty on investment decisions, the next subsection re-estimates equations (2) and (3) restricting attention to the sub-sample for whom information was provided over SMS before the phone survey.

Before we turn to the investment decision, however, we estimate the effects of information on focus to as a manipulation check of whether it indeed makes long-term returns top-of mind:

$$Y_i^j = \alpha + \beta_j T_i + \sum \gamma_k X_{k,i} + \varepsilon_i \quad (5')$$

In equation (5'), in the case of digit span, Y_i^j is given by the share of accurately recalled digits, across all versions of the test; in the case of stroop, Y_i^j is given by the share of correctly identified number of repetitions, across all versions of the test; $T_i = 1$ if caregiver i is informed about the predicted returns of the program on student i 's attendance, and $= 0$ otherwise; X_i is a vector of student and family characteristics, and ε_i is an error term. Once again, we resort to equation (6), drawing upon a summary measure of cognitive performance to account for family-wise error rate across multiple outcomes.

7.2 Results

Results for the summary measure of cognitive outcomes can be intuitively visualized in Figure 7. The distribution of cognitive performance for subjects informed over SMS (in blue) lies to the left of that of the control group (in red). Results in Table 8 confirm that, also consistent with the focus mechanism, subjects randomly assigned to receiving information about long-term returns perform significantly *worse* in incentivized cognitive tests: the treatment deteriorates cognitive performance (measured by the summary measure) by about 0.1 standard deviation – almost exactly the reverse of the effect of the priming. Effects are precisely estimated, statistically significant at the 5% level, and are robust to the inclusion of student and family characteristics and program's return category fixed effects.

[Figure 7]

[Table 8]

The information treatment does not systematically affect financial worries¹⁰, hence providing a clean test of the focus mechanism behind the psychological effects of poverty on investment decisions. Table 9 displays the results for the psychological effects of poverty on investment decisions, restricting attention to the sub-sample for whom information was provided over SMS before the phone survey. Columns (1) through (3) document that, consistent with the focus mechanism, within parents informed about predicted returns there are no differences across experimentally rich and poor in what comes to the demand for the program: neither the intercept nor the slope of willingness to pay with respect to returns of the program are systematically affected by the priming.

[Table 9]

Figure 8 sheds light on why making long-term returns top-of-mind mitigates the psychological consequences of poverty. Information almost completely reverses the negative slope of willingness to pay with respect to returns among experimentally poor caregivers.

[Figure 8]

Together, results document that receiving the text message prior to the call makes long-term returns top-of-mind, preserving parent's ability to evaluate such returns even under higher financial worries.

8 Alternative measure of predicted returns

Can we distinguish salience of long-term returns from other effects information? To do that, we resort to an alternative measure of returns of the SMS program: its predicted effects on the likelihood of the student advancing to high-school.

This strategy has two advantages. First, advancing to high-school is much more consequential than having fewer absences, directly mapping into higher expected earnings in the future. If the psychology of poverty still makes willingness to invest decrease with predicted returns in that case, then it would make

¹⁰ Results not show and available upon request.

for a much stronger case of its connection to potential poverty traps. Second, because we informed (half of) parents only about the predicted effects of the SMS program on *attendance* – but *not* on the likelihood of advancing to high-school –, then the effects of information in that case provide a clean test of salience of long-term returns as opposed to other potential effects of information, since the raw correlation between the predicted returns of the SMS program on attendance and those on the likelihood of passing 9th grade turns out to be nearly zero (−0.0091).

A regression tree based on student and family characteristics predicts that, for some families, the program does not affect the probability of completing 9th grade; for other families, the program increases it by 2, 3 or 4 percentage points. Following the same logic of equation (1), the algorithm picks caregiver’s schooling as the main dimension on which to partition the dataset, further allowing impacts to vary by student’s gender and by caregiver’s race.

At one extreme, boys whose caregivers are primary dropouts or never went to school are predicted not to benefit from the SMS program in what comes to their likelihood of passing 9th grade; at the other extreme, children of black caregivers whose educational achievement is complete primary education or higher are the ones who benefit the most from the SMS program, with a 4 percentage points higher likelihood of advancing to high-school.

We repeat the analyses of the effects of priming, experience and information on investment decisions, under this alternative measure of returns. Results are summarized in Table 10, which is divided in three panels: Panel A restricts attention to the subsample not informed about program’s predicted returns on attendance via SMS; Panel B restricts attention to the high-experience subsample not informed about program’s predicted returns on attendance via SMS; and Panel C restricts attention to the subsample informed about program’s predicted returns on attendance via SMS.

Although less precisely estimated, and understandably less stable in the presence of program’s return category fixed-effects (since our treatment conditions were not stratified by program’s effect on the likelihood of passing 9th grade), results are strikingly similar to those documented in the previous analyses. Panel A shows that the priming *decreases* willingness to invest in the SMS program by 4.5–5.5 p.p. per additional point that the program increases in student’s probability of advancing to high-school. Panel B shows that experience does not mitigate that effect: priming is estimated to *decrease* demand by 5.6–7.0 p.p. per additional point that the program increases in student’s probability of advancing to high-school, among high-experience subjects. Last, Panel C shows that making long-term returns top-of-mind makes financial worries inconsequential for investment decisions: priming has no effect on demand for the program among subjects informed about its predicted effects on attendance.

[Table 10]

Figure 9 highlights how using this alternative measure of predicted returns yields the same empirical regularities that we found before. Experimentally poor parents' willingness to invest slopes downward with respect to predicted returns – while it slopes upward amongst the experimentally rich (Panel A). While experience does not reverse demand's negative slope with respect to returns on investment (Panel B), information about long-term returns does (Panel C) – even though it refers to a different, uncorrelated dimension.

[Figure 9]

Such patterns confirm that focus is the fundamental mechanism behind those effects, and that the psychology of poverty has the potential to generate poverty traps.

9 Discussion and concluding remarks

Within the context of a large-scale field experiment in which parents were offered the opportunity to invest in an SMS educational program, we have shown that the psychology of poverty induces subjects to focus on short-term returns at the expense of accurately evaluating long-term returns, leading to sub-optimal investment decisions. This is the first paper to document that such mechanism can affect real economic decisions, and in particular those linked to investments in children's human capital – likely to affect inter-generational transmission of poverty.

Our findings may at least partly explain the results in Jensen (2010), since the effects of disclosing information to parents about the returns to schooling could be driven by returns being brought to the top-of-mind, rather than by an optimal response to more accurate beliefs. Even outside the context of investments in children, the psychology of poverty may help rationalize the findings of Cohen, Dupas and Schaner (2015), which document systematic misallocation of investments in malaria testing and treatment in Kenya. More broadly, poverty-induced focus on short-term cash flows may help explain the puzzling low take-up of preventive health care among the poor as soon as price is above zero (Kremer and Glennerster, 2011).

For social impact startups – which recently emerged as an important resource for the poor in settings where market-based solutions have the potential to fill in the gaps in Government services –, these observations have welfare implications in what comes to business-to-consumers (B2C) models. Leaving it for the poor to decide when to take up investments in children under cognitive load and financial worries is likely to induce misallocation. Business-to-business (B2B) or business-to-government (B2G) models that distribute such solutions at no cost to the poor (their collaborators and clients, in the former, or citizens, in the latter) have higher potential to promote better targeting and positive social impact.

In what comes to policy implications, our results suggest that popular instruments to boost investments in children among the poor, such as credit lines earmarked for education, may be insufficient to spark such investments. While liquidity constraints were absent from our experimental setup, the poor still failed to undertake profitable investment opportunities.

In contrast, adapting the environment to poor individuals' psychology – either by making decisions automatic, or by making long-term returns top-of-mind – may be key to mitigating the psychological effects of poverty, with the potential to break away from inter-generational poverty traps.

Moving forward, it would be useful to understand more deeply two features of our results. First, methodologically, it would be crucial to understand the precise conditions under which priming subjects about financial worries leads to better – rather than worse – performance in cognitive tests aimed at assessing brain's executive functions. Since our finding of enhanced focus in those tests under higher financial worries is at odds with those of previous studies, it would be useful to clarify the reasons for such differences. Since enhanced focus is what distinguishes mental bandwidth from competing mechanisms such as stress, being precise about when one should expect to observe enhanced focus rather than cognitive load is essential for the theory to be potentially refutable by evidence.

Second, it would be important to document how the psychology of poverty affects the demand for other investments as a function of their returns. Does the demand schedule always slope downward with respect to returns? This could shed light on the issue of what implications of enhanced focus on short-term returns can be generalized across different domains, and better inform interventions and policies targeted at increasing take-up and improving misallocation.

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Appendix A – Definition of dependent variables

FINANCIAL WORRIES

“How worried are you about not having money to pay all household bills at the end of this month? If you are not worried at all, press 0; if you are somewhat worried, press 1; if you are very worried, press 2; or if you are desperate, press 3.”

INVESTMENT DECISION

“You already earned R\$10 in airtime credit by answering this call until the end. Would you rather exchange those R\$10 for 6 months of weekly text messages about your child’s school life? If you would like to exchange airtime by the text messages, press 1; if you would like to keep the airtime, press 2; or to listen again, press 9.”

FOCUS

Stroop:

“Answer as fast as you can: how many times is number ‘9’ repeated in the following? 9 9 9 9 / 6 6 6 6 6 / 0 0 / 4 4 4 4 4”

Digit span:

“Please type the sequence of numbers as you hear it. 4 8 2 0 5 / 5 2 9 1 7 / 9 7 3 8 1 5 / 0 6 2 7 6 4”

Appendix B – Treatment and control conditions in each experiment

SURVEY EXPERIMENT

Control: “Please tell us after the BIP what would you do if your child’s school started charging R\$ 20 for school uniforms and you had to pay by the end of this month?”

Treatment: “Please tell us after the BIP what would you do if your child’s school started charging R\$ 400 for school uniforms and you had to pay by the end of this month?”

PRIOR EXPOSURE EXPERIMENT

Control: *At most* 1 school-wide SMS every other week with communication about school activities

Treatment: 1 SMS per week from school with child-specific message

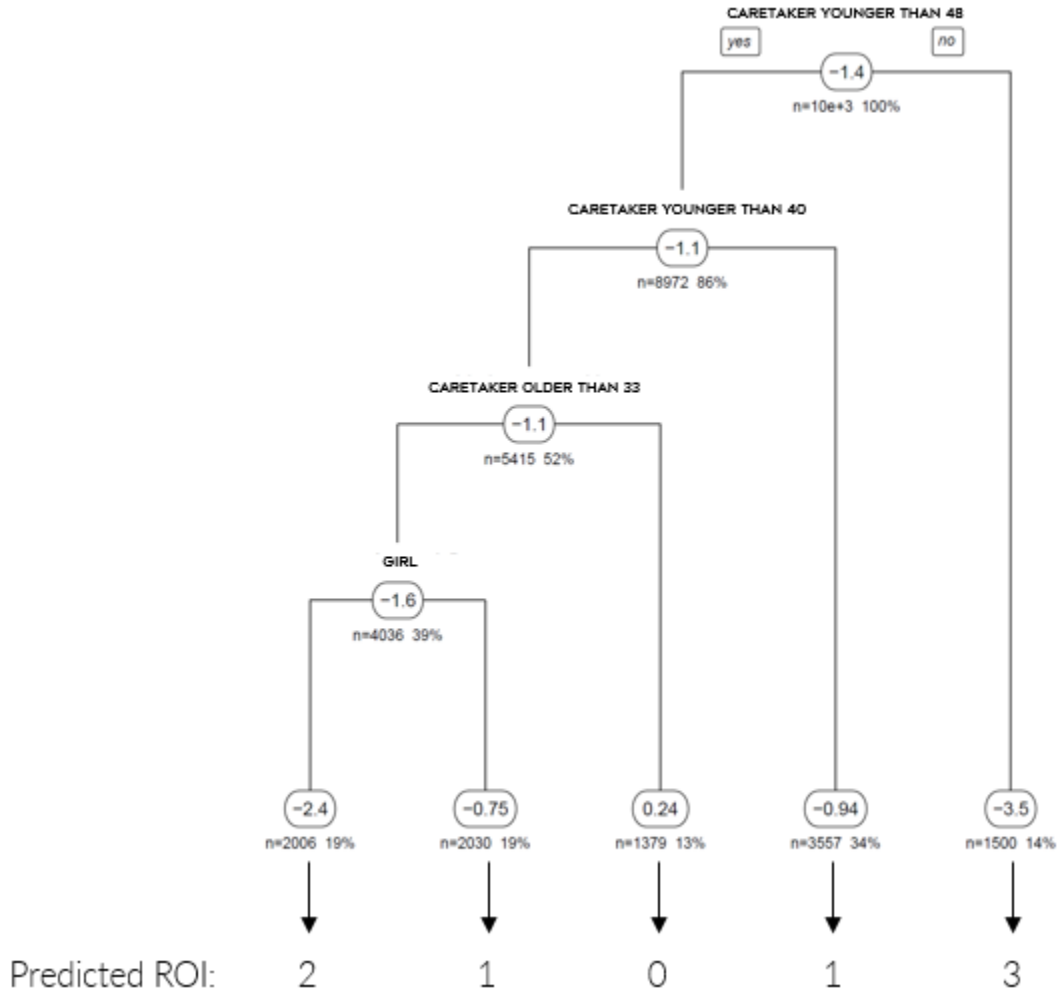
INFORMATION EXPERIMENT

Control: “Last year, 19,000 families in the State of São Paulo participated in the project, receiving weekly text messages about their children’s school life.”

Treatment: “Last year, we found out that sending messages about your child’s school life has the potential to decrease his/her absences by 0/1/2/3 over the course of 6 months.”

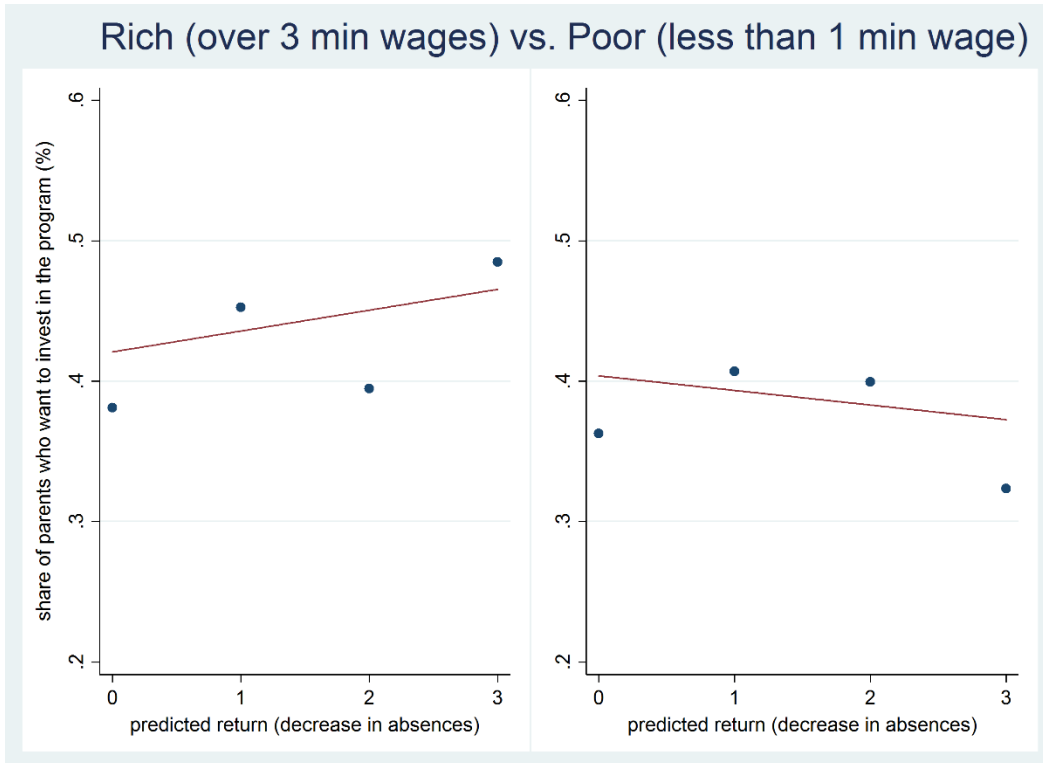
Appendix C – Figures

Figure 1 – Predicted treatment effects of the program on child’s absences within 6 months



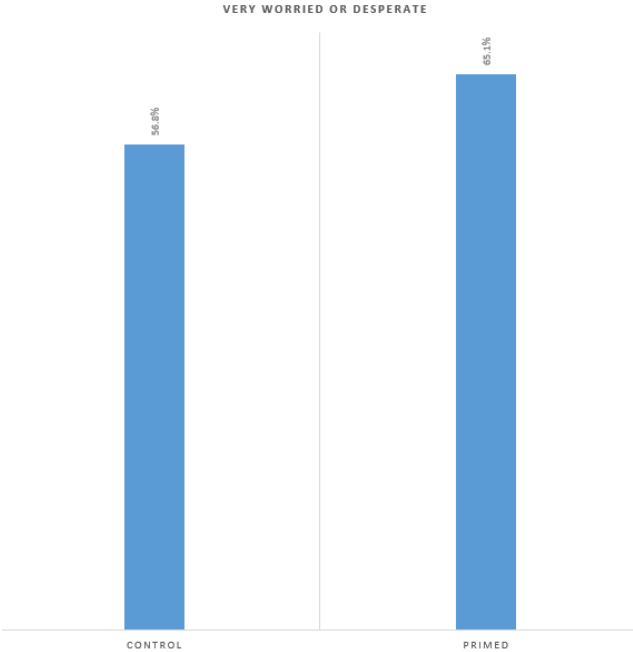
Notes: (1) Predicted treatment effects of the SMS program on attendance, using a regression tree based on the estimates of Cunha, Lichand, Madeira and Bettinger (2018), (2) $\hat{\beta}_i \sim f(\text{black}_i, \text{caretaker_black}_i, \text{age}_i, \text{caretaker_age}_i, \text{mother}_i, \text{girl}_i, \text{poor}_i, \text{lowS}_i)$, where $\hat{\beta}_i$ is the predicted treatment effect for family i , estimated as a function of child’s and caretaker’s race, age and gender, of family’s socioeconomic status and of caretaker’s education; (3) Parameters of the tree estimate available upon request.

Figure 2 – Investment decision as a function of predicted returns by income status



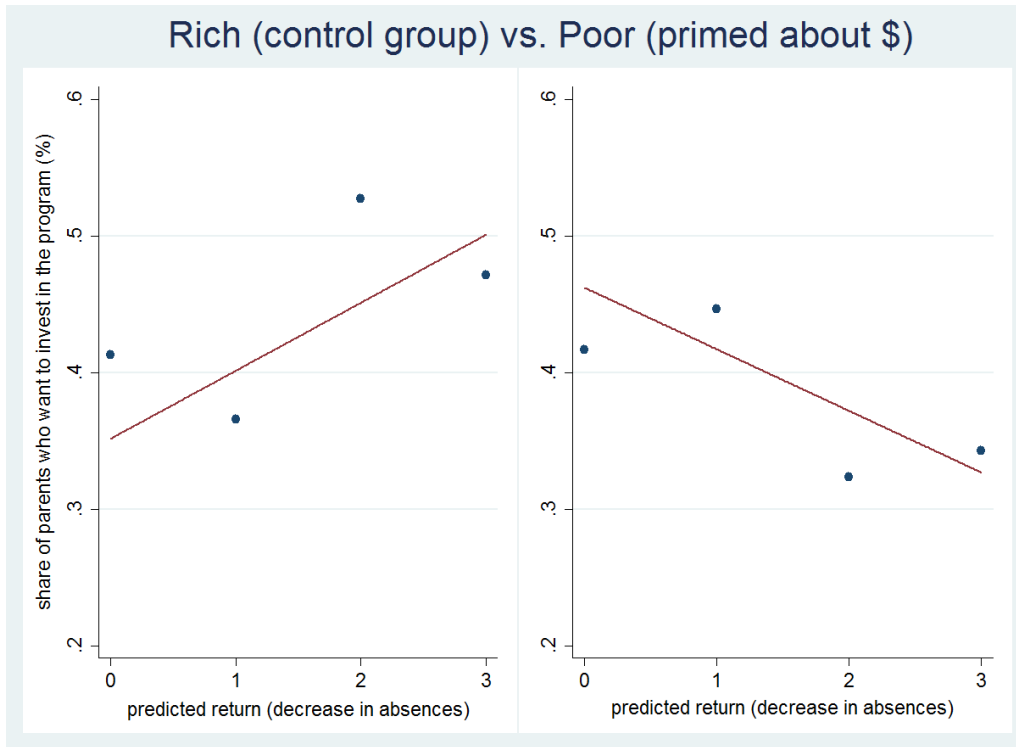
Notes: (1) Predicted returns = predicted treatment effects of the SMS program on attendance, using a regression tree based on the estimates of Cunha, Lichand, Madeira and Bettinger (2018); (2) Investment = 1 if subjects choose to exchange R\$ 10 in airtime credit (granted to all participants) for 6 months of the SMS program, and 0 otherwise; see Appendix A.

Figure 3 – Worries about household bills by priming status



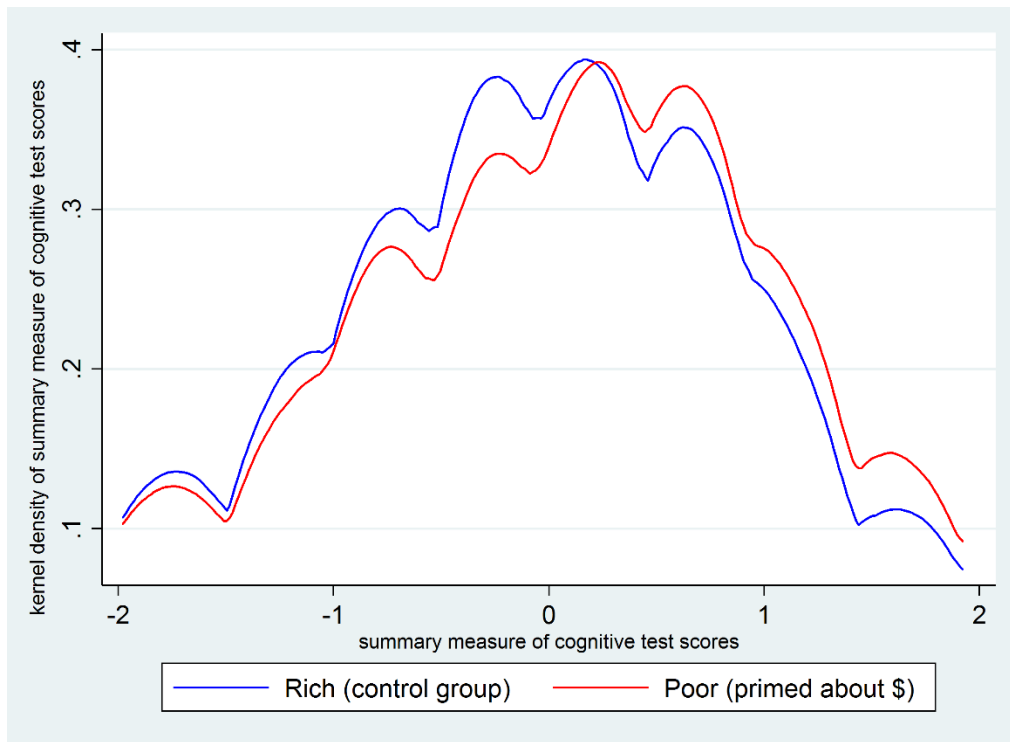
Notes: (1) Worried = 1 if subjects report being very worried or desperate about paying household bills by the end of the month, and 0 otherwise; see Appendix A; (2) Priming = 1 if subjects are primed to feel poorer by being asked what they would do if their child’s school started charging R\$ 400 for uniforms, and this amount was due by the end of the month, and 0 if they listen to the same question modified by replacing that amount for R\$ 20 (the control group); see Appendix A.

Figure 4 – Investment decision as a function of predicted returns by priming status, within sub-sample not informed about predicted returns over SMS



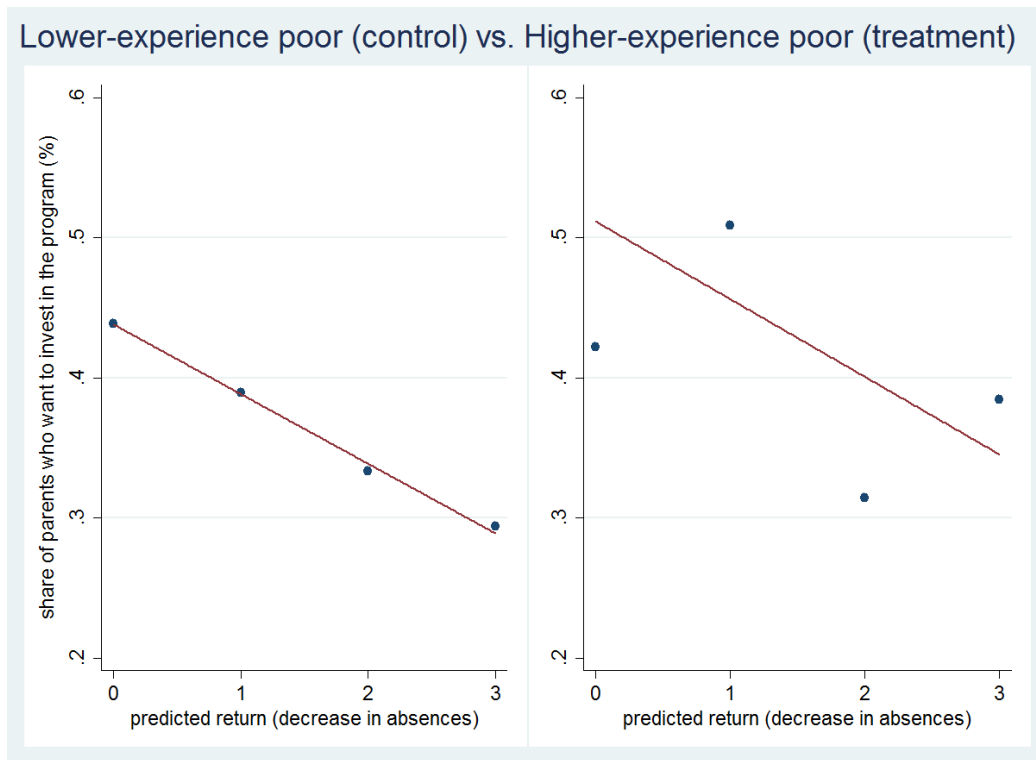
Notes: (1) Predicted returns = predicted treatment effects of the SMS program on attendance, using a regression tree based on the estimates of Cunha, Lichand, Madeira and Bettinger (2018); (2) Investment = 1 if subjects choose to exchange R\$ 10 in airtime credit (granted to all participants) for 6 months of the SMS program, and 0 otherwise; see Appendix A; (3) Priming = 1 if subjects are primed to feel poorer by being asked what they would do if their child’s school started charging R\$ 400 for uniforms, and this amount was due by the end of the month, and 0 if they listen to the same question modified by replacing that amount for R\$ 20 (the control group); see Appendix A.

Figure 5 – Distribution of summary measure of cognitive test scores by priming status, within sub-sample not informed about predicted returns over SMS



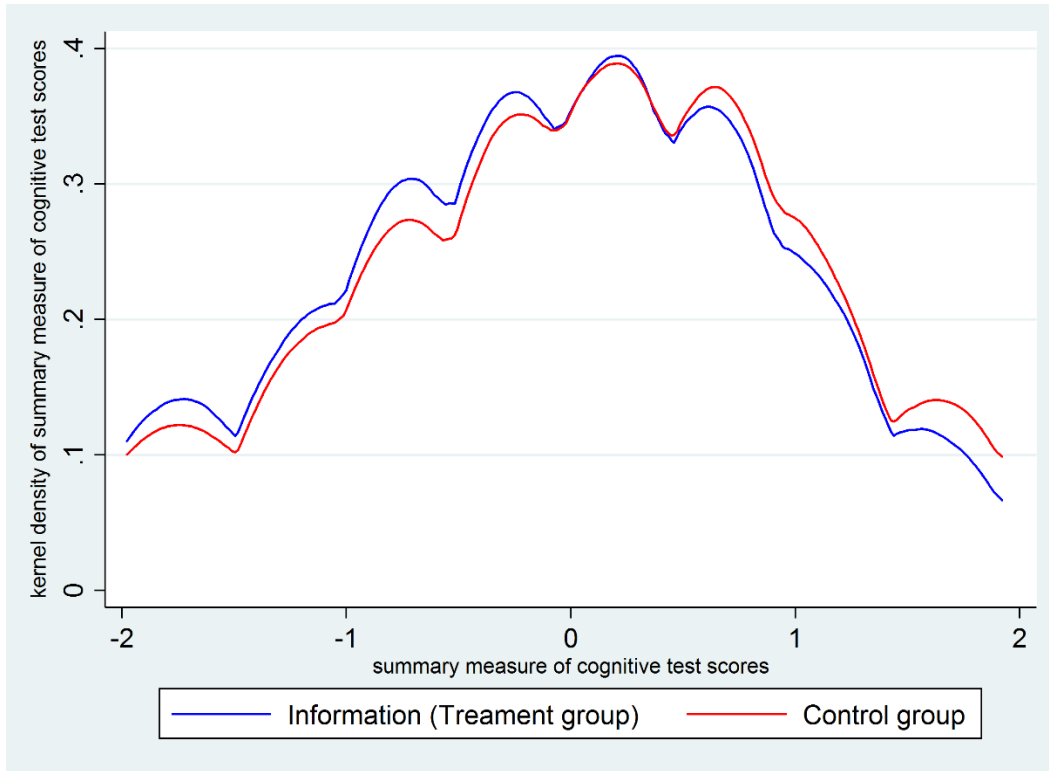
Notes: (1) Following Kling, Liebman and Katz (2004), summary measure $\tilde{Y}_{ji} = \sum \frac{1}{\hat{\sigma}_j} Y_{ji}$; where Y_{ji} is the score of subject i on cognitive test j , and $\hat{\sigma}_j$ is the sample standard deviation of test j 's score. Components of the summary measure are stroop and digit span test scores; see Appendix A; (2) Priming = 1 if subjects are primed to feel poorer by being asked what they would do if their child's school started charging R\$ 400 for uniforms, and this amount was due by the end of the month, and 0 if they listen to the same question modified by replacing that amount for R\$ 20 (the control group); see Appendix A.

Figure 6 – Investment decision as a function of predicted returns by experience status, within the sub-sample primed to feel poorer



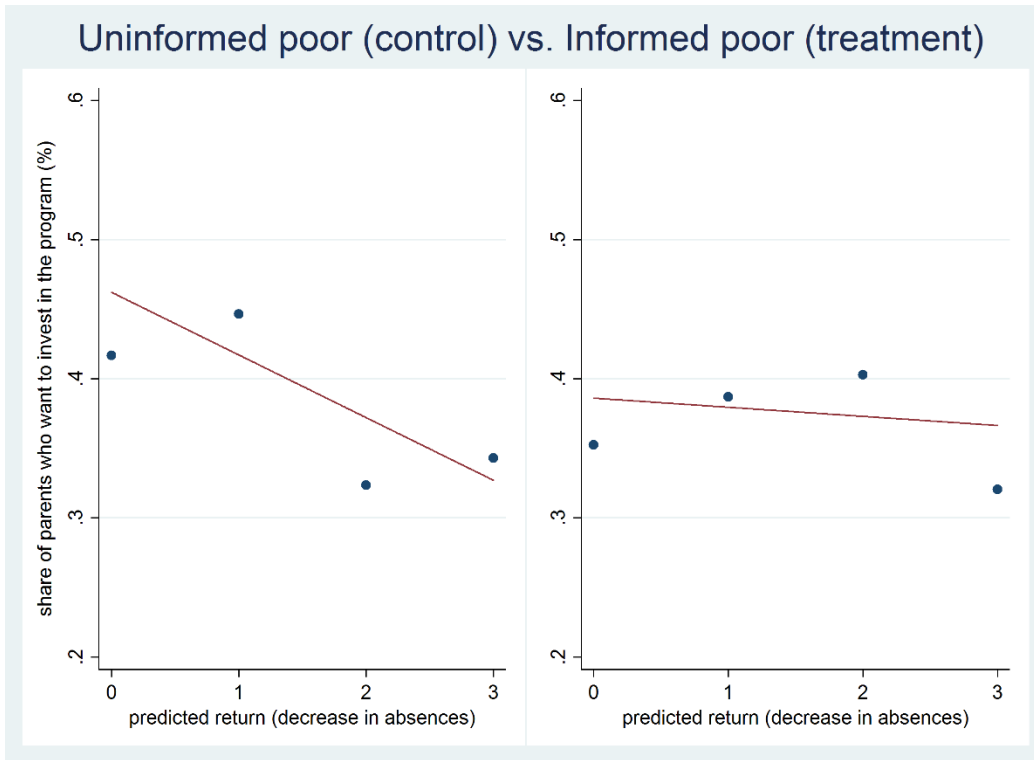
Notes: (1) Predicted returns = predicted treatment effects of the SMS program on attendance, using a regression tree based on the estimates of Cunha, Lichand, Madeira and Bettinger (2018); (2) Investment = 1 if subjects choose to exchange R\$ 10 in airtime credit (granted to all participants) for 6 months of the SMS program, and 0 otherwise; see Appendix A; (3) Experience = 1 if subjects were randomly assigned to the treatment group in Cunha, Lichand, Madeira and Bettinger (2018), and 0 otherwise; (4) Priming = 1 if subjects are primed to feel poorer by being asked what they would do if their child's school started charging R\$ 400 for uniforms, and this amount was due by the end of the month, and 0 if they listen to the same question modified by replacing that amount for R\$ 20 (the control group); see Appendix A.

Figure 7 – Distribution of summary measure of cognitive test scores by information status



Notes: (1) Following Kling, Liebman and Katz (2004), summary measure $\tilde{Y}_{ji} = \sum \frac{1}{\hat{\sigma}_j} Y_{ji}$; where Y_{ji} is the score of subject i on cognitive test j , and $\hat{\sigma}_j$ is the sample standard deviation of test j 's score. Components of the summary measure are stroop and digit span test scores; see Appendix A; (2) Information = 1 if subjects are informed over SMS about the predicted returns of the program for their child, 2 days before the phone survey, and 0 if they instead receive an SMS with the number of participants of the program in 2016 (the control group); see Appendix A.

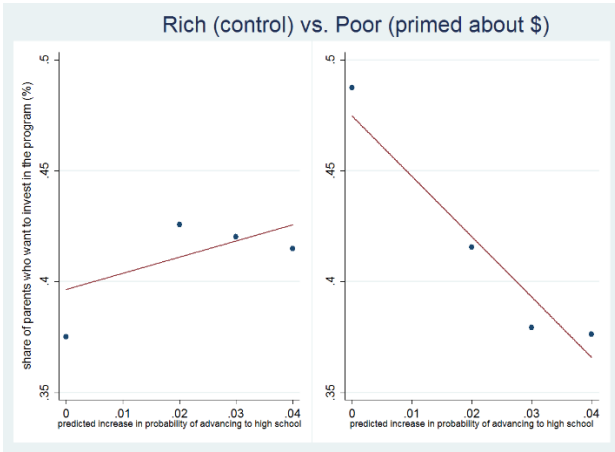
Figure 8 – Investment decision as a function of predicted returns by information status, within the sub-sample primed to feel poorer



Notes: (1) Predicted returns = predicted treatment effects of the SMS program on attendance, using a regression tree based on the estimates of Cunha, Lichand, Madeira and Bettinger (2018); (2) Investment = 1 if subjects choose to exchange R\$ 10 in airtime credit (granted to all participants) for 6 months of the SMS program, and 0 otherwise; see Appendix A; (3) Information = 1 if subjects are informed over SMS about the predicted returns of the program for their child, 2 days before the phone survey, and 0 if they instead receive an SMS with the number of participants of the program in 2016 (the control group); see Appendix A; (4) Priming = 1 if subjects are primed to feel poorer by being asked what they would do if their child’s school started charging R\$ 400 for uniforms, and this amount was due by the end of the month, and 0 if they listen to the same question modified by replacing that amount for R\$ 20 (the control group); see Appendix A.

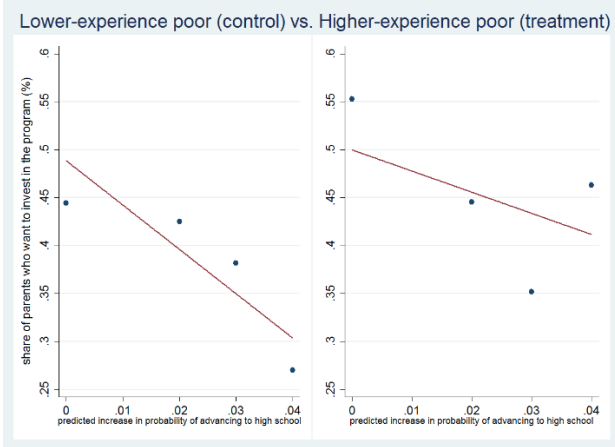
Figure 9 – Results under predicted returns for the likelihood of progressing to high school

Panel A – Priming within sub-sample not informed



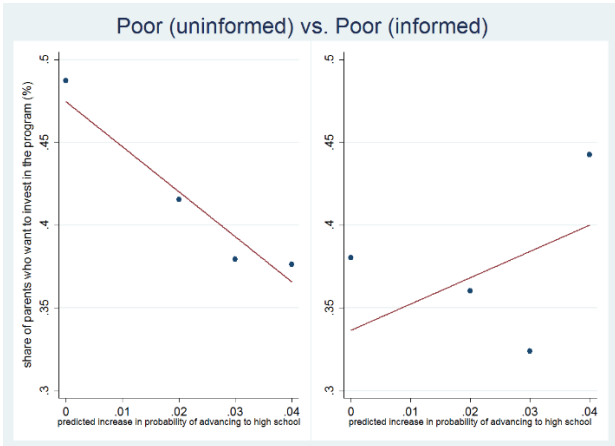
Notes: (1) Predicted returns = predicted treatment effects of the SMS program on attendance, using a regression tree based on the estimates of Cunha, Lichand, Madeira and Bettinger (2018); (2) Investment = 1 if subjects choose to exchange R\$ 10 in airtime credit (granted to all participants) for 6 months of the SMS program, and 0 otherwise; see Appendix A; (3) Priming = 1 if subjects are primed to feel poorer by being asked what they would do if their child’s school started charging R\$ 400 for uniforms, and this amount was due by the end of the month, and 0 if they listen to the same question modified by replacing that amount for R\$ 20 (the control group); see Appendix A.

Panel B – Experience



Notes: (1) Predicted returns = predicted treatment effects of the SMS program on attendance, using a regression tree based on the estimates of Cunha, Lichand, Madeira and Bettinger (2018); (2) Investment = 1 if subjects choose to exchange R\$ 10 in airtime credit (granted to all participants) for 6 months of the SMS program, and 0 otherwise; see Appendix A; (3) Experience = 1 if subjects were randomly assigned to the treatment group in Cunha, Lichand, Madeira and Bettinger (2018), and 0 otherwise; (4) Priming = 1 if subjects are primed to feel poorer by being asked what they would do if their child’s school started charging R\$ 400 for uniforms, and this amount was due by the end of the month, and 0 if they listen to the same question modified by replacing that amount for R\$ 20 (the control group); see Appendix A.

Panel C – Information within the poor



Notes: (1) Predicted returns = predicted treatment effects of the SMS program on attendance, using a regression tree based on the estimates of Cunha, Lichand, Madeira and Bettinger (2018); (2) Investment = 1 if subjects choose to exchange R\$ 10 in airtime credit (granted to all participants) for 6 months of the SMS program, and 0 otherwise; see Appendix A; (3) Information = 1 if subjects are informed over SMS about the predicted returns of the program for their child, 2 days before the phone survey, and 0 if they instead receive an SMS with the number of participants of the program in 2016 (the control group); see Appendix A; (4) Priming = 1 if subjects are primed to feel poorer by being asked what they would do if their child’s school started charging R\$ 400 for uniforms, and this amount was due by the end of the month, and 0 if they listen to the same question modified by replacing that amount for R\$ 20 (the control group); see Appendix A.

Appendix D – Tables

Table 1 – Balance tests

	Priming			Information			Experience		
	[C]	[T]	[T]-[C]	[C]	[T]	[T]-[C]	[C]	[T]	[T]-[C]
black	0.39 [0.0136]	0.40 [0.0138]	0.0127 [0.0194]	0.40 [0.0136]	0.39 [0.0138]	-0.00667 [0.0194]	0.41 [0.0161]	0.40 [0.0136]	-0.0120 [0.0211]
black_caretaker	0.41 [0.0137]	0.41 [0.0139]	0.00528 [0.0195]	0.42 [0.0136]	0.40 [0.0139]	-0.0156 [0.0195]	0.41 [0.0162]	0.43 [0.0137]	0.0157 [0.0213]
age	14.70 [0.0190]	14.67 [0.0192]	-0.0316 [0.0270]	14.71 [0.0189]	14.67 [0.0193]	-0.0363 [0.0270]	14.69 [0.0222]	14.70 [0.0187]	0.00813 [0.0290]
age_caretaker	39.36 [0.194]	39.43 [0.197]	0.0698 [0.277]	39.47 [0.194]	39.32 [0.198]	-0.147 [0.277]	39.24 [0.232]	39.44 [0.196]	0.199 [0.304]
mother	0.81 [0.0111]	0.79 [0.0113]	-0.0213 [0.0158]	0.81 [0.0111]	0.79 [0.0113]	-0.0226 [0.0158]	0.82 [0.0131]	0.79 [0.0110]	-0.0309* [0.0171]
girl	0.50 [0.0139]	0.51 [0.0141]	0.00556 [0.0198]	0.49 [0.0139]	0.52 [0.0141]	0.0369* [0.0198]	0.50 [0.0164]	0.51 [0.0139]	0.00546 [0.0215]
poor	0.17 [0.0103]	0.16 [0.0104]	-0.00555 [0.0146]	0.18 [0.0103]	0.15 [0.0104]	-0.0311** [0.0146]	0.17 [0.0124]	0.17 [0.0104]	0.00609 [0.0162]
low schooling	0.26 [0.0121]	0.25 [0.0123]	-0.00921 [0.0173]	0.26 [0.0121]	0.25 [0.0123]	-0.0139 [0.0173]	0.27 [0.0146]	0.27 [0.0123]	-0.00185 [0.0191]

Notes to Table 1:

(1) Priming = 1 if subjects are primed to feel poorer by being asked what they would do if their child's school started charging R\$ 400 for uniforms, and this amount was due by the end of the month, and 0 if they listen to the same question modified by replacing that amount for R\$ 20 (the control group); see Appendix A;

(2) Information = 1 if subjects are informed over SMS about the predicted returns of the program for their child, 2 days before the phone survey, and 0 if they instead receive an SMS with the number of participants of the program in 2016 (the control group); see Appendix A;

(3) Experience = 1 if subjects were randomly assigned to the treatment group in Cunha, Lichand, Madeira and Bettinger (2018), and 0 otherwise;

(4) Standard errors in brackets; *** p<0.01, ** p<0.05, * p<0.1.

Table 2 – Selective attrition tests

	Survey experiment		Information experiment		Exposure experiment	
	(1)	(2)	(3)	(4)	(5)	(6)
priming	0.00132 [0.00492]	0.00111 [0.00491]				
information			-0.00566 [0.00492]	-0.00557 [0.00491]		
experience					0.00629 [0.00569]	0.00643 [0.00569]
black		-0.000666 [0.00665]		-0.000725 [0.00665]		-0.00253 [0.00754]
black_caretaker		0.00847 [0.00662]		0.00845 [0.00662]		0.0109 [0.00752]
age		-0.00600* [0.00350]		-0.00606* [0.00350]		-0.00565 [0.00412]
age_caretaker		-0.00173*** [0.000296]		-0.00173*** [0.000296]		-0.00164*** [0.000331]
mother		0.0170*** [0.00593]		0.0169*** [0.00593]		0.0161** [0.00682]
girl		-0.00491 [0.00493]		-0.00495 [0.00493]		-0.00382 [0.00565]
poor		0.00980 [0.00685]		0.00977 [0.00685]		0.0115 [0.00777]
low schooling		-0.0283*** [0.00559]		-0.0284*** [0.00559]		-0.0259*** [0.00638]
constant	0.105*** [0.00347]	0.256*** [0.0534]	0.108*** [0.00348]	0.261*** [0.0534]	0.109*** [0.00431]	0.251*** [0.0626]
Observations	15,574	15,554	15,574	15,554	12,597	12,580
R-squared	0.000	0.006	0.000	0.006	0.000	0.006

Notes to Table 2:

(1) Priming = 1 if subjects are primed to feel poorer by being asked what they would do if their child's school started charging R\$ 400 for uniforms, and this amount was due by the end of the month, and 0 if they listen to the same question modified by replacing that amount for R\$ 20 (the control group); see Appendix A;

(2) Information = 1 if subjects are informed over SMS about the predicted returns of the program for their child, 2 days before the phone survey, and 0 if they instead receive an SMS with the number of participants of the program in 2016 (the control group); see Appendix A;

(3) Experience = 1 if subjects were randomly assigned to the treatment group in Cunha, Lichand, Madeira and Bettinger (2018), and 0 otherwise;

(4) Standard errors in brackets; *** p<0.01, ** p<0.05, * p<0.1.

Table 3 – Worries about household bills

	Very worried or desperate		
	(1)	(2)	(3)
priming	0.0826*** [0.0198]	0.0817*** [0.0198]	0.0820*** [0.0197]
black		-0.0134 [0.0268]	-0.0163 [0.0267]
black_caretaker		-0.00369 [0.0266]	-0.00247 [0.0266]
age		0.0121 [0.0146]	0.0134 [0.0145]
age_caretaker		-0.00153 [0.00148]	-0.00304 [0.00285]
mother		-0.0270 [0.0253]	-0.0199 [0.0253]
girl		0.0332* [0.0199]	0.0593** [0.0244]
poor		0.0197 [0.0275]	0.0197 [0.0275]
low schooling		-0.0705*** [0.0233]	-0.0719*** [0.0233]
constant	0.568*** [0.0139]	0.478** [0.225]	0.500** [0.244]
Predicted returns fixed-effects	No	No	Yes
Observations	2,424	2,424	2,424
R-squared	0.007	0.013	0.018

Notes to Table 3:

(1) Worried = 1 if subjects report being very worried or desperate about paying household bills by the end of the month, and 0 otherwise; see Appendix A;

(2) Priming = 1 if subjects are primed to feel poorer by being asked what they would do if their child's school started charging R\$ 400 for uniforms, and this amount was due by the end of the month, and 0 if they listen to the same question modified by replacing that amount for R\$ 20 (the control group); see Appendix A;

(3) Standard errors in brackets; *** p<0.01, ** p<0.05, * p<0.1.

Table 4 – Investment decision within sub-sample not informed about predicted returns over SMS

	Invests in SMS program [info = 0]		
	(1)	(2)	(3)
priming	-0.0104 [0.0273]	0.110** [0.0495]	0.111** [0.0499]
priming x predicted returns		-0.0948*** [0.0325]	-0.0950*** [0.0327]
predicted returns		0.0498** [0.0230]	0.0565** [0.0268]
black			-0.0299 [0.0374]
black_caretaker			0.0125 [0.0371]
age			0.0164 [0.0198]
age_caretaker			-0.00116 [0.00261]
mother			0.00266 [0.0358]
girl			-0.00856 [0.0291]
poor			0.00969 [0.0369]
low schooling			0.0111 [0.0320]
constant	0.415*** [0.0191]	0.352*** [0.0349]	0.152 [0.312]
Predicted returns fixed-effects	No	No	No
Observations	1,301	1,301	1,301
R-squared	0.000	0.007	0.008

Notes to Table 4:

(1) Investment = 1 if subjects choose to exchange R\$ 10 in airtime credit (granted to all participants) for 6 months of the SMS program, and 0 otherwise; see Appendix A;

(2) Predicted returns = predicted treatment effects of the SMS program on attendance, using a regression tree based on the estimates of Cunha, Lichand, Madeira and Bettinger (2018);

(3) Priming = 1 if subjects are primed to feel poorer by being asked what they would do if their child's school started charging R\$ 400 for uniforms, and this amount was due by the end of the month, and 0 if they listen to the same question modified by replacing that amount for R\$ 20 (the control group); see Appendix A;

(4) Information = 1 if subjects are informed over SMS about the predicted returns of the program for their child, 2 days before the phone survey, and 0 if they instead receive an SMS with the number of participants of the program in 2016 (the control group); see Appendix A;

(5) Standard errors in brackets; *** p<0.01, ** p<0.05, * p<0.1.

Table 5 – Cognitive performance within sub-sample not informed about predicted returns over SMS

	Summary measure cognitive tests			Stroop			Digit span		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
priming	0.103** [0.0421]	0.0956** [0.0418]	0.0953** [0.0419]	0.0783* [0.0421]	0.0718* [0.0419]	0.0724* [0.0420]	0.0798* [0.0449]	0.0734* [0.0446]	0.0711 [0.0446]
black		-0.0747 [0.0568]	-0.0749 [0.0569]		-0.0579 [0.0569]	-0.0578 [0.0570]		-0.0494 [0.0605]	-0.0517 [0.0605]
black_caretaker		-0.0264 [0.0565]	-0.0264 [0.0565]		-0.0261 [0.0566]	-0.0269 [0.0567]		-0.0203 [0.0601]	-0.0161 [0.0601]
age		-0.0762** [0.0304]	-0.0759** [0.0304]		-0.0614** [0.0305]	-0.0613** [0.0305]		-0.0492 [0.0320]	-0.0496 [0.0320]
age_caretaker		-0.00444 [0.00314]	-0.00214 [0.00601]		-0.000644 [0.00314]	0.00215 [0.00602]		-0.00948*** [0.00336]	-0.0127** [0.00640]
mother		0.0302 [0.0538]	0.0315 [0.0541]		0.0596 [0.0539]	0.0594 [0.0542]		-0.0495 [0.0579]	-0.0431 [0.0581]
girl		0.00244 [0.0421]	-0.0150 [0.0519]		-0.0183 [0.0422]	-0.0270 [0.0520]		0.0271 [0.0450]	0.00486 [0.0556]
poor		-0.0795 [0.0578]	-0.0802 [0.0579]		-0.0113 [0.0579]	-0.0105 [0.0580]		-0.108* [0.0614]	-0.113* [0.0615]
lowS		-0.223*** [0.0496]	-0.223*** [0.0497]		-0.243*** [0.0497]	-0.243*** [0.0498]		-0.173*** [0.0531]	-0.174*** [0.0531]
constant	-0.0502* [0.0294]	1.332*** [0.470]	1.246** [0.511]	-0.0382 [0.0294]	0.951** [0.471]	0.844* [0.512]	-0.0389 [0.0313]	1.175** [0.495]	1.312** [0.541]
Predicted returns FE	No	No	Yes	No	No	Yes	No	No	Yes
Observations	2,253	2,253	2,253	2,253	2,253	2,253	1,987	1,987	1,987
R-squared	0.003	0.022	0.022	0.002	0.018	0.018	0.002	0.018	0.020

Notes to Table 5:

(1) Following Kling, Liebman and Katz (2004), summary measure $\tilde{Y}_{ji} = \sum \frac{1}{\hat{\sigma}_j} Y_{ji}$; where Y_{ji} is the score of subject i on cognitive test j , and $\hat{\sigma}_j$ is the sample standard deviation of test j 's score. Components of the summary measure are stroop and digit span test scores; see Appendix A;

(2) Priming = 1 if subjects are primed to feel poorer by being asked what they would do if their child's school started charging R\$ 400 for uniforms, and this amount was due by the end of the month, and 0 if they listen to the same question modified by replacing that amount for R\$ 20 (the control group); see Appendix A;

(3) Information = 1 if subjects are informed over SMS about the predicted returns of the program for their child, 2 days before the phone survey, and 0 if they instead receive an SMS with the number of participants of the program in 2016 (the control group); see Appendix A;

(4) Standard errors in brackets; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6 – Investment decision within high-experience sub-sample not informed about predicted returns

	Invests in SMS program [info = 0 and experience = 1]		
	(1)	(2)	(3)
priming	-0.0661*	0.0559	0.0463
	[0.0391]	[0.0713]	[0.0718]
priming x predicted returns		-0.0960**	-0.0936**
		[0.0468]	[0.0474]
predicted returns		0.0406	0.0339
		[0.0309]	[0.0368]
black			-0.0495
			[0.0548]
black_caretaker			0.0603
			[0.0544]
age			0.0305
			[0.0302]
age_caretaker			0.00210
			[0.00366]
mother			-0.0544
			[0.0501]
Girl			-0.0415
			[0.0411]
Poor			-0.0663
			[0.0508]
low schooling			0.00757
			[0.0449]
Constant	0.509***	0.456***	0.00600
	[0.0271]	[0.0486]	[0.477]
Predicted returns fixed-effects	No	No	No
Observations	654	654	654
R-squared	0.004	0.011	0.023

Notes to Table 6:

(1) Investment = 1 if subjects choose to exchange R\$ 10 in airtime credit (granted to all participants) for 6 months of the SMS program, and 0 otherwise; see Appendix A;

(2) Predicted returns = predicted treatment effects of the SMS program on attendance, using a regression tree based on the estimates of Cunha, Lichand, Madeira and Bettinger (2018);

(3) Priming = 1 if subjects are primed to feel poorer by being asked what they would do if their child's school started charging R\$ 400 for uniforms, and this amount was due by the end of the month, and 0 if they listen to the same question modified by replacing that amount for R\$ 20 (the control group); see Appendix A;

(4) Information = 1 if subjects are informed over SMS about the predicted returns of the program for their child, 2 days before the phone survey, and 0 if they instead receive an SMS with the number of participants of the program in 2016 (the control group); see Appendix A;

(5) Experience = 1 if subjects were randomly assigned to the treatment group in Cunha, Lichand, Madeira and Bettinger (2018), and 0 otherwise;

(6) Standard errors in brackets; *** p<0.01, ** p<0.05, * p<0.1.

Table 7 – Benchmarking the effects of priming
 within high-experience sub-sample not informed about predicted returns

	Invests in SMS program [info = 0 and experience = 1]
priming	-0.0661* [0.0391]
poor	-0.0589 [0.0492]
low schooling	-0.0103 [0.0437]
Predicted returns FE	No
Observations	654

Notes to Table 7:

- (1) Investment = 1 if subjects choose to exchange R\$ 10 in airtime credit (granted to all participants) for 6 months of the SMS program, and 0 otherwise; see Appendix A;
- (2) Predicted returns = predicted treatment effects of the SMS program on attendance, using a regression tree based on the estimates of Cunha, Lichand, Madeira and Bettinger (2018);
- (3) Priming = 1 if subjects are primed to feel poorer by being asked what they would do if their child’s school started charging R\$ 400 for uniforms, and this amount was due by the end of the month, and 0 if they listen to the same question modified by replacing that amount for R\$ 20 (the control group); see Appendix A;
- (4) Information = 1 if subjects are informed over SMS about the predicted returns of the program for their child, 2 days before the phone survey, and 0 if they instead receive an SMS with the number of participants of the program in 2016 (the control group); see Appendix A;
- (5) Experience = 1 if subjects were randomly assigned to the treatment group in Cunha, Lichand, Madeira and Bettinger (2018), and 0 otherwise;
- (6) Standard errors in brackets; *** p<0.01, ** p<0.05, * p<0.1.

Table 8 – Effects of information on cognitive performance in incentivized tests

	Summary measure cognitive tests		
	(1)	(2)	(3)
information	-0.0934** [0.0421]	-0.104** [0.0419]	-0.104** [0.0419]
black		-0.0756 [0.0568]	-0.0756 [0.0569]
black_caretaker		-0.0276 [0.0565]	-0.0275 [0.0565]
age		-0.0791*** [0.0304]	-0.0790*** [0.0304]
age_caretaker		-0.00460 [0.00314]	-0.00275 [0.00601]
mother		0.0227 [0.0538]	0.0237 [0.0541]
girl		0.00694 [0.0421]	-0.0110 [0.0520]
poor		-0.0843 [0.0578]	-0.0852 [0.0579]
low schooling		-0.226*** [0.0496]	-0.226*** [0.0497]
constant	0.0459 [0.0295]	1.486*** [0.470]	1.419*** [0.511]
Predicted returns fixed-effects	No	No	Yes
Observations	2,253	2,253	2,253
R-squared	0.002	0.023	0.023

Notes to Table 8:

(1) Following Kling, Liebman and Katz (2004), summary measure $\tilde{Y}_{ji} = \sum \frac{1}{\hat{\sigma}_j} Y_{ji}$; where Y_{ji} is the score of subject i on cognitive test j , and $\hat{\sigma}_j$ is the sample standard deviation of test j 's score. Components of the summary measure are stroop and digit span test scores; see Appendix A; (2) Predicted returns = predicted treatment effects of the SMS program on attendance, using a regression tree based on the estimates of Cunha, Lichand, Madeira and Bettinger (2018);

(3) Information = 1 if subjects are informed over SMS about the predicted returns of the program for their child, 2 days before the phone survey, and 0 if they instead receive an SMS with the number of participants of the program in 2016 (the control group); see Appendix A;

(4) Standard errors in brackets; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 9 – Investment decision within sub-sample informed about predicted returns over SMS

	Invests in SMS program [info = 1]		
	(1)	(2)	(3)
priming	-0.00923 [0.0274]	-0.0327 [0.0502]	-0.0332 [0.0502]
priming x predicted returns		0.0197 [0.0330]	0.0191 [0.0330]
predicted returns		-0.0263 [0.0242]	-0.0244 [0.0274]
black			0.0315 [0.0368]
black_caretaker			0.0494 [0.0368]
age			-0.00141 [0.0207]
age_caretaker			0.00276 [0.00250]
mother			0.0398 [0.0344]
girl			-0.0629** [0.0288]
poor			-0.0795** [0.0399]
low schooling			-0.0380 [0.0328]
constant	0.386*** [0.0193]	0.419*** [0.0354]	0.319 [0.317]
Predicted returns fixed-effects	No	No	No
Observations	1,257	1,257	1,257
R-squared	0.000	0.001	0.016

Notes to Table 9:

(1) Investment = 1 if subjects choose to exchange R\$ 10 in airtime credit (granted to all participants) for 6 months of the SMS program, and 0 otherwise; see Appendix A;

(2) Predicted returns = predicted treatment effects of the SMS program on attendance, using a regression tree based on the estimates of Cunha, Lichand, Madeira and Bettinger (2018);

(3) Priming = 1 if subjects are primed to feel poorer by being asked what they would do if their child's school started charging R\$ 400 for uniforms, and this amount was due by the end of the month, and 0 if they listen to the same question modified by replacing that amount for R\$ 20 (the control group); see Appendix A;

(4) Information = 1 if subjects are informed over SMS about the predicted returns of the program for their child, 2 days before the phone survey, and 0 if they instead receive an SMS with the number of participants of the program in 2016 (the control group); see Appendix A;

(5) Standard errors in brackets; *** p<0.01, ** p<0.05, * p<0.1.

Table 10 – Investment decision as a function of predicted impact of the SMS program on child’s probability of advancing to high-school

Panel A – No information

	Invests in SMS program [info = 0]		
	(1)	(2)	(3)
priming	-0.0104 [0.0273]	0.0479 [0.0401]	0.061 [0.0437]
priming x predicted returns		-0.0449** [0.0229]	-0.0545** [0.0263]
Predicted returns FE	No	No	No
Observations	1,301	1,301	1,301
R-squared	0.000	0.004	0.006

Panel B – No information, High experience

	Invests in SMS program [info = 0 and experience= 1]		
	(1)	(2)	(3)
priming	-0.0661* [0.0391]	0.00468 [0.0588]	0.0183 [0.0636]
priming x predicted returns		-0.0563 [0.0353]	-0.0705* [0.0396]
Predicted returns FE	No	No	No
Observations	654	654	654
R-squared	0.004	0.009	0.024

Panel C – Information about returns on attendance

	Invests in SMS program [info = 1]		
	(1)	(2)	(3)
priming	-0.00923 [0.0274]	-0.00065 [0.0406]	-0.011 [0.0435]
priming x predicted returns		-0.00619 [0.0224]	0.000823 [0.0258]
Predicted returns FE	No	No	No
Observations	1,257	1,257	1,257
R-squared	0.000	0.001	0.015

Notes to Table 10:

(1) Investment = 1 if subjects choose to exchange R\$ 10 in airtime credit (granted to all participants) for 6 months of the SMS program, and 0 otherwise; see Appendix A;

(2) Predicted returns = predicted treatment effects of the SMS program on the likelihood of advancing to high-school, using a regression tree based on the estimates of Cunha, Lichand, Madeira and Bettinger (2018);

(3) Priming = 1 if subjects are primed to feel poorer by being asked what they would do if their child's school started charging R\$ 400 for uniforms, and this amount was due by the end of the month, and 0 if they listen to the same question modified by replacing that amount for R\$ 20 (the control group); see Appendix A;

(4) Information = 1 if subjects are informed over SMS about the predicted returns of the program *on attendance* for their child, 2 days before the phone survey, and 0 if they instead receive an SMS with the number of participants of the program in 2016 (the control group); see Appendix A;

(5) Standard errors in brackets; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.