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Working Paper

The psychological effects of poverty on investments in children's human capital

Working Paper, No. 349

Provided in Cooperation with:
Department of Economics, University of Zurich

Suggested Citation: Lichand, Guilherme; Bettinger, Eric; Cunha, Nina Menezes; Madeira, Ricardo (2020) : The psychological effects of poverty on investments in children's human capital, Working Paper, No. 349, University of Zurich, Department of Economics, Zurich

This Version is available at:
https://hdl.handle.net/10419/222350

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University of Zurich

Department of Economics

Working Paper Series

ISSN 1664-7041 (print)
ISSN 1664-705X (online)

Working Paper No. 349

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

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June 2020

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

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ABSTRACT: Poverty focuses attention on present needs. Does that mean that poor parents respond *inefficiently* to future returns on investments in their children's human capital – even when they would have the financial means to invest optimally? We study this question in the context of an educational program in Brazil whose predicted child-specific returns are known to the researchers, allowing us to compute optimal decisions. Using a lab-in-the-field experiment to make some parents worry more than others about pressing financial needs, we find that those in the treatment condition offered the opportunity to invest in that program *misallocate resources* relative to the control group: they not only invest significantly *less* when the program has *high returns*, but also, significantly *more* when predicted returns are *low*. We show that such *inefficient* responses are driven by poverty-induced *attention misallocation*, since (1) parents in the treatment condition perform *better* in cognitive tests that yield *small but immediate returns*, and (2) *increasing the salience* of returns before the experiment *eliminates* differential responses by those parents. Our results suggest that policy instruments to boost human capital investments among the poor, such as credit lines earmarked for education, may be insufficient to spark such investments when returns are high, and even lead to over-investment by those not expected to benefit from it.

Keywords: Psychology of poverty; Attention misallocation; Human capital; Poverty trap.

JEL codes: C93, D91, E24, I25, I26

[§] We would like to thank comments from Björn Bartling, Lorenzo Casaburi, Ernst Fehr, Anandi Mani, Michel Marechal, Frank Schilbach and Roberto Weber. We also thank Flavio Riva for excellent research assistance. All remaining errors are ours.

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1 Introduction

Poverty creates salient financial needs that require immediate attention. Since attention is limited (Kahneman, 2011), poverty effectively *reallocates* it to present needs at the expense of consideration of future payoffs. Under certain conditions, such poverty-induced focus on the present has been shown to be *excessive* (Lichand and Mani, 2020), impairing the quality of decision-making and productivity among the poor (Kaur et al., 2019). In a striking example, access to credit – tempting because of its present benefits, while its costs are only in the future – can make the poor *worse off than if credit were unavailable* (Shah, Shafir and Mullainathan, 2015). Investments in children have the features of credit reversed: their costs are in the present, while their benefits, only in the future. As such, paying *too much* attention to present needs is likely to *discourage* poor parents from undertaking those investments, even when they yield high returns.[1] This paper tests this hypothesis.

The poor have been shown to under-invest in their children's human capital for a variety of reasons, from liquidity constraints to over-optimism about their future willingness to trade off current costs against later returns (Kremer, Rao and Schilbach, 2019). Having said that, if financial worries induce the poor to inefficiently focus on the present, then one would expect under-investment *even if the poor had the means to finance those investments*, and *even if they could commit* (to overcome present-bias). This is relevant, because many governments and civil society organizations now provide credit lines to finance human capital investments, especially when it comes to children's education, in the hopes of allowing the poor to break away from poverty traps.[2] Such programs might be, however, insufficient if poverty leads parents to respond inefficiently to future investment returns because it captures their attention to present needs. In that case, poor parents might miss high-return opportunities for their children even when they could finance those investments.[3] Perhaps even worse, inattention to future returns might lead poor parents to take up credit to finance investments that do not pay off *even in expectation*, wasting scarce resources and falling into debt traps that make it even harder to escape poverty (Karlan, Mulainathan and Roth, 2019).

We show experimentally that pressing financial worries lead poor parents to *misallocate resources* to a large-scale educational program in Brazil, relative to other parents. Not only do parents made to feel poorer invest significantly *less* than the control group when predicted returns on investment are *high*, but they also invest significantly *more* when returns are *low*. We can trace resource misallocation to *attention misallocation* by documenting that parents made to feel poorer perform differentially in cognitive tests with small but immediate payoffs, and by showing that, when future returns are made salient, investment decisions are no longer different across the treatment and control groups.

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[1] In Glennerster and Kremer (2012)'s survey of the Development Economics literature on parental investments in children's health, they document that it is not only that the poor under-invest in children on average, but also that parents whose children's characteristics are associated with higher returns are *not more likely* to undertake those investments than other parents.

[2] See Lochner and Monje-Naranjo (2012) for a summary of the evidence on credit constraints for educational investments, including early investments in children (Section 4).

[3] In fact, poor parents often fail to take advantage of those programs, even when informed about their returns. While Jensen (2010) and Dizon-Ross (2019) document sizeable responses to information on returns to investments in children, Kremer and Holla (2011) and Glennerster and Kremer (2012) discuss several examples in Education and Health for which even informed parents under-invest. Bergman, Denning and Manoli (2019) show that information fails to increase enrollment among college students who fail to take advantage of tax benefits for college.

Studying this question outside the lab is challenging, for two main reasons. First, merely comparing how the poor and the rich make investments in children would confound several other differences, such as parents' knowledge about investment returns – a key determinant of human capital investments. To deal with this challenge, we resort to a lab-in-the-field experiment that *randomly assigns financial worries* within a sample of 2,300 caregivers of public school students, endowing them with R$ 10 (about USD 3, at the time) in airtime credit in exchange for answering an automated phone call. In that call, subjects are offered the opportunity to invest their endowment in signing up to an educational program that sends weekly text messages (SMS) with information about their child's attendance, punctuality and homework completion over the course of two school quarters. At the beginning of the call, we randomly assign subjects to financial worries by resorting to a survey experiment – a technique popular in cognitive psychology called *priming*, and increasingly used within Economics (e.g. Lichand and Mani, 2020, and Kaur et al., 2019, in the context of poverty's psychological effects).[4] The idea is that the priming allows researchers to emulate (for a short period of time) the psychological effects of poverty that, in reality, capture poor subject's attention *all the time*. For that reason, we call the treatment group in the survey experiment *experimentally poor*. We prime parents by asking them what they would do if their child's school started charging a high amount for textbooks and uniforms (R$ 400, or about USD 130 at the time), and that amount were due at the end of that month.[5] 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). Manipulation checks within a few minutes of the priming confirm that it makes experimentally poor parents 15% more likely to worry significantly about coping with household bills.

The second concern is that individual returns on investment are typically unobserved. In particular, the poor and the rich likely face very different return schedules (for instance, due to complementarities between formal schooling and home environment; e.g. Agostinelli, Saharkhiz and Wiswall, 2020), which should map into very different *optimal* investment decisions. As such, documenting that the experimentally poor invest differently than the control group would not necessarily point out to inefficiency. To deal with this challenge, we take advantage of the fact that all our subjects participated in a previous randomized control trial (RCT) that allows us to predict *child-specific returns* of that educational intervention. In Bettinger et al. (2020), a year prior to our experiment, caregivers of 9th graders were randomly assigned to receive weekly messages from that SMS program over the course of two school quarters. Using machine learning techniques (Athey and Imbens, 2017), we can predict the returns of investing on that educational program for every child.[6]

We use predicted returns to compute optimal investment decisions. This is a methodological improvement relative to an active literature in behavioral public economics that deals with the challenge of evaluating whether decisions are optimal by constructing a "counterfactual normative decision-maker" based

[4] See also Cohn et al. (2015), Gilad and Kliger (2008), Benjamin et al. (2010), Cohn et al. (2013), and Callen et al. (2014), in the context of influences on economic preferences.

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

[6] 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 increases grade promotion by 3 percentage points (reducing grade repetition by 1/3); see Bettinger et al., 2020. While average impacts are very high, different families have very different expected returns from the SMS program; see Section 3.2.

on survey data.[7] Such procedure assumes that any unobserved preferences are conditionally independent of bias; in turn, our approach *does not* rely on such strong assumption. The reason is that, while we also do not observe preferences, the net present value of the investment when its predicted returns are positive is *so high* in our setting that it is straightforward to infer optimal decisions, under minimal assumptions. Among parents whose children do not benefit from the SMS program, no one should optimally invest on it: the net present value of the investment is negative in that case. For all others parents, unless risk aversion is implausibly high, everyone should optimally invest on it: due to the high returns to an extra year of schooling in Brazil, the net present value of the investment for the average parent is nearly 1 minimum wage (~300 USD at the time).[8] As such, if financial worries increase the likelihood of investment among those with no returns from the program, or decrease it among those with positive returns, such effects are necessarily *inefficient*.

Such experimental design has multiple advantages. First, all caregivers in our sample are already familiar with the educational program at the time of the experiment.[9] That minimizes concerns with externally validity, and suggests that our experimental findings might extend even to educational and health investments that have been around for years. Second, we offer parents the opportunity to invest in the SMS program immediately after they are primed or not, without informing them about their child-specific predicted returns.[10] Eliciting investment decisions immediately after the survey experiment shuts down alternative mechanisms that could generate differences in investment decisions due by poverty – from differential nutrition (Schofield, 2018) to noise (Dean, 2020) to lack of sleep (Bessone et al., 2020).[11] Third, using airtime credit as endowment ensures liquidity constraints do not play a role. Since airtime is non-fungible (as, in Brazil, it cannot be transferred or used for making purchases), subjects' decision of whether to allocate it to the SMS program or not should be unaffected by potential effects that the priming could have on planned spending outside of the experiment. Fourth, we enforce parents' investment decisions without allowing for preference reversals in the future: parents' decisions are actually implemented at the end of the experiment, those who decide not to invest in the program have no future opportunity of doing so, and their endowment is converted into airtime credit – which they cannot transfer to the child or use to make other investments outside of the experiment.[12] That ensures commitment problems do not play a

[7] In this procedure, "optimal" decisions are those made by fully informed subjects (possibly augmented by other features such as lack of self-control problems; see for instance Alcott, Lockwood and Taubinsky, 2019). Decisions that deviate from that "normative" pattern are interpreted as *biased*.

[8] See subsection 3.1.

[9] In Bettinger et al. (2020), parents were assigned to either 1 SMS per week with a child-specific message, or to the control group, which received at most 1 school-wide SMS per month with communication about school activities. We use differences in prior exposure to the program to document that the psychological effects of poverty induce resource misallocation even among high-exposure parents; see sub-section 5.4.2.

[10] We cross-randomize parents to another experiment in which we inform half of them about the predicted returns of the program on their child's attendance. We restrict attention to uninformed parents at first, and later use this experiment to shed light on the mechanism behind the effects of financial worries on investment decisions.

[11] See Schilbach, Schofield and Mullainathan (2016) for a comprehensive survey.

[12] R$ 10 in airtime credit was highly valued by participants: it allowed unlimited internet surfing for 10 days at the time. Having said that, the nominal amount is too low to have freed resources towards alternative investments in children – it would not even have afforded a mechanical pencil in São Paulo State at the time.

role.[13] As such, we can document the *causal* effects of poverty-induced attention reallocation on a *real investment* in children's human capital, and clearly assess its efficiency implications.

We find that the experimentally poor respond *inefficiently* to investment returns, missing real opportunities to improve their children's educational achievement when returns are high, and wasting resources when returns are low. In the control group, 58.7% decide optimally (i.e. do not invest) when predicted returns are zero, whereas 41.5% decide optimally (i.e. invest) when predicted returns are positive.[14] Experimentally poor parents are almost 25% more likely to invest when the program has no benefits, relative to the control group, and their demand reacts substantially less to investment returns – parents in the treatment condition are 9.8 percentage points less likely to invest per predicted reduction in absences caused by the SMS program than parents in the control group. The magnitude of the effect is very large: it implies that, within the highest-return level, parents primed about financial worries are *only about half as likely to invest* in the program as those in the control condition.

Importantly, the fact that the experimentally poor invest *more* when investment returns are *low* rules out that different decisions across parents assigned to different treatment conditions are driven by alternative explanations, such as primed parents trusting schools to a lesser extent, or optimally cutting down on other educational investments in response to new information – since either would predict lower investments *across all return levels*. It also rules out that the subjects could be somehow responding more efficiently to beliefs about returns in response to the priming (despite our optimality argument), as its effects on investment decisions go in different directions throughout the spectrum of predicted returns.

Such patterns also stand in face of a range of robustness exercises. Our results are very robust to computing predicted returns with respect to either attendance or grade promotion. Else, allowing the effects of priming to vary with parents' income or education does not change the results, ruling out that heterogeneous treatment effects of the priming by predictors of investment returns could conflate the findings of resource misallocation.

Next, we document that these effects are driven by poverty-induced *attention misallocation*. To do that, as part of the phone survey, we subject parents to audio versions of attention, working memory and impulse-control tests, following Lichand and Mani (2020). Those tests are incentivized: the 20% top-performers earn extra R$ 2 (about 0.70 USD), creating an opportunity for short-term gains. In this type of tasks, it has been reliably shown that subjects display excessive focus on present needs, particularly within the income range of our study participants.[15] Based on those findings, we hypothesize that priming subjects about financial worries would *improve* their performance in short-term incentivized cognitive tests. Consistent with that hypothesis, experimentally poor subjects perform *significantly better* in those tests (by more than 0.1 standard deviation, about half the effect of having completed primary education on cognitive performance). While attending to present needs might seem efficient, we frame this effect as attention misallocation

[13] This is relevant in face of evidence that poverty induces present-bias (Bartos et al., 2018; Haushofer and Fehr, 2014).

[14] The large share of caregivers who invest even when returns are zero could be motivated by a multiplicity of factors, from experimenter demand bias to wrong beliefs about returns to aversion to inequality in educational outputs across siblings (Berry, Dizon-Ross and Jagnani, 2020).

[15] Lichand and Mani (2020) document that priming subjects to induce financial worries *improves* cognitive performance within short-term incentivized tasks at the per capita income levels of our study participants. Such global improvements in cognitive performance have also found in Kaur et al. (2019) and Shah, Shafir and Mullainathan (2019). This is in contrast to early experiments (Shafir and Mullainathan, 2013; Mani et al., 2013) that document that subjects primed about poverty perform worse in cognitive tests.

because it comes at the cost of generating inefficient responses to returns on children's human capital investments.

If effects are driven by attention misallocation, a key implication is that increasing the salience of investment's returns – by making them *top-of-mind* – should dampen the effects of the priming on the investment decision. We test this hypothesis by cross-randomizing parents to receiving information about the predicted returns of the SMS program in advance of the survey experiment. Half of our subjects are informed via text message 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, merely stating the number of families in the State who received messages from the SMS program in the previous year.

Consistent with attention misallocation, within the sample for whom future returns were *made salient*, the priming has *no effect* on parents' investment decisions: in that group, neither the intercept nor the slope of parents' investment as a function of predicted returns are systematically different between experimentally poor parents and those in the control group. That is the case even though there is just about the same scope for inefficiency within the sub-sample of informed parents: 60.2% of those decide optimally when the program has zero expected returns (compared to 57.1% among those uninformed), and 38% do so when it has positive returns (40.7% among those uninformed). As such, this result confirms that poverty's psychological effects are driven by attention misallocation.

We contribute to an active literature on the psychological consequences of poverty. While there is increasing evidence that poverty impairs executive functions, inducing individuals to focus on scarcity at hand at the expense of other dimensions (Mani et al., 2013; Haushofer and Fehr, 2014; Kaur et al., 2019; Lichand and Mani, 2020), this is the first paper to document its effects on human capital investments. Since educational achievement is directly consequential to future earnings, we show first-hand that the psychological effects of poverty can induce *inefficient* decisions with the potential to generate *inter-temporal poverty traps*.

Recent research shows that poor students underperform in face of financial worries (Duquennois, 2019). Our findings showcase that the psychological burden of poverty already hits one layer above: students from poor families might never even get the chance to perform at the same level if parents under-invest when returns on investment are high, even when they would have the means to finance (and commit to) such high-return opportunities.

Poverty-induced attention misallocation may shed light on the puzzling low take-up of preventive health care among the poor as soon as price is above zero, particularly among parents whose children would benefit the most from such investments (Glennerster and Kremer, 2012). Beyond investments in children, this mechanism might help rationalize the systematic misallocation of health investments in developing countries (such as Cohen, Dupas and Schaner, 2015, in the context of malaria testing and treatment, or Bai, Miguel and Rao, 2017, in the context of demand for and usage of health insurance). More broadly, poverty-induced attention misallocation might also help understand why the poor often miss high-return investment opportunities (from inefficient usage of fertilizer to under-adoption of productive technologies), a central element to the agenda of behavioral development economics (Kremer, Rao and Schilbach, 2019).

Last, our finding that informing poor parents about future returns on investment brings their decisions closer to the optimal adds to the research agenda on behavioral mechanisms behind the effects of informational interventions (Hanna, Mullainathan and Schwartzstein, 2014; Bettinger et al., 2020). As in Bettinger et al. (2020), our results showcase that attributing the effects of those interventions solely to

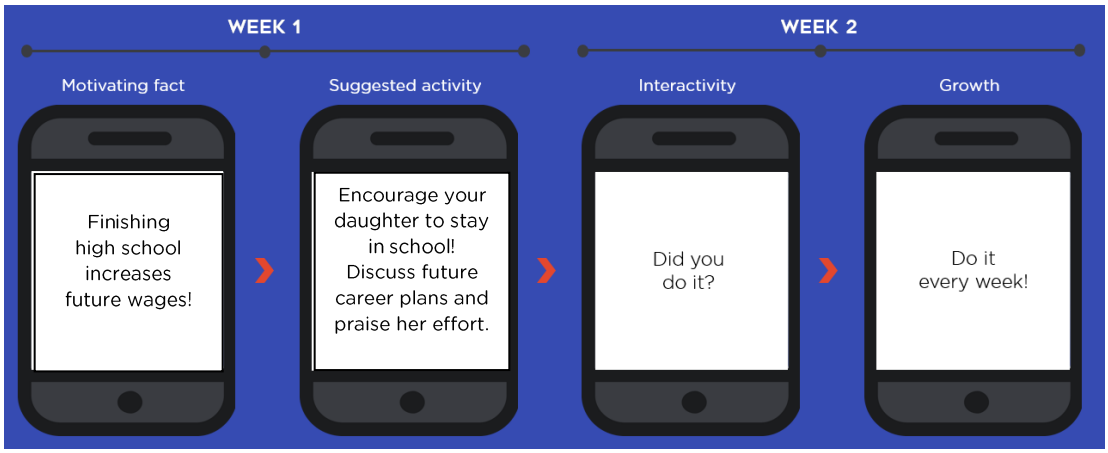
frictionless belief updating might provide an incomplete account of the mechanism behind the effects of those interventions on investments in children.

When it comes to policy implications, our results suggest that increasingly popular instruments to boost human capital investments among the poor, such as credit lines earmarked for education, may be insufficient to spark such investments when returns are high, and even lead to over-investment by those with low expected returns. In contrast, adapting the choice environment to shield poor individuals' investment decisions from the psychological burden of financial worries, either by making decisions automatic or by making future returns top-of-mind, may be key to break away from inter-generational poverty traps.

2 The educational program

The educational program we offer parents in the context of our experiment (Eduq+, powered by the Brazilian EdTech Movva), delivers nudges via text messages (SMS) directly to caregivers' mobile phones.[16] Nudges are organized in thematic sequences – comprised of four text messages each –, 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 (always non-curricular, as it is often the case that students' educational achievement is higher than that of their parents in Brazilian public schools); caregivers then receive an interactive message in the following week, posing them a question linked to the activity suggested the week before; last, a "growth" message meant to highlight that following students' school life more closely should be routine concludes the sequence. Content is catered to students' age group. In the case of our study sample, messages cover the challenges of being a teenager and how caregivers can help navigate that difficult phase, the importance of studying hard and of finishing high school, and issues linked to career choice.

Figure – Illustrative sequence of nudges from Eduq+



[16] Movva (http://www.movva.tech) delivers nudges to engage parents in their children's education across Brazil and Ivory Coast (as of 2019). One of the authors (Guilherme) is Movva's co-founder and chairman.

Bettinger et al. (2020) documents that Eduq+ increases attendance by 3 percentage points in Math and Language classes (or about 3 extra classes within 6 months), and improves Math test scores by 0.11 standard deviation (as if treated students were about 1 extra quarter ahead in school).[17,18]

3 Returns on investment and optimality

We are interested in predicting heterogeneous treatment effects of the intervention evaluated in Bettinger et al. (2020) to generate an *objective* measure of returns on investment *for each participant* in our study. Subsection 3.1 starts by explaining why this provides a methodological improvement relative to the literature when it comes to establishing the *normative counterfactuals* required to assess whether differences in behavior are in fact *inefficient*. Next, subsection 3.2 introduces the machine learning techniques we rely on to predict heterogeneous treatment effects, and presents the prediction results.

3.1 Normative Counterfactuals

We are ultimately interested in understanding whether financial worries among the poor lead them to inefficiently invest in their children's human capital. This is, however, a challenging exercise: even if poverty were randomly assigned, it would still be daunting to assume that any differences in investments between poor and rich parents would be necessarily inefficient. The reason is that it is not obvious that poor and rich families should optimally invest to the same extent, as the (typically unobserved) returns on investment might be very different across those sets of parents.

As an example of that problem, Burlacu et al. (2019) document that randomly assigned financial worries induce poor parents to spend more on household necessities at the expense of investments in children, relative to the control group, in an online experiment in the UK. Is that response inefficient? It is impossible to tell without imposing strong assumptions. It might well be that parents in the control group were inefficiently allocating their experimental budget to start with when it comes to maximizing expected utility (for instance, due to time-inconsistent preferences or cognitive constraints), such that financial worries actually bring parents closer to optimality, in the treatment group.

An active literature in behavioral public economics deals with the challenge of evaluating whether decisions are optimal by constructing a "counterfactual normative decision-maker" based on survey data – taking as "optimal" the decisions made by fully informed subjects (possibly augmented by other features such as lack of self-control problems; see for instance Alcott, Lockwood and Taubinsky, 2019). Decisions that deviate from that "normative" pattern are interpreted as *biased*.

This approach assumes that any unobserved preferences are conditionally independent of bias. In our setting, that would be equivalent to assuming that being informed about investment returns does not correlate with subject-specific returns. This is a strong assumption; it could very well be that

[17] See Figure 8 of Bettinger et al. (2020). The effect sizes we allude to correspond to those of 2 SMS per week.

[18] In Bettinger et al. (2020), a variation of Eduq+ that delivers only 1 SMS per week to caregivers, focused on a specific set of suggested activities (nudging parents to pay close attention to students' attendance, homework completion and punctuality in Math classes) is shown to 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).

complementarities between school and home environment (see Agostinelli, Saharkhiz and Wiswall, 2020) induce a positive correlation between wealth and educational returns and, hence – since wealthier subjects tend to be better informed –, between information about returns and returns themselves.

In turn, our approach *does not* rely on such strong assumption, for two reasons. First, because we observe the predicted returns of the program *for all study participants*, thanks to the combination of the experiment in Bettinger et al. (2020) with the machine learning techniques discussed in subsection 3.2. Second, because we can compute what the optimal decision should be within each level of predicted returns, as follows. The value of the outside option (keeping the R$ 10 endowment as airtime credit) is fixed, across all return levels. Since an additional school year at that stage increases future wages by R$ 135.76 (Brazilian Institute for Geography and Statistics, 2017), the net present value of that investment (using a yearly interest rate of 5%) is -R$ 10 for those who do not benefit from the program, and R$ 264, R$ 539 and R$ 1,087 for those whose child's probability of grade promotion increase by 1, 2, or 4 p.p., respectively (see subsection 3.2).[19] That implies that, unless risk aversion is implausibly high, all parents whose children experience positive returns from the program should invest on it.[20] As such, if financial worries increase investment among those with no returns from the program, or decrease it among those with positive returns, those effects are necessarily *inefficient*.

3.2 Machine Learning Predictions

We follow Athey and Imbens (2017) in implementing regression trees to partition the sample into subgroups driven by heterogeneity in Bettinger et al. (2020)'s experimental results. 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. While the procedure is known to over-fit in the case of extreme leaves, we are interested in the simplest possible prediction that is still statistically grounded. For our purposes, predicted heterogeneous treatment effects of the SMS program on students' educational outcomes provide an objective measure of returns on investment that we can use to analyze the psychological effects of poverty moving forward.

We use the algorithm to predict child-specific effects of Eduq+ on Math attendance and on the probability of grade promotion, allowing the algorithm to estimate different treatment effects by all students' and primary caregivers' characteristics from the baseline survey in Bettinger et al. (2020). Concretely, we estimate individual-level predicted impacts of the program, β_i, as a function of student's race, age and gender, and of their caregiver's race, age, gender, income and schooling:

$$\hat{\beta}_i \sim f(black_i, caregiver_black_i, age_i, caregiver_age_i, mother_i, girl_i, poor_i, lowS_i) \quad (1)$$

In equation (1), $black_i = 1$ if student i was declared to be brown or black, and 0 otherwise; $caregiver_black_i = 1$ if student i's primary caregiver declared him/herself to be black, and 0 otherwise; age_i and $caregiver_age_i$ stand for student i's and their caregivers' age, respectively; $mother_i = 1$ if the primary caregiver of student i was his/her mother, and 0 otherwise (father, grandparent, etc.); $girl_i = 1$ if

[19] Assuming public school students start working when they turn 18 and that employment lasts for the next 50 years.

[20] For instance, with log instantaneous utility, and simplifying the problem such that adults earn a constant wage if employed and zero otherwise, the expected unemployment rate would have to be over 40% for those within the lowest positive return level not to benefit from the program in expected utility terms; in contrast, the unemployment rate in Brazil at the time was around 12%.

student i was declared to be female, and 0 otherwise; $poor_i = 1$ if student i's household income was declared to be below 1 minimum wage, and $= 0$ otherwise; and $lowS_i = 1$ if student i's primary caregiver declared him/herself to be a primary-school drop-out or to have never attended school, and $= 0$ otherwise.

Figure 1 displays the regression tree estimated following this procedure for heterogeneous treatment effects of the educational program on Math attendance.[21] 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).

Some of the patterns generated by the regression tree can be attributed intuitive interpretations. The oldest caregivers – those 48 years old and above – are the ones who benefit the most from the SMS program when it 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 pay attention to the nudges to a greater extent. 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 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.

Figure 2 displays the regression tree estimated following this procedure for the effects of the SMS program on grade promotion. 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. 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 school 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 with 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.

An important outstanding issue is prediction accuracy. Since predicted returns are based on the first instance when those caregivers were exposed to the educational program – by the time their children were 9th graders –, to what extent should we expect students to experience the same returns as high school freshmen, when (at least for those assigned to the treatment group in the previous experiment) their caregivers would receive messages for the second time around?

The alternative of running the experiment with a new cohort of 9th graders would suffer from critical disadvantages. For one, participants would have no familiarity with the SMS program when making the investment decision, dismissing most of the attractive features that make our setting comparable to other human capital investments in children. Moreover, we would not be able to explore random variation in prior exposure to the intervention (see subsection 4.2.4) to shed light on the issue of external validity.

Instead, two pieces of evidence back up the argument that we should expect predicted effects to be reasonably stable even for participants of the previous experiment. First, in Bettinger et al. (2020) there is no evidence that effects decay over time: at least within the 6 months of the intervention, saturation does

[21] We round predicted returns to the nearest integer to avoid conveying decimal numbers to our subjects as part of the salience experiment (see subsection 4.2.3).

not kick in as authors estimate the effects of the program over the course of successive school quarters.[22] Second, Lichand and Wolf (2020), which evaluates the same educational intervention in Ivory Coast's public schools, documents similar effects on educational outcomes; grade repetition, for instance, falls by 1/3 amongst 6th graders, the same effect size documented in Bettinger et al. (2020) among 9th graders. Together, those results suggest that the effects of the intervention are expected to be similar even when caregivers have already become familiar with the nudges, and even across different age groups.

In the next sections, we draw upon those predicted returns to investigate whether financial worries cause the poor to under-invest in the educational program.

4 Empirical strategy

This section starts with a conceptual framework for the analysis that ensues, summarizing in subsection 4.1 the evidence on the psychological effects of poverty and the knowledge gap that this paper tries to fill. Subsection 4.2 introduces the experimental design, followed by a summary of the outcomes we draw upon in subsection 4.3. Last, subsection 4.4 describes the equations that we estimate.

4.1 Conceptual Framework

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 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 the field, Lichand and Mani (2020) document that priming poor farmers in Northeast Brazil about the risk of a drought indeed enhances farmers' relative performance in attention and memory tasks related to immediate needs (particularly, water), but adversely affects subjects' *overall* cognitive performance. Negative rainfall shocks produce exactly the same patterns, confirming that worries triggered by real economic shocks reallocate attention to present needs among the poor while impairing the foundations of decision-making.

Interestingly, the authors show that, for higher per capita income levels (such as those of our São Paulo sample), while attention reallocation still takes place, subjects actually perform *better* in short-term incentivized attention and memory tests. Similarly, in Kaur et al. (2019), which documents how distance to payday drives down factory productivity among the poor, priming subjects about financial worries actually *improves* piece-rate production. Reallocating attention away from long-term payoffs towards presents needs presumably generates costs; however, none of those studies have the means to document those long-term costs (or attention *misallocation*).

That is our main contribution: this is the first paper to document how poverty-induced attention reallocation translates into inefficiencies in real economic decisions with costly long-term consequences. Our focus on investments in children's human capital is no accident. Since those are a leading determinant of poverty's inter-generational transmission (Chetty and Hendren, 2018), if poor parents waste resources on

[22] Evidence on saturation and persistence of nudges to parents is still scarce (Bergman, 2019).

those investments when their returns are low or miss opportunities to invest when their returns are high, then that mechanism has the potential to generate poverty traps.

4.2 Experimental design

An early version of the experimental design and a pre-analysis plan were pre-registered at the AEA Social Registry on June 26, 2016 (AEARCTR-0001380).[23]

4.2.1 Effects of financial worries on investments in children: survey experiment

To causally identify the psychological effects of poverty on parents' investment decision, we resort to a survey experiment. We endow 2,223 parents of high-school freshmen with R$ 10 (about USD 3, at the time) in exchange for answering a phone survey (interactive voice response unit, IVR). Throughout that survey, subjects are offered the opportunity to invest this endowment in signing up for an SMS educational program over the next 6 months.

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 treatment group *experimentally poor*. Taking advantage of experimental variation in financial worries, we then analyze how it affects parent's likelihood of investing in their children's education. See Appendix A for the full script of how the investment decision was framed.

4.2.2 Design choices to rule out alternative mechanisms

Our experiment was carefully designed to distinguish the effects of poverty-induced attention reallocation from those of alternative mechanisms that could potentially lead parents to under-react to investment returns.

First, if we had not endowed subjects with resources that they could either invest or keep as airtime credit, but, rather, had given them the opportunity to invest their own resources, liquidity constraints could play a role. One reason would be if the priming changed subjects' marginal utility of consumption, inducing them to change planned spending at the time when they have to decide whether to invest in the SMS program. In turn, by endowing participants with airtime credit, we shut down liquidity constraints because the latter is *not fungible*: in Brazil, it cannot be used to make purchases or even transferred to someone else. As such, the decision to allocate it to the SMS program or not should be unaffected by whether the priming affects marginal utility of consumption outside of the experiment. While R$ 10 in airtime credit was highly valued by participants (it allowed unlimited internet surfing for 10 days at the time), the nominal

[23] https://www.socialscienceregistry.org/trials/1380. See Supplementary Appendix A for the early version of the pre-analysis plan in full.

amount is too low to have freed resources towards alternative investments in children – it would not even have afforded a mechanical pencil in São Paulo State at the time

Second, by eliciting investment decisions within 5 minutes of the survey experiment, we shut down alternative mechanisms that could generate differences in investment decisions due to financial worries albeit not because of poverty-induced attention reallocation, such as differential nutrition (Schofield, 2018) or lack of sleep (Bessone et al., 2020).

Third, by enforcing parents' investment decisions without allowing for preference reversals in the future, we shut down commitment problems as a potential mechanism for under-investment. This is important in face of the evidence that poverty causally affects time preferences (Bartos et al., 2018; Haushofer and Fehr, 2014). In our experiment, parents who decide not to invest in the program have no future opportunity of doing so, and their endowment is converted into airtime credit – which they cannot transfer to the child or use to make other investments outside of the experiment.

Last, since we actually implement parent's decisions, our experiment documents real consequences of the psychological effects of poverty on investments in children's human capital.

4.2.3 Attention reallocation: salience experiment

If mental bandwidth is the mechanism behind the psychological effects of poverty, then such effects should be mitigated by making future returns top-of-mind. We test this hypothesis by cross-randomizing parents to an intervention that increases the salience of the future returns of the SMS program.

To do that, half of participants are informed over SMS about the predicted effect of the program on their child's absences, just 2 days prior 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.

Our sample design cross-randomizes subjects to the priming and salience treatments, stratifying assignment to each cell by predicted returns of the SMS program on attendance and by treatment condition in Bettinger et al. (2020)'s experiment (see Section 4.2.4). Out of 12,597 caregivers, we successfully re-contacted 2,223 subjects in 2017, distributed quite symmetrically across assignment cells.[24]

Figure – Sample design

		Survey experiment	
		Control	Primed about financial worries
Salience experiment	Control	580	559
	Child-specific returns made salient	545	539

[24] Our sampling frame only excluded the pure control group from Bettinger et al. (2020) – i.e. subjects who did not receive *any* message from the SMS program over the previous year –, ensuring every participant in our experiment has at least *some* prior knowledge of the SMS program.

To disentangle the effects of salience from those of information, we replicate the analysis using parents' investment decisions as a function of the program's predicted effects on grade promotion; the reason is that the latter turn out to be uncorrelated with the child-specific predicted effects of the program on attendance. We find that making returns salient eliminates under-reaction to returns even in the absence of information; see subsection 6.4.

4.2.4 External validity: prior exposure experiment

A common criticism of Behavioral Economics is that enough market experience would eliminate departures from rational decision-making. In other words, systematic costly mistakes would be punished by the market and, eventually, disappear. Although this criticism does not stand even in the striking case of financial market transactions (Thaler, 2015), in the context of our experiment it could have implications for external validity. If parents under-react to the returns of the SMS program when deciding whether to invest, it could be that such conclusion does not carry over to the most common educational and health investments (from school enrollment to immunization), since families are very familiar with and often have previously decided on those.

While we cannot, in the context of our experiment, randomly assign different parents to new opportunities to invest in the SMS program over time, we can take advantage of random variation in the extent to which different participants had prior exposure to the program in the previous school year.

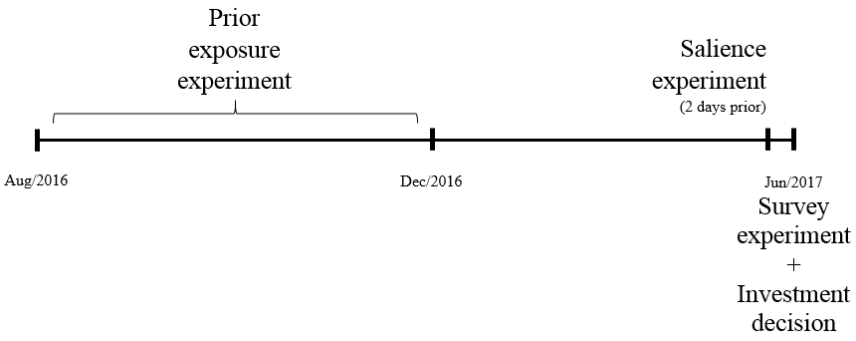
To do that, we take advantage of the prior experiment in Bettinger et al. (2020). In that study – conducted over the course of 20 weeks, in the year before our experiment –, the authors randomly assigned parents to either 1 SMS per week with a child-specific message, or to a control group, which received *at most* 1 school-wide SMS per month with communication about school activities. Naturally, it was easier to re-contact subjects who were treated in the previous experiment (58.4% of our sample); having said that, we still have a large sub-sample (945 subjects) assigned to the control group in the prior RCT, and our survey experiment is balanced within each sub-sample.

We call the treatment group in the prior RCT *high-exposure* in the context of our experiment. Manipulation checks at the end of the previous experiment confirm that parents assigned to the treatment group are much more likely to recall receiving text messages from the school (89.8%, versus 46.4% in the control group, a statistically significant difference at the 1% level).

4.2.5 Timeline

The timeline for the different experiments is outlined in the figure below. The prior exposure experiment takes place in the school year before the survey experiment, with communication with caregivers in the context of the SMS program taking place over the course of 18 weeks (see Bettinger et al., 2020). The salience experiment takes place 2 days before the survey experiment. 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.

Figure – Timeline



4.3 Data and outcomes

The key outcome of interest is parents' decision to invest in the SMS program, which we collect as part of the same phone survey through which we run the survey experiments. In that phone survey, we also elicit financial worries (see subsection 5.2), for manipulation checks, and measure cognitive outcomes (attention, working memory and impulse control; see subsection 6.2), to provide additional evidence on the mechanism behind the effects of financial worries. Those additional measures are collected only after the investment decision has been recorded, to avoid that eliciting financial worries ends up priming also the control group. The phone survey takes about 6 minutes in total).

To compute predicted returns, we combine administrative outcomes from the São Paulo State's Secretariat of Education on students' attendance and pass/fail status in the previous school year, and Bettinger et al. (2020)'s survey data on students' and their caregiver's characteristics (sex, age and race, for each, and caregivers' income and schooling), as well as each participants' assignment to treatment in that prior experiment, as described in subsection 3.2.

4.4 Estimation

We estimate the following equation:

$$Y_i = \alpha + \beta_0 T_i + \beta_r \ T_i \times R_i \ + \theta R_i + \gamma_k X_{k,i} + \varepsilon_i \tag{2}$$

In equation (2), $Y_i = 1$ if caregiver i decides to invest in the SMS program, and $= 0$ otherwise; $T_i = 1$ if caregiver i is primed about financial worries, and $= 0$ otherwise; 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 subsection 3.2; X_i is a vector of student and family characteristics, and ε_i is an error term. Given individual-level randomization and the fact that all surveys happen simultaneously, we do not cluster standard errors.

We use predicted returns with respect to attendance as our main measure of returns throughout, for two reasons. First, because we stratify the random assignment of financial worries by the latter. Second, because of the structure of our salience experiment, which was framed in terms of the effects of the SMS program on absenteeism. We turn to predicted returns with respect to grade promotion in the robustness checks, to showcase that results are not an artifact of a specific variable choice, and to help us disentangle the effects of salience from those of information (given that the two measures of predicted returns are basically uncorrelated; see subsection 6.4).

We are interested in whether poverty-induced attention reallocation leads parents to *misallocate resources* to the SMS program. We test whether the priming changes the likelihood of investing in the program when it has no returns (β_0), and in whether it changes the investment probability for each additional absence that the program is predicted to decrease (β_r). We hypothesize that $\beta_r \leq 0$, i.e. that experimentally poor parents under-react to the returns of the program, such that the investment probability increases with returns to a lesser extent than in the control group. An extreme version of the resource misallocation hypothesis would also entail $\beta_0 \geq 0$, with experimentally poor parents *over-investing* when the program has *no returns*.

We start by estimating equation (2) restricting attention to the control group of the salience experiment, i.e. the sub-sample for whom no information on child-specific returns was provided over SMS before the phone survey. In order to test the hypothesis of whether increasing the salience of future returns mitigates the effects of poverty-induced attention reallocation on investment decisions, we re-estimate equation (2) restricting attention to the sub-sample for whom information was provided over SMS before the phone survey. Last, to test the hypothesis of whether prior exposure mitigates those effects, we re-estimate equation (2) restricting attention to the high-exposure sub-sample for whom no information was provided over SMS before the phone survey. We also estimate the full-interaction model to formally test for differences in the coefficients of the priming across cells, by interacting different treatment indicators.

Cell sizes are large enough to allow us to detect even small effect sizes for the slope coefficient. In the prior exposure experiment, power calculations indicate that we can detect effects of at least 5.5 p.p. (12.6% of the control group average); in the salience and survey experiment, we can detect effect sizes of 4.1 p.p. or higher (less than 10% of the control group average). For the intercept, minimum detectable effects are much larger: 10.1 p.p. in the salience and survey experiment, and 14.8 p.p. in the prior exposure experiment.

5 Effects of Financial Worries on Investments in Children

This section starts by showcasing the characteristics of our sample in subsection 5.1, discussing selective non-response when it comes to participation in the follow-up to Bettinger et al. (2020)'s experiment, and balance tests in the context of the survey experiment. Next, subsection 5.2 presents manipulation checks to assess whether the priming affects financial worries, as it was designed to do. Subsection 5.3 then presents the results for the psychological effects of poverty on the investment decision, followed by a summary of robustness checks in subsection 5.4.

5.1 Selective non-response and balance

We have basic baseline information on students' and their caregivers' characteristics, coming from baseline data collection in the context of Bettinger et al. (2020). 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 caregivers 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 certain areas of São Paulo State, could apply to families living in slums). While families whose caregivers earn at least 3 minimum wages are by no means

rich in our setting (as our sample is exclusively of public school students, which are basically all low-income in Brazil at the time), we contrast those to families whose caregivers earn less than 1 minimum wage when exploring cross-sectional correlation between income and investments in children.

Even though treatment conditions were randomly assigned across all subjects in the sampling frame, because take-up of the phone survey is low – only 11.2% of the universe picks up and answers the call through the end – it may be the case that the sample we end up with is selected with respect to one or more treatment conditions, or unbalanced with respected to students' or caregivers' characteristics. For this reason, this subsection addresses selective non-response and balance tests.

Table 1 shows that some students' and caregivers' characteristics are systematically associated with non-response. Black or brown caregivers are more likely to take the survey (between 1 and 2.5 p.p.), as are younger caregivers (a 30 year-old parent is 4 to 7 percentage points more likely than a 60 year-old) and those with income above 1 minimum wage (between 1.4 and 3.5 p.p.). Even after controlling for income, age and race, caregiver's education significantly affects the chance that they answer the survey in full (completing primary education *decreases* take up by up to 3 p.p.). Most importantly, despite low take-up of the phone survey, participation is not selective with respect to any of the treatments: no treatment status leads caregivers to differentially answer the phone survey through the end. Differences are not only statistically insignificant at the 10% level, but also very small across all experimental conditions.

When it comes to balance tests with respect to the survey experiment, Table 2 shows that no student or caregiver characteristics is statistically different at the 10% level between the treatment and control groups within the control group of the salience experiment, consistent with random assignment. We also fail to reject the test of joint significance of covariates across treatment conditions (p-value 0.668).

5.2 Financial worries

We start by verifying whether the priming indeed increases financial worries as intended. We elicit financial worries categorically; subjects rate how worried they feel about not having enough money to pay all household bills at the end of the month ('not at all', 'somewhat worried', 'very worried' or 'desperate'; see Appendix A for the full script). We estimate the following equation:

$$Y_i = \alpha + \beta T_i + \gamma_k X_{k,i} + \varepsilon_i \tag{3}$$

In equation (3), $Y_i = 1$ 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.

In the control group, 56.8% of subjects report to be very worried or desperate, confirming that most of our sample is indeed poor. Table 3 investigates if the priming increases that share, in line with equation (3). Column (1) regresses the financial worries' indicator on the priming indicator; column (2) adds students' and caregivers' characteristics as controls; column (3) adds fixed effects for each predicted return level; and column (4) adds an interaction of priming with predicted returns to investigate whether the former increases worries differentially for students with higher returns from the SMS program, what could conflate considerations of resource misallocation.

Results are as follows. Column (1) confirms that the priming works: those made to worry about unexpected and urgent school expenses are 8.3 percentage-point more likely to report being very worried or

desperate about household bills due by the end of the month. Such effect is estimated with precision, statistically significant at the 1% level. Results are robust to controlling for students' and caregivers' characteristics (column 2), as well as to including randomization strata fixed effects (column 3). Column (4) shows that the priming does not increase worries differentially across return levels: the interaction coefficient is not only statistically insignificant, but also small (1.6 p.p., less than 3% the control group average), while the main effect is still estimated at 6.1 p.p. – an increase of over 10% relative to the control group average –, even though it loses statistical precision.

5.3 Investment decision

Next, we turn to differences in investment decisions across experimentally poor parents and the control group. Before we present the results of the experiment, we explore cross-sectional differences in income across subjects to motivate the analyses, documenting how the poorest caregivers in our sample invest in the educational program compared to those who are not as poor.

Figure 2 contrasts investment decisions amongst caregivers earning less than 1 minimum wage (shown on the right-hand-side panel) and those amongst caregivers earning over 3 minimum wages (shown on the left-hand-side panel). Among those who are not as poor, according to a linear approximation, about 44% of those caregivers invest even when the program has no effects on their children's attendance, and demand increases by 0.5 percentage points per absence reduced by the program. In contrast, among the poorest, not only is demand for the SMS program below that of the rich for every return level – what is not surprising, for poverty may decrease demand for costly investments for a variety of reasons –, but also, strikingly, its linear approximation slopes *downward* with predicted returns. According to that approximation, about 41% of caregivers invest even when the program has no predicted effects on their children's attendance, and demand *decreases* by 3.3 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 school, which may result in different optimal investment schedules.

For this reason, we now turn to the experimental findings, leveraging on the survey experiment to emulate poverty's psychological effects. Table 4 estimates the effect of the priming on the probability of investing in the SMS program, in line with equation (2), restricting attention to the control group of the salience experiment. Column (1) regresses the investment indicator in the priming indicator; column (2) adds the interaction of priming with predicted returns, to assess the former's effects on both the intercept and the slope of the demand for the SMS program as a function of predicted returns; and column (3) adds students' and caregivers' characteristics as controls and fixed effects for each predicted return level (columns 4 and 5 are discussed in subsection 5.4.1).

While on average the experimentally poor are not systematically less willing to invest in the program than the control group (column 1), columns (2) and (3) show that similar shares investing *across* return levels arise out of very different patterns *within* return levels. Essentially, there are systematic differences in both the intercept and the slope of the demand schedule as a function of predicted returns across the treatment and control groups, but those differences do not map into different shares on average because effects go in *different directions*. In line with the more extreme version of the resource misallocation hypothesis, (1) demand under-reacts to predicted returns among the experimentally poor – the probability

of investment *decreases with predicted returns* relative to the control group (9.8 percentage point per absence reduced, significant at the 1% level, in column 3), and (2) the experimentally poor are *much more likely to invest* in the program when it is *predicted to have no effects* (10 p.p., significant at the 10% level, in column 3).

Effect sizes are huge, yielding dramatic patterns. The experimentally poor invest 25% more than the control group average when the program has no predicted returns, but 44% less when its predicted returns are maximal. Strikingly, the experimentally poor waste resources when investment returns are low, and miss real opportunities to invest in their children when returns are high.

Figure 4 showcases Table 4's results graphically, residualizing the investment indicator and the predicted returns variable with respect to students' and caregivers' characteristics. Just as in Figure 3, demand slopes negatively with the program's returns among the (experimentally) poor; in Figure 4, though, such differences in intercepts and slopes can be causally attributed to financial worries.

5.4 Robustness checks

This subsection assesses whether results are sensitive to allowing the priming to affect investment decisions differentially by predictors of returns (subsection 5.4.1); whether having high exposure to the SMS program in the prior experiment eliminates under-reaction to investment returns (subsection 5.4.2); and whether results are an artifact of the measure of predicted returns with respect to attendance, or also hold with respect to the probability of grade promotion (subsection 5.4.3).

5.4.1 Confounding effects of return predictors

Since predicted returns are computed based on students' and caregivers' characteristics (a non-linear combination predicted by the regression tree introduced in subsection 3.2), one concern is that the differential effects of the priming on investment decisions across return levels are not capturing resource misallocation, but, rather, conflate heterogeneous treatment effects of the survey experiment with respect to subjects' characteristics. For instance, if caregivers' age (the most important predictor of returns with respect to attendance, in Figure 2) is correlated with income or education in our sample, and if the priming affects poorer or less educated subjects differentially, then it could be that the results captured in Table 4 are merely an artifact of such heterogeneous responses.

To investigate whether that is the case, in Table 4, column (4) controls for the interaction of priming with an indicator of low income (below 1 minimum wage), and column (5) controls for its interaction with an indicator of low educational achievement (incomplete primary education). Results are extremely robust to either: coefficients of the intercept and the slope do not or only barely change across specifications, with no changes to statistical precision.

5.4.2 Prior exposure

We now turn to a question linked to external validity. Is under-reaction an artifact of limited experience with the SMS program, or might this conclusion also hold for human capital investments that parents face recurrently, from school enrollment to preventive health care? We take advantage of the prior exposure experiment introduced in subsection 4.2.4 to answer that question.

Panel B in Table 2 shows that the priming exposure experiment is balanced within the sub-sample of high-exposure subjects in the control group of the salience experiment: no student or caregiver

characteristics is statistically different at the 10% level between the treatment and control groups, consistent with random assignment. We also fail to reject the test of joint significance of covariates across treatment conditions (p-value 0.730).

Table 5 estimates the effect of the priming on the probability of investing in the SMS program within the high-exposure sub-sample (that assigned to the treatment group in Bettinger at al., 2020's experiment), restricting attention to the control group of the salience experiment. As before, column (1) regresses the investment indicator in the priming indicator; column (2) adds the interaction of priming with predicted returns; column (3) adds students' and caregivers' characteristics as controls and fixed effects for each predicted return level, and columns (4) and (5) add interactions of the priming with the low income indicator and the low educational achievement indicator, respectively.

Prior exposure greatly increases demand for the SMS program: nearly 51% of parents invest in the control group within that sub-sample. The question is whether the priming leads to resource misallocation even amongst subjects who had previously engaged with the program to a greater extent. Table 5 documents that this is the case. In fact, experimentally poor parents are systematically less willing to invest on average (6.6 p.p.) across all return levels, relative to the control group. As a benchmark, poor individuals (those below 1 minimum wage) in our sample invest 7.9 p.p. less than rich ones (above 3 minimum wages). Hence, once differences in return schedules are taken into account and other confounding factors are partialled out, poverty's psychological effects on investments in children's human capital are almost as large as cross-sectional comparisons suggest.

Amongst high-exposure caregivers, the effect of the priming on the intercept of the demand for the SMS program is no longer statistically significant (although we can only detect very large effect sizes on the intercept within that sub-sample; see subsection 4.4). In turn, demand's slope still significantly decreases with returns (9.7 p.p. per absence reduced by the SMS program, nearly unchanged relative to Table 4, statistically significant at the 5% level).

In sum, prior exposure increases demand for the educational program for all subjects, but does *not* mitigate resource misallocation: muted responses to investment returns relative to the control group are no different according to prior exposure. Results are robust to allowing for independent effects of the priming on return predictors, in columns (4) and (5).[25]

Figure 5 helps explain those results. Restricting attention to the experimentally poor, while prior exposure shifts demand for the SMS program upwards – making high-exposure parents over 8 percentage points more likely to undertake the investment across all return levels –, it does not change the demand's slope with respect to predicted returns among those parents.

Last, Table 8 estimates the full-interaction model to formally test whether prior exposure mitigates the effects of the priming on the intercept and the slope of the demand for the SMS program as a function of predicted returns. Column 1 regresses the investment indicator on the priming indicator and a series of interactions. As before, the coefficient of the priming indicator stands for the effect of financial worries on the intercept, while that of its interaction with predicted returns, on the slope of the demand with respect to returns. Now, the interaction of the priming indicator with the prior exposure treatment indicator allows us to test whether the latter mitigates its effect on the intercept, and the triple interaction of priming, prior

[25] Supplementary Appendix B complements the analyses by showing that prior exposure does not systematically affect financial worries or cognitive outcomes.

exposure and predicted returns allows us to test whether the latter mitigates its effect on the slope. For completeness, we also include in the regression the prior exposure treatment indicator and its interaction with predicted returns. The regression also controls for students' and caregivers' characteristics, and for interactions of priming with indicators of low income and low educational achievement.

The results qualitatively match the patterns previously highlighted, although no coefficient is precisely estimated in column (1). The coefficient of the triple interaction is nearly 25% (with the opposite sign) of that of the priming interacted with predicted returns – consistent with financial worries still decreasing the slope of the demand for the program among those subjects. We come back to those results in the next subsection, when we consider predicted returns with respect to grade promotion.

5.4.3 Probability of grade promotion

Next, we consider whether results are robust to using a different measure of predicted returns of the SMS program: its child-specific predicted impacts on the probability of grade promotion, displayed in Figure 3. To do that, we replicate the analyses of the effects of the priming and of prior exposure on investment decisions using this alternative measure of returns.

We turn back to Table 8, which estimates the full-interaction model. That table allows us to not only formally document the effects of the priming on the intercept and the slope of the demand for the SMS program as a function of predicted returns with respect to grade promotion, but also to formally test whether prior exposure mitigates any such effects. Column (3) regresses the investment indicator on the priming indicator and a series of interactions. Different from column (1), it uses predicted returns with respect to grade promotion instead. The regression also controls for students' and caregivers' characteristics, and for interactions of priming with indicators of low income and low educational achievement. Such interactions are particularly important in the case of grade promotion, as Figure 3 indicates that caregivers' schooling is an important predictor of heterogeneous treatment effects of the SMS program.

Results are as follows. First, the priming systematically increases investment when the program has no returns (by 15.7 p.p., significant at the 10% level) and systematically mutes its response to predicted returns (by 5.1 p.p. for each additional percentage-point increase in the pass probability, significant at the 10% level). Once again, resource misallocation is huge: when the program has no predicted returns, the experimentally poor invest *nearly 50% more* than the control group, while they invest *over 10% less* in the maximal return level, relative to the control. Second, prior exposure does *not* systematically mitigate misallocation.

Figure 8 highlights how using this alternative measure of predicted returns yields the same empirical regularities that we found before. In Panel A, experimentally poor parents' demand for the SMS program has a higher intercept and a more negative slope with respect to predicted returns relative to the control group. In Panel B, prior exposure does not affect the slope of experimentally poor parents' demand.

Taking stock, we document that financial worries lead to systematic under-reaction to investment returns among the poor. The fact that this finding holds even when it comes to the returns of the program with respect to grade promotion strongly suggests that this mechanism could generate poverty traps – due to the close link between educational achievement and future labor market returns. Moreover, the fact the prior exposure to the program does not mitigate misallocation suggests that those findings might extent even to human capital investments that have been around for years. Next, we provide evidence that those effects are driven by poverty-induced attention reallocation.

6 Attention Misallocation

This section turns to the mechanism behind the effects of financial worries on investment decisions. We start by testing directly whether financial worries reallocate attention. We introduce our empirical strategy for estimating the effects of financial worries on cognitive outcomes, in subsection 6.1, followed by results in subsection 6.2. Next, subsection 6.3 tests an implication of the attention mechanism, by investigating whether making investment returns more salient mitigates resource misallocation triggered by financial worries. Last, since the salience experiment relies on informing caregivers about child-specific predicted returns of the SMS program, subsection 6.4 discusses how we distinguish the effects of salience from those of information.

6.1 Cognitive effects: outcomes and estimation

We start by providing direct evidence that financial worries reallocate attention in the context of our experiment. To do so, we include audio versions of cognitive tests in our phone survey, following Lichand and Mani (2020).[26] The cognitive outcomes we measure comprise tasks aimed at assessing attention, together with working memory and impulse control (so called brain's executive functions; Diamond, 2013).

We measure attention and working memory through digit span tests, in which subjects must remember as many digits as they can from the strings 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 one has just heard repeated multiple times, the correct answer is never the digit itself. See Appendix A for the full script of the cognitive tests in our phone survey. Such cognitive tests are incentivized in our phone survey: subjects are told they can earn extra R$ 2 (about 0.70 USD at the time) if they are amongst the 20% top-performers in those tests.

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

$$Y_i^j = \alpha + \beta_j T_i + \gamma_k X_{k,i} + \varepsilon_i \tag{4}$$

In equation (4), 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, we first normalize all outcomes to z-scores. Second, we run seemingly unrelated regressions (SUR) to compute an effect size β for each summary measure, given by equation (6):

[26] In Lichand and Mani (2020), those audio versions were validated through test-retest reliability metrics and correlation with face-to-face standard versions of those tests.

$$\hat{\beta} = \frac{1}{K}\sum_{j=1}^{K}\frac{\hat{\beta}_j}{\hat{\sigma}_{j_c}} \tag{5}$$

In equation (5), β_j are the point estimates obtained for ordinary least squares (OLS) regressions of Y^j on a particular treatment variable, σ_{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 β.

In Lichand and Mani (2020), priming subjects about financial worries deteriorates cognitive performance on average, but effects are highly heterogeneous according to municipality's per capita income. Among the poorest municipalities, the priming strongly worsens performance in attention, memory and impulse control tests, whereas in richest municipalities (with per capita income levels close to that of São Paulo State), it actually generates a cognitive bonus: the opportunity for short-term gains actually significantly improves performance incentivizes cognitive tests. As such, we are interested in whether $\beta \geq 0$, consistent with poverty-induced reallocation of mental bandwidth.

6.2 Effects of financial worries on cognitive outcomes

Table 6 estimates the effect of the priming on cognitive outcomes. Column (1) regresses the summary measure on the priming indicator; column (2) adds students' and caregivers' characteristics as controls and fixed effects for each predicted return level; column (3) adds the interaction of priming with predicted returns to investigate whether the former affects cognitive performance differentially for caregivers whose children have higher returns from the SMS program, what could conflate considerations of resource misallocation; columns (4) and (5) replace the dependent variable with the standardized measures of the components of the summary measure: digit span and Stroop, respectively.

Results confirm that, consistent with the mental bandwidth reallocation, experimentally poor subjects perform significantly *better* in those tests: the priming improves cognitive performance (measured by the summary measure) by over 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 1% level (column 1) and are robust to the inclusion of student and family characteristics and return level fixed effects (column 2). Allowing the priming to have differential effects by predicted returns barely changes its coefficient (in column 3, although it is now only imprecisely estimated), and rules out that differential cognitive effects might be driving under-reaction to investment returns. Last, columns 4 and 5 document that effects are not driven by any specific component; experimentally poor subjects do better in both Stroop and digit span (although neither are precisely estimated in isolation).

Figure 6 documents that the priming shifts the whole distribution of cognitive performance: the summary measure of cognitive outcomes for experimentally poor subjects (full line) is offset to the right relative to the control group (dashed line).

The improvement in cognitive function in response to priming is consistent with results in Shah, Mullainathan and Shafir (2019), Kaur et al. (2020) and Lichand and Mani (2020). All in all, the evidence indicates that financial worries induce subjects to focus on short-run returns, at the expense of properly evaluating long-term investment returns.

6.3 Making investment returns *top-of-mind*

Next, we turn to the issue of whether making investment returns more salient (or *top-of-mind*; Karlan et al., 2010) mitigates resource misallocation triggered by financial worries. To do that, we resort to our salience experiment, in which we informed half our subjects about child-specific predicted returns of the SMS program on attendance, via text message just 2 days before the phone survey; the control group received a neutral SMS without child-specific information (see Appendix B).

Panel C in Table 2 shows that the survey experiment is balanced within the treatment group of the salience experiment: only two characteristics – caregiver's age and predicted returns with respect to attendance – are statistically different (at the 10% and 5% levels, respectively) between the treatment and control groups, and the differences are very small. This is even below the expected number of false positives one would expect could arise out of chance across the 30 tests reported in Table 2. Most importantly, we fail to reject the test of joint significance of covariates across treatment conditions (p-value 0.351). In any case, all our regressions include predicted return category fixed-effects, since randomization was stratified by that variable.

Table 7 estimates the effect of the priming on the probability of investing in the SMS program, restricting attention to the treatment group of the salience experiment. As before, column (1) regresses the investment indicator in the priming indicator; column (2) adds the interaction of priming with predicted returns; column (3) adds students' and caregivers' characteristics as controls and fixed effects for each predicted return level, and columns (4) and (5) add interactions of the priming with the low income indicator and the low educational achievement indicator, respectively.

Consistent with the attention mechanism, there are *no differences* across experimentally poor parents and the control group within the sub-sample for whom investment returns were made salient: neither the intercept nor the slope of demand for the SMS program are systematically affected by the priming.

In Table 8, which estimates the full-interaction model to formally test whether salience mitigates any such effects, column (2) regresses the investment indicator on the priming indicator and a series of interactions, using predicted returns with respect to attendance. The regression also controls for students' and caregivers' characteristics, and for interactions of priming with indicators of low income and low educational achievement.

As before, the priming systematically increases investment when the investment has no returns (by 10.7 p.p., significant at the 10% level) and systematically mutes demands' response to predicted returns (by 9.7 p.p. for each additional increase in attendance, significant at the 1% level). Salience of investment returns *fully mitigates* both the intercept and the slope effects: not only are the effects of salience on each of those statistically significant (-13.2 p.p. on the intercept, significant at the 10% level, and 11.3 p.p. on the slope, significant at the 5% level), but, also, the combined effects of priming and salience on each are not statistically different from zero (p-values 0.645 and 0.674, respectively).

Figure 7 sheds light on why making investment returns top-of-mind mitigates the effects of financial worries. Restricting attention to the experimentally poor, salience almost completely reverses the negative slope of demand with respect to predicted returns among those parents.

Together with the findings about the effects of financial worries on cognitive outcomes, the results of the salience experiment strongly support the argument that the mechanism driving resource misallocation

triggered by financial worries is poverty-induced attention misallocation.[27] Experimentally poor parents become more attuned to short-term returns at the expense of attending to long-term returns of the educational investment; as a result, they waste resources when the investment has low returns, and miss opportunities to invest in their children when returns are high.

6.4 Salience vs. information

Since our salience experiment is based on informing each caregiver in the treatment group about the predicted return of the SMS program *specific to their child*, does it really capture the effects of attention reallocation? Or, instead, does it capture the effects of having caregivers update their beliefs about investment returns? The distinction matters; for the very least, policies aimed at making investment returns top-of-mind are much easier and less costly to implement that putting in place informational interventions (Bettinger et al., 2020).

To estimate the effects of salience alone, serendipity plays in our favor: as it turns out, predicted returns of the SMS program with respect to attendance are nearly uncorrelated with those on grade promotion (-0.0091). That happens because the SMS program increases attendance by basically the same extent among marginal and infra-marginal students when it comes to grade repetition. Because we never informed parents about the child-specific returns of the program with respect to the pass probability – only with respect to attendance –, when we estimate the effects of the priming on investment as a function of predicted returns with respect to grade promotion, we are essentially providing parents with information that is useless, in statistical sense, to predict the child-specific returns of the program with respect to the other dimension. Having said that, being assigned to the treatment group should make program's returns top-of-mind nonetheless. To summarize, resorting to our alternative measure of predicted returns provides a clean test of salience effects on investment decisions.

We turn back to Table 8, which estimates the full-interaction model. Column (4) regresses the investment indicator on the priming indicator and a series of interactions. Different from column (2), it uses predicted returns with respect to grade promotion as opposed to attendance. The regression also controls for students' and caregivers' characteristics, and for interactions of priming with indicators of low income and low educational achievement.

Once again, the priming systematically increases investment when the investment has no returns (by 17.2 p.p., significant at the 10% level) and systematically mutes demands' response to predicted returns (by 6.6 p.p. for each additional increase in attendance, significant at the 5% level). Salience of investment returns once again *mitigates* the intercept and the slope effects. While the effect of salience on the intercept coefficient is not precisely estimated, although as large as in the previous subsection (-14.1 p.p.), the combined effect of priming and salience on the intercept is not statistically different from zero (p-value 0.736). When it comes to the slope, the effect of salience is large and precisely estimated (-5.8 p.p., significant at the 10% level), and its combined effect with priming is also statistically different from zero (p-value 0.795).

Panel C in Figure 8 highlights how using this alternative measure of predicted returns yields the exact same empirical regularity that we found before. Among the experimentally poor, sharing information about

[27] Supplementary Appendix B complements the analyses by showing that salience does not systematically affect financial worries, and that it overturns the effects of the priming on cognitive outcomes.

investment returns greatly reduces the negative slope of the demand for the program with respect to returns – even when information refers to a different, uncorrelated dimension, statistically useless to predict returns with respect to the dimension at hand.

Consistent with Bettinger et al. (2020)'s results, those findings reveal that the effects of the informational intervention are driven by salience also in the context of this experiment, with implications for the design of policy interventions aimed at mitigating the adverse consequences of poverty-induced attention reallocation.

Most importantly, those results confirm that attention misallocation is the fundamental mechanism behind the effects of financial worries on investment decisions among the poor.

7 Concluding Remarks

Within the context of a lab-in-the-field experiment in which parents were offered the opportunity to invest in a real educational program, we have shown that poverty-induced attention misallocation leads them to focus on short-term gains at the expense of accurately evaluating investment returns. As a result, poverty causes parents to sub-optimally invest in their children's human capital: they waste resources when the program has low returns, and miss opportunities to invest when the program has high-returns – even in the absence of liquidity constraints or commitment problems.

Our results linking investment misallocation to attention misallocation among the poor might help explain puzzles related to under-investment in children's human capital and, more broadly, to misallocation of investments among the poor, across a variety of settings.

When it 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 and commitment problems were both absent from our experimental setup, the poor still failed to undertake profitable investment opportunities. Even worse, they also wasted scarce resources investing in the program to a much greater extent when it had no returns.

With financial distress on the rise among student debtors in the United States, according to multiple measures, such findings also link up to an active policy debate around the optimal design of student loans for higher education.[28] In the presence of poverty-induced attention misallocation, how those financial products are offered to those experiencing credit constraints is critical to determine their welfare implications.

More broadly, our experimental findings also have implications for how health or educational products are distributed to poor audiences. Business-to-consumer (B2C) models that sell directly to the poor, quite prevalent among startups backed by venture capitalists and philanthropic groups (such as online learning platform BYJU, in India, funded among others by the Chan Zukerberg Initiative), are likely prone to resource misallocation unless carefully designed to ensure that future returns are salient at the time of the investment decision. In contrast, distribution models that deliver such solutions at no cost to the poor (such as Khan Academy) have higher potential to promote better targeting and positive impacts. Byju Raveendram, founder of BYJU, argues that, compared to Khan Academy, the for-profit startup model has advantages:

[28] See, for instance, https://www.pewsocialtrends.org/2014/05/14/section-1-student-debt-and-overall-economic-well-being.

"We can invest so much more into making the content engaging and personalized".[29] Our results showcase that this model might have disadvantages, however, when prospective users are the ones making purchasing decisions subject to the psychological effects of poverty.

Adapting the environment to the poor individuals' psychology, either by making long-term returns top-of-mind, or by making decisions *automatic* (such as by granting universal access to such educational tools, paid for by governments or third-party organizations), may be key to mitigate the effects of poverty-induced attention misallocation, with the potential to break away from inter-generational poverty traps.

[29] See https://economictimes.indiatimes.com/small-biz/startups/mark-zuckerberg-bill-gates-try-opposite-paths-to-education-tech-in-india/articleshow/59246196.cms.

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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."

COGNITIVE TESTS

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 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 GROUP: "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 GROUP: "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

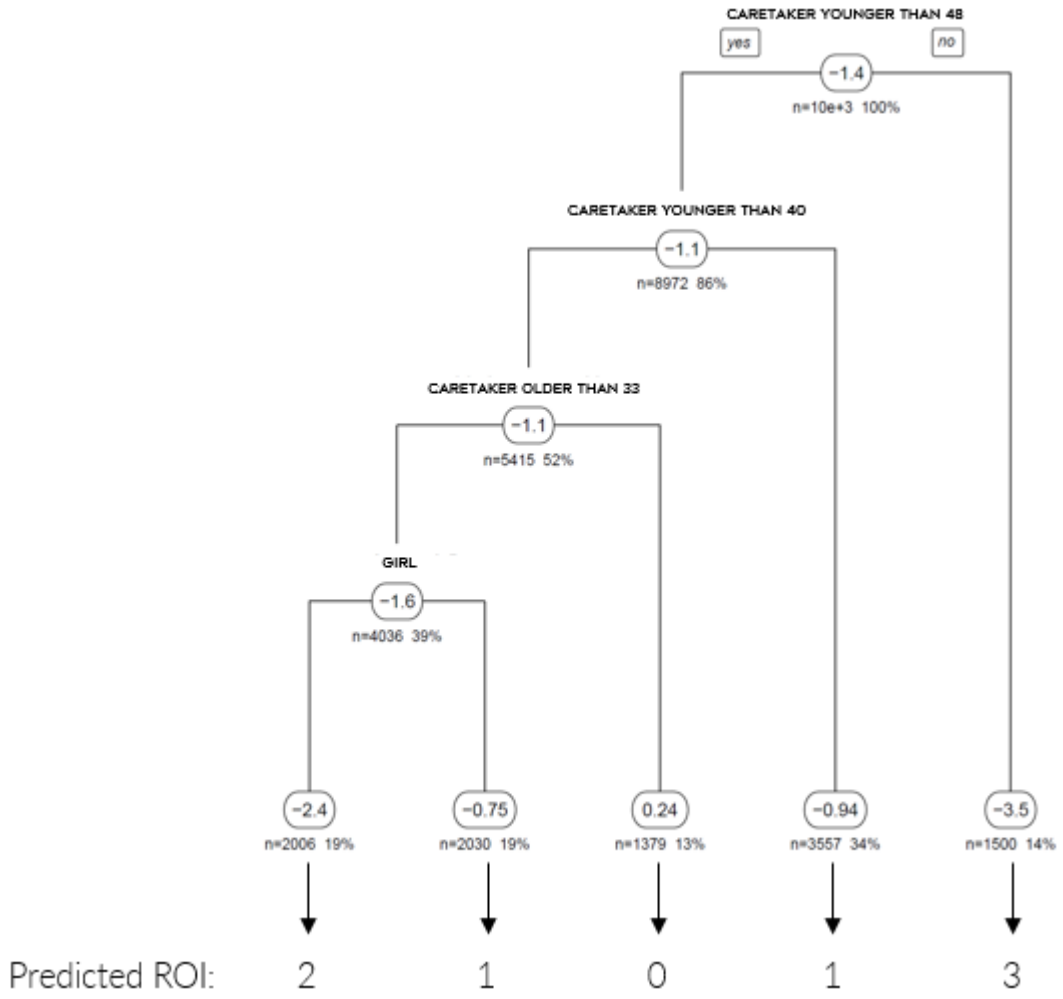
CONTROL GROUP: *At most* 1 school-wide SMS every other week with communication about school activities
TREATMENT GROUP: 1 SMS per week from the school with child-specific messages

SALIENCE EXPERIMENT

CONTROL GROUP: "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 GROUP: "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."

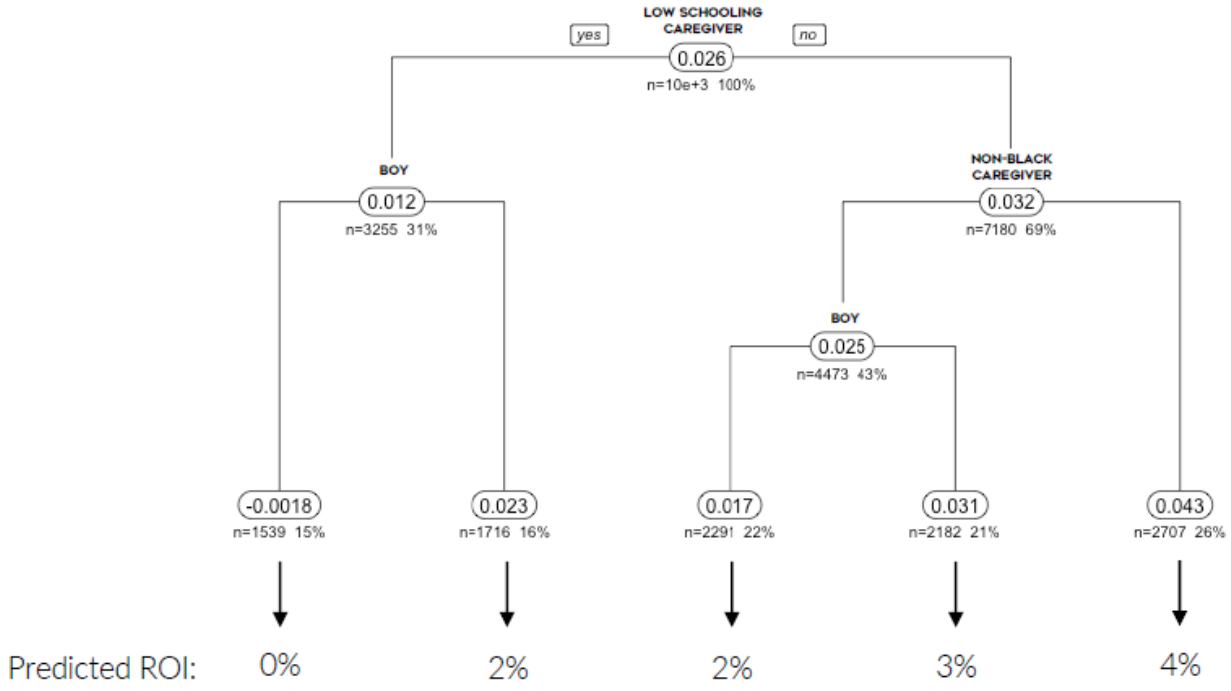
Appendix C – Figures

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



Notes to Figure 1:
(1) Predicted treatment effects of the SMS program on grade promotion, using a regression tree based on the estimates of Bettinger et al. (2020);
(2) $\hat{\beta}_i \sim f(black_i, caretaker_black_i,\ age_i, caretaker_age_i, mother_i, girl_i, poor_i, lowS_i)$, 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 estimation: split.Rule = "TOT", cv.option = "fit", minsize = 250, cv.Honest = F, split.Bucket = T, xval = 10, propensity = 0.8.

Figure 2 – Predicted treatment effects of the program on the probability of grade promotion



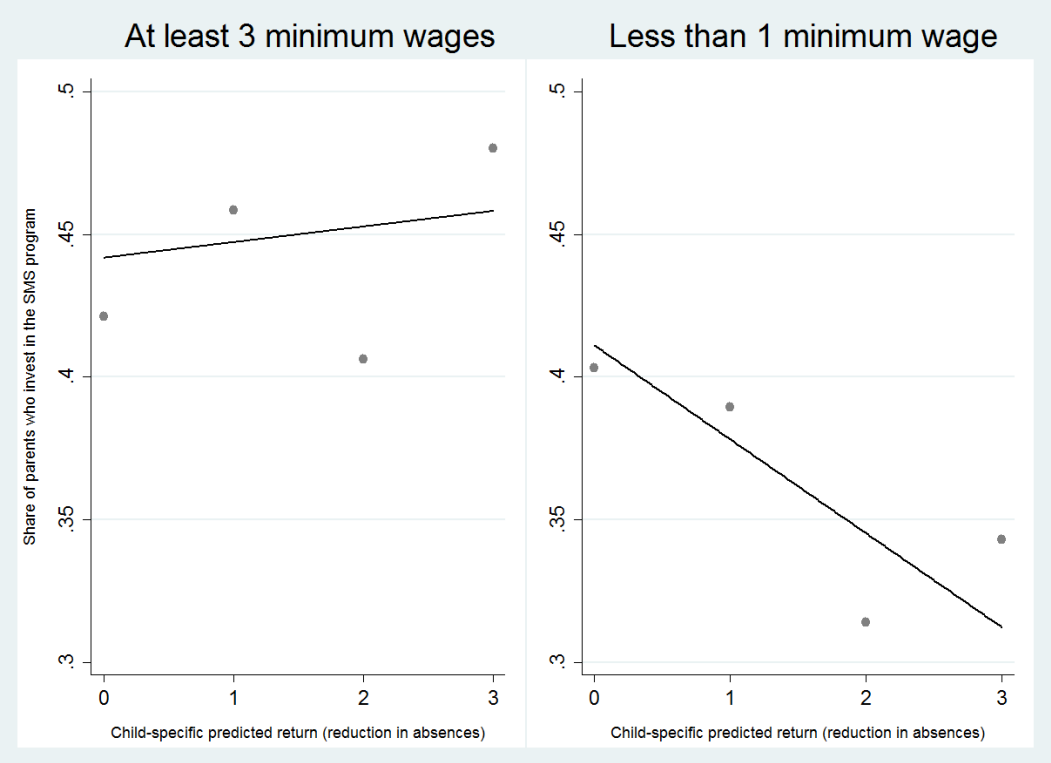
Notes to Figure 2:

(1) Predicted treatment effects of the SMS program on grade promotion, using a regression tree based on the estimates of Bettinger et al. (2020);

(2) $\hat{\beta}_i \sim g(black_i, caretaker_black_i, age_i, caretaker_age_i, mother_i, girl_i, poor_i, lowS_i)$, 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;

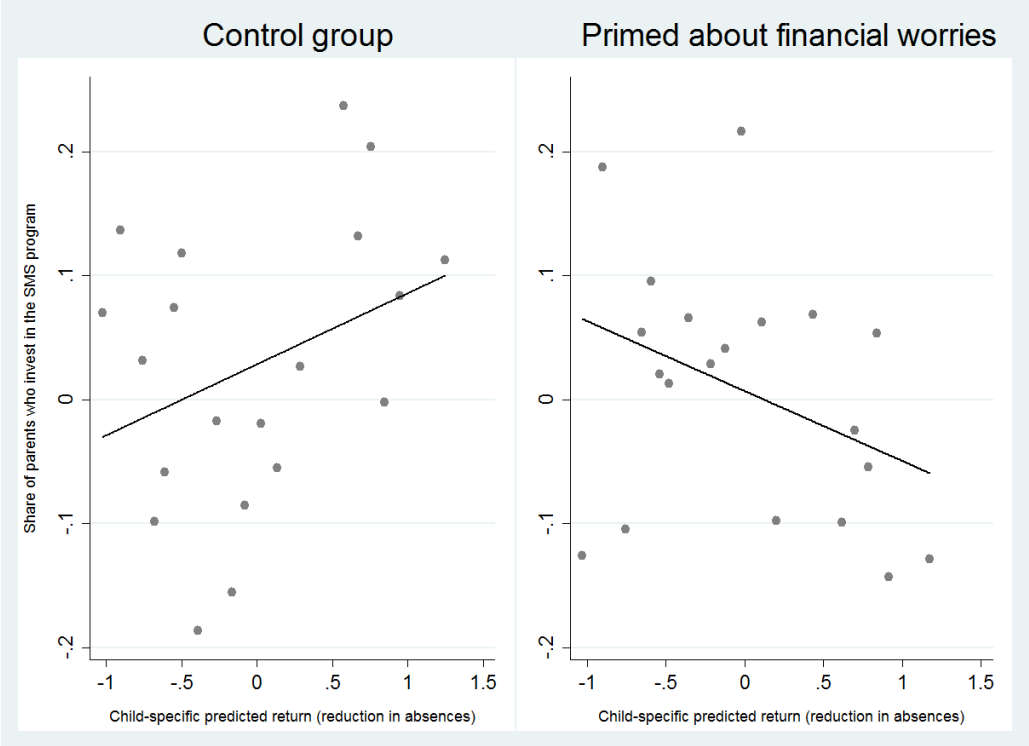
(3) Parameters of the tree estimation: split.Rule = "TOT", cv.option = "fit", minsize = 250, cv.Honest = F, split.Bucket = T, xval = 10, propensity = 0.8.

Figure 3 – Investment decision as a function of predicted returns, by caregivers' income



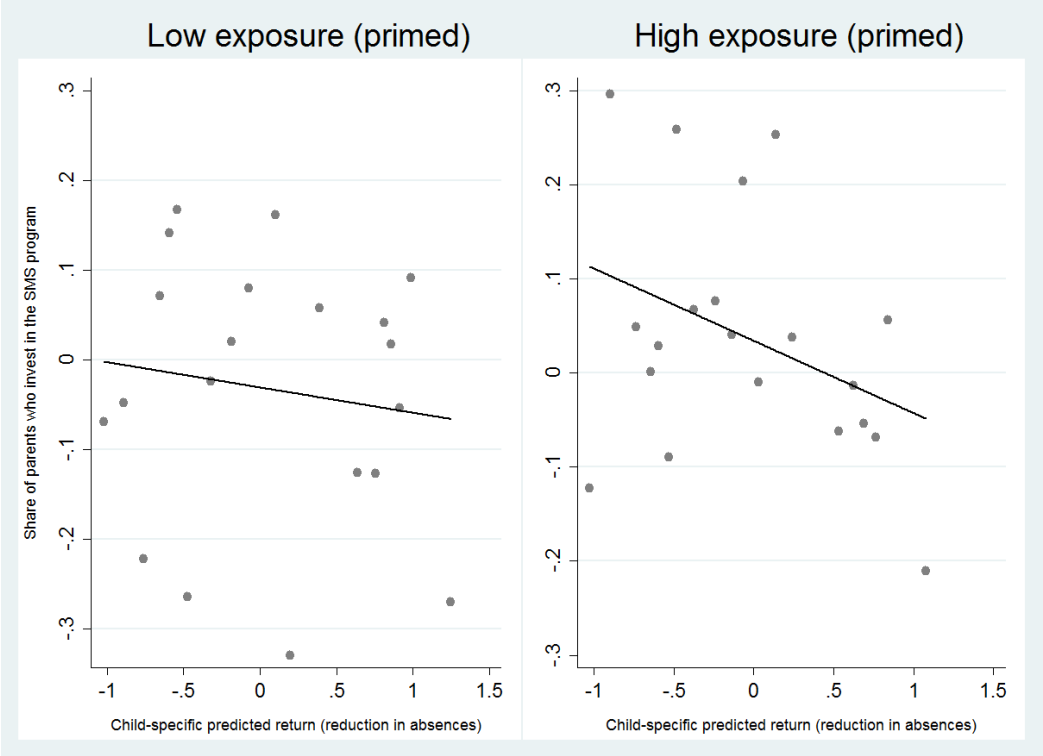
Notes to Figure 3:
(1) Both panels showcase binscatter plots with dependent variable = 1 if subjects choose to exchange R$ 10 in airtime credit for 6 months of the SMS program, and 0 otherwise (see Appendix A);
(2) The left-hand-side panel restricts attention to the sub-sample of caregivers with income of 3 minimum wages or higher, while the right-hand-side panel restricts attention to the sub-sample of caregivers with income of 1 minimum wage or lower;
(3) Child-specific predicted returns on school attendance generated from Bettinger et al. (2020)'s experimental results, using machine learning techniques (see Section 3).

Figure 4 – Investment decision as a function of predicted returns, by priming status (within control group of the salience experiment)



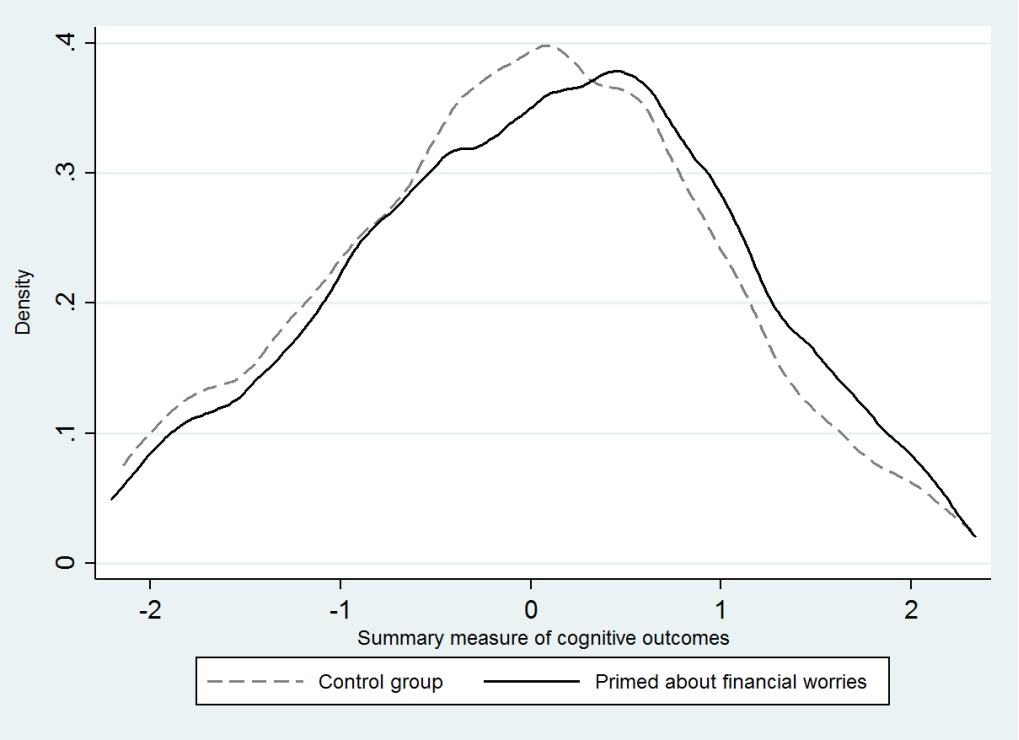
Notes to Figure 4:
(1) Both panels showcase binscatter plots with dependent variable = 1 if subjects choose to exchange R$ 10 in airtime credit for 6 months of the SMS program, and 0 otherwise (see Appendix A);
(2) Regressions within sub-sample not informed about child-specific predicted returns of the SMS program over text message (salience = 0; see Appendix B);
(3) The left-hand-side panel restricts attention to the control group of the survey experiment, while the right-hand-side panel restricts attention to the treatment group (those who are primed about financial worries; see Appendix B);
(4) Child-specific predicted returns on school attendance generated from Bettinger et al. (2020)'s experimental results, using machine learning techniques (see Section 3);
(5) Plot residualized with respect to controls, including student's race, age and gender, and their caregiver's race, age, gender, income and schooling.

Figure 5 – Investment decision of those primed about financial worries as a function of predicted returns, by prior exposure to the SMS program (within control group of the salience experiment)



Notes to Figure 5:
(1) Both panels showcase binscatter plots with dependent variable = 1 if subjects choose to exchange R$ 10 in airtime credit for 6 months of the SMS program, and 0 otherwise (see Appendix A) for those who are primed about financial worries (priming = 1; see Appendix B);
(2) Regressions within sub-sample not informed about child-specific predicted returns of the SMS program over text message (salience = 0; see Appendix B);
(3) The left-hand-side panel restricts attention to the control group of the prior exposure experiment, while the right-hand-side panel restricts attention to the treatment group (those who were assigned to the treatment group in Bettinger et al., 2020; see Appendix B);
(4) Child-specific predicted returns on school attendance generated from Bettinger et al. (2020)'s experimental results, using machine learning techniques (see Section 3);
(5) Plot residualized with respect to controls, including student's race, age and gender, and their caregiver's race, age, gender, income and schooling.

Figure 6 – Distribution of summary measure of cognitive outcomes by priming status

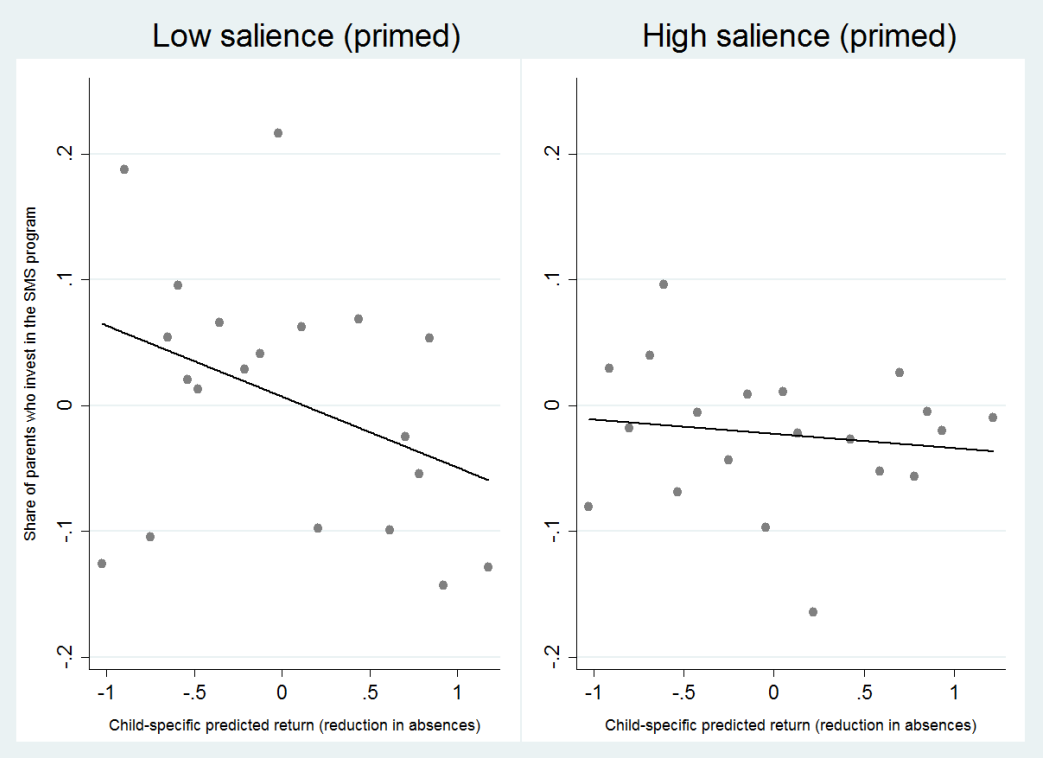


Notes to Figure 6:

(1) Kernel density estimates of summary measure of cognitive outcomes. 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) See Appendix B for the script of the survey experiment.

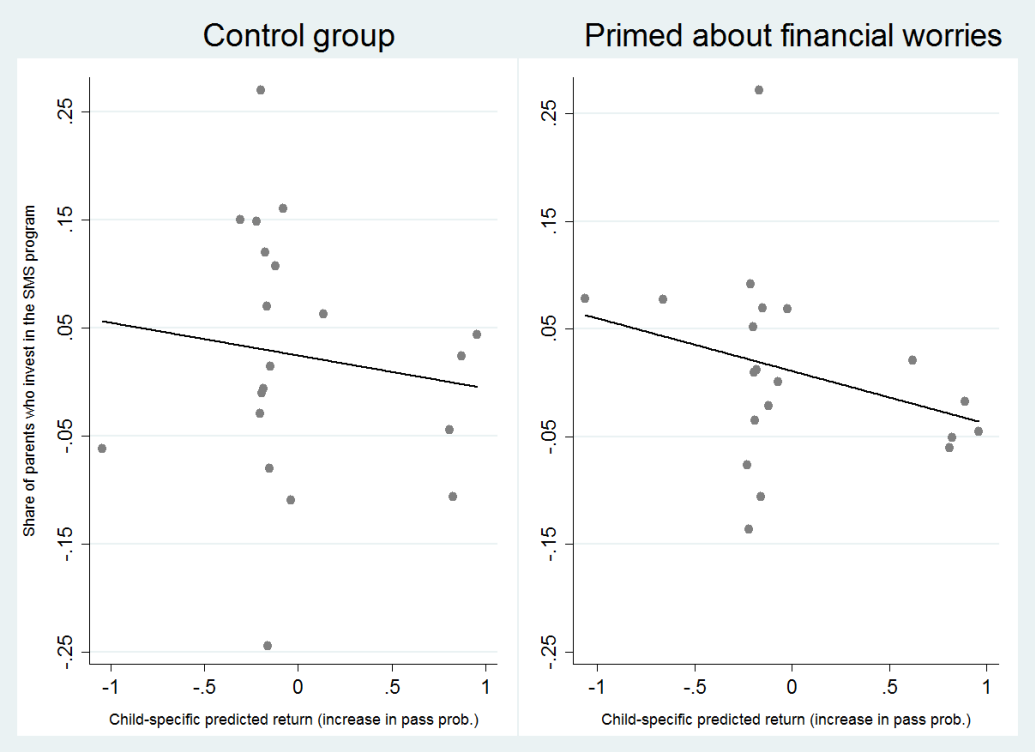
Figure 7 – Investment decision of those primed about financial worries as a function of predicted returns, by salience of future returns



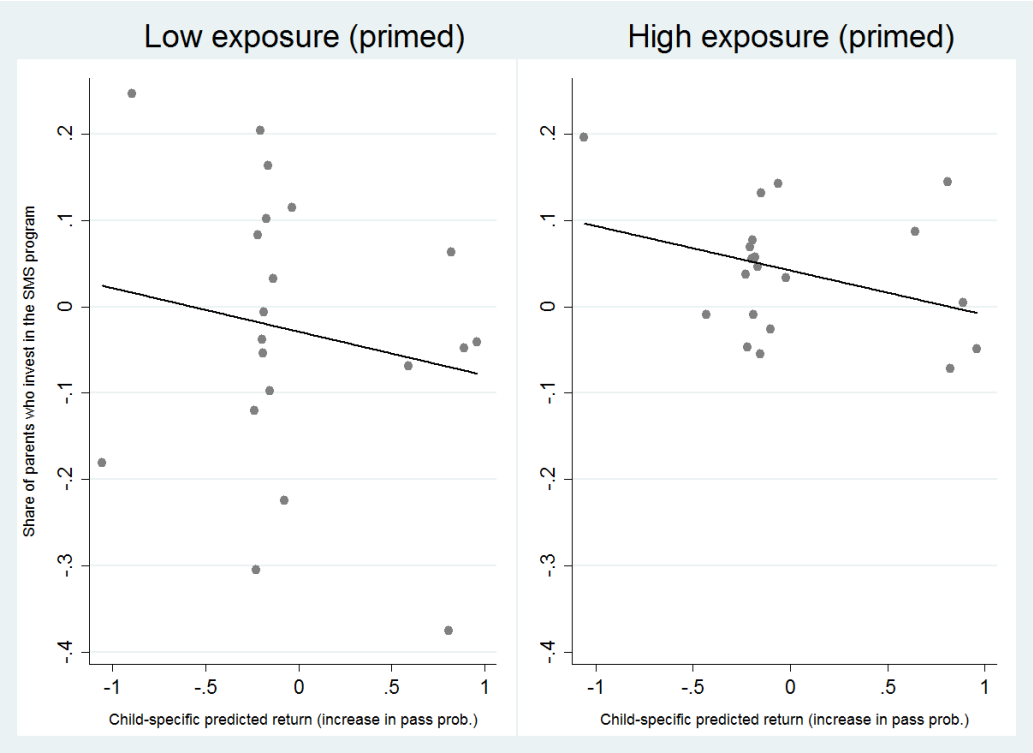
Notes to Figure 7:
(1) Both panels showcase binscatter plots with dependent variable = 1 if subjects choose to exchange R$ 10 in airtime credit for 6 months of the SMS program, and 0 otherwise (see Appendix A) for those who are primed about financial worries (priming = 1; see Appendix B);
(2) The left-hand-side panel restricts attention to the control group of the salience experiment, while the right-hand-side panel restricts attention to the treatment group (those informed about child-specific predicted returns of the SMS program over text message; see Appendix B);
(4) Child-specific predicted returns on school attendance generated from Bettinger et al. (2020)'s experimental results, using machine learning techniques (see Section 3);
(5) Plot residualized with respect to controls, including student's race, age and gender, and their caregiver's race, age, gender, income and schooling.

Figure 8 – Investment decision as a function of the predicted impact of the SMS program on the probability of grade promotion

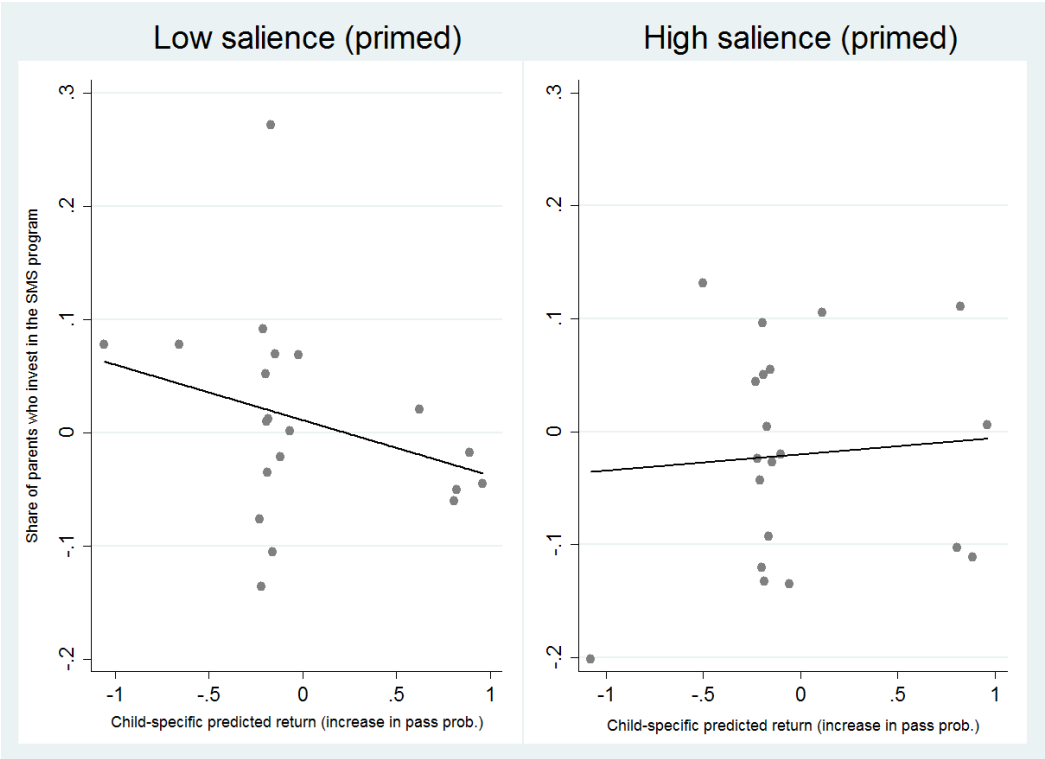
Panel A: Control vs. treatment in the survey experiment, among those in the control group of the salience experiment



Panel B: Control vs. treatment in the prior exposure experiment, among those primed about financial worries in the control group of the salience experiment



Panel C: Control vs. treatment in the salience experiment, among those primed about financial worries



Notes to Figure 8:
(1) Panel A documents the effect of being primed about financial worries on the decision to invest or not in the SMS program within the sub-sample assigned to control group of the salience experiment; Panel B, the effect of exposure to the SMS program, exploring random assignment to the treatment in Bettinger et al. (2020), among those primed about financial worries; and Panel C, the effect of being informed over text message about child-specific predicted returns of the SMS program on attendance, among those primed about financial worries. See Appendix B for the definition of treatment and control groups within each experiments);
(2) In each panel, both the LHS and the RHS showcase binscatter plots with dependent variable = 1 if subjects choose to exchange R$ 10 in airtime credit for 6 months of the SMS program, and 0 otherwise (see Appendix A);
(3) The left-hand-side panels restrict attention to the control group of each experiment, while the right-hand-side panels restricts attention to the treatment group (see Appendix B);
(4) Child-specific predicted returns on grade promotion generated from Bettinger et al. (2020)'s experimental results, using machine learning techniques (see Section 3);
(5) Plot residualized with respect to controls, including student's race, age and gender, and their caregiver's race, age, gender, income and schooling, as well as interactions of priming with an indicator of income below 1 minimum wage and with an indicator of caregiver with incomplete primary education.

Appendix D – Tables

Table 1 – Selective non-response tests

	Full sample			Sub-sample		
				Salience = 0	Salience = 0 & Prior exposure = 1	Salience = 1
	(1)	(2)	(3)	(4)	(5)	(6)
Priming	0.000522			-0.00253	-0.00407	-0.00344
	[0.00563]			[0.0128]	[0.0171]	[0.0123]
Prior exposure		0.00629				
		[0.00569]				
Salience			-0.00625			
			[0.00563]			
Priming x Predicted returns				0.00246	-0.0109	0.00257
				[0.00740]	[0.00990]	[0.00694]
Black or brown student				-0.00840	-0.0172	0.00130
				[0.0106]	[0.0142]	[0.0107]
Black or brown caregiver				0.0118	0.0242*	0.0106
				[0.0106]	[0.0143]	[0.0106]
Students' age				-0.00990*	-0.0154**	-0.000813
				[0.00579]	[0.00771]	[0.00588]
Caregiver's age				-0.00227***	-0.00191**	-0.00135***
				[0.000603]	[0.000802]	[0.000444]
Female student				0.0232**	0.00850	0.00799
				[0.00985]	[0.0131]	[0.00948]
Mother is the primary caregiver				-0.0116	-0.00264	0.00278
				[0.00819]	[0.0110]	[0.00802]
Less than 1 minimum wage				-0.0176*	-0.0144	-0.0347***
				[0.00907]	[0.0120]	[0.00897]
Incomplete primary education				0.0263**	0.0300**	-0.00471
				[0.0110]	[0.0147]	[0.0110]
Constant	0.112***	0.109***	0.116***	0.340***	0.422***	0.175*
	[0.00398]	[0.00431]	[0.00398]	[0.0893]	[0.119]	[0.0892]
Observations	12,597	12,597	12,597	6,313	3,595	6,267
R-squared	0.000	0.000	0.000	0.008	0.009	0.005

Notes to Table 1:

(1) All columns are OLS regressions with dependent variable = 1 if the subject answered the phone survey in full, and = 0 otherwise; (2) Priming = 1 if subjects are primed about financial worries, and 0 otherwise (see Appendix B); (3) Prior exposure = 1 if subjects were assigned to the treatment group in Bettinger et al. (2020), and 0 otherwise; (4) Salience = 1 if subjects are informed over text message about child-specific predicted returns of the SMS program on attendance, and 0 otherwise (see Appendix B); (5) Child-specific predicted returns on school attendance generated from Bettinger et al. (2020)'s experimental results, using machine learning techniques (see subsection 3.2); (6) Standard errors in brackets; *** p<0.01, ** p<0.05, * p<0.1.

Table 2 – Balance tests

Panel A: Survey experiment

	Salience = 0 sub-sample		
	Priming = 0	Priming = 1	[1]-[0]
Black or brown student	0.41	0.42	0.01
	[0.0205]	[0.0209]	[0.0292]
Black or brown caregiver	0.41	0.45	0.04
	[0.0206]	[0.0209]	[0.0293]
Students' age	14.72	14.68	-0.04
	[0.0280]	[0.0285]	[0.0399]
Caregiver's age	39.55	39.19	-0.36
	[0.289]	[0.294]	[0.412]
Female student	0.48	0.51	0.03
	[0.0208]	[0.0212]	[0.0296]
Mother is the primary caregiver	0.82	0.80	-0.02
	[0.0163]	[0.0166]	[0.0232]
Less than 1 minimum wage	0.19	0.19	0.01
	[0.0163]	[0.0166]	[0.0232]
Incomplete primary education	0.28	0.28	0.00
	[0.0186]	[0.0189]	[0.0265]
Predicted returns wrt attendance	1.27	1.29	0.02
	[0.0353]	[0.0359]	[0.0503]
Predicted returns wrt grade promotion	2.50	2.57	0.07
	[0.0530]	[0.0540]	[0.0757]
Observations			1,139
F-test (p-value)			0.668

Panel B: Prior exposure experiment

	Salience = 0 & Prior exposure = 1 sub-sample		
	Priming = 0	Priming = 1	[1]-[0]
Black or brown student	0.43	0.41	-0.02
	[0.0268]	[0.0279]	[0.0386]
Black or brown caregiver	0.43	0.44	0.02
	[0.0269]	[0.0280]	[0.0388]
Students' age	14.68	14.70	0.02
	[0.0354]	[0.0368]	[0.0510]
Caregiver's age	39.66	39.21	-0.45
	[0.382]	[0.398]	[0.551]
Female student	0.51	0.51	0.00
	[0.0271]	[0.0283]	[0.0392]
Mother is the primary caregiver	0.82	0.78	-0.04
	[0.0218]	[0.0227]	[0.0314]
Less than 1 minimum wage	0.22	0.17	-0.05
	[0.0149]	[0.0152]	[0.0310]
Incomplete primary education	0.28	0.27	-0.01
	[0.0243]	[0.0253]	[0.0351]
Predicted returns wrt attendance	1.31	1.25	-0.06
	[0.0456]	[0.0474]	[0.0658]
Predicted returns wrt grade promotion	2.52	2.59	0.07
	[0.0692]	[0.0720]	[0.0998]
Observations			654
F-test (p-value)			0.730

Panel C: Salience experiment

	Salience = 1 sub-sample		
	Priming = 0	Priming = 1	[1]-[0]
Black or brown student	0.40	0.42	0.02
	[0.0211]	[0.0212]	[0.0299]
Black or brown caregiver	0.43	0.43	0.00
	[0.0212]	[0.0213]	[0.0301]
Students' age	14.70	14.67	-0.04
	[0.0290]	[0.0291]	[0.0411]
Caregiver's age	38.95	39.76	0.808*
	[0.307]	[0.309]	[0.435]
Female student	0.53	0.51	-0.02
	[0.0214]	[0.0215]	[0.0304]
Mother is the primary caregiver	0.81	0.78	-0.03
	[0.0173]	[0.0174]	[0.0246]
Less than 1 minimum wage	0.16	0.14	-0.01
	[0.0153]	[0.0154]	[0.0217]
Incomplete primary education	0.27	0.25	-0.01
	[0.0188]	[0.0189]	[0.0266]
Predicted returns wrt attendance	1.21	1.34	0.12**
	[0.0359]	[0.0361]	[0.0510]
Predicted returns wrt grade promotion	2.58	2.57	-0.00
	[0.0526]	[0.0528]	[0.0745]
Observations			1,084
F-test (p-value)			0.351

Notes to Table 2:

(1) Panel A tests for differences in children's and their caregivers' characteristics across the treatment and control groups of the survey experiment within the sub-sample assigned to control group of the salience experiment; Panel B does so within the *high-exposure* sub-sample assigned to control group of the salience experiment; and Panel C, within the sub-sample assigned to treatment group of the salience experiment (see Appendix B);

(2) Priming = 1 if subjects are primed about financial worries, and 0 otherwise (see Appendix B);

(3) Prior exposure = 1 if subjects were assigned to the treatment group in Bettinger et al. (2020), and 0 otherwise;

(4) Salience = 1 if subjects are informed over text message about child-specific predicted returns of the SMS program on attendance, and 0 otherwise (see Appendix B);

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

Table 3 – Effects of the survey experiment on financial worries

	Very worried or desperate about household bills			
	(1)	(2)	(3)	(4)
Priming	0.0827***	0.0818***	0.0814***	0.0607
	[0.0213]	[0.0213]	[0.0212]	[0.0389]
Priming x Predicted returns				0.0161
				[0.0254]
Controls	No	Yes	Yes	Yes
Predicted returns fixed-effects	No	No	Yes	Yes
Control group average	0.564	0.564	0.564	0.564
Observations	2,103	2,103	2,103	2,103
R-squared	0.007	0.014	0.019	0.020

Notes to Table 3:

(1) All columns are OLS regressions with dependent variable = 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 about financial worries, and 0 otherwise (see Appendix B);

(3) Child-specific predicted returns on school attendance generated from Bettinger et al. (2020)'s experimental results, using machine learning techniques (see subsection 3.2);

(4) Controls include student's race, age and gender, and their caregiver's race, age, gender, income and schooling;

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

Table 4 – Effects of the survey experiment on investment decision
(within control group of the salience experiment)

	Invests in the SMS program				
	(1)	(2)	(3)	(4)	(5)
Priming	-0.0230	0.0474	0.103*	0.101*	0.101*
	[0.0293]	[0.0430]	[0.0533]	[0.0554]	[0.0561]
Priming x Predicted returns		-0.0547**	-0.0977***	-0.0976***	-0.0978***
		[0.0245]	[0.0347]	[0.0347]	[0.0347]
Priming x Less than 1 minimum wage				0.0113	
				[0.0755]	
Priming x Incomplete primary education					0.00922
					[0.0658]
Controls	No	No	Yes	Yes	Yes
Predicted returns fixed-effects	No	No	Yes	Yes	Yes
Control group average	0.434	0.434	0.434	0.434	0.434
Observations	1,139	1,139	1,139	1,139	1,139
R-squared	0.001	0.005	0.010	0.010	0.010

Notes to Table 4:
(1) All columns are OLS regressions with dependent variable = 1 if subjects choose to exchange R$ 10 in airtime credit for 6 months of the SMS program, and 0 otherwise (see Appendix A);
(2) Regressions within sub-sample not informed over text message about child-specific predicted returns of the SMS program on attendance (salience = 0; see Appendix B);
(3) Priming = 1 if subjects are primed about financial worries, and 0 otherwise (see Appendix B);
(4) Child-specific predicted returns on school attendance generated from Bettinger et al. (2020)'s experimental results, using machine learning techniques (see subsection 3.2);
(5) Controls include student's race, age and gender, and their caregiver's race, age, gender, income and schooling;
(6) Standard errors in brackets; *** p<0.01, ** p<0.05, * p<0.1.

Table 5 – Effects of the survey experiment on investment decision within high-exposure sub-sample (within control group of the salience experiment)

	Invests in the SMS program				
	(1)	(2)	(3)	(4)	(5)
Priming	-0.0661*	0.00283	0.0505	0.0479	0.0497
	[0.0391]	[0.0587]	[0.0721]	[0.0747]	[0.0753]
Priming x Predicted returns		-0.0554	-0.0966**	-0.0966**	-0.0967**
		[0.0352]	[0.0476]	[0.0477]	[0.0477]
Priming x Less than 1 minimum wage				0.0133	
				[0.100]	
Priming x Incomplete primary education					0.00327
					[0.0885]
Controls	No	No	Yes	Yes	Yes
Predicted returns fixed-effects	No	No	Yes	Yes	Yes
Control group average	0.509	0.509	0.509	0.509	0.509
Observations	654	654	654	654	654
R-squared	0.004	0.008	0.025	0.025	0.025

Notes to Table 5:

(1) All columns are OLS regressions with dependent variable = 1 if subjects choose to exchange R$ 10 in airtime credit for 6 months of the SMS program, and 0 otherwise (see Appendix A);

(2) Regressions within sub-sample assigned to the treatment group in Bettinger et al. (2020) (prior exposure = 1; see Appendix B) and not informed over text message about child-specific predicted returns of the SMS program on attendance (salience = 0; see Appendix B);

(3) Priming = 1 if subjects are primed about financial worries, and 0 otherwise (see Appendix B);

(4) Child-specific predicted returns on school attendance generated from Bettinger et al. (2020)'s experimental results, using machine learning techniques (see subsection 3.2);

(5) Controls include student's race, age and gender, and their caregiver's race, age, gender, income and schooling;

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

Table 6 – Effects of the survey experiment on cognitive outcomes
(within control group of the salience experiment)

	Summary measure of cognitive outcomes			Stroop	Digit span
	(1)	(2)	(3)	(4)	(5)
Priming	0.120***	0.114**	0.123	0.0795	0.102
	[0.0451]	[0.0448]	[0.0823]	[0.0826]	[0.0869]
Priming x Predicted returns			-0.00727	0.00485	-0.0142
			[0.0535]	[0.0537]	[0.0567]
Controls	No	Yes	Yes	Yes	Yes
Predicted returns fixed-effects	No	Yes	Yes	Yes	Yes
Observations	1,963	1,963	1,963	1,963	1,729
R-squared	0.004	0.026	0.026	0.018	0.026

Notes to Table 6:
(1) All columns are OLS regressions with a standardized cognitive outcomes as dependent variable. In columns 1-3, the dependent variable is 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, following Kling, Liebman and Katz (2004). Components of the summary measure are used as dependent variables in columns 4 (Stroop) and 5 (digit span); see Appendix A;
(2) Regressions within sub-sample not informed over text message about child-specific predicted returns of the SMS program on attendance (salience = 0; see Appendix B);
(3) Priming = 1 if subjects are primed about financial worries, and 0 otherwise (see Appendix B);
(4) Child-specific predicted returns on school attendance generated from Bettinger et al. (2020)'s experimental results, using machine learning techniques (see subsection 3.2);
(5) Controls include student's race, age and gender, and their caregiver's race, age, gender, income and schooling;
(6) Standard errors in brackets; *** p<0.01, ** p<0.05, * p<0.1.

Table 7 – Effects of the survey experiment on investment decision
(within treatment group of the salience experiment)

	Invests in the SMS program				
	(1)	(2)	(3)	(4)	(5)
Priming	-0.00862	0.00593	-0.0365	-0.0182	-0.0388
	[0.0296]	[0.0436]	[0.0539]	[0.0555]	[0.0554]
Priming x Predicted returns		-0.0109	0.0228	0.0216	0.0222
		[0.0240]	[0.0356]	[0.0356]	[0.0358]
Priming x Less than 1 minimum wage				-0.113	
				[0.0831]	
Priming x Incomplete primary education					0.0121
					[0.0680]
Controls	No	No	Yes	Yes	Yes
Predicted returns fixed-effects	No	No	Yes	Yes	Yes
Control group average	0.393	0.393	0.393	0.393	0.393
Observations	1,084	1,084	1,084	1,084	1,084
R-squared	0.000	0.000	0.026	0.027	0.026

Notes to Table 7:
(1) All columns are OLS regressions with dependent variable = 1 if subjects choose to exchange R$ 10 in airtime credit for 6 months of the SMS program, and 0 otherwise (see Appendix A);
(2) Regressions within sub-sample informed over text message about child-specific predicted returns of the SMS program on attendance (salience = 1; see Appendix B);
(3) Priming = 1 if subjects are primed about financial worries, and 0 otherwise (see Appendix B);
(4) Child-specific predicted returns on school attendance generated from Bettinger et al. (2020)'s experimental results, using machine learning techniques (see subsection 3.2);
(5) Controls include student's race, age and gender, and their caregiver's race, age, gender, income and schooling;
(6) Standard errors in brackets; *** p<0.01, ** p<0.05, * p<0.1.

Table 8 – Full-interaction model, by experiment and by measure of predicted returns of the SMS program

	Returns wrt attendance		Returns wrt grade promotion	
	Prior exposure	Salience	Prior exposure	Salience
	(1)	(2)	(3)	(4)
Priming	0.0687	0.107*	0.157*	0.172*
	[0.0604]	[0.0549]	[0.0943]	[0.0898]
Priming x Treatment	-0.0577	-0.132*	-0.0822	-0.141
	[0.0768]	[0.0758]	[0.0959]	[0.0951]
Priming x Predicted returns	-0.0438	-0.0974***	-0.0514*	-0.0661**
	[0.0392]	[0.0343]	[0.0307]	[0.0285]
Priming x Predicted returns x Treatment	0.0124	0.113**	0.0203	0.0584*
	[0.0509]	[0.0497]	[0.0338]	[0.0334]
Treatment	0.0850	0.0460	0.184***	-0.00142
	[0.0545]	[0.0532]	[0.0682]	[0.0671]
Predicted returns x Treatment	0.0280	-0.0684*	-0.0257	-0.0156
	[0.0373]	[0.0359]	[0.0242]	[0.0238]
Controls	Yes	Yes	Yes	Yes
Predicted returns fixed-effects	Yes	Yes	Yes	Yes
Control group average	0.345	0.434	0.345	0.434
Observations	2,223	2,223	2,223	2,223
R-squared	0.021	0.013	0.019	0.010

Notes to Table 8:

(1) All columns are OLS regressions with dependent variable = 1 if subjects choose to exchange R$ 10 in airtime credit for 6 months of the SMS program, and 0 otherwise (see Appendix A);

(2) In columns 1 and 2, predicted returns are with respect to attendance; in columns 3 and 4, with respect to the probability of grade promotion. Both child-specific measures are generated from Bettinger et al. (2020)'s experimental results, using machine learning techniques (see subsection 3.2);

(3) In columns 1 and 3, treatment corresponds to the prior exposure experiment; in columns 2 and 4, to the salience experiment;

(4) Priming = 1 if subjects are primed about financial worries, and 0 otherwise; prior exposure = 1 if subjects were assigned to the treatment group in Bettinger et al. (2020)' experiment, and 0 otherwise; and salience = 1 if subjects are informed over text message about child-specific predicted returns of the SMS program on attendance, and 0 otherwise. See Appendix B for a complete description of treatment and control groups within each experiment;

(5) Controls include student's race, age and gender, and their caregiver's race, age, gender, income and schooling, as well as interactions of priming with an indicator of income below 1 minimum wage and with an indicator of caregiver with incomplete primary education;

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

Supplementary Appendix A – Early Version of Pre-analysis Plan

Only an early version of the pre-analysis plan was registered at the AEA RCT Registry. It is presented in full in subsections A.1 through A.4 below. Subsection A.5 highlights the elements of the analyses that deviate from what had been specified in that pre-analysis plan.

A.1 Background

A growing body of evidence suggests that parents play a crucial role in shaping their children's behavior and performance in school (Houtenville and Conway, 2008; Nye, Schwartz and Turner, 2006; Barnard, 2004; Fan and Chen, 2001). In fact, differences in parental inputs are viewed as an important cause of intergenerational inequality (Becker and Tomes, 1979). This paper studies how poverty may impose a psychological tax on parenting, generating a poverty trap even on the absence of binding credit constraints for educational investments.

There is reason to think that this may be the case. While low-cost interventions have been shown to significantly increase parental engagement and students' outcomes (Kraft and Rogers, 2015; York and Loeb, 2014), such interventions tend to have significantly lower or no effects among minorities (black families in Chicago, in Fryer, Levitt and List, 2015, and Chinese families in San Francisco, in York and Loeb, 2014). One hypothesis for why that might be comes from the cognitive load/mental bandwidth theory (Mullainathan and Shafir, 2013): such parents might have less attention, memory and impulse control available to parenting, because poverty generates cognitive load (Mani et al., 2013; Lichand and Mani, 2016).

To test whether poor parents have a lower propensity to invest in their children due to cognitive load, we resort to survey experiments, priming some parents – but not others – about money, and then presenting them with the opportunity to undertake a costly educational investment in their child. The hypothesis of interest is whether those who are primed undertake such investment to a lesser extent.

Such empirical strategy raises three main concerns. First, if there is an effect, how do we know that it is driven by cognitive load, rather than just by a negative mental state following the arrival of bad news (Lerner et al., 2014)? Second, the fact that the investment is costly may confound the effect of cognitive load with that of credit constraints. And third, if the decision is hypothetical, then it is hard to get a sense of whether it matters quantitatively.

To deal with the first concern, we randomly assign subsamples of the subject pool to other sources of negative news: either a monetary shock of much lower magnitude, or a non-monetary shock. Moreover, we also try to delve more deeply into the mechanism, by varying the source of the large monetary shocks – either related or unrelated to educational expenses –, to address the possibility of mental accounting (Thaler, 1999).

To deal with the second concern, we create a decision that involves opportunity costs, but which does not require cash on hand. Specifically, we offer parents the choice between a free educational product for a longer period, or the same product for a shorter period coupled with an immediate top up in airtime credit. The idea is that if those who are primed about money choose the short-length plan, it cannot be because they were credit constrained to choose the longer-length one.

To deal with the third concern, we present parents with a decision about a real product. We offer enrollment in an SMS campaign that delivers content to support parenting, an increasingly popular intervention to foster parental engagement (York and Loeb, 2015), for either 2.5 or 5 months of the school year. Beyond having real consequences, we can actually measure the impact of parents' decisions on their children's education outcomes – attendance, grades and drop-out rates –. The psychological impact of poverty on those outcomes is then estimated by using priming status as an instrumental variable for parents' decisions between shorter- or longer-length plans.

Last, we also offer parents a savings technology, presenting all subjects with the trade-off between receiving an immediate 5-dollar airtime credit top-up for answering the survey, or a 10-dollar top-up one month later instead. Estimating the effects of the priming on subjects' discount rate provides us yet another way to benchmark priming's effect size on that educational decision.

While the cognitive load/mental bandwidth theory predicts that poverty might impose a psychological tax on poor parents' cognition, it also predicts that the poor should perform relatively better in tasks framed in monetary terms (Shah, Shafir and Mullainathan, 2015; Lichand and Mani, 2015). By precisely exploiting this mechanism, interventions to foster parental engagement among the poor might be improved by framing communication in monetary terms.

This paper also tests this hypothesis, by cross-randomizing how the educational investment is presented for parents (framed as either "good for your child" or "with potential to increase future wages"). We test whether the monetary framing increases uptake of the longer-length plan, particularly for the subjects primed about money.

A.2 Intervention and sample

Survey experiments are randomly assigned at student levels, within a sample of 60 Brazilian public schools, in order to estimate the psychological impacts of poverty on educational investments.

Using phone surveys, at the beginning of each call we randomly assign parents to one out of four possible questions, asking subjects to think about the consequences of: (i) a small monetary shock (a USD 10 unanticipated expenditure with a broken fridge; the control condition), (ii) a large monetary shock unrelated to education (a USD 100 unanticipated expenditure with a broken fridge), (iii) a large monetary shock related to education (a USD 100 unanticipated expenditure with school uniform and materials), and (iv) a large non-monetary shock (a unanticipated 7-week stop in water supply; the placebo condition).

Following that, we offer parents the opportunity to undertake an educational investment, by choosing the length during which they will be enrolled in an SMS campaign that delivers content to support parenting (at no cost). Subjects must decide between 2.5-months enrollment coupled with an immediate airtime credit top-up of 5 dollars, or 5-months enrollment with no top-up.

We cross-randomize how the educational investment is presented for parents (framed as either "good for your child" or "with potential to increase future wages").

Table - Research Design

	Non-monetary framing	Monetary framing
Small monetary shock	625 parents	625 parents
Large monetary shock unrelated to education	625 parents	625 parents
Large monetary shock related to education	625 parents	625 parents
Large non-monetary shock	625 parents	625 parents

A.3 Outcomes

The key outcome of interest is parents' decision to undertake or not the educational investment, which we will collect through the same IVR at which we run the survey experiments.

At the end of the intervention, the São Paulo Secretariat of Education will provide data on student attendance and grades in 2016 (per quarter), and enrollment in 2017. Moreover, the Secretariat implements annually a standardized test to all schools in the state of São Paulo, SARESP (System of School Performance Evaluation of the State of São Paulo). All students in grades 1st, 3rd, 5th, 7th, 9th of primary school and the final year of high school are tested on their knowledge of Mathematics and Portuguese.

A.4 Timeline & Milestones

#	Milestone	Target Start Date	Target End Date
1.	Meeting with the Regional Board of Education Directors and the São Paulo Secretariat of Education to explain the project	Apr-14	Apr-14
2.	Regional Board of Education Directors meet with their schools principals to explain the project	Apr-18	Apr-27
3.	Schools register to participate in the program (trough an online form)	Apr-18	Apr-27
4.	Meeting with Education Directors and school principals in each of the 5 Regional Board of Education head office to explain the project and distribute the enrollment material	May-9	May-17
5.	Schools organize meeting with parents to explain the project and obtain their cellphone and consent	May-10	May-30
6.	Teacher uploads parental enrollment information through secure website	May-10	June-2
7.	Randomization	June-3	June-5
9.	Baseline phone survey implementation	June-6	June-13
10.	SMS content and nudges begin	June-14	-
11.	End line phone surveys implementation	Dez-12	Dez-20
12.	SMS content and nudges end	Dez-20	-
13.	Impact Evaluation	Jan-30	Mar-31

A.5 Deviations from early version of pre-analysis plan

We had pre-registered the experimental design at the same time as the one in Bettinger et al. (2020), as we thought at the time the two would be conducted simultaneously. As such, the most important distinction from our analysis to that early version of the pre-analysis plan is that the latter did not set out to investigate demand for the SMS program as a function of predicted returns (since those would not have been available at that time).

Over the course of 2016, when the prior exposure experiment was taking place, we only piloted the survey and salience experiments analyzed in this paper, and concluded several changes were in order before the experiments were rolled out in 2017. Besides the issue of predicted returns – crucial for establishing whether differences in investment patterns induced by the priming are indeed inefficient –, we modified the two experiments in the following ways.

When it comes to the survey experiment, the early version of the pre-analysis envisioned different versions of the priming and control messages at the beginning of the phone call. Two arms were linked to small vs. large shocks related to education expenses (the ones that were ultimately kept through the main

experiment). There was an additional arm in which the priming was linked to a large shock unrelated to education, to assess whether it mattered that the shock and the investment were both within the same domain. The problem is that the shock we chose to use – a thought experiment in which the household's fridge had to be repaired – failed the manipulation test: financial worries did not increase nearly as much in response to that priming in the pilot. For this reason, we removed that variation from the main experiment. We also piloted a different version of the control condition: a thought experiment in which there was a lasting unanticipated cut in the household's water supply, a large shock unrelated to financial worries. Using that arm as a control condition seemed to yield similar results as using the small shock in the pilot; for that reason, we kept only the latter in the main experiment to maximize statistical power of the comparison and to keep it in line with the typical approach in prior studies.

In the salience experiment, we had envisioned that it would be also included as part of the phone survey, by framing the investment in alternative ways: in the control condition, we would merely state that the SMS program is "good for your child", while in the treatment condition, that it has the potential to "increase future wages". The pilot suggested, however, that mentioning wages just before the investment decision was also priming subjects about financial worries, conflating the two experiments. That led us to switch to implementing the salience experiment in a separate way, through text messages 2 days before the phone survey, in the main experiment. What is more, since in 2017 we could compute child-specific predicted returns, we used those predictions to make future returns top-of-mind in the treatment group, while for the control group we stated how many parents participated in the program in the previous year. To distinguish salience from information effects, we take advantage of the fact the child-specific predicted returns of the SMS program when it comes to attendance and the probability of grade promotion are uncorrelated (see subsection 6.4).

Supplementary Appendix B – Additional Results

Table B.1 presents balance tests for the prior exposure and salience experiments. In each case, only one variable is significantly different across the treatment and control groups, and neither test of joint differences is statistically significant, confirming that students' or their caregivers' characteristics do not confound the effects of the different treatments estimated in Table 8. Tables B.2 and B.3 document that neither prior exposure to the SMS program or salience of future returns significantly affect immediate financial worries. Table B.4 documents that while prior exposure does not affect cognitive scores, the salience intervention does: performance in short-term incentivized tests among those for whom future returns on investment are made salient decreases by over 0.1 standard deviation, relative to the control group. The effect size is nearly identical of that of the priming on cognitive scores, with the opposite sign – consistent with the finding that the salience intervention successfully shifted parents' focus away from short term returns, mitigating the effect of the priming on investment decisions.

Table B.1 – Balance tests in the prior exposure and salience experiments

Panel A: Prior exposure experiment

	Prior exposure = 0	Prior exposure = 1	[1]-[0]
Black or brown student	0.42	0.41	-0.01
	[0.0162]	[0.0137]	[0.0212]
Black or brown caregiver	0.42	0.43	0.01
	[0.0163]	[0.0137]	[0.0213]
Students' age	14.69	14.70	0.01
	[0.0222]	[0.0187]	[0.0290]
Caregiver's age	39.24	39.44	0.20
	[0.232]	[0.196]	[0.304]
Female student	0.50	0.51	0.01
	[0.0164]	[0.0139]	[0.0215]
Mother is the primary caregiver	0.82	0.79	-0.0309*
	[0.0131]	[0.0110]	[0.0171]
Less than 1 minimum wage	0.17	0.17	0.01
	[0.0124]	[0.0104]	[0.0162]
Incomplete primary education	0.27	0.27	0.00
	[0.0146]	[0.0123]	[0.0191]
Predicted returns wrt attendance	1.27	1.28	0.0117
	[0.0278]	[0.0234]	[0.0364]
Predicted returns wrt grade promotion	2.52	2.57	0.0510
	[0.0412]	[0.0348]	[0.0539]
Observations			2,223
F-test (p-value)			0.681

Panel B: Salience experiment

	Salience = 0	Salience = 1	[1]-[0]
Black or brown student	0.42	0.41	-0.01
	[0.0146]	[0.0150]	[0.0209]
Black or brown caregiver	0.43	0.43	0.00
	[0.0147]	[0.0150]	[0.0210]
Students' age	14.70	14.68	-0.02
	[0.0200]	[0.0205]	[0.0286]
Caregiver's age	39.37	39.35	-0.02
	[0.209]	[0.214]	[0.300]
Female student	0.50	0.52	0.02
	[0.0148]	[0.0152]	[0.0212]
Mother is the primary caregiver	0.81	0.79	-0.02
	[0.0118]	[0.0121]	[0.0169]
Less than 1 minimum wage	0.19	0.15	-0.0384**
	[0.0111]	[0.0114]	[0.0159]
Incomplete primary education	0.28	0.26	-0.02
	[0.0131]	[0.0135]	[0.0188]
Predicted returns wrt attendance	1.28	1.27	-0.00608
	[0.0250]	[0.0257]	[0.0358]
Predicted returns wrt grade promotion	2.53	2.58	0.0462
	[0.0371]	[0.0380]	[0.0531]
Observations			2,223
F-test (p-value)			0.589

Notes to Table B.1:
(1) Panel A tests for differences in children's and their caregivers' characteristics across the treatment and control groups of the prior exposure experiment; Panel B does for the salience experiment (see Appendix B);
(2) Priming = 1 if subjects are primed about financial worries, and 0 otherwise (see Appendix B);
(3) Prior exposure = 1 if subjects were assigned to the treatment group in Bettinger et al. (2020), and 0 otherwise;
(4) Salience = 1 if subjects are informed over text message about child-specific predicted returns of the SMS program on attendance, and 0 otherwise (see Appendix B);
(5) Standard errors in brackets; *** p<0.01, ** p<0.05, * p<0.1.

Table B.2 – Effects of the prior exposure experiment on financial worries

	Very worried or desperate about household bills			
	(1)	(2)	(3)	(4)
Prior exposure	0.0009	-0.0015	-0.0032	-0.0039
	[0.0216]	[0.0216]	[0.0216]	[0.0486]
Prior exposure x Predicted returns				0.0003
				[0.0171]
Controls	No	Yes	Yes	Yes
Predicted returns fixed-effects	No	No	Yes	Yes
Control group average	0.604	0.604	0.604	0.604
Observations	2,103	2,103	2,103	2,103
R-squared	0.000	0.007	0.010	0.010

Notes to Table B.2:

(1) All columns are OLS regressions with dependent variable = 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) Prior exposure = 1 if subjects were assigned to the treatment group in Bettinger et al. (2020), and 0 otherwise;

(3) Child-specific predicted returns on school attendance generated from Bettinger et al. (2020)'s experimental results, using machine learning techniques (see subsection 3.2);

(4) Controls include student's race, age and gender, and their caregiver's race, age, gender, income and schooling;

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

Table B.3 – Effects of the salience experiment on financial worries

	Very worried or desperate about household bills			
	(1)	(2)	(3)	(4)
Salience	0.0177	0.0167	0.0178	-0.0412
	[0.0213]	[0.0213]	[0.0213]	[0.0484]
Salience x Predicted returns				0.0231
				[0.0170]
Controls	No	Yes	Yes	Yes
Predicted returns fixed-effects	No	No	Yes	Yes
Control group average	0.596	0.596	0.596	0.596
Observations	2,103	2,103	2,103	2,103
R-squared	0.000	0.008	0.010	0.011

Notes to Table B.3:

(1) All columns are OLS regressions with dependent variable = 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) Salience = 1 if subjects are informed over text message about child-specific predicted returns of the SMS program on attendance, and 0 otherwise (see Appendix B);

(3) Child-specific predicted returns on school attendance generated from Bettinger et al. (2020)'s experimental results, using machine learning techniques (see subsection 3.2);

(4) Controls include student's race, age and gender, and their caregiver's race, age, gender, income and schooling;

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

Table B.4 – Effects of the prior exposure experiment on cognitive outcomes

	Summary measure of cognitive outcomes			
	Prior exposure experiment		Salience experiment	
	(1)	(2)	(3)	(4)
Treatment	0.0384	0.0410	-0.116**	-0.127***
	[0.0458]	[0.0455]	[0.0451]	[0.0448]
Controls	No	Yes	No	Yes
Predicted returns fixed-effects	No	Yes	No	Yes
Observations	1,963	1,963	1,963	1,963
R-squared	0.000	0.023	0.004	0.026

Notes to Table B.4:

(1) All columns are OLS regressions with standardized cognitive outcomes as dependent variable. In columns 1-4, the dependent variable is 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, following Kling, Liebman and Katz (2004). See Appendix A for the components of the summary measure;

(2) In columns 1 and 2, the independent variable is prior exposure, = 1 if subjects were assigned to the treatment group in Bettinger et al. (2020), and 0 otherwise. In columns 3 and 4, the independent variable is salience, = 1 if subjects are informed over text message about child-specific predicted returns of the SMS program on attendance, and 0 otherwise (see Appendix B);

(3) Child-specific predicted returns on school attendance generated from Bettinger et al. (2020)'s experimental results, using machine learning techniques (see subsection 3.2);

(4) Controls include student's race, age and gender, and their caregiver's race, age, gender, income and schooling;

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