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Poverty, Violence and Human Capital Formation

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Abstract

In recent years, there has been a growing commitment to studying the economic lives of the poor by better understanding their psychological lives (Banerjee and Duflo, 2007; Schilbach et al., 2016). These developments stem from the failure to empirically detect poverty traps, which have been at the core of the development literature for decades (Dean et al., 2017). Instead, empirical studies document the existence of very large rates of returns to investment, which, however, are not matched by observed consumption growth rates (Kremer et al., 2019). Several behaviors of the poor, which do not fit with traditional models, puzzled economists. Why do poor micro-entrepreneurs keep borrowing at extremely high interest rates instead of saving some of their profit to borrow less with each passing day (Ananth et al., 2007)? If using fertilizer has such high rates of return, why don't poor farmers purchase it (Duflo et al., 2008)? If the poor remain poor because they do not get enough calories, why do they spend their money on other things besides food (Banerjee and Duflo, 2007)? Such questions led to the rise of the subfield of Behavioral Development Economics, which applies insights from psychology and behavioral economics to study the economic behavior of the poor; trying to explain why and how it departs from standard economic models. Behavioral biases, studied extensively in Behavioral Economics, may be much more consequential for the poor. Failing to resist to the temptation of a hedonistic reward after a hard day of work will have very different implications for a poor person than for a rich one.

This thesis aims to contribute to this new strand of literature, in particular to one of its branches titled "the psychology of poverty", which studies the impact poverty has on cognitive function and economic behavior. One influential theory in this field is the scarcity/mental bandwidth theory (Mullainathan and Shafir, 2013), which states that poverty implies not only lack of financial resources, but also lack of mental resources to focus on other things besides pressing concerns. At any time, a poor person's mind will be preoccupied with worries about bills, school fees or health problems; and how to best manage all of them with very limited resources. While this makes the poor better at decisions regarding the pressing issue at hand (Shah et al., 2015), it also makes them neglect other important domains which may not appear urgent enough (Shah et al., 2012, 2015). While the theory may help explain many puzzling behaviors of the poor, up to now there has been little evidence on real-world economic outcomes. The first two chapters of this thesis try to bring the framework closer to real-world economic decisions even though restricted to the lab setting.

The main challenge with studying the psychology of poverty outside the lab is the fact that

even exogenous changes in income will affect several other channels besides mental bandwidth, making it very challenging to pin down the precise mechanism. Given this, the first two chapters are limited to varying mental bandwidth in a lab setting, keeping income fixed. The novel aspect is that the decisions participants make mimic closely everyday life purchasing decisions, involving real products. I note however, that due to limited funding and ethical considerations, in both chapters decisions are only weakly incentivized: only 1% of participants actually receive the goods they selected.

The first chapter explores the relationship between the psychology of poverty, investment in human capital, and financial incentives. Empirical evidence indicates that the poor are less attentive parents, investing less in the human capital of their children (McLoyd, 1998; Evans, 2004). This contributes to the inter-generational transmission of poverty because investing in human capital has extremely high rates of return, highest in early childhood (Cunha and Heckman, 2007; Cunha et al., 2010). The question is why don't the poor invest more? Traditional answers to this question put the blame on lack of knowledge of parenting practices, wrong beliefs on the expected returns or lower altruism. We propose an alternative explanation based on the scarcity theory. Poor parents may fail to invest the required time and resources in their child because their minds are preoccupied with other more urgent concerns. When there is uncertainty about how the next bill will be paid, spending time doing educational activities with the child may shift out of focus. When such behaviors keep repeating on a regular basis, a gap emerges between poor and non-poor children in the amount of cognitive and emotional stimulation they receive.

The challenge is how to test this hypothesis. Given the identification issues with disentangling such channels with observational data, we bring it to the lab. Parents of toddlers, living in the UK, are invited to participate in an online experiment. First, they are asked to answer how their family would deal with various hypothetical financial scenarios which vary in severity (hard for the treatment group, easy for the control group). Among the treated, the scenarios aim to bring financial worries to mind, trying to capture what people living in poverty experience on a regular basis. After completing the scenarios, parents receive a budget of £30 to be spent as they choose in an experimental market on 3 types of goods: necessities, child investment goods, and luxury goods.

Half of parents are incentivized to purchase more child investment goods by being offered a 50% discount. This treatment investigates if financial worries change how parents respond to such incentives, and is motivated by the results in Das et al. (2013) which find that accounting for household re-optimization in response to a policy is crucial when evaluating its effects.

We find that the incentive increases investment in human capital among lower income parents only when financial worries are not salient. When worries become salient, low income parents do not invest more but instead use the additional money to increase their demand of necessities. In addition, they also lower their demand for luxury goods to zero. When no discount is offered, we do not find financial worries to lower investment, which is likely to be explained by floor effects. Among higher income parents, financial worries do not affect behavior.

The effects among lower income participants are driven by those who were further away from their last paycheck at the time of the experiment - an indicator of real world monetary scarcity. This finding increases the external validity of our main results.

The second chapter departs from studying the human capital of children, focusing instead on the human capital of adults[1]. Addictive (or temptation) goods have been at the core of academic and policy debates for decades. With Becker and Murphy (1988), addiction was rationalized as a utility maximizing decision where the individual fully internalizes the costs of consuming such goods. In this framework, the only scope for intervention is to balance out the externalities – the costs that individuals place on society through consumption decisions (e.g. healthcare costs). Gruber (2001) questioned theoretically and empirically the rational framework, showing that with inconsistent time preferences, individuals do not fully internalize the cost of their behavior. Further studies have confirmed these findings which increased the scope of policy interventions (Gruber and Kőszegi, 2004; O'Donoghue and Rabin, 2006; Allcott et al., 2019a).

The most widely used policy tools to limit the over-consumption of temptation are "sin" taxes, popular among governments because they bring large revenues. However, such taxes have sparked debates regarding their effects on income distribution. Since the poor tend to spend a higher share of their budget on temptation, they are likely to pay a higher cost. On the other hand, they are also the ones expected to benefit more in terms of health by consuming less. Traditionally, such taxes were placed on tobacco and alcohol. Recently, several governments have started adding taxes also on the consumption of unhealthy foods, such as sugary drinks and beverages. Crucial to determining the effect of the tax is the elasticity of demand with respect to price and the degree to which individuals are not internalizing their choices (Allcott et al., 2019a).

The second chapter integrates the economics of temptation with the scarcity theory, and investigates if financial worries affect (i) the demand for temptation and (ii) the elasticities of demand with respect to price (sin taxes). The first question is not straightforward in the scarcity framework. While poverty is scarcity of financial resources, it is also scarcity of immediate

[1]However, the behavior studied is likely to have negative externalities also on children (e.g. domestic violence).

gratification. The poor have stressful lives and jobs which are often less rewarding and highly physically demanding. Compensating for these struggles is harder since they can only access a small set of potential alternatives to addictive goods (e.g. going to nice restaurant and travelling are not really in the choice set of the poor).

Following a similar design as in the first chapter but with a less specific population (adults living in the UK), we randomly trigger financial worries before asking participants to choose between necessities and temptation goods in an experimental market. The basket of temptation goods offered includes tobacco, alcohol and unhealthy foods and we simulate "sin taxes" by randomly increasing the price of temptation by 10% or 20%.

We find that triggering financial worries lowers the demand for temptation but also dampens demand elasticities. The effects are stronger among low income participants. When financial worries are salient, their demand curve is actually slightly upward sloping. The finding is puzzling: financial worries appear to limit over-consumption of temptation, but they also hurt the poor the most when additional taxes are introduced. We find suggestive evidence that both effects are mediated by an increased focus on urgent necessities.

The first two chapters integrated the scarcity framework into public policies. The results are very consistent across studies and have clear policy implications. Among the poor, when monetary concerns are top of mind: (i) incentivizing investments in human capital may not achieve its desired outcome, (ii) (dis)incentivizing consumption of temptation through new taxes may harm the poor the most since they do not lower their demands in response to price increases, which leads, through taxation, to a transfer of funds from the poor to the nonpoor without having any corrective effects (see Bernheim and Rangel, 2004; Bernheim and Taubinsky, 2018). However, I must note that both chapters make only speculative policy recommendations given that they lack the normative counterfactual. Further research is needed to rigorously establish the welfare implications of financial worries.

The third chapter takes a step back from economic decisions to studying how violence exposure affects cognitive function in children. Unfortunately violence and poverty are closely linked in a vicious cycle. Economically deprived neighborhoods are in general also more violent. In addition to monetary concerns, the minds of the poor are likely to be preoccupied with safety concerns.

This study attempts to apply the framework in Mullainathan and Shafir (2013), focusing on security concerns instead of monetary ones. While the link between the scarcity framework and violence as scarcity of security is novel and up for debate, the chapter is closely connected with the literature on the impact of emotions on cognition and decision making (Loewenstein

and Lerner, 2003; Lerner et al., 2003, 2015; Callen et al., 2014; Bogliacino et al., 2017). In a lab-in-the-field experiment, primary school children in El Salvador are randomly assigned to recall episodes of violence exposure before or after taking cognitive tests. I find that recalling violence exposure before taking the tests, increases cognitive performance by 0.2 standard deviations, effect significantly stronger for children reporting higher exposure. The estimates contrast previous findings on the effect of violence and cognitive function (Sharkey, 2010; Sharkey et al., 2012; Bogliacino et al., 2017) and call for further research in the field.

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Chapter 1

Psychology of poverty, financial incentives and parental investment in early childhood

with Anandi Mani, Piero Ronzani and Lucia Savadori

Empirical evidence on human capital of children indicates a strong correlation between low socioeconomic status and poor parental investment. This pattern is commonly attributed to lack of knowledge, resources or wrong beliefs on the returns to investment among such parents. This paper tests an alternative mechanism, based on scarcity: that pressing immediate financial concerns of poor parents capture their mental bandwidth, reducing their attention to important but less urgent parenting tasks. Using an online survey experiment, we study how financial concerns affect parental purchase decisions across three categories of goods and their responsiveness to discounts for child investment goods. We find that when offered a pure discount on child investment goods, all parents, rich and poor, respond rationally by spending more on them. However,triggering financial concerns leads to a divergence in their response: richer parents continue to demand more of child investment goods, but poorer parents shift purchases to household necessities, away from child investment goods and also personal goods. Our results show that mental bandwidth challenges (i.e. scarcity) could be a distinct reason why poor parents don't invest sufficiently in their child's human capital despite financial incentives, independent of their knowledge or beliefs.

JEL Codes: D11, D87, I14, I32, J24

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1.1 Introduction

The socioeconomic status of the family is a strong predictor of children's human capital in early years, affecting their outcomes throughout life (McLoyd, 1998; Garces et al., 2002). Poor children receive significantly less home-based stimulating experiences, warmth and support, and usually face more authoritarian parents and domestic violence (Evans, 2004). There is a growing consensus that early childhood represents a key window of opportunity for interventions to reduce human capital inequality and the inter-generational transmission of poverty (Almond and Currie, 2011; Walker et al., 2007; Cunha et al., 2010). The higher neuroplasticity of the brain in early childhood makes it more prone to fast growth but at the same time more vulnerable to shocks and environmental stressors (Thompson and Nelson, 2001; Heckman et al., 2006; Carneiro et al., 2003).

Even though the evidence on the benefits of early childhood interventions has increased substantially in the last decade, there are great challenges in scaling up the programs. Despite being cost-effective in the long run (Heckman et al., 2010), interventions are usually intensive and costly and can vary greatly in effectiveness (Gennetian et al., 2017). Engaging the parents seems to be key (Heckman et al., 2013) but treatment effects on parenting are mixed and tend to fade out after interventions end.

While the literature usually attributes the lack of parental investment of poor individuals to low levels of education or wrong beliefs on expected returns to investment (Attanasio, 2015), recent developments in the psychology of poverty suggests an alternative explanation (Gennetian et al., 2017). Poverty can affect the mental capacity to focus on other issues except for immediate pressing concerns by reducing mental bandwidth (Mani et al., 2013; Shah et al., 2012). This can come at the expense of decisions with long-term consequences, such as investing in human capital.

Spending time in educational activities with the child might mean not attending to more pressing concerns. The benefits, discounted from the future, are also subject to uncertainty. There is evidence indicating that poverty can impact all three dimensions - mental bandwidth, time preferences and risk preferences (Haushofer and Fehr, 2014; Mani et al., 2013), making it very hard to study the link between mental bandwidth and human capital investment outside the lab. Even if income shocks would be randomly assigned, the change in financial resources is expected to impact humcan capital investment through several other channels besides mental bandwidth.

Policies aimed at increasing human capital need also to account for the household response to the policy. Das et al. (2013) find, in two different country settings, that household re-optimize

their spending decision in response to policies providing school inputs to children, offsetting almost entirely their own inputs. Such responses could occur also in different settings: parents might substitute own time investments with the time spent by a professional in a home visiting program or could re-optimize spending on nutritional foods in response to a policy offering nutritional supplements.

Following these lines of research, the present study aims at understanding how the mental burden of poverty affects parental investment in human capital of children, testing also if it changes the response of parents to a policy offering investment goods at a subsidized cost.

We run a 2×2 experimental study in which participants, parents with children under the age of 4, living in the UK, are randomly assigned to one of four conditions resulting from the combination of a psychological manipulation with an economic one. The psychological manipulation, adapted from Mani et al. (2013), asks participants to respond how they would cope with various hypothetical financial scenarios. The treatment aims at mirroring the mental burden of poverty by making FW salient.

After completing the scenarios, parents proceed to the investment task. Parental investment decisions are simulated in an experimental setting by having participants choose how to spend a budget of £30 in an experimental market between groceries, educational goods for children and luxury goods sold at market prices. The three categories of goods proxy pressing needs, parental investment and temptation. The products were chosen based on their popularity on the websites of the retailers to ensure a higher probability that participants are highly familiar with them. Half of participant receive a 50% discount on the price of investment goods. The aim of this treatment is to identify if parents respond differently, when FW are salient compared to when they are not, to a policy incentivizing investment goods by making them cheaper. The task is weakly incentivized: 1% of participants are randomly chosen to actually receive the products they selected with the £30 budget.

Shah et al. (2015, 2018) show that poor can be better at recognizing trade-offs made against pressing needs and often behave more in line with traditional economic predictions. On one hand, assuming investment goods and pressing needs goods are normal goods and substitutes, lowering the price of investment goods should increase the demand for such goods. On the other hand, if FW increase the focus on pressing needs, the decrease in the price of investment goods allows parents to demand more pressing needs goods, while still demanding the same amount of investment goods as they would in the absence of the the incentive.

The results are as follows: among lower income parents, increasing FW changes how they respond to the financial incentive. In the baseline group, the discount increases the demand

for investment goods by £7 (roughly the control group mean, $p - value < 0.01$). In contrast, when offered the discount under high FW, parents do not invest significantly more (β = £1.94, $p - value$ = 0.43) but instead demand more pressing needs goods (β = £4, $p - value$ = 0.02), providing evidence for the increased focus effect. The difference between the estimates for the two groups receiving the discount is significant at 10% level. Among higher income parents, FW do not significantly impact behavior.

The results have relevant policy implications. When financial concerns are salient, policies aimed at enhancing investment in human capital might prove ineffective among low income households. Parent might use the extra resources (monetary, time) to address urgent needs. For instance, a parent receiving home visits could substitute regular time with the child with the time spent by a professional during the visit, and re-allocate the gained time on more pressing issues. Conversely, policies reducing the mental tax of poverty (insurance, simplifying information etc.) might have a positive indirect impact on investment in human capital without having to change parents' parenting knowledge or beliefs.

This paper is structured as follows. Section 3.2 provides an overview of the most relevant literature. Section 2.3 discusses the empirical design, dataset and main variables. Section 3.5 presents the main results and heterogeneous treatment effects. Finally, section 2.5 concludes the paper.

1.2 Literature

Parenting interventions usually assume that the information taught to parents will be implemented in daily life on a regular basis (Gennetian et al., 2017). Theoretical models of human capital development as in Cunha and Heckman (2007) assume that parents, using their knowledge and beliefs, make fully rational cost-benefit analysis when deciding how much time and resources to invest in the child. But, a poor parent, facing regular income instability might not succeed to tell a story to the child regularly because her mind is distracted with more pressing concerns, or because the discounted benefits appear smaller compared to the cost of not attending fully to the problems of today.

Macours et al. (2012) emphasize that the literature on early childhood development has focused more on supply-side interventions and less on what affects parental investment in children. Interventions focus on children's needs and hardly address parental behavior (Gennetian et al., 2017). Gennetian and Shafir (2015) points out that , to be successful, interventions need to induce regular practice from parents which usually live very financially unstable lives. This could explain why many costly early childhood interventions had modest results (Mayer et al., 2015). Even the Jamaican study, one of the most impressive long term early childhood inter-

ventions, evaluated in Gertler et al. (2013), had only a temporary impact on parenting. Initial lack of information cannot explain the dissipation of effects. Participating in the training might have made investing in children more salient but once the intervention ended, despite having the necessary knowledge, parents did not implement what they learned. This could be explained under our hypothesis.

In the past years, the literature documenting the impact of the psychology of poverty on economic outcome has grown substantially. In a seminal paper Mani et al. (2013) uses two study designs to test the impact of FW on mental function. In the first study, they make people from different socio-economic backgrounds think how they would deal with everyday financial decisions, either easy or difficult. Then they are given a fluid intelligence test. Poor individuals in the hard condition have significantly lower scores than poor in the easy condition, while for the rich there is no difference between the two conditions. In the second study, they find statistically significant differences in the mental capacities of sugarcane farmers before and after harvest. They experience cycles of poverty due to their inability to smooth consumption. Carvalho et al. (2016) tries to replicate the design of the second study in Mani et al. (2013) by applying it to the context of US poor before and after payday. They only found an impact on the degree of present bias for decisions with monetary rewards and no differences in cognitive functions, risk taking and other economic outcomes. However, it is worth noting that the farmers in Mani et al. (2013) experience much higher time lags between cycles of poverty and also face higher relative changes in income.

Using a difference-in-differences design, Gennetian et al. (2011) finds that incidence of disciplinary events is 40% higher at the end of the month for students from households receiving food stamps than for non-recipients in Chicago Public Schools. Unfortunately, due to lack of data, there are little insights on the potential mechanisms.

Deck (2015) summarizes the main experimental literature on the effect of cognitive load on risk preferences, impatience, generosity and strategic behavior. Under increased cognitive load individuals tend to be more risk averse and somewhat more impatient (also more likely to make more random choices). The results on generosity are mixed while in strategic games it lowers cooperation and sophistication in repeated games. Moreover, cognitive load reduces numeracy skills and increases anchoring.

Despite the rise of more realistic models of human behavior in economics, until recently, little attention was paid to how these models could apply to parenting. A recent experiment using behavioral insights (text reminders, goal-setting, and social rewards) shows promising results on parental engagement Mayer et al. (2015). The effect is not explained by the information

component of the treatment and it is strongest among present biased parents.

York et al. (2018) evaluate a low cost intervention with impressive impact on child literacy. The intervention delivered almost daily text messages to parents from low socio-economics backgrounds with easy to follow instructions for activities aimed at increasing early childhood literacy. The results are particularly impressive since most long term successful early childhood interventions are very costly and difficult to scale. However, the design does not allow to separate the impact of the information component from the effect of the daily reminders. The intervention might have increased salience of parental investment and reduced the required mental effort by simplifying the choice of parents and keeping the activities simple. In a SMS communication intervention with parents of 9th graders in Brazil, Cunha et al. (2017) find that the results are mainly driven by the effects of the salience component.

To our knowledge, to date Lichand et al. (2018) is the only study investigating the psychological effects of poverty on the willingness of parents to invest in an SMS educational program. The experiment involved over 2,500 parents of high-school students in Brazil, which have been enrolled in the program in the past 12 months. Similar to our design, before being offered to invest in the program, financial worries are randomly induced through hypothetical scenarios. Parents assigned to the difficult scenarios, over-invest in the SMS program when expected returns are low and under-invest when returns are expected to be high. Prior experience with the program does not reverse the trend, which disappears only the expected returns are communicated to the parents in advance. Our results complement the findings in Lichand et al. (2018) providing another context in which financial worries may lead to potentially misallocation of resources. Additionally, our design (i) provides clearer evidence of the underlying - change of focus towards pressing needs - mechanism, and (ii) approximates more closely the everyday life decisions that low-income people make. An important limitation of this work, which Lichand et al. (2018) addresses, is taking into account individual returns to investment, which we were unable to measure since (i) the study was performed online and (ii) the children in our target group are bellow the age of 4. Lacking this information, we are unable to determine whether financial worries lead to inefficiencies or not.

The main takeaway from these studies is that being poor might not only mean lack of financial resources but also scarcity of mental resources in making decisions and judgments (Schilbach et al., 2016). Gennetian and Shafir (2015) and Gennetian et al. (2017) discuss the importance of using behavioral economics when designing parenting interventions and give examples of how the insights could be used to improve existing programs. However, these recommendations rest on scarce evidence on the link between poverty induced monetary concerns and human capital investment. This is the gap this study tries to address. First, we try to answer whether monetary

concerns affect investment decisions. Second, we offer parents a financial incentive to invest and test whether their response to the incentive varies with the salience of monetary concerns. We provide evidence on the underlying mechanism driving the results: attention being reallocated towards pressing needs.

1.3 Empirical Strategy

This section presents the empirical strategy starting with the experimental design in subsection 1.3.1, followed by the description of the data collection in subsection 1.3.2. Subsection 1.3.3 presents descriptive statistics, and checks for balance and selective attrition across treatment arms.

1.3.1 Experimental Setting and Design

The experimental study was conducted online in the UK using an experimental platform called Prolific (Palan and Schitter, 2017). Prolific is a rapidly growing online service that facilitates social science research, offering nationally representative samples of participants according to the research questions of interest. (Peer et al., 2017). For instance, the median annual household income of participants in our study sample is £28,000 which closely corresponds to the UK median income[2].

The main motivation for our experiment is to understand how financial pressures may affect parents' attention towards their child's development and their responsiveness to typical policies that encourage them to do so. Our outcome measure(s) of interest are parents' purchase decisions across different types of goods, including child development (investment) goods.

Accordingly, our online experiment adopts a 2×2 design that has two key features: (1) Equal shares of participants randomly exposed to easy versus hard hypothetical scenarios, where the latter could trigger greater FW and (2) A 50% discount on the price of child investment goods, again randomly offered to half the participants. The discount represents a common type of financial subsidy that poor parents may be offered to encourage greater engagement with their child.

Our experimental design thus results in parents being randomly assigned to one of four groups, as described below:

1. **Easy Scenarios (ES)**: exposed to easy financial scenarios and baseline prices

2. **Hard Scenarios (HS)**: exposed to hard financial scenarios that could trigger FW and

[2]The mean income in our sample, also at £28,000, is somewhat lower than the UK mean for 2018 (£33,800), since we chose to over-sample poorer people for the purposes of our study.

baseline prices .

3. **Discount:** exposed to easy financial scenarios and 50% discount on child investment goods
4. **HS Discount:** exposed to hard financial scenarios and 50% discount on child investment goods.

Next, we describe the details of financial scenarios used to trigger FW and the financial subsidy (discount).

Financial Scenarios: Parents are asked how they will cope with each of 3 hypothetical scenarios involving income shocks, two of which are adapted from Mani et al. (2013) and a third shock that is particularly relevant for parents. The first scenario describes a drop in income due to higher prices of basic necessities; the second one describes a one-time income shock of large magnitude; the final scenario describes an increase in the cost of childcare in the UK. What varies between the groups that are exposed to the HS ('HS' and 'HS Discount') versus not ('ES' and 'Discount') is the severity of the price/cost increases and of the income shock[3].

Parents respond both with free-form answers on how they would cope with these scenarios. After completing their qualitative responses, they also respond to two questions about their how worried they are about (i) their current financial condition and (ii) being able to find money, in case of need, where the answers options are coded using Likert scales.

Parental Investment Task: Upon completing their responses to the 3 financial scenarios, parents are asked to allocate an experimental budget of £30 across three types of goods: (i) educational goods for children, (ii) groceries and (iii) luxury goods. The educational goods for children represent our measure of parental investment; groceries are a proxy for pressing family needs while the luxury goods allows for the presence of temptation.

To better proxy parental investment, only goods that require the time commitment of the parent were chosen, such as picture, story and activity books and educational games. This was to prevent the possibility of parents purchasing goods that could be substitutes for time spent with the child. We chose to offer goods with which parents are very familiar in order to avoid the risk the goods not being chosen because parents lack the experience to form beliefs on the expected returns from investing in such products. The price of the goods range from £3.5-£5.5 for most books and £4-£10 for the educational games.

Participants could choose from a wide selection of grocery goods, including basic food (cereals, meats, milk, cooking oils etc.) and cleaning and hygiene products from one of the

[3]The scenarios, the complete instructions and screenshots of the experimental market are available in Appendix 2.B

major low cost retailer in the UK. We chose brands which were most often purchased by the online customers of the retail store with prices ranging from less a £1 to £6.

The luxury goods include branded coffee, perfumes, glasses ad other products. The prices are higher, ranging from £5.5 to £27, to allow for costly falling into temptation. Due to ethical consideration we could not include the most typical goods associated with temptation such as tobacco and alcohol.

The total number of goods is 66 offering considerable variety. The order of the categories of goods and the order of goods within a category is randomized. We chose not to randomize all goods from all the categories to avoid high searching costs. Participants can observe the name, price and picture of the good and by clicking on the picture they can get all the information provided by a typical retailer.

The range of goods, the online retail store format and their offer at actual market prices all serve to make the experimental task very realistic. Further, participants were incentivized by having a one in 100 chance of receive the actual goods they choose. This probability number is as in the experimental design followed by Carvalho et al. (2016). Participants were also paid on a hourly wage basis for taking part in the survey.

Discount: Half of participants receive a 50% discount on all (child) investment goods. They are informed in the task instructions that some of the goods will have a 50% discount on the retail price. On screen, they see the old price crossed out and the new price written next to it [4].

This treatment aims to capture the response of parents to financial incentives on investment goods in times when FW are salient or not. The difference between the two groups will give us the effect of FW on the response to incentives to invest in human capital.

The difference in outcomes between the first two treatments (ES versus HS) aim to capture the effect of FW on parents' investment in their child's development, relative to other pressing household needs and items for their personal enjoyment. The difference between the groups (1) and (3) (ES versus Discount) captures the effect of offering the 50% discount on investment; it tests participant's responsiveness to price discounts (or price elasticity of demand). Finally, comparing the last two groups ((3) and (4)) (Discount versus HS Discount) captures how parents respond to financial incentives (i.e price subsidies) that encourage them to invest in their child's

[4]While this detail may suggest to participants the socially desirable answer, we wanted to rule out price salience as a potential mechanism: treatment effect being driven by HS participants failing to notice the price decreases, in addition to the shift in focus towards pressing needs effect which is the mechanism we are focusing on.

human capital development, when under the pressure of FW.

From a policy perspective, the treatment tries to identify if changing the trade-off between pressing needs and investment increases the salience of investing. If the poor are willing to invest in the child but fail to do so because their mental bandwidth is captured by pressing needs, the discount might generate an attention shift towards the opportunity of investing today at a lower cost. This is motivated by the fact that even small costs at the expense of pressing needs can impede the poor from adopting beneficial technologies (Kremer and Holla, 2009; Cohen and Dupas, 2010; Kremer and Glennerster, 2011) and by the results in Shah et al. (2015) who find that the poor can be better at trade-off thinking. On the other hand, if their mental bandwidth is fully captured by pressing needs, the discount could generate a strong income effect and weak substitution effect inducing participants to demand more pressing needs goods even if their relative price is higher.

Concerns and Limitations: The measure of parental investment is subject to several concerns and limitations. While the large range of choices should reduce this concern, there is the possibility of arbitrage. For instance, if parents have a a stronger preference for other investment goods than the ones available in the experiment, they could choose to purchase more of the other goods in the experiment and re-optimize their budget outside the experiment to purchase the desired investment goods. Secondly, we cannot control for the possibility of goods being resold. Participants could choose to select goods with higher reselling price, such as the luxury goods or the educational goods just because they would be easier to resell outside the experiment. However, if this would be the case, we would expect a much higher demand in the experiment for these types goods which is not the case. Finally, while we only offered investment goods requiring the time commitment of the parent, this does not necessarily imply that the goods will actually be used by the parents. In this sense, our proxy would capture more the intention of parents to invest than actual time investment. We try to provide evidence on the validity of the proxy in section 3.5 by measuring how it correlates with self-reported parenting practices and beliefs. If arbitrage, reselling or intention without action are severe concerns, we would expect no correlation with these measures, unless there is a strong social desirability driving the behavior of the participants. We hope that the online context of the experiment, the incentives (despite being weak) and the full anonymity limits this concern.

1.3.2 Data Collection and Sample Selection

Given our particular interest in child investment decisions under poverty within the UK, our eligibility criteria for participants were that they should (i) be UK residents (ii) with a child under 4 years of age (iii) and an annual household income below £50,000. We picked this age

group for the child, because it is widely emphasized as the key period in child development and also because children are less likely to be enrolled in formal education, thus spending more time with the parents. In addition, it ensured that the goods we selected require the time investment of the parent and cannot be used as a means to keep the child busy while the parent attends to other things. The choice of the upper income threshold was to over-sample lower income households which are the target group of this study. Given our criteria and the availability of participants who fit this profile in Prolific's database, we obtained a study sample of 349 participants which allows to detect an effect size above £3.6, at 5% significance level with 80% power. The minimum detectable effect increases to £5.5 when the sample is split in two groups according to income. The experiment was pre-registered on the *American Economic Association's registry for randomized controlled trials* with the id number AEARCTR-0003026.

All the registered participants were required to fill in an extended baseline survey, which gives access to a rich set of demographics. Information on income, family size or labor market outcomes (that could have changed since participants registered on Prolific) were also gathered at the end of the experiment. Additional information on financial strain and access to credit were also collected at the end of the experiment. Parents also fill a section on parenting practices and beliefs. For a full description of the scales used see the subsection 1.B.2 in Appendix 2.B.

1.3.3 Descriptive Statistics, Balance Checks and Selective Attrition

Data on key participant characteristics and balance checks for these across the different treatment arms are presented in Table 1.1. As mentioned earlier, the mean and median yearly household income in our sample (at around £28,000) is close the UK median income; since we did not sample high income participants, the sample income mean falls bellow the UK average of £33,800 in the year 2018. 63% of participants report living from paycheck to paycheck while 13% of household have yearly incomes below £13,000. The average age of people in our sample is 31 years, roughly 50% completed higher education and 59% are employed either full or part time. The average number of household members and children is 3.8 and 1.9 respectively. The average age of the youngest child is 22.7 months, 53% of them girls. In 34% of the households, there is more than one child in the targeted age group.

The last column of Table 1.1 displays the p-value associated with the F-test for the joint equality of averages across the treatment arms. Out of the 26 variables considered, 4 difference are statistically significant at 10% or 5% levels : the percentage of fathers (in ES and HS groups), the percentage of students (in the ES group relative to the other groups), the percentage of spouses reported to have received payments in the past month (in the HS and Discount groups) and the fraction of those likely to report living from paycheck to paycheck (in the

Table 1.1: Descriptive statistics and Balance Checks

	ES	HS	Discount	HS Discount	p-value
	(1)	(2)	(3)	(4)	(5)
Child gender (Male)	0.43	0.53	0.44	0.50	0.56
Age in months	21.31	23.00	22.84	23.80	0.43
No formal childcare	0.46	0.41	0.45	0.44	0.92
Any sibling bellow 4	0.35	0.33	0.36	0.34	0.97
Age of parent	30.92	31.99	31.74	31.39	0.54
Gender of parent (male)	0.09	0.14	0.24	0.23	0.02
Completed higher education	0.51	0.45	0.47	0.55	0.60
Student	0.12	0.06	0.03	0.05	0.08
Nationality UK	0.89	0.95	0.90	0.92	0.47
Country of birth UK	0.88	0.93	0.90	0.88	0.75
Language English	0.93	0.95	0.94	0.93	0.93
Household size	3.98	3.65	3.97	3.80	0.20
Number of children	2.02	1.79	2.02	1.88	0.43
Spouse or cohabiting partner	0.89	0.88	0.88	0.92	0.81
Yearly income per adult equivalent	14.27	14.73	15.06	15.48	0.65
Yearly household income	27.85	27.78	29.17	29.72	0.66
Poverty Index	-0.03	0.07	0.04	-0.05	0.56
Perceived SES (1-10 ladder)	4.81	4.76	4.76	4.92	0.91
Parent is employed	0.53	0.56	0.64	0.64	0.30
Spouse is employed	0.72	0.75	0.76	0.76	0.94
Any payment received in past month	0.61	0.71	0.67	0.58	0.27
Any payments received in past month (spouse)	0.59	0.77	0.70	0.54	0.02
Days since last payment	12.50	11.48	13.47	13.33	0.66
Lives from paycheck to paycheck	0.58	0.63	0.56	0.76	0.03
Has a credit card	0.68	0.74	0.64	0.69	0.61
Easy to get credit	0.55	0.40	0.57	0.51	0.13
Affordable credit rates	0.57	0.57	0.55	0.55	0.98
Observations	97	80	86	86	

Note: Columns (1) - (4) show the means across treatment arms. Column (5) displays the p-value associated with the F test of join orthogonality across treatment arms. Yearly household income is computed by dividing total yearly household income by the square root of household size. Poverty index is computed by averaging and standardizing the incidence of several income shocks in the previous year.

HS Discount group). One concern is that if the treatment affected the perception of financial scarcity of participants then it is plausible this induced them to feel more than they are living from paycheck to paycheck. The hypothesis is plausible since the groups are balanced in terms of income, subjective SES and incidence of poverty. If this is true, then lack of balance in this variable might actually hide a treatment effect. Adding it as an endogenous control would be troublesome. On the other hand, if this hypothesis is false, not including it could also bias the treatment effects estimates. We check this by running a robustness checks for the main results where we include it as control together with all the other unbalanced variables. Results are reported in Table A-14 in Appendix 2.A. We do not find meaningful differences between the estimates.

Given that the main results of this paper are based on analysis by income subgroups, in Tables A-2 and A-3 in Appendix 2.A we report balance checks separately for the lower and the

higher income groups. While power is lower, we find that the unbalanced variables are mostly driven by differences among treatment arms among the higher income group. For the lower income group, differences are smaller and none statistically significant. Nonetheless, throughout the analysis a wide range of covariates are included.

Selective attrition can be of particular concern in online experiments (Zhou and Fishbach, 2016; Horton et al., 2011). Participants face lower social and time costs of dropping out from the experiment than in lab experiments. This can become a concern if attrition becomes differential across treatment arms. This is plausible in our experiment – for instance, the hard financial scenarios could have induced negative feelings while the discount could have added an extra incentive for participants to complete the study.

Table A-1 reports regressions for attrition by treatment status. 39 participants did not complete the study, corresponding to 11% attrition rate, which is remarkably low for an online experiment (Zhou and Fishbach, 2016). Results in column (1) show that the decision to leave the survey is not differential across treatment arms. Column (2) controls for the fact that some participants could not advance past a page where the code was not compatible with non-updated browsers or with browsers on which the survey link was not tested[5]. The difference become even smaller and have higher associated p-values.

1.4 Results

This section begins with manipulation checks in subsection 1.4.1 and then proceeds to the validation of the investment task in subsection 1.4.2. Subsection 1.4.3 presents the main results. Next, subsection 1.4.4 presents heterogeneous treatment effect by the number of days since the household received its last paycheck.

1.4.1 Manipulation Check

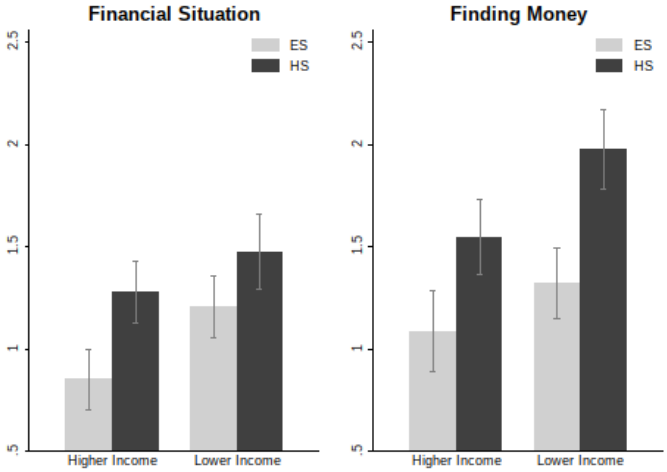
Two of our four treatments arms (HS and HS Discount) include psychological manipulations asking participants to respond to difficult financial scenarios. Our aim with these was to trigger worrying thoughts that occur often in the every day lives of the poor, to then examine their impact on child investment and other decisions. Participants free-form responses to how they would cope with each of the 3 scenarios provides qualitative evidence that the situations depicted were viewed as very hard to overcome and required difficult sacrifices.

While the qualitative evidence is suggestive, we also tried to quantify the effect of the

[5]Participants were informed that the survey application was tested only on the major browsers and that they should not participate if using other browsers. However, the platform does not allow us to screen out participants based on browser or operating system used.

treatment. After completing the scenarios and before being presented the investment task[6], participants were asked how worried they feel about (i) their financial situation and (ii) not being able to find money in case of need. The answers are coded with values between 0 and 3, with 0 representing 'not worried at all' to 3 representing 'desperately worried'. The results are presented in Figure 1.1 which plots the means with 95% confidence intervals for the treated and control group, separately for the lower income and higher income groups.

Figure 1.1: Manipulation checks: impact of increasing FW on worries about financial situations and finding money in case of need



Notes: The outcome variables in the figures are worries about the financial situations (left figure): "How worried do you feel about your financial situation?" and worries about finding money in case of need (right figure): "How worried do you feel about not being able to find money in case you really need it?". Both variables are coded as: 0 "not worried as all", 1 "somewhat worried", 2 "very worried" and 3 "desperately worried". The bars indicate the means while the brackets indicate 95% confidence intervals. ES/HS indicates being assigned to easy/hard financial scenarios.

As seen in Figure 1.1 responses to both questions show a clear increase in worries among the participants receiving the hard financial scenarios – among lower income participants, but also among higher income ones. The differences in worries responses between the HS and ES treatment arms is significant at the 1% level for both questions, across both income groups[7]. In fact, being exposed to adverse financial scenarios induces a bigger rise in the worry level about their current financial situation among higher income participants than lower income ones. This

[6]Since the manipulation check was performed before the task was introduced, we cannot establish whether being offered the discount had any (additional) effect on financial worries.

[7]Table A-7 in Appendix provides regression estimates for the impact of hard financial scenarios on worries across both income groups, controlling for child and household characteristics.

is not so surprising; after all, lower income participants are already starting out with greater concerns even in the absence of the hard scenarios. In fact, note that the worry levels of lower-income participants *not* exposed to the hard financial scenarios is already as high as those of the higher-income participants who are exposed to them.

Given that the manipulation check was performed before presenting the investment task, we are unable to test if the discount had any additional effect on financial worries. It may be that the discount triggered a response, reducing financial worries given the increased purchasing power, or increasing financial worries if it somehow reminds low-income people of their need to hunt for discounts when doing the groceries. We do not find any difference in the manipulation check between the HS and HS Discount groups before the investment task, but we acknowledge that the additional treatment may have created such a difference.

1.4.2 Correlation with parenting practices and beliefs

To asses to what extent investment in the task is predicted by self-reported parenting practices and beliefs, we estimate the following model:

$$Y_i = \beta_0 + \beta_1 P_i + X_i'\gamma + \epsilon_i \tag{1.1}$$

where Y_i is the amount (in pounds) allocated by parent i on a child investment goods, expressed at baseline prices, P_i is the measure of parenting practices or beliefs, X_i is a vector of child, parent and household covariates, goods order fixed effects and treatment status indicators.

Table 1.2 reports the $\widehat{\beta_1}$ estimates for the variables reported in the column titles. The variables in columns (5) and (8) are indexes of parenting practices and beliefs, computed following Kling et al. (2007) by first creating z indexes for each component - subtracting the mean of the control group and dividing by the standard deviation of the control group - and then averaging the indexes. The coefficients in all models point in the expected direction but not all are statistically significant.

Reading frequency and if the child was taken to a park or playground in the past week are associated with higher investment in the task, significant at 5 and 1%. How often the parent tried to teach the child new things and the time the child spends daily on TV, phone or tablet are not statistically significant but have the expected signs. Overall, the index of parenting practices is strongly correlated with demand for investment goods in the task.

Columns (6) trough (8) show the coefficients for the beliefs measures and the beliefs index. Parents who report that children develop at their own pace, independently of what parents do, purchase statistically less child investment goods. In contrast, parents who believe children who are read often in early childhood do better in school do not invest more. Note however, this

Table 1.2: Correlation between investment in the task and parenting practices/beliefs

	(1) Read	(2) Taught	(3) Tv	(4) Walk	(5) Index practices	(6) Own Pace	(7) Read Early	(8) Index beliefs
Coefficient	0.63**	0.35	-0.35	4.35***	2.69***	-1.25**	0.16	1.22
	(0.28)	(0.28)	(0.45)	(1.53)	(1.03)	(0.56)	(0.93)	(0.81)
Mean	5.07	4.37	1.94	0.90	-0.08	2.74	4.19	-0.10
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	349	349	349	349	349	349	349	349

Note: Estimates are obtained via OLS regressions. Each column presents estimates from a different model. What changes is the main explanatory variables which is listed in the column headers. The estimates in the row indicate the coefficient on the variables listed in the column headers. The variable in column (5) is an index of the z scores of the variables in columns (1)-(4). The variable in column (8) is an index of the z scores of the variables in columns (6)-(7). Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. The outcome variable in each model is expenditure on children goods in the task at baseline prices. All models include controls variables for individual and household characteristics, treatment status and order effects in the task.

second measure lacks variability, 85% of parents agree or strongly agree with the statement and only 2% disagree with it. This adds noise to the index of beliefs which is not statistically significant.

One potential limitation of the analysis is that it does not rule out the possibility that the observed correlation is driven by social desirability bias. Social desirability could lead to both higher demand for child investment goods and self-reporting of better parenting practices. The fact that the investment task is incentivized reduces this risk, but only to an extent since only 1 out of 100 parents actually receive the products they selected. Another factor that should reduce this risk is that the survey is online and fully anonymized.

Altogether, the results indicate that behavior in the task is strongly associated with self-reported parenting practices suggesting that the measured used promises to be a valid proxy for parental investment. While further tests are necessary (e.g. fully incentivizing the task), we consider this to be a valuable methodological contribution allowing further research on parental decision making in a lab setting. The task could easily be adapted to make it more suitable in a field setting as well.

1.4.3 Main Results

To identify the causal impact of the the HS and the Discount treatments on the demand for the three types of goods (investments, groceries and temptation), we estimate the following model:

$$Y_i = \beta_0 + \beta_1 HS_i + \beta_2 Discount_i + \beta_3 HSDiscount_i + \theta + X_i'\gamma + \epsilon_i \tag{1.2}$$

where Y_i is the amount (in pounds) expressed at baseline prices, allocated by parent i on a particular type of good. HS_i is 1 if exposed to the hard financial scenarios and had no discount, $Discount_i$ is 1 if offered the 50% discount on the investment goods and had easy scenarios while $HSDiscount_i$ is 1 if participants received hard scenarios and the discount. Note that $\widehat{\beta_3}$ indicates the difference in the outcomes between the HS Discount group and the ES group, therefore it should not be interpreted as an interaction term but as a separate treatment arm indicating the difference between the HS Discount group and the ES no-Discount group[8]. θ are fixed effects for the order in which the goods appeared on participants' screens during the task, while X_i is a vector of parent, child and household covariates: age and gender of child and parent, if parent has higher education, number of children, if there is another child in the same age group in the household, if there is a spouse or cohabiting partner, if the parents have regular jobs, their subjective socioeconomic status, a poverty indicator and income per square root of household size. ϵ_i is the error term. Throughout the analysis we use robust standard errors.

Table 1.3 shows OLS results separately for the two income groups – lower-income group in columns (1) to (3) and for the higher-income group in columns (4) to (6). The dependent variable in each of the columns is the expenditure on one of the three types of goods in the experimental budget allocation task[9]. Some strikingly interesting patterns emerge.

[8]We preferred using this specification instead of including an interaction term because our research question focuses the difference between the effect of the discount when financial concerns are salient compared to when they are not. In the specification in Equation 1.2 this would be tested using the hypothesis $\beta_3 - \beta_2 = 0$. In a model with interaction term, the coefficient on the interaction term would test if the interaction equals the sum of the individual effects.

[9]Since the dependent variable is censored, for robustness we run the main analysis using a Tobit model and report the estimates in Table A-7 in Appendix 2.A. Results are qualitatively similar and stronger in magnitude

We begin by interpreting the estimates for the the lower-income group. There is no differences in investment between the HS group and the ES group with estimates economically and statistically insignificant. This could be partly explained by floor effects because nearly half (47%) of low-income parents did not choose any investment goods at all. Negative treatment effects cannot be observed for participant who in the absence of the treatment would have also invested zero.

The difference between the estimates for the Discount group and the HS Discount group provide suggestive evidence of negative effects of FW on investment. Receiving the discount preceded by the easy scenarios, increases demand for investment goods by £7 which is roughly the mean of the control group, implying a very elastic response to the price change. However, when the hard scenarios precede the discount, the increase in demand is substantially lower and statistically insignificant. The difference between the coefficients for the two groups receiving the discount is statistically different from 0 at 10% level.

Turning to the estimates for the other types of goods, no coefficient is significant in the HS and Discount treatment groups. However, the estimates for the HS Discount group describe how FW impacted the response to the price change: the demand for grocery goods increases while the demand for temptation decreases to zero. Parents appear to be using the discount as means to increase their consumption of basic needs goods since they can purchase the same amount of investment goods at a lower cost. While we can only speculate, the decrease in the demand for temptation goods might be explained by a mechanism in line with Shah et al. (2015). The increased FW and discounted goods choice frame might induce parents to become more aware of the trade-off against pressing needs implied by the choice. If the increased FW increases focus towards pressing needs, parents identify trough the discount the opportunity to address them and subsequently lower the demand for non-necessity goods such as the luxury goods.

Among the higher-income group, we find, as for the lower-income group, that the HS does not statistically lower investment, the coefficient being actually positive with a p-value of 0.24. The financial incentive increases investment in both groups receiving the discount and, in contrast to the lower-income group, the estimate for the HS Discount group is actually larger than in the Discount group. In the models presented in columns (5) and (6), no coefficient on the treatment group is statistically different from zero, meaning that there is no treatment effects on the demand for groceries or luxury goods.

Order effects are presented in panel B[10]. To ease the interpretation, we will begin by look-

[10]Balance checks by the order of the goods are presented in Tables A-4, A-5 and A-6. Overall, the groups are well balanced with few exceptions.

Table 1.3: Treatment effects on demand for child investment goods, groceries and luxury goods

	Lower Income			Higher Income		
	(1) Investment	(2) Groceries	(3) Temptation	(4) Investment	(5) Groceries	(6) Temptation
Panel A: Treatment status						
HS	-0.31	-0.18	0.80	2.59	-2.32	-0.75
	(1.81)	(1.94)	(1.36)	(2.23)	(2.32)	(1.06)
Discount	7.32***	-1.09	0.41	5.45*	-0.29	0.67
	(2.65)	(2.01)	(1.26)	(2.85)	(2.19)	(1.31)
HS Discount	1.94	4.01**	-1.54*	7.12***	-3.00	2.26
	(2.47)	(1.69)	(0.88)	(2.32)	(2.01)	(1.39)
Panel B: Order effects						
Groceries 1st	-8.85***	7.40***	-0.26	-5.63**	6.06***	-2.65*
	(2.32)	(1.91)	(1.06)	(2.56)	(2.14)	(1.36)
Temptation 1st	0.67	0.68	0.22	-4.20	0.85	2.19*
	(2.54)	(1.93)	(1.18)	(2.82)	(2.22)	(1.28)
Groceries 2nd	-4.98*	4.12*	0.80	-0.058	1.50	-1.58
	(2.62)	(2.18)	(1.38)	(2.74)	(2.35)	(1.47)
Temptation 2nd	0.98	2.07	-1.68	-1.65	1.44	0.39
	(2.18)	(1.81)	(1.07)	(2.47)	(2.04)	(1.34)
Control Mean	7.14	20.00	1.95	6.40	20.59	2.13
p-value $\beta_{Discount} = \beta_{HSDiscount}$	0.07	0.01	0.05	0.61	0.23	0.33
p-value $\beta^{LI}_{HS} = \beta^{HI}_{HS}$	0.31	0.48	0.37			
p-value $\beta^{LI}_{Discount} = \beta^{HI}_{Discount}$	0.63	0.79	0.89			
p-value $\beta^{LI}_{HSDiscount} = \beta^{HI}_{HSDiscount}$	0.13	0.01	0.02			
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.08	0.11	0.04	0.04	0.07	0.13
Observations	182	182	182	167	167	167

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. Outcome variables are expenditure on child investment goods, groceries and luxury goods, expressed in pounds at baseline prices. Order effects represent the randomized order in which the three types of goods appeared in the investment task. Higher/lower income group denote the upper/lower 50% of the sample income per adult equivalent distribution. All models control for individual and household characteristics. HS indicates participants exposed to hard scenarios. Discount indicates parents who received a 50% discount on child investment goods. The first p-value listed is associated to the t-test of equality of the coefficients on the Discount group and the HS Discount group. The following p-values listed bellow test the equality of the coefficients from the regression on the lower income group (LI) and the higher income group (HI) estimated from models where each treatment assignment variable and each covariate is interacted with the income group dummy.

ing at the estimates in column (1) for the lower-income group. If groceries appear first and investment goods last, demand for investment goods decreases by £8.85 compared to the reverse ordering. If groceries appear second and investment last, demand decreases by £4.98 as opposed to when the order is reversed. The relative order of temptation and investment goods does not affect the demand of lower-income parents. It appears that groceries capture the attention of lower-income parents to the detriment of human capital investments. Within the higher-income group, order effects appear more general and not only specific to grocery goods:

whether groceries or temptation goods appear first and investment last has a similar negative effect on demand for investment. The order of the second category of goods does not affect the choices of higher income participants.

Order effects have high explanatory power (especially for the lower income group) and including them affects the treatment effects estimates without changing the main results, as reported in Tables A-15 and A-16 in Appendix 2.A. Finally, given the magnitude of these effects, we ask if they vary by HS status. In Tables A-17 and A-18 we report order effects for HS (columns 4-6) and ES (columns 1-3) participants. We find suggestive evidence that the order effects discussed in the previous paragraph are stronger when financial concerns are salient, especially for the lower income group. This provides additional evidence that the HS shifted focus towards pressing needs.

In Appendix 2.A we include several robustness checks. In Tables A-12 and A-13, we report estimates of the main model interacting treatment with the lower-income indicator and with the continuous measure of income, respectively. Results are consistent even though precision is affected, especially when using the continuous income measure. We are not powered enough to detect such effects given the sample size[11]. In addition, income is likely noisy and we did not measure other factors which may correlate with financial worries, such as assets. For instance, two households with similar yearly income, may have different levels of assets to use as buffer in case of a shock. Reflecting on how they would cope with a hypothetical shocks will likely trigger very different response depending on their buffer stock.

1.4.4 Heterogeneous Effects by Days Since Payment

Roughly 70% of lower-income participant state that they live from paycheck to paycheck. If they fail to smooth consumption within a payment cycle, they will experience periods of lower financial resources before payday. The fluctuations in financial resources are expected to lead to fluctuations in FW associated with poverty. If this is the case, we expect the days since payment to interact with the HS condition.

The number of days since payment is going to be correlated with the payment frequency. Whether a person is paid monthly or weekly is usually also an indicator of the job type. Furthermore, if only one spouse is employed, the number of days since payment might be correlated with income. We address these issues by controlling for payment frequency, presence of spouse,

[11]The pre-registered sample size of the experiment was 800 participants. Unfortunately, even though more than 1500 parents with children in the targeted age group are registered on Prolific, in practice only a small share proved to be active, forcing us to stop the data collection. This impedes us from addressing some of the pre-registered hypotheses due to very low power.

spouse employment status and all the other controls included in the main specification. In spite of this, the number of days since payment may not be as good as random since participants self select themselves into participating in the experiment. For instance, if participants who fail to smooth consumption are more likely to take part in experiments in given periods of the payments cycle (e.g. when money runs low), than omitted variable bias is a concern. We check if the number of days since payment is correlated with the demographics and report the results in Table A-11 in Appendix 2.A. Besides payment frequency and employment status, we do not find any of the demographics to strongly predict days since last payment.

We first test if the number of days since payment is associated with higher FW, conditional on all the covariates. Column (1) in Table 1.4 reports models running the worries index on days since payment using the sample of lower income parents. We do not find any association between the days since payment and worries, nor do we find that the HS treatment induces higher worries for participants further away from the last payment.

Columns (2) to (4) in Table 1.4 report estimates from regressions of the main outcome variables on treatment status interacted with the number of days since payment, controlling for order effects and demographics. Note, that the sample size is lower since some parents reported not having received any payments in the past month. Firstly, we observe in column (2) that the coefficient on the Discount group is and similar in magnitude to the coefficient for the HS Discount group. This means that for participants who are very close to payment dates, increasing FW does not affect how they respond to the discount. As the number of days since payment increases, the effect of the discount, for the group exposed to hard scenarios, decreases by £0.64 for each additional day[12].

We observe a similar, though reversed, pattern when looking at the demand for groceries in column (3). When FW are salient, increasing the relative price of groceries by offering a discount for investment goods, has the expected negative effect on demand (albeit not statistically significant) but only if participants are close to their last payment date. The estimate decreases towards zero with each additional days and even changes sign if more than 8 days since payment have passed. We observe a similar pattern for the group exposed to easy scenarios, but the coefficient changes sign only with more than 17 days since payment. Altogether, it appears that the main results among lower income participants is driven by participants further away from the last payment date[13]. I note however that power in columns (3) and (4) is very low (less than

[12]The power of the test is roughly 70%, bellow the 80% threshold

[13]In Appendix 2.B we reports several table where we check if this effect appears to be linear in number of days or not by splitting the sample according the several thresholds in terms of number of days since last paid and running the main specification. We find evidence that the effect is linear in the number of days since last

Table 1.4: Heterogenous treatment effects by days since payment among the lower-income group

	(1) Worries Index	(2) Investment	(3) Groceries	(4) Temptation
Days since last payment	-0.0058	0.0015	-0.096	0.058
	(0.013)	(0.19)	(0.20)	(0.13)
HS	0.52**	-1.32	-1.32	3.10
	(0.23)	(2.42)	(2.79)	(2.39)
Discount		12.9***	-7.85**	3.22*
		(4.40)	(3.37)	(1.85)
HS Discount		11.7**	-3.46	-0.27
		(4.87)	(2.97)	(1.23)
HS × Days since last payment	-0.0017	0.23	-0.050	-0.16
	0.017)	(0.22)	(0.25)	(0.17)
Discount × Days since last payment		-0.28	0.47*	-0.28**
		(0.28)	(0.24)	(0.14)
HS Discount × Days since last payment		-0.64**	0.43**	-0.037
		(0.26)	(0.21)	(0.13)
Controls	Yes	Yes	Yes	Yes
Order Effects	Yes	Yes	Yes	Yes
Observations	130	130	130	130

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. Outcome variables are the worries index, and expenditure on child investment goods, groceries and luxury goods, expressed in pounds at baseline prices. Higher/lower income group denote the upper/lower 50% of the sample income per adult equivalent distribution. All models control for individual and household characteristics. HS indicates participants exposed to hard scenarios. Discount indicates parents who received a 50% discount on child investment goods.

40%) for both the main effects and the interactions, raising concern regarding the replicability of our findings.

1.5 Conclusion

We find that increasing FW associated with poverty changes how lower-income parents respond to an incentive to invest in human capital. Subsidizing child investment goods increases investment in human capital only if FW are not salient. However, when FW become top of mind, parents do not use the incentive to invest more in human capital but to address pressing needs, demanding more groceries even though their relative price is higher. This is consistent with the change in focus towards pressing needs suggested by Shah et al. (2012, 2015, 2018).

Further evidence suggests how poverty captures the attention of parents. If pressing needs

payment.

goods were displayed first or second in their choice set, lower-income parents invest substantially less in human capital.

The main effects are driven by parents who are further away from their last paycheck. This increases the external validity of the results since the number of days since payment is an indicator of real-world monetary scarcity if household fail to smooth consumption.

In terms of policy implication, our results indicate that low-income parents may not respond as desired to policies aimed at increasing investment in human capital if monetary concerns are salient. What remains unanswered is: (i) how to shift attention to human capital when monetary concerns are salient and (ii) what would be the welfare implications of achieving such outcome. Would a policy shifting attention from pressing needs towards investing in human capital perpetuate poverty, by parents not fully addressing urgent needs, or would it break the inter-generational transmission of poverty. A well designed policy may need to first make needs "less pressing" (safety nets to protect the poor from unexpected shocks) and only then to try to shift the behavior of the poor towards increasing human capital investments. It may be that solving the first issue is sufficient to change behavior - if the poor gain the mental space required to focus on their children emotional and cognitive development.

Appendix

1.A Appendix A - Additional Results and Robustness Checks

Table A-1: Attrition by treatment status

	(1) Completed	(2) Completed
HS	-0.012	0.026
	(0.046)	(0.033)
Discount	0.0038	-0.011
	(0.044)	(0.038)
HS Discount	0.054	0.020
	(0.038)	(0.035)
Constant	0.89***	0.93***
	(0.030)	(0.025)
Observations	387	387
Exclude page with technical error on some OS	No	Yes

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. The outcome variables is 1 if the participant completed the experiment. HS indicates participants exposed to hard scenarios. Discount indicates parents who received a 50% discount on child investment goods. The constant gives the share of participants completing the experiment. Exclude page with technical error on some OS - indicates a page in the experiment where some participants had technical issues due to incompatibility with their operating systems. The app was tested on major browsers and operating systems, this was communicated to all participants, but in practice we could not screen out participants using other browsers or operating systems.

Table A-2: Descriptive statistics and balance checks for the lower income group

	(1) ES	(2) HS	(3) Discount	(4) HS Discount	(5) p-value
Child gender (Male)	0.41	0.53	0.50	0.41	0.54
Age in months	22.22	24.00	22.70	22.16	0.83
No formal childcare	0.43	0.47	0.48	0.38	0.82
Any sibling bellow 4	0.36	0.35	0.34	0.35	1.00
Age of parent	31.41	31.37	31.59	30.31	0.75
Gender of parent (male)	0.09	0.14	0.18	0.22	0.32
Completed higher education	0.47	0.35	0.41	0.32	0.50
Student	0.16	0.07	0.07	0.08	0.39
Nationality UK	0.84	0.95	0.93	0.95	0.18
Country of birth UK	0.86	0.93	0.93	0.92	0.57
Language English	0.93	0.95	0.93	0.95	0.96
Household size	4.22	3.65	4.20	3.95	0.14
Number of children	2.31	1.88	2.25	2.03	0.30
Spouse or cohabiting partner	0.83	0.77	0.80	0.84	0.84
Yearly income	9.99	9.74	9.71	9.44	0.89
Yearly household income	20.33	18.30	19.62	18.64	0.48
Poverty index	0.07	0.29	0.30	0.15	0.28
Perceived SES (1-10 ladder)	4.66	4.42	4.36	4.41	0.82
Parent is employed	0.45	0.51	0.52	0.57	0.71
Spouse is employed	0.66	0.60	0.66	0.57	0.80
Any payment received in past month	0.55	0.74	0.66	0.59	0.23
Any payments received in past month (spouse)	0.52	0.73	0.69	0.58	0.22
Days since last payment	11.82	11.08	10.06	9.61	0.81
Lives from paycheck to paycheck	0.62	0.77	0.66	0.78	0.24
Has a credit card	0.59	0.63	0.59	0.57	0.96
Easy to get credit	0.47	0.30	0.43	0.35	0.35
Affordable credit rates	0.64	0.70	0.70	0.65	0.87

Note: Columns (1) - (4) show the means across treatment arms. Column (5) displays the p-value associated with the F test of join orthogonality across treatment arms. Yearly household income is computed by dividing total yearly household income by the square root of household size. Poverty index is computed by averaging the incidence of several income shocks in the previous year. The sample used in the analysis are participants with yearly incomes bellow the median.

Table A-3: Descriptive statistics and balance checks for the higher income group

	ES	Prime	Discount	HS Discount	p-value
	(1)	(2)	(3)	(4)	(5)
Child gender (Male)	0.46	0.51	0.38	0.57	0.33
Age in months	19.95	21.84	22.98	25.04	0.14
No formal childcare	0.51	0.35	0.43	0.49	0.49
Any sibling bellow 4	0.33	0.30	0.38	0.33	0.89
Age of parent	30.18	32.70	31.90	32.20	0.07
Gender of parent (male)	0.10	0.14	0.31	0.24	0.07
Completed higher education	0.56	0.57	0.52	0.71	0.26
Student	0.08	0.05	0.00	0.02	0.25
Nationality UK	0.95	0.95	0.86	0.90	0.43
Country of birth UK	0.90	0.92	0.86	0.86	0.78
Language English	0.92	0.95	0.95	0.92	0.90
Household size	3.62	3.65	3.71	3.69	0.95
Number of children	1.59	1.68	1.79	1.78	0.72
Spouse or cohabiting partner	0.97	1.00	0.98	0.98	0.83
Yearly income	20.65	20.53	20.65	20.04	0.89
Yearly household income	39.03	38.80	39.18	38.10	0.93
povertyIndexSD	-0.18	-0.19	-0.24	-0.19	0.91
Perceived SES (1-10 ladder)	5.05	5.16	5.17	5.31	0.86
Parent is employed	0.64	0.62	0.76	0.69	0.54
Spouse is employed	0.82	0.92	0.86	0.90	0.57
Any payment received in past month	0.69	0.68	0.69	0.57	0.57
Any payments received in past month (spouse)	0.68	0.81	0.71	0.52	0.04
Days since last payment	13.35	12.00	17.09	16.10	0.15
Lives from paycheck to paycheck	0.51	0.46	0.45	0.73	0.02
Has a credit card	0.82	0.86	0.69	0.78	0.27
Easy to get credit	0.67	0.51	0.71	0.63	0.30
Affordable credit rates	0.46	0.43	0.38	0.47	0.84

Note: Columns (1) - (4) show the means across treatment arms. Column (5) displays the p-value associated with the F test of join orthogonality across treatment arms. Yearly household income is computed by dividing total yearly household income by the square root of household size. Poverty index is computed by averaging the incidence of several income shocks in the previous year. The sample used in the analysis are participants with yearly incomes above the median.

Table A-4: Balance checks for the lower income group by the order of the goods in the task

	(1) GIT	(2) GTI	(3) IGT	(4) ITG	(5) TGI	(6) TIG	(7) p-value
Child gender (Male)	0.43	0.49	0.54	0.48	0.41	0.46	0.72
Age in months	23.08	23.24	24.99	19.48	22.36	22.91	0.09
No formal childcare	0.51	0.44	0.50	0.44	0.41	0.37	0.66
Any sibling bellow 4	0.33	0.42	0.31	0.42	0.25	0.33	0.37
Age of parent	31.75	30.93	31.13	31.76	31.41	32.01	0.87
Gender of parent (male)	0.25	0.07	0.15	0.21	0.17	0.20	0.19
Completed higher education	0.49	0.51	0.53	0.50	0.44	0.48	0.95
Student	0.04	0.07	0.07	0.02	0.15	0.06	0.07
Nationality UK	0.90	0.89	0.96	0.94	0.93	0.83	0.24
Country of birth UK	0.90	0.87	0.91	0.89	0.93	0.85	0.78
Language English	0.98	0.85	0.97	0.95	0.93	0.93	0.09
Household size	3.67	4.04	3.85	3.85	4.02	3.69	0.40
Number of children	1.76	2.04	1.93	1.97	2.07	1.81	0.66
Spouse or cohabiting partner	0.86	0.91	0.88	0.89	0.93	0.87	0.86
Yearly income	13.76	15.53	14.91	15.44	13.87	15.61	0.48
Yearly household income	25.83	30.56	28.43	29.93	27.10	29.68	0.31
Poverty Index	0.11	-0.10	0.04	-0.12	0.15	-0.05	0.08
Perceived SES (1-10 ladder)	4.51	4.92	4.85	5.10	4.64	4.80	0.46
Parent is employed	0.69	0.51	0.62	0.56	0.56	0.61	0.53
Spouse is employed	0.61	0.82	0.76	0.74	0.78	0.74	0.21
Any payment received in past month	0.71	0.56	0.65	0.63	0.68	0.63	0.74
Any payments received in past month (spouse)	0.66	0.54	0.63	0.67	0.69	0.68	0.64
Days since last payment	12.46	12.65	12.31	15.09	12.47	10.80	0.53
Lives from paycheck to paycheck	0.65	0.55	0.65	0.61	0.75	0.56	0.26
Has a credit card	0.67	0.67	0.68	0.71	0.61	0.78	0.55
Easy to get credit	0.43	0.47	0.53	0.61	0.47	0.52	0.47
Affordable credit rates	0.53	0.55	0.49	0.60	0.56	0.65	0.58

Note: Columns (1) - (6) show the means across treatment arms. The column titles acronyms indicate the order of the goods in the task: "I" Investment goods, "G" Grocery goods, "T" temptation goods. Column (7) displays the p-value associated with the F test of join orthogonality across treatment arms. Yearly household income is computed by dividing total yearly household income by the square root of household size. Poverty index is computed by averaging the incidence of several income shocks in the previous year.

Table A-5: Balance checks for the lower income group by the order of the goods in the task

	(1) GIT	(2) GTI	(3) IGT	(4) ITG	(5) TGI	(6) TIG	(7) p-value
Child gender (Male)	0.45	0.45	0.49	0.50	0.34	0.56	0.70
Age in months	22.90	23.52	25.44	19.82	20.19	24.04	0.20
No formal childcare	0.45	0.41	0.41	0.50	0.47	0.40	0.97
Any sibling bellow 4	0.38	0.45	0.38	0.39	0.22	0.28	0.46
Age of parent	31.69	30.38	30.85	32.18	31.28	31.10	0.88
Gender of parent (male)	0.24	0.07	0.13	0.07	0.22	0.16	0.30
Completed higher education	0.38	0.41	0.41	0.46	0.38	0.32	0.94
Student	0.07	0.07	0.10	0.04	0.22	0.08	0.22
Nationality UK	0.90	0.93	0.92	0.93	0.94	0.84	0.82
Country of birth UK	0.90	0.93	0.90	0.89	0.94	0.88	0.97
Language English	0.97	0.93	0.95	0.93	0.94	0.92	0.99
Household size	3.83	4.34	4.13	3.96	4.16	3.64	0.43
Number of children	1.90	2.34	2.26	2.18	2.25	1.80	0.49
Spouse or cohabiting partner	0.79	0.86	0.79	0.75	0.88	0.76	0.78
Yearly income	9.29	10.54	9.30	9.64	9.83	10.11	0.65
Yearly household income	17.91	21.91	18.64	19.24	19.44	19.06	0.37
Poverty Index	0.18	0.06	0.23	0.05	0.44	0.14	0.26
Perceived SES (1-10 ladder)	4.00	4.76	4.51	4.96	4.09	4.60	0.23
Parent is employed	0.52	0.48	0.56	0.39	0.47	0.60	0.69
Spouse is employed	0.48	0.72	0.64	0.64	0.66	0.60	0.56
Any payment received in past month	0.76	0.59	0.64	0.57	0.59	0.64	0.72
Any payments received in past month (spouse)	0.78	0.48	0.55	0.67	0.64	0.63	0.35
Days since last payment	9.04	8.42	10.96	14.57	11.71	9.84	0.39
Lives from paycheck to paycheck	0.79	0.45	0.72	0.71	0.81	0.68	0.03
Has a credit card	0.55	0.62	0.51	0.64	0.53	0.76	0.42
Easy to get credit	0.34	0.41	0.41	0.50	0.28	0.44	0.61
Affordable credit rates	0.59	0.69	0.59	0.79	0.66	0.76	0.45

Note: Columns (1) - (6) show the means across treatment arms. The column titles acronyms indicate the order of the goods in the task: "I" Investment goods, "G" Grocery goods, "T" temptation goods. Column (7) displays the p-value associated with the F test of join orthogonality across treatment arms. Yearly household income is computed by dividing total yearly household income by the square root of household size. Poverty index is computed by averaging the incidence of several income shocks in the previous year. The sample used in the analysis are participants with yearly incomes bellow the median.

Table A-6: Balance checks for the higher income group by the order of the goods in the task

	(1) GIT	(2) GTI	(3) IGT	(4) ITG	(5) TGI	(6) TIG	(7) p-value
Child gender (Male)	0.41	0.54	0.62	0.47	0.48	0.38	0.52
Age in months	23.32	22.92	24.38	19.21	24.93	21.93	0.30
No formal childcare	0.59	0.46	0.62	0.38	0.33	0.34	0.13
Any sibling bellow 4	0.27	0.38	0.21	0.44	0.30	0.38	0.43
Age of parent	31.82	31.54	31.52	31.41	31.56	32.79	0.86
Gender of parent (male)	0.27	0.08	0.17	0.32	0.11	0.24	0.15
Completed higher education	0.64	0.62	0.69	0.53	0.52	0.62	0.77
Student	0.00	0.08	0.03	0.00	0.07	0.03	0.49
Nationality UK	0.91	0.85	1.00	0.94	0.93	0.83	0.22
Country of birth UK	0.91	0.81	0.93	0.88	0.93	0.83	0.64
Language English	1.00	0.77	1.00	0.97	0.93	0.93	0.01
Household size	3.45	3.69	3.48	3.76	3.85	3.72	0.45
Number of children	1.59	1.69	1.48	1.79	1.85	1.83	0.57
Spouse or cohabiting partner	0.95	0.96	1.00	1.00	1.00	0.97	0.61
Yearly income	19.65	21.10	22.45	20.23	18.66	20.36	0.03
Yearly household income	36.26	40.21	41.59	38.74	36.19	38.84	0.12
Poverty Index	0.01	-0.27	-0.22	-0.27	-0.19	-0.21	0.14
Perceived SES (1-10 ladder)	5.18	5.11	5.31	5.21	5.30	4.97	0.95
Parent is employed	0.91	0.54	0.69	0.71	0.67	0.62	0.14
Spouse is employed	0.77	0.92	0.93	0.82	0.93	0.86	0.43
Any payment received in past month	0.64	0.54	0.66	0.68	0.78	0.62	0.62
Any payments received in past month (spouse)	0.52	0.60	0.72	0.68	0.74	0.71	0.58
Days since last payment	18.57	17.11	13.91	15.54	13.26	11.64	0.28
Lives from paycheck to paycheck	0.45	0.65	0.55	0.53	0.67	0.45	0.46
Has a credit card	0.82	0.73	0.90	0.76	0.70	0.79	0.58
Easy to get credit	0.55	0.54	0.69	0.71	0.70	0.59	0.59
Affordable credit rates	0.45	0.38	0.34	0.44	0.44	0.55	0.73

Note: Columns (1) - (6) show the means across treatment arms. The column titles acronyms indicate the order of the goods in the task: "I" Investment goods, "G" Grocery goods, "T" temptation goods. Column (7) displays the p-value associated with the F test of join orthogonality across treatment arms. Yearly household income is computed by dividing total yearly household income by the square root of household size. Poverty index is computed by averaging the incidence of several income shocks in the previous year. The sample used in the analysis are participants with yearly incomes above the median.

Table A-7: Manipulation check - treatment effects on financial worries

	(1) Financial situation	(2) Finding money	(3) Index
Lower Income	0.318***	0.185	0.315**
	(0.105)	(0.134)	(0.130)
HS	0.485***	0.517***	0.611***
	(0.105)	(0.135)	(0.129)
Lower Income × HS	-0.243	0.0960	-0.108
	(0.156)	(0.190)	(0.188)
Constant	1.403***	2.029***	0.686*
	(0.304)	(0.374)	(0.365)
Controls	Yes	Yes	Yes
Adj. R^2	0.126	0.161	0.168
Observations	349	349	349

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. The dependent variables are worries about the financial situations: *How worried do you feel about your financial situation?* and worries about finding money in case of need: How worried do you feel about not being able to find money in case you really need it?. Both variables are coded as: 0 not worried as all, 1 somewhat worried, 2 very worried and 3 desperately worried. The index variable in the last column is computed the average of the z scores of the previous two dependent variables. The z scores are computed by subtracting the mean of the control group and then dividing by the standard deviation of the control group. Higher/lower income group denote the upper/lower 50% of the sample income per adult equivalent distribution. All models control for individual and household characteristics. HS indicates participants exposed to hard scenarios. Discount indicates parents who received a 50% discount on child investment goods.

Table A-8: Hypothetical scenarios answers statistics by treatment and income group

	Easy scenarios			Hard scenarios		
	(1) Higher	(2) Lower	(3) Diff	(4) Higher	(5) Lower	(6) Diff
S1:Able to raise money (0-1)	0.90	0.73	0.18***	0.59	0.33	0.27***
S1:Cause financial hardship	1.89	2.39	-0.50***	2.95	3.58	-0.62***
S1:Require sacrifices	2.04	2.44	-0.40***	3.10	3.49	-0.38***
S2:Not afford childcare	0.37	0.53	-0.16**	0.93	0.95	-0.02
S2:Budget cuts	2.17	2.41	-0.24*	3.50	3.61	-0.11
S2:Give up childcare	2.00	2.25	-0.25*	3.38	3.51	-0.13
S3: Maintain same lifestyle	3.22	2.77	0.45***	1.66	1.60	0.06
S3:Affect leisure	2.00	2.42	-0.42***	3.65	3.67	-0.02
Observations	81	102	183	86	80	166

Note: Estimates are obtained via OLS regressions. Means in column (1), (2), (4) (5), differences in means in columns (3) and (6). *, **, *** denote significance at the 10%, 5% and 1% levels respectively. Outcome variables are listed in rows. The variables in the 1st and 4th rows are measured on a "Yes" "No" scale, ther others on a 4 item Likert (0 Strongly dissagree - 3 Strongly Agree). Higher/lower denote the upper/lower 50% of the sample income per adult equivalent distribution.

Table A-9: Tobit regressions of treatment effects on demand for investment goods, groceries and luxury goods

	Lower Income			Higher Income		
	(1) Investment	(2) Groceries	(3) Temptation	(4) Investment	(5) Groceries	(6) Temptation
Panel A: Treatment status						
HS	3.21	0.69	3.62	2.59	-2.32	-0.75
	(3.79)	(2.37)	(7.93)	(2.23)	(2.32)	(1.06)
Discount	12.8***	-1.29	1.83	5.45*	-0.29	0.67
	(3.86)	(2.36)	(8.21)	(2.85)	(2.19)	(1.31)
HS Discount	3.03	4.92**	-20.0	7.12***	-3.00	2.26
	(3.96)	(2.45)	(13.2)	(2.32)	(2.01)	(1.39)
Panel B: Order effects						
Groceries 1st	-18.3***	8.73***	-3.45	-5.63**	6.06***	-2.65*
	(4.15)	(2.53)	(11.2)	(2.56)	(2.14)	(1.36)
Temptation 1st	-3.24	0.61	7.92	-4.20	0.85	2.19*
	(3.71)	(2.37)	(8.24)	(2.82)	(2.22)	(1.28)
Groceries 2nd	-9.59**	4.78*	9.76	-0.058	1.50	-1.58
	(4.00)	(2.53)	(8.88)	(2.74)	(2.35)	(1.47)
Temptation 2nd	0.33	2.05	-11.8	-1.65	1.44	0.39
	(4.02)	(2.50)	(11.0)	(2.47)	(2.04)	(1.34)
Control Mean	7.14	20.00	1.95	6.40	20.59	2.13
p-value $\beta_{Discount} = \beta_{HSDiscount}$	0.02	0.02	0.10	0.61	0.23	0.33
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2				0.04	0.07	0.13
Observations	182	182	182	167	167	167

Note: Estimates are obtained via Tobit regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. Outcome variables are expenditure on child investment goods, groceries and luxury goods, expressed in pounds at baseline prices. Order effects represent the randomized order in which the three types of goods appeared in the investment task. Higher/lower income group denote the upper/lower 50% of the sample income per adult equivalent distribution. All models control for individual and household characteristics. HS indicates participants exposed to hard scenarios. Discount indicates parents who received a 50% discount on child investment goods.

Table A-10: Heterogenous treatment effects by days since last payment for the full sample

	(1) Investment	(2) Investment	(3) Groceries	(4) Groceries	(5) Temptation	(6) Temptation
HS	-0.79	1.27	-0.21	-2.01	0.82	0.79
	(2.16)	(2.08)	(2.44)	(2.37)	(1.81)	(1.83)
Discount	9.16***	11.0***	-2.85	-3.84	0.73	0.18
	(3.24)	(3.27)	(2.71)	(2.56)	(1.65)	(1.57)
HS Discount	9.17**	11.2***	-1.30	-3.03	-0.57	-0.54
	(3.57)	(3.56)	(2.49)	(2.45)	(1.44)	(1.45)
Days since last payment	0.018	0.0012	-0.026	0.0040	-0.0065	-0.010
	(0.11)	(0.12)	(0.12)	(0.13)	(0.064)	(0.072)
HS × Days since last payment	0.11	0.026	-0.025	0.049	-0.056	-0.057
	(0.17)	(0.16)	(0.18)	(0.18)	(0.11)	(0.11)
Discount × Days since last payment	-0.11	-0.20	0.099	0.15	0.010	0.031
	(0.20)	(0.19)	(0.17)	(0.16)	(0.11)	(0.11)
HS Discount × Days since last payment	-0.22	-0.25	0.10	0.14	0.033	0.039
	(0.22)	(0.22)	(0.16)	(0.16)	(0.082)	(0.085)
Monthly Payment		2.91		-2.98		0.027
		(2.09)		(1.94)		(1.19)
Lives from paycheck to paycheck		-5.27***		4.29***		-0.54
		(1.77)		(1.49)		(0.86)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.10	0.14	0.06	0.10	0.03	0.02
Observations	257	253	257	253	257	253

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. Outcome variables are the worries index, and expenditure on child investment goods, groceries and luxury goods, expressed in pounds at baseline prices. Higher/lower income group denote the upper/lower 50% of the sample income per adult equivalent distribution. All models control for individual and household characteristics. HS indicates participants exposed to hard scenarios. Discount indicates parents who received a 50% discount on child investment goods.

Table A-11: Correlation between days since last payment and demographics controls among the lower-income group

	(1) Days since last payment
Monthly payments	14.9***
	(1.29)
Age of parent	-0.029
	(0.12)
Gender of parent (male)	-0.16
	(2.08)
Completed higher education	-0.41
	(1.30)
Age in months	0.021
	(0.073)
Child gender (Male)	-1.70
	(1.24)
Number of children	0.85
	(0.73)
Any sibling bellow 4	1.71
	(1.32)
Spouse or cohabiting partner	1.91
	(2.08)
Parent is employed	-2.16*
	(1.27)
Spouse is employed	-0.0024
	(1.94)
Perceived SES (1-10 ladder)	0.13
	(0.37)
Poverty index	-0.54
	(0.85)
Yearly income	-0.14
	(0.16)
Constant	2.67
	(3.86)
Observations	130

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. Higher/lower income group denote the upper/lower 50% of the sample income per adult equivalent distribution. Yearly household income is computed by dividing total yearly household income by the square root of household size. Poverty index is computed by averaging and standardizing the incidence of several income shocks in the previous year.

Table A-12: Treatment effects on demand for investment goods, groceries and luxury goods interacted with lower income indicator

	(1) Investment	(2) Investment	(3) Groceries	(4) Groceries	(5) Temptation	(6) Temptation
HS	3.11	2.78	-2.41	-1.99	-0.68	-1.00
	(1.98)	(2.05)	(2.28)	(2.24)	(1.07)	(1.04)
Discount	5.04*	5.30*	-0.80	-0.31	1.45	0.96
	(2.76)	(2.81)	(2.25)	(2.21)	(1.44)	(1.38)
HS Discount	6.24***	6.90***	-2.35	-2.57	2.40*	2.06
	(2.31)	(2.21)	(2.13)	(1.99)	(1.42)	(1.42)
Lower Income	0.74	1.34	-0.59	-1.01	-0.19	-0.10
	(1.78)	(1.78)	(2.07)	(2.05)	(1.13)	(1.25)
HS × Lower Income	-4.67*	-3.90	3.14	2.47	1.55	1.69
	(2.65)	(2.74)	(3.08)	(3.02)	(1.72)	(1.69)
Discount × Lower Income	-0.60	0.56	1.94	0.18	-1.17	-0.53
	(3.80)	(3.77)	(3.00)	(3.03)	(1.86)	(1.92)
HS Discount × Lower Income	-5.49	-5.40	7.24***	6.91***	-4.16**	-3.67**
	(3.43)	(3.28)	(2.76)	(2.60)	(1.62)	(1.67)
Controls	No	Yes	No	Yes	No	Yes
Adj. R^2	0.02	0.07	0.02	0.08	0.02	0.03
Observations	349	349	349	349	349	349

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. Outcome variables are expenditure on child investment goods, groceries and luxury goods, expressed in pounds at baseline prices. Order effects represent the randomized order in which the three types of goods appeared in the investment task. Higher/lower income group denote the upper/lower 50% of the sample income per adult equivalent distribution. All models control for individual and household characteristics. HS indicates participants exposed to hard scenarios. Discount indicates parents who received a 50% discount on child investment goods.

Table A-13: Treatment effects on demand for investment goods, groceries and luxury goods interacted with income

	(1) Investment	(2) Investment	(3) Groceries	(4) Groceries	(5) Temptation	(6) Temptation
HS	-1.38	-0.91	0.83	0.60	0.51	0.32
	(3.37)	(3.34)	(3.89)	(3.66)	(2.44)	(2.40)
Discount	3.01	5.87	2.11	-0.36	-0.28	0.11
	(4.05)	(4.20)	(3.63)	(3.73)	(2.56)	(2.65)
HS Discount	-3.45	-1.58	8.80**	7.28**	-3.80*	-3.20
	(4.57)	(4.49)	(3.50)	(3.42)	(2.09)	(2.19)
Yearly income	0.016	0.00014	0.049	0.079	-0.060	-0.072
	(0.13)	(0.14)	(0.17)	(0.17)	(0.11)	(0.13)
HS × Yearly income	0.13	0.11	-0.10	-0.083	-0.021	-0.029
	(0.22)	(0.22)	(0.25)	(0.23)	(0.14)	(0.13)
Discount × Yearly income	0.11	-0.024	-0.13	0.013	0.080	0.040
	(0.24)	(0.25)	(0.23)	(0.23)	(0.16)	(0.16)
HS Discount × Yearly income	0.46	0.39	-0.52**	-0.44*	0.29**	0.24*
	(0.29)	(0.28)	(0.23)	(0.23)	(0.14)	(0.14)
Controls	No	Yes	No	Yes	No	Yes
Adj. R^2	0.03	0.07	0.01	0.07	-0.00	0.02
Observations	349	349	349	349	349	349

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. Outcome variables are expenditure on child investment goods, groceries and luxury goods, expressed in pounds at baseline prices. Order effects represent the randomized order in which the three types of goods appeared in the investment task. Yearly income is divided by the square root of the household size. All models control for individual and household characteristics. HS indicates participants exposed to hard scenarios. Discount indicates parents who received a 50% discount on child investment goods.

Table A-14: Treatment effects on demand for investment goods, groceries and luxury goods among the lower income group including controls for variables unbalanced across treatment arms

	(1) Investment	(2) Investment	(3) Groceries	(4) Groceries	(5) Temptation	(6) Temptation
HS	-0.31	0.21	-0.18	-0.74	0.80	0.87
	(1.81)	(1.90)	(1.94)	(2.00)	(1.36)	(1.32)
Discount	7.32***	7.49***	-1.09	-1.38	0.41	0.46
	(2.65)	(2.69)	(2.01)	(1.99)	(1.26)	(1.25)
HS Discount	1.94	2.39	4.01**	3.62**	-1.54*	-1.51*
	(2.47)	(2.47)	(1.69)	(1.67)	(0.88)	(0.86)
Any payments past month (spouse)		-0.90		1.76		-0.34
		(2.20)		(1.71)		(0.92)
From paycheck to paycheck		-2.95		2.00		-0.079
		(1.99)		(1.71)		(1.10)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Unbalanced variables	No	Yes	No	Yes	No	Yes
Adj. R^2	0.08	0.08	0.11	0.11	0.04	0.03
Observations	182	182	182	182	182	182

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. Outcome variables are expenditure on child investment goods, groceries and luxury goods, expressed in pounds at baseline prices. Order effects represent the randomized order in which the three types of goods appeared in the investment task. Higher/lower income group denote the upper/lower 50% of the sample income per adult equivalent distribution. All models control for individual and household characteristics. HS indicates participants exposed to hard scenarios. Discount indicates parents who received a 50% discount on child investment goods.

Table A-15: Treatment effects on demand for investment goods, groceries and luxury goods for the lower income group, with and without order effects

	(1) Investment	(2) Investment	(3) Groceries	(4) Groceries	(5) Temptation	(6) Temptation
HS	-1.37	-0.31	0.80	-0.18	0.71	0.80
	(1.86)	(1.81)	(2.04)	(1.94)	(1.32)	(1.36)
Discount	5.43**	7.32***	0.61	-1.09	0.22	0.41
	(2.70)	(2.65)	(2.01)	(2.01)	(1.19)	(1.26)
HS Discount	0.89	1.94	4.91***	4.01**	-1.76**	-1.54*
	(2.65)	(2.47)	(1.85)	(1.69)	(0.83)	(0.88)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Order effects	No	Yes	No	Yes	No	Yes
Adj. R^2	0.01	0.08	0.04	0.11	0.02	0.04
Observations	182	182	182	182	182	182

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. Outcome variables are expenditure on child investment goods, groceries and luxury goods, expressed in pounds at baseline prices. Order effects represent the randomized order in which the three types of goods appeared in the investment task. Higher/lower income group denote the upper/lower 50% of the sample income per adult equivalent distribution. All models control for individual and household characteristics. HS indicates participants exposed to hard scenarios. Discount indicates parents who received a 50% discount on child investment goods.

Table A-16: Treatment effects on demand for investment goods, groceries and luxury goods for the higher income group, with and without order effects

	(1) Investment	(2) Investment	(3) Groceries	(4) Groceries	(5) Temptation	(6) Temptation
HS	2.82	2.59	-2.64	-2.32	-0.51	-0.75
	(2.14)	(2.23)	(2.26)	(2.32)	(1.05)	(1.06)
Discount	6.19**	5.45*	-0.79	-0.29	0.70	0.67
	(2.85)	(2.85)	(2.19)	(2.19)	(1.36)	(1.31)
HS Discount	7.11***	7.12***	-3.04	-3.00	2.36	2.26
	(2.35)	(2.32)	(2.12)	(2.01)	(1.47)	(1.39)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Order effects	No	Yes	No	Yes	No	Yes
Adj. R^2	0.03	0.04	0.03	0.07	0.09	0.13
Observations	167	167	167	167	167	167

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. Outcome variables are expenditure on child investment goods, groceries and luxury goods, expressed in pounds at baseline prices. Order effects represent the randomized order in which the three types of goods appeared in the investment task. Higher/lower income group denote the upper/lower 50% of the sample income per adult equivalent distribution. All models control for individual and household characteristics. HS indicates participants exposed to hard scenarios. Discount indicates parents who received a 50% discount on child investment goods.

Table A-17: Order effects on demand for investment goods, groceries and luxury goods for HS and ES treatment arms, among the lower-income group

	Easy Scenarios			Hard Scenarios		
	(1) Investment	(2) Groceries	(3) Temptation	(4) Investment	(5) Groceries	(6) Temptation
Groceries 1st	-5.39	6.18**	0.33	-12.0***	8.91***	-1.36
	(3.46)	(2.97)	(1.24)	(3.26)	(2.76)	(2.01)
Temptation 1st	4.59	-0.14	-1.32	-3.96	3.48	0.70
	(3.68)	(2.80)	(1.62)	(3.68)	(2.96)	(2.33)
Groceries 2nd	-2.15	2.28	2.12	-8.24**	6.16*	-0.74
	(3.36)	(2.97)	(1.46)	(3.97)	(3.22)	(2.47)
Temptation 2nd	1.93	1.21	-1.51	-3.39	5.56*	-1.79
	(3.04)	(2.64)	(1.51)	(3.35)	(2.81)	(1.96)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.15	0.03	0.10	0.10	0.18	-0.01
Observations	102	102	102	80	80	80

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. Outcome variables are expenditure on child investment goods, groceries and luxury goods, expressed in pounds at baseline prices. Order effects represent the randomized order in which the three types of goods appeared in the investment task. Higher/lower income group denote the upper/lower 50% of the sample income per adult equivalent distribution. All models control for individual and household characteristics. HS indicates participants exposed to hard scenarios. Discount indicates parents who received a 50% discount on child investment goods.

Table A-18: Order effects on demand for investment goods, groceries and luxury goods for HS and ES treatment arms, among the higher-income group

	Easy Scenarios			Hard Scenarios		
	(1) Investment	(2) Groceries	(3) Temptation	(4) Investment	(5) Groceries	(6) Temptation
Groceries 1st	-1.50	3.38	-2.58	-6.10	6.08*	-2.73
	(4.49)	(3.54)	(1.87)	(4.24)	(3.39)	(2.11)
Temptation 1st	-1.93	0.020	1.30	-3.88	0.76	2.89
	(5.20)	(3.99)	(2.27)	(3.45)	(3.05)	(1.87)
Groceries 2nd	5.27	-1.27	-2.12	-3.24	2.54	-0.44
	(4.77)	(3.64)	(2.26)	(3.20)	(3.13)	(2.23)
Temptation 2nd	3.78	-1.53	-0.80	-4.73	2.49	2.47
	(4.49)	(3.50)	(2.20)	(4.01)	(3.13)	(2.08)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	-0.05	0.02	0.03	0.10	0.01	0.14
Observations	81	81	81	86	86	86

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. Outcome variables are expenditure on child investment goods, groceries and luxury goods, expressed in pounds at baseline prices. Order effects represent the randomized order in which the three types of goods appeared in the investment task. Higher/lower income group denote the upper/lower 50% of the sample income per adult equivalent distribution. All models control for individual and household characteristics. HS indicates participants exposed to hard scenarios. Discount indicates parents who received a 50% discount on child investment goods.

Table A-19: Heterogenous effect by days since payment (less or more than 3 weeks) for the lower-income group

	<= than 21 days since pay			> than 21 days since pay		
	(1) Investment	(2) Groceries	(3) Temptation	(4) Investment	(5) Groceries	(6) Temptation
HS	0.35	-1.89	2.16	-2.68	5.41	-4.19
	(2.24)	(2.14)	(1.63)	(12.7)	(13.5)	(5.22)
Discount	11.9***	-6.34***	2.34*	8.01	3.71	-7.43
	(3.12)	(2.35)	(1.39)	(13.1)	(14.2)	(4.39)
HS Discount	7.33**	-1.22	-0.13	-10.5	15.8	-2.29
	(3.50)	(2.33)	(1.05)	(10.6)	(9.95)	(3.02)
p-value $\beta_{Discount} = \beta_{HSDiscount}$	0.21	0.03	0.06	0.16	0.37	0.35
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.22	0.17	0.08	-0.05	-0.57	0.12
Observations	100	100	100	30	30	30

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. Outcome variables are expenditure on child investment goods, groceries and luxury goods, expressed in pounds at baseline prices. Higher/lower income group denote the upper/lower 50% of the sample income per adult equivalent distribution. All models control for individual and household characteristics. HS indicates participants exposed to hard scenarios. Discount indicates parents who received a 50% discount on child investment goods.

Table A-20: Heterogenous effect by days since payment (less or more than 2 weeks) for the lower-income group

	<= 14 days since pay			> 14 days since pay		
	(1) Investment	(2) Groceries	(3) Temptation	(4) Investment	(5) Groceries	(6) Temptation
HS	0.35	-2.09	2.19	4.40	-5.65	1.36
	(2.44)	(2.35)	(1.84)	(6.47)	(6.72)	(2.91)
Discount	10.1***	-5.67*	2.84	6.98	3.49	-4.59**
	(3.57)	(3.10)	(1.86)	(5.25)	(4.64)	(1.96)
HS Discount	7.45*	-0.70	-0.75	-1.50	6.95	-2.36
	(4.05)	(2.80)	(1.31)	(5.87)	(5.24)	(2.22)
p-value $\beta_{Discount} = \beta_{HSDiscount}$	0.54	0.08	0.02	0.12	0.43	0.29
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.19	0.19	0.11	0.04	-0.08	0.17
Observations	82	82	82	48	48	48

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. Outcome variables are expenditure on child investment goods, groceries and luxury goods, expressed in pounds at baseline prices. Higher/lower income group denote the upper/lower 50% of the sample income per adult equivalent distribution. All models control for individual and household characteristics. HS indicates participants exposed to hard scenarios. Discount indicates parents who received a 50% discount on child investment goods.

Table A-21: Heterogenous effect by days since payment (less or more than 10 days - median split) for the lower-income group

	<= than 10 days since pay			> 10 days since pay		
	(1) Investment	(2) Groceries	(3) Temptation	(4) Investment	(5) Groceries	(6) Temptation
HS	0.010	-2.16	2.97	4.17	-5.37	1.41
	(2.61)	(2.36)	(2.07)	(6.01)	(6.48)	(2.94)
Discount	10.4**	-6.74*	3.29	6.89	2.40	-3.30*
	(4.40)	(3.46)	(2.05)	(4.46)	(4.23)	(1.86)
HS Discount	7.47*	-0.82	-0.72	-2.24	7.40	-1.38
	(4.05)	(2.84)	(1.38)	(5.24)	(4.57)	(2.04)
p-value $\beta_{Discount} = \beta_{HSDiscount}$	0.54	0.05	0.02	0.06	0.21	0.26
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.14	0.17	0.12	0.17	-0.00	0.12
Observations	77	77	77	53	53	53

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. Outcome variables are expenditure on child investment goods, groceries and luxury goods, expressed in pounds at baseline prices. Higher/lower income group denote the upper/lower 50% of the sample income per adult equivalent distribution. All models control for individual and household characteristics. HS indicates participants exposed to hard scenarios. Discount indicates parents who received a 50% discount on child investment goods.

1.B Appendix B - Experimental Design

1.B.1 Experimental Task

Financial Scenarios

Instructions - In the following section you will be presented 3 scenarios and asked to answer how you would go about dealing with the situations if they were to happen to you. Please take your time answering the questions. Try to have at least 3 sentences in your open question answers.

1. Imagine that an unforeseen event requires of you an immediate (£2000/£100) expense. You need to raise the money in less than a week.
    - Are there ways in which you may be able to come up with that amount of money on a very short notice? (yes/no)
    - How would you go about getting (£2000/£100) on a very short notice? Three sentences should be enough. (open)
    - To what extent do you agree with the following statements? (4 item Likert: strongly disagree - strongly agree)
        (a) "Coming up with (£2000/£100) on a very short notice would cause me longlasting financial hardship.
        (b) "Coming up with (£2000/£100) on a very short notice would require me to make sacrifices that have long-term consequences.
2. Due to a national policy change, there is an increase in the monthly cost of childcare by (£200/£10), which amounts to a total cost increase of (£2400/£120) a year. This increase is not reimbursable by any government funding scheme and it applies to all forms of childcare (nursery, kindergarten, childminder, nanny, au pair etc.).
    - Would it be difficult to afford childcare after the policy change? (yes/no)
    - How would you go about covering the cost of childcare after the policy change? Would you need to make any sacrifices and budget cuts every month? Three sentences should be enough. (open)
    - To what extent do you agree with the following statements? (4 item Likert: strongly disagree - strongly agree)
        (a) "Paying additional (£200/£10) a month for childcare would require difficult budget cuts and sacrifices every month.

(b) "Paying additional (£200/£10) a month for childcare would be too costly and it would probably require leaving the child in the care of relatives or becoming a stay-at-home parent.

3. Imagine that the economy is going through difficult times. Your household's monthly expenses increase by (£300/£15) due to higher food, energy and housing prices.

   - Please indicate to what extent do you agree with the following statement: "Given my situation, I would be able to maintain roughly the same lifestyle under those new circumstances. (4 item Likert: strongly disagree - strongly agree)
   - In what ways would the (£300/£15) increase in your monthly expenses would impact your leisure, housing or travel plans? What changes would you need to make? Three sentences should be enough. (open)
   - To what extent do you agree with the following statement: "The (£300/£15) increase in our monthly expenses would strongly impact our leisure, housing, or travel plans." (4 item Likert: strongly disagree - strongly agree)

Investment Task

Instructions: In the following task you have to choose what goods to purchase with a budget of £30 .

You will see a list of available goods, with a picture, title and the price displayed for each of them. The price of the goods is the retail price including the discounts offered by the retailer. If you need additional information on the goods, by clicking on the picture a new window will open with further details from the website of the retailer.

Some of the goods have a **discount of 50%** in addition to any discount of the retailer. These goods are the ones with two prices listed - one black and crossed out which is the retail store price and one red which is the price in the task.

By clicking on the **ADD button**, the goods will be added to the shopping cart. You can edit the shopping cart content at any time by clicking on the **Shopping Cart** section in the top-right side of your screen.

A new window will open with the goods already selected. You can modify the quantities of each good or remove them from the shopping cart. You can return to the main window at anytime by clicking on close, or anywhere outside the shopping cart window.

When you are satisfied with your selection, click on **Checkout** in the shopping cart window to proceed to the next page. Try to spend as close to the £30 budget as possible. To proceed

to the next page you need to spend a minimum of £28. Any remainder will be added as bonus payment on Prolific.

You can access these instructions at any time by clicking on the **Instructions** section in the top-left side of the page.

1 out of every 100 participants will be selected for payment. If you are selected, the goods will be delivered to a collection location of your choice at a date and time that is convenient for you. You can pick up your goods with the code we will send you.

Figure A-1: Main screen of the investment task

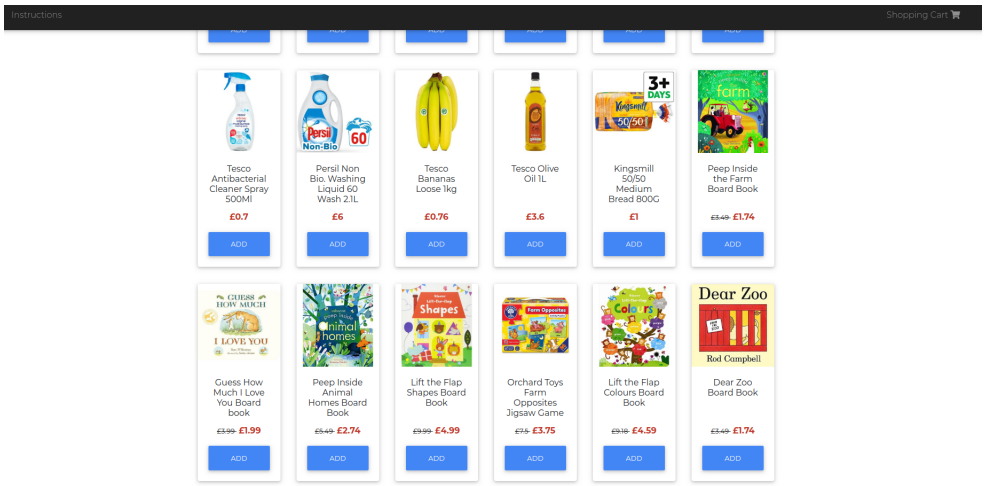
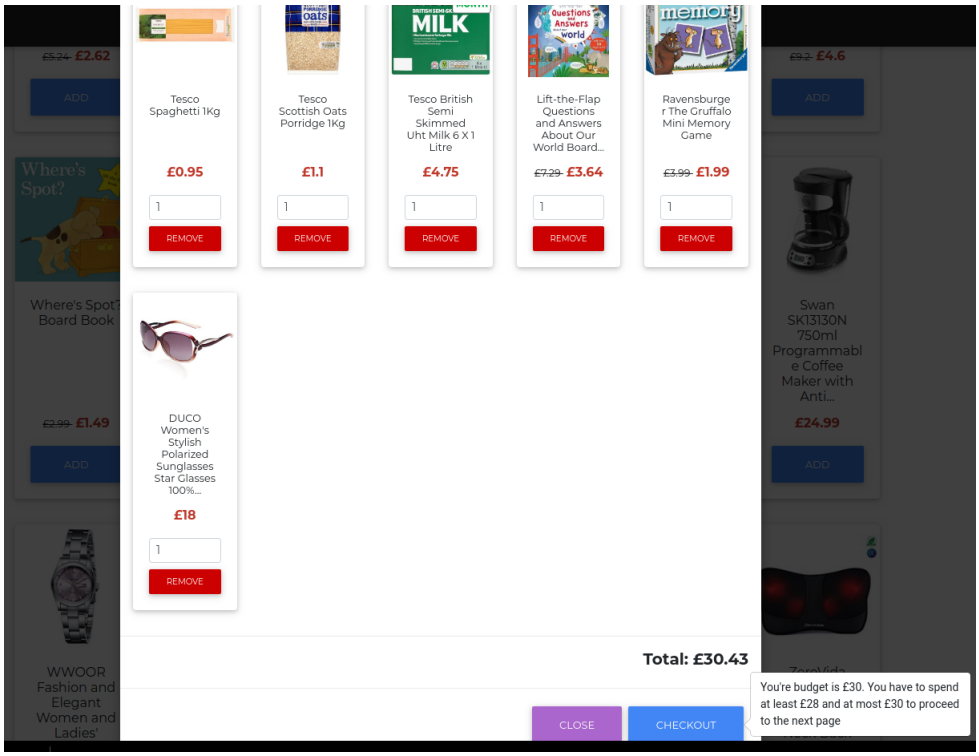


Figure A-2: Checkout screen of the investment task



1.B.2 Self-reported parenting practices and beliefs

The parenting practices index is constructed using 4 items adapted from the HOME and FCI indexes (Caldwell and Bradley, 2003; Hamadani et al., 2010), 3 of them used also in Boneva and Rauh (2016). Parents report: (i) on how many days in the past 7 days someone in the household read stories or looked at picture books with the child or (ii) tried to teach the child colors, shapes, numbers, letters or animals, (iii) whether the child was taken a walk in the park or to a playground in the past 7 days and (iv) how many hours a day on average does the child spend watching TV or using a phone, tablet or computer.

Parents are then asked two items reflecting parental beliefs on returns to investment. Adapted from Attanasio et al. (2015), parents state to what extend they agree or disagree that: (i) children develop at their own pace and there is not much parents can do about that and (ii) children who are often read stories and who hear many words in the early years do better in school.

Chapter 2

Blinded by worries: sin taxes and demand for temptation under financial worries

with Austeja Kazemekaityte, Piero Ronzani, and Lucia Savadori

Imposing "sin" taxes has been the preferred way governments tried to discourage the over-consumption of temptation (addictive goods) for decades. While traditionally such taxes addressed tobacco and alcohol, in recent years the debate shifted towards taxing also unhealthy foods. Such taxes are based on the premise that individuals may suffer from various behavioral biases which impedes them from acting in ways such that they would not impose harm on their future self or on society. This paper investigates a potential bias and how it affects demand for temptation: financial worries associated with poverty have been shown to shift attention towards pressing needs, often at the expense of forward looking decisions. In an online experiment with UK participants, we randomly induce financial worries and ask participants to allocate a budget between basic necessities and temptation goods in an experimental market. We randomly impose "taxes" on temptation by increasing its price. We find that, in the absence of any tax, inducing financial worries lowers demand for temptation, effect stronger for lower income participants. However, when financial concerns are salient, increasing the tax does not lower demand among lower-income participants. While financial worries constrain the consumption of temptation in the absence of tax changes, they might also hurt the poor when additional taxes are introduced given that they do not respond to further price increases.

JEL Codes: D12, D91, H23, H24, I12, I18, I31

Keywords: psychology of poverty, temptation, addiction, sin taxes, bandwidth, focus, scarcity

2.1 Introduction

The shift in behaviour concerning the over-consumption of temptation (addictive) goods, such as alcohol and tobacco, has been the goal of policy makers for decades. What has previously been understood as an irrational behaviour, has been reconsidered by Becker and Murphy (1988). In their model of *rational addiction*, an individual understands the costs incurred from the consumption of temptation, but continues to consume it if utility exceeds the future costs of consumption. However, this also makes consumption relatively sensitive to both current and future price of temptation. This constitutes the logic behind *sin taxes* - a mechanism levied on goods that produce negative effects and tend to be over-consumed.

In addition to maximization of tax revenue, sin taxes provide a two-fold corrective purpose. Firstly, they are built on the principle of a classical Pigouvian tax: externalities of temptation can affect people who are not consuming them directly, such as consumption of sugary drinks resulting in financial healthcare costs that are then borne by the whole population (Allcott et al., 2019b). In addition, a growing number of behavioural research has underlined the existence of *internalities* - harm experienced and not fully *internalized* - by individuals themselves. Among most common reasons are biases, misinformation or lack of information, as well as self-control issues and inconsistent time-preferences (Allcott et al., 2019b; Cutler et al., 2003). Therefore, sin taxes can also serve a purpose in terms of correcting internalities.

However, the welfare benefits from implementation of sin taxes might come with unequally distributed costs within a population. The regressivity of sin taxes is widely discussed in the literature. Evidence shows that consumption of such goods as junk food, sugary drinks, or tobacco, has a strong disproportionate socioeconomic concentration (Gruber, 2001; Allcott et al., 2019b; Colman and Remler, 2008; Maclean et al., 2014), which means that low-income consumers will pay more taxes levied on these goods. Although Gruber and Kőszegi (2004) argue that overall benefits from averted internalities offset the incurred costs and overturn the regressivity, the exact outcome depends on price elasticity of demand which, in turn, can also be affected by behavioural biases. Therefore, the overall welfare gains from sin taxes can end up being lower than expected solely because of behavioural aspects that are not considered in the classical economic models of addiction.

This paper explores a behavioral bias emerging from the psychology of poverty literature which has not been studied in the context of consumption of temptation and sin taxes. Poverty, scarcity of financial resources, has been shown to affect cognitive performance (Mani et al., 2013) and investment decisions in human capital, as seen in chapter 1 and in (Lichand et al., 2018). The proposed channel is scarcity shifting focus towards the scarce resource (Shah et al., 2018),

often at the expense of forward looking decisions. One feature of poverty not explored until now is that it also implies scarcity of immediate gratification. A person experiencing poverty has much stricter constraints when it comes to the choices she or he can make. Shopping for groceries means carefully selecting necessary items, not allowing much slack for goods that offer immediate pleasure. Temptation goods may serve this need and are much harder to substitute given a limited budget (the poor cannot afford going to cultural events, dining out or going on holidays as easily as the non-poor can). If low income people perceive temptation goods as scarce, attention may be redirected towards them which could be a channel explaining why the poor over-consume them. An alternative channel could operate through the link between poverty and self-control (Spears, 2011; Bernheim et al., 2015). Making difficult economic decisions under poverty may deplete self-control in other domains such as consumption of temptation. In this paper we investigate if demand for temptation changes when financial worries are salient. In addition, we investigate if such psychological mechanisms may change how people respond to sin taxes.

In an online experiment in the UK, 807 participants are first assigned to a psychological manipulation which aims to mimic the mental burden of poverty by making thoughts associated with economic vulnerability or lack of financial resources salient. For brevity, we will refer to this mechanism as increasing financial worries (FW henceforth). Adapted from Mani et al. (2013), the treatment consists of asking participants to ponder how their household could cope if they had to face various economic shocks. The treatment varies the severity of the shock, from mild for control group participants (Easy Scenarios ES group) to severe for the treatment group (Hard Scenarios HS group).

Next, participants are endowed with £30 budget to be spent on basic necessities (food, household products) or on temptation (alcohol, tobacco, sugary drinks, sweets, unhealthy snacks and personal luxury goods). At this stage a second treatment is introduced by exogenously varying the price of temptation: a random share of participants face the market price while the rest face higher prices by 10% or 20%. We will refer to this treatment group as the Tax group throughout the paper even though in the experiment it was not framed as a tax but as a price increase.

We find that increasing FW decreases the demand for temptation when no additional tax is applied. The magnitude of the effect is the equivalent of a 10% increase in price when FW are not made salient, suggesting that the shift in focus towards necessities may actually protect against the over-consumption of temptation. However, in line with Shah et al. (2012) and the results in chapter 1, we find that the shift in focus comes at a price: participants become far less sensitive to other relevant information, in our design this being an increase in price. Among the

HS group, the effect of the tax is statistically insignificant at all tax levels. In contrast, demand for temptation is highly elastic in the ES group.

We check if results vary by tertiles of income. As expected, both effects are stronger for low income participants. Increasing FW has a large effect on demand in the absence of the tax, but makes low income participants ignore the tax completely. Demand actually increases with tax when FW are salient. When FW are not made salient, low income participants react only to the 20% tax. Averaging the tax effects in the ES and HS conditions, low income participants have a flat demand curve. In contrast, high income participants respond strongly to the tax, especially in the ES condition. Increasing FW also appears to decrease the demand and to dampen their response to the tax. Among the middle tertile, increasing FW does not have any significant impact on demand at any tax level.

We investigate several alternative mechanisms for the observed effects in addition to the hypothesized effect: cognitive reflection, risk and time preferences, and affective states. We only find treatment effects among the lower income group. Increasing FW increases cognitive reflection and makes participants more likely to state that they purchased goods which are necessary in the purchasing task. We perform a mediation analysis and find that only the latter mediator has a strong explanatory power of the main effect suggesting that the change in focus towards necessities is a credible mechanism.

Welfare implications are not straightforward. Independently of the ES-HS condition, we find the elasticity of demand with respect to price to be increasing in income which would suggest such taxes to be highly regressive. Looking at the dynamics by ES-HS condition, we again find elasticity to increase with income when FW are not salient. Increasing FW does reduce demand by the highest amount for low income participants when no additional tax is added, but it also makes them ignore the tax altogether. From a policy perspective, the results suggest that low income participants may not respond optimally to sin tax increases in periods of economic instability.

This paper speaks to several literature. To our knowledge, this is the first paper to study addictive goods in the psychology of poverty framework and to provide causal evidence that perceived financial worries associated with poverty can lower the demand for addictive goods white distorting how people respond to sin taxes. In these lines, the paper contributes to the vast behavioral economics literature documenting the role played by psychological factors on (i) addiction (Gruber, 2001; Gruber and Kőszegi, 2004; Allcott et al., 2019a) and (ii) public policy in general (Amir et al., 2005; Bernheim and Rangel, 2007; Chetty et al., 2009; Congdon et al., 2011; Chetty, 2015; Bernheim and Taubinsky, 2018). Furthermore, the paper contributes to the

growing literature on the psychology of poverty (Mani et al., 2013; Mullainathan and Shafir, 2013) and in particular strengthening the finding that poverty shifts focus to pressing needs at the cost of under-weighting other relevant information (Shah et al., 2012, 2018; Lichand et al., 2018, see chapter 1). Finally, the paper contributes to the growing experimental literature using laboratory or field experiments to study public policies (Alm, 2010; Rees-Jones and Taubinsky, 2016; Mullainathan and Shafir, 2013; Taubinsky and Rees-Jones, 2017; Lunn and Choisdealbha, 2018).

The paper is organized as follows. Section 3.2 reviews the recent literature on sin taxes and psychology of poverty. Section 2.3 presents the experimental design, data details, descriptive statistics and balance checks, while section 3.5 discusses the main results. Finally, section 2.5 provides a discussion and concludes.

2.2 Literature

2.2.1 Sin taxes

The addictive behaviours have puzzled policy makers for decades: how does one construct impactful policies that deliver welfare benefits? The standard economic models of addiction discuss consumption decisions in light of classical rational behaviour of an economic agent, that at first glance seems to overlook the core idea of *addiction*. However, the *theory of rational addiction* introduced by Becker and Murphy (1988) showcase that addiction does not neglect rationality. According to their theory, individuals have, among others, consistent and intertemporally-dependent preferences for addictive goods. Consumers maximize their utility subject to costs of temptation: since goods are addictive, the past use has a direct impact on future consumption which comes with both monetary and, in case of such goods as tobacco or alcohol, health costs, but as long as it generates positive utility high enough to outweigh costs, individual continues to consume these goods. Nevertheless, addictive behaviours, though stemming from endogenous tastes, are still sensitive to the market prices of goods. For this reason, taxes are the standard economic way to regulate the over-consumption of temptation goods.

The standard theories underline the importance of government regulations in correcting *externalities* arising from consumption of temptation goods. As argued by Becker & Murphy, use of such goods as alcohol or tobacco is driven by rational choice therefore cost experienced by consumers themselves is fully *internalized*, while cost falling on a society is not. The concept of *externalities* implies that consumption of good harms society at large, for example smokers directly affecting non-smokers around them or avid consumers of sugar sweetened beverages using public medical resources to treat health issues resulting from the immoderate sugar intake (that could have otherwise been used for other medical emergencies) (Gruber and Köszegi,

2001; Allcott et al., 2019b). This Pigouvian tax principle constitutes the basis for so-called *sin taxes*, i.e. taxes put on goods that are associated with over-consumption and negative effects to both consumer and society. However, a more recent stream of behavioural literature also started drawing attention to the side of *internalities*. Various authors have argued that fully rational understanding of costs incurred by consumption of sin goods can be impaired by such factors as behavioural biases, misinformation, inconsistent time-preferences, or self-control (Allcott et al., 2019b; Cutler et al., 2003; Gruber and Köszegi, 2001). For example, a real life behaviour overlooked by classical economic models of addiction refers to the problem of time inconsistency: the future version of yourself tends to be more patient and to have a higher degree of self-control as compared to yourself in the present (Gruber and Köszegi, 2001). Here sin taxes can also work as an *internality* correcting mechanism: the costs incurred from the use of temptation good in the present period increase and push individual to reconsider the consumption of good, thus acting as her future self intended to.

However, when discussing taxes as a type of corrective mechanism, an important point to consider is the distribution of tax burden. Regressivity of sin taxes has been widely discussed in the literature. Evidence tends to point out that consumption of tobacco, sugar-sweetened beverages, or junk food is prevalent among lower socioeconomic status individuals (Gruber, 2001; Colman and Remler, 2008; Maclean et al., 2014; Allcott et al., 2019b). Low-income consumers might carry a bigger tax burden on their shoulders since they tend to consume more temptation goods. As Allcott et al. (2019b) point out, we need a clear distinction between the weight of tax burden and the overall harms and benefits. First, consumption decision depends on price elasticity of demand, meaning that although poorer individuals might consume more temptation goods *per se*, this does not in itself imply that their consumption levels will remain the same with increase in taxes. Moreover, if poorer households decrease their consumption of temptation goods such as tobacco or sugary drinks, this results in better health outcomes and, in turn, lower medical expenditures, thus overturning the regressivity argument (Gruber and Kőszegi, 2004; Allcott et al., 2019b). The problem arises when we discuss behavioural biases: choice of temptation goods can stem from such issues as misinformation or self-control, biases the estimated positive impact of sin taxes. Allcott et al. (2019a) estimate that, for example, sugar-sweetened beverage tax designed without addressing behavioural issues can result in $1 billion a year less of welfare gains in United States.

Much of theoretical and empirical literature has analyzed taxes levied on alcohol and tobacco, but the results are divergent. It is a well-established fact that higher future price of tobacco affects the consumption in the current period (Chaloupka, 1991; Becker et al., 1990). A recent stream of research, using more sophisticated modes of analysis, show that the effect

of tobacco tax might not be as high as previously considered (Maclean et al., 2014). Recent articles on tobacco consumption move outside the classical framework and consider other factors that affect the effectiveness of tax. For example, White and Ross (2015) found that smokers in Thailand compensate for an increase in a tobacco tax by switching to cheaper brand or to roll-your-own tobacco, or purchasing a lesser quantity of cigarettes. The compensating behaviour is also discussed by Adda and Cornaglia (2006): they find that smokers try to extract more nicotine per cigarette when taxes push the prices up. This suggests that relation between sin taxes and health outcomes does not necessarily have a positive causal direction. Goldin and Homonoff (2013) check for difference in salience of tobacco taxes (whether they are applied in the registry or are already calculated in the posted price) and find that lower income individuals are more attentive and reactive to taxes applied in the register. Moreover, estimates of tobacco expenditures suggest that tobacco taxes can be highly regressive (Evans et al., 1999a,b; Gruber and Koszegi, 2002). The situation is less straight-forward in terms of alcohol consumption. For example, Vandenberg and Sharma (2016) suggest that in Australia the argument of alcohol tax regressivity is not a big concern; biggest burden of tax falls on heavy drinkers shoulders and due to a more balanced distribution in population the regressive impact on taxation would be limited in its scope. Daley et al. (2012) argue that in US high-risk drinkers tend to come from disadvantaged population segments, but the highest share of alcohol taxes will be paid by lower-risk drinkers that come from higher socioeconomic status groups.

More recently attention of policy makers has shifted on a "newer" type of sin taxes - taxes on sugar-sweetened beverages (SSB). It is a well established fact that consumption of sugary drinks has become a significant source of caloric intake, contributing to sugar-related health issues (Allcott et al., 2019b; Dubois et al., 2017; Harding and Lovenheim, 2017). However, the consumption of such products, as in case of tobacco, varies strongly with the socioeconomic status, as lower income households consume more sugary drinks and have less elastic demand (Dubois et al., 2017; Wang, 2015; Allcott et al., 2019a). The regressive nature of SSB tax also depends on tax pass-through. Evidence from papers that study recently implemented SSB taxes is mixed depending on exact case, however it is not uncommon that taxes are fully shifted to consumers (Grogger, 2017; Cawley et al., 2018; Bonnet and Réquillart, 2013). In order to have a positive impact on health and avoid substitution effect, when instead of taxed sugary drinks consumers choose junk food, some suggest to introduce nutrient-specific taxes, such as tax on sugar instead of tax on soda (Harding and Lovenheim, 2017). Nevertheless, Allcott et al. (2019a) argue that though SSB tax would have a highly regressive nature in US, welfare benefits would be distributed more equally across all income groups and internality correction would be the strongest for lower income households.

United Kingdom is one of the more recent countries that have implemented sugar-sweetened beverage tax. Since 2018, any drink containing 8g or more of sugar per 100ml is taxed by 24p per liter, and 18p per liter if sugar content is between 5-8g of sugar. This does not apply to fruit juices with natural sugars and drinks high in calcium, such as 75% milk. This policy has been implemented as a mean to tackle the childhood obesity problem. The revenues from soda tax are used to fund development of sport programs and healthy food supply in the schools. In addition to SSB tax, UK has a long history of tobacco and alcohol duties. Among European Union countries, UK has one of the highest rates of beverage taxes among all categories (Angus et al., 2019). Duty rates differ based on type of beverage (beer, cider, wine, or spirit) and the strength of it, where drinks with higher strength are taxed more. For tobacco products UK government uses a so-called "tobacco tax escalator" which means that tax rises automatically by 2% above the inflation level every year since 2010 (Fuchs et al., 2019). Although there is a clear intention to regulate tobacco consumption, Partos et al. (2017) find that from 2002 to 2014 the gap between expensive and cheap tobacco products has remained wide enough for people to be able to switch between products, thus diminishing the *externality-internality* correcting effect.

2.2.2 Psychology of poverty

In recent years, a growing number of empirical studies investigated the *psychology of poverty* and its impact on economic decision making. Scarcity of resources shifts individual attention to that which is lacking, thus affecting cognitive functioning (Mullainathan and Shafir, 2013). This leads people to overlook certain information when making a decision (Mullainathan and Shafir, 2013) or keep monetary concerns on top of their mind even when it is not explicitly linked to the particular situation one is supposed to think about (Shah et al., 2018).

Such *scarcity mindset* has been shown to create a cognitive burden. In the work by Mani et al. (2013) cognitive functions were analyzed in the light of financial worries in two different settings - shopping mall in US and sugarcane farmers' villages in India. People of lower income performed much worse cognitively when they were asked to reflect how they with cope with hard financial scenarios as compared with easy ones. People of higher income were found to exhibit no difference in their scores on the cognitive test. In a natural experiment setting, sugarcane farms performed worse in cognitive tasks before harvests (period of higher financial scarcity) than after harvest.

In a similar design to the sugarcane farmers' experiment, Carvalho et al. (2016) administered before and after payday surveys to US households. Results indicate stronger present bias in decisions involving monetary rewards in the before-payday survey, but no differences are found in regard to risk preferences, cognitive functions, and quality of other decisions.

Persistent financial worries translate into a cognitive burden which in turn manifests itself in various domains. Experimental studies have shown that the effect of cognitive load is wide ranging, as summarized in work by Deck and Jahedi (2015). Under high cognitive load individuals tend to be more likely to anchor their decisions, exhibit lower numeracy skills, be more risk averse and more impatient in decisions involving money. Although this review suggest that impatience is not observed when decisions involve unhealthy snacks or consumption goods, other studies have shown that cues on the harshness of environment (such as information on economic crisis) increase the consumption of food that is high in calories and filling (Laran and Salerno, 2013). Women facing financial constraints have a tendency to eat more (Hill et al., 2013) and consume more beauty products (Hill et al., 2012) under the threat of scarce resources. A study on scarcity, consumer choice and neuroimaging by Huijsmans et al. (2019) has suggested that the increased focus on scarce resources decreases the activity in the brain center associated with goal-directed decision making and the effect is strongest when scarcity is preceded by the period of abundance, which would suggest that financial constraints interfere with the ability to follow goals in decision making.

Although some of the purchasing decisions might look sub-optimal when made under a financial constraint, poor consumer are found to be more focused on the greatest needs when evaluating the trade-offs in their consumption decisions (Shah et al., 2015). When faced with financial worries, parents tend to overlook the opportunity to invest in the human capital of their child by choosing necessities, such as groceries, instead of highly subsidized educational materials for children (see results in chapter 1). People with low income tend to exhibit higher consistency in choosing to travel in order to obtain a discount for a purchase despite the price of the good (Shah et al., 2015). They show higher efficiency in use of resources (Mullainathan and Shafir, 2013) and more choices of necessities as compared with discretionary goods (which are chosen more by people that are relatively financially unconstrained) (Cole et al., 2008).

The gap the study is trying to fill is studying temptation consumption decisions under financial worries. Previous studies have focused on time inconsistency as a main bias, affecting dis-proportionally the poor, and leading to the over-consumption of temptation (Gruber, 2001; Gruber and Kőszegi, 2004; Allcott et al., 2019a,b). The attention reallocation caused by focusing on monetary concerns may be an important additional factor affecting to a larger magnitude the consumption decisions of the poor.

2.3 Empirical Strategy

This section presents the empirical strategy starting with the experimental design in subsection 2.3.1, followed by the description of the data collection in subsection 2.3.2. Subsection 2.3.3

presents descriptive statistics, and checks for balance and selective attrition across treatment arms.

2.3.1 Experimental Design

The study has been run in May 2019, on Prolific, a crowdworking platform which has been noted for its better representativeness at national level (UK) compared to other widely used platforms (Peer et al., 2017; Palan and Schitter, 2018). Participants are paid on hourly wage basis with potential bonuses conditional on their performance in given experimental tasks. The study was pre-registered on the Open Science Framework (OSF) platform[1]. Sections of the analysis that deviate from the pre-registration are highlighted throughout the paper.

The main motivation of this study is to understand how financial worries (FW) affect: (i) the trade-off between addressing pressing needs and falling into temptation and (ii) the response to potential policies which aim to discourage the consumption of temptation. In practice, we manipulate the perceived FW through exposure to hypothetical financial scenarios and observe purchasing decisions in an experimental market where participants can choose to spend a fixed budget on necessities and temptation. A random subset of participant face higher prices of temptation goods than the retail prices. This treatment aims to mimic taxes on temptation or "sin taxes" how they are commonly referred to. We will refer to this treatment as the Tax condition throughout the paper. To two treatments are also interacted resulting in a 2x2 design as presented in Table 2.3.1.

Table 2.3.1: Experimental design

	Baseline prices	Higher prices
Easy Scenarios	ES group	Tax group
Hard Scenarios	HS group	HS Tax group

The rate the price increase was also assigned randomly to either a 10% or 20% level relative to the baseline prices[2]. Throughout the analysis, we will also explore this heterogeneity.

In what follows we will describe the treatments and the experimental market.

Financial Scenarios: Participants were asked to reflect how their household would cope with two income shocks: (i) a large one time shock and (ii) a deterioration in economic condi-

[1]To view the pre-registrations access `https://osf.io/fpkjw`

[2]We randomly assigned 40% of participants to the no tax group and 60% to the tax group (half to 10% tax and half to 20% tax). This was to have more power in detecting differences between the no-tax and the tax groups, than to detect differences between the two tax levels

tions at national level leading to higher costs of living. Adapted from Mani et al. (2013), the scenarios aim to trigger mental thoughts of economic vulnerability which participants from poor households are likely to experience often in their daily lives. Participants are asked to answer to both open questions and questions with Likert scales. What varies between conditions is the severity of the situations presented[3].

After completing the two scenarios, all participants were asked to state, on a Likert scale, how worried they are about (i) their financial situation and (ii) about not being able to find money in case of need. The aim of these questions is to test if the hard financial scenarios successfully triggered the response we described above.

Experimental market: Next, participants proceed to the main task. Each participant received an endowment of £30 which they could spend in the experimental market. They could choose from 66 items sold by one of the largest low cost retailers in the United Kingdom. The products were chosen based on their popularity on the online store platform of the retailer. Half of the items were basic necessities like bread, eggs, milk, or household items, such as washing liquid or cleaner. The prices ranged between £0.59 to £6. The other half were temptation goods, such as alcohol, tobacco or unhealthy foods (sweets, sugar sweetened beverages, chips etc.) with prices ranging from £1 to £20. Each product had a picture, name, price and a link to the retailer's online shop web-page with additional information on the product[4]. The interface looked very similar to a typical online shop. Participants could add goods to their shopping cart, increase quantities and revise their selection at any time. They had to spend at least £28 to advance to the next stage. The order of the goods was randomised. The task is weakly incentivized: 1 out of every 100 participants was randomly selected to receive the goods they selected on a date of their choice[5].

Participants assigned to the tax condition were informed that some of the goods have higher prices than the retail price but not by how much. In the task they could see the old price crossed out next to the new price. While this lacks realism as price and tax increases are rarely made salient, we did not want visual salience effects (as in Chetty et al., 2009) or effort (to discover which products have higher prices) to interact with the psychological treatment.

After completing the task, participants proceed to the survey section of the experiment. The variables measured are presented in subsections 2.3.2 and 2.4.3.

[3]See Appendix 2.B

[4]See Figures A-1 and A-2 in Appendix 2.B for screenshots of the task

[5]For ethical reasons, we preferred to have only a small share of participants to actually receive the products. In spite of the weak incentives, vast majority of participants reported that they chose what they would normally choose when they do the groceries and that they enjoyed doing the task

Limitations: While the experimental market featured products participants are familiar with and had an easy to use interface that made it feel like an ordinary online purchasing platform, there are several concerns regarding the extent to which the task can capture its real world counterpart. The first concern is that participants can substitute "extra-taxed" products in the experimental market with identical products at lower prices in the real world market. Thus participants in the tax condition could avoid the tax by simply re-optimizing their household consumption plans (e.g. buy more groceries in the experiment and more temptation outside the experiment). With this in mind, it is likely that the elasticities of demand estimated are upward biased. Second, participants could choose goods which have a higher reselling value to exchange them for cash outside the experiment. In our setting, such goods would likely be the temptation goods. However, we would expect a higher demand for temptation goods if this would be the case which does not match our data. Finally, whether the endowment was earned or not can matter in some settings (Harrison, 2007; Cherry et al., 2005; Luccasen and Grossman, 2017; Ackert et al., 2006). It is not clear however, if in our setting this would lead to a higher or lower demand for temptation or how it would interact with the psychological treatment.

2.3.2 Data and Power

Participants could not take part in the experiment if they were (i) bellow the age of 24, (ii) heavy drinkers (more than 14 units per week) or (iii) have undergone therapy for alcohol abuse. These variables are included in the Prolific's pre-screening database which means participants could not lie to be able to participate in our study. We decided to impose an age limit to screen out participants which may not be financially independent. Eligibility criteria (ii) and (iii) were added for ethical concerns. At the end of the study participants received a debriefing. The sample size (808 participants) was chosen, motivated by estimates from a previous study, to detect effects sizes above £3 by income subgroup at 5% significance level with 80% power. For the whole sample, the estimated minimum detectable effect size is around £1.5.

2.3.3 Descriptive Statistics, Balance Checks and Selective Attrition

Random assignment into treatment groups leads to causal inference if attrition was not influenced by treatment assignment and if the randomization was successful in terms of observable (and unobservable - not testable) characteristics. In this subsection, we evaluate both concerns and also present descriptive statistics.

Attrition. Online experiments often suffer from high rates of attrition, which when left unattended, can lead to flawed causal inferences (Zhou and Fishbach, 2016; Horton et al., 2011). Taking part in online experiments has lower fixed costs than laboratory experiments which usually require registering ahead of time and going in person to the lab. Furthermore, participants

can exit at any time without fearing any social punishment, from other participants or the experimenters. In our setting, both treatments could induce participants to exit the experiment before completion. Reflecting on ones financial vulnerability and facing price increases could trigger negative emotions which may increase the likelihood of dropping out of the study. If this were to happen, causal inference would be challenged since treated participants leaving the survey may be systematically different from those who opt to stay. We evaluate this by regressing the decision to drop out on treatment status. We consider only cases where participants left the survey when assigned or after being assigned to one of the treatments. Some participant left the survey prior to this and are not considered in the analysis. Only 39 participants dropped out, which represent less than 5% attrition rate, remarkable in an online experiment. Table A-1 in Appendix 2.A shows the results. Participants are slightly more likely to drop out when exposed to hard scenarios and taxed but the differences are small and statistically insignificant. Overall, the results show attrition is not a major concern for causal inference.

Balance Checks. Given the 2x2 experimental design, we need to evaluate whether randomization was successful for both treatments, accounting also for the interaction between the treatments. Table 2.3.2 shows means for the ES group in column (1), HS in column (2), tax group in column (3) and HS tax group in column (4). The last column displays the p-value associated with the F-test of joint significance of the differences between the treatment arms. Out of 14 comparisons, we find 2 variables to be significantly different across treatment groups. The HS tax group, in particular, has a lower share of overweight participants whereas the HS group has fewer participants which are parents. Neither variable is a strong predictor of behavior in the task. However, to alleviate concerns, we include them as covariates in all models. We also evaluate whether randomization was successful within the tax group across the two levels. Table A-2 in Appendix 2.A shows the means for the three tax level groups and the the p-value of the difference. Only the share of parents is statistically significant at 10% level and other 3 differences have low p-values. Again, to mitigate concerns, we also control for them throughout the analysis.

Descriptive Statistics. Females are over-represented in our sample with 68% of participants. The age of participants range from 26 to 86 with a mean of roughly 43 years. 56% attended university and about 72% are employed either full or part-time. Less than 7% are immigrants, 57% self-report being overweight and 17% are smokers. On average, participants report consuming around 3.3 units of alcohol a week. The average household size is 2.8, 60% of the sample are parents and the average total yearly household income is 36.8 thousand pounds with a median of 32.5 thousand pounds, both higher than the national levels in the UK. The large share of females is a major concern regarding the representativeness of our sample. The demand for temptation

Table 2.3.2: Descriptive statistics and Balance Checks

	(1) ES	(2) HS	(3) Tax	(4) HS Tax	(5) p-value
Female	0.72	0.64	0.70	0.68	0.41
Age	42.88	42.77	43.26	42.08	0.76
High education	0.58	0.53	0.57	0.57	0.85
Student	0.10	0.05	0.04	0.06	0.12
Employed	0.71	0.76	0.73	0.77	0.57
Nationality UK	0.94	0.93	0.96	0.91	0.23
Overweight	0.61	0.56	0.62	0.50	**0.04**
Alcohol consumption	3.43	3.40	3.22	3.15	0.80
Smoker	0.20	0.18	0.15	0.18	0.53
Household size	2.75	2.68	2.86	2.82	0.52
Parent	0.61	0.50	0.65	0.61	**0.03**
Subjective SES	5.20	5.18	5.16	5.35	0.60
Income	22.47	22.23	23.51	23.36	0.77
Observations	179	163	244	222	

Note: Columns (1) - (4) show the means across treatment arms. Column (5) displays the p-value associated with the F test of joint orthogonality across treatment arms. ES defines the control group exposed to easy scenarios and no tax. HS defines the treatment group exposed to the hard financial scenarios. Tax defines the treatment group exposed to the 10% or 20% tax. Subjective SES is measured on a scale (ladder) from 1 to 10, with 10 being represented by the the people who are better off (in terms of education, money and jobs) in the UK. Income is computed by dividing total yearly household income by the square root of the household size and is expressed in thousand pounds. Alcohol consumption is measured in units of alcohol per week (1 unit of alcohol = 1 small glass of wine; half pint of beer; pub measure of spirits). The higher number of observations in the Tax conditions is due to our sampling strategy (40% No Tax, 60%) which allows more power to detect difference between the two tax level conditions.

good is roughly twice as large for males than for female. Treatment effects are also stronger for males, thereby we could expect even larger treatment effects in a representative sample. We also acknowledge that the profile of the participant in online experiment might exclude certain relevant categories of people. Even though our experiment was mobile friendly, registering on the platform requires some level of proficiency with mobile and internet use, and having a bank account.

The main outcome variable we will use throughout the paper is total expenditure on temptation using baseline (no tax) prices. The distribution of the variable is strongly censored at 0, with about 37% of participants purchasing no temptation. Only 3%, spent all the budget on temptation. Pooling together all conditions, participants spent 73% of their budget on necessities but there is substantial variation. Across the subcategories of temptation goods, unhealthy food products and alcohol had the higher demands with mean expenditures at baseline price of £3.4 and £2.2 respectively. Tobacco and luxury items were demanded only by 2.35% and

3.74% of participants. We check to what extent income is associated with higher consumption of temptation using (i) self-reported behaviors and (ii) behavior in the experimental market. Panel A in Table A-5 in Appendix 2.A presents the correlation between income and self-reported consumption of temptation. Since we do not have information on consumption of unhealthy foods, we use weight as a proxy. We find that income predicts lower probability of being overweight or a smoker, but higher weekly alcohol consumption. Panel B in Table A-5 presents the correlation between income and expenditure in the task by subcategories of temptation goods, controlling for treatment assignment. Despite the fact that income predicts lower probability of being overweight, it does not predict higher demand for unhealthy foods in the task. On the other had, income does predict higher demand for alcohol and lower demand for tobacco, consistent with the correlations with self-reported behaviors presented above.

2.4 Results

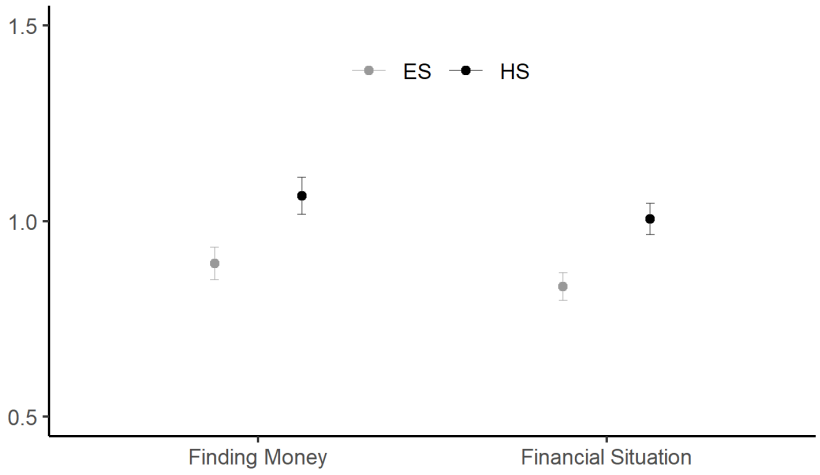
This section begins with manipulation checks in subsection 2.4.1. Subsection 2.4.2 presents the main results while subsection 2.4.3 investigates potential mechanisms explaining the main effects.

2.4.1 Manipulation Check

We begin by examining if being asked to reflect on difficult financial scenarios increases the salience of FW. In the baseline condition, 34% of participants report not being worried at all about their financial situation. 14.5% are very or desperately worried, the rest being somewhat worried. Similar proportions are observed for worries about not being able to find money in case of need. Among treated participants, the distribution shifts to the right. 21% report being worried or desperately worried and only 26% not being worried at all. Figure 2.4.1 summarizes these finding, by plotting means with standard errors for the two variable, treating them as continuous with 0 being "not worried at all", 1 "somewhat worried", 2 "very worried" and 3 "desperately worried". The differences between treated participants and control participants are large, similar in magnitude for both variables and highly statistically significant. Table A-3 in Appendix 2.A shows regression results of the two variables and a standardized index of them on treatment assignment, including covariates. The treatment leads to 0.25 standard deviations higher index of FW. Such an increase in FW is equivalent to having a lower total yearly household incomes by 17 thousand pounds.

Several of the covariates included have high explanatory power. Females, younger participants, immigrants, smokers, lower SES and lower income participants report significantly higher FW. Other variables associated with higher FW which are only marginally insignificant are be-

Figure 2.4.1: Manipulation checks: treatment effects on worries about financial situations and finding money in case of need by financial worries condition



Notes: The outcome variables in the figures are worries about finding money in case of need (left): "How worried do you feel about not being able to find money in case you really need it?" and worries about the financial situations (right): "How worried do you feel about your financial situation?" and . Both variables are coded as: 0 "not worried as all", 1 "somewhat worried", 2 "very worried" and 3 "desperately worried". The dots indicate the means while the brackets standard errors. ES indicate participants assigned to the easy scenarios, while HS indicate participants assigned to the hard scenarios.

ing overweight and being a parent. These results suggest a potential relationship between FW and variables indicating higher consumption of temptation goods. Drinking, however, is not associated with higher FW.

Next, we check if treatment effects vary by income. Previous research has shown that inducing FW impacts behavior only among lower income people (Mani et al., 2013, see chapter 1). Given the large sample size for a laboratory experiment and the fact that our sample has a higher income than the UK mean, we split our sample into three income groups - low, medium and high[6]. The average yearly household income is 16.6 thousand pounds for the low income group (close to the UK relative and absolute poverty line), 33 thousand pounds for the medium income group (close to the UK mean) and 60 thousand pounds for the high income group.

Figure A-2 in Appendix 2.A plots means and standard errors of the worries index by prime treatment status separately for each income group. Looking at the reference group (ES), there are stark differences, larger than 0.5 standard deviations, between the low and other income

[6]This deviates from the median split strategy specified in the pre-analysis plan. Based on previous studies performed on Prolific, we expected participants to have lower incomes that what we obtained in the sample. Since the focus of this research is household living in poor condition or at risk of falling into poverty, a median split for our sample would include a large number of households falling outside these categories.

groups [7]. While statistically significant, the differences between the medium and the high income group are smaller in magnitude, despite the fact that gap in average incomes between these group is much higher. This suggests that FW are particularly salient at low income levels and reduce at increasing rates at higher income levels. Even though the effect of the treatment is largest among the low income group, it is not statistically significant from the other two groups as evidenced in Table A-4 in Appendix 2.A.

2.4.2 Main Results

We begin the analysis by estimating the change in demand as a result of price increases independently of the ES-HS condition using the following specification:

$$Y_i = \alpha + Tax_i'\beta + X_i'\gamma + \epsilon_i \tag{2.1}$$

where Y_i is expenditure on temptation at baseline prices (or demand for temptation) and Tax_i is a vector of tax levels (0%, 10% and 20%). X_i is a vector of individual and household characteristics[8].

Next, we introduce the ES-HS condition in the model and allow it to interact it with the tax using the following specification:

$$Y_i = \alpha + \gamma HS_i + Tax_i'\beta + HS \times Tax_i'\delta + X_i'\gamma + \epsilon_i \tag{2.2}$$

where $HS = 1$ if assigned to the HS condition. $\widehat{\gamma}$ indicates the estimated effect of increasing FW in the no tax condition, the vector $\widehat{\beta}$ gives the effect of the taxes in the ES condition while $\widehat{\delta}$ indicates if the effect of the tax varies by ES-HS condition.

All models are estimated through OLS and use robust standard errors. Given the censored distribution of the outcome variable at 0, for robustness, we report also Tobit models estimates in Table A-7 in Appendix 2.A. Results are consistent across models.

Table 2.4.1 reports the results of the specification in Equation 2.1 in columns (1) to (4),

[7]It should be noted however than comparisons between income groups within the no prime (easy scenario) group may not reflect differences in financial worries which we would observe if no scenarios were administered. Qualitative and quantitative evidence from the answers of participants to scenarios' items suggest that the easy scenarios may have induced financial worries to lower income participants. Lower income participants are significantly more likely to report that even these hypothetical scenarios may significantly impact their lives. As a result, all comparisons between income groups within the easy scenario group may suffer from this issue.

[8]Covariates include: gender, age, education (1 if attended university), student status (1 if currently studying), employment status (1 if employed full or part time), whether the participant is overweight, medium-heavy drinker and smoker (all self-reported), parental status, household size, subjective socio-economic status (1 to 10 scale) and income per adult equivalent (income divided by the square root of household size).

Table 2.4.1: Treatment effect on demand for temptation

	(1)	(2)	(3) ES	(4) HS	(5)
Tax 10%	-1.21*	-1.14*	-1.91**	-0.34	-1.88**
	(0.66)	(0.65)	(0.94)	(0.91)	(0.93)
Tax 20%	-2.40***	-2.40***	-3.95***	-0.97	-3.85***
	(0.67)	(0.68)	(0.95)	(1.02)	(0.93)
HS					-2.12**
					(0.94)
HS × Tax 10%					1.57
					(1.29)
HS × Tax 20%					3.08**
					(1.37)
Control Mean	7.33	7.33	8.23	6.34	8.23
Controls	No	Yes	Yes	Yes	Yes
Adj. R^2	0.01	0.04	0.07	0.02	0.05
Observations	808	808	423	385	808

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. Covariates include individual and household characteristics. The sample used in the analysis either contains all observations (all columns with the title under the number), only participants exposed to easy scenarios (column (2)) or only participants exposed to hard scenarios (column (3)). The control means in column (1)-(4) are the average expenditures in the groups not exposed to a price increase. The control mean in column (5) reports the average expenditure in the group exposed to easy scenarios and no price increase. ES indicate participants assigned to the easy scenarios, while HS indicate participants assigned to the hard scenarios.

in column (1) without covariates, and including covariates in column (2). Columns (3) and (4) estimate the model separately for participants assigned to the ES and HS, respectively. Including covariates does not significantly alter the estimates, expected since randomization was successful. Being assigned to the 10% tax level decreases demand by £1.1 (14.4%), whereas the 20% tax decreases demand by roughly double, £2.4 (32.7%). Results indicate an elastic response to the increase in price.

The subgroup analysis by ES-HS condition, in Table 2.4.1, columns (3) and (4), shows that the results discussed above are mainly driven by the group assigned to the ES. Among

them, demand drops by 23% for the 10% tax group and 48% for the 20% tax group, implying demand elasticities greater than 2. In contrast, among the HS group, no estimate is statistically significant. The 10% tax leads to a drop in demand by 5% and by 15% for the 20% tax group, both inelastic. However, note that the control group mean in column (3) (ES no tax) is higher than the control group mean in column (4) (HS no tax). We explore this difference bellow.

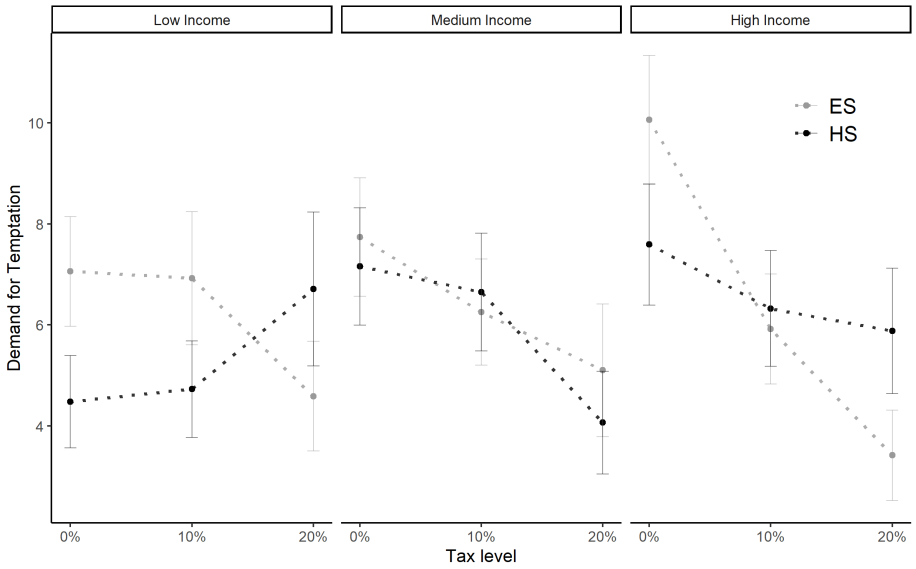
Table 2.4.1, column (5), reports the estimates of the specification in Equation 2.2. First, we observe that increasing FW leads to a large and significant drop of £2.1 (26%) in demand for temptation when no tax is added. The effect is equivalent to increasing price by 10% in the ES condition and suggests that FW may limit the over-consumption of temptation. However, the coefficients on the interaction terms indicate that any potential policy recommendations are far more nuanced. Their direction and magnitude suggest that increasing FW greatly attenuated the elasticities of demand with respect to price. The estimates on the interactions terms offset almost completely the effect of the tax, at each level. In terms of quantity demand at each tax by ES-HS condition, both ES and HS groups have roughly equal values at 10% tax level, while at the 20% level, the HS group actually demands more temptation than the ES group, albeit the difference is not statistically significant (see Figure A-1 in Appendix 2.A for a graphical representation of results in Table 2.4.1).

We now turn to the question of whether treatment affects vary by income. In subsection 2.4.1, we have shown that the psychological manipulation increased FW for all income groups by roughly the same level. However, it is unlikely that the effect of FW on behavior is linear. A FW "shock" for someone already experiencing a lot of FW will probably have a different impact that an equivalent shock for someone with little FW. Previous research by Mani et al. (2013) find effects only on the behavior of low income participants. In these lines, we test if the effects described previously vary by income tertile[9].

Figure 2.4.2 plots demand curves with standard errors by ES-HS condition. First, we note that the behavior of the low income group stands out. At baseline prices, increasing FW leads to a large drop in demand. When taxes are introduced and FW are less salient (ES condition), demand decreases only at 20% tax ($p - value = 0.043$). In contrast, when FW are top of mind, demand becomes slightly upward sloping in price, steepest at the highest tax. However It must be noted that we are not powered enough to detect such small effects ($p - value = 0.31$ for the

[9]We prefer to use income groups rather than the continuous measure of income because: (i) income is likely to be measured with error and (ii) other things besides income (such as assets, credit access, social capital) are likely to be very important in determining ones self perceived economic vulnerability and financial worries. In addition, given the U shape of the results, including income linearly would not suffice to capture the observed pattern by income group.

Figure 2.4.2: Demand for temptation goods at different tax levels by financial worries condition and income group



Notes: The outcome variable is expenditure on temptation at baseline prices. Dots indicate means by group at each level of the tax, while the brackets indicate standard errors. The labels on the horizontal axis represent indicators of income tertiles. Income is computed by dividing total yearly household income by the square root of the household size. ES indicate participants assigned to the easy scenarios, while HS indicate participants assigned to the hard scenarios

20% Tax estimate). In line with the results in chapter 1, we find that demand does not decrease when relative prices increase, suggesting that policies aiming to (dis)incentivize consumption of certain types of goods may not have the intended results when FW are top of mind. For the middle income group, the differences between the demand curves across the two ES-HS conditions are small and statistically insignificant. Finally, turning to the high income group, we observe some differences by ES-HS condition. Similar to the low income group, at baseline prices, demand is lower for the HS group. In addition, demand elasticities are also lower for this group. At 20% tax, demand becomes statistically higher in the HS condition. The graphical evidence is supported by regression estimates using the specification in Equation 2.2. Results are reported in Table A-6 in Appendix 2.A.

Averaging across the ES-HS conditions, the elasticities of demand with respect to price are increasing with income (see Table A-6, columns (1) to (3)). In addition, we observe the same pattern at the baseline (ES) condition, suggesting that for this sample "sin taxes" show signs of being regressive. Increased FW lead to the largest drop in demand for the low income group, but only when no additional tax is introduced. When coupled with an increase in tax, they appear to harm low income participants the most.

Note than one of the main limitation of the task, discussed in a previous section, is that

participants could just substitute taxed temptation goods in the experimental market with the same goods at lower price outside the experiment. This observation makes even more striking the fact that across both ES-HS conditions, the lower income group is insensitive to price increases.

2.4.3 Mechanisms

Up to this point, we interpreted the results in light of the mental bandwidth (scarcity) theory. Increasing FW shifts attention towards necessities at the cost of failing to fully notice the increase in prices. In this subsection, we explore further channels, motivated by previous results from the literature, and asses which are able to explain a higher share of main results: (i) increased FW lowers demand for temptation and (ii) increased FW reduces elasticities of demand with respect to price.

The mediator variables considered are: temporal discounting, risk attitudes, life satisfaction, cognitive reflection, and a necessity index. We motivate each variable individually and explain how they were measured.

Temporal discounting. Consumption of temptation is generally modelled in a dynamic framework (Becker and Murphy, 1988; Gruber, 2001; Gruber and Kőszegi, 2004; O'Donoghue and Rabin, 2003, 2006). With time consistent agents, ones discount rate will influence its consumption decision today. With time inconsistent agents, besides the discount rate, ones degree of present bias and sophistication will also weight in. As a measure of time preferences, we ask participants what would be the minimum amount of money they would prefer to receive today instead of receiving £200 in 2 months. The task has its limitations since it was not incentivized and does not allow to distinguish between discount rates and present bias.

Risk attitudes. Risk preferences are not usually included in models of addiction, however they are likely to play a role since the discounted negative effects vary by individual and are uncertain. Indeed, several studies, including this one, find a strong association between risk attitudes and consumption of temptation even though causality was not established (Anderson and Mellor, 2008; Dave and Saffer, 2008). We measure self-reported risk attitudes using an item from SOEP (Wagner et al., 2007). Participants are asked to reflect, on a scale from 1 to 10, in general, how willing they are to take risks, with 10 indicating the highest willingness.

Life satisfaction. In a review, Haushofer and Fehr (2014) propose affective states as the causal channel through which poverty can impact decision making among the poor. Given that our financial scenarios may have induced negative affect, we measure participant's life satisfaction by asking how satisfied they are with their lives on a 1-10 scale (Bjørnskov, 2010).

Cognitive Reflection. In Mani et al. (2013), asking participants to go through hard financial scenarios reduced both fluid intelligence and inhibitory control. We measure cognitive reflec-

tion, which relates to both fluid intelligence and inhibitory control, and is generally used as an indicator of System 1 - System 2 thinking. Schilbach et al. (2016) argue that when mentally taxed, people are less likely to use the reflective, System 2 thinking. We measure cognitive reflection using 3 items from the CRT-2 in Thomson and Oppenheimer (2016) which has the advantage of not requiring numeracy skills. A higher score indicates higher use of System 2 reflective thinking.

Focus on pressing needs. Finally, we compute a proxy for our proposed mediator - shift in attention towards pressing needs - by asking participants to state on a 4 item Likert scale if in the experimental market they chose goods which gives them pleasure or instead they chose goods which are necessary in the household. We reverse code the first item and compute an index of the two.

We acknowledge that at least some of the proposed mediators are likely to be measured with error. Due to time and budget constraints, none of the items are incentivized and some rely on simple measures. In addition, the mediation analysis rests on the hypothesis that treatment effects lasted throughout the experiment. There is the possibility that this is not true, at least for some of the participants. For instance, treatment effects may have lasted longer for lower income participants than for higher income participants. This increases the risk of failing to reject the null when it is actually false.

Table 2.4.2 reports treatment effects on the proposed variables separately by income tertiles. Only among the lower income group we observe any statistically significant coefficients. In particular, we find that increasing FW lowers the cognitive reflection score (significant at 10%) and increases the index of necessity (significant at 5%). The first effect could be a sign of increased System 1 thinking but it could also be just a replication of the finding of decreased cognitive functions in Mani et al. (2013). Higher activation of System 1 thinking could explain why increased FW lowers demand for temptation if the more automatic response of the low income group is to focus on necessities. On the other hand, lowered cognitive functions could explain why low income participants ignore price increases when FW are salient.

Having established that priming had an effect on cognitive reflection and the index of necessity only among the low income group, we proceed by assessing to what extent they can explain the main treatment effects by income subgroups. To asses the share of the main effects explained by the proposed mechanisms, we follow Heller et al. (2017) and compute the product of: (i) the treatment effect on the mediator (γ_m in Equation 2.3) and (ii) the coefficient on the mediator from a non-experimental regression on demand for temptation for the control group, controlling for all the covariates and for the tax levels (α_c in Equation 2.4) and divide by the (iii)

Table 2.4.2: Treatment effects on mediating variables by income group

	(1) Cognitive Reflection	(2) Necessity Index	(3) Life Satisfaction (1-10)	(4) Risk (1-10)	(5) Time Preferences
All	-0.11	0.12	-0.098	-0.0054	-3.54
	(0.072)	(0.071)	(0.12)	(0.15)	(4.71)
Control mean	2.09	-0.10	6.58	4.59	124.54
Lower Income	-0.22*	0.25**	-0.12	-0.043	-7.87
	(0.13)	(0.13)	(0.24)	(0.29)	(8.66)
Control mean	2.19	0.10	5.61	4.64	107.82
Middle Income	0.041	0.15	-0.36	-0.17	-10.5
	(0.13)	(0.12)	(0.23)	(0.25)	(8.33)
Control mean	1.89	0.06	6.59	4.35	126.69
Higher Income	0.00058	-0.031	0.100	0.087	11.1
	(0.13)	(0.13)	(0.19)	(0.26)	(8.14)
Control mean	1.98	-0.06	6.96	4.77	129.49

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. All models control for individual and household characteristics. Outcome variables are defined in column titles. Presented are coefficients and standard errors of the FW (financial worries) treatment status variable. Each panel presents estimates using different samples: whole sample (All), participants in the 1st income tertile (Low income group), 2nd income tertile (Middle income) or 3rd tertile (Higher Income). Outcome variables in columns (1) and (3) are standardized using the pure control group mean and standard deviation. Control means indicate the average of the outcome variable in each sample for participants exposed to easy scenarios.

size of the main treatment effect of increased FW (γ in Equation 2.2). In other words, we are weighting the treatment effect on the mediator with its correlation with the outcome variable, and divide by size of the main treatment effect - $(\gamma_m \times \alpha_c)/\gamma$. Bias-corrected confidence intervals are bootstrapped using 2000 repetitions. We then repeat the same steps to explain the share of the interaction term (δ in Equation 1.2) explained by these variables. For simplicity we treat the tax condition as continuous for this analysis.

$$M_i = \alpha_m + \gamma_m HS_i + \beta_m Tax_i + \delta_m HS_i \times Tax_i + \gamma_m X_i + \epsilon_i \tag{2.3}$$

$$Y_i = \alpha_c M_i + \beta_c Tax_i + \gamma_c X_i + \epsilon_i, \quad if \quad HS_i = 0 \tag{2.4}$$

Table 2.4.3: Share of main treatment effects explained by the candidate mediators

	(1) Cognitive Reflection	(2) Necessity Index
Share of the main effect	-0.02 [-0.49 0.20]	0.50 [0.13 1.86]
Share of the interaction term	-0.01 [-0.89 0.11]	0.38 [0.21 1.12]
Observations	271	271

Note: Bias-corrected confidence intervals in parentheses. The estimates are obtained by multiplying (i) the estimated treatment effect of being exposed to hard scenarios on the candidate mediating variable in the column header with (ii) the coefficient on the mechanisms from a regression on the main outome variable (expenditure on temptation) using only data from the control group and including covariates, and dividing by (iii) the main treatment effect of being exposed to hard scenarios on expenditure on temptation. The sample used in the analysis is the subsample of participants with incomes in the first income tertile (low income group).

Results are reported in Table 2.4.3[10]. The only variable which explains a large share of the main effect (50%) and the interaction effect (38%) is the index of necessity. Indeed, if we run the main specification including it as endogenous control, the estimates on the HS treatment and on the interaction term drop substantially towards zero. The cognitive reflection score does not seem to be a relevant mediator. This is due to the fact that the two variables have low predictive power of the outcome variable. To conclude, these findings suggest that main results for the low income group are driven to a large extent by a shift in focus towards pressing needs. This shift in focus however comes at the cost of failing to respond to the price increase. We do not find any evidence for why increased FW had an effect also on participants in the highest income tertile. Given our data, this remains a puzzle.

2.5 Conclusions

In an online experiment with UK participants, we investigate if inducing financial worries impacts purchasing decisions across two categories of goods: necessities and temptation. Additionally, we randomly increase the price of temptation to try to capture if financial worries might affect how people respond to "sin" taxes. In the absence of any price increase, financial

[10]Results for all variables by income groups are reported in Table A-8 in Appendix 2.A

worries appear to protect against the over-consumption of temptation, reducing its demand by the equivalent of a 10% price increase. In contrast, when the price of temptation increases, financial worries reduce the elasticity of demand with respect to price, suggesting that the protective effect comes at the cost of not fully noticing other relevant information.

Estimating the two effects by income tertile, we find both to be stronger among lower income participants. Among them, increasing financial worries actually leads to higher demand for temptation as its price increases, even though the upward slope is not signifficant. No effect is found for the middle income group. In contrast, among higher income participants increasing financial worries appears to lower demand for temptation while also decreasing their elasticity of demand with respect to price. We investigate several potential mechanisms and only find suggestive evidence for the lower income group. Consistent with our hypotheses, increasing financial worries appears to shift focus towards necessities indicated by the fact that treatment low income participants are more likely to report having chosen goods which are necessary for the household. The mediator captures a significant share of both effects.

In terms of policy implications, increasing sin taxes may hurt the poor the most if they are experiencing high financial worries. Absent of any tax increase, financial worries reduce demand for temptation, thus potentially protecting the poor from over-consuming such goods. But if additional sin taxes are introduced, the poor may not decrease their demand further. While we can only speculate, high financial worries may bring increased feelings of economic vulnerability and stress which are likely to produce disutility. Consumption of temptation goods may be a way to compensate such disutility, especially since lower income people can afford fewer substitutes (holidays, social and cultural events etc.). Introducing higher taxes on temptation makes them even harder to afford which may increase their desirability since they are even scarcer than before.

The study has its limitations. First of all, we do not know if the finding that poor fail to fully notice taxes when worried is short-lived or not. If the effect is only temporary and the poor end up adjusting their demand, then financial worries may protect them from over-consuming temptation. This is a relevant question for future research. Secondly, none of the proposed channels could explain why we observed treatment effects also among the richest. Lastly, in our experimental setting we taxed all temptation goods which is rarely the case in reality. Thus, we can not generalize our results to situations when only some types of temptation goods are taxed, allowing people to substitute them with other un-taxed goods. Lastly, caution is needed when discussing policy recommendations. Even though we observe that financial worries impact behavior, in the absence of estimates of marginal utilities of consumption, we are unable to determine whether the effects are optimal or not.

Appendix

2.A Appendix A - Additional Results and Robustness Checks

Table A-1: Attrition by Treatment Status

	(1)	(2)	(3)	(4)
HS	0.015			-0.012
	(0.015)			(0.021)
Tax		0.0070		
		(0.014)		
Tax 10%			0.0070	-0.0047
			(0.017)	(0.023)
Tax 20%			0.0069	-0.030
			(0.018)	(0.020
HS Tax 10%				0.0077
				(0.026)
HS Tax 20%				0.032
				(0.030)
Constant	0.039***	0.042***	0.042***	0.048***
	(0.0092)	(0.011)	(0.011)	(0.016)
Adj. R^2	0.00	-0.00	-0.00	0.00
Observations	847	847	847	847

Note: Results obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. The outcome variable equals 1 if the participant left the study from the first scenario onwards and 0 if the participant completed the survey. HS indicate participants assigned to the hard scenarios. HS Tax 10%/20% indicate treatment groups assigned to both hard scenarios and tax, and are not defined as interaction terms.

Table A-2: Balance checks across tax-level treatment groups

	(1) Tax 0%	(2) Tax 10%	(3) Tax 20%	(4) p-value
Female	0.68	0.70	0.68	0.88
Age	42.83	42.17	43.31	0.58
High education	0.56	0.55	0.59	0.55
Student	0.07	0.06	0.03	0.15
Employed	0.73	0.78	0.71	0.27
Nationality UK	0.94	0.92	0.95	0.45
Overweight	0.58	0.57	0.55	0.77
Alcohol consumption	3.42	3.13	3.25	0.58
Drinks moderate/high	0.28	0.24	0.24	0.38
Smoker	0.19	0.16	0.17	0.58
Household size	2.72	2.76	2.95	0.11
Parent	0.56	0.61	0.66	0.08
Subjective SES	5.19	5.25	5.25	0.88
Income	22.36	23.86	22.94	0.46
Observations	342	252	214	

Note: Columns (1) - (3) show the means across treatment arms. Column (4) displays the p-value associated with the the F test of joint orthogonality across treatment arms. Subjective SES is measured on a scale (ladder) from 1 to 10, with 10 being represented by the the people who are better off (in terms of education, money and jobs) in the UK. Income is computed by dividing total yearly household income by the square root of the household size and is expressed in thousand pounds. Alcohol consumption is measured in units of alcohol per week (1 unit of alcohol = 1 small glass of wine; half pint of beer; pub measure of spirits).

Table A-3: Manipulation check - treatment effects on financial worries

	(1) Financial situation	(2) Finding money	(3) Index
HS	0.19***	0.20***	0.25***
	(0.049)	(0.055)	(0.062)
Female	0.11*	0.14**	0.16**
	(0.055)	(0.061)	(0.069)
Age	-0.0077***	-0.013***	-0.013***
	(0.0026)	(0.0029)	(0.0033)
High education	0.075	0.053	0.083
	(0.054)	(0.063)	(0.070)
Student	0.019	-0.0049	0.0095
	(0.11)	(0.12)	(0.14)
Employed	-0.051	-0.016	-0.044
	(0.062)	(0.073)	(0.080)
Nationality UK	-0.22**	-0.26**	-0.31**
	(0.11)	(0.12)	(0.13)
Overweight	0.040	0.11*	0.094
	(0.051)	(0.057)	(0.064)
Smoker	0.14**	0.21***	0.23***
	(0.066)	(0.079)	(0.087)
Medium/high drinker	-0.024	-0.055	-0.049
	(0.054)	(0.061)	(0.068)
Parent	0.0041	0.20***	0.12
	(0.063)	(0.069)	(0.078)
Household size	0.0016	0.0053	0.0043
	(0.022)	(0.025)	(0.028)
Subjective SES	-0.12***	-0.13***	-0.16***
	(0.019)	(0.021)	(0.025)
Income	-0.013***	-0.018***	-0.020***
	(0.0026)	(0.0025)	(0.0031)
Adj. R^2	0.21	0.26	0.26
Observations	808	808	808

Note: All estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. The outcome variables are worries about the financial situations: *How worried do you feel about your financial situation?* and worries about finding money in case of need: How worried do you feel about not being able to find money in case you really need it?. Both variables are coded as: 0 not worried as all, 1 somewhat worried, 2 very worried and 3 desperately worried. The index variable in the last column is computed through the inverse covariance weigthing procedure in Anderson (2008) and standardized using the control group mean and standard deviation. Subjective SES is measured on a scale (ladder) from 1 to 10, with 10 being represented by the the people who are better off (in terms of education, money and jobs) in the UK. Income is computed by dividing total yearly household income by the square root of the household size and is expressed in thousand pounds.

Table A-4: Manipulation check - treatment effects on financial worries - by income tertiles

	(1) Financial situation	(2) Finding money	(3) Index
HS	0.22**	0.27**	0.31**
	(0.096)	(0.11)	(0.12)
Middle tertile	-0.42***	-0.47***	-0.57***
	(0.086)	(0.099)	(0.11)
Upper tertile	-0.50***	-0.56***	-0.68***
	(0.11)	(0.12)	(0.13)
HS × Middle tertile	0.00073	-0.055	-0.033
	(0.12)	(0.14)	(0.16)
HS × Upper tertile	-0.091	-0.17	-0.16
	(0.12)	(0.13)	(0.15)
Controls	Yes	Yes	Yes
Adj. R^2	0.245	0.296	0.302
Observations	808	808	808

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. The dependent variables are worries about the financial situations: *How worried do you feel about your financial situation?* and worries about finding money in case of need: How worried do you feel about not being able to find money in case you really need it?. Both variables are coded as: 0 not worried as all, 1 somewhat worried, 2 very worried and 3 desperately worried. The index variable in the last column is computed through the inverse covariance weigthing procedure in Anderson (2008) and standardized using the control group mean and standard deviation. All models include individual and household characteristics. HS indicates being assigned to the hard scenarios. Reference category is the lower income tertile (low income group). Income is computed by dividing total yearly household income by the square root of the household size and is expressed in thousand pounds.

Table A-5: Correlation between income and sin behaviors

Panel A: Self-reported behaviors	(1) Overweight	(2) Alcohol consumption	(3) Smoker
Income	-0.0021*	0.024***	-0.0037***
	(0.0012)	(0.0085)	(0.00082)
Panel B: Expenditure in the task	**(1) Unhealthy foods**	**(2) Alcohol**	**(3) Tobacco**
Income	0.0012	0.029**	-0.0085***
	(0.012)	(0.013)	(0.0031)

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. Outcome variables are listed in column headers. Outcome in Panel A are computed using survey items in Prolific's database. Alcohol consumption is expressed in units of alcohol per week. Outcomes in Panel B are total expenditures in the experimental market for each category of goods expressed at baseline prices. Panel B regressions include variables indicating treatment assignment as covariates. Income is computed by dividing total yearly household income by the square root of the household size and is expressed in thousand pounds

Table A-6: Treatment effects of prime and tax by income quartile

	(1) Lower	(2) Middle	(3) Higher	(4) Lower	(5) Middle	(6) Higher
Tax 10%	0.15	-0.55	-3.02**	0.17	-0.86	-3.88**
	(1.08)	(1.16)	(1.22)	(1.76)	(1.61)	(1.70)
Tax 20%	-0.83	-2.84**	-3.81***	-3.23*	-2.38	-6.39***
	(1.21)	(1.22)	(1.23)	(1.68)	(1.79)	(1.59)
HS				-3.13**	-0.44	-1.98
				(1.55)	(1.64)	(1.84)
HS × Tax 10%				0.39	0.63	1.66
				(2.33)	(2.35)	(2.44)
HS × Tax 20%				5.36**	-0.94	5.05**
				(2.50)	(2.35)	(2.45)
Control Mean	5.85	7.45	8.88	7.06	7.74	10.06
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.01	0.04	0.07	0.04	0.03	0.07
Observations	271	268	269	271	268	269

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. The outcome variable in all models is expenditure on temptation at baseline prices. The samples used in the analysis are participants in: the lower income tertile (columns (1) and (4)), the middle income tertile (columns (2) and (5)) and the upper income tertile (columns (3) and (6)). All models control for individual and household characteristics. Income is computed by dividing total yearly household income by the square root of the household size.

Table A-7: Tobit regressions of treatment effects on demand for temptation

	(1) All	(2) 1st Tertile	(3) 2nd Terile	(4) 3rd Tertile
HS	-2.98**	-5.08**	-0.11	-2.94
	(1.32)	(2.29)	(2.19)	(2.45)
Tax 10%	-2.23*	1.03	-0.49	-5.18**
	(1.32)	(2.37)	(2.28)	(2.32)
Tax 20%	-6.17***	-5.37**	-3.82	-9.68***
	(1.48)	(2.57)	(2.73)	(2.44)
HS × Tax 10%	1.61	0.62	-0.79	1.89
	(1.90)	(3.40)	(3.30)	(3.46)
HS × Tax 20%	4.86**	8.72**	-1.67	7.82**
	(2.11)	(3.74)	(3.60)	(3.54)
Control Mean	8.23	7.06	7.74	10.06
Controls	Yes	Yes	Yes	Yes
Observations	808	271	268	269

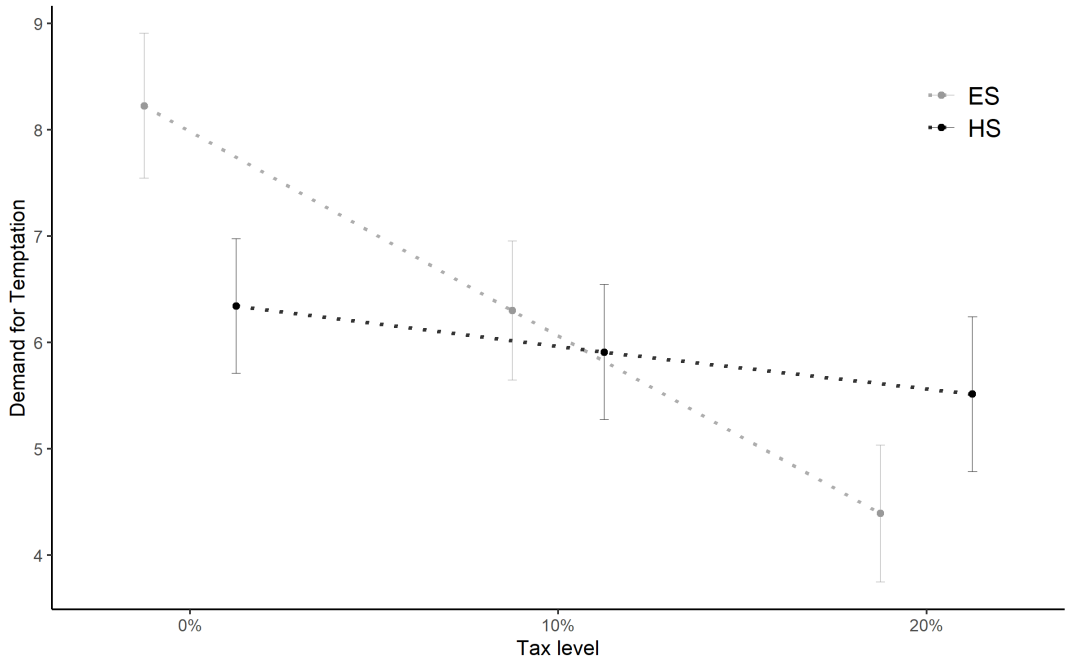
Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. All models control for individual and household characteristics. The outcome variable in all models is expenditure on temptation at baseline prices. The samples used in the analysis are: whole sample (column (1)), the lower income tertile (columns (2)), the middle income tertile (columns (3)) and the upper income tertile (columns (4))

Table A-8: Share of main treatment effects explained by the candidate mediators by income groups

	(1) Cognitive Reflection	(2) Necessity Index	(3) Life Satisfaction	(4) Risk (1-10)	(5) Time Preferences
Lower Income					
Share of the main effect	-0.02	0.50	-0.04	-0.04	-0.03
	[-0.49 0.20]	[0.13 1.86]	[-0.63 0.06]	[-1.12 0.09]	[-0.68 0.12]
Share of the interaction term	-0.01	0.38	-0.06	-0.09	0.02
	[-0.89 0.11]	[0.21 1.12]	[-1.25 0.09]	[-2.57 0.09]	[-.24 0.61]
Observations	271	271	271	271	271
Middle Income					
Share of the main effect	0.28	5.94	-0.64	0.43	0.63
	[-0.05 265]	[3.93 1070]	[-196 -0.13]	[0.07 389]	[0.07 115]
Share of the interaction term	.04	-1.32	-0.23	-0.49	-0.13
	[-1.14 7.36]	[-212 -0.01]	[-166 0.04]	[-2655 0.02]	[-142 0.14]
Observations	268	268	268	268	268
Higher Income					
Share of the main effect	0.12	0.56	0.1	-0.01	0.02
	[-0.53 6.42]	[-3.12 3.19]	[0.26 7.87]	[-2.42 1]	[-0.43 4.08]
Share of the interaction term	0.11	0.71	0.07	.01	-0.07
	[-0.44 1.73]	[-1.13 1.9]	[-0.19 1.82]	[-0.42 1.14]	[-1.68 0.26]
Observations	269	269	269	269	269

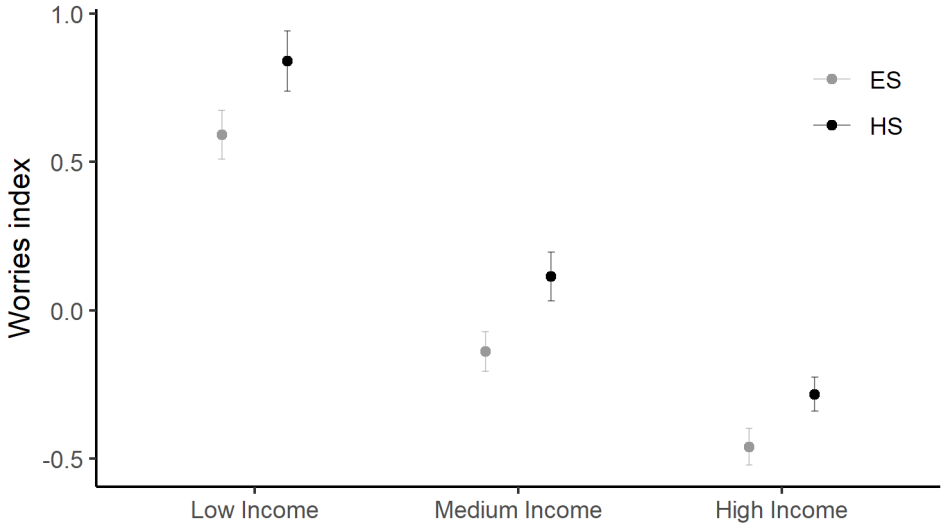
Note: Bias-corrected confidence intervals in parentheses. The estimates are obtained by multiplying (i) the estimated treatment effect of being exposed to hard scenarios on the candidate mediating variable in the column header with (ii) the coefficient on the mechanisms from a regression on the main outome variable (expenditure on temptation) using only data from the control group and including covariates, and dividing by (iii) the main treatment effect of being exposed to hard scenarios on expenditure on temptation.

Figure A-1: Demand for temptation goods at different tax levels by financial worries condition



Notes: The outcome variable is total expenditure on temptation at baseline prices. Dots indicate means by group at each level of the tax, while the brackets indicate standard errors. ES indicate participants assigned to the easy scenarios, while HS indicate participants assigned to the hard scenarios.

Figure A-2: Manipulation checks: treatment effects on worries index for each financial worries condition by income tertile



Notes: The outcome variables is an index of worries computed using the inverse covariance weighting method in Anderson (2008), standardized usig the control group mean and standard deviation. The variable used to compute the index are: (i) worries about financial situation and (ii) worries about not being able to find money in case of need. Both variables are coded as: 0 "not worried as all", 1 "somewhat worried", 2 "very worried" and 3 "desperately worried". The labels on the horizontal axis represent indicators of income tertiles. Income is computed by dividing total yearly household income by the square root of the household size. The dots indicate the means while the brackets standard errors. ES indicate participants assigned to the easy scenarios, while HS indicate participants assigned to the hard scenarios.

2.B Appendix B - Experimental Design

2.B.1 Experimental Task

Financial Scenarios

Instructions - In the following section you will be presented 2 scenarios and asked to answer how you would go about dealing with the situations if they were to happen to you. Please take your time answering the questions. Try to have at least 3 sentences in your open question answers.

1. Imagine that an unforeseen event requires of you an immediate (£2000/£100) expense. You need to raise the money in less than a week.

    - Are there ways in which you may be able to come up with that amount of money on a very short notice? (yes/no)
    - How would you go about getting (£2000/£100) on a very short notice? Three sentences should be enough. (open)
    - To what extent do you agree with the following statements? (4 item Likert: strongly disagree - strongly agree)
        (a) "Coming up with (£2000/£100) on a very short notice would cause me longlasting financial hardship.
        (b) "Coming up with (£2000/£100) on a very short notice would require me to make sacrifices that have long-term consequences.

2. Imagine that the economy is going through difficult times. Your household's monthly expenses increase by (£300/£15) due to higher energy and housing prices.

    - Please indicate to what extent do you agree with the following statement: "Given my situation, I would be able to maintain roughly the same lifestyle under those new circumstances. (4 item Likert: strongly disagree - strongly agree)
    - In what ways would the (£300/£15) increase in your monthly expenses would impact your leisure, housing or travel plans? What changes would you need to make? Three sentences should be enough. (open)
    - To what extent do you agree with the following statement: "The (£300/£15) increase in our monthly expenses would strongly impact our leisure, housing, or travel plans." (4 item Likert: strongly disagree - strongly agree)

Purchasing Task

Instructions: In the following task you have to choose what goods to purchase with a budget of £30 .

You will see a list of available goods, with a picture, title and the price displayed for each of them. The price of the goods is the retail price including the discounts offered by the retailer. If you need additional information on the goods, by clicking on the picture a new window will open with further details from the website of the retailer.

Some of the goods have a higher price than that of the retailer.

By clicking on the **ADD button**, the goods will be added to the shopping cart. You can edit the shopping cart content at any time by clicking on the **Shopping Cart** section in the top-right side of your screen.

A new window will open with the goods already selected. You can modify the quantities of each good or remove them from the shopping cart. You can return to the main window at anytime by clicking on close, or anywhere outside the shopping cart window.

When you are satisfied with your selection, click on **Checkout** in the shopping cart window to proceed to the next page. Try to spend as close to the £30 budget as possible. To proceed to the next page you need to spend a minimum of £28. Any remainder will be added as bonus payment on Prolific.

You can access these instructions at any time by clicking on the **Instructions** section in the top-left side of the page.

1 out of every 100 participants will be selected for payment. If you are selected, the goods will be delivered to a collection location of your choice at a date and time that is convenient for you. You can pick up your goods with the code we will send you.

Figure A-1: Main screen of the purchasing task

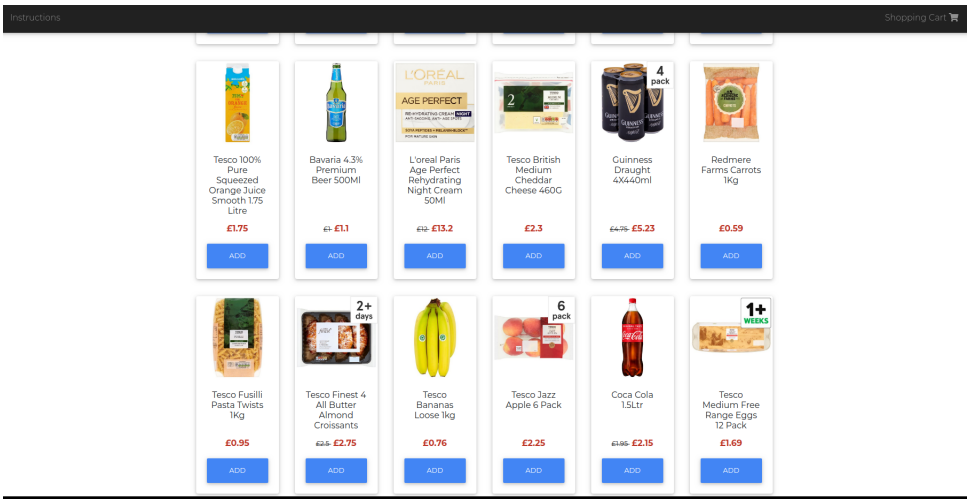
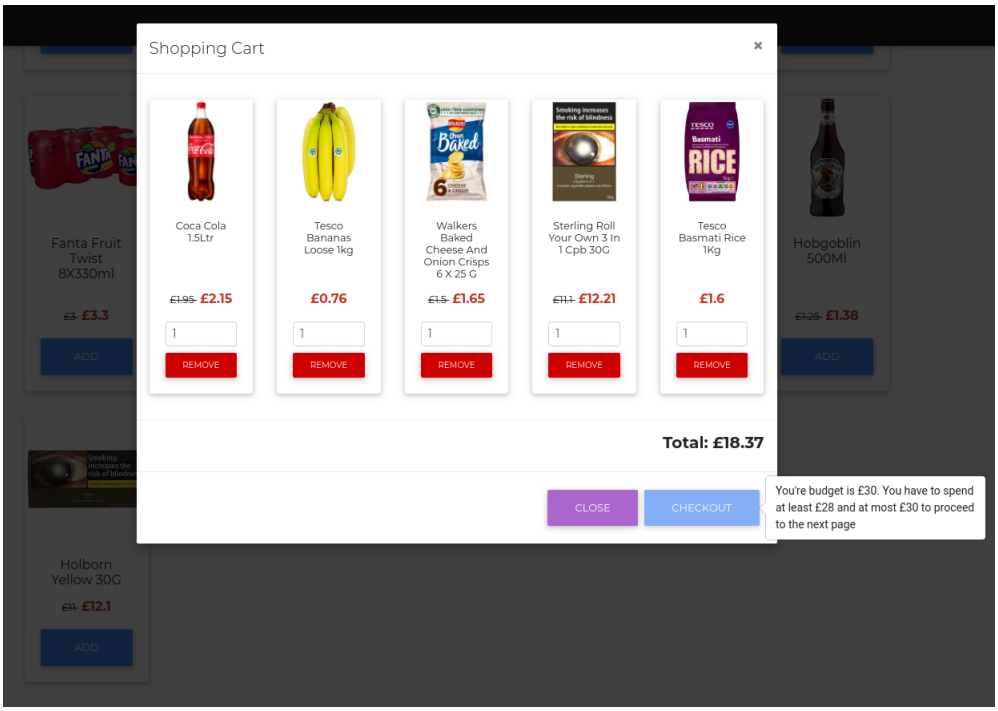


Figure A-2: Checkout screen of the purchasing task



2.B.2 Additional Variables

- **Life Satisfaction**: All things considered, how satisfied are you with your life as a whole these days? On a scale from 1 to 10, where 1 means you are completely dissatisfied and 10 means you are completely satisfied where would you put your satisfaction with life? (Bjørnskov, 2010)
- **Risk**: In general, how willing or unwilling you are to take risks? Please use a scale from 0 to 10, where 0 means completely unwilling to take risks and a 10 means you are very willing to take risks (Wagner et al., 2007)
- **Time Preferences**: What is the smallest amount of money to be received today that you would prefer to receiving £200 in 2 months?
- **Cognitive reflection**: 3 questions from Thomson and Oppenheimer (2016)

Chapter 3

Violence Exposure and Cognitive Function: Evidence from Primary School Students in El Salvador

Following a civil war lasting more than a decade, violence in El Salvador persists at extremely high levels with the rise of gangs controlling and fighting for territory. High and unanticipated fluctuations in violent crimes spread a general feeling of fear and panic among the general population. This paper investigates the impact of recalling violence exposure on cognitive functions among primary school students in El Salvador. 299 students are randomly assigned to recall episodes of violence either before or after taking cognitive tests. Recalling violence exposure prior to taking the cognitive tests, increases performance by 0.23 standard deviations. The effect size is larger with higher reported exposure and cannot be explained by students taking more time in answering the items on the tests. The direction and magnitude of results contrasts previous estimates from the literature and suggests that responses to violence are context specific and might vary with the intensity of the exposure. In a country plagued by violent crimes, the sharpening of the cognitive functions observed in this study could be a coping mechanism in the face of adversity.

JEL Codes: D91, J13, J24, I21, I24, I25, O15

Keywords: violence exposure, cognitive function, child development, organized crime, conflict, human capital[1]

[1]I am very grateful Jacopo Bonan for offering me the opportunity to work on this project. I want to thank him and to Arianna Galliera for all the work on the questionnaire and survey implementation. I would also like to thank Enrique Fatas, Chris Blattman, Marcela Ibañez and all participants at the Workshop on Behavioral Insights in Development and Peace building at the University of Göttingen for their comments and suggestions. I am also very grateful to Ana Conception Castillo (Conchi) and Silvia Rowe for all their support on the field and to my wonderful team of enumerators for all their hard work. Big thanks to Fondazione punto.sud, Soleterre, and all the other organizations involved for making this project a reality.

3.1 Introduction

High levels of violent crimes constitute an endemic issue in many developing countries. More than half of the developing world experienced civil wars since the end of the Second World War (Blattman, 2009) with conflicts often lasting for more than a decade. While in most countries violence subsides post-conflict, in El Salvador, this trend was interrupted by the rise of the gang culture in the beginnings of the 2000s. Ever since, the country has been experiencing massive fluctuations in homicides and kidnapping and has had one of the highest homicide rates in the world.

Violence exposure (VE further on) has been shown to have an impact over a multitude of behaviors, from risk and time preferences (Voors et al., 2012; Callen et al., 2014; Kim and Lee, 2014; Byder et al., 2015; Moya, 2018) to prosociality and political participation (Blattman, 2009; see Bauer et al., 2016, for a review). For a long time, economists considered such preferences as stable, trait-like features and only in recent times new evidence showed how environmental factors can shape them. Similarly, cognitive skills (or cognitive function) have been considered as a fixed inherited trait until the rise of the literature on human capital formation showing the crucial role played by the environment in shaping skills throughout life (Shonkoff and Phillips, 2000; Knudsen, 2004; Knudsen et al., 2006; Heckman, 2006; Almond and Currie, 2011; Walker et al., 2007; Anderson, 2008; Heckman et al., 2013).

Furthermore, a new strand of literature shows that when cognitive function, a limited resource, is allocated towards decisions that appear more urgent, there is less mental space for other, potentially important, but less urgent decisions (Shah et al., 2012; Mani et al., 2013; Mullainathan and Shafir, 2013). Applied to the context of violence and human capital, a child who is preoccupied with how to avoid being the victim of violence, may have less mental space for school tasks. If instead experiencing violence stems from poor performance (e.g. teachers, parents or classmates punishing poor results), more mental space may be directed towards school tasks in an attempt to avoid violence.

This paper investigates if VE impacts cognitive function in children. In a lab-in-the-field experiment, 299 primary school students from the department of Chalatenango in El Salvador, are randomly assigned to respond to an extensive survey section on VE just before (**treated**) or after (**control**) taking standard cognitive tests - Stroop and Raven Matrices. Control group students respond to the same VE section towards the end of the survey which allows to identify if treatment effects vary by self-reported VE. Methodologically, the study design relates to Callen et al. (2014) and Bogliacino et al. (2017), which combine recollection of fearful episodes with administrative or self-reported data on violence incidence.

First, I find that recalling VE before taking cognitive tests, increases performance by 0.23 standard deviations. The effect size is significantly larger for children reporting higher VE, which strengthens the credibility of the estimates. Among children with VE above the median, treatment effects are as large as 0.4 standard deviations. In contrast, no effect is found among children with VE bellow the median. I rule out the possibility that the differential effects are confounded by differences in socioeconomic status, noncognitive skills or home environment correlated with differences in VE.

I show that the findings are not simply driven by treated students spending more time solving the items on the tests. No differences are observed in the time spent solving the Raven Matrices or in the reaction in the Stroop test, which suggests that increased effort may not be a plausible explanation for the observed effects. I then check if being asked to recall VE earlier (treated) or later (control) in the survey, affected the intensity of VE reported. I find treated students to report significantly less VE than the control group students. If this finding reflects imbalances in actual VE between the groups, then internal validity would be severely questioned. However, this is unlikely since the groups are well balanced across a wide ranged of variable. A more credible explanation is that being asked sensitive information early in the survey may have lead to under-reporting. As the interviewed progressed, students may have felt more trusting of the enumerator. Under this hypothesis, the heterogeneous effects by VE are actually a lower bound estimate of the true effect.

The large and positive effect sizes contrast previous estimates from the literature. In a similar design with young adults in Colombia, Bogliacino et al. (2017) find negative effects of recalling VE on short term memory and cognitive control. A key distinction between the two studies, besides the targeted age group, is that Bogliacino et al. (2017) asked participants to recall an episode that triggered fear, thus priming a specific response, while in this study, children only report their VE without being primed a specific emotion. Other studies using quasi-experimental settings find mixed results. Sharkey (2010) and Sharkey et al. (2012) find, among students in Chicago, negative effects on cognitive performance of a homicide taking place in the neighborhood in the past 7 days. Effects are short-lived, dissipating after 7 days. In a developing country setting, Brown and Velásquez (2017) find that the sharp increase in violence following the onset of the Mexican Drug Wars does not have any impact on cognitive performance when looking at variation in homicides in the month before the survey.

This paper contributes to several strands of literature. Firstly, the results speak to the growing literature studying the impact of emotions on decision making (Loewenstein and Lerner, 2003; Lerner et al., 2003, 2015; Callen et al., 2014; Bogliacino et al., 2017). Secondly, it adds to the vast literature measuring the impact of environmental factors on human capital (Heckman,

2006; Almond and Currie, 2011). Most of this literature has neglected the impact that VE can have on human capital accumulation. This is partly due to the fact that most studies use data from the developed world where violence levels are far lower. The scarce existing literature on VE and cognitive performance finds mixed results (Sharkey, 2010; Sharkey et al., 2012; Jarillo et al., 2016; Brown and Velásquez, 2017; Bogliacino et al., 2017). The fact that results further contrast previous estimates raises questions on our understanding of the relationship between VE and cognitive performance. In particular, future research needs to explore if the relationship varies with age and context, and which methodological approach is more appropriate to capture it.

Thirdly, this paper adds to the literature on measuring skills and sensitive information in developing countries (Attanasio et al., 2015; Laajaj and Macours, 2017). I show that changing the order of survey sections can have a large impact on performance on cognitive tests and reporting of VE. Based on the findings of this paper, measuring sensitive topics should be carefully considered when designing surveys, as it can diferentially affect performance on cognitive tests.

Lastly, the design and results of this paper complement the growing literature on the psychology effects of scarcity on decision making. A main finding of this strand of literature is that scarcity of a resource increases focus towards it, often at the expense of other tasks (Mullainathan and Shafir, 2013; Shah et al., 2015, 2012). While studied mostly in the context of poverty, the scarcity theory is supposed to operate in all domains where a given resource is scarce (Mullainathan and Shafir, 2013). Violence is a form of scarcity of security which implies that similar psychological mechanisms may be at work. Among El Salvadorian children, reflecting on ones scarcity of security improves cognitive test scores. While only speculative, I provide the following interpretation. For children, their main mean of escaping present violence (from teachers, parents) and future violence (upward mobility) may be doing well at school tasks. Being asked to recall violence, may have triggered a focus response towards the task at hand, which resembled a regular school test, more so since it was administered at school during class hours. While suggestive, more research is needed to investigate which are the underlying mechanisms driving these results. The fact that no manipulation check was included is a limitation of this study.

This paper is structured as follows: section 3.2 presents previous findings from the literature while section 3.3 describes the context and the rise of violence in El Salvador. Section 3.4 presents the experimental design, data and descriptive statistics while sections 3.5 and 3.6 discusses the main results and robustness checks. Finally, section 3.7 concludes.

3.2 Literature

A growing number of empirical studies have documented the link between VE and economic outcomes (risk and social preferences especially). VE tends to increase risk aversion (the reverse was also observed) (Voors et al., 2012; Callen et al., 2014; Kim and Lee, 2014; Byder et al., 2015; Moya, 2018), whereas a stylized fact is emerging with respect to social preferences: VE fosters pro-social behaviors, at least at in-group level (see Bauer et al. (2016) for a review). In contrast, there is far less evidence on how VE impacts components of human capital such as cognitive function.

Cognitive function (or cognitive skills) has been shown to be more malleable during childhood due to the brain's higher plasticity (Almond and Currie, 2011; Walker et al., 2007; Anderson, 2008; Heckman et al., 2013). Viewed as an opportunity for remedial intervention, this finding also implies that children are particularly vulnerable to environmental shocks and stressors. VE can trigger psychological mechanisms related to fear, trauma or stress which can hinder cognitive performance (Sharkey, 2010). In contrast, there is also evidence suggesting that responses to traumatic events can be much more nuanced and can even lead to post-traumatic growth (Tedeschi and Calhoun, 2004).

Studies from developed countries are predominantly from high violence neighborhoods in Chicago. Sharkey et al. (2012) exploits variation in homicides incidence relative to the timing of interviews, to investigate if living in a geographical region where a homicide took place in the past 7 days, has an effect on executive function, effortful control, attention, impulse control and pre-academic achievement. Negative effects are found on attention and impulse control, less robust on pre-academic achievement, while no effect is found on executive function. Using a similar methodology, Sharkey (2010) finds negative effects on cognitive scores among blacks but not among Hispanics and argues this is due to homicides being more salient among blacks. Effects dissipate with distance in time from the homicide and become insignificant if more than 7 days passed.

In a similar setting and using data from primary sources, McCoy et al. (2015) find that the children from communities that experienced a homicide in the past week have faster response times and only marginally lower performance on cognitive tests. The former effect is driven by children with lower trait anxiety. McCoy et al. (2016) finds that VE is associated with higher selective attention towards negative stimuli, less likely to misjudge fear in other's faces and less likely to display internalizing behaviors (fear, anxiety, withdrawal) in the classroom, as reported by the teacher. This suggests that VE leads to the development of various forms of coping.

In a developing country setting, Bogliacino et al. (2017) use a similar design to the one used

in this study, to investigate if recalling VE impacts performance in short-term memory tasks, Stroop task and Raven matrices. In a first study using participants from Bogota, participants are randomly assigned to recall an episode from the previous year, related to violence, that caused anxiety or fear. Among participants living in districts with homicide rates above the 75th percentile, the treatment marginally lowers short-term memory. In a second study with a sample of internally displaced individuals, the treatment lowers the short term-memory and Stroop score of individuals above the 75th percentile of self-reported violence exposure. While following the same pattern, treatment effects on Raven score are not statistically significant.

In developing countries, researchers generally exploit sharp increases in violence from areas with civil conflicts or drug wars. Using the rapid increase in homicides following the onset of the Mexican war on drugs, Brown and Velásquez (2017) finds no effect of homicides rates in the previous month, on cognitive performance of youth on Raven matrices. The study does find a decrease in educational attainment and a decrease in employment, which is likely to be driven by the negative effects that the war on drugs had on economic outcomes (Robles et al., 2013). Using the same dataset, Nasir et al. (2016) find that in utero VE during the first two trimesters is associated with lower cognitive abilities, while exposure during the last trimester with higher cognitive abilities. The causal mechanisms, however, are not well understood. Using longitudinal school-level data on math test scores merged with local homicides data, Jarillo et al. (2016) find a decreases in test scores in schools where local homicides increased sharply, effect stronger for rural schools attributed to an increase in teacher and student absenteeism in periods of high violence.

The literature on VE is linked to the vast literature on neighborhood effects. In a highly debated randomized experiment, the *Moving to Opportunity (MTO)* study, a sample of U.S. families living in public housing were randomly assigned to receive vouchers allowing them to move to lower-poverty neighborhoods. Kling et al. (2007) look at outcomes 4-7 years after implementation and find no improvement in economic self-sufficiency or physical health but large gains in mental health for adults and female youth. Further positive effects for female youth were found on education, risky behaviors and physical health contrasted by large and negative effects on male youth. Long term evaluations in Ludwig et al. (2012) and Ludwig et al. (2013) confirm the medium-term effects even though attenuated. In a recent study Chetty et al. (2016) revisited the MTO experiment and finds that moving before age 13 predicts higher college attendance and earnings and lower single parenthood rates.

3.3 Context

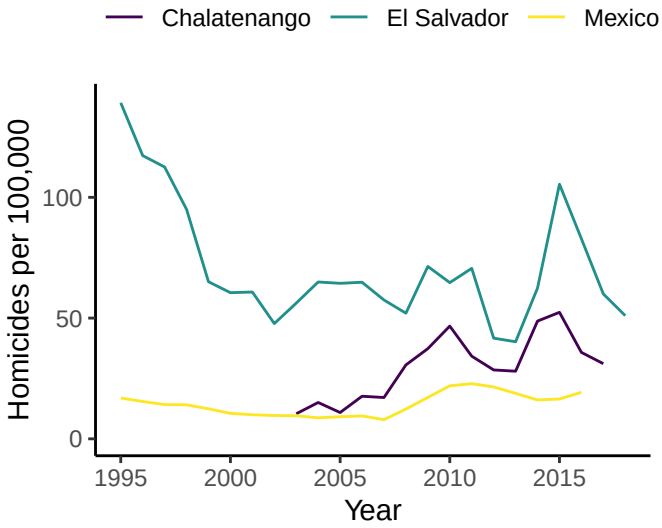
El Salvador suffered through 13 years of civil war triggered by increasing political and social unrest. In 1979, the government was overthrown by a coup of the civil-military Revolutionary Government Junta (JRG). The junta soon lost most of its civilian members turning into a brutal military government. Violence towards civilians increased and culminated with the assassination of Archbishop Romero in 1980. Several left-wing and guerrilla group united and formed the Farabundo Martí National Liberation Front (FMLN). In their first major attack in 1981, FMLN gained control over the departments of Morazán and Chalatenango. This turned the areas into main targets for the military groups leading to high number of causalities and even massacres, such as the 1981 El Mozote massacre in Morazán.

During the war, more than 1 million people were displaced and at least 75,000 killed, 85% by the military government according to the Truth Commission Report (Buergenthal, 1994). During the 1980s, a large number of migrants arrived in Los Angeles trying to escape the war. Due to lack of economic opportunities, children and youth joined the Eighteenth Street Gang or Barrio 18, one of the largest gangs in Los Angeles during those times and now one of the largest gangs in several Central American countries. Facing discrimination, El Salvadorian youth started their own gang, MS-13 or Mara Salvatrucha, which has grown to become the main rival of Barrio 18 in several Central American countries. Beginning in the 80s, and increasing with the 1996 Illegal Immigration Reform and Immigration Responsibility Act (IIRIRA) and the 2003 Homeland Security Act, El Salvadorians were deported in large numbers. Sviatschi (2018b) shows that deportations lead to the export of criminal capital and the rise of the gang culture in El Salvador.

With deportations increasing, homicides skyrocketed. Figure 3.3.1 shows the yearly homicides rates for the department of Chalatenango, El Salvador and Mexico. Homicide rates in Chalatenango are lower than the national average but their trends are closely correlated. Even though less violent than other departments, Chalatenango had a sharper increase in homicides than Mexico during the peak of the Mexican war on drugs from 2005 and 2010.

In 2012, homicides dropped sharply following a controversial truce between the two main gangs, MS-13 and Barrio 18, mediated by law enforcement officials. The truce ended with new elections in 2014, with the decision of the new government to take on a "mano dura" approach to gang violence. A rapid rise in homicides followed in 2015. Since then, however, the homicides have been declining. With no new truce in place, this is believed to be due to (i) increased sophistication of gangs transitioning away from extortion and kidnapping to international drug trafficking and selling of stolen arms and to (ii) stronger security measures in prisons, making it

Figure 3.3.1: Homicides per 100,000 inhabitants in Chalatenango, El Salvador and Mexico



Notes: The outcome variable is the yearly total number of homicides per 100,000 inhabitants in El Salvador, Mexico and the Department of Chalatenango.

harder for arrested gang leaders to coordinate criminal activities from within prisons (Asmann, 2018).

Comparing the trends in homicide rates for El Salvador and Mexico, we notice much higher volatility in the former. Sharp increases in homicides are usually sudden and unanticipated in El Salvador. Fear of violence is ever-present also among people living in areas less plagued by violence since gangs may expand their control over new territories at any times. Recent threats of mass deportations of around 200,000 Salvadorans from the president of the U.S. are contributing further to the fear that violence will explode once again. Many migrants fled violence and thus fear that upon return they will be targeted by the gangs (Sviatschi, 2018a). Deportations could also mean an additional import of criminal capital as the one documented in Sviatschi (2018b).

3.4 Empirical Strategy

This section presents the empirical strategy, starting with the experimental design in subsection 3.4.1 followed by the data collection in subsection 3.4.2. Subsection 3.4.3 describes the main variables, while section 3.4.4 presents descriptive statistics and checks for balance across treatment arms.

3.4.1 Experimental Design

Identifying the direct causal effect of VE on cognitive performance with observational data is subject to several endogeneity concerns. To overcome them, researchers often rely on longi-

tudinal surveys from areas exposed to conflicts. Even such plausible exogenous shocks are not bulletproof identification tools. First, violence could force certain types of people out from affected areas leading to selected migration out of treated areas. Second, the rise in violence risks being confounded or even caused by political or economic trends. This questions the validity of the parallel trend assumption required for causal identification in a difference-in-difference setting. Finally, even when violence incidence is as-good-as-random, understanding the mechanisms through which it affects behavior is challenging. In addition to direct psychological effects, violence is likely to impact local economic conditions. With observational data, it is difficult to establish which effects are more predominant or even if they affect behavior in the same direction. Lab experiments allow to single out such mechanisms, at the cost of external validity.

Violence can not be manipulated in a lab setting but recalling past exposure can be randomly assigned (Callen et al., 2014). Inducing affect, or priming, is a tool often used in psychological experiments. In recent years, it has grown in popularity also among economists. In a lab-in-the field experiment in Afghanistan, Callen et al. (2014) ask participants to recall an event from the past year that caused them fear and anxiety, and then observe lottery choices. Mani et al. (2013) ask shoppers in an US mall to respond to hypothetical financial scenarios which aim to trigger preoccupations with monetary concerns before taking cognitive tests. To increase external validity both experiments formulate ex-ante hypotheses on which characteristics are expected to be associated with higher treatment effects. For instance, Callen et al. (2014) finds stronger effects among participants with higher VE, while Mani et al. (2013) finds an effect only among the poor.

This paper follows a similar strategy to identify the causal effect of recalling VE on cognitive performance. 299 primary school students are randomly assigned to recall VE either before (*treatment group*) or after (*control group*) taking cognitive tests. Unlike other studies where the induction method requires participants to recall events that triggered a specific emotion (such as fear)(Callen et al., 2014; Bogliacino et al., 2017), in this study students are asked to respond to a detailed survey section on VE without being primed a specific emotional response.

Control group participants do not complete a neutral task, hence they took the cognitive tests earlier in the survey. This deviates from the standard protocol and was motivated by several factors[2]. Fist, as emphasized above, treatment effects are expected to be stronger for

[2]In the standard protocol, participants assigned to the control group also have to complete a neutral or less sensitive task which is not expected to induce any form of affect, or to induce it only to a lesser extent. In Callen et al. (2014), control group participants are asked to describe any event from the past year in the neutral condition. Other variants ask control group participants to follow through the exact task, changing just some parameters. For instance, in Mani et al. (2013), control group participants respond to the same financial situations but of

children more exposed to violence. To test this hypothesis, VE exposure data is needed for all participants. I could not rely on administrative VE data since targeted schools are geographically close to one another (from 2 to 30km). Thereby I use self-reported VE. Among treated, VE is measured during treatment. Control group students receive the same VE survey section but towards the end of the survey. Introducing a neutral condition for the control group, while still having them go through the VE section would have made the survey longer and risked lowering data quality [3]. Students were interviewed during school hours and we wanted to avoid them missing more than one class. Alternatively, I could have used a design as in Bogliacino et al. (2017), which use a standard priming protocol before measuring the outcomes, followed by a survey section on VE for both treated and control groups. But, again, this would have substantially increased the duration of the survey. One general concern with lacking a neutral condition is that before measuring the outcome, the treated group have to go through a longer survey which may lead to fatigue or boredom. Given the direction of the results, I argue that if fatigue or boredom played any role, then the estimated treatment effects are a lower bound of the true effect. On the other hand, taking the test later in the survey (treated group) may have allowed students more time to "warm up", that is to become more engaged with the enumerator, to get familiar with the survey and, as a result, to focus more on the cognitive tests. While I can not firmly rule out any of the two concerns, in subsection 3.6.4 I provide some evidence against both.

3.4.2 Data and Sampling

The survey was conducted between March and April 2019 in 8 schools in the department of Chalatenango, drawn from a pool of 12 schools that participate in an international project which aims to enhance social inclusion and reduce violence and dropout. A random sample of classes (totaling 468 students) were selected, stratified by school and cycle. Only the first two cycles were targeted which correspond to grades 0-6.

The project and the data collection were introduced to parents or legal guardians by local facilitators during school meetings and through home visits[4]. Consent forms were obtained for over 350 students. Stratified by school, cycle and gender, 250 students were randomly selected to participate in the survey. If a selected child was absent, a substitute student, sampled with

lower severity.

[3]Data collection was part of the baseline survey of an intervention and study on violence and behavior in children. Randomizing the order of the violence section was only an addition to this baseline survey.

[4]The schools have long term collaborations with the implementing organizations which facilitated the process of getting parents acquainted with the project and to consent the participation of their children in spite of the sensitive topics covered.

the same strategy, was interviewed. The final sample reached 299 students, higher than the target sample. When time allowed, enumerators interviewed also the substitutes[5].

The survey was conducted by local field workers, the majority of whom were studying or graduated in social work at the local university. They were recruited based on their experience of working with children. During training, special emphasis was placed on ethical concerns. In particular, enumerators were instructed to explain to the students that they are not obliged to respond to any questions that make them uncomfortable and that any information will be kept strictly confidential.

Before beginning the interviews, the school facilitators presented the project and the data collection to the entire class. Students were informed that they can only participate if parents gave their consent and that they are not obliged to participate if they do not want to. The interviews took place in reserved areas of the school to ensure privacy. The survey was administered using SurveyCTO, a mobile data collection tool that does not require internet connection making it appropriate in the development context. Visual cues were used to facilitate answers on the questions using Likert scales. Play materials (simple legos, puzzles or play dough) were handed if children appeared bored or disengaged. The interviews lasted between 20 minutes and 1 hour. Only one student refused to participate.

3.4.3 Variables

All students responded to the same survey. What changed was the order of sections, in particular when the VE section was administered, as shown below:

Survey design: Demographics ⇒ VE if **Treated** ⇒ Cognitive tests ⇒ Noncognitive skills, Gender Norms, Teacher support ⇒ VE if **Control** ⇒ Social support and Parenting

The interview started with a section on demographics and socioeconomic status. Next, the violence section was administered to treated students followed by the cognitive tests, while control students advanced directly to the cognitive tests. After, sections on noncognitive skills, attitudes, norms and teacher support followed. Next, the section on VE was administered to control group students. The survey ended with a section on parenting practices and social support.

This subsection proceeds with the description of the scales and tests used to construct the main variables - VE and cognitive performance - and the secondary outcome or control variables - noncognitive skills, teacher support, school support, social support and parenting.

[5]In general, children were eager to take part in the survey. We exclude from the analysis two students where the enumerators had problems with the mobile device and could not administer the cognitive tests properly.

Violence Exposure - The section on VE combined items adapted from the *Young Lives Study* (Boyden, 2012), the *Reduced Aggression/Victimization Scale* (Orpinas and Horne, 2006), the *School Relationships Questionnaire* (Wolke et al., 2000), the *Exposure to Neighborhood Violence Scale* (Attar et al., 1994), and the *Survey of Children Exposure to Community Violence Scale* (Richters and Saltzman, 1990). The items were translated and adapted to the local context and capture incidence and fear of violence and bullying at school and in the community. Items related to domestic violence and gang violence (local "pandillas") were excluded[6].

Table 3.4.1: Exposure to Violence Summary Statistics

	Mean	Never	Sometimes	Often	Always	Obs
Panel A: At School						
Classmates physically punished by teacher		0.81	0.13	0.03	0.02	299
Physically punished by teacher		0.94	0.05	0.00	0.00	299
Some students bother others		0.20	0.44	0.06	0.30	299
Students fight		0.24	0.47	0.07	0.22	299
Students get along well		0.05	0.37	0.11	0.48	299
Bullied by others		0.41	0.46	0.05	0.07	298
Belonging taken by others		0.55	0.36	0.03	0.06	299
Forbidden by others to play		0.60	0.32	0.02	0.05	298
Panel B: In the Neighborhood						
Trusts neighbors		0.18	0.26	0.04	0.50	295
Feels safe in the neighborhood		0.21	0.23	0.04	0.51	296
Treated bad in the neighborhood		0.87	0.09	0.01	0.04	299
Saw someone being beaten or shot	0.26					299
Heard gunshots	0.45					298
Family member was robbed	0.30					296
Member of family hurt other people	0.11					289

Note: Proportions in parenthesis. The missing values in the rows are due to the fact that the last four variables in the table are measure on an "Yes" or "No" scale. When computing the index, the item "Students get along well" is reverse coded.

Descriptive statistics are presented in Table 3.4.1. Over 40% of students report being the victim of some form of bullying, while more than 70% report teasing and fighting among their classmates. Regarding the neighborhood, roughly 20% report they never feel safe and that people can never be trusted. Close to 17% report that they have seen the teacher physically abusing other classmates, but only 6% report being the victims themselves. 44.6% heard shootings in the neighborhood, 30% report that one member of the family was robbed while 11% report that they have seen one member of their family hurting physically another person. Throughout the analysis, an index of VE is used. The index is constructed using the inverse covariance weighting

[6]The implementing organization was strongly against adding any questions on local gangs given the constant state of terror among the population. Also, they requested avoiding the topic of family violence out of fear of parents forbidding the children to take part in future activities of the project.

procedure from Anderson (2008). The method computes indexes which maximize the amount of information extracted from the items, hence weighting more new information as opposed to shared information. Throughout the analysis, I also used indexes of school VE and neighborhood VE, computed through the same method but using only their respective subscales. If a child refuse to answer, the mean value is imputed. I do not find non-response rates to vary by treatment (see Table A-3 in Appendix 2.A). However, I also report the results of the main analysis excluding children with non-responses in the items (see Table A-4 in Appendix 2.A).

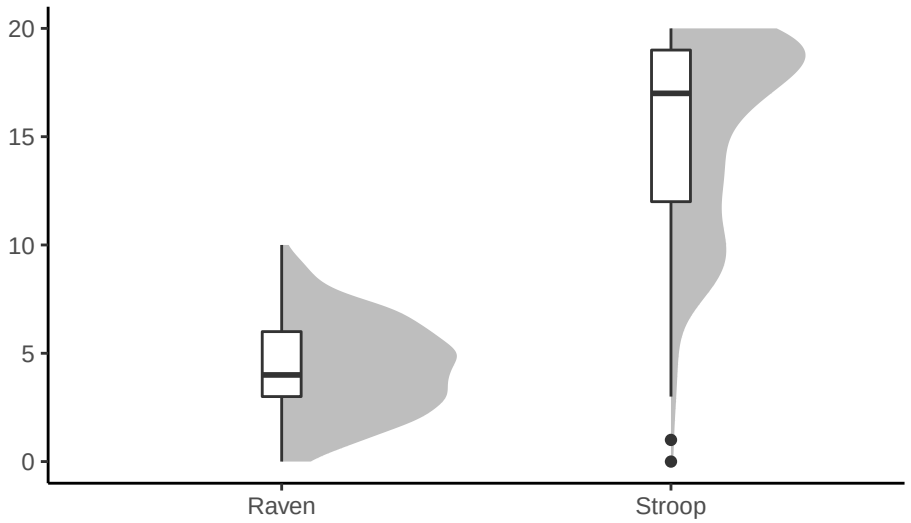
Cognitive skills - Students were administered two tests measuring two sub-components of cognitive functions: inhibitory control and fluid intelligence. Inhibitory control or self-control refers to one's ability to override impulses and ignore distractions (Rothbart and Posner, 1985). While extensively studied in psychology (Baumeister, 2002; Logan et al., 1997; Carlson and Moses, 2001), in recent decades it has gained the attention of economists and was included in economic theories in order to explain empirically observed dynamically inconsistent behaviors (Thaler and Shefrin, 1981; Laibson, 1997; Gul and Pesendorfer, 2001; Fudenberg and Levine, 2006). The ability to inhibit impulses appears to be directly linked with violent behavior. In a youth intervention in Chicago aimed at reducing crime and dropout through cognitive behavioral therapy techniques, Heller et al. (2017) find that the treatment effects can be partly attributed to improved control of automatic thoughts and impulses.

Impulse control is measured using the *Spatial Stroop Test*, a version of the Classic Stroop Test (Stroop, 1935) using spatial cues instead of verbal ones, thus not requiring literacy skills, and working memory only to a lesser extent. Participants are shown an arrow pointing either to the left or the right and are asked to press the button on the side the arrow is pointing regardless of whether the arrow is placed on left or on the right side of the screen. They begin with several examples to ensure full comprehension. Then they are instructed to place their thumbs or index fingers above the two buttons. Before proceeding to the actual test, several trial runs are performed. The test consisted of 20 trials, half congruent and half incongruent. Students were instructed that their final score will depend on their reaction time. Due to software limitations, we could not set a maximum amount of time per trial as it is done usually for such tests. However, the time spent on each trial was recorded. The final score is computed by summing up the number of correct trials, thus varying between 0 and 20.

Fluid intelligence is a higher-order cognitive function and measures one's ability to solve new tasks and adapt to new situations (Dean et al., 2017). Participants were administered a shortened version of the *Raven's Matrices Test* (Raven and Court, 1998), one of the most widely used tools for assessing fluid intelligence. The test consists of several images missing a piece, similar to a puzzle. Participants choose either the option they think correct from a set of six alternative or answer that they do not know. There are no time restrictions. A shortened version of 10 trials

was used. The difficulty was carefully chosen to ensure that there is no censoring at the ends of the distribution.

Figure 3.4.1: Raven matrices and Stroop test - kernel densities and box plots



Notes: The y axis indicates the score on the cognitive tests. The grey area plots vertically kernel densities. The thick horizontal line indicates the median , and the upper and lower edges of the rectangulars give the 25th and 75 percentile. Dots indicate outliers.

Figure 3.4.1 displays kernel densities with nested box plots for the scores on the two cognitive tests. The score on the Raven Matrices follows a bell shaped distribution with mean and median of roughly 4 correct trials out of 10. 6 children did not solve any trial and only 1 solved all 10. The score on the Stroop test has a distribution skewed towards the right with a mean of 15 and median of 17 correct trials. 43 students solved all the trails, censoring the distribution at the right tail. 16 students solved fewer than 8 trials which probably means at least some of them did not understand the rules or just played at random. Excluding them from the analysis does not have any impact on the main results.

Noncognitive skills - In the past two decades, a large body of literature has documented the crucial role noncognitive skills play in academic achievement and labor market outcomes (Heckman and Rubinstein, 2001; Heckman et al., 2006; Borghans et al., 2008; Kautz et al., 2014). Theoretical models were developed to explain the interplay between different types of skills (cognitive, noncognitive, health) and how they can be addressed through interventions in different cycles of childhood (Cunha et al., 2010; Attanasio, 2015). What is most worth noting is that noncognitive skills appear much more malleable throughout life. For instance, Blattman et al. (2017) find that improving noncognitive skills through cognitive behavioral therapy reduced crime and violence among criminally engaged men in Liberia.

I try to capture multiple dimensions of noncognitive skills adapting items from the *Generalized*

Self-Efficacy Scale (Luszczynska et al., 2005), the Young Lives Study, the German Socio-Economic Panel and two questions measuring shyness from Hopko et al. (2005). Note that treated students respond to this section after the VE while control students do it before. Thus, one could expect treatment to affect also this outcome. However, this outcome was not part of the analysis plan given the lack of empirical and theoretical foundations. Adding other outcomes would have also increased the probability of finding treatment effects by chance. Noncognitive skills were measured as part of the baseline survey of the project and are used in this paper as outcome variable only to describe the association between VE and skills. For transparency, I report results of models using all components of the noncognitive skills index in Table 3.6.3. None of the outcomes was affected by treatment.

Teacher, parental and social support - *Teacher support* is measured using 4 items from the Hungarian Life Course Survey. Students state to what extent teachers (i) encourage them to share their opinions in class, (ii) are interested in their personality, (iii) are helping them in case of need and (iv) treat all students equally. *Social support* is measured using items from the Young Lives Study, asking students if they have someone who could help in various situations: (i) problems at school, (ii) when they are worried about something,(iii) if someone treats them bad at school and (iv) when needing pocket money for school. Qualitative evidence gathered by the implementing organization suggested that students receive little support from parents. In order to measure *parental engagement*, we ask children how often parents/tutors engage in several activities with them: helping with schoolwork, organizing school materials, talking to the child about school, talking to teachers and going to parent meeting, checking their grades or asking them to skip school to help with house chores. All indexes are computed using the inverse covariance weighting method. Throughout the paper, they are used as control variables and also as potential confounds when analysing differential effect of the treatment by VE.

3.4.4 Descriptive Statistics and Balance Check

Table 3.4.2 presents summary statistics for the treated and control group along with p-values associated with the t-test of equality in means between the groups. Panel A shows the main characteristics of the students. The average age is 9.4 years, around 55% are girls, roughly 28% reported that they contributed financially to the family in the past year and around 13% said they missed at least one whole week of school.

Panel B presents household characteristics. The average household size is 4.2 with 2.1 children. 8% of students do not live with their mothers while a higher share of roughly 27.5% does not live with their fathers. Around 60% of parents did not go to high school and around 16-18% completed university. A large share of parents are self-employed, common small businesses in the region being sewing hammocks and selling cooked food, activities often involving children. Most households have electricity and a water source in the house but a smaller share has a designated

Table 3.4.2: Descriptive Statistics and Balance Checks

	Control		Treated		T - C	
	Mean	S.E.	Mean	S.E.	p-value	N
Panel A: Child variables						
Female	0.52	0.04	0.59	0.06	0.22	299
Age	9.38	0.16	9.38	0.21	0.99	299
Health worse	0.08	0.02	0.07	0.03	0.92	297
Sleeping hours	9.01	0.11	9.02	0.14	0.94	299
Time to school	13.05	1.10	14.29	1.47	0.40	291
Missed school (> 1 week)	0.12	0.03	0.14	0.04	0.66	299
Worked in the past year	0.26	0.04	0.30	0.05	0.45	299
Panel B: Household variables						
Household size	5.09	0.17	5.14	0.23	0.84	299
Children in household	1.99	0.09	2.23	0.12	0.05	299
Siblings	0.84	0.08	0.96	0.10	0.26	299
Lives with mother	0.92	0.02	0.92	0.03	0.89	299
Lives with father	0.69	0.04	0.76	0.05	0.19	297
Mother works abroad	0.07	0.02	0.07	0.03	0.88	299
Father works abroad	0.18	0.03	0.17	0.05	0.82	283
Low or no education (father)	0.48	0.05	0.52	0.06	0.50	265
University education (father)	0.12	0.03	0.16	0.04	0.32	265
Low or no education (mother)	0.53	0.04	0.50	0.06	0.69	296
University education (mother)	0.15	0.03	0.18	0.04	0.48	296
No work (father)	0.04	0.02	0.03	0.02	0.91	265
High skilled job (father)	0.20	0.04	0.20	0.05	0.99	265
No work (mother)	0.35	0.04	0.34	0.06	0.83	296
High skilled job (mother)	0.15	0.03	0.14	0.04	0.89	296
Kitchen separate room	0.80	0.03	0.80	0.05	0.94	299
Sleep in kitchen	0.09	0.03	0.10	0.03	0.86	299
Number of bedrooms	2.56	0.12	2.55	0.16	0.96	299
Electricity	0.99	0.01	0.98	0.01	0.43	299
Water source in house	0.96	0.02	0.96	0.02	0.95	299
Bathroom in house	0.71	0.04	0.80	0.05	0.08	299

Note: All variables are reported by the student. The p-value in the penultimate column is associated with the t-test of equality between the means across the two groups.

room for cooking or sanitary services inside the house.

The final column displays the p-value associated with the t-test of mean equality between the groups. Out of 28 comparisons, having sanitary services in the house is higher in treated groups, significant at 10%, while control groups household have fewer children on average, significant at 5% level. This is consistent with chance. The differences are somewhat contradictory as proxies of socioeconomic status and do not clearly suggest one group being poorer than the other. Nonetheless, I provide robustness checks showing that controlling for these variables does not affect the main findings.

3.5 Results

3.5.1 Violence Exposure as Predictor of Cognitive and Noncognitive Skills

Before proceeding to the core analysis, I present a descriptive analysis of the relationship between VE and skills (cognitive and noncognitive). In other words, I investigate the predictive power of VE in explaining differences in skills, before and after conditioning on other covariates. In addition to this, this exercise serves also as a form of validation of the measures used to construct the indexes of VE and noncognitive skills given that several different scales were adapted and combined. I restrict the analysis only to the control group and estimate the following specification through OLS regression models:

$$Y_i = \beta_0 + \beta_1 VE_i + X_i'\gamma + \epsilon_i \tag{3.1}$$

where Y_i is the skill outcome (cognitive or noncognitive), VE_i is the index of VE while X_i is a vector of child and household characteristics. In further specifications, VE is replaced with its sub-components, the indexes of VE at school and in the neighborhood.

Table 3.5.1 presents the estimates of Equation 3.1. In columns (1)-(4), the outcome variable is the index of cognitive skills. In columns (5)-(8) the outcome is the index of noncognitive skills. Columns (1)-(2) and (5)-(6) use as main explanatory variable the index of VE, while the other columns use the indexes of school and neighborhood VE. All even numbered columns include controls, while all odd do not. I remind that the index variables are standardized using the control group mean and standard deviation.

We first look at the association between VE and cognitive skills. Results in columns (1)-(2) show that the index of VE has only a marginally significant predictive power in explaining differences in cognitive skills. Breaking down the index in its sub-components, we observe that VE at school level does predict lower cognitive skills while neighborhood VE does not. The former estimate does decrease after covariates are included but remains significant at 10% level. Turning briefly to the covariates, as expected, age strongly predicts cognitive skills.

Columns (3)-(8) present a stronger and more stable relationship between VE and noncognitive skills. Even after conditioning on covariates, the estimates indicate that one standard deviations higher VE predicts lowers noncognitive skills by about 0.39 standard deviations. Neighborhood and school VE have coefficients of similar magnitudes and are both highly statistically significant. Among all covariates, only the parenting index shows a weakly significant correlation.

As emphasized above, this is a descriptive analysis and causality is not claimed for any of the models. The general identification problems: reverse causality, omitted variable bias and measurement error are likely to be present. While VE may indeed harm skills development, low skills is likely to trigger violence from others. For instance, children doing poorly in school may

Table 3.5.1: Violence Exposure and Human Capital

	Cognitive Skills				Noncognitive Skills			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VE	−0.14*	−0.15*			−0.41***	−0.39***		
	(0.08)	(0.08)			(0.08)	(0.08)		
School VE			−0.21**	−0.15*			−0.24***	−0.22***
			(0.10)	(0.09)			(0.06)	(0.06)
Neighborhood VE			0.01	0.01			−0.29***	−0.29***
			(0.08)	(0.08)			(0.09)	(0.10)
Female		−0.06		−0.07		0.15		0.20
		(0.16)		(0.16)		(0.17)		(0.17)
Age		0.24***		0.23***		0.03		0.02
		(0.05)		(0.05)		(0.05)		(0.05)
SES index		0.09		0.09		0.06		0.05
		(0.08)		(0.08)		(0.08)		(0.08)
Teacher support index		0.01		0.01		0.01		0.01
		(0.11)		(0.11)		(0.08)		(0.09)
Social support index		0.04		0.06		−0.09		−0.09
		(0.08)		(0.08)		(0.08)		(0.08)
Parenting index		−0.11		−0.10		0.11		0.14*
		(0.07)		(0.08)		(0.08)		(0.08)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	133	133	133	133	133	133	133	133
Adjusted R2	0.01	0.20	0.03	0.19	0.16	0.15	0.14	0.13

Note: All estimates are obtained via OLS regressions. Robust standard errors in parentheses. *, **, *** denote significance at the 10%, 5% and 1% levels respectively. Both outcome variables computed through inverse covariance weighting, and are standardized using the control group mean and standard deviation.

be punished by teachers, parents or their peers. Children with low social skills are likely to be the target of bullies both at school and in the neighborhood. The estimates presented above are suggestive of this possibility. We saw that school VE is correlated with both cognitive and non-cognitive skills, while neighborhood VE only predicts noncognitive skills. At school level, skills may be easily signalled and punished by teachers or peers. In the neighborhood, cognitive skills are harder to be signalled, while noncognitive skills are easier to be observed and punished if inadequate. Finally, the covariates included are likely to be measured with error. For instance, I do not have direct information on income and it is hard to expect children of this age to know the education or even the precise occupation of both their parents. Given this, it must be noted that the presented estimates may be upwardly biased.

3.5.2 Main Results

To test the null hypothesis that being assigned to recall VE before taking cognitive tests did not affect cognitive function, I estimate the following model:

$$Y_i = \beta_0 + \beta_1 Treated_i + X_i'\gamma + \epsilon_i \tag{3.2}$$

where Y_i represents the cognitive skills index for student i, $Treated_i = 1$ if recalled VE prior to taking the cognitive tests, while X_i is a vector of child and household characteristics. $\widehat{\beta_1}$ is the estimate of interest.

One concern is that treatment effects are independent of having been exposed to traumatic experiences and is just an artifact of being asked uncomfortable questions about violence. To rule out this possibility, I exploit the fact that control group students are asked the same VE questions, but later in the survey. Thereby, I am able to investigate whether treatment effects are increasing with higher VE, by estimating the following model:

$$Y_i = \beta_0 + \beta_1 Treated_i + \beta_2 VE_i + \delta Treated_i \times VE_i + X_i'\gamma + \epsilon_i \tag{3.3}$$

where VE_i represents the VE index for student i. All the other variables are defined as in Equation 3.2. $\widehat{\beta_1}$ estimate indicates the average treatment effect for students with $VE = 0$ (VE at the level of the mean of the control group). $\widehat{\delta}$ is the estimate of interest checking for heterogeneous treatment effects by VE. Estimates are obtained through OLS regressions. The outcome variable is standardized by subtracting the control group mean and dividing by the standard deviation of the control group. Thus, estimates are interpreted as average differences in terms of standard deviations relative to the control group. In all models, I use robust standard errors clustered at individual level, the unit of randomization. Contamination should not be a concern. Most students from a given class and grade were interviewed at the same time or within a narrow time window.

The main results are reported in Table 3.5.2. Columns (1) and (2) show the regression estimates of Equation (3.2) with and without controls. Recalling VE before taking the cognitive tests increases performance by above 0.2 standard deviations, estimate significant at 5% level [7]. Figure A-1 in Appendix 3.A illustrates kernel densities for the two groups. The treated group displays a shift in the distribution to the right but only at lower levels of cognitive skills. At higher levels, the differences between groups become small. The treatment appears to be improving the performance of children in the bottom part of the cognitive skills distribution.

Column (3) reports results of the specification in Equation 3.3. The estimate on the interaction term indicates that treatment effects are larger for students reporting higher VE, effect significant at 10% level. For students with $VE = 0$ (equal to the control group mean), the treatment effect is roughly 0.25 standard deviations. The effects drops to 0 for students with VE 1.25 standard deviations lower to the control group mean. Columns (4) and (5) report estimates separately for students with VE indexes bellow the median (column (4)) and above it (column (5)). Among the high exposure group, the estimates effect is approximately 4 times larger in

[7] I note that such an effect size, at 5% significance level, given the sample size, corresponds to only 60% power.

Table 3.5.2: Treatment effects of recalling violence exposure on cognitive performance

	All			Lower VE	Higher VE	All
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.24**	0.23**	0.25**	0.10	0.39***	0.24**
	(0.11)	(0.10)	(0.10)	(0.16)	(0.14)	(0.10)
VE			−0.12			
			(0.08)			
Treated × VE			0.20*			
			(0.10)			
School VE						−0.15*
						(0.08)
Neighborhood VE						0.02
						(0.08)
Treated × School VE						0.20*
						(0.12)
Treated × Neighborhood VE						0.01
						(0.11)
Controls	No	Yes	Yes	Yes	Yes	Yes
Observations	299	299	299	150	149	299
Adjusted R2	0.01	0.15	0.16	0.12	0.17	0.16

Note: All estimates are obtained via OLS regressions. The outcome variable in all models is the index of cognitive performance, computed through inverse covariance weighting, standardized using the control group mean and standard deviation. Robust standard errors in parentheses. *, **, *** denote significance at the 10%, 5% and 1% levels respectively. Constrols include individual and household characteristics - age, gender, SES index, teacher support index, social-support index and parenting index. VE denotes the index of violence exposure computed through inverse covariance weighting, standardized using the control group mean and standard deviation. School and neighborhood VE are computed through the same method using only the their respective subscales. The sample used in the analysis either contains all observations (columns (1)-(3) and (6)), only participants with VE bellow the median (column (4)) or only participants with VE above the median (column (5)).

magnitude (0.4 standard deviations) and highly significant. Among the low exposure group the estimated coefficient is statistically insignificant.

While I cannot rule out that the interaction effect might be confounded by other unobserved factors correlated with VE, I test if it is confounded by observed covariates likely to be correlated with VE. Table A-1 in Appendix 3.A reports results of regressions where treatment status is also interacted with indexes of noncognitive skills, socio-economic status, teacher support, and parenting. The main findings do not change and the estimates are stable in all models. In column (4), the interaction between treatment and parenting index is positive and significant, meaning

that cognitive performance among the treated is increasing in parenting support. Note that this finding should be interpreted with care given the high number of hypotheses tested. I treat as suggestive evidence regarding the role of social support in dealing with traumatic experiences.

Finally, I investigate if treatment effects are stronger for VE at school or in the neighborhood, and report the results in column (6) of Table 3.5.2. The main effect appear to be mostly driven by VE at school. The estimates on neighborhood VE are small and statistically not different from zero both for the term and the interaction. To sum up the results presented so far, recalling VE increases cognitive performance, effects driven by students with higher self-reported VE, particularly violence in the school context.

3.6 Robustness Checks

3.6.1 Time spent in cognitive tests by treatment status

Did treated students score higher in cognitive performance by spending more time solving the trials? This subsection investigates this question separately for each type of test, since differences in time spent have different interpretations. One one hand, in the Raven test, students were instructed that there is no time limit. If treated students took more time, this could indicate higher effort or higher willingness to revise their initial guesses. On the other hand, in the Stroop test, students were instructed that doing the trials as fast as possible is part of the game. If treated student were slower than main results of the analysis are upward biased. With a slower reaction time it is easier to inhibit the initial impulse. I test this by regressing time spent in the Raven (in minutes), median and mean reaction time in the Stroop (in milliseconds) on treatment status. I also allow the estimates to vary by VE since the main results were stronger for students with higher VE.

Table 3.6.1 reports the results. Treated children reporting higher VE spent slightly more time on average on the Raven Test. The difference is again small but only marginally insignificant at 10% level. Since the Raven time was not measured very precisely power is affected. These results can be viewed as suggestive evidence that high VE treated students did better on the Raven because they exerted more effort. Turning to the Stroop, treated students spent slightly more time but the differences are below 1/10 of a second and not statistically significant. Given that the median and mean reactions times are around 1.5 seconds on average, I do not consider these differences a major concerns.

Table 3.6.1: Time spend solving the cognitive tests by treatment condition

	Raven Time (1)	Stroop Mean Time (2)	Stroop Median Time (3)
Treated	−0.01	91.19	47.10
	(0.12)	(67.66)	(37.14)
VE	−0.22**	−11.92	−18.71
	(0.09)	(28.97)	(24.21)
Treated × VE	0.21	25.84	5.86
	(0.14)	(54.42)	(43.12)
Constrols	Yes	Yes	Yes
Observations	299	299	299
Adjusted R2	0.01	0.07	0.11

Note: All estimates are obtained via OLS regressions. Robust standard errors in parentheses. *, **, *** denote significance at the 10%, 5% and 1% levels respectively. All models control for individual and household characteristics - age, gender, SES index, teacher support index, social-support index and parenting index. Outcome variables are the time spent solving the Raven Matrices (in minutes), and the mean and median reaction time in the Stroop test (in milliseconds)

3.6.2 Treatment effects on recalling of violence exposure

The analysis of differential effects by self-reported VE in subsection 3.5.2 relies on the assumption that self-reported VE is independent of treatment assignment. However, students were asked to recall VE at different points in the survey and there is evidence that question ordering can affect survey responses (McFarland, 1981; Bowling, 2005). One concern is that, as the survey progresses, students can become fatigued and less willing to try to recall violent episodes. If control group students under-report VE, then the heterogeneous effects by VE are biased upwards since VE is negatively correlated with cognitive performance. On the other hand, being asked sensitive questions later in a survey may allow more time for the enumerator to gain the trust of the student. If students felt more comfortable sharing sensitive information later in the survey (control group), then the estimated heterogeneous effects would be downward biased. I test which of two scenarios is more plausible by regressing self-reported VE, VE at school and VE in the neighborhood on treatment assignment.

The estimate in column (1) of Table 3.6.2 hints towards the second scenario. Being asked questions related to VE early in the survey reduces self-reported VE by 0.25 standard deviations. Results in columns (2) and (3) indicate that this is mostly due to under-reporting of neighborhood violence. I speculate that this is due to the very sensitive nature of the neighborhood violence section. Among the most sensitive were questions on physical harm done to the family or by a family member, feelings of safety in the neighborhood and having heard gunshots in the neighborhood.

Table 3.6.2: Violence exposure recalled by treatment condition

	VE	School VE	Neighborhood VE
	(1)	(2)	(3)
Treated	−0.25**	−0.11	−0.34***
	(0.11)	(0.11)	(0.11)
Controls	Yes	Yes	Yes
Observations	299	299	299
Adjusted R2	0.06	0.02	0.04

Note: All estimates are obtained via OLS regressions. Robust standard errors in parentheses. *, **, *** denote significance at the 10%, 5% and 1% levels respectively. Constrols include individual and household characteristics - age, gender, SES index, teacher support index, social-support index and parenting index. VE denotes the index of violence exposure computed through inverse covariance weighting, standardized using the control group mean and standard deviation. School and neighborhood VE are computed through the same method using only their respective subscales.

One concern is that VE differences between the two groups are not due to the randomized VE section ordering but instead, due to chance, reflect actual imbalances between the two groups. I find this unlikely since the groups are well balanced across a wide range of characteristics as it was shown in Table 3.4.2. Furthermore, I do not find any differences in noncognitive skills as presented in Table 3.6.3 below. Since I have shown in subsection 3.5.1 that VE and noncognitive skills are highly correlated, if the two groups were not balanced we would expect to observe also differences in noncognitive skills by treatment status.

Table 3.6.3: Treatment effects on self-reported noncognitive skills

	Self Efficacy	Social Skills	Emotional Problems	Conduct Problems	Noncognitive Skills
	(1)	(2)	(3)	(4)	(5)
Treated	−0.06	0.05	0.03	−0.02	0.02
	(0.12)	(0.12)	(0.11)	(0.12)	(0.12)
VE	−0.02	−0.14*	0.35***	0.23***	−0.39***
	(0.09)	(0.08)	(0.08)	(0.08)	(0.08)
Treated × VE	−0.14	−0.03	−0.09	0.17	−0.04
	(0.13)	(0.13)	(0.12)	(0.12)	(0.13)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	299	299	299	299	299
Adjusted R2	0.13	0.08	0.08	0.09	0.16

Note: All estimates are obtained via OLS regressions. Robust standard errors in parentheses. *, **, *** denote significance at the 10%, 5% and 1% levels respectively. All models control for individual and household characteristics - age, gender, SES index, teacher support index, social-support index and parenting index. The outcome variables listed in the column headers are computed through inverse covariance weighting.

These results question the validity of the finding that the main results are stronger with higher VE at school but not with higher VE in the neighborhood. If neighborhood VE is more severely under-reported by the treated, then the heterogeneous treatment effects presented in column (6) of Table 3.5.2 are downward biased (in absolute terms).

The results presented in this subsection have methodological implications for survey design. First, I find that asking sensitive information before cognitive tests, has a strong and differential impact on cognitive performance. Thus, when measuring cognitive function, the survey sections preceding the tests should be chosen with great care not to impact certain types of respondents in a differential way. Second, asking sensitive information too early in the survey leads to severe under-reporting. As a result, I recommend that such information to be asked only when a relationship of trust between the enumerator and the respondent was clearly established.

3.6.3 Test order effects on cognitive performance

In subsection 1.3.1 I discussed the main concerns regarding the lack of a standard control condition in the experiment design and identified two potential scenarios: (i) taking the tests later in the survey may have increased fatigue or boredom among the treated, (ii) taking the tests later in the survey may have allowed the treated to get familiar with the survey and the interviewer which increased their focus on the tests. Under (i) the treatment effects are a lower bound of the true effects, while under (ii) the validity of the results is severely questioned. While I can not firmly rule out any of the two, I provide some evidence against both scenarios by taking advantage of the randomized order of the two cognitive tests. Under scenarios (1)/(2), I would expect participants to do better/worse on the first test and worse/better on the second test.

Table 3.6.4: Cognitive performance by order of cognitive tests

	All		Control group	
	Raven	Stroop	Raven	Stroop
	(1)	(2)	(3)	(4)
Raven first	−0.03	−0.07	0.18	0.53
	(0.10)	(0.23)	(0.16)	(0.35)
Controls	Yes	Yes	Yes	Yes
Observations	299	299	133	133
Adjusted R2	0.15	0.05	0.20	0.09

Note: All estimates are obtained via OLS regressions. The outcome variables are the score in the Raven matrices and Stroop test, standardized with the control group mean and standard deviation. Raven first indicates whether the Raven matrices test was administered first or not. Robust standard errors in parentheses. *, **, *** denote significance at the 10%, 5% and 1% levels respectively. Controls include individual and household characteristics - age, gender, SES index, teacher support index, social-support index and parenting index. The sample used in the analysis contains all observations (columns (1)-(2)) or only participants in the control group (columns (3)-(4))

Table 3.6.4 reports the estimated obtained from regressing performance in the two tests on the order in which the test were administered. Columns (1) and (2) report estimates for the whole sample, while in columns (3) and (4) the analysis is restricted to the control group. Control group students took the test earlier in the survey which may mean they were less "warmed-up" with the survey at the time of the tests. Thus, a higher order treatment effect may be expected for them. Across the entire sample, we do not find any evidence of order effects on performance in the two tests. Among the control group, the estimates are larger and positive for both tests, though none statistically significant. I emphasize once again, that this is only suggestive evidence against the two scenarios presented above. In the absence of a standard control group, these concerns signal a limitation of this study.

3.6.4 Additional Robustness Checks

Table A-4 in Appendix 3.A shows the results of several additional robustness checks, including the main results as reference in column (1). Up to now, I used indexes of SES and other variables as control variables, motivated by the fact that many of the variables may be measured with error. In column (2), I add controls for the two variables which were unbalanced by treatment condition: the number of children in the household and whether the bathroom is in the house. Including these variables, does not affect the main results in a meaningful way.

In column (3), I remove the outliers on the Stroop test on the premise that if they were, by chance, unevenly distributed among the two groups, the main treatment effects may be biased. In column (4), I remove all children who had non-responses in the VE section. In the main analysis their values were imputed using the mean of the item. Again, we observe that the main results are not sensitive to either robustness check.

Models in columns (5) and (6) add enumerator and school fixed effects, while in column (7), the model includes both types of fixed effects. The main estimates become smaller, the interaction effect losing also its significance. Note however, that some of the fixed effects have very small number of students. For instance, two of the enumerators interviewed less than 8 students and two of the schools have at most 10 students. In column (8), I include all the conditions used in the models in columns (2)-(7). The estimate on the term drops towards zero, while the interaction term does not change. Note that this is a very restrictive specification and 34 observations are dropped from the analysis.

Overall this exercise shows that the main results are relatively robust. While the estimates become attenuated in some specifications, we need to consider that, as discussed in the previous section, they are likely lower bound estimates of the true effect.

3.7 Discussion

Although I can not identify the channels through which recalling VE leads to an increase in cognitive performance, I can provide some suggestive evidence of channels which are unlikely to matter. First, I do not find treated students to report being more afraid (item measured in the noncognitive skills section), which is the emotion primed in Bogliacino et al. (2017) and Callen et al. (2014). Furthermore, I also do not find evidence that recalling VE made students perceive themselves as grittier in the face of adversity (self-efficacy scale), which would have suggested a form of post-traumatic growth response (Tedeschi and Calhoun, 2004).

This is not the first study to find conflicting evidence with previous results on the impact of violence on behavior. Voors et al. (2012) and Callen et al. (2014) find opposite effects of VE on risk taking, in spite of the fact that both studies use convincing identification strategies. The fact that my results contrast the findings in Bogliacino et al. (2017), even though using similar methodologies, suggests that responses to VE may be context specific. Since the cultural context is relatively similar in Colombia and El Salvador, one of the main difference between the two studies is the age of respondents. The second key difference is methological: in Bogliacino et al. (2017) a specific response (fear) is triggered, while in my study students simply recall VE without being induced a specific emotion. Thus, further research should identify the emotional responses triggered by VE and how they interact to produce changes in behavior.

Though speculative, I propose that the observed effects may relate to the literature on the psychology of poverty and in particular to the scarcity/mental bandwidth theory (Mullainathan and Shafir, 2013). The theory states that scarcity of a resource captures mental bandwidth, shifting focus towards it. Violence is also a form of scarcity but scarcity of security. The results of this paper and the results in Bogliacino et al. (2017) may differ because the experimental manipulation may have redirected mental focus in a different way. In El Salvador, education is highly promoted as a way to escape violence. Students may be conditioned to respond to scarcity of security by increasing focus and effort in school tasks, since doing well in school will result in a future with less violence. In addition, low academic performance is likely to be a driver of violence from teachers, parents or peers. Performing better at school could be a way to avoid being the victim. Recalling VE during the survey which also took place at school might have triggered the same psychological responses, leading to increased effort and attention at the task at hand. In contrast, it is unlikely that the same psychological responses operate with adults.

3.8 Conclusion

Through a lab-in-the-field experiment with primary school students in El Salvador, a country ravaged by violence, I investigate if recalling VE has an impact on cognitive performance. I find that recalling VE before taking cognitive tests, improves performance by 0.2 standard deviations,

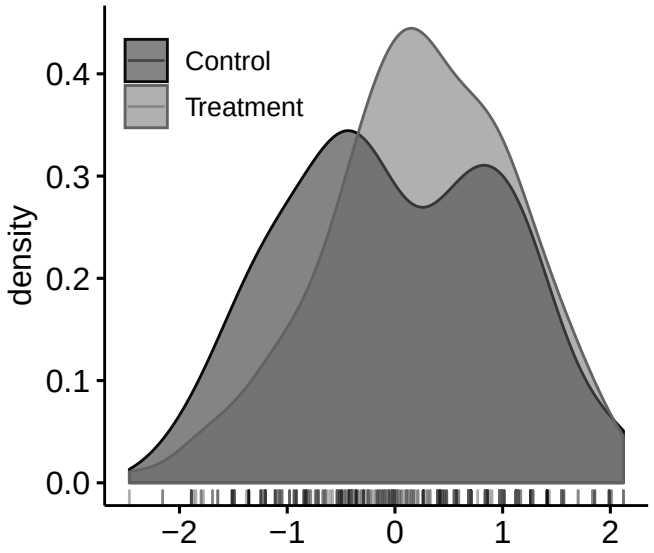
effect significantly stronger for students reporting higher VE. The data does not allow to inform on the potential channels but I can rule out increased fear and grit as potential mediators.One important contribution of the paper is methodological. I find that asking children to report sensitive information early in the survey leads to under-reporting.

The paper has several limitations. Firstly, further research is needed to understand if recalling VE has the same effects as experiencing it. Looking back at traumatic VE may trigger positive emotions such as pride of being able to overcome obstacles. Experiencing trauma however, at least on the short term, may trigger different responses. Secondly, we do not know how lasting the effects are. Sharkey et al. (2012) find their effect to dissipate after 7 days. One important question is if such potentially short term effects can impact academic achievement.

Appendix

3.A Appendix A - Additional Results and Robustness Checks

Figure A-1: Cognitive performance kernel densities by treatment



Notes: Treatment/control indicates participants randomly assigned to respond to the violence exposure survey section before/after taking the cognitive tests.

Table A-1: Treatment effects of recalling violence exposure on cognitive skills including interactions with noncognitive skills, SES, teacher support, parenting and social support

	Cognitive Skills				
	(1)	(2)	(3)	(4)	(5)
Treated	0.25**	0.25**	0.24**	0.26**	0.25**
	(0.10)	(0.10)	(0.10)	(0.10)	(0.11)
VE	−0.16**	−0.12	−0.13*	−0.15*	−0.12
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
Treated × VE	0.25**	0.20*	0.22**	0.24**	0.20*
	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)
Treated × Noncognitive skills	0.13				
	(0.11)				
Treated × SES index		0.00			
		(0.10)			
Treated × Teacher support index			0.14		
			(0.11)		
Treated × Parenting index				0.21**	
				(0.10)	
Treated × Social support index					−0.01
					(0.13)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	299	299	299	299	299
Adjusted R2	0.16	0.15	0.16	0.17	0.15

Note: All estimates are obtained via OLS regressions. Robust standard errors in parentheses. *, **, *** denote significance at the 10%, 5% and 1% levels respectively. All models control for individual and household characteristics - age, gender, SES index, teacher support index, social-support index and parenting index. The outcome variable in all models is the index of cognitive performance, computed through inverse covariance weighting, standardized using the control group mean and standard deviation.

Table A-2: Treatment effects of recalling violence exposure on cognitive skills computed by averaging the z scores of the two tests

	All			Lower VE	Higher VE	All
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.24**	0.24**	0.25**	0.13	0.37**	0.24**
	(0.11)	(0.11)	(0.11)	(0.17)	(0.15)	(0.10)
VE			−0.12			
			(0.08)			
Treated × VE			0.20*			
			(0.11)			
School VE						−0.15*
						(0.08)
Neighborhood VE						0.02
						(0.08)
Treated × School VE						0.20*
						(0.12)
Treated × Neighborhood VE						0.01
						(0.11)
Controls	No	Yes	Yes	Yes	Yes	Yes
Observations	299	299	299	150	149	299
Adjusted R2	0.01	0.12	0.12	0.08	0.14	0.16

Note: All estimates are obtained via OLS regressions. The outcome variable in all models is computed by averaging the Raven and Stroop tests z scores using the control group mean and standard deviation. Robust standard errors in parentheses. *, **, *** denote significance at the 10%, 5% and 1% levels respectively. Constrols include individual and household characteristics - age, gender, SES index, teacher support index, social-support index and parenting index. VE denotes the index of violence exposure computed through inverse covariance weighting, standardized using the control group mean and standard deviation. School and neighborhood VE are computed through the same method using only the their respective subscales. The sample used in the analysis either contains all observations (columns (1)-(3) and (6)), only participants with VE bellow the median (column (4)) or only participants with VE above the median (column (5)).

Table A-3: Number of non-responses in the violence section by treatment status

	VE	School VE	Neighborhood VE
	(1)	(2)	(3)
Treated	−0.01	−0.00	0.00
	(0.03)	(0.01)	(0.03)
Observations	299	299	299
Adjusted R2	-0.00	-0.00	-0.00

Note: All estimates are obtained via OLS regressions. The outcome variable is the number of items with non-response in the survey sections indicated in the column headers Robust standard errors in parentheses. *, **, *** denote significance at the 10%, 5% and 1% levels respectively.

Table A-4: Treatment effects of recalling violence exposure on cognitive skills

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	0.25**	0.21**	0.24**	0.22**	0.19*	0.21**	0.18*	0.07
	(0.10)	(0.10)	(0.10)	(0.11)	(0.10)	(0.10)	(0.10)	(0.10)
VE	−0.12	−0.14*	−0.13*	−0.15*	−0.14**	−0.09	−0.12	−0.17**
	(0.08)	(0.07)	(0.08)	(0.08)	(0.07)	(0.08)	(0.07)	(0.07)
Treated × VE	0.20*	0.21**	0.21**	0.23**	0.17*	0.15	0.14	0.19*
	(0.10)	(0.10)	(0.10)	(0.11)	(0.10)	(0.10)	(0.10)	(0.10)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Unbalanced controls	No	Yes	No	No	No	No	No	Yes
No Stroop Outlier	No	No	Yes	No	No	No	No	Yes
Listwise Deletion VE	No	No	No	Yes	No	No	No	Yes
Enumerator FE	No	No	No	No	Yes	No	Yes	Yes
School FE	No	No	No	No	No	Yes	Yes	Yes
Observations	299	299	287	276	299	299	299	265
Adjusted R2	0.16	0.19	0.18	0.15	0.18	0.21	0.22	0.30

Note: All estimates are obtained via OLS regressions. Robust standard errors in parentheses. *, **, *** denote significance at the 10%, 5% and 1% levels respectively. All models control for individual and household characteristics - age, gender, SES index, teacher support index, social-support index and parenting index. The outcome variable in all models is the index of cognitive performance, computed through inverse covariance weighting, standardized using the control group mean and standard deviation. VE denotes the index of violence exposure computed through inverse covariance weighting, standardized using the control group mean and standard deviation. Column (1) reports the results of the main analysis for reference. The unbalanced covariates included in the model reported in column (2) are number of children in the household and whether the household has a bathroom in the house. The model in column (3) excludes students with Stroop test bellow 8. The model in column (4) excludes students who had any items with non-response in the violence exposure section.

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