



# The preoccupied parent<sup>☆</sup>

## How financial concerns affect child investment choices

Sergiu Burlacu<sup>a</sup>, Anandi Mani<sup>b</sup>, Piero Ronzani<sup>c,\*</sup>, Lucia Savadori<sup>d</sup>

<sup>a</sup> FBK-IRVAPP, Italy

<sup>b</sup> Blavatnik School of Government, University of Oxford, United Kingdom

<sup>c</sup> International Security and Development Center, Berlin, Germany

<sup>d</sup> University of Trento, Italy

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### ABSTRACT

We examine the hypothesis that low parental investments in children may be a consequence of the adverse cognitive effects of poverty: financial worries preoccupy low-income parents with immediate concerns, shifting their attention away from stimulating parenting tasks. We test this hypothesis in an online experiment studying the purchase decisions of UK parents, including their responsiveness to financial subsidies for child development products. Both low and higher-income parents respond to a subsidy on such products, increasing their demands. However, when primed with financial worries under the same budget, low-income parents respond less to the subsidy, prioritizing instead the purchase of products addressing immediate household needs. This lower responsiveness to subsidies appears to be driven by worried parents further away from their last payday. Stronger safety nets and better alignment of financial subsidies to payday cycles may help achieve more investment and better child outcomes in poorer families.

### 1. Introduction

Being born into a poor family is a strong predictor of low human capital in a child's early years, affecting outcomes throughout life (Garces et al., 2002). No doubt, access to fewer learning resources and the lower quality of the living environment play a role in these outcomes. Policy responses to such disadvantages faced by poor children have been two-fold: better information and financial incentives for low-income families. However, neither approach has yielded consistent success, as evidence on the effectiveness of early childhood interventions and education subsidies suggests. Early childhood interventions that provide better information and guidance on parenting practices do improve outcomes (Heckman et al., 2013), but even in the best of

programs the effects on parental engagement often fade out when programs end (Gennetian et al., 2017). Equally, favorable effects of financial subsidies for children's education have been undermined because parents were found to cut back on their own expenditure on school inputs, reallocating resources to other household goods instead (Das et al., 2013).

Are such scant allocations of resources, time and attention to children by poorer parents carefully considered choices? Do these choices perpetuate poverty and render policy solutions ineffective or does poverty somehow push parents towards such choices? In this paper, we examine evidence for a hypothesis that suggests it may be the

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\* Corresponding author.

E-mail address: [ronzani@isdc.org](mailto:ronzani@isdc.org) (P. Ronzani).

latter: that it is the mental burden imposed by poverty itself that drives parenting choices and diminished attention to children.

Such a view draws on the recent literature on “scarcity”, which documents how financial worries under poverty deplete mental bandwidth (Mullainathan & Shafir, 2013). When a person is one missed rental payment away from eviction and has to juggle mounting expenses with irregular incomes, focusing on issues beyond pressing immediate needs becomes challenging. Less urgent (even if very important) everyday actions such as talking or reading to children or keeping track of their school work can get relegated to the periphery of attention (Evans, 2004; McLoyd, 1998). The cumulative impact of recurring crises on such everyday parenting choices could be considerable.<sup>1</sup>

This paper examines how such psychological effects of limited resources affect parents’ household budget allocation choices. We explore the hypothesis that financial worries may limit poorer parents’ attention to only the most immediate priorities, hence skewing their allocation decisions. In an online experiment with UK parents, we examine how financial worries affect parents’ budget allocation across immediate versus longer-term priorities, and their responsiveness to financial subsidies for the latter.

Our experiment used a cross-cutting design that sequentially combined a psychological intervention triggering financial worries with an economic intervention offering financial subsidies. For the first intervention, we used a psychological technique referred to as *priming*, commonly used in recent work in economics (Mani et al., 2013; Steele & Aronson, 1995). As in the latter paper, we asked richer and poorer parents how they would cope with hypothetical financial demands associated with everyday scenarios, in this case, in British life. Given that the same everyday scenarios are more financially challenging for the poor (Shah et al., 2018), such worries have been found to deplete the mental bandwidth of this group, more so than among better-off households. To examine whether the mental burden of these financial worries affects parents’ choices, we then asked them to allocate a budget of £30 in an experimental market across product categories reflecting immediate versus longer-term priorities. The economic intervention, embedded in this task, consisted of a 50% discount on items that fulfill a longer-term priority, child development products. Their choices were weakly incentivized, 1 out of every 100 parents received, in addition to the participation fee, the products chosen in the experiment.

Examining the purchase decisions of lower versus higher-income British parents across these different experimental treatments, we find some striking results.<sup>2</sup> When offered the 50% subsidy (discount) under easier financial scenarios, both poorer and richer parents respond positively, demanding more books and games for their children. In fact, lower-income parents are highly responsive, doubling their demand for such products. However, under hard financial scenarios, they become less responsive to the subsidy offered for child investment goods. Instead, they reallocate more of their budget to immediate family needs (groceries). In contrast, higher-income parents take equal advantage of the subsidy under easy and hard financial scenarios. In the absence of financial subsidies, we do not find exposure to hard financial scenarios to affect the budget allocations of either income group.

When a family’s finances are a tightrope walk, routine dips in income, for instance, closer to the end of a payday cycle, can feel quite challenging.<sup>3</sup> We use the variation in the time elapsed since the parents’

last paycheck as a proxy for financial strain. This allows us to investigate whether our results vary with the intensity of financial strain experienced by parents at the time of completing the experiment. While we acknowledge that the analysis is under-powered, we find evidence suggesting this to be the case: under harder financial scenarios, lower-income parents further away from their last payday appear to become less responsive to the child investment subsidy. In contrast, those who just got paid take as much advantage of it as parents of higher-income do.<sup>4</sup> Both sets of results suggest that financial worries capture (or “tunnel”) poorer parents’ attention towards immediate family needs, making them less responsive to subsidies for important longer-term priorities (child development). These behaviors accord well with the cognitive mechanism highlighted by the “scarcity” hypothesis: financial scenarios have no impact on poorer parents’ actual resources, but they influence their choices by making them *feel* poorer.

Our work contributes to the literature in three areas. First, we bring new insights to the literature on child development and human capital, on how cognitive demands of poverty explain low and erratic parental investment in children (Attanasio, 2015; Heckman, 2006; Heckman et al., 2006). Second, we add to the literature on under-reaction to taxes (and subsidies) by highlighting a scarcity-based mechanism as a cause. Existing explanations for this under-reaction include a lack of salience (or visibility) of taxes (Chetty et al., 2009), the complexity of tax rules (Miller & Mumford, 2015) and confusion (Feldman et al., 2016). Third, we contribute to the literature on the impact of scarcity on real-life decisions (Burlacu et al., 2022; Dalton et al., 2020; Kaur et al., 2021; Madeira et al., 2020), providing evidence on the important domain of parenting.<sup>5</sup> We note that our results on poorer parents’ purchases are not inconsistent with recent work that finds such consumers to be *less* susceptible to biases such as (lack of) tax salience (Goldin & Homonoff, 2013) or framing (Shah et al., 2015). While the poor may focus better on urgent priorities, the subsidy offered in our study is on products that fulfill a non-urgent goal (child learning).

Our findings point to three policy alternatives to encourage greater parental investment in children’s learning among poorer parents. First, better safety nets reducing financial stress may change how effective programs are at increasing parental engagement. Second, our payday results suggest that aligning the provision of child investment subsidies to payday cycles may induce greater responsiveness. A third approach could be to supplement financial subsidies with non-financial behavioral tools and nudges (such as text reminders, goal-setting, and social rewards) (Bettinger et al., 2021; Gennetian et al., 2017; Mayer et al., 2019; York et al., 2018).

## 2. Experimental setting

### 2.1. Experimental setting

We conducted our study in the United Kingdom (UK) in June 2018, using an experimental platform called Prolific (Palan & Schitter, 2017).<sup>6</sup> Given our interest in decision-making by poor parents in the UK, our eligibility criteria for participants were that they should (i) be UK residents (ii) with an annual household income below £50,000

<sup>1</sup> For instance, evidence shows that slower growth in the productive vocabulary of two-year-old children over time is largely accounted for by more limited conversation initiated by their mothers (Hoff, 2003).

<sup>2</sup> The income categories were split based on the median income for the UK in 2018 adjusted for household size (dividing by its square root).

<sup>3</sup> For instance, 52% of Britons with family responsibilities reported being unable to make ends meet for a full month (Office of National Statistics, 2019). In the US too, there is evidence of greater financial pressure before payday in poorer neighborhoods: worse nutrition (Gennetian et al., 2013) and more crime (Carr & Packham, 2019; Foley, 2011).

<sup>4</sup> Such shifts in parents’ priorities and attention away from their children because of financial pressures before payday may partly explain the 40% higher incidence of disciplinary events at school among children of US food stamp recipients at the end of the month (Gennetian et al., 2013).

<sup>5</sup> The first and the third papers cited examine impacts on productivity and consumption of temptation goods, while the last paper also studies parental behavior. It documents the inefficiency of parental expenditure on a *specific* product (text reminders to talk to children about schooling), while we track resource (re)allocation across competing family priorities.

<sup>6</sup> 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).

and (iii) a child under 4 years of age. Restricting household income to below £50,000 meant that we oversampled people from the lower end of the UK household income spectrum.<sup>7</sup> We picked the 0–4 age group for children because (i) it is widely emphasized as the key period in a child's development and (ii) parents' behavior has a larger influence on this process since children spend more time with them in this pre-formal-schooling phase.

The study was pre-registered on the American Economic Association's registry for randomized controlled trials (ID number AEARCTR-0003026). The experimental protocol was approved by the ethical committee of the University of Trento (003–2018).

## 2.2. Experimental design

Our aim is to understand how financial worries affect the resource allocations of poorer parents. Our hypothesis is that such an effect occurs because worries affect a parent's perception of trade-offs between immediate versus longer-term family priorities. To test this hypothesis, our experiment examines parents' online purchase decisions across three types of goods: those that cater to immediate household needs, to children's longer-term (cognitive and emotional) development and to parents' own short-run well-being. We also examine parents' responsiveness to financial subsidies used to encourage investment in children.

Our experiment adopts a  $2 \times 2$  design using two distinct treatments, one psychological and the other economic. First, equal shares of participants are randomly exposed to 'easy' versus 'hard' hypothetical scenarios, of which the latter are aimed to trigger greater financial worries. Following this, a 50% discount on the price of child investment goods is randomly offered to half the participants. The discount represents a common type of financial subsidy that poor parents may be offered to encourage increasing inputs for their child's development.

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

1. **Easy Baseline (EB)**: exposed to easy financial scenarios and baseline prices.
2. **Hard Baseline (HB)**: exposed to hard financial scenarios and baseline prices.
3. **Easy Discount (ED)**: exposed to easy financial scenarios and 50% discount on child investment goods.
4. **Hard Discount (HD)**: exposed to hard financial scenarios and 50% discount on child investment goods.

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

**Financial Scenarios:** Parents were asked how they would cope with each of 3 hypothetical scenarios, presented in random order, involving shocks to real income. Two of these shocks were adapted from [Mani et al. \(2013\)](#) and the third shock was one that specifically pertained to parents. The first scenario described a drop in real income due to higher prices of basic necessities; the second one described an unanticipated one-time expense; the final scenario described an increase in the cost of childcare in the UK. What differed between Hard ('Hard Baseline' and 'Hard Discount') versus Easy ('Easy Baseline' and 'Easy Discount') scenarios was the severity of the income shocks with which participants were presented. The detailed scenarios, complete instructions and sample screenshots of the experimental market are available in [Appendix B](#).

Parents responded to each of these scenarios with free-form and closed-form questions on how they would cope with each scenario.

<sup>7</sup> We acknowledge that the online experiments might exclude certain relevant categories of people, such as those with lower digital skills, the less informed or those close to the lower ends of the income distribution.

After their exposure to all three, they also answered two questions on how worried they were about (i) their current financial condition and (ii) being able to find money for an unanticipated expense, for which the answer options were coded using Likert scales. Responses to these questions serve as outcomes to check if our experimental manipulation actually triggered higher financial worries among participants exposed to Hard scenarios.

**Household Purchases Task:** Upon completing their responses to the three financial scenarios and the manipulation check, parents were presented with the main task of interest. They were asked to allocate an experimental budget of £30 across three types of goods: (i) educational goods for children (ii) groceries and (iii) luxuries (i.e. treats for themselves).<sup>8</sup> These product categories reflect parental priorities with different time horizons: educational goods serve a long-term priority (child development), groceries satisfy immediate family needs while non-essential items (luxuries) are catered to a parent's own short-term utility.

(a) *Child Investment goods:* This category included goods such as story and activity books and games that required (rather than substituted for) parental engagement. We chose only products parents were very familiar with, hence their value for their child's development was clear. Prices ranged from £3.5–£5.5 for most books and £4–£10 for the educational games. We aimed to capture both material and time investments, all products requiring the involvement of the parent and could not easily be enjoyed by the child in the absence of the parent.

*Discount/Financial Subsidy:* Half the participants received a 50% discount (subsidy) on all child investment goods. In the task instructions, they were informed that some of the goods have a 50% discount on top of the retail price. On the screen, they saw the baseline price crossed out and the discounted price written right next to it. In other words, the information on the discount (financial subsidy) was prominently displayed on the screen, in a format that shoppers typically view such information (see [Figs. B.1](#) and [B.2](#) in [Appendix B](#) for sample screenshots). This allows us to minimize the (lack of) information salience ([Chetty et al., 2009](#)) about the subsidy being a factor influencing purchase decisions.

(b) *Groceries:* In this category, participants could choose from a wide selection of goods, including food and cooking supplies (e.g. cereals, meats, milk, cooking oils etc.) as well as cleaning and hygiene products from one of UK's major retailers. We chose brands that were commonly purchased by online customers of this retail store, with prices ranging from less than £1 to £6.

(c) *Luxuries/Non-essential items:* Here, we offered branded coffee, perfumes, sun glasses and some other products that parents could exclusively consume. The prices for these goods were higher, ranging from £5.5 to £27, which allowed us to examine how financial pressures affect parents' susceptibility to (costly) temptations. Due to ethical consideration, we could not include goods most typically associated with temptation such as tobacco and alcohol.

In total, participants were offered 66 different items across the three product categories, presented in a typical online retail store format. The order of product categories (rather than of individual goods) was randomized across participants, to avoid high search costs. Overall, the product range, the online retail store format and use of actual market prices all served to make the experimental task very realistic. Furthermore, participants were also incentivized to take the task seriously with a 1% chance of receiving the actual goods they chose (following the

<sup>8</sup> To avoid the trouble of spending exactly £30 which would require cognitive effort and may result in the choice of some products just to reach the required value, we allowed a lower limit of £28 to proceed further with the survey. The remainder from £30 was transferred as a bonus payment on Prolific.

**Table 1**  
Descriptive statistics and Balance Checks.

	N (1)	EB (2)	HB (3)	ED (4)	HD (5)	p-value (6)
Child gender (Male)	349	0.43	0.53	0.44	0.50	0.56
Age in months	349	21.31	23.00	22.84	23.80	0.43
No formal childcare	349	0.46	0.41	0.45	0.44	0.92
Any sibling below 4	349	0.35	0.33	0.36	0.34	0.97
Age of parent	349	30.92	31.99	31.74	31.39	0.54
Gender of parent (male)	349	0.09	0.14	0.24	0.23	0.02
Completed higher education	349	0.51	0.45	0.47	0.55	0.60
Student	349	0.12	0.06	0.03	0.05	0.08
Nationality UK	349	0.89	0.95	0.90	0.92	0.47
Country of birth UK	349	0.88	0.93	0.90	0.88	0.75
Language English	349	0.93	0.95	0.94	0.93	0.93
Household size	349	3.98	3.65	3.97	3.80	0.20
Number of children	349	2.02	1.79	2.02	1.88	0.43
Spouse or cohabiting partner	349	0.89	0.88	0.88	0.92	0.81
Yearly income per adult equivalent	349	14.27	14.73	15.06	15.48	0.65
Yearly household income	349	27.85	27.78	29.17	29.72	0.66
Material Deprivation	349	-0.03	0.07	0.04	-0.05	0.56
Perceived SES (1-10 ladder)	349	4.81	4.76	4.76	4.92	0.91
Parent is employed	349	0.53	0.56	0.64	0.64	0.30
Spouse is employed	311	0.81	0.86	0.86	0.82	0.84
Any payments received in past month	349	0.61	0.71	0.67	0.58	0.27
Any payments received in past month (spouse)	311	0.59	0.77	0.70	0.54	0.02
Days since last payment	257	12.50	11.48	13.47	13.33	0.66
Has a credit card	349	0.68	0.74	0.64	0.69	0.61

Note: Column 1 indicates the number of observations. Columns 2–5 show the means across treatment arms. Column 6 displays the p-values associated with the F test of joint orthogonality across treatment arms. EB, HB, ED and HD indicate participants exposed to the four treatment arms: the first letter indicates exposure to easy (E) or hard (H) financial scenarios, while the second letter indicates being offered child investment products at baseline prices (B) or discounted by 50% (D). Yearly income per adult equivalent is computed by dividing total yearly household income by the square root of the household size. Material Deprivation is computed by averaging and standardizing the incidence of several income shocks in the previous year (not able to pay bills, rent, to afford heating, skipped meals, took loans from payday lenders, sought financial help from family or friends).

experimental design in [Carvalho et al., 2016](#)).<sup>9</sup> They also received a participation fee equal to the hourly UK minimum wage.

### Interpretation of Experiment Outcome Differences across Treatments

The difference in outcomes between the first two treatments (Easy Baseline versus Hard Baseline) aimed to capture how financial worries affect parents' investment in their child's development, relative to other basic household needs and items for their personal enjoyment. The difference between the treatments EB and ED (Easy Baseline versus Easy Discount) captures the effect of being offered the 50% discount on parents' investment in the child's development, testing their responsiveness to price discounts (or price elasticity of demand). Finally, comparing the last two groups (Easy Discount versus Hard Discount) captures how parents respond to financial incentives (i.e. discounts) that encourage them to invest in their child's development, when under greater financial worries.

**Further Considerations:** A few additional concerns come to mind, with regard to the experimental design outlined above. One potential concern is that parents' purchase decisions may not reflect their true preferences, either if there is scope to reallocate their expenditure budget outside of the experiment, or if there are opportunities for resale of products purchased within the experiment. However, such concerns would not explain differences in outcomes across different experimental treatments. Another concern regards the educational goods, which as mentioned previously, require parental time inputs. While we consciously chose these as proxy for parents' engagement with their child, we do not observe their actual actions after the purchase. In this sense, their purchase may reflect only their intention to engage with their child rather than their actual engagement. However, such a concern about the distinction between use and intent to use could

<sup>9</sup> To ensure high levels of privacy, we delivered the goods to a retail store chosen by the parents. The goods could be picked up in time slots selected by the parents using a code, thus not requiring any form of personal identification.

equally be applied to other goods not requiring a time input that are purchased in any experiment. Finally, parents could substitute purchasing the educational goods offered in the experiment with higher time investments in daily life. While possible, empirical evidence indicates that financial strain is associated with both lower time and material investments. ([Attanasio et al., 2022](#))

### 3. Data description

A sample of 349 participants on Prolific's website that met the eligibility criteria specified at the beginning of Section 2.1 participated in the experiment.<sup>10</sup> The survey administered by the platform at the time of registration (i.e. when participants joined Prolific) provided us with a rich set of demographic characteristics. Information on participants' allocation choices, financial situation and additional demographic characteristics (e.g. updated household income, days passed since the last payday) were obtained from the experimental task and a short survey that followed.

**Table 1** presents data on key participant characteristics and balance checks across treatment arms. The median and mean annual household income have similar values ( $\approx$ £28,000), likely due to the fact that we capped our participant pool at an annual income of £50,000.<sup>11</sup> On

<sup>10</sup> The target number of participants was 800, as reported in the pre-registration. While on Prolific the number of eligible parents was above 1000, it turned out that only a small share of them was in fact active. We kept the experiment open for several days and were constrained to stop the data collection when the number of daily submissions dropped close to 0.

<sup>11</sup> According to the Office for National Statistics (ONS) for 2018/19, the median and mean values of household income per adult equivalent using the modified OECD scale in the UK are £29,600 and £36,000, respectively. The values are larger than what we estimate for our sample using the same equivalence scale: roughly £20,800 and 21,300 pounds for the median and mean, respectively.

average, households have 3.9 members, including 1.9 children. We note the unexpected high share of mothers participating in the study.<sup>12</sup>

The last column of Table 1 displays the p-values associated with the F-test for the joint equality of averages across the treatment arms. Out of the 22 variables considered, three have differences with associated p-values below 10% or 5%: the percentage of fathers (lower in the Easy and Hard Baseline conditions,<sup>13</sup>) of students (higher in the Easy Baseline compared to the other treatments) and of spouses who received wages in the past month (higher in the Hard Baseline and Easy Discount conditions).

Given that the main results of the paper are reported by income subgroups, we also report balance checks separately for the lower and the higher income groups in Table A.1 in Appendix A. As in Mani et al. (2013) or Shah et al. (2012), we divide the sample based on a median split on total household income using the OECD square root equivalence scale, dividing total yearly household income by the square root of household size (OECD, 2011; Rights & Unit, 2008).<sup>14</sup> The imbalances appear to be more pronounced within the higher-income group than within the lower-income group. Nonetheless, to limit their role and improve the precision of our estimates, we include controls for the variables reported in Table 1 in our analyses.

*Selective attrition* can be of particular concern in online experiments, given lower social and psychological dropping-out costs than in lab experiments (Horton et al., 2011; Zhou & Fishbach, 2016). Fortunately, we find no evidence of selective attrition by treatment status (see Table A.2). The 11% attrition rate (39 participants), defined as the share of participants not completing the survey, is remarkably low for an online experiment (Zhou & Fishbach, 2016) and does not differ across treatment arms. These differences are even smaller if we account for attrition due to technical incompatibilities of participants' browsers.<sup>15</sup>

#### 4. Results: How financial concerns affect parents' household purchase decisions

We begin this section by estimating the effect of our experimental treatments on participants' worries about money, separately by income subgroups (Section 4.1). Section 4.2 then presents our main results on parental purchase decisions. Section 4.3 describes the heterogeneity of treatment effects, to further articulate the key mechanism of interest.

<sup>12</sup> At the time of the study, Prolific did not offer the possibility to request representative samples with respect to pre-selected characteristics (such as gender). Participation in the study operated on a first come, first-served basis.

<sup>13</sup> The gender of the parent is not correlated with the outcome variables in our sample. In all analyses we control for gender to reduce the imbalances reported in Table 1. However, results are unchanged if omitting it from the models, as shown in Table B.8 in Appendix B.

<sup>14</sup> Based on calculations using the 2018/19 "Households below average income" (HBAI) statistics, the household poverty lines before housing costs for couples with children and singles with children are £302.4 and £231 per adult equivalent per week, respectively. While our data is not ideal in order to compute the OECD modified scale weights, we took a conservative approach (i.e. over-estimating sample income per adult equivalent: we attribute a weight of 0.2 to all children even if some may be of adult age) and computed for each household the income per adult equivalent using also this scale, in order to compare it with the poverty lines from HBAI. We estimate that 59% of our lower-income sub-sample is below the poverty line while almost all the rest are below 50% above the poverty line. Likely due to our sampling strategy (only households with income below £50,000) and the possible profile of parents active on Prolific we did get a lower income group which is poor or near poor.

<sup>15</sup> Participants were informed that the survey application was tested only on the five major browsers and that they should not participate if using other browsers. However, the platform did not allow us to screen out participants based on the browser or operating system used.

#### 4.1. Effects of hypothetical scenarios on financial worries

Two of our four treatments arms (Hard Baseline and Hard Discount) ask participants how they would respond to challenging financial scenarios. Our aim here was to trigger worries commonly experienced by poor parents in their everyday lives, in order to then examine how such worries affect their family resource allocation decisions. Participants' free-form responses on how they would cope with such shocks provide qualitative evidence that the scenarios were perceived to be hard to overcome, requiring difficult sacrifices.

Following their free-form responses, we also tried to quantify the effect of these treatments, by asking participants how worried they were about (i) their financial situation and (ii) not being able to find money for an unanticipated expense, on a scale from 0 to 3 (with 0 representing "not worried at all" to 3 representing "desperately worried"). Fig. 1 plots the response means with 95% confidence intervals for participants exposed to easy versus hard financial scenarios, split by the median of the sample total household income per adult equivalent (i.e. lower versus higher income subgroups).

As seen in Fig. 1, responses to both questions show a clear increase in worries among participants exposed to the hard financial scenarios relative to the easy scenarios, that is significant at the 1% level.<sup>16</sup> Although there is a rise in self-reported worries among both lower and higher income participants, the levels of worries are consistently higher for the lower income group. In fact, with regard to their financial situation, we note that the worry levels of lower-income participants not exposed to the hard financial scenarios are already close to those of higher-income participants exposed to them.

#### 4.2. Parental purchase decisions

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

$$Y_i = \beta_0 + \beta_1 HB_i + \beta_2 ED_i + \beta_3 HD_i + \theta + X_i' \gamma + \epsilon_i \quad (1)$$

where  $Y_i$  is the amount (in pounds) expressed at baseline (i.e. no discount) prices, allocated by parent  $i$  on goods of a particular type.  $HB_i$  indicates being exposed to the hard financial scenarios and offered no discount (Hard Baseline).  $ED_i$  and  $HD_i$  indicate being offered a 50% discount on child investment goods after being exposed to easy and hard scenarios, respectively (Easy Discount and Hard Discount). The Control (omitted) arm is exposure to easy financial scenarios with no discount (Easy Baseline). Note that  $\hat{\beta}_3$  indicates the difference in the outcomes between the Hard Discount condition and the Easy Baseline condition, therefore it should not be interpreted as an interaction term but as a separate treatment arm.  $\theta$  are fixed effects for the order in which the categories of goods appeared on participants' screens during the task, while  $X_i$  is a vector of the parent, child and household characteristics listed in Table 1.<sup>17</sup> Throughout the analysis we use robust standard errors.

Table 2 presents ordinary least squares results, separately for the lower and higher income groups (below and above median income per adult equivalent). The dependent variable in each column is the expenditure on one of the three types of goods in the household purchase

<sup>16</sup> Table A.3 in Appendix provides regression estimates for the impact of hard financial scenarios on worries across both income groups, controlling for child and household characteristics.

<sup>17</sup> The following variables from Table 1 were excluded due to almost being collinear: number of children (Pearson correlation coefficient with household size = 0.93); nationality and language — highly correlated with country of birth.

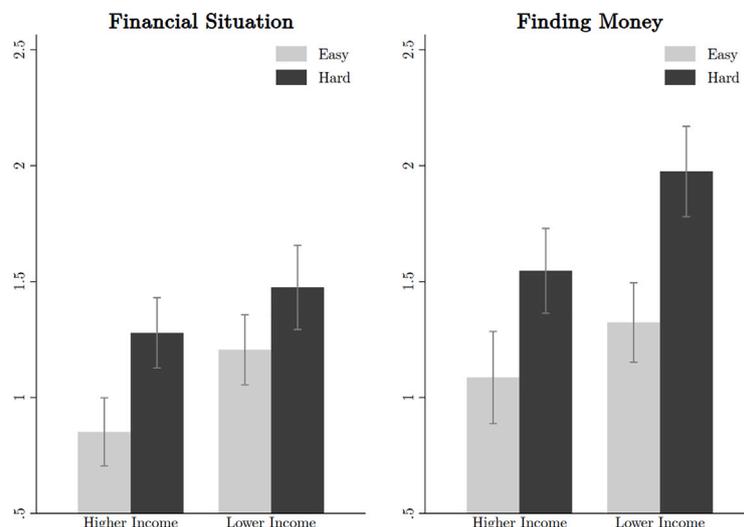


Fig. 1. Impact of Hard vs. Easy Hypothetical Scenarios on Financial Worries.

Notes: The outcome variables in the figures are responses to the question (left figure): "How worried do you feel about your financial situation?" and (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 at all", 1 "somewhat worried", 2 "very worried" and 3 "desperately worried". The bars indicate the means and the brackets indicate 95% confidence intervals. Easy/Hard indicates responses of individuals assigned to easy/hard financial scenarios respectively.

Table 2

Treatment effects on demand for child investment goods, groceries and luxury goods.

	Lower income			Higher income		
	(1) Investment	(2) Groceries	(3) Luxuries	(4) Investment	(5) Groceries	(6) Luxuries
Hard Baseline (HB)	-0.57 (1.93)	-0.44 (2.02)	1.11 (1.38)	3.14 (2.32)	-2.40 (2.33)	-1.19 (1.19)
Easy Discount (ED)	7.39 (2.73)	-1.57 (1.99)	0.71 (1.21)	5.38 (3.02)	0.44 (2.27)	0.0095 (1.29)
Hard Discount (HD)	1.90 (2.51)	3.79 (1.71)	-1.37 (0.84)	7.82 (2.54)	-2.74 (2.07)	1.61 (1.43)
EB Mean	7.14	20.00	1.95	6.40	20.59	2.13
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R <sup>2</sup>	0.04	0.07	0.01	0.02	0.07	0.14
Observations	182	182	182	167	167	167
<i>p-values - Tests:</i>						
<i>ED = HD</i>	0.078	0.005	0.053	0.454	0.166	0.301
<i>HB<sup>LI</sup> = HB<sup>HI</sup></i>	0.220	0.526	0.207			
<i>ED<sup>LI</sup> = ED<sup>HI</sup></i>	0.621	0.506	0.695			
<i>HD<sup>LI</sup> = HD<sup>HI</sup></i>	0.099	0.016	0.073			

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses. The outcome variables indicate expenditure on child investment goods, groceries and luxury goods, expressed in pounds at baseline prices. Higher/lower income groups denote the upper/lower 50% of the sample income per adult equivalent distribution. All models control for individual and household characteristics, and for the order in which the three types of goods appeared in the investment task. EB, HB, ED and HD indicate participants exposed to the four treatment arms: the first letter indicates exposure to easy (E) or hard (H) financial scenarios, while the second letter indicates being offered child investment products at baseline prices (B) or discounted by 50% (D). The first listed *p*-value (*ED = HD*) is associated with the t-test of equality of the Baseline Discount group and the Hard Discount group coefficients. The remaining *p*-values, listed with the superscript LI and HI, test the equality of the estimated treatments effects between the lower income group (LI) and the higher income group (HI) from models where each treatment assignment variable and each covariate is interacted with the income group dummy.

task, all expressed at baseline prices, thus the estimates reflect changes in demand.<sup>18</sup> Some striking patterns emerge in these expenditures.

We begin by discussing the findings for the lower-income group (Columns 1–3). In the absence of any change in prices, being exposed to hard financial scenarios does not have any impact on budget allocations. In part, this null result on demand for child investment products could be explained by floor effects: among lower-income parents in the Easy Baseline (EB) condition, 55% do not purchase any

child investment goods.<sup>19</sup> When exposed to easy hypothetical financial scenarios (Row 2), participants respond positively to discounts on educational (investment) goods for children. With a 50% discount they allocate over £7 more to child investment goods, an increase roughly equal to the control group’s entire spending on such goods (with no

<sup>18</sup> We estimate the equations for each of the three types of products as separate OLS regressions rather than as a Seemingly Unrelated regression (SUR). There is no efficiency gain from the latter approach in our context, since the set of regressors is identical across regressions (Wooldridge, 2013, chapter 16).

<sup>19</sup> Negative treatment effects cannot be observed for participants who, even in the absence of the treatment, would have invested nothing on these goods. In the other treatment arms, the shares are 44%, 32% and 54% in the Hard Baseline, Easy Discount and Hard Discount groups, respectively. For the higher-income group, the shares are 38.46% (Easy Baseline), 32.43% (Hard Baseline), 38.10% (Easy Discount), and 24.49% (Hard Discount).

discount).<sup>20</sup> There is no significant reallocation of the budget allocated to groceries or luxury goods. In contrast, lower-income participants become less-responsive to this same 50% discount (subsidy) if they are exposed to worry-inducing hard financial scenarios (Row 3). Their increased purchasing power due to the discount leads to an increase in demand for basic family needs (£3.79) and a reduction in demand for luxuries for themselves (£1.37), though the latter effect is not precisely estimated. The demand increase of £1.90 for educational (investment) goods is not statistically significant.

In addition to the changes in demand presented above (expenditure at baseline prices), we also explore changes in the shares, expressed in nominal terms (at experimental arms prices), of the £30 endowment allocated to each category of goods (see Table A.4 in Appendix A). Since they faced the same prices, again we observe no differences between the Hard Baseline and the Easy Baseline group. More interesting, also the discount when preceded by easy scenarios (Easy Discount) does not lead to a significant re-allocation of budget shares relative to the Easy Baseline group. This is because parents roughly double their demand for child investment goods in response to the 50% discount (thus keeping nominal expenditure constant), and do not re-allocate between the other two categories of goods. In contrast, being exposed to hard scenarios prior to being offered the discount does change nominal budget allocations. Though imprecisely estimated, we observe that parents spend about a third less of their budget on child investment products, almost cut down completely their allocation to luxuries and significantly increase the share of the budget allocated to groceries, an increase of 13 percentage points relative to the Easy Baseline mean of 67%.

Let us contrast the results presented above with the purchase behaviors of higher-income parents across treatments (Columns 4–6). As for the lower-income group, increasing financial worries while keeping prices fixed (Hard Baseline group, Row 1) does not lower demand for child investment goods. The value is actually higher by £3.14 though the effect is not precisely estimated ( $p$ -value = 0.179). In contrast to the lower income group, their response to the discount is similar irrespective of exposure to Easy versus Hard financial scenarios (i.e. Rows 2 and 3). In both cases, they spend more on child investment goods (by £5.38 and £7.82 respectively). Also, they do not change their expenditure on groceries or luxuries much. If anything, exposure to hard financial scenarios reduces their expenditure on groceries (by £2.74), but this is not statistically significant.

The contrast in the response to the discount of the lower versus higher income groups within the Hard Discount treatment is striking. It shows that financial worries can make poorer parents less-responsive to a financial subsidy for longer-term investments, because they trigger a *scarcity* mindset (Shah et al., 2015). Parents' attention is captured by immediate priorities (basic needs) over less urgent ones (even if important) such as their child's development. Higher-income parents remain responsive to subsidies likely because greater slack in their financial budget mitigates their worries.<sup>21</sup> Even though, as seen in Fig. 1, the priming did increase worries also among the higher-income group, it is unlikely that its effect on behavior is linear, i.e. unconditional on the baseline level of worries. In addition, the shocks described in the scenarios are more severe for the lower-income group as a proportion

of income. However, even if the presented shocks would have been proportional to income, it is unlikely they would have had the same effect on the higher-income group given that their expectations of actually incurring such shocks are likely much lower with respect to the lower-income group. As a result, it is less likely they would have actually triggered mental processes recurrent in daily life as in the case of the lower-income group.

#### 4.3. Heterogeneous effects by days since last payday

According to Britain's Office of National Statistics (ONS) Wealth and Assets Survey for the period April 2018–September 2019, 52% of Britons looking after family and home reported being unable to make ends meet for a full month. It is thus likely that people in our sample too experience greater financial strain further away from their last payday. As a further test of our hypothesis, we therefore examine how the impact of our experimental treatments differ by the time elapsed since participants' last payday. We acknowledge that the heterogeneity analysis is under-powered and results are only suggestive.<sup>22</sup> Nonetheless, the findings complement the main results and highlight a potentially important source of heterogeneity for future research in this area.<sup>23</sup>

We use a modified version of Eq. (1) as the specification. We introduce an additional variable, the number of days elapsed since the last paycheck received by the respondent or her/his spouse, and also include interaction terms with each of the treatment arms. We also add a fixed effect proxying whether the respondent or her/his spouse gets paid on a monthly basis as opposed to a more frequent payment cycle.<sup>24</sup>

Our results are reported in Table 3. The Easy Baseline condition remains the control condition, omitted from the regression. In this modified specification, the coefficient for the treatment dummy for any product category represents the average expenditure expressed at baseline prices (in pounds) on such products, of parents who participated on their payday. In any given column, the coefficient on the variable "Days since last payment" represents the change in expenditure on a specific product category for each additional day since the last payday, for parents in the Easy Baseline (control) condition. The interaction terms report the same change in expenditure effect under the other three treatments, relative to the Easy Baseline condition. Our sample sizes in this table are lower because some parents reported not having received any payments in the past month.<sup>25</sup>

<sup>22</sup> As mentioned previously, the effective sample size was lower than our objective (800) due to inactivity of parents in our target group on the platform.

<sup>23</sup> Two additional exploratory dimensions of mediation or heterogeneity pre-specified but dropped from the analyses are time preferences and loss aversion. Due to the fact that these dimensions were exploratory and the preferences estimates are noisy, especially considering the sample sizes lower than the initial target (raising additional power concerns for a heterogeneity analysis) we decided to remove them from the analyses.

<sup>24</sup> The indicator is computed based on the number of days between the last payday and the next expected payment. Values higher than 21 days are coded as monthly payments.

<sup>25</sup> We consider the possibility that parents reporting no payments interpreted the survey phrasing "payments in the past month" as referring to payments in the current calendar month and not the past 30 days (the experiment was conducted between the 21st and 25th of June, 2018). To account for the latter possibility, we assign several alternative values (25, 30, 35 and 40 days) for the 'days since last paycheck' variable for these parents, as a robustness check. Our results remain robust to this approach. Please refer to Appendix A, Table A.7 for these results. We note also that the variables reported in Table 1 are well balanced for this smaller sub-sample of lower-income participants too. The results are available in Tables B.9 and B.10 in Appendix B. Finally, we repeat the main analyses reported in Table 2 for the sub-sample reporting having received any payments in the past month. The results are available in Table B.11 in Appendix B. While less precisely estimated, the differences remain between the Easy Discount and Hard Discount conditions among the lower-income groups in terms of child investment goods and groceries.

<sup>20</sup> The estimated price elasticity of  $-2$  is in line with previous estimates of price elasticities for books as discussed in Ringstad and Løyland (2006). However, to our knowledge there are no studies estimating the price elasticity for educational games for children, the other type of goods in our basket of child investment goods.

<sup>21</sup> In Appendix A, Table A.5, we report a couple of robustness checks. First, we estimate the main model interacting the treatment with a continuous measure of income. Results remain qualitatively similar to those reported above, although precision is lower. Second, since the dependent variable is censored, we run the main analysis using a Tobit model as a robustness check. The results are qualitatively similar, see Table A.6 in Appendix A for details.

**Table 3**  
Heterogeneous treatment effects by days since last payment.

	Lower income			Higher income		
	(1) Investment	(2) Groceries	(3) Luxuries	(4) Investment	(5) Groceries	(6) Luxuries
Hard Baseline (HB)	-2.70 (2.54)	-0.90 (2.85)	3.90 (2.56)	3.58 (4.44)	-2.68 (4.57)	-1.50 (2.85)
Easy Discount (ED)	11.8 (4.36)	-8.19 (3.40)	4.48 (2.09)	10.9 (7.14)	0.91 (5.73)	-3.41 (3.41)
Hard Discount (HD)	11.0 (5.20)	-2.99 (3.06)	0.051 (1.18)	7.45 (7.04)	-2.16 (6.37)	1.87 (3.79)
Days since last payment (Pay)	-0.043 (0.22)	0.032 (0.20)	-0.0092 (0.12)	-0.10 (0.21)	0.20 (0.21)	-0.081 (0.10)
HB × Pay	0.29 (0.22)	-0.13 (0.24)	-0.14 (0.17)	-0.22 (0.28)	0.14 (0.28)	0.059 (0.15)
ED × Pay	-0.24 (0.29)	0.41 (0.24)	-0.27 (0.14)	-0.16 (0.36)	-0.13 (0.30)	0.22 (0.20)
HD × Pay	-0.61 (0.30)	0.35 (0.23)	-0.011 (0.12)	-0.038 (0.39)	0.040 (0.34)	-0.045 (0.18)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj. $R^2$	0.11	0.05	0.01	-0.04	-0.01	0.06
Observations	134	134	134	123	123	123
<i>p-values - Tests:</i>						
$ED = HD$	0.883	0.132	0.018	0.687	0.638	0.126
$ED \times Pay = HD \times Pay$	0.263	0.806	0.033	0.798	0.614	0.148
$HD_{LI} = HD_{HI}$	0.689	0.906	0.647			
$(HD \times Pay)_{LI} = (HD \times Pay)_{HI}$	0.246	0.457	0.877			

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses. The outcome variables indicate 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 and for the order in which the three types of goods appeared in the investment task. EB, HB, ED and HD indicate participants exposed to the four treatment arms: the first letter indicates exposure to easy (E) or hard (H) financial scenarios, while the second letter indicates being offered child investment products at baseline prices (B) or discounted by 50% (D). The first listed  $p$ -value ( $ED = HD$ ) is associated with the t-test of equality of the Baseline Discount group and the Hard Discount group coefficients for participants who participated in the study on their payday. The second listed  $p$ -value ( $ED \times Pay = HD \times Pay$ ) is associated to the t-test of equality of the coefficients on the Easy Discount group and the Hard Discount group interacted with the number of days since the last paycheck. The remaining  $p$ -values, listed with the superscript LI and HI, test the equality of the estimated treatments effects between the lower income group (LI) and the higher income group (HI) from models where each treatment assignment variable and each covariate is interacted with the income group dummy.

Columns 1 to 3 in Table 3 report our findings for the lower income group. Note the similar magnitudes of the coefficients for child investment goods under the Easy Discount and Hard Discount treatments (Rows 2 and 3, Column 1). They indicate that exposure to financial worries does not affect how lower income parents respond to the discount if they participated in the study on their payday, when they are less financially constrained. In the absence of worry triggers (i.e. in the Easy Discount treatment) lower income parents remain more responsive to discounts on child investment goods even further away from payday. They sacrifice fewer resources for their child's development (24 pence for each additional day) but appear to cut back on luxuries for themselves to buy more groceries. When under higher financial worries (Hard Discount condition) for each additional day since their last payday, parents cut back their spending on educational goods for their child by a higher amount (61 pence) and allocate 35 pence more to groceries instead, on average (Row 7, Columns 1 and 2).<sup>26</sup> Lower-income parents in both the Easy and Hard Discount treatments increase their spending on groceries for each additional day since their last payday. Their response is consistent with the financial tightening they naturally experience over the payday cycle, with or without the experimental worry prime. However, we find higher adverse effects on their spending on child products when under higher financial worries. In other words, it appears it may be the combination of financial strain and worries that make poorer parents less-responsive to financial subsidies for longer term (child development) priorities because their

<sup>26</sup> Since low-income parents in the Hard Discount treatment are, on average, 9.61 days away from their last payday (see Appendix Table A.2), this implies a reduction of £5.86 on child investment products and an increase of £3.36 on groceries within this group due to greater financial worries.

attention is captured by immediate needs (a phenomenon referred to as 'tunneling'). As for higher income parents their purchase decisions remain unaffected by the time since their last pay check (Columns 4 to 6).

The results in this sub-section lend further support to the conclusion that it is worries associated with tight financial constraints (rather than any unobserved attributes of lower income parents) that hinder their ability to prioritize their children's needs. It is this cognitive burden of poverty that makes them less-responsive to financial subsidies, rather than the (lack of) salience or complexity of the subsidies themselves. The above results for lower-income families in the UK also accord with documented evidence of worse outcomes in the days preceding payday (or welfare assistance) among residents of low income neighborhoods in the US: poorer cognitive function (Mani et al., 2020), more crime (Carr & Packham, 2019; Foley, 2011), less parental attention and a higher rate of disciplinary events among middle school children (Gennetian et al., 2013).

## 5. Discussion

In this paper, we have examined how worries from financial strain may distort parents' perceived trade-off between immediate family needs versus longer term ones like child development. We have shown evidence that such worries cause them to allocate more resources for immediate needs over child learning products, despite financial subsidies for the latter. Such everyday parenting choices correlate with lower skills among children of low-income households, which may perpetuate poverty across generations.

Our results highlight a distinct cognitive mechanism (i.e. worries under financial strain) lowering the responsiveness to financial subsidies among the poor. They suggest potential policy responses to

**Table A.1**  
Descriptive statistics and balance checks by income group.

	Lower income						Higher income					
	N	EB	HD	EB	HD	p-value	N	EB	HB	ED	HD	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Child gender (Male)	182	0.41	0.53	0.50	0.41	0.54	167	0.46	0.51	0.38	0.57	0.33
Age in months	182	22.22	24.00	22.70	22.16	0.83	167	19.95	21.84	22.98	25.04	0.14
No formal childcare	182	0.43	0.47	0.48	0.38	0.82	167	0.51	0.35	0.43	0.49	0.49
Any sibling below 4	182	0.36	0.35	0.34	0.35	1.00	167	0.33	0.30	0.38	0.33	0.89
Age of parent	182	31.41	31.37	31.59	30.31	0.75	167	30.18	32.70	31.90	32.20	0.07
Gender of parent (male)	182	0.09	0.14	0.18	0.22	0.32	167	0.10	0.14	0.31	0.24	0.07
Completed higher education	182	0.47	0.35	0.41	0.32	0.50	167	0.56	0.57	0.52	0.71	0.26
Student	182	0.16	0.07	0.07	0.08	0.39	167	0.08	0.05	0.00	0.02	0.25
Nationality UK	182	0.84	0.95	0.93	0.95	0.18	167	0.95	0.95	0.86	0.90	0.43
Country of birth UK	182	0.86	0.93	0.93	0.92	0.57	167	0.90	0.92	0.86	0.86	0.78
Language English	182	0.93	0.95	0.93	0.95	0.96	167	0.92	0.95	0.95	0.92	0.90
Household size	182	4.22	3.65	4.20	3.95	0.14	167	3.62	3.65	3.71	3.69	0.95
Number of children	182	2.31	1.88	2.25	2.03	0.30	167	1.59	1.68	1.79	1.78	0.72
Spouse or cohabiting partner	182	0.83	0.77	0.80	0.84	0.84	167	0.97	1.00	0.98	0.98	0.83
Yearly income	182	9.99	9.74	9.71	9.44	0.89	167	20.65	20.53	20.65	20.04	0.89
Yearly household income	182	20.33	18.30	19.62	18.64	0.48	167	39.03	38.80	39.18	38.10	0.93
Material Deprivation	182	0.07	0.29	0.30	0.15	0.28	167	-0.18	-0.19	-0.24	-0.19	0.91
Perceived SES (1-10 ladder)	182	4.66	4.42	4.36	4.41	0.82	167	5.05	5.16	5.17	5.31	0.86
Parent is employed	182	0.45	0.51	0.52	0.57	0.71	167	0.64	0.62	0.76	0.69	0.54
Spouse is employed	147	0.79	0.79	0.83	0.68	0.50	164	0.84	0.92	0.88	0.92	0.66
Any payment received in past month	182	0.55	0.74	0.66	0.59	0.23	167	0.69	0.68	0.69	0.57	0.57
Any payments received in past month (spouse)	147	0.52	0.73	0.69	0.58	0.22	164	0.68	0.81	0.71	0.52	0.04
Days since last payment	134	11.82	11.08	10.06	9.61	0.81	123	13.35	12.00	17.09	16.10	0.15
Has a credit card	182	0.59	0.63	0.59	0.57	0.96	167	0.82	0.86	0.69	0.78	0.27

Note: Columns 1 and 7 indicate the number of observations. Columns 2–5 and 8–11 show the means across treatment arms. Columns 6 and 12 displays the p-values associated with the F test of joint orthogonality across treatment arms. EB, HB, ED and HD indicate participants exposed to the four treatment arms: the first letter indicates exposure to easy (E) or hard (H) financial scenarios, while the second letter indicates being offered child investment products at baseline prices (B) or discounted by 50% (D). Yearly income per adult equivalent is computed by dividing total yearly household income by the square root of the household size. Material Deprivation is computed by averaging and standardizing the incidence of several income shocks in the previous year (not able to pay bills, rent, to afford heating, skipped meals, took loans from payday lenders, sought financial help from family or friends). The sample used in the analysis are participants declaring to have received any payments in the past month with yearly household income below the sample median in (Columns 2–6) and above the sample median (Columns 8–12).

**Table A.2**  
Attrition by treatment status.

	Completed the study	
	(1)	(2)
Hard Baseline (HB)	-0.011 (0.046)	0.029 (0.035)
Easy Discount (ED)	0.0059 (0.043)	-0.0089 (0.039)
Hard Discount (HD)	0.055 (0.039)	0.021 (0.035)
Easy Baseline (EB) Mean	0.89	0.92
Observations	387	374
Exclude page with technical error on some OS	No	Yes

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses. The outcome variable is 1 if the participant completed the experiment and 0 otherwise. EB, HB, ED and HD indicate participants exposed to the four treatment arms: the first letter indicates easy (E) or hard (H) financial scenarios, while the second letter indicates baseline (B) or 50% discounted (D) child investment products. Column 2 excludes participants who dropped out on a specific page of the experiment with technical errors due on some operating systems or browsers, coming after the scenarios and the main task. The app was tested on all 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.

mitigate such lower responsiveness: better alignment of the *timing* of such subsidies to parental income cycles, and the use of other *behavioral* interventions (such as setting and tracking goals, text reminders for activities with children and social rewards) to increase poor parents' engagement (Mayer et al., 2019). However, behavioral solutions may not always work, especially when prioritizing immediate needs may be or may seem like the efficient response to financial challenges. Social safety nets that make financial strain less cognitively burdensome may be needed to ensure better parenting outcomes for children from poorer families.

Further research is needed in order to pin down the precise mechanisms and devise cost-effective policy instruments which can increase

**Table A.3**  
Manipulation check — treatment effects on financial worries.

	(1)	(2)	(3)
	Financial situation	Finding money	Index
Hard	0.427 (0.105)	0.451 (0.134)	0.535 (0.128)
Lower income × Hard	-0.187 (0.150)	0.158 (0.180)	-0.0364 (0.177)
Controls	Yes	Yes	Yes
Adj. R <sup>2</sup>	0.126	0.161	0.168
Observations	349	349	349

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses. 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 at 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. Hard indicates participants exposed to hard scenarios, the baseline category being represented by participants expose to easy scenarios.

parental investments without having a negative impact on household finances. Our study is a one-shot experiment, whereas understanding dynamic effects is very relevant. For instance, financial worries may push lower income parents to procrastinate parental investments. For instance, parents may decide to postpone reading to the child today with the premise that skipping one day should not have any sizable effects on her/his development. However, given their financial vulnerability, skipped days may accumulate over time with life-long effects on child development. Or parents could postpone taking advantage of a given intervention (e.g. subsidy) and end up failing to ever benefit from it since in time it may shift out of attention. While priming financial worries in a longitudinal study is not feasible, alleviating worries

**Table A.4**

Treatment effects on the share of the 30 pounds endowment spent on child investment goods, groceries and luxury goods.

	Lower income			Higher income		
	(1) % investment	(2) % groceries	(3) % luxuries	(4) % investment	(5) % groceries	(6) % luxuries
Hard Baseline (HB)	-0.019 (0.058)	-0.015 (0.067)	0.037 (0.046)	0.12 (0.066)	-0.080 (0.078)	-0.040 (0.040)
Easy Discount (ED)	0.035 (0.057)	-0.052 (0.066)	0.024 (0.040)	-0.015 (0.064)	0.015 (0.076)	0.00032 (0.043)
Hard Discount (HD)	-0.080 (0.055)	0.13 (0.057)	-0.046 (0.028)	0.036 (0.056)	-0.091 (0.069)	0.054 (0.048)
EB Mean	0.24	0.67	0.06	0.21	0.69	0.07
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj. $R^2$	0.06	0.07	0.01	0.03	0.07	0.14
Observations	182	182	182	167	167	167
<i>p-values - Tests:</i>						
$ED = HD$	0.039	0.005	0.053	0.385	0.166	0.301
$HB^{LI} = HB^{HI}$	0.114	0.526	0.207			
$ED^{LI} = ED^{HI}$	0.563	0.506	0.695			
$HD^{LI} = HD^{HI}$	0.143	0.016	0.073			

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses. The outcome variables indicate the share of the £30 endowment allocated in nominal terms to child investment goods, groceries and luxury goods. Higher/lower income groups denote the upper/lower 50% of the sample income per adult equivalent distribution. All models control for individual and household characteristics, and for the order in which the three types of goods appeared in the investment task. EB, HB, ED and HD indicate participants exposed to the four treatment arms: the first letter indicates exposure to easy (E) or hard (H) financial scenarios, while the second letter indicates being offered child investment products at baseline prices (B) or discounted by 50% (D). The first listed  $p$ -value is associated with the t-test of equality of the Baseline Discount group and the Hard Discount group coefficients. The remaining  $p$ -values, listed with the superscript LI and HI, test the equality of the estimated treatments effects between the lower income group (LI) and the higher income group (HI) from models where each treatment assignment variable and each covariate is interacted with the income group dummy.

**Table A.5**

Treatment effects on demand for investment goods, groceries and luxury goods interacted with income.

	Investment (1)	Groceries (2)	Luxuries (3)
Hard Baseline (HB)	-0.097 (3.53)	-1.34 (3.76)	1.72 (2.45)
Easy Discount (ED)	7.51 (4.44)	-3.07 (2.56)	1.57 (2.61)
Hard Discount (HD)	-1.22 (4.41)	6.20 (3.43)	-2.75 (2.12)
Yearly income	-0.024 (0.15)	0.047 (0.17)	-0.037 (0.13)
HB × Yearly income	0.071 (0.23)	0.012 (0.24)	-0.11 (0.14)
ED × Yearly income	-0.12 (0.26)	0.17 (0.23)	-0.042 (0.16)
HD × Yearly income	0.37 (0.28)	-0.37 (0.23)	0.21 (0.14)
Controls	Yes	Yes	Yes
Adj. $R^2$	0.07	0.07	0.02
Observations	349	349	349

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses. The outcome variables indicate expenditure on child investment goods, groceries and luxury goods, expressed in pounds at baseline prices. Yearly income is total household income divided by the square root of the household size. All models control for individual and household characteristics, and for the order in which the three types of goods appeared in the investment task. EB, HB, ED and HD indicate participants exposed to the four treatment arms: the first letter indicates exposure to easy (E) or hard (H) financial scenarios, while the second letter indicates being offered child investment products at baseline prices (B) or discounted by 50% (D).

(e.g. through insurance against shocks or cash transfers) and observing changes in parental investments across time, including their responses to interventions such as subsidies, could be a fruitful direction for future research.

**Data availability**

Data available on OSF repository: [https://mfr.osf.io/render?url=https://osf.io/mfjzb/?view\\_only=1500e07cb8024255a6a26e8f26164982](https://mfr.osf.io/render?url=https://osf.io/mfjzb/?view_only=1500e07cb8024255a6a26e8f26164982).

**Appendix A**

See Tables A.1–A.7.

**Appendix B**

*B.1. Experimental task*

*B.1.1. 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.

- 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 you agree with the following statements? (4 item Likert: strongly disagree-strongly agree)
    - “Coming up with (£2000/£100) on a very short notice would cause me longlasting financial hardship”.
    - “Coming up with (£2000/£100) on a very short notice would require me to make sacrifices that have long-term consequences”.

- 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.).

**Table A.6**

Tobit regressions of treatment effects on demand for investment goods, groceries and luxury goods.

	Lower income			Higher income		
	(1) Investment	(2) Groceries	(3) Luxuries	(4) Investment	(5) Groceries	(6) Luxuries
Hard Baseline (HB)	3.36 (3.73)	0.53 (2.48)	5.42 (7.54)	4.52 (3.26)	-2.40 (2.79)	-10.6 (5.34)
Easy Discount (ED)	13.5 (4.00)	-1.84 (2.40)	6.23 (7.50)	4.66 (3.55)	0.85 (2.60)	-3.62 (5.14)
Hard Discount (HD)	3.09 (3.97)	4.65 (2.16)	-19.7 (9.06)	10.3 (3.15)	-3.61 (2.41)	2.07 (4.90)
EB Mean	7.14	20.00	1.95	6.40	20.59	2.13
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R <sup>2</sup>	0.04	0.03	0.12	0.04	0.03	0.14
Observations	182	182	182	167	167	167
<i>p-values - Tests:</i>						
$ED = HD$	0.013	0.008	0.014	0.105	0.089	0.190
$HB^{LI} = HB^{HI}$	0.742	0.430	0.057			
$ED^{LI} = ED^{HI}$	0.124	0.448	0.264			
$HD^{LI} = HD^{HI}$	0.109	0.011	0.048			

Note: Estimates are obtained via Tobit regressions with left and right censoring. Robust standard errors in parentheses. The outcome variables indicate 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, and for the order in which the three types of goods appeared in the investment task. EB, HB, ED and HD indicate participants exposed to the four treatment arms: the first letter indicates exposure to easy (E) or hard (H) financial scenarios, while the second letter indicates being offered child investment products at baseline prices (B) or discounted by 50% (D). The first listed *p*-value ( $ED = HD$ ) is associated with the t-test of equality of the Baseline Discount group and the Hard Discount group coefficients. The remaining *p*-values, listed with the superscript LI and HI, test the equality of the estimated treatments effects between the lower income group (LI) and the higher income group (HI) from models where each treatment assignment variable and each covariate is interacted with the income group dummy.

**Table A.7**

Sensitivity analysis: Heterogeneous treatment effects by days since last payment imputing the number of days since payment to participants reporting having received no payment in the last month.

	Lower income			Higher income		
	(1) Investment	(2) Groceries	(3) Luxuries	(4) Investment	(5) Groceries	(6) Luxuries
Value imputed = 25 days since last payment						
ED × Days since last payment	-0.33 (0.23)	0.42 (0.18)	-0.19 (0.12)	-0.19 (0.30)	-0.12 (0.25)	0.22 (0.16)
HD × Days since last payment	-0.58 (0.24)	0.39 (0.16)	-0.037 (0.080)	0.16 (0.31)	-0.19 (0.27)	0.099 (0.17)
p-value: $ED \times Pay = HD \times Pay$	0.385	0.848	0.136	0.362	0.799	0.496
p-value: $(HD \times Pay)_{LI} = (HD \times Pay)_{HI}$	0.060	0.066	0.471			
Value imputed = 30 days since last payment						
ED × Days since last payment	-0.29 (0.21)	0.37 (0.16)	-0.15 (0.11)	-0.23 (0.27)	-0.058 (0.22)	0.17 (0.14)
HD × Days since last payment	-0.50 (0.21)	0.34 (0.14)	-0.036 (0.070)	0.14 (0.26)	-0.21 (0.23)	0.13 (0.16)
p-value: $ED \times Pay = HD \times Pay$	0.427	0.885	0.202	0.261	0.540	0.796
p-value: $(HD \times Pay)_{LI} = (HD \times Pay)_{HI}$	0.056	0.039	0.333			
Value imputed = 35 days since last payment						
ED × Days since last payment	-0.26 (0.19)	0.31 (0.14)	-0.12 (0.097)	-0.24 (0.24)	-0.015 (0.20)	0.13 (0.12)
HD × Days since last payment	-0.42 (0.18)	0.30 (0.12)	-0.032 (0.061)	0.11 (0.21)	-0.20 (0.19)	0.14 (0.14)
p-value: $ED \times Pay = HD \times Pay$	0.463	0.920	0.273	0.201	0.376	0.958
p-value: $(HD \times Pay)_{LI} = (HD \times Pay)_{HI}$	0.058	0.026	0.262			
Value imputed = 40 days since last payment						
ED × Days since last payment	-0.23 (0.17)	0.27 (0.12)	-0.095 (0.087)	-0.24 (0.21)	0.013 (0.17)	0.10 (0.11)
HD × Days since last payment	-0.36 (0.16)	0.26 (0.11)	-0.028 (0.054)	0.086 (0.18)	-0.18 (0.16)	0.13 (0.12)
p-value: $ED \times Pay = HD \times Pay$	0.490	0.954	0.343	0.166	0.275	0.781
p-value: $(HD \times Pay)_{LI} = (HD \times Pay)_{HI}$	0.062	0.020	0.225			
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	182	182	182	167	167	167

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses. The outcome variables indicate 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 and for the order in which the three types of goods appeared in the investment task. EB, HB, ED and HD indicate participants exposed to the four treatment arms: the first letter indicates exposure to easy (E) or hard (H) financial scenarios, while the second letter indicates being offered child investment products at baseline prices (B) or discounted by 50% (D). The first listed *p*-value is associated to the t-test of equality of the coefficients on the Easy Discount group and the Hard Discount group interacted with the number of days since the last paycheck. The *p*-value with the subscripts LI and HI tests 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.

**Table B.8**

Treatment effects on demand for investment goods, groceries and luxury excluding gender from the list of covariates goods.

	Lower income			Higher income		
	(1) Investment	(2) Groceries	(3) Temptation	(4) Investment	(5) Groceries	(6) Temptation
Hard Baseline (HB)	-0.57 (1.93)	-0.43 (2.01)	1.07 (1.38)	2.94 (2.32)	-2.37 (2.33)	-1.14 (1.19)
Easy Discount (ED)	7.37*** (2.64)	-1.52 (1.98)	0.57 (1.25)	4.94* (2.93)	0.51 (2.18)	0.12 (1.29)
Hard Discount (HD)	1.88 (2.46)	3.84** (1.69)	-1.49* (0.87)	7.27*** (2.41)	-2.64 (2.01)	1.75 (1.41)
EB Mean	7.14	20.00	1.95	6.40	20.59	2.13
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R <sup>2</sup>	0.05	0.08	0.01	0.02	0.07	0.15
Observations	182	182	182	167	167	167
<i>p-values - Tests:</i>						
$ED = HD$	0.077	0.005	0.054	0.476	0.169	0.292
$HB^{LI} = HB^{HI}$	0.243	0.528	0.226			
$ED^{LI} = ED^{HI}$	0.538	0.490	0.802			
$HD^{LI} = HD^{HI}$	0.119	0.014	0.052			

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses. The outcome variables indicate expenditure on child investment goods, groceries and luxury goods, expressed in pounds at baseline prices. Higher/lower income groups denote the upper/lower 50% of the sample income per adult equivalent distribution. All models control for individual and household characteristics excluding the gender of the parent, and for the order in which the three types of goods appeared in the investment task. EB, HB, ED and HD indicate participants exposed to the four treatment arms: the first letter indicates exposure to easy (E) or hard (H) financial scenarios, while the second letter indicates being offered child investment products at baseline prices (B) or discounted by 50% (D). The first listed *p*-value ( $ED = HD$ ) is associated with the t-test of equality of the Baseline Discount group and the Hard Discount group coefficients. The remaining *p*-values, listed with the superscript LI and HI, test the equality of the estimated treatments effects between the lower income group (LI) and the higher income group (HI) from models where each treatment assignment variable and each covariate is interacted with the income group dummy.

**Table B.9**

Descriptive statistics and balance checks for the sub-sample reporting having received any payments in the past month.

	N (1)	EB (2)	HE (3)	ED (4)	HD (5)	p-value (6)
Child gender (Male)	257	0.40	0.52	0.47	0.52	0.46
Age in months	257	20.74	22.72	21.91	25.20	0.11
No formal childcare	257	0.43	0.40	0.47	0.48	0.80
Any sibling below 4	257	0.30	0.27	0.36	0.31	0.70
Age of parent	257	30.03	32.19	32.24	32.79	0.01
Gender of parent (male)	257	0.09	0.16	0.29	0.22	0.02
Completed higher education	257	0.51	0.46	0.42	0.52	0.67
Student	257	0.16	0.06	0.03	0.06	0.03
Nationality UK	257	0.91	0.97	0.94	0.98	0.30
Country of birth UK	257	0.89	0.94	0.94	0.93	0.60
Language English	257	0.93	0.97	0.95	0.96	0.68
Household size	257	3.86	3.57	3.94	3.87	0.16
Number of children	257	1.89	1.72	2.03	1.98	0.26
Spouse or cohabiting partner	257	0.87	0.85	0.86	0.91	0.82
Yearly income	257	15.22	14.58	14.59	15.75	0.72
Yearly household income	257	29.42	27.31	28.26	30.36	0.55
Material Deprivation	257	-0.04	0.10	0.05	0.01	0.61
Perceived SES (1-10 ladder)	257	4.77	4.78	4.74	4.98	0.84
Parent is employed	257	0.46	0.57	0.64	0.67	0.08
Spouse is employed	224	0.80	0.82	0.81	0.82	0.99
Any payment received in past month	257	0.83	0.84	0.85	0.89	0.81
Any payments received in past month (spouse)	224	0.82	0.93	0.89	0.84	0.27
Days since last payment	257	12.50	11.48	13.47	13.33	0.66
Has a credit card	257	0.69	0.72	0.62	0.74	0.51

Note: Column 1 indicates the number of observations. Columns 2–5 show the means across treatment arms. Column 6 displays the *p*-values associated with the F test of joint orthogonality across treatment arms. The analysis uses the sub-sample declaring to have received any payments in the past month. EB, HB, ED and HD indicate participants exposed to the four treatment arms: the first letter indicates exposure to easy (E) or hard (H) financial scenarios, while the second letter indicates being offered child investment products at baseline prices (B) or discounted by 50% (D). Yearly income per adult equivalent is computed by dividing total yearly household income by the square root of the household size. Material Deprivation is computed by averaging and standardizing the incidence of several income shocks in the previous year (not able to pay bills, rent, to afford heating, skipped meals, took loans from payday lenders, sought financial help from family or friends).

- 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”.

**Table B.10**  
Descriptive statistics and balance checks by income group for the sub-sample reporting having received some payments in the past month.

	Lower income						Higher income					
	N	EB	HD	EB	HD	p-value	N	EB	HB	ED	HD	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Child gender (Male)	134	0.33	0.50	0.56	0.39	0.22	123	0.48	0.55	0.38	0.61	0.28
Age in months	134	21.13	23.32	20.91	21.87	0.72	123	20.26	21.93	22.97	27.68	0.04
No formal childcare	134	0.41	0.47	0.47	0.35	0.76	123	0.45	0.31	0.47	0.58	0.22
Any sibling below 4	134	0.28	0.26	0.38	0.39	0.59	123	0.32	0.28	0.34	0.26	0.88
Age of parent	134	30.38	31.53	32.47	32.24	0.34	123	29.58	33.07	32.00	33.19	0.01
Gender of parent (male)	134	0.05	0.16	0.21	0.26	0.12	123	0.13	0.17	0.38	0.19	0.09
Completed higher education	134	0.44	0.34	0.41	0.39	0.86	123	0.61	0.62	0.44	0.61	0.39
Student	134	0.21	0.05	0.06	0.09	0.11	123	0.10	0.07	0.00	0.03	0.31
Nationality UK	134	0.90	0.97	0.94	1.00	0.29	123	0.94	0.97	0.94	0.97	0.90
Country of birth UK	134	0.90	0.95	0.94	0.96	0.77	123	0.87	0.93	0.94	0.90	0.79
Language English	134	0.95	0.97	0.94	1.00	0.65	123	0.90	0.97	0.97	0.94	0.66
Household size	134	4.05	3.53	4.21	4.13	0.05	123	3.61	3.62	3.66	3.68	0.99
Number of children	134	2.13	1.79	2.29	2.22	0.14	123	1.58	1.62	1.75	1.81	0.74
Spouse or cohabiting partner	134	0.79	0.74	0.76	0.83	0.86	123	0.97	1.00	0.97	0.97	0.82
Yearly income	134	10.38	10.00	9.59	9.47	0.69	123	21.31	20.58	19.90	20.41	0.61
Yearly household income	134	20.86	18.49	19.45	19.16	0.56	123	40.20	38.86	37.61	38.68	0.66
Material Deprivation	134	0.15	0.30	0.28	0.21	0.80	123	-0.28	-0.17	-0.19	-0.14	0.48
Perceived SES (1-10 ladder)	134	4.44	4.24	4.44	4.43	0.94	123	5.19	5.48	5.06	5.39	0.62
Parent is employed	134	0.33	0.50	0.50	0.61	0.18	123	0.61	0.66	0.78	0.71	0.52
Spouse is employed	104	0.77	0.75	0.77	0.63	0.70	120	0.83	0.90	0.84	0.93	0.60
Any payment received in past month	134	0.79	0.84	0.82	0.87	0.89	123	0.87	0.83	0.88	0.90	0.86
Any payments received in past month (spouse)	104	0.77	0.86	0.88	0.84	0.71	120	0.87	1.00	0.90	0.83	0.17
Days since last payment	134	11.82	11.08	10.06	9.61	0.81	123	13.35	12.00	17.09	16.10	0.15
Has a credit card	134	0.59	0.58	0.59	0.65	0.95	123	0.81	0.90	0.66	0.81	0.14

Note: Columns 1 and 7 indicate the number of observations. Columns 2–5 and 8–11 show the means across treatment arms. Columns 6 and 12 displays the p-values associated with the F test of joint orthogonality across treatment arms. EB, HB, ED and HD indicate participants exposed to the four treatment arms: the first letter indicates exposure to easy (E) or hard (H) financial scenarios, while the second letter indicates being offered child investment products at baseline prices (B) or discounted by 50% (D). Yearly income per adult equivalent is computed by dividing total yearly household income by the square root of the household size. Material Deprivation is computed by averaging and standardizing the incidence of several income shocks in the previous year (not able to pay bills, rent, to afford heating, skipped meals, took loans from payday lenders, sought financial help from family or friends). The sample used in the analysis are participants declaring to have received any payments in the past month with yearly household income below the sample median in (Columns 2–6) and above the sample median (Columns 8–12).

**Table B.11**  
Treatment effects on demand for investment goods, groceries and luxury goods for the sub-sample declaring to have received any payments in the past month.

	Lower income			Higher income		
	(1)	(2)	(3)	(4)	(5)	(6)
	Investment	Groceries	Temptation	Investment	Groceries	Temptation
Hard Discount (HD)	-0.39 (2.14)	-1.06 (2.28)	1.81 (1.38)	0.38 (2.97)	-0.51 (2.91)	-0.75 (1.51)
Easy Discount (ED)	9.06 (3.07)	-3.20 (2.36)	1.19 (1.29)	8.04 (3.85)	-0.60 (2.95)	0.11 (1.45)
Hard Discount (HD)	4.54 (3.38)	1.47 (2.26)	-0.72 (0.84)	6.96 (3.35)	-1.16 (2.97)	0.77 (1.70)
EB Mean	7.14	20.00	1.95	6.40	20.59	2.13
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R <sup>2</sup>	0.07	-0.00	-0.02	-0.01	0.02	0.07
Observations	134	134	134	123	123	123
<i>p-values - Tests:</i>						
ED = HD	0.221	0.045	0.110	0.801	0.851	0.718
HB <sup>LI</sup> = HB <sup>HI</sup>	0.834	0.881	0.213			
ED <sup>LI</sup> = ED <sup>HI</sup>	0.837	0.492	0.578			
HD <sup>LI</sup> = HD <sup>HI</sup>	0.612	0.480	0.432			

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses. The outcome variables indicate expenditure on child investment goods, groceries and luxury goods, expressed in pounds at baseline prices. Higher/lower income groups denote the upper/lower 50% of the sample income per adult equivalent distribution. The analysis uses the sub-sample declaring to have received any payments in the past month. All models control for individual and household characteristics, and for the order in which the three types of goods appeared in the investment task. EB, HB, ED and HD indicate participants exposed to the four treatment arms: the first letter indicates exposure to easy (E) or hard (H) financial scenarios, while the second letter indicates being offered child investment products at baseline prices (B) or discounted by 50% (D). The first listed p-value is associated with the t-test of equality of the Baseline Discount group and the Hard Discount group coefficients. The remaining p-values, listed with the superscript LI and HI, test the equality of the estimated treatments effects between the lower income group (LI) and the higher income group (HI) from models where each treatment assignment variable and each covariate is interacted with the income group dummy.

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)

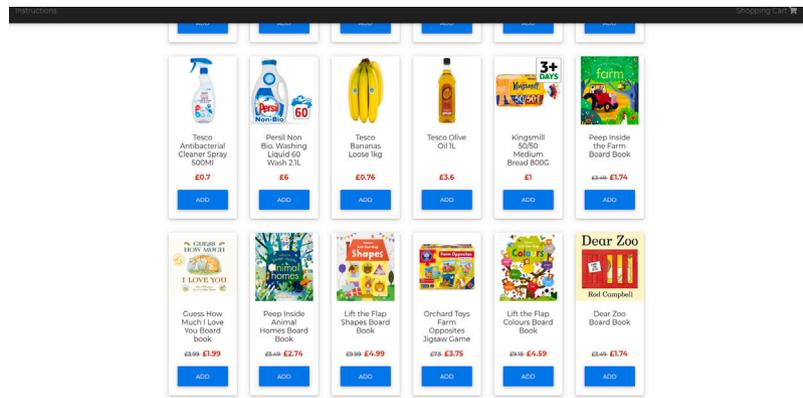


Fig. B.1. Main screen of the investment task.

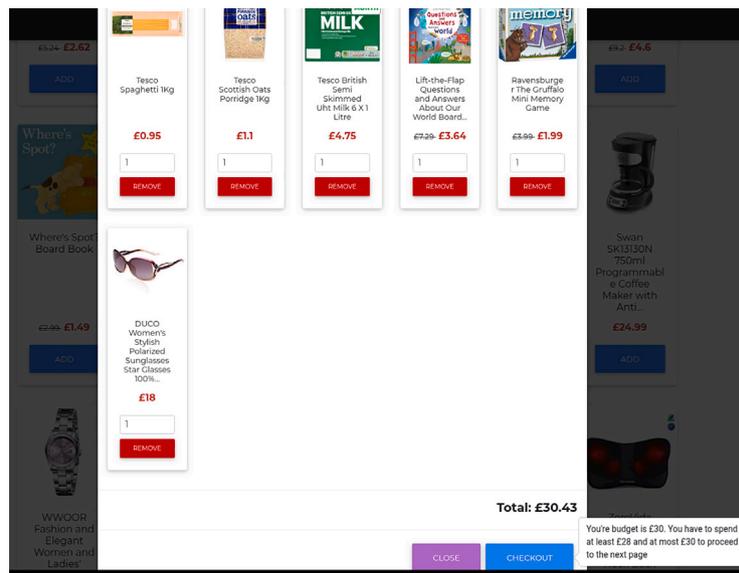


Fig. B.2. Checkout screen of the investment task.

- 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)

**B.1.2. 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.

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