Parent-Bias*

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Abstract

This paper uses a lab-in-the-field experiment in Malawi to document two new facts about how parents share resources with their children over time. First, for many parents, the *share* of the household budget that they plan to allocate to their children *increases with the time gap* between plans and actual consumption. Second, many parents systematically revise future plans *away from children's consumption* as the time gap between plans and actual consumption decreases – even when consumption is *still in the future*. Those patterns are consistent with parents discounting their future utility of consumption to a greater extent than that of their children. We document that parents characterized by such *asymmetric geometric discounting* display sizable preference reversals every period, a phenomenon we denote *parent-bias*. We find that, despite ambitious plans, those parents actually allocate *less* to their children in the present than other parents, and that such preferences are predictive of under-investment in children outside the lab. Commitment devices designed for present-bias do not mitigate parent-bias. Our findings provide a new explanation for under-investment in children and inform the design of new interventions to address it.

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[PRELIMINARY AND INCOMPLETE]

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"The task of anthropologists, and of economists also, is to collect anecdotes that may or may not lend themselves to testable conjecturing. In actual economic life, and in the writings of economists, the phenomena of how people treat the present and the future (and the past, too) offer a rich treasure trove of varied behaviors." – Samuelson (2008, p. 1)

1 Introduction

In the film *The Boy Who Harnessed the Wind*, a brilliant Malawian boy is barred from attending school after his parents fail to pay his school fees despite repeated promises. The lead character builds a windmill that helps his town escape famine, and eventually becomes an engineer. While his fate is exceptional, the starting point of the movie is all too common. Parents, all over the world, frequently fail to follow through on ambitious plans to invest in their children's health and education.[1]

While liquidity is likely to severely constrain those investments among the poor, interventions designed to relieve poverty often have only small effects on investments in children.[2] Instead, quasi-hyperbolic discounting is a leading explanation for gaps between past intentions and present actions. Time inconsistencies, typically in favor of instant gratification (*present-bias*), come to rationalize broken promises in general, and under-investment in children in particular. Having said that, present-bias *does not fully account* for deviations from exponential discounting in the way parents plan investments in children, or in the way the latter more often than not renege on those plans. To that effect, when the Malawian father in the movie realizes the family does not have as much resources as initially expected, he does not decrease spending equally across household members: adjustments to the family budget hurt investments in children *disproportionately.* Present-bias *cannot* rationalize that asymmetry.[3] In this paper, we document that such *asymmetric reversals* emerge as part of systematic patterns in how parents share resources with their children over time. We then introduce a new type of time preferences that can rationalize those patterns. We find such preferences to be *strongly correlated with real investments* in children's human capital in our sample, and *uncorrelated with present-bias.*

To document how parents plan to (and effectively do) share resources with their children in the future, and how those plans evolve over time, we conduct a lab-in-the-field experiment with 2,413 parents in Malawi. Measuring parents' time preferences is challeng-

[1] According to the 2018 World Development Report, while primary enrollment has increased substantially worldwide since 1970, secondary enrollment is still only 40% across Sub-Saharan Africa (p. 59), with huge differences between urban and rural areas (p. 62).

[2] Glennerster & Kremer (2012) and Kremer & Holla (2009) document that demand for educational and health investments in children falls very steeply as soon as prices are above zero (even if very low). Microcredit does not systematically increase investments in children Banerjee (2013). While conditional cash transfers and earmarked loans increase those investments, there is evidence that such effects might be driven by other mechanisms – from salience to present-bias (Glennerster & Kremer, 2012).

[3] Present-bias predicts only a *general tendency* to cut down on investment plans when their costs become immediate, *not* a tendency to cut down *differentially* across investment plans for parents and children. While asymmetric present-bias could explain this, we discuss in detail below how the phenomena we document are distinct from it.

ing, as it requires observing how they plan to share consumption within the household over time, and whether they actually stick to those plans. We follow the literature in moving away from using decisions about monetary payments to make inferences on time preferences; instead, we draw upon real consumption decisions that can be perfectly observed at the time of the experiment.[4] We study parents' decisions concerning a non-fungible, tempting and nutritious good – peanuts –, documenting their plans to split consumption with their children over different time horizons. The experiment takes place during the lean season in Malawi; as such, we can have subjects undertake payoff-relevant decisions in this setting at a reasonably low cost. Enumerators visit the parents three times. During the first visit (round 1), parents choose how they want to share the peanuts with one of their children by the time enumerators revisit them two days later (round 2) and four weeks later (round 3). There is no consumption at round 1. At round 2, parents have the opportunity to set new consumption plans (potentially different from their round 1 decision), allocating consumption in the present and in little less than four weeks. There is no decision at round 3; only consumption. Every allocation set by parents can be drawn to be implemented with positive probability, and peanuts are consumed in front of the enumerators – precluding side transfers. Such a design features several advantages. Peanuts shut down concerns with fungibility and arbitrage outside the lab. Moreover, different from the typical experiment to elicit time preferences, our design *does not allow* subjects to transfer resources *over time*. This feature allows us to easily capture how parents trade off their own and their children's consumption *within period*, and how those trade-offs *vary with the time gap* between plans and consumption. Last, allowing parents to revise their former decision enable us to document preference reversals.

We start by documenting two new facts about how a relevant fraction of parents allocates resources over time. The first is that many parents' planned budget shares allocated to children tend to *increase with the time gap* between plans and actual consumption. At round 2, almost one third of parents allocate a *higher share* of the total budget to their children in the future – nearly 46% higher than what they get allocated in the present. The second is that many parents revise their plans *away from children's future allocation* when the decision gets closer to actual consumption (even if still 28 days away). Such preference reversals are roughly 1/3 as common as present-bias in our sample, and are quantitatively large: for those parents, the round-3 budget share allocated to children decreases by 37% on average (compared to 24% average reallocations induced by present-bias)

We implement many design choices to rule out that such patterns could be rationalized

[4] As money is fungible, the timing of the payments and of the consumption acquired with it are not necessarily connected (Augenblick & Rabin, 2019). In addition, subjects may have arbitrage opportunities and their choices over monetary payments could simply reflect the interest rates they could access outside of the lab (Augenblick et al., 2015; Cubitt & Read, 2007). What is more, since parents control how money is spent outside the lab, decisions about how to split money between themselves and their children would be non-committal in the context of our experiment. Examples of real consumption in the time-preferences literature include real effort tasks (Augenblick et al., 2015; Augenblick & Rabin, 2019; Barton, 2015), irritating noises (Solnick & Waller, 1980) and squirts of juice (Brown & Camerer, 2009; McClure et al., 2007).

under standard time preferences. First, we can rule out learning about preferences between decision rounds: parents taste some peanuts before each round to minimize the risk of projection bias (Loewenstein et al., 2003) and children do not consume peanuts before the end of round 2. Second, rounds 1 and 2 are only two days apart, minimizing concerns with other shocks that could affect parents' information set when they can revise their plans. Comparing parents' responses to different interest rates at rounds 1 and 2, we can also rule out directly that parents' (expected) marginal utility of consumption at round 2 has changed between rounds.[5,6] Last, we document that parents do not strategically adjust their child's consumption outside of the experiment between visits.[7]

Most importantly, none of those facts can be accounted for by present-bias. Without shocks to the expected marginal utility of consumption between rounds, present-bias predicts that *children's budget shares should be constant* over time, and that plans should *only be revised once they have immediate consequences*. Instead, those stylized facts are consistent with *asymmetric geometric discounting* (AGD). For concreteness, consider a mother who discounts her children's future consumption to a lesser extent than her own. Those time preferences are a deviation from exponential discounting; she thinks that her *future self* will (or wishes that she would) be more generous towards her children than her *present self*, which in turn leads to systematic preference reversals: every period, she *reallocates budget shares*, away from her children's planned consumption and towards her own. For this reason, we call reversals generated by AGD preferences *parent-bias*. Under-investment in children (relative to her original plans) arises as a direct implication of being relatively less patient about her own future consumption. While preference reversals have been shown to be a necessity when multiple decision-makers with different discount rates allocate a single payoff stream for the group (Jackson & Yariv, 2015), parent-bias follows from a *single decision-maker* – parents – with *multiple discount rates* deciding on how to allocate a single payoff stream across multiple group members – themselves and their children.

Parent-bias has important implications because of the following. First, because AGD preferences are likely to be prevalent; after all, time inconsistencies often arise as a conflict between deliberation and affect (Loewenstein, 2018), and children bring about sharp tensions between the two: parents often aspire to provide a better future for their children, but are pressed against present needs. Second, because commitment devices that address present-bias, such as lock-boxes or illiquid accounts, *do not mitigate parent-bias*: preventing within-household reallocation requires commitment devices that are specifically designed to limit decision-makers ability to change past plans *differentially* across household members.

Using only round-1 decisions, we document that about 30% of subjects exhibit asymmetric geometric discounting, allocating higher budget shares to their child in later rounds.

[5] See appendix H

[6] In addition, we survey parents about liquidity constraints and hunger at every round, and document that the two facts are robust to controlling for those confounding factors.

[7] See Appendix I.

For those subjects, the average share allocated to their children increases from under 43% two days later to nearly 65% thirty days later. Allocating a larger share of consumption to the child in later time periods is not necessarily irrational: parents may, for instance, expect their child to have a higher marginal utility of consumption than their own, or a higher probability of survival further in the future. However, such time preferences strongly predict reversals. In just two days, AGD parents are 4 times more likely to reallocate away from their children's round-3 consumption than other parents. Strikingly, despite ambitious plans, parent-biased subjects end up allocating *significantly less* to their children in the present (about 4% less than other parents, a statistically significant difference).

Do AGD preferences also matter for investments in children outside the lab? We survey parents at round 3 about recent investments in the education and health of the child involved in the experiment. The correlation between real investments in children's health and education and AGD preferences among parents who value future consumption to a greater extent is negative and sizeable, with a correlation coefficient nearly identical to that between real investments and quasi-hyperbolic discounting.

Could asymmetric preference reversals be driven instead by asymmetric present-bias, whereby subjects display present-bias only (or to a greater extent) towards their own consumption?[8] We document that not to be the case. In a different part of the same lab-in-the-field experiment, we have parents split peanuts for their own consumption over time.[9] We define as present-biased those who revise their round-3 consumption plans downward when we revisit them two days later. We find that present-bias and parent-bias are indeed different phenomena: the latter it is not systematically correlated with the former, implying that it is not driven by quasi-hyperbolic discounting asymmetries across parent's and children's future consumption.[10,11] In sum, parent-bias is indeed a distinctive phenomenon linked to how parents treat the present and the future (in the words of the opening quote by Samuelson).

While our experiment is based on consumption allocation trade-offs *within periods* (as parents are not able to transfer resources across periods), most investments in children actually *also involve* trading off resources across periods. In fact, education or preventive health care are prototypical examples of investments in children's human capital with upfront costs. Do AGD preferences also generate under-investment when such trade-offs also present? Using a simple model, we show that, in the investment case, even parents sophisticated about their own bias are prone to preference reversals, and that naive parents

[8] Koelle & Wozny (2018) provide evidence that agents are less likely to exhibit present-bias when making decisions on behalf of others.

[9] Under different scenarios for interest rates, see Section 3.

[10] To measure the joint distribution of parent and present-bias, we also elicit respondents inter-temporal allocations. They can chose to allocate peanuts to be consumed *by parents only* at the end of the second and third visits, under different interest rates. At the end of the second visit, a random draw picks which of those two tasks will be implemented, and whether the decision made during the first or the second visit will be binding. Parents (and children, if the former task is picked) eat their allocated share of peanuts in front of enumerators.

[11] We find that subjects with asymmetric geometric discounting do *not* systematically exhibit higher β with respect to their own consumption (see appendix G).

massively under-invest relative to geometric discounters.[12] Calibrating the model with our estimates, we show that AGD parents' under-investments in their children lead to large welfare losses: naive parents' long-term utility is equivalent to that of consistent parents with *23% lower* income.

What policy instruments could potentially mitigate the reallocation effects of asymmetric geometric discounting? We start by evaluating a light touch intervention, inspired by evidence that mental accounting could help increase investments (Thaler, 1999). Along those lines, could *labeling children's consumption* at least partly mitigate parent-bias? To study this question, we randomly assign a sub-sample to a framing intervention: at the beginning of round 2, the starting point for subject's allocation decision is their round-1 allocation. As such, each parent's previous allocation to children's consumption is made salient at the time they have the opportunity to revise it, at round 2. The intervention, however, has no effects: we find that respondents with asymmetric geometric discounting assigned to labeling reallocate away from their children's planned consumption *just as much.*

Next, we turn to commitment devices. Demand for commitment designed to address present-bias is often moderate, even among subjects who are sophisticated about their time preferences, and plummets at positive prices (Ashraf et al., 2006; Augenblick et al., 2015; Kaur et al., 2015; Laibson, 2015).[13] Is that also the case for parent-bias? We offer subjects the possibility of committing to their allocation plans set at round 1, varying experimentally both the type and the price of commitment we provide. Participants are offered a probabilistic commitment device, in the spirit of Augenblick et al. (2015), which decreases the likelihood that the allocation set at round 2 is implemented instead of that set at round 1. This design ensures that both decisions can bind with positive probability and allows us to observe decisions by all subjects in both time periods. We find that AGD parents are more likely to demand commitment to stick to their planned allocation. Outside the lab, however, we find that AGD parents are not only less likely to invest in their children, they are also less likely to commit to their ambitious future plans. In a 6-month follow-up, we enter all parents into a lottery, and offer them the opportunity to commit lottery proceeds to tutoring for their child in case they win. We find that AGD parents are significantly *less* likely to commit than other parents.

We also use this follow-up survey to document that AGD preferences affect parents' plans with respect to their own children, but *not others* – and, hence, are different from time-increasing altruism more generally. We do so by eliciting parents' willingness to commit lottery proceeds to tutoring *someone else's child*. While all parents are systematically less likely to allocate lottery proceeds to tutoring another child, AGD parents are *not* differentially likely to do so.

[12] In our experiment, less than 1/3 of parents are sophisticated about parent-bias. Interestingly, we show that sophistication is not a unidimensional feature across biases: only a small share of subjects are sophisticated about both present-bias and parent-bias, while a larger share is sophisticated about one but not about the other.

[13] Exceptions are Ariely & Wertenbroch (2002); Beshears et al. (2020); Schilbach (2019)

Our study contributes to an active literature about behavioral biases linked to investments in children's human capital. Present-bias can explain under-investment in children, as naive parents systematically over-estimate the extent to which they will trade off costly investments and later returns in the future.[14] Preference reversals associated with present-bias have been extensively studied (Augenblick et al., 2015; DellaVigna & Malmendier, 2006), particularly within Development Economics (Ashraf et al., 2006; Gine et al., 2016; Tarozzi & Mahajan, 2011). In contrast, parent-bias has been overlooked. Strikingly, we find it to be *more predictive* of real investments in children's human capital than present-bias.

This paper also relates to a growing literature that investigates whether subjects tend to be more generous towards others in the future than in the present.[15] Our study is the first to elicit asymmetries in discount rates for parents' and their children's future consumption, and to document the consequences of such asymmetries for within-household allocation. A few papers have posited that discount rates differ across consumption goods (Ubfal, 2016), which could lead to poverty traps (Mullainathan & Shafir, 2013).[16] We extend those consumption models to also account for investments in children, and estimate the welfare implications of sub-optimal investments.

Last, our findings inform a range of new policy instruments that could address parent-bias and, potentially, increase investments in children – from school meals to illiquid savings accounts earmarked to children. At the same time, our results on low demand for commitment outside the lab suggest that it might be challenging to prevent parents from reallocating away from planned investments in children *even* if those instruments were in place.

2 A model of parental investments in children

In this section, we present a simple model of parental allocation decisions between themselves and their children over time, characterizing the predictions for within-household allocation trajectories in the presence of asymmetric geometric discounting (AGD). Subsection 2.1 starts with the consumption case, which abstracts from inter-temporal trade-offs and sophistication about preference reversals. Next, we consider the investment case in subsection 2.2.

[14] The theoretical link between present-bias and investments in children is noted in Glennerster & Kremer (2012), but there is limited empirical evidence documenting that link. One notable exception is Ringdal & Sjursen (2017) which shows that increasing the bargaining power of the most patient parent increases investments in children.

[15] In the absence of trade-offs between one's own and other's consumption, subjects tend to be paternalist, aligning choices for others with their own preferences, (Ambuehl et al., 2019; Krawczyk & Wozny, 2017; Uhl, 2011) or to display more patience for others than for themselves (Shapiro, 2010). When there are trade-offs, subjects tend to display time-inconsistent generosity (Koelle & Wozny, 2018). There is related evidence that generosity might interact not only with time preferences, but also with risk preferences (Exley, 2015) and preferences over fairness (Andreoni et al., 2018).

[16] There is a also a literature on different discount rates between different decision-makers within the household and its consequences for time inconsistencies (Jackson & Yariv, 2015, e.g.). More generally, for the consequences of intra-household bargaining, see Baland & Ziparo (2017); Chiappori (1988).

2.1 The consumption case

We depart from a simple three-period model of investments in children. Each household consists of one child and one parent. In each period $t \in \{1,2\}$, the parent decides how to allocate income (y, constant over time, for simplicity) between her own consumption (x_t) and that of her child (z_t) at $t \in \{2,3\}$. To focus on the dynamics of future plans, we abstract from consumption at $t = 1$; the parent only sets future allocations at this period, deciding on how to split consumption between herself and her child at $t = 2$ and $t = 3$. She revisits those two decisions at $t = 2$. At $t = 3$, consumption decisions can no longer be changed: the parent and her child consume according to the choice made at $t = 2$. We assume there is no uncertainty and no technology to smooth consumption over time (we relax the latter in the next subsection).

The parent derives instantaneous utility from consumption $u(x_t)$, and the child, $v(z_t)$, both increasing and strictly concave. The parent weights her child's instantaneous utility by an imperfect altruism parameter, $\alpha \geq 0$. She discounts her child's future consumption one period ahead by $\delta \in [0,1]$, and her own consumption one period ahead by $\theta\delta$, with $\theta \in [0,1]$.[17] While we abstract from present-bias in this simple formulation, Appendix G augments the model by allowing for quasi-hyperbolic discounting, with potentially different β's applying to the parent's and the child's future utility of consumption.[18]

Formally, the parent's utility maximization problem at $t = 1$ is as follows:

$$\underset{\{z_t, x_t\}_{t=2,3}}{\text{Max}} \ \theta\delta u(x_2^1) + \alpha\delta v(z_2^1) + (\theta\delta)^2 u(x_3^1) + \alpha\delta^2 v(z_3^1) \tag{1}$$

$$\text{s.t.}$$

$$\begin{cases} x_2 + z_2 \leq y \\ x_3 + z_3 \leq y, \end{cases}$$

where superscripts indicate that the decision is made at $t = 1$.

At $t = 2$, the parents' utility maximization problem becomes:

$$\underset{\{z_t, x_t\}_{t=2,3}}{\text{Max}} \ u(x_2^2) + \alpha v(z_2^2) + \theta\delta u(x_3^2) + \alpha\delta v(z_3^2) \tag{2}$$

[17] While this formulation is related to Ubfal (2016), which estimates heterogeneous discount rates across different goods, and to Banerjee & Mullainathan (2010), in which temptation goods are differentially discounted in the future, in our model, differences in discount rates can arise across consumption of *different subjects*, with consequential implications for parental decisions on behalf of the child. Moreover, our model gives rise to new insights, in particular when it comes to issues of sophistication and commitment to future plans (see Section 2.2) and welfare implications (see Section 5.4).

[18] We show that asymmetric present-bias is not observationally equivalent to AGD, since the former does not predict that budget shares allocated to children increase with the time gap between decision and consumption.

s.t.

$$\begin{cases} x_2 + z_2 \leq y \\ x_3 + z_3 \leq y, \end{cases}$$

First Order Conditions at $t = 1$ are given by: $\frac{u'(x_2^1)}{v'(z_2^1)} = \frac{\alpha}{\theta}$ and $\frac{u'(x_3^1)}{v'(z_3^1)} = \frac{\alpha}{\theta^2}$. At $t = 2$, they become : $\frac{u'(x_2^2)}{v'(z_2^2)} = \alpha$ and $\frac{u'(x_3^2)}{v'(z_3^2)} = \frac{\alpha}{\theta}$.

If the parent discounts her consumption and that of her child to the same extent, i.e. if $\theta = 1$, then FOCs are identical in both periods, and the share of consumption allocated to the child is constant over time.[19] In other words, the parent will not deviate from plans made at $t = 1$ when revisiting the decision at $t = 2$.

Conversely, if the parent discounts her own future consumption to a greater extent than that of her child, i.e. if $\theta < 1$, then she will plan to allocate a *larger share* of consumption to her child further in the future, an immediate implication of decreasing marginal utility of consumption.[20] Moreover, this will lead to preference reversals at $t = 2$: an AGD parent will deviate from her $t = 1$ plans when given the chance to update her decision at $t = 2$, reallocating consumption away from her child, towards herself.[21] Those predictions are indistinguishable from an alternative formulation in which the coefficient of imperfect altruism is time-increasing, with $\alpha_k^j < \alpha_{k+1}^j$. Empirically, we consider the distinction between parent-bias and time-increasing altruism (not specific to one's children) in subsection 6.3.4.

Figures 1a and 1b illustrate the differences in (planned) allocations between symmetric and asymmetric geometric discounters who face otherwise identical utility functions with a numerical example. Figure 1a showcases the decision for the former: they allocate the same budget share to their children to be consumed at $t = 2$ and $t = 3$, irrespective of when that decision is made. Figure 1b highlights that, in contrast, when AGD parents make allocation decisions in the first time period (the left-hand-side panel), they plan to be more generous towards their child in the later consumption period than when revising their decision in the second time period (the right-hand-side panel), when they update their round-1 decision to allocate less to their child in each time period. Allowing parents to be present-biased does not change Figure 1a, as long as the same β applies for both the parent's and the child's future consumption; section 5.3.3 discusses how that changes when we allow β's to be different.

Proposition 1 generalizes the model's predictions in the presence of AGD preferences.

Proposition 1: *AGD parents (1) allocate budget shares to their children increasing in the time gap between the decision and consumption, and (2) reallocate away from their children planned consumption (towards their own) at every period.*

[19] Let s_j^k be the budget share allocated on $t = k$ to child's consumption at $t = j$. Formally: $s_2^1 = s_3^1 = s_2^2 = s_3^2$ for consistent parents.

[20] Formally: $s_3^1 > s_2^1$ and $s_3^2 > s_2^2$ for AGD parents.

[21] Formally: $s_2^1 > s_2^2$ and $s_3^1 > s_3^2$ for AGD parents.

Proof: Generalizing the model's FOCs to decisions made at $t = j$ about consumption at $t = k$ yields:

$$\frac{u'(x_k^j)}{v'(z_k^j)} = \frac{\alpha}{\theta^{k-j}} \tag{3}$$

Due to decreasing marginal utility of consumption, if $\theta < 1$, then the child's budget share increases in $k - j$ (part 1) and decreases in j (part 2). ■

2.2 The investment case

In this section, we modify the previous model, allowing for inter-temporal transfers to study if the insights of the consumption model still hold in the presence of dynamic trade-offs, especially among sophisticated parents. In this model, AGD parents are still subject to preference reversals when it comes to child's planned consumption; as such, we investigate whether anticipating reallocation leads them to compensate by *over-investing* in children in the present, relative to symmetric parents.

To keep the analysis simple, we restrict attention to a two-period model, allowing parents to revise consumption decisions but not investment decisions over time. At $t = 1$, the parent chooses how much out of income y to consume, how much to allocate to her child's consumption at $t = 1$, and how much to invest in her child (I) – which yields gross return $R = 1 + r$ at $t = 2$. The parent also makes plans of how to split income $y + RI$ between herself and her child in $t = 2$. In other words, investments in children can be used as a savings vehicle to smooth consumption over time.[22]

The parents' utility maximization problem at $t = 1$ is as follows:

$$\underset{\{z_t, x_t\}_{t=1,2}, I}{\text{Max}} \; u(x_1^1) + \alpha v(z_1^1) + \theta\delta u(x_2^1) + \alpha\delta v(z_2^1) \tag{4}$$

$$\text{s.t.}$$
$$\begin{cases} x_1 + z_1 + I \leq y \\ x_2 + z_2 \leq y + RI \end{cases}$$

An important additional element of the investment model is the parent's belief about her future utility function. We define $\hat{\theta}$ as the parent's belief at $t = 1$ about the value that θ takes at $t = 2$. More precisely, following O'Donoghue & Rabin (1999), the agent thinks that her $t = 2$ utility function is: $\hat{\theta} u(x_2) + \alpha v(z_2)$, with $\hat{\theta} \in [\theta, 1]$. The sophisticated type anticipates correctly that her $t = 2$ utility function entails $\hat{\theta} = 1$. The naive type incorrectly believes that her $t = 2$ utility function entails $\hat{\theta} \in \theta, 1(fullynaiveif = \theta)$.

[22] Although extremely simple, the model can accommodate more complex elements; for instance, if parents can recover only a fraction of investments in children, that can be expressed as a lower interest rate.

Assuming a specific functional form for the parent's and the child's instantaneous utility function (CRRA) to obtain tractable results for comparative statics, Proposition 2 establishes that although investments in children are proportional to parents' degree of sophistication, AGD parents *do not over-invest* relative to symmetric discounters in order to compensate for future reallocation.

Proposition 2: *AGD parents of all types choose a lower level of investment than a symmetric geometric discounter with otherwise identical preferences. If the coefficient of constant relative risk aversion is larger than 1, then investments in children by AGD parents increase with their degree of sophistication.*

Proof: See Appendix J. ■

The second part of the proposition holds as long as the coefficient of constant relative risk aversion is larger than 1 (Holden & Quiggin, 2017 finds an average CRRA coefficient of 1.73 for Malawi). In that case, sophisticated AGD parents increase investments in anticipation of future reallocations away from children's planned consumption (although never as much as to completely mitigate the effects of AGD preferences on investments in children).

Figure 10 showcases a numerical example of optimal investment levels for the two extreme types – a naive agent (with $\hat{\theta} = \theta$) and a sophisticated agent (with $\hat{\theta} = 1$). It makes it clear that the lower $\hat{\theta}$, the larger the gap in investments between naive and sophisticated agents.

The results in this section also make it clear that instruments designed to mitigate present-bias (such as illiquid savings) only imperfectly address parent-bias: while sophisticated AGD parents could use investments in children as a way to mitigate the consequences of within-household reallocation by ensuring a larger resource pool in the future, that does not preclude deviations away from children's planned consumption (as the ratio of marginal utilities in each decision period remain the same as in Section 2.1).

In contrast, Proposition 3 establishes that if investments in children paid out directly as future consumption rather than non-earmarked resources – which, in practice, could be achieved with instruments such as school meal plans –, AGD parents would have (weakly) lesser scope for reallocation (strictly if a corner solution is reached at $t = 2$).

Proposition 3: *Commitment devices that pre-set future allocations to the child weakly increase her future consumption relative to commitment devices with identical gross returns that merely ensure a larger resource pool in the future.*

Proof: Denote as $\{z_t^*, x_t^*\}_{t=1,2}, I^*$ the solution to the parent's problem when investment pays out in cash in period $t = 2$. Now, let us modify the parents' utility maximization problem at $t = 1$ is as follows:

$$\underset{\{z_t,x_t\}_{t=1,2},I}{\text{Max}} u(x_1^1)+\alpha v(z_1^1)+\theta\delta u(x_2^1)+\alpha\delta v(z_2^1+RI) \tag{5}$$

$$\text{s.t.} \begin{cases} x_1+z_1+I\leq y \\ x_2+z_2\leq y \end{cases}$$

In the modified problem, investment I pays out directly as child's consumption in $t=2$, under the same gross interest rate $R=1+r$. Denote the solution to this new problem $\{\hat{z}_t,\hat{x}_t\}_{t=1,2},\hat{I}$.

In an interior solution, the ratio of marginal utilities at $t=2$ is the same in both problems:

$$\frac{u'(x_2^*)}{v'(z_2^*)}=\frac{u'(\hat{x}_2)}{v'(\hat{z}_2)}=\alpha \tag{6}$$

By that argument, if $R\hat{I}\leq z_2^*$, then we are at the interior solution and $\hat{z}_2^1-\hat{z}_2^2=z_2^{*,1}-z_2^{*,2}$. In contrast, if $R\hat{I}>z_2^*$, then $\hat{z}_2=R\hat{I}$, a corner solution, and it must be that $\hat{z}_2^1-\hat{z}_2^2<z_2^{*,1}-z_2^{*,2}$. ■

This result comes to show that parent-bias requires specific commitment devices to decrease the scope for within-household reallocation in the future – as commitment against present-bias is typically designed in line with equation 4 rather than equation 5.

3 Empirical strategy

This section describes the design of our experiments, data collection and estimation. Subsection 3.1 discusses how we elicit parents' planned budget allocations between themselves and their child over different horizons, and how we document preference reversals, followed by a discussion of identification concerns and the design choices we implement to address them in subsection 3.2. Next, subsection 3.3 introduces how we evaluate interventions with the potential to mitigate parent-bias. Subsection 3.4 then introduces how we elicit real-life investments in children and demand for commitment outside the lab. Last, subsection 3.5 summarizes the timeline of the experiments. All details of the experimental design and a pre-analysis plan were pre-registered at the AEA RCT Registry on November 06, 2018 (AEARCTR-0003535).[23]

3.1 Documenting AGD and present-bias

We design a lab-in-the-field experiment whose structure closely matches that of the consumption model (Section 2.1). We visit participants three times: at round 1 ($t=1$ in

[23]See Supplementary Appendix N for the pre-analysis plan in full.

the model), round 2 (two days later; $t = 2$) and round 3 (a month later; $t = 3$). At round 1, respondents are asked to make consumption plans for rounds 2 and 3 (Section 3.3 discusses how we offer some participants the opportunity to commit to those plans). At round 2, they make those consumption decisions again (Section 3.3 discusses how we frame that second decision relative to round-1 allocation in different ways). At the end of round 2, one allocation (that set at rounds 1 or 2) is randomly chosen to be implemented; this ensures that both decisions are consequential. Following Augenblick et al. (2015), on the absence of commitment, the round-2 decision is implemented with 90% probability. At round 3, respondents do not make consumption plans; they only consume according to the allocation drawn at round 2.

Our sample consists of 2,413 households across 80 villages in Malawi's Salima district. Households were eligible to be enrolled in the study if both parents lived at home, if they had at least one child aged between 3 and 12 years old, and if no one in the household was allergic to peanuts. If participants households had multiple children in that age range, we randomly selected one to participate in the experiment. Our sample comprises mostly women; only 9.66% are men, as fathers were often away from home working elsewhere during daytime.

We use peanuts as the experimental currency that participants allocate between themselves and their children over time. Peanuts are a familiar and tempting good, consumed and enjoyed by both parents and children in Malawi: 88.92% of adults and 97.63% of children in our sample report enjoying the peanuts we distributed. Because Malawi is a poor country, and we undertake our experiments during the lean season, our experimental currency is payoff-relevant.

Participants are asked to split the consumption of five packages of peanuts between themselves and their child to be consumed at rounds 2 and 3. Round-1 allocations are set by splitting five tokens between two plates, labelled "My child in two days" and "Myself in two days", and five tokens between two plates labeled "My child in a month" and "Myself in a month". Round-2 allocations are set by splitting five tokens between two plates, labelled "My child today" and "Myself today", and five tokens between two plates labeled "My child in 28 days" and "Myself in 28 days". At each round, respondents can only chose integer allocations. All parents make decisions by themselves at round 1; at round 2, some of them revise allocations together with their children (see Section 3.3).

This simple experimental design allows us to capture AGD preferences and to test the main predictions of the model. First, comparing round-1 allocations set for rounds 2 and 3 allows us to define AGD parents according to the model's prediction of time-increasing budget shares allocated to children. Second, comparing round-2 allocations for rounds 2 and 3 allows us to test the model's prediction that AGD parents allocate time-increasing budget shares to their children (to a greater extent than consistent parents). Third, comparing round-1 and round-2 allocation decisions for round 3 allows us to test the model's prediction that AGD parents display preference reversals for future consumption, reallocating away from their children's planned consumption as the time gap between decisions and actual consumption decreases (to a greater extent than consistent parents).

The comparison to consistent parents remarked in parentheses in the previous paragraph is needed because our definition of AGD preferences entails *measurement error*: there are other (rational) reasons for why parents might set time-increasing budget shares allocated to children at round 1. As such, the experiment allows us to document statistical relationships between the distribution of AGD preferences and those of the behaviors associated with those preferences as predicted by theory. The higher the extent of measurement error, the less likely it is that we can precisely estimate those relationships in the data.

3.1.1 Definitions

To fix ideas, this subsection rigorously defines how we capture AGD preferences and parent-bias in the data. We define a participant as asymmetric geometric discounter based solely on their round-1 decision: if s/he allocates a larger budget share to their child to be consumed at round 3 than at round 2. Formally, let $s^k_{j,i}$ be the share of peanuts allocated to the child's consumption at $t = j$ by parent i when the choice is made at $t = k$. Asymmetric geometric discounting is then defined as: $\mathbb{1}\{\theta_i < 1\} \Leftrightarrow s^1_{2,i} < s^1_{3,i}$. We define a participant as symmetric (or consistent) if s/he allocates constant budget shares to their child in rounds 1 and 2.[24]

In turn, we define parent-bias based on whether a participant changes decisions between rounds: if s/he decreases the budget share allocated to their child's round-3 consumption when deciding at round 2 relative to when deciding at round 1. Formally, let $s^k_{j,i}$ be the share of peanuts allocated to the child's consumption at $t = j$ by parent i when the choice is made at $t = k$. Parent-bias is then defined as: $s^2_{3,i} < s^1_{3,i}$.

3.2 Identification concerns and design choices

We implement a series of design choices to ensure that our experiment captures time preferences as intended, and to rule out alternative explanations for the stylized facts that we document. We describe those identification concerns and the design choices to address them over the next subsections.

3.2.1 Fungibility

Eliciting time preferences through lab experiments in hard: when experimental currencies are fungible, subjects' behavior may reflect arbitrage opportunities and interest rates they can access outside the lab rather than their true time preferences (Augenblick et al., 2015; Cubitt & Read, 2007). What is more, decisions about how to split fungible experimental currencies between themselves and their children would be non-committal in the context

[24] For completeness, we define a participant as 'child-biased' if s/he allocates a lower budget share to their child to be consumed at round 3 than at round 2.

of our experiment, as parents could always adjust spending outside the lab to compensate for decisions made within it.

To deal with that concern, we use peanuts as our experimental currency. Enumerators observe the consumption of peanuts and their children immediately at rounds 2 and 3. This ensures that consumption plans are implemented in conformity to decisions within the experiment. Enumerators gently require that each participant consumes all peanuts in front of them.[25] We did not experience non-compliance issues.

Having said that, it could still be the case that parents adjust their children's (planned) consumption outside the lab in between decision rounds. To rule that out, we survey parents about consumption assigned to children outside of the experiment at each round, and can control for that in our empirical analysis.

3.2.2 Projection bias

Projection bias (Loewenstein et al., 2003) reflects the fact that subjects do not realize that current consumption still affects future utility. That could lead to preference reversals regardless of deviations from geometric discounting, e.g. because parents realize that themselves (their children) like peanuts less (more) at the time than they had anticipated, after having consumed those in an earlier round.

To deal with that concern, no consumption decisions are implemented at round 1, ruling out that any changes between rounds reflect previous consumption patterns. Moreover, to mitigate projection bias, participants taste a small number of peanuts before each round, and are told that the rest of the experiment will focus on this type of peanuts. Last, we survey parents about their level of hunger and their consumption of peanuts at each round, and can control for those in our empirical analysis.

3.2.3 Experimenter demand bias

Testing whether AGD preferences are predictive of allocating time-increasing budget shares to children also at round 2 could potentially confound participants' desire to be consistent across decisions rounds, rather than deep preference parameters.

To deal with that concern, we assign a different enumerator to every participant at round 2, when they were asked to make new decisions. Incidentally, consistency pressures do not seem to be systematic in our sample, as framing round-2 decisions based on round-1 allocations (as a starting point for the round-2 allocation decision; see Section 6.2) has no significant effects.

[25] This research was approved by the University of Zurich's Economics Department Institutional Review Board. Participants consent to participation according to those terms before the experiment, and can opt out of it at any point in time.

3.2.4 Shocks to the (expected) marginal utility of consumption

Reversing plans over time does not necessarily implies bias. Subjects could rationally change their planned allocations between decision rounds because of shocks that change the ratio of their (expected) marginal utility of consumption relative to that of their children at rounds 2 and/or 3.

To minimize that concern, round 2 was scheduled to take place only two days after round 1, limiting the possibility of unexpected shocks to parents' and children's marginal utilities. What is more, as far as possible, the second visit took place at the same time as the first one. We also survey parents about liquidity constraints and hunger at every round, and can control for those in our empirical analysis.

3.2.5 Asymmetric present-bias

Even though AGD preferences generate predictions that cannot be rationalized by present-bias, as discussed in Section 2.1, the preference reversals that we denote parent-bias could be driven alternatively by parents displaying present-bias to a lesser extent (or not at all) when it comes to decisions about the future consumption of their children. If that were the case, then parent-biased reversals would correlate with quasi-hyperbolic time preferences.

In order to test that hypothesis, we need to capture present-bias besides AGD preferences. To do that, we have participants make decisions across two scenarios, presented in random order. One scenario was that described in Section 3.1, whereby respondents make decisions on how to split peanuts between themselves and their children over time. The alternative scenario had participants allocated consumption overtime just for themselves – a standard inter-temporal decision problem, under three interest rates. Participants had to split the consumption of three packages of peanuts for their own consumption between $t = 2$ and $t = 3$. For each package not consumed at $t = 2$, they received r additional packages at $t = 3$; $r \in \{0.5, 1, 1.5\}$.[26] Participants are told that every decision they made could come be implemented by the enumerators with positive probability. At the end of round 2, one scenario is randomly picked to be executed (each with probability 1/2). Within that scenario, a random draw decides whether the $t = 1$ or $t = 2$ allocation is implemented. If the inter-temporal scenario is drawn, one interest rate is randomly picked (each with probability 1/3). This design ensures all decisions are consequential with positive probability, and rules out income effects across different scenarios.

We define a participant as present-biased if s/he reallocates away from their future consumption in this scenario between decision rounds, decreasing their round-3 average consumption across interest rates when making the decision at round 2 relative to when making the decision at round 1.[27]

A different way to rule out that parent-biased reversals are driven by different β's is by

[26] To help with comprehension,the enumerators showed the respondents the options they could chose from, for each interest rate. Figure B.2 shows this for $r = 0.5$.

[27] Formally, let $s^k_{3,i}(r)$ be the share of packages allocated to be received $t = 3$ by participant i when deciding at $t = k$, for interest rate r. Present-bias is then defined as:

testing directly for asymmetric quasi-hyperbolic discounting for AGD parents in our data. If parents have different betas, then the *slope* of time-increasing budget shares allocated to children should change between rounds (since, at round 2, one of the periods is in the present).

Incidentally, the experimental scenario whereby parents make inter-temporal decisions for their own consumption allows testing whether parents' (expected) marginal utility of consumption changes across decisions rounds, by testing whether the slope of consumption trajectories with respect to the interest rate changes across round 1 and round 2 (from the Euler equation, $\frac{u'(x_2^*(r_i))}{u'(x_2^*(r_j))} = \frac{1+r_j}{1+r_i}$).

3.2.6 Even splits *on average*

In the experiment, participants have to make integer allocations decisions over an odd number of packages within each period. This design choice aims at mitigating experimenter demand bias by ruling out the alternative of an egalitarian split focal point. Having said that, this feature could have brought about a different concern: unable to implement even splits within each round, some participants might have tried to set up even splits *on average* – i.e. (2,3) and (3,2) allocations to be consumed at rounds 2 and 3 by themselves and by their child, respectively, or, similarly, (3,2) and (2,3). In the latter case, we would classify those parents as AGD preferences when, in truth, they are merely trying to enforce equal splits.[28]

To deal with that concern, we re-run round 1 of the experimental *twice* in the follow-up wave – one version exactly as in the baseline, and another version allowing participants to set allocations in increments of half packages (instead of than integer units), including the possibility of splitting peanuts equally with the child within each round. The prevalence of AGD preferences across the two versions of the experiment at the follow-up is very similar (23.2% in the original version, and 20.3% in the new version), ruling out that time-increasing budget shares allocated to children are an artifact of our design choices.[29]

3.3 Mitigating parent-bias

Next, we turn to interventions with the potential to mitigate parent-bias. To evaluate the causal effects of those interventions on preference reversals driven by AGD preferences, within our experiment we cross-randomize respondents to a framing intervention and to different offers of commitment to their round-1 decisions.

$$\mathbb{1}\{\beta_i < 1\} \Leftrightarrow \frac{1}{3}\sum_{r=0.5}^{1.5} s_{3,i}^2(r) < \frac{1}{3}\sum_{r=0.5}^{1.5} s_{3,i}^1(r)$$

[28] What is more, those parents might reverse future consumption plans for their children between rounds 1 and 2 not because of parent-bias but, rather, because they are indifferent between (2,3) and (3,2) allocations at round 3.

[29] As we do not replicate round 2 in the follow-up wave, we cannot document the extent of preference reversals at that time. Nevertheless, we document that AGD preferences measured at follow-up are significantly correlated with investments in children outside the lab; see Section 3.4.

3.3.1 Framing allocation decisions

Thaler (1999) suggests that earmarking funds for specific uses could help people resist the temptation to use them for another purpose. Earmarking lock-boxes to facilitate savings for health care has been evaluated by Dupas & Robinson (2013), with mixed results (effective for emergency spending, but ineffective for preventive health care). We evaluate whether labeling budget shares as *previously allocated to children* prevents AGD parents from parent-biased reallocations in the context of our experiment.

We randomly assign participants to one out of three conditions. In the *Control* condition, participants make their round-2 decisions starting from empty plates, just as all participants do at round 1. In the *Labeling* condition, participants start from their round-1 allocation: enumerators set up the initial distribution of peanuts across plates so as to match the allocation set by each participant at the previous round. Last, in the *Anchoring* condition, participants start from a random allocation of peanuts across plates.

Across all experimental arms, participants are free to change allocations are they please, regardless of initial conditions. The labeling condition allows us to test whether salience or mental accounting could mitigate parent-bias. In turn, the anchoring condition helps rule out that, if labeling does ultimately affect AGD parents' allocations at round 2, that its effect is merely driven by anchoring on initial conditions.

3.3.2 Commitment to future plans

Next, we describe how we offer participants to the opportunity to commit to their planned allocations. At round 1, after making allocation decisions across both experimental scenarios, all participants are offered the possibility to commit to their round-1 decision by taking up a *probabilistic commitment device*, in the spirit of Augenblick et al. (2015). This device decreases the likelihood that the round-2 allocation is implemented. Without commitment, the round-1 allocation is implemented with a 10% probability; with commitment, that probability increases to 90%.[30] We elicit demand for commitment against present-bias and parent-biased separately, allowing participants' take-up decision to vary across experimental scenarios.

We randomize the price of commitment: committing to round-1 decisions require participants to forego packages of peanuts from their own round-3 allocation (0.5, 1 or 1.5, randomly drawn from a uniform distribution).[31][32] Participants were asked a series of questions to ensure that they understood how the commitment device worked before being asked whether they wanted to commit to their round-1 choice. Nevertheless, throughout our main analyses, we ignore the price of commitment to ensure that the stylized fact of time-increasing budget shares allocated to children is not driven by parents misunder-

[30] That design ensure that even those who take up commitment make allocation decisions in both round, and that all allocation decisions are consequential.

[31] Figure B.3 displays the visual aid the enumerators showed the respondents.

[32] The payment of commitment was delayed to avoid low take-up driven by impatience (Casaburi & Willis, 2018).

standing how commitment is billed (see Section 4.1).[33]

[MOVE TO APPENDIX (Deviations from PaP)] Last, a subset of participants are offered the possibility to have their child present at the time of their round-2 decision. We introduced this alternative, real-life commitment strategy, as we were interested in whether that could be used as a realistic device for parents to tie their own hands.[34] We also randomly assigned another subset of parents to have their child participate at round 2 (mandated, not by choice), to document the causal effect of child participation on round-1 and round-2 allocations.[35] While we pre-registered that we would analyze those treatment effects in the context of interventions to mitigate parent-bias, it turns out that including children as part of the decision problem yield ambiguous theoretical predictions when it comes to its effects on allocation trajectories (see Appendix M.5). For this reason, we exclude those sub-samples from the analyses of the main paper, and compile a simple model and all empirical results linked to child participation in Appendix M.5.

Table B.1 summarizes the randomization process and the different treatment arms.

[Table B.1]

3.4 Investments and demand for commitment outside the lab

3.4.1 Real-life investments in children

Even though Section 2.2 establishes that the patterns for how AGD parents allocate consumption over-time between themselves and their children in the absence of inter-temporal trade-offs should also translate to the investment case, the artificial setup of our experiment has limits in what it allows us to say about the connection between AGD preferences and investments in children outside the lab. For this reason, we survey parents about real-life investments in children's health and education at the end of our experiment, to study whether AGD parents actually invest less in their children than symmetric geometric discounters.

Naturally, there are several challenges in documenting a causal relationship between AGD preferences and lower investments in children outside the lab. First, as discussed in Section 3.1, AGD preferences are measured with error, as there are other reasons for why parents might set time-increasing budget shares allocated to children at round 1 and because there is noise in how preferences translate into economic choices (Woodford, 2020). We already mentioned that measurement error only allows us to detect statistical relationships in the data, and makes it less likely that we are able to detect those relationships.

Second, even in the absence of measurement error, the preferences elicited through our experiments might not be the ones relevant for decisions outside the lab. As we only

[33] Section 5.3 shows that the correlation of AGD preferences with time-increasing budget shares allocated to children at round 2 and parent-biased preference reversals are robust to adjusting round-3 budget shares to the price of commitment.

[34] The cost of taking up that option was also defined in terms of having parents forego packages of peanuts from their round-3 allocation (0, 0.5, 1 or 1.5, randomly assigned).

[35] In that case, parents learned that this would be the case at the beginning of round 1.

capture time preferences of one decision-maker in the household (mostly mothers), it might be that real-life investment decisions reflect a combination of other preferences or even that, in an extreme case, are entirely determined by preferences we cannot observe.

Third, such preferences are not randomly assigned, such that AGD parents might display other characteristics associated with higher investments in children when contrasted with other parents (rather than with their conterfactual symmetric selves).

With those caveats in mind, we estimate the correlation between AGD preferences and real-life investments in children. At the end of round 3, we survey parents about actual investments in their children's education and health in the recent past, restricting attention to the child involved in the experiment. For children younger than 5 years old, we elicit expenses in preventive health care, child nutrition and early childhood programs, whether the child was subject to regular medical check-ups and attendance of early childhood development programs (these last two to capture parents' opportunity cost of time). For children older than 5 years old, we elicit school attendance, educational expenses incurred by parents, and parental engagement in their children's education (the latter to capture parents' opportunity cost of time).

We try to mitigate concerns about AGD preferences conflating other individual preference parameters by controlling for a range of parents' characteristics, including their discount rate δ – inferred from the inter-temporal experimental scenario in the experiment with the help of parametric assumptions.

3.4.2 Commitment to real-life investments children

The extent to which AGD parents demand commitment in the lab could be a misleading indication of their demand for commitment outside of it, especially in the presence of experimenter demand bias. For this reason, in the follow-up wave we elicit parents' willingness to commit resources to a real investment in their children's education.

To do that, we enter study participants into a lottery for a chance to win 2,000 kwachas (about 2 dollars at the time of the experiment), which they would receive approximately two months after the survey in case they were drawn. Before learning the outcome of the lottery, parents could choose to either receive the prize in cash or to commit those proceeds to one week of tutoring for their child (1 hour a day, for a week), delivered by a local NGO. Outside of our study, 1 week of daily 1-hour tutoring sessions costed exactly that nominal amount. To elicit parents' willingness-to-pay for commitment, the flexible option comes with extra cash (a bonus), and we ask participants to choose between flexibility or commitment for different bonus amounts. At the end of the survey, one bonus is randomly picked and the participants' decision for that amount is implemented – a Becker-DeGroot-Marschak (BDM) mechanism (Becker et al., 1964) that ensures that respondents' answers are incentive-compatible.[36] Since attaching a bonus to the flexible option may induce

[36]To ensure that the participants understand this experiment, they first do a practice run to measure their willingness-to-pay for a bar of soap.

experimenter demand bias (although not differentially for AGD parents), we also ask participants to chose between the flexible or commitment option with extra cash associated with commitment option, following Carrera et al. (2019).[37]

[MOVE TO APPENDIX (Deviations from PaP)] We also investigate whether AGD parents have a higher willingness-to-pay to open a savings account in their their child's name in a local bank, using a similar elicitation mechanism.[38] While we pre-registered that we would analyze that decision together with parent's demand for committing lottery proceeds to tutoring their child, the former is actually closer to labeling – since there are no constraints preventing legal guardians from withdrawing cash from their child's account before they turn 18. As such, we restrict attention to tutoring in the main text, and present additional results in Appendix **??**.[39]

3.5 Timeline

Figure K.3 presents the timeline of our experiments. Our baseline experiment was conducted between November 2018 and January 2019, and the follow-up experiment, between June and September 2019. Both experiments followed the same sample of households.

[Figure K.3]

4 New stylized facts about parents' dynamic allocations

This section documents the two new stylized facts about how parents systematically plan to and effectively allocate resources between themselves and their children over time, in subsections 4.1 and 4.2.

4.1 Stylized fact #1: For many parents, budget shares allocated to children *increase with the time gap* between the decision and actual consumption

Figure 2 presents the distribution of parents' round-1 allocations to their child's consumption at rounds 2 and 3. The distribution of children's planned consumption at round 3 is shifted to the right relative to that at round 2. In other words, parents tend to be more generous towards their children further in the future, the larger the time gap between the decision and actual consumption.

[37] This has the additional advantage of allowing us to assess how well understood this part of the experiment was by identifying respondents who exhibit a positive WTP for both the flexible option and the commitment. This is the case for only 17% of respondents who had chosen to commit lotter proceedings to tutoring; we exclude those participants from our regression analyses.

[38] We enter participants into a lottery for a chance to win 10,000 kwachas. Before learning the outcome of the lottery, parents could choose to either receive the prize in cash or to commit half of the lottery proceeds to a savings account in their child's name. When the latter option was chosen, our team accompanied the lottery winner and the child to the bank and assisted them with the paper work.

[39] We also pre-registered that we would assess parents' demand for commitment to their initial decision. This is equivalent to double commitment, anticipating that parents might deem their decision before learning the lottery outcome as cheap talk. We also present those results in Appendix **??**.

[Figure 2]

Could time-increasing budget shares allocated to children be an artifact of our experimental design, which only allows for integer allocations, leading some parents to allocate even splits *on average* (see Section 3.2.6)? Figure 2 shows that, consistent with the desire to be egalitarian across rounds, the majority of parents choose [(2,3);(3,2)] or [(3,2);(2,3)] allocations – where the first and second parentheses indicate allocations to be consumed at rounds 2 and 3 by themselves (first argument) and by their child (second argument), respectively. Having said that, even among such egalitarian parents, 38.91% choose [(2,3);(3,2)] and 51.96% choose [(3,2);(2,3)]; as such, the majority is more generous towards their child later in the future. Moreover, Figure 3 shows that allowing for non-integer allocations (including even splits in the parents' budget set within each round) at the follow-up wave still yields the same stylized fact.

[Figure 3]

Figure 3's left-hand-side panel displays round-1 allocations at the follow-up wave when parents were only allowed to choose integer allocations, while the right-hand-side panel displays allocations when half-package increments were allowed (including even splits within each period). In the RHS panel, 17.63% of parents still allocate increasing budget shares to their children (compared to 22.93% in the LHS panel).

At round 2, we observe very similar patterns. Figure 4 shows that, given the chance to revise their round-1 decisions, it is still the case that the distribution of consumption plans allocated to children at round 3 is shifted to the right relative to that set at round 2. 29% of parents allocate increasing shares of consumption to their children over time at that point, after which they have no further possibility of revising consumption plans. Among those parents, the average round-3 allocation set to children is 17.7% larger than that set for immediate consumption.

[Figure 4]

The incidence of time-increasing budget shares allocated to children is uncorrelated with liquidity constraints or hunger, minimizing concerns that this pattern is merely driven by fungibility with consumption outside of the experiment or projection bias.[40] Having said that, there are other, rational reasons for why parents might set time-increasing consumption patterns for their children relative to their own: in particular, they might expect different paths for their marginal utility of future consumption, e.g. because they might attribute different survival probabilities to themselves and their children. In turn, preference reversals cannot be accommodated by such rational expectations, especially when it comes to reallocation away from consumption plans still in the future, only two days after those plans were set. The next subsection turns to this new stylized fact.

[40] The correlation coefficient between time-increasing budget shares allocated to the child and self-reported liquidity constraints is -0.03 ($p = 0.67$), and that between the former and hunger is -0.01 for that reported by parents ($p = 0.76$) and 0.02 for that reported by children ($p = 0.52$).

4.2 Stylized fact #2: Many parents revise planned allocations to children downward *even before consumption time*

For this analysis, we restrict attention to parents in the control group of the framing intervention, to capture the extent of preference reversals in the absence of interventions with the potential to mitigate them. The probabilistic commitment device does not affect our analysis because round-2 decisions can still be implemented with positive probability (see Section 3.3.2). Figure 6b presents average round-1 and round-2 budget shares allocated to children to be consumed at round 3. 14.5% of parents reallocate away from their children's planned future consumption when given the opportunity to revise their decision, only two days later.

[Figure 6b]

The extent of reallocation is large. Figure 6b documents a 36.5% reduction in the share of consumption allocated to the child between decision rounds. The incidence of reallocations away from children's future consumption is uncorrelated with changes in liquidity constraints or hunger between rounds.[41]

It is useful to compare those reallocations to those induced by present-bias, captured by the alternative experimental scenario in which parents set inter-temporal allocations for their own consumption. In that scenario (also restricting attention to parents in the control group of the framing intervention), 24% of the parents reallocate away from future consumption – a larger share of parents who reallocate away from their children's future consumption in the main experiment. Nevertheless, the magnitude of present-biased reallocations is smaller: for those parents, round-3 consumption decreases by 29.4% on average.

5 Testing the model's predictions

Our simple model yields testable predictions connecting AGD preferences to the two stylized facts from the previous section. First, AGD parents' budget shares allocated to children should increase with the time gap between the decision and actual consumption (to a greater extent than symmetric parents). Second, AGD parents should revise future allocations to children downward as the decision horizon gets shorter (to a greater extent than symmetric parents). Subsections 5.1 and 5.2 test these hypotheses, followed by robustness tests in subsection 5.3. Next, subsection 5.4 computes welfare losses associated with AGD preferences by calibrating the investment model from Section 2.2 with the estimates based on our experimental findings. Last, we evaluate whether AGD preferences are predictive of real-life investments in children in subsection 5.5.

[41] The correlation coefficient between time-increasing budget shares allocated to the child and changes to self-reported liquidity constraints between rounds is 0.02 ($p = 0.58$), and that between the former and changes to hunger between rounds is 0.03 for that reported by parents ($p = 0.36$) and 0.04 for that reported by children ($p = 0.31$).

5.1 Prediction #1: AGD parents' budget shares allocated to children increase with the time gap between the decision and actual consumption

We categorize 31.1% of parents in our sample as AGD, based on setting time-increasing budget shares to their children at round 1.[42] The model predicts that AGD parents should allocate time-increasing budget shares to their children also at round 2 (to a greater extent than symmetric parents).

We test this hypothesis formally with the following regression:

$$s^2_{j,i} = \alpha + \gamma_0 (j-2) + \gamma_1 (\mathbb{1}\{\theta_i < 1\}) + \gamma_2 (\mathbb{1}\{\theta_i < 1\} \times (j-2)) + \lambda X_i + \varepsilon_{ij}, \tag{7}$$

where $s^2_{j,i}$ is the share of peanuts parent i allocates to their child at $t = 2$ to be consumed at $t = j \in \{2, 30\}$; $(j-2)$ is the number of days between the decision and consumption; $\mathbb{1}\{\theta_i < 1\}$ equals 1 if parent i is AGD, and 0 otherwise; X_i a vector of individual characteristics; and ε_{ij} is an error term. We are interested in testing $\gamma_2 \geq 0$.

Table 2 presents the results, restricting attention to the control group of the framing experiment. At round 2, both AGD and symmetric parents systematically set time-increasing budget shares to their children, but the former do so to a significantly greater extent. While children of symmetric parents receive on average a 12.38 p.p. higher share of peanuts to be consumed in 28 days than in the present at that point in time, those of AGD parents receive a 22.62 p.p. higher share in the future. Such increase in future budget shares allocated to children of AGD parents is statistically different from that of symmetric parents (at the 1% level) and large, equivalent to 38.7% of the mean budget share allocated to children over all parental decisions in our sample.

[Table 2]

Strikingly, Table 2 documents that, despite ambitious plans, AGD parents actually allocate systematically *less* to their children in the present: at round 2, they set a 1.94 p.p. lower share of peanuts to their children's consumption relative to symmetric parents; such difference is statistically significant at the 1% level.

5.2 Prediction #2: AGD parents decrease budget shares allocated to children in the future as it gets closer to consumption time

The model predicts that, between rounds 1 and 2, AGD parents should decrease budget shares allocated to their children at round 3 (to a greater extent than symmetric parents).

[42] Table 1 summarizes the distribution of AGD preferences in our sample. Panel A ignores the price of commitment to avoid artificially inducing time-increasing budget shares allocated to children if parents misunderstood that it was billed from their round-3 allocation (see Section 3.3.2); Panel B subtracts the price of commitment. Panel B documents that, in that case, 76.3% of parents set time-increasing budget shares allocated to children at round 1.

We test this hypothesis formally with the following regression:

$$s^k_{30,i} = \alpha + \gamma_0 \left(\mathbb{1}\{k=2\}\right) + \gamma_1 \left(\mathbb{1}\{\theta_i < 1\}\right) + \gamma_2 \left(\mathbb{1}\{\theta_i < 1\} \times \mathbb{1}\{k=2\}\right) + \lambda X_{ik} + \varepsilon_{ik}, \quad (8)$$

where $s^k_{30,i}$ is the share of peanuts parent i allocates to their child at $t = k \in \{0, 2\}$ to be consumed at $t = 30$; $\mathbb{1}\{k = 2\}$ equals 1 for allocation decisions undertaken at round 2, and 0 otherwise; $\mathbb{1}\{\theta_i < 1\}$ equals 1 if parent i is AGD, and 0 otherwise; X_{ik} a vector of (time-varying) individual characteristics; and ε_{ik} is an error term. We are interested in testing $\gamma_2 \leq 0$.

Table 4 presents the results, restricting attention to the control group of the framing experiment. At round 2, AGD parents reallocate 11.8 p.p. more away from their children's future consumption plans set just two days before relative to symmetric parents (Column 1). This is a large effect size, 18.3% of the average budget share allocated to children, and statistically significant at the 1% level. While present-biased reversals are constant over any decision horizon, parent-biased ones are proportional to the reduction in the time gap to consumption since the original decision. Under parametric assumptions, our estimates are consistent with a 48% reallocation away from children's consumption over a 30-day horizon, roughly the same magnitude as present-biased preference reversals in our sample.[43,44]

[Table 4]

Last, Column (2) in Table 4 shows that present-bias does *not* predict parent-bias in our sample. If anything, parents with quasi-hyperbolic discounting preferences *increase* their children's round-3 allocation between decision rounds (by 0.35 p.p., not statistically significant).

5.3 Robustness checks

We implement design choices in our experiment to minimize concerns with issues such as fungibility, projection bias and experimenter demand bias, as discussed in Section 3.2. Moreover, Section 4 showcases that the stylized facts are not an artifact of only allowing integer allocations, and presents evidence that liquidity constraints and projection bias (or changes in those) are uncorrelated with the prevalence of the behaviors characterized by the two facts across parents in our sample. In this subsection, we compile additional evidence against concerns about fungibility of our experimental currency with consumption outside

[43] Table **??** estimates how the log ratio of each parent's $t = 3$ consumption to that of their child changes with the time horizon for AGD parents. That allows us to back out an average value of θ of 0.89. Over a 30-day time horizon, those time preferences would translate into a 48% reallocation away from the child's consumption; see Appendix **??**.

[44] Present-bias is associated with a 39.6 p.p. reallocation from future consumption in our sample (see Table 3), 50% of the average round-1 allocation to be consumed 30 days later.

of our experiment, and against other potential explanations for the stylized facts that we document, namely shocks to the (expected) marginal utility of consumption between rounds, asymmetric quasi-hyperbolic discounting, and correlation of AGD preferences with other preference features.

5.3.1 Fungibility

Appendix I documents that parents do not systematically adjust their children's consumption outside of our experiment to compensate for allocation decisions within it. Table I.1 shows no correlation between the time of children's last meal (reported by parents) and budget shares allocated to children at round 1 (Column 1) or round 2 (Column 2). While parents might misreport that outcome due to experiment demand bias, using children's self-reported hunger instead yields identical results (Columns 3 and 4).

5.3.2 Shocks to the (expected) marginal utility of consumption

To rule out that correlated shocks before each round induce a spurious correlation between our measure of AGD preferences and the behavior behind the two stylized facts, we estimate whether parents' round-2 consumption in the inter-temporal experimental scenario responds differentially to interest rates across decision rounds. As explained in Section 3.2, in an interior solution, FOCs equalize the ratio of marginal utilities of consumption as a function of different interest rates within each round to the ratio of those gross interest rates; as such, shocks to parents' marginal utility need to translate into changes in their responses to interest rates. Table H.1 shows that is not the case: the slope of parents' round-2 consumption with respect to interest rates does not systematically changes from round 1 to round 2.

5.3.3 Asymmetric present-bias

As discussed in Section 2.1, asymmetric quasi-hyperbolic discounting (when parents display different β's when it comes to their own future consumption and that of their children) can rationalize why parents deviate from planned allocations when consumption becomes immediate but *not* as the time gap between decisions and actual consumption decreases when consumption plans are *still in the future*. In our experiment, asymmetric quasi-hyperbolic discounters would reduce their children's budget share to be consumed at round 2 when given the opportunity to revise allocations, but their round-1 and round-2 allocations set for round 3 (which remains in the future at the later round) would remain unchanged. Appendix G presents numerical simulations for parental allocations under different time preferences, highlighting the different patterns predicted by each for how budget shares allocated to children should change over time and between decision rounds.

Above and beyond the conceptual distinction, Panel A in Table A.1 shows that, in our data, there is no correlation between preference reversals across the different experimental scenarios that parents decide on. In other words, parents who reallocate away from initial plans when it comes to their own future consumption between decision rounds are not typically the ones who reallocate away from their children's future consumption. A Fisher exact test documents that the distribution of present-bias in our sample does not systematically differ by whether participants display present-bias (p-value= 0.47).

[Table A.1]

We can test directly for asymmetric quasi-hyperbolic discounting for AGD parents in our data by assessing whether, among those parents, the *slope* of time-increasing budget shares allocated to children systematically changes between rounds (see Section 5.3.3). We fail to reject that hypothesis. What is more, we estimate that such slope is the same among parents who reallocate away from their children's consumption between decision rounds (see Appendix G). We conclude that parent-bias is not driven by asymmetric quasi-hyperbolic discounting in our sample.

5.3.4 Correlation with other preference features

Last, we consider whether differences in within-household reallocation between AGD parents and others could be driven by the fact the former are also systematically different when it comes to other preference parameters, e.g. the extent to which they are exactly indifferent about how to split the budget between themselves and their children within each period.

To capture other characteristics that might influence the way parents' allocate resources between themselves and their children over time, we control for a range of individual characteristics, including the budget share each parent allocates at round 1 to be consumed by their children at round 2 ($s^1_{2,i}$). Table 5 shows that including those controls does not change the results: if anything, controlling for $s^1_{2,i}$ actually *increases* the extent of reallocation induced by parent-bias relative to symmetric parents.

[Table 5]

5.4 Welfare implications

This subsection estimates welfare losses due to parent-bias by calibrating our a simple investment model with the estimates based on our experimental findings. Welfare comparisons for subjects with time-inconsistent preferences are challenging, since welfare analyses traditionally assumes stable preferences (Bernheim & Taubinsky, 2019). Analogously to O'Donoghue & Rabin (1999), we use the symmetric geometric discounter agent as the normative standard ($\theta = 1$). We further assume that this agent's decision at $t = 1$ perfectly overlaps with the child's preferences at that point.

This allows us to derive the parent's long-run utility, and estimate welfare losses as the monetary compensation that a symmetric agent would require to achieve the same long-run utility as an AGD parents (as a % of their income).

As in Section 2.2, we assume that the parent's and the child's instantaneous utility function is CRRA, with coefficient of relative risk aversion γ. We calibrate welfare calculations with the median values of α and δ among AGD respondents in our sample (inferred from allocation decisions with the help of this functional form assumption), and with $\gamma = 1.73$ (the average value of the CRRA coefficient in Holden & Quiggin, 2017 within a sample of Malawian subjects). All details of the calibration are presented in Appendix K.

Figure 11 plots the income that a symmetric agent would require to obtain the same long-term utility as naive/sophisticated AGD agent with an income of $y = 1$. Our simulations showcase large welfare losses due to AGD preferences. At $\theta = 0.94$, the median value of θ among AGD parents in our sample, naive agents reach a long-term utility level equivalent to that of a symmetric agent with 23% lower income. Sophistication only partially mitigates welfare losses: sophisticated AGD agents' long-term utility is equivalent to that of a symmetric agent with 19% lower income.

[Figure 11]

5.5 Investments in children outside the lab

In this subsection, we assess whether AGD preferences are predictive of real-life investments in children. As discussed in Section 3.4, we analyze the correlation between AGD preferences and self-reported investments in children's education and health.

Since we elicit multiple outcomes to capture parental investments in children, we control for family-wise error rate in the context of multiple hypotheses testing by building a summary measure of investments in children Kling et al. (2007). We pre-registered that we would analyze separately investments in children between 3 and 5 years old and those between 6 and 12 years old. Each summary index measure is the equally weighted average of its standardized components.[45] We normalize each component by the mean and standard deviations of symmetric parents.

In the analyses, we control for individual characteristics, including the extent to which parents discount their own future consumption elicited in the inter-temporal experimental scenario and their degree of altruism towards their children.[46] We allow the effects of AGD to vary with parent' discount rate δ_i.

We formally test whether AGD preferences are predictive of real-life investments in children with the following regression:

$$Y_i = \gamma_0 + \gamma_1 \left(\mathbb{1}\{\theta_i < 1\}\right) + \gamma_2 \hat{\delta}_i + \gamma_3 \left(\mathbb{1}\{\theta_i < 1\} \times \hat{\delta}_i\right) + \lambda X_i + \varepsilon_i, \tag{9}$$

where Y_i is a summary measure of investments in parent i' child; $\mathbb{1}\{\theta_i < 1\}$ equals 1 if parent i is AGD, and 0 otherwise; $\hat{\delta}_i$ is parent i's discount rate inferred from the experiment; X_i is a vector of individual characteristics, including parent i's altruism, inferred from the experiment; and ε_i is the error term. We are interested in testing $\gamma_1 \leq 0$ and $\gamma_3 \leq 0$.

[45] Appendix **??** presents all details.

[46] Appendix **??** details how we compute those preference parameters for each participant.

Table 11 shows the results, restricting attention to 3-5 year-olds in Column (1) and 6-12 year-olds in Column (2). Both columns showcase similar patterns: among parents who value their future consumption the least, i.e. with $\hat{\delta_i} = 0$, AGD preferences correlates with higher investments in children. While, as expected, higher patience (higher $\hat{\delta_i}$) is associated with larger investments in children, the more patient the parent is, the more AGD preferences *decreases* investments in children. That pattern is the largest in magnitude and precisely estimated for younger children . In column (2), AGD preferences are associated with a 0.1 standard deviation increase in investments in children for parents who do not put any value on their own future consumption. However, this positive association decreases as parents value their own future consumption more, i.e. for higher $\delta\theta$: those parents who value their future consumption as much as their present one ($\theta\delta = 1$), AGD is associated with 0.06 s.d lower investments in children than equally patient parents.

[Table 11]

Those results come to show that AGD preferences matter for investments in children not only within the context of our experiment, but also when it comes to real-life investments in health and education, especially for the younger children. Given that AGD preferences are reasonably prevalent, parent-bias is likely a critical driver of why parents systematically under-invest in children in developing countries despite high returns. In contrast, present-bias does *not* significantly predict investments in children in our sample.

6 Testing interventions to mitigate parent-bias

In this section, we analyze two interventions designed to mitigate parent-bias reversals: reminding parents of their initial allocation decision in subsection6.2 and imposing that the child participates in the second round decision in subsection F. In that last subsection, we also measure the parents' willingness-to-pay to let their children participate in the second round decision.

6.1 Balance tests

Table A.2 shows that our sample is balanced across the framing treatment arms. More than 90% of parents in our sample are mothers, as fathers are often absent from home during the day. About 15% of households in our sample are Muslim, the rest identify as Christians. On average, the household has 2.2 children aged between 3-12 years old. The average age of a child taking part in our experiment is seven years old, and we manage to recruit an equal number of boys and girls. Respondents in our sample are poor: we measured credit constraint as the amount of money they reported being able to gather within a week for an emergency: on average, respondents would access three dollars. Finally, indexes of investments in children and health are balanced across treatment arms.

About 97% of respondents in our sample participate in all three waves of data collection. Table A.3 shows that attrition is uncorrelated with most treatment arms and baseline characteristics. One treatment arm and one baseline covariate are significantly correlated with non-attrition, but the significance is marginal ($p < 0.10$) and the differences are very small in magnitude and represents less than 4% of the average non-attrition in the whole sample.Tables A.4 and A.5 display balance tests for all different treatment arms. Whenever there are statistically significant differences in baseline covariates between non-attrited and attrited observations, they are very small in magnitude. We include baseline covariates as control in all our analyses.

[Table A.2]

6.2 Framing consumption decisions

Can reminding AGD respondents about their previous allocation decision mitigate preference reversals? To do so, we study the effect of the *labelling* treatment, in which parents started their within-household choice in round 2 with enumerators placing their round 1 set allocations in front of them, while respondents in the *Control* treatment started from empty plates. This subsection then turns towards distinguishing the effect of labelling vs. anchoring the respondent on a random allocation at the beginning of round 2.

We study the impact of labelling, and, more generally, of our experimental treatments by running the following regression:

$$\Delta s_{3i} = \alpha + \gamma_0 \mathbf{T_i} + \gamma_1 \mathbb{1}\{\theta < 1\} + \gamma_2 \mathbb{1}\{\theta < 1\} \times \mathbf{T_i} + \lambda X_{ki} + \epsilon_{3i} \tag{10}$$

where Δs_{3i} is the difference in share of peanuts to be consumed by subject i's child at $t = 3$ when making the decision at $t = 2$ instead of $t = 1$; $\mathbf{T_i}$ is a dummy variable equal to one if the respondent receives treatment $\mathbf{T}$, 0 otherwise.

Column (1) of table 8 presents the results from this regression. On average, labelling is associated with a 1.28pp. increase in the share of peanuts to be consumed by the child between consumption rounds. This effect is statistically insignificant. What's more, labelling does not mitigate the effect of AGD: for AGD parents, it is associated with a 0.2pp. additional decrease in the child's share of consumption. It is a very small effect, which is not statistically significant.

[Table 8]

In a robustness check, we assess whether the effect from labelling can be distinguished from the effect of anchoring on a random allocation. Remember that respondents in the *Anchoring* sub-sample started from plates filled with a random allocation. Column (2) of table 8 compares the effect of the two treatments in a sample restricted to the *labelling* and *Anchoring* sub-samples. Anchoring is associated with a 1.6pp. larger reallocation towards the child's consumption than labelling, an effect which is not statistically significant. For AGD respondents, anchoring is associated with a 1.69pp. further decrease in the child's share of consumption. Both of those effects are statistically indistinguishable from zero. The effect of labelling is not statistically different from that of anchoring the parents' decision with a random allocation.

Appendix M.2 explores whether the effectiveness of labelling varies alongside children's characteristics among parent-biased respondents. There does not seem to be significant variation alongside any of the potential heterogeneity traits we study.

On average, labelling seems to be an ineffective strategy to mitigate the AGD respondents' parent-biased reallocations.

6.3 Commitment to future plans

The previous section showed that the preference reversals associated with AGD were difficult to mitigate in the absence of commitment. This section studies whether AGD parents are sophisticated about their preference reversals (subsection 6.3.1), are more likely to demand commitment that increases the probability that their initial allocation decision is executed (subsection 6.3.2) and whether our experimental measure of AGD predicts the take-up of different types of commitment devices outside the lab (subsection 6.3.3).

6.3.1 Sophistication

As underlined in section2, sophistication about AGD matters to explain investments in children. In our experiment, we use parents' predictions about their future behavior as a measure of sophistication, following Augenblick & Rabin (2019) and Toussaert (2018). We have two strategies to elicit parents' beliefs about the future.

Our first strategy is to elicit parents' beliefs about the future behavior of other respondents. We let the respondents know that: "..we are asking many other households to make those decisions. Do you think that two days from now most other people will choose to give LESS / MORE / THE SAME AMOUNT OF peanuts to the child than they did today?" Those beliefs are incentivized: if respondents have accurately guessed the behavior of the majority of other respondents they get two extra packagets of peanuts. Respondents learn at the end of round 3 whether their guesses were correct.

Our second strategy is to ask the respondents to make prediction about their own future behavior. Those predictions are unincentivized in order not to distort round 2 decisions.

The literature suggests that beliefs about one's own future behavior inform one's beliefs about others (Toussaert, 2018) and this seems to hold true in our sample. When excluding the "Don't know" answers, the correlation between beliefs about others' future behavior and one's own is 0.241 ($p < 0.01$) in the case of the within-household choice and 0.225($p < 0.01$) in the case of the inter-temporal choice. Figures 7b and 7a show the distribution of beliefs about one's own future behavior, per prediction about others in the inter-temporal and within-household choices respectively.

[Figure 7b and 7a]

We define sophisticated present-biased respondents as respondents who reallocated away from $j = 3$ consumption when making the decision at $k = 2$ and predicted this behavior at $t = 1$ and sophisticated parent-biased respondents as respondents who reallocated away from the child's consumption when making the decision at $k = 2$ and predicted this behavior at $t = 1$.

Table 6 compares the percentage of sophisticated respondents among biased parents. The share of biased respondents who assumed that others would also be time-inconsistent is the same for both decisions: 36-38% of time inconsistent parents guessed that others would be inconsistent as well. In contrast, we observe more reluctance to predict their own reallocation away from the children in Scenario B than from the future in Scenario A. Indeed, 21.6% of respondents who reallocated away from future consumption in decision A predicted this behavior in round 1 but only 15.7% of parents who reallocated away from their child's consumption in decision B predicted it. Those two percentages are significantly different.

Those differences can inform the design of commitment devices targeting reallocation away from children. Parent-biased respondents seem to be aware that there is a general tendency to reallocate consumption away from the children, but are reluctant to admit that they follow this pattern. For instance, framing commitment devices as a way to signal a desire to stick to a generous consumption plan may be a more effective way to increase take-up than framing them as a remedy to self-control issues.

[Table 6]

Next, we extend the definition of sophistication to include all subjects (biased or not) who acted as anticipated in each scenario. The joint distribution of within-household and inter-temporal sophistication is in table 7. We learn that sophistication is not homogenous across domains: less than 9% of subjects are sophisticated about both inter-temporal and within-household behavior, and 35-40% of subjects are sophisticated in one domain only. The correlation between being sophisticated regarding one's own behavior in the inter-temporal and the within-household task is very small: 0.0166 and insignificant ($p = 0.4191$). Again, this matters for targeting. If parents are sophisticated regarding their time inconsistencies in one dimension but not the other, designing commitment devices to attract present-biased respondents may leave behind a large fraction of parent-biased subjects who could benefit from commitment.

[Table 7]

6.3.2 Demand for commitment in the lab

A subset of parents were offered a probabilistic commitment device that increased the probability that their round 1 choice was implemented over their round 2 choice, at different prices.

Figure 8 shows that the take-up of the probabilistic commitment device is high, even at high prices. More than 91% of respondents offered the possibility to take up the probabilistic commitment device in exchange of 0.5 or 1 packet of peanuts took it up in Scenario B, as did almost 88% of those offered the commitment device against 1.5 packets of peanuts.

[Figure 8]

Our main empirical specification to test whether parent-biased respondents demand more of each commitment device will be:

$$Y_i = \alpha + \gamma_0 \mathbb{1}\{\theta < 1\} + \gamma_1 Price_i + \gamma_2 Price_i \times \mathbb{1}\{\theta < 1\} + \lambda X_i + \epsilon_i \tag{11}$$

where Y_i is a dummy variable equal to one if the respondent took up the commitment device and $Price_i$ is the price at which the commitment device was offered.

AGD is associated with a 6.78 p.p. increase in the take up of the commitment device at null prices and a 1pp. increase in the take up of commitment when the price of commitment is one packet of peanuts. In column (2), we introduce price fixed effects to identify the average correlation between AGD and the take up of commitment. AGD is now associated with a 1pp. increase in the take-up of commitment on average, an effect which is small in magnitude (from an average uptake of 90%) and statistically insignificant. Columns (3)-(4) assess whether parents' beliefs over their future reallocation behavior predicts their reallocation decision. Column (4) explicitely shows that, at higher prices, parents who believe that others are more likely to reallocate towards themselves are also more likely to purchase the commitment device, suggesting that sophistication plays a role. In appendix M.4, we find that, on average, AGD parents of boys are 3.5pp. more likely to purchase commitment than non-AGD parents of boys, but that this association disappears for girls.

[Table 9]

As a benchmark, we also offered respondents the possibility to commit to a similar commitment device after making their Scenario A choices. This enables us to compare whether present-bias drives the demand for commitment in the inter-temporal choice. Columns (5-6) of table 9 formally test whether present-bias is associated with a higher take-up of the commitment device in Scenario A's inter-temporal allocation task . In columns (3) and (4), we study whether present-bias is associated more strongly with commitment uptake in Scenario A and we find that it is not the case. We find that the correlation between present-bias and the take up of commitment is negative, statistically insignificant and close to zero at all prices .

To summarize the findings of table 9: at low prices, AGD drives the demand for commitment to help respondents stick to their shared consumption plans, while present-bias does not drive the demand for commitment to a within-household allocation plan nor does it drive the demand for commitment in a traditional inter-temporal allocation task. In the setting of our experiment, commitment devices specifically designed to target parent-bias are therefore more likely to be taken up by the targetted beneficiaries than traditional commitment devices targeting present-bias. This is an important finding in light of the literature that has not found a systematic correlation between the uptake of commitment devices targetting present-bias and quasi-hyperbolic discounting (Bryan & Nelson., 2010; Laibson, 2015).

6.3.3 Demand for commitment outside the lab

In this subsection, we check whether AGD predicts whether parents will choose to purchase tutoring for their children when offered to do so, a real-stake decision. To do so, our analysis relies on the regression specified in equation (7), with the outcome variable being a dummy equal to one if the parents choose to purchase tutoring instead of receiving 2,000 MK, before learning the outcome of the lottery as described in subsection 3.4.1.

The take-up of tutoring is high: 84% of parents choose to receive tutoring for their child over receiving 2,000 kwachas in cash cards and 32% of those have a non-zero willingness-to-pay to commit to that decision.

[Table 12]

Table 12 shows that asymmetric geometric discounters are less likely to choose to receive tutoring over cash. The regression in column (1) shows that AGD respondents are 5.2pp. less likely to choose the tutoring for their children than non-AGD respondents. That effect is statistically significant but modest in magnitude as it represents about 7% of the mean of the outcome variable. The estimated effect remains constant when excluding children for which that question was asked hypothetically (children younger than six years old). The regression in column (3) looks at the combined effect of the parents' discount factor and AGD. Having a higher discount factor, hence valuing the future more, significantly increases the probability that parents will purchase the tutoring. However, being AGD undo this positive association: all AGD parents are less likely to choose the tutoring and AGD parents with a higher discount factor disproportionately so. This is in line with the findings of the previous subsection. In this specification, the coefficients on the interaction term and on the AGD dummy are neither statistically significant individually or jointly.

In the appendix, we conduct a series of robustness checks of this relationship between AGD and the willingness to invest in the tutoring. In columns (1)-(3) of table Q.2, we exclude households who did not have a school-age child and for whom the question had been asked hypothetically. Column (1) shows that being AGD remains associated with a statistically significant 5.2 pp. lower propensity to purchase the tutoring. Column (2) shows that a higher $\delta\theta$ is associated with a higher propensity to invest in tutoring but that AGD completely reverts this positive association.

In table Q.3, we define AGD based on the respondents' choice in the version of the game where equal splits are allowed. The same patterns emerge, although are less precisely estimated and smaller in magnitude. Column (1) shows that being AGD as measured in this manner reduces the propensity to pick the tutoring by 2.97pp., an estimate that is smaller in magnitude than in other specifications and statistically insignificant. Column (2) further finds that this measure of AGD still reverts the positive association between $\delta\theta$ and the propensity to chose the tutoring, but none of the estimates are statistically significant.

In columns (4)-(7) of table 12, we study the respondents' willingness-to-pay to commit to the tutoring decision. Columns (4) and (5) shows that AGD, alone or in combination with the discount factor, does not predict the probability that the respondent never commits to the tutoring decision, even at price zero. However, column (6) detects that, among respondents who are willing to commit, AGD decreases the willingness-to-pay for commitment by 63 kwachas, an effect which is statistically significant and large in magnitude (24% of the mean of the outcome variable). Surprisingly, the regression in column (7) finds that the parents' discount factor does not predict their willingness-to-pay for commitment, unless when interacting with AGD. Indeed, all AGD respondents are willing to pay less for commitment than non-AGD respondents, and the higher the discount factor, the more negative the association between AGD and the willingness-to-pay for commitment. The dummy for AGD and its interaction with the value of the discount factor are jointly statistically significant at the 10% confidence level. The estimates are very similar when we exclude households in which that question was asked hypothetically (see columns (3)-(6) of Table Q.2)), when we base our estimate of AGD on the experimental game that allows for equalitarian splits (see columns (3)-(6) of Table Q.3) or when we do not drop households who exhibit a positive (or negative) WTP for *both* flexibility and commitment (see columns (3)-(6) of Table Q.4).

The negative association between Asymmetric Geometric Discounting and the willingness-to-pay for commitment does not apply to all types of commitment devices. Putting money on a savings' account in the child's name is another commitment device that can help AGD parents stick to their plans.

Table 14 summarizes our analyses of the impact of AGD on the parents' demand for a savings account. They exclude respondents that have displayed a positive (or negative) WTP for both options in a given choice set. We find a precisely estimated null effect of being AGD on the WTP for a savings' account in the child's name, even when AGD is interacted with the discount factors that parents attach to their own consumption. The effect of being AGD on the parents' WTP for a savings account in their child's name instead of a savings account in their own name, a commitment device designed to address present-bias, is also null. In table Q.8, we use the estimate of AGD based on the experimental game in which parents can choose an equalitarian split to confirm the null correlation between AGD and the WTP for a savings account in the child's name. However, this regression does find that AGD parents are 10pp. less likely to always choose to open a savings account in their name rather than in their child's name, when the later option is free. TableQ.9 does not exclude respondents that report contradictory WTP, and does find a negative association between being AGD and the probability to always chose cash over a savings account in the child's name.

The results of this subsection therefore indicate that not only do AGD parents invest less in their children's education, they do not have a systematically higher demand for commitment to stick to plans to invest in their children.

[Table 14]

6.3.4 AGD preferences vs. Time-increasing altruism

Incidentally, this follow-up experiment documents that parent-bias emerges *exclusively* when AGD parents allocate future payoffs *between themselves and their children* – rather than more generally, as an outcome of time-increasing altruism towards others. We do so by eliciting the parents' willingness to allocate the lottery proceeds to tutoring *someone else's child* and their demand for commitment when it comes to that decision.

Using a BDM elicitation mechanism in the follow-up wave, we elicit parents' willingness to allocate lottery proceeds to tutoring *someone else's child*. To avoid reciprocity motives (Fark & Fischbacher, 2006), the parents are informed that the other child would be randomly chosen among other respondents' children, that they would not be informed of the child's identity and that the child's family would not be informed of the sender's identity.

Table 13 presents the results of that experiment. About 62% of the respondents choose to provide another child with tutoring. Column (1) finds a null effect of AGD on the respondents' willingness to provide another child with tutoring. Column (2) stacks the parents' tutoring decision for their own child and another child. Non-AGD parents are 23.6pp. less likely to provide tutoring for another person's child than for their own and AGD parents by 18.2pp. The coefficients on the dummy for AGD and on its interaction term with a dummy for someone else's child are jointly insignificant, which confirms that AGD does not predict investments in children other than one's own.

Columns (3) and (4) show that the probability that the respondents who chose tutoring decided not to commit to that decision, even when the commitment decision was free, is not different whether the target child is one's own or someone else's. This holds true for both AGD respondents and others. However, the regression in column (6) finds that AGD and non-AGD respondents are willing to pay 326 kwachas less to commit to the decision to provide tutoring to a child other than their own. Columns (5) and (6) demonstrate that AGD are not differently willing to pay to commit to their decision for another child.

[Table 13]

A series of robustness checks in the appendix confirms those results. In table Q.5, the sample excludes parents for whom the initial tutoring question was asked hypothetically. On average, parents are 23.6pp. less likely to chose the tutoring for a kid that is not their own, but AGD parents are equally likely to chose the tutoring for the other kid than non-AGD parents. Similarly, all parents have a lower WTP to commit to the tutoring decision if the child is not their own, but AGD parents not disproportionately s. In columns (1)-(2) of table Q.6, we also do not find an association between the willingness to purchase the tutoring for another child (and to commit to that decision)and AGD, when AGD is defined based on the experimental game where the parents are allowed to split the peanuts equally. In table Q.7, we do find that AGD parents have a slightly lower WTP to commit to the tutoring decision for another child than non-AGD parents, but those estimates include parents who exhibit a positive (or a negative) WTP to pay for both the flexible and the committed option.

Those results therefore suggest that asymmetric geometric discounting of parents' and children's future consumption is a specific phenomenon, different from general time-increasing altruism.

7 Concluding remarks

This paper is the first one to document the prevalence of asymmetric geometric discounting, its effects on parent-biased reallocations and to assess the effectiveness of different interventions to mitigate it.

We find that parent-bias is highly prevalent in our sample: in each decision round, about 30% of respondents in our sample exhibit time preferences which are consistent with asymmetric geometric discounting.

AGD induces large parent-biased preference reversals away from children's consumption. Preference-reversals driven by AGD reduce children's future share of consumption by 18%, within 2 days. We predict that asymmetric geometric discounters would reallocate 48% of their child's consumption towards themselves over a 30-day horizon, with log preferences and perfect altruism. We estimate that, in our sample, present-bias leads to a 50% reallocation away from future time periods over any time horizon. Reallocations due to AGD are therefore almost as large as those induced by present-bias in a 30-day horizon.

Interestingly, parent and present-bias are not correlated. Even though both biases leads to reallocation of equivalent magnitudes, policies targetting exclusively present-biased respondents would therefore fail to reach a substantial share of parent-biased respondents.

Roughly the same share (slightly over 1/3) of subjects are sophisticated about their inter-temporal or within-household biases. Having said that, sophistication is not homogeneous across domains: less than 10% of subjects are sophisticated about both inter-temporal and within-household behavior, while over 35% are sophisticated in just one domain.

While the literature has not found a systematic correlation between the uptake of commitment devices targetting present-bias and quasi-hyperbolic discounting, we find that AGD is predictive of the demand for commitment to a greater extent than present-bias is. AGD respondents are willing to forego some of their own future consumption to commit to plans of shared consumption with their children. In particular, AGD subjects seem to demand to let their children participate in allocation decisions.

What's more, parent-bias seems hard to mitigate in the absence of commitment: we found that labelling, i.e. reminding parents of their past allocation choices, did not decrease the reallocation effect of AGD. The effect of labelling is not different from that of anchoring the parents' decision with a random allocation. We also did not find that child participation decreased reallocations for AGD respondents.

Finally, we test whether our experimental measure of AGD predicts investments in children outside of the experiment. For respondents who discount their future consumption very highly, parent-bias is positively correlated with investments in children. At this level of discounting, valuing one's child's future consumption relatively more than one's own is associated with more investments in children. However, the more patient parents are relative to their own consumption, the stronger the negative correlation between parent-bias and investments in children becomes. Those are the parents who are more ambitious to make future investments in the future but have difficulties following through.

Designing interventions aiming at boosting investments in children therefore requires understanding the shape of parental time-preferences to be able to target parent-biased reversals in an effective way.

While the preferences that generate it may be prevalent everywhere, its consequences are expected to be particularly dramatic in developing countries, where investments in children's human capital are not institutionalized. When parents have to actively decided to follow through on past plans to invest in their children more often, their time preferences are expected to be more consequential.

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Appendices

A Balance and selective attrition tests

Table A.1: Distribution of parents' time preferences

Panel A: joint distribution of parents' reallocation behaviors in Scenario B and A			
		Scenario A	
		Did not reallocate away from own $t = 3$ consumption	Reallocated away from own $t = 3$ consumption
Scenario B	Did not reallocate away from child's $t = 3$ consumption	421	264
	Reallocated away from child's $t = 3$ consumption	66	48
Fisher exact test, p-stat : 0.470			
Sample limited to the control group.			

Panel B: Joint distribution of θ and β		
	$\beta \geq 1$	$\beta < 1$
$\theta \geq 1$	41.54%	27.33 %
$\theta < 1$	18.49 %	12.65%
Fisher exact test, p-stat : 0.683		
Full sample.		

Table A.2: Summary statistics and balance across treatment arms.
Framing of choice.

Variable	(1) Control	(2) labelling	(3) Anchoring	(4) Participation (imposed)	(5) p-stat
Mother	0.924	0.901	0.894	0.933	0.1116
	(0.265)	(0.299)	(0.308)	(0.249)	
Muslim	0.191	0.168	0.136	0.163	0.1081
	(0.393)	(0.374)	(0.343)	(0.369)	
Number of children	2.193	2.163	2.281	2.212	0.3771
	(0.992)	(0.979)	(1.076)	(1.030)	
Credit constraint, round 1	3.626	3.367	3.037	3.220	0.2712
	(5.711)	(5.318)	(4.211)	(5.084)	
Daughter	0.531	0.521	0.514	0.505	0.8382
	(0.499)	(0.500)	(0.500)	(0.501)	
Child age	7.034	7.202	7.030	7.108	0.7817
	(2.891)	(2.966)	(2.803)	(2.978)	
Spending on preventative healthcare, USD	0.195	0.168	0.289	0.364	0.4320
	(1.088)	(0.977)	(1.899)	(3.546)	
Index of investments in health	-0.022	0.003	0.013	0.005	0.4382
	(0.378)	(0.404)	(0.370)	(0.432)	
Index of investments in education	-0.041	0.035	-0.033	-0.008	0.4295
	(0.661)	(0.990)	(0.562)	(0.683)	
s_2^1 child's share	0.486	0.482	0.484	0.477	0.6893
$k = 1$ decision, $j = 2$ consumption	(0.112)	(0.121)	(0.118)	(0.117)	
Observations	817	405	405	406	
Test for joint orthogonality, multi-logit: p-stat:					0.2882

Table A.3: Test of differential attrition

	(1) Commitment	(2) Framing	(3) Baseline variables
Probabilistic, 1	0.000710		
	(0.00952)		
Probabilistic, 1.5	-0.00143		
	(0.00934)		
Child participation, Free	0.00627		
	(0.0191)		
Child participation, 0.5	-0.00519		
	(0.0193)		
Child participation, 1	-0.0382*		
	(0.0193)		
Child Participation, 1.5	-0.0299		
	(0.0182)		
labelling		0.0158	
		(0.0100)	
Anchoring		0.00342	
		(0.0100)	
Child participation		0.00589	
		(0.0100)	
Mother			0.0255*
			(0.0120)
Islam			0.0127
			(0.00948)
Number of children			0.00696
			(0.00355)
Credit constraint, round 1			0.00110
			(0.000680)
Female child			0.00146
			(0.00702)
Child age			0.000202
			(0.00123)
s_2^1			-0.0428
			(0.0305)
Constant	0.972***	0.967***	0.944***
	(0.00678)	(0.00578)	(0.0223)
N	2411	2032	2411
Mean	0.969	0.972	0.969

This table displays the results of three OLS regressions. The outcome variable is a dummy variable equal to 1 if the respondent was observed in all three waves of data collection and 0 otherwise. Column (1) shows how attrition varies according to the type and price of commitment offered to the respondent. The ommitted category is having been assigned to the probabilistic commitment device for a price of 0.5 packets of peanuts. Column (2) shows how attrition varies according to the framing of the second visit. The ommitted categoy is the control treatment arm. Column (3) shows the correlation between attrition and baseline characteristics. Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.4: Test of differential attrition, per treatment arm

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Probabilistic commitment device				Child participation		
	0.5	1	1.5	Free	0.5	1	1.5
Mother	0.00533	0.0686**	0.00649	-0.0258	0.0335	-0.0491	0.0518
	(0.0223)	(0.0232)	(0.0233)	(0.0489)	(0.0553)	(0.0668)	(0.0650)
Islam	0.0215	-0.00370	0.00400	0.0306	0.0308	0.0826	0.0282
	(0.0176)	(0.0163)	(0.0171)	(0.0435)	(0.0556)	(0.0766)	(0.0660)
Number of children	0.00583	0.0163**	-0.000286	0.0168	0.0185	-0.0384	0.00176
	(0.00648)	(0.00628)	(0.00631)	(0.0176)	(0.0234)	(0.0288)	(0.0264)
Credit constraint, round 1	0.000633	0.000908	0.00265*	0.00146	0.00216	-0.00236	-0.00148
	(0.00114)	(0.00120)	(0.00132)	(0.00325)	(0.00416)	(0.00891)	(0.00423)
Female child	0.000282	-0.0167	0.0165	-0.0246	0.0531	-0.0610	0.0288
	(0.0131)	(0.0125)	(0.0126)	(0.0335)	(0.0395)	(0.0544)	(0.0477)
Child age	0.00105	-0.000591	0.00200	-0.00389	-0.00240	-0.00192	-0.0159
	(0.00229)	(0.00214)	(0.00222)	(0.00581)	(0.00695)	(0.00965)	(0.00846)
s_2^1	-0.112	-0.0670	0.0368	0.293*	0.114	-0.356	-0.332
	(0.0577)	(0.0530)	(0.0555)	(0.131)	(0.163)	(0.256)	(0.197)
Observations	648	665	719	93	91	91	104

This table displays the results of three OLS regressions. The outcome variable is a dummy variable equal to 1 if the respondent was observed in all three waves of data collection and 0 otherwise. The outcome variable is regressed baseline variables, separately for each probabilistic commitment device and price. Standard errors in parentheses.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.5: Test of differential attrition, per framing treatment arm

	(1) Baseline	(2) labelling	(3) Anchoring	(4) Child's participation
Mother	0.0504*	0.00481	0.0461	-0.0286
	(0.0236)	(0.0222)	(0.0277)	(0.0325)
Islam	-0.00414	0.0213	-0.0141	0.0361
	(0.0159)	(0.0176)	(0.0252)	(0.0219)
Number of children	0.00982	-0.000331	0.00626	0.0126
	(0.00641)	(0.00680)	(0.00802)	(0.00791)
Credit constraint, round 1	0.00141	0.00118	0.00101	0.000969
	(0.00109)	(0.00123)	(0.00202)	(0.00160)
Female child	0.00265	-0.0132	0.0117	-0.0000987
	(0.0125)	(0.0131)	(0.0171)	(0.0162)
Child age	0.000102	0.00107	-0.00275	0.00575*
	(0.00221)	(0.00223)	(0.00309)	(0.00274)
s_2^1	-0.137*	0.0386	0.0174	-0.00889
	(0.0559)	(0.0541)	(0.0723)	(0.0728)
Observations	817	405	405	405

This table displays the results of three OLS regressions. The outcome variable is a dummy variable equal to 1 if the respondent was observed in all three waves of data collection and 0 otherwise. The outcome variable is regressed baseline variables, separately for each framing treatment arm. Standard errors in parentheses.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

B Experiment Design

Table B.1: Randomization

<table>
<tr><td></td><td colspan="8">Panel A: Type and price of Commitment
Number of respondents</td></tr>
<tr><td></td><td colspan="4">Probabilistic</td><td colspan="4">Child's participation (chosen)</td></tr>
<tr><td>Price of commitment</td><td colspan="2">0.5</td><td>1</td><td>1.5</td><td>Free</td><td>0.5</td><td>1</td><td>1.5</td></tr>
<tr><td>Total</td><td colspan="2">*649*</td><td>*665*</td><td>*719*</td><td>*93*</td><td>*91*</td><td>*91*</td><td>*104*</td></tr>
<tr><td></td><td colspan="4">Panel B: Framing of Round 2 decisions
Number of respondents</td><td colspan="4" rowspan="3"></td></tr>
<tr><td></td><td>Control</td><td>labelling</td><td>Random Anchoring</td><td>Child participation (imposed)</td></tr>
<tr><td>Total</td><td>*817*</td><td>*405*</td><td>*405*</td><td>*406*</td></tr>
</table>

Figure B.1: Timeline of the Experiment

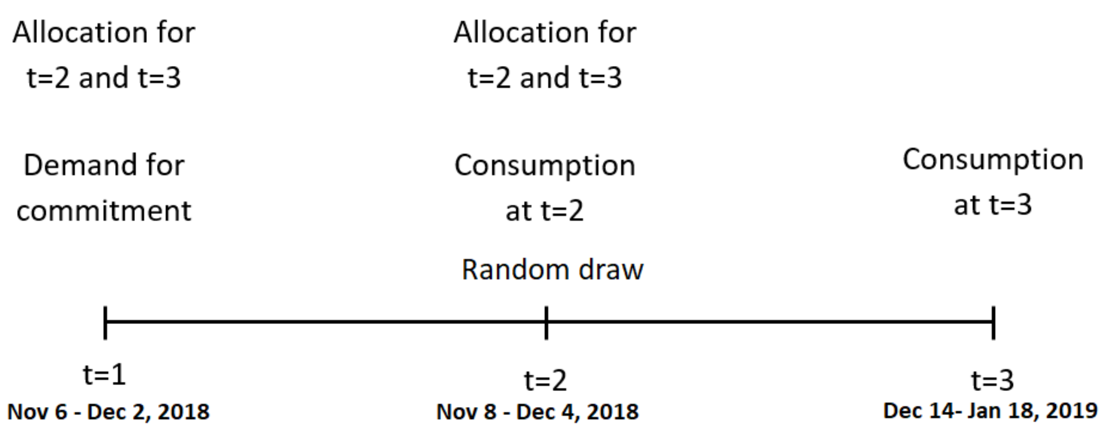
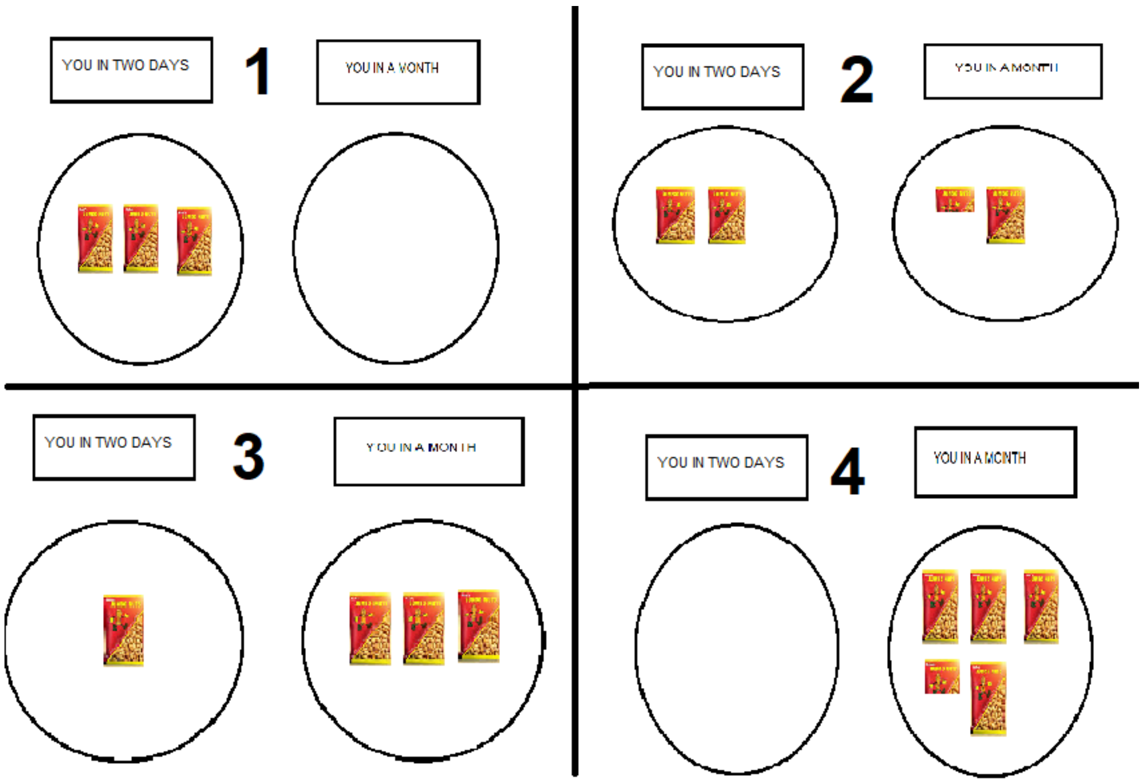
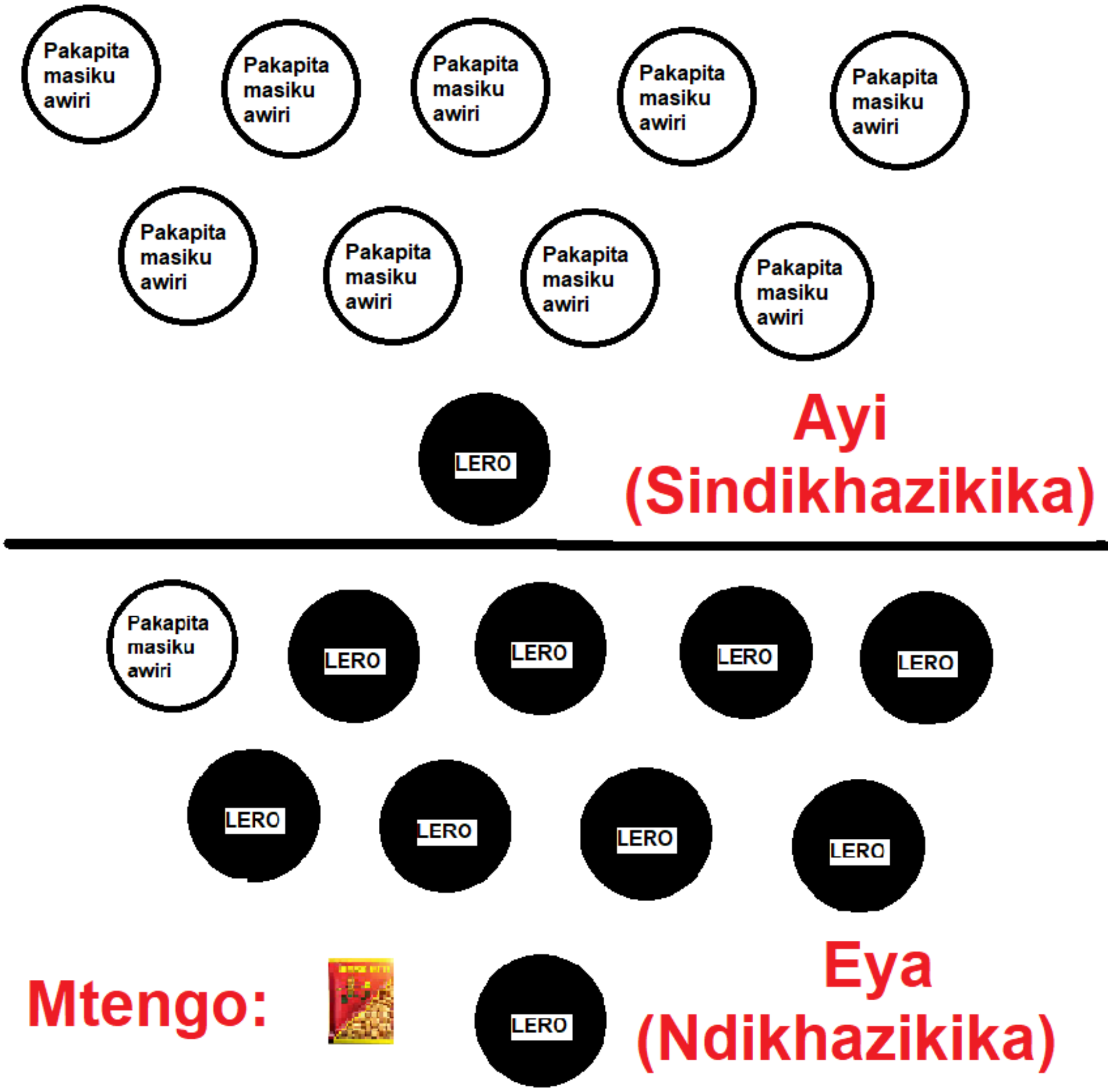


Figure B.2: Scenario A visual, round 1



Notes: This represents the visual aid that as presented to the respondents to help with the comprehension of the inter-temporal task of Scenario A, when making the choice for $r = 0.5$ in round 1. Similar visuals were presented to illustrate the choice under other interest rates and in round 2 as well.

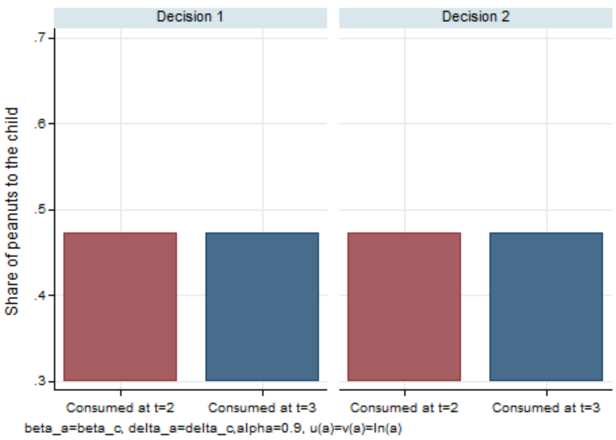
Figure B.3: Probabilistic Commitment Device visual



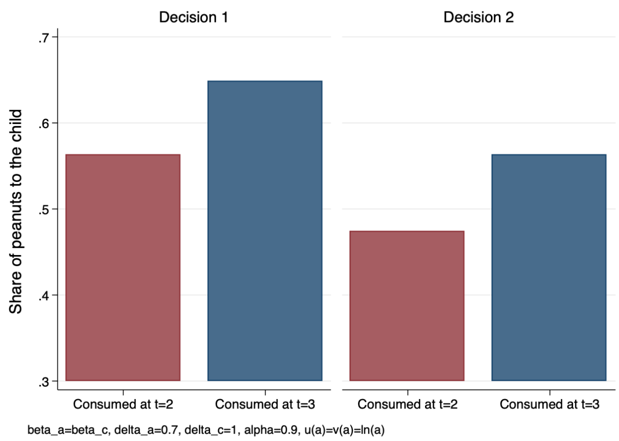
Notes: This represents the visual aid that as presented to the respondents to help with the comprehension of the probabilistic commitment device in round 1.

C Figures

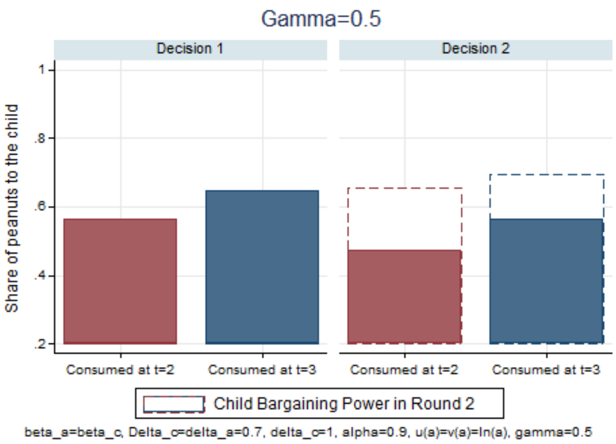
Figure 1: Evolution of the share allocated to the child per decision and consumption rounds.



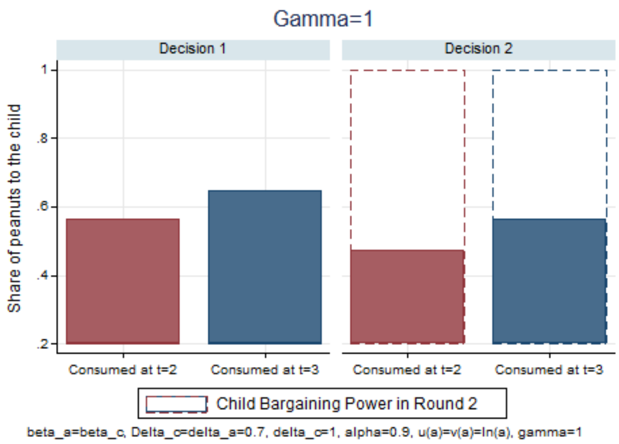
(a) Symmetric Geometric Discounters



(b) Asymmetric Geometric Discounters



(c) γ=0.5



(d) γ=1

Notes: Those four figures plot how the share of consumption allocated to the child varies across decision and consumption periods. In each figure, the left panel refers to the decision made at $t = 1$ and the right panel to the decision made at $t = 2$. The maroon bar represents the share of consumption to be consumed at $t = 2$ and the blue bar at $t = 3$. All the figures assume that the parent and the child have logarithmic utility function over consumption, that the parent's coefficient of imperfect altruism, α, is equal to 0.9, that $\delta\theta = 1$.

In figure 1a, the parent applies the same discount factor to her consumption and that of her children and the child has no bargaining power: i.e. $\theta = 1$ and $\gamma = 0$.

In figure 1b, $\theta < 1$ and the child has no bargaining power: $\gamma = 0$.

In figure 1c, the parameters are the same, but the child's bargaining power is increased: $\gamma = 0.5$. The child derives no utility from her parent's consumption and discount her future consumption by $\Delta_c = 0.7$. In figure 1d, the child's bargaining power is increased to $\gamma = 0.8$

Figure 2: Distribution of round 1 decisions: number of peanuts allocated to the child

Notes: This illustrates the distribution of round 1 allocation decisions in Scenario B. On the x-axis is the number of peanuts respondent allocated to be consumed by their children in round 2 (blue bar) or round 3 (maroon bar). The height of each bar represents the percentage of parents having chosen this allocation for a given consumption round.

Figure 3: Share of peanuts allocated to the child
Follow-up data collection, $t = 1$ decision

Notes: This represents the $t = 1$ distribution of the child's share of consumption when parents are asked to decide how to allocate five packets of peanuts between themselves and their child in the last wave of data collection. On the left-hand panel, parents are only allowed to allocate full packets of peanuts and on the right-hand panel, parents are allowed to allocate half-packets of peanuts. 22.93% of mothers in this wave allocated increasing shares when we did not allow for equal split and 17.63% did when we allowed for equal split of resources.

Figure 4: Distribution of round 2 decisions: number of peanuts allocated to the child

Notes: This illustrates the distribution of round 2 allocation decisions in Scenario B. On the x-axis is the number of peanuts respondent allocated to be consumed by their children in round 2 (blue bar) or round 3 (maroon bar). The height of each bar represents the percentage of parents having chosen this allocation for a given consumption round.

Figure 5: 30% of parents allocate increasing shares of consumption to their children at round 2

Notes: This shows the share of consumption going to the child in each consumption period when the parents make a decision at $t = 2$ among parents who allocate increasing shares of consumption to their children in round 2. For those parents, the $t = 3$ allocation is 45.91% larger than the $t = 2$ allocation on average. Sample restricted to parents in the control group.

Figure 6: Reallocations

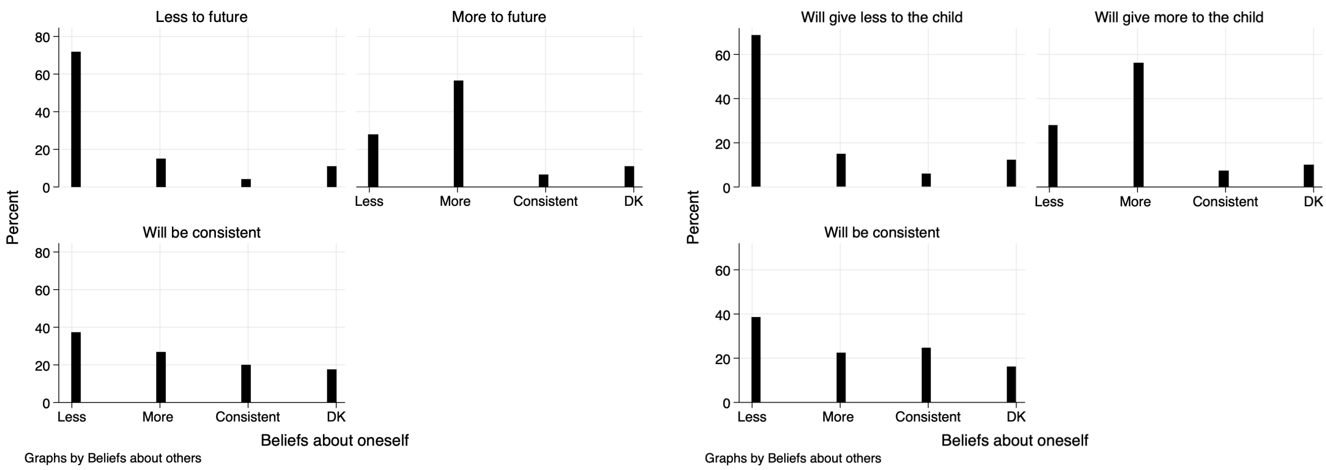
(a) Average $t = 3$ share of consumption among parents who allocate less to the future than planned
Present-bias (Scenario A)

(b) Average $t = 3$ children's share of consumption among parents who allocate less to their children than planned
Parent-bias (Scenario B)

Notes: Figure 6a shows the evolution of the share of consumption allocated to be consumed by the adult at $t = 3$ among respondents who reallocated towards their $t = 2$ consumption in Scenario A. Those represent 24.5% of the respondents. Figure 6b shows the evolution of the child's share of consumption at $t = 3$ among respondents who allocated less to their child than planned. Those represent 14.2% of the respondents. In both figure, the sample is restricted to respondents in the control group.

Figure 7: Correlation beliefs about others and oneself

(a) Inter-temporal decision (Scenario A) (b) Within-household decision (Scenario B)



Notes: Those two figures plot how (incentivized) beliefs about others' future behavior correlate with (un-incentivized) beliefs about one's own behavior, in Scenario A (Figure 7a) and B (Figure 7b). Each figure plots the distribution of beliefs about one's own behavior in round 2 depending on what the respondent guessed how the majority of other households would act in round 2. We exclude those who responded that they didn't know how the majority of others would behave in round 2.

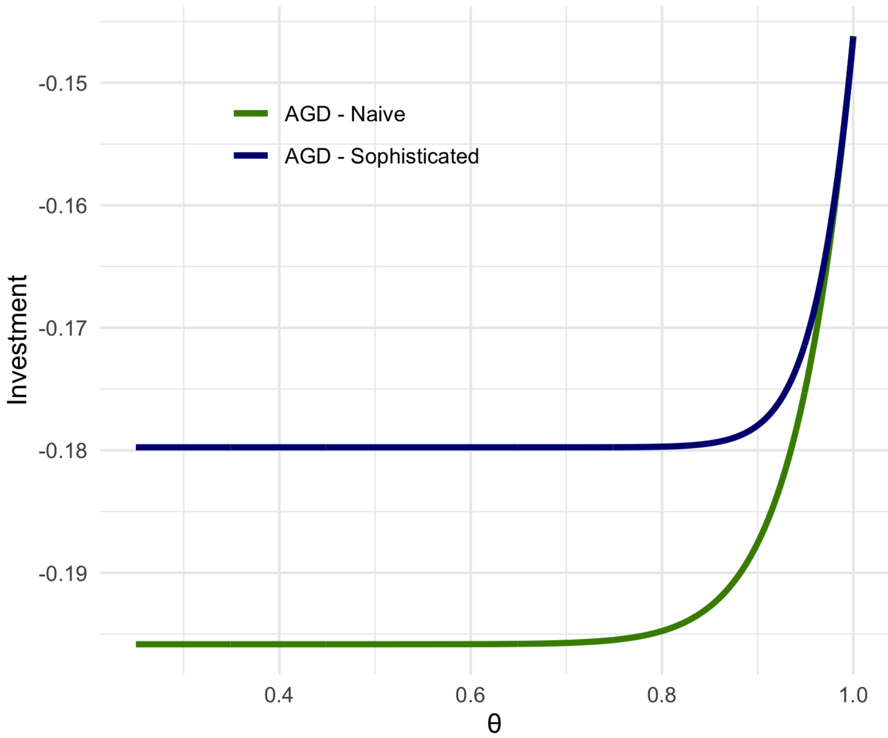
Figure 8: Demand for the probabilistic commitment device, Scenario B

Notes: This plots the share of respondents who took up the probabilistic commitment device to increase the probability that their round 1 allocation would be chosen over their round 2 allocation when making a decision in Scenario B, at different prices of commitment. The price of taking up the commitment device is expressed in packets of peanuts that are taken out of the parents' allocation in round 3. The sample is the sub-sample of parents who were offered the possibility to take up this commitment device.

Figure 9: Demand for child participation, Scenario B

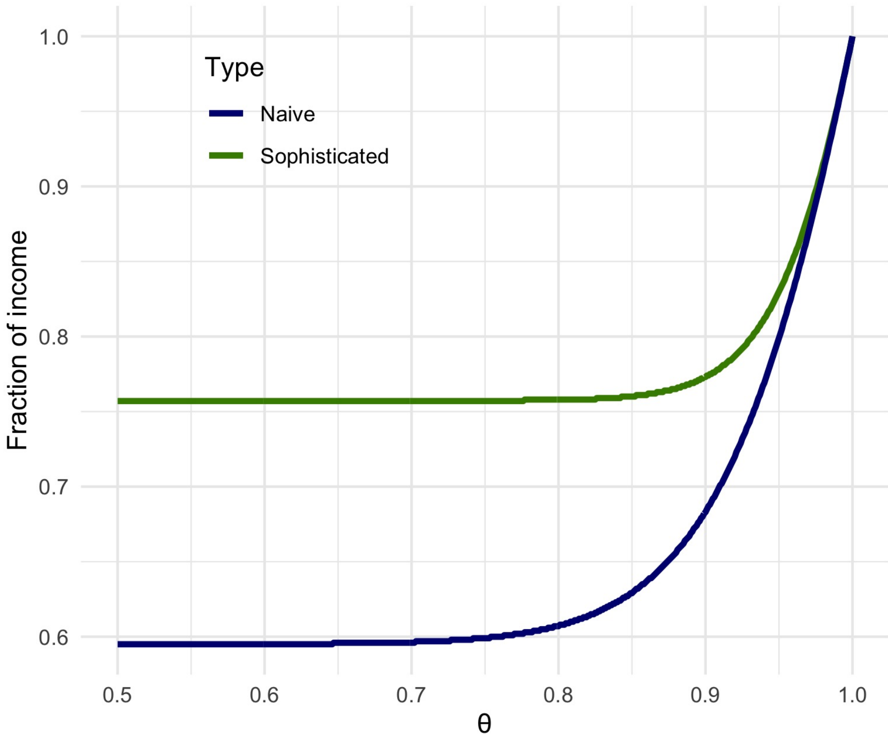
Notes: This plots the share of respondents who chose to involve their child in Scenario B's round 2 decision when offered to do so, at different prices of commitment. The price of chosing to involve the child is expressed in packets of peanuts that are taken out of the parents' allocation in round 3. The sample is the sub-sample of parents who were offered the possibility to involve their child. The maroon bar represents the share of non-AGD respondents who chose to involve their child and the blue bar the share of AGD respondents who do.

Figure 10: Optimal levels of investments in children



Notes: This figure represents the optimal level of investments in children, as a function of θ. It is derived from the model presented in appendix J with the second consumption period taking place 30 days after the first one, and uses the following parameters: $\alpha = 0.496$, $\gamma = 1.73$, $R = 1.05$, $y = 1$ and $\delta = 0.892$. The naive agent has beliefs $\hat{\theta} = \theta$ and the sophisticated agent that $\hat{\theta} = 1$.

Figure 11: Welfare losses in a model with investments



Notes: This figure plots the income that a SGD agent would require to obtain the same utility as naive and sophisticated AGD agents with an income endowment of 1. It is derived from the model presented in appendix J with the second consumption period taking place 30 days after the first one, and uses the following parameters: $\alpha = 0.496$, $\gamma = 1.73$, $R = 1.05$, $y = 1$ and $\delta = 0.892$. The naive agent has beliefs $\hat{\theta} = \theta$ and the sophisticated agent that $\hat{\theta} = 1$.

D Tables

Table 1: Distribution of within-household time preferences

Variable	**Mean**	**Adjusted**
	Panel A	
$\mathbb{1}\{\theta < 1\}$	0.311	
$\mathbb{1}\{\theta = 1\}$	0.543	No
$\mathbb{1}\{\theta > 1\}$	0.146	
	Panel B	
$\mathbb{1}\{\theta < 1\}$	0.763	
$\mathbb{1}\{\theta = 1\}$	0.093	Yes
$\mathbb{1}\{\theta > 1\}$	0.144	
N	2,411	

Notes: Those definitions are based on $t = 1$ decisions. Consistent respondents allocate the same share to be consumed by their child at $t = 2$ and $t = 3$. Parent-biased respondents allocate a larger share of consumption to their children in the later ($t = 3$) time period. Child-biased respondents allocate a larger share of consumption to their children in the earlier ($t = 2$) time period. *Panel A:*: The $t = 3$ share of child's consumption is computed as a share of a budget without adjusting for the price of commitment, irrespective of take-up. *Panel B*: The $t = 3$ share of child's consumption is computed as a share of a budget net of the price of commitment, if commitment has been taken up.

Table 2: Do children's shares of consumption increase in the time horizon?

	(1)	(2)
	All decisions	$t = 2$ decisions
	s_j^k	s_j^k
$j - k$	0.00321***	0.00442***
	(0.000181)	(0.000254)
$\mathbb{1}\{\theta < 1\}$	-0.0556***	-0.0194*
	(0.00684)	(0.00875)
$\mathbb{1}\{\theta < 1\} \times (j - k)$	0.00734***	0.00366***
	(0.000339)	(0.000526)
Control variables	Yes	Yes
Respondents	813	795
Mean	0.573	0.584

Notes: This table reports the estimated impact of the time horizon between the consumption decision at $t = k$ and the actual consumption at $t = j$ on the share of consumption that goes to the child in Scenario B. The unit of observation is one consumption decision. The outcome variable is the child's share of consumption at $t = j$ in a decision made at $t = k$. The sample is restricted to the control sub-sample to avoid any framing effect of our other treatment arms on the second round decisions. Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the order in which scenarios A and B were presented to the respondent and a dummy variable indicating whether the household is Muslim or not. Column (1) pools all decisions, column (2) restricts the analysis to the decisions made at $t = 2$. Standard errors in parentheses. * $p < 0.1$, ** $p < .05$, *** $p < .01$

Table 3: Present bias and preference reversals

	(1)
	s_{3r}^{k}
$\mathbb{1}\{k=2\}$	0.00296
	(0.00580)
$\mathbb{1}\{k=2\} \times \mathbb{1}\{\beta<1\}$	-0.396***
	(0.00942)
$\mathbb{1}\{\beta<1\}$	0.158***
	(0.0102)
Control variables	Yes
Mean s_{3ri}^{k} for $\mathbb{1}\{\beta<1\}$ subjects	0.792
N	14,142
Respondents	2,357

$\Rightarrow$ Scenario A Reallocation due to $\mathbb{1}\{\beta<1\}$: 50.00%

$\Rightarrow$ Implied $\beta = 0.5$

$\Rightarrow$ Implied Scenario A 30-day reallocation: 50.00%

Notes: This table reports the estimated impact of present-bias on reallocation decisions at $t = 2$ in Scenario A . The unit of observation is one consumption decision. The outcome variable is the share of consumption that respondents choose to receive at $t = 3$ in a decision made at $t = k$, at interest rate r.
Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the order in which scenarios A and B were presented to the respondent and a dummy variable indicating whether the household is Muslim or not. Standard errors in parentheses. * $p < 0.1$, ** $p < .05$, *** $p < .01$

Table 4: Asymmetric geometric discounting and preference reversals

	(1) s_3^k	(2) s_3^k
$\mathbb{1}\{k=2\}$	0.0530***	0.0171***
	(0.00588)	(0.00634)
$\mathbb{1}\{\theta<1\} \times \mathbb{1}\{k=2\}$	-0.118***	
	(0.0116)	
$\mathbb{1}\{\theta<1\}$	0.186***	
	(0.00812)	
$\mathbb{1}\{k=2\} \times \mathbb{1}\{\beta<1\}$		0.00350
		(0.0116)
$\mathbb{1}\{\beta<1\}$		-0.00584
		(0.0103)
$j-k$		
Control variables	Yes	Yes
Mean	0.525	0.524
Mean s_3^1 for $\mathbb{1}\{\theta<1\}$	0.645	
N	1608	1590
Sample	Control	Control
Column 1: $\Rightarrow$ Reallocation within 2 days: 18.3%		

Notes: This table reports the estimated impact of AGD on reallocation decisions at $t=2$ in Scenario B. The unit of observation is one consumption decision. The outcome variable is the child's share of consumption at $t=3$ in a decision made at $t=k$. The sample is restricted to the control sub-sample to avoid any framing effect of our other treatment arms on the second round decisions. Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the order in which scenarios A and B were presented to the respondent and a dummy variable indicating whether the household is Muslim or not. Standard errors in parentheses. * $p<0.1$, ** $p<.05$, *** $p<.01$

Table 5: Do symmetric geometric discounters value peanuts less?

	(1)	(2)	(3)
	Δs_3	$\Delta^2 s_3$	$\Delta^2 s_3$
$\mathbb{1}\{\theta < 1\}$	-0.137***	0.00519	0.00613
	(0.0118)	(0.00434)	(0.00455)
s_2^1	-0.259***		0.0121
	(0.0504)		(0.0227)
Control variables	Yes	Yes	Yes
Mean	0.0186	0.0235	0.0235
N	795	795	795

Standard errors in parentheses

* $p < 0.1$, ** $p < .05$, *** $p < .01$

Notes: The unit of observation is one consumption decision. In column (1), the outcome variable is the change in the child's share of consumption at $t = 3$ that occured between the $t = 1$ and $t = 2$ decisions. In columns (2) and (3), the squared value of that variable. In columns (1) and (3), we control for the share of consumption that the parents had chosen to allocate to the child at $t = 2$ in the initial consumption decision as a measure of how much parents value the consumption good. The sample is restricted to the control sub-sample to avoid any framing effect of our other treatment arms on the second round decisions. Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the order in which scenarios A and B were presented to the respondent and a dummy variable indicating whether the household is Muslim or not. Standard errors in parentheses. * $p < 0.1$, ** $p < .05$, *** $p < .01$

Table 6: Are biased respondents more sophisticated with respect to present or parent-bias?

(1)	(2)	(3)
Share of sophisticated respondents		
Panel A: Beliefs about others		
Reallocation towards the present	Reallocation towards the parent	p-value (1)-(2)
0.378	0.362	0.5024
(0.485)	(0.481)	
Panel B: Beliefs about self		
Reallocation towards the present	Reallocation towards the parent	p-value (1)-(2)
0.216	0.157	0.0030 ***
(0.412)	(0.364)	
N	945	686

Notes: This table reports the share of respondents who accurately predicted the direction of their $t = 2$ reallocation, or absence thereof, at $t = 1$. The sample is restricted to respondents in the Control group. Column (1) reports the fraction of sophisticates among respondents who reallocated away from the future in Scenario A. Column (2) reports the fraction of sophisticates among respondents who reallocated away from their child in Scenario B. Column (3) reports the p-value of a two-sided t-test of equality of means. In panel A, sophistication is measured with respect to the respondent's prediction about the behavior of "most other" participants. The elicitation of those beliefs was incentivized. In panel B, sophistication is measured with respect to the respondent's prediction about the behavior of her future self. The elicitation of those beliefs was unincentivized.

Table 7: Joint distribution of sophistication

		Panel A: Beliefs about others	
		Inter-temporal	
		Naive	Sophisticated
Within	Naive	55.08%	18.91%
Household	Sophisticated	17.34%	8.67%

		Panel B: Beliefs about self	
		Inter-temporal	
		Naive	Sophisticated
Within	Naive	49.96%	23.39%
Household	Sophisticated	17.68%	8.97%

Notes: This table reports the share of respondents who accurately predicted the direction of their $t = 2$ reallocation, or absence thereof, at $t = 1$, for both the within-household decision of Scenario B and the Inter-temporal decision of Scenario A. Panel A reports the fraction of respondents whose behavior at $t = 2$ was in line with the behavior they predicted most other respondents would adopt. The elicitation of those beliefs was incentivized. Panel B reports the fraction of respondents whose behavior at $t = 2$ was in line with the behavior they predicted they would adopt. The elicitation of those beliefs was unincentivized.

Table 8: The effect of framing

	(1) Δs_j		(2) Δs_j		(3) Δs_j
labelling	0.0128	*Anchoring*	0.0159	*Child*	0.107***
	(0.0132)		(0.0144)		(0.0266)
$\mathbb{1}\{\theta < 1\}$	-0.142***		-0.143***		-0.155***
	(0.0140)		(0.0140)		(0.0121)
labelling	-0.0201	*Anchoring*	-0.0329	*Child*	-0.128***
$\times\mathbb{1}\{\theta < 1\}$	(0.0237)	$\times\mathbb{1}\{\theta < 1\}$	(0.0305)	$\times\mathbb{1}\{\theta < 1\}$	(0.0282)
Control variables	Yes		Yes		Yes
Mean Δs_3^k in Control group for $\mathbb{1}\{\theta < 1\}$	-0.075		-0.075		-0.075
N	1194		1192		1188
Sample	*labelling* $\times$ *Control*		*Anchoring* $\times$ *labelling*		*Child* $\times$ *Control*

Notes: Column (1) reports the estimated impact of labelling on reallocation decisions at $t = 2$ in Scenario B. Column (2) reports the estimated impact of anchoring on reallocation decisions at $t = 2$ in Scenario B. Column (3) reports the estimated impact child participation plays to mitigate the effect of parent-bias. The outcome variable is the difference in between the share of peanuts allocated to be consumed by the child $t = 3$ when making the decision at $t = 2$ and $t = 1$. At the beginning of the second round, respondents in the labelling sub-sample are reminded of the allocation choices they have made in the first round. Respondents in the Anchoring sub-sample are shown random allocations. Respondents in the child participation (imposed) sub-sample make their second round decision with their child. In column (1), the sample is restricted to the control and labelling sub-samples to avoid any framing effect of our other treatment arms on the second round decisions. In column (2), the sample is restricted to the anchoring and labelling sub-samples to allow us to directly compare the effect of both treatments. In column (3), the sample is restricted to the control and the child participation (imposed) sub-samples. Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the order in which scenarios A and B were presented to the respondent and a dummy variable indicating whether the household is Muslim or not. Standard errors in parentheses. * $p < 0.1$, ** $p < .05$, *** $p < .01$

Table 9: Probabilistic commitment device

	(1)	(2)	(3)	(4)
(5)	(6)			
Took up the probabilistic commitment device				
		Scenario B		
Scenario A				
$\mathbb{1}\{\theta < 1\}$	0.0678*			0.0102
	(0.0378)	(0.0142)		
Price	-0.0210			
-0.00444				
	(0.0194)			
(0.0198)				
$\mathbb{1}\{\theta < 1\} \times$ *Price*	-0.0567			
	(0.0345)			
$\mathbb{1}\{\beta < 1\}$				
-0.00804	-0.0103			
(0.0350)	(0.0130)			
$\mathbb{1}\{\beta < 1\} \times$ *Price*				
-0.00177				
(0.0318)				
Belief Parent-Bias			-0.0160	-0.0951
			(0.0866)	(0.0621)
Price × Belief Parent-Bias			0.0189	0.0989*
			(0.0794)	(0.0558)
Belief Child-Bias			0.115	
			(0.0887)	
Price × Belief Child-Bias			-0.115	
			(0.0812)	
Price FE	No	Yes	No	No
No	Yes			
Control variables	Yes	Yes	Yes	Yes
Yes	Yes			
Mean for $\mathbb{1}\{\theta > 1\}$	90.0 %	90.0%		
Mean for $\mathbb{1}\{\beta = 1\}$				
91.8%	91.8%			
N	2027	2027	659	659
1988	1988			

Notes: This table reports the estimated impact of AGD (col. (1-2)), beliefs over reversals (col. (3-4)) and present-bias (col. (5-6)) on the demand for the probabilistic commitment device in Scenario B and A respectively. The outcome variable is a dummy equal to 1 if the respondent takes up the probabilistic commitment device, zero otherwise. Respondents choose whether to take up the commitment device after making allocation decisions during the first visit. Taking up the commitment device reduces the probability that round 2 decision will be implemented. The commitment device comes at a random price (0.5, 1 or 1.5 packet of peanuts deducted from the parents' allocation at $t = 3$.). Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the order in which scenarios A and B were presented to the respondent and a dummy variable indicating whether the household is Muslim or not. The sample is restricted to parents having been offered the probabilistic commitment device. Belief Parent-Bias: respondents predict that the majority of respondents would allocate more peanuts to themselves in the second round, in the incentivized task. Belief Child-Bias: respondents predict that the majority of respondents would allocate more peanuts to their children in the second round, in the incentivized task. In columns (3-4), the sample excludes parents who responded "Don't know". Standard errors in parentheses. * $p < 0.1$, ** $p < .05$, *** $p < .01$

Table 10: Demand for child participation

	(1)	(2)	(3)
	Took up child participation		
Price	-0.0127	0.0830	
	(0.0540)	(0.0853)	
$\mathbb{1}\{\theta < 1\}$	0.00359	0.198	0.109*
	(0.0955)	(0.174)	(0.0564)
$\mathbb{1}\{\theta < 1\}\times$ Price	0.134	-0.0204	
	(0.0983)	(0.156)	
Control variables	Yes	Yes	Yes
Price Fixed effects	No	No	Yes
Sample	All prices	Prices >0	All prices
Mean for $\mathbb{1}\{\theta \geq 1\}$	0.55	0.54	0.55
N	377	284	377

Notes: This table reports the estimated impact of AGD on the demand for child participation in Scenario B. The outcome variable is dummy equal to 1 if the respondent chose to involve her child in round 2 decision, zero otherwise. Respondents choose whether to involve their child after making allocation decisions in Scenario B during the first visit. Child participation comes at a random price (0, 0.5, 1 or 1.5 packet of peanuts deducted from the parents' allocation at $t = 3$). The sample is restricted to parents having been offered the possibility to involve their child in the second round decision. Column (2) restricts the analysis to respondents who were offered to let their child participate for non-zero prices. Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the order in which scenarios A and B were presented to the respondent and a dummy variable indicating whether the household is Muslim or not. Standard errors in parentheses. * $p < 0.1$, ** $p < .05$, *** $p < .01$

Table 11: AGD and investments in children

	(1) Index of investments, 3-5 years old	(2) Index of investments, 6-12 years old
$\mathbb{1}\{\theta < 1\}$	0.0643	0.103**
	(0.0623)	(0.0424)
$\delta\theta \times \mathbb{1}\{\theta < 1\}$	-0.0226	-0.0550**
	(0.0332)	(0.0232)
$\delta\theta$	0.0139	0.0190
	(0.0180)	(0.0126)
Control variables	Yes	Yes
N	848	1481

Notes: This table reports the estimated impact of AGD on investments in children. The outcome variable is a summary index variable of investments in children, whose components are described in appendix L. Among non-parent-biased parents, each component of the index has mean zero and standard deviation 1. Columns (1) focuses on investments in children five years old and younger and column (2) on investments in children older than six years old. $\delta\theta$ is an experimental measure of the discount factor parents attach to their own consumption. Its imputation is described in Appendix **??**. Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the order in which scenarios A and B were presented to the respondent a dummy variable indicating whether the household is Muslim or not and a measure of α. Standard errors in parentheses. * $p < 0.1$, ** $p < .05$, *** $p < .01$

Table 12: AGD and tutoring for one's child

	(1)	(2)	(3)	(4)	(5)	(6)
	Chooses tutoring		Commitment			
			Never commits		WTP	
$\mathbb{1}\{\theta < 1\}$	-0.0525***	-0.0223	0.0206	0.0124	-60.51***	-47.19
	(0.0191)	(0.0408)	(0.0329)	(0.0710)	(21.50)	(46.65)
$\delta\theta \times \mathbb{1}\{\theta < 1\}$		-0.0152		0.00375		-6.340
		(0.0177)		(0.0304)		(20.07)
$\delta\theta$		0.0126		-0.000904		12.34
		(0.00825)		(0.0138)		(8.956)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
$p-value$ joint significance						
Mean	0.842	0.842	0.449	0.449	257.7	257.3
N	2014	2012	1391	1390	767	766

Notes: This looks at the impact of AGD on parents' willingness to purchase tutoring for their child and to commit to that decision. In columns (1)-(2), the outcome variable is a dummy equal to 1 if the parent decided to receive tutoring for their children instead of 2,000 kwachas in cash if they earned it in the lottery. In columns (3)-(4), the outcome variable is a dummy equal to 1 if the respondent never chooses to commit to her decision even at price 0. In columns (5)-(6), the outcome variable is the WTP to commit. In columns (3)-(6), the sample is restricted to respondents having taken up the tutoring for their child and who do not exhibit a positive (or negative) willingness-to-pay for both the flexible option and the commitment. In columns (5)-(6), the sample is further restricted to respondents for which there is at least one non-negative price at which they choose to commit. $\delta\theta$ is an experimental measure of the discount factor parents attach to their own consumption. Its imputation is described in Appendix **??**. In this table, we use the measures of $\delta\theta$ and $\mathbb{1}\{\theta < 1\}$ elicited in the same wave of data collection as the outcome variables. $p-value$ test of joint significance of the coefficients on $\delta\theta \times \mathbb{1}\{\theta < 1\}$ and $\mathbb{1}\{\theta < 1\}$. Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the religion of the household, the level of education of the respondent, a dummy equal to one if the question was asked hypothetically. Standard errors in parentheses. * $p < 0.1$, ** $p < .05$, *** $p < .01$

Table 13: AGD and tutoring for another child

	(1)	(2)	(3)	(4)	(5)	(6)
	Chooses tutoring		Commitment			
			Never commits		WTP	
$\mathbb{1}\{\theta < 1\}$	0.000828	-0.0529***	0.0145	0.00450	-35.05	-50.65
	(0.0255)	(0.0205)	(0.0377)	(0.0445)	(24.47)	(31.22)
$\mathbb{1}\{Other\}$		-0.236***		-0.0111		-325.6***
		(0.0131)		(0.0166)		(13.65)
$\mathbb{1}\{Other\} \times \mathbb{1}\{\theta < 1\}$		0.0541*		0.0111		48.47
		(0.0290)		(0.0393)		(31.72)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
$p - value$ test of joint significance		0.9618		0.7221		0.6298
Mean	0.618	0.730	0.429	0.444	263.1	158.0
N	2015	4029	989	1576	565	738

Notes: This looks at the impact of AGD on parents' willingness to purchase tutoring for a child other than their own and to commit to that decision. In columns (1)-(2), the outcome variable is a dummy equal to 1 if the parent decided to provide tutoring instead of 2,000 kwachas in cash if they earned it in the lottery.

In columns (3)-(4), the outcome variable is a dummy equal to 1 if the respondent never chooses to commit to her decision even at price 0. In columns (5)-(6), the outcome variable is the WTP to commit. In columns (3)-(6), the sample is restricted to respondents having taken up the tutoring and who do not exhibit a positive (or negative) willingness-to-pay for both the flexible option and the commitment. In columns (5)-(6), the sample is further restricted to respondents for which there is at least one non-negative price at which they choose to commit. In columns (1), (3) and (5) the question is restricted to the parents' decision for a child other than their own. In columns (2), (4) and (6), the parents' decisions for their child and the other child are stacked and standard errors are clustered at the household level. $\mathbb{1}\{Other\}$ is a dummy equal to one if the target child is not the respondent's child. $p - value$ test of joint significance of the coefficients on $\mathbb{1}\{Other\} \times \mathbb{1}\{\theta < 1\}$ and $\mathbb{1}\{\theta < 1\}$. Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the religion of the household, the level of education of the respondent, a dummy equal to one if the question was asked hypothetically. Standard errors in parentheses. * $p < 0.1$, ** $p < .05$, *** $p < .01$

Table 14: AGD and willingness-to-pay for a savings account

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Cash vs. Child's savings account				Own vs. Child's savings account			
	Always chooses cash		WTP for child's savings account		Always chooses own savings account		WTP for child's savings account	
$\mathbb{1}\{\theta < 1\}$	-0.0234	-0.0398	-75.69	-102.8	-0.0286	-0.0467	-180.8	253.3
	(0.0151)	(0.0323)	(54.98)	(118.5)	(0.0185)	(0.0397)	(191.0)	(413.5)
$\delta\theta \times \mathbb{1}\{\theta < 1\}$		0.00798		13.50		0.00885		-221.2
		(0.0140)		(51.32)		(0.0171)		(188.1)
$\delta\theta$		-0.00630		29.49		0.00522		92.67
		(0.00640)		(23.67)		(0.00791)		(94.51)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$p - value$ test of joint significance		0.129		0.248		0.144		0.924
Mean	0.0805	0.0806	3664.4	3664.2	0.879	0.879	689.7	689.7
N	1850	1849	1701	1700	1756	1754	213	213

Notes: This looks at the impact of AGD on parents' willingness to pay to open a savings account for their child. In columns (1)-(2), the outcome variable is a dummy equal to 1 if the parent always chooses to receive cash instead of opening a bank account in their child's name, even when this option is free. In columns (3)-(4), the outcome variable is the willingness-to-pay for a savings account in the child's name among parents for which there exists a non-negative price at which they would take up the savings account. In columns (5)-(6), the outcome variable is a dummy equal to 1 if the parent always chooses to open a savings account in their name instead of their child's name, even when this option is free. In columns (7)-(8), the outcome variable is the willingness-to-pay for a savings account in the child's name instead of the parent's name among parents for which there exists a non-negative price at which they would take up the savings account in their child's name. All the regressions exclude respondents who declared a strictly negative (or positive) willingness-to-pay for each of options offered. $\delta\theta$ is an experimental measure of the discount factor parents attach to their own consumption. Its imputation is described in Appendix **??**. In this table, we use the measures of $\delta\theta$ and $\mathbb{1}\{\theta < 1\}$ elicited in the same wave of data collection as the outcome variables. $p - value$ test of joint significance of the coefficients on $\delta\theta \times \mathbb{1}\{\theta < 1\}$ and $\mathbb{1}\{\theta < 1\}$. Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the religion of the household, the level of education of the respondent. Standard errors in parentheses. * $p < 0.1$, ** $p < .05$, *** $p < .01$

E Modeling child's bargaining power

This appendix extends the model to include the child's bargaining power. Let Δ_c and Δ_a be the discounting factors that the child uses towards her own and her parent's future consumption respectively, and A be the child's coefficient of imperfect altruism.

Formally, let the child's objective function be given by:

$$\underset{(z_t)_{t=2,3},(x_t)_{t=2,3}}{\text{Max}} \beta\Delta_a Au(x_2^1) + \beta\Delta_a^2 Au(x_3^1) + \beta\Delta_c v(z_2^1) + \beta\Delta_c^2 v(z_3^1) \tag{E.1}$$

$$\text{s.t.} \begin{cases} x_2 + z_2 \leq y_2 \\ x_3 + z_3 \leq y_3 \\ y_3 = y_2 = y \end{cases}$$

For simplicity, we assume that the child only cares about her own consumption, i.e. that $A = 0$. We assume that the child only takes part in the $t = 2$ decision. At $t = 2$, we set the child's bargaining power to γ, and that of the parent to $1-\gamma$. Following Chiappori (1988), the within-household bargain at $t=2$ can be represented by:

$$\underset{(z_t)_{t=2,3},(x_t)_{t=2,3}}{\text{Max}} (1-\gamma)(u(x_2^1)+\beta\delta_a u(x_3^1)+\alpha v(z_2^1)+\alpha\beta\delta_c v(z_3^1))+\gamma(v(z_2^1)+\beta\Delta_c v(z_3^1)) \tag{E.2}$$

$$\text{s.t.} \begin{cases} x_2+z_2 \leq y_2 \\ x_3+z_3 \leq y_3 \\ y_3=y_2=y \end{cases}$$

The child's participation in the household decision changes the $t=2$ FOCs to:

$$\frac{u'(x_2^2)}{v'(z_2^2)}=\frac{\alpha(1-\gamma)+\gamma}{1-\gamma}$$

and

$$\frac{u'(x_3^2)}{v'(z_3^2)}=\frac{(1-\gamma)\alpha\delta_c+\gamma\Delta_c}{(1-\gamma)\delta_a}$$

Therefore, increasing the child's bargaining power in the $t=2$ decision increases the child's instantaneous share of consumption. Its effect on the child's share of consumption at $t=3$ is ambiguous and depends on the child's own discount factor.

F Effects of child participation

We now turn to a type of intervention specifically designed to address parent-bias: involving children in the decision-making process. This subsection explores whether child participation attenuates the reallocation effects of parent-bias and whether parents are willing to pay to let their children participate in the second decision. [47]

Column (3) in table 8 presents the results of a regression estimating the average impact of child participation on child's consumption, pooling all children in our Control and Imposed treatment arms. In the control group, AGD decreases the child's consumption between decision rounds by 15.5pp. We find that imposing child participation increases the child's $t=3$ share of consumption by 10pp. on average, but obtain an almost zero

[47] The model can be extended to incorporate interventions specifically targeted at parent-bias. We focus on child's participation in the household future allocation decision. In appendix E, we increase the child's bargaining power in the $t=2$ decision. This changes the F.O.C.s and increases the child's immediate consumption. Further, an increase in the child's bargaining power will partially overturn the preference reversals induced by parent-bias. Its effect on the child's share of consumption at $t=3$ is ambiguous and depends on the child's own discount factor. Figures 1c and 1d illustrate what happens to the child's share of consumption when the bargaining power of the child of parent-biased respondents increases from zero to 0.5 and 1 respectively.

effect of imposing child participation on the share of consumption of children of AGD respondents.

But do AGD respondents demand to involve their children in round 2 decision? Figure 9 plots the demand for child participation for non-AGD and AGD parents respectively.

[Figure 9]

At non-zero prices, the demand for child participation for AGD respondents is always higher than that of non-AGD respondents. If including the child in round 2 decision costs 0.5 packets of peanuts, 64% of parent-biased respondents chose to involve their child, while only 51% of non-AGD respondents do. 71% of parent-biased respondents vs. 52% of non-parent-biased respondents chose to involve their child in the decision, when doing so requires foregoing 1.5 packets of peanuts. This figure would suggest that child participation does not obey the law of demand and that the take-up is higher at higher prices. We take solace in the fact that differences in demand at different positive prices is not statistically significant.

Table 10 reports the estimated impact of parent-bias on the demand for child participation. The results from our baseline specification are presented in column (1): AGD is not associated with a higher uptake of child participation when the price of doing so is zero, but gets more strongly associated with participation when the price of letting the child participate increases. In column (2), we restrict our sample to parents who were offered to let their child participate at positive prices, and we find that AGD is associated with a 0.198 statistically insignificant increase in the take up of child participation. Including price fixed effects in column (3) enables us to conclude that AGD increases the take up of child participation by an average of 10.9pp., which is marginally statistically significant and large in magnitude as it represents almost 20% of the take-up among non-AGD parents.

[Table 10]

In appendix M.5, we find that AGD parents are more likely to chose to involve boys, non-first-born and younger children, but that the association between AGD and child participation disappears for other children. Those patterns are not surprising in light of the cultural norms prevalent in Malawi.

G Accounting for asymmetric quasi-hyperbolic discounting

In this appendix, we allow for asymmetric quasi-hyperbolic discount factors accross the parent's and the child's consumption. That would, for instance, allow the parents to be present-biased with respect to her own consumption, but not that of her child. Let parents discount all their children's future consumptions with a factor $\beta_c \leq 1$ and all their own future consumptions with a factor β_a, s.t. $\beta_a \leq \beta_c$ Let the parents discount their own consumption one period ahead and that of their child by the same factor δ.

At $t = 1$ parents optimize:

$$\underset{(z_t)_{t=2,3},(x_t)_{t=2,3}}{\text{Max}} \beta_a \delta u(x_2^1) + \beta_a \delta^2 u(x_3^1) + \alpha \beta_c \delta v(z_2^1) + \alpha \beta_c \delta^2 v(z_3^1)$$

$$\text{s.t.}$$

$$\begin{cases} x_2 + z_2 \leq y_2 \\ x_3 + z_3 \leq y_3 \\ y_3 = y_2 = y \end{cases}$$

At $t = 2$ they optimize:

$$\underset{(z_t)_{t=2,3},(x_t)_{t=2,3}}{\text{Max}} u(x_2^2) + \beta_a \delta u(x_3^2) + \alpha v(z_2^2) + \alpha \beta_c \delta v(z_3^2)$$

The F.O.Cs at $t = 1$ are are: $\frac{u'(x_2^1)}{v'(z_2^1)} = \frac{\alpha\beta_c}{\beta_a}$ and $\frac{u'(x_3^1)}{v'(z_3^1)} = \frac{\alpha\beta_c}{\beta_a}$. Therefore asymmetric quasi-hyperbolic discounting does not predict that the child's consumption shares increase with the time horizon.

The F.O.Cs are $t = 2$ are: $\frac{u'(x_2^2)}{v'(z_2^2)} = \alpha$ and $\frac{u'(x_3^2)}{v'(z_3^2)} = \frac{\alpha\beta_c}{\beta_a}$. Therefore, asymmetric quasi-hyperbolic discounting can explain that parents would deviate from planned consumption plans *when the moment comes to execute them* but *not* that parents would deviate from future consumption plans when they get closer in time.

H Analyzing the results of Scenario A

This appendix studies the prevalence of present-bias in our sample. The design of Scenario A provides us with three important informations: 1) whether subjects react rationally to the interest rate and therefore have understood our design, 2) whether subjects are present-biased on average and 3) whether there were unexpected shocks to the parents' marginal utility of consumption.

We formally test those hypotheses in the following regression:

$$s_{3ri}^k = \alpha + \gamma_1 r + \gamma_2 \mathbb{1}\{k = 2\} + \gamma_3 r \times \mathbb{1}\{k = 2\} + \lambda X_{ki} + \epsilon_{ki}$$

where s_{3ri}^k is the share of peanuts allocated in Scenario A to the $t = 3$ period, net of the price of commitment, by subject i when the choice is made at $t = k$, for interest r.

Testing hypothesis 1) is equivalent to testing $\gamma_1 > 0$ in the above regression. Testing hypothesis 2) is equivalent to testing $\gamma_2 < 0$ and hypothesis 3) requires us to test whether parents react differently to the interest rate in different time periods, i.e. whether $\gamma_3 = 0$.

Table H.1 presents the results from this regression.

First, subjects respond rationally to the law of demand, the share of peanuts allocated to the later time period increases when it is more rewarding to do so. Increasing the interest rate from 0.5 to 1.5 is associated with a statistically significant 22.8 pp. increase in the share of peanuts allocated to the later time-period.

Table H.1: Scenario A decision

	(1)
	$s_3^k r$
r	0.228***
	(0.00851)
$\mathbb{1}\{k = 2\}$	-0.146***
	(0.0128)
$\mathbb{1}\{k = 2\} \times r$	-0.00942
	(0.00996)
Control variables	Yes
Mean	0.621
Mean at $k = 1$	0.698
N	14142
Respondents	2357

Notes: This table reports the estimated impact of the interest rate and the time of decision on the share of peanuts that parents decide to receive in the later time period when making a decision in the inter-temporal allocation task in Scenario A. The unit of observation is one consumption decision. The outcome variable is the share of consumption that respondents choose to receive at $t = 3$ in a decision made at $t = k$, at interest rate r. Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the order in which scenarios A and B were presented to the respondent and a dummy variable indicating whether the household is Muslim or not. Standard errors in parentheses. * $p < 0.1$, ** $p < .05$, *** $p < .01$

Figure H.1: Decisions in Scenario A

Notes: This plots the share of peanuts that respondents choose to receive in round 2, i.e. the earlier time period, in Scenario A, for different interest rates. The left panel shows the choice respondents made in round 1 and the right panel the choices they made in round 2. The red lines plot the predicted relationship between the share of peanuts to be received in round 1 and the interest rate, based on an OLS regression.

Second, respondents are present-biased on average. When allocating peanuts between a present and a future time period, at $t = 2$, subjects allocate 14.6 pp less to the future time-period on average, than when allocating consumption between two future time periods, at $t = 1$. The reallocation away from future consumption is statistically significant and large in magnitude: it represents 20.9% of the share of peanuts allocated to $t = 3$ when the decision was made in the first visit.

Finally, we can reject the null hypothesis that the subjects react differently to the interest rate when making the decision at $t = 1$ and $t = 2$: the coefficient on the interaction term $r \times \mathbb{1}\{k = 2\}$ is statistically insignificant and small in magnitude. We can therefore rule out that the reallocations we observe between both decision periods are driven by unexpected shocks to the respondents' marginal utility of consumption.

Figure H.1 visualizes those three findings: subjects respect the law of demand, they reallocate more towards the $t = 2$ consumption when making the decision in round 2 than round 1 and they react similarly to the interest rate in both decision rounds.

I Substitution

Time-preference experiments relying on real consumption choice runs the risk that outside consumption may adjust in prevision to experimental consumption. Do parents who are more generous with their children in our experiment compensate by reducing the amount of child's consumption outside of our experimental setting? We cannot rule out that parents provided smaller meals to their children after the experiment, but we do not think that potential substitution alone explains why the share of consumption allocated to the child increases with the time horizon or why parent-bias time-preferences are associated with larger preference reversals, in line with the predictions of our model.

We are able to produce limited evidence that parents do not adjust the child's outside consumption to compensate the allocation decision made in our experiment. At the beginning of visits 1 and 2, we asked parents when they and their child had last eaten and whether the child was hungry. Note that at the end of round 1 parents are informed about the time that the research team will visit the village for the second visit. Parents can therefore adjust the child's outside consumption to the experiment by skipping meals or reducing the amount of food provided to the child in anticipation of the second round.

If parents adjust the child's consumption in anticipation of their generosity during our second round's visit, then the children of respondents who have allocated a larger share of consumption to be consumed by their children in round 2 will have eaten longer before the start of round 2 or be hungrier. We run two seemingly unrelated regressions regressing our measure of the child's outside consumption on their experimental share of consumption separately for round 1 and round 2 allocation decisions.

Table I.1 presents the results from those seemingly unrelated regressions with two sets of outcome variables: the number of hours since the child last ate (col. (1-2)) and whether the child is hungry (col. (3-4)). Both outcome variables are measured at the beginning of round 2. Columns (1-2) show no evidence that parents adjusted the time of their child's last meal to react to the generosity they exhibited in the first visit (col.(1)) or in the rest of the experiment (col. (2)). The absence of evidence of systematic adjustment of the child's consumption prior to the second visit is made starker when it comes to the child's hunger at the beginning of the experiment in columns (3-4). Note that 48% of parents report that their children are hungry at the beginning of the experiment. Yet, when, in the first round, parents allocated 1 pp. more towards their child's share of consumption in round two, the child is only 0.003 pp. more likely to arrive in round 2 experiment hungry (col.(3)). This is a negligible and statistically insignificant effect. The magnitude of the effect is even smaller when focusing on the decision made in round 2 (col (4)).

Table I.1: Substitution effect at the beginning of round 2

	(1)	(2)	(3)	(4)
	Number of hours since the child last ate		Child is hungry	
s_{ji}^k	0.00810	0.00408	0.00000522	0.00000129
	(0.0337)	(0.0396)	(0.000887)	(0.00104)
$s_{ji}^k \times \mathbb{1}\{j=2\}$	0.00242	0.00143	0.0000301	0.0000206
	(0.0681)	(0.0714)	(0.00179)	(0.00187)
$\mathbb{1}\{j=2\}$	0.0155	0.0152	0.00119	0.00119
	(0.150)	(0.151)	(0.0124)	(0.0124)
Control variables	Yes	Yes	Yes	Yes
k	1	2	1	2
Mean	5.33	5.33	0.48	0.48
N	1,590	1,590	1,590	1,590
Respondents	795	795	795	795

Notes: This table reports the results from two sets of seemingly unrelated regressions which assess whether parents adjust the child's outside consumption in anticipation of round 2. In colums (1-2) the outcome variable is the number of hours since the child last ate by the start of round 2. In colums (3-4) the outcome variable is a dummy variable equal to 1 if the respondent reports that the child is hungry at the beginning of round 2, 0 otherwise. In columns (1) and (3), the independent variables are the shares of peanuts that the parents allocated to the child in their first round decision. In columns (2) and (4), the independent variables are the shares of peanuts that the parents allocated to the child in their second round decision. Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the order in which scenarios A and B were presented to the respondent a dummy variable indicating whether the household is Muslim or not. Standard errors in parentheses. * $p < 0.1$, ** $p < .05$, *** $p < .01$

J Modeling investments in children

We follow the literature (Andersen et al., 2018) in using a Constant Relative Risk Aversion (CRRA) utility function to estimate the key parameters in our model. We assume that the parent attaches the same utility function to their own consumption and their child's.

$$v(c) = u(c) = \begin{cases} \frac{1}{1-\gamma}c^{1-\gamma}, & \text{if } \gamma \neq 1 \\ ln(c), & \text{otherwise} \end{cases} \tag{J.1}$$

Note that $\alpha \in [0,1]$ and $\theta \in [0,1]$

The $t = 1$ parental utility maximization problem is:

$$\underset{(z_t)_{t=1,2},(I_t)_{t=1},(x_t)_{t=1,2}}{\text{Max}} \quad u(x_1^1) + \theta\delta u(x_2^1) + \alpha v(z_1^1) + \alpha\delta v(z_2^1) \tag{J.2}$$

$$\text{s.t.}$$
$$\begin{cases} x_1 + z_1 + I_1 \leq y_1 \\ x_2 + z_2 \leq y_2 + RI_1 \\ y_1 = y_2 = y \end{cases}$$

Remember that the agent has beliefs over the shape of her $t = 2$ utility function : $\hat{\theta}u(x_2) + \alpha v(z_2)$, $\hat{\theta} \in [\theta, 1]$

The agent anticipates that her allocation at $t = 2$ will be:

- $z_2 = \alpha^{\frac{1}{\gamma}} \frac{(y+IR)}{\hat{\theta}^{\frac{1}{\gamma}}+\alpha^{\frac{1}{\gamma}}}$
- $x_2 = \hat{\theta}^{\frac{1}{\gamma}} \frac{y+IR}{\hat{\theta}^{\frac{1}{\gamma}}+\alpha^{\frac{1}{\gamma}}}$

Note that at similar levels of α, δ and γ, a symmetric geometric discounter with $\theta = 1$ will always invest more than an AGD agent with a lower θ.

We set: $C = (\frac{\alpha^{\frac{1}{\gamma}}}{\hat{\theta}^{\frac{1}{\gamma}}+\alpha^{\frac{1}{\gamma}}})^{1-\gamma}$ and $B = (\frac{\hat{\theta}^{\frac{1}{\gamma}}}{\hat{\theta}^{\frac{1}{\gamma}}+\alpha^{\frac{1}{\gamma}}}))^{1-\gamma}$. Plugging the anticipated values of z_2 and x_2 into the $t = 1$ utility function: $\frac{(y-I-z_1)^{1-\gamma}}{1-\gamma} + \alpha\frac{z_1^{1-\gamma}}{1-\gamma} + (\theta\delta B + \alpha\delta C)(\frac{y+IR}{1-\gamma})^{1-\gamma}$.

Setting the first order condition with respect to z_1 equal to zero yields: $z_1 = \frac{(y-I)\alpha^{\frac{1}{\gamma}}}{1+\alpha^{\frac{1}{\gamma}}}$.

The F.O.C. with respect to I is: $(y - I - z_1)^{-\gamma} = (\theta\delta BR + \alpha\delta CR)(y + IR)^{-\gamma}$

$$\Leftrightarrow y - I - z_1 = (\theta\delta BR + \alpha\delta CR)^{-\frac{1}{\gamma}}(y + IR)$$

$$\Leftrightarrow I = \frac{y\left((BR\delta\theta+CR\alpha\delta)^{\frac{1}{\gamma}}-1-\alpha^{\frac{1}{\gamma}}\right)}{(BR\delta\theta+CR\alpha\delta)^{\frac{1}{\gamma}}+R(1+\alpha^{\frac{1}{\gamma}})}$$

$$\Leftrightarrow I = \frac{y\left(\left((\frac{\hat{\theta}^{\frac{1}{\gamma}}}{\hat{\theta}^{\frac{1}{\gamma}}+\alpha^{\frac{1}{\gamma}}}))^{1-\gamma}R\delta\theta+(\frac{\alpha^{\frac{1}{\gamma}}}{\hat{\theta}^{\frac{1}{\gamma}}+\alpha^{\frac{1}{\gamma}}})^{1-\gamma}R\alpha\delta\right)^{\frac{1}{\gamma}}-1-\alpha^{\frac{1}{\gamma}}\right)}{\left((\frac{\hat{\theta}^{\frac{1}{\gamma}}}{\hat{\theta}^{\frac{1}{\gamma}}+\alpha^{\frac{1}{\gamma}}}))^{1-\gamma}R\delta\theta+(\frac{\alpha^{\frac{1}{\gamma}}}{\hat{\theta}^{\frac{1}{\gamma}}+\alpha^{\frac{1}{\gamma}}})^{1-\gamma}R\alpha\delta\right)^{\frac{1}{\gamma}}+R(1+\alpha^{\frac{1}{\gamma}})}$$

Hence the AGD agent's investment at $t = 1$ will depend on her anticipated value of θ at $t = 2$.

To model how AGD agent's investment at $t = 1$ depend on their level of sophistication, we set: $X(\hat{\theta}) = \left((\frac{\hat{\theta}^{\frac{1}{\gamma}}}{\hat{\theta}^{\frac{1}{\gamma}}+\alpha^{\frac{1}{\gamma}}}))^{1-\gamma}R\delta\theta + (\frac{\alpha^{\frac{1}{\gamma}}}{\hat{\theta}^{\frac{1}{\gamma}}+\alpha^{\frac{1}{\gamma}}})^{1-\gamma}R\alpha\delta\right)$, so $I(X(\hat{\theta})) = \frac{y\left(X(\hat{\theta})^{\frac{1}{\gamma}}-1-\alpha^{\frac{1}{\gamma}}\right)}{X(\hat{\theta})^{\frac{1}{\gamma}}+R(1+\alpha^{\frac{1}{\gamma}})}$, $\frac{\partial I}{\partial\hat{\theta}} = \frac{\partial I}{\partial X}\frac{\partial X}{\partial\hat{\theta}}$.

$$\frac{\partial X}{\partial\hat{\theta}} = \frac{R\,\alpha^{\frac{1}{\gamma}}\,(\gamma-1)\delta\,\hat{\theta}^{\frac{1}{\gamma}-1}\left(\alpha\left(\frac{\hat{\theta}^{\frac{1}{\gamma}}}{\hat{\theta}^{\frac{1}{\gamma}}+\alpha^{\frac{1}{\gamma}}}\right)^{\gamma}-\theta\left(\frac{\alpha^{\frac{1}{\gamma}}}{\hat{\theta}^{\frac{1}{\gamma}}+\alpha^{\frac{1}{\gamma}}}\right)^{\gamma}\right)}{\gamma\left(\hat{\theta}^{\frac{1}{\gamma}}+\alpha^{\frac{1}{\gamma}}\right)^{2}\left(\frac{\alpha^{\frac{1}{\gamma}}}{\hat{\theta}^{\frac{1}{\gamma}}+\alpha^{\frac{1}{\gamma}}}\right)^{\gamma}\left(\frac{\hat{\theta}^{\frac{1}{\gamma}}}{\hat{\theta}^{\frac{1}{\gamma}}+\alpha^{\frac{1}{\gamma}}}\right)^{\gamma}} = \frac{R\delta(1-\gamma)\alpha^{\frac{1+\gamma}{\gamma}}\,\hat{\theta}^{\frac{1}{\gamma-1}}\,(\theta-\hat{\theta})}{\left(\hat{\theta}^{\frac{1}{\gamma}}+\alpha^{\frac{1}{\gamma}}\right)^{\gamma}\gamma\left(\hat{\theta}^{\frac{1}{\gamma}}+\alpha^{\frac{1}{\gamma}}\right)^{2}\left(\frac{\alpha^{\frac{1}{\gamma}}}{\hat{\theta}^{\frac{1}{\gamma}}+\alpha^{\frac{1}{\gamma}}}\right)^{\gamma}\left(\frac{\hat{\theta}^{\frac{1}{\gamma}}}{\hat{\theta}^{\frac{1}{\gamma}}+\alpha^{\frac{1}{\gamma}}}\right)^{\gamma}}$$

As $\hat{\theta} \in [\theta, 1]$, $\frac{\partial X}{\partial\hat{\theta}} \leq 0$ if $\gamma < 1$, ≥ 0 otherwise.

Now, $\frac{\partial I}{\partial X} = \frac{(R+1)\,X^{\frac{1}{\gamma}-1}y\left(\alpha^{\frac{1}{\gamma}}+1\right)}{\left(R\,\alpha^{\frac{1}{\gamma}}+X^{\frac{1}{\gamma}}+R\right)^{2}\gamma}, > 0$

So: $\frac{\partial I}{\partial\hat{\theta}} = \frac{\partial I}{\partial X}\frac{\partial X}{\partial\hat{\theta}} \leq 0$ if $\gamma < 1$, ≥ 0 otherwise.

K Calibrating the model

We start our calibration exercise by assuming that $\gamma = 1.73$ which was the average value of the CRRA elicited by Holden & Quiggin (2017) among a sample of Malawian farmers, demographically very similar to the respondents in our sample.

K.1 Estimating $\theta\delta$

In scenario A, there are two days between the first and second visit, and 30 days between the first and third visit. The respondents' utility maximization problem in the first visit is therefore given by:

$$\underset{(x_t)_{t=2,30}}{\text{Max}} \ (\delta\theta)^2 u(x_2^0) + (\delta\theta)^{30} u(x_{30}^0)$$
$$\text{s.t.}$$
$$\begin{cases} x_2 + s_2 \leq y_2 \\ x_{30} \leq (1+r)s_2 \\ y_2 = y \end{cases}$$

The solution to the respondents' maximization problem is given by: $u'(x_2^0)(1+r) = (\delta\theta)^{28} u'(x_3^0 0)$.

Therefore, $\delta\theta = (\frac{u'(x_2^0)(1+r)}{u'(x_3^0)})^{\frac{1}{28}} = (\frac{(x_2^0)^{-\gamma}(1+r)}{(x_3^0)^{-\gamma}})^{\frac{1}{28}} = (\frac{(x_2^0)^{-\gamma}(1+r)}{((1+r)y - x_2^0)^{-\gamma}})^{\frac{1}{28}}$

The respondents have to decide how to allocate consumption between periods two and three for three interest rates: $r \in \{0.5, 1, 1.5\}$. For each interest rate, we imputed the value of $\delta\theta$ associated with the respondents' decision. Those three imputed values of $\delta\theta$ are pairwise significantly ($p < 0.0001$) and positively correlated. We then used their average as the value of $\delta\theta$.

The median value of $\delta\theta$ among all respondents is 0.8469 and among AGD respondents it is 0.8484.

K.2 Estimating α

We estimate α from the parents' utility maximization problem in round 2 in Scenario B:

$$\underset{(z_t)_{t=2,30},(x_t)_{t=2,30}}{\text{Max}} \ u(x_2^2) + (\delta\theta)^{28} u(x_3^2 0) + \alpha v(z_2^2) + \alpha(\delta)^{28} v(z_3^2 0)$$
$$\text{s.t.}$$
$$\begin{cases} x_2 + z_2 \leq y_2 \\ x_{30} + z_{30} \leq y_3 \\ y_{30} = y_2 = y \end{cases}$$

The value of α can be inferred by: $\alpha = \frac{u'(x_2^2)}{v'(z_2^2)} = \frac{(x_2^2)^{-\gamma}}{(z_2^2)^{-\gamma}}$. The median value of α among all respondents is 2.01, it is equal to 0.496 among AGD respondents.

Figure K.1: Distribution of $\delta\theta$

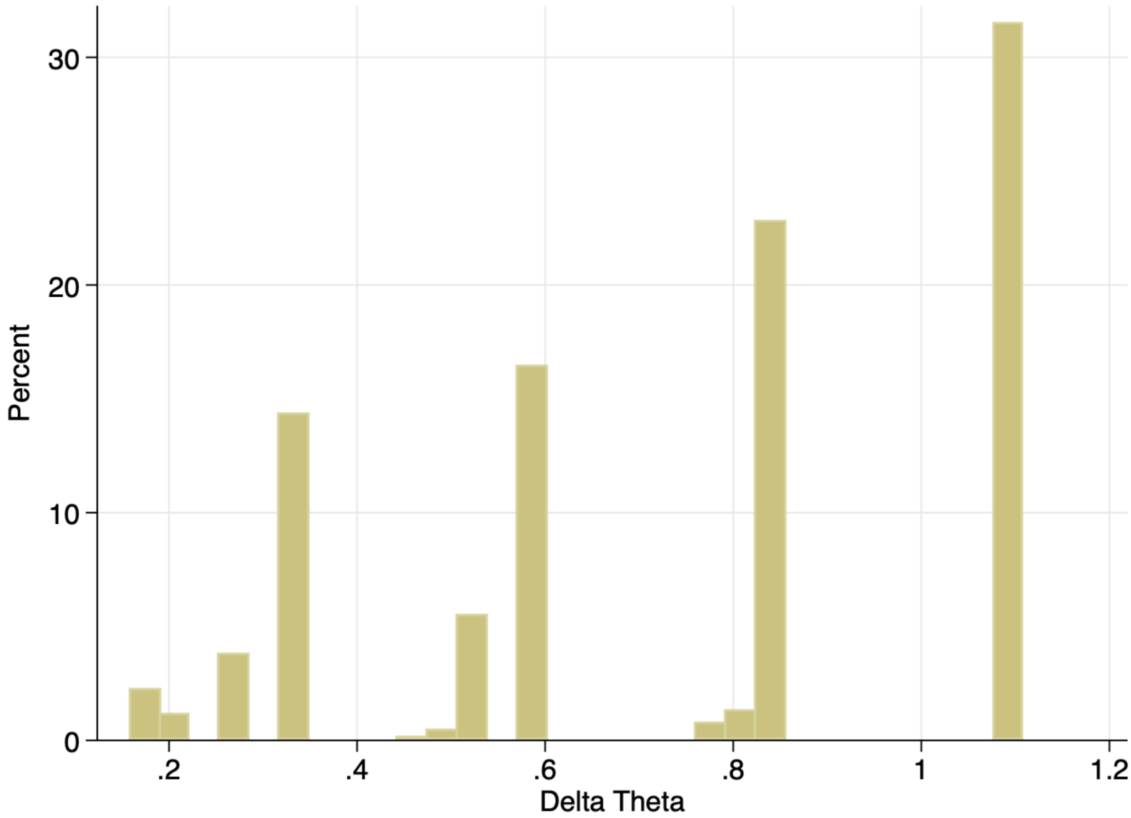


Figure K.2: Distribution of α

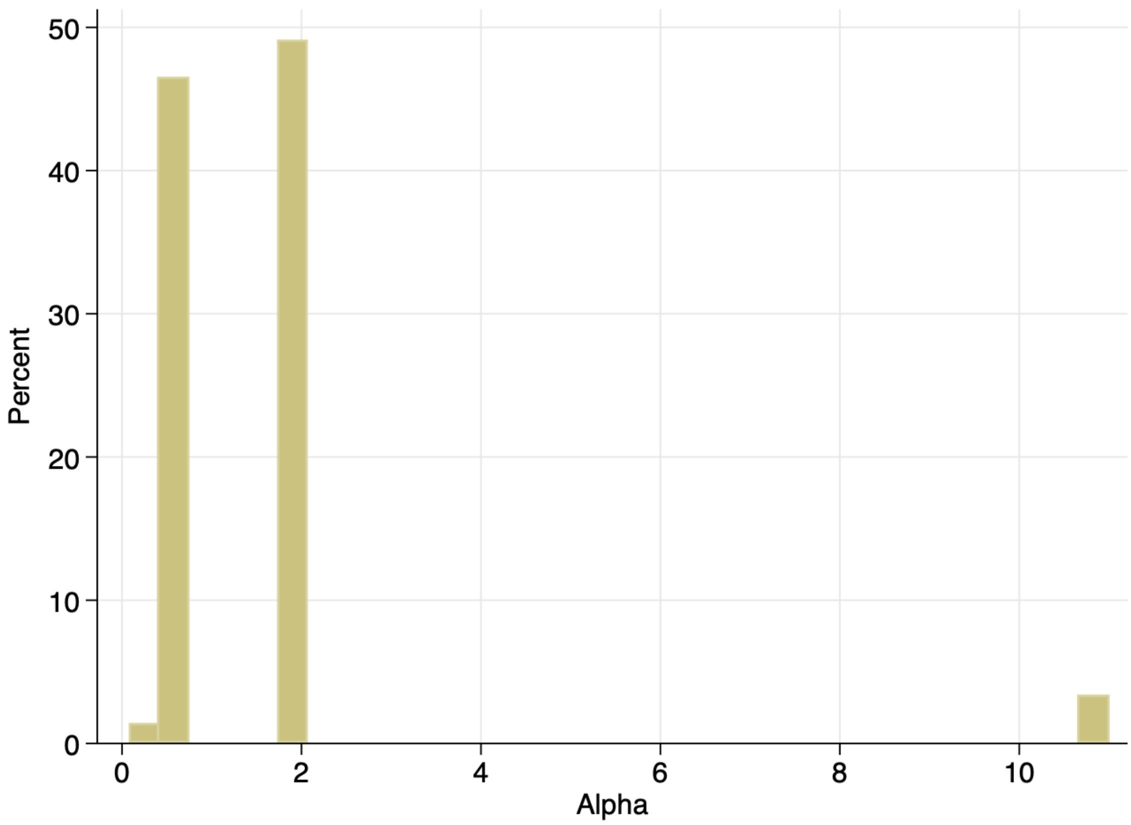
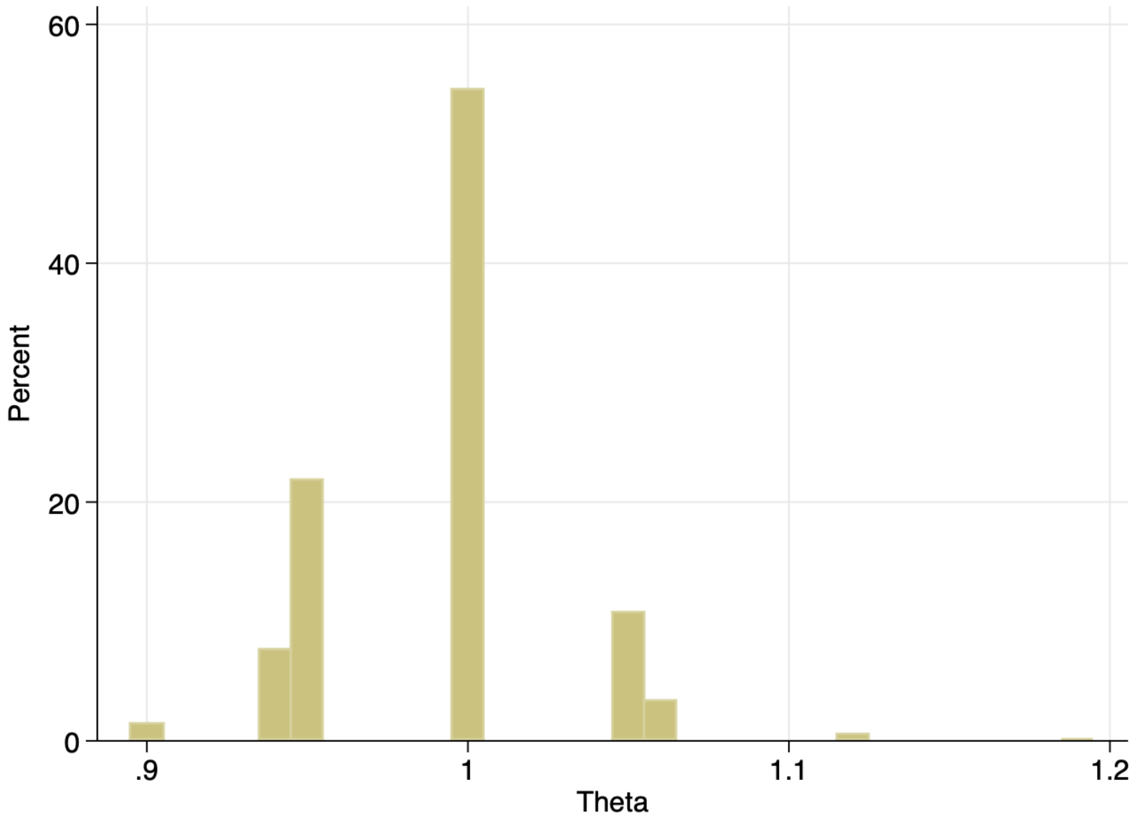


Figure K.3: Distribution of θ



K.3 Estimating θ

We estimate θ from the parents' round 1 decision in Scenario B.

$$\underset{(z_t)_{t=2,30},(x_t)_{t=2,30}}{\text{Max}} (\delta\theta)^2 u(x_2^0) + (\delta\theta)^{30} u(x_{30}^0) + \alpha(\delta)^2 v(z_2^0) + \alpha(\delta)^{30} v(z_{30}^0)$$
$$\text{s.t.}$$
$$\begin{cases} x_2 + z_2 \leq y_2 \\ x_{30} + z_{30} \leq y_{30} \\ y_{30} = y_2 = y \end{cases}$$

θ is given by: $\theta = (\frac{x_2^0}{x_{30}^0} \frac{z_{30}^0}{x_2^0})^{\left(\frac{-\gamma}{28}\right)}$ The median value of θ is equal to 1 among non-AGD respondents, and 0.95 among AGD respondents. From our estimates of $\theta\delta$ and θ, we estimate the value of δ for each respondent. The median value of δ is 0.81 for non-AGD respondents and 0.89 for AGD respondents

L Index of investments in children

For children younger than six years old, the index is composed of the following variables:

1. Mean expenses on preventative health-care in the 4 weeks before the experiment

2. Immunization against measles and rubella
3. Multiple Micronutrient powder in the 7 days before the experiment
4. Iron supplements in the 7 days before the experiment
5. Therapeutic food in the 7 days before the experiment
6. Supplementary food in the 7 days before the experiment
7. Vitamin A dose in the 3 months before the experiment
8. Drug for intestinal worms in the 6 months before the experiment
9. Growth check-up at under-5 clinic the 3 months before the experiment
10. Health check-up at under-5 clinic in the 3 months before the experiment
11. Number of days spent in an Early Childhood Development Program in the 7 days before the experiment
12. Expenses to send the child to the ECDP.

For children older than six years old, the index was made of items 1-8 listed above and the following additional measures:

13. Number of days the child attended school in the month before the experiment
14. School expenditures
15. Education support score.

M Heterogeneity

M.1 Preference reversals

Does parent-bias lead to reallocation of similar magnitude depending on the gender, age or birth order of the child? To test for heterogeneity in the effect of AGD alongside observable characteristics, we run the following regressions:

$$\Delta s_{3i} = \alpha + \gamma_0 \mathbb{1}\{Trait_i\} + \gamma_1 \mathbb{1}\{\theta < 1\} + \gamma_2 \mathbb{1}\{Trait_i\} \times \mathbb{1}\{\theta < 1\} + \lambda X_{ki} + \epsilon_{3i} \tag{M.1}$$

where $\mathbb{1}\{Trait_i\}$ is a vector of background characteristics along which we test for heterogeneity in the impact of asymmetric geometric discounting.

Table M.1 presents the results from this regression. Column (1) tests whether the magnitude of preference-reversals attributed to AGD is different on average for girls and for boys. Parent-bias reduces girls' consumption by 15.46pp. but boys' consumption by 17.3pp. The difference between both coefficients is statistically insignificant. Column (2) looks at whether being the first-born child in a family mitigates the reallocation effects of AGD. For non-first-born children AGD leads to a 15.25 pp. reallocation away from the child's $t = 3$ consumption, a statistically insignificant 1.55 pp. increase from an average reallocation effect of 16.8 for non-first-born children. Research in India (Jayachandran & Pande, 2017) suggests that favoritism toward the eldest son distorts resource allocation among children. Therefore, colum (4) interacts parent-bias with an interaction term for the gender and the birth order of the child. The parent-biased reallocation appear to be largest for non-first-born boys: AGD leads to a 15.3 pp. reallocation away from non-first boys' consumption in contrast to a 11.5 pp. reallocation away from first-born boys' consumption or a 10.4 pp. reallocation away from eldest girls' consumption.

Finally, column (3) focuses on whether the magnitude of parent-biased reallocations differ depending on whether the child is older or younger than eight years old, the median age in our experiment. While AGD leads to a 15.8 pp. reallocation away from the consumption of children younger than eight years old, it leads to a 17.02pp. reallocation away from the consumption of children who are at least eight years old, a negligible and statistically insignificant difference.

Therefore, gender and birth order seem to matter as little as age to explain variations in the effects of parent-bias across children's characteristics. None of the coefficients on heterogeneity traits are statistically significant across our four specifications. However, the patterns they suggest are interesting as they are counter-intuitive: we would expect that preferences for sons or older children would mitigate the parent-biased reallocations.

[Table M.1]

Table M.1: AGD and within-household preference reversals- Heterogeneity

	(1)	(2)	(3)	(4)
	Δs_3	Δs_3	Δs_3	Δs_3
$\mathbb{1}\{\theta < 1\}$	-0.173***	-0.168***	-0.158***	-0.153***
	(0.0195)	(0.0172)	(0.0185)	(0.0245)
$\times Trait =$ Girl	0.0184			0.00725
	(0.0280)			(0.0337)
$\times Trait =$ First Born		0.0155		0.00358
		(0.0309)		(0.0412)
$\times Trait =$ 8 years old and older			-0.0122	
			(0.0175)	
$\times Trait =$ Girl $\times$ First Born				0.0377
				(0.0612)
Control variables	Yes	Yes	Yes	Yes
Mean	0.00643	0.00643	0.00643	0.00643
Mean s_3^1 for AGD respondents if $Trait = 1$	0.635	0.642	0.641	
Mean s_3^1 for AGD respondents if $Trait = 0$	0.634	0.631	0.630	
p-value of difference in means	0.8696	0.1667	0.1804	
Respondents	795	795	795	795

Notes: This table reports how the estimated impact of parent-bias on reallocation decisions at $t = 2$ in Scenario B varies along some selected heterogeneity traits. The outcome variable is the difference in between the share of peanuts allocated to be consumed by the child $t = 3$ when making the decision at $t = 2$ and $t = 1$. The sample is restricted to the control sub-sample to avoid any framing effect of our other treatment arms on the second round decisions. Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the order in which scenarios A and B were presented to the respondent and a dummy variable indicating whether the household is Muslim or not. Each column additionnally controls for the level of the trait along which heterogeneity is measured. * $p < 0.1$, ** $p < .05$, *** $p < .01$

M.2 The effectiveness of framing interventions

To assess whether our framing interventions have a different impact depending on some observable characteristics, we use the following econometric specification:

$$\Delta s_{3i} = \alpha + \gamma_0 \times \mathbf{T} + \gamma_1 \mathbb{1}\{Trait_i\} + \gamma_2 \mathbb{1}\{Trait_i\} \times \mathbf{T} + \lambda X_{ki} + \epsilon_{3i} \tag{M.2}$$

We restrict our sample to AGD respondents to avoid triple-interaction terms which may be difficult to interpret.

Table M.2 shows no statistically significant differential effect of labelling alongside any of those heterogeneity traits.

Column (1) looks at the differential effects of labelling depending on the child's gender among AGD respondents. Those results suggest that labelling does not have an effect on boy's consumption: it increases consumption by 0.4 percentage point, an effect that is statistically insignificant and very close to zero as on average consumption decreases by 7.5pp. in the control group. For girls, labelling is associated with a further 1.7 pp. decrease in consumption, a statistically insignificant effect.

Column (2) focuses on the heterogeneous effect of labelling along birth order. labelling is found to have an almost zero effect for children who are not first-born (decreasing consumption by 1.3 pp.) and to have insignificant positive effects for eldest children: labelling leads to a 3pp. increase in the share of first-born children's consumption. Column (4) allows for the effects of labelling to differ per birth order and gender. Eldest sons seem to be the only group benefiting from labelling : their share of consumption increases by 6.37 pp., an effect which remains statistically insignificant.

Finally, column (3) shows that the age of the child does not influence the effect of labelling: both the coefficients on labelling and on the interaction terms are insignificant and very close to zero.

[Table M.2]

Table M.2: The effect of labelling: Heterogeneity

	(1)	(2)	(3)	(4)
	Δs_3	Δs_3	Δs_3	Δs_3
labelling	0.00382	-0.0135	-0.00331	-0.0171
	(0.0314)	(0.0247)	(0.0268)	(0.0394)
labelling $\times Trait =$ Girl	-0.0166			0.00620
	(0.0405)			(0.0499)
labelling $\times Trait =$ First born		0.0301		0.0808
		(0.0410)		(0.0612)
labelling $\times Trait =$ 8 years old and older			-0.00113	
			(0.0389)	
labelling $\times Trait =$ First born $\times$ Girl				-0.0882
				(0.0837)
Control variables	Yes	Yes	Yes	Yes
Mean in *Control*	-0.075	-0.075	-0.075	-0.075
N	370	370	370	370
Mean Δs_3 for AGD respondents in Control if $Trait = 1$	-0.064	-0.071	-0.093	
Mean Δs_3 for AGD respondents in Control if $Trait = 0$	-0.088	-0.078	-0.065	
p-value of difference in means	0.3167	0.7827	0.2611	
Share with $Trait =$ First Born		29.2%	Mean in *Control*	-0.071
Share with $Trait =$ Over 8		42.4%	Mean in *Control*	-0.093
Share with $Trait =$ Girl $\times$ First Born		16.2%	Mean in *Control*	-0.049

Notes: This table reports how the estimated impact of labelling on reallocation decisions at $t = 2$ in Scenario B varies alongside different heterogeneity traits for parent-biased respondents. The outcome variable is the difference in between the share of peanuts allocated to be consumed by the child $t = 3$ when making the decision at $t = 2$ and $t = 1$. At the beginning of the second round, respondents in the labelling sub-sample are reminded of the allocation choices they have made in the first round. The sample is restricted to the control and labelling sub-samples to avoid any framing effect of our other treatment arms on the second round decisions. The sample is restricted to parent-biased respondents only. Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the order in which scenarios A and B were presented to the respondent and a dummy variable indicating whether the household is Muslim or not. Each column additionnally controls for the level of the trait alongside which heterogeneity is measured. Standard errors in parentheses. * $p < 0.1$, ** $p < .05$, *** $p < .01$

M.3 Beliefs over preference reversals

We explore whether AGD respondents have different beliefs over the probability that otehrs will reallocate away from their children's consumption between decision rounds using the following specification:

$$Y_i = \alpha + \gamma_0 \mathbb{1}\{\theta < 1\} + \gamma_1 \mathbb{1}\{Trait_i\} + \gamma_2 \mathbb{1}\{Trait_i\} \times \mathbb{1}\{\theta < 1\} + \theta_p + \lambda X_i + \epsilon_i \tag{M.3}$$

where the outcome variable is a dummy equal to one if the respondent expects that most

others will reallocate away from their children's consumption between rounds, 0 otherwise. The results from this regression are presented in table M.3. Column (1) shows that AGD respondents are 1.97 pp. less likely than non-AGD parents to believe that others will reallocate away from their children when the child involved in the experiment is a boy and 8.8pp. if the child is a girl. Both effects are statistically insignificant. Column (2) shows that when the child involved is not a first-born, AGD parents are 4.72pp. less likely than non-AGD parents to believe that others will reallocate away from their children and 7.37pp. if the child is first-born. However, the difference between both is not statistically significant. Finally, column (3) shows that AGD parents' beliefs over reallocation is not influenced by their child's age. In the next subsection, we explore whether the fact that AGD parents are less likely to believe that others will reallocate away from their children's consumption when the child is a girl or first-born translates into a lower demand for commitment for those children.

M.4 Demand for commitment

We explore whether AGD respondents demand more or less commitment devices depending on observable characteristics of the child in this subsection.

$$Y_i = \alpha + \gamma_0 \mathbb{1}\{\theta < 1\} + \gamma_1 \mathbb{1}\{Trait_i\} + \gamma_2 \mathbb{1}\{Trait_i\} \times \mathbb{1}\{\theta < 1\} + \theta_p + \lambda X_i + \epsilon_i \tag{M.4}$$

where θ_p are price fixed effects. Where:

- $Y_i = 1$: if i took up the probabilistic commitment device;
- $Price$: Price of commitment;
- $\mathbb{1}\{\theta < 1\} = 1$ if respondent i is parent-biased;
- $\mathbb{1}\{Trait_i\}$: Vector of background characteristics along which we test for heterogeneity in the impact of parent-bias;
- θ_p: Price fixed effects;

Table M.3: Beliefs and heterogeneity

	(1)	(2)	(3)	(4)
	Most others will reallocate away from their children			
$\mathbb{1}\{\theta < 1\}$	-0.0197	-0.0472*	-0.0535*	-0.0144
	(0.0308)	(0.0251)	(0.0276)	(0.0365)
$\times Trait$ = Girl	-0.0683			-0.0637
	(0.0425)			(0.0504)
$\times Trait$ = First Born		-0.0265		-0.0191
		(0.0470)		(0.0685)
$\times Trait$ = 8 years old and older			-0.0006	
			(0.0433)	
$\times Trait$ = Girl $\times$ First Born				-0.0148
				(0.0940)
Control variables	Yes	Yes	Yes	Yes
Mean	0.393	0.393	0.393	0.393
Respondents	2404	2404	2404	2404
Mean for AGD respondents if $Trait = 1$	0.331	0.341	0.363	
Mean for AGD respondents if $Trait = 0$	0.385	0.362	0.353	
p-value of difference in means	0.1212	0.5977	0.7804	

Notes: This table reports how parents' beliefs over their future reallocating behavior varies alongside different heterogeneity traits. The outcome variable is a dummy equal to one if the respondent predicted at $t = 1$ that most others would allocate less than planned towards their own children at $t = 2$. This measure of beliefs was incentivized. Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the order in which scenarios A and B were presented to the respondent and a dummy variable indicating whether the household is Muslim or not. Each column additionnally controls for the level of the trait alongside which heterogeneity is measured. Standard errors in parentheses. * $p < 0.1$, ** $p < .05$, *** $p < .01$

- X_i: Vector of individual characteristics: age and gender of the respondent and the child, measure of credit constraints, religion of the household, order of the scenarios, number of children at $t = k$;

- ϵ_i: Standard error.

[Table M.4]

Table M.4 presents the results of this heterogeneity analysis for several observable characteristics: the child's gender (col (1)), birth order (col(2)), the child's age (col(3)) and the interaction between the child's gender and birth order (col (4)). Column (1) finds that AGD parents of boys are 3.5pp. more likely to commit to their Scenario B decision than non-AGD parents of boys, an effect which is statistically significant. However, AGD parents of girls are 1.37pp. less likely to commit to their decision than non-AGD parents of girls, a difference which is statistically significant, even though it remains small in magnitude given that 90.3% of respondents commit to their $t = 1$ decision in Scenario B. This is coherent with findings of the previous subsection that AGD parents were less likely to believe that others would reallocate away from the child's consumption if the child involved in the experiment was a girl, than a boy. Column (4) finds that the demand is even smaller for non-first-born girls. Columns (2) and (3) find no statistically significant difference in the effect of AGD on uptake of the commitment device according to the child's age or birth order.

M.5 The effect of child participation

In table M.5, we assess whether the effect of child participation varies alongside some observable characteristics of the child of AGD respondents. None of the interaction terms between heterogeneity traits and the child participation dummy is statistically significant. In line with our intuition, the effect of child participation is dampened for girls : having the child participate in round 2 decision actually reduces the share of consumption allocated to the child by 6.7 pp. if that child is a girl and 1.35pp. if that child is a boy. Counter-intuitively, the effect of child participation is (insignificantly) weaker for first-born (col. (2)) and the age of the child does not affect its impact (col. (3)). Column (4) allows for the effects of the child's gender and birth order to interact. Child participation seems to be particularly ineffective for first-born boys and girls of any birth order.

[Table M.5]

In table M.6, we estimate whether parents more likely to let their child participate in second round decisions more often depending on the child's age, gender or birth order.

[Table M.6]

Table M.4: Uptake of the probabilistic commitment device, Scenario B: Heterogeneity

	(1)	(2)	(3)	(4)
$\mathbb{1}\{\theta < 1\}$	0.0352**	0.000862	0.0152	0.0324
	(0.0179)	(0.0170)	(0.0186)	(0.0218)
$\times Trait$ = Girl	-0.0489*			-0.0602*
	(0.0275)			(0.0337)
$\times Trait$ = First Born		0.0303		0.00934
		(0.0291)		(0.0388)
$\times Trait$ = 8 years old and older			-0.0123	
			(0.0281)	
$\times Trait$ = First Born $\times$ Girl				0.0395
				(0.0581)
Control variables	Yes	Yes	Yes	Yes
Mean	0.903	0.903	0.903	0.903
N	2027	2027	2027	2027
Mean for AGD respondents if $Trait = 1$	0.883	0.915	0.900	
Mean for AGD respondents if $Trait = 0$	0.938	0.905	0.930	
p-value of difference in means	0.0167	0.6579	0.2210	

Notes: This table reports how the estimated impact of AGD on the demand for the probabilistic commitment device in Scenario B varies alongside different heterogeneity traits. The outcome variable is a dummy equal to 1 if the respondent takes up the probabilistic commitment device, zero otherwise. Respondents choose whether to take up the commitment device after making allocation decisions in Scenario B during the first visit. Taking up the commitment device reduces the probability that round 2 decision will be implemented. The commitment device comes at a random price (0.5, 1 or 1.5 packet of peanuts deducted from the parents' allocation at $t = 3$.) AGD is computed using the share of peanuts going to the child in a budget non-adjusted for the price of commitment based on the parents' decision at $t = 1$. Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the order in which scenarios A and B were presented to the respondent and a dummy variable indicating whether the household is Muslim or not. Control variables also include the level value of all the $Trait$ variables in all regressions and price fixed effects. The sample is restricted to parents having been offered the probabilistic commitment device. Standard errors in parentheses. * $p < 0.1$, ** $p < .05$, *** $p < .01$

Table M.5: The effect of child participation among AGD respondents: Heterogeneity

	(1)	(2)	(3)	(4)
	Δs_3	Δs_3	Δs_3	Δs_3
Child	-0.0135	-0.0344	-0.0385	-0.00136
	(0.0310)	(0.0270)	(0.0345)	(0.0388)
$Child \times Trait =$ Girl	-0.0539			-0.0609
	(0.0475)			(0.0535)
$Child \times Trait =$ First Born		-0.0214		-0.0320
		(0.0559)		(0.0676)
$Child \times Trait =$ 8 years old and older			-0.00246	
			(0.0467)	
$Child \times Trait =$ First Born $\times$ Girl				0.0174
				(0.119)
Control variables	Yes	Yes	Yes	Yes
Mean	-0.0916	-0.0916	-0.0916	-0.0916
N	1910	1910	1910	1910
Respondents	955	955	955	955
Mean Δs_3 for AGD respondents in Control if $Trait = 0$	-0.064	-0.071	-0.093	
Mean Δs_3 for AGD respondents in Control if $Trait = 1$	-0.088	-0.078	-0.065	
p-value of difference in means	0.3167	0.7827	0.2611	

Notes: This table reports how the estimated impact child participation varies alongside different heterogeneity traits for AGD respondents. AGD is computed using the share of peanuts going to the child in a budget non-adjusted for the price of commitment based on the parents' decision at $t = 1$. The outcome variable is the difference in between the share of peanuts allocated to be consumed by the child $t = 3$ when making the decision at $t = 2$ and $t = 1$. Respondents in the child participation (imposed) sub-sample make their second round decision with their child. Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the order in which scenarios A and B were presented to the respondent and a dummy variable indicating whether the household is Muslim or not. Control variables also include the level value of all the $Trait$ variables in all regressions. The sample is restricted to the control and the child participation (imposed) sub-samples and to parent-biased respondents. Standard errors in parentheses. * $p < 0.1$, ** $p < .05$, *** $p < .01$

Across all those specifications, we find that AGD respondents are between 17.4 and 16.4 pp. more likely to choose to involve their child in the second round decision. However, this association is particularly strong for boys, non-first-born and children younger than 8 years old. AGD-parents of girls are not more likely than non-AGD parents to involve them (col.(2)) and so are parents of older kids and first borns. This is in line with the (statistically insignificant) patterns of the effects of child participation we observed in table M.5, where we found that child participation is particularly ineffective at mitigating the effects of AGD for girl children, older children and first born.

Table M.6: Demand for child participation and AGD: Heterogeneity

	(1)	(2)	(3)	(4)
$\mathbb{1}\{\theta < 1\}$	0.174**	0.125**	0.189***	0.163**
	(0.0744)	(0.0620)	(0.0724)	(0.0828)
$\times Trait$ = Girl	-0.154			-0.0791
	(0.111)			(0.126)
$\times Trait$ = First Born		-0.103		0.0414
		(0.136)		(0.192)
$\times Trait$ = 8 years old and older			-0.206*	
			(0.113)	
$\times Trait$ = First Born $\times$ Girl				-0.278
				(0.267)
Control variables	Yes	Yes	Yes	Yes
Mean	0.586	0.586	0.586	0.586
N	377	377	377	377
Mean for AGD respondents if $Trait = 1$	0.930	0.958	0.955	
Mean for AGD respondents if $Trait = 0$	0.982	0.956	0.957	
p-value of difference in means	0.1730	0.9534	0.2210	

Notes: This table reports how the estimated impact of AGD on the demand for child participation in Scenario B varies alongside heterogeneity traits. The outcome variable is dummy equal to 1 if the respondent chose to involve her child in round 2 decision, zero otherwise. Respondents choose whether to involve their child after making allocation decisions in Scenario B during the first visit. Child participation comes at a random price (0, 0.5, 1 or 1.5 packet of peanuts deducted from the parents' allocation at $t = 3$). AGD is computed using the share of peanuts going to the child in a budget non-adjusted for the price of commitment based on the parents' decision at $t = 1$. The sample is restricted to parents having been offered the possibility to involve their child in the second round decision. Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the order in which scenarios A and B were presented to the respondent and a dummy variable indicating whether the household is Muslim or not. Standard errors in parentheses. Control variables also include the level value of all the $Trait$ variables in all regressions and price fixed effects. * $p < 0.1$, ** $p < .05$, *** $p < .01$

N Pre-analysis plan - Initial experiment

This pre-analysis plan was registered at the AEA RCT registry under AEARCTR-0003535 and is available at https://www.socialscienceregistry.org/trials/3535.

N.1 Introduction

The inability to invest in children's health and education has dramatic consequences on children's lives in developing countries. Under-five mortality rates are still dramatically high in Sub-Saharan Africa, where children are 15 times more likely to die before the age of five than children in developed countries. More than half of these early child deaths are due to conditions that could be prevented or treated if parents invested in preventative health products for their children (WHO, 2017). Nevertheless, households in developing countries typically do not access preventive health care, even when available at low cost (Glennerster & Kremer, 2012). In Malawi for instance, only 8% of children between 6 and 23 months old are fed a diet meeting the minimum acceptable dietary standards (DHS, 2017). 13.5% of children between 6 and 17 years old in poor Malawian households were temporarily withdrawn from school during the 2012-2013 academic year and non-illness related health care was purchased for only 0.7% of Malawian children under 10 years old (UNC-CH et al., 2014).

Parents' time-preferences seem to be a key mechanism behind under-investment in children's health (Glennerster & Kremer, 2012) and education. While parents' present-biases influences how much they invest in their children's human capital (Ringdal & Sjursen, 2017)) or preventative health care (Dupas & Robinson, 2013; Tarozzi & Mahajan, 2011), there are reasons to believe that other dimensions of time-preferences may have similarly crucial effects. In particular, one tends to have different time-preferences when making decisions for oneself than for others (Barton, 2015). Is it the case that such differences also arise when parents evaluate their own future consumption in contrast to that of their children?

This issue has not been investigated to date. It matters because, even if parents do not display present-bias, they can still display parent-bias: when parents discount children's future consumption by a higher factor than their own future consumption, they plan to allocate a higher share of the household budget to their children's consumption in the future, but systematically reverse those plans when such future arises.

How common is parent-bias? Are parents sophisticated about it? Is there demand for commitment? Do parents choose to involve their children in the decision to counterweight parent-bias? What is the joint distribution of present bias and parent bias? Can behavioral interventions such as labeling overturn preference reversals?

We depart from a simple theoretical model to study those questions. If parents discount their utility of future consumption to a greater extent than that of their children, they will systematically reverse plans to invest more in their children in the future.

This study will document whether parents make plans to invest in their children's in the future, but are tempted to reverse them at the moment the investment needs to be done, favoring their own consumption at the expense of investments in their children. Additionally, we will measure whether parents, at the time of making investment decisions, are aware of the risk that they could change their mind in the future, and demand commitment devices to help them stick to their plans. Answering these questions could contribute to explain why investments in children's health and education are so low. Moreover, it would produce important insights to design cost-effective interventions, such as commitment devices, to reduce the temptation to divert resources away from children.

We present here the design of a lab-in-the-field experiment designed to test the following hypotheses:

1. Do parents discount their own future consumption and that of their children differently?
2. Does this differential discounting give rise to within-household inconsistencies (parent-bias)?
3. Is there demand for commitment devices to help mitigate parent-bias, above and beyond demand for commitment devices that help mitigate classical present-bias?
4. Do parents demand to involve their children in future decisions as a commitment device?
5. Is the demand for commitment explained by parents' beliefs that they might be tempted to change their plans in the future?
6. Can labeling mitigate parent-bias?
7. Can encouraging children to participate in household decisions increase investments in children and mitigate parent-bias?

N.2 Model

N.2.1 General setting

Our intuition departs from a simple three-period model of parental utility maximization. At time $t = 1$, parents do not make any decision but make plans for two future time-periods, $t = 2$ and $t = 3$. At time $t = 2$, parents chose how much to consume (x_2), how much to save (s_2) and how much their children consume (z_t). At $t = 3$, parents chose how much to consume (x_3) and how much their children consume (z_3). This model differs from previous models of parental investments in children in the sense that parents discount their future consumption and that of their children's differently. To illustrate the dynamics of our model in a simplified way, let's start by assuming that the parents make two separate decisions:

- (1) *Inter-temporal decision for oneself*: How much parents want to consume themselves at $t = 2$ and $t = 3$,

- (2) *Within-household allocation*: How parents want to split a given amount of resources in a given time-period between themselves and their child.

Parents have the following discount functions:

- $(1, \beta_a \delta_a, \beta_a \delta_a^2 ...)$ for their own consumption;

- $(1, \beta_c \delta_c, \beta_c \delta_c^2 ...)$ for their child's consumption.

Parents have beliefs over their future discount functions:

- $(1, \hat{\beta}_a \delta_a, \hat{\beta}_a \delta_a^2 ...)$;

- $(1, \hat{\beta}_c \hat{\delta}_c, \hat{\beta}_c \hat{\delta}_c^2 ...)$.

Those beliefs imply that parents can have different levels of naiveté:

- Parents can be sophisticated: $\hat{\beta}_a = \beta_a$, $\hat{\beta}_c = \beta_c$, $\hat{\delta}_c = \delta_c$;

- Parents can be fully naive: $\hat{\beta}_a = 1$, $\hat{\beta}_c = 1$, $\delta_a = \hat{\delta}_c$;

- Parents can be partially naive and under-estimate the extent of their time-inconsistencies: $1 > \hat{\beta}_a > \beta_a$, $1 > \hat{\beta}_c > \beta_c$, $\delta_c > \hat{\delta}_c > \delta_a$.

N.2.2 First decision: Inter-temporal decision for oneself

At $t = 1$ parents optimize:

$$\underset{(x_t)_{t=2,3}, s_2}{\text{Max}} \beta_a \delta_a u(x_2^1) + \beta_a \delta_a^2 u(x_3^1)$$

$$\text{s.t.}$$

$$\begin{cases} x_2 + s_2 \leq y_2 \\ x_3 \leq (1+r) s_2 \\ y_2 = y \end{cases}$$

Where:

$u(x_t)$: parent's utility of consumption at time t;
β_a: quasi-hyperbolic discount factor that the parent uses towards her future consumption;
δ_a : discounting factor that the parent uses towards her future consumption;
r: interest rate on savings.
At $t = 2$ they optimize:

$$\underset{(x_t)_{t=2,3}, s_2}{\text{Max}} \; u(x_2^2) + \beta_a \delta_a u(x_3^2)$$

Comparing $t = 1$ and $t = 2$ FOCs brings to light time inconsistencies:

2$t = 1$ FOCs: $\frac{u'(x_2^1)}{u'(x_3^1)} = \delta_a(1 + r)$ $t = 2$ FOCs: $\frac{u'(x_2^2)}{u'(x_3^2)} = \beta_a\delta_a(1 + r)$ If $\beta_a < 1$, respondents will save less for their $t = 3$ consumption when making the choice at $t = 2$ ("x_2^2") than when making the choice at $t = 1$ ("x_2^1"). Those are traditional *present-biases*, emerging from quasi-hyperbolic discounting (Laibson, 1997). Depending on their level of sophistication,$t = 1$ parents have different beliefs concerning their future allocations, $\hat{x}_2^2$ and $\hat{x}_3^2$. They believe that their $t = 2$ FOCs will be: $\frac{u'(\hat{x}_2^2)}{u'(\hat{x}_3^2)} = \hat{\beta}_a\delta_a(1 + r)$.

- – For naive parents: $\hat{\beta}_a = 1$, so $\hat{x}_2^2 = x_2^1$ and $\hat{x}_3^2 = x_3^1$;
  – For sophisticated parents:$\hat{\beta}_a = \beta_a$ and $\frac{u'(\hat{x}_2^2)}{u'(\hat{x}_3^2)} = \beta_a\delta_a(1 + r)$;
  – For partially naive parents:$1 > \hat{\beta}_a > \beta_a$ and $\frac{u'(\hat{x}_2^2)}{u'(\hat{x}_3^2)} = \hat{\beta}_a\delta_a(1 + r)$.

Would those parents demand commitment to stick to their $t = 1$ plans? Let's assume there exists a commitment contract with direct implementation price p_s which would allow parents to stick to the plans they made at $t = 1$.

$t = 1$ parents will chose to commit to their $t = 1$ plan if:

$$\beta_a\delta_a(u(x_2^1) - u(\hat{x}_2^2)) + \beta_a\delta_a^2(u(x_3^1) - u(\hat{x}_3^2)) > p_s$$

It is easy to see that the right hand side of this equality is equal to 0 for naive parents: they will never chose to commit to their $t = 1$ allocation at a positive price. The WTP for commitment of sophisticated and partially naive parents depends on the shape of their utility function, the value of their discount factors and their beliefs over future discount factors.

If we assume a functional form for the parents' utility function, $u(x) = log(x)$, then the parents' WTP for commitment is given by the following condition:

$$\iff \frac{(1-\hat{\beta}_a\delta_a)^{\delta_a+1}}{\hat{\beta}_a^{\delta_a}} > e^{\frac{p_s}{\beta_a\delta_a}}(1 - \delta_a)^{1+\delta_a}$$

For partially naive parents, the higher $\hat{\beta}_a$, the lower the WTP for commitment. We can identify a cut-off price for sophisticated parents:

$$\frac{(1-\beta_a\delta_a)^{\beta_a\delta_a+\beta_a\delta_a^2}}{\beta_a^{\beta_a\delta_a^2}(1-\delta_a)^{\beta_a\delta_a+\beta_a\delta_a^2}} > e_s^p$$

N.2.3 Second decision: Within-household allocation

Parents have to make a plan to split income y_2 and y_3 with their child at $t = 2$ and $t = 3$ but do not have access to a technology to smooth consumption across time-periods. Parents can make this choice at $t = 1$ and $t = 2$, but the choice they make at $t = 2$ is binding for $t = 3$.

At $t = 1$ parents optimize:

$$\underset{(z_t)_{t=2,3},(x_t)_{t=2,3}}{\text{Max}} \quad \beta_a\delta_a u(x_2^1) + \beta_a\delta_a^2 u(x_3^1) + \alpha\beta_c\delta_c v(z_2^1) + \alpha\beta_c\delta_c^2 v(z_3^1)$$
$$\text{s.t.}$$
$$\begin{cases} z_2 + z_2 \leq y_2 \\ z_3 + z_3 \leq y_3 \\ y_3 = y_2 = y \end{cases}$$
Where:

$v(z_t)$: child's utility of consumption at time t;
α : utility weight that the parent attributes to child's utility (imperfect altruism);
δ_c : discounting factor that the parent uses towards her child's future consumption;
β_c: quasi-hyperbolic discount factor that the parent uses towards her child's future consumption.

At $t = 2$ the parents optimize:

$$\underset{(z_t)_{t=2,3},(x_t)_{t=2,3}}{\text{Max}} \quad u(x_2^1) + \beta_a\delta_a u(x_3^1) + \alpha v(z_2^1) + \alpha\beta_c\delta_c v(z_3^1)$$

Comparing $t = 1$ and $t = 2$ FOCs brings to light time inconsistencies: The $t = 1$ FOCs are:

$2\frac{u'(x_2^1)}{v'(z_2^1)} = \frac{\alpha\beta_c\delta_c}{\delta_a\beta_a} \; \frac{u'(x_3^1)}{v'(z_3^1)} = \frac{\alpha\beta_c\delta_c^2}{\delta_a^2\beta_a}$

The $t = 2$ FOCs are:

$2\frac{u'(x_2^2)}{v'(z_2^2)} = \alpha \; \frac{u'(x_3^2)}{v'(z_3^2)} = \frac{\alpha\beta_c\delta_c}{\delta_a\beta_a}$

Comparing those FOCs lead us to make the following observations:

1. The parents' preferred allocation will vary across time as long as $\delta_c \neq \delta_a$, even if $\beta_a = \beta_c = 1$, i.e. even if parents are not present-biased. If $\delta_a < \delta_c$, parents, will plan to allocate more to their children in later time periods and more to themselves in nearer time frames.
2. Even if $\beta_a = \beta_c$, if $\delta_c \neq \delta_a$, parents will renege on their round 1's preferred allocation when making the choice in round 2 again. In particular, if $\delta_a < \delta_c$, they will reallocate more towards their own consumption in round 2 and 3 than what they had initially planned to do. This is what we call *parent-bias.*

3. If $\beta_a \neq \beta_c$: the gap between round 2 and 3 allocations will increase. The reason for this increased gap is that in round 1, parents make decisions for two future allocations, rounds 2 and 3; while in round 2 this decision is made for a decision for a present and a future allocation. This is what we call *within-household present-biases.*

All parents have correct beliefs about their $t = 2$ within-household allocations:

- For all parents: $\frac{u'(\hat{x}_2^2)}{v'(\hat{z}_2^2)} = \frac{u'(x_2^2)}{v'(z_2^2)} = \alpha$

But, parents can have different beliefs about the $t = 3$ allocation they would chose at $t = 2$:

- $\frac{u'(\hat{x}_3^2)}{v'(\hat{z}_3^2)} = \frac{\alpha\hat{\beta}_c\hat{\delta}_c}{\delta_a\hat{\beta}_a}$

For this decision, we assume that parents can be sophisticated along two dimensions:

- They can be sophisticated regarding their present-biases: $\hat{\beta}_c = \beta_c$ and $\hat{\beta}_a = \beta_a$;

- They can be sophisticated regarding the difference between the factors with which they discount their own and their child's future consumption : $\hat{\delta}_c = \delta_c$ and $\hat{\delta}_a = \delta_a$;

Let's assume that for each future period, parents can commit to their $t = 1$ planned allocation for prices p_{w2} and p_{w3} respectively.

$t = 1$ parents will chose to commit to their $t = 2$ planned allocations if:

$$\beta_a\delta_a(u(x_2^1) - u(\hat{x}_2^2)) + \alpha\beta_c\delta_c(v(z_2^1) - v(\hat{z}_2^2)) > p_{w2}$$

Note that this price holds for all parents, irrespective of their sophistication.

Assuming the same marginal utility of consumption for the parent and the child: $u(a) = v(a) = log(a)$, we can derive a willingness to pay for commitment for the second time period:

$$\left(\frac{(1+\alpha)}{\delta_a\beta_a+\alpha\beta_c\delta_c}\right)^{\beta_a\delta_a+\alpha\beta_c\delta_c}(\delta_c\beta_c)^{\alpha\beta_c\delta_c}(\delta_a\beta_a)^{\beta_a\delta_a} > e^{p_{w2}}$$

If $\delta_c = \delta_a$, the parents are unwilling to pay for commitment.

$t = 1$ parents will chose to commit to their $t = 3$ planned allocations if:

$$\beta_a \delta_a^2 (u(x_3^1) - u(\hat{x}_3^2)) + \alpha \beta_c \delta_c^2 (v(z_3^1) - v(\hat{z}_3^2)) > p_{w3}$$

Assuming the same marginal utility of consumption for the parent and the child: $u(a) = v(a) = log(a)$, we can derive a willingness to pay for commitment when parents are fully sophisticated regarding their preferences :

$$\delta_a^{\delta_a^2 \beta_a} \delta_c^{\delta_c^2 \beta_c} \left(\frac{\delta_c \beta_c \alpha + \delta_a \beta_a}{\delta_c^2 \beta_c \alpha + \delta_a^2 \beta_a}\right)^{\delta_a^2 \beta_a + \alpha \delta_c^2 \beta_c} > e^{p_{w3}}$$

Parents who are sophisticated regarding β_a and β_c have the following WTP for commitment to the $t = 3$ allocation:

$$\frac{\delta_a^{\beta_a \delta_a} \delta_c^{2\alpha \beta_c \delta_c}}{\hat{\delta_c}^{\alpha \beta_c \delta_c}} \left(\frac{\delta_a \beta_a + \alpha \beta_c \hat{\delta_c}}{\delta_a^2 \beta_a + \alpha \hat{\delta_c^2} \beta_c}\right)^{\beta_a \delta_a + \alpha \beta_c \delta_c} > e^{p_{w3}}.$$

The WTP for commitment of parents who are sophisticated regarding δ_c but quasi-naive when it comes to β_a decreases the more naive the parents are (ie. the higher $\hat{\beta_a}$):

$$\delta_a^{\beta_a \delta_a} \delta_c^{\alpha \beta_c \delta_c} \left(\frac{\beta_a}{\hat{\beta}_a}\right)^{\beta_a \delta_a} \left(\frac{\delta_a \hat{\beta}_a + \alpha \beta_c \delta_c}{\delta_a^2 \beta_a + \alpha \delta_c^2 \beta_c}\right)^{\beta_a \delta_a + \alpha \beta_c \delta_c} > e^{p_{w3}}.$$

The table below summarizes the WTP for commitment for the inter-temporal and the within-household choices:

$\hat{\beta}_a \hat{\delta}_c$	Fully naive	Partially naive	Sophisticated
Fully naive	$p_s = 0$, $p_{w2} > 0$, $p_{w3} = 0$	$p_s = 0$, $p_{w2} > 0$, $p_{w3} > 0$ ($p_{w3} \downarrow$ if $\hat{\delta}_c \downarrow$)	$p_s = 0$, $p_{w2} > 0$, $p_{w3} > 0$ (but low)
Partially naive	$p_s > 0$ ($\downarrow$ if $\hat{\beta}_a \uparrow$), $p_{w2} > 0$, $p_{w3} = 0$	$p_s > 0$ ($\downarrow$ if $\hat{\beta}_a \uparrow$) , $p_{w2} > 0$, $p_{w3} > 0$ ($p_{w3} \downarrow$ if $\hat{\delta}_c \downarrow$)	$p_s > 0$ ($\downarrow$ if $\hat{\beta}_a \uparrow$), $p_{w2} > 0$($\downarrow$ if $\hat{\beta}_a \uparrow$), $p_{w3} > 0$
Sophisticated	$p_s > 0$, $p_{w2} > 0$, $p_{w3} = 0$	$p_s > 0$,$p_{w2} > 0$, $p_{w3} > 0$ ($p_{w3} \downarrow$ if $\hat{\delta}_c \downarrow$)	$p_s > 0$, $p_{w2} > 0$, $p_{w3} > 0$

N.2.4 Sample selection

This experiment will be conducted in 80 villages of Salima district in Malawi, with 2,400 participants. As this experiment will be conducted alongside another project's baseline survey, this sample size was based on the power calculations for our other project.

Within each village, our sample is built using a random walk approach: the enumerators assess the eligibility of every 5th or 4th house they encounter in the village while following a pre-determined path.

The households are considered eligible to participate in the experiment if:

1. There is at least one child aged 3-12 in the household,
2. Both parents live in the household,
3. Nobody is allergic to peanuts in the household.

The second criteria was added to guarantee that households in which we interrogate fathers are not on average different from households in which we interrogate mothers.

Only mothers are invited to take part in the experiment in 64 villages. In the remaining 16 villages, we randomly select whether the mother or the father, within the eligible household, will be invited to participate in the experiment. In those villages, we over-sample fathers to ensure that we will have a large enough number of fathers in our experiment. We aim to have 360 fathers in our sample.

If the household has more than one child aged 3-12, we will randomly select which child will be invited to take part in the experiment.

N.3 Experimental design

To be able to document the joint distribution of "Traditional" and "Within-household" time-inconsistencies, our experiment follows a three-step data collection process. We ask the parents to split the consumption of a tempting, non-fungible and immediately consumable good (peanuts) between them and their children and across time, cross-randomizing the type of commitment devices which are made available to the households. Our design allows us to observe the parents' plan at $t = 1$, its potential revision at $t = 2$ and the consumption of the good. Peanuts have been chosen because they are a nutritious food that Malawians are familiar with and because they are consumed by both parents and children in Malawi. The experiment took place during the lean season, when the stock of peanuts that households may have had at home from the previous harvest has been depleted. This timing ensures that peanuts are a tempting good.

N.3.1 Structure of the experiment

The date of the first visit is randomly assigned to villages. A respondent speaks to a different experimenter in each visit and the visits have been scheduled to take place at a similar time of the day. At the beginning of the first and second visits, the surveyor tells the parents that they are interested in learning about peanut consumption in Malawi and that, depending on the choices they make and a random implementation rule, they and their child may be invited to consume some peanuts and share their thoughts about their experience with the team. Note that informing the respondents from the onset that the peanuts will be consumed in front of the enumerator inform them that, depending on the random implementation rule, the decisions they make are binding.

The respondents are first invited to taste a small quantity of peanuts to ensure that they are making those decisions in a “hot” state.

The respondents are then presented with two different scenarios. The order in which the respondents are presented with the different scenarios will randomly vary.

In scenario Blue, the respondent allocates 3 packets of 15 grams of peanuts between $t = 2$ and $t = 3$, that they will consume themselves. Each packet of groundnuts whose consumption is delayed from $t = 2$ to $t = 3$ yields an interest rate of r. The team presents the different possible allocations to the respondent, who picks one. The respondent has to make this choice for three interest rates: 0.5, 1 and 1.5. A picture of the possible allocations is presented to the respondent to facilitate comprehension. In the framework of our model, this relates to the parent’ first decision: they have to chose how much to consume at $t = 2$ and $t = 3$: x_2^1 and x_3^1.

In scenario Red, the respondent allocates 5 packs of groundnuts between themselves and their child to be consumed at $t = 2$ and $t = 3$. This relates to our model’s second decision: the parents have a trade-off between their consumption and that of their children at $t = 2$ (x_2^1 and z_2^1) and at $t = 3$ (x_3^1 and z_3^1). To help with this decision, the parents are invited to share 5 packets of peanuts between two plates, one entitled "you, in two days", the other one “Your child in two days”. The enumerator records this decision. Then the parents are invited to do the same thing for the $t = 3$ allocation.

In the second visit, the respondents and the children are invited to meet the surveying team separately from the rest of the family. The respondents are invited to taste a small quantity of peanuts first and then asked how they would like to act in both scenarios.

At the end of the second visit, one of those scenarios is implemented according to the following random implementation rule:

1. Scenario Red or Blue is randomly picked,
2. $t = 1$ or $t = 2$ decision is randomly chosen to be executed,
3. If Scenario Blue is chosen, the interest rate that counts is randomly selected.

If scenario Blue is picked: the respondent is given the packets for the day.
If scenario Red is picked: the respondent and the child are given the packets for the day according to the chosen split.

While they eat the peanuts, they are asked a series of questions about peanuts and whether they are appreciating eating them. This ensures that the enumerator will observe the actual peanut consumption and that the respondent’s decision has been followed-through.

During the third visit, the peanuts that were allocated to be received on that day are distributed to the respondents and they are asked a series of questions about their peanut consumption to ensure that they are consuming the peanuts in front of the enumerator. At the end of the visit, the parents are also asked a series of survey questions about investments in children and whether they would be interested in a series of commitment devices, such a safe-boxes or a separate meal plan for their child.

N.4 Treatment arms

The subjects are randomized across treatment arms in two steps:

- They are first allocated to be offered different commitment devices: a "Probabilistic commitment" or "Child's participation (chosen)".

	Type of Commitment *Number of respondents*	
	Probabilistic	Child's participation (chosen)
Total	*2000*	*400*
Women	*1740*	*300*
Men	*260*	*100*

- Subjects allocated to being offered a probabilistic commitment device are then allocated to different framings of choice at $t = 2$:

	Framing *Number of respondents*			
	Baseline	Labeling	Random Anchoring	Child's participation (imposed)
Total	*800*	*400*	*400*	*400*
Women	*696*	*348*	*348*	*348*
Men	*104*	*52*	*52*	*52*

N.4.1 Commitment devices

Probabilistic commitment devices We offer the respondents in those treatment arms a probabilistic commitment device (following Augenblick et al. (2015)), which decreases the likelihood that the $t = 2$ allocation is chosen over the $t = 1$ allocation. In other treatment arms or if the respondents do not wish to take up a commitment device, the $t = 1$ decision will be executed with a 10% probablity. If the respondents take up a commitment device, the probability that the $t = 1$ decision will be executed increases to 90%. This allows us to observe both $t = 1$ and $t = 2$ decisions for all respondents, irrespective of commitment and guarantees the credibility of both decisions because the respondents are aware in each round that their decision can be selected to be executed.

The respondents are offered to take up a probabilistic commitment device after making a decision for each scenario during the first visit. We randomly vary the price of the commitment device: to purchase the commitment device, the respondent will have to forego 0.5/1/1.5 packets of peanuts at $t = 3$.

Child's participation (chosen) We ask respondents in this subsample whether they would like to invite their child to make the $t = 2$ decision for part Red with them. This could be a way for $t = 1$ parents to force their $t = 2$ self to stick to the plan they had made for their child.

We randomly vary the price of this commitment device: to purchase the commitment device, the respondent will have to forego 0/0.5/1/1.5 packets of peanuts at $t = 3$.

N.4.2 Framing of choices

Baseline The enumerators speak to the respondents alone during the second visit. The respondents are invited to taste a small quantity of peanuts at the beginning of the interview, explained the rules of the experiment one more time and asked how they would like to act in each scenario.

Labeling treatment The enumerators speak to the respondents alone during the second visit. The respondents are invited to taste a small quantity of peanuts at the beginning of the interview, explained the rules of the experiment one more time, presented with the allocation choice they have made in scenario Red at $t = 1$ and asked how they would like to act in each scenario.

Random Anchoring treatment The enumerators speak to the respondents alone during the second visit. The respondents are invited to taste a small quantity of peanuts at the beginning of the interview, explained the rules of the experiment one more time, presented with a random allocation in scenario Red and asked how they would like to act in each scenario. This treatment arm enables us to distinguish between the effect of labeling itself and of anchoring.

Child's participation (imposed) During the second visit, the children are asked to participate in part Red decision in this treatment arm. The respondents and the children are invited to taste a small quantity of peanuts at the beginning of the interview, explained the rule of the experiment. The respondent makes a decision on part Blue alone and on part Red jointly with the child. This treatment arm enables us to measure the impact of an increase in the child's bargaining power on parent-bias, without the self-selection inherent to parents having chosen to involve their child as a commitment device.

N.4.3 Survey instruments

Naive and sophisticates Understanding how sophisticated individuals are with regards to their future behavior is key to interpreting the demand for commitment devices. However, incentivizing questions eliciting beliefs about one's own future behavior can lead to changes in this future behavior (Acland & Levy, 2015) or can encourage individuals to use predictions about their own behavior as a commitment device (Augenblick & Rabin, 2019).

To circumvent this problem, we adopt a strategy following closely that of Toussaert (2018). After making a choice for each scenario, respondents are asked an incentivized question eliciting their beliefs about others' behavior:

- *Scenario Blue:* We are asking many other households to make those decisions. Do you think that two days from now most other people will...
  - Choose to receive MORE peanuts immediately than they did today?
  - Choose to receive LESS peanuts immediately than they did today?
  - Choose to receive the same amount of peanuts immediately as they did today?
- *Scenario Red:* We are asking many other households to make those decisions. Do you think that two days from now most other people will...
  - Choose to give LESS peanuts to the child than they did today?
  - Choose to give MORE peanuts to the child than they did today?
  - Choose to give the same amount of peanuts to the child as they did today?

Correctly predicting the behavior of the majority of the population will earn the respondents one additional packet of peanuts at the end of round three.

Those questions are motivated by research that shows that people use information about their own behavior to inform their beliefs about the behavior of others. People's beliefs about others' future behavior has been proven to correlate highly with beliefs about one's future behavior (Toussaert, 2018).

To assess this last claim in our sample, our survey instruments include unincentivized questions in which the respondent is asked to make prediction about her own behavior. For our incentivized question to be a valid measure of one's sophistication, the answers to both sets of questions must be correlated.

N.5 Empirical analysis

1. Do parents discount their own future consumption and that of their children differently?

Hypothesis 1a: Parents discount their own future consumption more than that of their children.

In terms of our model, this is equivalent to testing $\delta_a < \delta_c$. Parents who exhibit such preferences will choose to allocate more to their children in later time-periods. This is equivalent to testing H_0 vs. $H_A : \beta > 0$ in the following regression:

$$s^1_{ji} = \alpha + \beta * Thirdvisit_j + \epsilon_i$$

Where:

- s^1_{ji}, the share of peanuts to be consumed by respondent i's child at $t = j$ ($j \in \{2, 3\}$), when making the decision at $t = 1$;
- $Thirdvisit_j$: Dummy variable equal to 1 if the decision concerns the $t = 3$ allocation, 0 otherwise.

We will pool the observations from all our subsamples except "Child's participation (imposed)" to conduct this analysis. This would allow us to detect a 0.0886 s.d. difference in the share of peanuts allocated to the child at $t = 2$ and $t = 3$. For consistency with the rest of our analyses, we will also conduct the same regression in the "Probabilistic commitment device × Baseline" sample, which would allow us to detect a 0.1402 s.d. difference.

2. Does this differential discounting give rise to within-household inconsistencies (parent-bias)?

Hypothesis 2a: Parents exhibit parent-bias.

If $\delta_c > \delta_a$, parents will reallocate more peanuts towards their own consumption in round 2 than what they had initially planned to do. We will test the presence of parent-bias in our sample by testing $H_0 : \beta = 0$ vs. $H_A : \beta < 0$ in the following regression:

$$s^k_{2i} = \alpha + \beta * Secondvisit_k + \epsilon_i \text{ Where:}$$

- s^k_{2i}, the share of peanuts to be consumed by respondent i's child at $t = 2$, when making the decision at $t = k$, $k \in \{1, 2\}$;
- $Secondvisit_k$: Dummy variable equal to 1 if the decision is taken at $t = 2$, 0 otherwise.

We are testing whether parents allocate a smaller share of peanuts for their children to consume at $t = 2$ when making the decision at $t = 2$ rather than at $t = 1$. We will conduct this regression in the "Probabilistic commitment device $\times$ Baseline" sample, which would allow us to detect a 0.1402 s.d. difference.

Hypothesis 2b: Parents exhibit Within-household Present-Bias.

In terms of our model, if $\beta_a \neq \beta_c$: the gap between round 2 and 3 allocations will increase, depending on whether the parents' decision is made at $t = 1$ or $t = 2$. The reason for this increased gap is that in round 1, parents make decisions for two future allocations, rounds 2 and 3; while in round 2 this decision is made for a present and a future allocation.

We will measure the presence of within-household present-biases by testing $H_0 : \beta = 0$ vs. $H_A : \beta > 0$ in the following equation:

$$\Delta^k s_i = \alpha + \beta * Secondvisit_k + \epsilon_{ik} \text{ Where:}$$

- $\Delta^k s = s_3^k - s_2^k$: the difference between the share of peanuts allocated to be consumed by the child at $t = 3$ and $t = 2$ while making the decision at $t = k$
- $Secondvisit_k$: Dummy variable equal to 1 if the decision was taken at $t = 2$, 0 otherwise.

Additional descriptive statistics: Different types of within-household time-inconsistencies.

We allow for the presence of three different types of parents:

- Parent-biased parents who reallocate more towards their own consumption than they had originally planned, that is, for whom: $s_2^1 > s_2^2$;
- Consistent parents for whom: $s_2^1 = s_2^2$;
- Child-biased parents who reallocate more towards their child's consumption than they had originally planned, that is, for whom: $s_2^1 < s_2^2$.

We will plot the distribution of those three types of parents in our sample.

Additional descriptive statistics: Joint distribution of present-bias and within-household time-inconsistencies.

If $\beta_a < 1$, respondents will choose to receive more peanuts at $t = 2$ when making the choice at $t = 2$ than at $t = 1$ in scenario Blue. To understand the distribution of traditional present-biases, we will test $H_0 : \beta = 0$ vs. $H_A : \beta > 0$ in the following regression:

$$\bar{x}^k_{2i} = \alpha + \beta * Secondvisit_k + \epsilon_i \text{ Where:}$$

- $\bar{x}^k_{2i} = \frac{1}{3} \sum_{r=0.5}^{1.5} x^k_{2r}$, where x^k_{2r} is the number of peanuts allocated to be received in the earlier time period by respondent i when the choice is made at $t = k$ for interest r in scenario Blue.
- $Secondvisit_k$: Dummy variable equal to 1 if the decision is taken at $t = 2$, 0 otherwise.

We will pool the observations from all our subsamples except "Child's participation (imposed)" to conduct this analysis. This would allow us to detect a 0.0886 s.d. difference in the share of peanuts allocated to the earlier time period at $t = 1$ and $t = 2$. For consistency with the rest of our analyses, we will also conduct the same regression in the "Probabilistic commitment device × Baseline" sample, which would allow us to detect a 0.1402 s.d. difference.

We will define a present-biased respondent as a respondent for whom $\bar{x^k}_{2i} > 0$ and will plot the joint distribution of the present-biased and parent/child-biased respondents.

3. Is there demand for commitment devices to help mitigate parent-bias, above and beyond demand for commitment devices that help mitigate present-bias?

Hypothesis 3a: Parents demand commitment devices to help them stick to their within-household allocation plans.

2,000 respondents in our sample are offered a probabilistic commitment device to help them stick to their planned within-household allocation. They are offered this probabilistic commitment device at 3 different prices: 0.5/1/1.5 packets of peanuts. We will plot the demand curve for this commitment device, at different prices.

Hypothesis 3b: The demand for commitment devices to help parents stick to their within-household allocation plans is smaller than the demand for commitment devices to help them stick to their inter-temporal allocations.

The respondents are also offered a probabilistic commitment device to help them stick to their inter-temporal allocation. We will compare the demand for both types of devices by testing $H_0 : \beta = 0$ in the following regression:

$$TookUp_{ci} = \alpha + \beta WithinHousehold_c + \gamma PresentedFirst_{ci} + \epsilon_{ci} \text{ Where:}$$

- $TookUp_{ci}$: Dummy variable equal to 1 if respondent i took up commitment device c, 0 otherwise;

- $WithinHousehold_c$: Dummy variable equal to 1 if the commitment device targets the within-household allocation, 0 if it targets the inter-temporal allocation;
- $PresentedFirst_{ci}$: Dummy variable equal to 1 if commitment device c was the first commitment device to be offered to the respondent, 0 otherwise.

We will pool the observations from all our "Probabilistic commitment devices" subsamples except "Child's participation (imposed)" to conduct this analysis. This would allow us to detect a 0.0991 s.d. difference between the take-up of both types of commitment devices. For consistency with the rest of our analyses, we will also conduct the same regression in the "Probabilistic commitment device × Baseline" sample, which would allow us to detect a 0.1402 s.d. difference.

4. Do parents demand to involve their children in future decisions as a commitment device?

Hypothesis 4: Parents demand to involve their children in future decisions as a commitment device.

400 respondents in our sample are offered the possibility to involve their child in the second round's decision to help them stick to their planned within-household allocation. They are offered this probabilistic commitment device at 4 different prices: free/0.5/1/1.5 packets of peanuts. We will plot the demand curve for this commitment device, at different prices.

5. Is the demand for commitment explained by parents' beliefs that they might be tempted to change their plans in the future?

We will rely on our incentivized measure of beliefs regarding others' behavior to study whether sophistication is driving the demand for different commitment devices.

Hypothesis 5a: Parents who are aware of their own time inconsistencies will have a higher demand for the probabilistic commitment device than parents who are not.

Testing this hypothesis is equivalent to testing $H_0 : \beta_0 = \beta_1 = \beta_2 = \beta_3 = 0$ in the following specification:

$$TookUp_{wi} = \alpha + \beta_0 BeliefParentBias_i + \beta_1 Price_i * BeliefParentBias_i + \beta_2 BeliefChildBias_i + \beta_1 Price_i * BeliefChildBias_i + \beta_4 Price_{ci} + \epsilon_i$$ Where:

- $TookUp_{wi}$: Dummy variable equal to 1 if respondent i took up the within-household probabilistic commitment device ;

- $BeliefParentBias_i$: Dummy variable equal to 1 if respondent i believes that most others will choose to allocate LESS peanuts to their children at $t = 2$ than they did today;
- $BeliefChildBias_i$:Dummy variable equal to 1 if respondent i believes that most others will choose to allocate MORE peanuts to their children at $t = 2$ than they did today;
- $Price_i$: price of the commitment device.

Hypothesis 5b: Parents who are aware of their own parent-bias will have a higher demand to involve their child in the $t = 2$ decision than parents who are not.

Testing this hypothesis is equivalent to testing $H_0 : \beta_0 = \beta_1 = 0$ in the following specification:

$$TookUp_{pi} = \alpha + \beta_0 BeliefParentBias_i + \beta_1 Price_{pi} * BeliefParentBias_i + \beta_2 Price_{pi} + \epsilon_i$$

Where:

- $TookUp_{pi}$: Dummy variable equal to 1 if respondent i chose to involve the child in the $t = 2$ decision;
- $BeliefParentBias_i$: Dummy variable equal to 1 if respondent i believes that most others will choose to allocate LESS peanuts to their children at $t = 2$ than they did today.

6. Can labeling mitigate time-inconsistencies?

Hypothesis 6: Reminding parents of their past choices will decrease time inconsistencies.

Pooling samples from the "Baseline" and "Labeling" treatment arms, we will measure whether labeling help mitigate time-inconsistencies, by testing the null hypothesis $H_0 : \beta = 0$ vs. $H_A : \beta < 0$ in the following econometric specification:

$$\Delta s_{2i} = \alpha + \beta * Labeling_i + \gamma * X_i + \epsilon_i$$

Where:

- $\Delta s_{2i} = s^2_{2i} - s^1_{2i}$: the difference between the share of peanuts allocated to be consumed by the child at $t = 2$ while making the decision at $t = 1$ and $t = 2$;
- $Labeling_i$: Dummy variable equal to 1 if the respondent is in the labeling treatment, 0 otherwise;

- X_i: demographic variables: gender and age of the respondent and of the child.

This sample size enables us to detect a 0.1717 standard deviation decrease in the change of the share of peanuts allocated to the child following the introduction of labeling.

We will also look at the impact of labeling on the prevalence of Parent-bias and Child-bias separately.

Distinguishing between the role of labeling and anchoring We will distinguish between the role played by labeling and anchoring, by testing the null hypothesis $H_0 : \beta = 0$ in the following econometric specification in the pooled "Labeling" and "Anchoring" samples:

$$\Delta^A s_{2i} = \alpha + \beta * Anchoring_i + \gamma * X_i + \epsilon_i$$

Where:

- $\Delta^A s^2_{2i} = s^2_{2i} - s^A_{2i}$: Value of the difference between the share of peanuts allocated to the child at $t = 2$ and in the allocation presented to the parents;
- $Anchoring_i$: Dummy variable equal to 1 if the respondent is in the anchoring treatment, 0 otherwise;
- X_i: as defined above.

This sample size enables us to detect a 0.1983 standard deviation difference in the distance between the amount of peanuts allocated to the child by the parents at $t = 2$ and in the allocation presented to them.

7. Can encouraging children to participate in household decisions increase investments in children and mitigate parent-bias?

Hypothesis 7a: Making children participate in household decisions increases investments in children We will test this hypothesis by pooling the "Baseline" and "Child's decision

(imposed)" samples and testing $H_0 : \beta = 0 vs. H_A : \beta > 0$ in the following econometric specification:

$$s^2_{2i} = \alpha + \beta_1 * ChildDecision_i + \gamma * X_i + \epsilon_i$$

Where:

- s^2_{2i}: share of peanuts respondents i allocated to be received by the child at $t = 2$ while making the decision at $t = 2$;

- X_i as defined above;
- $ChildDecision_i$: dummy variable equal to 1 if the respondents are allocated to the "Child's decision (imposed)" sample.

This sample size enables us to detect a 0.1717 standard deviation increase in the share of peanuts allocated to the child following the increase in child's bargaining power.

Hypothesis 7b: Making children participate in household decisions decreases reallocation towards parents

We will test this hypothesis by pooling the "Baseline" and "Child's decision (imposed)" samples and testing $H_0 : \beta = 0 vs. H_A : \beta < 0$ in the following econometric specification:

$$\Delta s_{2i} = \alpha + \beta_1 * ChildDecision_i + \gamma * X_i + \epsilon_i$$

Where:

- $\Delta s_{2i} = s_2^2 - s_2^1$, where Δs_2 is the difference between the share of peanuts allocated to the child in the earlier time period when the choice is made at $t = 1$ and $t = 2$;
- X_i as defined above;
- $ChildDecision_i$: dummy variable equal to 1 if the respondents are allocated to the "Child's decision (imposed)" sample.

This sample size enables us to detect a 0.1717 standard deviation decrease in the change in the share of peanuts allocated to the child following the increase in child's bargaining power.

We will also look at the impact of the child's bargaining power on the prevalence of Parent-bias and Child-bias separately.

8. Heterogeneity analysis: do mothers and fathers discount the future differently?

We will look at whether mothers and fathers differ in terms of investments in children on different dimensions:

1. Do mothers plan to invest more in their children in the future? We will test $H_0 : \beta = 0$ in the following specification:

$$s_{2i}^1 = \alpha + \beta * Mother_i + \epsilon_i \text{ Where:}$$

- s^1_{2i}, the share of peanuts to be consumed by respondent i's child at $t = 2$, when making the decision at $t = 1$;
- $Mother_i$: Dummy variable equal to 1 if the respondent is a mother, 0 otherwise.

We will test this hypothesis in our baseline subsample. Our sample size allows us to detect a 0.2949 s.d. difference in the share of peanuts mothers and fathers plan to allocate for their child's $t = 2$ consumption, when making the decisionplan at $t = 2$.

2. Do mothers invest more in the children when the time comes? We will test $H_0 : \beta = 0$ in the following specification:

$$s^2_{2i} = \alpha + \beta * Mother_i + \epsilon_i \text{ Where:}$$

- s^1_{2i}, the share of peanuts to be consumed by respondent i's child at $t = 2$, when making the decision at $t = 2$;
- $Mother_i$: Dummy variable equal to 1 if the respondent is a mother, 0 otherwise.

We will test this hypothesis in our baseline subsample. Our sample size allows us to detect a 0.2949 s.d. difference in the share of peanuts mothers and fathers plan to allocate for their chil's $t = 2$ consumption, when making the decision at $t = 2$.

3. Are fathers more time-inconsistent than mothers? We will test $\beta = 0$ in the following specification:

$$\Delta s_2 = \alpha + \beta * Mother_i + \epsilon_i \text{ Where:}$$

- $\Delta s_2 = s^2_2 - s^1_2$, where Δs_2 is the difference between the share of peanuts allocated to the child in the earlier time period when the choice is made at $t = 1$ and $t = 2$.

We will test this hypothesis in our baseline subsample. Our sample size allows us to detect a 0.2949 s.d. difference in the change in the share of peanuts mothers and fathers plan to allocate for their child's $t = 2$ consumption, when making the decision plan at $t = 2$ and $t = 1$.

4. Do mothers demand more commitment devices to stick to their within-household allocation plans? We will test $\beta = 0$ in the following specification:

$$TookUp_{wi} = \alpha + \beta_0 Mother_i + \beta_1 Price_{wi} * Mother_i + \beta_2 Price_{wi} + \epsilon_i$$

Where:

- $TookUp_{wi}$ is equal to 1 if the respondent took up a probabilistic commitment device, 0 otherwise.

We will test this hypothesis in our "probabilistic commitment devices" sub-samples. Our sample size allows us to detect a 0.2084 s.d. difference in the take-up of the probabilistic commitment device between mothers and fathers.

5. Do mothers demand to let their children participate in the $t = 2$ decision more? We will test $\beta = 0$ in the following specification:

$TookUp_{pi} = \alpha + \beta_0 Mother_i + \beta_1 Price_{pi} * Mother_i + \beta_2 Price_{pi} + \epsilon_i$ Where:

- $TookUp_{pi}$ is equal to 1 if the respondent took up the child participation commitment device, 0 otherwise.

We will test this hypothesis in our "Child's commitment (chosen)" subsample. Our sample size allows us to detect a 0.3243 s.d. difference in the willingness to let the child participate between mothers and fathers.

N.5.1 Relationship between investments in children and time inconsistencies

This experiment is conducted alongside a baseline survey which enables us to measure investments in children's health and education. In particular, we are interested in the correlation between traditional present-biases and parents-biases with the following indicators of investments in children:

- Index of investments in children's health based on the equally weighted average of z-scores of the following variables:
  - Mean expenses on preventative health-care for children aged 0-12 years old in the 4 weeks before the experiment,
  - Dummy equal to 1 if the child has been vaccinated during the measles and rubella immunization campaign in July 2017,
  - Dummy equal to 1 if the child was given any drug for intestinal worms in the 6 months before the experiment,
  - Dummy equal to 1 if the child was given Multiple Micronutrient powder in the 7 days before the experiment,
  - Dummy equal to 1 if the child was given iron supplements in the 7 days before the experiment,
  - Dummy equal to 1 if the child was given therapeutic food in the 7 days before the experiment,

- Dummy equal to 1 if the child was given supplementary food in the 7 days before the experiment,
  - Dummy equal to 1 if the child was given a vitamin A dose in the 3 months before the experiment,
  - Dummy equal to 1 if the child has been taken to a well-baby or under-5 clinic for a health check up in the 3 months before the experiment,
  - Dummy equal to 1 if the child has been taken to a well-baby or under-5 clinic for a growth check up in the 3 months before the experiment.

- Index of investments in children's education based on the equally weighted average of z-scores of the following variables:
  - Mean expenses on education for children aged 2-12 years old,
  - Attendance to Early Childhood Development Programmes for children under 6,
  - For children aged 6-18: numbers of days the child attended school in the month before the experiment.

We will look at the average of those two summary variables among present-biased and parent-biased parents.

N.6 Randomization and attrition balance

The variables that will be used in tests of randomization balance and survey attrition are:

1. Gender of the respondent;
2. Religion of the household;
3. Number of children in the household;
4. Household's credit constraints;
5. Age of the selected child;
6. Mean expenses on preventative health-care for children aged 0-12 years old in the 4 weeks before the experiment;
7. An index of investments in children's health (see section N.5.1);
8. An index of investments in children's education;
9. Share of peanuts allocated to children at $t = 2$ in section blue ($t = 1$ decision).

N.6.1 Addressing attrition

We define attrition, $Attrition_i$, as the fact that a respondent is surveyed at $t = 1$, but not at $t = 2$ or $t = 3$. In the case of survey attrition, all the analyses described in section N.5 will be restricted to respondents that we observe in all three visits.

We will check for differential attrition using the variables listed above. All our attrition analyses exclude respondents from the "No first visit" treatment arm. Following Gerber & Green (2012), we want to check whether missingness is independent of potential outcomes (MIPO). In that case, our estimates would be unbiased, but our power would be lower. We will investigate whether MIPO holds, conditional on variables 1 to 10 listed above, testing the null hypothesis $H_0 : \beta = 0$ in the following equation:

$$Y_{i1} = \alpha + \beta * Attrition_i + \gamma * X_{i1} + \epsilon_i$$

Where:

- Y_{i1}: baseline outcome variable:
  - Quantity of peanuts choosen to be received at $t = 2$ in section A ($t = 1$ decision);
  - Quantity of peanuts choosen to be received at $t = 2$ in section B ($t = 1$ decision);
  - Share of peanuts allocated to parents at $t = 2$ in section C ($t = 1$ decision).
- X_{i1}: vector of respondent's characteristics: variables 1-10 listed above.

To evaluate whether the magnitude of attrition differs according to treatment arms, we test the null hypothesis : $H_0 : \beta_1 = ... = \beta_4 = 0$ in the following econometric specification:

$$Attrition_i = \alpha + \sum_{j=1}^{4} \beta_j * Treatment_j + \epsilon_i$$

Where:

- $Treatment_1$= 1 if the respondent is allocated to treatment arm " Child's participation (chosen)", 0 otherwise;
- $Treatment_2$= 1 if the respondent is allocated to treatment arm "Probabilistic × Baseline", 0 otherwise;
- $Treatment_3$= 1 if the respondent is allocated to treatment arm "Probabilistic × Labeling", 0 otherwise;
- $Treatment_4$= 1 if the respondent is allocated to treatment arm "Probabilistic × Random anchoring", 0 otherwise;

Finally, we will test whether attrited households had different baseline characteristics in different treatment groups. We will test the null hypothesis: $H_0 : \beta_1 = ... = \beta_4 = 0$ in the following econometric specification among attrited households :

$$Y_i|_{Attrition_i=1} = \alpha + \sum_{j=1}^{4} \beta_j * Treatment_j + \epsilon_i$$

If we find that attrition is non-negligible, we will use bounds following the methodology described in Lee (2009) in our assessment of the impact of labeling and all the regressions described above.

O Additional pre-analysis plan regressions

Some of the regressions that were in the original pre-analysis plan were not included in the main text of this paper, mostly because we realized that they did not adequately capture the dynamics of AGD and parent-bias reversals. Here are those regression results.

Table O.1: Testing hypothesis 1a

	(1) s^1_{ji}
$Thirdvisit_j$	0.0326***
	(0.00322)
Mean	0.502
N	4012

Notes: Standard errors clustered at the household level in parentheses, * $p < 0.1$, ** $p < .05$, *** $p < .01$. Sample: full sample, excluding child participation (imposed) treatment arm.

P Pre-analysis plan - Follow-up experiment

This pre-analysis plan was registered at the AEA RCT registry under AEARCTR-0004386 and is available at http://www.socialscienceregistry.org/trials/4386.

P.1 Abstract and Introduction

This document is an update to the study pre-registered with the AEA RCT Registry with ID AEARCTR-0003535. In a new wave of data collection who will start in June 2019, we will conduct a series of experimental games to: 1) assess whether parent-bias remains widespread in our sample when we allow respondents to chose an equal split of resources, 2) measure the correlation between our experimental measure of parent-bias and the take-up of commitment devices when making investment decisions, 3) contrast willingness-to-pay for commitment to stick to one's plans to invest in one's child and in another person's child and 4) estimate whether parent-biased respondents have a higher willingness-to-pay to open a bank account in their child's name. The present document outlines the experimental design and the econometric methods we will use to assess those three points.

We present here the design of experimental games to be included in an additional wave of data collection for our lab-in-the field experiment previously registered with ID: AEARCTR-0003535. Those games have been designed to test the following hypotheses:

1. Does the prevalence of parent-bias shrinks when we allow for an equal split of resources?

2. Do parent-biased respondents have a higher willingness-to-pay to stick to their plans to invest in their children?

3. Do respondents demand less commitment devices to stick to their plans to invest in someone else's child than in their own?

Table O.2: Testing hypothesis 2a

	(1)
	s^k_{2i}
Second Decision	0.0207***
	(0.00386)
Mean	0.496
N	1616

Notes: Standard errors clustered at the household level in parentheses, * $p < 0.1$, ** $p < .05$, *** $p < .01$. Sample: baseline sample and probabilistic commitment device. Note: This is not the specification we ended up settling on to study the relationship between AGD and preference reversals, because it does not differentiate between the second round behavior of AGD and non-AGD parents and does not allow us to identify whether AGD drives preference reversals away from the child's consumption.

Table O.3: Testing hypothesis 2b

	(1)
	$\Delta^k s_i$
Second Decision	-0.00357
	(0.00615)
Mean	0.0288
N	1616

Notes: Standard errors clustered at the household level in parentheses, * $p < 0.1$, ** $p < .05$, *** $p < .01$. Sample: baseline sample and probabilistic commitment device.

Table O.4: Testing hypothesis 3b

	(1)
	Took up the commitment device
Within-household	-0.00609
	(0.00583)
Presented first	-0.00915
	(0.00583)
Mean	0.906
N	4064

Notes: Standard errors clustered at the household level in parentheses, * $p < 0.1$, ** $p < .05$, *** $p < .01$. Sample: probabilistic commitment device. Note: This is not the specification we ended up settling on , because it does not allow us to identify whether AGD drives the demand for commitment.

Table O.5: Testing hypothesis 6

	(1)	(2)
	Δs_{2i}	
Labeling	0.00975	
	(0.00698)	
Anchoring		0.0111
		(0.00709)
Mean	0.0258	0.0263
N	1194	1192

Notes: Standard errors in parentheses, * $p < 0.1$, ** $p < .05$, *** $p < .01$. Sample: column (1): Labeling and Baseline, column (2): Anchoring and Baseline. Control variables: age and gender of the respondent and the child.

Table O.6: Testing hypotheses 7a and 7b

	(1) s_{2i}^2	(2) Δs_{2i}	(3) Parent-biased reversals	(4) Child-biased reversals
Child Participation	-0.00551	0.00300	0.0493**	0.0827***
(Imposed)	(0.00694)	(0.00736)	(0.0243)	(0.0284)
Mean	0.505	0.0237	0.194	0.310
N	1192	1192	1192	1192

Notes: Standard errors in parentheses, * $p < 0.1$, ** $p < .05$, *** $p < .01$. Sample: Child Participation (Imposed) and Baseline.

Table O.7: Testing the influence of the parent's gender on decisions

	(1) s_{2i}^1	(2) s_{2i}^2	(3) Δs_{2i}	(4) Probabilistic commitment	(5) Child Participation
Mother	-0.0224***	-0.0168**	0.00364	0.00426	0.291**
	(0.00793)	(0.00819)	(0.00865)	(0.0610)	(0.126)
Price				0.0145	0.269**
				(0.0541)	(0.118)
Price × Mother				-0.0586	-0.278**
				(0.0567)	(0.128)
Mean	0.484	0.509	0.0257	0.903	0.588
N	2411	2364	2364	2032	379

Notes: Standard errors in parentheses, * $p < 0.1$, ** $p < .05$, *** $p < .01$. Sample: column (4): probabilistic commitment device. Column (5) Child Participation (Chosen)

4. Do parent-biased respondents have a higher willingness-to-pay to open a savings' account in their child's name rather than their own?

P.2 Sample selection

This follow-up to our initial experiment will be conducted with mothers from the same sample of households as our initial experiment (AEA RCT Registry with ID AEARCTR-0003535). The final sample size is 2,411 households from 80 villages of Salima district in Malawi.

P.3 Design of the experimental games

P.3.1 Willingness-to-pay to commit to investments in children

Within this sample, if there is more than one primary-school age child in the household, we randomly select the child who will participate in this part of the experiment. If there is no primary-school age child in the household, those questions are asked hypothetically.

We start by telling respondents that they are entering a lottery in which they can earn 0 or 2,000 kwachas, that they will receive on September 1st, approximately 2 months after the interview. They only learn the outcome of the lottery at the end of the interview. They have the possibility to either receive the lottery price in cash card or to purchase one week (1 hour/day for a week) of tutoring for their child.

We give the parents the possibility to commit to this decision. They are given the choice between having or giving up the possibility to make this choice again just before receiving the money/the tutoring. The flexible option comes accompanied with a bonus. The participants make this decision for different bonus values. At the end of the survey, the participants learn which bonus has been randomly picked and their decision for that amount is executed. This design is a version of the Becker-DeGroot-Marschak mechanism (Becker et al., 1964) and ensures that all questions are incentive-compatible.

We measure the parents' willingness-to-pay for investments in children through a series of three to four interdependent binary choices between receiving money or the investment in the child, following a "staircase" procedure (Cornsweet, 1962). The sequence of interdependent questions we ask and the inputed willingness-to-pay for commitment is shown in Figure P.1

Attaching a bonus to the flexible option may be signalling to the parents what is the "right decision". In the spirit of Carrera et al. (2019), we also ask the respondent to chose between the flexible or commitment option with a positive bonus attached to the commitment option.

P.3.2 Willingness-to-pay to commit to investments in someone else's child

The respondents enter another lottery in which they can earn 0 or 2000 kwachas, that they would receive on September 1st. They are informed that they can chose between receiving that money in cash cards or to instead offer a week of tutoring to another person's child. They are informed that they will not know who this other child is andd that neither the beneficiary child nor her family would be informed of the respondent's identity, irrespective of their choice.

They are then given the choice between having or giving up the possibility to make this choice again just before receiving the money/the tutoring. The flexible option comes accompanied with a bonus. The participants make this decision for different bonus values. At the end of the survey, the participants learn which bonus has been randomly picked and their decision for that amount is executed. The sequence of interdependent questions we ask and the inputed willingness-to-pay for commitment according to Figure P.1

P.3.3 Willingness-to-pay to open a bank account in the child's name

The respondents enter a lottery in which they can earn 0 or 10,000 kwachas. Before learning the lottery outcome, they can choose between 2 options: 1- Receiving the whole money in cash; 2- Opening a savings account at the National bank in their child's name and depositing 5,000 kwachas. Our team will accompany the respondent and the child at the bank and help them with the paper work. The respondent will receive the remaining money in cash.

The respondent are asked this question, with a different "price" associated with each option. If the respondent earns 10,000 kwachas in the lottery, a price will be randomly chosen at the end of the interview and the respondent's decision at that price will be executed.

We measure the parents' willingness-to-pay for the savings' account through a series of three interdependent binary choices. The sequence of interdependent questions we ask and the inputed willingness-to-pay for the savings device is shown in Figure P.2. We also elicit the parents' willingness-to-pay to receive the whole money in cash.

Finally, we elicit the parents' relative willingness-to-pay to open a bank account in their name or in their child's name, following the same procedure.

P.4 Eliciting time-preferences

P.4.1 Parent-bias

Baseline measure:

We define parent-biased respondents as respondents who discount their own consumption to a larger extent than that of their children. We re-elicit parent-bias in this wave of data collection.

To do so, we ask parents to allocate five packs of peanuts between themselves and their child to be consumed two days later ($t = 2$) and a month later ($t = 3$). To help with this decision, the parents are invited to share 5 packets of peanuts between two plates, one entitled "you, in two days", the other one "Your child in two days". The enumerator records this decision. Then the parents are invited to do the same thing for the next allocation.

To ensure that all decisions are consequential, the parents are informed that a randomly drawn subset of the respondents will see their decision implemented.

Let s_2 be the share of peanuts that respondents allocate to be consumed by their child at $t = 2$ and s_3 the share of peanuts that respondents allocated to be consumed by their child at $t = 3$. We define parent-biased respondents as those deciding to allocate a larger share of peanuts to their child at $t = 3$ than $t = 2$: $\mathbb{1}\{\delta_c > \delta_a\} \Leftrightarrow s_2 < s_3$.

Parent-bias when allowing for an equalitarian split: We will study how the distribution of parent-bias changes when we allow respondents to chose an equalitarian split.

To do so, we ask parents to allocate five packs of peanuts between themselves and their child to be consumed two days later ($t = 2$) and a month later ($t = 3$), but we allow them to allocate half packets, so that they can choose a 2.5/2.5 allocation if needed. We still define parent-biased respondents as those deciding to allocate a larger share of peanuts to their child at $t = 3$ than $t = 2$

P.4.2 Respondent's discount factor towards their own consumption δ_a

We re-elicit δ_a in this wave of data collection, following the same methodology that we used at baseline, with a traditional inter-temporal decision task. The respondents split the consumption of three packages of peanuts, for their own consumption, between $t = 2$ and $t = 3$. For each package not consumed at $t = 2$, they received r additional packages at $t = 3$. The respondents were asked to make this decision for three interest rates: $r \in \{0.5, 1, 1.5\}$

The respondents' utility maximization problem is given by:

$$\underset{(x_t)_{t=2,3}}{\text{Max}} \ \beta\delta_a u(x_2^1) + \beta\delta_a^2 u(x_3^1)$$
$$\text{s.t.}$$
$$\begin{cases} x_2 + s_2 \leq y_2 \\ x_3 \leq (1+r)s_2 - c \\ y_2 = y \end{cases}$$

The solution to the respondents' maximization problem is given by: $u'(x_2^1)(1 + r) = \delta_a u'(x_3^1)$ Therefore, $\delta_a = \frac{u'(x_2^1)(1+r)}{u'(x_3^1)}$

To ensure that δ_a is defined even when the respondent choose to receive zero at $t = 2$, we assume the following utility function: $u(x_t) = log(x_t + 1)$. Hence, $\delta_a = \frac{(y-x_2^1)(1+r)+1}{(x_2^1+1)(1+r)}$

The respondents have to decide how to allocate consumption between periods two and three for three interest rates: $r \in \{0.5, 1, 1.5\}$. For each interest rate, we impute the value of δ_a associated with the respondents' decision. We use their average as the value of δ_a in our analysis.

P.5 Empirical analysis

1. Is parent-bias less frequent when we allow for an equalitarian split of the resources?

We test $H_0 : \gamma_0 = 0$ in the following regression:

$$\mathbb{1}\{\delta_{c,ik} > \delta_{a,ik}\} = \alpha + \gamma_0 \mathbb{1}\{Equalsplit\}_k + \lambda X_i + \epsilon_{ik} \tag{P.1}$$

Where:

- $\mathbb{1}\{\delta_c > \delta_a\} = 1$: if respondent i displays parent-biased time-preferences in decision k;
- $\mathbb{1}\{Equalsplit\}_k = 1$ if the task allows for the equal split of the resources between the parent and the child;
- X_i: Vector of individual characteristics: age and gender of the respondent and the child, measure of credit constraints, religion of the household, order in which the scenarios are presented to the respondents, number of children, education level of the respondent;
- ϵ_{ik}: Standard error clustered at the individual level.

This will enable us to detect a 0.0807 standard deviation difference in the prevalence of parent-bias once we allow respondents to chose an equal split.

2. Do parent-biased respondents have a higher willingness-to-pay to commit to investments in their children?

We test $H_0 : \gamma_0 = 0$ in the following regression equation:

$$WTP_i = \alpha + \gamma_0 \mathbb{1}\{\delta_{c,i} > \delta_{a,i}\} + \lambda X_i + \epsilon_i \tag{P.2}$$

Where:

- WTP_i: i's willingness-to-pay to commit to investments in the child;
- X_i: Vector of individual characteristics: age and gender of the respondent and the child, measure of credit constraints, religion of the household, order in which the scenarios are presented to the respondents, number of children, dummy variable is the question is hypothetical, education level of the respondent;
- $\mathbb{1}\{\delta_c > \delta_a\} = 1$: if respondent i is parent-biased, in our baseline measure;
- ϵ_i: Standard error.

We restrict our sample to those respondents who had say that they would take up the one week of tutoring for their child.

Assuming that 80% of the respondents in our sample take-up the tutoring for their children and that 30% of our sample is parent-biased (in line with our baseline measure without accounting for the price of commitment), this will enable us to detect a 0.1393 standard deviation difference in the willingness-to-pay for commitment to investments in children between parent-biased and non-parent-biased respondents.

We will also account for the fact that the effect of parent-bias can be mitigated by different levels of δ_a, with the following equation:

$$WTP_i = \alpha + \gamma_0 \mathbb{1}\{\theta < 1\} + \gamma_1 \delta_{a,i} + \gamma_2 \mathbb{1}\{\theta < 1\} \times \delta_{a,i} + \lambda X_i + \epsilon_i \tag{P.3}$$

Where $\delta_{a,i}$ is our experimental measure of the respondent's discount factor as elicited in

Scenario A.

Robustness checks

We will conduct a series of checks to ensure the consistency of our results:

- Excluding parents without primary-age school children who had just been asked the question hypothetically,
- Excluding parents who displayed a positive willingness-to-pay for both commitment and flexibility,
- Using our measure of parent-bias when allowing for an equalitarian split.

3. Do respondents have a lower willingness-to-pay to commit to investments in other people's children than their own?

We stack the parents' WTP to commit in their child and another person's child, and restrict our sample to those parents that chose to take up the tutoring for both.

We test $H_0 : \gamma_0 = 0$ in the following regression equation:

$$WTP_{ik} = \alpha + \gamma_0 \mathbb{1}\{Other\}_k + \gamma_1 \mathbb{1}\{\delta_{c,i} > \delta_{a,i}\} + \gamma_2 \mathbb{1}\{Other\}_i \times \mathbb{1}\{\delta_{c,i} > \delta_{a,i}\} + \lambda X_{ik} + \epsilon_{ik} \tag{P.4}$$

Where:

- WTP_{ik}: i's willingness-to-pay to commit to investment in child k;
- $\mathbb{1}\{Other\}_k = 1$ if child k is another person's child;
- ϵ_i: Standard error clustered at the individual level.

Assuming that 50% of the respondents in our sample take-up the tutoring for their children and that 30% of our sample is parent-biased (in line with our baseline measure without accounting for the price of commitment), this will enable us to detect a 0.1144 standard deviation difference in the willingness-to-pay for commitment to investments in someone else's child, and a 0.1548 standard deviation difference for parent-biased respondents.

Robustness checks

We will conduct a series of checks to ensure the consistency of our results:

- Excluding parents without primary-age school children who had just been asked the question hypothetically,
- Excluding parents who displayed a positive willingness-to-pay for both commitment and flexibility in either scenario,
- Using our measure of parent-bias when allowing for an equalitarian split.

4. Do parent-biased respondents have a higher willingness-to-pay to open a bank account in their child's name

We test $H_0 : \gamma_0 = 0$ in the following regression equation:

$$WTP_i = \alpha + \gamma_0 \mathbb{1}\{\delta_{c,i} > \delta_{a,i}\} + \lambda X_{ik} + \epsilon_{ik} \tag{P.5}$$

We will run this regression with two versions of the outcome variable:

- WTP to open a bank account in the child's name;
- WTP to open a bank account in the child's name instead of a bank account in the respondent's name.

Assuming that 30% of our sample is parent-biased, this will enable us to detect a 0.1246 standard deviation difference in the willingness-to-pay.

We will also account for the fact that the effect of parent-bias can be mitigated by different levels of δ_a, with the following equation:

$$WTP_i = \alpha + \gamma_0 \mathbb{1}\{\delta_{c,i} > \delta_{a,i}\} + \gamma_1 \delta_{a,i} + \gamma_2 \mathbb{1}\{\delta_{c,i} > \delta_{a,i}\} \times \delta_{a,i} + \lambda X_i + \epsilon_i \tag{P.6}$$

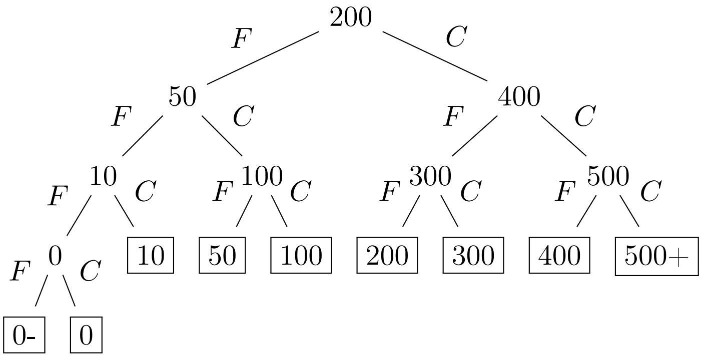
Robustness checks

We will conduct a series of checks to ensure the consistency of our results:

- Excluding parents who displayed a positive willingness-to-pay for both options,
- Using our measure of parent-bias when allowing for an equalitarian split.

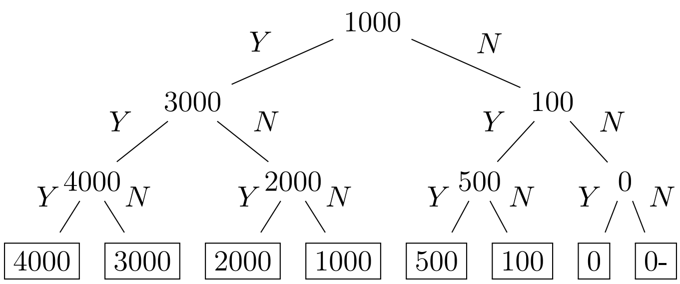
P.6 Figures

Figure P.1: Tree price: willingness-to-pay for commitment "Would you commit or choose the flexible option if the flexible option came with a bonus of X?"



Notes: F-branches correspond to parents choosing the flexible option. C-branches correspond to parents choosing commitment. The framed value at the end of the decision tree represents the willingness-to-pay for commitment implied by the respondents' sequential decisions.

Figure P.2: Tree price: willingness-to-pay for a savings' account "Would you open a savings' account in your child's name if it cost X MK?"



Notes: Y-branches correspond to parents choosing the savings account. N-branches correspond to parents choosing to receive the whole money in cash cards. The framed value at the end of the decision tree represents the willingness-to-pay for the savings account implied by the respondents' sequential decisions.

Q Additional follow-up pre-analysis plan regressions

Table Q.1: AGD when allowing for equal split

	(1)
	$\mathbb{1}\{\theta < 1\}$
Allowed for equal split	-0.0561***
	(0.0102)
Mean	0.208
N	3753

Standard errors in parentheses
* $p < 0.1$, ** $p < .05$, *** $p < .01$

Table Q.2: AGD and tutoring for one's child - excluding hypothetical questions

	(1)	(2)	(3)	(4)	(5)	(6)
	Chooses tutoring		Commitment			
			Never commits		WTP	
$\mathbb{1}\{\theta < 1\}$	-0.0520***	-0.0168	0.0130	-0.0194	-61.76***	-53.09
	(0.0194)	(0.0415)	(0.0334)	(0.0723)	(21.77)	(46.98)
$\delta\theta \times \mathbb{1}\{\theta < 1\}$		-0.0177		0.0155		-3.991
		(0.0180)		(0.0311)		(20.40)
$\delta\theta$		0.0144*		-0.00352		13.45
		(0.00840)		(0.0140)		(9.102)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
$p - value$ joint significance		0.2004		0.9333		0.0609 **
Mean	0.840	0.841	0.448	0.448	261.1	260.7
N	1965	1963	1355	1354	748	747

Notes: This looks at the impact of AGD on parents' willingness to purchase tutoring for their child and to commit to that decision. This excludes households for which that question was asked hypothetically because there is no school-age children. In columns (1)-(2), the outcome variable is a dummy equal to 1 if the parent decided to receive tutoring for their children instead of 2,000 kwachas in cash if they earned it in the lottery.In columns (3)-(4), the outcome variable is a dummy equal to 1 if the respondent never chooses to commit to her decision even at price 0. In columns (5)-(6), the outcome variable is the WTP to commit. In columns (3)-(6), the sample is restricted to respondents having taken up the tutoring for their child and who do not exhibit a positive (or negative) willingness-to-pay for both the flexible option and the commitment. In columns (5)-(6), the sample is further restricted to respondents for which there is at least one non-negative price at which they choose to commit. $\delta\theta$ is an experimental measure of the discount factor parents attach to their own consumption. Its imputation is described in Appendix **??**. In this table, we use the measures of $\delta\theta$ and $\mathbb{1}\{\theta < 1\}$ elicited in the same wave of data collection as the outcome variables. $p - value$ test of joint significance of the coefficients on $\delta\theta \times \mathbb{1}\{\theta < 1\}$ and $\mathbb{1}\{\theta < 1\}$. Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the religion of the household, the level of education of the respondent. Standard errors in parentheses. * $p < 0.1$, ** $p < .05$, *** $p < .01$

Table Q.3: AGD and tutoring for one's child when allowing for equalitarian split

	(1)	(2)	(3)	(4)	(5)	(6)
	Chooses tutoring		Commitment			
			Never commits		WTP	
$\mathbb{1}\{\theta < 1\}$	-0.0297	-0.0128	0.0692	0.0649	-81.80***	-122.6**
	(0.0251)	(0.0542)	(0.0423)	(0.0917)	(28.52)	(61.22)
$\delta\theta \times \mathbb{1}\{\theta < 1\}$		-0.00852		0.00182		20.15
		(0.0235)		(0.0396)		(26.53)
$\delta\theta \times \mathbb{1}\{\theta < 1\}$		0.0120		0.00617		6.766
		(0.00940)		(0.0157)		(10.13)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
$p - value$ joint significance		0.5443		0.2626		0.0102 ***
Mean	0.841	0.841	0.448	0.448	267.9	267.5
N	1464	1462	1012	1011	559	558

Notes: This looks at the impact of AGD on parents' willingness to purchase tutoring for their child and to commit to that decision. $\mathbb{1}\{\theta < 1\}$ is equal to 1 if the parents exhibited AGD when making the decision for in a setting allowing for equalitarian split. In columns (1)-(2), the outcome variable is a dummy equal to 1 if the parent decided to receive tutoring for their children instead of 2,000 kwachas in cash if they earned it in the lottery. columns (3)-(4), the outcome variable is a dummy equal to 1 if the respondent never chooses to commit to her decision even at price 0. In columns (5)-(6), the outcome variable is the WTP to commit. In columns (3)-(6), the sample is restricted to respondents having taken up the tutoring for their child and who do not exhibit a positive (or negative) willingness-to-pay for both the flexible option and the commitment. In columns (5)-(6), the sample is further restricted to respondents for which there is at least one non-negative price at which they choose to commit. $\delta\theta$ is an experimental measure of the discount factor parents attach to their own consumption. Its imputation is described in Appendix **??**. In this table, we use the measures of $\delta\theta$ and $\mathbb{1}\{\theta < 1\}$ elicited in the same wave of data collection as the outcome variables. $p - value$ test of joint significance of the coefficients on $\delta\theta \times \mathbb{1}\{\theta < 1\}$ and $\mathbb{1}\{\theta < 1\}$. Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the religion of the household, the level of education of the respondent, a dummy equal to one if the question was asked hypothetically. Standard errors in parentheses. * $p < 0.1$, ** $p < .05$, *** $p < .01$

Table Q.4: AGD and tutoring for one's child without excluding respondents with contradictory WTP

	(1)	(2)	(3)	(4)
	Commitment			
	Never commits		WTP	
$\mathbb{1}\{\theta < 1\}$	-0.00625	-0.0296	-51.99***	-22.50
	(0.0284)	(0.0612)	(17.01)	(36.72)
$\delta\theta \times \mathbb{1}\{\theta < 1\}$		0.0113		-14.22
		(0.0264)		(15.97)
$\delta\theta \times \mathbb{1}\{\theta < 1\}$		-0.00409		10.57
		(0.0121)		(7.226)
Control variables	Yes	Yes	Yes	Yes
$p - value$ joint significance		0.6439		0.1224
Mean	0.368	0.368	245.6	245.4
N	1695	1694	1071	1070

Notes: This looks at the impact of AGD on parents' willingness to commit to their decision to purchase tutoring, without excluding respondents that exhibit a positive (or negative) willingness-to-pay for both the flexible option and the commitment . $\mathbb{1}\{\theta < 1\}$ is equal to 1 if the parents exhibited AGD when making the decision for in a setting allowing for equalitarian split. In columns (1)-(2), the outcome variable is a dummy equal to 1 if the respondent never chooses to commit to her decision even at price 0. In columns (3)-(4), the outcome variable is the WTP to commit. The sample is restricted to respondents having taken up the tutoring for their child. In columns (3)-(4), the sample is further restricted to respondents for which there is at least one non-negative price at which they choose to commit. $\delta\theta$ is an experimental measure of the discount factor parents attach to their own consumption. Its imputation is described in Appendix **??**. In this table, we use the measures of $\delta\theta$ and $\mathbb{1}\{\theta < 1\}$ elicited in the same wave of data collection as the outcome variables. $p - value$ test of joint significance of the coefficients on $\delta\theta \times \mathbb{1}\{\theta < 1\}$ and $\mathbb{1}\{\theta < 1\}$. Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the religion of the household, the level of education of the respondent, a dummy equal to one if the question was asked hypothetically. Standard errors in parentheses. * $p < 0.1$, ** $p < .05$, *** $p < .01$

Table Q.5: AGD and tutoring for another child, excluding hypothetical questions

	(1)	(2)	(3)
	Chooses tutoring	Commitment	
	Never commits	WTP	
$\mathbb{1}\{Other\}$	-0.236***	-0.0111	-325.6***
	(0.0133)	(0.0166)	(13.65)
$\mathbb{1}\{Other\} \times \mathbb{1}\{\theta < 1\}$	0.0564*	0.0111	48.47
	(0.0294)	(0.0393)	(31.72)
$\mathbb{1}\{\theta < 1\}$	-0.0521**	0.00450	-50.65
	(0.0208)	(0.0445)	(31.22)
Control variables	Yes	Yes	Yes
Mean	0.729	0.444	158.0
N	3931	1576	738

Notes: This looks at the impact of AGD on parents' willingness to purchase tutoring for a child other than their own and to commit to that decision. In column (1), the outcome variable is a dummy equal to 1 if the parent decided to provide tutoring instead of 2,000 kwachas in cash if they earned it in the lottery. In column (2), the outcome variable is a dummy equal to 1 if the respondent never chooses to commit to her decision even at price 0. In column (3), the outcome variable is the WTP to commit. In columns (2)-(3), the sample is restricted to respondents having taken up the tutoring and who do not exhibit a positive (or negative) willingness-to-pay for both the flexible option and the commitment. In column (3), the sample is further restricted to respondents for which there is at least one non-negative price at which they choose to commit. $\mathbb{1}\{Other\}$ is a dummy equal to one if the target child is not the respondent's child. Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the religion of the household, the level of education of the respondent, a dummy equal to one if the question was asked hypothetically. Standard errors are clustered at the household level. * $p < 0.1$, ** $p < .05$, *** $p < .01$

Table Q.6: AGD and tutoring for another child when allowing for equalitarian split

	(1)	(2)	(3)	(4)	(5)	(6)
	Chooses tutoring		Commitment			
			Never commits		WTP	
$\mathbb{1}\{Other\}$		-0.224***		-0.00404		-339.1***
		(0.0148)		(0.0189)		(14.96)
$\mathbb{1}\{Other\} \times \mathbb{1}\{\theta < 1\}$		0.0407		0.00404		98.30**
		(0.0369)		(0.0514)		(42.31)
$\mathbb{1}\{\theta < 1\}$	0.00885	-0.0307	0.0602	0.0832	-35.10	-91.43**
	(0.0331)	(0.0263)	(0.0482)	(0.0573)	(33.02)	(42.55)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Mean	0.618	0.732	0.426	0.439	271.5	162.8
N	2015	2928	739	1188	424	558

Notes: This looks at the impact of AGD on parents' willingness to purchase tutoring for a child other than their own and to commit to that decision. AGD is determined based on the experimental game in which respondents are allowed to chose and equalitarian split. In columns (1)-(2), the outcome variable is a dummy equal to 1 if the parent decided to provide tutoring instead of 2,000 kwachas in cash if they earned it in the lottery.

In columns (3)-(4), the outcome variable is a dummy equal to 1 if the respondent never chooses to commit to her decision even at price 0. In columns (5)-(6), the outcome variable is the WTP to commit. In columns (3)-(6), the sample is restricted to respondents having taken up the tutoring and who do not exhibit a positive (or negative) willingness-to-pay for both the flexible option and the commitment. In columns (5)-(6), the sample is further restricted to respondents for which there is at least one non-negative price at which they choose to commit. In columns (1), (3) and (5) the question is restricted to the parents' decision for a child other than their own. In columns (2), (4) and (6), the parents' decisions for their child and the other child are stacked and standard errors are clustered at the household level. $\mathbb{1}\{Other\}$ is a dummy equal to one if the target child is not the respondent's child. Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the religion of the household, the level of education of the respondent, a dummy equal to one if the question was asked hypothetically. Standard errors in parentheses. * $p < 0.1$, ** $p < .05$, *** $p < .01$

Table Q.7: AGD and tutoring for another child without excluding contradictory WTP

	(1)	(2)	(3)	(4)
	Commitment			
	Never commits		WTP	
$\mathbb{1}\{\theta < 1\}$	-0.00310	-0.0131	-25.78	-43.35*
	(0.0318)	(0.0351)	(18.88)	(22.63)
$\mathbb{1}\{Other\}$		-0.0161		-294.9***
		(0.0151)		(10.75)
$\mathbb{1}\{Other\} \times \mathbb{1}\{\theta < 1\}$		-0.000179		43.09*
		(0.0347)		(22.99)
Control variables	Yes	Yes	Yes	Yes
Mean	0.340	0.354	262.0	142.8
N	1246	2224	822	1208

Notes: This looks at the impact of AGD on parents' willingness to purchase tutoring for a child other than their own and to commit to that decision. The sample does not exclude respondents who exhibit a positive (or negative) willingness-to-pay for both the flexible option and the commitment. In columns (5)-(6), the sample is further restricted to respondents for which there is at least one non-negative price at which they choose to commit.

Table Q.8: AGD and willingness-to-pay for a savings account - allowing for equalitarian split

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Cash vs. Child's savings account				Own vs. Child's savings account			
	Always chooses cash		WTP for child's savings account		Always chooses own savings account		WTP for child's savings account	
$\mathbb{1}\{\theta < 1\}$	-0.01000	-0.00557	-21.06	-92.90	-0.0993***	-0.0588	220.8	735.8
	(0.0195)	(0.0419)	(71.39)	(154.8)	(0.0239)	(0.0520)	(227.0)	(496.5)
$\delta\theta \times \mathbb{1}\{\theta < 1\}$		-0.00221		36.13		-0.0195		-249.2
		(0.0182)		(67.62)		(0.0224)		(211.7)
$\delta\theta$		0.000429		33.34		0.00511		155.5
		(0.00704)		(26.02)		(0.00886)		(119.6)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean	0.0757	0.0758	3679.1	3678.9	0.879	0.878	698.1	698.1
N	1347	1346	1245	1244	1284	1282	156	156

Notes: This looks at the impact of AGD on parents' willingness to pay to open a savings account for their child. AGD is computed using the experimental game allowing for equalitarian split. In columns (1)-(2), the outcome variable is a dummy equal to 1 if the parent always chooses to receive cash instead of opening a bank account in their child's name, even when this option is free. In columns (3)-(4), the outcome variable is the willingness-to-pay for a savings account in the child's name among parents for which there exists a non-negative price at which they would take up the savings account. In columns (5)-(6), the outcome variable is a dummy equal to 1 if the parent always chooses to open a savings account in their name instead of their child's name, even when the later option is free. In columns (7)-(8), the outcome variable is the willingness-to-pay for a savings account in the child's name instead of the parent's name among parents for which there exists a non-negative price at which they would take up the savings account in their child's name. All the regressions exclude respondents who declared a strictly negative (or positive) willingness-to-pay for each of options offered. $\delta\theta$ is an experimental measure of the discount factor parents attach to their own consumption. Its imputation is described in Appendix **??**. In this table, we use the measures of $\delta\theta$ and $\mathbb{1}\{\theta < 1\}$ elicited in the same wave of data collection as the outcome variables. Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the religion of the household, the level of education of the respondent. Standard errors in parentheses. * $p < 0.1$, ** $p < .05$, *** $p < .01$

Table Q.9: AGD and willingness-to-pay for a savings account - without excluding contradictory WTP

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Cash vs. Child's savings account				Own vs. Child's savings account			
	Always chooses cash		WTP for child's savings account		Always chooses own savings account		WTP for child's savings account	
$\mathbb{1}\{\theta < 1\}$	-0.0242*	-0.0387	-92.22	-2.985	-0.0333	-0.0305	-489.1***	-168.9
	(0.0138)	(0.0295)	(58.99)	(126.7)	(0.0223)	(0.0477)	(181.6)	(382.1)
$\delta\theta \times \mathbb{1}\{\theta < 1\}$		0.00707		-45.05		-0.00142		-165.8
		(0.0127)		(54.94)		(0.0206)		(173.8)
$\delta\theta$		-0.00490		49.54*		0.0185*		70.76
		(0.00595)		(25.83)		(0.00963)		(87.09)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean	0.0739	0.0740	3542.9	3544.3	0.765	0.765	1639.3	1639.3
N	2016	2014	1867	1865	2016	2014	473	473

Notes: This looks at the impact of AGD on parents' willingness to pay to open a savings account for their child. In columns (1)-(2), the outcome variable is a dummy equal to 1 if the parent always chooses to receive cash instead of opening a bank account in their child's name, even when this option is free. In columns (3)-(4), the outcome variable is the willingness-to-pay for a savings account in the child's name among parents for which there exists a non-negative price at which they would take up the savings account. In columns (5)-(6), the outcome variable is a dummy equal to 1 if the parent always chooses to open a savings account in their name instead of their child's name, even when this option is free. In columns (7)-(8), the outcome variable is the willingness-to-pay for a savings account in the child's name instead of the parent's name among parents for which there exists a non-negative price at which they would take up the savings account in their child's name. All the regressions include respondents who declared a strictly negative (or positive) willingness-to-pay for each of options offered. $\delta\theta$ is an experimental measure of the discount factor parents attach to their own consumption. Its imputation is described in Appendix **??**. In this table, we use the measures of $\delta\theta$ and $\mathbb{1}\{\theta < 1\}$ elicited in the same wave of data collection as the outcome variables. $p - value$ test of joint significance of the coefficients on $\delta\theta \times \mathbb{1}\{\theta < 1\}$ and $\mathbb{1}\{\theta < 1\}$. Control variables include the gender and age of the respondent, the gender and age of the child, a measure of credit constraints, the number of children, the religion of the household, the level of education of the respondent. Standard errors in parentheses. * $p < 0.1$, ** $p < .05$, *** $p < .01$