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Delegation and Children's Decision-Making: Evidence from Thailand*

Abstract

We study whether children's behavior changes when caregivers delegate a decision to them. Running an experiment with 956 caregiver-child pairs in Thailand, we let caregivers either retain decision rights or delegate them to children across four incentivized tasks covering time, risk, competitive, and social preferences. We find domain-specific effects of delegation. Delegation makes children more patient, but we find no effects on risk, competitive and social preferences. A simple theoretical framework can rationalize this pattern, as it predicts delegation to affect behavior most when caregivers' and children's preferences diverge substantially, as is primarily the case in the time-preference task.

JEL classification

C93, D91, J13

Keywords

delegation, children, autonomy, time preferences, reciprocity, lab-in-the-field

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1. Introduction

A large body of evidence shows that granting individuals autonomy in decision-making improves outcomes across multiple domains. In labor markets, allowing workers to choose their own salary or letting workers select their own projects has been associated with higher effort and productivity (Charness et al., 2012; Brandes and Darai, 2017; Sloof and von Siemens, 2021). In medical contexts, autonomy-supportive interventions – e.g., offering patients meaningful choice among treatment options, or providing a clear rationale when choice is limited – improve treatment adherence and long-term behavioral change, such as better glucose control among diabetes patients and sustained weight loss (Williams et al., 1996; Williams et al., 2004; Ng et al., 2012). More broadly, from the perspective of Self-Determination Theory (Deci and Ryan, 1985), autonomy is a key determinant of motivation and performance. It is also intrinsically valued: individuals are willing to pay to retain the right to decide for themselves (Bartling, Fehr, and Herz, 2014; Ferreira et al., 2020; Buffat et al., 2023).

The relevance of autonomy is not confined to adult populations. Experimental evidence shows that its motivational effects emerge early in life: children who enjoy control over certain aspects of an activity tend to exert more effort and perform better than children performing comparable activities without such choice, where decisions are instead imposed by authorities (Cordova and Lepper, 1996; Iyengar and Lepper, 1999; Patall, Cooper, and Robinson, 2008). In the context of parenting, autonomy-supportive practices have been associated with higher competence, self-confidence, and self-regulation of children, facilitating the development of independent decision-making skills (Grolnick and Ryan, 1989; Soenens and Vansteenkiste, 2010).[1]

While this latter strand of the literature provides important insights into the benefits of autonomy in parent–child interactions, it typically studies environments in which autonomy is exogenously imposed by design. In these settings, children either face externally imposed decisions or are directly granted choice, without observing that an authority had the option to

[1] Grolnick and Ryan (1989, p. 144) define `Autonomy Support' as "the degree to which parents value and use techniques which encourage independent problem solving, choice, and participation in decisions versus externally dictating outcomes, and motivating achievement through punitive disciplinary techniques, pressure, or controlling rewards."

intervene but deliberately chose not to do so. Yet, in many real-world interactions, autonomy emerges precisely through this margin: parents who hold decision-making authority may deliberately relinquish control and delegate decisions to their children. This distinction is important because delegation is not simply the absence of control. Rather, it involves children recognizing that parents have intentionally entrusted them with decision rights – an act that may itself shape behavior by conveying trust, responsibility, and recognition of the child's agency, while also activating reciprocity motives.

In this paper, we study 956 caregiver-child pairs (children aged 9-15) and explore whether caregivers' deliberate delegation of decision-making authority has a causal effect on children's behavior across a range of domains. Identifying this causal effect is empirically challenging, as it requires jointly observing caregivers and children in an environment where authority structures can be cleanly manipulated. In particular, identifying both the causal effect of delegation and the mechanisms through which it operates requires observing settings in which: (i) caregivers make decisions on behalf of children; (ii) caregivers can either retain authority or delegate decisions to children; (iii) children make decisions while being explicitly informed that authority has been deliberately delegated to them; and (iv) children make decisions independently, without any intervention by the caregiver. Such an environment is extremely difficult to study using observational data.

To overcome this challenge, we implement a lab-in-the-field experiment that reproduces conditions (i)–(iv) in a controlled setting, allowing us to identify whether the deliberate act of granting decision-making authority affects children's choices. Moreover, a novel feature of our study is that we examine whether the effects of delegation are domain-specific – that is, whether they arise only for the delegated decision itself – or whether they spill over into other decision domains.

In our experiment, caregiver-child pairs face four incentivized tasks capturing different domains of economic behavior. These tasks are divided into two groups: time/risk preferences, and competitive/social preferences. The experiment consists of two stages. In Stage 1, pairs are allocated to one of three treatments. In the *Children's Choice* treatment, children make all decisions; in the *Caregivers' Choice* treatment, caregivers decide on behalf of their children; and in the *Delegation* treatment, caregivers choose whether to retain decision rights or to delegate the decision to their child. Regardless of the treatment, in Stage 2, all children make decisions

autonomously, allowing us to study whether earlier exposure to delegation or parental control has persistent effects in domains where parents no longer intervene. The two task groups are implemented in different stages, so that participants face one group of tasks – either time/risk preferences or competitive/social preferences – in Stage 1 and the other group in Stage 2.

Our results show that the effects of delegation vary across decision domains. The strongest effects emerge in the domain of time preferences. Delegated children are willing to delay payment for smaller compensations and make a larger number of patient choices. We find weaker and more limited effects in the domain of risk preferences. In this case, delegation makes it more likely that children always choose the risky option. We find no evidence that delegation influences competitive choices or social preferences. Finally, granting decision-making authority in the competitive and social domain can spill over into choices in the time domain.

The contribution of the paper is threefold. First, we contribute to two related strands of the literature. We add to the literature on children's autonomy. As discussed above, existing work on children's autonomy typically studies environments where autonomy is exogenously imposed by design. We complement this work by providing causal evidence that autonomy emerging from deliberate delegation shapes children's choices. We also add to the experimental literature on parental decision-making. Existing work has focused mainly on the decision maker – on when and how parents intervene and what they choose on behalf of their children (e.g., Tungodden and Willén, 2023; Kiessling et al., 2026). We shift attention to the other side: how children's behavior is affected when parents choose to delegate decision-making authority to them.[2]

Second, by implementing four incentivized tasks – risk-taking, intertemporal choice, social preferences, and competitiveness – we show that the effects of delegation are domain-specific rather than general, and we provide a model that can rationalize this pattern. While delegation affects intertemporal choices, we find no comparable effects on risk-taking, social preferences, or competitiveness. Our evidence suggests that delegation influences behavior primarily in domains where caregivers' and children's ideal choices differ substantially, consistent with a mechanism in which delegation activates reciprocity. Taken together, these findings indicate that delegation does

[2] This focus connects our paper to a broader literature on intra-household allocation, which shows that even the identity of the person who receives resources within the household can shape outcomes (see, for instance, Berry, 2015). In both this literature and our setting, the experimenter initially assigns either resources (in that literature) or decision-making authority (in our setting) to the parent or the child, and examines how this allocation affects children's choices. Our setting differs, however, in that our primary interest is in identifying the causal effect of delegating decision-making authority from the parent to the child on this child's choices.

not induce a broad shift in children's preferences, but instead affects behavior selectively in contexts characterized by meaningful caregiver-child disagreement.

Third, zooming in on the results that we find for time preferences, our paper identifies a novel mechanism to increase patience.[3] Existing work has sought to promote delayed gratification through a wide range of interventions, including changes in default options (Sutter et al., 2015), framing of intertemporal choices (Faralla et al., 2021), and educational programs (Alan and Ertac, 2018). We add to this literature by showing that granting children decision-making authority can foster more patient intertemporal choices.

The remainder of the paper is organized as follows. In Section 2 we explain the experimental design. Section 3 presents a model to provide a theoretical framework for our study. Section 4 presents the main results, and section 5 concludes.

2. Experimental design

The experiment consists of two stages and three main treatments: *Children's Choice*, *Delegation*, and *Caregivers' Choice*. In all treatments, both children and parents are exposed to the same set of tasks, and preferences are elicited before any binding decisions are made.

Participants are caregiver-child pairs. Throughout the experiment, preferences of caregivers and children are elicited individually and privately. Children are informed about who makes the final decision affecting them, but they are not informed about their caregivers' preferences. Note that throughout the paper, we refer to the adult participant as "caregiver." In our sample, grandparents represent the largest share of caregivers (approximately 55%), while biological parents account for around 38%, with the remaining share corresponding to other caregivers.

2.1. Tasks

Children face four incentivized tasks, grouped into two pairs. For each task, they are first asked about their desired decision. Whether or not they can make the ultimate decision (or whether parents do that) is part of the experimental treatments that we describe below. The first pair consists of individual decision-making tasks: a time preference task and a risk preference task. The second

[3] A large body of evidence shows that impatience is a strong predictor not only of financial behavior, but also of a wide range of life outcomes, including educational attainment, health-related behaviors, and self-control more broadly (Castillo et al., 2011; Moffitt et al., 2011; Sutter et al., 2013; Cadena and Keys, 2015; Epper et al., 2026).

pair consist of tasks involving strategic or social considerations: a social preferences task and a competition task. Pairs of tasks are randomly allocated either to Stage 1 or Stage 2 of the experiment.[4]

Time preferences are elicited using a multiple price list in which children make ten choices between a fixed immediate payment of THB 200 and a payment delayed by one week, which starts at THB 210 and increases by THB 10 across decisions until THB 300. Risk preferences are measured using a standard choice list in which children make ten choices between a lottery that pays THB 200 or THB 0 with equal probability and a certain payment, which starts at THB 20 and increases by THB 20 across decisions until, in the last decision, it equals the highest prize of the lottery.

Social preferences are elicited using four binary allocation decisions in which children allocate money between themselves and another randomly selected child participating in the study (Bauer et al., 2014). In each decision, children choose between an equal allocation (THB 60 for themselves and THB 60 for the other child) and an alternative allocation: (60, 0), (60, 120), (120, 0), and (120, 180), where the first amount is for the child and the second for the other child.

Competitiveness is measured through a real-effort task in which children solve two-digit additions mentally for two minutes. Before the task, they choose between a piece-rate scheme, which pays THB 20 per correct answer, and a tournament against a randomly selected child from the same grade, which pays THB 40 per correct answer if the child obtains the higher score and nothing otherwise, with ties broken randomly. At the end of each stage, one decision is randomly selected for payment.

2.2. Treatments

The three treatments differ in who holds decision-making authority over the binding choice, and, crucially, in what children are told about that authority.

- In the *Children's Choice Treatment*, caregivers have no decision-making authority. Children make all binding decisions in the tasks. They are not informed of any potential involvement of the caregivers in the experiment, and caregivers' elicited preferences have no bearing on outcomes.

[4] See Appendix A for the complete detailed tasks and instructions.

- In the *Delegation Treatment*, caregivers are explicitly given the right to choose between implementing their own preferred choice or handing the decision over to their child. If caregivers delegate, the child is explicitly informed that their caregiver had the opportunity to decide, but chose instead to delegate the decision to them, meaning that the child can make the binding decision. If caregivers retained the decision, their preferred choice is implemented, and the child is informed that their caregiver chose to decide on their behalf. This design allows us to measure the effects of delegating decisions to children on their preferred choices, as – in case parents delegate the decision –the child ends up making the same type of binding choice as in *Children's Choice*, but in *Delegation* the child knows a caregiver could have made the decisions for them and voluntarily stepped back.
- In the *Caregivers' Choice* treatment*,* caregivers make the binding decision on behalf of their child, and the child is informed that the decision is made by the caregivers.

Across all treatments, children are informed about who makes the decision affecting them, but not about the preferences of their caregivers.

2.3. Stage 1: Individual preferences, parental authority and treatment assignment

At the beginning of Stage 1, children are introduced to the pair of tasks randomly assigned to that stage – either time/risk preferences or competitive/social preferences. Before knowing who will make the final decision (and thus in every treatment), children are asked to state which option they would prefer in each task if they were allowed to choose. These initial choices are non-binding (and thus still hypothetical) and serve to elicit children's preferences prior to any intervention.

Caregivers are introduced to the same tasks independently and are asked which option they would like the child to choose in each task. This allows us to elicit caregivers' preferences over their child's behavior before any decision rights are assigned.

After eliciting preferences from both children and caregivers, treatment assignment is randomized at the caregiver-child pair level and participants proceed to make binding decisions. Who makes the binding decision depends on the treatment assignment. In the *Children's Choice Treatment*, children then make the binding decision. In the *Caregivers' Choice Treatment*, caregivers make the binding decision on behalf of their child. In the *Delegation Treatment*, caregivers first decide whether to retain decision rights or to delegate the decision to their child.

When caregivers choose to delegate, children make the binding choice. Note that, in all treatments, binding decisions can differ from the hypothetical preferences.

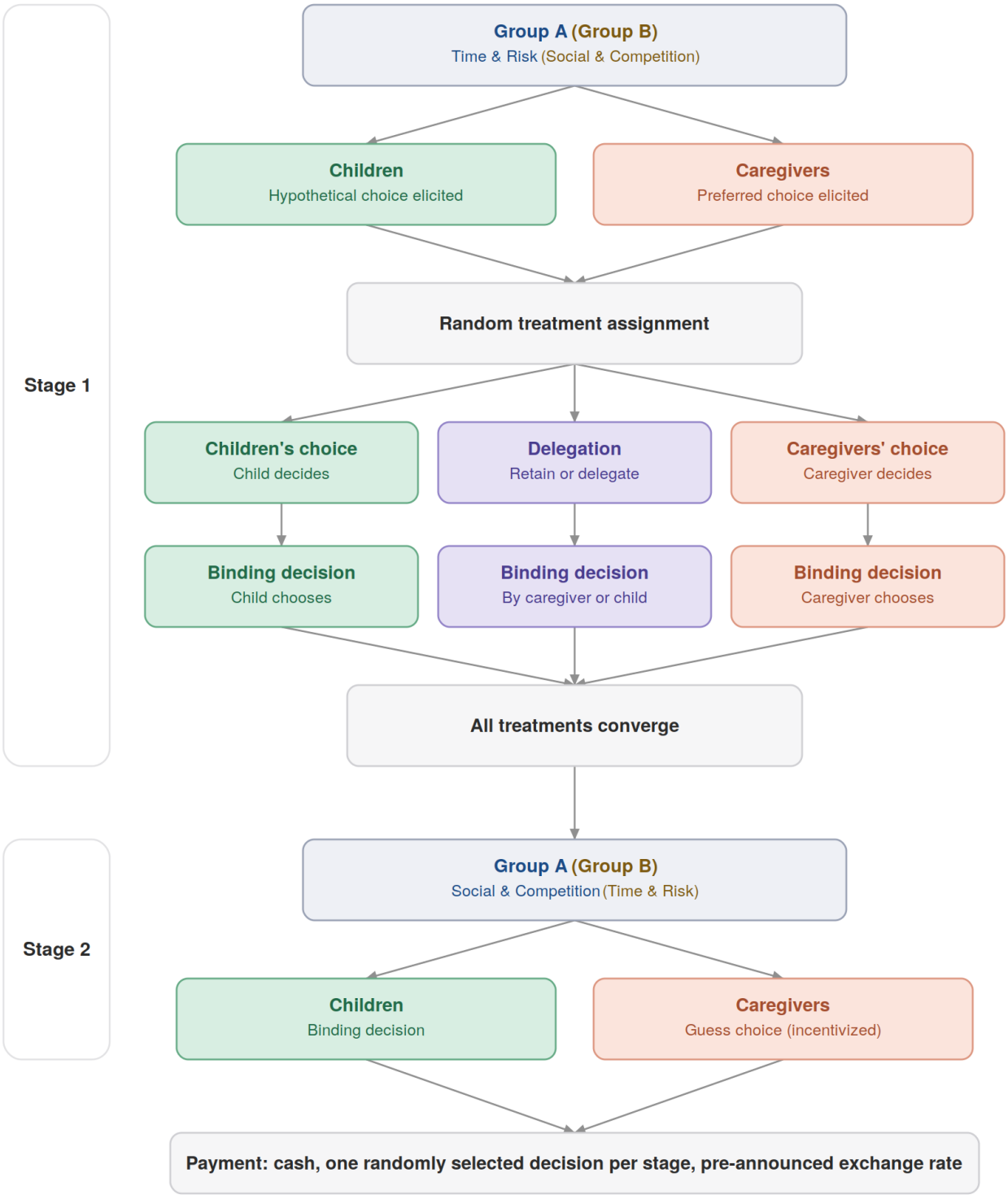
2.4. Stage 2: Subsequent choices

In Stage 2, children are introduced to the second pair of tasks. In this stage, all children make binding decisions, regardless of treatment. Caregivers no longer have the possibility to intervene.

This stage allows us to study whether earlier exposure to caregivers' decision-making or delegation affects subsequent choices in domains where caregivers do not exert control. Caregivers are asked to state their guesses for their child's choices in Stage 2. The guesses are incentivized: one task is randomly selected, and caregivers receive THB 100 if their guess matches their child's actual choice.

Figure 1 below summarizes the timing of the experiment.

Figure 1. Experiment timeline



2.5. Procedures

The experiment was conducted with caregiver-child pairs drawn from the RIECE Panel Data (RPD), an ongoing panel that has followed children and their caregivers in rural areas of Maha Sarakham and Kalasin provinces in Thailand since 2016. The RPD is an existing panel, independent of our experiment, and we use it as our sampling frame. The RPD comprises 1,666 children – aged one to four years at the start of the panel in 2016 – from 1,411 households, together with their parents or caregivers. The panel systematically collects survey and experimental data on children's development, health, education, and behavior, as well as household and parental characteristics.[5]

The RPD frame was constructed as follows. The panel covers 23 Tambons, the smallest official local government unit in Thailand, each comprising 8 to 24 villages. Within each Tambon, villages were classified into three size categories (small, medium, and large) based on the number of eligible children relative to other villages in the same Tambon, and one small, two medium, and one large village were randomly selected from each Tambon, yielding 92 villages. Because the small villages initially selected in two Tambons had fewer than three eligible children, an additional small village was drawn in each of these two Tambons, bringing the total to 94 villages. The frame was subsequently expanded to include 47 additional villages – two villages per Tambon, except one Tambon with three – comprising the largest villages not selected in the initial draw. In total, the RPD frame spans 141 villages, and the 1,666 children reported above include children drawn from these villages.[6]

For the present experiment, our sample was drawn from the subset of RPD households that were reachable and available for participation at the time of data collection. We targeted a sample of approximately 1,000 caregiver-child pairs and planned to conclude data collection once this target was reached, resulting in a final sample of 956 pairs, from 141 of the 141 RPD villages. Extending data collection further to reach 1,000 pairs (rather than 956) would have entailed substantial additional cost and time.

Sessions lasted approximately 30 to 45 minutes and were conducted by an experimenter who explained the instructions and monitored comprehension throughout; all decisions were recorded

[5] The economic characteristics of the rural Thai households in our study area are well documented by the Townsend Thai Project. See, for example, Townsend (1995) and Paweenawat and Townsend (2012).
[6] For more information on the RPD see: https://riped.org/data/riece-panel-data/.

privately using tablets. Children received on average TBH 406.39 (~ USD 12).[7] Parents received on average TBH 112 (~ USD 3.5).

3. The Model

In this section, we introduce a simple model of children's decision-making across the four tasks outlined in Section 2: time, risk, social, and competitive preferences. The model generates distinct equilibrium patterns in children's choices depending on (*i*) whether these choices arise from caregivers' delegation of decision-making authority, and (*ii*) the specific task being performed. Our focus is on comparing children's choices in the *Children's Choice* treatment with those in the *Delegation* treatment, while abstracting from a full model of caregivers' decisions regarding whether to delegate decision-making authority in the *Delegation* treatment.[8]

Our model consists of a representative caregiver (p) and a representative child (c). The utility function of the child is defined on (*i*) an economic decision $x_c^t \in X^t$ taken by the child, where X^t=$\mathbb{R}$ denotes the set of feasible choices and $t \in T$ indexes the task, (*ii*) the decision $x_p^t \in X^t$ the child believes the caregiver would choose on their behalf, and (*iii*) a preference vector $\boldsymbol{a}_c^{t,d}$, where $d = \{\emptyset, D\}$ denotes the treatment – either *Children's Choice* ($d = \emptyset$) or *Delegated* ($d = D$), where the latter refers to cases in which the caregiver delegates the decision in the *Delegation* treatment.[9] In line with our experimental design, we let $T = \{time, risk, social, competition\}$. More specifically, the child's utility function for task $t \in T$ is:

$$U_c^t\left(x_c^t; \boldsymbol{a}_c^{t,\emptyset}, x_p^t\right) = -\left\{\left(1-\beta_c^{\emptyset}\right)(\theta_c^t - x_c^t)^2 + \beta_c^{\emptyset}\left(x_p^t - x_c^t\right)^2\right\}, \quad (1)$$

$$U_c^t\left(x_c^t; \boldsymbol{a}_c^{t,D}, x_p^t\right) = -\left\{\left(1-\beta_c^{D}\right)(\theta_c^t - x_c^t)^2 + \beta_c^{D}\left(x_p^t - x_c^t\right)^2\right\}, \quad (2)$$

[7] To put this amount in context, the statutory minimum daily wage in Maha Sarakham and Kalasin – the two provinces from which our sample is drawn – was THB 347 and THB 349 per day, respectively, throughout 2025 (Ministry of Labour, Thailand, 2024). The average payment therefore corresponds to roughly 1.2 days of local minimum-wage earnings, a non-trivial amount for these households.

[8] In light of the results presented in Section 4, abstracting from the determinants of parents' delegation decisions is unlikely to be consequential for our analysis, given that approximately 90% of parents choose to delegate decision-making authority to their child.

[9] For tractability, we model economic decisions as continuous variables taking values over the same interval in all tasks, allowing us to easily characterize optimal choices. In the experiment, caregivers and children make discrete choices, and the set of available choices differs across tasks. We discuss the implications of this difference whenever relevant.

where $\boldsymbol{a}_c^{t,d} \in (\theta_c^t, \beta_c^d)$, for $d = \{\emptyset, D\}$, and $x_p^t \in X^t$. Here $\theta_c^t \in X^t$ denotes the child's intrinsic *ideal* point – that is, the choice the child would make absent any consideration for their caregiver's preferences. We assume that θ_c^t does not vary across treatments; that is, it is unaffected by whether decision-making authority is delegated to the child. This assumption is natural in our experimental setting, as the caregiver's one-shot, short-run decision to delegate is unlikely to influence the child's intrinsic *ideal* choice. The parameter $\beta_c^d \in [0,1]$ captures the weight the child places on aligning their choice with the caregiver's preferred decision. In this sense, β_c^d can be interpreted as a measure of altruism, reflecting the extent to which the child values matching the decision their caregiver would make for them.

Our main working assumption is $\beta_c^{\emptyset} < \beta_c^{D}$, implying that the child places greater weight on `coordinating' their choice with what they believe their caregiver would choose for them under *Delegation* treatment than under *Children's Choice* – that is, when they know that the caregiver could have chosen x_p^t but instead delegated decision-making authority. Like the effect documented in Charness et al. (2012), this shift may reflect reciprocity induced by the caregiver's decision to delegate. More broadly, this reasoning is in line with that in Doepke and Zilibotti (2017), whereby parents can take actions to influence their child's preferences.[10,11]

The child's utility functions in (1) and (2) borrow important features from the literature in organization economics.[12] In particular, they feature two components: an *adaptation* component – to which the child attaches weight $(1 - \beta_c^d)$ – and a *coordination* component – to which the child attaches weight β_c^d. The *adaptation* component reflects the child's desire to target their intrinsic *ideal* point, whereas the *coordination* component reflects the child's desire to align their decision with what they believe their caregiver would choose for them. The presence of this coordination motive both connects our framework to, and distinguishes it from, that of Doepke and Zilibotti (2017), where altruism operates from caregiver to child – through the caregiver's concern for the child's experience – but not in the reverse direction.

[10] In Doepke and Zilibotti (2017), parents make two types of child-rearing decisions. First, they can incur a cost to `mold' their child's preferences. Second, they can restrict or expand the child's set of feasible choices (X^t). One way to interpret our framework is that it combines these two channels: parents' decision to expand the child's choice set is assumed to also influence the child's preferences through the parameter β_c^d.

[11] Although our goal is not to formally model parents' decisions to delegate authority to their children, one could develop a framework – similar in spirit to Doepke and Zilibotti (2017) – in which parental utility depends on the child's choice set (X^t). In such a model, a parent would derive utility from the child's "experience," in our context from the act of letting the child make decisions themselves.

[12] See, for instance, Dessein and Santos (2006), Alonso et. Al. (2008), and Rantakari (2008).

From (1) and (2), the child optimally sets:

$$(x_c^t)^* = \begin{cases} \beta_c^{\emptyset}\left[x_p^t - \theta_c^t\right] + \theta_c^t & if \quad d = \emptyset \\ \beta_c^{D}\left[x_p^t - \theta_c^t\right] + \theta_c^t & if \quad d = D \end{cases}, \qquad (3)$$

under *Children's Choice* ($d = \emptyset$) and *Delegation* ($d = D$), for $t \in T$. From (3), the child's optimal decision for a given task $t \in T$ depends on (*i*) the weight β_c^d placed on matching the choice they believe their caregiver would make on their behalf (x_p^t), and (*ii*) the distance between that choice (x_p^t) and their intrinsic *ideal* point θ_c^t. We refer to the distance between θ_c^t and x_p^t as the degree of *incongruence* between the child and the caregiver. The following observations follow:

Remark 1. *For a given task $t \in T$, any difference in the child's optimal choice $(x_c^t)^*$ across treatments is driven by differences in the parameter β_c^d.*

Remark 2. *For a given task $t \in T$, the effect of delegation depends on the degree of incongruence between the child and the caregiver. When there is no incongruence, delegation has no effect on the child's optimal choice. Consequently, to the extent that incongruence varies across tasks, the effect of delegation may also vary across tasks.*

Taken together, these remarks imply that delegation affects the child's optimal choice by changing the weight placed on matching the caregiver's perceived choice, with the magnitude of this effect depending on the degree of incongruence between the child and the caregiver, which may vary across tasks.

We conclude by noting that, unlike in the model, caregivers and children make discrete choices in the experiment. This distinction is unlikely to matter when choices are sufficiently granular, as in the *time*, *risk*, and *social* tasks, but may be consequential when choices are binary, as in the *competition* task. Intuitively, even when the caregiver's preferred choice differs from the child's intrinsic ideal choice, the difference may be too small to induce the child to choose differently when delegated the decision in a binary task.

4. Results

This section is structured as follows. We begin by documenting the degree of incongruence between children's and caregivers' choices across the four decision domains (time, risk, competition, and social preferences), a key determinant of the effects of delegation in our theoretical framework (*Remark 2*). We then examine balance in observable characteristics across experimental conditions. Next, we test our hypothesis by analyzing the impact of delegation on children's behavior in each of the four decision domains. Finally, we explore the factors that explain caregivers' delegation choices.

4.1. Analysis of incongruence level

Our theoretical framework predicts that the effect of delegation on children's choices depends on two key ingredients: the shift in the weight (β_c^d) children place on aligning their choices with their caregiver's preferences, and the degree of incongruence between children's ideal points (θ_c^t) and caregivers' preferred choices (x_p^t). When there is little incongruence, delegation has limited scope to affect behavior, even if it increases β_c^d. When incongruence is large, delegation has greater potential to shift children's choices.

Table 1 compares children's decisions in the *Children's Choice* treatment – our proxy for children's ideal point θ_c^t, under the assumption that children in this treatment place approximately zero weight on their caregivers' preferences (i.e., $\beta_c^{\emptyset} \approx 0$) – with caregivers' decisions in the *Caregivers' Choice* treatment – which serve as our proxy for x_p^t. For the time preferences task, we report: i) the average switching point at which children (caregivers) become willing to delay payment, among those who switch, and ii) the average total number of patient choices. For the risk preferences task, we report: i) the average switching point at which children (caregivers) move from the risky to the safe option, among those who switch, and ii) the average total number of safe choices. For the competitive preferences task, we report the average percentage of children (caregivers) who choose to compete in the math task. Finally, we measure social preferences as

the average share of money allocated to the other child out of the total amount allocated to both oneself and the other child across the four prosocial games.

Table 1: Children's and caregivers' choices

	Time		*Risk*		*Competitive*	*Prosocial*
	Switching point	Patient choices (#)	Switching point	Safe choices (#)	Competition (%)	Money shared (%)
	(1)	(2)	(3)	(4)	(5)	(6)
Children's Choice	5.761	2.841	5.051	5.486	6.56	46.78
Caregivers' Choice	3.577	4.478	5.541	4.565	20.39	46.14
Difference	−2.184	1.637	0.490	−0.921	13.83	−0.64
p-value	[<0.001]	[0.006]	[0.476]	[0.040]	[<0.001]	[0.101]
Observations						
Children's Choice	71	183	118	183	183	183
Caregivers' Choice	52	92	61	92	103	103

Notes: "Children's Choice" refers to children's decisions in the *Children's Choice* treatment; "Caregivers' Choice" refers to caregivers' decisions in the *Caregivers' Choice* treatment. "Difference" is *Caregivers' Choice* minus *Children's Choice* (in percentage points for the percentage columns). P-values in brackets are from a two-sample test of proportions in column (5) and from two-tailed Mann–Whitney tests in the remaining columns. "Switching point" is computed only for decision makers who switch exactly once. Observations in *Caregivers' Choice* vary across columns because not all caregivers in the treatment made every decision.

Table 1 shows that the degree of incongruence between children and caregivers varies substantially across the four domains. In the time-preferences domain, caregivers choose a significantly lower switching point (3.577 vs. 5.761, $z = 3.390$, $p < 0.001$, two-tailed Mann-Whitney test) and make a significantly higher number of patient choices (4.478 vs. 2.841, $z = 2.755$, $p = 0.006$, two-tailed Mann-Whitney test) than children in the *Children's Choice* treatment. These differences point to substantial divergence between children's and caregivers' preferences. Consequently, if delegation increases the weight children place on aligning their choices with their caregivers' preferences (β_c^d), children have strong incentives to adjust their behavior towards their caregivers' preferred choices. This leads to our first hypothesis.

Hypothesis 1: Delegating decision-making to children leads them to switch earlier to the patient option and make a greater number of patient choices.

In the risk-preferences domain, the degree of incongruence between caregivers and children is more modest and not always statistically significant, with caregivers choosing a higher switching point (5.541 vs. 5.051, z = 0.713, p = 0.476, two-tailed Mann-Whitney test) and making a lower number of safe choices (4.565 vs. 5.486, z = 2.052, p = 0.040, two-tailed Mann-Whitney test) than children. Overall, these differences suggest greater congruence between caregivers' and children's risk preferences than in the time-preferences domain, leaving less scope for delegation to affect children's choices. This leads to our second hypothesis.

Hypothesis 2: Delegating decision-making to children has smaller effects on their risk choices than on their time preferences.

In the competitive domain, the gap between caregivers and children is large and statistically significant (20.39% vs. 6.56%, z = 3.515, p < 0.001, two-tailed test of proportions), suggesting substantial incongruence between their preferences. This divergence implies that delegation may have considerable scope to affect children's competitive choices. This leads to our third hypothesis.

Hypothesis 3: Delegating decision-making to children increases the proportion of children who choose to compete.

Finally, in the prosocial domain, the share of money allocated to others is virtually identical across treatments (46.14% vs. 46.78%, z = 1.638, p = 0.101, two-tailed Mann-Whitney test), indicating that caregivers and children share very similar prosocial preferences. Accordingly, the degree of incongruence between θ_c^t and x_p^t is limited, leaving little scope for delegation to affect children's choices. This leads to our fourth hypothesis.

Hypothesis 4: Delegating decision-making to children has no effect on their prosocial choices.

4.2. Balance Tests

Table B.1 in Appendix B shows that observable characteristics of children, households, decision makers, and caregivers are largely balanced across treatments. When testing for differences across the three treatments jointly, out of the 32 variables considered, only three differ significantly: the child's age, math scores, and the household wealth factor. These differences are small in magnitude and do not follow a systematic pattern across domains.[13]

The bilateral comparisons confirm this picture. When comparing the *Children's Choice* and *Delegation* treatments, the only significant differences are a lower proportion of girls and lower math scores in *Delegation*. When comparing the *Children's Choice* and *Caregivers' Choice* treatments, children in the *Caregivers' Choice* treatment are older and have slightly higher IQ scores, and the household wealth factor is higher. Finally, when comparing the *Delegation* and *Caregivers' Choice* treatments, children in the *Caregivers' Choice* treatment are significantly older, caregivers have more years of schooling, and children's baseline competitive choices are higher.

Despite the overall balance across treatments, in Section 4.4 (Tables 3-5) we report the results of the econometric analyses that control for these observable characteristics to account for any remaining differences across groups.

4.3. Effect of Delegation: General Descriptives

In this section, we examine the effect of delegating the decision on children's behavior. We begin by noting that an overwhelming majority of caregivers choose to delegate decision-making authority to their child. Out of 578 caregiver-child pairs assigned to the *Delegation* treatment, 517 caregivers choose to delegate, corresponding to 89.45% of the sample. This high take-up rate alleviates concerns that selection into delegation may drive our estimates of its effects on children's behavior. We nonetheless address this concern formally in Appendix F, where we show that our main results are robust to accounting for selection.[14]

[13] With 32 variables tested at the 5% level, about 1.6 significant differences would be expected by chance alone, even under perfect randomization. Observing three significant differences is therefore within the range one could expect from random variation.

[14] The Caregivers' Choice treatment was originally designed to investigate the potential negative effects of non-delegation by comparing decisions in Stage 2 from children whose caregivers retain decision-making authority despite

Table 2 reports results separately for each of the four decision domains (time, risk, competition, and social preferences), comparing children in the *Children's Choice* treatment with children in the *Delegation* treatment whose caregivers chose to delegate the decision. We use the same outcome measures as in Table 1. In addition, for the time- and risk-preference tasks, we report outcomes at the extensive margin: the proportion of children who choose to delay payment at least once, rather than always opting for immediate payment, and the proportion who choose the safe option at least once, rather than always opting for the risky option.

Table 2: Children's choices by treatment

	Time			*Risk*			*Competitive*	*Prosocial*
	Delays payment at least once (%)	Switching point	Patient choices (#)	Safe option at least once (%)	Switching point	Safe choices (#)	Competition (%)	Money shared (%)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Children's Choice	54.64	5.761	2.841	92.90	5.051	5.486	6.56	46.78
Delegated	59.03	4.229	3.699	85.69	4.494	5.456	9.86	46.85
Difference	4.39	−1.532	0.858	−7.21	−0.557	−0.030	3.30	0.07
p-value	[0.302]	[0.001]	[0.024]	[0.011]	[0.180]	[0.794]	[0.179]	[0.911]
Observations								
Children's Choice	183	71	183	183	118	183	183	183
Delegated	517	236	517	517	312	517	517	517

Notes: "Children's Choice" refers to children in the *Children's Choice* treatment; "Delegated" refers to children in the *Delegation* treatment whose caregivers delegated the decision to them. "Difference" is "Delegated" minus "Children's Choice" (in percentage points for the percentage columns). P-values in brackets are from two-sample tests of proportions in columns (1), (4), and (7), and from two-tailed Mann-Whitney tests in the remaining columns. "Delays payment at least once (%)" and "Safe option at least once (%)" are the shares of children who choose the delayed payment (safe) option in at least one of the 10 decisions (extensive margin). "Switching point" is the decision at which the child switches to the delayed payment (safe) option. It is computed only for children who switch exactly once; children who never switch or who switch more than once are excluded. In the time task, a lower switching point means the child needs less compensation to wait (more patient). In the risk task, a lower switching point means the child accepts a lower certain payment (more risk-averse). "Patient choices (#)" and "Safe choices (#)" are the numbers of delayed payment (safe) options chosen, computed for all children; children who never switch are coded as zero.

having the option to delegate with children in a setting where caregivers always make the decisions. However, given the very high take-up rate of delegation, the number of children whose caregivers choose not to delegate is too small to provide sufficient statistical power for this comparison. We therefore report the results for the Caregivers' Choice treatment in Appendix D, and caution that these findings should be interpreted with care.

"Competition (%)" is the share of children who choose tournament pay in the task. "Money shared (%)" is the amount allocated to the other person divided by the total amount allocated to self and other, summed across the four allocation games. Observations differ in columns (2) and (5) because the switching point is defined only for children who switch exactly once.

Table 2 first shows that delegation does not affect the extensive margin of patience: children in the *Delegation* treatment whose caregivers delegate are just as likely as those in the *Children's Choice* treatment to delay the payment at least once (two-sample test of proportions, $z = 1.032$, $p = 0.302$). In contrast, delegation affects the intensive margin. Conditional on choosing to delay payment, delegated children switch earlier – requiring less compensation to wait – than children in *Children's Choice* (two-tailed Mann–Whitney test, $z = 3.280$, $p = 0.001$). They also make a greater number of patient choices overall (two-tailed Mann–Whitney test, $z = 2.254$, $p = 0.024$).

Turning to risk preferences and the lottery task, we observe a different pattern from that found for time preferences. Here, delegation affects the extensive margin: children whose caregivers delegate are less likely than those in the *Children's Choice* treatment to choose the safe option at least once (two-sample test of proportions, $z = 2.541$, $p = 0.011$). In contrast, the intensive margin does not appear to be affected, as neither the switching point nor the number of safe choices differs significantly between the two groups (two-tailed Mann–Whitney tests, $z = 1.340$, $p = 0.180$ and $z = 0.261$, $p = 0.794$ for the switching point and the number of safe choices, respectively).[15,16]

Results also indicate that delegation has limited effects in the competitive and social domains. The last two columns of Table 2 show no significant difference in the proportion of children choosing to compete between those in the *Children's Choice* treatment and those whose caregivers delegate (two-sample test of proportions, $z = 1.344$, $p = 0.179$). Similarly, we find no significant difference in the share of money allocated to others between the two groups (two-tailed Mann–Whitney tests: $z = 0.111$, $p = 0.911$).

[15] The lower share of children ever choosing the safe option under *Delegation* than under *Children's Choice* does not appear to reflect an effect on risk preferences. Delegation leaves the intensive margin unchanged – neither the switching point nor the number of safe choices differs (Table 2) – and the entire difference reflects a shift in the mass of children at the dominated end of the list, who choose the lottery even in the final decision, where the certain payment equals its highest prize. Since a dominated choice cannot express a risk preference, and the difference is only marginal once math, literacy, and IQ are controlled for (see Table 4), this pattern points to inconsistent choice rather than to risk attitudes (Andersson et al., 2016).

[16] Figure E.1 in Appendix E further explores time and risk choices by depicting, among children who switch only once, the distribution of switching points.

4.4 Effect of Delegation: Econometric analysis

4.4.1 Time choices

To further examine the effects of delegation on children's choices while controlling for observable characteristics, Table 3 reports the results of a regression analysis using data from the *Children's Choice* and *Delegation* treatments. Columns (1)-(3) report average marginal effects from probit models in which the dependent variable is the probability of delaying the payment at least once. Columns (4)-(6) report OLS regressions in which the dependent variable is the switching point, among children who switch; an earlier switching point reflects greater patience, since less compensation is needed to make the child wait. Columns (7)-(9) report OLS regressions in which the dependent variable is the number of patient choices.

In columns (1), (4), and (7) the main explanatory variable is *Delegated*, a dummy equal to 1 if the decision was delegated and 0 otherwise. Columns (2), (5), and (8) include as explanatory variable *Female*, a dummy equal to 1 if the child is female; and the interaction between *Delegated* and *Female*. Columns (3), (6), and (9) replace *Female* and the *Female* × *Delegated* interaction with *Time/Risk delegation* – a dummy equal to 1 if delegation applied to the time or risk decision, and 0 if it applied to the competitive or social decision – and the *Time/Risk delegation* × *Delegated* interaction. All the models include a set of controls: *Primary*, a dummy equal to 1 if the child is in primary school; *Math*, the standardized score from a previously administered math test; *Literacy*, the standardized score from a previously administered literacy test; and *IQ*, the score from the Raven test.

Table 3: Regression analysis on time choices

	Delays payment at least once (Probit)			*Switching point* (OLS)			*Patient choices (#)* (OLS)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Constant				5.394***	6.368***	6.162***	1.121	0.420	0.366
				(1.642)	(1.728)	(1.617)	(1.225)	(1.237)	(1.220)
Delegated	0.062	0.122*	0.090	−1.713***	−2.564***	−2.318***	1.004***	1.423***	1.350***
	(0.046)	(0.066)	(0.066)	(0.512)	(0.823)	(0.705)	(0.344)	(0.444)	(0.453)
Female		0.172**			−1.782*			1.354***	
		(0.078)			(0.915)			(0.546)	
Delegated × Female		−0.103			1.163			−0.722	
		(0.092)			(1.035)			(0.675)	
Time/Risk delegation			0.130*			−1.343			1.230**
			(0.078)			(0.884)			(0.550)
Delegated × Time/Risk delegation			−0.051			1.070			−0.657
			(0.091)			(1.009)			(0.681)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
(Pseudo) R-squared	0.019	0.028	0.026	0.059	0.079	0.068	0.021	0.034	0.032
Mean (Children's Choice)	0.546	0.546	0.546	5.761	5.761	5.761	2.841	2.841	2.841
Observations	572	572	572	257	257	257	572	572	572

Notes: The sample comprises children in the *Children's Choice* treatment and children whose caregivers delegated the decision to them. Columns (1)–(3) report average marginal effects from probit models; columns (4)–(9) report OLS estimates. Robust standard errors in parentheses (delta method for marginal effects). Dependent variables are defined as in Table 2. In columns (4)–(6), the sample is restricted to children who switch exactly once, which accounts for the smaller number of observations. "Delegated" equals one if the child's caregiver delegated the decision, and zero for children in *Children's Choice*. "Female" equals one if the child is female. "Time/Risk delegation" equals one if delegation applied to the time or risk decision, and zero if it applied to the competitive or social decision. In columns (2) and (3), "Delegated" is the marginal effect of delegation for males (column 2) or for children whose delegation applied to the competitive or social decision (column 3); "Female" and "Time/Risk delegation" are marginal effects among children in *Children's Choice*; and the interaction terms are the difference in the marginal effect of delegation between the two groups, computed on the probability scale (Ai and Norton, 2003). All rows therefore have the same interpretation as the OLS coefficients. "Controls" include primary-school enrollment, math and literacy test scores, and IQ; their coefficients are omitted. "Mean (Children's Choice)" is the mean of the dependent variable among all children in *Children's Choice*, expressed as a proportion in columns (1)–(3). ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Columns (1)-(3) of Table 3 show that, consistent with the descriptive evidence, delegation does not significantly affect the probability of delaying the payment at least once. This result is robust to the inclusion of controls for children's skills and baseline patience. Column (2) indicates that girls in the *Children's Choice* treatment are more likely to delay the payment than boys; however, we find no evidence that delegation has a differential effect by gender (linear hypothesis test on *Female* and *Female* × *Delegated*, p = 0.154). Column (3) further shows that delegation does not increase the probability of postponing the payment even when delegation applies directly to the time/risk tasks (linear hypothesis test on *Time/Risk delegation* and *Time/Risk delegation* × *Delegated*, p = 0.101).

Columns (4)-(6) show that, conditional on choosing to delay the payment, children who are delegated switch earlier, requiring less compensation to wait, and this result is robust to controlling for children's skills and educational attainment. Column (5) shows that females switch earlier than males, but the coefficient is only marginally significant, and we find no evidence that delegation has a stronger effect for girls than for boys (linear hypothesis test on *Female* and *Female* × *Delegated*, p = 0.206). Column (6) shows that delegation significantly decreases the switching point to delay the payment even when delegation is on a different domain (competition and social preferences). We do not find evidence that the effect differs between direct and indirect delegation (linear hypothesis test on *Time/Risk delegation* and *Time/Risk delegation* × *Delegated*, p = 0.579).

Columns (7)-(9) focus instead on the other dimension of the intensive margin. Delegation significantly increases the number of patient choices, and this result also remains robust when controlling for children's skills and educational attainment. As shown in column (8), girls make more patient choices than boys, but again we find no evidence that delegation has a stronger effect for girls than for boys (linear hypothesis test on *Female* and *Female* × *Delegated*, p = 0.114). Finally, column (9) shows that delegation increases patient choices even when delegation does not apply directly to time decisions. We also do not find evidence that the effect differs between direct and indirect delegation (linear hypothesis test on *Time/Risk delegation* and *Time/Risk delegation* × *Delegated*, p = 0.149). This result, together with that of the switching point, points to the possibility that delegating decision rights from caregivers to children in a particular domain can have spillovers to decisions in another domain, meaning that granting the freedom to choose can have consequences beyond the domain for which the freedom is given.

Taken together, the regression results confirm the descriptive evidence. Delegation does not change whether children ever delay the payment, but it increases the number of patient choices. This effect is similar for boys and girls and arises whether delegation applies directly to the time task or indirectly through the competitive or social decisions. These results support our *Hypothesis 1*.[17]

Result 1: *Delegation affects the intensive margin in time preferences – making children switch earlier and choose the patient option more often – while leaving the extensive margin unchanged.*

4.4.2 Risk choices

Table 4 replicates the econometric analysis of Table 3 for risk choices. Using the same specifications and controls, the table reports results for both the probability of choosing the safe option at least once and the number of safe choices

[17] These findings are robust to correcting for multiple hypothesis testing. In Appendix C, we report Romano–Wolf adjusted p-values for the effect of delegation across all outcomes and domains. The effect on the number of patient choices remains significant at the 5% level (adjusted p = 0.019), and none of the effects in the risk, competitive, and prosocial domains survives the correction.

Table 4: Regression analysis on risk choices

	Safe option at least once (Probit)			*Switching point* (OLS)			*Safe choices (#)* (OLS)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Constant				5.614***	6.142***	5.007***	5.101***	4.800***	5.523***
				(1.422)	(1.496)	(1.443)	(1.124)	(1.173)	(1.161)
Delegated	−0.059*	−0.068*	−0.053	−0.027	−0.615	−0.080	−0.204	0.119	−0.278
	(0.032)	(0.037)	(0.037)	(0.439)	(0.658)	(0.590)	(0.335)	(0.500)	(0.469)
Female		−0.005			−0.996			0.521	
		(0.041)			(0.761)			(0.569)	
Delegated × Female		0.030			1.147			−0.630	
		(0.052)			(0.885)			(0.675)	
Time/Risk delegation			0.014			0.849			−0.654
			(0.041)			(0.754)			(0.564)
Delegated × Time/Risk delegation			−0.001			0.108			0.121
			(0.052)			(0.875)			(0.669)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
(Pseudo) R-squared	0.012	0.013	0.013	0.004	0.009	0.020	0.001	0.003	0.009
Mean (Children's Choice)	0.929	0.929	0.929	5.051	5.051	5.051	5.486	5.486	5.486
Observations	574	574	574	363	363	363	574	574	574

Notes: The sample comprises children in the *Children's Choice* treatment and children whose caregivers delegated the decision to them. Columns (1)–(3) report average marginal effects from probit models; columns (4)–(9) report OLS estimates. Robust standard errors in parentheses (delta method for marginal effects). Dependent variables are defined as in Table 2. In columns (4)–(6), the sample is restricted to children who switch exactly once, which accounts for the smaller number of observations. "Delegated" equals one if the child's caregiver delegated the decision, and zero for children in *Children's Choice*. "Female" equals one if the child is female. "Time/Risk delegation" equals one if delegation applied to the time or risk decision, and zero if it applied to the competitive or social decision. In columns (2) and (3), "Delegated" is the marginal effect of delegation for males (column 2) or for children whose delegation applied to the competitive or social decision (column 3); "Female" and "Time/Risk delegation" are marginal effects among children in *Children's Choice*; and the interaction terms are the difference in the marginal effect of delegation between the two groups, computed on the probability scale (Ai and Norton, 2003). All rows therefore have the same interpretation as the OLS coefficients. "Controls" include primary-school enrollment, math and literacy test scores, and IQ; their coefficients are omitted. "Mean (Children's Choice)" is the mean of the dependent variable among all children in Children's Choice, expressed as a proportion in columns (1)–(3). ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 4 shows that, once we control for observable characteristics, the effect of delegation on the extensive margin of risk choices becomes only marginally significant, indicating a weaker relationship than suggested by the descriptive evidence. In line with the descriptive results, we find no robust effects on the switching point at which children move from the risky to the safe option, among those who switch, or on the number of safe choices, suggesting that delegation does not affect the intensive margin. As in the time domain, there is no evidence of heterogeneous effects by gender ($p = 0.442$, $p = 0.814$, and $p = 0.763$ for the linear hypothesis test on *Female* and *Female* × *Delegated* on the probability of choosing a safe option, the switching point, and on the number of safe choices, respectively). Results also do not differ between direct and indirect delegation for the probability of choosing a safe option, or the number of safe choices ($p = 0.672$, and $p = 0.138$ for linear hypothesis test on *Time/Risk delegation* and *Time/Risk delegation* × *Delegated*, on the probability of choosing a safe option, and on the number of safe choices, respectively). For the switching point, we observe that, among delegated children, those for whom delegation applied directly to the time/risk pair choose a higher switching point than those delegated on the competitive/social pair ($p = 0.031$, for linear hypothesis test on *Time/Risk delegation* and *Time/Risk delegation* × *Delegated*). Taken together, these findings support *Hypothesis 2*, as summarized in *Result 2* below.

Result 2: *Delegation does not have an effect on the intensive margin of children's risk choices, and only marginally reduces the probability of choosing the safe option at least once. Its effects on risk choices are therefore smaller, and mostly insignificant, compared to those on time preferences.*

4.4.3 Competitive and prosocial choices

To examine the effects of delegation on children's competitive and prosocial choices, Table 5 reports the results of a regression analysis using data from the *Children's Choice* and *Delegation* treatments. Columns (1)-(4) report average marginal effects from a probit model in which the dependent variable is the probability of choosing to compete in the math task. In Column (1), the main explanatory variables are *Delegated*, a dummy equal to 1 if the decision was delegated and 0 otherwise, and *Hypothetical*, a dummy equal to 1 if the child chose to compete when the decision

was hypothetical. Column (2) additionally includes the interaction between *Hypothetical* and *Delegated*. Column (3) includes *Female*, a dummy equal to 1 if the child is a girl, and its interaction with *Delegated*. Column (4) includes *Competitive/Pro-social delegation*, a dummy equal to 1 if delegation applied to the competitive or prosocial decision. Columns (5)-(8) mirror the analysis from (1)-(4) and report results from OLS regressions where the dependent variable is the percentage of money shared with the other player in the social task. All models include the same set of controls as in Tables 3 and 4: *Primary, Math, Literacy, and IQ*.

Table 5: Regression analysis on competitive and prosocial choices

	Competition (Probit)				*Money shared* (OLS)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant					0.105***	0.143**	0.102***	0.413***
					(0.031)	(0.056)	(0.031)	(0.028)
Delegated	0.084**	0.038*	0.054	0.019	−0.001	−0.053	0.005	−0.008
	(0.041)	(0.021)	(0.038)	(0.030)	(0.009)	(0.057)	(0.012)	(0.010)
Hypothetical	0.731***	0.507***	0.729***		0.735***	0.655***	0.734***	
	(0.070)	(0.164)	(0.070)		(0.053)	(0.107)	(0.053)	
Delegated × Hypothetical		0.305*				0.111		
		(0.179)				(0.120)		
Female			−0.037				0.004	
			(0.041)				(0.016)	
Delegated × Female			0.033				−0.013	
			(0.053)				(0.018)	
Comp./Prosocial delegation				0.034				−0.025*
				(0.040)				(0.013)
Delegated × Comp./Prosocial delegation				0.033				0.020
				(0.050)				(0.015)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
(Pseudo) R-squared	0.483	0.486	0.487	0.043	0.469	0.471	0.471	0.055
Mean (Children's Choice)	0.066	0.066	0.066	0.066	0.468	0.468	0.468	0.468
Observations	294	294	294	574	294	294	294	574

Notes: The sample comprises children in the *Children's Choice* treatment and children whose caregivers delegated the decision to them. Columns (1)–(4) report average marginal effects from probit models; columns (5)–(8) report OLS estimates. Robust standard errors in parentheses (delta method for marginal effects). Dependent variables are defined as in Table 2; "Money shared" is a share of total amount allocated. "Delegated" equals one if the child's caregiver delegated the decision, and zero for children in *Children's Choice*. "Hypothetical" is the child's hypothetical choice in the same task. "Female" equals one if the child is female. "Comp./Prosocial delegation" equals one if delegation applied to the competitive or social decision, and zero if it applied to the time or risk decision. In columns (2)–(4), "Delegated" is the marginal effect of delegation when the interacted variable equals zero, the other main effects are marginal effects among children in Children's Choice, and the interaction terms are the difference in the marginal effect of delegation between the two groups (Ai and Norton, 2003), as in the OLS columns. When "Hypothetical" is included, the Comp./Prosocial delegation terms are omitted because of collinearity, so columns (4) and (8) exclude "Hypothetical" and use the full sample. "Controls" include primary-school enrollment, math and literacy test scores, and IQ. "Mean (Children's Choice)" is the mean of the dependent variable among all children in *Children's Choice*, as a proportion. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 5 shows that delegation does not have a robust effect on children's competitive or prosocial choices. At first sight, Hypothesis 3 seems to be supported by the data when we look at columns (1) and (2) of Table 5. In the Probit analysis, the coefficient on *Delegated* is positive and statistically significant in Column (1), suggesting that delegation may increase the probability of competing. Column (2) suggests that this effect is concentrated among children who would also compete in the hypothetical decision: the effect of delegation is 3.8 percentage points ($p < 0.10$) for children who would not compete hypothetically, and the interaction is positive but only marginally significant ($p < 0.10$). However, adding further controls lets the support for Hypothesis 3 vane, for which reason these first results should be interpreted with caution. Given the very low rates of competition observed in both the *Children's Choice* (6.56%) and *Delegation* (9.86%) treatments, there is limited variation in the dependent variable, which constrains the ability of the model to precisely identify the effect of delegation. Consistent with this, the coefficient on *Delegated* becomes insignificant in Columns (3) and (4), indicating that the result in Column (1) is not robust across specifications. For prosocial behavior, delegation has no significant effect across any of the OLS specifications in Columns (5)-(8). The only robust predictor across both domains is *Hypothetical*, which enters with a large and significant coefficient in all specifications where it is included, confirming that children's initial preferences are strongly correlated with their binding choices. This leads to our next result, which support *Hypothesis 4* but not *Hypothesis 3*. As discussed at the end of Section 3, one possible explanation for the lack of unanimous support for *Hypothesis 3* is that, unlike choices in the time domain, competitive choices are binary: children must either choose to compete or not. Consequently, aligning their choices with those of their caregivers requires a complete reversal of their initial decision. Such a change may be too costly, even when delegation increases the weight children place on matching their caregivers' preferences.

Result 3: *Delegation does not play a systematic role in children's competitive or prosocial choices, leaving both mostly unaffected.*

4.5 Delegation decision

This section explores caregivers' decisions to delegate and the factors that may explain them. Table 6 reports results from Probit models in which the dependent variable is the probability that the decision is delegated. All specifications use data exclusively from the *Delegation* treatment.

Column (1) includes children characteristics as explanatory variables: *Female,* a dummy equal to 1 if the child is a girl, and 0 otherwise; *Primary,* a binary variable equal to one if the child is in primary school, and 0 if the child is in high school; *Math,* the standardized score from a previously administered math test; *IQ,* the score from a Raven test; *Patience,* a dummy equal to one if the child is willing to wait for a reward; and *Literacy,* the score obtained in a previously administered literacy test.

Column (2) instead includes caregiver characteristics: *Female,* a dummy equal to 1 if the caregiver making the delegation decision is female; *Parent,* a dummy equal to 1 if the caregiver is a parent, and 0 otherwise; *Secondary*, a dummy equal to 1 if the caregiver has secondary education; *Tertiary*, a dummy equal to 1 if the caregiver has tertiary education; and standardized measures of three parenting styles (authoritative, authoritarian, and permissive).[18] Column (3) includes both children and caregiver characteristics as explanatory variables. All columns include *Time/risk delegation*, a dummy equal to 1 if the delegation decision concerns time or risk preferences, and 0 if it concerns competitive or social preferences.

[18] Parenting styles are measured using the Parenting Styles and Dimensions Questionnaire (PSDQ; Robinson et al., 2001). We construct indices following the original instrument by averaging the relevant questionnaire items (or subdimensions, where applicable) to obtain authoritative, authoritarian, and permissive parenting style scores, which are subsequently standardized to have mean zero and unit variance before entering the regressions.

Table 6: Determinants of the delegation decision

	Caregiver delegates (Probit)		
	(1)	(2)	(3)
Child characteristics			
Female	0.017 (0.030)		0.013 (0.029)
Primary	−0.023 (0.029)		−0.026 (0.029)
Math	0.009 (0.015)		0.012 (0.014)
Literacy	0.017 (0.015)		0.017 (0.015)
IQ	−0.000 (0.001)		−0.000 (0.001)
Patience	0.049 (0.031)		0.048 (0.030)
Caregiver characteristics			
Female		−0.070 (0.046)	−0.053 (0.046)
Parent		−0.014 (0.039)	0.031 (0.043)
Secondary education		−0.019 (0.039)	−0.048 (0.043)
Tertiary education		−0.007 (0.060)	−0.033 (0.069)
Authoritative		0.007 (0.013)	0.007 (0.014)
Authoritarian		−0.006 (0.014)	0.010 (0.015)
Permissive		−0.001 (0.014)	−0.014 (0.014)
Time/Risk delegation	0.009 (0.029)	−0.012 (0.025)	0.011 (0.028)
Pseudo R-squared	0.023	0.011	0.037
Mean (Delegation)	0.894	0.894	0.894
Observations	442	576	441

Notes: The sample comprises caregivers in the *Delegation* treatment. The dependent variable equals one if the caregiver delegated the decision to the child, and zero otherwise. The table reports average marginal effects from probit models. Robust standard errors in parentheses (delta method). ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 6 shows that none of the observable characteristics included in the regression significantly predicts the probability of delegation. This absence of statistically significant effects

should be interpreted with caution, however. Recall that around 90% of caregivers choose to delegate the decision, leaving a relatively small number of observations in which delegation does not occur. As a result, the limited variation in the dependent variable reduces statistical power and constrains our ability to precisely identify the determinants of the delegation decision.

5 Conclusions

We study whether children's behavior changes when their caregivers deliberately choose to delegate a decision to them, rather than making it on their behalf. To identify this effect, we implement a lab-in-the-field experiment with 956 caregiver-child pairs in rural Thailand. In our main treatment, caregivers are randomly assigned to either retain authority or delegate decisions across four incentivized tasks to their children. Our design allows us to cleanly separate the effect of delegation from simple autonomy, since children in the *Delegation* treatment observe that their caregiver had the option to decide but deliberately chose not to. To identify the effects of delegation, we can compare children's preferences in this main treatment with another treatment (*Children's Choice*) in which they make the decisions for sure and parents cannot interfere.

Our main finding is that delegation affects children's behavior in a domain-specific way. The strongest and most robust effects arise in the intertemporal domain: delegated children switch to the patient option earlier and make a larger number of patient choices overall. Effects on risk preferences are only marginal. Delegation has no significant effect on competitive or prosocial choices.

We provide a theoretical framework that rationalizes this pattern. In the model, delegation shifts the weight children place on aligning their choices with what they believe their caregiver would choose for them – an effect we interpret as reciprocity triggered by the deliberate act of granting decision rights. Whether this shift in weights actually changes behavior depends on the degree of incongruence between children's and caregivers' preferred choices. In the time domain, caregivers and children disagree substantially, and the decision is continuous, giving children room to adjust gradually toward their caregiver's preferred choice. In the competitive domain, conflict is also large, but the binary nature of the decision makes coordination too costly. In the risk and prosocial domains, caregivers and children are more aligned to begin with, leaving little scope for delegation to matter.

Taken together, these results suggest that delegation does not induce a broad shift in children's preferences, but instead operates selectively in contexts where there is meaningful disagreement between caregivers and children and the decision structure allows for gradual adjustment.

A specific feature of our results refers to the identification of a novel mechanism to promote patience in children – one that operates through the relationship between caregiver and child rather than through changes in the choice architecture or educational interventions. Given the well-documented role of patience in long-run educational and economic outcomes (e.g., Moffitt et al., 2011; Alan and Ertac, 2018), this finding may have relevant implications for the design of programs targeting children's intertemporal decision-making.

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Appendices

Appendix A. Tasks and Instructions

A.1. Tasks

This section describes the parameters of the four incentivized tasks. All amounts are in Thai Baht (THB). Children and caregivers face the same tasks.

Time preferences. Children make ten choices between Option A, a fixed payment of THB 200 received on the day of the experiment, and Option B, a payment received one week later. The amount of Option B starts at THB 210 and increases by THB 10 in each decision, up to THB 300 (Table A.1).

Table A.1: Time preferences task

Decision	*Option A (today)*	*Option B (in one week)*
1	200	210
2	200	220
3	200	230
4	200	240
5	200	250
6	200	260
7	200	270
8	200	280
9	200	290
10	200	300

Notes: Amounts in THB.

Risk preferences. Children make ten choices between Option A, a lottery that pays THB 200 or THB 0 with equal probability, and Option B, a certain payment. The amount of Option B starts at THB 20 and increases by THB 20 in each decision, up to THB 200 (Table A.2). In the last decision, the certain payment equals the highest prize of the lottery. If a risk decision is selected for payment and the child chose the lottery, the payment is determined by a random draw in front of the child.

Table A.2: Risk preferences task

Decision	*Option A (lottery)*	*Option B (certain)*
1	50% 200 / 50% 0	20
2	50% 200 / 50% 0	40
3	50% 200 / 50% 0	60
4	50% 200 / 50% 0	80
5	50% 200 / 50% 0	100
6	50% 200 / 50% 0	120
7	50% 200 / 50% 0	140
8	50% 200 / 50% 0	160
9	50% 200 / 50% 0	180
10	50% 200 / 50% 0	200

Notes: Amounts in THB.

Social preferences. Following Bauer et al. (2014), children make four binary allocation decisions between themselves and another, randomly selected child participating in the study. In each decision, Option A is an equal allocation of THB 60 to each child, and Option B is an alternative allocation (Table A.3).

Table A.3: Social preferences task

Game	*Option A (Self, Other)*	*Option B (Self, Other)*
Social	(60, 60)	(60, 0)
Envy	(60, 60)	(60, 120)
Sharing	(60, 60)	(120, 0)
Efficiency	(60, 60)	(120, 180)

Notes: Amounts in THB. "Self" is the amount received by the child making the decision, and "Other" the amount received by the other child.

Competitive preferences. Children solve two-digit additions mentally for two minutes. Before the task, they choose between two payment schemes. Under the piece-rate scheme, they receive THB 20 per correct answer. Under the tournament scheme, they compete against a randomly selected child from the same grade: if they obtain the higher score, they receive THB 40 per correct answer, and otherwise they receive nothing. Ties are broken randomly. Children are also asked to guess the rank of their score among children in the same grade participating in the study, from 1 (highest) to 10 (lowest), and receive THB 20 if their guess is correct. If the

competitive decision is selected for payment and the child chose the tournament, the reward is determined once all children in the study have completed the task, and it is delivered at a later date.

Payment. At the end of each stage, one decision is randomly selected for payment, and the child draws the lottery personally.

A.2. Instructions

The experiment was conducted in Thai by trained enumerators, and all decisions were recorded on tablets. Below we report an English translation of the scripts that enumerators read aloud to participants. Text in parentheses was addressed to the enumerator and was not read aloud.

We report the scripts for the version in which the time and risk tasks are implemented in Stage 1 and the social and competitive tasks in Stage 2. In the other version, the order of the two pairs of tasks is reversed and the scripts are otherwise identical. Treatment-specific text is indicated in brackets.

In every task, participants first answered comprehension questions. If an answer was incorrect, the enumerator explained the task again, up to three attempts. Before each new decision, participants were told: "Now, let's make a new choice. Each item is independent, please set aside your previous response."

A.2.1. Caregivers' instructions

Introduction

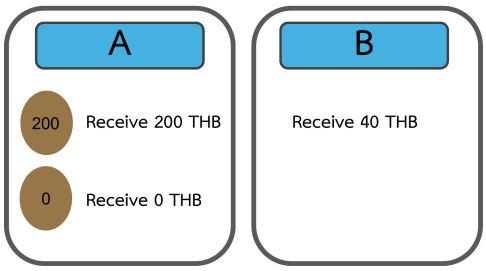
"Good [morning/afternoon]. Today I have brought some games for you to play. You will be able to earn real money from these games. Before we begin, the research team will provide a participation fee of 100 THB per person, which I will transfer to you prior to starting the games. This amount is separate from any reward you may earn after completing the games. To process the transfer, I would like to ask for your bank account details before we begin."

"Today I would like to ask you some questions. After we finish, I will visit your child at school to play some games, and your child will receive a monetary reward upon completion. According to the game rules, I must first collect your responses before visiting your child. You will play the same games that your child will play. Please listen to the instructions carefully, as your decisions will affect the reward your child receives."

Stage 1: Preferred choices.

Risk task: "In this game, your child might choose between Option A or Option B. If Option A is chosen, an egg would be drawn to determine the reward, with equal probability (50/50). Drawing the egg marked 200, your child receives 200 THB. Drawing the egg marked 0, your child receives 0 THB. If Option B is chosen, your child receives a guaranteed 40 THB." The lottery was presented to participants as shown in Figure A.1.A.

Figure A.1.A



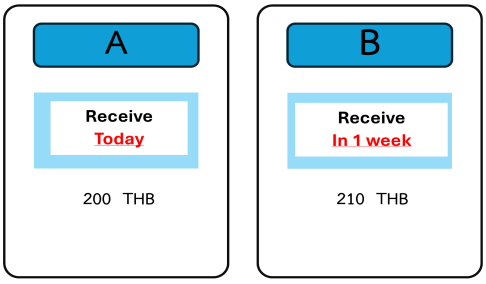
Comprehension questions:

- "Between Option A and Option B, which option requires an egg draw to determine the reward?"
- "Which option gives a guaranteed payment without an egg draw?"
- "For someone who chooses Option A, how much do they receive if they draw the egg marked 200? And the egg marked 0?"
- "For someone who chooses Option B, how much do they receive?"

"I will now ask you to indicate which option you would like your child to choose, Option A or Option B. There are no right or wrong answers. Please choose whichever option you prefer. I would also like to remind you that this is a research study, please do not be concerned that this constitutes gambling." For each of the ten decisions: "Which option would you like your child to choose, Option A or Option B?"

Time task: "In this game, your child will choose between receiving money today or waiting one week. Option A: Receive 200 THB today. Option B: Receive 210 THB in one week." The time task was presented to participants as shown in Figure A.1.B.

Figure A.1.B



Comprehension questions: "If Option A [Option B] is chosen, when will your child receive the money, and how much?"

The choices were then elicited for the ten decisions as in the risk task.

Stage 1: Decision rights

"You have now completed all responses for this stage. Later today, I will visit your child to play the same games."

[Children's Choice]: "Your child will receive a reward from one randomly drawn item only, determined by a lottery that your child draws personally."

[Delegation]: "Your child will receive a reward from one randomly drawn item only, determined by a lottery that your child draws personally. However, under the rules of this game, you will decide whether to play on your child's behalf or let your child play independently.

( ) Play on my child's behalf

( ) Let my child play by themselves."

- If the caregiver delegates: "You have chosen to let your child play by themselves. Later today, I will visit your child to play the game."

- If the caregiver retains the decision: “You have chosen to play on your child’s behalf. I will now ask you to play the game again. Once you have finished, I will show your child your responses, and your child will draw a lottery to receive a reward from one item only.”

[Caregivers’ Choice]: “Under the rules of this game, you will play on your child’s behalf. I will now ask you to play the game again. Once you have finished, I will show your child your responses, and your child will draw a lottery to receive a reward from one item only.”

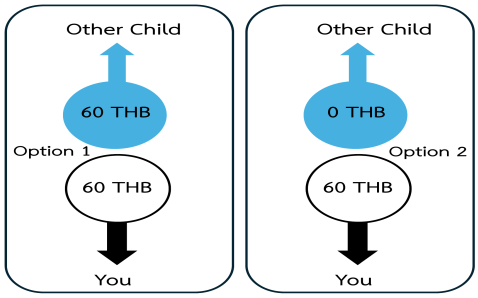
Caregivers who play on their child’s behalf (Caregivers’ Choice, and Delegation when the decision is retained) then go through the risk and time tasks again, with the same explanations and comprehension questions. Each decision is now binding: “I will now ask you to make your selection. There are no right or wrong answers. Please choose whichever option you prefer. Which option do you choose, Option A or Option B?”

Stage 2: Guesses.

“For the next game, I will ask you to guess what your child will choose when playing this game. After your child finishes playing, your child will draw one lottery containing a question number. The system will then display your child’s answer for that question. If your guessed answer matches your child’s actual choice, you will receive a reward of 100 THB.”

Social task: “In the following game, your child will allocate a reward between themselves and one other child participating in this study, who will be randomly selected later.” The enumerator then explains the options and asks the comprehension questions: “If Option 1 [Option 2] is chosen, how much does your child receive? How much does the other child receive?” The social task was presented to participants as shown in Figure A.1.C.

Figure A.1.C



"We will now guess your child's choices. Please note that your response here will affect your reward. If your guess is correct, you will receive 100 THB." For each of the four decisions: "Please guess your child's choice. Which option do you think your child will choose?"

Competitive task: "In the next game, your child will solve two-digit mental addition problems. Your child must choose between two formats. Under Option 1 (piece rate), each correct answer earns 20 THB. Under Option 2 (tournament), the child competes against a randomly selected child from the same grade: if the child scores higher, each correct answer earns 40 THB; if lower, the child receives 0 THB. Ties are broken randomly.

- Guess: "Your response here will affect your reward. If your guess is correct, you will receive 100 THB. Please guess your child's choice. Which format do you think your child will select?"
- Preferred choice: "I have one more question. Your answer does not need to match your previous response. Which one would you like your child to choose?"
- Rank: "Among all children from the same grade level participating in this study, where do you think your child's score will rank? Rank 1 = highest score; Rank 10 = lowest score. If your guess is correct, you will receive 100 THB."

Closing

"For the guess tasks you have just completed, I will visit your child later today to play the games. After your child finishes playing, your child will draw a lottery on your behalf. If your guess is correct, the reward will be transferred to the bank account you provided at the beginning. Payment details will be written on a payment form and sent home with your child for your records."

A.2.2. Children's instructions

Introduction

"Good [morning/afternoon]. Today I have brought several games for you to play, and you will receive a monetary reward after completing the games. You will receive a real payment from one randomly drawn item only, determined by a lottery. Let's get started."

Stage 1, Round 1: Preferred choices (non-binding)

The risk and time tasks are explained as in Section A.2, addressing the child directly ("you" instead of "your child"), and the same comprehension questions are asked. The enumerator then says: "We will now begin Round 1. There are no right or wrong answers. I would also like to remind you that this is a research study, please do not be concerned that this constitutes gambling. In this round, I would like you to tell me: if you had the chance to play by yourself, which option would you choose, Option A or Option B?" The child answers the ten risk and the ten time decisions.

Stage 1, Round 2: Binding choices

[Children's Choice]: "You have now completed Round 1. Under the rules of Round 2, you will play again. Your choices in this round will affect your reward. You will receive a real payment from one randomly drawn item only, determined by a lottery."

[Delegation, delegated]: "You have now completed Round 1. I visited your caregiver at home and asked them to choose between playing Round 2 on your behalf or letting you play Round 2 by yourself. Your caregiver chose to let you play Round 2 by yourself."

[Delegation, non-delegated]: "You have now completed Round 1. I visited your caregiver at home and asked them to choose between playing Round 2 on your behalf or letting you play Round 2 by yourself. Your caregiver chose to play Round 2 on your behalf. Your caregiver has already played the game. You will receive a real payment from one randomly drawn item from your caregiver's choices only, determined by a lottery. I will now show you the choices your caregiver made."

[Caregivers' Choice]: You have now completed Round 1. I visited your caregiver at home and asked them to choose how to play Round 2 on your behalf. I will now show you the choices your caregiver made.

In Children's Choice, and in Delegation when the caregiver delegates, the child goes through the risk and time tasks again with the same explanations and comprehension questions, and each decision is binding: "I will now ask you to make your choice. There are no right or wrong answers. Which option do you choose, Option A or Option B?" When the caregiver decides, the child is shown the caregiver's choices for the twenty decisions instead.

Stage 1, Payment

"You have now completed all items, 20 in total. I will now ask you to draw a lottery to receive your reward from one item only. You will draw the lottery yourself. Each one has a number on it. Whichever number you draw, you will receive the reward from that item." If the drawn item is a lottery and Option A was chosen, the child draws an egg to determine the reward. If the drawn item is a time decision and the delayed option was chosen, the enumerator writes the amount and the payment date on a form for the child to keep, and the money is delivered on that date.

Stage 2

"Now let's continue with the remaining games. After you finish, I will ask you to draw a lottery again to receive your reward from one item only. You will draw the lottery yourself. Each one has a number on it. Whichever number you draw, you will receive the reward from that item."

In this stage, all children make binding choices, regardless of treatment.

Social task: "In the following game, you will allocate a reward between yourself and one other child participating in this study, who will be randomly selected later." The enumerator explains the options and asks the comprehension questions for each of the four decisions. Then: "I will now ask you to make your choice. There are no right or wrong answers. Which option do you choose, Option 1 or Option 2?"

Competitive task: "In the next game, you will solve two-digit mental addition problems. You must choose between two formats." The options are described as in Section A.1. "Which format do you choose?"

Rank: “Among all children from the same grade level participating in this study, where do you think your score will rank? Rank 1 = highest score; Rank 10 = lowest score. If your guess is correct, you will receive 20 THB.”

Addition task: “A problem will appear on the screen. Enter your answer in the box and tap ‘Next’. You have 2 minutes. Please solve the problems mentally, do not say your answers out loud.”

Stage 2, Payment

One of the five decisions is drawn by lottery, as in Stage 1. If the child chose the tournament, the reward is determined once all children in the study have completed the game, and it is delivered at a later date.

Caregiver’s guess

“You have now completed all games. Earlier today, I asked your caregiver to guess which option you would choose for each item. I will now ask you to draw one lottery to see whether the answer from the drawn item matches what your caregiver guessed. If your caregiver’s guess is correct, your caregiver will receive 100 THB.” The child is shown the caregiver’s guess and the child’s own choice, and the result is recorded on the payment form sent home with the child.

Appendix B. Balance tests

To assess whether the different treatments were conducted with similar subject pools, Table B.1 examines selection across treatments along several variables. At the child level, we include demographic variables such as gender and age, as well as educational indicators, including whether the child is enrolled in primary education. We also consider measures of cognitive ability and academic performance, such as standardized math and literacy scores and IQ test results.

At the household level, we include indicators of socioeconomic status, such as a household wealth index and family composition (e.g., number of siblings). In addition, we report characteristics of the decision maker or caregiver, including gender, age, educational attainment, and family role.

Finally, we include measures of baseline preferences elicited prior to treatment assignment, covering time, risk, competitive, and social preferences. Since these hypothetical decisions were made before subjects were informed about their treatment, they are elicited under identical conditions for all participants. This allows us to assess whether initial preferences are balanced across treatments and provides an additional check on the quality of the randomization.

Table B.1: Balance across treatments

	Means				*p-values*		
	Children's Choice (1)	Delegation (2)	Caregivers' Choice (3)	All treatments (4)	Children's Choice vs. Delegation (5)	Children's Choice vs. Caregivers' Choice (6)	Delegation vs. Caregivers' Choice (7)
Child							
Female (%)	54.1	45.3	51.3	0.074	0.039	0.584	0.150
Age (years)	11.803	11.779	12.123	0.015	0.735	0.027	0.004
Primary school (%)	44.3	46.7	42.1	0.501	0.562	0.665	0.259
Math score (standardized)†	0.145	−0.106	0.098	0.006	0.014	0.541	0.085
Literacy score (standardized)†	−0.016	−0.060	0.099	0.203	0.426	0.562	0.093
IQ (Raven test)†	85.322	86.177	88.233	0.056	0.266	0.017	0.059
Wait (%)†	69.7	68.9	66.9	0.835	0.848	0.568	0.611
Sweet reward (%)†	58.0	56.6	56.1	0.932	0.754	0.722	0.902
Math rank	5.219	5.063	4.979	0.556	0.412	0.417	0.877
Household							
Wealth index†	−0.129	−0.071	0.221	0.021	0.304	0.028	0.064
Number of siblings	0.437	0.548	0.574	0.058	0.056	0.086	0.835
One parent at home (%)	25.1	25.1	23.6	0.904	0.999	0.727	0.667
Two parents at home (%)	29.0	31.5	33.3	0.651	0.511	0.360	0.643
Decision maker							
Female (%)	84.2	87.4	88.7	0.421	0.266	0.195	0.620
Married (%)	70.5	73.4	69.7	0.464	0.227	0.549	0.619
Parent (%)	35.0	40.0	37.9	0.762	0.474	0.560	0.992
Grandparent (%)	56.8	53.8	53.8	0.548	0.449	0.874	0.329
Years of schooling	7.177	7.645	7.870	0.183	0.149	0.069	0.421
Math rank	4.191	4.485	4.220	0.189	0.241	0.888	0.165
Caregiver							
Female (%)	84.2	87.7	86.2	0.479	0.214	0.585	0.571
Age (years)	54.720	54.324	54.103	0.882	0.977	0.645	0.566
Parent (%)	32.2	32.4	30.8	0.916	0.977	0.759	0.682
Grandparent (%)	59.0	60.2	57.9	0.848	0.775	0.833	0.578
Years of schooling	7.126	7.227	7.774	0.154	0.815	0.062	0.036
Patient choices (#)†	5.400	5.633	5.497	0.828	0.556	0.795	0.788

	Means				*p-values*		
	Children's Choice (1)	Delegation (2)	Caregivers' Choice (3)	All treatments (4)	Children's Choice vs. Delegation (5)	Children's Choice vs. Caregivers' Choice (6)	Delegation vs. Caregivers' Choice (7)
Risky choices (#)†	3.331	3.334	3.075	0.283	0.801	0.424	0.203
Child's hypothetical choices							
Time: switching point	5.219	4.634	5.182	0.538	0.411	0.927	0.263
Time: patient choices (#)	3.165	3.800	3.272	0.248	0.145	0.735	0.274
Risk: switching point	5.729	5.050	4.409	0.094	0.185	0.035	0.286
Risk: safe choices (#)	5.176	5.178	5.641	0.516	0.977	0.338	0.274
Competition (%)	10.87	9.57	17.48	0.160	0.715	0.189	0.030
Money shared (%)	46.55	47.59	48.13	0.401	0.296	0.302	0.725
Observations	183	578	195				

Notes: Columns (1)–(3) report means by treatment. Columns (4)–(7) report p-values. Column (4) is from an F-test of the joint significance of the treatment indicators in an OLS regression of each variable on these indicators, with robust standard errors. Columns (5)–(7) compare pairs of treatments using two-sample tests of proportions for binary variables and two-tailed Mann–Whitney tests for all other variables. "Decision maker" refers to the adult who made the decisions in the experiment, and "Caregiver" to the child's primary caregiver as recorded in the RPD. "Child's hypothetical choices" are the choices children made in the non-incentivized hypothetical version of each task; variables are defined as in Table 1. The RPD is the RIECE Panel Data, the panel from which the sample was drawn.
† Variables from previous waves of the RPD; the number of observations varies with the wave in which each variable was collected.

Appendix C. Multiple Hypothesis Testing

We report the effect of delegation on several outcomes in each of four decision domains. Because our central claim is selective – delegation shifts intertemporal choice and leaves the remaining three domains unchanged – its credibility rests on showing that the time-domain results are not an artefact of multiplicity. This appendix reports familywise and false-discovery-rate corrections for every hypothesis we test in the paper.

Our primary method is the Romano-Wolf (2005) stepdown procedure, which controls the family-wise error rate and, because it bootstraps the joint distribution of the test statistics, accounts for the correlation among outcomes rather than assuming independence. We use 5,000 bootstrap replications and the implementation of Clarke, Romano and Wolf (2020). As robustness we report Holm–Bonferroni, and Anderson's (2008) two-stage sharpened q-values, which control the false discovery rate rather than the family-wise error rate. Unadjusted p-values are reported alongside adjusted ones throughout.

We test the average effect of delegation in each of the four domains: time, risk, competition, prosocial. Every model regresses the outcome on the Delegated indicator and the controls used in the main specifications (Primary, Math, Literacy and IQ), with robust standard errors, on the pooled sample of children in the *Children's Choice* treatment and children whose caregiver chose to delegate.

Table C.1 reports the primary layer: one outcome per domain, four hypotheses. We exclude the two switching points from this layer for two reasons. First, the switching point is defined only for children who switch, and switching is itself an outcome of treatment, so conditioning on it selects on a post-treatment variable. Second, for a child who switches once, the switching point and the number of patient (safe) choices are the same statistic up to a monotone transformation, so entering both would place a near-duplicate hypothesis in the family. Table C.2 reports the disaggregated layer with all eight outcomes. The Holm–Bonferroni column assumes independence and is reported only as a conservative floor.

Binary outcomes (delaying payment at least once, choosing the safe option at least once, and competing) are estimated as linear probability models rather than probit models as in Tables 3-5. With base rates close to the boundary, such as 6.6% for competition and 92.9% for the safe option in the *Children's Choice* treatment, the bootstrap distribution of a probit test statistic has a large mass in the tails, which inflates the distribution of the maximum statistic and mechanically

penalizes every hypothesis in the family. The linear probability model is stable at these base rates, and its coefficients are directly comparable to the average marginal effects reported in the main text.

Table C.1: Multiple hypothesis testing, primary outcomes

			p-values				
	Coefficient	N	Unadjusted	Romano–Wolf	Holm	Sharpened q	Estimator
Time							
Patient choices (#)	1.004 (0.344)	572	0.003	0.019	0.014	0.014	OLS
Risk							
Safe choices (#)	−0.204 (0.335)	574	0.542	0.794	1.000	0.375	OLS
Competitive							
Competition (%)	0.038 (0.026)	574	0.136	0.366	0.408	0.205	LPM
Prosocial							
Money shared (%)	0.001 (0.008)	574	0.845	0.846	1.000	0.423	OLS

Notes: Each row is a separate regression of the outcome on Delegated, controlling for primary-school enrollment, math and literacy test scores, and IQ, on the sample of children in the *Children's Choice* treatment and children whose caregivers delegated the decision to them. Robust standard errors in parentheses. Competition is estimated as a linear probability model (LPM). The family comprises the four hypotheses in the table. "Unadjusted" is the conventional p-value; "Romano–Wolf" is the Romano–Wolf (2005) stepdown adjusted p-value, computed with 5,000 bootstrap replications (Clarke, Romano, and Wolf, 2020); "Holm" is the Holm–Bonferroni adjusted p-value; and "Sharpened q" is Anderson's (2008) two-stage sharpened q-value, which controls the false discovery rate. N is the number of observations.

Table C.2: Multiple hypothesis testing, all outcomes

			p-values				
	Coefficient	N	Unadjusted	Romano–Wolf	Holm	Sharpened q	Estimator
Time							
Delays payment at least once (%)	0.063 (0.046)	572	0.174	0.492	0.697	0.175	LPM
Switching point	−1.713 (0.512)	257	0.001	0.011	0.006	0.007	OLS
Patient choices (#)	1.004 (0.344)	572	0.003	0.028	0.024	0.013	OLS
Risk							
Safe option at least once (%)	−0.053 (0.026)	574	0.040	0.203	0.239	0.080	LPM
Switching point	−0.027 (0.439)	363	0.951	0.974	1.000	0.387	OLS
Safe choices (#)	−0.204 (0.335)	574	0.542	0.866	1.000	0.375	OLS
Competitive							
Competition (%)	0.038 (0.026)	574	0.136	0.492	0.681	0.171	LPM
Prosocial							
Money shared (%)	0.001 (0.008)	574	0.845	0.974	1.000	0.387	OLS

Notes: Specification as in Table C.1. The family comprises the eight hypotheses in the table. Binary outcomes are estimated as linear probability models (LPM). In the switching-point rows, the sample is restricted to children who switch exactly once. Adjusted p-values and q-values are defined as in Table C.1 and computed within this family.

Both time-domain results are robust to the correction at conventional levels, in both the primary and the disaggregated analysis, and under either correction procedure. Across all eight outcomes the switching point retains an adjusted p-value of 0.011 and the number of patient choices 0.028 -equal to 0.019 when we consider the four primary outcomes-. Both time results also fall well below conventional thresholds under the false-discovery-rate criterion (q = 0.007 and 0.013).

In line with the conclusions in the main text, no other hypothesis survives. The extensive margin of risk-taking, which is marginal before correction (p = 0.040), rises to 0.203, consistent with our interpretation in Section 4 that the difference likely reflects inconsistent choice at the dominated end of the list rather than risk attitudes. The competition and prosocial outcomes are not significant before correction and remain far from significance after it. The smallest effects the

design could detect with 80% power at the 5% level are 0.26 standard deviations for the number of safe choices, 0.29 for the probability of competing, and 0.25 for the share of money given to the other child. We can therefore rule out moderate effects of delegation in these three domains, but not small ones.

The correction therefore reproduces the domain-specific pattern reported in the main text: every result we interpret survives, and every result we decline to interpret does not.

Appendix D. Children's decisions in Non-delegated and *Caregivers' Choice* treatment

Table D.1 reports the choices of children whose caregivers chose not to delegate and of children in the *Caregivers' Choice* treatment, whose caregivers decided for them in Stage 1. For comparison, it also reports the choices of children in the *Children's Choice* treatment and of delegated children. All choices reported here are made in Stage 2, when every child decides individually regardless of treatment. As noted in the main text, the non-delegated group is small (only 61 children), which leaves the comparisons involving this group underpowered. We therefore report them for completeness and do not draw inferences from them.

Table D.1: Choices of non-delegated children and Caregiver's Choice treatments

	Time			*Risk*			*Competitive*	*Prosocial*
	Delays payment at least once (%)	Switching point	Patient choices (#)	Safe option at least once (%)	Switching point	Safe choices (#)	Competition (%)	Money shared (%)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Children's Choice	54.64	5.761	2.841	92.90	5.051	5.486	6.56	46.78
Delegated	59.03	4.229	3.699	85.69	4.494	5.456	9.86	46.85
Non-delegated	66.67	3.538	4.000	90.00	4.412	5.733	19.35	45.16
Caregivers' Choice	67.96	5.175	3.854	90.22	4.091	6.039	8.70	47.56
p-values								
Non-delegated vs. Children's Choice	[0.218]	[0.043]	[0.124]	[0.577]	[0.486]	[0.819]	[0.018]	[0.158]
Non-delegated vs. Delegated	[0.408]	[0.338]	[0.553]	[0.509]	[0.924]	[0.775]	[0.093]	[0.153]
Caregivers' Choice vs. Children's Choice	[0.028]	[0.308]	[0.014]	[0.619]	[0.543]	[0.362]	[0.519]	[0.818]
Caregivers' Choice vs. Non-delegated	[0.894]	[0.036]	[0.798]	[0.832]	[0.635]	[0.678]	[0.106]	[0.141]
Observations								
Children's Choice	183	71	183	183	118	183	183	183
Delegated	517	236	517	517	312	517	517	517
Non-delegated	30	13	30	30	17	30	31	31
Caregivers' Choice	103	57	103	103	66	103	92	92

Notes: "Children's Choice" refers to children in the *Children's Choice* treatment; "Delegated" and "Non-delegated" refer to children in the *Delegation* treatment whose caregivers did and did not delegate the decision to them,

respectively; "Caregiver's Choice" refer to children in the *Caregiver's Choice* children. Non-delegated children are those whose caregiver chose not to delegate the decision in that domain (time and risk, or competitive and social), so their number differs across domains. All choices are made in Stage 2, when every child decides individually regardless of treatment. p-values in brackets are from two-sample tests of proportions in columns (1), (4), and (7), and from two-tailed Mann–Whitney tests in the remaining columns. Variables are defined as in Table 1 and 2. Observations differ in columns (2) and (5) because the switching point is defined only for children who switch exactly once.

Table D.1 shows that children in the *Caregivers' Choice* treatment and non-delegated children behave similarly in most domains. The only significant difference is that non-delegated children switch to the patient option earlier (3.538 vs. 5.175, $p = 0.036$). Compared with children in the *Children's Choice* treatment, children in the *Caregivers' Choice* treatment are more patient: they are more likely to delay payment at least once (67.96% vs. 54.64%, $p = 0.028$) and make more patient choices (3.854 vs. 2.841, $p = 0.014$). Non-delegated children also switch to the patient option earlier than children in the *Children's Choice* treatment (3.538 vs. 5.761, $p = 0.043$) and choose to compete more often (19.35% vs. 6.56%, $p = 0.018$). Finally, non-delegated and delegated children do not differ significantly in any domain (all $p > 0.09$). Apart from the higher competition rate of non-delegated children, we find no significant differences in the risk, competitive, and prosocial domains.

Appendix E. Distribution of time and risk preferences

Figure E.1 further explores time and risk choices for children in the *Children's Choice* treatment and those who are delegated in the *Delegation* treatment. In particular, among children who decide to switch at some point, the figure depicts the proportion switching at each decision level. We define four categories: i) children who choose to switch at the first opportunity, ii) those who switch between the second and fifth election, iii) those who switch between the sixth and ninth elections, and iv) those who switch only at the last chance. Earlier switching implies that children require less compensation to delay the payment in the time decision and are therefore more patient. In the risk task, earlier switching indicates that the child needs a lower fixed payment to choose the safe option.

Figure E.1. Distribution of switching points for time and risk preferences

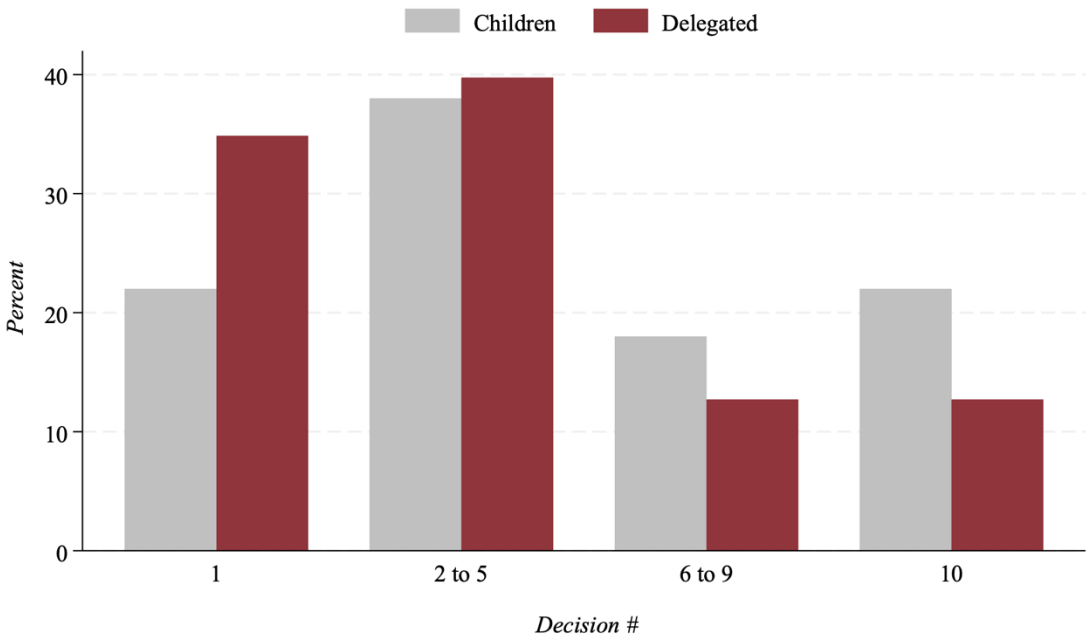


Figure E.1.a: Time switching point

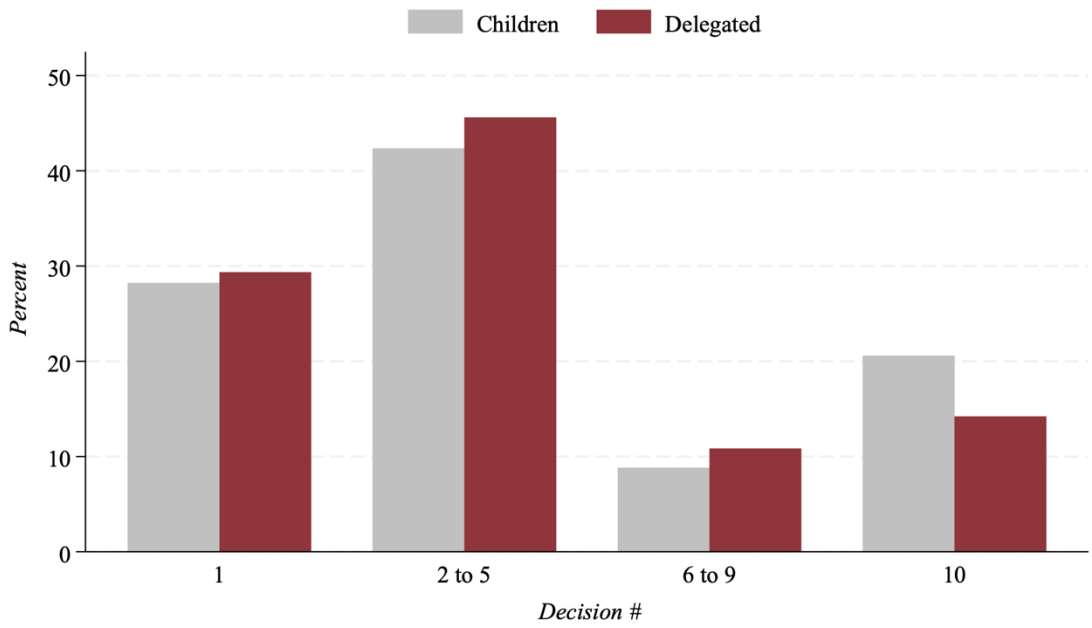


Figure E.1.b: Risk switching point

Notes: The sample is restricted to children who switch, and the switching point is conditional on consistency (children who switch only once).

Panel a in Figure E.1 shows clear differences in the distribution of switching points between the *Children's Choice* and *Delegation* treatments for time decisions. Among delegated children, switching to the patient option is more concentrated at earlier decision levels. For instance, the share of delegated children switching at the first opportunity is 34.85%, compared to 22% in the *Children's Choice*. Conversely, children in the *Children's Choice* are more likely to switch only at the last opportunity (22%) than delegated children (12%). Overall, these patterns indicate a leftward shift in the distribution of switching points when children are delegated. Formal tests of distributional differences confirm that the two distributions are statistically different (Pearson chi-squared test, $\chi^2 = 10.850$, $p = 0.020$).

Panel b in Figure E.1 shows that the distribution of switching points in the risk task is broadly similar across *Children's Choice* and Delegated. In particular, differences across early and late switching categories are less pronounced than those observed for time preferences. Formal tests of distributional differences confirm that the two distributions are not statistically different (Pearson chi-squared test, $\chi^2 = 3.941$, $p = 0.268$). Overall, these patterns suggest that, although delegation

affects the extensive margin of choosing the safe option, it is associated with only limited shifts in the distribution of switching points in the risk task.

Appendix F. Selection

A potential concern with our main estimates is that the 10% of caregivers did not delegate when given the opportunity and their children may differ systematically from those who did. If so, the delegated group would be positively selected on characteristics such as patience or self-control, and differences we attribute to delegation might instead reflect pre-existing differences in children's characteristics. We address this issue with two robustness checks following two complementary approaches: (i) the instrumental-variable strategy corrects for selection by isolating the random variation in delegation, and (ii) the trimming exercise bounds selection under a worst-case assumption.

The two robustness checks point to the same conclusion: selection does not drive our main findings.

F.1. Instrumental Variable Strategy

The first robustness check relies on an Instrumental Variable (IV) strategy. This strategy separates *being assigned to the Delegation treatment* (which is random) from *being delegated* (which depends on parental choice). Formally, the endogenous variable is whether the child was delegated to, and the instrument is the random assignment to the *Delegation* treatment.

Estimating this by two-stage least squares isolate the variation in delegation driven solely by random assignment, and therefore free of the caregiver's selection into delegating.

The instrument satisfies the three required conditions. *Relevance* holds by construction: roughly 90% of children are delegated to in the *Delegation* treatment, so assignment predicts delegation almost perfectly, as confirmed by the very large first-stage F-statistics reported in Table F.1. *Independence* follows from random assignment to the *Delegation* treatment, which by construction is unrelated to any child or household characteristic (Table B.1). The *exclusion restriction* requires that assignment affect children's behavior only through actual delegation. The channel that could threaten it runs through the non-delegated children, who learn that their caregiver could have delegated but chose to retain the decision –an experience children in *Children's Choice* do not have. If this altered their behavior, assignment would affect outcomes

through a channel other than delegation. The evidence presented in Table D.1 shows only limited support for such a channel, supporting the exclusion restriction here as well.

Because delegation is not possible in the *Children's Choice* treatment, non-compliance is one-sided: assignment to *Delegation* can fail to produce delegation (never-takers), but assignment to *Children's Choice* can never produce delegation (no always-takers exist by construction). This structural feature rules out defiers, so monotonicity holds automatically, and the IV estimator can be interpreted as a Local Average Treatment Effect (LATE; Angrist, Imbens, and Rubin, 1996)): the effect of delegation for compliers – children whose caregiver delegated *because* they were assigned to the *Delegation* treatment. Because non-compliance is one-sided, all delegated children are compliers, so the LATE coincides with, rather than merely approximates, the average effect of delegation among the delegated.

Table F.1 reports the 2SLS estimates for the eight main outcomes considered throughout the paper. The endogenous variable is *Delegated*, instrumented by assignment to the *Delegation* treatment. As in the main analysis, all models include controls for *Primary*, *Math*, *Literacy*, and *IQ*.

Table F.1: Instrumental-variable (2SLS) estimates

	Time			*Risk*			*Competitive*	*Prosocial*
	Delays payment at least once	Switching point	Patient choices (#)	Safe option at least once	Switching point	Safe choices (#)	Competition	Money shared
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	0.247*	5.452***	1.166	0.962***	5.093***	5.476***	0.018	0.405***
	(0.149)	(1.617)	(1.166)	(0.092)	(1.391)	(1.066)	(0.091)	(0.027)
Delegated	0.070	−1.833***	1.061***	−0.054**	−0.052	−0.184	0.046*	0.002
	(0.049)	(0.534)	(0.361)	(0.027)	(0.458)	(0.352)	(0.027)	(0.008)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
OLS estimate (Table C.2)	0.063	−1.713	1.004	−0.053	−0.027	−0.204	0.038	0.001
First-stage F-statistic	6413.2	3188.8	6413.2	6477.0	4640.1	6477.0	8296.3	8296.3
R-squared	0.024	0.048	0.019	0.008	0.002	0.001	0.007	0.051
Observations	600	268	600	602	379	602	596	596

Notes: 2SLS estimates. The endogenous variable is "Delegated," which equals one if the child's caregiver delegated the decision, instrumented by random assignment to the *Delegation* treatment. The sample comprises all children in the *Children's Choice* and *Delegation* treatments, including non-delegated children. Binary outcomes in columns (1), (4), and (7) are estimated as linear probability models. In columns (2) and (5), the sample is restricted to children who switch exactly once. Variables are defined as in Table 1 and 2. "Controls" include primary-school enrollment, math and literacy test scores, and IQ. "OLS estimate (Table C.2)" reports the corresponding OLS estimate of "Delegated" on the sample of children in *Children's Choice* and delegated children. Robust standard errors in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Once selection into delegation is removed, the effect on patience remains. Delegated children switch to the patient option earlier and make a larger number of patient choices. The 2SLS estimate on the number of patient choices (1.061) is close to the OLS estimate using actual delegation status reported in the main analysis (1.004, Table 3), indicating that selection into delegation does not meaningfully bias the main results. The IV estimates therefore reinforce *Result 1*.

F.2. Trimming robustness check

Finally, the trimming robustness check drops the least patient children from the *Children's Choice* treatment and asks whether the effect of delegation still holds. Suppose the 10% of caregivers who chose not to delegate did so because they had private information that their children were less patient or more impulsive. In that case, the Delegated group would be positively selected on patience, and our estimate would overstate the true effect of delegation. To bound this concern, we remove from the *Children's Choice* treatment the 10% of children with the least patient choices – the children whose exclusion would matter most if selection were operating – and re-estimate the effect against this selected group.[19] If the effect of delegation survives, selection cannot account for it, and the trimmed estimate provides a conservative lower bound on the true effect.

Because many children in *Children's Choice* are tied at the extreme value of these outcomes (0 patient choices; a switching point of 10), we implement the trim by randomly selecting the required number of children among those at the tied extreme, repeating this procedure 1,000 times.

Table F.2 reports the estimates on the trimmed sample, which comprises the Delegated children and the children in the *Children's Choice* treatment after removing the least patient 10% by their binding choices. As before, the two columns report the switching point, conditional on consistency, and the number of patient choices, and all models include controls for *Primary*, *Math*, *Literacy*, and *IQ*. Additionally, we report the average estimate across repetitions, together with the share of repetitions in which the effect remains significant at conventional levels.

[19] This is explicitly a worst-case bounding exercise, not a causal identification strategy: we test whether the effect could be an artifact of the most extreme conceivable selection, but do not claim that the trimmed sample is the correct comparison group. This bounding logic follows the trimming approach in the sample-selection literature (e.g., Lee, 2009) and relies on the same monotonicity condition established in Section F.1.

Table F.2: Trimming estimates

	Time	
	Switching point	Patient choices (#)
	(1)	(2)
Constant	4.738***	1.495
	(1.628)	(1.271)
Delegated	−1.248**	0.698*
	(0.522)	(0.361)
Controls	✓	✓
Untrimmed estimate (Table 3)	−1.713	1.004
SD of coefficient across replications	0.059	0.029
Replications significant at 5% (%)	100.0	38.8
R-squared (average)	0.038	0.014
Observations (average)	251	556

Notes: OLS estimates on a trimmed sample of children in the *Children's Choice* treatment and delegated children, from which the least patient 10% of children in *Children's Choice* are removed: those with the highest switching points in column (1) and the fewest patient choices in column (2). Because the cutoff for the least patient 10% coincides with the extreme value of each outcome (a switching point of 10 in column (1); 0 patient choices in column (2)), the children to remove are drawn at random among those tied at that value, and the procedure is repeated 1,000 times. Coefficients and R-squared are averages across replications, and standard errors in parentheses are averages of the within-replication robust standard errors; significance stars are based on these averages. "SD of coefficient across replications" is the standard deviation of the "Delegated" coefficient across the 1,000 draws, and "Replications significant at 5% (%)" is the share of replications in which it is significant at the 5% level. "Observations (average)" varies slightly with the children drawn. "Untrimmed estimate (Table 3)" reports the corresponding estimate on the full sample. In column (1), the sample is restricted to children who switch exactly once. "Controls" include primary-school enrollment, math and literacy test scores, and IQ. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Results in Table F.2 show that delegated children switch to the patient option earlier and, on average, make more patient choices than the trimmed *Children's Choice* sample, even under this extreme worst-case exclusion. The average estimate on the switching point (-1.248) remains significant at the 5% level in all 1,000 replications, regardless of which children are randomly excluded. The average estimate on the number of patient choices (0.698) is smaller in absolute value than the one obtained in the main analysis (1.004, Table 2) -as expected once the least patient children are removed from the comparison group- and remains significant at the 5% level in 38.8% of replications.

Taken together with the IV estimates in Section F.1, the effect of delegation on patience holds across the two robustness exercises and both outcome measures, with the number of patient choices showing a comparatively weaker, though still positive, effect under this most conservative test.