

Early Education, Preferences, and Decision-Making Abilities

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One way to advance our understanding of individual differences in decision-making is to study the development of children's decision-making. This paper studies the causal effects of daycare attendance on children's economic preferences and decision-making abilities, exploiting a lottery system that randomized admissions into oversubscribed daycare centers in Rio de Janeiro. Overall, daycare attendance had no effect on either economic preferences or decision-making abilities. It did increase, however, aversion to disadvantageous inequality (i.e., having less than one's peer). This increase is driven mostly by girls, a result that reproduces in a different study that randomized admissions into preschool education.

JEL codes: J13, I20, D91, J16, D81

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“What we are is what we have become, and the process through which we become what we are is likely to hide important clues.” (Brocas & Carrillo 2020)

1. Introduction

Behavioral economics has made it all the more important to understand individual differences in economic preferences and in decision-making abilities (see e.g., Taubinsky & Rees-Jones 2018). Such understanding may help us to better predict how different individuals will respond to a policy; to identify target groups for choice architecture and for nudges; and potentially inform the design of de-biasing policies.

Arguably, one way to advance our understanding of why some people are more patient, risk averse, altruistic, or better decision-makers than others is to study the development of children’s decision-making (see e.g. Sutter et al. 2019; Brocas & Carrillo 2020; List et al. 2021).² For one, individual differences that emerge in childhood may persist into adulthood.³ Childhood – and early childhood in particular – is a period of rapid, malleable development in which gaps in cognitive skills and noncognitive skills open up (e.g., Cunha et al. 2006) so it is quite possible that childhood circumstances may originate differences in economic preferences and in decision-making abilities. Cappellen et al. (2020), for example, show that disparities in early childhood education may give rise to differences in social preferences.

This paper studies the causal effects of daycare attendance on children’s economic preferences and decision-making abilities by combining a randomized experiment with incentivized experiments. In 2007, the local government of Rio de Janeiro, Brazil used a lottery system to determine admissions into public, free-of-charge daycare centers serving (mostly) children of poor families living in “*favelas*” (see Attanasio et al. 2022). Whenever a daycare center was oversubscribed (for a given age group), a draft was run. Applicants with lower numbers were invited to enroll while other applicants were put on a waiting list for further openings. We document that the “lottery winners” (hereafter, treatment) were significantly more likely to attend daycare than the “lottery losers” (hereafter, control).

² Harbaugh et al. (2001) makes a similar argument.

³ There is for example evidence that individuals with greater self-control in early childhood also exhibit greater self-control in adulthood (Casey et al. 2011; Benjamin et al. 2020).

We conducted four incentivized experiments with about 2,100 of these children – including both treatment and control children – nine years after the lottery assignment. These experiments were designed to measure both economic preferences as well as the quality of children’s decision-making. In three of them, participants made a series of binary choices. In the “toys task”, they chose between different toys. In the “risk task”, they chose between a risky option and a riskless option. In the “sharing task”, each of the two options paid a number of tokens to the participant and a number of tokens to another, anonymous child. Finally, a modified version of the marshmallow test (Mischel et al. 1989) was conducted. We study four measures of preferences: delayed gratification, risk aversion, and two measures of social preferences, children’s aversion to being at a relative disadvantage (“disadvantageous inequality”) and their aversion to being at a relative advantage (“advantageous inequality”).⁴

Four different measures of the quality of children’s decision-making are constructed. Two measure violations of transitivity, one in the toys task and the other in the sharing task. A third measures dominated choices in the risk task. Finally, a fourth measures violations of either monotonicity or transitivity in the risk task. We begin by showing that the different measures of decision-making abilities are correlated with each other and are correlated with a measure of intelligence, the Wechsler Intelligence Scale for Children-IV (Wechsler 2003). Interestingly, the associations between the different measures of decision-making abilities change little if we adjust for IQ, suggesting that the measures of decision-making abilities are in fact capturing the quality of participants’ decision-making, which is a different construct from intelligence.

Overall, we find that daycare attendance had no effect on economic preferences and no effect on decision-making abilities. There is, however, one exception: treatment children exhibited greater aversion to disadvantageous inequality than control children. In two of the sharing task’s trials, the participant had to choose between an equitable allocation that paid the same to each child and an allocation in which the participant received less than the other child. Treatment children were 5.5 percentage points more likely to choose the equitable option in these trials than the control children (p-value of 0.006). This effect remains significant even after adjusting for multiple hypothesis testing (p-value of 0.014). This result is also consistent with Cappelen et al. (2020),

⁴ Our focus on preferences for inequality is motivated by an emerging literature highlighting the importance of these preferences for support for redistributive policies (Kerschbamer & Müller 2020; Epper et al. 2020).

who find that children randomly assigned to participate in a preschool program exhibited greater inequality aversion than children assigned to a control group.

We also leverage the daycare experiment to study the origins of gender differences in social preferences. Men and women tend to behave differently when it comes to other-regarding preferences (e.g., Croson and Gneezy 2009). Are there biological explanations for why women tend to be more prosocial than men? Or do boys and girls develop different social preferences because they are raised differently from very early on (Beal 1994; Eagly 1997; Witt 1997)? The daycare experiment represented an exogenous change in the environment of boys and girls. Evidence of differential effects on boys and girls would provide support for the latter hypothesis.

The intent-to-treat results show that daycare attendance increased girls' aversion to disadvantageous inequality but had no effect on the aversion of boys. In the two trials of the sharing task mentioned before, treatment girls were 9.5 percentage points more likely to choose the equitable option than control girls (p-value of 0.001). In contrast, one cannot reject that treatment and control boys were equally likely to make such choices (p-value of 0.482). Even if we adjust for 16 hypothesis tests (8 outcomes \times 2 genders), the increase in the aversion of treatment girls to disadvantageous inequality remains statistically significant (p-value of 0.003).

To get a sense of the magnitude of these effects, we estimate the willingness to pay to avoid disadvantageous inequality, separately by gender. When they were behind, treatment girls were willing to pay twice as much as control girls – 0.26 tokens vs 0.13 tokens – to reduce the other child's payoff by 1 token. There is no effect of daycare attendance on the willingness to pay of boys (control and treatment boys were willing to pay about 0.18 and 0.19 tokens respectively to reduce the other child's payoff by 1 token).

As no pre-analysis plan was registered for this project, there is a legitimate concern about whether the effect of daycare attendance on the aversion of girls to disadvantageous inequality is spurious. Cappelen et al. (2020) provides an opportunity to investigate whether these differential effects by gender hold in a different sample. While these results were not included in the published paper, in their publicly-available code Cappelen et al. (2020) break their results by gender. We reproduce these results in the paper for the purposes of comparison. They show that the effects of preschool attendance on inequality aversion that Cappelen et al. (2020) document are mostly driven by girls—which is consistent with our findings in the experiment in Rio. Despite the relatively small size of the sample, preschool attendance led to a statistically significant increase

in the inequality aversion of girls, while one cannot reject the hypothesis that it had no effect on the inequality aversion of boys.

List et al. (2021) argue that one reason for studying children’s decision-making is that “children are active participants in their human capital production process”; their preferences and decision-making abilities may influence investments in their human capital with long term-consequences. We provide some support for this hypothesis. Our results show that aversion to disadvantageous inequality predicts scores in *Prova Rio*, standardized Math and Portuguese exams administered to 3rd grade students studying in public schools in Rio de Janeiro. These results are consistent with a model that predicts that children who dislike falling behind will put more effort and study harder to avoid scoring lower than her peers. The association between aversion to disadvantageous inequality and grades is robust to controlling for IQ and to controlling for all other measures of economic preferences and decision-making abilities.

This paper contributes to the literature on the causal effects of education on economic preferences and on decision-making abilities (Bettinger & Slonim 2006; Jakiela et al. 2015; Perez-Arce 2017; Andreoni et al. 2019; Banks et al. 2019; Cappelen et al. 2020; Chuan et al. 2022) and to a growing literature on children’s decision-making (see e.g. Harbaugh et al. 2001; Castillo et al. 2011; Castillo et al. 2018; Sutter et al. 2019; Castillo et al. 2019; Brocas & Carrillo 2020; Castillo et al. 2020;⁵; List et al. 2021). We are aware of only two other randomized experiments that have studied the causal effects of early education on children’s decision-making (Bettinger & Slonim 2006; Cappellen et al. 2020).⁶ One distinction of this study is to study the effect not only on economic preferences but also on decision-making abilities.

The rest of the paper is structured as follows. Section 2 describes the lottery system that generated the exogenous variation in daycare attendance, shows that control and treatment groups were balanced in terms of their characteristics, and that the lottery is a strong predictor of daycare attendance. Section 3 discusses the four incentivized experiments that were administered to measure children’s preferences. Section 4 estimates the effects of daycare attendance. Section 5 investigates whether the effects of daycare attendance on aversion to disadvantageous inequality

⁵ See also special issue of Journal of Economic Behavior & Organization “Understanding Cognition and Decision Making by Children.” Journal of Economic Behavior 179 (2020): 623-806.

⁶ Bettinger & Slonim (2006) leverage a lottery that allocated scholarships among families with children in kindergarten all the way to 8th grade. Alan & Ertac (2018) and Alan et al. (2019) study educational interventions specifically designed to increase patience and to increase grit.

vary by gender. Section 6 investigates whether the effects on inequality aversion vary by gender in a distinct sample. Section 7 shows that aversion to disadvantageous inequality is associated with scores in standardized Math and Portuguese exams. Section 8 speculates on why the daycare lottery may have affected the social preferences of girls but not of boys and concludes.

2. The Randomized Experiment

To study the causal effects of daycare attendance, we take advantage of a lottery system used by the local government of the city of Rio de Janeiro to determine admissions into daycare centers. In Brazil, preschool education, which is optional, is provided by local governments. Rio's local government runs free-of-charge public daycare centers which enroll children ages 0-4. The centers are open for 9.5 hours on weekdays. The majority of the families seeking to enroll their children in the free-of-charge public daycare centers are poor and live in *favelas*, Brazilian slums.⁷

A lottery system was used in 2008 to assign available slots among applicants, since demand was much larger than supply. The assignment was done separately by daycare center and by age group. Children who attended the center in the previous year were automatically enrolled. High-priority applicants and applicants with special needs were the next in line. If the number of remaining slots available (for that age group at the given center), S , was smaller than the number of applications for these slots, a separate lottery was run (for that age group and center). Random numbers were assigned and the S applicants with the lowest numbers were invited to enroll. The other applicants were put on a waiting list for further openings in the order of their lottery numbers.

We study the admissions process that took place at the end of 2007 for the 2008 academic year. There were 25,511 applicants for 11,640 slots. 947 of them were identified as high-priority and 660 as children with special needs. Lotteries were run to assign the remaining 10,033 slots among the other 23,904 applicants. While many children on the waiting list were eventually invited to enroll, we show that the lottery outcome is nevertheless a strong predictor of daycare attendance.

⁷ The mean and the median of monthly household income among applicant families were approximately \$440 and \$267 in September of 2007, respectively. The mean and the median of monthly household per capita income were about \$70 and \$110. By comparison, in 2010 the mean monthly household income for families living in the City of Rio de Janeiro was approximately \$1,938 and the median was \$1,000. Ninety eight percent of caregivers reported that they needed to enroll their children in daycare in order to work.

Attanasio et al. (2022) selected a sample of 4,350 applicants in 232 different daycare-center-by-age-group drafts to measure the impacts of attending these daycare centers.⁸ They conducted surveys in 2008 ($N = 3,776$), 2012 ($N = 1,462$)⁹, and 2015 ($N = 2,050$) and studied the effects on children’s cognitive function, anthropometrics, and behavior; on labor market outcomes of household members; and on the economic standing of the household (e.g., income and assets). See Attanasio et al. (2022) for further details about how the sample of 4,350 children was selected.

To study the effects of daycare attendance on economic preferences and on decision-making abilities, we surveyed 2,113 of the children from the sample in late 2016, early 2017 – 9 years after those selected in the lottery started attending daycare. Most of the children were at this time between 9 and 13 years old. While large, the rates of attrition are comparable to other long-run longitudinal surveys in poor countries.¹⁰ The attrition rates among the control children are 3-4 percentage points larger than the rates among treatment children (see Appendix Table 1). Appendix Table 2 shows however that, at least *in terms of observables*, there is no differential selective attrition between the treatment and control children.

It is worth noting the unusually challenging circumstances under which the study was conducted. Most participants lived in *favelas* in areas that were not infrequently of restricted access because they were controlled by drug-trafficking gangs or by militias. Families often lived in crowded spaces with multiple family members sharing one or two bedrooms. Tracking study participants was not any easier given that dwellings in these areas typically have no official addresses.

We start by documenting that pre-treatment variables are balanced for the sample of children analyzed in this paper. The first column of Table 1 shows means for “control children”, i.e., the children with higher lottery numbers who were put on the waiting list. The second column shows differences in means between the control and the “treatment children” (i.e., the children with the

⁸ We only use information from the original waiting list, before applicants had opportunity to accept or reject their placements (as opposed to using lottery status after some original winners already refused the offered slot, which was then offered to the first available person in the waiting list), thereby avoiding concerns about randomized list designs raised in Chaisemartin and Behagel (2020).

⁹ Because of budget constraints, the 2012 surveyed targeted only 60% of the original sample.

¹⁰ The original sample drawn based on the information from the applications consisted of 4,350 children, which would imply an attrition rate of 51.8%. The first survey, which was conducted in 2008, managed to survey just 3,776 of these children. That is, almost 15% of the original sample was lost in 6 months (the time between when applications were submitted and when the 2008 survey was conducted). Such attrition occurred mostly because the application data did not have accurate contact information of the applicant-families. Relative to the 2008 survey, the attrition rate was of 44%.

lower numbers who were invited to enroll). It reports results from regressions of the dependent variable listed in the row (e.g., an indicator for male) on an indicator for treatment assignment and on fixed effects specific to each childcare-center-by-age-group draft. The third column shows robust standard errors. There are very small differences between treatment and control.

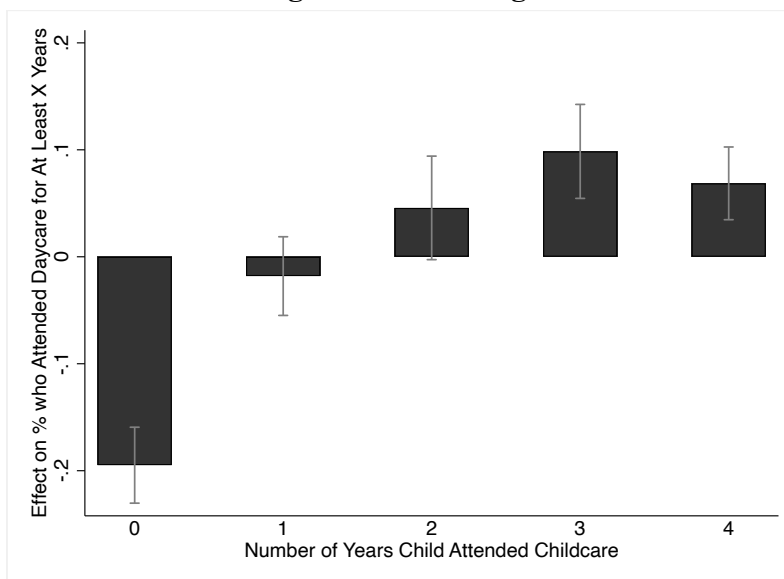
Table 1: Balance of Controls

	Mean among Control	Regression- Adjusted Difference	Robust Standard Errors
Male Child	0.52	0.01	0.02
White Child	0.29	0.04	0.02
Black Child	0.13	-0.02	0.01
Mixed Race Child	0.54	-0.02	0.02
Other Race Child	0.03	0.00	0.01
Birthweight (in kilos)	3.19	0.02	0.03
Birth Height (in cms)	49.26	0.26	0.20
Planned Birth	0.34	-0.02	0.02
First Born	0.42	-0.02	0.02
Age of the Mother at Birth	20.30	0.07	0.23
Prenatal Care	0.96	0.00	0.01
Natural Birth Delivery	0.69	-0.02	0.02
Premature Birth	0.12	0.02	0.02
Breastfed up to 6 Months	0.78	-0.04	0.02
Household per Capita Income	521.81	-54.32	35.86
Household Size	4.71	0.22	0.21
Age of Caregiver	29.83	-0.25	0.44
Caregiver can Read and Write	0.96	0.02	0.01
Caregiver Finished Middle School	0.68	0.03	0.02
Caregiver Graduated from High School	0.32	0.04	0.02
Caregiver has a College Degree	0.01	0.01	0.00

Notes: The table investigates whether the treatment and control children are comparable in terms of predetermined characteristics. The first column shows means among the control children. The second column reports the coefficient on the treatment indicator from regressions of the dependent variable listed in the row on the treatment indicator and on dummies specific for childcare center \times age group. The third column shows robust standard errors. $N = 2,113$.

Figure 1 shows estimates of the impact of winning the lottery on the probability of attending daycare for at least 0, 1, 2, 3, and 4 years.¹¹ Treatment children were 20 percentage points more likely to ever attend daycare than control children. Treatment assignment increased the probability of attending daycare for 3 years or more, for example, by 10 percentage points. Appendix Table 3 shows the corresponding regression estimates.

Figure 1: First Stage



Notes: The figure shows that treatment children were more likely to attend daycare than the control. The bars show intent-to-treat estimates of the effects of daycare attendance on the fractions of study participants attending daycare for at least 0, 1, 2, 3, and 4 years. The brackets show 95% confidence intervals. $N = 1,857$.

Children selected in the lottery were guaranteed a slot in 2008 in the daycare center that they had applied to, but enrollment was optional. Being placed on the waiting list also did not prevent control children from attending childcare, since they could apply for multiple daycare centers, be eventually called from the waiting list, attend a private daycare center, or enter the lottery in the following years. Therefore, a non-negligible fraction of the control children eventually attended daycare in 2008 or in subsequent years.

¹¹ The surveys conducted by Attanasio et al. (2021) in 2008, 2012, and 2015 collected detailed data on daycare attendance in each semester. The variable *years of daycare attendance* is 0 if a child never attended daycare, 1 if a child attended for 1 or 2 semesters, 2 if she attended for 3 or 4 semesters, 3 if she attended for 5 or 6 semesters, and 4 if she attended for more than 6 semesters. The variable is available for only 1,857 participants because some of the children who participated in the incentivized experiments were not interviewed in 2008, 2012, or 2015.

3. Incentivized Experiments

This section discusses the four different incentivized experiments that we conducted. They are explained below in the order they were administered.

Toys Tasks

In each trial of this task, the participant had to choose between two toys. There were 15 trials in total corresponding to all two-way comparisons between six different toys (rope, Rubik’s cube, flute, slinky, set of pick-up sticks, and yo-yo – all familiar to children in this setting). The task, adapted from Brocas et al. (2019), was designed to capture violations of transitivity.

Risk Task

In this task, the participant had to choose between a risky option and a riskless option. The task had 15 trials in total, consisting of all two-way, riskless-vs-risky comparisons between three different riskless options and five different risky options. The three riskless options paid either 3, 6, or 9 Brazilian quarters.¹² The five risky options paid a low or a high amount depending on the outcome of a coin toss: (i) 0 or 3 quarters; (ii) 0 or 9 quarters; (iii) 0 or 15 quarters; (iv) 3 or 6 quarters; or (v) 3 or 12 quarters. The interface of the risk and of the sharing tasks are shown in Appendix Figure 1 and Appendix Figure 2.

Sharing Task

In this task, the participant had to choose between two different allocations; each allocation paid a number of tokens to the participant and a number of tokens to another, anonymous child (the tokens could be exchanged for toys at the end of the survey). The task had 10 trials in total, consisting of all two-way comparisons between the following five allocations:¹³

¹² At the end of 2016, one US dollar was worth about 3.25 Brazilian Reals. Thus, a Brazilian quarter was worth about 7.7 cents of US dollar.

¹³ We followed Sheskin et al. (2014)’s lead when picking the sharing task’s trials. They define four different types of trials. In a “Costless DI Trial”, the participant can avoid a relative disadvantage at no cost to herself; in a “Costly DI Trial”, she has to reduce her own payoff in order to equalize payoffs. In contrast, in a “Costly AI Trial” she has to reduce her payoff in order to *enact* a relative advantage. In a “Costless AI Trial”, she can enact such advantage at no cost to herself. We designed one trial of each type: choice between (a) and (b) is a Costless DI Trial; choice between (a) and (c) is a Costly DI Trial; choice between (a) and (d) is a Costless AI Trial; and choice between (a) and (e) is a Costly AI Trial. In order to be able to measure violations of transitivity, we administered the ten trials that correspond to all two-way comparisons between these five allocations.

- (a) 3 for the participant and 3 for the other child
- (b) 3 for the participant and 6 for the other child
- (c) 4 for the participant and 6 for the other child
- (d) 3 for the participant and 1 for the other child
- (e) 2 for the participant and 0 for the other child.

Four trials are of particular interest because they isolate inequality aversion. In two trials, the participant chose between the equitable allocation in which the two children get the same and an allocation in which she received *less* than the other child – in particular, the trial in which she chose between (a) and (b) and the trial in which she chose between (a) and (c). We will refer to these two trials as the “disadvantageous inequality (DI) trials.” In other two trials, the participant chose between the equitable allocation and an allocation in which she received *more* than the other child – the trial in which she chose between (a) and (d) and the trial in which she chose between (a) and (e). We will refer to these two trials as the “advantageous inequality (AI) trials.”

Common Setup of Tasks Administered using a Tablet

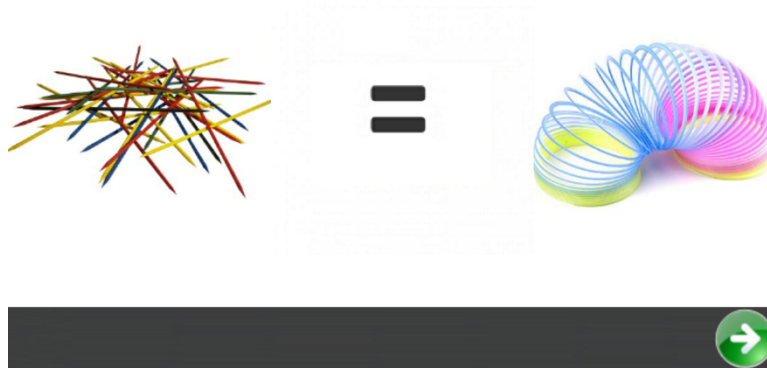
The three tasks discussed above, the toys task, the risk task, and the sharing task, were administered using a tablet. The use of a tablet was a deliberate choice intended to retain as much as possible, in the face of the challenging circumstances of the fieldwork, the controlled environment that is characteristic of laboratory experiments. It reduced the potential for enumerator errors and for discrepancies between them in how the survey was implemented.

The three tasks shared the same setup. First, in all three tasks the participant had to choose one of two options. Second, the tasks shared a similar interface; one option was shown on the left-hand side of the screen and the other on the right-hand side (it was randomized which option was shown on the left and which was shown on the right). To illustrate, Figure 2 shows a screenshot of one of the toys task’s trials (in this example, the participant could choose between a set of pick-up sticks and a slinky). The participant indicated her choice by clicking on her preferred option; she could also reveal indifference by clicking on the equal sign shown in the middle of the screen between the two options.¹⁴ Third, we randomized the order in which the different trials of a task were

¹⁴ Once the participant clicked on an option (or on the equal sign), a large green check mark showed over the option (or over the equal sign) to indicate her selection (see Appendix Figure 3). The participant then had to click on the green arrow at the bottom right of the screen to confirm her choice and move to the next trial.

presented. Fourth, at the end of the survey, one trial of each task was randomly selected and the participant's choice in the selected trial was implemented.¹⁵

Figure 2: Interface of Toys Task



Notes: The figure illustrates the interface of the toys task. The participant had to choose between the toy on the left and the toy on the right. She indicated her choice by clicking on her preferred option. She could also reveal indifference by clicking on the equal sign. After making her choice, she had to click on the green arrow at the bottom right to confirm her choice and move to the next trial.

Finally, all three tasks started with a tutorial. The tutorials consisted of a series of screens, each containing a different audio recording with instructions.¹⁶ While the audio played, the enumerator would point to the different parts of the screen that the audio was referring to (e.g., the equal sign).¹⁷ Next, the enumerator asked the participant a series of scripted questions designed to assess the participant's understanding. If the participant answered incorrectly, the enumerator would

¹⁵ In the toys task, the participant received the toy she chose in the selected trial (e.g., rope). In the sharing task, the participant was paid the number of tokens corresponding to the participant's allocation in the choice she had picked in the selected trial – the tokens could then be exchanged for toys. For example, if in the selected trial the participant's choice allocated X tokens to the participant, she got to choose X toys among the 5 remaining ones (i.e., a puzzle cube, a flute, a slinky, a set of pick-up sticks, and a yo-yo). In the risk task, if in the selected trial the participant had chosen the risky option a token was tossed to determine her payment.

¹⁶ The use of audio recordings was an attempt at making the instructions as uniform as possible, minimizing differences across enumerators.

¹⁷ The text of the audios was shown at the top of the screen. Enumerators were instructed to read the instructions to the participant if the audio could not be heard because of background noise.

explain what the correct answer was and why. The tutorials were followed by two practice trials.¹⁸ See Appendix for more on tutorials and practice trials.

Adaptation of Marshmallow Test

We measured self-control by conducting a modified version of the marshmallow test (Mischel et al. 1989) adapted for the circumstances under which the surveys were conducted. As mentioned before, most children lived in small houses in *favelas* with limited living space shared by a relatively large number of household members. We used a foldable child's tent to simulate a controlled environment. The enumerator set up the tent and asked the child to sit inside. A plate with one bonbon was then put in front of the participant, who was told that she would get a second bonbon if she waited for the enumerator to come back (without standing up or opening the tent).¹⁹ The child was given the second bonbon if she waited for 25 minutes.

Economic Preferences

To calculate risk tolerance, we first measured for each trial the standard deviation of the payoff of the choice selected by the participant. We then averaged over a participant's 15 trials. Two measures of social preferences were constructed. Our measure of aversion to *disadvantageous* inequality is the number of times in the two DI trials in which the participant chose the equitable allocation over an allocation in which she got *less* than the other child. Our measure of *advantageous* inequality aversion is the number of times in the two AI trials in which the participant chose the equitable allocation over an allocation in which she got *more* than the other child.²⁰ Finally, the measure of delayed gratification is the amount of time the participant managed to wait.

¹⁸ The enumerator held the tablet during the instructions. When it was time for the child to make her choices (including in the practice trials), the enumerator would pass the tablet to the child, who would then hold it in her hands.

¹⁹ The tent had a small hole in its top. The enumerator placed the tablet on the top of the tent so she could watch the plate from the top through the tablet's camera.

²⁰ In the Costless AI trial, the participant could choose between an equitable allocation that paid 3 to each child and an allocation that paid 3 to the child and 1 to the other child. In the Costly AI trial, the participant could choose between the equitable allocation that paid 3 to each child and an allocation that paid 2 to the child and 0 to the other child.

Decision-Making Abilities

We construct four different measures of decision-making abilities. Two measure violations of transitivity; one measures violations of transitivity in the toys task and the other violations in the sharing task. A preference relation satisfies transitivity if the decision-maker (i) finds option x at least as good as y and (ii) finds option y at least as good as option z , then (iii) she must find option x at least as good as option z . A non-transitive preference ordering implies a violation of the General Axiom of Revealed Preference (GARP). Choi et al. (2014) and Kariv and Silverman (2013) argue that consistency with GARP is a necessary condition for high-quality decision-making. This view draws on Afriat (1967), which shows that, if an individual's choices satisfy GARP, there exists a well-behaved utility function that can rationalize such choices.

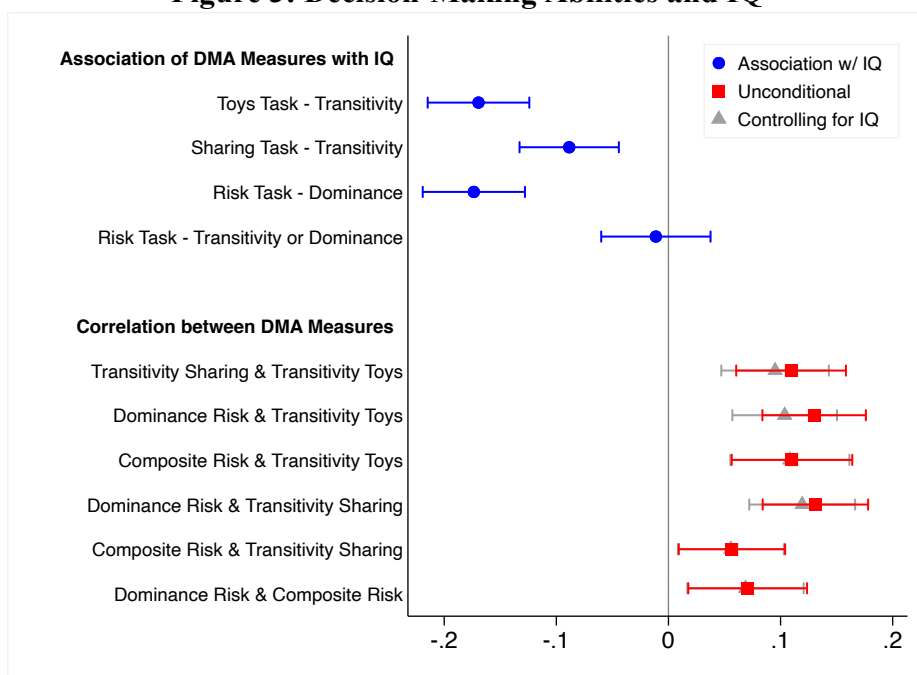
We measured violations of transitivity as follows. We picked a given set with three options (e.g., rope, flute, and yo-yo) and investigated if the three decisions involving comparisons of these three options (i.e., rope vs flute; rope vs yo-yo; and flute vs yo-yo) violated transitivity. In the toys task, there were 20 different three-options sets. We repeated the same procedure for each set and calculated the total number of violations over the 20 sets. In the sharing task, we calculated the total number of violations over the 10 different three-options sets.

Consistency with transitivity may be viewed as too low a standard of decision-making quality because it treats any set of choices with the same number of transitivity violations as equally high-quality. A more stringent requirement would also require monotonicity. The risk task provides two different measures of decision-making abilities. One is the total number of times in which the participant chose a dominated option (e.g., the participant chose the riskless option that paid 3 for sure over the risky option that paid either 3 or 12). Eight of the task's 15 trials included a dominated option.

Because the risk task did not involve all comparisons (e.g., there were no trials in which the participant chose between two riskless options), it is not possible to measure transitivity in the same way as in the toys and sharing tasks. Nevertheless, one can construct a measure that captures violations of either transitivity or monotonicity. To illustrate, imagine the participant chose (0,9) over (3,3) and chose (3,3) over (0,15) – where the two coordinates refer to the number of quarters the child would get depending on the outcome of the coin toss. While there was not a trial in which the participant had to choose between (0,9) and (0,15), we can still infer that, if she would have chosen (0,9) over (0,15), she would have satisfied transitivity but violated monotonicity. If she

would have chosen (0,15) over (0,9), she would have satisfied monotonicity but violated transitivity. There are in total 14 pairs of binary choices that would imply a violation of either transitivity or monotonicity. We calculate the total number of pairs of the participant’s choices that violated either one of them. Importantly, this measure of decision-making ability is constructed using the seven trials in which there was no dominated choice. In other words, the two measures of decision-making abilities constructed from the choices in the risk task use mutually exclusive sets of trials.

Figure 3: Decision-Making Abilities and IQ



Notes: The blue circles in the top panel show the association of the measures of decision-making abilities with IQ. The red squares in the bottom panel show how the different measures of decision-making abilities are correlated with each other. The gray triangles in the bottom panel show how these correlations are affected when one controls for IQ. “Composite Risk” is the measure that captures a violation of either transitivity or monotonicity in the risk task. Brackets show 95% confidence intervals. All variables were standardized to have a standard deviation of one. $N = 1,790$.

Because these measures are more unusual than the measures of preferences, we show some descriptive statistics in Figure 3.²¹ The blue circles in the top panel show the association of the measures of decision-making abilities with a measure of intelligence, the Wechsler Intelligence

²¹ Appendix Figure 5 documents the association of the measures of economic preferences with the different measures of decision-making abilities.

Scale for Children-IV (Wechsler 2003).²² The red squares in the bottom panel investigate whether the different measures of decision-making abilities are correlated with each other. The gray triangles in the bottom panel show what happens to these correlations when one controls for the IQ measure. The brackets show 95% confidence intervals.²³ Appendix Figure 4 shows a corresponding figure for economic preferences.

Figure 3 offers three take-aways. The first is that the measures of decision-making abilities are associated with IQ (with the exception of one): more intelligent children are less likely to violate transitivity and to violate dominance, as the blue circles in the top panel show. There is for example a 0.17 correlation between IQ and violations of transitivity in the toys task.

The second take-away is that the measures of decision-making abilities are associated with each other, as shown by the red squares in the bottom panel of the figure. Children who exhibited lower decision-making abilities in one task also exhibited lower decision-making abilities in a different task. For example, children who violated transitivity in the toys task were more likely to pick dominated choices in the risk task (correlation of 0.11).

The third take-away is that the measures of decision-making abilities continue to be associated with each other *even if we control for IQ*, as the gray triangles in the bottom panel show. In fact, most of these correlations change very little. Overall, these results suggest that the measures of decision-making abilities are in fact capturing the quality of participants' decision-making, which is a different construct from intelligence.

We proceed now to study the effects of daycare attendance on economic preferences and on decision-making abilities.

4. Effects of Daycare Attendance

Figure 4 shows the reduced-form effects of daycare attendance. The top panel shows the effects on economic preferences. The bottom panel shows the effects on the different measures of decision-making abilities. To facilitate the comparison, all measures were standardized to have a

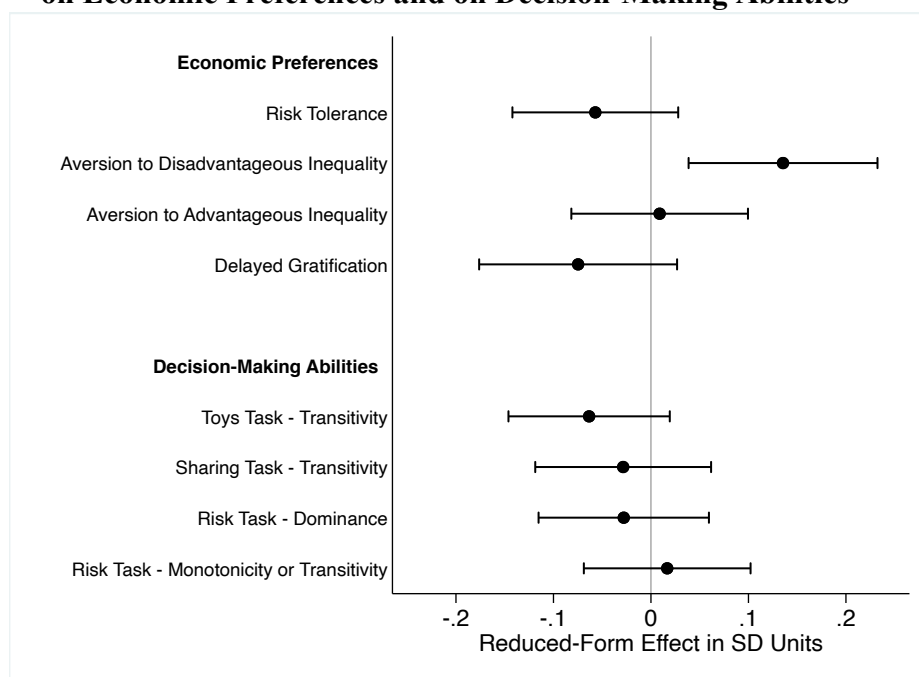
²² The scale, which was administered in 2015 (i.e., eight years after treatment children started attending daycare), aggregates distinct measures of verbal comprehension, perceptual reasoning, working memory, and processing speed. See Attanasio et al. (2022) for details.

²³ Appendix Figure 6 shows that the measures of economic preferences tend to be positively correlated with measures of family SES – family income, family's assets, or mother's education – and negatively correlated with the measures of decision-making abilities. However, these associations are often not statistically significant.

standard deviation of one. The vertical gray line marks no effect. The brackets show 95% confidence intervals.

Overall, daycare attendance had no effect on economic preferences and no effect on decision-making abilities. There is, however, one exception: daycare attendance increased aversion to disadvantageous inequality. In particular, winning the daycare lottery increased the aversion to receiving less than the other child by 0.13 standard deviations. Corresponding regression estimates are shown in Appendix Table 4.

Figure 4: Reduced-Form Effects of Daycare Attendance on Economic Preferences and on Decision-Making Abilities



Notes: The figure shows the reduced-form effects of daycare attendance on economic preferences (top panel) and on decision-making abilities (bottom panel). All measures were standardized to have a standard deviation of one. $N = 1,858$; $N = 2,113$ for all other measures.

There is an increasing recognition in economics that the common notion that nonsignificant results are non-informative is misguided; in some cases, nonsignificant results may be more informative than significant results (Abadie 2020). The informativeness of nonsignificant results depends in part on how wide confidence intervals are (Romer 2020). The upper bounds of the 95% confidence intervals permit us ruling out that attending daycare for one year *increased* risk tolerance by more than 0.04 standard deviations (SD); aversion to advantageous inequality by

more than 0.15 SD; and self-control by more than 0.04 SD. For the different measures of decision-making abilities, the lower bounds of the of the 95% confidence intervals permit ruling out that attending daycare for one year *improved* decision-making abilities by more than 0.22 SD (transitivity in toys task); 0.18 SD (transitivity in sharing task); 0.18 SD (dominated risk choices); and 0.11 SD (transitivity or monotonicity in risk task).²⁴ These null results are also consistent with Attanasio et al. (2022) who find that the intervention had no sustainable effects on cognitive function, including on executive function.

Notice the confidence intervals are in a way artificially inflated because the types of experimental measures studied are known to be noisy (Gillen et al. 2019). To illustrate, we take advantage that multiple trials were conducted in the toys task, risk task, and sharing task to estimate the amount of measurement error in the measures of decision-making abilities.²⁵ We estimate that the confidence intervals are from 20% to 25% larger because of measurement error.

Because Figure 4 estimates effects on eight different outcomes, there is naturally a concern about multiple hypothesis testing (List et al. 2019). We proceed therefore to calculate Romano-Wolf p-values which adjust for multiple hypothesis testing (Romano and Wolf 2005a; 2005b). One advantage of this approach is that it takes into account the dependence structure of the test statistics. We find the effect of daycare attendance on aversion to disadvantageous inequality remains statistically significant even after one adjusts for multiple hypothesis testing (p-value of 0.014). The p-values for all other outcomes are above 0.63 – see Appendix Table 4.²⁶

It is also reassuring that the finding that daycare attendance increased aversion to disadvantageous inequality is consistent with the results of Cappelen et al. (2020), who studied the Chicago Heights Early Childhood Center (CHECC) project.²⁷ The project randomly assigned

²⁴ These estimates take into account that on average the treatment group attended daycare for 0.643 more years than the control group.

²⁵ To illustrate, take the measure of dominated choices in the risk task. There were eight trials that included a dominated choice. We randomly split these eight trials into two sets with 4 trials each. Let DMA_1 be the number of dominated risk choices in the first set and DMA_2 be the number of dominated risk choices in the second set. We then ran a regression of DMA_1 on DMA_2 . The coefficient on DMA_2 corresponds to the “true variance” in this metric of decision-making ability divided by the sum of the true variance and the variance of measurement error.

²⁶ The p-values for the other outcomes are: 0.682 (risk tolerance); 0.948 (aversion to advantageous inequality); 0.627 (delayed gratification); 0.629 (violations of transitivity in toys task); 0.948 (violations of transitivity in the sharing task); 0.948 (dominated choices in the risk task); and 0.948 (violations of transitivity or of monotonicity in the risk task).

²⁷ Andreoni et al. (2019) study the effects of CHECC on time preferences and find no effects. Recent work by Chuan et al. (2022) study the impacts of the program on a broader set of time preferences. They also investigate whether the impacts vary by curriculum.

households with children ages 3 or 4 living in the low-performing, urban school district of Chicago Heights to either no intervention or to preschool.²⁸ Cappelen et al. (2020) find that the treatment children implemented less inequality than the control children in two experiments, “the luck experiment” and “the merit experiment”, in which the participant decided whether and how to reallocate between two other children who had a different number of stickers from each other.²⁹

5. Differences by Gender

The daycare experiment provides an opportunity to study the origins of gender differences in social preferences. Men and women tend to behave differently when it comes to other-regarding preferences (e.g., Croson and Gneezy 2009). At the same time, there is an increasing interest in understanding the origins of the gender differences in preferences (Falk & Hermle 2018). Are there biological underpinnings of such differences? Or do boys and girls develop different social preferences because they are raised differently from very early on?

The daycare experiment represented an exogenous change in the environment of treatment boys and girls. If the program had differential effects on the social preferences of boys and girls, it would provide evidence that the gender differences in social preferences is at least partly explained by differences in how boys and girls are raised. Figure 5 investigates this hypothesis, estimating the effects on the aversion to disadvantageous inequality separately for boys (blue squares) and girls (pink circles).

The results indicate that the previously shown result that daycare attendance increased the aversion to disadvantageous inequality is driven mostly by girls.³⁰ The top panel of the figure shows the reduced-form effect on our measure of disadvantageous inequality aversion, namely the number of times in the two DI trials in which the participant chose the equitable allocation over the alternative allocations in which she received less than the other child. Treatment girls chose the equitable allocation about 0.19 times more than control girls (p-value of 0.001).^{31,32} This result

²⁸ There was a third study arm that was randomly assigned to a parenting program. We do not discuss this arm here because there is no comparable program in Rio’s context.

²⁹ The two experiments differed in terms of the source of inequality between the two other children. See Section 6.

³⁰ Appendix Table 5, Appendix Table 6, and Appendix Table 7 show the balance test and the first-stage estimates, separately by gender.

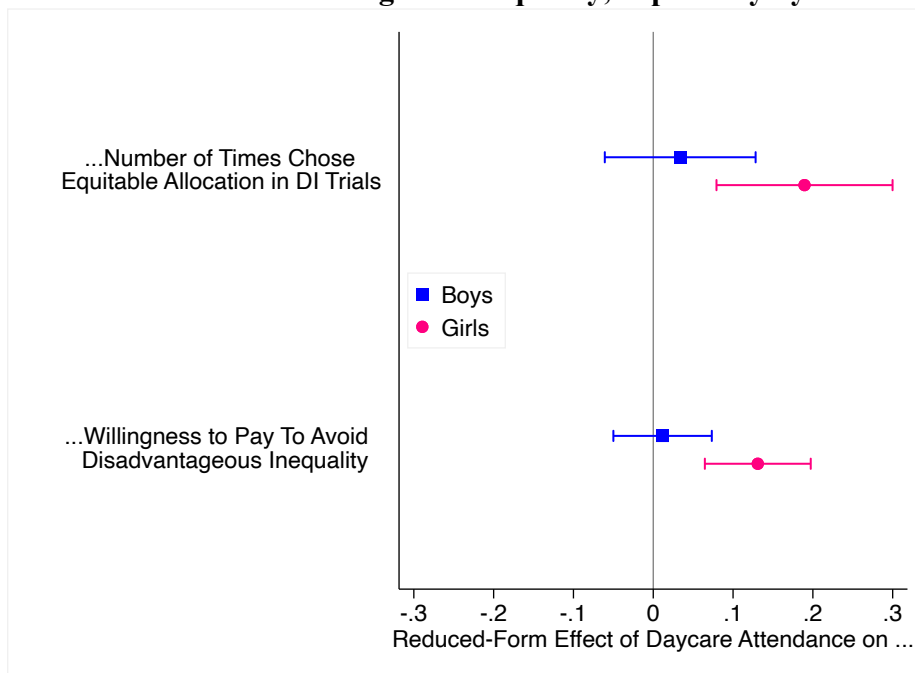
³¹ On average control girls chose in the two trials the equitable allocation 1.17 times.

³² Treatment girls were more likely to reject being at a relative disadvantage even in the “Costly DI Trial” when they had to reduce their own payoffs in order to enact equality. See Appendix Figure 7.

is particularly striking if we consider that a substantial fraction of the control group attended daycare. Moreover, the increase in the aversion of treatment girls to disadvantageous inequality remains statistically significant even if we adjust for 16 different hypothesis tests (8 outcomes \times 2 genders) (p-value of 0.003) – see Appendix Table 8.³³

In contrast, we find no effect on boys (p-value of 0.482). The effect on the “behindness aversion” of girls is large enough to close the gap that existed between control boys and control girls. On average, control boys chose the equitable allocation 0.14 times more than control girls (p-value of 0.006). In contrast, we cannot reject the hypothesis that treatment boys and treatment girls made this choice the same number of times (p-value of 0.667).

Figure 5: Reduced-Form Effect of Daycare Attendance on Aversion to Disadvantageous Inequality, Separately by Gender



Notes: The figure shows the reduced-form effect of daycare attendance on aversion to disadvantageous inequality, separately by gender. The blue squares show the effects on boys. The pink circles show the effect on girls. The top panel shows the effect on the number of times participants chose the equitable allocation in the two DI trials. The bottom panel shows the effect on the willingness to pay to avoid disadvantageous inequality, which is the number of tokens the participant was willing to give up to reduce the other child’s payoff by 1 token. Brackets show 95% confidence intervals. All regressions included dummies specific for each childcare center \times age group. $N = 2,113$.

³³ Appendix Table 8 also shows the effects on the other outcomes, separately by gender.

The magnitudes of the effects in the top panel are not easily interpretable. For this reason, the bottom panel of the figure studies the effect on the willingness to pay to avoid disadvantageous inequality. The willingness to pay is estimated using Fehr and Schmidt (1999)'s model of inequality aversion. In their model, the utility of an individual is given by:

$$u(x, y) = x - \beta_{DI} \max\{y - x, 0\} - \beta_{AI} \max\{x - y, 0\} \quad (1)$$

where x is the individual's payoff and y is the payoff of the other individual. The second term in this function measures the disutility from disadvantageous inequality, and β_{DI} the degree of aversion to disadvantageous inequality. The third term measures the disutility from advantageous inequality, and β_{AI} the degree of aversion to advantageous inequality.³⁴ All ten trials of the sharing task are used to estimate β_{DI} and β_{AI} . The willingness to pay to avoid disadvantageous inequality is given by:^{35,36}

³⁴ Fehr and Schmidt (1999) assume that $\beta_{DI} \geq \beta_{AI}$, reflecting that individuals dislike more inequality that is to their disadvantage. They also assume that $\beta_{AI} < 1$. If β_{AI} were equal to one, the individual would be willing to throw away one dollar in order to reduce her relative advantage in one dollar. For the specific purposes of their paper, Fehr and Schmidt (1999) further assume that $\beta_{AI} \geq 0$. They recognize however that some individuals may like to be better off than others. There is evidence for example that children often choose to be at a relative advantage (Rochat et al. 2009; LoBue et al. 2009; Blake & McAuliffe 2011; Smith et al. 2013; Sheskin et al. 2014; Qiu et al. 2017).

³⁵ The willingness to pay to avoid *advantageous* inequality is given by:

$$WTP_{AI} = - \left. \frac{\frac{\partial U}{\partial y}}{\frac{\partial U}{\partial x}} \right|_{x > y} = \frac{-\beta_{AI}}{1 - \beta_{AI}}$$

WTP_{AI} is the number of tokens the participant is willing to give up to *increase* the other child's payoff by 1 token.

³⁶ The utility gain of choosing option j over option k is:

$$U_j - U_k = \Delta x_{jk} - \beta_{DI} \Delta DI_{jk} - \beta_{AI} \Delta AI_{jk}$$

where $\Delta x_{jk} = x_j - x_k$; $\Delta DI_{jk} = \max\{y_j - x_j, 0\} - \max\{y_k - x_k, 0\}$; $\Delta AI_{jk} = \max\{x_j - y_j, 0\} - \max\{x_k - y_k, 0\}$.

We obtain estimates of β_{AI} and β_{DI} by estimating the following Probit model:

$$\Pr(\text{Choose } j \text{ over } k) = \Pr(U_j \geq U_k) = \Phi(\Delta x_{jk} - \beta_{DI} \Delta DI_{jk} - \beta_{AI} \Delta AI_{jk})$$

where $\Phi(\cdot)$ is the cumulative distributive function of the standard normal. Participants could indicate indifference between two options. In these cases, we randomized whether the choice was coded as having chosen option j or as having chosen option k . The fraction indicating indifference ranged from 0.43% to 4.16% depending on the trial.

$$WTP_{DI} = \frac{\frac{\partial U}{\partial y}}{\frac{\partial U}{\partial x}} \Bigg|_{x < y} = \frac{-\beta_{DI}}{1 + \beta_{DI}} \quad (2)$$

which corresponds to the number of tokens the participant is willing to give up to *reduce* the other child’s payoff by 1 token.

Treatment girls were willing to pay more to avoid disadvantageous inequality than control girls, as the bottom panel of Figure 5 shows (corresponding regression estimates are shown in Appendix Table 9). When they were behind, treatment girls were willing to pay twice as much as control girls – 0.26 tokens vs 0.13 tokens – to reduce the other child’s payoff by 1 token. We find no effect of daycare attendance on the willingness to pay of boys. Control and treatment boys were willing to pay about 0.18 and 0.19 tokens respectively to reduce the other child’s payoff by 1. As a reference, in the two disadvantageous inequality trials, the participant received either 3 tokens less than the other child or 2 tokens less than the other child.

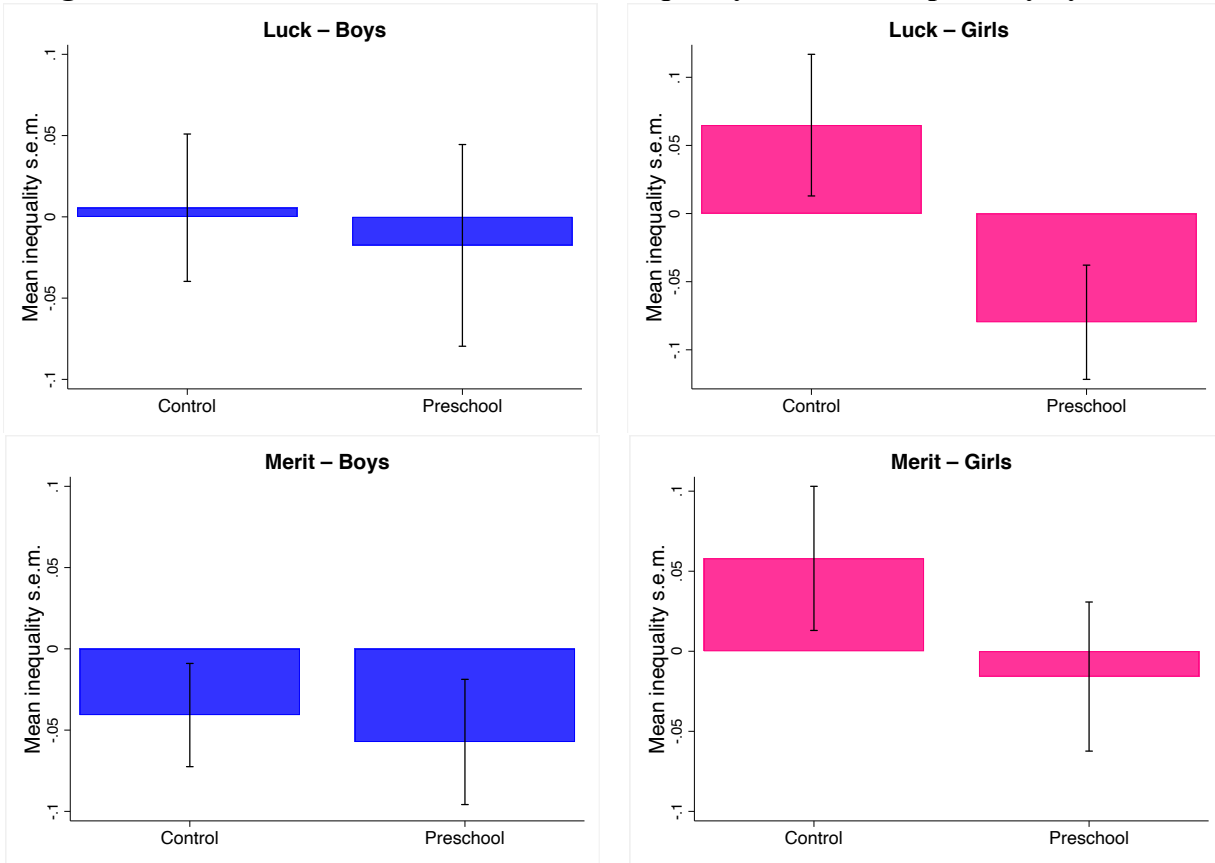
As no pre-analysis plan was registered for this project, there is a legitimate concern about whether the effect on the aversion of girls to disadvantageous inequality documented in Figure 5 is spurious. Cappelen et al. (2020) provides an opportunity to investigate whether these differential effects by gender hold in a different sample.

6. Differential Effects by Gender Hold in Cappelen et al. (2020)’s Sample

As discussed above, Cappelen et al. (2020) find that preschool-intervention children implemented less inequality than the no-intervention children in two experiments in which they had to decide whether and how to reallocate between two other children who had a different number of stickers from each other. These results are shown in the bottom panel of Cappelen et al. (2020)’s Figure 2.

The published version of Cappelen et al. (2020) does not show separate results by gender. However, in their publicly-available code they estimate how preschool attendance affected boys and girls. This section reproduces this particular set of results.

Figure 6: Effects of CHECC Preschool on Inequality Aversion, Separately by Gender



Notes: This figure shows averages for the control and preschool groups in Cappelen et al. (2020)’s inequality and merit experiments. The left column shows averages for boys. The right column shows averages for girls. The outcome, “Mean Inequality s.e.m.,” is the absolute difference between the number of stickers of each child. It is normalized by subtracting its mean (among all participants) and dividing it by 10, which is the total number of stickers. We followed all of Cappelen et al. (2020)’s conventions so this figure can be compared to the bottom panel of Cappelen et al. (2020)’s Figure 2. The brackets show 95% confidence intervals. $N = 69$ (Control Boys); 39 (Preschool Boys); 57 (Control Girls); and 44 (Preschool Girls).

Figure 6 shows figures that are comparable to the bottom panel of Cappelen et al. (2020)’s Figure 2. The only distinction is that separate graphs are shown for boys and girls. The top panel shows results for the luck experiment.³⁷ The bottom panel shows results for the merit experiment.³⁸ The left column shows results for boys while the right column shows results for girls. The variable shown on the vertical axis, mean inequality s.e.m., is the absolute difference between the number

³⁷ In the merit experiment, the participant was informed that the two other children had completed a memory task and that the child who had performed better in the task had earned 8 stickers while the other child had earned 2 stickers. The participant was then given the option to reallocate the stickers between these two children.

³⁸ In the luck experiment, the inequality was the result of luck rather than merit. The experimenter flipped a coin in front of the participant, which determined which of the two other children was the “lucky” child who would earn 10 stickers, and which was the “unlucky” child who would earn no stickers. The participant was then given the option to reallocate the stickers between the “unlucky” and the “lucky” child.

of stickers of each child. It is standardized by subtracting its mean (among all participants) and dividing it by 10, which is the total number of stickers. We followed all of Cappelen et al. (2020)'s conventions so Figure 5 can be easily compared to the bottom panel of the paper's Figure 2.

The figure indicates that the effects of the CHECC Preschool on social preferences documented in Cappelen et al. (2020) are mostly driven by girls. The figures on the right column show that preschool attendance increased the inequality aversion of girls. In contrast, the figures on the left column show that the inequality aversion of boys remained roughly the same. Appendix Table 10 shows regression results. The point estimates indicate that preschool girls implemented 36%-41% less inequality (depending on the specification) than control girls in the luck experiment. Combining the luck and merit experiments, we find that preschool girls implemented 35%-39% less inequality than control girls in the two experiments. Appendix Tables 11, 12, and 13 show that the controls are balanced both for the entire sample and if we break by gender.

There are at least three interesting take-aways from the comparison of the experiments in Chicago and in Rio. First, they provide some sort of external validity, indicating that the effects of early education on social preferences are not specific to a specific setting, program, or age group. Second, they indicate that the effects are long-lasting. Cappelen et al. (2020) find effects 3.5 year after the intervention started. We find effects 9 years after treatment children started attending daycare. Finally, they suggest that the aversion to inequality may apply not only to cases in which the decision-maker is an active participant but also to cases in which the decision-maker is not directly involved, as it was the case in Cappelen et al. (2020).

7. Aversion to Disadvantageous Inequality Predicts Field Behavior

List et al. (2021) argue that one reason for studying children's decision-making is that "children are active participants in their human capital production process"; their preferences and decision-making abilities may influence investments in their human capital with long term-consequences. In this section, we investigate this hypothesis by combining data from the incentivized experiments in Rio with administrative data on test scores. In particular, we study the relationship between the economic preference affected by daycare attendance – namely, aversion to disadvantageous

inequality – with scores in standardized Math and Portuguese tests, our *proxies* for human capital investments.³⁹

Intuitively, competitive children, who dislike losing or falling behind, may put extra effort and study harder in order to avoid such outcome.⁴⁰ We adapt Fehr and Schmidt (1999)’s model present above to formalize this intuition. Let the student’s utility of scoring g on the test to be:

$$g(e) - \beta_{DI} \max\{\bar{g} - g(e), 0\} - \beta_{AI} \max\{g(e) - \bar{g}, 0\} - c(e) \quad (3)$$

where $g(e)$ depends on the student’s level of effort e , which has a cost of $c(e)$. \bar{g} is the average test score. We assume that $g'(\cdot) > 0$, $g''(\cdot) < 0$, $c'(\cdot) > 0$, and $c''(\cdot) > 0$. The second term captures the student’s disutility of scoring lower in an exam than one’s peers while the third term captures the disutility of scoring higher than one’s peers.

The first-order condition is:

$$g'(e)[1 + \beta_{DI}I\{g(e) < \bar{g}\} - \beta_{AI}I\{g(e) > \bar{g}\}] - c'(e) = 0 \quad (4)$$

We have then that:

$$\frac{de}{d\beta_{DI}} = -\frac{\frac{\partial FOC}{\beta_{DI}}}{\frac{\partial FOC}{\partial e}} = -\frac{g'(e)I\{g(e) < \bar{g}\}}{g''(e)[1 + \beta_{DI}I\{g(e) < \bar{g}\} - \beta_{AI}I\{g(e) > \bar{g}\}] - c''(e)} \geq 0 \quad (5)$$

because $g'(\cdot) > 0$, $g''(\cdot) < 0$, $\beta_{DI} \geq 0$, $\beta_{AI} < 1$, and $c''(\cdot) > 0$.⁴¹ The model predicts that the level of effort – and consequently the test score – is a weakly increasing function of the aversion to disadvantageous inequality.⁴² This equation captures the intuition mentioned above that a

³⁹ Several studies have studied the relationship between children’s decision-making and field behavior and outcomes (see e.g., Castillo et al. 2011, Castillo et al. 2018 2019; Castillo et al. 2020; Chuan et al. 2022).

⁴⁰ Several studies have noticed that there may be a link between inequality aversion and competitiveness (e.g., Bartling et al. 2009; Balafoutas et al. 2012; Dasgupta et al. 2019). We note however that we use the term “competitive” in a slightly different way. In economics, “competitiveness” is typically used to refer to self-selection into competitive environments.

⁴¹ Fehr and Schmidt (1999) assume that $\beta_{AI} < 1$. If β_{AI} were equal to one, the individual would be willing to throw away one dollar in order to reduce her relative advantage in one dollar.

⁴² The model has a second prediction. The level of effort – and consequently the test score – is a weakly decreasing function of the aversion to advantageous inequality:

$$\frac{de}{d\beta_{AI}} = -\frac{\frac{\partial FOC}{\beta_{AI}}}{\frac{\partial FOC}{\partial e}} = -\frac{-g'(e)I\{g(e) > \bar{g}\}}{g''(e)[1 + \beta_{DI}I\{g(e) < \bar{g}\} - \beta_{AI}I\{g(e) > \bar{g}\}] - c''(e)} \leq 0$$

competitive student will put more effort in order to increase her grades and avoid falling behind her peers.

We test these predictions using data on the scores of a subsample of our study participants in Portuguese and Math exams of *Prova Rio*. Those are standardized exams administered to all 3rd grade students from public schools (run by the local government) who were in attendance on the examination date.^{43,44}

Table 2: Choices in Sharing Task Predict Test Scores

	<i>Math Grade</i>			<i>Portuguese Grade</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
Aversion to Disadvan. Inequality	0.25 (0.04)	0.12 (0.04)	0.11 (0.04)	0.25 (0.04)	0.14 (0.04)	0.13 (0.04)
Cognitive Function		0.44 (0.03)	0.41 (0.03)		0.37 (0.03)	0.34 (0.03)
Risk Tolerance			0.00 (0.02)			-0.01 (0.02)
Aversion to Advan. Inequality			0.05 (0.04)			0.09 (0.04)
Delayed Gratification			0.01 (0.00)			0.01 (0.00)
Violations of Transitivity in Toys Task			0.00 (0.01)			-0.01 (0.01)
Violations of Transitivity in Sharing Task			-0.01 (0.03)			-0.03 (0.03)
Dominated Risk Choices			-0.04 (0.02)			-0.02 (0.02)
Violations of Transitivity or Monotonicity in Risk Task			0.00 (0.02)			0.00 (0.02)

Notes: This table investigates whether aversion to disadvantageous inequality is associated with scores in standardized tests. The dependent variable in the first three columns is the student's standardized score in *Prova Rio*'s Math exam. In the last three columns, it is the student's standardized score in *Prova Rio*'s Portuguese exam. The measure of aversion to disadvantageous inequality is the number of times that the participant chose the equitable allocation in the two DI trials. The measure of cognitive function is the Wechsler Intelligence Scale for Children-IV. All regressions control for gender. $N = 1,006$.

⁴³ Exams are also administered to 7th graders but our cohort had not reached the 7th grade yet.

⁴⁴ Survey data were linked to the test scores data using the child's name and if necessary the child's date of birth and the names of the child's mother and father. 1,213 children of the 2,113 children who participated in our study were linked. For 1,006 of them, we also have a measure of IQ available. We suspect that many children could not be linked because they are either attending private schools or are attending public schools run by the state government rather than the local government.

Table 2 investigates whether the model prediction is supported by the data. The first three columns show results from regressions where the dependent variable is the student's standardized score in *Prova Rio*'s Math exam. In the last three columns, the dependent variable is the student's standardized score in *Prova Rio*'s Portuguese exam. Columns (1) and (4) study the unconditional association between the test score and the aversion to disadvantageous inequality (measured as the number of times in the two DI trials in which the participant chose the equitable allocation over the alternative allocations in which she received less than the other child). Naturally, these associations may be subject to confounds. To partly address this concern, columns (2) and (5) include a measure of IQ, the Wechsler Intelligence Scale for Children-IV (Wechsler 2003). The scale, which was administered in 2015 (i.e., eight years after treatment children started attending daycare), aggregates distinct measures of verbal comprehension, perceptual reasoning, working memory, and processing speed – see Attanasio et al. (2022) for details. Finally, columns (3) and (6) control for all other measures of economic preferences and decision-making abilities.

Table 2 supports the model prediction that the test scores should be a weakly increasing function of the aversion to disadvantageous inequality. Students with greater aversion to disadvantageous inequality scored higher both in the Math as well as in the Portuguese exam. Importantly, this result is robust to controlling for IQ and to controlling for all other measures of economic preferences and decision-making abilities. Except for aversion to disadvantageous inequality, none of the other measures of economic preferences and of decision-making abilities predict the scores in both exams. Dominated risk choices predicts the score in the Math exam but not in Portuguese. Aversion to advantageous inequality and delayed gratification predict the score in the Portuguese exam but do not predict the Math score.⁴⁵ So while the predictive power of behindness aversion supports the claim that children's preferences and decision-making abilities may influence investments in their human capital, there is limited evidence that this is the case for the other experimental measures.

⁴⁵ The finding that aversion to advantageous inequality is not predictive of the score in the Math exam is consistent with Fehr and Schmidt (1999)'s intuition that individuals dislike more inequality that is to their disadvantage than they dislike inequality that is to their advantage.

8. Discussion and Conclusion

We investigated the role that early education plays in children’s economic preferences and decision-making abilities. In 2007, the local government of the city of Rio de Janeiro used a lottery system to determine admissions into oversubscribed public free-of-charge daycare centers, which was exploited to identify intent-to-treat effects of daycare attendance. Nine years after those who were admitted started attending daycare, we surveyed about 2,100 of the applicant-children. Four incentivized experiments were conducted to measure both economic preferences as well as the quality of children’s decision-making.

Overall, daycare attendance affected neither economic preferences nor decision-making abilities. There was one exception however: daycare attendance did increase children’s aversion to disadvantageous inequality. More specifically, it increased the aversion of girls to being behind with no effect on the aversion of boys. Notably, this differential result by gender replicates in Cappelen et al. (2020)’ sample, which was randomized into a preschool program.

A natural question then is why the daycare lottery changed the social preferences of girls but not of boys. One possible explanation is that it changed girls’ perceptions of gender roles, including perceptions about how accommodating they are expected to be and how acceptable it is for girls to be more concerned with their own personal wellbeing and less concerned with the wellbeing of others.

But why would their perceptions have changed? One reason is that in Brazil families and daycare centers seem to model gender roles differently (see e.g. Santos 2017). Another reason is the empowerment that the mothers of treatment children seemed to have experienced.⁴⁶ Using data from the 2008 survey, Attanasio et al. (2022) show that, after gaining access to daycare services, mothers of treatment children reduced the amount of time they spent on childcare and increased their labor supply, both in the extensive and intensive margins.⁴⁷ The families of treatment children also had higher incomes, suggesting greater financial independence. Indeed, when asked whether they felt that things were under their control, mothers of treatment children reported experiencing

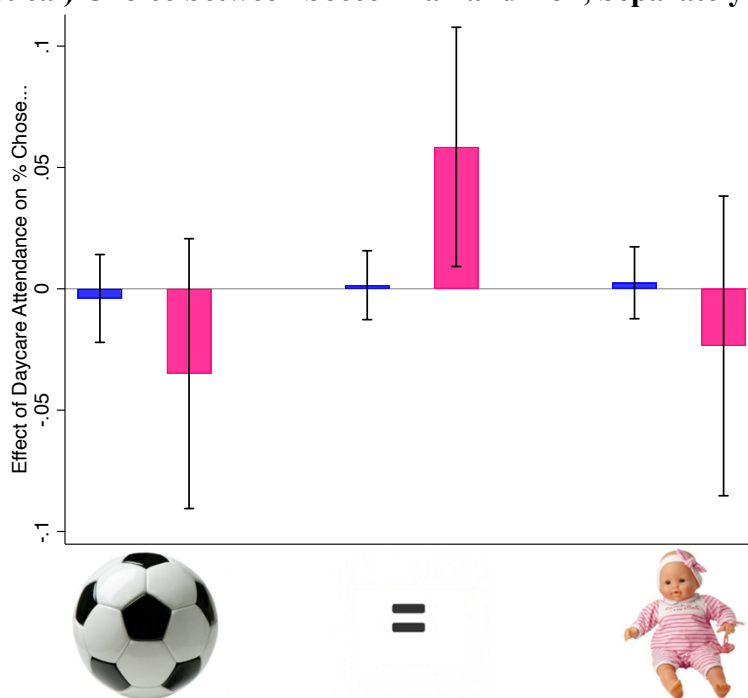
⁴⁶ Most of the study participants live in “favelas”, where machista gender values are influenced by the culture of local drug-trafficking gangs (Barker 2000; Zaluar 2010). *Machismo* is usually described as exaggerated masculinity. It manifests itself in the belief that females are inferior to males, and should be submissive to males.

⁴⁷ The 2008 survey did not ask questions specifically about the child’s mother. Instead, it asked questions about the child’s “main caregiver.” In more than 75 percent of the cases the main caregiver was the child’s mother.

this feeling more frequently than the mothers of control children did. It is plausible that such empowerment would touch the sons and the daughters of these women differently.

Unfortunately, no data were collected on the children’s perceptions of gender roles. Nevertheless, we can look at indirect evidence to try to assess whether such perceptions changed. In the toys task’s first practice trial, participants had to choose between a soccer ball and a doll. This decision is interesting because traditionally in Brazilian culture there are very clear gender expectations about these toys: boys are expected to play soccer and to never play with dolls; girls are expected to play with dolls and to be uninterested in soccer. Indeed, more than 98% of control boys chose the ball over the doll while less than 29% of the control girls made the same choice. Figure 7 shows the intent-to-treat effect of daycare attendance on the choices of boys and girls.

Figure 7: Reduced-Form Effect of Daycare Attendance on (Hypothetical) Choice between Soccer Ball and Doll, Separately by Gender



Notes: The figure shows the intent-to-treat effect of daycare attendance on children’s choices in the first practice trial of the toys task in which the participant had to choose between a soccer ball and a doll. The first set of two bars on the left shows the effect on the fraction who chose the ball. The middle set of bars shows the effect on the fraction who reported being indifferent between the ball and the doll. The last set of bars on the right shows the effect on the fraction choosing the doll. The blue bars show the effects on boys’ choices. The pink bars show the effect on girls’ choices. Brackets show 95% confidence intervals. The images shown above of the ball, of the equal sign representing indifference, and of the doll are the same images that children saw on

the tablet screen when they had to make their choices. All regressions included dummies specific for each childcare center \times age group. $N = 2,113$.

Treatment girls were about 6 percentage points (or 47%) more likely to be indifferent between the ball and the doll than control girls did. This is the best piece of evidence that the daycare lottery *may* have changed the perceptions of treatment girls of gender roles. It is suggestive at best, if for no other reason than that, according to the point estimate, the lottery *reduced* the fraction of girls choosing the ball (that said, one cannot reject that the lottery had no effect on the fraction of girls choosing the ball). It is interesting to notice that daycare attendance had no effect at all on the boys' preferences over these two toys, which is consistent with how treatment affected the aversion to disadvantageous inequality of girls but not of boys.

While the hypothesis that the effect of daycare attendance on girls' social preferences was caused by a change in their perceptions of gender roles is plausible enough, there is one reason to give pause. If the hypothesis were correct, we would expect the relationship between daycare attendance and girls' aversion to disadvantageous inequality to weaken when one controlled for the children's choices between the ball and the doll; for the amount of time their mothers spent on children; their mothers' employment; or their work hours. In practice, the relationship changes little. At the end of the day, we are not able to reach a definite conclusion as to why girls grew more averse to being behind. We hope future work can shed more light on the channels through which early education affects social preferences.

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Appendix Table 1: Attrition

	All		Boys		Girls	
	Application	2008 Survey	Application	2008 Survey	Application	2008 Survey
Treatment	0.04 [0.01]	0.03 [0.02]	0.03 [0.02]	0.03 [0.02]	0.03 [0.02]	0.04 [0.02]
Mean of Y among						
Control	0.47	0.52	0.53	0.53	0.52	0.51
Observations	4,349	3,776	2,038	1,964	1,876	1,810

Notes: The table investigates whether there were differential rates of attrition between treatment and control. The dependent variable is an indicator for whether the child was interviewed in the 2016-17 survey with the incentivized experiments. The odd columns measure attrition relative to the sample of 4,349 children which was drawn for the study based on the information available in the applications. The even columns measure attrition relative to the first survey conducted in 2008. Robust standard errors between brackets.

Appendix Table 2: Selective Attrition

	<i>Surveyed × Treatment</i>		<i>Surveyed</i>		<i>Treatment</i>		<i>Number of Observations</i>
	Coeff.	SE	Coeff.	SE	Coeff.	SE	
Male child	-0.03	[0.03]	0.02	[0.02]	0.04	[0.02]	3,774
White Child	0.03	[0.03]	-0.06	[0.02]	0.01	[0.02]	3,764
Black Child	4.44E-3	[0.02]	0.01	[0.02]	-0.01	[0.02]	3,764
Mixed Race Child	-0.05	[0.03]	0.04	[0.02]	0.02	[0.02]	3,764
Other Race Child	0.01	[0.01]	0.01	[0.01]	-0.01	[0.01]	3,764
Birthweight (in kilos)	-0.02	[0.04]	0.01	[0.03]	0.04	[0.03]	3,742
Birth Height (in cms)	0.45	[0.28]	0.10	[0.20]	-0.21	[0.20]	3,722
Planned Birth	-0.05	[0.03]	0.01	[0.02]	0.05	[0.02]	3,770
First Born	-0.01	[0.03]	-0.04	[0.02]	-0.01	[0.02]	3,764
Age of the Mother at Birth	0.14	[0.32]	0.05	[0.23]	0.01	[0.23]	3,767
Prenatal Care	0.01	[0.02]	0.02	[0.01]	-0.01	[0.01]	3,765
Natural Birth Delivery	0.01	[0.03]	-0.02	[0.02]	-0.03	[0.02]	3,768
Premature Birth	0.03	[0.02]	4.37E-4	[0.02]	-0.01	[0.02]	3,762
Breastfed up to 6 Months	-0.02	[0.03]	0.03	[0.02]	-0.01	[0.02]	3,770
Household per Capita Income	-240.97	[150.46]	63.62	[108.09]	199.95	[108.96]	3,562
Household Size	0.32	[0.28]	0.19	[0.20]	-0.11	[0.20]	3,592
Age of Caregiver	-0.09	[0.62]	0.97	[0.45]	-0.13	[0.45]	3,776
Caregiver can Read and Write	0.01	[0.01]	-4.88E-3	[0.01]	0.01	[0.01]	3,768
Caregiver Finished Middle School	-0.01	[0.03]	0.01	[0.02]	0.04	[0.02]	3,404
Caregiver Graduated from High School	0.02	[0.03]	-0.03	[0.02]	0.02	[0.02]	3,404
Caregiver has a College Degree	0.01	[0.01]	-0.02	[0.01]	-4.63E-3	[0.01]	3,404

Notes: This table investigates the hypothesis that the differential attrition between the treatment and control groups was random. Each row reports results from a separate regression of the dependent variable labeling the row on an indicator for being selected in the daycare lottery (“Treatment”), an indicator for having participated in the survey conducted to measure social preferences (“Surveyed”), an interaction of the two, and a constant. The first three set of columns reports the estimates while the last column gives the number of observations. The odd columns show the point estimates. The even columns show robust standard errors between brackets.

Appendix Table 3: First Stage

	<i>1 if Attended Daycare for ...</i>				
	0 Years	1 Year	2 Years	3 Years	4 Years
Treatment	-0.19 [0.02]	-0.02 [0.02]	0.05 [0.02]	0.10 [0.02]	0.07 [0.02]
Mean of Y among Control	0.22	0.17	0.22	0.26	0.13

Notes: The table shows that treatment children were more likely to attend daycare than the control. The dependent variables are indicators for participants attending daycare for 0, 1, 2, 3, and 4 years. $N = 1,857$.

Appendix Table 4: Intent-to-Treat Effects of Daycare Attendance

	Economic Preferences				Decision-Making Abilities			
	<i>Risk Tolerance</i>	<i>Aversion to Disadvan. Inequality</i>	<i>Aversion to Advan. Inequality</i>	<i>Delayed Gratification</i>	<i>Violations Transitivity (Toys)</i>	<i>Violations Transitivity (Sharing)</i>	<i>Dominated Risk Choices</i>	<i>Transitivity or Monotonicity (Risk)</i>
Treatment	-0.122 (0.092) [0.682]	0.109 (0.040) [0.014]	0.006 (0.032) [0.948]	-0.580 (0.400) [0.627]	-0.194 (0.128) [0.629]	-0.032 (0.052) [0.948]	-0.054 (0.086) [0.948]	0.038 (0.098) [0.948]
Mean Y Cntrl	5.309	1.239	1.522	19.820	2.858	0.744	2.485	1.149
Observations	2,113	2,113	2,113	1,858	2,113	2,113	2,113	2,113

Notes: This table estimates intent-to-treat effects of daycare attendance on economic preferences and on decision-making abilities. All regressions include dummies specific for each childcare center \times age group. Robust standard errors between parentheses. Romano-Wolf p-values which adjust for multiple hypothesis testing between brackets (Romano and Wolf 2005a; 2005b). Estimates are slightly different from Figure 4 in the paper because the measures are not standardized.

Appendix Table 5: Balance of Controls among Boys

	Mean among Control	Regression- Adjusted Difference	Robust Standard Errors
White Child	0.28	0.03	0.03
Black Child	0.13	-0.02	0.02
Mixed Race Child	0.57	-0.02	0.03
Other Race Child	0.03	4.9E-3	0.01
Birthweight (in kilos)	3.23	0.03	0.04
Birth Height (in cms)	49.48	0.41	0.25
Planned Birth	0.33	-0.01	0.03
First Born	0.45	-0.04	0.03
Age of the Mother at Birth	20.26	-0.16	0.33
Prenatal Care	0.96	-0.01	0.01
Natural Birth Delivery	0.69	-0.03	0.03
Premature Birth	0.12	0.04	0.02
Breastfed up to 6 Months	0.79	-0.06	0.03
Household per Capita Income	552.43	-61.19	43.88
Household Size	4.92	-0.08	0.32
Age of Caregiver	29.39	0.01	0.64
Caregiver can Read and Write	0.96	0.02	0.01
Caregiver Finished Middle School	0.69	0.04	0.03
Caregiver Graduated from High School	0.34	0.03	0.03
Caregiver has a College Degree	0.01	4.7E-3	0.01

Notes: The table investigates whether the treatment and control boys are comparable in terms of predetermined characteristics. The first column shows means among the control boys. The second column reports the coefficient on the treatment indicator from regressions of the dependent variable listed in the row on the treatment indicator and on dummies specific for childcare center \times age group. The third column shows robust standard errors. $N = 1,108$.

Appendix Table 6: Balance of Controls among Girls

	Mean among Control	Regression- Adjusted Difference	Robust Standard Errors
White Child	0.30	0.04	0.03
Black Child	0.14	1.72E-3	0.02
Mixed Race Child	0.52	-0.05	0.03
Other Race Child	0.03	2.99E-4	0.01
Birthweight (in kilos)	3.15	-0.01	0.04
Birth Height (in cms)	49.03	-0.15	0.33
Planned Birth	0.35	-0.02	0.03
First Born	0.40	0.01	0.03
Age of the Mother at Birth	20.34	0.36	0.35
Prenatal Care	0.95	0.01	0.01
Natural Birth Delivery	0.69	-0.04	0.03
Premature Birth	0.12	0.01	0.02
Breastfed up to 6 Months	0.78	-0.03	0.03
Household per Capita Income	490.16	-10.97	26.64
Household Size	4.49	0.63	0.36
Age of Caregiver	30.30	-0.58	0.71
Caregiver can Read and Write	0.97	0.02	0.01
Caregiver Finished Middle School	0.67	-1.6E-3	0.03
Caregiver Graduated from High School	0.30	0.05	0.03
Caregiver has a College Degree	2.4E-3	0.01	0.01

Notes: The table investigates whether the treatment and control girls are comparable in terms of predetermined characteristics. The first column shows means among the control girls. The second column reports the coefficient on the treatment indicator from regressions of the dependent variable listed in the row on the treatment indicator and on dummies specific for childcare center \times age group. The third column shows robust standard errors. $N = 1,005$.

Appendix Table 7: First Stage, Separately by Gender

	<i>1 if Attended Daycare for...</i>				
	0 Years	1 Year	2 Years	3 Years	4 Years
Boys * Treatment	-0.05 [0.03]	-0.01 [0.03]	0.05 [0.04]	0.01 [0.03]	-0.01 [0.04]
Treatment	-0.17 [0.02]	-0.01 [0.03]	0.02 [0.03]	0.09 [0.03]	0.07 [0.03]
Boys	0.05 [0.03]	-0.02 [0.03]	-0.04 [0.03]	0.02 [0.03]	-0.01 [0.02]
Mean of Y among Control Girls	0.20	0.18	0.24	0.25	0.13

Notes: The table shows that treatment boys and treatment girls were more likely to attend daycare than their control counterparts. The dependent variables are indicators for participants attending daycare for 0, 1, 2, 3, and 4 years. $N = 1,857$.

Appendix Table 8: Intent-to-Treat Effects of Daycare Attendance, Separately by Gender

	Economic Preferences				Decision-Making Abilities			
	<i>Risk Tolerance</i>	<i>Aversion to Disadvan. Inequality</i>	<i>Aversion to Advan. Inequality</i>	<i>Delayed Gratification</i>	<i>Violations Transitivity (Toys)</i>	<i>Violations Transitivity (Sharing)</i>	<i>Dominated Risk Choices</i>	<i>Transitivity or Monotonicity (Risk)</i>
Treatment × Boys	-0.056 (0.122) [0.908]	0.034 (0.048) [0.891]	0.017 (0.045) [0.908]	-1.463 (0.548) [0.074]	-0.334 (0.183) [0.429]	-0.127 (0.071) [0.429]	-0.144 (0.111) [0.712]	-0.177 (0.123) [0.712]
Treatment × Girls	-0.201 (0.133) [0.613]	0.190 (0.056) [0.003]	-0.002 (0.044) [0.989]	0.330 (0.564) [0.944]	-0.027 (0.184) [0.989]	0.071 (0.075) [0.864]	0.045 (0.119) [0.973]	0.278 (0.156) [0.387]
Boys	0.209 (0.137)	0.137 (0.049)	-0.156 (0.046)	1.104 (0.531)	-0.354 (0.189)	0.066 (0.074)	0.035 (0.118)	0.035 (0.156)
Mean <i>Y</i> Cntrl	5.309	1.239	1.522	19.820	2.858	0.744	2.485	1.149
<i>Observations</i>	2,113	2,113	2,113	1,858	2,113	2,113	2,113	2,113

Notes: This table estimates intent-to-treat effects of daycare attendance on economic preferences and on decision-making abilities, separately by gender. All regressions include dummies specific for each childcare center × age group. Robust standard errors between parentheses. Romano-Wolf p-values which adjust for multiple hypothesis testing between brackets (Romano and Wolf 2005a; 2005b).

Appendix Table 9: Willingness to Pay to Avoid Disadvantageous Inequality and Willingness to Pay to Avoid Advantageous Inequality

	<i>Disadvantageous Inequality</i>			<i>Advantageous Inequality</i>		
	Cntrl	Treat	Diff.	Cntrl	Treat	Diff.
Boys	0.18 (0.03)	0.19 (0.02)	0.01 (0.03)	-0.04 (0.02)	-0.01 (0.02)	0.03 (0.03)
Girls	0.13 (0.02)	0.26 (0.03)	0.13 (0.03)	0.05 (0.02)	0.06 (0.02)	0.01 (0.02)
Boys - Girls	0.05 (0.03)	-0.07 (0.03)	-0.12 (0.04)	-0.09 (0.03)	-0.07 (0.02)	0.01 (0.04)

Notes: This table estimates of the willingness to pay of boys and girls to avoid disadvantageous inequality and to avoid advantageous inequality, separately by treatment assignment. The willingness to pay to avoid disadvantageous inequality is the number of tokens the participant is willing to give up to *reduce* the other child's payoff by 1 token. The willingness to pay to avoid advantageous inequality is the number of tokens she is willing to give up to *increase* the other child's payoff by 1 token. The third and last columns show estimates of the treatment-control difference. The fifth row shows estimates of the boy-girl difference. All regressions include dummies specific for each childcare center \times age group. Bootstrapped standard errors between parentheses (500 replications). N Choices = 21,130. N Participants = 2,113.

Appendix Table 10: Effects of CHECC Preschool, Separately by Gender

	Luck				Merit and Luck			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Preschool × Boys		0.07 [0.11]		0.05 [0.11]		0.08 [0.09]		0.07 [0.09]
Preschool	-0.11 [0.05]	-0.14 [0.07]	-0.10 [0.05]	-0.13 [0.07]	-0.09 [0.04]	-0.13 [0.06]	-0.08 [0.04]	-0.12 [0.06]
Boys		-0.02 [0.07]		-0.01 [0.07]		-0.06 [0.06]		-0.05 [0.05]
<i>Demographic Controls?</i>	N	N	Y	Y	N	N	Y	Y
Mean Y among Control Girls	0.35	0.35	0.35	0.35	0.33	0.33	0.33	0.33
Observations	298	298	298	298	297	297	297	297

Notes: This table show regressions estimated using Cappelen et al. (2020)’s data. “Luck” is the chosen level of inequality in the luck experiment. “Merit and Luck” is a combined measure of the chosen levels of inequality in the luck and merit experiments. “Preschool” is an indicator for participants randomly assigned to the preschool arm. All regressions include an indicator for participants assigned to the parenting program arm and its interaction with gender (these coefficients are suppressed for ease of exposition). The demographic controls include age and dummies for black and Hispanic. Included but not reported are controls for the time of day when the child took part in the experiment and experimenter fixed effects. We followed all of Cappelen et al. (2020)’s conventions so the estimates can be compared to the results shown in Cappelen et al. (2020)’s Table 3. The odd columns, which do not break by gender, reproduce the results shown in columns (7)–(10) of Cappelen et al. (2020)’s Table 3. Robust standard errors between brackets.

Appendix Table 11: Balance of Controls in Cappelen et al. (2020)

	Mean among Control	Preschool vs Control Difference	Robust Standard Errors
Female	0.46	0.08	0.07
Age	7.56	0.08	0.08
Black	0.19	0.04	0.06
Hispanic	0.76	-0.07	0.06
White	0.05	0.01	0.03
Time of Experiment	9.97	-0.09	0.29

Notes: The table investigates whether control children and children assigned to the preschool arm in Cappelen et al. (2020) are comparable in terms of predetermined characteristics. The first column shows means among the control children. The second column reports the difference in means between the control and preschool children. The third column shows robust standard errors. “Time of Experiment” is the time of day when the child took part in the experiment using a 24-hour clock. The table is comparable to Cappelen et al. (2020)’s Table 1. $N = 302$.

Appendix Table 12: Balance of Controls among Boys in Cappelen et al. (2020)

	Mean among Control	Preschool vs Control Difference	Robust Standard Errors
Age	7.61	0.12	0.12
Black	0.16	-0.03	0.07
Hispanic	0.79	-0.02	0.08
White	0.06	-0.01	0.05
Time of Experiment	9.73	0.37	0.45

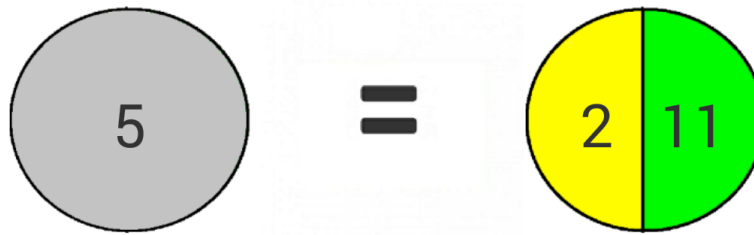
Notes: The table investigates whether control boys and boys assigned to the preschool arm in Cappelen et al. (2020) are comparable in terms of predetermined characteristics. The first column shows means among the control boys. The second column reports the difference in means between the control and preschool boys. The third column shows robust standard errors. “Time of Experiment” is the time of day when the child took part in the experiment using a 24-hour clock. The table is comparable to Cappelen et al. (2020)’s Table 1. $N = 154$.

Appendix Table 13: Balance of Controls among Girls in Cappelen et al. (2020)

	Mean among Control	Preschool vs Control Difference	Robust Standard Errors
Age	7.51	0.05	0.11
Black	0.22	0.09	0.09
Hispanic	0.73	-0.11	0.09
White	0.05	0.02	0.05
Time of Experiment	10.25	-0.57	0.37

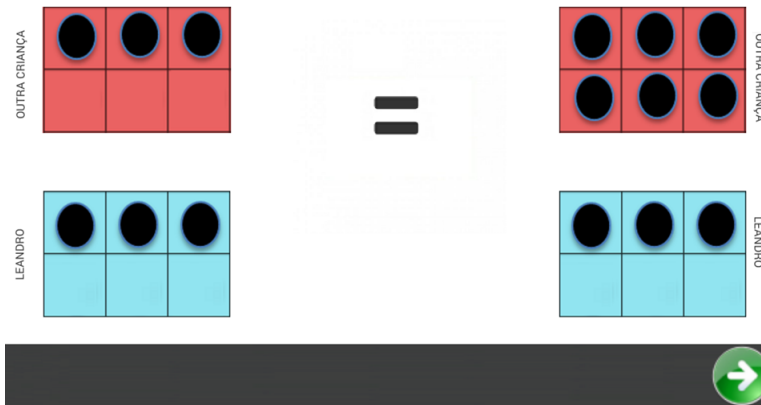
Notes: The table investigates whether control girls and girls assigned to the preschool arm in Cappelen et al. (2020) are comparable in terms of predetermined characteristics. The first column shows means among the control girls. The second column reports the difference in means between the control and preschool girls. The third column shows robust standard errors. “Time of Experiment” is the time of day when the child took part in the experiment using a 24-hour clock. The table is comparable to Cappelen et al. (2020)’s Table 1. $N = 148$.

Appendix Figure 1: Interface of Risk Task



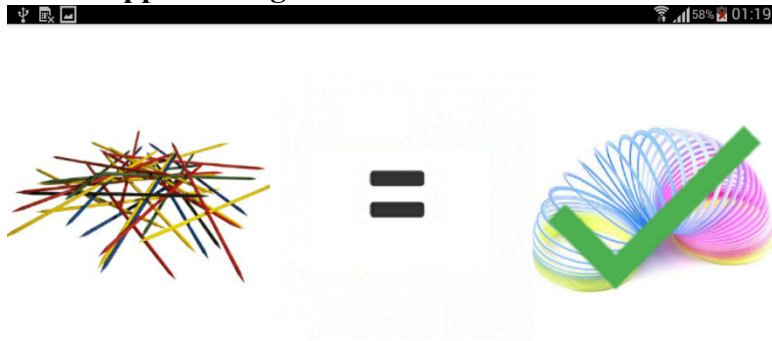
Notes: This figure shows the interface of the risk task. The riskless option was represented by a gray circle showing the number of Brazilian quarters that the participant would get if she chose the riskless option (in the example, 5 quarters). If the participant chose the risky option, a customized coin was tossed to determine the participant's payment. One side of the coin was yellow and the other green. The risky option was represented by a circle with two halves. The yellow half showed the participant's earnings if the coin landed on yellow (in the example, 2 Brazilian quarters). The green half showed her earnings if the coin landed on green (in the example, 11 quarters).

Appendix Figure 2: Interface of Sharing Task



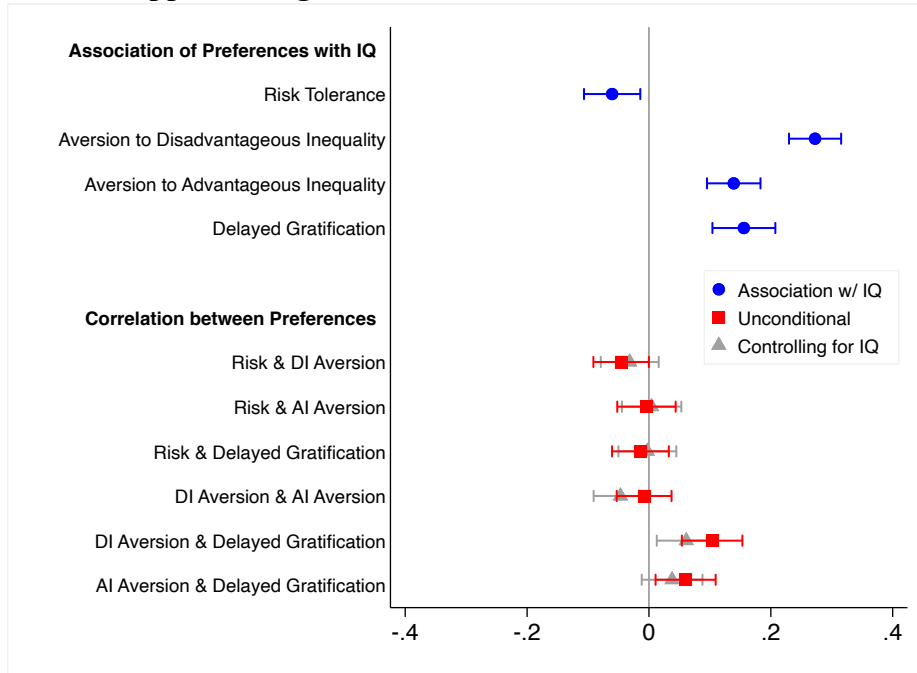
Notes: The figure shows the interface of the sharing task. The participant had to choose between the allocation on the left and the allocation on the right. The top, red rectangle showed the number of tokens the option paid to the other child. The bottom, blue rectangle showed the number of tokens the option paid to the participant. The rectangles were also labeled. The top, red rectangle was labeled “Other Child.” The bottom, blue rectangle was labeled with the participant’s first name.

Appendix Figure 3: Choice Confirmation



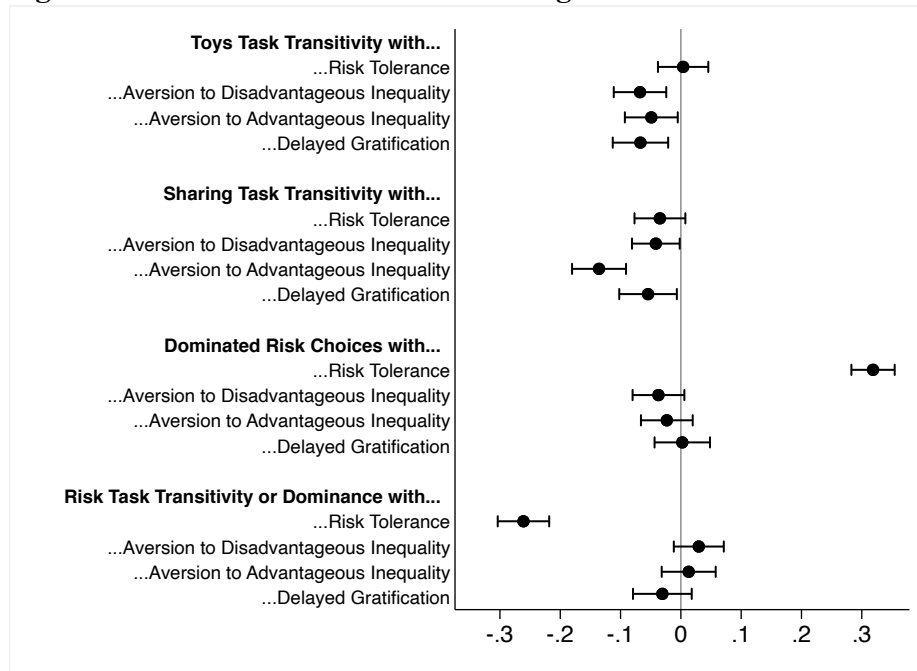
Notes: After the child clicked on an option, a green check mark appeared over the selected option (here, the slinky) to indicate that the choice had been registered.

Appendix Figure 4: Economic Preferences and IQ



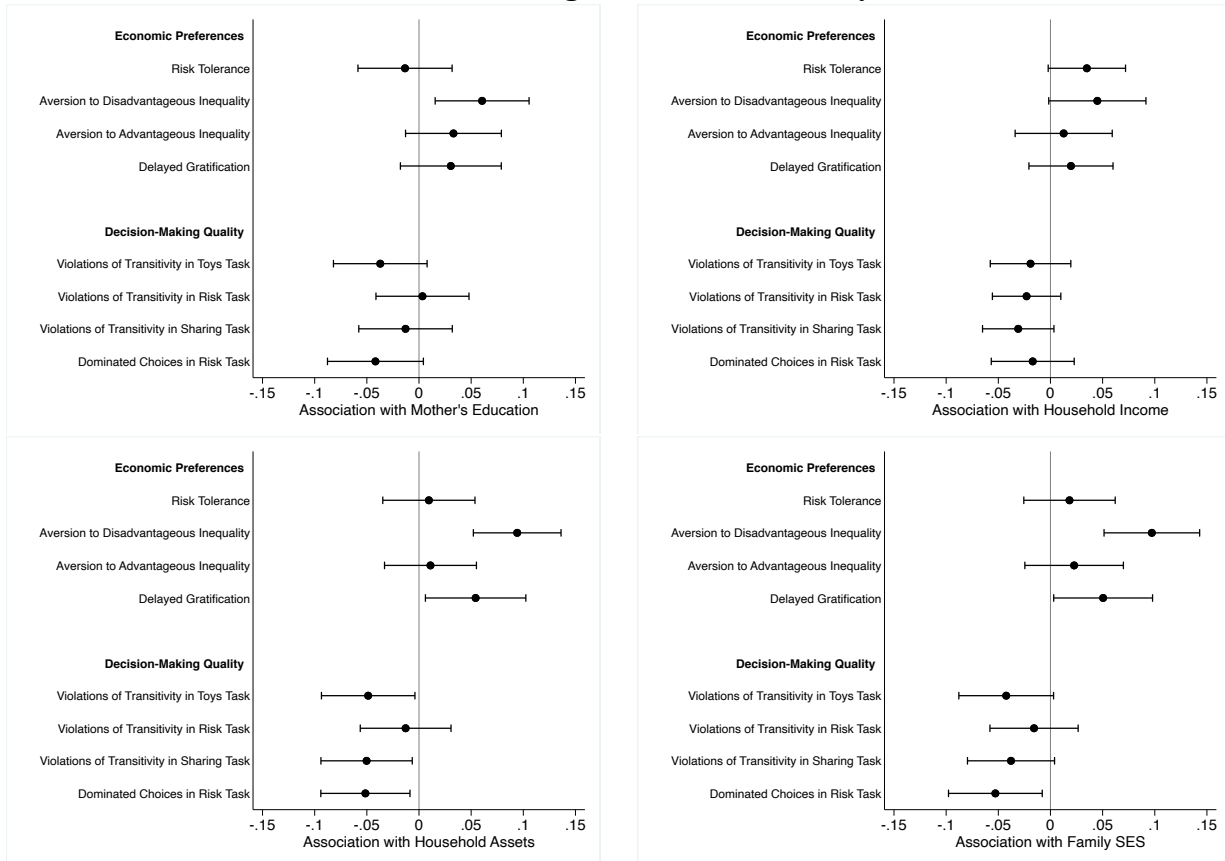
Notes: The blue circles in the top panel show the association of the measures of economic preferences with IQ. The red squares in the bottom panel show how the different measures of economic preferences are correlated with each other. The gray triangles in the bottom panel show how these correlations are affected when one controls for IQ. Brackets show 95% confidence intervals. All variables were standardized to have a standard deviation of one. $N = 1,790$ in general. $N = 1,599$ for associations including the measure of delayed gratification.

Appendix Figure 5: Association of Decision-Making Abilities with Economic Preferences



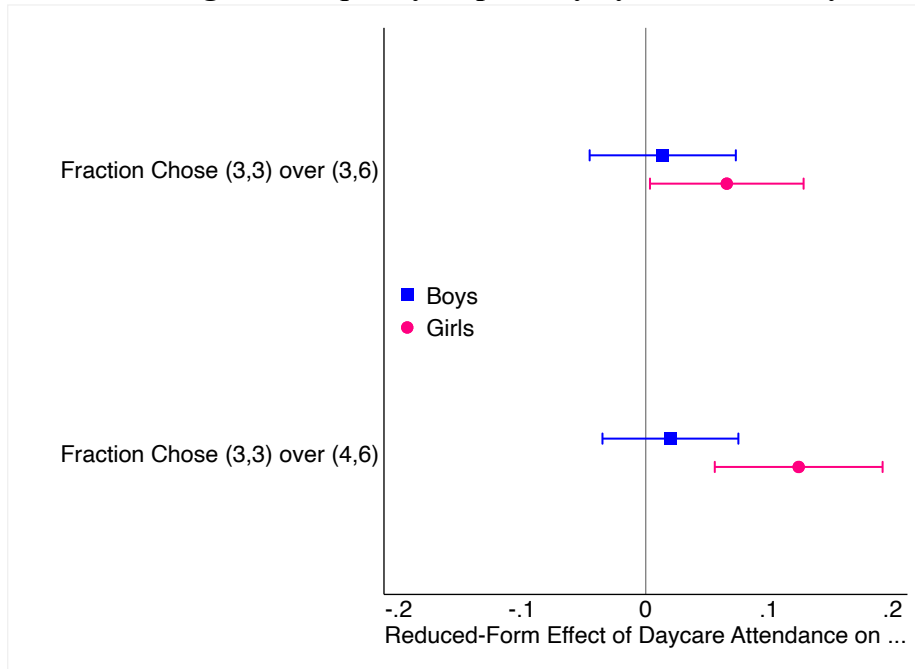
Notes: The circles show the associations between the different measures of decision-making abilities with the economic preferences measures. Brackets show 95% confidence intervals. All variables were standardized to have a standard deviation of one. $N = 2,113$ in general. $N = 1,858$ for associations including the measure of delayed gratification.

Appendix Figure 6: Association of Economic Preferences and Decision-Making Abilities with Family SES



Notes: This figure shows the association of economic preferences and decision-making abilities with different measures of family socioeconomic background, including mother's education (top left), household income (top right), household assets (bottom left), and a composite measure that is the first principal component of a principal component analysis of all these three measures (bottom right). The measure of mother's education is an indicator for whether mother finished middle school. Brackets show 95% confidence intervals. All variables were standardized to have a standard deviation of one. $N = 1,825$ in top left with the exception of delayed gratification ($N = 1,603$); $N = 2,017$ in top right with the exception of delayed gratification ($N = 1,775$); $N = 2,017$ in bottom left with the exception of delayed gratification ($N = 1,775$); and $N = 1,825$ in bottom right with the exception of delayed gratification ($N = 1,603$).

Appendix Figure 7: Reduced-Form Effect of Daycare Attendance on Aversion to Disadvantageous Inequality, Separately by Gender and by Trial



Notes: The figure shows the reduced-form effect of daycare attendance on aversion to disadvantageous inequality, separately by gender and by trial. The blue squares show the effects on boys. The pink circles show the effect on girls. There were two DI Trials. In the Costless DI Trial, the participant had to choose between the following two options: (a) 3 for the participant and 3 for the other child and (b) 3 for the participant and 6 for the other child. In the Costless DI Trial, the participant could avoid a relative disadvantage at no cost to herself. In the Costly DI Trial, the participant had to choose between option (a) and option (c) 4 for the participant and 6 for the other child. In the Costly DI Trial, the participant had to reduce her own payoff in order to equalize payoffs. The top panel shows the intent-to-treat effect of daycare attendance on the fraction of participants who chose option (a) in the Costless DI Trial. The bottom panel shows the effect on the fraction of participants who chose option (a) in the Costly DI Trial. Brackets show 95% confidence intervals. All regressions included dummies specific for each childcare center \times age group. $N = 2,113$.

Experimental Instructions

The instructions consisted of a series of screens, each containing a different audio. While the audio played, the enumerator would point to the different parts of the screen that the audio was referring to (e.g., the equal sign). The text of the audios was shown at the top of the screen. Enumerators were instructed to read the instructions to the participant if the audio could not be heard because of background noise. Next, the enumerator asked the participant a series of scripted questions designed to assess comprehension. If the participant answered incorrectly, the enumerator would explain what the correct answer was and why.

The script of the audio is shown below in black font. The sentences in blue were read after the audio – they were shown on the screen. The sentences in green were read after the audio – they were not shown on the screen. Finally, sentences in italics and in red consist of instructions for the enumerator (which were not read to the participant).

B1 🎵

In this game, you will choose the toy you like best.

The screen shows two toys (*point to the toy on the left, then point to the toy on the right*). Click on the toy you like best. If you like both toys equally, click on the equal sign in the middle of the screen (*point to the equal sign*).

To confirm your choice, click on the arrow at the bottom of the screen (*point to the arrow*).

Let's try it out together. Which toy do you like best? The ball (*point to the ball*) or the doll? (*point to the doll*)

< **IF they prefer one of the two toys** >

Ok, you like the [ball/doll] better. Click on the [ball/doll] (*point to the [ball/doll]*).

< **IF they like both equally** >

Ok, you like the ball and the doll equally. Click on the equal sign (*point to the equal sign*).

(Wait for the child to choose an option)

Ok.

Now click on the arrow to confirm your choice (*point to the arrow*).



B2

Now it's your turn. Which toy do you like best? The ball or the spinning top? Where should you click?

< **IF THE ANSWER IS CORRECT** >

< **IF they prefer one of the two toys** >

Ok. You clicked on [the ball/the top] because you like [the ball/the top] better.

< **IF they like both equally** >

Ok. You clicked on the equal sign because you like both the ball and the spinning top equally.

< **IF THE ANSWER IS WRONG** >

< **IF they prefer one of the two toys** >

No, if you like [the ball/the top] better, you should have clicked on [the ball/the top] (*point out to the ball/the top*).

< **IF they like both equally** >

No, if you like the ball and the top equally, you should have clicked on the equal sign (*point out to the equal sign*).

(Wait for the child to choose an option)

Now click on the arrow to confirm your choice.



B3

Let's try again. Which toy do you like better? The doll or the spinning top? Where should you click?

< IF THE ANSWER IS CORRECT >

< IF they prefer one of the two toys >

Ok. You clicked on [the doll/the top] because you like [the doll/the top] better.

< IF they like both equally >

Ok. You clicked on the equal sign because you like both the doll and the spinning top equally.

< IF THE ANSWER IS WRONG >

< IF they prefer one of the two toys >

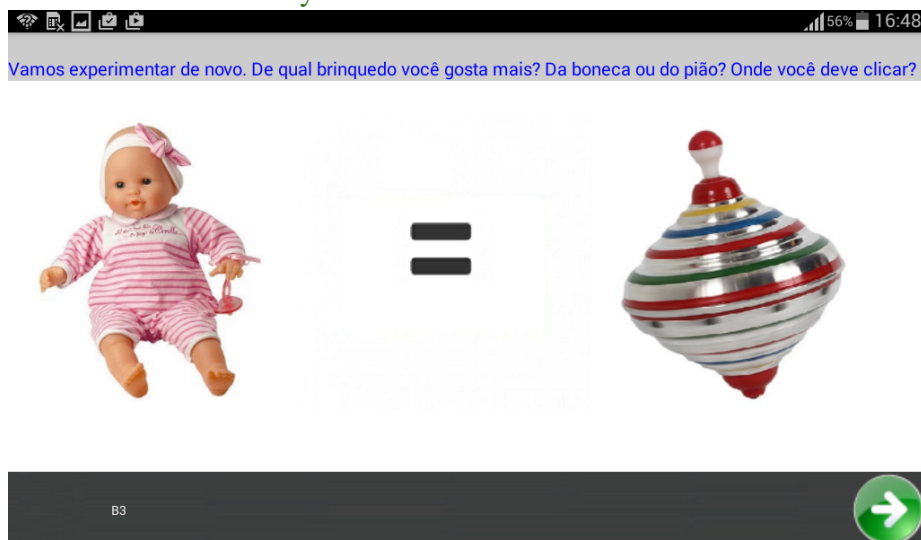
No, if you like [the doll/the top] better, you should have clicked on [the doll/the top] (*point out to the doll/the top*).

< IF they like both equally >

No, if you like the doll and the top equally, you should have clicked on the equal sign (*point out to the equal sign*).

(Wait for the child to choose an option)

Now click on the arrow to confirm your choice.



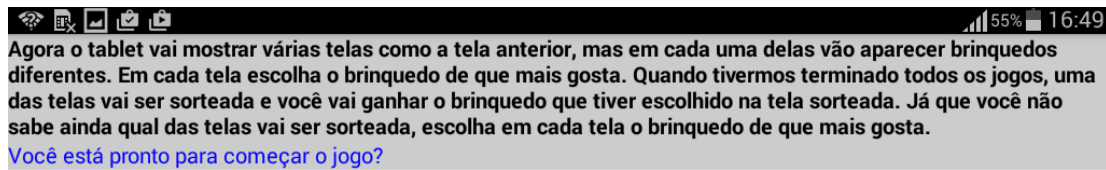
B4 🎵

Now the tablet will show several screens like the previous one, but different toys will appear on each screen. On each screen, choose the toy you like the most.

When we finish all the games, one of the screens will be drawn and you will win the toy you have chosen on the drawn screen.

Since you don't know which screen will be drawn yet, choose your favorite toy on each screen.

[Are you ready to start the game?](#)



continuar



M1 🎵

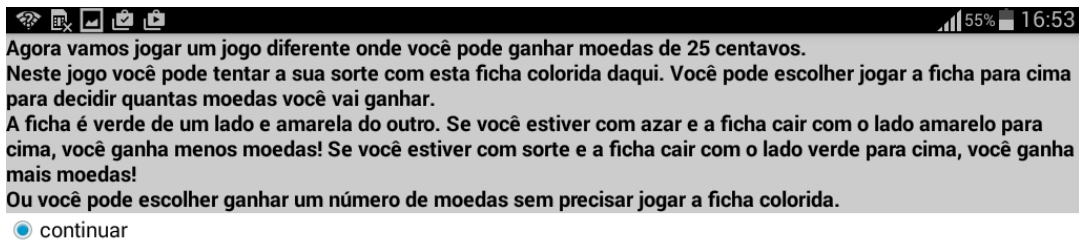
Now let's play a different game in which you can win 25 cent coins (*show a 25 cent coin*).

In this game you can try your luck with this colored coin here (*show the coin*). You can choose to toss the colored coin in the air (*pretend to toss the colored coin in the air*) to decide how many coins you will win.

The coin is green on one side (*show the green side of the coin*), and yellow on the other (*show the yellow side of the coin*).

If you are unlucky and the coin lands with the yellow side up (*show the yellow side up*), you get fewer coins. If you are lucky and the coin lands with the green side up (*show green side up*), you get more coins!

Or you can choose to win a number of coins without having to play the colored coin.



M2

Let's look at an example.

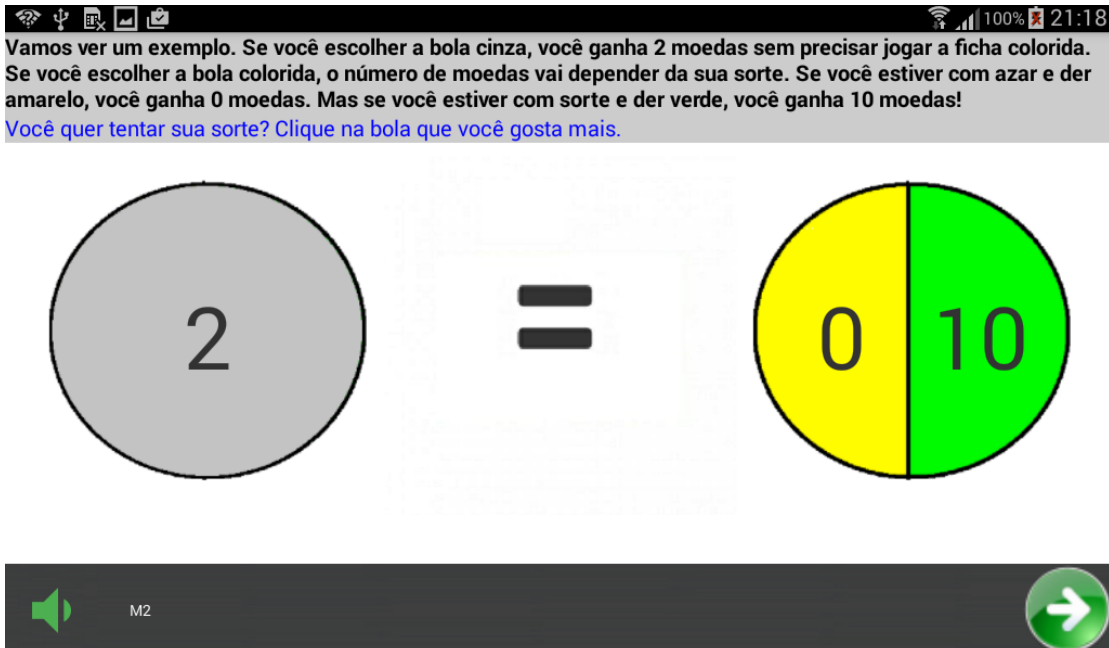
If you choose the gray circle (*point to the gray circle*), you get 2 coins without having to toss the colored coin (*point to the number 2*).

If you choose the colored circle (*point to the colored circle*), the number of coins will depend on your luck. If you are unlucky and it lands on yellow (*show the yellow side of the coin*), you get 0 coins (*point to number 0*). But if you are lucky and it lands on green (*show the green side of the coin*), you get 10 coins! (*point to number 10*)

Do you want to try your luck? Click on the circle you like best.

(Wait for the child to click on a circle)

To confirm your choice, click on the arrow at the bottom of the screen (*point to the arrow*).



Vamos ver um exemplo. Se você escolher a bola cinza, você ganha 2 moedas sem precisar jogar a ficha colorida. Se você escolher a bola colorida, o número de moedas vai depender da sua sorte. Se você estiver com azar e der amarelo, você ganha 0 moedas. Mas se você estiver com sorte e der verde, você ganha 10 moedas!

Você quer tentar sua sorte? Clique na bola que você gosta mais.

The screenshot shows a mobile application interface. At the top, there is a status bar with icons for Wi-Fi, USB, and battery (100%), and the time 21:18. Below the status bar is a text box with the following content: "Vamos ver um exemplo. Se você escolher a bola cinza, você ganha 2 moedas sem precisar jogar a ficha colorida. Se você escolher a bola colorida, o número de moedas vai depender da sua sorte. Se você estiver com azar e der amarelo, você ganha 0 moedas. Mas se você estiver com sorte e der verde, você ganha 10 moedas! Você quer tentar sua sorte? Clique na bola que você gosta mais." Below the text box are two large circles. The left circle is gray and contains the number "2". The right circle is split vertically, with the left half yellow and containing "0", and the right half green and containing "10". An equals sign "=" is positioned between the two circles. At the bottom of the screen, there is a dark gray bar containing a green speaker icon, the text "M2", and a green circular button with a white right-pointing arrow.

M3

Let's see if you understood. How many coins do you get if you choose the gray circle?

< IF THE ANSWER IS CORRECT >

Ok. If you choose the gray circle (*point to the gray circle*), you get 7 coins (*point to the number 7*).

< IF THE ANSWER IS WRONG >

No, if you choose the gray circle (*point to the gray circle*), you get 7 coins (*point to the number 7*).

Let's pretend that you chose the colored circle. Toss the coin to find out how many coins you would get. (*Give the colored coin to the child*).

< IF IT'S YELLOW >

It's yellow! How many coins would you earn?

< IF THE ANSWER IS CORRECT >

Ok. If you chose the colored circle (*point to the colored circle*) and were unlucky enough for it to land on yellow, you would win 1 coin (*point to the number 1*).

< IF THE ANSWER IS WRONG >

No, if you chose the colored circle (*point to the colored circle*) and were unlucky enough for it to land on yellow, you would get 1 coin (*point to number 1*).

See? In this example, if you are unlucky and it lands on yellow, you get fewer coins (*point to the number 1*) than if you had chosen the gray circle (*point to the number 7*). Do you understand?

What if it was green (*simulate the coin falling green side up*)? How many coins would you get?

< IF THE ANSWER IS CORRECT >

Ok. If you chose the colored circle (*point to the colored circle*) and you were lucky enough for it to land on green, you would win 8 coins (*point to the number 8*).

< IF THE ANSWER IS WRONG >

No, if you chose the colored circle (*point to the colored circle*) and were lucky enough for it to land on green, you would win 8 coins (*point to the number 8*).

< IF IT'S GREEN >

It's green! How many coins would you win?

< IF THE ANSWER IS CORRECT >

Ok. If you chose the colored circle (*point to the colored circle*) and were lucky enough for it to land on green, you would win 8 coins (*point to number 8*).

< IF THE ANSWER IS WRONG >

No, if you chose the colored circle (*point to the colored circle*) and were lucky enough for it to land on green, you would win 8 coins (*point to number 8*).

What if it was yellow (*simulate the coin dropping yellow side up*)? How many coins would you win?

< IF THE ANSWER IS CORRECT >

Ok. If you chose the colored circle (*point to the colored circle*) and were unlucky enough for it to land on yellow, you would win 1 coin (*point to the number 1*).

< IF THE ANSWER IS WRONG >

No, if you chose the colored circle (*point to the colored circle*) and were unlucky enough for it to land on yellow, you would get 1 coin (*point to number 1*).

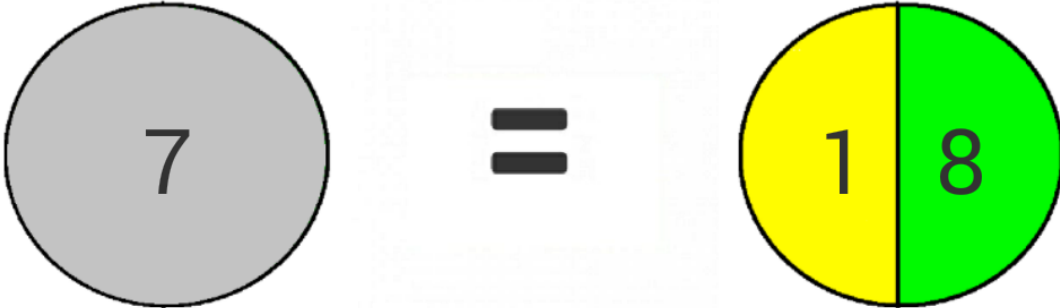
See? In this example, if you are unlucky and it lands on yellow, you get fewer coins (*point to the number 1*) than if you had chosen the gray circle (*point to the number 7*). Do you understand?


Do you want to try your luck? Click on the circle you like best.

(Wait for the child to click on a circle)

Ok. Now click on the arrow to confirm your choice.

Vamos ver se você entendeu. Quantas moedas você ganha se escolher a bola cinza? Vamos fingir que você escolheu a bola colorida. Jogue a ficha para descobrir quantas moedas você ganharia. Deu [cor]! Quantas moedas você ganharia? E se desse [cor], quantas moedas você ganharia? Você quer tentar sua sorte? Clique na bola que você gosta mais.



M3 

M4

Let's try this one more time. Which of the two circles depends on your luck?

< IF THE ANSWER IS CORRECT >

Ok. The colored circle (*point to the colored circle*) depends on your luck.

< IF THE ANSWER IS WRONG >

No, the colored circle (*point to the colored circle*) depends on your luck.

If you choose the colored circle (*point to the colored circle*), how many coins do you get if it lands on yellow?

< IF THE ANSWER IS CORRECT >

Ok. You get 6 coins (*point to the number 6*) if you choose the colored circle and it lands on yellow.

< IF THE ANSWER IS WRONG >

No, you get 6 coins (*point to the number 6*) if you choose the colored circle and it lands on yellow.

If you choose the colored circle (*point to the colored circle*), how many coins do you get if it lands on green?

< IF THE ANSWER IS CORRECT >

Ok. You get 10 coins (*point to the number 10*) if you choose the colored circle and it lands on green.

< IF THE ANSWER IS WRONG >

No, you get 10 coins (*point to the number 10*) if you choose the colored circle and it lands on green.

How many coins do you get if you choose the gray circle (*point to the gray circle*)?

< IF THE ANSWER IS CORRECT >

Ok. You get 15 coins (*point to the number 15*) if you choose the gray circle.

< IF THE ANSWER IS WRONG >

No, you get 15 coins (*point to the number 15*) if you choose the gray circle.

Let's pretend that you chose the colored circle. In this example, even if you are lucky and it lands on green, you get fewer coins (*point to the number 10*) than if you had chosen the gray circle (*point to the number 15*). Do you get it?

Do you want to try your luck? Click on the circle you like best.

(Wait for the child to click on a circle)

Ok. Now click on the arrow to confirm your choice.

Vamos tentar mais uma vez. Qual das duas bolas depende da sua sorte? Se você escolher a bola colorida, quantas moedas você ganha se der amarelo? Se você escolher a bola colorida, quantas moedas você ganha se der verde? Quantas moedas você ganha se escolher a bola cinza? Você quer tentar sua sorte? Clique na bola que você gosta mais.

15 = 6 10

M4

M5 🎵

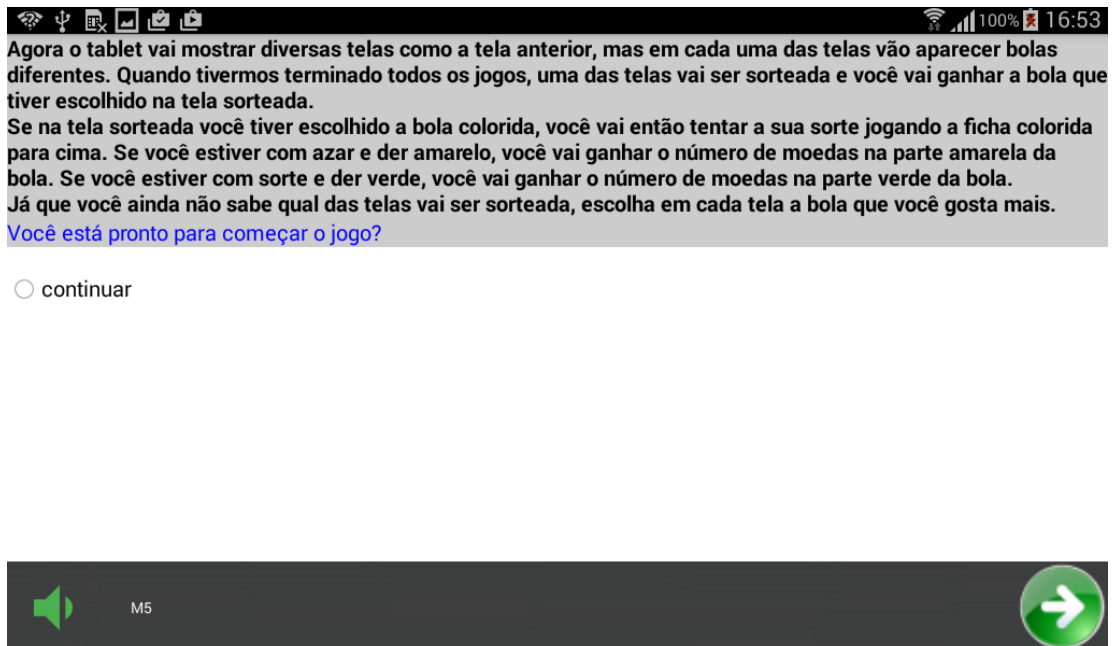
Now the tablet will show several screens like the previous screen, but different circles will appear on each screen. When we've finished all the games, one of the screens will be drawn and you'll get the circle you've chosen on the drawn screen.

If on the drawn screen you have chosen the colored circle, you will then try your luck by tossing up the colored coin. If you are unlucky and it lands on yellow, you will win the number of coins on the yellow part of the circle. If you are lucky and it lands on green, you will win the number of coins in the green part of the circle.

Since you don't know which screen will be drawn yet, choose the circle you like best on each screen.

Remember, there is no right or wrong in this game. Sometimes it might be better to choose the colored circle, and sometimes it might be better to choose the gray circle. You have to decide which circle you like best.

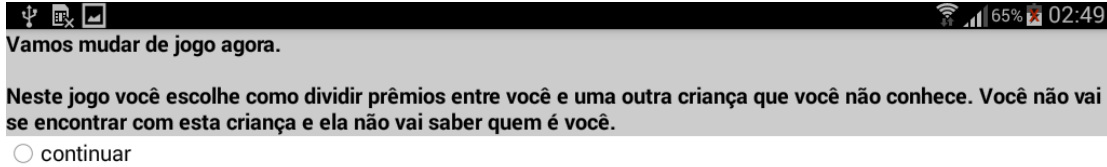
Are you ready to start the game?



D1 🎵

Let's change the game now.

In this game you will choose how to split prizes between you and another child you don't know. You will not meet this child, and they will not know who you are.



D2

The screen shows 6 prizes (*point to the 6 circles*) and 2 boxes (*point to the two boxes*). The blue box is yours (*point to the box underneath*). The red box is the other child's (*point to the top box*).

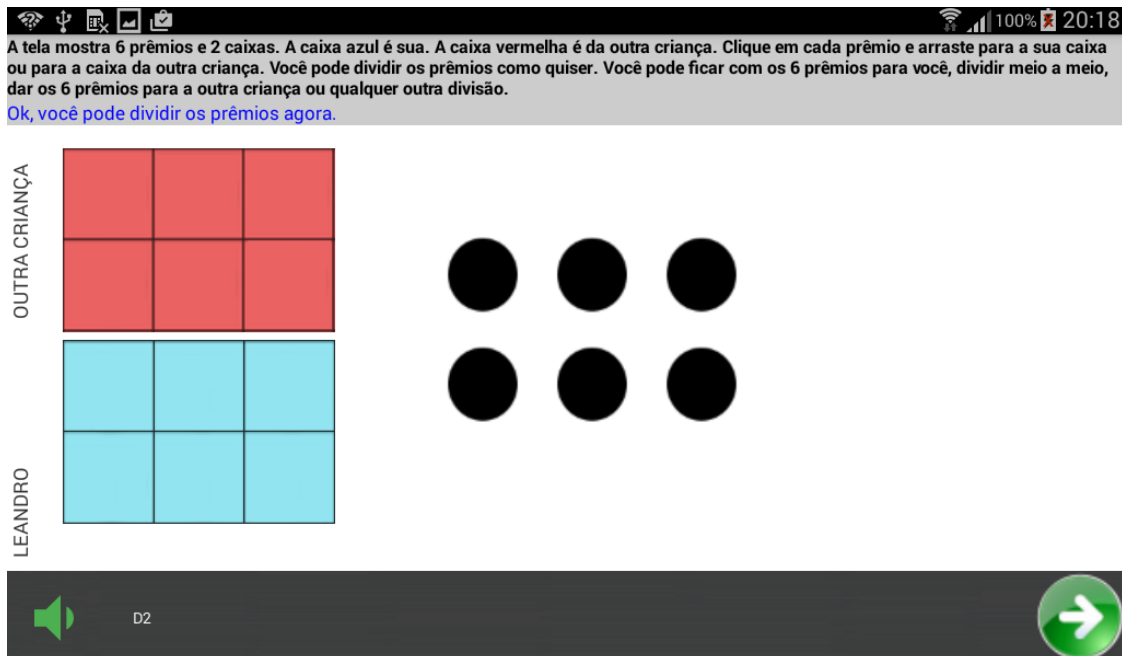
Click on each prize and drag it to your box (*click on a circle and drag it to the bottom box. Return the circles to the center*)... or to the other child's box (*click on a circle and drag to the top box. Return the circles to the center*).

You can divide up the prizes however you like. You can keep the 6 prizes for yourself, divide them in half, give the 6 prizes to the other child, or any other division.

Okay, you can divide the prizes now.

(Wait for the child to divide the 6 prizes between the two boxes)

Ok. Now click on the arrow to confirm your choice.



A tela mostra 6 prêmios e 2 caixas. A caixa azul é sua. A caixa vermelha é da outra criança. Clique em cada prêmio e arraste para a sua caixa ou para a caixa da outra criança. Você pode dividir os prêmios como quiser. Você pode ficar com os 6 prêmios para você, dividir meio a meio, dar os 6 prêmios para a outra criança ou qualquer outra divisão. Ok, você pode dividir os prêmios agora.

OUTRA CRIANÇA

LEANDRO

D2

D3 🎵

Now let's play a similar game.

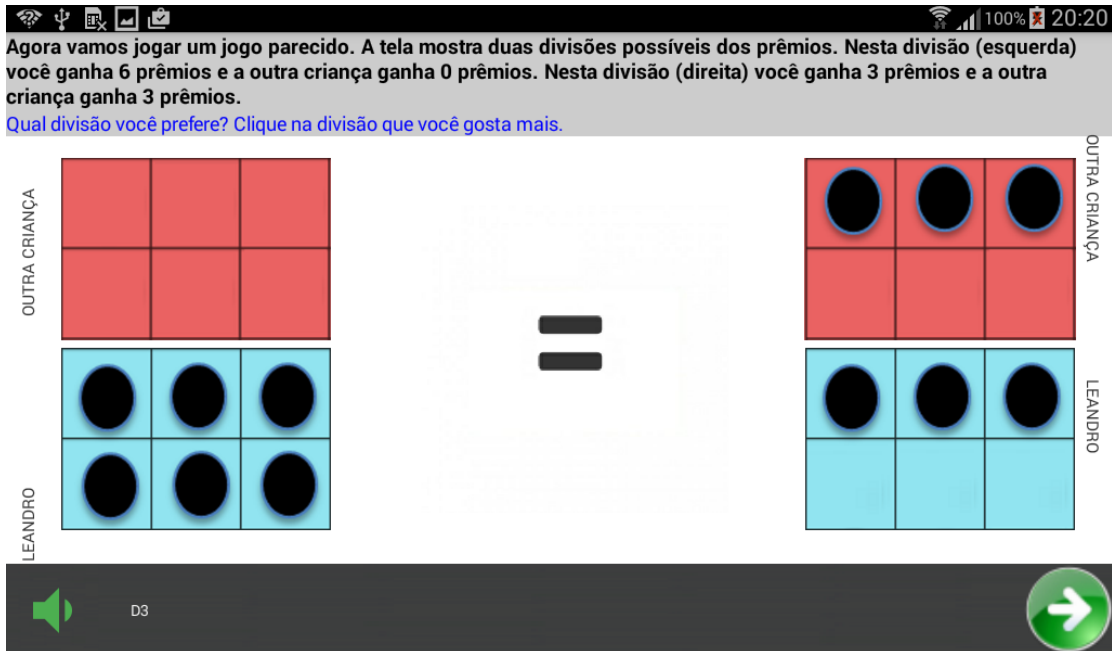
The screen shows two possible divisions of the prizes. In this division (*point to the left column*) you get 6 prizes (*point to the box on the bottom left*) and the other child gets 0 prizes (*point to the box on the top left*).

In this division (*point to the right column*) you get 3 prizes (*point to the box on the bottom right*) and the other child gets 3 prizes (*point to the box on the top right*).

Which division do you prefer? Click on the division you like best.

(Wait for the child to choose an option)

To confirm your choice, click on the arrow at the bottom of the screen (*point to the arrow*).



D4

Let's see if you understand.

How many prizes do you win if you choose this prize division (*point to the left column*)?

< IF THE ANSWER IS CORRECT >

Ok. You win 1 prize if you choose this prize division (*point to the box at the bottom left*).

< IF THE ANSWER IS WRONG >

No, you get 1 prize if you choose this prize division (*point to the box in the lower left corner*).

How many prizes does the other child get if you choose this prize division (*point to the left column*)?

< IF THE ANSWER IS CORRECT >

Ok. The other child wins 1 prize if you choose this prize division (*point to the box in the upper left corner*).

< IF THE ANSWER IS WRONG >

No, the other child wins 1 prize if you choose this prize division (*point to the box in the upper left corner*).

How many prizes do you get if you choose this prize division (*point to the right column*)?

< IF THE ANSWER IS CORRECT >

Ok. You get 2 prizes if you choose this prize division (*point to the box in the bottom right corner*).

< IF THE ANSWER IS WRONG >

No, you get 2 prizes if you choose this prize division (*point to the box in the lower right corner*).

How many prizes does the other child get if you choose this prize division (*point to the column on the right*)?

< IF THE ANSWER IS CORRECT >

Ok. The other child wins 6 prizes if you choose this prize division (*point to the box in the upper right corner*).

< IF THE ANSWER IS WRONG >

No, the other child wins 6 prizes if you choose this prize division (*point to the box in the upper right corner*).

Which division do you prefer? Click on the division you like best.

(Wait for the child to click on an option)

Ok. Now click on the arrow to confirm your choice.

Vamos ver se você entendeu. Quantos prêmios você ganha se escolher esta divisão de prêmios (esquerda)? Quantos prêmios a outra criança ganha se você escolher esta divisão de prêmios (esquerda)? Quantos prêmios você ganha se você escolher esta divisão de prêmios (direita)? Quantos prêmios a outra criança ganha se você escolher esta divisão de prêmios (direita)? Qual divisão você prefere? Clique na divisão que você gosta mais.

OUTRA CRIANÇA

LEANDRO

OUTRA CRIANÇA

LEANDRO

D4

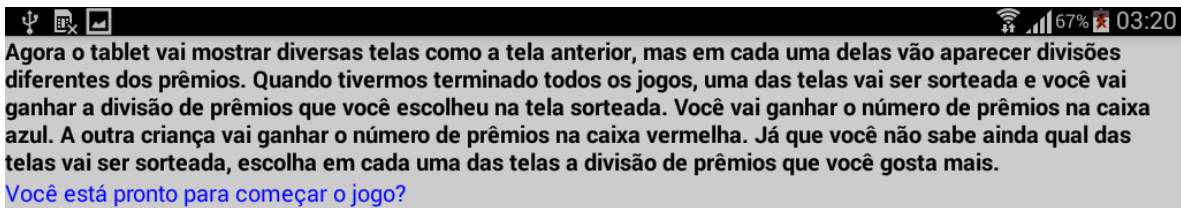
D5 🎵

Now the tablet will show several screens like the previous screen, but different prize divisions will appear on each screen.

When we have finished all the games, one of the screens will be drawn and you will win the prize division you chose on the drawn screen. You will win the number of prizes in the blue box. The other child will win the number of prizes in the red box.

Since you don't know yet which screen will be drawn, choose on each screen the prize division you like best.

Are you ready to start the game?



continuar



Important guidelines about the Marshmallow Test

1. Before starting the test ask the child if he/she wants to go to the bathroom.
2. Have all the equipment ready before you ask the child to enter the tent. Otherwise the child ends up waiting longer than 25 minutes.
3. Only start the timer when you close the tent after giving all the instructions to the child.
4. Ask the child not to talk during the task.
5. Ask other people in the environment not to talk to the child.
6. Do not respond if the child asks questions after the test has started (e.g., "Will it take much longer?")

Speech in green. Action in red.

1. *Talk to the mother or guardian about the test procedures (away from the child).*
2. *Ask the child if he/she wants to go to the bathroom first.*
3. *Set up the tent.*
4. Which bonbon would you most like to eat? The “sonho de valsa” bonbon or the “ouro branco” bonbon? *[show the child the bonbons. They have to choose one of the two types of bonbons, we cannot mix the bonbons in the task].*
5. Do you want 1 or 2 bonbons?
6. *Ask the child to enter the tent and sit on the floor, facing the entrance to the tent.*
7. *Place the empty plate n°1 on the floor in front of the child.*
8. *Place the bell in front of the plate.*
9. *Place the tablet on top of the tent, with the camera positioned in the designated recording slot.*
10. *Check that the plate appears in the video.*
11. *Check that the camera is zoomed out.*
12. *Check the lighting. If necessary, place a portable light next to the plate.*
13. *Start recording.*
14. *Unwrap a bonbon and place it in the middle of plate n°2.*
15. Don't eat it yet. I have to tell you the rules of the game first.
16. *Place the plate n°2 with the bonbon on top of the empty plate n°1.*
17. Here's the game. I'm going out.
18. If you wait for me to come back without touching the bonbon *[point to the bonbon]*, without getting up, and without opening the tent, you get to keep two bonbons. Okay?
19. But if you don't want to wait, you can ring the bell like this *[ring the bell]* and I'll come back when you want.
20. Only if you ring the bell, you can NOT have 2 bonbons, only 1.

21. If you wait for me to come back without getting up, without opening the tent, and without touching the bonbon, what do you get?
22. And if you don't want to wait for me to come back and call me by ringing the bell, what do you get? *Wait for the child to answer and make sure he understands the rules.*
23. Then you can choose. You can wait for me to come back and get 2 bonbons *[make the number 2 with your index and middle finger].*
24. Or you can ring the bell to call me back and then you get only 1 bonbon *[make the number 1 with your index finger].*
25. *If the child doesn't want to wait, end the game and end the recording.*
26. Ok. See you later!
27. *Leave. Close the tent door.*
28. ***Start the timer.***
29. *The interviewer should watch what the child does on the tablet recording screen.*
30. *Wait for 25 minutes before returning or until the child rings the bell, touches the bonbon, opens the tent, or stands up.*
31. ***Stop the timer as soon as one of these things happens.***
32. *If the child waits until you return without touching the bonbon, without getting up, and without opening the tent, give him 1 extra bonbon for a total of 2 bonbons. Otherwise, give only 1 bonbon.*
33. Okay. You can eat the bonbon (the bonbons) now.
34. *Turn off the camera.*

PRIZE DRAW

Now it's time for us to draw the prizes. Are you ready?

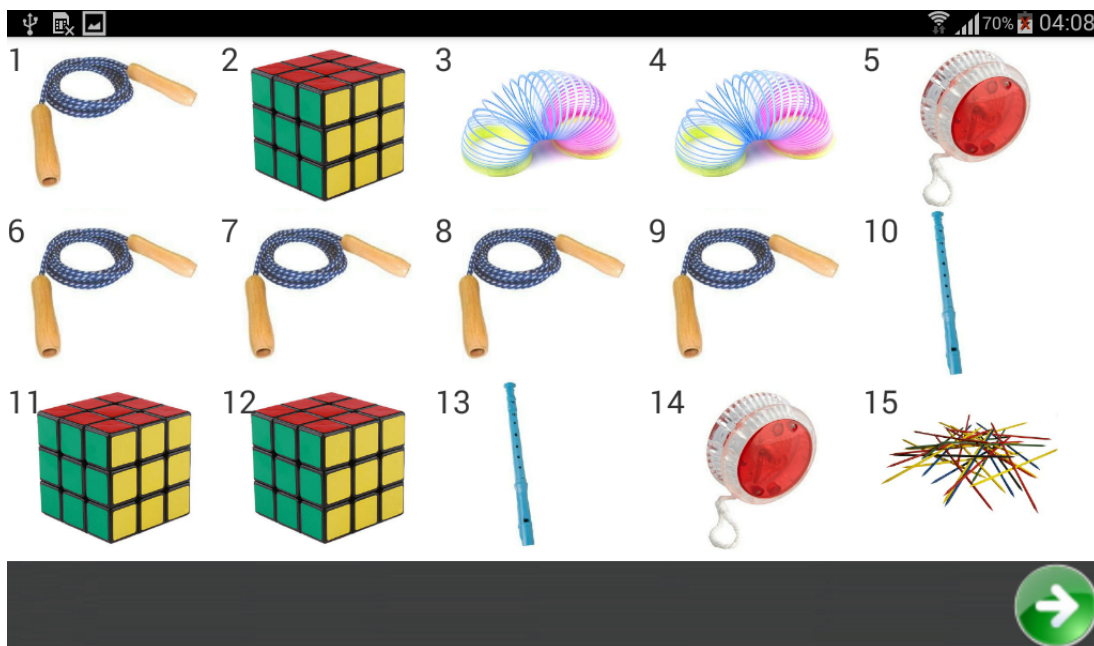
First, let's find out which toy you will win. This screen shows all 15 choices you made in the toy game (*show the tablet screen to the child so he or she can see it*).

Let's do the following. I have here 15 pebbles with numbers from 1 to 15 (*show the child the pebbles*). I'm going to put these 15 pebbles in this bag (*put the pebbles in the bag*). I'm going to shake the bag, and then you're going to take a pebble out of it.

You will get the toy that corresponds to the number you draw. If, for example, you draw number 5, you will win (*indicate the toy shown below number 5*).

Ready to draw a pebble? Good luck!

You drew the number (*say the number you drew*). You won (*indicate the toy shown below the number drawn*). Congratulations!



Now, let's draw how many coins you will earn. This screen shows all 15 choices you made in the circle game (*show the child the tablet screen so he can see it*).

Again, this bag has 15 pebbles with numbers from 1 to 15 (*place the pebbles inside the bag*). I will shake the bag and then you will draw a pebble from it.

Ready to draw a pebble? Good luck!

You drew the number (*say the number you drew*).

< IF YOU CHOSE THE GRAY CIRCLE IN THE DRAW GAME >

You won (*say the number on the circle below the number drawn*) coin(s).
Congratulations!

< IF YOU CHOSE THE COLORED CIRCLE IN THE DRAW GAME >

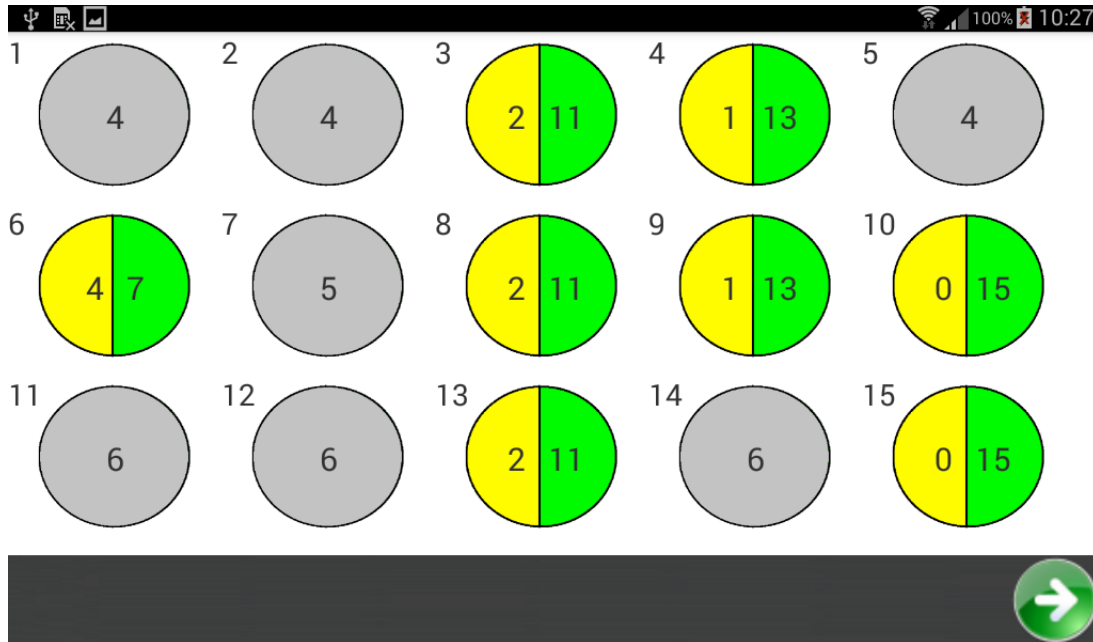
If it lands on yellow, you win (*say the number on the yellow part of the circle below the number drawn*) coin(s).

If it lands on green, you win (*say the number on the green part of the circle below the number drawn*) coin(s).

Now you can throw the coin.

The coin landed on the (*yellow/green*) side.

You won (*say the number on the yellow/green part of the circle underneath the number drawn*) coin(s). Congratulations!



Finally, we will draw your prize for the box game. This screen shows all 10 choices you made in the box game (*show the child the tablet screen so they can see it*).

This die here has 10 sides numbered 0 through 9, where 0 equals 10. You will roll the die.

You will win the number of prizes shown below the number that comes out on the die. If, for example, the die lands on number 1, you win (*say the number of prizes below number 1*) prizes.

If the die lands on number 0, you win the number of prizes shown under number 10, that is, (*say the number of prizes under number 10*) prizes.

Ready to roll the die? Good luck!

The die landed on (*say the number drawn*). You won (*say the number of prizes shown below the number drawn*) prizes. Congratulations!

Now you can choose your prizes. The options are as follows: [*show the five toys outside the toy won in the task "Which toy do you like best?"*]

1 2 3 4 5

●	●	

●	●	●

●	●	●

●	●	

●	●	●

6 7 8 9 10

●	●	

●	●	●

●	●	●

●	●	●

●	●	●
●		

