Early Education, Preferences and Decision-Making Abilities

Joana Cardim (Education Policy Institute)
Pedro Carneiro (University College of London, IFS, CEMMAP, FAIR-NHH)
Leandro S. Carvalho (University of Southern California)
Damien de Walque (Development Research Group, The World Bank)[1]

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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.

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[1] Cardim; Education Policy Institute, 150 Buckingham Palace Road, London SW1W 9TR, UK (joana.almeidacardim@gmail.com) Carneiro: University College London, Department of Economics, Gower Street, London WC1E 6BT, UK (p.carneiro@ucl.ac.uk); Carvalho: University of Southern California, Center for Economic and Social Research, 635 Downey Way, Los Angeles, CA 90089-3332 (leandro.carvalho@usc.edu); de Walque: Development Research Group, The World Bank, Washington, DC 20433 (ddewalque@worldbank.org). Jiewen Luo provided excellent research assistance. This paper benefited from discussions with Isabelle Brocas, Juan Carrillo, Marco Castillo, Gary Charness, Niree Kodaverdian, Ragan Petrie, and Anya Samek, Lise Vesterlund, and from the feedback of participants of the Experiments which Children and Non-Standard Subjects Workshop at the 2019 North American Meetings of the Economic Science Association. We thank Elisabet Meireles, Luiz Eduardo Guedes, Mary Fuhs, and Jorge Alberto dos Santos for help with the survey, and the IADB for letting us use test score data that they collected. Research reported in this publication was supported through the Research Support Budget and the Knowledge for Change Program (KCP) at World Bank, the NIA grant 1K01AG066999-01A1 (Carvalho), the ESRC for CEMMAP (ES/P008909/1) and the European Research Council through grant ERC-2015-CoG- 692349 (Carneiro). Cardim gratefully acknowledges financial support by FCT – Fundação para a Ciência e Tecnologia - SFRH/BD/131707/2017. This study was approved by the Committee of the Secretaria Municipal de Saúde in Rio de Janeiro, Brazil (Plataforma Brasil) on 3/8/2016, (Study ID:1.466.676) and by the IRB at the University of Southern California University Park, on 2/8/2016 (Study ID: UP-15-00641) and registered in the AEA RCT Registry (AEARCTR-0004536). The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors. They do not necessarily represent the views of the World Bank and its affiliated organizations, or those of the Executive Directors of the World Bank or the governments they represent, or the National Institutes of Health. The authors declare that they have no relevant or material financial interests that relate to the research described in this paper.

"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 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 forthcoming).[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 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 field 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).

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[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 field experiments with about 2,100 of these children – including both treatment and control children – nine years after the lottery assignment. Four field experiments were conducted 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 to measure self-control. While the risk task measures risk aversion, the sharing task was designed to measure children's aversion to being at a relative disadvantage ("disadvantageous inequality") and 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). They are also consistent with Cappelen et al. (2020), who

[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).

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 effect on boys and girls would provide support for the latter hypothesis.

The 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 (intent-to-treat estimate) 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 × 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 our 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.

At last, we 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 their 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. List et al. (forthcoming) 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. These results provide some support for this hypothesis.

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) 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. forthcoming). 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 experimental tasks 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 vary by

[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.

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 discusses some possible explanations for why daycare attendance affects only girls' aversion to disadvantageous inequality. Section 9 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 introduced 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 after that (for that age group at the given center) was smaller than the number of applications for these slots, a lottery was run. Random numbers were assigned and 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.

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 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

[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%.

	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

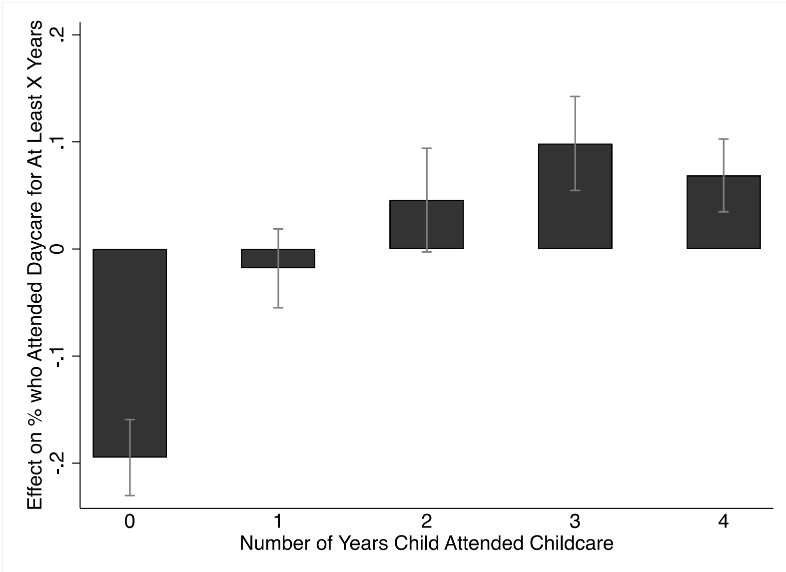
Notes: The table investigates whether the treatment and control children are comparable in terms of predetermined characteristics. The first column shows means among the control children. The second column reports the coefficient on the treatment indicator from regressions of the dependent variable listed in the row on the treatment indicator and on dummies specific for childcare center × 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

[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 experimental tasks were not interviewed in 2008, 2012, or 2015.

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 5 shows the corresponding regression estimates for the entire sample, which are similar to the ones shown here.

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.

3. Experimental Tasks

We discuss in this section the four different experimental tasks that were 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: (a) 0 or 3 quarters; (b) 0 or 9 quarters; (c) 0 or 15 quarters; (d) 3 or 6 quarters; or (e) 3 or 12 quarters. The interface of the risk and of the sharing tasks are discussed in the Appendix.

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]

(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

[12] At the end of 2016, one US dollar was worth about 3.25 Brazilian Reais. 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 € 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.

(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 two other 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."

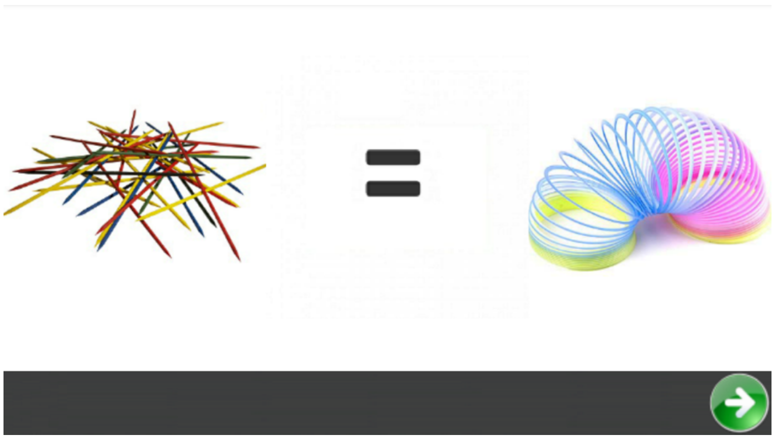
Common Setup of Tasks Administered using a Table

The three tasks discussed above, the toys task, the risk task, and the sharing task, were administered using a tablet and had 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 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

[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 1). 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.

[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.



Notes: The figure illustrate 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 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 to meet the challenging circumstances in which the surveys were conducted. Most children lived in small houses in *favelas* (Brazilian slums) with limited living space shared by a relatively large number of household members. We used a foldable child's tent to simulate a

[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.
[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.

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 participated managed to wait.

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.

[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.

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.

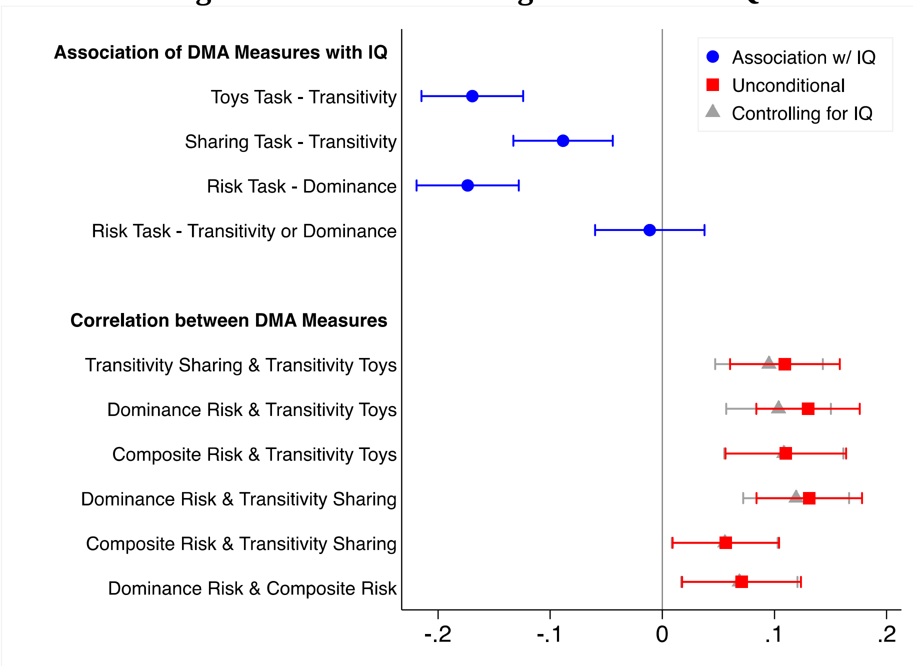
Consistency with transitivity may be viewed as too low a standard of decision-making quality because it treats any set of choices with the same number of transitivity violations as equally high-quality. A more stringent requirement would also require monotonicity. The risk task provides two different 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). 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.

Because these measures are more unusual than the measures of preferences, we show some descriptive statistics in Figure 3. 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

Scale for Children-IV (Wechsler 2003).[21] The red squares in the bottom panel investigates 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.

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.

The figure 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

[21] 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.

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.

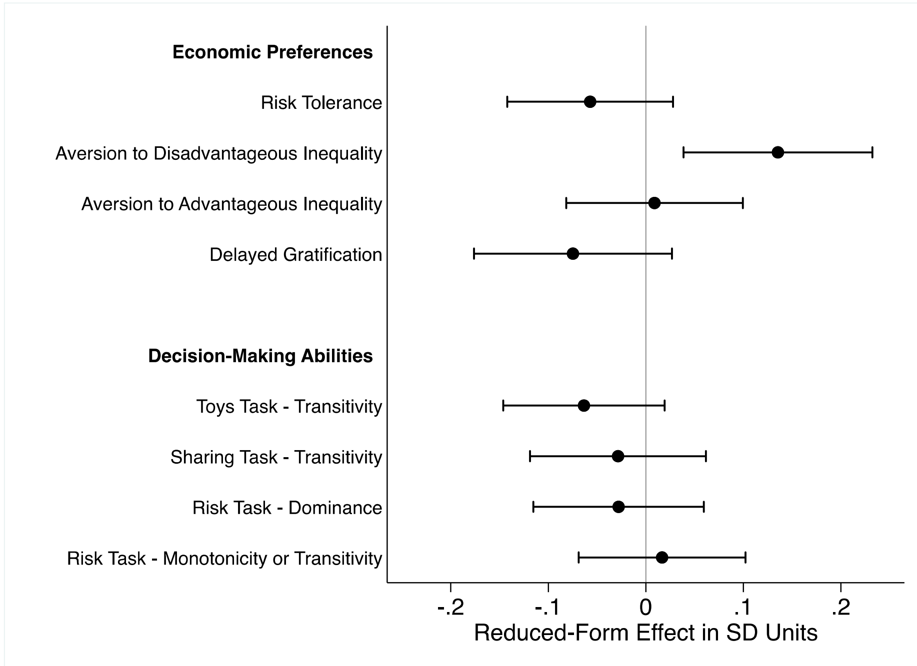
We proceed now to study the effects of daycare attendance on economic preferences and on decision-making abilities.

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 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.

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).[22] 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.

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

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.[23] 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.[24]

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. The project randomly assigned 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.[25] 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.[26,27]

[23] 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.

[24] 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).

[25] 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.

[26] The point estimates suggest that preschool attendance increased inequality aversion in the merit experiment, but this result in particular is not statistically significant.

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

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 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)?

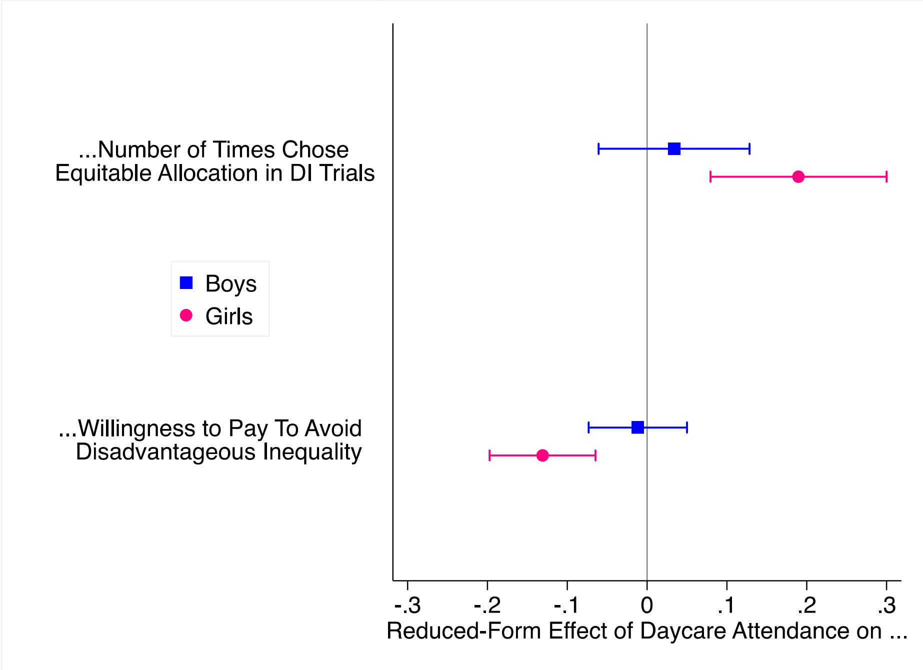
The daycare experiment represented an exogenous change in the environment of treatment boys and girls. If the program had differential effects on the social preferences of boys and girls, it would provide evidence that the gender differences in social preferences is at least partly explained by differences 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. 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).[28] This result 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 × 2 genders) (p-value of 0.003).

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).

[28] On average control girls chose the equitable allocation 1.17 times

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 × age group. N = 2,113.

The magnitudes of the effects in the top panel are not easily interpreted. 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.[29] 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:[30,31]

$$WTP_{DI} = \left.\frac{\frac{\partial U}{\partial y}}{\frac{\partial U}{\partial x}}\right|_{x<y} = \frac{-\beta_{DI}}{1+\beta_{DI}} \tag{2}$$

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. 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.

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[29] 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).

[30] 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.

[31] 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:

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

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.

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.

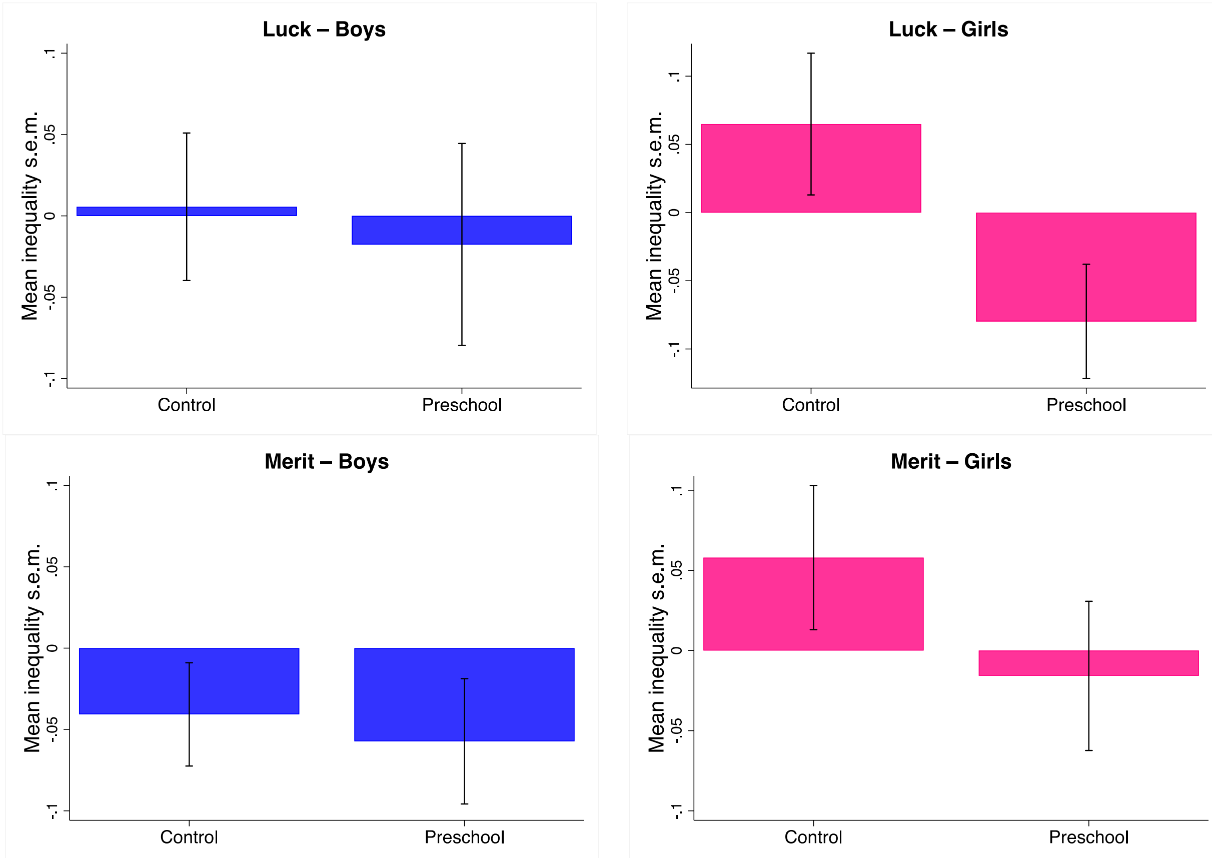
The published version of Cappelen et al. (2020) does not show separate results by gender. However, in their publicly-available code they estimate how preschool attendance affected boys and girls. This section reproduces this particular set of results.

Figure 6 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.[32] The bottom panel shows results for the merit experiment.[33] 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 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.

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

[32] 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.

[33] 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.



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).

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 12 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 13, 14, and 15 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. (forthcoming) 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 field experiment 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.

Intuitively, competitive children, who dislike losing or falling behind, may put extra effort and study harder in order to avoid such outcome.[34] 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:

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

where $g(\mathrm{e})$ 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:

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

[34] 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.

We have then that:

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

because $g'(\cdot) > 0$, $g''(\cdot) < 0$, $\beta_{DI} \geq 0$, $\beta_{AI} < 1$, and $c''(\cdot) > 0$.[35] The model predicts that the level of effort – and consequently the test score – is a weakly increasing function of the aversion to disadvantageous inequality.[36] This equation captures the intuition mentioned above that a 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.[37,38]

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

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[35] 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.

[36] 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:

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

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

[38] 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.

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: Choices in Social Preferences Task Predict Test Scores

	Math Grade			*Portuguese Grade*		
	(1)	(2)	(3)	(4)	(5)	(6)
Aversion to Disadvan. Inequality	0.25	0.12	0.11	0.25	0.14	0.13
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Cognitive Function		0.44	0.41		0.37	0.34
		(0.03)	(0.03)		(0.03)	(0.03)
Risk Tolerance			0.00			-0.01
			(0.02)			(0.02)
Aversion to Advan. Inequality			0.05			0.09
			(0.04)			(0.04)
Delayed Gratification			0.01			0.01
			(0.00)			(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			0.00
			(0.02)			(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. N = 1,006.

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.[39] 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.

8. Discussion and Conclusion

We investigated the role that early education plays in gender differences in social preferences. 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 can be 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.

Social preferences were measured by having participants choose between two allocations that paid different amounts of tokens to the participant and to another child who was anonymous. For example, the participant had to choose between an equitable allocation in which each child got 3 tokens and an allocation in which the participant got 3 tokens and the other child got 6 tokens. The decisions were designed to measure children's aversion to disadvantageous inequality and their aversion to advantageous inequality.

The results show that daycare attendance increased the aversion of girls to being at a relative disadvantage. It had however no effect on the aversion of boys to disadvantageous inequality. It also had no effect on the desire of either boys or girls to get ahead of the other child. The estimated effect of daycare on the behindness aversion of girls is robust to adjusting for multiple hypothesis testing.

We try to address concerns about external validity in two ways. First, we document that disadvantageous inequality is associated with the test scores that participants get in a standardized exam that 3rd grade students in public schools in Rio de Janeiro have to take, which suggests that the experimental measure of behindness aversion partly captures some of the aversion to

[39] 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.

disadvantageous inequality that participants may exhibit in real-life situations. Second, we reproduce results from Cappelen et al. (2020)'s publicly-available code that show that the causal effects of preschool on inequality aversion that they document are mostly driven by girls: Preschool seemed to have increased the inequality aversion of girls while the inequality aversion of boys remained roughly the same.

An important line of future research will be to identify the mechanisms though which early education affects social preferences. Attending daycare or preschool may affect children in at least two broadly-defined ways. One is that children's social preferences may change because of the experiences and interactions that they have at daycare or at preschool. In other words, it is what happens at daycare or preschool that is formative. Alternatively, it may be the changes that happen at children's homes when they start attending daycare or preschool that affect their inequality aversion. Children spend a substantial amount of time at daycare or at preschool and, as a consequence, spend less time at home with their families than they would have otherwise. This also frees caregivers to increase their labor supply, which in turn may increase household income (Attanasio et al. 2022). Appendix Table 17 and Appendix 18 look briefly at these hypotheses. The evidence indicates that daycare attendance has the largest effects on girls of mothers with less education, which *suggests* that differences between gender expectations at home and at the daycare center may play a role.

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Appendix Table 1: Attrition

	All		Boys		Girls	
	Application	2008 Survey	Application	2008 Survey	Application	2008 Survey
Treatment	0.04	0.03	0.03	0.03	0.03	0.04
	[0.01]	[0.02]	[0.02]	[0.02]	[0.02]	[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 was 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 experimental tasks. 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

	Surveyed × Treatment		*Surveyed*		*Treatment*		*Number of*
	Coeff.	SE	Coeff.	SE	Coeff.	SE	*Observations*
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: 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 × age group. The third column shows robust standard errors. N = 1,108.

Appendix Table 4: 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 5: First Stage

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

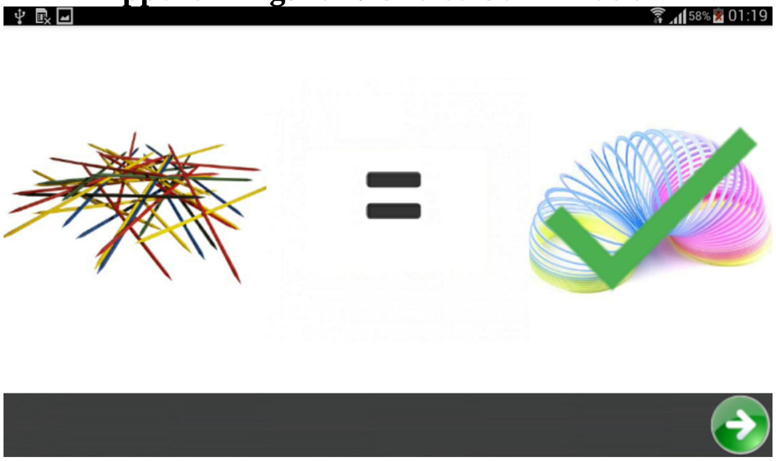
Notes: The table shows that treatment children were more likely to attend daycare than the control. The dependent variables are indicators for participants attending daycare for 0, 1, 2, 3, and 4 years. *N* = 1,857.

Appendix Table 6: First Stage, Separately by Gender

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

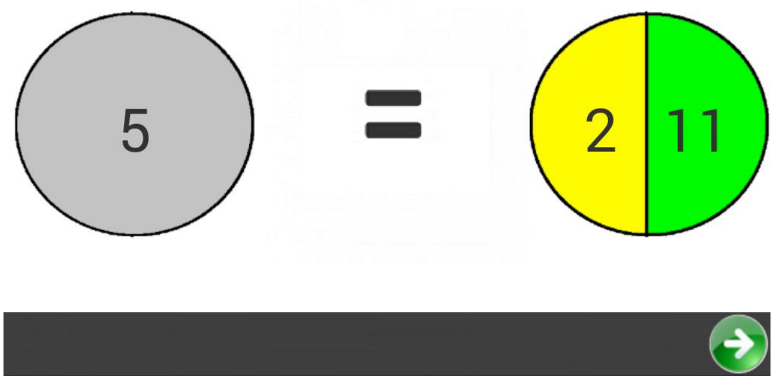
Notes: The table shows that treatment boys and treatment girls were more likely to attend daycare than their control counterparts. The dependent variables are indicators for participants attending daycare for 0, 1, 2, 3, and 4 years. *N* = 1,857.

Appendix Figure 1: 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 2: 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 Table 7: Intent-to-Treat Effects in the AI and DI Trials, Separately by Gender

	Costless DI (3,3) ≻ (3,6)	*Costly DI* (3,3) ≻ (4,6)	*Costless AI* (3,3) ≻ (3,1)	*Costly AI* (3,3) ≻ (2,0)
Boys * Treatment	-0.05	-0.10	-0.02	0.04
	[0.04]	[0.04]	[0.04]	[0.03]
Treatment	0.07	0.12	0.02	-0.03
	[0.03]	[0.03]	[0.03]	[0.02]
Boys	0.06	0.07	-0.06	-0.10
	[0.03]	[0.03]	[0.03]	[0.02]
Mean of Y among Control Girls	0.69	0.48	0.69	0.90

Notes: This table shows intent-to-treat estimates of the effect of daycare attendance on the fraction in each trial choosing the equitable allocation (3,3). All regressions include dummies specific for each childcare center × age group. Robust standard errors between brackets. N = 2,113.

Appendix Table 8: Intent-to-Treat Effects in the AI and DI Trials

	Costless DI (3,3) ≻ (3,6)	*Costly DI* (3,3) ≻ (4,6)	*Costless AI* (3,3) ≻ (3,1)	*Costly AI* (3,3) ≻ (2,0)
Treatment	0.04	0.07	0.01	-0.01
	[0.02]	[0.02]	[0.02]	[0.02]
Mean of Y among Control	0.72	0.52	0.66	0.86

Notes: This table shows intent-to-treat estimates of the effect of daycare attendance on the fraction in each trial choosing the equitable allocation (3,3). All regressions include dummies specific for each childcare center × age group. Robust standard errors between brackets. N = 2,113.

Appendix Table 9: Correlation between Outcome Measures

	Costless DI (3,3) ≻ (3,6)	*Costly DI* (3,3) ≻ (4,6)	*Costless AI* (3,3) ≻ (3,1)	*Costly AI* (3,3) ≻ (2,0)	Violations Transitivity	Risk Tolerance	Self-Control
Costly DI (3,3) ≻ (4,6)	0.44 (1,016)						
Costless AI (3,3) ≻ (3,1)	0.01 (1,016)	0.07 (1,016)					
Costly AI (3,3) ≻ (2,0)	-0.11 (1,016)	-0.14 (1,016)	0.41 (1,016)				
Violations Transitivity	-0.08 (1,016)	-0.05 (1,016)	-0.04 (1,016)	-0.02 (1,016)			
Risk Tolerance	-0.06 (1,016)	-0.06 (1,016)	-0.01 (1,016)	0.02 (1,016)	0.06 (1,016)		
Self-Control	0.12 (882)	0.05 (882)	0.05 (882)	-0.01 (882)	-0.02 (882)	-0.02 (882)	
Cognitive Functioning	0.26 (869)	0.24 (869)	0.21 (869)	0.05 (869)	-0.14 (869)	-0.11 (869)	0.13 (769)

Notes: This table shows the correlation between the different outcome measures. The sample is restricted to the control children. Number of observations between parentheses.

Appendix Table 10: Robustness to Multiple Hypothesis Testing

	# of Times Chose (3,3) in …		Violations Transitivity	Risk Tolerance	Self-Control
	DI Trials	AI Trials			
Treatment	0.11	0.01	-0.19	-0.01	-0.58
	[0.02]	[0.84]	[0.42]	[0.42]	[0.42]
Mean of Y among Control Children	1.24	1.52	2.86	0.46	19.82

Notes: This table investigates whether the estimated intent-to-treat effect of daycare attendance on aversion to disadvantageous inequality is robust to adjusting for multiple hypothesis testing. It reports between brackets Romano-Wolf p-values which adjust for multiple hypothesis testing (Romano and Wolf 2005a; 2005b). The first two dependent variables are respectively the number of times that the participant chose the equitable allocation in the DI trials (a measure of aversion to disadvantageous inequality) and the number of times that the participant chose the equal option in the AI trials (a measure of aversion to advantageous inequality). "Violations Transitivity" is the number of violations of transitivity in the experimental task in which the participant had to choose between two toys. "Risk Tolerance" is the coefficient of variation of the selected option (averaged over the 15 trials). "Self-control" is the number of minutes the child waited in the marshmallow task. All regressions include dummies specific for each childcare center × age group. Robust standard errors between brackets. N = 2,113 for all dependent variables except for "Self-Control". N = 1,858 for "Self-Control".

Appendix Table 11: Robustness of Grade Result

	Math Grade			*Portuguese Grade*		
# Times Chose (3,3) in DI Trials	0.13		0.13	0.15		0.15
	(0.04)		(0.04)	(0.04)		(0.04)
# Times Chose (3,3) in AI Trials		0.06	0.07		0.11	0.12
		(0.04)	(0.04)		(0.04)	(0.04)
Cognitive Function	0.44	0.46	0.43	0.37	0.38	0.35
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)

Notes: This table investigates whether the model predictions are supported by the data. This sample includes treatment and control children. 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 first independent variable is the number of times that the participant chose the equitable allocation in the DI trials. The second independent variable is the number of times that the participant chose the equitable allocation in the AI trials. All regressions control for a measure of IQ, the Wechsler Intelligence Scale for Children-IV. N = 1,006.

Appendix Table 12: Effects of CHECC Preschool, Separately by Gender

	Luck				Merit and Luck			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Preschool × Boys		0.07		0.05		0.08		0.07
		[0.11]		[0.11]		[0.09]		[0.09]
Preschool	-0.11	-0.14	-0.10	-0.13	-0.09	-0.13	-0.08	-0.12
	[0.05]	[0.07]	[0.05]	[0.07]	[0.04]	[0.06]	[0.04]	[0.06]
Boys		-0.02		-0.01		-0.06		-0.05
		[0.07]		[0.07]		[0.06]		[0.05]
Demographic Controls?	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 13: 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 14: 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

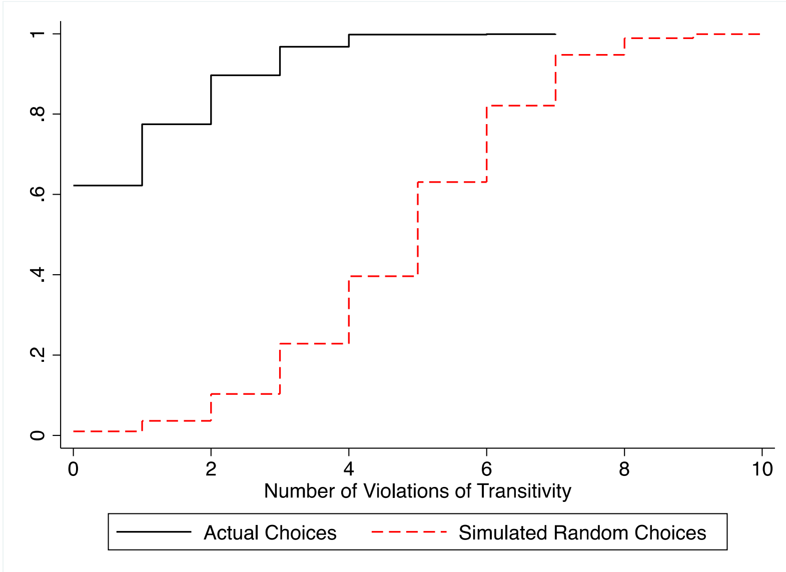
Notes: The table investigates whether control boys and boys assigned to the preschool arm in Cappelen et al. (2020) are comparable in terms of predetermined characteristics. The first column shows means among the control boys. The second column reports the difference in means between the control and 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 15: 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 3: Cumulative Distribution of Number of Violations of Transitivity in the Social Preferences Task



Notes: The black solid line shows the cumulative distribution function of the number of violations of transitivity of the control group in the social preferences task. The red dashed line shows the cumulative distribution function of the number of violations of transitivity obtained by simulating random choices. *N* actual choices = 1,016 individuals making in total 10,160 actual choices. *N* simulated choices = 10,000 individuals making in total 100,000 simulated choices.

Appendix Table 16: Lee Bounds

	Costless DI (3,3) ≻ (3,6)	*Costly DI* (3,3) ≻ (4,6)	*Costless AI* (3,3) ≻ (3,1)	*Costly AI* (3,3) ≻ (2,0)
Point Estimate (*N* = 1,005)	0.05	0.12	0.03	-0.02
	[0.03]	[0.03]	[0.03]	[0.02]
Relative to Application (*N* = 2,081)				
Lower Bound	0.02	0.08	-2.10E-03	-0.03
	[0.03]	[0.04]	[0.03]	[0.02]
Upper Bound	0.14	0.20	0.12	0.09
	[0.05]	[0.05]	[0.05]	[0.05]
Relative to 2008 Survey (*N* = 1,811)				
Lower Bound	0.03	0.09	0.01	-0.03
	[0.03]	[0.04]	[0.03]	[0.02]
Upper Bound	0.13	0.18	0.10	0.06
	[0.05]	[0.04]	[0.05]	[0.05]
Mean of Y among Control	0.69	0.48	0.69	0.90

Notes: As shown in Appendix Table 1, the attrition rate was higher for the control group than for the treatment group. The table investigates the sensitivity of the estimated treatment effects on girls to this differential attrition by estimating Lee Bounds (2009). The first row shows the point estimates. The rest of the table reports the bounds. The first set of bounds considers attrition relative to the sample of girls originally drawn to study the impacts of daycare attendance (*N* = 2,081). The second set of bounds considers attrition relative to the first survey conducted in 2008 (*N* = 1,811). The point estimates reported here are different from the point estimates reported in Table 6 because in order to make the point estimates reported here as comparable as possible to the estimated Lee bounds, the regressions run to obtain such estimates did not include dummies specific for each childcare center × age group (while the regressions shown in Table 6 did). Standard errors between brackets.

Appendix Table 17 and Appendix Table 18 look at two broadly-defined mechanisms for how daycare attendance affects the aversion of girls to disadvantageous inequality. One is that girls' social preferences may change because of the experiences and interactions that they have at daycare (hereafter, the "childcare center channel"). Alternatively, it may be the changes that happen at children's homes when they start attending daycare that affect their inequality aversion (hereafter, the "family channel"), such as spending more time at the daycare center and less time at home, an increase in the labor supply of mothers, and/or an increase in household income.

Appendix Table 17's first two columns investigate the childcare center channel by leveraging the lottery. In particular, they exploit the random deviation of actual average characteristics of the children selected in a lottery draft (for a given childcare class) from the average characteristics of the children who were entered in the draft. The first specification investigates whether the effect of daycare attendance depends on the fraction of classmates who were boys ("Fraction Classmates Boys" is the deviation of the realized gender composition of lottery winners from the gender composition of applicants). The second specification investigates whether the effect depends on how old classmates were ("Average Age of Classmates" is the deviation of the realized average of lottery winners from the average age of applicants). We do not find evidence for either of these two hypotheses. The third specification uses the number of candidates per slot as a *proxy* for the quality of the daycare center under the assumption that there is more competition for higher-quality daycare centers ("Acceptance Rate" is the number of lottery winners divided by the number of applicants). Finally, the fourth specification calculates a daycare-center-specific treatment effect on weight-for-age as an alternative *proxy* of quality ("Center-Specific Effect on Weight-for-Age" is the average weight-for-age of children selected in drafts of a given daycare center minus the average weight-for-age of children who were entered in these drafts but were put on a waiting list).[40] We find no evidence that supports the childcare center channel.

Appendix Table 17: Mechanisms – Daycare Centers

	Mechanisms Childcare Center			
	Number of Times Chose (3,3) in DI Trials			
Girls * Treatment	0.19 [0.06]	0.19 [0.06]	0.19 [0.06]	0.19 [0.06]
Girls * Treatment * Fraction Classmates Boys	0.25 [0.62]			
Girls * Treatment * Average Age of Classmates		-0.36 [0.97]		
Girls * Treatment * Acceptance Rate			-0.27 [0.31]	
Girls * Treatment * Center-Specific Effect on Weight-for-Age				-0.02 [0.06]
Mean of Y among Control	1.24	1.24	1.24	1.24
Observations	2,110	2,110	2,110	2,085

[40] We use at weight-for-age because the evidence indicates that this is one of the outcomes that was most affected by daycare attendance, it is available for a large fraction of the overall sample, and it was measured in multiple waves.

Appendix Table 18 investigates the family channel. The first column reproduces the intent-to-treat treatment effect of daycare attendance on the inequality aversion of girls (measured as the number of times they chose (3,3) in the two DI trials). Columns (2), (3), and (4) investigate what happens to the estimate of the treatment effect when we control for proxies for potential mediators, namely the number of childcare hours, the number of work hours of the caregiver, and household income. While daycare attendance had large effects on all of these three variables (reducing childcare hours, increasing workhours and household income), there is no evidence that they mediate the treatment effect on inequality aversion. In the last column, the indicator for treatment assignment is interacted with indicators for whether the caregiver completed middle school and whether the caregiver completed high school (37% of the caregivers of control girls completed middle school and 30% completed high school). The evidence shows that daycare attendance had the largest effects on girls of less educated mothers.

Appendix Table 18: Mechanisms – Family

	Mechanisms Family				
	Number of Times Chose (3,3) in DI Trials				
Girls * Treatment	0.20 [0.06]	0.18 [0.06]	0.20 [0.06]	0.20 [0.06]	0.36 [0.10]
# of Childcare Hours		-0.01 [0.01]			
# of Workhours			1.5E-03 [0.01]		
Household Income				0.05 [0.06]	
Girls * Treatment * Caregiver Middle School					-0.27 [0.13]
Girls * Treatment * Caregiver High School					-0.21 [0.12]
Mean of Y among Control	1.25	1.25	1.25	1.25	1.25
Observations	2,013	2,013	2,013	2,013	1,825

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.

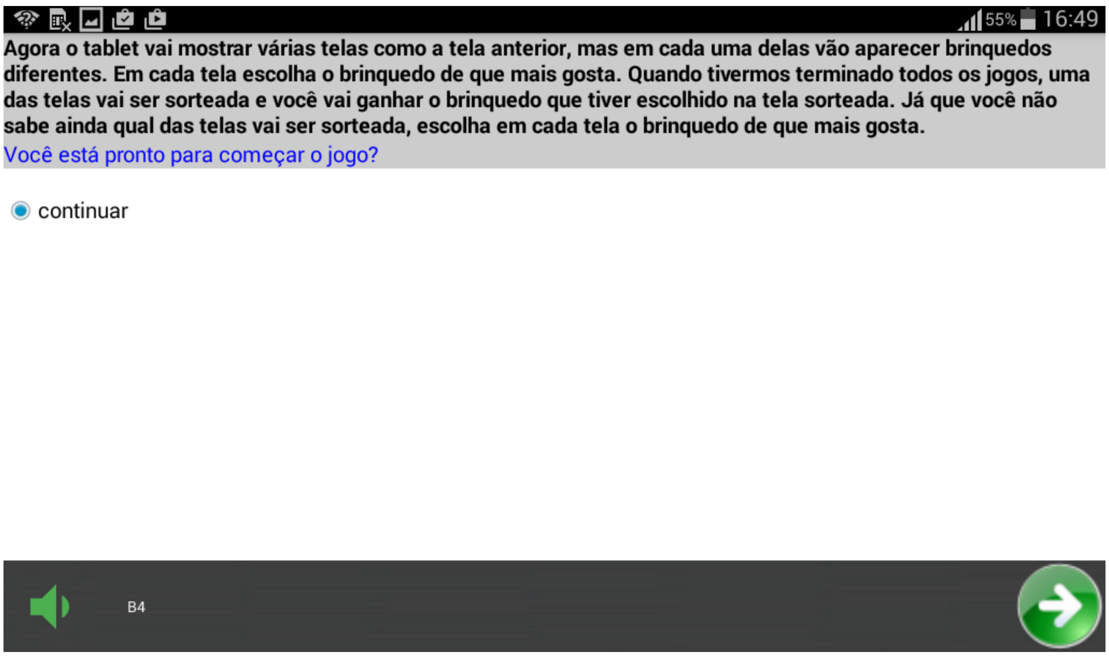
B4 ♫

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?



M1 ♫

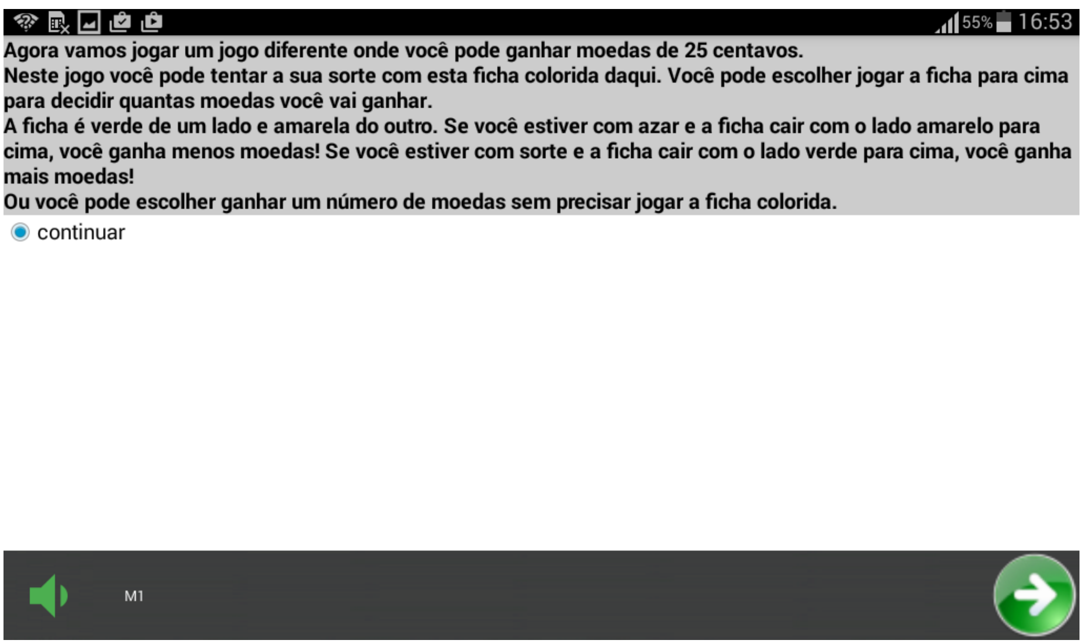
Now let's play a different game in which you can win 25 cent coins *(show a 25 cent coin).*

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 ♫

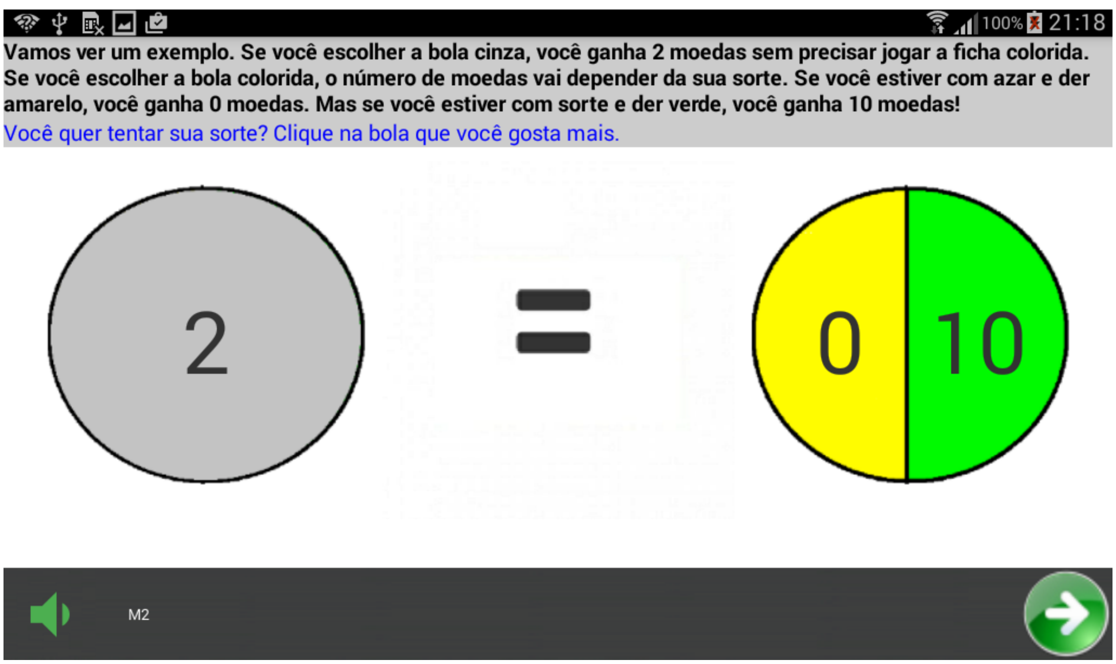
Let's look at an example.

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)*.



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** >

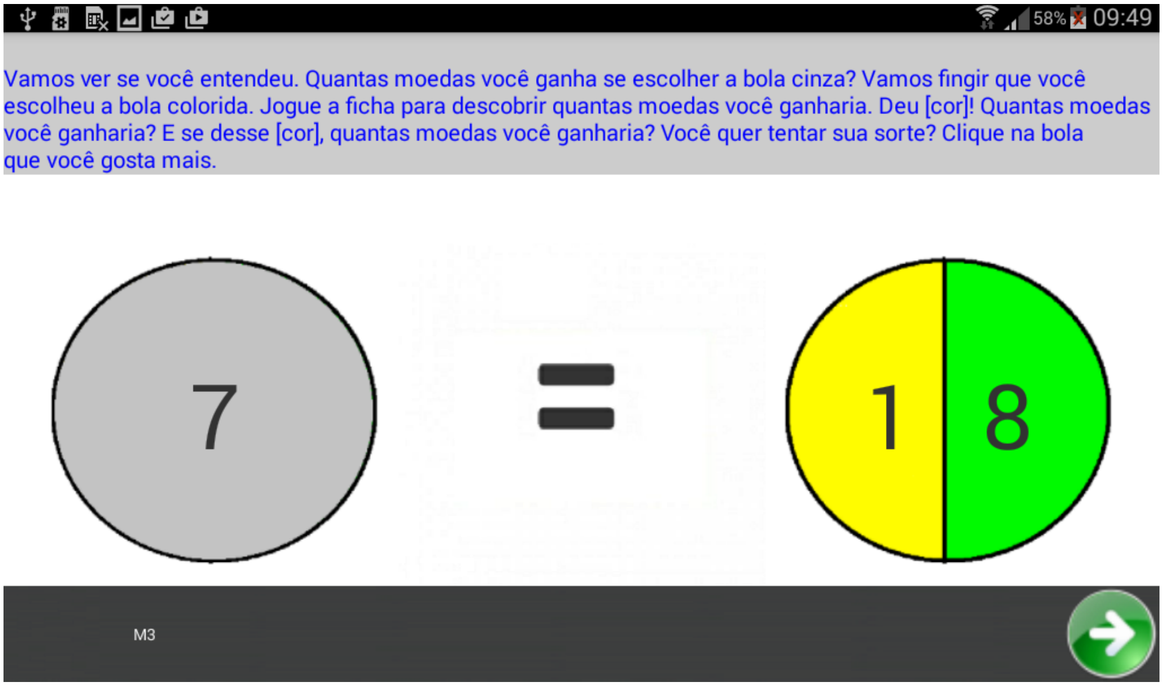
No, if you chose the colored circle *(point to the colored circle)* and were unlucky enough for it to land on yellow, you would get 1 coin *(point to number 1).*

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.



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.

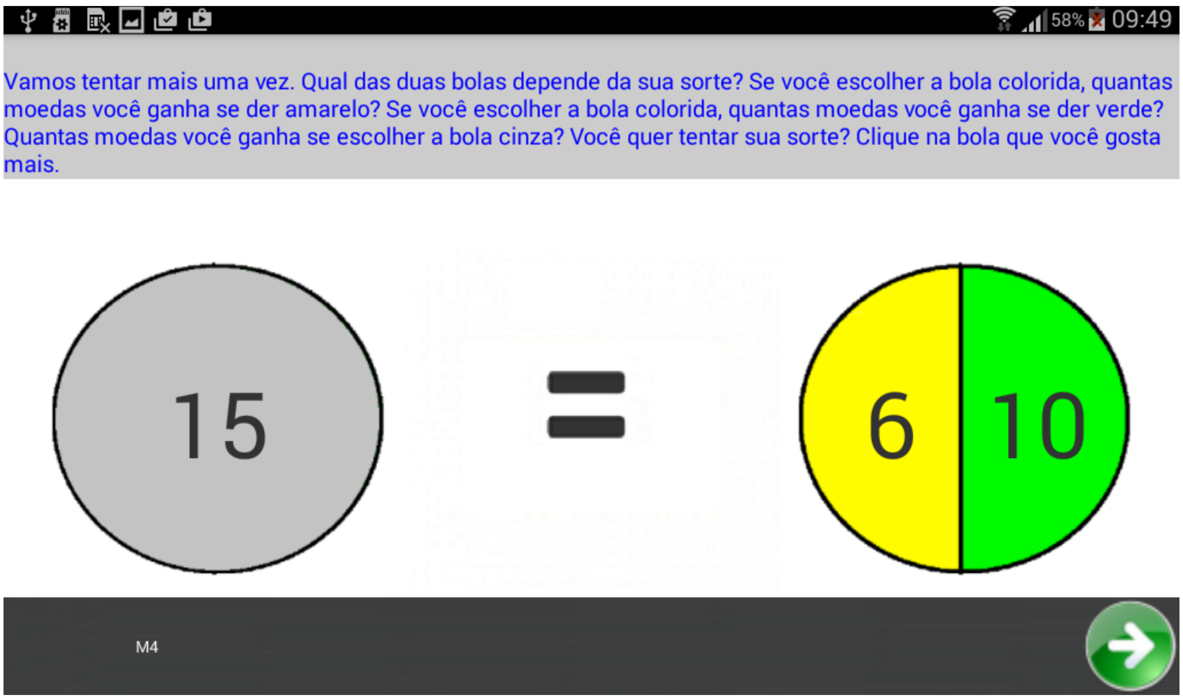
< IF THE ANSWER IS WRONG >

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.



M5 ♫

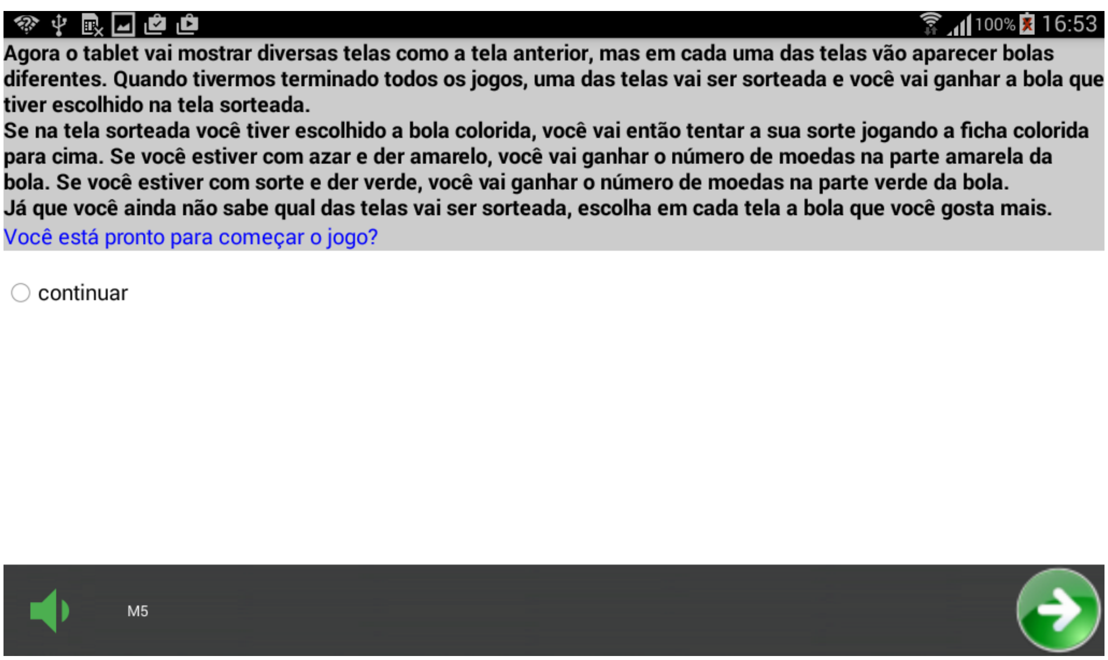
Now the tablet will show several screens like the previous screen, but different circles will appear on each screen. When we've finished all the games, one of the screens will be drawn and you'll get the circle you've chosen on the drawn screen.

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.

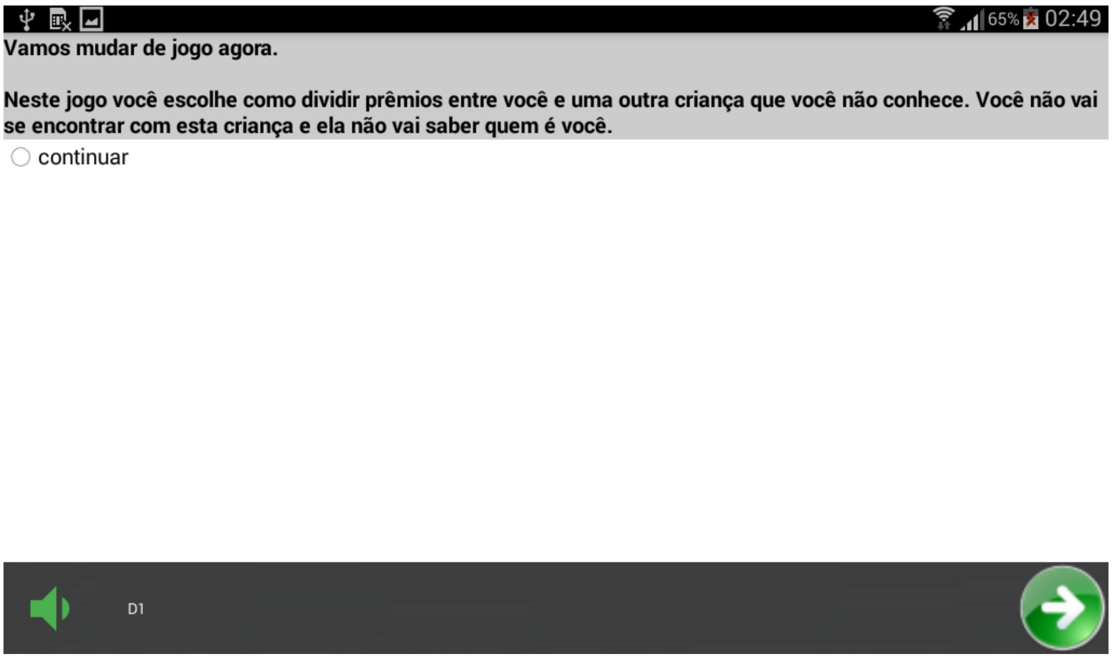
Are you ready to start the game?



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 ♫

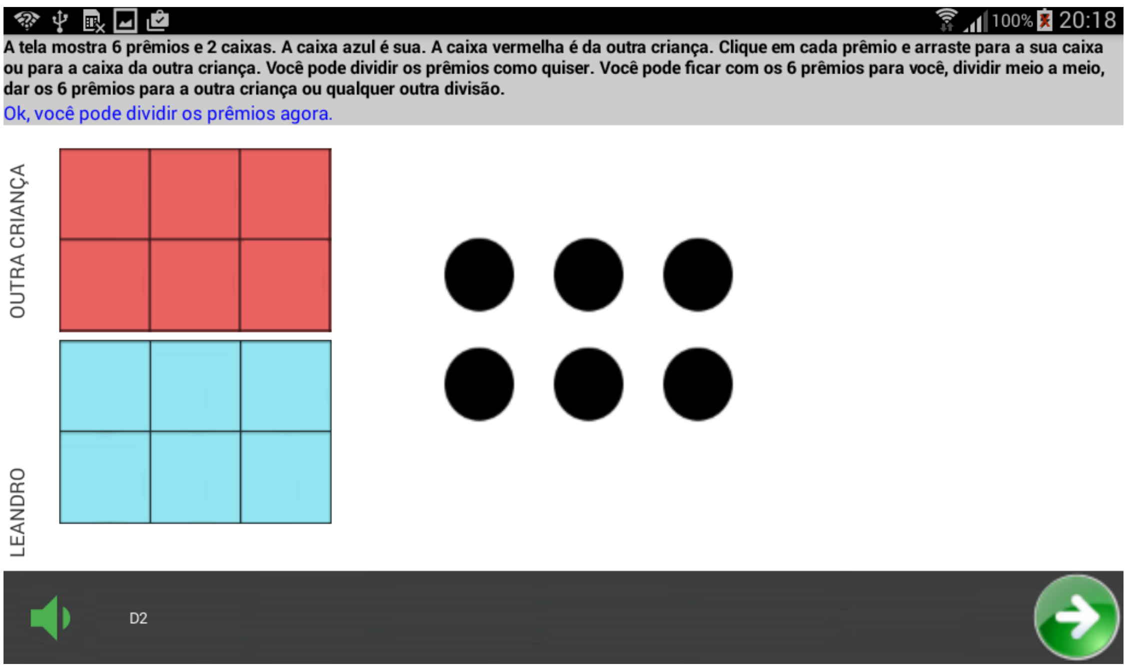
The screen shows 6 prizes *(point to the 6 circles)* and 2 boxes *(point to the two boxes)*. The blue box is yours *(point to the box underneath)*. The red box is the other child's *(point to the top box)*.

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.



D3 ♫

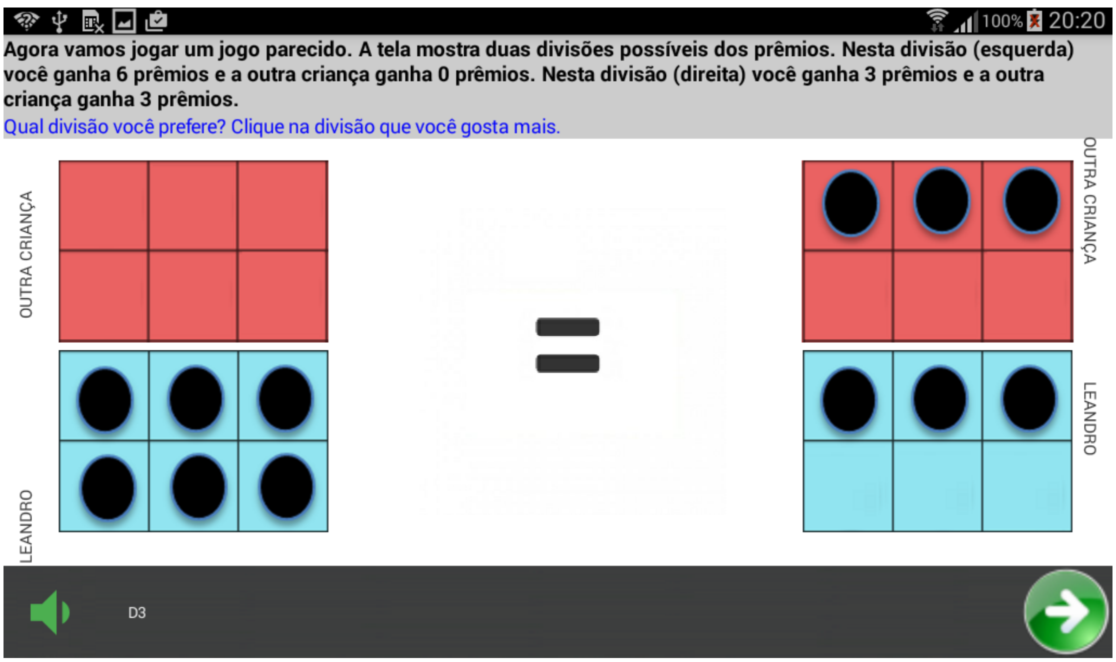
Now let's play a similar game.

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 >

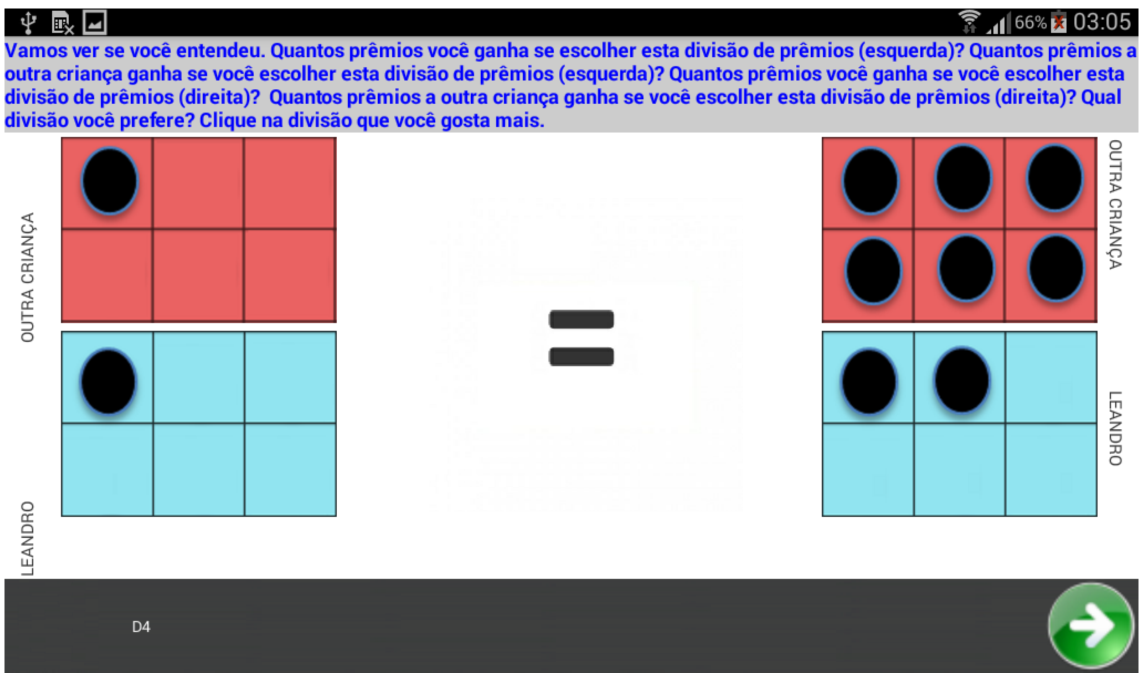
Ok. The other child wins 6 prizes if you choose this prize division *(point to the box in the upper right corner).*

< 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.



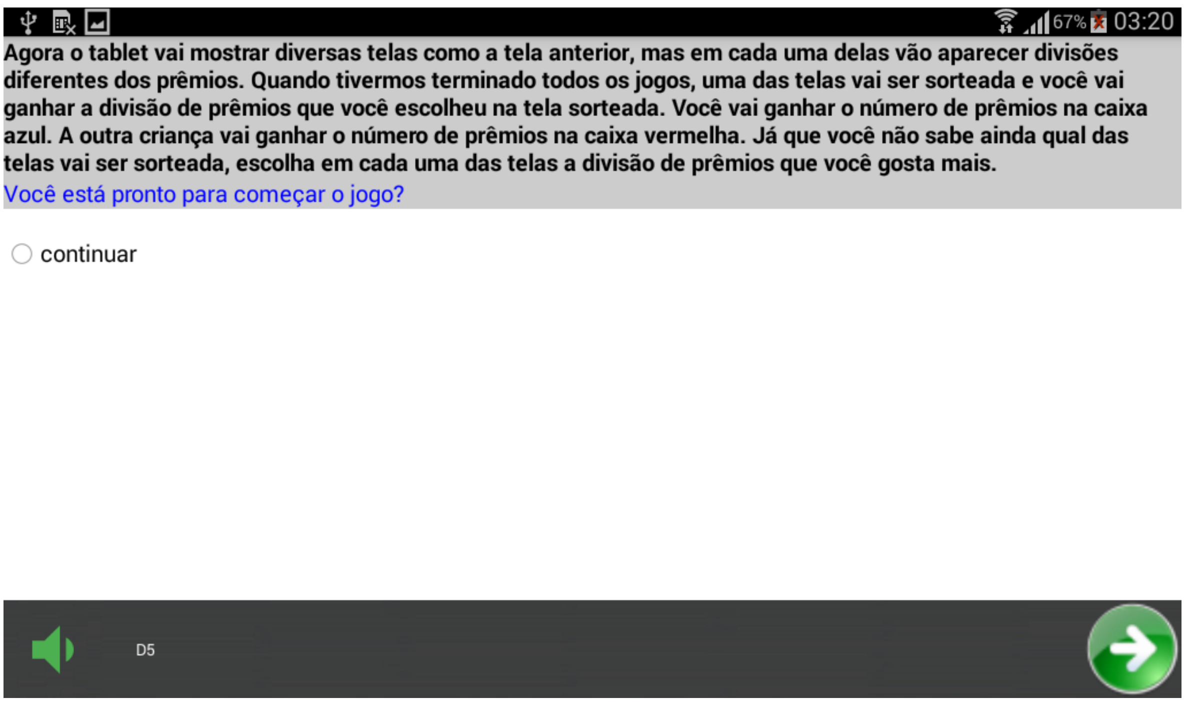
D5 ♫

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?



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 nº1 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 nº2.*
15. Don't eat it yet. I have to tell you the rules of the game first.
16. *Place the plate nº2 with the bonbon on top of the empty plate nº1.*
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?

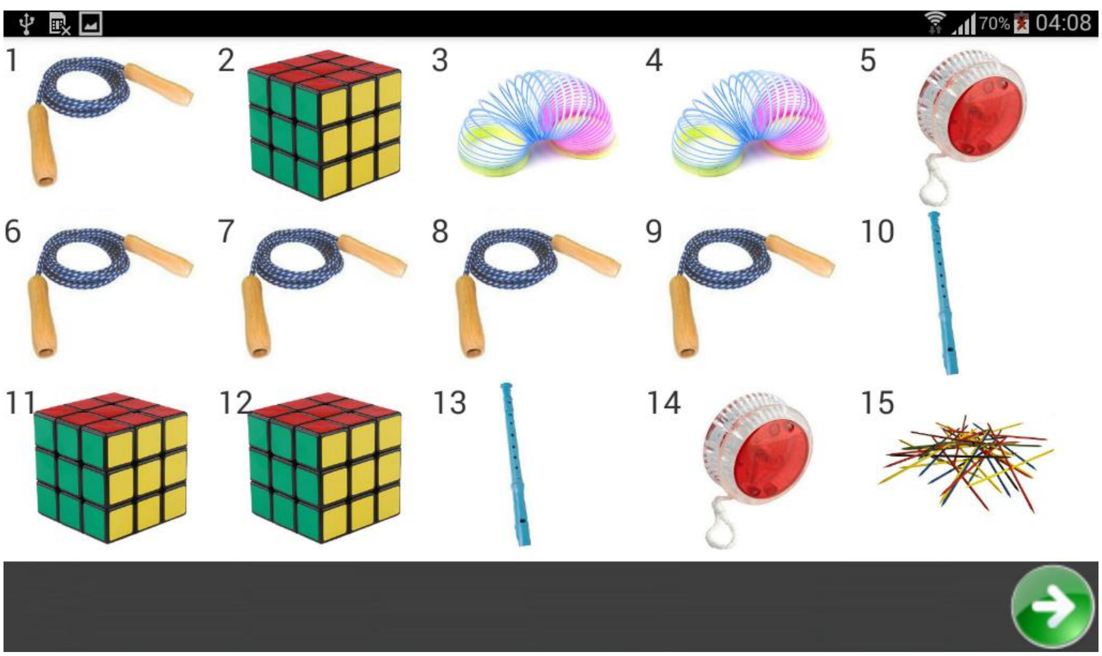
First, let's find out which toy you will win. This screen shows all 15 choices you made in the toy game *(show the tablet screen to the child so he or she can see it).*

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 >

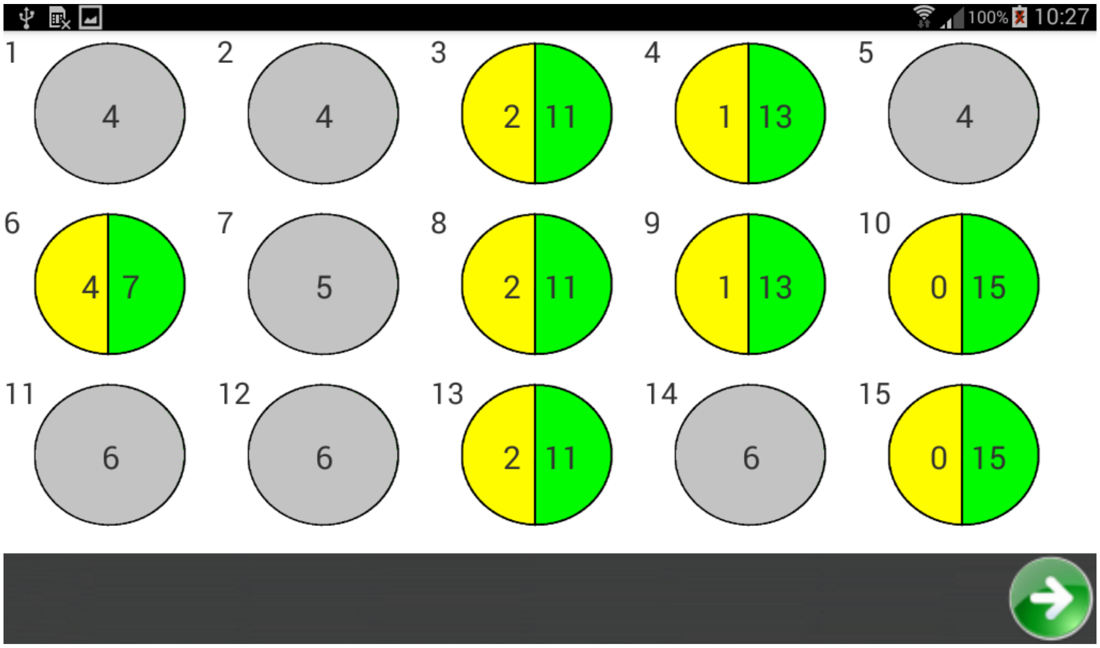
If it lands on yellow, you win *(say the number on the yellow part of the circle below the number drawn)* coin(s).

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.

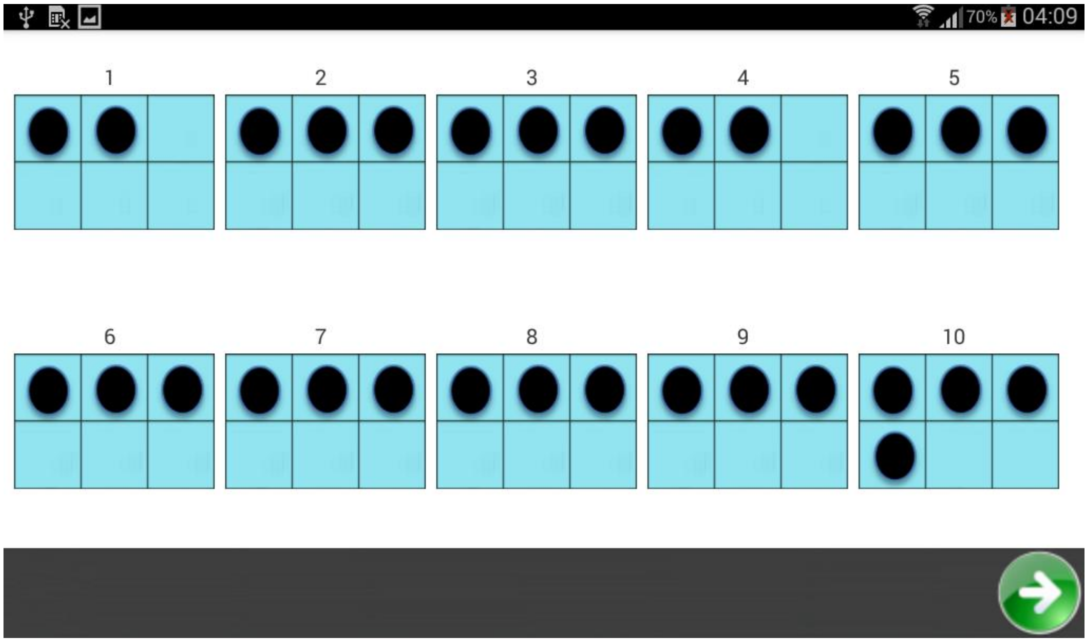
You will win the number of prizes shown below the number that comes out on the die. If, for example, the die lands on number 1, you win *(say the number of prizes below number 1)* prizes.

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?"]*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