Journal of Development Economics 185 (2027) 103945

Contents lists available at ScienceDirect

Journal of Development Economics

journal homepage: www.elsevier.com/locate/devec

Regular Article

Cash transfers and social preferences of children

Johannes Haushofer [a,1], Magdalena Larreboure [b,2], Sara Lowes [c,d,e,f,*,3], Leon Mait [g]

[a] Cornell University, Department of Economics and Brooks School of Public Policy, United States of America
[b] Harvard University, Public Policy, GSAS, United States of America
[c] UC San Diego, Department of Economics, United States of America
[d] NBER, United States of America
[e] BREAD, United States of America
[f] CEPR, United States of America
[g] Princeton University, Department of Psychology, United States of America

ARTICLE INFO

Dataset link: 10.17632/xt9g8tmsvg.1

JEL classification:
D31
I38
C93
C92
O12

Keywords:
Cash transfers
Social preferences
Children
Behavioral games

ABSTRACT

We study the effects of a large unconditional cash transfer program on social preferences of children in rural Kenya using a randomized controlled trial. We measure the social preferences of 4022 children with survey questions and incentivized behavioral games three years after the cash transfer. First, we find no persistent economic effects of the program. Additionally, we find no consistent evidence that children from the treatment group or that children from the spillover group are more or less prosocial than children from the control group. However, we find some evidence of reduced psychological well-being among adults and children in spillover households.

1. Introduction

Social preferences affect a variety of economic outcomes and behaviors (see e.g., Falk et al., 2018; Fehr et al., 2022). While an extensive literature has focused on the causes and consequences of prosocial preferences in adults, there is less evidence on what shapes prosocial behavior in children. Understanding the drivers of prosocial behavior in children is important, as their preferences are still forming at this developmental stage, and behaviors learned in childhood may carry over into adulthood.

Much of the literature on social preference formation in children has focused on the role of education and socialization (e.g., Huppert et al., 2019; Kosse et al., 2020; Rao, 2019). In this paper, we examine the effects of household wealth. In adults, socioeconomic status (SES) has been shown to correlate with various social preferences, such as redistribution preferences (Andersen et al., 2020; Fehr et al., 2022; Andreoni et al., 2017), generosity and trust (Korndörfer et al., 2015; Piff et al., 2010), and feelings of entitlement (Côté et al., 2021). In children, there is mixed correlational evidence that those from higher-SES families behave more prosocially than other children (Bauer et al., 2014; Benenson et al., 2007). In a review of the literature on economic games in children and adolescents, Sutter et al. (2019) find that a number of studies demonstrate children from lower-SES backgrounds are less altruistic and more spiteful than their higher-SES counterparts. For instance, Kosse et al. (2020) report that children from higher-SES households are about 22.6% of a standard deviation more prosocial than those from lower-SES households. However, a number of other studies have found the opposite — that children from lower-SES are more altruistic (Chen et al., 2013; Miller et al., 2015).

That said, there is limited causal evidence on the effects of wealth on social preferences in children, primarily because there is rarely exogenous variation in wealth. We leverage an unconditional cash transfer

* Corresponding author at: UC San Diego, Department of Economics, United States of America.
E-mail addresses: haushofer@cornell.edu (J. Haushofer), mlarreboure@fas.harvard.edu (M. Larreboure), slowes@ucsd.edu (S. Lowes), lmait@princeton.edu (L. Mait).
[1] Website: www.johanneshaushofer.com.
[2] Website: www.mlarreboure.com.
[3] Website: www.saralowes.com.

https://doi.org/10.1016/j.jdeveco.2026.103945
Received 26 December 2025; Received in revised form 8 September 2026; Accepted 11 September 2026
Available online 18 September 2026

(UCT) program in Kenya in which households randomly received a large UCT to examine how receiving a large cash transfer affects the formation of social preferences in children. Kenya is an appropriate setting for this study not only because unconditional cash transfers have become an important anti-poverty program there, but also because the average Kenyan resembles the average world citizen much more closely than the average North American or European (Henrich et al., 2010).

Between 2017 and 2018, the Busara Center implemented a UCT program in Nakuru County, Kenya. Of the 120 villages in the study sample, 60 villages were chosen as treatment villages. Within treatment villages, households were eligible for the program if they were low income (as indicated by living in a house without brick, stone, or metal walls). In treatment villages, 540 eligible households were randomly selected to receive a cash transfer of KES 50,000, which was approximately USD 1076 PPP at the time of the study. This transfer corresponds to about 20 months of per capita control group consumption. Additionally, 1077 eligible households neighboring the treatment households were selected as a spillover group. In the 60 control villages, 1545 households who met eligibility requirements comprised the control group.

An evaluation of this UCT one year after the intervention found that it increased recipients' monthly consumption by 20% and asset holdings by 47% relative to the control group (Haushofer et al., 2020). The study also found evidence of psycho-social benefits: adults in treated households reported greater well-being and lower stress. The initial evaluation found no evidence of negative effects on spillover households in terms of consumption, assets, or psychological well-being.[4]

To measure the medium-run effects of the UCT on the social preferences of children, we implement surveys and behavioral games with children aged 6 to 17 at the time of our study in treatment, spillover, and control households in 2021. Our final sample includes 4022 children (820 from treatment households, 1308 from spillover households, and 1894 from control households) from 1687 distinct households. All children in a household that fall within the targeted age range are included in the sample. The surveys include modules measuring mental health, envy, trust, social closeness, sharing norms, zero-sum thinking, and locus of control. The behavioral games are standard economic games that we modified to be appropriate for children and adolescents and for the context. The games include the dictator game (DG), ultimatum game (UG), third party dictator game (3PD), joy of destruction game (JOD), and public goods game (PGG). We also survey adult household heads to measure the medium-run impacts of the UCT transfer on consumption, assets, and well-being.

We find several key results. First, we examine whether the UCT had medium-run economic and psychological effects on recipient households and their neighbors. Three years after the program was completed, treatment households showed no significant increases in monthly consumption or total assets, despite significant gains documented one year after the cash transfer. The lack of longer-run impacts in our setting is consistent with evidence on limited persistence of both one-off, lump-sum transfers, and ongoing transfers after they conclude (Haushofer and Shapiro, 2018; Blattman et al., 2020; Baird et al., 2019). At the same time, the broader cash-transfer literature documents sustained positive impacts on consumption, income, assets, and psychological well-being, particularly for ongoing programs (Crosta et al., 2024). Our results therefore contribute specifically to the medium-run evidence on completed transfers, where persistence is more limited.

Adults in the treatment group do report higher levels of happiness, but the estimated effect of the treatment on an index of various psychological questions is close to 0 and statistically insignificant. However, we find some evidence of negative spillover effects of treatment on psychological well-being. Adults in spillover households report lower levels of psychological well-being, particularly a decrease in life satisfaction and an increase in depression. Consistent with the survey results for adults, children in spillover households also report lower life satisfaction and higher levels of depression. These results suggest the presence of some negative spillover effects several years after the program. This is consistent with some isolated findings of negative psychological spillovers in the cash transfer literature (Baird et al., 2013). However, it is not consistent with the bulk of the evidence, which suggests that spillover effects on non-recipient well-being are close to zero (Crosta et al., 2024; McGuire et al., 2022). One possibility for the negative spillovers we observe here is that short-run psychological spillovers are positive because they track positive consumption spillovers (Egger et al., 2022); but when the direct consumption gains fade over time, what remains are differences in visible markers of wealth — e.g. durable assets, housing, livestock — and these might generate social-comparison effects. A second possibility is that cash injections to nearby households disrupt pre-existing informal sharing or risk-pooling arrangements, reducing the resources or social support available to non-recipients in the medium run.

We then examine how the UCT program has shaped the social preferences of children in treatment and spillover households relative to control households three years after the program. Because of the lack of medium-run economic effects, these results speaks to the question whether cash transfers can have social effects that last beyond the economic effects.[5] There are several candidate mechanisms that could generate persistent impacts of a change in household resources during childhood on preferences later on, even if the initial economic gains are not persistent. First, additional resources may relax constraints on investments in children's human capital, including schooling, nutrition, and health, which in turn can shape cognitive and socio-emotional development (Heckman, 2006; Almond and Currie, 2011). Second, higher household income may alter children's time use during formative periods, e.g. by reducing child labor and increasing opportunities for play and social interaction, which may influence the formation of norms and social preferences (Attanasio et al., 2010; Edmonds, 2006). Third, improvements in household financial security may reduce stress and improve psychological well-being among both parents and children, potentially affecting parenting practices and the environment in which preferences are formed (Baird et al., 2013; Evans and Kim, 2013; Haushofer and Shapiro, 2016; Mani et al., 2013).

The children complete five standard economic exchange games with anonymous children from a community like their own. They are told the age and gender of the other child with whom they are paired in each round of the game. Compared to children in control households, treated children do not behave differently in their allocation of resources to other players in the individual games (DG, UG, JOD, 3PD, and PGG) or when considering an overall pro-social behavior index. Thus, we find no evidence that the cash transfer increased altruism or adherence and enforcement of social norms, nor that it affected the willingness to destroy another's endowment or contribute to a public good.

While we find no positive effects on prosocial behavior for children in treated households, reassuringly, we find only weak evidence of negative effects of the cash transfer program on the prosocial behavior of children in spillover households. Spillover children do not behave differently in their allocation of resources to other players in the DG, UG, JOD, 3PD, and PGG, nor when considering an overall pro-sociality

[4] The original study also included a treatment group that received a five-week psychotherapy program, "Problem Management Plus", and a group that received both the cash transfer and the psychotherapy. The psychotherapy alone had no statistically significant impact on any of the main outcomes, and the impacts of the combined cash and psychotherapy treatment were similar to those of cash alone. In this paper, we recruit members of households in "cash-only" villages, spillover households in "cash-only" villages, and control households; we exclude those that received the psychotherapy program.

[5] However, our results still hold when examining heterogeneous effects for the subset of households that did experience medium-run economic benefits from the cash transfer.

index. Importantly, our relatively large sample size enables us to rule out changes larger than 4 percentage points (pp) for all games except the JOD, for which we can reject changes larger than 10 pp. This suggests that living near families that received the transfers did not have a negative impact on the prosocial preferences of children in spillover households. The point estimates for the share of the endowment sent in the DG and share offered in the UG are negative and become significant when including enumerator fixed effects, but these effects are small in magnitude — less than a 3% change relative to the control group mean.

The survey measures of prosocial norms and behavior mirror these null findings. We observe no changes for children in both treatment and spillover households in our survey measures of benign or descriptive envy, trust, perceived social closeness, zero-sum thinking, locus of control, or perceptions about sharing. An exception is malicious envy, where children in treatment households report higher levels of envy.

Overall, we find little effect of UCTs on the prosocial behavior of children three years after the program, both in the treatment and in the spillover group. However, this null effect may mask heterogeneity by "other player" characteristics. We hypothesized that the effects of cash transfers may differ based on the relative wealth of the other player. Therefore, for a subset of the games (DG, UG, and JOD), the children complete two additional rounds in which we also tell them whether the other player is from a household that is richer than their own or poorer than their own. The order of these subsequent two versions is randomized for each individual and game. We find little evidence for behavior differences when children are paired with poorer versus richer partners. However, in the JOD, children from spillover households are more likely to destroy the endowment of partners from poorer households. Thus, spillover children are not less prosocial in general, but they are potentially more spiteful towards those who are less well-off. We also examine heterogeneity by respondent characteristics — such as age, gender, and baseline wealth. We find no consistent evidence of heterogeneity.

Our results contribute to understanding how wealth shapes prosocial behavior and preferences. A large literature in political economy and economics has examined preference for redistribution and how this varies by income (Fehr et al., 2022; Korndörfer et al., 2015; Piff and Robinson, 2017). We build on this literature by leveraging experimental variation in a large wealth shock. Thus far, the evidence on the relationship between prosociality and SES among children has been mixed, with studies documenting evidence in both directions (Bauer et al., 2014; Benenson et al., 2007; Chen et al., 2013; Miller et al., 2015; Sutter et al., 2019). In fact, in our control group, the correlation between baseline household assets and pro-social behavior is negative — children from wealthier households are less prosocial (see Appendix Figures A.2 and A.3). We provide causal evidence that an exogenous increase in wealth does not have a causal effect on prosocial behavior among treated children.

Second, we contribute to the literature on social-preference formation in children (for a review, see Sutter et al., 2019; List et al., 2023). Childhood is an important time for the formation of skills and behaviors that affect life outcomes (Almås et al., 2010; Cappelen et al., 2020; Fehr et al., 2008; Heckman, 2006; Kosse et al., 2020; Cobo-Reyes et al., 2020). Even short-term shocks can affect long-run preferences of children and young adults, particularly during the so-called "impressionable" years (see e.g. Giuliano and Splilimbergo, 2024). Past work has focused on the role of schooling, socialization, and SES (Cappelen et al., 2020; Kosse et al., 2020), as well as how exposure to individuals from different backgrounds may affect social-preference formation (Rao, 2019).

We build on the literature on the social-preference formation in children in several ways. First, we build a dataset with extensive survey and experimental data from over 4000 children in a setting where children are underrepresented in research.[6] This allows us to provide well-powered evidence on the causal effect of a wealth shock on the formation of children's social preferences. Second, we estimate the effect of an exogenous wealth shock on both children of recipient households and children of nearby spillover households. Thus, we can measure both the direct and indirect effects of exposure to a wealth shock. We also examine heterogeneity by the partner's relative wealth and other characteristics. Our results suggest that overall, such shocks do not have a significant effect on social preferences of children in treatment households after three years. With 80% power, we can reject effect sizes larger than 3 pp for the DG, and between 2–10 pp for other games.

Finally, we speak to the literature on the effects of UCTs. Many papers have documented the positive benefits of UCTs for participants' assets, consumption, food security, and psychological well-being. Arnold et al. (2011), Baird et al. (2013) and Haushofer and Shapiro (2016). We find that these benefits do not persist three years after the intervention, consistent with Baird et al. (2019) and Blattman et al. (2020). Recent work explores the effects of cash transfers on adult preference formation (Cerkez et al., 2023), finding no long-run effects. We contribute to this literature by showing that cash transfers have little impact on the social preferences of children three years later. It is important to note, however, that this result is obtained in a setting where cash transfers also had no large impacts on other outcomes at this time horizon. Additionally, recent studies provide mixed evidence on potential unintended consequences of cash transfers, including price effects (Filmer et al., 2021; Egger et al., 2022). Our results complement this literature by showing negative spillover effects of UCTs on psychological well-being for adults and children.

2. Study design

2.1. Unconditional cash transfer treatment

Our sample includes individuals who participated in a RCT evaluating UCTs, conducted between May 2017 and January 2018 in Nakuru County, Kenya by the Busara Center (Haushofer et al., 2020). Households were sampled as follows (see Fig. 1). First, 60 villages in Nakuru County were randomly selected to receive the cash treatment. Potential study households within each village were identified by houses built without brick, stone, or metal walls, as a proxy for low income. Once identified, ten of these houses in each village were randomly selected. The two eligible households nearest to each of the initially selected ten were also included in the sample. Within each cluster of three households, one was randomly selected to receive the UCT, while the other two acted as a "spillover" condition. One adult in each of the selected households was chosen as the recipient (in the case of the treatment households) or the survey participant (in the case of the spillover households). Another 60 villages acted as "control" villages, for which participants were simply surveyed without any intervention. Eligible control households were identified in the same manner as in the treatment condition, with every household participating in the survey.

In the original trial, those in the cash treatment group (i.e., 540 households across 60 villages) received a UCT of KES 50,000 (equivalent to USD 1076 PPP at the time of the study). Cash transfers were delivered either as lump-sum transfers or in five weekly installments of KES 10,000, randomly assigned at the village level.[7]

[6] As a point of reference, the next largest sample is from Rao (2019), with 2362 children and Huppert et al. (2019), with 2000 children. Other sample sizes in articles on social preferences in children are: Bauer et al. (2014): 1000, Kosse et al. (2020): 607, Almås et al. (2017): 523, Chen et al. (2013): 469, Benenson et al. (2007): 360, Harbaugh and Krause (2000): 310, Cappelen et al. (2020): 300, Fehr et al. (2008): 229, Sutter (2007): 200, and Ben-Ner et al. (2017): 147.

[7] The differences between these two conditions are not of interest to the current study.

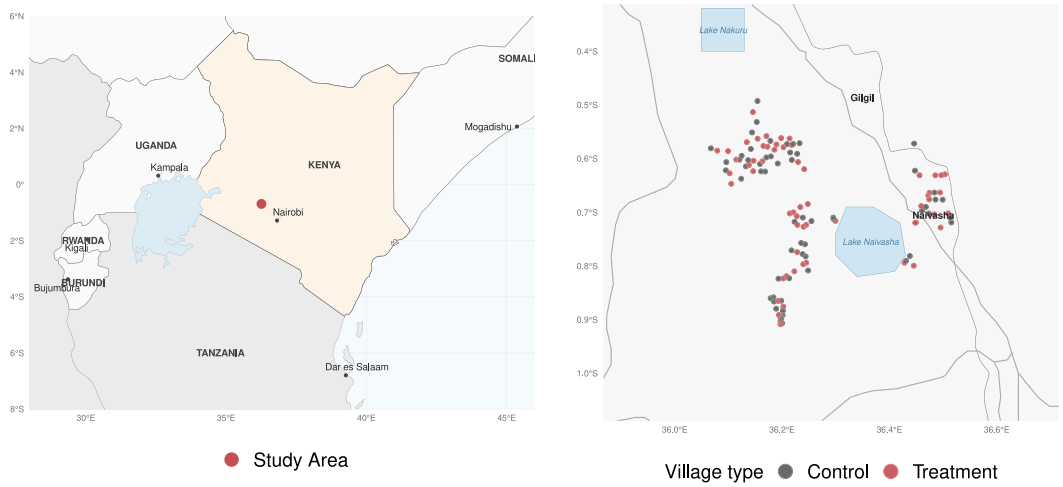

Fig. 1. Study area. Notes: The figure presents the control and treatment villages included in our study in Nakuru County, Kenya.

Before participants received a cash transfer, they were contacted over the phone or in person by a Busara field officer, who informed them they had been entered into a lottery and had been selected to receive KES 50,000. The field officer emphasized that the cash transfer was entirely unconditional. They told participants: "The money is yours to do whatever you like with – we have no preferences about what you do with the money. You should use it however you think best".

We follow up with the study sample three years after the completion of the study. This allows us to look at the medium-to-longer run effects of cash transfers on the prosocial preferences of children. However, given that these data were not collected immediately after the program, we cannot speak to the short-run effects of the cash transfers on social preferences. We think that the medium-run effects should be of interest given that the socialization of children happens over many years, and a large literature suggests that even transient shocks in childhood can shape adult behavior (Giuliano and Splilimbergo, 2024).

2.2. Sampling strategy

Between August 2018 and May 2019, 521 treatment, 1045 spillover, and 1473 control households participated in the endline survey described above. Among them, 362 treatment, 733 spillover, and 1019 control households had at least one child aged 6–17 years.[8] Our sample of children was selected from those children who were present at both the baseline and endline rosters of the initial study and who are in the targeted age range. Due to budget constraints, we did not collect data from every child in the full sample. Rather, we selected all treatment households and interviewed all children in our target age range in these households. To select households from the spillover and control groups, we randomly dropped the same number of households from spillover and control households (155 each). Dropping full households from the sample allowed us to collect data from all children in the targeted age range within the remaining households.

In total, we completed the surveys and behavioral games with 4022 children in 1687 households (93% of the sample of children that met our criteria). This corresponds to 820 children in the treatment households, 1308 children in the spillover households, and 1894 children in the control households. We also targeted one adult per household for a short survey with modules on assets, consumption, and psychological well-being. We were able to complete 95% of adult surveys, corresponding to 1656 adults when we restrict the sample to those whose children also completed the activities. We report tracking rates at the household level by treatment group in Table A.1 and by baseline characteristics in Table A.2. We did not experience differential attrition of children or adults by treatment status, nor by baseline characteristics. We also show balance on baseline observables across treatment arms in our final sample in Table A.3. We compare baseline characteristics at the household level, and demographics and game comprehension at the child level. Our sample is mostly balanced across treatment arms, apart from higher M-Pesa access (a mobile money transfer system that allows people to deposit and withdraw money at numerous locations throughout the country) and slightly lower psychological well-being in the treatment group.

2.3. Data collection

We contacted selected households to collect the survey and game data from children and survey data from adults. The behavioral games and surveys were completed in one-on-one sessions with the respondent and the enumerator. Each session consisted of a survey and standard economic games (i.e., DG, UG, 3PD, JOD, and PGG). The survey and the games were administered on touchscreen tablets, using SurveyCTO software for the surveys and oTree software for the games. Field officers read instructions to the participants in Swahili or English, depending on participant preference, and provided examples of how to play the games using physical props to maximize comprehension.

The visit took the following structure. After obtaining parental consent and child assent, the child completed a short survey to measure their self-reported social preferences. Specifically, we asked about participants' mental health, dispositional and descriptive envy, feelings of trust and closeness toward various other parties, zero-sum thinking, locus of control, and perceived social norms surrounding sharing resources (earned either by luck or hard work). The survey included some standard modules: CESD-10 module for children to measure self-reported depression (Weissman et al., 1980), World Value Survey's happiness and life satisfaction questions, Children's Hope Scale (Snyder et al., 1997), the Benign and Malicious Envy Scale (BeMaS; Lange and Crusius, 2015), and the Inclusion of Others in the Self scale (Aron et al., 1992). We designed original survey modules to measure descriptive envy, trust, zero-sum thinking, locus of control, and perceived norms surrounding sharing.

[8] We focus on this age range for our study to ensure comprehension of the tasks and survey questions. We found it difficult to administer the survey and tasks to younger children in our piloting. Additionally, this age range allows children to be of a sufficient age when exposed to the treatment. Children under 6 years old at the time of our study would have been younger than 2 years old at the time of the treatment.

The child then completed the experimental tasks. Each child completed three rounds of four different experimental games, and one round of the PGG, all of which are described in detail below. Game order was randomized at the participant level. Before each game, the child received extensive instructions and examples, and completed comprehension questions to ensure understanding. For games played multiple times, the child was asked whether they remembered the rules; if not, they were reminded of the rules prior to continuing.

In the DG, the participant was assigned to be player 1. Player 1 chooses how much of a 10-token endowment to send to player 2. The participant indicated how much they wanted to allocate to the other player by dragging 10 tokens presented on a tablet screen into one of two baskets, representing the participant's and the other player's payout. See Figure D.1 for an example of how the task appeared on the tablet. The outcome of interest is the amount the participant sends to player 2. Versions of this game have previously been used in several studies, including by Habyarimana et al. (2007), Berge et al. (2015), and Hoffman et al. (1996) as well as in studies using children as young as 3 years old (Benenson et al., 2007; Blake and Rand, 2010; Smith et al., 2013; Chernyak et al., 2018). This game is widely considered to be a measure of altruism.

In the UG, player 1 receives an endowment of 10 tokens and proposes a split of that endowment to player 2 (see Figure D.2 for an example). Player 2 can then decide to accept or reject player 1's offer. If player 2 accepts, both players receive the allocation proposed by player 1. If player 2 rejects, both players receive 0 tokens. Versions of this game have been used in several studies in children (e.g., Wittig et al., 2013; Sutter, 2007; Takagishi et al., 2010). Participants in our study played both as player 1 (UG1), for which the outcome of interest is the share of tokens allocated to the other player, and as player 2 (UG2), where the outcome of interest is whether they accept or reject the offer from the other player. The tasks measure attitudes around fairness.

The 3PD is similar to the DG except that, rather than dividing the 10-token endowment between themselves and another player, the participant divides the endowment between two anonymous players (see Figure D.4 for an example) and the participant cannot keep any tokens for themselves. Variations of this game have previously been used in children (e.g., Cappelen et al., 2020). The outcome of interest is the distribution participants make as an indication of egalitarianism, particularly as it is influenced by the other players' characteristics. Given that it measures fairness preferences, rather than pro-sociality or social preferences related to decisions that impact the self, we do not include it in our index of pro-sociality.

The JOD, first introduced by Zizzo and Oswald (2001), allows participants to use their own endowment to reduce the endowment of another player. The participant and their partner both receive an endowment of 10 tokens, and the participant can then decide to reduce their partner's endowment by giving up some of their own endowment. There is an exchange rate of 2:1; for each token the participant spends, their partner loses two tokens (see Figure D.5 for an example). This task measures an individual's willingness to engage in costly destructive behavior, interpreted as a sign of envy. Versions of this game have been previously used in rural African settings to measure inequality aversion and retaliation (Kebede and Zizzo, 2015), though to our knowledge it has never been employed in children.

In the PGG, participants play in groups of four. All four players are given an endowment of 10 tokens, any proportion of which they can "invest" in the public good. The total amount invested in the public good gets doubled, and this doubled amount is then evenly divided between the four players. To maximize personal payoffs, it is in participants' interest to keep their entire endowment for themselves and "free-ride" on the other three players' investment. However, to maximize communal payoffs, all participants should invest their entire endowment. The key measure is how much participants contribute to the public good, that is, whether they act in line with free-riding or communal maximization (see Figure D.6 for how the game appeared to the participants). This game has typically been used in adults, though it has recently been adapted for children (e.g., da Silva et al., 2016; Vogelsang et al., 2014). To capture average prosocial behavior across games, we construct an overall Pro-Sociality index. This index is built by adding the share sent in the Dictator Game, the share offered in the Ultimatum Game, (1 - share destroyed) in the Joy of Destruction Game, and the share allocated to the group in the Public Goods Game, and dividing this sum by 4. Higher values indicate more prosocial behavior across all games.

For each game, participants were randomly paired with either one or several other players, depending on the game. Participants were not informed of their partner's exact identity, nor was a partner ever present. In the first (neutral) round of each game, participants were simply told that their partner was from a village like their own and were informed of their partner's age and gender. Once the participant played all games in the neutral version, they proceeded to the wealth-information rounds. Participants played two additional consecutive rounds of the DG, UG, 3PD, and JOD with wealth information, where they were informed whether the other player was richer or poorer, either relative to the participant's household (DG, UG, JOD) or relative to each other (3PD). We estimate the relative wealth using household assets at the first endline. For all games, participants were told the other players would have the same information about them. In all games and rounds, the identity of the other players was randomized at the individual level. The participants were paired with each gender with equal probability. However, we over-weighted the probability of being paired with someone close in age.

Once the participant had completed all the games, their payouts were calculated. Compensation consisted of a participation reward and task-based payout, which varied according to the choices both participants and their partners made during each game. Tokens were used instead of money, which participants then converted into prizes. The prize options and the number of tokens required to purchase each prize were shown to the children before the session. Prizes were selected based on preferences expressed by participants during piloting and included school-related supplies such as colored crayons, pencils, sharpeners, erasers, notebooks, and complete geometry sets, ensuring that the stakes were salient and valued by participants (see Figure D.7 for pictures).

To assess the persistence of the cash transfers' impact on household consumption, asset ownership, and individual well-being, we surveyed adults in the household. We used the same consumption and assets modules used in Haushofer et al. (2020) for comparison with the previous endline. The individual well-being module was a short survey including the World Value Survey's happiness and life satisfaction questions, the MacArthur Scale of Subjective Social Status, and PHQ-2 (a 2-item measure of depression).

3. Estimating equations

We present our estimating equations for the children's behavioral game data and for the child and adult surveys. For the game behavior analysis, we are interested in how receiving the cash transfer or living near those who received a cash transfer affects game behavior. We also account for the partner's identity (e.g., gender and age) in each game. The estimating equations vary slightly based on whether the game is the neutral round, in which children only had information on the other player's gender and age, or the subsequent rounds where children also received information on the other player's relative wealth.

We first estimate the effects of treatment, pooling all rounds of the games, with the following estimating equation:

$$y_{ijhv} = \alpha_{a(i)} + \alpha_{a(j)} + \alpha_{g(i)} + \alpha_{g(j)} + \beta_1 CashTransfer_{ihv} + \beta_2 Spillover_{ihv} + \mathbf{X_{hv}}\boldsymbol{\beta} + \varepsilon_{ijhv} \quad (1)$$

In Eq. (1), y_{ijhv} is the outcome of interest, where i indexes the child of parent h in village v who is paired with another child j. In addition

to fixed effects for the respondent's age ($\alpha_{a(i)}$) and gender ($\alpha_{g(i)}$), we include fixed effects for the other player's age and gender, denoted by $\alpha_{a(j)}$ and $\alpha_{g(j)}$. $\mathbf{X_{hv}}$ is a vector of household-level stratification variables, including indicators for having M-Pesa access at baseline and being above the median at baseline on the psychological well-being index and the asset index. Note, our specification follows Haushofer et al. (2020) as specified in our pre-analysis plan (Haushofer et al., 2021).[9] We also include an indicator for the neutral version of the games, i.e., whether the game included information about the other player's relative wealth. Standard errors are clustered at the village level, which is the level at which cash treatment was assigned.

Additionally, we ask whether game play differs based on the relative wealth of the other player. We estimate this using the two rounds of game play in which the child receives wealth information as follows:

$$\begin{aligned} y_{ijhv} =& \alpha_{a(i)} + \alpha_{a(j)} + \alpha_{g(i)} + \alpha_{g(j)} + \beta_1 CashTransfer_{ihv} \\ &+ \beta_2 Spillover_{ihv} + \mathbf{X_{hv}}\boldsymbol{\beta} + \beta_3 PoorerPartner_{ijh} \\ &+ \beta_4 PoorerPartner \times CashTransfer_{ijhv} \\ &+ \beta_5 PoorerPartner \times Spillover_{ijhv} + \epsilon_{ijhv} \end{aligned} \tag{2}$$

In Eq. (2), y_{ijhv} is the outcome of interest exclusively for the wealth information games. The variables are defined as in Eq. (1). The variable $PoorerPartner_{ijh}$ is an indicator variable equal to 1 if child j comes from a family poorer than child i's family. The base category here is a richer partner. Variables $PoorerPartner \times CashTransfer_{ijh}$ and $PoorerPartner \times Spillover_{ijh}$ are interaction terms between the relative wealth of the partner and the treatment status.

Our estimating equation for the child and adult survey outcomes is as follows:

$$y_{ihv} = \beta_1 CashTransfer_{ihv} + \beta_2 Spillover_{ihv} + \mathbf{X_{hv}}\boldsymbol{\beta} + \epsilon_{ihv} \tag{3}$$

In Eq. (3), i indexes the child of parent h in village v. For children, $\mathbf{X_{hv}}$ is the same vector of stratification variables as above. For adults, we include the gender of the respondent. For children, we include fixed effects for the child's age and gender. Standard errors are clustered at the village level.

To address concerns about multiple hypothesis testing for the survey data, we construct an array of indices on psychological well-being, envy, trust, closeness, zero-sum thinking, locus of control, and perceived norms surrounding sharing money earned through luck or hard work, which serve as dependent variables. For the adult survey, the consumption, assets, and psychological well-being dependent variables are built following Haushofer et al. (2020), for direct comparison. Details on the survey and variable construction can be found in Appendix C.1.

4. Results

Table A.3 presents summary statistics for our sample of children that participated in all versions of the games. There are no differences between treatment, control, and spillover groups in terms of demographics: gender, age, years of education, an indicator for their education being on track with age, and household size. We may have expected that treatment affected schooling outcomes, but we find no evidence that treatment children have more years of education or are more likely to be on track for their age. We also present measures of game comprehension: number of incorrect test questions before the games, comprehension score given by enumerators (on a 1–5 scale), and minutes spent completing the games. We do not find differences across treatment arms.

[9] The only difference is we do not include the Intimate Partner Violence Index at baseline as a stratification variable control, because it is missing for 27% of our sample and is not relevant for our outcomes of interest.

5. Medium-run impact on economic and psycho-social outcomes

5.1. Effects on economic outcomes 3 years after the intervention

We first examine whether the results documented at the household level from the first endline (i.e., one year after the intervention) persist three years after the intervention. These results are presented in Table 1. The first 3 columns present the results from the first endline in 2018. Columns (4)–(6) present the results from 2021.

The results from 2021 show no significant differences between the treatment or the spillover conditions compared to the control regarding consumption or assets. However, the second endline also shows higher standard deviations, which could contribute to the loss of statistical significance. We investigate this pattern and find that the increase in dispersion is largely proportional to the increase in means: the coefficient of variation is stable or decreasing for 10 of 14 well-defined outcomes, suggesting that the higher variances reflect general economic growth rather than a structural change in the distribution. We formally test whether treatment effects differ across endlines by interacting treatment indicators with a wave indicator in a pooled panel regression restricted to the matched sample; we cannot reject equality of impacts for 13 of 15 cash outcomes and all 15 spillover outcomes (Appendix Table B.1). Thus, while our point estimates at the second endline are not statistically significant, we cannot rule out that the true effects are similar in magnitude to those at the first endline but are estimated with less precision.

As a result, our study of effects on social behavior in children should be interpreted as asking whether cash transfers can have persistent social impacts in a context where the economic effects have not clearly persisted — though we note the distinction between a precisely estimated null and an imprecisely estimated effect that may or may not have faded.

5.2. Effects on psycho-social well-being

The effects of the treatment on participants' psycho-social well-being are summarized in Fig. 2, and its corresponding Table B.2. The itemized results for children and adults are presented in Figures B.1 and B.2, respectively. Individuals in the spillover condition reported lower psychological well-being, compared to both the control and treatment conditions.

Among children (Panel A), spillover participants reported worse mental health compared to both treatment and control participants. This is driven by life satisfaction, where spillover children reported scores lower than both treatment and control participants. Additionally, treatment children reported higher malicious envy than both spillover and control children. For the other psycho-social outcomes (indices of benign or descriptive envy, trust, social closeness, zero-sum thinking, or locus of control) there are no significant differences across conditions.

Among adults (Panel B), spillover participants reported higher depression and lower life satisfaction compared to control participants. Meanwhile, treatment participants reported greater happiness than control participants (though happiness did not significantly differ between treatment and spillover participants). Finally, despite not seeing any sustained differences in assets or consumption, treatment adults relative to spillover adults did report higher subjective social status (as measured by higher scores on the MacArthur Ladder).

To understand the relationship between parents' and children's psycho-social well-being, we first examined a simple correlation between the children's mental health index and their parent's psychological well-being index for those in the control group. The correlation coefficient is 0.11 and the associated t-statistic is 4.75, suggesting that parent and child mental health are correlated (See Figure B.3). We further analyze the heterogeneity in children's scores as a function of their parents' Psychological Well-Being Index (PWBI), specifically whether the PWBI was above or below median (See Table B.3). Children with

Table 1
Persistent effects of cash transfers on consumption and assets.

	First endline 2018/2019			Second endline 2021			
	Pure control mean (1)	Treatment (2)	Spillover (3)	Pure control mean (4)	Treatment (5)	Spillover (6)	N (7)
Consumption in USD, PPP							
Monthly per-capita non-durable consumption	44.279	9.177***	4.460*	54.505	3.533	2.430	1657
	(31.128)	(2.673)	(2.659)	(44.381)	(3.834)	(3.284)	
Expenditure on food	30.277	5.061**	2.558	33.374	2.887	2.941	1657
	(22.442)	(1.960)	(2.023)	(27.662)	(2.606)	(2.212)	
Expenditure on temptation goods	0.089	0.059	0.039	0.577	0.088	0.090	1657
	(0.465)	(0.048)	(0.033)	(2.325)	(0.177)	(0.152)	
Expenditure on personal and household items	3.876	0.665*	0.141	7.782	0.705	0.234	1657
	(4.438)	(0.372)	(0.307)	(8.776)	(0.738)	(0.620)	
Expenditure on housing repair or improvement	0.885	1.537***	0.208	2.801	0.304	−0.258	1655
	(4.034)	(0.473)	(0.264)	(9.008)	(0.613)	(0.467)	
Expenditure on education	5.878	0.670	0.975**	5.175	−0.454	0.093	1655
	(6.720)	(0.507)	(0.428)	(7.302)	(0.440)	(0.422)	
Medical expenditure	1.604	0.114	0.312	1.620	0.260	−0.029	1652
	(3.714)	(0.239)	(0.289)	(4.076)	(0.276)	(0.242)	
Social expenditure	0.821	0.168	0.079	1.584	0.187	0.153	1657
	(1.382)	(0.106)	(0.104)	(2.866)	(0.212)	(0.204)	
Assets in USD, PPP							
Total value of assets household owns	597.326	200.062***	29.561	882.691	120.142	156.589*	1657
	(672.919)	(51.838)	(42.283)	(1101.462)	(82.290)	(94.153)	
Value of productive assets	40.753	9.403**	1.526	68.214	−1.697	14.665	1657
	(53.554)	(4.604)	(3.241)	(181.117)	(13.408)	(17.863)	
Value of vehicles	145.924	84.961***	3.575	250.752	51.995	−1.095	1657
	(389.662)	(30.106)	(22.765)	(559.816)	(39.110)	(30.985)	
Value of furniture	339.558	77.570***	22.452	411.765	49.233	54.332*	1657
	(265.375)	(26.699)	(23.239)	(313.242)	(31.701)	(30.623)	
Value of durables	147.147	36.492***	1.901	210.172	20.363	4.971	1657
	(170.714)	(11.296)	(10.123)	(224.860)	(17.371)	(15.452)	
Net value of financial assets	−76.056	−8.364	0.108	−114.245	19.520	36.845	1655
	(246.419)	(19.572)	(15.303)	(461.514)	(30.315)	(25.474)	
Value of land	18 620.896	−1577.538	−2933.065	23 708.940	−1272.021	−2144.260	1550
	(33 573.411)	(2117.901)	(2364.379)	(37 239.212)	(2776.041)	(2414.703)	

Notes: The table presents OLS estimates of treatment effects. The outcome variables, measured in 2018/2019 (1 year after the intervention) and in 2021 (3 years after the intervention) are listed on the left. We restrict the sample of the first endline to those households surveyed in the second endline. The first and fourth columns shows the control group mean and standard deviation (in parentheses) of the outcome variable. Standard errors, clustered at the village level, are shown in parentheses for the Treatment and Spillover columns.

* $p < 0.1$.

** $p < 0.05$.

*** $p < 0.01$.

parents whose PWBI was below the median also have significantly lower mental health, driven by lower levels of happiness and life satisfaction. Additionally, children with parents who have low PWBI report a lower closeness index (i.e., feeling less close to their family, friends, and others in their village) and a higher descriptive envy index (i.e., observing more envy among others they know). The effect of parents' PWBI on children's psycho-social outcomes persists regardless of treatment condition. Given that adults in the spillover condition report lower psychological well-being, the direct effect of parents' mental health on that of their children likely explains spillover children's lower reported mental health.

In summary, we find little evidence of persistent direct treatment effects on psycho-social outcomes. Again, therefore, our results should be interpreted as addressing the question whether cash transfers can have persistent effects on social behavior that outlast their effects on psycho-social outcomes.

5.3. Behavioral game results

We present means of game behavior for each game across treatment groups, gender, age groups, and family composition in Fig. 3 (see Table A.4 for a summary statistics table). In Figure A.1 we present histograms of the distribution of our main game outcome variables.

Fig. 4 presents the results of our analysis for eight outcomes across our five behavioral games, and the overall Pro-Sociality index. Panel A shows the main effects estimated following Eq. (1). There are no significant effects of the cash transfer on children's behaviors in the games for children in treated or spillover households.

Panel B of Fig. 4 presents the results for the games in which participants received information on the relative wealth of their partners, following Eq. (2). It plots the coefficient on treatment or spillover interacted with being paired with a poorer partner. There is little evidence of differential behavior by treatment or spillover status when paired with a poorer partner. The only significant interaction that emerged was for the JOD game: Children from spillover households destroyed a larger share of their partners' endowment when the partner was poorer. Appendix Table A.5 is the corresponding table for this figure. Appendix Table A.6 presents the results when we restrict our sample to games played in the neutral version only. Appendix Table A.7 presents the results when we restrict our sample to the two rounds with wealth information on the other player and include respondent fixed effects. In both cases, the results are unchanged.

As an aside, we note that there is little evidence of differential behavior when playing with a poorer versus a richer partner independently of treatment: Panel B of Table A.5 shows coefficients on "Partner Poorer", which can be interpreted as differential behavior in the control group towards poorer partners. The coefficients are small and mostly not statistically significant. An exception is that control group participants are 1.8 percentage points less likely to accept offers of poorer compared to richer partners in the UG, significant at the 10% level. Only when participants act as third-party allocators in the 3PD, do they exhibit some redistributive preferences: The average share allocated to the poorer player is 0.6 (with 0.4 to the richer player).

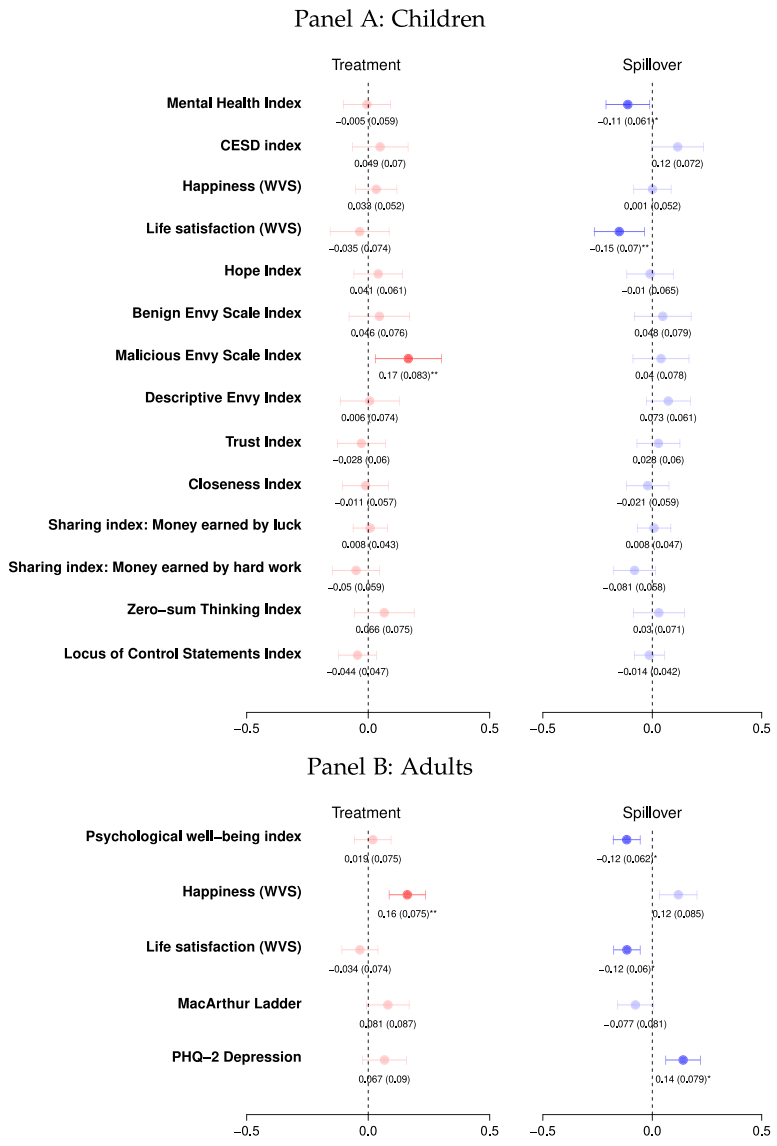

Fig. 2. Effects of cash transfers on psycho-social well-being.
Notes: Estimated treatment coefficients for mental health survey outcomes, 3 years after the original intervention. Each row represents a different outcome variable, including indices and their components. Each row displays our main two treatment effects: cash treatment (left), and spillover (right). 90% confidence intervals are shown around each point estimate. Estimates which are significantly different from zero at the 10 percent level are shown in darker shades.

To help interpret these null results and to alleviate concerns about statistical power, we use the standard errors obtained in each estimation to calculate the minimum effect sizes we would be able to detect with 80% probability. We illustrate these results in Fig. 5. Depending on the outcome, we can reject changes in game behavior of sizes ranging from 2 to 10 pp, which corresponds to differences of around 1/5 to 1 out of the 10 tokens used in each game. Specifically, in the DG and PGG, we can reject effect sizes larger than 3 pp. In the UG, we can reject changes greater than 2 pp in the share of tokens offered, and greater than 3 pp in the share of tokens accepted. For the JOD, we can reject a 10 pp change in the "Destroyed Any" indicator, and a shift of over 7 pp in the share of tokens destroyed. In the 3 PD, we can reject changes of at least 4 pp. Thus, our analysis suggests we have power to reject fairly small effect sizes.

Our results contrast with the existing literature that suggests children from low-SES backgrounds are less altruistic and more spiteful (Sutter et al., 2019; Bauer et al., 2014; Benenson et al., 2007; Chernyak et al., 2018). When wealth is exogenously varied, it does not have a sizable impact on social preferences. We are able to reject effect sizes smaller than those found in Bauer et al. (2014) and Chernyak et al. (2018) (16 pp and 46 pp respectively).[10] One caveat is that, while our design exogenously varies household wealth, the experimental wealth gap has largely dissipated by the time we measure children's social preferences. Our results therefore speak to the persistence of wealth effects on preferences, and may not directly contradict cross-sectional findings that reflect ongoing differences in SES.

A related concern is whether the games reliably capture the prosocial constructs they are designed to measure. To assess this, we follow the psychometric framework of Laajaj and Macours (2021). The four sharing games (DG, UG1, JOD share not destroyed, PGG) show reasonable internal consistency (Cronbach's $\alpha = 0.594$) and pairwise correlations ranging from 0.39 to 0.64 (Appendix Table A.8)). The

[10] Sutter et al. (2019) is a review and Benenson et al. (2007) does not report coefficients.

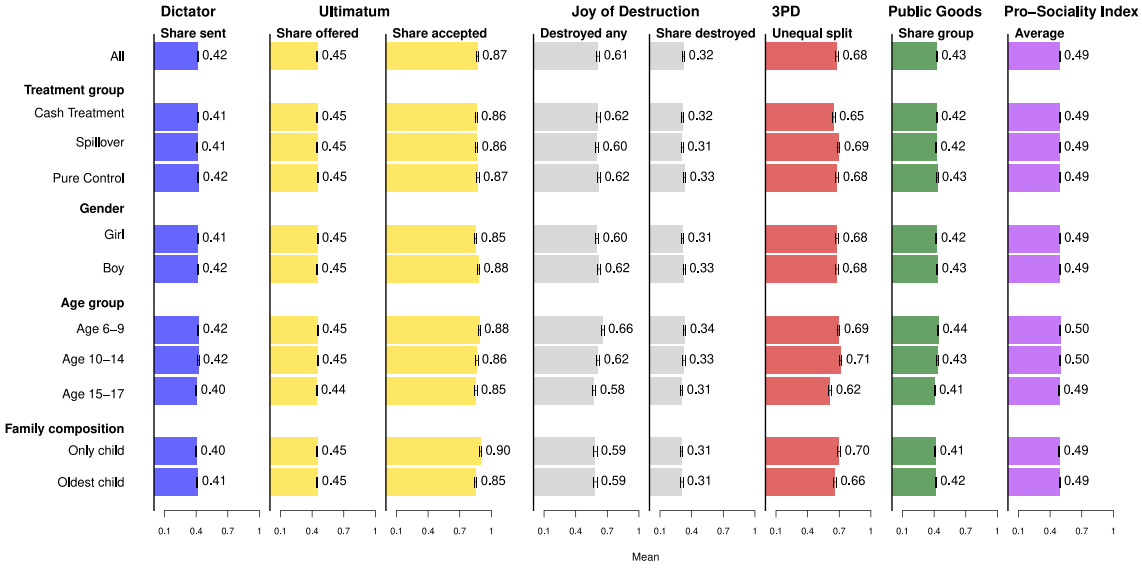

Fig. 3. Game behavior summary statistics.
Notes: Fig. 3 shows means for the game behavior outcome variables for the full sample of children participants and sub-groups of it; by treatment group, gender, age group, and family composition type. The brackets represent 95% confidence intervals, calculated as the mean ±1.96 standard errors, where the standard error is derived from the sample standard deviation divided by the square root of the sample size ($n = 4022$). Means are annotated to the right of each bracket.

composite prosociality index has an intra-class correlation of 0.555 across the three plays per child, indicating that most outcome variance reflects stable between-child differences rather than play-specific noise. We further assess convergent and discriminant validity by correlating each game outcome with 14 self-reported indices from the child survey; correlations are small in magnitude — as is typical for comparisons between incentivized behavior and self-report — but mostly align with theoretically expected directions, while near-zero correlations with Raven's matrices and CESD depression confirm that the games are not simply proxying for cognitive ability or mood (Appendix Table A.9).

5.4. Heterogeneous effects

We find no consistent evidence of treatment-effect heterogeneity by respondent characteristics (age, gender), household baseline wealth and household medium-run treatment effects on assets and consumption, village-level socioeconomic conditions, or partner attributes in economic games. For example, if we construct an indicator for medium-run economic benefits from the cash transfer, we do not find any evidence that children from those households that did sustain economic benefits – based on assets and consumption indexes – behave differently. However, some patterns emerge: Spillover children from wealthier households tended to share more in certain games but were also more likely to destroy in the JOD. Children in high-inequality villages gave more and were more likely to reject offers in the UG. Detailed results are provided in Appendix Section A.1 for interested readers.

5.5. Robustness

We undertake a number of sensitivity checks to test the robustness of our findings as specified in our pre-analysis plan (Haushofer et al., 2021). In Figure A.4 we present sensitivity of our results on game behavior to adding different sets of fixed effects. Specifically, we control for day-of-week fixed effects, whether they play with the poorer partner first in the wealth information games, and enumerator fixed effects.

While the first two fixed effects do not substantially change our results, the results are somewhat sensitive to the inclusion of enumerator fixed effects. Once we add enumerator fixed effects, we find that children in spillover households share on average 0.14 tokens less (out of 10) in the DG and 0.12 tokens less in the UG. Even if we cannot reject that living near families that received UCTs negatively impacted the prosocial preferences of children in spillover households, these results are small in magnitude, representing a change of less than 3% relative to the control mean.

We also calculate p-values using randomization inference following Young (2018). The randomization inference p-values are reported in brackets in Table A.5 and yield virtually the same results.

Finally, we check for and omit influential observations using DFBETAS (Belsley et al., 1980) in Tables A.10 and B.4 for children's game behavior and psycho-social well-being, respectively. Results remain largely similar, except for a single result in the DG, where children in the treatment group appear to be more prosocial. However, this result is small in magnitude, less than 3 pp, which is the minimum effect size we can reject.

6. Conclusion

This paper examines how cash transfers affect the prosocial behavior of children in both treated and spillover households, using surveys and behavioral games with over 4000 children in Kenya. While most evaluations of cash transfers tend to focus on the economic and psychological benefits, we provide causal evidence on the effects of cash transfers on the prosocial preferences of children. We find little evidence for positive effects on the prosocial behavior of children in treatment households. We also do not find robust negative effects on the prosocial behavior for spillover children. We find some evidence of worse mental health for spillover adults and children three years after the intervention. A possible reason for these null results is that the cash transfers had no persistent economic or psycho-social impacts; future work might elucidate whether temporary changes in wealth affect social behavior while the economic changes are ongoing. Our results provide an important upper bound on the persistence of any such effects.

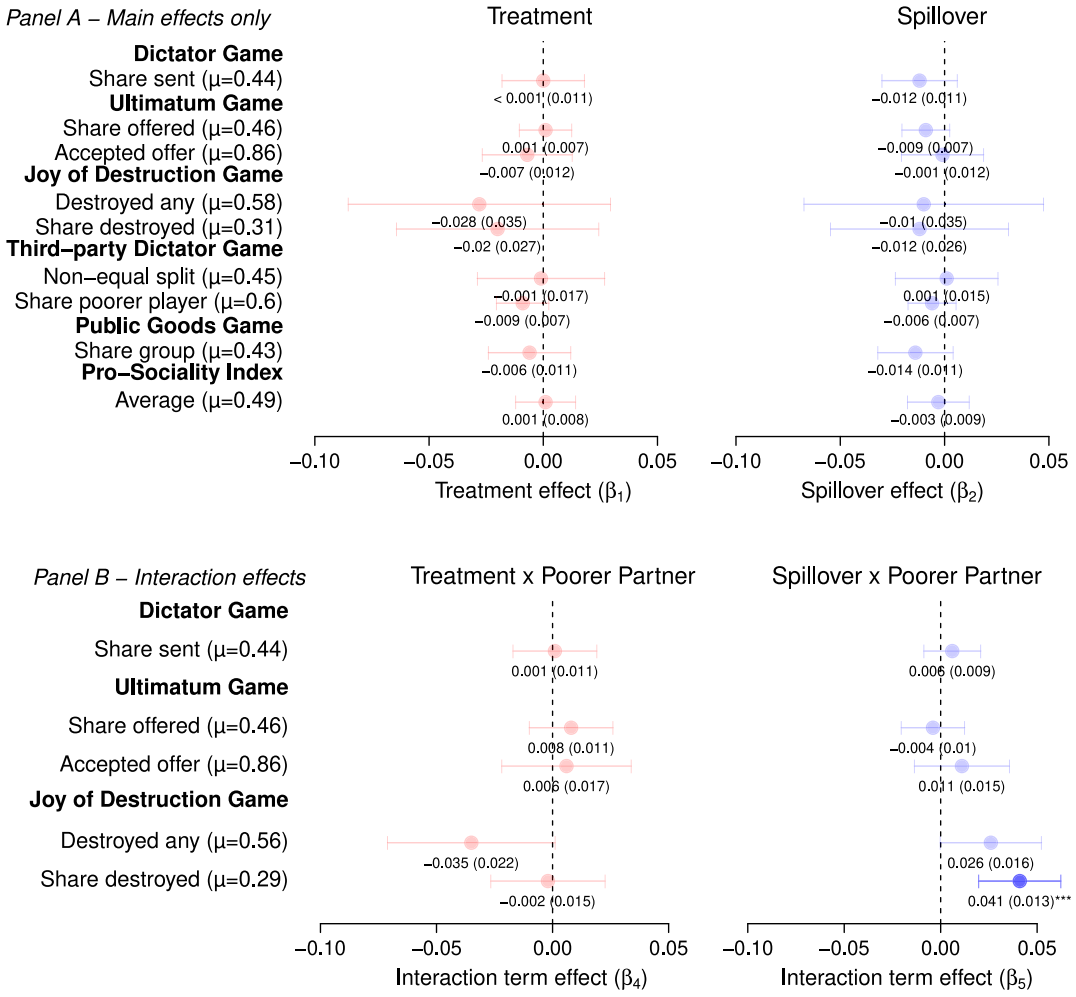

Fig. 4. Effect of cash transfers on game behavior.
Notes: The figure presents estimated treatment coefficients for games behavior outcomes, 3 years after the original intervention. Each row represents a different outcome variable. In Panel A, each row displays our two main treatment effects, estimated by Eq. (1): cash treatment (left), and spillover (right). In Panel B, each row presents the interaction terms of cash treatment and spillover with the relative wealth of partner indicator. 90% confidence intervals are shown around each point estimate. Estimates which are significantly different from zero at the 10 percent level are shown in darker shades. Note that for the JOD outcomes, lower values of destruction correspond to higher prosocial behavior.

CRediT authorship contribution statement

Johannes Haushofer: Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Magdalena Larreboure:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Sara Lowes:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Leon Mait:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Acknowledgments

We thank the participants for their time and engagement. We are grateful to the Busara Center for Behavioral Economics in Nairobi—especially Isaac Obara, Esther Owelle, the field team led by Joshua Omare and Denis Mutai, and Brendah Khalifwa for programming the games. We also thank Cuong Pham Vu for excellent research assistance and Andre Gray, Paul Niehaus, and Tom Vogl for their feedback. The experiment was pre-registered (AEARCTR-0007738; Haushofer et al., 2021) and approved by Princeton University's IRB (IRB #0000007875).

Funding

Funding was provided by the National Science Foundation (NSF) and the Weiss Fund.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jdeveco.2026.103945.

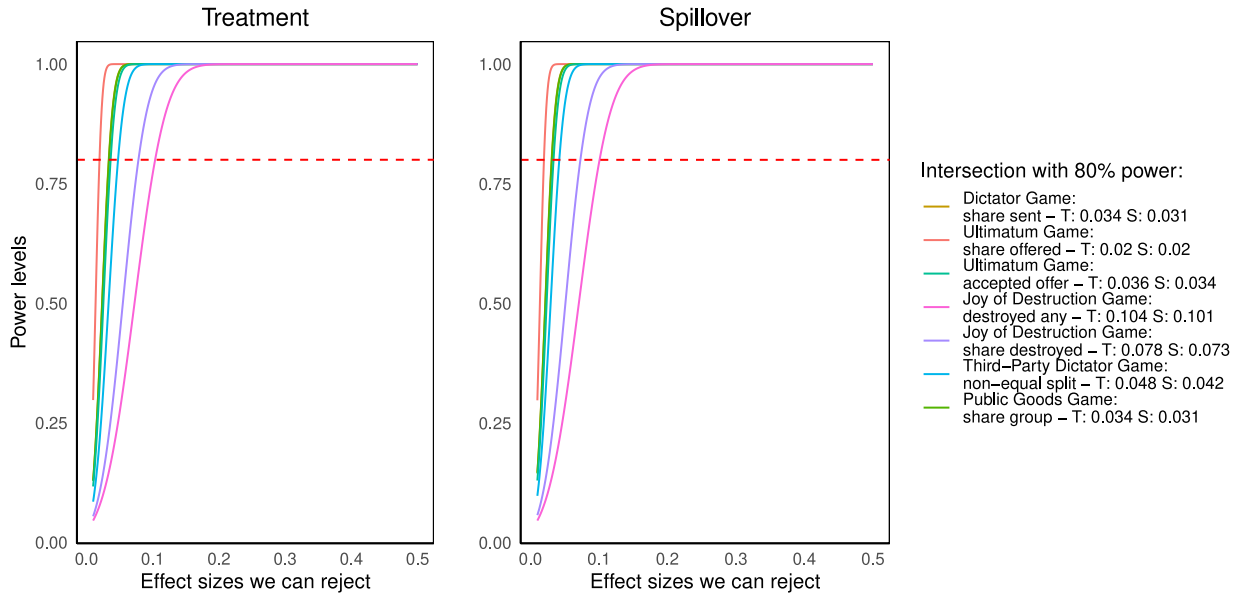

Fig. 5. Power analysis for game behavior outcomes.
Notes: Fig. 5 shows the treatment and spillover effect sizes that we are able to reject across different levels of power, using the standard errors obtained for each outcome in our estimation of Eq. (1). The red horizontal striped lines show the 80% power level. The effect size value that intersects with this line are shown in the label for each game outcome, where T stands for treatment and S for spillover.

Data availability

Data and code for replication are available for download, doi: 10.17632/xt9g8tmsvg.1.

References

Almås, Ingvild, Cappelen, Alexander W., Salvanes, Kjell G., Sørensen, Erik Ø., Tungodden, Bertil, 2017. Fairness and family background. Politics, Philos. Econ. 16 (2), 117–131.

Almås, Ingvild, Cappelen, Alexander W., Sørensen, Erik Ø., Tungodden, Bertil, 2010. Fairness and the development of inequality acceptance. Science 328 (5982), 1176–1178.

Almond, Douglas, Currie, Janet, 2011. Human capital development before age five. Handb. Labor Econ. 4B, 1315–1486.

Andersen, Asbjørn G., Franklin, Simon, Kotsadam, Andreas, Somville, Vincent, Villanger, Espen, Getahun, Tigabu, 2020. Does Wealth Reduce Support for Redistribution? Evidence from an Ethiopian Housing Lottery. Discussion Paper Series in Economics 18/2020, Norwegian School of Economics, Department of Economics.

Andreoni, James, Nikiforakis, Nikos, Stoop, Jan, 2017. Are the rich more selfish than the poor, or do they just have more money? A natural field experiment. w23229.

Arnold, Catherine, Greenslade, Matthew, Conway, Tim, 2011. DFID Cash Transfers Literature Review. Technical Report, UKAID Department for International Development.

Aron, Arthur, Aron, Elaine N., Smollan, Danny, 1992. Inclusion of other in the self scale and the structure of interpersonal closeness. J. Pers. Soc. Psychol. 63 (4), 596.

Attanasio, Orazio P., Fitzsimons, Emla, Gomez, Ana, 2010. Child education and work choices in the presence of a conditional cash transfer programme in rural Colombia. Econ. Dev. Cult. Chang. 58 (2), 181–210.

Baird, Sarah, De Hoop, Jacobus, Özler, Berk, 2013. Income shocks and adolescent mental health. J. Hum. Resour. 48 (2), 370–403.

Baird, Sarah, McIntosh, Craig, Özler, Berk, 2019. When the money runs out: Do cash transfers have sustained effects on human capital accumulation? J. Dev. Econ. 140.

Bauer, Michal, Chytilová, Julie, Pertold-Gebicka, Barbara, 2014. Parental background and other-regarding preferences in children. Exp. Econ. 17 (1), 24–46.

Belsley, David, Kuh, Edwin, Welsch, Roy, 1980. Detecting influential observations and outliers. In: Regression Diagnostics. John Wiley & Sons, Ltd, pp. 6–84.

Ben-Ner, Avner, List, John A., Putterman, Louis, Samek, Anya, 2017. Learned generosity? An artefactual field experiment with parents and their children. J. Econ. Behav. Organ. 143, 28–44.

Benenson, Joyce F., Pascoe, Joanna, Radmore, Nicola, 2007. Children's altruistic behavior in the dictator game. Evol. Hum. Behav. 28 (3), 168–175.

Berge, Lars Ivar Oppedal, Bjorvatn, Kjetil, Galle, Simon, Miguel, Edward, Posner, Daniel N., Tungodden, Bertil, Zhang, Kelly, 2015. How Strong are Ethnic Preferences? Technical Report, National Bureau of Economic Research.

Blake, Peter R., Rand, David G., 2010. Currency value moderates equity preference among young children. Evol. Hum. Behav. 31 (3), 210–218.

Blattman, Christopher, Fiala, Nathan, Martinez, Sebastian, 2020. The long-term impacts of grants on poverty: Nine-year evidence from Uganda's youth opportunities program. Am. Econ. Rev.: Insights 2 (3), 287–304.

Cappelen, Alexander, List, John, Samek, Anya, Tungodden, Bertil, 2020. The effect of early-childhood education on social preferences. J. Political Econ. 128 (7), 2739–2758.

Cerkez, Nicolas, Khan, Adnan Q., Rasul, Imran, Shoaib, Anam, 2023. Big push pro-poor policies and economic preferences: Evidence from a partial population experiment.

Chen, Yongxiang, Zhu, Liqi, Chen, Zhe, 2013. Family income affects children's altruistic behavior in the dictator game. PLoS One 8 (11), e80419.

Chernyak, Nadia, Harvey, Teresa, Tarullo, Amanda R., Rockers, Peter C., Blake, Peter R., 2018. Varieties of young children's prosocial behavior in Zambia: The role of cognitive ability, wealth, and inequality beliefs. Front. Psychol. 9, 2209.

Cobo-Reyes, Ramon, Dominguez, Jose J., García-Quero, Fernando, Grosskopf, Brit, Lacomba, Juan A., Lagos, Francisco, Liu, Tracy Xiao, Pearce, Graeme, 2020. The development of social preferences. J. Econ. Behav. Organ. 179, 653–666.

Côté, Stéphane, Stellar, Jennifer E., Willer, Robb, Forbes, Rachel C., Martin, Sean R., Bianchi, Emily C., 2021. The psychology of entrenched privilege: High socioeconomic status individuals from affluent backgrounds are uniquely high in entitlement. Pers. Soc. Psychol. Bull. 47 (1), 70–88.

Crosta, Tommaso, Karlan, Dean, Ong, Finley, Rüschenpöhler, Julius, Udry, Christopher R., 2024. Unconditional cash transfers: A Bayesian meta-analysis of randomized evaluations in low and middle income countries.

da Silva, Phiética Raíssa Rodrigues, Boccardi, Natalia Andrea Cracciun, Dutra, Natalia Bezerra, Hattori, Wallisen Tadashi, Yamamoto, Maria Emília, Alencar, Anuska Irene, 2016. Stickers versus wafers: The value of resource in a public goods game with children. Estud. Psicol. (Natal) 21 (2), 117–124.

Edmonds, Eric V., 2006. Child labor and schooling responses to anticipated income in South Africa. J. Dev. Econ. 81 (2), 386–414.

Egger, Dennis, Haushofer, Johannes, Miguel, Edward, Niehaus, Paul, Walker, Michael W., 2022. General equilibrium effects of cash transfers: experimental evidence from Kenya. Econometrica.

Evans, Gary W., Kim, Pilyoung, 2013. Childhood poverty, chronic stress, self-regulation, and coping. Child Dev. Perspect. 7 (1), 43–48.

Falk, Armin, Becker, Anke, Dohmen, Thomas, Enke, Benjamin, Huffman, David, Sunde, Uwe, 2018. Global evidence on economic preferences. Q. J. Econ. 133 (4), 1645–1692.

Fehr, Ernst, Bernhard, Helen, Rockenbach, Bettina, 2008. Egalitarianism in young children. Nature 454 (7208), 1079–1083.

Fehr, Ernst, Epper, Thomas, Senn, Julien, et al., 2022. Other-Regarding Preferences and Redistributive Politics. Technical Report.

Filmer, Deon, Friedman, Jed, Kandpal, Eeshani, Onishi, Junko, 2021. Cash transfers, food prices, and nutrition impacts on ineligible children. Rev. Econ. Stat. 1–45.
Giuliano, Paola, Splilimbergo, Antonio, 2024. Aggregate shocks and the formation of preferences and beliefs.
Habyarimana, James, Humphreys, Macartan, Posner, Daniel N., Weinstein, Jeremy M., 2007. Why does ethnic diversity undermine public goods provision? Am. Political Sci. Rev. 709–725.
Harbaugh, William, Krause, Kate, 2000. Children's altruism in public good and dictator experiments. Econ. Inq.
Haushofer, Johannes, Lowes, Sara, Mait, Leon, 2021. The effects of cash transfers on social preferences. AEA RCT Regist. (AEARCTR-0007738).
Haushofer, Johannes, Mudida, Robert, Shapiro, Jeremy P., 2020. The comparative impact of cash transfers and a psychotherapy program on psychological and economic well-being.
Haushofer, Johannes, Shapiro, Jeremy, 2016. The short-term impact of unconditional cash transfers to the poor: experimental evidence from Kenya. Q. J. Econ. 131 (4), 1973–2042.
Haushofer, Johannes, Shapiro, Jeremy, 2018. The long-term impact of unconditional cash transfers to the poor: Experimental evidence from Kenya.
Heckman, James J., 2006. Skill formation and the economics of investing in disadvantaged children. Science 312 (5782), 1900–1902.
Henrich, Joseph, Heine, Steven J., Norenzayan, Ara, 2010. The weirdest people in the world? Behav. Brain Sci. 33 (2–3), 61–83.
Hoffman, Elizabeth, McCabe, Kevin, Smith, Vernon L., 1996. Social distance and other-regarding behavior in dictator games. Am. Econ. Rev. 86 (3), 653–660.
Huppert, Elizabeth, Cowell, Jason M., Cheng, Yawei, Contreras-Ibáñez, Carlos, Gomez-Sicard, Natalia, Gonzalez-Gadea, Maria Luz, Huepe, David, Ibanez, Agustin, Lee, Kang, Mahasneh, Randa, et al., 2019. The development of children's preferences for equality and equity across 13 individualistic and collectivist cultures. Dev. Sci. 22 (2), e12729.
Kebede, Bereket, Zizzo, Daniel John, 2015. Keep up with the winners: Experimental evidence on risk taking, asset integration, and peer effects. Eur. Econ. Rev. 79, 59–79.
Korndörfer, Martin, Egloff, Boris, Schmukle, Stefan C., 2015. A large scale test of the effect of social class on prosocial behavior. PLoS One 10 (7), e0133193.
Kosse, Fabian, Deckers, Thomas, Pinger, Pia, Schildberg-Hörisch, Hannah, Falk, Armin, 2020. The formation of prosociality: causal evidence on the role of social environment. J. Political Econ. 128 (2), 434–467.
Laajaj, Rachid, Macours, Karen, 2021. Measuring skills in developing countries. J. Hum. Resour. 56 (4), 1254–1295.
Lange, Jens, Crusius, Jan, 2015. Dispositional envy revisited: Unraveling the motivational dynamics of benign and malicious envy. Pers. Soc. Psychol. Bull. 41 (2), 284–294.
List, John A., Petrie, Ragan, Samek, Anya, 2023. How experiments with children inform economics. J. Econ. Lit. 61 (2), 504–564.
Mani, Anandi, Mullainathan, Sendhil, Shafir, Eldar, Zhao, Jiaying, 2013. Poverty impedes cognitive function. Science 341 (6149), 976–980.
McGuire, Joel, Kaiser, Caspar, Bach-Mortensen, Anders M., 2022. A systematic review and meta-analysis of the impact of cash transfers on subjective well-being and mental health in low- and middle-income countries. Nat. Hum. Behav. 6 (3), 359–370.
Miller, Jonas G., Kahle, Sarah, Hastings, Paul D., 2015. Roots and benefits of costly giving: Young children's altruism is related to having less family wealth and more autonomic flexibility. Psychol. Sci. 26 (7), 1038.
Piff, Paul K., Kraus, Michael W., Côté, Stéphane, Cheng, Bonnie Hayden, Keltner, Dacher, 2010. Having less, giving more: the influence of social class on prosocial behavior. J. Pers. Soc. Psychol. 99 (5), 771.
Piff, Paul K., Robinson, Angela R., 2017. Social class and prosocial behavior: Current evidence, caveats, and questions. Curr. Opin. Psychol. 18, 6–10.
Rao, Gautam, 2019. Familiarity does not breed contempt: Generosity, discrimination, and diversity in Delhi schools. Am. Econ. Rev. 109 (3), 774–809.
Smith, Craig E., Blake, Peter R., Harris, Paul L., 2013. I should but I won't: Why young children endorse norms of fair sharing but do not follow them. PLoS One 8 (3), e59510.
Snyder, C. Richard, Hoza, Betsy, Pelham, William E., Rapoff, Michael, Ware, Leanne, Danovsky, Michael, Highberger, Lori, Ribinstein, Howard, Stahl, Kandy J., 1997. The development and validation of the children's hope scale. J. Pediatr. Psychol. 22 (3), 399–421.
Sutter, Matthias, 2007. Outcomes versus intentions: On the nature of fair behavior and its development with age. J. Econ. Psychol. 28 (1), 69–78.
Sutter, Matthias, Zoller, Claudia, Glätzle-Rützler, Daniela, 2019. Economic behavior of children and adolescents–A first survey of experimental economics results. Eur. Econ. Rev. 111, 98–121.
Takagishi, Haruto, Kameshima, Shinya, Schug, Joanna, Koizumi, Michiko, Yamagishi, Toshio, 2010. Theory of mind enhances preference for fairness. J. Exp. Child Psychol. 105 (1–2), 130–137.
Vogelsang, Martina, Jensen, Keith, Kirschner, Sebastian, Tennie, Claudio, Tomasello, Michael, 2014. Preschoolers are sensitive to free riding in a public goods game. Front. Psychol. 5, 729.
Weissman, Myrna M., Orvaschel, H., Padian, Nancy, 1980. Children's symptom and social functioning: Self-report scales. J. Nerv. Ment. Disord. 168 (12), 736–740.
Wittig, Martina, Jensen, Keith, Tomasello, Michael, 2013. Five-year-olds understand fair as equal in a mini-ultimatum game. J. Exp. Child Psychol. 116 (2), 324–337.
Young, Alwyn, 2018. Channeling Fisher: Randomization tests and the statistical insignificance of seemingly significant experimental results*. Q. J. Econ. 134 (2), 557–598.
Zizzo, Daniel John, Oswald, Andrew J., 2001. Are people willing to pay to reduce others' incomes? Ann. d'Econ. Stat. 39–65.