Parental Resource Allocation: The Policy Elasticity of Child-Related Spending

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Abstract

We study how mothers and fathers allocate resources to young children in a low-income setting, and how these allocations respond to interventions that aim to improve early childhood development. We embed a lab-in-the-field allocation experiment with 1,330 parents of children under age two into the Kizazi Kijacho (KK) randomized controlled trial in Tanzania, which cross-randomizes a parenting program and gender-targeted unconditional cash transfers (UCTs). In the experiment, parents divide an endowment between themselves, their spouse, and their child, and then across spending categories. A random half complete a hypothetical and the other half an incentivized version with purchases from a pop-up shop. We show that the hypothetical and incentivized versions yield statistically similar allocations, validating the hypothetical task. In the control group, mothers allocate about 3 percentage points (10 percent) more of the endowment to the child than fathers, a difference entirely driven by children's clothing while we find no differences for food, shoes, and toys. KK program arms leave mothers' allocations unchanged but increase paternal allocations to the child when households receive UCTs, especially when fathers are recipients or when UCTs are combined with parenting. These shifts close the mother-father gap in child-related spending at status quo. Follow-up evidence indicates no undoing of experimental allocations and no crowding out of later child purchases. Our findings reconcile the rationale for targeting transfers to women and posit that, for policy design, we should not only investigate who is the more child oriented at status quo, but also who is most responsive to policy.

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1 Introduction

A key question in poverty and social protection policy is how to increase *parental investments* in children, including time, engagement, and child-directed spending. Many programs, including government child benefits and conditional or unconditional cash transfers, pursue this objective by transferring resources to mothers (Fiszbein and Schady, 2009; Duflo, 2012; Crosta et al., 2024). Targeting women is also motivated by gender equity goals. A recent systematic review finds that social safety nets can improve women's economic achievements and some dimensions of agency, with substantial heterogeneity by context and design (Peterman et al., 2025). Evidence on whether cash transfer programs increase child-directed spending, and through which parent, is mixed. Estimated effects vary by spending category and depend on how preferences are elicited (Bastagli et al., 2016; Almås et al., 2018; Baird et al., 2014; Molina Millan et al., 2016; Field and Maffioli, 2025; Weaver et al., 2025; Crosta et al., 2024).

The common rationale for targeting cash transfers to mothers is that doing so will increase child-directed spending. For this to hold, two conditions must be satisfied. First, mothers must, on average, place greater weight than fathers on child-specific purchases. Second, mothers must have sufficient agency and control over resources to act on those preferences. Our study directly addresses the first condition and speaks to the policy margin relevant for the second. We do not measure empowerment per se. Instead, we observe each parent's allocation in a standardized task and interpret the chosen budget shares as a reduced-form outcome that reflects preferences, salience, and perceived decision rights.[1] This framework also motivates a complementary route to higher child-directed spending: engaging fathers to shift their allocations. In many low- and middle-income settings, men hold greater intra-household bargaining power due to norms and earnings patterns (Jayachandran and Voena, 2025). If fathers' allocations are more elastic to program levers than mothers', for example through the labeling of unconditional cash transfers (Benhassine et al., 2015), engaging fathers may be a high-return margin even when mothers have stronger preferences for child-directed spending at status quo (Evans and Jakiela, 2024; Jeong et al., 2021, 2024).

We study maternal and paternal allocations to children using a lab-in-the-field experiment with 1,330 mothers and fathers of children under age two in Tanzania. The experiment elicits preferences in three phases. First, participants divide an endowment between themselves, their spouse, and their child with allocations framed as making

[1] We use "allocation share", i.e., the share of resources allocated to different members of the household, as a reduced-form object. It may incorporate preferences, information, and salience, norms, and perceived decision rights. We do not separately identify empowerment or bargaining weights.

purchases for the respective household member. Second, each allocated amount from the first phase is further divided into subcategories, allowing us to study preferences for different types of spending on children such as clothes, food, toys, and more. Third, participants are asked to use their allocation decision from the first phase to purchase actual goods from a pop-up shop that stocks a selection of items in each spending category (men's, women's, and children's items). Importantly, we randomize whether the task is incentivized or is completed hypothetically, i.e., without the purchase of actual items in the third phase. The variation allows us to make a methodological contribution by testing whether the hypothetical task version, which is cheaper to implement, validates against the use of economic incentives.

Our experiment is embedded in the *Kizazi Kijacho* ("The Next Generation") randomized controlled trial (KK RCT) in the Dodoma region of Tanzania (Almås et al., 2023). In the KK RCT, households were recruited during pregnancy and assigned to one of three arms that aim to improve early child development: (i) six bi-monthly unconditional cash transfers (UCT) starting in pregnancy, (ii) a parenting intervention with home visits and group sessions aligned with UNICEF's Nurturing Care framework through roughly 24 months after birth, and (iii) a combined Parenting+UCT arm. Within the UCT arm (not in Parenting+UCT), the recipient is randomized between mothers and fathers, allowing us to compare maternal versus paternal transfer receipt. We implement the allocation experiment with parents when children are just under age two. This design lets us contrast *levels* of intra-household resource allocation, i.e., who allocates more in the absence of interventions, with *elasticities*, i.e., whose allocations move when exposed to a parenting intervention, a UCT program, or both.

We report four main findings. First, the hypothetical allocation task validates against the incentivized version: estimates are statistically indistinguishable, which supports the use of a low-cost hypothetical measure for preference measurement and program evaluation. Second, in the KK RCT control group, mothers allocate on average 3.3 percentage points (10%) more of the total endowment to the child than fathers, an effect entirely driven by higher maternal spending on children's clothing. For other subcategories (shoes, food, toys), we find no differences between mothers and fathers. Third, we find no treatment effects on mothers' allocations in any treatment arm. Fourth, fathers respond strongly to program levers. In our preferred specification, when fathers are the UCT recipients their allocation to the child increases by about 5.9 percentage points (an 18 percent increase relative to the control mean), and the Parenting+UCT arm (with transfers targeted to mothers) increases fathers' child share by about 4.7 percentage points (around 14 percent). The Parenting-only arm and the UCT arm

where transfers are targeted to mothers alone also yield positive point estimates for fathers (around 2.5 to 3 percentage points), but these are less precisely estimated and not statistically significant at conventional levels.

Taken together, our findings call for a more nuanced discussions about the effects of gender targeting. Mothers are more child-oriented at status quo, but program-induced movement concentrates among fathers. This implies that policies can complement targeting of mothers with explicit engagement of fathers when the objective is to raise child-directed spending. The pattern also helps explain why comparisons of maternal and paternal transfer recipients often show small average differences in child expenditures: fathers move up toward mothers' baseline level while mothers do not change (Benhassine et al., 2015; Akresh, De Walque and Kazianga, 2016; Armand, 2016; Almås, Haushofer and Kjelsrud, 2019; Almås, Somville and Vanderwalle, 2020). Finally, the increase in fathers' allocations under the parenting plus cash package, despite cash being targeted to mothers, points to within-couple channels such as information, salience, or norms rather than transfer receipt alone, consistent with the view that engaging fathers can operate through mechanisms beyond direct control of funds (Jeong et al., 2021; Evans and Jakiela, 2024; Jeong et al., 2024).

To argue for internal validity of our results, we address the "undoing problem", a common criticism of intra-household experiments (Munro, 2018). Observed behavior in an experiment may be reversed or partially offset by subsequent behavior, for example by an unobserved reallocation of experimental incentives among household members after participants leave the field laboratory or by crowding out of future household purchases. In a phone follow-up four weeks later, we find that only 2 percent of households report giving away, selling, or exchanging any experimental items, and we find no evidence that pop-up shop purchases crowd out subsequent child spending. Thereby, we reduce concern that our the resource allocations in the experiment simply displace later investments.

Our study makes three main contributions. First, we provide new findings about levels and spending categories of parental allocations. At face value, our results on parental preferences at status quo lend support to the rationale for maternal cash transfers, *i.e.*, that mothers spend more of available resources on children than fathers (Armand et al., 2020; Weaver et al., 2025). However, we add important nuance. In particular, we show that the difference between mothers and fathers is entirely driven by spending on clothing. This reconciles the evidence of several studies that focus on purchases of children's clothes and argue that mothers spend more (**?**Attanasio and Lechene, 2002; Bobonis, 2009; Rubalcava, Teruel and Thomas, 2009) with studies that look at other spending categories and document no differences between parents (Haushofer and Shapiro, 2016;

Ringdal and Sjursen, 2021; Cherchye et al., 2025).

Second, we show that program levers move fathers, not mothers. These results align with evidence that active fathering benefits child development and that engaging men remains an important and underused channel (Jeong et al., 2016; Rothenberg and Bornstein, 2024; Jeong et al., 2021; Evans and Jakiela, 2024; Jeong et al., 2024). They also help explain why studies that compare maternal and paternal transfer recipients often find small average differences in child expenditures (Benhassine et al., 2015; Akresh, De Walque and Kazianga, 2016; Armand, 2016; Almås, Haushofer and Kjelsrud, 2019; Almås, Somville and Vanderwalle, 2020): in our setting, fathers catch up to mothers' baseline level when programs engage or resource them.

Third, we make a methodological contribution. We develop a simple lab-in-the-field allocation task that elicits budget shares across household members and spending categories, and we validate a low-cost hypothetical version against an incentivized version with real purchases. The two versions yield statistically indistinguishable estimates, which supports the use of the hypothetical measure for cost-effective program evaluation. The task builds on modified dictator-game designs and in-field purchasing to enhance relevance (Engel, 2011; Doñate-Buendía, García-Gallego and Petrović, 2022; Bakhtiar et al., 2025).

More broadly, our results complement evidence that social safety nets can enhance women's economic achievements and aspects of agency (Peterman et al., 2025) by showing that, for the specific objective of increasing spending on children, programs also benefit from activating the paternal margin. The most child-oriented parent at baseline is not necessarily the most policy-responsive parent; program design should account for both.

2 Data and Experimental Design

2.1 Study Setting and the Kizazi Kijacho RCT

We conducted a lab-in-the-field resource allocation experiment with parents in the Dodoma region of Tanzania, embedded in the *Kizazi Kijacho* ("The Next Generation") large-scale multi-arm clustered randomized controlled trial (KK RCT) by Almås et al. (2023) that was implemented between 2022 and 2025. Communities were randomly assigned to one of three intervention arms or to a control group. The three arms were: (i) unconditional cash transfers (UCT) of 109,000 Tanzanian Shilling (TZS, about USD 47) paid every two months over the duration of 12 months (six total transfers) starting in

pregnancy, (ii) a parenting program with monthly home visits and group sessions aligned with UNICEF's Nurturing Care framework through roughly 24 months after birth, and (iii) a combination of Parenting (for 24 months) and a bi-monthly UCT of 77,000 TZS (for 12 months). Within the UCT arm the transfer recipient was randomized between mothers and fathers; in the Parenting+UCT arm, transfers were targeted to mothers. We implemented the allocation experiment when target children were just under age two. For further details on the KK design, see Appendix A-4 and Almås et al. (2023).

2.2 Participants and Timing

We invited 2,722 households from the KK control group and the three treatment arms that had completed the first follow-up survey in the KK RCT to attend lab sessions held in community centers in November and December 2024. Importantly, at the time of data collection, the last transfer in the UCT and the Parenting+UCT treatment arms was paid at least six months before we measure allocation preferences in the experiment while the parenting intervention in the Parenting and the Parenting+UCT groups were still ongoing, see Appendix A-4 for a timeline.

In total, 2,540 households attended (93.3% of those invited) and all participants gave informed consent before taking part. Of those who showed up, 2,081 arrived with both baseline-identified caregivers present (81.9% of those who showed up, 76.5% of those invited). Appendix Table A-1 compares households that attended the lab sessions with those that did not, and, among attendees, contrasts couples in which both baseline-identified caregivers were present with households where only one caregiver attended. Couples that show up tended to be older on average for both mothers and fathers, the father was more likely to be employed and report to be in good health, the household was more likely to own agricultural land, and their target child was about 0.3 months younger and had more siblings. Among attendees, households that arrived as a couple had mothers with lower mobile phone ownership, better reported health, fathers with lower educational attainment on average, a higher employment rate, higher likelihood to report good health, and these households were more likely to own agricultural land, less likely to have a female household head, and were less likely to belong to the KK RCT control group.

Participation in the allocation task was designed for couples in which both baseline-identified caregivers attended together. If one caregiver was absent, another adult household member of the same gender could attend as a replacement, and the household could still complete the allocation task. We flag such cases and exclude them from the ana-

lytic sample (73 households).[2] Households headed by a single caregiver could attend the session but did not complete the allocation task.

Among eligible couples with both baseline-identified caregivers present, we randomized the respondent identity for the allocation task to Mother, Father, or Couple. If the mother or father was selected, the other caregiver was dismissed before tasks commenced to allow the selected respondent to decide privately. Each caregiver received a show-up fee of TSh 5,000 (about USD 2). This paper analyzes the individual-respondent sample of baseline-identified mothers and fathers only and excludes couple sessions, yielding $N = 1,330$. Couples sessions and ancillary tasks are analyzed separately. An eligibility summary is provided in Appendix Table A-2.

2.3 Experimental Design

Each individual respondent completed two tasks: (i) a resource allocation task studied here and (ii) a task on intra-household decision-making analyzed elsewhere. Task order was randomized. For each task, we also randomized whether it was completed hypothetically or incentivized.

In the resource allocation task, participants received an endowment of TSh 40,000 (real or hypothetical) and allocated it within the household in three phases. First, they divided the endowment across three recipients: themselves, their spouse, and the target child, framed as purchases for each recipient.[3] Second, they allocated each recipient's amount across predefined subcategories. For adults, these were clothing, food, entertainment, health and wellness products, and technical equipment. For children, these were clothing, shoes, food, toys, learning materials, and health and wellness products. The endowment was represented by forty TSh 1,000 bills placed on a cardboard with labeled compartments; the decision granularity was therefore TSh 1,000.

The hypothetical experiment concluded after these first two steps, and participants received no goods or cash. In the incentivized version, a third phase followed in which participants used the phase-one recipient allocations to purchase real goods from a pop-up shop stocked with men's, women's, and children's items at multiple price points

[2]We define a "replacement caregiver" as an adult household member who attended the session in place of a baseline-identified caregiver. Replacement attendance was permitted for session logistics, and the allocation task could be completed. For the analyses in this paper, we restrict to respondents who are baseline-identified mothers or fathers and exclude all observations with a replacement caregiver present.

[3]The task was explicitly framed as using the endowment to purchase goods that fit into distinct categories, i.e., typical goods for men, typical goods for women, and typical goods for children under the age of three. Examples for these goods were contextualized for the Tanzanian study population and a photo of the items was used as an illustrative aid.

(see also Bakhtiar et al., 2025).[4] Participants had to spend all recipient allocations on goods and could not keep cash. Upon entering the shop, respondents could request a reallocation across recipients to match desired items; such changes were recorded. Unless otherwise noted, our main outcomes and estimates use the phase-one allocations that determined intended spending across recipients.

2.4 Outcomes and Estimands

Our primary outcome is the share of the total endowment allocated to the target child in phase one of the allocation task, denoted y_{ic} for respondent i in community c. We also consider category-specific shares of the total endowment from phase two. Unless stated otherwise, outcomes use phase one allocations that determined intended spending across recipients.[5]

All regressions include the pre-specified covariate vector $\mathbf{X}_{ic}$ (facilitator identifier, task order, mother and father's age and education, target child age and sex, survey month fixed effects, and all unbalanced observable characteristics, see Section 2.5). District fixed effects μ_c are also included.[6] Standard errors are clustered at the community level.

Validation of the allocation instrument. We assess whether the hypothetical version of the allocation task reliably measures (i) levels, (ii) mother–father differences, and (iii) the distribution of allocations relative to the incentivized version. Let T_{ic} indicate incentivized delivery ($T_{ic} = 1$ if incentivized) and G_{ic} indicate that the respondent is a mother ($G_{ic} = 1$ for mothers). We estimate:

$$y_{ic} = \alpha + \beta_0 T_{ic} + \beta_1 (T_{ic} \times G_{ic}) + \beta_2 G_{ic} + \gamma' \mathbf{X}_{ic} + \mu_c + \varepsilon_{ic}. \tag{1}$$

For each gender, we test equality of the full distribution of y_{ic} across task versions using two-sample Kolmogorov–Smirnov tests and display kernel densities using a common bandwidth.

[4] The item selection was identical across experimental sessions and was chosen as a results of extensive scoping activities, including opinion polls of out-of-sample mothers and fathers on the usefulness of certain items in each category. Additionally, we visited typical markets in rural and urban areas to ensure that the selected items are commonly sold. See Appendix Table A-7 for an overview of all items offered in each category.

[5] Reallocations requested at the shop are recorded and analyzed in robustness checks. They are not used to construct the primary outcome.

[6] The inclusion of district fixed effects differs from our pre-registered specification where we included community fixed effects. However, the random selection of mothers or fathers as the participant means that there are communities where only mothers or only fathers were selected. If we were to include both community fixed effects and the gender of the participant, those observations would be dropped.

Testing for differences between mothers and fathers in the control group. Restricting to the KK RCT control group, we estimate:

$$y_{ic} = \alpha + \beta G_{ic} + \gamma' \mathbf{X}_{ic} + \mu_c + \varepsilon_{ic}. \tag{2}$$

Here, β captures the average difference in the child share between mothers and fathers in the absence of interventions. We repeat this analysis for the pre-specified category outcomes.

Effects of the KK RCT on allocations. Following our pre-registration, we estimate intention-to-treat effects separately for mothers and for fathers. Let $T_{i,s}$ be the treatment status for participant i where s indicates the treatments (parenting, UCT, and parenting + UCT, respectively). With Control as the omitted group, we estimate:

$$y_{ic} = \alpha + \theta_s T_{i,s} + \delta \mathbf{X}_i + \varepsilon_i. \tag{3}$$

In line with our pre-registration, we analyze the mother and father samples separately and distinguish between participants that were targeted by the UCT in the UCT group (Targeted UCT) and those that were not targeted (Non-targeted UCT).[7]

2.5 Sample and Balance

Our descriptives focus on the analytic sample of baseline-identified mothers and fathers who completed the individual version of the allocation task. Table 1 reports pre-experiment characteristics overall and by respondent assignment and provides two-sided t-tests of equality (mother vs father sample).

The mothers are on average 27 years old, 64% of them have completed primary school or more, and 51% of mothers are formally or self-employed. Just over half of all mothers own a mobile phone and 72% report to be in at least good health. In contrast, fathers are, on average, 34 years old and 70% of them have completed primary school. Employment rates and self-reported health are similar to mothers.

At the respective day of the experiment, the target children in our sample were, on average 20.9 months old, and 51% of them were boys while each child has, on average, just under three siblings. More than 70% of households in the sample own agricultural land, 13% own non-agricultural land, 4% have ever received a payment by the Tanzanian

[7] In our pre-registration, we specified to limit the sample for equation (3) in the UCT group to the respective UCT target beneficiaries. However, we believe that also including participants who were not targeted in the specification improves on the generality of our results. A robustness analysis where we restrict our sample is presented in the Appendix.

Table 1: Descriptive statistics and balance tests.

	Full sample		Mother sample		Father sample		
	Mean	Observations	Mean	Observations	Mean	Observations	p-value
Mother characteristics							
Age	27.01	1330	26.92	755	27.13	575	0.554
Finished primary school	0.64	1330	0.64	755	0.63	575	0.677
Employed	0.51	1330	0.50	755	0.53	575	0.367
Has mobile phone	0.54	1329	0.54	754	0.54	575	0.931
Reports good health	0.72	1330	0.72	755	0.72	575	0.905
Father characteristics							
Age	34.16	1330	34.05	755	34.30	575	0.620
Finished primary school, father	0.70	1330	0.71	755	0.68	575	0.182
Employed	0.57	1330	0.54	755	0.60	575	0.024
Reports good health	0.74	1330	0.72	755	0.77	575	0.033
Target child characteristics							
Age, in months	20.92	1330	20.97	755	20.85	575	0.266
Is boy	0.51	1330	0.48	755	0.53	575	0.076
Number of siblings	2.78	1330	2.74	755	2.83	575	0.509
Household characteristics							
Owns agricultural land	0.72	1330	0.72	755	0.73	575	0.576
Owns non-agricultural land	0.13	1330	0.12	755	0.14	575	0.236
Ever received TASAF	0.04	1330	0.03	755	0.04	575	0.325
Mother is household head	0.07	1329	0.09	754	0.04	575	0.001
Treatments							
Control group	0.26	1330	0.27	755	0.26	575	0.588
Parenting+UCT	0.25	1330	0.25	755	0.24	575	0.550
Parenting	0.24	1330	0.21	755	0.26	575	0.030
UCT	0.25	1330	0.26	755	0.24	575	0.328

Notes: Participant, spouse, target child, and household characteristics as elicited during the baseline data collection in the KK RCT as well as treatment assignment in the KK RCT. The target child age (in months) is adjusted for the timing of the experiment. Characteristics are presented for the full sample and separately by assignment of mother or father to participate. P-values refer to a mean comparison t-test between mother and father sample.

Social Action Fund (TASAF), Tanzania's national social protection scheme, and for 7% of households, the mother is the household head.

Characteristics are very similar across the two groups. The only notable differences are a higher share of households with father respondents that were assigned to the Parenting arm, better reported paternal health and father employment in the father sample (significant at the 5 percent level), a slightly higher share of boys in father-respondent households (marginal at conventional levels), and a higher likelihood of the mother being the household head in the mother sample (significant at the 1 percent level).

In Appendix Table A-3 and Table A-4, we also present balance tests by task version (hypothetical versus incentivized) and by KK RCT treatment assignment. For our analyses, we control for all unbalanced characteristics in the applicable specification.

2.6 Follow-up consumption survey

To assess whether in-kind incentives were undone or displaced later investments, we conducted a phone follow-up with a random subsample of households four weeks after the session. The phone survey was implemented with a one-third subsample of KK households already enrolled in longitudinal phone follow-ups since baseline. The survey elicited whether experimental items were sold, exchanged, or given away, and collected information on household consumption since the session. We analyze these patterns as a validity check in Section 3.4.

3 Results

3.1 Validation: Incentivized and Hypothetical Versions

As discussed in Section 2.4, we validate the allocation instrument along three dimensions: levels for mothers and fathers separately, the mother–father gap, and the distribution of allocations across task versions.

Levels. The mean share allocated to children by mothers is 36.1% in the incentivized task and 37.3% in the hypothetical task (two-sample t-test $p = 0.237$). For fathers, the mean share is 36.7% in the incentivized task and 37.0% in the hypothetical one (two-sample t-test $p = 0.773$). Thus, the share allocated to the child is not statistically different in the hypothetical and incentivized versions for both mothers and fathers. This is confirmed in regression form (see Appendix Table A-5) that includes district fixed effects, pre-specified covariates and all imbalanced observable characteristics following balance tests in Appendix Table A-3.

Mother–father gap. The interaction between the hypothetical indicator and the mother indicator is close to zero and not statistically different from zero (Appendix Table A-5), implying that the mother–father difference is invariant to task version.

Distributions. Figure 1 compares kernel densities of the child share by version for fathers (Panel A) and mothers (Panel B). Kolmogorov–Smirnov tests do not reject equality of the distributions for either gender (fathers: $p = 0.780$, mothers: $p = 0.819$). Consistent with our pre-registration, we therefore pool the incentivized and hypothetical versions in all subsequent analyses.

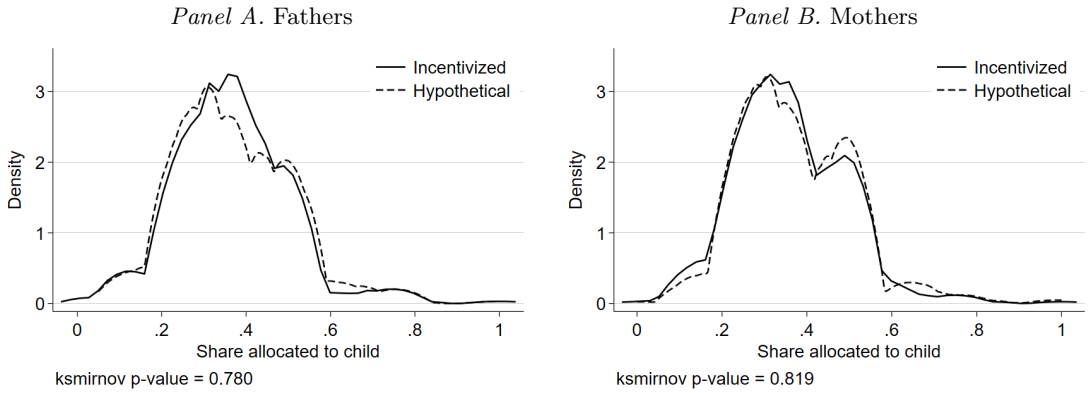


Figure 1: Distribution of share allocated to the child.

Notes: The figure plots the distribution of the share allocated to the child for the incentivized (solid line) and the hypothetical task versions (dashed line) for fathers in panel A and mothers in panel B. Below each plot, we additionally report the p-value of a two-sample Kolmogorov-Smirnov test on the equality of distributions.

3.2 Maternal and Paternal Preferences at Status Quo

To test for a difference between maternal and paternal preferences in households that did not receive an intervention, we compare the allocation decisions by mothers to the allocation decisions by fathers for all participants from households in the KK RCT control group, see Table 2 for results. Column (1) includes no controls or district fixed effects. Column (2) adds district fixed effects, and Column (3) adds controls.

Fathers in the control group allocate 33.6% of the endowment to the target child. Mothers allocate 3.4 to 4.1 percentage points more, a significant difference at the 1% to 5% level. Hence, we find evidence in favor of higher maternal spending: mothers allocate about 10 to 12% more to their child than fathers.

The second phase of the resource allocation task allows us to decompose the parental preferences we observe in the first phase into different spending types. In particular, we can study (i) whether mothers and fathers prioritize different spending categories, and (ii) whether one or several of the spending categories drive the difference in allocation to the child between mothers and fathers in the first phase.

In Figure 2, we plot the share of the total endowment allocated to the six different spending categories for children in the KK RCT control group: clothes, shoes, food, toys, learning materials, and health expenses. We observe that both fathers and mothers prioritize spending on children's clothes, i.e., it is the subcategory that parents, on average, allocate the most resources to. At the same time, the descriptive results also suggest that the difference in spending preferences between fathers and mothers in the

Table 2: Parental preferences in the KK RCT control group.

	(1)	(2)	(3)
Mother respondent	0.039**	0.041***	0.034**
	(0.015)	(0.015)	(0.016)
Father mean	0.336	0.336	0.336
District FE	No	Yes	Yes
Controls	No	No	Yes
R-squared	0.019	0.039	0.200
Observations	350	350	350

Notes: The table reports OLS regression results comparing paternal and maternal preferences in the control group of the KK RCT. The control variables include experimenter fixed effects, the mother's and father's age and education level, whether the allocation task was played first, and the target child age (in months) and gender. We further control for all additional unbalanced characteristics, see Table 1, including whether the father is employed, whether he reports to be in good health, and whether the mother is the head of household. Missing values for the participant's age and education, as well as the child's age are imputed with the district-specific median value. Standard errors (in parentheses) are clustered on the community level. ***, **, and * indicate significance at the 1,5, and 10% level.

first phase is entirely driven by spending on children's clothes. This result is supported by a formal regression, see Appendix Table A-6. Mothers spend 2.7 percentage points more of the total endowment on children's clothing, while there is no significant difference for any other spending categories.

3.3 The Effect of Early Childhood Development Interventions on Parental Spending

The main treatment arms of the KK RCT enable us to study parental preferences for resource allocation to children in different contexts. In Table 3, we report regression results for mothers in Panel A and fathers in Panel B. First, we note that neither treatment has an effect for mothers, with reasonably precise estimates of zero in all arms. In contrast, the share of the endowment allocated to the child increases by 4.8 percentage points in the Parenting+UCT arm ($p = 0.019$) and by 6.1 percentage points when the household received UCTs targeted to the father (Targeted UCT, $p = 0.004$). For fathers in households where UCTs were targeted to the mother only (Non-targeted UCT) and in the Parenting-only arm, the coefficients are positive but smaller in magnitude (3.0 and 2.3 percentage points, respectively) and not statistically significant at conventional levels ($p = 0.124$ and $p = 0.146$, respectively).

Taken together, our results suggest that UCTs induce fathers to spend a higher share of available household resources on their child, especially when fathers are the designated recipients or when cash is combined with parenting support. Compared to the level of resource allocation to children by mothers, which is unaffected, fathers essentially catch

Table 3: KK RCT treatment effects with UCT targeting.

Panel A: Mothers							
	Phase 1	*Phase 2*					
	(1) Allocation child	(2) Clothes	(3) Shoes	(4) Food	(5) Toys	(6) Learning	(7) Health
Parenting+UCT	-0.008	-0.001	-0.003	-0.002	0.004	0.000	-0.008
	(0.016)	(0.010)	(0.005)	(0.006)	(0.004)	(0.005)	(0.005)
Parenting	-0.005	-0.006	-0.001	0.000	0.001	0.004	-0.003
	(0.015)	(0.008)	(0.004)	(0.007)	(0.004)	(0.005)	(0.005)
Targeted UCT	0.010	0.007	0.000	-0.001	0.003	-0.004	0.004
	(0.016)	(0.010)	(0.006)	(0.007)	(0.004)	(0.004)	(0.006)
Non-targeted UCT	0.003	0.016	0.004	-0.004	0.002	-0.007*	-0.008
	(0.019)	(0.011)	(0.006)	(0.007)	(0.005)	(0.004)	(0.005)
Control group mean	0.374	0.130	0.068	0.068	0.036	0.037	0.037
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.095	0.148	0.061	0.065	0.118	0.111	0.126
Observations	754	754	754	754	754	754	754
Panel B: Fathers							
	Phase 1	*Phase 2*					
	(1) Allocation child	(2) Clothes	(3) Shoes	(4) Food	(5) Toys	(6) Learning	(7) Health
Parenting+UCT	0.048**	0.013	0.003	0.005	0.018***	0.001	0.008
	(0.020)	(0.009)	(0.006)	(0.008)	(0.005)	(0.006)	(0.006)
Parenting	0.024	0.018*	-0.004	0.002	0.003	-0.004	0.008*
	(0.017)	(0.009)	(0.006)	(0.008)	(0.004)	(0.006)	(0.005)
Targeted UCT	0.061***	0.023**	0.003	0.019	0.005	0.004	0.007
	(0.021)	(0.010)	(0.007)	(0.013)	(0.006)	(0.008)	(0.006)
Non-targeted UCT	0.031	0.013	-0.006	0.013	0.005	-0.006	0.012*
	(0.020)	(0.010)	(0.006)	(0.011)	(0.005)	(0.007)	(0.007)
Control group mean	0.336	0.101	0.065	0.063	0.031	0.042	0.033
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.101	0.193	0.127	0.085	0.118	0.165	0.137
Observations	575	575	575	575	575	575	575

Notes: The table reports OLS regression results for a test for treatment effects in the KK RCT. All models include district fixed effects and a selection of control variables, which include experimenter fixed effects, the participant's age and education level, whether the allocation task was played first, and the target child age (in months) and gender. We additionally control for all unbalanced characteristics, see Appendix Table A-4, including indicators for employment for the mother and whether the mother owns a mobile phone. If a participant allocated nothing to the child in the first phase, the second phase is skipped. Missing values for the participant's age and education, as well as the child's age are imputed with the district specific median value. Standard errors (in parentheses) are clustered on the community level. ***, **, and * indicate significance at the 1,5, and 10% level.

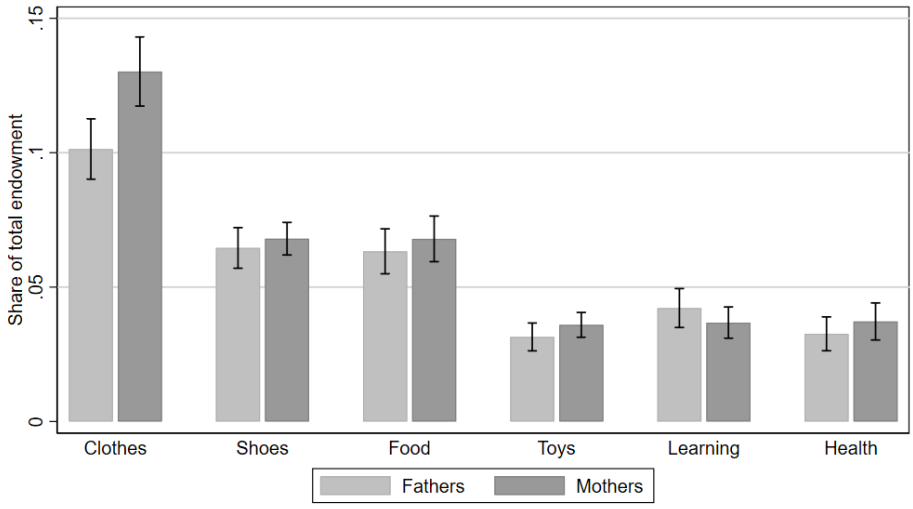


Figure 2: Second phase allocation for the target child in KK RCT control group.

Notes: The figure plots the share of the total endowment allocated to the different categories for the spending on children in the second phase of the resource allocation task, split by whether the participant is the father (light gray) or the mother (dark gray). Only considers the KK RCT control group sample. Whiskers indicate the 95% confidence interval.

up. The UCT component appears necessary to increase paternal spending on children, but the role of targeting the UCT to the father is ambiguous: the coefficients on Targeted and Non-targeted UCT are not statistically different from each other.

In columns 2 to 7 of Table 3 we study the allocation in the second phase, repeating the estimation of equation (3) for each spending category. For mothers, neither treatment leads to noteworthy changes in their resource allocation to different spending types. For fathers, we see that they increase their spending on clothes when targeted by a cash transfer. In the case of Parenting+UCT, fathers increase their spending on toys.

3.4 The Undoing Problem

All incentivized experiments on household resource allocation face an identification challenge that arises from a potential unobserved re-allocation of experimental incentives among household members after participants leave the field laboratory. For example, under low bargaining power, mothers may have the resources they allocated to themselves or to their children taken from them by the husband, who would effectively undo the allocation decision observed in the experiment. If present, this *undoing problem* can compromise internal validity.

The incentivization through actual purchases from our pop-up shop should reduce the

undoing problem by design. That is, in the third phase of our experiment, participants commit to an allocation of resources that is (i) observed while participants are still in the experimental setting, and (ii) is more difficult to undo after the experiment than a cash incentive payment (see Munro, 2018, for a discussion on reversible behavior). While cash payments can simply be appropriated, in-kind transfers would need to first be sold or exchanged, in particular those that are of no use across spending categories, e.g., parents have no inherent use for a toy beyond giving it to their children.

Reallocation at the pop-up shop. Whereas the immediate purchase of goods in the experiment should reduce the undoing problem by design, the practicalities of the pop-up shop deserve further discussion. The item selection in the shop needs to harmonize several considerations. First, one wants to offer a large variety of goods to cater to different individual preferences. Second, the shop should offer low-, medium-, and high-value items to allow for sufficient coverage of all possible allocation amounts from the first phase of the experiment. However, there are logistical constraints tied to the procurement of a large variety of goods and the transportation to often rural locations, forcing the researcher to commit to a reasonably small item selection. Finally, there are practical limitations on offering perishable items such as food as well as goods that would necessitate stocking them in different sizes such as shoes or ready-to-wear clothing.

In many field applications of such a pop-up shop, including ours, it is therefore inevitable that the selection of goods will not satisfy all participants' tastes in all categories. To recognize this limitation, we allowed participants to reallocate their endowment between categories (male, female, and children goods) when they visited the pop-up shop. The reallocation was only allowed upon request by the participant and all participants were blind to this possibility when making their original allocation decision in the first phase of the task, preserving its validity as our main outcome of interest. Crucially, any reallocation was recorded, allowing us to study this behavior as a part of a potential undoing problem.

The request to reallocate is common, with 73% of participants choosing a different allocation of resources when they visit the pop-up shop, 92% of which reallocate resources to or from the target child. In Figure 3, we plot the average share of the total endowment reallocated to the child in the third phase by mothers and fathers in each of the KK RCT groups. Since participants in the hypothetical version did not visit the pop-up shop, we only plot results for the 669 participants who completed the incentivized version. Throughout all treatment groups for both mothers and fathers, it is evident that participants reallocate a substantial share of the endowment *away* from the child.

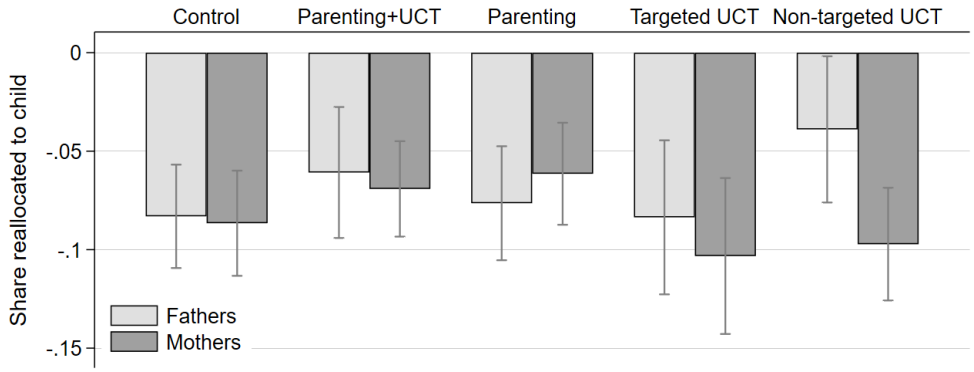


Figure 3: Reallocation to the child in the third phases of the experiment.

Notes: The figure plots the share of the total endowment reallocated to the child in the third phase of the experiment for mothers (dark gray) and mothers (light gray) by treatment group. The sample is limited to the 703 participants that completed the task in the incentivized version and therefore visited the pop-up shop.

On average, the share allocated to the child decreases by 7.6 percentage points between the first and third phase of the experiment (6.9 percentage points among fathers and 8.1 percentage points among mothers), while the share allocated to the mother and father increase by 3.7 and 3.8 percentage points, respectively.

We test for differences in the reallocation at the pop-up shop more formally through linear regression, see Table 4. Similar to the descriptive results, the regressions suggest that the reallocation away from the child occurs across the sample. We find no difference between mothers and fathers (see Column 1) and no systematic differences between KK RCT groups when studying the father and mother sample separately (see columns 2 and 3). The exception are marginally significant estimates suggesting that fathers in the Parenting+UCT and Non-targeted UCT groups reallocate less resources away from the child.

For a reallocation to female and male category goods, i.e., a reallocation of resources to the mother or father, see columns 4 to 9. We find that mothers re-allocate more to female goods than fathers (Column 4) and that fathers in the Parenting+UCT and Parenting groups do not reallocate resources to their spouse. For reallocations to the father, we find neither differences between mothers and fathers, nor between the different treatment arms.

In summary, the purchase behavior in the pop-up shop suggests that a reallocation away from the target child is common but is neither driven by fundamental differences between mothers and fathers nor the result of the KK RCT treatments. A residual explanation is the item selection in the pop-up shop that we discuss above. Children's

Table 4: Reallocation at the pop-up shop.

	Reallocation to child			*Reallocation to mother*			*Reallocation to father*		
	All	Fathers	Mothers	All	Fathers	Mothers	All	Fathers	Mothers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Mother respondent	-0.007			0.024*			-0.017		
	(0.015)			(0.013)			(0.012)		
Parenting+UCT		0.058*	0.026		-0.053**	-0.020		-0.005	-0.006
		(0.032)	(0.029)		(0.027)	(0.027)		(0.031)	(0.022)
Parenting		0.027	0.037		-0.072**	-0.043		0.045	0.006
		(0.032)	(0.030)		(0.029)	(0.027)		(0.035)	(0.020)
Targeted UCT		-0.024	-0.037		-0.004	0.031		0.028	0.006
		(0.035)	(0.036)		(0.035)	(0.036)		(0.036)	(0.025)
Non-targeted UCT		0.061*	-0.017		-0.043	0.002		-0.017	0.014
		(0.036)	(0.028)		(0.030)	(0.029)		(0.031)	(0.022)
Father mean	-0.069			0.023			0.046		
Control group mean		-0.083	-0.087		0.053	0.052		0.030	0.035
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.100	0.198	0.170	0.092	0.203	0.136	0.101	0.167	0.174
Observations	669	289	380	669	289	380	669	289	380

Notes: The table reports OLS regression results on the difference between the share of the endowment allocated to a respective category between the first and third phase of the experiment. The sample is restricted to participants that completed the incentivized version and therefore visited the pop-up shop. All models include district fixed effects and a selection of control variables which include experimenter fixed effects, the participant's age and education, whether the allocation task was played first, and the target child's age (in months). Columns 1, 4, and 7 additionally control for all unbalanced characteristics, see Table 1, including whether the father is employed, whether he reports to be in good health, and whether the mother is the head of household. Columns 2, 3, 5, 6, 8, and 9 additionally control for unbalanced characteristics, see Appendix Table A-4, including whether the mother is employed and she owns a mobile phone.. ***, **, and * indicate significance at the 1,5, and 10% level.

items may have been comparatively less suitable to participants' taste than the selection in the male and female categories. Under the restriction that receiving part of the endowment in cash was not an option, participants then opted to reallocate their resources away from less preferred child items to goods for themselves or their spouse.

Selling or exchanging goods. Another form of the undoing problem is selling or exchanging the goods from the experiment. To identify such behavior, we focus on the phone survey conducted four weeks after the experiment. Here, we contacted 467 of the 1,330 households that we analyze in this study, 245 of which received in-kind transfers because they completed the resource allocation task in the incentivized version.[8] Besides information on general consumption, we specifically asked respondents what they did with the goods they chose from the pop-up shop in the experiment. Only five households (2% of the sample) report that they either gave away, exchanged, or sold at least some of the goods, a behavior that can be consistent with the undoing problem. The overwhelming majority of households report to already use the received goods or to save them for

[8]Households that completed the hypothetical version of the allocation task and therefore did not receive in-kind incentives instead received cash incentives based on their decisions in the second experimental task.

Table 5: Household consumption after the experiment.

	Fathers			*Mothers*		
Total consumption of:	Children	Male	Female	Children	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)
% of pop-up shop purchases for:						
Target child	364.605*			-137.004		
	(191.855)			(134.674)		
Father		-188.380**			56.583	
		(88.460)			(58.193)	
Mother			59.561			37.701
			(119.391)			(71.569)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.319	0.293	0.284	0.139	0.199	0.171
Observations	108	108	108	156	156	156

Notes: The table reports OLS regression results on the self-reported total households consumption of different household members (children, male adults, and female adults) in the four weeks after the experiment. The dependent variable in each category is the total consumption of garments, footwear, and other personal products for children, men, and women, respectively. The independent variables are transformed to indicate the effect of a percentage point increase in the share of the endowment used on pop-up shop purchases. The sample is restricted to participants that completed the incentivized version and therefore visited the pop-up shop. All models include district fixed effects and a selection of control variables which include experimenter fixed effects, the participant's age and education, whether the allocation task was played first, and the target child's age (in months) and gender. ***, **, and * indicate significance at the 1,5, and 10% level.

future needs.

Post-experiment consumption. Finally, we use the data from the follow-up phone survey on households' consumption to study whether the purchases from the pop-up shop induce changes in what otherwise would have been purchased and consumed in the period after the experiment. That is, households may use the selection of goods in our pop-up shop as a substitute for purchasing these same or similar items later on. Should the pop-up shop purchases for children crowd out future investments in children's clothing, toys, or education, our measurement of parental spending on children in the experiment would be biased upwards.

In Table 5, we test whether the pop-up shop purchases crowd out future consumption within spending categories. Specifically, we test how a one percentage point increase in the share of the total endowment used for selecting child (male, female) items affects the self-reported consumption of child (male, female) items in the four weeks after the experiment (in TSh). Consumption in each category includes purchases for footwear, clothing, and other personal items, e.g., razor blades and wallets for men, cosmetics and sanitary products women, and baby powder and towels for children. We do not find crowding out of purchases for children, independent of whether the father or the mother participated in the resource allocation task, see columns 1 and 4. In fact, we find weak evidence that a household's spending on child items *increases* with the father's

spending on child items in our shop (see Column 1, $p = 0.062$). Overall, results suggest that parents do not use the children specific goods obtained from the pop-up shop as a substitute for investing in their offspring later on. Additionally, we find that a higher allocation of fathers on themselves in the experiment crowds out own consumption in the period afterwards, see Column 2. Consumption of households where the mother participated in the experiment appears to be unaffected by the items obtained from the pop-up shop.

4 Conclusion

We integrate a lab-in-the-field experiment on parental resource allocation into a large-scale randomized controlled trial in Tanzania to study how mothers and fathers allocate resources to young children and how these allocations respond to interventions that aim to improve early childhood development. Our design allows us to compare the behavior of mothers and fathers in a common, standardized decision problem, to validate a low-cost hypothetical version of the task against an incentivized version with real purchases, and to study how cash transfers and parenting support shape parental spending preferences more than half a year after the last transfer.

Three empirical patterns stand out. First, in the absence of interventions mothers allocate more of a given endowment to the child than fathers. In the KK RCT control group, mothers' child share exceeds fathers' by around 3–4 percentage points, or about 10–12 percent of the fathers' mean. This difference is entirely driven by spending on children's clothing; for food, shoes, and toys, mothers and fathers allocate very similar shares. These facts help reconcile clothing-focused evidence on maternal targeting with work that finds little difference across broader spending categories.

Second, the KK RCT interventions leave mothers' allocations unchanged but significantly increase fathers' child-directed spending. Fathers' child share increases by about 6 percentage points when fathers receive UCTs, and by about 5 percentage points in the combined Parenting+UCT arm where UCTs are received by mothers. When UCTs are targeted to mothers alone and in the Parenting-only arm, the effects for fathers are smaller in magnitude, around 3 and 2.5 percentage points, respectively, and imprecisely estimated. Taken together, these results indicate that lifting resource constraints in early life shifts fathers' allocations toward children, while mothers' allocations are remarkably stable. The difference between targeting transfers to fathers versus mothers is modest in magnitude and not statistically significant, so average child spending can look similar across maternal and paternal transfer designs because fathers catch up to mothers'

baseline level when either parent is resourced.

Third, we provide evidence that our experimentally elicited allocations are behaviorally meaningful and not strongly undone after the session. The hypothetical and incentivized versions of the task yield indistinguishable means, gaps, and distributions by parent gender. Reallocations at the pop-up shop move resources away from children on average, but in similar ways for mothers and fathers and across KK arms, consistent with a constrained item menu rather than strategic undoing. In follow-up phone surveys, only about 2 percent of households report selling, exchanging, or giving away any experimental items, and we detect no crowding out of subsequent child-specific consumption by pop-up shop purchases. These checks bolster the internal validity and practical usefulness of the allocation task as a measure of parental preferences over spending.

Our findings bridge two strands of evidence that have often appeared in tension. In the control group, mothers are more child-oriented than fathers in the sense of allocating more of a fixed budget to the child, which supports the rationale for targeting transfers to women. On the margin, however, it is fathers who adjust their allocations in response to cash and parenting interventions, while mothers do not. This pattern helps explain why studies comparing maternal and paternal transfer recipients often find small average differences in child expenditures: in our setting, fathers move up to mothers' baseline allocations when programs resource or engage them.

Turning to policy, our findings indicate that it is not always optimal to target mothers rather than fathers in programs aimed at improving child development. Even when mothers spend more on children under the status quo and are the primary caregivers, fathers may respond more strongly when they are targeted. These insights extend beyond our specific study context. Globally, program design should go beyond identifying the status quo, and also consider which caregiver is likely to be most responsive to the policy in terms of investments in children.

Our results point to several avenues for future work. First, one may seek to explain why paternal allocation preferences are more elastic to policy. A possible explanation is the relaxation of household credit constraints which, in their traditional role as breadwinners and primary decision-makers, may disproportionately affect men. If the UCTs in the KK RCT enabled fathers to satisfy their family's subsistence needs, child investments may have been prioritized next. Also, a relaxation of resource constraints may have allowed fathers to work less and instead spend time on childcare duties, inducing more resource allocation to children. Alternatively, the findings may suggest that shifting fathers' roles—for example by enhancing their sense of responsibility and strengthening their identity as parents—produces larger behavioral changes in households where moth-

ers are already the primary caregivers and have an established identity as mothers. A different explanation may be that fathers may have responded to the minimal labeling of the cash transfer schemes. While transfers were unconditional, households were generally aware that they participated in the program because of the birth of their child. Fathers may have perceived that the transfers should benefit children and acted accordingly.

Second, we study allocations at a specific point in the child's early life but it would be valuable to understand whether paternal adjustments persist as children age. A promising sign for persistence can already be drawn from our study. That is, we study allocation preferences more than 6 months after the last transfer in the Kizazi Kijacho UCT program. With an effect of the UCT on fathers' resource allocation to children, we demonstrate that paternal allocation preferences do not revert immediately when transfers end. Third, we focus on a resource allocation margin. Integrating time use and direct measures of parenting behavior would help to map spending responses into broader early childhood development outcomes. Finally, our validation and undoing checks suggest that simple hypothetical allocation tasks can be informative and scalable tools for program evaluation and design in low-resource settings. Replicating this approach in other contexts and with different program mixes could clarify when and how engaging fathers is a high-return margin for raising investments in children.

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Appendix — For Online Publication

A-1 Attrition and Additional Summary Statistics

Table A-1: Attrition from the experiment by characteristics

	Showed up vs. did not show up.					Came as couple vs. did not come as couple				
	Mean	Observations	Mean	Observations	p-value	Mean	Observations	Mean	Observations	p-value
Mother characteristics										
Age	27.07	2540	25.07	182	0.000	27.13	2081	26.80	459	0.340
Finished primary school	0.64	2540	0.59	182	0.145	0.64	2081	0.66	459	0.488
Employed	0.51	2540	0.52	182	0.936	0.51	2081	0.52	459	0.889
Has mobile phone	0.56	2538	0.59	182	0.443	0.54	2079	0.62	459	0.003
Reports good health	0.71	2540	0.75	182	0.334	0.73	2081	0.66	459	0.003
Father characteristics										
Age	34.35	2540	33.09	182	0.075	34.37	2081	34.26	459	0.825
Finished primary school, father	0.71	2540	0.75	182	0.339	0.71	2081	0.75	459	0.050
Employed	0.54	2540	0.46	182	0.032	0.56	2081	0.45	459	0.000
Reports good health	0.71	2540	0.59	182	0.001	0.73	2081	0.58	459	0.000
Target child characteristics										
Age, in months	20.98	2540	21.30	182	0.028	20.95	2081	21.11	459	0.106
Is boy	0.51	2540	0.57	182	0.120	0.50	2081	0.53	459	0.194
Number of siblings	2.80	2540	1.95	182	0.000	2.83	2081	2.65	459	0.186
Household characteristics										
Owns agricultural land	0.70	2540	0.62	182	0.024	0.72	2081	0.63	459	0.000
Owns non-agricultural land	0.13	2540	0.16	182	0.324	0.13	2081	0.14	459	0.678
Ever received TASAF	0.04	2540	0.06	182	0.319	0.04	2081	0.06	459	0.058
Mother is household head	0.08	2538	0.08	182	0.958	0.07	2079	0.14	459	0.000
Treatments										
Control group	0.27	2540	0.29	182	0.664	0.26	2081	0.31	459	0.069
Parenting+UCT	0.24	2540	0.21	182	0.422	0.25	2081	0.21	459	0.134
Parenting	0.24	2540	0.27	182	0.305	0.24	2081	0.25	459	0.595
UCT	0.25	2540	0.23	182	0.499	0.25	2081	0.23	459	0.360

Notes: Overview of attrition rates from the experiment by observable father, mother, target child, and household characteristics, as well as KK RCT treatment assignment.

Table A-2: Show-up, eligibility, and analytic sample.

Invited households	2,722
Showed up at session	2,540
Both baseline-identified caregivers present	2,081
Assigned to Couple	678
Assigned to Mother	764
Assigned to Father	639
Included in analytic Mother sample	755
Included in analytic Father sample	575
Complete analytic sample	1,330

Table A-3: Sample characteristics with balance test between hypothetical and incentivized sample.

	Full sample					Mother sample					Father sample				
	Hypothetical		Incentivized			Hypothetical		Incentivized			Hypothetical		Incentivized		
	Mean	Obs	Mean	Obs	p-value	Mean	Obse	Mean	Obs	p-value	Mean	Obs	Mean	Obs	p-value
Mother characteristics															
Age	27.05	661	26.97	669	0.821	27.04	375	26.79	380	0.607	27.07	286	27.20	289	0.811
Finished primary school	0.62	661	0.65	669	0.281	0.63	375	0.65	380	0.555	0.61	286	0.65	289	0.338
Employed	0.53	661	0.50	669	0.321	0.53	375	0.48	380	0.156	0.52	286	0.53	289	0.906
Has mobile phone	0.54	660	0.54	669	0.790	0.54	374	0.54	380	0.986	0.55	286	0.53	289	0.700
Reports good health	0.71	661	0.73	669	0.455	0.72	375	0.72	380	0.897	0.70	286	0.75	289	0.198
Father characteristics															
Age	34.15	661	34.17	669	0.955	34.37	375	33.74	380	0.359	33.86	286	34.74	289	0.230
Finished primary school, father	0.71	661	0.69	669	0.351	0.73	375	0.69	380	0.241	0.68	286	0.68	289	0.926
Employed	0.58	661	0.56	669	0.560	0.56	375	0.53	380	0.393	0.60	286	0.61	289	0.919
Reports good health	0.72	661	0.76	669	0.078	0.70	375	0.74	380	0.244	0.75	286	0.80	289	0.174
Target child characteristics															
Age, in months	20.96	661	20.87	669	0.401	20.95	375	20.98	380	0.842	20.97	286	20.73	289	0.137
Is boy	0.51	661	0.50	669	0.699	0.48	375	0.49	380	0.685	0.56	286	0.51	289	0.293
Number of siblings	2.89	661	2.68	669	0.150	2.87	375	2.61	380	0.153	2.91	286	2.76	289	0.538
Household characteristics															
Owns agricultural land	0.69	661	0.75	669	0.023	0.67	375	0.77	380	0.002	0.73	286	0.73	289	0.986
Owns non-agricultural land	0.12	661	0.14	669	0.255	0.13	375	0.11	380	0.532	0.11	286	0.18	289	0.020
Ever received TASAF	0.03	661	0.04	669	0.594	0.04	375	0.03	380	0.520	0.03	286	0.06	289	0.161
Mother is household head	0.06	660	0.08	669	0.214	0.07	374	0.10	380	0.223	0.04	286	0.04	289	0.696
Treatments															
Control group	0.28	661	0.25	669	0.169	0.28	375	0.26	380	0.396	0.28	286	0.24	289	0.261
Parenting+UCT	0.25	661	0.25	669	0.898	0.27	375	0.24	380	0.268	0.21	286	0.27	289	0.136
Parenting	0.24	661	0.23	669	0.657	0.20	375	0.22	380	0.481	0.29	286	0.24	289	0.162
UCT	0.23	661	0.27	669	0.089	0.24	375	0.28	380	0.196	0.22	286	0.26	289	0.271

Notes: Participant, spouse, target child, and household characteristics as elicited during the baseline data collection in the KK RCT as well as treatment assignment in the KK RCT. The target child age (in months) is adjusted for the timing of the experiment. Characteristics are presented for the full sample and separately by assignment of mother or father to participate, each of which is again presented separately for the incentivized and hypothetical task version. P-values refer to the respective mean comparison t-test between hypothetical and incentivized task versions.

Table A-4: Sample characteristics with balance test between KK RCT groups.

	Control		Parenting + UCT			Parenting			UCT		
	Mean	Obs	Mean	Obs	p-value	Mean	Obs	p-value	Mean	Obs	p-value
Mother characteristics											
Age	27.13	350	26.72	330	0.418	26.85	313	0.592	27.32	337	0.711
Finished primary school	0.61	350	0.63	330	0.560	0.66	313	0.160	0.65	337	0.230
Employed	0.57	350	0.48	330	0.019	0.49	313	0.033	0.51	337	0.147
Has mobile phone	0.51	350	0.53	329	0.653	0.55	313	0.332	0.58	337	0.078
Reports good health	0.71	350	0.71	330	0.984	0.74	313	0.392	0.72	337	0.714
Father characteristics											
Age	34.36	350	33.69	330	0.343	34.26	313	0.886	34.31	337	0.947
Finished primary school, father	0.71	350	0.66	330	0.207	0.72	313	0.643	0.71	337	0.920
Employed	0.57	350	0.59	330	0.611	0.55	313	0.622	0.57	337	0.962
Reports good health	0.73	350	0.72	330	0.966	0.77	313	0.191	0.75	337	0.404
Target child characteristics											
Age, in months	21.13	350	20.68	330	0.002	21.04	313	0.541	20.80	337	0.014
Is boy	0.51	350	0.47	330	0.215	0.54	313	0.564	0.51	337	0.857
Number of siblings	2.75	350	2.71	330	0.840	2.85	313	0.652	2.81	337	0.758
Household characteristics											
Owns agricultural land	0.73	350	0.74	330	0.951	0.71	313	0.473	0.71	337	0.464
Owns non-agricultural land	0.10	350	0.12	330	0.376	0.17	313	0.008	0.14	337	0.067
Ever received TASAF	0.04	350	0.02	330	0.180	0.03	313	0.225	0.06	337	0.415
Mother is household head	0.07	350	0.07	329	0.824	0.06	313	0.810	0.07	337	0.863

Notes: The table reports summary statistics for different household members for all households in the experiment as observed in the second survey wave of the KK RCT. The respective p-value column for each treatment group reports results of a mean comparison t-test of the respective treatment group to the control group average.

A-2 Additional Results

Table A-5: Validation of hypothetical task.

	(1)	(2)	(3)
Incentivized allocation task	-0.003	-0.004	0.000
	(0.011)	(0.011)	(0.012)
Mother × Incentivized	-0.008	-0.008	-0.009
	(0.015)	(0.015)	(0.015)
Mother respondent	0.003	0.002	0.001
	(0.011)	(0.011)	(0.011)
$\beta_0 + \beta_1 = 0$	0.249	0.259	0.345
District FE	No	Yes	Yes
Controls	No	No	Yes
R-squared	0.001	0.011	0.052
Observations	1330	1330	1330

Notes: The table reports OLS regression results comparing incentivized and hypothetical task versions. By interacting an indicator for whether the task was completed in the incentivized format with an indicator for whether the participant is the mother of the target child, we validate the task for both fathers (baseline category) and mothers. The model includes district fixed effects and a selection of control variables, which include experimenter fixed effects, the mothers' and fathers' age and education, whether the allocation task was played first, and the target child age (in months) and gender. We additionally control for all unbalanced characteristics, see Appendix Table A-3, including whether the father reports to be in good health, as well as household ownership of agricultural and non-agricultural land. Missing values for the participant age and education, as well as the child's age are imputed with the district specific median value. Standard errors (in parentheses) are clustered on the community level. ***, **, and * indicate significance at the 1,5, and 10% level.

Table A-6: Parental preferences in the KK RCT control group, categories.

	Clothes (1)	Shoes (2)	Food (3)	Toys (4)	Learning (5)	Health (6)
Mother respondent	0.027***	0.003	0.004	0.003	-0.007	0.004
	(0.008)	(0.005)	(0.006)	(0.004)	(0.005)	(0.005)
Father mean	0.101	0.065	0.063	0.031	0.042	0.033
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.149	0.103	0.150	0.169	0.157	0.154
Observations	350	350	350	350	350	350

Notes: The table reports OLS regression results comparing paternal and maternal preferences in the control group of the KK RCT for the second phase of the allocation task. The control variables include experimenter fixed effects, the mother's and father's age and education level, whether the allocation task was played first, and the target child age (in months) and gender. We further control for all additional unbalanced characteristics, see Table 1, including whether the father is employed, whether he reports to be in good health, and whether the mother is the head of household. Missing values for the participant's age and education, as well as the child's age are imputed with the district-specific median value. Standard errors (in parentheses) are clustered on the community level. ***, **, and * indicate significance at the 1,5, and 10% level.

A-3 Experimental Material and Instructions

Table A-7: Pop-up shop item selection.

Category	*Value*	*Item*	*Description*
Male	Low	Razor	Disposable razor used to shave a beard.
		Socks	Pair of socks for men (standard size).
	Medium	Belt	Leather belt worn by men
		Kikoi	Traditional rectangle of woven cloth, usually worn by men as a type of sarong.
	High	Watch	Male watch.
		Wallet	Leather wallet for men.
Female	Low	Sanitary pads	Pack of ten sanitary pads for women.
		Hand mirror	Portable hand mirror.
	Medium	Khanga	Traditional colorful fabric, used as a dress by women in East Africa.
		Watch	Female watch.
	High	Leather handbag	Leather handbag for women.
		Kitambaa	Large piece of cloth usually used by women to sow clothing for herself.
Child	Low	Baby powder	Small bottle of lightly perfumed pure baby powder.
		Bell ringer toy	Small handheld toy with a ringing bell.
	Medium	Towel	Baby towel.
		Sippy cup	Plastic sippy cup for toddlers and babies.
	High	Shawl	Baby blanket/shawl.
		Toy set	A toy set with a variety of building blocks for children to play with.

Notes: Selection of items available in the pop-up shop. Low-value goods were priced at TSh 1,000 to 2,000 per item, medium-value goods between TSh 8,000 and 12,000, and high-value goods between TSh 15,000 and 20,000.

A-4 KK RCT details

The Kizazi Kijacho research program includes a large-scale multi-arm clustered RCT implemented between 2022 and 2025 in 314 communities across the Dodoma region in Tanzania, see Figure A-1 for a timeline and see Figure A-2 for a map of all study communities. The RCT comprised three survey waves: a baseline survey conducted during pregnancy, the first follow-up when the children were approximately 1 year old and the second follow-up when the children averaged 2 years of age. At baseline, 3,580 pregnant women living across 314 communities were recruited to the sample and randomized at the community level into a control group (including 856 households in 81 communities) and several treatment arms: (i) a group of 762 households in 79 communities, who received six bi-monthly unconditional cash transfer of TSh 109,000 (USD 47) starting from pregnancy (UCT group), (ii) a group of 763 households in 73 communities who between pregnancy and age 2 received a parenting intervention (monthly home visits and group sessions) delivered by existing community health workers, which combines the Care for Child Development package by UNICEF and the World Health Organization with elements of the Reach Up parenting program (Grantham-McGregor et al., 1991; Grantham-McGregor and Smith, 2016), tailored to the Tanzanian context (Parenting group), and (iii) a group of 728 households in 77 communities who received both the parenting intervention from pregnancy until approximately age 2 and six bi-monthly UCTs of TSh 77,000 (Parenting+UCT group) between pregnancy and age 1.[9]

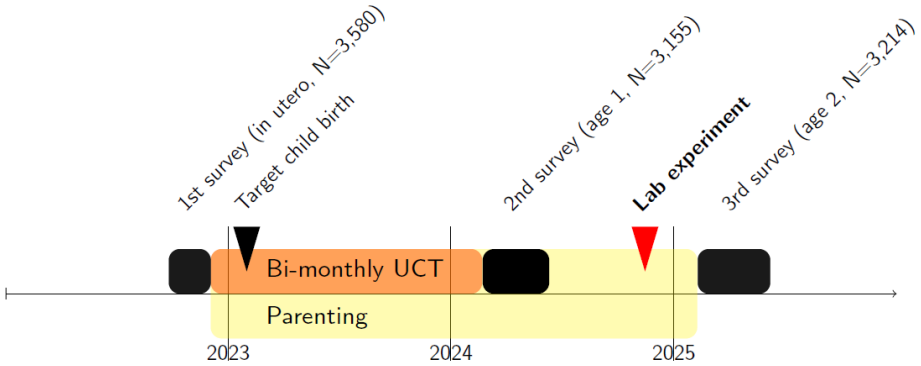


Figure A-1: KK RCT and experiment timeline.

As illustrated in Figure A-1, the treatments officially started when the mothers were still pregnant with the study target children. In the UCT group, the recipient of the transfers was randomized to either be the female (hereafter mother) or male primary caregiver (hereafter father) of the target child while in the Parenting+UCT group, the

[9]The size of the cash transfer in the Parenting+UCT group was calibrated to match the average of maximum and minimum bi-monthly transfer amounts disbursed per family with a child under the Tanzanian Social Action Fund (TASAF), Tanzania's national social protection scheme. In the UCT group, the TSh 109,000 transfer additionally included the estimated bi-monthly cost per child of delivering a parenting intervention. Thereby, both groups have comparable policy costs.

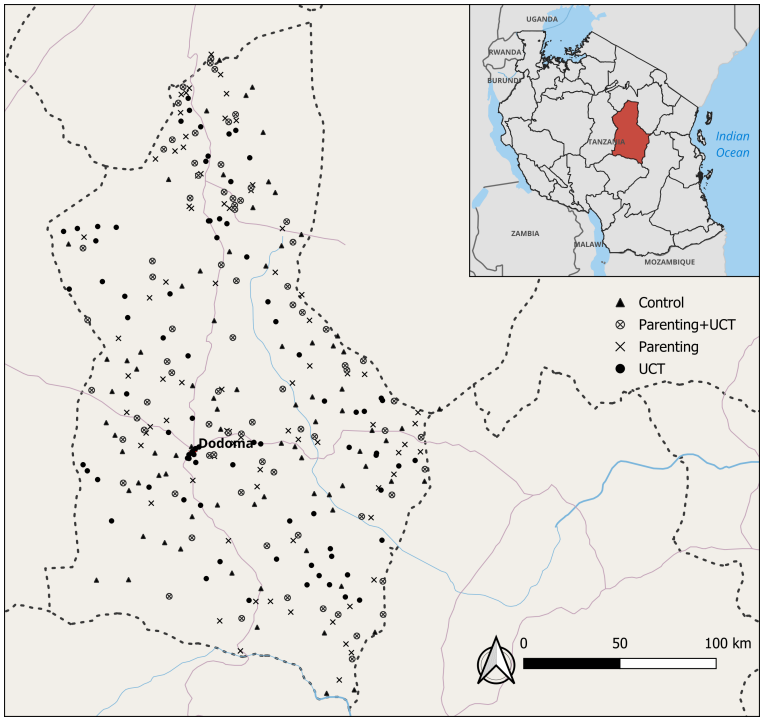


Figure A-2: Map of communities with experimental sessions.

transfer was always targeted to the mother. The bi-monthly cash transfers (six in total) continued until the target children were approximately 1 year old. In contrast, the monthly parenting intervention in both the Parenting and the Parenting+UCT groups ran until the target children were on average 2 years old. The parenting intervention included monthly home visits (starting from pregnancy) and monthly group sessions (starting 6 months after birth) and was generally received by the mother (the primary caregiver) while fathers were encouraged to participate but were not directly targeted.

In the first half of 2024, when the target children were approximately one year old, the first follow-up survey was conducted with 3,155 households. Main reasons for 12% attrition between baseline and first follow-up were (a) migration out of the study region, and (b) the death of the target child. Six to nine months after the first follow-up, these 3,155 households were then invited to participate in the lab experiment, which we detail below. The RCT concluded with a second follow-up survey with 3,214 households in the first half of 2025, when the target child averaged two years of age.[10] For more information about the RCT design and implementation as well as sample characteristics at baseline, see Almås et al. (2023).

Embedding the lab experiment in the KK RCT allows us to study parental preferences for household resource allocation in several different contexts while keeping the

[10]In contrast to the first follow-up survey, in the second follow-up survey households where the target child had passed away after birth were included in the second follow-up.

age of the child and household characteristics comparable. That is, we observe allocation preferences (i) at status quo, (ii) after households have received six bi-monthly UCTs during the first year of the target child's life, (iii) after households have received a parenting intervention from pregnancy until about 22 months after the child's birth, and (iv) after households have received both this UCT and parenting intervention combined. The variation in the transfer recipient in the UCT group additionally allows us to study the effects of targeting the cash transfer on parental spending preferences by both caregivers.