Cash Transfers, Trust, and Inter-household Transfers

EXPERIMENTAL EVIDENCE FROM TANZANIA

David K. Evans and Katrina Kosec

ABSTRACT

Institutionalized conditional cash transfer (CCT) programs may affect important aspects of pre-existing, informal safety nets such as inter-household transfers and trust among community members. We use a randomized controlled trial to test the impact of CCTs on various measures of trust and informal safety nets within communities in Tanzania. We find evidence that the introduction of a CCT program increased program beneficiaries' trust in other community members and their perceived ability to access support from other households (e.g., childcare). Although CCTs reduced the total size of transfers to beneficiary households in the community in the short run (after 1.75 years of transfers), that reduction had disappeared 2.75 years after transfers began. Taken together, our evidence suggests that formal CCT programs do not necessarily crowd out informal safety nets in the longer term, and they may in fact boost trust and support across households.

Cash Transfers, Trust, and Inter-household Transfers: Experimental Evidence from Tanzania

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1   Introduction

The transfer of cash and other goods to protect the well-being of households experiencing poverty and adverse shocks extends far back in history, and has occurred largely through informal transfers from household to household. Today, more and more countries are implementing government-administered safety nets like conditional cash transfer (CCT) programs to protect households from extreme poverty.[1] The introduction of CCT programs may reduce the need for households to rely on each other for monetary or non-monetary assistance. However, one concern with this diminishing need is that formal programs may crowd out informal transfers, such that—if formal systems are disrupted or the size of formal transfers proves inadequate to fully insure against shocks—households may be left with insufficient access to consumption smoothing mechanisms and other forms of assistance. At the same time, CCTs may boost beneficiaries' engagement with other community members—potentially increasing access to both informal transfers, other support, and trust in other community members.

In this paper, we evaluate the impact of the randomized introduction of a pilot CCT program in Tanzania in 2010 on trust and informal safety nets across households. We utilize individual- and household-level data from two follow-up surveys to analyze the effects of the program after 1.75 years of transfers and after 2.75 years. We consider both measures of formal and informal transfers into and out of households as well as more subjective measures of trust in and feelings of being supported by other community members, thus providing a relatively nuanced look at the impacts of the program.

We find that trust in community members is unaffected after 1.75 years of transfers, but that it increases after 2.75 years have passed. By that time, beneficiary households report increases in trust both in people in general and in community members, as well as trust in shopkeepers and teachers specifically. Likewise, after 2.75 years, beneficiary households report positive impacts on two non-monetary aspects of informal safety nets, including the

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[1]Some cash transfer programs come with conditions on household behaviors (CCTs) and others do not (unconditional cash transfers), but almost none specifically restrict how money is spent.

perceived ability to access childcare from other households and a greater likelihood that someone from another household has turned to their household for assistance with a personal problem recently.

The impact on total inter-household monetary transfers dips after 1.75 years but then recovers by the time 2.75 years have passed; in other words, by 2.75 years, beneficiary households report receiving a comparable amount in inter-household transfers as they did at baseline. (At the extensive margin, we do see some evidence of an enduring reduction in the proportion of households that report receiving inter-household transfers.) Even in the shorter-term follow up, the apparent crowd-out of total inter-household transfers is small, equal to less than 16 percent of the size of formal government transfers.

These results contribute to the literature in several ways. To our knowledge, this is the first study from Sub-Saharan Africa to examine the relationship between CCTs and either trust in other individuals or informal safety nets (including both transfers and other forms of support). On such outcomes, one might expect very different results across countries and regions due to a variety of factors including variation in baseline levels of trust (Knack and Keefer, 1997). The one other study to examine the relationship between cash transfers and interpersonal trust in the region examines the impact of unconditional cash transfers (in Kenya) and finds no impact on trust (Haushofer and Shapiro, 2016). However, unconditional and CCTs may vary significantly in their impact on relationships between beneficiaries and other members of the community: because of the imposition of conditions, beneficiaries may be disgruntled or resentful toward government and even toward other members of the community (if those community members play a role in targeting). On the other hand, conditions may lead community members to view beneficiaries as more deserving than they would otherwise. Further, the conditions themselves may require beneficiary households to increase engagement with service providers and other community members (e.g., near schools and clinics). That said, unconditional transfers may also practically boost engagement in education and health services by boosting income, which may similarly increase

interaction with other community members.[2] Both types of transfers may involve community meetings (as is the case with the program we evaluate in Tanzania), both to manage the program and to provide information about it, which could also serve to increase community engagement.

The only other evidence on CCTs and something akin to trust in other community members comes from lab-in-the-field evidence in Colombia, where CCT communities showed higher levels of cooperation than non-CCT communities (Attanasio et al., 2009). We also provide novel estimates of impact on trust in particular community members, including teachers and shopkeepers.[3]

While there is a larger literature on the relationship between CCTs and inter-household transfers, it almost entirely stems from Latin America—e.g., Colombia (Garcia and Cuartas, 2021), Mexico (Albarran and Attanasio, 2003; Attanasio and Ríos-Rull, 2000), Honduras, and Nicaragua (Olinto and Nielsen, 2006). This previous work examines the impact of CCTs on informal transfers at a single point in time, whereas we observe an evolving relationship between CCTs and informal transfers over time. Our findings also relate to larger literatures on how formal insurance can crowd out informal insurance—both lab-in-the-field evidence, such as Auriol et al. (2020) and Cecchi et al. (2016), and quasi-experimental evidence, such as Strupat and Klohn (2018)—and how the broader range of social safety nets (including pensions) affect informal transfers Nikolov and Bonci (2020).

2 Experiment

The Tanzania Social Action Fund (TASAF) and World Bank officials conceived the Tanzania Community-Based Conditional Cash Transfer (CB-CCT) pilot program and outlined its

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[2]The evidence on unconditional cash transfers and educational participation is largely but not universally positive, whereas the evidence on health services is weaker (Pega et al., 2022; Haushofer and Shapiro, 2016; Kilburn et al., 2017).

[3]Several studies examine the impact of CCTs on trust in government or other institutions: e.g., in Tanzania (Evans et al., 2019b) and Peru (Camacho, 2014). Kosec and Mo (2022) show impacts of unconditional cash transfers on trust in government and political institutions in Pakistan, with the largest trust improvements from receiving transfers when (for exogenous reasons) citizens feel relatively poor and deprived.

design during November 2007 – September 2008 (see full program timeline in Table A1). TASAF is a government agency tasked with promoting economic and social development, established in 2000. During September – November 2008, TASAF carried out program sensitization at the regional, district, ward, and community levels in preparation for planned enrollment of beneficiaries during the fall of 2009. The program began delivering transfers in January of 2010. Its aims were to increase investments in health for young children (ages 0–5) and the elderly (over 60) and to increase educational investments for children aged 7–15. Eighty eligible communities in three districts (Chamwino, Bagamoyo and Kibaha) were randomized into treatment and control groups of 40 communities each, stratified on district and community size.[4] Randomization was carried out after identification of (potential) beneficiary households in all 80 villages. At village meetings held prior to randomization, TASAF communicated program logistics and benefit amounts to households identified as prospective beneficiaries, explained that half of villages would receive the program by 2010, and noted that the remaining (control) villages would receive the program in late 2012, as well as that the program would continue in treatment villages.[5] As planned, the program was implemented for 2.75 years in treatment communities only, and then extended to all 80 communities after our last round of data collection.

Treatment households received transfers every two months. Transfer amounts were linked to household size and composition. At the time of our first (2011) follow-up survey, the most recent transfer payment received by the median beneficiary household (they received payments every other month) was 22,500 Tanzanian Shillings (TSH).[6] At the time of our second (2012) follow-up survey, the most recent transfer payment received by the median beneficiary household was 30,000 Tanzanian Shillings. Converting both of these amounts to

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[4]The three districts were not randomly selected, but rather selected to be in rural areas proximate to Dar es Salaam (a major port city and the seat of government) and Dodoma (the capital); in particular, Bagamoyo is 70 km from Dar es Salaam, Chamwino is 500 km from Dar es Salaam but 40 km from Dodoma, and Kibaha is 35 km from Dar es Salaam.

[5]We are not aware of how discussions of program longevity were handled, but we are not aware of a date for conclusion of the program being communicated in any villages prior to our endline survey.

[6]Using the average exchange rate in August 2011 (1641.63 TSH per 1 US dollar) (Exchange Rates UK, 2011), this is 13.71 USD.

constant, 2009 TSH,[7] they amount to 18,800 TSH and 21,610 TSH, respectively. At both midline and endline, these equalled about 13 percent of household expenditures during the same two month period.[8]

The program involved a number of individual-level conditions. Healthcare conditions applied only to young children and the elderly. Children aged 0–5 had to visit a public health clinic (for either preventive or curative care) six or more times per year (for children aged 2–5, the condition relaxed to only two visits per year beginning in 2012), while those age 60 or over had to visit at least once per year. Additionally, children aged 7–15 had to stay enrolled in school and maintain an 80 percent attendance record. While we lack precise data on the strictness of enforcement, few households (2–3 percent) reported having experienced transfer reductions due to program non-compliance during the midline and endline surveys.

Elected community management committees (CMCs) in each village worked with TASAF to select prospective beneficiary households before randomization into treatment and control.[9] All communities had, in the past, implemented community development projects with resources from TASAF, and the CMC was responsible for administering those resources. TASAF asked the CMC to survey the poorest half (roughly) of households, collecting data on eight household characteristics that would subsequently enter a proxy means test (PMT): roof material, light supply, water supply, type of toilet, ownership of four different assets (vehicle/motorcycle, radio, iron, poultry), number of windows on the house, household size, and number of meals eaten per day. TASAF carried out the PMT, producing a ranking of households by poverty level, which it then submitted to the CMC and village leader for

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[7]We use consumer price index averages for Tanzania for 2009, 2011, and 2012 from World Bank (2020) to convert these amounts into constant, 2009 TSH.

[8]Statistics cited here use information on the most recent TASAF CCT program transfer to reduce concerns with inaccurate recall (e.g., such as might emerge in asking individuals to total a full year's worth of transfers) and isolate CCT program transfers from other types of government support. Note that in the analyses in the paper (e.g., Tables 1 and 5), however, we utilize amounts of total government transfers (which may include CCT program as well as other transfers from government) received over the previous 12 months— which permits side-by-side comparisons of government transfers with other forms of transfers (individuals and non-governmental organizations, or NGOs) over the same time frame (i.e., 12 months).

[9]CMC elections were convened at village meetings; in program communities, between 10 and 14 members were elected via secret ballot. Those who received financial training and successfully managed a past TASAF-supported project were eligible to run.

approval. Households generally reported high levels of satisfaction with their CMCs.

CMCs existed in both treatment and control communities. In control villages, the CMC continued to exist, but did not play an active role in administering resources during the time of the program. CMC oversight and validation was intended to promote community buy-in. Following beneficiary selection, CMCs in treatment villages continued to screen potential beneficiaries, communicate program conditions, and transfer funds. The CMCs also helped monitor conditions; every four months, they were responsible for collecting monitoring forms from health clinics and schools, updating TASAF records, delivering warnings to those not meeting conditions, making home visits when needed, and conducting regular awareness sessions. After 1.75 years of transfers, over 86 percent of beneficiary households reported a visit to their household by a CMC member since the program began, and only 1.5 percent reported a CMC member asking for part of their transfer.

3 Data and empirical strategy

We evaluate the impacts of the CCT program using three waves of data collected on beneficiaries in treatment communities and would-be beneficiaries in control communities. In total, 1,764 households (883 in treatment communities and 881 in control communities), including a total of 6,918 individuals, participated in a baseline survey in early 2009 (a random subset of households identified as sufficiently poor to receive transfers). No data were collected from households not identified as sufficiently poor. Table A1 presents the chronology of the impact evaluation and the program. Payments began in early 2010, a midline survey was conducted during July–September 2011 (about 1.75 years after transfers began), and an endline survey was conducted during August–October 2012 (about 2.75 years after transfers began).[10]

There is no evidence of differential attrition of either individuals or households across the

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[10]In cases where households split into two, we visited both households. Both households share the treatment status of the original household from which they split.

treatment and control groups in our study (Evans et al., 2019a). This is true for both survey rounds and is robust to various specifications, including those interacting treatment with various individual- and household-level characteristics and checking for the joint significance of those interactions and treatment in predicting attrition. The rate of household attrition was 8.5 percent at midline, and 13.0 percent at endline.[11]

We recover causal intent-to-treat estimates from the following empirical specification:

$$Y_{it} = \beta_0 + \beta_1 2011_t + \beta_2 2012_t + \delta_1 T_i \times 2011_t + \delta_2 T_i \times 2012_t + \alpha_i + \epsilon_{it} \tag{1}$$

where i indexes individuals and t indexes the survey round (baseline in 2009, midline in 2011, or endline in 2012). Y_{it} is an outcome, α_i are individual fixed effects, T_i is an indicator for one's community being assigned to treatment, 2011_t is an indicator for midline, and 2012_t is an indicator for endline. For some outcomes, we only gathered data at endline, and thus our analysis of them utilizes a sample that is roughly one third the size of that used for other outcomes; we regress the outcome on T_i, district fixed effects, a control for logged community population, and a vector of household-level controls measured at baseline.[12] We estimate ordinary least squares regression models, including linear probability models where our outcomes are indicator variables. Standard errors are clustered at the village level, which is the unit of randomization. In addition to estimating effects overall, we also consider effects according to the household's baseline asset wealth level (defining less-poor households as those with above-median asset wealth, and the poorest households as those with below-median asset wealth).

Y_{it} comprises several different outcome variables. First, we consider several measures of

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[11]For households in treatment communities, these figures were 8.3 percent and 11.8 percent, respectively. For households in control communities, they were 8.6 percent and 14.1 percent, respectively. Of the 1,764 households visited at baseline, four declined to be interviewed during the baseline visit; our analysis of attrition by midline and by endline omits these four observations in order to consider attrition after baseline.

[12]These include head age, age squared, gender, education level, and literacy status; dummies for the number of household members; dummies for an improved roof, improved floor, toilet, and piped water; and an asset index that is the first principle component from a principle components analysis using indicators for 13 durable assets.

communal trust, which are in all cases indicator variables. We consider two variables present in all survey rounds: indicators for believing most people can be trusted, and for believing community members can be trusted. We also consider six variables present only at endline. Two are indicators for believing that trust in people in general has improved over the last three years (i.e., since baseline), and for believing that trust in community members has improved over this period. Four are indicators for trusting to a great or very great extent shopkeepers, teachers, nurses and doctors, and strangers. Teachers as well as nurses and doctors would be encountered by beneficiaries owing to the conditions of the CCT program, while shopkeepers and strangers might be encountered as members of the local CMCs elected to manage the program.

Second, we consider three indicator variables capturing access to informal safety nets. These include indicators for knowing someone outside the household willing to provide 15k Tanzanian shillings (TSH) suddenly and willing to provide childcare suddenly, and an indicator for someone with a personal problem having turned to the household for assistance sometime in the past 12 months. Third, we consider both indicator variables for having received various forms of assistance from different places, as well as continuous variables for the amount of assistance received from each source. These places including individuals, government or TASAF, and NGOs, while the sources include cash, food, and other in-kind transfers.

Finally, in addition to incoming transfers, we consider the amount of transfers the household paid out to others. We consider the overall amount, as well as the amounts in cash, food, and other in-kind transfers.

We compare treatment and control households at baseline to ensure comparability on observable characteristics (Table 1). Of 27 variables compared, we observe only a handful of significant differences. Treatment households are six percentage points less likely to have an improved floor and six percentage points less likely to state that people in the community can be trusted. They are also slightly less likely to receive assistance from other individuals,

which is reflected in both the binary (whether or not they received any assistance) and the continuous (how much assistance they received) outcome variables. Because we include household fixed effects, these modest differences at baseline should not affect our estimates.

Table 1: Baseline balance

Outcome	Treatment (T) Mean	N	Control (C) Mean	N	Difference (T-C) Mean	S.E.
Panel A: Household and community characteristics						
Household characteristics						
Dummy - household has improved roof	0.33	880	0.37	878	-0.04	(0.06)
Dummy - household has improved floor	0.03	880	0.09	878	-0.06**	(0.02)
Dummy - household has toilet facilities	0.69	880	0.76	879	-0.07	(0.04)
Dummy - household has piped water	0.30	880	0.32	879	-0.01	(0.08)
Dummy - head of household is male	0.63	879	0.59	878	0.04	(0.03)
Share of households reporting a ... exists in village						
Parent association	0.14	40	0.13	40	0.01	(0.02)
Health committee	0.61	40	0.59	40	0.02	(0.04)
Panel B: Outcomes at baseline						
Trust						
Dummy - leaders can generally be trusted	0.81	878	0.80	873	0.01	(0.03)
Dummy - most people can be trusted	0.26	875	0.23	874	0.03	(0.03)
Dummy - community people can be trusted	0.59	876	0.53	873	0.06*	(0.03)
Dummy - received ... gifts/assistance from individuals in last 12 months						
Dummy - cash	0.24	874	0.28	876	-0.04	(0.03)
Dummy - food	0.21	869	0.27	874	-0.06**	(0.03)
Dummy - other in-kind	0.22	875	0.26	872	-0.04	(0.03)
Dummy - received ... gifts/assistance from NGOs in last 12 months						
Dummy - cash	0.01	879	0.01	879	-0.01	(0.01)
Dummy - food	0.01	880	0.03	879	-0.02	(0.02)
Dummy - other in-kind	0.03	880	0.05	878	-0.02	(0.02)
Dummy - received ... gifts/assistance from government in last 12 months						
Dummy - cash	0.01	880	0.00	879	0.01	(0.01)
Dummy - food	0.02	879	0.02	879	0.00	(0.01)
Dummy - other in-kind	0.03	880	0.02	879	0.01	(0.01)
Amount (1000s constant 2009 TSH) of transfers received in last 12 months from...						
Anyone	18.38	880	25.46	879	-7.08**	(3.44)
Individuals	16.85	880	23.62	879	-6.77*	(3.48)
Government or TASAF	0.83	880	0.46	879	0.37	(0.22)
NGOs	0.70	880	1.38	879	-0.68	(0.49)
Amount (1000s constant 2009 TSH) of transfers in last 12 months given in...						
Any form	1.32	880	1.73	879	-0.41	(0.67)
Cash	0.42	880	0.41	879	0.009	(0.22)
Food	0.48	880	0.66	879	-0.17	(0.25)
Other in-kind	0.41	880	0.66	879	-0.25	(0.50)

Notes: Treatment indicates assignment to treatment. TASAF is the Tanzania Social Action Fund, the government agency responsible for implementing the cash transfer program. Standard errors are clustered at the village level. Outcome variables only available at endline are not shown. Baseline transfer amounts reported (expressed in 1000s of constant, 2009 TSH over the past 12 months) compare with average midline and endline transfers (expressed in 1000s of constant, 2009 TSH) over the past 12 months from government or TASAF in treatment village of 86.52 and 63.22, respectively. *** indicates p<0.01; ** indicates p<0.05; and * indicates p<0.10.

4 Results

First, we examine impacts of government-provided CCTs on trust. Trust can be viewed as a critical requirement for the proper functioning of informal insurance arrangements between households. We find no evidence that receiving CCTs eroded beneficiaries' trust in others after 1.75 years. We observe a statistically insignificant decline of 4.8 percentage points in the likelihood of trusting most people, and a statistically insignificant, 1.3 percentage point increase in the likelihood of trusting community members, as shown in Table 2. However, after an additional year (2.75 years of transfers), we observe a statistically significant (at the 0.05 level), 7.2 percentage point increase in trust of community members (though we find null impacts on trust in most people).

Table 2: Communal Trust

	Dummy - ... can be trusted		Dummy - trust in ... has improved over last 3 years		Dummy - trusts ... to a great/very great extent			
	Most people	Community members	People in general	Community members	Shopkeepers	Teachers	Nurses & doctors	Strangers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment × 2011 (midline)	-0.048	0.013						
	(0.036)	(0.027)						
Treatment × 2012 (endline)	0.034	0.072**	0.032**	0.039***	0.098***	0.053**	0.041	0.029
	(0.041)	(0.035)	(0.015)	(0.012)	(0.033)	(0.025)	(0.028)	(0.021)
2011 (midline)	0.427***	0.664***						
	(0.034)	(0.027)						
2012 (endline)	0.046*	0.633***						
	(0.027)	(0.029)						
R^2	0.198	0.369	0.029	0.030	0.037	0.038	0.035	0.031
Baseline mean	0.242	0.556						
Observations	4920	4924	1570	1570	1570	1570	1570	1570

Source: Authors' calculations based on baseline (2009), midline (2011), and endline (2012) household survey data.
Notes: Columns 1 - 2 include household fixed effects (the number of observations differs slightly due to small numbers of people answering only one of these two questions). Columns 3 - 8 are outcomes only available at endline, and thus these specifications include only one round of data and do not use household fixed effects. Instead, they utilize controls including district fixed effects and household-level controls measured at baseline capturing head age, age squared, gender, education level, and literacy status; dummies for the number of household members; dummies for an improved roof, improved floor, toilet, and piped water; and an asset index that is the first principle component from a principle components analysis using indicators for 13 durable assets. Treatment estimates are estimates of the effect of living in a treatment village (intent to treat). Standard errors are in parentheses and clustered at the village level. *** indicates p<0.01; ** indicates p<0.05; and * indicates p<0.10.

This time path of findings on trust is striking when considered relative to the existing literature on cash transfers and trust. Haushofer and Shapiro (2016) observe inter-household trust after just one year of receiving cash transfers and find no change. Attanasio et al. (2009) compare communities that have received CCTs for more than two years to communities that

have not and find higher levels of social capital in CCT communities. While this relationship may vary across settings for multiple reasons, the fact that we see no change initially but then a positive change later potentially reconciles previous results. If increases in trust among beneficiaries in part stem from greater engagement with members of the community, then that trust could take time (and repeated interactions and engagement) to develop after the initiation of cash transfers.

More detailed data on perceived changes in trust and on trust of particular groups (which are only available in our endline survey) show further potential changes in trust patterns. At endline, individuals were first asked whether their level of trust in people in general, and in community members, increased over the last 3 years (i.e., since baseline). We find that by endline, beneficiary households report significant improvements in trust in both groups. Next, individuals were asked about their level of trust in shopkeepers, teachers, nurses and doctors, and strangers. We coded a set of dummy variables for trusting each group to a great or very great extent. Considering the impacts of treatment at endline, point estimates are in all cases positive, and they are statistically significant at conventional levels for the cases of shopkeepers and teachers. Thus, we see a broad pattern of increased trust in others after the program has been in place for a significant period of time.

These novel findings complement previous analysis from the same project in Tanzania showing that CCTs boost trust in local elected leaders (Evans et al., 2019b). Shopkeepers and teachers are groups that beneficiaries may have greater reason to engage with after receiving CCTs, just as beneficiaries may have more interaction or awareness of elected leaders. This is also consistent with evidence from Peru that CCTs boosted trust in local institutions for beneficiary households (Camacho, 2014).

Second, we examine whether beneficiary households are more likely to report engagement in informal safety nets (Table 3). These include measures of monetary and other support from community members. While households do not report any change in knowing someone from whom they believe they could borrow a significant sum of cash, they do report positive,

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significant impacts on two other measures of informal safety nets. On the receiving end, they are significantly more likely (8.7 percentage points) to report knowing someone who they believe could provide childcare suddenly. On the providing end, they are 4.5 percentage points more likely to have had someone with a personal problem turn to their household for assistance. These findings on household perceptions of access to multiple safety nets are a complement to aforementioned work on social capital in the face of CCTs in Colombia (Attanasio et al., 2009), which relied on lab-in-the-field games. In both cases, CCTs were associated with increased social capital.

Table 3: Informal Safety Nets

	Dummy - knows someone outside the household willing to provide...		
	15k TSH suddenly	Childcare suddenly	Dummy - someone with a personal problem has turned to HH for assistance in the past 12 mos.
	(1)	(2)	(3)
Treatment × 2012 (endline)	0.048	0.087***	0.045**
	(0.030)	(0.032)	(0.022)
R^2	0.055	0.045	0.070
Observations	1570	1570	1570

Source: Authors' calculations based on endline (2012) household survey data.
Notes: TSH means Tanzanian shillings; HH means household. These outcomes were only available at endline, and thus specifications include only one round of data and do not use respondent fixed effects. All specifications use controls including district fixed effects and household-level controls measured at baseline capturing head age, age squared, gender, education level, and literacy status; dummies for the number of household members; dummies for an improved roof, improved floor, toilet, and piped water; and an asset index that is the first principle component from a principle components analysis using indicators for 13 durable assets. Treatment estimates are estimates of the effect of living in a treatment community (intent to treat). Standard errors are in parentheses and clustered at the community level. *** indicates p<0.01; ** indicates p<0.05; and * indicates p<0.10.

Third, we examine monetary and in-kind transfers across households and from other sources (specifically, from government and from NGOs). We observe that at baseline, roughly one in four households reports receiving cash, food, or other in-kind assistance from other individuals (Table 4). In contrast, earlier work from Mexico found that fewer than 6.5 percent of households in control villages were receiving interhousehold transfers (Attanasio and Ríos-Rull, 2000).

We find some evidence of negative impacts on interhousehold transfers on the extensive

margin—i.e., whether or not households receive transfers. Specifically, there is a significant decline of 8.8 percentage points in the likelihood of receiving cash from other individuals after 1.75 years of transfers (at midline), but the coefficient declines in magnitude, to 6.0 percentage points after 2.75 years (at endline), and is only marginally significant. Transfers of food from individuals also decline by 7.2 percentage points at midline, though this reduction in food transfers is smaller in magnitude and statistically insignificant by endline. The only other significant effect in any survey round is our finding that beneficiary households are much more likely to report receiving transfers from the government at both midline and endline, as expected (i.e., households are receiving the formal CCTs they are due to receive). We see no evidence of a change in transfers from NGOs.

Table 4: Assistance Received From Individuals, Government, and NGOs by Type

	Dummy - received gifts/assistance from ... in the past 12 months								
	Individuals			Govt or TASAF			NGOs		
	Cash	Food	Other in-kind	Cash	Food	Other in-kind	Cash	Food	Other in-kind
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment × 2011 (midline)	-0.088**	-0.072**	-0.036	0.878***	0.000	-0.003	-0.002	-0.011	0.016
	(0.035)	(0.032)	(0.036)	(0.017)	(0.009)	(0.016)	(0.007)	(0.018)	(0.013)
Treatment × 2012 (endline)	-0.060*	-0.041	-0.042	0.899***	-0.002	-0.003	-0.003	0.005	0.014
	(0.035)	(0.035)	(0.034)	(0.019)	(0.007)	(0.010)	(0.009)	(0.018)	(0.018)
2011 (midline)	0.348***	0.409***	0.218***	0.009	0.010	0.012	0.006	0.005	-0.026*
	(0.035)	(0.028)	(0.030)	(0.010)	(0.007)	(0.011)	(0.006)	(0.018)	(0.014)
2012 (endline)	0.298***	0.341***	0.152***	0.018	-0.003	0.005	0.009	0.004	-0.020
	(0.031)	(0.028)	(0.031)	(0.011)	(0.007)	(0.008)	(0.008)	(0.015)	(0.017)
R^2	0.105	0.264	0.070	0.806	0.036	0.069	0.047	0.040	0.101
Baseline mean	0.257	0.243	0.242	0.009	0.017	0.023	0.010	0.020	0.039
N	4953	4955	4951	4953	4954	4955	4954	4955	4954

Source: Authors' calculations based on baseline (2009), midline (2011), and endline (2012) household survey data.
Notes: All specifications include household fixed effects. Treatment estimates are estimates of the effect of living in a treatment village (intent to treat). The number of observations differs slightly across specifications due to small numbers of people with missing data for some and not other transfer types. TASAF is the Tanzania Social Action Fund, the government agency responsible for implementing the cash transfer program. Standard errors are in parentheses and clustered at the village level. *** indicates p<0.01; ** indicates p<0.05; and * indicates p<0.10.

On the intensive margin, considering the amount of transfers received (here we include in the regression those receiving transfers of zero), households report somewhat smaller transfers from other individuals after 1.75 years of the program (measured in thousands of constant, 2009 TSH, where the cash value of in-kind transfers is added to the value of any cash received). However, there is no such crowd out of transfers from individuals after 2.75 years

(Table 5).[13] Even after 1.75 years, the crowd-out is modest: the reduction in transfers from individuals is under 16 percent of the value of the increase in formal government transfers. Specifically, 13,799 constant 2009 TSH (the reduction in transfers from individuals) is just under 16 percent of the additional 86,280 constant 2009 TSH received from government or TASAF.[14]

At the same time, households are no more likely to provide assistance to other households; Table 6 considers as outcomes the value (measured in thousands of constant, 2009 TSH) of assistance given to others in cash, food, other in-kind, and the total of the three, revealing null impacts of treatment on all four outcomes for both follow-up survey rounds. This lack of an increase on transfers out of beneficiary households is distinct from recent findings in Bangladesh, where ultra-poor households that receive an unconditional asset transfer were more likely both to give and to receive transfers (Gulesci, 2020). Both our results on incoming and outgoing transfers hold if we winsorize the top one percent of observations to account for outliers in transfer amounts (see Tables A2 and A3, respectively).

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[13]We use consumer price index averages for Tanzania for 2009, 2011, and 2012 from World Bank (2020) to convert all transfer amounts into constant, 2009 TSH.

[14]Table 5 highlights a reduction in government transfers from the CCT program between midline and endline (i.e., living in a treatment village provides an additional 86,280 constant 2009 TSH in government transfers at midline, but a more modest, 63,150 constant 2009 TSH increase at endline.) There are several possible reasons for this. First, at midline, households receiving some government transfers had received such transfers more times than had households at endline—possibly indicating that the midline took place right after receipt of a transfer payment while the endline took place right before it. Second, when households split, we tracked all such households and retained all in our sample—meaning that the share of households in treatment villages actually receiving transfers declined over time. Finally, given this outcome captures all types of government assistance, it is possible that by endline, CCT program transfers had crowded out other forms of government assistance to a greater extent than they did at midline (when the program was still relatively new). Regardless, level differences over time should not affect our treatment estimates, given our inclusion of year fixed effects.

Table 5: Amount of Assistance Received From Individuals, Government, and NGOs

		Total transfers from ... in the past 12 months		
	Total	Individuals	Govt or TASAF	NGOs
	(1)	(2)	(3)	(4)
Treatment × 2011 (midline)	71.72***	-13.79***	86.28***	-0.77
	(5.950)	(4.230)	(4.216)	(0.587)
Treatment × 2012 (endline)	58.68***	-3.82	63.15***	-0.47
	(5.892)	(5.121)	(2.626)	(0.803)
2011 (midline)	34.50***	33.52***	-0.07	1.05**
	(4.155)	(3.795)	(1.423)	(0.486)
2012 (endline)	28.06***	27.28***	0.17	0.77
	(4.330)	(4.217)	(1.121)	(0.658)
R^2	0.304	0.215	0.398	0.096
Baseline mean	21.915	20.229	0.648	1.038
Observations	4955	4955	4953	4955

Source: Authors' calculations based on baseline (2009), midline (2011), and endline (2012) household survey data.

Notes: Transfer amounts are measured in thousands of constant 2009 Tanzanian Shillings (TSH). All specifications include household fixed effects. Treatment estimates are estimates of the effect of living in a treatment village (intent to treat). The number of observations differs slightly across specifications due to small numbers of people with missing data for some and not other transfer types. TASAF is the Tanzania Social Action Fund, the government agency responsible for implementing the cash transfer program. Standard errors are in parentheses and clustered at the village level. *** indicates p<0.01; ** indicates p<0.05; and * indicates p<0.10.

That the CCT program had an enduring impact (at midline and endline) on the likelihood of receiving transfers from other individuals at all (extensive margin results of Table 4), but had no impact on the amount of transfers received from other individuals (Table 5) suggests nuanced impacts on individuals' transfer-providing behavior. It suggests no enduring crowd-out (after all, by endline, program beneficiaries collectively receive the same amount of transfers from other individuals on average), but greater likelihoods both of individuals receiving zero and of receiving higher than pre-program individual transfer amounts. This could be consistent with better targeting of informal transfers by other individuals: for example, if the program shed greater light on who are the poorest among the poor during the process of door-to-door visits to complete a poverty census, perhaps individuals began concentrating their transfers amongst the poorest of the beneficiaries, and reducing to zero their transfers to less-poor program beneficiaries. To consider this possibility, we looked for heterogeneous treatment effects according to baseline household asset wealth (Table A4 for the extensive margin and Table A5 for the intensive margin).

We do not find evidence consistent with improved targeting of the poorest of the poor by individuals. Among beneficiaries, the reductions in the likelihood of receiving transfers

from other individuals that decline in magnitude over time are actually driven by the poorest beneficiaries, defined as those with below-median baseline asset wealth. As Table A4 reveals, we identify sizeable, significant negative effects on whether or not the poorest received transfers of cash from other households and near-zero, insignificant effects among the less-poor beneficiaries. (Remember that all recipients were targeted by poverty status, so all are considered to be at least somewhat poor by their communities.) Among the poorest, the reduction in whether or not they received transfers of cash from other households endures through 2.75 years as it did for the overall sample, although the point estimate after 2.75 years is about half its size at 1.75 years. While the finding that the poorest experienced the greatest crowding out is surprising, it is also consistent with evidence from Burkina Faso, where private transfers were most sensitive to income gains (not necessarily from CCTs) among the poorest of the poor (Grimm et al., 2021).

Another possibility is that these findings are due to the distinct social networks of the poorest of the poor compared to less-poor households. Specifically, poorer individuals may have similarly poor individuals within their social network, for whom the utility loss of providing an informal transfer to another household may be proportionately greater (e.g., in the case of a household which can barely afford a minimally sufficient diet). Such households may be precisely those that eliminate their transfers to a household once they learn that household is receiving government support from a CCT program. Meanwhile, households with above-median baseline asset wealth may have slightly better-off households in their social network, which are less likely to react to their receipt of transfers from a CCT program by withdrawing this informal support (as providing informal transfers has a smaller negative impact on the giving household's utility).

While that could explain the shift in whether or not households received transfers, when we consider the amount of transfers received (Table A5), we see statistically significant declines in the amount of transfers from individuals after 1.75 years that are statistically indistinguishable between the poorest and the less-poor. For both the poorest and the less

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poor, we see no declines in transfers from individuals after 2.75 years, suggesting that the crowd out of inter-household transfers on the intensive margin fades out completely over time, regardless of the severity of a household's poverty. By endline, the poorest households may be more likely to face a situation of zero transfers from individuals, but on average they receive about the same amount of transfers from individuals as they did before the CCT program (suggesting that at least some of them observe increases from their pre-program levels of transfers from individuals).

Together, these results suggest at least some changes in transfer behavior that endure to endline. Specifically, for the poorest households, we observe a movement toward more instances of zero transfers alongside more instances of increased transfer amounts, relative to pre-program amounts. However, the results are inconsistent with an enduring crowd-out of total transfers—either overall (Table 5), for the poorest beneficiaries, or for less-poor beneficiaries (Table A5). This lack of an overall crowd-out over time is distinct from previous work from Mexico suggesting that CCTs do crowd out private transfers (Albarran and Attanasio, 2003; Attanasio and Ríos-Rull, 2000). At the same time, evidence from Colombia shows that CCTs actually crowd in private transfers (Garcia and Cuartas, 2021), potentially through enhanced social capital—leading households in the community to be more likely to help beneficiary households in the face of adverse shocks. Our finding of increased trust may point toward a similar phenomenon. One previous piece of evidence may also help to explain our lack of crowd-out findings. Olinto and Nielsen (2006) compare the impact of CCTs on private transfers in Honduras and in Nicaragua. In Nicaragua, the transfers were more than five times as large (relative to household expenditures) as in Honduras; at the same time, the evidence suggests that CCTs crowded out private food transfers in Nicaragua but not in Honduras. In the program we examine in Tanzania, the CCTs are the equivalent of about 13 percent of household expenditures, which falls somewhere between the level in Honduras and in Nicaragua. The relatively low level of transfers in Tanzania combined with high poverty rates may indicate that the CCTs were not sufficient to eliminate informal transfers.

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Considering transfers paid out, we observe no pattern of difference between the poorest and the less poor in whether they provide assistance to other community members; for both groups, there are no impacts of formal CCTs on transfers to other households of any kind (cash, food, or other in-kind), as shown in Table A6.

Table 6: Amount of Assistance Given to Community Members by Type

		Assistance given in the past 12 months in ...		
	Total	Cash	Food	Other in-kind
	(1)	(2)	(3)	(4)
Treatment × 2011 (midline)	0.58	0.16	-0.18	0.60
	(0.682)	(0.230)	(0.435)	(0.425)
Treatment × 2012 (endline)	0.12	-0.03	-0.17	0.21
	(0.631)	(0.284)	(0.373)	(0.295)
2011 (midline)	-0.03	-0.07	0.88*	-0.88
	(0.860)	(0.190)	(0.466)	(0.745)
2012 (endline)	0.59	0.30	1.00***	-0.22
	(0.974)	(0.242)	(0.365)	(0.825)
R^2	0.005	0.003	0.006	0.010
Baseline mean	1.523	0.411	0.572	0.536
Observations	4955	4955	4955	4955

Source: Authors' calculations based on baseline (2009), midline (2011), and endline (2012) household survey data.
Notes: Transfer amounts are measured in thousands of constant 2009 Tanzanian Shillings (TSH). All specifications include household fixed effects. Treatment estimates are estimates of the effect of living in a treatment village (intent to treat). Standard errors are in parentheses and clustered at the village level. *** indicates p<0.01; ** indicates p<0.05; and * indicates p<0.10.

5   Conclusion

We use a randomized controlled trial to test the impact of CCTs on trust, inter-household transfers, and beneficiary perceptions of access to other forms of social support in Tanzania. We observe impacts at two periods of time, which is distinct from most other work in this area. We see no change in trust levels after 1.75 years but we do observe positive changes in trust after 2.75 years, both in community members generally and in specific members of the community, such as shopkeepers and teachers. Beneficiary households also report a higher likelihood of believing they can rely on someone in the community for childcare and a higher likelihood that someone in the community has turned to them for help with a personal problem. We see a short-term dip in total transfers received from other individuals, but by endline, the total transfers have returned to levels that are statistically indistinguishable from

those at baseline. Looking beyond CCTs, the wide variation in results from previous studies on the interactions between formal safety nets and informal transfers (Nikolov and Bonci, 2020) suggests that crowding out will depend on the structure of the program—e.g., the size of transfers, the completeness of coverage, and program duration.

A key limitation of our work is that we do not estimate impacts on non-beneficiaries. Our baseline and follow-up household surveys interviewed only beneficiaries in treatment villages and would-be beneficiaries (selected before randomization of villages into treatment and control) from control villages. The targeting process for the cash transfer program sought to benefit the most vulnerable households in the village, but as these were vulnerable areas in a low-income country, it is likely that there are vulnerable households among non-beneficiary households as well. How cash transfers affect informal transfers to and from both vulnerable and less vulnerable non-beneficiary households is a question for future research. Existing evidence on non-beneficiary households in the context of cash transfer programs suggests negative impacts on subjective well-being (Haushofer et al., 2019) and on trust in community institutions and governance (Camacho, 2014; Kosec and Mo, 2022).

Our findings highlight the value of measuring impacts across multiple time periods, as results may well not be stable over time. They also continue to point to the many impacts that CCTs may have beyond the commonly stated objectives of alleviating extreme poverty and boosting human capital investments. Ultimately, we find little evidence that CCTs erode existing safety nets. On the contrary, our evidence suggests that at least for beneficiaries, engagement with and trust in community members may be enhanced as a result of the program.

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Online Appendix

Table A1: Timeline of CCT program and impact evaluation

Timing	Activity
November 2007 - September 2008	Program design
September - November 2008	Sensitization at regional, district, ward, and community levels
January - May 2009	Baseline survey
September - October 2009	Enrollment of beneficiaries
January 2010	First payments made to beneficiary households
July - September 2011	Midline survey and first round of qualitative data collection
August - October 2012	Endline survey
July - August 2013	Second round of qualitative data collection

Table A2: Amount of Assistance from Community Members, Top 1 Percent of Observations of Outcome Winsorized

		Total transfers from ... in the past 12 months		
	Total	Individuals	Govt or TASAF	NGOs
	(1)	(2)	(3)	(4)
Treatment × 2011 (midline)	72.72***	-10.09***	82.66***	-0.17
	(5.057)	(3.497)	(3.487)	(0.280)
Treatment × 2012 (endline)	57.77***	-3.79	63.21***	-0.20
	(5.015)	(4.125)	(2.627)	(0.340)
2011 (midline)	30.96***	28.52***	0.57	0.10
	(3.291)	(2.850)	(1.269)	(0.268)
2012 (endline)	27.46***	26.03***	0.22	0.23
	(4.223)	(3.848)	(1.116)	(0.288)
R^2	0.397	0.208	0.605	0.164
Baseline mean	20.736	18.615	0.648	0.804
Observations	4955	4955	4953	4955

Source: Authors' calculations based on baseline (2009), midline (2011), and endline (2012) household survey data.
Notes: Transfer amounts are measured in thousands of constant 2009 Tanzanian Shillings (TSH). The highest 1 percent of observations are winsorized. All specifications include household fixed effects. Treatment estimates are estimates of the effect of living in a treatment village (intent to treat). TASAF is the Tanzania Social Action Fund, the government agency responsible for implementing the cash transfer program. Standard errors are in parentheses and clustered at the village level. *** indicates p<0.01; ** indicates p<0.05; and * indicates p<0.10.

Table A3: Amount of Assistance to Community Members, Top 1 Percent of Observations of Outcome Winsorized

		Assistance given in the past 12 months in ...		
	Total	Cash	Food	Other in-kind
	(1)	(2)	(3)	(4)
Treatment × 2011 (midline)	-0.15	0.07	-0.22	0.08
	(0.405)	(0.110)	(0.302)	(0.061)
Treatment × 2012 (endline)	-0.53	-0.04	-0.44	0.11
	(0.402)	(0.132)	(0.269)	(0.074)
2011 (midline)	1.01**	-0.01	0.83***	-0.05
	(0.387)	(0.091)	(0.286)	(0.075)
2012 (endline)	1.31***	0.23*	1.02***	0.06
	(0.418)	(0.123)	(0.250)	(0.083)
R^2	0.011	0.005	0.011	0.010
Baseline mean	0.947	0.238	0.444	0.102
Observations	4955	4955	4955	4955

Source: Authors' calculations based on baseline (2009), midline (2011), and endline (2012) household survey data.
Notes: Transfer amounts are measured in thousands of constant 2009 Tanzanian Shillings (TSH). The highest 1 percent of observations are winsorized. All specifications include household fixed effects. Treatment estimates are estimates of the effect of living in a treatment village (intent to treat). Standard errors are in parentheses and clustered at the village level. *** indicates p<0.01; ** indicates p<0.05; and * indicates p<0.10.

Table A4: Assistance from Community Members, by Baseline Asset Wealth

	Dummy - received gifts/assistance from ... in the past 12 months								
	Individuals			Govt or TASAF			NGOs		
	Cash	Food	Other in-kind	Cash	Food	Other in-kind	Cash	Food	Other in-kind
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment effect for less-poor (midline)	0.019	-0.062	-0.028	0.841***	-0.022*	-0.032**	-0.008	-0.015	0.014
	(0.044)	(0.044)	(0.043)	(0.030)	(0.012)	(0.015)	(0.010)	(0.021)	(0.018)
Treatment effect for less-poor (endline)	-0.019	-0.033	-0.032	0.876***	-0.020**	-0.002	0.003	0.017	0.022
	(0.043)	(0.047)	(0.047)	(0.027)	(0.009)	(0.017)	(0.011)	(0.025)	(0.023)
Treatment effect for poorest (midline)	-0.175	-0.082	-0.042	0.909	0.020	0.023	0.002	-0.007	0.017
	(0.040)***	(0.040)	(0.047)	(0.013)***	(0.013)	(0.023)	(0.008)	(0.018)	(0.016)
Treatment effect for poorest (endline)	-0.090	-0.053	-0.049	0.918	0.015	-0.001	-0.008	-0.006	0.008
	(0.048)*	(0.042)	(0.043)	(0.020)***	(0.012)	(0.013)	(0.013)	(0.016)	(0.020)
Observations	4,953	4,955	4,951	4,953	4,954	4,955	4,954	4,955	4,954
R-squared	0.111	0.266	0.074	0.808	0.063	0.093	0.054	0.045	0.104
p-value of difference (midline)	<0.001	0.703	0.793	0.0271	0.0148	0.0438	0.412	0.634	0.894
p-value of difference (endline)	0.222	0.704	0.766	0.152	0.0337	0.972	0.454	0.265	0.539

Source: Authors' calculations based on baseline (2009), midline (2011), and endline (2012) household survey data.
Notes: All specifications include household fixed effects. Treatment estimates are estimates of the effect of living in a treatment village (intent to treat). Poorest is defined as a household have baseline asset wealth at the median or below, while less-poor is defined as having above-median baseline asset wealth. TASAF is the Tanzania Social Action Fund, the government agency responsible for implementing the cash transfer program. Standard errors are in parentheses and clustered at the village level. *** indicates p<0.01; ** indicates p<0.05; and * indicates p<0.10.

Table A5: Amount of Assistance from Community Members, by Baseline Asset Wealth

		Total transfers from ... in the past 12 months		
	Total	Individuals	Govt or TASAF	NGOs
	(1)	(2)	(3)	(4)
Treatment effect for less-poor (midline)	68.366***	-16.157**	85.104***	-0.576
	(8.847)	(6.262)	(6.071)	(0.921)
Treatment effect for less-poor (endline)	55.301***	-5.897	61.482***	-0.007
	(7.287)	(6.188)	(3.263)	(0.724)
Treatment effect for poorest (midline)	74.51***	-11.74***	87.20***	-0.950
	(6.206)	(4.139)	(4.588)	(0.666)
Treatment effect for poorest (endline)	61.88***	-1.882	64.77***	-0.886
	(7.236)	(6.362)	(3.017)	(1.124)
Observations	4,955	4,955	4,953	4,955
R-squared	0.308	0.221	0.400	0.105
p-value of difference (midline)	0.520	0.504	0.750	0.728
p-value of difference (endline)	0.457	0.600	0.359	0.439

Source: Authors' calculations based on baseline (2009), midline (2011), and endline (2012) household survey data.

Notes: Transfer amounts are measured in thousands of constant 2009 Tanzanian Shillings (TSH). All specifications include household fixed effects. Treatment estimates are estimates of the effect of living in a treatment village (intent to treat). Poorest is defined as a household have baseline asset wealth at the median or below, while less-poor is defined as having above-median baseline asset wealth. TASAF is the Tanzania Social Action Fund, the government agency responsible for implementing the cash transfer program. Standard errors are in parentheses and clustered at the village level. *** indicates $p<0.01$; ** indicates $p<0.05$; and * indicates $p<0.10$.

Table A6: Amount of Assistance to Community Members, by Baseline Asset Wealth

		Assistance given in the past 12 months in ...		
	Total	Cash	Food	Other in-kind
	(1)	(2)	(3)	(4)
Treatment effect for less-poor (midline)	-0.182	0.316	-0.537	0.062
	(1.051)	(0.483)	(0.756)	(0.189)
Treatment effect for less-poor (endline)	0.197	0.342	-0.401	0.198
	(1.178)	(0.548)	(0.680)	(0.419)
Treatment effect for poorest (midline)	1.215	0.0412	0.106	1.049
	(0.899)	(0.152)	(0.335)	(0.768)
Treatment effect for poorest (endline)	0.0309	-0.346	0.00182	0.227
	(0.546)	(0.265)	(0.338)	(0.401)
Observations	4,955	4,955	4,955	4,955
R-squared	0.008	0.004	0.007	0.022
p-value of difference (midline)	0.320	0.599	0.381	0.215
p-value of difference (endline)	0.897	0.271	0.585	0.959

Source: Authors' calculations based on baseline (2009), midline (2011), and endline (2012) household survey data.

Notes: Transfer amounts are measured in thousands of constant 2009 Tanzanian Shillings (TSH). All specifications include household fixed effects. Treatment estimates are estimates of the effect of living in a treatment village (intent to treat). Poorest is defined as a household have baseline asset wealth at the median or below, while less-poor is defined as having above-median baseline asset wealth. Standard errors are in parentheses and clustered at the village level. *** indicates $p<0.01$; ** indicates $p<0.05$; and * indicates $p<0.10$.