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Cash Transfers Increase Trust in Local Government

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Abstract

How does a locally-managed conditional cash transfer program impact trust in government? On the one hand, delivering monetary benefits and increasing interactions with government officials (elected and appointed) may increase trust. On the other hand, imposing paternalistic conditions, leading some to experience feelings of social stigma or guilt, and potentially permitting capture by local elites could reduce trust. This paper answers this question by exploiting the randomized introduction of a locally-managed transfer program in Tanzania in 2010, which included popular election of community management committees to run the program. The analysis reveals that cash transfers can significantly increase trust in leaders. This effect is driven by large increases in trust in elected leaders as opposed to appointed bureaucrats. Perceptions of government responsiveness to citizens' concerns and honesty of leaders also rise; these improvements are largest where there are more village meetings at baseline. One of the central roles of village meetings is to receive and share information with village residents. One indicator that governance may have improved is that records from school and health committees are more readily available in treatment villages. Notably, while the stated willingness of citizens to participate in community development projects rises, actual participation in projects and the likelihood of voting does not. Concerns that local management of a cash transfer program will destroy trust in government or reduce the quality of governance appear unfounded—especially in high-information contexts.

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1 Introduction

How does government provision of social protection impact trust in government, and how does the quality of information available to citizens moderate this relationship? Existing research shows that citizens selectively reward government for providing social protection. While several studies show that its provision increases voter turnout and support for incumbent politicians (Chen, 2013; Linos, 2013; Layton and Smith, 2015; Marschall et al., 2016), Mettler and Stonecash (2008) find that it may lower the likelihood of voting, and Ellis and Faricy (2011) find that public opinion is unaffected by the level of federal social spending.

The very nature of social protection programs makes their net effect on trust in government ambiguous. On the one hand, they deliver monetary and other benefits that should improve livelihoods, and they signal to citizens the value their government places on their welfare (Hunter and Sugiyama, 2014). Social protection programs may also help individuals build stronger social relationships and make them cooperate more (Adato, 2000; Camacho, 2014; Attanasio et al., 2009, 2015). And Camacho (2014) shows that they may increase individuals' exposure to and trust in certain public sector institutions.[1] All of these factors may raise trust in government. On the other hand, many such programs impose paternalistic conditions which could sour state-society relations (Freeland, 2007). Further, participation in them may carry a social stigma (Mettler and Stonecash, 2008; Chong et al., 2009; Camacho, 2014; Oduro, 2015) or otherwise cause social tensions between beneficiaries and non-beneficiaries (Adato, 2000; Adato and Roopnaraine, 2004; Cruces and Rovner, 2008; MacAuslan and Riemenschneider, 2011; Ellis, 2012), which might reduce civic engagement and lower trust in government. Provision of social protection may also be openly politicized, or citizens may fear that it is politicized; this could affect perceptions of government and its fairness (Bruhn, 1996; Dahlberg and Johansson, 2002; Guo, 2009; Costa, 2011; Brollo and Nannicini, 2012; Aytaç, 2014).

The effects of social protection on trust in government are arguably even more am-

biguous in the case of a locally-managed program. In such a context, individuals are more likely to have personal relationships with the individuals overseeing the program—both those making payments and those monitoring compliance with conditions of the program. As Mansuri and Rao (2004) show, community-based and -driven development projects tend to be dominated by elites, and often do not effectively target the poor. If elite capture were to occur, or if personal relationships were to otherwise compromise the integrity of a social protection program's administration, exposure to it could plausibly harm trust in government; it might make salient that politics is elite-dominated or that particular individuals or segments of society have been excluded. Of course, such a program might also be better-administered than expected and thus have the opposite effect on trust.

We contend that a key factor moderating how receipt of social protection affects trust in government is the availability of information—on government decision-making broadly-speaking as well as on the program specifically. We empirically examine both the question of how government provision of social protection impacts trust in government, and how information moderates the relationship, in the context of Tanzania's pilot, community-managed conditional cash transfer program. In 2010, the Government of Tanzania randomized 80 study villages into treatment and control groups of 40 villages each, with control villages to receive the program with a 2.5 year delay. The program conditioned receipt of transfers on child enrollment in and attendance at school and on health clinic visits by both children and the elderly. In all 80 study villages, citizens elected a community management committee (CMC) via secret ballot to select beneficiaries and run the program. In the study context, village meetings play a central role in disseminating information to village residents. We define a high information environment to be one in which there was an above-median number of village assembly meetings prior to program implementation (i.e. four or more per year, which is the number the village council is officially required to hold). While village meetings may have other effects than conveying information, and while variation in meetings is not randomly-assigned, we argue that this is a central goal of meetings, and we show that there

are few statistically significant correlates of meetings.

In high-information environments, we anticipated at least three differences in how a local community-managed social protection program is carried out: First, the process by which beneficiaries were selected should be clearer and more transparent, reducing suspicions among both beneficiaries and non-beneficiaries that beneficiaries were selected for political, clientelistic, or personal rather than poverty-related reasons. This should reduce feelings of guilt among beneficiaries and envy among non-beneficiaries, engender goodwill toward the community, and allay suspicions of government malfeasance. Second, individuals should be more aware of who makes decisions and better understand the duties and obligations of government related to operation of the program and supply of the services on which the program is conditioned—namely, education and health care. This includes knowledge of requirements related to staffing facilities, record-keeping, sharing information with citizens, and responding to citizens' concerns, among others. This information should in theory increase citizens' expectations of government throughout the time the program is in place, and this should in turn put pressure on government (especially elected as opposed to appointed leaders) to live up to these higher expectations. Finally, individuals should better understand how they themselves can influence the program's implementation—specifically, through voting and attendance at village meetings.

We find that cash transfers can significantly increase trust in leaders. This effect is driven by large increases in trust in elected leaders as opposed to appointed bureaucrats. Perceptions of government responsiveness to citizens' concerns and honesty of leaders also rise, and these improvements are largest where there are more village meetings at baseline. One indicator that governance may have actually improved as a result of the CCT program is that records from school and health committees are more readily available in treatment villages. However, the availability of records unrelated to the conditions of the program—including finance/ planning committee and defense/ security committee records—did not change, suggesting that improved government record-keeping may be restricted to items

directly related to the program. Notably, while trust among villagers increases and stated willingness to participate in community projects rises, actual participation in community projects and voting remain unchanged. Overall, concerns that local management of a cash transfer program will destroy trust in government or reduce the quality of governance appear unfounded—especially in high-information contexts.

The remainder of the paper is organized as follows: Section 2 provides background information on local governance in Tanzania, as well as the pilot CCT program whose impacts we evaluate. Section 3 describes the evaluation design, data, and outcomes of interest. Section 4 presents our empirical specification and balance tables showing the outcome of our randomization. Section 5 characterizes our main empirical results. Section 6 concludes.

2 Background

2.1 Local government in rural Tanzania and trust

The importance of trust in local leadership is partly driven by the key role local leaders play in community development. Figure 1 illustrates the structure of government in Tanzania. As of 2012, the country was organized into 30 regions comprised of 169 districts. Each district is comprised of a set of wards which are then further sub-divided into villages. In Tanzania, each village has a Village Council, 15 to 25 members in size, which is elected by the Village Assembly (all residents at least 18 years old) every five years, one year prior to the general election. At least a quarter of Village Council members seats are reserved for women (Simonen, 2010). The Village Council is overseen by a Village Chairman, who is also elected by the Village Assembly every five years. The Village Chairman coordinates with district officials, manages local development projects, and presides over village meetings. Each village also has a Village Executive Officer (VEO) who is responsible for supervising village developmental activities, maintaining law and order, and often serving as non-voting secretary for the Village Council. The VEO is not elected, but is appointed by the District

Executive Director (REPOA, 2008). Decisions about development planning and budgeting are made at the district level (Venugopal and Yilmaz, 2010).

Despite the importance of local policy makers in development, recent Afrobarometer surveys suggest that respondents in surveyed countries in Africa as a whole, in surveyed countries in East Africa, in Tanzania, as well as in our three study regions in Tanzania have less trust in local government councils than they do in parliament/ national assemblies or in the president (Figure 2). In Tanzania, while more than 80 percent of respondents interviewed during 2005–2016 reported trust in national leadership, the comparable figure for local leadership was under 75 percent (Afrobarometer, 2005, 2008, 2012, 2016).[2] Statistics for our three study regions in Tanzania are similar to those for Tanzania as a whole, though trust in local government is slightly lower and trust in national leadership is slightly higher. Given increasing pressure around the world for governments to decentralize, or devolve authority to lower levels of government (Gadenne and Singhal, 2014), it is increasingly important to understand what drives trust in local government, and how trust might be raised. Our focus on village meetings as a source of information is rooted in their centrality in Tanzania as a means of keeping citizens informed and enabling them to participate in and influence policy making. In rural villages, these meetings and the Village Council are the two central institutions of governance (Venugopal and Yilmaz, 2010). Village meetings should, by law, be called every 3 months, and the full Village Assembly is invited to attend.[3] The two key figures in calling village assembly meetings are the Village Chairperson, who is elected, and the Village Executive Office, who is a bureaucrat appointed by the district. Meetings are advertised both by formal means, such as posting a notice or sending someone from house to house, and by local/ informal means, such as signaling with drums the night before a meeting. Miguel (2004) notes that "village meetings are held for local elections, to discuss development projects, and to disseminate information from higher levels of government (for example, to promote HIV/AIDS awareness), and they serve as the focal point for local politics." Village leaders also report on financial resources gathered from village residents

and how those have been used.

The development literature places substantial weight on participatory democracy; for example, Verba et al. (1995) finds that village meetings may improve governance by providing a forum for citizens to scrutinize their elected representatives. Chattopadhyay and Duflo (2004) find that village leaders invest more in infrastructure directly relevant to the needs of their own gender. And Duflo and Topalova (2004) show that reserving a portion of village council head positions for women leads to more drinking water facilities in the village.

In rural Tanzania, the evidence is mixed on the role of village meetings. Citizens often use village meetings to inform leaders about the state of local government performance and complain when it is unsatisfactory (Lawson and Rakner, 2005). However, the Village Assembly does not have legislative or executive powers, and several studies of rural Tanzania have found that the "grassroots" contribution of the Village Assembly to village decision-making is minimal and ineffective REPOA (2007); Norman and Massoi (2010).

2.2 Pilot CCT program

Tanzania's pilot CCT program, implemented by the Tanzania Social Action Fund (TASAF, a social fund agency of the Tanzanian government), began delivering transfers in January 2010. Its aims were to increase investments in health for young children (ages 0–5) and the elderly (ages 60 and over) and to increase educational investments for children aged 7–15. It operated in three districts—Bagamoyo (70 km from Dar es Salaam), Chamwino (500 km), and Kibaha (35 km)—where 80 eligible study villages were randomized into treatment and control groups of 40 villages each, stratified on village size and district.[4] Randomization was carried out after identification of potential beneficiary households in all 80 villages. At village meetings held prior to randomization, TASAF communicated that control villages would receive the program in late 2012, and the program would continue in treatment villages. Median village size was quite small (560 households at baseline, in 2009), and every village had both a primary school and a public dispensary or health center, facilitating fulfillment

of program conditions.

Treatment households received transfers every two months. Transfer amounts ranged from US $12 to US $36, depending on household size and composition. The CCT provided US $3 per month for orphans and vulnerable children up to age 15 (approximately 50 percent of the food poverty line) and US $6 per month for vulnerable individuals age 60 or older. In our follow-up surveys, the median size of the last transfer is US $14.12; assuming six annual payments of this size, this is about 13 percent of annual household expenditures.

While CCT payments were made at the household level, conditions applied at the individual level.[5] Children aged 0–5 had to visit a health clinic at least six times per year (the condition was relaxed for children aged 2–5 to two visits per year starting in 2012),[6] those age 60 or over had to visit at least once per year, and no health conditions applied to others. Both preventive and curative visits fulfilled the health clinic visit conditions of the program, though visits had to be to a public facility (either a dispensary, health center, or hospital). There were no further restrictions on the timing of visits, nor on the services to be received. Children aged 7–15 had to enroll in school and maintain an 80 percent attendance record.

TASAF worked with an elected community management committee (CMC) in each village to select beneficiary households.[7] The CMC surveyed the poorest half of households, collecting data on eight household characteristics: 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 then carried out a proxy means test to propose a ranking of households by poverty level, for CMC and village leader approval. On average, 23 percent of households became beneficiaries.[8]

This oversight and validation helped 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 played a key role in monitoring conditions; they were responsible for collecting monitoring forms from health clinics and

schools, updating records, delivering warnings when conditions were not met, making home visits to stay abreast of developments in beneficiary households, and conducting regular awareness sessions. A year and a half into the program, over 86 percent of beneficiary households reported that a member of the CMC had visited their household since the program began, and only 1.5 percent reported being asked for part of their transfer.

CMCs existed in both treatment and control communities. All communities had, in the past, implemented community development projects with resources from TASAF, and the CMC was responsable for administering those resources. In control villages, the CMC continued to exist, but did not play an active role in administering resources during the time of the conditional cash transfer program.

3 Evaluation Design and Data

3.1 Evaluation design

We evaluate the impacts of the CCT program using three waves of data on beneficiaries and would-be beneficiaries (no data were collected from those not selected to be beneficiaries). Table 1 presents the chronology of the program and impact evaluation. A baseline survey was carried out during January–May 2009 and payments began in January 2010. A midline survey was conducted during July–September 2011 (18-21 months after transfers began) and an endline survey was conducted during August–October 2012 (31–34 months after transfers began).[9] The baseline survey included 1,764 households (a subset of beneficiary households) comprised of 6,918 individuals. The quantitative data collection was supplemented by two rounds of qualitative data collection (following the midline and endline surveys) employing focus group discussions and in-depth interviews.

3.2 Data and outcomes

We collected information on five broad outcomes. First, we wanted to learn about trust in leaders. In each of thee three survey rounds, we collected data from the household head[10] on whether leaders can generally be trusted. At endline, we delved further into this outcome by asking whether the head's trust in village leaders had improved over the last three years. We additionally asked at endline about trust levels on a scale from 1 to 5 for four leaders/ groups: the village executive officer (VEO), the village chairman (VC), the village council, and the community management committee (CMC). Respondents were informed that on this scale, 1 meant trusting to a very small extent and 5 means trusting to a very great extent. From these ratings we constructed dummy variables for trusting the leader/ group to a great or very great extent.

Second, we wanted to learn about perceptions that leaders are doing their jobs correctly—that is, that they are responsive, honest, and hard-working. Our outcomes include a dummy for household heads believing that local government and leaders take citizens' concerns into account "a lot," a dummy for believing that honesty of local government and leaders has improved in the last three years, a dummy for being somewhat or very satisfied with the work of the village council, and for being very satisfied with the work of the village council. We further considered dummies for the individual considering village school and health facilities to be "good or excellent."

Third, we wanted to know about the quality of government record-keeping and transparency. These outcomes were only collected at endline. They include dummies for leaders being able to show reports for four types of village committees: finance and planning, defense and security, school, and health; a dummy for the household head reporting that the village council publicly posts information on projects and finances (i.e., how much money the village council receives and spends, and what they accomplish); and another dummy for that information on projects and finances being freely available.

Fourth, we collected data on voting and civic engagement. Specifically, we asked if the

individual voted in the 2012 CMC election, and if they voted in the most recent village council election.[11] We also coded a dummy for the head having attended a village assembly or village council meeting within the last 12 months.

Fifth, we looked at outcomes related to community development and involvement of the household head. Specifically, we coded dummies for willingness to contribute time to communal projects that do not directly benefit the household, for committing money to such projects, for having worked with fellow villagers for the benefit of the community within the last year, and for having contributed labor to a community development project. We also coded a dummy for whether the household head indicated participating in more civic groups than they did three years ago. Finally, we used village-level data from all three survey rounds on whether there was a parent association and a health committee operating in the village.

4 Methods and Empirical Strategy

4.1 Empirical specification

We carried out follow-up surveys in 2011 and in 2012 to capture both short-term (1.5 years) and medium-term (2.5 years) impacts of the program. Given random assignment to treatment, we recover causal intent-to-treat estimates from the following empirical specification:

$$c_{it} = \beta_0 + \beta_1 2011_t + \beta_2 2012_t + \delta_1 T_i \times 2011_t + \delta_2 T_i \times 2012_t + \alpha_i + \epsilon_{it} \quad (1)$$

where i indexes individuals and t indexes the survey round. c_{it} is a trust-related outcome, α_i are individual fixed effects, T_i=1 in a village assigned to treatment and zero otherwise, 2011_t=1 at the time of the midline survey (July–September 2011) and zero otherwise, and 2012_t=1 at the time of the endline (August–October 2012) and zero otherwise. When we consider a household-level outcome, i instead indexes households.

For outcomes that were present only in the endline surevey, we obtain intent-to-treat

estimates from the following empirical specification:

$$c_{id} = \theta_0 + \theta_1 W_i + \gamma_1 T_i + \gamma_2 T_i \times 2012_t + \mu_d + u_{id} \tag{2}$$

where μ_d are district fixed effects and W_i is a vector of household- and village-level controls including the head's age, age^2, sex, and education level; dummies for household size, having an improved roof, having an improved toilet, having an improved floor, and having piped water; the 2009 village population; and the first principal component from a principal components analysis (PCA) using information on ownership of 13 household assets at baseline. In treatment villages, 9.0% of households did not receive treatment—likely due to last-minute changes in community prioritization or household refusal. In control villages, 0.6% of households received treatment—likely due to their proximity to a treatment village. As a result, our intent-to-treat estimates represent a lower bound on the actual impact of receiving transfers.

4.2 Heterogeneous treatment effects examined

We estimate the overall impacts of the CCT program as well as its heterogeneous impacts by the baseline (2009) number of village meetings. We divide villages into two types: those with above-median and below-median 2009 meetings.[12] Villages with below-median 2009 meetings held at least four meetings during the year—the number they are supposed to hold, given that government intends to hold meetings quarterly. This provides insight into how one measure of the quality of the information environment could moderate the effects of a cash transfer program on trust and rural governance more broadly.

4.3 Outcome of the randomization

Despite randomization of villages into treatment and control, it is possible that some observable characteristics of treatment villages are significantly different than those of control

villages. If this were the case, one would worry that treatment is correlated with observed and unobserved omitted variables. We address this concern in two ways. First, we show that randomization generally led to balance between treatment and control villages. Second, we use household fixed effects to account for baseline imbalances whenever possible.

In Table 2, Panel A, we examine differences in baseline means between treatment and control groups for an array of demographic and housing characteristics. Households in the treatment and control groups are balanced on the head's age, sex, and education level, and the presence of an improved roof, access to toilet facilities, and access to piped water. The only significant difference is that treatment households are less likely to have an improved floor (significant at the 5 percent level). A comparison of baseline sample means in treatment and control villages reveals balance on most of our outcomes as well (Table 2, Panel B). Across 23 outcomes available at the time of the baseline survey, for only four are there significant differences at the 10 percent level between the treatment and control groups. For outcomes only available at endline, we show descriptive statistics in Table 3. Differences across groups at endline obviously do not inform us about the state of balance at baseline. We posit that the broad balance we observe across baseline characteristics – including trust in leaders – is likely to extend to these other measures, but this is ultimately untestable, and we rely on the randomized nature of the program and controls for identification.

4.4 Attrition

Between baseline and midline, 8.6 percent of households attrited from the sample, and between baseline and endline, 13.2 percent of households attrited. Evans et al. (2017) show that this attrition is not correlated with treatment or with a variety of household covariates interacted with treatment. F-statistics for the joint significance of the treatment dummy and these interaction terms indicate that these coefficients are never jointly significant. Overall, we conclude that attrition does not affect the internal validity of our results.

5 Results

In this section, we present the impacts of Tanzania's CCT program on a variety of trust-related outcomes. First, we consider the impacts on trust in and satisfaction with village leaders. Second, we examine the impacts on village and household level measures of local government transparency and record-keeping. Third, we consider impacts on voting behavior and civic engagement. Fourth, we examine how treatment affected voting and civic engagement. Fifth, we consider impacts on community development and involvement. Finally, we explore the robustness of our results to corrections for multiple hypothesis testing.

5.1 Trust in leaders

In Table 4, we document the impact of the CCT on self-reported trust in village leaders. At midline (1.5 years after treatment began), treatment is associated with a 5.2 percentage point increase in the share of households reporting that leaders can generally be trusted. At endline (2.5 years after treatment began), this effect is of a similar magnitude but is more statistically significant. These treatment effects represent about a 6.5 percent increase over the baseline mean of 0.81. Furthermore, households in treatment villages at endline were 4 percentage points more likely to report a belief that trust in leaders had improved over the last 3 years, i.e., since the program began. The coefficients on the time trends in column 1 help us understand from where these effects on trust in leaders stem: an increase in trust in treatment villages, or a decrease in trust in control villages. We see that in control villages, trust in leaders had dropped by a statistically insignificant 0.027 percentage points by midline, and by a statistically significant 0.052 percentage points by endline; this represents a 3.4 percent drop in trust levels in control villages between baseline and midline (i.e. over 1.5 years) and a 6.5 percent drop between baseline and endline (i.e. over 2.5 years). Comparing these estimates to the coefficients on treatment interacted with the midline and endline dummies, we see that trust increased modestly in treatment villages at midline (0.052

- 0.027 = 0.025 percentage points), and increased minimally in treatment villages at endline (0.055 - 0.052 = 0.003 percentage points). Thus, the CCT—especially by endline—largely prevented the erosion of trust seen in control villages.

Absent a general erosion of trust in Tanzania between baseline and endline, one might worry that reductions in trust in control villages are due to a "sour grapes" effect, whereby those who had hoped to received transfers were disappointed when they ultimately did not. If this were the case, cash transfers might not actually increase trust at all; learning about a program and then not getting it could sap trust. Our evidence suggests that this did not take place here: Afrobarometer data on Tanzania surrounding our study period show a trend of reduced trust in all types of leaders during 2008–2012—the president, parliament, and local government—that is similarly-sloped to the decrease in trust in village leaders we observe in control villages during 2009–2012 (Afrobarometer, 2008, 2012).[13] This is consistent with a negative secular trend in trust in leaders in Tanzania that similarly affected our study villages, rather than a "sour grapes" effect due to the CCT program itself.[14] Figure 3 illustrates these trends. Data on trust in the VEO, village chairman, village council, and community management committee (CMC) were only collected as a part of the endline survey. Cross-sectional analysis reveals that treatment is positively associated with each outcome, although the effect of treatment is not statistically significant for the VEO and village chairman.[15] Notably, the VEO is the one leader listed who is not locally elected, but rather appointed by the district. Treatment is associated with an 8 percentage point increase in trusting the village council to a great or very great extent and a 26 percentage point increase in trusting the CMC to a great or very great extent. The latter effect is unsurprising, given the role of the CMC in distributing the cash transfers to beneficiary households.

Panel B shows that the impact of the program on trust in leaders, at least at endline, is entirely concentrated in villages that had more village meetings at baseline. For villages with more meetings, general trust in leaders was 7 percentage points higher and they were

7 percentage points more likely to report an improvement in trust during the course of the program. In those villages, as a result of treatment we observe significantly higher trust – relative to control villages that also had more village meetings at baseline – in the village chairman (8 percentage points), the village council (8 percentage points), and the CMC (28 percentage points). Importantly, however, there are no impacts, even in villages with above-median meetings, on support for the VEO – an appointed bureaucrat. For villages with fewer meetings, the only statistically significant effect is on the CMC (again, the distributors of the cash transfers), and every effect has a smaller point estimate (although statistical power is limited, such that only the improvement in trust is statistically different across communities with more vs. fewer meetings at baseline).

5.2 Satisfaction with local government

Table 5 reports the effects of being in the treatment group on satisfaction with the performance of local government. Households receiving treatment are 7 percentage points more likely to report that local government leaders take citizens' concerns into account "a lot" (column 1) and 4 percentage points more likely to say that honesty of local government and leaders has improved over the last three years (column 2). Reinforcing the increased trust in the village council that we saw in Table 4, treatment is associated with a significant increase in being either somewhat (6.5 percentage points) or very satisfied (10 percentage points) with the work of the village council (column 4). On the other hand, households reported higher perceptions of the quality of schools and health facility at midline, but by endline, perceptions had returned to close to baseline levels (columns 5 and 6). This may reflect an initial favorable perception due to increased use and awareness of school and health services (and potentially even higher quality public services), but increased exposure can also lead to changes in reference points when one grows accustomed to the services. As with trust in local leaders, these effects are entirely concentrated in villages with more baseline village meetings (Panel B). For those villages, the association between treatment and all 6 outcomes

is positive and statistically significant at either the 0.05 or 0.01 level. Households report that local government leaders are 13 percentage points more likely to take citizens' concerns into account a lot. Households are 21 percentage points more likely to be very satisfied with the work of the village council. Even the improved perceptions of schools and health facilities persist to endline. Treatment effects for villages with fewer were significantly different from those with more for all but one outcome.

5.3 Government record-keeping

Beyond perceptions, there is some indication that treatment villages keep better records (Table 6). Specifically, treatment is associated with a significantly higher likelihood that the village can show reports on education (24 percentage points) and health (also 24 percentage points) from the two months prior to the the interview (columns 5 and 6). Such reports are important since they make policy making in these domains more transparent and can thus create opportunities for citizens to weigh in on policy decisions. We do not observe the same improvement in record keeping in other areas, such as finance and planning, or defense and security (columns 1 through 4). This may well be a consequence of the fact that the cash transfers are conditioned on health and education, and the communities have the responsibility to monitor those conditions. It could also reflect the fact that, due to the program, citizens had a renewed interest in using the services (since using them is a requirement to receive transfers) and thus greater knowledge and interest in government decisions about these services—pressuring government to release information about schools and health care facilities. A positive spillover benefit of the conditions may thus be improved record keeping in these areas. Households, however, do not report any increased transparency regarding village council finances, on average (columns 7 and 8). (Village council finances do not include the cash transfers, which operate through the CMC.) But households in villages with more meetings were significantly more likely to report that village council finances were publicly posted or freely available than were households in villages with fewer meetings

(Panel B, columns 7 and 8). We did not estimate heterogeneous treatment effects for the village-level specifications (columns 1-6) because there are only 80 observations.

5.4 Voting and civic engagement

The increased trust in village leaders and improved perceptions of their work does not appear to translate to increased political activity. Treatment is not associated with a higher incidence of attending a village assembly or council meeting, nor of voting in the village council election (Table 7, columns 1 and 2). However treatment has a large impact on voting in the last CMC election (column 3). Beneficiary households have a direct financial interest in the CMC, as it administers the cash transfer program. The results on attending village council meetings or voting for the last village council are similar across villages with more and fewer meetings, but the likelihood of voting in the CMC election is twice as high in villages with more meetings (26 versus 13 percentage points), despite being statistically significant for both types of villages (Panel B).

5.5 Community trust and networks

To this point, we have demonstrated increased trust and confidence in village leaders as a result of the CCT. One potential manifestation of that is through political action, which we do not observe. But another manifestation is through increased community action, as village leaders can play a role in encouraging or facilitating community development projects. Table 8 reports the CCT's effect on household participation in community development. Households do report an increased willingness to contribute to community projects (columns 1 and 2), whether through time (5 percentage points) or money (6 percentage points), but we observe no evidence that households translate that willingness into action: households in treatment villages are no more likely to have worked on community development projects in the last year (columns 3 and 4). More households in CCT villages do report the existence of a health committee, but not a parent association, despite the fact that neither was

near-universal at baseline (columns 6 and 7). Households in treatment villages do report a small but highly significant increase in membership in community groups overall (column 5). Across villages with more or fewer meetings, the increased willingness to contribute is concentrated among villages with fewer meetings (Panel B), but there is still no effect on actual labor contribution to community projects in the last year. The increased reported existence of health committees is observed in both types of villages.

5.6 Robustness checks

Because this program tests a wide array of outcomes, several of which were identified after the program was underway, it is important to correct our findings for potential false-positive results resulting from testing multiple hypotheses. One popular method is the Benjamini-Krieger-Yekutieli (BKY) method, which controls for the false discovery rate (Benjamini et al. 2006). In other words, in limits the "expected proportion of rejections are type I errors," or false positives (Anderson 2008).[16] Table 9 shows the results of our analysis. We use two approaches, first grouping the results all together (column 8) and then by table (column 10). In total, we observe 41 statistically significant impacts of the cash transfer program in our main results tables (Tables 4 through 8). When we group all hypotheses together, 19 remain significant; when we group by table, 14 remain significant. There are some results within each table, with the highest concentration of robust results in our findings on trust of leaders (Table 4), the ability of communities to show records (Table 6), and community development and involvement (Table 8). On the whole, our main findings – that conditional cash transfers increase trust in leaders, improve record keeping in areas related to the transfers, and increase reported willingness to participate in community projects – hold, as well as our important "non-findings," that such increased trust does not appear to translate into voting behavior or community action.

We still demonstrate evidence for our heterogeneous treatment effects result – that the trust impacts of the program are concentrated in villages with more meetings at baseline. Of

16 significant effects for villages with more meetings in the main tables, 4 or 5 of those remain significant, depending on the grouping of hypotheses. But those that remain significant still suggest that the effect on leaders being trustworthy, on trusting the village chairman, and on the existence of a village health committee remain.

Regarding our heterogeneous treatment analysis, we test whether or not having more village meetings at baseline may potentially be a proxy for some other village characteristic (Table 10). For example, do smaller, or wealthier, villages have more meetings? We first show that treatment status does not predict more meetings (column 1). We next show (column 2) that across 11 additional village characteristics, only one—average household consumption—is a statistically significant correlate of baseline meetings. However, we see no relationship between household wealth and the number of village meetings, as measured by an asset index. The lack of statistical significance on the asset index is noteworthy; asset ownership and other measures of socioeconomic status are often used instead of consumption when measuring poverty (Sahn and Stifel, 2000; Morris et al., 2000; Filmer and Pritchett, 2001; Ferguson et al., 2003; Bader et al., 2017). We also see no significant relationship between the share of household heads with formal education and village meetings. Arguably, asset ownership and formal education are better indicators of long-term wealth than consumption, which fluctuates more rapidly over time. Thus, we observe little evidence that additional meetings are merely a stand-in for other village characteristics (including wealth), as opposed to an opportunity to share information with village residents and allow them to voice opinions and preferences.

6 Conclusion

This paper provides evidence that, after 2.5 years, Tanzania's conditional cash transfer (CCT) program increased trust in leaders. This effect is driven by large increases in trust in elected leaders as opposed to bureaucrats. Perceptions of government responsiveness to

citizens' concerns and honesty of leaders also rise. The results are strongest in communities with more village meetings at baseline. One indicator that governance may have actually improved is that records from school and health committees are more readily available in treatment villages. Notably, while stated willingness of citizens to participate in community development projects rose, actual participation in projects and the likelihood of voting remained unchanged.

Overall, our study has at least two key lessons. First, concerns that local management of a cash transfer program will destroy trust in government or reduce the quality of governance appear unfounded—especially in high-information contexts. Second, while the difference in treatment effects according to baseline village meetings cannot be causally attributed to the meetings, we provide at least some support to the argument that having information available to citizens can improve the impacts of a community-driven development project (in this case a conditional cash transfer program) on community trust.

Unfortunately, there are several questions that we are unable to answer with this survey. We are unable to clarify the mechanism through which this CCT program operates. The results could be driven by something as simple as a small reduction in poverty (Bastagli et al., 2016) and improved health (Evans et al., 2017). Or perhaps simply being given what is essentially a gift changes one's outlook. Due to the survey design[17] we cannot measure the impact of this CCT program on nonbeneficiaries within a treatment village, and it is possible that those effects are negative (Adato, 2000; Adato and Roopnaraine, 2004; Haushofer et al., 2015). However, we do not observe retaliation towards beneficiaries: the program actually improved safety nets for beneficiaries and we find no evidence of a significant reduction in transfers that beneficiaries receive from other households.

We believe that this paper contributes to a better understanding of some of the less obvious effects of CCTs. More research is needed to understand how nonbeneficiaries are impacted, but absent negative effects for that group, community trust should be added to the list of areas that have been shown to benefit from a tiny investment in the neediest.

Notes

[1]Specifically, Peru's Juntos CCT increased trust in institutions related to the conditions of Juntos: the national office in charge of identity registration, the Ministry of Health, and the Ministry of Education. However, it decreased trust in the institution that handled the complaints related to program targeting.

[2]It is worth noting that trust in all levels of government is higher in Tanzania than in either East Africa or Africa as a whole.

[3]A recent sample of 29 villages in northern Tanzania suggests significant variation in household attendance of the village assembly. The share of households that had participated in a village assembly in the past 12 months ranged from 8 to 75 percent, and the average was 34 percent.

[4]At baseline, villages ranged from 64 to 10,078 households. The average size was 980 households, and the median size was 560 households

[5]We lack administrative data on compliance with conditions. However, in each follow-up survey, we asked: "[For your last transfer payment,] did you receive less money than you usually get?" and "What do you think was the reason?" While one may hesitate to admit to non-compliance (for fear of sanction) and while this cannot tell us how many households had at least one payment reduced (it only tells us about the last), this gives some indication of compliance levels. At midline and endline respectively, 1.9 and 3.0 percent of treatment households reported receiving less than usual for a reason related to not meeting conditions.

[6]As our endline survey was carried out during August–October 2012, we define compliance with clinic visit conditions at endline for 2–5 year olds as having two or more clinic visits in the last year.

[7]CMC elections occurred at village meetings; 10–14 members were elected, with secret ballots. To run, a candidate had to have received financial training and successfully managed a past TASAF-supported project.

[8]Households that have a blood relative on the CMC do not seem to receive beneficial treatment. The difference in both the reported number of payments received and the reported amount of last payment is statistically insignificant.

[9]Household members who migrate were tracked if they migrated within the village and were less than 19 years old or at least 60 years old. If the member migrated outside the village, the enumerator's supervisor decided whether tracking was feasible — for example, potentially feasible if the member migrated to a neighboring village or an area where future enumeration was planned.

[10]At baseline, 61% of households had male heads and their average age was 68 years.

[11]For the case of CMC elections, we consider only individuals in the 72 villages of our sample of 80 that had already held their 2012 CMC election; as we enumerated around the timing of the CMC elections, 5 villages had not yet held their election. Further, we have missing data on the timing of the last election for

3 villages.

[12]36 of the 80 villages had three or fewer meetings while the remaining 44 had at least four meetings.

[13]Afrobarometer data on Tanzania are not available for 2008.

[14]Specifically, the percentage of Tanzanians saying they trust the president, parliament, and the local government council either "somewhat" or "a lot" dropped from 88.6, 84.1, and 75.8 in 2008 to 74.2, 77.0, and 67.5 in 2012, respectively (Afrobarometer, 2008, 2012). In comparison, in our sample the share of respondents saying that they can generally trust leaders in their village declined from 80.5 percent in 2009 to 74.7 percent in 2012—a similarly-sloped reduction during the period.

[15]We do not have political knowledge questions like "Who is your VEO?" or "Who is the president?"

[16]Controlling the false discovery rate contrasts with controlling the family-wise error rate, or the "probability of rejecting at least one null hypothesis." FWER is most appropriate for cases where the cost of a false rejection of the null hypothesis has strong policy implications (Anderson 2008).

[17]Treatment was randomly assigned at the village level.

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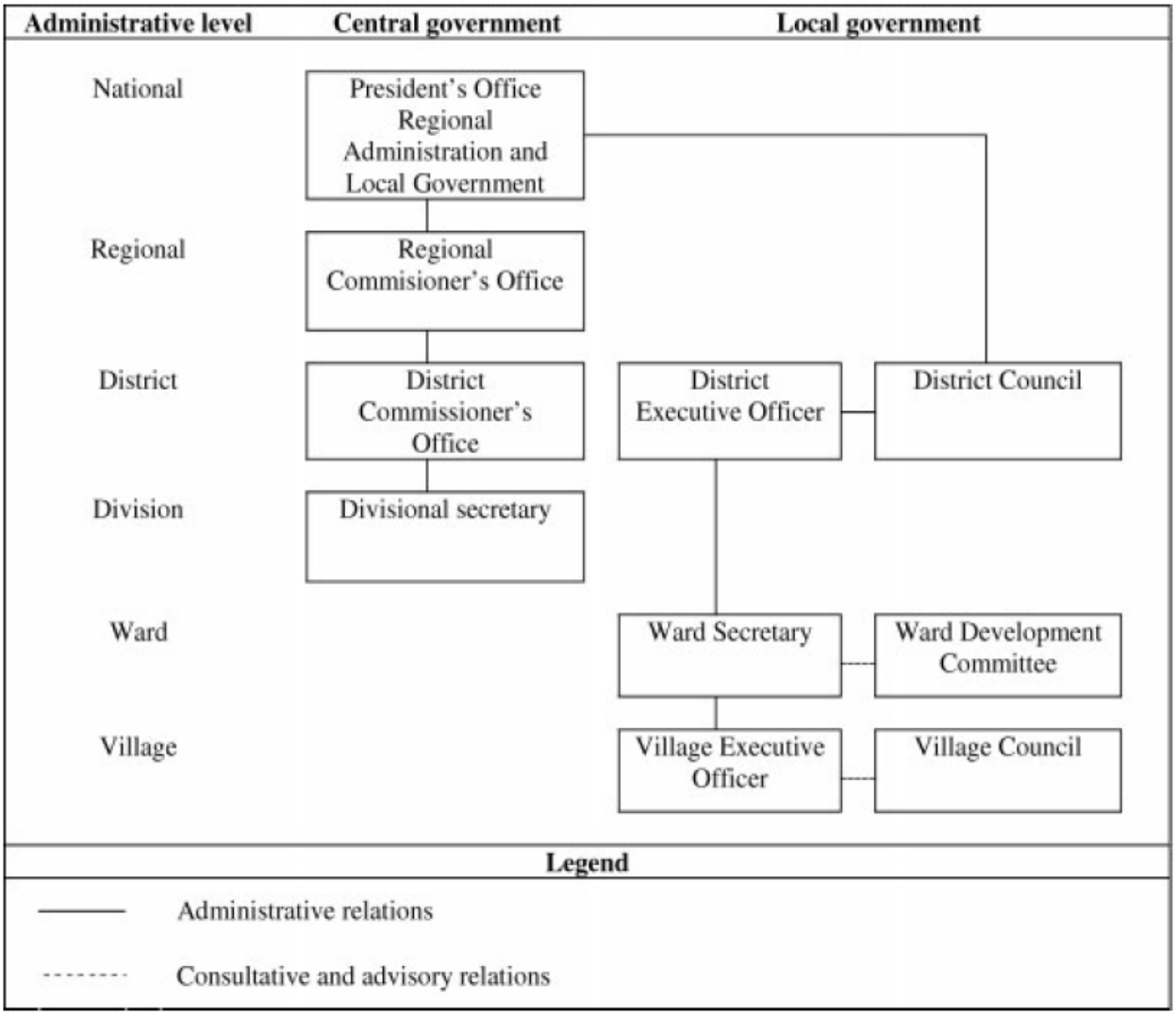
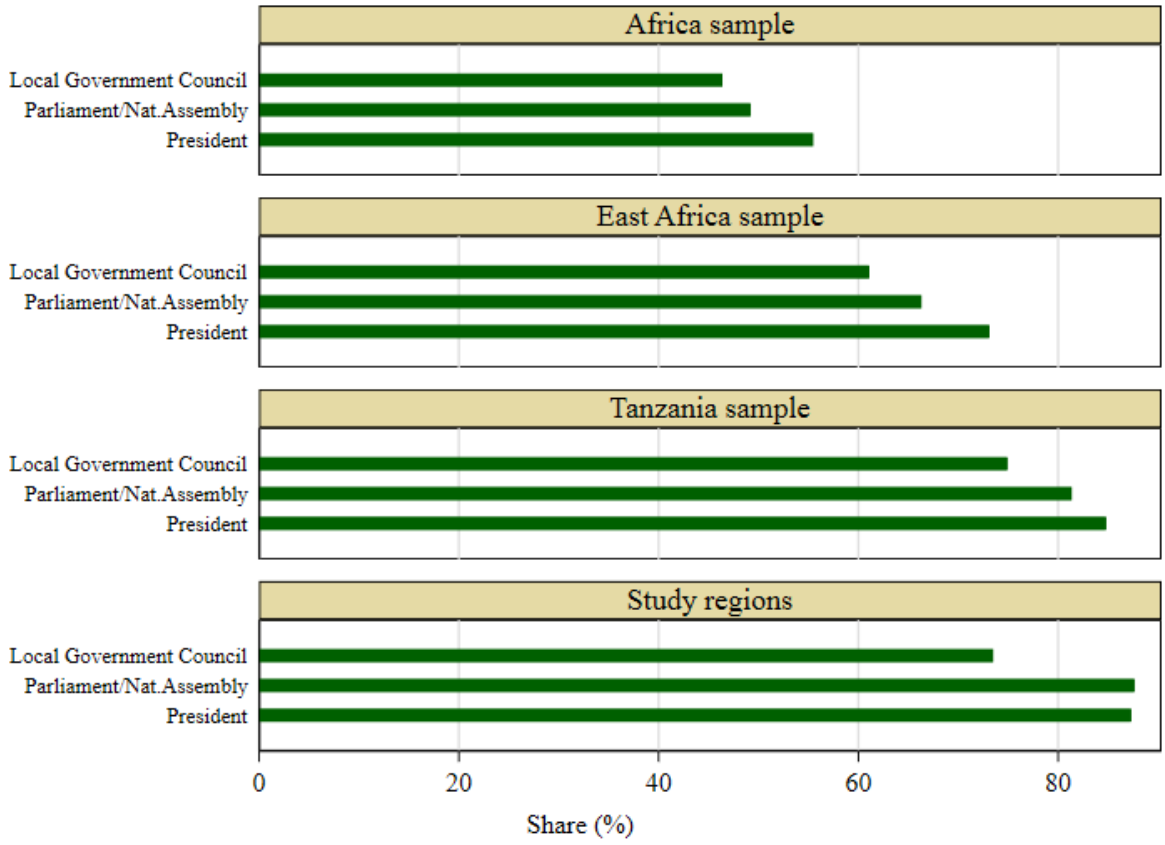
Figure 1: Local governance structure



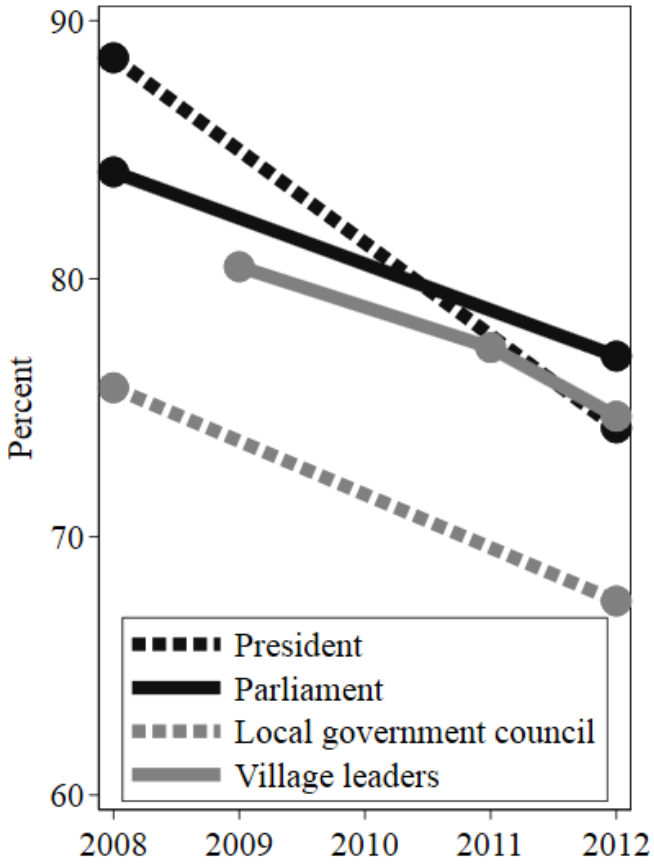
Source: Baker et al. (2002); Lund (2007)

Figure 2: Trust in leadership



Source: Afrobarometer (2005, 2008, 2012, 2016)
Notes: The bars measure the share of individuals who indicate that they trust "somewhat" or "a lot" over 4 different rounds of the Afrobarometer. Responses for a given year are weighted according to the national population and distribution of the sample based on individual selection probabilities (i.e. based on region, gender, urban-rural distribution, and size of household and enumeration area). When averaging across all four rounds, each round is weighted equally. The list of countries included in Afrobarometer has grown over time. To exclude any composition effects, we only include countries that were present in all four rounds. The 18 countries included in the Africa sample are: Benin, Botswana, Cape Verde, Ghana, Kenya, Lesotho, Madagascar, Malawi, Mali, Mozambique, Namibia, Nigeria, Senegal, South Africa, Tanzania, Uganda, Zambia, and Zimbabwe. The three countries included in the East Africa sample are Tanzania, Uganda, and Kenya. The two study regions are Pwani (Bagamoyo and Kibaha districts) and Dodoma (Chamwino district).

Figure 3: Trust in leadership over sample period



Source: Afrobarometer (2008, 2012) and authors' calculations based on baseline (2009), midline (2011), and endline (2012) Tanzania pilot CCT impact evaluation household survey data.
Notes: The lines measuring the trust in the president, parliament, and the local government council are from Afrobarometer. They represent the share of individuals who indicate that they trust "somewhat" or "a lot". Responses are weighted according to selection probabilities (i.e. based on region, gender, urban-rural distribution, and size of household and enumeration area). Trust in village leaders is a measure from the household survey, and it is measured only in the control communities.

Table 1: 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 2: Baseline balance

	Treatment (T)		Control (C)		Difference (T-C)	
Outcome	Mean	N	Mean	N	Mean	S.E.
Panel A: Individual and 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)
Panel B: Outcomes						
Dummy—leaders can generally be trusted	0.81	878	0.80	873	0.01	(0.03)
Dummy—considers community good or excellent: school	0.84	879	0.86	879	-0.02	(0.02)
Dummy—considers community good or excellent: health facilities	0.70	880	0.72	879	-0.02	(0.03)
Dummy - contributed labor to CDP in past year	0.36	880	0.35	879	0.01	(0.04)
Share of households reporting a exists in village: Parent association	0.14	40	0.13	40	0.01	(0.02)
Share of households reporting a exists in village: Health committee	0.61	40	0.59	40	0.02	(0.04)
Panel C: Village characteristics						
Number of community meetings in 2009	3.15	40	3.35	40	-.20	(0.24)
Number of beneficiary households in 2009	148	34	134	29	14	(25)
Number of households in village in 2009	869	39	1091	39	-222	(334)
Share of village households that are beneficiaries	0.30	33	0.19	28	0.11***	(0.03)

Notes: Treatment indicates assignment to treatment. Standard errors are clustered at the village level. *** indicates p<0.01; ** indicates p<0.05; and * indicates p<0.10.

Table 3: Descriptive statistics for outcomes only available in the endline survey

	Pooled			Treatment			Control		
	N	Mean	SD	N	Mean	SD	N	Mean	SD
Trust in leaders									
Dummy—believes trust in community leaders has improved over last 3 years	1600	0.097	0.297	821	0.118	0.323	779	0.076	0.265
Dummy—trusts to a great/very great extent: VEO	1600	0.541	0.498	821	0.568	0.496	779	0.513	0.500
Dummy—trusts to a great/very great extent: village chairman	1600	0.587	0.493	821	0.614	0.487	779	0.558	0.497
Dummy—trusts to a great/very great extent: village council	1600	0.524	0.500	821	0.568	0.496	779	0.479	0.500
Dummy—trusts to a great/very great extent: village CMC	1599	0.567	0.496	821	0.706	0.456	778	0.420	0.494
Satisfaction with government									
Dummy—believes local government and leaders take concerns into account "a lot"	1599	0.185	0.389	821	0.228	0.420	778	0.140	0.347
Dummy—believes honesty of local gov. and leaders has improved in last 3 years	1599	0.114	0.318	821	0.138	0.345	778	0.089	0.284
Dummy—somewhat or very satisfied with the work of the village council	1531	0.786	0.410	791	0.829	0.376	740	0.741	0.439
Dummy—very satisfied with the work of the village council	1531	0.234	0.424	791	0.288	0.453	740	0.177	0.382
Government records									
Dummy—can show report from last 2 months: Village Council	80	0.700	0.461	40	0.775	0.423	40	0.625	0.490
Dummy—can show report from last 2 months: Village Assembly	80	0.488	0.503	40	0.450	0.504	40	0.525	0.506
Dummy—can show report from last 2 months: village finance/planning committee	80	0.637	0.484	40	0.550	0.504	40	0.725	0.452
Dummy—can show report from last 2 months: village security committee	80	0.525	0.503	40	0.500	0.506	40	0.550	0.504
Dummy—can show report from last 2 months: village school committee	80	0.463	0.502	40	0.575	0.501	40	0.350	0.483
Dummy—can show report from last 2 months: village health committee	80	0.450	0.501	40	0.550	0.504	40	0.350	0.483
Dummy—VC usually publicly posts info about finances and accomplishments	1598	0.151	0.359	821	0.157	0.364	777	0.145	0.353
Dummy—info on how much VC receives, spends, and their work is freely available	1598	0.255	0.436	821	0.252	0.435	777	0.257	0.437
Voting behavior									
Dummy—voted in the 2012 CMC election	1436	0.384	0.486	720	0.489	0.500	716	0.278	0.448
Dummy—voted in last VC election	1599	0.567	0.496	821	0.571	0.495	778	0.563	0.496
Dummy—attended a village assembly or village council meeting in last yr	1599	0.670	0.470	821	0.700	0.458	778	0.638	0.481
Community contributions									
Dummy—would contribute time to a communal project	1600	0.512	0.500	821	0.540	0.499	779	0.483	0.500
Dummy—would contribute money to a communal project	1600	0.404	0.491	821	0.432	0.496	779	0.374	0.484
Dummy—worked with others in village for community benefit	1600	0.181	0.385	821	0.199	0.399	779	0.163	0.370
Dummy—household participates in more groups than three years ago	1600	0.027	0.164	821	0.038	0.191	779	0.017	0.128

Notes: Treatment indicates assignment to treatment. Standard errors are clustered at the village level. There were 8 villages without 2012 CMC elections. *** indicates p<0.01; ** indicates p<0.05; and * indicates p<0.10.

Table 4: Trust of Leaders

	Dummy -		Dummy - trusts ... to a great or very great extent			
	believes leaders can generally be trusted	believes trust in community leaders has improved over last 3 years	VEO	Village chairman	Village council	CMC
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Effect of assignment to treatment						
Treatment × 2011 (midline)	0.052*					
	(0.031)					
Treatment × 2012 (endline)	0.054**	0.038**	0.038	0.045	0.078**	0.258***
	(0.027)	(0.017)	(0.036)	(0.033)	(0.036)	(0.032)
2011 (midline)	-0.027					
	(0.022)					
2012 (endline)	-0.052***					
	(0.016)					
R^2	0.003	0.023	0.052	0.040	0.051	0.127
Baseline mean	0.805					
Observations	5007	1594	1594	1594	1594	1593
Panel B: Heterogeneous treatment effects by village meetings						
Treatment effect for villages with fewer (midline)	0.041					
	(0.051)					
Treatment effect for villages with more (midline)	0.059					
	(0.036)					
Treatment effect for villages with fewer (endline)	0.039	-0.003	0.008	0.019	0.053	0.236***
	(0.035)	(0.018)	(0.054)	(0.048)	(0.057)	(0.055)
Treatment effect for villages with more (endline)	0.073*	0.072***	0.077	0.080*	0.113**	0.282***
	(0.037)	(0.026)	(0.048)	(0.047)	(0.046)	(0.040)
p-value of difference (midline)	0.777					
p-value of difference (endline)	0.518	0.022	0.357	0.367	0.407	0.502
Baseline mean for villages with fewer	0.782					
Baseline mean for villages with more	0.823					

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

Notes: Fewer refers to those residing in villages in the bottom half of the distribution of baseline village meetings per year, while more refers to those in the top half. Column 1 includes household fixed effects. Columns 2 - 6 include controls for age, age^2, sex, and education level of the household head. Also included are dummies for district, household size, having an improved roof, having an improved toilet, having an improved floor, having piped water, logged 2009 village population, and the first principal components from a PCA using information on ownership of 13 household assets at baseline. 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 5: Government Responsiveness and Honesty

	Dummy—household head believes that ...		Dummy ... with the work of the village council		Dummy—considers ... good or excellent	
	Local gov't & leaders take citizens' concerns into account "a lot"	In last 3 years, honesty of local gov't & leaders has improved	Somewhat or very satisfied	Very satisfied	School	Health facility
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Effect of assignment to treatment						
Treatment × 2011 (midline)					0.096**	0.127**
					(0.044)	(0.048)
Treatment × 2012 (endline)	0.072***	0.040*	0.065***	0.096***	0.049	0.029
	(0.024)	(0.020)	(0.022)	(0.030)	(0.035)	(0.040)
2011 (midline)					-0.175***	0.131***
					(0.030)	(0.035)
2012 (endline)					-0.231***	0.120***
					(0.023)	(0.026)
R^2	0.042	0.041	0.042	0.050	0.061	0.019
Baseline mean					0.851	0.709
Observations	1593	1593	1525	1525	5,034	5,035
Panel B: Heterogeneous treatment effects by village meetings						
Treatment effect for villages with fewer (midline)					0.090*	0.024
					(0.054)	(0.059)
Treatment effect for villages with more (midline)					0.095	0.209***
					(0.064)	(0.70)
Treatment effect for villages with fewer (endline)	0.017	-0.032	0.034	-0.022	-0.005	-0.084
	(0.029)	(0.022)	(0.034)	(0.031)	(0.048)	(0.057)
Treatment effect for villages with more (endline)	0.126***	0.103***	0.097***	0.208***	0.099**	0.129**
	(0.031)	(0.031)	(0.030)	(0.041)	(0.047)	(.051)
p-value of difference (midline)					0.957	0.048
p-value of difference (endline)	0.010	0.001	0.175	0.000	0.128	0.006

Source: Authors' calculations based on endline (2012) Tanzania pilot CCT impact evaluation household survey data.

Notes: Fewer refers to those residing in villages in the bottom half of the distribution of baseline village meetings per year, while more refers to those in the top half. Columns 1-4 include controls for age, age^2, sex, and education level of the household head. Also included are dummies for district, household size, having an improved roof, having an improved toilet, having an improved floor, having piped water, logged 2009 village population, and the first principal components from a PCA using information on ownership of 13 household assets at baseline. Columns 5-6 include household fixed effects. 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 6: Government Record-Keeping and Transparency

	Dummy - can show village ... committee report from last 2 months						Information on how much money VC receives, spends, and what they accomplish	
	Village Council	Village Assembly	Finance / Planning	Defense/ security	School	Health	Dummy—VC usually posts this information	Dummy—this information is freely available
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Effect of assignment to treatment								
Treatment × 2012 (endline)	0.167	-0.129	-0.136	-0.007	0.241**	0.235**	0.004	-0.026
	(0.115)	(0.113)	(0.110)	(0.117)	(0.115)	(0.108)	(0.030)	(0.035)
Observations	80	80	80	80	80	80	1,592	1,592
R^2	0.039	0.107	0.142	0.060	0.071	0.122	0.051	0.049
Panel B: Heterogeneous treatment effects by village meetings								
Treatment effect for villages with fewer (endline)							-0.061	-0.085**
							(0.037)	(0.042)
Treatment effect for villages with more (endline)							0.065	0.037
							(0.041)	(0.048)
p-value of difference (endline)							0.018	0.044

Source: Authors' calculations based on endline (2012) Tanzania pilot CCT impact evaluation household survey data.
Notes: Only month and year information (no day of month) is available for the defense/security, school, and health reports. The outcome was coded 1 if the number of days between the endline interview (for which we know exact date) and the meeting was less than 62 days (2 months). The date of the meeting was arbitrarily chosen to be the 15th of the month reported. Fewer refers to those residing in villages in the bottom half of the distribution of baseline village meetings per year, while more refers to those in the top half. Columns 1 - 3 includes controls for logged 2009 village population and district controls. Heterogeneous treatment effects were not calculated for these three village-level outcomes. Columns 4 - 5 include controls for age, age^2, sex, and education level of the household head. Also included are dummies for district, household size, having an improved roof, having an improved toilet, having an improved floor, having piped water, logged 2009 village population, and the first principal components from a PCA using information on ownership of 13 household assets at baseline. 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 7: Voting and Civic Engagement

		Dummy—voted in ... election	
	Dummy—attended a village assembly or village council meeting in last 12 mo	Last village council	2012 CMC
	(1)	(2)	(3)
Panel A: Effect of assignment to treatment			
Treatment × 2012 (endline)	0.028	-0.026	0.190***
	(0.029)	(0.033)	(0.037)
R^2	0.090	0.057	0.083
Observations	1593	1593	1430
Panel B: Heterogeneous treatment effects by village meetings			
Treatment effect for villages with fewer (endline)	-0.010	-0.064	0.126**
	(0.046)	(0.039)	(0.057)
Treatment effect for villages with more (endline)	0.054	0.024	0.260***
	(0.036)	(0.051)	(0.048)
p-value of difference (endline)	0.288	0.169	0.074

Source: Authors' calculations based on endline (2012) Tanzania pilot CCT impact evaluation household survey data.
Notes: There were 8 villages without 2012 CMC elections. Fewer refers to those residing in villages in the bottom half of the distribution of baseline village meetings per year, while more refers to those in the top half. All specifications include controls for age, age^2, sex, and education level of the household head. Also included are dummies for district, household size, having an improved roof, having an improved toilet, having an improved floor, having piped water, logged 2009 village population, and the first principal components from a PCA using information on ownership of 13 household assets at baseline. 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 8: Community Development and Involvement

	Dummy - would contribute ... to a communal project that does not directly benefit household		Dummy - in last year has ...		Dummy - household participates in ...	Share of households reporting that a ... exists in village	
	Time	Money	Worked with villagers for benefit of community	Contributed labor to a CDP	More groups than they did three years ago	Parent association	Health committee
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Effect of assignment to treatment							
Treatment × 2011 (midline)				0.015		0.031	0.158**
				(0.042)		(0.042)	(0.061)
Treatment × 2012 (endline)	0.049*	0.059**	0.020	0.006	0.027***	0.015	0.068
	(0.025)	(0.026)	(0.023)	(0.038)	(0.008)	(0.035)	(0.042)
2011 (midline)				-0.129***		0.046	-0.099*
				(0.030)		(0.030)	(0.051)
2012 (endline)				-0.213***		0.087***	0.067**
				(0.025)		(0.024)	(0.030)
R^2	0.054	0.052	0.084	0.064	0.056	0.128	0.171
Baseline mean				0.358		0.138	0.604
Observations	1594	1594	1594	5035	1594	240	240
Panel B: Heterogeneous treatment effects by village meetings							
Treatment effect for villages with fewer (midline)				-0.036		0.012	0.113*
				(0.060)		(0.066)	(0.057)
Treatment effect for villages with more (midline)				0.059		0.047	0.181*
				(0.057)		(0.053)	(0.095)
Treatment effect for villages with fewer (endline)	0.091**	0.078*	-0.009	-0.029	0.022	0.021	0.032
	(0.039)	(0.040)	(0.034)	(0.054)	(0.014)	(0.055)	(0.067)
Treatment effect for villages with more (endline)	0.016	0.045	0.041	0.036	0.033***	0.018	0.090*
	(0.034)	(0.034)	(0.028)	(0.052)	(0.010)	(0.043)	(0.052)
p-value of difference (midline)				0.254		0.681	0.544
p-value of difference (endline)	0.158	0.519	0.236	0.386	0.544	0.966	0.490

Source: Authors' calculations based on baseline (2009), midline (2011), and endline (2012) Tanzania pilot CCT impact evaluation household survey data.
Notes: Columns 1, 2, 3, and 5 include controls for age, age^2, sex, and education level of the household head. Also included are dummies for district, household size, having an improved roof, having an improved toilet, having an improved floor, having piped water, logged 2009 village population, and the first principal components from a PCA using information on ownership of 13 household assets at baseline. Column 4 includes household fixed effects. Columns 6 and 7 include village 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 9: Robustness testing: multiple hypothesis testing

						BKY values grouped ...			
						... all together		... by table	
Table	Treatment estimate	Outcome	Column	Estimate	P	BKY	> 0.10?	BKY	> 0.10?
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
4	T×2011	Dummy—leaders can generally be trusted	1	0.052*	0.094	0.176	x	0.134	x
4	T×2012	Dummy—leaders can generally be trusted	1	0.054**	0.045	0.123	x	0.099	
4	T×2012×more	Dummy—leaders can generally be trusted	1	0.073*	0.054	0.130	x	0.106	x
4	T×2012	Dummy—trust in community leaders has gotten better over last 3 years	2	0.038**	0.028	0.101	x	0.080	
4	T×2012×more	Dummy—trust in community leaders has gotten better over last 3 years	2	0.072***	0.007	0.038		0.034	
4	T×2012×more	Dummy—trusts village chairman to a great/very great extent	4	0.080*	0.090	0.176	x	0.134	x
4	T×2012	Dummy—trusts village council to a great/very great extent	5	0.078**	0.034	0.105	x	0.083	
4	T×2012×more	Dummy—trusts village council to a great/very great extent	5	0.113**	0.016	0.065		0.056	
4	T×2012	Dummy—trusts the CMC in village to a great/very great extent	6	0.258***	0.000	0.000		0.000	
4	T×2012×fewer	Dummy—trusts the CMC in village to a great/very great extent	6	0.236***	0.000	0.001		0.000	
4	T×2012×more	Dummy—trusts the CMC in village to a great/very great extent	6	0.282***	0.000	0.000		0.000	
5	T×2012	Dummy—local government and leaders take into account a lot the concerns	1	0.072***	0.003	0.021		0.011	
5	T×2012×more	Dummy—local government and leaders take into account a lot the concerns	1	0.126***	0.000	0.002		0.002	
5	T×2012	Dummy—In the last 3 years, the honesty of local gov't and leaders has improved	2	0.040*	0.055	0.130	x	0.063	
5	T×2012×more	Dummy—In the last 3 years, the honesty of local gov't and leaders has improved	2	0.103***	0.001	0.013		0.010	
5	T×2012	Dummy—somewhat or very satisfied with the work of the village council	3	0.065***	0.005	0.026		0.011	
5	T×2012×more	Dummy—somewhat or very satisfied with the work of the village council	3	0.097***	0.002	0.016		0.010	
5	T×2012	Dummy—very satisfied with the work of the village council	4	0.096***	0.002	0.017		0.010	
5	T×2012×more	Dummy—very satisfied with the work of the village council	4	0.208***	0.000	0.000		0.000	
5	T×2011	Dummy—considers community school good or excellent	5	0.096**	0.030	0.101	x	0.039	
5	T×2011×fewer	Dummy—considers community school good or excellent	5	0.090*	0.095	0.176	x	0.089	
5	T×2012×more	Dummy—considers community school good or excellent	5	0.099**	0.040	0.115	x	0.049	
5	T×2011	Dummy—considers community health facilities good or excellent	6	0.127**	0.010	0.050		0.018	
5	T×2012×more	Dummy—considers community health facilities good or excellent	6	0.129**	0.013	0.058		0.022	
5	T×2011×more	Dummy—considers community health facilities good or excellent	6	0.209***	0.004	0.023		0.011	
6	T×2012	Dummy—can show village school committee report from last 2 months	5	0.241**	0.039	0.115	x	0.225	x
6	T×2012	Dummy—can show village health committee report from last 2 months	6	0.235**	0.033	0.105	x	0.225	x
6	T×2012×fewer	Dummy—info on how much VC receives, spends, & their work freely available	8	-0.085**	0.046	0.123	x	0.225	x
7	T×2012	Dummy—voted in 2012 CMC election	1	0.190***	0.000	0.000		0.000	
7	T×2012×fewer	Dummy—voted in 2012 CMC election	1	0.126**	0.030	0.101	x	0.076	
7	T×2012×more	Dummy—voted in 2012 CMC election	1	0.260***	0.000	0.000		0.000	
8	T×2012	Dummy—would contribute time to a communal project	1	0.049*	0.056	0.130	x	0.206	x
8	T×2012×fewer	Dummy—would contribute time to a communal project	1	0.091**	0.024	0.091		0.151	x
8	T×2012	Dummy—would contribute money to a communal project	2	0.059**	0.023	0.089		0.151	x
8	T×2012×fewer	Dummy—would contribute money to a communal project	2	0.078*	0.055	0.130	x	0.206	x
8	T×2012	Dummy—household participates in more groups than they did three years ago	5	0.027***	0.001	0.011		0.022	
8	T×2012×more	Dummy—household participates in more groups than they did three years ago	5	0.033***	0.001	0.013		0.022	
8	T×2011	Share of households reporting a village health committee exists in village	7	0.158**	0.012	0.056		0.124	x
8	T×2011×fewer	Share of households reporting a village health committee exists in village	7	0.113*	0.051	0.130	x	0.206	x
8	T×2011×more	Share of households reporting a village health committee exists in village	7	0.181*	0.061	0.134	x	0.206	x
8	T×2012×more	Share of households reporting a village health committee exists in village	7	0.090*	0.086	0.171	x	0.258	x

Source: Authors' calculations based on baseline (2009), midline (2011), and endline (2012) Tanzania pilot CCT impact evaluation household survey data.
Notes: Treatment effect estimates with p-values<0.10 displayed. Midline and endline treatment effects are abbreviated T×2011 and T×2012, respectively. More and fewer refer to whether the village has more or fewer village meetings than the median village in the sample. Column refers to the column in which the estimate appears in the original table. BKY stands for Benjamini, Krieger, and Yekutieli q-values—the smallest level at which the the null hypothesis that the coefficient is 0 is rejected. An x in Columns 8 and 10 indicates that the q-value exceeds a standard significance level of 0.10).

Table 10: Baseline correlates of having more village meetings

	(1)	(2)
Treatment village	-0.055	-0.062
	(0.112)	(0.124)
Share of village that trusts leaders		1.198*
		(0.619)
Gini coefficient of total consumption		0.357
		(1.247)
Logged 2009 village population		-0.096
		(0.081)
Average of household asset index		-0.144
		(0.128)
Logged average household consumption		0.560**
		(0.275)
Number of household members under 18 years		-0.242
		(0.255)
Number of household members 18+ years		-0.112
		(0.201)
Average household head age		0.009
		(0.023)
Share of household heads with some education		-0.217
		(0.606)
Literacy index		0.520
		(0.984)
Average village age		-0.013
		(0.033)
Chamwino	0.015	0.211
	(0.139)	(0.289)
Kibaha	0.212	0.273*
	(0.131)	(0.151)
Constant	0.513***	-1.658
	(0.103)	(2.347)
Observations	80	80
R^2	0.038	0.210

Source: Authors' calculations based on baseline (2009) Tanzania pilot CCT impact evaluation household survey data.
Notes: Average refers to village average at baseline. Literacy index is the village literacy rate adjusted for age. Standard errors are in parentheses and clustered at the village level. *** indicates p<0.01; ** indicates p<0.05; and * indicates p<0.10.