Corruption and Accountability at the Grassroots Level: An Experiment on the Preferences and Incentives of Village Leaders

Malte Lierl[*]

This version: September 20, 2018
First version: January 31, 2014

Abstract

This article examines the willingness of elected and non-elected village leaders to misappropriate public resources for personal gain. A behavioral experiment with local leaders in 48 Tanzanian villages reveals that elected village leaders are intrinsically less willing to misappropriate public funds than externally appointed village leaders and ordinary citizens in the same communities. However, actual corruption outcomes depend less on the village leaders' intrinsic preferences than on the social and electoral incentives village leaders are facing in their community. At the village level, corruption is highly conspicuous and difficult to hide. Therefore, village leaders have to anticipate the consequences of their actions for their social reputations and for their future social interactions in the village. These informal social incentives affect not only elected leaders, but also non-elected leaders, and appear to offset some of the advantages of electoral selection.

[*]German Institute of Global and Area Studies (GIGA). Contact: malte.lierl@giga-hamburg.de.

Ethnographic studies have long provided examples of corruption by village leaders that was met with impunity (Bierschenk and de Sardan, 1997; Platteau and Gaspart, 2003; Ensminger, 2007). Interviewed for this study, village residents in Tanzania were very forthcoming with their own examples. They complained that food aid was not handed out to the intended beneficiaries, that agricultural subsidy vouchers were misallocated, that village leaders used and sold communal land as if it were their private property, that contributions were collected for projects that were never realized, that funds disappeared from village coffers, and so forth. Throughout the areas covered by this study, village leaders were frequently perceived as corrupt.[1]

The conspicuousness of village-level corruption, along with the close social proximity between those who steal public resources, and those who are negatively affected by it, raise intriguing questions for politicial science. First, information assymmetries between citizens and politicians, often considered an important cause of political corruption (Ferraz and Finan, 2008; Pande, 2011), have little explanatory power in small-scale communities like villages that tend to be high-information environments with little anonymity (Grossman, 2011, Ch. 1). Dense social interaction and familial ties between residents facilitate information exchange and make it difficult for village leaders to hide corruption. Voters, on the other hand, have intimate knowledge of candidates for village leadership positions, much more so than in higher-level elections. Yet, village-level corruption persists.

Second, the close social proximity between village leaders and village residents implies that citizens can sanction local leaders not just at election time, but also continually in everyday social interactions (Tsai, 2007). This raises the question to what extent corruption outcomes depend on the *social incentives* village leaders are facing. The concept of "social incentives" dates back at least to Olson (1965, Ch. IIc). It will be used to describe extrinsic motivations that arise from a person's desire maintain a positive reputation in their social environment or elicit positive reactions from others, or to avert negative reactions or negative reputational consequences.

To help us understand why village-level corruption persists, this paper will explore two fundamen-

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[1] In 32 randomly sampled villages in Tanzania's Hanang and Mufindi districts (a subset of the villages surveyed for this study), residents were asked whether "most people in the village think that the village government is corrupt". Across villages, between 20 and 81 percent of residents affirm this statement. In 21 of 32 villages a majority of residents agree with it.

tal questions about village leaders' motivations. (1) In deciding to misappropriate public funds, are village leaders influenced by extrinsic social incentives (including concerns for their reputation, anticipated social sanctions, and concerns for their re-election prospects), or are their choices a reflection of their intrinsic social preferences (i.e. their intrinsic willingness to refrain from misappropriating public funds, independent of other people's reactions)? (2) Do elected and non-elected village leaders differ with respect to their intrinsic social preferences and the impact of extrinsic social incentives on their choices?

A field-based behavioral experiment was carried out with elected and non-elected village leaders in 48 villages in Tanzania, in 2013/14. Since village governments in Tanzania are headed jointly by a locally elected village chairperson and an externally appointed village executive officer (VEO), the behavior of elected and non-elected village leaders can be compared both within the same communities and across a diverse sample of villages that share the same institutional features.

The paper first examines village leaders' intrinsic willingness to refrain from misappropriating public funds, which can be understood as a type of social (or other-regarding) preference. To measure these misappropriation preferences, village leaders' behavior, along with that of a sample of local residents, was observed in a generic (and thus highly comparable) decision situation, where they were given the opportunity to anonymously misappropriate their co-villagers' contributions to a group fund.

Next, the paper examines how village leaders' willingness to misappropriate contributions changes, if citizens in their village can potentially hold them accountable for their actions. To this end, it was varied experimentally whether the people who were affected by village leaders' decisions to misappropriate contributions (in the experimental setting) could personally attribute the misappropriation to them. Simultaneously, the experiment varied whether the people who were affected by village leaders' misappropriation of contributions were residents of their own village, or strangers with whom they were unlikely to ever interact after the experiment. Village leaders could only expect to be held accountable for their actions or to suffer reputational consequences, if those actions were both personally attributable to them and there was a realistic possibility of repeated

interaction outside the experiment.

Finally, the experimental results are validated with observational data on the misappropriation of public resources in natural settings: Can the two concepts measured in the experimental setting – village leaders' intrinsic preferences and the effect of social incentives – explain why in some villages more corruption can be observed than in others? To this end, data was collected on two indicators of misappropriation of public resources at the village level. One indicator relates to rent extraction by the elected village chairperspon, while the other relates to rent extraction by the externally appointed village executive officer.

Across these three parts of the research agenda, consistent results emerge. First, with respect to village leaders' rent extraction preferences, it is shown that elected village chairpersons are intrinsically less willing to misappropriate public funds than externally-appointed village executive officers and randomly sampled village residents. Second, the impact of social incentives is ambiguous. The need to anticipate the reputational consequences of their choices and their co-villagers' reactions to them does not uniformly result in less misappropriation. If social incentives come into play, the choices of elected and non-elected leaders become indistinguishable. Third, when village leaders' choices in the experiment are compared to corruption outcomes in natural settings, their responses to social incentives, rather than their intrinsic preferences, are strongly associated with variation in local corruption outcomes. The latter result is particularly important, because it suggests that the experiment was successful at measuring behavioral responses that also determine corruption outcomes in the real world.

Contributions to the Literature

In several ways, the findings of this study shed new light on the relationship between elections and corruption in local-level governance. By showing that the rent extraction preferences of elected leaders differ from those of non-elected leaders and the general population, the study provides original evidence of electoral selection effects. This is important, because very few prior studies

had ever directly measured the social preferences of public decision-makers. Notably, Baldassarri and Grossman (2013) measured directed and generalized altruism through dictator games with village leaders in Uganda, Baldassarri and Grossman (2013); Grossman (2014) with leaders of farmer groups in Uganda, Beekman, Bulte, and Nillesen (2014) with chiefs in Liberia, and Kosfeld and Rustagi (2015) with forest group leaders in Ethiopia. Along with Fehr and List (2004), who measured trust and trustworthiness of CEOs, and Sheffer, Loewen, Soroka et al. (2018), who measured cognitive biases of parliamentarians, this growing literature has begun quantify how the behavioral traits of elites differ from those of the general population. Yet, a direct comparison of the social preferences of elected and non-elected leaders in the same context had been lacking. This study shows that electoral selection can be more effective at identifying public-spirited leaders than centralized appointments. This is all the more remarkable, because village elections are not always perfectly competitive, and competition is sometimes more likely to occur at the candidate selection stage (Lund and Saito-Jensen, 2013, 107).

By providing evidence that village leaders' misappropriation of public funds is not just a function of their intrinsic social preferences, but also shaped by the need to take their co-villagers' reactions into consideration, this study complements prior research on role of informal social sanctioning capacity in local-level governance (Miguel and Gugerty, 2005; Tsai, 2007). While prior studies were observational, this study provides the first experimental evidence of the impact of informal social incentives on the behavior of village leaders and validates it through comparisons with real-world corruption outcomes.

Finally, the findings of this study are consistent with emerging evidence that democratically electing village leaders seems to make little difference with regard to the misappropriation of public resources (Alatas, Banerjee, Hanna et al., 2013; Beath, Christia, and Enikolopov, 2014). Even though elected village chairpersons, externally appointed village executive officers and ordinary citizens in this study had very different social preferences, their behavior became statistically indistinguishable once their co-villagers could hold them accountable for their actions. Informal social incentives and the fact that corruption and misappropriation are very difficult to hide in small-scale communities, might thus explain why democratically elected village leaders misappropriate public resources at

similar rates as non-elected village leaders.

Why Tanzania?

While village-level corruption occurs all over the world, the Tanzanian case offers several distinct advantages to study village leaders' motives. First, misappropriation of village resources has very direct consequences for people's livelihoods. Village governments in Tanzania regulate and manage village affairs rather autonomously, collecting their own revenues through local contributions, organizing the provision of public goods (such as the maintenance of school buildings and water supply), allocating communal land, and controlling the distribution of public benefits, such as food aid and agricultural subsidies. In doing so, village governments are only weakly accountable higher-level authorities, which implies that village leaders carry a great deal of personal responsibility that can potentially be exploited.

Second, Tanzanian villages have unusually homogeneous political institutions and histories. Modern Tanzanian villages did not evolve organically, but were established through state intervention and forced resettlement in the 1970s, under the ideology of *ujamaa* socialism. From this time period, villages have inherited a set of externally imposed governance institutions that are identical throughout the country. Yet, villages differ greatly in their ability to provide public goods to local residents. This cannot easily be attributed to historical differences in local institutions, as it may be in other countries (Berger, 2009; Acemoglu, Reed, and Robinson, 2014). Institutional homogeneity increases the external validity of the study and provides a backdrop against which alternative explanations for variation in local governance quality can be tested.

Third, variation in local governance quality cannot easily be attributed to ethnic cleavages, unlike in neighboring countries (Miguel and Gugerty, 2005; Habyarimana, Humphreys, Posner et al., 2009). In Tanzania, ethnic identities have very little political salience, due to deliberate nation building efforts, a national language (Miguel, 2004), and the elimination of tribal authorities as mediators between citizens and the state (Boone and Nyeme, 2015). This makes Tanzania an excellent case

to study leader behavior and social norm enforcement more directly.

Finally, the dual leadership structure of Tanzanian village governments makes it possible to compare the behavior of elected and appointed leaders within the same village context. Tanzanian village governments are led by an elected village chairperson (*mwenyekiti wa kijiji*) and an externally appointed village executive officer (*mtendaji wa kijiji*, abbreviated: VEO). While their formal roles differ (Osafo-Kwaako, 2012, 25), they manage village affairs jointly and in consultation with each other. The appointed VEO acts as a village manager and is typically in charge of liaising with the state bureaucracy and managing village funds and projects. The elected chairperson presides over council and committee meetings and tends to have greater influence over allocative decisions and rule-making in the village than the VEO. It is not always obvious who wields more power in a given village.[2]

Part 1: Measuring Village Leaders' Misappropriation Preferences

To understand village leaders' motivation to misappropriate public funds – or to refrain from doing so – we first turn to their social preferences. In behavioral economics, "social preferences" describe intrinsic motivations for other-regarding behavior, such as altruism, reciprocity, inequity aversion etc. (Levitt and List, 2007). In this sense, social preferences are distinct from extrinsic incentives to comply with social norms, such as rewards or sanctions by others, social recognition, reputational consequences, and the risk of criminal punishment in the case of illegal actions. For example, individuals' behavior in an anonymous dictator game can be used to quantify generalized altruism: How much economic self-interest is someone willing to sacrifice in order to bestow benefits upon an unknown other person?

Analogously, if village leaders did not have to fear that anyone else would ever find out if they misappropriated public funds, the extent to which they voluntarily forego the opportunity to enrich themselves would reflect their intrinsic motivation to refrain from misappropriation. In reality,

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[2]In 10 out of 32 surveyed villages in Hanang and Mufindi Districts, a majority of respondents attributed more power to the appointed VEO than to the elected chairperson.

however, village leaders' rent extraction is far from anonymous. Villages are small communities with dense familial ties and social networks. If funds are siphoned off from village projects, or public benefits are misallocated, such information spreads rapidly. Village leaders therefore have to anticipate how their co-villagers will react.

To measure village leaders' intrinsic willingness to refrain from misappropriating public funds, they must be shielded from any external consequences on their reputations, re-election chances or future social interactions in their village. This is done through a stylized decision exercise. In the decision exercise, study participants were given the opportunity to actually misappropriate money that a sample of their co-villagers had contributed to a group fund. Such decision exercises are sometimes referred to as 'experimental games' (Henrich, Boyd, Bowles et al., 2004, Ch. 3), 'behavioral games' (Grossman and Baldassarri, 2012) or 'artefactual field experiments' (Harrison and List, 2004). However, this terminology may be confusing, because the decision exercise has nothing to do with "playing a game", nor is it a field experiment in the sense most political scientists would understand the term. The advantage of using a neutrally-framed, artificially created decision situation is that the outcomes are highly comparable – between different types of village leaders and between village leaders and ordinary village residents.

The decision exercise and its practical implementation are described in detail below. Conceptually, it resembles a voluntary contributions public goods game, with an added stage in which any fraction of the pooled payoff can be misappropriated by one group member, instead of being shared equally among the group members. Similar setups have been used in a laboratory experiment by Lierl (2016) and in the "bosses and kings" games in Elinor Ostrom's last working paper (Cox, Ostrom, and Walker, 2011). Of interest here is how much of the group members' voluntary contributions study participants were willing to misappropriate.

Design of the decision exercise

In the decision exercise, study participants from a village were divided into groups of eight co-villagers. Per village, there were multiple such groups. Study participants did not know who else

from their village was assigned to their group, but they knew that all group members were from their own village. Every group member was given an initial endowment of 1000 Tanzanian Shillings[3] and the opportunity to voluntarily and anonymously contribute any fraction of this endowment to a group fund. The study participants were informed that their contributions to the group fund would be pooled, doubled by the experimenter, and eventually distributed in equal shares to all eight group members. However, before the payoff was disbursed, one of the eight group members would be given the opportunity to anonymously misappropriate money from the group fund and to keep that money for her- or himself, so that only the remainder of the group fund would be shared equally among the group members. All study participants were asked to confidentially propose what fraction of the group fund they would misappropriate, if they were given the opportunity to do so. They were informed that one of the eight misappropriation proposals per group would eventually be implemented, and all eight proposals had an equal chance of being selected. Thus, by proposing to partly or wholly misappropriate the group fund, the study participants could enrich themselves at the expense of the other group members, if their proposal was implemented.

Figure 1 summarizes the different steps of the decision exercise and how they were implemented. The instructions to study participants were framed neutrally, carefully avoiding any concepts that could have primed them about moral norms or socially desireable behavior. Words like "misappropriation", "misappropriation", "corruption", "village leader" or "public goods" were deliberately avoided. The decision situation was explained as follows:

- [...] I would like to invite you to take part in an exercise which is about money.
  First, we will be collecting money from you as well as from seven other people
  who took part in the survey. Right now, I will not tell you who these other seven
  people are. However, we are asking each of them to contribute real money [...].

- Let me explain what we will do with the money we collect in this exercise: For

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[3]The stakes in the exercise were meaningful, but low enough to minimize the risk of conflict after the experiment (an ethical consideration). Across four decision exercises, study participants earned on average 6000 Tanzanian Shillings (TZS). This corresponded approximately to a daily wage for unskilled labor in the rural areas at the time. The maximum amount study participants could theoretically misappropriate in a single decision exercise was 16000 TZS.

each 100 Shilling coin a person contributes, we will add another 100 Shilling
coin. This is going to be the GROUP MONEY. After we have collected the contributions
from all eight participants in the group, we will distribute the group money among
these eight participants.

- How are we going to distribute the group money among the participants? We will
  ask each of the eight people to make a proposal on how to distribute the group
  money. Then we will draw one of these eight proposals at random. Then we will
  split the money according to the proposal that was drawn.

- In their proposal, each participant must indicate what share of the group money
  they want to allocate to themselves, and what share of the group money they want
  to have split equally among all members of the group.

Following this initial explanation, further details were provided, including on how study partic-
ipants' decisions were recorded and how and when their payoff would be disbursed. Extensive
comprehension checks and repetitive instructions at every step of the decision exercise ensured that
all study participants had fully understand every aspect of the decision situation before they were
allowed to proceed. The full set of instructions and comprehension questions is available online
(SI.4).

Prior to making their decisions, study participants were informed that:

- Your decision on how many of the ten 100 Shilling coins you contribute to the
  group will be SECRET.

- Your proposal on how the split the group money will be SECRET in this exercise.
  I will record on the computer how much you proposed to keep for yourself, but
  we will not tell anybody about it.

- The other seven participants will all be FROM YOUR OWN VILLAGE. We will ask each
  of them to make a proposal on how to split the group money between themselves
  and the group.

In addition to the neutral framing and the comprehension checks, further precautions ensured that study participants' misappropriation proposals were not confounded by external influences or strategic considerations. First, to prevent that study participants' misappropriation proposals depended on the size of the group fund, the total amount of contributions remained unknown to the study participants. Instead of proposing to misappropriate a specific amount of money from the group fund, study participants indicated, in a simple, figurative way, what *fraction* of the group money they would misappropriate. Second, to maximize privacy, the decision exericse was carried out in a one-on-one setting. Besides the interviewers and research supervisors, no other individuals were allowed to be present (with the exception of newborns worn by their mothers). The interviewers were instructed not to watch the study participants during their actual contribution decisions and misappropriation proposals, which were indicated by allocating tokens between two jars. After the decisions were made, the interviewers would count the tokens in each jar and record this information on a tablet computer. Third, to ensure that decisions were not affected by inter-temporal preferences, study participants were made aware that they would receive their entire payoff at a specified date when the data collection team returned to their village. This means that the fraction of their endowment they decided to keep for themselves in the contribution stage, as well as any money they misappropriated from the group fund was disbursed to them at the exact same time as their share of the group payoff was disbursed. Hence, no inter-temporal tradeoff was involved in study participants' decisions.

The fact that all parts of study participant's payoff were disbursed at the same time also ensured that study participants' choices did not depend on how much they trusted the research team: There was no tradeoff between keeping money or entrusting it to the interviewers. All decisions involved money that would be managed by the research team anyway. While being the subject of much consideration at the research design stage, in practice there was no indication that any study participant worried about being cheated by the research team.

The field team went to great length to ensure that study participants trusted them. To prevent misinformation about the purpose of the research activities, public meetings were held in the study villages to inform anyone interested about the study and answer village resident's questions. At

these meetings, any village resident could verify their household's inclusion into the sampling frame, before the study participants were sampled in public. That way, all study participants, as well as their co-villagers, knew that they were selected at random. When administering the informed consent procedure to study participants, the field team insisted on first hearing and answering study participants' questions, before accepting their consent. Furthermore, a phone number was left at each village office, where complaints about the study could be directed. No complaints were received.

Finally, to ensure that village residents were unsuspecting about their village leaders' participation in the study, the interviewers were instructed to approach the village leaders only after the decision exercise had been completed with all other study participants from the village.

Results

In the decision exercise, not a single village leader proposed to misappropriate all of their co-villagers' contributions to a public good, even though they could not be held accountable for their choices. Comparing misappropriation proposals, Figure 2 (top panel) reveals that elected village chairpersons are significantly less willing to misappropriate contributions than externally appointed VEOs or randomly sampled adult village residents. On average, elected chairpersons proposed to extract 19.5 percent of their co-villagers' contributions, whereas appointed VEOs extracted 29.2 percent and ordinary residents 37.5 percent. It can be rejected that elected chairpersons misappropriate their co-villagers' contributions at rates similar to appointed VEOs ($p = 0.004$, Mann-Whitney test) or ordinary residents ($p = 6 \cdot 10^{-6}$) from the same sample of villages (see SI.4 for robustness checks).

The more public-spirited preferences of elected chairpersons are also reflected in their contribution decisions in the first stage of the decision exercise, which were non-attributable in all experimental conditions (Figure 2, bottom panel). Elected chairpersons contributed more generously to the public good than ordinary residents ($p = 0.004$, Mann-Whitney test). This could be due to greater altruism towards their co-villagers, or due to greater trust in their co-villagers. However, their

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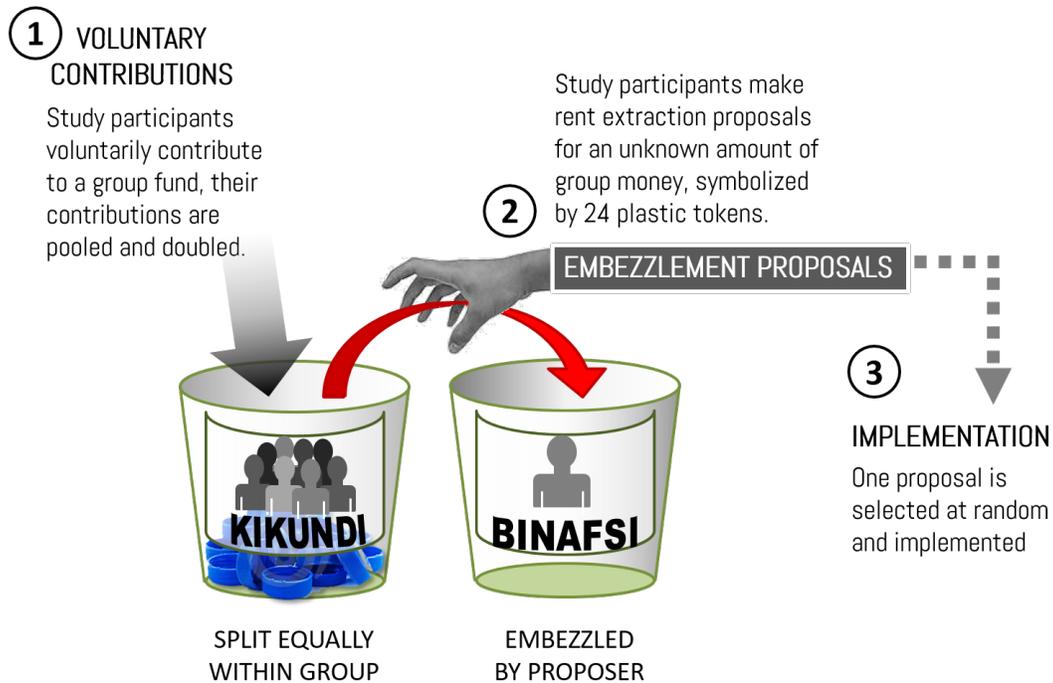
(1) In the first stage, each study participant is placed into a group of eight unidentified individuals and receives an endowment of ten 100 Shilling coins. She or he decides how many coins to keep for her-/himself and how many coins to contribute to a group fund that benefits themselves and seven other people. All contributions to the group fund are pooled and doubled by the experimenter.

(2) In the second stage, every group member proposes what fraction of the group fund they would personally appropriate, if they were given the possibility of doing so. They do so without knowing the total amount of the group fund or the individual contributions of other group members. The group fund is represented to them by 24 tokens in a jar labeled ``Group" in Kiswahili (with a pictogram of eight stylized people). To make a rent extraction proposal, study participants move the desired fraction of tokens out of the ``Group" jar into an identical jar labeled ``Self" (with a pictogram of one stylized person). The fraction of the group fund study participants moved into the ``Self" jar in the second stage indicates their willingness to misappropriate group resources.

(3) In the third stage, one of the eight rent extraction proposals is selected at random and implemented.

Figure 1: Summary of the decision exercise.

Misappropriation Preferences (*co-villager/non-attributable condition*)

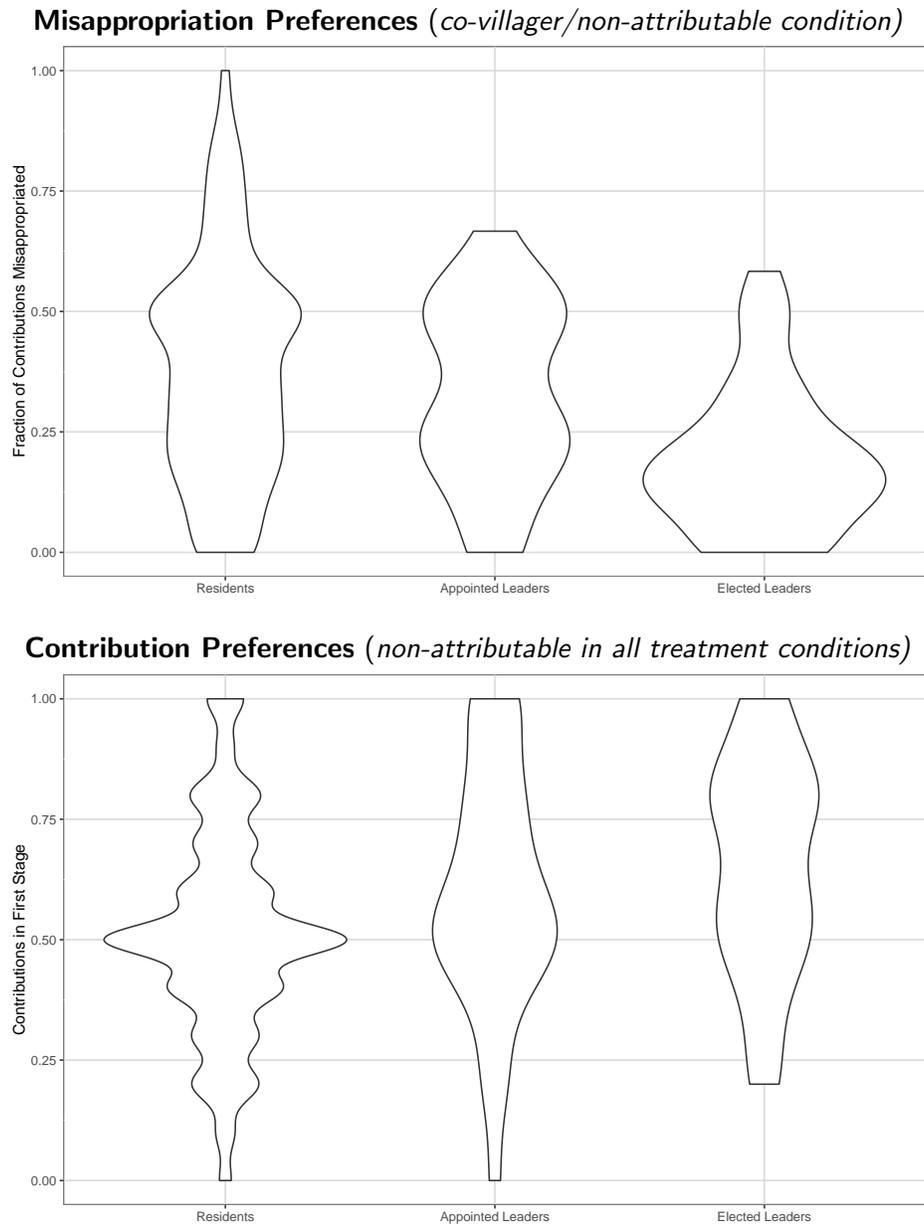## Contribution Preferences (*non-attributable in all treatment conditions*)

Figure 2: Social preferences. The violin plots visualize the distribution of misappropriation preferences (top) and contribution decisions (bottom) of village leaders and randomly sampled residents.

contributions are statistically indistinguishable from those of VEOs.

Elected chairpersons' greater willingness to refrain from misappropriating contributions is also present in interactions with strangers. In the stranger/non-attributable condition, elected chairpersons propose to misappropriate fewer contributions than ordinary residents. This suggests that the elected chairpersons differ from the population not merely with respect to their co-villager bias, as Baldassarri and Grossman (2013) observe in dictator games with Ugandan village leaders, but are more public-spirited in general.

A comparison of village leaders' misappropriation preferences to residents of their own villages lends further support to the idea that the more pro-social preferences of chairpersons are a consequence of electoral selection. Within their respective villages, the elected chairpersons, but not the externally appointed VEOs, rank among the individuals who are least willing to misappropriate contributions. In only two out of 32 villages, the elected chairperson misappropriated more than the median resident, whereas for VEOs this was the case in 13 out of the same 32 villages. A similar pattern exists with respect to the mean (rather than median) misappropriation proposals. Thus, elected chairpersons are systematically more pro-social than other adult citizens in their villages, but the same cannot be said of the externally appointed VEOs.

Finally, villages with more public-spirited residents are more likely to elect a public-spirited leader. Figure 3 shows a positive relationship between the misappropriation preferences of village residents and those of their elected village chairperson, whereas no such relationship exists for externally appointed VEOs. To rule out that this statistical relationship merely reflects naturally occurring intra-group correlation, the sample correlation coefficient between the chairperson's misappropriation proposal and the mean misappropriation proposal among ordinary residents of their village ($r = 0.49$) was compared to the correlations between randomly drawn residents and the mean misappropriation choices among the other 13 ordinary residents in their village. Estimated via re-sampling, the probability that the misappropriation choices of randomly sampled ordinary residents were as strongly correlated with the mean misappropriation proposal in the village as that of the elected chairperson is very low ($p = 0.006$). This also holds for the correlation between the

chairperson's and the median misappropriation proposal in the village ($r = 0.47$, $p < 0.001$). No such pattern exists for externally appointed VEOs ($r = 0.03$, $p = 0.6$).
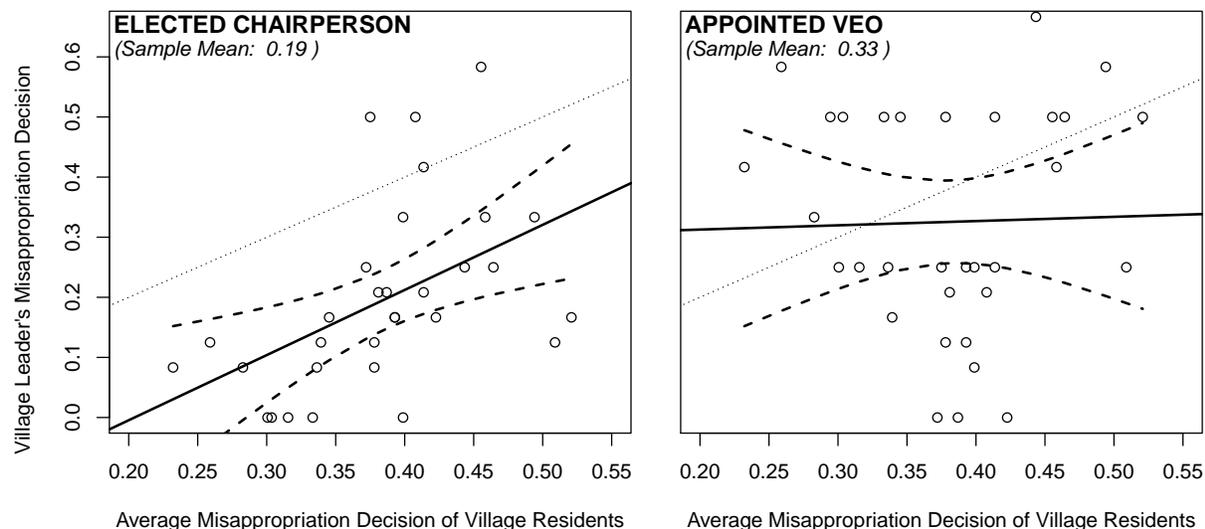

Figure 3: Village leaders' misappropriation decisions vs. average misappropriation by ordinary residents in their village (co-villager/non-attributable treatment, Hanang and Mufindi districts). Most elected chairpersons (left) are below the dotted identity line, i.e. they misappropriate less than the average resident in their village. This is not the case for appointed VEOs (right).

Part 2: Identifying the Effects of Social Incentives

We next turn to the question if village leaders' willingness to misappropriate contributions is determined by their intrinsic preferences, or shaped by extrinsic social incentives or accountability pressures. As the previous section has shown, village chairpersons are intrinsically less willing to misappropriate public funds than externally appointed VEOs or ordinary village residents. But how will their misappropriation decisions change if their choices can have consequences for their reputations, social interactions and (in the case of village chairpersons) their re-election prospects outside the experiment? Will elected and non-elected village leaders continue to behave differently?

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Experimental Design

To test how village leaders' willingness to misappropriate public funds is influenced by social incentives, two cross-cutting experimental manipulations were imposed on the aforementioned decision situation, resulting in a 2×2 factorial design. First, it was varied whether study participants' misappropriation proposals were guaranteed to remain confidential, or if they would, ex post, be personally attributable to them by the other group members, if they had been selected. Second, it was varied whether groups consisted exclusively of co-villagers, or exclusively of mutual strangers who were all from different villages. This resulted in a total of four experimental conditions: co-villager/attributable, co-villager/non-attributable, stranger/attributable, stranger/non-attributable. Similar cross-cutting manipulations of observability and social distance have been used in the literature to distinguish between preference- and incentive-based motives for pro-social behavior, for example in laboratory dictator games (Habyarimana, Humphreys, Posner et al., 2004; Charness and Gneezy, 2008; Leider, Möbius, Rosenblat et al., 2009) and by Jakiela and Ozier (2016) in a field setting.

A detailed and formal explanation of the identification strategy is provided in the appendix (SI.5). In brief, the research design logic is as follows: In the co-villager setting, personal attributability of their misappropriation proposals forces study participants to anticipate the consequences of their decisions for their reputations and future social interactions in their village, after the experiment (Ligon and Schechter, 2012; Sircar and van der Windt, 2013; Sircar, Turley, van der Windt et al., 2018). This is not the case in the stranger setting, because mobility in rural Tanzania is low, distances are large and it is highly unlikely that people would be able to identify a random individual from a different village by name. However, attributability can also have independent behavioral effects in both the co-villager and stranger conditions. Even minimal cues of observability can be sufficient to modify individuals' behavior in social dilemma situations (Bateson, Nettle, and Roberts, 2006; Rigdon, Ishii, Watabe et al., 2009). To separate the impact of social incentives, which occur in interactions with co-villagers, from any independent behavioral effects of attributability, the analysis focuses on the interaction effect of the co-villager and attributability treatments.

The co-villager and attributability treatments were designed as follows: In the *co-villager condition*, all eight group members were from the same village. In the *stranger condition*, no two members were from the same village. In both conditions, the group members were unidentified ex ante. Study participants were only informed of whether the other seven group members were co-villagers from their own village or mutual strangers from seven different villages in the district. To obscure the group members' identity in the co-villager setting, the decision exercise was carried out simultaneously with two other very similar experiments, for which the same sampling procedure and the same survey instruments were used. Since the combined research activity included a total of 46 adult residents and two leaders in every village, study participants were unable to infer ex-ante who was assigned to their group of eight, even if they knew who else in their village was among the 48 research participants.

The *non-attributable* and *attributable* conditions differed by whether study participants' misappropriation proposals were guaranteed to remain confidential, or would be revealed ex post to the other members of the group, in case they were randomly selected for implementation. In the non-attributable condition, only the amount misappropriated would be indicated to the group, but not the identity of the group members or the identity of the person whose misappropriation proposal was selected. This was achieved by collecting study participants' decisions in confidential, one-on-one settings and instructing the interviewers to approach the village leaders discreetly after the decision exercises had been completed with the other participants in their village. In the attributable condition, study participants were told that not only the amount of group money that had been misappropriated would be indicated to the group, but also the name of the group member whose rent extraction proposal had been selected. Thus, in the non-attributable conditions, study participants could not be held accountable for their decisions by others, since they could always pretend that someone else's misappropriation proposal had been selected.

Before study participants were allowed to proceed with their decisions, their understanding was verified through comprehension checks. Full comprehension of the experimental condition was both a methodological and an ethical requirement. It allowed study participants to assess whether their decisions could have consequences for their future social interactions and reputations in their

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village, and to adjust their decisions accordingly.

The risk that village leaders' choices in the experiment were affected by interference from outside authorities is negligible, because the stakes in the decision exercise were low and misappropriation of contribution in an experiment is not formally illegal. Thus, citizens had no basis to file a complaint about a village leader's behavior in a research study, and local authorities had no reason or legal mandate to act upon such information.

The four experimental conditions were randomly assigned at the individual level, with equal probabilities. To accomplish this with a modest sample size per village, all study participants had to take part in the decision exercise four times, each time in a different experimental condition and with a different group. The order of the four experimental conditions was randomized at the individual level. Since study participants' decisions in their second, third and fourth exercises were inevitably affected by conditioning effects from their prior decisions, only the data from study participants' first decisions are used to draw causal inferences, in a between-subjects design. Table SI-1 shows that the four experimental groups are well balanced with respect to village leaders' baseline characteristics. Within-subject variation, using the data from study participants' later decisions, is leveraged to validate the experiment, as explained in Part 3.

Results

Figure 4 visualizes the average effects of the attributability treatment on study participants' willingness to misappropriate contributions in the co-villager and stranger conditions. To account for the censoring of misappropriation proposals at zero and one, which is potentially exacerbated by the low stakes in the experiment, Figure 4 reports Tobit coefficients. Tobit coefficients estimate the effects on study participants' *latent* willingness to misappropriate contributions, rather than on average misappropriation, which is affected by floor- and ceiling effects. That being said, the Tobit coefficients do not differ substantially from OLS estimates (Table SI-2).
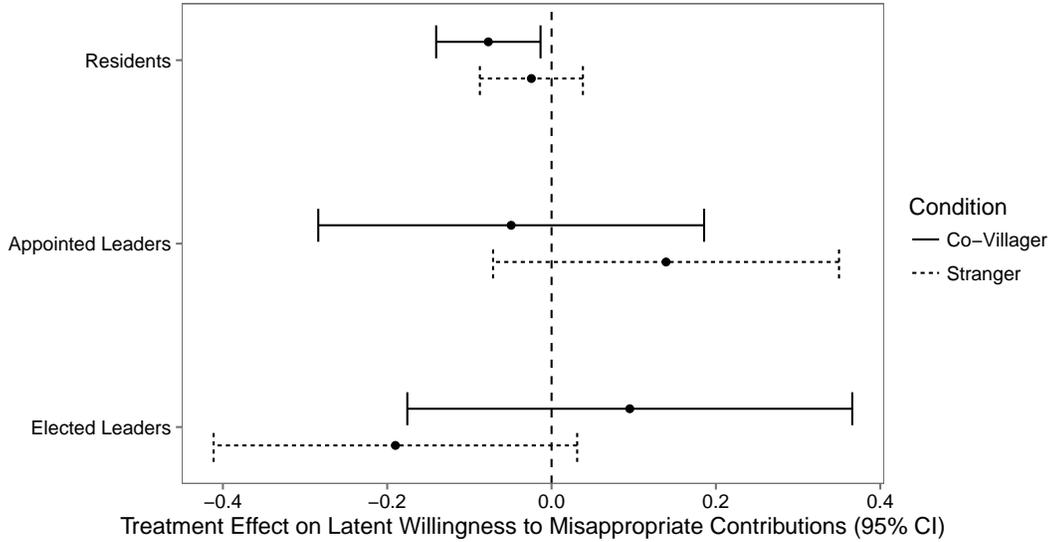
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Figure 4: Effects of the attributability treatment in the co-villager and stranger settings on latent willingness to misappropriate contributions. Estimated via Tobit regression with district fixed effects (Table SI-2).

For ordinary residents, the attributability treatment significantly reduces misappropriation in the co-villager condition (by an estimated 6 percentage points, from 38 percent in the non-attributable condition), but to a weaker extent also in the stranger condition. The interaction effect remains insignificant. This means that social incentives have no detectable average effect on village residents' willingness to misappropriate their co-villagers' contributions. However, it does not imply that the impact of social incentives is uniformly zero – it could be heterogeneous.

For village leaders, the estimates are much noisier, due to the smaller sample size, but potentially also due to treatment effect heterogeneity. For appointed VEOs, attributability appears to decrease misappropriation in the co-villager setting, but to increase it in the stranger setting. In neither condition, these effects are statistically significant. For elected chairpersons, these relationships seem to be reversed. Interestingly, the pattern is similar in each of the three districts covered by the study (see Table SI-3).

The interaction effect of the co-villager and attributability treatments is negative for appointed VEOs and positive for elected village chairpersons, although in both cases there is a non-negligible

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probability of no interaction effect ($p = 0.099$ for elected chairpersons and $p = 0.235$ for appointed VEOs). Thus, we cannot conclusively determine that social incentives had a nonzero average effect on either appointed VEOs or elected chairpersons.

Theoretically, social incentives should have heterogeneous effects on individuals' choices, because their underlying preferences differ. If a person is intrinsically motivated to conform to a particular social norm, there is no reason why social incentives should cause them to act differently. By contrast, if a person's preferences are at odds with what is deemed socially appropriate, they will have an incentive to deviate from their preferences. If a village community tolerates or even encourages some level of misappropriation, the influence of social incentives could go both ways. Village leaders who are intrinsically less willing to misappropriate contributions than their community expects them to might have an incentive to misappropriate more in public than in private. Village leaders who are more selfish than their co-villagers are willing to accept might have an incentive to refrain from misappropriation in public. These possibilities are discussed later in the article.

Despite the inconclusive average effects for both types of village leaders, it can be rejected that elected chairpersons are more strongly disciplined by social incentives than appointed VEOs. The probability that the co-villager and attributability treatments have a more negative interaction effect for chairpersons is $p = 0.028$ (one-sided t-test on Tobit coefficient, see Table SI-4). Unlike in the co-villager/non-attributable condition, where elected chairpersons significantly less willing to misappropriate contributions than appointed VEOs, their misappropriation choices in the co-villager/attributable condition are no longer statistically distinghishable. If their co-villagers can find out about it, VEOs propose to misappropriate on average 33 percent of contributions, elected chairpersons 31 percent and ordinary residents 32 percent.[4] Thus, once social and electoral incentives come into play, the more pro-social preferences of elected village leaders seem to no longer make a difference.

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[4]Restricting the data to Hanang and Mufindi districts for comparability.

Part 3: Comparing Misappropriation in the Experiment and in Natural Settings

Given the potentially ambiguous effects of social incentives, how can we know if social incentives actually matter, or if village leaders' misappropriation of public resources is merely driven by their intrinsic preferences? To investigate this, village leaders' choices in the experiment are compared to misappropriation outcomes in natural settings. The basic idea is the following: If village leaders' individual responses to social incentives in the experiment are predictive of their rent extraction behavior in natural settings (outside the experiment), two conditions must have been satisfied: First, the experiment was sensitive enough to pick up the influence of social incentives on village leaders' choices. Second, social incentives actually matter for real-world outcomes – misappropriation outcomes cannot just be driven by village leaders' social preferences. A comparison between the quantities of interest in the experiment (village leaders' misappropriation preferences and their response to social incentives) and village leaders' misapproriation choices in natural settings is therefore not only an indirect test of whether social incentives matter, but it also helps to validate the experimental design.

Empirical strategy

To examine the relationship between villager leaders' behavior in the experiment and in natural settings, the study leverages data on within-subject changes in village leaders' misappropriation decisions across their four experimental treatments. Although within-subject comparisons cannot identify individual-level treatment effects in a descriptive sense, because later decisions can be confounded by carry-over or panel conditioning effects from earlier decisions,[5] they do contain valuable information about heterogeneity in individual-level treatment responses.

The double-difference in village leader's misappropriation decisions c_i across their four experimental conditions (co-villager/attributable, co-villager/non-attributable, stranger/attributable, stranger/non-

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[5]Subjects may use earlier decisions as reference points for later decisions or may be primed by prior treatments, so prior experimental conditions can influence later decisions.

attributable) can be understood as the sum of the true individual-level effect of social incentives a_i^* and unobservable carry-over or panel conditioning effects from prior decisions u_i.

$$c_i^{ca} - c_i^{cn} - (c_i^{sa} - c_i^{sn}) = a_i^* + u_i$$

Let's denote this quantity by $\tilde{a}_i$, i.e. $\tilde{a}_i = a_i^* + u_i$. The carry-over and conditioning effects u_i make it impossible to interpret $\tilde{a}_i$ descriptively as individual-level treatment effects. However, in comparisons *between* subjects, $\tilde{a}_i$ can be thought of the best-available measurement of individual-level treatment effects, with u_i being the measurement error.

Consider the regression model

$$r_i = \beta_0 + \beta_1 \tilde{a}_i + \epsilon_i$$

where r_i are real-world rent extraction outcomes in the village. Since u_i is caused by the random sequencing of treatments within the experiment (the treatment order is randomized at the individual level), it is safe to assume that it is independent of unobservables ϵ_i outside the experiment, implying that $E[u_i \epsilon_i] = 0$. Under this assumption, the coefficient β_1 estimates the relationship between a_i^* (the extent to which social incentives constrain a village leader's misappropriation choices in the experiment) and r_i (real-world rent extraction outcomes outside the experiment) with attenuation bias. Attenuation bias implies that it will be more difficult to reject the hypothesis that $\beta_1 = 0$.

This gives us a conservative test of whether the behavioral responses observed in the experiment actually matter for real-world outcomes, i.e. of the construct validity of the experiment. The validation strategy consists of two parts: convergent and discriminant validation (see e.g. Adcock and Collier, 2001, 540f). The first step is to examine if the effects of social incentives observed in the experiment correlate with real-world rent extraction outcomes, i.e. to test if $\beta_1 > 0$ (despite the presence of attenuation bias). If village leaders' misappropriation choices are *not* constrained by social incentives, they should be driven by their rent extraction preferences, measured by their rent extraction decision in the co-villager/non-attributable condition, c_i^{cn}. Therefore, two additional specifications are reported, in addition to the above-mentioned model: A regression of r_i on c_i^{cn},

as well as on both $\tilde{a}$ and c_i^{cn} (Tables 2 and 3). Bounding the sign and magnitude of measurement error bias is more complicated, if the measurement error of multiple regressors is correlated, but under realistic assumptions the hypothesis tests remain conservative (Appendix SI.6).

The second step of the validation strategy is to verify if these observational relationships are consistent with expectations, i.e. that the correlations between village leaders' behavior in the experiment and real-world outcomes only exist where they would be expected. For that purpose, the analysis exploits differences in the formal responsibilities of elected chairpersons and appointed VEOs. The village chairperson's behavior in the experiment should be more correlated with rent extraction outcomes in the chairperson's domain of responsibility, but not as much with the rent extraction outcomes in the VEO's domain of responsibility, and vice versa. In terms of the above regression model, we should expect $\beta_1^{\text{CHAIR}} < \beta_1^{\text{VEO}}$ for measures of rent extraction that are within the elected chairperson's domain of responsibility, and $\beta_1^{\text{VEO}} < \beta_1^{\text{CHAIR}}$ for measures of rent extraction that are within the appointed VEO's domain of responsibility. To preempt concerns about data mining, the validation strategy and outcomes of interest were specified in a pre-analysis plan (see Appendix SI.7).

Data

As a measure of rent extraction in the VEO's predominant area of responsibility, Table 1 uses a survey-based measure of village residents' trust that contributions to village projects are managed well. Since the management of projects and contributions is the responsibility of appointed VEOs, residents' trust should primarily depend on the VEO's misappropriation behavior, rather than the chairperson's. On a scale from 1 (do not trust at all) to 4 (trust completely), the average level of trust is 2.49. By village, these estimates range from 1.73 to 3.47.

As a measure of rent extraction by elected village leaders, Table 2 uses the fraction of eligible households who were offered an agricultural input voucher by the village government in 2012, approximately 12-18 months prior to this study. In 2012, the National Agricultural Input Voucher Scheme (NAIVS) made subsidy vouchers for seeds and fertilizer available to farm households in

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selected areas of the country, including the three districts from which the study villages were sampled. Village governments received a contingent of vouchers in proportion to the number of eligible farm households within their village. Within villages, the allocation process left significant room for embezzlement by village leaders. The distribution was organized at the discretion of the village government, via a village voucher committee that was elected by the village assembly and reported to the village chairperson. Awareness of the eligibility criteria for input vouchers among the intended beneficiaries was low (Malhotra, 2013). Ample anecdotal evidence suggests that voucher committees favored the families of elected leaders, by allocating large numbers of vouchers to multiple members of their households, or listing infants or deceased people as voucher recipients. Pan and Christiaensen (2012) estimate that 60 % of vouchers in the Kilimanjaro region in 2009 were allocated to the households of elected village officials and 16 % to the households of members of the village voucher committees. The more the distribution of vouchers is skewed in favor of the village chairperson and the people close to him or her, the lower should be the proportion of randomly sampled village households who report that they have been offered vouchers by the village government. In the data collected for this study, the estimated proportions of voucher recipients among randomly sampled households vary considerably by village, ranging from 4 % (s.e. 4.7) to 54 % (s.e. 7.4).

Results

Consistent with expectations, village residents have greater trust that their contributions to village projects are managed well, the more the VEO's misappropriation in the experiment was reduced by social incentives (Table 1). For every ten percentage points by which social incentives reduced the VEO's misappropriation, residents' average self-reported trust was between 0.02 and 0.1 points higher (95 percent CI), on a scale from one to four (Table 1, Column 6). While the effect appears small, it should be kept in mind that it is estimated with attenuation bias. Furthermore, there is no evidence for such an association with the village chairperson's behavior in the experiment (Table 1, Columns 2 and 3). Finally, the validation results can be reproduced even if the relationships are estimated separately in every district (see Table SI-7). However, in Hanang district it is the VEO's

misappropriation preference, rather than the VEO's response to social incentives, that is strongly associated with residents' trust, suggesting that social incentives are weaker there.

	Residents' Trust that Contributions are Managed Well					
	(Scale: 1=Do not trust at all... to 4=Trust completely..., mean=2.48)					
	Elected Chairperson			Appointed VEO		
	(1)	(2)	(3)	(4)	(5)	(6)
OLS coefficients						
Effect of social incentives on leader behavior in the experiment $c^{ca} - c^{cn} - (c^{sa} - c^{sn})$		0.1 (0.18)	0.066 (0.17)		-0.6* (0.22)	-0.6* (0.21)
Leader's misappropriation preference c^{cn}	-0.38 (0.34)		-0.35 (0.35)	-0.25 (0.28)		-0.26 (0.26)
District & interviewer effects	yes	yes	yes	yes	yes	yes
Individual-level controls	yes	yes	yes	yes	yes	yes
Observations	463	463	463	783	783	783

*The table reports coefficients from OLS regressions. Standard errors in parentheses (adjusted for clustering by village). Individual-level controls are gender, age, age squared and years of education. Omission of the controls does not alter the conclusions (Table SI-5). *$p < 0.05$ (two-sided).*

Table 1: VEOs' misappropriation in the experiment and village residents' trust that contributions are managed well.

Household was Offered an Agricultural Input Voucher
(sample restricted to farm households, mean=0.29)

	Elected Chairperson			Appointed VEO		
	(1)	(2)	(3)	(4)	(5)	(6)
Marginal Effects from Logit Regressions						
(evaluated at zero, holding the other variables at mean)						
Effect of social incentives on		-.21*	-.22**		-.00026	-.0026
leader behavior in the experiment		(.085)	(.074)		(.067)	(.060)
$c^{ca} - c^{cn} - (c^{sa} - c^{sn})$						
Leader's misappropriation preference	.025		-.085	-.10		-.10
c^{cn}	(.14)		(.19)	(.11)		(.10)
District & Interviewer effects	yes	yes	yes	yes	yes	yes
Individual-level controls	yes	yes	yes	yes	yes	yes
Observations	1354	1354	1354	1543	1543	1543

*The table reports marginal effects from logit regressions of whether a the respondent's household was offered an agricultural input voucher by the village government in 2012 on the elected village chairperson's decisions in the experiment (columns 1 and 2) and the appointed VEO's decisions in the experiment (columns 3 and 4). The sample was restricted to farm households. In Mpanda DC, two villages were excluded from the analysis, because they were not part of the National Agricultural Input Voucher Scheme in 2012. Standard errors in parentheses (adjusted for clustering by village).
$p < 0.05$ (two-sided).

Table 2: Rent extraction by elected village chairpersons.

Analogously, the misallocation of agricultural input vouchers was lower, the more social incentives reduced the elected chairperson's misappropriation in the experiment (or the less social incentives increased the chairperson's misappropriation, Table 2). If social incentives decreased the chairperson's misappropriation by ten percentage points (from a mean of 19.5 percent in the co-villager/non-attributable condition), the expected proportion of farm households that were offered a voucher package would be 0.7 to 3.6 percentage points higher (95 percent CI). As expected, no evidence for such a correlation with the VEO's behavior (Columns 5 and 6). The analysis controls for interviewer and district effects, as well as variables that could have influenced a household's chance of receiving vouchers through mechanisms other than misappropriation: The amount of land owned by the household, the number of vouchers allocated to the village per capita, whether

the household is female-headed, demographic characteristics of the respondent. The results are robust to omission of these controls (Table SI-6). They can can also be reproduced separately in each of the districts covered by the study (Table SI-8).

In sum, the validation tests in Tables 1 and 2 strongly support the claims that (1) the experiment measured a behavioral response that is predictive of rent extraction outcomes in natural settings (for both types of village leaders in the expected ways) (2) village leaders' rent extraction behavior is actually constrained by extrinsic social incentives, as measured in the experiment, rather than being solely a function of their intrinsic social preferences.

Discussion

In examining how preferences and incentives shape village leaders' willingness to misappropriate contributions to public goods, the article has covered a lot of ground. Table 3 summarizes the central results.

Social preferences and electoral selection effects

The first part of this study focused on village leaders' social preferences. It established that most village leaders are intrinsically willing to refrain from misappropriating contributions, at least to some extent. This is consistent with behavior that is commonly observed in trust games, where trustees, even in the absence of repeated interaction, typically return enough to make contributing worthwhile (Johnson and Mislin, 2011).

Furthermore, the data conveyed a remarkably clear result with regard to electoral selection effects. Elected village chairpersons were significantly less willing to misappropriate their co-villagers' contributions than externally appointed VEOs or randomly sampled village residents. These differences could be a consequence of voters' deliberate choices (who may be familiar with candidates' character from day-to-day interactions). Alternatively, they could be a by-product of voting on other characteristics correlated with pro-social preferences. Even the very experience of having been elected

General Hypotheses	Comparison of Elected & Non-elected Leaders
Social preferences	*Electoral selection effects*
Expectation: Even if nobody can hold them accountable for their actions, most village leaders will, at least to some extent, voluntarily refrain from embezzling public funds.	**Expectation:** If nobody can hold them accountable for their actions, elected village leaders will refrain from misappropriation to a greater extent than externally appointed village leaders or ordinary village residents.
Result: Confirmed.	**Result:** Confirmed. Chairpersons are intrinsically less willing to misappropriate public funds than VEOs or ordinary residents.
	Observation: More pro-social villages systematically elect more pro-social chairpersons.
Impact of social incentives	*Added effect of electoral incentives*
Expectation: If other people in their village can attribute their actions to them, village leaders respond differently than if strangers can attribute their actions to them.	**Expectation:** Conditional on their misappropriation preferences, elected village chairpersons are more strongly disciplined by social incentives than externally appointed VEOs.
Result: Inconclusive. It is possible that the effects are heterogeneous.	**Result:** Disconfirmed.
Validity of the experiment	*Differences between chairperson's and VEO's domains of responsibility*
Expectation: Village leaders' response to social incentives in the experiment correlate with misappropriation outcomes in natural settings (conditional on their intrinsic misappropriation preferences).	**Expectation:** Elected chairperson's misappropriation behavior in the experiment is predictive of misallocation of agricultural subsidy vouchers, appointed VEO's misappropriation behavior in the experiment is predictive of trust that project contributions are managed well.
Result: Confirmed.	**Result:** Confirmed.

Table 3: **Results Overview.**

could have caused village chairpersons to become more pro-social. Corazzini, Kube, Maréchal et al. (2014) suggest that elected leaders feel bound by campaign promises and therefore display different social preferences after having gone through an election. More speculatively, elected leaders' choices could be influenced by feelings of reciprocity towards their voters. Disentangling these potential mechanisms is beyond the scope of this study, but would be a promising objective for future research.

Finally, it was found that pro-social villages systematically elect more pro-social village leaders, but no such pattern existed with externally appointed VEOs. This result persisted even after correcting for the naturally-occurring statistical similarity among people from the same village, so it is not just a consequence of the fact that village chairpersons are locals, whereas some VEOs were originally outsiders who relocated to the village. Instead, the result could be a direct consequence of electoral selection, since more pro-social voters would plausibly care more about having a pro-social village chairperson.

Social and electoral incentives

The second part of the study focused on the impact of social incentives on village leaders' willingness to misappropriate their co-villagers' contributions. The results were ambiguous, but average effects might obscure important underlying heterogeneity. In interpreting the results and defining the future research agenda, several potential sources of heterogeneity should be considered.

- Some village leaders might have a stronger incentive to comply with social norms than others, because they depend more on the goodwill or cooperation of their co-villagers. Numerous variables could influence the balance of power between village leaders and residents, such as the personality of the village leader, the community's collective action capacity, and the leader's embeddedness in kinship or other social network structures.

- Calculated norm transgressions could be a way of asserting social power, if they are met with impunity. This could motivate village leaders (especially elected leaders whose position is less secure), to conspicuously misappropriate public funds.

- In clientelist systems, where politicians are expected to share the spoils of office with their supporters, being willing to divert or misappropriate public resources in public might be perceived as a signal of political competence.

- Some communities might regard village leaders as entitled to a certain share of public resources, as a type of informal compensation. Especially for elected village chairpersons (who, unlike salaried VEOs, are not formally compensated for their duties), any compensation would necessarily be informal. This could produce peculiar incentives: If a particular village perceives their chairperson to be entitled to a certain share of public resources, and this is what the village leader's informal compensation conists of, they would have every reason to stick to this bargain in public, even if privately they would prefer not to misappropriate anything.

In sum, we cannot assume that there is a social norm to always and completely refrain from misappropriation. Instead, there could be a distinct socially appropriate level of misappropriation in every community. Explaining variation in village residents' tolerance of misappropriation is beyond the scope of this article, but it is a promising field for future field-based behavioral research.

An second take-away point from Part 2 is that social incentives had less of a disciplining effect on elected village leaders than on appointed village leaders, in the sense that they did less to decrease their willingness to misappropriate contributions. In private, elected chairpersons were significantly less willing than appointed VEOs to misappropriate their co-villagers' contributions. In public, their behavior was statistically indistinguishable.

Comparing village leaders' behavior in the experiment and in natural settings

Part 3 removed some of the ambiguity about the impact of social incentives, by testing if village leaders' responses to social incentives in the experiment correlated with their misappropriation choices in natural settings. For both types of village leaders and only in their respective areas of responsibility, this was confirmed. However, it was not their intrinsic misappropriation preferences, but rather the added effect of social incentives that strongly correlated with misappropriation outcomes in natural settings. While these correlations should not be mistaken as causal inferences,

they are consistent with the idea that village leaders' misappropriation choices are actually influenced by social incentives, rather than being wholly determined by their social preferences, but the influence of social incentives is heterogeneous. Since none of these correlations could have been expected if social incentives either had no meaningful effect on village leaders' real-world misappropriation choices, or if the experiment had failed to identify these effects, these observational results also validate the experiment.

Conclusions

This article has examined the willingness of elected and non-elected village leaders to misappropriate public funds for private gain. Through a field-based behavioral experiment with the leaders of 48 villages in Tanzania, it shed light on the village leaders' social preferences, as well as on the impact of social incentives on their willingness to enrich themselves.

The article highlighted electoral selection effects: Elected village leaders were found to have more pro-social preferences than the average citizen or their appointed counterparts, and more pro-social villages systematically elected more pro-social village leaders. Village elections in Tanzania, despite being not always perfectly competitive, perform better at identifying public-spirited village leaders than the bureaucratic recruitment process of village executive officers (VEOs).

Favorable electoral selection effects, however, do not imply that elected leaders will necessarily engage in less corruption. In natural settings, village leaders cannot just follow their intrinsic preferences, but also have to consider the reactions of their co-villagers and the reputational consequences of their choices. To estimate the impact of these social incentives, the experiment manipulated if village leaders' misappropriation choices were confidential or attributable to them, and whether people from their own villages could attribute their choices to them, or strangers with whom they were unlikely to ever interact in person. The results were ambiguous. While social incentives may have caused some village leaders to refrain from misappropriating public funds, others may have had an incentive to misappropriate more in public than they would privately have chosen to.

Different potential explanations were discussed.

Social incentives, despite their potentially heterogeneous influence, seem to matter for real-world corruption outcomes. How the village chairperson and the VEO responded to social incentives in the experiment correlates with indicators of their corruption behavior in natural settings. This suggests that village leaders' willingness to misappropriate their co-villagers' contributions is not merely driven by their intrinsic social preferences, but also shaped by social incentives. However, those social incentives may not uniformly *reduce* misappropriation.

The broader implications of this study concern several important debates in the political economy of corruption and development: The ability of electoral democracy to contribute to anti-corruption, the role of informal accountability mechanisms, and the methodological advantages of field-based behavioral experiments. The study has shown that village elections can be successful at identifying public-spirited leaders, more so than centrally appointing them or selecting them at random from the population. However, some of the advantages of electoral selection appear to be offset by the social and electoral incentives village leaders are confronted with. It is therefore important to study not just the political selection process, but also the accountability pressures and informal social incentives local leaders are confronted with once they are in office. These insights will hopefully prompt future research on corruption by local leaders, and they also highlight the potential of field-based behavioral experiments as a method of examining elite behavior.

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