

What Motivates Village Leaders to Refrain from Misappropriating Public Resources?

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Abstract

This paper examines potential causal mechanisms through which democratic elections can affect local-level corruption. It compares the motives of elected and appointed village leaders to refrain from misappropriating public resources, considering both their intrinsic preferences and the extent to which their choices are constrained by concerns for their social reputations and future interactions in the village. Based on a behavioral experiment with local leaders in 48 Tanzanian villages, the paper shows that elected village leaders are intrinsically more motivated to refrain from rent extraction than externally appointed village leaders or ordinary citizens. However, the benefits of electoral selection can be undermined if elected leaders face incentives that discourage voluntary restraint.

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In many countries, the capacity for external oversight of local governments is very limited, as is the capacity for judicial enforcement against corrupt local leaders. In these environments, local decision makers have considerable opportunities to extract rents, for example by embezzling public funds, misallocating program benefits, or collecting bribes. Can democratic elections reduce such rent extraction, if top-down legal enforcement capacity fails to deter it? In this paper, I show that local elections can be remarkably effective at producing public-spirited leaders who are intrinsically motivated to refrain from rent extraction, compared to bureaucratic appointments of local leaders or their random selection among local residents. However, I also argue that the benefits of electoral selection can potentially be undermined by informal, non-electoral accountability pressures that discourage voluntary restraint.

Prior research on the impact of democratic elections has been inconclusive. Existing work has mostly focused on the *responsiveness* of leaders to their constituents (Martinez-Bravo et al., 2011; Grossman, 2014), or their *legitimacy* (Baldassarri and Grossman, 2011; Grossman and Baldassarri, 2012; Baldwin and Mvukiyehe, 2015). Only two studies have directly examined the impact of elections on the misappropriation of public resources by local leaders, by comparing localities with elected leaders to localities with non-elected leaders. Both yield ambiguous results. In Indonesia, Alatas et al. (2013) find no difference in the misappropriation of local development funds between villages with elected leaders to villages with centrally appointed leaders. In Afghanistan, Beath et al. (2014) find that the introduction of elected village councils did not decrease the embezzlement of food aid. These aggregate results raise important questions about the underlying mechanisms. If elected leaders engage in just as much rent extraction as non-elected leaders, does this mean that non-electoral accountability mechanisms can perfectly substitute for electoral accountability? Or does it mean that democratic elections are no more effective than other forms of political selection at identifying and retaining public-spirited leaders? Or do elected leaders face perverse incentives that are not conducive to eliminating rent extraction?

The contribution of this paper is to shed light on these potential mechanisms, by comparing the motives of elected and non-elected village leaders to refrain from rent extraction. I consider both their intrinsic motivation of to refrain from rent extraction (which can shed light on electoral

selection effects), and the extent to which their choices are constrained by anticipated bottom-up accountability pressures in their community. To compare the preferences and constraints of elected and non-elected leaders, I carried out a behavioral study with 77 elected and appointed village leaders in Tanzania, taking advantage of the fact that Tanzanian villages are governed jointly by a locally elected chairperson and an externally appointed village executive officer. Within their village, the two leaders face the same social environment and serve the same set of constituents. However, they differ in the way they were selected and in the means by which their constituents can hold them accountable.

By confronting elected and non-elected village leaders with an identical decision situation that involved an opportunity to actually misappropriate other people's contributions to a public good, I am able to measure village leaders' willingness to misappropriate public resources in a highly comparable manner and to contrast it with the behavior of randomly sampled ordinary citizens. To reveal study participants' intrinsic rent extraction preferences, the decision situation was designed to obscure the identity of the person who misappropriated the contributions. This shielded study participants from having to consider potential social sanctions, reputational consequences or electoral impacts that might occur if their choices became publicly known in their village.

Additionally, I test how village leaders' rent extraction behavior is affected by the need to anticipate that their co-villagers can hold them accountable for their decisions. To accomplish this, I manipulated whether study participants' rent extraction decisions could be personally attributed to them. This forced village leaders to anticipate the real-life consequences of their decisions for their reputations, future social interactions and reelection chances outside the experiment. However, since attributability can also have independent effects on pro-social behavior (Bateson et al., 2006; Charness and Gneezy, 2008; Rigdon et al., 2009), it is necessary to separate these independent behavioral responses from the effect of anticipated accountability pressures in the village. To do so, I additionally manipulated whether the people who can attribute the village leaders' rent extraction decisions to them are their co-villagers, or strangers with whom they are unlikely to ever interact in person. I focus on how the effects of attributability differ between the co-villager and stranger settings, as a measure of the extent to which village leaders' rent extraction decisions are

constrained by anticipated bottom-up accountability pressures in their village.

The experiment yields several novel insights. First, I find that elected village leaders are intrinsically more willing to refrain from rent extraction than externally appointed village leaders or the average adult citizen from their village. This finding is important, because it is the first quantitative evidence that the social preferences of elected leaders differ from those of non-elected leaders and the general population. Furthermore, it demonstrates that even in a weakly institutionalized and single-party dominated environment, village elections can be effective at identifying public-spirited leaders. Second, I find that externally appointed village leaders – who lack electoral incentives and cannot easily be fired – nevertheless have an incentive to refrain from misappropriating contributions, if they need to anticipate consequences of their choices for their reputations and future social interactions in the village. This finding is important, because it is the first direct experimental evidence that non-elected leaders are constrained by informal, bottom-up accountability pressures in their communities. It complements prior research on role of informal social sanctioning capacity in local-level governance, which had been limited to indirect, observational comparisons (Vedeld, 2000; Miguel and Gugerty, 2005; Tsai, 2007). Third, I find no evidence that the need to anticipate their co-villagers’ reactions causes elected village leaders to refrain from misappropriating contributions any more than they would intrinsically be willing to. This could partially be a consequence of electoral selection effects, but I argue that selection effects cannot be the whole story. Individual responses to village-level accountability pressures are heterogeneous, and some elected leaders appear to have an incentive to engage in greater rent extraction if their co-villagers can hold them accountable. I draw on prior literature to identify several potential explanations, including clientelistic norms, status concerns or conformity pressures.

Methodologically, this paper differs in important ways from other “lab-in-the-field” studies. First, it does not examine the causal effect of a hypothetical, simulated, or highly stylized treatment. Instead, it could be described as a “field-in-the-lab” experiment, because it evaluates the effect of a real-world treatment (anticipated social interactions outside the experiment) on individuals’ behavior in a laboratory-like stylized and precisely observed decision situation. Second, it is one of only very few behavioral studies conducted on actual local leaders (see also Baldassarri and

Grossman (2013); Beekman et al. (2014); Grossman (2014) and Kosfeld and Rustagi (2015)). This is important, because the social preferences of elites can differ substantially from those of the general population (Fehr and List, 2004). Third, unlike most other behavioral experiments, this study incorporates an explicit strategy to validate that the experimental results are informative about analogous outcomes in natural settings, leveraging within-subject variation in the treatments and original data on rent extraction outside the experiment.¹ By doing so, this study is also the first to measure and quantify the effect of formal and informal accountability pressures on leader behavior in a cross section of communities.

The paper proceeds as follows. I first outline two alternative theoretical perspectives on rent extraction by village leaders and their observable implications with regard to the impact of democratic elections on rent extraction. Second, I explain why village governance in Tanzania is a particularly advantageous test case. Third, I provide details on the research design and the experimental procedures. Fourth, I compare village leaders' intrinsic motivation to refrain from rent extraction. Fifth, I examine to what extent the rent extraction decisions of elected and appointed village leaders are constrained by external accountability pressures, i.e. the need to take the reactions of their community into consideration. Sixth, I demonstrate that village leaders' behavior in the experiment correlates with their rent extraction behavior in natural settings, leveraging within-subject variation in the experimental treatments and original data on pre-specified measures of rent extraction in the villages. Lastly, I discuss the implications and limitations of these results, followed by a brief conclusion.

Elections and Rent Extraction at the Village Level

Corruption by village leaders can profoundly impact the everyday lives of many of the world's 3.3 billion rural residents, especially in low-income countries. Village residents in Tanzania were very forthcoming with examples when interviewed for this study. They complained that food aid

¹See also Grossman and Baldassarri (2012); Kosfeld and Rustagi (2015) as examples of behavioral experiments that are checked against observational data.

or agricultural subsidy vouchers were not handed out to the intended beneficiaries, 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 were embezzled from village coffers, and so forth. Throughout the study areas, village leaders were widely perceived as corrupt.²

Yet, village leaders often get away with corruption, even though it can be painfully obvious to local residents (Bierschenk and de Sardan, 1997; Platteau and Gaspard, 2003; Ensminger, 2007). This is particularly puzzling in the case of democratically elected village leaders, given that there is not only one, but several potential mechanisms through which elections could be expected to reduce rent extractions by local leaders. For instance, elections could facilitate the selection of public-spirited leaders who are intrinsically motivated to refrain from rent extraction (Besley, 2005), or cause incumbents to adopt more pro-social preferences (Corazzini et al., 2014). Alternatively, elections could create incentives for office-motivated incumbents to refrain from rent extraction (Ferejohn, 1986; Fearon, 1999; Ferraz and Finan, 2011; Myerson, 2015). Why, then, do elected village leaders appear to engage in just as much rent extraction as non-elected leaders (Alatas et al., 2013; Beath et al., 2014)?

Village-level governance is an important test case, because it could be considered a best-case scenario for bottom-up political accountability. While failures of electoral accountability have often been attributed to information asymmetries (see Pande (2011) and Gottlieb (2015) for excellent discussions of recent work), small-scale communities like villages 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 to hide corruption.

Moreover, there are fewer structural impediments to citizens' ability to sanction corrupt or non-performing politicians at the village level than at higher levels of governance. In many countries (including in Tanzania) village election laws include recall provisions that enable voters to replace local incumbents at any point in the electoral cycle. Furthermore, citizens can sanction local

²In 32 randomly sampled villages in Tanzania's Hanang and Mufindi districts, I asked residents 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.

leaders not only at the polls, but also continually in everyday social interactions (Tsai, 2007). This means that important theoretical obstacles to electoral accountability can easily be circumvented, including the discrete spacing of elections, problems of issue aggregation and especially term limits, low re-election probabilities or a lack of office motivation (Besley, 2006). If village leaders plan to spend the rest of their lives in their village, they should *always* have a strong incentive to remain in good graces with their constituents, even after their term in office and regardless of whether they seek reelection.

It is all the more puzzling why corruption and rent extraction remain widespread at the village level. Since obvious institutional impediments to citizens' ability to monitor and sanction village leaders are lacking, research should focus on a more fundamental question: Is it rent extraction by village leaders a problem of norm enforcement capacity, or rather a problem of local norms? In other words, do we observe corruption, because village residents lack the power to mount effective bottom-up accountability pressures on their leaders? Or do village residents not hold their leaders to high standards of public-spiritedness, even though they have the power to discipline them?

Distinguishing between these two alternative theoretical perspectives is the key to understanding how democratic elections can impact village-level rent extraction. If local leaders engage in rent extraction because citizens lack the power to discipline them, then the impact of elections on local rent extraction should primarily depend on their effectiveness at selecting public-spirited leaders. Electoral selection determines the extent to which incumbents are intrinsically motivated to refrain from rent extraction. Additionally, electoral selection shapes the electoral incentives office-motivated incumbents are confronted with (Fearon, 1999). For office-motivated incumbents, more efficient electoral selection should reinforce their incentives to refrain from rent extraction, because voters could be confident to identify a better alternative, should they decide to remove a corrupt incumbent. Hence, the extent to which democratic elections can reduce rent extraction should directly depend on their efficiency at identifying public-spirited leaders.

By contrast, if village-level corruption is primarily a problem of local norms, rather than a problem of norm enforcement capacity, electoral selection would play a fundamentally different role. The

more easily citizens can discipline incumbents, the less they would be compelled to elect leaders who voluntarily comply with social norms or expectations. Paradoxically, this means that in communities where village leaders are socially expected to extract rents (for example because of group-based loyalty obligations or claims for patronage benefits), electoral selection could plausibly favor public-spirited candidates, and yet incumbents might face social pressures to engage in rent extraction. In other words, if village residents have the ability to sanction local leaders non-electorally, electoral selection effects (and any resulting reelection incentives) could easily be offset by conflicting non-electoral accountability pressures. Hence, if local norms force village leaders to deviate from their preferences, elections might fail to reduce rent extraction, even if they perform well at selecting public-spirited leaders.

Thus, the two alternative theoretical perspectives on village-level rent extraction have different observable implications regarding the relationship between electoral selection effects and rent extraction outcomes. By disentangling selection effects (or differences in elected and non-elected leaders' intrinsic motivation to refrain from rent extraction) from the effects of bottom-up accountability pressures on village leaders' choices, we can therefore gain a better understanding of the conditions under which democratic elections can contribute to reducing village-level rent extraction. Elected village leaders could be simultaneously constrained by electoral and non-electoral accountability pressures, and those might create conflicting incentives. Studies of the impact of elections should therefore look beyond the narrow question whether reelection incentives discourage corruption by local incumbents [Ferraz and Finan \(2011\)](#). The importance of such electoral incentives should be seen in relation to elected leaders' *intrinsic* motivation to refrain from corruption, and in comparison to the incentives *non-elected* leaders would have been confronted with in the same situation.

Why Tanzania?

The Tanzanian case offers several distinct advantages to study village leaders' motives for engaging in or refraining from rent extraction. First, rent extraction is of great practical relevance. Village

governments regulate and manage village affairs very 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. From this time period, villages have inherited a set of externally imposed governance institutions that are identical throughout the country (see Appendix A-0). 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 (Iyer, 2010; Berger, 2009; Acemoglu et al., 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 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 the sources of leader behavior more directly, by exploring how heterogeneous social norms and norm enforcement capacity in different villages affect their decision-making.

Finally, the dual leadership structure of Tanzanian villages makes it possible to compare the behavior of elected and appointed leaders within the same village context. This is an important advantage, because it obviates the need to find counterfactual sets of villages in which local norms and norm enforcement capacity are independent of local leadership institutions.

Elected and Appointed Village Leaders

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). Villages additionally have an elected council and different purpose-specific committees (land management, budget, education, water, voucher allocation, etc.). The village assembly, constituted by all adult residents of the village, has the ultimate decision authority over village affairs. However, from a practical perspective, executive and agenda setting powers are concentrated among the chairperson and the VEO. While their formal roles differ (Osafa-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.³

A key difference between the village chairperson and the VEO is the mechanism by which they are selected. Village chairpersons are locally elected. They do not automatically receive a compensation for their duty and they can be removed either at regular elections or if the village petitions for recall.⁴ By contrast, VEOs are salaried public officials. They are selected based on formal qualifications by the district administration, three administrative tiers above the village. As public servants, VEOs cannot easily be dismissed. At worst, they can be posted to a different village. This can happen if a village makes persistent, collective complaints to the district administration. The hurdles for removing a VEO are therefore higher than voting an incumbent chairperson out of office, and the potential consequences for the VEO are mild, given that their position is exceedingly secure and they have the outside option of rotating to a different post if their village no longer tolerates them.

Both leaders should nonetheless have a strong incentive to maintain a good standing in their

³In 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.

⁴Village elections are organized within the village under the aegis of a district election officer. Public figures in the village (e.g. teachers) manage the polling processes and count the votes. For recall elections, the district election officer is invited as guest of honor to a village assembly. In the presence of the district election officer, village leaders can then be voted out of office on the spot (interview with a district election officer, August 2014).

community. In the performance of their duties, they depend on the quasi-voluntary cooperation of village residents (Lierl, 2015). In everyday life, their actions can have consequences for their personal reputations, their social status and their everyday social interactions in the village, as well as potentially for their families.

Research Design

To understand how the rent extraction behavior and the accountability of elected and non-elected village leaders differ, the preceding discussion has outlined three important research objectives. First, to measure and compare the rent extraction preferences of elected and appointed village leaders, i.e. their intrinsic motivation to refrain from misappropriating public resources. Second, to test whether village leaders' rent extraction decisions are determined by their preferences alone, or constrained by extrinsic incentives, originating from the need to anticipate the consequences of their choices for their reputations and future social interactions in the village (including their re-election prospects, in the case of elected leaders). I will refer to this category of extrinsic incentives as "bottom-up accountability pressures" (see also Serra, 2012). Third, to examine whether differences in village leaders' preferences and responses to accountability pressures in the experiment are associated with variation in rent extraction in natural settings, in a cross-section of villages.

Measuring village leaders' willingness to misappropriate public resources

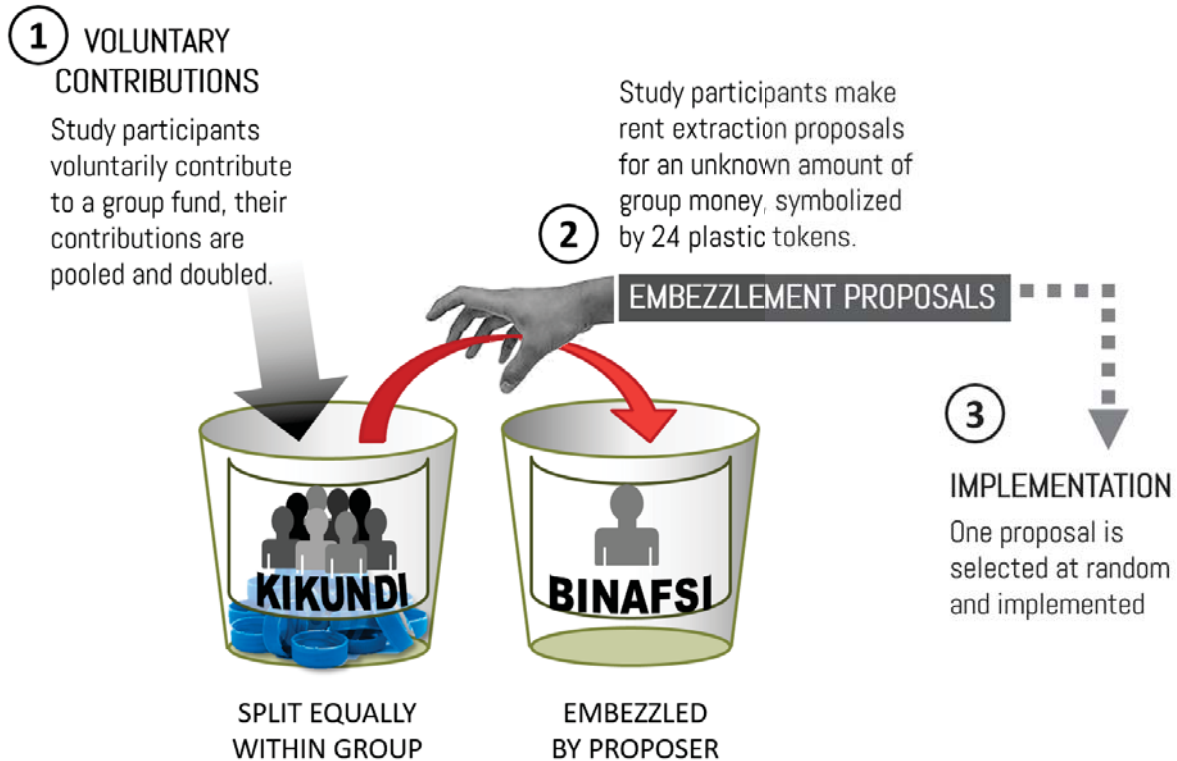
To measure study participants' willingness to misappropriate public resources, I confronted them with a generic, artificially induced decision situation in which they had the opportunity to embezzle other people's contributions to a public good. This decision exercise was carried out with the elected chairperson, the appointed VEO, as well as with randomly sampled adult citizens in every village. The decision exercise involved collecting monetary contributions from village residents and pooling the contributions into a group fund, where they were doubled by the experimenter. One study participant per group was given the opportunity to personally appropriate any fraction of

the group fund, and the remainder of the group fund was shared equally among all contributors. In practice, all study participants were asked to indicate what fraction of the contributed group fund they would misappropriate, if they were given the opportunity to do so. Eventually, one rent extraction proposal per group would be selected at random and implemented. It was known to the study participants that every group member’s proposal had an equal chance of being selected. Thus, by proposing to partly or wholly misappropriate the group fund, study participants could potentially enrich themselves at the expense of the other group members.⁵

The steps of the decision exercise are outlined in Figure 1. Various precautions were taken to ensure that the rent extraction proposals directly quantified study participants’ willingness to embezzle money from the group fund, minimizing confounding influences of framing effects, expectation formation, inter-temporal considerations, or social desirability bias. First, a set of comprehension checks ensured that all participants fully understood the logic of the exercise, before they were allowed to make their decisions (see instructions and comprehension questions in Appendix A-7). Second, the exercise was framed neutrally to the study participants as a “decision exercise”. References to concepts that could prime the study participants about social norms or the social consequences of their decisions were carefully avoided. The decision exercise was carried out in privacy. Besides the interviewers and research supervisors, no other individuals were allowed to be present, except for small children accompanying their parents. The interviewers were instructed not to watch the study participants during their actual contribution and rent extraction decisions. Afterwards, they would count the tokens in each jar and record this information on a tablet computer. Third, the total amount of available group resources was unknown to the study participants, so that information about the size of the pie should not have influenced decisions. Study participants knew only their own contribution, but not those of the other study participants. They indicated, in a simple, figurative way, what *fraction* of the group money they would capture. Additionally, study participants’ contribution decisions and their expectations about the size of the pie were also

⁵The stakes in the exercise were meaningful, but low enough to minimize the risk of serious conflict after the experiment, which was an ethical consideration. Across four decision exercises, study participants earned on average 6000 Tanzanian Shillings (TZS), which corresponded approximately to a daily wage for unskilled labor in the rural areas at the time of the study. The maximum amount study participants could theoretically appropriate in a single decision exercise was 16000 TZS, if all group members contributed their entire endowment, and one group member captured the entire group fund.

recorded (Appendix A-4). Fourth, to ensure that decisions were not affected by inter-temporal preferences, or by study participants' trust in the research team, 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 payoff included the fraction of their endowment they decided to keep for themselves in the contribution stage, plus their share of the group payoff, and for those whose misappropriation proposals had been selected, additionally the fraction of the group money they had allocated to themselves.



- (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.

In every village, study participants were divided into several different groups, and it was not revealed ex ante who the other group members were. To prevent study participants from being primed about their village leaders' participation, interviewers were instructed to approach the

village leaders discreetly after the decision exercises had already been completed with the other participants in their village. The decision exercise was preceded by a survey that asked about individual characteristics and respondents' daily life in the village.

Identifying the effect of village-level accountability pressures

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' rent extraction decisions were guaranteed to remain confidential, or personally attributable to them ex post by the other group members (if 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.

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. Once the decisions from all group members had been recorded, the individual payoffs would be calculated and the research team would return to the study participant's village on a specified date to disburse the payoff. 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.⁶

As long as study participants could be guaranteed that their rent extraction proposals would remain confidential and that their payoff would not be revealed to anyone else, study participants could always pretend that someone else's proposal had been selected. This means that in the non-attributable conditions, study participants could not be held accountable for their decisions by others. Rent extraction proposals in the non-attributable conditions therefore reflect study participants' own preferences, i.e. their intrinsic motivation to refrain from misappropriating money from the group fund, as opposed to potential extrinsic accountability pressures by which their decisions might otherwise be constrained. Rent extraction preferences could differ between the co-villager and stranger conditions, if individuals exhibit directed altruism or co-villager bias (Baldassarri, 2012; Baldassarri and Grossman, 2013; Binzel and Fehr, 2013), or if individuals have more information about their co-villagers than about strangers.

In order to identify the average effect of anticipated bottom-up accountability pressures in the village on study participants' rent extraction decisions, I examine how the effects of personal attributability differ between the co-villager and stranger conditions, holding everything else constant.⁷ In the co-villager setting, personal attributability of their rent extraction decisions forces study participants

⁶Before study participants were allowed to proceed with their decisions, their understanding of these conditions 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 village, and to adjust their decisions accordingly.

⁷Cross-cutting manipulations of observability and social distance have previously been used in laboratory experiments to distinguish between preference and incentive-based motives for sharing in dictator games (Habyarimana et al., 2004; Charness and Gneezy, 2008; Leider et al., 2009) and by Jakiela and Ozier (2012) in a field setting. The use of this technique on public decision-makers is novel, as is the use of behavioral experiments as a comparative measure of the extent to which local leaders' decisions are constrained by the anticipation of accountability pressures.

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). The chances of knowing a randomly selected co-villager by name are high, and information shared with other residents about a particular individual's rent extraction decision could be expected to spread fast within the village. This is not the case in the stranger setting. Since the mobility of most study participants is very limited and distances between villages in a district are large (up to several hours by all-terrain vehicle), it is very unlikely that group members in the stranger condition would know a particular group member, expect to consciously interact with her or him in the future, or take action to identify that group member's village to relay information about her or his decision.

By comparing the effects of attributability between the co-villager and stranger conditions, it is therefore possible to disentangle the effect of having to anticipate reciprocity, sanctioning or reputational consequences in future social interactions from any independent behavioral effects of attributability. This is important, considering that prior research has shown that minimal cues of observability alone can be sufficient to modify individuals' behavior in social dilemma situations (Bateson et al., 2006; Rigdon et al., 2009; Ekström, 2012). The identification strategy is formally explained in Appendix A.2, and its underlying assumptions were pre-specified in a pre-analysis plan (see Appendix A.6).

Given that the stakes in the decision exercise were low, the risk that external top-down accountability pressures on village leaders confound the interpretation of the experiment is minimal. While appointed VEOs are formally accountable to the district authorities, there is no legal basis for citizens to file a complaint about a village leader's behavior in a research study, nor for local authorities to act upon such information. Hence, if appointed VEOs refrain from rent extraction in the experiment because their co-villagers can attribute their decisions to them, then this must be due to accountability pressures that originate from within in the village, rather than from external, top-down oversight.

The four experimental conditions were randomly assigned at the individual level, with equal proba-

bilities. 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 are inevitably contaminated by treatment order and conditioning effects from their prior decisions, only the data from study participants' first decisions are used to draw causal inferences. Appendix Tables A.1.1-A.1.3 show that the four experimental groups are well balanced with respect to village leaders' baseline characteristics. The data from the later decisions are used for validation, as it will be explained after a presentation of the main results.

Sampling and Study Population

The study was carried out in three rural districts in different regions of Tanzania: Mpanda DC (Katavi), Hanang District (Manyara) and Mufindi District (Iringa). In each district, 16 villages were sampled at random.⁸ In Mpanda DC, seven adult residents per village were randomly sampled for the experiment, as well as the VEO, but the village chairperson participated in a different experiment. Subsequently, in Hanang and Mufindi, the number of randomly sampled adult residents per village was increased to 14 and both the VEO and the village chairperson were included in this experiment. Details on the sampling procedure are provided in Appendix A.7.

To persuade the village leaders to participate in the study, their participation was presented to them as an opportunity to also experience the decision exercise that had been carried out with their co-villagers, and to earn a payoff like the other study participants. In the 48 study villages, 46 VEOs participated in the study. In two villages, the position was vacant. Of 32 chairpersons targeted for this study, 31 agreed to participate, one was in the hospital.

⁸In Hanang District, 12 out of a total of 68 villages were excluded from the sampling frame, because they were not covered by the National Agricultural Input Voucher Scheme (NAIVS), to which an important observational outcome measure of the study relates. In Mpanda DC, one village collectively chose not to participate in the study. This village was replaced by a random village from the same ward.

Results

Elected Village Leaders Are Intrinsically Less Willing to Extract Rents

A comparison of study participants' rent extraction proposals in the co-villager/anonymous condition reveals that elected village chairpersons are intrinsically less willing to misappropriate contributions to a public good than externally appointed VEOs or ordinary residents (Figure 2, top panel). On average, elected chairpersons proposed to extract 20 percent of their co-villagers' contributions, whereas appointed VEOs extracted 36.5 percent and randomly sampled residents 37.8 percent. The hypothesis that elected chairpersons misappropriate their co-villagers' contributions at rates similar to appointed VEOs or ordinary residents from the same sample of villages can be rejected ($p = 0.046$ in both cases, using Mann-Whitney tests with Holm-Bonferroni adjustment for two comparisons, restricting the data to study participants' first decisions).⁹

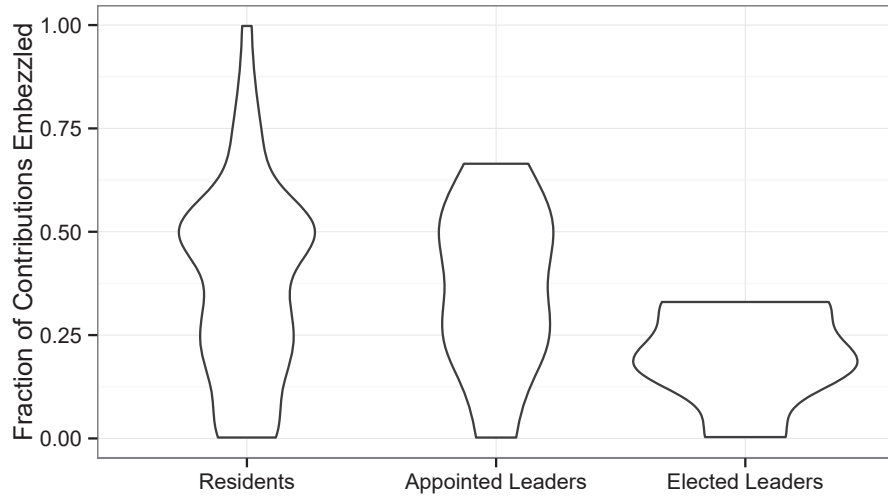
The more public-spirited preferences of elected chairpersons are also reflected in their contribution decisions in the first stage of the decision exercise, which were non-attributable in all experimental conditions (Figure 2, bottom panel). Elected chairpersons contributed more generously to the public good than ordinary residents ($p = 0.016$, Mann-Whitney test, all treatments, restricting the data to study participants' first decisions).¹⁰ This could be due to greater altruism towards their co-villagers, or due to greater trust in their co-villagers. However, their contribution behavior in the first stage is statistically indistinguishable from that of VEOs.

Finally, elected chairpersons' greater willingness to refrain from misappropriating contributions is also present in interactions with strangers. Even in the stranger/non-attributable condition, elected chairpersons are significantly less willing to misappropriate contributions than ordinary residents (although their behavior becomes statistically indistinguishable from that of appointed VEOs). This suggests that elected village chairpersons in Tanzania do not merely differ from the

⁹If study participants' second, third and fourth decisions are included (at the risk of introducing bias from carry-over effects between treatments), mean rent extraction by elected chairpersons' and ordinary residents remains similar (at 19.5 and 36 percent), whereas mean rent extraction by appointed VEOs' drops to 29.2 percent. The hypotheses that the elected chairperson's capture rate is similar to that of appointed VEOs ($p < 0.001$) or ordinary residents ($p = 0.004$) are rejected (Mann-Whitney tests, Holm-Bonferroni adjustment for two comparisons).

¹⁰ $p < 0.001$ if the data from later decisions are considered as well.

Rent Extraction Preferences (*co-villager/non-attributable condition*)



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

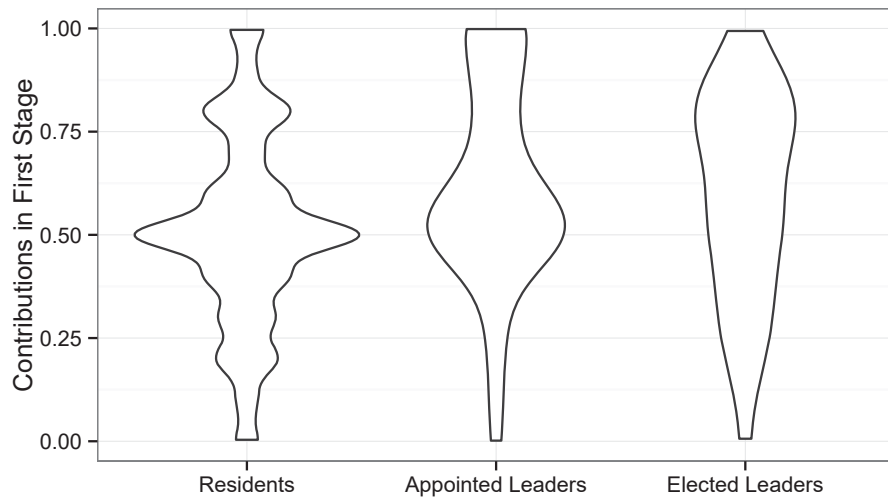


Figure 2: The violin plots contrast the distribution of rent extraction preferences (top) and contribution decisions in the first stage (bottom) for the different types of study participants.

population 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.

Do Village Elections Promote Public-Spirited Leadership?

To investigate whether villages systematically elect leaders who are more public-spirited than ordinary citizens, or whether this is merely an aggregate phenomenon, I compare the rent extraction preferences of elected village chairpersons to the mean and median rent extraction preferences of ordinary residents of their own villages. Within their respective villages, the elected chairpersons, but not the externally appointed VEOs, rank among the individuals who are least willing to extract rents in the co-villager/non-attributable condition.¹¹ In only two out of 32 villages the elected chairperson captured more than the median resident in the co-villager/non-attributable condition, whereas for VEOs this was the case in 13 out of the same 32 villages. The same pattern can be reproduced with mean, rather than median, rent extraction proposals among randomly sampled residents of the village (Figure 3). Thus, village elections systematically produce chairpersons who are more pro-social than other adult citizens in their villages, but this cannot be said about the appointing process of VEOs.

If voters who are more public-spirited are also more inclined to vote for a public-spirited leader, then we should expect that more pro-social villages systematically elect more pro-social chairpersons. The data confirm this expectation (Figure 3). Moreover, no such relationship exists for externally appointed VEOs. To rule out that the relationship between village residents' and their chairperson's preferences merely reflects naturally occurring intra-group correlation, I compare the sample correlation coefficient between the chairperson's rent extraction preference and the mean or median rent extraction preference among ordinary residents of their village ($r = 0.49$ in both cases) to the correlations between randomly drawn residents and the mean or median rent extraction preference among the other 13 ordinary residents in their village. Estimated via re-sampling, the probability

¹¹Within the sample from their village, the capture proposals of elected chairpersons in the co-villager/non-attributable condition ranked on average 11.5 out of 16, which is considerably lower than the capture proposals of appointed VEOs (average rank: 8.4 out of 16) or ordinary residents (average rank: 7.6 out of 16).

that the rent extraction preference of randomly sampled ordinary residents is as strongly correlated with the median or mean rent extraction preference in the village as that of the elected chairperson is very low ($p < 0.01$).¹²

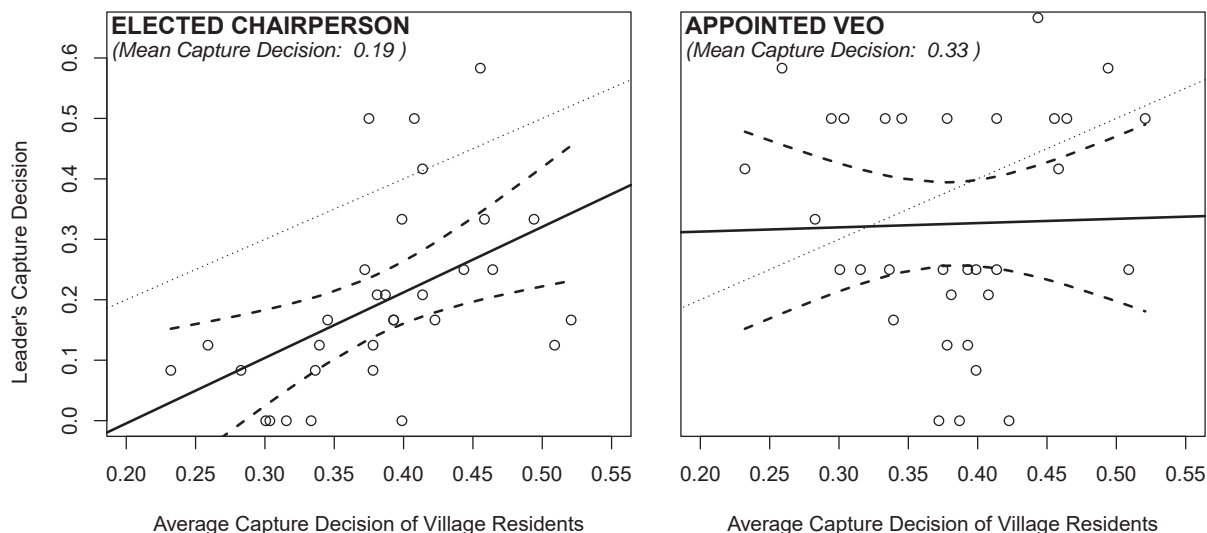


Figure 3: Village leaders' rent extraction decisions vs. mean rent extraction decision of ordinary residents in their village (co-villager/non-attributable treatment, Hanang and Mufindi districts). Left: Most elected chairpersons capture less than the average resident in their village (dotted line). More public-spirited villages tend to elect more public-spirited chairpersons. Right: Appointed VEOs do not always capture less than the average resident in their village (dotted line).

Elected Village Leaders Lack Incentives to Refrain from Rent Extraction

Even though elected village chairpersons are intrinsically less willing to misappropriate their co-villagers contributions than ordinary residents or appointed village executive officers, their average rent extraction decisions become indistinguishable once their co-villagers can hold them accountable for their choices (see Figure A.3.1 in the appendix). Knowing that their co-villagers will find out about it, elected chairpersons, like appointed VEOs, misappropriate on average 30 percent of their co-villagers' contributions to the public good. For elected chairpersons, this is an estimated

¹²Based on 1000 random draws, the expected Pearson correlation coefficient between the rent extraction preferences of ordinary residents and the mean or median rent extraction preference among the other 13 ordinary residents in the village is 0.071, which is similar to the correlation between the appointed VEOs' rent extraction preference and the mean ($r = 0.067$) or median ($r = 0.03$) rent extraction preference in their village.

ten percentage points higher than their average rent extraction proposal in the co-villager/non-attributable condition, although the estimate is noisy, given the small sample size (the corresponding p-value from a two-sided t-test is 0.51).

To rule out that the differences in village leaders' rent extraction behavior between the attributable and non-attributable conditions are merely driven by reactions to observability, rather than by anticipated future social interactions and reputational consequences, I compare the average effects of attributability between the co-villager and stranger settings. Figure 4 provides the most conservative treatment effect estimates. These estimates are noisy, possibly due to the small sample size, treatment effect heterogeneity, and floor and ceiling effects (given that village leaders were not dealing with large amounts of money in the experiment). For elected chairpersons, attributability appears to decrease average rent extraction in the stranger setting, while increasing it in the co-villager setting. For appointed VEOs, these relationships seem to be reversed. Interestingly, the pattern remains similar if each of the three districts are studied separately (Table A.3.2).

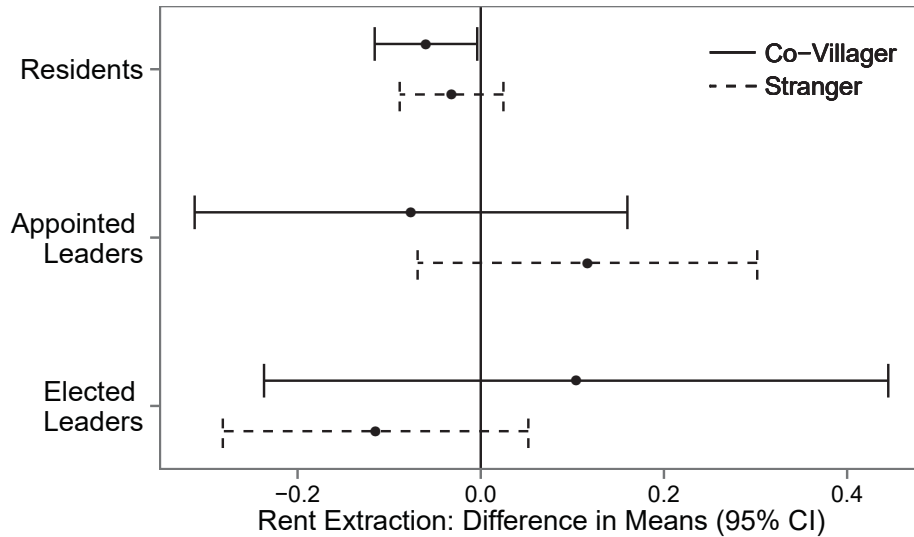


Figure 4: Average treatment effects of attributability in the co-villager and stranger settings.

To correct for the censoring of embezzlement proposals at zero and one (which might be due to the fact that village leaders were not dealing with large amounts of money in the experiment), Table 1 reports Tobit coefficients. Of interest is the interaction effect between the co-villager and

attributability treatments, which reflects to what extent village leaders' rent extraction decisions in the co-villager/attributable condition are constrained by actual, anticipated bottom-up accountability pressures in the village. This coefficient is negative for VEOs, suggesting that the need to anticipate accountability pressures in their village reduces average rent extraction by VEOs. By contrast, the coefficient is positive for the elected chairperson. The hypothesis that bottom-up accountability pressures in the village have a more negative average effect on rent extraction by elected chairpersons than on rent extraction by appointed VEOs can be rejected ($p = 0.023$, one-sided t -test on Tobit coefficient, see Table A.3.1). As a nonparametric alternative, I consider the sharp null hypothesis that there is no difference in the effects of attributability between the co-villager and stranger conditions for any VEO or chairperson. Among VEOs, this sharp null hypothesis can be rejected at $p = 0.042$, using a permutation test (re-randomizing the co-villager/stranger treatment labels). For elected chairpersons, it cannot be rejected ($p = 0.17$). Thus, at the very least, there is no evidence that the need to anticipate bottom-up accountability pressures in the village would reduce average rent extraction by elected village chairpersons, whereas this appears to be the case for appointed VEOs.

Rent Extraction Decisions			
<i>(Fraction of Contributions Captured)</i>			
	(1)	(2)	(3)
	Randomly Sampled Residents	Elected Chairpersons	Appointed VEOs
<i>Tobit coefficients</i>			
Co-villager	-0.0312 (0.0232)	-0.066 (0.11)	0.16 (0.10)
Attributable	-0.0211 (0.0222)	-0.13 (0.080)	0.23* (0.11)
Co-villager × Attributable	-0.031 (0.0364)	0.24 ⁺ (0.14)	-0.30* (0.15)
Constant	0.204*** (0.0461)	0.40** (0.11)	-0.15 (0.15)
Interviewer effects	yes	yes	yes
District effects	yes	yes	yes
<i>N</i> (study participants)	561	31	46
Missing observations	2	1	2
Censored at zero	61	5	6
Censored at one	10	1	1

Robust standard errors in parentheses (Column (1): clustered by village).

⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ (two-sided)

Table 1: Effect of having to anticipate accountability pressures on rent extraction decisions. The table reports coefficients from Tobit regressions of the fraction of contributions misappropriated on the experimental treatments and their interaction term, as well as interviewer and district indicators. OLS coefficients (Table A.3.3), and separate analyses by district (Table A.3.2) are reported in in the appendix. The data is restricted to subject’s first decisions (between-subject analysis) to prevent confounding by treatment order and panel conditioning effects.

Validation

To verify that the treatment effects in the experiment actually reflect village leaders’ anticipation of real-world accountability pressures in their village, I leverage within-subject changes in village leaders’ rent extraction decisions across the four experimental treatments and analyze their relationship with real-world rent extraction outcomes outside the experiment. Data on changes in

individuals' rent extraction behavior across their four decisions contain valuable information about heterogeneity in individual-level treatment responses. Heterogeneous effects in the experiment should be predictive of real-world rent extraction outcomes only if the experiment succeeded at identifying the influence of village-level accountability pressures on village leaders willingness to misappropriate public resources and such accountability pressures actually matter in practice.

For example, if changes in village leaders' behavior across the different experimental conditions are driven by the anticipation of real-world accountability pressures outside the experiment, we should expect village leaders' responses to the experimental treatments to depend on the social norms and expectations that prevail in their village, as well as on their co-villagers' ability to enforce such norms. More specifically, if village leaders were completely unconstrained in their rent extraction decisions, i.e. if they did not face any meaningful bottom-up accountability pressures in their village, we should expect that their real-world rent extraction behavior correlates primarily with their rent extraction preferences (which are measured in the co-villager/attribution condition), but, conditional on their rent extraction preferences, not with the changes in their rent extraction decisions across the different experimental treatments. Conversely, if village leaders' rent extraction choices in natural settings are constrained by bottom-up accountability pressures, their revealed rent extraction preferences in the experiment should have no bearing on real-world rent extraction outcomes. Instead, conditional on their rent extraction preferences, the changes in village leaders' rent extraction decisions across the experimental conditions might correlate with their rent extraction behavior in the real world, but only if the experiment actually makes such real-world accountability pressures salient.

It is important to note within-subject comparisons cannot identify individual-level treatment effects in a descriptive sense, because later decisions in the experiment can be confounded by carry-over or panel conditioning effects from earlier decisions. Thus, the difference in differences in a village leader's rent extraction decisions c_i across the four experimental conditions (co-villager/attribution, co-villager/non-attribution, stranger/attribution, stranger/non-attribution) should depend on both the unobservable individual-level causal effect of bottom-up accountability

pressures, denoted by a_i^* and the carry-over or panel conditioning effects from prior decisions.¹³

$$\tilde{a}_i = c_i^{ca} - c_i^{cn} - (c_i^{sa} - c_i^{sn}) = a_i^* + \underbrace{u_i}_{\text{Carry-over/panel conditioning effects}}$$

Carry-over/panel conditioning effects

While the carry-over and conditioning effects u_i make it impossible to descriptively interpret \tilde{a}_i as treatment responses at the individual level, they can be thought of as measurement error in comparisons across subjects. Given that u_i is mainly caused by the sequencing of treatments within the experiment, but the treatment order is randomized at the individual level, we can assume it to be largely independent of unobservables ϵ that determine the real-world rent extraction outcomes outside the experiment, implying that $E[u_i\epsilon_i] = 0$. Under this assumption, the regression model

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

estimates the relationship between the extent \tilde{a} to which a village leader is constrained by accountability pressures in the experiment and real-world rent extraction outcomes r with attenuation bias. Attenuation bias implies that it will be more difficult to reject the hypothesis that $\beta_1 = 0$.

Given that real-world rent extraction outcomes should depend on village leaders' rent extraction preferences, if village leaders do not face any meaningful bottom-up accountability pressures, I additionally report regressions of r on a measure of their rent extraction preferences (their rent extraction decision in the co-villager/non-attributable condition, c_i^{cn}), as well as of r on both \tilde{a} and c_i^{cn} (Tables 2 and 3).¹⁴ If either the experimental treatments failed to make bottom-up

¹³These carry-over or panel conditioning effects arise, because study participants may use earlier decisions as reference points for later decisions, and the experimental condition to which a study participant is exposed first may influence later decisions through priming effects. For example, if study participants are first exposed to the co-villager/attribution condition, they may have been primed to think about potential consequences of their choices outside the experiment, or the absence thereof, in later decisions.

¹⁴Bounding the sign and magnitude of measurement error bias is more complicated if we assume a model $r_i = \beta_0 + \beta_1\tilde{a}_i + \beta_2\tilde{p}_i + \epsilon_i$, where both a^* and village leaders' rent extraction preference p^* are measured with error and these errors are correlated, i.e. $\tilde{p}_i = c_i^{cn} = p_i^* + v_i$ and $E[u_i v_i] \neq 0$. In this case, let $\beta = (\beta_0, \beta_1, \beta_2)'$, $x_i' = (1, a_i^*, p_i^*)$ and $w_i = (0, u_i, v_i)'$. Then $E[\hat{\beta}] = \beta - E[(x_i x_i')^{-1}]E[w_i w_i']\beta$. Furthermore, $E[w_i w_i']\beta = (0, E[u_i^2]\beta_1 + E[u_i v_i]\beta_2, E[u_i v_i]\beta_1 + E[v_i^2]\beta_2)'$. Theoretically, we should expect that $\beta_1 \leq 0$ and $\beta_2 \leq 0$. Under this assumption, hypothesis tests will still be biased against a rejection of the null hypotheses $H_0 : \beta_1 \geq 0$ and $H_0 : \beta_2 \geq 0$, i.e. they will be conservative, as long as $E[u_i v_i] > 0$. However, even if $E[u_i v_i] < 0$, tests of the null hypotheses will remain conservative unless the

accountability pressures in the village salient, or if such accountability pressures had no substantive impact on the behavior of village leaders, then we should not expect a correlation between r and \tilde{a} (or $\beta_1 < 0$), conditional on village leaders' rent extraction preferences. In other words, rejecting $\beta_1 = 0$ can be thought of as a form of convergent validation to establish the construct validity of the experimental design (see e.g. [Adcock and Collier, 2001](#), 540f). If changes in village leaders' behavior across the experimental treatments can be shown to be correlated with rent extraction outcomes in natural settings, despite the presence of attenuation bias, it is plausible that the experiment actually measures the extent to which village leaders' rent extraction decisions are constrained by bottom-up accountability pressures in their village. To prevent concerns about data mining, the validation strategy and outcomes of interest were specified in advance in a pre-analysis plan (see Appendix A.6).

To rule out that the experiment picks up unknown village-level conditions that are somehow correlated with rent extraction outcomes in the village, but do not actually have anything to do with constraints on village leaders' choices, we can add further contrast by comparing the rent extraction behavior of elected chairpersons and appointed VEOs in the experiment to rent extraction outcomes within and outside their respective areas of responsibility. Given that elected chairpersons tend to have more influence over allocative decisions and appointed VEOs tend to have more influence over the management of village budgets, we should expect $\beta_1^{\text{CHAIR}} < \beta_1^{\text{VEO}}$ for measures of rent extraction that are within the elected chairperson's predominant area of responsibility, and $\beta_1^{\text{VEO}} < \beta_1^{\text{CHAIR}}$ for measures of rent extraction that are within the appointed VEO's predominant area of responsibility. This can be thought of as a form of discriminant validation.

As a measure of rent extraction in the VEO's predominant area of responsibility, I use a survey-based measure of village residents' trust that contributions to village projects are managed well. Since the management of projects and contributions is normally the responsibility of the appointed VEO, the extent to which residents in the village trust that their contributions to village projects are managed well should primarily depend on the VEO's rent extraction behavior, rather than the

correlation between u and v is so strong that the resulting additional bias completely offsets the attenuation bias that is still present. In the case of $H_0 : \beta_2 \geq 0$, this would only be the case if the bias term $E[(x_i x_i')^{-1}]_{[3, \cdot]} E[u_i v_i] \beta_1$ offsets the attenuation bias $E[(x_i x_i')^{-1}]_{[3, \cdot]} E[v_i^2] \beta_2$.

elected chairperson's. On a scale from 1 (do not trust at all) to 4 (trust completely), the average level of trust among ordinary village residents in the study is 2.49. By village, these estimates range from 1.73 (s.e.=0.24) to 3.47 (s.e.=0.26).

Consistent with the validation strategy, Table 2 shows that village residents' trust that their contributions to village projects are managed well is greater the more the VEO's misappropriation of contributions in the experiment was reduced by the need to anticipate accountability pressures. For every ten percentage points by which accountability pressures reduced the VEO's rent extraction in the experiment, residents' self-reported trust was, on average, between 0.02 and 0.1 points higher, on a scale from one to four (Table 2, Column 6). While this effect appears small, it should be kept in mind that it is estimated with attenuation bias. Furthermore, in line with expectations, there is no evidence for such an association with the behavior of the village chairperson (Table 2, Columns 2 and 3), but also no evidence against it. Finally, the validation results can be reproduced if the relationships are estimated separately by every district (see Table A.5.2).

Residents' Trust that Contributions Are Managed Well						
<i>(Scale: 1="Do not trust at all..." to 4="Trust completely...", mean=2.48)</i>						
	Elected Chairperson			Appointed VEO		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>OLS Coefficients</i>						
Effect of accountability pressures on leader behavior in the experiment $c^{ca} - c^{cn} - (c^{sa} - c^{sn})$.10 (.19)	0.057 (0.18)		-.60** (.22)	-0.64** (0.21)
Leader's rent extraction preference c^{cn}	-0.32 (0.36)		-0.29 (0.37)	-0.25 (0.30)		-0.26 (0.28)
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

Standard errors in parentheses (adjusted for clustering by village). ⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$ (two-sided)

Table 2: Rent extraction by appointed VEOs. The more village-level accountability pressures reduce the VEOs' willingness to extract rents in the experiment, the greater is village residents' trust that contributions to village projects are managed well in the real world. The table reports coefficients from OLS regressions. Individual-level controls are gender, age, age squared and years of education.

Omission of the controls or estimation via an ordered logit model do not alter the conclusions (Tables A.5.4 and A.5.5).

As a measure of rent extraction by elected village leaders, I use the fraction of eligible households who were offered an agricultural input voucher by the village government in 2012.¹⁵ Village governments were supposed to receive a contingent of vouchers in proportion to the number of eligible households within their village. The distribution of the vouchers to farm households in each village was organized at the discretion of the village government, via a village voucher committee (VVC) that was elected by the village assembly and typically reported to the village chairperson. Hence, this indicator can be assumed to be more reflective of rent extraction by the elected village chairperson than by the appointed VEO. The more the distribution of vouchers is skewed in favor of the village leaders and the people close to them, the lower should be the proportion of randomly sampled village households who report that they have been offered vouchers by the village government. In my data, the estimated proportions of voucher recipients among randomly sampled households vary considerably by village, ranging from 4 % (s.e. 4.7 p.p.) to 54 % (s.e. 7.4 p.p.).

¹⁵In 2012, the National Agricultural Input Voucher Scheme (NAIVS) made subsidy vouchers for agricultural inputs (improved seeds and fertilizers) available to farm households in selected areas of the country, including the three districts from which the study villages were sampled. Awareness of the eligibility criteria for input vouchers among the intended beneficiaries was very low (Malhotra, 2013). Ample anecdotal evidence suggests that voucher committees favored leaders' families, by allocating large numbers of vouchers to multiple members of the leaders' households, or by 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.

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)
<i>Marginal Effects from Logit Regressions</i>						
<i>(evaluated at zero, holding the other variables at mean)</i>						
Effect of accountability pressures on leader behavior in the experiment $c^{ca} - c^{cn} - (c^{sa} - c^{sn})$		-.21* (.085)	-.22** (.074)		-.00026 (.067)	-.0026 (.060)
Leader's rent extraction preference c^{cn}	.025 (.14)		-.085 (.19)	-.10 (.11)		-.10 (.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

Standard errors in parentheses (adjusted for clustering by village). ⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$ (2-sided)

Table 3: Rent extraction by elected village chairpersons. 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. The individual-level controls are variables that could have influenced a household's chance of receiving vouchers through mechanisms other than elite capture (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, as well as interviewer effects and district effects, see Table A.5.1). The results are nearly unaltered if the controls are omitted (Table A.5.6).

Table 3 shows that elite capture of agricultural input vouchers is lower the more the elected chairperson's misappropriation of contributions in the experiment was reduced (or the less it was increased) by the need to anticipate accountability pressures. If accountability pressures decreased the chairperson's rent extraction in the experiment by ten percentage points (from the mean embezzlement rate of 19.5 percent in the co-villager/non-attributable condition), then we would expect the proportion of randomly sampled farm households in the village who were offered a voucher package to be 0.7 to 3.6 percentage points higher. In the case of the VEOs, there is, as expected, no evidence in favor or against such a relationship (Columns 5 and 6). Again, the validation results can be

reproduced separately in each of the districts covered by the study (Table A.5.3).

Discussion

This paper raised the question whether rent extraction is a problem of norm enforcement capacity towards local leaders, or rather a problem of local norms in the villages. Collectively, the experimental results are more consistent with the second explanation. If citizens lacked the ability to sanction their village leaders, we would not expect the effects of attributability to differ between the otherwise identical co-villager and stranger conditions. Instead, the experiment shows that village leaders' rent extraction behavior – even that of non-elected leaders – depends on whether they have to take into consideration how their choices could affect their reputations and future social interactions in their village. Furthermore, the magnitude of this effect correlates with rent extraction outcomes in natural settings. This suggests that village leaders' rent extraction decisions are not exclusively determined by their social preferences. Instead, the incentives village leaders are facing when their co-villagers can hold them accountable for their rent extraction decisions offset the benefits of electoral selection. Some elected leaders appear to engage in greater rent extraction than they would be willing to if their choices could not be attributed to them.

These results have important implications regarding the mechanisms through which democratic elections could impact rent extraction by local leaders. The data show that village elections systematically produce leaders who are intrinsically less willing to misappropriate their co-villagers' contributions to a public good than ordinary citizens or appointed village executive officers. This finding is important in and for itself, because it provides direct evidence that the social preferences of elected leaders differ from those of non-elected leaders and the general population. It is also surprising, considering that village elections are often very weakly institutionalized and single-party dominated.

Two potential causal mechanisms could explain why elected village leaders are more public-spirited. Either, public-spirited candidates have a systematic advantage in village-level elections, or the expe-

rience of having been elected might influence or transform the preferences of incumbents (Corazzini et al., 2014). Qualitative evidence points in the direction of selection effects: In an open-ended survey question, most study participants mentioned public-spiritedness, along with assertiveness and the ability to mediate in conflicts, as examples of personal qualities that increase a person's chances of becoming a village chairperson. In close-knit village communities, these characteristics can easily be inferred from past social interactions or candidates' social reputations. Given the single-party dominance of the CCM in Tanzanian village elections, these selection effects have most likely occurred at the candidate selection stage. Case study evidence suggests that within-party primary elections at the village level can be quite competitive (Lund and Saito-Jensen, 2013, 107). Consistent with electoral selection effects, elected village leaders also differ on important social characteristics from appointed village executive officers and ordinary residents: They tend to be older, more likely to be male, have larger households, and live closer to the village center (see Tables A.2.1 - A.2.3 in the appendix).

The results further suggest that elected and appointed village leaders differ in how their rent extraction behavior is constrained by bottom-up accountability pressures in the village. Average rent extraction by appointed VEOs is reduced if they need to anticipate that their rent extraction decisions can have real-world consequences for their reputations and future social interactions outside the experiment. This is an important result, because it provides the first experimental evidence that informal, non-electoral bottom-up accountability pressures matter for the behavior of local leaders. Surprisingly, however, an analogous effect cannot be observed for elected village chairpersons, even though they face not only the possibility of informal sanctions in everyday life, but are additionally subject to electoral scrutiny.

In part, this difference might be due to selection effects. If elected chairpersons are intrinsically more willing than appointed VEOs to comply with their co-villagers' expectations, then they should be less constrained by bottom-up accountability pressures. However, a comparison of average effects might obscure important sources heterogeneity within either category of village leaders. The extent to which individual leaders' choices are constrained by bottom-up accountability pressures might not only depend on their social preferences, but also on the norms and norm enforcement capacity

of the community they live in. Consistent with this possibility, I find that the manner in which individual village leaders adjust their decisions in response to bottom-up accountability pressures in the experiment correlates with pre-specified measures of their rent extraction behavior in natural settings. This is the case for both the appointed VEO and the elected chairperson in their respective areas of responsibility. Although these correlations cannot be interpreted as causal relationships, they do suggest that (1) the interpretation of the key quantities of interest in the experiment is plausible, and (2) that variation in rent extraction across villages is in some way related to the village leaders' responses to accountability pressures. Hence, bottom-up accountability pressures might actually constrain elected leaders' rent extraction decisions, but in a way that causes some to misappropriate more than they would otherwise be willing to, while causing others to refrain from misappropriation.

Why would some elected village leaders become more willing to extract rents if their co-villagers can hold them accountable for it? One possibility is that rent extraction is socially expected from them or electorally rewarded. In a clientelistic setting, publicly visible rent extraction might help elected leaders assert their social status. It could also signal competence, if voters expect their leaders to capture rents and redistribute them to their supporters. Under this clientelist logic, elected and appointed village leaders would face different types of accountability pressures, because only elected chairpersons would need to serve a clientelistic support base within the village, whereas VEOs would merely need to stay out of trouble by minimizing complaints from their village. A second possibility is that rent extraction by elected chairpersons is not considered corruption, but rather a type of informal compensation. Contrary to appointed VEOs, elected village chairpersons are not formally compensated for their duties. Thus, if rent extraction is a socially accepted form of remuneration for elected leaders, elected leaders may have an incentive to publicly assert their claim to a certain share of public resources, even if privately they do not have a preference for rent extraction. A third possibility is that village leaders strive to conform to how ordinary residents would act, if their choices are personally attributable to them. Since elected chairperson extract on average fewer rents in the co-villager/non-attributable condition than ordinary residents in the co-villager/attributable condition, this would imply that elected

chairpersons would have to capture more in when it is attributable to them, if they want to conform to the public behavior of ordinary residents. In contrast to the elected chairpersons, VEOs engage on average in more in the co-villager/non-attributable condition than ordinary residents in the co-villager/attributable condition, so they should have an incentive to capture less when it is attributable to them. Analogously, this mechanism could explain why there is no average effect on ordinary residents.

Conclusions

This paper has investigated two important channels of influence through which democratic elections could have an impact on rent extraction by village leaders in low-income countries: leader preferences and accountability pressures in the villages. Three conclusions have emerged from this discussion. First, elected village chairpersons in Tanzania are intrinsically less willing to misappropriate their co-villagers' contributions to a public good than ordinary citizens or externally appointed village executive officers. This is a systematic pattern across villages. Consistent with the idea that more public-spirited voters care more about having a public-spirited leader, elected chairpersons are more public-spirited than the majority of ordinary residents in their village, and more public-spirited villages tend to elect more public-spirited chairpersons.

Second, the need to anticipate the interpersonal and reputational consequences of their choices reduces average rent extraction among appointed village executive officers, but not among elected village chairpersons. Some elected chairpersons actually appear to have an incentive to capture more when their co-villagers can hold them accountable for it. This result has two implications: On the one hand, it demonstrates the relevance of informal, non-electoral accountability pressures, because electoral incentives do not matter for appointed village executive officers. On the other hand, it suggests it would be wrong to assume that village residents will hold their elected leaders to high standards of integrity, if they are given the means to do so.

Third, village leaders' responses to accountability pressures in the experiment correlate with their

rent extraction behavior in the real world, as measured by the misallocation of agricultural input vouchers and the extent to which residents trust that contributions to village projects are managed well. This result not only validates the experiment, but also confirms that bottom-up accountability pressures matter for governance outcomes at the village level, even for non-elected leaders.

In sum, these findings have important implications for how we should think about leader accountability in small-scale, weakly institutionalized communities. If democratic elections fail to reduce rent extraction by village leaders, this might not be a consequence of dysfunctional electoral selection. Even Tanzania's single-party dominated and weakly institutionalized village elections perform better at identifying public spirited village leaders than random selection of citizens or external bureaucratic appointments. Instead, elections might fail to reduce rent extraction because of the local norms and social expectations elected leaders are confronted with. Rather than assuming that corruption and rent extraction by local leaders necessarily indicate a lack of bottom-up accountability, future research should therefore aim at better understanding the social norms and expectations elected leaders are confronted with. If local norms and social expectations are incompatible with voluntary restraint in rent extraction, interventions that promote greater transparency or greater citizen participation, such as community monitoring or participatory budgeting, might be ineffective or even counterproductive at reducing the misappropriation of public resources by elected leaders.

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SUPPLEMENTARY INFORMATION

What Motivates Village Leaders to Refrain From Misappropriating Public Resources?

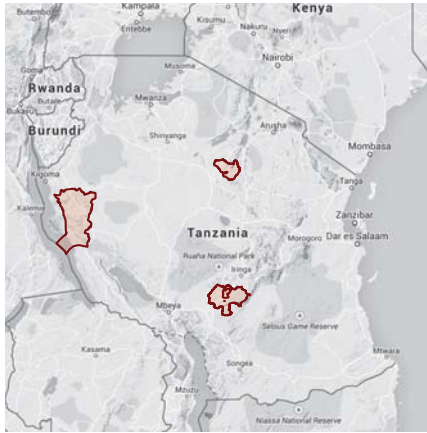
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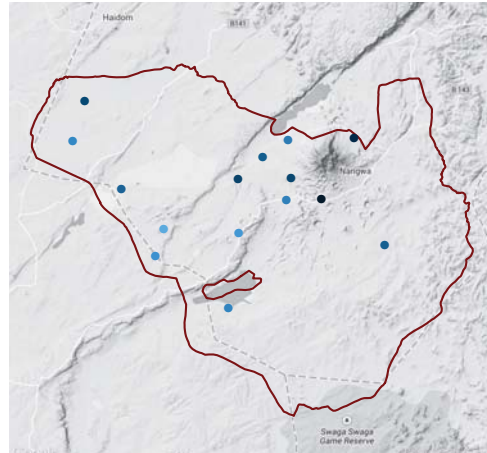
Case and Context

Study Sites

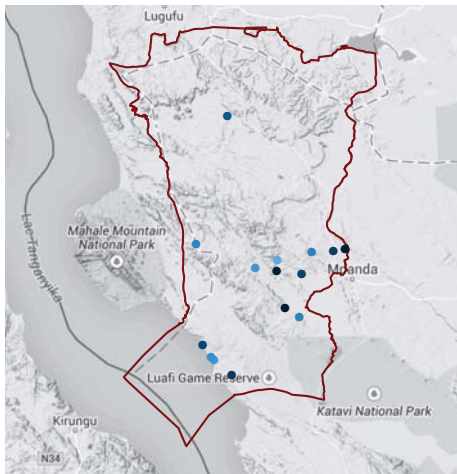
Location of study sites within Tanzania



Villages in Hanang District



Villages in Mpanda DC



Villages in Mufindi District

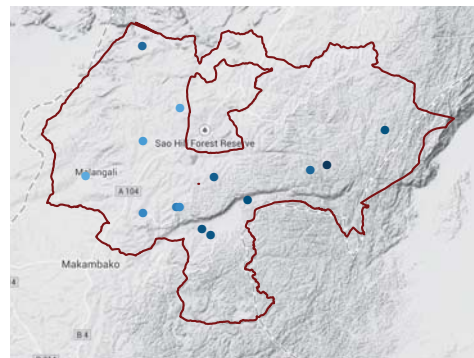


Figure A.0.1: Location of the villages studied.

A Brief History of Village Institutions in Tanzania

Contemporary village institutions in Tanzania originate from the post-independence period. Between 1969 and 1975, a massive state-led villagization program transformed the economic and political organization of rural Tanzania, with the goal of extending state control over the peasant economy and creating amenable political-economic conditions to implement president Nyerere's vision of *ujamaa* socialism (Thiele, 1986; Scott, 1998). At independence in 1961, the rural population lived almost exclusively in scattered hamlets. Under the villagization program, 13 million rural residents were resettled into newly incorporated villages, at first through economic incentives, then through overt coercion (Coulson, 1982).¹

The scale and intensity of Tanzania's villagization is unparalleled in the region. By some accounts, the proportion living in villages had increased to 10 percent by 1973 and then to 85 percent within a 20 month period after the forced resettlement policy had been adopted (Osafa-Kwaako, 2012, 13). By 1974/75 villagization was complete. As a result of villagization, 8,000 out of Tanzania's currently about 10,000 villages are the product of state-led resettlement (Shivji, 2002). Of the remaining villages, many were created later through administrative mergers or divisions. Most modern villages in Tanzania have therefore not evolved organically, but are the product of deliberate social and institutional engineering, within a narrow time period between 1969 and 1975.

The forced resettlement of Tanzania's rural population disrupted the social fabric of peasant communities, remaining traditional authority structures, as well as traditional land tenure patterns and farming practices (Shivji, 2002, 19). At the same time, deliberate nation building efforts and the propagation of a national language greatly decreased the salience of ethnic identities (Miguel, 2004).

¹Historians and political anthropologists have mostly interpreted the villagization policy as a deliberate move by the state to gain control of peasant production (Shivji, 1976, Thiele, 1986:541), by concentrating geographically dispersed populations and rendering them accessible to state intervention (Scott, 1998). Coulson (1982) argues that the timing of the forced villagization policy followed from intensified efforts of the state authorities to incorporate local-level governance into the evolving one-party state. During colonial times, local governance was characterized by indirect rule through 'native authorities'. Native authorities were abolished with independence and regional and district administrations established as the main intermediaries between the state and the rural society. Starting in 1969, the government began to promote the voluntary establishment of socialist *ujamaa* villages, often using financial incentives, famine relief in areas affected by crop failure, and government-planned agricultural investments. In a 1972 policy change that was referred to as "decentralization", the government abolished local administrations and replaced them with deconcentrated branches of the central government bureaucracy. It was at this time that the incentive-based resettlement turned into a concerted, coercive effort throughout the country.

It has been argued that land tenure practices in Tanzanian villages further eliminated incentives to embrace ethnic identities (Boone and Nyeme, 2015), facilitating the sustained abandonment of customary authority and tribal identity.

Throughout the country, a uniform set of institutions was imposed in the newly created villages. The original institutional framework for village administration was coded in the 1975 Villages Act ('Villages and Ujamaa Villages (Registration, Designation and Administration) Act, 1975, Government Notice No. 162, August 22nd, 1975.') and was reformed and consolidated in the 1982 'Local Government Act (District Authorities)' that reintroduced local self-administration and strengthened self-governance in the villages. Village institutions were geared towards establishing comprehensive state control over all aspects of the peasant economy and consolidating one-party rule at the local level.

Tanzania's unique institutional legacy and history of state-society relations in the rural areas are important for our understanding of contemporary problems of rent extraction at the village level. Rent extraction opportunities for village leaders are inextricably linked to the leadership institutions Tanzanian villages have inherited from the villagization period.

“Village governments [...] replicated the authoritarian structures and practices of the state and the ruling party. This paved the way in many villages for the rise of petty tyrants in connivance with district bosses to the detriment of the mass of villagers.”
(Shivji, 2002, 20)

Opportunities for rent extraction at the village level were inherent in the political economy of villagization. Forced resettlement, in particular, advanced the dependence of agricultural production on the state. Among farmers who had been removed from their land and were still without houses in their new villages, agricultural production during the first planting seasons declined dramatically (Coulson, 1982). Previously autarkic farmers now depended on famine relief provided by the state and on input provision for the subsequent years. Price controls for agricultural products prevented this dependency relationship from being reversed (ibid.). The newly created village administrations were pivotal in controlling access to productive inputs and in providing public services to peasants

whose traditional coping mechanisms had been disrupted. This dependency of farmers on their village administrations meant that local leaders had vast opportunities for rent extraction from the outset, by embezzling inputs while suppressing resistance of village residents.

It is important to note that rent extraction by village leaders is not an exclusive feature of villages that were created through forced resettlement, but rather a consequence of a political economy that relied on state control of peasant production by means of economic dependency. This pattern could also be observed in other African countries at the time (Bates, 1981). Some scholars argue that villages formed voluntarily during the period of government incentives were even more corrupt from the outset (Coulson 1982:290ff.):

“A paradoxical consequence of villages created to get concessions from the government was that often they were either initiatives taken by rich peasants, or the villagers elected rich peasants to lead them, on the grounds that such people would be the most experienced in negotiating with the government.” (Coulson, 1982, 291)

From the 1980s onwards, economic liberalization and later the introduction of multi-party democracy diminished the direct involvement of village governments in agricultural production. However, it also consolidated their role as gatekeepers and middlemen between the village residents and the state bureaucracy by eliminating the parallel structures of the ruling party. To this day, village leaders have remained in a position that enables them to extract rents from allocative decisions and control over public resources with relative impunity.

Authority and Accountability of Village Governments

Villages in Tanzania, like in many other countries of the world, are self-governing in the provision important local public goods and in regulating village affairs. Regulatory authority over village affairs belongs to the village assembly, which is constituted by the adult residents of the village. Village assemblies can agree on village by-laws, as well as purpose-specific and general contributions to the village budget, and they can specify penalties for non-compliance. The executive leadership of a village consists of an elected village chairperson (*mwenyekiti wa kijiji*), an externally

appointed village executive officer (*mtendaji wa kijiji*, abbreviated: VEO), and different purpose-specific committees (land management, budget, education, water, voucher allocation, etc.), which vary in number and size across villages. Most day-to-day governance decisions in the village are made more or less independently by the village government, consisting of an elected village council (*baraza*) and the executive leadership of the village.

Examples of local public goods that are provided at the village level include the maintenance of school buildings, water pumps, village roads, village offices, health posts, and shared agricultural infrastructure (grain storage, tractors, mills, cattle dips, etc.). Often, villages also manage common-pool resources, such as communal land and communal forests (Shivji, 2002; Brockington, 2008). Additionally, some villages employ individuals to provide services to the community, for example as school guards.

The structural problems involved in the provision of such local-level public goods are similar in many parts of the world: Villages have to mobilize internal revenues and in-kind contributions, which are not only hard to enforce, but also vulnerable to capture by local elites. Tanzanian villages are no exception, neither with regard to the difficulty of mobilizing contributions at the local level (Lund, 2007; Brockington, 2008; Fjeldstad, 2001, 295ff), nor with regard to the risk of rent extraction by local leaders (Kelsall et al., 2005; Brockington, 2007; Lund, 2007, 315; Pan and Christiaensen, 2012; Lund and Saito-Jensen, 2013). In the villages that have been surveyed for this study, between 60 and 100 percent of citizens affirm that there are “serious problems with non-payment of contributions to village projects and activities”. In 21 out of 32 villages surveyed, a majority of sampled residents agreed with the statement that “most people in the village think that the village government is corrupt”.

As far as the enforcement of contributions is concerned, some villages possess informal coercive mechanisms that are internal to the villages. Most villages in Tanzania have enforcement volunteers (*mgambo*) who are trained in disciplinary action. Some villages additionally maintain an armed village militia (*sungusungu*), and it is not uncommon that the militia becomes involved the enforcement of contributions. Crucially, however, these enforcement structures are based on volun-

tary collective action within the village (Abrahams, 1989) and their success varies with pre-existing collective action capacity (Paciotti et al., 2013).

Although village leaders have the possibility of appealing to external law enforcement institutions, for example by taking non-contributors to court, the costs of doing so and the time it takes to complete a court case are often in no relation to the payoff. In situations of massive non-compliance, there is very little village leaders can do to enforce the payments and inputs that are needed to complete a public project. Given their lack of direct coercive capacity, village leaders typically appeal to citizens' moral responsibilities for their communities, rather than to the threat of enforcement.

A.1 Identification Assumptions

To identify the extent to which village leaders' discretion to misappropriate public resources is constrained by the need to anticipate the consequences of their choices for their reputations and future social interactions in their village, the experiment focuses on how the effects of personal attributability of their rent extraction decisions differ between a co-villager and a stranger setting. This identification strategy is based on two assumptions. First, that the only situation in which an individual's rent extraction decision can be influenced by village-level accountability pressures is when the respondent knows that (1) her or his proposal will be observable ex post, (2) to people with whom she or he is going to interact again in the future. Second, that if no village-level accountability pressures existed, any direct effects of observability would, in expectation, be the same in the co-villager and stranger conditions. Under these assumptions, and given the random assignment of the treatments, the average effect of having to anticipate the consequences of rent extraction for individuals' reputations and future social interactions in their village (which I refer to as "bottom-up accountability pressures") is identified by the difference in the treatment effects of ex-post observability between the co-villager and the stranger conditions.

To see this, let $C \in \{0, 1\}$ indicate the co-villager treatment, $P \in \{0, 1\}$ indicate the ex-post attributability treatment and $A \in \{0, 1\}$ the presence of social incentives or accountability pressures

whose average effect we would like to identify. By the first assumption, $P = 0$ or $C = 0$ implies that $A = 0$.

Let $Y(C, P, A)$ denote the potential outcomes associated with C, P and A . For any unit i drawn from the study population, the potential willingness to capture contributions to the public good is given by $y_i(C = c_i, P = p_i, A = a_i)$.

The individual treatment effect of social incentives on unit i 's willingness to capture public resources is defined as

$$\delta_i \equiv y_i(C = 1, P = 1, A = 1) - y_i(C = 1, P = 1, A = 0)$$

Here, the quantity $y_i(C = 1, P = 1, A = 0)$ is unobservable, because, by definition, social incentives or accountability pressures are forcibly present whenever subjects know that their decisions are going to be observable to their co-villagers. For any unit i , we can observe either $y_i(C = 1, P = 1, A = 1)$, $y_i(C = 1, P = 0, A = 0)$, $y_i(C = 0, P = 1, A = 0)$, or $y_i(C = 0, P = 0, A = 0)$, depending on i 's treatment status.

Let $\pi_i(c_i) \equiv y_i(C = c_i, P = 1, A = 0) - y_i(C = c_i, P = 0, A = 0)$ denote the direct effect of ex-post observability on unit i 's capture decision. In the co-villager condition, ignorability of the ex-post observability treatment of potential outcomes identifies only the combined treatment effect of the ex-post observability treatment and social incentives, $E[\Pi + \Delta | C = 1]$. In the stranger condition, ignorability of the ex-post observability treatment identifies $E[\Pi | C = 0]$.

In order to identify the average treatment effect of social incentives, we must assume that $E[\Pi | C = 1] = E[\Pi | C = 0]$.² Then, the parameter $E[\Delta]$ is identified as the difference in differences between

²This is a milder assumption than imposing that $\pi_i(1) = \pi_i(0) \forall i$. The direct effects of observability in the absence of accountability pressures do not have to be identical in the co-villager and stranger conditions for all individuals. I merely assume that on average these effects are the same.

the four treatment conditions:

$$\begin{aligned}
 E[\Delta] &= E[\Delta|C = 1] = E[\Pi + \Delta|C = 1] - E[\Pi|C = 0] \\
 &= E[Y(C = 1, P = 1, A = 1)] - E[Y(C = 1, P = 0, S = 0)] \\
 &\quad - (E[Y(C = 0, P = 1, A = 0)] - E[Y(C = 0, P = 0, A = 0)])
 \end{aligned}$$

A.2 Treatment Group Sizes and Balance Tests

Appointed Village Executive Officers (VEO)						
	All	Co-villager Experiment		Stranger Experiment		p-Value
		Non-Attributable	Ex-Post Attributable	Non-Attributable	Ex-Post Attributable	(Kruskal-Wallis)
<i>Mean values by treatment condition</i>						
Proportion female	0.13	0.12	0.31	0	0.08	0.129
Age (years)	44	45	40.7	45.3	45.7	0.418
Distance to village center (minutes)	32.5	33.8	31.43	26	39.67	0.805
Household size	2.8	3.7	3.3	2.1	2.7	0.106
Education (years)	10.0	10.0	10.2	10.2	9.7	0.946
Risk preference	0.37	0.4	0.4	0.17	0.47	0.525
<i>N</i>	46	8	13	13	12	

Table A.2.1: Covariate Balance for Appointed VEOs.

Elected Village Chairpersons						
	All	Co-villager Experiment	Stranger Experiment			p-Value
		Non-Attributable	Ex-Post Attributable	Non-Attributable	Ex-Post Attributable	(Kruskal-Wallis)
<i>Mean values by treatment condition</i>						
Proportion female	0.06	0	0.17	0.1	0	0.531
Age (years)	51	50.5	50.8	51.2	51.2	0.892
Distance to village center (minutes)	25.3	27.5	16.3	34.5	19.4	0.626
Household size	5.3	4.2	7.5	4.9	5.1	0.846
Education (years)	7.1	6.7	7.8	7.2	6.8	0.656
Risk preference	0.4	0.2	0.63	0.26	0.47	0.478
<i>N</i>	31	6	6	10	9	

Table A.2.2: Covariate Balance for Elected Chairpersons.

Randomly Sampled Village Residents						
	All	Co-villager Experiment		Stranger Experiment		p-Value
		Non-Attributable	Ex-Post Attributable	Non-Attributable	Ex-Post Attributable	(Kruskal-Wallis)
<i>Mean values by treatment condition</i>						
Proportion female	0.37	0.32	0.44	0.33	0.4	0.15
Age (years)	39.5	41.0	37.2	39.1	40.3	0.12
Distance to village center (minutes)	41.9	39.8	37.2	43.9	46.2	0.70
Household size	3.28	3.22	3.31	3.29	3.31	0.97
Education (years)	5.6	5.5	5.7	6.1	5.2	0.18
Risk preference	0.47	0.46	0.51	0.43	0.47	0.56
<i>N</i>	561	161	119	147	134	

Table A.2.3: Covariate Balance for Ordinary Residents.

A.3 Additional Results on the Effects of Accountability Pressures

Average rent extraction decisions by experimental condition

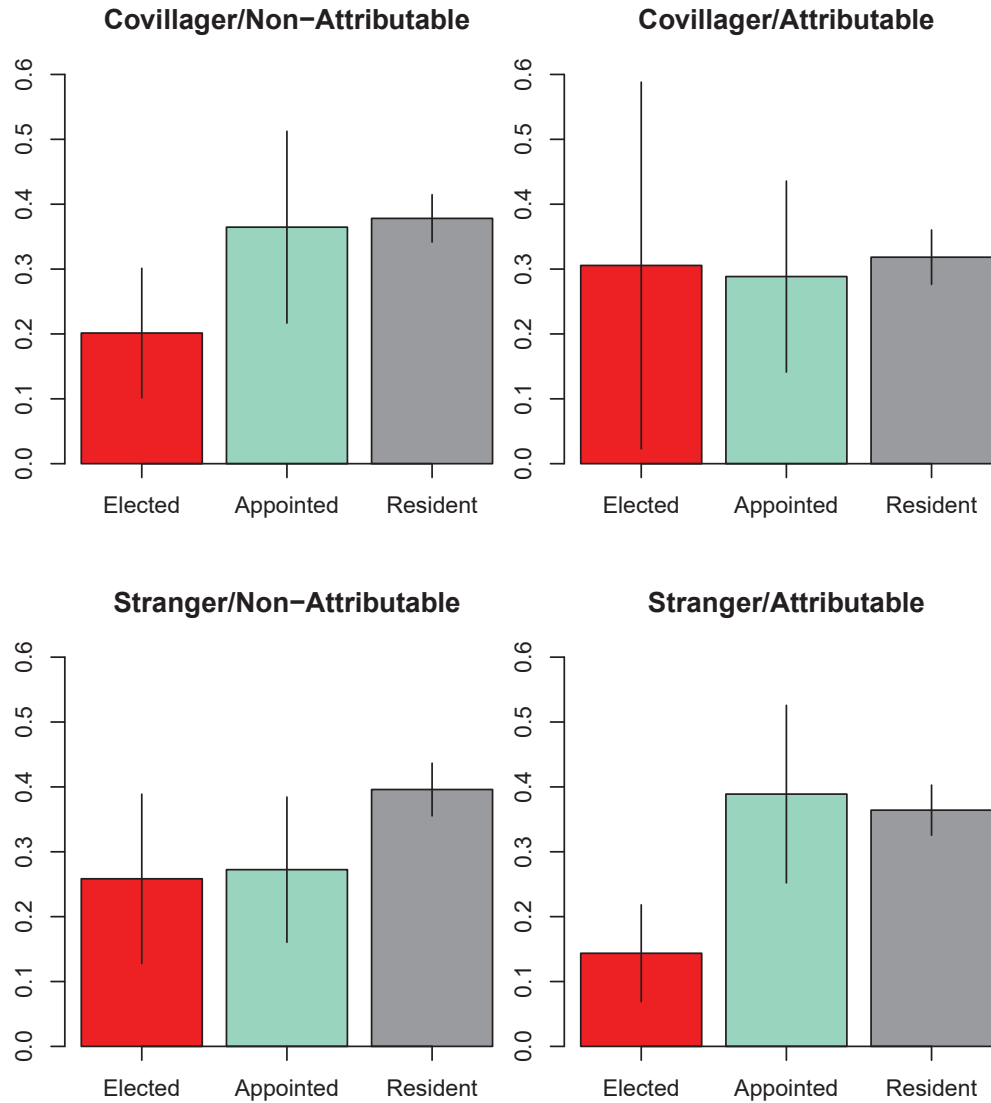


Figure A.3.1: Mean rent extraction decisions of elected leaders, appointed leaders and randomly sampled residents, by experimental condition. The data is restricted to study participants' first decision exercises, to rule out bias from treatment order, panel conditioning and carry-over effects. The vertical axes indicate the fraction of the group resource study participants proposed to capture. The lines represent 95 % confidence intervals. Note that the mean embezzlement proposals of elected chairpersons, appointed VEOs and randomly sampled residents are very similar in the co-villager/attributable condition.³

³If the data from the later decisions is included, there are differences between the mean capture decisions of the

Testing for differential effects between elected chairpersons and appointed VEOs

Fraction of Contributions Misappropriated	
<i>Tobit Coefficients</i>	
Constant	0.016 (0.152)
Chairperson	0.034 (0.086)
Co-villager	0.12 (0.098)
Co-villager × Chairperson	-0.14 (0.15)
Attributable	0.18 ⁺ (0.11)
Attributable × Chairperson	-0.28* (0.14)
Co-villager × Attributable	-0.23 (0.15)
Co-villager × Attributable × Chairperson	0.42* (0.21)
Interviewer Effects	yes
District Effects	yes
<i>N</i>	77

Robust standard errors in parentheses

⁺ $p < 0.1$ * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.3.1: Differential effects between elected chairpersons and appointed VEOs: The table reports coefficients from a Tobit regression of village leaders' capture proposals on treatment conditions (using study participants' first treatments). Of interest is the coefficient on the triple interaction between the co-villager and stranger treatments and whether the study participant is the village chairperson.

three types of study participants, but they appear to be less pronounced than in the co-villager/non-attributable condition. In pooled data for the co-villager/attributable condition, elected chairpersons embezzled with 21 percent on average about 13.8 percentage points less than randomly sampled adult residents and 6.8 percentage points less than appointed VEOs within the same districts. Given that elected chairpersons embezzled less than their co-villagers in all of the other three treatment conditions, it is possible that the differences in the co-villager/attributable rent extraction decisions of elected chairpersons and appointed VEO in the pooled data are merely an artifact of carry-over effects from previous decisions.

Results by District

	Fraction of Contributions Misappropriated					
	Mpanda DC Chair	VEO	Hanang District Chair	VEO	Mufindi District Chair	VEO
Co-villager		0.19 (0.20)	-0.15 (0.14)	0.28 (0.20)	-0.075 (0.19)	0.084 (0.14)
Attributable		0.78** (0.17)	-0.36 ⁺ (0.16)	0.34 (0.21)	0.014 (0.14)	-0.11 (0.11)
Co-villager × Attributable		-0.61* (0.23)	0.34 (0.26)	-0.43 (0.29)	0.35 (0.24)	-0.078 (0.18)
Constant		-0.12 (0.16)	0.49* (0.21)	-0.22 (0.33)	0.17 (0.20)	0.44* (0.16)
Interviewer Effects		yes	yes	yes	yes	yes
Observations	0	14	15	16	16	16
Censored at zero		4	3	1	2	1
Censored at one		0	0	1	1	0

Standard errors in parentheses.

⁺ $p < 0.1$ * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.3.2: Results by district. Across all districts, the coefficient on the interaction effect of the co-villager and observability treatments has the same sign (positive for elected chairpersons, negative for appointed VEOs). In Mpanda DC, the decision exercise was carried out only with appointed VEOs, not with elected village chairpersons.

Estimation of Treatment Effects via OLS Regression

Fraction of Contributions Misappropriated						
	Randomly Sampled Residents		Elected Chairpersons		Appointed VEOs	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>OLS coefficients</i>						
Co-villager	-0.018 (0.024)	-0.029 (0.022)	-0.057 (0.084)	-0.047 (0.097)	0.092 (0.095)	0.15 (0.094)
Attributable	-0.033 (0.027)	-0.025 (0.022)	-0.12 (0.077)	-0.080 (0.073)	0.12 (0.090)	0.21 ⁺ (0.11)
Co-villager × Attributable	-0.028 (0.038)	-0.021 (0.033)	0.22 (0.17)	0.17 (0.11)	-0.19 (0.14)	-0.28 ⁺ (0.14)
Constant	0.40*** (0.018)	0.24*** (0.038)	0.26*** (0.067)	0.38*** (0.097)	0.27*** (0.057)	-0.13 (0.15)
Interviewer Effects		yes		yes		yes
District Effects		yes		yes		yes
<i>N</i>	561	561	31	31	46	46

Standard errors in parentheses: clustered by village in columns (1) and (2), HC2 standard errors in columns (3)-(6).
⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.3.3: OLS regressions of capture proposals on treatment indicators, using study participants first treatments (between-subjects design).

A.4 Contribution Behavior and Expectations about the Size of the Group Resource

The following analyses estimate how study participants' expectations about the size of the group fund are influenced by the experimental treatments. By altering expectations about how much of the group fund is going to be misappropriated, the experimental treatments could have had an indirect effect on contribution behavior in the first stage of the decision exercise. If these indirect effects are collectively anticipated, they might subsequently influence study participants' expectations about the other group members' contributions. Through this indirect chain of causality, the experimental treatments could have an effect on study participants' expectations about the size of the group

fund.

Unless there is a substantively important causal relationship between study participants' expectations about the size of the group resource and their willingness to extract rents, an effect of the experimental treatments on study participants' expectations about the size of the group fund should not affect the interpretation of the main experimental results. However, if such a causal relationship exists, then the indirect effects of the experimental treatments due to endogenous changes in study participants' expectations could either reinforce or attenuate the overall effects of co-villager and attributability treatments. This would make it more challenging to attribute the interaction effect of the co-villager and attributability treatments directly to the necessity of anticipating real-world accountability pressures, because in forming their expectations about the size of the group fund, study participants would also anticipate changes in contribution behavior.

I address this potential concern by investigating more closely how contribution behavior and expectations about the size of the group fund are affected by the experimental treatments (Tables A.4.1, A.4.2 and A.4.3). Among ordinary residents, there is a positive interaction effect of the co-villager and attributability treatments on both the contributions and, consistent with this, on study participants' expectations about the size of the group fund. However, there is no significant interaction effect on actual misappropriation of the group fund (Table A.4.3). Among appointed VEOs and elected chairpersons, no significant effect of the experimental treatments on contribution behavior and the expected size of the group fund can be detected (Tables A.4.1 and A.4.2).

For both types of village leaders, the interaction effect of the co-villager and attributability treatments on the expected size of the group fund is of the same sign as their interaction effect on actual misappropriation. Therefore, it would seem unlikely that the latter effect is driven by changes in study participants' expectations, unless a greater expected size of the group fund actually causes village leaders to misappropriate a greater fraction of it. However, this does not seem very plausible. If individuals exhibit reciprocity, they should be less willing to misappropriate contributions, the more they expect others to have contributed. Furthermore, individuals who are more pro-social might also expect others to be more pro-social (either because social preferences are correlated

within villages, or because individuals form their expectations about the behavior of others by extrapolating from their own behavior), which would suggest a negative relationship between rent extraction proposals and expected total contributions.

Indeed, as evident in Figure A.4.1, for all types of study participants rent extraction decisions are negatively associated with their expectations about the size of the group resources. These correlations do not convey information about the direction of causality, or even imply the existence of a causal relationship. However, they make it seem unlikely that a negative interaction effect of the co-villager and attributability treatments on VEOs' average rent extraction would be due to decreased expectations about the size of the group fund, or that a positive interaction effect on the chairpersons' average rent extraction would be due to increased expectations about the size of the group fund.

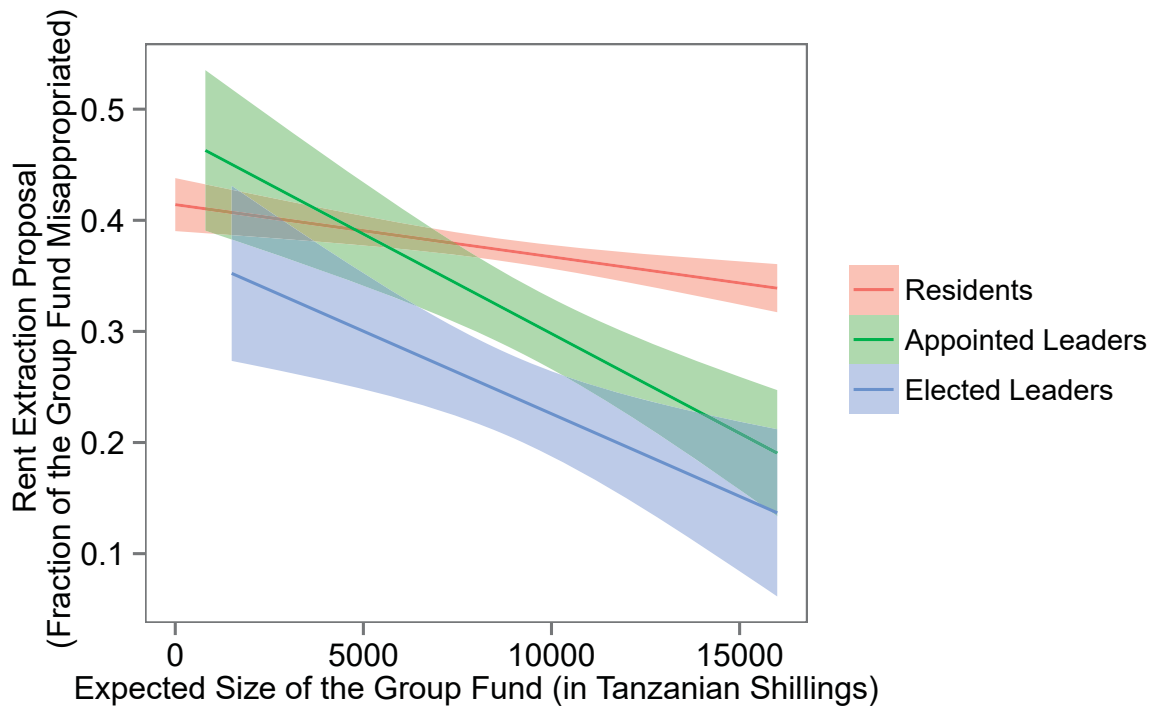


Figure A.4.1: The greater the expected size of the group fund, the lower is the fraction of the group fund study participants propose to misappropriate.

To sum up, there is no indication that the interaction effects of the co-villager and attributability treatments on the average rent extraction decisions of village leaders were reinforced by changes in

their expectations about the size of the group fund. However, a potential caveat remains, because it cannot be definitively ruled out that for some individuals the experimental treatments affected their rent extraction decisions also indirectly, by altering their expectations about the size of the group fund.⁴ It is important to note that the possibility of indirect effects via changes in study participants' expectations does not invalidate the overall interpretation of the experiment. The reason why the experimental treatments could potentially affect expectations about the size of the group fund is that study participants might anticipate the direct effects of the experimental treatments on the rent extraction decisions. If the experimental treatments had no direct effects on rent extraction, then there would be no reason to assume that they should alter individuals' contribution behavior.

⁴For scholars aiming to reproduce and extend this research, it might therefore be advisable to alter the procedures in a way that allows for the treatment condition to be revealed only after the contribution stage. Then, study participants' contribution behavior and expectations about the size of the group fund would be completely unaffected by the treatment condition, which would make causal attribution unambiguous. However, it would come at the cost of making the procedure more complicated and more difficult to understand for study participants.

Elected Chairpersons			
	Contribution <i>(as a fraction of endowment)</i> (1)	Expected Total Amount <i>(as a fraction of theoretical maximum)</i> (2)	Fraction Misappropriated (3)
	<i>Tobit Coefficients</i>	<i>OLS Coefficients</i>	<i>Tobit Coefficients</i>
Co-villager	.14 (.10)	.12 (.11)	-.066 (.11)
Attributable	-.12 (.13)	-.13 (.11)	-.13 (.080)
Co-villager × Attributable	.16 (.13)	.062 (.16)	.24 ⁺ (.14)
Interviewer effects	yes	yes	yes
District effects	yes	yes	yes
Observations	31	31	31
Missing Observations	1	1	1
Censored at zero	1	.	5
Censored at one	1	.	1

Standard errors in parentheses. ⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table A.4.1: Effects of the experimental treatments on the voluntary contributions of elected chairpersons, their expectations about the total size of the group fund, and their rent extraction decisions. To prevent confounding by treatment order and carry-over effects, the data is restricted to study participants first decisions (between-subjects analysis).

Appointed Village Executive Officers

	Contribution <i>(as a fraction of endowment)</i> (1)	Expected Total Amount <i>(as a fraction of theoretical maximum)</i> (2)	Fraction Misappropriated (3)
	<i>Tobit Coefficients</i>	<i>OLS Coefficients</i>	<i>Tobit Coefficients</i>
Co-villager	-.09 (.12)	.037 (.098)	.16 (.10)
Attributable	-.15 (.11)	.0062 (.10)	.23* (.11)
Co-villager × Attributable	.10 (.16)	-.056 (.15)	-.29* (.15)
Interviewer effects	yes	yes	yes
District effects	yes	yes	yes
Observations	46	46	46
Missing	2	2	2
Censored at zero	1	.	6
Censored at one	8	.	1

Standard errors in parentheses. ⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table A.4.2: Effects of the experimental treatments on the voluntary contributions of appointed VEOs, their expectations about the total size of the group fund, and their rent extraction decisions. To prevent confounding by treatment order and carry-over effects, the data is restricted to study participants first decisions (between-subjects analysis).

Ordinary Residents			
	Contribution <i>(as a fraction of endowment)</i> (1)	Expected Total Amount <i>(as a fraction of theoretical maximum)</i> (2)	Fraction Misappropriated (3)
	<i>Tobit Coefficients</i>	<i>OLS Coefficients</i>	<i>Tobit Coefficients</i>
Co-villager	.0097 (.023)	-.029 (.031)	-.029 (.023)
Attributable	.013 (.030)	-.0022 (.025)	-.021 (.022)
Co-villager × Attributable	.062 ⁺ (.035)	.085* (.038)	-.033 (.036)
Interviewer effects	yes	yes	yes
District effects	yes	yes	yes
Observations	560	558	560
Missing	3	5	3
Left-censored	17	.	60
Right-censored	25	.	10

Standard errors in parentheses (adjusted for clustering by village). ⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table A.4.3: Effects of the experimental treatments on the voluntary contributions of ordinary village residents, their expectations about the total size of the group fund, and their rent extraction decisions. To prevent confounding by treatment order and carry-over effects, the data is restricted to study participants first decisions (between-subjects analysis).

A.5 Additional Details on Validation

Descriptive Statistics on Real-World Measures of Elite Capture

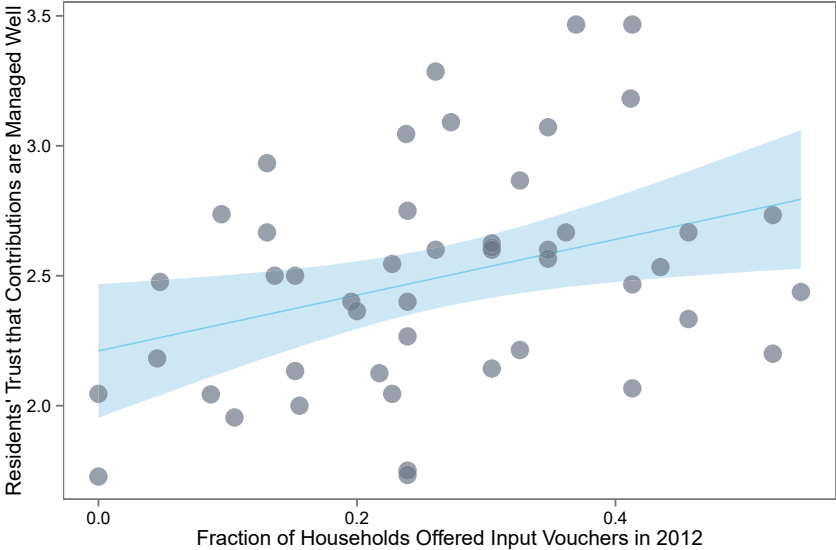


Figure A.5.1: Joint distribution of the two measures of village leaders' rent extraction in the real world.

	(1)
	Household was offered vouchers in 2012
Age of respondent	0.0240*** (0.00486)
Age ²	-0.000262*** (0.0000561)
Respondent is female	0.0662** (0.0255)
Household is female-headed	-0.0695* (0.0313)
log(Farm size (ha))	0.0458** (0.0156)
Vouchers allocated to the village per capita	0.0254 (0.0764)
Constant	-0.283** (0.0976)
<i>N</i>	1605
<i>R</i> ²	0.0365

Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.5.1: OLS regression of whether a household was offered an agricultural input voucher package in 2012 on the control variables included in the model in Table 3.

Validation Results by District

Residents' Trust that Contributions are Managed Well						
<i>(Scale: 1="Do not trust at all..." to 4="Trust completely...")</i>						
	Mpanda DC		Hanang District		Mufindi District	
	Chair	VEO	Chair	VEO	Chair	VEO
<i>OLS coefficients</i>						
Effect of accountability pressures on leader's capture decision	-0.725** (0.231)	-0.965** (0.230)	-0.128 (0.227)	0.0783 (0.447)	-0.0354 (0.224)	-0.614* (0.209)
Leader's capture preference	-0.0993 (0.231)	-0.506 (0.407)	0.224 (0.450)	-0.933* (0.426)	-1.083 (0.589)	0.686 (0.504)
Constant	2.626*** (0.240)	2.683*** (0.205)	2.391*** (0.455)	2.939*** (0.440)	3.161*** (0.232)	2.651*** (0.208)
Observations	328	305	226	241	237	237

Standard errors in parentheses (adjusted for clustering by village).

⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.5.2: Results by district.

Household Was Offered Input Vouchers in 2012						
<i>(sample restricted to farm households)</i>						
	Mpanda DC		Hanang District		Mufindi District	
	Chair	VEO	Chair	VEO	Chair	VEO
<i>Logit coefficients</i>						
Effect of accountability pressures on leader's rent extraction decision	-1.3*** (0.38)	-0.55 (0.35)	-1.3*** (0.38)	-0.56 ⁺ (0.34)	-1.0** (0.32)	0.37 (0.30)
Leader's rent extraction preference	-0.57 (0.49)	-0.42 (0.84)	1.34*** (0.31)	-0.26 (0.63)	-1.73 (1.08)	-0.13 (0.72)
Constant	-1.2*** (0.22)	-1.4*** (0.27)	-1.3*** (0.14)	-0.81*** (0.21)	-0.23 (0.26)	-0.57 ⁺ (0.32)
<i>N</i>	274	251	679	724	728	728

Cluster-robust standard errors in parentheses (clustered by village).

⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.5.3: Results by district.

Alternative Specifications

Residents' Trust that Contributions are Managed Well						
<i>(Scale: 1="Do not trust at all..." to 4="Trust completely...", mean=2.48)</i>						
	Elected Chairperson			Appointed VEO		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>OLS Coefficients</i>						
Effect of accountability pressures on leader's rent extraction decision $c_i^{co} - c_i^{ca} - (c_i^{so} - c_i^{sa})$	0.074 (0.17)	0.077 (0.17)	0.066 (0.18)	-0.66** (0.19)	-0.62** (0.20)	-0.60** (0.212)
Leader's rent extraction preference c_i^{ca}	-0.40 (0.38)	-0.45 (0.38)	-0.35 (0.36)	-0.28 (0.29)	-0.27 (0.28)	-0.26 (0.27)
Constant	2.4*** (0.13)	4.1*** (0.52)	4.0*** (0.56)	2.5*** (0.12)	3.8*** (0.49)	4.0*** (0.48)
Interviewer effects		yes	yes		yes	yes
Individual-level controls			yes			yes
Observations	463	463	463	783	783	783

Standard errors in parentheses (adjusted for clustering by village).

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.5.4: OLS regressions with and without individual-level controls. Individual-level controls are gender, age, age squared and years of education.

Residents' Trust that Contributions are Managed Well		
<i>Scale: 1 ("Do not trust at all") to 4 ("Trust completely")</i>		
	Chairperson	VEO
	(1)	(2)
<i>Ordered Logit Coefficients</i>		
Effect of Accountability Pressures on Leader's Rent Extraction Decision	0.13 (0.26)	-0.86** (0.30)
Leader's Rent Extraction Preference (co-villager/non-attributable)	-0.56 (0.58)	-0.45 (0.42)
N	463	783

Standard errors in parentheses (adjusted for clustering by village)

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.5.5: Ordered Logit regressions of the village residents' average trust that their contributions to village projects are managed well on the elected village chairperson's and the appointed VEO's decisions in the experiment. The outcome was self-reported on a four point scale, ranging from 1="no trust at all" to 4="trust completely". The results are robust

to controlling for individual characteristics (gender, age, education), interviewer effects and district effects. The marginal effects of the VEO's response to social incentives on the predicted probability of each trust level are visualized in Figure 4.

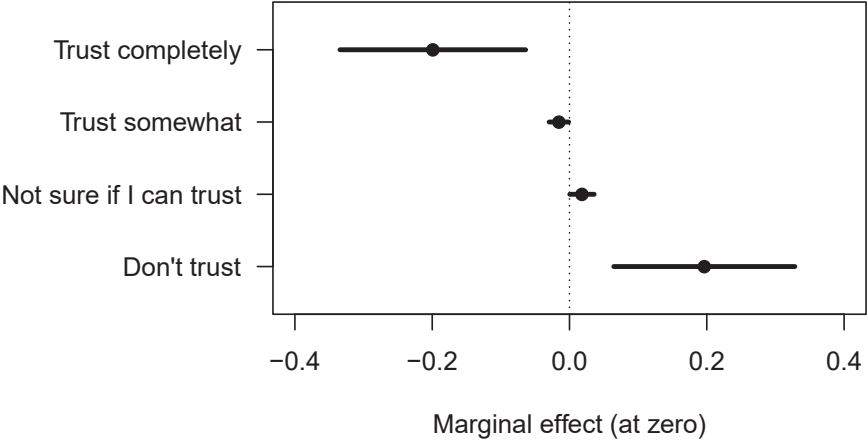


Figure A.5.2: Marginal effects of the VEO's response to social incentives in the experiment on the predicted probability of each response. The error bars represent 95 % confidence intervals.

Household was Offered an Agricultural Input Voucher
(sample restricted to farm households, mean=0.287)

	Elected Chairperson			Appointed VEO		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Logit Coefficients</i>						
Effect of accountability pressures on leader's capture decision $c_i^{co} - c_i^{ca} - (c_i^{so} - c_i^{sa})$	-0.74** (0.27)	-0.89** (0.32)	-1.1*** (0.31)	-0.21 (0.24)	-0.17 (0.27)	-0.067 (0.30)
Leader's capture preference c_i^{ca}	0.093 (0.64)	-0.31 (0.78)	-0.44 (0.89)	0.021 (0.47)	-0.30 (0.41)	-0.37 (0.49)
Constant	-0.81*** (0.15)	-0.094 (0.45)	-4.0*** (0.72)	-0.87*** (0.18)	-0.37 (0.31)	-3.61*** (0.63)
Interviewer effects		yes	yes		yes	yes
Individual-level controls			yes			yes
<i>Marginal Effects (evaluated at zero, holding the other variables at mean)</i>						
Effect of Social Incentives on Leader's Capture Decision	-0.16 (0.060)	-0.19 (0.070)	-0.23 (0.069)	-0.44 (0.050)	-0.036 (0.056)	-0.014 (0.061)
Observations	1407	1406	1354	1703	1703	1522

Standard errors in parentheses (adjusted for clustering by village).

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.5.6: Logit regressions with and without individual-level controls. The controls are: the amount of land owned by the household (log-transformed), the number of vouchers allocated to the village per capita, whether the household is female-headed, the respondent's age, age squared, gender and years of education, interviewer effects and district effects.

A.6 Research Process and Research Design Registration

The data collection for this study was carried out between June 2013 and March 2014 as part of a larger dissertation research project on the role of social incentives and collective action capacity in village self-governance. The project comprised three different experiments, two surveys of citizens, one survey administered to a focus group of village leaders, the collection of administrative records, and open-ended debriefing interviews with study participants. This paper exclusively focuses on results of an experiment designed to measure the influence of accountability pressures on individuals' willingness to capture other people's contributions to a public good, comparing the behavior of

elected leaders, appointed leaders and ordinary citizens.

Both the interpretation of the treatment effects in the experiment and the validation strategy were documented in a research design document that was uploaded to the EGAP (Experiments in Governance and Politics) registry on June 16th, 2013.⁵ This document also covered the two other experiments that were carried out simultaneously. Further results will be reported in separate research papers (including (?)). As a consequence, the research design document was relatively broad, reflecting the original motivations for the study and its place in a broader research agenda, but not the full details on tests that were later carried out to scrutinize the argument of this paper. The research design document was also inevitably preliminary, since both the behavioral and survey-based outcome measures used in this study are innovative and no prior experience with their feasibility and data quality existed. In particular, it was unclear whether it would be feasible at all to include both types of village leaders into the study, and in which of the experiments. After first practical experiences with the implementation of the experiments had been collected in Mpanda DC, a pre-analysis plan with more specific hypotheses was added on November 7th, 2013, which also included preliminary results from Mpanda DC.

In both the original and the updated research design documents, three key elements of this study remained unaltered. First, the proposed interpretation of the treatment effects and decision outcomes in the experiment. Specifically, this includes (1) the interpretation of the interaction effect of the co-villager and ex-post observability treatments as a measure of study participants' responses to social incentives or accountability pressures in their village, (2) the interpretation of study participants' capture decisions in the co-villager/non-attributable treatments as a measure of their capture preferences, i.e. their intrinsic willingness to refrain from capturing their co-villagers' contributions to a public good, and (3) the research objective of distinguishing between preference- and incentive-based motives for refraining from capture. A second element that has remained unaltered is the combination of a between-subjects design (based on subjects' first treatments) for the purpose of drawing causal inferences with within-subject variation in the treatments to obtain proxy measures for individual treatment responses that could be used in cross-sectional comparisons to validate

⁵<http://e-gap.org/design-registration/gated/gated-lierl/>

the interpretation of the experiment. A third idea that has remained unaltered is the validation strategy which focuses on examining the correlation between study participants' behavior in the experiment and two outcomes of interest, namely (1) the capture of agricultural input vouchers, and (2) residents' trust in their leaders.

The research design document was amended after data collection had been completed in Mpanda DC. In particular, after it had become apparent that it would be feasible to persuade village leaders in large enough numbers to participate in the experiment, a component was added that focused specifically on comparing village chairpersons, VEOs and randomly sampled residents, which became the basis for this paper. In the subsequent data collection in Hanang and Mufindi Districts, both the chairperson and the VEO took part in the same experiment. The number of ordinary residents sampled per village was doubled, so that there were now two groups of eight individuals per village who took part in the decision exercise. The following hypotheses were specified:

- Hypothesis G.1a: On average, village leaders (village executive officers and village chairpersons) capture fewer contributed group resources in the co-villager/non-attributable condition of the accountability game than randomly sampled village residents.
- Hypothesis G.1b: Social incentives have a more pro-social influence on the allocation behavior of village leaders (village executive officers and village chairpersons) in the accountability game, compared to the influence of social incentives on the behavior of randomly sampled village residents.
- Hypothesis G.2: There is a negative relationship between the effect of social incentives on the village leaders' allocation to the group in the accountability game and measures of elite capture in the village.
- Hypothesis G.3: There is a positive relationship between the effect of social incentives on the village leaders' allocation to the group in the accountability game and measures of village residents' trust in their leaders.

While the proposed analytical strategy for each of these hypotheses has not been substantively altered, a deviation from the pre-analysis plan concerns the measure of citizens' trust in village leaders, to be used to validate the experiment. In the pre-analysis plan, priority had been given to a behavioral measure; the proportion of study participants who authorized their VEO or their chairperson to pick up their (ex ante unknown) payoff from the decision exercise for them. As a backup, a self-reported behavioral measure was mentioned in the pre-analysis plan, namely "In the past three months, how many times did you go to the village office for some request, some problem, to get information or to get something else?". Initially with the idea of validating these behavioral measures, a third, survey-based measure was included in the study, but omitted from the pre-analysis plan, which more directly measured citizens' expectations about capture of contributions to public goods in their village: "When the village government collects money for a collective purpose in your village, do you trust that the money will be managed well?". Since both of the behavioral outcome measures were later found to be problematic, this survey based measure of citizens' expectations about capture was used instead.

The reason for this substitution was that these behavioral outcome measures were not fully thought through. With regard to authorizing village leaders, study participants were asked to authorize their village leaders only for the event that they were personally unavailable to pick up their payoff the agreed on date and time, and another person of their choice whom they had authorized was unavailable as well. Study participants were asked, sequentially, (a) whether they would like to authorize a person of their choice to pick up their payoff for them if they were unavailable themselves, (b) whether they would like to also authorize the VEO as a backup if the person of their choice whom they had authorized was unavailable (or if they had not authorized anybody else in the first place), and (c) whether they would like to also authorize the village chairperson as a backup if the person of their choice whom they had authorized was unavailable (or if they had not authorized anybody else in the first place). As a measure of citizens' expectations about capture, this variable is problematic in several ways. (a) It reflects a situation where village leaders have the possibility of secretly embezzling study participants' money, rather than a situation of public capture, which is usually much more conspicuous. (b) The study participants' decision is

meaningless if they had previously authorized a person of their choice and strongly believe that this person (or they themselves) are able to pick up their payoff. (c) Whether they authorized the chairperson may depend on whether they had authorized the VEO in the previous question. With regard to making requests at the village office, this question does not reflect whether these requests are voluntary. Assuming that they mostly are, a greater utilization of the village office would nonetheless only indicate a greater reliance on the village government in a broad sense, but not directly citizens' expectations about capture.

Number of times the respondent has made a request at the village office within the past three months		
	(1) Chairperson	(2) VEO
<i>Coefficients from Negative Binomial Regression</i>		
Effect of accountability pressures on leader's capture decision	-0.26 (0.18)	-0.34 (0.27)
Leader's capture preference	0.22 (0.43)	-0.58* (0.26)
Constant	0.77*** (0.23)	0.82*** (0.19)
Interviewer effects	yes	yes
District effects	yes	yes
<i>N</i>	1414	1766

Standard errors in parentheses, adjusted for clustering by village.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ (two-sided)

Table A.6.1 Relationship between leader's behavior in the experiment and the frequency at which ordinary citizens in their village make requests at the village office.

A.7 Sampling of Study Participants

The sampling of ordinary village residents was preceded by a listing of all households in each of the subvillages.⁶ The listing was carried out by village residents who were hired for this task, typically

⁶In Mpanda DC, study participants were sampled from existing resident lists that were maintained by the village government. Many were updated recently in conjunction with the 2012 Census. While it did not appear that the

the subvillage chairpersons. To ensure that the listing was complete, public meetings were held at which everyone could verify their inclusion into the list and the lists were handed over to the village government for their perusal. The listing agents knew about this in advance and therefore had an incentive to carry out a complete listing. The public meetings also served to inform the residents about the planned research activity, to answer questions, and to build trust, so that most residents felt comfortable volunteering to participate in the study.

Within villages, the sample was stratified by subvillage, with sample sizes proportional to the number of households in each subvillage. Within subvillages, households were sampled at random with equal probabilities of inclusion.⁷ In the sampled households, either a male or a female household member between 18 and 70 years of age was invited to take part in the study, the gender being chosen at random. If more than one person of the chosen gender met the inclusion criteria, then the person whose birthday was coming up soonest was invited. If no household member met the inclusion criteria and consented to participating in the study, then the household was replaced by another randomly selected household from the same subvillage.⁸

quality and completeness of these lists was systematically related to governance outcomes in the village, the approach was nonetheless abandoned to eliminate the risk of sampling bias.

⁷The sample was drawn at the public meeting, using a tablet computer application developed for this purpose. The transparent demonstration of the sampling process was important to build trust in the villages.

⁸The number of households sampled was 50 % greater than the number of participants needed for the study. The reserve households would be approached in a random order of priority.

A.8 Instruction Scripts for the Decision Exercise

ACC

GENERAL INSTRUCTIONS

ENG

[BLOCK 1]

Thank you for participating in the survey. Now 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 after we have done the interview with them.

Let me explain what we will do with the money we collect in this exercise: For each 100 Shilling coin a person contributes, we will add another 100 Shilling coin. Then we will put all the contributions from you and the other seven people together. 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.

Thus, if YOUR PROPOSAL is drawn and you proposed to have all the money split equally among all members of the group, then you will get the same share of the group money as everyone else. On the other hand, if you proposed to keep all of the group money for yourself, then you will get all of the group money, and none of the other seven participants will get anything from the group money. If you proposed to keep half of the group money for yourself and to have the other half split equally among all members of the group, then you will get half of the group money, plus your share in the other half of the group money which is being split equally among all eight participants.

Of course, this is what happens if YOUR PROPOSAL is drawn. If ANOTHER PERSON'S PROPOSAL is drawn, then the money will be distributed according to that person's proposal, not according to your proposal.

We will come back on [INSERT DATE AND TIME] to give people their share of the group money from each of their exercises. We cannot give you the money before [INSERT DATE], because we first have to go around and collect the money and the proposals from each of the eight people, including you.

Now let me ask you some questions. It's ok if you don't answer the questions correctly at first; I will just repeat the instructions of the exercise.

- From how many people will we collect money in this exercise?
- What will happen with the money we collected?
- Among whom is the group money going to be distributed?
- How is the money going to be distributed?
- How do we select the proposal according to which the money is distributed?
- What are we asking you to do in this exercise?
- If your proposal is drawn, what will happen to the money you proposed to allocate to the group?
- If your proposal is drawn, what will happen to the money you proposed to allocate to yourself?
- When will you get your share of the money?
- Why can we not give the money to you before?

[BLOCK 2]

Great, I think you understand. First we will be collecting group money, then we will ask each group member to make a proposal on how to distribute the group money.

Let me give you some examples:

- If all eight people contribute 1000 Shillings each, then there will be 16,000 Shillings in group money. If a proposal is drawn that states that all group money should be split equally among the eight people, then each will get 2000 Shillings back from the group money.
- If there are 16,000 Shillings in group money and a proposal is drawn that states that half of the group money should be split equally among the eight people and the other half should be given to the proposer, then the proposer will get $8,000+1000=9,000$ Shillings back from the group money and the other seven people will each get 1000 Shillings back.
- If there are 16,000 Shillings in group money, and a proposal is drawn that states that one fourth of the group money should go to the proposer and three fourth should be split equally, then the proposer will get $4000+1500=5500$ Shillings back from the group money and the other seven people will each get 1500 Shillings back.
- If three people contribute 1000 Shillings each, two people contribute 800 Shillings each, four people contribute 100 Shillings each and one person contributes nothing, then there will be $5000*2=10,000$ Shillings in group money.
- If nobody contributes anything, then there will be no group money to distribute.

Do you understand why, or should I explain this again?

[BLOCK 3]

I will give you ten 100 Shilling coins [SHOW THE COINS, BUT DO NOT HAND THEM OVER YET]. You will have to decide how many coins you want to share with the group and how many coins you want to keep for yourself.

Please put all of the coins that you want to keep for yourself into the jar that is labeled "SELF". We will keep this money for you and give it to you when we come back on [INSERT DATE].

Please put all of the coins that you want to share with the group into the jar that is labeled "GROUP". We will double the money you put into the group jar and put it together with the contributions from the seven other participants of this exercise.

When we come back on [INSERT DATE], we will give each participant their share of the group money.

But before that, all players have to make their proposals on how to distribute the group money. To make your proposal, I will put twenty four bottle caps into the group jar. This will symbolize the group money. By taking bottle caps out of the "GROUP" jar and placing them into the "SELF" jar, you can indicate what share of the group money you propose to keep for yourself. The remaining share will be split equally among all eight participants, including you.

Now let me ask you some more questions. It's ok if you don't answer the questions correctly at first; I will just repeat the instructions of the exercise.

- What will happen to the money that you put in the "SELF" jar?
- What will happen to the money that you put in the "GROUP" jar?
- If you want to propose to keep half of the group money for yourself, how do you do that?

Before you make your decision, please be aware of the following:

1. Your decision on how many of the ten 100 Shilling coins you contribute to the group will be **SECRET**.
2. Your proposal on how to split the group money will **NOT BE SECRET** in this exercise. I will record on the computer how much you proposed to keep for yourself. If your proposal is selected, then we will tell each of the seven other participants in this activity that it was YOUR proposal which was selected. If your proposal is not selected, then we won't tell anybody about your proposal.
3. 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.

Let me ask you some final questions:

- Where are the other seven participants in this exercise from?
- Will we tell anybody how many of your ten 100 Shilling coins you contributed to the group?
- Will we tell anybody how much of the group money you proposed to keep for yourself?
- Will we tell the village government how much of the group money you proposed to keep for yourself?
- Are people allowed to talk with others about this information?

Here I have twenty four bottle caps in the jar with the "GROUP" symbol. These bottle caps symbolize the group money.

To make your proposal, please leave the bottle caps that you propose to allocate to the group in the jar and take the bottle caps that you propose to allocate to yourself out of the "GROUP" jar and put it into the jar with the "SELF" symbol, then I will record your decision on the computer.

Before you make your decision, please be aware of the following:

1. Your decision on how many of the ten 100 Shilling coins you contribute to the group will be **SECRET**.
2. Your proposal on how to 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.
3. 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.

Let me ask you some final questions:

- Where are the other seven participants in this exercise from?
- Will we tell anybody how many of your ten 100 Shilling coins you contributed to the group?
- Will we tell anybody how much of the group money you proposed to keep for yourself?
- Will we tell the village government how much money you proposed to keep for yourself?

Here I have twenty four bottle caps in the jar with the "GROUP" symbol. These bottle caps symbolize the group money.

To make your proposal, please leave the bottle caps that you propose to allocate to the group in the jar and take the bottle caps that you propose to allocate to yourself out of the "GROUP" jar and put it into the jar with the "SELF" symbol, then I will record your decision on the computer.

ACC	STRANGER/PUBLIC	ENG
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Before you make your decision, please be aware of the following:

1. Your decision on how many of the ten 100 Shilling coins you contribute to the group will be **SECRET**.
2. Your proposal on how to split the group money will **NOT BE SECRET** in this exercise. I will record on the computer how much you proposed to keep for yourself. If your proposal is selected, then we will tell each of the seven other participants in this activity that it was YOUR proposal which was selected. If your proposal is not selected, then we won't tell anybody about your proposal.
3. The other seven participants will all be **FROM DIFFERENT VILLAGES OF THIS DISTRICT**. We will ask each of them to make a proposal on how to split the group money between themselves and group. No two people in the group will be from the same village.

Let me ask you some final questions:

- Where are the other seven participants in this exercise from?
- Will we tell anybody how many of your ten 100 Shilling coins you contributed to the group?
- Will we tell anybody how much of the group money you proposed to keep for yourself?
- Will we tell the village government how much of the group money you proposed to keep for yourself?
- Are people allowed to talk with others about this information?

Here I have twenty four bottle caps in the jar with the "GROUP" symbol. These bottle caps symbolize the group money.

To make your proposal, please leave the bottle caps that you propose to allocate to the group in the jar and take the bottle caps that you propose to allocate to yourself out of the "GROUP" jar and put it into the jar with the "SELF" symbol, then I will record your decision on the computer.

ACC**STRANGER/SECRET****ENG**

Before you make your decision, please be aware of the following:

1. Your decision on how many of the ten 100 Shilling coins you contribute to the group will be **SECRET**.
2. Your proposal on how to 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.
3. The other seven participants will all be **FROM DIFFERENT VILLAGES OF THIS DISTRICT**. We will ask each of them to make a proposal on how to split the group money between themselves and group. No two people in the group will be from the same village.

Let me ask you some final questions:

- Where are the other seven participants in this exercise from?
- Will we tell anybody how many of your ten 100 Shilling coins you contributed to the group?
- Will we tell anybody how much of the group money you proposed to keep for yourself?
- Will we tell the village government how much of the group money you proposed to keep for yourself?
- Are people allowed to talk with others about this information?

Here I have twenty four bottle caps in the jar with the "GROUP" symbol. These bottle caps symbolize the group money.

To make your proposal, please leave the bottle caps that you propose to allocate to the group in the jar and take the bottle caps that you propose to allocate to yourself out of the "GROUP" jar and put it into the jar with the "SELF" symbol, then I will record your decision on the computer.

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