

Elections and Embezzlement

Experimental Evidence from Burkina Faso

Malte Lierl



WORLD BANK GROUP

Development Research Group

Impact Evaluation Team

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Abstract

Can democratic elections reduce rent extraction by public decision makers? Existing research suggests that reelection incentives can reduce the embezzlement of public funds. This paper examines three additional mechanisms through which democratic elections could have an impact on embezzlement, even in the absence of reelection incentives: (1) electoral selection effects, (2) social norms and norm enforcement, and (3) citizens' trust in decision makers. Evidence from an experiment with 472 groups of citizens in rural Burkina

Faso suggests that electoral selection favors benevolent candidates. Furthermore, elections increase citizens' willingness to punish corrupt decision makers, even if their ability to do so remains unchanged. However, these beneficial effects of elections are offset by an unexpected adverse effect: elections cause citizens to trust decision makers more than they should be trusted. These findings have important implications for the role of information in electoral democracy.

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Elections and Embezzlement: Experimental Evidence from Burkina Faso

Malte Lierl*

JEL codes: D72, D73, C92, C93, H11

On October 31, 2014, a popular uprising in Burkina Faso put an end to president Blaise Compaoré's attempt to manipulate the constitution to allow himself and the dominant ruling party to stay in power. Frustrated with decades of political corruption, citizens and civil society organizations took the future of their country into their own hands and demanded free and fair elections. Compaoré was forced to resign and left the country. After a year of political transition and a failed coup by the former regime's security apparatus, peaceful national elections were held in November 2015, followed by municipal elections in May 2016. The transition to competitive, multi-party democracy brought greater political freedom and more active political competition to Burkina Faso. But is it reasonable to hope that competitive elections will also bring a reduction in corruption?

The connection between electoral democracy and corruption is important for various reasons, one of them being that elections are merely one of several methods of delegating democratic authority. Instead of being elected, public decision makers could alternatively be appointed by higher-level authorities (e.g. regional governors by the national government), identified by deliberation and

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consensus (e.g. heads of international organizations), or even selected at random (e.g. important executive, legislative and judicial positions in Athenian democracy were filled by lot). Yet, despite potential alternatives, nearly all contemporary democracies rely on elections to fill the most important positions of power. In fact, it is the demand for free and fair elections for which citizens risked their lives in pro-democracy protests, such as in Tunisia in 2011 and in Burkina Faso in 2014 and 2015. What makes elections so special, relative to other democratic methods of delegating public authority? Can elections actually lead to better governance outcomes, and if so, through what mechanisms?

One of the most prominent arguments in favor of elections is political accountability. Elections are a mechanism through which citizens can potentially reward or punish public decision makers. For example, [Ferraz and Finan \(2011\)](#) show that reelection incentives reduced corruption by local politicians in politically contested Brazilian municipalities. However, in other contexts, electoral accountability is often inherently limited. Such limitations can be due to institutional features, for example if term limits eliminate reelection incentives. However, even if reelection is possible, local party hegemony can render elections uncompetitive. Moreover, the discrete timing of elections forces voters to consider valence issues, such as corruption, simultaneously with programmatic issues, which may cause voters to prefer certain candidates for programmatic or ideological reasons, even if they are known to be corrupt. Finally, even if voters do have the ability to punish corrupt incumbents electorally, they may not have a preference for doing so, as studies in Brazil ([Winters and Weitz-Shapiro, 2013](#)) and India ([Banerjee et al., 2014](#)) have suggested. As a consequence, the importance of electoral accountability for public corruption can easily be overstated.

It is also possible that electoral accountability merely substitutes for other channels of political accountability. Rather than articulating their discontent at the polls, citizens could make complaints to higher-level authorities, participate in public protests, or exert accountability pressures through informal social sanctions ([Tsai, 2007](#)). If alternative forms of accountability exist, the impact of reelection incentives on corruption outcomes could again be marginal. To understand how elections can affect governance outcomes, such as corruption and embezzlement, it is therefore important to look beyond electoral accountability and explore potential additional mechanisms.

This paper will examine three alternative channels of influence, other than electoral accountability, through which democratic elections could have an impact on corruption outcomes. First, electoral selection effects. Do elections enable citizens to select benevolent decision makers who are intrinsically motivated to refrain from corruption? Second, social norms. Do elections influence how much corruption or embezzlement citizens are willing to tolerate? Third, citizens' trust in politicians. How do elections affect expectations about corruption, and what does this imply for governance outcomes?

To shed light on these mechanisms, a field-based behavioral experiment was carried out with 472 groups of voting-age citizens in 118 rural municipalities in Burkina Faso. The experiment was designed to identify the impact of elections on the embezzlement of public resources, focusing on a situation in which citizens' *ability* to sanction public decision-makers is held constant by design. There were no re-election incentives in the experiment. Instead, citizens were able to sanction decision makers by other means, independently of whether the decision makers were elected or not. The experiment examined a very straightforward social dilemma situation: Groups of citizens received a budget of unknown magnitude that could be embezzled by a decision maker in their midst. It was varied whether decision makers were elected or randomly selected. After a decision maker was selected, it was additionally varied whether the decision maker would have private information about the group budget, i.e. the ability to conceal embezzlement, or whether the budget would be transparent to the group. Using this experimental setup, the paper makes three important contributions to our understanding of the causal mechanisms through which elections can affect the embezzlement of public funds.

A first contribution is to shed light on electoral selection effects. Electoral selection effects have long been suspected to play a powerful role in preventing corrupt candidates from gaining access to public office (Besley, 2005). Yet, empirical evidence on corruption and electoral selection is almost completely lacking. Lierl (2014) shows that village elections can be effective at identifying benevolent leaders who are intrinsically more motivated to refrain from embezzlement than externally appointed village leaders or ordinary village residents. However, villages are high-information environments, where voters frequently interact with candidates in everyday life. This paper examines

electoral selection effects in a low-information environment, in which voters have only limited opportunities to learn about candidates' true character and intentions. It is shown that competitive elections can favor benevolent candidates even if voters and candidates are mutual strangers and are given only ten minutes of face-to-face communication in a group setting.

A second contribution is to test whether elections reduce citizens' tolerance for embezzlement. This speaks to the larger question of whether a formal institution like elections can influence informal social norms and voluntary norm enforcement behavior. Prior research has examined the impact of elections on voluntary compliance with authorities ([Baldassarri and Grossman, 2011](#); [Grossman and Baldassarri, 2012](#)) and collective action ([Baldwin and Mvukiyehe, 2015](#)). This paper proposes that elections might additionally change the social norms by which decision makers are evaluated, as well as citizens' willingness to enforce such norms. Consistent with either mechanism, the experiment reveals that elected decision makers are punished more severely for embezzlement than non-elected decision makers, even after correcting for endogenous electoral selection effects and adaptive behavior on the part of decision makers.

A third contribution is to examine the impact of elections on citizens' trust in public decision makers. It is shown that elections caused citizens to underestimate the embezzlement of public funds. By contrast, if decision makers were randomly selected, citizens' expectations are unbiased. The practical implications of this finding are troubling. As [Gottlieb \(2016\)](#) shows in a field experiment in rural Mali, citizens are, in fact, biased towards being too trusting towards local decision makers. This paper complements [Gottlieb \(2016\)](#), by suggesting that biased perceptions of governance quality can actually be caused or aggravated by the fact that decision makers are elected.

Taken together, the findings of this study have important theoretical implications: They suggest that democratic elections have the potential to reduce the embezzlement of public resources, even if they do not increase citizens' ability to sanction public decision makers. However, the findings also highlight that access to information plays an important enabling role. Information neutralizes citizens' tendency to be overly trusting towards elected decision makers. If citizens lack access to information, their trust can be exploited by self-interested decision makers, which diminishes the

otherwise beneficial effects of elections.

On a methodological level, the study is innovative in several respects. It is one of the largest behavioral experiments in political economy that has been carried out directly in the population of interest. From the population of 118 rural municipalities in Burkina Faso, stratified by village, 2,360 study participants were sampled at random. The research objectives, as well as the analytical approach, were pre-specified in a detailed analysis plan. The experiment used innovative techniques to minimize interviewer effects and to preserve procedural rigor and confidentiality while making a complex group decision exercise accessible to illiterate populations. This included intuitive touch screen interfaces to record decisions and synchronized video instructions in nine vernacular languages, integrated into a tablet application that controlled and monitored the actions of human facilitators. As a result, the experiment achieved a level of data quality and replicability that is on par with laboratory studies, while studying a decision situation that involved face-to-face interaction between study participants in a challenging field setting.

The paper proceeds as follows. Section two contrasts elections with other methods of delegating democratic authority. Section three explains the case selection and context of this research. Section four describes the experimental design and procedures. Section five presents the aggregate effects of elections and transparency on the embezzlement of group resources, followed by a discussion of the underlying causal mechanisms. Section six discusses the construct validity, theoretical generalizability and external validity of the experiment. Section seven discusses the implications for the role of information in electoral democracy. Section eight concludes by summarizing the central findings.

Elections and Alternatives

There are several different ways in which democratic authority can be delegated to individual decision makers: through competitive elections, through bureaucratic appointments, by consensus, or by randomly selecting a decision maker (also known as sortition). The need to choose between al-

ternative methods of identifying public decision makers arises in many domains of society, including government. For instance, decentralization reforms frequently create new democratic institutions at the local level, such as municipal councils, mayors, management committees, etc. Whenever new institutions are created, it is a first-order concern to ensure that they serve the collective interests of their constituents, rather than becoming vehicles for rent extraction (Myerson, 2015). Similar institutional design questions arise outside of government, for example in community development programs (where a major concern is the capture of projects by local elites (Mansuri and Rao, 2013)), in membership-based organizations (e.g. associations, cooperatives (Grossman, 2014), or user groups), and in corporate governance. In each of these cases, the need to minimize opportunities for embezzlement or rent extraction is a universal concern.

A small but growing number of rigorous experimental and observational studies have begun to systematically compare democratic elections to other types of political institutions, including traditional authorities (Baldwin and Mvukiyehe, 2015; Beath et al., 2014), single-party rule (Martinez-Bravo et al., 2014), direct democracy (Olken, 2010), and bureaucratic appointments (Alatas et al., 2013; Lierl, 2014). Of these, Alatas et al. (2013); Beath et al. (2014) and Lierl (2014) directly focus on the embezzlement of public resources. Alatas et al. (2013) find no difference in embezzlement outcomes between elected and appointed village leaders in Indonesia. Lierl (2014) finds that elected village leaders in Tanzania are more benevolent than appointed village leaders with regard to their social preferences, but that embezzlement outcomes become indistinguishable once social and reputational incentives are taken into consideration. Beath et al. (2014) find that the existence of elected village councils alongside traditional authorities in Afghanistan increased embezzlement overall, but reduced embezzlement if the councils were given exclusive decision authority. These ambiguous results suggest that the impact of democratic elections on embezzlement is context-dependent, which shifts the focus towards understanding the underlying causal mechanisms.

In this paper, elected decision makers will be compared to randomly selected decision makers, which is a particularly useful comparison case to illuminate the causal mechanisms by which elections can affect embezzlement outcomes. First, random selection is a benchmark against which electoral selection effects can be quantified. In sufficiently large numbers, randomly selected de-

cision makers statistically represent the constituent population. In other words, the behavior of randomly selected decision makers reveals how ordinary citizens would act if they were put in a position of responsibility. Randomly selected decision makers or policies have therefore become a standard reference point in the nascent literature on the behavioral effects of elections ([Baldassarri and Grossman, 2011](#); [Grossman and Baldassarri, 2012](#); [Corazzini et al., 2014](#)) and voting ([Bó et al., 2010](#)).

Second, random selection of decision makers is a practical alternative to elections, if a political system is at risk of being captured by powerful individuals for their personal advantage. In fact, societies that selected their leaders at random largely did so with the explicit objective of leveling the political playing field ([Carson and Martin, 1999](#); [Frey and Steiner, 2014](#)). In medieval Italian city states, the random selection of political leaders ensured the frequent rotation of power between rivaling families. When several Swiss towns began to select their mayors at random in the 17th century, they did so to ensure that nobody would gain disproportionately from holding office ([Carson and Martin, 1999](#), 22). Even in recent years, policy makers have experimented with the random selection of decision makers, for example to prevent local development grants from being captured by village chiefs in Sierra Leone ([Voors et al., 2017](#)).

Third, a comparison between elections and random selection is appealing, because both institutions are similarly egalitarian, which distinguishes them from any other method of delegating democratic authority (Table 1). Random selection gives every eligible constituent an equal chance at obtaining a position of responsibility, whereas elections give every eligible voter an equal say in the selection process. Bureaucratic appointments, by contrast, involve exclusionary selection criteria. Selection by consensus, while being inclusive of the constituent population, privileges influential constituents or coalitions via their disproportionate bargaining power. To the extent that procedural egalitarianism matters for the perceived fairness of a political institutions, both elections and random selection might rank highly on procedural fairness, since both are founded on egalitarian premises. This assumption will be verified directly.

	Competitive	Participatory	Egalitarian
Election	✓	✓	✓
Selection by consensus	✓	✓	
Bureaucratic appointment	✓		
Random selection			✓

Table 1. Ideal-typical methods of delegating democratic authority. Elections differ from random selection in that they allow for competition between candidates and collective participation in the delegation of authority. Both aspects are also relevant in comparing elections to other democratic and non-democratic methods of assigning authority.

While elections and random selection are similar on procedural egalitarianism, they differ in two important respects: Elections are participatory and competitive, whereas random selection leaves citizens passive and does not give them a choice. As far as embezzlement outcomes are concerned, this means that any impact of elections, relative to random selection, is most likely a consequence of political competition or of political participation, rather than of concerns about procedural equality.

There are several ways in which embezzlement outcomes could be influenced through the electoral competition mechanism. First, through reelection incentives, which can create accountability pressures on public decision makers (Ferraz and Finan, 2011). Second, competition causes candidates to make campaign promises, which might continue to influence their behavior through a moral commitment or lying aversion mechanism (Corazzini et al., 2014). Third, competition leads to electoral selection effects, as a result of which the social preferences of elected decision makers differ from those of the general population (Lierl, 2014). In this study, the previously studied effects of reelection incentives and campaign promises are eliminated by design, which makes it possible to directly study the impact of electoral selection effects.

The participatory nature of elections could also have important consequences for embezzlement outcomes. Elections require citizens to collectively express their consent to the delegation of authority. Electoral participation thus establishes an implicit social contract between citizens and elected decision makers. Such a social contract could make norms of trusteeship salient. Since

the embezzlement of public funds is a particularly blatant violation of the principle of trusteeship, elected decision makers who embezzle public funds might encounter different reactions from citizens than randomly selected or bureaucratically appointed decision makers.

Finally, both the anticipation of electoral selection effects and the social norms that are attached to electoral delegation could interact to shape citizens' trust in public decision makers. Citizens may expect different levels of embezzlement from elected decision makers than from randomly appointed decision makers. As long as citizens' expectations are empirically correct, they should have no bearing on the behavior of decision makers. However, if elections cause citizens to systematically underestimate the willingness of decision makers to embezzle public funds, self-seeking decision makers could exploit their trust. Transparency may be necessary to correct such perceptive biases.

By examining each of these potential channels of influence in detail, this paper aims to provide scholars, policy makers and citizens with a better understanding of the mechanisms through which elections can actually contribute to reducing rent extraction by public decision makers.

Case, Context, and Study Population

In recent years, Burkina Faso has undergone several changes in local-level political institutions. In the context of decentralization reform, nationwide local elections were introduced in 2006 and subsequent elections were held in 2012. Following the popular uprising in 2014, all elected local governments were dismissed and replaced by centrally appointed special delegations. A majority of citizens in the rural areas welcomed their dismissal, given that the previous municipal governments were elected during a period of single-party dominance and were widely perceived as corrupt and prone to embezzlement. In principle, however, most citizens continued to prefer elections over central appointments and other forms of selecting local decision makers. In May 2016, local elections were held to replace the centrally appointed special delegations. The current national government is again considering a revision of the local government code.

In debates on political decentralization in Burkina Faso, it is not uncommon for policy makers

to question citizens' ability to make efficient electoral choices at the local level, especially in the rural areas. Voters in rural Burkina Faso overwhelmingly live in poverty. The vast majority have been excluded from educational opportunities. Literacy rates are low, with only 16.3 percent of the study population having ever attended school. Without widespread literacy and formal education, and often even without electricity and cell phone coverage, word-of-mouth and radio are by far the most important sources of political information at the local level. However, the presence of municipal-level politics in public discussion falls far behind national-level politics.

In such a low-information and educationally disadvantaged environment, are democratic elections an efficient way of selecting public decision makers? Are voters able to identify benevolent candidates? How do elections influence citizens' expectations towards and evaluation of public decision makers? How willing are citizens to sanction embezzlement by local decision makers, and does this depend on whether they are elected or not? To improve the evidence base on these questions, this study was carried out with voting-age citizens in rural Burkina Faso between June and July 2015, prior to the transitional municipal elections of 2016.

The study participants are a stratified random sample of the population of all 118 rural municipalities that are located within six of Burkina Faso's 13 regions: Cascades, Centre-Est, Centre-Nord, Centre-Sud, Plateau Central, and Sahel (Figure 1). Within each municipality, ten villages were sampled at random. In municipalities with fewer than ten villages, all villages were included. Per village, two voting-age adults were invited to take part in the study. The invited study participants were sampled at random (with equal inclusion probabilities) from a census of the voting-age population in each village (census lists of residents between 18 and 70 years of age in 2014).

The 2,360 study participants provide a typical picture of the voting-age population in the rural areas of Burkina Faso. Only 16.3 percent of study participants report ever attending school (weighted population estimate for the six study regions: 17.8 percent).¹ Among those who attended school, the mean level of schooling is 5.4 years. Median age among study participants is 36 years (population estimate: 36 years) and 53.5 percent of study participants are female (population estimate: 52.1

¹Another 11.2 percent report to have attended coranic school (population: 11.6 percent).

percent). 11.0 percent report to have ever held a position of responsibility in their community (population estimate: 9.2 percent) and 62.7 percent report to have voted in the 2012 municipal elections (population estimate: 60.1).

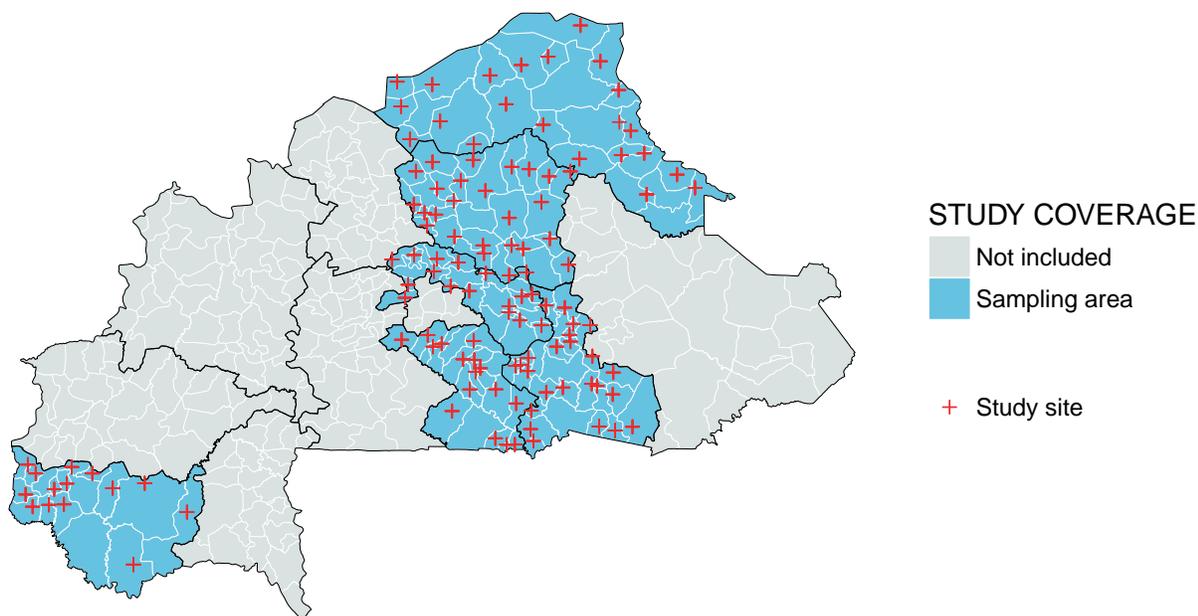


Figure 1. Study sites and sampling areas.

Experimental design

In the experiment, groups of five citizens, each from different villages in a municipality, were confronted with a simple decision situation with real stakes: An unknown amount of money was allocated to the group and a decision maker within the group could potentially embezzle these group funds for personal gain. It was varied experimentally whether the groups elected their decision maker, or whether the decision maker was appointed at random. Independently, and only after the decision maker selection had been selected, it was also randomized whether the decision maker had private information about the amount of money that was to be distributed (no transparency) or whether this amount was public knowledge within the group (transparency). Ex-ante, the chance of the decision maker having private information was 50 percent. The election and transparency treatments were assigned at the group level, in a 2×2 factorial design. Treatment assignment was

blocked by municipality, with four groups in each of 118 municipalities, resulting in a total of 472 groups in the experiment. Of the 472 groups, the experiment was successfully implemented in 471 groups.

Decision Situation

The experimental protocol consisted of the following steps:

1. Baseline decisions: Prior to the experiment, all study participants submitted a proposal on how to split 5,000 CFA Francs² between themselves and the group. The fraction allocated to the group would be shared equally among all group members, including the proposer, so that the most equitable choice was to allocate the entire amount to the group. One proposal per group would be selected at random and implemented. Through this baseline decision, study participants were familiarized with the decision situation and their individual allocation preferences were recorded. The results of the baseline decisions were not announced until the end of the meeting, to ensure that the actual experiment was not affected by income effects or information spillovers from the baseline stage.
2. Cheap talk/communication: The groups were informed that in the next stage, a substantially larger amount of money would be provided to the group. This time, only one group member would be able to decide over the group money, but the other group members would have the ability to reward or punish the decision maker at a cost to themselves. The groups were given ten minutes to freely discuss (without intervention by the facilitator) who would be the best person to make the allocation decision and how the money should be split between the group and its decision maker. This cheap talk phase gave study participants the opportunity to make inferences about other candidates' preferences through verbal and nonverbal cues. The communication took place prior to revealing the procedure by which the decision maker would actually be selected, so that the content of the communication itself was unaffected by the elections treatment.

²Approximately 10 USD at the time of the study.

3. Announcing the selection method: It was announced to the group how the decision maker was going to be selected (elected by the group or appointed at random). Furthermore, it was announced that there would be an equal chance that the available amount of group money would either be private information of the decision maker, or revealed to all group members.
4. Selection of the decision maker: Depending on the treatment condition, the decision maker was elected or randomly selected. In the election condition, all group members used a touchscreen secret ballot (Figure A-1) to cast votes for two different candidates out of the five group members. By asking group members to vote for two candidates in the first round, it was avoided that all group members just voted for themselves. Runoff elections were held with the candidates ranked first and second. If candidates tied in the first round, there could be more than two runoff candidates and subsequent runoff elections were held until one candidate won an absolute majority. In the runoff elections, group members cast one vote. The candidate who won an absolute majority became the decision maker. In the random selection condition, the decision maker was selected with equal probabilities among the five group members.
5. Announcing the transparency/no-transparency condition: All group members were informed whether the decision maker would have private information about the size of the pie (the amount of money to be shared within the group), or if this would be public information within the group. The actual amount of group money was not revealed yet, it was merely announced whether the amount would be revealed after the allocation decision had been made. If the amount of group money remained private information of the decision maker (the no-transparency condition), the other group members could not find out how much the decision maker had embezzled. If the total amount of group money was to be revealed to the other group members (the transparency condition), they would be able to find out how much the decision maker had embezzled.
6. Allocation decision: On a touchscreen and in privacy, the decision maker allocated banknotes worth 10,000 Francs (approx. 20 USD) between two stacks (Figure 2). One stack was to be

split equally among all five members of the group, including the decision maker. The other stack was to be captured by the decision maker and not shared with anyone else. Again, the most equitable choice was to allocate the entire budget to the group stack, in which case the decision maker received an equal share in the group fund. By allocating any part of the group fund to the personal stack, the decision maker could enrich her-/himself at the expense of the other group members.

7. Announcing the allocation outcome: At the time of the allocation decision, the total group budget was known only to the decision maker. Once the allocation was made, it was announced to the other group members how much money the decision maker had allocated to the group. In the public information condition, it was additionally announced how much money the decision maker had kept for her-/himself.
8. Costly sanctioning opportunity: After learning how much money was allocated to them by the decision maker (and, in the public information condition, how much money the decision maker had embezzled), the other four group members were given the opportunity to reward or punish the decision maker at a cost to themselves. For this purpose, every group member received a budget of 1,000 Francs. This sanctioning budget did not have to be fully expended, and the group members were the residual claimants. To reward the decision maker, group members could anonymously send money to her/him. To punish the decision maker, group members could anonymously pay to have money deducted from the decision maker's payoff. For every 300 Francs that were to be deducted from the decision maker's payoff, group members had to pay 100 Francs.



Figure 2. Touch screen interface to record the embezzlement decision. All group members were identified by lanyard badges in different colors. Decision makers divided the group fund between the group (represented by the badge symbols of all five group members including their own) and themselves (represented by only their own badge symbol). Decision makers could freely move the 1,000 FCFA and 500 FCFA notes between the two fields on the touch screen. Once all banknotes were allocated, decision makers confirmed their choice by touching a fingerprint symbol.

Field Procedures

The sampled study participants were randomly assigned into groups and invited to a central meeting point. To replace no-shows, additional backup participants were sampled from each of the villages

and also invited to the study site. If necessary, the backup participants would substitute for missing study participants from their village, following a precisely specified replacement protocol. The groups for the experiment were formed by randomly dividing the 20 study participants in a municipality into four groups of five individuals, subject to the constraint that the five members of a group had to be from different villages in the municipality. The groups were strictly separated to prevent information spillovers.

Prior to the decision exercise, all study participants completed a baseline survey. The baseline survey gathered information about local governance quality in the municipality and citizens' experiences with municipal authorities, as well as data on individual-level characteristics, such as age, education, occupation, household size, leadership experience, electoral participation in the last elections, and their trust in the municipal administration.

Following the baseline survey, the baseline decision exercise and the actual experiment were carried out under the supervision of two facilitators. Study participants were not allowed to communicate during the exercise, except when they were asked to. To minimize facilitator effects, the instructions for the decision exercise were video recorded, dubbed in nine vernacular languages, and played back to the study participants in their respective language. The sequencing of instructions, comprehension checks and decisions was programmed into a tablet computer application that forced the facilitators to abide by the exact same procedures in every group. The tablet app simultaneously collected background data through which the facilitators' performance could be monitored.

The video instructions were divided into multiple logical blocks. Each video block was followed by a set of comprehension checks. If a study participant in the group did not pass a comprehension check, the video block was re-played to the entire group until all participants had understood the instructions. Voting and allocation decisions were made on specifically designed touch screen interfaces. The video instructions included demonstrations of the tablet interface. Additionally, a practice tablet was passed around so that study participants could familiarize themselves with the interface. Pilot tests confirmed that the touch screen ballots and allocation interfaces were easily understood, especially by illiterate study participants.

The automated sequencing of instruction blocks also ensured that facilitators themselves did not know the treatment condition a group was assigned to, until the moment when they actually had to announce the selection method and the transparency/no-transparency to the group. This guarantees that the baseline decisions and the cheap talk/communication stage were unaffected by the facilitators' knowledge of the treatment condition.

Outcome data were collected at various stages of the decision exercise. They included (1) study participants' expectations about the amount embezzled by the decision maker, (2) the decision maker's allocation decision, (3) the amounts spent on rewarding or punishing the decision maker, and (4) an endline question about the perceived fairness of the procedure by which the decision maker was selected.

Data Analysis and Research Transparency

The experimental design, along with a detailed explanation of the research design choices and the trade-offs involved were documented in a pre-analysis plan.³ The pre-analysis plan organized the research questions hierarchically into primary outcomes and analyses related to the underlying causal mechanisms. The analysis plan also included the program code for a first pass of data analysis. This paper closely follows the pre-analysis plan, summarizing and interpreting the insights gained from these pre-specified analyses and scrutinizing the results through additional tests.

In this paper, both unweighted and population-weighted analyses of the experimental data are presented. Due to the sampling procedure, individuals from regions with fewer municipalities, municipalities with fewer villages, and villages with fewer voting-age residents were relatively more likely to be included. Therefore, analyses at the level of individual group members were weighted by the inverse of each study participants' probability of being included into the sample.

Population inferences at the level of decision makers are more difficult to accomplish, given that the election outcomes may depend on the group composition, which in turn depended on individual

³The pre-analysis plan was finalized prior to accessing any data from the experiment and posted to the EGAP design registry. An anonymized version is available at <http://bit.ly/2mRx13F>.

group members' inclusion probabilities. To address this challenge, observations from individual decision makers were weighted by the simulated likelihood that they would have been elected if the experiment had been carried out with self-weighted sample of the population of each municipality. To simulate voting outcomes in a self-weighted population sample, weighted bootstrap samples were drawn from each group (using the group members' sampling weights) and the entire election process was simulated for each bootstrap iteration via their recorded vote choices in the first round and runoff elections. For comparability, the weights for randomly selected decision makers were obtained analogously, simulating random selection instead of elections. Thus, an individual decision maker's weight can be interpreted as the estimated proportion of decision makers represented by this individual, if the experiment had been carried out infinitely many times on self-weighted samples of the respective municipality's voting-age population.

Results

The experiment illuminates three causal pathways through which democratic elections can reduce the embezzlement of group funds, even if there are no re-election incentives, i.e. even if elections do not increase group members' *ability* to sanction the decision maker. First, electoral selection effects. Second, changes in the group members' willingness to punish the embezzlement of group funds. Third, changes in group members' trust towards the decision maker.⁴ Before presenting mechanism-specific results, the aggregate effects of the elections and transparency treatments will be analyzed.

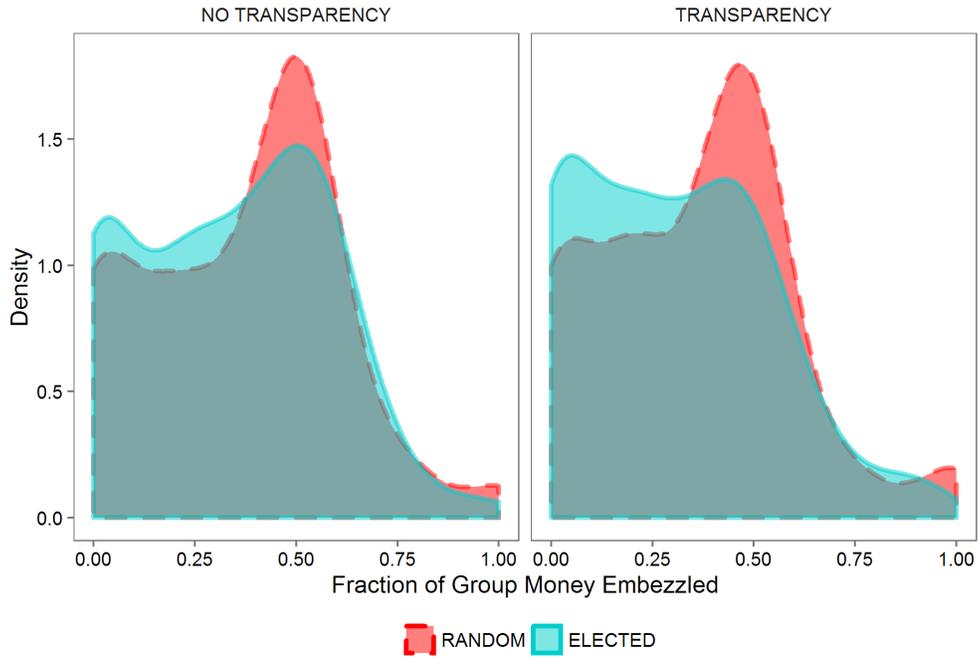
Overall, elections reduced the embezzlement of group resources in the symmetric information (transparency) condition of the experiment, but their effect remained ambiguous under asymmetric information. In the absence of transparency, elections reduced average embezzlement by 0.1 standard

⁴There are several additional potential mechanisms through which the overall effects of elections and transparency on embezzlement could have been mediated: For example, the experience of having been elected could have made decision makers intrinsically less selfish, caused them to reciprocate the support of their constituents, or increased their sensitivity to social observability, independently of actual differences in their constituents' sanctioning behavior. While these additional mechanisms remain speculative, they should be considered in interpreting the *overall* impact of the experimental treatments. However, they have no bearing on the *mechanism-specific* results in this paper.

deviations (s.e. 0.13, unweighted) and by 0.23 standard deviations (s.e. 0.13, unweighted) if embezzlement was transparent. The null hypothesis of no reduction in embezzlement was rejected only in the transparency condition ($p = 0.032$ unweighted, $p = 0.005$ weighted). Figure 3 visualizes the distribution of embezzlement outcomes by treatment condition. Table A-1 presents average treatment effect estimates with and without adjustment for group-level covariates.

The interaction effect of the election and transparency treatments is negative, but statistically indistinguishable from zero. Thus, elections and transparency appear more likely to be complements in reducing embezzlement, rather than substitutes. However, it cannot definitively be ruled out that elections have similar effects under symmetric and asymmetric information. A more disaggregated analysis of the underlying causal mechanisms can help us understand in what ways elections and transparency actually interact.

Panel A. Unweighted Results



Panel B. Extrapolation to Self-Weighted Population Sample



Figure 3. Distribution of embezzlement decisions by experimental condition (kernel density estimates, $n=471$). Top row: unweighted results. Bottom row: extrapolation to self-weighted population samples, based on the simulated likelihood of being selected as decision maker.

Electoral Selection Effects

To estimate the extent of electoral selection effects, the baseline embezzlement preferences of elected decision makers are compared to those of individuals in the random selection condition. The baseline decisions can unambiguously be attributed to individuals' preferences to refrain from embezzlement, rather than to incentives resulting from the other group members' anticipated reactions, because the baseline decision situation was fully anonymous and did not include a sanctioning stage.

Compared to individuals in the random selection condition, election winners were more benevolent at baseline ($p = 0.026$, Mann-Whitney U test; see also Fig. A-1 for a comparison of winning and losing candidates). Moreover, candidates who are more benevolent commanded greater vote shares (Table 2). This relationship is even more pronounced if ballot order effects are controlled for. Thus, the results suggest that benevolent candidates have an electoral advantage, even in a low-information environment. The social preferences of elected decision makers differ from those of randomly selected decision makers and therefore from those of the general population.

Voters' ability to elect benevolent candidates could be a consequence of their social intelligence, or of statistical discrimination. Subtle verbal or nonverbal cues in the candidates' communication might enable voters to draw inferences about the candidates' social preferences. Alternatively, voters might intentionally or unintentionally prefer certain observable candidate characteristics that correlate with benevolent social preferences, such as gender or age. Identifying the heuristics that enable voters to select benevolent candidates in low-information settings remains an open question for future research. In this study, it remains observationally equivalent whether voters discriminated on their beliefs about candidates' benevolence, or based on other characteristics that are correlated with benevolence.⁵ The data do reveal, however, a substantial correlation between citizens' expectations and decision makers' actual embezzlement behavior ($r = 0.51$). This suggests that citizens are able to predict benevolence at least to some extent (see Table A-5).

⁵A third possibility is that altruistic voters may have deliberately favored individuals whom they consider most deserving of a greater payoff – for example a visibly poor group member – and may have been willing to tolerate embezzlement by that person out of directed altruism. If this happens in the experiment, but not in natural settings, the study would be *underestimating* voters' ability to elect public-spirited decision makers.

<i>DV: Vote Share (First Round)</i>				
	population-weighted		unweighted	
	(1)	(2)	(3)	(4)
<i>OLS coefficients</i>				
Fraction embezzled (baseline)	-0.046 (0.028)	-0.051* (0.028)	-0.041 (0.025)	-0.047* (0.025)
Distance from optimal ballot position ⁶		-0.031*** (0.011)		-0.041*** (0.0093)
Constant	0.42*** (0.012)	0.47*** (0.020)	0.42*** (0.011)	0.48*** (0.018)
Groups	236	236	236	236
Observations	1180	1180	1180	1180

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

Table 2. A greater willingness to embezzle group resources is associated with a lower vote share. Columns (1) and (2): Vote shares are population-weighted. Columns (3) and (4): Unweighted data, overrepresenting voters from low-population areas.

Citizens' Willingness to Sanction Embezzlement

In all four experimental conditions, study participants were willing to sanction the embezzlement of group funds at a cost to themselves, even though they had no instrumental reason for doing so. This behavior is consistent with evidence from ultimatum games and other allocative dilemmas, where individuals willingly forgo own payoffs in order to prevent others from receiving a greater, unequal payoff (e.g. [Henrich et al., 2004](#); [Fehr and Fischbacher, 2004](#); [Leibbrandt and López-Pérez, 2012](#)). Of the study participants, 63.3 percent in the transparency condition and 63.7 percent of study participants in the private information condition spent money to reward or punish the decision maker. Study participants who rewarded or punished the decision maker spent on average 20.3 percent of their sanctioning budget. High levels of embezzlement by the decision maker

⁶Due to a recording error, information on ballot order is missing for 20 groups containing 100 individuals. Since ballot order was randomized at the group level, distance from the optimal ballot position has been imputed at mean for those groups.

typically attracted punishments, while very low levels of embezzlement and complete abstinence from embezzlement tended to be rewarded.

To understand the causal impact of elections on citizens' willingness to sanction embezzlement, the main object of interest is the functional relationship between embezzlement decisions and expected sanctioning outcomes, denoted by $g(X, E)$, where X is the fraction of group resources embezzled and E is an indicator of the elections treatment. Since embezzlement decisions X are post-treatment outcomes, the observed relationship between embezzlement decisions and sanctioning outcomes will be confounded by adaptive behavior on the part of the decision maker. If the decision maker strategically anticipates the sanctioning behavior of the other group members, embezzlement choices will be endogenous. To address this endogeneity concern, the post-treatment embezzlement decisions X are instrumented by the decision maker's baseline embezzlement preferences B , using a nonparametric instrumental regression model that is additively separable in the influence of unobservables (Horowitz, 2011).

$$S = g(X, E) + U$$

$$\mathbb{E}[U|B = b, E = e] = 0 \text{ for all } b, e$$

This model choice has the advantage that it does not impose functional form assumptions on the sanctioning function $g(X, E)$, since sanctioning behavior could be non-linear in the extent of embezzlement. Estimates of the first-stage relationships confirm that the decision maker's baseline embezzlement preferences are a strong instrument for both actual and suspected embezzlement (Table A-4).

Figure 4 presents nonparametric estimates of $g(X, E)$ in the transparency and private information conditions. If embezzlement is transparent, elected decision makers face more severe punishments than randomly appointed decision makers for the most common levels of embezzlement (greater than zero and up to about two-thirds of the endowment). At very high levels of embezzlement, it appears that the sanctioning function for elected decision makers is non-monotonic and punishments decrease again. However, these values of the sanctioning function are of little substantive relevance,

since 86 percent of elected decision makers preferred to embezzle less than two-thirds of the group fund at baseline. Most likely, the non-monotonicity of the sanctioning function at high levels of embezzlement is driven by outliers. There are very few data points at high levels of embezzlement. Additionally, no such pattern exists in the private information condition, where high embezzlement rates are almost as obvious as in the transparency condition, because the group members receive only a minimal payoff or no payoff at all.

In the private information condition, sanctioning behavior is estimated both as a function of suspected embezzlement and as a function of actual embezzlement. Relative to the average suspected embezzlement in the group, elected decision makers are punished more severely for moderate to high embezzlement than non-elected decision makers. Among decision makers who are believed not to have embezzled anything, elected decision makers are rewarded more generously than non-elected decision makers (Column 3 of Figure 4). As a function of actual embezzlement, sanctioning behavior no longer differs significantly between elected and randomly appointed decision makers, except at very high levels of embezzlement. The reason may be that high levels of embezzlement are increasingly obvious to the other group members, because the group fund was known to be greater than 5,000 Francs. If the decision maker left less than 5,000 Francs to the group, it was obvious that she or he must have embezzled at least some of the group money. Thus, while citizens are more willing to punish elected decision makers if they suspect that they engaged in substantial embezzlement, this may not translate into greater sanctions as a function of actual embezzlement, because citizens' expectation formation may be influenced by elections as well.

If elections cause citizens to *underestimate* how much embezzlement has taken place, their greater willingness to sanction elected decision makers might not translate in greater actual punishments for a given level of embezzlement. Citizens' expectations of how much the decision maker actually embezzled are illustrated in the bottom row of Figure 4, suggesting that this could be true. To test more directly whether elections bias citizens' expectations, the next step is to estimate how elections and transparency shape citizens' expectations about embezzlement.

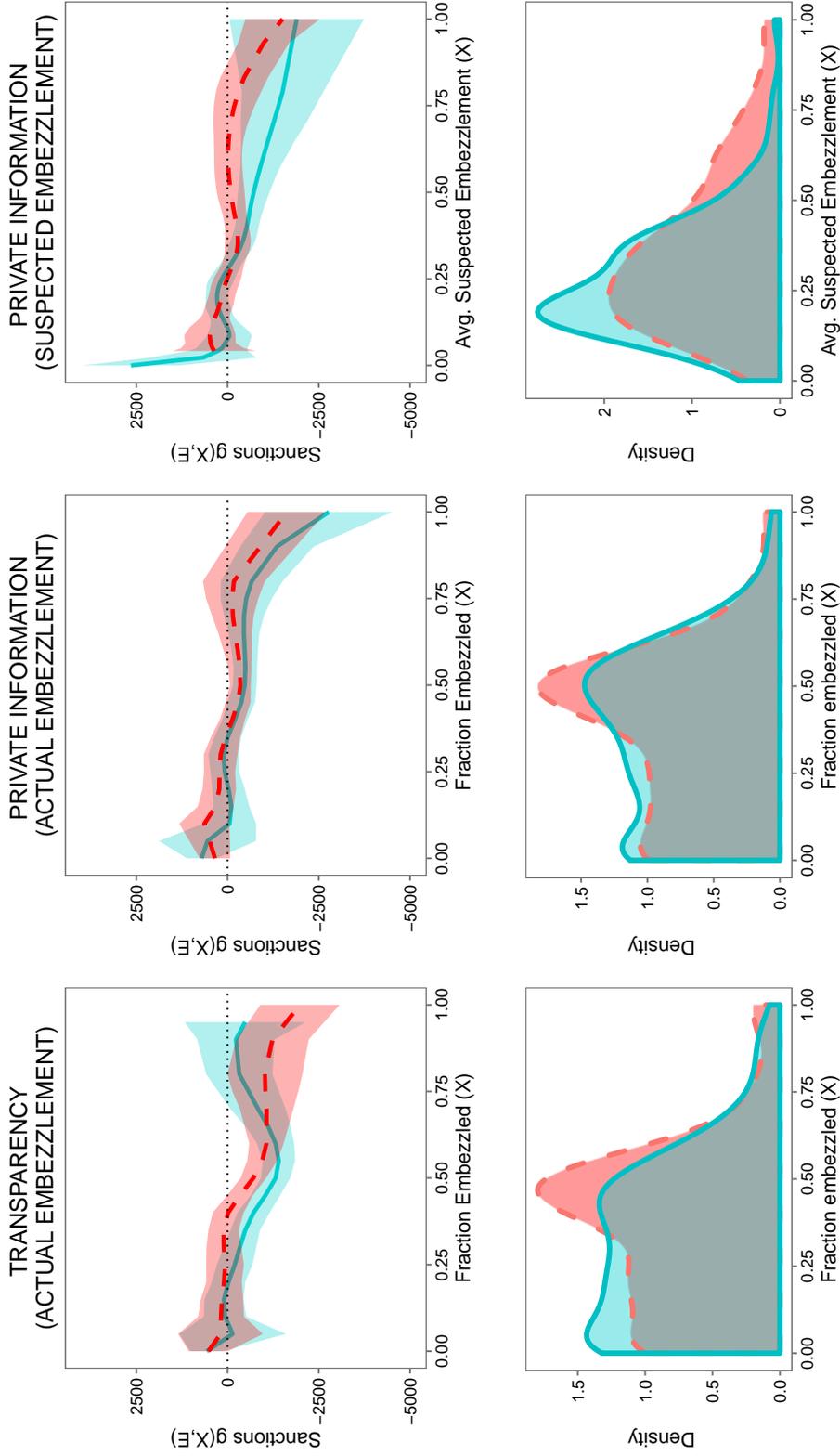


Figure 4. Combined rewards and punishments as a function of actual or suspected embezzlement. Top row: Nonparametric instrumental variable estimates of the sanctioning function, estimated via cubic B-splines, using Landweber-Fridman regularization (Racine and Nie, 2014). Embezzlement choices are instrumented by baseline embezzlement preferences. Bottom row: Distribution of embezzlement outcomes (kernel density estimates). The green solid lines represent elected decision makers, the red dashed lines represent randomly appointed decision makers.

Trust in the Decision Maker

In line with the previous argument, elections increased group members' trust in their decision maker, which is measured by their stated expectations of how much embezzlement has taken place. As evident in Figure 5, citizens' expectations in the absence of elections and transparency are unbiased. Both elections and transparency increased citizens' trust in the decision maker, but more so than they actually decreased embezzlement.

In the private information condition, average suspected embezzlement was 0.37 SD lower (s.e. 0.11) for elected decision makers than for randomly selection decision makers (Figure 5 and Table A-2). Transparency, by contrast, reduced suspected embezzlement only in the random selection condition (by an estimated 0.25 SD, s.e. 0.11), but not in the election condition (zero effect, s.e. 0.15 SD). In other words, the fact that a decision maker has been elected has a greater effect on trust than actual accountability. It is possible that citizens assume elected decision makers to be intrinsically motivated to refrain from embezzlement, whereas their trust in randomly selected decision makers can still be increased by transparency.

The actual behavior of elected decision makers does not justify the trust citizens place in them. Table 3 uses the difference between expected and actual embezzlement as a metric of bias in study participants' expectations about embezzlement. Comparing this difference across experimental conditions, it is evident that elections caused citizens to trust decision makers more than they should be trusted.

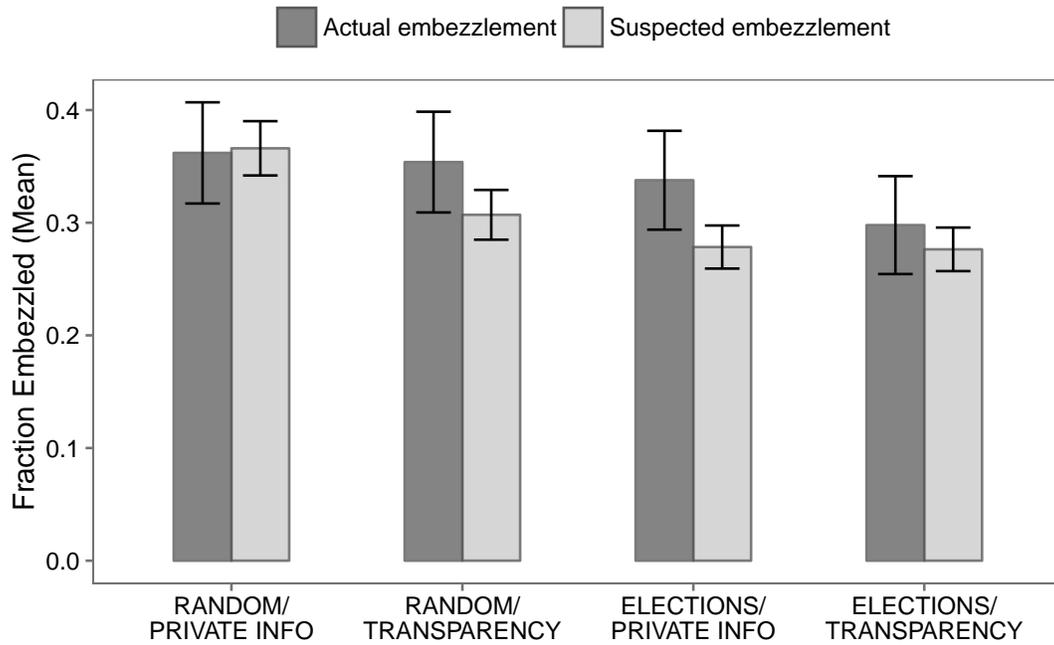


Figure 5. Citizens underestimate embezzlement by elected decision makers. The bars represent, by treatment condition, unweighted means (with 95% CI) of actual and suspected embezzlement.

DV: Difference between the expected and the actual fraction embezzled

	Population-weighted (1)	Unweighted (2)
Intercept	0.0023 (0.015)	0.0041 (0.019)
Elections	-0.055** (0.023)	-0.063** (0.026)
Transparency	-0.037** (0.021)	-0.051** (0.025)
Elections × Transparency	0.059* (0.031)	0.089** (0.035)
Obs.	1884	1884

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

Table 3. Elections and transparency bias citizens' expectations. The table shows OLS coefficients, the dependent variable being the difference between suspected and actual embezzlement.

Construct Validity, Generalizability, and External Validity

To what extent can we draw conclusions from the experiment about embezzlement of public funds in natural settings? To answer this question as thoroughly as possible, it is helpful to first examine the construct validity of the experimental setting itself, i.e. the question whether embezzlement in the experimental setting is a plausible model of the embezzlement of public funds in natural settings. After establishing that the experimental decision situation sufficiently resembles the management of public funds in natural settings, the discussion will be extended to the theoretical generalizability of the experimental results, as well as to the representativeness of the study population.

Construct Validity

The experiment confronted decision makers with a decision problem that differed in several ways from more natural opportunities for rent extraction by public decision makers. The embezzlement choices in the experiment were highly stylized, in the sense that any contextual elements that did not directly pertain to the problem of choosing between an equitable allocation of group funds and self-interested appropriation were eliminated by design. For example, the instruction videos cautiously avoided any type of normative framing, including any references to the normatively loaded concept of embezzlement. Moreover, even though the experimental decision concerned a non-trivial amount of real money, there was no specific purpose attached to the group fund, other than its equitable distribution within the group. In reality, public funds are often raised or earmarked for a specific purpose, and their misappropriation creates an efficiency loss, because a particular public good or service is underprovided. In the experiment, embezzlement did not cause any efficiency loss. Its consequences were purely distributional, while the overall size of the pie remained the same.

Yet, despite these obvious differences between the experimental setting and real-world problems of managing public funds, study participants overwhelmingly thought of the decision situation in those terms. At the end of the decision exercise, study participants were asked what real-life situation the decision exercise reminded them of (Figure 6). This was an open-ended question; no

response options were read to the study participants. For almost two-thirds of study participants, the experimental decision situation resembled problems of governance or public management. Of the study participants, 39.6 percent named very pertinent situations in which public resources are managed on behalf of a group: Management of an association, a savings group or a self-help group, municipal or village governance, management of public services, national politics, and even decisions over household finances. Another 16.8 percent felt reminded of national, local, or other elections, and 5.1 percent described the situation as an instance of corruption, trusteeship, or violations of trust more generally (e.g. “how our leaders steal from us”), without giving a specific example. Only approximately 4 percent of the study participants thought of the decision situations in completely different terms.⁷ The remaining answers lacked specific examples, but were not inconsistent with the purpose of the experiment.⁸ These minor exceptions notwithstanding, the virtual absence of incorrect interpretations suggests that any additional framing of the decision situation would have been unnecessary and potentially detrimental, as it could have introduced social desirability bias or distracted from the basic logic of the decision situation.

As far as the elections treatment is concerned, construct validity is not an issue, because the elections of decision makers in the experiment were real and not in any way stylized or simulated. Study participants had no difficulties understanding the procedures and their meaning. A voting booth and a touch screen ballot ensured that vote choices were anonymous and not observable to the facilitators. Votes were counted automatically and vote counts were reported back to the group, to determine the candidates for the runoff stage. Thus, the elections in the experiment did not substantively deviate from familiar procedures in natural settings, be it to elect the leader of a community organization or the president of a country.

⁷3.2 percent thought of it as an education or learning activity, 0.6 percent compared it to a lottery, and only three individuals primarily described it as a research or measurement exercise.

⁸These answers either mentioned abstract moral values (honesty, altruism, solidarity, and mutual understanding) without describing specific real-life situations (7.3 percent) or described the decision situation in general terms (e.g. “managing money” or “sharing money”, 2.8 percent). 4.8 percent did not feel reminded of any specific real-life situation and 15.9 percent chose to skip the question.

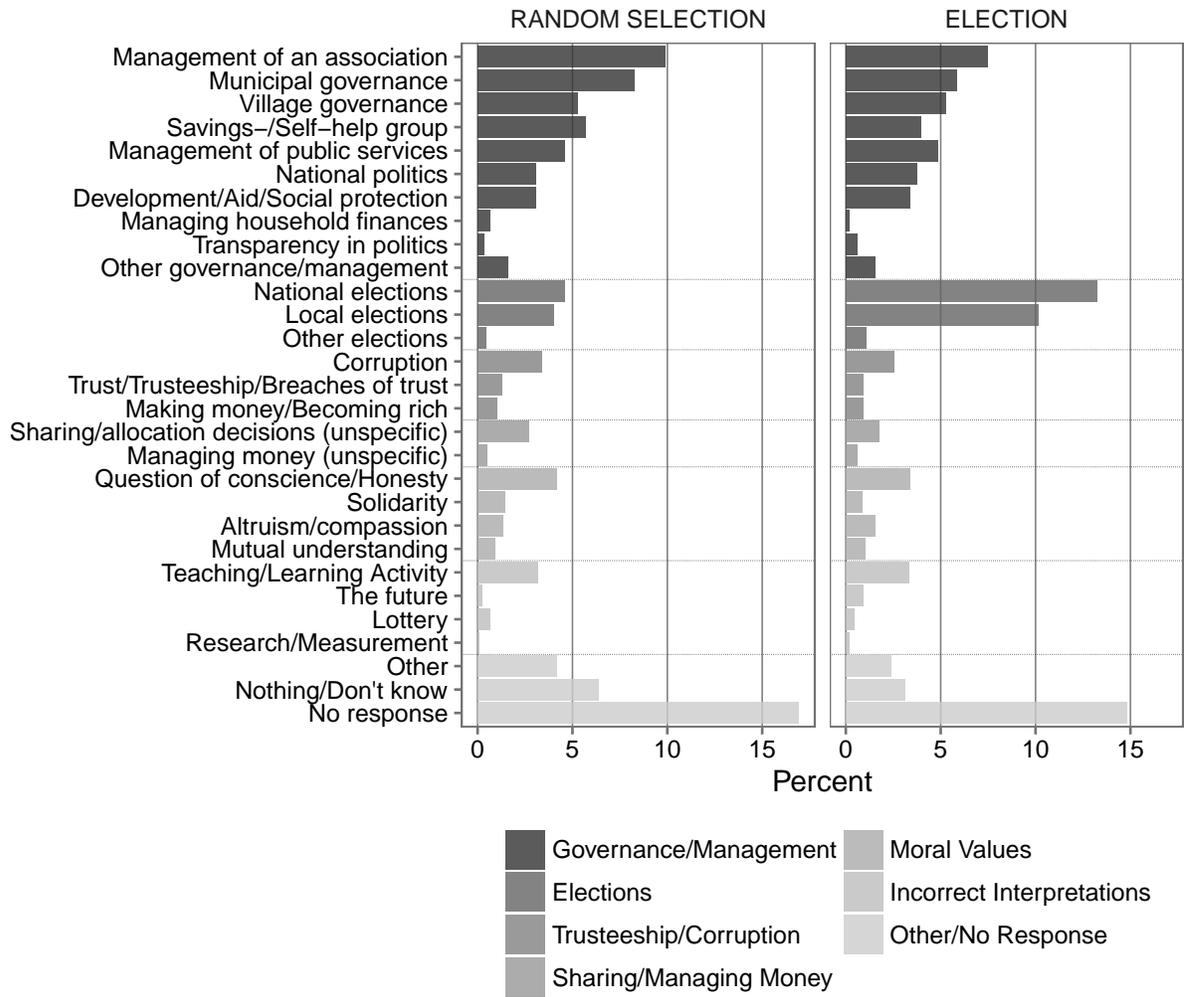


Figure 6. Study participants’ interpretations of the experimental decision situation. Classification of responses to the open-ended question: “*What situation in real life did the decision exercise remind you of?*” ($n = 2350$).

Generalizability

The parsimonious and stylized nature of the experimental setting has distinct advantages when it comes to causal attribution and generalizability. By minimizing contextual influences, the experimental setting strengthens causal attribution to the mechanisms of interest. Additionally, for each of the three main findings, there are certain features of the experimental setting that strengthen

their generalizability to natural settings.

The electoral advantage of benevolent candidates

The evidence on positive electoral selection effects is strengthened by the fact that it was obtained in a low-information setting with minimal prior communication between candidates and voters. To create a low-information environment, group members were sampled from different villages within a municipality and their real names were not revealed, so that voters' choices in the experiment were unlikely to be influenced by pre-existing social ties or knowledge of candidates' social reputations. Communication between group members was limited to ten minutes of group conversation immediately prior to the selection of the decision maker. This made it potentially more difficult for voters to identify benevolent candidates. Additionally, the study was carried out on an educationally disadvantaged population with only limited prior exposure to truly competitive democracy. If mostly illiterate voters, socialized in a dominant-party regime, are able to elect benevolent decision makers with minimal information, we might expect that the results generalize to settings where voters have more information and more experience with electoral competition. Indeed, the result is consistent with prior evidence of positive electoral selection effects in village elections ([Lierl, 2014](#)), a high-information setting where voters interact with candidates in everyday life and know their social reputations.

In future research, it will be important to investigate whether the electoral advantage of benevolent candidates extends to situations that involve professional politicians. While higher-level elections, such as municipal or national elections, are typically low-information environments (given that there is only limited and superficial interaction between voters and candidates), professional politicians might be particularly skilled at manipulating and deceiving voters. Corrupt politicians might be able to overcome their electoral disadvantage either by subtle manipulation, or by engaging in clientelism, vote-buying or identity-based appeals. Therefore, it should be a priority for future research to extend the measurement of social preferences to professional politicians, to understand the heuristics that enable voters to identify benevolent candidates, and to investigate the strategies

by which self-seeking candidates might be able to overcome their electoral disadvantage. But even if it turns out that professional politicians are able to effectively deceive voters, the experimental results might still be informative for elections in non-professionalized governance bodies, such as citizen committees, school boards, or associations.

Elected decision makers are sanctioned more severely

A second insight from this study is that citizens are less willing to tolerate embezzlement by elected decision makers than by non-elected decision makers. Several features of the experimental setting ensure that this effect can be causally attributed to citizens' *preferences* to sanction embezzlement, rather than to changes in their ability to do so, to some form of instrumental rationality, or to conformity effects. First, citizens' opportunity costs of sanctioning the decision maker were held constant across the election and random appointment conditions. In this respect, the experimental setting intentionally deviated from some natural settings, where reelection opportunities or other features of electoral democracy might alter the opportunity costs of sanctioning corrupt decision makers. The advantage is the experimental results cannot have been caused by endogenous changes in the opportunity costs of sanctioning the decision maker. Second, by eliminating repeated interaction and communication during the sanctioning stage, the experimental setting removed any instrumental rationality for engaging in costly sanctioning behavior, as well as any possibility for group members to coordinate around sanctioning the decision maker, or to observe each others' sanctioning behavior. Consequently, the treatment effect cannot have been caused by changes in group-level collective action capacity, incentives to conform to socially desirable behavior, reputational concerns, or any other form of instrumental rationality.

The neutral framing of the experimental setting ensured that the effect of elections on citizens' willingness to sanction embezzlement was not due to any cues in the instruction script that could have primed them to hold elected decision makers to higher moral standards than non-elected decision makers. Moreover, by conditioning on actual embezzlement outcomes, it was ruled out that differences in sanctioning behavior between the election and random selection conditions were

merely driven by positive or negative reciprocity towards the decision maker.

It also appears unlikely that the effect of elections on sanctioning behavior was driven by spite or envy towards the decision maker. This might have been the case if group members believed that the elected decision makers obtained their position in an unfair manner. However, an overwhelming majority (94 percent) of study participants in the election condition considered the selection procedure to be fair (Table A-3). This is an even greater proportion than in the random selection condition (91 percent) and the difference is significant at $p = 0.03$ (two-sided test of proportions).

Thus, the most plausible explanation for the effect of elections on study participants' willingness to sanction embezzlement has to do with social norms: Citizens either apply different social norms to elected and non-elected decision makers, or they are more willing to enforce such norms towards elected decision makers. This is consistent with the idea that elections make norms of trusteeship salient, because citizens actively participate in the delegation of authority.

Citizens are too trusting towards decision makers

A third insight from the experiment is that elections bias citizens' expectations, causing them to trust elected decision makers more than they should. This finding is strengthened by external evidence that citizens *are actually too trusting* towards elected local decision makers. [Gottlieb \(2016\)](#) shows that voters in rural Mali tend to overestimate the performance of elected local decision makers, unless they are presented with credible benchmarks that correct their misperception. Given that rural populations in Mali and Burkina Faso are culturally, economically and geographically proximate, it would not be surprising if similar evidence were to be found in other West African societies and beyond. The contribution of this study goes beyond Gottlieb's 2016 findings, by showing that elections can *cause* such misperceptions.

External Validity

In addition to the design features that strengthen causal attribution and theoretical generalizability of the main findings, the study also benefits from the representativeness of its subject pool. In most laboratory and field-based behavioral studies, external validity is severely limited by the fact that they are conducted on convenience samples, often from a single location. Carried out on randomly sampled rural, voting-age citizens in six regions of Burkina Faso, this study produced population-weighted estimates that are statistically representative for the population of interest.

Implications for the Role of Information in Electoral Democracy

The findings of this study suggest a novel perspective on the role of information in electoral democracy. Conventional wisdom holds that citizens' access to information is vitally important for electoral democracy, because a lack of information constrains citizens' ability to select competent and benevolent decision makers and to hold incumbents accountable (Fearon, 1999; Ferraz and Finan, 2008, 2011). Without challenging the importance of information for retrospective electoral accountability, this study has two additional implications for the role of information in electoral democracy. First, information might be less important for electoral selection than previously assumed. Second, access to information matters for accountability, because citizens are otherwise biased towards trusting elected decision makers more than they should.

With regard to electoral selection, the experiment has demonstrated that elections can favor benevolent candidates even in a low-information environment, corroborating evidence of positive electoral selection effects in a high-information setting (Lierl, 2014). In this study, voters and candidates were mutual strangers. Communication between them was limited to ten minutes of unstructured deliberation in a group setting. Even after this minimal opportunity for cheap talk, elections were superior to random selection at selecting benevolent decision makers. Thus, detailed information about candidates might be less important for electoral selection outcomes than conventionally assumed. Voters' social intelligence or statistical discrimination might enable them to identify

benevolent candidates even in the absence of verifiable factual information.

Voters' ability to identify benevolent candidates also has implications for electoral accountability. As [Fearon \(1999\)](#) points out, it is only rational for voters to replace a corrupt or non-performing incumbent, if they can expect to actually elect a better candidate. This means that electoral accountability directly depends on voters' ability to identify benevolent candidates with limited information: The more electoral selection favors benevolent candidates, the more difficult it becomes for corrupt incumbents to be reelected.

The experiment produced two additional insights that have important implications for the relationship between information access and electoral or non-electoral accountability. First, elections appear to increase citizens' willingness to sanction embezzlement. Second, elections bias citizens' perceptions of decision makers' willingness to embezzle public resources, causing them to be too trusting towards elected decision makers. As a consequence, citizens' increased willingness to sanction embezzlement might not translate into greater accountability pressures on elected decision makers, unless citizens are simultaneously provided with information about the true extent of embezzlement. If citizens lack access to such information, self-interested decision-makers can exploit the unjustified trust elections have bestowed on them. Conversely, if embezzlement is transparent, these misperceptions are corrected. Therefore, information plays an important enabling role for elections to reduce embezzlement through a social norm and sanctioning mechanism.

Finally, there is one additional way in which information access might interact with voters' willingness to sanction embezzlement: Citizens' willingness to punish corrupt decision makers might depend on their certainty about their beliefs. Citizens might be more willing to punish decision makers based on facts, rather than mere suspicion. If that is the case, information would be important for electoral democracy not only because it corrects perceptive biases, but also because it inherently increases citizens' willingness to sanction embezzlement. This hypothesis can directly be tested in the experiment. Figure 7 visualizes average rewards and punishments for given levels of embezzlement. It appears that study participants are more willing to punish the decision maker based on factual knowledge in the transparency condition, compared to suspected embezzlement

in the private information condition. Thus, transparency can increase accountability in two ways: By correcting citizens' tendency to underestimate embezzlement by elected decision makers and by removing their hesitation to punish based on suspicion.

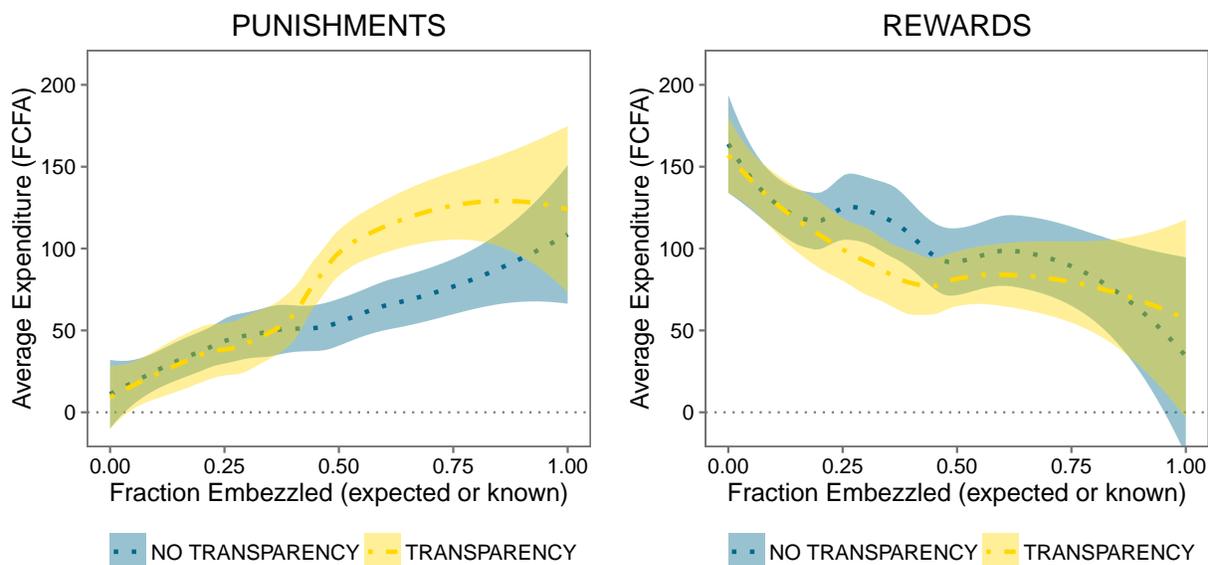


Figure 7. Study participants spent more money on punishing decision makers if the extent of embezzlement was known, rather than suspected (left panel). For rewards, the opposite appears to be the case (right panel). The graphs show LOESS estimates with 95 percent confidence intervals.

Conclusions

This study has presented evidence that democratic elections can reduce the embezzlement of public resources, even if they do not increase citizens' ability to hold decision makers accountable. This is important, because electoral accountability is often constrained by term limits or insufficient political competition, or rendered irrelevant by alternative formal or informal accountability mechanisms. By experimentally comparing democratic elections to randomly selected decision makers, this paper illuminated three additional causal mechanisms through which elections could have an impact on the embezzlement of public resources.

First, electoral selection effects benefit benevolent candidates who are intrinsically motivated to

refrain from embezzlement. The experiment demonstrated that this is the case even in a low-information environment, and in a study population that lacks access to formal education and to prior experience with fair and competitive democratic elections. The result solidifies the emerging evidence that elected local leaders have more pro-social preferences than appointed leaders or the general population (Lierl, 2014). It also highlights the importance of further research on the social preferences of politicians and on the strategies by which corrupt candidates might be able to overcome their electoral disadvantage.

Second, elections lower citizens' tolerance for embezzlement. If a decision maker was elected instead of randomly appointed, study participants incurred greater personal costs to punish embezzlement. Since study participants had no instrumental reason for sanctioning the decision maker, and both selection, it appears that elections either changed the social norms by which citizens evaluate embezzlement, or their willingness to enforce such norms.

Third, elections caused citizens to underestimate how much embezzlement actually takes place. This finding is not only novel, but also consistent with prior evidence that citizens are actually too trusting towards elected local leaders (Gottlieb, 2016) and that informing citizens in advance about their entitlements can substantially reduce embezzlement (Banerjee et al., 2016). It is also highly problematic from a normative point of view, because it implies that elections might enable corrupt decision makers to exploit citizens' trust.

Transparency might therefore play an important enabling role for elections to reduce public corruption. While elections increase citizens' willingness to sanction embezzlement, a lack of transparency can undermine this effect, because citizens are less suspicious towards elected decision makers than towards non-elected decision makers. Information about the true extent of embezzlement counteracts citizens' tendency to trust elected leaders more than they should be trusted.

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Appendix

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Treatment effects on embezzlement

<i>DV: Fraction of Group Resources Embezzled</i>				
	Unweighted		Weighted	
	(1)	(2)	(3)	(4)
<i>OLS coefficients (standard errors in parentheses)</i>				
β_1 : Elections	-0.025 (0.030)	-0.013 (0.028)	-0.029 (0.029)	-0.025 (0.026)
β_2 : Transparency	-0.0086 (0.030)	0.0031 (0.028)	-0.010 (0.029)	0.0011 (0.026)
β_3 : Elections \times Transparency	-0.031 (0.043)	-0.037 (0.039)	-0.047 (0.042)	-0.045 (0.037)
<i>P-Values for pre-specified hypothesis tests</i>				
Hypothesis 1a				
$H_0 : \beta_1 \geq 0$	0.207	0.321	0.160	0.172
$H_0 : \beta_1 + \beta_3 \geq 0$	0.032	0.037	0.005	0.004
Hypothesis 1b				
$H_0 : \beta_2 \geq 0$	0.388	0.544	0.366	0.516
$H_0 : \beta_2 + \beta_3 \geq 0$	0.093	0.114	0.026	0.050
Hypothesis 2				
$H_0 : \beta_3 \leq 0$	0.767	0.825	0.871	0.890
Observations (groups)	471	471	471	471
Municipality fixed effects	yes	yes	yes	yes
Group-level covariates		yes		yes

Table A-1: OLS estimates of average treatment effects on embezzlement. All specifications include municipality fixed effects, but omission of municipality effects does not noticeably change the results. Columns (2) and (4) include group-level covariates: Average baseline embezzlement preference, number of women in the group, ethno-linguistic fractionalization of the group, geographic fractionalization of the group (villages of origin), number of pre-existing social ties in the group (one missing value imputed at mean), highest level of education represented in the group (four missing values imputed at mean).

Treatment effects on trust in the decision maker

<i>DV: Suspected Embezzlement</i>						
	Population-weighted			Unweighted		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>OLS coefficients</i>						
Elections	-0.080*** (0.025)	-0.083*** (0.025)	-0.087*** (0.021)	-0.088*** (0.025)	-0.084*** (0.025)	-0.087*** (0.021)
Transparency	-0.063** (0.027)	-0.066** (0.026)	-0.064*** (0.023)	-0.059** (0.028)	-0.060** (0.027)	-0.064*** (0.023)
Elections×Transparency	0.050 (0.035)	0.059* (0.035)	0.062 (0.029)	0.057 (0.035)	0.059* (0.034)	0.062** (0.029)
Constant	0.36*** (0.035)			0.37*** (0.02)	0.34*** (0.029)	
Observations (individuals)	1884	1776	1776	1884	1776	1776
Individual-level covariates		yes	yes		yes	yes
Municipality effects			yes			yes

Standard errors in parentheses, adjusted for clustering by group.

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

Table A-2. Estimates of average treatment effects on group members' expectations. Individual-level covariates are age, gender, years of education, and baseline embezzlement preference.

Treatment effects on perceived fairness of the selection procedure

<i>DV: Perceived Procedural Fairness</i>				
	Population-weighted		Unweighted	
	(1)	(2)	(3)	(4)
<i>Proportion perceiving random selection as fair</i>				
Control group	0.918	0.918	0.915	0.915
<i>Change in proportion (estimated via Logit regression)</i>				
Elections	0.034*** (0.013)	0.031*** (0.000017)	0.024* (0.013)	0.020 (0.013)
Observations	2263	2127	2263	2127
Nonresponses (dropped)	92	88	92	88
Individual-level covariates		yes		yes

Standard errors in parentheses, adjusted for clustering by group.

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

Table A-3. Estimates of treatment effects on whether group members considered the procedure by which the decision maker was selected fair. Individual-level covariates are age, gender, years of education, baseline embezzlement preference, and transparency treatment.

Electoral selection effects

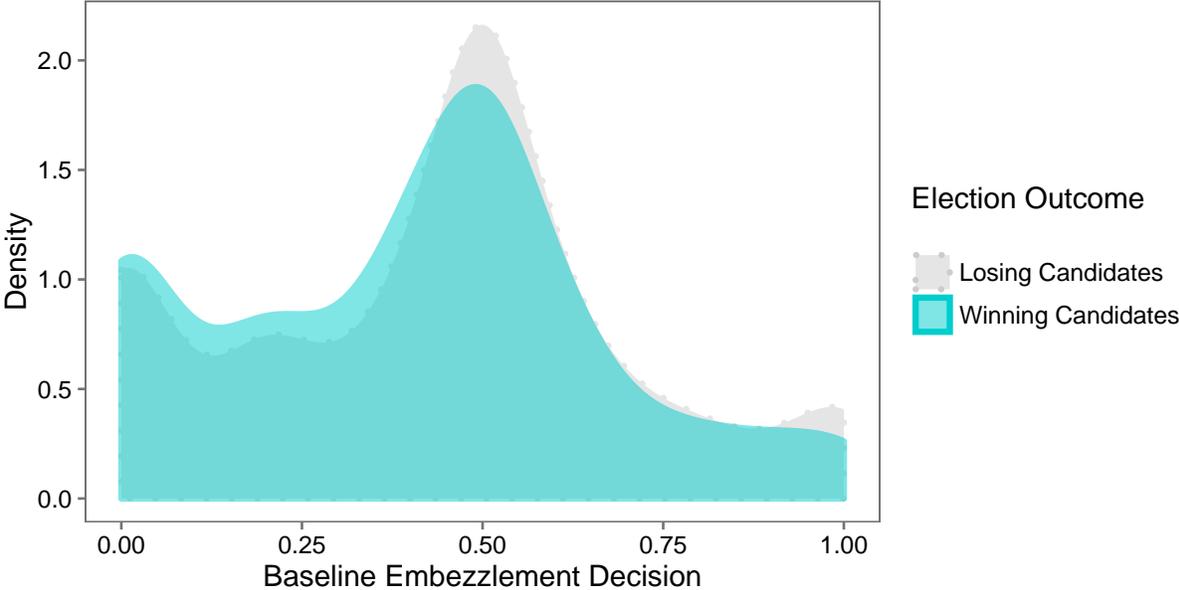


Figure A-1. Baseline embezzlement preferences of winning and non-winning candidates (kernel density estimates, $n_{winners} = 236$, $n_{losers} = 944$).

Decision makers' baseline preferences predict actual and suspected embezzlement

	<i>DV: Actual Embezzlement</i>			<i>Avg. Suspected Embezzlement</i>
	Pooled (1)	Transparency (2)	Private Information (3)	Private Information (4)
<i>OLS coefficients</i>				
Baseline preference	0.56*** (0.046)	0.56*** (0.064)	0.56*** (0.033)	0.26*** (0.064)
Baseline preference × Elected	-0.016 (0.066)	0.0042 (0.089)	-0.041 (0.098)	-0.054 (0.092)
Elected	-0.022 (0.032)	-0.043 (0.044)	0.00050 (0.047)	-0.062 (0.044)
Constant	0.12 (0.023)	0.12 (0.032)	0.13 (0.033)	0.25 (0.031)
Observations (groups)	471	471	471	471
adj. R^2	0.37	0.40	0.34	0.13
F-statistic	93.92	53.81	41.13	13.12

Standard errors in parentheses.

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

Table A-4. Decision makers' baseline embezzlement preferences are strongly correlated both with their later embezzlement choices (Columns 1 to 3) and other group members' expectations (Column 4).

Suspected embezzlement correlates with actual embezzlement

<i>DV: Suspected Embezzlement</i>				
	Population-weighted		Unweighted	
	(1)	(2)	(3)	(4)
<i>OLS coefficients</i>				
Actual embezzlement	0.58*** (0.031)	0.57*** (0.033)	0.51*** (0.037)	0.49*** (0.037)
Baseline embezzlement preference		0.026 (0.018)		0.067*** (0.019)
Intercept	0.11*** (0.011)	0.10*** (0.011)	0.14*** (0.012)	0.11*** (0.013)
Observations	1884	1884	1884	1884
Groups	471	471	471	471
adj. R^2	0.35	0.35	0.27	0.27

Standard errors in parentheses, adjusted for clustering by group.

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

Table A-5. Study participants' expectations correlate with actual embezzlement.

Touch screen ballot

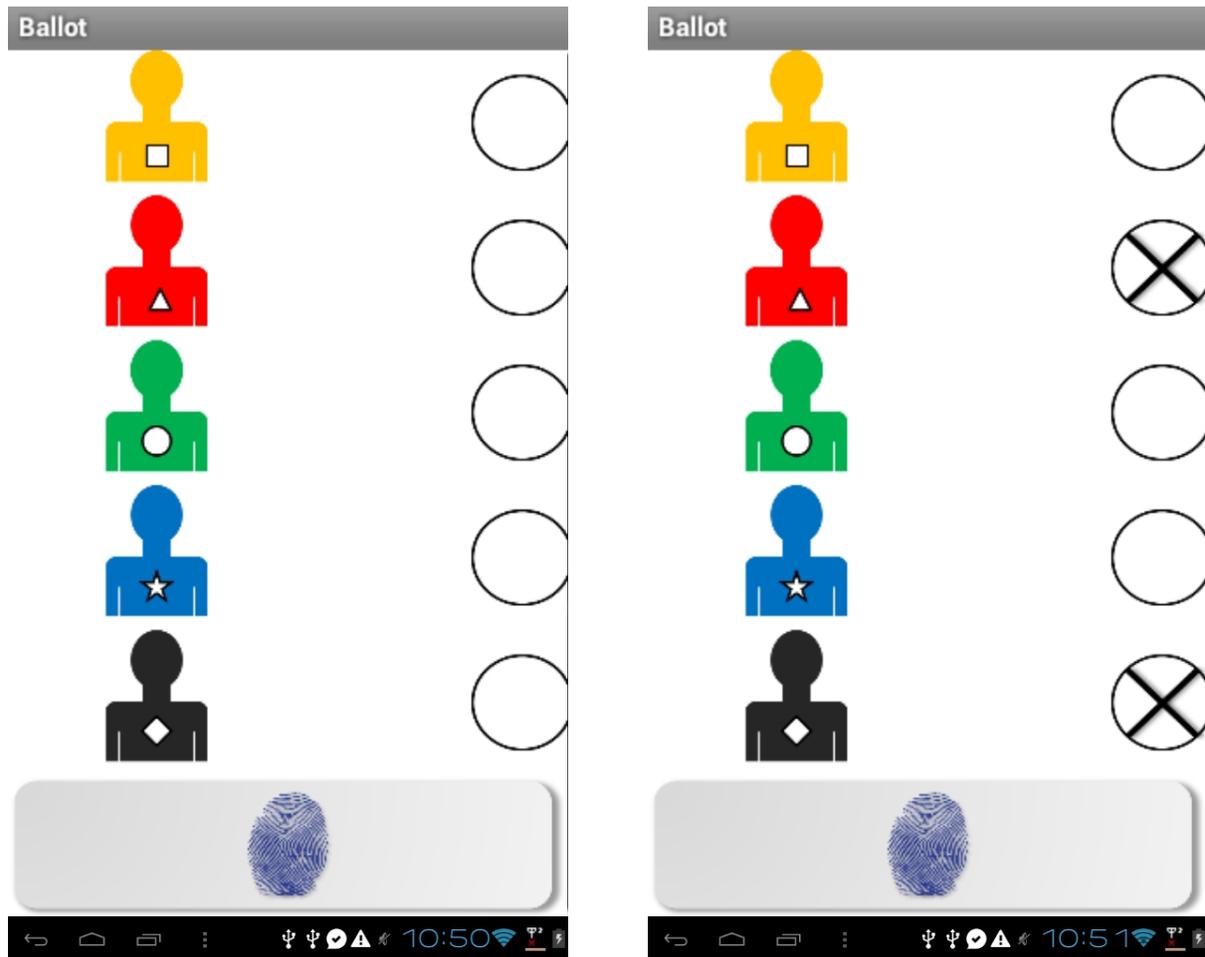


Figure A-2. Touch screen ballots. On the ballot, candidates were identified by their badge colors. The ballot order was randomized at the group level. Two candidates had to be selected in the first stage, one candidate in the runoff stage. Votes were cast secretly in a voting booth and submitted by touching the fingerprint symbol. After submitting, a lock screen was displayed and the vote was counted automatically. The tablet was returned to the facilitator, who would unlock it with a password and activate a blank ballot for the next voter.