Who's Watching? Effects of Monitoring on Strategies for Corruption: A Field Experiment in the Congo

PRELIMINARY. NOT FOR CITATION OR CIRCULATION.

Raul Sanchez de la Sierra*
Peter van der Windt†
Macartan Humphreys‡

January 27, 2015

Abstract

Existing research has found mixed evidence on the effects of different types of monitoring aimed at rendering local elites accountable. Is community-based monitoring more effective than external monitoring by aid donors? Does one undermine or complement the other? We study the effects of pre-announcing that audit information will be shared with communities, or with donors, in the context of a village-level, unconditional cash transfer of $1,000 in 457 villages in Eastern Congo. We find that announcing that populations or donors will be informed shifts beliefs by local elites in the expected direction, but only when communicated separately. Threats of oversight — from the population or from donors — have no discernible effects on the levels of fund misuse. However, we find suggestive evidence that these announcements affect the *type* of strategies employed by committees for corruption, substituting activities towards less visible types of fraud.

*Harvard University. Corresponding author: rsanchezdelasierra@fas.harvard.edu. This research was undertaken in the context of a larger study of development interventions in the DRC; we thank the IRC and CARE International for their partnership in that research, and 3ie for financial support. The analysis for that research was pre-registered. We did not register this audit study. The full list of people who played critical roles in making this work possible is long and we refer readers to the acknowledgments in our study "Social and Economic Effects of *Tuungane*".

†NYU Abu Dhabi
‡Columbia University

1 Introduction

Weak institutions and elite capture can be a major barrier to public goods provision and economic growth in the developing world (Acemoglu and Robinson, 2006; Krueger, 1974; Olken, 2007; Shleifer and Vishny, 1993). A large number of interventions has attempted to "fix" institutions and improve the accountability of local elites by providing exposure to democratic practices. However, these interventions have largely proven unsuccessful at improving elite performance (Casey et al., 2013; Fearon et al., 2009; Humphreys et al., 2014).

The standard theoretical framework of elite behavior suggests a number of hypotheses on how best to discipline local elites. One approach, drawing on Becker and Stigler (1974), emphasizes the right combination of monitoring and punishment may discipline civil servants. One well-known problem, however, are the incentives of those policing the civil servant: achieving the efficient outcome depends on their objective function, ability to observe the civil servants' multi-dimensional actions, and ability to collude with the civil servant. These issues are particularly salient in the context of the aid industry: donors may have their own agenda, uninformed beliefs about local preferences, and may be unable to observe local elites perfectly. A second major problem is that in addition to the donors, populations may also hold their leaders accountable. The presence of multiple principals can give rise to coordination problems (Hammond and Knott, 1996; Gailmard, 2009; Dixit et al., 1997; Stiglitz, 1985) worsening the performance of the agent. The impact of monitoring of local elite by multiple principals, thus, remains an empirical question.

In this paper we examine the impact of different types of publicity of future audits on elite behavior. To do so this study makes use of an unconditional cash transfer project named

RAPID ("Recherche-Action sur les Projets d'Impact pour le Développement") implemented in a sample of 457 villages of the Democratic Republic of the Congo. These RAPID communities received block grants of $1,000 which they could spend freely in the span of two months for each village, with no oversight on management and minimal guidance on spending categories. A major benefit of this approach is that by exploring the behavior of RAPID's management committees we observe elite behavior in a real context, such as Olken (2007). To learn about the impact of monitoring, and whether community-based monitoring is more effective than external monitoring, or vice-versa, we exploit a design with encouragement to take-up treatment where we randomly select communities for different types of monitoring. Specifically, we implemented RAPID following a factorial design with two treatment: one in which committee members, before the distribution of funds, were told that audit results would be shared internally with the village population, and one in which committee members were told that audit results would be shared externally with international donors.

We find evidence that our announcements are credible. Announcing that a future audit will be shared with (or protected from) other principals has an influence on the committee's self-reported beliefs about who will have access to the results from the audit. In those communities where we announced that audit results will be shared with the population (donor), committees are 13% (11%) more likely to believe that that the population (donors) will be informed. These results are in the context of already high baseline beliefs: 60% (82%) of committees believe that results will be shared with population (donors) in the absence of an announcement. Furthermore, our results suggest possible information overcrowding: announcing that the audit report will be shared with one group (population or donors) *completely* crowds out the effect of announcing that the audit report will also be shared with another group (donors or population).

This study only finds weak evidence that the distribution of information about the type of audit has an effect on elite behavior. We find that announcing that the audit report will be shared with donors and/or the population does not affect our measures of elite performance and community satisfaction with elite behavior. However, we find suggestive evidence that these announcements do affect the type of strategies employed by committees for corruption: with committees reallocating fraud from more observable margins (fake purchases) towards harder to observe, margins of evasion (exchange rate manipulations). The reallocation of effort is not however associated with efficiency losses, as is theoretically possible.

This paper's findings have implications for both theory and policy. It is widely believed that institutions are a key ingredient to development (Acemoglu et al., 2001; Acemoglu and Robinson, 2006). However, the few attempts to "fix" institutions have proven unsatisfactory (Casey et al., 2013; Fearon et al., 2009; Humphreys et al., 2014). A major lesson from this type of interventions is that they are grounded on weak theoretical foundations: why would exposure to different governance practices for a limited period lead to a change in future behaviors without altering fundamentals? We improve upon this literature by framing committee behavior from the lens of the common agency theory in order to motivate our intervention. If committees behave in anticipation of sanctions from their principals (the population, or the donors), then improving the quality of the signal about the performance of the agent could empower the principals and discipline the agent, but it may also have deleterious effects (Gailmard, 2009). Understanding net effects has implications for the informational design of public projects as well as aid interventions.

This paper also joins a growing literature on tax evasion by firms and individuals. We examine avoidance behavior by local elites, not firms, in a context with multidimensional actions

and find evidence that civil servants substitute fraud away from arguably observable categories to least observable ones. This echoes emerging results in the tax evasion literature. Based on Allingham and Sandmo (1972), this literature suggests that tax evasion will be decreasing in monitoring and punishment. However, when multiple margins of evasion are available, improving monitoring on one margin could lead to evasion substitution to another, potentially costlier margin (Carrillo et al., 2014; Piketty et al., 2014).

Our experiment is closely related to a series of experimental studies that assess the effects of monitoring on performance. Olken (2007), who randomly assigns village road construction projects to different types of audits in Indonesia, finds that monitoring by the state is more effective than additional community-based monitoring. Our results differ from his in two ways. First, instead of focusing on monitoring by the state, we focus on international donors. This is important, because we introduce a new principal, potentially relevant in a large number of developing countries. It is widely believed that actions and expectations of donors potentially undermine local accountability due to strategic substitution (see for instance, the dependency theories). However, their monitoring may affect elite performance directly, either by improving effort or by distorting effort towards observable, less socially desirable activities. Second, in Olken (2007) it is difficult to separate the effectiveness of different agencies to implement audits or the differential abilities of agencies to impose sanctions conditional on the audit. By focusing on changing the *audience* of the audit, but holding constant the agency that implements it, we are able to identify uniquely the effect of future sanctions and ignore the effect of auditing technology. Thus our inquiry links the study of audits to the broader study of transparency and accountability.

The remainder of the paper is organized as follows. Section 2 presents the background.

Section 3 presents the empirical strategy, and Section 4 present the results. We conclude in Section 5.

2 Context

Our study draws on variation in treatment introduced as part of the RAPID project, implemented in Eastern Congo in 2011-12. The RAPID project — a village level, unconditional cash transfer program — was implemented as part of a larger strategy to assess the effects of a development intervention taking place in 560 villages drawn randomly from Congo's South Kivu, Maniema, Tanganyika and Haut Katanga provinces.

The area of project implementation figured centrally in the violence that has engulfed the country over the last two decades. Located in the east, it was home to the start of the First and Second Congolese Wars (1996-1997 and 1998-2003). The latter, with the direct involvement of eight African nations and 25 armed groups, has been the deadliest war in modern African history.[1] Despite the formal end to the war in July 2003, the study area continues to be an epicenter of conflict.[2]

Following decades of war, years of exposure to "decentralized despotism" (Mamdani, 1996) by the Belgian colonial authorities and the subsequent kleptocracy of the Mobutu regime, the Congolese state is unable to provide even basic services — including security — in rural areas.[3] In consequence, governance and public goods provision in the Congo can be best characterized as a local affair, where public goods are organized at village level, often under the supervision

[1] See IRC (2007).
[2] For a discussion of the Congolese conflict see Autesserre (2010) and Prunier (2009).
[3] E.g. (Autesserre, 2010; Sanchez de la Sierra, 2014; Schatzberg, 1997).

of traditional authority such as village chiefs and chiefdom kings. In addition, international donors also play a large role in the provision of public goods. However as they do so, they often seek to avoid working with the state and with traditional authorities (Humphreys et al., 2014).

These assertions are confirmed by our data. We asked 2,800 randomly selected individuals in our 560 villages about who initiated the development projects that took place in the village over the last twelve months. Of those that replied, less than 3% mentions that the government initiated the project. In contrast, respondents largely claim that those responsible for public goods provision are NGOs (55%) or other actors internal to the village such as villagers (16%), the development committee (8%), or local traditional authorities (7%). The claim that development actors frequently adopt the view that local traditional authorities are despotic and hinder development, is confirmed in some academic accounts of local authority (Acemoglu et al., 2014; Murphy, 1990; Mamdani, 1996).[4] It is in this setting that we introduce an unconditional cash transfer to be used for a village-level development project.

3 Research Strategy

3.1 The RAPID project and variation in treatment

Between October 2010 and February 2012, in collaboration with two local universities (Université de Lubumbashi and Université Officielle de Bukavu) and funded by DFID, we implemented RAPID to observe elite and community behavior in the context of a public goods project.

As part of the RAPID process 457 village (of an initial target pool of 560 villages) were

[4]Yet, at the same time there is recent empirical evidence pointing out that chiefs command the respect of rural people (Logan (2013)) and that they might be particularly good project managers (Turley et al. (2014)).

selected to participate in an unconditional cash transfer program in which they would receive grants of $1,000 to be used on projects that benefit the village.[5] Communities were asked to identify and implement projects subject to minimal constraints. The key constraints were that some uses were ruled out if these were likely to result in harm (such as the purchase of weapons) and the grant had to be spent out within a two month period. There was a moderate encouragement towards distributive projects but these were not required. There was no guidance of any form given as to who should manage the funds and how decisions should be made.

RAPID entailed four village visits. In step A, the whole community was informed of the development project and of the following three steps, and informed that they will have to identify at most eight representatives, with whom the project will interact in later steps. A week later, during Step B, our teams returned to meet the representatives: the 'RAPID committee'. In step C, the committee received the funds privately and then the community had forty days to implement the project. In Step D, at the end of two months, an extensive audit and household survey was conducted. See Table 5 in the Appendix for an overview of the stages of the RAPID intervention.

To learn about the impact of monitoring, and whether these interventions result in elites that are more accountable to their populations or instead to external principals, we implemented RAPID following a factorial design with two treatments: T^I, in which audit results would be shared *internally* with the village population, and T^E in which audit results would be shared *externally* with international donors. The 457 villages were randomly assigned into one of four information treatment conditions (see Table 1). Specifically, during Step B the project teams

[5]Due to security and logistic challenges, the full set of villages targeted for the RAPID intervention was not reached, however, by design village level missingness is orthogonal to the treatments.

communicated privately to the committee members in all villages that there is the possibility that an audit will take place. Those communities assigned to T^I were, in addition, informed that a public meeting would be organized during Step D in which the village population would be informed about the use of funds and management by the committee and irregularities that were encountered. Those communities assigned to T^E were told that the audit, with the results of audits of other villages, will be shared with international development organizations active in the region. In contrast, those communities not selected into the donor-sharing treatment were specifically guaranteed that donors will *not* have access to the results of the audit (a strategy that was agreed to in advance with the donors). Randomization was implemented privately, with "quasi-blocking" on location as well as on the Tuungane intervention.[6] All villages were assigned to each condition with equal probability ($p = 0.25$).

Table 1: Number of Villages by Treatment

		T^E: **Donors informed** ("External")	
		Yes	No
T^I: **Population informed** ("Internal")	Yes	n=113	n=114
	No	n=119	n=111

3.2 Outcomes and Measurement

We are interested in both the beliefs of the committee that the population/donor will receive information about the audit results, and subsequent changes in their behavior. To measure

[6] That is, since not all randomization blocks were of equal size we ordered all blocks by location, randomized the order of villages within blocks, and assigned treatment type vector $1, 2, 3, 4, 1, 2, ...$ to the randomly ordered set. This procedure ensures that all blocks with of size n contain at least k units (and no more than $k + 1$) in each treatment condition, where k is the quotient of n divided by 4.

beliefs among the committee, we conduct a group interview with the RAPID committee during Step D where we ask who will learn about the audit results.[7]

To test whether the interventions had an impact on behavior, we explore three primary types of behavior: 1) elite performance, 2) elite substitution between different margins of fraud, and 3) elite actions intended to please donors which are potentially inefficient.

Reliably measuring such outcomes using simple surveys is often difficult for the simple reason that few respondents would feel comfortable discussing issues related to fraud. To address this concern, we draw on an extensive behavioral data collected by our auditing teams that visited each of the villages during Step D at the conclusion of the RAPID project. The audit strategy was designed over a two-month period together with our almost 100 surveyors to discover all possible strategies that committees could use as fraud, and the strategies we could employ to discover these.[8]

A key set of audit activities were conducted in all RAPID villages. To prevent collusion between committee members, one auditor conducted private interviews with two randomly selected committee members while at the same time the remainder of the committee was simultaneously interviewed by another auditor (from this latter interview we derive our measure of committee beliefs). In those villages where infrastructure was created, the auditors would find evidence of the project and assess quality. In those villages where RAPID was used for distributional projects, the auditor would obtain from the committee a list of beneficiaries and randomly select ten beneficiaries. The auditor would then verify the existence of these beneficiaries and implement a short interview in private to estimate the value distributed to this

[7] "Qui va apprendre les résultats de l'audit du projet RAPID?"

[8] Five checklists were made, one for a different type of fraud: 1) misreporting of purchases, 2) distribution to beneficiaries, 3) financial irregularities, 4) irregularities by the committee, and 5) the audit. Table 6 in the appendix gives an overview of the first. See pages 257-259 in Humphreys et al. (2011).

beneficiary. To detect fraud due to misreporting of purchase prices, quantities and exchange rates used, the auditors also visited the local market to collect data on current and retrospective prices for the objects that the committee reported to have purchased, as well as evidence of actual purchases. We group the data into three dimensions of elite behavior:

1. Elite performance: We measure the amount (of the $1,000) that the auditors were unable to trace; the amount (of the $1,000) that the auditors found was retained by elites; complaints about the project by respondents (given in private during a household survey in Step D).

2. Fraud substitution: We investigate fraud due to over-reporting of the dollar-to-francs exchange rate (while the project envelope is in dollars, the local expenses are in dollars and Congolese francs); fraud due to over-reporting of value of purchases for distribution (the committee may claim not existent expenses, either by over-reporting purchase prices or quantities purchased); fraud due to over-reporting of the quantity distributed (conditional on a given quantity purchased, the committee may decide to keep a disproportionate amount for themselves, but claim it was distributed); and other sources of fraud. We expect that committees anticipate that fraud due to manipulation of purchases and distribution are easier to audit, since this information can be directly obtained in the market and the households. However, manipulation of the exchange rate may be harder to audit due to recall error since exchange rates vary over time and space.

3. Process modifications: We expect the elite to anticipate that donors, and citizens, have a preference for democratic decision processes and for basic accounting quality. Committees might substitute towards activities that are costly and may not be socially desirable. We measure the number of meetings that took place in the village during the project (as reported by households, the chief, and the committee) and the quality of accounting as judged by our

auditors (presence of accounting form, expenses justified with credible receipts).

3.3 Estimation

Since the treatment is randomized at the village level, we can use simple ordinary least-square regressions to produce unbiased estimates of the impact of the treatment. Given the factorial design we use a model with dummy variables for each treatment and an interaction term.[9] We take account of randomization blocks — to reduce variance and not to control for imbalance — by employing quasi-block fixed effects.[10]

We note that because the treatments are encouragements to shift the beliefs of the committee members about properties of a future audit, "compliance" is likely to be incomplete. Beliefs may not respond to the treatment for a variety of reasons: the committee may not understand the messages given during Step B; they may not believe or may forget the information, and so on. The results should therefore be read as the effect of providing information about different audit dissemination strategies to development committees, some of whom may absorb the message while others do not. In additional material (Appendix D), we also provide estimates of Local Average Treatment Effects (LATE), which seek to assess the effects of belief shifts for only those committees whose beliefs comply with our information treatments. These results are not included in our main analysis however because the multiple channels through which the treatment could operate make it difficult to defend the exclusion restriction needed to assess

[9]Specifically, we use the following regression specification to estimate the intention to treat (ITT) effect: $Y_i = \beta_j + \beta_1 XT_i^I + \beta_2 XT_i^E + \beta_3 ET^I T_i^E X + \varepsilon_i$, where $i = 1, ..., N$ indicates the village, or in some cases the respondent, j the block, and Y_i indicates the behavioral analyzed. $T_i^I \in \{0, 1\}$ indicates whether village i was assigned to the "internal" treatment, $T_i^E \in \{0, 1\}$ indicates whether village i was assigned to the "external" treatment, and $T_i^I T_i^E$ their interaction.

[10]For integer reasons the randomization procedure used a partial blocking strategy as described in footnote 6 above. The fixed effects employed uses the randomization blocks for all blocks with four or more units and aggregated together all blocks in the same geographic area that had fewer than four units.

the effects due to changes in the beliefs of the surveyed elites alone.

4 Results

4.1 Shifting beliefs

Who do RAPID committee members think will learn about the audit results? And how are these beliefs affected by our treatment? The basic relations are given in Table 2. A number of features stand out. Even in the absence of any treatment, expectations are high that the audit information will be shared. In the $T^E = T^I = 0$ condition, a full 79% of RAPID committees believe that the donors will be informed, and more than half (55%) expect that the results will be shared with the village population. The high numbers believing that donors will receive the information is particularly striking since in this condition committees were specifically told that they would not be. These messages were clearly not seen as credible for many. The fact that members of the RAPID committee are more likely to expect information to be shared with donors than with the population is also striking and is consistent with views that local elites expect greater scrutiny from international actors than domestic constituencies — or at least that development aid is structured to support village-to-donor accountability primarily.

Moving to the effect of the treatment, we see that, compared to the control condition, announcing that the audit results will be shared with the donors (population) at the end of the project increases the belief the donors (population) learn about the audit. Table 3 estimates the magnitude of these effects directly, taking account of the quasi-block fixed effects. We find that estimated effects are 11 percentage points for the external treatment on the belief that

Table 2: Committee Beliefs Under Different Treatment Conditions

	$T^E = 0$ No External	$T^E = 1$ External	
$T^I = 0$	79%	**88%**	Beliefs about external audit
No Internal	55%	54%	Beliefs about internal audit
$T^I = 1$	79%	**71%**	Beliefs about external audit
Internal	**71%**	**57%**	Beliefs about internal audit

Notes: Moving from the top-left cell to the bottom-left cell clock-wise we have for "Belief about external (internal) audit" a total of 85, 87, 84, 85 (81, 83, 91, 87) observations.

donors will be informed, and 13 percentage points for the internal treatment on the belief that the population will be informed. This result is significant at the 10% and 5% levels respectively in a two tailed test. As expected, the effects of the internal (external) treatment on beliefs that the donors (population) will be informed about audit results are close to zero. Given the high baseline expectations compliance rates are not high. The effects among those that do not expect an audit is large however: an estimated 21% do not expect donors to be informed in the control condition, and this drops by almost half to 12% when the external treatment is delivered (see Table 2). Those committees not expecting the population to receive the information drops by a third when the internal treatment is delivered (from 45% to 29%).

Table 3: The effects of treatments on reported beliefs

Outcome		T^I	T^E	$T^I T^E$
Committee beliefs				
(1) C believes donors will be informed	estimate	$-.03$	.11	$-.19$
	(s.e.)	(.06)	$(.06)^*$	$(.09)^{**}$
(2) C believes population will be informed	estimate	.13	$-.01$	$-.09$
	(s.e.)	$(.05)^{**}$	(.06)	(.08)

Notes: *** p<0.01, ** p<0.05, * p<0.1. This table presents the results of a linear probability model regressing beliefs on the monitoring treatments. Regressions include randomization block fixed effects.

Strikingly however we can also see from Table 2 and Table 3 that, compared to those villages that received only one treatment, those villages that received both the donor and the population treatment have a *lower* expectation that the audit results will be shared. For beliefs that donors will be informed the interactive effect is statistically significant ($p < 0.01$), and *more than offsets* the main effects. One explanation for this can be a crowding-out effect as a result of information overload: committee members had difficulties understanding and retaining information about both treatments. This proposition can be particular relevant in areas where education levels are low. Another possibility is that multiple treatments were not deemed credible, that is that sometimes one threat is more compelling than two. A key substantive implication of these results on beliefs is that effects on behavior are likely to arise only for villages receiving one or other, but not both, of the treatments.

4.2 Shifting behavior

The treatments led to changes in beliefs. Did they also lead to a change in elite behavior? Table 4 shows the main results.

From the first panel of Table 4 we see that there is no evidence that either treatment affects our measures of elite performance (lines 1-3). While we see a marginal (and non significant) reduction in complaints by citizens, our measures record a small rise in the amounts captured by elites as well as a rise in the amounts that cannot be traced by our auditors (and missing via exchange rate fraud, missing purchases, and other types of disappearances).

We also do not find strong results related to process (lines 7-9). We see some evidence that elites hold more meetings especially when they expect internal monitoring, which is consistent

Table 4: Behavioral Results

Outcome		**CONS**	T^I	T^E	$T^I T^E$
Elite performance					
(1) Amount missing	estimate	135.52	11.69	18.63	−7.77
	(s.e.)	(17.87)***	(26.77)	(28.37)	(43.77)
(2) Amount captured by elite	estimate	17.46	12.64	−3.22	−6.49
	(s.e.)	(42.21)***	(68.89)	(62.58)	(90.75)
(3) Index of private complaints	estimate	.19	−.03	−.02	.03
	(s.e.)	(.01)***	(.01)	(.01)	(.02)
Fraud substitution					
(4) Est. exchange rate manipulation	estimate	.35	19.29	19.93	−33.57
	(s.e.)	(3.76)	(10.17)*	(9.84)**	(15.33)**
(5) Amount missing from other strategies	estimate	135.29	-11.74	-2.15	29.82
(quantity manipulation, other)	(s.e.)	(17.78)**	(26.52)	(28.00)	(42.48)
(6) *Share* of amount missing that is due	estimate	0.01	0.07	0.06	-0.06
to exchange rate manipulation	(s.e.)	(0.01)	(0.03)**	(0.03)**	(0.04)
Process modifications					
(7) Number of meetings	estimate	.78	.11	.05	−.1
	(s.e.)	(.03)***	(.05)**	(.05)	(.08)
(8) Presence of accounting form	estimate	82.2	1.71	6.71	−13.14
	(s.e.)	(3.7)***	(5.24)	(5.06)	(7.6)*
(9) Share of expenses credibly justified	estimate	46.28	1.94	3.7	−9.38
	(s.e.)	(3.68)***	(5.18)	(4.91)	(7.58)

Notes: *** p<0.01, ** p<0.05, * p<0.1. This table presents the ITT results using a linear probability model. Lines (1)-(3) present the results on elite performance, (4)-(6) present the results on elite substitution between types of fraud, lines (7)-(9) present the results on actions of the elite aimed at satisfying donors. Standard errors of household level observations are clustered at the village level. All regressions include randomization block fixed effects.

with a sharing of responsibility for decisions made. But we see surprisingly weak effects on the propensity to undertake basic accounting tasks that would satisfy donors, such as ensuring the presence of account forms (the *absence* of accounting forms drops by a third under the T^E only treatment) or the effort to justify receipts credibly. While the evidence of an increase in meetings may be seen as a positive effect of accountability by some, we emphasize that there is no evidence here that it is accompanied by gains in actual performance.

From the second panel (lines 4-6) however we see suggestive evidence that although we do not see reductions in the amount of diversion, there are changes in the form of diversion.

We posited that committees willing to undertake fraud but anticipate that the audit results would be shared, are more likely to engage in fraudulent activities that are less observable. It is relatively easy to spot incorrect inflation of local purchase prices, input quantities, or the number of beneficiaries. In contrast, it is substantially more difficult to find fraud by exchange rate manipulation: while the project funds were delivered in dollars, the purchases are made in dollars and Congolese francs. Committees can inflate the price of francs per dollar in the belief that our enumerator teams have difficulties to derive historical local exchange rates or expect that auditors would not investigate this 'less obvious' form of fraud. Consistent with this proposition, we find that when the committee anticipates that the results of the audit will be shared with donors, the share of missingness that is attributable to exchange rate manipulations increases from close to 0 to 6 or 7 percent, depending on the treatment. As with the beliefs we see evidence of a negative interactive effect, although in this case the magnitude is not so large — here the interaction term implies complete substitution, but no more than that.

It is important to highlight that not only the sign but also the magnitudes are similar for both interventions. This suggests that the fact of monitoring is more important than who is doing it.

5 Conclusion

Weak institutions has been identified as an important cause for underdevelopment. However, to date we know little about how to improve the functioning of existing institutions in the developing world. One strategy that has been particularly popular in recent years is to improve the accountability of local elites (Casey et al., 2013; Fearon et al., 2009; Humphreys et al.,

2014). However, a number of important questions remain unanswered. To whom should local elites be accountable? Is community-based monitoring more effective than external monitoring by for example donor agencies? Does one substitute for or complement the other? To answer these questions, we study the impact on elite behavior of different types of publicity of future audits in the context of an unconditional cash transfer of $1,000 in 457 villages in Eastern Congo.

We find that announcing that populations or donors will be informed shifts beliefs in the expected direction, but only when communicated separately. Puzzlingly, announcing that both actors will receive information does not shift beliefs about either. Threats of oversight — from the population or from donors — have no discernible effects on the overall levels of fund misuse. However, we find suggestive evidence that these announcements affect the type of strategies employed by committees for corruption: substituting activities towards less visible types of fraud. Community-based and donor monitoring have similar effects, suggesting that the fact of monitoring may be more important than who is doing the monitoring.

The idea that providing information about financial improprieties should reduce the incidence of those improprieties is simple and obvious. Yet theoretical puts this simple logic into question and empirical work, including ours, finds surprisingly mixed support for it. The findings in this study do not suggest that the performance of institutions cannot be improved by the design of information flow and monitoring. They do however raise two notes of caution. First, they highlight how interventions that attempt to change the design of information might be constrained by the subjects' capacity to absorb information, the credibility of that information, and the pre-existing prior beliefs about information flows. The surprising negative interaction terms we find on the effects of treatment on beliefs are especially instructive. Second, the results

demonstrate empirically how even credible threats about future monitoring may be ineffective when subjects have access to multiple margins of fraud. As monitoring improves, elites may tend to reallocate corruption towards less observable fraud. While such behavior may in theory produce overall adverse effects, we do not find evidence for such adverse effects here. The overall effects of such interventions, and whether the net effects are likely to be positive or negative, thus depends crucially on the available dimensions of fraud.

References

Acemoglu, Daron and James Robinson, *Economic Origins of Dictatorship and Democracy*, London: Cambridge, UK: Cambridge University Press, 2006.

_ , Simon Johnson, and James Robinson, "The Colonial Origins of Comparative Development: An Empirical Investigation," *American Economic Review*, 2001, *91* (5), 1369–1401.

_ , Tristan Reed, and James A Robinson, "Chiefs: Elite Control of Civil Society and Economic Development in Sierra Leone," *Journal of Political Economy*, 2014, (Forthcoming).

Allingham, Michael G. and Agnar Sandmo, "Income tax evasion: a theoretical analysis," *Journal of Public Economics*, November 1972, *1* (3-4), 323–338.

Autesserre, Séverine, *The Trouble with the Congo.*, New York City: Cambridge University Press, 2010.

Becker, Gary S. and George J. Stigler, "Law Enforcement, Malfeasance, and Compensation of Enforcers," *The Journal of Legal Studies*, January 1974, *3* (1), 1–18.

Carrillo, Santiago, Dina Pomeranz, and Monica Singhal, "Tax me if you can: Evidence on Firm Misreporting Behavior and Evasion Substitution," 2014.

Casey, Katherine, Rachel Glennerster, and Edward Miguel, "Reshaping Institutions: Evidence on Aid Impacts using a Preanalysis Plan," *Quarterly Journal of Economics*, 2013, *127* (4), 1755–1812.

Dixit, Avinash, Gene M Grossman, and Elhanan Helpman, "Common Agency and Coordination: General Theory and Application to Government Policy Making," *Journal of Political Economy*, August 1997, *105* (4), 752–69.

Fearon, James D., Macartan Humphreys, and Jeremy Weinstein, "Can Development Aid Contribute to Social Cohesion after Civil War?," *American Economic Review: Papers & Proceedings*, 2009, *99* (2), 287–291.

Gailmard, Sean, "Multiple Principals and Oversight of Bureaucratic Policy-Making," *Journal of Theoretical Politics*, April 2009, *21* (2), 161–186.

Hammond, Thomas H. and Jack H. Knott, "Who Controls the Bureaucracy? Presidential Power, Congressional Dominance, Legal Constraints, and Bureaucratic Autonomy in a Model of Multi-Institutional Policy-Making," *Journal of Law, Economics, and Organization*, 1996, *12(1)*, 119–166.

Humphreys, Macartan, Raul Sanchez de la Sierra, and Peter Van der Windt, *Tungane I: Outcomes and Data Sources* 2011.

_ , _ , and Peter Van der Windt, "Social Engineering in the Tropics: Evidence from 1000 villages in Eastern Congo," 2014.

IRC, *Mortality in the Democratic Republic of Congo*, IRC, 2007.

Krueger, Anne O, "The Political Economy of the Rent-Seeking Society," *American Economic Review*, June 1974, *64* (3), 291–303.

Logan, Carolyn, "The Roots of Resilience: Exploring Popular Support for African Traditional Authorities," *African Affairs*, June 2013, *112* (448), 353–376.

Mamdani, M., *Citizen and Subject: Contemporary Africa and the Legacy of Late Colonialism*, Princeton: Princeton University Press, 1996.

Murphy, William P., "Creating the Appearance of Consensus in Mende Political Discourse," *American Anthropologist*, 1990, *92* (1), 24–41.

Olken, Benjamin A., "Monitoring Corruption: Evidence from a Field Experiment in Indonesia," *Journal of Political Economy*, 2007, *115*, 200–249.

Piketty, Thomas, Emmanuel Saez, and Stefanie Stantcheva, "Optimal Taxation of Top Labor Incomes: A Tale of Three Elasticities," *American Economic Journal: Economic Policy*, February 2014, *6* (1), 230–71.

Prunier, Gerard, *Africa's World War*, Oxford: Oxford University Press, 2009.

Sanchez de la Sierra, Raul, "Defining the state: when do criminal monopolies of violence behave as states?," *Unpublished*, 2014.

Schatzberg, Michael G., "Beyond Mobutu: Kabila and the Congo," *Journal of Democracy*, 1997, *8* (4), 70–84.

Shleifer, Andrei and Robert W Vishny, "Corruption," *The Quarterly Journal of Economics*, August 1993, *108* (3), 599–617.

Stiglitz, Joseph E, "Credit Markets and the Control of Capital," *Journal of Money, Credit and Banking*, May 1985, *17* (2), 133–52.

Turley, Ty, Maarten Voors, Erwin Bulte, Andreas Kontoleon, and John A. List, "Chief for a Day! Chiefs, Participatory Development and Elite Capture in Sierra Leone," *Working paper*, 2014.

Appendices

A RAPID Sequence of Steps

Table 5: The RAPID Project

Stage	Description	Features
A	Team A schedules village meeting and conducts surveys	Initial meeting with the village chief to ask him/her to convene a public meeting at which a minimum share of the village population is required to attend. Survey is conducted among 5 randomly selected households.
	Village meeting and additional surveys	The RAPID project is described in a public village meeting. Measures of the quality of participation are taken. The village is asked to take steps towards determining how to use the project funding and identify representatives (with no guidance). The population is informed that at least $900 will be made available. Surveys are conducted with selected groups of those present during the meeting.
B	Collection of forms	Meeting with committee members only. Measures are taken of the village's decisions regarding how to use funding and who is entrusted to manage it. The committee members are informed *in private* that the amount provided to villages will be $1,000 ($100 more than announced to the village), and of the type of audit that will be undertaken.
C	Disbursement of funds by IRC and CARE	$1,000 are disbursed in private to a select group of members identified by the management committee.
D	Auditing	Auditing is undertaken to track the use of all funds, and measure capture, efficiency, transparency, and the accountability mechanisms that were established.
	Follow-up surveys	Surveys are conducted among 10 randomly selected households (5 are those surveyed during Step A). Measures are included to determine the transparency of the RAPID process, the quality of participation in village decision-making, and the efficiency and equity of outcomes.

Notes: Key features of the $1,000 unconditional cash transfer program.

B Treatment Scripts

Complete scripts to be read to the RAPID committee in Step B were provided for each of the four combinations of treatment. These scripts are factorial in nature, all containing a common base script, and either including one or other script relating to external monitoring, and either including or not including a script relating to community monitoring. Translations of each of the complete scripts are provided below.

B.1 Script for condition $T^I = 0, T^E = 0$

As we said before, we will send teams to implement audits in some villages that are taking part in the program. It is important that you know what these audits are.

- If this village is audited, we will examine the use that this village has made of project funds.
- While the results of the audit will be used for our research on the situation in East Congo, we can guarantee you that the name of this village will not be shared. The results of the audit will be anonymized. Moreover, no development agency, including the funders of this project will be told how this village in particular has used the funds or about the results of the audit. In addition we can guarantee you that how you manage the funds will not have any effect on decisions regarding whether this particular village will be selected for future development projects.

B.2 Script for condition $T^I = 1, T^E = 0$

As we said before, we will send teams to implement audits in some villages that are taking part in the program. It is important that you know what these audits are.

- If this village is audited, we will examine the use that this village has made of project funds.
- When we have verified the information, we will hold a meeting with the population to present the results of the audit and tell the whole population how these funds were used and how the funds were managed by the representatives, as well as of any irregularities that we uncover.
- While the results of the audit will be used for our research on the situation in East Congo, we can guarantee you that the name of this village will not be shared. The results of the audit will be anonymized. Moreover, no development agency, including the funders of this project will be told how this village in particular has used the funds or about the results of the audit. In addition we can guarantee you that how you manage the funds will not have any effect on decisions regarding whether this particular village will be selected for future development projects.

B.3 Script for condition $T^I = 0, T^E = 1$

As we said before, we will send teams to implement audits in some villages that are taking part in the program. It is important that you know what these audits are.

- If this village is audited, we will examine the use that this village has made of project

funds.

- The results of the audit will be studied by an international research team to determine how this village made use of development funds. The results of the audit, along with the results from audits from other villages, will be shared with international development agencies that are active in the region.

B.4 Script for condition $T^I = 1, T^E = 1$

As we said before, we will send teams to implement audits in some villages that are taking part in the program. It is important that you know what these audits are.

- If this village is audited, we will examine the use that this village has made of project funds.

- When we have verified the information, we will hold a meeting with the population to present the results of the audit and tell the whole population how these funds were used and how the funds were managed by the representatives, as well as of any irregularities that we uncover.

- The results of the audit will be studied by an international research team to determine how this village made use of development funds. The results of the audit, along with the results from audits from other villages, will be shared with international development agencies that are active in the region.

C Auditor Check-list

Table 6: The Auditor check-list to detect fraud stemming from mis-reporting of purchases

Survey item	Strategy of the committee	Action necessary by the auditor
DA 53	**The committee may inflate each price**	**The committee may try to conceal the relationship with the auditor must verify the actual price at time of purchase and the amount indicated:**
		Talk to the mothers of the village (often it is the mothers who know more about market prices)
		If possible, go check the market (or in lieu of purchase), talk to the vendor, other vendors to confirm these mothers and
DA 54	**The committee may falsify receipts**	**The auditor shall verify the receipts obtained:**
		Check with the committee (the date of receipt, the seal on the receipt, writing of the receipt (which should be different from writing about FC)
		Check receipts with others that the committee, who can tell you if any, and if these suppliers are credible or friends of the committee
DA 55	**The committee can make a plot with suppliers to lie to you**	**The auditor should verify that suppliers do not lie:**
		Check if the suppliers change their speech in front of other people
		If it is a market, ask other suppliers
		Ask other people present at the location of suppliers
DA 56	**The committee may use the property as to deceive you: he can buy goods of inferior quality**	**The auditor should verify the quality of what was purchased:**
		Inform yourself about the different types of existing quality in the middle and prices (either mothers or market)
		Inquire about how to check the quality of the objects in question
DA 57	**The committee may add false transportation costs**	**The auditor should verify the true costs of transport:**
		Make sure the committee has directed the transport indicated by asking several people in the village -
		Learn about the real cost of transport in question in the village —
DA 58	**The committee can show the property or facilities that already exist and claim they were made by the RAPID project**	**The auditor should verify that the facilities (or goods) did not already exist:**
		Installation: check its age and condition. Ask villagers the date of construction of the facility
		Property distribution: when you are with the beneficiaries, ask to see distributed objects and ask the household how long the property exists there in the household and where is it from (the committee may also distribute goods he had already stored somewhere)
DA 59	**The Committee can claim that the project was completed while he has not been**	**The auditor should verify the existence of the project:**
		Facility: Field
		Distribution: With the 10 beneficiaries
DA 60	**Use local materials and pretend they were purchased elsewhere**	**The auditor should verify that the materials have been purchased elsewhere by going to check with the seller and talking to the population**
DA 61	**The committee may use the seasonal variation in prices**	**The auditor must ensure the price verification that this is the price at the time of purchase, not today**
DA 62	**The committee may try to conceal the relationship with suppliers**	**The auditor should verify the relationship:**
		Calling on the suppliers themselves
		Talking with people in the village Suppliers

D LATE results

Table 7: Results (LATE analysis)

Outcome		T^I	T^E	$T^I T^E$
Elite performance				
(1) Amount missing	estimate	240.47	42.66	−172.6
	(s.e.)	(730.91)	(284.41)	(651.53)
(2) Amount captured by elite (raw)	estimate	63.65	−11.45	41.92
	(s.e.)	(335.28)	(121.4)	(259.82)
(3) Index of private complaints (HH)	estimate	−.48	−.22	.29
	(s.e.)	(.55)	(.25)	(.49)
Ways that funds are missing				
(4) Est. exchange rate manipulation	estimate	610.74	272.47	−406.71
	(s.e.)	(729.41)	(305.23)	(628.25)
(5) Amount missing from other strategies	estimate	−383.62	−220.44	217.22
(quantity manipulation, other)	(s.e.)	(828.36)	(345.86)	(708.40)
(6) *Share* of amount missing that is due	estimate	2.47	0.77	-1.96
to exchange rate manipulation	(s.e.)	(2.40)**	(0.87)**	(2.03)
Modifications in Process				
(7) Number of meetings (HH)	estimate	1.82	.67	−.8
	(s.e.)	(3.13)	(1.27)	(2.77)
(8) Presence of accounting form (%)	estimate	88.21	62.29	−82.68
	(s.e.)	(320.58)	(123.48)	(266.65)
(9) Share of expenses credibly justified (%)	estimate	0.24	0.33	0.04
	(s.e.)	(1.85)	(0.65)	(1.75)

Notes: *** p<0.01, ** p<0.05, * p<0.1. This table presents the LATE results using treatment to instrument for the beliefs of committee members. The exclusion restriction requires assuming that effects pass through committee member beliefs only and not through beliefs of other actors, such as traditional authorities or populations. Lines (1)-(3) present the results on elite performance, (4)-(6) present the results on elite substitution between types of fraud, lines (7)-(9) present the results on actions of the elite aimed at satisfying donors. HH indicates household level data. Standard errors of household level observations are clustered at the village level. All regressions include randomization block fixed effects.