

## 9. Centralised versus decentralised monitoring in developing countries: a survey of recent research

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### Summary

We consider the effectiveness of centralised and decentralised monitoring using a theoretical framework of factors affecting each approach. Centralised monitoring is more costly, yet more professional. However, the monitors themselves are not directly affected by the activity they are monitoring, so they may have less at stake in policies or services working well. By contrast, in community monitoring local people and civil society have high stakes in improving local outcomes. In the political economy literature, top-down audits have been seen as more effective in certain types of activities (like procurement) where detailed documentation exists, and where corruption can be more clearly defined as compared to mismanagement. Community monitoring has had higher efficacy when collective action problems can be solved, when monitoring teams have a sense of agency, and when the composition of teams is more homogeneous. Community monitors have deeper knowledge of local agents, so that (*ceteris paribus*) this approach should be less costly for the government because monitoring resources can be targeted better. However, both local monitoring and local agents may suffer from problems of elite capture.

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Corruption is a global phenomenon with varying effects, both on the economy and society. It inhibits economic growth (Li, Xu, and Zou 2000; Mauro 1995), affects business operations, employment and investments (Colonnelli and Prem 2022; Shleifer and Vishny 1993; Hanousek and Kochanova 2016) and reduces tax revenue along with the effectiveness of a variety of financial assistance and public programmes (Tanzi and Davoodi 2001). The IMF has recently put governance issues at the forefront of its macroeconomic policies,<sup>1</sup> the 16th SDG has emphasised the importance of effective governance, and the World Bank has recognised that Covid-19 created new opportunities for corruption that will become manifest only later, and released a report on enhancing government transparency (Bajpai and Myers 2020). So, the need for better governance has never been more apparent.

In a seminal paper, Bardhan and Mookherjee (1998) discussed the trade-off between centralisation and decentralisation in public service delivery from the point of view of reducing corruption.<sup>2</sup> A centralised system is prone to officials taking bribes from non-targeted beneficiaries of a targeted public service delivery programme. This is traded off against elite capture, to which decentralised systems are more prone. Their main conclusion is that the comparison is ambiguous in the absence of institutional detail. While centralised governance has had the advantage of performing better at scale, decentralised systems have been better at targeting the intra-community beneficiaries of anti-poverty programmes. They also emphasise the importance of enhancing local democracy and reducing asset inequality in order to prevent elite capture; this change is an essential condition for decentralisation to succeed.

These considerations are reflected in some of the empirical literature: Wade (2000) argued that India's corruption in the context of irrigation was due to the centralised nature of the bureaucracy. On the other hand, Treisman (2000) concluded that corruption is more likely to be a problem in federal governments. A review of 56 studies by Shah, Thompson and Zou (2004) found that decentralisation sometimes improved service delivery and corruption among other benefits, but at other times worsened it, with the pattern holding across a large range of countries. While tackling corruption is central to many decentralisation programmes, the emerging literature on the comparative efficacy of centralised versus decentralised monitoring of corruption remains rather disperse. In this survey we compile the existing literature, develop a theoretical framework for comparing the efficacy of these alternative anti-corruption monitoring strategies, and then review the existing empirical evidence in the light of this theoretical model. This enables us to identify the conditions under which centralised and decentralised monitoring strategies may function better – and our conclusions echo many of the propositions in early theoretical work by Bardhan and Mookherjee (1998).

The remainder of the chapter is organised as follows: Section 9.1 provides a look ahead to the key issues in comparing the two types of monitoring, and some brief signposts to our approach and conclusions. Section 9.2 provides a conceptual framework to understand how different types of audits can help

in reducing corruption. Section 9.3 covers third-party audits at central level, and Section 9.4 focuses on community monitoring, In Section 9.5 we look at the literature that combines both top-down and bottom-up monitoring. Our conclusions highlight some open questions.

## 9.1 Understanding top-down and bottom-up monitoring

In top-down or third-party audits a specifically assigned independent organisation audits the activities of other institutions in the public sector. For example, in India the Comptroller and Auditor General (CAG) is an independent auditing agency responsible for auditing the expenditures and receipts of all central and state government departments and other public sector organisations. In Brazil, a similar function is performed for municipalities by the Comptroller-General of the Union (CGU), including judicial powers to prosecute. An alternative bottom-up approach seeks to allow local communities to collectively monitor the performance of an activity, a programme, a policy, or an organisation in the social welfare sector.

Until recently the data to study the efficacy of such audits in developing countries has not been available to researchers. One of the main constraints in studying corruption has been the measurement of corruption outcomes (Olken and Pande 2012). Some interesting findings have come from a recent literature (for example, Avis, Ferraz, and Finan 2018; Ferraz and Finan 2008; Ferraz and Finan 2011; and others), based on a few selected countries (like Brazil) that have made data available for research. Decentralised monitoring has also been acquiring increasing importance – over the last decade, the World Bank has dedicated about \$85 billion towards local participatory developments. There have been some reviews and meta-analyses of community monitoring (for example, Mansuri and Rao 2013; Molina et al. 2016). Yet there is no comprehensive survey of the relationship between the two approaches and the similarities (or otherwise) in the findings. Our main contribution here is to present some selected studies on both types of monitoring, their design, and the similarities in questions asked, as well as their key results. Such a comparative overview of both together can best enable scholars to advance the literature in more useful ways. Papers are selected for the survey mostly on the basis of being able to provide credible causal evidence and our coverage is restricted mainly to developing countries.

We also provide a conceptual framework that highlights the different ways in which we expect top-down audits to differ from bottom-up approaches. Building on the criminal deterrence literature we highlight the importance of the probability of audits (whether top-down or bottom-up), the likelihood of punishment in each case, the type of punishment, and so on in deterring corrupt behaviours. The framework allows us to explain (for instance) why top-down audits are more likely to suffer from problems such as collusion or an inability to target the right types of corruption, while bottom-up

monitoring is likely to suffer from collective action problems. We believe this framework helps to reconcile the different ways that empirical work has approached these two types of monitoring. Methodologically, the literature on community monitoring is based mainly on experimental evidence, while the literature on top-down audits is based on observational data. Studies of top-down audits have mostly focused on corruption outcomes (which are hard to measure), while in contrast studies of community monitoring have tended to analyse more outcome measures that are easier to measure. However, the main research questions across both types of study have been similar in addressing how to tackle the moral hazard problem where an agent (government official or politician) has incentives misaligned with citizens' interests.

In terms of our framework, we find that studies on top-down audits typically focus on how the intensity of audits affects outcomes, whether past audits affect corruption outcomes, and which types of activities are more affected, while those on bottom-up monitoring focus on interventions that aim at increasing the transactions costs of corruption by empowering citizen monitoring groups with information on their entitlements.

Looking ahead to our results, they suggest that top-down audits have high efficacy in reducing corruption when legal punishments follow any audit findings. However, this may not translate into better economic outcomes. For some types of public good services, where it is not easy to document wrong-doing (such as health), top-down audits may be counterproductive. On the other hand, for procurement they are more likely to work in reducing corruption when punishment is well-defined. The literature has progressed methodologically on the lines of being able to measure corruption outcomes and using random audits to identify causal effects. The main gap is to extend these studies to other settings for external validity, because this kind of innovative research has largely focused on Brazil and a few other Latin American countries. Extending the research to cover the eventual outcomes of policy may be fruitful, but the links between the intermediate outcomes are equally important to understand the logical chain.

In community monitoring what has worked is not just information by itself but the task being monitored: how complex it is, whether it is a private or public good, what support is given in negotiating with the service providers, whether community monitors feel empowered enough to change things, what punishment is available for being caught, and the composition of community monitoring committees. Focusing as they do on outcomes, an open question for this literature is why the results are mostly negative – very few studies find significant or substantial effects from better community monitoring on final outcomes. The political economy of power relations seems to be a key reason. Since voting is one of the few ways in which communities can hold politicians accountable, linking the success of community monitoring to voting outcomes might be an avenue to explore this issue. Afridi et al. (2021) is one of the few studies on this topic.

Some of the issues that arise in top-down audits also arise in community monitoring such as the effects on corruption not translating into outcomes for similar reasons, variations across the type of service being monitored, and the salience of the punishment available for wrong-doing. However, the collective action problem is unique to community monitoring as well as the lack of empowerment versus the authority and power of the central audit agency. The strength of community monitoring lies in the greater knowledge in the community about the quality of public services they have been provided with. Unempowered community monitors may not be effective and the types of services that are best served by community monitoring include health, education, and public service delivery programmes, rather than procurement, where top-down audits have the edge. Studies that compare the two types of monitoring (Olken 2007) or that combine them (Serra 2011) are rare, but perhaps deserve further attention from scholars.

## 9.2 Modelling incentives for honest or dishonest behaviour

Consider an agent who chooses an action  $a_i \in [0, 1]$ , where 0 stands for honesty and 1 for complete dishonesty, in order to create a revenue stream  $X$  out of which a proportion  $\beta$  can be skimmed off in corruption ( $\beta$  measures the opportunity for corruption or the transaction costs associated with it). The agent gets paid a fixed wage  $w$  for doing their job. The actual amount of revenues lost due to corruption is private information to the agent who holds that job, creating a moral hazard problem. However, with some probability  $q$ , audits take place after the corruption has taken place. Audits can be third-party and top-down audits (assumed to be carried out by an independent regulator) or can be bottom-up social audits (or both). Audits have two consequences: they can expose corruption with some probability and, where exposure occurs, they may lead to punishment with some probability. We assume here that only corrupt agents are caught with probability 1, although everyone is monitored – that is, there are no mistakes in the monitoring technology. When corruption is caught, the agent can be punished – via a legally mandated punishment  $F$ , which could either be a fine or jail term (there may or may not be limited liability), or the agent can also be removed from the job, for example via elections or dismissal. An alternative punishment for corrupt agents is loss in status,  $S$ , caused by reputational loss or a loss of self-image due to its exposure. We assume that punishment occurs with probability  $m_k$ , where  $k$  can be either a top-down or a social audit.

We can interpret the agent as an official or an elected representative (as much of the empirical literature on audits reviewed here also does). When corruption is revealed via audits, for example, voters can discipline incumbents by throwing out corrupt incumbents. Here  $f_k$  is interpreted as the probability of losing the next election and  $F$  can be interpreted as the present value of future rents, that is, the opportunity cost of corruption.

$$EU_i(a_i) = (1 - q)g(a_i\beta X + w) - q f_k(F + S)a_i \tag{1}$$

We assume that the function  $g$  is sufficiently concave to guarantee an interior solution. The fine and loss of status are increasing linearly in  $a_i$  (the action). (See Chalfin and McCrary (2017) for similar functions used in the criminal deterrence literature.)

The timeline is the following. (1) An audit probability  $q$  is announced, (2) the agent gets wage  $w$  and chooses  $a_i \in [0, 1]$ , (3) the audit takes place, and (4) payoffs are realised. Implicitly we assume that the agency can commit to a certain  $q$ . There may be situations where instead audits respond to changes in  $a_i$  (a simultaneous move game).

Solving by backward induction, for the choice of  $a_i$ , we have:

$$(1 - q)g'(a_i, \beta X, w)\beta X = qf_k(F + S) \tag{2}$$

Since  $g'(a_i)$  is decreasing in  $a_i$  there is a unique solution  $a_i^*$  which is decreasing in  $q, F, f_k, S, w$  and increasing in  $\beta X$  (the proportion captured by corruption). The first-order conditions imply  $(1 - q)g'(a_i, \beta X, w)\beta X = qf_k(F + S)$ , or:

$$\frac{qf_k(F + S)}{g'(a_i, w)\beta X} = \frac{1 - q}{q} \tag{3}$$

Given the concavity of  $g$  we can predict that:

- a. The lower the transaction costs, the higher the level of the corrupt action.
- b. The higher the punishment, the lower the corrupt action.
- c. The higher the probability of being monitored, the lower the level of the corrupt action.

This is line with Becker (1968), where increasing the threat of audit is a substitute for higher punishment. Taking a simple example:

$$g = \ln(\beta X + w)$$

So, we have,

$$\frac{\partial a_i^*}{\partial q} = \frac{-1}{q^2 f_k(F + S)}$$

That is, the agent’s responsiveness to a change in the probability of audit is lower when  $q$  is high and when punishment is high.

This benchmark model can be extended to include the type of the agent. Some agents are intrinsically motivated to be honest ( $a_i = 0$ ), others less so. We assume that only dishonest agents are ever caught (since there are no mistakes in the monitoring technology). If agents are honest then the utility function needs to be modified:

$$U_i(a_i) = g_i(w) - qz_i \quad [4]$$

where  $g_i$  is the level of intrinsic motivation (potentially a function of wages) and  $z_i$  is the loss in utility due to being monitored when the agent is honest. We assume that a fraction  $1/\gamma$  of the agents are completely honest. While  $\gamma$  is known, the type of any given individual is private information, known only to themselves. Now we discuss what centralised and decentralised monitoring can do to reduce corruption.

First, we consider the choice of a centralised agency,  $A$ , who can deduce the  $a_i^*(q)$ . The agency is assumed to care about minimising corruption. The cost of auditing is linear  $cq$ , with  $c > 0$ . We abstract from issues of collusion or capture, although they are important issues that have not been tackled in the empirical literature. Moreover we have abstracted from the career concerns of audit officers; these may be important but they have not been discussed in the empirical literature. For an official  $U_A = B(a_i^*(q)) - cq$ , where  $B(\cdot)$  measures the social benefits from reducing corruption (such as better public service delivery). Consider what happens if

- a. An increase in  $q$  occurs. This is costly and depends on the cost parameter  $c$ . The cost includes the salaries of administrative staff, the expenses associated with audits and so on. Moreover, since individual types are not known,  $q$  cannot be conditioned on type.
- b. Punishment parameters vary:  $\frac{\partial a_i^*}{\partial q}$  may depend on the punishment parameters as well as  $\beta X$ . In our example higher punishment and higher  $q$  are substitutes: responsiveness to higher  $q$  decreases as punishment increases, and vice versa (as  $q$  goes up, so the disincentive effect of greater punishment levels falls off).

Therefore the optimal choice of  $q$  (the probability of audits occurring) will depend on all these parameters – in the example the optimal probability depends on the initial level of  $q$ ,  $F, f_k$  (that is, all the factors that affect responsiveness). Although we did not model it, the optimal audit probability can also depend on an individual's risk preferences via their responsiveness of  $a_i$  to  $q$ .

Now we consider a decentralised choice of  $q$  (the probability of audits) – either through community monitoring or social audits. The obvious problem here is the collective action problem – do citizens internalise the social benefits of monitoring when a public good is provided? Typically, community monitoring has been studied for health and education or poverty alleviation programmes – and here, while there are some private benefits from undertaking monitoring, the social benefits are likely larger. Assume there is a community of  $n$  citizens who are the main beneficiaries of a public service programme. Assume that the utility of an individual citizen is represented by:

$$U_j(q_j) = V_j(a_i^*(q) - c_j(q_j, q_{-j})) \quad [5]$$

Let  $Q$  denote the equilibrium level of  $q = f(\bar{q})$  that the agent faces as a result of strategic interactions between the citizens. An individual citizen 1 can decide how much effort to spend on monitoring. If others increase their monitoring and the policy benefit delivered is a public good, then citizen 1 may decide to lower their own monitoring (strategic substitutes). Alternatively, if more monitoring by others encourages citizen 1 to monitor as well, that could imply strategic complementarity. Strategic substitutability is more likely for monitoring activities involving pure public goods, for example road-building for a village. Complementarity is more likely for some types of monitoring activities, such as attending weekly meetings to hold officers to account. Cost could be individual or could depend on how many others monitor – for example, if there is intimidation of community auditors by officials or incumbents then the more that other citizens monitor the lower the chances that any individual would be punished for monitoring.

If there is strategic substitutability, then we can expect free riding – leading to a suboptimal  $Q$ . Consider a society of  $n = 2$  individuals. Suppose only one person is needed to monitor  $q = \max(q_1, q_2)$ , and costs depend only on each individual's own  $q_j$  but private benefits are a proportion  $\alpha_j$  to each individual, that is,  $V_j = \alpha_j g'(a_i^*)$ , then no one contributes if the private marginal benefit  $\alpha_j g'(a_i^*)$  is less than the private marginal cost  $c'_j(q_j)$  of doing so. This is suboptimal when aggregate marginal benefits are larger than the social marginal cost (which is also  $c'_j(q_j)$ ).

Strategic complementarity can be modelled by assuming instead that  $q = q_1 q_2$ . In this case monitoring incentives increase with the monitoring intensity of other citizens. This generates a coordination game: if the individual citizen's beliefs are that others will monitor, then their own incentives to monitor increase: if, for example,  $q_2$  is sufficiently high, then individual 1 would choose  $q_1 = 1$ , otherwise  $q_1 = 0$ . So, there will be two pure strategy equilibria: one where both citizens monitor and one where no one does.

In practical terms, community monitoring usually involves delegation to a team. In this case the analysis above applies not to all citizens but just to members of the team. Another important issue is the composition of the monitoring team – if the team is less empowered than the agents whom they are monitoring then 'elite capture' may subvert the process. This is also related to building a sense of agency among the team members.

With decentralised monitoring, local communities have better information on the types of agents. They can target monitoring more efficiently. Then  $a_i^*$  can be affected by the following:

- a. If monitoring activities are strategic substitutes, then the higher the difference between individual and group payoffs, the lower  $q$ .
- b. If strategic complementarities exist in monitoring, then the higher the beliefs on others' participation  $q_{-j}$ , the higher  $q$ .

- c.  $\frac{\partial a_i^*}{\partial q}$  depends on the punishment parameters as well as  $\beta X$ . This is the same as in top-down audits.
- d. Targeting monitoring resources towards dishonest agents (who are assumed to be known with a higher probability than in the case with top-down monitoring).
- e. The composition of the monitoring team, the degree to which they have 'agency'.
- f. Changes in the transaction costs of corruption  $\beta$ . Providing information/awareness of entitlements may decrease  $\beta$ , leading to changes in equilibrium  $Q$ .

In the following sections, we organise the literature according to our framework above. Section 9.3 looks at third-party audits and the impact on corruption.

### 9.3 Centralised monitoring

The conceptual framework presented above suggests that changes in the intensity of monitoring have an effect on agent behaviour, although the effect is via forward-looking agents: they decide their behaviour anticipating  $q$ . The literature however has focused on the effect of *past* audits. Theoretically past audits should not have an effect unless there is no commitment on  $q$ , or there are selection effects, or there is a difference between the actual and perceived risk of being caught. (See also Malmendier (2021) on how experiences shape reasoning.) There may also be some differences in information between the law enforcers and agents (Apel 2013). There is some analogous work on deterrence in criminal behaviour in the law and economics literature (see the survey by Chalfin and McCrary 2017). Indeed Becker (1968) showed that theoretically increasing the threat of punishment (with full commitment) was more likely to lower crime than increasing the size of the punishment for risk-averse individuals. We explore key issues first in Brazilian studies of municipality audits, then in studies from other countries, and last in studies of individual decision makers.

#### Brazil – randomised audits

The empirical literature on centralised audits departs from the theory in a number of ways. The theory suggests that varying the announced  $q$  can have an effect on corruption. Avis, Ferraz, and Finan (2018) used publicly available audit reports of municipalities from Brazil's anti-corruption audit programme, which started in 2003. It is implemented through the autonomous Controladoria-Geral da União (Office of Comptroller-General, hereafter abbreviated to CGU). CGU randomly choose municipalities every month from a sample of all Brazilian municipalities with fewer than 450,000 inhabitants.

CGU auditors inspect the chosen municipality's accounts and also carry out physical verification of public works and service delivery. The auditors also meet with local officials and members of the community. Based on their findings, a report is prepared, which is presented to higher authorities for action.<sup>3</sup> A summary of the principal findings for the audited municipalities is then also released to the media and posted on the internet (Ferraz and Finan 2008; Ferraz and Finan 2011).<sup>4</sup> The announced  $q$  is, therefore, equal across municipalities. The types of punishment include legal action ( $F$  in the model), as well as perception of the incumbent mayor by voters ( $f_k S$  in the model).

What Avis, Ferraz, and Finan (2018) found, however, was that municipalities that had been audited in the past had corruption levels 8 per cent lower than others. Past audits also affect the behaviour of neighbouring municipalities with local media, who get to know about the results of nearby audits. Corruption levels went down by 7.5 per cent due to an additional neighbour being audited. Having had an audit in the past also increased the chances of legal action being taken against the mayor by 20 per cent. They attribute the lower corruption levels to the higher perceived credibility of associated legal punishment by decision makers who have been audited and the neighbouring municipalities. In the criminal deterrence literature this effect is referred to as specific deterrence, as opposed to general deterrence (Chalfin and McCrary 2017).

The Avis, Ferraz, and Finan (2018) structural model allowed them to run some alternative policy simulations to understand which policies would help to reduce corruption most effectively – in effect asking which of the punishments outlined in the model work best to reduce corruption – increasing audit probabilities ( $q$ ), improving the exposure of corruption to voters via audit reports ( $f_k$ ), increasing the legal costs of being caught for corruption ( $F$ ), and improving the educational/occupational background of candidates running for office ( $\gamma$ ). Out of these they found the largest effects for increasing the legal costs of corruption – in line with Becker (1968), if we assume risk aversion among agents as well as increasing  $q$ .

Zamboni and Litschig (2018) designed a randomised policy experiment in Brazil to ask the following questions: (i) does a higher probability of getting audited (a higher  $q$ ) discourage rent extraction by local government officials? And (ii) does the higher audit probability have a differential effect on different sectors – procurement versus health service delivery and targeted cash transfers? According to the framework we proposed, it is more difficult to deter corruption in sectors with high possibilities of extracting rent – in their setting this was procurement. In addition, the efficacy of  $q$  is affected by the levels of punishment and the probability of legal sanctions. In Brazil the chances of punishment and the level of punishment were much higher for procurement-related irregularities. This effect should dominate the higher  $\beta X$  in procurement relative to other sectors. So, Zamboni and Litschig (2018) hypothesised that the responsiveness of corruption in procurement to higher audit risk would be greater because punishments for such corruption types is

higher in Brazil, involving long jail terms and fines. By contrast, in health service delivery the type of corruption is in the nature of absenteeism from work, and targeted cash transfers, where corruption is highly visible.

The Zamboni and Litschig (2018) experiment was run jointly with the Comptroller-General of the Union (CGU) and involved the randomisation of 120 municipalities into two groups, a high audit risk group, exposed to an audit risk of 25 per cent and a low audit risk group (control group), exposed to an audit risk of 5 per cent. Results show that corruption was affected the most in procurement, especially in those programmes that allowed greater discretion for the officials (opportunities for corruption in our conceptual framework).

In relation to the lower efficacy of audits in the case of service provision, the authors conceded that it was challenging to detect the inconsistencies in service provision (of health or targeted transfers) through a CGU audit. Even if they were detected, punishment involved at most the loss of that official's job. Public complaints were not recorded on paper anywhere, and the officials were able to dispute these complaints. This made it difficult for the auditors to verify which of the competing claims was true.

However, it is much easier to detect irregularities in procurement with audits because local officials were required to document the purchasing process in a detailed manner, and their punishments were relatively severe, including not only job termination but also potential fines and a jail term too. Thus, these findings suggest that increasing the probability of an audit alone is not sufficient to deter rent-taking, and it might prove futile to do so for programmes that are targeted based on easily observable individual or household characteristics (like cash transfer programmes).

Ferraz and Finan (2008) also exploited the randomised timing and public dissemination of the audits conducted in Brazil to investigate whether voters punished politicians who were exposed as corrupt due to audits – that is, they examined how large  $F$  was in the context of municipal elections in Brazil. Theoretically, Persson and Tabellini (2002) and Besley and Prat (2006) argue that making more information available to voters should lead to better accountability via re-elections. In order to test this proposition, exposure to information should be exogenous, otherwise (for instance) an observed correlation between high exposure and low re-election could simply be capturing a greater presence of media in places that are also more competitive, or where voters are more aware of local politics, or where voters are more likely to be affected by corruption and therefore put more effort into finding out about it.

They compared the electoral outcomes of mayors eligible for re-election between municipalities audited pre and post the 2004 local elections (covering July 2003 to June 2005).<sup>5</sup> If information on corruption is a salient factor affecting re-election, then there should have been a significant difference between re-election rates in those municipalities that were audited before the election versus those audited after the election. The municipalities where audits took place after the election counted here as a control group – in the

sense that audit findings are not able to affect an election held before they are produced, while the treatment group was those that had audits before election. Ferraz and Finan (2008) measured corruption as the number of violations associated with the sum of fraud in procurement, diversion of public funds, and/or over-invoicing. Media sources were measured using the number of locally present radio stations in a municipality. This allowed the authors to test whether the audits had any differential impact across areas with or without a strong presence of local media.

Overall, the authors found that on average the electoral performance of mayors audited before elections was not significantly different from those audited after them. However, once they compared mayors with the same measured corruption levels, they found a 17 per cent reduction in the probability of re-election if the audit was done before the election rather than after. This probability reduced more when corruption was higher. Where audits found no corruption the chances of re-election increased for incumbent mayors audited before elections. These effects were more prominent in municipalities where local radio was present. The evidence is consistent with a narrative where voters have a prior belief about incumbent corruption and then revise it upwards or downwards based on the reports.

Voters may have many different reasons to punish corrupt incumbents. For example, in Brazil, the discretionary funds allocated to municipalities are reduced where mayors have been found to be corrupt. Brollo (2008) used this reduction to show that voters actually punish politicians who are responsible for a reduction in transfers to their municipality, rather than politicians who are exposed as corrupt. Even if voters punish corrupt politicians, it may not follow that politicians reduce corruption as a result.

Ferraz and Finan (2011) also argued that politicians do respond to re-election incentives in local governments in Brazil. They used the political agency framework of Besley (2006) as a conceptual framework. The model is based on voters deciding to re-elect an incumbent, without observing his or her type or actions but based on a signal from voters' own utilities, which are affected by actions of incumbents. The model predicts that mayors who face re-election incentives will be less dishonest than those who do not because corrupt (type) mayors wanting to be re-elected can foster support by behaving like non-corrupt mayors and not indulging in rent-seeking activities. Corrupt mayors thus exploit the information asymmetry with voters. So, mayors who are audited and face re-election should turn out to be less corrupt than those who are audited but do not face re-election. The empirical finding was that municipalities with mayors in their first term had a significantly lower percentage of stolen resources as compared to those with mayors in their second (and hence) last term. Thus, the results on re-election incentives were consistent with the theoretical model.

Their evidence therefore complements Ferraz and Finan (2008), who showed that voters make use of the publicly available information to punish corrupt politicians. Together, the authors say, these results imply that electoral

accountability acts as a powerful tool to align politicians' actions with the voters' preferences. In terms of our conceptual framework, the link from audits to information exposure to punishment at the ballot box to incumbents then reducing corruption in response to re-election motives is complete.

Ultimately, the primary purpose of reducing corruption via audits is to improve economic performance. Reducing corruption may come at the cost of a loss in intrinsic motivation for agents who are honest or who are afraid to make mistakes and as a result decide not to take risky decisions. Colonnelli and Prem (2022) evaluated the impact of audits on firm performance and local economic activity. They used the extensive audit data available in Brazil, with a particular focus on government procurement records. The anti-corruption programme in Brazil allowed them to address issues of measurement via audit measured corruption and endogeneity (firm-level activity, economic activity and corruption are simultaneously determined), taken care of by the random audits.

They combined audit reports and the administrative matched employer-employee data (as well as some censuses on retail and service sector firms) on the Brazilian formal sector. This data is used to compare the economic outcomes of randomly audited municipalities (treatment) with either later randomly audited or never-audited municipalities (control). Results suggest that treatment municipalities experience higher levels of economic activity, improved access to finance, and more entrepreneurship as compared to the control ones. These findings imply that the anti-corruption crackdown positively affected the local economy and thus lend empirical support to the 'sand in the wheel' view (reduction in corruption increases economic activity). They complement their results with a firm-level analysis. Like the municipal level analysis, the firm-level analysis is based on a comparison between firms involved in dubious government procurement ('corrupt firms') and similar firms operating in the same sector that are situated in never-audited municipalities (control group). A dynamic difference-in-difference specification is used to show that corrupt firms that are audited perform better than the control group. Their results show that it is precisely those firms that rely on government (procurement) that benefit the most from the anti-corruption crackdown. Overall, the results of the paper consistently support the 'sand in the wheel' argument, that is, corruption acts as an institutional failure, while suggesting costs and distortions to firms dependent on the government as the primary channel through which corruption hinders overall economic growth and firm performance. Moreover, they find no support of a politician selection channel (at local level) from audits but rather a disciplining channel.

In contrast, Lichand, Lopes, and Medeiros (2016) found a negative impact of audits on health outcomes in Brazil. The main idea is that audits and the punishment from audits might lead bureaucrats to take fewer decisions and to reduce procurement. As our model suggests, since audits cannot be targeted to dishonest politicians they may reduce incentives to work among honest agents as well. They analysed the effects of audits both on corruption

within the health sector and on downstream outcomes in Brazil's health sector. Their data is on the incidence of corruption in health transfers between 1997 and 2007, captured extensively in the audit reports. They employed a difference-in-difference (pre and post audit, procurement-related transfers vs other transfers) strategy to tease out the causal effects of audits on corruption in procurement-related transfers in the health sector. Moreover, they used the effects of announced audits on neighbouring municipalities to tease out the effects of announcement of audit vs actual audit. If officials reacted to audits in nearby areas, they attributed it to a behavioural response. Results show that both corruption and procurement irregularities within health transfers came down as a result of the audit programme in Brazil. On the other hand, public spending witnessed a decline, as a result of which infrastructure and medication suffered. At the same time, linked to the reduction in corruption, mismanagement rose, especially in problems linked to the stock of medication and quality of health infrastructure. Thus, the programme brought about a reduction in procurement purchases, either because bureaucrats could no longer capture rents or because they were scared of being caught and punished. Consistent with the impact of the programme on bureaucratic performance, a detrimental impact on the health indicators is also seen. A comparison between the indicators directly concerned with municipalities' health spending – like preventable deaths – to those that are not – like the deaths caused by external causes – shows how the audit programme, despite reducing corruption, considerably worsened the quality of health services over the long term. Overall, the paper provides evidence that anti-corruption programmes might have an adverse consequence for social welfare.

### Other countries – non-random audits

Bobonis, Cámara Fuertes, and Schwabe (2016) employed similar measures of corruption as Ferraz and Finan (2008; 2011) to answer whether monitoring corrupt activities (audits) induce a *sustained* reduction in corruption. Like Avis, Ferraz, and Finan (2018), their research question was whether past audits help to reduce current corruption. The Bobonis, Cámara Fuertes, and Schwabe study used the timing of municipality audits in Puerto Rico between 1987 and 2005. In Puerto Rico, municipalities are audited by an autonomous audit authority (Office of the Comptroller of Puerto Rico – OCPR) in a pre-specified order that was determined in the 1950s. Once an auditing round is completed, the next one follows the same order. The empirical strategy in this paper exploits the differences among municipalities who are audited before an election (timely audit) and those audited after an election (untimely audit). However, they found the opposite result on the long-run effects of past audits on corruption.

The key reason for this contrast across the two studies is that in Puerto Rico audits occur at predetermined times, as opposed to randomly in Brazil. Pre-fixed audits are different in the sense that an agent would know exactly when

they are due to be audited,  $q = 1$  in some years and  $q = 0$  in other years. Therefore (out of the fraction  $\gamma$  of agents who are not always honest) we should see corruption being low when the audit is due, assuming that there is a credible punishment for it. Bobonis, Cámara Fuertes, and Schwabe considered the punishment as being the exposure of corruption to voters, as a result of which voters would throw out corrupt incumbents. Whether such a disciplining effect lasts longer depends on what we assume about voters' behaviour, term limits, and the time horizons during which incumbents expect to be in power. In their setting, turnover of politicians is high, so that these time horizons are short. In addition, there may be selection effects (see for example, Persson and Tabellini 2002, Ch. 4), where voters use signals of dishonesty to throw out corrupt incumbents: this effect leads to lower corruption over time.

So, the hypothesis is that timely audits lead to sanctioning effects where voters punish incumbents who are shown to be corrupt. Incumbents expecting an audit before election will then reduce corruption in response, implying lower irregularities when there are timely audit reports. If audits induce a positive selection of less corrupt politicians, then the lower corruption for municipalities with timely audits would lead to lower corruption in the long run as well. Bobonis, Cámara Fuertes, and Schwabe (2016) measured the short-run impact of timely audits on corruption by regressing irregularities in year  $t$  on whether audits were carried out in the two years prior to election. The long-run impact was measured by regressing irregularities four years later on audits in year  $t$ . Finally, they tested for the impact of the timeliness of audits reports on re-election rates. They showed that timely audits induce a significant short-term reduction in municipal corruption levels of approximately 67 per cent, as well as an increase in mayors' re-election rates in audited municipalities. Yet there were no significant differences in the long-term corruption levels between those municipalities that had timely audits versus those that did not. The authors deduced that selection of politicians happens, but not on honesty – rather, voters seem to reward competence. The combination of sanctioning effects in the short term and selection effects towards competence in the long run can explain their findings.

The issue of punishment for those caught by top-down audits is also taken up in greater detail by Michael Mbate in the chapter in this book on parliamentary sanctions and local accountability (2023, Chapter 8). In their former territories, the British colonisers left a legacy of parliamentary democracy along with supreme audit institutions and parliamentary sanctions for those caught by the audit agency. In practice, however, political economy concerns (like partisan loyalties) have often led to low punishment by parliamentary sanctions.

### Studying audit impacts on individual decision makers or units

Kleven et al. (2011) studied the role of audits on tax evasion by looking at individual-level behaviours in an advanced economy, Denmark. Similarly to

Avis, Ferraz, and Finan (2018) and Bobonis, Cámara Fuertes, and Schwabe (2016), this paper provides further evidence of the efficacy of audits. They studied the effects of past audits as well as anticipated audits on tax evasion. Their main contribution is to show that audits affect only corruption in discretionary income – in terms of the model – this suggests that audits have a greater effect when the opportunity for corruption –  $\beta X$  is higher. Thus they echo the results of Zamboni and Litschig (2018). These papers implicitly test the interaction of  $q$  with  $\beta X$  and  $F$  – while Zamboni and Litschig (2018) considered the interaction of  $F$  with  $q$ , Kleven et al. (2011) studied the interaction of  $q$  with  $\beta X$ .

The authors based their theoretical model on the tax evasion model of Allingham and Sandmo (1972).<sup>6</sup> They designed a field experiment that imposed different audit regimes on randomly chosen taxpayers. Their sample consisted of 40,000 individuals who duly filed income tax. The first stage involved the random selection of half of these taxpayers for unannounced audits of tax returns filed in 2007. The rest of them remained unaudited. The first randomisation exercise allows for the estimation of the impact of past audits on future reported income, which is carried out as a comparison of the two groups in the subsequent year. The second stage of the experiment is based on an arbitrary selection of employees in both audit and no-audit groups for pre-announced audits of tax returns filed in 2008 (letters were sent out to announce the audits in advance). The authors examined the threat as well as the no-threat group to study the impact of the possibility of an audit on the reported income. The experiment resulted in an almost negligible tax evasion rate for income subject to third-party reporting, while the tax evasion rate was substantial for self-reported income. Prior audits and threat-of-audit letters had a significant impact on the tax evasion associated with self-reported income.

In all of the papers considered so far, the audit agency is assumed to be independent (and evidence is provided to show that it is) and non-corruptible. It is still an open question under what conditions audit can make outcomes worse due to bribing of auditors. However, Duflo et al. (2013) did this for a private sector firm – they showed that, when firms pay for their own audits, the conflict of interests results in underestimation of irregularities. Chander and Wilde (1992) explore this question theoretically and show that tax audits can reduce tax evasion when auditors are honest but, when that is not the case, audit design can lead to surprising results. The model allows for collusion between taxpayers and auditors. The audit agency is treated as a separate player who is interested in maximising expected tax revenue net of costs of audit. Taxpayers who are audited pay the additional tax due plus a penalty. Taxpayers will be willing to pay bribes (dishonest evaders) to auditors when the cost of paying tax plus the fine is higher than the expected cost of bribing the auditor and being caught with some probability. Auditors are willing to accept bribes when the private costs to them of being caught are lower than the bribe income. The model shows that dishonest evaders are more likely

than honest evaders to evade taxes. Since the returns from evasion increase when tax rates go up, and the returns from bribing also go up, it is likely that higher taxes lead to higher audit and lower revenues for the tax authority. In some cases, the tax agency may prefer to forgo auditing altogether as it is costly and collusion leads to too low tax collections. We do not know of any empirical work on collusion between a governmental audit agency (SAI) and auditees – however, Duflo et al. (2013) is an example of a study where there is collusion between private sector firms and a third-party audit agency.

To summarise, in this section we find that electoral incentives can be a powerful force in reducing corruption via voters' information on politicians' corruption. However, the effects of electoral punishment are short-lived, and weaker than non-electoral (judicial) punishment. Audits have more of a disciplining effect in reducing corruption rather than a selection effect. The effects of audits on corruption vary by the nature of the service being audited – for example, procurement is more responsive to the threat of audit than, say, absenteeism of workers owing to both the rules being defined much more clearly and the punishment being higher. In terms of effects on ultimate outcomes, there is some evidence that audits may not help in improving outcomes due to the problem of the auditees not willing to take the risk of being caught inadvertently. This is especially interesting due to recent interest in outcome-based auditing.<sup>7</sup>

## 9.4 Community monitoring

Community monitoring is a part of the broader concept of community participation schemes, which Mansuri and Rao (2013) suggested can be looked at as a solution to 'civil society failures' when people who live in geographical proximity to each other are unable to solve collective action problems. In this survey, however, we are more focused on enhancing grassroots participation by enabling the community to monitor, which can sometimes improve upon top-down monitoring in curbing corruption for several reasons. Service or benefit recipients have better information on corruption. They have stronger incentives to watch the service providers in order to avoid any costs generated by corruption. Community monitoring also create higher non-monetary costs (the fear of social disapproval and sanctions,  $S$  in the model) faced by the officials (World Bank 2003; World Bank 2007). We first discuss the political economy literature on community monitoring and corruption (which is still quite limited) before moving on to community monitoring and the effect on performance in service delivery.

Banerjee et al. (2018) studied the impact of providing information to targeted beneficiaries in a redistribution programme in Indonesia. We can view the intervention as changing the transaction costs of corruption,  $\beta X$ : an external agency provides information on entitlements so that users can understand where they are being cheated. In a subsidised rice programme, they tested

the impact of providing information on whether people were eligible and on the amount of the subsidy they were entitled to. Eligible households received a 26 per cent increase in subsidy despite imperfect implementation. Indeed, the more relevant information they were given (such as co-pay amounts), the better off the households were. Moreover, when the list of eligible households was made publicly available, the benefits received increased further. The study highlights the importance of improving the bargaining position of beneficiaries by giving them more information (or reducing  $\beta$ ) and using public sanctions. The main contribution was that here an information-only treatment seemed to work when the public service was easily observed – but such an approach may not work in procurement.

In a similar study by Fiala and Premand (2018), the researchers partnered with the Inspectorate General (IG) of the Ugandan government, an independent arm of the government responsible for fighting corruption, in order to provide training in social accountability and provide information on project performance for a large-scale public development plan. Training covered how to monitor and report mismanagement, while information was also provided on the quality of services across different communities. The study found that the combined effect of both the training and the informational treatment was significantly better than these treatments handled individually. Both types of interventions aimed at increasing the transaction costs of corruption. Moreover, the impact was much higher in the areas which local officials reported as being highly mismanaged or corrupt.

Social audit is a special form of community monitoring that combines elements of top-down audits with elements of community monitoring. Social audits have a long history in India, starting with the MKSS (Mazdoor Kisan Shakti Sangathan), a voluntary organisation in Rajasthan in 1990. Accordingly, we focus first on a few studies based in India, describing the process in some detail to show how it combines elements of information, community participation and top-down audits.

Most studies of social audits focus on Andhra Pradesh (AP), one of the large states in India that has had exceptional performance in conducting social audits. Although the efforts of a majority of states in carrying out social audits have been disappointing, the SSAAT (Society for Social Audit Accountability and Transparency) in Andhra Pradesh successfully institutionalised this process (Aakella and Kidambi 2007; Aiyar and Mehta 2015). This arm of the Department of Rural Development conducted regular social audits of projects in all the districts of AP that formed part of a large employment guarantee programme (whose acronym is MGNREGA) operating in rural areas across India since 2005. The results have shown that, even if they are not specifically focused on corruption, social audits may help to improve performance outcomes.

Singh and Vutukuru (2010) studied the impact of social audits on the size of MGNREGA as well as its payment process. The demand for MGNREGA drives its size, which means that the state has to employ all those who register

for the scheme for up to 100 days. It is a wage employment programme, and the guidelines make it clear that workers should receive their wages every week or not later than 15 days after the completion of work. The authors therefore look at timely payments by comparing the percentage of workers who received their payments within 15 days versus those with overdue payments. They also followed up on the change in these proportions after one round of audit, assuming that the impact would be fully reflected in the next year. Fifty-five treatment *mandals* (a type of local government area) were selected based on the timing of the social audit. These *mandals* had a round of social audit in the latter half of 2006–07 (December 2006 to March 2007). This was followed by the selection of control *mandal* for each treatment *mandal*. The control *mandals* were the ones where a social audit was conducted after September 2007, which meant that they had no social audit in 2006–07. The results showed significant improvement in the person-days generated, a key measure of the size of the programme. However, social audits failed to have an impact on the proportion of timely payments. Singh and Vutukuru (2010) suggested that the high demand for employment from MGNREGA put enormous pressure on the delivery system for payments.

Afridi and Iversen (2014) used a much larger sample to study the impact of social audits on employment generation and complaints of irregularities registered under MGNREGA. They found insignificant effects on corruption, which they attributed to the lack of punishment mechanisms. Since these audits were not randomly done, they relied instead on a strategy of analysing changes in irregularities found by the audit teams over successive rounds of audit for the same subdistrict. The original social audit reports (three rounds) from Andhra Pradesh are used for the years 2006–10. The authors narrowed down the sample to 300 gram panchayats (GPs, or village council areas) in eight districts of AP, and research focused on different types of complaints (related to labour, materials, and provision of worksite facilities) and the programme and employment expenditure under the programme.

The dependent variables were the social audit findings for a particular GP, and NREGA performance measures such as the programme expenditure and employment generation in a GP. The independent variable was an audit variable.<sup>8</sup> The regression estimates suggested an insignificant impact of social audits on both employment generation and the total number of irregularities. However, there was a marginal (not significant) decline in the complaints related to the labour-related irregularity, matched by an increase in the material-related irregularities. Although social audits are useful in detecting irregularities, it is hard to say whether they alone can help deter malpractice in any way. A process that ensures follow-up of social audit findings and punishes the transgressors strictly seems to be needed.

Afridi et al. (2021) then used the same setting to ask whether electoral punishment after social audits exposed corruption was a suitable deterrent to corruption. They used data on village elections in 2006 and data on irregularities over the five-year electoral term of incumbent village heads. Similar to the

findings of Avis, Ferraz, and Finan (2018), they found that electoral punishment was not a sufficient deterrent. As electoral competition increased, the labour-related irregularities went down somewhat but the material-related irregularities were not responsive. Moreover, when elections were very close, even labour-related irregularities went up with competition.

Molina et al. (2016) conducted a meta-analysis of 15 studies (up until 2013) on a large number of community monitoring interventions (CMI) including information campaigns, citizen scorecards, and social audits. They analysed the effects on corruption outcomes as well as service delivery. On average they found positive effects of CMIs in reducing corruption and improving service delivery, but there was a lot of heterogeneity. The interventions that had the biggest impact seemed to be those that aimed at increasing citizen participation and specifically included tools to monitor politicians (for example, Olken 2007). Other reasons for failure included collective action problems – citizens maybe did not participate due to lack of information about their entitlements (Banerjee et al. 2010), free riding incentives, pessimistic beliefs about the social auditors' incentives, the lack of redress mechanisms (Afridi and Iversen 2014), doubts about the response of service providers or about the beliefs of other citizens to participate. All these factors may have depended on the degree of inequality or ethnic fractionalisation (for example, Björkman and Svensson 2010).

Turning to community monitoring, in their the P2P (Power to the People) study, Björkman and Svensson (2009) analysed the impact of a randomised field experiment conducted in all four regions of Uganda on the quantity and quality of health care provision – quantities being measured by daily patient registers, immunisation cards, and so on. They focused on local community-based monitoring of public health care providers. Fifty public dispensaries and the respective users of health care services in nine districts were randomly assigned into the treatment and control group. Each treatment facility and its community had a unique report card, through which information on the quality of services, comparisons with other health facilities, and so on were disseminated based on the surveys in their areas. A style of local NGO (non-governmental organisation) called a community-based organisation (CBO), promoted village and staff meetings. These meetings were crucial in making each community in charge of establishing ways of monitoring their provider, after a series of initial meetings. Thus, these interventions were ways to improve  $q$  (the likelihood of audits) that were determined endogenously. They addressed  $\beta$  via giving better information to the recipients of the service and also helped in training them to solve the collective action problems and create the sense of agency needed to make community monitoring work. The treatment communities became more involved and began monitoring the health unit extensively as a result of the intervention. A year later and significant improvement in the weight of infants, declines in the under-five mortality rate, and higher utilisation of health care services were observed in the treatment groups when compared with the control group. These results imply

that changes in the quality and quantity of health care providers could be attributed to the behavioural changes of the staff.

In contrast to these positive effects of community monitoring on providers' behaviour, in Brazil, Zamboni and Litschig (2018) showed that top-down audits did not improve health service delivery because of the lower punishments involved. Björkman Nyqvist et al. (2017) went back into the field to study the long-run impact of the intervention. They presented evidence to show the long-run benefits of the intervention and also that a *crucial part of the intervention was the provision of information on performance*. Perhaps social sanctions work better as punishment mechanisms in the setting of health provision, where exposure is less of an issue relative to awareness about entitlements.

The results of the 'P2P' research have recently been challenged by a larger study in the same setting of the Ugandan health sector (Raffler and Parkerson 2019). They found only modest positive effects from community monitoring on treatment quality and patient satisfaction over 20 months, but no changes in utilisation rates or the health outcomes used in the P2P study. Moreover, they found that the effects of community monitoring were negligible by themselves but did have a significant impact on change when coupled with top-down oversight. This might also plausibly be explained by very different (and improved) baseline measures.

An interesting study that shows how important it is to interpret outcomes with caution is that by Christensen et al. (2021). They looked at West Africa's response to the Ebola crisis. Based on an experiment they had run two years before the outbreak in Sierra Leone, they looked at two treatments aimed at improving the quality of service delivery by health workers – one was community monitoring and the other was status rewards for health workers. They found that before the outbreak service quality improved from the treatments, and that they also led to higher reporting of cases with lower mortality from Ebola.

Banerjee et al. (2007) and Banerjee et al. (2010) focused on educational services in India, conducting a survey in a rural district in Uttar Pradesh. The authors surveyed village education committee (VEC) members,<sup>9</sup> rural households, parents, and teachers, regarding the educational services and their own participation in the delivery of educational outcomes. Their primary survey showed that 30–40 per cent of students between the ages of six and 14 were unable to do basic arithmetic operations, read simple texts, and write a basic sentence correctly. Furthermore, teachers, parents, and VEC members did not seem to be fully aware of the range of the problem. Parents and VEC members were unaware of the essential roles they played within the academic system. The baseline findings of the survey pointed to a significant gap in knowledge regarding the status of education within the villages.

Banerjee et al. (2010) then studied the impact of public action campaigns on local participation in VECs in Uttar Pradesh (UP), one of the most populous and poorest states in India. They analysed whether information and

participation in VECs improved the learning outcomes of children in the schools. To do this, the authors designed three interventions and assessed their impact on local participation and whether they can improve school functioning. The interventions all target  $\beta$  via information and training in community monitoring, as well as a task that improves the agency and capability of the citizens. A country-level education NGO called Pratham then evaluated and compared the results of the three interventions, designed to enhance community participation. Pratham teams facilitated the village meetings and encouraged discussions as a part of the first intervention. They convinced village administrators to share information about the structure of local service delivery at these meetings. Pratham activists distributed pamphlets post meetings. These pamphlets described the responsibilities of VEC members and the training of individual VEC members.

The second intervention provided the same information as the first one, along with the training of the community members so they could undertake a simple reading test with children. Community members were asked to prepare report cards on the state of enrolment and learning in their village. The village-wide meetings involved the presentation of information from these report cards. The idea of this intervention was to provide citizens with tools to measure learning that could improve participation and effectiveness. The third and final intervention included all elements from the first two but added the recruitment of one or more volunteers per village. They were given a week's training in a pedagogical technique for teaching necessary reading skills developed and implemented by Pratham. The trained volunteers were then responsible for holding reading camps in the villages, with classes daily outside school for two months. This intervention allowed individuals to try and improve learning among children directly.

An evaluation of surveys conducted post interventions showed that none of the three methods led to a significant increase in the involvement by any of the players (the parents, the VEC, or the teacher). They also failed to improve school performance (measured by the attendance of children and teachers' or community participation in schools). It is hard to say why, because the mobilisation did not entirely fail. Almost everybody in the villages turned up for the meetings planned by Pratham. Moreover, the third intervention led to a massive volunteer mobilisation, followed by a great response by the parents outside the school system. The results from the third intervention showed that teaching children how to read is not an impossibly difficult task. In the context of UP, these results imply that providing information on the status of education and the existing institutions of participation alone was not sufficient to promote beneficiary involvement in public schools.

On a positive note, though, the results suggest that information combined with the offer of a right course of action can result in collective action and improve outcomes. There was a greater willingness of individuals to help improve the situation (via volunteer teaching) rather than undertake collective action to reform institutions and systems. The authors suggested

that this could be explained by pessimism on the part of community members about being able to influence outcomes. In the one part of the intervention that did not require official functionaries, however, the researchers found positive results.

Following on from this study, Pandey, Goyal, and Sundararaman (2009) ran a community-based information campaign on health and school performance in the form of a cluster randomised control trial (RCT) in 610 villages across three Indian states. They found notable positive impacts on teacher effort, and delivery of entitlements, with less effect on educational outcomes. They reached similar conclusions as Banerjee et al. (2010) about the delivery of final learning outcomes, which is a puzzle since teacher effort went up and one year later they find an increased demand for services in UP.

By contrast, in Andrabi, Das, and Khwaja (2017), providing information via report cards on children's test scores relative to a mean test score across the village had positive effects on parental awareness about private school quality. It also led to positive outcomes on learning as well as reduced prices for private school fees. The research suggested that *comparative* information on children's test scores within and across schools was useful in encouraging participation and accountability. Afridi et al. (2020) conducted a randomised report card campaign where contiguous village councils in the Indian state of Rajasthan were randomly assigned to either a control group or to one of four treatment groups in which student report cards on curriculum-based tests were provided to schools, to parents, or both. They found no changes in academic performance in public schools, but student performance in private schools improved by one-third of a standard deviation when parents and schools could simultaneously place themselves in the distribution of scores in the neighbouring villages. There was no systematic change in performance for any treatment that involved only schools, or where households were not informed about the relative performance of all schools in the community. They reconciled the divergent findings of Banerjee et al. (2010) and Andrabi, Das, and Khwaja (2017), by suggesting that the design of information campaigns – that is, ensuring common knowledge of relative (rather than absolute) school quality – and provider incentives can both play a critical role in improving learning outcomes. Overall, these results suggest that, when providing information to the recipients of public services, their being able to benchmark performance is key. Again, these types of intervention raise the transaction costs  $\beta$  in officials engaging in corruption or equivalently not putting in full effort.

The next two papers were aimed at improving the beliefs and agency of the community monitors themselves, which can impact collective action. In Ugandan schools, Barr et al. (2012) ran a field experiment to tease out the reasons why scorecards or informational interventions had succeeded in some cases but not in others. They found that treatments encouraging the community to develop their own goals and objectives/plans on monitoring (designing the score cards) were more likely to succeed because they encouraged

cooperation (tested using a public goods contribution game) and therefore improved collective action. They concluded that small changes in the design of participatory interventions can have large effects.

Pradhan et al. (2014) undertook an innovative RCT to improve educational outcomes in Indonesia by instituting elections for the school committee members and facilitating deliberations between committee members and the village council (linkage) – all this in addition to the traditional ways of improving community participation such as grants and training. Linkage with the village elected council body meant that the school committee had a greater bargaining power in effecting change. A second treatment was on electing the members in the committee – if instead the school could choose its own members then their monitoring might not be very effective. Thus, in terms of our framework, these interventions aimed at changing the collective action payoffs by (i) changing the composition of the committee and (ii) increasing the probability that action would be taken.

Effects from how the community monitoring teams are composed were studied by Björkman and Svensson (2010). They built on the P2P study to show that ethnically fractionalised communities did much worse than homogenous communities in generating participation and monitoring. This suggests that collective action is affected by the fractionalisation of the team: higher free riding or coordination problems being higher could explain these results. Björkman Nyqvist et al. (2017) conducted a survey of the villages four years and an information treatment and found persistent effects. They concluded that a necessary condition for building participatory community monitoring is to provide both information and the tools to use information to monitor providers (the traditional tools to encourage community participation).

How much do citizens actually participate in social accountability? Despite the hype, participative democracy and community engagement agendas may not be very useful if citizens are not empowered enough to make their views known. There may be many reasons for this. For instance, the bureaucrats responsible for encouraging local accountability are the often the same officials who have the least incentives to do so. Large inequalities in status between different participants can be another constraint. This question was studied by Parthasarthy, Rao, and Palaniswamy (2019) using natural language processing techniques on a corpus of village assembly transcripts from rural India. They found that women were significantly less likely to speak in these meetings, and when they did speak they were much less likely to be heard. When women are local leaders, however, this dynamic changes.

To summarise, the literature has studied two types of interventions in community monitoring: (i) Information treatments where relevant information on public services such as entitlements, score cards on performance, and so on are provided and (ii) training on how to monitor effectively. The results have largely confirmed the problems due to a lack of empowerment, and elite capture, as well as difficulties from free riding. The exact design of the intervention is important. There are few studies that examine how to break the

constraints imposed by political economy – that is, issues of elite capture are still first-order.

## 9.5 Studies comparing top-down versus social audits

The first paper in this section made a clear comparison between the two techniques and compared their final impact on corruption levels in Indonesia. The intervention involved aimed to increase  $q$  (the frequency of audits) in top-down audits and to increase  $\beta$  in community monitoring. Olken (2007) conducted randomised field experiments for both top-down and bottom-up monitoring in 600 villages for a year (from September 2003 to September 2004). The timing of the experiment matched the nationwide village-level infrastructure project (construction of roads) in Indonesia, which allows randomly selected villages to undertake projects and who were subsequently audited by the central government audit agency. The probability of audit for treated villages thus went up from 4 per cent to 100 per cent. The villages were informed regarding the audit treatment only when they received the funding for construction and before it began, so that the project funding and design remained exogenous to the experiment.

The first part of the experiment captured the impact of top-down or external monitoring. It produced a significant reduction in missing expenditures. However, the evidence suggested that it was more the threat of audit rather than the audit itself that had the effect, since the audit reports conducted were mainly on procedural issues rather than addressing corruption *per se*. This is in contrast to the results discussed earlier (such as Bobonis, Cámara Fuentes, and Schwabe (2016) or Avis, Ferraz, and Finan (2018)), where past audits had an effect rather than the threat of audits.

The second part of the experiment was subdivided into two smaller experiments, designed to increase grassroots participation in the monitoring process and analyse the overall impact on corruption. The first subpart of the second experiment aimed to encourage direct participation by sending out invitations to village-level accountability meetings where the project officers would explain and account for how they spent the project funds. The second part added anonymous comment forms, which were distributed alongside the invitations to meetings;<sup>10</sup> the idea here was to allow villagers to convey information about the project without any fear. Of course, Olken noted that, if numbers in a village were small, anonymity might not be perfect even with such forms. Two different forms of distribution of forms were used – one where the village government distributed forms and one where the schools did so.

The second experiment succeeded in gearing up community participation in the monitoring process. However, the change in the attitudes of people in the treatment villages did not translate into a reduction in missing expenditures. What was striking, however, was that treatment areas saw a reduction in

missing labour expenditures, but no effect on missing materials expenditures. Olken (2007) speculates that distribution via schools bypasses the local government and also keeps a check on the village elites to ensure that they do not direct the comment forms to their supporters. Thus, for bottom-up monitoring to lower corruption it is important to prevent elite capture and problems of free riding in monitoring.

Another paper by Serra (2011) captures the potential effectiveness of a combined accountability system (because bottom-up monitoring can trigger top-down auditing). A lab experiment about bribery was used to capture the strategic interaction between private citizens and public officials for the provision of a public good under different anti-corruption systems: one with no monitoring but just top-down auditing (external controls in the form of a fine applied with a low probability) and the other using combined monitoring (where citizens could report corrupt officials that would lead to top-down auditing). The game was played by a total of 180 Oxford University students who randomly took up the roles of private citizens and public officials in groups of 15 and filled in a questionnaire after participating in the experiment. In the set-up public officials had a choice of demanding or not demanding a bribe from the private citizens, and, if they chose to demand it, they could also choose to demand any bribe amount they wished. The private citizens also had the option of deciding whether and how much to pay as a bribe. The payoffs generated by a bribe made the briber and the bribee better off but made others in the society worse off. The analysis of data collected from the questionnaires showed that combined monitoring reduced bribe-demanding behaviour by the public officials in the game, which ultimately lowered corruption. In contrast, top-down auditing alone did not significantly reduce the officials' tendency to demand bribes. Serra's suggested explanations include the extra risk of social disapprobation with the bottom-up method, the risk of betrayal by the bribe giver, and erroneously ascribing higher probabilities to being caught. This preliminary evaluation of different policies suggests that a system in which bottom-up monitoring triggers top-down auditing (with some probability) could be efficient in curbing corruption.

## Conclusions

Our survey of the literature has presented some of the seminal recent research on monitoring both by third-party audits (top-down) and community monitoring. The main findings from the literature can be summarised as follows. Top-down audits causally reduced corruption in Brazil via electoral accountability – incumbents who were corrupt were less likely to be re-elected and this motivated them to reduce corruption. Past audits reduced corruption not only when the electoral punishment is higher but more so when judicial punishment was used. However, in Puerto Rico, anticipated audits did not significantly reduce corruption – audits seemed to lead to a selection of more competent politicians rather than more honest politicians.

The type of public service also matters. Decision makers in procurement respond more to audits due to the presence of clear rules and regulations in procurement, greater discretion in the use of funds, as well as higher punishment for violators. In contrast, in services where punishment is lower and the financial aspects of irregularities are lower (absenteeism rather than outright theft), audits had less of an effect on corruption. Audits might also have negative effects when they lead to demotivated bureaucrats. In terms of our theoretical framework, the literature has focused mainly on responsiveness to changes in  $q$ , and in punishment – both the chances of being punished  $f_k$  and the size of punishment  $F$ .

The findings on community monitoring are less clear. In India, social audits had positive effects in reducing some types of irregularities, where stakeholders were directly affected. But in terms of performance outcomes the jury is still out. In terms of more broadly defined community participation, the literature has focused mainly on two types of interventions – informational-only treatments and information accompanied by tools to encourage participation. Results suggest that collective action problems are pervasive. Agents who are not empowered did not engage unless they had some control on the final decisions. More fractionalised groups had more severe problems and, even within groups, women were less likely to speak up. Elite capture and intimidation by elites is another important problem. As in top-down audits, successful community monitoring may also lead to worse outcomes when there is too much interference in the working of the service providers. In understanding the heterogeneous effects of community monitoring, policy design seems important – less-complex tasks may respond better to community monitoring. This insight carries over to combinations of top-down and social audits. In terms of our framework, the literature has focused on changes in  $q$  (collective action-changing composition, providing incentives, and information on stakes involved), as well as the transaction costs of corruption or low effort  $\beta$ .

A thoughtful review by Hollyer (2012) covers the shortfalls of experimental and quasi-experimental literature on both top-down and bottom-up monitoring. Both top-down and bottom-up anti-corruption interventions have been successful in some instances and failed at some. The author points out that in fact, despite causality being established credibly in many interventions, the causal chains involved are quite long and so it is hard to identify why the intervention worked in some conditions but not in others. Variation in formal and informal institutions may affect the effectiveness of interventions, especially the composition of institutions that regulate the relationship between politicians, bureaucrats, and citizens. The competitiveness of political contestation, the nature of civil service rules, and the independence of oversight institutions are some of the crucial factors. The efficiency of the attempts to combat corruption is contingent on factors that tend to vary across countries, regions, municipalities, and villages. Hollyer (2012) suggested two ways of overcoming the problems faced in measuring the nature of the impact of anti-corruption interventions. One is a design-based approach – theory can

be used to supervise experimental designs and case selection to allow for stronger tests of the effects of the treatments. A second mitigating approach involves meta-analysis or aggregation across multiple studies.

Some open questions remain for further research. First, in both top-down and social audits, the gap between finding irregularities/corruption and finding improvements in outcomes (the ultimate goals of audit) is considerable. Not only did studies fail to find effects on outcome variables; sometimes they found them in the wrong direction. Understanding the conditions and processes for which top-down audits work well and those under which social audits work well seems a first-order question. This is especially important as many supreme audit authorities are moving towards a system of outcome-oriented audits. Second, the literature on the efficacy of audits in relation to punishments is sparse, yet this may also be related to what types of processes are more suitable for top-down audits. For community monitoring there is a much greater wealth of information on outcomes, while in top-down audits there are only limited studies, most of which are focused on South America, with the heavy representation of Brazil. This may be due to the fact that it is much easier to get NGOs than supreme audit institutions to collaborate on research. The most promising lines of enquiry here seem to be on the composition and design of audit teams when the aim is to get representation, voice, and impact. As with top-down audits, here too developing an outcome orientation is important.

Some main policy prescriptions also emerge from our survey. Monitoring ultimately aims to improve outcomes and not just lower corruption, though that is an important mediating link. Since top-down audits are costly, it is worthwhile knowing whether they improve citizens' wellbeing. For instance, if audits reduce officials' or policymakers' motivation to take risks with potentially useful but difficult projects, or, if they are difficult to press charges on, citizens' welfare may not be improved. In addition, while activities like procurement lend themselves to audits naturally, the same cannot be said of other public service delivery where community monitoring is more suitable. So top-down audits should be used only in contexts similar to procurement, where the documentation to take legal action exists.

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## Endnotes

<sup>1</sup> <https://www.imf.org/en/Publications/Policy-Papers/Issues/2018/04/20/pp030918-review-of-1997-guidance-note-on-governance>

- <sup>2</sup> Updated in Mookherjee and Bardhan (2005).
- <sup>3</sup> CGU office in Brazil, Brazilian federal accountability office, public prosecutors and municipality legislative branch.
- <sup>4</sup> Avis, Ferraz, and Finan (2018) measured corruption as the log of number of irregularities classified as either moderate or severe in audit reports, mismanagement as the number of irregularities registered under administrative and procedural issues, and as an indicator variable whether legal action had ever been taken against a mayor in a municipality. Irregularities associated with mismanagement remain unaffected by the history of audits in a municipality. Interestingly, they do not find much evidence of selection effects: if increased information about elected leaders causes voters to throw out corrupt incumbents, then over time the fraction of corrupt leaders should go down in audited municipalities, but they do not find this.
- <sup>5</sup> Mayors in Brazil face a two-term limit, wherein a mayor who gets elected for the first time serves their first term and becomes eligible to stand for re-election. If re-elected, the mayor who is in his/her second term faces a term limit of standing for further elections.
- <sup>6</sup> The model by Allingham and Sandmo (the AS model) focuses on studying the choice of a taxpayer who trades off the benefits from tax evasion and the risky costs and fines from detection.
- <sup>7</sup> See for example, <https://timesofindia.indiatimes.com/india/cag-to-introduce-outcome-based-audits/articleshow/64482292.cms>
- <sup>8</sup> The first round of audit is considered as a reference point.
- <sup>9</sup> The VECs exist in every village in Uttar Pradesh. They consist of the elected head of village government, the headteacher of the government school, and three parents of students enrolled in government schools in the village.
- <sup>10</sup> The comment forms were filled by the villagers and submitted before the village meetings. They were summarised and read out during the meeting.

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