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Chapter 8 Using Field Experiments to Understand Information as an Antidote to Corruption

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CHAPTER 8

USING FIELD EXPERIMENTS TO UNDERSTAND INFORMATION AS AN ANTIDOTE TO CORRUPTION

Matthew S. Winters, Paul Testa and
Mark M. Fredrickson

ABSTRACT

In observational data, access to information is associated with lower levels of corruption. This chapter reviews a small but growing body of work that uses field experiments to explore the mechanisms behind this relationship. We present a typology for understanding this research based on the type of corruption being addressed (political vs. bureaucratic), the mechanism for accountability (retrospective vs. prospective), and the nature of the information provided (factual vs. prescriptive). We describe some of the tradeoffs involved in design decisions for such experiments and suggest directions for future research.

INTRODUCTION

When information is scarce, corruption thrives. This claim is consistent with the logic of principal–agent theory and finds empirical support in

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a variety of settings. Political agents always possess information that their principals do not, which creates opportunities for corruption. Environments and institutions that ameliorate this asymmetry should therefore reduce corruption. Cross-national data show that a free press (Brunnetti & Weder, 2003; Freille, Haque, & Kneller, 2007; Lessmann & Markwardt, 2010; Stapenhurst, 2000), an educated populace (Ahrend, 2002), and the existence of public disclosure laws (Djankov, La Porta, Lopez-de-Silanes, & Shleifer, 2010) are all associated with lower levels of corruption. Individual-level data similarly reveal that information about bureaucratic procedures reduces the likelihood of having to engage in corrupt transactions in order to obtain goods and services from the government (Deininger & Mpuga, 2005).

Illustrating that a link exists between corruption and the availability of information, however, does not show the mechanisms through which increases in information reduce corruption. In fact, if both reduced corruption and increased information result from some other characteristic of societies, such as economic development or openness to trade, then the observed relationship between information and corruption may be spurious. A recent and growing body of literature addresses these concerns through the use of field experiments that explore the causal relationships between higher levels of information and lower levels of corruption. Understanding these processes in more depth is an important step in the fight against corruption, since identifying the mechanisms connecting increased information and reduced corruption allows for the development of a more effective anticorruption toolkit. With an eye toward this important policy goal, this review presents a typology to organize the work on information and corruption, describes design issues facing such experiments, and then suggests avenues for further research and real world policy interventions.

We also highlight three important results emerging from this young experimental literature. First, there is some evidence that information about political corruption results in depressed turnout. We raise the question of what this implies for long-run accountability relationships. Second, the evidence suggests that there is little benefit to information campaigns that urge voters to oppose corruption. Finally, we see that other types of broad informational campaigns might reduce inappropriate or illegal practices, but the studies so far have not been able to determine if these campaigns directly change political agents' behavior or if rather they first change citizens' behavior, which then elicits a change in politicians' actions.

INFORMATION AND CORRUPTION: THEORETICAL AND CONCEPTUAL ISSUES

Corruption is commonly defined as the abuse of public office for private gain (Nye, 1967; Rose-Ackerman, 1978, 1999).¹ It is useful to further distinguish between acts of political corruption (those committed by elected officials) and acts of bureaucratic corruption (those committed by nonelected public officials or civil servants). While the two are related (e.g., elected officials may require payments from their appointed subordinates, which those bureaucrats in turn obtain through the collection of bribes from citizens), we believe that there are important conceptual distinctions between the two that correspond to distinctions in the way that information provision can be used in addressing each.

Political and bureaucratic corruption differ in the types of agents involved, the acts of corruption committed, the likelihood of corruption being discovered and punished, the way in which punishment is enacted, and the consequences of punishment. That said, both types of corruption can be understood as violations of the expectations of delegation within a principal–agent relationship and so fit into the same basic framework.

Principal–agent theory begins from the premise that there are tasks to be delegated by one actor (i.e., the principal) to another (i.e., the agent). The extent to which the agent will fulfill the tasks delegated by the principal is assumed to be problematic. That is, unless preferences are perfectly aligned between principals and agents, agents will have incentives either to not perform tasks or to perform tasks in a fashion that is not completely in accord with what the principal desires. Principals must therefore establish monitoring and punishment mechanisms in order to create an incentive structure under which the agent will more faithfully fulfill the delegated task according to the principal's desire (Besley, 2006, Chap. 3).

Corruption is a violation of the delegation relationship. If citizens elect a politician, the authority to make governing decisions is delegated to that politician on the assumption that the politician will make decisions that are in the citizenry's best interest. Insofar as the politician makes decisions that are in her best interest but not in the best interest of the citizens (e.g., taking bribes when choosing a company with which the government will do business), this violates the intention of the principal (Gambetta, 2002). Similarly, if a politician delegates authority to a bureaucrat on the assumption that the bureaucrat will run a government service in the best interests of the community and that bureaucrat instead takes advantage of

her position to extract bribes from citizens seeking services, this is a violation of the principal's intent.

In both cases, corruption is a result of asymmetric information. Agents (either elected or bureaucratic officials) possess information that their principals (either voters or the general public) do not, and it is this lack of information that creates the opportunity for corruption to occur. Agents weigh the benefits that can be gained from corruption against the expected costs of getting caught, which are a function of the availability of information and the likelihood of punishment (Becker & Stigler, 1974; Persson & Tabellini, 2000). Holding the punishment mechanism constant, when information is scarce, the probability that a corrupt act will be discovered is low, and so the expected cost of corruption is low, resulting in more corruption. Similarly, holding information constant, when the punishment is weak, the expected cost of corruption is also low, resulting in more corruption. If there is either little information that might reveal corruption or else little punishment when corruption is exposed, then agents will act with impunity.

The information referred to so far has to do with the actions being taken by the agent: is the agent performing her duties according to the principal's preferences or not? But the likelihood of the punishment mechanism being used is itself also a function of the information available to principals. That is, we must ask if the relevant principals are aware of the procedures through which a poorly performing agent can be punished. Specifically, do citizens think of removing politicians from office as an appropriate mechanism for addressing political or bureaucratic corruption? And are citizens aware of ways in which they can report poorly performing bureaucrats in order to address bureaucratic corruption?

Therefore, we emphasize here that principals can become better informed on one of two dimensions: either they can gain more information about the way in which agents are performing, or else they can gain more information about how to respond to poor agent performance. The first type of information is information that principals can act on, while the second type of information is information about the actions that principals can or should take.

Insofar as increased information of either type reduces corruption, this might occur because of retrospective or prospective mechanisms. For political corruption, reduced corruption comes from a retrospective mechanism when voters opt to select less corrupt politicians and from a prospective mechanism when politicians, fearing not being (re)elected, opt to avoid corruption. For bureaucratic corruption, reduced corruption comes

from a retrospective mechanism when higher-level officials or politicians punish corrupt bureaucrats and from a prospective mechanism when bureaucrats, aware of the recourse that principals have, opt to avoid corruption.

In this sense, then, it is not just the receipt of new information by principals that matters for reducing corruption, but it is also the impressions of agents that principals either do or could have access to this information. If principals obtain new information and act on it, then corruption will be reduced through retrospective mechanisms. If agents preempt their own corrupt behaviors on the grounds that principals might obtain information about corruption, then prospective mechanisms are responsible for reducing corruption.

Field experiments are a powerful tool for understanding the mechanisms underlying the relationship between information and corruption (Humphreys & Weinstein, 2009; Shadish & Cook, 2009), and experimental methods have seen increasing use throughout economics and political science (Druckman, Green, Kuklinski, & Lupia, 2006). As this volume demonstrates, corruption is an active topic of experimental inquiry both in the lab and in the field. Manipulations designed to increase access to and the quality of information have the potential to be fruitful in terms of explaining the best anticorruption interventions. As we have noted, information asymmetries are at the core of principal–agent dilemmas. Manipulating how citizens, politicians, and bureaucrats access information is critical to giving principals the necessary tools to control agents. Additionally, informational treatments often are relatively inexpensive manipulations. Several studies that we cover in this review simply increase access to already existing information; the corruption-decreasing results suggest inexpensive, effective policy recommendations.²

Based on distinguishing between political and bureaucratic corruption, information about behavior and information about recourse, and prospective and retrospective mechanisms, Table 1 shows the eight categories of mechanisms that result for thinking about how information might reduce corruption.

So far, the field experimental literature on information and corruption has largely grouped into two categories: those studying political corruption have examined how the provision of information about behavior leads to retrospective voting decisions by citizens, while those studying bureaucratic corruption have examined how the provision of information about recourse leads to prospective corruption decisions by bureaucrats. There also are studies of the effects of informational campaigns on appropriate voting behavior that examine both retrospective and prospective mechanisms for

Table 1. Pathways by Which Improved Information Might Reduce Corruption.

	Political Corruption		Bureaucratic Corruption	
	Information about behavior	Information about recourse	Information about behavior	Information about recourse
Retrospective mechanism	Voters vote corrupt politicians out of office (Banerjee, Kumar, Pande, & Su, 2011; Chong et al., 2010; de Figueiredo et al., 2011)	Voters choose to vote corrupt politicians out of office (Banerjee, Green, Green, & Pande, 2010)	Corrupt bureaucrats are removed from their positions	Citizens report corrupt bureaucrats, leading to their removal from their positions
Prospective mechanism	Politicians reduce corruption because of (re)election concerns	Politicians reduce corruption because of (re)election concerns (Banerjee et al., 2011; Vicente, 2010)	Bureaucrats reduce corruption because of fears of being removed from their positions (Olken, 2007)	Bureaucrats reduce corruption because of fears of being removed from their positions (Olken, 2007; Peisakhin, forthcoming; Peisakhin & Pinto, 2010)

reducing corruption and one study that might be seen as providing information about behavior in the context of bureaucratic corruption. To the best of our knowledge, there are no studies that investigate the other three possible mechanisms, something that we address when discussing avenues for future research.

The rest of the chapter is organized as follows. The third section outlines the review of the studies. The fourth section examines experiments related to political corruption, while the fifth section engages with those related to bureaucratic corruption. The sixth section describes a variety of challenges involved in designing field experiments related to corruption. The seventh section suggests directions for future research.

USING FIELD EXPERIMENTS TO STUDY INFORMATION AND CORRUPTION

Field experiments involving information and corruption can be defined by three basic questions. First, does the experimental intervention respond to political or bureaucratic corruption? Second, what information is conveyed in the intervention? Third, how is the information intended to affect corruption? In theory, these are all potential themes for experimental manipulation. In practice, pragmatic issues of resources, relevance, and measurement often determine the orientation of experiments.

For example, the electorate is a more accessible population for treatment than elected officials. As principals, they are more likely to be lacking in information and thus more likely to respond to a specific information intervention. On the other hand, to examine prospective mechanisms, the targets of the intervention should be the agents (although it may be the case that the intervention works by supplying information to the principals and then looking for behavioral changes among the agents).

In fact, for most of the studies we review, there is an important caveat: measuring responses to information is not the same as determining the final effects of information on corruption. Often the behaviors which are easiest to observe, such as voting or turnout behavior, offer only indirect insight into the overall effects of a particular intervention on corruption. Because corruption is difficult to observe and measure, the long-term effects of an intervention are often assumed in studies rather than demonstrated.³

We begin by reviewing work that looks at how voters respond to specific information about political corruption and then look at evidence on how voters react to more general anticorruption campaigns. Then we consider

studies addressing how political agents respond prospectively to anti-corruption campaigns addressed at their voting principals. Finally, we look at research that examines how information about accountability and monitoring might prospectively reduce bureaucratic corruption.

INFORMATION AS AN ANTIDOTE TO POLITICAL CORRUPTION

Retrospective Reactions by Voters to Behavioral Information

A key question in political science is whether voters will use information about corruption to hold their elected officials accountable (Besley & Pratt, 2006; Persson & Tabellini, 2000; Winters & Weitz-Shapiro, 2011). At a micro-level, there is evidence that voters, in fact, will hold corrupt politicians accountable (Ferraz & Finan, 2008), although it may be the case that the media needs to emphasize the salience of corruption charges in order for voters to act on this information (Chang, Golden, & Hill, 2010). There is evidence that unaddressed corruption reduces citizens' confidence in their government and even in democracy itself (Canache & Allison, 2005; Gingerich, 2009; Seligson, 2002).

Field experiments pursuing the question of how voters react to information about political corruption build off a framework provided by research in the "get out the vote" literature (e.g., Gerber & Green, 2000; Gerber, Green, & Larimer, 2008). Get out the vote studies look at the effect of information on turnout, while corruption field experiments examine the way in which information influences both voters' turnout decision and their vote choice. The underlying assumption is that vote decisions motivated by information about corruption will improve accountability and ultimately reduce corruption.

In a model example of this type of research, de Figueiredo, Hidalgo, and Kasahara (2011) distributed information about corruption charges against both candidates in a mayoral election in Brazil. The authors distributed 187,177 fliers to households in the vicinity of 200 voting locations. Half of the households received fliers detailing the charges against a candidate from a center-right party, accused of using public funds to take out political ads in newspapers. The other half received fliers about a left-party candidate accused of giving a no-bid contract to a nongovernmental organization (NGO) that she founded. A control group of 200 polling locations received no flier.

For one of the two treatments, the authors show the retrospective impact of corruption information on voter behavior. While the flier for the center-right candidate had no effect on vote choice or turnout, the treatment for the left-party candidate resulted in a negative vote shift of 2.6 percentage points relative to the control group and caused a 1.2 percentage point drop in turnout. (The turnout result is particularly notable in this case, given that Brazil has mandatory voting.) For one of the two candidates, increased information about the past corruption history of that candidate resulted in significant changes to voter behavior of the type that we would expect when voters want to hold corrupt politicians accountable.

The authors follow up their field experiment with a survey experiment assessing the effects of the fliers on candidate evaluation and vote choice. Again they find the effects are stronger and more significant for the flier of the left-party candidate. They suggest a variety of potential causes for the heterogeneity in treatment effects, including the differences in the nature of the corruption charges facing each politician, the plausibility of the allegations, the distribution of core and swing voters in each party, and candidate-specific characteristics (in particular, gender) that influence how voters respond to corruption.

Chong, DeLao, Karlan, and Wantchekon (2010) report preliminary findings from a field experiment in Mexico that suggest a more complicated link between corruption, information, and accountability. The authors administer three treatments. The first group received information from federal audits on overall spending patterns in a municipality. The second group received this information as well as information on the percent of spending directed toward the poor. The third group received information on the percent of spending that was labeled in federal audits as unauthorized or irregular – this was presumed by the authors to convey information about corruption in the incumbent government. The authors, taking into account the level of corruption in a district, find that the corruption treatment lowered turnout by 6 percentage points and that this decrease in participation led to a decline in the incumbent party's vote share. While the information is specific to a voter's municipality, because of the single-term limits of Mexico's mayors, voters are being asked to make generalizations about party corruption based on the performance of an outgoing incumbent. The significant treatment effect therefore indicates that corruption allegations, in fact, will taint the image of those associated with the incumbent politician.

Like de Figueiredo et al. (2011), the Chong et al. (2010) result is also notable for the fact that it shows how increased information about

corruption can dampen turnout. As the authors point out, significant previous literature has assumed that increased information will result in increased electoral participation – that citizens will be inspired to act on information, turning out and voting corrupt or poorly performing politicians out of office. However, both of these experiments show that information about corruption can actually depress turnout, which may ultimately suggest less government accountability. The long-run impact of these increases in information corruption needs to be studied for this reason.

In a field experiment that involves government performance generally without a direct corruption treatment, [Banerjee et al. \(2011\)](#) provide residents in a random sample of slums in India's capital of Delhi with information on their local official's legislative performance and the incumbent's qualifications relative to his or her two main challengers. As outcome variables, the study looks at turnout and voting patterns. (As discussed below, the study also examines the prevalence of vote-buying.)

The information intervention in this study was multifaceted. First, households in treatment slums received a door-to-door campaign informing them of the responsibilities of legislators. (This first element of the campaign treats voters with information about what is normatively right.) Then, each household in the treatment areas received a newspaper containing a report card with information on the incumbent's performance in the legislature (measured by legislative attendance and participation and the distribution of allotted development funds), the performance of an incumbent in a neighboring district and information on the qualifications of the incumbent and the incumbent's two main challengers. Finally, there were also public readings of the newspaper to assure that the information reached as many citizens in the treatment areas as possible.

Results were measured using electoral returns, observational data from polling districts, and surveys of households in treatment and control slums after the election but before the results were announced. In this experiment where the treatment information was about general government performance, average turnout was 2 percentage points higher in treatment versus control slums. Furthermore, voters appear to be sophisticated in their evaluations of the information. For the best-performing incumbent, the treatment increased their vote share by 7 percent. Voters responded to candidate qualifications relative to the other competitors. For example, incumbent vote share increased with the number of noncollege-educated challengers.

While the findings from this intervention are clear, it has less to say about the relationship between corruption information and retrospective voter

actions. The treatment lacked clear-cut information about corruption that would provide voters with a basis for holding officials accountable on the performance criterion that concerns us most here. The authors found only weak results for the effects of candidate criminality (something on which information was available). If the treatment information described the incumbent as having been charged with a crime, the magnitude of the treatment's positive effect on turnout was reduced. But the overall effect of the treatment on turnout was still positive, and there was no effect on the incumbent's vote share in the cases where the treatment information included a description of the incumbent's possible criminality.

These three field experiments all set out to test how voters retrospectively react to information about corruption (or criminality). In all three cases, there is some evidence that information about corruption appears to reduce voter turnout. In the Mexico study, this is true even though the incumbent politician cannot run for reelection. In the Brazil study, this is observed in one of two treatments. In the India study, overall turnout is not reduced, but the size of the increase in turnout due to the information treatment is attenuated when the incumbent is accused of criminal wrongdoing.

This reduction in turnout may very well call into question the extent to which information about corruption leads to increased accountability. If corruption information is inspiring voters to stay home rather than to go to the polls and vote against corrupt candidates, then a key part of the accountability equation is missing. On the other hand, in both the Brazil and Mexico studies, incumbent vote share drops under the treatment condition. [Chong et al. \(2010\)](#) explicitly link this drop in vote share to the decrease in turnout that they observe. This indicates that voters, in fact, may be holding these corrupt politicians accountable but that some citizens implement the accountability relationship by passively refusing to vote for the corrupt incumbent rather than by actively voting against him or her.

Retrospective Reaction by Voters to Information about Recourse

If the studies just reviewed take as their starting point the idea that accountability relationships might fail because voters lack information about the prevalence of corruption, an alternative explanation for accountability failures focuses on the salience of corruption in the minds of voters when they make electoral decisions. Whereas the first set of campaign experiments gives voters information about politician corruption

and examines how they react to it, anticorruption campaign experiments tell citizens that they should vote against corruption and then see whether or not they in fact do vote against more corrupt politicians. Rather than assuming that voters will reject corruption and then testing to see if they follow through when they learn about corrupt candidates, here voters are told that they should reject corruption or should use their voting power to hold corrupt candidates accountable, and then the experimenters test to see if this encourages voting against corrupt candidates. This type of intervention therefore is about changing voters' decision-making processes or the salience of a particular issue, as compared to assuming that voters have anti-corruption preferences and simply need information on which to act.

Banerjee et al. (2010) use this approach in a randomized field experiment conducted in rural India. Noting that voting in India often occurs along ethnic and caste lines, the authors construct two treatments administered to villages in the northern Indian state of Uttar Pradesh. Both treatments encouraged voters to base their voting decisions on development issues. The caste treatment encouraged voters to vote on development issues rather than along caste lines. The corruption treatment encouraged voters to vote for "clean politicians that care about your development needs."

As with Banerjee et al. (2011), the treatment campaign was multifaceted. The experimenters placed posters around the treatment villages. They then convened a series of meetings, some of which specifically targeted groups like the elderly or women, to allow the treatment villages to discuss issues of importance to them. Finally, puppet shows in the evening delivered the core campaign message, either juxtaposing caste-oriented and development-oriented candidates as characters or else a corrupt candidate and a development-oriented candidate.

The contrast between the caste and corruption treatments was stark. The caste treatment increased turnout by 7.69 percent and decreased voting for the caste-preferred party by 9 percent among lower caste members. The decrease in vote share for the caste treatment was concentrated in races where a candidate was charged with a heinous crime (an offense associated with a jail term of over five years). On the other hand, the corruption treatment produced no significant results.

The authors hypothesize that voters were unsure about how to respond to the anticorruption campaign. Whereas the caste campaign explicitly undercut a dominant decision heuristic (i.e., vote for the party associated with one's own caste), the corruption campaign did not provide such explicit guidance about how to act.

Figuring out when normative campaigns will or will not be successful should be a focus of additional research. The lack of results in [Banerjee et al. \(2010\)](#) may point toward the importance of providing actionable, particularistic information ([Winters & Weitz-Shapiro, 2011](#)) rather than simply providing general information about the ills of corruption. In this regard, the null results from rural India may simply confirm the underlying assumption of the studies described above that voters already hold anticorruption preferences. Insofar as this is true, it is unnecessary to tell voters that they should vote against corruption.

*Prospective Reactions by Politicians to
Information about Recourse*

The provision of information can affect the behavior of both principals and agents. The act of informing voters about what practices are corrupt potentially can make politicians less likely to use those practices. Prospective changes in the behavior of agents are difficult to measure, yet existing research offers some insights.

In their experiment from urban slums in India, where the slums were treated with information about the voting process, legislator responsibilities, and candidate characteristics, [Banerjee et al. \(2011\)](#) found that the incidence of vote-buying decreased by 19 percentage points in the treatment areas. (They stationed observers at both treatment and control polling stations to look for visible evidence of parties distributing cash, liquor, food, clothes, or refreshments to voters.)

This very large negative effect on vote-buying might be the result of changes either in party operative behavior or else voter behavior. The authors admit that it is difficult to assess whether their informational campaign about appropriate election procedures made voters less willing to sell their votes or rather made party workers less willing to distribute bribes in the areas where the intervention took place. So ultimately the result may come either from changes in politician behavior as a result of information or instead from changes in politician behavior as a result of changes in voter behavior, itself a result of the information intervention. (Or the treatment effect might result from both mechanisms.)

[Vicente \(2010\)](#) examines the incidence of vote-buying in more detail through a field experiment conducted during the 2006 elections in Sao Tome and Principe. The campaign consisted of a door-to-door informational

campaign sponsored by the country's electoral commission. Individuals received an informational leaflet stressing the illegal nature of vote-buying. The leaflet was supplemented by face-to-face conversations with the canvassers who emphasized that, even if individuals accepted "gifts," they should not let these bribes influence their voting decisions. Through a panel survey and analysis of electoral results, Vicente shows that the campaign reduced both the frequency and price of vote-buying. The treatment also decreased perceptions among voters that money could buy their votes. Like the studies discussed above, Vicente also finds that turnout in treated districts was lower by 2–6 percentage points and that these effects had a negative impact on the challenger's vote share.

Vote-buying is an illegal behavior that can broadly be considered corruption of the democratic process if not "the abuse of public office for private gain" per se. Although as with [Banerjee et al. \(2011\)](#), it is difficult to know whether it was voters, candidates, or both who initially responded to the information treatments, Vicente argues that the decrease in both the frequency of vote-buying and the price of bought votes was due to a drop in demand by candidates that resulted from their perception that vote-buying was less likely to affect voting behavior after the informational campaign. If the decrease in vote-buying is, in fact, the result of choices being made by party operatives to not engage in behavior that voters have been informed is illegal, then this is prospective action taken on the part of politicians to reduce malfeasance in the face of an informational campaign.

Future research in this area should try to isolate whether politicians change their behavior in reaction to the anticorruption information itself or rather only in the wake of citizens themselves first responding to the anticorruption stimulus. We envision experimental designs that attempt to randomize treatment across both candidates and voters. For example, in one treatment, candidates can be informed that an anticorruption campaign similar to [Vicente's \(2010\)](#) will be conducted during the election, except then the campaign is not actually undertaken. In a second treatment condition, candidates can be informed of the campaign, and voters also actually receive an intervention. In a third group, only voters receive the treatment. The challenge to such an approach is that the establishment of a pure control for comparison may be impossible, given the limited number of candidates who might be running in a given constituency or race. Still, with the right design and experimental conditions, researchers may be able to draw further insights into how their interventions affect the relative behavior of citizens and politicians.

INFORMATION AS AN ANTIDOTE TO BUREAUCRATIC CORRUPTION

Prospective Reactions by Bureaucrats to Behavioral Information and Information about Recourse

When an information treatment is itself tied to a mechanism for accountability, the direct effects on corruption and the behavior of agents may be easier to measure. A final set of field experiments about the impact of information on corruption uses the threat of audits and realized requests for information as treatments and then measures the behavior of bureaucratic agents in the delivery of some observable good or service. Here information does little to change the behavior of principals (i.e., those who are ultimately expected to benefit from the goods and services) but rather informs agents about the level of monitoring and accountability to which they might be subject.

In a randomized field experiment involving road projects in 600 Indonesian villages, [Olken \(2007\)](#) assesses the effects on corruption of manipulating both “top-down” and “bottom-up” mechanisms for accountability. In the top-down treatment, the probability of a government audit of the projects’ expenses was increased from 4 percent to certainty. Auditing with certainly presumably increased the expectation of implementing bureaucrats that any corrupt activities in the project might be revealed. The bottom-up interventions were designed to increase grassroots monitoring by inviting villagers to participate in community meetings and also by giving them the opportunity to express the views through anonymous comment cards. Once again, bureaucrats might have expected corrupt activities to be revealed through these mechanisms and therefore have refrained from engaging in them.

Olken finds that informing implementing agents about the likelihood of top-down monitoring is more effective at reducing corruption than increasing grassroots participation. Increasing the probability of government audits to certainty reduced the amount of missing expenditures in road construction by 8 percentage points. While the bottom-up interventions increased community participation and reduced missing labor expenditures, they had no significant impact on overall missing expenditures. Olken hypothesizes that the failure of the grassroots interventions to produce stronger results may be due to the weaker individual incentives for monitoring projects providing public goods like roads. He suggests that

grassroots monitoring may be a more successful in reducing corruption in programs that provide more particularistic goods, such as medical and school supplies, where the individual incentives for monitoring are higher. It may also be the case that grassroots monitoring takes time to develop as an effective mechanism for accountability.

A major advantage of Olken's study is his precise measurement of corruption. By creating sample roads and extensively surveying local markets to figure out the costs of goods and labor, he was able to measure the discrepancy between constructed roads and the official reports about their construction, providing an estimate of the amount of resources lost to corruption.

Further evidence that simply providing additional information to bureaucrats on the monitoring ability of principals may be enough to change agent behavior comes from Peisakhin and Pinto's (2010) study that makes use of India's Right to Information Act (RTIA). Slum dwellers in New Delhi were randomly assigned to three treatment groups and a control group. In the control group, participants followed the standard procedures in applying for a ration card. In one treatment group, applications were submitted with a letter of recommendation from a local NGO. In the second treatment group, applications were followed by an information request on the status of their application made under the auspices of the RTIA. In the third group, applications were made through a middleman who paid a bribe (or "speed money") to a local official.

The NGO letter and RTIA request treatments convey information about the informal and formal channels through which citizens can try to exercise a degree of accountability over bureaucrats. The letter from the NGO conveys informally that the citizen is connected to a local organization presumed to have some political clout that may influence agents' behavior. The RTIA request represents a more formal channel of accountability in which bureaucratic agents are reminded that information about their performance is openly available to the public (and to their superiors). Furthermore, the failure to fulfill an RTIA request can result in a range of legal penalties for civil servants.

The primary dependent variable was the median time for receiving a ration card. As expected, the bribe group received ration cards in the least amount of time with a median wait time of 82 days. However, all of the respondents in the RTIA request group received a ration card, and wait times were dramatically less when compared to the control group and the NGO treatment. In fact, when accounting for the time taken to file

the RTIA requests, the difference between the RTIA treatment and bribery treatment medians was only 11 days. In other words, when principals informed bureaucrats of their knowledge of a mechanism available to them for monitoring and possibly sanctioning bureaucrats, those bureaucrats responded in the same way as if they were receiving a bribe. The prospective behavioral change on the part of those bureaucrats both reduced corruption and improved the quality of government service to citizens. The authors posit that the dramatic reductions in wait time are “not so much because of the Act’s penalty provisions, which are rarely used, but rather because in India’s ultra-competitive bureaucracy, any blemish on a public servant’s career can negatively affect his chances of promotion” (Peisakhin & Pinto, 2010, p. 275).

Peisakhin (forthcoming) finds similar results from an experiment using voter registration in Delhi as the dependent variable. Two populations in Delhi, the urban poor and the middle class, were randomly assigned to two treatment conditions. In one, their applications were submitted with a bribe paid through a middleman to speed up the request. In the second intervention, the applications were followed by an RTIA request asking about the status of the voter’s application and the average wait time of the process. A third pool of applicants with no bribe or RTIA request served as a control group. Again the RTIA request was an effective tool for reducing wait times as well as for reducing differences in the ease of registering between middle-class residents and slum dwellers. In both cases, the threat of being cited for a violation under the RTIA and the subsequent consequences for the bureaucrat’s chances at being promoted in the civil service led to prospective changes in behavior.

We see this as a particularly fruitful area for future research. The connections between the intervention and improved government service provision are quite clear here and lead directly to actionable policy interventions.

OPEN ISSUES IN THE DESIGN OF ANTICORRUPTION FIELD EXPERIMENTS

In this section, we discuss some of the tradeoffs that must be made in experimental design and some of the obstacles to being able to study the anticorruption effects of information.

Experimental Design

The first design challenge for researchers is selecting a target population.

Selecting the proper level (e.g., electoral precinct, individual households with in a city) is often a decision based on the cell of [Table 1](#) in which the study resides, constrained by the need to randomize at the lowest level possible ([Robinson, 1950](#)). The experiments on retrospective behavior by voting principals in the wake of information revelation (e.g., [Banerjee et al., 2011](#); [Chong et al., 2010](#); [de Figueiredo et al., 2011](#)) or information campaigns ([Banerjee et al., 2010](#)) tend to base their conclusions on aggregate-level (e.g., voting precinct) data, although some authors also supplement that data with individual-level survey data. In these cases, even though the behavioral changes happen among individual voters who react to corruption information, the treatment is administered and outcomes are measured at an aggregate level where results are based on the decisions of many voters. The research on prospective behavior by political agents in the wake of information campaigns ([Vicente, 2010](#)) also measures outcomes at an aggregate level.

For the studies of prospective behavior by bureaucratic agents in the wake of an information campaign, the unit of analysis is more directly the one where we would expect change to happen. In this literature, the environments of individual transactions ([Peisakhin, forthcoming](#); [Peisakhin & Pinto, 2010](#)) or of individual projects that might become subject to corruption ([Olken, 2007](#)) are altered, and the outcomes are observed at the transaction level or project level.

After defining a target population, the researcher must design an effective informational treatment. It must be salient, credible, and accessible. If the outcome is a behavior and not just an attitude about corruption, the treatment must also be actionable.

The null effects in the rural experiment run by [Banerjee et al. \(2010\)](#) do not necessarily mean that normative campaigns about fighting corruption are worthless, but they may suggest that they are ineffective without specific information about particular politicians on which voters can actually take action. On the other hand, the [Peisakhin and Pinto \(2010\)](#) results reveal how a legal mechanism (i.e., the ability to request information under the RTIA) can constrain the behavior of bureaucrats even without the mechanism being fully implemented – it is the mere invocation of the mechanism that results in changed behavior.

Having decided on the information to be conveyed through treatment, researchers must then select how and when to convey it to the target

population. The format, source, and timing of information matters. As written material may be a poor choice for a largely illiterate population, [Banerjee et al. \(2010\)](#) used entertainment (a puppet show) to deliver the anticorruption message to communities. A partnership with local NGOs or newspapers to distribute information can lend credence to the message. In [Banerjee et al. \(2011\)](#) a widely read vernacular newspaper, *Dainik Hindustan*, published the report cards and eight local NGOs delivered the various stages of the intervention. The threat here is that the choice of the wrong partner (e.g., an organization perceived as biased) may contaminate results. Both [Chong et al. \(2010\)](#) and [de Figueiredo et al. \(2011\)](#) distributed fliers through direct marketing firms, which may raise concerns about how individuals interpret the credibility of the information conveyed, particularly, if the format leads individuals to interpret the message in an explicitly political or partisan matter. If subjects see the corruption message as further evidence of corrupt partisan activities, the intervention may have the opposite effect than intended. Appealing to credible sources, [de Figueiredo et al. \(2011\)](#) address some of these concerns by using a newspaper article about the corruption charges against a candidate and citing specific court case numbers on their fliers.

For retrospective voting decisions, in particular, the time at which information is distributed is likely to impact the size of the treatment effect. Information conveyed closer to elections has more salience to voters: for instance, [Pereira, Melo, and Figueiredo \(2009\)](#) find that information about corruption only influences vote choice when released in an election year. Similarly, informational campaigns are likely to have the largest effect on the way that citizens vote if they are conducted in very close proximity to an election.

The timing of the interventions varies in the electoral campaigns discussed above. [de Figueiredo et al. \(2011\)](#) administer treatment in the four days before the mayoral election. [Chong et al. \(2010\)](#) deliver treatment approximately one week before the election. The newspaper treatment in [Banerjee et al. \(2011\)](#) was conducted approximately 10 days before the election, while the treatment in rural villages in [Banerjee et al. \(2010\)](#) experiment occurred over two months leading up to the election. Prior research on framing effects suggests that the impact of experimental interventions on opinions such as candidate preference is powerful but short lived (e.g., [Chong & Druckman, 2010](#); [Druckman & Nelson, 2003](#); [Gaines, Kuklinski, & Quirk, 2007](#)). In a randomized field experiment conducted during the 2006 Texas gubernatorial campaign, [Gerber, Gimpel, Green, and Shaw \(2010\)](#) find that campaign ads delivered via radio and TV had

noticeable effects on candidate evaluation but that these effects were indiscernible a mere two days after the intervention. As seen in the [Banerjee et al. \(2010, 2011\)](#) experiments, informational treatments might be administered in several phases to ensure that the treatment is sufficiently strong enough to generate observable effects. Future field experiments examining the effects of information on corruption should seek to understand the effects of the timing of the intervention on the final results of treatment.

Spillover (i.e., interference between units) can affect any field experiment, but information treatments strike us as especially likely to suffer from spillover. Humans share information throughout their daily interactions. Experiments that rely on easily shared information, therefore, face a larger risk of spillover than do those that use physically applied treatments or nontransferable goods or experiences. The dominant method of treatment in these studies has been printed materials ([Banerjee et al., 2010, 2011](#); [Chong et al., 2010](#); [de Figueiredo et al., 2011](#); [Vicente, 2010](#)). Once read, these flyers can either be directly shared with others, or the information can be repeated verbally or through other channels – perhaps to control subjects. Broadcast messages in the form of public speaking or entertainment are subject to the same direct observation or repetition and may inadvertently expose control subjects to the treatment.

In several of the cited articles, the hypothesized mechanisms assert that information influences behavior through social channels. [Banerjee et al. \(2011\)](#) argue that educated individuals in treated units communicate information to less educated individuals. Just as those authors expect information to travel between individuals within a treatment unit, so too might information travel across treatment unit boundaries. In fact, researchers may find themselves in a double bind when designing treatment materials. For the manipulation to be effective, it must capture the attention of subjects, present information in a novel way, and inform. These same characteristics make it more likely to be shared by treatment subjects with control subjects.

The primary danger of spillover effects is that most statistical techniques make independence assumptions, which in turn imply the Stable Unit Treatment Value Assumption ([Rubin, 1980](#)). Where the treatment of one unit changes the outcomes of another unit, average treatment effects are no longer valid. Not all statistical techniques require independence, and opportunities exist to account for or model interference directly ([Bowers & Fredrickson, 2011](#)).

We can think of spillover occurring as either the direct or indirect effect of treatment administration. For treatments conducted in public view, such as

informational campaigns, it may be impossible to hide the alternative treatment levels from subjects in control conditions. In one quality-of-governance experiment where legislators were randomly assigned to be publicly reviewed on a website, this happened within the view of legislators in the control group (Malesky, Schuler, & Tran, 2011). Consequently, the result that treated legislators spoke less than their control counterparts may have resulted from the treatment encouraging treated legislators to remain quiet or from control legislators being emboldened by the knowledge that they were not being tracked.⁴ Spillover may also be indirect, the result of interaction between treated and control units. While we often think of spillover as the result of treated individuals communicating with control individuals, spillover effects may be exhibited at the aggregate level (e.g., precincts or cities). Chong et al. (2010) randomly assigned treatment levels to precincts within a municipality. While the authors provide detailed information supporting the claim that the implementers of the treatment conditions did not cross precinct boundaries, we know little about social patterns across precinct boundaries. Do residents frequently communicate across these borders? Do they attend common churches, have children in the same schools, play on the same soccer teams, and work at the same businesses? While such interactions are not immediate proof of spillover, they are opportunities for residents in treated precincts to relay information that will influence the outcomes in nontreated precincts (or vice versa).

At a minimum, there may be opportunities to measure spillover in the form of manipulation checks. Subjects should know about their own treatment but be unaware of alternative treatments. Checks via survey instruments may provide evidence that spillover did or did not occur, though explicit recall does not exhaustively prove that a subject was not influenced in more subtle ways. Behavioral checks are also possible. For example, in Aker, Collier, and Vicente (2010), treated subjects in the “hotline treatment” group were encouraged to send text messages reporting electoral fraud; members of the control group should have been unaware of the phone number for sending in SMS messages. Any text messages received from nontreated units would be immediate cause to suspect spillover effects.

Researchers can also guard against spillover at the design stage, though often at a cost of experimental validity. The “lab in the field” design retains much of the control of the laboratory but is implemented outside of a formal scientific environment. For example, in a field experiment about corrupt grading practices in Burkina Faso, Armantier and Boly (2008) avoided opportunities for subject interaction by separating subjects into

separate rooms. Like a laboratory experiment, however, when such forced separation occurs, subjects will not be experiencing treatment in the most natural environment.

Separating subjects can be built into the randomization phase of the design as well. A researcher can attempt to separate units geographically such that there is little perceived probability of spillover (Wantchekon, 2003). Careful consideration of how to properly randomize is paramount. Concerned about potential spillover effects from treating multiple polling locations in a single village, Banerjee et al. (2010) first randomly assigned treatment, then adjusted treatment such that no two polling places within the same village received different treatments. This adjustment addressed the spillover concerns but then required the researchers to use an instrumental variables analysis rather than a simple difference of means analysis that would have been possible under a conventional randomization.

Measuring Results

Elsewhere in this volume, Sequeira categorizes types of corruption measures. In informational field studies, the two dominant measures have been correlational measures and direct observation. In the majority of the studies reviewed above, the dependent variable is not corruption per se but rather some observable behavior that is assumed to affect corruption. The most easily accessible of these are measures of turnout and vote share (e.g., Banerjee et al., 2010, 2011; de Figueiredo et al., 2011). The assumption is that information about corruption enhances electoral accountability, which should in the long-run reduce corruption. As a correlational measure, the research can only really comment directly on decisions made by voters; to study corruption outcomes would require additional measurement of actual corruption. Field experiments have been at the forefront of direct observation of corruption, asking questions such as: Were fewer public funds missing (Olken, 2007)? Were more services delivered on time (Peisakhin, forthcoming; Peisakhin & Pinto, 2010)? Were corrupt practices like vote-buying less prevalent (Banerjee et al., 2011; Vicente, 2010)? While the effects on corruption in these cases are more certain, the mechanism – likely prospective changes in the behavior of agents – is inferred rather than observed. These examples show the tensions between correlational measures and direct observation. Correlational measures can support hypothesized mechanisms, but do not provide direct evidence of changes to levels of corruption, while direct observation provides direct evidence of levels but cannot offer any

insight into the intermediate mechanisms between the treatment and the outcome.

Indeed, unobserved mechanisms in experimental research are the norm rather than the exception, and understanding mechanisms is a more difficult problem than it first appears. Researchers observe the input of treatment and the output of some observed measure, but the connection between the two can be hard to illuminate.⁵ We acknowledge that the finding that an informational treatment controls corruption is an important discovery, but we also challenge future researchers to design experiments that will help to reveal the mechanisms at work. For example, [Banerjee et al. \(2010\)](#) hypothesize that information flows from more educated to less educated citizens. Additional experimentation could illuminate this relationship. Pairing subjects and randomly assigning an information treatment to the more or less educated partner would provide insight into this hypothesized mechanism. [Chong et al. \(2010\)](#) and [de Figueiredo et al. \(2011\)](#) find that information reduces voter turnout, but we do not yet know from these studies if information (either directly or through reduced voter turnout) controls corruption. To address this need, we can study various downstream attitudinal and behavioral impacts of the treatment to establish an effect of corruption.

More precise correlational measures also help illuminate mechanism. Interpreting what voters are indicating with their votes is a difficult task. In some studies, there is a behavioral measure of the demand for accountability. Participants are given the opportunity to engage in a costly act that demonstrates their demand for policy changes and/or less corruption. If the information intervention is successful, then participants in the treatment group should be more willing to undertake the costly act. [Vicente \(2010\)](#), [Batista and Vicente \(2011\)](#), and [Paler \(2011\)](#) all measure the extent to which subjects are willing to take the time and effort to return a postcard as a behavioral measure of citizen demand for political accountability. Similarly, [Aker et al. \(2010\)](#) allow participants to text policy proposals and submit open letters to newspapers. These measures, as compared to incumbent vote share, more directly measure citizen demands for accountability, moving this critical piece of the story from the role of a presumed mediator to an observable outcome of the treatment.

ISSUES TO ADDRESS IN FUTURE RESEARCH

Field experiments can offer important insights into the relationship between information and corruption. The previous sections have outlined the current

state of research in this area and put forth some of the common challenges faced in these studies. This section contains some general guidelines and suggestions for future research. Broadly our comments address the way treatments can be designed and how effects can be generalized and measured over time.

Manipulating Specificity, Credibility, and Accessibility

We began this review with a typology for organizing field experiments assessing the effects of information on corruption. We believe that the basic questions we have used to organize this relatively nascent field of study can also guide further research. In providing suggestions to help scholars design effective experiments, we will return to some of these basic questions about what type of information matters, for whom does it matter, and how should it be delivered.

Information matters, but not all information matters equally. Furthermore, the information that matters most in regards to corruption may be the most difficult to obtain. Scholars focused on bureaucratic corruption have the benefit of being able to design experiments around discrete and observable instances of corruption. The central finding that information about recourse reduces corruption seems relatively clear. Replication of these results in different settings with different mechanisms of accountability could provide further confidence in the robustness of this result and new information about the relative usefulness of different forms of recourse.

Studies focusing on political corruption face a more difficult task. Extant work on political corruption has either assumed anticorruption preferences exist and focused on providing particularistic information for voters (Banerjee et al., 2011; Chong et al., 2010; de Figueiredo et al., 2011) or attempted to make corruption more salient under the assumption that voters would act on their existing information to achieve electoral accountability (Banerjee et al., 2010).

The implications of these studies for corruption are ambiguous. In some cases, individuals clearly respond to information, but their behavior and preferences are not always consistent. Why does information about corruption in some cases lead to greater or lower turnout? Why did voters in Brazil respond to one candidate's corrupt record but not another's (de Figueiredo et al., 2011)? We believe these questions necessitate further research into how information about corruption is framed and the effects of the institutional context in which such information is delivered.

We have argued that corruption will change citizen's preferences when the information is specific, credible, and accessible (Winters & Weitz-Shapiro, 2011). Concerns about the accessibility of information are most easily assessed by researchers manipulating the medium (e.g., print vs. radio) and presentation (e.g., text vs. figures, or impersonal vs. personal). Researchers have less freedom to manipulate the credibility and specificity of information – particularly about political corruption – although partisan and non-partisan sources, for instance might be assumed to be more or less credible to some people. Manipulating specificity might involve using hypothetical vignettes or purposefully obscuring portions of corruption allegations. The salience of corruption is a function of both an individual's personal experiences and the broader environment in which they are situated. If researchers believe that corruption is a problem but not salient in the minds of voters, information campaigns can attempt to change this (Banerjee et al., 2010) but likely need to include particularistic information that makes the issue relevant to the individual.

One area of opportunity to increase salience may lie in social connections. Field research has already shown that engaging social pressure can raise the salience of elections where voters would otherwise not pay much attention (Gerber et al., 2008). Social cues about the importance or prevalence of corruption may generate enough interest in citizens to reach the critical levels of salience necessary for effective responses. Engaging with social networks requires a mode of analysis that is often neglected in field experiments, as we note in a previous section. We see many opportunities to correct this shortcoming and strengthen corruption research simultaneously. Additionally, researchers may attempt to capitalize on situations where external events such as campaigns, government audits and/or media investigations have made the issue salient in the minds of voters. For interventions which occur over a long period of time, randomizing the timing of interventions in treatment groups can provide another variable of interest, allowing researchers to assess changes in the effects of information as it is delivered farther from or closer to an election.

Future research concerning the salience and framing of corruption may wish to borrow from the extensive literature on economic voting.⁶ A great deal of debate in this literature centers around whether voters base their decisions on individualistic “pocketbook” concerns (“how am I doing?”) or more general “sociotropic” assessments (“how is society doing?”). The percent of citizens concerned about corruption is consistently high (Canache & Allison, 2005; Razafindrakoto & Roubaud, 2010) while the percentage that has actually experienced corruption is much lower

(Razafindrakoto & Roubaud, 2010; Seligson, 2002, 2006). Scholars may wish to assess the effectiveness of anticorruption when informational appeals are framed in terms of specific individual harms caused by corruption, when they are framed in terms of broader social ills associated with corruption, and when the two messages are combined.

We suspect that the credibility of information is a function of the reliability of the source and the content of the claim. More work comparing the effectiveness of face-to-face interventions with less personal appeals seems fruitful. Lab experiments, where there is a greater degree of experimental control, may be useful complements to field work, helping researchers establish what information voters find credible about corruption. So-called “lab in the field” designs may provide unique opportunities to combine the advantages of both the control of the laboratory with the realism of the field. For example, researchers can apply novel video treatments or use other media to capture the attention of busy subjects (Dunning & Harrison, 2010).

A final field for further research concerns information that affects the behavior of agents. Elected officials are difficult targets for treatment and prospective changes in their behavior are difficult to observe with certainty. This task is not impossible, however. Malesky et al. (2011) randomly select delegates to the Vietnamese National Assembly to have their behavior publicly documented on the website of a national newspaper during a national query session. They find that treated delegates speak less frequently and less critically of the national government. These changes are largest for members where the access to information (as measured by Internet penetration in their district) is highest, suggesting that the members are responding prospectively to changes in the level of information constituents will possess about their actions. Future work may wish to inform a random set of elected officials that their performance will be monitored and that these results will be made publicly available to their constituents and then compare their performance to a baseline performance of legislators not subject to higher scrutiny.

Learning More about Institutions and Accountability

We have argued that for information to be an effective tool against corruption it must be actionable. To a certain extent, actionability varies with institutional setting. There currently exists a debate in the corruption literature about what types of institutions facilitate the use of accountability

mechanisms. For instance, Gerring and Thacker (2004) argue that parliamentary systems and unitary governments promote clearer lines of responsibility because there are fewer levels of hierarchy and fewer veto points. Kunicová and Rose-Ackerman (2005), on the other hand, argue that countries with single member districts and open list proportional representation tend to have lower levels of corruption because of the increased ability to hold particular individuals responsible in such systems. Tavits (2005) offers a more encompassing definition of clarity of responsibility which she claims outperforms other institutional measures. Insofar as similar information interventions might be repeated across different institutional contexts, this would offer a chance to explore how voters in different environments use information and to adjudicate some of the competing claims in the observational literature. Does particularistic information about corrupt politicians have the same effects on turnout and vote share in proportional representation systems as in majoritarian systems? Do informational campaigns about the value of opposing corruption have a greater impact in one type of system over another?

The studies in this chapter have largely been conducted within single institutional contexts. We see great opportunity in spreading field experiments across multiple institutional systems. Imagine a study in which an informational campaign about the dangers of vote-buying is conducted in both a country with a proportional representation country system and a country with a majoritarian system. Alternatively, institutional variations might under some circumstances be manipulated directly to add a causal interpretation to the results. While this chapter has maintained a strict focus on informational manipulations, opportunities exist to combine these efforts with studies manipulating institutions directly.

Effects Over Time

A related challenge to generalizing results is understanding how the effects of an intervention will persist and change over time. Reinikka and Svensson (2005) demonstrate dramatic reductions in missing school expenditures following a newspaper campaign in Uganda, suggesting that policy interventions can make a clear difference. However, it is difficult to anticipate how actors will respond over time to a change in the environment. Are changes in agent behavior simply a temporary response to the intervention? If an intervention becomes the norm, will agents substitute one form of corruption with another? As an example of agents strategically

adjusting their behavior, [Olken \(2007\)](#) found that while overall missing expenditures declined with announced audits, the incidence of hiring relatives increased.

The information campaign experiments ([Banerjee et al., 2011](#); [Vicente, 2010](#)) involve manipulations that we would expect to have persistent effects. In the case of [Vicente \(2010\)](#), will vote-buying remain less prevalent in treated areas in the next election? For [Banerjee et al. \(2011\)](#), will citizens in treated areas continue to deemphasize caste as a voting heuristic in subsequent elections? The data collection costs to answer these kinds of questions are relatively low, and the benefits from knowing more about the impact of informational campaigns over time are large, since they inform policy makers whether they need to repeat campaigns on an annual basis or not.

Studying the downstream effects of the particularistic information treatments is also interesting. Is it possible that [de Figueiredo et al.'s \(2011\)](#) campaign fliers have had a lasting effect on voter impressions of the candidates involved in the experiment? [Chong et al. \(2010\)](#) demonstrate how incumbent behavior affects voters' attitudes toward the incumbent's party even when the individual himself is not running for reelection; does this effect persist for subsequent elections? Do the villages in [Olken \(2007\)](#) where audits were announced with certainty (and then conducted) have persistently lower levels of corruption because relevant officials are more concerned about audits in the future even though the announcement has not been repeated? Do individual bureaucrats in [Peisakhin and Pinto \(2010\)](#) who have been treated with the RTIA letter act differently when considering future applications because they are more aware of citizens being willing to invoke that particular legal right? Much can still be learned from these experiments by studying the downstream impacts.

We encourage researchers embarking on new field studies to take pains to carefully catalog the design and implementation of field experiments for later follow up. Recording GPS coordinates, taking additional contact information and finding the contact information of friends, keeping records of the materials used, and other documentation will allow the scientific community to return to the field in the future to see what the long-term effects of various informational treatments on corruption appear to be. While many of the results have been positive with respect to short-term effects, a growing body of literature indicates that many anticorruption interventions may create perverse long-term incentives for principals ([Bobonis, Cámara Fuertes, & Schwabe, 2011](#)). Verifying whether these manipulations have long-term effects, if any, is a valuable research goal.

CONCLUDING REMARKS

We proposed three dimensions along which field experiments about the relationship between information and corruption vary: (1) whether they address political or bureaucratic corruption; (2) whether they involve information about specific behaviors or instead about norms and possible recourse; and (3) whether they are intended to invoke retrospective or prospective mechanisms by which corruption is reduced. We find that the extant literature is clustered in terms of interventions that (1) provide information about politicians' behavior and are meant to reduce corruption through the retrospective voting mechanism; (2) provide informational campaigns about appropriate behaviors for politicians and are meant to reduce political corruption through prospective self-sanctioning of behavior; and (3) provide information about possible recourse and are meant to reduce bureaucratic corruption through prospective behavioral changes. We also catalogued studies that (4) provide information about appropriate behaviors for politicians with the goal of having citizens hold them electorally accountable and (5) provide information about likely behavioral revelation that is meant to reduce bureaucratic corruption through prospective behavioral changes.

Field experiments in some of the other categories are certainly possible. Do informational campaigns or the revelation of information about bureaucratic corruption result in political principals removing corrupt bureaucrats from office (a retrospective mechanism for reducing corruption)? This is something worthy of study from the perspective of knowing how citizen pressure for better government service delivery will affect the actions of politicians who are supposed to be monitoring and supervising bureaucrats on behalf of the citizenry. Given the limited amount of work that has been done on studying bureaucratic corruption so far, it seems like there is the potential for major contributions through studying both prospective and retrospective changes to the bureaucracy.

In terms of changing the dynamics of political corruption, does the provision of specific information impact the quality of candidates who choose to stand for office? That is, knowing that previous officials have been exposed as corrupt, will corrupt entrants opt not to stand for office, improving the overall pool of candidates who run (a prospective mechanism for reducing corruption)?

The experimental study of how improved information reduces corruption is still very much in its infancy. We have good theoretical reasons to suspect that information will reduce corruption; we have macro-level evidence that

corruption levels are lower in places where information is more readily accessible; and we are beginning to accumulate evidence on how specific information and general informational campaigns work when it comes to reducing corruption. But there is much work still to be done, particularly insofar as not all of the studies reviewed above consistently produced the expected results. Why did particularistic information change vote totals in some contexts but not others? Why were some types of informational campaigns successful and not others? Only through repeated experimentation and replication across different contexts will we be able to figure out the answers to these questions.

NOTES

1. For an overview of the issues relating to the definition and measurement of corruption see [Kurer \(2005\)](#).

2. One of the classic results of this sort is [Reinikka and Svensson \(2005\)](#) in which the authors show that publicizing central education budget allocations resulted in a fourfold increase in the average proportion of grants reaching schools. That is, before the intended allocations were publicized, schools were receiving, on average, only 20 percent of the funds to which they were entitled; after a newspaper campaign was implemented, schools received, on average, 80 percent of the funds to which they were entitled. With information more widely available, intermediaries were more circumspect about stealing funds. Although not about corruption per se, [Björkman and Svensson \(2009\)](#) document how the provision of a citizens report card containing information on the performance of health clinics relative to other clinics and national standards resulted in increases in staffing, service provision, and cleanliness of clinics and ultimately in health outcomes.

3. For a discussion of the different ways in which authors have tried to measure corruption, see [Sequeira's](#) chapter in this volume.

4. Spillover shares many characteristics with noncompliance, where subjects do not follow the assigned treatment regime. Noncompliance is often characterized by units selecting whether to receive the treatment or not. The dangers of noncompliance in field experiments are well documented (see [Gerber and Green, 2000](#); [Hansen and Bowers, 2009](#); [Imai, 2005](#)), and we see no reason to restate the issues here, other than to note that information and corruption field experiments are subject to the same issues. See [Chong et al. \(2010\)](#) for an example where noncompliance takes the form of incomplete treatment application in some districts.

5. Experimental research establishes causal relationships between treatment and any number of outcomes. Relationships among outcomes are harder to establish. For example, assume we observe that an information treatment increases citizen demand for accountability while simultaneously increasing the number of anticorruption candidates running for office. We can argue that the treatment causes both of these outcomes, but the experimental design does not indicate that either citizen demand causes more anticorruption candidates or anticorruption candidates raise

citizen demand for accountability. Statistical methods exist to establish the strength of these relationships, but require assuming the direction of causality among outcomes (Baron & Kenny, 1986). The logical basis, and even necessity, of these tools are questioned by some researchers who see establishing primary effects as a more desirable aim (Green, Ha, & Bullock, 2010). As a middle ground, specialized research designs can help illuminate more about mechanism than a simple randomization (Imai, Keele, & Yamamoto, 2010).

6. Gomez and Wilson (2001) provide an overview of the literature.

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