

# Can information that raises voter expectations improve accountability? A field experiment in Mali

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## Abstract

This paper studies the political effects of a particular type of information intervention: one that improves voter expectations of government capacity. I argue that if citizens systematically underestimate what their government is capable of, then voting on the basis of performance is of little consequence, and politicians in turn have little incentive to perform well. I report evidence from a randomized field experiment in Mali that tests whether improving voter information about the scope of government effects voting behavior. A civic education course was provided to 370 villages in 64 randomly assigned municipalities dispensing information on the responsibilities of local government and the basics of democratic accountability to all treated villages, with an additional component on relative government performance to half of treated villages. A survey was then conducted in the 64 treated and 31 control municipalities. Voting simulations show that people in treated villages are more likely to vote based on performance: a poor-performing candidate had to pay more to buy votes of citizens in treated communities, and the votes of citizens in control communities were more easily swayed by dimensions such as kinship or gift-giving. Suggestive evidence points to two possible mechanisms underlying this behavior: treatment raised expectations of local government and improved coordination among voters. A behavioral outcome measure – the likelihood that villagers challenge local leaders at a town hall meeting – confirms the positive treatment effects found in the survey measures.

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The recent emergence of free and fair elections in many developing countries has frequently failed to produce the expected increase in government accountability. Information asymmetries between voters and political elites are a key determinant of democratic failure. For voters to discipline leaders at the ballot box, they must have a minimum level of information, both about the candidates for whom they are voting and the democratic system in which they participate. Particularly in developing countries, this information is often lacking, which may help explain poor reform. The relationship between voter information and political behavior is poorly understood, however. Empirical studies show different kinds of information have widely varying effects on political outcomes (Pande, 2011).

This paper studies the effects of a particular type of information intervention: one that improves voter expectations of government capacity. I argue that if citizens systematically underestimate what their government is capable of, then evaluating politicians on the basis of performance is of little consequence. Politicians in turn are stripped of incentives to perform and a bad governance equilibrium ensues.

I report evidence from a randomized field experiment in Mali that tests whether improving voter information about the scope of government impacts voting behavior. A civic education course was provided to 370 villages in 64 randomly assigned municipalities. The course dispensed information on the responsibilities of local government and the basics of democratic accountability to all treated villages, with an additional component on relative government performance to half of treated villages. A survey was then conducted in the 64 treated and 31 control municipalities. Voting simulations show that people in treated villages are more likely to vote based on performance: a (hypothetical) poor-performing candidate had to pay more to buy votes of citizens in treated communities, and the votes of citizens in control communities were more easily swayed by dimensions such as kinship or gift-giving. Some evidence points to two possible mechanisms underlying this behavior: treatment raised expectations of local government and improved coordination among voters. Behavioral outcomes such as participation in civic activities and challenging local leadership are also measured in the experimental sample. While survey results are the focus of this paper, I briefly discuss treatment effects on one behavioral outcome, the likelihood of challenging leaders at a town hall meeting, to add confidence to the findings.

The theoretical literature suggests strongly that information about government performance is a necessary condition for political accountability (Ferejohn 1986; Besley 2006). When voters are well-informed about how politicians govern, they can make welfare-enhancing decisions at the ballot box. With uncertainty about politicians' performance, good performers cannot guarantee reelection so it is harder to motivate it. Poor-performing or unrepresentative politicians do not get sanctioned as often as they would in a higher-information setting. This intuition is closely related to modernization theory which links education more broadly to democracy. Explaining early evidence of the correlation

between literacy and democratic stability, Lipset (1959) says education “increases [the] capacity to make rational electoral choices.” Similarly, Almond and Verba (1963) recognize that formal institutions are not sufficient to sustain democracy, which also requires a civic culture. Their democratic citizen is not only one who is “active in politics,” but also one who is “well informed” and makes decisions “on the basis of careful calculation as to the interests and the principles he would like to see furthered.” Finally, providing information directly to voters can reduce their vulnerability to influence by political brokers. Villalon (1999) and Beck (2008) show the importance of brokers, especially in reaching under-informed populations.

Empirical studies support these theoretical claims of the positive effects of information on voter behavior and politician performance (Besley and Burgess 2002; Ferraz and Finan 2008a; Reinikka and Svensson 2005).<sup>1</sup> Finkel and Smith (2011) show that civic education, in particular, had salutary political effects in Kenya such as increasing knowledge, values and participatory inclinations as well as eventually creating opinion leaders who transmit new ideas within their networks. And Kramon (2011) finds that more educated voters in Kenya are less likely to prefer vote-buying candidates. However, recent information experiments in India (Banerjee et al., 2010a), Mexico (Chong et al., 2012) and Brazil (de Figueiredo et al., 2011) suggest that conveying information to voters about the misbehavior of politicians does not always have the intended effect of encouraging voters to sanction corrupt incumbents, particularly in low- and middle-income democracies. In the India study, an anti-corruption information campaign had no effect on voter behavior; in the Mexico study, information about corrupt incumbents led voters to turn out less and not necessarily to sanction more; and in Brazil, publicizing an incumbent’s corruption charges had no effect on turnout or sanctioning. If voters already believe their candidates are corrupt, or think little is at stake in local government, then it is not surprising that providing additional information about government misconduct has no effect on voter behavior. Or in the cases of Mexico and Brazil, providing information about performance can even increase voter apathy or disillusionment.

I argue in the next section that a different type of information may be necessary to shift voter behavior in such settings: information about the stakes or capacity of government as opposed to actual government activity. Particularly in developing countries, voters are likely to underestimate the value of democratic government—either because of a history of unresponsive autocratic regimes, or because information asymmetries are severe and voters know little about the true capacity of government. There is some survey evidence on the value citizens place on democracy in developing nations. Among the 20 countries in the 2008 Afrobarometer survey, twenty percent of respondents say they approve of one-party

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<sup>1</sup>Keefer and Khemani (2011) is an exception. They find that increased radio access in Benin does improve public service provision, but the responsible mechanism is not improved government accountability but rather improved household investment in children’s education.

rule and fifteen percent of military rule. When asked directly, 70 percent say democracy is preferable to other kinds of government, and similarly, only 60 percent of respondents gave this answer in the 2008 Latinobarometer survey. When asked how satisfied they are with the way democracy is working in their own country, an average of 48 percent of Afrobarometer respondents said they were satisfied compared to 58 percent of respondents in the 2007 Eurobarometer surveys across 31 European countries. One potential explanation for this ambivalence toward democracy is beliefs about the weakness of electoral accountability: about half of Afrobarometer respondents say elections do not enable voters to remove from office leaders who do not do what the people want.

If voters underestimate the value of government, their calculus at the voting booth will differ from that of voters who perceive a more important role of government in their lives. I argue that information that raises voter expectations (aspirations) of government performance should make performance-based voting more likely, thereby improving accountability. Two types of information can raise voter expectations: information about what governments are legally responsible for doing and information about what governments are capable of doing, e.g. what resources they have at their disposal. Especially where there are multiple levels of government such as in decentralized systems, voters may be uncertain about which level of government is responsible for providing a particular public service, and that uncertainty can benefit elected officials at the ballot box. Voters may also be uncertain about the size of the government budget or the types of projects the government is capable of implementing, making proper evaluations of performance difficult. One of the treatment conditions in the Mexico experiment cited earlier provides evidence that budget information can produce effects on voter behavior. When information was disseminated on the total amount of money allocated to local governments and the amount spent by year end, voters turned out more and sanctioned the incumbent more often when the percentage of funds spent was low (Chong et al., 2012).

One potential reason that previous information interventions contribute to voter apathy rather than increased sanctioning may be that the information provided confirms voter beliefs about their own poor-performing politicians without providing hope that better politicians exist. In the information experiments in Brazil and Mexico, only performance information about candidates in the voter's district was provided. Ferraz and Finan's (2008b) evaluation of the Brazilian audit program, on the other hand, shows that when a larger set of performance data is publicly released, voters sanction corrupt incumbents more often. Similarly, results from another experimentally assigned information campaign in the Dehli slums accord with theoretical expectations: access to information increases voter turnout and vote share for better performing incumbents (Banerjee et al., 2010b). In this case, voters received details about legislator responsibilities as well report cards for multiple candidates allowing them to better calibrate

their expectations. The difference in outcomes between these two studies and the ones cited earlier may be attributable to the fact that voters in the latter case received proof that some politicians were less corrupt than their own.

The experiment described in this paper consists of two treatment conditions reflecting two different information interventions that policymakers might practically choose to administer. The first treatment consists of a basic civic education course that provides both types of information previously described.<sup>2</sup> In particular, participants learn about the responsibility of local governments to provide a menu of public goods, and they learn about the average size of local government budget in their region. If voters are constrained by uncertainty about government capacity as well as uncertainty about actual government performance, then this basic civics course is not sufficient on its own. Even if voters learn what they can expect of their government, they cannot properly evaluate politicians when they are uncertain about the incumbent's performance record. A second treatment condition provides the civics course plus additional information about *relative* government performance. This has the advantage of giving participants new information about the incumbent's performance record as well as reinforcing what voters can expect of their governments. Because the performance information is relative to how other comparable governments are performing, participants learn even more about what their government is capable of or what is in the feasible set of government actions.

One motivation for studying these two types of interventions is practical: for policymakers, the decision to produce each type of intervention is distinct. Civics curricula can be taught over a long period of time without being updated, and can be distributed uniformly across the country. Relative performance information, on the other hand, needs to be updated frequently, tailored to particular districts, and is costly to collect, not to mention potentially politically contentious. While the research design does not permit a direct comparison of these two information interventions since the second treatment condition is a combination of the two,<sup>3</sup> we can robustly make the following assessments: 1) whether basic civic education is sufficient to change the political outcomes being studied, and 2) whether a combination of the interventions produces effects that are significantly different from just providing the first. If we assume additivity in the treatments, we can make conjectures about whether the first type of information intervention is necessary for the second to be successful or whether the relative performance information can achieve effects on its own.

The next section develops a theory of the impact of information about potential government per-

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<sup>2</sup>See Gine and Mansuri (2011) for an example of an experimental intervention that provided civic information, in particular on the importance of voting and the secret ballot. In treated Pakistani communities, women turned out more and exercised more independence in candidate choice.

<sup>3</sup>A third treatment with only relative performance information would have been optimal, but only two treatments were possible given resource constraints and power considerations.

formance on voter behavior. It then derives predictions and observable implications tested later in the paper. The third section outlines the research design, explaining how the experiment was implemented. The fourth section specifies the measurement strategy and the fifth performs analyses of the data. I discuss results in the final section.

## Theory

As recognized by Barro (1973), politicians face powerful temptations to act against the public interest for private gain. In a democracy, voters can control such temptations or better align politician actions with their own interests through the electoral mechanism. Either by sanctioning poor-performing incumbents or selecting candidates with good reputations, voters can influence the actions of politicians. However, information asymmetries between the voter and the politician can lead to problems of adverse selection and moral hazard. In both cases, government accountability is weakened and the voter is less well off.

Voting decisions in a competitive democracy can be characterized as prospective in which the voter selects the candidate whose expected future performance they prefer, or retrospective in which the voter evaluates incumbent politicians according to some threshold or criteria (see Fearon 1999). The availability of information conditions the ability of voters to make sound judgments. In the first case, if there is poor information about candidate qualifications or insufficient performance records to build a reputation, then candidates become less distinguishable from one another. Adverse selection may occur in which undesirable politicians are elected to government more often than voters would prefer. In the second case, poor information about incumbent government performance also results in worse outcomes for voters in equilibrium. Moral hazard describes the situation in which politicians can act corruptly without electoral retribution because voters do not have precise enough information to sanction them.

I argue in this paper that a new type of information asymmetry affects the voter's ability to hold politicians accountable. Not only are voters uncertain about past government performance, they can also be uncertain about government capacity. I formalize this argument by extending classic moral hazard models of accountability (Barro, 1973; Ferejohn, 1986; Fearon, 1999; Besley, 2006). I focus on the sanctioning mechanism rather than selection because information about government capacity more clearly affects voter's performance evaluation of politicians than beliefs about candidate types. The selection models rely on voters distinguishing between good and bad types of candidates, which increased expectations of government should not, at least initially, help them do.

Before introducing uncertainty about capacity, I review the logic of the classic moral hazard models.

Voters condition their electoral decisions on government performance in the previous term, which they usually infer from some measure of their own welfare. Voters have a decision rule that allows them to translate perceived government performance into an action. The simplest rule is a cut point (as in Fearon 1999): if welfare exceeds a certain level, voters favor the incumbent, but if welfare falls below that cut point or performance standard then voters prefer the challenger. In Fearon's model, voters face a trade-off. They want to set the cut point as high as possible to extract maximum utility from the incumbent, but setting it too high will discourage the incumbent, giving him incentive to shirk or under-perform. An important comparative statics result is that the more precision with which voters know actual government policy, the higher they will set the cut point. In equilibrium, incumbents in a high-information setting will perform better due to this increased threat from voters. The predictions of the model substantiate the empirical findings cited earlier that increased access to information improves government accountability.

Modeling the problem in this way makes two implicit assumptions that while innocuous in some contexts, may be problematic in others. First, the above setup assumes the primary dimension along which voters make decisions is a performance criterion, which is not necessarily the case in many developing country settings where gift-giving or ethnic ties can prevail. Second, it is implicit that voters have full information about government capacity. For voters to generate an appropriate cut point, they must know what optimal government performance looks like. In other words, for voters to be able to accurately evaluate their slice of the pie, they must know how big the pie is. Incomplete or biased information about government capacity will influence voter choice of a cut point with implications for their ability to appropriately sanction politicians. I argue that this is an unexplored aspect of the information story: information asymmetries about the capacity of government can also impact the voters' ability to control politicians.

Results of the household survey conducted in Mali in conjunction with the experiment provide some evidence that voters often prioritize other dimensions than government performance in their vote choice, and that knowledge of government capacity is low. While 64 percent of people said they prioritized performance when deciding who to vote for, 48 percent said the receipt of gifts from candidates was the primary criteria *other* people use when voting.<sup>4</sup> Given the potential stigma attached to admitting that gifts have more sway than performance in one's own vote choice, the latter figure is likely a better approximation of the truth. When asked why people would not vote on performance, about half of respondents said a lack of information, a quarter said the government is not capable of doing much and

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<sup>4</sup>Because these criteria are not generally mutually exclusive, respondents were asked to rank criteria in order of importance rather than choose just one.

another quarter said people vote based on personal needs and interests. More than a third of people did not know the local government has a budget to invest in the commune and about half are under the false impression that the local budget is insufficient to finance even small development projects such as a well. In Mali's 2008 Afrobarometer survey, more people said local government was bad at providing information about the budget than providing information about their actual projects. Comparing Mali to other African countries in the Afrobarometer survey, Mali scores better than average on subjective measures such as these evaluating government provision of civic information. However, on objective measures of civic information such as the ability to name the local MP and the Finance Minister, Mali scores twelfth and eighteenth out of nineteen.

If the assumptions about performance-based voting and complete information about budgets are relaxed, information can influence voter behavior in new and important ways. Instead of just improving the electorate's perception of actual government performance, information can also improve voter beliefs about potential government performance. Especially if voters in developing country settings typically underestimate local government capacity, providing new information about what local governments can do or are responsible for doing should change voter beliefs. First, increasing information about government capacity should improve the selection of cut points by voters, or increase the minimum standard of governance that voters require in order to re-elect incumbents. Second, by changing beliefs about the capacity of local government, information should make the performance dimension more salient relative to other voting criteria.

In related work, James Fearon and I formalize these intuitions with an electoral model that takes into account voter uncertainty about the size of the public budget. We consider a single village that either has a representative voter or can be assumed to all vote as one. The village votes whether to keep or drop the incumbent politician in successive elections at times  $t = 1, 2, \dots$ . The politician gets  $w < 1$  every period he is in office which includes both a wage and other informal benefits to office. With probability  $p \in (0, 1)$  the politician has an available budget of size 1 (relative to  $w$ ) that would be sufficient to provide a public good in the village such as a well or improvements to a community school or clinic.

The politician knows if the funds are available while the village does not. This is justified in the current context where voters know there is a commune-level budget that can fund public goods, but they do not know if there is enough in any given period to support a public good project in their specific village. They also do not know the nature of coalition politics on the council that might randomly decide whether their specific village could get a project. We also assume villages cannot observe the whole set of public goods provided to all villages in the entire commune. This is another aspect of poor voter



information about government performance.

In each period, the politician observes whether funds are available and then chooses whether to build the project and how much to dispense in campaign gifts,  $x < w$ . His period payoff is  $w - x + 1$  if funds are available but he does not build a project and  $w - x$  if he does build an available project. After observing the amount of gifts dispensed and whether a project is built, the voter chooses to keep or drop the incumbent. The village's payoff is  $g + x$  if a project is built and  $x$  otherwise.  $g > 1$  is the village's value of the public good, reflecting its positive externalities and assuming the village prefers the project than having the money to spend on other things. Future payoffs are discounted by  $\delta \in (0, 1)$  each period, assuming for simplicity that a politician receives zero each subsequent period after getting kicked out of office.

Given this setup and forcing a Markov equilibrium with stationary strategies, the voter can condition re-election on both the value of the gifts provided in the campaign, and on the whether a new public good is built in the village. This gives the voter essentially two feasible "electoral control strategies." We call the first the *peanuts* strategy where the voter conditions their vote only on the receipt of campaign gifts. The second, we call the *big ticket* strategy in which the voter conditions her vote on gifts and the public good, voting against the incumbent if the public good is not provided. In both strategies, we assume retrospective voting using a cut point or threshold.

Since the voter observes  $x_t$  in period  $t$ , suppose the voter conditions re-election only on whether  $x \geq k$ , where  $k > 0$  is the cut point. (Here, we assume the incumbent is liquidity constrained and so cannot provide more than  $w$  in gifts in each campaign). The incumbent's payoff for supplying  $k$  in gifts each period is then

$$V_I^P = (1 - p)(w - k + \delta V_I^P) + p(w - k + 1 + \delta V_I^P) = \frac{w - k + p}{1 - \delta}$$

The incumbent prefers to provide  $k$  each period rather than shirk and get kicked out of office when

$$w - k + \delta V_I^P \geq w, \text{ or}$$

$$k \leq \delta(w + p).$$

So, the maximum the village can get each period from this "peanuts" equilibrium is the smaller of  $w$  and  $\delta(w + p)$ . Note that while some accountability is possible here in the form of private transfers, no public goods are provided even when they are available.

Alternatively, the village can vote against the incumbent if he fails to distribute  $x_t \geq k$  in gifts *or* if he fails to deliver a public good. In this incentive scheme, the incumbent loses office with probability  $1 - p$  in each period even if he is complying, making it harder to motivate him to provide public goods. This is the cost of the moral hazard problem arising from the voter's uncertainty of the size of the budget for their particular village.

The incumbent’s payoff under this “big ticket” scheme is

$$V_I^{BT} = (1 - p)w + p(w - k + \delta V_I^{BT}) = \frac{w - pk}{1 - \delta p}.$$

Incentive compatibility requires that the incumbent’s payoff from providing the gifts and project in a period when the project is available is at least as great as what he could get by providing zero gifts, consuming the project money, and losing office. Thus,

$$w - k + \delta V_I^{BT} \geq w + 1, \text{ or}$$

$$k \leq \delta(w + p) - 1.$$

This is impossible when  $\delta(w + p) \leq 1$ , which is more likely the lower is  $p$  or the belief that the budget is sufficient to fund a public good in the village. The problem here is that the public good is thought to be rare enough that if the voter were to condition on it, the politician would almost always do better to shirk or not provide either gifts or public goods and get kicked out of office. In this case, the village does better employing the “peanuts” electoral strategy which at least ensures them gifts in every period. In fact, in this incentive scheme, politicians’ ability to consume the funds for public goods is implicitly used to give them a stronger incentive to provide things of value to voters in the form of gifts or other more fungible goods. Thus, if the availability of large projects is in fact low, or if voters incorrectly believe that funds are rarely adequate, then it may be impossible to induce politicians to provide public goods when they are available. Voters may have to settle for inducing politicians to compete by offering campaign gifts or other personal services.

From this discussion, I derive the following testable predictions:

- H1:* Increasing voter information about potential government performance,  $p$ , will increase the cut point at which poor-performing candidates are sanctioned.
- H2:* Since the “big ticket” equilibrium does not exist for small enough  $p$ , increasing voter information about potential government performance will increase the likelihood of voting along the performance dimension.

In this setup, the village or voter is a unitary actor and votes as if they are decisive. One aspect of the voter’s decision that the model (and others like it) does not capture is the effect of a voter’s belief about how other people will vote. This may make sense where voting is anonymous such that choosing one candidate over another has no immediate or material benefit. However, in the Malian context as in many other developing democracies, there are threats to anonymity. For instance, about 20 percent of individuals surveyed thought that their vote choice could be discovered with some positive probability. And, because electoral precincts are often coterminous with village boundaries, election results are knowable at the village level so entire villages can be sanctioned by politicians.

For the case in which the actor is the voter not the village, imagine one voter has information that  $p$  is high. Under the “big ticket” strategy, he should kick out an incumbent who does not provide a public good. A second voter has a different belief: that  $p$  is low. This voter instead conditions his vote only on gifts and decides to keep the incumbent. If enough villagers are like the second voter, the incumbent will win and distribute gifts to his supporters. In this case, there is a real cost to the first voter choosing the “big ticket” strategy, and he would only want to do so if he thought enough other voters were doing the same.

In the presence of vote-buying or patronage politics and some ability of the candidates to punish voters, the voter’s decision can be conceived of as a coordination problem. Specifically, it resembles a *stag hunt* in game-theoretic terms in which there are two equilibrium outcomes: cooperation over a risky, but mutually preferred option or mutual defection for a less-preferred, but safer option. In the first equilibrium, voters coordinate on voting for a high-performing candidate and (sometimes) receive the preferred public goods. In the second equilibrium, voters instead opt for the more certain outcome, voting for a low-performing candidate in exchange for a small gift or patronage.

If the assumption of a unitary and decisive voter is relaxed, a voter’s decision calculus can be influenced by their beliefs about how others will vote. A voter is more likely to hold the government accountable for public goods provision, or apply the “big ticket” strategy, when they believe others are also using that strategy. A private information signal will cause a voter to update his own belief about the value of  $p$ . If the signal is public, it will also cause him to update his belief about other voters’ value of  $p$ . Where an individual’s participation is contingent on the participation of others, Chwe (2001) shows that a particular kind of communication, namely communication that creates common knowledge, allows people to effectively solve the coordination problem.

Common knowledge is a phenomenon in which everyone knows that everyone else knows something, knows that everyone else knows they know it, and so on, *ad infinitum*. This is distinct from mutual knowledge in which a group of people know the same thing but are unaware of what other people in the group know. Common knowledge can be created through a public announcement of information among a group of people who are aware of each other’s receipt of the information. If there are strategic complementarities to participation in an action, e.g. higher payoffs the more people participate, then a public signal can increase participation not only by changing beliefs about the underlying payoffs, but by changing beliefs about the likelihood of that outcome. For example, Yanagizawa (2009) shows that in Rwanda, participation in violence was increased by radio propaganda not only by increasing beliefs about the underlying value of violence, but by increasing beliefs about the extent to which others would participate.

If we conceive of the voter's choice as a coordination problem, then there are strategic complementarities to voting based on performance. Gains to disciplining the leader based on public goods provision only accrue if enough people are voting this way. As such, we might expect that a more widely disseminated signal would have a greater effect on voter behavior. For example, a poorly disseminated signal may affect a voter's own beliefs, but have insufficient effect on his beliefs about others (so the voter reasons similarly to how he would without the information signal). A widely disseminated signal, however, will change both an individual's own valuation of  $p$  as well as their beliefs about others' valuation of  $p$ . This discussion suggests the following hypothesis:

*H3:* The more public the information signal, the greater the treatment effect on voter behavior.

## Research design

This project examines the impact of a new kind of information on voter behavior by manipulating voter information through a randomly assigned intervention in one emerging democracy, Mali. Preliminary research in Mali suggests why the type of information provided in previous empirical studies and experiments may not be sufficient to induce change in voter or government behavior: voters have systematically low expectations of government. This is not a surprising finding in light of a number of features of Mali that are often consistent with other young democracies.

In a new democracy, voter expectations are largely informed by the behavior of previous governments. In the case of Mali, the previous government was a military regime which was not representative of or responsible to its subjects. If elected officials continue to perform poorly, voters will have no reason to update their expectations of government. This is particularly likely in countries like Mali with low levels of education and poor information infrastructure such that people have little opportunity to learn about the new system of democratic governance. Many newly democratized nations are also relatively unequal and have small middle classes such that interests of the powerful elite diverge from the poor majority. Further, this poor majority was on the sidelines of the transition to democracy in Mali which occurred due to a combination of outside pressure and elite bargaining rather than a concerted push from below.

Democratic since 1992,<sup>5</sup> Mali remains highly rural and economically underdeveloped. Malians are twice as poor and half as literate as those in the average sub-Saharan African country, with a literacy rate in Mali of 24 percent and GNI per capita at 500 USD. This apparent failure of democracy to improve development outcomes cannot be attributed to weaknesses in the formal democratic institutions

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<sup>5</sup>On March 22, 2012, Mali underwent a military coup jeopardizing its status as a stable democracy. After about three weeks, the country was returned to civilian rule albeit with continuing involvement from the junta responsible for the coup.

themselves. Not only have Malian elections been deemed free and fair, but there have been two peaceful transitions of power between parties and there is relatively high media freedom and freedom of association. To better understand how democracy is failing in Mali, I examine the role of voter low expectations in reducing government accountability. I do so by systematically raising voter expectations and studying the effect.

A Malian policy innovation permits me to manipulate voter beliefs within independent and locally-governed units. In 1996, Mali decentralized politically and territorially. The 703 newly demarcated *communes* would democratically elect councils with autonomous control over local budgets. The average population of a rural commune is 13,000. Cross-sectional data collected by the government demonstrate there is significant variation in local government performance in the extent to which public goods are provided. One analysis suggests some of that variation can be attributed to access to information and political competitiveness at the local level (Gottlieb, 2010).

While local government decisions occur at the commune level, organizational capacity exists at an even lower level – the village. Communes are subdivided into an average of 15 villages. A customary village chief resides in each village and is an important interlocutor between the villagers and the commune government. For example, village chiefs frequently attend budget and planning meetings and play an important role in tax collection. Communes vary greatly in population density and size. In the largest communes, the distance from one village to another can be as much as a day’s walk. The intervention will be carried out at the village level rather than at the commune level to ensure broader access to information.

In the following sections I describe a field experiment in rural communes in Mali to test the theory that information that sufficiently raises citizen expectations can have an important effect on governance. Because I randomly assign groups to treatment, I overcome endogeneity problems which are of concern in this case. In the absence of random assignment, the treatment effect would likely be biased upward. Well-informed voters may have more information precisely because their government is better behaved. Or some unobserved factor such as strong social networks may cause both informed citizens and well-behaved governments. By randomly assigning an informational “treatment,” I can identify whether information deficits have an important effect on failures of democratic accountability at the local level in Mali.

Mali is in some ways an easy test of the hypotheses because voters are particularly under-informed and have demonstrated low expectations of local government. As a result, the generalizability of the findings are limited to places that, like Mali, exhibit low levels of information about government capacity. The findings may be most relevant in countries that are also decentralized and where voters are uncertain

about the division of responsibilities between different levels of government. In addition, rural and isolated populations are likely to be most affected by information asymmetries regarding government budgets and government authority.

### **The treatments: two types of information interventions**

Both treatment conditions consist of a civics course conducted at the village level. Voluntary participants receive a series of 3-hour sessions conducted by a trained Malian instructor in the local language. This mode of dissemination rather than radio or posted signs was motivated by the low level of comprehension found in preliminary surveys. A review of existing civic education efforts in Mali found a lack of any systematic form of civic education.<sup>6</sup> Students receive some information about government in high school, but fewer than 3 percent of respondents in the household survey report having finished secondary school. Course sessions strike a balance between prepared materials (pre-recorded audio and color posters) to maximize homogeneity of treatment, and interactive exercises (role plays and question and answer sessions) to maximize comprehension among participants.

There are two treatment groups (T1, T2) and one control group of equivalent sizes. The control group does not receive any intervention. T1 receives basic civic information over 2 course sessions while T2 receives an additional session on relative government performance, totaling 3 course sessions. In the first two course sessions, participants learn about the average size of local government budgets in their region. To facilitate comprehension, participants are given examples of the types of goods that can be provided with that amount of money. Participants learn about the division of responsibilities among the different levels of government, in particular the responsibility of local government to provide local public goods such as primary health care, access to clean water and primary education. Finally, information is provided about the basics of the democratic process such as how elections work and the right of citizens to obtain public information and participate in public meetings.

The addition of relative performance information in the second treatment condition is partly motivated by the fact that voters may be subject to two types of information asymmetries. If voters lack information not only about government capacity, but also about actual government performance, then the first intervention may not be sufficient on its own. Even high aspirations among citizens may have no effect if voters cannot properly evaluate whether government has met their expectations. Participants receive information about how their government performed relative to other local governments, some of which is collected by the course participants themselves. Performance indicators include the number of

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<sup>6</sup>Civic education was officially eliminated during the Traoré regime in 1972 and reintroduced in 2009. Evidence of this reintroduction was not apparent at the time of the study.

public meetings held by the commune council, the distribution of projects between the commune seat and outlying villages, the number of development projects in the village funded by the commune, and the rate of tax recovery in the village.

Another motivation for studying these two types of interventions is practical since policymakers may be faced with a choice between implementing a civics course or collecting and disseminating performance data on politicians. The two interventions are not entirely distinct theoretically, however, because relative performance information not only gives a sense of the incumbent's performance record, it also helps set voter expectations. The design permits us to answer the following questions: 1) is the basic civic education component sufficient to change the political outcomes under study, and 2) does a combination of the two types of information interventions produce effects that are significantly different from just providing the first?<sup>7</sup> Resource constraints and power considerations precluded a third treatment group providing only relative performance information without the basic civics course. This means we cannot directly answer the question of whether relative performance information is sufficient to change the political outcomes we care about. However, if we assume additivity in the two treatments, we can conjecture whether the first type of information intervention is necessary for the second to be successful or whether the relative performance information can achieve significant effects on its own.

## Sample

The experimental sample consists of 95 communes in the five districts of Kati, Koulikoro, Segou, Macina, Baraoueli. Each commune is randomly assigned to one of three groups: control, T1 or T2. Without *a priori* expectations about the relative strength of the two treatments, I maximized power by making each of the three groups the same size. A blocked randomization design stratified the sample prior to randomization on three variables related to information provision and government accountability: geographic region, whether the mayor elected in 2009 is an incumbent, and a composite commune-level development index<sup>8</sup>. Due to budget and time constraints, studying all villages in each commune (average 18 villages per commune) was not feasible. Balancing requirements for power and practical considerations, I chose to randomly sample 6 villages per commune in each of the groups.<sup>9</sup> Six of the sample communes have fewer than 6 villages, so all villages are sampled in those communes. The total

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<sup>7</sup>Because no group received a 3-course treatment with only basic civics information (and no relative performance information), we cannot cleanly distinguish between the effect of the addition of relative performance information and the effect of a longer treatment.

<sup>8</sup>The development index is produced by UNDP's Observatoire de Développement Humain Durable and includes measures of electrification, telecommunication, population size and public goods.

<sup>9</sup>The village that serves as the commune seat is always included among the 6 villages. Because the consent of the mayor was required to work in the commune, and because the mayor generally resides or at least has close ties in the commune seat, I was advised that it would be politically difficult to exclude the commune seat from the sample.

number of villages in the experimental sample is 556.<sup>10</sup>

## Compliance

Participants voluntarily self-selected into the course following a village-wide assembly that provided details about the course and an open invitation to participate. Participants differ systematically from non-participants within the treatment communes. Some details about participants such as age and occupation were collected during the intervention itself. I combine this with additional data from the survey, inferring participation from answers to two survey questions about knowledge of a civics intervention in the village and participation in that civics intervention. About 43 percent of participants were women and the average age of participants was 45, higher than the average adult age of about 40 years old among survey respondents. Participants are better socially connected than non-participants: about 48 percent of self-reported participants say they are related to the chief compared to only 34 percent among other respondents. They are less likely to be from a minority ethnic group in their village and are more educated. 70 percent of self-reported participants say they attended some school while only 56 percent of other respondents report any schooling experience.

An average of 30 villagers participated in the course in each village. With a little over 1000 residents and 200 households in the average village, this is a small proportion of course participants. Therefore, it may seem surprising if any impacts of the intervention are detected by the survey instrument which samples a mostly random sample of households regardless of participation in the course. Because local leaders were assumed to be more likely to participate in the course, leaders are oversampled in the survey. In addition, tight-knit social networks within villages are an important conduit of information. In an experimental information intervention in Pakistan, for example, treatment effects were just as large on treated women as they were on their untreated female neighbors (Gine and Mansuri, 2011).

One characteristic of Malian villages that facilitates the spread of information is the *concession*, a compound or a grouping of households comprised of members of the same extended family. Households of the same concession live in close proximity, often enclosed by a single wall. The women generally prepare food together while the men will often farm the same or neighboring plots of land. The Pakistani experiment found that information only traveled successfully within networks of the same gender, which is likely similar in Mali. However, this experiment unlike theirs targets both men and women and succeeded in reaching almost equivalent numbers of each gender.

A follow-up survey in a random sample of treated villages in the Koulikoro region investigated the

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<sup>10</sup>One commune is missing observations for one of the six villages due to refusal by the chief to allow the enumerators to work there.



distribution of participants among concessions. Participants in the course came from concessions with 14 adult members on average. Accounting for concessions that sent multiple members to participate in the course, about 95 adult members of the village share a concession with at least one course participant. This is equivalent to about 38 percent of the reported adult population. Thus through the spread of information within concessions, treatment could have indirectly reached a relatively high proportion of the village, to say nothing of the potential for information to spread between concessions. During this follow-up survey, a small number of participants and non-participants were asked about information dissemination regarding the civics course. About half of participants said they spoke about the course to others, and about a third of non-participants said they learned something about the course from people in their community.

## Measurement strategy

A household survey measures levels of civic knowledge, beliefs about government capacity and behavior in hypothetical voting simulations<sup>11</sup>. The survey samples 10 households (one person randomly selected per household) in each of the 6 sample villages. Of the 10 households, 6 are selected randomly with gender balance among men and women and a randomization method that ensures geographic representation across the village. The remaining 4 household surveys are targeted at local leaders: the women’s leader, the youth leader, the head of the village association, and the village assistant elected during the civics course (or a reasonable alternative in control villages). These leaders were targeted with the intention of better capturing civic activity. The assumption is that if there are changes to civic activity in a given village, these leaders are more likely to be involved in such activities or at least know about them. This design also permits a study of the differential impacts of the course on leaders relative to ordinary households.

Though outcomes are measured at the individual, village and commune levels, analyses will use the commune as the unit of analysis (unless otherwise specified), averaging over outcomes when necessary. Accounting for blocked randomization, average treatment effects will be estimated using the following equation:

$$ATE_j = \frac{1}{N} * \sum_{b=1}^N (y_{T_j b} - y_{Cb})$$

where  $b$  is the block assigned to each commune in randomization,  $N$  is the total number of blocks (31 or 32),  $y_{Cb}$  is the average outcome in the control group in block  $b$ , and  $y_{T_j b}$  is the average outcome

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<sup>11</sup>Measuring actual voting behavior would be ideal, however, the next municipal election occurs only in 2014. While behavior in hypothetical voting simulations will not perfectly predict actual voting behavior, the survey measurement was designed in such a way as to minimize bias and the ability of the respondent to game the questions.

Table 1: Baseline comparison between randomly assigned groups

	Mean for control	T1 - C	T2 - C	N
Incumbent mayor	0.226	0.055 (0.111)	0.055 (0.111)	63
Wealth index	0.031	0.029 (0.183)	0.039 (0.185)	63
Arrondissement	0.387	-0.168 (0.116)	-0.106 (0.120)	63
Peri-urban	0.000	0.063 (0.044)	0.094* (0.053)	63
Majority party	0.258	0.054 (0.115)	0.148 (0.119)	63
Number of villages	17.645	-0.645 (2.717)	2.824 (3.360)	63

Standard errors in parentheses. Significance levels: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

in treatment group  $j \in \{T1, T2, T\}$  in block  $b \in [1, 32]$ . When comparing T to C,  $y_{T_j b} = \frac{y_{T_1 b} + y_{T_2 b}}{2}$ . Standard errors will be calculated using the standard deviation of the difference in means between  $T_j$  and C by block.

Because the number of sample communes is not divisible by 3, one block has 2 observations rather than 3. By random chance, these observations are in the 2 treatment groups. In general, the incomplete block will be dropped from the analysis.

For some analyses, I run additional specifications with controls at the commune, village, and individual levels. Commune-level controls include baseline characteristics on which the randomization was stratified – mayor incumbency and poverty level – as well as whether the commune is an arrondissement seat<sup>12</sup> or peri-urban<sup>13</sup>. Table 1 shows the difference in the mean value of these variables across groups. I include the two variables used for stratification since exact matching was not possible across all blocking variables simultaneously. The balance table also includes two additional variables that are used in testing for differential treatment effects: whether the commune council has a majority party and number of villages in the commune. In only one case is there a significant difference in pre-treatment characteristics: there are more peri-urban communes in T2 than control. Individuals in peri-urban environments should be better informed than those in rural areas due to better access to schools and information infrastructure. If it is the case that treatment is less effective for more informed participants, then the treatment should work less well in peri-urban communes, biasing against a significant finding.

<sup>12</sup>Arrondissement is the next higher administrative division after commune. Before decentralization, the arrondissement was the lowest level of administration. Communes that are the former seat of an arrondissement tend to be more developed because of prior investments in health, education and other infrastructure.

<sup>13</sup>I define a commune as peri-urban if it is both contiguous with Bamako and densely populated. There are 5 communes in the sample of 95 that fit this description. Based on observations made during the study, these communes are quite different from the rest of the sample. For example, employment is much higher (even if much of it is in the informal sector), many more residents are new migrants from rural areas rather than autochtones, and material wealth is greater.

Village- and individual-level controls are characteristics that may influence the outcomes of interest such as level of civic information and voting behavior, but should not be affected by treatment. Due to lack of data, these variables are measured at the time of the post-treatment survey. However, they are generally static and will proxy for baseline characteristics. Village-level characteristics include distance of the village to the commune seat and number of concessions in the village. Individual-level controls include age, schooling, whether the respondent is one of the four targeted leaders, gender, minority status<sup>14</sup>, radio listenership, assets (TV and vehicle), relationship to the chief, size of concession, and travel outside the village.

## Data analysis plan

After receiving the data but prior to analyzing it, I developed a data analysis plan that specified which measures I would use to test different hypotheses. As discussed by Casey et al. (2011), the registration of data analysis plans prior to analyzing results ties the hands of researchers and prevents cherry-picking of positive results. Appendix 1 features a table linking hypotheses to measurable outcomes and summarizing the results of each test. This paper does not discuss all hypotheses, but I include them in the appendix for completeness. This paper also focuses on the measurement strategies that are most robust or least likely to suffer from bias or measurement error. For example, I privilege the analysis of survey experiments and observations of actual behavior over traditional survey questions.

## Data analysis

### Did the intervention increase voters' valuation of the performance dimension?

I use evidence from two voting simulations conducted during the survey to evaluate hypotheses about the effect of information on voter behavior. These exercises were designed to minimize the effect of survey biases on the estimation of treatment effects. Because respondents in the treatment group may have adopted new ideas about normatively “correct” behavior as a result of the treatment, they may be differentially subject to social desirability bias or desire to please the enumerator. As a result, treatment effects generated by some survey questions may reflect changes in norms about what is socially desirable rather than changes in honest reports of past or future behavior.

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<sup>14</sup>63 percent of survey respondents belong to the Bambara ethnic group with the next largest group being the Peulh at 12 percent. Villages tend to be relatively ethnically homogenous, so I am more interested in whether someone is a minority within a village rather than whether they are a minority ethnic group among Malians on the whole. For lack of disaggregated census data on ethnicity, I code a survey respondent as a minority if they belong to an ethnic group to which less than a quarter of the other survey respondents in that village belong. Using this coding scheme, 12 percent of respondents are coded as minorities.

One example of a treatment effect on respondent norms reflected in biased survey responses is the self-reporting of campaign gifts. The survey asked whether the respondent had ever received a gift from a candidate during an election. About 57 percent of individuals in the control group said they never had while this number was higher in T1 (61%) and T2 (65%). The difference between the control group and T2 is significant at conventional levels, and differences become greater and more significant with the introduction of pre-treatment covariates. Due to randomization and the fact that all previous elections occurred prior to treatment, there should be no significant difference in actual gift-giving between treated and control communes. The difference in self-reporting implies that treatment strengthened the norm against vote-buying or for performance-based voting, causing people to be less likely to admit to receiving gifts from candidates.<sup>15</sup>

The voting simulations are constructed so that the respondent cannot easily game the question or infer what is socially desirable. In each voting simulation, the respondent is given a choice between two candidates who are described by the enumerator. The first simulation measures the respondent's valuation of a high-performing candidate by assessing the price at which their vote can be bought by a low-performing candidate. There is evidence that treatment makes votes more expensive to buy. The second simulation measures the salience of two dimensions – shared kinship and the village chief's political preference – relative to the performance dimension. Here, evidence suggests that treatment makes the performance dimension more salient relative to these other voting criteria.

***H1: Increasing information about potential government performance will increase the cut point at which poor-performing candidates are sanctioned***

In the theory presented earlier, the cut point  $k$  at which voters sanction incumbents is increasing in  $p$  in both the “peanuts” and “big ticket” equilibrium. If the treatment increased voter knowledge with respect to local government capacity, essentially an exogenous positive shock to  $p$ , we should expect to see voters sanctioning at higher cut points in the treated groups. To test this, a voting simulation in the survey aims to estimate the voter's valuation of a high-performing candidate relative to a low-performing one. Using a monetary scale, I assess the voter's willingness to pay for a good candidate, or more precisely, willingness to accept a gift from a bad candidate in exchange for their vote.

The survey employs a voting simulation which gives respondents a choice between a high-performing candidate and a low-performing candidate in a hypothetical election.<sup>16</sup> If the respondent initially chooses

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<sup>15</sup>While the course highlighted the importance of voting for a candidate based on performance, there was no explicit discussion of vote-buying.

<sup>16</sup>The “high-performing” candidate is described as the current mayor who built a development project in the commune every year of his mandate, but does not give gifts to supporters. The “low-performing” candidate is described as someone who only makes promises to do better than the current mayor, saying he will build wells in all villages in the commune if

the high-performing candidate, the low-performing candidate offers the respondent a monetary gift in exchange for his vote. If simply asked about willingness to sell one’s vote, social desirability bias would likely have caused more voters in the treatment group to refuse than in control. To minimize this bias, I created a scale of prices at which the respondent could sell their vote. Because the respondent was not aware of the elements or limits of this scale, intuiting the most socially desirable response is difficult.

To evaluate Hypothesis 1, I examine the average price at which respondents are willing to sell their vote. Choosing the high-performing candidate at higher offers from the low-performing candidate is equivalent to sanctioning the low-performing candidate more often, or under a wider range of parameters. Thus, I generate support for the hypothesis if treatment causes the voters to choose the high-performing candidate at higher offers from the low-performing candidate.

When initially given the choice, 74 percent of survey respondents choose the high-performing candidate.<sup>17</sup> these respondents are asked whether they would switch their vote to the other (low-performing) candidate if he offered a gift of about 1 USD. If the respondent refuses, there are 3 consecutive amounts offered up to 20 USD. On average, 22.5% of these respondents agree to switch their vote for some amount of money<sup>18</sup> – slightly but insignificantly more in the control group (23.9%) than the 2 treatment groups (21.7%, 22.8%).

Table 2 shows treatment effects on the average price for which a vote can be bought by the low performing candidate in each treatment group. This analysis is performed only among respondents who said they were willing to sell their vote at some price.<sup>19</sup> The first column provides evidence of the average treatment effect for the full sample. The second column restricts the sample to those respondents who received this voting simulation first out of two. To mitigate priming effects and the influence of behavior in the first simulation on the second, the order of the two simulations was randomly assigned for each survey prior to implementation. The simulation could occur either at the beginning of the survey, just after questions about demographic information, or toward the end.

I find evidence of a positive effect of treatment on the price the low-performing candidate must pay to buy the respondent’s vote. The magnitude of the coefficients are larger for T2 than T1 and larger in the restricted sample than in the full sample. Only the coefficient on T2 in the restricted sample is significant at conventional levels. The difference in vote price between T1 and T2 is not statistically

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elected.

<sup>17</sup>This relatively high vote share for the low-performing candidate may be attributed to anti-incumbency bias which is quite high in Mali, or the fact that the “high-performing” candidate was explicitly said not to give gifts to supporters.

<sup>18</sup>Significant predictors of willingness to switch one’s vote include being a woman, having less education, and not being a leader. Interestingly, respondents of ethnic minority status in the village are significantly less likely to say they could be bought off. This result supports the idea that gifts are less credible or a less meaningful signal when the giver is from a different ethnic or kin network.

<sup>19</sup>The same pattern of results holds when different prices are imputed for missing values or respondents who were unwilling to sell their vote.

significant. These patterns hold with the inclusion of controls. The standard deviation of the dependent variable is 3.16 in the restricted sample and 4.00 in the subsample. T2 increases the price by about a half of a standard deviation or 1 to 2 USD. For the more than half of rural Malians living on less than a dollar a day, this is not an insignificant sum.

Table 2: Treatment effect on willingness to accept gift for vote (in USD)

	Full sample	Subsample
Control mean	8.33	7.20
T1 (ATE)	0.63 (0.672)	0.839 (0.778)
T2 (ATE)	1.35 (0.908)	2.15* (1.11)

Standard errors in parentheses.

Significance levels: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**H2: Increasing information about potential government performance will increase the likelihood of voting along the performance dimension**

The proposed theory predicts that low values of  $p$ , or a low enough likelihood that there are sufficient funds in the budget for a public good project in their village, will make the “big ticket” incentive scheme less feasible. The information intervention should thus increase the likelihood that a voter uses the “big ticket” electoral strategy. A second simulated election between new Candidates A and B examines whether the respondents in the treated group are more likely to vote based on performance criteria.

To minimize the effect of social desirability bias, I implement a survey experiment in which each respondent is randomly assigned to one of three versions of the candidate descriptions: baseline, kin and chief. The only difference between the versions is that in the kin condition, the Candidate A is given the same last name as the respondent to signal kinship. In the chief condition, the Candidate A is given the support of the village chief. These cues are designed to be subtle and the respondent receives only one version of the candidate pair so is not cognizant of the experimental manipulation.

The two candidates described to the respondent are purposefully ambiguous on the performance dimension so that effects of the kin and chief cues can be better observed. Candidate A built a well during his tenure as mayor – a concrete but relatively meager public goods record, while the Candidate B held annual public budget debates during his tenure as mayor. Once read the candidate pairing, the survey respondent is asked to choose his preferred candidate.

The following analysis focuses on the change in mean vote share for Candidate A across each of the survey conditions. Table 3.A presents the mean vote share for Candidate A across each condition. Assuming that kinship and chief support are two salient dimensions for Malian voters, the mean vote

Table 3: Effect of treatment on voting criteria

A. Mean vote share for Candidate A			
	Control	T1	T2
Baseline	0.260	0.287	0.400
Kin	0.380	0.364	0.385
Chief support	0.340	0.308	0.367
B. First differences			
	Control	T1	T2
Difference (Kin - Baseline)	0.120*** (0.025)	0.077** (0.033)	-0.015 (0.028)
Difference (Chief - Baseline)	0.080** (0.030)	0.021 (0.027)	-0.033 (0.035)
C. Difference-in-differences			
	T1 - C	T2 - C	T2 - T1
Kin condition	-0.045 (0.045)	-0.135*** (0.037)	-0.092** (0.045)
Chief condition	-0.062 (0.040)	-0.116** (0.047)	-0.054** (0.039)

Standard errors in parentheses. Significance levels: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

share for Candidate A is expected to be higher in the kin and chief support conditions compared to the baseline. Table 3.B presents the differences in means between each survey condition. Within the control group, Candidate A receives a significantly higher share of the votes in the kin and chief conditions compared to the baseline condition. Within treatment groups the only significant difference is between the kin a baseline conditions for T1.

Because treatment may have affected voter preferences over candidates in the baseline, the relevant test is whether the difference in vote share for Candidate A between conditions (kin/chief vs. baseline) is different in the treatment groups compared to the control. If treatment increases the salience of the performance dimension relative to the kin and chief dimensions, then there should be a smaller difference in vote share for Candidate A within the treatment conditions. A difference-in-difference estimation analyzes treatment effects on whether the change in Candidate A's vote share across survey conditions varies significantly with treatment.  $H2$  is confirmed if the difference in vote share between the baseline and other conditions is smaller for the treatment groups than for control.

The difference-in-difference estimates in Table 3.C reveal a negative treatment effect on the likelihood of respondents to vote along the kinship or chief support dimensions.<sup>20</sup> Voters in the control group were

<sup>20</sup>An additional indication of the relative valuation of the performance dimension by survey respondents is how they rank voting criteria. About 63 percent of people in the control group rank performance first (before gift-giving, kinship, and village chief support) compared to about 65 percent of respondents in each treatment group. The positive effect of treatment is not significant at conventional levels in either case. Priming may be a concern. If prior survey questions primed respondents to what was socially desirable, then the high rates at which people say they prioritize performance may mask underlying treatment effects.

more likely to vote for Candidate A when he was either a kin or had the chief's backing than they were in the treatment groups. The difference-in-difference estimators are significant for T2 but not for T1. In addition, the difference between the effect of T2 and T1 is significant in this case. The same patterns hold when the sample is restricted to only those respondents who received this voting simulation first in the survey, except that the difference-in-difference estimates are generally greater in magnitude.

The kin and chief dimensions should be most salient for people who stand to benefit from membership in a local social network. For instance, aligning with the chief's preference would only be a good strategy for someone who believed their well-being to be linked with the chief's well-being. The survey asks whether the respondent is a relative of the chief which is a strong indication of membership in local social networks. As expected, the kin and chief dimensions are more salient among respondents who say they are relatives of the chief. In the control group, Candidate A receives 22 percent more of the votes under the kin condition compared to the baseline condition among the chief's relatives compared to only a 7 percent bump among non-relatives. Re-running the difference-in-difference analysis featured in Table 3 among the subset of self-reported relatives of the chief (39 percent of the respondents), treatment effects are larger and more significant. The difference-in-difference estimators become significant for T1 where they were not in the full sample of respondents. This analysis indicates that treatment has a larger effect on increasing voting along the performance dimension when these non-performance dimensions are more salient.

### **Did the intervention change behavior?**

To assess whether these behavior changes in hypothetical situations are reflected in actual changes in behavior, I discuss the effect of treatment on one observable outcome we might expect to change with treatment. While the ballot box is one place the voter can challenge poor performers, there are opportunities to do so in between elections. In recent years, Mali's Ministry of Territorial Administration along with the German aid agency (GIZ) have been promoting an annual town hall meeting called a "restitution publique." During these meetings, commune leaders provide a financial and administrative account of the previous year to members of all the villages in the commune. This is one of the only formal opportunities for villagers to exchange with commune leadership.

According to GIZ, substantive participation in past meetings has been quite low which they attribute to lack of information and fear of speaking in public. Using the subsample of communes that conducted town hall meetings following the civics course, I assess treatment effects on observed villager behavior during the meetings. Trained observers recorded details of the way in which attendees at the meetings participate. Here, I analyze the extent to which participants intervened during the meetings to challenge



their elected leadership in some way.

Because the subset of communes receiving a town hall meeting is not a random sample, we have to rule out the possibility of selection bias. GIZ originally budgeted for the entire experimental sample to receive a town hall meeting between March and June of 2011 (shortly after the completion of the civics course). Due to budget constraints, they funded meetings in only one-third of communes in the original experimental sample. The criteria they used in selecting these communes included the recency of previous town hall meetings, organizational capacity in the geographic area, and relationships with higher-level authorities. While some of these criteria might influence our dependent variable of civic participation at the meetings, all of them are orthogonal to whether or not a commune was treated. A balance test on pre-treatment characteristics reveals no significant difference in the communes that received and did not receive a town hall meeting on any dimension including poverty level, population size, mayor incumbency, competitiveness of elections, presence of local radio, or remoteness.

In the subsample of communes holding a town hall meeting, 8 are in the control group, 13 in T1, and 12 in T2. Trained observers who were blind to treatment status attended each meeting and recorded details of all participant interventions. For the dependent variable of interest, number of challenges to leadership, I code as 1 each time a participant at the meeting intervened with a complaint or challenge to the commune government and aggregate them at the commune level. Regressing this dependent variable on an indicator of treatment, Table 4 reports the average treatment effect on the number of town hall participant interventions per commune. Because of the small number of observations, exact p values are also calculated using randomization inference and compared to the asymptotic approximations. The effect is positive in every case, with treatment increasing the number of challenges on average by almost half. The effect is only significant at the 10 percent level when comparing the pooled treated groups with the control, which is not surprising given the very small sample size.

Table 4: Average treatment effect on challenges to leadership during town hall

Mean number of challenges by group			
	Control	T1	T2
Challenges	4.125	5.93	5.83
N	8	13	12
Mean differences			
	Difference	p value (2-sided)	exact p <sup>†</sup>
T1 - C	1.80	0.125	0.148
T2 - C	1.71	0.109	0.178
T - C	1.76	0.086	0.113

<sup>†</sup>Exact p values calculated using randomization inference.

Women were far less likely than men to intervene during the town hall meetings. Only about 5 percent of all meaningful interventions (questions, comments, challenges) came from female participants. Of the

three women in the sample who openly challenged their leadership during the meetings, all were from treated communes and originated from villages that received treatment.

## **Mechanisms**

The analyses of hypothetical voting behavior suggest that the civics course increased the value and salience of the performance dimension. If public goods are strictly preferred to private goods such as gifts or patronage, two plausible mechanisms may explain how the civics course is affecting voting behavior: raised expectations and improved voter coordination. First, treatment may be increasing the voter's individual valuation of public goods relative to private goods through higher expectations of feasible local public goods policies. Second, treatment may be increasing the voter's expectation that other voters will opt for the public goods candidate, thus improving the ability of voters to coordinate on their jointly preferred option. Both mechanisms are explored using available data.

### **Raising expectations**

Following the data analysis plan, a composite index of relevant survey items is created to perform a mean effects analysis of treatment effects on voter expectations. Component survey questions include whether the local government is responsible for providing a range of public goods, beliefs about the size of the local budget and number of expected future projects, and beliefs about how democracy works. Since there are multiple outcomes assessing a single hypothesis, I perform a mean effects analysis using the summary index. As suggested by Anderson et al. (2008), the component outcomes that comprise the index are demeaned by subtracting the control group mean and then converted into effect sizes by dividing by the control group standard deviation. The expectations index then equally weights each component.

Table 5 reports treatment effects on the expectations index using a variety of econometric specifications to allow for the inclusion of covariates. Model 1 is the basic specification, using a paired t-test as described in the measurement strategy. Models 2-5 test whether any treatment effect is robust to the inclusion of control variables. Models 2-5 are regressions of the expectations index on treatment status employing fixed effects for block to account for the stratified randomization of communes to treatment groups. Model 2 adds enumerator fixed effects. Model 3 adds commune-level controls. Model 4 adds village-level controls and Model 5 individual-level controls.

In Models 1 - 3, the unit of analysis is the commune or commune pair. In Models 4 and 5, the unit of analysis is the individual. Because randomization occurred at the level of the commune and not the individual, the model accounts for clustering of individuals within villages and villages within communes.

Green and Vavreck (2008) analyze alternative estimation approaches for cluster-randomized experiments and conclude that random effects regressions produce standard errors that are more reliable than robust cluster standard errors. While random effects regressions produce treatment estimates that are more efficient than aggregate-level OLS, they find standard errors are downwardly biased, particularly when the number of clusters is small.

To permit the inclusion of village- and individual-level controls and to compare treatment estimates across specifications, Models 4 and 5 analyze individual-level data with a mixed model fit using restricted maximum likelihood.<sup>21</sup> Random effects are modeled at the commune and village levels to account for dependence among individuals within villages and among villages within communes. Here, random rather than fixed effects are used because communes and villages were chosen at random from a larger set so we can assume that specific commune or village effects are uncorrelated with assignment to treatment.

Commune-level effects of treatment  $y_c$  are estimated using the following equation:

$$y_c = \beta_0 + \beta_1 T1_c + \beta_2 T2_c + X'_c \Pi + W'_c \Gamma + \varepsilon_c$$

where  $X_c$  is a vector of commune-level controls and  $W_c$  is a fixed effect for block, the unit on which randomization was stratified.

Individual-level effects of treatment  $y_{ivc}$  for individual  $i$  in village  $v$  are estimated using the following equation:

$$y_{ivc} = \beta_0 + \beta_1 T1_c + \beta_2 T2_c + X'_c \Pi + Z'_{vc} \Gamma + S'_{ivc} \Sigma + W'_c \Gamma + \alpha_c + \gamma_{vc} + \varepsilon_{ivc}$$

where  $Z_{vc}$  is a vector of village-level controls,  $S_{ivc}$  is a vector of individual-level controls,  $\alpha_c$  are random effects for commune and  $\gamma_{vc}$  are random effects for village.

Estimated treatment effects are all positive, but they are only significant at conventional levels in Model 4. The treatment effects account for about a one-quarter to one-third standard deviation change. There is no significant difference in the outcome measure between T1 and T2. Interestingly, many of the covariates have significant relationships with the expectations index. On average, more distant and smaller villages have much lower expectations of the commune government. Men, leaders, people with some formal schooling, people who spend more of their time in the commune, wealthier respondents and those who listen to the radio all have higher expectations on average.

### Improving voter coordination

The theory section described the voter's choice as a coordination problem in which he can get a small value with high probability if he chooses a candidate giving gifts or patronage, or he can get the higher-

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<sup>21</sup>I use the `xtmixed` command in Stata.

Table 5: Effect on expectations index

Variable	Coefficient				
	M1	M2	M3	M4	M5
T1	0.031 (0.036)	0.027 (0.024)	0.028 (0.026)	0.046* (0.028)	0.047 (0.028)
T2	0.015 (0.036)	0.040 (0.024)	0.037 (0.026)	0.047* (0.028)	0.045 (0.028)
Incumbent			-0.015 (0.129)	-0.032 (0.136)	-0.093 (0.141)
Wealth index			0.009 (0.113)	0.043 (0.120)	0.019 (0.121)
Arrondissement			0.030 (0.038)	0.034 (0.040)	0.036 (0.040)
Peri-urban			0.084 (0.066)	0.017 (0.072)	0.032 (0.073)
Village distance				-0.024*** (0.005)	-0.019*** (0.005)
Concessions				0.000** (0.000)	0.000* (0.000)
Age					-0.005 (0.003)
Schooling					0.053*** (0.008)
Leader					0.019* (0.010)
Woman					-0.154*** (0.010)
Minority					-0.002 (0.014)
Radio listening					0.018*** (0.003)
Own TV					0.021* (0.011)
Own vehicle					0.019* (0.011)
Chief relation					0.015 (0.010)
Concession size					-0.001 (0.002)
Travel					-0.028** (0.013)
Intercept	0.002 (0.026)	0.057* (0.031)	0.061 (0.042)	0.133 (0.111)	0.077 (0.114)
N	31	95	95	5,452	4,813
Enumerator FE	N	Y	Y	Y	Y

Standard errors in parentheses. Significance levels: \* p<0.10, \*\* p<0.05, \*\*\* p<0.01.

valued public goods only if enough other people coordinate on voting for the better-performing candidate. The civics course could plausibly increase voters' ability to coordinate during elections. It could improve voter expectations of how other people will vote by providing public information to a group of voting-age citizens in a village that diffuses to some extent through communication after the course. It may also generate new norms regarding public communication about political issues. Alternatively, voters in a village may already be coordinating on voting for a shared kin or on the chief's preferred candidate. If this is the case, the treatment which was shown to reduce reliance on these two dimensions in favor of performance could also reduce the extent to which voters coordinate on voting for a kin or the chief's candidate.

These implications are tested in the data by examining responses to the second voting simulation. Respondents were not only asked which candidate they would vote for, but what they predicted the candidate vote share in their village would look like. One aspect of voter coordination is assessed by comparing voter predictions of the vote share for Candidate A with actual vote share for Candidate A in their village. Recall that voters received three different versions of Vignette B (baseline, kin and chief support conditions), so the analysis will also be conducted separately among participants receiving the same version.

Accuracy of prediction is measured by the variance of voters' guesses around the true vote share. Comparing the mean squared error or mean distance of the voter's prediction from the truth in each treatment group provides one indication of ability to coordinate. Table 6.A reports the effect of treatment on the mean squared error for each of the three versions of the survey as well as pooling all versions together. In every case, the treatment effect is negative, or voters are better at predicting vote share after treatment. Evidence of a treatment effect is much stronger for the baseline condition than the kin and chief conditions, and only the effect of T2 is significant at conventional levels. With the addition of controls, the treatment effect becomes larger and more significant for the baseline condition and smaller and less significant for the kin/chief conditions.

Within treatment groups, the control group is better at guessing in the kin/chief conditions than in the baseline condition. In general, the treated groups have less accurate guesses in the kin/chief conditions than the baseline (except in T1, guesses in the chief condition are slightly more accurate than in the baseline). To show this, I perform a difference-in-difference analysis in Table 6.B. Compared to voters in the treated groups, voters in the control group are better able to coordinate in the kin and chief conditions compared to the baseline condition (recall that smaller coefficients imply better coordination). The difference-in-difference estimator is significant for T2 but not for T1. One interpretation of these results is that the performance dimension is most salient in the treatment groups, causing treated voters

Table 6: Effect on voter coordination within villages

A. Mean squared error around true vote share for Candidate A			
	Baseline	Kin condition	Chief condition
Control mean	0.146	0.122	0.130
T1 (ATE)	-0.027 (0.020)	-0.012 (0.013)	-0.006 (0.016)
T2 (ATE)	-0.041** (0.019)	-0.007 (0.011)	-0.004 (0.015)
B. Difference-in-differences			
	Kin - Baseline	Chief - Baseline	
T1 - C	0.016 (0.015)	0.022 (0.014)	
T2 - C	0.034** (0.015)	0.035** (0.014)	

Standard errors in parentheses. Significance levels: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

to coordinate better in the baseline condition; however, the kin/chief dimensions are most salient in the control group, causing treated voters to coordinate relatively worse in the kin/chief conditions compared to the baseline condition. These countervailing effects may explain why we do not see a significant treatment effect on coordination within the kin and chief conditions.

## Differential effects

This section tests whether treatment effects depend systematically on the strength of the treatment, initial levels of information, and the level of party competition in a commune. The theory generated one observable implication for differential treatment effects related to the public-ness of the information signal. I operationalize a test of this hypothesis and analyze two additional heterogeneous treatment effects. First, since treatment is working through the introduction of new information, then treatment should have greater effects on less informed populations. In Kenya for example, a national civic education program was found to have greater effects on Kenyans with less education and less access to information than the average (Finkel and Smith, 2011). Second, there are competing hypotheses regarding the relationship between political competitiveness and access to information, so I test whether treatment works better in more or less competitive places. The experiment was designed to capture main effects of the civics course. Further slicing the data in search of differential treatment effects risks diluting the power of the original analysis: as the number of hypotheses tested in the data is multiplied, so too are the chances of finding confirmatory tests. As such, evidence for all four heterogeneous treatment effects should be considered suggestive and not given the same weight as evidence for the main effects.

### **H3: The more public the information signal, the greater the impact on voter behavior**

The main results provide evidence that the information intervention increased an individual's likelihood of voting for the higher-performing candidate. If it also increased an individual's expectation that other people would change their voting behavior in the same way, then a stronger or more widely dispersed signal should elicit greater effects. This prediction is tested in the data by measuring treatment strength as proportion of villages per commune treated.

The civics course is a public signal that was provided to some proportion of the relevant voting bloc of citizens in every commune. The participants in the course were made aware of how many other villages in their commune received the course. Because only six villages were treated in every commune and communes vary in size, some communes had a majority of their villages treated while some had a minority of their villages treated.

Table 7 reports treatment effects conditional on the proportion of villages per commune treated. I regress the dependent variable of the price at which the respondent is willing to sell their vote to the low performing candidate (in USD) on the treatment indicators and an interaction term between the treatment indicators and an indicator of whether a majority of villages in the commune received treatment. Because communes in which a majority of villages are treated are also smaller in size, the effects might be driven by some other aspect of small communes such as wealth or population size. To address this, a wealth index and population size are controlled for in Model 2. In Model 3, I introduce all individual, village, and commune level controls from the most fully specified model in Table 5. There is evidence of a differential treatment effect for T2: the coefficients on the interaction terms are large and positive for both treatment groups, but only significant for T2.

#### **Treatment effects conditional on baseline knowledge**

If treatment is indeed working through people acquiring new information about their government, then it should be less effective among people or places that are already well-informed. Since the experiment did not include a baseline survey, prior information levels are not directly observed. Comparing treatment effects conditional on how respondents answered survey questions about civic information is problematic because the treatment impacted self-reported levels information as well as the outcomes of interest in similar ways. As a proxy for pre-treatment levels of information, I use an indicator of whether the survey respondent came from a commune seat or not. This does not suffer the same endogeneity problems as post-treatment measures of information because treatment should not affect whether a survey respondent lives in the commune seat. Residing in the commune seat is highly correlated with reported levels of

Table 7: Effect of information conditional on signal strength on WTA (in USD)

Variable	Coefficient		
	M1	M2	M3
T1	-0.082 (1.026)	-0.181 (1.015)	-1.294 (1.271)
T2	-0.778 (1.030)	-1.161 (1.013)	-1.424 (1.177)
Villages $\leq$ 12	-3.609*** (1.336)	-3.725*** (1.305)	-3.349* (1.745)
Majority x T1	2.307 (1.657)	2.211 (1.625)	3.250 (2.169)
Majority x T2	5.364*** (1.889)	5.373*** (1.853)	5.721** (2.830)
Population		0.000 (0.000)	0.000 (0.000)
Wealth index		-5.993* (3.116)	-4.995 (3.460)
Intercept	9.704*** (0.734)	9.552*** (0.890)	8.157** (4.091)
N	95	95	811
R <sup>2</sup>	0.175	0.250	
Other controls	N	N	Y

Standard errors in parentheses.

Significance levels: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

information: individuals in the commune seat score one-third of a standard deviation higher on the information index.

Conditioning on residence in the commune seat, I examine differential treatment effects on the willingness-to-pay question. First, people residing in the commune seat were less willing to sell their vote at any price. 90 percent of respondents residing in the commune seat never agreed to sell their vote compared to only 83 percent of respondents residing outside the commune seat. Table 8 reports treatment effects on willingness to ever sell one's vote in columns one and two. Among outlying villages, there is evidence of a positive effect of both treatment conditions, significant only for T2. For respondents in the commune seat, there is no evidence of a significant treatment effect and coefficients go in the opposite direction.

Because of this significant difference in the proportion of people willing to sell their vote at some price, I take it into account when measuring treatment effects on the willingness-to-accept outcome. Instead of dropping observations for people unwilling to sell their vote as before, I assign them a high price. The price I choose for this test is one that would have come next on the price scale, or double the highest price that was offered. The test works similarly for higher prices, but the higher the price, the less we observe variation in other responses. Columns 3 and 4 report treatment effects on this new willingness-to-accept variable for the two populations of interest: respondents residing inside and outside the commune seat.



Here again, there is evidence of a positive treatment effect in outlying villages, significant only for T2, and no effect for respondents residing within the commune seat. Respondents living in and outside of the commune seat differ on other dimensions as well, so additional causal mechanisms could be working in the same direction. However, given the distances between the commune seat and outlying villages as well as the concentration of information infrastructure inside the commune seat, I argue that this is a reasonable proxy for prior levels of information which should explain at least some of the variation behind the heterogeneous treatment effects.

Table 8: Effect of information conditional on residence in commune seat

	Willingness to sell vote		Price of vote	
	Outlying villages	Commune seat	Outlying villages	Commune seat
Control mean	0.813	0.911	15,549.79	18,230.38
T1	0.018 (0.023)	-0.016 (0.041)	516.168 (553.894)	-229.301 (778.475)
T2	0.032* (0.019)	-0.023 (0.032)	1104.568 ** (451.807)	-327.419 (626.778)
N	95	95	95	95

Standard errors in parentheses. Significance levels: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

### Information conditional on competition

Increased levels of competition in democracies has been shown to reduce corruption (Ferraz and Finan, 2011; Rose-Ackerman, 1978), undermine clientelism (Magaloni et al., 2007; Weitz-Shapiro, 2012), increase pro-growth policies (Besley et al., 2010), and limit state exploitation by political parties (Grzymała-Busse, 2007). We might also expect that higher levels of competitiveness increase a voters likelihood of voting based on a performance dimension since sanctions are more credible in a competitive environment. The relationship between information and competition is less clear. One could reasonably argue that they are substitutes or complements in the production of accountability. On the one hand, party competition may substitute for voter access to information because parties will discipline each other and provide useful performance information to voters. If this were the case, then information interventions would be less effective in competitive systems because voters already get the information they need from competing parties. On the other hand, party competition may complement voter access to information. For example, parties may be better able to compete and mobilize voters when costs to disseminating information are lower. Or if voters know they have a real choice between competitive candidates or parties, then information would mean more to them than if a single party had a monopoly on political power. If this were the case, then information interventions should work better in more competitive areas.

The variation in political competitiveness among Mali’s local governments provides an opportunity to test the nature of this conditional relationship. Mali’s local governments are run by a council of directly elected representatives and an indirectly elected mayor. Councilmembers are allocated seats based on proportional representation. Because the mayor and major policies are voted upon using majority rule within the council, a party with a majority of seats on the council has a virtual monopoly on political power. As an indicator of political competitiveness, sample communes are thus divided into those with a majority party on the commune council (about one-third) and those without.

Table 9 reports the conditional effect of information on both voter expectations and voter behavior. The dependent variable in columns 1 and 2 is the expectations index described earlier. The dependent variable in columns 3 and 4 is the willingness-to-accept measure indicating the amount a poor-performing candidate would have to pay to win a vote from the respondent. To assess the conditional treatment effect on these outcomes, I regress each independent variable on the treatment indicator, an indicator of whether there is a majority party on the town council and an interaction term. Models 1 and 3 are basic specifications in which the commune is the unit of analysis. Models 2 and 4 use all controls from the most fully specified model in Table 2, so the individual is the unit of analysis.

Table 9: Effect of information conditional on political competitiveness

	Expectations index		WTA (in USD)	
	M1	M2	M3	M4
T1	0.087** (0.041)	0.078** (0.038)	1.314 (1.184)	0.554 (1.577)
T2	0.104** (0.043)	0.100** (0.039)	2.190* (1.242)	0.929 (1.576)
Majority party	0.036 (0.066)	0.067 (0.057)	1.600 (1.919)	0.118 (2.318)
Majority party x T1	-0.179** (0.084)	-0.057 (0.077)	-1.497 (2.453)	0.531 (3.000)
Majority party x T2	-0.228*** (0.082)	-0.139* (0.075)	-0.926 (2.381)	2.461 (2.948)
Intercept	-0.009 (0.029)	0.004 (0.128)	6.778*** (0.839)	-0.603 (5.570)
N	95	4,813	95	392
R <sup>2</sup>	0.287		0.086	
Block fixed effects	Y	Y	Y	Y
Other controls	N	Y	N	Y

Standard errors in parentheses.

Significance levels: \* p<0.10, \*\* p<0.05, \*\*\* p<0.01.

The test provides strong evidence of a complementary relationship for the voter expectations index. This echoes the finding in Gottlieb (2010) in which access to local radio had a greater positive effect on local public goods provision where parties were most competitive. In Models 1 and 2, the coefficient on the interaction term is very large and significant for both T1 and T2 in the base model though

loses significance for T1 with the addition of controls. Further, the coefficient on the treatment effect is larger and more significant in this specification than when the interaction term was left out. There is no evidence of an interactive effect for the willingness-to-pay variable. The coefficient on the interaction term is negative as before, but it is small and insignificant.

These results can be interpreted to mean that treatment significantly increased voter expectations in communes where there is no majority party on the town council. Conversely, treatment significantly reduced voter expectations in communes where there is a majority on the town council, or little political competition. These divergent results were perhaps masking the presence of a treatment effect in the initial test without the interaction term. Although the experiment was not designed to robustly test differential treatment effects, post hoc reasoning can support why such different effects emerge. In places where voters have a real opportunity to vote out the incumbent, learning about the prospect of better governance should raise their expectations. However, where voters are faced with an incumbent party that has a monopoly on political power, they may instead become more resigned knowing there is little they can do to effect change in their local government.

## **Unintended consequences on leader behavior**

There is evidence that the civics course impacted citizens in a variety of ways: they are more likely to vote based on performance, have higher expectations of their local government, and challenge their local leadership more often. How will local governments respond as a result? On the one hand, elected leaders might act more responsibly, knowing they are now being scrutinized by voters. On the other hand, leaders might make more of an attempt to hide their misbehavior from more watchful citizens. Surveys with elected local government officials from each of the sample communes generate some evidence for how leadership responds to the introduction of a civic information course, at least in the short-term.

A list experiment in the survey measures the likelihood of leaders to campaign on transparency. It does so by asking how many of the following strategies they will use in the next election:

1. Give gifts to your party faithful
2. Win the support of village chiefs by offering them things
3. Develop ideas for the betterment of your commune
4. Campaign for transparency in the management of the commune budget

In a random half of surveys, the fourth strategy is removed. The outcome of interest is the mean difference between how many strategies leaders choose when given the transparency option and when not given

the transparency option as evidence of whether the leader cares about campaigning on transparency or thinks it is a good way to mobilize voters. Treatment effects are analyzed by examining the difference in this mean difference between treatment groups.

Table 10: Mean number of strategies chosen by leader in list experiment

Variable	Coefficient		
	T1 + T2	T1	T2
Transparency x Treat	-0.81 (0.508)	-0.46 (0.673)	-0.43 (0.376)
Transparency	1.25 (0.398)***	1.10 (0.466)**	1.38 (0.495)***
Treat	-0.08 (0.337)	-0.30 (0.436)	-0.01 (0.229)
Intercept	1.83 (0.271)***	1.91 (0.304)***	1.76 (0.321)***
N	93.00	62.00	62.00
R <sup>2</sup>	0.26	0.32	0.34

Standard errors in parentheses.

Significance levels: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 10 shows the results of a difference in difference estimation for each treatment group as well as the treatment groups combined. The difference-in-difference estimator in the first row shows some evidence of a negative treatment effect on government transparency. As expected, there is strong evidence that the number of strategies leaders say they participate in increases when the transparency option is added. Treatment has no effect on the number of strategies chosen when the transparency option is absent. However, when the transparency strategy is added to the menu of options, leaders in treated communes appear less likely to opt for that strategy than leaders in control communes. In the first column where the combined treatment groups are compared to the control group, a leader in a control commune chooses 3.11 strategies on average when the transparency option is available whereas the average leader in a treated commune chooses 2.22 strategies. The coefficient on the difference-in-difference estimator in this analysis is significant at  $p = 0.117$ , close to conventional levels of significance.

A second indication of government response to treatment is the frequency with which they hold public meetings. Leaders are asked during the survey and probed for details of each event to improve accuracy of the responses. Table 11 shows the average treatment effect on the frequency of public meetings held by leaders. Again, leaders in treated communes appear less transparent. Particularly in T2, they hold fewer public meetings post-treatment.

Table 11: Frequency of public meetings held by incumbent post-treatment

Control mean	7.19
T1 (ATE)	-2.61 (1.64)
T2 (ATE)	-2.90 (1.52)*

Standard errors in parentheses.  
Significance levels: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## Discussion

Taken together, these analyses provide strong evidence of an effect of the more intensive information treatment (T2) on voting behavior, weak evidence of an effect of the less intensive treatment (T1) on voting behavior, and strong evidence that the intervention worked to improve voter expectations in politically competitive communes. I find some support for two different mechanisms explaining changes in voting behavior: increased expectations and improved voter coordination. Since these findings are all generated by survey measures, one concern may be the relevance of the intervention for behavioral outcomes. Using observations at town hall meetings, I demonstrate that survey-based findings are consistent with treatment effects on actual behavior. People in the treatment groups were significantly more likely to challenge their local leadership at these meetings. There is also suggestive evidence that elected leaders become less transparent in response to treatment. Leaders in treated communes appear less likely to campaign on transparency and hold fewer public meetings.

Hypotheses regarding differential treatment effects are also evaluated in the data. There is supportive evidence that treatment works better when provided to a majority of villages in the commune suggesting that voters are considering the way other people will behave when they make their own electoral decisions. As expected, I find treatment has bigger effects on voting behavior among the least informed populations – those living in villages outside the municipal seat. Finally, I find evidence of a complementary relationship between political competitiveness and information. Treatment has a significant positive impact on voter expectations among communes where no party holds a majority of seats on the commune council and a significant negative impact on expectations where there is a monopoly on political power.

While there is little difference in treatment effects between T1 and T2 for the information and expectations outcomes, the differences are large and sometimes significant for the tests on voting behavior. Comparing actual course participants<sup>22</sup> in T1 communes versus T2 communes provides another estimation of the difference in effects between treatment groups. Confirming the previous findings, there is no

<sup>22</sup>Course participants are identified by their answer to a question on the survey regarding recent participation in a civics course.

significant difference between T1 and T2 among course participants for the information and expectations indices. However, among these self-reported course participants, T2 has a significantly larger effect in the voting simulations and improves coordination among voters significantly more than T1.

The findings are suggestive of an additive effect of information about government capacity and performance. Information about government capacity works to change expectations, but is not sufficient to induce changes in behavior. To affect voting behavior, additional information about government performance is required. In analyses of the willingness-to-accept variable, the magnitude of the effect of T2 is about twice as big as that of T1. In other words, T2 increases a voter's valuation of the performance dimension about twice as much as T1. If the effects of the information interventions are indeed additive, then the findings suggest that the relative performance information on its own would not be sufficient to produce changes in behavior. But together with information about government capacity, the two types of information generate significant change.

The results of this study may interest donors, NGOs and policymakers facing the challenge of improving democratic accountability. Whether governments are engaged in corruption or fail to be responsive to citizen demands, a key barrier to progress is the inability of citizens to make informed decisions at the ballot box. Previous studies have shown that expanding access to information is a relatively cheap and effective way of improving electoral accountability. This project shows a new type of information is important to provide as part of information campaigns, in addition to information about politician performance. For citizens to adequately evaluate politicians, they require civic information about the responsibilities of government and the size of the budget. Evidence of negative impacts on government transparency provide a cautionary tale of potentially perverse consequences of increasing voter information. Further research could help determine whether these are merely short-term effects, and whether voters will eventually sanction non-transparent politicians providing incentives for transparency in the long-term. The results of this experiment demonstrate that even a very brief civics course can effectively convey both information about what governments can and should do, generating significant impacts on the way people say they will vote and on whether they challenge their leadership.

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## Appendix 1: Linking hypotheses to measurable outcomes

Hypothesis	Outcome Measures	Results
The implementation of the civics course effectively increased voter beliefs about local government's potential to benefit them.	Q37-41, 53-4, 57: Evaluates voter knowledge of local government's responsibilities and capacity.	<u>Expectations index:</u> T1 & T2: Weakly confirmed;
	Q65-Q69: Evaluates voter understanding of how democracy works.	T1 & T2: Strongly confirmed when interaction term with majority party is included
	Q63: Assesses beliefs about the anonymity of voting.	
	Q74: Asks about retrospective voting.	
New beliefs about government capacity will increase the cut-point at which voters sanction incumbents.	Candidate vignette A: Evaluates the voter's willingness to pay for a high-performing politician.	T1: Weakly confirmed; T2: Strongly confirmed (for some specifications)
	Candidate vignette B: Evaluates the saliency of the performance dimension versus 2 others.	T1: Weakly confirmed; T2: Strongly confirmed
	Q70,76: Voters rank dimensions along which they and their neighbors vote.	T1 & T2: Weakly confirmed
Believing gains from high-performing politicians to be greater, voters should be more willing to incur costs to learn about actual government performance.	Q42-49: Evaluates voter knowledge of local council.	<u>Information index:</u> T1: Weakly confirmed; T2: Strongly confirmed
	Q55, 75: Evaluates voter knowledge/awareness of local government performance/activities.	
	Town hall meeting: Rate of participation.	Null finding
	Event log: Rate of participation in village and commune meetings.	Null finding
Increased expectations will engender greater dissatisfaction of current local government performance and lead to more instances of civic activity such as organizing or petitioning local officials.	Q60-2: Preference for alternatives to democracy.	Null finding
	Q77-84, 87: Elicits voter satisfaction/trust of local government performance.	T1: Weakly confirmed; T2: Weakly confirmed
	Q27-31 & Event log: Have you organized with other people to address a problem or contacted a local leader of any sort.	Survey Q's: Weakly confirmed for T2 Event log: Null finding
	Town hall meeting: Likelihood of petitioning leaders.	T1 & T2 pooled: Strongly confirmed
Increased knowledge about government capacity among voters will lead to more transparency of local government.	Q50: Did the government inform people more often about local meetings.	Null finding
	Politician survey: Frequency of public meetings.	T1: Weakly confirmed; T2: Strongly confirmed
	List experiment in politician survey: Assesses the extent to which politicians will campaign on a message of transparency.	T1: Weakly confirmed; T2: Weakly confirmed