Working paper

International Growth Centre

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A Field Experiment in Mali

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October 2012

When citing this paper, please use the title and the following reference number:
S-5007-MLI-1

DIRECTED BY LSE UNIVERSITY OF OXFORD

FUNDED BY UKaid from the British people

Does raising voter expectations improve accountability? A field experiment in Mali

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October 5, 2012

Abstract

I argue that if citizens systematically underestimate what their government can and should do for them, then they will hold politicians to a lower standard and sanction poor performers less often. A large-scale experiment across 95 localities in Mali in which some voters received information about potential government performance identifies effects of raising voter expectations. Survey experiments on the intent to vote (N=5,560) suggest that people in treated villages are indeed more likely to sanction poor performers and vote based on performance more often. There is also support for the idea that voting is a strategic calculation in which an individual's actions are contingent on beliefs about others: treatment improved voter coordination and worked better when provided to a majority of villages. A behavioral outcome – the likelihood that villagers challenge local leaders at a town hall meeting – adds external validity to survey findings. Contrary to expectations, increasing voter information appears to decrease politician transparency, at least in the short-run.

*Email: jgott@stanford.edu. Website: www.stanford.edu/~jgott. I am grateful for invaluable guidance and support from James Fearon, Beatriz Magaloni, Jeremy Weinstein, and Saumitra Jha, and for helpful comments and suggestions from Guy Grossman, Mike Tomz, and members of the IGC Working Group on Political Economy, FSI Action Fund seminar, and Stanford Comparative Politics Workshop. This research would not have been possible without the superior research assistance of Sidi Zeda and Peter LeFrancois as well as the tireless efforts of the Malian instructors and enumerators who crossed rivers and mountains to collect data. I thank the National Science Foundation, the International Growth Centre, the Freeman Spogli Institute, and Stanford University for financial support. All errors are my own.

1 Introduction

The recent emergence of free and fair elections in many developing countries has frequently failed to produce the expected increase in government performance. Cross-country studies show the positive correlation between democratic institutions and public goods provision breaks down in poor places(Boix, 2001; Ross, 2006; Collier and Rohner, 2008). Some existing explanations are that poverty and inequality facilitate patronage politics (Pande, 2007) and reduce public sector wages (Montinola and Jackman, 2002), improving opportunities for political corruption. So are poor and unequal societies stuck with failing democratic institutions? Motivated by insights from formal theory and a puzzle presented by the empirical literature, this paper offers one novel explanation for democratic failure – low voter expectations – and reports findings of a randomized intervention designed to raise voter standards of politician performance.

Until the eve of March 22, 2012, the West African country of Mali was lauded by the international community as a stable democracy that promoted necessary civil and political liberties. While a coup in an incredibly poor and relatively new West African democracy is perhaps unsurprising, the way in which most Malians shrugged their shoulders and did not contest the junta that put an end to their twenty years of democracy[1] speaks to an underexplored reason for the failure of democracy to engender accountability. Citizens expected little of their democratically elected government – so little that it was not worth fighting for.

I argue that low voter expectations led citizens to hold government to low performance standards that politicians subsequently lived down to without fear of electoral retribution. Acemoglu and Robinson (2000) explain that the expansion of the franchise and wealth redistribution in the West was driven by elite fear of popular unrest and revolution. It appears such a threat was largely lacking in Mali, which can help explain the failure of the government to redistribute, or be held accountable to voter interests. While poor performance is partly an economic problem – poor states have less to redistribute and weaker state institutions to implement policies (Fukuyama, 2004); it is also an information problem. Governments often have the capacity to perform better than they actually do. But when under-informed voters underestimate the government's true capacity, they will be satisfied with and demand less.

Information about government performance more generally has been shown to be a necessary condition for political accountability (Ferejohn 1986; Besley 2006). When voters are uncertain about the actions of their politicians, good performers cannot guarantee reelection so it is harder for voters to motivate them. As a result, poor-performing 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 and democracy.

[1]The one peaceful demonstration against the junta was overshadowed by much larger demonstrations in support of it. Newspapers were equally for and against the junta, and political parties were also split (with the parties running promising candidates for the scheduled presidential elections mostly against).

Observing the correlation between literacy and democratic stability, Lipset (1959) explains, 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 reduces 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 largely accord with these theoretical predictions of the positive effects of increasing access to information on voter behavior and politician performance (Besley and Burgess 2002; Ferraz and Finan 2008a; Reinikka and Svensson 2005).[2] 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 field experiments that manipulate specific types of information and study their effects produce more mixed results.[3] In India, an anti-corruption information campaign had no effect on voter behavior (Banerjee et al., 2010a). In Mexico, a flier providing information about corrupt incumbents led voters to turn out less and not necessarily to sanction more (Chong et al., 2012). And in Brazil, publicizing candidates' corruption charges had no effect on turnout or sanctioning (de Figueiredo et al., 2011). 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.

This paper studies the effects of a new type of information intervention: one designed to improve the accuracy of voter expectations of government performance. To identify the effects of such an intervention on voter and politician behavior, I conducted a randomized field experiment among decentralized local governments in Mali. Because localities with better-informed voters may exhibit higher levels of government accountability for any number of reasons, experimentally manipulating voter access to information permits precise causal inference. A civic education course was provided to 370 villages in 64 randomly assigned municipalities. The course dispensed information on the responsibilities of local government, the size of local government budgets (to ensure accurate expectations of capacity), and the basics of the democratic process to all treated villages, with an additional component on relative government performance to half of treated villages. Voter and leader surveys with embedded experiments were then conducted in the 64

[2]Keefer and Khemani (2011) is an exception. They find that increased radio access in Benin does improve public service provision, but the mechanism is not improved government accountability but rather improved household investment in children's education.

[3]See Pande (2011) for a comprehensive review of information experiments in developing countries.

treated and 31 control municipalities to evaluate impacts. While survey results are the focus of this paper, I briefly discuss treatment effects on the likelihood of challenging leaders at a town hall meeting to add external validity to the survey findings.

The intervention appears to have significantly raised voter expectations, at least in localities with sufficient political competition. 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. The data suggests that voters' enhanced ability to coordinate over candidate choices may be one mechanism underlying this behavior. There is further evidence that an individual's actions are influenced by beliefs about others: effects are stronger when a majority of the voting population is treated. Contrary to expectations, there is some evidence that politicians become less transparent following treatment.

The main contribution of the paper is demonstrating the relevance of a new type of information in voter decision-making. I argue that voter uncertainty over the potential for governments to perform (as opposed to actual government performance) constrains voters' ability to evaluate politicians, thus undermining government accountability. A formalization of the argument models an information asymmetry between the voter and politician regarding the availability of a budget to provide public goods in a particular village. The voter can either condition re-election of the incumbent on the receipt of campaign gifts or on gifts and whether any public goods were provided. If the probability of there being a budget for public goods is low, or if the voter thinks it low, then the politician can get sanctioned even when he is acting responsibly. This makes it more attractive for the incumbent to shirk, leaving the voter with nothing (including gifts).

A comparative static of the model is the more uncertain a voter is about whether the government has a budget for public goods in their village, the less likely they are to ever condition their vote on the provision of public goods. The hypotheses derived from the model are directly tested using survey experiments. The findings provide some evidence in support of the hypotheses. In particular, information about potential government performance makes voters more likely to sanction poor performers and vote based on performance, at least in hypothetical simulations.

A second contribution of the paper is to help make sense of mixed findings from previous information interventions. One reason that previous information interventions contributed to voter apathy rather than increased sanctioning of poor-performing politicians may be that new information only confirmed voter beliefs about poor-performing politicians without providing evidence that better performance is feasible.[4] In 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 allowing voters to make *relative* performance evaluations, voters

[4]Bhavnani (2012), for example, finds that Indian citizens grossly overestimate the corruption of their politicians.

sanction corrupt incumbents more often. Similarly, results from another experimentally-assigned information campaign in New Delhi 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, allowing them to better calibrate their expectations, as well report cards for multiple candidates, permitting evaluations of relative politician performance.

A third contribution of the paper is evaluating the effects of information on both voters and politicians within the same study. Previous information experiments have focused on impacts on voter behavior and have not explored general equilibrium effects. The anticipated outcome of improving information about government performance is not only that voters will sanction poor performance more often, but that government performance will improve on the whole. This outcome depends not only on voter behavior, but also on politician response to the intervention. Politicians may either respond to the threat of sanctions by behaving better, or they may try to manipulate the information context to maintain a status quo of poor performance. Humphreys and Weinstein (2012), for instance, provide suggestive evidence that when information about MP performance is disseminated in Uganda, politicians are able to counteract or obfuscate such information within their constituencies. Suggestive findings in this paper are similar: politicians in treated communes appear to be less transparent than their counterparts in untreated communes, holding fewer public meetings and reporting they will not campaign on transparency in the next election.

Finally, this paper contributes to an important policy question and provides practical information for policymakers. While it is unlikely that incumbent governments will spontaneously decide to provide the type of information contained in the experimental intervention thereby constraining their own actions, other relevant actors could realistically implement a similar information intervention. In developing countries, external bilateral and multilateral donors are often actively engaged in promoting civic education and disseminating political information. In countries with a reasonably developed media, the press could disseminate the type of civic information analyzed in this experiment. And in decentralized countries, the national government may be relatively disinterested in the outcomes of local elections and could decide to support a policy on improving civic education.[5]

The next section develops a theory of the impact of information about potential government performance on voter behavior. It then derives predictions and observable implications tested in section 6. The third section describes the institutional context in which the experiment is implemented. The fourth section outlines the research design and the fifth specifies the measurement strategy. I discuss results in the final section.

[5]This is the case in Mali. The Ministry governing decentralized localities is actively involved in improving local governance and showed an interest in the results of this experiment.

2 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 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. [6] 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 potential government performance or government capacity. I formalize this argument by extending classic moral hazard models of accountability (Barro, 1973; Ferejohn, 1986; Fearon, 1999; Besley, 2006) in which voter uncertainty stems from asymmetric information regarding politician performance. Dal Bó and Powell (2009) similarly extend asymmetric-information models of international conflict by introducing uncertainty over the size of state spoils in contrast to the standard uncertainty over the cost of fighting or distribution of power. They recognize that a lack of transparency in the incumbent's management of state resources creates an information asymmetry in which "the government is assumed to know the size of the spoils or 'pie' while the opposition only has a rough idea about its size." In their model, this type of information asymmetry leads to costly and inefficient conflict over state resource allocation. In our case, voter uncertainty over the size of the pie leads to less accountable government behavior.

To formalize this intuition, I rely on a sanctioning rather than selection model. The problem being modeled is one of voter inability to recognize poor government performance, not the inability of voters to distinguish among different politician types. The sanctioning model is also more appropriate for understanding how the information intervention should

[6] See Fearon 1999 for a discussion of sanctioning and selection models.

affect voter and politician behavior. Information that raises voter expectations of potential government performance should help voters better evaluate perceived government performance and sanction misbehavior, thereby disciplining politicians. In contrast, this type of information should not impact a voter's ability to distinguish among politician types.

Before formalizing a new type of information asymmetry, 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: 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. 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 underperform. 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. Predictions of these models 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. In Section 3, I show how both assumptions are violated in the Malian context.

This is an unexplored aspect of the accountability problem in developing democracies. If 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.

2.1 Setting up the model

These intuitions are formalized in an electoral model that takes into account voter uncertainty about the size of the public budget.[7] 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 $t = 1, 2, \ldots$. 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 provide a public good in the village such as a well or improvements to a community school or clinic.

There is asymmetric information about the budget: the politician knows if the funds are available while the village does not. This is justified in the current context where voters sometimes know there is a commune-level budget that can fund public goods, but they more often 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. Assume villages cannot observe the whole set of public goods provided to all villages in the entire commune – another aspect of poor voter information about government performance.

The sequence of the game is as follows. In each period t, the politician observes whether funds are available (recall: funds of size 1 are available with probability p). He then chooses whether to build the public goods project and how much to dispense in campaign gifts, $k \leq w$. After observing the amount of gifts dispensed, k, and whether a project is built, the voter chooses to keep or drop the incumbent.

The incumbent's period payoff is $w - k + 1$ if funds are available but he does not build a project and $w - k$ otherwise. 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.

The village's period payoff is $g + k$ if a project is built and k 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.

2.2 Equilibria

Consider Markov equilibria in which voters use the same rule in each period and can condition reelection on observed gifts, public goods, or both. This gives the voter essentially two feasible "electoral control strategies": the *peanuts* strategy where the voter conditions her vote only on the receipt of campaign gifts, and the *big ticket* strategy

[7] I recognize and thank James Fearon for his collaboration in formalizing the model.

in which the voter conditions her vote on gifts and the public good, voting against the incumbent if the public good or sufficient gifts are not provided.

Both strategies assume retrospective voting using a cut point or threshold. Since the voter observes k_t in period t, suppose the voter conditions re-election on whether $k_t \geq \overline{k}$, where $\overline{k} > 0$ is the cut point. (For simplicity, assume the incumbent is liquidity constrained and so cannot provide more than w_t in gifts in each campaign).

In the peanuts scenario, the village's strategy is

$$a_v^P(\overline{k}) = \begin{cases} re-elect & if \quad k_t \geq \overline{k} \\ sanction & if \quad k_t < \overline{k} \end{cases}.$$

Let V_I^P be the equilibrium expected payoff for a politician who has a probability p of having the funds to build a public goods project, who decides to provide gifts k but not to provide public goods, and who acts optimally in each period.

$$V_I^P = (1-p)(w - k_t + \delta V_I^P) + p(w - k_t + 1 + \delta V_I^P).$$

At the cut point $\overline{k}$,

$$V_I^P = \tfrac{w-\overline{k}+p}{1-\delta}$$

For certain values of the parameters, the incumbent will prefer to shirk and get kicked out of office. The incumbent will only provide $\overline{k}$ in gifts if what he can get from holding office in the future is greater than what he can get by shirking. In other words, the incumbent will have an incentive to provide voters with gifts rather than keep $\overline{k}$ for himself if

$$IC^P\text{: } \delta V_I^P \geq \overline{k}.$$

Substituting for V_I^P and solving for $\overline{k}$,

$$\overline{k} = \delta(w + p),$$

and because it is not feasible for the politician to provide more than w by the illiquidity assumption,

$$\overline{k} = min(w,\ \delta(w + p)).$$

So, the maximum the village can get each period in this peanuts equilibrium is the smaller of w and $\delta(w+p)$. The expected equilibrium payoff for the village is then

$$V_v^P = min(w,\ \delta(w+p)).$$

Note that while some accountability is possible here in the form of private transfers, no public goods are provided even when they are available – the voter ignores the incumbent's record on public goods and votes only according to whether he provides sufficient campaign gifts.

Alternatively, the village can employ the big ticket strategy, voting against the incumbent if he fails to distribute $k_t \geq \overline{k}$ in gifts *or* if he fails to deliver a public good. In this big ticket scenario, the village's strategy is

$$a_v^{BT}(\overline{k}) = \begin{cases} re-elect\ if\ k_t \geq \overline{k}\ and\ g > 0 \\ sanction\ otherwise \end{cases}.$$

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.

Let V_I^{BT} be the equilibrium continuation value for a politician who has a probability p of having the funds to build a public goods project, who decides to provide a public good g and gifts k, and who acts optimally in each period.

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

At the cut point $\overline{k}$,

$$V_I^{BT} = \tfrac{w-p\overline{k}}{1-\delta p}.$$

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,

$$IC^{BT}\colon\ w - k_t + \delta V_I^{BT} \geq w + 1,$$

substituting for k_t and solving for $\overline{k}$ at the point of inequality,

$$\overline{k} = min(w,\ \delta(w+p) - 1).$$

In this big ticket equilibrium, it is impossible for the village to set a non-zero cut point $\overline{k}$ when $\delta(w+p) - 1 \leq 0$. In such a case, the village cannot extract any campaign gifts from the politician, and because the incentive compatibility constraint is not met, the politician prefers to shirk on the provision of public goods as well. When $\delta(w+p) - 1 > 0$, however, the expected equilibrium payoff for the village is

$$V_v^{BT} = \delta(w+p) - 1 + p * g.$$

2.3 Comparative statics

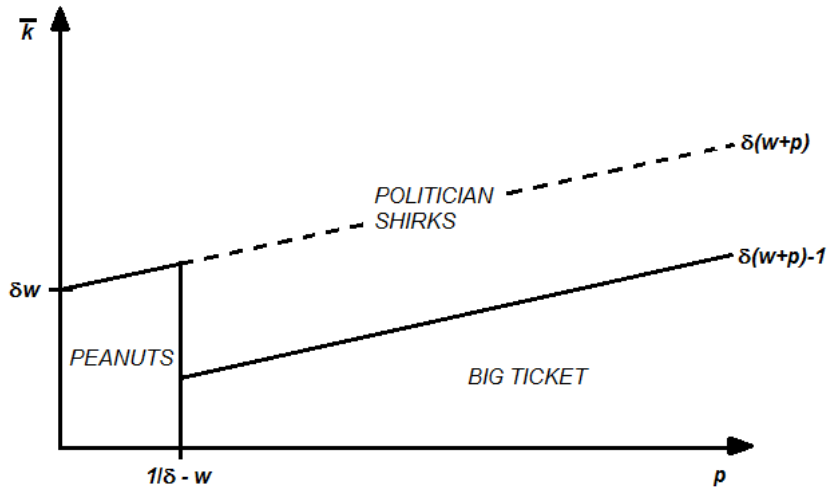
In the big ticket equilibrium, the politician prefers to shirk when $\delta(w+p) \leq 1$, or $p \leq \frac{1}{\delta} - w$. This is more likely the lower is p, or the belief that the budget is sufficient to fund a public good in the village. The problem here is that the public good is thought to be rare 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.

Figure 1 shows the equilibrium outcomes for different values of p and $\overline{k}$. At low values of p and $\overline{k}$, the village is better off employing the peanuts incentive scheme. At higher values of p and lower values of $\overline{k}$, the village is better off employing the big ticket strategy. Even though fewer gifts are distributed under this scheme, the village is better off in expectation because of the better probability of getting the higher-valued public good. At high values of $\overline{k}$, the politician will always shirk.

2.4 Hypotheses

Shown graphically in Figure 1, increasing the village's belief p, that the local government has a budget to provide a public goods project will impact the village's equilibrium behavior. First, within each of the two equilibrium solutions, peanuts and big ticket, marginal increases in p will result in marginal increases in $\overline{k}$, or the maximum cut point at which the village can condition re-election and avoid shirking by the politician. Second, if an increase in p crosses the threshold of $\frac{1}{\delta} - w$, then the village will move from playing the peanuts strategy to the big ticket strategy in equilibrium.

Figure 1: Equilibrium outcomes for different values of p and $\overline{k}$



An increase in p may result from local changes such as an increase in tranfers to the government budget or an increase in the competitiveness of an election. One straightforward way to exogenously manipulate p is to provide villages with new information. I posit that improving information to villages about what the local government *can* and *should* do, or the potential for local governments to perform, will result in an increase in p. This assumes that voters have downwardly-biased beliefs about potential government performance.

From this discussion, I derive the following testable predictions:

H0: Improving information about potential government performance, will increase voter expectations of politician performance, p.

H1: Improving information about potential government performance, will increase the cut point, $\overline{k}$, at which poor-performing candidates are sanctioned.

H2: Since the "big ticket" equilibrium does not exist for small enough p, improving information about potential government performance will increase the likelihood of voting along the performance dimension.

In the above setup, the village (or representative voter) is a unitary actor and votes as if it 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 strategic actor is the voter not the village and there is some probability of an individual's vote being discovered, imagine one voter has information that p is high. Under the "big ticket" strategy, she 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 her vote only on gifts and decides to keep the incumbent. If at least half of 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 she would only want to do so if she 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.

Solving coordination problems can be aided by the opportunity to communicate with other players in the game or by making one equilibrium outcome more focal. An information intervention could increase coordination by both means. First, participants have an opportunity to discuss their views on voting. Second, the intervention makes performance-based voting more salient. If the inability to coordinate is one constraint on voter behavior, it follows that:

H3: Introducing public information about potential government performance, p, will increase the ability of voters to coordinate on the high-performing candidate.

If the assumption of a unitary and decisive voter is relaxed, a voter's decision calculus can also 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 can 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 other's valuation of p. This discussion suggests:

$H4$: The more public the information signal, the greater the treatment effect on voter behavior.

2.5 Information conditional on competition

The model does not make clear predictions about the role of political competition. In the extreme cases where elections are strictly uncompetitive or strictly competitive, there is no role for information. Where voters have no real choice in their voting decision, then manipulating voter information should have no influence on their behavior. Where parties are maximally competitive, a key electoral strategy is to disseminate information to voters effectively eliminating information asymmetries between voters and politicians. In reality, party systems are unlikely to fall into either of these extreme categories, but rather exhibit some intermediate level of competition.

In such contexts, the relationship between information and competition is less clear. Marginally increasing 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 reasonably expect higher levels of competitiveness to increase a voter's likelihood of voting based on a performance dimension since the threat of sanctioning poor performance is more credible in a competitive environment. Like access to information, political competitiveness on its own has clear benefits to accountability. However, one could reasonably argue

that information and competition are substitutes *or* complements in the production of accountability.

On the one hand, marginal improvements in party competitiveness may substitute for marginal improvements in 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, marginal improvements in party competitiveness may serve as a complement to increases in voter access to information. For instance, increasing the range of choices among competitive candidates could make information about those candidates more valuable. In this case, information interventions should work better in more competitive systems.

2.6 General equilibrium effects

The comparative statics derived from the model imply that higher levels of information should lead voters to sanction poor-performing incumbents more often at election time. The change in voter behavior, however, is not an end in itself, but rather a means to an end: better government performance. Over time, politicians should respond to an increased real or perceived threat of sanctions by improving their performance. This is not a foregone conclusion but a question for research.

How will politicians respond to changes in voter behavior? On the one hand, elected leaders might act more responsibly, knowing their actions are now being scrutinized more acutely by voters. On the other hand, leaders might make more of an attempt to hide their misbehavior from more watchful citizens (see for example Humphreys and Weinstein 2012). Section 6.3 also finds that leaders appear less transparent where voters receive an information treatment.

3 Institutional context

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. Democratic since 1992,[8] 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 themselves. Not only have Malian elections been deemed free and fair, but there have been two peaceful

[8]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.

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 across independent and locally-governed units. In 1996, Mali decentralized politically and territorially. The 703 newly demarcated *communes* democratically elect councils with autonomous control over local budgets. The average population of a rural commune is 13,000. Though commune governments can and do levy taxes, revenue is small relative to public expenditures that fall under the commune government's purview. However, a para-statal agency, the ANICT (Agence Nationale d'Investissements dans les Collectivités Territoriales,) was established in 2001 to alleviate capacity problems in communes. It transfers funds for annual development projects to each commune using an allocation rule based on the commune's poverty level, population and distance to major cities.

The support of ANICT provides commune governments with non-trivial resources to provide public goods. According to a UN-administered commune-level survey, ANICT funds make up more than half of the average commune government budget whereas locally-generated funds comprise only 15 percent. There is significant variation in the extent to which public goods are provided that cannot be explained by the size of the commune budget. In related work using commune-level data for all of Mali, I suggest that some of the variation can be attributed to citizen access to information and political competitiveness at the local level (Gottlieb, 2010).

Survey evidence shows that Malian voters have low expectations, both with regard to what the local government is capable of doing and what it is responsible for doing. The household survey conducted in conjunction with the experiment indicate that more than a third of people do 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 most recent Afrobarometer survey (2008), about a third of respondents said the local government was responsible for managing schools and health clinics while a half of respondents named the central government. In addition, 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.

Perhaps a symptom of low expectations, respondents in my survey often prioritize other dimensions than government performance in their vote choice. 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.[9]

[9]Because these criteria are not generally mutually exclusive, respondents were asked to rank criteria

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 another quarter said people vote based on personal needs and interests.

That voters have low expectations of government is not a surprising finding in light of a number of features of Mali that are often consistent with other young democracies.[10] In new democracies, 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.

4 Research design

In the following sections I describe a field experiment in 95 rural communes in Mali that tests whether and how information that raises citizen expectations effects voter and politician behavior. Because treatment is conditional on random assignment, 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. Voters may have more information about potential government performance precisely because their government is better behaved. Or some unobserved factor such as strong social networks may produce both informed citizens and well-behaved governments. By randomly assigning an informational "treatment," I can identify whether a particular type of information deficit has 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 underinformed 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.

in order of importance rather than choose just one.

[10] Pande (2007), for example, argues that poverty and inequality can facilitate patronage politics and with it, greater opportunities for political corruption.

4.1 Treatments: "basic" & "mixed" information interventions

Two treatment interventions are provided in the form of a civics course conducted at the village level. 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 was carried out at the village level rather than at the commune level to ensure broader access to information.

Voluntary participants received a series of 3-hour sessions at one-week intervals. Sessions were 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 of the democratic process found in preliminary surveys. A review of existing civic education efforts in Mali found a lack of any systematic form of civic education.[11] 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 struck 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.

Two concrete types of information can raise voter expectations of potential government performance: information about what governments are legally responsible for doing and information about what governments are capable of doing, e.g. what resources they actually 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. Beath et al. (2012) discuss the negative effects of this type of uncertainty in Afghanistan in reporting the results of an experiment in local governance. They find that the institution of local councils *per se* does not improve the equity of distribution of food aid; it is only when councils are clearly assigned responsibility for distribution that outcomes improve. They conclude that "institutional arrangements with diffuse responsibility – caused by the presence of multiple institutional structures with no clear hierarchy or the mandated involvement of groups external to customary governance structures – provoke opportunistic behavior by political actors."

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. For instance, one treatment condition in the Mexico information experiment (Chong et al., 2012) disseminated information on the total amount of money

[11] 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.

allocated to local governments along with the amount spent by year end. This treatment, unlike in the corruption treatment condition described earlier where voters learned about the percent of public funds that were misused, had significant effects on voter behavior. The supply of information about how much local governments spent relative to the total budget caused voters to turn out more and sanction the incumbent more often when the percentage of funds spent was low. While the authors do not interpret the finding this way, the evidence supports my hypothesis that the lack of information about potential government performance is a constraint on accountability.

A "basic" civics course provides both types of information aimed at raising voter expectations of potential government performance: information about what local governments can and should do. In particular, participants learn about the responsibility of local governments to provide a menu of local public goods such as clean water, primary health care and primary schooling. They learn about the size of the local government budget with concrete examples of public services that can be realized with that amount of funding. To ensure comprehension of these facts, basic information about democracy and decentralization in the Malian context are also provided.[12]

If voter behavior is constrained by uncertainty about potential government performance 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 if they lack sufficient information about the incumbent's performance record. If the basic treatment elicited null results, it would be impossible to distinguish whether 1) the type of information asymmetry being addressed is simply not a constraint to government accountability, or 2) the type of information provided is necessary but not sufficient to change behavior. To help distinguish between these two alternative hypotheses, a second treatment condition supplements the basic civics course with additional information about relative government performance.

Participants in this "mixed" treatment condition receive information about how their government performed relative to other local governments. Performance indicators include the number of development projects in the village funded by the commune, the distribution of projects between the commune seat and outlying villages, and the number of public meetings held by the commune council. Some of this data comes from national statistics while other data such as the distribution of projects within the commune is collected by the course instructor and course assistant who is elected from among the course participants. Each performance indicator is presented as an index comparing the commune government in question to other communes in the sample. This has the advantage of giving participants new information about the incumbent's performance record while at the same time reinforcing what voters can expect of their governments. Because the performance information is relative to how other comparable governments

[12]Course material was developed in collaboration with the Malian Ministry of the Interior (MATCL). Course activities and supplementary audio-visual material were borrowed from Malian NGOs and a national civic education program, PNEC. An outline of the course is provided in Appendix 2 and the full course curriculum is available upon request.

are performing, participants learn about government capacity or what is in the feasible set of government actions.

Another motivation for studying these two treatment conditions is practical. For policymakers, the decision to carry out each type of intervention involves different stakes. Civics curricula can be taught over a long period of time without being updated, and can be disseminated 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. 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.

This design can answer the following questions:

- Is the basic civic education component sufficient to change the political outcomes under study?
- Does a combination of the two types of information, about potential government performance and about actual government performance, produce effects that are significantly different from just providing the first?

Because no group received the longer, 3-session treatment with only basic civics information (and no relative performance information), we cannot rigorously distinguish the effect of relative performance information from the effect of a longer treatment.[13]

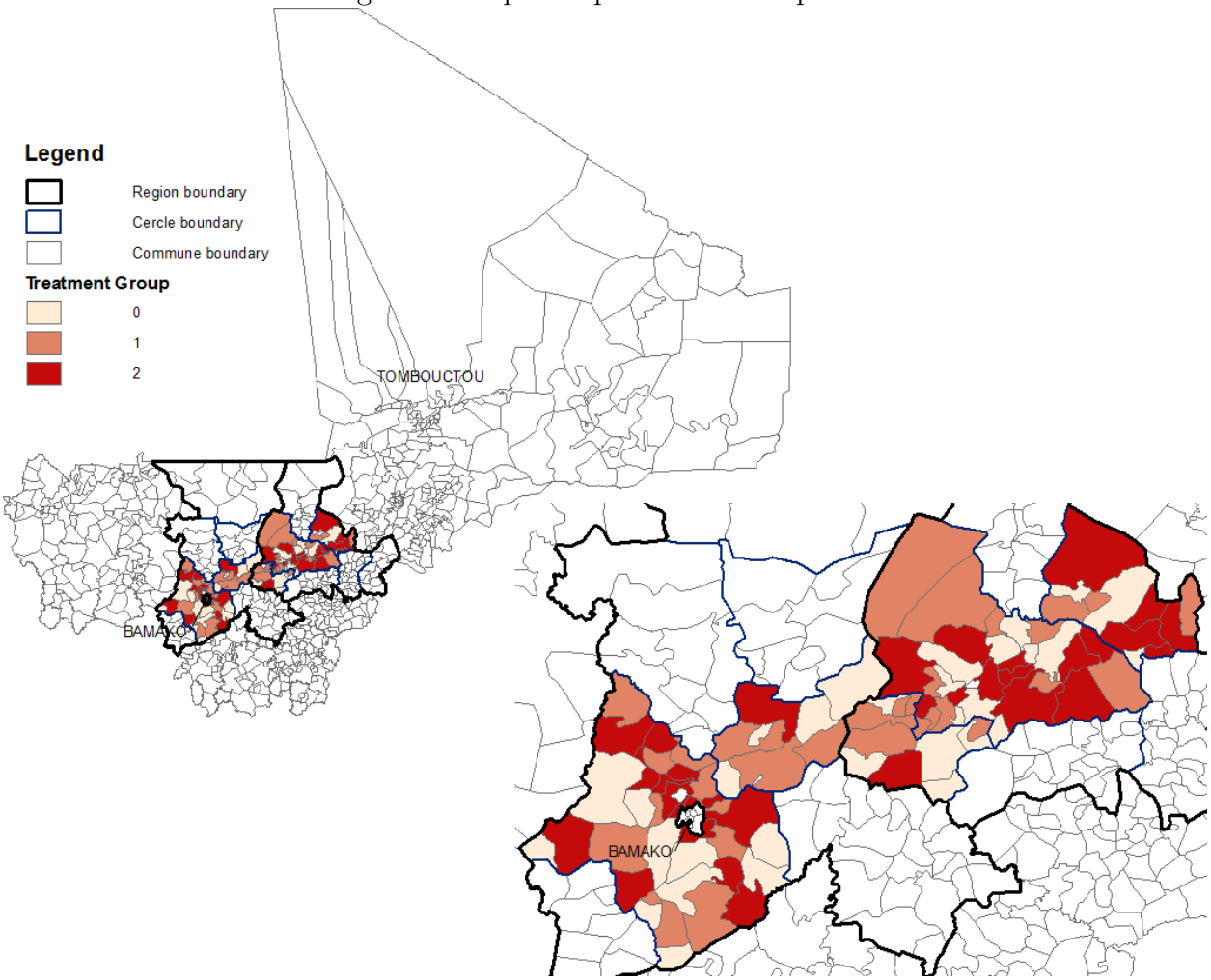
4.2 Sample

The experimental sample consists of the 95 rural communes in the five *cercles* or districts of Kati, Koulikoro, Segou, Macina, Baraoueli (see Figure 2). These cercles, located along the Niger River, are in two of Mali's most populous regions, Koulikoro and Segou.[14] Each commune is randomly assigned to one of three groups: control (T0), the basic treatment condition (T1) or the mixed treatment condition (T2). The control group does not receive

[13] Another important comparison would be a group that received only information about actual government performance and control, permitting a test of the unique effect of information about actual government performance. Resource constraints and power considerations, however, precluded a third treatment condition. Given that the effect of information about actual government performance has been studied extensively in other contexts, I focused on the two treatments involving information about potential government performance. The absence of such a third treatment means we cannot directly answer the question of whether relative performance information on its own is sufficient to change the political outcomes under study. Previous experimental results suggests that it may not be.

[14] This particular sample of communes was chosen to overlap with the German donor GIZ's intervention zone. Donor-funded town hall meetings were a key measurement strategy designed to evaluate impacts of the experimental intervention. While GIZ budgeted for town hall meetings in the entire sample in 2011 and signed an MOU to this effect, they only completed 33 due to later policy changes.

Figure 2: Map of experimental sample



any intervention and is visited for the first time during the survey. T1 and T2 receive basic civic information over 2 course sessions and T2 receives an additional session on relative government performance, totaling 3 course sessions. The intervention in the 64 treated communes took about two months to complete. There is about a 3-month lag between treatment and the household, chief and politician surveys.

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. Using a block randomization design, I stratified the sample prior 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[15]. By chance, 32 communes were assigned to the two treatment conditions, T1 and T2, and 31 communes to the control group.

[15]The development index is produced by UNDP's Observatoire de Développement Humain Durable and includes measures of electrification, telecommunication, population size and public goods.

Due to budget and time constraints, studying every village in each commune (averaging 18 villages per commune) was not feasible. Balancing requirements for power and practical considerations, I randomly selected 6 villages from each commune in the sample.[16] Six of the sample communes have fewer than 6 villages, so all villages are sampled in those communes. The total number of villages in the experimental sample is 556.[17]

As is clear from the map in Figure 2, treatment and control communes are not clustered geographically. In other words, control communes are often bordered by communes that receive treatment. Spillover from treatment communes into control communes is thus a possibility. However, the communes in the sample are typically geographically spread out with poor road infrastructure linking one village to another. Any information that spilled over into control communes in spite of these challenges would bias the estimated treatment effect downward, making spillovers effects less of a concern for the purpose of this analysis.

4.3 Compliance

Within treated communes, individual participants voluntarily self-selected into treatment following a village-wide assembly that provided details about the course and an open invitation to participate. As might be expected, participants differ systematically from non-participants within the treatment communes.[18] About 43 percent of participants were women whereas women comprise about 50.5 percent of the population in the sample regions. The average age of participants was 45, higher than the average age among survey respondents of about 40 years old. Participants are better socially connected than non-participants: about 48 percent of self-reported participants (which includes some people who said they participated but likely did not) are related to the chief compared to only 34 percent of other respondents. They are less likely to be from a minority ethnic group in their village, and are better educated. 70 percent of self-reported participants say they attended some school while only 56 percent of other respondents report any schooling.

An average of 30 villagers participated in the course per village. With a little over 1000 residents in the average village, this is a small proportion of course participants.

[16]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 infeasible to exclude the commune seat from the sample.

[17]One commune is missing observations for one of the six villages due to refusal by the village chief to allow the enumerators to work there.

[18]Some participants details were collected during the course itself; others are from survey data. I infer course participation from affirmative answers to two survey questions about awareness of a civics intervention in the village and participation in that civics intervention. Likely due to affirmation bias, there is dramatic over-reporting of participation as evidenced by a quarter of respondents from the control group saying they participated in a civics course. To my knowledge, there was no other civics course carried out in sample communes in recent years.

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 untreated female neighbors as they were on treated women (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 comprising members of the same extended family. Households of the same concession live in close proximity, often enclosed by a single wall. The women of a concession typically prepare food together and the men often farm the same or neighboring plots of land. Gine and Mansuri's information experiment found that information only traveled successfully within networks of the same gender, which is likely similar in Mali given the structure of social and economic activities. However, the present experiment targets both men and women and succeeded in reaching almost equivalent numbers of each gender.

I conducted a follow-up survey in a random sample of treated villages to investigate the distribution of participants among concessions as one way of understanding within-village spillover of information. Village chiefs or their representatives were given the participant roster and asked to group participants by concession. Chiefs were able to identify over 92 percent of course participants. On average, 18 concessions per village sent at least one member to participate in the course. Lacking data on numbers of concessions per village[19], we rely on reports by the chief. In the commune seats, chiefs report an average of 661 "small" concessions made up of one or a couple household units and 27 "big" concessions. In outlying villages, chiefs report an average of 77 small concessions and 9 big concessions. Participants in the course came from concessions with 11 adult members on average, indicating relatively large concessions.

On average, 193 adult members of treated villages share a concession with at least one course participant. Considering that about half of village residents are under the age of 18, we can estimate that about two-fifths of adults in the average treated village either attended the civics course or share a concession with another adult who did. Thus through the spread of information within concessions, treatment could have indirectly reached a relatively high proportion of village residents, 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 other members of the village, and about a third of non-participants said they learned something about the course from people in their community.

[19]The Malian census reports numbers of households rather than numbers of concessions.

5 Measurement strategy

A household survey measures levels of civic knowledge, beliefs about government capacity and behavior in hypothetical voting simulations[20]. The survey is conducted with one person in 10 different households in each of the sample villages. Of the 10 households, 6 are selected randomly using a sampling method that ensures geographic representation across the village. Stratifying on gender, individuals within households are randomly selected. The remaining 4 surveys are conducted with targeted local leaders: the women's leader, the youth leader, the head of the village association, and the village assistant elected during the civics course[21].

In all rural villages, one person is recognized as the leader of these three groups which are responsible for economic or cultural activities within the village. The women's leader, for example, might organize the village's women around a lucrative activity such as cloth dying or soap-making. The youth leader might fundraise for sports equipment and organize tournaments. The head of the village association is often responsible for leading public works projects such as road-brushing.

These leaders are targeted first, because they are more likely to have participated in the civics course and second, 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 unit of randomization, the commune, as the unit of analysis (unless otherwise specified), averaging over outcomes when necessary. Accounting for blocked randomization, estimation of average treatment effects[22] will be estimated using the following basic equation:

$$y_c = \beta_0 + \beta_1 T1 + \beta_2 T2 + W_c' \Gamma + \varepsilon_c$$

[20]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.

[21]In control villages where no course was held to elect a village assistant, a reasonable alternative is chosen. The profile of the elected village assistant tended to be the person in the village who was a frequent intermediary between the commune administration and the village. As a result, enumerators in control communes sought out the person in the village described as such an intermediary.

[22]All analyses estimate average treatment effects (ATE). Unsurprisingly, estimation of average treatment effects on the treated (ATT) or on just those respondents who reported participation in the course yields larger and more significant coefficients.

where y_c is an outcome measure in commune c; T_1 and T_2 are indicators of whether the commune received Treatment 1 or 2; W_c is a fixed effect for block[23], the unit on which randomization was stratified;ε_c is an error term; and β_1 and β_2 are the parameters of interest, average treatment effects for Treatment 1 and Treatment 2.

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[24] or peri-urban[25]. The balance test in 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 one party on the commune council holds a majority of seats 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.

Table 1: Balance test comparing baseline characteristics among groups

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

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

Village- and individual-level controls are characteristics that may influence the outcomes

[23]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.

[24]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.

[25]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.

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[26], radio listenership, asset ownership, relationship to the chief, and travel outside the village.

Heterogeneous treatment effects with respect to political competition will be measured by interacting an indicator for treatment and an indicator for competitiveness. 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 proxy for political competitiveness, sample communes are thus divided into those with a majority party on the commune council (about one-third) and those without.

5.1 Pre-analysis plan

After receiving the data but prior to analyzing it, I developed a pre-analysis plan that specified which measures I would use to test different hypotheses. As discussed by Casey et al. (2011), the registration of pre-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.[27] This paper does not analyze all hypotheses proposed in the pre-analysis plan, but I include them in the appendix for completeness. The data analyses focus on the hypotheses directly generated by the theory and 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.

[26] 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 then a quarter of the other survey respondents in that village belong. Using this coding scheme, 12 percent of respondents are coded as minorities.

[27] The paper also discusses two hypotheses that were no foreseen in the pre-analysis plan: H3 on coordination and H4 on dosage effects.

6 Data analysis

H0: Improving information will increase voter expectations

Following the data analysis plan, I create a composite index of relevant survey items to estimate average treatment effects on voter expectations. Because multiple outcomes assess a single hypothesis, I perform a mean effects analysis using the summary index. As suggested by Anderson et al. (2008), 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 outcome. 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, the number of future projects expected to be implemented by the council, and beliefs about how democracy works.[28]

Table 2 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 the equation described in the measurement strategy. Models 2-5 test whether a treatment effect is robust to the inclusion of control variables. They regress the expectations index on treatment status, again 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, these latter models account for dependence among individual observations within villages and among village observations within communes.[29] 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.[30] Random effects, rather than fixed effects, are modeled at the commune and village levels 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.

Average treatment effects on commune-level outcomes y_c are estimated using the following equation:

[28]The questions about how democracy works are arguably irrelevant to voter expectations of government's potential performance. I include them in this analysis because they were specified in the data analysis plan. However, when they are dropped from the component index, the magnitude of the treatment effect is about twice as large, and treatment effects become (and remain) significant in Model 2 specified for Table 2.

[29]Green and Vavreck (2008) analyze alternative estimation approaches for cluster-randomized experiments and find relatively minor differences, but that random effects regressions produce slightly less biased standard errors than robust cluster standard errors.

[30]I use the xtmixed command in Stata.

$$y_c = \beta_0 + \beta_1 T1_c + \beta_2 T2_c + X_c'\Pi + W_c'\Gamma + \varepsilon_c$$

where X_c is a vector of commune-level controls.

Average treatment effects on individual-level outcomes 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, α_c are random effects for commune and γ_{vc} are random effects for village.

Estimated treatment effects in Models 1 - 5 are all positive, but they are only significant at conventional levels in Models 4 and 5 when village-level controls are included. The treatment effects account for about a one-quarter to one-third standard deviation change. There is no significant difference between T1 and T2. Many of the covariates have statistically significant relationships with the expectations index in the expected direction. 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.

Models 6 and 7 report the effect of information on voter expectations conditional on electoral competition, a specification that was not included in the pre-analysis plan. To analyze this differential treatment effect, I regress the expectations index on an indicator treatment, an indicator of whether there is a party with a majority of seats on the town council[31] and an interaction term between the two. Model 6 is basic the specification without any controls in which the commune is the unit of analysis. Model 7 uses all controls from the specification in Model 5.

These tests provide interesting evidence of a heterogeneous treatment effect in which information and political competition are complementary. In other words, the information treatment increases expectations only where there is substantial political competition. 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 both Models 6 and 7, 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. The coefficient on the treatment effect is larger and more significant in this specification than when the interaction term was left out. The negative sign on the interaction term can be interpreted to mean that treatment significantly lowered voter expectations of government performance in communes where there is a majority on the

[31]Recall that policies on the council are decided upon by majority rule so a party holding a majority of council seats essentially has a monopoly over decisionmaking power.

Table 2: Treatment effect on expectations index

Variable	Coefficient						
	M1	M2	M3	M4	M5	M6	M7
T1	0.031	0.027	0.028	0.046*	0.045	0.087**	0.078**
	(0.036)	(0.024)	(0.026)	(0.028)	(0.027)	(0.041)	(0.038)
T2	0.015	0.040	0.037	0.047*	0.048*	0.104**	0.110**
	(0.036)	(0.024)	(0.026)	(0.028)	(0.027)	(0.043)	(0.038)
Incumbent			-0.015	-0.032	-0.093		0.173
			(0.129)	(0.136)	(0.141)		(0.215)
Wealth index			0.009	0.043	0.019		0.153
			(0.113)	(0.120)	(0.121)		(0.132)
Arrondissement			0.030	0.034	0.036		-0.021
			(0.038)	(0.040)	(0.040)		(0.047)
Peri-urban			0.084	0.017	0.032		0.007
			(0.066)	(0.072)	(0.073)		(0.078)
Village distance				-0.024***	-0.019***		-0.019***
				(0.005)	(0.005)		(0.005)
Concessions (in 1000s)				0.023**	0.018*		0.017*
				(0.009)	(0.009)		(0.009)
Age					-0.007**		-0.006**
					(0.003)		(0.003)
Schooling					0.058***		0.058***
					(0.008)		(0.008)
Leader					0.020*		0.020**
					(0.010)		(0.009)
Woman					-0.152***		-0.152***
					(0.009)		(0.009)
Minority					0.000		0.001
					(0.013)		(0.013)
Radio listenership					0.019***		0.019***
					(0.003)		(0.003)
Asset ownership					0.025***		0.025**
					(0.010)		(0.010)
Chief relation					0.013		0.014
					(0.009)		(0.009)
Travel					-0.028**		-0.028**
					(0.012)		(0.012)
Majority party						0.036	0.067
						(0.066)	(0.057)
Majority party x T1						-0.179**	-0.057
						(0.084)	(0.077)
Majority party x T2						-0.228***	-0.139*
						(0.082)	(0.075)
Intercept	0.002	0.057*	0.061	0.133	0.091	-0.009	0.006
	(0.026)	(0.031)	(0.042)	(0.111)	(0.110)	(0.029)	(0.127)
N	95	95	95	5,452	5,372	95	5,372
Enumerator FE	N	Y	Y	Y	Y	N	Y

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

town council, or little political competition. Conversely, treatment significantly raised voter expectations in communes where there is no majority party on the town council.

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 this differential treatment effect, post hoc reasoning can support why such different effects emerge. First, voters may calibrate expectations based on the level of competition. In places where voters have a real opportunity to vote out the incumbent, learning about the prospect of better governance should raise their expectations while they may instead become more resigned in places with a dominant party knowing there is little they can do to effect change in their local government. Second, more dominant parties may be better able to manipulate the information provided to voters following treatment, thus lowering voter expectations or undermining recently acquired knowledge.

6.1 Voting simulations

I use evidence from two voting simulations conducted during the survey to evaluate hypotheses about the effect of information on voter behavior. One advantage of voting simulations is the ability to manipulate specific candidate characteristics and thus isolate the effect of those changes on voter behavior. Candidates in actual elections are different across multiple dimensions, making it difficult to identify the effect of any single characteristic (see Carlson 2010 for another use of voting simulations). A second advantage of the voting simulations I employ in the survey is they minimize the effect of survey biases on the estimation of treatment effects. The voting simulations are constructed so that the respondent cannot easily game the question or infer what is socially desirable. Other survey items may be subject to social desirability bias or desire to please the enumerator because respondents in the treatment group may have adopted new ideas about normatively "correct" behavior as a result of the treatment. Treatment effects generated by some survey questions may thus reflect changes in norms about what is socially desirable rather than changes in honest reports of past or future behavior.

One example of a treatment effect on respondent norms 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.[32]

[32]While the course highlighted the importance of voting for a candidate based on performance, there was no explicit discussion of vote-buying.

Each of the two voting simulations in the survey provides evidence in support of a hypothesis derived in the theory section. 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. In support of H1, 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, supportive evidence of H2 suggests that treatment makes the performance dimension more salient relative to these other voting criteria.

H1: Improving information will increase the cut point at which candidates are sanctioned

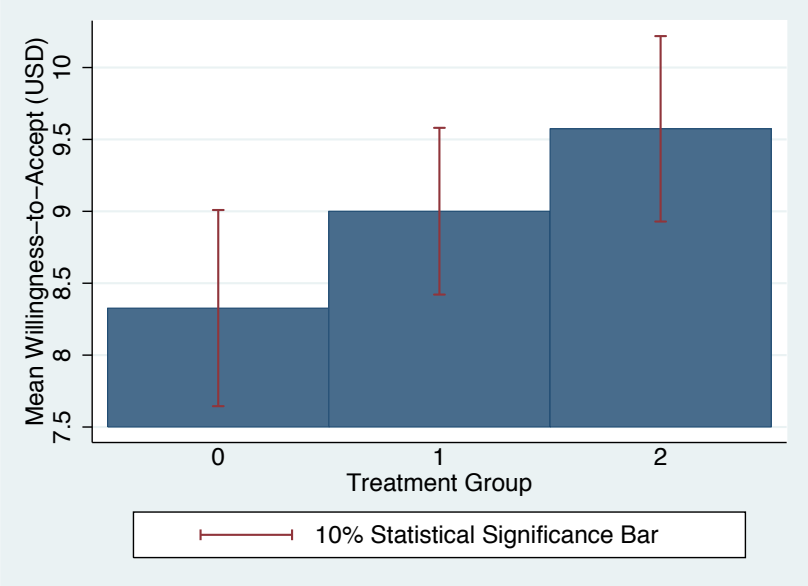
In the theory presented earlier, the cut point k at which voters sanction incumbents is increasing in p in both the "peanuts" and "big ticket" equilibrium. If the treatment increased voter knowledge with respect to local government capacity, essentially an exogenous positive shock to p, we should expect 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.[33] If the respondent initially chooses 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 more 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.

[33]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 elected.

Figure 3: Votes more expensive to buy in treated groups



When initially given the choice, 74 percent of survey respondents choose the high-performing candidate.[34] 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[35] – slightly but insignificantly more in the control group (23.9%) than the 2 treatment groups (21.7%, 22.8%).

Figure 3 shows the average price for which a vote can be bought by the low performing candidate in each treatment group. Table 3 summarizes treatment effects on vote price for various specifications with the basic specification from Figure 3 in the first column. To mitigate priming effects of prior survey questions and the influence of behavior in one voting simulation on the other, the order of the two voting 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. The second two columns of Table 3 restrict the sample to respondents who received this voting simulation first in the survey. The third two columns test whether the treatment effect is conditional on political competition. Models 2, 4 and 6 include all control variables from the most fully specified model.[36]

[34]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.

[35]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.

[36]Here, a likelihood ratio test fails to reject the null hypothesis that a standard regression model with no group-level random effects is a better fit than the mixed model. As a result, I employ a standard

There is some evidence of a positive effect of the mixed treatment (T2) on the price the low-performing candidate must pay to buy the respondent's vote. The coefficient on T2 is relatively large and significant at 10 percent both in the restricted sample and in the test conditional on competition; the coefficient on T1 is never significant.[37] Consistent with the results in the previous section, treatment makes it even harder to buy votes in places where there is greater political competition as measured by the absence of a majority party on the local council. The addition of control variables attenuates the effect of T2 in each case, but only slightly. The standard deviation of the dependent variable is 3.00 in the full sample and 4.00 in the restricted sample. 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 3: Treatment effect on willingness to accept gift for vote (in USD)

	Full sample		Restricted sample		Conditional effect	
	M1	M2	M3	M4	M5	M6
Control mean	8.31	5.95	7.19	3.34	8.01	3.26
T1 (ATE)	0.71	0.315	0.938	0.766	1.03	0.857
	(0.769)	(0.502)	(0.934)	(0.703)	(0.973)	(0.856)
T2 (ATE)	1.28*	0.511	2.06**	1.49*	1.85*	1.16
	(0.769)	(0.570)	(0.934)	(0.768)	(1.02)	(0.889)
Majority party					1.13	0.417
					(1.58)	(1.75)
Majority party x T1					-1.21	-0.255
					(2.02)	(2.14)
Majority party x T2					-1.82	0.834
					(1.96)	(1.88)
Controls	N	Y	N	Y	N	Y

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

The above analyses are performed only among respondents who said they were willing to sell their vote at some price. Because there is no significant treatment effect on whether a respondent agrees to sell their vote, this should be an unbiased test of the price at which a vote is sold, conditional on being willing to sell it. However, among certain subgroups of the population, this is not the case. For example, 40 percent of female respondents in the control group agreed to switch their vote for some amount of money compared to only 34 percent in T1 and T2 (the difference is significant at the 10% level). As one way of taking into account these potential biases, I re-run the analysis imputing a high price for missing values or respondents who said they were unwilling to sell their vote. I then check for heterogeneous treatment effects within social categories that might influence respondent behavior: leader status, gender, and schooling. Results are presented in Table 4 where M2 is run on a restricted set of observations in which this voting simulation occurred first in the survey. All models drop observations in which the respondent initially voted for the low-performing candidate so did not subsequently receive the vote-buying questions.[38]

linear regression model clustering standard errors at the commune level. The results are not substantively different across models.

[37]The difference in vote price between T1 and T2 is not statistically significant.

[38]For each analysis, I use a mixed model to account for random effects at the commune and village

Table 4: Heterogeneous treatment effect on willingness to sell vote (in USD)

	M1	M2	M3	M4	M5
T1	0.843	0.633	1.564*	0.216	-0.426
	(0.810)	(0.808)	(0.919)	(0.912)	(0.966)
T2	0.587	1.567*	1.188	-0.071	-0.578
	(0.815)	(0.814)	(0.929)	(0.919)	(0.982)
Leader			5.056***		
			(0.716)		
Leader x T1			-1.699*		
			(1.010)		
Leader x T2			-1.578		
			(1.017)		
Woman				-8.572***	
				(0.729)	
Woman x T1				1.997*	
				(1.023)	
Woman x T2				2.285**	
				(1.029)	
School					3.667***
					(0.654)
School x T1					1.680**
					(0.757)
School x T2					1.259*
					(0.743)
Intercept	33.460***	32.316***	31.408***	36.697***	31.417***
	(2.051)	(2.175)	(2.076)	(2.122)	(2.039)
N	4,090	2,009	4,090	4,052	4,081

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

As in Table 3, there is no significant treatment effect in the full sample but a significant and positive treatment effect of T2 in the restricted sample. Larger and more significant treatment effects emerge in the tests of heterogeneous effects. On average, leaders were much less willing to sell their vote than non-leaders. Perhaps because of this important initial difference, we see a significant positive effect of T1 on non-leaders that disappears for leaders. Women, on the other hand, were much more willing to say they would sell their vote on average. The positive and significant coefficients on the interaction terms can be interpreted to mean that both T1 and T2 had positive effects on women and no significant effect on men. Unsurprisingly, respondents with some formal schooling were also less willing, on average, to say they would sell their vote. Unlike leaders, however, treatment made these educated respondents even more sensitive to treatment, increasing the price at which they would sell their vote, in spite of their higher starting price. These observations are consistent with the notion of treatment being effective at imparting norms to subjects – the people less likely to be aware of them already (women) and the people most sensitive to them (educated) show larger treatment effects.

H2: Improving information will increase the likelihood of voting along the performance dimension

The comparative statics derived from the model 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 employs the "big ticket" electoral strategy, or conditions his vote based on the provision of public goods rather than just private transfers.

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 relative to other non-performance dimensions. The two non-performance dimensions made salient in this exercise are kinship and support of the village chief. Shared lineage between the voter and politician as well as a village chief who serves as a local intermediary for a particular candidate both signal the possibility of greater private electoral transfers.

The two candidates described to the respondent in this exercise each have a different record of performance. Candidate A built a well in one village 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. These descriptions are purposefully ambiguous on the performance dimension and were pre-tested to ensure substantial variation in preferences. The survey respondent is given a binary choice between Candidates A and B to reveal their preference for a particular type of performance.

levels, including fixed effects for block. Results hold when the full set of controls are included. Similar results also hold when a Tobit model is employed to censor the imputed values on the right-hand side and when an ordered probit model is employed, treating the imputed values as the last category.

To minimize the effect of social desirability bias, I implemented a survey experiment in which each respondent was 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, Candidate A is given the same last name as the respondent to signal kinship. In the Chief condition, Candidate A is given the support of the village chief. The Baseline condition gives only the previously-described performance information. The Kin and Chief cues are designed to be subtle. Further, each respondent received only one version of the candidate pair so is unlikely to be cognizant of the experimental manipulation.

The following analysis focuses on the change in mean vote share for Candidate A (the candidate that was manipulated) across each of the survey conditions. Table 5.A presents the mean vote share for Candidate A across each version of the candidate pairing. If kinship and chief support are two salient dimensions for Malian voters, the mean vote share for Candidate A should be higher in the Kin and Chief conditions compared to the baseline. Table 5.B presents the differences in means between each survey version. 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 and Baseline conditions for T1.

Because treatment may have affected voter preferences over candidates in the Baseline condition, 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 decreases the salience of the Kin and Chief dimensions relative to the performance dimension, 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 5.C reveal a negative treatment effect on the likelihood of respondents to vote along the kinship or chief support dimensions.[39] Voters in the control group were 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

[39]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.

Table 5: 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***	0.077**	-0.015
	(0.025)	(0.033)	(0.028)
Difference (Chief - Baseline)	0.080**	0.021	-0.033
	(0.030)	(0.027)	(0.035)
C. Difference-in-differences			
	T1 - C	T2 - C	T2 - T1
Kin condition	-0.045	-0.135***	-0.092**
	(0.045)	(0.037)	(0.045)
Chief condition	-0.062	-0.116**	-0.054**
	(0.040)	(0.047)	(0.039)

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

magnitude. In sum, this exercise demonstrates that treatment caused respondents to rely on the kinship or chief support dimensions less in their voting calculus, thus increasing the salience of the performance dimension relative to the other two.

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 5 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.

As another robustness check, I examine whether respondent behavior is consistent across the two voting simulations. One would expect that a respondent who values the performance dimension more should both be harder to buy off in the first voting simulation and less tempted by the kin or chief dimensions in the second voting simulation. The

data supports this expectation. There is a strong negative correlation between the price at which a respondent can be bought off in the first simulation (imputing high values for respondents who said they would not sell their vote at any price offered) and the likelihood of voting for the candidate described as a kin or having chief support in the second simulation.

H3: Introducing public information will increase 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-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. 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.

Among 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

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	-0.012	-0.006
	(0.020)	(0.013)	(0.016)
T2 (ATE)	-0.041**	-0.007	-0.004
	(0.019)	(0.011)	(0.015)
B. Difference-in-differences			
	Kin - Baseline	Chief - Baseline	
T1 - C	0.016	0.022	
	(0.015)	(0.014)	
T2 - C	0.034**	0.035**	
	(0.015)	(0.014)	

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

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 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.

H4: 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, 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 ($Nvillages \leq 12$). 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, population size, or number of villages. To address this, I control for number of villages, population size and wealth index in Model 2. In Model 3, I introduce all individual, village, and commune level controls from the most fully specified model in Table 2. 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.

These results can be interpreted to mean that T2 had no effect in communes where a minority of villages were treated and in communes where a majority of villages were treated, T2 had a significant positive effect on the price at which respondents say they are willing to sell their vote. Recall that surveys were conducted only in villages that received treatment, so the interaction effect is not measuring greater spillovers of information. Rather, one explanation is the majority effect is a function of voter consciousness of greater probability of a coordination effect. In other words, where the majority of villages are treated, there is a greater probability that voter coordination across those villages would result in a real impact on electoral outcomes.

6.2 External validity of survey outcomes

In this section, I assess the external validity of the results from the voting simulations, or whether behavior changes in hypothetical situations are reflected in actual changes in behavior. I discuss the effect of the information intervention on one observable outcome we might expect to change with treatment: petitioning leaders. While the ballot box is one place the voter can challenge poor performers, there are opportunities to do so between elections. In recent years, Mali's Ministry of Territorial Administration along with the German aid agency (GIZ) have been promoting annual town hall meetings called a "restitution publique." During these meetings, commune leaders publicly provide a financial and administrative account of the previous year to representatives from all 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. Trained observers recorded details of attendee participation during the meetings. Here, I analyze treatment effects on one observation recorded at the meetings: the extent to which participants challenge their elected leadership.

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

Variable	Coefficient		
	M1	M2	M3
T1	-0.082	-0.179	-1.226
	(1.026)	(1.024)	(1.289)
T2	-0.778	-1.133	-1.222
	(1.030)	(1.065)	(1.254)
Nvillages $\leq$ 12	-3.609***	-3.757***	-3.656**
	(1.336)	(1.360)	(1.856)
Majority x T1	2.307	2.204	3.327
	(1.657)	(1.642)	(2.189)
Majority x T2	5.364***	5.326***	5.494*
	(1.889)	(1.936)	(2.893)
Nvillages		-0.005	-0.042
		(0.050)	(0.083)
Population		0.000	0.000
		(0.000)	(0.000)
Wealth index		-5.926*	-3.351
		(3.225)	(4.751)
Intercept	9.704***	9.641***	9.880*
	(0.734)	(1.313)	(5.339)
N	95	95	811
R^2	0.175	0.251	
Other controls	N	N	Y

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

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. A F-test or Hotelling test of the joint equality of means for all covariates provides further evidence of balance. The null hypothesis of the joint equality of means cannot be rejected as shown in the last row of Table 8.

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

Table 8: Balance test on whether town hall meeting occurred in commune

	Mean	Difference in means
Wealth index	0.050	0.011
		(0.159)
Population (in 1000s)	22.267	-3.517
		(4.437)
Nvillages	17.063	3.905
		(2.718)
Mayor incumbency	0.254	0.027
		(0.097)
Majority party	0.349	-0.068
		(0.103)
Local radio	0.254	0.090
		(0.099)
Remoteness index	2.294	0.263
		(0.206)
Arrondissement	0.286	0.027
		(0.100)
F-test	F statistic	p-value
	0.923	0.516

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

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 9 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 9: 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†
T1 - C	1.80	0.125	0.148
T2 - C	1.71	0.109	0.178
T - C	1.76	0.086	0.113

†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.

6.3 Impact 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. This section analyzes the effects of treatment on politician behavior. Surveys were conducted with one elected local government official from each of the sample communes.[40] Results generate some evidence of how leadership responds to the introduction of a civic information course, at least in the short-term. In the pre-analysis plan (H7), I predicted that increases in voter information should increase politician transparency. The results presented below suggest the opposite.

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, or 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	-0.46	-0.43
	(0.508)	(0.673)	(0.376)
Transparency	1.25	1.10	1.38
	(0.398)***	(0.466)**	(0.495)***
Treat	-0.08	-0.30	-0.01
	(0.337)	(0.436)	(0.229)
Intercept	1.83	1.91	1.76
	(0.271)***	(0.304)***	(0.321)***
N	93.00	62.00	62.00
R^2	0.26	0.32	0.34

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

[40]The survey targeted adjunct mayors, one of three councilmembers that sits on the commune bureau with the mayor. Adjuncts are generally the most informed about commune politics and council affairs.

Table 10 shows the results of a difference in difference estimation for each treatment group as well as the treatment groups combined. The unit of analysis is the commune and fixed effects for block are included. 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 of Table 10 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. The magnitude and significance of the coefficient is slightly attenuated by the addition of the standard commune controls. However, including further controls from the politician survey about the politician himself (whether he spent significant time outside the commune and whether he is related to the mayor) and about the politicians's party (whether they disseminated gifts or gave speeches during the previous election campaign) actually increases the size and significance of the difference-in-difference estimator.

A second indication of the local council's response to treatment is the frequency with which they hold public meetings. The surveyed leaders, adjuncts of the mayor, were asked questions about activities undertaken by the commune council in the months since the treatment was implemented. The leaders were then probed for details of each event to improve validity of the responses. Table 11 shows the average treatment effect on the frequency of public meetings held by the commune council since treatment.

Again, the elected leadership in treated communes appears less transparent than in control communes. Particularly in T2, the surveyed councilmembers report that the mayor's office held fewer public meetings post-treatment. There is a high probability that the councilmembers were aware of the treatment. First, the civics course always occurred in the commune seat where councilmembers work and often live. Second, a component of the civics course asked participants to seek out information about commune activities, so councilmembers in treated communes were more likely solicited for information from villagers than in control communes.

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.

7 Discussion

Taken together, these analyses provide evidence of an effect of the mixed information treatment (T2) on voting behavior, but little evidence of an effect of the basic treatment (T1). Both treatments increased voter expectations of government performance, particularly in communes with some political competition or without a majority party. In the first voting simulation, T2 increased the average price at which a politician would have to buy votes. In the second voting simulation, T2 decreased the salience of the two non-performance dimensions (kin and chief support) relative to the performance dimension. It is particularly striking that we see these effects of treatment due to 1) the brevity of the civics intervention, 2) relatively low power of the statistical analyses, and 3) the fact that outcomes are measured using a representative sample of villagers, many of whom did not participate in treatment.

There is also support for the idea that, at least in this context, voting is a strategic calculation in which beliefs about the actions of others affect an individual's actions. Consistent with the interpretation that voters face a coordination problem, the mixed treatment intervention enabled voters to better coordinate along the performance dimension. Treatment also appears to work 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.

Since the above findings are generated by survey measures, one concern may be the relevance of the intervention for behavioral outcomes. Using observations of participants at town hall meetings, I demonstrate that survey-based findings are consistent with treatment effects on actual behavior. People in treated communes were more likely to challenge their local leadership at these meetings.

While there is little difference in treatment effects between T1 and T2 for outcomes measuring changes expectations, the differences in estimated treatment effects for T1 and T2 are large and sometimes significant in tests of voter behavior. Comparing actual course participants in T1 communes versus T2 communes provides another estimation of the difference in effects between treatment groups. Confirming the previous findings, there is no significant difference between T1 and T2 among course participants for the expectations index. 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 potential and actual government performance. Information about potential government performance is necessary 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 first voting simulation, 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 actual performance information on its own would not be sufficient to produce changes in behavior (similar to previous experimental findings). But together with information about potential government performance, the two types of information generate significant change.

For those concerned with improving government performance, changes in voter behavior is only a means to an end. As one way of testing whether more discerning voters lead to better politician performance, I examined politician behavior along two dimensions. Leaders in treated communes appear less likely to campaign on transparency and less likely to hold public meetings. These analyses indicate that elected leaders become less transparent in response to treatment, suggesting that the information intervention is not sufficient to change leader behavior, at least in the short-run.

Politician ability to manipulate information is one explanation for existing information asymmetries, but this cannot answer the question of why opposing parties or dissatisfied voters fail to seek out information and provide it to other voters. Related work (Gottlieb, 2012) suggests that opposing politicians may fail to provide information because incentives to collude are stronger than incentives to compete. In addition, analysis of civic activity data collected by the author suggests that important socio-economic inequalities impede mobilization by voters, in particular those who are worst-off under the status quo.

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.

However, evidence of negative impacts of information provision on politician 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 voter behavior.

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Appendix 1: Linking hypotheses to measurable outcomes

Hypothesis	Outcome Measures	Results
H0: 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.	*Expectations index :* T1 & T2: Weakly confirmed; (Strongly confirmed when interaction term with majority party is included)
	Q65-Q69: Evaluates voter understanding of how democracy works.	
	Q63: Assesses beliefs about the anonymity of voting.	
	Q74: Asks about retrospective voting.	
	Q86: Asks which development projects they expect of current local government.	
H1 & H2: 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: Null finding; T2: Strongly confirmed (in restricted sample)
	Candidate vignette B: Evaluates the saliency of the performance dimension versus 2 others.	T1: Null finding; T2: Strongly confirmed
	Q70,76: Voters rank dimensions along which they and their neighbors vote.	T1 & T2: Null finding
H5: 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.	*Information index:* 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
H6: 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.	Null finding
	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: Strongly confirmed for T2 Event log: Null finding
	Town hall meeting: Likelihood of petitioning leaders.	T1 & T2 pooled: Weakly confirmed†
H7: Increased knowledge about government capacity among voters will lead to more transparency of local government.	Q50: Did the government inform people more often about the town hall meeting.	Null finding†
	Politician survey: Frequency of public meetings.	T1: Weakly disconfirmed; T2: Strongly disconfirmed
	List experiment in politician survey: Assesses the extent to which politicians will campaign on a message of transparency.	T1 & T2 pooled: Weakly disconfirmed

Strongly confirmed: treatment effect in hypothesized direction significant at least at 10% level in basic specification
Weakly confirmed: treatment effect only significant under some specifications with controls
Null finding: treatment effect not significant at conventional levels
Disconfirmed: significant treatment effect in opposite direction than hypothesized
†Reduced power due to town hall meetings being held only in a subset of sample communes

Appendix 2: Course curriculum outline

1. What is democratic decentralization?
    (a) Voting exercise to elect course assistant who will collect information about the village and commune as part of the course
    (b) Brief history of democracy and decentralization in Mali
    (c) Levels of territorial administration and their different responsibilities
    (d) Main differences between democratic and authoritarian systems
    (e) How the commune government engages in local development
        i. Local actors involved in development
        ii. Responsibilities/capacities of the local government and its actors
2. Role of the citizen
    (a) Rights and responsibilities of the citizen
        i. Pay taxes
        ii. Right to information
        iii. Participation (public meetings, elections)
    (b) Key commune activities that affect citizens
        i. Planning (requirement to involve citizens in the process)
        ii. Budget creation
            A. Size of current budget and sample line items
            B. Project to be funded with 2010 ANICT funds
            C. Example of Dioro commune where public debate over the budget occurred
        iii. Obligation of government to communicate policies and decisions to the population
        iv. Vital records such as voter registration and birth certificate (with fee amount for each service)
    (c) Avenues of recourse (each with examples of how successfully implemented in other communes)
        i. The prefecture: an apolitical administrative unit that oversees communes
        ii. Lobbying the commune council
        iii. Sanctioning past performance
            A. Questioning leaders at public meetings, particularly the public restitution
            B. Not voting for incumbents who performed poorly
3. Actual government performance

(a) Governance indices, commune level

   i. Number of development projects realized in your village
   ii. Percent of commune development projects realized in the commune seat vs. other villages
   iii. Number of meetings held by commune council

(b) Governance indices, village level

   i. Number of meetings attended by your village chief
   ii. Number of times village chief transmitted information from meetings back to village
   iii. Rate of tax recovery in village

(c) Examples of successful governance in other communes

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