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Breaking the Clientelistic Voting Equilibrium: The Joint Importance of Salience and Coordination

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Abstract

Scholars argue that access to information about a politician's programmatic performance helps voters reward (punish) good (poor) performers. But in clientelistic democracies, where resources are made conditional on electoral behavior, voters may not want to defect from voting for a clientelistic candidate if they do not believe that others will. We argue that two conditions must hold for information about politician performance to exercise its intended effect: voters must care about the information provided *and* believe that others in their constituency care as well. Experimental evidence from legislative elections in Benin reveals that voters rewarded good programmatic performance only when information was both accompanied by a civics message *and* widely disseminated within the electoral district. Otherwise, access to positive legislative performance information actually lowered incumbent vote share. These results demonstrate the joint importance of salience and voter coordination in shaping information's impact and breaking the clientelistic voting equilibrium.

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1 Introduction

Electoral accountability is a central idea in democratic politics, but, in practice, poorly performing politicians are often voted into office again and again. A prevailing explanation for this pattern is that voters lack basic information that would allow them to distinguish between good and bad candidates (Pande, 2011; Banerjee et al., 2011; Ferraz and Finan, 2008). Increase access to information about how incumbent politicians perform in office, and they will reward the better performing candidates, or so the logic goes.

Yet recent empirical investigations of the effect of information on electoral behavior in low- and middle-income democracies have uncovered mixed results. In some cases, information provision has led voters to punish poorly performing politicians (Banerjee et al., 2011; Ferraz and Finan, 2008). But in other cases it has had no effect (Humphreys and Weinstein, 2012; Lieberman, Posner and Tsai, 2014), and sometimes voters punished challengers as well as incumbents (Chong et al., 2015). These findings raise a puzzle: under what conditions will voters reward (punish) incumbents on the basis of information about their performance while in office?

We address this question in the context of clientelistic democracies, settings where politicians and parties generally seek votes through the conditional provision of selective and particularistic benefits, rather than policies, programs, and public goods.¹ Our focus is on voter responsiveness to information about incumbent performance on a programmatic, rather than a clientelistic, dimension. *Under what conditions will voters in a clientelistic democracy condition their votes on programmatic information?* Since voter preferences and action can drive elites to shift away from the use of clientelism (e.g. Weitz-Shapiro, 2012), an answer to this question contributes to the literature seeking to identify the conditions under which clientelistic politics shift to more programmatic politics, a central concern in comparative political economy.

We argue that two conditions must hold for voters in clientelistic democracies to condition their votes on programmatic performance information.² First, voters must care about the programmatic performance dimension about which they have information, and believe that it is relevant for their welfare. This condition alone, however, is unlikely to be sufficient. Because access to valued resources in clientelistic democracies is often made conditional on past electoral behavior, voter beliefs about how other groups of voters are likely to vote are also important. If they go it alone, they risk losing out on key material rewards.

Voters in clientelistic democracies thus face a clientelistic voting equilibrium: they have little incentive to vote along a new programmatic performance dimension unless they believe others will as well. Scholars (e.g., Lieberman, Posner and Tsai, 2014) have suggested that these factors, among many others, might condition the impact of political information on citizen behavior. But no overarching theory has been offered to address how moderators might interact to condition the impact of political information on citizen behavior. We push the frontier by theorizing and experimentally testing the ways in which salience *and* coordination interact to jointly shape the voter calculus. Indeed, we show that the joint importance

¹This definition follows Chandra (2004), who provides a similar definition of a “patronage democracy.”

²These are moderators that we theorized *ex ante* (in a pre-analysis plan registered with EGAP <http://egap.org/content/registration>) would matter for the effectiveness of information provision.

of salience and strategic coordination are particularly helpful in understanding the effect of information on voter behavior in clientelistic contexts where voters value targeted transfers over more programmatic activities and believe there are material costs to failing to coordinate on the perceived winner under the status quo.

Our setting is the 2015 legislative elections in Benin, one such clientelistic democracy, where members of the National Assembly are charged with legislative and executive duties, rather than with constituency development, but where voters vote primarily along the constituency development dimension (Hounkpe and Warren, 2012; Banegas, 1998; Koter, 2016; Wantchekon, 2003). We conducted a large-scale field experiment that delivered information about one dimension of programmatic performance – legislative performance – of incumbent politicians running in the election.³ Villages were randomly assigned to receive this legislative performance information (or not). Additionally, the content and method of the delivery were randomly assigned. To assess the importance of information salience, some villages received a civics message that illustrated the relevance of the legislative performance dimension to voter welfare, while others did not. To assess the importance of within-group voter coordination, voters in some villages received the information privately, while others received it publicly (at a village screening). To assess the importance of across-group voter coordination, some villages were the only ones in their areas to receive performance information (and were told as much), while others were one of many villages in their areas to receive the information (and were told as much). Our argument, that both salience and coordination must hold in order to observe a move away from clientelistic voting and toward programmatic voting, is explicitly tested in the villages that received the information in a widely disseminated civics campaign.

Using official village-level data on incumbent vote share, we indeed find that voters rewarded (punished) good (poor) legislative performance only when the information was provided as part of a widely-disseminated civics campaign: voters received the information with a civics message *and* were informed that people in other villages in the district had received the same information. We find no differences between private and public provision, suggesting that within-community coordination was not a constraint. Finally, we find that when positive legislative performance information was not widely disseminated across the district, this information unexpectedly *lowered* support for the incumbent.

This latter result raises a new question: under what *conditions* is information about good legislative performance received as bad news to voters? While our experiment was not designed to answer this question, we leverage additional qualitative and quantitative data collected through focus groups, elite interviews, and our baseline survey. These data produce two key insights that, taken together, offer a plausible explanation for the unexpected finding. First, in the absence of a civics message, voters generally value transfers, not legislative performance. On its own, this unsurprising feature of Beninese voters cannot explain the negative effect of information: weak preferences for legislative performance should lead to *null*, not negative results of our experiment. Second, and importantly, some voters also perceive legislative performance and the ability to provide transfers as substitutes rather than complements. That is, incumbents that perform well legislatively are expected to perform relatively worse in the provision of transfers. Under these conditions, “good” legislative performance information may be interpreted as bad news about the incumbent – absent interventions that increase the salience of legislative performance

³We conducted this study in conjunction with a multi-country effort to accumulate knowledge about the relationship between information and accountability across contexts.

for voters. A more general point is that voters may value some dimensions over others, and information about one dimension may have unexpected effects on beliefs about alternative dimensions. Just as Vaishnav (2012) found that in some contexts voters may actually value criminality, our study stresses the need to understand how voters in a given context view different dimensions of politician performance before attempting to predict how information about politician performance will affect vote choice.

This paper makes a number of contributions. Foremost, our findings highlight the joint importance of information salience and voter coordination in facilitating the impact of programmatic performance information on voter behavior in clientelistic contexts where there are both clientelistic and programmatic dimensions of politician performance (Lindberg, 2010) and voters value the former over the latter. Importantly, we show that the salience of information cannot be considered in isolation. Rather, in clientelistic contexts where voters expect bloc rewards for coordinating around candidates, shifting the salience of a particular performance dimension is unlikely to affect electoral behavior in expected ways without the ability of voters to coordinate around newly salient information. Our study thus advances the literature that, in the face of null or surprising results (Humphreys and Weinstein, 2012; Lieberman, Posner and Tsai, 2014; Chong et al., 2015), has called for more attention to the moderators of information's impact. We do so not by considering any single moderator in isolation but rather by considering the implications of their interaction.

Finally, our experiment brings together the literature on clientelism with the literature on information and accountability. Scholarship on clientelism often focuses on structural conditions (Hicken, 2011; Kitschelt and Kselman, 2013; Stokes et al., 2013) or policies (De La O, 2015; Larreguy, Marshall and Trucco, 2015) that lead to programmatic politics. Meanwhile, the literature on information and accountability studies individual-level behavior but less often focuses on switches from personalistic to programmatic voting (c.f. Wantchekon and Fujiwara 2013; Keefer and Khemani 2014). Our study investigates micro-level influences on personalistic and programmatic voting behavior and finds evidence that information signals – that take seriously the clientelistic context in which voters are operating – have the potential to make programmatic information relevant for voter behavior.

2 Information Salience, Strategic Coordination and Vote Choice

Our understanding of the relationship between voter information and politician behavior is informed by standard moral hazard models of electoral accountability (Barro, 1973; Ferejohn, 1986). These models assume that an elected politician is motivated both by being in office and by any personal benefits he can obtain through the misappropriation of public funds for private purposes. Voter preferences are unaligned with politicians inasmuch as they want to minimize misappropriation. As in standard principal-agent setups, the better the information the principal (voter) has about the agent (politician), the better the principal is able to control the behavior of the agent. Voters with imperfect information follow a decision rule that allows them to translate perceived welfare into vote choice (Fearon, 1999). Providing information about actual government policy or incumbent behavior then reduces the error with which voters translate their own welfare into beliefs about actions of the incumbent government. This logic yields the following standard expectation, one of the main hypotheses specified in our pre-analysis plan:

Hypothesis 1 *Increasing access to positive (negative) performance information about an incumbent will have a positive (negative) effect on electoral support for that incumbent.*

This expectation, however, is based upon two implicit assumptions that may not always hold, particularly in clientelistic democracies. First, the hypothesis assumes that voters believe the new performance information is relevant for their welfare. Second, it assumes that voters are non-strategic and do not condition their votes on expectations about the behavior of others. And yet, in clientelistic democracies, where informal patron-client ties dictate voting behavior and rewards, voters face no incentive to defect from a clientelistic electoral equilibrium where they vote and expect others to vote for the best-performing clientelistic candidate. This implies that any information intervention must consider a) the relative salience of the information provided to the voter and b) how the targeted nature of clientelist benefits gives rise to incentives for strategic coordination that can make even salient information difficult for voters to act on.

2.1 Information Salience and Dimensions of Politician Performance

The first assumption implicit in Hypothesis 1 is that voters believe the new performance information is relevant for their welfare – that is, the information is *salient*. However, there are generally myriad dimensions of politician performance, and these dimensions may vary in their salience in the minds of voters. In clientelistic democracies, for example, politicians often spend – and are expected to spend – significant time and effort performing informal tasks that sustain clientelistic relationships (Hounkpe and Warren, 2012; Lindberg, 2010). Yet this highly salient activity may not be what legislators are tasked with *de jure*. If perceived salience weights the value of information about a particular dimension of politician performance in a voter’s calculus, then information about a weakly salient dimension may be insufficient to change voter behavior, especially if voters have strong priors about another highly salient dimension.

For the purposes of illustration and application to our empirical context, we distinguish between two performance dimensions that are potentially differentially salient in the minds of voters: legislation and transfers. By legislation, we mean performance in activities related to making laws and holding other branches of government accountable, e.g. participating in policy debates, overseeing the executive, and representing constituent interests during the policymaking process. By transfers, we instead mean activities related to handling the needs of individuals or particular groups or areas, i.e., resources, inducements, or favors that are targeted by the politician directly to individuals or a locality, with or without the expectation of *quid pro quo* at election time.⁴

If voters value transfers over legislation or believe that legislative performance is not sufficiently relevant to their welfare (which we will show is true in ours and other contexts), then information about legislative performance alone may have little impact on voter behavior. But information interventions can additionally provide signals about the salience of the dimension about which information is being provided. We thus test the following hypothesis, specified in our pre-analysis plan:

⁴Distinguishing legislative performance from transfers is standard in the literature, although the terminology differs across studies. For example, Dixit and Londregan (1996) distinguish between programmatic and tactical redistribution, the latter of which is the corollary to our transfers term. Magaloni (2006) distinguishes an incumbent’s economic performance from the transfers that the incumbent provides certain groups or individuals. We use transfers in a broad sense, in the way that Magaloni (2006) employs the term.

Hypothesis 2 *Increasing the salience of the legislative performance dimension will amplify the effect of information about legislative performance on voter behavior.*

Our experiment tests this salience hypothesis with a treatment arm that provides legislative performance information in conjunction with a civics message explicitly designed to increase the salience of this dimension.

2.2 The Problem of Voter Coordination

Also implicit in Hypothesis 1 is that voters are non-strategic, or at least that information will affect a voter's calculus independently of how it affects others' evaluations of the candidate. If, however, voters are strategic, then an information signal may not elicit the predicted effect if it changes only a voter's personal valuation of the candidate but does not change beliefs about other voters' behavior.

Voters should be motivated to coordinate with one another on a particular candidate when there are strategic complementarities – or perceived benefits to voting at high rates for a particular candidate. In a primarily programmatic electoral system, voters are considered to be strategic when they choose a candidate they think is more likely to win at the expense of a more ideologically preferred candidate (McKelvey and Ordeshook, 1972).⁵ In primarily clientelistic systems where there are few ideological attachments and where the provision of transfers is often *contingent* on voting behavior,⁶ voters may instead be motivated to coordinate their votes because of the expectation of targeted collective goods if they do. This type of party behavior resembles “contingent prize allocation” (Smith and Bueno De Mesquita, 2012) and is particularly useful where groups of voters are separated into geographically distinct precincts, e.g. villages, such that targeted collective goods are semi-excludable and vote outcomes are observable at the group level. In either case, strategic voters have to coordinate on a particular candidate. Additionally, in a case of multiple salient performance dimensions, strategic voters may also have to coordinate on which dimension of performance drives their electoral decision (see Gottlieb (2016) for evidence of this).

Below, we address two types of coordination problems that arise from this set-up – within- and across-group, and how the nature of information dissemination moderates outcomes in the presence of each. Morris and Shin (2002) identify that, in the presence of strategic complementarities, *public* dissemination of information will have an independent effect from the content of the information itself. Because of the coordination motive, the common knowledge produced by public information – that others are also taking up the knowledge and everyone knows it – increases its potential impact. Arias et al. (2017) show that such a coordination effect can be facilitated by information dissemination through tight-knit social networks; but as long as voters know enough about fellow constituents to update their beliefs about how others will act in response to new information, such a mechanism is not required. How widespread public information has to be for these effects to be felt depends on the nature of the coordination problem. We address two levels of coordination problems that voters in a clientelistic democracy might face.

⁵Cox (1994) later shows that in multimember districts, voters who care only about the outcome of the election will strategically desert both candidates who are “too weak” (submarginal) and candidates who are “too strong” (supermarginal).

⁶Clientelism involves the allocation of targeted transfers and particularistic benefits in exchange for votes (Hicken, 2011)

2.2.1 Within-group Coordination

In an electoral system that rewards bloc voting, voters in a village or community can increase the likelihood of a collective transfer if they coordinate on the same candidate.⁷ One implication of this sort of strategic voting that distinguishes it from decision-theoretic predictions is that a voter may forgo his or her own preferences if they do not align with the preferences of the local majority.

Take the case of a village in which the incumbent is viewed negatively and priors are to coordinate on another candidate in an upcoming election. If a positive legislative performance signal is provided to a voter, that voter may not change her vote in favor of the incumbent if she believes sufficient others in the village will not do the same. This is particularly problematic if voters have a difficult time sharing information within the village. In this case, providing a public rather than private information signal about the quality of the candidate may be necessary to change voter behavior. We thus test the following hypothesis, pre-specified in our pre-analysis plan.

Hypothesis 3 *The provision of public information will have a greater impact on voter behavior than the provision of private information.*

In our experiment, we test this within-group coordination hypothesis with treatment arms that either provide information to voters in private in their homes, or in public at a community event.

2.2.2 Across-group Coordination

In an electoral system where voters prefer transfers over legislative performance, generating favorable electoral support for a strong legislative performer may be elusive even if an entire village is given public information about that high performer. While voters in the village may now genuinely prefer a better legislative-performing candidate, they may suffer in a clientelistic setting if they are the only community to make this electoral choice. For example, if candidates can monitor villages via polling station returns, then winning candidates can withhold collective transfers from the village that bloc voted on the stronger legislative performer.⁸ This scenario requires coordination not only within villages, but across the electoral district. Thus, voters receiving information about legislative performance *and* the value of the legislative dimension may switch to voting for a better-performing candidate only if they believe sufficient others in their *electoral district* will do the same such that their preferred candidate has a reasonable chance of winning.

This logic implies that increasing the proportion of villages within an electoral district with access to infor-

⁷This setup abstracts away from the fact that voters also want to coordinate on the candidate most likely to win in the constituency for two reasons: 1) if voters are uncertain how others in their village will vote, they are likely even more uncertain about which candidate is favored to win and 2) it is possible that bloc voting for a losing candidate is preferred to the village splitting its vote because opposition parties may also have a budget to distribute transfers. We consider the problem of inter-village coordination next, which may additionally apply here.

⁸Indeed, focus group respondents in Benin report that electoral rewards and punishments are distributed to the village as a bloc: those that vote in large numbers for the incumbent are typically rewarded with “gifts” and money; those that do not are punished with neglect. This is particularly feasible in contexts like ours where election results are made available at the village level and party agents are permitted at each polling station, ensuring close monitoring of local-level support. In a similar context, Gottlieb and Larreguy (2016) more systematically show that politicians are good at identifying local levels of support across villages.

mation will amplify the impact of that information on voting behavior. We test the following hypothesis, pre-specified in our pre-analysis plan:

Hypothesis 4 *Increasing the proportion of communities in an electoral district with access to information will increase the impact of that information.*

Our experiment tests this across-group coordination hypothesis by randomly varying the proportion of communities in an electoral district that receive legislative performance information, and informing voters accordingly. Importantly, we can also test whether increasing salience alone is sufficient, or whether facilitating coordination within or across the electoral constituency is also necessary to change voter behavior.

3 Setting

We test our hypotheses in Benin, a West African country representative of other clientelistic democracies in that voters value transfers over legislation (Wantchekon, 2003; Hounkpe and Warren, 2012).⁹ Similarly in Ghana, MPs are held accountable for constituency service and clientelistic activities more than legislative activity (Lindberg, 2003, 2010). And in Mali, voters focus on the transfers dimension, even though activities in this area are entirely informal (Soumano, 2011).

As with other former French and Portuguese colonies (Barkan and Mattes, 2014), Benin's legislature is charged only with legislative and executive oversight duties, not with transfers to the constituency.¹⁰ At the same time, two previous studies (Fujiwara and Wantchekon, 2013; Keefer and Khemani, 2014) demonstrate the possibility of shifting voter attention away from constituency transfers and toward legislative appeals.

Although Benin has a proportional representation system with multi-member districts,¹¹ in practice, many legislators focus on and "take care of" a particular commune within their constituency, facilitating a one-to-one correspondence of incumbent legislator to commune.¹² Pre-experiment focus groups confirm that villagers can name and agree on a single legislator as their incumbent representative. We thus restrict our experimental sample to communes in which the incumbent legislator was running again and our local partner organization verified this one-to-one correspondence.¹³

⁹While targeted transfers may come in the form of a collective good, the public goods outputs of legislative performance are of a much more universal nature.

¹⁰While Benin's is a multi-member district proportional representation system, the district magnitude is relatively low. This may contribute to more personalistic politics in the country, despite the electoral system.

¹¹Administratively, Benin is divided into 12 departments with two legislative constituencies in each, for a total of 24 constituencies. The next administrative level down is the commune, and there are, on average, three communes per constituency. Villages (or their urban equivalent, quarters) then nest within communes.

¹²Voters elect an average of 3.5 deputies per constituency. With 77 total communes distributed among the constituencies, there are 3.2 communes per constituency, on average. This makes feasible, as a rule of thumb for voters and legislators, a one-to-one mapping of communes to legislators. We note that this mapping in practice is consistent with expert evaluations of the party system in Benin as fragmented and weak (Banégas 2003; Gazibo 2012).

¹³In the conclusion, we discuss the implications of this sampling decision for the generalizability of our results.

4 Research Design

This section details the experimental design that we use to test Hypotheses 1 through 4. The experiment involves the dissemination of information about incumbent legislative performance, via videos, in advance of Benin's April 26, 2015 National Assembly elections.¹⁴ We begin this section by describing the details of each treatment condition. The scripts used in each of the treatment videos are provided in Appendix B. Second, we describe our randomization procedure. Finally, we discuss details of the implementation.

4.1 Performance Information Treatment

Treated participants in the study were given information about their incumbent legislator's relative performance in the National Assembly. We provided this information in the form of a video in order to hold constant the exact wording and tone of delivery across treatment conditions while making the information accessible to people of all education levels, literate and illiterate. In the video, a male actor read a script in a neutral tone, as a news caster or radio host might. The video included graphics to illustrate key points. It was recorded in French and then dubbed in local languages as necessary.

The information provided was drawn from official reports of the Office of the President of the National Assembly.¹⁵ The video provided performance information about an incumbent legislator's: 1) rate of attendance at legislative sessions, 2) rate of posing questions during legislative sessions, 3) rate of attendance in committees, and 4) productivity of committee work (the number of laws considered by the committee). The video provided raw data for each of these four performance indicators and presented two summary indicators. The first, an index of plenary performance on a scale of 1-10, took the average of normalized scores on the first two indicators: attendance and participation during full legislative plenary sessions. The second, an index of committee performance also on a scale of 1-10, took an average of the normalized scores on the second two indicators: attendance at committee meetings and productivity. To further synthesize the performance information, we produced a global performance index which averaged scores from the first two indices. In Appendix E, we discuss our efforts to validate these indices using a separate dataset on politician wealth and interviews with legislators.

Figure B.1 shows two examples of how the information was presented. Bar graphs highlight the performance of the legislator responsible for that commune relative to other legislators in the department (a local average)¹⁶ and the country (national average). Red (Green) bars were used when the incumbent's performance fell below (above) the local average.¹⁷

¹⁴In designing the experiment, we followed the ethical principles agreed upon by the Metaketa initiative, as outlined in the joint metaketa pre-analysis plan: that the intervention consist of information that existed in the political system, be provided with consent, in a non-partisan way, without deception, and in cooperation with a local group.

¹⁵Reports are supposed to be made publicly available but, in practice, are difficult to obtain.

¹⁶There are two constituencies per department and thus on average 6-7 legislators per department.

¹⁷Pre-intervention focus groups confirmed that the video content was comprehensible to villagers in Benin.

4.2 Civics Condition

To test the importance of information salience, the experiment also varied whether voters heard a civics message emphasizing the importance of the legislative performance dimension. Treated participants were shown a video with either *only* the information about relative legislator performance (*Info Only*), or that same information *plus* the civics message (*Civics*). The civics message described the main responsibilities of legislative deputies. It then provided three concrete examples of how legislative performance (or lack thereof) can impact voter welfare. A positive example of good legislation was the passage of an anti-graft law requiring public servants to disclose assets. A negative example of a missed opportunity was the failure of the legislature to vote on and pass a health insurance scheme that was proposed in 2008. Finally, a positive example of executive oversight detailed how the legislature opposed changes to the Constitution proposed by the president that would expand his power.

The civics treatment was provided to treated participants immediately before they received the legislative performance information. Note that participants in the civics condition also heard the information provided in the information-only condition.

4.3 Coordination Conditions

To test our predictions about within- and across-group voter coordination, we also varied the method by which the information was disseminated. First, treated participants received the intervention either privately by watching a video on a smartphone in the respondent's household (*Private*) or publicly through the screening of the same video via a projector in a public location in the village (*Public*). The public treatment was designed to facilitate intra-village coordination.

Second, participants were told during the intervention how many other villages in their commune were receiving legislative performance information. This *high dosage* treatment was designed to facilitate inter-village coordination, and is described in greater detail below.

4.4 Randomization

We implemented a two-stage randomization procedure. First, we randomly assigned each of the 30 communes in our sample to either the low or the high dosage condition, blocking on incumbent legislative performance, which is observed at the commune level, and on north/south, since being in the culturally distinct north or south of the country is an important moderator of political behavior in Benin (Adida, 2015). Within 4 blocks (high and low performance in the north and south) of communes, we assigned half to high-dosage and half to low-dosage treatment.

Second, we randomly assigned treatment conditions within communes. The unit of randomization was the rural village or its equivalent urban quarter, the lowest level of social and territorial organization. In high dosage communes, we randomly assigned each village/quarter to one of five conditions: 1) Informa-

tion Only/Private, 2) Information Only/Public, 3) Information + Civics/Private, 4) Information + Civics/Public, or 5) Control. Three villages/quarters in each of the 15 high dosage communes were randomly assigned to one of the four treatment conditions, and the remainder villages/quarters in the commune were assigned to the control group. Thus, in the high dosage communes, we have a 2×2 factorial design with a pure control group. To increase statistical efficiency, we assigned villages to experimental conditions while stratifying on urban/rural status and electoral competitiveness of the village in the previous legislative election.

In the low dosage communes, where only one village/quarter received treatment, we randomly assigned units to one of two conditions: 1) Information + Civics/Public, or 2) Control. One village/quarter was assigned to the treatment condition, while the remainder were assigned to control. We use the Information + Civics/Public condition in the low dosage communes because we believed *a priori* that it would have the strongest effect, thus making it harder for us to detect dosage effects.

Table 1 summarizes the experimental design. In low dosage communes, there are 15 treated units (villages/quarters) and 643 controls. In high dosage communes, there are 45 treated units in each of the 4 treatment conditions (180 treated in total) and 486 controls. As specified in our pre-analysis plan, we use all non-treated villages/quarters in our sample communes as controls, which substantially increases our statistical power. A baseline survey was conducted in all treated villages/quarters (180 in high dosage and 15 in low dosage), in three control villages in each high dosage commune (45 total), and in one control village in each low dosage commune (15 total), for a total of 225 units in the survey sample.

In each private condition village/quarter, 40 participants were shown one of the treatment videos. In each public condition village/quarter, 60 people were recruited and invited to the public screening, and on average about 50 participants attended (standard deviation is 13).¹⁸ The average village/quarter in our sample contains about 320 households, meaning that an individual from 12-15 percent of households was treated.¹⁹ We expect substantial within-household information transmission and we found experimental and qualitative evidence of significant information transmission within communities (results discussed below), which means that many more people in each community were likely exposed to the information in the treatment.

More details on our randomization procedure can be found in Appendix A. In particular, Figure A.1 outlines the sampling and randomization procedure and shows each of the experimental conditions with the sample size of villages and survey respondents in each condition. Then, Figure A.2 geographically plots the sampled villages and quarters in each of the 30 sample communes.

4.5 Implementation

We designed and conducted the experiment in collaboration with the Centre de Promotion de la Démocratie et du Développement (CEPRODE), an independent, non-governmental, non-partisan Benin-based orga-

¹⁸We treated/invited an equal number of men and women.

¹⁹In urban quarters, the average number of registered voters was about 1200, which means that at most 3-4 percent of registered voters were treated in urban areas. In rural villages, the average number of registered voters was about 870, and so at most 4.5-6 percent of registered voters were treated.

nization. To avoid overlap with the two-week period of campaigning prior to the election, the experiment and baseline survey were conducted from March 9 - April 9, 2015.

Two representatives from CEPRODE implemented the experiment in each treated village/quarter. Upon arrival, they sought permission from the local chief or leader to conduct the study, informing them it was a collaboration between CEPRODE and researchers from American universities.²⁰ Individuals from the community were then randomly sampled, given information about the project, informed that it was a collaboration between a local NGO and American researchers, and given an opportunity to consent to participate.

A subset of consenting participants took the baseline survey. Following the survey (or immediately following consent), participants in the *private* condition were shown the video (either *info only* or *info only + civics*) on a smart phone. Participants in the *public* condition were invited to attend a community-level screening of the videos later in the day (where either *info only* or *info only + civics* were shown).

5 Data and Estimation Strategy

To measure the effect of the treatments on aggregate outcomes at the level of treatment assignment, we collected administrative data on party vote shares at the polling station level and then aggregated to the village level. We were able to match 2015 polling station data to all villages in our experimental sample except for one treated village and two surveyed control villages, which we drop from the analysis.²¹ Including control villages that were not surveyed, among all villages and quarters in our original sample of 30 communes, we were able to match 88% to the 2015 outcome data. On most pre-treatment characteristics unmatched and matched villages are statistically indistinguishable (see Table C.1 in Appendix C). In Appendix C, we also show evidence of balance across high and low dosage communes as well as between treatment and control groups within the low and high dosage communes.

We conducted a panel survey of villagers in all treatment villages and a subset of control villages (see discussion above and Appendix A). The endline survey contained a measure of self-reported voting behavior. We do not rely on the endline survey data here for two reasons: first, we uncovered significant patterns of differential attrition across treatment conditions making certain inferences subject to bias; and second, we expect this self-reported measure of vote choice to be subject to social desirability bias after having provided positive and negative performance information about legislators (evidence of both are illustrated in Appendix D).²² Our behavioral measure helps avoid these problems, and is conservative in the sense that we uncover far fewer significant treatment effects when we analyze the official results (the full survey-data results are presented in Appendix H).

²⁰One community leader refused consent. This village was replaced at random by another village from the same commune and block.

²¹In one village, enumerators administered both private screenings and a public screening, although in both cases they showed the same video (Info-Only). We thus exclude this village from our analyses comparing public and private conditions.

²²Our pre-analysis plan specifies that we will privilege behavioral data over survey data in the case in which our self-reported outcomes suffer from such problems.

5.1 Defining Positive and Negative Information

Our theoretical predictions are conditional on the nature of the information provided. That is, we expect voters to respond differently to information about legislators that is positive and to information that is negative (though we note here and specify below that our analysis is intra-legislator: we compare villages that do and do not receive information about the same legislator). Since the information provided explicitly compares the incumbent legislator's performance to the performance of legislators in the surrounding area (those in the same department), we code positive and negative information relative to this local benchmark. More specifically, we define the information as positive if the incumbent's overall score is better than that of other deputies in the department. Poor legislative performers are those whose overall legislative score is worse than that of other legislators in this local area. This coding rule was pre-specified in our pre-analysis plan prior to project implementation.²³

5.2 Model Specification

As pre-specified, we divide the sample into communes where the incumbent is a strong legislative performer and communes where the incumbent is a poor legislative performer and run analyses separately. To analyze treatment effects of receiving good [bad] news about one's incumbent, we estimate the following model using OLS

$$E(Y_{ij}|Positive_j[Negative_j]) = \beta_0 + \beta_1 T_{ij} + \mu_k \quad (1)$$

where Y_{ij} represents the vote share of the incumbent deputy's party in village i of commune j , and μ_k represents a complete set of block fixed effects.²⁴ Our use of block fixed effects ensures that our experimental estimates are being driven by comparisons of similar villages/quarters within the same commune and *with the same incumbent*.

T is a generic treatment variable indicating the treatment status of village i in commune j . In some models, we pool all treatment conditions and estimate the impact of receiving *any* information. In others, we use indicators for different treatment arms, with Control as the omitted category. We cluster standard errors by commune-treatment condition. We pre-specified that we would run all analyses with and without additional pre-treatment covariates. Because the results are substantively unchanged with covariates and because we lose the majority of our sample when we include covariates from the survey, we present results without controls in the body of the paper and results with controls in Appendix L.3.

²³Ideally, our measure of positive and negative information would capture the difference between the information provided and the prior beliefs of those receiving the information. Since we do not have information on voter priors non-surveyed control villages, we leverage the fact that voters in Benin are generally quite uninformed about the legislative performance of their incumbents. In our baseline survey, 54% of participants report they "do not know" whether their incumbent's legislative performance is better than that of other deputies in the local area. Additionally, interviews with Beninese radio hosts confirm that voters have no access to this type of information, and therefore no way of forming such beliefs. We therefore treat voters as having highly diffuse priors about incumbent legislative performance, allowing us to define positive and negative information based solely upon the information provided in the experiment.

²⁴In low dosage communes, blocks are simply the commune. In high dosage communes, treatment is assigned within three sub-blocks within each commune (rural-competitive, rural-noncompetitive, and urban). There are therefore 45 blocks (15 communes x 3 sub-commune blocks) in high dosage areas and a total of 60 in the full sample.

6 Results

We begin by presenting the mean incumbent vote share (with standard deviations) in each experimental condition in Table 2 for the sample of voters who received positive information about their legislator's performance. Several patterns stand out. First, in low dosage communes, incumbent vote share is substantially *lower*, by about 15 percentage points, in treatment than control villages.²⁵ In high dosage communes, incumbent vote share is also lower in the Info Only (T1) conditions than in Control. These patterns suggest that positive legislative performance information generates a negative effect on incumbent support. Second, incumbent vote share is higher in the Civics (T2) condition relative to Control. Thus, positive information seems to increase incumbent vote share only when information is disseminated widely across the commune (high dosage) and when the information is coupled with the civics message.

Table 3 presents the mean incumbent vote shares in the group of communes that received negative legislative performance information. The descriptive differences across experimental conditions in negative information communes are smaller. While in low dosage communes, incumbent vote share is somewhat lower in treatment than in control, the difference is much smaller than in positive information communes. And in high dosage communes, there are no major differences between treatment and control villages.

Table 4 presents regression results estimating the impact of providing positive performance information on incumbent vote share.²⁶ Column 1 tests the impact of receiving any information treatment in the full sample of communes. The coefficient is very close to zero, suggesting no average impact of the provision of positive information. Column 2 shows that access to positive legislative information in low dosage communes has a negative, and statistically significant, effect on incumbent vote share. The magnitude is about 14 percentage points (95% confidence interval (CI) from -24 to -5). Column 3 shows that positive information has a very small and statistically insignificant effect on incumbent vote share in high dosage communes. In sum, the evidence does not corroborate H1. Rather, positive information *decreases* incumbent vote share in low dosage communes, a result we return to in the next section.

In Column 4, we test the salience hypothesis, H2. In the Info Only condition, positive information has a negative but not statistically significant impact on incumbent vote share. By contrast, positive performance information has a positive and significant effect in the Civics condition. Increasing the salience of legislative performance information increases the vote share of incumbents by 4.3 percentage points in high dosage communes (the 95% confidence interval runs from 0.5 to 8). We can also reject the null hypothesis that the effect of Civics is equivalent to the effect of Info Only ($p = 0.004$). This evidence corroborates H2 for the subsample of high dosage communes. When legislative performance information is made more salient and is distributed to a sufficiently large proportion of the electoral district, its impact increases. Otherwise, the impact of positive information has a null or even a negative effect.

²⁵While the number of villages in this condition is small, the difference holds up to a number of robustness tests. See Appendix L.

²⁶We do not make corrections for multiple comparisons since we pre-specified that we would correct for comparisons of hypotheses within a theoretical family, not across unique theories which our different treatments constitute. The fact that we cannot test for most of our secondary hypotheses using administrative data (they concern survey data) makes it difficult to check for robustness to multiple comparisons with the false discovery rate (FDR) correction we proposed. However, given their associated p values, our two main significant findings would likely be robust to an FDR correction even if most of the other proposed tests in their theoretical family were confirmed with confidence.

Turning to the coordination hypotheses, Column 5 presents tests of H3 on the importance of within-group coordination. There is little difference between the private and public effects, and we cannot reject the null that the effects are the same ($p = 0.18$). The interaction model in Column 6 tests H4 on across-group coordination. The results show that the effects of the Civics treatment are statistically different in low and high dosage communes (as indicated by the coefficient on Civics x High Dosage).²⁷ Thus, the Civics condition improves the vote share of good legislative performers, but only when the information is disseminated widely across the commune. The Civics condition fails to benefit good legislative performers in low dosage communes, however, a result which is consistent with the interpretation forwarded in the theory section that voters are acting strategically.²⁸ In other words, even though treated voters may now privately prefer a legislatively high-performing incumbent in low dosage, they consider the fact that insufficient other voters have similarly updated to risk defecting from the status quo.

As pre-specified, we also conducted the main analyses on the sample of communes in which participants received negative performance information. Overall, we do not find a strong effect of access to negative information about legislative performance (see complete results in Appendix Table G.1). However, when accompanied by a widely-disseminated civics message, access to negative performance information lowers support for the worst performing politicians. This result is shown in Table 5 where we separate the sample into four subgroups corresponding to the information provided to participants in the experiment: information that the incumbent was “much worse,” “worse,” “better,” or “much better” than the local average. Categories were defined using quartiles of the performance score in each department.²⁹ We find the Civics treatment had a negative effect on the vote share of those who performed “much worse” ($p = 0.09$). Thus, voters do punish the worst performing incumbents, but only when the information is disseminated in combination with the Civics treatment and widely across the commune.

In a final set of analyses, presented in Appendix I, we relax the binary definition of good and bad news and allow the type of information to vary over the 10-point index. Two important patterns emerge strengthening our results. First, the impact of treatment in low dosage communes becomes more negative as the incumbent’s performance score increases (the interaction term is not significant). Second, the impact of Civics is increasing and eventually becomes positive as the incumbent’s performance score increases (this interaction term is statistically significant).

²⁷All treated units in low dosage received the civics treatment. Because dosage is a commune-level variable and is therefore perfectly correlated with the blocks, we cannot estimate the independent effect of dosage in the fixed effects framework. For this reason, a dummy variable indicating dosage is not included in the model. We can, however, estimate the interaction between dosage and treatment. The results are qualitatively and statistically the same if we run the model without the block fixed effects and include the dummy variable indicating dosage in the model.

²⁸In Appendix J, we examine whether spillover effects from treatment villages to control are contributing to the differential effect in high dosage communes. We show there are no significant differences in the incumbent vote share across control villages in high dosage relative to low dosage, which we would have expected were there spillovers. Interestingly, there is some evidence that among control communities, respondents in high dosage areas gave more accurate assessments of their politician’s legislative performance than in low dosage areas indicating potential spillovers of information that do not translate into behavioral changes at the polls.

²⁹Quartile ranges vary by department, but at the national level the bottom quartile (“much worse”) includes incumbents with scores of 3.8 and below, the second quartile (“worse”) includes incumbents with scores of 3.8-5.5, the third quartile (“better”) includes those with scores of 5.5-6.3, and the upper quartile (“much better”) includes those with scores above 6.3.

6.1 Robustness

The negative effect of positive information in low dosage communes was not anticipated, and so we have conducted a number of different analyses to assess its robustness. First, to ensure that the result is not being driven by one or a few anomalous but high influence communes, we re-run the low dosage analysis removing each commune from the sample iteratively. In Appendix L, we show that the results are robust to these tests.

Second, we were concerned that, despite the random assignment of dosage, there may have been idiosyncratic differences between good performing incumbents in the high and low dosage communes. We thus investigated each of the incumbent's individually, and found no differences in terms of their political party affiliations or connections to the president's party.³⁰

Third, we considered that the "good news" provided in the low dosage communes may have been less positive than the "good news" provided in high dosage. We find, however, that on average the good performers in low dosage communes are *better* performers than the good performers in high dosage communes (7.26 average score in low dosage versus 6.07 average score in high dosage). Figure L.1 displays the distributions of the performance scores of good performers in high and low dosage communes, confirming that the good performers in low dosage communes actually scored better than the good performers in high dosage communes. Finally, Appendix L also shows that the results are not sensitive to the use of block fixed effects and are robust to the inclusion of weights that account for differences in the sizes of each block.

7 Discussion

This study generates three key sets of results. First and foremost, we showed that access to information leads voters to reward good legislative performance and punish poor legislative performance *only* when that information is accompanied by an intervention designed to increase the salience of the legislative performance dimension *and* an effort to facilitate across-village coordination. Second, we found that, rather than having no effect, access to positive legislative performance information alone actually *lowered* incumbent vote share. Third, we found no difference between the public and private methods of information delivery. In this section, we bring additional qualitative and quantitative evidence to bear on the latter two unanticipated findings.

Our evidence is from the following sources. In January 2015 (two months before experimental treatments were administered), we conducted focus groups with a random sample of 160 voters across eight representative villages.³¹ Then, in March 2015, we conducted interviews with nine journalists from public, private, and community radio stations with differing political orientations (national, opposition, and

³⁰During these elections, there were a number of high level defections from the president's party, the FCBE. One of our concerns was that a large number of the defectors may have been by chance incumbents in the low dosage communes. We did not find this to be the case.

³¹The eight communes sampled were stratified by region (north or south) and rural-urban. Within these communes, we sampled both competitive and non-competitive villages/quarters, based on 2011 legislative results.

centrist) in both Northern (Parakou) and Southern (Cotonou, Ouidah) regions.³² We also conducted two phases of legislator interviews.³³ Last, in March 2015, we conducted a baseline survey of more than 6,000 eligible voters participating in our experiment.³⁴

7.1 The Negative Effect of Positive Information

Two key insights into voter beliefs and preferences about politician performance come from our non-experimental data. First, voters place little to no value on the legislative performance dimension, especially relative to the transfers dimension. And second, some voters and politicians perceive performance along the legislative and transfers dimensions to be substitutes.

In focus group discussions, most participants claimed that they consider, first and foremost, whether or not the candidate will help develop the local community (e.g. build schools, install potable water infrastructure) when making voting decisions. A non-trivial number also said they consider who gives the most presents or money during the campaign. Interviews with politicians and radio hosts further confirmed voter preferences for constituency transfers. One accomplished legislator reported that voters challenge him because he has not built roads or schools directly in their villages. Radio hosts said listeners do not care about legislative performance and, as a result, though journalists have access to legislative sessions, they seldom report on the performance of legislators during those sessions.³⁵ A survey experiment in our baseline survey that described a hypothetical candidate as either providing transfers, performing legislative duties, or neither shows that respondents prefer candidates who provide transfers, but performing legislative duties is not rewarded by respondents.³⁶ In sum, our evidence is consistent with past research on Benin which shows that voters often value transfers over legislative performance (e.g, Wantchekon, 2003).

Our baseline data further suggests that legislative performance may be taken as a negative signal about transfer capabilities. In evaluations of real candidates in the baseline survey (see Table 6), we find that positive legislative performance is negatively associated with evaluations of the incumbent, while support for the incumbent is positively associated with the receipt personal transfers.³⁷ In other words, patterns in the baseline survey data are consistent with voters treating legislative performance and transfer capabilities as substitutes.

Our interviews with legislators also suggest that politicians do indeed have limited time and resources, and that, for at least some, time spent on legislative duties is time not spent on transfers. A handful of legislators lamented this, implying their responsibilities in Porto Novo prevent them from helping their

³²These are Radio Tokpa, Radio Golfe, Radio Kpassse, Radio Planète, Radio Soleil, Radio Le Matinal, Radio ORTB, Radio Arzeke, and University Radio.

³³The two rounds were conducted in October-November 2014 and in March 2015. We attempted to stratify on rural-urban, political competitiveness, and legislator performance in selecting interviewees, though final interviews were subject to legislator availability.

³⁴See Appendix A for our sampling strategy.

³⁵Most radio hosts disseminate information that describes the role of legislators and stresses the importance of legislative performance without providing this performance information directly.

³⁶See Appendix M for further details.

³⁷Table 6 shows the results of regressing intent to support the incumbent (a village average from the baseline survey) on the incumbent's overall performance score, controlling for the variables on which we stratified random assignment of treatment.

community. One went so far as to recognize that by prioritizing legislative duties, he is jeopardizing his own chances of reelection. Note, however, that a handful of legislators diverge from this view: they divide their time between constituency transfers and legislative tasks, and do not perceive any issue with this division of labor. Even so, all legislators characterize this as an allocation decision in the face of fixed time and resources.

7.1.1 A Plausible Explanation: Substitutes vs. Complements

It is thus plausible in the Benin context that the negative effect of access to positive legislative performance information on vote share in the absence of a widely disseminated civics campaign is due to voters hearing the “good news” about strong legislative performance as bad news about their preferred type of politician activity (one who provides transfers). Using our unexpected finding as an opportunity for theory generation, we thus propose a new hypothesis that fits the Benin case but must be subjected to future investigation: When two dimensions of candidate performance are perceived as substitutes rather than complements, increasing access to positive performance information about the less salient one will have a negative effect on electoral support for that incumbent. In effect, this highlights a third assumption in the standard model that does not appear to be met in our context: that information about one dimension of performance has no impact on voter perceptions of performance in other, potentially more valued, dimensions. We derive this hypothesis more formally in Appendix F, but provide some additional intuition here.

While the proposition that performance dimensions may be substitutes is a general one, we find the most realistic application to be the case of our two dimensions: legislation and transfers. A voter in any electoral system in which constituency transfers are salient may perceive information about the legislative dimension to also be a signal of the politician’s performance along the transfers dimension. For instance, a voter may perceive performance on legislative activities and constituent transfers to be positively correlated, or complements, if she believes that good performance on one dimension translates to good performance on all dimensions. In this case, positive information about the legislative dimension should convey a positive signal about transfers, and the expectations of the standard model would hold. Alternatively, a voter may perceive the dimensions to be negatively correlated, or substitutes. As in our case, she may see the politician as having a budget constraint on his time, such that spending effort along one dimension will necessarily detract from the other. Or, she might believe that the types of politicians that perform well on one dimension will lack the ability or disposition to perform well on another.³⁸ In this case, positive information about the legislative dimension could convey a negative signal about the transfers dimension, which could actually lead voters to vote against the incumbent.

7.2 Public versus Private Dissemination

We also found that public dissemination of information *within* villages had little impact relative to private delivery, even though the number of villages treated across constituencies mattered. One explanation

³⁸For example, a “rule-follower” type is more likely to do well on the formal activity of legislating but less likely to do well on informal activities required to conduct transfers.

for this finding is the tight-knit characteristic of villages in Benin: information spread within villages is facilitated by close relationships and frequent conversations among villagers.³⁹ Information gathered during focus groups supports this interpretation.

We additionally exploit the fact that we randomly assigned some individuals to receive a survey and no intervention (control) and some individuals to receive a survey with the intervention (treatment) in Private condition villages. When comparing reported support for and views of the incumbent across these treatment and control individuals *within* Private villages, we observe no detectable treatment effects. These results, which are presented in more detail in Appendix J, provide evidence of strong information transmission within the village – so much so that the responses of people who are not directly given treatment are equivalent to the responses of people directly given treatment within the same village. Barriers to coordination *within* the village appear to be fairly minimal in Benin. By contrast, our results on the impact of dosage show that inter-village coordination poses a significant constraint.

8 Conclusion

Social science research investigating the relationship between access to information about politician performance and electoral outcomes makes straightforward theoretical predictions but yields inconclusive empirical results, particularly among clientelistic democracies. Our paper advances knowledge about information and electoral behavior in these contexts by examining the joint moderating roles of information salience and voter coordination. With a field experiment in Benin, we found that only when information was provided with (1) a civics message highlighting the relevance of legislative performance for voter welfare *and* (2) a coordination message highlighting the fact that other villages in the constituency are receiving the same information, did it translate as expected into electoral behavior. Remove one of these two conditions and the impact of positive performance information on voter support for the incumbent is null ... or even negative.

When our intervention increased the salience of the legislative dimension but did not encourage cross-village coordination (low dosage), positive performance information had a statistically significant *negative* impact on electoral support for the incumbent. We investigate reasons for this unexpected finding using evidence from elite interviews, focus groups, and a baseline survey of more than 6,000 eligible voters. Two relevant findings emerge: Beninese voters value transfers over national legislation and view legislative performance as a substitute for efforts to provide such transfers. We thus propose as a plausible new implication of our argument that substantiates the surprising finding: information about “good” legislative performance will be received as bad news about the incumbent when two dimensions of candidate performance are perceived as substitutes rather than complements and information is provided about the less salient one. Such a proposition underscores a third weakness of the standard model – its assumption that information about one dimension of performance does not affect voter perceptions of other performance dimensions – which must be tested in other contexts before being confirmed.

³⁹The intra-village coordination problem might also be solved by local brokers (chiefs, religious leaders or other local authorities), although Koter (2013b) argues that these local brokers are notably less common in Benin than in other West African countries.

In highlighting the joint importance of salience and strategic voting as moderators of information provision, this study advances a number of important literatures. First, our results have implications for the literature on information and accountability, which has found mixed results on the impact of information access. While salience appears in the literature as a constraint to information's effect on voter behavior, we introduce the idea that strategic considerations are additionally important in a clientelistic context where a sufficient share of the constituency must receive common knowledge about an undervalued dimension in order for information to take effect. Furthermore, our study is the first to manipulate these conditions in a real election to study their impact on voter behavior. Our finding that both conditions must hold for information to have its intended effect can be generalized to specific contexts: clientelistic democracies where legislators engage in both formal, programmatic activities and informal, personalistic ones. Manipulating salience on its own is insufficient where a minority of voters risks losing targeted benefits for defecting from the status quo. Similarly, only manipulating the ability to coordinate is insufficient where there are distinct, potentially competing performance dimensions and voters place little relative weight on the one for which they receive new information.

Second, our results have implications for the conditions under which citizens move from clientelistic to more programmatic voting behavior. Here, the move required not just a civics message aimed at shifting private preferences but also that the civics message be widely disseminated (and be known to be widely disseminated) across an incumbent's voter base. An observational study from Benin argued similarly that access to communal radio messages highlighting the advantages of public goods diminished support for transfers relative to public goods (Keefer and Khemani, 2014). Our study offers a theoretical explanation for, and provides experimental evidence to support this claim. We show that both the civics and the coordination aspects of that kind of intervention are crucial in bringing about changes in citizens' behavior.

The findings also open up fruitful avenues for future research. For instance, we do not make claims here about general equilibrium effects on electoral accountability. We use information about legislative performance in the previous term and do not attempt to assess whether politicians will change their legislative behavior in the future now that this information has been provided (c.f., Humphreys and Weinstein, 2012). The longer term effects of our intervention on accountability could be assessed in future research. Our sample was also limited to those communes where we were able to draw a clear one-to-one mapping between legislator and commune, as in single-member districts. Further, our specific results may travel best to other countries where legislators do not have constituency development funds or are not formally tasked with constituency development, which appears to be the case for most non-Anglophone African countries (Barkan and Mattes, 2014). Finally, our proposed theory that, under certain conditions, positive performance information can have a negative effect on incumbent vote share could be tested in new settings.

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Tables and Figures

Table 1: Experimental Design

	Low Dosage (15 communes)				High Dosage (15 communes)		
	Control	Private	Public		Control	Private	Public
Control	643			Control	486		
Info Only				Info Only		45	45
Civics + Info Only			15	Civics + Info Only		45	45

Cells represent the number of villages or urban quarters in each experimental condition. In private condition villages/quarters, 40 randomly selected individuals were shown the treatment videos. In public condition villages/quarters, 60 randomly selected individuals were invited to the public screening of the treatment videos, and on average about 50 individuals attended the screenings.

Table 2: Mean Incumbent Vote Share by Treatment Arm for Positive Information

	ControlLD	T2T4LD	Control	T1T3	T1T4	T2T3	T2T4
Vote share for the incumbent	0.298 (0.188)	0.149 (0.148)	0.340 (0.232)	0.297 (0.188)	0.294 (0.219)	0.391 (0.239)	0.327 (0.217)
Observations	259	6	259	27	26	27	27

Standard deviations in parentheses. LD in the column title signifies low dosage treatment.

Table 3: Mean Incumbent Vote Share by Treatment Arm for Negative Information

	ControlLD	T2T4LD	Control	T1T3	T1T4	T2T3	T2T4
Vote share for the incumbent	0.332 (0.229)	0.257 (0.261)	0.402 (0.212)	0.413 (0.260)	0.429 (0.154)	0.389 (0.221)	0.425 (0.233)
Observations	381	9	225	18	18	18	18

Standard deviations in parentheses. LD in the column title signifies low dosage treatment.

Table 4: The Impact of Positive Information on Incumbent Vote Share

VARIABLES	(1) All	(2) Low Dose	(3) High Dose	(4) High Dose	(5) High Dose	(6) All
Received any treatment	-0.001 (0.014)	-0.144* (0.043)	0.012 (0.014)			
Performance information only				-0.019 (0.013)		-0.019 (0.013)
Civic + performance information				0.043* (0.019)		-0.144* (0.043)
Public treatment					-0.004 (0.018)	
Private treatment					0.027 (0.019)	
High Dosage x Civics						0.187* (0.046)
Constant	0.319* (0.002)	0.306* (0.001)	0.333* (0.004)	0.333* (0.004)	0.332* (0.004)	0.319* (0.002)
Observations	758	392	366	366	364	758
R-squared	0.391	0.120	0.612	0.617	0.611	0.398

In parentheses, robust standard errors clustered by commune in model 1 and commune x treatment, otherwise. Models include block fixed effects. * p<0.05

Table 5: The Marginal Effects of Information Only and Civics in Each Category of Performance (high dosage communes)

	(1) Much Worse	(2) Worse	(3) Better	(4) Much Better
Civics	-0.031 (0.018)	0.030 (0.030)	0.038 (0.025)	0.054* (0.024)
Info Only	-0.022 (0.033)	0.046 (0.043)	-0.039* (0.015)	0.017 (0.024)
Constant	0.441* (0.003)	0.359* (0.007)	0.347* (0.006)	0.305* (0.005)
Observations	162	135	235	130
R-squared	0.724	0.304	0.630	0.587

In parentheses, robust standard errors clustered by commune x treatment. Models include block fixed effects. * p<0.05.

Table 6: Pre-treatment Correlates of Voting for the Incumbent

	(1)
Overall performance score	−0.026 (0.014)
Time incumbent spends in village	0.017 (0.023)
Received personal assistance from incumbent	0.607* (0.168)
Vote Margin	0.107 (0.068)
Urban	0.030 (0.034)
Constant	0.403* (0.082)
Observations	248

Robust standard errors clustered at the commune level in parentheses. * $p < 0.05$.

Supporting Information:

Salience and Coordination Jointly Moderate the Impact of Information on Vote Choice

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A Additional details about randomization and sampling

Figure A.1 summarizes the structure of our design and Figure A.2 geographically plots the sampled villages and quarters in each of the 30 sample communes.

Figure A.1: Flow Chart Outlining Sampling and Randomization to Experimental Conditions

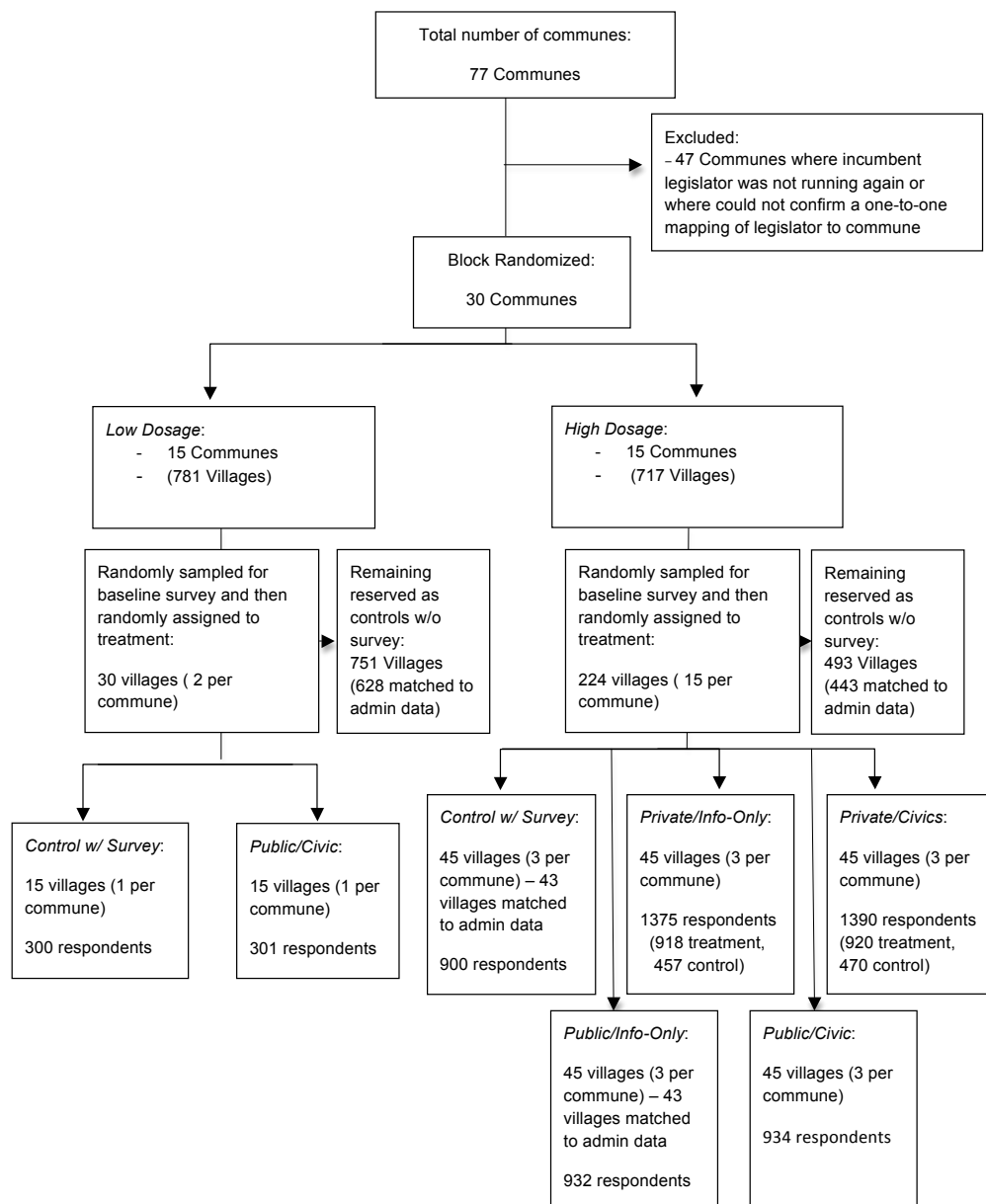
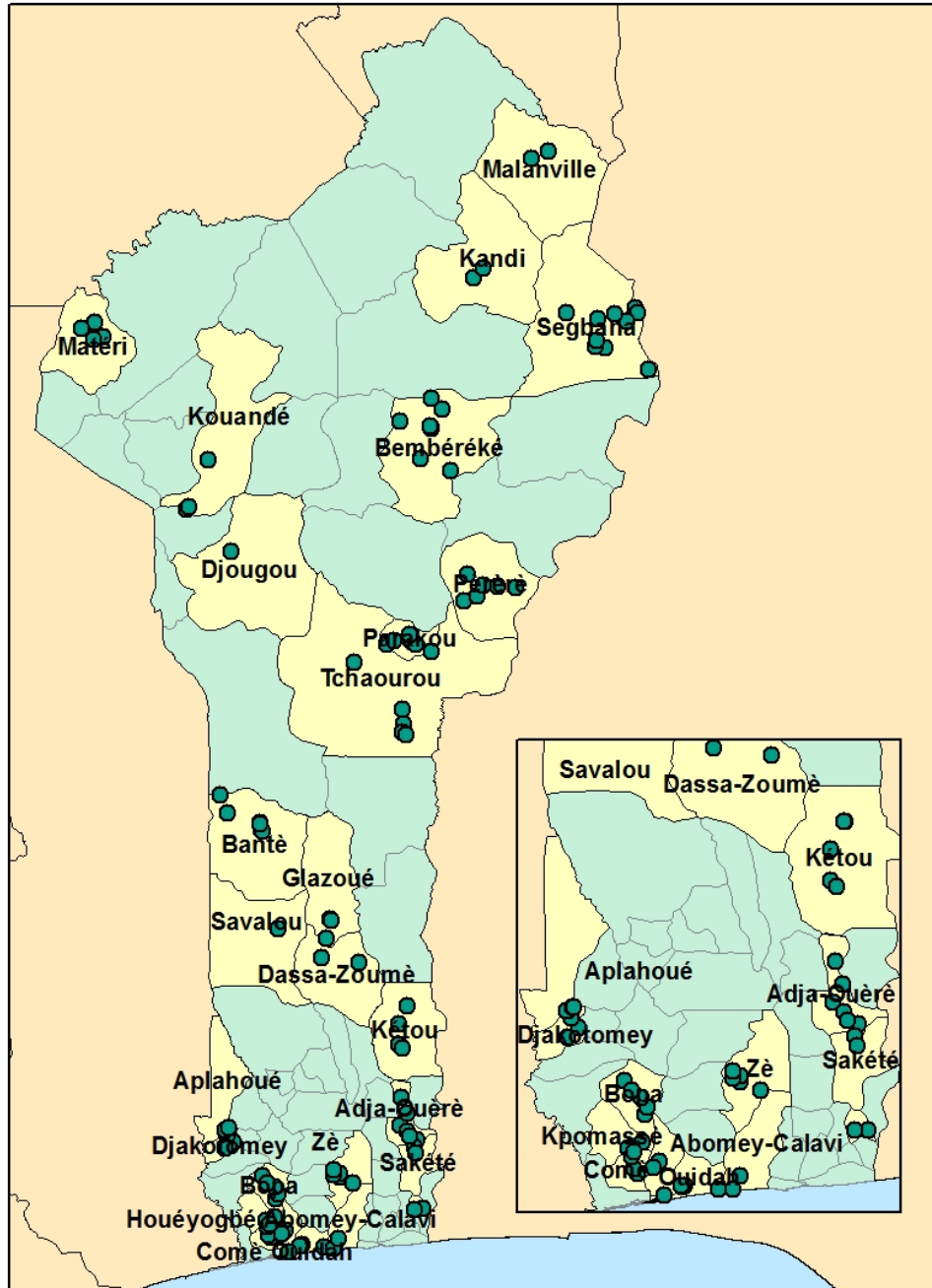


Figure A.2: Sample Communes and Villages/Quarters



Note: The map displays all sample communes (in yellow) and sample villages and urban quarters.

A.1 Blocked randomization

Recall, our within-commune random assignment of villages into treatment group is stratified on electoral competitiveness and urban/rural status. To generate our measure of electoral competitiveness for the purposes of block randomization, we used village-level elections results data from Benin's 2011 legislative elections. We calculated the village-level vote margin in those elections (winner voteshare - second place party voteshare) and defined villages as electorally competitive if the vote margin was below the median and non-competitive if not. The median vote margin in our sample of communes was about 0.21. Our measure of urban/rural comes from Benin's census, which classifies each commune (in which villages are nested) as either urban or rural. We created a dummy variable indicating location in a rural area. About a quarter of localities in our sample were urban. Unexpectedly, competition and rural status were not correlated.

Due to the considerably larger number of rural villages in the sample, we generated three blocks within which to assign treatment: urban, rural/competitive, and rural/non-competitive. Within each high dosage commune, we then randomly assigned one locality from each block to each of our four treatment conditions and our control condition with survey.⁴⁰ The remaining localities served as additional control communities in analyses using administrative data.

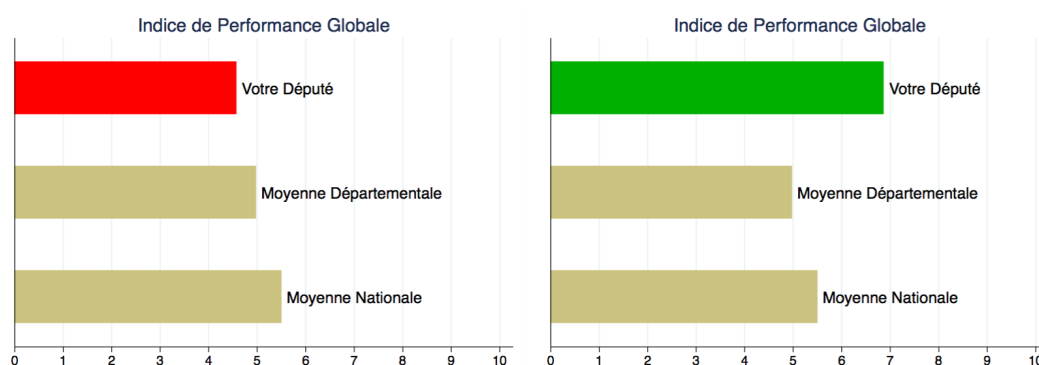
A.2 Sampling for the survey

We collected panel survey data through a baseline in-person survey and an endline phone survey conducted directly after the election. The identities of the respondents were re-confirmed in the endline survey by calling the phone number given in the baseline survey and asking for confirmation of respondents' first names and ages. To discourage attrition, one-third of total compensation per respondent was transferred as phone credit only after completion of the endline survey. In designing the study, we allowed for a possible 50% attrition rate between surveys and achieved a lower attrition rate (44%).

⁴⁰There are only 4 urban quarters in a couple of our sample communes. Because we needed a block size of at least 5 to ensure probability of assignment to treatment of less than 1, we added the largest rural village from the commune to that block in these rare cases.

B Video Treatments

Figure B.1: Example Intervention Bar Graph



Note: The figure presents two examples of bar graphs presented during the intervention. The figure on the left represents an incumbent who performed poorly relative to the local and national mean. The figure on the right represents an incumbent who performed well relative to the local and national mean.

B.1 Information Only Condition Script (in English)

Especially if you live outside the capital, it is not always a simple matter to know how well your national deputy is performing in Porto Novo. This information, however, can be useful during election time when you decide on who you want to represent you in the national assembly.

We have collected data from the national assembly here in Benin about how all the deputies perform their responsibilities. In light of the upcoming elections on March 31, we want to share with you some information about your own deputy, and how he performs relative to other deputies in Benin. There are many ways we could evaluate the performance of a deputy in the national assembly, but we choose to focus on two key aspects that directly correspond to a deputy's formal responsibilities: his performance in plenary sessions and his performance in committee. Working with researchers in the United States, the Centre d'Études et de Promotion de la Démocratie à Cotonou worked for several months to gather information about the legislative performance of Benin's 83 deputies during the previous 4-year mandate. We obtained reports from the President of the National Assembly that detail all the activities undertaken by the assembly and its deputies. We use the information in these reports to evaluate the performance of each deputy so that we can present to you a clear and concise report of how your deputy is doing. As I mentioned, we evaluate deputies on two themes: their performance in plenary sessions and their performance in committees. Plenary sessions are when deputies meet in the national assembly to vote

on laws and oversee activities by the president and his government. The assembly holds an average of X sessions per year. We evaluate a deputy's performance in plenary sessions first by his rate of attendance at these meetings. Rates of attendance vary from X% to Y%. Second, we assess plenary performance by whether the deputy poses questions, oral or written, about the laws being discussed or the president's activities being evaluated as a sign of their active participation. The majority of deputies, X%, never ask any questions. However, some deputies are very involved asking up to 70 questions.

Much of the legislative work of deputies gets done outside of plenary sessions and in committees. These committees are organized by theme such as committees on finance or on education where deputies meet to discuss how to make laws pertaining to that theme. We measure a deputy's performance in committee by whether or not the deputy is a member of the committee, by how productive the committee is or how many laws it considered, by how many meetings the committee held, and by the deputy's rate of attendance at those meetings.

I've just given you a lot of information about how we measure and evaluate the performance of deputies. I know that it is a lot to keep in your head which is why we try to condense the information as much as possible. So, we have created two scores: one for plenary performance and one for committee performance—that summarize how your deputy is doing on each aspect of his job relative to other deputies in Benin. These scores are just a combination of all the information I mentioned. Later, I will present the total score for your deputy on each of the two themes, and the combined score. I will also tell you all the ingredients that went into creating these scores for your deputy. If you want to know more, you can always contact [the cooperating NGO] at the number provided to you.

Benin is comprised of 12 départements. In each département, there are two constituencies (circonscriptions). All the towns and villages in a constituency vote together to elect 2 to 5 deputies to represent them. The number of deputies each constituency gets is based on population size. For example, your constituency is here and has this many deputies. The other constituency in your département has this many deputies. Today, we will provide you with information about the performance of [NAME OF DEPUTY]. Though he is one of [NUMBER] deputies in your constituency, our sources tell us that he is the most important deputy in this commune, [NAME OF COMMUNE].

First, I will tell you about how [NAME] performed in plenary sessions. He attended [RAW NUMBER] sessions of the X total plenary sessions. In other words, he attended [NUMBER OUT OF TEN] plenary sessions. He asked [RAW NUMBER] questions during these sessions. Remember, while most deputies ask

no questions, some ask up to 70. Combining these two measures, we give [NAME] a score of [NUMBER] out of 100 on the plenary performance index. As you can see, your deputy did [BETTER/WORSE] on this measure than other deputies in your département. And he did [BETTER/WORSE] than the national average for all the deputies in Benin.

Second, I will tell you about how [NAME] performed in committee sessions. [NAME] [IS/NOT] a member of a legislative committee. [HE IS A MEMBER OF X COMMITTEE]. This committee is one of the [MORE/LESS] productive committees and treated [RAW NUMBER] of laws during its tenure. This committee held [RAW NUMBER] meetings. Your deputy, [NAME], attended [NUMBER OUT OF TEN] of these meetings.

Combining these measures, we give [NAME] a score of [NUMBER] out of 100 on the committee performance index. As you can see, your deputy did [BETTER/WORSE] on this measure than other deputies in your département. And he did [BETTER/WORSE] than the national average for all the deputies in Benin. If we combine the scores for our two indices together, we see that your deputy, [NAME] performed [BETTER/WORSE] in total than other deputies in your département, and [BETTER/WORSE] than the national average for all the deputies in Benin.

So, to summarize all the information I have just told you: Your deputy, [NAME], is [MUCH/A LITTLE] [BETTER/WORSE] than other deputies in Benin when it comes to performing his legislative responsibilities. This is mostly because he: a. Does(n't) attend plenary sessions, b. Does(n't) participate actively in plenary sessions, c. Is(n't) a member on a committee, d. Does(n't) attend committee meetings.

B.2 Civics Condition Script (in English)

I would like to talk to you about the National Assembly: specifically about the roles and responsibilities of deputies elected to the National Assembly and about how their activities in the National Assembly in Porto Novo can affect you and your family.

There are 83 deputies elected across the country, including the deputies from this constituency. Deputies are charged with three main roles. First, they are responsible for legislation, which means making laws that can have an impact on your daily life. Second, deputies are responsible for oversight—that is, for holding the President accountable, for making sure that he respects the laws and people of Benin. Third, deputies are responsible for representation—that is, for conveying your needs to the government and for

explaining the actions of the government to you.

Let me discuss each of these responsibilities in turn. Some of this you may know already and some may be new information.

As I mentioned, the first main responsibility of deputies is Legislation. Either the President (and his ministers) or individual deputies can have an idea for a new law. They write that idea down as a proposed law, called a "bill." The President or Deputy submits the bill to the head of the National Assembly. After the head of the National Assembly declares it admissible, the bill is sent to a committee made up of deputies who have expertise in the matters raised by the bill. For instance, if the bill concerns education, it will be sent to the Committee on Education, Culture, Employment and Social Affairs for study. That committee then meets in order to study and review the bill carefully and issues a report about the bill that is then circulated and presented to all of the members of the National Assembly. Members of the National Assembly then debate the committee's report and each article of the bill in a full session in the capital in Porto Novo. During this time, individual deputies can make public statements about their positions as to whether the bill is good or bad for their constituents and for Benin as a whole. They can try to persuade other deputies to vote a certain way. After the debate, the deputies then each vote to pass or not to pass the bill. A bill passes if a majority of deputies present vote "yes" to the bill. The National Assembly passes approximately 25 laws each year. It is important to note that only deputies who attend their assigned committee meetings and who attend and participate in the full sessions of parliament can influence which laws pass and which do not.

The second main responsibility of deputies is Oversight. As I mentioned, oversight means holding the President accountable and making sure that he respects the laws and people of Benin. One very important way in which deputies are authorized by law to engage in oversight is by intervening in the process by which the national budget is crafted each year. In fact, by law, the National Assembly is the institution that can oversee the President's budget and make sure that it reflects the needs of the people of Benin. Each year the President proposes a budget—that is, he proposes the total amount of money that will be spent on executing national policies and projects in that coming year, and he proposes how that money will be divided across projects and across different parts of the country. The most important committees in the National Assembly for overseeing this proposal for spending money are the Finance and the Planning committees. Deputies on these two committees are supposed to meet regularly in order to analyze and study the proposed budget. These two committees review more bills than any other committees in the National Assembly. They can make recommendations to the President about ways to amend the

budget before it is presented to the National Assembly. All deputies can also vote to approve the President's budget once it is presented to the full Assembly. When the budget is implemented, the National Assembly can make recommendations to the President if they observe that the budget is not being spent properly.

Another important way in which deputies can engage in oversight is by making sure that any proposed legislation or ordinance put forward by the President is in compliance with the Constitution and with all electoral laws. Deputies on the Legal Committee of the National Assembly are charged with studying any bills that would change rules about elections or the powers of the President and with making reports on their legality to the full National Assembly. This committee reviews the third largest set of bills each year, after the sets reviewed by the Finance and Planning committees. If any change is proposed to the Constitution of Benin, at least three-fourths of the members of the National Assembly have to vote to approve the change before it can move forward.

The third main responsibility of deputies is Representation. As you know, deputies are elected to serve particular constituencies, including the constituency in which this village is located. As citizens, you are very busy with meeting your daily needs and those of your family. You cannot travel to the capital to tell the President what your needs are. Instead, that is part of the deputies' job. They are charged with communicating your needs and the needs of other voters in this constituency to the National Government. The deputies can do this by raising questions and concerns about national legislation in their assigned committee meetings and when bills are debated in full sessions of the National Assembly. During those times, deputies can make clear to other politicians whether or not the law is in your best interests—that is, whether or not it is in the interests of voters in the deputies' home constituencies. Deputies can also come up with new ideas for legislation, based on their understanding of your needs. If deemed admissible for review by the head of the National Assembly, these new ideas—written down as bills—will then be reviewed by committees and debated by all deputies who attend the full meetings of the National Assembly. Again, it is important to note that only deputies who attend committee meetings and full National Assembly meetings, and who participate by asking questions and voicing your concerns, can fulfill their responsibilities of legislating, engaging in oversight and representing your needs in the capital.

Now, you may still be thinking that none of these activities has much to do with your welfare. But let me give you some examples of ways in which what deputies do in Porto Novo does matter for the quality of your life and that of your family.

One example is the anti-graft law that the National Assembly passed in August 2011. This is a law that requires Benin's top leaders, civil servants, central directors of the administration, project managers and accountants of any public body to disclose their assets when they enter and leave office. The law is intended to help prevent corruption so that the money in the national budget is spent on you, the citizen, and not on lining the pockets of powerful people. The deputies in the National Assembly are the ones who had to review and approve this law. Their work in the National Assembly in Porto Novo is thus relevant to ensuring that resources get to you.

Here is another example. Le Régime d'assurance maladie universelle (RAMU) is a proposed national program that would help the people of Benin access healthcare. It would help you if you have trouble paying for medical treatment. The consequences of getting sick can be financially disastrous for you and your family if you do not have the money to pay for healthcare. If it becomes law, RAMU would help you. It would help the poor; it would help farmers; it would help students; it would help taxi and moto drivers; it would help people who are informally employed. It would cover visits to the doctor, costs of staying in or being treated in the hospital, costs of medication, transportation to the hospital or doctor and tests to know if you are sick.

The idea for the program was conceived by the Council of Ministers in 2008, and since 2011, there have been small versions of the program operating in some villages, known as "zones sanitaires." In 2014, the President established a National Steering Committee. But in order to become a program that operates across the whole country, RAMU has to be approved by a vote in Parliament. In other words, the national deputies have to do the work of evaluating and voting on the proposed law before it can become an implemented national program that can help you pay if you get sick. The President has said that RAMU is a national priority. But the performance of the national deputies is crucial if the proposal is actually to become law. Whether your national deputy shows up and participates in Parliament has an impact on whether RAMU becomes law and thus on whether you and your family get help if you are sick.

Third, let me give you an example of Parliament's important role in presidential oversight. In 2009, President Boni Yayi sent a proposal (known as a "projet de loi") to the National Assembly that sought to revise Benin's constitution, which has not been changed since it was enacted in 1990. The proposal was again sent to the National Assembly in 2013. The proposal went to the Law Committee of the National Assembly but did not make it to the Assembly for a vote. Members of the public began opposing the proposal fearing it was a way for President Yayi to extend his presidential mandate. Benin citizens came together to voice their opinion and created movements against the changes such as the "Red Wednesday" move-

ment ("Mercredi rouge"). Leaders of several political parties came forward to oppose the proposals, stating that the public was not ready for a change to the constitution, and citing more important issues for the president to concentrate on. Even pro-presidential members of the Law Committee were against the changes, and large majority of the committee's members voted to reject the proposal on September 24th, 2013. In this sense, legislative representatives not only exercised their right as a check to the executive but also represented the public interest which voiced its disapproval of any constitutional changes. These are just a few examples of how the performance of your national deputy—his participation in committees and in plenary sessions of the National Assembly, his willingness to ask questions and voice positions on legislation and to exercise presidential oversight—are important for your daily lives.

The full text of the Information Only Condition Script is then inserted here.

B.3 Dosage Conditions

Before the treatment videos were shown, participants in high and low dosage communes were told the following:

1. High Dosage: You have been selected through a random process to participate in a research study about the performance of your deputies in the National Assembly. Your community is one of 12 villages or quarters in your commune receiving this exact same video. Many other communes in Benin are also part of the study.
2. Low Dosage: You have been selected through a random process to participate in a research study about the performance of your deputies in the National Assembly. Your community is the only one in the commune receiving this information.

After the video treatment videos were shown, participants were told the following:

1. High Dosage: Remember, your village/quartier is one of 12 villages or quartiers in your commune receiving this video.
2. Low Dosage: Remember, your village/quartier is the only one in the commune receiving this vido.

C Balance Tests

Table C.1: Balance Across Villages Matched and Unmatched to Administrative Data

	Mean Unmatched	Mean Matched	Difference	P-Value
Registered Voters (log)	6.34	6.53	.19	.09
Urban	.28	.23	.06	.14
Turnout	68.39	67.54	.86	.66
Competitive (dichotomous)	.52	.45	.07	.18
Incumbent Performance	4.98	5.19	.22	.69
North	.39	.46	.07	.65

P-values generated from tests in which we cluster on commune.

Table C.2: Balance Between High and Low Dosage Communes

	Mean High Dosage	Mean Low Dosage	Difference	P-Value
Registered Voters (log)	6.3	6.52	.22	.18
Urban	.21	.25	.04	.41
Competitive (dichotomous)	.42	.5	.07	.25
Vote Margin	.28	.24	.05	.19
Overall Performance	4.97	5.35	.38	.67

P-values generated from tests in which we cluster on commune.

Table C.3: Balance in High Dosage Communes

	Control	Info Only/Private	Info Only/Public	Civics/Private	Civics/Public
Registered Voters (log)	687.5	828.26 (.08)	1066.25 (.06)	1110.73 (.00)	807.27 (.02)
Urban	.18	.03 (.00)	.34 (.00)	.27 (.07)	.29 (.01)
Competitive (dichotomous)	.41	.49 (.11)	.48 (.30)	.5 (.14)	.48 (.25)
Vote Margin	.29	.25 (.14)	.23 (.05)	.26 (.44)	.31 (.47)
Overall Performance	4.89	5.26 (.14)	5.19 (.21)	5.23 (.17)	5.23 (.17)

P-values in parantheses indicate significance of difference between the mean and each treatment group and the control group mean.

P-values generated from tests in which we cluster on commune.

Note: Because of our blocking and randomization process, there is a lack of balance in the raw means on urban and number of registered voters. This occurred because our rural blocks, where there are also fewer registered voters, contain larger numbers of units than our urban blocks. Since all non-treated units are used as controls, on average the proportion of rural areas in control is lower than in treatment. This lack of balance is not a problem as we use block fixed effects in all of our analyses, which controls for the urban/rural difference.

Table C.4: Balance in Low Dosage Communes

	Mean Treatment	Mean Control	Difference	P-Value
Registered Voters (log)	933.27	1024.24	90.97	.76
Urban	.27	.25	.01	.92
Competitive (dichotomous)	.53	.5	.04	.78
Vote Margin	.25	.24	.01	.86
Overall Performance	5.42	5.35	.07	.9

D Attrition and Response Bias in the Survey Data

D.1 Differential Attrition

As indicated in our pre-analysis plan, we test for differential rates of attrition across treatment conditions and with respect to pre-treatment covariates. In Table D.1, we find that participants in the public treatment condition were substantively and significantly less likely to attrit than those in control while participants in the private treatment condition were potentially more likely to attrit, although this latter relationship is not significant. Comparing this to treatment effects on self-reported voting behavior, we find positive and significant effects of treatment in the public condition. If the sample of participants from the public treatment group are qualitatively different than those in the control group, we cannot rule out that this treatment effect is due to selection of different types of individuals rather than actual change in opinions.

Table D.1: Differential Rates of Attrition by Treatment Condition

VARIABLES	(1) Endline	(2) Vote choice	(3) Endline	(4) Vote choice
Public treatment	-0.050* (0.030)	-0.048* (0.027)		
Private treatment	0.023 (0.030)	0.021 (0.027)		
Civic + performance information			-0.011 (0.028)	-0.017 (0.026)
Performance information only			-0.018 (0.034)	-0.011 (0.030)
Constant	0.469*** (0.024)	0.530*** (0.022)	0.473*** (0.024)	0.533*** (0.022)
Observations	6,352	6,352	6,352	6,352
R-squared	0.113	0.092	0.109	0.089

In parentheses, robust standard errors clustered by commune x treatment condition.

Models include block fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

In Table D.2, as pre-specified, we show whether certain pre-treatment covariates are predictors of attrition, and whether these covariates predict attrition differentially across treatment groups. In The first two columns, we see that women, less educated participants, rural participants and those in less politically competitive constituencies were significantly more likely to attrit. When interacted with treatment, female and coethnicity with the incumbent produce statistically significant coefficients. In columns 3 through 6, we see that women were even more likely to attrit in treatment relative to control and coethnics were less likely to attrit in treatment than control.

Table D.2: Differential Rates of Attrition by Pre-treatment Covariates and Treatment

VARIABLES	(1) Endline	(2) Vote choice	(3) Endline	(4) Vote choice	(5) Endline	(6) Vote choice
Received any treatment	-0.046 (0.036)	-0.034 (0.034)	-0.077** (0.030)	-0.062** (0.030)	0.037 (0.038)	0.035 (0.037)
Female	0.047*** (0.018)	0.061*** (0.017)	-0.008 (0.028)	0.026 (0.030)		
Coethnic with incumbent	-0.012 (0.028)	0.005 (0.026)			0.074** (0.036)	0.077** (0.035)
Positive prior	0.028 (0.023)	-0.001 (0.021)				
Years of education	-0.007*** (0.002)	-0.005** (0.002)				
Urban	-0.265*** (0.067)	-0.233*** (0.067)				
Vote margin (top 2 parties) in previous election	0.188** (0.082)	0.188** (0.075)				
Treatment x Female			0.094*** (0.031)	0.067** (0.032)		
Treatment x Coethnicity					-0.110** (0.043)	-0.103** (0.042)
Constant	0.476*** (0.051)	0.514*** (0.049)	0.471*** (0.026)	0.515*** (0.026)	0.420*** (0.028)	0.479*** (0.028)
Observations	2,713	2,713	6,128	6,128	6,072	6,072
R-squared	0.137	0.113	0.121	0.101	0.115	0.094

In parentheses, robust standard errors clustered by commune x treatment condition.

Models include block fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

D.2 Response Bias

One way to measure the treatment effect on response bias is to compare data points that appear in both the survey and behavioral data. We have measures of voter turnout from both the endline survey and from the administrative data. One would expect social desirability bias to motivate respondents to over-report voter turnout. This indeed seems to be the case as about 90% of participants self-report voting in the legislative elections while actual voter turnout in our sample of villages is only about 70%. Some of this is likely due to problems of ecological inference to the extent that we do not know if we have a representative sample of registered voters. While voting age was a requirement to enter into our sample, voter registration was not. This potential ecological inference problem should not, however, be different across treatment and control groups. Thus, we can get a clean estimate of whether the difference between self-reported and behavioral measures are significantly different across treatment and control groups.

To do this, we create a village-level variable for our 255 sample villages that takes the difference between the mean self-reported turnout rate from the survey and the official turnout rate. About 10% of villages in our sample under-report turnout relative to the official results while the vast majority over-report. To test whether over-reporting is different by treatment condition, we simply regress the constructed measure of deviation on treatment. Table D.3 shows that treatment has a positive but statistically insignificant effect on over-reporting. This is driven entirely by places that received bad news about the incumbent. There, we see a substantively large (over 10 percentage points) and statistically significant effect of treatment on over-reporting.

Table D.3: Treatment Effects on Over-reporting of Voter Turnout

VARIABLES	(1)	(2)
Treatment	0.047 (0.029)	0.114** (0.045)
Good News		0.087* (0.052)
Treatment x Good News		-0.115* (0.059)
Constant	0.157*** (0.025)	0.105*** (0.039)
Observations	231	229
R-squared	0.011	0.028

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table D.4: Treatment Effects on Reporting of Incumbent Voteshare

VARIABLES	(1)	(2)
Treatment	-0.027 (0.041)	-0.086 (0.062)
Good News		-0.008 (0.071)
Treatment x Good News		0.120 (0.082)
Constant	0.177*** (0.036)	0.170*** (0.054)
Observations	231	229
R-squared	0.002	0.034

Standard errors in parentheses

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

In Table D.4, we present results from a similar analysis that examines incumbent voteshare. Here, we calculate the deviation as the difference between the reported incumbent voteshare as reported in the survey and the official voteshare. Positive values thus indicate over-reporting of votes for the incumbent, while negative values indicate under-reporting. In the absence of response bias, we would expect these deviations to be uncorrelated with treatment status and with the content of the information provided.

The results in column 2 provide some evidence of response bias in the survey. In treated areas that received bad news about the incumbent, the reported voteshare is 8 percentage points lower than the official voteshare. This coefficient is not statistically significant but is not far from conventional cutoffs ($p = .16$). This pattern is consistent with under-reporting of votes for the incumbent in treated areas where bad performance information was provided. In good news areas, the marginal effect of treatment on the deviation is about 3.5 percentage points (calculated by adding the treatment coefficient to the interaction term between treatment and good news). This is also consistent with over-reporting of votes for the incumbent in good news areas, although the marginal effect is not statistically significant.

Finally, we can also examine evidence of response bias by comparing the official number of votes received by the incumbent in a village to the number of survey respondents who report to us that they voted for the incumbent. In a small number of villages in our sample, the number of votes officially received by the incumbent was small enough that it was possible for the survey to over-estimate the number of votes received by the incumbent. Table D.5 shows that there are 7 villages in our sample where the reported number of votes in the survey was *greater* than the official number of votes received by the incumbent. Of these, 6 are treated villages and 4 are in good news communes. This is very clear evidence

Table D.5: Over-Reporting of Votes for the Incumbent on the Survey

<i>Village</i>	<i>Treatment</i>	<i>Good News</i>	<i>Reported Votes (survey)</i>	<i>Official Votes</i>
GBESSOU	No	No	6	0
KPAFE	Yes	No	6	0
KPAVIEDJA	Yes	No	6	0
NIAROGNINON	Yes	Yes	5	1
ALLANWADAN	Yes	Yes	7	1
ZOUNTA	Yes	Yes	3	1
OROUKAYO	Yes	Yes	14	10

of over-reporting of votes for the incumbent in the survey. While we cannot make these types of direct comparisons for most villages in the sample – in most cases the incumbent receives more votes than the number of voters surveyed – these patterns should raise concerns about survey response bias in all villages.

E Validating the Performance Index

To validate our performance measure, we examine whether our index correlates well with an alternative – and independently created – proxy for legislative performance: the legislator’s professional background prior to holding office. Exploring the rising cost of campaigns and the role of money in politics in Benin, Koter (2016) shows that wealthy individuals (business people and customs officials) have more than quadrupled their presence in parliament while the presence of the less wealthy, intellectual class (teachers, lawyers, academics) who comprised the vast majority of parliamentary seats early in Benin’s democracy has been steadily declining. While the latter politicians are considered better qualified to fulfill the formal duties of their position, the former are more valuable to parties because of their ability to buy votes. Combining our performance index with occupational data collected by Koter (2016), we see that wealthier politicians perform about 50 percent less well than other parliamentarians on components of the index such as attendance at plenary sessions and committee meetings. This increases our confidence that the performance index is measuring true legislative capacity.

More anecdotally, our elite interviews during an extraordinary session of parliament also revealed types consistent with our index. An example of a “good” performer we interviewed was a retired agronomist, who complained that he entered politics to address the concerns of his impoverished rural neighbors through legislation but was disappointed to learn that most politicians enter parliament to advance personal aims rather than the interests of the nation. Meanwhile, “bad” performers were difficult to interview because they were not even in the capital during the extraordinary session of parliament.⁴¹ In short, “good” performers according to our index were indeed politicians interested in lawmaking and who were active during an extraordinary session of parliament. By contrast, “bad” performers according to our index were notably absent from the capital, and in some cases, from the country.

⁴¹ Of the parliamentarians interviewed, only one was a bad performer, and we had to travel to his home constituency as he does not typically attend parliamentary sessions. Other reasons we were unable to interview bad politicians included the legislator’s simple refusal to participate, business travel to Niamey or Brussels, our inability to locate the legislator, or the legislator’s lack of fluency in French.

F A Theory of Performance Dimensions as Substitutes

Given our two dimensions, which can take various forms depending on the country context, a voter's calculus depends on her evaluation of the politician along each dimension and the weight she assigns each dimension. We use a simple decision-theoretic framework to increase precision and generate predictions. The voter's decision calculus is given by the following equation where Y is the total value the voter places on the candidate, p is the weight she places on the legislative dimension, and LE' and TR' are her prior evaluations of the legislative and transfers dimensions, respectively.

$$Y = p(LE') + (1 - p)(TR') \quad (2)$$

Let's say we now introduce LE , an information signal about the legislative dimension. The effect of LE on Y will depend on the relative weighting as specified in Equation 2, but also on the voter's perceived interaction between the two dimensions. We define $F(LE)$ as a function that translates a signal about the legislative dimension into a signal about the transfers dimension. If the two dimensions are complements, then $F(LE)$ is positive; if they are substitutes, $F(LE)$ is negative; and if they are orthogonal, $F(LE)$ is 0. Thus, for a voter who fully updates her valuation of the legislative dimension given the signal, LE will induce the voter to newly value the candidate as follows:

$$Y^{LE} = p(LE) + (1 - p)(TR' + F(LE)) \quad (3)$$

To get the effect of the signal LE on the voter's valuation of the candidate, we simply subtract the new value from the old:

$$\Delta Y = Y^{LE} - Y = p(LE - LE') + (1 - p)(F(LE)) \quad (4)$$

From Equation 4, we can clearly see that as long as $p \neq 0$ (the voter places nonzero weight on the legislative dimension) and $F(LE) \geq 0$ (the dimensions are not substitutes), then Hypothesis 1 holds unambiguously. If, however, the two dimensions are substitutes, then whether Hypothesis 1 holds depends on p . Conditional on the function $F(LE)$, there will be some level $p > p^*$ for which Hypothesis 1 will hold. For

values $p < p^*$, we should instead observe the opposite as defined by the following alternative hypothesis:

Hypothesis 1a *When $F(LE) < 0$, or the two dimensions of candidate performance are substitutes, then for values of $p < p^*$, increasing access to positive legislative performance information about an incumbent will have a negative effect on electoral support for that incumbent.*

Note that this prediction does not necessarily hold symmetrically for negative performance information. If the dimensions are substitutes because, say, politicians face a budget constraint on their time, then learning that politicians are not spending time in the legislature does not necessarily imply they *are* spending time on transfers, even though the reverse is true. Indeed, one could easily imagine a low-quality politician spending time neither on legislating nor on transfers. In other words, if the two dimensions of candidate performance are substitutes, positive legislative performance information might be expected to have a negative effect on electoral support for the incumbent but negative legislative performance information might be expected to have no effect.⁴²

Combining this insight with the main hypotheses and from the paper, we can derive a joint proposition summarizing the conditions under which information should have a positive effect on incumbent vote share. For ease of exposition, consider a simplification of Equation 2 where rather than being a continuous variable, $p \in 0, 1$. Thus, $E[p]$ represents the share of voters in a constituency who condition their vote on the basis of legislative performance. We also define s^* as the share of votes a candidate needs in the constituency to obtain a seat. For consistency with our empirical set-up, we limit our below analysis to the case in which $E[p]$ is sufficiently small that *ex ante* coordination on a good legislative candidate is unlikely.

Proposition 1 *If prior expectations are that $E[p]$ is sufficiently small and transfer and legislative dimensions are substitutes, then positive legislative information about the incumbent candidate will make voters more likely to vote for the incumbent if and only if the voter believes sufficient voters in the constituency also update their priors about the value of p such that $E[p] > s^*$.*

⁴²Additionally, there are two cases in which we should see no effect of legislative information on incumbent support: 1) when $p = p^*$ and the increase along the legislative dimension is exactly canceled out by the decrease along the transfers dimension, and 2) when no weight is put on the legislative dimension ($p = 0$) and the two dimensions of candidate performance are orthogonal ($F(TR) = 0$).

G The Impact of Negative Information

Table G.1 presents results on the impact of negative information. As we discuss in the main body of the paper, we find little overall evidence that access to negative information about legislative performance impacts voter behavior. Instead, we find that access to negative information lowers vote share for the worst performers when that information is accompanied by a widely-disseminated civics message.

Table G.1: The Impact of Negative Information on Incumbent Vote Share

VARIABLES	(1) Low Dose	(2) High Dose No Civics	(3) High Dose No Civics	(4) All High Dose	(5) All Communes
Treatment	-0.064 (0.049)	0.014 (0.029)			
Private treatment			0.006 (0.040)		
Public treatment			0.022 (0.039)		
Performance information only				0.010 (0.028)	0.010 (0.027)
Civic + performance information				-0.003 (0.018)	-0.064 (0.048)
High Dosage x Civics					0.061 (0.051)
Constant	0.297*** (0.001)	0.403*** (0.003)	0.403*** (0.003)	0.404*** (0.005)	0.327*** (0.001)
Observations	746	261	261	297	1,043
R-squared	0.588	0.597	0.597	0.599	0.609

In parentheses, robust standard errors clustered by commune in model 1 and commune x treatment, otherwise. Models include block fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

H Replication of Main Results Using the Survey Data

Table H.1: The Impact of Positive Information on Incumbent Vote Share (Survey Data)

VARIABLES	(1) Low Dose	(2) High Dose No Civics	(3) High Dose No Civics	(4) All High Dose	(5) All High Dose	(6) All Communes
Received any treatment	-0.021 (0.071)	0.084** (0.039)				
Private treatment			0.022 (0.040)	0.045 (0.038)		
Public treatment			0.145*** (0.047)	0.129*** (0.045)		
Performance information only					0.090** (0.043)	0.090** (0.043)
Civic + performance information					0.074* (0.041)	-0.021 (0.068)
High Dosage x Civics						0.094 (0.080)
Constant	0.474*** (0.046)	0.437*** (0.031)	0.437*** (0.031)	0.437*** (0.034)	0.438*** (0.035)	0.441*** (0.032)
Observations	149	875	875	1,519	1,519	1,668
R-squared	0.428	0.181	0.191	0.163	0.157	0.182

In parentheses, robust standard errors clustered by village.
Models include block fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table H.2: The Impact of Positive Information on Incumbent Vote Share with Controls for Predictors of Attrition (Survey Data)

VARIABLES	(1) Low Dose	(2) High Dose No Civics	(3) High Dose No Civics	(4) All High Dose	(5) All High Dose	(6) All Communes
Received any treatment	-0.019 (0.073)	0.085** (0.039)				
Private treatment			0.016 (0.040)	0.046 (0.038)		
Public treatment			0.154*** (0.048)	0.132*** (0.045)		
Female	0.093** (0.043)	-0.013 (0.031)	-0.017 (0.031)	-0.006 (0.023)	-0.005 (0.024)	0.004 (0.022)
Coethnic of incumbent	0.038 (0.058)	-0.035 (0.045)	-0.063 (0.044)	-0.049 (0.035)	-0.040 (0.035)	-0.034 (0.033)
Performance information only					0.091** (0.042)	0.092** (0.042)
Civic + performance information					0.076* (0.041)	-0.011 (0.069)
High Dosage x Civics						0.088 (0.080)
Constant	0.398*** (0.060)	0.462*** (0.041)	0.480*** (0.041)	0.468*** (0.040)	0.463*** (0.041)	0.458*** (0.038)
Observations	148	872	872	1,505	1,505	1,653
R-squared	0.438	0.181	0.193	0.165	0.159	0.184

In parentheses, robust standard errors clustered by village.
Models include block fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table H.3: The Impact of Negative Information on Incumbent Vote Share, Survey Data

VARIABLES	(1) Low Dose	(2) High Dose No Civics	(3) High Dose No Civics	(4) All High Dose	(5) All High Dose	(6) All Communes
Received any treatment	-0.328*** (0.065)	0.003 (0.065)				
Private treatment			0.002 (0.071)	-0.035 (0.067)		
Public treatment			0.005 (0.071)	-0.043 (0.066)		
Performance information only					-0.010 (0.067)	-0.010 (0.067)
Civic + performance information					-0.065 (0.065)	-0.328*** (0.062)
High Dosage x Civics						0.263*** (0.090)
Constant	0.688*** (0.045)	0.529*** (0.059)	0.529*** (0.059)	0.534*** (0.061)	0.534*** (0.061)	0.553*** (0.054)
Observations	168	687	687	1,200	1,200	1,368
R-squared	0.216	0.239	0.239	0.254	0.257	0.252

In parentheses, robust standard errors clustered by village.
Models include block fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

I Analysis of Continuous Measure of Performance

In the main paper, we separate incumbents into two groups: those with good and those with bad performance. This is consistent with the content of the treatments and our pre-analysis plan. In this section, we also analyze the continuous measure of performance, which participants were exposed to both numerically and in the form of bar graphs. In these analyses, we use the full sample of communes. We use the overall performance index score, which runs from 0-10, and interact that score with our treatment indicators. The coefficients on the treatment indicators can be interpreted as the impact of each treatment in areas with the worst performers. The interaction terms providing information about how the effect of treatment changes as the performance score increases.

Table I.1 presents the results. Two important patterns emerge. First, column 1 shows that the impact of treatment is negative in low dosage communes where the incumbent has the worst performance scores (the coefficient is not significant). The interaction term shows that this effect becomes even more negative as the incumbent's performance score increases (the interaction term is not significant). Qualitatively, the story is similar to the one that emerges from the main analyses. Second, column 4 shows that the impact of civics is negative in the communes where the incumbent has performed the worst (the coefficient is not significant). However, the effect of civics is increasing and eventually becomes positive as the incumbent's performance score increases (the interaction term is statistically significant). By contrast, the interaction between information only and the index is essentially zero.

Table I.1: The Impact of Information on Incumbent Vote Share

VARIABLES	(1) Low Dose	(2) High Dose No Civics	(3) High Dose No Civics	(4) All High Dose	(5) All Communes
Received any treatment	-0.030 (0.112)	-0.012 (0.043)			
Private treatment			0.020 (0.048)		
Private x Overall Index (continuous)			-0.005 (0.007)		
Public treatment			-0.043 (0.051)		
Public x Overall Index (continuous)			0.007 (0.008)		
Treatment x Overall Index (continuous)	-0.011 (0.019)	0.001 (0.007)			
Performance information only				-0.013 (0.042)	-0.007 (0.014)
Info Only x Overall Index (continuous)				0.001 (0.007)	
Civic + performance information				-0.031 (0.025)	-0.030 (0.112)
Civics x Overall Index (continuous)				0.011** (0.005)	-0.011 (0.019)
High Dosage x Civics					0.000 (0.115)
Civics x Dosage x Overall Index					0.021 (0.019)
Constant	0.318*** (0.001)	0.366*** (0.002)	0.366*** (0.002)	0.364*** (0.003)	0.341*** (0.002)
Observations	655	572	572	662	1,317
R-squared	0.464	0.633	0.634	0.618	0.550

In parentheses, robust standard errors clustered by commune in model 1 and commune x treatment, otherwise. Models include block fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

J Spillovers

Table J.1 tests for spillovers by examining what happens in control villages. Different incumbent vote share in high dosage than low dosage would be evidence of spillovers, but Table J.1 shows the vote share for the incumbent is no different in high- and low-dosage control villages. The last two columns further confirm there is no independent effect of dosage other than through its effects on treated villages. Because block fixed effects would prevent comparisons across dosage levels (blocks are nested within dosage levels), we instead run a multi-level model using random effects for blocks. These regressions include both treated and control villages.

Table J.1: Comparing Incumbent Vote Share in Control Villages by Dosage

VARIABLES	(1) Whole Sample	(2) Positive Info	(3) Negative Info	(4) Positive Info	(5) Negative Info
Dosage	0.069 (0.064)	0.034 (0.071)	0.106 (0.095)	0.022 (0.070)	0.089 (0.089)
Treatment				-0.145*** (0.043)	-0.064 (0.050)
Dosage x Treatment				0.154*** (0.046)	0.068 (0.054)
Constant	0.300*** (0.043)	0.306*** (0.025)	0.296*** (0.064)	0.293*** (0.031)	0.321*** (0.062)
Observations	1,607	645	962	757	1,043
R-squared	0.020	0.006	0.037		

In parentheses, robust standard errors clustered by commune.
Models include block fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Next, we examine whether voters know more in control villages in high dosage than in low dosage communes. To do so, we construct a variable of the extent to which the survey respondent's reported posterior assessment of the incumbent's legislative performance reflects the true value of the incumbent performance on the same 4-point scale. We take the absolute value of the difference in scores such that a lower value reflects higher knowledge. Table J.2 demonstrates that, in places receiving good news, respondents in high-dosage control villages are better informed than in low-dosage control villages. This provides some evidence of information spillovers that do not, however, seem to translate into spillovers in behavior as in the previous table.

Related to the unexpected finding that the public dissemination of the information had no moderating impact on voter behavior, relative to private dissemination, we found evidence of intra-village spillovers within private dissemination villages. Tables J.3 and J.4 show results from regressing incumbent support (at the individual level) on treatment assignment, comparing individuals within private villages who were

Table J.2: Accuracy of Voter Posteriors in Control Villages by Dosage

VARIABLES	(1) Whole Sample	(2) Positive Info	(3) Negative Info
Dosage	-0.278 (0.174)	-0.571** (0.270)	0.002 (0.175)
Constant	1.087*** (0.157)	1.317*** (0.246)	0.879*** (0.143)
Observations	628	326	302
R-squared	0.027	0.102	0.000

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

In parentheses, robust standard errors clustered by village.

*** p<0.01, ** p<0.05, * p<0.1.

assigned to treatment with individuals within those villages assigned to control.

Table J.3: Treatment Effects Within Private Dissemination Villages, Good News

	InfoOnlyVillages	CivicsVillages	AllPrivateVillages
Info-Only	-0.148 (0.228)		-0.186 (0.237)
Info-Only + Civics		-0.044 (0.206)	-0.017 (0.189)
Constant	-0.020 (0.263)	0.044 (0.246)	0.017 (0.179)
N	313	387	700

Robust standard errors in parentheses clustered by village. Inverse probability weights.

*p < 0.10, **p < 0.05, ***p < 0.01.

DV is binary (1=voted for incumbent, 0=voted for another/did not vote) based on survey data.

Table J.4: Treatment Effects Within Private Dissemination Villages, Bad News

	InfoOnlyVillages	CivicsVillages	AllPrivateVillages
Info-Only	-0.162 (0.237)		0.107 (0.286)
Info-Only + Civics		0.272 (0.236)	0.006 (0.299)
Constant	0.234 (0.325)	-0.301 (0.353)	-0.035 (0.238)
N	281	296	577

Robust standard errors in parentheses clustered by village. Inverse probability weights.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Dependent variable is binary (1=voted for incumbent, 0=voted for another or did not vote) from survey data.

K Impacts on Voter Turnout

Tables K.1 and K.2 report impacts of treatment on voter turnout replicating the exact specification from Table 4 in the paper. Where villages received good news about incumbent legislative performance, there are consistently negative and significant effects of treatment on voter turnout – although this is limited to high dosage communes only. Where villages received bad news, there is only evidence of a reduction in turnout in high-dosage, public treatment villages.

Table K.1: The Impact of Positive Information on Voter Turnout

VARIABLES	(1) All Communes	(2) Low Dosage	(3) High Dosage	(4) High Dosage	(5) High Dosage	(6) All Communes
Received any treatment	-0.02** (0.01)	0.03 (0.02)	-0.03** (0.01)			
Performance information only				-0.04** (0.02)		-0.04** (0.02)
Civic + performance information				-0.02* (0.01)		0.03 (0.02)
Public treatment					-0.04** (0.02)	
Private treatment					-0.02* (0.01)	
High Dosage x Civics						-0.05** (0.02)
Constant	0.72*** (0.00)	0.71*** (0.00)	0.73*** (0.00)	0.73*** (0.00)	0.73*** (0.00)	0.72*** (0.00)
Observations	735	368	367	367	365	735
R-squared	0.34	0.33	0.34	0.34	0.35	0.34

In parentheses, robust standard errors clustered by commune in model 1 and commune x treatment, otherwise. Models include block fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table K.2: The Impact of Negative Information on Voter Turnout

VARIABLES	(1) All Communes	(2) Low Dosage	(3) High Dosage	(4) High Dosage	(5) High Dosage	(6) All Communes
Received any treatment	0.01 (0.01)	0.04 (0.04)	0.00 (0.01)			
Performance information only				-0.00 (0.01)		-0.00 (0.01)
Civic + performance information				0.01 (0.01)		0.04 (0.04)
Public treatment					-0.00 (0.01)	
Private treatment					0.01 (0.02)	
High Dosage x Civics						-0.03 (0.04)
Constant	0.69*** (0.00)	0.69*** (0.00)	0.68*** (0.00)	0.68*** (0.00)	0.68*** (0.00)	0.69*** (0.00)
Observations	1,029	731	298	298	298	1,029
R-squared	0.29	0.31	0.16	0.16	0.16	0.29

In parentheses, robust standard errors clustered by commune in model 1 and commune x treatment, otherwise. Models include block fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

L Robustness Checks

L.1 The Negative Effect of Good Information

Table L.1 presents results from a series of robustness checks on one of the main results presented in the paper: that the effect of positive performance information is negative in low dosage communes. In the first column, we replicate the results from the main specification. In the second column, we present results from the main specification including weights to account for the differences in block size. In the third column, we remove the block fixed effects. Finally, to ensure that the results are not being driven by an outlier commune – which is possible given the small number of communes in the low dosage, good news sample – we estimate the treatment effect in analyses with each commune removed. These results are presented in the final six columns of the table. The results in each model are comparable statistically and substantively.

Table L.1: Robustness Tests: Negative Effect of Good News in Low Dosage

VARIABLES	(1) Main Specification	(2) Weights	(3) No Fixed Effects	(4) Dangbo Removed	(5) Dassa-Zoume Removed	(6) Gogounou Removed	(7) Kerou Removed	(8) Kpomasse Removed	(9) Za-Kpota Removed
Treatment	-0.14*** (0.04)	-0.14*** (0.04)	-0.16** (0.06)	-0.13** (0.05)	-0.13** (0.05)	-0.13** (0.05)	-0.14** (0.05)	-0.19*** (0.02)	-0.14** (0.05)
Constant	0.22*** (0.00)	0.18*** (0.04)	0.31*** (0.02)	0.31*** (0.00)	0.22*** (0.00)	0.22*** (0.00)	0.22*** (0.00)	0.22*** (0.00)	0.22*** (0.00)
Observations	392	265	392	342	301	326	349	319	323
R-squared	0.12	0.49	0.01	0.10	0.17	0.13	0.07	0.13	0.13

In parentheses, robust standard errors clustered by commune.

Models include block fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

L.2 The Effect of Civics

Table L.2 presents robustness checks on a second key result: that the civics treatment has a positive effect on incumbent voteshare in high dosage communes when good news is provided. Column 1 presents the main specification. Column 2 includes weights. Column 3 only includes commune fixed effects. Column 4 does not include any fixed effects. The remaining columns estimate the main specification, with each commune in the good news, high dosage sample removed one by one. The results show that the main finding is robust. It is only in column 4 that the coefficient is not statistically significant. However, the coefficient is of the same magnitude and the larger standard error is due to the decrease in statistical efficiency associated with dropping the fixed effects.

Figure L.1: The Distribution of the Incumbent's Performance Score in Good News Commune, by Dosage Level

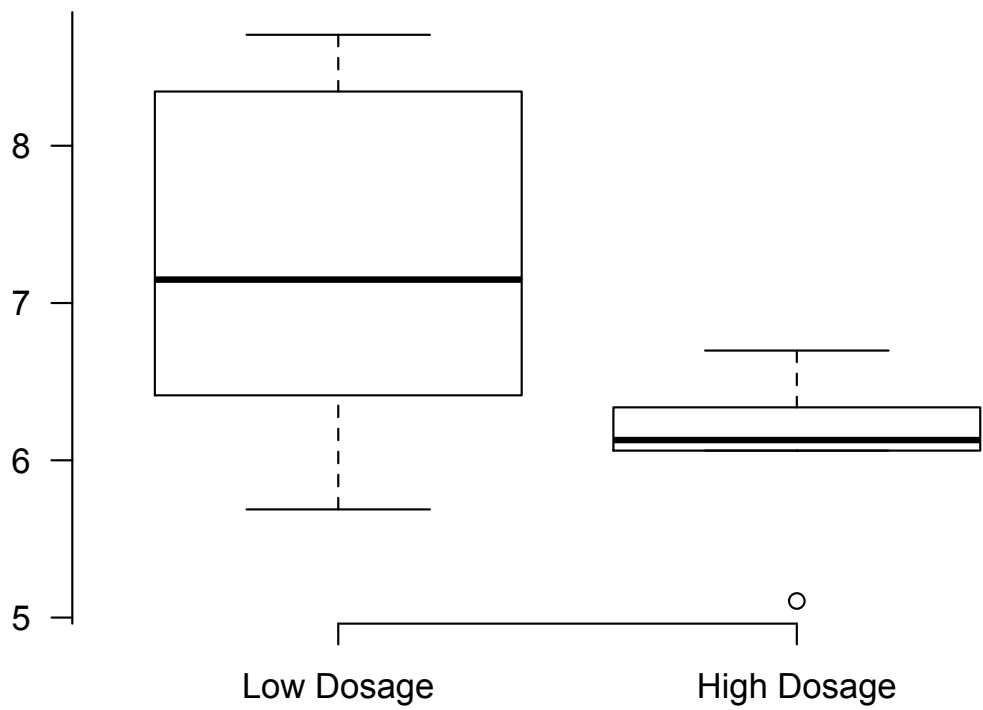


Table L.2: Robustness Tests: Positive Effect of Civics in High Dosage

VARIABLES	(1) Main Specification	(2) Weights	(3) Commune Fixed Effects	(4) No Fixed Effects	(5) Bante Removed	(6) Bembereke Removed	(7) Bopa Removed	(8) Come Removed	(9) Ketou Removed	(10) Kouande Removed	(11) Segbana Removed	(12) Tchaourou Removed	(13) Ze Removed
Performance information only	-0.02 (0.01)	-0.02 (0.03)	-0.02 (0.01)	-0.03 (0.08)	-0.02 (0.01)	-0.01 (0.01)	-0.02 (0.02)	-0.02 (0.02)	-0.02 (0.01)	-0.02 (0.02)	-0.02 (0.01)	-0.03* (0.01)	-0.03** (0.01)
Civic + performance information	0.04** (0.02)	0.04 (0.02)	0.05*** (0.02)	0.04 (0.07)	0.04** (0.02)	0.03* (0.02)	0.06*** (0.02)	0.04* (0.02)	0.06*** (0.02)	0.03 (0.02)	0.05** (0.02)	0.05** (0.02)	0.03 (0.02)
Constant	0.30*** (0.03)	0.30*** (0.03)	0.17*** (0.01)	0.32*** (0.06)	0.37*** (0.02)	0.29*** (0.03)	0.29*** (0.03)	0.30*** (0.03)	0.29*** (0.03)	0.30*** (0.03)	0.29*** (0.03)	0.30*** (0.03)	0.30*** (0.03)
Observations	365	365	613	613	331	332	308	333	327	316	339	333	301
R-squared	0.62	0.63	0.56	0.00	0.61	0.64	0.62	0.60	0.61	0.57	0.63	0.59	0.65

In parentheses, robust standard errors clustered by commune x treatment. Models include block fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

L.3 Controlling for Village Size and Pre-treatment covariates

In this section, we examine whether our results are robust to the inclusion of a dummy variable for village size and pre-treatment covariates. For the former, as pre-specified in our analysis plan, we interact each treatment variable with a categorical variable for population quartile. Because we do not have village-level census data for population size, we instead use the number of registered voters to construct our population quartiles. Table L.3 replicates our main results table but interacts each treatment indicator with the categorical variable for population quartile. Here, the base category is the largest village or 4th quartile. The negative impact of positive information in the low dosage villages is larger (more negative) in the 1st and 3rd quartile relative to the 4th – although only the latter is statistically significant. Similarly, the information only treatment in high dosage has a seemingly more negative effect in villages smaller than the 4th quartile, but these differences are not statistically significant. We also see the magnitude of the positive coefficient on civics is largest in the smallest villages, but again the difference in impact across village size is not statistically significant. While most differences by village size are not significant, the magnitude of the effect tends to be larger in smaller which is what we expected.

To test robustness to pre-treatment covariates, we use control variables that were gathered as part of our baseline survey. Because we conducted the baseline survey in a subset of the control units in each sample commune, the number of observations in this analysis is substantially smaller, which limits our statistical precision. In addition, the sample itself is slightly different, as it does not include the full set of administrative controls. In Table L.4, we first show that our main results hold when we conduct the main analysis on the smaller sample that received the survey (without including pre-treatment covariates). We lose considerable statistical power in this analysis – for example, the analysis in column 1 includes only 12 observations – but the results are qualitatively the same as our main results.

In Table L.5, we introduce a number of pre-treatment controls specified in the pre-analysis plan under covariate adjustment.⁴³ We average the response of each respondent in each village/quarter to produce a measure for each unit in the sample. While these analyses are under-powered because of the smaller sample sizes, the results are qualitatively similar to the main results. We are not able to run the covariate adjustment for the low dosage only sample due to the insufficient number of observations (12).

⁴³The only pre-specified covariate we do not include is opinion leader fractionalization. This variable had an unusually high rate of missing values (72%) and most names of opinion leaders are reported only once leading to very little variation in the fractionalization variable. Because names are recorded in an open-ended format, there are many cases in which two names are similar but we cannot know if they refer to the same person. Further, many people only report first names, so the opposite error is possible – that we would infer two people with the same name are the same person when they are not.

Table L.3: The Impact of Positive Performance Information by Village Size

VARIABLES	(1) Low Dose	(2) High Dose No Civics	(3) High Dose No Civics	(4) All High Dose
Treatment	-0.176*** (0.020)	-0.011 (0.026)		
1st quartile	-0.017 (0.013)	-0.026 (0.029)	-0.027 (0.030)	-0.029 (0.028)
2nd quartile	-0.014 (0.019)	0.002 (0.016)	0.002 (0.016)	-0.002 (0.016)
3rd quartile	0.004 (0.015)	-0.016 (0.025)	-0.017 (0.025)	-0.018 (0.025)
Treatment x Quartile 1	-0.025 (0.023)	-0.003 (0.070)		
Treatment x Quartile 2	0.145* (0.080)	-0.011 (0.035)		
Treatment x Quartile 3	-0.076*** (0.022)	-0.031 (0.043)		
Public treatment			-0.010 (0.027)	
Private treatment			-0.014 (0.036)	
Public x Quartile 1			-0.044 (0.112)	
Public x Quartile 2			-0.048 (0.037)	
Public x Quartile 3			-0.006 (0.053)	
Private x Quartile 1			0.027 (0.070)	
Private x Quartile 2			0.017 (0.051)	
Private x Quartile 3			-0.059 (0.047)	
Civic + performance information				0.027 (0.043)
Performance information only				-0.020 (0.026)
Civics x Quartile 1				0.121 (0.135)
Civics x Quartile 2				-0.068 (0.067)
Civics x Quartile 3				0.058 (0.071)
Info only x Quartile 1				-0.000 (0.063)
Info only x Quartile 2				-0.003 (0.036)
Info only x Quartile 3				-0.009 (0.043)
Constant	0.313*** (0.011)	0.344*** (0.016)	0.344*** (0.016)	0.344*** (0.014)
Observations	392	311	310	365
R-squared	0.124	0.646	0.647	0.624

In parentheses, robust standard errors clustered by commune in model 1 and commune x treatment, otherwise.
Models include block fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table L.4: The Impact of Positive Performance Information (Official Results Using Only the Sample Where a Survey Was Conducted)

VARIABLES	(1) Low Dose	(2) High Dose No Civics	(3) High Dose No Civics	(4) All High Dose	(5) All Communes
Treatment	-0.107 (0.107)	-0.034 (0.030)			
Private treatment			-0.029 (0.028)		
Public treatment			-0.041 (0.040)		
Performance information only				-0.029 (0.030)	-0.029 (0.031)
Civic + performance information				0.028 (0.030)	-0.107 (0.080)
High Dosage x Civics					0.135 (0.086)
Constant	0.256*** (0.076)	0.325*** (0.026)	0.326*** (0.027)	0.326*** (0.026)	0.320*** (0.025)
Observations	12	78	78	132	144
R-squared	0.515	0.779	0.780	0.641	0.643

In parentheses, robust standard errors clustered by commune in model 1 and commune x treatment, otherwise. Models include block fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table L.5: The Impact of Positive Performance Information with Pre-Treatment Controls (Official Results Using Only the Sample Where a Survey Was Conducted)

VARIABLES	(1) High Dose No Civics	(2) High Dose No Civics	(3) All High Dose	(4) All Communes
Treatment	-0.035 (0.034)			
Private treatment		-0.041 (0.031)		
Public treatment		-0.027 (0.047)		
Performance information only			-0.038 (0.036)	-0.038 (0.037)
Civic + performance information			0.037 (0.036)	-0.071 (0.081)
High Dosage x Civics				0.107 (0.088)
Female	-0.091 (0.437)	-0.054 (0.446)	-0.305 (0.355)	-0.330 (0.361)
Years of education	0.007 (0.020)	0.007 (0.021)	0.026** (0.013)	0.027** (0.013)
Coethnic with incumbent	-0.152 (0.096)	-0.155 (0.099)	-0.140** (0.060)	-0.140** (0.061)
Age	0.000 (0.004)	0.000 (0.004)	0.005 (0.004)	0.005 (0.004)
Poverty index	0.013 (0.094)	0.008 (0.102)	0.030 (0.089)	0.048 (0.091)
Information from radio	-0.156 (0.179)	-0.147 (0.190)	-0.221 (0.152)	-0.215 (0.148)
Information from newspaper	-0.356 (0.915)	-0.254 (0.894)	-0.386 (0.468)	-0.376 (0.466)
Information from television	-0.489** (0.219)	-0.491** (0.225)	-0.498** (0.209)	-0.513** (0.209)
Ethnic Homogeneity	-0.109 (0.097)	-0.116 (0.098)	-0.099 (0.085)	-0.101 (0.086)
Ballot secrecy can be violated	-0.102 (0.178)	-0.099 (0.182)	-0.085 (0.116)	-0.088 (0.116)
Parties know how the village voted	0.101 (0.132)	0.095 (0.134)	0.337*** (0.093)	0.337*** (0.092)
Constant	0.634* (0.335)	0.614* (0.346)	0.430* (0.241)	0.440* (0.247)
Observations	78	78	132	144
R-squared	0.821	0.822	0.716	0.716

In parentheses, robust standard errors clustered by commune x treatment.

Models include block fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

The sampling procedure for the baseline survey occurred as follows: enumerators used a random walk procedure to select compounds in which to administer the baseline survey and/or intervention. Within compounds, individual respondents were randomly selected from the list of adult members of the compound while alternating on gender. As a condition of participating in the baseline survey, respondents had to have access to a cell phone.⁴⁴ Respondents were then recontacted by phone during the endline survey. A total of 3,419 individuals participated in the baseline *and* endline surveys (6,132 in the baseline), with an additional 6,174 receiving the intervention (or an invitation to the public screening) but no survey. To maximally harmonize public and private treatments, we endeavored to treat the same numbers of individuals per village across conditions. Thus, we provided the private treatment to 40 individuals in each private village even though we surveyed only a random half of those.

⁴⁴They were not required to own a cell phone. The cell phone to which they had access could belong to a friend or relative.

M Candidate Vignette

Our baseline survey data also provides evidence of voter preferences for transfers over legislative performance. A survey experiment presented respondents a short description of a hypothetical candidate. One of three versions of the vignette was randomly assigned to respondents: control, legislative, and transfers. In the legislative version, we added to the baseline description, “He has been an active participant in the National Assembly in Porto Novo.” In the transfers version, we added, “He has sponsored community activities in this village.” Voters were then asked to rate their likelihood of voting for the candidate on a scale of one to seven. Figure M.1 summarizes the level of support for the hypothetical candidate in each condition. Results indicate that adding a sentence about candidate transfers, on average, significantly increases the likelihood of voting for the candidate relative to both the control condition and relative to the legislative condition. This effect, suggesting that voters indeed prefer transfers over legislative performance, is statistically significant at the 95% confidence level.⁴⁵ Just as interesting is the null result we obtain when our hypothetical candidate performs well on legislation: here, survey respondents do not show a preference for legislative performers over our control candidate.

⁴⁵The effect persists in a multivariate model that controls for treatment blocs, and clusters the standard errors at the village level.

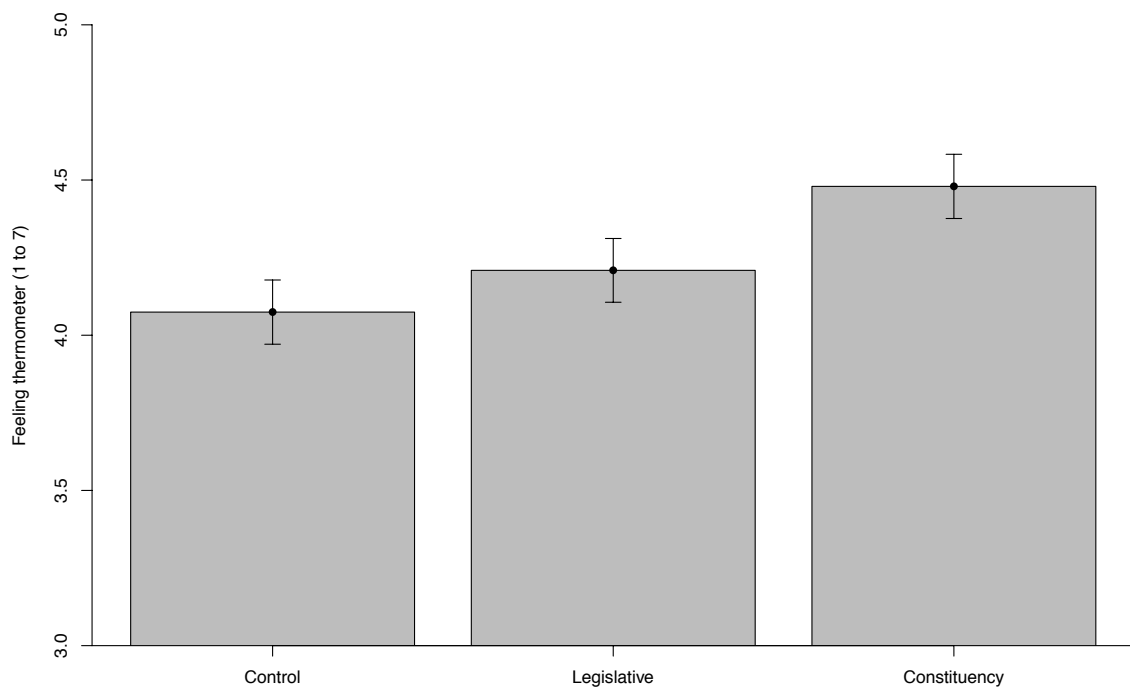


Figure M.1: Support for Candidate

N Deviations from Pre-Analysis Plan

In this section, we describe several ways in which our data analysis deviates from the original pre-analysis plan. In each instance, the main conclusions of the study are unaffected.

First, our econometric specification differs from the specification presented in the pre-analysis plan. Our objective was to estimate the average effect of private provision, public provision, information only, and civics, respectively. We thus wrote the following model:

$$E(Y_{ij}|Positive_j[Negative_j]) = \beta_0 + \beta_1 InformationOnly_i + \beta_2 Civics_i + \beta_3 Private_i + \beta_4 Public_i + \mu_k \quad (5)$$

where μ_k represent block fixed effects. After the data were collected, however, it became clear that it is not possible to estimate the above model. Rather, the average effect of information only and civics, on the one hand, and public and private, must be estimated in separate models. In the paper, the models thus take the following form:

$$E(Y_{ij}|Positive_j[Negative_j]) = \beta_0 + \beta_1 InformationOnly_{ij} + \beta_2 Civics_{ij} + \mu_k \quad (6)$$

$$E(Y_{ij}|Positive_j[Negative_j]) = \beta_0 + \beta_1 Private_{ij} + \beta_2 Public_{ij} + \mu_k \quad (7)$$

In Table N.1 below, we present the results from these models.

Second, our pre-analysis plan includes the hypothesis about the effect of dosage (H7) under the category of secondary outcomes and conditional effects rather than a main hypothesis. In the paper, we treat this as a main hypothesis for two reasons. Dosage was a randomly assigned treatment built into our design – not a pre-treatment conditioning variable or secondary dependent variable like the other hypotheses in the category. This supports the idea that it was a key theory we wanted to test prior to rolling out the experiment. While the dosage and public treatments both address theoretical questions about voter coordination, the motivating theories are distinct in that they test the importance of across-group and within-group coordination, respectively, which are independent and separable mechanisms. On the

other hand, another of the pre-specified main hypotheses – the joint effect of the Civics and Public treatments (H4) – is not explicitly tested in the paper. This is because the Public treatment did not have a direct effect, so it did not make sense to ask whether that null effect was then amplified in some conditions. The data further show that this joint effect is also null.

Third, our pre-analysis plan specified that we would use inverse probability weighted regressions to account for differences in block size in our analysis of the official results. These differences arise because the three blocks we use for within-commune stratification – urban, rural/competitive, and rural/non-competitive – are of unequal sizes. As a result, villages are assigned to treatment and control across blocks with different probabilities. In the main analyses in the paper, we do not use inverse probability weighting because we now believe that the block fixed effects are sufficient to control for these differences. The tables in Appendix L show that our main results are robust to the use of inverse probability weights. For completeness, Table N.1 below presents all of the results using inverse probability weights. We note that the coefficient on civics in the good news high dosage communes (column 3), is not statistically significant at conventional levels using this approach. However, the p-value is 0.103. We thus interpret the results as consistent with those presented in the main body of the paper.

Finally, in the main paper, we estimate the interaction between treatment and dosage in models that include block fixed effects. This approach allows us to estimate the interaction between treatment and dosage but, because all units within blocks are assigned to the same dosage condition, it does not permit us to estimate the impact of dosage itself. In the pre-analysis plan, we pre-specified that we would control for urban/rural and electoral competitive but not include blocked fixed effects in order to estimate the dosage effect. Columns 4 and 8 of Table N.1 present these results. The main conclusions of the paper are not affected by these differences in specification.

Table N.1

VARIABLES	(1) Good News Low Dose	(2) Good News High Dose	(3) Good News High Dose	(4) Good News All Communes	(5) Bad News Low Dose	(6) Bad News High Dose	(7) Bad News High Dose	(8) Bad News All Communes
Public, Civics	-0.140*** (0.039)				-0.053 (0.031)			
Performance information only		-0.020 (0.025)				0.008 (0.034)		
Civic + performance information		0.037 (0.022)				-0.007 (0.025)		
Private treatment			0.009 (0.026)				-0.014 (0.028)	
Public treatment			0.007 (0.025)				0.014 (0.031)	
Received any treatment				-0.142** (0.053)				-0.129 (0.092)
Treatment x Dosage				0.149* (0.087)				0.127 (0.123)
Dosage				0.038 (0.067)				0.068 (0.091)
Constant	0.302*** (0.033)	0.332*** (0.018)	0.332*** (0.018)	0.315*** (0.035)	0.289*** (0.027)	0.402*** (0.018)	0.402*** (0.018)	0.372*** (0.071)
Observations	265	365	365	630	390	297	297	687
R-squared	0.490	0.629	0.615	0.119	0.827	0.685	0.687	0.144