

Pre-Analysis Plan:

Can Common Knowledge Improve Common Goods? A Field Experiment in an African Democracy

Claire Adida Jessica Gottlieb Eric Kramon Gwyneth McClendon

March 8, 2015

Abstract

Under what conditions does information about politician performance affect electoral behavior in developing democracies? This document presents the pre-analysis plan for a national-level field experiment in the lead-up to Benin's 2015 legislative elections. This field experiment, part of EGAP's First Regranting Initiative, has two objectives. First, to test whether information about MP performance affects vote choice. Second, to investigate the potential effects of two conditioning factors: the salience of the information to voters' wellbeing; and the mode of information-delivery (private vs. public). We combine a number of data collection methods, e.g. surveys, electoral results and hearsay ethnography, to offer a comprehensive understanding of the ways in which information may improve democratic accountability in an African democracy.

1 Motivation and background

How can we better understand the contexts under which information about politician performance affects citizens' electoral behavior?¹ Research so far has offered inconclusive evidence about the effects of legislator information on voter behavior; in turn, scholars are now trying to understand the conditions under which information might have an effect. We address this question with a field experiment in Benin, in which we propose to treat citizens with information about legislator performance (in the run-up to the country's April 2015 legislative elections) while also studying two moderating factors that have thus far received little attention in the literature: (1) the expectations that citizens hold about their legislators' activities and performance, and (2) whether performance information is delivered privately or in a space allowing for coordination across voters. We locate our study in Benin where voters know little about how well national deputies perform formal legislative activities and instead vote largely based on the provision of clientelistic constituency services (Hounkpe and Warren, 2012).

Benin is a key site for this research, because opinion of and knowledge about the legislature is low and voter coordination potentially crucial. Previous field experiments in Benin (Wantchekon, 2003; Fujiwara and Wantchekon, 2013) demonstrate the possibility of shifting voter attention from clientelist to programmatic appeals. But these studies focus on prospective campaign promises, rather than on retrospective incumbent performance, and they focus on presidential appeals rather than on voter-legislature linkages. The latter is an extremely important area for increased accountability in African politics. This is particularly true for Benin, where the Constitution explicitly assigns the legislature the role of keeping a check on executive power. Another study from Benin shows that plausibly exogenous access to communal radio messages highlighting the advantages of public goods diminishes support for patronage relative to public goods (Keefer and Khemani, 2014). Our study will test whether these survey-based findings are borne out in actual elections.

We propose to implement a field experiment around the 2015 National Assembly elections,

¹This project has received IRB approval from UCSD, TAMU, George Washington University, and Harvard University. Data collection is scheduled to begin in January 2015.

currently scheduled for April 26. The National Assembly is Benin's only legislative body, comprised of 83 deputies elected for 4-year terms. The constitution grants it powers of legislation, executive oversight and representation. According to the African Legislatures Project (ALP), however, voter evaluations of deputies rely largely, not on legislative activities, but instead on clientelist support such as financing funerals, paying school fees, and providing jobs (Hounkpe and Warren, 2012). This pattern may, in part, be due to an attribution problem. Parties are weakly institutionalized in Benin, so voters tend to evaluate individual candidates when considering local public goods spending. But, when it comes to evaluating performance within the national assembly, where 35 parties have formed 3 coalitions: if a constituent favors a particular piece of national legislation, it may be difficult for her to know which of the parties or individual deputies succeeded or failed in passing the legislation.

This pattern of voters' not holding legislators accountable for legislative activity occurs in other parts of Sub-Saharan Africa as well. Research in Ghana (Lindberg, 2003), for instance, suggests that MPs are frustrated by the extent to which voters hold them accountable for constituency service which impedes their ability to spend time on more publicly beneficial services such as legislative activity. Based on preliminary interviews with several legislators, Benin deputies seem to be similarly interested in whether there are interventions that can shift voters toward giving more consideration to legislative performance.

In a context of low public knowledge and opinion about the Assembly, we propose to provide voters attributable information about legislative activity that might yield changes in voter beliefs and actions. The type of information we provide on legislative performance is likely to be new. If these measures of performance are also perceived by voters to be relevant to their own welfare (e.g. active advocacy, oversight and policymaking in the capital is likely to bring more "pork" home), then the provision of information on legislative activity should lead voters to reward high-performing and to punish low-performing politicians.

We also propose to compare the effects of providing attributable incumbent performance information alone with providing that information alongside a "civics message", where we *explicitly*

provide arguments about the important implications of national legislation for voters' daily lives. Such arguments come in the form of positive examples of socially beneficial legislation and negative examples of missed opportunities to improve citizen welfare due to poor performance. An instance of the latter, a universal health insurance scheme (RAMU) has been prevented from taking effect due to the Assembly's failure to pass the requisite legislation. If the information alone (though new and attributable) is not perceived to be relevant to voter welfare, we expect that providing it in conjunction with a civics message will increase the salience, and thus the impact, of the information.

Benin presents an additional constraint to holding its legislative deputies accountable: the difficulty with which voters can coordinate around sanctioning that deputy for good or poor performance. Because voters in Benin have strong expectations that others will vote based on constituency service rather than legislative performance, they might consider a performance-based vote a wasted ballot. Shifting the dimension along which voters coordinate from clientelist favors to programmatic achievements requires not only changing individual preferences, but changing expectations of other people's preferences or actions. The production of common knowledge may thus act as a key conditioning factor for the effectiveness of information provision on legislative performance. In our intervention, we test this possibility by varying the delivery of the information intervention – providing information privately or to groups of voters – to address the coordination problem.

We propose to provide all information about legislative performance and its importance for voter welfare through videos, thus holding constant the content and tone of delivery. In some conditions, we offer voters private screenings of the video, while in others we offer public ones.

2 Theory

Drawing from the literature on information and voting, our intervention will test two key conditioning variables for the effect of information on voter behavior: (1) the baseline expectations

voters hold about legislative responsibility and performance, and (2) the interest voters have in coordinating their vote.

Experiments disseminating incumbent performance information in India (Banerjee et al., 2011), Mexico (Chong et al., 2011), Brazil (Ferraz and Finan, 2008), Puerto Rico (Bobonis, Cámara Fuertes and Schwabe, 2012), and Uganda (Humphreys and Weinstein, 2012) have met with mixed results in improving sanctioning of poor performers. Information sometimes removes poorly performing politicians, but sometimes it has no effect, and sometimes it depresses turnout and punishes challengers. It is clear that we need to pay more attention to the moderators of the relationship between performance information and voting (Pande, 2011; Azulai et al., 2014).

A few studies have taken up this call. For instance, Gordon, Humphreys and Weinstein (2013) and Dewan, Humphreys and Rubenson (2013) explore the conditioning effects of partisan and third-party endorsements on information credibility. Gottlieb (2013) implements a civics education treatment to encourage citizens in Mali to expect more from government and then tests for increased attention to performance in a simulated voting exercise.

Our proposed study pushes the frontier further. We take up Gottlieb's suggestion that, where voters currently have low knowledge about and opinions of representative institutions, the impact of performance information may be heightened when voters are exposed to arguments about the quotidian importance of legislation. We test this insight in a real-world, national election.

We further introduce the possibility that coordination may be critical. Voters should be motivated to coordinate with one another on a particular candidate when there are strategic complementarities – or benefits to voting at high rates for a particular candidate. This will be the case where political parties engage in "contingent prize allocation" (see Smith and Bueno De Mesquita 2012) or reward club goods to groups or localities that vote for them at high rates. These benefits only accrue to individuals if sufficient voters in their group or locality elect a particular candidate, or conversely, programmatic or club goods can be denied to groups that do not vote at sufficiently high rates (see Magaloni's discussion of the PRI's punishment regime in Magaloni (2006)). We examine the extent to which voters perceive politicians to be guided by the logic of contingent

prize allocation with baseline survey questions about 1) whether legislators are aware of village levels of electoral support (*Bq44*) and 2) whether legislators condition transfers on that support (*Bq45*).

Other studies (e.g. Humphreys and Weinstein 2012) have used public workshops to disseminate information but have not distinguished this method from private provision, which they simultaneously employ. We furthermore do not involve the candidates themselves in the public intervention, thereby reducing the possibility of elite capture. In sum, ours is the first to directly test whether the delivery of information on legislator performance is more likely to affect voter preferences and behavior if it is delivered in a public setting, where common knowledge can be produced.

While we aim to reduce elite capture by disseminating information at a very local level (the village or urban quarter) and explicitly not involving legislative deputies in the information provision, we cannot guarantee that deputies will not find out about the project and attempt to obstruct or obscure the information being provided. We measure the extent to which parties know about and co-opt the information provided in our treatments by asking questions on the endline survey about whether participants heard parties talking about the project (*Eq29*) and/or discounting information provided by the project (*Eq30*). If parties do try to undermine the information provided by the treatment, it should bias us against finding a treatment effect as incumbents with poor performance records are likely to make their performance appear less bad to constituents.

The literature on information and voting identifies a number of additional variables that might condition the relationship between information and voting. We specify these below, as they will inform our tests for heterogeneous treatment effects.

1. Individual level

- Priors
 - about the importance of legislative performance: Gottlieb (2013) informs us that the impact of performance information may be conditional on whether voters have

low prior opinions of legislators (which is the case in Mali, where she performed her study). In other words, if voters hold prior beliefs about the obsolescence of legislators, their response to performance information may be heightened if the information is delivered in conjunction with explicit civics messages about the quotidian importances of legislative activity.

- about the preference for the incumbent: Humphreys and Weinstein (2012) explicitly analyze their treatment effects of performance information as a conditional one: they condition this effect on individuals' prior preferences for the legislator. Individuals who receive positive information about an incumbent they already liked will respond differently than individuals who receive positive information about an incumbent they did not like. Here, we operationalize incumbent preference by looking at baseline warmth towards the incumbent and coethnicity.
- about the belief regarding incumbent legislative performance: an extension of Humphreys and Weinstein (2012) is that individuals will respond differently to performance information if it contradicts their prior beliefs than if it confirms their prior beliefs.
- Gender: Wantchekon (2003) argues that women, especially rural women, may be more responsive to a candidate's national legislative platform because they tend to be excluded from most forms of clientelist redistribution. Additionally, given women's lower education levels on average, we expect to see a greater marginal effect of performance information on women. If this is the case, we expect women to respond more strongly than men to information about legislative performance.
- Education: Verba et al. (1995) shows us that education is positively correlated with political participation, and that it allows citizens to acquire civic skills and use those to communicate effectively with politicians. If this is the case, then participants with higher levels of education may be better able to integrate the information we provide in our treatments, and thus to respond. At the same time, at the highest levels of education,

respondents may be less responsive to legislator performance because they have already acquired this information on their own. Gottlieb (2013) finds that this is the case in her work. We therefore expect that performance information will have an inverse U-shaped relationship with education level.

- **Ethnic attachment:** Bratton and Kimenyi (2008), in their analysis of political preference in Kenya, distinguish between survey respondents who identify first and foremost in ethnic terms and those who identify in non-ethnic terms (such as class, religion, or gender). *Ethnics*, they find, prioritize ethnic identity in their political preferences; but *non-ethnics* prioritize policy issues and performance instead. Consistent with this insight, we propose that the effectiveness of our intervention will vary with the strength of an individual's ethnic attachment. *Ethnics* will be less responsive to performance information across the board, while *non-ethnics* will be more responsive.
- **Clientelism:** Pre-existing clientelist linkages may also condition the impact of information dissemination. In particular, voters with clientelist connections to incumbents may be less responsive to performance information. This may be because they are held accountable for their vote by party activists (Stokes, 2005) or because they are dependent on clientelist resource flows from the incumbent.

2. Constituency level

- **Perceived anonymity of voting:** We expect that villages where individuals do not perceive perfect ballot-secrecy are villages where individuals are more susceptible to social norms. Scholars have shown that social norms support in-group cooperation (Habyarimana et al. (2007)). We thus expect to observe a greater effect of our common knowledge treatment in those villages.
- **Density of opinion leaders:** Villages with more opinion leaders receiving treatment are likely to be villages where changes to social norms are more likely to be implemented. Following our claim above, we expect that villages with a greater density of opinion

leaders receiving treatment will see a greater effect of common knowledge. Additionally, villages with more available opinion leaders or more fractionalization among them may be more open to new information and ideas.

- Urban/rural: Harding (2010) finds that support for incumbent governments is significantly higher among rural residents compared to urban residents in Afrobarometer data. Koter (2013) confirms this pattern in Senegal, and attributes it to rural Senegalese's greater susceptibility to clientelism. We therefore expect performance information to hold a weaker effect among rural residents than among urban residents. At the same time, rural constituencies tend to be more homogenous (Wantchekon (2003)) and thus to vote more as a bloc; hence, we expect that our common knowledge treatment will have stronger effects in rural areas relative to urban areas.
- Electoral competitiveness: In very competitive places, information is most likely to be provided to voters by opposition parties, reducing the marginal effect of additional information. In very uncompetitive places, there is little chance of the incumbent losing, so the marginal effect of information is small, but for a different reason. We thus expect the relationship between information and competitiveness to be an inverse-U-shaped. It is in middling levels of political competitiveness where we should expect the biggest effect of information.
- Ethnic homogeneity: One of the mechanisms through which our public information dissemination campaign can affect vote choice is via social norms. Habyarimana et al. (2007) have shown that social norms explain why two individuals in Uganda are more likely to cooperate if they share an ethnicity than if they do not. Similarly, villages with greater ethnic homogeneity are villages where those social norms will enhance the effect of our public intervention. We thus expect a stronger effect of the public intervention in more ethnically homogenous villages.

3 Experimental design

3.1 Political context

The political office we study in this project is the legislative member of Benin's unicameral National Assembly. Members, called deputies, are elected through a system of party-list proportional representation to a 4-year term. Currently, Cauri Forces for an Emerging Benin, the party of the president, holds 41 seats in the legislature and a broad alliance of opposition parties, Unite the Nation, holds 30 seats. The remaining 12 seats are held by 6 unaligned parties.

Benin's 12 departments are divided into two legislative constituencies. These are multi-member districts with an average of 3.5 legislative seats for a total of 83 deputies in the national legislature. In addition to legislative deputies that represent them at the national level, citizens also elect local councils that serve at the commune level. Benin's 77 communes are nested within constituencies so there are, on average, 3 communes within each constituency.

In a proportional representation system with multi-member districts, it is unclear who the voter considers the incumbent when making her electoral decision. On programmatic issues, the voter might sanction or reward the incumbent party or opposition coalition. However, on particularistic issues, a voter's calculus is less clear. If a voter is generally dissatisfied with the performance of the deputies in their constituency, how will they decide which of the several available parties to reward or sanction in the next election?

There is a strong trend in Benin for which we have collected qualitative evidence² that citizens within a commune consider one of the several deputies they elect in their greater constituency as their primary deputy. This simplifies the voter's calculus and the ability to measure the impact of information on vote choice. We thus restrict our sample to 30 communes for which we are able to identify a one-to-one mapping of deputy to commune, and where the incumbent is running

²In the 24 focus groups performed in 8 villages in each of our block types, the majority of participants agreed on a single deputy who was particularly responsible for their community.

again in 2015.³ Our design thus treats the deputy who has been informally allocated to a particular commune as the “incumbent” candidate for the voters within that commune.

3.2 Sampling strategy

We implement a 2x2 factorial design with a pure control. Participants either receive information about legislator performance (the common intervention) or this same information plus additional information highlighting the importance of legislative performance for ordinary citizens’ lives (the civics message: more below). Participants either receive this information privately by watching the video on a smartphone in the respondent’s household or publicly through the screening of the same video via a projector in a public location in the village or quarter.

The five conditions being studied are as follows, although conditions 2 and 3 are always in combination with one of conditions 4 and 5:

1. Control: no intervention
2. T1: provision of information about legislator performance
3. T2: provision of information about legislator performance *and* a civics message
4. T3: provision of information via a private mechanism
5. T4: provision of information via a public mechanism

Each of the four treated groups will thus receive one of two information types and one of two delivery mechanisms. The control condition receives no intervention. To increase power and exploit an opportunity to measure treatment spillovers, we survey a random sample of individuals in private treatment villages who do not receive the treatment. These individuals provide an additional control group and will be discussed at greater length when we explain the survey sample.

³Our local partner organization performed this one-to-one mapping using local knowledge and contacts. We further observe the legitimacy of our assumption by asking survey respondents to identify the legislative deputy who is most responsible for their village in *Bq23*.

Table 1: Experimental Conditions and Treatment Assignments (in high-dosage communes)

	Control	Private (T3)	Public (T4)	Total
Control (with survey)	A (N=45)			
(w/o survey)	(N=504) ⁴			
Performance (T1)		B (N=45)	C (N=45)	D (N=90)
Performance + Civics (T2)		E (N=45)	F (N=45)	G (N=90)
Total		H (N=90)	I (N=90)	

In the next section, we motivate the theoretical import of an additional experimental treatment, whether treatment dosage, or the density of treated villages within the next largest administrative unit (the commune) conditions the effect of treatment. For this test, we randomly assign the high- or low-dosage condition at the commune level. Our total experimental sample includes 30 communes with 15 in each dosage category. The sample comprises 225 villages in high-dosage communes and 30 villages in low-dosage communes for a total of 255. Our main hypotheses tests occur within high-dosage communes so we focus the rest of this subsection on explaining assignment to treatment within these communes and turn to dosage in the next subsection.

In high-dosage communes, we randomly assign the 225 sample units to one of the 4 equally-sized treatment arms and the control arm with surveys for a total of 45 units in each arm. While the baseline and endline surveys will be conducted in 45 of the control villages, administrative data will be collected on all polling stations in the sample communes to serve as a larger control group for the analysis of electoral outcomes. Rationale for the sample size is discussed in the section on power analysis. Table 1 displays each of the treatment conditions and the projected sample size in each treatment condition.

⁴While 15 of the villages in each sample commune will be visited to carry out one of the 5 experimental conditions, we use administrative data from the remaining 504 villages across our 15 sample communes to conduct an additional comparison.

Our research design will thus address the following theoretical questions:

1. Can information about the implications of legislative activities increase the impact of performance-related information on electoral behavior?
2. Relative to private information, can common knowledge help voters coordinate their electoral behavior?

3.3 Dosage concerns

The unit of randomization is the rural village or urban quarter which are administrative equivalents in Benin.⁵ These units are the lowest level of social and territorial organization in Benin and thus allow us to treat the highest proportion of individuals within our sample units. Further, villages (though less so quarters), form a cohesive social structure such that information spillovers are likely within, but not necessarily, across units.⁶

Our prior is that an individual's electoral calculus is affected by her beliefs about how others in her village and her constituency will vote. The logic of contingent prize allocation suggests that voters within a village will be incentivized to coordinate their votes with other members of the village to yield the greatest possible plurality for a given candidate. If parties are indeed allocating resources on the basis of this logic, then a village voting largely for a losing candidate is a risky strategy. Voters are thus affected by beliefs about how other villages in their constituency will vote as well other voters in their own village.

If it is true that voters will only react to new information if they believe sufficient other villages are receiving that information, then we risk undermining our experiment if we treat too few villages

⁵On average, there are 35.6 rural villages and 15.4 urban quarters per commune. There are 1,253 registered voters in the average urban quarter and 846 registered voters in an average rural village.

⁶Though we posit this here, we can test for it empirically in our data by comparing spillovers both across and within villages. The former we are able to get at through comparing control villages across high- and low-dosage communes. We assess the latter by comparing treated to "control" participants within private treatment villages where we survey but do not treat a sample of participants.

in a constituency. Given that we are treating communes like single-member districts, we calculate dosage at the commune level. With approximately 48 villages/quarters per commune and 4 seats per constituency, we estimate that one-quarter or 12 treated villages per commune is an appropriate threshold for voters to believe that sufficient other voters may change their behavior in response to the information treatment. Thus, rather than assigning treatment to villages across the universe of communes, we constrain assignment such that each of 15 communes has 12 treated units (and 3 control).

In order to test our proposition that voter behavior is conditioned by expectations of how other villages will vote, we add one additional treatment condition: a low-dosage treatment. Rather than assigning 12 villages to treatment per commune, we assign one (and an additional one to control with baseline/endline surveys). In light of the 15 high-dosage communes, we do this in 15 additional low-dosage communes increasing our experimental sample from 225 villages/quarters to 255. To reduce variation in the treatment effect, we assign only one version of treatment to the unique village in each of the 15 low-dosage communes. As a bias against our proposition, we assign the treatment we think is strongest *a priori*, the combination of T2 and T4. Due to the low cluster-level sample size of 30, we only have a power of 0.6 to detect an effect of the dosage treatment. Given the relatively small marginal cost of adding 15 new treatment and 15 new control units, we consider this a worthwhile exercise.

3.4 Block Randomization

Because villages within communes share unique characteristics such as geographic location and, importantly, traits of the incumbent, we block randomize within communes. Within each commune, we randomly assign 3 rural villages/quarters to each of the 4 unique treatment conditions and control with survey condition. To increase statistical efficiency and improve our ability to experimentally identify conditional treatment effects, we further stratify treatment assignment within communes on two variables we expect to moderate the effect of treatment: urban/rural status and

electoral competitiveness of the village in the previous legislative election.

To generate our measure of electoral competitiveness, we use village-level elections results data from Benin's 2011 legislative elections. We calculate the village-level vote margin in those elections (winner voteshare - second place party voteshare) and define villages as electorally competitive if the vote margin is below the median and non-competitive if not. The median vote margin in our sample of communes is 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 create a dummy variable indicating location in a rural area. About a quarter of localities in our sample are urban. Unexpectedly, competition and rural status are not correlated.

Due to the considerably larger number of rural villages in the sample, we generate three blocks within which to assign treatment: urban, rural/competitive, and rural/non-competitive. Within each commune, we then randomly assign one locality from each block to each our four treatment conditions and our control condition with survey.⁷ The remaining localities serve as additional control communities in analyses using administrative data. Because blocks are of unequal size within and across communes, we use inverse probability weighting in our regression analysis to account for varying probabilities of assignment to treatment.

At the individual level within villages, we will stratify survey respondents and recruited participants on gender to permit analyses of heterogeneous treatment effects. Because incumbent performance, observed at the commune level, is also expected to mediate the effect of treatment, we will ensure balance on this variable among the high- and low- dosage communes. To create our measure of incumbent performance, we will use the data on legislative performance that we collect and disseminate as part of the intervention. Being in the culturally distinct north or south of the country is also an important moderator of political behavior, we block on this as well. Within these 4 blocks (high and low performance in the north and south), we assign half to high-dosage and half to low-dosage treatment.

⁷There are only 4 urban quarters in a couple of our sample communes. Because we need a block size of at least 5 to ensure probability of assignment to treatment of less than 1, we add the largest rural village from the commune to that block in these rare cases.

4 Hypotheses

4.1 Assumptions

We first propose that providing civic information will increase the effect of performance information on voter behavior by increasing voters' valuation of the performance dimension. Such a proposition rests on the assumption that voters underestimate the value of the performance dimension. *H0a* tests the assumption that voters in Benin condition their vote on their legislator's ability to provide public goods less often than they condition their vote on private transfers or the provision of club goods.

Our second hypothesis proposes that common knowledge increases the impact of performance information on voting behavior through the mechanism of voter coordination. Such a proposition rests on the assumption that voters face a coordination problem. *H0b* tests the assumption that voters in a particular locality have an incentive to coordinate on a single candidate. Such an assumption is supported by the idea of prize pivotality in Smith and Bueno De Mesquita (2012) in which politicians condition the provision of club goods to a group or locality on their relative vote share from that group or locality.

Finally, the private versus common knowledge interventions are expected to produce a differential treatment effect because of the differential impact on beliefs about how other people will vote. For this to be true, the treatments should hold everything else constant, e.g. information content, comprehension. *H0c* validates the assumption that T3 and T4 generate equivalent knowledge and learning.

H0a: At baseline, voters in Benin are more likely to vote on the dimension of constituency service than legislative performance.

H0b: Villages in Benin are rewarded for bloc voting; in other words, parties and politicians condition the distribution of club goods to a particular locality on their vote share in that locality. Thus, voters have an incentive to coordinate on voting for a particular candidate.

H0c: Voter comprehension of the information provided in T1 and T2 should be equivalent across the private (T3) and public (T4) interventions.

4.2 Main Hypotheses

H1 : Increasing access to attributable legislative performance information about an incumbent will have an effect on electoral support for that incumbent.

- *H1a*: Positive information will increase support for the incumbent.
- *H1b*: Negative information will decrease support for the incumbent.

H2 : A civics message providing explicit arguments about the value of legislator performance for voter welfare (increasing the salience of legislator performance) will amplify the effect of information on voter behavior.

H3 : The provision of public information will have a greater impact on voter behavior than the provision of private information.

H4 : Public provision will have the greatest impact in combination with the civics message.

4.3 Hypotheses on Secondary Outcomes and Conditional Effects

On “Theory” 1 (Salience)

H5 The civics message increases the importance the voter attaches to legislator performance information when making voting decisions.

H6 The civics message will have a greater effect among individuals who have lower prior beliefs about the *value* of legislator performance.⁸

⁸The prior value a voter accords to legislative performance is different from the prior belief about the level of legislative performance. The former has to do with salience of the performance dimension or the elasticity of vote choice with respect to performance information.

On “Theory” 2 (Common Knowledge and Coordination)

H7 Increasing the dosage of treatment in a commune increases the impact of information.

H8 Public provision of information about legislator performance has a larger effect on voter turnout than private provision.

H9 Public provision increases the voter’s perception that others will attach importance to legislator performance information when making voting decisions.

H10 Public provision increases the voter’s ability to coordinate by improving expectations of how others will vote.

H11 Public provision should have a stronger effect in rural relative to urban areas, because incentives to vote as a bloc are stronger in rural areas.

H12 Where voters believe their vote can be monitored or observed, public provision will have an even greater marginal impact than private provision.⁹

H13 Private provision will have a stronger impact where the ethnic homogeneity of the village is greater.

H14 Public provision will have a relatively stronger impact where the density of recognized opinion leaders among randomly selected intervention participants is greater.

On “Theory” 0 (Effect of Any Information)

H15 Information about legislator performance increases voter turnout.

H16 The impact of information will be increasing in the differential between prior beliefs about incumbent legislative performance and the actual performance information given.

⁹The logic is that, in such contexts, the cost to voting based on performance is higher. Common knowledge and coordination are therefore more important.

H17 The impact of information will be increasing in the differential between the type of performance information given (good/bad) and the voter's prior preference for the incumbent.¹⁰

H18 Pre-existing clientelist linkages will condition the impact of information:

- *H18a*: Negative information will have a weaker effect when the voter and incumbent have a pre-existing clientelist linkage.
- *H18b*: Positive information will have a stronger effect when the voter and incumbent have a pre-existing clientelist linkage.

H19 Ethnic linkages will condition the impact of information:

- *H19a*: Negative information will have a weaker effect when the voter and incumbent are coethnics.
- *H19b*: Positive information will have a stronger effect when the voter and incumbent are coethnics.
- *H19c*: These effects will be bigger among individuals with a stronger attachment to their ethnic group.

H20 The impact of information will be higher in information-poor contexts where voters lack other sources of information about incumbent performance.

H21 Because information may already be high in very competitive districts and not matter in very uncompetitive districts, the relationship between the impact of information and political competitiveness will have an inverse-U shape.

H22 The effect of information will be greater the higher the incumbent is on their party's list (because the candidate is more viable).¹¹

¹⁰Prior preference here refers to warmth toward the incumbent rather than belief about candidate quality. This concept is distinct if we believe that preferences over incumbents can be motivated by considerations other than legislative performance.

¹¹While we state the hypothesis in this way, we find it plausible that we could discover the opposite relationships. The higher up an incumbent is on the party list, the more likely that incumbent is to win office, regardless of what the individual voter decides. This could depress the impact of information.

H23 Information will have a greater marginal effect on women than men.

H24 The marginal impact of information will be increasing in education level.

Incumbent Response

H25: Poor performing incumbents will react to the intervention by challenging the credibility of the information provided.

H26: Since the information will be available more publicly in the public provision condition, we should observe more backlash against the intervention in the common knowledge group.

H27: Incumbents in treated areas should invest more heavily in vote buying in an effort to counteract the negative impact of performance information.

H28: Poor performing incumbents in electorally competitive areas should be most likely to invest more heavily in vote buying.

5 Description of treatments

5.1 Manipulating information type

Common intervention

Previous studies suggest that relative performance information is superior to absolute performance information. Drawing on minutes and other documents from the legislative record (collected by a research assistant in November-December 2014), our baseline intervention will thus provide relative performance information about deputies’:

1. attendance at legislative sessions
2. active participation and speaking during legislative sessions

3. attendance/participation in committees

4. productivity of committee work

While we provide raw data for each of these four performance indicators, e.g. rate of attendance for legislative sessions, we try to synthesize the relative performance information so it is more easily digested and remembered by participants. Specifically, we create two performance indicators. The first, an index of plenary performance, takes 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, takes an average of the normalized scores on the second two indicators: attendance at committee meetings and productivity. Productivity is defined as the number of laws considered by the committee. To further synthesize the performance information, we produce a global performance index which averages scores from the above two indices.

This performance information will be provided orally as well as visually in the videos, using easy-to-read graphs and clear explanations of each performance indicator and its meaning. While constituencies have more than one deputy, each deputy is in practice in charge of a particular commune (and thus the villages nested within those communes). To ensure that the information provided is highly attributable to an individual incumbent, for each commune, we will produce a series of bar graphs to highlight the performance of the legislator responsible for that commune relative to other legislators in the department (a local average) and the country (national average).

As Benin does not employ a Constituency Development Fund or like program, and records of deputies' personal spending in constituencies are not kept, we rely on legislative performance information. We also use the first alternative information intervention (T2) to highlight further the consequences of legislative performance for citizen welfare and local public goods. Research in Ghana (Lindberg, 2003) suggests that MPs are frustrated by the extent to which voters hold them accountable for constituency service which impedes their ability to spend time on more publicly beneficial services such as legislative activity. Based on preliminary research, Benin deputies seem to be similarly interested in whether there are interventions that can shift voters' expectations in

this regard.

Civics message

While T1 (the common intervention) provides only information about relative legislative performance, T2 additionally provides a “civics message” explicitly stressing the implications of poor legislator performance for voter welfare. This treatment allows us to evaluate the extent to which legislator performance is conditional on the ex-ante expectations of citizens. Since Benin is a context in which voter expectations of legislative deputies are likely low, it provides an important opportunity to systematically vary expectations and assess heterogeneous treatment effects by level of ex-ante expectations. Data from initial focus groups are consistent our assumptions that voters have initially low expectations of legislative deputies. We will assess this assumption with data from the baseline survey.

The civics message (also conveyed in video format) first describes in detail the main responsibilities of legislative deputies, namely, their responsibility for legislation, executive oversight and representation. It then provides three concrete examples of how legislative performance (or lack thereof) can impact voter welfare. A positive example of good legislation is the passage of an anti-graft law requiring public servants to disclose assets. A negative example of a missed opportunity is 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 details how the legislature opposed changes to the constitution proposed by the president that would expand his power.

5.2 Manipulating the delivery mechanism

To the extent that voters in clientelist democracies face a choice between punishing poor incumbent performance or rewarding direct transfers of patronage from the incumbent, they have a coordination problem. If enough voters threaten sanctions for poor performance, the incumbent has an incentive to perform well; however, if insufficient voters choose to sanction, the incumbent will

prefer to provide private rather than public benefits. Those voters (or groups of voters) who sanction incumbents based on public provision risk punishment through the withholding of patronage. Relative to private knowledge, common knowledge can impact voter beliefs about how others will make decisions. The degree of perceived common knowledge generated by a particular information intervention can thus condition the impact of information. We use the focus groups and baseline survey to assess our assumption that voters in Benin believe their votes can be monitored and sanctioned, individually and as a group.

In order to study the implications of voter ability to coordinate, we vary the delivery mechanism of the information treatment. Information is either delivered in a public or private format. The public provision of information has the potential to be a bundled treatment, or manipulate several things at once. For instance, public delivery might not only make private information public, but it might also provide participants the opportunity to comment on information or deliberate amongst themselves. Such interaction might also increase comprehension levels among participants.

In order to isolate the specific mechanism of coordination, which is driven only by changes in other-regarding beliefs, we deliver the information treatment through a pre-recorded film. If the information were delivered live, the person delivering the information may be tempted to alter the tone or message as a function of the audience or context. Such heterogeneity across treatment reduces precision and may even bias outcomes. Further, the film puts distance between the provider and recipient of information, discouraging exchange and making communication among recipients of a public treatment easier to monitor and control. During training, enumerators are explicitly instructed to only answer clarification questions that reiterate information contained in the film and never to introduce opinions or new information. Finally, the delivery through film allows us to standardize a number of delivery aspects that would otherwise increase the noise to signal ratio.

Several manipulation checks test whether these alternative mechanisms drive changes in behavior. First, enumerators record instances of participant exchange – with the enumerator and with fellow participants. Second, local hearsay ethnographers are asked to record instances of deliberation among villagers in both public and private villages. Third, we ask a series of comprehension

questions of a random sample of participants in the “common knowledge” and “private information” villages to assess whether the public treatment increases comprehension of the information provided.

Common knowledge

In public (“common knowledge”) villages, the assigned information type will be provided through the screening of the information video in a public location. A random sample of 60 villagers (rather than the entire village) will be invited to the film screening, as an attempt to treat a representative sample of the village and to aid in holding the number of treated individuals across “private information” and “common knowledge” villages roughly constant. Enumerators responsible for screening the film will be trained by project staff on group management in the interest of monitoring and reducing public exchange among participants. Rather than disseminating information at the constituency level as did Humphreys and Weinstein (2012), we will do so at the lowest level of social organization – the village – to increase potential impact. The information dissemination will not be led or endorsed by a particular candidate or party as in Humphreys and Weinstein (2012) or Fujiwara and Wantchekon (2013), so as to reduce the possibility of elite capture and political spin. The public screenings will allow villagers to observe everyone else to whom the information is being provided.

Private information

In “private information” villages, the assigned information type will be provided to randomly sampled individual households in one-to-one interactions, rather than in a public venue. The same film shown at the public screening will be shown to individuals by enumerators via a smartphone in the privacy of their own home. Villagers may still communicate the information they have received to other households but they will have to undertake such coordination costs themselves, rather than having wide information dissemination performed for them as in common knowledge villages. In

addition, any communication by villagers does not confer the benefit of perceived changes in social norms or of common knowledge that occurs when information is delivered publicly to a group – only the latter method sets expectations about what others in the group know, and what others know they know. It is our view that this type of information is more likely to solve the coordination problem than *ad hoc* private communication within the village.

5.3 Manipulating dosage

Treatment dosage is manipulated at the commune level. Half of communes in the sample are high-dosage with 12 treated villages each. Half of the communes in the sample are low-dosage with 1 treated village each. The only difference in the actual delivery of information is that participants in both public and private conditions are informed, prior to the showing of the film, of the number of other villages in their commune that receive treatment. This same information is repeated at the end of the film as a reminder.

5.4 Survey sample

To measure voter priors about politician performance, other conditioning variables, and administer the private treatment, we will conduct a survey with 6,000 respondents in March-April 2015. The baseline survey and recruitment to the public screenings will occur at the same time (with the screening taking place immediately after the survey is completed in those villages). For the purposes of power, we only require 20 survey respondents in each village. However, we aim for a larger impact of the intervention on the community by recruiting 60 individuals to the public screenings.

Our prior is that two-thirds, or 40 invitees, will attend the public screening. To maximally harmonize public and private treatments, we endeavor to treat the same numbers of individuals per village across conditions. Thus, we provide the private treatment to 40 individuals in each private village even though we only survey a random half of those. One potential problem with delivering

private information is that recipients may have incorrect beliefs about who else is receiving the information. If, for example, private information respondents believe that everyone in the village receives treatment, this would undermine the differentiation in other-regarding beliefs across treatment groups. Therefore, recipients of the private information treatment will be told, “You are one of a small number of individuals from this village who have been selected to participate in this study.”

The clustered nature of our sampling strategy significantly limits our statistical power. We will thus also survey a random sample of “control” individuals in each private treatment village enabling us to compare treated and control individuals within private villages without clustering on standard errors. Weighing budget and time constraints, we randomly sample 10 individuals in private treatment villages to receive only a survey. These will then be compared to the 20 randomly sampled individuals who receive a survey and the private intervention. This increase in power will be particularly useful for comparing the relative impacts of the two information types (with and without a civics message) within private delivery villages. It will also allow us to estimate spillovers of the private treatment arm by comparing “control” individuals in private treatment villages to individuals in control villages.

6 Ethical Considerations

This experiment has received approval from four United States university Institutional Review Boards: UCSD, Texas A&M, George Washington University, and Harvard University. In addition, we have sought approval from the National Statistics Office of Benin (Le Conseil National de la Statistique et de l’Analyse économique) and from the President of Parliament. To guard against enumerators running into any interference in the field, we distribute a copy of the permission letter from the President of Parliament to each. The experiment is being implemented by a Beninois non-governmental organization with deep ties to the country and deep knowledge of the political context.

In designing the experiment and in seeking permissions, we gave particular weight to the following ethical considerations as well as to those in the EGAP meta-pre-analysis plan:

Informed Consent: We ensured that the experimental protocol requires informed consent from all study subjects—that is, from all voters surveyed, provided with the private interventions, and invited to the public screenings. Participation in both the baseline and the endline surveys is entirely voluntary and can be terminated by the respondents at any time. Respondents are asked to indicate consent to participate before completing each survey. Respondents in the T3 condition are given performance information during administration of the survey after they have consented to participation in the study. Respondents in the T4 conditions are invited to the public screening during the survey after they have consented to participation in the study and are told that attendance at the public screening is voluntary.

Minimizing Risks to Subjects: We designed the survey questions to focus on non-sensitive information. Data will be collected and recorded so as to remain confidential. We also designed certain questions that might be more sensitive (e.g. reported vote choice) so that respondents could report their answers in relative privacy. For instance, rather than reporting the name of the party out loud, respondents in the endline survey are given the opportunity simply to say yes or no to a party name stated by the enumerator on the other end of the phone line. All respondents will be compensated for their time participating in the study at rates calibrated to average daily wages in Benin. Approvals from local chiefs or other applicable elders will be sought first in each village or neighborhood unit before conducting the surveys and public screenings.

Minimizing Risks to Other Affected Parties: We decided to ask for permission from the President of Parliament before conducting the experiment because that office is in a position to evaluate the risks of the study to politicians and parties and to society more generally. Politicians are not themselves subjects of the experiment, but the parties' vote shares might be affected by the experiment depending on its results. To err on the side of incorporating risks to the politicians themselves in the approval of the study, we sought Parliamentary review. We did not, however, seek consent from each and every individual legislator. Since the purpose of the study is to evaluate whether

information interventions can help voters hold their representatives more accountable, we viewed it as a disservice to voters and to Beninois society more generally to allow poorly performing politicians, for instance, to act as veto players over the study.

Eliminating the Possibility of Deception: We designed the interventions to consist of objective, uncontroversial and reliable performance measures. We rely on data collected by the President of Parliament so as not to introduce information out of context. We are confident that the interventions introduce voters to important, non-deceptive, and context-appropriate information.

7 Measurement instruments

7.1 Surveys

We will conduct two surveys: a baseline in-person survey prior to intervention and a phone survey directly after the election among the same respondents. The draft questions for both surveys are included in the Appendix. The identities of the respondents will be re-confirmed in the endline survey by asking again for respondents' first names and ages. We allow for a possible 50 percent attrition rate between surveys. To discourage attrition, one-third of total compensation per respondent will be transferred as phone credit only after completion of the endline survey.

Table 2 summarizes the links between our hypotheses and specific survey items. For the assessment of our main hypotheses (H1-H4), we privilege the vote choice outcomes (measured at the individual-level through survey data and at the aggregate level through electoral returns) but we will also collect endline measures of perceptions of incumbent traits (effort, honesty) and of strength of attachment to the party for which respondents voted. We plan to examine these outcomes as well.

When there are several indicators that measure the same construct, we will use an equally-weighted mean effects index as appropriate. We will use the raw data to first determine whether indicators belong in the same index. And we will analyze data from survey experiments separately

from other question data since comparisons can only be made across experimental conditions rather than across individuals.

Table 2: Summary of Hypotheses and Relevant Variables

<i>Hypothesis</i>	<i>Outcome Variable</i>	<i>Operationalization</i>
H0a	Value Placed on Constituency Service Others' Valuing Constituency Service	Bq21, Bq39, Bq40-Bq42 Bq38
H0b	Perception of Bloc Voting Rewards	Bq44, Bq45
H0c	Comprehension of Treatments	Bq47-Bq54
H1-H4	Individual Vote Choice Village-level Vote Choice Candidate/Party Evaluation	Eq5 Incumbent Vote Share Eq9, Eq14-Eq21
H8, H15	Village-level Turnout Individual Turnout	% Registered Voters Voted Eq3, Eq4
H5	Posterior Value of Legislative Performance Information	Eq11, Eq6
H9	Others' Value on Legislative Activity	Eq12 ¹²
H10	Ability to Coordinate	Eq13 ¹³
	<i>Conditioning Variable</i>	<i>Operationalization</i>
H6	Prior Value of Legislative Performance	Bq39
H7	Dosage Level	Indicator for high- or low-dosage
H11	Rural	Bq59
H12	Beliefs about Vote Anonymity	Eq22
H13	Village Ethnic Homogeneity	Ethnic Fractionalization
H14	Opinion Leader Presence	Bq16
H16	Prior Beliefs about Performance	Bq30-Bq37
H17	Prior Preferences for Incumbent	Bq18 ¹⁴ , Bq24, Bq10, Bq25, Bq28
H18	Clientelism	Bq28
H19	Ethnic Attachment	Bq11
H20	Village Information Access	Radio Question in Enumerator Survey
H21	Electoral Competition	2011 Competitiveness
H22	List Position	List Position
H23	Female	Bq4
H24	Education	Bq7
H25:-H26:	Parties' Reactions to CEPRODE	Eq30, Eq29
H27:-H28:	Vote Buying	Eq37, Eq38

¹²We are incentivizing this question with a small remuneration for best guess to encourage thoughtful and honest answers from respondents.

¹³Ibid.

¹⁴This indicator is multiplied by -1 for respondents whose party ID is not the same as that of the incumbent deputy's and takes a value of 0 if the respondent answers "Don't know".

7.2 Village election results

To measure the effect of the treatments on aggregate outcomes, we will collect data on turnout and party vote shares at the polling station level, to be aggregated to the village level. Combining official polling station voter registration information from the Electoral Commission with the data we collect at the polling stations, we will also examine how parties with poorly performing politicians fare in treated constituencies relative to untreated and how parties with well-performing politicians fare in treated constituencies relative to untreated. These aggregate level data will allow us further to assess the main hypotheses and the conditional hypotheses (summarized in Table 2).

In order to evaluate our conditional hypotheses based on local context, we also collect data on the 2011 legislative elections, the ethnic make-up of villages, and the list position of candidates. We ask enumerators to report access to radio stations in each village (which they can access from their smartphones) in order to judge the quality of the information environment.

7.3 Qualitative Measurement

We also intend to integrate qualitative measurement into our research design. In a randomly selected subset of villages, we will gather qualitative data through “hearsay ethnography” (Watkins and Swidler, 2009). Developed to study the impact of health interventions (Watkins, Swidler and Biruk, 2011), hearsay ethnography involves the employment of local informants who keep journals. When possible, two local residents (one man, one woman) will be hired in a random subset of villages to write journal entries documenting public conversations they hear and recording events and activities about politics and the election campaign in their community. These entries will allow us to learn about the extent to which people in the community are talking about the information intervention, the ways in which people interpret the intervention and coordinate around it (or do not), and potentially the ways that local political party activists respond to the intervention. These data will help us get inside the “black box” and better understand the mechanisms driving our results. They will also provide an unusually rich window into the everyday politics of political campaigns.

8 Estimation

Before presenting formal hypothesis tests, we will first present the means and standard deviations of incumbent voteshare and voter turnout in each cell of our experimental design (presented in Table 1). We will present this information using official village-level election results and our own survey data.

8.1 Baseline Specifications

We present baseline specifications for our analysis of survey data and official elections results data. While we analyze both, we privilege the survey data for the following reasons. First, we are able to treat only a certain number of individuals and compounds in each village and, while these numbers sometimes constitute significant portions of total village populations, our strategy may not be enough to change aggregate results. Second, our theory is fundamentally an individual-level theory and so we plan to privilege findings at the individual-level from the survey data, if there is a conflict.¹⁵

Survey Data

Our hypothesis tests require a definition of positive and negative information. With our survey data, we will code individuals as having received good or bad news as follows. First, we define P_{ik} as individual i in commune k 's prior belief about the incumbents legislative performance. We define Q_k as the information provided about the incumbent in the intervention. We measure P_{ik} in the survey on the same 4-point scale in which Q_k is presented in the intervention (much worse, a little bit worse, a little better, or much better than the other deputies in the departement). We next define a dummy variable, G , which takes a value of 1 if $Q_k > P_{ik}$ or $Q_k = P_{ik}$ and $Q_k > \bar{Q}_d$, where d

¹⁵The plan to privilege the survey results is, however, dependent on a successful survey in which, for instance, we achieve minimal attrition and see little evidence of response bias (e.g. using the turnout validation question). In cases of a survey with these kinds of problems, the aggregate vote results may be privileged.

denotes the departement and \bar{Q}_d is the departemental average. Subjects with $G = 1$ have received positive information. The remaining subjects have received negative information ($G = 0$).

To leverage the statistical power that comes from individual-level randomization in our private (T3) condition, we conduct two sets of analyses. One includes only villages/quartiers in the private (T3) condition. The other includes all 255 villages/quartiers.

The T3 specifications are as follows:

$$E(Y_{ij}|G = 1, T3 = 1) = \beta_0 + \beta_1 T1_{ij} + \beta_2 T2_{ij} + \mu_j \quad (1)$$

$$E(Y_{ij}|G = 0, T3 = 1) = \lambda_0 + \lambda_1 T1_{ij} + \lambda_2 T2_{ij} + \mu_j \quad (2)$$

Y_{ij} takes a value of one if voter i in village j voted for the incumbent, and 0 if not. μ_j are block fixed effects. β_1 and β_2 give the average effect of T1 and T2 for those who received positive information in the private condition, while λ_1 and λ_2 estimate the same quantities for those who received negative information.

The full sample specifications are as follows:

$$E(Y_{ij}|G = 1) = \beta_0 + \beta_1 T1_{ij} + \beta_2 T2_{ij} + \beta_3 T3_{ij} + \beta_4 T4_{ij} + \gamma_1 C_{ij}^{T1} + \gamma_2 C_{ij}^{T2} + \mu_j \quad (3)$$

$$E(Y_{ij}|G = 0) = \lambda_0 + \lambda_1 T1_{ij} + \lambda_2 T2_{ij} + \lambda_3 T3_{ij} + \lambda_4 T4_{ij} + \theta_1 C_{ij}^{T1} + \theta_2 C_{ij}^{T2} + \mu_j \quad (4)$$

The β s and the λ s are the average effects of each treatment for those who receive positive and negative information, respectively. C_{ij}^{T1} and C_{ij}^{T2} are dummies indicating whether the subject is a control subject in the T1 or T2 conditions. The reference category is the control subjects in control villages. The γ s and the θ s estimate the within-T3-village spillover effects on the non-treated in

both T1 and T2. In these models, we cluster standard errors by village since treatments in the public (T4) condition are assigned at that level.

Official Elections Results

We will also analyze official elections results data from each village/quartier in all 30 of our sample communes. When we analyze these data, we are unable to use information on voter priors. Instead, we assume — and will seek to validate with our baseline survey — that most voters are highly uncertain in their beliefs about incumbent legislative performance (which we believe is very valid in our context). This assumption allows us to use Q_k to define positive and negative information. We define positive information as $Q_k > \bar{Q}_d$, where \bar{Q}_d is the departemental average. Negative information is defined as $Q_k < \bar{Q}_d$. We then estimate average effects as follows:

$$E(Y_j|Q_k > \bar{Q}_d) = \beta_0 + \beta_1 T1_j + \beta_2 T2_j + \beta_3 T3_j + \beta_4 T4_j + \mu_j \quad (5)$$

$$E(Y_j|Q_k < \bar{Q}_d) = \lambda_0 + \lambda_1 T1_j + \lambda_2 T2_j + \lambda_3 T3_j + \lambda_4 T4_j + \mu_j \quad (6)$$

Here, Y_j is the voteshare of the incumbent’s party in village j , and the other parameters correspond to those above.

Since we use all non-treated villages in sample communes as control units, villages are assigned to treatment and control with different probabilities.¹⁶ Probabilities of assignment to treatment differ because of the varying size of communes: we always treat 12 localities per commune which may be slightly above or below a quarter of total localities. Furthermore, the three blocks we use for within-commune stratification — urban, rural/competitive, and rural/non-competitive — are of unequal sizes, so villages are assigned to treatment and control across blocks with differ-

¹⁶Within each block, villages are assigned to one of the four treatment groups with probability of about $\frac{1}{16}$ (probability of assignment to one of the four treatment condition is about $\frac{1}{4}$) and to the control group with probability of about $\frac{3}{4}$. This difference is due to the fact that we oversample control villages to strengthen statistical power.

ent probabilities. To account for these differences, we use inverse probability weighted regressions for these specifications.

Since we are treating 40 people per unit (village or quartier), it is possible that the effect of treatment will vary by village size. For example, effects on official results might be stronger in smaller places where we are treating a higher proportion of potential voters. As a secondary test, we will also therefore examine whether treatment effects vary by unit size.

To do so, we create a set of dummy variables indicating the size of the village/quartier. We create 4 dummies, which will correspond to each quartile of the distribution of village/quartier population (smallest, small, big, biggest). We then interact these dummies with each of the treatment indicators. We also include each dummy in the model. We select this approach in order to avoid imposing functional form assumptions on the relationship between village/quartier size and the effects of each treatment (as would be the case if we interacted the linear measure of village/quartier population with each treatment indicator).

8.2 Estimating Dosage Effects

We also examine whether treatment effects vary in the high and low dose communes. To do so we first define a dummy variable, T_j , which takes a value of 1 if village j is in *any* of the treatment conditions, and 0 if the village is in the control group. We also define a dummy variable, S_k , which takes a value of 1 if the commune is a high dose commune, and 0 otherwise. We will estimate the following model:

$$Y_{jk} = \alpha + \theta_1 T_{jk} + \theta_2 S_{jk} + \theta_3 T_{jk} S_k + X_i' \Gamma + \varepsilon_{ij} \quad (7)$$

We will estimate this model in the full sample to estimate average effects. We will also estimate the model in the positive information ($G=1$) and negative information ($G=0$) sub-samples. Because we are seeking to identify the effect of dosage, which is assigned at the commune level,

we cluster standard errors by commune. Because we want to identify the effect of S_j , we do not include commune fixed effects in this model, only controls for urban/rural and electoral competitiveness (village-level factors).

θ_3 is the coefficient of interest. It indicates whether treatment effects are attenuated or amplified by dosage in the commune. θ_2 indicates the extent to which *control* villages in high dosage areas are impacted by the treatment of villages in the same commune. If θ_2 is different from zero, this suggests that treatment has spillover effects onto non-treated villages within the same commune.

8.3 Tests of Conditional Hypotheses

We test for heterogenous effects using interaction analysis. For each covariate, X_{ij} (if individual level) and X_j if village level, we estimate the following:

Private condition only:

$$E(Y_{ij}|G = 1, T3 = 1) = \beta_0 + \beta_1 T1_{ij} + \beta_2 T2_{ij} + \beta_3 T1_{ij}X_{ij} + \beta_4 T2X_{ij} + \beta_5 X_{ij} + \mu_j \quad (8)$$

$$E(Y_{ij}|G = 0, T3 = 1) = \lambda_0 + \lambda_1 T1_{ij} + \lambda_2 T2_{ij} + \lambda_3 T1_{ij}X_{ij} + \lambda_4 T2X_{ij} + \lambda_5 X_{ij} + \mu_j \quad (9)$$

We take the same approach as above when introducing estimates of heterogenous effects in Equations 3, 4, 5, and 6. Table 2 specifies how we construct each X_{ij} and X_j and links these measures to each conditional hypothesis.

8.4 Covariate Adjustment

We will run all specifications first without control variables other than the block fixed effects. However, we expect several individual- and village-level variables to correlate with response to

treatment. To improve precision of estimates, we will also run specifications with appropriate controls. We will also present balance tables of control variables across the treatment arms.

Individual-level control variables include all those used in conditional hypotheses – gender, education level, ethnic identity – as well as age and poverty. We will additionally control for whether the individual was identified by other participants as an opinion leader.

Village-level control variables include those used in conditional hypotheses – number of information sources, ethnic homogeneity, perception of anonymity, and density of opinion leaders receiving treatment. Because diversity of views among opinion leaders may also make participants more accepting of new information, we will additionally control for fractionalization of opinion leaders or the sum of squared shares of respondents mentioning each opinion leader.

In addition to covariate adjustment, we will also check that our results are robust to the timing/order of the intervention in each village and enumerator fixed effects.

8.5 Multiple Testing Correction

Within each of the three main theories, we are interested in testing secondary outcomes and conditional effects that are relevant only to one of theories. We will first present uncorrected p -values for all tests. We will then use the results of each family of tests to assess the overall evidence in favor of a particular theory. Thus, for each family of hypotheses nested within one of the main theories, we will adjust p -values counting the number of multiple comparisons within that family of hypotheses only. For these hypothesis tests, we will present both nominal p -values and corrected p -values.

In this case, following Adida et. al (2015),¹⁷ we will correct p -values using a false discovery rate (FDR) correction to control the Type-1 error rate.¹⁸ We will control the FDR at level 0.05. Thus, for a given randomization with m (null) hypotheses and m associated p -values, we order the realized nominal p -values from smallest to largest, $p_{(1)} \leq p_{(2)} \leq \dots \leq p_{(m)}$. Let

¹⁷“Political Information and Electoral Choices: A Pre-meta-analysis Plan.”

¹⁸See Benjamini and Hochberg (1995).

k be the largest i for which $p_{(i)} \leq \frac{i}{m}0.05$.

Then, we reject all $H_{(i)}$ for $i = 1, 2, \dots, k$, where $H_{(i)}$ is the null hypothesis corresponding to $p_{(i)}$. The number of multiple comparisons for each theory is derived directly from our section on hypotheses. They are as follows¹⁹:

- Theory 0: Effect of Information (11 hypotheses on voter behavior + 4 hypotheses on incumbent response)
- Theory 1: Salience (4 hypotheses)
- Theory 2: Common Knowledge and Coordination (10 hypotheses)

8.6 Contingencies

Attrition

We will test for two forms of attrition in our survey sample. First, we will determine whether rates of attrition vary across conditions. Second, we determine whether the predictors of attrition differ across groups. We will do the first by comparing mean attrition in treatment and control groups. We do the second by interacting the treatment indicators with a set of baseline covariates and giving the F-statistic for all of the interacted variables.

If we find no problematic attrition, we will proceed without corrections. If we observe differential attrition, we will use bounding techniques (such as Lee or Manski bounds) to provide estimates of the magnitude of bias that could have resulted from differential attrition. We will also test whether the main results are robust to the observed rate of differential attrition.

Non-compliance

We will present Intention-to-Treat Estimates (ITT).

¹⁹Hypothesis 4 on the interaction between Theory 1 and 2 is counted in both of the two families of theories.

9 Power analysis

9.1 Village-level analysis

Building upon Wantchekon (2003), we employed the following parameters in our analysis of the impact of information on vote share:

- Mean: 0.74
- Standard deviation: 0.25
- Treatment effect size: 0.1

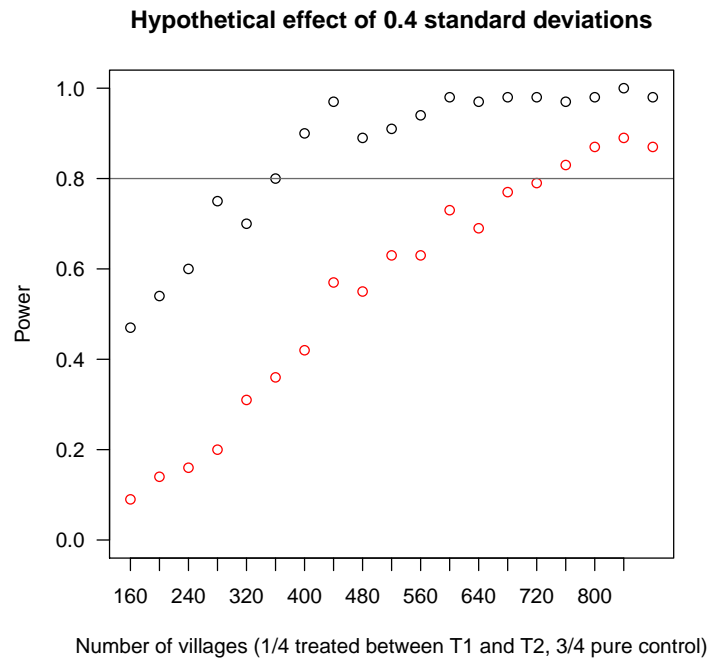
To conserve power, we employ a factorial design for our 4 treatment arms such that each treatment effect can be estimated using half of the treated sample. Thus, the following power calculations assume two treatment arms. Because it is cheaper to collect data in control villages, we test for a sample size in which 1/4 of the villages are assigned to the two treatment arms and 3/4 are assigned to control.

To estimate power in this factorial design, we consider the power necessary to test whether both treatment arms are significant (**red** in the below graph), a more stringent requirement than asking whether one of the two is significant (**black** in the below graph). Figure 1 shows that for a power of 0.8 and a statistical significance level of 0.05 (two-tailed test), we would need 720 villages total, or 180 treated villages and 540 control. Because of the factorial design, we would assign 45 villages to each of the 4 treatment arms.

9.2 Individual-level analysis

The power analysis at the village level is more restrictive than the individual as long as intraclass correlation is less than 1. Here, we examine whether using the 45 treatment villages per individual

Figure 1: Power analysis at the village level



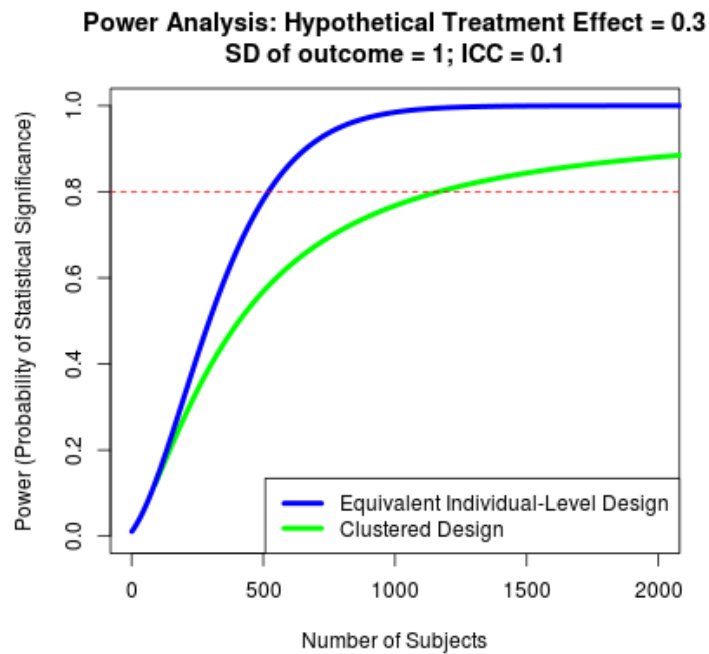
arm within high-dosage communes, we can detect effects in each of the four separate treatment arms rather than the two 90-village mixed treatment arms that we get from the factorial design.

In the below power calculation, to be highly conservative, we made the Bonferroni correction for multiple hypothesis testing by dividing alpha by 4. We then assumed a slightly smaller effect size of 0.3 and an intraclass correlation of 0.1 following treatment effect sizes on household surveys in villages in Gottlieb (2013). As indicated in Figure 2, a comparison between a treatment arm with 45 villages and an equally sized control group, requires a cluster size of 1165 or an average of 13 individuals per cluster. We intend to survey at least 20 households at baseline, so this allows for attrition of more than 1/3 of the sample between baseline and endline.

10 Timeline

Oct-Dec 2014: Legislative performance information collected; baseline survey and interventions planned; focus groups conducted as part of design of interventions. IRB approvals obtained at

Figure 2: Power analysis at the individual level



authors' respective institutions. Local approval in progress.

Jan-February 2015: Baseline survey and intervention text piloted; intervention video produced.

Early March 2015: Enumerators trained.

Mid-March to mid-April 2015: Baseline survey and intervention implemented; qualitative data collection.

April 26, 2015: Legislative elections.

April 27-Early May 2015: Post-election phone survey conducted; election returns collected.

June-Dec 2015: Data analysis and write-up.

References

- Azulai, Michel, Oriana Bandiera, Florian Blum, Henrik Kleven, Eliana La Ferrara, Gerard Padró and Catalina Michelle Tejada. 2014. State Effectiveness, Growth, and Development. Technical report International Growth Centre.
- Banerjee, Abhijit, Selvan Kumar, Rohini Pande and Felix Su. 2011. “Do informed voters make better choices? Experimental evidence from urban India.” *Unpublished manuscript* .
URL: <http://www.povertyactionlab.org/node/2764>
- Bobonis, Gustavo J, Luis R Cámara Fuertes and Rainer Schwabe. 2012. The dynamic effects of information on political corruption: Theory and evidence from Puerto Rico. Technical report Working Papers, Banco de México.
- Bratton, Michael and Mwangi S. Kimenyi. 2008. *Voting in Kenya: putting ethnicity in perspective*. University of Connecticut Department of Economics Working Paper 2008-09.
- Chong, Alberto, O De La, L Ana, Dean Karlan and Leonard Wantchekon. 2011. Looking beyond the incumbent: The effects of exposing corruption on electoral outcomes. Technical Report 17679 National Bureau of Economic Research.
- Dewan, Torun, Macartan Humphreys and Daniel Rubenson. 2013. “The Elements of Political Persuasion: Content, Charisma, and Cue.” *The Economic Journal* 124:257–292.
- Ferraz, Claudio and Frederico Finan. 2008. “Exposing corrupt politicians: The effects of Brazil’s publicly released audits on electoral outcomes.” *The Quarterly Journal of Economics* 123(2):703–745.
- Fujiwara, Thomas and Leonard Wantchekon. 2013. “Can informed public deliberation overcome clientelism? Experimental evidence from Benin.” *American Economic Journal: Applied Economics* 5(4):241–255.
- Gordon, Grant, Macartan Humphreys and Jeremy Weinstein. 2013. “Transparency and Spin: How Political Communication Reinforces (or Undermines) Political Accountability.” *EGAP Pre-analysis Plan* .
- Gottlieb, Jessica. 2013. Peasants into democrats: Evaluating the determinants of democratic failure in Mali PhD thesis Stanford University.
- Habyarimana, James, Macartan Humphreys, Daniel N. Posner and Jeremy M. Weinstein. 2007. “Why does ethnic diversity undermine public goods provision?” *American Political Science Review* 101(4).
- Harding, Robin. 2010. Urban-Rural differences in support for incumbents across Africa. Working paper no. 120 Afrobarometer.
- Hounkpe, Mathias and Shana Warren. 2012. ALP Benin interim country report. Technical report African Legislatures Project.

- Humphreys, Macartan and Jeremy Weinstein. 2012. "Policing Politicians: Citizen Empowerment and Political Accountability in Uganda Preliminary Analysis." *Columbia and Stanford Universities: Working Paper* .
- Keefer, Philip and Stuti Khemani. 2014. "Radio's impact on preferences for patronage benefits." *World Bank Policy Research Working Paper* (6932).
- Koter, Dominika. 2013. "Urban and rural voting patterns in Senegal: the spatial aspects of incumbency, c. 1978-2012." *The Journal of Modern African Studies* 51(4):653–679.
- Lindberg, Staffan. 2003. "'It's Our Time to Chop'": Do Elections in Africa Feed Neo-Patrimonialism rather than Counter-Act It?" *Democratization* 10(2):121–140.
- Magaloni, Beatriz. 2006. *Voting for Autocracy: Hegemonic Party Survival and its Demise in Mexico*. New York: Cambridge University Press.
- Pande, Rohini. 2011. "Can informed voters enforce better governance? Experiments in low-income democracies." *Annual Review of Economics* 3(1):215–237.
- Smith, Alastair and Bruce Bueno De Mesquita. 2012. "Contingent prize allocation and pivotal voting." *British Journal of Political Science* 42(02):371–392.
- Stokes, Susan C. 2005. "Perverse accountability: A formal model of machine politics with evidence from Argentina." *American Political Science Review* 99(03):315–325.
- Verba, S., Schlozman, K. L., and Brady, H. 1995. *Voice and equality: civic voluntarism in American politics*. Cambridge: Harvard University Press.
- Wantchekon, Leonard. 2003. "Clientelism and voting behavior: Evidence from a field experiment in Benin." *World Politics* 55(3):399–422.
- Watkins, Susan Cotts and Ann Swidler. 2009. "Hearsay ethnography: Conversational journals as a method for studying culture in action." *Poetics* 37(2):162–184.
- Watkins, Susan Cotts, Ann Swidler and Crystal Biruk. 2011. Hearsay ethnography: A method for learning about responses to health interventions. In *Handbook of the Sociology of Health, Illness, and Healing*. Springer pp. 431–445.

Appendix

Baseline Survey (In-Person)

Bq1 Village/Quartier Name:

Bq2 Interview Start Time:

Bq3 What is your first name and phone number? [If respondent does not have own phone, ask for best number at which to reach them and ask if this is the respondent's number or that of another person as well as the identity of that other person if applicable.]

Bq4 What is your sex?

- Male
- Female

Bq5 How old are you? [used to calculate birth year]

Bq6 Which language do you speak at home?

- French
- Fon
- Adja
- Barber
- Dendi
- Yoruba
- Nago
- Otamari
- Peulh

- Yoa
- Boo

Bq7 How many years of schooling have you completed? [If respondents says, completed primary school, enter 6; if respondent says completed secondary school, enter 13 (10 for completed junior high school, 13 for both junior and senior high school; if respondent says completed tertiary education, enter 16; if respondent says completed Master's degree, enter 20.]

Bq8 Were most of these years in a Koranic school?

- Yes.
- No.

Bq9 In general, how do you rate your living conditions compared to those of other Beninois?

Would you say they are:

- Much worse
- worse
- the same
- better
- much better

Bq10 Which is your ethnic group?

- Bariba
- Fon
- Adja
- Dendi
- Yoruba
- Nago

- Ditamari
- Peulh
- Yoa
- DK/Refused
- Other:

Bq11 Let us suppose you had to choose between being Beninois and being [the identity of the respondent's ethnic group]. Which of the following statements best expresses your feelings?

- I feel only Beninois
- I feel more Beninois than (respondent's ethnic group)
- I feel equally Beninois and (respondent's ethnic group)
- I feel more (respondent's ethnic group) than Beninois
- I feel only (respondent's ethnic group)

Bq12 How many people reside in your concession (compound)?

Bq13 Are you an active member of the following organizations? By "active member", we mean that you attend most or all meetings or that you hold a leadership position within the organization. (Choose all that apply.)

- The (village or quartier) chief's council
- A professional association
- A women's association
- A youth association
- A political association

Bq14 What is the news source you use most frequently to learn about politics?

- Radio
- Newspaper
- Television
- Word of Mouth
- Other (specific outlet if mentioned):

Bq15 When you are with family and friends, how often do you talk about political issues?

- Never
- Sometimes
- Frequently

Bq16 Outside of the people living in your compound, who is the person or persons in this village/quartier whose opinion you most value on political matters? (record names, relationships to respondent and positions)

Bq17 If the upcoming legislative elections were held today, which party would you vote for?

Bq18 [For the party mentioned in *Bq17*]: On this scale of one to seven, where seven means you are very attached to [RESPONDENT'S PARTY], and one means you are not very attached to [RESPONDENT'S PARTY], what degree of attachment do you feel for [RESPONDENT'S PARTY]?

Bq19 Did you vote in the last national assembly elections in 2011?

- Yes.
- No. If no, skip to *Bq21*.

Bq20 If yes, which party did you vote for?

- FCBE

- UN
- Amana Alliance
- G13 Baobab
- AFU
- Cauris 2 Alliance
- UPR
- UB

Bq21 Imagine the following incumbent candidate is on the list of a legislative party in the National Assembly elections next month. Originally from a neighboring village/quartier, this candidate is, say, 49 years old. He finished secondary school in the department capital and then obtained a Masters in Law. [He has sponsored community activities in this village/been an active participant in the National Assembly in Porto Novo.] He has already been in the National Assembly for 4 years and wants to extend his mandate. On a scale of 1-10 (show ladder), how likely would you be to vote for this person's party in the upcoming legislative elections? [Randomly assigned to 1 of : control, constituency service attribute, or legislative performance]

Bq22 How likely do you think it is that powerful people can find out how you vote, even though there is supposed to be a secret ballot in this country?

- Not at all likely
- Not very likely
- Somewhat likely
- Very likely

Bq23 Which national deputy is most responsible specifically for this village/quartier? [FILL IN NAME, OR DK]

Bq24 [If does not mention [NAME OF PRINCIPAL DEPUTY] in *Bq23*]: According to our sources [NAME] is the deputy primarily responsible for this village/quartier.] Is [NAME] originally from this village/quartier?

- Yes.
- No.

Bq25 Thinking of the [NAME OF PRINCIPAL DEPUTY], would you say that you share the same ethnic group as this candidate?

- Yes.
- No.
- Don't know/refused.

Bq26 Now I am going to ask you some more questions about [NAME OF PRINCIPAL DEPUTY]. How much time does [NAME OF PRINCIPAL DEPUTY] spend in this village/quartier each month? Does he/she come:

- Once a week
- Once a month
- Once every few months
- Once a year
- When there is an election
- Not at all
- Don't know

Bq27 On which of the following activities do you think [NAME OF PRINCIPAL DEPUTY] spends the most time:

- Helping individual people solve their problems

- Providing public goods in villages/quartiers—schools, roads, clinics
- Making national policies and laws
- Overseeing the actions of the President
- Other:
- They don't much of anything (except maybe visit villages during election time).
- Don't know.

Bq28 Has [DEPUTY NAME] or someone from their party ever provided assistance to you or someone in your family in an emergency or when you had a personal problem?

- Yes
- No

Bq29 How likely is it that the [DEPUTY NAME], or someone from their party, will offer something, like food, or a gift, or money, in return for votes in the upcoming election?

- Not at all likely
- Not very likely
- Somewhat likely
- Very likely

Bq30 In the National Assembly, the deputies' job is to attend plenary sessions and to ask questions. [NAME OF PRINCIPAL DEPUTY], does he participate much more, a little more, a little less or much less than other deputies in this Department?

- Much more
- A little more
- A little less
- Much less

Bq31 How certain are you about your response to this question?

- Very certain
- Certain
- Not certain
- Very uncertain

Bq32 Deputies can also work on legislative committees by attending meetings and dealing with laws. [NAME OF PRINCIPAL DEPUTY], does he work in committees much more, a little more, a little less, or much less than other deputies in this Department?

- Much more
- A little more
- A little less
- Much less

Bq33 How certain are you about your response to this question?

- Very certain
- Certain
- Not certain
- Very uncertain

Bq34 Now, we have mentioned that deputies participate in plenary sessions and participate in legislative committees. Considering all of these activities together, do you think [NAME OF PRINCIPAL DEPUTY] does a little more, a little less, or much less than other deputies in this Department?

- Much more
- A little more

- A little less
- Much less

Bq35 How certain are you about your response to this question?

- Very certain
- Certain
- Not certain
- Very uncertain

Bq36 In your opinion, does [NAME OF PRINCIPAL DEPUTY] make much more, a little more, a little less or much less effort to get things done than other deputies in this Department?

- Much more
- A little more
- A little less
- Much less

Bq37 How surprised would you be to hear from a credible source about corruption involving your national deputy? Would you say you would be

- Very surprised
- Somewhat surprised
- Not too surprised
- Not surprised at all

Bq38 Imagine the following incumbent candidate is on the list of a legislative party in the National Assembly elections next month. He has, say, a Masters in Law. [He has sponsored many community activities in this village/He has been an active participant in the National Assembly in Porto Novo.] In your opinion, approximately what percentage of *others* in this

village do you think would be likely to vote for this candidate? [Randomly assign 1 out of 3 conditions: control, constituency service, legislative performance]

Bq39 Your legislative deputy spends time doing two main things: bringing goods and services to your community and making laws in Porto Novo. Compared with bringing goods to your community, do you think making laws in Porto Novo is:

- Much less important.
- A little less important.
- A little more important.
- Much more important.

Bq40 I am going to read you a list of activities in which your national deputy could be involved. Suppose you could receive information about one of these things. I'd like to ask you to tell me about which of these activities you would most like to receive information:

- (a) How well the politician performs his/her duties in the national legislature, for example, attendance in plenary sessions and committee meetings,
- (b) Whether the politician has been engaged in corruption,
- (c) Whether the politician has been accused of committing a crime,
- (d) Whether the politician is effective at delivering services and bringing benefits to this community

Bq41 Now, thinking of the previous question, please tell me a second activity about which you would like to receive information about your national deputy [read three options not previously chosen]

Bq42 Now, thinking of the previous question, please tell me a third activity about which you would like to receive information about your national deputy. [read two options not previously chosen]

Bq43 Suppose that you received information about a politician's performance in office. Which of the following sources would you trust the most [second most; third most] for that information? [READ OPTIONS]

- Local politician
- Flyer or pamphlet or presentation from an NGO
- Face-to-face contact with a person conducting a survey
- Face-to-face contact with an influential member of your community
- In a debate between candidates
- Other

Bq44 In your view, when a lot of people in this village/quarter vote for one particular party, do the legislators from that party know that this village/quarter was very supportive?

- Yes.
- No.

Bq45 If yes, do the legislators thank this village/quarter by giving it more money than other villages?

- Yes.
- No.

Bq46 How likely do you think it is that the counting of votes in the upcoming National Assembly election will be fair?

- Not at all likely.
- Not very likely.
- Somewhat likely.

- Very likely.
- Don't know.

After INTERVENTION, in random sample of participants in T1 and T2:

Bq47 Who is the national deputy most responsible for this village/quarter?

Bq48 To which parties does (he/she) belong?

Bq49 [NAME OF PRINCIPAL DEPUTY], does he participate in plenary sessions of the National Assembly much more, a little more, a little less or much less than other deputies in this Department?

- Much more
- A little more
- A little less
- Much less

Bq50 How certain are you about your response to this question?

- Very certain
- Certain
- Not certain
- Very uncertain

Bq51 [NAME OF PRINCIPAL DEPUTY], does he work in committees much more, a little more, a little less, or much less than other deputies in this Department?

- Much more
- A little more

- A little less
- Much less

Bq52 How certain are you about your response to this question?

- Very certain
- Certain
- Not certain
- Very uncertain

Bq53 If you had to guess, how many other people in this village/quarter do you think also have received the information you did today about national deputies' performances? [NUMBER]

Bq54 If you had to guess, how many other villages/quartiers in this commune do you think also have received the information you did today about national deputies' performances? [NUMBER]

From Enumerator About the Interview:

Bq55 Interview end time:

Bq56 Was the respondent alone or surrounded by other people during the interview?

Bq57 If with others, how many people?

Bq58 In what language(s) was the interview conducted?

Bq59 Was the interview conducted in an urban or rural area?

Bq60 What material was the house in which the interview was conducted made of?

Bq61 What was the material of the roof of the house in which the interview was conducted?

Bq62 What is the main energy source for the household?

- electricity supplied by SBEE
- generator
- kerosene lamp
- battery flashlights
- candles
- solar energy
- other (specify):
- none

Bq63 What is the main source of water for the household?

- pipes/hose into the home
- water tower with hose in the neighborhood
- closed well
- open well
- lake or other

From Enumerator About Him/Herself:

Bq64 Name:

Bq65 Male/female

Bq66 Do you live in an urban or rural area?

Bq67 What is your ethnic group?

- Bariba
- Fon

- Adja
- Dendi
- Yoruba
- Nago
- Ditamari
- Peulh
- Yoa
- DK/Refused
- Other:

Bq68 Language spoken at home:

Bq69 Age:

Endline Survey (Phone)

Eq1 To verify, what is your first name?

Eq2 To verify, how old are you?

Eq3 In the most recent legislative elections on April 26, some people were not able to vote. How about you—were you able to vote?

- Yes, voted.
- No, could not vote. Skip to *Eq7*.

Eq4 If yes, [verification question].

Eq5 If yes: we would like to know which party you voted for. Many people do not feel comfortable telling us this information out loud. That is okay. I am going to read a list of parties

that ran in the election. After each one, please say yes if you voted for the party, no if you did not. I will read all the parties first so you can hear them. Then, I will repeat them slowly, pausing after each one, so you can answer yes or no to each. [Revise party list below as necessary.]

- FCBE

- Yes.

- No.

- UN

- Yes.

- No.

- Amana Alliance

- Yes.

- No.

- G13 Baobab

- Yes.

- No.

- AFU

- Yes.

- No.

- Cauris 2 Alliance

- Yes.

- No.

- UPR

- Yes.

- No.
- UB
 - Yes.
 - No.
- Other

Eq6 What was most important to you when deciding which party to support in the National Assembly election? [Open ended: Enumerator codes each of the following elements of answers]:

- Identity (ethnicity; group representation)
- Personal benefits targeted at voter or their family
- Local benefits
- Performance in national legislature
- Ability of the politician (efficacy)
- Character of politician (integrity)
- Endorsements by others (leaders; family members)

Eq7 If no, why didn't you vote?

- You didn't have the time.
- You were not registered.
- You could not find the polling station.
- You did not like any of the parties.
- You could not find your name in the register.
- You were told not to vote by someone else

Eq8 [If did not vote:] If you had voted on April 26, which party would you most likely have voted for?

Eq9 [For the party mentioned in *Eq5* or *Eq8*]: On this scale of one to seven, where seven means you feel really strongly about voting for the [RESPONDENT'S PARTY in *Eq5* or *Eq8*], and one means you do not feel strongly at all about voting for the [RESPONDENT'S PARTY], how strongly did you feel about voting for the [RESPONDENT'S PARTY]?

Eq10 [If respondent reported voting for (or hypothetically voting for if did not vote) a different party than reported in the vote intention question in the baseline]:

In the survey we conducted before the election, you indicated that you intended to vote for [PARTY reported in *Bq17*]. On this scale of one to seven, where seven means you now feel really strongly about that party, and one means you do not now feel strongly at all about that party, how strongly do you feel now feel about [PARTY reported in *Bq17*]?

[NB: We pre-specify these two questions about party attachment but they are subject to change should the technological necessities of programming them for enumerators' smartphones require it.]

Eq11 Imagine the following incumbent candidate is on the list of a legislative party in the National Assembly elections next month. Originally from a neighboring village/quartier, this candidate is, say, 49 years old. He finished secondary school in the department capital and then obtained a Masters in Law. [He has sponsored community activities in this village/been an active participant in the National Assembly in Porto Novo.] He has already been in the National Assembly for 4 years and wants to extend his mandate. On a scale of 1-10, how likely would you be to to vote for this person's party in the upcoming legislative elections? [Randomly assigned to 1 of : control, constituency service attribute, or legislative performance]

Eq12 We are surveying 19 other people in this village. In your best guess, where will most of them place themselves on the ladder of support for the candidate we just described? If you get the

correct answer, you will earn [] amount.

Eq13 To the best of your knowledge, what percentage of this village voted for the [PARTY RESPONDENT VOTED FOR – if didn't vote, PARTY indicated in *Eq8*]? If you get the correct answer, you will earn [] amount.

Eq14 Now I want to ask you a few questions about [NAME OF PRINCIPAL DEPUTY]. Answer as best you can. Does he participate in plenary sessions of the National Assembly much more, a little more, a little less or much less than other deputies in this Department?

- Much more
- A little more
- A little less
- Much less
- Don't know.

Eq15 How certain are you about your response to this question?

- Very certain
- Certain
- Not certain
- Very uncertain

Eq16 [NAME OF PRINCIPAL DEPUTY], does he work in committees much more, a little more, a little less, or much less than other deputies in this Department?

- Much more
- A little more
- A little less

- Much less
- Don't know

Eq17 How certain are you about your response to this question?

- Very certain
- Certain
- Not certain
- Very uncertain

Eq18 Considering plenary participation and committee participation together, do you think [NAME OF PRINCIPAL DEPUTY] does a little more, a little less, or much less than other deputies in this Department?

- Much more
- A little more
- A little less
- Much less
- Don't know

Eq19 How certain are you about your response to this question?

- Very certain
- Certain
- Not certain
- Very uncertain

Eq20 In your opinion, does [NAME OF PRINCIPAL DEPUTY] make much more, a little more, a little less or much less effort to get things done than other deputies in this Department?

- Much more
- A little more
- A little less
- Much less
- Don't know

Eq21 How surprised would you be to hear from a credible source about corruption involving your national deputy? Would you say you would be

- Very surprised
- Somewhat surprised
- Not too surprised
- Not surprised at all
- Don't know

Eq22 How likely do you think it is that powerful people can find out how you vote, even though there is supposed to be a secret ballot in this country?

- Not at all likely
- Not very likely
- Somewhat likely
- Very likely

Eq23 Did a representative of the Centre d'Etude et de Promotion de la Democratie visit your home?

- Yes.
- No.
- Don't recall.

Eq24 Did a representative of the Centre d'Etude et de Promotion de la Democratie visit the homes of others in your village/quartier?

- Yes.
- No.
- Don't know/don't recall.

Eq25 Did you attend a village/quartier meeting organized by the Centre d'Etude et de Promotion de la Democratie?

- Yes.
- No.
- Don't recall.

Eq26 Did you attend rallies for any political parties prior to the election?

- Yes.
- No./Don't recall. Skip to *Eq30*.

Eq27 If so, for which parties?

Eq28 In the week before the election did you hear of [NAME OF PRINCIPAL DEPUTY] or someone from his/her party making statements about the deputy's performance in the National Assembly in Porto Novo?

- Yes.
- No.
- Don't recall.

Eq29 During campaigning season, did you hear parties talk about information given out in village/quartier gatherings organized by the Centre d'Etude et de Promotion de la Democratie?

- Yes.
- No.
- Don't recall.

Eq30 If yes, did you hear the parties say that the information provided by the Centre d'Etude et de Promotion de la Democratie was inaccurate or accurate?

- Accurate.
- Inaccurate.
- Didn't comment on accuracy.
- Don't recall.

Eq31 Before the election, did you discuss your vote with other people in this village/quartier?

- Yes.
- No.
- Don't recall.

Eq32 With whom do you remember discussing your vote? (record name, relationship to respondent and/or position in village/quartier)

Eq33 Did you try to persuade anyone else in this village or another village to vote for a particular party or national deputy?

- Yes.
- No.
- Don't recall.

Eq34 Whom did you try to persuade? (record name, relationship to respondent and/or position in village)

Eq35 Did parties contesting the legislative elections try to convince voters in this village/quartier to support their policies?

- Yes.
- No.
- Don't know/don't recall.

Eq36 If yes, how did they do so? [open]

Eq37 Did any of the following things happen during the campaigning season, from what you remember? Parties handed out money or gifts to voters in this village/quartier.

- Yes.
- No.

Eq38 Parties gave money or gifts to you or a member of your household.

- Yes.
- No.

Eq39 Parties scared or intimidated voters in this village/quartier.

- Yes.
- No.