Appendix for 'When Does Information Influence Voters? The Joint Importance of Salience and Coordination'

A Additional details about randomization and sampling

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

A.1 Blocked randomization

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

Due to the considerably larger number of rural villages in the sample, we generated three blocks within which to assign treatment: urban, rural/competitive, and rural/non-competitive. Within each high dosage commune, we then randomly assigned one locality from each block to each our four treatment conditions and our control condition with survey.[1] The remaining localities served as additional control communities in analyses using administrative data.

A.2 Sampling for the survey

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

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

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

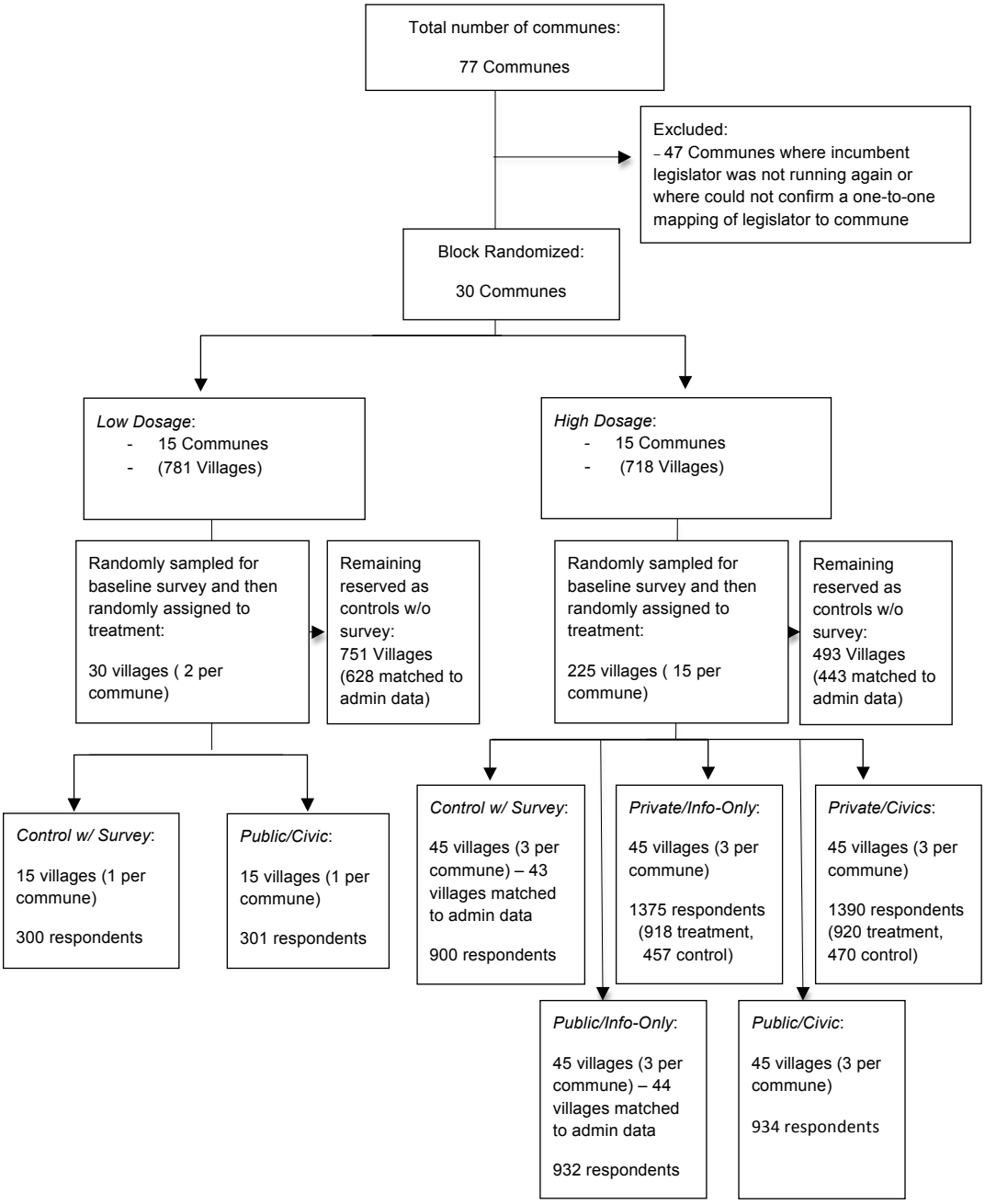
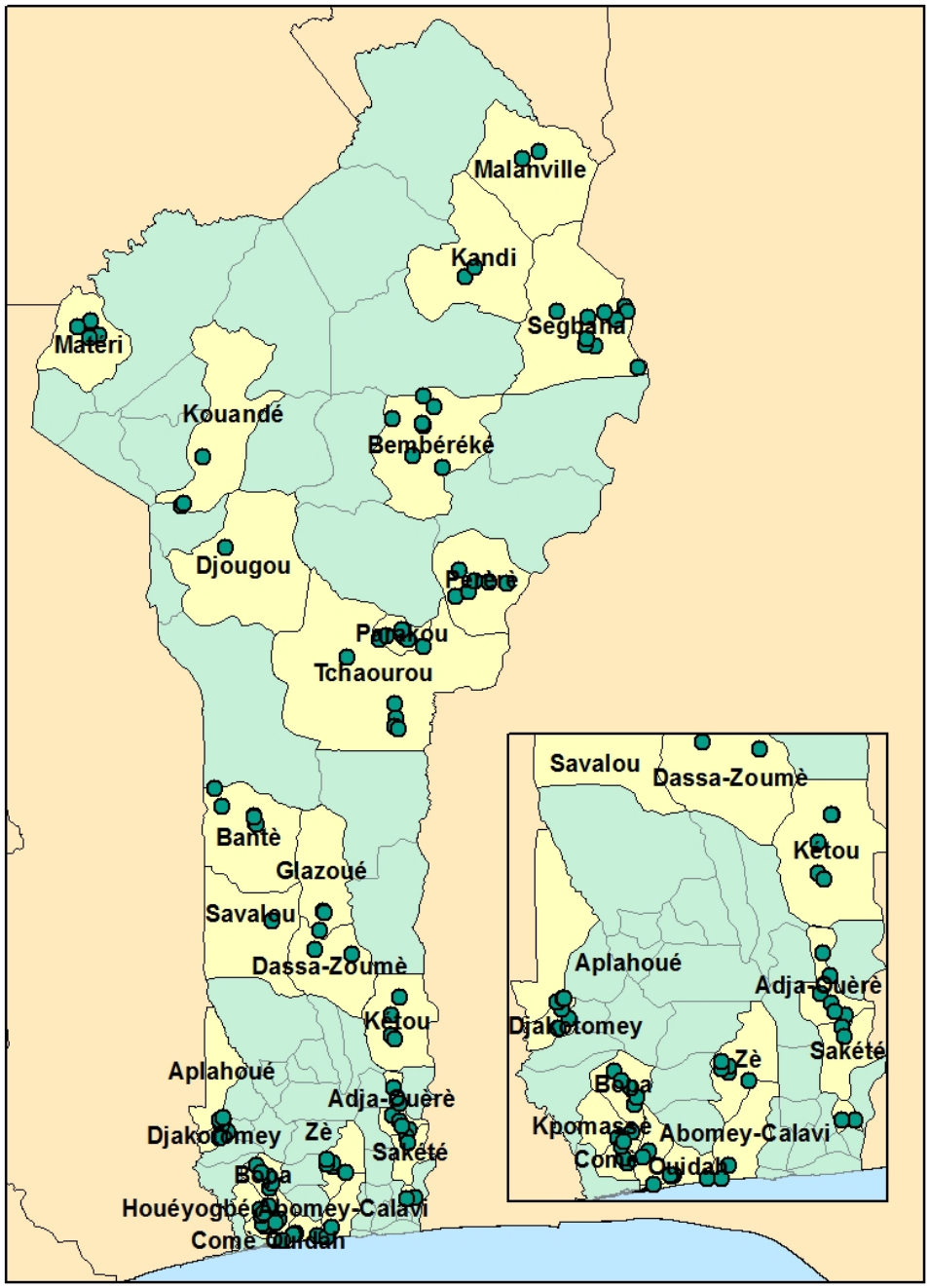


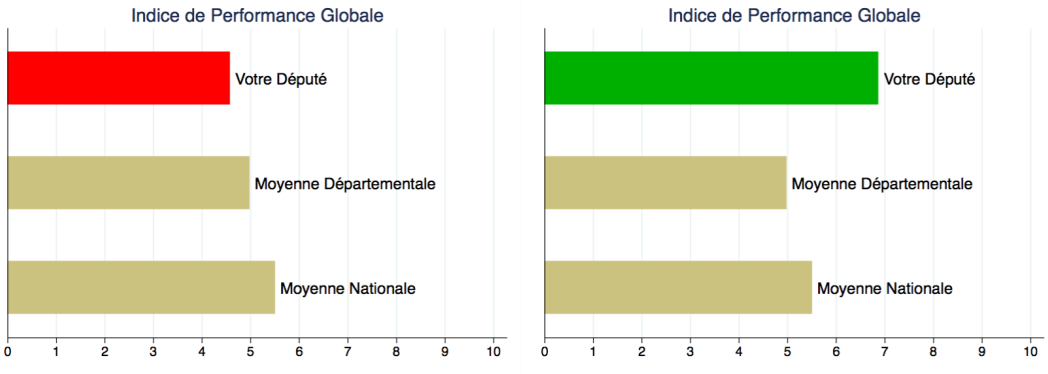
Figure A.2: Sample Communes and Villages/Quarters



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

B Video Treatments

Figure B.1: Example Intervention Bar Graph



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

B.1 Information Only Condition Script (in English)

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

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

or the president's activities being evaluated as a sign of their active participation. The majority of deputies, X%, never ask any questions. However, some deputies are very involved asking up to 70 questions.

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

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

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

First, I will tell you about how [NAME] performed in plenary sessions. He attended [RAW NUMBER] sessions of the X total plenary sessions. In other words, he attended [NUMBER OUT OF TEN] plenary sessions. He asked [RAW NUMBER] questions during these sessions. Remember, while most deputies ask no questions, some ask up to 70. Combining these two measures, we give [NAME] a score of [NUMBER] out of 100 on the plenary performance index. As you can see, your deputy did [BETTER/WORSE] on this measure than other deputies in your département. And he did [BETTER/WORSE] than the national average for all the deputies in Benin.

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

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

deputy, [NAME] performed [BETTER/WORSE] in total than other deputies in your département, and [BETTER/WORSE] than the national average for all the deputies in Benin.

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

B.2 Salience Condition Script (in English)

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

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

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

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

The second main responsibility of deputies is Oversight. As I mentioned, oversight means holding the President accountable and making sure that he respects the laws and people of Benin. One very important way in which deputies are authorized by law to engage in oversight is by intervening in the process by which the national budget is crafted each year. In fact, by law, the

National Assembly is the institution that can oversee the President's budget and make sure that it reflects the needs of the people of Benin. Each year the President proposes a budget—that is, he proposes the total amount of money that will be spent on executing national policies and projects in that coming year, and he proposes how that money will be divided across projects and across different parts of the country. The most important committees in the National Assembly for overseeing this proposal for spending money are the Finance and the Planning committees. Deputies on these two committees are supposed to meet regularly in order to analyze and study the proposed budget. These two committees review more bills than any other committees in the National Assembly. They can make recommendations to the President about ways to amend the budget before it is presented to the National Assembly. All deputies can also vote to approve the President's budget once it is presented to the full Assembly. When the budget is implemented, the National Assembly can make recommendations to the President if they observe that the budget is not being spent properly.

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

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

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

One example is the anti-graft law that the National Assembly passed in August 2011. This is a law that requires Benin's top leaders, civil servants, central directors of the administration, project managers and accountants of any public body to disclose their assets when they enter and leave office. The law is intended to help prevent corruption so that the money in the national budget

is spent on you, the citizen, and not on lining the pockets of powerful people. The deputies in the National Assembly are the ones who had to review and approve this law. Their work in the National Assembly in Porto Novo is thus relevant to ensuring that resources get to you.

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

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

Third, let me give you an example of Parliament's important role in presidential oversight. In 2009, President Boni Yayi sent a proposal (known as a "projet de loi") to the National Assembly that sought to revise Benin's constitution, which has not been changed since it was enacted in 1990. The proposal was again sent to the National Assembly in 2013. The proposal went to the Law Committee of the National Assembly but did not make it to the Assembly for a vote. Members of the public began opposing the proposal fearing it was a way for President Yayi to extend his presidential mandate. Benin citizens came together to voice their opinion and created movements against the changes such as the "Red Wednesday" movement ("Mercredi rouge"). Leaders of several political parties came forward to oppose the proposals, stating that the public was not ready for a change to the constitution, and citing more important issues for the president to concentrate on. Even pro-presidential members of the Law Committee were against the changes, and large majority of the committee's members voted to reject the proposal on September 24th, 2013. In this sense, legislative representatives not only exercised their right as a check to the executive but also represented the public interest which voiced its disapproval of any constitutional changes. These are just a few examples of how the performance of your national deputy—his participation in committees and in plenary sessions of the National Assembly, his willingness to ask questions and voice positions on legislation and to exercise presidential oversight—are important for your daily lives.

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

B.3 Dosage Conditions

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

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

2. Low Dosage: You have been selected through a random process to participate in a research study about the performance of your deputies in the National Assembly. Your community is the only one in the commune receiving this information.

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

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

2. Low Dosage: Remember, your village/quartier is the only one in the commune receiving this vido.

C Balance Tests

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

	Mean Unmatched	Mean Matched	Difference	P-Value	P-Value
Registered Voters (log)	6.34	6.54	.2	.09	.
Urban	.28	.23	.06	.14	.
Turnout	68.34	67.59	.75	.7	.
Competitive(dichotomous)	.52	.45	.07	.18	.
Incumbent Performance	4.98	5.19	.22	.69	.
North	.39	.46	.07	.65	.

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

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

	Mean High Dosage	Mean Low Dosage	Difference	P-Value
Registered Voters (log)	6.3	6.52	.22	.18
Urban	.21	.25	.04	.41
Competitive(dichotomous)	.42	.5	.07	.26
Vote Margin	.28	.24	.05	.19
Overall Performance	4.97	5.35	.38	.67

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

Table C.3: Balance in High Dosage Communes

	Control	Info Only/Private	Info Only/Public	Civics/Private	Civics/Public
Registered Voters (log)	687.5	835.91	1066.25	1110.73	807.27
		(.09)	(.06)	(.00)	(.02)
Urban	.18	.31	.34	.27	.29
		(.00)	(.00)	(.07)	(.01)
Competitive(dichotomous)	.4	.5	.48	.5	.48
		(.07)	(.29)	(.13)	(.24)
Vote Margin	.29	.25	.23	.26	.31
		(.13)	(.05)	(.43)	(.48)
Overall Performance	4.89	5.23	5.19	5.23	5.23
		(.17)	(.21)	(.17)	(.17)

P-values in parentheses indicate significance of difference between the mean of each treatment group and the control group mean.
P-values generated from tests in which we cluster on commune.

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

Table C.4: Balance in Low Dosage Communes

	Mean Treatment	Mean Control	Difference	P-Value
Registered Voters (log)	933.27	1024.24	90.97	.76
Urban	.27	.25	.01	.92
Competitive(dichotomous)	.53	.49	.04	.77
Vote Margin	.25	.24	.01	.86
Overall Performance	5.42	5.35	.07	.9

D Validating the Performance Index

To validate our performance measure, we examine whether our index correlates well with an alternative — and independently created — proxy for legislative performance: the legislator's professional background prior to holding office. Exploring the rising cost of campaigns and the role of money in politics in Benin, Koter (2017) shows that wealthy individuals (business people and customs officials) have more than quadrupled their presence in parliament while the presence of the less wealthy, intellectual class (teachers, lawyers, academics) who comprised the vast majority of parliamentary seats early in Benin's democracy has been steadily declining. While the latter politicians are considered better qualified to fulfill the formal duties of their position, the former are more valuable to parties because of their ability to buy votes. Combining our performance index with occupational data collected by Koter (2017), we see that wealthier politicians perform about 50 percent less well than other parliamentarians on components of the index such as attendance at plenary sessions and committee meetings. This increases our confidence that the performance index is measuring true legislative capacity.

More anecdotally, our elite interviews during an extraordinary session of parliament also revealed types consistent with our index. An example of a "good" performer we interviewed was a retired agronomist, who complained that he entered politics to address the concerns of his impoverished rural neighbors through legislation but was disappointed to learn that most politicians enter parliament to advance personal aims rather than the interests of the nation. Meanwhile, "bad" performers were difficult to interview because they were not even in the capital during the extraordinary session of parliament.[2] In short, "good" performers according to our index were indeed politicians interested in lawmaking and who were active during an extraordinary session of parliament. By contrast, "bad" performers according to our index were notably absent from the capital, and in some cases, from the country.

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

E A Theory of Performance Dimensions as Substitutes

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

$$Y = p(LE') + (1-p)(TR') \tag{1}$$

Let's say we now introduce LE, an information signal about the legislative dimension. The effect of LE on Y will depend on the relative weighting as specified in Equation 1, but also on the voter's perceived interaction between the two dimensions. We define $F(LE)$ as a function that translates a signal about the legislative dimension into a signal about the transfers dimension. If the two dimensions are complements, then $F(LE)$ is positive; if they are substitutes, $F(LE)$ is negative; and if they are orthogonal, $F(LE)$ is 0. Thus, for a voter who fully updates her valuation of the legislative dimension given the signal, LE will induce the voter to newly value the candidate as follows:

$$Y^{LE} = p(LE) + (1-p)(TR' + F(LE)) \tag{2}$$

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

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

From Equation 3, we can clearly see that as long as $p \neq 0$ (the voter places nonzero weight on the legislative dimension) and $F(LE) \geq 0$ (the dimensions are not substitutes), then Hypothesis 1 holds unambiguously. If, however, the two dimensions *are* substitutes, then whether Hypothesis 1 holds depends on p. Conditional on the function $F(LE)$, there will be some level $p > p^*$ for which Hypothesis 1 will hold. For values $p < p^*$, we should instead observe the opposite as defined by the following alternative hypothesis:

Hypothesis 5 *When $F(LE) < 0$, or the two dimensions of candidate performance are substitutes, then for values of $p < p*$, increasing access to positive legislative performance information about an incumbent will have a negative effect on electoral support for that incumbent.*

Note that this prediction does not necessarily hold symmetrically for negative performance information. If the dimensions are substitutes because, say, politicians face a budget constraint on their time, then learning that politicians are not spending time in the legislature does not necessarily imply they *are* spending time on transfers, even though the reverse is true. Indeed, one could easily imagine a low-quality politician spending time neither on legislating nor on transfers. In

other words, if the two dimensions of candidate performance are substitutes, positive legislative performance information might be expected to have a negative effect on electoral support for the incumbent but negative legislative performance information might be expected to have no effect.[3]

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

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

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

F The Impact of Negative Information

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

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

	(1)	(2)	(3)	(4)	(5)
		High Dose	High Dose	All High	All
VARIABLES	Low Dose	No Civics	No Civics	Dose	Communes
Received any treatment	-0.049	0.014			
	(0.044)	(0.029)			
Private treatment			0.006		
			(0.040)		
Public treatment			0.022		
			(0.039)		
Performance information only				0.010	0.010
				(0.028)	(0.027)
Civic + performance information				-0.003	-0.049
				(0.018)	(0.043)
High Dosage x Civics					0.047
					(0.047)
Constant	0.332***	0.403***	0.403***	0.404***	0.363***
	(0.001)	(0.003)	(0.003)	(0.005)	(0.002)
Observations	390	261	261	297	687
R-squared	0.603	0.597	0.597	0.599	0.612

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

G Robustness Checks

G.1 Restricting to Respondents Who Identified One Incumbent for Their Commune

Table G.1 presents survey results when restricting the sample to respondents who identified the incumbent in our design as *the* incumbent in charge of their commune.

G.2 The Negative Effect of Good Information

Table G.2 presents results from a series of robustness checks on one of the main results presented in the article: that the effect of positive performance information is negative in low dosage communes. In the first column, we replicate the results from the main specification. In the second column, we present results from the main specification including weights to account for the differences in block size. In the third column, we remove the block fixed effects. Finally, to ensure that the results are not being driven by an outlier commune — which is possible given the small number of communes in the low dosage, good news sample — we estimate the treatment effect in analyses with each commune removed. These results are presented in the final six columns of the table. The results in each model are comparable statistically and substantively.

Table G.1: The Impact of Positive Information on Incumbent Support, Respondents Who Identified Their Incumbent Only

VARIABLES	(1) Low Dose	(2) High Dose	(3) High Dose	(4) High Dose	(5) All Communes
Received any treatment	-0.158**	0.125**			
	(0.071)	(0.048)			
Information only				0.129**	0.129**
				(0.055)	(0.055)
Salience				0.122**	-0.158**
				(0.049)	(0.067)
Private			0.085*		
			(0.048)		
Public			0.181***		
			(0.057)		
High Dosage x Salience					0.280***
					(0.083)
Constant	0.555***	0.462***	0.463***	0.462***	0.471***
	(0.050)	(0.042)	(0.042)	(0.042)	(0.039)
Observations	84	779	779	779	863
R-squared	0.522	0.211	0.218	0.211	0.245

In parentheses, robust standard errors clustered by village.
Models include block fixed effects. *** p<0.01, ** p<0.05, * p<0.1.
Includes only respondents who identified the incumbent as responsible for their commune.

Table G.2: Robustness Tests: Negative Effect of Good News in Low Dosage

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

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

G.3 The Effect of Salience

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

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

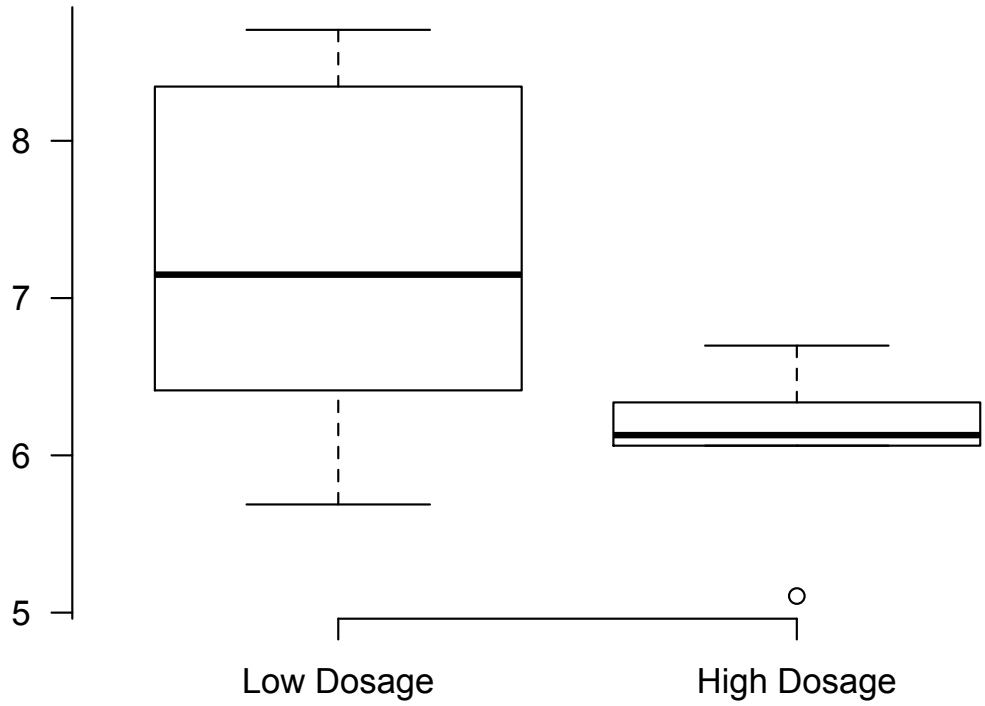


Table G.3: Robustness Tests: Positive Effect of Salience in High Dosage

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

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

G.4 Controlling for Village Size and Pre-treatment covariates

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

To test robustness to pre-treatment covariates, we use control variables that were gathered as part of our baseline survey. Because we conducted the baseline survey in a subset of the control units in each sample commune, the number of observations in this analysis is substantially smaller, which limits our statistical precision. In addition, the sample itself is slightly different, as it does not include the full set of administrative controls. In Table G.5, we first show that our main results hold when we conduct the main analysis on the smaller sample that received the survey (without including pre-treatment covariates). We lose considerable statistical power in this analysis — for example, the analysis in column 1 includes only 12 observations — but the results are qualitatively the same as our main results.

In Table G.6, we introduce a number of pre-treatment controls specified in the pre-analysis plan under covariate adjustment.[4] We average the response of each respondent in each village/quarter to produce a measure for each unit in the sample. While these analyses are under-powered because of the smaller sample sizes, the results are qualitatively similar to the main results. We are not able to run the covariate adjustment for the low dosage only sample due to the insufficient number of observations (12).

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

Table G.4: The Impact of Positive Performance Information by Village Size

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

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

Table G.5: The Impact of Positive Performance Information (Official Results Using Only the Sample Where a Survey Was Conducted)

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

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

Table G.6: The Impact of Positive Performance Information with Pre-Treatment Controls (Official Results Using Only the Sample Where a Survey Was Conducted)

VARIABLES	(1) Coordination Info Only	(2) Coordination Info Only	(3) Coor-dination	(4) All Communes
Treatment	-0.035			
	(0.034)			
Private treatment		-0.041		
		(0.031)		
Public treatment		-0.027		
		(0.047)		
Performance information only			-0.038	-0.038
			(0.036)	(0.037)
Salience + performance information			0.037	-0.071
			(0.036)	(0.081)
Coordination x Salience				0.107
				(0.088)
Female	-0.091	-0.054	-0.305	-0.330
	(0.437)	(0.446)	(0.355)	(0.361)
Years of education	0.007	0.007	0.026**	0.027**
	(0.020)	(0.021)	(0.013)	(0.013)
Coethnic with incumbent	-0.152	-0.155	-0.140**	-0.140**
	(0.096)	(0.099)	(0.060)	(0.061)
Age	0.000	0.000	0.005	0.005
	(0.004)	(0.004)	(0.004)	(0.004)
Poverty index	0.013	0.008	0.030	0.048
	(0.094)	(0.102)	(0.089)	(0.091)
Information from radio	-0.156	-0.147	-0.221	-0.215
	(0.179)	(0.190)	(0.152)	(0.148)
Information from newspaper	-0.356	-0.254	-0.386	-0.376
	(0.915)	(0.894)	(0.468)	(0.466)
Information from television	-0.489**	-0.491**	-0.498**	-0.513**
	(0.219)	(0.225)	(0.209)	(0.209)
Ethnic Homogeneity	-0.109	-0.116	-0.099	-0.101
	(0.097)	(0.098)	(0.085)	(0.086)
Ballot secrecy can be violated	-0.102	-0.099	-0.085	-0.088
	(0.178)	(0.182)	(0.116)	(0.116)
Parties know how the village voted	0.101	0.095	0.337***	0.337***
	(0.132)	(0.134)	(0.093)	(0.092)
Constant	0.634*	0.614*	0.430*	0.440*
	(0.335)	(0.346)	(0.241)	(0.247)
Observations	78	78	132	144
R-squared	0.821	0.822	0.716	0.716

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

H Deviations from Pre-Analysis Plan

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

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

$$E(Y_{ij}|Positive_j[Negative_j]) = \beta_0 + \beta_1 InformationOnly_i + \beta_2 Salience_i + \beta_3 Private_i + \beta_4 Public_i + \mu_k \quad (4)$$

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

$$E(Y_{ij}|Positive_j[Negative_j]) = \beta_0 + \beta_1 InformationOnly_{ij} + \beta_2 Salience_{ij} + \mu_k \quad (5)$$

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

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

Second, our pre-analysis plan includes the hypothesis about the effect of dosage (H7) under the category of secondary outcomes and conditional effects rather than a main hypothesis. In the article, we treat this as a main hypothesis for two reasons. Dosage was a randomly assigned treatment built into our design – not a pre-treatment conditioning variable or secondary dependent variable like the other hypotheses in the category. This supports the idea that it was a key theory we wanted to test prior to rolling out the experiment. While the dosage and public treatments both address theoretical questions about voter coordination, the motivating theories are distinct in that they test the importance of across-group and within-group coordination, respectively, which are independent and separable mechanisms. On the other hand, another of the pre-specified main hypotheses – the joint effect of the Salience and Public treatments (H4) – is not explicitly tested in the article. This is because the Public treatment did not have a direct effect, so it did not make sense to ask whether that null effect was then amplified in some conditions. The data further show that this joint effect is also null.

Third, our pre-analysis plan specified that we would use inverse probability weighted regressions to account for differences in block size in our analysis of the official results. These differences arise because the three blocks we use for within-commune stratification — urban, rural/competitive, and rural/non-competitive — are of unequal sizes. As a result, villages are assigned to treatment and control across blocks with different probabilities. In the main analyses in the article, we do not use inverse probability weighting because we now believe that the block fixed effects are sufficient to control for these differences. The tables in Appendix G show that our main results are robust to the use of inverse probability weights. For completeness, Table H.1

below presents all of the results using inverse probability weights. We note that the coefficient on salience in the good news high dosage communes (column 3), is not statistically significant at conventional levels using this approach. However, the p-value is 0.103. We thus interpret the results as consistent with those presented in the main body of the article.

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

Table H.1: Pre-specified estimation

VARIABLES	(1) Good News No Coord	(2) Good News Coord	(3) Good News Coord	(4) Good News All Communes	(5) Bad News No Coord	(6) Bad News Coord	(7) Bad News Coord	(8) Bad News All Communes
Public, Salience	-0.140***				-0.053			
	(0.039)				(0.031)			
Performance information only		-0.020				0.008		
		(0.025)				(0.034)		
Salience + performance information		0.037				-0.007		
		(0.022)				(0.025)		
Private treatment			0.009				-0.014	
			(0.026)				(0.028)	
Public treatment			0.007				0.014	
			(0.025)				(0.031)	
Received any treatment				-0.142**				-0.129
				(0.053)				(0.092)
Treatment x Coordination				0.149*				0.127
				(0.087)				(0.123)
Coordination				0.038				0.068
				(0.067)				(0.091)
Constant	0.302***	0.332***	0.332***	0.315***	0.289***	0.402***	0.402***	0.372***
	(0.033)	(0.018)	(0.018)	(0.035)	(0.027)	(0.018)	(0.018)	(0.071)
Observations	265	365	365	630	390	297	297	687
R-squared	0.490	0.629	0.615	0.119	0.827	0.685	0.687	0.144

I Attrition and Response Bias in the Survey Data

I.1 Differential Attrition

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

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

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

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

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

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

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

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

I.2 Response Bias

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

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

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

	(1)	(2)
VARIABLES		
Treatment	0.050*	0.110**
	(0.029)	(0.044)
Good News		0.080
		(0.051)
Treatment x Good News		-0.106*
		(0.059)
Constant	0.157***	0.112***
	(0.026)	(0.039)
Observations	237	237
R-squared	0.013	0.026

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

In Table I.4, we present results from a similar analysis that examines incumbent voteshare. Here, we calculate the deviation as the difference between the reported incumbent voteshare as reported in the survey and the official voteshare. Positive values thus indicate over-reporting of votes for the incumbent, while negative values indicate under-reporting. In the absence of response

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

VARIABLES	(1)	(2)
Treatment	-0.019	-0.096
	(0.041)	(0.061)
Good News		-0.016
		(0.071)
Treatment x Good News		0.134
		(0.081)
Constant	0.170***	0.179***
	(0.036)	(0.053)
Observations	237	237
R-squared	0.001	0.039

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

bias, we would expect these deviations to be uncorrelated with treatment status and with the content of the information provided.

The results in column 2 provide some evidence of response bias in the survey. In treated areas that received bad news about the incumbent, the reported voteshare is almost 10 percentage points lower than the official voteshare. This coefficient is not statistically significant but is not far from conventional cutoffs ($p = .12$). This pattern is consistent with under-reporting of votes for the incumbent in treated areas where bad performance information was provided. In good news areas, the marginal effect of treatment on the deviation is about 3.8 percentage points (calculated by adding the treatment coefficient to the interaction term between treatment and good news). This is also consistent with over-reporting of votes for the incumbent in good news areas, although the marginal effect is not statistically significant.

Finally, we can also examine evidence of response bias by comparing the official number of votes received by the incumbent in a village to the number of survey respondents who report to us that they voted for the incumbent. In a small number of villages in our sample, the number of votes officially received by the incumbent was small enough that it was possible for the survey to over-estimate the number of votes received by the incumbent. Table I.5 shows that there are 7 villages in our sample where the reported number of votes in the survey was *greater* than the official number of votes received by the incumbent. Of these, 6 are treated villages and 4 are in good news communes. This is very clear evidence of over-reporting of votes for the incumbent in the survey. While we cannot make these types of direct comparisons for most villages in the sample — in most cases the incumbent receives more votes than the number of voters surveyed — these patterns should raise concerns about survey response bias in all villages.

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

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

J Replication of Main Results Using the Survey Data

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

VARIABLES	(1) Low Dose	(2) High Dose No Civics	(3) High Dose No Civics	(4) All High Dose	(5) All High Dose	(6) All Communes
Treatment	-0.021	0.084**				
	(0.071)	(0.039)				
Private treatment			0.022	0.045		
			(0.040)	(0.038)		
Public treatment			0.145***	0.129***		
			(0.047)	(0.045)		
Performance information only					0.090**	0.090**
					(0.043)	(0.043)
Civic + performance information					0.074*	-0.021
					(0.041)	(0.068)
dose_civics						0.094
						(0.080)
Constant	0.468***	0.437***	0.437***	0.437***	0.438***	0.440***
	(0.046)	(0.031)	(0.031)	(0.034)	(0.035)	(0.032)
Observations	153	875	875	1,519	1,519	1,672
R-squared	0.421	0.181	0.191	0.163	0.157	0.182

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

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

VARIABLES	(1) Low Dose	(2) High Dose No Civics	(3) High Dose No Civics	(4) All High Dose	(5) All High Dose	(6) All Communes
Treatment	-0.019	0.085**				
	(0.073)	(0.039)				
Private treatment			0.016	0.046		
			(0.040)	(0.038)		
Public treatment			0.154***	0.132***		
			(0.048)	(0.045)		
Female	0.106**	-0.013	-0.017	-0.006	-0.005	0.005
	(0.043)	(0.031)	(0.031)	(0.023)	(0.024)	(0.022)
Coethnic with incumbent	0.036	-0.035	-0.063	-0.049	-0.040	-0.034
	(0.058)	(0.045)	(0.044)	(0.035)	(0.035)	(0.033)
Performance information only					0.091**	0.092**
					(0.042)	(0.042)
Civic + performance information					0.076*	-0.011
					(0.041)	(0.069)
dose_civics						0.088
						(0.080)
Constant	0.388***	0.462***	0.480***	0.468***	0.463***	0.457***
	(0.059)	(0.041)	(0.041)	(0.040)	(0.041)	(0.038)
Observations	152	872	872	1,505	1,505	1,657
R-squared	0.433	0.181	0.193	0.165	0.159	0.184

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

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

VARIABLES	(1) Low Dose	(2) High Dose No Civics	(3) High Dose No Civics	(4) All High Dose	(5) All High Dose	(6) All Communes
Treatment	-0.308***	0.003				
	(0.065)	(0.065)				
Private treatment			0.002	-0.035		
			(0.071)	(0.067)		
Public treatment			0.005	-0.043		
			(0.071)	(0.066)		
Performance information only					-0.010	-0.010
					(0.067)	(0.067)
Civic + performance information					-0.065	-0.308***
					(0.065)	(0.062)
dose_civics						0.243***
						(0.090)
Constant	0.678***	0.529***	0.529***	0.534***	0.534***	0.550***
	(0.044)	(0.059)	(0.059)	(0.061)	(0.061)	(0.054)
Observations	158	687	687	1,200	1,200	1,358
R-squared	0.212	0.239	0.239	0.254	0.257	0.251

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

K Analysis of Continuous Measure of Performance

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

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

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

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

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

L Spillovers

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

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

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

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

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

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

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

VARIABLES	(1) Whole Sample	(2) Positive Info	(3) Negative Info
Dosage	-0.291	-0.571**	-0.017
	(0.176)	(0.270)	(0.177)
Constant	1.099***	1.317***	0.899***
	(0.158)	(0.246)	(0.145)
Observations	626	326	300
R-squared	0.030	0.102	0.000

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
In parentheses, robust standard errors clustered by village.
*** p<0.01, ** p<0.05, * p<0.1.

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

	Info Only Villages	Info + Salience Villages	All Private Villages
Information Only	-0.148		-0.186
	(0.228)		(0.237)
Information Only + Salience		-0.044	-0.017
		(0.206)	(0.189)
Constant	-0.020	0.044	0.017
	(0.263)	(0.246)	(0.179)
N	313	387	700

Notes: Robust standard errors in parentheses clustered by village. Inverse probability weights.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.
DV is binary (1=voted for incumbent, 0=voted for another/did not vote) based on survey data.

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

	Info Only Villages	Info + Salience Villages	All Private Villages
Information Only	-0.162		0.107
	(0.237)		(0.286)
Information Only + Salience		0.272	0.006
		(0.236)	(0.299)
Constant	0.234	-0.301	-0.035
	(0.325)	(0.353)	(0.238)
N	281	296	577

Notes: Robust standard errors in parentheses clustered by village. Inverse probability weights.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.
Dependent variable is binary (1=voted for incumbent, 0=voted for another or did not vote) from survey data.

M Impacts on Voter Turnout

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

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

VARIABLES	(1) All Communes	(2) Low Dosage	(3) High Dosage	(4) High Dosage	(5) High Dosage	(6) All Communes
Received any treatment	-0.03**	0.02	-0.03***			
	(0.01)	(0.02)	(0.01)			
Performance information only				-0.04**		-0.04**
				(0.02)		(0.02)
Civic + performance information				-0.02*		0.02
				(0.01)		(0.02)
Public treatment					-0.04**	
					(0.02)	
Private treatment					-0.02*	
					(0.01)	
High Dosage x Civics						-0.04*
						(0.02)
Constant	0.72***	0.72***	0.73***	0.73***	0.73***	0.72***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	613	247	366	366	365	613
R-squared	0.33	0.32	0.34	0.35	0.35	0.34

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

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

VARIABLES	(1) All Communes	(2) Low Dosage	(3) High Dosage	(4) High Dosage	(5) High Dosage	(6) All Communes
Received any treatment	0.01	0.04	0.00			
	(0.01)	(0.03)	(0.01)			
Performance information only				-0.00		-0.00
				(0.01)		(0.01)
Civic + performance information				0.01		0.04
				(0.01)		(0.03)
Public treatment					-0.00	
					(0.01)	
Private treatment					0.01	
					(0.02)	
High Dosage x Civics						-0.03
						(0.04)
Constant	0.70***	0.72***	0.68***	0.68***	0.68***	0.70***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	677	379	298	298	298	677
R-squared	0.27	0.29	0.16	0.16	0.16	0.27

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

N Candidate Vignette

A survey experiment was administered in both the baseline and endline survey to gauge respondents' relative valuation of the two dimensions of performance relevant in our study: clientelistic transfers and legislative performance. In each survey experiment, we presented respondents a short description of a hypothetical candidate. One of three versions of the vignette was randomly assigned to respondents at baseline and endline: control, legislative, and transfers. In the legislative version, we added to the baseline description, "He has been an active participant in the National Assembly in Porto Novo." In the transfers version, we added, "He has sponsored community activities in this village." Voters were then asked to rate their likelihood of voting for the candidate on a scale of one to seven. We present results from each survey below. Because the vignette condition was assigned independently at baseline and endline, we consider them as separate experiments.

N.1 Baseline Survey

Our baseline survey data provides evidence of voter preferences for transfers over legislative performance. Figure N.1 summarizes the level of support for the hypothetical candidate in each condition. Results indicate that adding a sentence about candidate transfers, on average, significantly increases the likelihood of voting for the candidate relative to both the control condition and relative to the legislative condition. This effect, suggesting that voters indeed prefer transfers over legislative performance, is statistically significant at the 95% confidence level.[5] Just as interesting is the null result we obtain when our hypothetical candidate performs well on legislation: here, survey respondents do not show a preference for legislative performers over our control candidate.

N.2 Endline Survey

Our endline survey data provides evidence that the Salience treatment indeed affected recipients' valuation of the legislative performance dimension. In Figure N.2, we see that support for a hypothetical candidate that performs well on the legislative dimension is statistically significantly higher in the Salience treatment relative to both the Information Only Treatment and Control groups. We can also confirm that the transfers dimension is the most valued dimension among Control participants, reflecting the pattern we observed prior to treatment in the baseline vignette experiment.

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

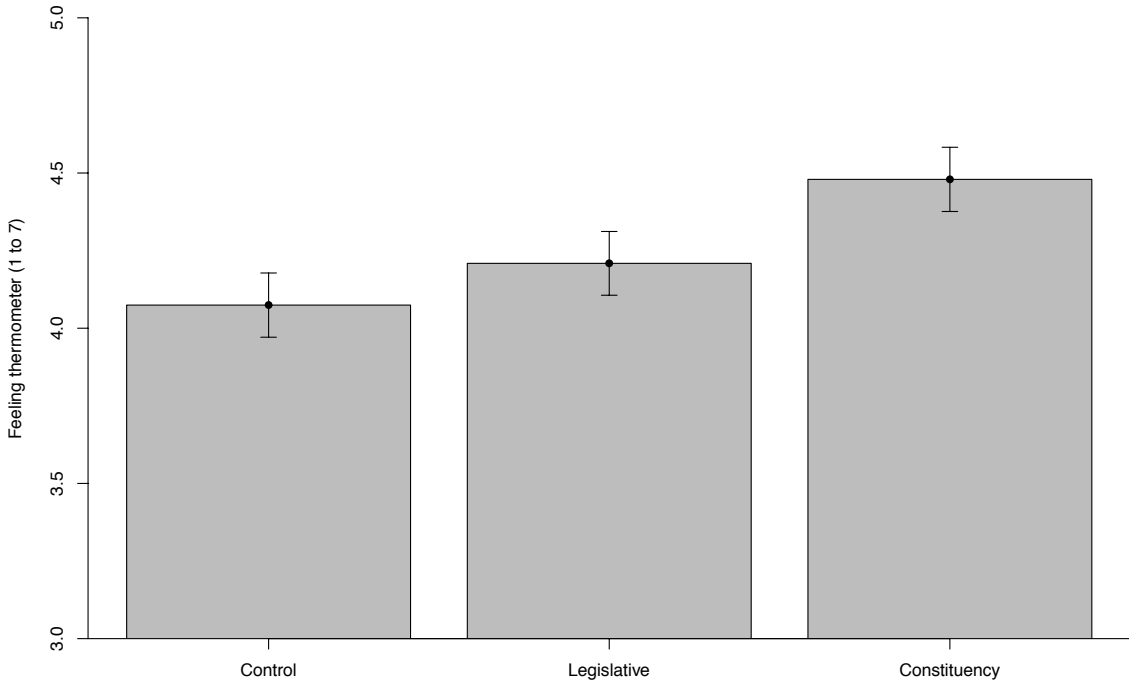


Figure N.1: Support for Candidate

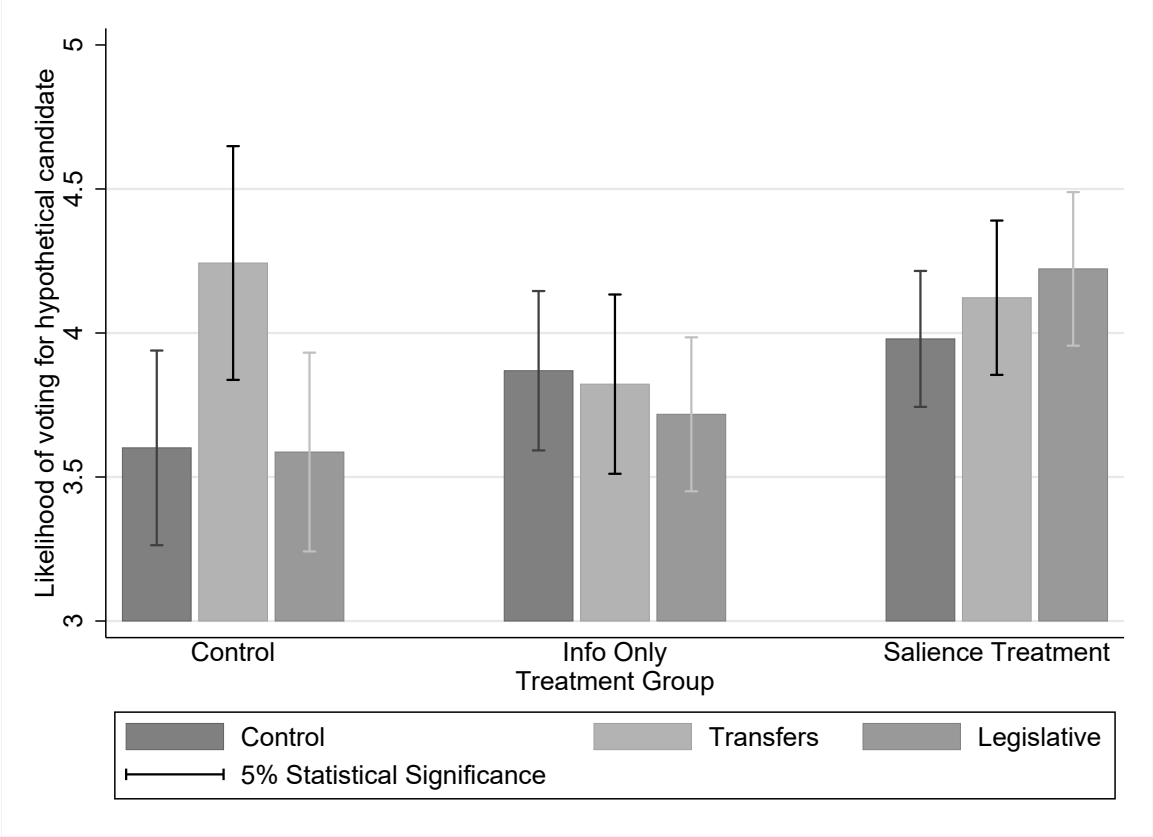


Figure N.2: Effect of Good News (relative to priors) on Support for Hypothetical Candidate

References

Koter, D. (2017). Costly electoral campaigns and the changing composition and quality of parliament: Evidence from benin. *African Affairs 116*(465), 573–596.