Supplemental Materials for "Reducing or Reinforcing In-group Preferences?"

A Additional Results and Robustness Checks

A.1 Socio-Economic Characteristics of Incumbent Coethnics and Non-Coethnics

Table A.1: Socio-Economic Characteristics of Incumbent Coethnics and Non-Coethnics

	Coethnics	Non-Coethnics	Difference	P-Value
Female	.49	.51	.02	0
Age	36.47	36.01	.46	.41
Yrs of Education	3.5	3.27	.23	.27
Electricity in Household	.32	.34	.02	.47
Quality of Housing Material	2.1	2.07	.03	.22
School in Village	.9	.94	.05	.1
Borehole in Village	.89	.91	.02	.43
Health Clinic in Village	.47	.54	.06	.22

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

A.2 Distribution of Candidate Ethnicities

Figure A.1: Distribution of Candidate Ethnicities by Electoral Circonscription

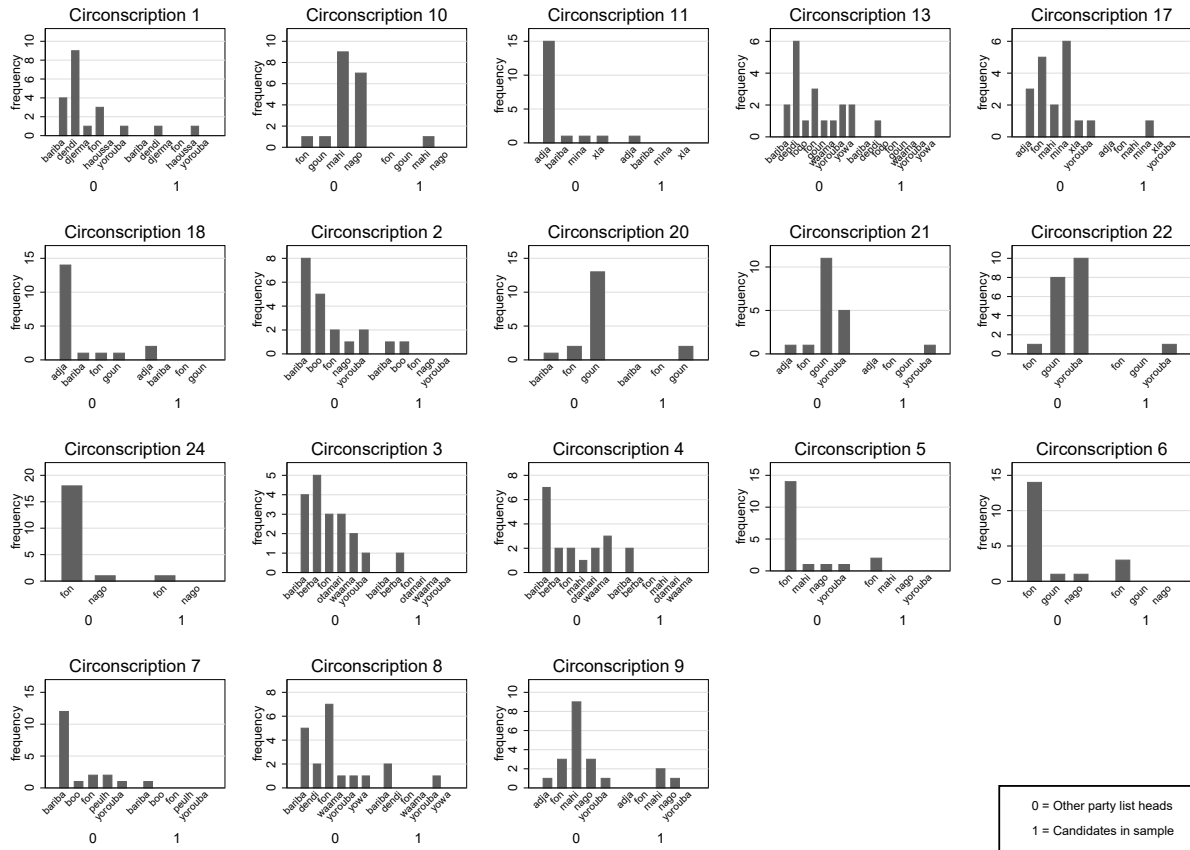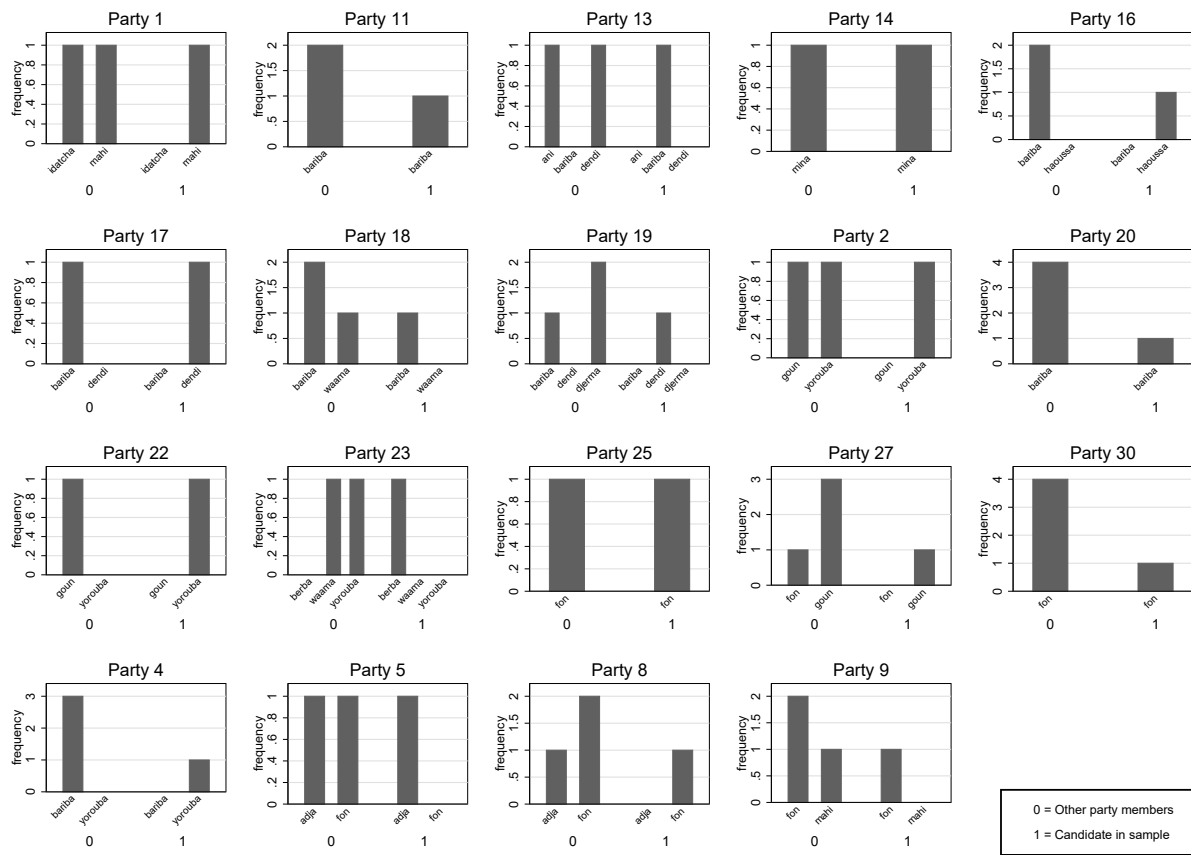0 = Other party list heads

1 = Candidates in sample

Figure A.2: Distribution of Candidate Ethnicities by Party List

Note: This figure shows 19 of the 30 party lists for which we were able to code candidate ethnicity for more than our sample politician.

A.3 Interaction Models and Control Variables

Table A.2: Interaction Models and Models with Controls, Survey Data

VARIABLES	(1) Good	(2) Good	(3) Bad	(4) Bad	(5) Good	(6) Bad
Treatment	0.007	-0.000	-0.169*	-0.210**	0.049	-0.328***
	(0.052)	(0.059)	(0.089)	(0.090)	(0.071)	(0.099)
Treatment x Coethnicity	0.110*	0.131*	0.124	0.123	0.118	0.207**
	(0.066)	(0.071)	(0.094)	(0.086)	(0.078)	(0.099)
Coethnic with incumbent	-0.124**	-0.144**	-0.107	-0.095	-0.136*	-0.194**
	(0.060)	(0.066)	(0.090)	(0.076)	(0.069)	(0.097)
Female		-0.007		0.020		
		(0.024)		(0.035)		
Age		0.001		-0.000		
		(0.001)		(0.001)		
Years of education		-0.000		-0.005		
		(0.003)		(0.004)		
Household electricity		-0.023		-0.016		
		(0.029)		(0.045)		
Quality of housing material		0.046*		-0.010		
		(0.024)		(0.045)		
School in the community		-0.111		-0.073		
		(0.071)		(0.107)		
Borehole in the community		0.044		0.075		
		(0.081)		(0.096)		
Health clinic in the community		0.017		0.044		
		(0.036)		(0.069)		
Treatment x 2011 Incumbent Vote					-0.058	0.158**
					(0.069)	(0.077)
2011 Incumbent Vote					0.064	-0.062
					(0.065)	(0.072)
Constant	0.517***	0.455***	0.642***	0.637***	0.483***	0.747***
	(0.044)	(0.126)	(0.081)	(0.157)	(0.065)	(0.091)
Observations	1,657	1,444	1,356	812	1,342	1,100
R-squared	0.184	0.197	0.246	0.248	0.191	0.237

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A.3: Treatment Effects by Ethnic Connection and Level of Performance With Controls for Prior Partisanship, Survey Data

VARIABLES	(1) Good Info Full Sample	(2) Good Info Coethnic	(3) Good Info Non-Coethnic	(4) Bad Info Full Sample	(5) Bad Info Coethnic	(6) Bad Info Non-Coethnic
Treatment	0.08**	0.14***	0.02	-0.10*	-0.04	-0.28***
	(0.04)	(0.05)	(0.07)	(0.06)	(0.06)	(0.08)
2011 Incumbent Vote	0.02	0.04	-0.05	0.06*	0.08*	0.03
	(0.04)	(0.05)	(0.06)	(0.04)	(0.04)	(0.07)
Constant	0.43***	0.37***	0.53***	0.55***	0.49***	0.69***
	(0.04)	(0.05)	(0.07)	(0.06)	(0.06)	(0.08)
Observations	1,353	845	497	1,102	782	318
R-squared	0.19	0.23	0.19	0.23	0.25	0.26

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A.4: Interaction Models, Official Results

VARIABLES	(1) Good Performance	(2) Bad Performance	(3) Good Performance	(4) Bad Performance	(5) Good Performance	(6) Bad Performance
Treatment	-0.060	-0.201	0.007	-0.223**	0.019	-0.184**
	(0.057)	(0.123)	(0.056)	(0.096)	(0.047)	(0.086)
Treatment x Coethnic (50)	0.155**	0.164				
	(0.068)	(0.155)				
Coethnic village (50 percent)	-0.111	-0.124				
	(0.079)	(0.145)				
Treatment x Coethnic (70)			0.082	0.232*		
			(0.075)	(0.121)		
Coethnic village (70 percent)			-0.159**	-0.274*		
			(0.073)	(0.145)		
Treatment x Coethnic (90)					0.072	0.203*
					(0.076)	(0.110)
Coethnic village (90 percent)					-0.096	-0.293***
					(0.067)	(0.098)
Constant	0.545***	0.645***	0.545***	0.717***	0.503***	0.703***
	(0.057)	(0.106)	(0.049)	(0.094)	(0.036)	(0.063)
Observations	137	103	137	103	137	103
R-squared	0.604	0.615	0.610	0.637	0.596	0.639

Robust standard errors in parentheses

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

A.4   Treatment Effects Among Members of the Ethnic Minority in Villages

Table A.5: Comparing Treatment Effect Among Ethnic Majority and Minority in Village

VARIABLES	(1) Good Info Coethnic	(2) Good Info Non-Coethnic	(3) Bad Info Coethnic	(4) Bad Info Non-Coethnic
Treatment	0.12***	-0.04	-0.04	-0.21*
	(0.05)	(0.07)	(0.06)	(0.12)
Treatment x Ethnic Minority in Village	-0.02	0.18	0.08	0.13
	(0.19)	(0.13)	(0.15)	(0.22)
Ethnic Minority in Village	-0.04	-0.02	-0.06	-0.16
	(0.19)	(0.12)	(0.13)	(0.16)
Constant	0.41***	0.49***	0.56***	0.63***
	(0.04)	(0.06)	(0.06)	(0.10)
Observations	1,030	627	948	408
R-squared	0.22	0.19	0.28	0.23

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

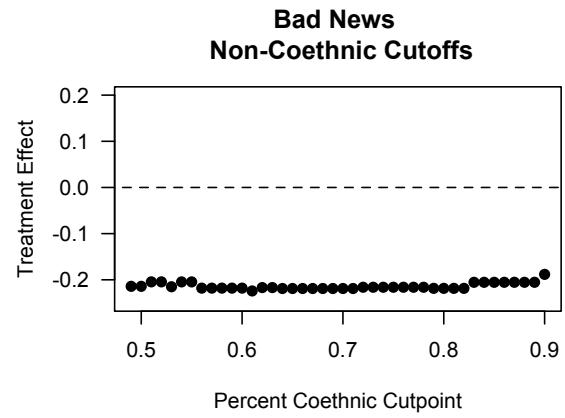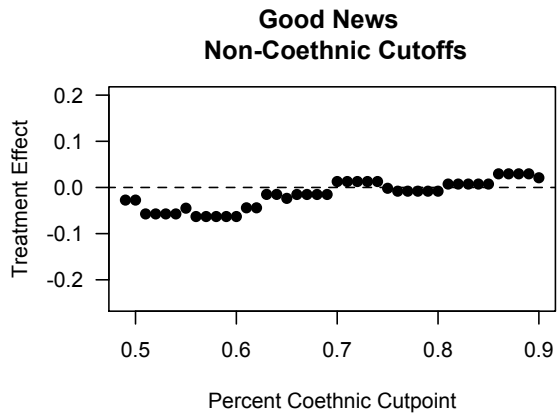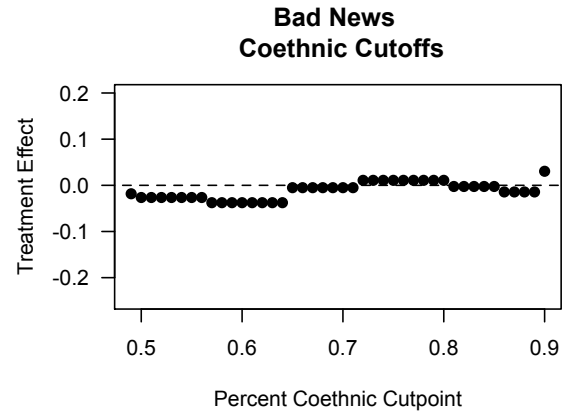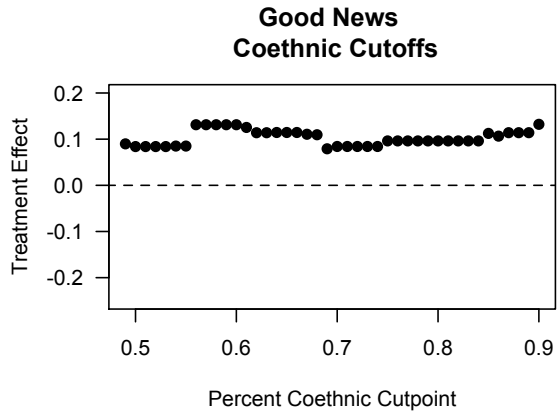## A.5 Treatment Effects Among Members of the Ethnic Minority in Communes

Table A.6: Comparing Treatment Effect Among Ethnic Majority and Minority in Commune

VARIABLES	(1) Good Info Coethnic	(2) Good Info Non-Coethnic	(3) Bad Info Coethnic	(4) Bad Info Non-Coethnic
Treatment	0.10**	-0.06	-0.04	-0.22**
	(0.05)	(0.07)	(0.06)	(0.10)
Treatment x Ethnic Minority in Commune	0.15	0.16	0.06	0.33*
	(0.13)	(0.11)	(0.13)	(0.19)
Constant	0.40***	0.50***	0.55***	0.54***
	(0.04)	(0.05)	(0.05)	(0.08)
Observations	1,030	627	948	408
R-squared	0.22	0.19	0.28	0.23

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

A.6 Robustness to Alternative Thresholds of Coethnicity

Good News
Coethnic Cutoffs

Bad News
Coethnic Cutoffs

Good News
Non-Coethnic Cutoffs

Bad News
Non-Coethnic Cutoffs

A.7 Voter Turnout Results

This section presents the results of analyses in which the dependent variable is voter turnout rather than vote share. Table A.7 presents the results using official administrative election results. Positive performance information has no impact on turnout in the full sample. The impact of positive performance information is also not conditioned by coethnicity. Negative performance information, by contrast, increases voter turnout by about 2 percentage points in the full sample. The effect is concentrated among non-coethnic villages, but none of the results is statistically significant.

Table A.8 presents results of the same tests using survey data. The evidence in this table should be interpreted with caution given well-known problems of social desirability bias in survey measures of voter turnout. Once again, we find no evidence that positive performance information has an effect on voter turnout, and the results do not vary by coethnicity. Negative performance information increases voter turnout, and the effect is similar for coethnics and non-coethnics.

In sum, we find evidence that negative information increases voter turnout, but that positive information has no effect on participation. Coethnicity does not, however, moderate these effects. Indeed, in an interaction model using the full sample, the coefficient on the interaction term between treatment and coethnicity is statistically insignificant – in contrast to tests using vote share as an outcome. Together, this suggests that voter preferences rather than voter mobilization is the mechanism driving the ethnically motivated electoral results.

Table A.7: Voter Turnout: Treatment Effects by Ethnic Connection and Level of Performance, Official Results

VARIABLES	(1) Good Info Full Sample	(2) Good Info Coethnic (50)	(3) Good Info Non-Coethnic (50)	(4) Bad Info Full Sample	(5) Bad Info Coethnic (50)	(6) Bad Info Non-Coethnic (50)
Treatment	-0.01	0.01	-0.03	-0.03*	-0.02	-0.06*
	(0.02)	(0.02)	(0.04)	(0.02)	(0.02)	(0.03)
Constant	0.70***	0.72***	0.66***	0.72***	0.71***	0.73***
	(0.02)	(0.02)	(0.04)	(0.02)	(0.02)	(0.03)
Observations	142	92	50	106	72	34
R-squared	0.45	0.55	0.50	0.45	0.50	0.65

VARIABLES	(1) Good Info Full Sample	(2) Good Info Coethnic (70)	(3) Good Info Non-Coethnic (70)	(4) Bad Info Full Sample	(5) Bad Info Coethnic (70)	(6) Bad Info Non-Coethnic (70)
Treatment	-0.01	0.01	-0.05	-0.03*	-0.03	-0.05*
	(0.02)	(0.03)	(0.04)	(0.02)	(0.02)	(0.03)
Constant	0.70***	0.72***	0.71***	0.72***	0.73***	0.71***
	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)
Observations	142	72	70	106	59	47
R-squared	0.45	0.58	0.57	0.45	0.44	0.71

VARIABLES	(1) Good Info Full Sample	(2) Good Info Coethnic (90)	(3) Good Info Non-Coethnic (90)	(4) Bad Info Full Sample	(5) Bad Info Coethnic (90)	(6) Bad Info Non-Coethnic (90)
Treatment	-0.01	0.01	-0.03	-0.03*	-0.04	-0.05*
	(0.02)	(0.05)	(0.03)	(0.02)	(0.02)	(0.02)
Constant	0.70***	0.73***	0.70***	0.72***	0.74***	0.71***
	(0.02)	(0.05)	(0.02)	(0.02)	(0.02)	(0.02)
Observations	142	48	94	106	54	52
R-squared	0.45	0.57	0.51	0.45	0.35	0.64

Dependent variable is the voter turnout rate in the village/quartier. This table presents results using different cutpoints for defining a village as a coethnic village. In the top panel, villages are coded as coethnic if over 50 percent of survey respondents are coethnics of the incumbent. In the middle panel, villages are coded as coethnic if over 70 percent of survey respondents are coethnics of the incumbent. In the bottom panel, villages are coded as coethnic if over 90 percent of survey respondents are coethnics of the incumbent. Robust standard errors clustered by commune-treatment are in parantheses. Each model uses block fixed effects. *** p<0.01, ** p<0.05, * p<0.1

Table A.8: Voter Turnout: Treatment Effects by Ethnic Connection and Level of Performance, Survey Data

VARIABLES	(1) Good Info Full Sample	(2) Good Info Coethnic	(3) Good Info Non-Coethnic	(4) Bad Info Full Sample	(5) Bad Info Coethnic	(6) Bad Info Non-Coethnic
Treatment	-0.01	0.00	-0.02	0.04**	0.03*	0.03
	(0.03)	(0.04)	(0.03)	(0.02)	(0.02)	(0.03)
Constant	0.89***	0.88***	0.90***	0.87***	0.88***	0.89***
	(0.03)	(0.04)	(0.02)	(0.01)	(0.02)	(0.03)
Observations	1,906	1,176	715	1,507	1,051	453
R-squared	0.12	0.13	0.12	0.07	0.09	0.08

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

A.8 Multiple Comparisons Corrections

In our pre-analysis plan, we specified that we would first present unadjusted p values and then, for a family of hypotheses nested within one of the main theories, we would adjust p values for the multiple comparisons within that family. The key argument in this study about the moderating effect of coethnicity with the incumbent falls under our first family of hypotheses about the effect of information on voter behavior. Within this family, we registered 11 hypotheses. This subsection adjusts p values to account for these multiple comparisons.

Our pre-analysis plan specifies that we adjust p values using a false discovery rate (FDR) correction to control the Type-1 error rate, and that we control the FDR at the 0.05 significance level. This method requires ordering the tests from smallest to largest nominal p value. Tables A.9 and A.10 do this for 9 of the 11 hypotheses in this family on the effect of good performance information and bad, respectively.[1]

For the good performance information tests, none survive the p value adjustment exercise. The interaction with coethnicity ($p = 0.095$) as the second smallest p value, for example, would require $p < .05/2 = .025$ to be considered statistically significant. That said, the implications from the previous section are reinforced here: coethnicity remains the most statistically significant moderator of good performance information. For the poor performance information tests, the first three hypotheses survive the multiple comparison corrections. The test of coethnicity does not; as the fifth most significant test, the p value would have to be less than 0.01 for it to be considered statistically significant.

Although we report these multiple comparison corrections for transparency, we also recognize they are very conservative by design, especially when accounting for the entire universe of possible tests in an experiment. We are reassured that the patterns we see in the survey data with respect to the coethnicity argument are almost exactly replicated in the same analyses using a different dataset. If random chance produced the relatively small p value on the coethnicity interaction term, then the probability that that same, relatively robust pattern shows up in a completely different dataset is exceedingly small.

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[1]The test of the argument about the conditioning effect of list order of the incumbent drops out due to insufficient variation in the data. The test of the argument about the conditioning effect of other media outlets also drops out because of poor data quality on accessible radio stations in each village.

Table A.9: Comparing Outcomes within the Same Family of Hypotheses, Good Performance Information

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7) Turnout	(8)	(9)
Treatment	0.067*	0.007	0.114**	0.105	0.055	0.011	-0.011	0.083*	0.062*
	(0.056)	(0.894)	(0.014)	(0.118)	(0.208)	(0.868)	(0.703)	(0.090)	(0.069)
Treatment x Incumbent Coethnicity		0.110*							
		(0.095)							
Coethnic with incumbent		-0.124**							
		(0.041)							
Treatment x Female				-0.095					
				(0.118)					
Female				0.077					
				(0.166)					
Treatment x Incumbent Preference				-0.006					
				(0.587)					
Preference for Incumbent Party				-0.002					
				(0.877)					
Treatment x Education					0.003				
					(0.601)				
Years of education					-0.003				
					(0.595)				
Treatment x Positive Prior						0.031			
						(0.694)			
Positive Prior						-0.059			
						(0.385)			
Treatment x Electoral Competition								-0.014	
								(0.845)	
Electoral Competitiveness								-0.065	
								(0.440)	
Treatment x Personal Help									-0.008
									(0.957)
Received Help									0.031
									(0.826)
Constant	0.448***	0.517***	0.409***	0.399***	0.459***	0.547***	0.890***	0.476***	0.451***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	1,672	1,657	1,671	886	1,667	810	1,906	1,660	1,657
R-squared	0.181	0.184	0.182	0.209	0.181	0.239	0.115	0.181	0.180

Robust standard errors clustered by village; uncorrected p values in parentheses. Models include block fixed effects.
*** p<0.01, ** p<0.05, * p<0.1. Incumbent vote share is the DV in all but model 7.

Table A.10: Comparing Outcomes within the Same Family of Hypotheses, Poor Performance Information

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7) Turnout	(8)	(9)
Treatment	-0.086	-0.169*	-0.116**	-0.007	-0.008	-0.095	0.039**	-0.140*	-0.090
	(0.121)	(0.059)	(0.050)	(0.897)	(0.904)	(0.105)	(0.011)	(0.094)	(0.124)
Treatment x Incumbent Coethnicity		0.124							
		(0.187)							
Coethnic with incumbent		-0.107							
		(0.236)							
Treatment x Female			0.067						
			(0.288)						
Female			0.004						
			(0.937)						
Treatment x Incumbent Preference				-0.006					
				(0.519)					
Preference for Incumbent Party				0.012					
				(0.155)					
Treatment x Education					-0.020***				
					(0.005)				
Years of education					0.013**				
					(0.038)				
Treatment x Positive Prior						-0.249***			
						(0.003)			
Positive Prior						0.135*			
						(0.066)			
Treatment x Electoral Competition								0.121	
								(0.297)	
Electoral Competitiveness								-0.100	
								(0.343)	
Treatment x Personal Help									0.023
									(0.858)
Received Help									-0.068
									(0.524)
Constant	0.572***	0.642***	0.570***	0.463***	0.514***	0.543***	0.875***	0.617***	0.578***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	1,358	1,356	1,358	632	1,324	631	1,507	1,330	1,357
R-squared	0.243	0.246	0.247	0.267	0.244	0.238	0.074	0.251	0.245

Robust standard errors clustered by village; uncorrected p values in parentheses. Models include block fixed effects.
*** p<0.01, ** p<0.05, * p<0.1. Incumbent vote share is the DV in all but model 3.

A.9 Addressing differential attrition and response bias in survey

Using data from the endline survey to estimate treatment effects is subject to two potential problems of bias. First, there was substantial attrition between the baseline and endline surveys and differential attrition patterns across treatment and control could lead us to make biased inference. Second, the question of whether and for whom the respondent voted for is subject to social desirability bias. Depending on the respondent's perception of the enumerator and research project's partisan leanings, they may have been tempted to dissemble when answering the vote choice question. Our use of administrative data to evaluate impacts of treatment – and the similarity in findings across data sources mitigates the potential problems introduced by these sources of bias. However, we explore here the extent of the problems in the survey data and the direction of the bias potentially introduced.

A.9.1 Differential attrition

It is unsurprising that, with about half of the participants surveyed in person at baseline being unresponsive or unavailable for the telephone endline survey, the group of individuals who did participate would be different than those who attrited. Indeed, as shown in columns 1-2 of Table A.11, the endline participants are more likely to be male, better educated, and wealthier.[2] Although these differences limit the generalizability of our findings to a more specific subset of Beninese, they do not necessarily imply problems of bias for making causal inference. That said, there is differential attrition across treatment and control groups that could induce such bias.

Examining key covariates collected on participants at baseline, we find that women are significantly more likely to attrit in treatment than in control and coethnics are significantly less likely to attrit in treatment than in control (see columns 3-6 of Table A.11). To the extent there is gender balance across ethnic groups, we are less concerned about our main findings being threatened by the first pattern; they could, however, be subject to bias as a result of the second.

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[2]Here, priors are measured on a 4-point scale where higher numbers indicate beliefs about better legislative performance. *Good News* is a binary indicator of whether the commune's deputy scored better than the local average on our performance index used in the treatment.

Table A.11: 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	(7) Endline	(8) Vote choice
Treatment	-0.039	-0.025	-0.077***	-0.062**	0.037	0.035	-0.068	-0.060
	(0.035)	(0.032)	(0.029)	(0.028)	(0.035)	(0.035)	(0.051)	(0.041)
Female	0.043***	0.060***	-0.008	0.026				
	(0.017)	(0.017)	(0.027)	(0.029)				
Coethnic with incumbent	-0.008	0.008			0.074**	0.077**		
	(0.026)	(0.026)			(0.035)	(0.034)		
Years of education	-0.006***	-0.004*						
	(0.002)	(0.002)						
Urban	-0.069	-0.061						
	(0.100)	(0.091)						
Poverty Level	0.042**	0.051***						
	(0.016)	(0.016)						
Positive Prior	0.016	0.001					0.002	-0.014
	(0.018)	(0.017)					(0.020)	(0.020)
Positive Prior x Good News	-0.015	-0.015					0.015	-0.003
	(0.024)	(0.024)					(0.044)	(0.044)
Treatment x Female			0.094***	0.067**				
			(0.030)	(0.032)				
Treatment x Coethnicity					-0.110***	-0.103***		
					(0.040)	(0.038)		
Treatment x Positive Prior							0.032	0.033
							(0.038)	(0.038)
Treatment x Good News							0.108	0.056
							(0.139)	(0.137)
Treatment x Positive Prior x Good News							-0.037	-0.013
							(0.045)	(0.046)
Constant	0.451***	0.517***	0.471***	0.515***	0.420***	0.479***	0.410***	0.533***
	(0.059)	(0.057)	(0.025)	(0.024)	(0.028)	(0.029)	(0.074)	(0.072)
Observations	2,698	2,698	6,128	6,128	6,072	6,072	2,721	2,721
R-squared	0.129	0.109	0.121	0.101	0.115	0.094	0.121	0.099

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

We consider how the differential patterns of attrition with respect to ethnicity might bias the direction of our results. Among coethnics, treated individuals are less likely to attrit than people in the control group so these treated respondents are more representative of the population. The more narrow set of control group participants, being more urban, wealthy and more educated on average, may be more likely to vote for the incumbent either because they already know the incumbent is a good performer or because economic voting would lead to greater pro-incumbency among the wealthy. Both possibilities would bias against finding a treatment effect among good news communes; toward finding an effect in bad news communes. On the other hand, the narrower set of control participants may also be more likely to be critical of the incumbent – more educated citizens are often found to be more distrusting of government producing greater anti-incumbency. This possibility would bias us toward finding a treatment effect in good news communes; against finding an effect in bad news communes.

It is not obvious which direction we should expect the bias go in, so we control for predictors of attrition in the aggregate to mitigate the imbalance across the samples – at least on observable characteristics. Table A.12 shows that our main findings from Table **??** are robust to adding these controls.

Table A.12: Replicating Main Results With Controls for Predictors of Attrition

VARIABLES	(1) Good Info Full Sample	(2) Good Info Coethnic	(3) Good Info Non-Coethnic	(4) Bad Info Full Sample	(5) Bad Info Coethnic	(6) Bad Info Non-Coethnic
Treatment	0.07*	0.12***	0.00	-0.08	-0.03	-0.19**
	(0.04)	(0.04)	(0.06)	(0.06)	(0.05)	(0.09)
Female	-0.00	-0.01	0.02	0.06**	0.08***	0.01
	(0.02)	(0.03)	(0.04)	(0.03)	(0.03)	(0.05)
Years of education	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Poverty Level	-0.02	-0.03	0.02	-0.04	-0.06*	0.02
	(0.02)	(0.02)	(0.04)	(0.03)	(0.03)	(0.05)
Constant	0.45***	0.41***	0.49***	0.54***	0.50***	0.61***
	(0.03)	(0.04)	(0.05)	(0.06)	(0.05)	(0.10)
Observations	1,667	1,028	625	1,324	914	408
R-squared	0.18	0.22	0.19	0.24	0.28	0.23

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

A.9.2 Social desirability bias

We do find some evidence of potential response bias in reporting vote share for the incumbent in our survey measure. In treated areas that received bad news about the incumbent, the reported vote share is 8 percentage points lower than the official vote share – a pattern consistent with under-reporting of votes for the incumbent due to having received bad performance information about the incumbent. Similarly in good news areas, there is evidence consistent with over-reporting of votes for the incumbent.

These patterns are consequential for making inference about the overall effect of treatment from the survey data. However, if response bias affects ethnic groups equally, then the main findings of the study that compare outcomes by ethnicity are less subject to concerns about biased interpretation. To test whether ethnic groups differentially generate response bias in the reporting of vote share for the incumbent, we test whether the coethnicity of the enumerator conditions outcomes. The idea here is that the main reason coethnics might dissemble at different rates is because they perceive the normatively correct answer differently depending on who is asking the question. For instance, a respondent might be more likely to falsely report voting for a high-performing coethnic incumbent if the enumerator is also a coethnic because the norm of rewarding a coethnic politician is more likely to be reinforced when speaking with another coethnic.

Table A.13 demonstrates that coethnicity mostly does not condition reporting of vote share for the incumbent. In tests of the full sample of good news or bad news areas, we interact enumerator coethnicity with incumbent coethnicity (which is what we think should matter most). The coefficient on enumerator coethnicity and on its interaction terms with incumbent coethnicity are not close to statistical significance in these models (columns 1 and 4). This alleviates the concern that differential response bias by ethnicity on the survey could be driving the main results in the study. That said, when we divide the sample into whether respondents are coethnics of the enumerator, we see some evidence that enumerator coethnicity matters in column 5. Here, irrespective of treatment group, coethnics with the enumerator are more likely to report voting for the incumbent even after receiving bad news. Although this is some indication of the existence of response bias, the difference in bias across coethnics and non-coethnics of the incumbent is not statistically significant as we see in column 4 which mitigates the concern.
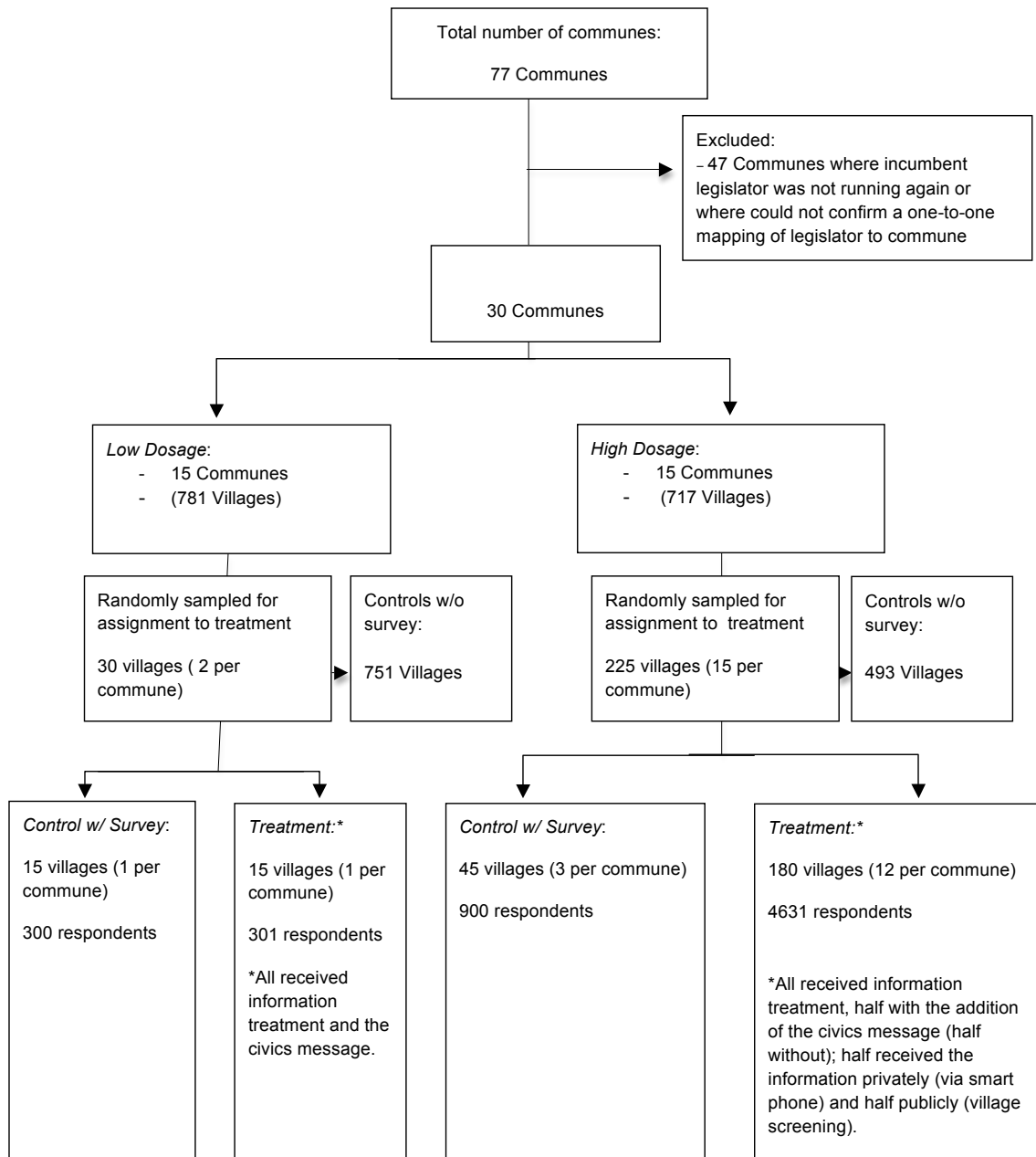
Table A.13: Is Reported Vote Choice Conditioned by Enumerator Coethnicity?

VARIABLES	(1) Good Info Full Sample	(2) Good Info Coethnic	(3) Good Info Non-Coethnic	(4) Bad Info Full Sample	(5) Bad Info Coethnic	(6) Bad Info Non-Coethnic
Treatment	0.02	0.12**	0.03	-0.16	-0.01	-0.16
	(0.06)	(0.06)	(0.06)	(0.11)	(0.08)	(0.10)
Coethnic with Enumerator	0.13	-0.02	0.16	0.02	0.21*	0.00
	(0.12)	(0.10)	(0.13)	(0.14)	(0.12)	(0.15)
Treatment x Enumerator Coethnicity	-0.13	-0.01	-0.13	-0.04	-0.07	-0.07
	(0.13)	(0.10)	(0.13)	(0.13)	(0.10)	(0.14)
Coethnic with incumbent	-0.10			-0.16		
	(0.08)			(0.12)		
Treatment x Incumbent Coethnicity	0.08			0.14		
	(0.09)			(0.13)		
Enumerator Coethnicity x Incumbent Coethnicity	-0.15			0.11		
	(0.16)			(0.16)		
Treatment x Enumerator Coethnicity x Incumbent Coethnicity	0.15			-0.04		
	(0.17)			(0.17)		
Constant	0.50***	0.41***	0.47***	0.64***	0.46***	0.60***
	(0.05)	(0.05)	(0.06)	(0.11)	(0.08)	(0.10)
Observations	1,657	1,030	627	1,356	948	408
R-squared	0.19	0.22	0.19	0.25	0.28	0.23

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

B Further Details on Experimental Design

B.1 Geographic Sampling and Randomization Procedures

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                    ┌─────────────────────────────┐
                    │  Total number of communes:  │
                    │                             │
                    │       77 Communes           │
                    └─────────────────────────────┘
                                 │
                                 │          ┌──────────────────────────────────┐
                                 │          │ Excluded:                        │
                                 ├─────────►│ – 47 Communes where incumbent    │
                                 │          │ legislator was not running again │
                                 │          │ or where could not confirm a     │
                                 │          │ one-to-one mapping of legislator │
                                 │          │ to commune                       │
                                 │          └──────────────────────────────────┘
                    ┌─────────────────────────────┐
                    │       30 Communes           │
                    └─────────────────────────────┘
```

Low Dosage:	*High Dosage*:
- 15 Communes	- 15 Communes
- (781 Villages)	- (717 Villages)

Randomly sampled for assignment to treatment	Controls w/o survey:	Randomly sampled for assignment to treatment	Controls w/o survey:
30 villages (2 per commune)	751 Villages	225 villages (15 per commune)	493 Villages

Control w/ Survey:	*Treatment:**	*Control w/ Survey*:	*Treatment:**
15 villages (1 per commune)	15 villages (1 per commune)	45 villages (3 per commune)	180 villages (12 per commune)
300 respondents	301 respondents	900 respondents	4631 respondents
	*All received information treatment and the civics message.		*All received information treatment, half with the addition of the civics message (half without); half received the information privately (via smart phone) and half publicly (village screening).

Note: **Randomized 30 communes to high/low dosage, blocking on North/South, high/low incumbent performance. After randomly sampling villages within each commune, randomly assigned villages to treatment/control within three blocks (nested within communes): urban, rural/electorally competitive, rural/electorally uncompetitive.**

B.2   Survey Sampling Procedures

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

In each Private village, 20 people were randomly selected both to take the survey and to receive the intervention, 20 people were randomly selected only to receive the intervention, and 10 people were randomly selected to serve as control individuals and thus took the survey but received no intervention. In Public villages, 20 people were randomly selected to take the survey and be invited to receive the intervention at a public workshop, and an additional 40 people were randomly selected only to be invited to receive the intervention. We sampled in this way so as to ensure that roughly the same number of people would be treated in both Private and Public villages.[4]

B.3   Additional Notes on Ethical Considerations

In designing the experiment, we followed the principles agreed to by the entire Metaketa Initiative, as noted in the article. We obtained permission from the traditional authority in each village and also individually from all subjects who participated in the survey (in both treatment and control villages) and received the information intervention. Important, we also sought and obtained permission from the President of the National Assembly, an actor who could evaluate risks to the candidates running the communes in the experimental sample. Following the guidelines for the Metaketa initiative, we did not seek consent from every individual legislator. Because the purpose

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[3]According to the most recent round of Afrobarometer surveys in Benin (2014), approximately 77% of Beninois own a cell phone (http://www.afrobarometer.org). Still, respondents were not required to own a cell phone to participate in our intervention. The cell phone to which they had access could belong to a friend or relative.

[4]On average, 55 individuals in Public villages attended the video screenings (range from 20 to 70), indicating a reasonable balance of treated individuals across Public and Private villages.

of the study was 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. To ensure that the information we distributed was objective, context-appropriate and reliable, we utilized data collected by the legislature itself long before any knowledge of its use in an intervention like ours. We note that this data is meant to be publicly available.

B.4 Treatment Conditions

In addition to randomizing whether voters received legislative performance information or not, our study design manipulated the content of the treatment message to vary the signal of the importance of the legislative performance dimension. Treated participants were shown a video with either *only* the information about relative legislator performance (*Info Only*), or that same information *plus* an additional message highlighting the importance of legislative performance to voter welfare (*Civics*).[5]

We also varied the method by which the information was disseminated. Treated participants received the intervention either privately by watching a video on a smartphone in the respondent's household (*Private*) or publicly through the screening of the same video via a projector in a public location in the village or quarter (*Public*).

In addition, we varied dosage — the density of treated villages in the commune. Participants were told during the intervention how many other villages in their commune were receiving legislative performance information. In high dosage communes, we randomly assigned 3 villages to each of the four combinations of content plus dissemination method (Info-Only/Private, Info-Only/Public, Civics/Private, Civics/Public), for a total of 12 villages treated with legislative performance information. In low-dosage communes, we assigned only one village to treatment (Civics/Public).

In high dosage communes, we implement a factorial design with four treatment conditions (Info-Only/Private, Info-Only/Public, Civics/Private, and Civics/Public) and a control group. Because our pre-specified hypotheses make predictions about how ethnicity conditions the effect of

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[5]For those in the Civics experimental condition, the video contained a civics message that first described the main responsibilities of legislative deputies, namely, their responsibility for legislation, executive oversight and representation. It then provided three concrete examples of how legislative performance (or lack thereof) can impact voter welfare. A positive example of good legislation was the passage of an anti-graft law requiring public servants to disclose assets. A negative example of a missed opportunity was the failure of the legislature to vote on and pass a health insurance scheme that was proposed in 2008. Finally, a positive example of executive oversight detailed how the legislature opposed changes to the Constitution proposed by the president that would expand his power. The *Civics* treatment was designed to encourage voters to switch to voting along a legislative performance dimension. The text of the video script for the Civics condition can be found in the appendix.

any of these treatments, we collapse these categories into one *Treatment* group in the main analyses of this article. We introduce these distinctions here in order to be clear about our experimental design and because, as we detail below, these different treatments become useful in that they allow us to test competing explanations for the main results we present.

B.4.1 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 CEPRODE 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.4.2 Civics Condition Script (in English)

NOTE: The second half of this script is identical to the information only *script.*

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.

But, 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

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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 CEPRODE 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.4.3 Dosage Treatment

Our sample included the 30 communes for which we could strongly confirm a one-to-one mapping of commune to incumbent candidate. Of those 30, we randomly assigned 15 to each dosage category (high or low).[6] Within communes, the unit of randomization was the rural village or urban quarter. These units are the lowest level of social and territorial organization.

Within each low dosage commune, we randomly select two villages and then randomly assign one to Civics/Public and the other to control. Because in each high-dosage commune 12 villages were assigned to treatment and three to control, and in each low-dosage commune one village was assigned to treatment and one to control, the sample comprises 225 villages in high-dosage communes and 30 villages in low-dosage communes, for a total of 255.

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[6]When assigning dosage, we blocked on incumbent legislative performance, which is observed at the commune level and on region (north/south). Within 4 blocks (high and low performance in the north and south) of communes, we assigned half to high-dosage and half to low-dosage treatment.

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 quartiers 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 video.

B.4.4 Linking the Results to the Companion Paper

Appendix B.4 describes each of the treatment arms in our experimental design. In a companion paper, we present the full results from this design. The analysis produces two main results. First, we find that in low dosage communes, positive performance information actually had a *negative* impact on the vote share of the incumbent. Second, we find that in high dosage communes, positive performance information has the expected positive effect in the civics treatment condition (the *Civics* treatment was designed to encourage voters to value the legislative performance dimension). The first finding was unanticipated; the second, consistent with our expectations.

The interpretation of the results centers on the notion that politicians engage in different types of activities and that these dimensions of performance may be perceived as substitutes rather than complements in the minds of voters. In our context, legislators generally engage in legislative activities and constituency service that involves the provision of local collective and private goods to voters. We leverage qualitative evidence collected in Benin that supports the interpretation that voters in the experiment perceived "good" legislative performance information as "bad" news about constituency service (which they ultimately care most about), unless the information is distributed widely across the commune and in combination with the civics treatment.

In this Appendix section, we discuss how the results in this study relate to those presented in the companion paper. First, we discuss the results in low dosage communes (communes with only one treated village), where positive performance information reduced the vote share of incumbents in treated relative to control villages. In this treatment condition, positive information generated a negative response to the incumbent. In the current paper, we show that, aggregating across treatment conditions, negative information has a negative impact on vote share when provided to non-coethnics of the incumbent, but no impact on incumbent vote share when the information was provided to the incumbent's coethnics. If the interpretation of legislative and constituency service dimensions as substitutes is true, we should expect that *the negative effect of positive information in low dosage communes will be larger (in absolute value) among non-coethnics and close to zero among coethnics* (because only non-coethnics punish negative information). Table B.1 shows that this is indeed the case. In low dosage, positive information communes, the average treatment effect is negative.[7] Columns 2 and 3 show that this negative effect is driven by a large negative effect among non-coethnic villages.[8]

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[7]In the companion paper, we use all villages/quarters in a commune as the control group (even those where we did not conduct surveys), which increases our statistical power. The negative coefficient presented in that paper is statistically significant. In this paper, we can only analyze the sample where baseline surveys were conducted because we only have ethnicity information for these villages/quarters.

[8]Due to the small sample sizes, the p-values should be interpreted with caution. We emphasize the direction and the magnitude of the effects rather than their statistical significance.

Table B.1: Treatment Effects by Ethnic Connection and Level of Performance, Official Results Data

VARIABLES	(1) Good Info Full Sample	(2) Good Info Coethnic (70)	(3) Good Info Non-Coethnic (70)	(4) Good Info Coethnic (90)	(5) Good Info Non-Coethnic (90)
Treatment	-0.06	0.05	-0.36*	0.13	-0.17
	(0.13)	(0.17)	(0.16)	(0.26)	(0.23)
Constant	0.53***	0.47***	0.63***	0.53**	0.49**
	(0.09)	(0.12)	(0.09)	(0.19)	(0.14)
Observations	12	7	5	5	7
R-squared	0.84	0.91	0.97	0.90	0.84

The sample only includes low dosage, good news communes.

The second set of main results shows that information has the expected effect when it is distributed widely across the commune and voters are told as much (high dosage) *and* the civics treatment is included. Tables B.2 and B.3 thus present the coethnicity analyses using only data from the control group and the high dosage/civics treatment arm. The results are consistent with, and in some instances stronger than (in terms of the magnitude of the effects),[9] the results presented in the main body of this paper.

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[9]The results are generally less statistically strong because of the much smaller sample sizes.

Table B.2: Effect of *Civics* Treatment by Ethnic Connection and Level of Performance in High-Dosage Communes, Official Results

VARIABLES	(1) Good Performance Coethnic (50)	(2) Good Performance Non-Coethnic (50)	(3) Bad Performance Coethnic (50)	(4) Bad Performance Non-Coethnic (50)
Treatment	0.090	-0.044	0.008	-0.276
	(0.058)	(0.097)	(0.092)	(0.206)
Constant	0.467***	0.474***	0.488***	0.579***
	(0.045)	(0.060)	(0.082)	(0.160)
Observations	46	29	38	15
R-squared	0.778	0.594	0.769	0.750
VARIABLES	(1) Good Performance Coethnic (70)	(2) Good Performance Non-Coethnic (70)	(3) Bad Performance Coethnic (70)	(4) Bad Performance Non-Coethnic (70)
Treatment	0.061	-0.001	0.054	-0.193
	(0.077)	(0.101)	(0.074)	(0.155)
Constant	0.446***	0.505***	0.499***	0.514***
	(0.059)	(0.067)	(0.065)	(0.126)
Observations	37	38	31	22
R-squared	0.832	0.629	0.823	0.684
VARIABLES	(1) Good Performance Coethnic (90)	(2) Good Performance Non-Coethnic (90)	(3) Bad Performance Coethnic (90)	(4) Bad Performance Non-Coethnic (90)
Treatment	0.107	0.039	0.059	-0.192
	(0.090)	(0.080)	(0.074)	(0.120)
Constant	0.399***	0.483***	0.500***	0.516***
	(0.069)	(0.056)	(0.065)	(0.096)
Observations	25	50	28	25
R-squared	0.849	0.605	0.864	0.691

The sample only includes control and civics condition units in high dosage. This table presents results using different cutpoints for defining a village as a coethnic village. In the top panel, villages are coded as coethnic if over 50 percent of survey respondents are coethnics of the incumbent. In the middle panel, villages are coded as coethnic if over 70 percent of survey respondents are coethnics of the incumbent. In the bottom panel, villages are coded as coethnic if over 90 percent of survey respondents are coethnics of the incumbent. Robust standard errors clustered by vilage are in parantheses. Each model uses block fixed effects. *** p<0.01, ** p<0.05, * p<0.1

Table B.3: Effect of *Civics* Treatment by Ethnic Connection and Level of Performance in High-Dosage Communes, Survey Data

VARIABLES	(1) Good Performance Coethnic	(2) Good Performance Non-Coethnic	(3) Bad Performance Coethnic	(4) Bad Performance Non-Coethnic
Treatment	0.132**	0.003	-0.020	-0.243**
	(0.051)	(0.065)	(0.051)	(0.112)
Constant	0.381***	0.516***	0.546***	0.555***
	(0.045)	(0.050)	(0.044)	(0.095)
Observations	543	336	473	227
R-squared	0.201	0.185	0.323	0.227

Robust standard errors in parentheses

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

The sample only includes control and civics condition units in high dosage.

C   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, for the conditional analyses of treatment effects by coethnicity at the village level, we did not pre-specify a threshold at which to establish a coethnic vs. a non-coethnic village when using the administrative data. We thus demonstrate robustness of the analysis to three reasonable threshold levels.

Second, in the PAP, we pre-specify an interaction specification for tests of conditional hypotheses. When we evaluate ethnicity as a moderator of information processing in this paper, we include sub-sample analyses by coethnicity in the main body of the paper and the pre-specified interaction analysis in the appendix because the former specification provides a more direct tests of the predictions of the three alternative explanations that form our theoretical framework.

Third, the analyses of the comprehension test data were not pre-specified. We exploit this data to provide additional evidence in favor of the mechanism of motivated reasoning – which is the argument that our pre-specified analyses support. Our results would stand without this additional test, but we show it to provide additional confirmation from an alternative source that ethnicity is indeed motivating how respondents process information in the comprehension survey.

Finally, in the robustness test in Appendix Table A.2, we do not include a control variable for whether or not the respondent is an opinion leader because the raw data we collected on that variable was insufficient to construct a usable measure of opinion leader status.