

How Coethnicity Moderates the Effect of Information On Voting Behavior: Experimental Evidence from Benin*

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Abstract

Scholars have argued that poor information and ethnic politics both pose significant challenges to democratic accountability. If voters do not have access to information allowing them to discriminate between good and bad performers, or if voters are motivated by expressive ethnic voting, they are unable or unwilling to vote poorly-performing politicians out of office. In this paper, we investigate the relationship between ethnicity, performance information, and voting with a field experiment around Benin's 2015 legislative elections. We collect both attitudinal and behavioral data to investigate if and why ethnicity moderates the relationship between information access and voting behavior. Our results indicate that voters reward good performers only if they are coethnics, and punish bad performers only if they are non-coethnics. We further show that coethnics are more likely to accurately recall performance information if it is positive, while they are less likely to accurately recall it if it is negative. Taken together, our results suggest that ethnic identity conditions, via motivated reasoning, how voters process and act on information about politics. These findings improve our theoretical understanding of ethnic politics, contribute new attitudinal and behavioral evidence for how it manifests in a real election, and contribute to research on the relationship between information and accountability.

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Many of the world's democracies are plagued by poor electoral accountability. Two prominent explanations for this trend are that voters do not have access to information that might allow them to vote poorly-performing politicians out of office, and that voters are motivated by coethnicity, regardless of politician performance. On the former, access to information is central to most models of electoral accountability (Pande, 2011; Fearon, 1999). If voters lack information about politicians' performance, they are unable to reward or punish incumbents for their performance while in office. Ethnic politics can be equally problematic. For instance, if voters support their ethnic kin for expressive reasons, they may forgive poor performance from coethnics (Long and Gibson, 2015) and give politicians little incentive to perform well while in office. In this paper, we examine the relationship between these two potential sources of weak accountability. Does access to information about politician performance weaken or amplify voter preferences for coethnic candidates?¹

The existing literature offers conflicting answers to this question, in part because it does not test the potential mechanisms linking ethnicity, information, and voting.² In this paper, we gain a more precise understanding of the challenges to democratic accountability by laying out and testing three such alternative mechanisms in the context of a real election in Benin. First, we consider the possible informational value of the ethnic label. Some scholars have argued that in contexts where access to other sources of political information is limited, voters are likely to rely on ethnicity as a signal about the qualities and likely performance of candidates (Ferree, 2006, 2010; Conroy-Krutz, 2013). For this body of literature, ethnic voting arises because of information scarcity. Expand access to information about politicians and their performance, and the importance

¹This paper is part of an experimental study on information and accountability in Benin. In a companion paper, we present the main results that fall from the full experimental design (Adida et al., 2015). In this paper, we focus on information and ethnic politics. In our pre-analysis plan, we specified that we would test how coethnicity with the incumbent moderates the impact of information. We also specified hypotheses about other moderators of the impact of information on voting behavior. We focus on ethnicity here because it is of most theoretical interest and because it allows us to speak to the ethnic politics literature, in addition to the literature on information and accountability. In Appendix F.3, we present results on these other potential moderators.

²Ferree (2006) offers one of the first systematic tests of mechanisms underlying co-racial voting in South African elections. We see our paper as contributing to this effort in two ways: first, we bring new, field experimental evidence to bear on adjudicating among mechanisms linking information, coethnicity, and voting; second, we add a motivated reasoning hypothesis, which is new for the comparative politics ethnicity literature.

of ethnic ties to voting behavior should decrease for all voters. Second, we consider the possible instrumental reasons for ethnic voting. If expectations of ethnic favoritism are sufficiently strong, voters may expect to benefit only from the performance of their coethnics, leading them to respond to performance information about their coethnics and to ignore performance information about non-coethnics (Carlson, 2015). Third, we consider the possible effects of ethnicity on information processing. The literature on motivated reasoning illustrates that individuals often (subconsciously) process information in ways that conform to their pre-existing views, more readily accepting information that fits with their priors or reinforces their sense of self while scrutinizing and being more likely to discard information that does not (Bolsen, Druckman and Cook, 2014; Taber and Lodge, 2006; Kunda, 1990, 1987). In this case, voters may update their beliefs only when presented with positive information about coethnics and with negative information about non-coethnics.

We evaluate these competing expectations with data from a large-scale field experiment conducted around the 2015 National Assembly elections in Benin, a democratic West African country where the information environment is poor and ethnicity is highly salient. In the experiment, villages and urban neighborhoods were randomly assigned to receive information about the legislative performance of incumbent politicians running in the election.³ Individuals and villages in our sample vary in their ethnic connection to the incumbents in their area — some are coethnics with the incumbent while others are not — which allows us to examine how ethnic ties condition the impact of information access.

Using both behavioral data from official village- and neighborhood-level election results and panel survey data, we provide evidence that is most consistent with a theory of motivated reasoning. That is, we find that access to information amplified voter preferences for coethnic politicians and reinforced voter biases against non-coethnic candidates. Voters rewarded the good performance of their coethnics, but did not reward the good performance of non-coethnics. They punished the poor performance of non-coethnics, but did not punish the poor performance of coethnics. In short, positive information increases support for coethnics but has no impact on support for

³The informational intervention focuses specifically on the tasks for which deputies are formally charged. We discuss details of the information treatment in subsequent sections.

non-coethnics, while negative information decreases support for non-coethnics and has no impact on support for coethnics. We uncover these patterns in both panel survey data and in official administrative data, which alleviates concerns about social desirability bias and differential attrition in the survey.

To further probe the plausibility of the motivated reasoning mechanism, we analyze data from a comprehension survey that was conducted immediately following the provision of information. The results show that the incumbent's coethnics were more likely than non-coethnics to accurately recall the information provided in the experiment if the information was positive. When the information suggested that the incumbent was a poor performer, on the other hand, coethnics of the incumbent were substantially *less* likely to accurately recall the information. These additional results are consistent with the notion that ethnic identity conditions, via motivated reasoning, how voters process information about incumbent performance.

This study advances our theoretical understanding of ethnic voting and further explains its empirical manifestations; it also contributes a more precise account of the relationship between information and accountability. First, we advance theories of ethnic voting. This literature has often been characterized by debates about whether ethnicity drives political behavior for psychological or instrumental reasons. While our evidence does not rule out instrumental considerations, it does highlight an additional mechanism that has received less attention in the literature on African politics: the role of ethnic identity in conditioning how voters process and ultimately respond to information about politics. Second, we advance the empirical literature on ethnic voting by combining an experiment with data on behavioral outcomes from a real election. Common ways to test the relative importance of information about performance (or performance evaluations) and ethnic identity, as well as their interaction, are through the use of survey data and survey experiments. However, response bias in observational surveys poses a serious threat: many people seek to avoid appearing “tribal” on surveys (Carlson, 2016). Survey experiments can help with response bias and with causal identification (Adida, 2015; Adida, Davenport and McClendon, Forthcoming; Carlson,

2015; Conroy-Krutz, 2013),⁴ but the results may not generalize to real elections. Our methodological approach helps to overcome these limitations by analyzing experimental data about behavior in a real election. Finally, we contribute to the literature on information and accountability. That literature has so far uncovered mixed results as to whether and how access to performance information influences voter behavior. Our findings shed light on the puzzle by highlighting a key moderating factor, ethnic identity, that conditions how voters respond to interventions designed to enhance information access.

In what follows, we first offer a theoretical account of the mechanisms linking ethnicity, information, and voting. We then introduce our experimental design and strategy. Next, we present results showing that coethnicity enhances the effect of good performance with null effects for bad performance, while non-coethnicity does just the opposite: it enhances the effect of bad performance with null effects for good performance. We then further probe the mechanisms underlying these findings, and show results consistent with motivated reasoning. Finally, we conclude with implications for our understanding of ethnic voting and democratic accountability.

1 Ethnicity, Information, and Voting

Theories of political accountability generally posit that increased access to information about politician performance shapes voting behavior, helping voters to distinguish between strong and weak performers (Fearon, 1999; Pande, 2011). Yet empirical evidence to date suggests that the relationship between access to performance information and voting behavior is not straightforward. Some field experiments have found that providing performance information to voters does indeed result in the punishment of poorly performing politicians (Banerjee et al., 2011; Ferraz and Finan, 2008). But other experimental work has been unable to reject the null hypothesis that increased access to performance information has no effect on citizen behavior (Humphreys and Weinstein,

⁴Moreover, because information about performance may be endogenous to ethnicity — if, for example, people only seek out information that affirms their preference for coethnics — it becomes difficult to disentangle the importance of information versus identity, or to determine how ethnicity conditions the impact of information.

2012; Lieberman, Posner and Tsai, 2014). Some experimental studies have even found that the provision of performance information resulted in the punishment of challengers as well as incumbents (Chong et al., 2011). The literature clearly needs to turn its attention toward the moderators of the relationship between information and accountability.

Voter-politician coethnicity might be an important moderator. Ethnicity is understood here to mean a politically and socially constructed group identity, based on real or perceived descent (Chandra, 2006). A significant body of research documents that voters in many parts of the world, including in our study site, are more likely to vote for coethnic candidates than for non-coethnic candidates (e.g., Adida, 2015; Bratton and Kimenyi, 2008; Ferree, 2006; Heath, Verniers and Kumar, 2015; Hutchings and Valentino, 2004; Horowitz, 1985; Posner, 2005).⁵ However, there is considerable debate about the drivers of ethnic voting.

One strand of the literature stresses coethnicity as a heuristic. This research argues that, in low information environments, voters use coethnicity as a short-cut for evaluating candidates' preferences and performance (Conroy-Krutz, 2013; Ferree, 2006). In contrast to other candidate attributes, coethnicity is often easily observable (Chandra, 2007).⁶ People also tend to assume that members of their in-groups share their same preferences and are highly capable (Brady and Sniderman, 1985; Fiske, Cuddy and Glick, 2007; Ferree, 2010).⁷ Absent other information on candidates' preferences and performance, coethnicity might then serve as a low-cost signal that voters should support a given politician (Popkin, 1991).

Another strand of the literature stresses the instrumental value of supporting coethnic candidates. This research argues that voters expect coethnic candidates to favor them when delivering goods and services. Several studies have shown evidence of political ethnic favoritism when it comes to constituency service (e.g., Butler and Broockman, 2011; McClendon, Forthcoming) and

⁵Chauchard (2016) points out that ethnic cues may also affect voting when choosing among non-coethnics.

⁶This does not mean that voters always identify coethnics without error (Habyarimana et al., 2009; Harris and Findley, 2014). Instead, it means that relative to other politician characteristics, coethnicity is more easily surmised. Chandra (2007) points out that ethnicity is often determined on the basis of highly visible attributes such as name, physical features, speech and dress.

⁷There is some evidence that coethnics do share similar policy preferences under some circumstances (Lieberman and McClendon, 2013).

to the delivery of local public goods (Burgess et al., 2015; Kramon and Posner, 2013, Forthcoming; Franck and Rainer, 2012), so these expectations may not be unreasonable (c.f., Kasara, 2007). That voters hold these expectations may in turn mean that voters consider performance information only when that information is about coethnics (Carlson, 2015). Voters may value strong coethnic performers over weak coethnic performers but may care little about the performance records of non-coethnics since they are unlikely to benefit from non-coethnic performance once in office. Voters living in an area dominated by non-coethnics might be an exception (Ichino and Nathan, 2013). These voters might consider the performance record of a non-coethnic candidate who shares the dominant ethnicity of the local area because they expect that a strong non-coethnic performer would deliver local public goods to their area.

Finally, there is the possibility that candidates' ethnicity affects the way voters process information about them. Social identity theory argues that people derive self-esteem from seeing their groups do well relative to other groups (e.g. Tajfel, 1974; Lieberman, 2009). These "psychic benefits" from ingroup status may mean that voters are motivated to support coethnic candidates over non-coethnic candidates in order to see their in-group do well (Chandra, 2007; Kasara, 2007).⁸ This "motivated reasoning"⁹ might in turn mean that voters process performance information about coethnic politicians and about non-coethnic politicians differently (Bolsen, Druckman and Cook, 2014; Taber and Lodge, 2006). They might update their beliefs about coethnic candidates only when provided positive performance information about those candidates.

These existing arguments yield very different predictions about the interaction between voter-politician coethnicity and increased access to politician performance information. For instance, if coethnicity is simply a heuristic for evaluating performance in the absence of actual performance

⁸We do not spend much time discussing purely expressive theories of ethnic voting—that is, the idea that voters would ignore all performance information in order to vote for their in-group (Horowitz, 1985)—because the literature (e.g., Ferree, 2010; Long and Gibson, 2015; Carlson, 2015) has shown that ethnic voting is likely to be much more complex. In any case, our results do not seem consistent with purely expressive ethnic voting since performance information does have an effect on vote choice under certain conditions.

⁹A body of research, particularly in American Politics, calls this phenomenon motivated reasoning (see, Bolsen, Druckman and Cook, 2014; Taber and Lodge, 2006), but comparative politics has paid less attention to the cognitive reasoning functions of ethnicity. See, however, (Horowitz and Long, n.d.) for a recent exception examining the relationship between voter-politician coethnicity and "wishful thinking", as well as (Adida et al., 2016) for evidence consistent with non-rational coethnic voting in the case of coethnics of the spouse of Benin president Boni Yayi.

information, then increased access to information on politician performance should weaken the association between coethnicity and vote choice for everyone (Conroy-Krutz, 2013). Alternatively, if ethnic voting is about expected ethnic favoritism, then voters should be influenced by new performance information *only* when it is about coethnics (unless they live in an area dominated by non-coethnics). Last, if ethnicity affects information processing, then increased access to politician performance information should affect vote choice only when it conforms to existing coethnic biases (e.g., when positive performance information is provided to coethnics or when negative information is provided to non-coethnics). In other words, there are at least three hypotheses about how coethnicity might moderate the relationship between access to performance information and vote choice:

Hypothesis 1 *If coethnicity serves primarily as a heuristic for gauging performance, then increased access to actual performance information should weaken the association between coethnicity and incumbent support and should do so among all voters, regardless of their ethnic group memberships.*

Hypothesis 2 *If coethnicity serves primarily as a signal that voters will be favored, then increased access to performance information should influence vote choice only among voters who are coethnics of the candidate.*

- *However, ethnic minority voters may be influenced by access to information about non-coethnic candidates.*

Hypothesis 3 *If voters engage in motivated reasoning, then increased access to performance information should influence vote choice only when it is positive news about a coethnic or negative news about a non-coethnic.¹⁰*

¹⁰Our pre-analysis plan specifies that we will test how coethnicity with the incumbent conditions the impact of information. The specific hypotheses on ethnicity that we preregistered match this third hypothesis.

2 Empirical strategy

In this section, we provide context about the setting in which we deployed our experiment. We then develop our research design, and describe the data we collected and our measurement strategy.

2.1 Setting

We adjudicate between the differing hypotheses above about how coethnicity mediates the relationship between information and electoral behavior with a data collection effort in an African democracy, Benin, where the political salience of ethnicity has already been established (Wantchekon, 2003). We focus our analysis on an electoral race about which voters have relatively poor information – legislative elections – and thus where providing information about incumbent performance has a greater potential to cause voters to update their beliefs about the candidates running.

Benin has been considered a stable democracy since it first transitioned to holding free and fair elections in 1990. Deputies in the national assembly are elected in multi-member districts by proportional representation.¹¹ While some of the informal performance activities of these deputies, such as physical presence in the constituency or targeted transfers to the community, are observable to voters, the activities that deputies are formally slated to do – legislative and executive oversight duties – is much less visible to voters. Few media outlets report on legislative activity and information about legislative performance is not readily available. It is important to note that while voters in Benin care more about constituency service than they do legislative performance (Adida et al., 2015), most constituency service activities fall outside the realm of what legislators are formally tasked with doing – in large part because they are given no budget with which to make policy for, or provide services to, their specific constituency.

In a companion paper (Adida et al., 2015), we discuss the extent to which voters in Benin care about legislative performance compared to other activities in which legislators might engage, such

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

as individual- or village-targeted transfers. We find that at baseline voters in Benin clearly valued transfers over legislative performance and may even have considered legislative performance to be a substitute for transfers to the village. But, in many of the treatment conditions in our experiment, voters were successfully moved to care about legislative performance and to consider strong legislative performance to be “good news.”¹² In this paper, we consider whether their response to legislative performance information was further conditioned by coethnicity (or lack thereof) with the incumbent. For the sake of transparency and in order to use all of our data, we show results when analyzing behavior in all treatment conditions combined, compared to control. But we also confirm that our results hold in the subset of treatment conditions in which we can be *sure* that voters viewed legislative performance as a salient legislator activity (see Tables F.3 and F.4 in Appendix F.2).

A challenge of working within the setting of legislative elections in Benin is that representatives are elected via proportional representation and so the direct accountability link between voters and politicians is weaker than it would be in a single-member district. Information provided about one incumbent politician may have differential effects on voting depending on voter priors about the performance of other politicians in the same district. A singular feature of Benin’s legislative system helps mitigate this problem. Voters elect an average of 3.5 deputies per constituency, and with 77 total communes distributed among the constituencies, there are 3.2 communes per constituency on average. This makes feasible, as a rule of thumb for voters and legislators, a one-to-one mapping of communes to legislators. Indeed, in practice, each legislator focuses on and “takes care of” a particular commune within his constituency, thus facilitating a one-to-one correspondence of incumbent legislator to commune.¹³

¹²The field experiment consisted of 4 variants of treatment described in Appendix A, as well as variation in how many villages within an incumbent’s assigned commune were given the information. Voters in each treatment condition received the same relative performance information; what varied was whether the performance information included an additional message about the importance of legislative activity to voters’ wellbeing (a civics message), whether the information was provided publicly or privately, and how widely the information was disseminated in the incumbent’s constituency. Where a civics message was widely disseminated, we are confident that voters viewed strong legislative performance as important and positive.

¹³We note that this mapping in practice is consistent with expert evaluations of the party system in Benin as fragmented and weak. Parties are created and dismantled frequently, lack programmatic character, and reflect instead the personality of their founder(s) (Banégas 2003; Gazibo 2012). Pre-experiment focus groups also confirm that villagers

2.2 Experimental Design

To identify the causal effect of information conditional on ethnicity, we follow a recent series of studies that implement experimental designs in the field. Because of the inference problem produced by the fact that certain types of people are more likely to be politically informed, and thus vote differently from less informed voters, an experimental manipulation is advantageous. Furthermore, the level of voter information is a relatively simple construct to manipulate externally, and to do so in a way that avoids spillovers or violations of the independence of treatment assumption. In our particular case, we cluster treatment assignment within villages which is the unit at which information is most likely to travel. Cross-village information transmission is possible but less common, and would bias against our finding a treatment effect. While our conditioning variable, coethnicity, is not externally manipulated, we measure pre-treatment self-reports of coethnicity with the incumbent and assume ethnicity remains fixed during the period of study, e.g. we do not expect to see sorting across ethnicities as a consequence of our treatment.

Our experiment thus manipulates access to a village’s information about the incumbent legislative representative; details of the treatment are described below. Because of the challenge of attributing performance in the constituency to a specific legislator, we restrict our experimental sample to 30 communes in which our local partner organization firmly verified a one-to-one correspondence and in which the incumbent legislator was running again. We further verified the one-to-one correspondence in our baseline survey by asking respondents to identify pre-treatment the legislative deputy who is most responsible for their village.¹⁴ Because levels of performance and other incumbent characteristics vary dramatically across incumbent legislators contributing to potentially problematic heterogeneity across treatment effects, we conduct a within-legislator design in which villages within each of our 30 communes are randomly assigned to treatment and control conditions. When evaluating treatment effects, we thus take the across-commune average of within-commune effects. We explain our assignment strategy further below.

can name and agree on a single legislator as their incumbent representative.

¹⁴On average, 58.6% correctly identified the full name of their legislator.

Treated villages in the study were given information about the incumbent legislator's relative performance in the National Assembly in the form of a video. This mode of delivery ensured consistency in the wording and tone of the message across the sample while at the same time making the information accessible to people of all education levels, literate and illiterate.¹⁵ The video also approximates how media outlets might deliver information about candidates in a real-world setting. In the video, a male actor reads a script in a neutral tone, as a news caster or radio host might, and graphics illustrate key points. The text was recorded in French and then dubbed in local languages as necessary.

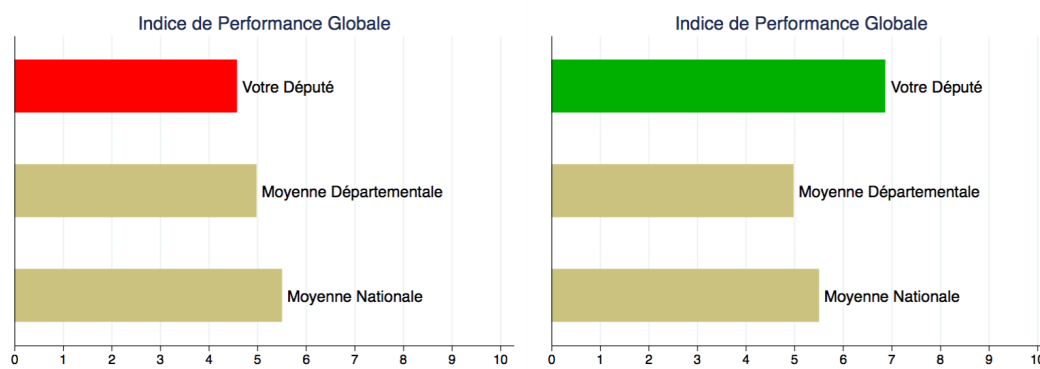
The information provided was drawn from official reports of the Office of the President of the National Assembly that, while supposedly public, required extensive time and effort to obtain. From the reports, the authors produced a set of relative performance indices drawn from a set of indicators about an incumbent legislator's: 1) rate of attendance at legislative sessions, 2) rate of posing questions during legislative sessions, 3) rate of attendance in committees, and 4) productivity of committee work (the number of laws considered by the committee). While the video provides raw data for each of these four indicators, it displayed graphics like those in Figure 1 of three key performance indices to increase comprehension and recall by participants: an index of plenary performance on a scale of 1-10 that takes the average of normalized scores on attendance and participation during full legislative plenary sessions, an index of committee performance also on a scale of 1-10 that takes an average of the normalized scores on attendance at committee meetings and productivity, and a global performance index which averages the first two indices.

Treatment was administered directly after a baseline survey that was also conducted (without the intervention) in control villages. Survey respondents were randomly selected in each village through a random-walk procedure (see Appendix C for additional details). On average, 47 people per village received treatment.

The treatment was conducted over the course of one day sometime within the month prior to

¹⁵We presented the video to focus groups in rural villages prior to implementation, which confirmed that the information and images were accessible and comprehensible to villagers in Benin.

Figure 1: Example Intervention Bar Graph



Note: Performance indices are constructed relative to both other legislators in the department (a local average) and the country (national average). Red bars are used when the incumbent’s performance falls below the average and green bars when the incumbent’s performance is above the average.

the 2015 legislative elections. These elections were particularly salient among voters because of a fear that the incumbent president, if he secured a majority in the assembly, would pass a law dismantling term-limits and allowing him to run for a third-term. As evidence, voter turnout was particularly high – around 66 percent, which is almost 10 points above the previous legislative elections and on par with the most recent presidential elections.

After selecting our communes based on the one-to-one mapping exercise described above, we drew our sample of villages (or their urban equivalent, quarters) and assigned them to treatment or control.¹⁶ To increase statistical efficiency, we sample and randomize while stratifying on urban/rural status and electoral competitiveness of the village in the previous legislative election. Electorally non-competitive urban areas are rare, so we construct three blocks: urban, rural-competitive, and rural-non-competitive. Within each high dosage commune, we randomly select five villages/quarters from each of the three blocks to form the sample.¹⁷ We then randomly assign each of the five to an experimental condition.

¹⁶We additionally vary whether communes receive a high dosage of treatment, e.g. a large number of treated villages, or a low dosage of treatment. The specific procedure for randomizing dosage treatment at the commune level is in Appendix B, but we do not disaggregate results by dosage in this paper as it is not directly related to the theory we are testing.

¹⁷In some cases, there were only five villages/quarters in the block, in which case all five were selected.

2.3 Data and Measurement

We use data from two sources: administrative election results and a panel survey. We describe each dataset in turn and then discuss how we construct our key variables.

To measure the effect of treatment on aggregate outcomes, we collected administrative data on election outcomes at the polling station level. We were able to match 2015 polling station data to all villages in our experimental sample except for one treated village and two surveyed control villages. These villages thus drop out of our analysis. When considering all villages or quarters in our original sample of 30 communes (including control villages that were not surveyed), which we obtained from the 2011 legislative electoral results¹⁸, we were able to match 88% of villages/quarters to the 2015 outcome data.¹⁹

To measure the effect of treatment at the individual level, we collected panel survey data through a baseline in-person survey conducted 2 weeks to 1 month prior to the election and an endline phone survey conducted immediately after the election. The identities of the respondents were re-confirmed in the endline survey by calling the phone number provided 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%). A total of 3,419 individuals participated in the baseline *and* endline surveys (6,132 in the baseline).

Pre-treatment, we have village-level data on measures we used for blocking – urban/rural, incumbent legislative performance, and electoral competitiveness from the 2011 legislative elections. We also have village-level vote margin and the number of registered voters. In Appendix E, we use these data to provide evidence of balance across treatment groups.

¹⁸We thank Amanda Pinkston for generously sharing her data with us.

¹⁹On pre-treatment characteristics (registered voters logged, urban/rural, turnout, competitiveness, incumbent legislative performance, and north/south), unmatched and matched villages are statistically indistinguishable. See Table E.1 in Appendix E. An exception is that unmatched villages tend to be slightly smaller in terms of number of registered voters.

Our key dependent variable is voting for the incumbent. At the aggregate level, we use administrative data to calculate vote share for the incumbent party at the polling station level in our experimental sample and then aggregate it to the village level ($N = 1,499$) in the case of multiple polling stations per village. At the individual level, we use self-reports of voting for the incumbent party in the endline survey. The exact question, asked only of individuals who reported voting, is:

We would now like to know which political party you voted for in the legislative elections. Your response is entirely confidential and it will not be shared with anyone outside of the research team. We would like to know if you voted for the political party of [NAME OF PRINCIPAL DEPUTY]. The name of the party is [PARTY NAME] and its symbol is the [PARTY SYMBOL]. Just answer YES or NO. Did you vote for the party of [NAME OF PRINCIPAL DEPUTY]?

Our measure of coethnicity with the incumbent is self-reported on the baseline survey. The specific question is:

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

In all analyses, we separate the effect of learning the incumbent was a good relative performer from the effect of learning the incumbent was a bad relative performer. We do this because we expect voters to respond differently to information about legislators that is positive versus information that is negative. In our analysis of the official election results, we leverage the fact that the information provided in the intervention explicitly compares the incumbent legislator's performance to the performance of deputies in the surrounding area (those in the same department). We therefore code positive and negative information relative to this local benchmark. More specifically, we define the information as positive if the incumbent's overall score is better than that of other deputies in the department. Poor legislative performers are those whose overall legislative

score is worse than that of other legislators in this local area.²⁰ In our analysis of the survey data, we code positive and negative information relative to participant priors. In our baseline survey, we asked about the incumbent's relative performance, using the same scale that is provided in the intervention. We code the information as positive if the information provided in the intervention was better than the respondent's prior, and negative if it was worse. In instances where the information in the intervention is the same or when the respondent reports that they do not know about the incumbent's legislative performance, we follow the coding rule used with the official results data.²¹

3 Does Ethnicity Condition the Impact of Information?

How does ethnicity condition the impact of performance information? Figures 2 and 3 summarize our main results using the official election results data and the survey data, respectively. The figures present the average treatment effect in four sub-groups of the sample: coethnic villages (or survey respondents) with good performing incumbents, non-coethnic villages (or survey respondents) with good performing incumbents, coethnic villages (or survey respondents) with poor performing incumbents, and non-coethnic villages (or survey respondents) with poor performing incumbents. We estimate treatment effects using OLS with block fixed effects. The results in both figures are similar. When coethnics receive positive information, support for the incumbent increases. When non-coethnics receive that same positive information, it has no effect on voting behavior. When coethnics receive negative performance information, on the other hand, it does not impact their support of the incumbent, while non-coethnics who receive negative information are much less

²⁰This coding rule was pre-specified in our pre-analysis plan prior to project implementation.

²¹For those whose priors match the information in the intervention, the logic is that the intervention should make them more confident in their assessment. For example, if their prior is that the incumbent is a bad performer and they receive information that validates that prior, they will become more confident in their beliefs. For those who have no priors (54% of baseline participants), the logic is that the intervention provides them with the only information that they have. For both groups, it is thus reasonable to code the information they receive as good or bad based off of the incumbent's objective performance relative to others in the local area. These coding rules were pre-specified in our pre-analysis plan prior to project implementation.

likely to support the incumbent.²²

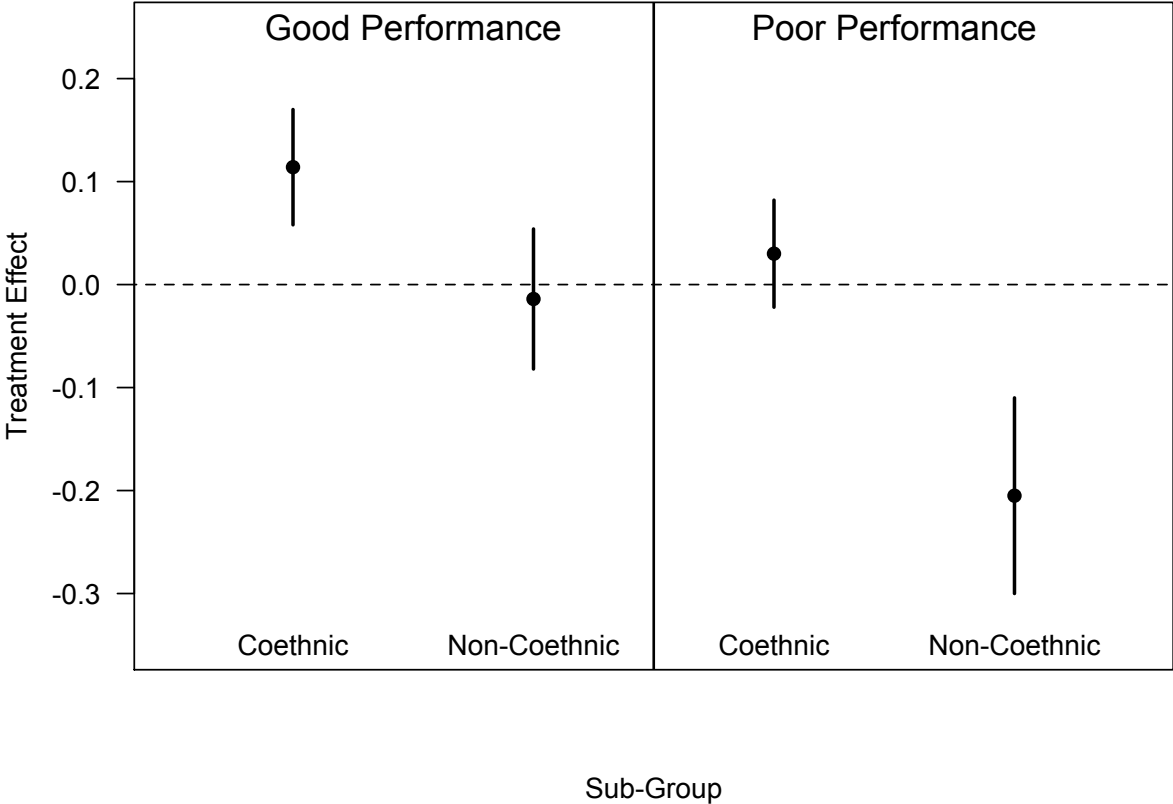
Turning to the numerical results, Table 1 presents the results using the official village/quarter-level election data. Columns 1 and 4 in each panel examine the unconditional treatment effect in good performance and poor performance communes, respectively. In good information communes, treatment has a positive but not statistically significant effect. In bad information communes, treatment has a negative but not statistically significant effect.

The table also presents the treatment effect in coethnic and non-coethnic villages/quarters and in good and bad performance communes. In the top panel, we code villages/quarters as being coethnic with the incumbent if greater than 50 percent of baseline survey respondents report that they are coethnic with the incumbent. In the middle panel, we code villages/quarters as being coethnic with the incumbent if greater than 70 percent of baseline survey respondents report that they are coethnic with the incumbent. In the bottom panel, we code villages/quarters as being coethnic with the incumbent if greater than 90 percent of baseline survey respondents report that they are coethnic with the incumbent.

The pattern of results in each panel is consistent. In coethnic villages/quarters, positive performance information increases the voteshare of the incumbent (column 2). These effects are statistically significant in the top and bottom panels. In non-coethnic villages/quarters, on the other hand, the impact of positive performance information is close to zero and the effects are not significant at conventional levels (column 3). In coethnic areas, negative performance information has no impact on incumbent voteshare (column 5). The coefficients in column 3 are substantively small and cannot be statistically distinguished from zero. By contrast, negative performance information reduces the voteshare of incumbents in non-coethnic areas (column 6). In sum, positive performance information increases the voteshare of the incumbent in coethnic areas but has no impact on the incumbent's voteshare in non-coethnic areas. Negative performance information, on

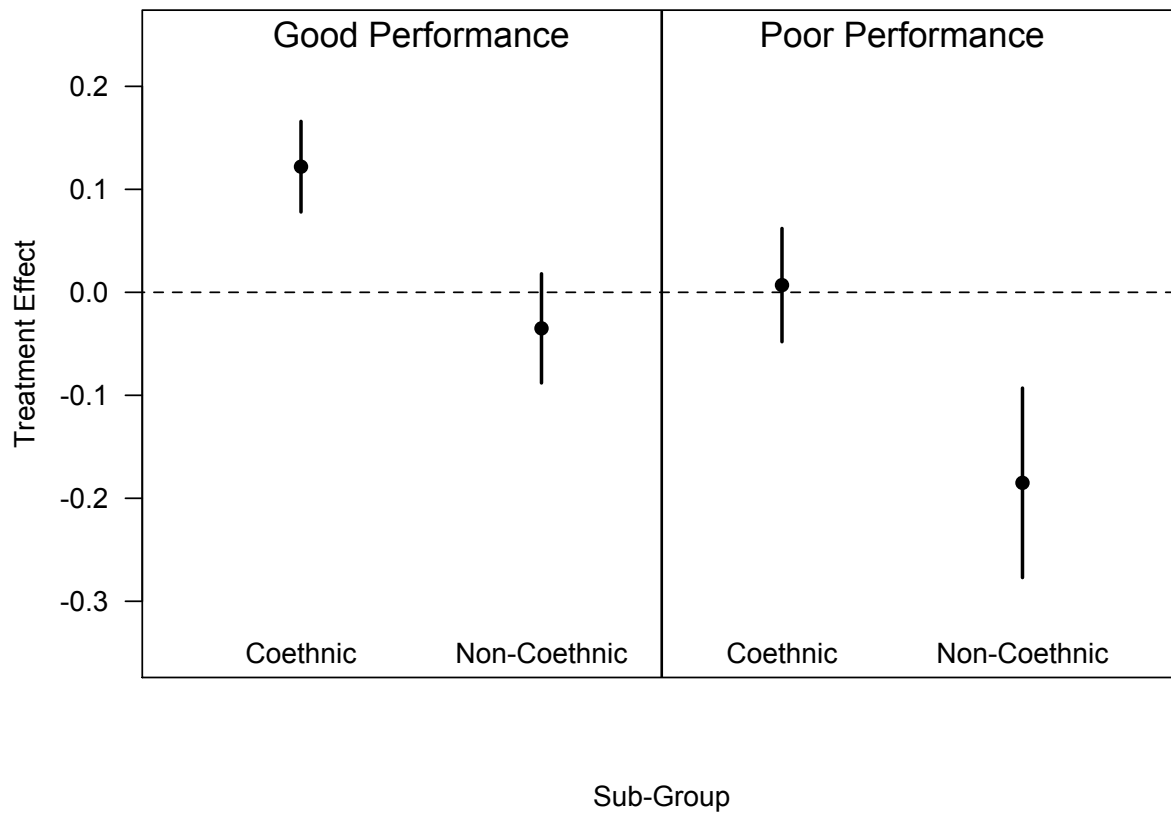
²²Coethnicity was one of several moderators that we were interested in testing in this project (and that we have pre-specified hypotheses about). We focus on ethnicity here because it is of greatest theoretical interest. However, in Appendix F.3 we show that other moderators are less important empirically. The only pre-specified moderator that has a similar effect as ethnicity is a dummy variable indicating whether or not the incumbent is from the village originally, which is highly correlated with ethnicity.

Figure 2: Treatment Effects by Ethnic Connection and Level of Performance, Official Results Data



The dependent variable is incumbent voteshare in the village, calculated from official election results. The figure presents the average treatment effect (and one standard error above and below the estimate) in four sub-groups: co-ethnic villages with good performing incumbents, non-coethnic villages with good performing incumbents, coethnic villages with poor performing incumbents, and non-coethnic villages with poor performing incumbents. In this figure, coethnic villages are coded using the 90% cutoff. Treatment effects are estimated using OLS with block fixed effects.

Figure 3: Treatment Effects by Ethnic Connection and Level of Performance, Survey Data



The dependent variable is an individual's vote for the incumbent, calculated using the survey data. The figure presents the average treatment effect (and one standard error above and below the estimate) in four sub-groups: coethnics with good performing incumbents, non-coethnics with good performing incumbents, coethnics with poor performing incumbents, and non-coethnics with poor performing incumbents. Treatment effects are estimated using OLS with block fixed effects.

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

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Good Info Full Sample	Good Info Coethnic (50)	Good Info Non-Coethnic (50)	Bad Info Full Sample	Bad Info Coethnic (50)	Bad Info Non-Coethnic (50)
Treatment	0.04 (0.03)	0.09** (0.04)	-0.03 (0.05)	-0.09 (0.07)	-0.02 (0.09)	-0.21 (0.17)
Constant	0.48*** (0.03)	0.47*** (0.03)	0.46*** (0.03)	0.57*** (0.07)	0.52*** (0.09)	0.62*** (0.14)
Observations	137	89	48	103	72	31
R-squared	0.59	0.70	0.56	0.60	0.67	0.58

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Good Info Full Sample	Good Info Coethnic (70)	Good Info Non-Coethnic (70)	Bad Info Full Sample	Bad Info Coethnic (70)	Bad Info Non-Coethnic (70)
Treatment	0.04 (0.03)	0.08 (0.05)	-0.02 (0.06)	-0.09 (0.07)	-0.01 (0.07)	-0.22* (0.12)
Constant	0.48*** (0.03)	0.43*** (0.05)	0.53*** (0.04)	0.57*** (0.07)	0.53*** (0.06)	0.62*** (0.10)
Observations	137	66	71	103	58	45
R-squared	0.59	0.72	0.59	0.60	0.76	0.63

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Good Info Full Sample	Good Info Coethnic (90)	Good Info Non-Coethnic (90)	Bad Info Full Sample	Bad Info Coethnic (90)	Bad Info Non-Coethnic (90)
Treatment	0.04 (0.03)	0.11* (0.06)	0.03 (0.05)	-0.09 (0.07)	-0.01 (0.07)	-0.21** (0.09)
Constant	0.48*** (0.03)	0.41*** (0.05)	0.48*** (0.04)	0.57*** (0.07)	0.55*** (0.06)	0.60*** (0.08)
Observations	137	44	93	103	53	50
R-squared	0.59	0.83	0.51	0.60	0.77	0.58

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 parentheses. Each model uses block fixed effects. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2: Treatment Effects by Ethnic Connection and Level of Performance, Survey Data

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Good Info Full Sample	Good Info Coethnic	Good Info Non-Coethnic	Bad Info Full Sample	Bad Info Coethnic	Bad Info Non-Coethnic
Treatment	0.07* (0.03)	0.12*** (0.04)	0.01 (0.05)	-0.09 (0.06)	-0.03 (0.05)	-0.19** (0.09)
Constant	0.45*** (0.03)	0.40*** (0.04)	0.49*** (0.05)	0.57*** (0.05)	0.55*** (0.05)	0.60*** (0.08)
Observations	1,672	1,030	627	1,358	948	408
R-squared	0.18	0.22	0.18	0.24	0.28	0.22

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

the other hand, has no impact on incumbent voteshare in coethnic areas and a negative impact on voteshare in non-coethnic areas.

We also confirm in Table F.3 that these patterns hold when using only the treatment conditions in which we are confident that voters viewed legislative performance as an important and positive means of improving voter welfare (Adida et al., 2015). Because we consider only a subset of all treatment conditions here, we lose statistical power, but the patterns remain the same.

Table 2 presents results from similar analyses using the survey data. While limitations of the survey data are the potential biases associated with social desirability bias and attrition — neither of which pose a problem for the official behavioral results presented above — an advantage of the survey data is that we do not need to make an ecological inference about how ethnicity conditions the impact of information. In Appendix G, we discuss the extent to which inferences made from our survey data might be biased. Several tests to mitigate problems of attrition and response bias demonstrate that our conclusions are relatively robust.

The results from the survey analysis are almost identical to the results from the analysis of the official election results. When the incumbent’s coethnics receive positive information about performance, they are about 12 percentage points more likely to report having voted for the incumbent (column 2). When non-coethnics receive the same positive information, however, they

are no more likely to report voting for the incumbent (column 3). When coethnics of the incumbent receive negative performance information, they are no more or less likely to report supporting the incumbent (column 5). Yet when non-coethnics receive negative information, they are about 19 percentage points less likely to report voting for the incumbent (column 6). Once again, the evidence supports the claim that voters in this context reward good performance only if the incumbent is their coethnic, while they punish bad performance only when the incumbent is from a different ethnic group.

4 Why Does Ethnicity Condition the Impact of Information?

The results presented above demonstrate that ethnic identity conditions the impact of information on voting behavior. Coethnics of the incumbent are only responsive to performance information when it is positive, while non-coethnics are only responsive to the information when it is negative. In this section, we consider potential explanations for these results, as well as their implications for theories of ethnic voting.

4.1 Ethnicity as Informational Shortcut

Our evidence is *not* consistent with models that emphasize the role of ethnicity as a heuristic for evaluating candidate preferences and performance. According to this set of arguments, ethnic voting emerges because of information scarcity and, as a result, any increased access to actual performance information should weaken the link between coethnicity and incumbent support (Hypothesis 1). Yet we find that ethnicity conditions the way in which voters respond to performance information, and that voters respond to information in ways that amplify, rather than diminish, the association between coethnicity and incumbent support.

4.2 The Instrumental Explanation

The instrumental argument implies that only coethnics of the incumbent will change their behavior in light of performance information (Carlson, 2015). Our main results are not consistent with this hypothesis, as we find that coethnics are only responsive to positive information, while non-coethnics do change their behavior when they receive negative information. To further evaluate this potential explanation, we follow Ichino and Nathan (2013) and analyze how the behavior of non-coethnics of the incumbent is moderated by local ethnic demography. If the instrumental argument is correct, we should observe non-coethnics of the incumbent behaving as if they are coethnics of the incumbent *if* they are living in areas where the majority of the population is coethnic with the incumbent. In such areas, non-coethnics can expect to benefit from ethnic favoritism, even if they are not ethnically linked with the incumbent (Ichino and Nathan, 2013).

The instrumental hypothesis thus implies that the impact of treatment on non-coethnics should be conditioned by the ethnic demographics of the village or quarter in which a voter lives. To test this expectation, we use the survey data to create a village-level continuous measure, *share of coethnics in the village*. We estimate a model in which we interact this continuous measure with our treatment indicator *and* a dummy variable indicating whether or not the respondent is coethnic with the incumbent.²³ Since triple interactions are difficult to interpret, we present the full results in the appendix (Table F.7) and present the results graphically here. We note that these analyses were not pre-specified.

Figure 4 presents the results. It displays estimated treatment effects (with 90 percent confidence intervals) for non-coethnics living in villages/quarters where 50 percent or more of the population is a coethnic with the incumbent. Once again, the patterns are not consistent with the instrumental logic. The provision of positive information has no impact on the voting behavior of non-coethnics, even when they are living in villages in which most residents are coethnics of the incumbent. Similarly, the provision of negative information has no significant treatment effects on

²³We use the survey data in these analyses because we need information on the ethnic demography of the village and of the respondent.

non-coethnics living in coethnic areas. If anything, the provision of negative information seems to enhance support among non-coethnics living in the areas most densely populated by coethnics of the incumbent, which is the opposite of what we would expect from the instrumental argument. However, this pattern should be interpreted with caution as the marginal effects are imprecisely estimated at the higher end of the coethnic share in the village variable.

4.3 The Motivated Reasoning Explanation

Finally, we examine the possibility of motivated reasoning as an explanation for our results. Our main experimental findings are consistent with the motivated reasoning hypothesis, which implies that performance information should influence vote choice only when it is positive news about a coethnic or negative news about a non-coethnic.

To further probe the possibility of the motivated reasoning explanation, we analyze data from a comprehension survey that was conducted *immediately* after the performance treatment was provided. About 30 percent of all treated participants were selected at random for participation in the comprehension survey. In the comprehension survey, we asked respondents to report on the relative performance of their incumbent in plenary and committee work. That is, we asked respondents to provide us with the information to which they had been exposed just minutes earlier. We leverage these survey questions to test this psychological mechanism. We note that these analyses were not pre-specified.

First, we create a dummy variable that takes a value of 1 if the respondent provides the correct answer to the plenary and committee performance comprehension questions, respectively, and 0 otherwise. Table 3 examines whether coethnicity with the incumbent is associated with correct answers in both the good and bad performance information communes. The results are generally consistent with the motivated reasoning explanation. In good performance areas, coethnics of the incumbent are significantly *more* likely to accurately recall the plenary information. They are not significantly more likely to do so in the bad information areas. On the committee score, coethnics

Figure 4: Local Ethnic Demography and the Impact of Treatment on Non-Coethnics of the Incumbent

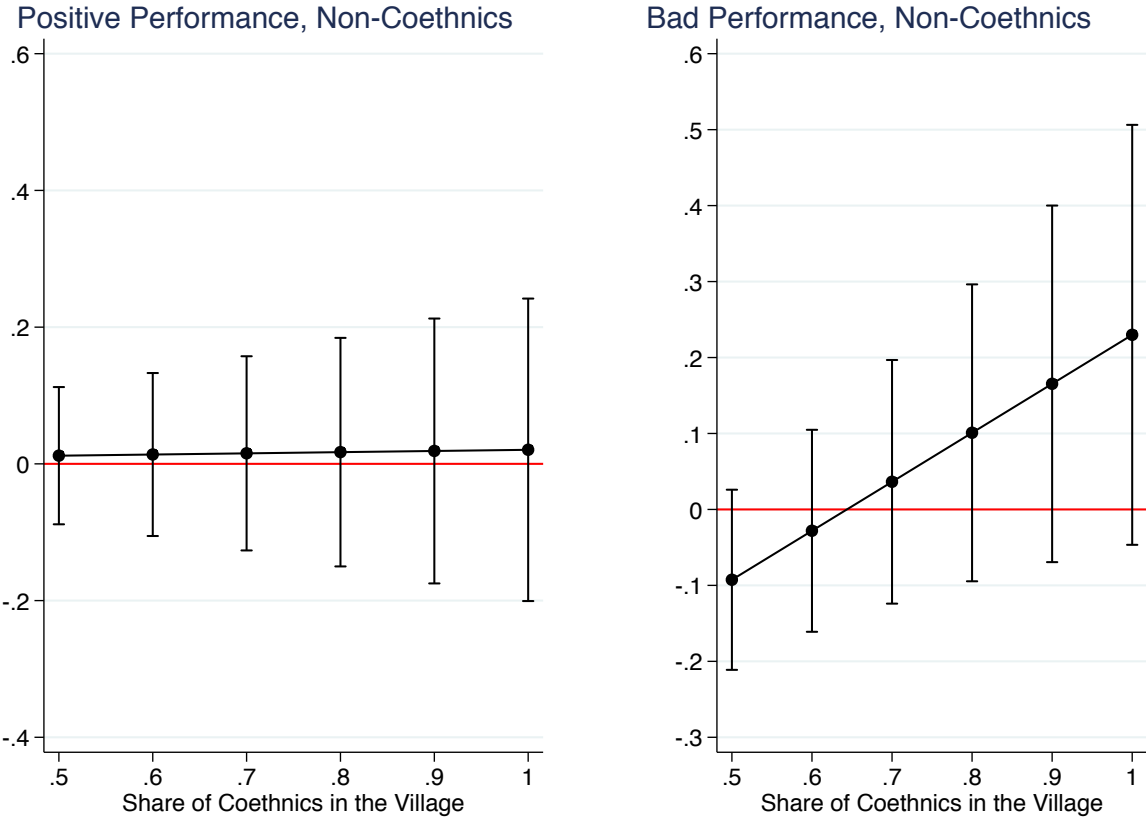


Table 3: Coethnicity, Performance, and Comprehension

VARIABLES	(1)	(2)	(3)	(4)
	Good Performance Plenary	Bad Performance Plenary	Good Performance Committee	Bad Performance Committee
Coethnic with incumbent	0.097** (0.046)	0.053 (0.054)	-0.000 (0.045)	-0.210*** (0.051)
Constant	0.406*** (0.036)	0.460*** (0.045)	0.370*** (0.035)	0.508*** (0.042)
Observations	476	403	476	402
R-squared	0.009	0.002	0.000	0.040

Dependent variable takes a value of 1 if the respondent accurately reported the incumbent's performance score in plenary and committee during the comprehension survey immediately following the treatment. The sample only includes those who were randomly selected to participate in the comprehension survey. Standard errors in parantheses. *** p<0.01, ** p<0.05, * p<0.1

are not more likely to accurately recall the information if it was positive, but they were significantly and substantially *less* likely to accurately recall the information if it was negative. Thus, coethnics appear more likely to accurately report the performance information if the information is positive and less likely to accurately report it if it is negative. Voters appear to have updated their beliefs only when the information was consistent with being able to view coethnics in a positive light and non-coethnics in a negative light.

We also analyzed patterns of “Don’t Know” responses to the same two comprehension questions. We create a dummy variable that takes a value of 1 if the respondent said “I don’t know” to the comprehension questions, and 0 otherwise. If the results are driven by motivated reasoning, we may observe an association between coethnicity, negative performance information, and “don’t know” responses. We expect that voters will be less likely to process, and thus learn, new information if that information is negative information about a coethnic. In a context where legislative performance information is scarce,²⁴ we might expect more “don’t know” responses when negative information about a coethnic is provided.

The results in Table 4 are fairly consistent with this expectation. First, coethnics of the incumbent are less likely to provide a “don’t know” response when the information is positive, although the associations are not significant (columns 1 and 3). Second, and importantly, coethnics of the

²⁴More than 50% of respondents in the baseline survey asked about these same activies said they did not know.

Table 4: Coethnicity, Performance, and Don't Know Responses on the Comprehension Survey

VARIABLES	(1)	(2)	(3)	(4)
	Good Performance Plenary	Bad Performance Plenary	Good Performance Committee	Bad Performance Committee
Coethnic with incumbent	-0.023 (0.039)	0.067* (0.035)	-0.044 (0.039)	0.064* (0.035)
Constant	0.245*** (0.030)	0.073** (0.029)	0.255*** (0.030)	0.073** (0.029)
Observations	476	403	476	402
R-squared	0.001	0.009	0.003	0.008

Dependent variable takes a value of 1 if the respondent responded “don't know” to the questions about the incumbent's performance score in plenary and committee during the comprehension survey immediately following the treatment. The sample only includes those who were randomly selected to participate in the comprehension survey. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

incumbent are significantly more likely to provide a “don't know” response when the information is negative (columns 2 and 4), and here, the effect reaches statistical significance. The results thus provide evidence that coethnics and non-coethnics of the incumbent process information differently and in ways that reinforce preferences and beliefs about their ethnic kin.

5 Conclusion

With attitudinal and behavioral data collected as part of a large-scale field experiment around Benin's 2015 legislative elections, this paper has investigated how and why ethnic identity conditions the impact of information on voting behavior. We show that voters only respond to information about good incumbent performance if they share an ethnic tie with the incumbent. Positive information increases support for the incumbent among coethnics but has no impact on the voting behavior of non-coethnics. We also show that voters only respond to negative performance information if they are *not* members of the same ethnic group as the incumbent. Negative information has no impact on the voting behavior of the incumbent's coethnics, while non-coethnics punish incumbents for poor performance. We find consistent effects across both attitudinal measures of ethnic voting via a panel survey, *and* official electoral results, increasing our confidence in our findings. We examine three sets of potential explanations for these results. Our evidence is most

consistent with arguments that emphasize how identity shapes information processing. In particular, the results suggest that many voters engage in motivated reasoning (Bolsen, Druckman and Cook, 2014; Taber and Lodge, 2006).

While we present field experimental evidence from Benin, we expect our results to extend to other democracies where ethnic identities are salient and where voter access to information about incumbents is limited. These conditions characterize many of the world's democracies, including those in sub-Saharan Africa (Posner, 2005), Asia (Chandra, 2007), and Eastern Europe and Central Asia (Hale, 2008). To the extent that other types of identities — partisan, class, religious, and so on — may also impact how voters process information, our findings may also extend to contexts where non-ethnic differences structure electoral competition. Indeed, our results resonate with a body of research in American politics that investigates how partisan and other identities shape information processing and knowledge about politics.

Our paper makes a number of contributions. First, our results have implications for the literature on ethnic voting. We evaluate several theories about the relationship between coethnicity, performance information, and voting, and provide evidence that is most consistent with arguments emphasizing how identity shapes information processing. Our results thus emphasize a mechanism that has considerable attention in American politics, but limited attention in the literature on political behavior in developing democracies. In showing that ethnic identity shapes how individuals process information about politics, and that it does so in ways that may serve to amplify voter preferences for coethnic candidates, our paper shows that ethnic voting may persist in part because identity conditions how voters respond to and process information about the political world. Second, we advance the empirical literature by using a field experiment to investigate how ethnic identity shapes voter responsiveness to performance information. While prior research has examined similar questions using survey experiments (Carlson, 2015; Conroy-Krutz, 2013), we move the evidence base forward by studying how ethnicity shapes voter responsiveness to performance information in the context of an election. Finally, our results contribute to the literature on information and accountability. This literature has produced mixed results on the effects of information

access and political behavior. Our results suggest that ethnic identity is an important moderator that should be investigated in future research on information and accountability.

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Appendix

A Description of 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*).²⁵

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

²⁵For 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.

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

B 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).²⁶ 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.

C 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 while alternating on gender. As a condition of participating in the baseline

²⁶When 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.

survey, respondents had to have access to a cell phone.²⁷ 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, while 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.²⁸

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 (2015) 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 major-

²⁷They were not required to own a cell phone. The cell phone to which they had access could belong to a friend or relative.

²⁸On 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.

ity 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 (2015), 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.²⁹ 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.

²⁹Of 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 Balance Tests

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

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

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

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

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

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

Table E.3: Balance in High Dosage Communes

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

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

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

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Table E.4: Balance in Low Dosage Communes

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

F Additional Results and Robustness Checks

F.1 Interaction Models

Table F.1: Interaction Models, Survey Data

VARIABLES	(1) Good Performance	(2) Bad Performance
Treatment	0.007 (0.052)	-0.169* (0.089)
Treatment*Coethnic	0.110* (0.066)	0.124 (0.094)
Coethnic with incumbent	-0.124** (0.060)	-0.107 (0.090)
Constant	0.517*** (0.044)	0.642*** (0.081)
Observations	1,657	1,356
R-squared	0.184	0.246

Robust standard errors in parentheses

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

Table F.2: Interaction Models, Official Results

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

F.2 Effects in Civics Condition in High Dosage

Table F.3: Treatment Effects by Ethnic Connection and Level of Performance, Official Results

VARIABLES	(1)	(2)	(3)	(4)
	Good Performance Coethnic (50)	Good Performance Non-Coethnic (50)	Bad Performance Coethnic (50)	Bad Performance Non-Coethnic (50)
Treatment	0.090 (0.058)	-0.044 (0.097)	0.008 (0.092)	-0.276 (0.206)
Constant	0.467*** (0.045)	0.474*** (0.060)	0.488*** (0.082)	0.579*** (0.160)
Observations	46	29	38	15
R-squared	0.778	0.594	0.769	0.750

VARIABLES	(1)	(2)	(3)	(4)
	Good Performance Coethnic (70)	Good Performance Non-Coethnic (70)	Bad Performance Coethnic (70)	Bad Performance Non-Coethnic (70)
Treatment	0.061 (0.077)	-0.001 (0.101)	0.054 (0.074)	-0.193 (0.155)
Constant	0.446*** (0.059)	0.505*** (0.067)	0.499*** (0.065)	0.514*** (0.126)
Observations	37	38	31	22
R-squared	0.832	0.629	0.823	0.684

VARIABLES	(1)	(2)	(3)	(4)
	Good Performance Coethnic (90)	Good Performance Non-Coethnic (90)	Bad Performance Coethnic (90)	Bad Performance Non-Coethnic (90)
Treatment	0.107 (0.090)	0.039 (0.080)	0.059 (0.074)	-0.192 (0.120)
Constant	0.399*** (0.069)	0.483*** (0.056)	0.500*** (0.065)	0.516*** (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 village are in parentheses. Each model uses block fixed effects. *** p<0.01, ** p<0.05, * p<0.1

Table F.4: Treatment Effects by Ethnic Connection and Level of Performance, Survey Data

VARIABLES	(1)	(2)	(3)	(4)
	Good Performance Coethnic	Good Performance Non-Coethnic	Bad Performance Coethnic	Bad Performance Non-Coethnic
Treatment	0.132** (0.051)	0.003 (0.065)	-0.020 (0.051)	-0.243** (0.112)
Constant	0.381*** (0.045)	0.516*** (0.050)	0.546*** (0.044)	0.555*** (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.

F.3 Results on Other Potential Moderators of Information

Table F.5: Comparing Ethnicity to Other Potential Moderators, Good Performance

VARIABLES	(1)	(2)	(3)	(4)	(5)
Treatment	0.007 (0.052)	0.030 (0.040)	0.062* (0.034)	0.114** (0.046)	0.055 (0.043)
Treatment x Coethnic	0.110* (0.066)				
Coethnic with incumbent	-0.124** (0.060)				
Treatment x Home Village		0.141* (0.080)			
Incumbent Home Village		-0.111 (0.077)			
Treatment x Help in the Past			-0.008 (0.151)		
Received personal assistance from incumbent			0.031 (0.140)		
Treatment x Female				-0.095 (0.061)	
Female				0.077 (0.056)	
Treatment x Years of Education					0.003 (0.006)
Years of education					-0.003 (0.005)
Constant	0.517*** (0.044)	0.476*** (0.036)	0.451*** (0.029)	0.409*** (0.041)	0.459*** (0.038)
Observations	1,657	1,662	1,657	1,671	1,667
R-squared	0.184	0.181	0.180	0.182	0.181

Table F.6: Comparing Ethnicity to Other Potential Moderators, Poor Performance

VARIABLES	(1)	(2)	(3)	(4)	(5)
Treatment	-0.169*	-0.088	-0.090	-0.116**	-0.008
	(0.089)	(0.065)	(0.058)	(0.059)	(0.067)
Treatment x Coethnic	0.124				
	(0.094)				
Coethnic with incumbent	-0.107				
	(0.090)				
Treatment x Home Village		0.002			
		(0.101)			
Incumbent Home Village		-0.029			
		(0.093)			
Treatment x Help in the Past			0.023		
			(0.131)		
Received personal assistance from incumbent			-0.068		
			(0.106)		
Treatment x Female				0.067	
				(0.063)	
Female				0.004	
				(0.056)	
Treatment x Years of Education					-0.020***
					(0.007)
Years of education					0.013**
					(0.006)
Constant	0.642***	0.579***	0.578***	0.570***	0.514***
	(0.081)	(0.060)	(0.054)	(0.054)	(0.063)
Observations	1,356	1,354	1,357	1,358	1,324
R-squared	0.246	0.246	0.245	0.247	0.244

F.4 Tests of the Instrumental Hypothesis

Table F.7: Ethnic Demography, Coethnicity, and the Impact of Information

VARIABLES	(1) Good Performance	(2) Bad Performance
Treatment	0.003 (0.078)	-0.415*** (0.151)
Coethnic with incumbent	-0.098 (0.235)	-0.058 (0.181)
Share of Coethnics in the Village	-0.091 (0.167)	-0.687** (0.293)
Treatment x Share of Coethnics in the Village	0.017 (0.182)	0.645** (0.284)
Coethnics x Share of Coethnics in the Village	-0.010 (0.322)	0.233 (0.297)
Treatment x Coethnic	0.194 (0.245)	0.129 (0.196)
Treatment x Coethnic x Share of Coethnics in the Village	-0.109 (0.342)	-0.340 (0.325)
Constant	0.560*** (0.073)	0.942*** (0.169)
Observations	1,657	1,356
R-squared	0.186	0.256

Robust standard errors in parentheses

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

G 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.

G.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 G.1, the endline participants are more likely to be male, better educated, and wealthier.³⁰ While 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 G.1). To the extent there is gender balance across ethnic groups, we are less concerned about our main findings being threatened by

³⁰Here, 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.

the first pattern; they could, however, be subject to bias as a result of the second.

Table G.1: Differential Rates of Attrition by Pre-treatment Covariates and Treatment

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Endline	Vote choice	Endline	Vote choice	Endline	Vote choice	Endline	Vote choice
Treatment	-0.039 (0.035)	-0.025 (0.032)	-0.077*** (0.029)	-0.062** (0.028)	0.037 (0.035)	0.035 (0.035)	-0.068 (0.051)	-0.060 (0.041)
Female	0.043*** (0.017)	0.060*** (0.017)	-0.008 (0.027)	0.026 (0.029)				
Coethnic with incumbent	-0.008 (0.026)	0.008 (0.026)			0.074** (0.035)	0.077** (0.034)		
Years of education	-0.006*** (0.002)	-0.004* (0.002)						
Urban	-0.069 (0.100)	-0.061 (0.091)						
Poverty Level	0.042** (0.016)	0.051*** (0.016)						
Positive Prior	0.016 (0.018)	0.001 (0.017)					0.002 (0.020)	-0.014 (0.020)
Positive Prior x Good News	-0.015 (0.024)	-0.015 (0.024)					0.015 (0.044)	-0.003 (0.044)
Treatment x Female			0.094*** (0.030)	0.067** (0.032)				
Treatment x Coethnicity					-0.110*** (0.040)	-0.103*** (0.038)		
Treatment x Positive Prior							0.032 (0.038)	0.033 (0.038)
Treatment x Good News							0.108 (0.139)	0.056 (0.137)
Treatment x Positive Prior x Good News							-0.037 (0.045)	-0.013 (0.046)
Constant	0.451*** (0.059)	0.517*** (0.057)	0.471*** (0.025)	0.515*** (0.024)	0.420*** (0.028)	0.479*** (0.029)	0.410*** (0.074)	0.533*** (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 G.2 shows that our main findings from Table 2 are robust to adding these controls.

Table G.2: Replicating Main Results Controlling for Predictors of Attrition

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Good Info Full Sample	Good Info Coethnic	Good Info Non-Coethnic	Bad Info Full Sample	Bad Info Coethnic	Bad Info Non-Coethnic
Treatment	0.07* (0.04)	0.12*** (0.04)	0.00 (0.06)	-0.08 (0.06)	-0.03 (0.05)	-0.19** (0.09)
Female	-0.00 (0.02)	-0.01 (0.03)	0.02 (0.04)	0.06** (0.03)	0.08*** (0.03)	0.01 (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 (0.02)	-0.03 (0.02)	0.02 (0.04)	-0.04 (0.03)	-0.06* (0.03)	0.02 (0.05)
Constant	0.45*** (0.03)	0.41*** (0.04)	0.49*** (0.05)	0.54*** (0.06)	0.50*** (0.05)	0.61*** (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.

G.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 voteshare – 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 paper 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 G.3 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 paper. 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. While 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 G.3: Is Reported Vote Choice Conditioned by Enumerator Coethnicity?

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Good Info Full Sample	Good Info Coethnic	Good Info Non-Coethnic	Bad Info Full Sample	Bad Info Coethnic	Bad Info Non-Coethnic
Treatment	0.02 (0.06)	0.12** (0.06)	0.03 (0.06)	-0.16 (0.11)	-0.01 (0.08)	-0.16 (0.10)
Coethnic with Enumerator	0.13 (0.12)	-0.02 (0.10)	0.16 (0.13)	0.02 (0.14)	0.21* (0.12)	0.00 (0.15)
Treatment x Enumerator Coethnicity	-0.13 (0.13)	-0.01 (0.10)	-0.13 (0.13)	-0.04 (0.13)	-0.07 (0.10)	-0.07 (0.14)
Coethnic with incumbent	-0.10 (0.08)			-0.16 (0.12)		
Treatment x Incumbent Coethnicity	0.08 (0.09)			0.14 (0.13)		
Enumerator Coethnicity x Incumbent Coethnicity	-0.15 (0.16)			0.11 (0.16)		
Treatment x Enumerator Coethnicity x Incumbent Coethnicity	0.15 (0.17)			-0.04 (0.17)		
Constant	0.50*** (0.05)	0.41*** (0.05)	0.47*** (0.06)	0.64*** (0.11)	0.46*** (0.08)	0.60*** (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.