

Debates:

Voting and Expenditure Responses to Political Communication

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Abstract

Candidate debates have a rich history and remain integral to contemporary campaign strategy. There is, however, no evidence that they affect voter behavior. The scarcity of political information in the developing world offers an attractive testing ground. Using experimental variation in Sierra Leone, we find that public debate screenings build political knowledge that changes the way people vote, which triggers a campaign expenditure response by candidates, and fosters accountability pressure that disciplines the subsequent spending of elected officials. We parse the effects of information conveyed about policy versus charisma, and find that both matter. The results show how political communication can trigger a chain of events that begins with voters and ultimately influences policy.

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1. Introduction

Debates among candidates for public office have a rich history and offer a unique platform for candidates to communicate with voters. The Lincoln-Douglas senatorial debates of 1858 are a famous early example in the United States. As distinct from other information sources, debates reveal the relative policy positions and competence of rival candidates, cover challengers in an equal fashion to incumbents, and convey comprehensive information ranging from qualifications to more intangible attributes like persuasiveness and charisma. These features have led to some memorable, and highly influential, contests including the first televised presidential debates between Kennedy and Nixon in 1960, and between Mitterand and Giscard in 1974.

Today debates are significant campaign events: large numbers of voters watch them (84 million tuned in to the first Clinton-Trump debate, the largest in American history¹); they generate a flurry of media commentary and analysis of candidate performance; and pundits pore over polling data to assess their effects on public opinion (see for example, Shear [2012]). There is, however, no definitive evidence and no consensus on whether debates have any impact on voter behavior. While the relevant literature is large (see Hellweg, Pfau and Brydon [1992] for review), it relies primarily on cross-sectional analysis of opinion polls with familiar identification challenges.

In the developing world, debates are less common but arguably no less important. Indeed, the relative scarcity of political information creates scope for the effects of debates to be more pronounced, persistent, and directly linked to electoral outcomes. Allowing candidates to stand on equal footing and express their views on key policy issues could facilitate the election of more competent individuals. And, by informing voters about these policy commitments and the resources available to those elected, debates could foster accountability pressure that disciplines the subsequent behavior of the winners in office.

This paper evaluates these claims via a large-scale experiment conducted during the 2012 Parliamentary elections in Sierra Leone. The study randomized exposure to debates at three levels—individual, polling center and constituency—to capture the effects of debates on, and the interactions among, voters, candidates and elected officials. We find that debates have strong direct

¹ According to Nielsen data cited in Stelter (2016).

impacts on voters, which trigger indirect effects on candidate campaign activity, and ultimately influence the performance of elected politicians.

We first show that debates have substantial impacts on voter behavior. To capture these effects, we worked with an independent media partner to host, film, and disseminate debates in fourteen constituencies. We randomly allocated a “road show” across 224 polling centers that screened videotapes of the debates in large public gatherings in the five weeks leading up to the election. We find that watching debates led to higher political knowledge, including awareness of candidate policy stances; and improved alignment between voter policy preferences and those of their selected candidate. Importantly, the gains in political knowledge translated into changes in votes cast: we document a 3.5 percentage point average increase in vote shares for the candidates who performed best during the debates, which is significant in both our exit poll data and in the National Electoral Commission’s official voting returns. Despite historical ties between ethnic groups and political parties, candidates who debated well attracted votes from both loyalists and rival ethnic groups (leading to no net impact of debates on the incidence of ethnicity-based voting). Together these results document a high degree of voter responsiveness to information.

Consistent with theory, we find an endogenous response by participating candidates who increased their campaign expenditure in communities where debate screenings were held. While candidates were not informed of which polling centers received screenings, such large public gatherings in rural areas would be easy for candidates to track. We find that candidates increased their campaign effort, as measured by the number of in-person visits, and the number and value of gifts, in communities where screenings were held. Increased effort and expenditure is consistent with a “swing” voter investment model if the debates made exposed areas appear more competitive, either by making expected vote margins narrower or more uncertain.² Our theory suggests the effects on competition should be largest where a trailing candidate outperforms the initial frontrunner during the debate, which is exactly what we see in the data.

Our third, more speculative, set of results traces the effects of debates all the way to policy, where

² See Lindbeck and Weibull (1987), Dixit and Londregan (1996, 1998), and Bardhan and Mookherjee (2010); and also Eifert, Miguel and Posner (2010) and Casey (2015) for applications of political competition to ethnic politics.

we find some evidence that participation in debates enhanced the subsequent accountability of elected MPs. To assess these effects, we randomly selected 14 constituencies from a pool of 28 to host debates, and then tracked the performance of all 28 general election winners over their first year in office. We find that debates had positive impacts on constituency engagement and public spending: treated MPs, for example, held twice as many meetings with their constituents and spent 2.5 times as much of their discretionary public funds on development projects (as verified by field audits). We find no evidence for effects on participation in Parliamentary sittings or on consistency in promoting the MP's priority sector. While the small sample at this level makes our conclusions more tentative, the finding that debates could enhance accountability, even in areas where direct electoral pressure is limited, is important and particularly so for newer democracies.

To better understand what drives the initial response of voters to debates, we disentangle the influence of information conveyed about policy stance from candidate persona, and find that voters require information on both to change behavior. A suite of treatment arms randomized at the individual level isolates the "hard facts" content, covering policy and professional qualifications that could easily be delivered in other formats, from the coverage of candidate charisma and persuasiveness that is specific to debates. Some voters watched brief "get to know you" videos of the candidates speaking informally about themselves and their hobbies, which capture persona but exclude policy. Others listened to a radio summary that articulated all the facts about policy positions and professional experience covered during the debates, but conveyed nothing about persona. Still others watched the full debate. (All three arms were delivered privately to individuals on a tablet device.) Voters updated their views of candidates in response to information on facts and personality, but only debates move them into better policy alignment with candidates and trigger changes in vote choice. This suggests that while both policy preference and persona matter, the combination delivered by debates is more powerful than either factor in isolation.

Debates when viewed in large public screenings are more powerful than those viewed in private. The screenings represent a compound treatment that combines the debate content with social mobilization or common knowledge generation from the public gathering, alongside the extra campaign attention that endogenously tracked the road show. While smaller in magnitude, the private viewing estimates are significant, which shows that debate exposure has direct effects on

voter behavior net of any social mobilization or campaign effects.

Together, these experiments speak to the central problem in political economy of whether elections effectively discipline candidates and incumbent office holders. Our paper shows how political communication—specifically via interparty debates—can trigger a chain of events that begins with voters, flows through candidates, and ultimately impacts policy. This disciplining effect can hold even in a relatively new democracy with strong regional voting patterns.

The literature on debates in American politics is large but inconclusive. There are few studies credibly identifying causal effects (Prior [2012]) and those that do produce mixed results (Fridkin *et al.* [2007], Wald and Lupfer [1978], Mullainathan, Washington and Azari [2010]). Our private viewing experiments extend this literature by unpacking voter responses to the distinct types of information delivered and documenting effects on actual votes cast. The scale and intensity of the group screenings offers a new contribution, which interestingly generates effects that are similar in magnitude, and yet much more persistent, than those found for one-sided campaign advertising in wealthier countries (Gerber *et al.* [2011] in the U.S. and Kendall, Nannicini and Trebbi [2015] in Italy). Potential dilution of the effect—via diminishing marginal returns to information or drowning out by the deluge of political commentary—is less likely in low information environments like Sierra Leone, where debate effects persisted over several weeks. Our context further affords an unusual degree of control over media exposure, and our results preview the role a more developed media might play in poor countries (see also Paluck and Green 2009).

Publicizing debates is typically the purview of mass media outlets. Standard models show how access to politically informative news enhances voter responsiveness to politician quality and effort, which in turn strengthens incentives for politicians to perform in office (Stromberg 2015). Our results on voter responsiveness are consistent with evidence that media coverage of politics affects party choice (Enikolopov, Petrova and Zhuravskaya [2011]) and voting based on candidate quality (Ferraz and Finan [2008]);³ and our findings for enhanced campaign expenditure and MP performance resonate with evidence that more informed electorates attract greater public funding

³ See also Gentzkow (2006) and Gentzkow, Shapiro and Sinkinson (2011) on turnout, and DellaVigna and Kaplan (2007) on biased coverage and voting.

(Stromberg 2004), suffer less leakage (Reinikka and Svensson 2005), and see their needs better met (Besley and Burgess 2002). Our constellation of results mirrors that in the seminal contribution of Snyder and Stromberg (2010), who trace the effects of increasingly informed voters, through greater politician effort, to better policy outcomes (see also Fujiwara 2015 on policy and Casey 2015 on campaign responses).

The rest of the paper is structured as follows. Section 2 discusses the institutional context. Sections 3 and 4 explain the research design and estimation strategy. Section 5 presents estimates for treatment effects on voters, candidates, and elected officials. Section 6 explores mechanisms. Section 7 discusses practical challenges in iterating between pre-specified analyses and learning from data across experiments. Section 8 concludes with policy considerations.

2. Institutional Context

Sierra Leone has 112 Parliamentary constituencies, which are single member jurisdictions elected by first-past-the-post plurality. The winning MP represents the local area, containing approximately 40,000 residents, in the national legislature. In these elections, the ethnic composition of voters in a given constituency predicts the corresponding party vote shares with remarkable accuracy. These correlations arise from historical ties between the All People's Congress (APC) party and the ethnic groups in the North, most prominently the Temne; and between the Sierra Leone People's Party (SLPP) and groups in the South, most prominently the Mende (see Kandeh [1992]). As an example of the contemporary strength of these loyalties, 89 percent of respondents in the control group of this study reported voting for the MP candidate from the party that is historically associated with their ethnic group.

The reach of mass media is particularly constrained: television ownership is below 10% and adult illiteracy near 70%. Radio, however, has emerged as an important source of political information: 43% of voters cite it as their primary source, followed by friends and family (33%). The civil society organization who hosted the debates we study, Search for Common Ground (SFCG), is an active and respected contributor and provides a range of radio programs focused on their mission of promoting peace and reconciliation. How unusual are debates in this context? At the regional level, interest in debates has increased markedly over the past decade with at least thirteen African

countries holding candidate debates.⁴ In Sierra Leone, debates remain rare but not unheard-of: Presidential debates were held before the 2007 and 2012 elections, however no host succeeded in getting both major party candidates to participate in the same debate. The dissemination vehicle studied in this experiment, via mobile cinema, was certainly novel.

The debates themselves had the following format. SFCG invited candidates from the three largest parties who were contesting a given seat to participate.⁵ The SFCG moderator began by introducing the candidates and explaining the basic roles and responsibilities of an MP. A casual “get to know you” section followed, where the candidates spoke informally about their family and hobbies. The moderator then posed a series of national and local policy questions, and gave each candidate two to three minutes to respond to each question. We focus data collection around four questions that were standardized across all 14 debates: the candidate’s top priority for additional government spending; how they would use the constituency facilitation fund (CFF) (an untied 43.8 million Leones, US\$ 11K, grant given annually to each MP); whether they would vote in favor of the Gender Equity Bill (GEB), a 30% quota for women’s representation in government; and their assessment of the implementation of free healthcare (FHC), a major initiative by the incumbent government to provide free care to children under five and pregnant or nursing women. These questions aimed to capture salient policy discussions of the time that the incoming Parliament would have some role in resolving. All debates were conducted in Krio, Sierra Leone’s *lingua franca*. SFCG chose filming locations that were relatively controlled environments, with few spectators present.

Why study debates? There is growing evidence in development economics that providing information to voters can change their behavior (see Pande [2011] for review). Studies like Banerjee *et al.* (2011), among others, show that providing specific information about candidate qualifications can impact voting. Debates are distinctive because they provide more comprehensive information about candidates, including about persuasion and charisma, which are plausibly productivity-enhancing traits in legislatures. Comprehensiveness eases concerns that

⁴ Source: www.debatesinternational.org/countries, accessed 2 February 2016.

⁵ These are the APC, SLPP, and the latter’s splinter party, the People’s Movement for Democratic Change (PMDC). No other parties won seats in the previous election, and these parties respectively held 59, 39 and 9 percent of the seats in Parliament at the time.

increasing transparency along one dimension will simply reallocate politician effort towards those more observable actions, regardless of their impact on welfare (e.g. Liessem and Gersbach [2003]). Pragmatically, it makes it harder for politicians to unravel the impact of the intervention: it is easier for them to discredit a scorecard-style information campaign (Humphreys and Weinstein [2012]) than a video of their own statements. And, by covering a range of issues and allowing candidates to make a positive case, debates may be less likely to backfire than single issue interventions, which have been found in some cases to depress turnout (Chong *et al.* [2015]) and increase vote buying (Cruz, Keefer and LaBonne [2015]).

Debates are similar to the town hall meetings studied in the pioneering work of Leonard Wantchekon and co-authors.⁶ They find that public deliberation between a representative of one party and their constituents decreases the prevalence of clientelism and increases electoral support in Benin and the Philippines. We instead focus on the interaction between rival parties, where the head-to-head debates were designed to reveal information about the relative quality and policy differences between candidates. Information on the complete choice set straightforwardly helps voters identify the candidate associated with the highest utility level (in the tradition of Hotelling [1929]); and matters more if voting exhibits context dependence, where relative comparisons are also relevant (Callander and Wilson [2006]). Both sets of studies generate optimistic results about the potential for information provision to help strengthen democratic processes in new or weakly institutionalized states.

3. Research Design

We designed our experiment to test the different steps in the accountability chain which runs from more informed voters to more accountable politicians. Specifically, we ask four main questions: do debates impact voters? Do they impact candidates on the campaign trail? Do they affect the performance of elected politicians? And if so, which mechanisms explain these results? Answering these questions required randomization at three distinct levels, as shown in Figure 1 and described in detail below. While the accountability chain runs from the bottom upwards (from voters to politicians), in describing our research design we follow the sequence of randomization which starts at the top, with politicians and constituencies, and proceeds downwards through

⁶ See Wantchekon (2003), Fujiwara and Wantchekon (2013), and Wantchekon *et al.* (2015).

polling stations to eventually reach individual voters.

Lottery #1 Testing whether being part of debates as a candidate changes one’s behavior as an elected MP requires randomizing at the constituency level, our highest level of aggregation. We randomly assigned 14 out of 28 constituencies to host a debate. We chose the 28 constituencies based on metrics we thought would predict competitiveness, including vote margins in the previous election.⁷ *Ex post* our sample constituencies were neither the most nor least competitive: the realized 2012 vote margins range from 14 to 75 percentage points in our sample (compared to the national distribution that ranges from 1 to 91 percentage points). Randomization to treatment was stratified on the degree of ethnic-party bias. All randomizations were done on a computer. We revisited the winners in all 28 constituencies 18 months after they were inaugurated to collect data on their performance in office (question 3 in Figure 1). Appendix A compares characteristics of constituencies, candidates and winning MPs across treatment assignment, and shows that this first randomization achieved reasonable balance.

Lottery #2 To test whether debates changed voter and candidate behavior we randomly varied exposure to the debates within treated debate constituencies. Search for Common Ground took debates on a “road show” in the five weeks leading up to the election with public screenings in 112 randomly chosen polling centers across the 14 treatment constituencies (another 112 polling centers were assigned to control). Polling centers are typically a primary school or community center, and are where citizens register (all citizens had to register anew for this election) and later vote. SFCG projected a video of the relevant debate at a convenient public place, usually on the side of the polling center itself. These events drew large crowds and lasted for a couple of hours. Survey teams later visited voters in all 224 polling centers on and around Election Day to understand what voters learned from the debates, how it affected their views on policy, and whether it impacted their vote choice (question 1 in Figure 1).

To reduce spillovers, we sampled 224 polling centers that are somewhat smaller (471 total registered voters) and located further away from their nearest neighboring center (2.4 miles) than

⁷ The other metrics we used to predict competitiveness included ethnic-partisan bias favouring one party over the other and whether the seat recently changed hands.

the population in general. Typical protocols for the screenings were as follows: communities were notified in advance and invited to attend the screening; 25 randomly selected residents were offered a small incentive (10 cooking spice cubes) to attend (and 16 of them were surveyed at the time); the video was played once in a pause and play format that inserted translation into the relevant local language; and then played a second time without translation. Secondary screenings were held earlier in the day in the largest accessible satellite communities (85 in total). All survey respondents (for lotteries 2 and 3) were sampled from a household listing of registered voters that we conducted a few months before these interventions. Appendix A compares voter characteristics across treatment assignment and validates the polling center-level randomization. Overall, the mobile cinema visited one quarter of all polling centers in these fourteen constituencies. As the centers were small and not everyone in the catchment area attended, we estimate (very roughly) that 6% of registered voters in these constituencies were directly exposed.

Candidates are not static agents. If debates change voter knowledge and decision making this will alter candidates' optimal allocation of campaign effort and expenditure. We use lottery 2 to test this proposition: namely that having more informed voters makes an area more contestable, increases the return to effort, and thus increases effort around treatment polling centers. Recall that while we did not inform candidates about where screenings were held, these salient public events would be easy for candidates to track after they occurred. Here we estimate whether candidates respond to public information shocks by doubling down with greater campaign effort or by avoiding these newly informed voters (question 2).

Lottery #3 The third and final lottery aims to unpack different potential mechanisms through which debates might impact voter behavior. Debate screenings are a compound treatment with many elements including the different types of information conveyed in the debate itself, plus the accompanying public gathering, surveys and extra campaign attention. To isolate mechanisms we administered different slices of the debate and survey experience to voters, via a tablet device viewed privately at their residence. These individual-level experimental arms were implemented in 40 polling centers that do not overlap with the public screening lottery sample.

Within each of these polling centers, we randomized individuals to six distinct treatment arms: (i)

debate treatment, where individuals were shown the exact same video from the public screening but now on a tablet; (ii) “get to know you” treatment, where individuals were shown a short video of the candidates speaking informally about their hobbies and interests; (iii) “radio report” treatment, where individuals listened to a journalistic summary of the main policy positions articulated by the candidates during the debates; (iv) surveyed control, where individuals were given the same survey as the one that accompanied treatments i to iii, but were not shown any media; and (v) pure control, where individuals were not surveyed until Election Day, and whose only contact with the research team at time of treatment implementation was to record basic demographics. A sixth arm participated in a lab-in-the-field experiment (analyzed in related work) that exposed voters only to photos and 20 second video clips of candidates (to test for example, whether voters could infer candidate ethnicity from physical appearance). No other political information was conveyed and this arm is thus grouped with the controls. We assigned 400 individuals per treatment arm and 600 to the surveyed control group. Appendix B presents voter characteristics across treatment arms and validates that this third individual randomization created reasonably balanced groups.

4. Hypotheses and Econometric Framework

If debates are to increase the accountability of politicians, they must start by creating more informed voters who are willing to change how they vote based on this information. We thus start by testing whether debates were successful in informing voters (hypothesis 1). To understand how being more informed changed voter decisions, we test 4 related hypotheses. Do debates: increase policy alignment between voters and their chosen candidate (H2); persuade voters to adopt their preferred candidate’s policy stances (H3); increase vote shares for the candidate that performed best in the debates (H4); and increase voter willingness to vote across party lines (H5)? In an environment of strong ethnicity-based loyalties, we thought it important to test whether exposure to rival candidates in a neutral forum increases openness to other parties, even if it does not change how people vote (H6). Finally we examine the impact of debates on voter turnout (H7). As discussed in Section 7 these hypotheses, associated outcome measures, and econometric specifications were pre-specified, as were those in subsequent sections about candidates, elected MPs and mechanisms.⁸

⁸ We lodged a pre-analysis plan (PAP) that details the hypotheses, outcomes, and econometric specifications for

We estimate treatment effects for the public screenings as follows:

$$Y_{ipc} = \beta_0 + \delta T_{pc} + \mathbf{X}'_{ipc} \boldsymbol{\Pi} + \mathbf{Z}'_{pc} \boldsymbol{\Gamma} + \mathbf{W}'_{ipc} \boldsymbol{\Psi} + c_p + \varepsilon_{ipc} \quad (1)$$

where outcome Y (e.g. vote choice) is measured for individual i registered in polling center p within Parliamentary constituency c ; T is an indicator variable equal to one if the polling center received a public screening (in lottery 2); \mathbf{X} is a vector of indicator variables that denote the stratification bin from which exit poll respondents were drawn (based on age and gender); \mathbf{Z} is a vector of indicator variables that denote the stratification bin from which the polling center was drawn (based on the number of registered voters and distance to nearest neighboring center); \mathbf{W} is a vector of individual controls (years of schooling and radio ownership); c is a set of constituency-specific fixed effects (the level of debate); and ε is an idiosyncratic error term clustered at the polling center level. The coefficient of interest is δ , which captures intention-to-treat effects, where 82% of exit poll respondents in treated polling centers indicated that they attended a debate screening, as did 4% of those in the control group.⁹

We apply the same framework to analyze the mechanisms driving voter behavior. The only difference is that we now examine the relative effects of multiple treatment arms, in the form of:

$$Y_{ihtpc} = \beta_0 + \delta T_{htpc} + \mathbf{X}'_{hpc} \boldsymbol{\Pi} + \mathbf{Z}'_{pc} \boldsymbol{\Gamma} + \mathbf{W}'_{ihtpc} \boldsymbol{\Psi} + c_p + \varepsilon_{ihtpc} \quad (2)$$

where outcome Y (i.e. vote choice) is measured for individual i living in household h assigned to treatment arm t registered in polling center p located in Parliamentary constituency c ; T is a dummy variable indicating assignment to treatment arm t (in lottery 3); \mathbf{X} is a vector of indicator variables that denote the stratification bin from which the household was drawn¹⁰; and \mathbf{Z} , \mathbf{W} , c and ε remain

analysis of voters and candidates on November 20, 2012, while the exit poll survey (the main source of data) was still in the field. At that time, we planned an iterative series of data analysis and updates to the PAP as we moved from one experiment to another. This, however, created problems of credibility and communication, which we discuss in Section 7. In response to these concerns, we have scrapped all subsequent revisions to the PAP and implement the analysis as originally specified in this first plan for Tables 1, 2, 3 and 5. A separate plan was lodged on 2 June 2014 to govern analysis of elected MP behavior (presented in Table 4), while data collection on CFF spending and constituency engagement was still in the field. The PAPs are in Appendix C and D where we flag minor deviations with endnotes. We registered our trial with the American Economic Association's registry when it opened (<https://www.socialscienceregistry.org/trials/26>).

⁹ To avoid differential attrition or selection across the road show assignment, all exit poll respondents were drawn from the original household listing (in both treated and control polling centers).

¹⁰ To stratify, we first divided households into bins based on the gender composition of registered voters (as collected in the earlier household listing), assigned treatment arms at the household level within each bin, and then selected one respondent per household to participate.

as defined in (1). For each treatment arm, the coefficient of interest is δ_t , the average treatment effect for treatment t compared to the control group, where controls include the surveyed and “pure” control arms as well as the lab-in-the-field arm (that delivered no political information). Tests of relative effects take the form $\delta_t \neq \delta_{-t}$. As we had perfect compliance and minimal attrition (6 percent overall), average treatment effect estimates for the individual treatment arms are comparable to treatment-on-the-treated effects.

For candidates, we have only one hypothesis: that candidate allocation of campaign effort and expenditure is responsive to debate publicity. We are interested in whether campaign investments complement or substitute for public debate screenings. This hypothesis captures an endogenous response of candidates to the road show. To test it, we use the same econometric specification as in (1), save the outcomes are linked to individual candidates: e.g., an outcome Y (such as receiving a gift) is measured for individual i in relation to candidate m where the individual is registered in polling center p within constituency c . This analysis thus leverages detailed campaign data on individual voter-candidate pairs.

We estimate effects for both individual outcomes and hypothesis-level indices (following Kling, Liebman and Katz [2007]), and adjust standard errors to account for the number of tests we run within and across hypotheses (following Benjamini, Krieger and Yekutieli [2006] and Anderson [2008]). We also report the per comparison, or “naïve,” p -value for all estimates, which are appropriate for those with an *a priori* interest in the specific outcome or hypothesis presented. Note that it is easier to adjust for the number of regressions run when these regressions are pre-specified (see Section 7). Where theory only supports one direction of effect we bolster statistical power by using pre-specified one-sided tests. Unless otherwise stated, results in our discussion are based on one-sided tests in the direction indicated in the hypothesis statement.¹¹

Answering our last question, about how debates impact policy, necessarily moves us forward in time, to June 2014, which is eighteen months after the MPs took office. Here we test whether

¹¹ One sided tests make little substantive difference in this analysis: overall, we report treatment effects for 45 individual outcome measures concerning voters, candidates and politicians in Tables 2, 3 and 4. Twenty seven estimates have p -values less than 0.050 under our preferred specification. Of these, five estimates fall below the 95% confidence level when we remove controls and conduct two-sided tests, where the highest resulting p -value is 0.105.

participating in a debate as a candidate affects the subsequent effort and performance of elected officials. We organize outcomes under four policy areas, namely that participation in debates increases: i) development expenditure under the constituency facilitation fund (CFF); ii) constituency engagement; iii) activity in Parliament; and iv) consistency with pre-election promises.

We estimate the following model:

$$Y_{ic} = \beta_0 + \delta T_c + \mathbf{X}'_i \boldsymbol{\Pi} + \lambda_c + \varepsilon_{ic} \quad (3)$$

where Y is outcome for MP candidate i who won the seat for constituency c , T is an indicator signaling that the constituency was assigned to a pre-Election debate, \mathbf{X} is a vector of MP-level controls {gender, public office experience} selected by their contribution to increasing the R^2 in analysis of the control group data¹², and λ_c are fixed effects for the randomization strata from lottery 1 (three bins of ethnic-party bias measured at the constituency level). Tests are one-sided in the direction of better performance. Given the small sample at this level, standard error estimators that are robust to heteroskedasticity are likely downward biased. To reduce this bias, we present standard errors that are the maximum value of conventional ordinary least squares and bias corrected HC₂ estimators in MacKinnon and White (1985), following discussion in Angrist and Pischke (2009). We do not have power to adjust for multiple inference at this level.

Outcome data draw on several sources. We surveyed all candidates in treated and control constituencies pre-election, and surveyed the 28 winning MPs shortly after the election. The 14 treated winners were also given a video of the debate they participated in, edited to include only their own statements, and told how many voters had seen their debate. Performance outcomes for the winners were drawn from Parliamentary administrative records, MP self-reports, and extensive fieldwork in their home constituencies.

5. Results

5.1. Effects of Public Debate Screenings on Voters

¹² We did not pre-specify the control set. As a robustness check, Appendix H presents results for a conservative specification that excludes these controls and further uses 2-sided tests. Only one estimate that is significant in our preferred specification falls (just) below 90% confidence in the robustness check, with associated p -value of 0.105.

Table 1 presents an overview of how voters respond to MP debates, organized around our seven hypotheses. We find that exposure to public screenings increases political knowledge, moves voters into better policy alignment with their selected candidate, increases vote shares for candidates who performed the best during the debates, and enhances voter openness to participating candidates. Treatment effect estimates for these five hypothesis-level indices are significant at above the 95% confidence level when considered on their own, and generally remain above 90% confidence under various adjustments for multiple inference and allowance for two sided tests.¹³ We find little support for the hypotheses that debates affect voting along ethnic-party lines or turnout (H5 and H7).

More specifically, watching debates increases the mean effect on political knowledge by 0.30 standard deviation units (standard error 0.03) across the 16 individual outcomes included. To give a sense of magnitude and substantive content, Table 2 unpacks this index into its component measures. For example, the percentage of voters who could correctly state the amount in the constituency facilitation fund (CFF), allowing for a generous range around the true figure of 43.8 million Leones, rises from 3.4% in control centers to 17.4% in treatment areas, a fivefold increase. Voters also gain a better sense of what elected officials are meant to do in office: the number of correctly reported MP roles and responsibilities increases significantly. They learn about policy stances: for each of (up to) three participating candidates, on each of three national policy issues, voter ability to correctly place the candidate on the specific policy spectrum increases significantly (at 99% confidence) for 8 of 9 estimates. As an example, the proportion of voters who could correctly identify the SLPP candidate's first priority for government spending doubles, from 14 to 29%.

The statistical strength of these results is largely unchanged when we adjust p -values to control for the false discovery rate (FDR) across all 30 outcomes in Table 2, and together suggest that watching debates substantially increases voters' political knowledge. Recall that respondents experienced a one- to five-week lag between exposure to debates and the exit polls, indicating that the gains in knowledge were relatively persistent. We find little evidence of heterogeneous effects,

¹³ The estimates for H3 Persuasion and H4 Vote for Best fall to 80% confidence under the conservative family-wise error rate correction in column 4.

save that women appear to learn somewhat less from the debates than men (Appendix E).

The next step in the accountability chain requires that these gains in knowledge translate into changes in the way people view policy and whom they vote for on Election Day. The first positive evidence for this is that voters use their enhanced policy knowledge to move into better policy alignment with their chosen candidate. Alignment is measured as a match between the voter's reported policy position in the exit poll and the position the candidate they voted for expressed during the debate. Estimates suggest that debate exposure increases policy alignment by 0.104 standard deviation units (s.e. 0.035) on average across three national policy issues discussed during the debates, which is highly significant (Table 1). To provide a sense of magnitude, consider the results in Panel B of Table 2. The empirical match between the voter's first priority issue and the view articulated by their chosen candidate during the debate increases by 9.0 percentage points (s.e. 3.1) on a base of 42.5%. We find similar effects for free healthcare. We see no effect for the gender equity bill, although note that there was little divergence in views expressed during the debates (only two candidates voiced strong objection to the bill).

What drives this improvement in policy alignment? There are two potential mechanisms discussed in the literature: voters choose candidates based on previously determined policy preferences (as predicted by canonical proximity voting models, originating with Hotelling), or they update their policy positions based on comments from the candidates (see Abramowitz 1978 and Lenz 2009 for evidence from the Carter-Ford Presidential debates). While we originally hoped to be able to parse these two channels, and hence have two distinct hypotheses about alignment and persuasion, our research design is not well suited to do so. The central limitation is the lack of baseline data on policy preferences for the control group. While we explored some strategies to work around this—like comparing rates of policy alignment between party stalwarts and more loosely attached voters, or looking at more or less partisan policies—none of them proved particularly satisfying. What is clear is that voters strongly moved into alignment, regardless of the channel. Below, we explore which aspects of the debate experience appear to drive this convergence.

Fundamental to strengthening the accountability between voters and politicians is whether information provision changes how people vote. We find that voting patterns in polling stations

where debates were screened are statistically different from those in control areas. Specifically, we document a significantly larger share of votes cast for the candidates who performed best during the debates. Estimates for the hypothesis-level index suggest an increase of 0.076 standard deviation units (s.e. 0.044), significant at 95% confidence on a per comparison basis (which remains above 90% confidence for three of the four standard error adjustments in Table 1). This index compiles two measures of debate performance: one determined by the audience and another by our expert panel. Audience judgments were recorded in a survey that immediately followed the implementation of the group-level screening. We define the best performer for voters in a given polling center based on the opinions of voters in all other centers, excluding the center of interest. The expert panel consists of twenty-five members of government and civil society who watched the debate videos and scored candidate responses to each debate question (note that even for these experts, debate performance is an inherently subjective concept). These two sets of evaluations coincide on who performed best in 10 of the 14 debates. Where they diverge, the expert panel was more likely to pick a less popular candidate, including one from the PMDC, the smallest party that was not very competitive in this election (they won no seats nationwide).

Table 2 Panel C reports treatment effects for these two measures in our exit poll data, and adds two comparable measures defined in the National Electoral Commission's (NEC) official polling-center level returns.¹⁴ The correlation between party vote shares measured across the two datasets is 0.93 for the APC and 0.92 for the SLPP, suggesting that misreporting of vote choice in the exit polls is not a major concern. All four treatment effect estimates for votes for the debate winner are positive, and three are significant at 95% confidence. The estimate that is largest in magnitude is votes for the candidate that audience members judged to have performed best, measured in the exit polls, where we see a 4.4 percentage point (s.e. 2.2) increase in votes for the debate winner. The corresponding estimate using the official NEC returns is 3.1 percentage points (s.e. 1.7). While the two are not statistically distinguishable from each other, it makes sense that the point estimate in the NEC data is smaller, since the returns include votes from peripheral villages not exposed to treatment. Note that vote shares for candidates who won the debates were already high (71% in

¹⁴ The NEC sample excludes one constituency where the SLPP candidate was disqualified immediately before the Election but his name remained on the ballot. A full 48% of ballots cast were deemed invalid (many of which were likely SLPP votes). The winner was eventually determined via the courts. Treatment effect estimates are similar with its inclusion (0.032**, s.e. 0.015 for expert panel and 0.029**, s.e. 0.016 for audience pick, $N = 224$).

the NEC returns for control areas), indicating that in this set of constituencies, the candidate who was locally popular tended to also perform best during the debates.

The environment in which debates took place was one where vote choice is heavily correlated with ethnicity. If voters changed how they voted based on the information conveyed by debates, a naïve assumption is that this would necessarily be associated with an increase in cross-ethnic voting. Instead what we find is that while voters move back and forth across ethnic-party lines to support strong debate performers, this has no net effect on the overall incidence of ethnically-aligned voting. In Table 1, the coefficient for the mean effects index for hypothesis 5 is small in magnitude and not statistically distinguishable from zero. How can we reconcile a four percentage point shift in votes toward the debate winner, with no commensurate change in voting along ethnic lines? First, note that a move toward the debate winner only leads to crossing party lines if the voter is from a rival ethnic group. Voters traditionally loyal to the debate winner should neither change their vote nor cross ethnic lines after exposure. This is what we see in the data. For historically aligned voters, there is no treatment effect (1.6 percentage points, s.e. 1.4) of watching the debate on their vote choice, as presumably they were already planning to vote for that candidate. These voters constitute 81% of the study sample and have baseline rates of 90% voting for the aligned candidate (i.e. debate winner) in the control group. By contrast, voters from ethnic groups historically affiliated with the rival party (i.e. the candidate running against the debate winner), represent only 7% of the sample and have a much larger treatment effect estimate of 10.6 percentage points (s.e. 7.5), which is significant at 92% confidence in a one-sided test.¹⁵

Second, we should expect more voters to move toward the debate winner where the rival party candidate strongly outperforms the local favorite. Consistent with this, for the subsample where the audience deemed that the “outsider” candidate (who received only 26% of votes in the control group) won the debate, the treatment effect on votes for the winner is four times larger than in the full sample (19.1 percentage points, s.e. 11.0, $N = 381$) and significant at 94% confidence in a one-sided test. Thus the effects on switching one’s vote to the debate winner are concentrated in “upset”

¹⁵ The remaining 12% of the sample are voters from ethnic groups that do not have strong historical ties to either party, so are excluded from the crossing party lines estimate. About half (57%) of these voters chose the debate winner in the control sample. The point estimate on the treatment effect for this group is also large, at 10.1 percentage points (s.e. 8.4), but not statistically significant (one-sided p -value of 0.115).

contests where the lagging candidate strongly outperformed the frontrunner. Note that these two tests were not pre-specified and are implemented on small subsamples, so should be considered exploratory in nature.

Estimates for our sixth hypothesis suggest that exposure to the debates enhances voter openness to candidates, as measured by ten point likeability scales. In Table 1, we see that the treatment effect for the mean effect index is 0.113 standard deviation units and highly significant (s.e. 0.049). Voters update positively for both candidates from their own and from their rival party in Table 2, although not significantly so. Consistent with basic learning models, the strongest updating appears to be for the lesser known third party candidates. The fact that voter appraisals rise across the board is reassuring in a world where political opponents are often demonized and a context where violent clashes between supporters of different parties are not unknown. It is also important for securing candidate participation in future debates.

Lastly, we find little evidence that exposure to debates affects turnout. The mildly negative estimate in the exit poll data is countered by a null result in the official NEC returns in Table 2, and by a positive estimate in the private viewing experiments (results not shown). Note that baseline turnout is very high, at 89 percent in the NEC data, leaving us little power to detect effects in either direction.

5.2. Endogenous Response by Candidates

How do candidates on the campaign trail respond to these large public information shocks? Table 3 presents evidence that candidates increase their campaign effort and expenditure in areas where debate screenings are held. Estimates capture effects on voter reports of having received a gift from the particular candidate, the monetary value of the gift (expressed in logs), and the number of times the candidate visited the community, all with reference to the weeks leading up to the election. The treatment effect for the hypothesis-level index is 0.103 standard deviation units (s.e. 0.039), significant at 99% confidence under a two-sided test.

All nine components of the index, covering MP candidates from each of three parties and each of three campaign outcomes, are positive in sign (panel B). The two main parties, the APC and the

SLPP, show a relatively consistent 16 percent increase in effort in treatment areas averaged across all measures, though no individual measure is statistically different from zero. Third party candidates, who generally had less of a chance of winning, appear to respond more strongly to the road show: estimates for each of the three PMDC campaign measures are statistically significant and represent a 170 percent increase in effort averaged across the three measures. To provide a sense of magnitude, just under ten percent of voters receive any gift, and this rate increases by around one percentage point (or nine percent) in treatment areas (Panel C). The log value of gifts received increases by 31 percent. If we convert this back to the underlying raw values, essentially most people receive nothing but for those that do, the value of gifts increases from roughly 2.1 to 2.8 dollars in total across candidates. Visits from candidates rise from an average of 1 to 1.2 per community. We find little evidence of shifts in campaign effort by candidates for other elections (e.g. Local Council) or party officials in response to the road show (Appendix F).

What drives this reallocation of campaign effort? One explanation is that by equipping voters with greater political knowledge and changing their voting choices, debate screenings make these areas more competitive. This would be consistent with a standard swing voter model (Lindbeck and Weibull [1987]). Extending the exploratory analysis above (and again noting that it was not pre-specified), the treatment effect on the campaign index is five times larger in the constituency where the “outsider” candidate won the debate (at 0.41 standard deviation units, s.e. 0.16) compared to the other constituencies in the sample, which is precisely where the debates have the largest impact on the competitiveness of the race. The coefficient on this difference (0.33, s.e. 0.16) is significant at 95% confidence. Note, however, that the coefficient for the remaining constituencies, where the screenings *de facto* make the races less competitive as the locally popular candidates performed better in the debates, remains positive and statistically significant at 95% confidence (0.08, s.e. 0.04). This can be reconciled with the idea of greater competition if the debates makes vote shares in screening communities more uncertain, as recall that the actual impact of the debates on voting is not revealed until Election Day. This is consistent with the extended model in Casey (2015), where information increases voter responsiveness to individual candidate attributes, thereby making it harder to infer vote shares from the ethnic composition of a locality, and thus widening the set of potentially competitive areas.

To assess whether this spending works to reinforce or unwind the impact of the screenings, we explore whether the intensity of the campaign response covaries with candidate performance during the debate. Figure 2 reveals an inverted U-shaped relationship between the size of treatment effect on campaign expenditure and the share of audience members who said that candidate won the debate (Panel A).¹⁶ This suggests that the campaign response to the road show is strongest where the debates themselves were most closely contested. There is also some asymmetry in the tails, where the treatment effect estimate for candidates who received the fewest audience votes is negative (at left) while estimates for those who received many votes (at right) are positive although noisily estimated. The relatively stronger campaign response by those who performed well versus poorly works to reinforce the impact of debates. Panel B presents the same estimates for third party candidates and shows that they respond most strongly where they performed well during the debate. This again suggests that the spending response is strongest where the debates worked to increase the competitiveness of the race.

5.3. Policy Response by Elected Members of Parliament

The final step in the accountability chain, and one that is rarely tested, requires moving from effort during the election to the effort and behavior of the winning candidates once in office. Table 4 presents results for measures of public spending, constituency engagement, activity in Parliament, and consistency with pre-election policy priorities. Estimates for the mean effects index across all 9 underlying outcomes is 0.298 standard deviation units, which is significant at 95% confidence under a one-sided test. While this result that post-Election policy responds to candidate debates is substantively important, it is estimated on a limited sample and is thus more speculative than results presented for voters and campaign spending.

Beginning with the outcome that most directly enhances constituency welfare, we find significantly higher spending on development projects by MPs who participated in a debate as candidates. Recall that the constituency facilitation fund (CFF) is an annual allotment of 43.8 M Leones (approximately US\$ 11,000) intended to support the development of, and the MP's own transport to, their constituency. MPs are fairly unconstrained in how they spend this money and are not subject to monitoring or reporting requirements. During the debates, each candidate was

¹⁶ Estimates control for the underlying ethnic-party loyalty of the constituency.

asked to articulate their plans for spending the CFF. All candidates, save one, promised to spend some, if not all, of the funds on development projects. To compile data on how the CFF was actually spent, we first surveyed each elected MP to generate a detailed itemized list of expenditures and project locations for the first CFF allotment. Our research teams then conducted exhaustive field work to verify these expenditures in the MP's home constituency, which involved in-person visits and physical examination of all purported projects, and multiple interviews with community leaders, clinic staff, teachers and residents of villages where money was reported to have been spent. We did not attempt to verify the MP's own transport expenses, so unaccounted for funds represent either legitimate travel costs or leakage. Note, however, that substantially larger travel expenses in the control group is not consistent with the evidence below that control MPs held fewer meetings with their constituents.¹⁷

For the control group, Table 4 shows that only 36 percent of the \$11,000 allotment could be verified as spent on the development of the constituency. The treatment effect estimate of 54.7 (s.e. 31.7) suggests that MPs who participated in the debates spent more than 2.5 times as much on verifiable development expenditures. The effect is significant at 95% confidence and the point estimate corresponds to average gains of roughly six thousand dollars per constituency. Appendix G transparently plots the distribution of this outcome by treatment assignment. Comparing the two subplots shows that the positive treatment effect estimate is driven by differences in both tails: there are more low values among control MPs and more high values among treated MPs. Estimates are robust to dropping the top outlier (treatment effect of 46.5, s.e. 29.1 and one-sided p -value 0.06) and to using a binary outcome measure equal to one for any nonzero amount of verified development spending (treatment effect of 0.29, s.e. 0.18 and one-sided p -value 0.06).

We also find positive and significant effects of debate participation on subsequent constituency engagement. Participating MPs made on average 1.3 (s.e. 0.6) additional community visits, on a base of 2.9, and held 1.1 (s.e. 0.6) more public meetings, on a base of 1.0. These represent increases of 145 and 210 percent, respectively, and are both significant at 95% confidence under one-sided tests. Overall, these positive results are fairly robust. Treatment effect estimates for the hypothesis-

¹⁷ It also cannot be explained by differential distance to the capital or availability of major roads as both of these characteristics are well balanced across treatment assignment (Appendix A).

level index and all three individual outcomes discussed remain at or above 89% confidence in the conservative specification with no controls and two-sided tests (Appendix H). Estimates for a Type S (for “sign”) error rate on the hypothesis-level index are reassuringly low over a reasonable range of scaled down true effect sizes.¹⁸

By contrast, we find little evidence for treatment effects on the activity level of elected MPs during sittings of Parliament or on their consistency in promoting their pre-Election priorities. Outcomes cover the period from when MPs were inaugurated in December 2012 through the end of 2013, or 57 sittings in total. Specifically, there is no difference in the number of sittings attended, committees joined or public statements made in Parliament (note the low control mean of four statements). Regarding policy consistency, we defined the priority sector for each MP based on their pre-election response to the question, “If you had to prioritize one issue in Sierra Leone to receive additional funding in the national budget, what issue would you prioritize?” The modal response was education (44 percent), followed by roads, health and agriculture (each with 15 percent). Treated MPs, whose answers were publicized in the debates, were no more likely to make public statements during a Parliamentary agenda item concerning their preferred sector, although note that only one MP in the sample did so. They similarly were no more likely to join committees dedicated to that sector, and their constituents are no more likely to report that they focus on that sector. We were not able to evaluate consistency in voting in line with pre-stated positions on key national policy issues of interest, as relevant bills have either not yet been introduced (including the gender equity bill) or were passed unanimously (including a freedom of information act).

What drives the positive overall policy effect? A leading interpretation is accountability pressure: many more voters now know how much money the MP has at his disposal, know what he promised to spend it on, and are familiar with the roles and responsibilities of office. These more informed voters could potentially take political action, and the MPs are aware of this threat. At the same

¹⁸ Gelman and Carlin (2014) recommend reporting the Type S error rate when working with noisy estimates. A Type S error is the probability, for a given true effect size, that a hypothetical replication yields an estimate with the incorrect sign, conditional on it being statistically significant. If the true effect on MP accountability equals what we found for candidates’ campaign response (roughly one third of the accountability estimate), the error rate would be five percent, which is reassuringly low. It is only when we scale down the true effect size by a large amount that we begin to see nontrivial Type S error rates: for example, if the true effect size is only one eighth of our estimate, the error rate would be 27 percent.

time, the watchfulness of the media might be more salient to treated MPs, as they interacted with SFCG during the debate.¹⁹ Standard media models suggest that both are needed: it is the combination of an engaged public and an observant media that generates accountability effects.²⁰ A third explanation is that asking candidates to make a plan for how they would spend their CFF increased their likelihood of sticking to this plan, just as asking voters to make a plan for voting has been found in some cases to increase the likelihood of individuals undertaking the cost of turning out (Nickerson and Rogers, 2010).

If debates made voting more responsive to competence, these effects could instead be explained by electoral selection. This we can rule out conclusively, as recall that these races proved to be not particularly competitive: the vote margin in the most competitive race was 14 percentage points. Even if we apply our largest estimated treatment effect on vote shares, which was 19 percentage points where the “outsider” won the debate, and assume 100% screening attendance by voters registered to any of the one quarter of polling centers visited by the road show, the resulting movement in vote shares (4.75 percentage points) would not have changed the outcome of any of the fourteen elections. If the program were scaled up, however, there could be potential impacts on MP selection.

Relatedly, it is highly unlikely that these results are driven by candidate selection by parties. As we gave the central party bosses a list of planned debate constituencies shortly before candidate registration closed, they could have strategically responded by allocating different candidates to those races. If the attributes the parties thought were associated with favorable debate performance also correlated with performance in office, then the treatment effect would be operating through a change in the candidate pool instead of the accountability and commitment channel. While this would constitute an exciting general equilibrium response worth exploring in future, it is unlikely to hold in this experiment. Most importantly, the debates themselves began well after the close of candidate registration, so the parties would have had to reshuffle candidates in anticipation of a new, unproven concept, and they had very little time to do so. Moreover, Appendix A presents

¹⁹ By contrast, the salience of the research would have been equivalent for treated and control MPs, who were asked the same questions pre- and post-Election, and were not contacted about their CFF expenditures until a year later.

²⁰ In another context, this might suggest a weaker response by term limited politicians, however there are no term limits for MPs in Sierra Leone.

little evidence that candidate characteristics vary systematically across constituencies assigned to debates participation and controls: while candidates in treated constituencies had somewhat less political experience; measures of age, gender, years of schooling, managerial experience, ethnicity and pre-election quiz scores are all comparable across the two groups.

6. Mechanisms

What is it about debates—the revelation of policy positions or the showcasing of candidate personalities—that voters respond to? Does it matter if the debates are viewed in a communal or private setting? Perhaps our results have little to do with the debate itself and capture the effects of interviewing people in-depth about their political views in the lead up to an election. In this section we seek to unpack mechanisms and better understand which aspects of the debate experience drive the observed effects on voters.

6.1. Parsing the Effects of Different Types of Information

We designed the private viewing experiments (lottery 3) to decompose debates into their core informational components and assess which pieces of information matter most for changing voter behavior. Results at the top of Table 5 suggest that voters update their views of candidates based on both policy and charisma, but that only access to the combination of both types of information motivates them to change their vote.

Panel A compares voter response to the debates, the get to know you videos, and the radio reports (all delivered privately via tablet). Estimates in column 1 show that all three arms were effective in transmitting political information: the treatment effect on the mean effect index is positive and significant at 99% confidence for each. As expected, the coefficients for debates (0.135, s.e. 0.024) and the radio reports (0.111, s.e. 0.021) are much larger than that for the get to know you videos (0.042, s.e. 0.016), differences that are highly statistically significant. This is especially true (by design) for placing candidates on the three policy spectra, where there is no effect of the get to you know video. Interestingly though, voters discerned just as much useful information about candidate characteristics—like who was better educated and which one had more public office experience—by watching the five minute get to know you video as they were after watching 45 minutes of debate (results not shown). These topics were generally not asked directly, but could

plausibly be inferred from the candidate's manner of speech, physical carriage, or confidence. While the overall impact on knowledge is slightly larger for debates than the radio reports, the difference is not statistically distinguishable from zero (row 5).

Notably, only debates moved voters into better policy alignment with the candidates they selected. The treatment effect for debates (0.080, s.e. 0.029) is positive and significantly larger than that for the other two arms, which are both indistinguishable from zero. For the get to know you videos, this is intuitive and consistent with the null result on policy knowledge. For the radio reports, however, it implies that the acquired knowledge of policy positions did not translate into better policy alignment as it did for the debates. Similarly, only the debates arm had an impact on votes for the debate winner (0.058, s.e. 0.040), which is statistically larger than the result for the radio reports. The fact that radio was equally as effective in building knowledge, but only debates impacted policy preferences and voting choices, suggests a key role for personality in persuading voters to change their behavior. This resonates with results in Druckman (2003), who revisits the first Kennedy-Nixon Presidential debate in the U.S. and finds that watching the debate on television has more pronounced effects than listening to it on the radio (see also McKinnon, Tedesco and Kaid [1993]).

6.2. Public versus Private Dissemination

Why were the treatment effects observed in the group screenings experiment (lottery 2) larger in magnitude than those found for the individual private viewing experiment (lottery 3)?²¹ Since many aspects of the experience differ across these two modes of debate delivery, we will not be able to pin down exact mechanisms, but can speculate on potential key drivers of the divergence.

A key difference is that public screenings involved large public gatherings of a couple hundred people, while the individual treatment had respondents watch the debate alone on a tablet.²² Consistent with a substantive role for social mobilization, lab experiments show that exposure to

²¹ Communities were not randomly allocated into the public and private dissemination experiments even though both experiments took place in the same constituency at the same time. Thus in this section we are comparing coefficients from two separate experiments, not coefficients from two arms in the same experiment.

²² The content of the debate films was exactly the same under the two conditions. Other differences in delivery are that individual treatments were administered in larger polling centers (as measured by total registered voters); and the implementation procedures varied, where group screenings played music before the debates, played the debates twice, and had simultaneous translation into the relevant local language.

the reactions of audience members—either real or fabricated—affects evaluations of debate performance and candidate attributes (Fein, Goethals and Kugler [2007], Davis, Bowers and Memon [2011]). The public nature of group screenings may also generate common knowledge that eases coordination problems and reinforces the messages conveyed (Chwe [2001]). Note that the papers by Wantchekon and co-authors cited earlier all involve public treatments, where groups of voters come together in town hall meetings. The second difference is that it would have been very difficult for candidates to track the locations of the individual experiments and respond with greater campaign expenditure. Assuming that voters value the additional candidate visits and gifts, the uptick in campaign effort could contribute to a larger total effect for the group screenings.

Panel B of Table 5 presents the cleanest comparison of the two delivery mechanisms by limiting the group screening estimates to the eight constituencies where the individual treatments were also implemented, and restricting the individual estimates to comparisons between the debates and pure control arms. First, note that the qualitative pattern of effects for the two delivery modes is the same: strong positive treatment effects on political knowledge, policy alignment, and votes for the debate winner. Second, note that the treatment effect for the group screening is larger in magnitude than that of the individual viewing everywhere save on votes for the best performer, where it is equal. This difference is even more pronounced if we scale up the intention-to-treat effects for the group screening to estimate average treatment effects on compliers, which is more directly comparable to the individual treatments where compliance was near perfect. For political knowledge, for example, the coefficient increases to 0.426 which is two and a half times the size of the private viewing estimate. These differences are consistent with the idea that watching the films in a group setting facilitated discussion among voters that clarified and reinforced the information about candidates and policy conveyed by the debates. The fact that point estimates for votes for the debate winner are the same across modes suggests that any impact of additional campaign effort did not translate into differences in vote choices, perhaps because the candidates who responded most strongly were from the relatively uncompetitive third party.

6.3. Survey Priming

How much of the effects on voters can be attributed to the content of the treatment itself as compared to the experience of being surveyed in depth about one's political views? This

distinction is important in light of findings that the act of surveying has nontrivial impacts on behavior (Zwane *et al.* [2011]). Using two separate estimation techniques, we find significant priming effects on general political knowledge. Reassuringly, all results hold net of these effects.

In Panel A of Appendix I, we estimate the effect of being asked questions about politics for those who did not see the debate. We compare those who are surveyed to “pure” controls in the individual-level experiment. At the time of treatment implementation, surveyed controls were given the same survey that accompanied the debates treatment. This may have primed respondents to seek out information on outcome variables of interest or increased the salience of political information from other sources in the weeks leading up to the Election. By contrast, “pure” controls were asked only basic demographic questions, and were not asked any political questions until the exit polls. Estimates in column 3 suggest that the survey experience on its own led to a 0.034 standard deviation unit (s.e. 0.017) increase in political knowledge. Column 1 compares those in the debate arm to surveyed controls and estimates a 0.146 standard deviation unit (s.e. 0.026) increase in knowledge, which can be attributed to the content of treatment, above and beyond the survey experience. Together, these estimates suggest that survey priming accounts for one fifth of the total treatment effect on political knowledge. There is only one other marginally significant priming effect, however it does not replicate in the larger sample of Panel B.

We next test the effect of being asked about politics amongst those who watch the group screening debate: i.e. whether the survey reinforced the impact of the screenings and helped voters absorb the information in debates. We compare treatment effect estimates for those assigned to treatment with survey versus “pure” treatment (all controls are “pure” controls). Estimates in Column 1 of Panel B suggest that the “pure” treatment effect of watching the debate without being surveyed is a 0.282 standard deviation unit (s.e. 0.034) increase in political knowledge. In column 3, there is evidence for an additional 0.032 standard deviation unit (s.e. 0.021) effect of being surveyed alongside treatment, suggesting that the survey reinforcing effect similarly accounts for roughly a tenth of the total effect on knowledge. For policy alignment and voting outcomes the “pure” treatment effect remains positive and highly significant, and there is no evidence of survey reinforcing effects.

7. A Failed Experiment in Iterative Pre-Specification

The replication crisis in psychology (Open Science Collaboration 2015) and concerns about *p*-hacking in economics (Brodeur et al. 2016) have encouraged economists to explore specifying their analysis plans before embarking on empirical analysis. Given the novelty of this approach, there remains considerable debate about whether and how to use pre-analysis plans (PAPs) in economics (see Casey, Glennerster and Miguel [2012], Olken [2015], Glennerster [2017], and Coffman and Niederle [2015] for discussion of pros and cons). The most obvious constraint is that it greatly reduces researcher ability to learn from the data and optimize her analysis in an iterative fashion. Relieving this constraint was of particular interest to us as we conducted multiple experiments in close succession and hoped to analyze them in sequence, applying the lessons learned from the first to analysis of the second, with intervening steps to pre-specify at each stage.

Specifically, we planned to first analyze the mechanism experiment (lottery 3) and then revise the PAP before analyzing the public screening experiment (lottery 2). In theory, this is unequivocally advantageous: the dynamic adaption does not constitute datamining because the revised hypotheses are tested on a new sample with a new exogenous source of variation. In practice however, this created problems of credibility and communication. Olken (2015) recommends finalizing the details of the pre-specified analysis once the endline data have been collected but before treatment status has been unblinded, as is common in health studies. There are, however, no mechanisms for researchers in economics to credibly demonstrate that they did not look at the data while they finalized their pre-analysis documents. In our case, we did not finalize the revisions to the PAP for the public screening experiment until after we had the exit poll data for 10 months.²³ Infrastructure for credibly committing to analysis plans has been created recently for economics (AEA registry) and political science (EGAP registry), however there is no equivalent infrastructure to lock away data while PAPs are finalized.

Even if there were, the challenges to blinding authors to incoming data are nontrivial. An important role of researchers in running a high quality field experiment is checking the quality of data as it comes in: are enumerators using too many replacements? Are treatments being

²³ During these months we analyzed the mechanism experiment, prepared data collection instruments for MP behavior, and wrote a detailed revised PAP for precisely how we would analyze the screening experiment.

implemented according to plan? Researchers cannot be blinded to location or treatment assignment during this process: they need to know where problems are occurring and send extra support to struggling field teams, and monitor and respond to the quality of process data coming in from treatment areas. Some medical studies solve these issues by having two separate teams, one that monitors data as it comes in and one that does the final analyses—indeed separation of teams is recommended in Food and Drug Administration guidelines²⁴ (see also discussion in Olken 2015). But experiments in development economics are run for a fraction the cost of most medical trials and simply do not have the resources for two independent, firewalled teams. Even if they did, separation foregoes the potential insights researchers gain in interpreting data when they are closely involved in collecting it.

Using administrative data for MP performance outcomes in our longer run experiment (lottery 1) caused similar timing and credibility challenges. Administrative data are usually collected continually. In our case, public records of MP attendance and speeches in parliament began accumulating as soon as the new parliament convened. While we avoided compiling and examining these data before completing our second PAP, we have no credible way to demonstrate this.

Our solution to the credibility challenge was to exhaustively document and time stamp every change we made to our original “untainted” PAPs that were lodged before we had access to the relevant data.²⁵ This created communication problems that undermined the guarantees of transparency. Pre-specifying analysis is only useful in reducing *p*-hacking and selective reporting if readers can check that the final analysis is based on the PAP. When the PAP reports a myriad of redlined edits, it places a burden on the reader to track and discern, at a substantial information disadvantage, the trivial from the consequential revisions. Even reading the adaptive PAP can become, in the words of one of our reviewers, “almost unbearable.” We therefore admitted defeat

²⁴ See “Guidance for Industry E9 Statistical Principles for Clinical Trials” Section IV.F.

²⁵ This first plan governing voter and candidate outcomes was lodged on 20 November 2012, which was 3 days after the Election and 8 days before data collection ended. With subsequent advances in digital data collection, wifi coverage, and cloud computing, lodging PAPs after data collection begins raises its own credibility concerns. This technology was not available in rural Sierra Leone at the time of this study, affording us no opportunity to take a “sneak peak” at the exit poll data. The second plan governing elected MP outcomes was lodged 2 June 2014 while data collection on MP performance was still in the field.

for an adaptive plan and reverted back to the original untainted pre-specifications.²⁶

Alternative mechanisms for keeping *p*-hacking in check in economics is high quality peer review, which stresses tests for alternative specifications and greater reliance on theory (Glennerster 2017). The conclusion of reviewers and editors was that the credibility of our results relied more on these than our adaptive PAP. We recognize that there is considerable divergence within the economics community on this point. Our view is that simple transparent PAPs can be helpful in specific situations. In our case, the PAP is useful in maximizing statistical power by pre-specifying one-sided tests where theory suggested a clear direction of effect; and providing a clear framework for adjusting for multiple inference.

8. Conclusion

These experiments suggest that voters acquire significant political knowledge from watching candidate debates, knowledge that persists over a number of weeks, and importantly, influences their vote choice on Election Day. By equipping voters with knowledge that changes their voting behavior, debate screenings further attracted greater campaign investment by participating candidates. This spending response is consistent with debate exposure making vote margins appear narrower or more uncertain *ex ante*, even in areas where it was revealed *ex post* that debates favored the more popular candidate. Debates convey comprehensive information about candidates—including charisma, professional qualifications, and policy stances—and the combination of factors appears more powerful than each in isolation. Over the longer run, participation in debates enhanced the accountability pressure on elected officials, increasing their subsequent engagement with constituents and expenditure on development projects. The finding that debates seem to strengthen accountability, even in relatively uncompetitive areas where direct electoral pressure is limited, is important.

From a policy perspective, this project demonstrates that interparty debates are logistically feasible to host and disseminate, and could be replicated on a larger scale: indeed, debates between MP

²⁶ Our results do not differ much if we use the adaptive PAP, presented in an earlier version of this paper that can be found with a description of what adaptations were made throughout the process, in our AEA trial registry. The move to drop the adaptations came in response to reviewers' concerns.

candidates have been held in at least four African countries since our initial experiment, explicitly citing the Sierra Leone case as an example. In considering the costs and benefits of scaling up, fixed video production costs for the debates themselves were modest in this setting: roughly five thousand dollars per constituency. The point estimate on increased development expenditure associated with debate participation is large enough to fully cover this cost. In terms of marginal dissemination costs, the mobile cinema in rural areas was a relatively resource intensive way to publicize the debates. Mobile cinemas in urban areas could reach substantial numbers at lower cost. In settings where mass media penetration is higher, dissemination via television or radio broadcast are obvious alternatives. While the individual treatments suggest that video is more effective than audio alone, the radio report we tested was rather dry summary of the facts and included no clips of the actual debate. A livelier program that captures a real time debate between candidates in the recording studio might come closer to the impacts of the film screening, and could reach large voting audiences at negligible marginal cost.

One could imagine multiple equilibria that might arise if debates were taken to scale. At the pessimistic end, politicians could learn to game the debates and unravel any benefit to voters. Candidates could, for example, coordinate on making only vague statements so that debates do not reveal their relative policy positions and the public record contains no concrete promises for voters to later follow up on. The novelty value of debates might also fade over time, making each subsequent debate less interesting to voters and less impactful for electoral and policy outcomes. More optimistically, the knowledge that debates provide information to voters could drive candidate effort and policy more in line with the interests of citizens. Incumbent awareness that debate videos exist and could be used to hold them to account could further motivate better performance in office. And, by making voting more responsive to candidate quality, debates could strengthen incentives for political parties to invest in recruiting more competent candidates. We leave these questions of effects at scale and persistence over repeated events to future research.

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Figure 1: Experimental Design

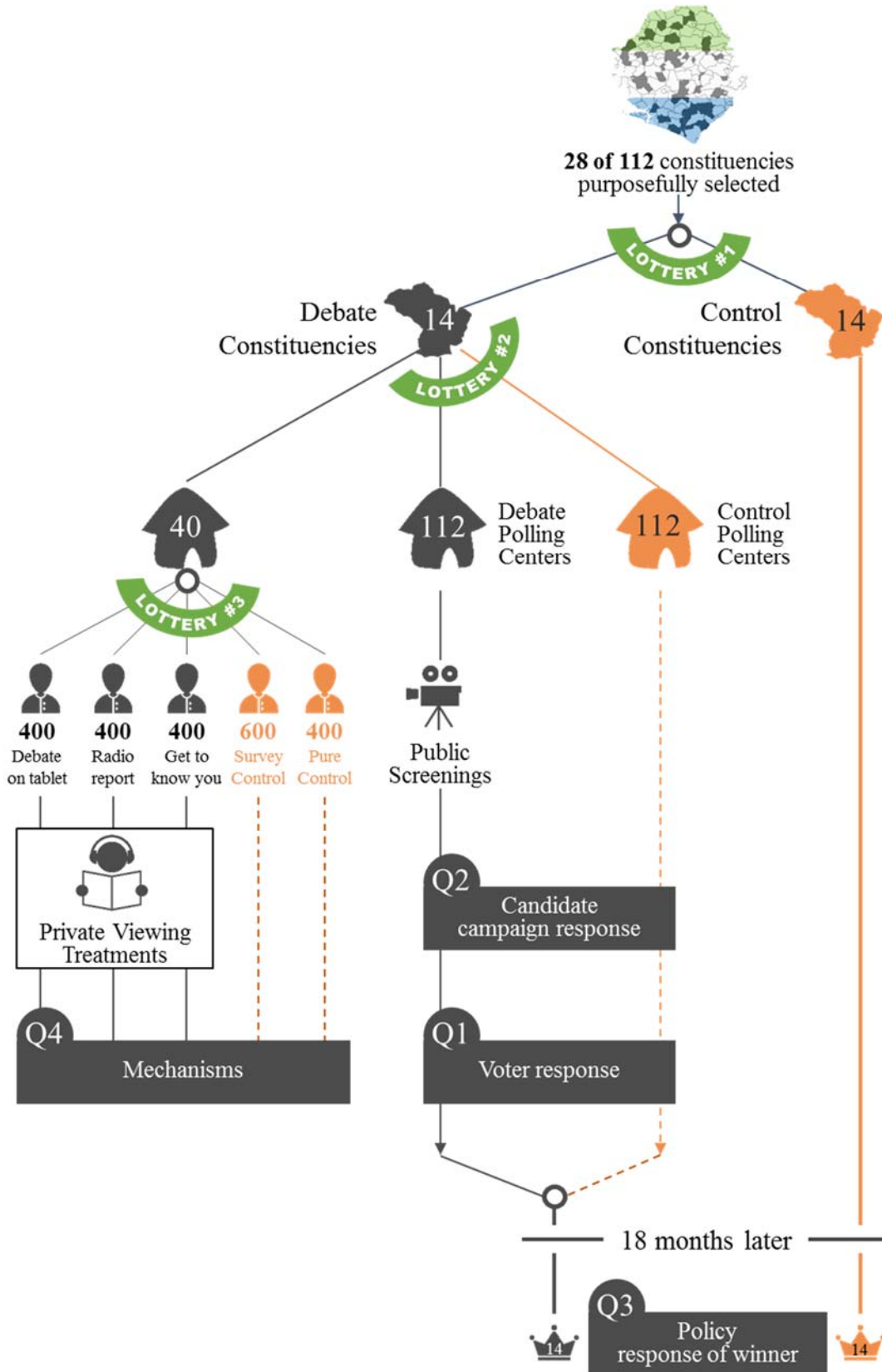
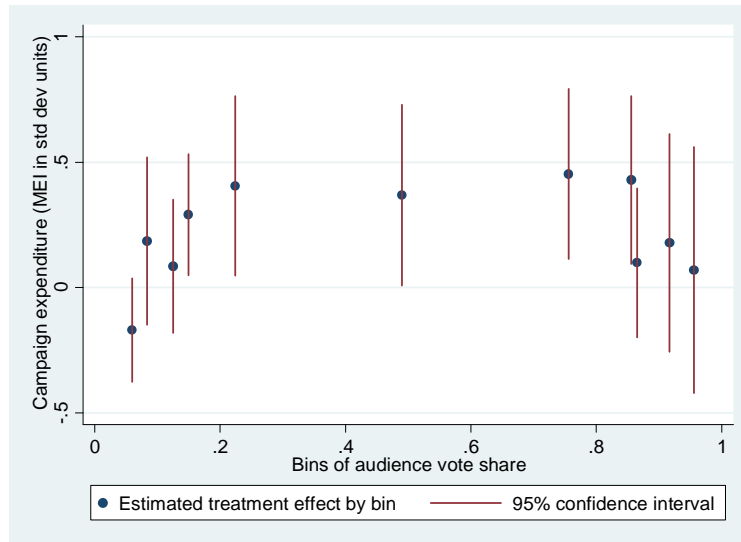


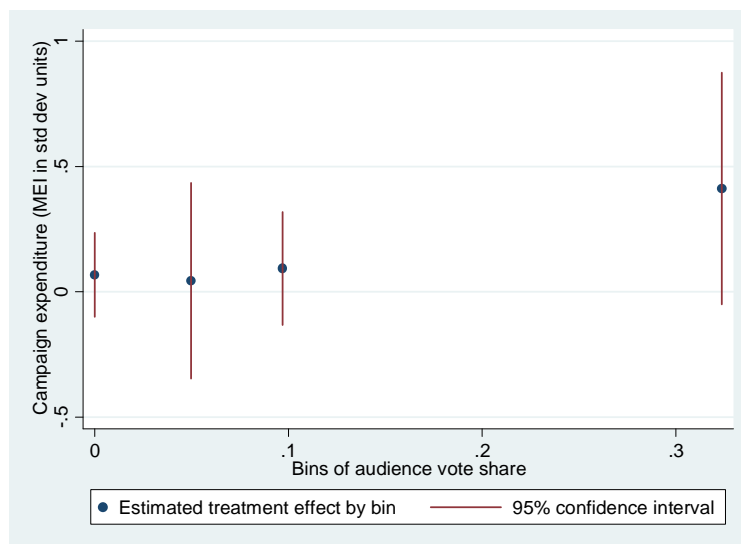
Figure 2: Heterogeneous Spending Response by Debate Performance

Panel A: Campaign spending by audience support, major party candidates



Notes: This figure explores whether candidate performance during the debate drives the intensity of their campaign spending response to the road show. The inverted U-shaped relationship between the expenditure response and audience assessment of who won the debate suggests that candidates responded most strongly when debate winner was closely contested. In this analysis: i) each dot represents the estimated coefficient on the interaction between treatment assignment and 11 equally sized bins of the share of audience members designating a given candidate as the debate winner; ii) the specification controls for the underlying ethnic-party loyalty of the constituency, randomization strata, and constituency fixed effects; iii) the reference bin on the left is the treatment effect estimated for the worst performing candidates, who received less than 8 percent of audience votes; and iv) the underlying unit of observation is the voter-candidate pair, for major parties only, $N=10,488$.

Panel B: Campaign spending by audience support, third party candidates



Notes: This figure applies the analysis in Panel A to third party candidates. The positive coefficient on the far right bin of audience support suggests that third party candidates responded to the road show most strongly when they had performed well during the debate. The unit of observation is the voter-candidate pair, for minor parties only, $N=3,299$.

Table 1: Voters - Summary of Public Debate Screening Impacts

Hypothesis	Treatment effect	Naïve p -value (1 sided)	FDR q -value (1 sided)	FWER p -value (1 sided)	Naïve p -value (2 sided)	FDR q -value (2 sided)
	(1)	(2)	(3)	(4)	(5)	(6)
H1 Political Knowledge (16 outcomes)	0.302*** (0.030)	<0.001	<0.001	<0.001	<0.001	0.001
H2 Policy Alignment (3 outcomes)	0.104*** (0.035)	0.002	0.006	0.012	0.003	0.010
H3 Persuasion (2 outcomes)	0.066** (0.037)	0.040	0.035	0.194	0.079	0.059
H4 Vote for Best (2 outcomes)	0.076** (0.044)	0.042	0.035	0.194	0.083	0.059
H5 Cross party lines (3 outcomes)	-0.022 (0.031)	0.757	0.338	0.943	0.484	0.108
H6 Openness (3 outcomes)	0.113** (0.049)	0.011	0.020	0.080	0.022	0.038
H7 Turnout (1 outcome)	-0.203 (0.114)	0.962	0.379	0.951	0.075	0.059
Observations	5,400					

Note: This table presents treatment effect estimates for the public debate screenings on voter behavior, summarized at the hypothesis level. In this analysis: i) significance levels indicated by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ based on our preferred specification of one-sided tests in the direction of the hypothesis statement in the pre-analysis plan (PAP) adjusted to control the false discovery rate (or the proportion of Type I errors) following Benjamini, Krieger and Yekutieli (2006) and Anderson (2008) (in column 3); ii) hypothesis-level mean effects indices are constructed following Kling, Liebman and Katz (2007) and expressed in standard deviation units, with missing values for component measures imputed at random assignment group means; iii) robust standard errors clustered by polling center; iv) all specifications include years of schooling and radio ownership, randomization stratification bins for the polling center (number of registered voters and distance to next nearest) and respondent (youth status and gender); v) adjustments to control the familywise error rate (or the probability of making any Type I error) computed following Westfall and Young (1993) and Anderson (2008); and vii) data source is the exit poll survey.

Table 2: Voters (Cont.) - Impacts of Public Screenings by Outcome

Hypotheses and outcomes	Control mean	Treat. effect	Std. error	p-value 1-sided	Signifi- cance	FDR q- value	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: H1 Political Knowledge							
Knows amount in constituency facilitation fund (CFF)	0.034	0.140	0.018	<0.001	***	<0.001	5,400
Knows who is entitled to free healthcare (FHC)	0.706	0.057	0.033	0.042	**	0.030	5,399
Knows the gender equity bill (GEB) is 30%	0.352	0.012	0.030	0.344		0.186	5,398
Knows MP job responsibilities (out of 3)	0.555	0.218	0.070	0.001	***	0.003	5,400
Knows APC candidate's name	0.442	0.183	0.034	<0.001	***	<0.001	5,058
Knows PMDC candidate's name	0.115	0.106	0.031	<0.001	***	<0.001	3,291
Knows SLPP candidate's name	0.395	0.169	0.031	<0.001	***	<0.001	5,400
Knows APC candidate's first priority issue	0.190	0.087	0.030	0.002	***	0.004	5,057
Knows PMDC candidate's first priority issue	0.099	0.064	0.026	0.008	***	0.010	3,288
Knows SLPP candidate's first priority issue	0.142	0.150	0.028	<0.001	***	<0.001	5,398
Knows APC candidate's view of FHC	0.252	0.197	0.035	<0.001	***	<0.001	4,579
Knows PMDC candidate's view of FHC	0.119	0.007	0.036	0.421		0.213	2,812
Knows SLPP candidate's view of FHC	0.123	0.072	0.029	0.007	***	0.009	4,921
Knows APC candidate's position on GEB	0.285	0.095	0.035	0.004	***	0.006	5,058
Knows PMDC candidate's position on GEB	0.244	0.209	0.052	<0.001	***	<0.001	3,291
Knows SLPP candidate's position on GEB	0.331	0.155	0.038	<0.001	***	<0.001	5,400
Panel B: H2 Policy Alignment							
Voter's view on FHC matches that of chosen candidate	0.394	0.092	0.035	0.004	***	0.007	4,714
Voter's view on GEB matches that of chosen candidate	0.613	-0.025	0.024	0.847		0.394	5,147
Voter's priority issue matches that of chosen candidate	0.425	0.090	0.031	0.002	***	0.005	5,147
Panel C: H4 Vote for Best							
Voted for debate winner, as judged by expert panel	0.712	0.011	0.022	0.312		0.175	5,212
Voted for debate winner, as judged by audience	0.803	0.044	0.022	0.022	**	0.018	5,212
Vote share of debate winner, expert panel (NEC returns)	0.617	0.035	0.016	0.027	**		206
Vote share of debate winner, audience (NEC returns)	0.711	0.031	0.017	0.046	**		206
Panel D: H5 Cross party lines							
Voted across ethnic-party lines	0.107	-0.011	0.013	0.804		0.394	4,562
Voted for a different party for MP in 2012 than in 2007	0.163	0.004	0.019	0.414		0.213	4,399
Voted for a different parties for different offices	0.058	-0.009	0.010	0.824		0.394	5,204
Panel E: H6 Openness							
Likeability rank for own party's candidate (10 point scale)	7.971	0.251	0.237	0.145		0.083	5,147
Likeability rank for third party candidate	2.369	0.586	0.271	0.016	**	0.015	3,291
Likeability rank for rival party's candidate	3.395	0.112	0.224	0.310		0.175	4,893
Panel F: H7 Turnout							
Voter turnout verified by voter registration card stamp	0.984	-0.026	0.015	0.959		0.446	5,331
Voter turnout, ballots/registered voters (NEC returns)	0.886	0.000	0.007	0.479			206

Note: This table presents treatment effect estimates for all individual outcome measures concerning voter response to public debate screenings. In this analysis: i) significance levels * p < 0.10, ** p < 0.05, *** p < 0.01 based on one-sided tests in the direction pre-specified in the PAP; ii) robust standard errors clustered by polling center; iii) specifications include years of education, radio ownership, randomization stratification bins, and constituency fixed effects; iv) false discovery rate (FDR) adjustments computed following Benjamini, Krieger and Yekutieli (2006) and Anderson (2008) across all 30 outcomes; v) data source is the exit poll survey or the National Electoral Commission (NEC) official polling center-level returns; and vii) the NEC returns exclude one constituency where the SLPP candidate was disqualified immediately before the election (see footnote 14).

Table 3: Candidates - Impacts of Public Screenings on Campaign Spending

	Control mean	Treatment effect	Percent change	Std. error	Naïve p- value (2 sided)	FDR q- value (2 sided)	<i>N</i>
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel A: Hypothesis-level campaign effect</i>							
Mean effects index (9 outcomes)	0.000	0.103		0.039	0.008***		5,400
<i>Panel B: Estimates for individual campaign outcomes</i>							
Percent received gift from the APC candidate	15.970	1.133	7%	2.736	0.679	0.652	5,056
Percent received gift from the SLPP candidate	8.895	0.719	8%	2.007	0.720	0.652	5,398
Percent received gift from the PMDC candidate	0.671	1.352	201%	0.609	0.026**	0.087*	3,220
Value of gift from the APC, ln(value+1)	0.412	0.122	25%	0.098	0.215	0.357	4,990
Value of gift from the SLPP, ln(value+1)	0.210	0.078	35%	0.063	0.219	0.357	5,348
Value of gift from the PMDC, ln(value+1)	0.014	0.034	246%	0.014	0.016**	0.087*	3,213
Number of visits by APC candidate	1.292	0.147	11%	0.137	0.283	0.396	5,057
Number of visits by SLPP candidate	1.273	0.070	5%	0.186	0.708	0.652	5,400
Number of visits by PMDC candidate	0.353	0.219	62%	0.093	0.019**	0.087*	3,291
<i>Panel C: Average outcome across parties</i>							
Percent received a gift	9.572	0.850	9%	1.662	0.609		5,400
Value of gift received, ln(value+1)	0.235	0.079	31%	0.051	0.123		5,397
Number of community visits	1.061	0.117	11%	0.117	0.316		5,400

Note: This table estimates the campaign response by MP candidates who participated in a debate to the allocation of group screenings across polling centers. In this analysis: i) significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ based on two-sided tests; ii) the mean effects index in row 1 is constructed following Kling, Liebman and Katz (2007) and expressed in standard deviation units, with missing values for component measures imputed at random assignment group means; iii) the individual outcomes in all other rows are expressed in units natural to the measure; iv) robust standard errors clustered by polling center; v) specifications include years of education, radio ownership, randomization stratification bins and constituency fixed effects; vi) adjustments to control the false discovery rate (FDR) computed following Benjamini, Krieger and Yekutieli (2006) and Anderson (2008); vii) value of gift is expressed as natural log of value x (in thousand Leones) plus 1 and percent in column 3 refer to change in underlying value x ; viii) data source is the exit poll survey; and ix) estimates in panel C were not pre-specified.

Table 4: Winning MPs - Impacts of Debate Participation on Policy

	Control mean	Treatment effect	Std. error max of (OLS, HC ₂)	Naïve p - value (1 sided)	N
	(1)	(2)	(3)	(4)	(5)
Panel A: Hypothesis-level policy effect					
Mean effects index (9 outcomes)	0.000	0.298	0.159	0.037**	28
Panel B: Estimates for individual policy outcomes					
Development spending verified in the field (% 2012 CFF)	35.56	54.74	31.71	0.049**	27
Total number of constituency visits	2.915	1.316	0.619	0.022**	28
Total number of public meetings held with constituents	1.018	1.089	0.606	0.043**	28
Percent of 2012-13 sittings attended (out of 57 total)	76.69	3.371	3.003	0.137	28
Total public comments in Parliamentary sittings 2012-13	4.286	-1.569	2.224	0.244	28
Committee membership (total number)	3.929	0.524	0.625	0.206	28
Total public comments in priority sector agenda items	0.154	-0.170	0.166	0.862	27
Membership in priority sector committee	0.231	0.201	0.187	0.147	27
Constituent assessment of focus on priority sector	0.571	-0.343	0.150	0.984	27

Note: This table leverages the constituency-level randomization to estimate the effects of participating in a debate as a candidate on the subsequent performance of the elected MP in office. In this analysis: i) significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ based on one-sided tests in the direction prespecified in the PAP; ii) hypothesis-level mean effects indices are constructed following Kling, Liebman and Katz (2007) and expressed in standard deviation units, with missing values for component measures imputed at random assignment group means; iii) estimates for individual outcomes are expressed in units natural to the measure; iv) the standard error presented is the maximum value of conventional OLS and bias corrected HC₂ estimators in MacKinnon and White (1985), following discussion in Angrist and Pischke (2009); v) specifications include gender, previous elected office experience and stratification bins for the constituency (3 bins of ethnic-party bias); and vi) missing values for priority sector outcomes are from one control MP who did not provide a pre-election priority and for development spending are from one treated MP who did not take office until December 2013 (one year after the election) and thus did not receive the 2012 CFF.

Table 5: Mechanisms Explored

	H1 Political Knowledge		H2 Policy Alignment		H4 Vote for Best	
	Treatment effect (Std. error)	Naïve <i>p</i> -value	Treatment effect (Std. error)	Naïve <i>p</i> -value	Treatment effect (Std. error)	Naïve <i>p</i> -value
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Private Viewing Experiments</i>						
Debate (vs controls)	0.135*** (0.024)	<0.001	0.080*** (0.029)	0.005	0.058* (0.040)	0.077
Get to Know You (vs controls)	0.042*** (0.016)	0.006	0.007 (0.026)	0.396	0.004 (0.037)	0.454
Radio (vs controls)	0.111*** (0.021)	<0.001	-0.041 (0.025)	0.947	-0.046 (0.043)	0.851
Difference: Debate vs GTKY	0.093*** (0.027)	<0.001	0.073** (0.033)	0.027	0.054 (0.045)	0.231
Difference: Debate vs Radio	0.025 (0.021)	0.234	0.120*** (0.032)	<0.001	0.104** (0.052)	0.046
Observations	1,698					
<i>Panel B: Private Viewing versus Public Screening</i>						
Private Viewing	0.172*** (0.027)	<0.001	0.086** (0.039)	0.017	0.105** (0.052)	0.026
Observations	748					
Public Screening	0.352*** (0.038)	<0.001	0.129*** (0.047)	0.004	0.100* (0.068)	0.072
Observations	3,507					

Notes: i) Panel A considers the suite of treatment arms randomized at the individual-level, presenting results for absolute treatment effects compared to control group data (in rows 1 to 3) as well as relative effects of the three treatments compared to one another (in rows 4 to 5); ii) Panel B estimates voter response to debates when delivered privately and by public group screenings (last rows), for comparability the group screening sample is limited to the 8 constituencies where the individual-level treatments were also administered, and the individual level estimates are limited to the debates treatment arm versus the "pure" control groups; iii) significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ based on per comparison one-sided tests in the direction prespecified in the PAP (except for the difference rows which are 2 sided); iv) hypothesis-level mean effects indices are constructed following Kling, Liebman and Katz (2007) and expressed in standard deviation units, with missing values for component measures imputed at random assignment group means; v) robust standard errors clustered by polling center; vi) specifications include respondent education and radio ownership, stratification bins for the randomization procedure and constituency fixed effects.

SUPPLEMENTAL ONLINE MATERIALS

“Debates: Voting and Expenditure Responses to Political Communication”

By Bidwell, Casey and Glennerster

Contents

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Appendix A: Balance Table for Lotteries 1 and 2

	Control mean (1)	Treatment mean (2)	Without strata diff. (3)	Without strata std. err (4)	With strata diff. (5)	With strata std. err (6)	<i>N</i> (7)
Panel A: Voters							
Age	40.31	40.00	-0.31	0.47	-0.30	0.45	5,413
Farmer	0.83	0.81	-0.02	0.03	-0.02	0.02	5,260
Female	0.52	0.52	0.00	0.01	0.00	0.01	5,414
Does not speak Krio	0.21	0.19	-0.01	0.03	-0.01	0.02	5,414
Married polygamously	0.27	0.28	0.01	0.03	0.01	0.02	5,414
Household owns a radio	0.69	0.69	-0.01	0.03	-0.01	0.02	5,405
Years of schooling	1.81	1.95	0.14	0.19	0.14	0.13	5,409
Voted for the APC MP candidate in 2007	0.60	0.60	0.00	0.05	0.00	0.02	4,520
Member of ethnic group historically loyal to the APC	0.62	0.60	-0.02	0.06	-0.03*	0.02	4,740
Member of ethnic group historically unaffiliated	0.13	0.12	-0.01	0.03	-0.01	0.02	5,412
Target respondent replaced with alternate respondent	0.10	0.09	-0.01	0.01	-0.01	0.01	5415
Panel B: Candidates							
Age	48.36	45.22	-3.13	2.38	1.39	3.84	64
Female	0.06	0.14	0.07	0.07	0.14	0.14	67
Sitting incumbent MP	0.23	0.14	-0.09	0.08	-0.07	0.17	67
In last job, managed ten or more employees	0.38	0.37	-0.01	0.12	0.07	0.20	64
Any elected office experience	0.42	0.19	-0.22**	0.09	-0.21	0.18	67
Quiz score naming line ministry counterparts (of 3)	1.03	0.78	-0.26	0.22	-0.31	0.39	66
Member of ethnic group historically unaffiliated	0.17	0.14	-0.03	0.12	-0.15	0.13	66
Years of schooling	14.76	14.47	-0.29	0.55	-0.65	0.83	65
Panel C: Winning MPs							
Age	46.00	47.50	1.50	3.80	1.39	3.82	26
Female	0.07	0.21	0.14	0.13	0.14	0.14	28
Sitting incumbent MP who won re-election	0.29	0.21	-0.07	0.17	-0.07	0.17	28
In last job, managed ten or more employees	0.58	0.64	0.06	0.20	0.07	0.20	26
Any elected office experience	0.50	0.29	-0.21	0.19	-0.21	0.18	28
Quiz score naming line ministry counterparts (of 3)	1.15	0.86	-0.30	0.39	-0.31	0.39	27
Member of ethnic group historically unaffiliated	0.23	0.07	-0.16	0.14	-0.15	0.13	27
Years of schooling	16.00	15.36	-0.64	0.77	-0.65	0.80	26
2012 winning margin (1st vs 2nd place finisher)	0.49	0.46	-0.03	0.07	-0.03	0.06	28
Panel C: Constituencies							
Distance from constituency centroid to Freetown (km)	148.21	150.69	2.48	37.90	2.48	31.92	28
Distance from centroid to district headquarters (km)	26.82	28.28	1.46	6.56	1.46	6.54	28
Distance from centroid to nearest major road (km)	7.13	8.81	1.68	3.52	1.68	3.36	28
Total kilometers of major roads in the constituency	36.69	28.68	-8.02	10.35	-8.02	10.62	28
Expected ethnic-party bias, absolute value, range: [0,1]	0.54	0.47	-0.07	0.08	-0.07*	0.04	28
Total registered voters	24,848	23,072	-1,777	3,520	-1,777	3,482	28
Seat changed parties in previous (2007) election	0.29	0.21	-0.07	0.17	-0.07	0.17	28
Sitting MP incumbent is a candidate in the race	0.43	0.36	-0.07	0.19	-0.07	0.19	28
2007 winning margin (1st vs 2nd place finisher)	0.28	0.26	-0.02	0.06	-0.02	0.06	28
Population share of unaffiliated ethnic groups	0.12	0.13	0.00	0.05	0.00	0.05	28

Note: This table compares average characteristics of observations assigned to treatment and control groups. Panel A concerns voters as a validation of the polling center randomization (lottery 2), while panels B-D validate the constituency randomization (lottery 1). In this analysis: i) significance levels indicated by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ based on two-sided tests; ii) robust standard errors clustered by polling center in panel A and by constituency in panel B, conventional OLS standard errors in panels C and D; and iii) estimates in columns 3 and 4 exclude the randomization strata and estimates in columns 5 and 6 include the strata.

Appendix B: Balance Table for Lottery 3

	Debates	Get to know	Radio	Lab	Pure	Surveyed
		you video	report	controls	controls	controls
	mean	mean	mean	mean	mean	mean
	(std. error)	(std. error)	(std. error)	(std. error)	(std. error)	(std. error)
	(1)	(2)	(3)	(4)	(5)	(6)
Age	42.62 (0.85)	41.35 (0.83)	41.24 (0.89)	41.82 (0.83)	42.33 (0.86)	42.62 (0.71)
Farmer	0.72 (0.02)	0.76 (0.02)	0.77 (0.02)	0.75 (0.02)	0.76 (0.02)	0.78 (0.02)
Female	0.52 (0.03)	0.54 (0.03)	0.54 (0.03)	0.54 (0.03)	0.55 (0.03)	0.54 (0.02)
Does not speak Krio	0.17 (0.02)	0.18 (0.02)	0.16 (0.02)	0.21 (0.02)	0.19 (0.02)	0.19 (0.02)
Married polygamously	0.40 (0.03)	0.31 (0.02)	0.34 (0.02)	0.34 (0.02)	0.30 (0.02)	0.37 (0.02)
Household owns a radio	0.65 (0.02)	0.59 (0.03)	0.62 (0.03)	0.62 (0.02)	0.59 (0.03)	0.62 (0.02)
Years of schooling	2.54 (0.21)	2.16 (0.20)	2.17 (0.20)	2.03 (0.19)	1.89 (0.17)	2.20 (0.16)
Voted for the APC MP candidate in 2007	0.64 (0.03)	0.71 (0.03)	0.65 (0.03)	0.69 (0.03)	0.68 (0.03)	0.68 (0.02)
From ethnic group historically loyal to APC	0.71 (0.02)	0.72 (0.02)	0.72 (0.02)	0.75 (0.02)	0.72 (0.02)	0.73 (0.02)
From ethnic group historically unaffiliated	0.10 (0.02)	0.07 (0.01)	0.09 (0.01)	0.08 (0.01)	0.07 (0.01)	0.08 (0.01)
Respondent attrition in exit poll	0.07 (0.01)	0.08 (0.01)	0.06 (0.01)	0.04 (0.01)	0.06 (0.01)	0.07 (0.01)
Observations	399	402	392	407	399	601

Note: This table presents the mean and standard error of respondent characteristics in the six different treatment arms administered under the individual private viewing experiments (lottery 3). In this analysis, no randomization strata are included.

Appendix C: Pre-analysis plan for voters and candidates

SIERRA LEONE 2012 ELECTIONS PROJECT

PRE-ANALYSIS PLAN: POLLING CENTER LEVEL INTERVENTIONS

PIs: Kelly Bidwell (IPA), Katherine Casey (Stanford GSB) and Rachel Glennerster (JPAL MIT)

20 November 2012

This study examines the impact of providing citizens with information about Parliamentary candidates via structured inter-party debates in the lead up to the Sierra Leone November 2012 Elections. Randomization and treatments were conducted on multiple levels: constituency, polling center and individual (details on sampling and randomization are available in the project's "Sampling Procedures" document). This pre-analysis plan governs the analysis of the polling-center level treatment only. It was written and registered with the Abdul Latif Jameel Poverty Action Lab before fieldwork for the exit poll, which is the primary source of data for this analysis, was completed (where the current estimated completion date is 22 November 2012). This document is the first installment in a planned sequence of registry and data analysis, where we will next: (i) register separate plans for the individual-level and constituency-level treatments; (ii) analyze treatment effects for the individual-level treatments; (iii) examine the distribution of outcomes for the control group polling centers in the exit poll data; (iv) analyze the expert panel scoring of debates and the before/after debate surveys; (v) register an update to this document reflecting learning from steps 2 to 4; and then (vi) analyze treatment effects at the polling-center level in the exit poll and voting returns data

1. Background

Our NGO partner, Search for Common Ground, hosted and filmed debates between Parliamentary candidates in 14 constituencies. We randomly selected these constituencies from what we estimated would be the 28 most competitive constituencies, stratifying on the strength of the ethnic bias favoring one party over the other. Within constituencies, polling centers that were sufficiently small (fewer than ~900 registered voters) and far apart from their nearest neighbor (at least ~ one mile) were randomly assigned to treatment and control groups.

Treatment at the polling center level consisted of an evening showing of a video tape of the relevant debate projected at a convenient public place, usually the polling center itself, in the weeks leading up to the Election. Typical protocol for these screenings was as follows: host polling center and satellite communities were notified in advance and invited to attend the screening; 25 randomly selected residents (using data from an earlier listing exercise) were provided a small incentive (10 Maggi spice cubes for cooking) to attend the screenings; the video was played once in a pause and play format that inserted translation into the relevant local language after each question; the video was played a second time with or without translation; and a secondary screening was held in the largest accessible satellite village earlier in the day, in most cases without translation.

We hypothesize that this video screening intervention may have treatment effects on three different sets of actors: voters, candidates and centralized parties, which we will treat as distinct domains. For each set, we lay out a series of hypotheses regarding the likely areas of impact with corresponding outcome measures below. We will provide treatment effects with unadjusted (or per comparison) p-values for all outcomes specified in this document for all domains. We will also compute mean effects indices by hypothesis and

correct for multiple inference across outcomes within a hypothesis. Since we have multiple hypotheses regarding voters, we will further make adjustment at the hypothesis-level in domain A (as specified below) but will not make adjustments across domains.

2. Domain A: Effects of PC-level Debates on Voters

This domain explores the effects of polling center debate screenings on voter knowledge, behavior and vote choice.

A. Econometric specifications

Analysis of treatment effects will take the form of:

$$Y_{ipc} = \beta_0 + \delta T_{pc} + \mathbf{X}'_{ipc} \boldsymbol{\Pi} + \mathbf{Z}'_{pc} \boldsymbol{\Gamma} + \mathbf{W}'_{ipc} \boldsymbol{\Psi} + \mathbf{c}_p + \varepsilon_{ipc} \quad (1)$$

where outcome Y (i.e. vote choice) is measured for individual i registered in polling center p within Parliamentary constituency c ; T is an indicator variable equal to one if the polling center received the debate video screening treatment; \mathbf{X} is a vector of indicator variables that denote the stratification bin from which exit poll respondents were drawn (where the bins were constructed by age and gender); \mathbf{Z} is a vector of indicator variables that denote the stratification bin from which the polling center was drawn (where the bins were constructed by number of registered voters and distance to nearest neighboring center); \mathbf{W} is a set of additional control variables that will be determined from analysis of the control group data and will vary by hypothesis with an eye toward identifying individual characteristics that do not vary with treatment and that help explain variation in a particular outcome (i.e. education and radio ownership are likely positively correlated with general political knowledge)ⁱ; \mathbf{c} is a set of constituency-specific fixed effects (the level of debate and candidates); and ε is an idiosyncratic error term clustered at the polling center level. Our main specification includes the full set of controls (\mathbf{X} , \mathbf{Z} and \mathbf{W}); we will also show results for the sparser specification that includes only the stratification variables as controls (\mathbf{X} and \mathbf{Z} only) as a robustness check. The coefficient of interest is δ , the average treatment effect. Unless otherwise stated, all tests will be one-sided in the direction indicated below. The primary source of data is the individual-level exit polls.

Additional analysis will use polling-center level voting returns data from the National Electoral Commission (NEC), taking the form:

$$V_{pc} = \beta_0 + \delta T_{pc} + \mathbf{Z}'_{pc} \boldsymbol{\Gamma} + \mathbf{U}'_{pc} \boldsymbol{\Psi} + \mathbf{c}_p + \varepsilon_{pc} \quad (2)$$

where V is the outcome (vote share, turnout rate) measured for the polling center p within Parliamentary constituency c ; \mathbf{U} is a vector of polling center control variables to be determined from analysis of the control group community survey exit poll data; and other terms remain as above. We will run two specifications: (i) the main specification will include the additional “pure” control polling centers that were not treated nor surveyed in the exit poll and will omit any elements of \mathbf{U} that are not available for these centersⁱⁱ; and (ii) a robustness check specification that omits the “pure” control centers and includes the full set of polling center characteristics in \mathbf{U} .

We will test for heterogeneous treatment effects at the level of constituency, candidate and voter, adjusting for multiple inference within each level (i.e. grouping together the tests for all of the voter-level sub-groups

when adjusting standard errors). Specifically we will test for differential effects along the following dimensions:

- **Candidate divergence and competitiveness (primary):** the impact of debates should be increasing in the revealed divergence in policy positions and competence of the participating candidates as measured by expert panel and audience rankings from the before/after debate surveys and the interaction of divergence with voting behavior in control polling centers (i.e. if vote shares for the debate winner are already very high in control areas, there is little scope to increase them even if performance in the debate was lopsided). This will involve testing for heterogeneous effects across constituencies (i.e. how the constituency-level ATE varies along key dimensions like ethnic bias) and across two bins of constituencies (i.e. those where one would expect larger versus smaller effects).
- **Lesser known candidates (secondary):** voter response to strong (weak) performance by less well known candidates (including PMDC, female and non-incumbents) may be stronger than that for other better known candidates, as voters may have greater scope for updating their beliefs
- **Subgroup analysis (primary):** the voting literature suggests that the impact of debates could vary by gender, age, and level of political informedness / naïvete. These tests will be two-sided. We further predict weaker effects for people who do not speak Krio well and may have had trouble understanding the debate.

We further plan to conduct descriptive analysis in the following areas:

- **Spillovers:** establish whether controls saw / heard about the debates in the exit polls; test whether the impact of debates is positive and decreasing in distance from nearest treated polling center in voting returns data
- **Dissipation of effects:** test whether the impact of debates weakens as the time between the debate screening and Election Day increases
- **Treatment saturation:** test whether the treatment effect is increasing in treatment saturation at the level of polling center; verify that the TOT effect is greater than ITT at the individual level (if some residents of treated polling centers did not attend the screening)
- **Reaction to polling center results:** test whether responses to the exit poll survey systematically vary between those who were surveyed before versus after preliminary results were posted on polling centers
- **Impacts on competitiveness:** calibrate the expected impact of debates on the competitiveness of races if taken to scale, based on estimated TEs on vote shares. Also use voter ratings of Presidential candidates to link and order ratings of MP candidates across constituencies to estimate the impact of sending the best candidate of a given party to other constituencies

B. Hypotheses and Outcomes

In what follows we organize hypotheses and outcomes into three families: (i) “Vote choice” concerns changes in actual votes cast, which is the ultimate objective yet will be difficult to influence if stronghold candidates that already have significant advantages perform better in the debates; (ii) “Voting knowledge and behavior” reflects the informedness of voting choices and political participation, which are important in their own right and may serve as a necessary but not sufficient step between the status quo and attaining

the ultimate objective of changing votes cast; and (iii) “Secondary outcomes” regarding citizen perceptions of politics that are interesting yet less directly linked to the debate experienceⁱⁱⁱ. Multiple inference corrections will be implemented across outcomes within hypothesis and across hypotheses within family.

- **Vote choice outcomes**

- a. Hypothesis 1: Exposure to debates increases (reduces) **vote shares** for the candidate that performed the best (worst) in the debates^{iv}
 - i. TE measured by vote choice in exit poll data (primary test) and in electoral returns (lower power, secondary test)
 - ii. Debate winner / loser measured by audience ratings and expert assessment
- b. Hypothesis 2: Exposure to debates increases the willingness to **vote across party lines**
 - i. TE measured by vote choice and ethnicity in exit polls (primary test, limited to members of affiliated tribes); reduced forecasting power of ethnic census shares on electoral returns (lower power, secondary test)
 - ii. Two additional (primary) measures expand the concept to incorporate non-affiliated tribes: voting for a different party for MP in 2012 than in 2007; and splitting ticket for MP (i.e. party MP different than party Pres or party LC)

- **Voting knowledge and behavior outcomes**

- a. Hypothesis 3: Exposure to debates increases **political knowledge** and leads to more informed voting
 - i. TE measured for general political knowledge as mean index on ability to name MP roles, CFF amount, healthcare entitlement, gender equity percentage
 - ii. TE measured for individual candidate attributes as ability to name candidates (primary), and mean index on distinguish better educated, public office experience, incumbency and more likely to report personal characteristic as primary determinant of voting choice (secondary)
 - iii. TE measured for candidate policy stances by ability to correctly place candidate view on Gender equity, first priority issue, free health care implementation
- b. Hypothesis 4: Exposure to debates mobilizes the public and leads to greater **turnout**
 - i. TE measured by turnout question in exit polls (primary) and electoral returns (lower power, secondary)
- c. Hypothesis 5: Exposure to debates increases **policy alignment**
 - i. TE measured by match between voters position expressed in exit poll question and reported stance of their selected candidate expressed in the debate on gender equity, priority issues, CFF disclosure^v, and/or free health care implementation. Note that alignment measures will be tailored by constituency to reflect the actual divergence (avoiding a lack thereof) amongst candidates
- d. Hypothesis 6: Exposure to debates **persuades** voters to adapt their preferred candidate’s policy stances
 - i. TE measured by voter opinion on free healthcare implementation and position on Gender Equality Bill that matches their candidates stance presented in the debate. These measures will be tailored by constituency to reflect candidate positions expressed in the debate, but in most cases we expect APC-(SLPP-)leaning voters

to express a more positive (negative) view of FHC implementation compared to their counterparts in control areas, and for treated voters to report greater support of the GEB.

- e. Hypothesis 7: Exposure to debates enhances **voter openness** to other parties
 - i. TE measured by higher likeability ratings for all candidates (i.e. own party, rival party, and third party where applicable) in exit polls

- **Secondary outcomes**

- a. Hypothesis 8: Exposure to debates increases the perceived **legitimacy** of elections
 - i. TE measured by increasing confidence that elections are free and fair in exit polls, decreased violence at polling centers (exit poll and NEC incident reports)
- b. Hypothesis 9: Exposure to debates increases **interest in politics**
 - i. TE measured by question on frequency of discussing politics in exit poll
- c. Hypothesis 10: Exposure to debates **does not increase electoral misconduct**
 - i. Lack of TE documented by questions regarding police presence, inappropriate influence, election officials wearing party colors and election officials verbally encouraging specific vote choices

3. Domains B and C: Effects of PC-level Debates on Candidate and Party Campaigning

These two domains capture potential effects of the polling center-level screenings on the campaign strategies of candidates and political parties. As candidates and party officials are two different sets of actors we treat them as different domains but combine the exposition of the approach here as it is the same for both.

A. *Econometric specification*

Analysis of treatment effects will take the form of:

$$Y_{impc} = \beta_0 + \delta T_{pc} + \mathbf{X}'_{ipc} \boldsymbol{\Pi} + \mathbf{Z}'_{pc} \boldsymbol{\Gamma} + \mathbf{W}'_{ipc} \boldsymbol{\Psi} + \mathbf{c}_p + \varepsilon_{ipc} \quad (3)$$

where outcome Y (i.e. receiving a gift) is measured for individual i in relation to candidate m where the individual is registered in polling center p within Parliamentary constituency c ; T is an indicator variable equal to one if the polling center received the debate video screening treatment; \mathbf{X} is a vector of indicator variables that denote the stratification bin from which exit poll respondents were drawn (where the bins were constructed by age and gender); \mathbf{Z} is a vector of indicator variables that denote the stratification bin from which the polling center was drawn (where the bins were constructed by number of registered voters and distance to nearest neighboring center); \mathbf{W} is a set of additional control variables that will be determined from analysis of the control group data with an eye toward identifying individual characteristics of political gift receipt; \mathbf{c} is a set of constituency-specific fixed effects (the level of debate and candidates); and ε is an idiosyncratic error term clustered at the polling center level. Data concerning candidate expenditure will come from the individual-level exit polls; while exit poll data (and analysis) for party support will come from (and be conducted at) the community-level. Hypotheses here are two tailed, as candidates and parties could plausibly treat campaign effort/expenditure as a substitute for the screening publicity, or they could

compensate for the greater competitiveness of the race by allocating more effort/resources to treatment areas.

We will further test for heterogeneous effects along three dimensions:

- **Debate performance:** As the response of candidates and parties could vary by how well their candidate performed in the debate, we will test for heterogeneous effects by relative debate performance and degree of performance divergence as measured by expert panel and audience rankings in the before/after debate survey.
- **Party:** budget and strategy may vary by party, so we will test for differential response from the incumbent (APC), opposition (SLPP) and third party (PMDC).
- **Competitiveness:** size of response is likely decreasing in the expected vote margin.

B. Hypotheses and Outcomes

In what follows we organize hypotheses and outcomes by domain (candidate versus party).

- **MP Candidate outcomes**
 - a. Hypothesis 1: Candidate allocation of **campaign effort and expenditure** is responsive to debate publicity
 - i. TE measured by receipt of any campaign gift, type and value of the gift, number of candidate visits in the 6 weeks leading up to the Election
- **Party outcomes**
 - a. Hypothesis 1: Party allocation of **campaign support** is responsive to debate publicity
 - i. TE measured by allocation of number of visits by party officials and party candidates for all races, number of political rallies, number of posters and number of gifts distributed in the community in the 6 weeks leading up to the Election¹

ⁱ We use these endnotes to clarify and flag minor deviations from what is pre-specified here and estimates in the main text of the paper. Regarding Equation (1), since we did not specify the exact algorithm we would use to select controls, we simply use the two written here—years of schooling and radio ownership—in all specifications.

ⁱⁱ These tests on the NEC data are part of the secondary tests that appear in Table 2. Here we commit to the specification that uses the additional 29 “pure” control polling centers. These, however, were randomized out of our study sample before we defined the stratification bins we use throughout our analysis, which was a mistake, so we must alter the main specification somewhat to include these extras. Treatment effect estimates remain similar with their inclusion.

ⁱⁱⁱ We did not find many interesting results for secondary outcomes so report only primary outcomes.

^{iv} In response to referee comments, we modify this measure to be based on the votes of audience members in all other polling centers outside the particular center of interest.

^v As there was little divergence in candidate responses to this question—all save one said they would support the CFF disclosure bill—we removed it from the exit poll survey and did not collect data on alignment for this policy. Not excluding it in the PAP was an oversight.

¹ Note regarding interpretation: there is some overlap between the information collected for the party and the MP above. The questions on the community survey for party cover gifts from party officials and candidates for any office, where the offices are President, MP, Local Councillor and Council Chair.

Appendix D: Pre-analysis plan for elected MPs

SIERRA LEONE 2012 ELECTIONS PROJECT

PRE-ANALYSIS PLAN: POLLING CENTERⁱ LEVEL INTERVENTIONS

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This study examines the impact of providing citizens with information about Parliamentary candidates via structured inter-party debates in the lead up to the Sierra Leone November 2012 Elections. Earlier PAPs govern the analysis of treatment effects on voter behavior and candidate/party response up through Election Day. This pre-analysis plan sets out the main parameters of analysis for post-Election effects of debates on the behavior of election winners who are now serving in Parliament, over the course of their first year to 18 months in office. The research design corresponds to the highest level of randomization for this project, where 14 of what we expected to be the 28 most closely contested constituencies were randomly assigned to participation in debates and the remaining 14 were assigned to the control group. This plan was lodged in the AEA trial registry on June 2, 2014, which is before any data analysis began, and before much of the data collection was completed (where the current anticipated completion of fieldwork for the suite of CFF surveys is June 7th, 2014, and data entry from these paper surveys will follow).

A key part of this analysis is an attempt to substantiate on the ground the development expenditures the MPs claimed to have made under their first annual constituency facilitation fund (CFF) and post-election engagement with constituents. The fieldwork plan for this involves triangulation of responses across a number of different respondents and surveys, each of which may be more or less informed and credible, which will complicate data analysis. Our plan moving forward is thus to: i) lodge this initial PAP before field work ends to lock in our main domains of hypothesized effects; ii) enter and clean all survey data; iii) analyze data from the control sample only to generate a more specific list of indicators across surveys and analysis procedures; iv) data enter and code official Parliamentary records by topic; v) lodge an update to this plan with more specific indicators and strategies of triangulation across sources; and v) conduct analysis of treatment effects. The advance analysis of the control sample (item iii above) aims to accomplish a few objectives. First, we will assess variation in outcome measures and baseline levels of activity, to refine the set of outcomes sensible for analysis. Second, we will test our operating assumptions that residents in the constituency headquarter towns (respondents in the main town CFF surveys) have good information about MP activity throughout the constituency and are relatively similar in their views of MP performance compared to those in smaller villages. To do so, we will use the target village responses as a cross check. If we find that either of these assumptions do not hold empirically, we will devise a strategy for how best to make use of the information collected in the (non-random) target village sample. Third, we will assess how well we can determine the accuracy of responses from different sources. We anticipate that the judgments by our enumerators on the relative truthfulness across respondents will be sufficient here, but if we find that the enumerators were unable to make decisive rankings and that there is considerable differences in view across respondents, then we will develop a strategy for balancing and reconciling conflicting reports.

Key caveat: It is important to note that power at this level is limited due to the small sample, so we will be particularly cautious in interpreting a lack of evidence of treatment effect as suggestive of evidence of no effect in practice.

1. Background

All candidates in the 28 constituencies were surveyed in the pre-Election period. After that, the control group in the 14 control constituencies were otherwise not contacted by the research team. Candidates in the treatment group were invited to participate in a structured inter-party debate that was moderated and filmed by our NGO partner, Search for Common Ground. The debates were then taken on a polling-center level “road show” in a randomly selected 112 of 224 polling centers plus an additional 85 screenings in satellite villages. We estimate that roughly 19,000 individuals were exposed to this treatment. Additional individual-level screenings were held in a separate set of 40 polling centers. Early in their tenure, winners in the treatment group were shown a video of the debate they participated in, edited down to include only their own statements, to remind them of the commitments they made during the debate and explain how many of their constituents saw the debate via the road show. The research hypothesis is that the publicity of the debates screenings could help solve the candidate commitment problem and thereby enhance the consistency of elected MP behavior with their pre-election promises and generally enhance accountability pressure toward better performance in office.

Second caveat: There are two key ways in which the roadshow treatment is considerably less intense than other obvious ways to disseminate the debates to voters. First, the road show was shown to a relatively small subset of constituents: a back of the envelope calculation would put this figure at around 3%. Broader dissemination, e.g. via radio, would reach many more. Second, the MPs themselves were not present at these screenings, so if the winners did not understand or internalize the number of constituents exposed, it is unlikely to affect their future behavior.

Data for this segment of the analysis draws on multiple sources: i) the official Votes and Proceedings produced by Parliament administration (V&Ps); ii) the official Hansards produced by Parliament administration; iii) committee assignments and minutes of committee meetings, produced by committee clerks; iv) MP candidate pre-election survey; v) winning MP post-election follow-up survey (supplemented with post-survey follow-up phone calls to clarify CFF project locations and expenditures); vi) CFF main community questionnaire; vii) CFF clinic follow-up questionnaire; viii) CFF verification sheet; ix) CFF school follow-up questionnaire; and x) CFF Target village community questionnaire. The first 8 sources apply to all MPs in a standard and equal fashion. The last two sources do not, as they are sampled based on MP reports about the location of school support and general development projects, and are intended primarily as an input into the verification sheet. We will also use information from these latter two sources descriptively to cross check our main assumptions about the level of informedness of main town and clinic respondents and their similarity to more rural constituents.

2. Domain D: Effects of PC-level debates on Elected Officials - Hypotheses and data sources

This sections specifies the main areas of hypothesized effects and lists the corresponding sources of data. In general, we will look for effects in both “hard facts,” for example CFF expenditures that are verified via field visits, as well as in MP behavioral or priming responses, where they may be more likely to claim better performance in self-reports.ⁱⁱ

A. Activity in Parliament

Hypothesis: Accountability pressure of constituent exposure to debates is expected to increase the activity and engagement level of elected MPs. All tests are one-sided towards increased activity. Relevant indicators:

- i) **Attendance** in Parliamentary session as recorded in the V&Ps
- ii) **Participation** via making public statements in Parliamentary sessions as recorded in the V&Ps and Hansards
- iii) **Committee** membership
 - a. Number of committees serving on as compiled by the Clerk of Parliament
 - b. Attendance in committee meetings as recorded by committee clerks. This data will need to be assessed for completeness and accuracy before proceeding with analysis as many MPs serve on multiple committees and recordkeeping may vary substantially across clerks.ⁱⁱⁱ
- iv) **(Secondary)^{iv} Self-reported** MP activity in follow-up survey regarding discussing topics with other MPs, raising issues during committee meetings, and other promotional work (GEB: Q17, 18, Issue: Q21, 22, 23)

B. Consistency with pre-election promises

Hypothesis: The publicity of the debates helps solve the candidate commitment problem and makes their post-election behavior in Parliament more consistent with their pre-Election promises. All tests are one-sided towards increasing consistency. Relevant indicators:

- i) Participation in **Parliamentary session in key priority areas**, where pre-election priorities were collected in the MP candidate survey and in the debates for treated MPs, and post-election participation is recorded in the V&Ps and Hansards
- ii) **Voting** in accordance with pre-election stated preferences for the Gender Equity Bill (when it arises in Parliament); for the Freedom of Information Bill; and votes that relate to the sectors specified as first priority issues as recorded in V&Ps and Hansards (relevant votes need to be identified and coded)^v
- iii) **Membership in committees** that govern stated key priority issues
- iv) **Constituent assessment of consistency** with and **performance in promoting** priority areas in CFF Main Town and Clinic surveys (QC5-C14 in main; QC11-15 in clinic)
- v) **(Secondary)** Correspondence between **MP self-reports** in pre- and post-Election surveys (GEB Q15, Issue Q19, CFF Q24, Transparency Q25)

C. Constituency engagement

Hypothesis: Accountability pressure of constituent exposure to debates is expected to increase post-election engagement with constituents. All tests are one-sided towards increased engagement. Relevant indicators:

- i) Number of **visits** to constituency as verified across the CFF main community and clinic surveys (QM2-5 in Main and M2-5 in Clinic)
- ii) Number of **substantive meetings** held with constituents as verified across the CFF main community survey (Main QM6-8, with truthfulness check QM8)
- iii) **(Secondary) Self-reported visits** to constituency in the MP follow-up survey (Q28-30)

- iv) **(Secondary) Self-reported number of substantive meetings** in the MP follow-up survey (Q31-32)
- v) **(Secondary) Number of clinic oversight visits** captured in clinic survey (QC1-5)

D. CFF spending

Hypothesis: Accountability pressure of constituent exposure to debates is expected to increase development expenditure under the CFF (and potentially through mobilizing other funds, TBD). All tests are one-sided towards increased engagement. Relevant indicators:

- i) Overall **proportion of CFF funds spent** that can be verified; and **proportion dedicated toward constituency development** as opposed to transport^{vi}. Many sources to this, but primary metric should be summarized in the CFF verification sheet. Triangulation from: CFF projects in Main (QC15-17; and Section P); Target (QC18-19, and Section P); contributions to clinic development (QC6-9 in clinic survey); contributions to scholarships and school development (QC3-9 in school survey); and note truthfulness assessment questions at end of relevant sections
- ii) **(Secondary) Self-reported** expenditures in MP follow-up survey (Q33) and post-survey clarification phone calls – proportion reported and proportion for development;
- iii) **(Secondary) Self-reported biggest accomplishments** as MP in follow-up survey (Q34)

3. Descriptive analysis

These surveys also were designed to collect indicators that flesh out other areas of primarily descriptive analysis.

- A. Descriptive analysis of Target village and School survey responses to questions about MP consistency, MP visits, meetings, and performance
- B. MP self-reports of participation in key areas will be used to select relevant Hansards and cross-check official records in MP follow-up survey: Q13-14 general debate, Q16 GEB, Q20 priority issue, Q26-27 on Freedom of info to both cross check and potentially add nuance to TE estimate of accountability and activity level if find variation in abstention or failed participation attempts
- C. Content or textual analysis of the V&Ps and Hansards

Two other areas relate to earlier stages of the research design but the data for which was collected in the MP follow-up survey that this plan governs.

- D. Secondary data on party response to assignment of treatment and control in MP follow-up survey Q8-9 campaign support
- E. Check on T/C balance and/or (rule out) party selection response to treatment assignment in MP follow-up survey Q10-12 quiz questions. Supplement this with data on candidates in pre-election survey

ⁱ We use these endnotes to clarify and flag minor deviations from what is pre-specified here and estimates in the main text of the paper. Note first the typo in the title: it should read “constituency” not “polling center” level randomization.

ⁱⁱ Since we did not specify the econometrics here, we use minimal controls (and include a robustness check in appendix H with no controls) and report only one mean effects index for all 9 underlying outcomes.

ⁱⁱⁱ There were no attendance records for the vast majority of committees so this outcome is dropped.

^{iv} We did not find many interesting results for secondary outcomes so report only primary.

^v These bills either did not come up for a vote or were voted on unanimously so these outcomes are dropped.

^{vi} Clarification: this is one single outcome, as we did not attempt to verify non-development expenditures.

Appendix E: Heterogeneous treatment effects

We find little evidence for systematic heterogeneity in treatment effects on voters by socio economic or demographic indicators. The following table estimates heterogeneous effects by subgroups of gender, age and lack of fluency in Krio (the language of the debates). Specifications use the hypothesis level mean effects index and include all subgroup terms and their interaction with treatment status in a single regression. Across the fifteen estimates of interest, only the negative coefficient on political knowledge for women (-0.08 standard deviation units, s.e. 0.02) is significant at conventional levels. This suggests that women acquired only 75% as much political knowledge from the debates when compared to men. We find little evidence that voter responsiveness varied with the expected competitiveness of the race, based on 2007 vote margins, or with candidate performance in the debate, based on expert panel scores (results not shown). Our results also do not appear to be driven by large effects in any particular constituency. As an example, the treatment effect estimate on voting for the debate winner is robust to excluding each constituency one by one.

Considering dissipation of effects over time, we find suggestive evidence for an immediate drop in political knowledge gains in the days after treatment, but no evidence for additional decay between treatment exposure and the election. Confining attention to the treatment group, voter knowledge doubled from the before- to after-screening surveys: voters on average correctly answered 24 percent of political knowledge questions at baseline, which jumped to 46 percent immediately after watching the group screening. By the time of the exit poll, this percentage had fallen to 40, implying that a third of the initial gains had dissipated. Similar estimates obtain for those who watched the debate privately via tablet. Bringing in the control group, we estimate whether this attenuation covaries with the time lag between the screening and the exit poll, which ranges from 6 to 35 days. The time variation is not random, so estimates rely on the assumption that factors determining field deployment (e.g. remoteness) are orthogonal to voter responsiveness to treatment. Here we find no evidence for heterogeneity over time: effects for those treated far from the election, e.g. 30 days earlier, are similar to estimates for those treated close to the election, e.g. within 10 days of the exit poll. Our interpretation is that some knowledge gains dissipate quickly after exposure, while the remaining gains persist for several additional weeks.

Appendix Table E: Treatment Effect Heterogeneity, Subgroup Analysis

	H1. Political knowledge	H2. Policy alignment	H4. Votes for debate winner
	(1)	(2)	(3)
Female * Treatment	-0.076*** (0.021)	0.037 (0.031)	-0.034 (0.039)
Youth * Treatment	0.022 (0.020)	-0.033 (0.031)	0.070 (0.045)
No Krio * Treatment	-0.025 (0.042)	0.038 (0.061)	0.107* (0.061)
Treatment	0.313*** (0.030)	0.095** (0.042)	0.050 (0.050)
Female	-0.064*** (0.017)	0.024 (0.027)	0.060* (0.035)
Youth	-0.009 (0.014)	0.011 (0.022)	-0.027 (0.033)
No Krio	-0.098*** (0.025)	-0.022 (0.037)	0.064* (0.036)
Observations	5,246		

Note: This table estimates heterogeneous effects of the debate group screenings by subgroups of voters. In this analysis: i) significance levels indicated by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ based on two-sided tests; ii) robust standard errors clustered by polling center; iii) all specifications include stratification bins for the polling center (number of registered voters and distance to next nearest) and constituency fixed effects; iv) specifications further include additional control variables years of schooling, polygamous marriage, farming occupation and radio ownership; v) treatment effects are on the hypothesis-level mean effects indices that are constructed following Kling, Liebman and Katz (2007) and expressed in standard deviation units, with missing values for component measures imputed at random assignment group means; vi) data source is the exit poll survey from the group screening sample; and vii) the PAP also specified the politically informed as a subgroup, but as our measure of this is potentially endogenous to treatment we omit it from this analysis.

Appendix F: Campaign results for other party officials

We find little evidence that centralized party bosses and candidates for President, Local Councillor and Local Council Chair altered their campaign strategy in response to dissemination of the MP candidate debates. While the treatment effect for the mean effects index in the following table is positive in sign (0.08 standard deviation units), it is not significant at conventional levels (s.e. 0.05 and p -value 0.11). Similarly, while the majority (16 of 21) of treatment effect estimates for individual outcomes are positive, none are significant at conventional levels. This can be viewed as a pseudo placebo test: candidates for offices not involved in the debates should not alter their campaign strategy in response to the MP debate road show. This would make sense if the parties did not strongly coordinate campaigns across candidates for different offices, or if the road show was not a salient enough event to justify reallocating campaign support from other party members to support the participating candidates. While this seems plausible, we do not place too much weight on this interpretation for two reasons. The sample for this community-level survey is small ($N = 224$), so power to reject the null is limited. And, the community survey questions bundled together the campaign efforts of all party officials and candidates for all offices, which includes Parliament, so they do not clearly exclude the MP candidates as one would do for a true placebo.

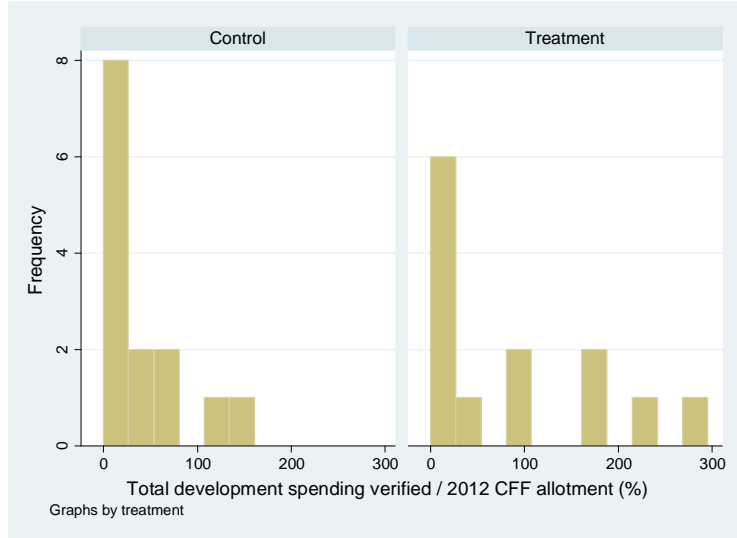
Appendix Table F: Campaign Response of Other Party Officials to Public Screenings

	Control mean	Treatment effect	Standard error	Naïve <i>p</i> -value (2 sided)	FDR <i>q</i> -value	<i>N</i>
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Hypothesis C1. Mean Effects Index (all 21 outcomes)</i>	0.000	0.082	0.052	0.113		224
Any visits by party officials, APC	0.819	0.004	0.060	0.941	0.99	210
Number of visits by party officials, APC	1.857	0.427	0.383	0.266	0.99	210
Any political rallies, APC	0.248	0.045	0.055	0.412	0.99	207
Number of political rallies, APC	0.467	0.046	0.147	0.753	0.99	209
Any party officials distributed gifts? APC	0.481	0.080	0.063	0.206	0.99	205
Number of community members receiving gifts, APC	0.295	0.069	0.044	0.114	0.99	208
Number of posters displayed in community, APC	0.699	-0.022	0.042	0.601	0.99	207
Any visits by party officials, PMDC	0.368	0.008	0.069	0.912	0.99	133
Number of visits by party officials, PMDC	0.515	0.386	0.342	0.260	0.99	134
Any political rallies, PMDC	0.044	-0.028	0.022	0.198	0.99	134
Number of political rallies, PMDC	0.044	0.004	0.043	0.934	0.99	134
Any party officials distributed gifts? PMDC	0.045	-0.003	0.032	0.928	0.99	133
Number of community members receiving gifts, PMDC	0.025	-0.002	0.017	0.924	0.99	133
Number of posters displayed in community, PMDC	0.235	0.048	0.052	0.353	0.99	132
Any visits by party officials, SLPP	0.739	0.051	0.062	0.409	0.99	222
Number of visits by party officials, SLPP	1.679	0.066	0.230	0.773	0.99	224
Any political rallies, SLPP	0.159	0.011	0.044	0.809	0.99	213
Number of political rallies, SLPP	0.315	-0.079	0.104	0.448	0.99	219
Any party officials distributed gifts? SLPP	0.368	0.078	0.072	0.278	0.99	213
Number of community members receiving gifts, SLPP	0.226	0.038	0.040	0.339	0.99	214
Number of posters displayed in community, SLPP	0.555	0.047	0.035	0.187	0.99	221

Note: This table estimates the campaign response of party officials not directly involved in the MP debates to the allocation of the debate public screenings. In this analysis: i) significance levels +*p* <0.10, * *p* <0.05, ** *p* <0.01 based on two-sided tests; ii) robust standard errors clustered by polling center; iii) specifications include stratification bins for the polling center (number of registered voters and distance to next nearest) and constituency fixed effects; iv) hypothesis-level mean effects indices are constructed following Kling, Liebman and Katz (2007) and expressed in standard deviation units, with missing values for component measures imputed at random assignment group means; v) adjustments to control the false discovery rate (FDR) computed following Benjamini, Krieger and Yekutieli (2006) and Anderson (2008); and vi) data source is the community level exit poll survey.

Appendix G: Distribution of verified CFF expenditures

Appendix Figure G: Histogram of CFF Spending by Treatment Assignment



Notes: This figure plots the distribution of total development expenditures that could be verified on the ground through detailed field visits to each MP’s home constituency, scaled by the amount of the 2012 constituency facilitation fund (CFF) allotment, separately for control (on the left) and treated MPs (on the right). Comparing the two subplots shows that the positive treatment effect estimated in Table 4 is driven by differences in both tails: there is a higher frequency of low values among control MPs, as well as a larger number of high values among treated MPs. Values above one hundred percent reflect the fact that fieldwork occurred after the first 18 months in office (i.e. potentially capturing more than one annual CFF allotment) and that MPs are free to raise additional monies to supplement the CFF.

Appendix H: Robustness Check on MP Performance Estimates

	Control mean	Treatment effect	Std. error max of (OLS, HC ₂)	Naïve <i>p</i> - value (2 sided)	N
	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Hypothesis-level policy effect</i>					
Mean effects index (9 outcomes)	0.000	0.263	0.147	0.074*	28
<i>Panel B: Estimates for individual policy outcomes</i>					
Development spending verified in the field (% 2012 CFF)	35.56	49.46	29.31	0.105	27
Total number of constituency visits	2.915	1.169	0.595	0.061*	28
Total number of public meetings held with constituents	1.018	1.006	0.575	0.080*	28
Percent of 2012-13 sittings attended (out of 57 total)	76.69	4.225	3.105	0.174	28
Total public comments in Parliamentary sittings 2012-13	4.286	-1.214	1.906	0.530	28
Committee membership (total number)	3.929	0.429	0.557	0.449	28
Total public comments in priority sector agenda items	0.154	-0.149	0.149	0.328	27
Membership in priority sector committee	0.231	0.120	0.185	0.517	27
Constituent assessment of focus on priority sector	0.571	-0.352	0.142	0.021**	27

Note: This table replicates estimates from Table 4 under the conservative specification of no control variables and two-sided tests. In this analysis: i) significance levels + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$ based on two-sided tests; ii) standard errors are the maximum value from conventional OLS and bias corrected HC₂ estimators in MacKinnon and White (1985), following discussion in Angrist and Pischke (2009); iv) specifications include only the 3 randomization stratification bins of ethnic-party bias; and v) hypothesis-level mean effects indices are constructed following Kling, Liebman and Katz (2007) and expressed in standard deviation units, with missing values for component measures imputed at random assignment group means.

Appendix I: Survey Priming and Reinforcing Effect

Panel A: Survey Priming Effects Across Private Viewing Arms

Mean effects index by hypothesis	Treatment effect beyond survey:		Survey priming effect:	
	Debates vs surveyed controls		Surveyed vs "pure" controls	
	Treatment effect	Naïve p-value	Treatment effect	Naïve p-value
	(Std. error)	(1 sided)	(Std. error)	(1 sided)
	(1)	(2)	(3)	(4)
H1. Political Knowledge	0.146*** (0.026)	<0.001	0.034** (0.017)	0.025
H2. Policy Alignment	0.078*** (0.032)	0.010	0.012 (0.038)	0.373
H4. Vote for best	0.023 (0.053)	0.334	0.077* (0.053)	0.077
Observations	933		935	

Panel B: Survey Reinforcing Effects Across Public Screening Arms

Mean effects index by hypothesis	"Pure" treatment effect:		Survey reinforcing effect:	
	Debates without survey vs controls		Interaction of debates with survey	
	Treatment effect	Naïve p-value	Treatment effect	Naïve p-value
	(Std. error)	(1 sided)	(Std. error)	(1 sided)
	(1)	(2)	(3)	(4)
H1. Political Knowledge	0.282*** (0.034)	<0.001	0.032* (0.021)	0.067
H2. Policy Alignment	0.105*** (0.041)	0.005	-0.002 (0.030)	0.526
H4. Vote for best	0.082** (0.048)	0.043	-0.010 (0.035)	0.611
Observations	5,400		5,400	

Note: This table decomposes the total treatment effect of debates into a survey priming or reinforcing effect (in Columns 3 to 4) and a direct effect of the debate content net of survey effects (Columns 1 to 2). In this analysis: i) significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ based on one-sided per comparison tests; ii) robust standard errors clustered by polling center; iii) specifications include years of schooling, radio ownership, stratification bins for the relevant randomization procedure and constituency fixed effects; iv) hypothesis-level mean effects indices are constructed following Kling, Liebman and Katz (2007) and expressed in standard deviation units, with missing values for component measures imputed at random assignment group means; and v) mean effects indices are standardized with respect to the pure control group for all of panel A.