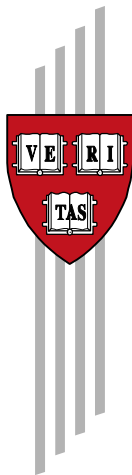


# **Who Debates, Who Wins? At-Scale Experimental Evidence on Debate Participation in a Liberian Election**

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# Who Debates, Who Wins? At-Scale Experimental Evidence on Debate Participation in a Liberian Election\*

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## Abstract

We examine how candidate selection into the supply of policy information determines its electoral effects. In a nationwide debate initiative designed to solicit and rebroadcast policy promises from Liberian legislative candidates, we randomized the encouragement of debate participation across districts. The intervention substantially increased the debate participation of leading candidates but led to uneven electoral returns for these candidates, with incumbents benefiting at the expense of challengers. These results are driven by differences in compliance: complying incumbents, but not challengers, positively selected into debate participation based on the congruence of their policy priorities with those of their constituents.

**JEL codes:** D72, O12

**Keywords:** accountability, information, selection

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# 1 Introduction

Following classic models of electoral accountability (Barro, 1973; Ferejohn, 1986; Fearon, 1999; Holmstrom, 1999), much extant empirical research considers how supplying citizens with policy information, generally about their incumbents, affects what they know about candidates, how they evaluate those candidates, and ultimately their voting behavior.<sup>1</sup> While often demonstrated to promote programmatic competition, this work overlooks the strategic decision that candidates make in supplying policy information to the public in the first place. Candidates in weakly-institutionalized democracies may face strong incentives to limit the amount and dissemination of their policy promises, which might worsen their electoral prospects or restrict their ability to deviate once in office (Fujiwara and Wantchekon, 2013). Designing scalable interventions that promote programmatic competition, therefore, requires a better understanding of when and why candidates choose to supply policy information and what the consequences are when they do.

We study the electoral consequences of the supply of policy information by legislative candidates in Liberia, where clientelism is pervasive and the media sector is weak (Bowles et al., 2019a). In an effort to enhance programmatic electoral competition in the 2017 legislative election, USAID and the international NGO Internews organized standardized debates in every electoral district. The moderators asked participating candidates a series of questions regarding their policy priorities, which often included district schools, primary healthcare facilities, and infrastructural investments. Rather than large townhall-style debates, the emphasis was on soliciting concrete policy promises from the candidates that would then be rebroadcast multiple times by community radio stations.

We partnered with USAID and Internews to experimentally study how the effects of their large-scale debate initiative were shaped by the participation of leading candidates, which they were concerned would be low. The participation of leading candidates (incumbents and their most serious challengers) in the debates was considered important, first, so that citizens would pay attention to the debates and, potentially, engage more with the debate information as a result. Second, because few other channels for disseminating policy information exist, debate participation was a primary way that constituents could learn about candidates' policy

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<sup>1</sup>Ferraz and Finan (2008), and Larreguy et al. (2019) show that local media reports on incumbent performance enhance electoral accountability. However, a series of localized information dissemination campaigns fail to consistently replicate these media effects (Banerjee et al., 2011; Chong et al., 2015; Bhandari et al., 2019; Dunning et al., 2019). Closest to our work, recent studies analyze whether debates can lead citizens to make more informed voting decisions (Bidwell et al., 2019; Platas and Raffler, 2019).

platforms and then vote for those candidates with priorities closest to their own.

Candidate participation in debate initiatives cannot be taken for granted. One leading debate organizer notes, “The greatest universal challenge that [debate] sponsors face regardless of country or culture is convincing candidates to take part in debates” (National Democratic Institute, 2014). The decision to participate is risky, particularly in clientelistic settings where the returns to programmatic competition might be limited and uncertain. Candidates *ex ante* risk performing poorly, revealing their policy priorities to be disconnected from their constituents’, and restricting their ability to target campaign promises to small groups of influential voters. These risks are especially pronounced for leading candidates, who emerged within the existing clientelistic equilibrium, enjoy greater resources for on-the-ground campaigning, and are more likely to be attacked by opponents to gain publicity.

Accordingly, there are abundant examples in both developing and developed countries of leading candidates who refused to participate in debates. Prominent examples include President Jimmy Carter in the first 1980 U.S. presidential debate, President Yoweri Museveni in all 2016 Ugandan presidential debates, Prime Minister Theresa May in all debates with other party leaders ahead of the 2017 general election in the U.K., and President Uhuru Kenyatta in one of the 2017 Kenyan presidential debates. Revealingly, President Kenyatta argued, “I decided that he [referring to his main challenger Raila Odinga] will debate alone because I have nothing to debate with him. I will not waste my time there.” Bowles et al. (2019b) find that incumbent candidates in Sub-Saharan Africa have participated in just 60% of all presidential debates.

To examine how candidate participation affected the electoral consequences of the debates, we randomly assigned an intervention designed to more intensively persuade candidates to participate in their debates in treated districts compared to normal intensity in inviting candidates in control districts. Our intervention was aimed at mitigating the concerns of leading candidates regarding personal attacks and biased moderators, and at highlighting that the emphasis of the debates would be narrowly focused on policy proposals. The intervention substantially increased the participation of the leading candidates in treated districts where incumbent participation rates increased by a third to 50%, while the participation of their most credible challengers increased by a fifth to 60%. There was a negligible treatment effect for marginal candidates, who participated at higher rates absent the intervention.

We assess the effect of increased debate participation by leading candidates across various indexes of outcomes. We start by examining effects on citizens’ exposure to the debates, learning, and political engagement. Next, we consider how leading candidates adjusted their

campaigning strategies. Then, we consider the effects on voting outcomes. Lastly, we examine how differences in the selection of candidates into debate participation explains the uneven electoral consequences of the initiative. In doing so, we make use of a rich set of original data sources, including a nationwide panel survey of more than 4,000 citizens, a survey of more than 600 candidates who ran in the election, a survey of more than 50 radio stations, full transcripts from debates, around 20 focus groups, and polling station-level electoral results.

We find that the debate participation of leading candidates matters: citizens in treated districts were 0.3 standard deviations (sd) more intensively exposed to debate content than those in control districts. Similarly, citizens in treated districts were 0.13 sd and 0.15 sd respectively more likely to exhibit knowledge about the debates themselves and of poorly-understood policy issues discussed in each debate than citizens in control districts. As our qualitative accounts corroborate, citizens were more interested in and responsive to debate information when leading candidates participated. Increased exposure to information about relevant candidates led to increased political engagement. In treated districts, citizens engaged in 0.25 sd greater political information acquisition, and 0.15 sd greater discussion and coordination with others than in control districts.

The consequences for candidates varied. Increased exposure to the debates led citizens in treated districts to update positively (but weakly) about incumbents' competence and policy priorities, becoming more certain in their knowledge in the process. However, in treated districts, citizens experienced negligible gains in certainty about the competence or policy priorities of challengers, and updated weakly *negatively* about their competence. This is consistent with qualitative evidence suggesting that participating incumbents dominated their debates. Simultaneously, aided by an increase in demand from radio stations, incumbents increased their radio campaigning in treated districts while challengers reduced their on-the-ground campaigning. This suggests that incumbent debate participation had a deterrent effect on challengers' campaign efforts.

Ultimately, the intervention led to improved electoral outcomes for incumbents in the treated districts, according to both self-reported and polling station voting outcomes. Remarkably, 50% of incumbents in treated districts won re-election, compared to 43% in control districts. These changes in electoral outcomes occurred particularly in districts where we predicted incumbents to perform well in the debates and where incumbents had policy platforms that were better aligned with the priorities of their constituents. The selection into the supply of policy promises by leading candidates therefore led to broad increases in citizen exposure, engagement, and

learning, but uneven electoral consequences that favored incumbents over their challengers.

These uneven electoral consequences are explained by differences in the compliance behavior of incumbents compared to their challengers. Incumbents who complied with the intervention were not well known by their constituents beforehand, but their policy priorities were well aligned with those of their constituents. In other words, incumbents recognized whether their policy priorities matched those of their constituents and whether their constituents knew this. When incumbents would benefit from broadcasting their policies, they then chose to comply with the intervention, which helped them at the ballot box. Complier challenger candidates, about whom citizens were also very uncertain, often lacked the political sophistication to correctly assess the returns to debate participation. If anything, they negatively self-selected into debate participation on the basis of the overlap between their policy priorities and those of their constituents. Importantly, this pattern of compliance largely mirrors the self-selection of candidates into debate participation in control districts. This suggests that the treatment effects of the intervention are likely representative of those of the overall debate initiative.

Variation in candidates' ability to correctly evaluate the returns to debate participation, therefore, critically determined the electoral consequences of the initiative. This differential ability to select into supplying policy information may also condition the effect of similar large-scale initiatives. Most of the non-participating incumbents had policy preferences that were very poorly aligned with those of their constituents. Had all candidates participated, therefore, the results could even have reversed what we find. Such results would match those of randomized small-scale debate interventions, all of which ensured that leading candidates participated (Bidwell et al., 2019; Platas and Raffler, 2019). Our research shows that candidate attendance, and therefore the external validity of such results, cannot be presumed when scaling such initiatives because their effects depend on the attendance of these leading candidates. Indeed, districts that were not assigned to the invitation intervention saw only 35% of incumbents and 50% of challengers participating in the debate.

This result speaks to recent work highlighting the importance of experimentation at scale, since the effect of small-scale interventions might differ substantially when scaled (Al-Ubaydli et al., 2017; Banerjee et al., 2017; Muralidharan and Niehaus, 2017). In particular, Al-Ubaydli et al. (2017) point to individual non-adoption of the treatment as a key factor explaining the difference in outcomes between small-scale and large-scale interventions. This lack of compliance may be especially pronounced when candidates face incentives not to participate in initiatives designed to move away from the clientelistic equilibrium from which they often benefit. Our

results highlight the importance of investigating who selects into programmatic initiatives to understand their effects when scaled.

More specifically, our findings contribute to the literature on information and electoral accountability. This literature shows that such accountability is enhanced through revelations of past incumbent performance via broadcast and social media (Ferraz and Finan, 2008; Enríquez et al., 2019; Larreguy et al., 2019), but not necessarily via localized information dissemination campaigns (Banerjee et al., 2011; de Figueiredo et al., 2013; Chong et al., 2015; Bhandari et al., 2019; Dunning et al., 2019). Closer to our work, recent studies that analyzed the effect of small-scale debate initiatives in which the participation of the leading candidates was ensured found a leveling of the playing field favoring challenger candidates (Bidwell et al., 2019; Platas and Raffler, 2019). Other work similarly shows that citizens exposed to the campaign promises of all candidates learn about candidate policy priorities and vote accordingly (Cruz et al., 2019). However, none of these studies address how the results of their information dissemination initiatives might be conditioned by the strategic decision of candidates to supply such information or not, which in turn may affect their external validity.

Lastly, our results also contribute to recent experimental work designed to reduce clientelistic campaign practices common in developing democracies (Finan and Schechter, 2012; Cruz et al., 2018; Duarte et al., 2019). Fujiwara and Wantchekon (2013) show that programmatic platforms transmitted through town hall meetings reduced vote buying and increased electoral support for the candidates involved. Vicente (2014), Hicken et al. (2018), Blattman et al. (2019), Bobonis et al. (2019), and Vasudevan (2019), however, show that interventions designed to combat vote-buying practices while facilitating competition on the basis of policy vary in their success. Our results underscore the importance of variation in the suitability for programmatic competition among candidates who became prominent under the existing clientelistic equilibrium. This suggests that recruiting new citizens to run for office and training them in the articulation of policy platforms could be important forces in shifting towards more programmatic political equilibria.

The paper proceeds as follows. In Section 2 we outline Liberia's political context, legislative candidacy, and electoral campaigning. In Section 3 we describe the debate initiative and our randomized intervention before providing descriptive evidence on the debates. Section 4 reports our hypotheses, and Section 5 describes the set of original data sources we draw on to evaluate them. Section 6 reports our estimation strategy. Section 7 presents results, which we discuss in Section 8 when we analyze candidate selection into debate participation. We

conclude in Section 9.

## 2 Background

### 2.1 Electoral context

Since its emergence from civil war in 2003, Liberia has held three presidential elections (2005, 2011, and 2017), three legislative elections (2005, 2011, and 2017), and two senatorial elections (2005 and 2014). The focus in this paper is on the House of Representatives election of October 2017, in which each of 73 electoral districts elected a single representative for a six-year term in a first-past-the-post electoral system. Key responsibilities of House members include making laws, controlling access to development funds, and allocating and implementing public goods. Representatives are rewarded with an annual salary over \$200,000 USD in a country with an annual per capita income of \$900 (IREDD, 2016).<sup>2</sup> The relatively low barriers to candidacy combined with a fragmented party environment mean that there are usually a large number of candidates for each office. In the 2017 election there were 984 candidates from 26 different political parties, with between 3 and 28 candidates per district. Incumbents sought re-election in nearly 90% of the districts. As one editorial opined, “Rest assured that this is not a healthy expression of diverse opinions. Everyone wants a piece of the pie.” (Glencorse and Yealue, 2017).

Once in office, legislator performance is varied, with some lawmakers having close to perfect attendance while others attending as few as 45% of legislative sessions (IREDD, 2016). Dissatisfaction with incumbent performance is widespread, with two-thirds of citizens mistrusting their representative and disapproving of their incumbent’s performance (Afrobarometer, 2015). More than half of citizens report seeing their representatives only at election time, once every six years (USAID, 2018). This dissatisfaction is not the result of citizens being unaware of who their legislator is—92% of our citizen survey correctly named their legislator—but rather the lack of mechanisms to hold politicians accountable for poor performance. There is a paucity of credible information about political activity as a result of an underdeveloped media sector and poor infrastructural conditions.

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<sup>2</sup>In addition, representatives are often gatekeepers to the rich natural resource wealth of the country by controlling access to concessionary agreements. A major corruption scandal in 2016, for example, revealed that both the Speaker of the House of Representatives and the Chair of the incumbent Unity Party had conspired to illegally broker an iron ore concession in exchange for bribes (Global Witness, 2016).



## 2.2 Who runs for office?

We draw on an original survey of 612 candidates to provide some descriptive evidence of candidate characteristics.<sup>3</sup> Throughout this paper we distinguish between three *predicted leading candidates* per district and *other candidates* for theoretical, practical, and measurement reasons. We further divide *predicted leading candidates* into *incumbents* and *predicted challengers*.<sup>4</sup> This assignment of 219 predicted leading candidates—64 incumbents and 155 predicted challengers—and 765 *other candidates* is described fully in Appendix A.1.

### I. Candidate Characteristics

	Age (1)	University educated (2)	Ran before (3)	Govt. job before (4)	NGO job before (5)	Advocacy experience (6)	Campaign expenditure (7)	Radio station (8)
Incumbent	55.8	0.68	1.00	0.48	0.35	0.87	\$61,458	0.16
Challenger	48.9	0.64	0.43	0.30	0.38	0.88	\$41,282	0.06
Other	47.7	0.53	0.22	0.32	0.42	0.85	\$30,083	0.03

Table displays mean values of column variables for incumbents, challengers, and other candidates according to our survey. ‘Age:’ candidate age in years. ‘University educated:’ candidate has completed university. ‘Ran before:’ candidate ran for office at least once before. ‘Govt. job before:’ candidate has held non-elected government job before. ‘NGO job before:’ candidate has worked for an NGO before. ‘Advocacy experience:’ candidate reports having worked on an advocacy campaign before. ‘Campaign expenditure:’ self-reported campaign spending in USD. ‘Radio station:’ candidate either owns or manages a radio station.

In Table I we provide descriptive statistics by candidate category. Candidates generally come from Liberia’s elite, with more education than an average citizen, and are overwhelmingly male (84%). Incumbents are older and possess higher levels of education than challenger candidates. They are much more likely to possess prior experience in a non-elected government job and are less likely to have experience working for an NGO. Almost a third of all candidates have previously run for office, and most report experience in advocacy campaigns in their districts. Candidates report spending substantial amounts—on average above \$30,000—on their campaigns. Incumbents, however, report spending 50% more than predicted challengers and 100% more than other candidates. The differences between incumbents and challengers are consistent with the substantial literature on the resource advantages enjoyed by incumbents in developing democracies.

<sup>3</sup>The response rate is balanced across treatment groups both overall and within candidate categories.

<sup>4</sup>The objective with this categorization was not to predict actual election outcomes but to facilitate analysis by identifying three candidates per district who had genuine chances of success—qualitatively, there exists a long tail of candidates who run primarily to enhance their profiles and secure post-electoral favors (Spatz and Thaler, 2018). Moreover, the definition of *actual leading candidates*, those whose vote share ranked in the top three of their district, might be endogenous to our intervention.

## 2.3 Campaigning and policy promises

Electoral campaigns are marked by local rallies at which candidates distribute gifts in cash or kind to generate support. Nearly 80% of surveyed candidates reported visiting most or all communities in their district, while nearly half reported distributing gifts in most or all communities. During campaign season, incumbents in particular orchestrate mass campaigns to buy turnout and truck voters from the capital to their districts (Bowles et al., 2019a). A USAID survey in 2015 found that 35% of respondents were personally given money in exchange for their vote (USAID, 2015).

In this clientelistic context, candidates face few incentives to widely disseminate policy promises. The candidates exhibit great awareness of the differences in the *types* and *credibility* of policy promises delivered at local rallies versus over the radio, as Table II shows. Overall, candidates believe that promises made on the radio are more credible than those made at rallies, but they acknowledge the low likelihood of any campaign promise being kept. Relevant for our later results, incumbents appear to be more sophisticated in this regard.

II. Candidate Attitudes Towards Policy Promises

Candidate type	Different promises (1)	Rally credibility (2)	Radio credibility (3)
Incumbent	0.73	0.19	0.26
Challenger	0.70	0.12	0.14
Other	0.67	0.16	0.15

Table displays mean values of binary column variables for incumbents, challengers, and other candidates based on our surveys. ‘Different promises:’ candidate believes that different promises are made on radio versus in-person campaigning. ‘Rally credibility:’ candidate believes that promises made at rallies are very likely to be fulfilled. ‘Radio credibility:’ candidate believes that promises made on radio are very likely to be fulfilled.

Because candidates lack incentives to publicize policy promises, the wide dissemination of policy platforms across districts is extremely rare.<sup>5</sup> As such, candidate campaigns generally lack policy platforms, focusing instead on promises of local development (which rarely come to fruition) delivered through on-the-ground campaigning buoyed by vote buying. The absence of programmatic information is facilitated by a fractured media landscape. Radio stations are a potentially important source of access to political information: radio ownership is high at 83%,

<sup>5</sup>One of the country’s most prominent newspapers, the *Daily Observer*, built a “promises tracker” ahead of the election where candidates could outline their policy platforms. No incumbent did this.

and 62% of Liberian respondents report listening to news on the radio every day (Afrobarometer, 2015). However, because the radio industry lacks regulation, the market is fragmented, access to electricity is sporadic, and sources of commercial revenue are scarce, radio stations frequently become the mouthpieces of particular political figures and local firms (Kamara, 2017). Indeed, as Table I shows, many incumbents actually own their own radio stations and many more candidates are informally connected to other stations.

### 3 Candidate debates

#### 3.1 Debate structure

In the run-up to the October 2017 elections for the House of Representatives, Internews Liberia led a nationwide debate initiative that they hoped would push back against Liberia's clientelistic equilibrium and establish the beginnings of a programmatic one.<sup>6</sup> Internews partnered with several Liberian journalist associations to organize debates across different parts of the country.<sup>7</sup> In each district, a local journalist was responsible for conducting research about the issues relevant to constituents, publicizing the debate, and moderating it. The debates took place from mid-August to mid-September 2017. In total, 129 debates were held across all 73 districts.<sup>8</sup>

Debate venues were mostly administrative buildings, town halls, and schools. Every debate followed a simple and uniform structure. First, candidates were asked to outline their campaign policy promises. The moderator then posed the same questions to each candidate in turn, and each candidate was allowed three minutes to respond. The first question in each debate was related to the management of the County Social Development Fund (CSDF), which is poorly managed, with little oversight or input from citizens. Second, candidates were asked about how they would spend their Legislative Support Project (LSP) discretionary funds. After these standardized questions, candidates were asked two or three questions about relevant local issues based on research conducted by the moderator. Moderators intervened to prevent candidates from making personal attacks on other candidates.

Debate content was disseminated by partnering with at least one community radio station in each district that would broadcast and later rebroadcast the debate on average six times,

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<sup>6</sup>Other organizations held debates for the presidential race; those debates are not the focus here.

<sup>7</sup>These partners were the Press Union of Liberia (PUL), Liberia Media for Democratic Initiatives (LMDI), and the Center for Media Studies and Peacebuilding (CEMESP).

<sup>8</sup>In districts with a large number of candidates, multiple debates were held, generally on the same day, with candidates randomly assigned to a debate.

Figure 1. District Debate (Montserrado D3)



with rebroadcasting concentrated in the 10 days before the election.<sup>9</sup> Internews chose 43 radio stations to rebroadcast the debates, making selections based on the signal strength of the station to maximize audience size and discarding any stations that were managed or owned by candidates running for office in that district.<sup>10</sup> We estimate that nearly 90% of the population was covered by a signal from the station broadcasting that district's debate.

### 3.2 Invitation intervention

Whether to participate in a debate represents a strategic decision by candidates based on whether disseminating policy information will help or hurt their electoral bid. In the context of Liberia, as in many developing democracies, leading candidates generally emerge by developing clientelistic machines that mobilize voters on their behalf (Bowles et al., 2019a). The returns to programmatic competition for these candidates were deeply uncertain and, potentially, negative (Fujiwara and Wantchekon, 2013). Many leading candidates expressed hesitation regarding debate participation, and the partners were concerned that the attendance of the leading candidates at the debates would be low, which could undermine the potential impact of the initiative.

<sup>9</sup>The audience in attendance at each debate was around 100 people. Election-related violence is a concern in Liberia and so, to minimize the risk of conflict, in-person audiences for the debates were purposefully kept small.

<sup>10</sup>The debates were broadcast by fewer than 73 radio stations since some had the ability to broadcast debates in more than one district. A few stations were discarded due to political affiliations, primarily because they could not be relied upon to replay the debates in full with no editing.

The risks to debate participation were particularly acute for the leading candidates, who were frequently concerned that their proposed policies and competence would be attacked by other candidates and that moderators would be biased against them. Furthermore, poor performance in the debate might hurt their candidacy, while committing to a policy platform would restrict their ability to deviate on both the campaign trail and, eventually, once in office. Participation also involved non-trivial direct costs given the difficulties of travel in Liberia's rainy season. Leading candidates, who possessed greater resources available for on-the-ground campaigning than other candidates, additionally faced greater opportunity costs of participation. For less-relevant candidates with more limited resources, debate participation offered a much clearer positive expected return: they lacked the resources to buy votes or hold rallies, so participating in the debate represented a significant opportunity for them to publicize their name and policy priorities.

Therefore, to evaluate the impact of leading candidates' selection into supplying programmatic information, we randomized the level of effort associated with debate invitation across districts.<sup>11</sup> Rather than experimentally varying the extensive margin of whether candidates were invited at all, in our treated districts we significantly increased the intensity of invitation activities.<sup>12</sup>

Candidates in control districts were contacted by the relevant Liberian journalist association who invited them to attend and provided logistical information about the debate. In treatment districts, this was augmented with three additional components aimed at persuading leading candidates to attend. First, we sent official invitations via email from Internews with USAID branding as far ahead of the debates as possible. These invitations included logistical details and instructions for candidates to contact Internews if they had any doubts about the debates.<sup>13</sup> Second, we made phone calls to all candidates around two days before each debate to persuade them to attend. These were mostly conducted by a high-profile Liberian radio journalist who is widely known and respected by politicians. These calls were designed to address any concerns candidates had about the debates and to clarify the objective, structure, and unbiasedness of

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<sup>11</sup>Randomizing invitation effort at the candidate level would have raised serious concerns in terms of ethics and fairness to candidates.

<sup>12</sup>We also cross-randomized the extent of debate rebroadcasting, which ultimately had no effect because citizens were likely to hear their district debate even in districts without intensive rebroadcasting, likely reflecting the level of citizen interest in the debates. Because candidates were unaware of any differences in future rebroadcasting efforts, we present results where we pool over rebroadcasting intensity for clarity of exposition. According to our candidate survey, candidates believed that debates would be rebroadcast roughly two times, with no statistical difference by treatment assignment.

<sup>13</sup>USAID was the donor funding the debate initiative. We expected their branding to be persuasive since our candidate survey indicates that more than 43% of the candidates report having worked for an international NGO.

the debates.<sup>14</sup> Third, we sent SMS reminders to all candidates on either the evening before or the morning of the debate with reminders and information on where to go.

By emphasizing the credibility of the debates and providing information about the structure of the debates, the additional invitation efforts served to reduce uncertainty about the returns to debate participation. As a result, while the intervention was administered to all candidates in treated districts, as highlighted by our pre-analysis plan, we expected it to be more likely to affect the participation decision of leading candidates for the reasons discussed above. Among these candidates, we expected that the intervention would particularly affect the participation of those who were most uncertain about the return to debate participation.

### **3.3 Debate participation**

While the debates were well-attended overall, there was substantial variation across districts, ranging from 11% to 100%. Overall 59% of candidates participated, which broke down to 48% of incumbents compared to 60% of challengers and other candidates. As shown in Table III, the reasons cited for participation by our candidate survey respondents varied. Non-incumbent candidates cited their democratic duty more than any other reason, while for incumbents the most common reason was the opportunity to showcase their policy platforms to voters. All candidates viewed the debates as an opportunity to publicize their campaigns, but challengers put more emphasis than other candidates on the opportunity to demonstrate their competence. Only a small share of candidates pointed to radio broadcasting as a reason for their participation or admitted to attending in order to attack other candidates. These differences suggest a greater degree of political sophistication among incumbent candidates, who prioritized the instrumental value of the debates in publicizing their policy priorities.

Our candidate survey is also informative about the reasons why candidates were not present at their debate, although candidates predictably cite logistical issues rather than the electoral risks they faced in participation. Over 50% of non-participating candidates cited late notice, while 30% claimed that they did not receive an invitation. Nearly 20% mentioned road conditions to justify their absence.

Next, we analyze the debates themselves. Using transcript data we confirm that, on the whole, the unbiased rules of debate moderation were kept and candidates were given equal time to outline their policy priorities (see Table IV). Candidates varied in how they emphasized

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<sup>14</sup>Candidates were informed that questions would be asked about relevant local policy issues but were not provided specific questions.

### III. Reasons Cited For Debate Participation

Candidate type	Duty (1)	Policies (2)	Competence (3)	Publicity (4)	Radio (5)	Attack (6)
Incumbent	0.40	0.80	0.27	0.40	0.07	0.07
Challenger	0.61	0.48	0.37	0.35	0.02	0.07
Other	0.54	0.52	0.25	0.43	0.01	0.09

The table displays mean values of binary column variables among incumbents, challengers and other candidates based on our surveys. The candidates were allowed to cite more than one reason for debate participation. ‘Duty:’ cited democratic duty. ‘Policies:’ cited opportunity to present policy platform. ‘Competence:’ cited opportunity to show off competence. ‘Publicity:’ cited opportunity for free campaign publicity. ‘Radio:’ cited the benefits of radio broadcasting reaching a large audience. ‘Attack:’ cited opportunity to attack other candidates.

their own qualifications during introductions, with incumbents focusing on their experience while challengers and other candidates highlighted their educational achievements. The most commonly cited policy priorities related to district primary schools, health facilities, and the quality and extent of roads. However, incumbents spoke at greater length about both the County Social Development Fund (CSDF) and the Legislative Support Project (LSP) funds, reflecting their first-hand experience with these funds. Finally, incumbents were much more likely to both be attacked by other candidates and attack others, as their attendance seemed to act as a focal point for other debate participants.

### IV. Transcript Descriptive Statistics

Candidate type	Intro words (1)	Education emphasis (2)	Experience emphasis (3)	CSDF words (4)	LSP words (5)	Attacked (6)	Attacker (7)
Incumbent	340.3	0.22	0.33	398.2	224.0	0.19	0.15
Challenger	352.0	0.30	0.26	284.7	218.0	0.04	0.04
Other	345.9	0.27	0.19	269.8	203.7	0.03	0.03

Table displays mean values of column variables among incumbents, challengers and other candidates based on our surveys. ‘Intro words’: number of words spoken in introduction. ‘Education emphasis’: candidate highlighted their education in introduction. ‘Experience emphasis’: candidate highlighted their experience in introduction. ‘CSDF words’: number of words spoken about ways to improve management of County Social Development Funds. ‘LSP words’: number of words spoken about priorities for spending Legislative Support Projects funds. ‘Attacked’: candidate was verbally attacked by another candidate. ‘Attacker’: candidate verbally attacked another candidate.

Qualitative evidence from focus groups suggests that citizens were affected by candidate participation in the debates and found the presentation of concrete policy platforms to be novel. As one participant said, “Before the debate, the word ‘platform’ was a strange word to me” (Vai Town, 26 September 2017). Many commented that the debates increased information available about candidates, noting that “in the past, there was no opportunity created for voters to engage

candidates in understanding their platforms” (Foya, 20 September 2017). As a result, it is not surprising that citizens took note of participation decisions, highlighting that “we wanted to see all the six candidates at this debate but only two appeared, which is not good because we are not hearing from [the] other four candidates” (Massabolahun, 21 September 2017). Some even wanted debates to be mandatory: “There should be a law binding all candidates to attend the debate... You can’t be somebody who wants to represent me if you don’t turn up” (Vai Town, 26 September 2017).

Our qualitative evidence also suggests that the debates caused voters to change their assessment of the candidates. One participant stated, “The debate changed my attitude toward candidates and helped me discover the hidden secret of some candidates” (Kolahun, 18 September 2017). Similarly, another participant mentioned, “For me, when I reached there, the first person I wanted to vote for ... well, my mind did not go on him. When I entered inside the debate and heard them speak my mind started going on another candidate” (Klay, 27 September 2017). Several focus groups pointed to the lack of specificity in candidates’ promises and the mixed quality of policy platforms. As an example, one participant indicated that “some of the candidates were not detailed in their explanation on how they going to tackle these sectors” (Voinjama, 12 September). In particular, some respondents argued that the policy platforms of challengers were often weaker than those of the incumbents, as exemplified by one person’s comment that “I did not hear anything new from candidates contesting against the incumbent because the incumbent was already doing most of these things” (Kolahun, 18 September 2017).

## 4 Hypotheses

Our goal is to assess the electoral consequences of a shock to the supply and dissemination of policy promises by leading candidates. We evaluate the impact of our randomized intervention on candidate participation in the debates, citizen exposure to the debates, citizen beliefs about candidates’ competence and policy promises, citizen political engagement, candidate campaigning, and ultimate electoral outcomes. We refer to high-intensity invitation districts as treated districts.<sup>15</sup>

**H1** A larger higher share of candidates will attend the debate in treated districts

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<sup>15</sup>In our pre-registered analysis, we made hypotheses based on the intensity of the intervention as a function of both the invitation and rebroadcasting treatments, rather than referring to the effects of each intervention individually. As a result, our hypotheses are largely unchanged despite pooling the ineffective intensive rebroadcasting treatments. We document the limited divergences from our pre-analysis plan in Appendix A.3



than in control districts. This increase in participation will be larger for leading candidates than for other candidates.

The invitation intervention tries to induce candidate participation in the debates by ensuring candidates are fully informed about the debate format, allaying concerns about bias, reducing uncertainty, and persuading them to participate. Our first hypothesis, therefore, is that this added recruitment effort will lead to increased participation. We expect that a greater share of the candidates in treated districts will participate than in control districts, and that this effect is concentrated among the leading candidates, both incumbents and predicted challengers, in each race. As mentioned earlier, incumbents and leading challengers face higher opportunity costs and risks of participation, and we anticipate that the invitation intervention will have a larger effect on their participation decisions because we expect that more marginal candidates participate at high rates even absent the intervention.

**H2** Citizens will be more exposed to the debates in treated districts than in control districts.

We anticipate that more citizens in treated districts will be exposed to the debates than in control districts because the participation of a higher share of leading candidates should increase citizen interest in the debate. In principle this operates on both the extensive margin, increasing the probability that citizens will hear the debate, and the intensive margin, with citizens paying more attention to the content conditional on hearing it. This hypothesis, while important, is not obvious. Radio stations were contracted to rebroadcast the debates the same number of times across treatment and control districts, the campaigning season is frenetic, and our intervention represented a relatively small signal in a great deal of electoral noise. We nonetheless expect that the debates will attract citizen interest, especially when leading candidates attend.

**H3** Citizens will increase their political engagement in treated districts compared to control districts.

Due to the increased amount of candidate information conveyed to voters through the debate initiative in treated districts, we expect broad treatment effects on political engagement compared to control districts. In particular, we expect that greater exposure to relevant political information from leading candidates in treated districts will increase citizens' demand for even more political information, both through their social networks and from the media. We hypothesize that this increased access to information about leading candidates in treated districts will

lead to greater discussion of the debates with others and coordinated vote choices relative to control districts (Morris and Shin, 2002; Arias et al., 2019; Enríquez et al., 2019).

**H4** Citizens' evaluations of candidate policy promises and competence will be affected by debate exposure in treated districts.

We anticipate that increased exposure to the debates in which leading candidates participate, as well as subsequent political information acquisition, in treated districts will lead citizens to learn more about the participating candidates. With so many candidates competing, we expect that listening to a set of candidates answer standardized questions will offer an unusual opportunity to acquire and compare information on their policy positions and competence. This updating could result either from shifting citizens' beliefs about candidate competence and policy priorities or by reducing the uncertainty around them.

**H5** Citizens in treated districts will not experience more intensive on-the-ground campaigning by candidates in the run-up to the election.

In contrast to smaller-scale localized informational interventions, we hypothesize that the ability of candidates to increase their on-the-ground campaigning efforts in response to increased debate attendance by leading candidates will be limited. Since the debates are broadcast across the entire district, we consider it unlikely that candidates can spatially target ground campaigning effort towards areas where voters are more exposed to the debate. Further, since candidates are unaware of rebroadcasting plans, they have little time to respond to the rebroadcasts themselves.

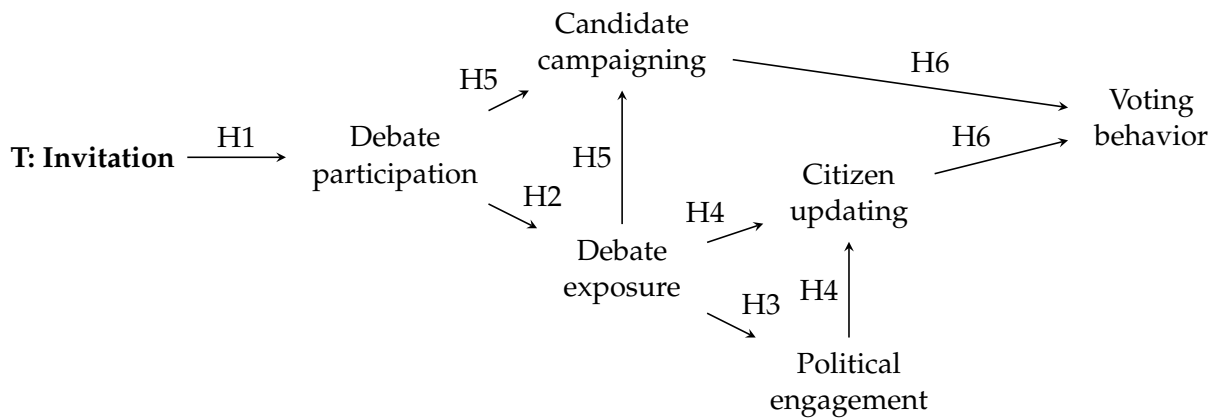
In fact, debates could crowd out on-the-ground campaigning by leading candidates persuaded to participate. By attending, candidates commit themselves—at some level—to campaigning on a more programmatic basis rather than focusing on the distribution of cash and promises at the village level. In addition, if debate participation benefits certain candidates their opponents might then rationally reduce campaigning effort.

**H6** Citizens in treated districts will be more likely to vote for candidates induced to participate in the debates who (a) perform well, and (b) are aligned with their policy preferences.

Ultimately, we anticipate that treatment assignment will lead to changes in citizens' vote choices. If citizens in treated districts are more exposed to debate content, and therefore to the policy

promises of the higher share of leading candidates participating, then we expect two results. First, citizens in treated districts will be more likely to vote for candidates who performed better in the relevant debate compared to those in control districts. Second, given the programmatic focus of the initiative, candidates who are better-aligned with the priorities of their electorate will accrue more votes in treated districts compared to control.

Figure 2. Hypothesized Effects of Intervention



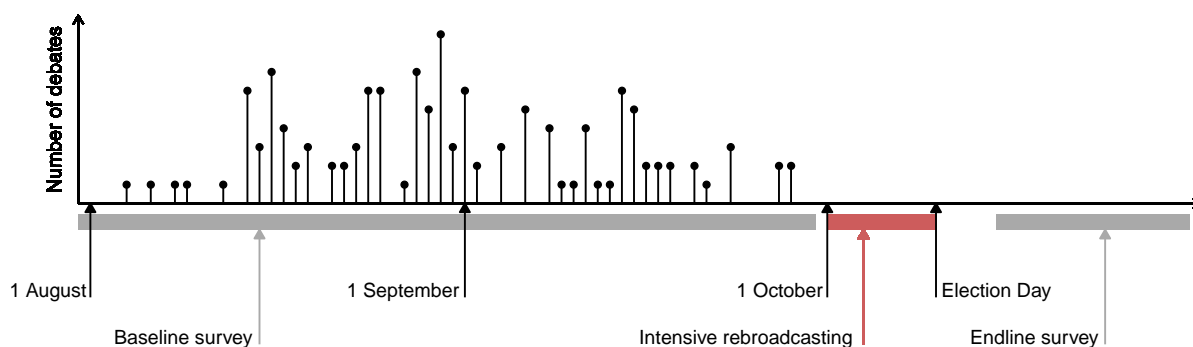
We had no prior expectations relating to whether patterns of self-selection into debate participation would differ by candidate type. In other words, we did not anticipate whether the extent of citizen-candidate policy priority overlap among those selecting into debate participation would correlate with incumbency status. Our analysis of such self-selection into debate participation, which we consider central for interpreting the overall effects of the intervention, therefore follows our presentation of the pre-registered hypotheses. We present the relationships among our pre-registered hypotheses in Figure 2.

## 5 Data

### 5.1 Data sources

Our primary data source is a panel survey of 4,060 registered voters conducted in all 73 electoral districts in the country. In these interviews, enumerators used tablet computers while making phone calls to respondents sampled from the universe of active cell phone numbers on the country’s largest mobile network. The distribution of observations per electoral district naturally reflects cell phone penetration and rurality. As the descriptive statistics shown in Table A1 indicate, the sample is older, more male, and better educated than the average Liberian.

Figure 3. Timeline of Debates Initiative



In Figure 3 we show a timeline of the debates and data collection. Our survey began in early August, right before the first debates. Most data collection was completed by early September but concluding the baseline survey in several electoral districts took several more weeks.<sup>16</sup> The overlap of the baseline survey and the live debates is not a major concern. First, we control for any potential baseline debate exposure using the date on which respondents were interviewed. Second, for variables that were collected only for the endline survey, the timing of the baseline survey is irrelevant. Lastly, the intensive rebroadcasting of debates took place beginning October 1, by which time 88% of baseline data had been collected.

We use several other data sources. First, we use polling station-level election results to assess effects of the intervention on administrative electoral outcomes. Second, we conducted a survey of more than 600 of the candidates who ran in the election. We employ this data to provide descriptive evidence on candidacy, validate important aspects of the intervention, and provide further evidence of the mechanism behind our results. Third, we use debate transcripts from each debate for descriptive evidence on the debates themselves.<sup>17</sup> Fourth, we conducted a survey of more than 50 radio stations to gather descriptive evidence about the stations and to validate their frequency of rebroadcasting the debates.

## 5.2 Outcome variables

To assess whether the invitation intervention, the debates themselves, and the debate rebroadcasting were properly implemented, we exploit two main pieces of data. For candidate debate

<sup>16</sup>Since the cellphone-number sample was stratified at the county-level, sampling in some districts proved difficult, especially when the county contained both urban and rural districts since, in these cases, most calls went to those in the urban districts, and so achieving a sufficient sample in the rural districts took longer than anticipated.

<sup>17</sup>Internews partnered with the *Daily Observer* newspaper and hired trained journalists to transcribe each debate.

participation, we use data from administrative debate reports as well as debate transcripts. For radio rebroadcasting, we use data from the rebroadcast schedules contracted with each of the radio stations and Naymote, a youth organization, which was hired to tune into each scheduled transmission to ensure debates were being played, unedited and on schedule. We complement this data with responses to our survey of radio stations to assess whether contracted and non-contracted stations also rebroadcast the debates or related content at other times.

To measure our key outcome variables, we rely on our voter survey and polling station-level data. For all outcome variables, we provide general descriptions in the relevant regression tables while details on their construction are in Appendix A.4. Whenever relevant, we aggregate related outcome variables using standardized z-scores as described by Kling et al. (2007). To measure debate exposure, we asked respondents whether they heard the debate as well as factual questions about the debate to confirm their exposure. We assess effects on political engagement using survey questions about individual demand for political information and coordination within their social networks. We measure turnout using administrative data. In Table A2 we provide descriptive statistics for all the raw respondent level outcome variables.

To assess individual beliefs about the candidates in their districts, we ask respondents about only three *predicted leading candidates*, as described in Section 2.2. This is both because asking about up to 28 candidates would have made the survey prohibitively time-consuming and because we had theoretical reasons to expect that the invitation intervention should affect the attendance decision of the most relevant candidates to a greater degree than it would affect other candidates. We ask individuals about their exposure to the campaign efforts of each of these predicted leading candidates as well as their beliefs about the candidates' competence and policy platforms. For all respondent-candidate dyads, we split the analysis into the incumbent and a pooling of the predicted challengers. Ultimately we are interested in respondent vote choices, which we validate using polling station-level data. In Table A3, we provide descriptive statistics for all the raw respondent-candidate level outcome variables.

Because we had no control over the local issues that would be discussed in the debates, all questions about policy priorities were open-ended. These include both the questions about the policy priorities of the voters as well as their perceptions about the priorities of the *predicted leading candidates*. To analyze these priorities, open survey responses were coded by independent coders with no knowledge of treatment assignment.

### 5.3 Interaction variables

As stated in H6, we expect that voting outcomes will be affected by two key variables: candidate performance in the debate and the extent of alignment between citizens' and candidates' policy priorities. In our survey analysis, we asked citizens about who they thought won their district debate. This performance measure, however, is naturally correlated with treatment assignment and so in the main analysis we construct a *predicted* candidate-level measure of debate performance,<sup>18</sup> and use this to predict performance for the full sample of candidates irrespective of actual participation.

We measure the extent of preference alignment between respondents and candidates using data from our baseline survey in which we asked respondents to name their top three policy priorities in their district as well as to name what they believe are the top three policy priorities for each of the three predicted leading candidates. We aggregate this latter measure across respondents to the district level to create a measure of each candidate's policy priorities. We then calculate the share of a respondent's top issues that are shared with each candidate to create a measure of preference alignment. We also create a district level version of preference alignment where we calculate the average of this variable at the district-level. However, because our sample is not representative of the district as a whole, we consider the district-level measure of preference alignment to be a noisy measure of the match between candidates' policy priorities and all voters in the district. We provide these results for completeness.

## 6 Estimation

### 6.1 Treatment assignment and balance

We randomly assigned all 73 districts into one of two treatment conditions: low invitation effort (the control group) or high invitation effort (the treatment group). To assign treatment conditions, we first pre-stratified based on which of the debate partners was running that district's debate. This is because the capacity of the debate organizers varied substantially in terms of their ability to attract candidates and organize the logistics of the debates. Second, we blocked on a set of pre-treatment covariates at the district-level to maximize power.<sup>19</sup> This strategy generated 19 blocks, with 3 or 4 districts per block. We then randomly assigned 38

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<sup>18</sup>We do this by flexibly estimating a LASSO model of the debate performance outcomes of participating candidates on pre-determined covariates including their gender, incumbency status, party, baseline citizen assessments of their competence, policy priorities, and certainty regarding both competence and policy priorities.

<sup>19</sup>We blocked on variables described in Panel A of Table A1.

districts to the treatment and 35 to control. Pre-treatment covariates at the district, individual, polling station, and candidate levels are well-balanced across treatment conditions. Full details are provided in Appendix A.2.

## 6.2 Estimating equations

Taking the case where the respondent-candidate is the unit of observation, we estimate:

$$y_{icdeb} = \beta T_{db} + \mathbf{X}_{icdb} + \mathbf{Z}_{db} + \theta_e + \eta_b + \epsilon_{icdeb}, \quad (1)$$

where  $y_{icdeb}$  is the outcome for respondent  $i$  regarding candidate  $c$  in district  $d$  interviewed by enumerator  $e$  in block  $b$ .<sup>20</sup>  $T_{db}$  is an indicator for districts assigned to treatment.  $\eta_b$  are randomization block fixed effects and  $\theta_e$  are survey enumerator fixed effects. Throughout, we include both district-level covariates  $\mathbf{Z}_{db}$  and individual-level covariates  $\mathbf{X}_{icdb}$  for which we provide descriptive statistics in Panels A and B of Table A1, respectively. While we have good balance on these covariates we include them to improve precision. We cluster standard errors at the district level, our level of treatment assignment, and report three pre-registered approaches to weighting. At the individual level, we report specifications that are unweighted, with observations weighted by the inverse of the number of respondents in that district-wave (1/Obs), and by the number of registered voters in that district divided by the number of respondents in that district-wave (Reg/Obs). The weighted specifications help account for variation in the number of survey respondents by electoral district.

We consider an analogous specification to that of Equation (1) for electoral outcomes available at the polling station-level. In this case, instead of individual-level controls, we control for polling station-level variables listed in Panel C of Table A1. At the polling station-level, we report specifications that are unweighted, with observations weighted by the inverse of the number of polling stations in that district (1/PS), and by the number of registered voters at that polling station (Reg). The weighted specifications help account for variation in the number of polling stations by electoral district.

Whenever we have a panel for a given question where the outcome is continuous, we consider the continuous change in that variable between baseline and endline as an outcome  $\Delta y_{icdeb}$ . When the outcome is binary, we construct an indicator for whether the coded response changed between waves. The estimating equation remains the same aside from controlling

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<sup>20</sup>This estimation approach extends to cases where the respondent is the unit of observation,  $y_{ideb}$ , and where the candidate is the unit of observation,  $y_{cdb}$ .

for whether respondents were interviewed at baseline before or after the first broadcast of their district debate and its interaction with treatment assignment. Lastly, we also make use of specifications where we interact treatment assignment with candidate-level covariates  $X_{cdb}$ , which applies to the interactions discussed in Section 5.3.

Our coefficient of interest in Equation (1) is  $\beta$ , which recovers the reduced-form or intent-to-treat treatment effect of the invitation intervention. While the treatment effect of debate participation, or the instrumental variable estimate that we would recover by instrumenting debate participation with assignment to treatment, is of great theoretical interest, some of our estimates indicate that the exclusion restriction condition that is required for the IV estimate to be unbiased is unlikely to hold. Lastly, while reduced-form estimates recover the effects of the intervention for complying candidates, in Section 8 we argue that, due to the nature of selection into debate participation, these estimates speak to the effect of the debate initiative more generally.

## 7 Results

Our results suggest rich and consistent consequences of the debate initiative, and in particular the invitation intervention, on political outcomes. We show that the intervention was successful as candidates, particularly leading candidates, were more likely to attend their debates in treated districts (H1). We then consider the consequences of this increase in participation in five steps. First, citizens were more exposed to the debates (H2) and their engagement with the election increased (H3). Second, citizens updated more positively about incumbents' priorities and competence than about their challengers' (H4). Third, challengers decreased their on-the-ground campaigning efforts, while incumbents increased their radio campaigning (H5). Fourth, citizens voted more often for their incumbent, especially when the incumbent performed well or matched baseline voter policy preferences (H6). Fifth, in Section 8, we establish that this updating and electoral consequences are explained by variation in compliance by candidates: differences in political sophistication led incumbents, but not their challengers, to positively select into participation based on the congruence of their policy priorities with those of their constituents.



## V. Candidate Debate Participation

	(1)	(2)	(3)
<b>A. Share of candidates</b>			
<i>Invite</i>	0.077 (0.034)	0.065 (0.030)	0.092 (0.033)
Control Mean	0.542	0.573	0.557
Observations	4060	4060	4060
Weight	No	1/Obs	Reg/Obs
<b>B. Incumbent</b>			
<i>Invite</i>	0.212 (0.083)	0.177 (0.073)	0.234 (0.083)
Control Mean	0.280	0.372	0.299
Observations	4060	4060	4060
Weight	No	1/Obs	Reg/Obs
<b>C. Share of challengers</b>			
<i>Invite</i>	0.212 (0.074)	0.144 (0.063)	0.220 (0.067)
Control Mean	0.492	0.554	0.528
Observations	4060	4060	4060
Weight	No	1/Obs	Reg/Obs
<b>D. Share of other candidates</b>			
<i>Invite</i>	0.003 (0.030)	0.008 (0.028)	0.009 (0.029)
Control Mean	0.562	0.583	0.575
Observations	3991	3991	3991
Weight	No	1/Obs	Reg/Obs

Outcome variables are the share of the respective set of candidates (all, incumbent, predicted challenger, other candidate) who attended a debate out of all candidates in that district. Panels A-C have 4060 observations; Panel D has fewer due to only three candidates running in two districts (and hence no 'other candidates' defined).

All specifications are estimated using OLS and include block FE, enumerator FE, district-level and individual-level controls. For weighted specifications, 'Obs' is the number of observations in that district and 'Reg' is the number of registered voters in that district. Standard errors clustered at the district-level in parentheses.

## 7.1 Intervention implementation

We start by testing H1 to assess whether the intervention had its intended effect. Table V reports treatment effects on the attendance of candidates at the debates.<sup>21</sup> Column 1 in Panel A suggests that the invitation intervention led to a 7.7 percentage point (pp) (14% relative to the control mean) increase in the share of total candidates attending the debates in treated districts. In Panel B we show that incumbents were 21.2 pp (76%) more likely to attend in treated districts, and in Panel C we show that predicted challenger candidates were 21.2 pp (43%) more likely to attend. Lastly, Panel D reports no treatment effect on other candidates, reinforcing our expectation that the invitation intervention would mainly affect the debate participation decision of leading candidates.<sup>22</sup>

## 7.2 Exposure to debates

We next report the effects of treatment assignment on our measures of citizen exposure to the debates (H2), which hypothesizes that citizens will be more interested in the debates when leading candidates attend. In Panel A of Table VI, we use a standardized index of our measures of direct exposure to the debates (whether respondents heard the debate and how often they heard them). The results indicate that citizens in treated districts had exposure 0.304 standard deviations (sd) higher than those in control districts.<sup>23</sup>

In Panel B, we use an index reflecting factual knowledge about the debates themselves. The results suggest that citizens in treated districts had factual knowledge of the debates 0.125 sd higher than citizens in control districts. Given that our endline survey began around a month after the election, the persistence of this effect suggests meaningful differences in debate exposure. In Panel C we use an index reflecting factual knowledge about a national policy issue, the management of County Social Development Funds, which was asked in every debate. We find treatment effects of around 0.156 sd on correctly learning about management issues of these poorly-understood funds. This suggests that the debates were relatively effective at conveying policy information, particularly when leading candidates participated. We provide results

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<sup>21</sup>In Appendix Table A13 we show that there is no difference in how often the debates were rebroadcast. This is measured either based on the radio monitors who tuned into each contracted rebroadcast, or based on our radio station survey that included radio stations not contracted to rebroadcast but recorded as present in a district debate.

<sup>22</sup>In Appendix Table A12, we further show that whether the eventual election winner attended and the share of actual challengers who attended was substantially affected by treatment assignment, as expected.

<sup>23</sup>Importantly for our ability to pool districts assigned to different rebroadcasting intensity, as we show in Appendix A.3, there were no differential treatment effects on citizen exposure to the debates when pooling districts that were cross-randomized to different rebroadcasting intensities or when restricting to districts assigned to high invitation intensity.

## VI. Debate Exposure

	(1)	(2)	(3)
<b>A. Debate listening index</b>			
<i>Invite</i>	0.304 (0.102)	0.341 (0.105)	0.424 (0.107)
Observations	4060	4060	4060
Weight	No	1/Obs	Reg/Obs
<b>B. Debate knowledge index</b>			
<i>Invite</i>	0.125 (0.063)	0.126 (0.058)	0.165 (0.059)
Observations	4060	4060	4060
Weight	No	1/Obs	Reg/Obs
<b>C. Policy knowledge index</b>			
<i>Invite</i>	0.156 (0.089)	0.230 (0.121)	0.189 (0.100)
Observations	4060	4060	4060
Weight	No	1/Obs	Reg/Obs

Outcome variables: Panel A: a standardized index of (1) indicator for whether the respondent had not heard their district debate at baseline but had at endline (2) indicator for whether the respondent had heard the debate at endline (3) the number of times the respondent had heard the debate at endline. Panel B: a standardized index of (1) indicator for whether the respondent's stated debate winner actually attended the debate (2) share of candidates respondent claims participated (3) share of predicted leading candidates respondent claims participated. Panel C: a standardized index of the change in how many factual questions about CSDF management respondents answered correctly between baseline and endline.

All specifications are estimated using OLS and include block FE, enumerator FE, district-level and individual-level controls. For weighted specifications, 'Obs' is the number of observations in that district and 'Reg' is the number of registered voters in that district. Standard errors clustered at the district-level in parentheses.

disaggregating the components of these indices in Table A14. These results provide strong support for the idea that the invitation intervention meaningfully affected citizen exposure to the debate content because more leading candidates attended.<sup>24</sup>

### **7.3 Effects on political engagement**

The qualitative evidence we presented in Section 3.3 suggests that the debates had a catalyzing effect on political engagement. In this section we quantitatively test whether, by increasing the attendance of leading candidates, the invitation intervention affected the political engagement of citizens and ultimately their turnout (H3). Panel A of Table VII reports a 0.251 sd increase in treated districts on a standardized information demand index capturing how much political information citizens demanded just before the election by listening to the radio, discussing politics with friends, and seeking other forms of political information.

In line with these results, in Panel B there are strong treatment effects on a standardized coordination index capturing whether citizens discussed the debates with others and whether they believed these discussions led them to coordinate their vote choices. Specifically, citizens exhibit a 0.153 sd increase in coordination in treated districts. Both panels then provide strong evidence that, when exposed to debates in which relevant leading candidates participated, citizens demanded more political information and coordinated their vote choices. We provide results decomposing these indices in Table A15.

Next, we provide evidence that the citizen engagement caused by the invitation intervention ultimately led to higher turnout. In Panel C of Table VII, we use administrative polling station data on turnout. Turnout in the 2017 House of Representatives election was on average 1.5 pp higher at polling stations in treated districts than the control mean of 70.1 percent (2%). Overall, the results provide strong evidence that greater candidate participation in the debates increased political engagement of citizens, which in turn generated actual electoral mobilization.

### **7.4 Effects on beliefs about candidates**

We next analyze the effect of the intervention on beliefs about the competence and policy priorities of predicted leading candidates (H4). We first assess treatment effects on the standardized change in citizens' reported certainty about the competence (columns 1-3) and priority issues (columns 4-6) of incumbents, shown in Panel 1.A. of Table VIII. The results indicate that citizens

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<sup>24</sup>This is also consistent with the theory that more deliberation between relevant candidates will increase voters' attention (Wantchekon et al., 2018).

## VII. Political Engagement

	(1)	(2)	(3)
<b>A. Political information demand index</b>			
<i>Invite</i>	0.251 (0.078)	0.313 (0.090)	0.300 (0.091)
Observations	4060	4060	4060
Weight	No	1/Obs	Reg/Obs
<b>B. Debate coordination index</b>			
<i>Invite</i>	0.153 (0.065)	0.158 (0.061)	0.193 (0.061)
Observations	4060	4060	4060
Weight	No	1/Obs	Reg/Obs
<b>C. Polling station-level turnout</b>			
<i>Invite</i>	0.015 (0.005)	0.012 (0.005)	0.015 (0.005)
Control Mean	0.701	0.699	0.699
Observations	5386	5386	5386
Weight	No	1/PS	Reg

Outcome variables: Panel A: standardized index of variables measuring (1) change in how much respondents listened to the radio (2) change in how much they discussed politics with their friends (3) how much they accessed other sources of political information. Panel B: index of variables measuring (1) how much respondents discussed the debate with friends (2) how much this discussion led to coordinating their vote choices. Panel C: polling station-level turnout in House of Representatives election.

Specifications using citizen survey outcomes are estimated using OLS and include block FE, enumerator FE, district-level and individual-level controls. For weighted specifications, 'Obs' is the number of observations in that district and 'Reg' is the number of registered voters in that district. Specifications using polling station-level data are estimated using OLS and include block FE, district-level and polling station-level controls. For weighted specifications, 'PS' is the number of polling stations in that district and 'Reg' is the number of registered voters at that polling station. Standard errors clustered at the district-level in parentheses.

in treated districts became significantly more certain about incumbent competence and priority issues, with standardized treatment effect sizes of 0.178 and 0.169 sd, respectively. In Panel 1.B. there is no evidence that citizens in treated districts became more certain about the competence of challengers, and mixed evidence that those citizens became more certain about challengers' priority issues.

We then assess treatment effects on the standardized change in citizens' beliefs about the competence (columns 1-3) and priority issues (columns 4-6) of predicted leading candidates, shown in Panel 2. These treatment effects suggest positive updating regarding incumbents and negative updating for their challengers, but the estimates are imprecise. Panel 2.A. reports suggestive sizable, but statistically insignificant at conventional levels, treatment effects that citizens feel more positive about incumbent competence and that they have learned more about their policy priorities. Specifically, for both outcomes, the treatment effect is around 0.1 sd. In contrast, Panel 2.B. indicates that citizens updated negatively about the competence of their challengers and they did not learn about the policy priorities of those candidates. The treatment effect on challenger competence is 0.1 sd and marginally significant ( $p < 0.1$ ).

The results provide evidence that aligns with the focus group evidence presented in Section 3.3. Inducing the participation of leading candidates increased listeners' certainty about the competence and priority issues of those candidates. Indeed, as we show in Section 8, the candidates induced to participate by the intervention were those about whom citizens were initially most uncertain. This increase in certainty mainly for the incumbent, who spoke significantly more on the policy issue questions for which they possessed more experience. Similarly, the suggestive positive treatment effects on citizens' perceptions about incumbent competence but negative perceptions about challenger competence indicate that incumbents were likely to outperform challengers when they were induced to participate. Overall the evidence supports the idea that incumbents but not their challengers benefited both in terms of increased voter information about their policy priorities and somewhat in terms of perceptions of their competence.

## **7.5 Candidate response and campaigning**

Next, we consider treatment effects on candidates' campaign efforts (H5). In Table IX, we report results on standardized indices of survey responses regarding "on-the-ground" campaigning by candidates in respondents' towns (Columns 1-3) and "radio" campaigning (4-6). The on-the-ground campaigning index incorporates candidates' visits, distribution of leaflets, and vote

VIII. Updating About Candidates

1. Uncertainty	Certainty about competence			Certainty about policy		
	(1)	(2)	(3)	(4)	(5)	(6)
<b>A. Incumbent</b>						
<i>Invite</i>	0.178 (0.105)	0.186 (0.107)	0.179 (0.084)	0.169 (0.074)	0.195 (0.080)	0.192 (0.074)
Observations	3496	3496	3496	3496	3496	3496
Weight	No	1/Obs	Reg/Obs	No	1/Obs	Reg/Obs
<b>B. Challengers</b>						
<i>Invite</i>	0.037 (0.066)	0.046 (0.074)	0.025 (0.070)	0.139 (0.061)	0.118 (0.073)	0.098 (0.067)
Observations	8684	8684	8684	8684	8684	8684
Weight	No	1/Obs	Reg/Obs	No	1/Obs	Reg/Obs
2. Levels	Beliefs about competence			Learning about policy		
	(1)	(2)	(3)	(4)	(5)	(6)
<b>A. Incumbent</b>						
<i>Invite</i>	0.098 (0.075)	0.066 (0.089)	0.093 (0.082)	0.089 (0.065)	0.125 (0.089)	0.091 (0.073)
Observations	3496	3496	3496	3496	3496	3496
Weight	No	1/Obs	Reg/Obs	No	1/Obs	Reg/Obs
<b>B. Challengers</b>						
<i>Invite</i>	-0.078 (0.075)	-0.147 (0.088)	-0.096 (0.077)	0.038 (0.065)	0.027 (0.086)	0.063 (0.080)
Observations	8684	8684	8684	8684	8684	8684
Weight	No	1/Obs	Reg/Obs	No	1/Obs	Reg/Obs

Panels 1.A and 1.B: the outcome variable in columns 1-3 is the standardized change in certainty respondents express about candidate competence between baseline and endline, and in columns 4-6 is the standardized change in certainty respondents express about candidate priority issues between baseline and endline. Panels 2.A and 2.B: the outcome variable in columns 1-3 is the standardized change in respondent perception about candidate competence between baseline and endline, and in columns 4-6 is the standardized learning that respondents reflect about candidate priority issues between baseline and endline.

All specifications are estimated using OLS and include block FE, enumerator FE, district-level and individual-level controls. For weighted specifications, 'Obs' is the number of observations in that district and 'Reg' is the number of registered voters in that district. Standard errors clustered at the district-level in parentheses.

buying in respondents' towns. The radio campaigning index captures candidate presence on the radio.

### IX. Candidate Campaigning

	Ground			Radio		
	(1)	(2)	(3)	(4)	(5)	(6)
<b>A. Incumbent</b>						
<i>Invite</i>	-0.055 (0.043)	-0.075 (0.050)	-0.052 (0.051)	0.082 (0.037)	0.087 (0.040)	0.092 (0.042)
Observations	3492	3492	3492	3496	3496	3496
Weight	No	1/Obs	Reg/Obs	No	1/Obs	Reg/Obs
<b>B. Challengers</b>						
<i>Invite</i>	-0.060 (0.031)	-0.073 (0.029)	-0.076 (0.032)	-0.025 (0.028)	-0.004 (0.027)	-0.018 (0.028)
Observations	8676	8676	8676	8684	8684	8684
Weight	No	1/Obs	Reg/Obs	No	1/Obs	Reg/Obs

Outcome variable in columns 1-3 is a standardized index of how often candidates (1) visited (2) distributed leaflets (3) bought votes in respondents' communities during campaigning. Outcome variable in columns 4-6 is a standardized measure of how often respondents heard candidates on the radio in the two weeks before the election.

All specifications are estimated using OLS and include block FE, enumerator FE, district-level and individual-level controls. For weighted specifications, 'Obs' is the number of observations in that district and 'Reg' is the number of registered voters in that district. Standard errors clustered at the district-level in parentheses.

In Panel A, there is a significant increase in incumbent exposure on the radio in treated districts, but no significant treatment effect on on-the-ground campaigning by incumbents. Specifically, there is a positive treatment effect of .082 sd in incumbent radio campaigning. In contrast, Panel B reports evidence of negative treatment effects on on-the-ground campaigning by challengers, but no treatment effect on challenger radio exposure. Challengers reduced their on-the-ground campaigning by .06 sd in treated districts. We provide results disaggregating the on-the-ground campaigning index in Table A16.

This pattern of results suggests that the presence of incumbents in the debates deterred challengers, who spent less time campaigning in person around their district in treated districts than did challengers in control districts. Moreover, incumbent debate performance led incumbents to increase their use of radio campaigning to complement the rebroadcasting of the debates. Suggestively, in our candidate survey, 77% of incumbents believed that the debates led radio stations to issue invitations for interviews more frequently, while only 63% of challengers believed they received more invitations. In addition, 68% of incumbents report that the debates



led them to change their campaign strategy whereas only 52% of challengers reported a change. Restricting the data to candidates who said the debates affected their mode of campaigning, 60% of incumbents said they affected their radio appearances while only 40% said they affected their on-the-ground campaigning. By contrast, only 27% of challengers said the debates affected their radio appearances compared to 73% who said they affected their on-the-ground campaigning.

## 7.6 Voting Behavior

Our results indicate that the intervention was most beneficial for incumbents induced to participate. While both incumbent and challenger candidates were more likely to attend the debates in treated districts, politically motivated citizens learned more and felt more positively about incumbent competence and policy priorities than those of their challengers. In turn, incumbents increased their campaigning efforts over the radio, while challengers reduced theirs on the ground. Here, we provide evidence that these changes ultimately affected voting behavior.<sup>25</sup>

Table X presents treatment effects on vote choice (H6), using both our citizen survey (Panel 1) and polling station-level electoral returns (Panel 2). Panel 1 presents a set of specifications testing for whether there were treatment effects on vote choice, defined as whether the respondent reported voting for a given candidate at endline.<sup>26</sup> Columns 1-3 present the main effects of the treatment assignment. Columns 4-6 present specifications in which we interact treatment assignment with the standardized measure of predicted debate performance described in Section 5.3. In columns 7-9, we interact treatment assignment with the standardized measure of preference alignment between the citizen and the candidate described in the same section.

In Panel 1.A., focusing on the incumbent, we find strong positive treatment effects on vote choice. Incumbents experienced a 4.5 pp increase in respondent vote choice in treated districts. Moreover, the significantly positive interactions with both debate performance and policy priority alignment indicate that this effect is greater among incumbents who were predicted to perform well in the debates and whose policy priorities align with those of their electorates. In contrast, focusing on predicted challengers in Panel 1.B., there are broadly negative main effects and little evidence of significant interactions. Challengers experienced a significant 4.8 pp drop among respondents' vote choice in treated districts.

<sup>25</sup>Appendix Table A17 reports that citizens in treated districts show no change in the reasons they cited for their vote choice in the baseline versus endline surveys, which thus cannot account for our results on voting.

<sup>26</sup>We pre-registered using an outcome variable defined as vote switching towards a given candidate. However, since few respondents indicated a concrete vote choice at baseline, using either measure produces qualitatively identical results. Focusing on endline responses allows us to compare voter responses to polling station outcomes.

## X. Voting Outcomes

	Main effect			Std. Performance			Interaction term: Std. Policy match		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<b>1. Respondent-level</b>									
<b>1.A. Incumbent</b>									
<i>Invite</i>	0.045	0.051	0.035	0.049	0.054	0.039	0.041	0.045	0.032
	(0.020)	(0.023)	(0.020)	(0.020)	(0.023)	(0.020)	(0.019)	(0.023)	(0.019)
<i>Invite</i> × $\widehat{\text{Std. performance}}$				0.067	0.059	0.039			
				(0.038)	(0.036)	(0.031)			
<i>Invite</i> × Std. policy match							0.042	0.051	0.052
							(0.016)	(0.017)	(0.018)
Control Mean	0.278	0.282	0.288	0.278	0.282	0.288	0.278	0.282	0.288
Observations	3496	3496	3496	3496	3496	3496	3496	3496	3496
<b>1.B. Challengers</b>									
<i>Invite</i>	-0.048	-0.039	-0.036	-0.051	-0.039	-0.039	-0.048	-0.039	-0.036
	(0.014)	(0.013)	(0.014)	(0.017)	(0.015)	(0.016)	(0.014)	(0.013)	(0.014)
<i>Invite</i> × $\widehat{\text{Std. performance}}$				-0.015	-0.002	-0.026			
				(0.046)	(0.039)	(0.043)			
<i>Invite</i> × Std. policy match							-0.006	-0.004	-0.003
							(0.007)	(0.008)	(0.008)
Control Mean	0.156	0.156	0.156	0.156	0.156	0.156	0.156	0.156	0.156
Observations	8684	8684	8684	8684	8684	8684	8684	8684	8684
Weight	No	1/Obs	Reg/Obs	No	1/Obs	Reg/Obs	No	1/Obs	Reg/Obs
<b>2. Polling station-level</b>									
<b>2.A. Incumbent</b>									
<i>Invite</i>	0.042	0.037	0.041	0.045	0.036	0.044	0.045	0.041	0.043
	(0.024)	(0.023)	(0.024)	(0.023)	(0.021)	(0.023)	(0.023)	(0.022)	(0.023)
<i>Invite</i> × $\widehat{\text{Std. performance}}$				0.082	0.099	0.081			
				(0.027)	(0.030)	(0.026)			
<i>Invite</i> × Std. policy match							-0.016	-0.017	-0.017
							(0.028)	(0.029)	(0.029)
Control Mean	0.246	0.250	0.250	0.246	0.250	0.250	0.246	0.250	0.250
Observations	4618	4618	4618	4618	4618	4618	4618	4618	4618
<b>2.B. Challengers</b>									
<i>Invite</i>	-0.029	-0.031	-0.028	-0.033	-0.036	-0.031	-0.029	-0.029	-0.029
	(0.012)	(0.012)	(0.012)	(0.013)	(0.012)	(0.013)	(0.012)	(0.013)	(0.012)
<i>Invite</i> × $\widehat{\text{Std. performance}}$				-0.034	-0.031	-0.037			
				(0.027)	(0.029)	(0.026)			
<i>Invite</i> × Std. policy match							-0.000	-0.009	0.005
							(0.019)	(0.017)	(0.019)
Control Mean	0.113	0.112	0.112	0.113	0.112	0.112	0.113	0.112	0.112
Observations	11385	11385	11385	11385	11385	11385	11385	11385	11385
Weight	No	1/PS	Reg	No	1/PS	Reg	No	1/PS	Reg

Panels 1.A. and 1.B: Outcome variable is an indicator for whether respondent expressed voting for either the incumbent (Panel A) or a predicted challenger (Panel B) at endline. Columns 1-3 report the main effects, Columns 4-6 include interactions of treatment assignment with standardized candidate-level measures of predicted debate performance, and Columns 7-9 include interactions with standardized respondent-candidate-level measures of preference alignment measured at baseline. Panels 2.A. and 2.B: Outcome variables are vote shares over the number of registered voters for the incumbent (Panel A) or predicted challengers (Panel B) using polling station-level electoral data. We use the district-level analogues of the interaction terms used in Panel 1. Section 5.3 explains these interaction terms further.

Specifications using citizen survey outcomes are estimated using OLS and include block FE, enumerator FE, district-level and individual-level controls. For weighted specifications, 'Obs' is the number of observations in that district and 'Reg' is the number of registered voters in that district. Specifications using polling station-level data are estimated using OLS and include block FE, district-level and polling station-level controls. For weighted specifications, 'PS' is the number of polling stations in that district and 'Reg' is the number of registered voters at that polling station. Standard errors clustered at the district-level in parentheses.

In Panel 2 of Table X, we use polling station-level data to augment our survey results. These results reinforce our findings. In Panel 2.A., we find that incumbent vote share in treated districts was 4.2 pp higher in treated districts than in control districts, albeit somewhat more noisily estimated. Incumbent vote share is larger in districts where their predicted debate performance was higher. However, we find no significant interaction with our district measure of policy priority overlap here, which we attribute to the fact that our sample of district respondents might be a noisy measure of the true district policy priority overlap. In Panel 2.B., we continue to find that challenger vote share in treated districts is 2.9 pp smaller than in control districts, and there is no evidence of interactions with predicted debate performance or overlap in policy priorities between challengers and citizens in their districts. The polling station results broadly support the survey results and suggest positive electoral consequences for incumbents in treated districts but negative consequences for their challengers.

## 8 Candidate selection into participation

Our results suggest that electoral gains accrued to incumbent candidates in treated districts, particularly when they performed well in their debates and showed that their policy priorities matched those of their constituents. Importantly, the results from our survey of vote choice and polling station outcomes mirror actual election outcomes: 50% of incumbents in treated districts won re-election compared to 43% in control districts.<sup>27</sup> In a context where approval of incumbent performance is generally low, as discussed in Section 2.1, these results might seem surprising. Understanding why this happened demands an analysis of compliance with the invitation treatment and hence candidate selection into the debates themselves. Few incumbents attended in control districts—just 35%—whereas the additional invitation effort increased this number to around 50% in treated districts.

We show that these complier incumbents self-selected into debate participation, particularly in terms of the degree of their policy alignment with citizens in their districts, more positively than challenger candidates did. We focus on standardized candidate-level measures of (1) baseline policy alignment with citizens in the district; (2) baseline citizen uncertainty about candidate policy priorities; (3) baseline citizen assessments of candidate competence; (4) baseline citizen uncertainty about candidate competence. Following Abadie (2003) and Angrist and Fernandez-Val (2013), we first compute the average of those variables for the various compliance

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<sup>27</sup>This difference is not surprising given that more than 35% of races are decided by winning margins of less than five percentage points, which approximates the treatment effects on voting outcomes shown in Table X.

groups: compliers (candidates induced to participate in the debates due to the invitation intervention), always-takers (candidates who would have participated absent the intervention), and never-takers (candidates who would not have participated even with the intervention).

In Panel A of Table XI we provide this descriptive evidence for incumbent candidates. We find that always-taker incumbents were best aligned with the policy priorities of citizens in their districts. Complier incumbents were aligned better than never-taker incumbents, but worse than always-takers. Importantly, however, and consistent with the idea that debate intervention allayed concerns about the risk of debate attendance, there was much less certainty about the policy priorities of complier incumbents than either always-taker or never-taker incumbents. We find a less stark difference in terms of self-selection on competence across groups, but again we find that there was less certainty about the competence of the compliers at baseline.

Focusing on challenger candidates in Panel B, we find a much less clear pattern of self-selection into the debates. While always-takers do not seem to self-select relative to never-takers based on their policy alignment with citizens, compliers *negatively* do. Similarly, while always-takers positively self-selected in terms of competence, the positive self-selection of compliers is substantially greater. As with incumbents, there was much less certainty about the policy priorities and competence of complying challengers than either always-taker or never-taker challengers.

#### XI. Characterizing Compliers

Variable	Compliers (1)	Always-takers (2)	Never-takers (3)
<b>A. Incumbents</b>			
Policy match	-0.22	0.60	-0.47
Policy certainty	-1.00	0.14	0.06
Competence	-0.04	0.08	-0.07
Competence certainty	-1.10	0.29	-0.01
<b>B. Challengers</b>			
Policy match	-1.38	0.17	0.14
Policy certainty	-1.19	0.12	0.10
Competence	0.45	0.06	-0.18
Competence certainty	-0.50	0.19	-0.19

We standardize each variable within the relevant candidate type. Estimates are the kappa-weighted means for various compliance groups by incumbency status: compliers, always-takers and never-takers, following Abadie (2003) and Angrist and Fernandez-Val (2013).

We provide additional graphical evidence regarding the nature of candidate selection into the debates using non-parametric regressions to examine the probability of a given candidate

being an always-taker or complier across different values of the baseline characteristics we focus on. Figure 4 presents these results in a light shade for always-takers and in a dark shade for compliers. The plots in the top panel again suggest strongly positive self-selection among always-taker incumbents based on their policy alignment with constituents. Among candidates in districts assigned to the control, incumbents with policy alignment 1 sd above the mean participated at rates of around 75%. Those with policy alignment 1 sd below mean participated less than 20% of the time. With respect to complying incumbents, the plots suggest that the invitation intervention induced the participation of incumbents at intermediate levels of policy alignment, which is natural given that those incumbents with the highest levels of policy alignment were likely always-takers. In turn, we continue to observe no self-selection based on perceptions of competence. With respect to certainty about both candidate policies and competence, again we corroborate a strong positive self-selection among always-taker incumbents in that those candidates were relatively well-known to citizens along both dimensions.

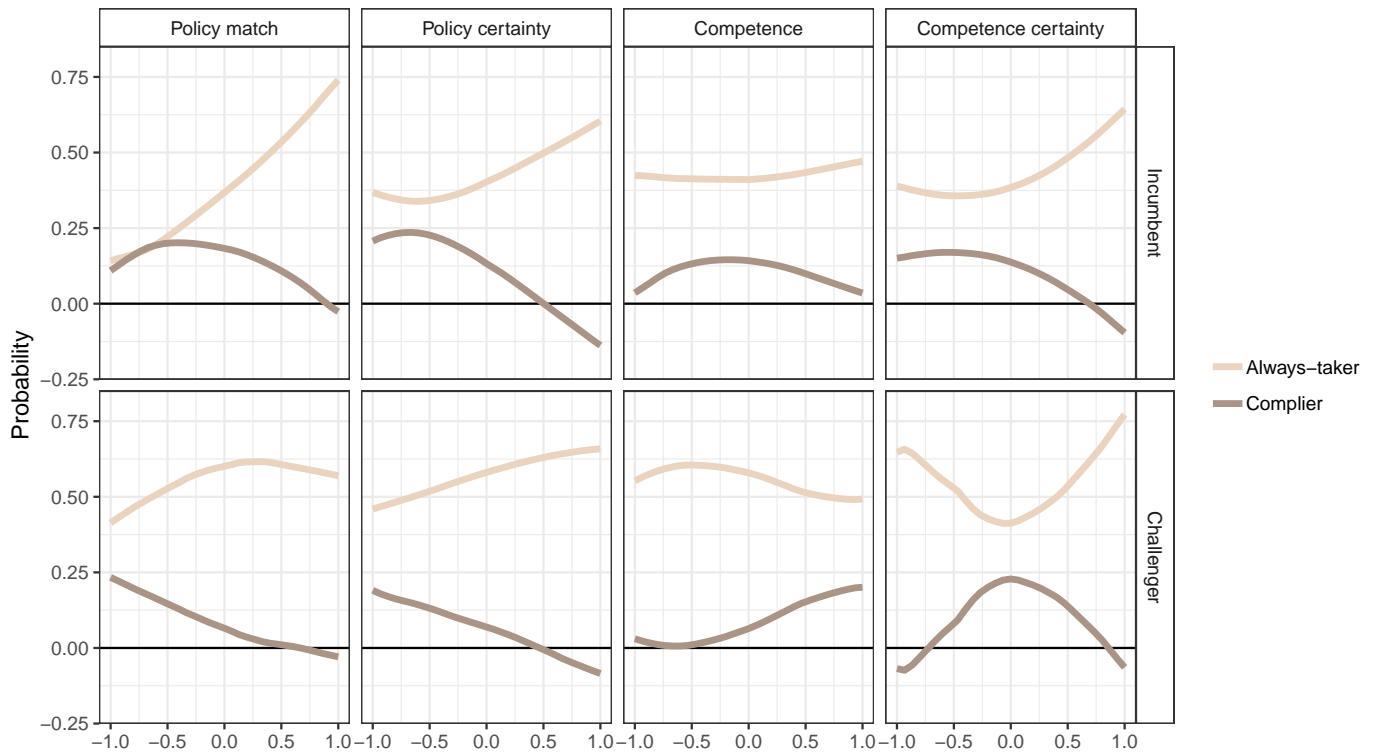
The plots in the bottom panel, in turn, confirm a substantially weaker self-selection of always-taker challengers on both candidate policy alignment and competence. With respect to complying challengers, in contrast with complying incumbents, we see a negative self-selection on policy alignment and a positive selection on competence. However, as with complying incumbents, we observe that there is substantially less certainty about both their policy priorities and competence.

Overall, the evidence indicates that incumbent candidates were much better at self-selecting into debate participation than challengers; they seemed better able to recognize when their policy priorities aligned with those of their constituents and when constituents were uncertain of this alignment. This effect is clear for both always-takers and compliers, which suggests that the treatment effects of the invitation intervention are likely representative of those of the overall debate initiative. In turn, across candidate types, selection on perceptions of overall competence was a lot weaker. Given the specific focus of the debates on policy issues, it appears that incumbents were better equipped to correctly assess the returns to debate participation.<sup>28</sup> Challenger candidates, who frequently had less political sophistication and experience, were less successful at calculating the returns to participation.

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<sup>28</sup>It is worth considering whether this sophistication is a product of prior selection or experience accrued over incumbency. While underpowered to conduct a regression discontinuity design to account for selection into incumbency, this exercise provides suggestive evidence that it is experience, rather than selection into incumbency, that accounts for our observed results.

Figure 4. Complier Status by Baseline Candidate Characteristics



Note: Plot presents fitted values from a non-parametric regression of compliance status across standardized values of four baseline characteristics for incumbents (top row) and challengers (bottom row). At a given value of each baseline characteristic, the fitted value indicates the probability of a candidate type being an always-taker or a complier. Optimal bandwidths computed using Calonico et al. (2018).

## 9 Conclusion

We evaluate an intervention intended to vary the participation of candidates in a nationwide debate initiative that was designed to elicit and disseminate programmatic promises from candidates running for a seat in Liberia's House of Representatives in 2017. Policy promises were elicited from candidates who participated in debates and disseminated via radio rebroadcasting. Incumbents who were induced to attend their debates by their intervention benefitted at the ballot box. These incumbents self-selected into the debates when their policy priorities were aligned with their constituents, and thus they dominated their debates by proposing policy platforms closer to those preferred by their electorates than platforms espoused by their challengers. Our results suggest that both the debate content and the mode of dissemination mattered for this outcome. When relevant candidates attended their debates voters paid substantially more attention to the debate information and subsequently engaged in more political information acquisition, discussion, and coordination with others. Incumbents induced to participate in the debates increased their radio exposure, aided by increased demand from radio stations, while their challengers reduced their on-the ground campaigning.

Our results point to the challenges of transitioning away from the low-accountability equilibrium characterizing many clientelistic democracies. By inducing a subset of self-selected candidates to compete on a more programmatic basis, our intervention had uneven electoral consequences that favored incumbents. In this context, there may exist substantial returns to incumbency for some incumbents, as challengers are usually less experienced and poorly equipped for programmatic competition. Incumbents, meanwhile, may possess informational advantages enabling them to better evaluate the return to participation in such initiatives. If such democratic initiatives are to enhance competition and contribute to shifting from a clientelistic towards a more programmatic political equilibrium, then they must ensure that candidate incentives are aligned towards participation and that less experienced candidates receive additional training to face a more leveled playing field. How to best achieve this remains a question for future research.

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## A Appendix for Online Publication

### A.1 Classifying candidates as leading candidates

For each candidate we constructed an indicator variable for whether the candidate was a *predicted leading candidate*. We constructed this indicator as follows, in a sequential fashion until there were three per district: (1) if a candidate was the incumbent; (2) if the candidate ran in the 2011 election and placed 2nd or 3rd; and (3) if the candidate was from a top party. We defined top parties as, sequentially, the incumbent Unity Party (UP), Coalition for Democratic Change (CDC), Liberty Party (LP), the Alternative National Congress (ANC) and the All Liberia Party (ALP). This process resulted in three selected candidates in all districts. These *predicted leading candidates* are then split into two groups: whether the candidate is the *incumbent* or whether they are a *predicted challenger*, i.e., a non-incumbent predicted leading candidate. The incumbent ran in 64/73 (88%) of races, and so in the remaining 9 districts all three of these candidates are coded as challengers. One additional incumbent ran in a new district and is consequently coded as a challenger. Validating our indicator for top candidates with actual electoral results, we find that in 50% of cases our predicted leading candidates came in the top three in their district, and in 71% of cases came in the top five. Given our aim to identify a set of relevant candidates who had plausible chances at electoral success and voters would be interested in, we consider the exercise to be successful.

We show in Table A12 that, using the ‘actual’ leading candidates who placed in the top three in the election—whether *actual leading candidates*, *winner* or *actual challengers*—generates a similarly strong first stage on debate participation. Using this alternative categorization generates a set of qualitatively similar results, albeit with a more restricted sample of only those candidates who were both *predicted* and *actual* leading candidates in the citizen-candidate level analysis. However, given the effects we find on voting outcomes, we consider it likely that the definition of *actual leading candidates* is endogenous to our intervention. These additional results are available on request.

### A.2 Balance

We report balance on pre-treatment covariates at the district, individual, polling station and candidate levels. Balance is assessed by estimating Equation (1) for each covariate as an outcome, but omitting the individual-level  $X_i$  and district-level  $Z_d$  as controls. Across the different specifications, we present the coefficient on the treatment indicator *Invite* to test for

evidence of imbalance between treatment groups. For district-level specifications, we report specifications where districts are unweighted and where we weight by the number of registered voters, and use heteroskedasticity-robust standard errors. For individual-level specifications, we restrict to the the endline survey sample and consider two types of outcomes. First, we assign district-level outcomes to individuals in this sample. Second, we use individual-level covariates collected in the survey itself. We use weights as described in Section 6.2 and cluster at the district level. For the polling station-level specifications, first we assign district-level outcomes to each polling station in that district and second we use polling station-level variables using the fact that 90% of polling places in 2017 were also used in the 2011 election. For all new polling places we assign district-level averages. We present an unweighted specification, one where we weight by the inverse of the number of polling stations in that district and one where we weight by the number of registered voters in that polling station, and again cluster at the district level. Lastly, for the candidate-level specification we present an unweighted specification assessing balance on characteristics drawn from our candidate survey, as well as a weighted specification where we weight by the inverse of the number of responding candidate types in a given district. We refer throughout to imbalance on the unweighted specification since patterns of limited imbalance are generally shared irrespective of weighting schemes.

In Table A4 we report balance at the district level. In the unweighted specification, 0 (0) out of 18 covariates are imbalanced at the 5% (10%) level. In Table A5 we report balance in the endline survey sample when we assign district-level covariates to respondents. In the unweighted specification we find that 2 (2) out of 18 covariates are imbalanced. In Table A6 we report balance in the endline survey sample using individual-level covariates. In the unweighted specification, 1 (1) out of 4 covariates are imbalanced. In Table A7 we report balance at the polling station level. We find evidence of imbalance on 0 (1) covariates out of 18 covariates.

In Table A9 we assess evidence of imbalance on incumbent quality by treatment assignment. In the unweighted specification, we find imbalance on 0 (0) covariates out of 3. In Table A10 we test for imbalance at the candidate-level using our survey of candidates who ran in the election. Importantly, in Column 1 we demonstrate balanced response rates to our post-election survey across all candidates, incumbents and challengers across treatment groups. Using the full sample of candidates, we find imbalance on 1 (2) covariates out of 8. Restricting to incumbents we find imbalance on 0 (1) covariates. Restricting to predicted challengers we find imbalance on 2 (3) covariates. We consider balance at the candidate-level to be good particularly given our primarily descriptive employment of this data.

Overall we find little evidence of aggregate imbalance—whether on political or non-political variables—and, when applicable, we control for the variables we blocked on throughout the analysis to deal with whatever imbalance that might exist.

### **A.3 Divergences from Pre-Analysis Plan**

This study was pre-registered with EGAP (ID: 20171024AA) and AEA (ID: AEARCTR-0002553) under the title “Turning Up, Tuning In, Turning Out: Experimental Evidence from Liberia.” Pre-registration took place before endline data collection and any data analysis. In this section we describe the differences between our PAP and the final paper, as well as the logic behind them.

#### **A.3.1 Data and estimation**

We reorganized some of categorizations of variables from the PAP to fit into more coherent groupings. This comprised combining ‘Knowledge about candidates’ and ‘Beliefs over candidate competence’ into ‘Effects on beliefs about candidates;’ and ‘Voter coordination’ and relevant parts of ‘Debate exposure’ into ‘Effects on political engagement’.

As we discuss in the paper, we cross-randomized a separate intervention to vary the intensity of debate rebroadcasting (with either two or ten rebroadcasts) which ultimately had little effect. Since this additional intervention had no effect and candidates were unaware of rebroadcasting plans, we pool over rebroadcasting intensity for clarity of exposition. Importantly, however, we made no multiplicative hypotheses – rather, all our hypotheses were with respect to the overall *intensity* of the debates initiative and focused on those districts assigned to both high invitation intensity and high rebroadcasting intensity. We can demonstrate that all our key results also hold under this factorial design, but pooling rebroadcasting loses relatively little granularity and gains substantially in power. In Table A19 we show that the rebroadcasting intervention did not lead to significantly positive effects on debate exposure in either the full sample or the sample restricted to respondents in intensive-invitation districts, in spite of being correctly implemented. As discussed, this lack of effects stems from number of citizens who heard their district debates when they were only broadcast a few times.

The estimating equation we use in the paper is closest to what we called our ‘base specification’ in our PAP (Equation 5). We additionally pre-registered the possibility of constructing an individual-level instrument for the debate attendance of candidates, leveraging random assignment of candidates to debates with the incumbent and at different times of day in districts

where more than one debate was held. We found such an instrument to be underpowered due to the number of districts which only ended up holding one debate and so do not report results using it. We also pre-registered a local regression discontinuity design (Equation 8) leveraging quasi-random assignment to respondents being interviewed before or after the live debate in their district at baseline, but lacked sufficient within-district variation to pursue this. Finally, we pre-registered the use of one-tailed tests but report two-tailed tests throughout to be conservative.

We did not pre-register outcomes relating to overall turnout in the election nor the distinction we use between ‘on-the-ground’ and ‘radio’ campaigning by candidates. Additionally, in our PAP we pre-registered the use of a jackknife measure of debate performance; given that actual debate performance is likely a function of treatment assignment we instead use the predicted measure of performance described in Section 5.3.

While our pre-registered hypotheses make reference to the distinction between incumbents and challenger candidates, particularly differential treatment effects of the intervention on their participation, we did not pre-register the descriptive analysis we perform regarding their intervention compliance behavior that we discuss in Section 8.

### A.3.2 Hypotheses

We reorganised and grouped many of our pre-registered hypotheses, which were generally made with reference to individual outcome variables, into more coherent aggregated clusters. Out of the 27 hypotheses we pre-registered, results directly testing 19 of them are presented in the final paper.<sup>29</sup> The eight missing hypotheses fall into two categories. First, we do not report results relating to the hypotheses using within-district variation in whether citizens at baseline were interviewed before or after their district debate had been broadcast for the first time due to the lack of variation mentioned above.

Second, we do not report results for our pre-registered set of hypotheses relating to citizen attitudes towards the media and the electoral process. We anticipated that citizens in districts assigned to more intensive debates would update positively about the neutrality and contribution of the media to the electoral process due to the novelty and unbiasedness of the debate structure. We found little systematic evidence of this happening – we interpret this to be consistent with the campaigning response of incumbent candidates campaigning more aggressively on the

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<sup>29</sup>Broadly we aggregated hypotheses from ‘Debate exposure and knowledge about candidates’ and ‘Beliefs about candidates’ into H4; hypotheses from ‘Preferences and voting behavior’ into H6a/H6b; ‘Media consumption, attitudes, and institutions’ and ‘Debate exposure and knowledge about candidates’ into H3.

radio in these districts where they were more likely to participate. We report these results for completeness in Table A18. The final paper therefore contains substantially less emphasis on the intervention affecting media credibility than our PAP.

#### A.4 Variable construction

In this appendix we document the construction of all variables used in the analysis. Unless otherwise noted, these variables come from our panel survey of citizens where we refer to specific items in our baseline and endline survey instruments using the format *wave-question*, where *wave* is represented by *B* (baseline) or *E* (endline) and *question* is simply the question on the relevant instrument. Both survey instruments can be found online at <http://egap.org/registration/2899>.

As described in Section 6, whenever we asked the same question in both baseline and endline we use the difference as an outcome. We preserve whether variables are discrete or continuous. For indices, we standardize each component such that units in the control group have zero mean and standard deviation of one then average them, following Kling et al. (2007).

Table V:

- *Share of candidates*: share of the total candidates in that district who participated in their district debate.
- *Incumbent*: indicator for whether incumbent participated in their district debate.
- *Share of challengers*: share of the predicted challengers in that district (see Appendix A.1) who participated in their district debate.
- *Share of other candidates*: share of non-predicted candidates in that district (see Appendix A.1) who participated in their district debate.

Table VI:

- *Debate listening index*: standardized index of:
  - *Change in heard debate*: indicator variable for whether the respondent heard their district debate between baseline (B-Q7) and endline (E-Q14) surveys.
  - *Heard debate*: indicator variable for whether the respondent heard their district debate at endline (E-Q14).
  - *Number of times heard*: continuous variable for the number of times respondents reported hearing their district debate at endline (E-Q15).



- *Debate knowledge index*: standardized index of:
  - *Debate winner attended debate*: indicator for whether respondent’s named debate winner actually attended the debate (E-Q17).
  - *Stated share of participating candidates*: continuous variable for the share of candidates in respondent’s district they recall participating in debate (E-Q16).
  - *Stated share of participating leading candidate*: continuous variable for the share of leading candidates in respondent’s district they recall participating in debate (E-Q39.2, E-Q39.4, E-Q39.6).
- *Policy knowledge index*: standardized index of:
  - *Manager of CSDF*: change in whether respondents switch towards correctly identifying Representatives as the primary controller of CSDF (B-Q9, E-Q9).
  - *CSDF reporting requirement*: change in whether respondents correctly switch towards believing that it is a legal obligation to report CSDF allocation (B-Q10, E-Q10).
  - *CSDF citizen involvement requirement*: change in whether respondents correctly switch towards believing that it is a legal obligation to involve citizens in CSDF allocation decisions (B-Q11, E-Q11).

Table VII:

- *Political information demand index*: standardized index of:
  - *Change in radio listening*: Respondents’ change in listening to radio between baseline (B-Q6, E-Q6).
  - *Demand for non-radio information sources*: How frequently respondents sought political information from non-radio sources such as newspapers, television and the internet (E-Q7).
  - *Change in political discussion with friends*: Change in how frequently respondents discussed political issues with friends, family, neighbors and other members of the community (B-Q8, E-Q8).
- *Debate coordination index*: standardized index of:
  - *Discussed debate with friends*: How frequently respondents discussed the debate content with others (E-Q19).

- *Discussion led to coordination*: Whether this discussion led respondents, along with others, to agree on one particular candidate to vote for (E-Q20).
- *Polling station-level turnout*: defined as number of votes cast in House of Representatives election divided by the total number of registered voters at that polling station. Source: National Elections Commission.

Table VIII:

- *Certainty about competence*: standardized change in how sure respondents were about the competence of specific predicted leading candidates between baseline (B-Q22, B-Q24, B-Q26) and endline (E-Q34, E-Q36, E-Q38).
- *Certainty about issues*: standardized change in how sure respondents were about the priority issues of specific predicted leading candidates between baseline (B-Q16, B-Q18, B-Q20) and endline (E-Q28, E-Q30, E-Q32).
- *Beliefs about competence*: standardized change in how competent respondents believe specific predicted leading candidates were between baseline (B-Q21, B-Q23, B-Q25) and endline (E-Q33, E-Q35, E-Q37).
- *Learning about policy*: standardized change in the share of candidate priority issues that citizens name between baseline (B-Q15, B-Q17, B-Q19) and endline (E-27, E-29, E-31). We define candidate priorities using the aggregate of citizen beliefs over a given candidate's priorities measured in the baseline survey.

Table IX:

- *Ground*: standardized index of:
  - Whether candidate distributed leaflets or posters in respondent's community (E-Q41.1, E-Q41.2, E-Q41.3).
  - Whether candidate made campaign visits to respondent's community (E-Q41.1, E-Q41.2, E-Q41.3).
  - How frequently other people in their community voted for a given candidate in exchange for money, food or other gifts (E-Q40.1, E-Q40.2, E-Q40.3).
- *Radio*: standardized measure how how frequently respondents heard candidates on the radio in the two weeks before the election (E-Q39.1, E-Q39.3, E-Q39.5).

Table X:

- Panel 1:
  - *Main effect*: indicator for whether a respondent named a specific predicted leading candidate as their vote choice at endline (E-Q45).
  - *Interaction: Performance*: measure of predicted debate performance of a specific predicted leading candidate, generated by flexibly estimating observed measure of debate performance and predicting out-of-sample on candidates who did not participate.
  - *Interaction: Priority match*: measure of preference alignment between respondent and a specific predicted leading candidate. Defined as the share of the three priority issues the respondents name in their districts at baseline (B-Q13) that are shared with the priorities of a given candidate based on aggregating citizen perceptions of that candidates' priorities at baseline (B-Q15, B-Q17, B-Q19).
  
- Panel 2:
  - *Main effect*: Vote share of candidate at polling station-level.
  - *Interaction: Performance*: District-level analogue of respondent-level interaction.
  - *Interaction: Priority match*: District-level analogue of respondent-level interaction.

## A1. Descriptive Statistics

	Mean	SD	Min	Max
<b>A. District-level variables (<math>n = 73</math>)</b>				
Scheduled debate week	4.18	1.39	1.00	8.00
Number of debates in district	2.08	0.66	1.00	4.00
Number of candidates (2017)	13.55	4.81	3.00	28.00
Incumbent ran in election (2017)	0.84	0.37	0.00	1.00
Share of repeat candidates (2017)	0.26	0.11	0.06	0.53
Log registered voters (2017)	10.23	0.40	9.27	11.06
1st voteshare (2011)	0.31	0.13	0.12	0.82
2nd voteshare (2011)	0.18	0.05	0.10	0.36
3rd voteshare (2011)	0.13	0.03	0.05	0.25
Voteshare HHI (2011)	0.19	0.11	0.07	0.69
Turnout (2011)	0.66	0.05	0.56	0.75
Log population density (2008)	-9.51	1.76	-11.91	-5.21
Share over 18 (2008)	0.48	0.02	0.43	0.54
Share with secondary education (2008)	0.15	0.05	0.04	0.28
Share with GSM coverage (2015)	0.71	0.30	0.01	1.00
Share owns a radio (2016)	0.74	0.12	0.38	1.00
Share gets radio news often (2016)	0.76	0.12	0.50	1.00
Avg. N radio stations covering each town (2016)	10.98	7.60	0.00	23.36
<b>B. Individual-level variables (<math>n = 4060</math>)</b>				
Male	0.75	0.43	0.00	1.00
Age	31.73	9.27	18.00	99.00
Completed primary school	0.07	0.26	0.00	1.00
Completed secondary school	0.29	0.46	0.00	1.00
Completed university	0.14	0.34	0.00	1.00

Table presents descriptive statistics relating to all control variables used in the regression analyses. *Sources:* **District-level variables:** Debate variables from Internews. All 2017 and 2011 variables come from National Elections Commission (NEC). All 2008 variables come from 2008 Population and Housing Census. 'Share with GSM coverage' comes from Collins Mobile Coverage Explorer. 'Share owns a radio' and 'Share gets radio news often' come from Afrobarometer. 'Avg. N radio stations covering each town' comes from Internews. **Individual-level variables:** All come from researchers' panel survey. **Polling station-level variables:** Radio station variables come from Internews. All other variables come from NEC. 90% of polling stations were in locations where a polling place (Voter Registration Center, VRC) existed in 2011. For new polling stations we assign district-level averages.

A2. Descriptive Statistics (Respondent level outcomes)

	Mean	SD	Min	Max
<b>Table V</b>				
Share of candidates attended debate	0.59	0.22	0.11	1.00
Incumbent attended debate	0.35	0.48	0.00	1.00
Share of challengers attended debate	0.60	0.37	0.00	1.00
<b>Table VI</b>				
Heard debate between baseline and endline	0.09	0.29	0.00	1.00
Heard debate at endline	0.21	0.41	0.00	1.00
Number of times heard debate	0.46	1.06	0.00	24.00
Debate winner attended debate	0.32	0.47	0.00	1.00
Stated share of participating candidates	0.12	0.29	0.00	2.53
Stated incumbent debate participation	0.16	0.34	0.00	1.00
Switches to correct CSDF controller	0.25	0.43	0.00	1.00
Switches to correct requirement for CSDF reporting	0.25	0.43	0.00	1.00
Switches to correct requirement for CSDF citizen engagement	0.25	0.43	0.00	1.00
<b>Table VII</b>				
Change in radio listening	0.26	2.17	-6.00	6.00
Demand for non-radio information sources	5.01	2.15	0.00	7.00
Change in political discussion with friends	-0.07	2.30	-6.00	6.00
Discussed debate with friends	1.01	1.61	0.00	5.00
Discussion led to coordination	1.06	1.76	0.00	5.00

Table presents descriptive statistics relating to all unstandardized, disaggregated outcomes variables used in the respondent-level regressions as outcome variables.

### A3. Descriptive Statistics (Respondent-candidate level outcomes)

	Incumbent				Challengers			
	Mean	SD	Min	Max	Mean	SD	Min	Max
<b>Table VIII</b>								
Change in certainty of candidate competence	0.08	1.40	-4.00	4.00	0.36	1.53	-4.00	4.00
Change in certainty of candidate priority issues	-0.03	1.63	-4.00	4.00	-0.02	1.58	-4.00	4.00
Change in assessment of candidate competence	0.28	1.44	-4.00	4.00	0.12	1.23	-4.00	4.00
Change in share of candidate priorities named	0.02	0.41	-1.00	1.00	0.03	0.42	-1.00	1.00
<b>Table IX</b>								
Frequency of hearing candidate on radio	2.95	1.14	0.00	5.00	2.67	1.13	0.00	5.00
Candidate distributed leaflets in locality	0.93	0.25	0.00	1.00	0.90	0.30	0.00	1.00
Candidate visited locality	0.81	0.39	0.00	1.00	0.74	0.44	0.00	1.00
Frequency of candidate vote buying	2.28	1.39	1.00	5.00	2.10	1.28	1.00	5.00
<b>Table X (Panel 1)</b>								
Votes for candidate	0.31	0.46	0.00	1.00	0.15	0.35	0.00	1.00
Measure of predicted debate performance	0.27	0.18	0.00	0.70	0.12	0.15	0.00	0.83
Measure of policy match	0.42	0.27	0.00	1.00	0.42	0.28	0.00	1.00
<b>Table X (Panel 2)</b>								
Vote share	0.26	0.22	0.00	0.99	0.11	0.16	0.00	0.99
Measure of predicted debate performance	0.25	0.19	0.00	0.68	0.12	0.13	0.00	0.75
Measure of policy match	0.43	0.09	0.16	0.59	0.43	0.09	0.00	0.64

Table presents descriptive statistics relating to all unstandardized, disaggregated outcomes variables used in the respondent-candidate level regressions as outcome variables.

#### A4. District-level Balance

	<u>VS of 1st place (2011)</u>		<u>VS of 1st place (2011)</u>		<u>VS of 3rd place (2011)</u>	
<i>Invite</i>	-0.011 (0.024)	-0.015 (0.025)	-0.005 (0.010)	-0.008 (0.010)	-0.000 (0.007)	-0.001 (0.006)
Control Mean	0.322	0.309	0.178	0.183	0.129	0.129
	<u>Turnout (2011)</u>		<u>Share ran in 2011</u>		<u>VS HHI (2011)</u>	
<i>Invite</i>	-0.004 (0.009)	-0.004 (0.008)	-0.012 (0.024)	-0.019 (0.024)	-0.006 (0.020)	-0.009 (0.020)
Control Mean	0.661	0.667	0.258	0.266	0.198	0.189
	<u>N. candidates (2017)</u>		<u>Incumbent ran (2017)</u>		<u>Number of debates</u>	
<i>Invite</i>	-0.509 (0.833)	-0.670 (0.928)	0.037 (0.079)	0.016 (0.076)	-0.083 (0.126)	-0.131 (0.140)
Control Mean	13.634	14.780	0.850	0.883	2.115	2.256
	<u>Log pop. dens. (2008)</u>		<u>Log reg. voters (2017)</u>		<u>GSM coverage (2016)</u>	
<i>Invite</i>	0.254 (0.365)	0.160 (0.370)	0.056 (0.061)	0.060 (0.055)	0.032 (0.062)	0.005 (0.055)
Control Mean	-9.847	-9.108	10.214	10.351	0.645	0.773
	<u>Share owns radio (2015)</u>		<u>Share radio news (2015)</u>		<u>Radio coverage (2016)</u>	
<i>Invite</i>	-0.019 (0.025)	-0.034 (0.022)	-0.017 (0.026)	-0.038 (0.025)	-0.109 (1.347)	0.024 (1.384)
Control Mean	0.755	0.773	0.767	0.780	10.051	12.830
	<u>Debate week</u>		<u>Share sec. ed. (2008)</u>		<u>Share 18+ (2008)</u>	
<i>Invite</i>	0.028 (0.215)	-0.030 (0.209)	0.004 (0.010)	0.003 (0.010)	0.000 (0.004)	0.001 (0.004)
Control Mean	3.980	3.979	0.137	0.154	0.483	0.487
Observations	73	73	73	73	73	73
Weight	None	Reg	None	Reg	None	Reg

Descriptions of all variables can be found in Table A1.

All specifications are estimated using OLS and include block FE. Standard errors clustered at the district-level in parentheses.

A5. Individual-level Balance (district covariates)

	<b>VS of 1st place (2011)</b>			<b>VS of 1st place (2011)</b>			<b>VS of 3rd place (2011)</b>		
<i>Invite</i>	0.007 (0.025)	-0.009 (0.021)	-0.014 (0.021)	-0.011 (0.009)	-0.005 (0.008)	-0.008 (0.009)	-0.000 (0.007)	-0.000 (0.006)	-0.001 (0.005)
Control Mean	0.298	0.308	0.308	0.185	0.182	0.183	0.129	0.128	0.129
	<b>Turnout (2011)</b>			<b>Share ran in 2011</b>			<b>VS HHI (2011)</b>		
<i>Invite</i>	0.001 (0.007)	-0.004 (0.007)	-0.004 (0.007)	-0.043 (0.019)	-0.012 (0.021)	-0.019 (0.020)	0.007 (0.021)	-0.005 (0.017)	-0.007 (0.017)
Control Mean	0.665	0.659	0.667	0.275	0.270	0.266	0.181	0.189	0.188
	<b>N. candidates (2017)</b>			<b>Incumbent ran (2017)</b>			<b>Number of debates</b>		
<i>Invite</i>	-0.642 (0.757)	-0.549 (0.709)	-0.710 (0.795)	0.010 (0.068)	0.034 (0.068)	0.014 (0.066)	-0.117 (0.108)	-0.091 (0.108)	-0.139 (0.120)
Control Mean	15.084	13.963	14.833	0.900	0.858	0.883	2.291	2.152	2.265
	<b>Log pop. dens. (2008)</b>			<b>Log reg. voters (2017)</b>			<b>GSM coverage (2016)</b>		
<i>Invite</i>	0.207 (0.317)	0.246 (0.312)	0.149 (0.317)	0.072 (0.052)	0.055 (0.052)	0.059 (0.047)	0.029 (0.050)	0.032 (0.053)	0.004 (0.047)
Control Mean	-8.975	-9.611	-9.096	10.342	10.215	10.354	0.795	0.695	0.774
	<b>Share owns radio (2015)</b>			<b>Share radio news (2015)</b>			<b>Radio coverage (2016)</b>		
<i>Invite</i>	-0.041 (0.020)	-0.020 (0.022)	-0.035 (0.019)	-0.035 (0.025)	-0.017 (0.022)	-0.039 (0.022)	-0.172 (1.222)	-0.138 (1.159)	-0.010 (1.190)
Control Mean	0.771	0.750	0.774	0.778	0.767	0.780	13.552	11.275	12.895
	<b>Debate week</b>			<b>Share sec. ed. (2008)</b>			<b>Share 18+ (2008)</b>		
<i>Invite</i>	-0.035 (0.193)	0.031 (0.184)	-0.026 (0.179)	0.006 (0.008)	0.004 (0.009)	0.002 (0.009)	-0.001 (0.004)	0.000 (0.004)	0.001 (0.003)
Control Mean	4.162	4.148	3.980	0.160	0.146	0.154	0.488	0.485	0.487
Observations	4061	4061	4061	4061	4061	4061	4061	4061	4061
Weight	No	1/Obs	Reg/Obs	No	1/Obs	Reg/Obs	No	1/Obs	Reg/Obs

Descriptions of all variables can be found in Table A1.

All specifications are estimated using OLS and include block FE. Standard errors clustered at the district-level in parentheses.



A6. Individual-level balance (individual covariates)

	Survey date			Education		
<i>Invite</i>	-1.063 (0.523)	-0.796 (0.572)	-1.006 (0.573)	0.067 (0.086)	0.102 (0.084)	0.081 (0.075)
Control Mean	71.801	71.153	71.422	6.586	6.447	6.534
	Age			Male		
<i>Invite</i>	0.163 (0.439)	0.359 (0.464)	0.225 (0.437)	0.011 (0.015)	0.025 (0.016)	0.021 (0.016)
Control Mean	31.728	32.103	31.877	0.746	0.744	0.740
Observations	4061	4061	4061	4061	4061	4061
Weight	No	1/Obs	Reg/Obs	No	1/Obs	Reg/Obs

All specifications are estimated using OLS and include block FE. Standard errors clustered at the district-level in parentheses.

A7. Polling Station-level Balance (district covariates)

	<b>VS of 1st place (2011)</b>			<b>VS of 1st place (2011)</b>			<b>VS of 3rd place (2011)</b>		
<i>Invite</i>	-0.015 (0.021)	-0.010 (0.021)	-0.015 (0.021)	-0.009 (0.009)	-0.005 (0.008)	-0.009 (0.009)	-0.001 (0.005)	-0.000 (0.006)	-0.001 (0.005)
Control Mean	0.308	0.309	0.309	0.183	0.184	0.184	0.129	0.129	0.129
	<b>Turnout (2011)</b>			<b>Share ran in 2011</b>			<b>VS HHI (2011)</b>		
<i>Invite</i>	-0.004 (0.007)	-0.004 (0.007)	-0.004 (0.007)	-0.016 (0.020)	-0.012 (0.021)	-0.018 (0.020)	-0.008 (0.017)	-0.006 (0.017)	-0.008 (0.018)
Control Mean	0.665	0.667	0.667	0.266	0.266	0.266	0.188	0.188	0.188
	<b>N. candidates (2017)</b>			<b>Incumbent ran (2017)</b>			<b>Number of debates</b>		
<i>Invite</i>	-0.648 (0.771)	-0.506 (0.715)	-0.677 (0.798)	-0.050 (0.067)	-0.019 (0.070)	-0.053 (0.066)	-0.124 (0.118)	-0.083 (0.109)	-0.130 (0.121)
Control Mean	14.651	14.794	14.794	0.883	0.890	0.890	2.239	2.256	2.256
	<b>Log pop. dens. (2008)</b>			<b>Log reg. voters (2017)</b>			<b>GSM coverage (2016)</b>		
<i>Invite</i>	0.178 (0.322)	0.255 (0.314)	0.154 (0.320)	0.062 (0.048)	0.056 (0.052)	0.060 (0.047)	0.010 (0.048)	0.032 (0.053)	0.005 (0.047)
Control Mean	-9.223	-9.097	-9.097	10.325	10.352	10.352	0.753	0.775	0.775
	<b>Share owns radio (2015)</b>			<b>Share radio news (2015)</b>			<b>Radio coverage (2016)</b>		
<i>Invite</i>	-0.031 (0.019)	-0.019 (0.022)	-0.033 (0.019)	-0.037 (0.021)	-0.017 (0.022)	-0.038 (0.022)	-0.001 (1.201)	-0.100 (1.157)	-0.065 (1.199)
Control Mean	0.770	0.773	0.773	0.778	0.780	0.780	12.485	12.919	12.919
	<b>Debate week</b>			<b>Share sec. ed. (2008)</b>			<b>Share 18+ (2008)</b>		
<i>Invite</i>	-0.041 (0.176)	0.029 (0.185)	-0.046 (0.178)	0.003 (0.009)	0.004 (0.009)	0.002 (0.009)	0.002 (0.003)	0.000 (0.004)	0.001 (0.003)
Control Mean	4.007	3.987	3.987	0.152	0.154	0.154	0.486	0.487	0.487
Observations	5383	5383	5383	5383	5383	5383	5383	5383	5383
Weight	No	1/PS	Reg	No	1/PS	Reg	No	1/PS	Reg

Descriptions of all variables can be found in Table A1.

All specifications are estimated using OLS and include block FE. Standard errors clustered at the district-level in parentheses.

A8. Polling Station-level Balance (PS covariates)

	N. PS in VRC (2017)			N. PS in VRC (2011)		
<i>Invite</i>	-0.099 (0.247)	0.008 (0.226)	-0.121 (0.249)	-0.114 (0.185)	-0.011 (0.183)	-0.142 (0.186)
Control Mean	3.605	3.807	3.807	3.399	3.571	3.571
	New VRC			Urban		
<i>Invite</i>	-0.014 (0.014)	-0.008 (0.016)	-0.015 (0.013)	0.067 (0.058)	0.079 (0.056)	0.063 (0.059)
Control Mean	0.111	0.101	0.101	0.299	0.326	0.326
	Reg. Voters (2017)			Reg. Voters (2011)		
<i>Invite</i>	2.247 (6.170)	4.668 (6.416)	0.686 (4.775)	-53.204 (90.770)	0.239 (89.758)	-67.706 (90.943)
Control Mean	403.792	418.480	418.480	1439.977	1526.086	1526.086
	Turnout (2011)			Invalid votes (2011)		
<i>Invite</i>	0.014 (0.009)	0.011 (0.009)	0.015 (0.009)	0.002 (0.003)	0.004 (0.003)	0.002 (0.003)
Control Mean	0.620	0.620	0.620	0.066	0.066	0.066
	Radio covered			Radio intensity		
<i>Invite</i>	-0.016 (0.054)	0.012 (0.052)	-0.025 (0.054)	0.001 (0.004)	0.003 (0.004)	-0.000 (0.004)
Control Mean	0.848	0.858	0.858	0.046	0.045	0.045
Observations	5383	5383	5383	5383	5383	5383
Weight	No	1/PS	Reg	No	1/PS	Reg

Descriptions of all variables can be found in Table A1. Analysis throughout is at the polling station-level, where multiple polling stations exist within a single location called a VRC. 1780/2080 VRCs existed in the 2011 election; for these 300 new VRCs we assign district-level averages.

All specifications are estimated using OLS and include block FE. Standard errors clustered at the district-level in parentheses.

### A9. Incumbent Balance

<i>(Invitation, Rebroadcasting)</i>	<b>Attendance</b>		<b>Absent</b>		<b>Distant</b>	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Invite</i>	0.034 (0.032)	0.019 (0.030)	-0.003 (0.020)	0.013 (0.024)	-0.025 (0.028)	-0.023 (0.024)
Mean	0.791	0.807	0.117	0.114	0.079	0.066
Observations	73	73	73	73	73	73
Weight	None	Reg	None	Reg	None	Reg

Outcome variables are plenary session attendance measures taken from legislator score-cards for 2016. Legislators either attend, are absent, or are away from Monrovia for each plenary session.

All specifications are estimated using OLS and include block FE. Standard errors clustered at the district-level in parentheses.

### A10. Candidate-level Balance

	Response (1)	Age (2)	Univ. ed. (3)	Radio (4)	Male (5)	Ran before (6)	Gov job (7)	Advocacy (8)	NGO (9)
<b>A. All candidates</b>									
<i>Invite</i>	-0.028 (0.031)	-0.379 (1.064)	0.012 (0.049)	0.020 (0.013)	0.009 (0.027)	-0.024 (0.032)	-0.028 (0.030)	0.064 (0.025)	0.065 (0.036)
Control Mean	0.63	48.48	0.55	0.03	0.85	0.30	0.34	0.83	0.38
Observations	984	608	612	612	612	612	612	612	612
<b>B. Incumbents</b>									
<i>Invite</i>	0.139 (0.131)	2.394 (4.085)	0.061 (0.210)	-0.030 (0.200)	0.121 (0.174)	0.000 (.)	-0.394 (0.212)	-0.121 (0.174)	0.061 (0.255)
Control Mean	0.42	55.31	0.69	0.15	0.77	1.00	0.69	0.92	0.23
Observations	64	31	31	31	31	31	31	31	31
<b>C. Challengers</b>									
<i>Invite</i>	0.010 (0.083)	3.875 (1.655)	-0.005 (0.106)	-0.002 (0.052)	0.027 (0.067)	0.134 (0.102)	0.036 (0.086)	0.155 (0.065)	0.175 (0.103)
Control Mean	0.64	47.31	0.65	0.06	0.90	0.35	0.31	0.81	0.29
Observations	155	99	102	102	102	102	102	102	102

Panel A presents unweighted balance tests for the full set of candidates in survey, Panel B restricts to incumbent candidates, Panel C restricts to predicted challenger candidates. Outcome variables are: response rate to survey; age in years; indicator for whether candidate completed university; indicator for whether they own or manage a radio station; indicator for candidate being male; indicator for candidate having run for office before; indicator for candidate having a government job before; indicator for candidate having advocacy experience; indicator for candidate working for an NGO before.

All specifications are estimated using OLS and include block FE. Standard errors clustered at the district-level in parentheses.

A11. Candidate-level Balance (weighted)

	Response (1)	Age (2)	Univ ed (3)	Radio (4)	Male (5)	Ran before (6)	Gov job (7)	Advocacy (8)	NGO (9)
<b>A. All candidates</b>									
<i>Invite</i>	-0.045 (0.033)	-0.171 (0.899)	-0.013 (0.056)	0.030 (0.017)	0.025 (0.031)	-0.031 (0.036)	-0.018 (0.039)	0.043 (0.027)	0.057 (0.037)
Mean	0.63	48.48	0.55	0.03	0.85	0.30	0.34	0.83	0.38
Observations	984	608	612	612	612	612	612	612	612
<b>B. Challengers</b>									
<i>Invite</i>	-0.037 (0.064)	4.918 (1.815)	0.042 (0.105)	0.008 (0.064)	0.008 (0.068)	0.153 (0.106)	0.054 (0.094)	0.144 (0.068)	0.165 (0.101)
Mean	0.78	47.31	0.65	0.06	0.90	0.35	0.31	0.81	0.29
Observations	155	99	102	102	102	102	102	102	102

In this table we weight observations by the inverse of the number of responding candidate types in a given district. Since there is one incumbent per district, for incumbents this would be identical to Panel B of Table A10 and thus we omit it. Panel A presents balance tests for the full set of candidates in survey, Panel B restricts predicted challenger candidates. Outcome variables are: response rate to survey; age in years; indicator for whether candidate completed university; indicator for whether they own or manage a radio station; indicator for candidate being male; indicator for candidate having run for office before; indicator for candidate having a government job before; indicator for candidate having advocacy experience; indicator for candidate working for an NGO before.

All specifications are estimated using OLS and include block FE. Standard errors clustered at the district-level in parentheses.

A12. Candidate Debate Participation (supplementary)

	(1)	(2)	(3)
<b>A. Election winner</b>			
<i>Invite</i>	0.253 (0.097)	0.202 (0.089)	0.275 (0.093)
Control Mean	0.501	0.520	0.474
Observations	4060	4060	4060
Weight	No	1/Obs	Reg/Obs
<b>B. Share of actual challengers</b>			
<i>Invite</i>	0.267 (0.068)	0.176 (0.068)	0.237 (0.068)
Control Mean	0.488	0.572	0.525
Observations	4060	4060	4060
Weight	No	1/Obs	Reg/Obs
<b>C. Share of actual other candidates</b>			
<i>Invite</i>	0.018 (0.039)	0.029 (0.037)	0.036 (0.038)
Control Mean	0.563	0.584	0.584
Observations	3991	3991	3991
Weight	No	1/Obs	Reg/Obs

Outcome variables are the share of the respective set of candidates (winner, actual challenger) who attended a debate out of all candidates in that district. Actual challengers are defined as candidates who ranked in the top three in their race in the election but were not the incumbent. Actual other candidates are those who did not rank in the top three. Panels A and B have 4060 observations; Panel C has fewer due to only three candidates running in two districts (and hence no 'actual other candidates' defined).

All specifications are estimated using OLS and include block FE, enumerator FE, district-level and individual-level controls. For weighted specifications, 'Obs' is the number of observations in that district and 'Reg' is the number of registered voters in that district. Standard errors clustered at the district-level in parentheses.

A13. Debate Rebroadcasting

	(1)	(2)	(3)
<b>A. Radio monitors</b>			
<i>Invite</i>	0.387 (0.998)	-0.700 (0.860)	-0.206 (0.974)
Control Mean	5.230	5.618	5.466
<b>B. Radio survey</b>			
<i>Invite</i>	1.055 (1.044)	0.326 (0.906)	0.746 (0.954)
Control Mean	7.473	7.702	7.698
Observations	4060	4060	4060
Weight	No	1/Obs	Reg/Obs

Outcome variables are, in Panel A, the number of contracted rebroadcasts confirmed by radio monitors and, in panel B, Number of rebroadcasts based on survey of radio stations, including those not contracted to rebroadcast but recorded as being present in the debate venue.

All specifications are estimated using OLS and include block FE, enumerator FE, district-level and individual-level controls. For weighted specifications, 'Obs' is the number of observations in that district and 'Reg' is the number of registered voters in that district. Standard errors clustered at the district-level in parentheses.



A14. Debate Exposure (supplementary)

	(1)	(2)	(3)
<b>A. Debate listening index</b>			
<b>(1) Change in heard debate</b>			
<i>Invite</i>	0.080 (0.029)	0.080 (0.026)	0.102 (0.027)
Control Mean	0.084	0.082	0.082
<b>(2) Heard debate</b>			
<i>Invite</i>	0.038 (0.022)	0.035 (0.020)	0.050 (0.021)
Control Mean	0.195	0.202	0.193
<b>(3) Number of times heard</b>			
<i>Invite</i>	0.085 (0.045)	0.104 (0.046)	0.120 (0.045)
Control Mean	0.420	0.440	0.420
<b>B. Debate knowledge index</b>			
<b>(1) Debate winner attended debate</b>			
<i>Invite</i>	0.075 (0.031)	0.078 (0.029)	0.096 (0.029)
Control Mean	0.291	0.297	0.283
<b>(2) Stated share of participating candidates</b>			
<i>Invite</i>	0.023 (0.015)	0.024 (0.015)	0.031 (0.014)
Control Mean	0.111	0.115	0.111
<b>(3) Stated share of participating leading candidates</b>			
<i>Invite</i>	0.030 (0.017)	0.026 (0.016)	0.039 (0.016)
Control Mean	0.145	0.153	0.148
<b>C. Policy knowledge index</b>			
<b>(1) Manager of CSDF</b>			
<i>Invite</i>	0.041 (0.029)	0.053 (0.038)	0.044 (0.032)
Control Mean	0.237	0.231	0.243
<b>(2) CSDF reporting requirement</b>			
<i>Invite</i>	-0.011 (0.032)	0.018 (0.035)	-0.008 (0.033)
Control Mean	0.247	0.249	0.247
<b>(3) CSDF citizen involvement requirement</b>			
<i>Invite</i>	0.094 (0.040)	0.113 (0.051)	0.114 (0.045)
Control Mean	0.246	0.246	0.244
Observations	4060	4060	4060
Weight	No	1/Obs	Reg/Obs

All outcome variables are described in Section A.4.

All specifications are estimated using OLS and include block FE, enumerator FE, district-level and individual-level controls. For weighted specifications, 'Obs' is the number of observations in that district and 'Reg' is the number of registered voters in that district. Standard errors clustered at the district-level in parentheses.

A15. Political Engagement (supplementary)

	(1)	(2)	(3)
<b>A. Political information demand index</b>			
<b>(1) Change in radio listening</b>			
<i>Invite</i>	0.285 (0.134)	0.451 (0.164)	0.370 (0.160)
Control Mean	0.233	0.213	0.232
<b>(2) Demand for non-radio information sources</b>			
<i>Invite</i>	0.143 (0.075)	0.168 (0.085)	0.179 (0.083)
Control Mean	4.970	4.800	4.932
<b>(3) Change in political discussion with friends</b>			
<i>Invite</i>	0.510 (0.189)	0.516 (0.226)	0.491 (0.193)
Control Mean	-0.124	-0.106	-0.117
<b>B. Debate coordination index</b>			
<b>(1) Discussed debate with friends</b>			
<i>Invite</i>	0.207 (0.102)	0.222 (0.095)	0.275 (0.095)
Control Mean	0.936	0.953	0.908
<b>(2) Discussion led to coordination</b>			
<i>Invite</i>	0.282 (0.107)	0.285 (0.101)	0.343 (0.103)
Control Mean	0.956	0.974	0.928
Observations	4060	4060	4060
Weight	No	1/Obs	Reg/Obs

All outcome variables are described in Section A.4.

All specifications are estimated using OLS and include block FE, enumerator FE, district-level and individual-level controls. For weighted specifications, 'Obs' is the number of observations in that district and 'Reg' is the number of registered voters in that district. Standard errors clustered at the district-level in parentheses.

A16. Candidate Campaigning (supplementary)

	(1)	(2)	(3)
<b>A. Incumbent</b>			
<b>(1) Candidate visited community</b>			
<i>Invite</i>	-0.027 (0.019)	-0.038 (0.023)	-0.032 (0.023)
Control Mean	0.823	0.824	0.820
<b>(2) Candidate distributed leaflets</b>			
<i>Invite</i>	-0.012 (0.010)	-0.014 (0.012)	-0.012 (0.011)
Control Mean	0.935	0.933	0.932
<b>(3) Vote buying for candidate</b>			
<i>Invite</i>	0.035 (0.039)	0.059 (0.039)	0.070 (0.041)
Control Mean	2.258	2.223	2.231
Observations	3492	3492	3492
<b>B. Challenger</b>			
<b>(1) Candidate visited community</b>			
<i>Invite</i>	-0.024 (0.012)	-0.019 (0.010)	-0.024 (0.012)
Control Mean	0.756	0.753	0.753
<b>(2) Candidate distributed leaflets</b>			
<i>Invite</i>	-0.013 (0.006)	-0.016 (0.007)	-0.017 (0.007)
Control Mean	0.905	0.908	0.907
<b>(3) Vote buying for candidate</b>			
<i>Invite</i>	-0.008 (0.038)	-0.025 (0.039)	-0.021 (0.037)
Control Mean	2.109	2.145	2.132
Observations	8676	8676	8676
Weight	No	1/Obs	Reg/Obs

All outcome variables are described in Section A.4.

All specifications are estimated using OLS and include block FE, enumerator FE, district-level and individual-level controls. For weighted specifications, 'Obs' is the number of observations in that district and 'Reg' is the number of registered voters in that district. Standard errors clustered at the district-level in parentheses.

A17. Vote Choice Reason

	<b>Campaign promises</b>			<b>Expectations</b>		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Invite</i>	0.006 (0.010)	0.002 (0.012)	0.000 (0.012)	0.018 (0.019)	0.005 (0.021)	0.024 (0.018)
Control Mean	0.033	0.036	0.035	0.067	0.064	0.063
	<b>Competence</b>			<b>Experience</b>		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Invite</i>	0.013 (0.030)	0.016 (0.032)	0.001 (0.033)	-0.024 (0.026)	-0.001 (0.030)	-0.008 (0.028)
Control Mean	0.196	0.209	0.201	0.262	0.254	0.258
Observations	4060	4060	4060	4060	4060	4060
Weight	No	1/Obs	Reg/Obs	No	1/Obs	Reg/Obs

Outcome variable is an indicator for whether respondents switched towards citing candidate campaign promises, expected policy by the candidate, candidate competence or candidate experience as their main reason for their vote choice.

All specifications are estimated using OLS and include block FE, enumerator FE, district-level and individual-level controls. For weighted specifications, 'Obs' is the number of observations in that district and 'Reg' is the number of registered voters in that district. Standard errors clustered at the district-level in parentheses.

A18. Broader Consequences

	(1)	(2)	(3)
<b>A. Media: trust and bias</b>			
<i>Invite</i>	-0.007 (0.013)	-0.003 (0.013)	-0.004 (0.013)
Observations	4060	4060	4060
Weight	No	1/Obs	Reg/Obs
<b>B. Media: helps democracy</b>			
<i>Invite</i>	-0.003 (0.020)	-0.004 (0.026)	-0.015 (0.025)
Observations	4060	4060	4060
Weight	No	1/Obs	Reg/Obs
<b>C. Electoral attitudes</b>			
<i>Invite</i>	0.010 (0.027)	0.013 (0.031)	0.004 (0.029)
Observations	4060	4060	4060
Weight	No	1/Obs	Reg/Obs

Outcome variables are all z-score indices. Panel A: extent to which the media (1) was unbiased during election (2) gave equal coverage of candidates (3) is trustworthy. Panel B: media (1) helps select competent representatives (2) ensures representatives reflect views of voters. Panel C: elections (1) help select competent representatives (2) ensure representatives reflect views of voters.

All specifications are estimated using OLS and include block FE, enumerator FE, district-level and individual-level controls. For weighted specifications, 'Obs' is the number of observations in that district and 'Reg' is the number of registered voters in that district. Standard errors clustered at the district-level in parentheses.

A19. Rebroadcasting Intervention

	(1)	(2)	(3)
<b>A. Full sample</b>			
<i>Rebroadcast</i>	0.149 (0.093)	0.113 (0.088)	0.145 (0.102)
Observations	4060	4060	4060
Weight	No	1/Obs	Reg/Obs
<b>B. Respondents in intensive-invite districts</b>			
<i>Rebroadcast</i>	-0.021 (0.156)	-0.021 (0.128)	0.021 (0.156)
Observations	2252	2252	2252
Weight	No	1/Obs	Reg/Obs

Outcome variable is our standardized index of debate exposure. In Panel A, we show no overall effects on debate exposure. In Panel B, we show no effects on debate exposure if we restrict to respondents in those districts assigned to high invitation intensity.

All specifications are estimated using OLS and include block FE, enumerator FE, district-level and individual-level controls. For weighted specifications, 'Obs' is the number of observations in that district and 'Reg' is the number of registered voters in that district. Standard errors clustered at the district-level in parentheses.