Deliberation and Ethical Voting Behavior: Evidence from a Campaign Experiment in Benin

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*T*he article provides experimental evidence of the effect of candidate-citizen town-hall meetings on voters' political behavior. The intervention took place prior to the March 2011 elections in Benin and involved 150 randomly selected villages. In the treatment group, candidates held town-hall meetings where voters deliberated over their electoral platforms. The control group was exposed to the standard campaign—that is, one-way communication of the candidate's platform by himself or his local broker. We find that town-hall meetings led to a more informed citizenry and higher electoral participation, which diverged little along socioeconomic lines. We also observe a lower effectiveness of vote-buying attempts where town halls took place. This is consistent with town-hall deliberation promoting what we call more "ethical" voters.

INTRODUCTION

One of the main normative appeals of democracy lies in the ability of its citizens to publicly engage with key issues through thoughtful, informed, and open-minded discussion. In its earliest forms, deliberation took precedence over the process of elections and voting itself; in fact, voting had to *follow* deliberation such as in the assembly debates of ancient Greece or in the city-states of the Middle Ages.[1]

According to its advocates, public deliberation serves critical functions in a democratic society. First, deliberation performs an informational and educational role as it requires individuals to understand the issues under discussion, thus serving as "school(s) of [the] public spirit"—according to John Stuart Mill (Chambers 2018, 60). Second, and inspired by Rousseau's idea of the general will, public deliberation among equals can prompt individuals to seek solutions and policy outcomes in the interest of all as opposed to solely self-serving ones (Cohen 2010, 127 cited by Chambers 2018, 57). Third, deliberation could also lead to more epistemically sound, namely better, decisions in benefit of the *polis* (Mill [1859] 1962). For instance, among contemporary thinkers, Macedo (2010) and Rawls (1997) deemed these exercises in "public reasoning" as conducive to policies consistent with the principles of freedom and equality. Finally, the deliberation of legislation and policies can strengthen the link between the public and office-holders, key for democratic legitimacy (Dryzek et al. 2019; Fishkin and Mansbridge 2017, 7).

Yet public deliberation is still a foreign experience to many citizens. Nonetheless, there has been a recent effort to revitalize it in the form of citizen forum initiatives, participatory budget procedures, community-driven development policy (Casey 2018), and deliberative polling exercises (Fishkin 1997; Luskin, Fishkin, and Jowell 2002), to name a few. The goal of these initiatives is to better integrate the public in decision-making processes and help bridge the gap between office-holders, policy-formulation, and the public. More importantly, these initiatives aim to improve representative democracy by promoting greater political engagement, greater tolerance, and open-mindedness; encourage the acquisition and diffusion of political knowledge among its citizens; and ultimately lead to decisions focused on the "common good," as intended in its original formulation. In other words, they may lead to more informed, engaged, and public spirited citizens or what we call more "ethical" voters.[2]

In this article, we examine how a particular deliberative intervention—that of the candidate-citizen town-hall meeting—may deepen citizens' democratic ethos and behavior in a sub-Saharan African setting. To do so, we implemented a nationwide political experiment in which we assigned the top three candidates of the 2011 Beninese presidential election to deliver their campaign platforms in two main ways. While villages in the treatment group held town-hall meetings organized by one of the candidates—and attendees

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[1] Deliberation comes from the Latin *libra*, which means scale, weight, or balance. In its original meaning, to deliberate is to de-weight, de-scale, de-balance one's opinions (Hansen 2004, 80).

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[2] This depiction is consistent with "true democrats" from public opinion studies (Carlin and Singer 2011); and with "ethical voters" from the game theoretical literature (Feddersen and Sandroni 2006) which are similar in principle.

deliberated over the candidates' policy—those in the control saw their usual campaign strategies, mainly rallies organized by the candidates' local brokers. In total, 150 villages were randomly selected from 30 of the country's 77 districts. Sixty villages were assigned to the town-hall meeting treatment, while 90 control villages saw their usual campaign mode. Based on this assignment, we then examine how the treatment impacted voting outcomes using village administrative records as well as individual information on vote-buying offers, information levels, and campaign assessments using post-electoral survey data.

Results from this intervention show that: first, town-hall meetings had a positive effect on political participation (turnout), particularly in villages treated by opposition candidates. Given the known costs of voting and the strong preexisting support for the treated candidate (by design), this finding is consistent with deliberation increasing political participation for intrinsic reasons—an important trait of "ethical" voters.[3] Furthermore, these differences in turnout do not vary by key socioeconomic traits such as gender, poverty, or educational achievement, suggesting that town-hall meetings did not selectively incentivize participation along social cleavages in a way that exacerbated political inequality or merely reflects existing power structures.

Second, on average, there are no effects of town-hall meetings on official vote shares. Yet this masks important variation driven by vote-buying attempts: individuals in treated villages who received cash handouts actually exhibit a lower willingness to vote for the treated candidate, especially in those villages in which the incumbent party held the town-hall meetings. In contrast, we find no differential effect of the treatment on vote-shares along important demographic traits. Additional analysis shows that those in treated villages who received a handout were also more likely to characterize political campaigns in negative terms. The latter is consistent with town-hall meetings raising the expectations of political behavior such that voters might be punishing the incongruity between the incumbents' interest in policy and community input with his vote-buying attempts.

Examining the causal mechanisms, we show that much of the impact of deliberation could be driven by better political information. Specifically, respondents in treated villages were more likely to know the platform of the candidate they favored and this information appears to be of higher quality. For instance, they were better able to mention specific aspects of their preferred candidates' platform (i.e., social project and micro-credit program) and traits (i.e., achievements, quality, and skills) as reasons behind their support than those in control villages. The latter was particularly the case for opposition candidates with whom the population may be less familiar with in the first place. In contrast, we find less support for other mechanisms,

such as town-hall meetings prompting higher than usual levels of discussion and coordination among voters.

Contributions to the Literature

These findings contribute, first, to two current debates in the democratic deliberation literature: one centered on the role of social inequality on deliberation processes and outcomes, and the second on the ability of deliberative exercises to meaningfully impact political behavior. Our findings showing that town-hall deliberation leads to higher turnout and a lower effectiveness of handouts is consistent with deliberation fostering certain democratic norms (Cohen 1997; Fishkin 1997; Gutmann and Thompson 1998) while not necessarily demobilizing voters who may shy away from potential conflict arising from these discussions (Mutz 2006).[4] In this sense, our results demonstrate how these exercises can have a visible impact on the ballot box.

Moreover, the lack of heterogeneous effects along key dimensions of social inequality runs against the idea of policy divergence among groups found elsewhere (Sunstein 2002). Rather, it suggests that differences in education access (or social status) do not necessarily preclude deliberation exercises (Fishkin et al. 2017; Siu 2017) even if certain groups do exhibit lower deliberative abilities (Gerber et al. 2018).[5] In our case, the effects of town-hall meetings do not simply reflect or exacerbate power differences that exist outside of the deliberative venue—a valid concern when conducting these exercises (see Mansbridge 1983; Mendelberg, Karpowitz, and Goedert 2014).

Our second main contribution is to the empirical literature on the effects of information on voting behavior. While some studies show that policy and politician performance information (see Arias, Pachón, and Marshall 2017; Chong et al. 2015; de Figueiredo, Hidalgo, and Kasahara 2023; Gilens 2001) as well as door-to-door canvassing (Bergan et al. 2005; Gerber and Green 2000; Green and Gerber 2019; Hillygus and Shields 2014; Pons 2018) may impact citizen's opinions and political behavior, others show a small to negligible impact. In fact, recent meta-analyses (Kalla and Broockman 2018) combined with coordinated experimental trials (Dunning et al. 2019) show that this type of private canvassing and information sharing may have little impact on voters' behavior. However, Dunning et al. (2019) leave open the possibility that *publicly* disseminated information may be better at eliciting these effects. Our findings speak directly to this possibility: town halls appear to lead to more and higher quality political information which could explain the high turnout. This is likely because the public nature of town halls and stronger interpersonal interactions may be more effective at informing and motivating voters (Esterling, Neblo, and Lazer 2011) and shifting their

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[3] Thus in-line with Riker and Ordershook's (1968) "civic duty" explanation.

[4] For example, we do not see town-hall meetings *lowering* turnout on average.

[5] Fishkin (1995) also finds no polarization resulting from deliberation.

expectations (Adida et al. 2020), particularly in some sub-Saharan Africa settings (Paller 2019).

Third, our results also contribute to the literature on the role of political campaigns in sub-Saharan Africa. The use of town halls represents a "different" form of campaigning, one whose impact on mobilization does not rely on patronage promises (Lindberg 2010), their own ethnic identities (Horowitz 2016; Posner 2005), or that of their spouses (Adida et al. 2016). Rather, these town halls—centered on the interaction between citizens and politicians—served as a forum in which citizens' opinions are taken into account, a form of institutionalized "dignified public expression" (Paller 2019). Although we do not find higher vote shares for the candidate conducting them (on average), town halls did manage to more effectively communicate and mobilize voters relative to their usual campaign approach.

Finally, and relatedly, the article illustrates some of the limits to vote buying as an electoral strategy by showing how town-hall meetings not only rendered vote-buying attempts ineffective for the distributing candidate, but actually ended up hurting him. Our findings suggest that these meetings might have changed the expectations citizens have of their leaders (Gottlieb 2016), for example, by raising the standards of political campaigning (Carlin and Moseley 2015, 15). This has been shown to be the case in Nigeria, where vote buying created disillusionment among citizens and reduced political participation (Bratton 2008), yet, contrasts with findings from Vicente (2014) who finds that handouts actually increased turnout. Further research could pin down exactly the impact of cash handouts on voter engagement and participation.

Our article differs from previous campaign interventions involving town-hall meetings in several ways. First, a previous intervention involving town halls conducted in 2006 found that programmatic platforms might be at least as effective as clientelist ones in mobilizing turnout (no differences).[6] However, in that case, the experiment had a relatively small sample size, limited regional coverage, and did not uncover the causal mechanism through which town-hall meetings improve electoral support (Fujiwara and Wantchekon 2013). In contrast, the sample analyzed in this article includes districts from all provinces in the country and involved the top three candidates in the election. We also collect information on the two main mechanisms (coordination and information) to better explore the channels of causality hypothesized in Fujiwara and Wantchekon (2013). Finally, the presence of broad-based as opposed to issue parties in national elections gives these findings a wider applicability than those presented in López-Moctezuma et al. (2022), which examines deliberation mechanisms in closed-party list legislative elections in the Philippines.

Overall, our findings suggest that town-hall meetings are generally an effective means of transmitting political information and promoting participation, in line with previous interventions of this type.[7] However, the electoral impact of town-hall meetings is likely shaped by institutional features such as the party system and preexisting levels of political competition. For instance, López-Moctezuma et al. (2022) show that issue-based parties can use town halls to elicit support from particular constituencies in the Philippines. In this article, the broad-based nature of the parties involved likely explains the across-the-board mobilization and not that of particular demographics—highlighting the importance of the party system. Likewise, town halls in more competitive environments have a larger impact on raw votes, with evidence of this in Fujiwara and Wantchekon (2013) and a lack of it in our own sample, which privileged candidates' strongholds. Finally, executive versus legislative and national versus local elections may elicit different levels of interest and excitement which may extend to voters' response to the intervention.[8] Future research can further explore the applications (and limitations) of town-hall meetings as a campaign instrument.

CONTEXT

The experiment took place in Benin, a country in sub-Saharan Africa that transitioned to democracy in the early 1990s by relying on "national conferences" as a mechanism to gain support from major political forces. These conferences facilitated negotiations between stakeholders and the military regime, leading to a peaceful transition. This historical detail is relevant to our intervention as it also involved a similar national policy conference, with the three major political parties seeking a consensus on pressing policy issues of the 2011 campaign, and to gain consent for the experimental intervention.

In fact, Benin ranks among the top 10 most democratic countries in Africa, and is a case in which certain pre-conditions for conducting town halls are met: vibrant and competitive elections, a commitment to democracy as the preferred form of government,[9] with most of the population (70.6%) identifying Benin as such.[10] In our own survey, a high percentage (72%) says they would not feel embarrassed (gêné) of voting for someone outside their ethnic group, suggesting that

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[6] Similar to Fujiwara and Wantchekon (2013), we find no effect on official vote shares.

[7] Other town-hall interventions in Nigeria (Collier and Vicente 2014) led to lower perceived violence and higher turnout. In Liberia, a civic education program led to greater enthusiasm for electoral participation (Mvukiyehe and Samii 2015).

[8] A potential reason why there is no turnout bump in López-Moctezuma et al. (2022).

[9] Results from Afrobarometer 2011/2013 show that 75.9% of respondents agree with the statement "Democracy being preferable to any other kind of government."

[10] In the same Afrobarometer survey, 43.3% of respondents say that Benin is a democracy with minor problems, whereas 27.3% consider it a full democracy (total 70.6%).

voters value and understand democracy and voting may not be a mechanical act based on ethnicity, for example.

However, alongside its democratic successes, the prevalence of vote-buying practices and clientelism is still a concern—and considered by some a drag to the country's development. For instance, analysts blame the poor economic performance in Benin on vote buying and patronage politics,[11] leading to less foreign direct investment than Côte d'Ivoire and 10 times less than Burkina Faso.[12] For some observers, the 2011 election saw rampant vote buying (Souaré 2011), particularly from the incumbent party, which was accused of electoral corruption and extreme politicization of the public administration.[13] An estimated $45 million out of $50 million was spent during the campaigns on cash distribution, gifts and gadgets, and payment to local brokers. Despite these irregularities in the process, the 2011 electoral results could be deemed as "fair" (Souaré 2011).

Aside from vote buying and clientelism, the presence of strong ethno-regional cleavages is a salient, and at times challenging, feature of Beninese politics in the post-military rule period.[14] In particular, the ethnic Bariba in the North, Yoruba in Southeast, and Fon in South-Central regions have vied for power in all of the recent elections (Adida et al. 2016). In fact, similarly to the 2006 presidential elections, the 2011 one was also a contest along regional lines: the top three candidates were Yayi Boni, a "Northerner" and former President of the West African Development Bank, running as the incumbent candidate from the Force Cowrie for Emerging Benin party (FCBE). The main opposition candidate was Adrien Houngbedji, a "Southerner," former cabinet member and the candidate of the Party for Democratic Renewal (PRD) who joined with three other parties in coalition (Union fait la nation—UN). Finally, Abdoulaye Bio Tchane (ABT), an economist and former Director of the Africa Department at the IMF, ran in a distant third place and drew most of his support from the northwest (Donga province). In the end, the incumbent candidate won in the first round with 53.64% of the vote, Houngbedji received 35.64 %, and ABT took 6.14%.

EXPERIMENTAL DESIGN

The experiment took place in three phases. The first phase involved a national conference that took place on February 5, 2011. The goal of the conference was to promote policy-centered discussions involving the three major candidates for the election and academics. The conference covered five policy issues: mathematics education, emergency healthcare, youth employment, rural infrastructure, and corruption. There were about 70 participants between representatives of the three main candidates, members of the National Assembly, Development Agencies, NGOs, and academics including the Dean for Research at the University of Abomey Calavi, an academic institution in Benin. The conference also generated five reports, one for each topic, which would serve as policy blueprints for the candidates in the upcoming campaign.[15]

In addition to the policy discussion, the conference helped build trust and transparency between the experimental team and the candidates, facilitating their participation and cooperation with the experiment. All three campaigns participated voluntarily and no deception was involved: the team was clear about what the treatment consisted of—implementing town-hall meetings in designated villages and to follow their usual campaign strategy elsewhere—and the candidates knowingly agreed to it.

Sampling

The experiment followed a randomized geographical block design with treatments assigned to 60 randomly selected subunits (villages) and 90 randomly selected villages serving as control. The sampling procedure was as follows: first, we excluded the city of Cotonou due to its high population density and the high risk of contamination between treatment and control groups. Second, with the exception of the mountainous Atakora province, we used a simple proportionality rule to determine the number of districts (communes) to be selected in each of the 11 remaining departments (provinces).[16] Using a random number generator, we selected two treatment districts in Alibori, the department with the smallest number of districts, and four from Zou, the department with the highest.[17] In total, we selected 30 communes or districts out of the 77 total in the country. Figure A.1 in the Supplementary Material shows the geographic location of selected communes (districts). With districts settled, we then proceeded to randomly select five villages per district (two as treated and three as control). This led to a total of 60 treated villages and 90 serving as control. Since our treatment only covered about 4% of villages on average, with a typical district having 52 villages, there was a limited incentive for candidates to significantly alter their

[11] Jeune Afrique (2011).

[12] See Jeune Afrique (2011).

[13] There were also claims of improper management of voter registration, particularly in opposition strongholds, which meant the election had to be postponed twice (Gilliss 2013, 55).

[14] In our own survey, respondents express less support to the idea that politicians should serve all regions as opposed to only that from where s/he is from (66% support).

[15] Local TV media coverage of the conference can be found at https://www.youtube.com/watch?v=9xXJImyrPic and https://www.youtube.com/watch?v=MvlLDLNMJs8.

[16] There are 12 provinces in the country; we excluded Cotonou from the analysis.

[17] The total number of districts in the analysis per province is: Mono: 3; Donga: 2; Plateau: 2; Collines: 3; Kouffo: 3; Atakora: 2; Zou: 4; Alibori: 2; Oueme: 3; Borgou: 3; Atlantique: 3. Littoral (capital Cotonou) was excluded.

campaign strategies or for other candidates to contest these villages in response to town halls.

Once districts and villages were drawn, we assigned them to the three front runners of the election based on two rationales. The first one was to minimize any potential social harm from the experiment itself. Of special concern was the possibility of significantly altering electoral outcomes. The second rationale was to try to make the assignment's goals compatible with the candidate's incentives—as to ensure maximum compliance with the experimental protocol. In other words, the intervention should not astray too far from the strategy candidates would have followed anyway. For this purpose, candidates were assigned to conduct town halls in randomly drawn villages that fell within their regional strongholds, as it would minimize potential harmful impacts and ensure maximum compliance.

Based on the strong regional cleavage present in Benin (Adida et al. 2016), the incumbent Yayi was assigned to implement the town-hall treatment in his Northern strongholds (provinces of Alibori, Atakora, Borgou, and Collines) where its pre-electoral preference averaged 95.7% in the assigned districts within these provinces—and which were also the main source of support for his 2006 victory. He would end up winning the election with no less than 73% of the vote in each of these provinces. Table A.1 and Figure A.2 in the Supplementary Material list the districts assigned to each candidate and their geographic location, respectively.

In turn, the main opposition candidate Adrien Houngbedji (UN coalition) was assigned to districts in his own Southern strongholds: Atlantique (where his father is from), Kouffo, Mono, Oueme (where he is originally from), Plateau, and Zou—all neighboring provinces.[18] In the selected districts from these provinces, the pre-electoral survey suggested an average support of 62.8%, higher than his final national vote share of 36%—and consistent with their opposition stronghold status. More importantly, the assignment to these southern districts was compatible with his own strategy of trying to "unify and rally the South in his support" (Adida et al. 2016; Soudan 2010).

In the same vein, the third candidate, Abdoulaye Bio Tchane (ABT)—a long-shot challenger who was also a northerner—was mostly popular in northern districts among the incumbent strongholds of the Atakora and Donga provinces (the latter his place of birth). Although our pre-electoral survey puts his support in around 9% nationally (ultimately he would earn 6% of the vote), he reached 40% in our pre-electoral survey in these districts. While he was not necessarily viable to win the presidency, he was closely watched as someone who could potentially split the incumbent vote (Souaré 2011) providing a path for Houngbedji (UN opposition party). The implementation of the town-hall meetings took place in the 2 weeks between February 20 and March 6, 1 week prior to the election on March 13, 2011. A map of treatment and control villages and districts can be seen in Figure 1.

Treatment

A team of one research assistant from the Institute of Empirical Research in Political Economy and one activist working for the candidate generally organized three meetings in each of their assigned treatment villages.[19] The substantive content of these meetings would be the same across all candidates—based on the agreed February 5th policy blueprints and implemented in full collaboration with the assigned candidates' team. Villagers were informed of the date and the agenda by a village crier. Table 1 shows basic descriptive statistics of the meetings.

According to the meetings' attendance sheet, the typical meeting was attended by around 43–49 individuals, each representing around 10% of registered voters in treated villages, on average.[20] Due to reporting issues, there are missing attendance rosters from 11 villages in which town halls took place; therefore, we show that our main results are robust to instrumenting villages with complete data using the randomized assignment (Panel E of Table A.6 in the Supplementary Material). From the available data, we nonetheless know that attendees were relatively diverse in terms of gender: although males made up a majority of participants on average, Table 1 shows that female participation grew as the meetings progressed. In terms of topics, the first meeting generally focused on Health and Education, the second one focused on Rural Development, Jobs, and Unemployment, whereas the third one focused on Political Governance.[21]

The meetings proceeded as follows: the research team introduced the main topic in light of the blueprint of the February 5th conference, listened to the public's concerns, and made some policy proposals in response. Villagers debated the policy proposals among themselves and with the representatives leading to further recommendations. In some cases, groups of villagers, especially women, caucused prior to the town hall to discuss the relevant issues and a representative spoke on their behalf at the actual meeting. In all cases, attendees made suggestions for improving the proposals and even put forward new ones.

For example, the Health and Education meeting at the Bobo-Bobo village in the Karimama district—conducted with the incumbent's team—started by highlighting the main problems in health and education—namely, the "lack of qualified teachers," "lack of students," and "lack of monitoring of children" in these schools. The campaign then proposed to "establish schools closer to the village," to "improve schools' infrastructure," and to

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[18] There is no overlap in provinces assigned to the incumbent and the main opposition party.

[19] In seven villages, there was an additional fourth meeting, in three of these cases, it served as a recapitulation of previous topics, whereas in the other four, it was a continuation of a previously discussed topic.
[20] Under the assumption of no repeats, a maximum of 34% of registered voters on average could have personally attended.
[21] The order of the topics but not the content itself varied slightly in some villages.
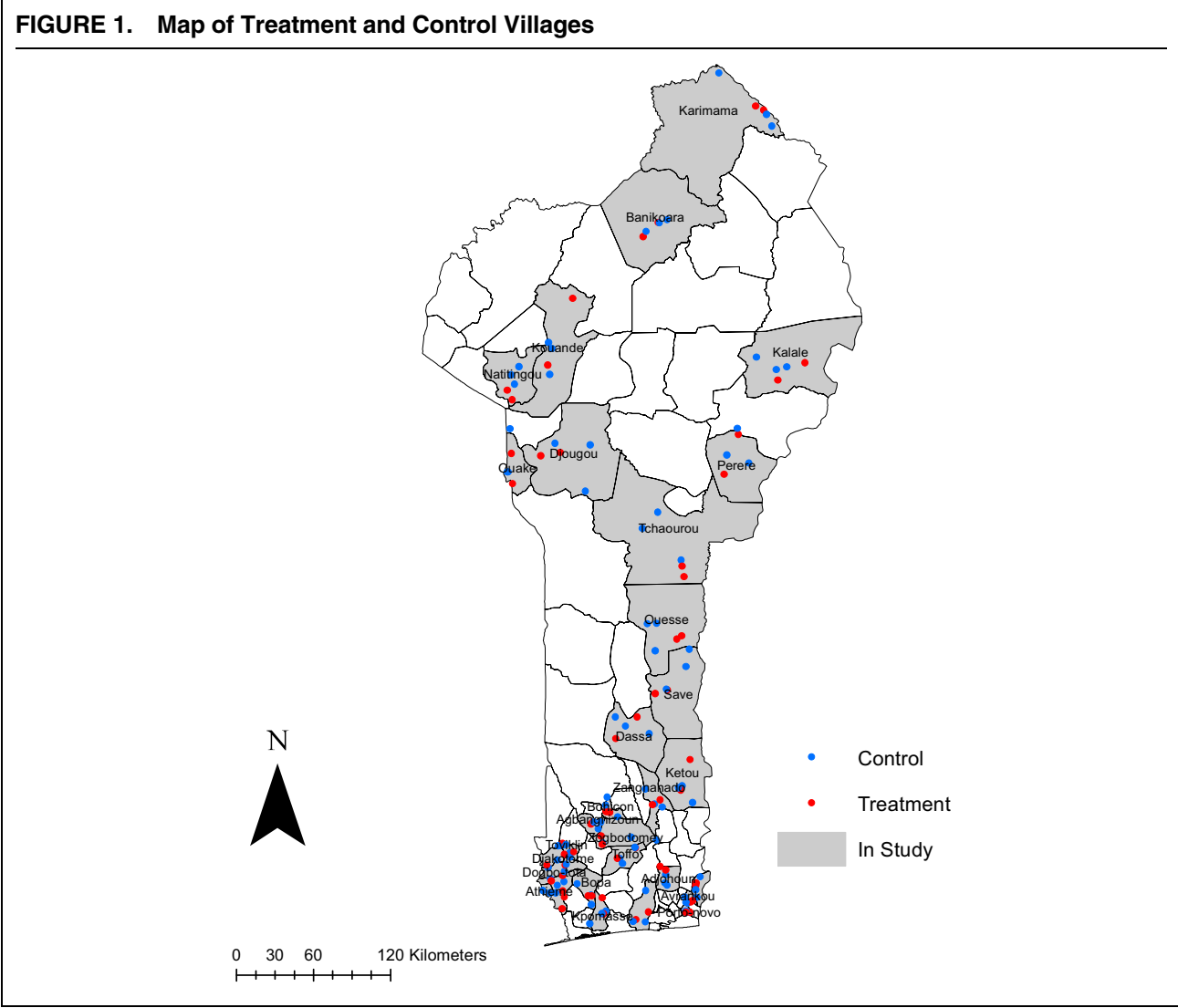
FIGURE 1. Map of Treatment and Control Villages

TABLE 1. Town-Hall Descriptive Statistics

	(1)	(2)
Meeting #	Modal Topic	Mean # Participants
Meeting 1	Health and Education	Total: 43.64 (24.9) Male 27.04; Female 16.6
Meeting 2	Rural Development, Jobs, and Unemployment	Total: 49.1 (24.7) Male 26.8; Female 22.2
Meeting 3	Political Governance	Total: 48.13 (26.9) Male 23.9; Female 24.22

Note: Standard deviation in parentheses. Due to reporting issues, the attendance sheet and policy summary for 11 meetings that took place are missing. In addition, two villages have missing attendees data for one of the meetings.

"create a supervisor administrative position in the town center to keep children in school." The debate was then open to participants and further recommendations were made, such as "improving the recruitment and training of teachers," "repairing the main road that leads to the village," "providing financial assistance to farmers so that they can maintain their children at school," and "build a new health center."

Through this back and forth, participants at the meetings suggested amendments to the proposal putting them in a more local context. For example, while some villages suggested that medical care should be improved by focusing on prenatal care, others raised the problem of snake bites. The team then summarized the main points raised during the meetings in a written report to be transmitted to the candidate via his campaign manager. Each meeting lasted about 90 minutes. There was no cash distribution during the town-hall meeting and neither the presidential candidate, nor a major political figure such as the local mayor or MP was in the audience.

Control

In villages selected to serve as control, candidates were told to follow their usual mode of campaigning, which does not include town-hall meetings. This often involved a local mayor, MP, or a political figure (the local broker) organizing two to three rallies, sometimes in the presence of the candidate himself. In these events, the representative of the candidate (or the candidate himself) made a speech that outlined the policy agenda. A typical rally began with 30 minutes of music, followed by the introduction of the speaker for 10 minutes. The speaker then discussed a proposal for about a half hour. For example, in one village, the incumbent outlined his education policy for 30 minutes, particularly his plan to build new classrooms and educational infrastructure across the country. After the speech, there was another 30 minutes of dancing and music before the rally ended. There was no debate or audience participation, but instead a festive atmosphere of celebration with drinks, music, and sometimes cash and gadget distribution. Participants came from several villages and attendance varied from eight hundred villagers to three thousand or more (if news spread). The rallies lasted a total of about 2 hours. Thus, rallies were a type of one-way communication strategy of platforms combining programmatic and, in some cases, clientelist campaign promises.

Remark

Based on these descriptions, the treatment differed from the control in the following: (1) In contrast to the usual mode of campaigning, town-hall meetings are two-way communication events between candidates and voters: participants are introduced to the candidate's platform, ask clarifying questions, and adapt and amend the platform based on local conditions. Participation is entirely voluntary. (2) A rally naturally draws far more people than a town-hall meeting. (3) While town-hall meetings cost about $2 per participant, a rally costs at least $15 per participant (based on our estimates). (4) Every rally is run by a local or national celebrity (the mayor, MP, or a broker), often involving some form of cash or gift distribution, with the candidate being sometimes present.[22] While the first two

differences are essential elements of the treatment, the last two may work against finding a positive treatment effect on certain political outcomes, such as turnout.

DATA AND ESTIMATION

Our empirical analysis uses different sources of data. First, prior to conducting the town-hall meetings, we surveyed prospective voters in all 60 treatment villages and 90 control villages and collected pre-treatment demographic, political, and economic information, such as age, gender, ethnicity, education level, occupation, and political preferences and knowledge. The survey mostly took place between February 20 and 28 and had a total of 4,572 respondents. Second, during the town halls, we collected information on attendees and their gender composition, topics, proposals, and recommendations for descriptive purposes as shown in Table 1.

After the election, we returned to these villages and ran a second survey collecting information on candidate and platform knowledge, views on the campaign, electoral preference and participation, as well as the prevalence of cash handouts, among others.[23] The goal of the post-electoral survey was to explore the causal mechanisms by looking at whether particular groups of voters were more (less) impacted by the treatment and had 5,082 participants. All surveys were conducted with the consent of respondents.

Finally, as soon as the polls closed, the research teams also went to the relevant polling stations to record turnout and electoral support for the candidates involved in the experiment in all 30 communes and 150 villages, generating village-level measures of electoral outcomes which are otherwise unavailable.

One initial question about the data is whether there are systematic differences between the official election returns and self-reported voting behavior—for example, arising from the survey sampling. At first glance, it is clear that official participation rates and vote shares for treated candidates were lower on average (87% and 52%, respectively) than those reported in the post-electoral survey (94% for turnout and 70% for vote choice). Albeit other work in Benin document similar discrepancies for the 2015 legislative elections (Adida et al. 2019), in Table A.2 in the Supplementary Material, we examine whether these differences vary with the treatment assignment. As shown, this is not the case for our turnout data, yet self-reported support for opposition candidates appear to be systematically higher than official figures in opposition treated areas. Because the latter could be driven by supporters of opposition candidates oversampled in the survey,

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[22] By not getting the local broker directly involved in the town-hall meetings and not distributing cash and/or gadgets to participants, we

were in fact working against a positive treatment. The presence of the mayor, the MP, or a candidate himself would probably have boosted the audience, and gifts to the participants would likely not have turned them against the candidate.
[23] Our enumerators started fielding the survey the day after the election (March 14) and ran it until March 31, with the bulk of the interviews conducted between March 16 and 18.

TABLE 2. Covariate Balance: Respondents in Treatment and Control Villages					
	(1)	(2)	(3)	(4)	(5)
Variable	Treat mean	Control mean	N	Treat effect	p-value
	Individual-level data				
Male	0.606	0.586	4,572	0.019	0.154
Age	36.97	37.08	4,409	−0.186	0.748
Fon ethnicity?	0.380	0.384	4,572	−0.002	0.962
No. of languages	1.995	1.943	4,571	0.052	0.180
Any education	0.488	0.461	4,561	0.028	0.199
Marital status, 1=monogamous, 0 = else	0.432	0.415	4,571	0.017	0.332
Political information: know mayor	0.711	0.693	4,559	0.015	0.482
Political information: know president (Yayi)	0.959	0.964	4,546	−0.005	0.487
Will vote upcoming elections?	0.968	0.975	4,526	−0.007	0.407
Prefer treated candidate?	0.72	0.68	4,184	0.032	0.115
Currently employed?	0.560	0.524	4,511	0.038	0.047
Do you enjoy a regular income?	0.222	0.226	4,371	−0.007	0.661
Are you a farmer?	0.526	0.511	4,510	0.010	0.652
	Village-level data				
Registered voters	782.98	922.76	150	−139.78	0.135

Note: Specifications include commune (block) fixed effects and are estimated using ordinary least squares. *P*-values clustered at the village level are reported in column 5.

throughout the analysis, we focus on the official administrative results.

Estimation

Given the nature of the data, we estimate the treatment effects using the following equations. First, for village-level outcomes:

$$Y_{jc} = \alpha_c + \beta_1 T_{jc} + u_{jc}, \qquad (1)$$

where Y_{jc} is the village j turnout or vote share for the treated candidate in commune c. In all specifications, we include commune (district) fixed effects α_c to account for stratification. The key independent variable is T_{jc}, the treatment, which takes a value of one if the village is in the treatment group and zero if it is in the control group. All standard errors are clustered at the village level (which is the level of variation of the treatment). When using the post-electoral survey to assess the effect of town-hall meetings on other outcomes, our specification differs from Equation 1 by adding the subscript i, such that

$$Y_{ijc} = \alpha_c + \beta_1 T_{jc} + u_{ijc}, \qquad (2)$$

where Y_{ijc} is the response of individual i, village j, and commune c. We again include commune/district fixed effects α_c to account for stratification. As in Equation 1, the key independent variable is T_{jc}, and we also always cluster the standard errors at the village level.

Covariate Balance

One concern with the analysis is that of possible imbalance between treatment and control groups. Specifically, the sampled villages could exhibit preexisting heterogeneity, even with randomization, that would confound our results. In Table 2, we test the null hypothesis of no statistically significant difference in pre-treatment variables (on average) between treatment and control groups across a range of demographic, political, and socioeconomic variables potentially correlated with political behavior.

As shown, respondents assigned to treatment and control exhibit similar traits on average. Regarding demographics, there are no significant gender, age, marital, education, or ethnicity differences among the two groups. In terms of economic characteristics, treatment villages seem to have more currently employed individuals, although there are no differences among those receiving a regular income and those working in the agricultural sector. More importantly, there are little preexisting differences in levels of political information—measured by whether respondents know the mayor of their village or the incumbent President (Yayi).

There are also no differences in (i) the intention to vote in the upcoming elections and (ii) whether they preferred the candidate that will run the town halls (at this point of the campaign) in both treatment and control groups. The latter is reassuring given political engagement, preferences, and information are key outcomes and mechanisms of the town-hall treatment.

Finally, there are no significant differences in village-level average number of registered voters, thus showing we are not necessarily including larger (smaller) villages.

In addition to balance in the whole sample, Tables A.3 and A.4 in the Supplementary Material look at covariate balance between treatment and control in communes assigned to opposition candidates (Table A.3) or to the incumbent (Table A.4). The findings largely mirror those of the whole sample with some marginally statistically significant effects in expressing preexisting support for the treated candidate ($p = 0.08$) in opposition districts and a marginally more male ($p = 0.083$), more educated ($p = 0.056$), and less willingness to vote (p=0.09) in treated incumbent districts, with the latter difference running against our hypothesized effect. Although it appears that the sample restriction maintains the randomization in the subsamples in all other aspects, we nonetheless include these and other covariates as robustness exercises.

Manipulation Check

Finally, before delving into our findings, Table A.5 in the Supplementary Material verifies that our treatment (town-hall meetings) actually had the intended effect and induced the response it was supposed to. Based on our post-electoral survey, columns 1–3 of Table A.5 show that respondents in villages with town-hall meetings were significantly more likely to recall meetings organized by political activists in their village than respondents in control villages. However, likely due to the way the post-electoral survey was implemented,[24] the self-reported likelihood of attending is higher than the maximum number of potential attendees (assuming no repeats)—32% or 34% of registered or actual voters on average, respectively—thus suggesting over sampling of town-hall attendees.[25] However, because we rely exclusively on the village-level randomized assignment (ITT) and not on self-reported attendance, this is less of a concern for our main village-level estimates. Furthermore, in all our individual-level specifications, we always adjust for oversampling of meeting attendees by weighting our estimates by the proportion of self-reported attendees relative to independently recorded ones (by the experimental team) using inverse probability weighting.

RESULTS

In this section, we examine the impact of town-hall meetings on political behavior. Our first dependent variable of interest is turnout. This is a fundamental variable for the functioning of democracy, and has generated a great deal of interest in experimental political science (cf. Gerber and Green 2000; Green and Gerber 2005; 2019). Normatively, political participation is seen as key to the legitimacy of democratic outcomes, with poor turnout potentially reflecting citizen's alienation from public life or a lack of civic engagement. Instrumentally, high turnout is seen as a key to the success of certain candidates, particularly in competitive elections. Finally, in a context such as Benin, where most voting is considered to take place along ethnic lines (Adida et al. 2016) or driven by clientelist (Wantchekon 2003) or vote-buying offers (Nichter 2008), it is important to explore other drivers of political participation.

Turnout

Figure 2 starts by presenting the official political participation data in treatment and control villages. Turnout tended to be very high, greater than 80% in most villages albeit visibly higher in treated (solid) rather than control villages (dash). This turnout rate is comparable to the one in the 2006 elections, which was of about 88%. There was even 100% turnout in 12 of the 150 villages in 2011 due to same day voter registration.[26] In addition, a number of control villages had turnout rates between 40% and 60%, which was not the case in treated locations.[27] In this sense, town-hall meetings reduced the likelihood of very low turnout.

In Table 3, we present the ordinary least squares estimates of the treatment effect on turnout as measured by official statistics. Column headers indicate whether we are referring to the entire sample (Overall), to a sample in which an opposition candidate was treated either from the UN or ABT party (Opposition), or to villages where Yayi was the treated candidate (Incumbent). Since our measure of turnout is the proportion of registered voters casting a vote, the results presented in Panel A suggest that participation is approximately 3.3 percentage points (pp) higher in treatment villages than in control ones. In turn, villages in opposition communes exhibit a 2.65 pp difference in turnout in contrast to their control counterpart. In other words, turnout was much higher in villages where the opposition parties (ABT or UN) organized the town-hall meetings. However, the effect on turnout for villages in which the incumbent (Yayi) organized the meetings is actually larger in magnitude (around 5 pp) but less precisely estimated likely due to the lower number of villages. Given the high rates of turnout in

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[24] The survey was conducted using a "random walk" procedure starting from the center of each village and randomly choosing the direction and dwelling that would be interviewed. The quota was of 40 respondents for treated villages and 30 for control ones.

[25] We thank an anonymous referee for pointing this out. Oversampling would occur if those closer to the village center are also more likely to attend the town-hall meetings.

[26] These cases were equally distributed between treatment (6) and control (6). Prior to the election, the government updated the country's voter roll, allegedly to selectively register voters in the incumbents' strongholds while removing those in opposition areas (registered voters went from 4.3 million to 3.5 million in 2011). The update led to boycotts by opposition parties (Souaré 2011, 85–6) and to election day adjustments.

[27] Results are robust but less precise to excluding one village in the control with extremely low turnout. This village also exhibited very low turnout in the 2015 legislative elections.

FIGURE 2. Turnout
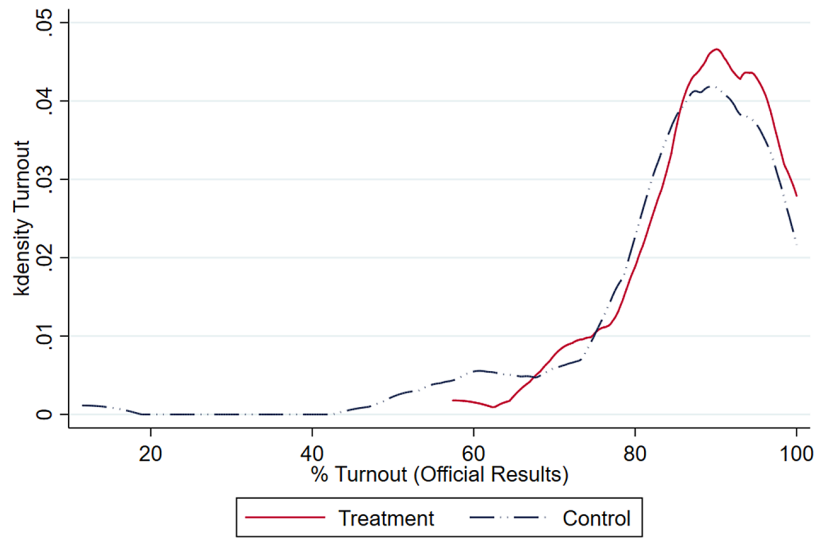
TABLE 3. Treatment Effect on Turnout

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All	Opp	Inc	All	Opp	Inc	All	Opp	Inc
	Panel A			Panel B			Panel C		
	DV: Village Turnout						DV: Survey Turnout		
Treatment	3.309** (1.619)	2.653* (1.572)	5.111 (4.301)				2.098* (1.124)	2.154 (1.301)	1.824 (1.885)
% Village attendees				8.854*** (3.242)	8.541*** (3.234)	13.628 (18.582)			
No. of obs.	150	110	40	139	106	33	4,587	3,540	1,047

Note: ITT Estimates. Village-level data. Robust standard errors in columns 1–6. Robust standard errors clustered at the village level in parentheses in columns 7–9. All specifications include a commune fixed effect. The sample size for columns 4–6 is smaller as there was missing detailed information on the meetings for 11 villages. Columns 7–9 are weighted by the self-reported over actual attendance to town-hall meetings. Turnout and Survey Turnout values are rescaled to values 0–100 to ease interpretation. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

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general—around 85% for control villages—these effects are far from negligible.[28]

As a cross-validation exercise, Panel B shows that the effect is stronger in villages where the share of town-hall attendees relative to registered voters is higher.[29] The latter is particularly the case in villages in which the opposition candidates were treated. To fix ideas, on average, turnout would be 8.8 pp higher in a hypothetical village in which a 100% of registered voters attended the meetings. Although we do not know the actual share of attendees (only the maximum possible

in a village),[30] it still suggests that the treatment effect is larger in villages where more people attended and (or) where enthusiastic groups of attendees participated multiple times. Finally, Panel C shows similar effects when using self-reported turnout from the post-electoral surveys.

Other robustness exercises show that estimates are larger and more precise if we weight our estimates by the share of attendees to the town halls vis-à-vis registered voters (Panel A of Table A.6 in the Supplementary Material).[31] Similarly, estimates are similar to those in the baseline, albeit less precise when excluding

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[28] For the average village with 780 voters, it would represent 23 more votes.

[29] The total number of attendees is the sum of recorded attendees across all meetings.

[30] We only know total attendees, not total unique attendees.

[31] This adjustment de-weighs villages where smaller fractions of registered voters attended the town hall.

TABLE 4. Treatment Effect on Vote Choices

	(1)	(2)	(3)	(4)	(5)	(6)
	Overall	Opposition	Incumbent	Overall	Opposition	Incumbent
	DV: Village Vote Share			DV: Survey Choice		
Treatment	−0.196	−0.048	−0.604	7.312***	9.548***	−2.949
	(2.128)	(2.514)	(4.047)	(2.423)	(2.760)	(3.396)
No. of obs.	150	110	40	4,130	3,139	991

Note: ITT Estimates. Robust standard errors in parentheses for columns 1–3. Robust standard errors clustered at the village level in parentheses for columns 4–6. All specifications include a commune fixed effect. Columns 1–3 use village-level data. Columns 4–6 use post-electoral survey data and are weighted by the self-reported over actual attendance to town-hall meetings. Village Vote Share and Survey Choice are rescaled to values 0–100 (%), to ease interpretation. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

extremely low turnout villages (Panel B) or those with 100% turnout due to the same day registration (Panel C). However, we find larger coefficients when accounting for the pre-treatment covariates marginally unbalanced in Tables A.3 and A.4 of the Supplementary Material such as the pre-electoral intention to vote as well as gender, education and employment status (Panel D). In Panel E, we also adjust our turnout measures to account for discrepancies whereby the total reported party votes are larger than the reported sum of votes.[32] In doing so, the overall effect is similar to the baseline. Finally, results are robust to instrumenting treated villages for which we have the complete meetings data with the randomized assignment (Panel F), leading to larger estimates.

Vote Choice

In the presence of higher political participation, did these translate into increased support for the treated candidates? Below we present the results from estimating Equation 1 using official vote-share records at the village level, which we then cross-check using individual-level responses from the post-electoral survey (Equation 2), with the caveat that the latter appears systematically larger than what official results would suggest for opposition candidates.

As shown in Table 4, there are no differences in average vote shares among treated and control villages when looking at administrative data (Panel A), yet there are sizeable differences when using post-electoral surveys (Panel B). Specifically, village-level data coefficients tend to be small and imprecisely estimated. Even after adjusting our vote-share measures taking into account these discrepancies in vote tallies, we find no treatment effects.[33] In contrast, self-reported vote choices show a positive and significant effect of around 9 pp, especially for opposition candidates. This

discrepancy is likely due to the survey sampling procedure, which could have led to a particular set of voters in opposition communes, leading to differences with the aggregate voting results. Nonetheless, the survey can still inform us some of the channels or mechanisms through which town-hall meetings impact voting behavior.

Overall, the analysis of electoral outcomes shows that the treatment may have persuaded some citizens to turn out to vote but do not necessarily translate into higher *official* vote shares for treated candidates. Given that in these villages, the likelihood of pivotality and the instrumental value of voting are also very small (by design), it suggests that higher political participation is likely driven by changes in its inherent valuation. Moreover, since we do not see a corresponding increase in vote shares, it is likely that this mobilization effect occurred across-the-board and not privileging particular constituencies that would have translated into vote-share differences. In the next section, we explicitly evaluate this possibility.

Convergence or Divergence in Voting Behavior?

One of the key debates among deliberative theorists is the extent to which these exercises might be driven or reflecting different forms of inequality (Fishkin and Mansbridge 2017, 9). For instance, certain citizens may have lower deliberative abilities that would limit their ability to contribute to the discussion (Gerber et al. 2018). Lower educational backgrounds may pose constraints to the quality and impact of deliberation (Fishkin et al. 2017). Or, societies polarized along gender or economic lines might reflect those tensions in the meeting's dynamics. For this reason, it is important to examine whether these effects on turnout and vote choice actually differ along social, economic, or demographic lines. To explore this heterogeneity, we use village-level averages of individual-level characteristics from the post-electoral survey and estimate the following model:

$$Y_{jc} = \alpha_c + \beta_1 T_{jc} + \beta_2 Z_{jc} + \beta_3 Z_{jc} \times T_{jc} + u_{jc}, \qquad (3)$$

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[32] We believe these discrepancies are due to data-entry mistakes when adding the vote totals. Nonetheless, we show the robustness of our findings to adjusting our turnout and vote-share measure accordingly.

[33] Results available in Wantchekon and Guardado (2023).

where Y_{jc} measures the village-level turnout or vote-share outcome of village j in commune c, Z_{jc} is a vector of village-averages of different individual traits (education, income, and gender), and $Z_{jc} \times T_{jc}$ is the interaction effect between the treatment indicator and the covariates mentioned above. All other terms are the same as those from Equation 1 and results are weighted by the inverse of reported to recorded meeting attendance.

Columns 1–3 of Table A.7 in the Supplementary Material look at turnout and show little variation in the effect of the treatment depending on village-level traits such as gender, poverty, or education. In general, the average effect of the treatment on turnout is positive once we condition on these controls, although coefficients may be less precise due to a larger number of variables and limited number of observations. In all, the lack of differences is a welcome finding given the legitimacy-enhancing and civic duty value attributed to political participation.

In the case of vote choices, it is theoretically possible that town-hall meetings disproportionately appeal to certain groups versus others. For instance, the information provided by town-hall meetings might lead some individuals to find certain platforms more attractive than others. Meetings focused on childhood education would resonate more among those with school-aged children. In other words, group divergence in vote choices (vis-à-vis turnout) might be less of a normatively undesirable consequence. However, in this case, we find limited evidence for systematic divergence in vote choices by sociodemographic traits such as gender, poverty, and education.[34] The only exception is the positive effect of the treatment on vote shares among those better educated in incumbent-treated villages (column 6 in Panel C). However, as mentioned above, this finding reflects more the ability of town halls to appeal to the incumbent's base in this strongholds (those with formal education and ethnically Fon, as his wife) while not demobilizing other groups.

In all, the lack of differential effects among different groups suggests that the deliberative exercise is not necessarily reproducing certain power structures—for example, disproportionately (de)mobilizing some groups versus others.[35] However, it should be noted that although these covariates do not influence voting behavior depending on treatment assignment, they still have an independent effect *on average* (Table A.8 in the Supplementary Material). For instance, villages with more educated individuals report higher turnout. Similarly, "poorer" and female respondents generally preferred opposition candidates, to name some key differences.

CHANNELS OF CAUSALITY: INFORMED AND ETHICAL VOTERS?

So far, results suggest that town halls led to higher political participation and that this effect does not vary by specific demographics. While this result is consistent with higher intrinsic valuations of political participation, in this section, we further explore the ways in which town-hall meetings could have led to more informed, engaged, public spirited, namely "ethical" voters.

Information

To start, we examine the impact of town-hall meetings on self-reported knowledge and the quality of this information in treated versus control villages using Equation 2.

As shown in columns 1–3 of Table 5, town-hall deliberation invariably led respondents to report greater knowledge of their preferred candidate platform, regardless of whether the treated candidate was the incumbent or the opposition parties. Moreover, as shown in columns 4 and 5, citizens in treated villages report greater knowledge of the candidates' platforms even in villages in which official turnout figures are below the median (column 4) or even below the 25th percentile (column 5). This suggests that the treatment had effects across the board, albeit the effect is larger in higher turnout villages. Moreover, in columns 6–8, the combination of treated villages and knowledgeable voters leads to even higher turnout rates in treated villages versus control villages.[36]

Importantly, the town-hall effects on information do not significantly vary by gender, income, or the education background of respondents as shown in Table A.9 in the Supplementary Material. The only difference is that the treatment seems to have a lower informational impact among those with higher levels of education, a largely expected result. The latter is encouraging, as it suggests these meetings are not selectively "informing" certain voters versus others. This finding also mirrors the "convergence" in electoral behavior across these demographics shown in Table A.7 in the Supplementary Material.

While Table 5 shows that the treatment led to more self-reported knowledge of the candidates' platforms, Table 6 probes whether this information is of higher *quality*. Columns 1–6 show that the treatment not only led to more general knowledge, but was especially effective in conveying *specific* information about the issues proposed and the particular traits that made a certain candidate attractive. This was particularly the case of opposition candidates: columns 3 and 4 show that those who consider the opposition candidate ABT "best" are also better able to recall specific issues and traits of the candidates to back this support, relative to those in control villages who also consider him "best."

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[34] Other cases present starker differences along these lines (e.g., López-Moctezuma et al. [2022] for the Philippines case) likely due to the reliance on single-issue parties.

[35] Previous literature (e.g., Fishkin 1997) has primarily focused on the immediate effects of deliberation.

[36] Evaluated at the mean level of self-reported platform knowledge (50.67%), column 6 implies a treatment effect of 0.799 pp, in column 7 is 1.28 pp.

TABLE 5. Treatment Effect on Self-Reported Information

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Overall	Opposition	Incumbent	Overall	Overall	Overall	Opposition	Incumbent
	DV: Know Platform			DV: Know Platform		DV: Official Turnout		
				> 50th pct turnout	> 25th pct turnout			
Treatment	0.138***	0.122***	0.215***	0.160***	0.133*	−10.118*	−15.810***	−3.034
	(0.024)	(0.025)	(0.058)	(0.040)	(0.071)	(6.083)	(5.904)	(15.497)
Treatment × "Know Platform"						0.215**	0.336***	0.068
						(0.106)	(0.107)	(0.227)
"Know Platform"						0.050	−0.058	0.222
						(0.090)	(0.088)	(0.233)
No. of obs.	4,383	3,328	1,055	2,206	1,109	150	110	40

Note: ITT Estimates. Robust standard errors clustered at the village level in parentheses in columns 1–5. Robust standard errors in parentheses for columns 6–9. All specifications include a commune fixed effect. Columns 1–5 use individual-level data from the post-electoral survey and therefore are weighted by the self-reported over actual attendance to town-hall meetings. Columns 6–8 use village level averages. "Know Platform" takes the values 0–1 in columns 1–5 (individual-level data) and 0–100 in columns 6–8 as the share of respondents reporting platform knowledge in the village. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE 6. Treatment Effect on Specific Knowledge of Preferred Candidate

	(1)	(2)	(3)	(4)	(5)	(6)
	DV- Preference and Specific Knowledge					
Treated	Opp. UN traits	Opp. UN issues	Opp. ABT traits	Opp. ABT issues	Incumbent traits	Incumbent issues
Treatment	0.060**	0.037**	0.060*	0.048***	0.026	−0.065
	(0.028)	(0.018)	(0.030)	(0.016)	(0.052)	(0.043)
No. of obs.	2,760	2,760	600	600	1,060	1,060

Note: ITT Estimates. Robust standard errors clustered at the village level in parentheses. All specifications include a commune fixed effect. The two main dependent variables take the value of 1 if the self-reported reasons for why s/he prefers this candidate are his personal traits (columns 1, 3, and 5) or his campaign issues (columns 2, 4, and 6), 0 if there is no specific information reported. All specifications are weighted by the self-reported over actual attendance to town-hall meetings in that village. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

However, the coefficient on traits appears larger than that related to issues, suggesting that the former might be easier to recall than policy proposals. We find no similar impact in the case of the incumbent (columns 5 and 6). Further, robustness shows that the treatment effect on information is similar if not larger when including pre-treatment political knowledge.[37] In all, findings from Tables 5 and 6 support the claim of town-hall meetings transmitting better political information than that obtained via normal campaigning.

Additional probes of this mechanism show that this knowledge did not necessarily come from increasing the number of interlocutors in their village or house-

hold.[38] Neither did the treatment impact the number of sources from which respondents received their political information.[39] If anything, the treatment may have reduced the frequency of discussions, particularly in places where the opposition candidates organized the town halls. The latter does not mean that information sharing from meeting attendees to others did not take place or did not matter, but rather that these took place within their usual networks and at the usual intensity rather than leading to new interlocutors or more frequent discussions.

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[37] Knowledge of the incumbent and of their town mayor; see Table A.10 in the Supplementary Material.

[38] "Do you discuss politics with… members of your household/ village?"

[39] For example, combining the number and intensity of interlocutors from out of their village / their own ethnicity / or from another ethnicity. Results available upon request.

Alternative Accounts

While information is a plausible driver of the results, it is also possible that the public nature of town-hall meetings led individuals to coordinate on a specific action (e.g., turning out to vote). However, results from Table A.11 in the Supplementary Material show that the treatment effect did not vary depending on different proxies of active coordination such as discussing politics with their village (Panel A) or household (Panel B). Instead, it is the presence of town-hall meetings and greater platform knowledge a key driver of the result. To illustrate, Panel C of Table A.11 presents a horse race comparison of the effect of platform knowledge vis-à-vis proxies of coordination showing that the treatment effects are largely driven by those with higher self-reported platform knowledge. While this finding rules out active forms of coordination, it does not preclude the possibility that town halls changed the expectations of voters about others' behavior, a different form of coordination (Adida et al. 2020).

Finally, descriptive evidence from those attending the town halls also credits the meetings with providing greater information in a greater degree than facilitating coordination. For example, 93% of those who report attending the meetings say it helped them have better information about what the candidate would do once in office. In contrast, a lower (but not negligible) share of attendees (55%) report the meetings helped villagers "know what other villages think"—a proxy for coordination. Albeit in line with the mechanism proposed, these responses should be interpreted descriptively rather than causally as they come from the self-selected sample of attendees.

In all, town-hall meetings appear to better convey the content of the candidates' platforms, thus potentially having a persuasive effect on their willingness to turn out to vote even if the instrumental impact of their vote is low. The question is: do these translate into other behaviors consistent with the goals of deliberative democracy?

Vote-Buying Attempts

In this section, we examine how town halls may have impacted the effectiveness of vote-buying attempts. For instance, it is possible that town-hall meetings made citizens *less* receptive to vote-buying attempts in general. Since town-hall meetings may lead individuals to value the democratic process for intrinsic reasons, they could come to "[…] see vote-buying as a direct affront to their political belief system" (Carlin and Moseley 2015, 15). Participants could have also perceived a cognitive dissonance between the stated purpose of town-hall deliberations—programmatic appeals—and other (mis)behavior of politicians.

To probe whether town-hall meetings impacted the distribution of cash handouts, we examine whether there are heterogeneous effects depending on the presence of cash distribution. The latter is highly plausible, given that an estimated $50 million was spent by the candidates during the election, some of it likely on targeted

vote buying. For instance, it was well known that one of the strategies of the incumbent was to engage in this practice to consolidate the northern regions' vote and secure his reelection (Souaré 2011).

As a starting point, we should note that there is no difference, on average, between the share of respondents who received money versus those who did not in treated versus control villages. For example, a simple difference in means shows that around 29.80% of respondents in control villages received a handout, while this share is 29.79% in treated ones. Even after conditioning on those who attended a town-hall meeting in treated villages, the differences are negligible: 29.4% of those who attended a town-hall meeting were targeted with cash handouts, while 30.1% of those who did not attend were offered cash. The difference is, therefore, small and not significant at the 95% level ($p = 0.3$). These lacks of differences persist even after weighting by the number of respondents in the sample. The latter suggests that the distribution of cash handouts was largely orthogonal to the intervention itself, and driven by other political calculations.[40]

Table 7 examines the electoral impact of the treatment depending on whether the respondent received a cash handout or not. The results from columns 1–3 show that money had *no* differential effect on turnout at the village level. This result stands in contrast with results showing the importance of cash handouts for turnout "buying" (Kramon 2009; Nichter 2008) and highlights the potential limits of this strategy in the presence of other modes of campaigning, namely deliberative town-hall meetings.

In terms of vote shares, columns 4–6 show that cash handouts actually have a negative effect on vote shares for the treated candidate, particularly for the incumbent. Under the very plausible assumption that the incumbent—due to its outsized resource availability—is primarily distributing cash handouts in these villages, this suggests that cash handouts actually undermined the incumbent and benefited the opposition.

In fact, given villages assigned to the incumbent were actually his strongholds (their average pre-electoral preference is 94%), it supports qualitative accounts that cash handouts were more likely to be distributed by his campaign.[41] In contrast, cash handouts have a smaller and less precise effect in opposition districts, but in the same direction, consistent with their more limited resources and allegedly lower reliance on this strategy in the first place. It should also be noted that the effect of cash handouts is not driven by simply capturing the effect of propaganda or other campaign paraphernalia, as the coefficients remain similar when controlling for these measures (Table A.12 in the Supplementary Material).

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[40] The distribution of cash handouts in treated villages would not be necessarily come from rallies, as treated candidates agreed to not use them in a treated village. However, respondents might have obtained it through other means such as targeted exchanges.

[41] Given the already strong leaning for the incumbent, it would be less efficient to distribute cash handouts in these locations by the opposition.

TABLE 7. Voting Behavior, Policy Deliberation, and Vote-Buying Attempts

	(1)	(2)	(3)	(4)	(5)	(6)
	Overall	Opposition	Incumbent	Overall	Opposition	Incumbent
	DV: Turnout			DV: Vote Share		
Treatment I	2.055	1.266	2.895	5.839	3.767	12.584*
	(3.108)	(2.885)	(7.676)	(3.705)	(4.679)	(6.367)
Treatment × Cash Handout	0.042	0.045	0.091	−0.202**	−0.124	−0.546***
	(0.078)	(0.076)	(0.204)	(0.087)	(0.102)	(0.188)
Cash Handout	0.051	0.055	0.008	−0.174*	−0.219*	0.038
	(0.072)	(0.079)	(0.160)	(0.094)	(0.118)	(0.136)
No. of obs.	150	110	40	150	110	40

Note: ITT Estimates. Robust standard errors in parentheses. All specifications include a commune fixed effect and use official turnout and vote share data as well as village-level aggregates from the post-electoral survey. The Cash Handout variable is rescaled to values 0–100 (percentage of offers per village) to ease interpretation. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

A key question of this analysis is whether the negative electoral impact of cash-handouts in treated villages is driven by higher standards of campaigning among politicians. To explore this possibility, we use respondent's assessment of the 2011 campaign (whether it was instructive and useful versus "useless") to examine variation among those who received handouts in treated versus control villages. As shown in Table A.13 in the Supplementary Material, respondents report lower perceived instructiveness and higher "uselessness" of the campaign if targeted with handouts in treated villages relative to those in control. Although the effect is more precise in opposition-treated areas, incumbent-treated ones exhibit similar magnitudes and sign.

In all, the above estimates suggest that cash distribution may have actually undermined the electoral prospects of candidates engaged in town-hall meetings leading to its lower effectiveness. While previous findings show that cash distribution may not have a tangible impact on vote shares (Guardado and Wantchekon 2018), this article shows that the effect is not just zero but actually negative when accounting for the town-hall intervention. Interestingly, while the treatment effect on vote shares does not greatly vary by gender, education, or income, it does so by the distribution of cash handouts. Future research can thus explore the impacts of democratic norms, primed by deliberation, on the success of vote-buying efforts.

CONCLUDING REMARKS

We conducted a field experiment in Benin to investigate the effect of a deliberative campaign intervention on key normative goals of democracy. Specifically, we study the effects of town-hall meetings whereby one of our research assistants and one activist working with a presidential candidate organized three events of around 90 minutes in assigned treatment villages. During these meetings, attendees made suggestions for improving the candidates' platforms which were then transmitted to the candidate via his campaign manager (*two-way communication*). In the control villages, campaigns followed the usual script: candidates organized rallies with around 30 minutes of music, followed by the presentation of their platform for about 30 minutes and another 30 minutes of dancing and music before the rally ended (*one-way communication*).

We find that these seemingly minor differences in campaigning led to higher electoral participation—namely, individuals more willing to participate in the democratic process (turnout) and lowered the effectiveness of vote-buying. Importantly, these differences were not selectively driven by key demographic and economic traits of individuals that might have reproduced preexisting power structures.

Furthermore, we show these effects come along with more and higher quality information in treated versus control villages, which could have persuaded them to turn out to vote, despite the fact that the instrumentality or pivotality of their vote is particularly low (by design). In fact, respondents in treated villages were more likely to mention issues and traits of their preferred candidates' platform as reasons behind their support, particularly for lesser known opposition candidates. The latter is consistent with the accounts of town-hall attendees, who credit the meetings with allowing them to know better what the candidate would do once in office.

These findings further our understanding of the educational and informational role of public deliberation among its citizens—an outcome early democratic theorists strongly supported. The fact that their effects are visible after the election (to post-electoral surveys) attests to their potential use in other settings beyond that of Benin.

SUPPLEMENTARY MATERIAL

To view supplementary material for this article, please visit https://doi.org/10.1017/S0003055423000813.

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CONFLICT OF INTEREST

The authors declare no ethical issues or conflicts of interest in this research.

ETHICAL STANDARDS

The authors affirm that this article adheres to the APSA's Principles and Guidance on Human Subject Research. This research project was conducted by the Benin-based IREEP institute (*Institut de Recherche Empirique en Economie Politique*) in agreement with the main political parties involved and following all local regulations.

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