Deliberative Electoral Campaigns and Transition from Clientelism: Evidence from a Field Experiment in Benin*

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Abstract

This paper provides experimental evidence on the effect of town hall meetings on voting behavior. The experiment took place during the March 2011 elections in Benin and involved 150 randomly selected villages. The treatment group had town hall meetings where voters *deliberated* over their candidate's electoral platforms with *no cash distribution*. The control group had the standard campaign, i.e. *one-way communication* of the candidate's platform by himself or his local broker, followed (most of the time) by *cash distribution*. We find that the treatment has a positive effect on turnout. In addition, using village level election returns, we find no significant difference in electoral support for the experimental candidate between treatment and control villages. However, post-election individual surveys suggest a positive treatment effect on electoral support. Finally, we find that the positive treatment effect is driven in large part by active information sharing by those who attended the meetings

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1 INTRODUCTION

Public goods such as rural infrastructure, public education and universal health care play a crucial role in promoting economic development.[1] However, in many developing countries clientelist electoral incentives work against the provision of public goods and promote various forms of corruption. This may take the form of cash distribution during political campaigns to buy votes, or lucrative patronage jobs after the election to reward local brokers who helped deliver those votes. As such, clientelism profoundly shapes the conduct of elections and government policies, and is at the heart of the study of governance in developing countries.

The political science literature has focused primarily on uncovering the structural causes of clientelism, and on measuring its effects, and has not provided much insight on institutional reforms that would facilitate the emergence of efficient, non clientelist politics. For this to be possible, one should primarily view clientelism as, above all, a political strategy. More precisely, it is the outcome of the strategic interaction between patrons, brokers and voters. In this game, politicians offer public or private goods to voters (as electoral platforms, then as government policy when elected). In addition, they offer jobs or cash to brokers to secure electoral support from voters. Then, brokers mobilize voters by (at least in part) distributing public or private goods. Finally, voters turnout and vote. The strategic environment might vary greatly from one district or country to another; politicians, voters and brokers might be of any type (i.e. clean or corrupt, shortsighted or long-sighted), rationality might be bounded, enforcement of electoral rules might be weak, and commitment to future actions might be limited. Whatever the context, analyzing this game can help predict the predominance of various clientelist practices such as pure patronage, or "prebendalism". In so doing, it can help to guide empirical research.

One possible prediction arising from this set-up is as follows: If an incumbent patron can commit to give out the job after the election (i.e. there is no challenger), then we have pure patronage. If she can't (there is a challenger), then she may have to pay the broker "enough" money up-front before electoral uncertainty is resolved. Furthermore, if we consider at least a two- period electoral cycle, the broker may require "prebends", in order to secure early payoffs for future services.[2] That is, assuming

[1] See Keefer and Khemani [2003] for a discussion of the role of broad public goods in reducing poverty. See also St-Paul and Verdier [1993] for the effect of public education on growth and López-Casasnovas et al [2005], Sala-I-Martin [2002], Howitt [2005] for a survey of the literature on health and development.

[2] Van de Walle (2010) defines patronage as "the practice of using state resources to provide jobs and services for political clients", and prebendalism as, "the practice of giving an individual a public office in order for him/her to gain access to

the broker already has a patronage job, if the patron cannot commit to the security of this job, (e.g. because the political process is competitive), then she might let the broker "steal" state resources ahead of the next election, especially if she needs his financial support to funds her campaign. This theoretical prediction contradicts the dominant view in political science which states that prebendalism might be more prevalent under less competitive (autocratic) political systems, not in competitive (democratic) governments.[3]

The fact that this form of clientelism is prevalent under some autocratic regimes may be due to weak state capacity, not to regime type. As a result, democratization may not lead to less prebendalism, unless it comes with effective anti-corruption measures. This result also suggests that decentralization might limit clientelism. Indeed, helping the broker get elected as mayor, governor or MP might eliminate the need to secure him a patronage job. The relationship between the broker and the patron would evolve from that of local agent working to get a patron elected in exchange for cash or a job, to that of mutual insurance between elected officials trying to improve their respective electoral fortunes.

Incentives for grand corruption in clientelist networks might be limited if the patron can bypass the broker and directly take his message to voters. This would avoid the up-front service fee together with the need to commit future government resources to the broker in exchange for his effort to take voters to the polls. This strategy was an essential component of candidate Obama's election campaign in the 2008 US presidential election (especially during the democratic primaries) and of the 2009 Morales campaign in Bolivia. The strategy consist of replacing brokers with a network of young activists who engage local voters either through social media, town hall meetings or door-to-door campaigning in the context of an institutionalized "proximity" electoral campaign.

In this paper, we provide a randomized evaluation of a version of this strategy. The experiment took place during the March 2011 presidential election campaign in Benin, and involves 150 villages randomly selected from 30 of the country's 77 districts. Voters from 60 villages (the treatment group) attended town hall meetings and deliberated over candidates' policy platforms. Others from 90 villages (the control group) attended rallies organized by candidates' local brokers. We find that town hall meetings have a positive effect on measures of turnout, the result being stronger for the opposition candidates.

state resources for personal enrichment."

[3]See Van de Walle (2010)

Using village level election returns, we find no significant difference between treatment and control villages in terms of electoral support for the candidate running the experiment. However, individual post-election surveys suggest a positive and significant treatment effect on those who did attend the meetings. Examining the causal mechanisms, we show that much of the impact of the meetings is through active information sharing by those attended.

Clientelist practices are very difficult to measure and evaluate. To see this, assume there is an unusual increase in votes for an incumbent candidate in an electoral district. This happens after a broker working on behalf of the candidate, distributed cash and gifts to a number of voters in the district. Our immediate reaction would be to attribute this to vote-buying. However, before we reach this conclusion, we need, first, to find evidence for vote-switching, following cash/distribution (see Nichter, 2011). In addition we need to check if this vote switching was not driven by policy instead of money. Indeed, it is quite possible that during her tenure, the candidate might have built a new school in the district. It also possible that her choice of broker might have signaled quite clearly that she values education highly (e.g. the broker may be a popular teacher and therefore a potential minister of education!). In other words, voters might have voted in the same proportion for the candidate, regardless of whether they have received cash/gifts or not. Therefore, an increase in electoral support as a result of cash distribution is not sufficient evidence for vote-buying.

This intuition is supported in our data: We find that the segment of the electorate that received cash (30% of registered voters) may have voted the same way if they had not received any money. Comparing voting behavior of those who received money and that of those who did not, we find no significant difference between these two groups. The result indicates that, strictly speaking, vote-buying might be, at least in part, an illusion. We use this result to show that the effects of deliberative campaigns on voters is not driven by cash distribution.

1.1 Relation with the literature

Several recent experimental studies investigate the extent to which policy information can help mitigate clientelist practices (see Barnejee, 2011, Chong et al, 2011). They find that information about policy and performance can effect turnout and voting behavior. But these experiments adopted a rather normative approach. The campaign messages were designed and implemented in collaboration with social activists outside the political process and the results indicated how voters would have reacted to an exogenous

information shock. However, in real elections, policy information is channelled to voters, not through NGOs, but through candidates in the form of campaign messages, in a way that is consistent to a vote-maximizing strategy. This paper experiments with a communication strategy of policy information that has been adopted and exogenously implemented by *candidates* themselves in collaboration of researchers. The experiment is therefore incentive compatible in the sense that it increases both voter information and candidates' electoral support.

The experiment contributes to the growing literature on deliberation (Gutman and Thompson [1996], Fishkin [1997]). We find that, in addition to lab and focus groups, public deliberation can promote "enlightenment" and civic engagement, even in the context real elections. More specifically, we find that voters who participated in the meetings claim to be better informed and tend to campaign actively on behalf the candidate. Town Hall meetings work particularly well for opposition candidates but equally well for educated and non educated voters.

More generally, the paper contributes to current debates on transitions from patronage politics and clientelism.[4] The literature uses historical evidence to show the way in which economic growth and demographic shifts, a meritocratic civil service, the introduction of the secret ballot and the shrinking costs of mass communication contributed to the breakdown of patronage politics and clientelist networks. There has been no discussion in the literature of the impact of changes in campaign strategies and levels of policy information.

This paper is the third in a series of electoral experiments conducted in Benin aimed at investigating the determinants of clientelism and proposing institutional remedies. The first experiment took place during the 2001 elections in Benin and tested the effectiveness of clientelist versus programmatic electoral campaigns on voting. We found that a clientelist treatment has a positive effect on electoral support and programmatic treatment costs votes. However, the conditional treatment effect of a programmatic campaign was positive for women, more informed voters, and co-ethnics. The question arising from this experiment is whether one could refine the programmatic treatment to make it as effective as the clientelist treatment. This issue was addressed by the follow up experiment in 2006, which found that programmatic platforms might be at least as effective as clientelist ones if they are informed by research. But the results are limited by the fact that the "information effect" could not be separated from the

[4] Golden and Picci 2008, and Golden 2004 for Italy, Sorauf, [1959], Folke, Hirano and Snyder (2011) for the US, among others.

"town hall meetings effect". In addition, due to data limitation, the experiment failed to uncover the causal mechanism whereby town hall meetings improve electoral support. In response to these limitations, the 2011 experiment narrowly focused on town hall meetings. We also collected detailed information on the conduct of the town hall meetings, which enabled us to identify mediating variables and the channel of causality.

The rest of the paper is organized as follows. The next section presents the context in which the experiment took place. Section III discusses the experimental design, section IV the data and the main results. Section V concludes.

2 CONTEXT

The experiment took place in Benin (formerly Dahomey). The country is among the top ten most democratic countries in Africa, but only 31th in terms of human development, and 18th in terms of economic governance.[5] Despite being far more democratic and politically stable, Benin attracted five times less foreign direct investment than Cote d'Ivoire and ten times less than Burkina Faso.[6]

Several analysts blamed the poor economic performance in Benin on clientelism and electoral corruption[7]. Indeed, before the 2011 presidential elections, the incumbent party had been accused of prebendalism and extreme politicization of public administration. An estimated $45 million has been spent during the campaigns on cash distribution, gifts and gadgets, and payment to local brokers. In all likelihood, the bill was picked up by local or foreign "electoral investors" in return for various forms of favors.

The elections were the second since 1990 without the traditional "big men" Kerekou and Soglo. The top three candidates were Yayi Boni, a former President of the West African Development Bank, running as the incumbent candidate, Adrien Houngbedji, a former cabinet member in Kerekou's government and the candidate of the Party for Democratic Renewal (PRD), and Abdoulaye Bio Tchane (ABT), an economist and former Director of the Africa Department at the IMF. The campaign started on February 10 and ended on March 12, 2011. In the end, the incumbent candidate won in the first round by 53.16%. Houngbedji received 35.66 % of the vote and ABT took 6.29%.

[5] See the Mo Ibrahim foundation report on governance. (www.moibrahimfoundation.org)
[6] See Jeune Afrique, Hors Serie, No 27 (Etat de l'Afrique).
[7] Jeune Afrique, No 27, 2011.

3 THE EXPERIMENT

The experimental process started with a policy conference that took place on February 5, 2011. The goal was to promote policy debates involving candidates and academics and to build trust between the experimental team and the candidates. The conference covered five policy issues: mathematics education, emergency health care, youth employment, rural infrastructure, and corruption. There were about 70 participants and five reports. There were also representatives of the three main candidates, members of the National Assembly, Development Agencies, NGOs and a large number of academics including the Dean for Research at the University of Abomey Calavi, the main research university in Benin.

The experiment followed a randomized block design with treatments being assigned to 60 randomly selected sub-units (villages), in 30 randomly chosen units (electoral districts). In each study district, we selected 2 treatment villages and 3 control villages. The country has 77 districts (called communes) divided in 12 provinces (called departments). There is an average of 52 villages per district and 6 districts per province. The sampling procedure is as follows: first, we excluded the littoral department that includes the city of Cotonou because of the high risk of contamination between treatment and control groups due to Cotonou's high population density. Second, with the exception of mountainous Atakora department, we used a simple proportionality rule to determine the number of districts to be selected in each of the 10 remaining departments. Using a random number generator, we selected two treatment districts in Alibori, the department with the smallest number of districts, and 4 from Zou, the department with the highest. Then we used the same procedure to select 5 villages in each district, and assigned two to the treatment group and three to the control group. For the post election survey, we interviewed a representative sample of 30 households in each selected village.[8] In collaboration with the campaign managers of the three candidates, districts and villages were assigned to the three candidates participating in the experiment (see the list candidate-village pairs in appendix).

Treatment: A team of one research assistant of the IREEP and one activist working for the candidate organized two meetings in each of their two assigned treatment villages. Every villager was informed of the date and the agenda meeting by a village crier. The agenda was education and health for the first meeting, and rural infrastructure and employment for the second. The research team introduced the topics based on the results of the February 5 policy conference. Villagers debated the policy proposals

[8] A sample of 30 districts, 150 villages and 30 households per village would generate **statistical power of 0.80 to detect a treatment effect of size 0.20**.

and made suggestions. The team summarized the main points raised during the meetings in a written report that was transmitted to the candidate via his campaign manager. Each meeting lasted about 90 minutes. There was no cash distribution and no major political figures such as local mayors or MPs were in the audience.

Control: A local mayor, MP, or political figure (the local broker) organized two to three rallies, sometimes in the presence of the candidate himself. The representative of the candidate made a speech outlining the policy agenda and personal attributes of the candidate. There was no debate, 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 800 villagers to 3,000 or more. The rallies lasted approximately two hours.

Remark: The town hall meetings held in the treated villages were different from the rallies held in control villages in at least four ways: (1) In contrast to rallies, which consist of one-way communication from candidate to voter, town hall meetings are interactive, two-ways communications. Participants are introduced to candidate platforms, can ask clarifying questions, and can propose amendments to the platform based on local conditions. As a result, they are more likely to generate "transparent platforms." (2) While town hall meetings cost about $2 per participant, a rally costs about $15 at least according to our estimates,[9] (3) A rally draws far more people than a town hall meeting does; and (4) Every rally is run by a local or national celebrity (the mayor, MP or a broker) and involves some form of cash or gift distribution.[10]

We collected two types of experimental data. The first originates from the electoral commission: as soon as the polls were closed, the research teams went to the relevant stations to record the level of turnout and the electoral support for the candidates involved in the experiment in the 30 study communes and 150 study villages. These reports allowed us to produce village level measures of electoral outcomes. The second type of data originates from several rounds of pre- and post-election surveys. We collected pre-treatment demographic, political, and economic information from a sample of would-be voters in both treatment and control areas. The survey items included each respondent's age, gender, ethnicity, education level, assets, as well as political preferences and knowledge. The second survey also covered

[9] At least 40% of this cost is direct transfer to the local broker.

[10] By *not* getting the local broker directly involved in the town hall meetings and not distributing cash and/or gadgets to participants, we were biasing the results against a positive treatment. The presence of the mayor, the MP or a candidate himself would have boosted the audience, and gifts to the participants would certainly not have turned them against the candidate.

key features of the town hall meetings such as attendance according to participant gender and profession, the issues raised, and final resolutions of the meetings. The post-election survey data was collected after the election and covered the standard demographic and economic variables in addition to self-reported turnout, voting behavior, meeting attendance, and exposure to civic education.

4 THE DATA AND THE RESULTS

4.1 INTERNAL VALIDITY AND EMPIRICAL STRATEGY

We first verify the effectiveness of randomization in generating balanced covariates. More precisely, we test the null hypothesis of no significant difference between the means of pre-treatment variables in the treatment and control groups. We look at a wide range of demographic, political and socioeconomic variables including gender, income, education level, and age, political knowledge and participation.

Table 1 indicates that there is no significant difference between the means of any of the variables, with the exception of expected turnout and education. Indeed, the expected level of turnout is 3% in control villages and voters in treatment villages are slightly more educated than those in control villages. The difference are significant only at 95% level.[11]

Insert Table 1 here

The first dependent variables is turnout and electoral participation. Turnout is a fundamental variable of interest in the study of democracy, and has generated a great deal of interest in experimental political science. Gerber and Green [2000 and 2003] found that canvassing and face-to-face voter mobilization stimulates turnout in various types of elections. The conventional wisdom in comparative politics is that clientelism and vote-buying are the most reliable way to drive voters to the polls (Brusco, Nazareno and Stokes [2004], Nichter, [2008]). Thus, in advanced democracies, proximity campaigns based on policy messages are effective, in Africa and Latin America, monetary incentives, personal gifts and other forms of short terms benefits are essential to get voters to the polls and there is no much interest in

[11] Thus, in estimating the effect of town hall meetings on turnout, we have to take into consideration the fact that there might be a higher propensity to turnout in control villages. We will also need to control explicitely for education.

policy (cite). We investigate the effectiveness of town hall meetings, a version of proximity campaign. on turnout.

Even if the treatment improves turnout, it is unlikely to be adopted unless it improves the electoral prospects for the treated candidates. This is particularly true if they believe, as the literature suggest, that voters do not care about policy (Keefer and Khemani, 2007, etc...). Our second dependent variable of interest is voting for the treated candidate. For robustness check, we will complement our measure of voting at both village and individual level, with an individual rank-order of candidates. Thus, we measure voting under simple majority rule and under Borda rule or approval voting. We will also desegregate the voting results for incumbent and opposition candidates.

The main independent variable is treatment status. As in 2001 experiment, we investigate the relative effectiveness of the treatment on women and on those with more schooling, by introducing gender and education as our other two independent variables.

A limitation in the estimation of treatment effect, is the endogeneity of the attendance to town hall meetings. We use treatment status as an instrument attendance at both village and individual level.

In order to investigate the mechanism of the treatment effect, we will consider two possible mediating variables: platform transparency and active information sharing. Presumably, participants to town hall meeting might turnout at higher rate and vote for the treated candidate, because the meetings enable a better understanding of the candidate's platform or generate a willingness to actively campaign on his behalf. We will estimate the relative contribution of either variable to the treatment effect.

Finally, we investigate vote buying and how it might affect the treatment effect. We compare the role of money on the vote in both treatment and control groups by comparing the electoral behavior of those who receive money and those did not.

4.2 TURNOUT

We first evaluate the effect of the treatment on measures of political participation. We use both the village level outcomes collected on election day and the post-election self-reported measure. For the individual level measure, we test for the treatment effect on turnout by estimating the following linear probably model.

$Y_{ij} = z_{ij}a + T_{ij}\beta + z_{ij}T_i\gamma + u_{ij}$

$u_{ij} id \sim N(0, \Omega_i)$

where Y_{ij} is a categorical variable that takes the value of one if individual j in village i provides a positive response to the question "did you vote?", and zero otherwise; z_{ij} is a vector of individual characteristics for individual j in village i such as gender and education and T_{ij} is the categorical variable for treated individual j in village i. The key independent variable is T_{ij}, the treatment, which takes the value of one if the respondent was in the treatment group and zero if the respondent was in the control group.

For the village level measure, we estimate the linear model

$$\begin{aligned} Y_i &= z_i a + z_i T_i \gamma + u_i \\ & \quad u_i i d \sim N(0, \Omega_i) \end{aligned}$$

The village level data (see Table 2) suggest that a positive treatment effect. The result is significant at 95% level. More specifically, Table 2, Panel A suggests that treatment increases turnout by 5% in all communes. When we disaggregate by candidates, the effect remains only for the opposition designated areas.

The magnitude of the effect is similar for individual level measures. In Table 2, Panel B, town hall meetings increases self reported turnout in all districts by 4%. In this case, the results hold in both opposition and incumbent districts and the effect is significant at 99% level. Thus, turnout was significantly higher in the treatment villages than in control villages, despite the fact that villagers did not receive more cash or gift (see details in section VIII, below).

Insert Table 2

Next, we investigate the treatment effect, conditional on the level of education and gender. We find that education has no effect. However, female voters who attended meetings are more likely to turnout that those who did not. In addition, women who did not attend are less likely to vote than women in the control group. (Panel C).

The results indicate that voters in Benin respond to policy messages in the context of town hall meetings as much those in New Haven (Connecticut) respond to these messages in the context in canvassing and door-to door campaigns (Green and Gerber, 2000).

4.3 VOTING

Does increased turnout as result of the treatment, translates into higher electoral returns for the treated candidates? We address this question by estimating the treatment effect on voting. As in the previous section, we will use the village level and individual level survey data. Table 3, Panel A indicates that meetings have no effect on voting overall in the village level data. The same holds for electoral support for each candidate individually. As for the individual level data, attending the meeting increases by 16% the vote for the treated candidate in all communes (see Table 3 Panel B). The desegregated results are 21.68% in opposition communes, and not significant in for the incumbent communes.

The conditional treatment effect for education is not significant. However, in contrast with turnout, the women who attended the meetings were not any more likely to support the treated candidate than those who did not attend.

Insert Table 3 and 4 here

Thus, at the very least, town hall meetings is a far more efficient strategy to generate votes than "standard" campaign strategies. It is at least as electorally effective, and far less costly. Voters in Benin can be responsive to non-material incentives when they attend town hall meetings. .

4.4 ENDOGENEITY OF ATTENDANCE

Villages are exogenously assigned to town hall meetings. However, the individual or collection decision to attend these meetings might be endogenous. Thus there might be observable or non observable variables that affect both attendance and turnout or vote. As a result, OLS would give biased estimates of the treatment effect. In order to deal with this problem, we instrument attendance by treatment status and estimate the effect of the attendance using an IV two stage least square model.

Table 5, Panel A indicates that the effect of attendance on village level turnout persists and is of the roughly the same magnitude as in the OLS model. We find that turnout increases by 3.5 % in all communes, but by 5% in incumbent-controlled communes. However the effect of self-reported, individual level turnout disappears when we use the IV2SLS model.

Insert Table 5 here

Table 6 presents the IV estimates of the treatment effect on votes. The results in panel indicates that, at the average attendance level, an additional individual that participates at the meeting contributes to half percentage point increase in the vote for the treated candidate in all communes, by 0.8% in opposition communes. The results are nearly identical in panel B. An additional individual attendant increases the treated candidate vote share by 0.3% in all communes and 0.8 % in opposition-controlled communes. Note that the IV results indicate positive treatment effects on votes in both village level estimates and individual level estimates. In the OLS model, only individual level estimates are significant.

Insert Table 6 here

4.5 CAUSAL MECHANISM

Our town hall meeting experiment is part a recent trend in experimental research directed at the rigorous evaluation of the effects of institutions and decision-making processes such as community deliberation (Fearon et al., 2009), plebiscites (Olken, 2010), campaign strategies (Wantchekon, 2010), and school-based management (Blimpo and Davis, 2011). The distinctive feature of these experiments is that subjects are randomly assigned to a decision making process that then itself endogenously generates a policy, which ultimately affects an outcome of interest such as student learning, voter turnout, or child mortality rates. As discussed in Atchade and Wantchekon (2009), process experiments present the following challenge: how can researchers disentangle the intrinsic institutional effects from the policy effects? In order to accomplish this, we need to deal more broadly with the issue of causal mechanisms. We must explain how the institutional treatments affect intervening variables, which themselves produce the observed final outcome.

One way to study such causal mechanisms is to control for possible mediating variables when estimating the effect of the treatment. The coefficients of the mediating variables allow us to estimate the contribution of each variable to the final observed outcome. An alternative strategy is to estimate the average treatment effect (ATE) in the presence of specific, measurable, mediator variables. Imai et al. (2011) propose just such a methodology and allow researchers to quantify the effect of a treatment on an outcome, holding the treatment constant and varying the levels of the intervening variable.

In the context of the present study, the effect of town hall meetings on voter turnout or vote choice may be caused by enhancing the clarity of candidate proposals (platform transparency), or instead

by increasing post-meeting activism and information sharing by those who attended the meetings. In other words, town hall meetings could enable voters to have better information about the candidates' platforms, and candidates could simultaneously develop stronger connections with voters. In addition, better informed voters may volunteer after the meetings to mobilize other less informed voters on behalf of the candidate.

Following the standard strategy, we contrast the effect of active information sharing with that of "platform transparency." We construct an "active information sharing" variable from responses to the survey questions: "Did you share the results of the town hall meetings with other members of the communities?" and, "Who were they?" The "audience" variable is derived from the question: "How do you think the meetings influence your vote?" The available responses were: (a) "they help learn who other villagers will vote for" (to proxy for the voter coordination mechanism); (b) "they help learn more about the candidate policy agenda" (the platform transparency mechanism); (c) "they show that the candidate is willing to listen to voters" (the attentive candidate mechanism). We then constructed a simple average of these factors under the name "audience."

We find no support for the proposed causal channel on voter turnout (see Table 7A). The effects on voter choice were more mixed. Panel B of Table 7A presents OLS regression results on the effects on candidate vote choice. The coefficient for the effect of "active information sharing" is .051 (or 5%) and that of "audience" .023 (or 2.3%). The results are, however, significant only at the 90% level. Panel B of Table 7B (the mediation analysis) presents the effect of each channel conditioning on the treatment status. We find that the "audience" mechanism accounts for 1.25% of the treatment effect and active information sharing for 50% of the observed effect. More precisely, the Average Causal Mediation Effect (ACME 1 and ACME 0) of audience effects is .002, while the total effect of the treatment is .166. Since .002 is 1.25% of the total effect of the treatment, then audiences effects account for a relatively small proportion of the effect. The effect of sharing information is .089 of the total treatment effect (.151), which is around 50% of the observed effect.[12] In other words, holding a town hall meeting had a positive effect on candidate vote choice, because those who attended a meeting shared its results with others voters.

[12]ACME 1 and ACME 0 stand for Average Causal Mediation Effect. They account for the average effect of the treatment that goes through the mediation variable. In this case, the mediation variable are audience effects in the first column, and sharing information in the second column. (see Imai, 2011). .

Insert Table 7A and Table 7B

4.6 CASH DISTRIBUTION AND TREATMENT EFFECTS

The results presented above are quite surprising in light of existing findings in political science that rule out that African voters could respond to policy platforms as opposed to short term economic incentives.[13] The experimental evidence presented here demonstrates that voters can also be swayed by policy messages, particularly if those messages are deliberated on at town hall meetings. The rigorous evaluation of the effect of the distribution of cash or gifts on turnout and voting behavior is beyond the scope of the paper. Given the pervasiveness of cash distribution in pre-election political campaigns in Benin, however, we do need to address the following question: Did money play a role in making the town hall meetings effective? It is possible that voters who chose to attend the meetings were bribed, meaning they might have received cash and gifts in addition to the treatment. If that were the case, the experimental treatment might be capturing the combined effect of money and deliberated platforms on turnout and vote choice.

As noted, there was strictly no cash or gift distribution before or after the town hall meetings. However, there were individuals from the treatment villages who did receive cash and gifts during the elections, almost certainly from "non-treated" candidates campaigning in the commune.[14] These individuals represent about 25.6% of those who attended the meetings and 29.8% of registered voters in the treated villages. The evidence suggests that cash and gifts were distributed in treatment and control villages in almost exactly the same proportion (29.79 in the treatment group and 29.8 in the control group). Moreover, according to a simple two sided t-test of mean equality we cannot reject the null hypothesis that both means are the same.

In addition to this evidence, the results presented in Table 8 indicate that there is virtually no interference of money on the estimated treatment effects. The estimates of the treatment effects for the entire sample (panel A) and with the sample restricted to voters who did not receive cash or gifts (panel B) are very similar. The coefficients for self-reported turnout are slightly lower in panel A than in panel B. The reverse is true for village level turnout, where the coefficient is slightly lower in panel

[13] See Kramon (2009), Banegas (2003), Vicente (2009) among others. See also Lindberg and Morrison **2008**, Wantchekon (2003, 2006) that shows that under some conditions voters can respond to policy platforms. But the evidence were not conclusive.

[14] By non-treated candidate, I mean those are not participating in the experiment in a given commune.

B, and loses significance. As for the effect on vote share, the treatment effect is one percentage point higher for those who did not receive cash and gifts as measured by a self-reported survey item. Thus, the treatment effects are nearly identical in the samples with and without cash and gift distribution.

We also present results from a regression including the interaction effects between treatment status and cash and gift provision. As indicated in column 1, panel C table 8. receiving money in the treatment villages did not lead to an increase in turnout (in fact, it decreased it). However, in control villages, money had a slight effect on turnout The coefficients for village-level turnout were not significant, however (column 2, panel C). Finally, receiving money has no effect on vote choice for the treated candidate in treatment villages. It has a negative and significant effect on treated candidate vote choice in the control villages (Column 2, panel C).

Overall, the evidence suggests that money did not interfere with the treatment, and did not contribute to the treatment effect on vote choice or turnout. In addition, even in the absence of treatment, money did not seem to have much of an effect on political outcomes. As indicated in Figure 1, the distribution of turnout in the control group is nearly identical between those who received money and those who did not. Again, a more rigorous analysis of the effectiveness of vote buying or turnout buying is left for future research.

Insert Table 8 and Figure 1

5 CONCLUDING REMARKS

In this paper, we report the results of a unique, nationwide experiment to test the effectiveness of town hall meetings during election campaigns on turnout and vote choice. We provide evidence suggesting that voters in Benin do respond to policy platforms, provided that they are deliberated on with voters. The evidence also suggests that town hall meetings and deliberated platforms have a positive effect on turnout in large part because those who attended the meetings tend to actively share the information with other villagers. We find that town hall meetings contribute to the transparency of electoral platforms, and to a better, perhaps more personal connection between candidates and voters. Interestingly, the results are not driven by years of schooling, income, or even cash distribution, which suggests that poverty, low level of education, and vote-buying do not prevent voters from being swayed by policy platforms.

The results also suggest that significant efficiency gains could be made by all candidates, and especially opposition candidates, by holding fewer rallies and more town hall meetings. This is because, based on our estimates, rallies are about eight times more expensive than town hall meetings, and yet they tend to generate relatively lower turnout and lower vote shares for the candidate who organizes them. In addition, rallies tend to be organized by local brokers, some of whom may force candidates to commit to giving them cabinet positions or patronage jobs after the election. Town hall meetings, by contrast, can be run by "foot soldiers," campaign activists who may have less claim to patronage jobs after the election. As a result, campaign strategies based on town hall meetings are far less constraining in terms of the process of government formation. Finally, an electoral strategy that gives more weight to town hall meetings should help significantly reduce the cost of running for office, and the excessive reliance on wealthy local and international sponsors. This could reduce one form of corruption in government, the "pay back" to electoral investors.

There are several directions for future research. In terms of experimental studies of clientelism, we plan to improve the external validity of our findings by replicating the experiment in other African countries and in the context of other types of elections, such legislative or municipal contests. This would allow for a more detailed investigation of the causal mechanisms, particularly the interaction between platform transparency, active information sharing, as well the effect of money on political outcomes. We also plan to test alternative institutional remedies to patronage politics such as a meritocratic civil service and decentralization.

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Table 1: **COVARIATE BALANCE**

Variable Label	Treatment	Control	Difference	p-value
Demographic Variables				
Female	0.61	0.59	-0.02	0.08
	(0.01)	(0.01)	(0.01)	
Age	36.97	37.08	-0.11	0.40
	(0.35)	(0.29)	(0.46)	
Number of spoken languages	1.99	1.94	0.05	0.99
	(0.02)	(0.02)	(0.02)	
Education	0.49	0.46	-0.03*	0.04
	(0.01)	(0.01)	(0.02)	
Political Variables				
Do you know your Mayor?	0.71	0.69	-0.02	0.10
	(.01)	(.01)	(0.01)	
Do you know Yayi Boni?	0.96	0.96	0.01	0.81
(incumbent candidate)	(0.00)	(0.00)	(0.01)	
Will you vote? (% Yes)	1.05	1.08	-0.03*	0.05
	(0.01)	(0.01)	(0.02)	
Term limits	2.47	2.64	-0.17	0.00
	(0.04)	(0.04)	(0.06)	
Economic Variables				
Steady Income	1.78	1.77	0.00	0.64
	(0.01)	(0.01)	(0.01)	
Landholding	1.47	1.49	-0.02	0.16
	(0.01)	(0.01)	(0.02)	

TABLE 2: TREATMENT EFFECT ON TURNOUT

Panel A: Turnout (Village Level)

	Overall	Opposition	Yayi
$\text{Treat}_{\text{vil}}$	3.301*	4.848*	3.510
	(1.91)	(1.99)	(0.65)
_cons	85.5***	87.75***	81.26***
	(48.02)	(48.84)	(19.37)
N	150	110	40

Panel B: Turnout (Individual Level)

	Overall	Opposition	Yayi
$\text{Treat}_{\text{ind}}$	0.0405***	0.0420***	0.0359**
	(5.36)	(4.68)	(2.58)
_cons	0.931***	0.928***	0.946***
	(143.48)	(108.40)	(112.40)
N	5009	3694	1315

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Specifications for ABT and UN are not shown for reasons of space.

$\text{Treat}_{\text{ind}}$ refers to individuals who received the treatment (town-hall meetings)

$\text{Treat}_{\text{vil}}$ refers to villages which were assigned to the treatment

TABLE 3: TREATMENT EFFECT ON VOTES

Panel A: Vote Outcomes (Village Level)

	Overall	Opposition	Yayi	ABT	UN
$\text{Treat}_{\text{vil}}$	-0.522	-0.0479	-1.826	-0.294	0.00685
	(-0.24)	(-0.02)	(-0.40)	(-0.03)	(0.00)
_cons	53.00***	43.22***	79.91***	27.64**	46.68***
	(12.56)	(11.24)	(18.47)	(2.86)	(12.21)
N	150	110	40	20	90

Panel B: Vote Outcomes (Individual Level)

	Overall1	Opposition1	Yayi1	ABT1	UN1
$\text{Treat}_{\text{ind}}$	16.09***	21.68***	0.970	25.62***	20.74***
	(12.21)	(12.52)	(0.74)	(1.33)	(7.20)
_cons	66.32***	56.13***	94.14***	39.62***	59.84***
	(16.00)	(13.33)	(30.61)	(5.34)	(13.15)
N	4518	3279	1239	643	2636

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Specifications for ABT and UN are not shown for reasons of space.

$\text{Treat}_{\text{ind}}$ refers to individuals who received the treatment (town-hall meetings)

$\text{Treat}_{\text{vil}}$ refers to villages which were assigned to the treatment

TABLE 4: CONDITIONAL TREATMENT EFFECTS (GENDER AND EDUCATION)

Panel A: Turnout, Gender and Education (Individual Level)

	Overall	Overall	Opposition	Opposition	Yayi	Yayi
$\text{Treat}_{\text{ind}}$	0.0396***	0.0252	0.0417***	0.0189	0.0337*	0.0393
	(5.25)	(1.87)	(4.65)	(1.12)	(2.42)	(1.79)
Female	-0.0261***	-0.0360***	-0.0273***	-0.0381***	-0.0241	-0.0300*
	(-3.79)	(-4.55)	(-3.34)	(-4.06)	(-1.91)	(-2.07)
Education	0.00544	0.00711	0.000371	-0.000777	0.0192	0.0295*
	(0.77)	(0.87)	(0.04)	(-0.08)	(1.50)	(1.98)
Female · $\text{Treat}_{\text{ind}}$		0.0395*		0.0434*		0.0267
		(2.50)		(2.31)		(0.92)
Female · $\text{Treat}_{\text{ind}}$		-0.00472		0.00732		-0.0407
		(-0.30)		(0.39)		(-1.40)
_cons	0.940***	0.943***	0.940***	0.945***	0.943***	0.942***
	(114.86)	(109.37)	(89.04)	(85.58)	(90.07)	(82.11)
N	5009	5009	3694	3694	1315	1315

Panel B: Vote, Gender and Education (Individual Level)

	Overall	Overall	Opposition	Opposition	Yayi	Yayi
$\text{Treat}_{\text{ind}}$	16.13***	14.07***	21.76***	20.76***	0.999	2.505
	(12.24)	(6.01)	(12.56)	(6.39)	(0.76)	(1.20)
Female	-0.0983	-0.354	-0.725	-0.834	1.025	1.874
	(-0.08)	(-0.25)	(-0.45)	(-0.45)	(0.84)	(1.32)
Education	-1.588	-2.499	-2.453	-2.846	0.778	0.972
	(-1.25)	(-1.70)	(-1.47)	(-1.49)	(0.60)	(0.64)
Female · $\text{Treat}_{\text{ind}}$		1.037		0.504		-3.306
		(0.38)		(0.14)		(-1.19)
Female · $\text{Treat}_{\text{ind}}$		3.423		1.514		-0.673
		(1.26)		(0.42)		(-0.24)
_cons	67.08***	67.60***	57.63***	57.87***	93.42***	93.00***
	(15.90)	(15.90)	(13.22)	(13.12)	(29.46)	(29.13)
N	4518	4518	3279	3279	1239	1239

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Specifications for ABT and UN are not shown for reasons of space.

$\text{Treat}_{\text{ind}}$ refers to individuals who received the treatment (town-hall meetings)

$\text{Treat}_{\text{vil}}$ refers to villages which were assigned to the treatment

TABLE 5. EFFECT OF ATTENDANCE ON TURNOUT - IV RESULTS

Panel A: Turnout (Village Level)			
Commune	All	Opposition	Yayi
Attendance	0.169**	0.138	0.251*
	(0.0857)	(0.106)	(0.137)
Constant	85.34***	87.46***	79.59***
	(1.746)	(1.893)	(3.200)
Observations	150	110	40
R-squared	0.020	0.019	0.025
Panel B: Turnout (Individual Level)			
Commune	All	Opposition	Yayi
Attendance	0.000980	0.00110	0.000623
	(0.000940)	(0.00111)	(0.00178)
Constant	0.907***	0.901***	0.928***
	(0.0329)	(0.0393)	(0.0595)
Observations	5,020	3,700	1,320
R-squared	0.001	0.000	0.001

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Robust standard errors clustered at the commune level in parentheses

2SLS. Instrument: $\text{Treat}_{\text{vil}}$.

Instrumented: individual town-hall meeting attendance

TABLE 6: EFFECT OF ATTENDANCE ON VOTES - IV RESULTS

Panel A: Votes (Individual Level)

Commune	All	Opposition	Yayi
Attendance	0.00593**	0.00850***	-0.00102
	(0.00250)	(0.00324)	(0.00220)
Constant	0.504***	0.324**	0.987***
	(0.106)	(0.126)	(0.0681)
Observations	4,529	3,285	1,244
R-squared	0.003	0.005	0.000

Panel B: Votes (Village Level)

Commune	All	Opposition	Yayi
Attendance	0.00309**	0.00459***	-0.000794
	(0.00129)	(0.00160)	(0.00126)
Constant	0.674***	0.572***	0.953***
	(0.0460)	(0.0459)	(0.0212)
Observations	150	110	40
R-squared	0.008	0.026	0.010

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

2SLS. Instrument: $\text{Treat}_{\text{vil}}$.

Instrumented: individual town-hall meeting attendance

TABLE 7.A CHANNELS OF CAUSALITY: INFORMATION SHARING AND AUDIENCE EFFECTS ON TURNOUT AND VOTE

OLS RESULTS			
Panel A: Turnout (Individual Level)			
Commune	All	Opposition	Yayi
Share Information	0.00388	0.00575	0.000864
	(0.0127)	(0.0136)	(0.0215)
Audience Effects	-0.000948	-0.00802	0.0187*
	(0.00874)	(0.0112)	(0.00966)
$\text{Treat}_{\text{vil}}$	0.00887	0.0161	-0.0270
	(0.0306)	(0.0337)	(0.0177)
Constant	0.965***	0.980***	0.938***
	(0.0496)	(0.0585)	(0.0490)
Observations	727	530	197
R-squared	0.003	0.004	0.039
Panel B: Vote Outcomes (Individual Level)			
Commune	All	Opposition	Yayi
Share Information	0.0517*	0.0306	0.0629
	(0.0292)	(0.0390)	(0.0464)
Audience Effects	0.0234*	0.0311*	-1.72e-06
	(0.0122)	(0.0154)	(0.00644)
$\text{Treat}_{\text{vil}}$	0.140	0.150	-0.0559
	(0.114)	(0.128)	(0.0506)
Constant	0.706***	0.658***	1.005***
	(0.126)	(0.147)	(0.0178)
Observations	701	508	193
R-squared	0.044	0.036	0.076

Standard errors clustered at the commune level in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Specification for ABT and UN not shown for space reasons.

$\text{Treat}_{\text{vil}}$ refers to villages which were assigned to the treatment

Includes controls for age, education and gender

Share Information: Did you share information about the meeting with other people?

Audience: (1) The meeting help you know what other villagers think

Audience: (2) You get to know the candidate better after the meeting

Audience: (2) You felt listened after the meeting

TABLE 7.B CHANNELS OF CAUSALITY: INFORMATION SHARING AND AUDIENCE EFFECTS ON TURNOUT AND VOTE

	MEDIATION ANALYSIS			
	Votes	Votes	$\text{Turnout}_{\text{ind}}$	$\text{Turnout}_{\text{ind}}$
$\text{Treat}_{\text{vil}}$	0.162	0.0600	0.0106	0.0439
	(0.109)	(0.108)	(0.0331)	(0.0431)
Share Information		0.215***		0.0166
		(0.0434)		(0.0107)
Audience Effects	0.0280**		-0.000610	
	(0.0104)		(0.00827)	
Constant	0.707***	0.620***	0.965***	0.886***
	(0.124)	(0.123)	(0.0494)	(0.0495)
R-squared	0.035	0.091	0.003	0.010
ACME1	.002	.089	-.0001	.006
ACME0	.002	.089	-.0001	.006
DirectEffect1	.163	.061	.011	.044
DirectEffect0	.163	.061	.011	.044
TotalEffect	.166	.151	.01	.051
CI Zero	No	No	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Standard errors clustered at the commune level in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Specification for ABT and UN not shown for space reasons.

$\text{Treat}_{\text{vil}}$ refers to villages which were assigned to the treatment

Includes controls for age, education and gender

Share Information: Did you share information about the meeting with other people?

Audience: (1) The meeting help you know what other villagers think

Audience: (2) You get to know the candidate better after the meeting

Audience: (2) You felt listened after the meeting

TABLE 8. MONEY ON POLITICAL OUTCOMES

Panel A: Entire Sample

VARIABLES	$Turnout_{ind}$	$Turnout_{vil}$	$Votes_{ind}$	$Votes_{Opp}$	$Votes_{Yayi}$
$Treat_{ind}$	0.0412***		14.97***		
	(0.00809)		(3.023)		
$Treat_{vil}$		3.334*		-1.131	0.403
		(1.704)		(1.687)	(2.091)
Constant	0.931***	85.45***	66.93***	36.67***	55.69***
	(0.00744)	(1.728)	(4.795)	(4.098)	(3.762)
Observations	5,009	5,113	4,518	5,113	5,113
R-squared	0.006	0.020	0.021	0.001	0.000

Panel B: Only those who did NOT receive Money

VARIABLES	$Turnout_{ind}$	$Turnout_{vil}$	$Votes_{ind}$	$Votes_{Opp}$	$Votes_{Yayi}$
$Treat_{ind}$	0.0516***		15.89***		
	(0.0112)		(3.298)		
$Treat_{vil}$		2.996		-1.802	0.942
		(1.860)		(1.908)	(2.336)
Constant	0.918***	85.23***	70.33***	37.83***	55.67***
	(0.00974)	(1.890)	(5.045)	(4.626)	(4.404)
Observations	3,475	3,501	3,085	3,501	3,501
R-squared	0.008	0.016	0.026	0.001	0.000

Panel C: Interactive Effects

VARIABLES	$Turnout_{ind}$	$Turnout_{vil}$	$Votes_{ind}$	$Votes_{Opp}$	$Votes_{Yayi}$
$Treat_{ind}$	0.0516***		15.89***		
	(0.0112)		(3.298)		
$Treat_{vil}$		2.996		-1.802	0.942
		(1.861)		(1.908)	(2.336)
Money	0.0429***	1.056	-11.47***	-2.678	-0.0663
	(0.0117)	(1.397)	(3.880)	(3.702)	(3.459)
$MoneyXTreat_{ind}$	-0.0352**		-2.140		
	(0.0159)		(2.860)		
$MoneyXTreat_{vil}$		0.847		1.583	-1.518
		(1.717)		(2.165)	(2.288)
Constant	0.918***	85.23***	70.33***	37.83***	55.67***
	(0.00975)	(1.890)	(5.046)	(4.627)	(4.405)
Observations	4,939	4,987	4,460	4,987	4,987
R-squared	0.011	0.023	0.037	0.002	0.001

Robust standard errors in parentheses

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

Standard errors clustered at the commune level in parentheses

$Treat_{ind}$ refers to individuals who received the treatment (town-hall meetings)

$Treat_{vil}$ refers to villages which were assigned to the treatment

MONEY DISTRIBUTION BY TREATMENT STATUS

	Control	Treatment	Total
No Money	1835	1666	3501
	70.20	70.21	70.20
Money	779	707	1486
	29.80	29.79	29.80
Total	2614	2373	4987
	100	100	100

TWO-SAMPLE T TEST.

Group	Observations	Mean	Std. Err.
Control	2614	.298	.008
Treatment	2373	.297	.009

Ho: diff = 0
t = .0058

ATTENDANCE DISTRIBUTION BY MONEY STATUS

	No Money	Money	Total
No Attend	2608	1087	3695
	74.6	73.2	74.18
Attend	888	398	1286
	25.4	26.8	25.82
Total	3496	1485	4981
	100	100	100

TWO-SAMPLE T TEST.

Group	Observations	Mean	Std. Err.
No Attend	3496	.254	.007
Attend	1485	.268	.011

Ho: diff = 0
t = -1.033

FIGURE 1. DISTRIBUTION OF TURNOUT BY MONEY STATUS IN CONTROL VILLAGES).

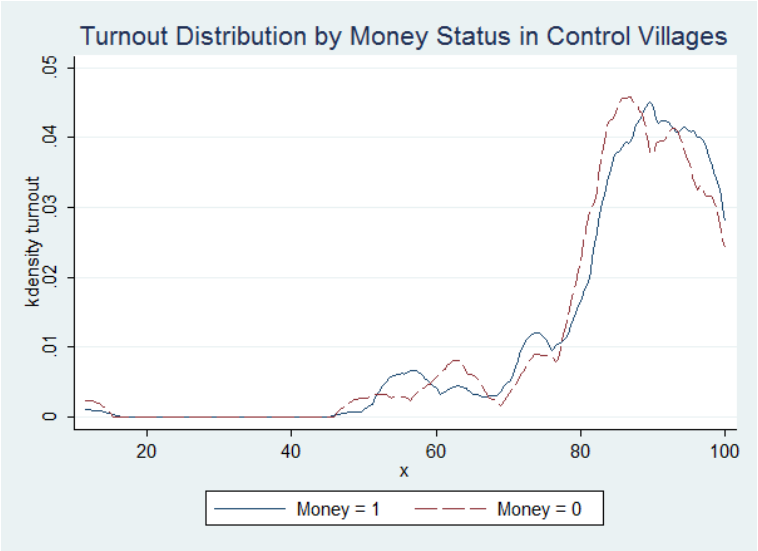

FIGURE 2. DISTRIBUTION OF OPPOSITION VOTES BY MONEY STATUS IN CONTROL VILLAGES).