

Can Informed Public Deliberation Overcome Clientelism? Experimental Evidence from Benin[†]

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This paper studies the electoral effects of town hall meetings based on programmatic, nonclientelist platforms. The experiment involves the cooperation of leading candidates in a presidential election in Benin. A campaign strategy based solely on these meetings was assigned to randomly selected villages and compared to the standard strategy of clientelist rallies. We find that treatment reduces the prevalence of clientelism and does not affect turnout. Treatment also lowers the vote shares for the candidate with a political stronghold in the village and is more effective in garnering votes in regions where a candidate does not have a political stronghold. (JEL D72, O17)

Elections in developing countries are often characterized by clientelism—the practice of garnering the vote of constituencies through gifts and the promise of favors and patronage. Research in economics and political science suggests that such targeted redistribution is inefficient but electorally effective. In contrast, broad public good provision is associated with better economic outcomes but is politically costly. The literature suggests no real alternative to clientelism, at least in the short run; the practice is perceived as a reflection of agrarian social relations and ethnic cleavages. Promises of broad public good provision tend not to be credible in new democracies where politicians and parties have not interacted long enough with voters.¹

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¹ Easterly and Levine (1997, 1207) argue that Africa's lack of "growth-promoting public goods" explains its "tragic growth performance," since clientelist promises are a more effective way to gain voter support. Anderson, Francois, and Kotwal (2011) provide evidence of the negative economic effects of clientelism in village India. The structural causes of clientelism are discussed in Lemarchand (1972) and van de Walle (2003, 2007). Keefer and Vlaicu (2008) address the issue of the relative credibility of broad and clientelist campaign promises. Finally, a World Bank (2009, xvii) memo states that "the lack of broad policy-based parties that can make credible commitments to voters" as the first reason why "Benin's political economy hampers the adoption of growth policies."

These arguments present a determinist view of clientelism that parallels modernization theory's argument that democratization is only possible with a sufficiently high level of economic development (Lipset 1959). Similarly, nonclientelist or programmatic politics can appear only when countries reach a sufficiently high level of economic and political development. These notions were perhaps reinforced by Wantchekon's (2003) findings that randomly assigned clientelist messages were more effective than broad-based ones (regarding nationwide issues) in generating voter support in the 2001 Beninese presidential election. It is fairly possible that voters respond positively to clientelism simply because they are usually never exposed to a credible alternative.

In this paper, we test these notions with a field experiment that evaluates an electoral campaign exemplifying an alternative to clientelist practices: candidate-endorsed town hall meetings discussing specific policy platforms of broad-based public provision, followed by voters' deliberation (an open debate of the policy proposed in the meeting). The specific platforms were drawn from the conclusions of a meeting of academic experts. The experiment involves the cooperation of actual leading candidates of the 2006 presidential election in Benin, who adopted our alternative campaign strategy in randomly selected villages, while pursuing the standard clientelist strategies in control villages.

This experiment differs from the one in Wantchekon (2003) in two important dimensions. First, the nonclientelist campaign message in that experiment was relatively vague and did not provide specific policy platforms. Second, treatment in this experiment includes public deliberation; voters were invited to debate the platforms in the town hall meetings.

We find that our alternative strategy, when compared with standard clientelist rallies, reduced self-reported measures of clientelism, with no effect on voter turnout (measured in official electoral results).² This suggests that information-based campaigns can limit certain aspects of clientelism while being just as effective in mobilizing voter turnout. This is of particular importance given evidence that vote buying is used to mobilize turnout (Nichter 2008).

Estimates based on official electoral results indicate that, on average, there is no statistically significant effect on the vote shares of the candidates running the town meetings (i.e., experimental candidates). However, treatment reduces the votes of the "dominant" (i.e., most voted) candidate in the village. Our suggested interpretation is that in information-deficient and clientelist environments, the dominant candidate (likely the most effective in vote-buying and exploiting ethnic identities) has an advantage in boosting his vote share. The arrival of more information and deliberation reduces his votes.

The average effect described above masks an interesting heterogeneity in effects. When candidates endorse treatment in areas in which they are "dominant," it has a

²Given our multiple measures of clientelism-related practices, we use a procedure for multiple outcome testing suggested by Kling, Liebman, and Katz (2007) to test the overall effect on the family of variables. While this has advantages in allowing for correct inference and avoiding the pitfalls of "cherry-picking" reported results, it has the disadvantage of leaving unclear exactly which practices are affected by treatment. It must be highlighted, however, that most of the variables on the index deal with voter information and deliberation of politics. Only one variable measures the extent of vote buying, and although the estimated effect is sizable and negative, it is not statistically significant.

negative effect on their vote shares. When they do so outside their strongholds, treatment substantially boosts their votes. This result holds “within candidates”—the same candidate has a positive (negative) treatment effect when he is nondominant (dominant).³

On one hand, our results confirm the “determinist view” of clientelist politics by indicating that candidates with a stronghold in a region lose votes by deviating from clientelism. On the other hand, it also challenges it by demonstrating that the same candidate may find abandoning clientelism in favor of our information-based campaigning to be an effective strategy elsewhere. Moreover, our results suggest an interesting possibility, that every candidate may find it optimal to follow clientelism in his strongholds while pursuing our treatment strategy in his opponents’ strongholds, implying that every candidate may find it worthwhile to “abandon clientelism” somewhere, and everywhere there can be a candidate that can gain by “abandoning clientelism.”

Our treatment combines two mechanisms that can affect voter behavior: provision of information (the policy platforms) and public deliberation—the information is given in a public meeting, and voters are allowed to debate over the platforms afterward. The effects of information on voter behavior in developing countries is addressed in a growing number of studies. Pande (2011) provides a survey and discusses the theoretical mechanisms at play. The role of public deliberation on voter behavior has received less attention in the literature. It allows voters to learn about each others’ preferences, beliefs, and expectations. This can generate clearer benchmarks for voters to evaluate different candidates, and may facilitate coordination across voters, affecting their choice of candidate. This paper does not attempt to disentangle the effects of information from those of deliberation, given that treatment involves both mechanisms, and controls receive neither mechanism.⁴

The remainder of the paper is as follows. Section I briefly discusses the theoretical considerations and describes the experiment’s context, design, and empirical strategy, while Section II reports the results. Section III concludes the paper.

I. Experimental Design

A. Background

Benin exemplifies the case of an African country with thriving (if young) democratic institutions but poor governance and economic performance. The World Bank (2009) discusses why the 1989 democratic transition has been a “success” that is “remarkable when compared to the experience of other French-speaking countries of the region” (World Bank 2009, 121). However, the same report

³Section I details how the villages where a candidate is dominant or not are defined (based on predetermined variables, so as to avoid sample selection bias) and the possibility that individual candidates are driving the results.

⁴The experimental literature can be broadly categorized into testing if information on (incumbent) candidate performance raises political accountability (Ferraz and Finan 2008; Chong et al. 2011; Banerjee et al. 2011; Bobonis, Cámara-Fuertes, and Schwabe 2011), and estimating the effects of information on issues and policies (Collier and Vicente 2011; Giné and Mansuri 2011; Banerjee et al. 2012). The current paper fits both categories, as it deals with a discussion of policy issues that is inserted in specific candidates’ campaigns. Austen-Smith and Feddersen’s (2006) model examines deliberation in the presence of preference uncertainty.

notes that the “puzzle” that “economic growth has not matched the vibrancy of its elections” can be explained by “clientelist promises to narrow groups of citizens” (World Bank 2009, 121–22). In particular, it notes that Beninese parties’ “extent of programmatic orientation is among the lowest [...] in Africa” (World Bank 2009, xviii).

Presidential elections use the runoff system, where a first round election is held and the two candidates with the most votes face off in a second round (the runoff) against each other.⁵ Elections are at large, so the entire country functions as one single district.

The experimental process started with a policy conference that took place on December 2005, entitled “Elections 2006: What policy alternatives?” There were approximately 40 participants and 4 panels (education, public health, governance, and urban planning). Four policy experts wrote reports describing government performance in those four areas and outlined recommendations based on academic research and best practices in policy implementation. The report contains a wide range of policy proposals, such as community-funded health insurance, school-based management, random audits of politicians, and other anti-corruption measures.

After the conference, the campaigns of four presidential candidates (Boni Yayi, Adrien Houngbedji, Bruno Amoussou, and Lehady Soglo) volunteered to experiment with the proposed campaign strategies. These 4 were the main contenders in the presidential election (their national vote shares were 36 percent, 24 percent, 16 percent, and 9 percent, respectively). They entered an agreement with the Beninese Institute for Empirical Research in Political Economy (IERPE) that involved following the experimental protocol described below. However, missing data issues led to the exclusion of observations related to Amoussou’s participation, so that all results are based on the participation of the three remaining candidates. These data issues are discussed below.⁶

B. Randomization

Before defining treatment and control, it is useful to specify how villages were assigned to each of these statuses. Benin contains over 3,000 villages (*quartiers*) in 77 communes. The experiment consisted of assigning different villages from 12 different communes to treatment and control status. Each participating candidate was responsible for the experiment in specific communes. Henceforth, the candidate in charge of running the experiment in a commune is referred to as the experimental candidate (or EC).

⁵If a candidate obtains more than 50 percent of the votes in the first round, he is declared president. This electoral rule is also known as the plurality rule with a runoff or the dual-ballot system.

⁶Boni Yayi was the former president of the West African Development Bank, running as an independent candidate supported by a coalition of small parties. Adrien Houngbedji is a former cabinet member in the incumbent government, and the candidate of the Party for Democratic Renewal. Bruno Amoussou was the Social Democratic Party candidate. Lehady Soglo, the son of former president Nicephore Soglo, was the candidate of Renaissance du Benin. There were 26 candidates competing in the election, but only these 4 were able to secure more than 5 percent of the vote.

The candidates' campaigns were asked to suggest the communes in which they would run the experiment, with the requirement that they had plans to campaign relatively intensively in them. A commune could not have more than one EC, although it was not the case that two campaigns suggested the same commune. In most cases, though not all, the EC was responsible for a commune where he had a political stronghold (where he was expected to receive the majority of votes).

Within a commune, four villages were chosen to be part of the experiment. Randomization was stratified geographically in the following manner. Within each commune, one village was randomly assigned to the treatment group, and the remaining three to the control group. The only exception to the rule is that in one commune (Dangbo), three villages were randomly assigned to treatment, and nine to the control group. This commune was itself divided into 3 separate strata, totaling 14 strata in 12 communes (14 treatment and 42 control villages). The stratified randomization guarantees a perfect balance regarding any characteristic that varies only at the commune level.⁷

Campaigns varied in the extent to which they were willing or able to participate in the experiment. In particular, Yayi's campaign was comprised of a coalition of several smaller supporting parties, of which several had interest in supporting the experiment. This led to Yayi being the EC in seven communes, while Houngbedji and Soglo were the EC in three strata each. Amoussou was only willing to cooperate in one commune, which was later dropped from the estimations due to missing data. The online Appendix presents our main results excluding the communes where Yayi is the EC. The qualitative results remain, suggesting Yayi's larger participation in the experiment not to be consequential to the results. A full list of participating villages, their commune, treatment status, and their EC is provided in the online Appendix. All control and treatment villages combined make up less than 2 percent of the Beninese electorate.

C. Treatment

In small villages in Benin, local events for presidential campaigns are usually carried out by surrogates or middlemen (usually a local politician), without the presence of the actual candidate. A typical campaign event in Benin is a festive rally where cash and gifts are distributed. The rally is punctuated by short meetings during which surrogates make predominantly targeted or clientelist electoral promises, with relatively few broad policy promises. Banégas (1998, 2003) describes electoral campaigns in Benin.

In the experiment, treatment consisted of the substitution of these rallies by visits from a team of IERPE staff carrying out a town hall meeting following specific instructions. First, they introduced themselves and the candidate they were representing. Second, they gave a 15-minute speech on the key problems facing the country and the specific solutions suggested by the candidate (based on the conference report). Supervision ensured that town hall meetings were uniform even if

⁷Due to missing data issues, estimations will be based on 12 communes (12 treatment and 33 control villages), as will be discussed further below.

endorsed by different candidates. The speech triggered an open debate in which the issues raised were contextualized, and the proposals made were amended by the participants. Meetings lasted between 90 minutes and two hours, and occurred twice a week in the three weeks before election day (March 5, 2006). The distribution of t-shirts and promotional wall calendars was allowed, while the giving of cash or other gifts was prohibited in treated villages.

The control group experienced the usual campaign events (rallies) run by surrogates, with no restriction on distribution of gifts and cash. Candidates agreed not to personally visit the area nearby an experimental village. There was remarkable compliance by all parties involved with the rules of the experiment. Town hall meetings had between 50 and 200 participants, and 70 percent of the population of each village attended one or more.⁸

D. Data

This paper combines data from two sources. The first one is based on our survey of a representative sample of individuals aged 18 years or over in a subset of participating villages. For each randomization stratum, one treated and one control village was surveyed. Eighty respondents were interviewed in each village in the five days immediately after the election (before election results were announced). The surveyed control was randomly chosen in each stratum.⁹ In the remainder of the paper, we use only the village-level averages from this survey. Due to logistical (travel and scheduling) difficulties, research staff was unable to reach the Toffo commune and survey its control and treatment villages. Hence, data from this commune is not included in any of the estimations (including those based on electoral results, discussed below). It must be noted that the reason leading to Toffo not being surveyed is not specific to events in its control or treatment villages, which could potentially generate bias in our results.

The second source of data was the official village-level electoral results, provided by the National Electoral Commission. The results include information on voter registration, turnout, and votes for each candidate in the first round of the election. Such results were missing for the commune of Save, which is hence not included in any of the estimations, and two control villages in Kandi and one in Bembereke.¹⁰ The next subsection discusses further how this missing data is accounted for in the estimation.

The estimations are carried out in two samples (representing the same regions). Both the survey-based and electoral data-based samples have 12 treatment villages, while the former has 12 and the latter has 33 controls. The only commune (Toffo)

⁸The number of actual town hall meetings was six in all treated villages, per the experimental protocol. In two treated villages (in the communes of Dangbo and Zagnanado), there were reports of IERPE staff carrying out much smaller (about ten people) informal meetings. According to their reports, there were also about five to seven smaller, more local and less formally organized rallies in some control villages, although the precise location of those is not known.

⁹We tested for significant differences between randomly chosen surveyed control and other controls in the electoral data (described below). We did not find significant differences between them at the usual levels of significance. Results are omitted due to space considerations.

¹⁰Our main results are robust to excluding the entire communes (strata) of Bembereke and Kandi.

that had Amoussou as the EC is not included in estimations. The online Appendix presents the results including this commune using the electoral data. Given these relatively small sample sizes, we report randomization inference tests for all estimates in the paper, as discussed in the subsection below.

E. Estimation

We estimate treatment effects via the following OLS regression:

$$(1) \quad Y_{is} = \alpha + \beta T_{is} + \gamma_s + \varepsilon_{is},$$

where Y is the outcome of interest for village i in randomization stratum s . T_{is} is an indicator for treatment status, and γ_s is a full set of stratum indicators. β is the treatment effect. Regressions are weighted such that all strata have the same weight in the regression. Nonweighted results are similar to the ones reported. For all estimates of β , we provide the heteroskedasticity-robust standard errors, as well as p -values from a two-sided randomization inference test of zero treatment effects. This test consists of reassigning (using the exact same stratified randomized procedure described above) the treatment and control status in the sample and reestimating β using this placebo assignment multiple (1,000) times. Under the null hypothesis of zero treatment effects, the proportion of reestimated β s that are larger (in absolute value) than the actual β provides a p -value for such null hypothesis. This procedure has the advantage of providing inference with correct size regardless of sample size.

II. Results

A. Effectiveness of Randomization

We first verify the effectiveness of randomization in generating balanced covariates. Table 1 presents the estimated treatment effect (β) for a host of survey-based characteristics in panel A. The variables are village averages of individual dummy indicators based on survey responses (except for average age, measured in years). The averages for the control group are also presented in column 1, and, hence, Table 1 also serves as a summary statistics table to characterize the sample villages. Treatment effects are small and none are statistically significant at the 5 percent level in a t -test based on the standard errors (which are reported on column 3) and all but one in the randomization inference (p -values reported on column 4). Two or three variables (depending on the test) are significant at the 10 percent level. Table 1 indicates that villages participating in the experiment are rural, reliant on family farms, and have little access to infrastructure (electrical lighting). Most respondents (67 percent) have not finished primary schooling and less than 1 in 10 finished secondary schooling or a higher degree.

Panel B of Table 1 tests for balance in the election results-based sample. Only data on the total number of registered voters in the village is provided, as this is the only variable that cannot be seen as a possible outcome (such as turnout or vote

TABLE 1—VILLAGE CHARACTERISTICS IN CONTROL AND TREATMENT GROUPS

	Control mean (1)	Treat.-control (2)	Standard error (3)	Randomization inference <i>p</i> -value (4)
<i>Panel A. Survey data</i>				
Female	0.496	0.009	(0.006)	0.169
Age (in years)	41.707	0.390	(1.133)	0.743
Fon ethnicity	0.501	-0.008	(0.006)	0.210
Yoruba ethnicity	0.141	0.018	(0.012)	0.032
French speaker	0.270	-0.011	(0.031)	0.768
Fon speaker	0.529	0.042	(0.021)*	0.046**
Christian	0.486	0.100	(0.047)*	0.058
Muslim	0.222	-0.062	(0.044)	0.171
Primary schooling	0.245	0.021	(0.029)	0.507
Secondary schooling or higher	0.087	0.028	(0.014)*	0.082*
Single	0.034	0.017	(0.008)	0.052*
Married (monogamous)	0.520	0.007	(0.039)	0.860
Married (polygamous)	0.348	-0.038	(0.044)	0.405
Has regular income	0.408	-0.028	(0.033)	0.424
Owens farm	0.754	-0.075	(0.063)	0.291
Electrical lighting at home	0.052	0.038	(0.029)	0.262
Member of Assoc./NGO	0.364	-0.004	(0.043)	0.929
<i>Panel B. Electoral data</i>				
Registered voters (in thousands)	1.245	-0.054	(0.460)	0.908

Notes: Column 1 reports the mean of the corresponding variable for the control group. Column 2 reports the difference in means between treatment and control group (β from equation (1)). Column 3 reports its robust standard error. Randomization strata dummies are included in all regressions. Column 4 reports the *p*-values based on a two-sided randomization inference test statistic that the placebo coefficients are larger than the actual. The *p*-values were computed based on 1,000 random draws. Number of observations is 24 (panel A) and 45 (panel B). See text for more information on the variables.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

shares), since voter registration closed before the experiment began. The average village is small (in both control and treatment groups), with about 1,250 voters.¹¹

B. Clientelist Practices

Our survey carried several questions related to voter behavior, attitudes, and beliefs. Many could be associated with a broad definition of clientelism, raising the pitfalls associated with testing multiple hypotheses. Focusing on the outcomes with statistically significant results would be misleading, as their nominal level of significance is not the true probability of rejecting the null when they are in fact part of a larger family of tests. We address this issue using Kling, Liebman, and Katz's (2007) procedure. The sign of all variables is altered so that positive values are associated with clientelist practices. Let $k = 1, \dots, K$ denote the outcomes of interested, and μ_k and σ_k denote k 's average and standard deviation in the control group.

¹¹ We do not have reliable population estimates for these villages, as these are very small administrative units.

TABLE 2—TREATMENT EFFECTS

	Control mean (1)	Treat.-control (2)	Standard error (3)	Randomization inference <i>p</i> -value (4)
<i>Panel A. Survey data</i>				
Clientelism index	0.000	−0.227	(0.079)**	0.024**
Clientelism index (exclud. vote-buying)	0.000	−0.223	(0.097)**	0.049**
Vote-buying	0.216	−0.044	(0.028)	0.166
<i>Panel B. Electoral data</i>				
Turnout/registered voters	0.819	0.015	(0.059)	0.764
Residual votes/turnout	0.067	−0.008	(0.013)	0.508
Vote share—experimental candidate	0.529	−0.055	(0.050)	0.250
<i>Vote shares, by candidate position in the village</i>				
1st place	0.665	−0.073	(0.032)**	0.038**
2nd place	0.158	0.036	(0.015)**	0.070*
3rd place	0.059	0.039	(0.013)**	0.005***
4th place	0.036	0.011	(0.014)	0.279
5th and lower placed	0.082	−0.013	(0.015)	0.334
Herfindahl-Hirschman Index	0.512	−0.085	(0.039)**	0.034**

Notes: Column 1 reports the mean of the corresponding variable for the control group. Column 2 reports the difference in means between treatment and control group (β from equation (1)). Column 3 reports its robust standard error. Randomization strata dummies are included in all regressions. Column 4 reports the *p*-values based on a two-sided randomization inference test statistic that the placebo coefficients are larger than the actual. The *p*-values were computed based on 1,000 random draws. Number of observations is 24 (panel A) and 45 (panel B). The variables entering the Clientelism Index are based on questions if respondent: discusses politics with someone, discusses politics with members of other ethnic groups, knows platform of one candidate, found platform convincing, found campaign informative, was informed of candidate qualifications, was informed of country's problems, received cash from campaign, and the number of candidates she knew. See text and the online Appendix for more information on the variables.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

The index is then given by $I = (1/K) \sum_{k=1}^K (k - \mu_k) / \sigma_k$. Hence, this index is the mean standardized effect of all outcomes. By estimating the treatment effect on I , we can test if treatment has an overall effect on the whole family of variables.

The index is based on nine different variables, each coming from different questions. These are based on voter perceptions of the campaign (e.g., finding the campaign informative, knowing a candidates platform) and reported actions (e.g., discussing politics with those outside their ethnic group, having received gifts or cash from campaigns). All survey questions that could be interpreted as a practice related to clientelism were included in the index to avoid another channel of possible tendentious reporting. Hence, in constructing the index we erred on the side of inclusiveness. The online Appendix provides the treatment effects for each individual outcome included in the index, and also details the individual questions.¹²

Panel A of Table 2 presents the estimated treatment effect on the clientelism index. The control mean is (by construction) zero, and treatment lowers the index by 0.227 average standard deviations, an effect that is significant at the 5 percent level both in the

¹²In the vast majority of cases, the estimated treatment effects have the expected sign and relatively sizable magnitudes, but they are usually statistically insignificant at the 5 percent level.

standard *t*-test and the randomization inference test. This suggests that treatment shifts voter behavior in ways that are consistent with smaller presence of clientelist practices.

Among the individual components of the index, only one deals with vote buying, while the remaining variables deal with voter information. Hence, we separate these two issues, reporting the results recalculating the clientelism index while excluding the vote-buying variable. The results, reported on the second line of Table 2, are virtually the same as the ones using the “entire” clientelism index.

Panel A of Table 2 also reports the treatment effect on vote buying (the share of respondents that reported receiving cash from campaigns). The control group mean is 21.6 percent, which falls to 17.2 percent in the treatment group. While this is a sizeable reduction, the effect is not statistically significant (randomization test *p*-value is 0.166). The relatively low reported occurrence of vote buying in control villages could be driven by misreporting by respondents or that vote buying is not so prevalent in these areas, or is at least very targeted, as suggested by Finan and Schechter’s (2012) analysis of Paraguayan campaigns. Unfortunately, our data does not allow us to separate these possibilities. Note that our experimental protocol prohibited only ECs from handing out cash gifts in their assigned treated villages, so that any of the 25 other candidates could have distributed cash, and may have decided to increase (or decrease) their vote buying in response to the new campaign strategy in place.

C. Turnout and Vote Shares

Panel B of Table 2 provides the results using the electoral data. First, we find an almost zero (and statistically insignificant) effect on turnout and the shares of votes that are residual (left blank or not cast to a specific candidate). The small effects on turnout should be interpreted within the context of high turnout (82 percent of registered voters) and the fact that there was no time for citizens to register to vote in response to treatment. In this context, they suggest that an information-based campaign can be as effective as clientelist rallies in motivating voters to attend the polls.

Panel B of Table 2 also presents the effects on vote shares of experimental candidates. Treatment leads to a reduction of 5.5 percentage points, although this effect is not statistically significant. Panel B of Table 2 also presents results on candidates vote shares given their position within each village (i.e., not in the region or a country as whole). The results indicate that the candidate with the most votes in a village loses 7.3 percent of total votes in treated villages, an 11 percent reduction in his vote share. Most of these votes are gained (equally) by the second- and third-place candidate. While these effects are significant at the 5 percent level using both *t*-tests and randomization inference,¹³ they are not in the case of the vote shares of fourth-place and the remaining (fifth- and lower) placed candidates, whose point estimate is also small. Another way of observing this reduction of vote share concentration is

¹³The exception is the randomization inference test for the vote share of second-place candidates (*p*-values = 0.070). Both tests for the third-place candidate case, however, are significant at the 1 percent level.

through the estimated effect on a Herfindahl-Hirschman Index (HHI) of vote shares, which falls by 0.085 from a control mean of 0.512.¹⁴

The results in panel B of Table 2 provide direct evidence that the treatment affected actual voter behavior at the polls. More specifically, a sizable subset of voters switch from voting for the most popular candidate in their village to voting for other candidates. These results, however, do not specify why such a pattern of results occur. First, it is possible that treatment affects vote shares of specific candidates, or that it lowers the vote share of the EC. The evidence we find suggests that neither of these explanations can account for the results in the previous section.

One possible reason for the negative effect on top candidate vote share could be that treatment lowers the vote share of a specific candidate (or a subset of them), which tended to place first in sampled villages. For example, it is possible that treatment reduces Yayi's vote share, and since Yayi is both the top candidate and the experimental candidate in many villages, this could mechanically generate the results in panel B of Table 2. The online Appendix provides the estimated treatment effect on the vote share of all candidates running in the election. The treatment effects are always very small and indistinct from zero, so this possibility cannot account for the results on Table 2.

In the majority of villages, the top candidate was also the EC. This raises the possibility that the results are mechanically driven by treatment having a negative effect on the vote share of the EC, a result similar to Wantchekon's (2003) experiment. While Table 2 reports that the treatment effect on the vote share of the EC is not statistically significant (p -values larger than 20 percent in both tests), the point estimate implies a sizable reduction (5.5 percentage points), so that this possibility cannot be ruled out.

To further address this issue, we exploit the fact that the EC is not the most voted candidate (or even among the most voted candidates) in all villages in the experiment. We estimate the effect in two separate subsamples: one where the EC is "dominant" and is expected to be the first-place candidate, and one where he is not. While in the former subsample, the vote share of the most voted candidate and the EC are likely the same, in the latter subsample that is not the case, allowing us to estimate the effect of treatment on the vote share of the EC separately from the effect on "top candidate."

The subsamples are defined entirely based on predetermined variables (2001 election results and ethnic composition) that could not be affected by the experiment, mitigating selection bias issues. Specifically, the vote share of each EC was (separately) regressed on the commune-level vote shares of the five top candidates in 2001, the size of the communes electorate (in logs) and two variables measuring the share of voters from the Fon and Yoruba ethnicity (from our survey). This regression uses only observations from the control group of our experiment. For each commune, we predicted the vote share of the ECs and defined the dominant candidate as the one with the largest predicted vote share. Note that entire communes (both treatment and controls of the same strata) were assigned to each subsample, maintaining the stratified-randomization balance intact.¹⁵

¹⁴The HHI equals the sum of squared vote shares, hence, ranging from one (one candidate gets all votes) to zero (a very large number of candidates with equal vote shares). The inverse of the HHI is also referred to as the effective number of candidates (or parties), since n candidates with equal vote share have a HHI of $1/n$.

¹⁵The top five (most voted) candidates in the 2001 election were M. Kerekou, A. Hounbedji, B. Amoussou, N. Soglo, and S. Lafia. They collectively obtained 94 percent of all votes in the sample communes. Only the vote shares

The EC was not dominant in three communes: Abomey-Calavi, So-Ava, and Come. Soglo was the EC in the former two, while Yayi was the EC in the latter. The online Appendix lists the dominant candidate for each commune. The results must therefore be taken with caution, as they are based on relatively small samples (three treatment and nine control villages). It must be noted, however, that the randomization inference provides accurate inference (p -values) even in samples of this size.

Table 3 reports the treatment effects estimated separately for these two subsamples. In the communes where the EC was the dominant candidate (panel A), we observe a large negative effect of treatment on EC vote share (13 percentage points).¹⁶ In the subsample where the EC was not the dominant candidate (panel B), treatment increases the support for this candidate by an even larger amount (16.8 percentage points). These effects are significant at the 5 percent level in both tests.

The treatment effect on the share of top candidates is negative in both subsamples, although only significant (at the 10 percent level, in both tests) for the case where the EC is dominant. In the case with nondominant ECs, the estimated reduction in the votes for first-place candidates is less than a fourth of the ECs gain. This implies that treatment shifts votes not only from the top candidate to the EC, but also from other lower placed candidates. Hence, the positive effect on EC vote share is not driven by treatment boosting lower ranked candidate vote shares coupled with the EC not being a top candidate. Treatment specifically boosts the vote share of ECs when they are not dominant.

Note also that this pattern is not driven by differences in the distribution of experimental candidates across each subsample. This can be seen by reestimating equation (1), allowing the treatment effect to be interacted with a dummy indicator for each of the ECs, hence, estimating the treatment effect on the vote share of each candidate. We do so separately for each subsample, allowing us to observe the effect of treatment on EC vote share when he is dominant and when he is not.

These results are provided in Table 3, under the “by candidate” headers. The point estimates indicate that all ECs have a large positive treatment effect when they are not dominant, and a large negative effect when they are. While some of these results are not statistically significant (depending on the test used), it must be noted that some of these estimates rely on a small number of observations (e.g., Yayi is not dominant in only one commune, and Soglo is dominant in only one commune). More importantly, the point estimates provide no support to the notion that the differential effects in the two subsamples are driven by differences in their ECs.

As a robustness check, the online Appendix presents the estimates of Tables 2 and 3, excluding the six communes where Yayi was the EC. All the qualitative results remain, suggesting that the above pattern of results is not specific to Yayi’s (relatively larger) participation in the experiment.

of these five candidates were available in the 2001 commune-level results. Of these candidates, only Hounbedji and Amoussou ran for office in 2006. They both agreed to participate in our experiment. N. Soglo is the father of L. Soglo, who is also an EC.

¹⁶In the sample where the EC is dominant, in most of the cases he is the most voted candidate in all the villages in the commune. The two exceptions are the communes of Tanguieta and Zagnanado. This explains why the effects on first-place vote share and EC vote share are not the same in panel A of Table 3.

TABLE 3—TREATMENT EFFECTS BY DOMINANCE OF CANDIDATES

	Control mean (1)	Treat.-control (2)	Standard error (3)	Randomization inference <i>p</i> -value (4)
<i>Panel A. Subsample where experimental candidate is dominant</i>				
Vote share – 1st place	0.699	–0.084	(0.041)*	0.062*
Vote share – experimental candidate	0.669	–0.129	(0.047)**	0.023**
<i>Vote share of experimental candidate, by candidate</i>				
Houngbedji	0.765	–0.122	(0.079)	0.128
Yayi	0.681	–0.137	(0.072)	0.130
Soglo	0.221	–0.109	(0.099)	1.000
<i>Panel B. Subsample where experimental candidate is not dominant</i>				
Vote share – 1st place	0.561	–0.039	(0.036)	0.527
Vote share – experimental candidate	0.107	0.168	(0.057)**	0.016**
<i>Vote Share of experimental candidate, by candidate</i>				
Yayi	0.259	0.293	(0.073)***	0.233
Soglo	0.031	0.106	(0.036)**	0.065*

Notes: Column 1 reports the mean of the corresponding variable for the control group. Column 2 reports the difference in means between treatment and control group (β from equation (1)). Column 3 reports its robust standard error. Randomization strata dummies are included in all regressions. Column 4 reports the *p*-values based on a two-sided randomization inference test statistic that the placebo coefficients are larger than the actual. The *p*-values were computed based on 1,000 random draws. Number of observations is 33 (panel A) and 12 (panel B). See text for more information on the variables.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

D. Interpretation

The results on vote shares can be summarized in the following manner: (i) treatment lowers the vote share of the dominant (most voted) candidate in the village; (ii) treatment has a large positive impact on the vote shares of ECs when they are not the dominant candidate, but a large negative effect when the EC is dominant.

A possible explanation for item (i) is that in an information-deficient and clientelist environment, the dominant candidate has an advantage in boosting his vote share. Voters may find it “natural” to vote for the candidate with a stronghold in the area. The arrival of more information and deliberation leads to more electoral competition.

Item (ii) indicates that a candidate can gain a substantial amount of votes by endorsing town hall meetings in the strongholds of his opponents. Hence, it suggests the possibility of an alternative to clientelism. Note that this result is not driven by differential campaigning in control villages when the experimental candidate is dominant or not. The experimental protocol required candidates to campaign with similar intensity in control villages both in their strongholds and outside them, and the assignment of candidates to communes was made in a way to align this with candidates’ previous plans. To the best of our knowledge—based on reports from IERPE staff—the number of control rallies by an experimental candidate was homogeneous throughout communes. Of course, there are caveats to this interpretation as we do not observe the different inputs (quality of middlemen, their effort, budget, etc.) that entered into the clientelist rallies at different places.

Finally, our findings of no effects on turnout suggest that information, when compared to clientelism, does not discourage voters from going to the polls. Although

the experiment does not directly address issues related to campaign costs, the fact that providing information for a large group of voters is likely cheaper than giving cash to them raises the interesting possibility of information-based campaigns being a more cost-effective way of mobilizing voters than cash distribution.¹⁷

III. Conclusion

In a field experiment in Benin, we facilitated an expert-led, information-based, deliberative campaign and compared its effect on political behavior to standard clientelist rallies. We find that such a campaign has a negative effect on self-reported measures of clientelism, and no effect on observed voter turnout.

Results based on election results indicate that the vote share of the “dominant” (most voted) candidate is reduced by treatment, even in cases where the dominant candidate is the one endorsing our campaign strategy. We also find that candidates experience substantial vote gains by engaging in our treatment campaign in areas where they are not dominant. We interpret this as the arrival of information and deliberation having an effect in reducing the vote share of the candidate who is in the best position to exploit targeted redistribution.

Moreover, the results shed some light on clientelism’s persistence while also suggesting the possibility of an effective (from a self-interested politician’s point of view) alternative in some contexts. On one hand, the negative effect of treatment on the vote shares of endorsing candidates where they are “dominant” helps explain why they use clientelist strategies. On the other hand, the positive effect of endorsing town hall meetings in areas where they are not dominant suggest that candidates can gather a much larger number of votes by entering their opponents strongholds with a campaign based on information rather than clientelism. This would imply that every candidate may find it optimal to follow clientelism in his strongholds while pursuing our alternative strategy in his opponents’ strongholds. Given that a vote in any region is worth the same for a politician (elections are at large), this logic introduces the possibility of an equilibrium in which all candidates enter their opponents’ strongholds with an information-based campaign, generating an overall more informed, more competitive, and less clientelist political environment. Such results, however, rely on the strategic considerations and general equilibrium effects of our treatment campaign strategy. These are issues that the experiment can shed little light on, since it involves a limited number of villages covering a small fraction of the total electorate.

Attempting to understand the role of such strategic considerations and general equilibrium (“scale-up”) effects are likely fruitful directions for future research. Another such direction would be to investigate the role of the information provided in the campaigns. Our experiment does not provide variation in information content within treated villages, leaving to future studies the task of determining which issues have more effect on voter behavior, the best way to frame information, and the best source for content. Finally, the results from this experiment may be dependent on its particular context (small villages in Benin), and its replication to other regions would also be valuable.

¹⁷The costs of providing treatment in this experiment, using IERPE staff, are unlikely to be representative of the costs candidates face in the case of widespread scale-up of the strategy.

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