

CAN INFORMED PUBLIC DELIBERATION OVERCOME CLIENTELISM? EXPERIMENTAL EVIDENCE FROM BENIN*

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Abstract

This paper provides experimental evidence on the effect of "informed" town hall meetings on electoral support for programmatic, non-clientelist platforms. The experiment takes place in Benin and involves real candidates running in the first round of the 2006 presidential elections. The treatment is a campaign strategy based exclusively on town hall meetings during which policy proposals made by candidates are "specific" and informed by empirical research. The control is the "standard" strategy based on campaign rallies followed by targeted or clientelist electoral promises. We find that the treatment has a positive effect on self-perceived knowledge about policies and candidates. The data also suggests a positive effect of the treatment on turnout and electoral support for the candidates participating in the experiment. The results suggest that new democracies may contain electoral clientelism by institutionalizing the use of both town hall meetings in electoral campaigns and policy expertise in the design of electoral platforms.

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INTRODUCTION

Broad public goods such as public education and universal health care play a crucial role in promoting economic development.¹ But, electoral promises to provide such goods are ineffective. By contrast, targeted redistribution and clientelism are bad for development, but they are quite effective in generating electoral support.² Wantchekon (2003) provides experimental evidence on factors that reduce the electoral appeal of such clientelist platforms: gender, ethnic solidarity and access to information. He finds that women, voters from same ethnic group as the candidate and those with better media access tend to be more receptive to public good platforms. But the paper proposes no solution, no alternative to clientelism.

The literature in economics and political science has similar shortcomings. Most scholars tend to assume the impossibility of any real alternative to clientelism, at least in the short run. Lemarchand (1972) and van de Walle (2007) view clientelist practices as the reflection of agrarian social relations and ethnic cleavages that will persist, unless these relations evolve and become more impersonal and these cleavages become less politically salient.³ For Keefer (2007), clientelism dominates in new democracies because political parties have not been around long enough to overcome the credibility gap associated with the broad based public goods promises. They make clientelist promises because it is easier for voters to believe these than promises to deliver public goods. Implicitly, clientelist practices can recede only when democratic institutions are old enough and politicians have interacted long enough with voters to boost their credibility.

There is a clear parallel between these structural arguments about the persistence of clientelist practices and modernization theory. According to modernization theory, democratization becomes possible only when countries reach a sufficiently high level of economic development. A similar argument in our context would be that non-clientelist or programmatic politics can appear only when countries reach a sufficiently high level of political development. Przeworski et al (1997) have shown that democratization is possible at low levels of economic development so long as key political actors choose the "right" strategies.

¹See 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.

²See Wantchekon (2003), Alesina and Rodrik (1994), Alesina, Baquir and Easterly (1999)

³See Kitschelt Herbert and Steven Wilkinson (2007) for a general survey.

Analogously, we aim to show that programmatic politics are possible even in presence of "backward" social relations and strong ethnic cleavages. This may happen if, for instance, voters and politicians's expectations are "properly" aligned so that public goods promises become more credible. We therefore need to identify campaign strategies that would shape voters' expectations about politician future actions.

We believe that finding and empirically testing these strategies is crucial to current debates on clientelism. First, such results would give more prominence to the idea that clientelism is, in large part, driven by political circumstances rather than by structural factors. Second, these findings would provide badly needed practical guidance on how to overcome political obstacles to policy-making in developing countries.⁴ Third, if clientelism and low development are mutually reinforcing as the literature suggests, this would mean that developing countries are "trapped" in a bad political economic equilibrium: low development strengthens clientelist politics, which lowers development even further. In the absence of exogenous structural shocks that could change the nature of this equilibrium, the only way to break the clientelist trap is to introduce strategies that shape expectations of voters and of politicians in a way that makes public goods platforms electorally effective.

In this paper, we propose and experimentally test such strategy: electoral campaigns based on town hall meetings, led by "expert-designed" and specific policy promises. We find that such strategies have a positive effect on some measures of voter information, turnout and electoral support for the candidate running the experiment. Given the data limitations of the study, there is a possibility that the positive effect of the treatment on turnout and voting was due to chance. But these results represent a sharp contrast with the results of the 2001 experiments where public goods platforms were found to have a negative and significant effect on electoral support for the candidate running the experiment. We conclude that political parties can overcome the need to win votes through targeted policies or even through the distribution of largesse to a set of voters by improving the extent to which their policy promises are informed by empirical policy research. To put it differently, policy targeting and policy specificity may be substitutes. One may contain electoral clientelism by institutionalizing the use of both town hall meetings in electoral campaigns and policy expertise in the design of electoral platforms.

One important question arising from the result is whether the effect is mostly driven by the information content of the platform or by institution of the town-hall meeting. In

⁴See Persson Torsten and Guido Tabellini (2000) and Bardhan (2002) for a general discussion and survey.

other words, we may want to isolate the intrinsic effect of public deliberation through town meetings from the effect of showing policy expertise. Using results derived by Atchade and Wantchekon (2009), we derive a lower bound to the intrinsic town hall effect and discuss how this issue can be dealt with in similar future projects.

Why would town meetings and expert information generate electoral support for programmatic platforms? This is because expert information generates specific platforms and town meetings facilitate voter coordination.⁵ When platforms are specific, they enable parties to make promises on public goods and transfers that are credible⁶. When voters interact in town meetings, they learn about each other's preferences and beliefs. As such, the treatment generates clear benchmarks candidates have to meet to avoid being punished by voters in future elections. The meetings also facilitate coordination between voters in punishing those politicians who failed to keep their promises. Thus, through a variety of mechanisms, the treatment makes policy promises more transparent and credible and hence more likely to generate electoral support.

Relation with the literature

There have been several studies in contemporary political science that provide empirical evidence of the effect of increased policy knowledge and various forms of public deliberation on political behavior. The political ignorance literature in American politics indicates that increased policy information affects policy preferences and voting behavior. In particular, Gilens [2001] finds that the dissemination of facts about crime rates and foreign aid have a significant effect on policy preferences and political judgments. However, the results generated in these studies are based on US data and it is unclear if the information effect would transcend ethnic or religious cleavages, or if the effect would be of the same magnitude given the low level of formal education in most developing countries.⁷

The deliberative democracy literature (Gutman and Thompson [1996] and Fishkin [1997] among others) find that public deliberation promotes “enlightenment”, consensus, and civic engagement. One limitation of this literature is that it is mostly theoretical and the evidence that it provides comes from deliberative polls and focus groups, rather than from the field.

⁵ Austin Smith and Federson (2008)

⁶This contrasts with Keefer and Vlacu (2008) who suggests that credibility comes with age of party politics.

⁷See also Bartels [1996] and Delli Carpini Michael and Scott Keeter [1996].

As a result, it is unclear how policy information provided in the context of public deliberation would affect voting behavior in real elections.

A key to our theoretical framework is that specific platforms and village discussions in town hall setting make public goods promises more credible because benchmarks are clear and voters can coordinate to punish candidates in future elections. This argument relates to the literature on voter coordination, which describes situations where voters have heterogeneous expectations about the future election outcomes⁸. The paper also relates to models of information revelation in committee debates in the presence of individuals' private preferences.⁹ But in the voter coordination literature, coordination is shaped by expectations about future elections, not the information content of platforms and the outcome of public discussion of these platforms. In addition, the information revelation literature is mostly theoretical and is applied to decision-making in committees, not to voting in large elections.

The paper also contributes to the literature on access to information, political institutions, and local public goods. Olken [2008] provides experimental evidence from Indonesia that suggests that direct elections are better than representative-based meetings in generating popular satisfaction and support for local public goods. Reinikka and Svensson [2005] find that media access reduces local capture of public funds and subsequently leads to higher school enrollment and test scores. However, these studies focus on local public goods and ignore political incentives at the national level.

The methodology, the context, and the results of the Benin experiment reported in Wantchekon [2003] are particularly relevant to the current study. That experiment aimed at testing the effectiveness of clientelist versus programmatic electoral campaigns on voting. The experiment consisted of randomized trials in 24 villages in which politicians used either a clientelist, programmatic, or neutral election campaign. The results suggested that the clientelist electoral campaign is most effective, and that the programmatic election campaign costs votes. However, women, more informed voters, and co-ethnics of the candidate running the experiment were more responsive to programmatic platforms than men, less informed voters, and non co-ethnics (respectively). One question that arises from that study is whether clientelism is the only effective electoral campaign strategy. Indeed, it could well be the case that the lack of electoral support for programmatic platforms compared to clientelist platforms was due to its use of overly general or vague campaign messages such

⁸See. Melbane (2000) and Melbane and Seckon (2002), Cox (1997) among others.

⁹Austen Smith and Feddersen (2008)

as:

“Our party stands for democracy and national solidarity. If elected, our candidate will engage in a nationwide reform of the education and the health care system.”

It is possible that the results would have been different if the candidate taking part in the experiment, had made more specific and informed policy promises such as:

“if elected I will provide full medical insurance for all HIV patients and provide free primary education in all rural schools. I will pay for these programmes by cutting subsidies to cotton growers by 50%”.

We address this question by providing evidence from a follow-up to the 2001 Benin experiment. The experiment took place in Benin and involves candidates running in the first round of the 2006 presidential elections. However, in this case, the treatment is a mechanism for generating programmatic platforms, namely a two-stage deliberative campaign. The first stage was a conference involving academics, policy experts, all major candidates, and political parties that were represented in the National Assembly. The second stage was a series of town hall meetings during the electoral campaign. The meetings were led by party activists informed by the results of the policy conference. In other words, in this experiment, districts and villages were *randomly* assigned to *non-clientelist institutions* for generating electoral support, e.g. "informed" town hall meetings. The first part of the treatment (expert deliberation) enables parties to devise very specific policy platforms and the second part (town meetings) allow these platforms to be delineated to and amended by voters. The control units were assigned to the standard clientelist institutions, e.g. cash distribution, promises of targeted redistribution at festive campaign meetings.¹⁰

The current experiment is step forward, compared to the old one, in terms of external validity. The new experiment covers more regions, allowing for more variation in background conditions. While the 2001 experiment took place only in rural districts with weak media access that were strongholds of the experimental candidates; the current experiment took place in both urban and rural districts. There are districts with very strong media coverage and others with weak media coverage. Some of the districts are strongholds of a given

¹⁰See Banegas (1998) for a vivid description of institutions governing clientelist electoral campaigns in Benin.

candidate, others are competitive. While only "dominant" candidates were involved in the 2001 experiment, the new experiment has minority candidates or underdogs and majority or dominant candidates.

The rest of the paper is organized as follows. Section II presents the context, section III the design the experiment, section IV the data and the main results. We then discuss issues related to external validity and the implications of the results for institutional design in section V. Section VI concludes.

II. CONTEXT

The experiment took place in Benin (formerly Dahomey), a West African country located between Togo and Nigeria, with a population of about eight million people. Benin was colonized by France in 1894, but gained independence in 1960. The first twelve post-independence years were characterized by political instability, with alternation of civilian and military rule. The country experienced its fifth and last military coup in 1972. The coup paved the way for a dictatorial regime led by Mathieu Kerekou, which lasted for eighteen years.

Benin achieved a successful transition in 1989 from a Leftist dictatorship towards a pluralist democracy. Since then, democratic institutions have been strengthened with four presidential elections in which incumbents lost twice.¹¹ There has been high turnover in the National Assembly. In 2006, the country ranked 2nd in Africa and 26th in the World in terms of freedom of the press by "Reporters without Borders".

Despite progress towards democratic consolidation, economic performance has been very weak. According to the Benin Country Memorandum published by the World Bank in June 2008, the country has a lower per-capita growth rate, and weaker institutional performance (law enforcement, regulatory agencies and government effectiveness), than other African democracies. Corruption is widespread and the country is ranked quite low in terms of its governance index (37th in Africa).

Using evidence from the Database on Political Institutions, the World Bank Country Memorandum on Benin finds that while 60% of the top four parties in a typical democracy can be described as programmatic, in Benin none of them can be described as such. This

¹¹ Presidents are elected by a plurality runoff system. That is, if no candidate achieves a majority during the first round, a second round is organized for the top two candidates on the list and the plurality winner is elected.

is quite surprising for a country with a long leftist tradition that has experienced seventeen continuous years of democracy. In fact, all of the top four parties in Benin were founded by either Marxist or leftist ideologues (Amoussou Bruno of the Parti Social Democrate and Saka Lafia of the Union pour Democratie et la Solidarité), or by market reform ideologues (Nicephore Soglo of the Renaissance du Benin and to a lesser degree Adrien Houngbedji of the Parti pour le Renouveau Democratique).¹²

In addition, the first four years of democracy under a technocratic Soglo government and a programmatic “Renaissance du Benin Party” were characterized by a high growth rate (6.2% from 1990 to 1994) and good governance indicators.¹³ The move away from relatively programmatic to much more clientelist politics started with the 1996 campaign that led to the return to power (by the means of democratic elections) of the former dictator Mathieu Kerekou. He won by capitalizing on accusations that his opponent was undermining democratic pluralism, and by promising smaller parties better access to government. In short, programmatic politics dominated in the first post-transition election because there was a strong demand for market reforms, and because the top candidate in that election was an experienced technocrat and a credible and competent reformer. Clientelism dominated from 1996 to 2006 when Kerekou needed it to reward members of the broad coalition of small parties that brought him back to power in 1996. Thus programmatic and clientelist politics are strategic choices driven by electoral circumstances.

However, better governance under Soglo seemed to have come at the expense of democratic pluralism, and better democratic pluralism under Kerekou came at the expense of good governance.¹⁴ The goal of this experiment is to propose a set of conditions under which one might have good governance *and* democratic pluralism.

The 2006 presidential elections were the first since 1990 without the traditional “big men” Kerekou and Soglo. They were ineligible to run under the age limits and term limits set by the constitution. There were twenty-six candidates competing in the election, but only four were serious contenders capable of securing more than two percent of the vote. The top two candidates were Yayi Boni, a former President of the West African Development Bank, running as an independent candidate but supported by a coalition of small parties

¹²If it had not boycotted the national conference, the Parti Communiste du Benin (PCB), a hardline communist party, would have been one of the top parties.

¹³See Wantchekon and Ngomo [2001].

¹⁴To put it differently, economic indicators were much better under Soglo than they were under Kerekou, but “freedom indices” were higher under Kerekou than they were under Soglo.

(as Kerekou was in 1996), and Adrien Houngbedji, a former cabinet member in Kerekou's Government, and the candidate of the Party for Democratic Renewal (PRD). The other serious candidates with some outside chance of making it to the second round were Amoussou Bruno of the Social Democratic Party (PSD) and Lehadi Soglo, the son of former president Nicephore Soglo, and the candidate of Renaissance du Benin (RB). The main theme of the election was better governance with strong anti-corruption measures and better public services.¹⁵

III. THE EXPERIMENT

The experimental process started with a policy conference that took place on December 22, 2005, entitled "Elections 2006: What policy alternatives?". There were about forty participants and four 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 practice in policy implementation.¹⁶ All the parties represented in the National Assembly were represented at the conference. There were also representatives of several NGOs and officials from the European Union, the Konrad Adenauer Foundation (a co-sponsor of the event along with the Institute for Empirical Research in Political Economy in Benin). The proceedings of the conference can be download from *www.ireep.org* and the final report is available upon request.

The final report contains a wide range of policy proposals such as community-funded health insurance, school-based management, and random audits of politicians and other anti-corruption measures in the spirit of Svensson and Reinikka [2003].

After the conference several political parties and candidates volunteered to experiment with the proposed campaign strategies. Together, these parties represent a projected 85% of the electorate. They are: Union pour la Democratie et la Solidarité (UDS), Impulsion pour le Progres et la Democratie (IPD), Congrès Africain pour le Progres (CAP-SURU), Renais-

¹⁵See Gisselquist [2006] for a detailed report and analysis of the election. See also Banegas [1998] for a study on clientelism in Benin.

¹⁶The four experts were Professor Leonard Fourn who teaches Public Health at the University of Abomey Calavi, Dr. Hamissou Oumarou, an Education Expert from Niger, Dr Mouftaou Laleye, who taught Public administration at the University of IFE in Nigeria, and Mr Todjinou Jean Bosco, an architect and Urban Planning specialist.

sance du Benin (RB), Parti Social Democrate du Benin (PSD) and Parti du Renouveau Democratique (PRD).

The experiment followed a randomized block design with treatments being assigned to 12 randomly selected subunits (villages), in 12 randomly chosen units (electoral districts) in the population, which consists mostly of stronghold districts in Benin that are dominated by the four experimental candidates i.e. the candidates participating in the experiment. The selection process is as follows:

Denote by N_s the number of electoral districts controlled by candidate $s \in \{1, 2, 3, 4\}$, where candidate s is an experimental candidate. Then $N = \sum N_s$ is the total number of electoral districts involved in the experiment. Within each electoral district j , there are n_j villages. The randomization process consists of the following four steps:

Step 1. There are 77 districts in the country. Candidate s or a party endorsing s randomly draws 1 to 3 districts (say j , k and/or l) out of N_s districts, depending on the size of N_s .

Step 2. There are on average 50 villages per commune in the country. Candidate s randomly draws one village from the n_j villages in district j and randomly draws one village from the n_k villages in district k , and assigns the village picked to treatment.

Step 3. Among the $n_j - 1$ remaining villages in district j and the $n_k - 1$ remaining villages in district k , the candidate removes from the pool those villages that are contiguous or in the immediate vicinity of the village picked in stage 2. Then draws randomly two to three villages from the remaining villages in districts j and k . The two or three villages picked serve as the comparison group. For the geographic locations of the participating districts and villages, see the the Benin Map in appendix.

Once the assignment of electoral districts to treatment and control groups was completed, there were pre-electoral surveys on the policy priorities of voters in the treatment units. Finally, teams of campaign workers were instructed with specific policy responses to voters' concerns about the quality of public schools, youth employment, malaria prevention, etc. They were also given specific instructions on how to run the town meetings: First, they introduce themselves and the candidate they are representing. Next, they give a fifteen minute speech on the key problems facing the country and the specific solution suggested by the candidate. The speech triggers an open debate in which the issues raised are contextualized, and the proposals made are amended by the participants. The meeting would last between ninety minutes and about two hours. The teams would run six to ten such meetings over two weeks in each village. There were about 50 to 200 participants in each

town meeting, and treated villages ranged from 360 to 2,926 inhabitants. In our estimation, about 70% of the population of each village attended one or more town meetings.¹⁷

While villages in treatment groups received and deliberated over informed and broad-based policy proposals, villages in the control groups received a mixture of targeted or clientelist campaign promises as well as very few broad but *less informed* policy promises. Indeed, 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 of the candidate make predominantly targeted electoral promises.¹⁸

There were no major differences between treatment and control groups in terms of exposure to and intensity of political campaigns. In each group and each village there were eight to ten meetings, and campaign workers in the treatment group had about the same level of education as those in control villages (about two years of college).¹⁹

After the elections, we collected at the relevant sites, data on turnout in treatment and control precincts with the help of representatives of the National Electoral Commission. We also surveyed a representative sample in each group on demographic variables (age, gender, marital status and ethnic affiliation), socioeconomic variables (educational attainment, economic activities, and assets) and political variables (preferences over candidates and voting behavior). The survey data covers all districts except Toffo.

IV. THE DATA AND THE RESULTS

INTERNAL VALIDITY AND COMPLIANCE

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-

¹⁷The treatment met the requirement for efficient public deliberation discussed in Lupia (2002). He wrote: “For a deliberative endeavor to increase participation, or affect how a target audience thinks about an important political matter, its informational content must, at a minimum: (1) attract the audience’s attention and hold it for a non-trivial amount of time; (2) affect the audience’s memories in particular ways (not any change will do); and (3) cause them to retain subsequent beliefs – or choose different behaviors – than they would have had without deliberation.”

¹⁸For details on the nature of electoral campaigns in Benin, see Banegas [2003].

¹⁹We can also assert that campaign workers in treatment groups were not more motivated than the ones in control groups. In fact, teams in control villages could well be more motivated because they were better paid and had closer contacts with the candidates.

treatment variables in the treatment group and the control group. We focus on the following covariates: ethnic ties, education level, media use, gender, and age. The selection of these covariates is motivated by the results of the 2001 Benin experiments indicating a significant conditional effect of gender, ethnic affiliation, education, access to media, and public goods treatment on voting.

Table 1A indicates that there is no significant difference between the means of these variables in the treatment and the control groups, except for education and to a lesser degree newspaper readership. In other words, voters in treatment groups are, on average, more educated than those in the control group. But there is no significant difference in terms of gender, age, ethnic ties, and media use. Therefore, in estimating the treatment effect, we need at least to control for education.

Insert Table 1A here

Next, as a check of internal validity, we investigate whether voters exposed through public deliberation to expert policy information were less involved in clientelist practices. Indeed, one of the terms of the contract between IREEP (the institute running the experiment) and the candidates participating in the experiment was that while T-shirts and calendars could be distributed to voters in treatment villages as in all other villages, there would be no cash distributions by them in the treatment villages. Thus, a further test of internal validity would be whether treated voters were less likely to receive cash from candidates than untreated voters.

Table 1B suggests that a lower proportion of voters in treatment villages reported to have received cash during the campaign than in control villages (16% to 21% for cash).²⁰ However, there is a much smaller difference across groups in terms of those who received T-shirts (2.0% to 2.1%), or calendars (7.2% to 7.27%).²¹

VOTER INFORMATION

We first evaluate the effect of the treatment on a measure of voter information. First, note that a significantly larger number of respondents in the treatment group described the

²⁰Other candidates who are part of the experiment in the district can distribute cash to their supporters. This may explain why some voters in treatment villages did receive cash.

²¹Given the pledge by the experimental candidates to restrain from clientelist practices in treatment villages, there was lower supply of cash in treatment villages.

campaign as informative, in comparison with the control group (65.6% in the treatment group as opposed to 58.9% in the control group). Thus, the treatment was effective in making treated subjects believe they were more informed than those who were not treated.

The results clearly highlight the contrast between treatment and control villages. Both types of villages were given very similar levels of attention by the parties, as evidenced by the proportion of voters who received T-shirts and calendars in each group. However, voters in treated villages received less cash and were better informed than voters in control villages.

Insert Table 1B here

In the post-election survey, voters were asked the following three questions: (1) Did the campaign give you information about the quality of the candidates? (2) Did the campaign give you information about government and how it functions? (3) Did the campaign give you information about the problems facing the country?

The question that best captures the concept of voter information is the one on the problems facing the country and to a less degree the one on the quality of the candidates. Information on governments is a measure of the level of civic education rather than a measure of voter information. Thus, we will focus our attention on (1) and (3).

We test for the treatment effect on voter information, by using the following probit model.

$$P(Y_{ij} = 1 | z_{ij}, T_i) = P(z_{ij}a + T_i\beta + x_{ij}T_i\gamma + u_{ij} > 0)$$

$$u_i \stackrel{id}{\sim} N(0, \Omega_i)$$

where Y_{ij} is a categorical variable that takes the value of one if individual j in village i provides a positive response to questions (1), (2) and (3), and zero otherwise; z_{ij} is the vector of individual characteristics for individual j in village i , and T_i is the categorical variable for treatment in village i . The vector z_{ij} includes variables such as age, gender, level of educational attainment, ethnic ties with the candidate, and media access. Income level was measured by using an index of housing quality, constructed from factor analysis of five independent variables (roofing, ground, number of rooms, etc.). The key independent variable is T_i , 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.

In each specification we present the results without any controls, then we control for the two covariates that are not balanced between treatment and control groups (i.e. education and media access). Finally, we control for all potentially relevant covariates.

Tables 2A, 2B, present the results for information about policy and candidates. In all specifications except one, the treatment has a positive and significant effect on policy information. The results are significant at the 99% level without clustering and the 90% level with clustering. As for information about the candidates, the treatment has a positive effect in all specifications. The results are significant at the 99% level without clustering and the 95% level with clustering. Education and gender are highly correlated with voter information. More specifically, male voters are more likely to find the campaign informative with regards to policies and candidates. Ethnic ties are a good predictor of voter information about candidates, but not about policies, and media access has no significant effect.

Insert Tables 2A and 2B here

We now turn to the variable measuring how effective the campaign was in informing voters about government and its functions. Table 2C suggests that the treatment did not have any effect in any of the specifications. However, as expected the education coefficient is positive and significant. In contrast to voter information, the media coefficient is positive and significant, which indicates that media access plays a more meaningful role in civic education than in campaign information.

Insert Table 2C

The result on self-perceived knowledge about government is very much expected since the campaign provides virtually no information about government. The goal of the experiment was to inform voters about the candidates' programmes, not about the internal structure of the government.

In summary, the treatment makes the electoral process more transparent, and voters better informed or simply better citizens. For proponents of deliberative democracy and those who care about the quality and quantity of political participation, this should be the ultimate prize of our political experiment (Habermas [1996] and Lupia [2008]). The results clearly indicate that deliberation in the form of town hall meetings is relevant for citizenship not only in localized policy debates as in Luskin, Fishkin and Jowell, (2002), but also in the context of real elections, when the stakes are very high. We make this point more transparent in this next section by investigating the effect of the treatment on turnout

TURNOUT

Turnout is a fundamental variable of interest in the study of democracy and political participation that 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. In this paper, we find that deliberative campaign strategies can improve civic engagement in the form of higher turnout.

The turnout data was collected at various voting booths immediately after all the votes were counted. We collected the results from eight districts involved in the experiment.²² As we mentioned earlier, the data on electoral behavior originated from a survey that took place five days after the election.

Table 3 suggests that turnout was significantly higher in treatment villages than in control villages, in seven out of the eight districts. There is only one case (the district of Comè) in which the treatment village (70%) has a smaller turnout than the control village (77%).

Insert Table 3

In other words, under a variety of background conditions (e.g. rural versus urban districts, Muslim versus Christian districts, etc.), and under a variety of platforms generated by the assigned treatment, we find that the expert-led deliberative campaign generated increased turnout by an average of 7.3%. The total number of registered voters in the electorate was 3,917,865. This means that there would have been on average 286,004 more voters at the polls, if the experiment were run in all 77 communes in the country.

The conventional wisdom in comparative politics is that clientelism and vote-buying are the only reliable way to drive voters to the polls (Brusco, Nazareno and Stokes [2004]. Banegas (2003) has gone further and presented clientelist redistribution as a form of civic virtue and a decisive factor in voter turnout. In sharp contrast with these claims, more voters went to the polls in treatment groups than in the control groups (81% to 73.87%) despite the fact that fewer of them received any largesse from the political parties during the campaign (16% to 21%). An estimated 5.5% of the electorate received cash and did not vote, and 68% of the electorate voted despite not receiving cash. In addition, the evidence suggests that an informed or treated voter who did not receive cash was much more likely

²²Due to logistical problems, the results from the districts of Zagnanado, So Ava and Zagnanado were not available to us on the election day, so those districts were not included in Table 1A.

to turnout to vote than a voter who received cash but was less informed (68% to 15.3% of registered voters). In other words, information is clearly a more powerful tool in driving voters to the polls than monetary incentives. Therefore, if it could be cheaply provided, information might be a more cost-effective way of mobilizing voters than cash-distribution.

VOTING

Table 4A uses data collected from the electoral commission on the outcome of the election in treatment and control villages. Overall, the experimental candidates garnered 66.7% of the vote in the treatment villages, compared with 60.7% in the control villages. In one commune (Kandi) the results were approximately the same for the experimental and control villages. In four out of seven cases, the experimental candidate gained more votes in the treatment villages, with the treatment effect being particularly strong in Gadome I and Yaoui.

However, in two districts out of seven the experimental candidates fared better in the control villages. For instance, in Kouande, the experimental candidate gained a slightly higher percentage of votes in the control versus treatment group. This may be explained by an unexpected rally by the candidate participating in the experiment in that district, Yayi Boni, just two days before the election. There were no such rallies in any other district participating in the experiment.

Insert Table 4A here

There are three districts that took part in the experiment that are missing from Table 4A because we could not get an accurate vote count from these districts on election day (Abomey Calavi, So Ava and Zagnanado). In these districts, the participating political party was Renaissance du Benin and the experimental candidate was Lehadi Soglo. In sharp contrast with the other experimental candidates, Soglo was the underdog in each district. Table 4B presents the vote shares of the candidate using the post-election surveys. The results are strikingly similar to the one described in Table 4A. In two districts (So Ava and Abomey Calavi) the treatment effect is positive and in one district (Zagnanado) the effect is negative. Thus there is a strong indication that the average treatment effect would have remained positive had we included the three missing districts.

Insert Table 4B here

By design, most districts selected for the experiment (8 out of 12) were those where the candidate was expected to win more than 50% of the vote and all the other 25 candidates put together would receive 50% of the vote or less. As a result, the campaign strategies of the other candidates could be, in those cases, considered as fixed and would have no effect on the platform of the candidate participating in the experiment. Thus, we can ignore any strategic interaction between that candidate and the other candidates. In addition, because we have balanced covariates across treatment and control, the results give us an indication of how much more or less support “our” candidate may have had if the experimental campaign were run in every village of the district.

The results suggest that Yayi would have received 17.7% more votes in the district of Ouesse, and 11% more in district of Bembereke, if he were to scale up the experiment in those districts. Furthermore, assuming that each voter receives \$20 during the campaign, that means he would have spent \$56,237 less in the rural district of Ouesse, while improving his vote share by 18%. Nationwide candidate Yayi would have saved \$1,074,308, while improving his vote share by at least 6%. The money saved in Ouesse would have been enough to build two additional fully equipped health clinics in a district that currently has only one.

Now assume that in the next election all the other candidates were to run the type of campaign prescribed by the experiment, so that running such a campaign gives no sizeable electoral advantage for any candidate. It will still be the case that 7% of additional voters would have been informed about the government policies and the candidates, and the candidates themselves would have saved \$4,701,436 in terms of campaign spending, which represents about 14% of public spending on primary education in Benin.²³ In addition, knowing that information is at least as powerful as vote-buying in generating electoral support, candidates might have an incentive to invest relatively more in generating informative platforms than distributing cash and gadgets. This would improve the quality of political participation in the country.

Finally, the fact that the effect of the information treatment in the 2006 experiment bears strong similarities to the clientelist treatment effect of the 2001 experiment suggests that expert information could be a reliable substitute to or possible “cure” to clientelism.

As an alternative test for the treatment effect on voting, we use the probit model presented

²³According to government reports, public spending on primary education was about about 31 millions US dollars in 2007.

in Section IV. But here, Y_{ij} is a categorical variable that takes the value of one if individual j in village i votes for the “experimental” candidate in the 2006 election and zero otherwise.

Table 4C indicates that the treatment has no effect on voting behavior, which is quite surprising given the results described in Table 4A and 4B. This is probably due to the fact that the post-election survey data was collected a week after the election and two days after the results were announced. Yayi Boni, the main experimental candidate, won the first round of the election by ten points, and it is likely that respondents in areas where he did less well might have exaggerated their electoral support for him after learning the results.

For instance, in the districts where we ran the experiment, Yayi’s vote share is 31% higher in the post-election survey than in the election-day vote count. Thus, if he were to do better in treatment areas than in control areas on election day, this margin would be much narrower after the results were announced. It is therefore safe to conclude that the results in Table 4C underestimated the effect of the treatment on voting behavior. However, we find that, as one would expect, voting is positively correlated to ethnic ties, but negatively correlated with education, which is puzzling. Finally, the treatment effect conditional on the respondent having access to the media is negative and significant at 90% level. Thus, the effect of the media on voting is marginally negative for those who were treated and strongly positive for those who were not, which means that treatment may help overcome inequalities in access to information.

Insert Table 4C

The question arising from the results described in section IV is whether the estimated treatment effect is driven by the information content of platforms or by the fact they were delivered through town hall meeting. This section draws from Atchade and Wantchekon (2009) and tries to estimate the pure town hall meeting effect.

Estimating the Town Hall Meeting Effect

The Town Hall meeting effect is difficult to pin down because there is no treatment group where *specific programmatic platforms* were delivered at rallies.²⁴ Instead, we have groups

²⁴If such group were to exist, we can derive this effect by simply comparing the outcomes under specific programme and town hall meetings to the outcome under specific programmes and rallies.

(i.e. the control groups) that were exposed to *clientelist platforms* at rallies. Because of this missing data problem, we make three fairly reasonable assumptions that help to assess the effect of town hall meetings.²⁵

The first assumption is that clientelist platforms presented in control villages were "specific." This assumption means that the only real difference between the platforms in treatment groups and those in control groups was that one was more programmatic and focused on the problems of the country and the other was more targeted and focused on the problems of the village. This is a standard assumption in the clientelist literature (see Keefer and Vlacu (2007)).

The second assumption is that, when delivered at rallies, specific clientelist platforms would gather at least as much support as specific programmatic platforms. The assumption is based on the results the 2001 experiments in Benin, which suggest that clientelist platforms do better than programmatic platforms under a wide range of conditions (See Wantchekon [2003]). The main lesson from that experiment was that clientelist platforms require much less explanation and clarification than programmatic platforms. Therefore clientelist platforms are more appropriate for rallies than any form of programmatic platforms.

The final assumption is that, despite citizens' participation, town hall meetings only marginally change the candidates' platforms. In other words, treatment villages always choose the specific programmatic platform proposed by the candidate. This assumption is based on the qualitative evidence from the proceedings of the meetings.²⁶

More formally, define Z the assignment indicator of villages to town meetings (process 1) or to campaign rallies (process 0). We define D a dichotomous variable with $D = 0$ if the town meetings did not substantially amend the policy proposal of the candidate and $D = 1$ if the villagers through the town meetings have substantially amended the candidate's proposal.

The electoral outcome $Y(0)$ is the (counterfactual) outcome of platform $D = 0$ in the control village and $(Y(1, 0), Y(1, 1))$ the outcome in the in treatment villages. where $Y(1, 0)$ is the outcome when there was town hall meeting, i.e. $Z = 1$ and the platform was not amended by participants to the town-meetings, i.e. $D = 0$. The outcome $Y(1, 1)$ occurs

²⁵This section draws from Atchade and Wantchekon (2009).

²⁶In fact, any evidence to the contrary would have been quite surprising (e.g. politicians altering significantly their platforms after town hall meeting with voters)

when there is town hall meeting, i.e. $Z = 1$ and the platforms were amended by voters, i.e. $D = 1$. The goal is to estimate $\tau = \mathbb{E}(Y(1, 0) - Y(0))$, the intrinsic town meeting effect.

As we mentioned earlier, the drawback of the experiment is that neither participation to town meetings nor the proceedings of the debates (which define D) were systematically recorded. But, as we mentioned earlier, this is not a problem here since town hall meetings do not significantly alter the candidates' platforms. Therefore, D is always 0.

Denote $Y^*(0)$ the outcome variable in control groups. Since the clientelist campaign message in a control groups is different from the treatment group message, we assume that $Y^*(0) \neq Y(0)$. In addition,

$$Y^*(0) \geq Y(0).$$

which captures the fact that, at rallies, specific clientelist platforms would work better than specific programmatic platforms. We have

$$\mathbb{E}(Y(1, 0) - Y(0)) = \mathbb{E}(Y(1, D) - Y(0)) \geq \mathbb{E}(Y(1, D) - Y^*(0)) = \tau^*$$

In the case where $Y(1, 0)$ is the voting outcome as discussed in section IV., $\tau^* = \mathbb{E}Y(1, D) - Y^*(0)$ is simply the estimated treatment effect presented in Table 2B, which indicates that the treatment has no effect on voting behavior. The value of τ^* is a lower bound on $\tau = \mathbb{E}(Y(1, 0) - Y(0))$, which is a measure the town hall effect. Thus, the effect is stronger than the "no effect" result in Table 2B. From this result, we conclude that, at the very least, Town Hall meetings are highly likely to have positive effect on electoral support for candidate running the experiment.

V. EXTERNAL VALIDITY

Randomized evaluation is strong on the quality of causal identification (internal validity), but weak on generalizability (external validity), i.e. whether the results are robust to changes in the background conditions of the experiment. According to Rodrik [2008], a reasonable step towards improving external validity is to make the target population more representative and the theoretical foundations of the experiment more explicit, as well as incorporating as much variation in the covariates as possible. We now explain the way in which the current experiment deals with these issues, in particular, representativeness of the target population and variation in the background conditions.

First, the districts involved in this experiment were drawn from all provinces, and as

a result, from all major ethno-linguistic groups of the country (Atakora-Donga, Borgou-Alibori, Zou-Collines, Oueme-Plateau, Atlantic-Littoral and Oume-Plateau). In the 2001 experiment, neither Oueme Plateau nor Atakora-Donga were represented. Thus, the current population under treatment is more representative in terms of social and demographic conditions.

Second, while in the 2001 experiment only strongholds were selected, in this study we included two districts (Come and Tanguieta) that was expected to be competitive. Remarkably, the experimental candidate in these districts complied with the experimental protocol despite the obvious risk associated with running an experimental campaign in a competitive political environment. It is also remarkable that the risk seemed to have paid off: in Come for instance, the candidate had a much larger voter share (+14%) in treated village than in the non-treated villages.

Third, while in the 2001 experiment all candidates were expected to win at least 60% of the vote locally, the current experiment includes three districts out of twelve where the experimental candidate, namely Soglo, was the underdog. We find that the average treatment effect was positive not only with the candidates favored to win, but also with the underdog.

Because the current project covers more regions than the previous experiment, there was more variation in terms of background conditions. There are districts with large Christian populations (e.g. Dangbo) and others with large Muslim populations (e.g. Kandi). There are urban districts (e.g. Kandi and Abomey Calavi) and rural districts (e.g. Ouesse and Kouande). There are districts with strong media coverage (e.g. Bembereke) and others with weak media coverage (e.g. Ouesse). Some experimental districts have stronger ties with neighboring countries such as Nigeria and Togo (e.g. Dangbo and Come) and others have virtually no ties with neighboring countries and are insulated (e.g. Ouesse and Bembereke).²⁷

Finally, according to Ravaillon [2008], threats to external validity also arise when policy experiments are designed and implemented by outsiders such as non-governmental organizations (NGOs). He wrote:

the very nature of the intervention may change when it is implemented by a government rather than an NGO. This may happen because of unavoidable differences in (inter alia) the quality of supervision, the incentives facing service

²⁷The national scope of the experiment and the fact that it involves all major parties and candidates limits, but does not eliminate, concerns of partial equilibrium effects.

providers, and administrative capacity. (p. 17)

Like government in policy experiments, parties and candidates are the relevant actors in political experiments. Therefore, threats to external validity are limited in our experiment by the fact that it involved real candidates competing in real elections. The very fact that candidates agree to run such an experiment is an indication that the treatment is fairly realistic, and responds to electoral incentives. If, besides being realistic, the treatment is proven to be electorally effective, it will be more much more likely to be adopted by politicians in future elections.

VI. IMPLICATIONS FOR INSTITUTIONAL REFORM

We show that a two-stage public deliberation over policies led by experts can improve electoral support for those policies. The candidates could win more votes from the electorate if they were to switch from the standard campaign message to platforms informed by research and best practice and chose to communicate these platforms through town meetings. If a given policy is known by voters and politicians to be both welfare improving and electorally effective, then that policy is more likely to be adopted by politicians as electoral platforms in future elections, and more likely to be implemented once the politician is in office. Therefore, one should take advantage of advances in liberal democracy and the improved political autonomy of civil society in Africa, to push for institutional change that allows for more public deliberation over policy choice and better use of expert policy information generated by academics and development agencies. This is because, as we show in this paper, town meetings informed by serious policy research can help improve civic engagement and electoral support for good governance, and hence make the selection and implementation of “good” policies more likely. One may achieve this goal by institutionalizing the generation and use of expert policy information by parties, governments, and civil society organizations.

It might therefore be helpful to set up in Benin and perhaps other African countries, a “council of experts”, a permanent and independent academic institution to advise and assist political parties and the government in designing and evaluating policies, and in setting development priorities. This institution could bear some similarities to the Intergovernmental Panel on Climate Change (IPCC), but with a different focus. It could also bear some

resemblance to the Brookings Institution and the National Bureau of Economic Research (NBER) in the US, albeit with a broader mandate.²⁸

Institutionalizing the use of expert information is at the heart of Chinese economic reforms. According to Ravallion (2008: 2), the guiding principle of these reforms was the idea that public action should be based on evaluations of experiences with different policies. As a result, in 1978 the Chinese Communist Party set up a research group that studied local experiments on the decollectivization of farming. The results of these local policy experiments helped convince national policy makers of the merits of scaling up these policies.

The idea of a council of experts as an institutional response to policy failure in democracies was also raised in December 2004 by Andy Rooney, a TV commentator from the popular CBS news magazine *60 minutes*. Responding to popular criticisms of the Bush Administration’s policy in Iraq, Mr Rooney suggested the creation of a “Smart Board”, where college professors would advise and speak out publicly on major policy decisions. According to Mr. Rooney, members of the “Smart Board” would be elected for a two-year term by all college professors. Board members would discuss key policy initiatives, and give their best advice to Congress and to the President. There would be no compulsion for Congress and the President to accept this policy advice, but the board’s opinions would be made public, thereby putting pressure on politicians.

This rather wild proposal, which was treated as a joke by television audiences across the country, has the merit of pointing quite seriously to the role of “expert” knowledge in democratic politics and the benefits of institutionalizing non-partisan policy expertise.

VII. CONCLUDING REMARKS

A field experiment was conducted in Benin to investigate the effect of an expert-led deliberative campaign on political behavior. We find that the campaign or the treatment has a positive effect on a self-reported measure voter information, turnout, and voting for the candidate running the experiment. We use the result to provide an empirical justification for the creation of “councils of experts” that would systematically evaluate policy initiatives, advise local and national governments, political parties and civil society organizations, and lead public discussions around election times, or other critical junctures of national

²⁸Obviously, keeping this institution non-partisan and immune from political pressure might be difficult. But the study of the conditions for this non partisanship is beyond the scope of this paper.

policy-making. We argue that by engaging voters and political actors, councils of experts would not only help create an electoral constituency for good governance, but also improve transparency and accountability in governments.

The results lend some support to our earlier claim that clientelism is driven by political conditions, namely the transparency of programmatic platforms and by town-meetings. The result might have been different if voters or clients were economically dependant on local patrons, as in agrarian societies with powerful landed elites such as in Latin American countries. In that case, the clientelist equilibrium may have been more robust and the effect of the information treatment less effective.

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 elections. We also plan to further explore theoretically the role of expert information, and of other institutional arrangements, in reducing clientelism and improving efficiency in the provision of public goods.

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Table 1A: Summary Statistics

Variable	Oberv.	Mean	Std. Dev.	Control Mean	Experimental Mean
Age	2132	41.91	14.68	41.69	42.11
Gender Male=1	2153	.50	.50	.50	.49
Went to school=1	2153	.34	.47	.47	.48
Education level	2153	.45	.70	.42	.49**
Radio=1	2153	.87	.34	.87	.87
Television=1	2153	.14	.35	.13	.15
Newspaper=1	2153	.03	.17	.02	.04*
Ethnic ties with candidate	2153	.59	.49	.57	.61

Note: *significant at 10%; **significant at 5%; ***significant at 1 %

Table 1B: Internal Validity and Compliance

Variable	Control group	Treatment group
T-shirt	.020	.021
Calendar	.072	.076
Cash	.207	.165
Informative campaign?	.588	.641
Inf. about candidates	.461	.528
Inf. about issues	.326	.379
Inf. about government	.193	.202

Table 2A: Information - Candidates

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	.169*** (.055)	.169** (.066)	.167*** (.056)	.167** (.072)	.156*** (.058)	.156** (.061)
Education			.314*** (.59)	.314*** (.075)	.198*** (.064)	.198*** (.076)
Gender (male=1)					.351*** (.061)	.351*** (.055)
Age					-.001 (.002)	-.001 (.002)
Ethnic Ties					.487*** (.086)	.487* (.288)
Media			-.281*** (.054)	-.281* (.166)	-.245*** (.061)	-.245 (.168)
Discussion					-.211*** (.037)	-.211** (.091)
Observations	2073	2073	2073	2073	2052	2052
Pseudo R ²	.015	.015	.034	.034	.079	.079
Clustered Standard Errors	No	Yes	No	Yes	No	Yes

Note: The estimation method is probit. Standard errors in parentheses. Clustering is at the Commune level. All models include candidate fixed effects. *significant at 10%;

significant at 5%; *significant at 1%

Table 2B: Information - Problems Facing Country

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	.153*** (.058)	.153* (.091)	.143** (.058)	.143 (.094)	.177*** (.060)	.177* (.104)
Education			.426*** (.061)	.426*** (.064)	.339*** (.065)	.339*** (.071)
Gender (male=1)					.236*** (.063)	.236*** (.063)
Age					.002 (.002)	.002 (.003)
Ethnic Ties					-.016 (.092)	-.016 (.193)
Media			-.151*** (.057)	-.151 (.153)	-.014 (.064)	-.014 (.116)
Discussion					-.288*** (.039)	-.288** (.121)
Observations	2073	2073	2073	2073	2052	2052
Pseudo R ²	.046	.046	.066	.066	.099	.099
Clustered Standard Errors	No	Yes	No	Yes	No	Yes

Note: The estimation method is probit. Standard errors in parentheses. Clustering is at

the Commune level. All models include candidate fixed effects. *significant at 10%;

significant at 5%; *significant at 1%

Table 2C: Information - Government & Functions

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	.049 (.065)	.049 (.125)	.031 (.066)	.031 (.134)	.009 (.069)	.009 (.141)
Education			.516*** (.067)	.516*** (.118)	.408*** (.073)	.408*** (.119)
Gender (male=1)					.394*** (.073)	.394*** (.089)
Age					-.001 (.002)	-.001 (.003)
Ethnic Ties					.389*** (.103)	.389 (.239)
Media			.146** (.064)	.146 (.170)	.167** (.072)	.167 (.154)
Discussion					-.122*** (.044)	-.122* (.062)
Observations	2073	2073	2073	2073	2052	2052
Pseudo R ²	.075	.075	.106	.106	.134	.134
Clustered Standard Errors	No	Yes	No	Yes	No	Yes

Note: The estimation method is probit. Standard errors in parentheses. Clustering is at the Commune level. All models include candidate fixed effects. *significant at 10%;

significant at 5%; *significant at 1%

Table 3: Voter Turnout

Commune	Village	Party	Status	Share	Total
Kandi	Thya	UDS	T	83.0	601
			C	73.6	55,895
Bembereke	Mani	UDS	T	83.3	193
			C	78.1	41,958
Ouesse	Yaoui	CAP	T	80.1	2300
			C	68.2	56,537
Save	Okounfo	CAP	T	88.6	1,118
			C	74.5	44,290
Come	Gadome I	IPD	T	70.3	1,515
			C	77.8	33,819
Toffo	Dame	PSD	T	78.6	2926
			C	61.1	32959
Dangbo	Mitro	PRD	T	86.80	134
			C	86.22	40875
Kouande	Orou-Kayo	IPD	T	78.8	1007
			C	71.9	34046

Note: T means Treatment and C means Control

Table 4A: Vote Shares of Experimental Candidates (official results)

Commune	Village	Party	Status	Vote shares.	Vote Total
Kandi	Thya	UDS	T	71.5	601
			C	72.8	29,524
Bembereke	Mani	UDS	T	64.3	193
			C	73.3	24,007
Ouesse	Yaoui	CAP	T	80.4	1,495
			C	62.7	24,186
Save	Okounfo	CAP	T	72.0	713
			C	61.6	20,314
Come	Gadome I	IPD	T	54.3	578
			C	32.3	8,500
Dangbo	Mitro	PRD	T	59.4	413
			C	54.1	2509
Kouande	Orou-Kayo	IPD	T	60.7	482
			C	68.3	17160
Tanguieta	Taicou	IPD	T	25.98	1216
			C	22.42	1320

Table 4B: Candidates' Vote Shares in Missing Districts (estimates from samples)

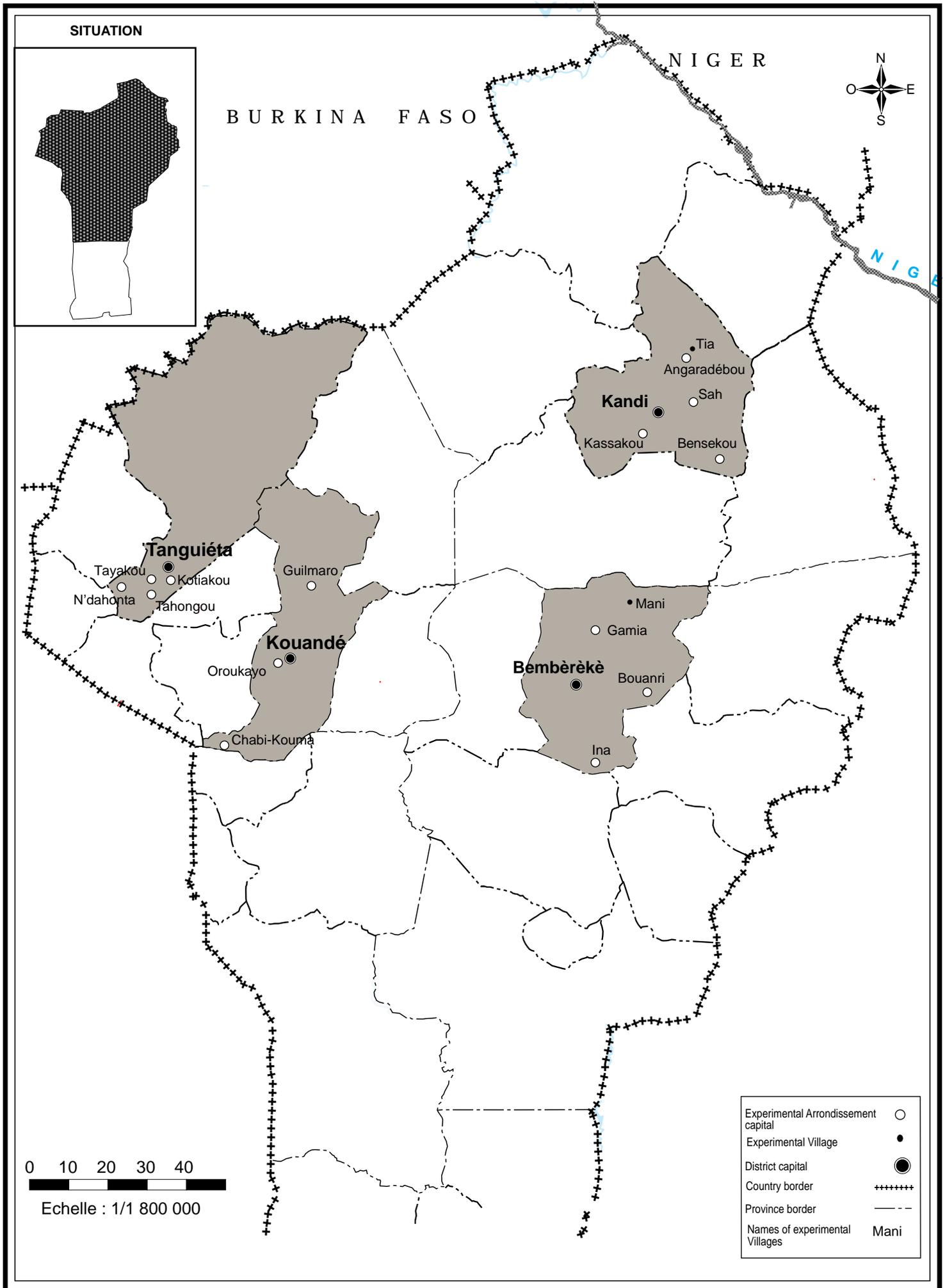
Commune	Village	Party	Status	Vote shares	Total
Zagnanado	Don-Tan	RB	T	8.8	7 (80)
			C	35.4	39 (82)
Abomey-Calavi	Tokan	RB	T	10.1	8 (79)
			C	1	1 (80)
So Ava	Lokpodji	RB	T	35.	28 (79)
			C	0	0 (80)

Table 4C: Vote for Experimental Candidate

	(1)	(2)	(3)	(4)
Treatment	-.025	-.019	-.050	-.181
	(.286)	(.284)	(.278)	(.205)
Education		-.247**	-.227**	-.253
		(.119)	(.107)	(.159)
Media		.059	.011	.316
		(.218)	(.198)	(.255)
Gender (male=1)			-.095	-.059
			(.061)	(.107)
Ethnic Ties			.742***	.639**
			(.277)	(.327)
Treatment* Media				-.578*
				(.351)
Treatment*Gender				-.081
				(.137)
Treatment*Ethnic Ties				.234
				(.476)
Treatment*Education				.043
				(.164)
Observations	2058	2058	2058	2058
Pseudo R ²	.374	.379	.391	.399

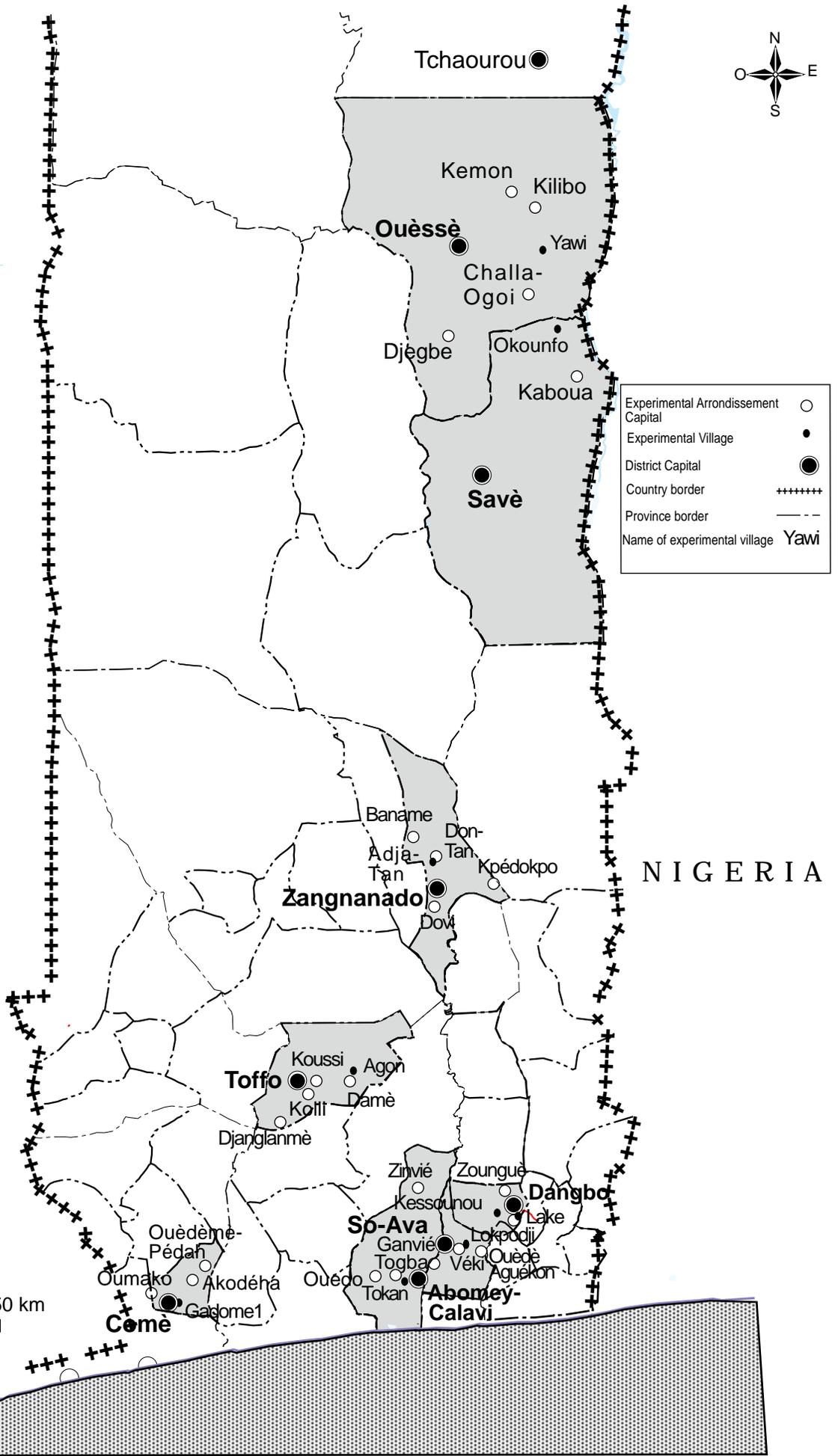
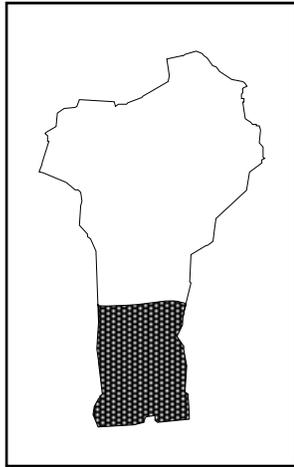
Note: The estimation method is probit. Standard errors in parentheses, clustered at the Commune level. All models include candidate fixed effects. *significant at 10%; **significant at 5%; ***significant at 1%

MAP OF EXPERIMENTAL DISTRICTS AND VILLAGES



MAP OF EXPERIMENTAL DISTRICTS AND VILLAGES

SITUATION



Experimental Arrondissement Capital	○
Experimental Village	●
District Capital	●
Country border	+++++
Province border	- - -
Name of experimental village	Yawi

TOGO

NIGERIA

0 10 20 30 40 50 km

Echelle : 1/1 200 000