

EXPERT INFORMATION, PUBLIC DELIBERATION, AND ELECTORAL SUPPORT FOR GOOD GOVERNANCE: EXPERIMENTAL EVIDENCE FROM BENIN

LEONARD WANTCHEKON[‡]

September 13 , 2008

Abstract

This paper provides experimental evidence on the effect of "informed" public deliberation 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 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 and targeted or clientelist electoral promises. We find that the treatment has a positive effect on voter information about policies and candidates. We also find that turnout and electoral support for the candidates participating in the experiment were higher in treatment villages than in control villages. We argue that political parties can overcome the need for distributing favors in order to win votes, by improving the extent to which their policy promises are informed by empirical policy research.

*Professor of Politics and Economics, New York University.

[†]I am grateful to seminar and conference participants at Oxford University, London School of Economics, the University of British Columbia, Yale University, as well as the American Political Science Association and African Studies Association annual meetings for comments. I am also grateful to Thomas Bierschenk, Torun Dewan, Alan Gerber, James Hollyer, Macartan Humphreys, Alessandro Lizzeri, Gerard I Padro Miquel, Jas Sekhon, and Pedro Vicente for their thoughtful comments and suggestions. Special thanks to Gregoire Kpepede and Alexandre Biaou for implementing the project for the IERPE (Benin), and to Moussa Blimpo, Fernando Garcia, particularly Robin Harding, and Sarah Weltman for providing superb research assistance. The theory section draws from an ongoing joint project with Alejandro Corvalan. The Konrad Adenauer Foundation provided financial support for part of the project. I am solely responsible for any remaining errors.

INTRODUCTION

Broad public goods play a crucial role in promoting economic growth and reducing social inequalities.¹ For instance, universal health care policies significantly reduce the gap in child mortality rates between poor and rich households, and a well-functioning public education system is key to narrowing the gap in reading skills between high and low ability students.² Following Lizzeri and Persico [2004], one might expect such policies to emerge in low income countries as they become more democratic. Yet, despite democratic change in Africa, there is widespread government failure in generating broad and universal public policies (Kitschelt and Wilkinson [2007] and Van de Walle [2007]). The reason is simple: even in well-functioning democracies, policies are not selected on the basis of their contribution to growth and development by a social planner or a benevolent philosopher-king. Instead, they are selected by politicians on the basis of their electoral appeals (Dixit and Londregan [1996] and Lizzeri and Persico [2001], among others). Furthermore, when voters are divided along ethnic lines or are uninformed, inefficient and clientelistic policies are more likely to be preferred to “programmatic”, well-conceived and broad policies (Wantchekon [2003], Easterly and Levine [1997]). According to Keefer and Vlacu [2008], this might be explained by the fact that accountability mechanisms work better with targeted, clientelist promises than programmatic, broad based public goods promises.³

A natural question arising from these findings is whether programmatic platforms would ever be electorally effective, and what institutional innovation can help mitigate clientelist practices. One possible solution that has recently been discussed in the literature is *access to information*. Wantchekon and Vermeersh [2007] find evidence that respondents who have more access to media outlets (radio and newspapers) have a less negative response to public goods platforms. This evidence is indicative of the role of information in improving the

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 a cross-country comparison in Early Grade Reading Assessment (EGRA) conducted by RTI international.

³See also Keefer [2007]. Keefer and Vlacu [2008] present a model in which politicians in new democracies who have a credibility deficit can reduce this deficit either through repeated interactions or through targeted transfers. They show that in equilibrium, politicians will prefer targeted transfers, which leads to high levels of corruption and low levels of public goods provision. An implicit assumption is that politicians' lack of credibility makes voters prefer targeted transfers to public goods.

electoral appeal of non-clientelist platforms. However, it is unclear whether the information effect is causal, since access to media was not randomly assigned in the 2001 Benin experiment. Perhaps those with better media access were more informed about the candidates and their platforms, and about government in general. They may also have been more educated and wealthy. The goal of this paper is to isolate the effect of information regarding programmatic public goods platforms on voting.

There have been several studies in contemporary political science that provide empirical evidence of the effect of increased policy knowledge on voting. 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.⁴

Another important contribution to the debate about the political effect of information is the deliberation literature. Gutman and Thompson [1996] and Fishkin [1997] 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. As a result, it is unclear how policy information provided in the context of public deliberation would affect voting behavior in real elections.

This paper also contributes to the literature on media access, 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.

⁴See also Bartels [1996] and Delli Carpini Michael and Scott Keeter [1996].

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 more 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 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, henceforth the experimental candidate, 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. As with the 2001 experiment, the follow-up took place in Benin and involves real candidates running in real elections (the first round of the 2006 presidential race). 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 meetings during the electoral campaign. The meetings were led by party activists, inspired and 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. an expert led public deliberation process. 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.⁵

We find that treatment has a positive effect on voters' self-reported level of information about policies and candidates. In addition, the actual election results suggest that, overall, the treatment has a positive and significant effect on both turnout and voting. We conclude that political parties can overcome the need to win votes through targeting or even distributing 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, and one may contain electoral clientelism by institutionalizing the use of policy expertise in the design of electoral platforms.

I. A SIMPLE THEORETICAL FRAMEWORK

Why the treatment would generate electoral support for programmatic platforms?. This is because expert deliberation generates specific platforms and public deliberation facilitate voter coordination. When platforms are specific, they enable parties to make promises on public goods and transfers that are credible. 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. It 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.

In this section use a basic extension of the standard model of redistributive politics to formalize the following argument: If the treatment makes electoral promises to various groups more transparent, then it will have a positive effect on electoral support for the candidate participating in the experiment.⁶

We consider a redistributive model with two parties, a Western Party W and an Eastern Party E , and N groups of voters. We assume that parties only care about winning and that voters have preferences over consumption (i.e. transfers and public goods), and ideology or

⁵See Banegas (1998) for a vivid description of institutions governing clientelist electoral campaigns in Benin.

⁶The standard model of redistributive politics was first developed by Lindbeck and Weibull [1987], and later by Dixit and Londregan [1987] as well as Lizzeri and Persico [2004]

identity. Each of the N groups in the political community has the same mass of individuals $\frac{1}{N}$. We also assume, as in Lizzeri and Persico [2004], that each citizen is endowed with ω_i units of consumption goods, so that the aggregate resources in the economy is given by $\omega = \sum \frac{1}{N}\omega_i$. Government can tax individual endowment to its maximum and taxation is non-distortionary, so that only the aggregate level of resources in the country matters, not its distribution.

Each voter has ethnic affiliations and is assumed to care about the fact that a member of his or her ethnic group or someone relatively close to his or her ethnic group is elected.⁷ We define by x a voter's level of ethnic attachment, where x is the realization of a random variable X with a cumulative distribution function F and density f . The parties W and E compete for election, making a binding promise of a transfer y_i and level of public good G to each group. That means the promises are not tailored to the level of ethnic attachment of the voter (which is not observed) but to his or her group.

A voter x votes for E if

$$U(y_i^E, G^E) - U(y_i^W, G^W) > x$$

Thus, the probability that a group i voter votes for party E , given the menu of transfers and the level of public goods is:

$$F(U(y_i^E, G^E) - U(y_i^W, G^W)).$$

The total vote share of the Eastern party is given by

$$S_L = \frac{1}{N} \sum_{i=1}^N F(U(y_i^E, G^E) - U(y_i^W, G^W))$$

The total vote share of the Western Party is $(1 - S_L)$.

Given W 's platform, E would chose its own platform to maximize its vote share, subject to the non-negativity constraints: i.e.

$$G^E \geq 0; \text{ and } y_i^E \geq 0$$

⁷The voter might also dislike the fact that a candidate from a specific ethnolinguistic origin is elected.

as well as the aggregate budget constraint

$$\omega \geq G^E + \sum y_i^E$$

We assume that voters perfectly observe the promises made to members of their respective group, but have imperfect knowledge about transfers made to members of other groups. In other words, *there is no transparency* over the level of aggregate transfers and therefore over the level of public goods.⁸ We want to show that provision of public goods is more likely, when politicians are more transparent about aggregate transfers. We first analyze the no-transparency case and then move to the full-transparency case.

- **The no-transparency case**

Since there is imperfect information about the level of aggregate transfers, voting decisions will be based on beliefs about the parties' offers to other groups and hence the level of public goods they will provide once in office. Given these beliefs, parties will choose the optimal provision of public goods in order to maximize their vote shares. Assume the offer to group i by E is y_i^E . The only consistent belief for voters is that E will offer no public goods once in office. Given this belief, parties will solve for transfers allocations that maximize transfers under the non-negativity and resource constraints, leading to $G^E = G^W = 0$

- **The full-transparency case.**

In this case the parties can credibly announce a level of public goods. They are also fully transparent about the level of transfers to other groups. In contrast with the no-transparency case, voters believe that public goods will be provided with probability 1, and parties will maximize their vote shares under the non-negativity and resource constraints. Thus, there will be a positive amount of public goods and consequently less transfers for each group than in the no-transparency case.

In summary, if the treatment increases voter information over policies and the credibility of public goods platforms, then voters would be more responsive to public goods platforms. Our experimental evidence suggests that this responsiveness, by and large, translates into

⁸This is a special case of a more general model of transparency in policy-making developed by Gavazza and Lizzeri [2008].

higher turnout and more votes for the candidate in treatment villages than in control villages. But before we provide details of the experimental design and the results, we introduce its context.

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.⁹ 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 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

⁹Presidents 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.

(Nicephore Soglo of the Renaissance du Benin and to a lesser degree Adrien Houngbedji of the Parti pour le Renouveau Democratique).¹⁰

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.¹¹ 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.¹² 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 (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

¹⁰If 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.

¹¹See Wantchekon and Ngomo [2001].

¹²To 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.

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.¹³

III. THE EXPERIMENT

The experiment investigates the effect of expert information and specific policy proposals on political outcomes. The treatment to be evaluated is a two-stage public deliberation process designed to generate a programmatic, non-targeted electoral platform. The first stage involves political parties and is led by policy experts. The second stage draws on the outcome of the first stage and involves voters and is led by party activists. Thus, the treatment is *not* a pre-designed, pre-crafted platform or a vignette that would be read to voters. The treatment is a process, game form, or more precisely, a *mechanism* for generating political platforms or campaign messages. To put it differently, we are investigating voters' responses to a randomly assigned mechanism for generating electoral platforms (expert-led public deliberation), not voters' responses to a randomly assigned platform.¹⁴

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.¹⁵ 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

¹³See Gisselquist [2006]) for a detailed report and analysis of the election. See also Banegas [1998] for a study on clientelism in Benin.

¹⁴Another example of a randomized evaluation of a political mechanism is Benjamin Olken's (2008) study of the comparative effect of direct elections as opposed to representative-based meetings on popular satisfaction and support for local public goods.

¹⁵The 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.

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), Renaissance 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 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 assign 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 minutes 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.¹⁶

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 a short meeting during which surrogates of the candidate make predominantly targeted electoral promises.¹⁷

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).¹⁸

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

¹⁶The 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.”

¹⁷For details on the nature of electoral campaigns in Benin, see Banegas [2003].

¹⁸We 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.

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-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 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). 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%).¹⁹

Finally, a significantly larger number of respondents in the treatment group described the 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

VOTER INFORMATION

We first evaluate the effect of the treatment on voter information. 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

¹⁹Given the pledge by the experimental candidates to restrain from clientelist practices in treatment villages, there was lower supply of cash in treatment villages.

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.

POLICIES AND CANDIDATES.—

Tables 2A and 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

GOVERNMENT.—

We now turn to the variable measuring how effective the campaign was in informing voters about government and its functions. Since the campaign provides virtually no information about government, this variable measure voters' prior knowledge about political institutions in the country. Table 2C suggests that the treatment did not have any effect in any of the specifications. This is very much expected since the goal of the experiment was to inform voters about the candidates' programmes, not about the internal structure of the government. 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

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 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

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.²⁰ As we mentioned earlier, the data on electoral behavior originated from a survey that took place five days after the election.

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.

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

²⁰Due 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.

3,917,865. This means that there would have been 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 to turnout to vote than a voter who received cash but was less informed (68% to 15.3%). 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.²¹ 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 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

²¹According to government reports, public spending on primary education was about about 31 millions US dollars in 2007.

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 Tangieta) 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: in Come for instance, the candidate had a much larger voter share (+14%) in that 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).²²

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 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

²²The 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.

information generated by academics and development agencies. This is because, as we show in this paper, public deliberation 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.

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 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 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.

It is striking that the response from voters was as positive in the 2001 experiment, when the treatment was clientelist, as it was in the 2006 experiment, when the treatment was an expert-led deliberative campaign. This lends support to our theoretical argument which suggests that clientelism is driven by political conditions, namely the transparency of programmatic platforms. 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.

REFERENCES

- Alesina Alberto, Reza Baqir and William Easterly. 1999. "Public Goods and Ethnic Divisions", *Quarterly Journal of Economics*, CXIV, 1243-1284.
- Alesina, Alberto and Dani Rodrik. 1994. "Distributive Politics and Economic Growth", *Quarterly Journal of Economics*, 109, 465-490.
- Banegas, Richard. 1998. "Bouffer l'Argent, Politique du Ventre, Democratie et Clientelisme au Benin" in Jean-Louis Briquet et Frederic Sawicki (eds) *Clientelisme Politique dans les Societes Contemporaines*, Presses Universitaires de France.
- Banegas, Richard. 2003. *La Democratie a Pas de Cameleon: Transitions Imaginaires Politiques au Benin*. Karthala
- Bartels, Larry. 1996. "Uninformed Votes: Information Effects in Presidential Elections." *American Journal of Political Science* 40:1 194-230.
- Boulaga, Eboussi. 1993. *Les Conferences en Afrique Noire*. Paris: Editions Karthala.
- Brusco Valeria, Marcelo Nazareno, and Susan Stokes. 2004. "Vote Buying in Argentina," *Latin American Research Review*, 39(2):66-88, June 2004
- López-Casasnovas Guillem, Berta Rivera and Luis Currais (eds). 2005. *Health and Economic Growth: Findings and Policy Implications* MIT Press.
- Degboe Kuassi. 1995. *Elections et Realites Sociologiques du Benin*. Cotonou: Intermonde Editions.
- Delli Carpini Michael X. and Scott Keeter. 1996. *What Americans Know about Politics and Why it Matters*. *What Americans Know about Politics and Why it Matters*. New Haven, CT: Yale University Press.
- Duflo, Esther, Rachel Glennerster, and Michael Kremer. 2006. "Using Randomization in Development Economics Research: A Toolkit," Working Paper, MIT.
- Dixit Avinash and John Londregan. 1996. "The Determinants of Success of Special Interest in Redistributive Politics." *Journal of Politics*, Vol. 58, pp. 1132-1155.

- Easterly William. 2001. *The Elusive Quest for Growth: the Economists and Misadventures in the Tropics*. Cambridge: MIT Press.
- Easterly William and Ross Levine. 1997. Africa Growth Tragedy: Policies and Ethnic Divisions. *Quarterly Journal of Economics*, 112, 1203-1250.
- Fishkin James. 1997. *The Voice of the People: Public Opinion and Democracy*. New Haven: Yale University Press New Haven.
- Gavazza Alessandro and Alessandro Lizzeri. 2008. "Transparency and Economic Policy," Working Paper, New York University.
- Gerber Alan and Donald P. Green and David Nickerson. 2003. Getting out the Vote in Local Elections: Results from Six Canvassing Experiments. *The Journal of Politics*, Vol. 65, No. 4, Pp. 1083–1096
- Gerber Alan and Donald P. Green. 2000. The Effects of Canvassing, Phone Calls, and Direct Mail on Voter Turnout: A Field Experiment. *The American Political Science Review*, Vol. 94, No. 3, pp. 653-663
- Gilens, Martin. 2001. "Political Ignorance and Collective Policy Preferences." *American Political Science Review* 95(2):379-396.
- Gisselquist, Rachel. 2006 "Benin's 2006 Presidential Elections," Working Paper. MIT.
- Gutman Amy and Dennis Frank Thompson. 1996. *Democracy and Disagreement: why moral conflict cannot be avoided in politics*. Cambridge, MA: Harvard University Press.
- Keefer, Philip and Razvan Vlaicu. 2007. Democracy, Credibility, and Clientelism. *Journal of Law, Economics, and Organization*. Forthcoming.
- Philip Keefer and Stuti Khemani. 2005. Democracy, Public Expenditures, and the Poor: Understanding Political Incentives for Providing Public Services. *World Bank Research Observer* Vol. 20: 1-27;
- Kitschelt Herbert and Steven Wilkinson (eds). 2007. *Patrons or Policies? Patterns of Democratic Accountability and Political Competition*, New York: Cambridge University Press

- Habermas, Jürgen. 1996. *Between Facts and Norms: Contributions to a Discourse Theory of Law and Democracy*. Cambridge, MA: MIT Press.
- Hellbrunn, John R. 1993. "Social Origins of National Conferences in Benin and Togo". *Journal of Modern African Studies* v 31: 277-99.
- Howitt, Peter. 2005. Health, Human Capital and Economic Growth: A Shumpeterian Perspective. *In Health and Economic Growth: Findings and Policy Implications*, edited by Guillem Lopez-Casasnovas, Berta Rivera and Luis Currais. Cambridge, MA: MIT Press, 2005, 19-40.
- Lindbeck, Assar and Jurgen Weibull. 1987. "Balanced-Budget Redistribution as the Outcome of Political Competition," *Public Choice*, Vol. 52, pp. 273-297.
- Lizzeri, Alessandro and Nicola Persico. 2004. Why Did the Elites Extend the Suffrage? Democracy and the Scope of Government, With an Application to Britain's "Age of Reform". *Quarterly Journal of Economics*. Vol. 119, No. 2, pp. 707-765.
- Lizzeri, Alessandro and Nicola Persico. 2001. The Provision of Public Goods under Alternative Electoral Incentives. *The American Economic Review*, Vol. 91, No.1 pp. 225-239.
- Lupia, Arthur. 2002. "Deliberation Disconnected: What it Takes to Improve Civic Competence." *Law and Contemporary Problems* 65: 133-150.
- Lupia, Arthur. 2008. Beyond Facts and Norms: Contributions of Social Science to Deliberative Legitimacy. Working Paper. University of Michigan.
- Luskin, R.C. Fishkin, J. and Jowell, R, 2002. "Considered opinions: Deliberative polling in Britain." *British Journal of Political Science* 32: 455-487.
- Nwajiaku, Kathryn. 1994. "The National Conferences in Benin and Togo Revisited." *Journal of Modern African Studies* v 32: 429-47.
- Nielsen, François. 1985. "Toward a Theory of Ethnic Solidarity in Modern Societies", *American Sociological Review*, 50, 133-149.
- Olken, Benjamin. 2008. "Political Institutions and Local Public Goods: Evidence from a Field Experiment in Indonesia. Harvard University Working Paper.

- Ravallion, Martin. 2008. "Evaluation in the Practice of Development," Policy Research Working Paper 4547, World Bank.
- Reinnika, Ritva and Jakob Svensson. 2005. Fighting Corruption to Improve Schooling: Evidence from a Newspaper Campaign in Uganda. *The Journal of the European Economic Association*. Vol. 3, No. 2-3, Pages 259-267.
- Rodrik. Dani. 2008. The New Development Economics. We Shall Experiment. Shall We Learn? Working Paper, Kennedy School of Government.
- Rooney, Andy. 2004. Let us Have a Smart Board:
<http://www.cbsnews.com/stories/2004/05/11/60minutes/rooney/main616858.shtml>
- Saint Paul Gilles and Thierry Verdier. 1993. - "Education, Democracy and Growth" *Journal of Development Economics*, 42, 399-407.
- Sala- I- Martin Xavier. 2002. "Poor People are Unhealthy People...and Viceversa", Proceedings of the International Meeting of Health Economics, Paris 2002.
- Van de Walle, Nicolas. 2007 "Meet the New Boss, Same as the Old Boss? The Evolution of Political Clientelism in Africa." In Herbert Kitschelt and Steven Wilkinson, eds., *Patrons, Clients, and Policies: Patterns of Democratic Accountability and Political Competition*, pp. 112-149. Cambridge University Press.
- Wantchekon, Leonard. 2003. Clientelism and Voting Behavior: Evidence from a Field Experiment in Benin. *World Politics*, Vol. 55, No. 3, 399-422.
- Wantchekon, Leonard and Paul Ngomo. 2001. Democratic Consolidation in Benin Lessons from the 1996 Presidential Election. Nordic Africa Institute Publications, Issue No 2.
- Wantchekon, Leonard and Christel Vermeersch. 2007. "Information, Social Networks and the Demand for Public Goods: Experimental Evidence from Benin". NYU Working paper.
- World Bank. 2008. Benin Country Memorandum. Draft. June 2008.

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)

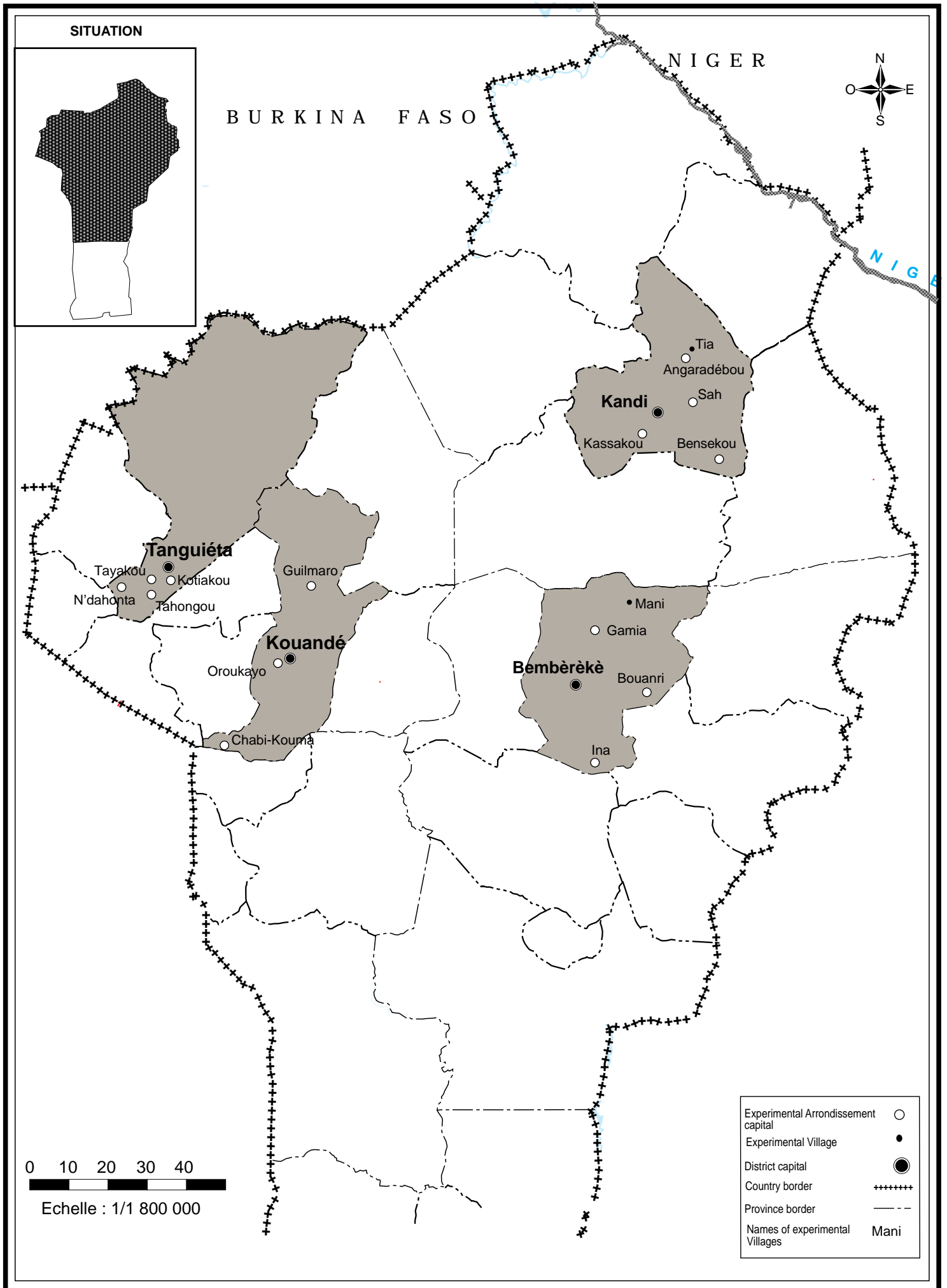
Commune	Village	Party	Status	Vote shares	Total
Zagnanado	Don-Tan	RB	T	8.8	7 (80)
			C	35.4	39 (82)
Abomey-Calavi	Tokan	RB	T	10.1	8 (79)
			C	1	1 (80)
So Ava	Lokpodji	RB	T	35.	28 (79)
			C	0	0 (80)

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

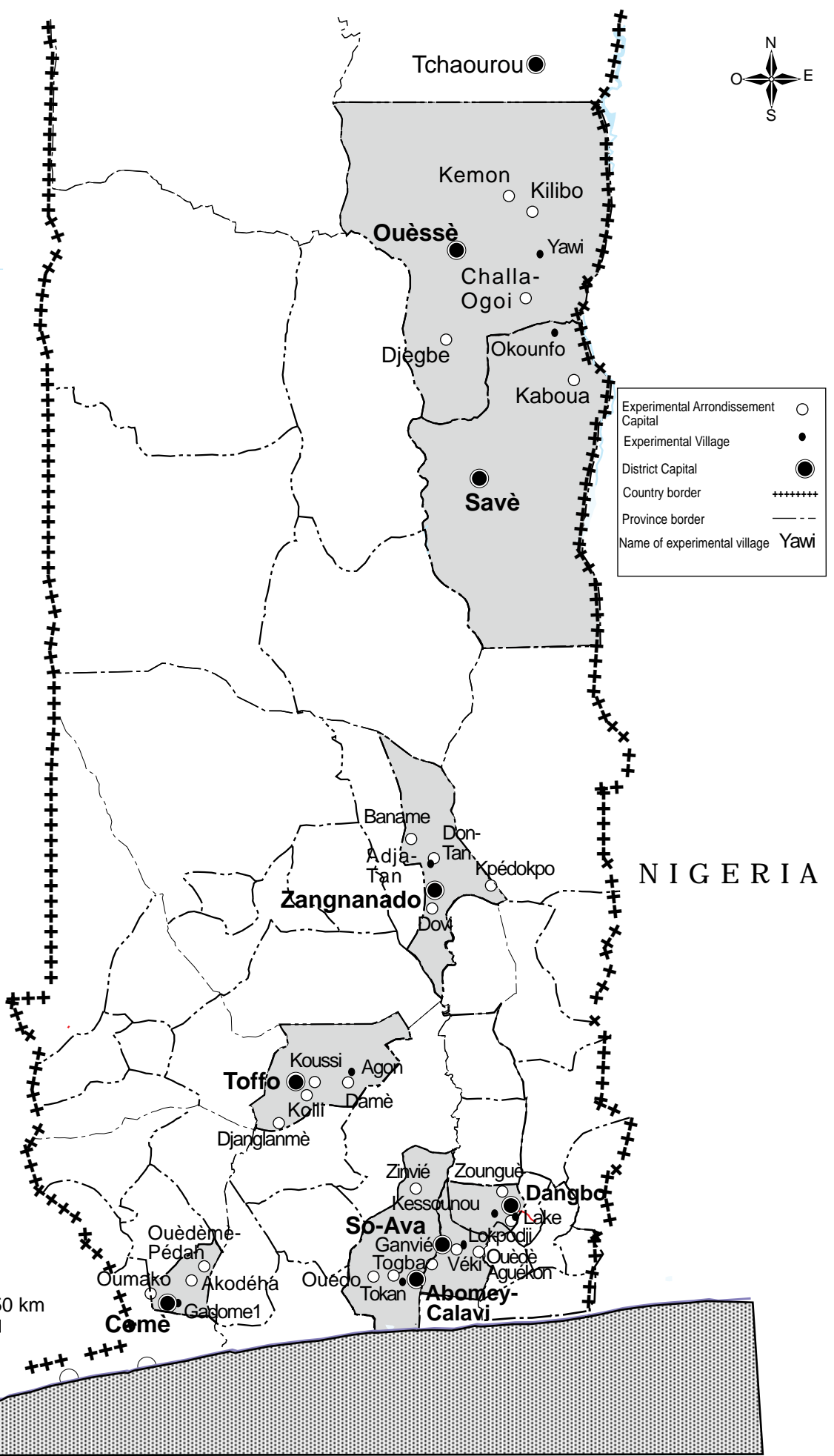
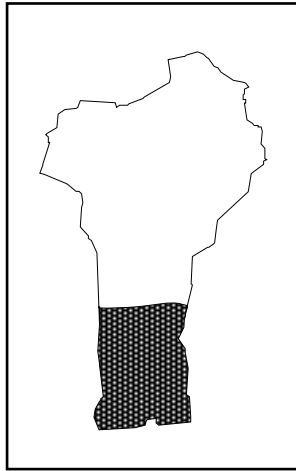
Note: The estimation method is probit. Standard errors in parentheses, clustered at the Commune level. All models include candidate fixed effects. *significant at 10%; **significant at 5%; ***significant at 1%

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