Randomized Evaluation of Institutions: Theory with Applications to Voting and Deliberation Experiments*

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Abstract

We study causal inference in randomized experiments where the treatment is a decision making process or an institution such as voting, deliberation or decentralized governance. We provide a statistical framework for the estimation of the intrinsic effect of the institution. The proposed framework builds on a standard set-up for estimating causal effects in randomized experiments with noncompliance (Hirano-Imbens-Rubin-Zhou [2000]). We use the model to re-analyze the effect of deliberation on voting for programmatic platforms in Benin (Wantchekon [2008]), and provide practical suggestions for the implementation and analysis of experiments involving institutions.

1 Introduction

Randomized experiments are a widely accepted approach to infer causal relations in statistics and the social sciences. The idea dates back at least to Neyman (1923) and Fisher (1935) and has been extended by D. Rubin and coauthors (Rubin (1974), Rubin (1978), Rosenbaum and Rubin (1983)) to observational studies and to other more general experimental designs. In this approach, causality is defined in terms of potential outcomes. The causal effect of a treatment, say Treatment 1 (compared to another treatment, Treatment 0) on the variable Y and on the statistical unit i is defined as $Yi(1) - Yi(0)$ (or its expected value) where $Yi(j)$ is the value we would observe on unit i if it receives Treatment j. The estimation of this effect is problematic because unit i cannot be given both Treatment 1 and Treatment 0. Randomizing the assignment of units to treatments

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allows us to overcome this difficulty. To estimate the causal effect of a treatment, two random samples of units are selected, the first group is assigned to Treatment 0 and the second group to Treatment 1. The difference in the sample means of Y (or some other statistic of interest) over the two groups is used as an estimate of the causal effect of the treatment. The main idea is that randomization eliminates (at least in theory) any systematic difference between the two samples. [1]

The past ten years have seen a sharp increase in the use of randomized experiments in development economics and political science. Researchers and policy makers have become increasingly concerned about the identification of the effects of programmes in face of "complex and multiple channels of causality" (Banerjee and Duflo [2008]. p. 2). Most of the early experiments in economics were interested in identifying the causal effects of various education inputs such as textbooks, and the student-teacher ratio on learning; others looked at the effect of the treatment of intestinal worms on various measures education outcomes of the effect of job training programmes on unemployment rate. Randomized field experiments in political science have primarily focused on studying the way in which various techniques of voter mobilization (mail, canvassing, telephone) affect voter turnout.[2] More recent work covers a very wide range of topics such a women leadership, corruption, conditional cash transfer programmes, clientelist and programmatic politics. They also use increasingly refined and reliable identification strategies. (See Duflo (2008) and Gerber and Green (2007) for a survey).

In nearly all previous research, the treatment is conceived and designed by the experimenter and assigned to an individual or a group of individuals. There might be compliance problems, i.e. individuals in active treatment groups might choose ex post to enter the control group or vice-versa (see Imbens and Rubin (1997) and Angrist, Imbens and Rubin (1996)). It might also not be legally feasible to assign individuals to treatment or control groups, so the experimenter simply encourages individuals to take treatment 1 (and individuals so encouraged comprise the treatment group) (Hirano, Imbens, Rubin and Zhou (2000)). The policy to be evaluated might lack clarity or its implementation might be imperfect (Harrison, Lau and Rutström (2005)). In all these cases, there is a difference between the treatment assigned and the treatment received and this has been dealt with in a variety of ways by the encouragement design, non-compliance and treatment uncertainty literature.

Now assume that the treatment or the policy to be evaluated is an unknown outcome of a well specified process. That is, groups of individuals are randomly assigned to decision-making processes that allow them to pick the treatment they will eventually receive. For instance, instead

[1] See Holland (1986) among others for a review.

[2] Gosnell (1927), Elderveld (1956), Adams and Smith (1980), Miller, Bositis and Baer (1981) and more recently Green and Gerber (2000).

of assigning schools to textbooks, flip charts or deworming treatments, we assign them a decision-making process over these three possible treatments,whereby parents and teachers use a simple majority voting rule to decide whether all the classrooms should receive textbooks or flip charts, or all the students should be treated with deworming drugs. Instead of majority rule, the decision-making process could be a strict proportionality rule: if α percent of the parents and teachers prefer X, then a proportion α of the school budget should be spent on X. This type of experiment would help identify the causal effect of the education inputs, when they are endogenously selected by parents and teachers. It could also help identify the intrinsic effect of majority or proportionality rule, and this result would have implications for evaluating not only education policies, but other public policies. The study would also contribute to empirical studies of institutions by providing a rigorous test of the causal effect of majority and proportionality rule on a variety of outcomes.[3]

Our empirical strategy consists first, of estimating the policy effect, by matching units within the treatment group with similar propensity scores and different policy outcomes. Then, assuming that policy selection is conditional only on observed covariates, we can derive the institutional effect by subtracting the estimated policy effect from the "total" treatment effect,i.e. the difference in means between treatment and control group observations. When the number of treatment groups is limited, we propose consistent estimates of institutional treatment effects by modeling explicitly individual choices in the treatment groups.

Our research question and strategy bear some similarity with Dal Bo, Foster and Putterman (2008). They present the results from a laboratory experiment designed to encourage cooperative behavior in prisoner dilemma games. The find that, the "policy" designed to encourage such behavior is more effective when it is chosen endogenously than when it is imposed on the players. They conclude that democracy may have direct effect on behavior. As in Dal Bo and al (2008), our control institution is exogenous and we estimate selection effect, but our set up is very different: it is grounded in the Rubin Model of Causality and its application to randomized evaluation of public policies and to field experiments. So we appeal to different traditions in the experimental literature.

One important area of application of our model is Community Driven Development (CDD), which is currently the fastest growing form of development assistance. They consist of public projects (e.g. infrastructures, public health, education) in which local communities have broad decision-making power, especially on issues financial management. Despite the centrality of CDD programmes in current development debates, there is little reliable evidence on their effectiveness.[4]

[3]The study would also be of great interest for policy-makers since it incorporates political economy considerations in the impact evaluation of education inputs on learning.

[4]See Mansuri and Rao [2004], Arcand and Bassole [2007].

According to Mansuri and Rao [2004], "not a single study establishes a causal relationship between any outcome and participatory elements of community-based development project (p.1). There is, however, a sense in which these projects tend to be dominated by elites and generate worse development outcomes in more unequal and institutionally weak environments. In short, the working of the CDD programmes may generate specific political outcomes (e.g. elite capture) or specific policy outcomes (.e.g education reform), and there is no systematic way to disentangle of the pure political or policy effect from the intrinsic institutional effect. We propose an empirical strategy that consists of estimating the policy or political effect by matching treated villages that have different policy or political outcomes, and then estimate the intrinsic effect by subtracting the policy effect from the total ITT effect of CDD programmes.

Besides CDD projects, there are at least two recent papers that explicitly integrate institutions or decision-making processes in field experiments. Olken (2008) provides experimental evidence from Indonesia on the effect of direct democracy on support for public goods provision. The experiment involves 49 villages that were assigned to select development projects either through direct elections or meetings of local leaders. In each village, there is one general project proposed by the village at large and one women's project in which only female voters are allowed to participate in the selection process. The author finds that direct participation has a positive effect on satisfaction among villagers, knowledge about the project and willingness to contribute, but finds no significant difference between direct democracy and representative-based meetings in terms of the project picked. In a paper using similar approach, Wantchekon (2008) provides experimental evidence on the combined effect of "informed" non-clientelist platforms and public deliberation on electoral support for political candidates. The experiment takes place in Benin and involves 5 candidates running in the first round of the 2006 presidential elections. The treatment to be evaluated is a two-stage public deliberation process. In the first stage, policy experts helped candidates design electoral platforms that are specific and transparent in terms policy promises. In the second stage (during the elections), there were town meetings in treatment villages, while there were rallies in control villages. The author finds that the treatment (specific platforms and town meetings) has a positive effect on voter information about policies and candidates. He finds that both turnout and electoral support for the candidate running the experiment was higher in treatment areas than in control areas (even though the turnout result was much more significant than electoral support result).

One important limitation of these two papers is that they could not always isolate the intrinsic effect of the institutions from the effect of the selected policy. For instance, in Olken (2008) satisfaction is higher under direct democracy than under representative meetings in general interest projects and women projects; but, in women projects, it is unclear if the result was driven by democracy or by the policy outcome chosen by democracy. Indeed, in projects selected by women, the type of projects selected under democracy were different from the ones selected under representative meetings. Thus, the difference in satisfaction could well be driven at least in part by differences in policy selected under the two political mechanisms. In addition, even in the case of general interest projects, a simple comparison of groups that have selected the same policy under direct democracy and representative-based meeting can lead to a selection bias, since the selection of policy is endogenous. As for Wantchekon (2008), treatment groups have town meetings where specific policies were discussed as opposed to control villages where rallies where held and mostly clientelist platforms were presented. The paper did not investigate whether the effect of the treatment was driven by the information content of the electoral platforms or by the institution of the town meetings. The goal of this paper is to provide a statistical model that disentangle these two effects thereby helping to identify the intrinsic effect of the institution.

In the next section, we will present the statistical framework. We then apply it to the town meeting experiment in Benin and provide practical suggestions for estimating intrinsic causal affects of institutions.

2 The Model

2.1 Defining the causal effects

Suppose we have two collective policy decision-making processes or institutions denoted 0 and 1. The processes are assigned to communities, i.e. groups of individuals. For simplicity, we assume that Process 0 is the control, which consists of applying exogenously a clearly defined policy (called Treatment 0) to the community; whereas in Process 1, the community is given the possibility of choosing through some decision-making process (e.g. voting, deliberation) any treatment in a set $\{0, \ldots, L\}$. The Treatment 0 from that set is the same as the treatment applied under Process 0. Let Y denote an outcome variable of interest that will be measured after the Treatment is applied. Let Z be an indicator variable that denotes which process is applied to the community. Let $D \in \{0, \ldots, L\}$ be the treatment choice made by the community under Process 1. [5]

For a randomly selected individual, let $Y(0)$ be the potential outcome we would observed on that individual had her community assigned to Process 0 (and thus policy 0). Similarly, let

[5] We should note that D is not an intermediate variable that lie in the path between Z and Y (i.e. a mediating variable). Instead, it is the always endogenous outcome of an institution (process 1), not a potentially exogenous outcome of an exogenously assigned policy. (See Imai et al [2008] for an analysis causal mediation effects).

$Y(1,d)$ be the potential outcome we would observe on that individual had her community assigned to Policy d under Process 1. Let $D \in \{0,\dots,L\}$ denotes the policy chosen by the community under Process 1. We define the causal effect of Process 1 (compared to Process 0) as

$$\tau_0 = \mathbb{E}\left[Y(1,D) - Y(0)\right]. \tag{1}$$

The effect τ_0 corresponds to the overall effect of Process 1 versus Process 0 and includes both the effect of the selected policy D and the effect of the decision-making process. By encouraging people participation and exchange of information, the decision-making process itself can have a substantial effect on the outcome variable Y. We call such effect the intrinsic effect of the decision-making institution defined as

$$\tau_2 = \mathbb{E}\left[Y(1,0) - Y(0)\right]. \tag{2}$$

We also introduce the causal effect of Treatment d versus Treatment 0 (under Process 1) for the "treated". This is defined as:

$$\tau_{1,d} = \mathbb{E}\left[Y(1,d) - Y(1,0)|D = d\right] := \frac{\mathbb{E}\left[(Y(1,d) - Y(1,0))\,\mathbf{1}_{\{D=d\}}\right]}{\mathbb{P}\left(D=d\right)}.$$

This quantity measures the intrinsic effect of policy d versus policy 0 under Process 1 given that policy d is chosen. Clearly, the overall effect of Process 1 versus Process 0 can be written as $\tau_0 = \tau_1 + \tau_2$ where the term τ_1 can be further decomposed as a weighted average of the intrinsic conditional effect of the policies $\tau_{1,d}$. This is done in the following Proposition.

Proposition 2.1. *We have*

$$\tau_0 = \tau_2 + \sum_{d=1}^{L} \tau_{1,d}\, \mathbb{P}\left(D=d\right).$$

Proof. Clearly, $\tau_0 = \tau_1 + \tau_2$, where $\tau_1 = \mathbb{E}\left(Y(1,D) - Y(1,0)\right)$. And

$$\begin{aligned}
\mathbb{E}\left(Y(1,D) - Y(1,0)\right) &= \mathbb{E}\left[\sum_{d=0}^{D} \mathbf{1}_{\{D=d\}}(Y(1,d) - Y(1,0))\right] = \sum_{d=1}^{D} \mathbb{E}\left[\mathbf{1}_{\{D=d\}}\left(Y(1,d) - Y(1,0)\right)\right] \\
&= \sum_{l=1}^{D} \tau_{1,d}\mathbb{P}\left(D=d\right).
\end{aligned}$$

□

2.2 Connection with the literature

As discussed in the introduction, there is a growing number of field experiments in the empirical social sciences where the experimental design falls in the model described above. This is the case for example for Olken (2008) which provides experimental evidence from Indonesia on the effect of direct democracy on support for public goods provision. Another example mentioned above is Wantchekon (2008) which provides experimental evidence on the combined effect of "informed" platforms and public deliberation on electoral support for programmatic, non-clientelist platforms.

The set up presented above is similar to the framework of randomized experimentation with encouragement of (2; 20; 14). Indeed, in designs with encouragement, individuals are encouraged to take a particular treatment but are ultimately free to comply or not with the proposed treatment. Similarly, in the design above, communities assigned to Process 1 can choose any policy in the set $\{0, \ldots, D\}$. But there is the important different here in that we are mainly interested in the intrinsic effect of Process 1. This corresponds to the intrinsic effect of the encouragement in designs with encouragement. This effect is of little interest in this type of design and, in order to identify the main effect of the treatment, is typically set to zero through the so-called inclusion-exclusion assumption (see e.g. (2)).

As in the encouragement design literature, the causal effect τ_0 can be seen as an Intent-To-Treat estimator, which focuses on the causal effect of the assignment rather than on the causal effect of the treatment (policies). But the complication here is that in additional to the individual effect of each policy, τ_0 also contains the intrinsic effect of Process 1.

Our framework is also related to the mediation analysis of (16). Although the two models are formally similar, our policy choice variable D is not a mediating variable. As a results the causal effects of interest in the two frameworks are different. The intrinsic causal effect of Process 1 (τ_2) defined above corresponds to what (16) called *the controlled direct effect of the treatment*. This *controlled direct effect of the treatment* differs from the *causal mediation effect* investigated in (16).

2.3 Statistical estimation

The causal effect τ_0 is estimable from the design. If the assignment to the two processes is randomized, then τ_0 can be estimated by comparing the average outcome over the communities under Process 1 and the communities under Process 0. But τ_2, the intrinsic effect of Process 1 cannot be estimated without further assumptions. For example, a simple comparison of the outcome of the communities under Process 1 that have selected Treatment 0 and the communities under Process 0 will not give τ_2 in general unless the policy choice is ignorable - that is, the policy choice does not depend on the expected outcome.

Consider a community with n individuals. Let $\vec{Y}(1,d) = (Y_1(1,d),\dots,Y_n(1,d))$ be their vector of counterfactual outcome variables under Process 1 and Policy d. We assume that individual i possesses a covariate $X_i \in \mathcal{X}$ and we denote $\vec{X} = (X_1,\dots,X_n)$. In order to separate τ_1 and τ_2, we assume that $\vec{Y}(1,d)$ and D are conditionally independent given $\vec{X}$. This is the strong ignorability assumption of Rubin & Rosenbaum (1983).

(A):

$$\mathbb{E}\left[\vec{Y}(1,d)\mathbf{1}_{\{D=d\}}|\vec{X}\right] = \mathbb{E}\left(\vec{Y}(1,d)|\vec{X}\right)\mathbb{P}\left(D=d|\vec{X}\right), \quad d=0,\dots,L.$$

Then we define the propensity score function

$$\alpha_d(x) := \mathbb{P}\left(D=d|\vec{X}=x\right).$$

Proposition 2.2. *Assume (A). Suppose that $\alpha_d(x) > 0$. Then for any $1 \le i \le n$,*

$$\frac{Y_i(1,d)\mathbf{1}_{\{D=d\}}}{\alpha_d(\vec{X})}$$

and $Y_i(1,d)$ have the same expectation.

Proof. The proof is a straightforward application of (A) and by conditioning on $\vec{X}$

$$\begin{aligned}
\mathbb{E}\left[\frac{Y_i(1,d)\mathbf{1}_{\{D=d\}}}{\alpha_d(\vec{X})}\right] &= \mathbb{E}\left[\mathbb{E}\left(\frac{Y_i(1,d)\mathbf{1}_{\{D=d\}}}{\alpha_d(\vec{X})}|\vec{X}\right)\right] = \mathbb{E}\left[\frac{1}{\alpha_d(\vec{X})}\mathbb{E}\left(Y_i(1,d)\mathbf{1}_{\{D=d\}}|\vec{X}\right)\right], \\
&= \mathbb{E}\left[\mathbb{E}\left(Y_i(1,d)|\vec{X}\right)\right] = \mathbb{E}(Y_i(1,d)).
\end{aligned}$$

□

This proposition shows that under (A), the different causal quantities defined above can be estimated. How to estimate these quantities depends on the design of the experiment. We show on an example how to estimate $\tau^{(2)}$. We suppose that assumption (A) holds. Suppose also that we have K communities indexed with k from 1 to K and that community k has n_k individuals. Denote $Y_{k,i}(0)$ (resp. $Y_{k,i}(1,d)$) the counterfactual outcome of individual i in community k if that community is assigned to Process 0 and Policy 0 (resp. Process 1 and Policy d). We will use the notation $\vec{Y}_k(0) = (Y_{k,1}(0),\dots,Y_{k,n_k}(0))$ and $\vec{Y}_k(1,d) = (Y_{k,1}(1,d),\dots,Y_{k,n_k}(1,d))$. Let $Z_k = 0$ if community k is assigned to Process 0 and $Z_k = 1$ otherwise and let D_k denotes the policy chosen by the community k. For simplicity, we assume that the variables $\left(Z_k, D_k, \vec{Y}_k(0), \vec{Y}_k(1,0), \ldots, \vec{Y}_k(1,L)\right)$ are independent with the same distribution and that for each k, the initial assignment Z_k is completely randomized. That is Z_k and $(D_k, Y_k(0), Y_k(1,0), \ldots, Y_k(1,L))$ are independent. For the k-th community, we observe Z_k, D_k and for the i-th individual in the k-th community, we observe $Y_{k,i}$ where

$$Y_{k,i} = Y_{k,i}(0)\mathbf{1}_{\{Z_k=0\}} + \mathbf{1}_{\{Z_k=1\}} \sum_{d=0}^{L} Y_{k,i}(1,d)\mathbf{1}_{\{D_k=i\}}.$$

In other words, if $Z_k = 0$, we observe $Y_{k,i} = Y_{k,i}(0)$, if $Z_k = 1$ and $D_k = 0$, we observe $Y_{k,i} = Y_{k,i}(1,0)$ etc... We introduce the estimator

$$\hat{\tau}_K^{(2)} = \frac{\sum_{k=1}^K \alpha_0^{-1}(\vec{X}_k)\mathbf{1}_{\{Z_k=1\}}\mathbf{1}_{\{D_k=0\}}\bar{Y}_{k,\cdot}}{\sum_{k=1}^K \mathbf{1}_{\{Z_k=1\}}} - \frac{\sum_{k=1}^K \mathbf{1}_{\{Z_k=0\}}\bar{Y}_{k,\cdot}}{\sum_{k=1}^K \mathbf{1}_{\{Z_k=0\}}},$$

where $\bar{Y}_{k,\cdot} = n_k^{-1}\sum_{i=1}^{n_k} Y_{k,i}$. We make the convention that $0/0 = 0$. Given Proposition 2.2, it is easy to see from the expression why the estimates should be consistent. That is as K converges to infinity, $\mathbb{E}(\hat{\tau}_K^{(2)})$ should converge to τ_2.

Proposition 2.3. *Assume (A) and $\alpha_0(x) > 0$. Then $\mathbb{E}(\tau_K^{(2)})$ converges in probability to τ_2 as $K \to \infty$.*

Remark 2.1.

1. More can be said. If the counterfactual variables have finite second moments, (A), $\alpha_0(x) > 0$ and the independence assumption implies that there exists $\sigma^2 > 0$ such that

$$\sqrt{K}\left(\hat{\tau}_K^{(2)} - \tau_l\right) \xrightarrow{w} \mathcal{N}\left(0, \sigma_2^2\right), \quad \text{as } K \to \infty. \tag{3}$$

   This is a standard application of the central limit theorem. The expression of the variance σ^2 is slightly involved and can be difficult to estimate. In practice, a simpler approach to evaluating the precision of $\hat{\tau}_2$ and make inferences is to use bootstrapped standard errors.

2. In practice, $\alpha_0(x)$ is rarely known and needs to be estimated. We can do this through methods such as multinomial logit modeling. We assume that the decision process works as follows: The k-th community assigns utility $U_{k,d}$ to Policy d and choose the policy with the highest utility. We model $U_{k,d}$ as

$$U_{k,d} = \beta_{k,d}V_{k,d} + \epsilon_{k,d},$$

   where $V_{k,d}$ represents the preference of community k for Policy d and $\epsilon_{k,d}$ is an error term that we assume follows an extreme type-I distribution (Gumbel distribution). $\beta_{k,d}$ are policy-specific and community-specific parameters. The preference $V_{k,d}$ can be observed by surveying the community. It is then well-known (Mc Fadden (1973)) that then we have

$$\mathbb{P}\left(D_k = d|\beta, V_k\right) = \frac{e^{\beta_{k,d} V_{k,d}}}{\sum_{j=1}^{K} e^{\beta_{k,j} V_{k,j}}},$$

which provides the probabilistic model for D. We can then treat $\beta_{k,d}$ as random effects and build a hierarchical model which will pool the communities together for a better estimation of $\beta_{k,d}$. These are standard modeling techniques that can be implemented once data becomes available.

3. If we replace the function $\alpha_0(x)$ by an estimate of $\mathbb{P}\left(D_k = d|\beta, V_k\right)$ above, what can we say about the asymptotic properties of the resulting estimators? There are some indications in (13) that such estimators continue to perform well, sometimes better than $\hat{\tau}_K^{(2)}$.

3 Town meeting campaign experiment in Benin

The town meeting campaign experiment investigates the effect of public debates around specific and informed policy platforms on turnout and voting, in the context of 2006 presidential elections in Benin. [6] In treatment groups, political parties systematically hold town-meetings where expert-informed campaign messages were delivered to and debated by voters. In control villages, the messages were delivered mostly through campaign rallies, with no public debates.[7] Thus, the treatment is *not* a pre-designed, pre-crafted platform or a vignette that would be read to voters. Instead, it is a process for generating political platforms more or less endogenously through a combination or policy proposals by candidates or their representatives and public debates involving voters. The goal of the paper is evaluate the effect of the process on voting.

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 amend the policy proposal of the candidate and $D = 1$ if the villagers through the town meetings have substantially amended the candidate proposal. In other words, when $D = 0$ in a treated village, that village has received the same 'treatment' as a control village where no town meeting were held. The electoral outcome is $Y(0)$ in control villages and $(Y(1,0), Y(1,1))$ in treatment villages. Following the framework developed above, we define $\tau_0 = \mathbb{E}\left(Y(1,D) - Y(0)\right)$ the total effect of Process 1, $\tau_1 = \mathbb{E}\left(Y(1,1) - Y(1,0)|D = 1\right)$ the intrinsic effect of information and $\tau_2 = \mathbb{E}\left(Y(1,0) - Y(0)\right)$ the intrinsic town meeting effect. The goal is to evaluate τ_2.

[6] The first part of the section draws mostly from Wantchekon (2008)
[7] For details about the experimental protocol, see Wantchekon [2008].

The drawback of the experiment is that a number of important variables necessary to carry out the above analysis have not been recorded. Indeed, neither participation to town meetings nor the proceedings of the debates (which define D) were not systematically recorded. Moreover, the message delivered in the control group was quite different from the policy-based platforms delivered in treatment groups.

To address these issues we make a number of assumptions. Firstly, we assume that, despite citizens' participation, town meetings only marginally change the candidates platforms. This is based on the qualitative evidence from the proceedings of these meetings. We can formalize this assumption mathematically as

$$\mathbb{P}\left(D=0|X=x\right)=\alpha_0(x)=1, \quad \text{for all} \quad x\in\mathcal{X}.$$

In other words, the villagers almost always choose the policy-based platform proposed by the candidate.

Secondly, denote $Y^\star(0)$ the outcome variable in control groups. Since the typical campaign message in control groups is a clientelist message substantially different from the treatment group message, $Y^\star(0)\neq Y(0)$ in general. We assume, that electoral support for the candidate in the control group would have been better has he ran a clientelist platform. That is,

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

In other words, we assume that the platform delivered in control groups will never do worse on the voting outcome (in control villages) than the policy-based platform in treatment groups. This is reasonable assumption that is based on the evidence provided in Wantchekon (2003), suggesting that clientelist platforms perform much better than programmatic platforms in all experimental conditions.

Under the above assumption,

$$\tau_2=\mathbb{E}\left(Y(1,0)-Y(0)\right)=\mathbb{E}\left(Y(1,D)-Y(0)\right)\geq\mathbb{E}\left(Y(1,D)-Y^\star(0)\right).=\tau^\star$$

The right-hand side of this equation, $\tau^\star$, has been estimated in Wantchekon (2008), by comparing the voting outcome in treatment groups to the outcome in control groups. We focus on two outcomes of interest: voter information and voting behavior.

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 capture 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.

Tables 1A and 1B (see appendix) present the results on policy and candidate information. 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. We therefore conclude that the intrinsic effect of town meeting on voter information is positive and significant.

Voting behavior Table 2A (in appendix) 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.

Wantchekon (2008) also uses a probit model to test the effect of the treatment on voting.

$$\begin{aligned} P\left(Y_{ij}=1|z_{ij},T_i\right) &= P\left(z_{ij}a+T_i\beta+x_{ij}T_i\gamma+u_{ij}>0\right) \\ & \quad u_i \overset{id}{\sim} N(0,\Omega_i) \end{aligned}$$

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, 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.

Table2B in appendix indicates that the treatment has no effect on voting behavior, which is a bit surprising given the results described in Table 2A [8] Thus, our model indicates that, at the very

[8] 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

least, town meetings help annihilates any electoral advantage that clientelist platforms might have over programmatic platforms. In other words, programmatic platforms might be more electorally effective than clientelist platform provided that they are communicated to voters through town meetings

4 Practical Implications

In the previous section, we tried to make the most out of the data available to estimate the causal effect of town meetings on electoral support for programmatic platforms. But, there are aspects of the design of the experiment that clearly needs to be improved for better identification of the causal effects town meetings. Here are key steps that we need to be taken to improve experimental studies involving institutions and processes.

First, to ensure internal validity, the institution to be evaluated has to be clearly defined and the rules that govern its implementation stated clearly and unequivocally: who are the players involved, who has the right to move first, or second, etc...? What are the policy alternatives and how individual preferences over those policies are aggregated? This aspect of the experimental design is usually well developed in Olken (2008) and Wantchekon (2008) but less so in CDD projects [9].

Second, since we are suggesting the use of propensity score matching of treated units for the estimation of policy effects, there needs to be detailed information on the implementation of the institution, particularly background data on treated communities and individuals. In a democracy experiment, we would need to know those who voted, and their demographic as well as social characteristics. In a deliberation experiments, we need document class and ethnic cleavages in community[10], who took part in the meeting, what proposals or amendments they made. In short, we need to document and measure key aspects of the deliberative process.

Third, we need to document the institutional outcomes. (e.g. the "resolutions" of each town meeting, the voting outcomes) and the final outcomes of interest (satisfaction, levels of the public goods, poverty, etc...).

The policy effect will be evaluated by estimating the difference in the final outcome of interest, between similar treated villages and individuals who choose different policies or projects.

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.

[9] See Arcand and Bassole (2008) and Mansuri and Rao (2004).

[10] This is true for democracy experiments as well.

The intrinsic institutional effect is the difference between the total ITT effect and the estimated policy effect.

As we mentioned earlier, in the democracy experiment in Indonesia, Olken (2008) finds no difference in institutional outcomes under direct democracy and representative meetings, for general projects. Therefore, in that case, the total ITT effect on citizen satisfaction coincides with the intrinsic institutional effect. He did not, however, estimate the institutional effect for women projects because, in that case, projects selected tend to differ across institutions.

Finally, our model relies on the assumption that the control institution is exogenous or that the policy in the control group is exogenously imposed to the community. We avoided the complication of comparing two endogenous institutions and having two moving parts. We therefore suggest that the "control institution" always be an exogenously imposed policy against which the treatment institution and its policy outcomes would be compared.

5 Conclusion

We propose a framework for estimating the intrinsic impact of a decision-making process(or institution) in experiments where such a process is randomly assigned to groups of individuals who then decide which treatment to receive. In our framework, a randomized evaluation of institutions has the structure of group-based encouragement design with multiple choice over treatments or policies. The main challenge in such experiments is to separate the institutional effect from the policy effect.

Our empirical strategy consists first, of estimating the propensity to adopt a policy among individuals in the treatment group. Then, assuming that policy selection is conditional only on observed covariates, we can compute the policy effect. Finally, we can derive the institutional effect by subtracting the estimated policy effect from the "total" treatment effect,i.e. the difference in means between treatment and control group observations.

Our results could help improve our understanding of how results from policy experiments would change when they are brought to scale, when institutional constraints are integrated into the analysis. In addition, our paper contributes to the ever growing literature in the social sciences on the effects of institutions by proposing an experimental strategy for estimating the direct effect of institutions on behavior.

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APPENDIX

Table 1A: Information - Candidates

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	.169***	.169**	.167***	.167**	.156***	.156**
	(.055)	(.066)	(.056)	(.072)	(.058)	(.061)
Education			.314***	.314***	.198***	.198***
			(.59)	(.075)	(.064)	(.076)
Other controls	No	No	Np	No	Yes	Yes
					(.037)	(.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%. Other controls include Gender, age, ethnic affiliation, media access.

Table 1B: Information - Problems Facing Country

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	.153***	.153*	.143**	.143	.177***	.177*
	(.058)	(.091)	(.058)	(.094)	(.060)	(.104)
Education			.426***	.426***	.339***	.339***
			(.061)	(.064)	(.065)	(.071)
Other controls	No	No	No	NO	Yes	Yes
					(.039)	(.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

Table 2A: 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

Note: T means Treatment and C means Control

Table 2B: Vote for Experimental Candidate

	(1)	(2)	(3)	(4)
Treatment	-.025	-.019	-.050	-.181
	(.286)	(.284)	(.278)	(.205)
Education		-.247**	-.227**	-.253
		(.119)	(.107)	(.159)
Other conttrols	No	No	Yes	Yes
				(.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%