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Experiments on Clientelism and Vote-Buying*

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Abstract

In this chapter we present a critical survey of experiments on political clientelism and vote-buying. We claim that through randomization and control, field experiments represent an important tool for answering causal questions, while list experiments provide useful methods that improve the hard task of measuring clientelism. We show that existing experimental research gives answers to the questions of why clientelism is effective for getting votes and winning elections, who relies more on this strategy —incumbents or challengers—, how much clientelism takes place, and who tend to be the favorite targets of clientelistic politicians. The relationship between clientelism and other illicit strategies for getting votes, such as electoral violence and fraud, has also been analyzed through experimental interventions. Experiments have also studied mechanisms and policies for overcoming clientelism. Finally, we show that external validity is a major source of concern that affects this burgeoning literature.

1 Introduction

Do politicians allocate public goods to citizens or voters to maximize social welfare or merely to win elections? Are they programmatic or opportunistic? Most studies in political science and economics point to the latter. In apparently dissimilar places such as Argentina (Calvo and Murillo 2004; Stokes 2005; Weitz-Shapiro 2011), Japan (Kobayashi 2006), Mexico (Greene 2001; Diaz-Cayeros et al. 2011), Southeast Asia (Scott 1972), Tropical Africa (Lemarchand 1972), Colombia (Leal and Davila 1990), Italy (Golden and Picci 2008), and many others, there seems to be a common trend: the use of clientelism,

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vote-buying, patronage, and machine politics as strategies to win elections and to remain in office. Even though political clientelism is an old phenomenon that social scientists have tried to explain through different approaches (Scott 1972; Lemarchand 1972; Stokes 2005), there has been a renewed interest by political scientists and economists, with new questions and novel techniques.

There are many open questions: when is clientelism effective? What is the relationship between poverty, income inequality and clientelism? Previous questions are plagued by endogeneity. For instance, clientelism can be considered both a cause and a consequence of poverty and inequality. This is because poor and unequal societies tend to generate politicians that offer goods and services—even money—in exchange for votes from citizens whose immediate material needs may make them ignore ideological platforms. Moreover, a clientelistic system can perpetuate poverty and inequality if the public goods provision is inefficient and wasteful.

Furthermore, because clientelism is to an extent illegal, it is hard to detect and measure. For instance, in surveys that ask individuals about their exposure to clientelism, the probability of misrepresentation of true preferences is not orthogonal to important covariates, such as income or education. Therefore, measurement error is another important source of potential endogeneity. Moreover, omitted variables that usually are hard to measure, like civic duty or honesty, might act as confounders that affect clientelism and other covariates. For this reason, traditional techniques that rely on observational data, usually surveys, in some cases may lead to wrong conclusions and biased estimations. In this context, field experiments represent a feasible alternative because randomization of treatment might help to achieve balance in both observable and unobservable covariates, allowing a correct estimation of causal effects where clientelism and vote-buying are involved. Additionally, novel techniques for the formulation of sensitive topics, such as list experiments, notably improve measurement and estimation of clientelism.

The goal of this paper is to discuss the main findings encountered by the growing literature in political economy that use experimental techniques to study clientelism—the exchange of material goods and services for political support—and vote-buying—the exchange of cash for votes before the election. For this purpose, first we argue that experimental methods provide advantages for studying clientelism and vote-buying as compared to observational studies. We stress that randomization and control, two typical allies of experimentation, allow us to tackle the typical causal challenges. Additionally, we describe and analyze the main findings of this literature with respect to

the nature of clientelism. In this vein, we discuss if clientelism is effective for making candidates win elections; we determine, following these studies, how often clientelism and vote-buying take place; we discuss if these practices are more advantageous for incumbents or for challengers; and a description of the demographics of clientelism is given by determining groups of voters that are targeted. Additionally, we relate clientelism to other available illegal techniques for winning elections, such as coercion and electoral fraud. We propose alternative mechanisms for overcoming clientelism and describe how experiments reveal these strategies as relevant. Finally, a discussion on the external validity of these experiments is given.

2 Experiments versus Observational Studies

Observational studies have dominated the empirical analysis of clientelism. The works of Stokes (2005), Brusco et al. (2004), Calvo and Murillo (2004), Nichter (2008), and Szwarcberg (2011), among others, represent an invaluable source of descriptive information on the activities and tactics employed by politicians and the populations usually targeted in clientelistic settings. Nonetheless, a big disadvantage of using this type of data is that establishing causal relationships is hard or unfeasible in many cases. As stated by Vicente and Wantchekon (2009) “[o]bservational studies always struggled with selection into policy adoption. And convincing instrumental variables, disentangling causality from simple correlation, are not possible to find in a systematic way” (p. 7). *It is fair to conclude that in the case of non-experimental data in many situations, endogeneity, in the form of reverse causality, measurement error, or omitted-variable bias, tends to be an important issue that might lead to incorrect conclusions.*

Based on these caveats, we agree with Morton and Williams (2010, p. 12) when they explain why political scientists rely on experiments:

During the last decades of the twentieth century, some political scientists began to worry that the restrictiveness of non-experimental data prevented researchers from asking important causal empirical questions [...] Because of the belief in the inability of survey data to answer causal questions, some researchers turned to experimental methods.

Then, what are the advantages of experimental data? Two basic features make this methodology interesting for answering causal questions: random assignment and control. Proper random assignment, commonly achieved in the experiments on clientelism

discussed here, guarantees balance on observed (and unobserved) covariates among treatment and control units. Control allows the researcher to manipulate, or at least attenuate, the effects of variables that affect both treatment and outcome variables.

Through their designs, the authors analyzed in this paper claim that they can draw causal conclusions from their studies. Vicente (2010, p. 7), for instance, argues that “[t]he exogenous variation produced by the intervention allows us to identify the effects of the campaign on perceptions about the prevalence of vote-buying (...) and the reduced-form effect on voting behavior, as given by voter turnout and candidate selection.” And he adds (p. 7): “Ultimately, our objective is to infer effects of vote-buying on voting behavior.” Wantchekon (2003, p. 402) goes even further and claims that “survey methods do not provide reliable and unbiased measures of clientelism because it (clientelism) is perceived by most politicians and voters as morally objectionable.” Furthermore, “we are left with subjective assessments of the intensity of clientelist appeals based on competing value judgments by social scientists.”

Consequently, we claim that random assignment and control are properties of experiments on clientelism that make the task of establishing causal relations less difficult. Additionally, list experiments and educational campaigns acting as instrumental variables have improved the hard task of measuring a morally condemned activity. Naturally, experimental methods impose some analytical costs, because even if treatment effects are significant, in many cases it is not clear which underlying mechanisms explain the causal relationships found. Additionally, and as it is discussed at the end of this paper, external validity is an important topic that in many cases reduces the predictive power of experimental studies.

In sum, experiments have helped researchers reduce measurement error, control for reverse causality, and attenuate the pervasive effects of omitted variables, thus improving the estimation of the causal effects associated to clientelism and vote-buying. Nonetheless, it is important to recognize that observational studies still are a valuable source of data and information that have characterized the activities associated to clientelism. We do not pretend to dismiss observational studies, as they have contributed to the understanding of what is clientelism, who tends to get involved, and where does it take place. A natural next step is to examine which short and medium term strategies are useful for overcoming clientelism. In this vein, we think that experimental methods have a lot to say in this respect if they increase the internal validity of the analysis. In fact, as it will become clear in the discussion presented below, experiments per se also provide very

valuable observational data that allows researchers answer important causal questions.

3 Is Clientelism Effective?

Is clientelism really advantageous for politicians that rely on it for getting votes?¹ This is not a minor question, given that the problem of enforceability is at the core of clientelism studies. A double commitment problem defines patronage politics: if a candidate offers goods or money in advance to an election, what makes citizens comply and subsequently vote for him? Or on the contrary, if the candidate promises resources upon being elected, in the form of jobs or contracts for instance, what makes him honor the agreement once he gets elected? Theoretically, different explanations have been offered (see Stokes 2005; Smith and de Mesquita 2011; Robinson and Verdier 2003; Gallego forthcoming). It is clear that if the commitment issues are not resolved, clientelism is not effective. What results have been encountered by the experimental literature?

In many aspects, Wantchekon (2003) represents the first attempt to employ experimental methods for studying political clientelism. Using Benin's 2001 presidential election, this field experiment uses novel techniques for testing several hypotheses linked to vote-buying. *Real* candidates competing in *real* elections are examined in this study, with the main goal of determining if clientelism is always a winning strategy. The author also wants to determine if incumbents tend to have advantages over challengers, the relationship between gender and clientelism, and the effects of ethnic affiliation.

For such purpose, in this experiment randomly selected villages are exposed to different political platforms during the electoral campaign. Eight noncompetitive districts are selected, four of them dominated by the incumbent and the remaining four by the opposition. In each district villages are divided into three subgroups: the first type (treatment) is exposed to a purely clientelistic platform, the second one (treatment) to a purely public policy platform, while the third one (control) to a mixture of both messages. Naturally, the goal is to determine how this random allocation of platforms affects electoral behavior in treatment and control villages.

Wantchekon (2003) claims that his experiment validates the view that in Benin electoral politics are dominated by clientelism. Overall, he finds that the average voting returns for experimental candidates is 0.84, 0.69, and 0.74, in clientelist, public policy,

¹Certainly, the fact that candidates are professional politicians and that they rely on clientelism reveals that it is beneficial. But compared to other techniques for getting votes, is it more effective?

and control villages respectively. Hence, even using official aggregate data, candidates get higher returns when they use a clientelistic message. These results are reinforced when survey data is used in which subjects are asked about their voting behavior after the elections take place. The results show that the clientelist treatment has a positive and significant effect for all candidates. Additionally, for certain candidates (northern, regional, and incumbents), the public policy platform has a negative effect, meaning that the experimental candidate gets a lower vote share when a programmatic platform is used, as compared to the mixed alternative. Consequently, at least in Benin, clientelism seems to be a better strategy for getting votes.

Vicente (2010) also exploits a real election in an African country for understanding the main consequences of vote-buying. A field experiment is conducted during the 2006 presidential election in Sao Tome and Principe, a two-island country in West Africa. With the support of the National Electoral Commission of this nation, a voter educational campaign is conducted in 40 randomly selected enumeration areas which constitute the treatment locations, while 10 areas were left as control.

The intervention consists of a campaign in which leaflets, stressing the illegal character and pervasive nature of vote-buying, are distributed, read, and discussed door-to-door. A panel survey is conducted before and after the election, gathering perceptions about vote-buying and actual electoral behavior. Official electoral data at the ballot station level is also employed. Here, the anti-vote-buying campaign is intended to be an instrument for measuring clientelism: if it has a negative and significant relationship with vote-buying, any effects of the campaign on electoral behavior unravels the effects of clientelism *per se*.

It is evident that in Vicente (2010) the educational campaign against vote-buying is a clever instrument for measuring this practice. He finds clear effects of this campaign on vote-buying perceptions and political behavior. The clearest effect is on the perception that vote-buying affected voting and that voting happened in good conscience. There is also a negative effect on the frequency of witnessing vote-buying transactions, which is significant using post-election data only, but not when using a difference-in-difference approach that incorporates pre-electoral data, and on the price of votes. Hence, the author claims that the campaign is effective at reducing vote-buying, precisely because vote-buying takes place and is effective. In the absence of vote-buying there should be no differences between treatment and control units. In terms of actual electoral outcomes, as opposed to surveys, Vicente finds that treatment “caused a decrease in voter turnout,

of the order of 2 to 6 percentage points”. Hence, it is possible to infer that vote-buying is effective at increasing voter participation in Sao Tome and Principe.

If clientelism is effective and makes certain candidates win elections, a big puzzle is to explain why both politicians and voters comply and honor their agreements, even though they might have incentives to betray. Based on his empirical evidence, Vicente offers a brief discussion about the mechanisms that might explain why vote-buying takes place at all. In his survey, respondents are asked about possible techniques used by politicians to enforce these transactions: “buying identity cards without which a voter cannot vote (...) asking for a photograph of the filled ballot paper at the ballot station, and substituting blank ballot paper (...) for prefilled ballot paper” (p. 15). If respondents widely report that these methods are commonly used by politicians, it would be possible to infer that vote-buying is explicitly monitored and enforced by candidates. But this happens not to be the case. Only 14 percent of those reporting being offered “gifts” in exchange for votes admit that any of these methods were used to enforce the transaction. Hence, Vicente concludes that this supports the idea that vote-buying is *self-enforcing*. However, it is not completely clear what kind of self-enforcing mechanism is working in this case. Moreover, this specific conclusion is not supported by experimentation.

Finan and Schechter (forthcoming) study the relationship between reciprocity and vote-buying, in order to determine in which way politicians overcome commitment problems inherent to this type of interaction. For this purpose, the authors combine experimental data eliciting subjects’ social preferences, especially reciprocity, with survey evidence on vote-buying episodes experienced by citizens. In this paper, Paraguayan voters, as well as middlemen that act as brokers between politicians and citizens, are analyzed. In fact, one of the main contributions of the paper is to emphasize the important role played by brokers.

The paper uses different sources for gathering data on clientelism. First, the study exploits the fact that households surveyed for a longitudinal study since 1991, sent a member to participate in 2002 in a series of experiments measuring trust, trustworthiness, and risk aversion. Second, the study utilizes data from the 2007 round of the aforementioned survey which includes a module on vote-buying. Finally, a middlemen survey is conducted in 2010, providing data on brokers’ perceptions of who was bought in past elections and citizens’ preferences. At the end of the survey, middlemen play a reciprocity game. Combining these different sources, Finan and Schechter try to establish who tends to be targeted by candidates within clientelistic transactions and in

which way social preferences affect this targeting.²

Finan and Schechter (forthcoming) try to open the black box of self-enforcing clientelism.³ For these authors, brokers and social preferences are the key components of a self-enforcing mechanism. First, politicians channel resources through brokers because they have direct contact and a deep knowledge of the constituency. The authors show empirically that the middlemen “do remarkably well in predicting not only the observable attributes of villagers, but also their social preferences.” The results are surprising. For instance, the correlation between middlemen’s reports of villagers’ years of schooling and what villagers report is 0.73. They can correctly name the spouses of the villagers 84 percent of the time. Hence, at least from these findings, it is possible to infer that knowledge about potential voters is one of the main reasons why politicians hire intermediaries. What is more important: middlemen are able to predict villagers’ social preferences. For instance, based on the results of a dictator game and the posterior survey to middlemen, the authors find that they can predict correctly 66 percent of the time if a villager sent at least half of his endowment. However, compared to a baseline of 50% for randomly guessing, a 33% increase is quite interesting, but less impressive.

Social preferences are the second component of Finan and Schechter’s argument. Empirical results reveal that middlemen target individuals they believe are reciprocal. Moreover, villagers that in fact are more reciprocal, and not only viewed as more reciprocal by middlemen, are more likely to be awarded. Hence, from these results the authors infer that vote-buying is self-enforcing and effective because parties use brokers that have a good knowledge of voters and use this knowledge to identify who is reciprocal. Therefore, these types of voters reciprocate gifts by voting for the clientelistic candidates.

Based on the evidence presented above, we conclude that experiments reveal that clientelism happens, increases vote shares for both incumbents and challengers, augments turnout, and is enforced through complex mechanisms that go beyond monitoring and coercion.⁴

²It should be clear that Finan and Schechter (forthcoming) use a hybrid methodological strategy, in which both observational and experimental data are used to understand some of the main characteristics of clientelism. As such, the paper is an interesting example of an eclectic approach to the empirical analysis of clientelism.

³For an alternative theoretical approach, see Gallego (forthcoming).

⁴Accepting that social preferences play an important role does not imply that monitoring is unnecessary. The fact that politicians invest in costly monitoring mechanisms reveals that it is still an important

4 Incumbents versus Challengers: the Supply Side of Clientelism

There are many reasons to believe that incumbents have advantages over challengers in the use of clientelism. To some degree, the fundamental problem of patronage politics is credibility and enforceability. Voters have to believe that candidates will comply with their commitments. If we think of cash as the main resource offered to voters, it is reasonable to suppose that incumbents, through their access to public spending, have more budget and flexibility. Challengers typically rely on personal funding or private sponsors. Moreover, if we talk about jobs or contracts, incumbency advantage is even more evident (see Robinson and Verdier 2003). Public office can be used before the election to offer jobs to local leaders and other intermediaries in order to make agreements credible. Discretion over public spending also gives incumbents an important source of contracts to offer potential political clients. In this section we review empirical findings of the experimental work on clientelism, in order to determine if incumbency advantage is a reality. This is important in order to establish possible mechanisms for overcoming clientelism.

The results found in Wantchekon (2003) support the incumbency advantage argument. According to the survey data, in experimental districts which are strongholds for the incumbent candidate, 89.7 percent of the subjects under the clientelist treatment voted for the incumbent, compared to 69.3 percent under the public-policy treatment and 83.5 percent under the control. This means that the clientelist treatment effect (6.2 percent) is positive and significant at a 90 percent confidence level. On the contrary, the public-policy treatment effect (-14.1 percent) is negative and significant at a 99 percent level. Hence, compared to villages in which a mixed message is utilized, the incumbent gets better results in clientelist villages and worse results in public-policy villages. *Clientelism is profitable and better than programmatic politics for the incumbent.* For the opposition candidate the situation is a bit different. In his strongholds, 68.1 percent of the voters under the clientelist treatment, 49.3 percent under the public-policy treatment, and 50.9 percent under the control, support him. Again, the clientelist treatment effect (17.2 percent) is positive (and in this case significant at a 99 percent level), but the public-policy treatment effect is not significantly different from zero. To some degree, the probit analysis presented by the author reinforces the result that incumbents and challengers face a positive clientelist treatment effect.⁵

component of patron-client relationships.

⁵However, it is important to check the results of the probit analysis because some of the coefficients

Vicente has a different perception. First, he differentiates between vote-buying and clientelism. According to his definition, vote-buying is “the exchange of cash for votes before the election” (p.2). Hence, for him it is not the same as clientelism, defined as the exchange of favors, like jobs, conditional on the politician winning the election. Given this distinction, the author concludes that incumbents are better at establishing clientelistic relationships, while challengers rely more easily on vote-buying. The goal of this paper is not to discuss which is the best definition of clientelism or what are its differences with vote-buying, patronage, machine or pork-barrel politics. The goal is to discuss the empirical results found by the experimental literature.

Based on his distinction, Vicente claims that his intervention is a campaign against *vote-buying*. Consequently, if this method is mainly used by challengers and not by incumbents, the voter educational campaign should be particularly harmful for the former. In the survey, subjects are asked about which candidate used vote-buying during the campaign. About 90 percent of the respondents claim that both candidates, the incumbent and the challenger, used this tactic. There is no significant difference between the perceived prevalence of vote-buying by the two candidates. Nonetheless and despite these average results, significant treatment effects are found: the education campaign has a negative and significant effect on vote-buying activities where treated subjects witness a lower level of vote-buying, but this impact is bigger, in magnitude and statistical significance, for the opposition candidate. For the incumbent, treatment implies a reduction in vote-buying of between 5 to 6 percentage points, while the decrease is of between 8 to 10 percentage points for the challenger. Hence, this result supports the idea that vote-buying—exchange of cash for votes *before* the election—is a strategy used by both candidates, but in more intensity by the challenger.

These findings are based on citizens’ perceptions captured by the post-intervention survey. Nonetheless, the author finds effects of the treatment on actual electoral outcomes as well. The campaign (treatment) caused a shift of votes from the opposer to the incumbent, as the former’s vote share decreases, while the latter’s augments. The magnitude of the effect is of about 4 percentage points and is statistically significant. Vicente claims that this result confirms his hypothesis that challengers rely more on vote-buying.

Is this argument credible? Although the statistical result is undeniable, we cannot be sure about the exact mechanism that makes the anti vote-buying campaign beneficial

are not significant.

for the incumbent and harmful for the challenger. It could be the case that, even though the campaign is conducted by an independent organization, citizens perceive it as a government action trying to combat something harmful for democracy, given its illegal nature. If citizens associate the campaign to the government, not surprisingly in treatment locations more people will shift from the challenger to the incumbent. In other words, an unintended side effect of the campaign would be to generate a positive image for the incumbent. Perhaps, the author could have controlled for this alternative channel by asking respondents about their perception of who conducted the educational campaign.

5 How Much Clientelism?

As we argued above, the illegal nature of vote-buying and clientelism makes difficult the task of measuring and detecting it with accuracy. However, some of the studies analyzed in this survey give a quantitative measure of how many citizens tend to be involved in vote-buying transactions. Naturally, the numbers differ across nations and depend on the methodology employed for measuring it. In many empirical studies, including some of the ones discussed here, it is found that less than 50% of the electorate gets involved in clientelism. Given that many of the existing models focus on explaining why clientelism exists and why patrons and clients comply, it is important to note that these models account for a minority of the population. In other words, even though we do not pretend to dismiss the importance of clientelism, it is responsible to admit that models do not predict that a majority of the electorate vote for other reasons.

Finan and Schechter, using their multiple sources of information, not only show that clientelism happens and is effective, but also offer a possible explanation as discussed above of why it is self-enforcing. Both in the household and middlemen surveys respondents are asked about their exposure to clientelism. Households are asked if during the 2006 election any political party offered them money, food, payment of utility bills, medicines, and/or other goods. Middlemen were asked if their parties offered each individual any of these. Also, if any other party made offerings of this kind to the individuals. From the household survey, the authors find that 26 percent were offered something in exchange for their votes. This is a non-negligible result, especially if we consider that in surveys in which respondents are asked directly about their experience, there tends to be underreporting. Results are even higher when middlemen are interviewed. In fact, middlemen report that 33 percent of the individuals were offered

something by their parties, while 46 percent were offered something by “any other” party. Nonetheless, given the sensitive nature of vote-buying, can we really trust what respondents answer in these types of surveys?

List experiments became an important subset of survey experimentation and as an alternative method for eliciting truthful answers to sensitive questions.⁶ Naturally, when subjects are asked about their exposure to clientelism, it is likely that social-desirability bias might lead them to under-report their real exposure. Gonzalez-Ocantos et al. (2012) claim that social-desirability bias is an important source of non-random measurement error in studies that ask directly about vote-buying. To show this, they conduct a list experiment immediately after the 2008 municipal elections in Nicaragua.

In their analysis the survey sample is split into random halves. Both groups are asked to determine from a set of activities, *how many* were used by politicians during the last electoral campaign. For the control group the list includes:

- Putting campaign posters in the neighborhood/city
- Visiting your home
- Advertising on TV or radio
- Threatening you to vote for them

The treatment group includes the same options, plus an additional alternative:

- Giving you a gift or doing you a favor

As a consequence of randomization and assuming that there are no design effects⁷ and that respondents give truthful answers about the sensitive item, any difference between the mean number of activities listed in the treatment and control groups represents the estimate of the proportion of vote-buying in the population. In the absence of vote-buying the difference between both means should be zero. The authors employ this technique for different sub-groups (partisan, socio-economic, and gender), to determine the amount of clientelism existent in each category.

⁶For other applications of list experiments in the social sciences see Kuklinsky et al. (1997), Streb et al. (2008), or Heerwig and McCabe (2009).

⁷There are no design effects in a list experiment when respondents give the same answers to control items, no matter if the sensitive item is included or not (see Blair and Imai (2012) for a formal approach to list experiments).

Gonzalez-Ocantos et al. (2012) argue that list experiments represent a better technique for measuring how much clientelism exists in an election. To show this, the authors compare the results of a survey in which respondents are asked directly about vote-buying, with the count obtained from the list experiment. The results are quite interesting. The mean number of items listed by the control group, in which only four activities are included, is 2.06. For the treatment group, whose list includes the sensitive item on vote-buying as well as the four items included for the control group, the average number of items reported is 2.31. Hence, due to random assignment to treatment and control groups, the authors conclude that the estimated percentage of respondents receiving gifts during this election in Nicaragua is 24 percent. This difference between treatment and control is statistically significant.

The authors compare this percentage of people to the amount of clientelism reported by subjects when asked directly about the sensitive topic. In this case, only 2.4 percent of the respondents admit that they received a gift in exchange for their vote. Nonetheless, it would have been interesting to compare the result of the list experiment not only with the percentage of people *admitting having received* a gift, but also with the percentage *having been offered* a gift. These two percentages might be quite different, and perhaps the latter is not very different to the one encountered through the list experiment. Naturally, this huge difference, of more than 20 percentage points, is statistically significant. The big question that derives from these results is: Which percentage represents an accurate measure of the amount of clientelism existent in the election? According to Gonzalez and Ocantos (2012) we should not trust respondents when they are asked directly because of the social-conformity bias: the illegal nature of vote-buying leads many respondents to lie and hide their real exposure to clientelism.

However, it is important to note that there is another way in which subjects can be asked about their exposure to clientelism: to ask about the exposure of neighbors, relatives, and friends. When asked about their neighborhood, 17.7 percent of the respondents answered in the affirmative. In this case, the 6.7 percentage point difference between the direct question, about clientelism in the neighborhood, and the list experiment outcome, is not statistically significant. Indeed, the authors recognize that “while the direct individual measure greatly underestimates the degree of vote buying, the neighborhood measure may provide a better aggregate estimate of vote buying”. Therefore, the list experiment suggests that in the case of clientelism, the social-desirability might be an issue only if respondents are asked about their personal exposure. Of course, this fact

does not mean that asking about neighbors' exposure is a better technique, as it might provide a very biased estimation of how much clientelism is taking place.⁸

Corstange (2010) uses an augmented list experiment to analyze the market for votes in Lebanon, a country he characterizes as prone to clientelistic linkages as it is a “fragile, developing democracy, (...) (with) middling income level, Latin American levels of income inequality, and African levels of social diversity” (p.8). Three testable hypotheses drive this analysis. First, as opportunities to sell votes increase, vote-buying should augment. Therefore, vote-buyers are more inclined to target voters that are easily monitored. Second, if sellers' reservation prices increase, vote-buying decreases. Hence, disinterested voters that have a weak or null party affiliation are more elastic, making them “cheaper” and more attractive for buyers.⁹ And third, the ability of buyers to price-discriminate depends on how competitive this market is. Naturally, the price of votes will differ in a monopsony situation in which there is a unique machine employing clientelism, versus a duopsony or a competitive market in which two or more parties establish clientelistic ties with voters.

As in Gonzalez-Ocantos et al. (2012), in this paper list experiments are justified as a method for eliciting truthful answers for sensitive questions. The main methodological difference between the two papers is that Corstange uses an *augmented* list experiment, in which the control group answers each list item, including the sensitive item, individually—thus for every item we have a percentage of respondents that give a yes to that item. The rest of the structure is the same.¹⁰ A list that includes a sensitive factor, such as exposure to clientelism, is given to the treatment group, and respondents in this group are asked to determine how many of them, but not which factors influenced their decision to vote.¹¹ This method is useful as it allows researchers to conduct a

⁸Suppose, for instance, that a town is inhabited only by four citizens. Only one of them is offered cash prior to the election and the other three observe it. When asked about *neighbors'* exposure to clientelism, three citizens respond affirmatively. This would lead us to conclude, erroneously, that 75 percent of the electorate are exposed to clientelism, when only 25 percent is really exposed.

⁹The author claims that even though this argument can also work for poor voters because for them the marginal utility of income is higher, it is not necessarily true that “poor” is the same as “cheap”. If poor voters have strong ideological ties, it might be quite expensive to buy them.

¹⁰It should be noted that the augmented list experiment adds an assumption about the comparability of form. That is, it is assumed that respondents provide answers on lists the same way they do when asked about items sequentially. Certainly, this assumption is debatable.

¹¹Subjects are asked which of the following reasons influenced their decision to vote and for whom: 1) You read a newspaper covering the campaign. 2) You read candidates' campaign platforms. 3) Someone offered you personal services, a job, or something similar. 4) You discussed with your friends about the

multivariate analysis of the determinants of clientelism (Corstange 2009).¹²

Corstange (2010) also uses his list experiment to compare the amount of clientelism reported by citizens when asked directly, versus the sensitive-question technique. As it was described before, in augmented list experiments subjects in the control group are asked to answer each item individually, including the sensitive item—clientelism as a determinant of their voting behavior. Hence, this provides a measure of how much clientelism is reported when asked directly. The result does not differ much from what is found in some observational studies. About 26 per cent of the respondents admit that offers of jobs, personal services, etc. affected their decision to vote.

However, the results are quite different when the list-experiment technique is used. For this purpose, a conventional difference-in-means test is conducted, comparing the average number of items listed in the treatment group with the equivalent in the control. The average in the treatment is 1.84 items, versus 1.29 in the control. This means that about 55 per cent of the Lebanese electorate are estimated to have established clientelistic ties with politicians—more than double of the percentage reported through the direct question.

Vicente (2010) also offers a quantitative magnitude of vote-buying in Sao Tome and Principe. The survey implemented in both treatment and control locations asks respondents about their exposure to this practice, as well as their experience witnessing vote-buying in their neighborhoods. Overall, 38 percent of the respondents acknowledge that an acquaintance has been offered something in exchange for their votes. In terms of their neighborhoods, 66 percent claim that they have witnessed vote-buying taking place where they live. Moreover, of those subjects admitting an acquaintance being offered, 90 percent report some form of acceptance. Vote-buying seems to be attractive for voters.

6 Demographics of Clientelism: The Demand Side

To some degree, our discussion about incumbents versus challengers deals with the supply side of clientelism. Nonetheless, on the other side of the coin, the demand, is also important for the analysis of patronage politics. Receivers of “gifts” during the electoral process, and in many cases, after it, can be classified in many ways. In terms of social

campaign.

¹²Nonetheless, see Blair and Imai (2012) for a discussion on how to conduct a multivariate analysis of traditional list experiments.

status, for instance, it is possible to identify if the rich, the middle, or the poor class tend to be bought more frequently (see Kitschelt and Wilkinson 2007; Stokes 2005; Calvo and Murillo 2004). In terms of gender, to determine if men or women are better targets; ethnic composition is another plausible criteria, leading to the discussion if politicians distribute the pork to their co-ethnics (see Chandra 2004; Fearon 1999)); a final marker that has been studied extensively uses political preferences as the distinguishing trait, with the distinction between core supporters and swing voters as the most frequent aspect analyzed (see Dixit and Londregan 1996; Gallego forthcoming). The purpose of this section is to discuss how the experiments on clientelism and vote-buying have identified the elements that constitute the demand for clientelism.

Gonzalez-Ocantos and his coauthors use their list experiment to determine how prevalent clientelism is among different subgroups. For this purpose, they divide their sample along different categories (income, age, gender, among others) and determine the difference between the mean number of items listed by respondents in the treatment and control groups. This gives an estimate of the proportion of respondents exposed to clientelism within each group. One of the most surprising results of the study is that the poor are not found to be significantly exposed to clientelism. The difference between treatment and control is 0.23, implying that 23% of the poor were exposed to clientelism, but this number is not statistically significant. However, it is important to note that when the authors talk about the “poor”, only 22% of the sample is employed. Naturally, this low N affects the calculation of the standard errors, distorting the inferential analysis. The only socio-economic group where the authors find significance is the middle class, which constitutes 50% of the sample, where 26% is exposed to vote-buying. Hence, given that sub-classification into economic groups significantly reduces the power of the statistical analysis, from this study it is hard to conclude if the poor are more or less exposed to clientelism, as other studies suggest.¹³ Additionally, to determine if the poor are more or less exposed than the middle, it would be necessary to determine if the 23% is significantly different from the 26% (probably not), but again, the small sample sizes greatly affect the analysis.

In terms of education, people without any level show no significant exposure to clientelism, which is another surprising result. But again, the fraction of the sample in this category is low, just a 9.5% of the total sample. For other levels, the differences

¹³The fact that the sample is smaller for the poor than for the middle-income category seems suspicious of how representative it is, especially for a developing country like Nicaragua.

between treatment and control are significant. Surprisingly, the difference increases with the level of education, with 37.4% of the respondents with college education being exposed to clientelism. As with income, these results contradict conventional wisdom, which usually states that the poor and uneducated tend to be the preferred targets of clientelistic politicians. The big problem of this approach is that sub-classification significantly reduces the sample size, making accurate inferential conclusions difficult to achieve. Perhaps the use of techniques like the ones proposed by Blair and Imai (2012) will help us understand more who tends to be targeted.

Gonzalez-Ocantos et al. (2012) also find that people who believe in the secrecy of the ballot show no significant exposure to clientelism, with the opposite result for those who believe it is not secret. In other words, if you believe your vote can be monitored, you are more likely to be targeted. But this result is hard to interpret. Is it the case that you do not believe in the secrecy of the ballot precisely because you were offered something in exchange for your vote, so you think that the politician will guarantee that the deal is enforced? Or, is it the case that politicians have the capacity of identifying, perhaps through brokers, who doubts the secrecy of the ballot and to target those citizens, anticipating that it is easier to enforce the relation with them?

Finally, other socio-demographic results worth noting from this study are that 25.5% of those living in rural areas (significant at a 95% confidence level) and 23.6% living in urban areas (significant at a 90% level) are exposed to clientelism. The difference between treatment and control groups is not significant for men, while the 33.3% difference for female is significant at the 99% confidence level. A 25.1% (38.8%) difference is also significant for those more than 50 years old (between 30 and 49 years), but the difference is not significant for those between 18 and 29 years old. Consequently, from this study we can conclude that females and citizens above the 30-year threshold are attractive targets for politicians in Nicaragua, while no significant difference is found among rural or urban populations. Additionally, it is found that independents and followers of all parties received gifts from politicians. Accordingly, it is not necessarily true that only swing voters are targeted (Nichter 2008).

Finan and Schechter (2011), who use reciprocal preferences to explain why clientelism is self-enforcing, ask themselves if turnout-buying is a reasonable alternative explanation for their findings. Remember that in their research candidates use brokers to target reciprocal citizens, because this type of voter is more likely to comply and establish clientelistic relations. According to Nichter (2008), given that voting preferences are

unobservable but the actual act of voting is not, politicians prefer to buy turnout instead of preference. In such a case, individuals are targeted according to their political preferences. In the vote-buying story (Stokes 2005), weak opposers are bought to make them change their vote and support the clientelistic candidate and not the opposition. In the turnout-buying story (Nichter 2008), supporters are bought to make them turn out on election day and support their preferred candidates. To test these competing mechanisms, Finan and Schechter exploit the fact that the middlemen survey conducted in Paraguay asks brokers if they believe each citizen is a strong supporter of his party.

Their results are not completely conclusive. Using as a dependent variable, whether or not the middlemen's party offered something to each individual, the authors find that parties do not particularly target their strongest supporters. This leads them to conclude that the turnout-buying story is not supported by the data. Nonetheless, this seems to be a fast conclusion. The regression also reveals that weak supporters are more likely to be targeted, so in fact it is hard to conclude that opposers are targeted, which would be a necessary result to infer that the vote-buying story better describes their empirical findings. Hence, we conclude that it is not completely clear from this study, in terms of political preferences, who is being bought. Although to be fair to the authors, both a turnout-buying and a vote-buying story are compatible with their reciprocal-preferences mechanism. *Homo reciprocans* can be the favorite targets of clientelistic politicians, whether they buy them to make them switch their votes or to make them turn out in their favor.

It is natural to infer that the more competitive or closed an election is, the more important swing voters become. Hence, if politicians target swing voters, clientelism should be more intense in closed elections where the final result is determined by a small margin. Vicente (2010) presents some observational evidence —as opposed to the experimental approach followed to answer other questions and described above— in favor of this argument. Using a measure of electoral competitiveness in Sao Tome and Principe,¹⁴ the author finds that in more competitive districts vote-buying is more prevalent and that this supports the swing-voter theory. However, we should be careful before jumping to this conclusion. The author finds that more vote-buying takes place¹⁵ when elections are competitive, but from this result we do not know who is being bought. It could be the case that swing voters are encouraged to support one candidate or the

¹⁴The author uses the absolute value of the difference in vote shares between the incumbent party and the main opposition parties for the 2006 parliamentary election.

¹⁵Or at least, is more perceived to take place.

other as in Vicente's story, but it could also be the case that politicians are buying turnout by targeting their supporters that otherwise would abstain. This alternative explanation would be supported if it is true that in competitive districts turnout is higher.¹⁶

Moreover, vote-buying associated to both the incumbent and the challenger is perceived as more frequent as competitiveness increases. Therefore, it is not possible to conclude that the tightness of the election makes a particular candidate more prone to use vote-buying services.

Additionally, Vicente finds that vote-buying is more frequent in rural locations compared to urban ones. This can be due to the fact that monitoring and enforceability is easier in small rural communities, in contrast to big urban centers where anonymity is greater. These results have to be interpreted with caution, because in most specifications the urban dummy is negative but not significant. Only for vote-buying associated to the challenger are statistically significant effects found.

The relationship between clientelism and gender is important if we want to determine the effect of campaign platforms on electoral results, and as a consequence, the effect of these results on public good provision. For instance, if in a given region or within a specific ethnic group men are more likely to receive job offers, it is likely that women will be less receptive to platforms in which public positions are offered. If the female population is more sensitive to education and health issues for their communities, a message of programmatic policies might have a bigger impact on them than a clientelistic platform. If it is true that men and women have different policy preferences and that these differences might lead to different policy outcomes (see Duflo (2005) or Chattopadhyay and Duflo (2004) for interesting discussions), it would not be surprising that clientelistic and programmatic platforms have a differential effect according to gender. Wantchekon (2003) presents some interesting results on the relation between gender and clientelism.

The author finds, for instance, that compared to a mixed platform, a purely clientelistic or a pure programmatic one has a more positive effect on women compared to men. Thus, for example, in opposition strongholds in which a clientelistic platform is used, 80.6% of the women supported the experimental candidate, compared to 61.6%

¹⁶This is a regularity that in general holds. The identification problem in such case is that if turnout is higher in more competitive locations it is not clear if it is because politicians are buying turnout, or because voters perceive that the probability of being pivotal is higher.

of the men. The 18.6% difference is statistically significant at any level. Nonetheless, the gap is bigger when a programmatic platform is used. In such case, the opposition candidate receives support from 70.7% of the female population, versus a low 32.5% of the male population, implying a significant 38.2% difference between both groups. Clearly, in these districts men are much less receptive than women to public policy proposals. *These results suggest that, at least in opposition strongholds, women have a stronger preference for opposition candidates and these gaps are bigger when a purely programmatic platform is employed.*

The programmatic treatment also leads to positive and significant differences between women and men (i.e. situations in which a higher proportion of females than men prefer the candidate not offering clientelism) in Northern, National, and Incumbent strongholds.¹⁷ Overall, the results from this field experiment support the idea that men and women have different preferences and react differently to the type of platform offered by the candidates. In fact, Wantchekon offers two possibilities for explaining these differences in the case of Benin:

“As for the gender gap result, there are two potential explanations. The first is that because women are excluded from the most common forms of redistribution, they are more responsive to platforms stressing public health or education reforms. The second explanation is occupational choice. Fachchamps and Gabre-Madhin indicate that while men dominate agricultural production, 80 percent of interregional traders in Benin are women. Thus, a significant proportion of rural women travel weekly to other regions of the country and speak several languages. Those women are likely to be better informed about social and economic conditions in the country than male voters and will tend to value broad-based public policy” (pp. 418-419)

Therefore, we can conclude that the experiment is successful in showing that there are gender differences in terms of proneness to clientelism, but the exact reason for why this gap exists remains an open question.

7 Clientelism, Violence, and Electoral Fraud

Clientelism is just one, plausibly very effective, strategy for winning elections. Politicians can use other illegal methods for getting votes, such as fraud and coercion. It is clear

¹⁷No significant effects are found in Local and Southern districts.

that any electoral strategy poses costs and benefits for a candidate. Usually the final goal is to win an election, so the benefits should be judged in terms of the probability of getting into office conditional on using certain strategies. In the case of clientelism, this probability depends on how well enforced is the tacit agreement between the candidate and his clients. In terms of costs, besides the direct payments that a patron makes, in terms of cash, jobs, contracts, etc., we need to consider judicial costs. Here, even though clientelism is illegal in many nations, it is rarely punished and hard to detect.

Other strategies, such as electoral coercion,¹⁸ have a different balance. They might be extremely effective at increasing the probability of victory, but might be very costly in terms of judicial consequences. In many cases the use of violence is considered a much worse crime than simply buying votes. Hence, politicians face a trade off: clientelism reduces punishment costs but has an inherent risk of being ineffective, while coercion maximizes the probability of victory but implies high risks of punishment. Given these considerations, under what circumstances is it better to use one strategy over the other? Are these strategies complements or substitutes? Which one is better for incumbents? Which one for challengers? In this section, we review how the current body of literature has tried to answer this question.

Collier and Vicente (2011) conduct a field experiment in Nigeria during the 2007 national and state-level elections. Although their main interest is to study the effects of violence on electoral behavior, some of the findings of this paper contribute to the understanding of clientelism. In this case, the community campaign is done against electoral violence. Randomization is implemented across neighborhoods and villages in six Nigerian states and half of the selected locations receive a campaign conducted by the NGO ActionAid. Intervention includes “carefully crafted town meetings, popular theater, and the distribution of campaign material, standardized across all locations” (p. 3). The purpose of this intervention is to induce experimental subjects to oppose voter intimidation by reducing the perceived threat to individual voters. A direct message of voting against violent politicians is also employed, although explicit names of candidates are not used.

As in Vicente (2010), a panel survey is conducted in the 24 experimental enumeration areas, with rounds before and after the election. Surveys ask respondents about their perceptions of violence, as well as their political and electoral behavior. Although

¹⁸We define electoral coercion as the (threat of) use of violence and intimidation to force voters to support a candidate or to deter them from voting for somebody else.

it focuses on electoral violence, this study is closely linked to clientelism because incumbents and challengers dispose of several strategies for getting votes, intimidation and vote-buying being two of them. Thus, if the intervention has a clear effect on one of these strategies, via substitutability or complementarity, it also has an effect on the other one.

It is important to note that Collier and Vicente (2010) conduct a randomized intervention in Nigeria similar to the one conducted by Vicente (2010) in Sao Tome and Principe. However, the difference is that in this case the educational campaign conducted in treatment villages is against electoral violence. The authors find that the campaign increases voter turnout by at least 7 percentage points, leading them to conclude that violence suppresses participation.¹⁹ Hence violence, at least in the Nigerian context,²⁰ has a negative effect on turnout, which contrasts with the positive effect that clientelism has. Contrasting between presidential and gubernatorial elections, the authors find that the effect of violence is statistically more significant in the latter case. This suggests that violence and intimidation are used more intensively and are more effective in local contests.

Who uses violence strategically? Collier and Vicente find that for the presidential election, the anti-violence campaign reduces significantly the vote share of one of the weak opposition parties. In gubernatorial elections, it is found that the campaign has a positive effect on incumbent parties. This result permits the author to infer that intimidation is mainly used by weak opposition parties and not by the incumbent. This contrasts with Acemoglu et al. (2010) and Gallego (2011) who show that intimidation by paramilitaries in Colombia benefited parties affiliated to the incumbent.

Is it possible to conclude that challengers rely on violence and vote-buying to win elections, while incumbents prefer fraud and clientelism? This seems to be a fair conclusion from Vicente's research agenda, although apparently there is a contradiction between Vicente (2010) and Collier and Vicente (2010). In the latter, the authors state that:

¹⁹In this type of experiment, an anti-violence (anti-clientelism) campaign has qualitatively opposite effects than violence (clientelism). In other words, the intervention counteracts the effect of the strategy employed by the candidate.

²⁰Other authors also analyze the effect of violence on political behavior. See, for instance, Gallego (2011) for Colombia, Kibris (2011) for Turkey, Wilkinson (2006) for India, and Berrebi and Klor (2008) for Israel.

We argue that ballot fraud is likely to advantage incumbents (...). Vote-buying is also expected to benefit incumbents, as these politicians are expected to have more money available, may be suspected of subverting the secrecy of the ballot through the control of the electoral infrastructure (...) and may be more convincing in proposing to “clients” (p.29).

But this contrasts with Vicente’s (2010) statement that:

[T]he campaign (against vote-buying) caused a shift of votes towards the incumbent and from the challenger, as given by the increase in the incumbent’s score and reduction in the challenger’s share of the vote (...). These findings are consistent with the idea that the challenger relies more strongly on vote-buying as a means of campaigning (p. 18).

What is clear is that we do not have any theoretical or empirical reason to believe that incumbents have no incentives to complement fraud and clientelism with violence and vote-buying. It is true that for incumbents, fraud is feasible if they control the electoral commission and clientelism is likely through jobs and contracts. But cash and weapons are also available, perhaps in larger quantities, for the incumbent.

To determine who employs which method, Collier and Vicente use the competitiveness of Nigerian districts as a proxy for parties’ strength in each location.²¹ It is assumed that when the absolute difference between the vote shares of the incumbent and the opposition parties is low, indicating high competitiveness, the incumbent is challenged and has to use more intensively his preferred strategies in order to win the election. Hence, in their empirical analysis electoral competitiveness is the main independent variable. The dependent variables are measured in a subjective scale from 1 to 4 and correspond to measures of illicit behavior. These variables come from answers to the questions: “Did competition between political parties lead to violent conflict?”, to measure electoral violence; “How free and fair were the elections in terms of counting the votes?”, for fraud; and “Did someone from a political party offer something like food or a gift, in return for votes?”, for vote-buying. Hence, the authors want to determine how intense each of these illicit behaviors are as a function of parties’ relative strength.

Naturally, unobservable variables could determine both electoral competitiveness and illicit behavior. Therefore, an instrumental variable approach is followed. Collier and

²¹Note that in this case the authors exploit some of the observational data produced by the experiment. Nonetheless, this part of the analysis is non-experimental, because it is not possible to randomize the competitiveness of an election across locations.

Vicente use treatment assignment of their intervention—whether a locality received the anti-violence campaign or not—as an exogenous source of variation. Given that the campaign affects citizens’ voting behavior, the instrument (treatment status) is correlated with the plausibly endogenous regressor—competitiveness. For the exclusion restriction to be satisfied in this case, it is necessary to assume that the campaign affects parties’ illicit behavior only through its impact on competitiveness (below, we discuss the plausibility of this assumption). Moreover, randomization of the treatment is not a weak instrument if the anti-violence campaign effectively influences voters’ behavior and electoral results, which seems to be the case given that in treatment locations there was a shift of votes from the challenger to the incumbent.

OLS and IV estimations—with and without individual and location-level controls—reveal that in more competitive places, vote-buying and fraud are perceived as more serious problems. Hence, the stronger the challenger in a location, the more the perceived intensity of illicit behavior associated to the incumbent. The result is quite different for violence. When electoral competition is high, the threat of violence is perceived as low. For the authors, these results support the idea that violence is mainly used by weak opposition parties, while fraud and vote-buying are employed by threatened incumbents.

Can we trust these results? Two caveats should be considered: first, a methodological one. Is the exclusion restriction satisfied in this case? Certainly, it is easy to recognize that the treatment assignment has an effect on competitiveness. But this might not be the only channel from treatment assignment to perceptions of illicit behavior. The fact that in some localities enumerators and members of ActionAid conduct campaigns against violence might alter candidates’ behavior. In treatment locations parties could be more cautious while carrying out fraud, buying votes, or intimidating voters, simply because they perceive that an external agent is present in those places. Hence, perceived illicit behavior would vary across treatment and control locations, not only because of differences in electoral competitiveness but also as a result of parties’ divergent behavior. This would lead to a violation of the exclusion restriction, and consequently, to biased estimates of the effect of competitiveness on perceived illicit behavior.

The second caveat is of interpretation. The fact that vote-buying is perceived as more severe in competitive areas does not necessarily mean that challengers use it exclusively. Perhaps, this result reveals that when elections are competitive *everybody* relies more on clientelism—both incumbents and challengers. The empirical specification proposed by the authors does not allow determination of who is buying votes more intensively,

because respondents are asked in general about vote-buying. Probably, if they had been asked separately about incumbent and challenger vote-buying behavior, it would be possible to determine who uses it more frequently.

Summing up, we conclude that the question of which parties—incumbents or challengers—use which strategies—vote-buying or violence—for winning votes, remains as an unresolved issue. To conclude this section, it is important to note that from the techniques used by Vicente (2010) and Collier and Vicente (2010) it is not possible to determine if treatment—an educational campaign against vote buying or clientelism—is affecting voters’ behavior or candidates’ campaigning. It could be case that the intervention simply indirectly coaches treated subjects on how to answer the surveys. Therefore, readers should interpret the results of these experiments with caution.

8 Overcoming Clientelism

Existing literature on clientelism focuses on structural conditions that give rise to this practice. As such, poverty and income inequality are frequently pointed as the usual suspects. Hence, from this literature it would be fair to conclude that only growth and development will lead nations to the consolidation of political institutions in which programmatic platforms and the efficient provision of public goods prevail over clientelistic methods for winning elections. Is this satisfying? How long should the developing world wait until adequate standards are achieved? Is it always true that developed nations are not victims of patronage, vote-buying, clientelism, or machine politics? Is it naive to think that there might be short and medium-term instruments for overcoming clientelism? The purpose of this section is to review some of the solutions proposed by the experimental literature.

When analyzing Vicente’s (2010) experiment, implicitly we discussed a possible solution for vote-buying: an educational campaign may reduce the perception that vote-buying is driving the results and in fact reduces the amount of transactions witnessed in a given location. This result underscores the important role that information and education have. Nonetheless, the finding that these campaigns mainly affect challengers creates a puzzle. Why are educational campaigns against vote-buying not massively promoted by incumbents, if in the end they would represent a net benefit for them?²²

²²It is true that according to this study incumbents are also negatively affected by anti-vote-buying campaigns. But if in the end the effect on challengers is stronger, the relative benefit for the incumbent

Moreover, given the distinction that Vicente makes between vote-buying and clientelism, which strategy would be feasible for overcoming the latter?

Banerjee et al. (2011) present an interesting and insightful study of the role of information in determining voting behavior and electoral results. As such, it is possible to infer from this paper that information diffusion about candidates serves as an instrument for overcoming clientelism. The authors conduct a field experiment in which access to information about candidates' quality and performance is randomized across slums in Delhi. Taking advantage of the 2008 state legislature election in India's capital city, 200 randomly selected slums receive pamphlets and newspapers containing report cards with information on incumbents' performance across the categories of legislative activity, committee attendance, and public spending. Report cards also inform about criminal records, education, and wealth of the candidates.

This experimental design is used by the authors to determine how information affects voter turnout, electoral results, and vote-buying. The intervention reveals that compared to control slums, turnout increased by two percentage points in treatment locations. Average incumbent vote-shares are not altered by treatment assignment, but the authors find that incumbents that perform worse and that face challengers with higher quality, get less votes. Additionally, cash-based vote-buying, as reported by randomly assigned observers to 61 polling stations, was 19 percent less likely to occur in treatment locations. Perhaps, the paper's main finding is to show in which way information affects voting behavior: citizens do not react to any type of information. Voters use information along dimensions of quality and performance that affect their well-being, and when possible, compare incumbents with candidates of neighboring locations.

In terms for strategies for overcoming clientelism, a very important result of the paper is that cash-related vote-buying is less likely to be observed in treatment locations. The authors interpret this finding as the result of a shift in voters' willingness to sell their votes. But how do we know this is the right interpretation? Instead, it might be the case that candidates rely less on vote-buying in treatment locations because they are aware that certain "outsiders" are monitoring the process.

It could be argued that party campaigning is not significantly altered by the intervention because, as the authors reveal, there is no treatment effect on other relevant variables, such as non-cash gift-giving. But we doubt that the authors have good measures or perfectly observe these other strategies. In fact, dummy variables, indicating should be positive because generally what matters is not turnout, but vote shares.

whether gift-giving is observed or not, are used for this analysis, based on what secret observers report. It is somehow naive to think that gift-giving will completely disappear in treatment locations. The fact that treatment has no significant effect on the gift-giving dummy simply means that this strategy did not completely disappear in treatment locations. Nonetheless, it could be the case that it is used less. We do not know if this is the case because we do not have a continuous measure of gift-giving. The point is that if the intervention affects campaigning (and we have some evidence of that because cash bribing is lower), we will not know if the effect of information on turnout, vote-shares, and clientelism is mediated by changes in the behavior of voters, candidates, or both.

Summing up, from Banerjee et al. (2011) we can conclude that the diffusion of information about candidates matters for overcoming clientelism. However, the mechanism is not absolutely transparent because we do not know if such diffusion alters more voters' or candidates' behavior.

Wantchekon (2011) claims that in patronage systems, a mechanism for counteracting clientelism is to break the link that brokers establish between candidates and voters. Specifically, town hall meetings in which candidates discuss directly with voters about their platforms and make credible promises on public goods and transfers, constitute an alternative for bypassing the fee implied by the usage of brokers for accessing citizens. Frequently, this fee takes the form of grand corruption and inefficient allocation of resources. To defend the argument that town hall meetings can help overcome clientelism, Wantchekon conducts a field experiment during the 2011 presidential election in Benin.

In this experiment a randomized evaluation is conducted in 150 villages selected at random from 30 of the country's 77 districts. In 60 of these villages (treatment) citizens can attend town hall meetings and deliberate about candidates' policy platforms. In the remaining 90 villages, candidates rely on traditional clientelistic strategies, in many cases through rallies organized by local leaders in which cash transfers are frequent. Specifically, in treatment villages a research team organizes two meetings prior to the election in which the agenda is composed by education and health in the first meeting, and rural infrastructure and employment for the second. Villagers are informed and invited to participate in these meetings. During approximately 90 minutes, villagers debate on policy platforms and in the end a written report, given afterwards to candidates, includes citizens' concerns and suggestions. It is important to note that experimental candidates commit not to use intermediaries such as local mayors nor cash transfers in treatment locations. The situation is quite different in control villages. There, the

mayor, MP, or some other political figure, acts as the broker and organizes two to three rallies, in some cases in the presence of the candidate himself. In an environment of festivity and celebration no debates are held and in many cases cash distribution takes place. Hence, Wantchekon seeks to compare the electoral impact of two alternative strategies for obtaining votes: traditional clientelism in which brokers are used to link voters and candidates, versus town hall meetings that serve as a mechanism for making platforms transparent and credible.

To begin with, it is important to note that the experiment exhibits internal validity as balance is achieved in important covariates such as gender, education level, ethnic ties, among others. In order to determine the effects of treatment on political behavior, the author uses both self-reported individual level post-election data, and village-level information collected on election day. At the village level, the author finds that town hall meetings have a positive and significant effect on participation: “turnout was significantly higher in treatment villages than in control villages, despite the fact that villagers were less likely to receive cash and did not directly meet either the candidate or the local broker”.²³ Hence, town hall meetings could be considered an alternative mechanism for mobilizing citizens to turn out and as a substitute for cash transfers. This result is reinforced when using self-reported voting behavior surveys. Naturally, a significant question arises: if the town hall meeting strategy is so effective and as opposed to vote-buying is not illegitimate, why is it not widely applied?

Treatment affects turnout. Does it alter voting behavior as well? Unfortunately, the results are not completely conclusive in this respect. When using village-level data, Wantchekon finds that meetings have no effect on voting preferences overall. Nonetheless, a positive and significant treatment effect is found when analyzing individual-level data. Citizens who participate in town hall meetings are more likely to vote for the experimental candidate.

What is the underlying mechanism behind these results? Why would town hall meetings have a positive effect on voters and become a feasible strategy for overcoming clientelism? To solve this puzzle, the author introduces a useful question in the post-election survey. In particular, respondents are asked how much influence on voting they think the meetings have. Three options are included: 1) They help villagers learn for

²³However, when disaggregating by type of candidate, this result only remains for opposition candidates. Nonetheless, for individual level data, the treatment effect is positive and significant for both incumbent and opposition candidates.

whom will other citizens vote (voter coordination). 2) They help learn more about a candidate's policy agenda (platform transparency). And 3) They show that the candidate is willing to listen to voters (attentive candidate). The author regresses respondents' vote choices in each of these categories and finds that the most plausible mechanism is platform transparency. Citizens that believe that town hall meetings make platforms visible are more likely to vote for the experimental candidate. In general, no significant effects are found for the other mechanisms.

Summing up, in this section we recognized that existing experimental and non-experimental literature on clientelism frequently relies on long-term economic development as the only way for escaping from clientelism. Vicente (2010) claims that an educational campaign against vote-buying is a possible solution to this problem. But it remains unclear why incumbents do not use campaigns of this type more often, specially given the result that challengers are the ones who rely more on vote-buying. Additionally, even though educational campaigns seem to have an effect on vote-buying, it is not clear if they also have an effect on clientelism given Vicente's distinction between both practices.²⁴ Banerjee et al. (2011) suggest that information about candidates' qualities and characteristics diminishes vote-buying. However, it is not clear if their intervention affects voters, candidates, or both. Finally, Wantchekon (2011) offers a plausible strategy for overcoming clientelism: pre-election town hall meetings in which candidates discuss public policies with citizens. They seem to have a positive effect on turnout, making them an alternative mobilization strategy and a plausible substitute for cash transfers and other patronage mechanisms. Apparently, these effects are driven by the fact that meetings make candidates' promises on public goods and transfers both transparent and credible.

9 External Validity of the Experiments

External validity is one of biggest concerns faced by the experimental approach.²⁵ Obviously, the experiments presented above are also subject to the problem of generalizability: how valid are these results if we consider broader populations and different institutional

²⁴This aspect is important because in some cases clientelism could be considered immoral and illegal, which is not necessarily true for vote-buying. Hence, it would be important to determine if educational campaigns suppress both illegal and immoral activities, or just the latter.

²⁵This concern not only exists in the social sciences, but even in other fields in which randomized controlled trials are employed, such as medicine (see Rothwell 2005).

contexts? Would we get the same results if we carry out the same experiments in different settings (Banerjee and Duflo 2009)? As it has been stressed above, experiments based on randomization are strong on internal validity, but often produce results that can be contested on the grounds of external validity (Rodrik 2008). In the words of (Duflo 2006, 27) “[e]ven if the choice of the comparison and treatment groups ensures the internal validity of estimates, any method of evaluation is subject to problems with external validity due to the specific circumstances of implementation. That is, the results may not be able to be generalized to other contexts.” The goal of this section is to discuss this common limitation of experiments, that is also evident in the pieces presented in this survey. We do not want to cover all the experiments conducted so far: we hope that through some examples the reader can become more critical about some of the limitations faced by this approach.

It is natural to ask if the results found by Wantchekon (2003, 2011), Collier and Vicente (2010), and Vicente (2010), in the context of very particular West African countries such as Benin, Nigeria, and Sao Tome and Principe, can be generalized to other regions in which clientelism is widespread, such as Latin America or South East Asia. For instance, one of the main findings of Vicente is that the educational campaign against vote-buying causes a shift of votes from the challenger to the incumbent, suggesting that the former relies much more on this strategy than the latter. The author claims that this is due to the fact that the incumbent has more access to other resources for getting votes, such as jobs or contracts, while the challenger needs to rely on cash. Vicente (2010, p.5) states that Sao Tome and Principe “was constituted as a semi-presidentialist democratic regime, with most executive/legislative powers attributed to the National Assembly, from which the government emerges, but important arbitrage and foreign affairs authority given to the president”. In terms of external validity a natural question arises: in countries in which the president has more or less power (i.e. in presidential or parliamentary regimes), is it still the case that an educational campaign against vote-buying shifts the votes from the challenger to the incumbent? Different institutional settings might alter the resources available for the incumbent for buying votes, leading to divergent directions of change in voting behavior when educational campaigns against vote-buying are conducted.

List experiments are also subject to this problem, because the surveys are conducted in particular countries and under specific economic, social, and institutional settings. Consider, for instance, the evidence provided by Gonzalez-Ocantos et al. (2012). After

dividing the sample along different income categories, the authors find that 26% of the citizens belonging to the middle class are exposed to clientelism. Nonetheless, it is important to recognize that perhaps the entire sample in Nicaragua is poor, in terms of relative degrees of poverty. Therefore, a big critique against extrapolating from this study is external validity. It is much cheaper to buy off the “middle class” in Nicaragua than buy the middle class in a country like Argentina. This does not mean that no lessons can be learned from the study. Perhaps other countries in Central America or even in Africa share some of the socio-economic characteristics of Nicaragua. But the fact that in this analysis the middle class, and not the poor, exhibit significant levels of clientelism does not mean that in other cases the same result holds. It could be the case that in countries in which the middle class is more expensive to buy, politicians prefer to target the poor. Replication of the analysis in other contexts and settings seems to be the right path for determining if the Nicaraguan results can be extrapolated. Of course, the publication bias is a major obstacle, but we think there is still some space for determining, through similar methodologies, how much clientelism takes place along different categories in other countries.

The external validity problem is particularly relevant when we analyze policies or interventions aimed to ameliorate certain pervasive phenomenon that affects a society. As it was described above, Banerjee et al. (2011) show in which way information diffusion might help overcome vote-buying. However, it is surprising that in this paper there is no discussion about the external validity of the experiment. It is clear that the context in which the intervention takes place is very specific. First, because it occurs entirely in an urban setting. What are the effects of information on rural populations, where education levels and access to news tend to be lower? Second, the authors claim that over 20% of Delhi’s population lives in slums. What is the target population of the analysis? Are the participants even representative of Delhi? Finally, the sample of slums is drawn from jurisdictions in which incumbents are likely to stand for reelection. Are the effects of information different in places where incumbents do not run for reelection? Can we extrapolate the results found in this paper to contexts in which reelection is prohibited?

Summing up, external validity is a real threat that the experimental approach to clientelism and vote-buying must face. It is true that in many cases randomization improves the internal validity of the analysis, as it increases the quality of causal identification. Nonetheless, many of these results can be contested on the grounds of extrapolation

and internal validity is not the only thing that matters. We agree with Rodrik (2008, p.21) when he claims that “the typical study based on a randomized field experiment says very little about external validity. If there are some speculations about the background conditions which may have influenced the outcomes and which do or do not exist elsewhere, they are offered in passing and are not central to the flow of the argument”. Hence, we consider that researchers using this methodology could considerably increase the effort devoted to convincing the reader of the external validity of the study.

10 Conclusion

In this paper we analyzed the main experimental studies on clientelism and vote-buying. We argued that experimental techniques represent a method for estimating causal relationships and for solving many of the difficulties that observational studies cannot handle. Through control and randomization, experiments allow researchers to isolate causal effects and to unravel some of the main causes and consequences of clientelism. Additionally, list experiments represent a novel technique for asking sensitive questions and improving our estimation of how much clientelism takes place during an election. All these improvements provided by experimental methods are relevant because clientelism and vote-buying continue to be common strategies employed by politicians to win elections, as revealed by Wantchekon (2003) and Vicente (2010). Nonetheless, it is important to note that even though these studies reveal that clientelism is effective, still little is known about the actual mechanisms employed by candidates to enforce a relation in which defection is tempting for any of the participants in the transaction. To some degree the findings of Finan and Schechter (forthcoming) reveal that social preferences and brokers are fundamental in enforcing clientelistic relationships, but still better designs are needed in order to determine if other mechanisms, such as monitoring, coercion, or tacit cooperation through repetition, also play an important role.

Another fundamental question that the literature seeks to answer is that of who uses which method. Some authors claim that clientelism is favored by incumbents, while vote-buying is advantageous for challengers. In particular, Vicente (2010) defends this argument. But it is not clear if the design of his experiment reveals the underlying mechanism that causes this effect. For instance, if an educational campaign against vote-buying affects challengers more, it might be because voters associate the campaign with the incumbent. Therefore, future interventions have to be careful in isolating the underlying mechanisms that alter citizens' behavior.

In terms of how much clientelism takes place, we can conclude that the range of the proportion of citizens that admit to having been exposed to clientelism is ample. In some cases more than 50% of the population admit to having been exposed. In others, this percentage is much lower. If it is true that field and list experiments provide better techniques for measuring vote-buying and clientelism, it is desirable to employ comparable methods and to establish the determinants of clientelism intensity. Why is the proportion of citizens whose votes are bought higher in some places than others?

Finally, it is important to note that this burgeoning literature that uses experiments to understand clientelism and vote-buying will be useful if it reveals which strategies help nations overcome clientelism. Currently, educational campaigns and deliberation appear to be short-term alternatives. Probably other possibilities exist as well. Hence, randomization of different institutional settings should let us determine the effectiveness of other strategies. For instance, if direct election of local authorities, as opposed to appointment by central governments, has any effect on the role that brokers and intermediaries play, experiments that randomize this variable and then measure its implications on resource allocation and public good provision, might help us identify those institutional arrangements that will help nations overcome clientelism.

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