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# I Sell My Vote, and So What? Incidence, Social Bias, and Correlates of Clientelism in Colombia

**ABSTRACT** Exchanging one's vote for particularistic benefits—practices usually grouped under clientelism—is often thought to weaken programmatic links between citizens and politicians and disincentivize public good provision, as well as undermine voter autonomy and the ideal role of elections. However, empirically analyzing this key phenomenon for the working of democracies entails formidable challenges. We conduct list experiments on a large sample of households to estimate the incidence of clientelistic vote buying, as well as the extent to which respondents refrain from openly recognizing this behavior. Nearly one out of every five respondents engage in clientelism, and, surprisingly, they do not feel ashamed to admit it. Guided by the existing literature and systematically verifying the sensitivity of the results to model specification, we examine the robust correlates of clientelism and discuss the implications of our key findings.

*JEL Codes:* C83, C93, D72, D73

*Keywords:* Clientelism, vote buying, social desirability bias, list experiments

This paper examines the exchange of votes in elections for targeted transfers or benefits—practices usually grouped under clientelism. This phenomenon has fundamental economic and political implications. Clientelistic vote buying is key to understanding the relationship between citizens and the state in a democracy, influences economic policies and outcomes, and shapes incentives for corruption.

The prevalence of these practices is thought to be prejudicial for democracy, as it undermines more programmatic links between citizens and politicians.

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The standard view in political science and economics emphasizes that, with clientelism, politicians focus on providing particularized benefits for powerful minorities rather than public goods for the general welfare.<sup>1</sup> In addition, in a clientelistic exchange, receiving a particularized benefit is contingent on political support, and the benefit can be withdrawn if such support is lacking.<sup>2</sup> This contingency undermines elections as instruments of representation, and since immediate material benefits may be especially pressing for vulnerable voters, it also endangers equality of political rights and traps vulnerable voters in these relationships.<sup>3</sup> Finally, by relying on public funds for the reproduction of the clientelistic network, clientelism can also incentivize arbitrary rules of redistribution and corruption in the public sector.<sup>4</sup>

Despite the importance of this phenomenon, examining it empirically is challenging. Clientelism illustrates one major obstacle to empirical research in the social sciences: the difficulty of eliciting honest answers on surveys, especially with respect to sensitive topics such as discrimination, corruption, and illegality. Respondents may avoid truthful answers when asked, and the nature of these behaviors implies that there are often no other reliable records. Several survey techniques have been developed to address these difficulties. Among them, list experiments have received considerable attention and have been used in a variety of applications, usually as an alternative to randomized response techniques (RRT).<sup>5</sup> This alternative, also known as the item count technique (ICT) or the unmatched count, is based on the premise that “if a sensitive question is asked in an indirect fashion, respondents may be more willing to offer a truthful response even when social norms encourage them to answer the question in a certain way.”<sup>6</sup>

1. Bates (1981); Kitschelt (2000); Stokes (2005, 2007).

2. Stokes (2007); Hicken (2011). As Stokes 2005 puts it, this is a “perverse accountability” system: it is not citizens who punish politicians when they fail to fulfill their promises and programs in office but politicians who may punish citizens for not supporting them in the polls.

3. Stokes and others (2013); Bobonis and others (2017).

4. Stokes and others (2013); Maiz and Requejo (2001); Camacho and Conover (2011).

5. Imai (2011). These applications include discrimination against African Americans and other minority or marginalized groups and attitudes toward food, risky sexual behaviors, and other sensitive or illegal actions. List experiments have also been used to study electoral behavior (González-Ocantos and others 2012; Holbrook and Krosnick 2010; Corstange 2010, 2012), as we do in this paper. They have recently been used in Colombia to study support for certain groups, particularly military and rebel groups (Matanock and García-Sánchez 2011a, 2011b; Steele and Shapiro 2012), as well as clientelism (García-Sánchez and Pantoja 2015).

6. Blair and Imai (2012). A detailed explanation of the method and new developments can be found in Imai (2011) and Blair and Imai (2012). We discuss the details of our own implementation below.

We apply list experiments to a large sample of households to explore the incidence and main correlates of clientelistic vote buying. Our data are drawn from the politics module of the *Encuesta Longitudinal Colombiana de la Universidad de los Andes* (ELCA).<sup>7</sup> The ELCA is the first large-scale household panel survey in Colombia, with roughly 10,000 households that are representative of urban Colombia and five rural regions. The baseline survey was conducted in 2010. In 2013, the first follow-up included the politics module, with a number of questions on political participation and interest, sources of information, ideological positions, and the key sensitive items we focus on. The module was administered to one member of the household (the household head or his or her partner, with random assignment when both were available).

We pursue several objectives. First, we measure the incidence of both vote buying and the social stigma associated with it. Our list experiment, whose underlying assumptions we validate, quantifies vote buying while dealing with the potential biases in citizen responses. To evaluate the associated bias in citizen responses, we (randomly) divide our sample into respondents whose behavior is derived from the list experiments and those who are directly asked about their experience with clientelism. The difference between these two sets of responses measures the extent of social desirability bias and reflects how willing respondents are to admit to engaging in clientelism when asked indirectly as opposed to directly. In our setting, however, there is no difference: both methods suggest that almost one out of five respondents engage in clientelism. The lack of social desirability bias is consistent with vote buying carrying no social stigma in the population. This has important implications for the resulting obstacles to combat vote buying. Moreover, this lack of bias applies overall, as well as across a number of individual characteristics. This is important since, when present, social desirability bias is plausibly nonrandom, and analyses based on direct questions may produce different determinants than those based on list experiments.<sup>8</sup> On a more practical level, it is good news for our empirical analysis, since we can confidently use answers to direct questions to examine clientelistic vote buying.<sup>9</sup>

7. Bernal and others (2014); Fergusson and Riaño (2014).

8. González-Ocantos and others (2012).

9. Multivariate methods based on list experiments are available, but it is more precise to rely on simple regressions of answers to the direct question. However, we also verified that the reported robust correlations with direct questions are in line with those using multivariate techniques for list experiments. Also, since the sample asked directly was randomly selected, and since we observe no differential attrition, we can use this sample to infer the prevalence of general vote buying.

Second, we use the resulting data to review the incidence and main correlates of clientelism, using the existing literature as a guide for our analysis. Third, we conduct a sensitivity analysis that helps us identify the variables that are robustly correlated with clientelism. This crucial step protects us against the *ex post* rationalization and specification search. The results on robust (and nonrobust) correlates inform our understanding of vote buying and have implications for efforts to overcome clientelism, and the comparison of our findings with those reported in the literature also illustrates the richness of the data set. Fourth, we also go beyond the robust determinants to discuss the broader implications on the potential detrimental implications of clientelistic vote buying for economic policy and performance by showing its correlation with municipal-level political features that might shape the quality of representation and policies.

Finally, for all these reasons, our analysis can also be a first step for further analyses of these data. The data are freely available for download from the project's website.<sup>10</sup> The 2013 round contains the list experiment described in this paper, and baseline characteristics are also available for the 2010 panel.

The paper proceeds with a brief examination of the empirical methods. The next section describes the key questions, validates the underlying assumptions, and explains the extreme bounds methodology, a sensitivity analysis approach we implement to assess the robustness of the correlations between clientelism and other variables. We then report the main results on the incidence of clientelism and document the absence of social desirability bias in our survey responses. When presenting our main findings, we use the existing literature as a guide to study the most salient features of the data and describe the robust correlates of clientelism. Finally, we reflect on the relevance of our findings on several dimensions and offer our conclusions.

## Empirical Methods

Our vote-buying list experiment seeks to capture the essential features of a clientelistic exchange where material benefits are traded for political support. Not all benefits can be considered clientelism, however. For example, a party may target a set of policies to benefit a particular ethnic minority in hopes of attracting greater support from this group. Stokes describes such targeted redistribution (or pork-barrel politics, where politicians redistribute benefits

10. See the website <https://encuestalongitudinal.uniandes.edu.co/en/>.

to a particular area, such as a congressional district) as having “some public-good quality: they redistribute resources from classes of non-beneficiaries to classes of beneficiaries, but within a class of beneficiaries, particular people who qualify cannot be excluded.”<sup>11</sup> Thus our question emphasizes the quid pro quo nature of the exchange. Also, support may include voting for the patron or persuading others to do so, mobilizing acquaintances, working for the party’s campaign, and so forth. In our empirical measure, we focus on the specific action of exchanging benefits for one’s vote (vote buying) since it can be measured more accurately. While citizens may interpret giving their political support differently, it is unlikely that this is the case when asked about giving their vote.

As Nichter notes, scholars mean different things when they refer to (clientelist) vote buying, thus confounding different phenomena with potentially different empirical relationships.<sup>12</sup> Two key aspects of the differences in definitions of vote buying are the timing of the benefits (most notably, shortly before or during elections versus after elections) and the types of benefits (cash or other goods and services). Nichter defines clientelist vote buying as “the distribution of rewards to individuals or small groups during elections in contingent exchange for vote choices.”<sup>13</sup> According to this definition, rewards are general in the sense that they could either be cash or other goods and services, but specific in that they must be provided during elections (excluding, for example, post-election benefits, jobs, or public programs). While we concur with Nichter’s broad definition of rewards, we disagree about the timing, as several benefits are delivered or available only after the election, even when support is narrowly defined as voting.<sup>14</sup>

Respondents are randomly assigned to various groups. Households in the treatment group are told, “I will read a list of five (5) things people have in mind when deciding whom to vote for. I want you to tell me how many of these five things you have taken into account when voting for a candidate.

11. Stokes (2007, p. 605).

12. Nichter (2014).

13. Nichter (2014, p. 316).

14. There are other important distinctions. For example, the benefits exchanged may or may not come from public resources (such as public sector jobs, access to government programs, or transfers of public funds). Stokes (2007) suggests referring to *patronage* as a type of clientelism that relies on government funds to deliver benefits. We cannot explore all these distinctions in a survey embedded in an already long and demanding questionnaire for households. We therefore focus on an important and concrete form of clientelistic vote buying, while amply defining the timing and sources of the funds to help capture its presence despite likely variations.

Do not tell me which, only how many.”<sup>15</sup> Then they are handed a card with the following options:

1. The information about the candidate on the radio or television.
2. What you read about his or her government plan.
3. **The benefits, gifts, or jobs the candidate offered you in exchange for your vote.**
4. The conversations you had with your friends about the candidate.
5. The candidate’s party.

In the first control group (Control 1), respondents are confronted with a similar prompt and list, except the sensitive item is not in the list (marked in bold above for emphasis, but not on the list used in the survey).<sup>16</sup>

The premise of the experiment is that when one asks about vote buying indirectly, through the use of a list, individuals are willing to answer truthfully even if social norms suggest that there is a “correct” answer. Since respondents in the treatment group differ from those in Control 1 only in that they are presented with vote buying as an option in the list, the difference in the number of actions reported by the two groups estimates the proportion of individuals that have participated in vote buying.

Following the application of the list experiment, those in Control 1 (who have not seen the sensitive item) are asked directly: “Could you tell me if,

15. In Spanish the script was as follows: “Le leeré una lista de cinco (5) cosas que la gente tiene en cuenta para decidir por quién votar. Quiero que me diga cuántas de estas cinco ha tenido usted en cuenta para votar por un candidato. No me diga cuáles, únicamente cuántas.

1. La información sobre el candidato que usted oyó en la radio o televisión.
2. Lo que usted leyó en el programa de gobierno del candidato.
3. Los beneficios, regalos, o trabajos que el candidato le ofreció a usted a cambio de su voto.
4. Las conversaciones sobre el candidato que usted tuvo con amigos.
5. El partido del candidato.

16. The wording is similar to Corstange (2012) and Çarkoğlu and Aytac (2015), but implements a slightly different strategy. In the control group, these authors ask about each item individually. This has the advantage of more efficient multivariate estimation. However, the risk is that respondents answer differently when asked item by item than when asked about the full list, thus producing a design effect (Corstange 2009; Blair and Imai 2012). We err on the safe side. Also, since the respondent is asked about what factors shaped the decision to support (vote for) a particular candidate, with exchange of vote for benefits as one of the options, the question is unlikely to be interpreted differently by different respondents. Pilot testing followed by debriefing also showed that respondents interpreted the question as intended. Later we show that our vote-buying measure correlates intuitively with electoral practices akin to a very clientelistic pattern of political exchange, further validating our measure.

when deciding whom to vote for, you have taken into account the benefits, gifts, or jobs that a candidate offered in exchange for your vote?" A third group, Control 2, was not presented with the list experiment; respondents in this group were only asked this question directly.<sup>17</sup>

The key assumption that individuals responding to the treatment and control questions are similar must hold in order for our exercises to be valid. While this should be the case by design since the groups were assigned randomly, we used the baseline survey from 2010 to verify balance on a number of observable baseline characteristics. Also, since randomization could fail in the field, we double-checked balance on covariates in the follow-up 2013 survey when the list experiments were conducted. In the online appendix, tables A-2 to A-3 show there are no systematic differences between the treatment and control groups, suggesting that their composition is unlikely to contaminate our results.<sup>18</sup>

Two additional assumptions underlie the ability of the list experiments to faithfully capture the behavior of interest. First, we require that there be no design effects: adding the sensitive item must not affect the response on the sum of control items. Second, we require that there be no sensitive liars: respondents must answer truthfully to the sensitive item.<sup>19</sup>

Formally, let  $j = 1, \dots, J$  control items and one ( $j = J + 1$ ) sensitive item, and let  $Z_{ij}(t)$  indicate  $i$ 's implied answer for item  $j$  under treatment status  $t = 0, 1$  (where  $t = 0$  denotes the control list and  $t = 1$  the treatment list).<sup>20</sup> Also, let  $Y_i(0) = \sum_{j=1}^J Z_{ij}(0)$  and  $Y_i(1) = \sum_{j=1}^{J+1} Z_{ij}(1)$  be the potential answer  $i$  would give under control or treatment list, respectively. Then there are no design effects if:

**Assumption 1. (No design effects)** For each  $i = 1, \dots, N$ ,

$$\sum_{j=1}^J Z_{ij}(0) = \sum_{j=1}^J Z_{ij}(1) \text{ or, equivalently, } Y_i(1) = Y_i(0) + Z_{i,J+1}(1).$$

17. Since we do not find significant differences in responses to the direct question between Control 1 and Control 2 in either experiment, we include both groups throughout when examining the direct questions.

18. In 2010, the ELCA surveyed a total of 9,830 households, and these were randomly allocated to the three treatment groups for the second wave in 2013: 3,535 to *Treatment*, 3,146 to *Control 1*, and 3,149 to *Control 2*. In surveys of households in 2013, the sample fell to 9,234, but attrition was balanced across groups, and, consistent with this, online appendix table A-2 shows the balance on covariates persisted. After further restricting the sample to keep households with complete information on the covariates, we have 7,940 households divided into three groups of 2,856, 2,564, and 2,520 households. Online appendix table A-3 shows these three groups are also balanced on covariates. The online appendixes are available at <http://bit.ly/2QzMEZT>.

19. To spell out the main assumptions for the validity of list experiments, we follow Blair and Imai (2012).

20. Notice also that  $Z_{ij}(t) \in \{0, 1\}$ .



Notice that this does not require individuals to answer truthfully to control items, only that the answer *to the sum of control elements* does not change when respondents are confronted with the treated and control prompts.<sup>21</sup>

The no-liar condition is:

**Assumption 2. (No liar)** For each  $i = 1, \dots, N$ ,

$$Z_{i,J+1}(1) = Z_{i,J+1}^*$$

where  $Z_{i,J+1}^*$  denotes the latent response to the sensitive item in the list.

While the validity of these assumptions is not directly testable, Blair and Imai propose hypothesis-testing procedures that we perform to validate our experiment.<sup>22</sup> The results of these tests and their details are available in online appendix A.3. We fail to reject the null hypothesis of no design effects, as well as of the two most common sources of liars: ceiling and floor effects.

Next, we estimate the incidence of electoral clientelism. When the described assumptions hold, the difference in means between treatment and control groups is an unbiased estimator of the incidence of the sensitive item.<sup>23</sup> This estimator is

$$(1) \quad \hat{\tau} = \frac{1}{N_1} \sum_{i=1}^N T_i Y_i - \frac{1}{N_0} \sum_{i=1}^N (1 - T_i) Y_i,$$

where  $Y_i$  is the answer of individual  $i$ ;  $N_1$  and  $N_0$  are the sizes of treatment and control groups, respectively; and  $T_i$  indicates the treatment status.

Table 1 takes a first broad view of the results for the treatment and control lists. The average number of factors people take into account when voting is 1.59 in the control list and 1.75 in the treatment list. This suggests that about 15.75 percent of people ( $1.75 - 1.59$ ) exchange their vote for benefits, gifts, or jobs. This is our basic unconditional estimate of electoral clientelism.

21. When this fails, the resulting bias may go in different directions depending on the underlying source of difference. One simple example arises with ceiling effects: suppose most people take into account all the things in the control list. When confronted with the control list, the respondent might have no problem answering “J” because all items are nonsensitive. But when confronted with the treatment list, the respondent (trivially if involved in the sensitive behavior, but plausibly even if not) might want to understate the number, fearing it would expose him or her. In this case, we would observe an underreporting of the sensitive behavior.

22. Blair and Imai (2012).

23. Glynn (2013).

**TABLE 1. Response Frequency for the List Experiment and Unconditional Estimator<sup>a</sup>**

Response value	Control group 1		Treatment group	
	Frequency	Proportion (%)	Frequency	Proportion (%)
0	168	6.6	129	4.5
1	1,185	46.2	1,221	42.8
2	874	34.1	980	34.3
3	212	8.3	343	12.0
4	125	4.9	139	4.9
5			44	1.5
Average	1.587 (0.018)		1.746 (0.019)	
No. observations	2,564		2,856	
Difference-in-means estimator			0.1588 (0.0261)	

a. The table lists the frequency in the reported number of factors people take into account when voting. The treatment list includes the same options as the corresponding control list, plus the following sensitive item: “The benefits, gifts, or jobs the candidate offered you in exchange for your vote.” The difference-in-means estimator corresponds to the unpaired difference-in-means estimator given by equation 1. Standard errors are in parentheses.

This simple difference in means can also be computed for subsets of the population to study possible correlates of the sensitive behavior. However, this procedure is not statistically efficient.

Instead, we estimate the following model:

$$(2) \quad Y_i = f(X_i, \gamma) + T_i g(X_i, \delta) + \varepsilon_i.$$

For individual characteristics  $X_i$ , flexible functions  $f$  and  $g$ , and parameters  $\gamma$  and  $\delta$ , the resulting nonlinear least squares estimator is consistent but inefficient because it does not use all the available information about the joint distribution of  $(Y_i(0), Z_{i,J+1}^*)$ .<sup>24</sup> To overcome this limitation, Imai proposes an additional maximum-likelihood estimator that models the joint distribution of  $(Y_i(0), Z_{i,J+1}^*)$  and treats  $Z_{i,J+1}^*$  as (partially) missing data.<sup>25</sup> We use this method in our analysis, and, as Glynn shows, we can consistently estimate the

24. This estimator includes both the difference-in-means estimator when  $f(x, \gamma) = \gamma$  and  $g(x, \delta) = \delta$ , and a linear estimator with interaction terms if  $f(x, \gamma) = x^T \gamma$  and  $g(x, \delta) = x^T \delta$ . The linear specification is easy to interpret, but it does not take into account that the response variables are bounded. Most of these models are estimated assuming logistic regression submodels:  $f(x, \gamma) = \mathbf{E}(Y_i(0)|X_i = x) = J * \text{logit}^{-1}(x^T \gamma)$  and  $g(x, \delta) = \Pr(Z_{i,J+1}^* = 1|X_i = x) = \text{logit}^{-1}(x^T \delta)$ .

25. Imai (2011). Of course, the partially missing data come from the control list that does not disentangle  $Z_{i,J+1}^*$  and  $Z_{i,J}^*$ .

probability that an individual with characteristic  $x$  will engage in a sensitive behavior (that is,  $\Pr(Z_{i,J+1}^* = 1|X = x)$ ).<sup>26</sup>

In addition to estimating the incidence of vote buying and its correlates, we can investigate the extent of the social desirability bias when the experimental design includes the direct question about the sensitive item. As is the case in our application, individuals who are *not* shown the treatment list (since observing the sensitive item on the list may influence them) are asked directly about the sensitive behavior. Comparing the incidence calculated using list experiments with the direct answer measures the average social desirability bias in the population, indicating the extent to which individuals want to conceal their behavior when asked directly. We can also move beyond this to estimate the social desirability bias for different types of individuals, following Blair and Imai.<sup>27</sup> Let  $Z_{i,J+1}(0)$  be  $i$ 's potential answer to the direct question in the control group. Then the social desirability bias for individuals with characteristics  $X_i = x$  may be defined as

$$(3) \quad S(x) = \Pr(Z_{i,J+1}^* = 1|X_i = x) - \Pr(Z_{i,J+1}(0) = 1|X_i = x),$$

for any  $x \in X$ . As long as the no-design and no-liar assumptions hold, the first term can be estimated using the multivariate techniques discussed above. The second can be estimated directly with a regression of  $Z_{i,J+1}(0)$  on observables (using logit, for instance).

### *Uncovering Robust Correlates of Clientelism*

To systematically establish which variables are most robustly correlated with clientelism and avoid the risk of data mining, we follow two parallel paths. First, we select our list of covariates following the theoretical and empirical literature on clientelism and vote buying. Second, even with a judicious choice of covariates, there is scope for conscious or unconscious specification search leading to different, but actually not robust, conclusions. We use the extreme-bounds methodology.<sup>28</sup> This exercise allows us to succinctly uncover the robust correlates of vote buying, without focusing attention on a subset of

26. Glynn (2013).

27. Blair and Imai (2012).

28. Leamer (1985); Levine and Renelt (1992); Sala-i-Martin (1997). This method has been widely used to study the correlates of economic growth (Reed 2009) and other topics (Hafner-Burton 2005; Sturm and de Haan 2005; Wang 2010; Gassebner, Lamla, and Vreeland 2013).

potential models that fall in line with our priors. However, since this method does not provide a basis for causal interpretation, we emphasize all our findings are mere correlations.<sup>29</sup>

Consider the following regression for outcome  $y_i$  for individual  $i$ :

$$y_i = \alpha_j + \beta_j c_i + \gamma_j x_{ij} + \theta_j f_i + \varepsilon_{ij},$$

where  $c_i$  is a correlate of interest and  $f_i$  is a set of controls that is always included in the regressions. We include in  $f_i$  a full set of region fixed effects (nine in total) that account for the potential heterogeneity in the presence of clientelism and beliefs about democracy across regions in Colombia, and we include the lower direct terms when examining interaction terms for covariates in  $c_i$ .<sup>30</sup> In our application,  $y_i$  is the answer to the *direct* question on clientelism (taking advantage of the lack of social desirability bias reported below). Thus  $y_i$  is a dummy variable (which indicates whether the respondent accepts benefits, gifts, or jobs in exchange for his or her vote). Finally,  $X$  is a pool of all correlates different from  $c_i$  and  $f_i$ , and  $\mathbf{x}_{ij}$  is a vector of up to three variables in  $X$ .<sup>31</sup>

The method proceeds by estimating this regression for all possible  $\mathbf{x}_{ij}$  (that is, for all  $j$ ), and obtaining  $\beta_j$  and its standard error  $\sigma_j$ . In Leamer's original formulation, the lower extreme bound is simply the lowest value of  $\beta_j - \tau\sigma_j$ , and the upper extreme bound is the largest value of  $\beta_j + \tau\sigma_j$ , with  $\tau$  the critical value for the confidence level.<sup>32</sup> In this approach,  $c_i$  is considered a robust correlate of  $y_i$  when the lower and upper bounds have the same sign. This criterion may be overly conservative, potentially declaring a correlation fragile on the basis of a single model.<sup>33</sup>

Sala-i-Martin instead proposes analyzing the entire distribution of  $\beta_j$  and finding the cumulative density function to the left and right from zero.<sup>34</sup> The

29. Angrist and Pischke (2010).

30. The nine regions are Atlántica, Atlántica Media, Bogotá, Central, Centro-Oriente, Cundi-Boyacense, Eje Cafetero, Oriental, and Pacífica. See Leal Buitrago and Dávila Ladrón de Guevara (2010).

31. The number of variables  $x_{ij}$  to be included is up to the researcher. However, the limit of three variables has been a convention in the literature. See, for instance, Levine and Renelt (1992) and Achen (2005).

32. Leamer (1985).

33. This turns out to be the case for most correlates of cross-country growth. See Sala-i-Martin (1997) and Levine and Renelt (1992).

34. Sala-i-Martin (1997).

largest of these two,  $CDF(0)$ , is the proportion of interest because it indicates where the coefficient is concentrated. Assuming normality, the densities can be recovered from the mean ( $\beta$ ) and standard deviation ( $\sigma$ ) of the distribution. These can be directly computed using the estimated  $\beta_j$  and  $\sigma_j$  as

$$\beta = \sum_j \phi_j \beta_j \text{ and } \sigma = \sum_j \phi_j \sigma_j,$$

where  $\phi_j$  are weights proportional to some goodness-of-fit measure, like the adjusted  $R^2$  or the integrated likelihood. However, with endogenous covariates the unweighted version may be preferable since endogenous regressions will have a better fit. The normality assumption can also be relaxed, computing  $CDF(0)_j$  for each regression and only then finding the (weighted) average  $CDF(0)$ .<sup>35</sup> In this approach, variables that appear to be significantly correlated with the outcome are those with a (weighted)  $CDF(0)$  larger than 0.95, or another benchmark confidence level.

In our exercise, we report Leamer's extreme bandwidth, the average (weighted and unweighted) parameters, and the cumulative density (both assuming and relaxing normality).<sup>36</sup>

## Incidence of Vote Buying and (No) Social Desirability Bias

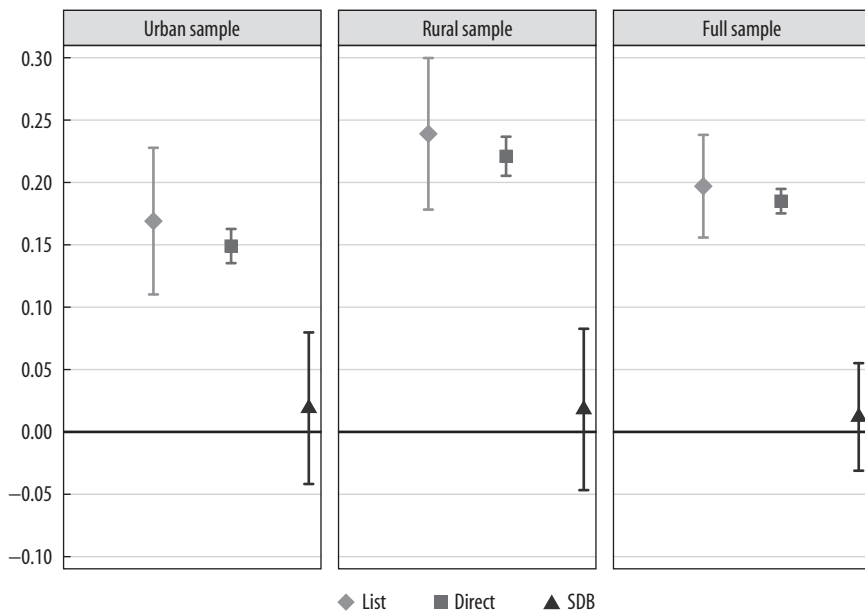
Figure 1 plots the incidence of clientelism and provides evidence of the absence of social desirability bias. The point estimate for the prevalence of clientelism in the full sample is 18.5 percent (with a standard error of 0.005). This estimate differs from the one obtained by the simple difference-in-means estimator of 15.75 percent because it employs a different, and superior, estimation procedure. In particular, here we are using the maximum likelihood estimator of equation 2, exploiting the joint distribution of  $(Y_i(0), Z_{i,j+1}^*)$  and the full set of characteristics ( $X_i$ ) listed in figure 2 as controls.<sup>37</sup>

35. Some variations in the approach include weighting parameters with other measures of goodness of fit (Sala-i-Martin, Doppelhofer, and Miller 2004; Hegre and Sambanis 2006) and using other types of regression models such as logit or ordered probits (Bjørnskov, Dreher, and Fischer 2008; Moser and Sturm 2011; Gassebner, Lamia, and Vreeland 2013). Computational improvements are discussed in Hlavac (2016).

36. Our results are similar when weighting with the likelihood or adjusted  $R^2$  (we report the latter), or when running probit instead of linear models.

37. We experimented with alternative sets of controls, and the average incidence is not sensitive to these changes. Table A-1 in the online appendix (<http://bit.ly/2QzMEZT>) describes all variables used in the analysis.

**FIGURE 1. Incidence and Social Desirability Bias of Clientelism<sup>a</sup>**

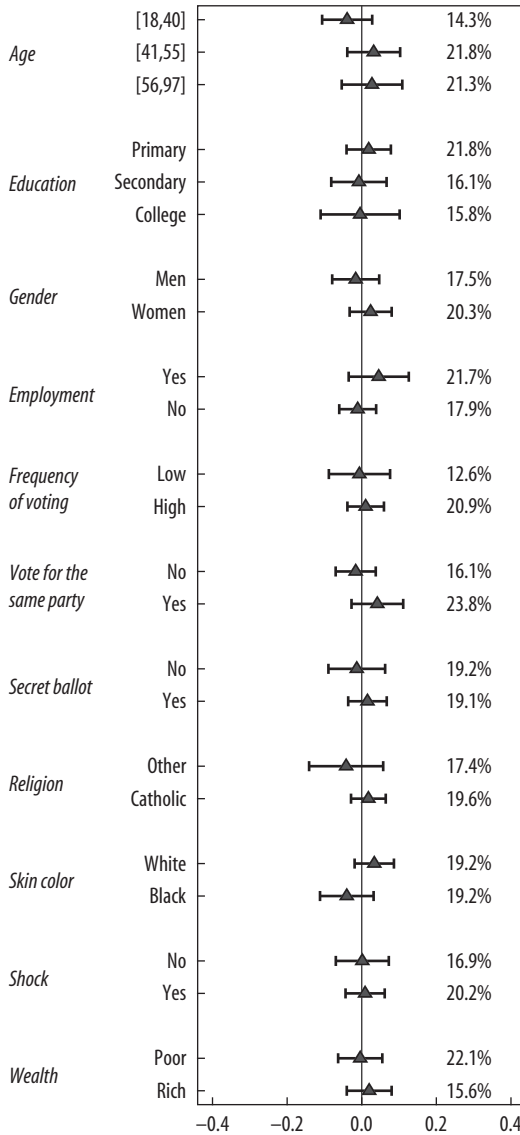


a. Incidence of clientelism as implied by the list experiment (diamond), direct question (square), and the difference between these two measures, capturing the extent of social desirability bias (triangle). Lines mark the 95 percent confidence bounds. Estimates in this figure control for the set of individual characteristics listed in figure 2. The list experiment sample in urban and rural areas is 2,683 and 2,737 households, respectively. The direct question sample includes 2,537 urban households and 2,554 rural households.

Since rural and urban areas are very different, we show estimators separately as well. The estimated incidence is lower in urban areas (when the question is asked directly, 14.9 percent, with a standard error of 0.007) than in rural areas (22.1 percent, with a standard error of 0.008). Using the list experiment, the conclusions and corresponding numbers are very similar (though the estimation precision is lower, as expected). Consequently, the point estimate for the social desirability bias is very small, less than two percentage points, and not significant in either area.

Both the level of clientelism and the absence of social desirability bias contrast with other findings in the literature. For example, Corstange finds that approximately one-fourth of Lebanese voters admitted to selling their vote when asked directly, but inferring from the list experiment suggests that nearly 50 percent had done so, which produced a large social desirability bias

**FIGURE 2. Incidence and Social Desirability Bias of Clientelism across Different Covariates<sup>a</sup>**



a. This figure shows the social desirability bias estimate (and its 95 percent confidence interval) across different individual characteristics, following equation 3. Percentage values on the right represent the estimated incidence of clientelism using the list experiments (all point estimates are significant at the 95 percent confidence level). Standard errors are computed using Monte Carlo simulations, and estimations control for the set of variables listed in the table. See table A-1 in the online appendix for a description of all variables (<http://bit.ly/2QzMEZT>). Online table A-7 reports the point estimates for this figure and the results for urban and rural areas.

of 25 percentage points (twice the direct answer).<sup>38</sup> Moreover, the willingness to admit to this behavior varies across religious beliefs.<sup>39</sup> In contrast, we find that people are just as willing to admit to clientelism when asked directly as when presented with a list, and that the lack of social desirability holds not just on average but for different types of respondents. In figure 2 we illustrate this by following the approach described in equation 3 to evaluate the possible correlates of social desirability bias. Across all respondent types we find no social desirability bias.

We conclude that most types of people are willing to openly admit the extent to which they exchange votes for favors. We find similar results when exploring other possible individual characteristics. One possible explanation is that respondents in our survey are comfortable enough with survey officers, having been visited by the organization three years earlier for the baseline survey and answering a long questionnaire, to provide honest answers. While little evidence is available, it appears more likely that this is not simply a feature of our data but rather a reflection of broad acceptance of the practice of vote buying in Colombia. For instance, García-Sánchez and Pantoja, in the 2015 Colombian chapter of the Latin American Public Opinion Project (LAPOP), use list experiments to test for vote buying, positive (and negative) patronage—promising to give (or threatening to remove, respectively) benefits contingent on electoral support—and direct intimidation.<sup>40</sup> Their estimates differ from ours: they find no significant evidence of vote buying or negative patronage in these areas, though the incidence of intimidation and positive patronage is estimated at approximately 7 percent of voters in both cases. Yet like us, they find no social desirability bias. Using their data, figure 3 shows estimates that, though much noisier, given their smaller sample, reveal no evidence of social desirability bias.

The difference in incidence, however, may reflect the very specific sample used in the LAPOP study. Indeed, the LAPOP survey also asked about electoral clientelism using a direct question in the full sample and found numbers similar to ours: around 15 percent reported having at least one experience of electoral

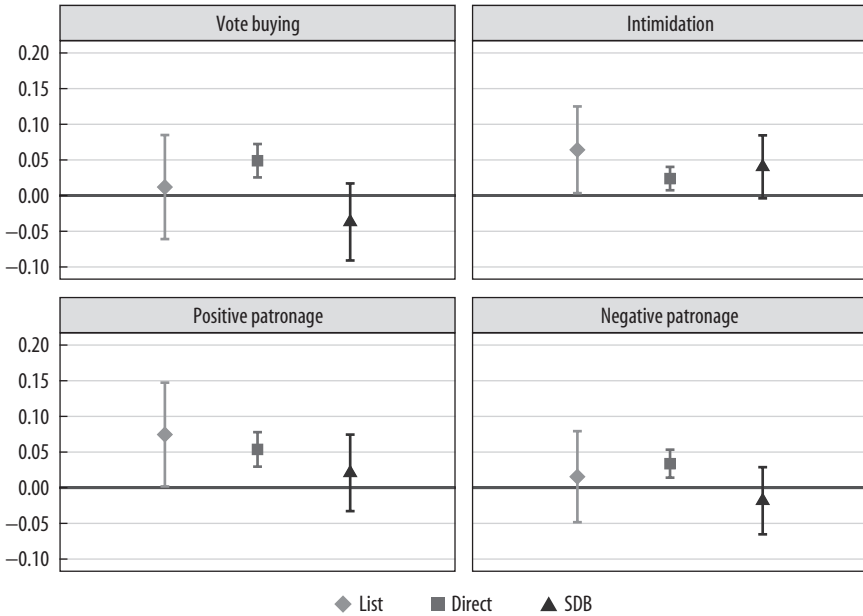
38. Corstange (2012). In Çarkoğlu and Aytaç (2015), asking the question directly suggested an estimated 16 percent prevalence of vote buying, which increased to 35 percent using the list. González-Ocantos and others (2012) conducted a similar experiment in Nicaragua and found that 24 percent of registered voters (compared to 2 percent of those asked directly) were *offered* a chance to sell their vote (a wording that may fail to capture effective vote-buying transactions).

39. The Sunni are most willing to openly acknowledge engaging in this practice, followed by Christians and then Shia.

40. García-Sánchez and Pantoja (2015).



**FIGURE 3 . Incidence and Social Desirability Bias in LAPOP Sample<sup>a</sup>**



a. For each electoral practice, the figure shows the incidence as implied by the list experiment (diamond), direct question (square), and the difference between these two measures, capturing the extent of social desirability bias (SDB, triangle). Lines mark the 95 percent confidence bounds. Data from LAPOP-Colombia, 2015 (García, Montalvo, and Seligson, 2015). Sample size in the list experiment and the direct question is, respectively, as follows: 650 and 327 individuals for vote buying; 653 and 335 individuals for intimidation; 649 and 335 individuals for positive patronage; and 648 and 326 individuals for negative patronage.

clientelism.<sup>41</sup> The similarity of these numbers again suggests that citizens are openly willing to admit the extent of electoral clientelism, a feature that may reflect the nature of a political equilibrium in which clientelism and a weak state reinforce each other as normal features of the political landscape.<sup>42</sup> This hypothesis is also consistent with citizens being willing to report the value-added tax.<sup>43</sup>

41. Çarkoğlu and Aytaç (2015). The LAPOP question asked about *offers* rather than *exchanges*: “And during the last election in YEAR, how often, if ever, did a candidate or someone from a political party offer you something, like food or a gift or money, in return for your vote?”

42. Fergusson, Molina, and Robinson (2017).

43. Fergusson, Molina, and Riaño (2017).

Finally, though vote buying is very prevalent in our sample and not far from that in LAPOP, the figure is smaller than in the papers using list experiments in other countries. Apart from higher real incidence, the distribution of abstention in the samples may play a role. Indeed, if one out of five people have sold their vote but only three out of five people actually turn out to vote, the impact of vote buying almost doubles. Using a self-reported turnout question for the local elections of 2010 and a question on how frequently the respondent participates in elections, we find that voters do engage in more vote buying than nonvoters. Nonvoters still report vote buying, but of course this may be due to their behavior in other elections in which they did turn out. In both cases, vote buying is close to 20 percent for frequent voters versus 10–14 percent for infrequent voters.

## Correlates of Vote Buying

In this section we present the main correlates of clientelism, implementing the methodology described earlier.<sup>44</sup> Our inclusion of relevant variables was guided by a review of the most relevant literature, but in the text below we discuss the literature just briefly and only in connection with the most salient results. Our focus is on the most important debates on possible determinants and on those for which we have particularly useful empirical measures in our data set.

Table 2 reports descriptive statistics. Definitions and sources for all variables are in table A-1 in the online appendix (<http://bit.ly/2QzMEZT>). To facilitate identifying the magnitude of the correlations, we standardize all variables in the regression analysis. In table 3 we report the results of the extreme-bounds methodology analysis when applied to our direct question on clientelism (exchange of gifts, jobs, or benefits). Table 4 shows the results of a similar analysis for interaction terms (where the lower-order direct effects are always included in the regression). To easily identify the most important correlates, in these tables variables are sorted from most to least robustly (or significantly) correlated with clientelism.<sup>45</sup>

44. Online appendix A.4 also reports simple alternative bivariate and multivariate regressions, which produce similar conclusions.

45. We need to run  $\binom{41}{3} + \binom{41}{2} + \binom{41}{1} = 11,521$  regressions to assess the robustness of each estimate.

**TABLE 2. Summary Statistics<sup>a</sup>**

<i>Variable</i>	<i>No. obs.</i>	<i>Mean</i>	<i>Median</i>	<i>Std. dev.</i>	<i>Min</i>	<i>Max</i>
<i>A. Dependent variable</i>						
Clientelism	4,962	0.183	0.000	0.387	0.000	1.000
<i>B. Covariates</i>						
Age	4,962	46.641	46.000	12.464	19.000	97.000
Agree with bribery	4,962	0.137	0.000	0.344	0.000	1.000
Authorities violate law	4,962	0.337	0.000	0.473	0.000	1.000
Catholic	4,962	0.820	1.000	0.384	0.000	1.000
Education	4,962	0.457	0.000	0.498	0.000	1.000
Evangelical/Pentecostal	4,962	0.126	0.000	0.331	0.000	1.000
Fractionalization	4,962	0.621	0.641	0.102	0.448	0.834
Gini of land properties	4,962	0.661	0.693	0.132	0.404	0.921
Gov. against inequality	4,962	0.936	1.000	0.244	0.000	1.000
Government role	4,962	0.885	1.000	0.320	0.000	1.000
Guerrillas	4,962	0.208	0.000	1.551	0.000	21.085
Household expenses	4,962	0.092	0.000	0.153	0.000	1.000
Homicide rate	4,962	26.284	18.349	25.282	0.000	163.159
Independent	4,962	0.151	0.000	0.358	0.000	1.000
Justice into own hands	4,962	0.294	0.000	0.456	0.000	1.000
Lands	4,962	0.034	0.000	0.182	0.000	1.000
Left ideology	4,962	0.055	0.000	0.228	0.000	1.000
Left dominated	4,962	0.087	0.000	0.209	0.000	0.750
Negative reciprocity	4,962	0.189	0.000	0.392	0.000	1.000
Neighbor cell phones	4,962	0.131	0.000	0.337	0.000	1.000
Neighbor loans	4,962	0.199	0.000	0.400	0.000	1.000
No left-right contender	4,962	0.391	0.000	0.488	0.000	1.000
Other religion	4,962	0.021	0.000	0.144	0.000	1.000
Own welfare	4,962	0.949	1.000	0.220	0.000	1.000
Paramilitaries	4,962	1.440	0.000	7.261	0.000	68.367
Party identity	4,962	0.163	0.000	0.370	0.000	1.000
Party recall	4,962	0.468	0.000	0.499	0.000	1.000
Persuasion	4,962	0.162	0.000	0.369	0.000	1.000
Polarization	4,962	0.833	0.858	0.090	0.537	1.000
Population density	4,962	0.001	0.000	0.002	0.000	0.013
Popular vote	4,962	0.955	1.000	0.206	0.000	1.000
Positive reciprocity	4,962	0.971	1.000	0.167	0.000	1.000
Right ideology	4,962	0.264	0.000	0.441	0.000	1.000
Right dominated	4,962	0.109	0.000	0.140	0.000	0.750
Rural population	4,962	0.356	0.416	0.275	0.002	0.940
Secret ballot	4,962	0.701	1.000	0.458	0.000	1.000
Shock	4,962	0.680	1.000	0.467	0.000	1.000
State presence	4,962	-0.060	0.564	1.880	-7.305	3.126
Use of violence	4,962	0.193	0.000	0.395	0.000	1.000
Wealth	4,962	-0.288	-0.719	2.650	-5.296	5.887
Win margin	4,962	0.160	0.131	0.113	0.001	0.423
Woman	4,962	0.575	1.000	0.494	0.000	1.000

a. Clientelism is a dummy variable that equals one if the respondent has taken into account the benefits, gifts, or jobs that a candidate offered in exchange for his vote and zero otherwise. Covariates and their sources are described in table A-1 in the online appendix. The sample of respondents is the set of individuals in Control group 1 and Control group 2 as described in the text, namely, those asked directly about clientelism.

**TABLE 3 . Robust Correlates of Clientelism<sup>a</sup>**

Variable	Leamer's		Coefficient		Standard error		CDF(0)	
	Lower	Upper	Weighted	Unweighted	Weighted	Unweighted	Weighted*	Unweighted
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(9)
1. Persuasion	0.076	0.152	0.116	0.116	0.015	0.015	1.000	1.000
2. Agree with bribery	0.044	0.114	0.081	0.082	0.015	0.015	1.000	1.000
3. Other religion	-0.085	-0.017	-0.047	-0.047	0.009	0.009	1.000	1.000
4. Negative reciprocity	0.023	0.097	0.064	0.064	0.014	0.014	1.000	1.000
5. Party recall	0.018	0.100	0.063	0.063	0.013	0.013	1.000	1.000
6. Wealth	-0.157	-0.027	-0.085	-0.085	0.021	0.021	1.000	1.000
7. Positive reciprocity	0.005	0.050	0.029	0.029	0.010	0.010	0.999	0.999
8. Right ideology	-0.010	0.070	0.038	0.038	0.014	0.014	0.993	0.996
9. Justice into own hands	-0.018	0.066	0.034	0.034	0.013	0.013	0.991	0.996
10. Age	-0.064	0.009	-0.028	-0.028	0.013	0.013	0.983	0.984
11. Secret ballot	-0.058	0.006	-0.027	-0.027	0.013	0.013	0.978	0.979
12. Use of violence	-0.022	0.061	0.030	0.030	0.014	0.014	0.978	0.986
13. No left-right contender	-0.008	0.076	0.032	0.032	0.016	0.016	0.974	0.976
14. Party identity	-0.037	0.065	0.025	0.025	0.015	0.015	0.930	0.953
15. Authorities violate law	-0.027	0.051	0.020	0.020	0.014	0.014	0.913	0.926
16. Win margin	-0.141	0.029	-0.020	-0.020	0.016	0.016	0.900	0.903
17. Polarization	-0.235	0.050	-0.025	-0.025	0.021	0.021	0.882	0.884
18. Evangelical/Pentecostal	-0.065	0.091	0.015	0.015	0.013	0.013	0.866	0.882
19. HH expenses	-0.058	0.037	-0.018	-0.018	0.017	0.017	0.847	0.854
20. Pop. density	-0.090	0.051	-0.020	-0.020	0.020	0.020	0.836	0.848
21. Fractionalization	-0.229	0.070	0.016	0.016	0.018	0.018	0.813	0.804
22. Shock	-0.025	0.043	0.013	0.013	0.014	0.014	0.812	0.815
23. Rural population	-0.118	0.039	-0.021	-0.021	0.024	0.024	0.793	0.809
24. Homicide rate	-0.040	0.066	0.015	0.015	0.019	0.019	0.792	0.796
25. Neighbor cell phones	-0.029	0.046	0.011	0.011	0.014	0.014	0.774	0.781

(continued)

**TABLE 3 . Robust Correlates of Clientelism\* (Continued)**

Variable	Leamer's		Coefficient		Standard error		CDF(0)	
	Lower (1)	Upper (2)	Weighted (3)	Unweighted (4)	Weighted (5)	Unweighted (6)	Weighted* (7)	Unweighted (9)
26. Independent	-0.026	0.041	0.010	0.010	0.014	0.014	0.747	0.750
27. Neighbor loans	-0.038	0.059	0.012	0.012	0.019	0.019	0.734	0.739
28. Right dominated	-0.029	0.051	0.008	0.008	0.014	0.014	0.709	0.715
29. Left dominated	-0.099	0.092	-0.018	-0.018	0.035	0.035	0.695	0.696
30. State presence	-0.049	0.035	-0.007	-0.007	0.016	0.016	0.677	0.680
31. Education	-0.044	0.054	0.005	0.005	0.013	0.013	0.665	0.648
32. Guerrillas	-0.043	0.033	0.003	0.003	0.010	0.010	0.663	0.613
33. Own welfare	-0.033	0.027	-0.005	-0.005	0.013	0.013	0.643	0.645
34. Gov. against inequality	-0.035	0.026	-0.005	-0.005	0.014	0.014	0.629	0.629
35. Government role	-0.030	0.038	0.005	0.005	0.015	0.015	0.624	0.625
36. Popular vote	-0.039	0.031	-0.005	-0.005	0.016	0.016	0.620	0.621
37. Catholic	-0.090	0.089	0.003	0.003	0.014	0.014	0.620	0.598
38. Lands	-0.041	0.033	-0.004	-0.004	0.015	0.015	0.615	0.610
39. Paramilitaries	-0.033	0.029	0.002	0.002	0.011	0.011	0.593	0.561
40. Left ideology	-0.044	0.034	-0.002	-0.002	0.013	0.013	0.583	0.559
41. Woman	-0.036	0.044	0.000	0.000	0.015	0.015	0.562	0.503
42. Gini of land properties	-0.057	0.044	0.000	0.000	0.017	0.017	0.555	0.509

a. The table reports statistics based on the extreme-bounds methodology described in the text. The dependent variable is a dummy variable indicating whether, when deciding for whom to vote, the respondent has taken into account the benefits, gifts, or jobs that a candidate offered in exchange for the vote. The independent variable of interest is indicated in each row. In each case, a sensitivity analysis is conducted by also including all possible permutations of up to three of the additional covariates listed in the table (that is,  $\binom{41}{3} + \binom{41}{2} + \binom{41}{1} = 11,521$  regressions per covariate). Region fixed effects are always included, and standard errors are clustered at the community level. Lower and Upper are Leamer's lower and upper extreme bounds for the coefficient of interest (that is, for the resulting coefficient of the variable listed in each row) at the 95 percent confidence level. The adjusted  $R^2$  is used to weight statistics where indicated. The  $CDF(0)$  measures the largest accumulated density of the resulting coefficients of interest to the left or right of zero (whichever is largest). Column 7 reports the weighted  $CDF(0)$  assuming non-normality, while column 8 (column 9) reports the weighted (unweighted)  $CDF(0)$  assuming normality. See table A-1 in the online appendix for a description of all variables (<http://bit.ly/2QzMEZT>).

### *Individual Characteristics*

A key focus of the literature is what types of individual are more likely to be targeted with vote buying. A higher monetary value is more likely to produce an exchange for poorer voters or those for whom the marginal benefit of a given reward is higher, for instance because they are in a situation of distress.

We use rich data on household assets to construct a wealth index and use questions on expenditure and on the incidence of negative shocks for the household to test whether poorer people and those in distress sell their votes more often. Table 3 suggests that wealthier households (row 6) are indeed significantly less likely to sell their votes; 100 percent of the coefficients are concentrated on the negatives (the value of  $CDF(0)$  is one regardless of the precise assumptions used). Those experiencing negative shocks (row 22) are not more susceptible to clientelism at conventional levels (though a nonnegligible 81 percent of the coefficients are greater than zero), nor are those with higher levels of expenditure per capita (row 19).

Individuals who believe more strongly in the rule of law and democracy should also presumably be less willing to compromise their ideals and sell their vote. We rely on five questions on the degree to which respondents agree with the following statements: it is important that rulers are elected by popular vote (not significant in row 36 of table 3); it is sometimes justified to pay a bribe (positive and significant in row 2 of table 3); when the government does not punish criminals, people can take justice into their own hands (positive and significant in row 9); sometimes the use of violence is justified (positive and significant in row 12); and to capture criminals, authorities should sometimes violate the law (positive and marginally significant in row 15). Surprisingly, the most direct measure of belief in electoral democracy (“it is important that rulers are elected by popular vote”) does not appear to robustly correlate with clientelism. However, the next four variables, which capture different dimensions of tolerance of antisocial or illegal behavior, always point in the same direction and are positively correlated.<sup>46</sup>

We also analyze questions on political knowledge, political engagement, and partisan affinity to explore the idea that those who are not interested in politics value their vote less and can be easily swayed (that is, they have

46. We also include variables that capture individuals’ beliefs about the role that the government (as opposed to the people) plays in determining individual welfare, labeled “Government role” (row 35) and “Own welfare” (row 33), and about how actively the state should combat inequality (row 34). None of these variables are robustly correlated with clientelism.

low reservation prices, just as the poor might).<sup>47</sup> But party recall is positive and significant in row 5 of table 3, party identity is positive and significant in row 14, and persuasion is positive and significant in row 1.<sup>48</sup>

### *Features Facilitating Enforcement*

Vote buying is subject to severe commitment problems, as voters may renege and vote for an opposing candidate. Parties may also renege by failing to deliver promised benefits in the future.<sup>49</sup> Many of these problems are solved if there is no secret ballot and politicians can effectively monitor vote choice. Conversely, the more opaque and anonymous voting is, the less likely vote buying is to occur. Some papers provide evidence that innovations like secret ballots or electronic voting prevent clientelism.<sup>50</sup>

However, clientelism also persists with the use of secret ballots.<sup>51</sup> One possibility is that, where the ballot is effectively secret and monitoring is difficult or impossible, a feeling of reciprocal obligation may sustain clientelism.<sup>52</sup> Reciprocity not only overcomes commitment problems, but politicians may also prefer reciprocal voters because they can better predict how they will vote and because reciprocal voters may be better at maintaining the long-term relationship of mutual cooperation often associated with clientelism.

Empirically, this motivates examining the connection between vote buying and available measures of positive reciprocity (agreement with the statement

47. Corstange (2010).

48. Other individual covariates included in the analysis are age, which correlates negatively with clientelism (row 10), and gender—women show no differential behavior (row 41). Religion is broken down into the two largest religious groups, Catholicism and Protestantism (rows 37 and 18) (see table 2), and other religious minorities as a whole (row 3); only the latter group robustly reveals less clientelism (compared to atheists or agnostics, the excluded category). We are unable to explore a few potentially relevant variables owing to lack of data. In particular, more risk-averse individuals may prefer a possibly more certain, targeted clientelistic exchange over an uncertain programmatic exchange. To the extent that clientelistic benefits accrue sooner, more impatient individuals may also be more willing to sell their votes.

49. Robinson and Verdier (2013) propose that clientelism often takes the form of (inefficient) public employment because it helps overcome the two-sided credibility problem.

50. For example, Lehoucq and Molina (2002); Fujiwara (2015).

51. The insertion of political machines into dense voters' social networks, another potential solution to the enforcement problem (Stokes 2005; Stokes and others 2013), does not appear to be significant in our regressions, at least as captured by variables such as the number of neighbors for whom respondents have a cell phone number or the number of neighbors from whom they can request a loan, or by comparing rural and urban areas, since social connections tend to be thinner in the latter.

52. Lawson and Greene (2014); Finan and Schechter (2012).

“you should always help those who help you”) and negative reciprocity (agreement with the statement “whoever harms me, pays for it”). In table 3 the expected positive connection is quite clear and robust (rows 4 and 7).

We also directly asked about belief in the secret ballot. As expected, believing that the ballot is secret is significantly and negatively correlated with clientelism in table 3 (row 11). This belief is important in itself and could also interact with the reciprocity and network measures, since where voting is thought to be secret, other enforcement mechanisms must be present.<sup>53</sup> Table 4 examines this issue by reporting the relevant interaction terms; none are robustly correlated with our key outcome variable.

### *Contextual Factors*

The political and socioeconomic context may play an important role in the prevalence of clientelism beyond simply influencing individual characteristics. For instance, the closer parties are to one another ideologically, the more likely vote buying is to occur, as citizens are more indifferent to the identity of the winner.<sup>54</sup> We explore this possibility by looking at whether responses correlate with the municipality having no right-wing or left-wing party (among the top two) in the most recent mayoral election.<sup>55</sup> We find that there is a positive association (row 13). Other contextual factors that might facilitate clientelism, however, are not robustly correlated with vote buying in our sample.<sup>56</sup>

### *Core versus Swing Voters*

A key element of the debate on who is targeted with vote buying is whether politicians focus on swing or core voters, or even opponents.<sup>57</sup> We have only implicitly referred thus far to this debate, because theoretical predictions on swing versus core voters are ambiguous unless one carefully measures

53. Stokes and others (2013).

54. Stokes (2005, 2007).

55. We use the party classification of Fergusson and others (2017).

56. These include economic inequality (land Gini coefficient, row 42), an index of municipality state presence (row 30), other proxies for law and order, such as the homicide rate (row 24), the presence of guerrillas (row 32), and the presence of paramilitaries (row 39), or an interaction between the degree of electoral competition with variables that presumably influence who gets targeted. This last exercise is motivated by the evidence in Corstange (2010), for Lebanon, suggesting that districts characterized by monopsonistic buyers are better able to discriminate and target voters more willing to sell than in areas with dueling machines.

57. Lindbeck and Weibull (1987); Stokes (2005).



**TABLE 4. Clientelism: Interaction Terms\***

Variable	Leamer's		Coefficient		Standard error		CDF(0)	
	Lower (1)	Upper (2)	Weighted (3)	Unweighted (4)	Weighted (5)	Unweighted (6)	Weighted* (7)	Unweighted (9)
1. Right ideology × Right dominated	0.008	0.077	0.042	0.042	0.016	0.016	0.996	0.996
2. Win margin × Negative reciprocity	-0.053	0.010	-0.022	-0.022	0.015	0.015	0.930	0.930
3. Secret ballot × Positive reciprocity	-0.032	0.011	-0.010	-0.010	0.010	0.010	0.850	0.850
4. Left ideology × Left dominated	-0.012	0.030	0.009	0.009	0.010	0.010	0.815	0.816
5. Secret ballot × Negative reciprocity	-0.044	0.018	-0.012	-0.012	0.015	0.015	0.795	0.795
6. Win margin × Positive reciprocity	-0.026	0.019	-0.004	-0.004	0.010	0.010	0.643	0.642
7. Win margin × Wealth	-0.039	0.043	0.005	0.005	0.016	0.016	0.623	0.617
8. Secret ballot × Rural population	-0.023	0.031	0.003	0.003	0.012	0.012	0.607	0.607
9. Win margin × Secret ballot	-0.026	0.032	0.003	0.004	0.013	0.013	0.603	0.604
10. Win margin × Rural population	-0.042	0.052	0.003	0.003	0.017	0.017	0.594	0.581
11. Secret ballot × Neighbor cell phones	-0.031	0.027	-0.002	-0.002	0.014	0.014	0.550	0.550

a. The table reports statistics based on the extreme bounds methodology described in the text. For each row, we run a set of regressions varying all possible groups of up to three covariates (see table 3), in addition to the interaction listed in the row, the lower-order direct terms, and region fixed effects. The dependent variable of interest is a dummy variable indicating whether, when deciding for whom to vote, the respondent has taken into account the benefits, gifts, or jobs that a candidate offered in exchange for the vote. Standard errors are clustered at the community level. Lower and Upper are Leamer's lower and upper extreme bounds for the coefficient of interest (that is, for the resulting coefficient of the variable listed in each row) at the 95 percent confidence level. The adjusted  $R^2$  is used to weight statistics where indicated. The  $CDF(0)$  measures the largest accumulated density of the resulting coefficients of interest to the left or right of zero (whichever is largest). Column 7 reports the weighted  $CDF(0)$ , assuming no normality, while column 8 (column 9) reports the weighted (unweighted)  $CDF(0)$  assuming normality. See table A-1 in the online appendix for a description of all variables (<http://bit.ly/20zMEZT>).

the different types of clientelistic vote buying (such as turnout buying, abstention buying, persuasion), as well as the type of inducements delivered.<sup>58</sup> The comparative static predictions for each strategy also differ.

Thus the core versus swing voter debate is most likely to move forward with surveys explicitly designed to test some of the mechanisms mentioned above. This is not the case for our data, so we run a few correlations that must be examined with these caveats in mind. According to Gans-Morse, Mazzuca, and Nichter, the salience of political preferences and the level of political polarization should reduce vote buying.<sup>59</sup> We can use the questions on party identity and political persuasion to measure the salience of political preferences. As discussed above, these features correlate positively with clientelism. When instead examining the correlation using a polarization index, table 3 (row 17) suggests a negative sign, though the *CDF*(0) values fall a bit short of the 95 percent benchmark (an index of electoral fractionalization is also not significant in row 21).<sup>60</sup>

One very crude way to look at whether core, swing, or opponent voters are targeted is to combine people's self-declared ideology with that of the dominant party in each location (defined as dummy variables for whether the incumbent mayor is right wing or left wing). Assuming that the dominant party has better clientelistic networks, then observing that in right-dominated (left-dominated) areas those with right-wing (left-wing) views are more likely to participate in clientelism could indicate that core voters are being targeted. Instead, if in right-dominated (left-dominated) areas those with left-wing (right-wing) views are more likely to participate in clientelism, this could signal persuasion buying.<sup>61</sup> When we include such interactions in table 4, we

58. Cox and Kousser (1981); Cox and McCubbins (1986); Calvo and Murillo (2004); Nichter (2008); Albertus (2013); Gans-Morse, Mazzuca, and Nichter (2014); Stokes (2005).

59. Gans-Morse, Mazzuca, and Nichter (2014).

60. Reynal-Querol (2002).

61. We are aware that ideally the notion of "core" or "loyal" voters should be based on individuals' ideological predispositions toward the clientelistic party (Stokes 2007), and measuring such tendencies is plagued with endogeneity problems: do people report an affinity with (or indifference to) a given party or ideology because they are ideologically predisposed or because they have been bought in the past? Nor is identifying the "clientelistic" party simple. Much of the literature focuses on monopsonistic vote buying (such as, in the models of Stokes [2005], and Gans-Morse, Mazzuca, and Nichter [2014], only one party buys votes), since there is often one dominant clientelistic party with access to the necessary networks and resources. This could justify the assumption that the dominant party is the clientelistic party. However, dueling party machines are also possible, and in Colombia most parties are believed to engage in clientelism; competition for clients leads some parties to participate in a form of "market" clientelism (Gutiérrez and Dávila 1998; Dávila Ladrón de Guevara 1999; García-Sánchez 2002; Gutiérrez 2007).

find a positive and significant coefficient for the interaction of right ideology with right domination, but not for the left interactions. This finding could reflect that in Colombia, core supporters are targeted and left-wing parties are excluded from clientelistic networks.<sup>62</sup> Notice also that the direct correlation of clientelism with right ideology, in row 8 in table 3, is positive.

## Discussion and Implications

In this section, we discuss the relevance of our findings, centering on the implications for economic policy and for some of the key debates in the economics of vote buying.

### *Social Desirability Bias and Fighting Clientelism*

One of the most remarkable results presented above is the absence of social desirability bias in reports of clientelism. This result holds not only across different subgroups of the population (figure 2 above), but also across different geographic regions, as we show in figure 4. Clientelism varies by region: the coast (Atlántica and Atlántica Media) features the highest incidence, nearing 25 percent, while in Bogotá it is approximately 12 percent. But one feature remains constant: respondents admit at similar rates when asked directly versus indirectly with the list experiment, implying a widespread lack of social stigma.

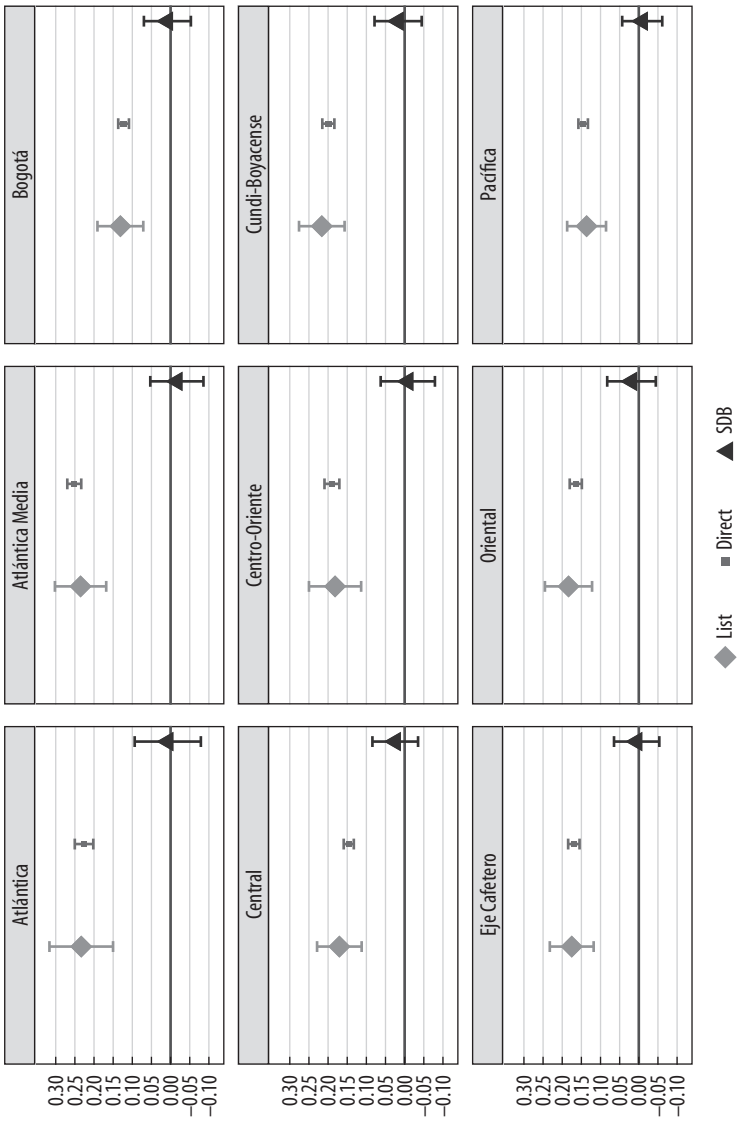
This result highlights possible obstacles to overcoming clientelism. Like any other unethical behavior, vote buying is hard but not impossible to defeat. Several works have shown significant success in fighting corrupt or undesired behaviors using relatively cheap, simple interventions that leverage people's concerns about their social image.<sup>63</sup> However, that vote buying appears to be considered so normal in the Colombian context likely limits the effectiveness of these types of interventions. More precisely, rather than leveraging an existing social norm, eliminating this practice might first require changing existing views to a new social understanding that clientelism is undesirable. But changing social norms with simple policy interventions is much more challenging.<sup>64</sup>

62. Fergusson and others (2017).

63. See, for instance, Perez-Truglia and Troiano (2015) for efforts to reduce tax evasion and DellaVigna and others (2017) for social appeals to increase voting.

64. Bicchieri and Mercier (2014).

**FIGURE 4. Incidence and Social Desirability Bias of Clientelism across Regions<sup>a</sup>**



a. Incidence of clientelism in different regions of Colombia as implied by the list experiment (diamond), direct question (square), and the difference between these two measures, capturing the extent of social desirability bias (triangle). Lines mark the 95 percent confidence interval. Estimates in this figure control for the set of individual characteristics listed in figure 2. For a distinction between urban and rural regions, see table A-1 in the online appendix (<http://bit.ly/2QzMEZ7>).

### *The Significance of the Main Correlates of Vote Buying*

In addition to investigating social desirability bias, our second main contribution is uncovering the robust correlates of clientelism. While we remain cautious by not providing causal interpretations, the results provide informative *prima facie* evidence that supports some theories and challenges others. We now discuss some of the most important findings and their implications.

**STRONG ELECTORAL INSTITUTIONS.** In line with the literature emphasizing enforcement issues in clientelistic vote buying, we find that clientelism is more prevalent among more reciprocal voters and among those who believe that voting is not secret. That reciprocity facilitates clientelism is potentially troubling: it implies, at least in theory, that it could be sustained without a secret ballot via a self-enforcing relationship without monitoring. However, the fact that belief in a secret ballot is negatively correlated with vote buying is consistent with the idea that better electoral institutions and citizen confidence in them can help reduce vote buying.

**ECONOMIC VULNERABILITY.** In our results, wealthier individuals appear to engage less in clientelism, fitting theories where poverty is both fertile ground for and a consequence of this practice.<sup>65</sup> By affecting more vulnerable voters, clientelism can consolidate a vicious cycle: it predates (and reproduces) poverty and vulnerability, amplifying political and economic inequality.<sup>66</sup> In this context, interventions that aim to improve the sources of income of the most vulnerable, and to reduce their dependency on politicians, could reduce the incidence of vote buying.<sup>67</sup>

**INTEREST IN POLITICS.** One intriguing finding, which runs contrary to several theories of clientelism, is that individuals who are most interested in politics are *more* clientelistic.<sup>68</sup> Typically, these theories conclude that more engaged voters are either harder to persuade to vote for an opposing candidate

65. A caveat is that while wealth is a robust correlate of clientelism, the association is not very strong (clientelism is 0.09 standard deviations lower among households that are one standard deviation wealthier than the average).

66. Fergusson (2017).

67. See Bobonis and others (2017) on Brazil and Blattman, Emeriau, and Fiala (2017) on Uganda.

68. Lindbeck and Weibull (1987); Dixit and Londregan (1996). This is captured empirically in our data by whether or not they persuade others to vote, identify with a party, or recall which party they voted for in the most recent election.

in exchange for particularly targeted benefits, or a waste of resources in the case of sympathetic candidates, as they will turn out to vote enthusiastically in any case.<sup>69</sup>

Examining what underlies this correlation in greater detail is relevant to developing ways to combat vote buying. For instance, one hypothesis is that where clientelism is the “usual” form of political participation (a feature consistent with the lack of social desirability bias in our data), clientelism and interest in politics may be complements rather than substitutes. We hypothesize that this positive average correlation between these measures of political engagement and clientelism reveals how deeply entrenched clientelism is in Colombian society, to the extent that those who are more actively concerned about political matters are more likely to accept these forms of electoral exchange.<sup>70</sup> In other words, clientelism may be *the* way to get involved in politics in some contexts, rather than a substitute for doing so.

Since we are observing the outcome of a realized transaction, a related possibility is that more interested and engaged voters are disproportionately approached to sell their vote because they are easier to identify by candidates. Moreover, these citizens may be especially valuable for politicians, as they may have a multiplier effect by persuading other voters.

**BELIEFS.** When we examine the different sets of covariates, it is clear that individual beliefs tend to be robustly associated with clientelism. Specifically, individuals with less favorable views of the rule of law (in particular, those who believe that bribing, resorting to violence, or taking justice into their own hands may be justified) have a higher incidence of clientelistic vote buying. In contrast, some of the other household or individual features and, more clearly, contextual factors such as the electoral features of jurisdiction (also discussed in the context of the swing versus core debate) are not very robustly associated with clientelism.

One possible reason for these results, in line with our interpretation of the lack of stigma for clientelism, is that only when an individual’s entire set of ethical principles changes does engagement with vote buying also change. In other words, even though some contextual factors may matter, a

69. Stokes (2007); Cox (2010). In line with these ideas, Corstange (2012) finds that disinterested voters are more likely to sell their vote in Lebanon. However, more consistent with our findings, Çarkoğlu and Aytaç (2015) find that in Turkey, those with weak partisan attachments sell their votes less.

70. For a related discussion, see Fergusson, Molina, and Robinson (2017).

key underlying root of clientelistic behavior appears to be a worldview that considers this practice natural.<sup>71</sup>

**NONROBUST CORRELATES.** Identifying covariates that are *not* robustly correlated is also informative. Consider one example. A common presumption is that education helps reduce clientelism. Yet our specifications suggest a nonrobust correlation of clientelism with educational attainment (row 31 in table 3), in spite of factors like reverse causality likely making it *more* likely to find a negative correlation. But our exercise also implies that researchers keen on finding this correlation might have focused on several combinations that produce significant results (65 percent of our coefficients are indeed less than zero!). While this does not fully prove that education is irrelevant, researchers emphasizing a correlation that we have proven not to be robust will have to make a compelling case for their specification given the nature of our procedure.

**VALIDATING OUR MEASURES OF VOTE BUYING AND FURTHER IMPLICATIONS.** Finally, while we focus in this paper mostly on determinants of clientelism, we end with a brief discussion of the implications of prevalent clientelistic vote buying, which also serves to gauge the validity of our vote buying estimates. Figure 5 presents the municipality-level average of our vote-buying measure and its relation with four outcomes of interest.

In panel A of figure 5, we show that the average incidence of vote buying is strongly associated with the proportion of preferential voting (that is, for specific candidates in open party lists for Congress) at the municipality level in 2014. Preferential voting has long been associated with clientelistic practices where personal relations supersede programmatic links.<sup>72</sup> This exercise not only validates the relevance of our measure by showing that vote buying is higher where we would expect it to be, but also reveals that vote buying is part of a specific form of political exchange that is often considered detrimental.<sup>73</sup> For further validation, panel B shows that there is also a higher prevalence of vote buying in places with a higher risk of electoral fraud, as reported by the Colombian Electoral Observation Mission (MOE).

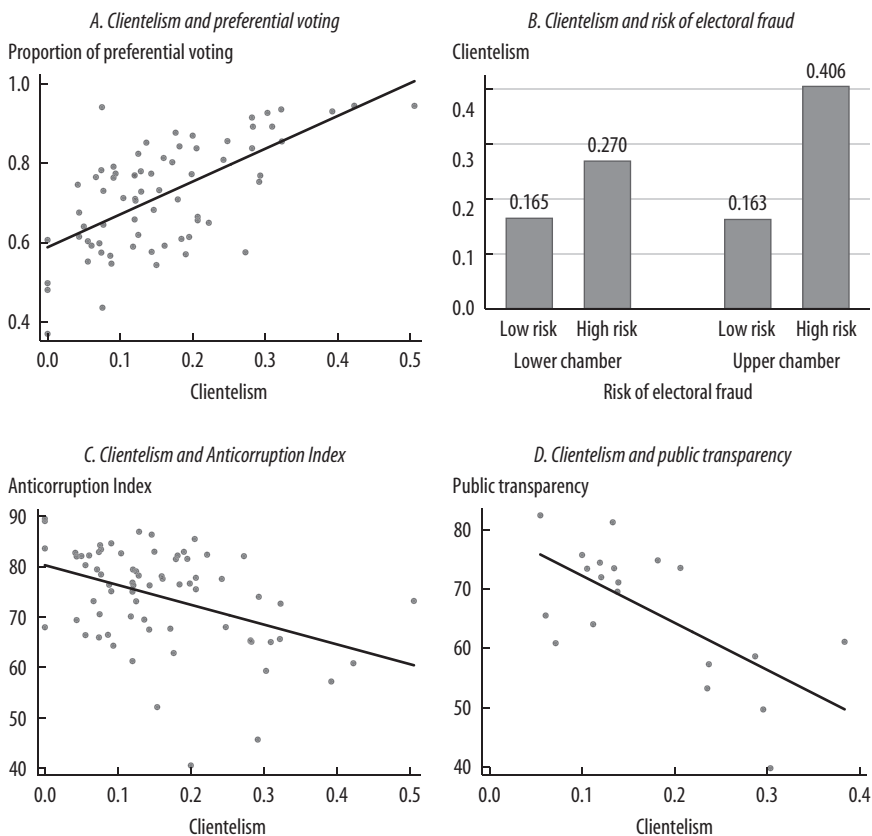
Likewise, and also important for the implications, panels C and D show that vote buying is negatively correlated with measures of transparency and

71. We must recognize, however, that particularly when comparing the importance of beliefs to more aggregate contextual variables, less variation in the latter may also partly explain the lack of significant correlations.

72. Putnam, Leonardi, and Nanetti (1994).

73. Sani and Radelli (1992); Marsh (1985).

**FIGURE 5. Municipality-Level Correlation with Alternative Measures of Clientelism<sup>a</sup>**



a. Panels A, B, and C show correlations at the municipal level; panel D, at the department level. Clientelism is the average incidence (at the municipal or department level) of vote buying measured by the direct question. See table A-1 in the online appendix for a description of all variables.

anticorruption efforts. These correlations are consistent with the idea that clientelism, which relies on public funds for the reproduction of the clientelistic network, can create an incentive structure motivating corruption and arbitrary redistribution and policy targeting.<sup>74</sup> Such distortions can be a key source of inefficiency.<sup>75</sup>

74. For related ideas and evidence, see Stokes and others (2013); Maiz and Requejo (2001); Singer (2009); and Camacho and Conover (2011).

75. Olken and Pande (2012).



## Conclusion

As Stokes and others put it, the transition from a situation where voters, rather than trading their votes for cash or other private benefits, vote for parties that offer public policies of which they approve, is a transition to a more democratic polity.<sup>76</sup> The transition away from clientelism is thus part of a process of democratic consolidation. Nevertheless, clientelistic exchanges are prevalent in many societies, especially in new and poor democracies. Economic development and the processes that accompany it may help weaken clientelism, yet this is hardly a sufficient condition. Moreover, as several scholars emphasize, clientelism may not simply flourish in poverty but can also help reproduce it. Understanding the key drivers of clientelism is therefore extremely important.

Nevertheless, empirical studies of clientelism must confront measures that, if available at all, are often imprecise or plagued with reporting biases. In this paper, we were able to estimate the incidence of clientelism using list experiments applied to a large sample of households, while demonstrating that social desirability in respondents' claims did not bias our measures. Estimates of illegal or sensitive activities are often unavailable, too coarse to relate with individual behavioral responses, or potentially plagued with reporting and other measurement errors. In our setting, we have two key ingredients presenting a rare opportunity: (1) a very comprehensive survey with a wealth of information from survey respondents to examine the drivers and consequences of clientelistic vote buying, and (2) direct evidence that responses are not hampered by reporting biases.

We provided an overview of the resulting incidence and main correlates of clientelism. Since, as noted, clientelism is prevalent in many countries, our analysis is relevant beyond Colombia. Moreover, the richness of the micro-economic information should serve to carefully examine the mechanisms involved, thus enabling researchers to draw useful lessons about possible general forces at play rather than restrict the analyses to simple observations of prevalence in the Colombian case.

In our review, we focused on the likely determinants of clientelism, highlighting the correlations that fall in line with some of the most prevalent existing theories and others that seem to challenge them and open potential avenues for new ideas. Advances in this direction are especially relevant to explore what factors may help explain the persistence or decline of clientelistic

76. Stokes and others (2013).

vote buying. We discuss the substantive significance and implications of the robust correlations we uncover, as well as of the apparent lack of social stigma associated with vote buying. Exploring the implications of clientelism for citizens (both the economic and political repercussions) is equally important, as we illustrated by looking at aggregate correlations with other electoral practices and overall corruption. This is another relevant area for future research that we expect to undertake using the wealth of information described in this paper.

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