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Source: *American Journal of Political Science*, January 2012, Vol. 56, No. 1 (January 2012), pp. 202-217

Published by: Midwest Political Science Association

Stable URL: https://www.jstor.org/stable/23075152

Vote Buying and Social Desirability Bias: Experimental Evidence from Nicaragua

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Qualitative studies of vote buying find the practice to be common in many Latin American countries, but quantitative studies using surveys find little evidence of vote buying. Social desirability bias can account for this discrepancy. We employ a survey-based list experiment to minimize the problem. After the 2008 Nicaraguan municipal elections, we asked about vote-buying behavior by campaigns using a list experiment and the questions traditionally used by studies of vote buying on a nationally representative survey. Our list experiment estimated that 24% of registered voters in Nicaragua were offered a gift or service in exchange for votes, whereas only 2% reported the behavior when asked directly. This detected social desirability bias is nonrandom and analysis based on traditional obtrusive measures of vote buying is unreliable. We also provide systematic evidence that shows the importance of monitoring strategies by parties in determining who is targeted for vote buying.

Clientelistic electoral linkages are characterized by a transaction of political favors in which politicians offer immediate material incentives to citizens or groups in exchange for electoral support.[1] Vote buying, which is a more particularized form of clientelism involving the exchange of goods for votes at the individual level (Stokes 2007), has generated numerous ethnographies and surveys to measure its incidence and test-related hypotheses. While qualitative research routinely finds vote buying to be pervasive in the developing world (e.g., Auyero 2001), individual-level surveys often uncover low levels of such exchanges (e.g., Transparency International 2004). If respondents are reluctant to admit to receiving gifts in exchange for votes, then surveys could systematically understate the amount of vote buying. Moreover, if this measurement error is nonrandom, then empirical results about the dynamics of vote buying derived from surveys are on a shaky foundation. This article uses a survey experiment to lessen social desirability bias, describe whom campaigns targeted with vote buying in an election, and demonstrate that this measurement error is nonrandom.

The academic consensus is that the exchange of gifts and favors for votes has deleterious consequences for

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The authors would like to thank Darren Davis, Fran Hagopian, Victoria Murillo, Anabella Expaña-Nájera, Simeon Nichter, Ana de la O, Maya Parson, Shannon Walsh, and Rebecca Weitz-Shapiro for generous feedback; Paul Avey, Robert Braithwaite, and Christopher Sullivan for their collaboration in designing the survey; and *Borge y Asociados* for fielding the survey. Nickerson would like to thank the Center for the Study of Democratic Politics at Princeton University for the time to write the article and the Dean of the College of Arts and Letters at the University of Notre Dame for providing funds for the survey. Appendices and material for replicating the statistical analysis can be found at www.nd.edu/~dnickers.

[1] Programmatic linkages, by contrast, are based on the promise of indirect benefits resulting from investment in public goods or on direct benefits distributed by public bureaucracies according to standardized rules (Kitschelt 2000).

American Journal of Political Science, Vol. 56, No. 1, January 2012, Pp. 202–217

 DOI: 10.1111/j.1540-5907.2011.00540.x

democracy.[2] Vote buying (and clientelism more generally) shortens the time horizons of public policies, thereby generating development and poverty traps (Magaloni 2006); inverts the rationale of vertical accountability, making voters and not politicians subject to monitoring and control (Stokes 2005); and, when practiced primarily by those in control of public monies, hinders the creation of a level and competitive political playing field. It is therefore imperative not only to study the causes, consequences, and dynamics of vote buying, but also to estimate its pervasiveness in any given polity.

Scholars have employed a number of methodological approaches to studying vote buying. Most of the existing knowledge on vote buying is based on qualitative ethnographies detailing the ways in which clients interact with political brokers and how clients view such exchanges (see Stokes 2007 for a review). Based on these studies, the widely held assumption is that clientelistic electoral linkages are pervasive, particularly but not exclusively, in politics in the developing world (Kitschelt 2000) and that the poor are the most likely targets of such strategies. Although these studies have greatly increased our understanding of the processes undergirding vote buying in specific contexts, this qualitative literature is limited by its inability to measure the extent of vote buying and test competing theories about party macrostrategy.

In response to this disadvantage, some scholars have used mass surveys to study vote buying. In stark contrast to ethnographic studies, survey data often show that small percentages of respondents report personally receiving a good or favor from a political party when asked about it directly (e.g., Transparency International 2004). For example, Stokes (2005) finds that only 7% of Argentine respondents reported receiving goods for their votes, even though ethnographic work shows that vote buying is a key component of modern Argentine politics (Auyero 2001; Levitsky 2003). Most surveys reported single-digit vote-buying rates, with those reporting double-digit rates restricted to Southeast Asian democracies (Schaffer 2007). These types of findings leave many scholars optimistic about the low incidence of vote buying (e.g., Seligson et al. 2006, 93). These data collected by surveys require researchers to assume that measurement error in survey items on vote buying is negligible or at least random.

To determine if respondents are targeted for vote buying, researchers generally ask respondents directly whether they have received a gift or favor from a political party or candidate. Survey items that ask about stigmatized or illegal attitudes or behaviors often produce social desirability bias, as many respondents wish to present themselves in a favorable light to interviewers (Bradburn et al. 1978; DeMaio 1984; Nadeau and Niemi 1995; Tourangeau and Yan 2007). Since vote buying is illegal in most countries and generally associated with a negative social stigma due to its disjuncture with democratic norms and association with poverty, items that directly ask respondents about it are likely subject to significant levels of measurement error.

Consequently, the use of direct, obtrusive survey items may provide inaccurate estimates of aggregate levels of vote buying and bias analyses of the targeting strategies of political parties due to systematic patterns in social desirability bias. While such bias has not gone completely unacknowledged by scholars (Brusco, Nazareno, and Stokes 2004, 69–72; Stokes 2005, 321, fn. 20), existing alternatives, such as those asking about vote buying occurring in respondents' neighborhoods, do not sufficiently eliminate social desirability concerns, nor do they allow particularly precise understandings of whom parties and candidates target for vote buying. As a result, the implications of significant measurement error for empirical analyses have largely been ignored. Without more valid and reliable measures of vote buying, advancing the clientelism research agenda will be difficult.

To assess the validity of these arguments, we employed a measurement technique to determine the degree to which social desirability bias affects direct vote-buying items and how such bias is distributed across relevant variables. We surveyed registered voters in Nicaragua immediately after the 2008 municipal elections and found that nearly a quarter of respondents received a gift or favor in exchange for their vote according to an unobtrusive "list experiment." In contrast, less than 3% of respondents reported that they had received a gift or favor when asked directly. Not only does the direct measure vastly underestimate vote buying, but the social desirability bias is also nonrandom and correlated with politically important variables. This finding implies that analyses employing direct measures are likely to yield inaccurate results. We also find that political campaigns target individuals who think parties can monitor their vote, thus overcoming the commitment problems inherent to these transactions.

Response Bias and Vote Buying

When respondents fear expressing socially undesirable attitudes or admitting to participating in stigmatized

[2] Some authors have pointed to potential benefits of vote buying and clientelism in institutionalizing political systems. See Huntington (1968) and Philipson and Snyder (1996).

or illegal behavior, social desirability bias can occur (Bradburn et al. 1978; DeMaio 1984; Johnson and Van de Vijver 2003). Some respondents misrepresent themselves to show favorable images of themselves to interviewers on subjects such as racial attitudes (Berinsky 1999; Kuklinski, Cobb, and Gilens 1997; Kuklinski et al. 1997), religious attitudes and behaviors (Kane, Craig, and Wald 2004; Presser and Stinson 1998), sexual behavior (Tourangeau and Smith 1996), drug use (Bachman and O'Malley 1981; Tourangeau and Smith 1996), and voter turnout (Silver, Anderson, and Abramson 1986).[3]

Survey respondents may view standard vote-buying items as sensitive due to the practice's illegality, its contradiction of democratic norms, and its association with negative stigmas attached to poverty (Stokes 2005, 321, fn. 20). Thus, asking directly about receiving a gift in exchange for a vote may result in social desirability bias and the underreporting of its incidence. This makes it particularly difficult to reliably estimate the proportion of people receiving goods, much less determine what type of people parties target for vote buying.[4]

Although scholars sometimes recognize the bias inherent in their direct measure of vote buying, they too easily dismiss the problem in the testing of their models. Following King, Keohane, and Verba (1994, 157–68), some have argued that when vote buying is the dependent variable, measurement error is of less concern, since measurement error in the dependent variable generally only decreases the efficiency of the estimate (i.e., the standard errors increase and the statistical significance of coefficients decrease), while it is unlikely to bias the coefficients. Thus, they argue, measurement error places a greater burden on the researcher by making it more difficult to find significant associations (e.g., Stokes 2005, 321, fn. 20).

This reasoning, however, is not entirely convincing since it is based on the assumption that error in direct measures of vote buying is nonsystematic (i.e., random) and constant across all relevant subgroups. Measurement error that systematically varies across subgroups would bias inferences (King, Keohane, and Verba 1994, 155–57). For example, adjudicating between game theoretic models often hinges on the decision of parties to target either weakly opposed voters for vote buying (e.g., Stokes 2005) or strong supporters for turnout buying (e.g., Nichter 2008). Assume that contrary to both of these models, parties distribute gifts to both groups at equal levels or that they distribute goods randomly. If strong supporters see gift dispensation by their party as a commendable component of clientelistic "problem solving networks" (Auyero 2001), they may underreport vote buying to a lesser extent than weak supporters. Similarly, compared to strong supporters, swing voters may be relatively reluctant to admit being offered gifts because they are consciously "selling" their vote. In either case, systematic social desirability bias would cause traditional analysis to wrongly suggest weakly opposed citizens are less likely to receive gifts or favors than strong supporters. Thus, findings from direct questions that core supporters are more likely to be targeted are not on firm empirical ground.

The primary alternative to the direct individual item is asking about vote buying more indirectly by inquiring about vote buying in the respondent's neighborhood, but this tactic also introduces potentially nonrandom measurement error.[5] Unfortunately, respondents may remain hesitant to answer this item truthfully due to social desirability concerns. It is also unclear that indicating vote buying in one's neighborhood is a good proxy for individual-level vote buying. Response bias can result if supporters of a given party report vote-buying operations by rival organizations in their neighborhood to discredit them. Additionally, questions regarding campaign vote buying in neighborhoods have the obvious problem of not directly or reliably capturing who is being targeted. Such items may simply capture the public nature of vote buying and not its frequency. For example, neighborhood measures may overestimate vote buying in urban areas and underestimate the frequency in rural areas where the probability of a clientelistic exchange being observed by other citizens is lower. Thus, it is entirely plausible that measurement error in neighborhood estimates of vote-buying behavior is correlated with politically relevant variables and can lead to biased inferences as well.

[3] While not all survey items exhibit response bias, questions that appear either threatening—due, for instance, to the illegality of the behavior—or sensitive—as a result of negative social stigmas—are most likely to cause significant misreporting and thereby increase measurement error.

[4] Brusco, Nazareno, and Stokes (2004, 69–72) note that as a result of these considerations, it is nearly impossible to provide an accurate point estimate of vote buying and suggest that the best estimate lies somewhere in between the estimates provided by individual and neighborhood vote-buying items.

[5] Asking whether the respondent has turned or would turn to a party operative for help has also been used as an alternate dependent variable. While this might improve inferences, such items are less direct measures of vote buying and assume that the same people who turn to parties for help also are given gifts in return for votes and people who do not seek assistance are not provided gifts. This empirically verifiable assumption is hard to test given the high likelihood of bias in the measurement of each variable.

The List Experiment

In order to reduce social desirability bias in the measure of vote buying, we employed an unobtrusive measurement technique known as the list experiment (or unmatched item count technique). While this is the first attempt to use a list experiment to gauge levels of vote buying, political scientists have successfully used list experiments to study a number of other topics subject to social desirability bias, including racism (Kuklinski, Cobb, and Gilens 1997; Kuklinski et al. 1997) and self-reported voter turnout (Holbrook and Krosnick 2010).[6]

The logic of the list experiment is straightforward. First, the survey sample is split into random halves: a treatment and a control group. Each group is read the same question and shown a card with the response options, which differs only in the number of response categories:

> I'm going to hand you a card that mentions various activities, and I would like for you to tell me if they were carried out by candidates or activists during the last electoral campaign. Please, do not tell me which ones, only **HOW MANY**.

For the control group, the following campaign activities are listed and read to respondents:

- they put up campaign posters or signs in your neighborhood/city;
- they visited your home;
- they placed campaign advertisements on television or radio;
- they threatened you to vote for them.

The treatment group is shown and read a fifth category, placed in the third response position:

- they gave you a gift or did you a favor[7]

The question does not ask respondents to reveal to the interviewer the specific activities parties or activists practiced. The respondents only have to tell the interviewer how many activities were carried out, so the question provides the respondent a high degree of anonymity since the interviewer cannot ascertain which activities the respondent indicates. Because respondents intuitively understand this anonymity, social desirability pressures should be reduced, providing less incentive to underreport vote buying.

Since respondents were randomly assigned to the treatment and control groups, the two groups will be identical on both observable and unobservable characteristics, in expectation. Thus, an estimate of the proportion of respondents receiving a gift or favor can be derived simply by comparing the average number of items indicated by the respondents in each group to the list-experiment question. If no vote buying occurred, there would be no difference in the mean number of items reported by each group on average. Systematic differences in the means provide a point estimate on the number of people reporting vote-buying activity. For example, if the average number of items indicated by the control group is 2.10 and the average number of items indicated by the treatment group is 2.35, then we can conclude that 25% of respondents received a gift or favor ($2.35 - 2.10 = 0.25$, and $0.25*100 = 25\%$). The list experiment can also estimate means within different subgroups of the overall sample (e.g., partisan, socioeconomic, gender). Estimates of social desirability bias across subgroups can be determined by the absolute difference between the list experiment estimates and the estimates derived by both direct individual and neighborhood items.

The Nicaraguan Case

The central aim of this article is to show that the methodology usually employed to measure the pervasiveness of vote buying and its political dynamics is potentially biased. We use the list experiment to demonstrate this empirically with survey evidence from Nicaragua following the country's November 2008 municipal elections.

Over the past two decades, two main political factions have become dominant in Nicaragua's political system, alternating in power on several occasions. The former guerrilla movement and current leftist party, *Frente Sandinista de Liberación Nacional* (FSLN),[8] controls the presidency

[6] For a discussion of the validity of the technique and associated strengths and weaknesses, see Appendix C in the online supporting information. We complement this with the results of a validation experiment conducted in Uruguay and Honduras.

[7] We included the final category (threatening the respondent) in order to reduce the chance of a ceiling effect (see Kuklinski et al. 1997). Even with this item, though, 26 respondents indicated all five items, suggesting that some ceiling effect may have occurred. However, such an effect would only serve to depress estimates, thereby working against our social desirability bias hypothesis. The English and Spanish versions of the instrument can be found in Appendix B in the online supporting information.

[8] The party is headed by Daniel Ortega, who was a member of the *Junta Nacional de Reconstrucción* that took office when dictator Somoza was overthrown in July 1979. When the FSLN came to dominate the junta, Ortega became the de facto ruler of Nicaragua. In 1984, through competitive elections, Ortega became president for the period 1985–1990, and more recently, in 2006, he was re-elected.

and maintains a firm support base around 40%, with electoral strongholds in poor urban areas. Since the late 1990s, the *Partido Liberal Constitucionalista* (PLC) has positioned itself as the primary center-right opposition to the Sandinistas by attracting support from the middle classes, small businesses, and poor voters in rural areas (Guzmán and Pinto 2008).[9] The rightist parties when considered together have generally held more than half of the vote share (around 55%), but in the 2006 presidential elections votes were spread across several parties.[10]

The 2008 municipal elections were not heavily contested in all parts of the country, but high levels of competition in some of the largest and most important municipalities in the country, including the capital Managua and León, led the major parties to use every tool at their disposal to secure victory (Marti I Puig 2008).[11] Although media sources were attentive to vote buying during the campaign and reported that "food stamps, rice, beans . . . and 25 thousand stoves were distributed,"[12] how widely vote buying was practiced is unclear.

A strong history of patrimonialism and clientelism more generally suggests that vote buying should be prevalent in Nicaragua. Dye and Close (2004) argue that under Somoza's dictatorial regime (1939–79), Nicaraguans experienced the establishment of a patrimonial state in which the distinction between public and partisan realms was blurred.[13] This long-standing tradition of using state resources to oil political machines enables the capture of state bureaucracies to reward political allies and to funnel resources to the government's electoral base in contemporary Nicaraguan politics (Marenco 2004).[14]

Given the wide-ranging forms of corruption in Nicaragua, one might wonder why parties need to engage in vote buying at all. Not only were the 2008 municipal elections in Nicaragua tainted by accusations of electoral fraud, but the Supreme Electoral Council also made a series of rulings before the election that systematically benefited the FSLN (Greene 2010). Furthermore, voters can be wooed with the promise of patronage jobs and the provision of club goods, neither of which falls under our strict definition of vote buying. With so many tools at a regime's disposal, vote buying may be "unnecessary." However, there are three reasons that parties may still decide to engage in vote buying. First, vote buying is probably less costly than electoral fraud, so parties may buy votes in order to obviate the need to engage in postballot forms of electoral corruption. Second, vote buying could be a logical extension of goods and services provided in nonelectoral time periods. The vote buying immediately prior to the election might even be a necessary reminder of the ongoing largess provided to the voter by the party. Finally, elections are competitive and parties seize every opportunity to secure votes. Just as campaigns hang posters, air radio ads, and stage rallies regardless of their effectiveness, campaigns may engage in vote buying to gain an edge on (or keep up with) the competition. Thus, the documented presence of alternative forms of electoral corruption does not rule out the utilization of vote buying as a campaign strategy.

While research on the actual incidence of vote buying—rather than broader clientelism—in Nicaragua is limited, extant literature suggests that vote buying is an important component of electoral competition in the country. Nicaraguan parties across the ideological spectrum engage in clientelistic practices both in and out of power (Guzmán and Pinto 2008). Even though vote buying is considered an electoral crime according to Nicaraguan Electoral Law,[15] enforcement is nonexistent and vote buying has become a common mobilization

[9] PLC leaders governed Nicaragua for two consecutive periods: Arnoldo Alemán between 1997 and 2002, and Enrique Bolaños between 2002 and 2007.

[10] Since the 2006 presidential election, and with the return of Montealegre to the PLC, the other rightist party, *Alianza Liberal Nicaragüense* (ALN), has lost its political appeal. In the 2008 municipal elections, ALN obtained only 2.1% of the valid votes.

[11] According to the electoral council's provisional results, the Sandinistas won 94 of the 146 municipalities, including Managua. With 70% of the votes counted in Managua, the *Consejo Supremo Electoral* (CSE) claimed that FSLN candidate Alexis Arguello received 51.3% of the valid votes and defeated PLC's Eduardo Montealegre, who garnered 46.5%. These results were challenged by the opposition and international observers. During several days after the election, the Nicaraguan capital was the scene of riots and confrontations between the Liberals, who were claiming fraud, and the Sandinistas, who were "defending the people's choice." Despite pressures from the international community, the opposition's claim did not advance and the electoral council proclaimed the FSLN as the winner of the elections. In January 2009, several civil society organizations presented 100,000 signatures to the CSE demanding a recount of votes. As of December 2010, the state has still not released official election results. Currently, the official website makes no mention of the election. See http://www.cse.gob.ni.

[12] *Envío*, October 2008, report No. 309.

[13] See Millet (1977) for a history of the partisan use of public monies, especially during Somoza's regime. See Vargas (2004) for more recent accounts of this phenomenon.

[14] Vote buying is only but one option in the large "menu of manipulation" used by incumbents seeking to secure electoral victory (Schedler 2002). Indeed, the Liberal opposition claimed outright fraud in vote counts in Managua (see fn. 13).

[15] Article 174 allows campaign activity but is unequivocal with respect to the distribution of goods in exchange for votes: "anyone who bribes someone else in order to support any specific candidacy, vote for a specific option, or abstain from voting will be sanctioned with six to twelve months in prison" (*Law 331*, Electoral Law [January 19, 2000], Title XIV, Unique Chapter on Electoral Crimes, Article 174; translation is ours).

strategy. Parties on the left and right have resources to develop and maintain clientelistic networks. In the context of the 2008 elections, both sides could engage in vote buying since the FSLN had access to government resources and the PLC possessed strong ties to business interests and still controlled some subnational offices. In addition, being the second poorest country in Latin America, both leftist and rightist parties in Nicaragua have low-income core constituencies that allow them to engage successfully and efficiently in vote-buying strategies alongside or in detriment of programmatic ones because of the high relative value that inexpensive gifts have for these voters.[16] In this respect, Nicaraguan politics is different from political practices in countries where vote buying is usually studied (e.g., Argentina) because two, rather than one, well-articulated machines extensively practice vote buying. This depiction of Nicaraguan politics anticipates high levels of vote buying but hints that vote buying might be accepted as "politics as usual" by respondents, who would therefore be less concerned with social desirability.

An alternative view of Nicaraguan political culture examines the lasting effect of the Sandinista revolution on political attitudes and behaviors and argues that the movement that overthrew Somoza created a context unfavorable to clientelistic relationships (Anderson and Dodd 2005, 2009). According to this literature, the Sandinista-led social revolution helped to foster a highly participatory society hostile to vertical ties between party leaders and citizens. In contrast to its nonrevolutionary neighbors, Nicaraguans and their political institutions have demonstrated less authoritarianism and have tended to be more politically engaged and tied to political parties (Booth and Richard 2006). However, these same authors have argued that this distinctiveness has decayed over time, as the differences between Nicaragua and its neighbors in electoral behavior have largely diminished (Booth and Richard 2006), but this trend may not hold at the local level (Anderson and Dodd 2009). Given this grassroots nature of the Nicaraguan political culture, these authors would anticipate a strong social stigma against the practice of vote buying, suggesting that even if the practice has become a part of everyday politics, the cultural legacy of the revolution may still induce significant socially desirable responding in the context of a survey.

To gauge the extent to which vote buying is stigmatized in Nicaraguan political culture, we added two survey questions to an omnibus, face-to-face survey of Nicaraguan adults conducted in September 2010 (nearly two years after the election we studied).[17] The first question directly asked about the likelihood that survey respondents would admit to receiving a gift. The second question sought to measure the stigma associated with vote buying by asking about the acceptability of a hypothetical patron-client exchange.[18]

TABLE 1 Likelihood of Social Desirability Bias in Nicaragua

Likelihood of Admitting to Receiving Gift on a Survey		Acceptability of Hypothetical Vote-Buying Exchange	
Very Likely	5.7%	Totally Acceptable	1.8%
Somewhat Likely	14.2%	Acceptable	6.3%
Not Very Likely	20.1%	Understandable but not Acceptable	11.7%
Not at All Likely	51.3%	Unacceptable	40.3%
		Totally Unacceptable	36.5%
Don't Know / No Answer	8.7%	Don't Know / No Answer	3.5%
N	1008		1008

The results (Table 1) provide strong evidence against the hypothesis that social desirability bias should be minimal due to the everyday nature of practice. Over 70% of respondents reported that they thought it was not very or not at all likely that a person would admit to having taken part in a vote-buying exchange, while fewer than 20% thought it would be very or somewhat likely that a person would admit to selling his or her vote on a survey. Results are similar when respondents were asked to evaluate the acceptability of a hypothetical patron-client exchange. Over 75% of respondents thought that the hypothetical exchange was unacceptable or totally unacceptable, while only 8% thought that the situation was acceptable or totally acceptable. These two items demonstrate a strong normative stigma against vote buying in the context of surveys conducted in Nicaragua. This finding suggests that social desirability bias can be strong even

[16] For a similar argument based on the Argentine case, see Calvo and Murillo (2004). According to the World Bank (2005), the regional average of per capita GDP in Latin America and the Caribbean during the period 1990–2006 was $3,489 (constant US dollars), but only $451 in Nicaragua.

[17] For a full description of the survey design, see Appendix B in the online supporting information.

[18] Spanish and English versions of the survey items can be found in the online supporting information.

in a country in which clientelism is a deeply embedded practice.[19]

In order to pilot test our survey questions, get a feel for campaign dynamics, and generate hypotheses to test with the data generated during the 2008 municipal election, one of the authors conducted fieldwork that documented the existence of well-articulated political brokerage networks managing highly detailed personal information about the needs of their potential clients on behalf of candidates. These networks of local brokers allow political parties to tailor the goods and services provided to the needs of particular voters and distribute them in exchange for their votes. For example, Alexis Arguello, the Sandinista candidate for mayor of Managua, on October 22, 2008, visited the *Asentamiento Hugo Chávez Frías*, one of the many shantytowns surrounding Managua, and delivered precisely nine wheelchairs to the nine handicapped people in the neighborhood.[20] According to Nicaraguan political analysts interviewed during fieldwork, the conservative parties' networks operate in a similar fashion. The PLC has built deeply rooted clientelistic networks in the rural areas of the country, delivering goods such as chickens or cows in order to mobilize voters.[21] These kinds of goods are clearly considered part of a political transaction. According to a Nicaraguan social researcher, "construction materials, cattle, money, and medicine are traditionally the most frequent goods delivered in electoral campaigns in exchange for votes."[22]

More recently, the Sandinistas have taken advantage of government-sponsored (and Sandinista-controlled) *Consejos de Poder Ciudadano* (Citizen Power Councils, CPCs) for clientelistic purposes.[23] Distribution of goods through the CPC is not part of an official welfare program or part of policy-targeting techniques, since they are manipulated under a clientelistic logic (Marti I Puig 2009).[24] Furthermore, such brokerage networks play a key role not only in distributing goods and favors but also in monitoring the behavior of clients. According to a multilateral agency official, citizens believe that their votes can be monitored by the FSLN and by the Nicaraguan Intelligence Agency, *Dirección General de Inteligencia Civil*. It is in the interest of political parties to foster this misconception because it makes noncompliance by gift recipients less likely.[25]

Survey Description

We conducted a nationally representative face-to-face survey between November 25 and December 8, 2008, two weeks after the municipal elections.[26] The survey was a multistage random sample with the electoral registry serving as the sampling frame with voting centers (precincts) randomly chosen proportionate to department and municipality size. There were 84 final sampling points (segments), including 12 respondents per segment who were randomly assigned to treatment and control surveys.[27] The American Association of Public Opinion Research (AAPOR) response rate 3 for the survey was 77% and resulted in 1,008 respondents.

In addition to the list experiment described previously, the survey instrument also included a number of other items used in the empirical analysis. To compare and contrast the different techniques for detecting vote buying, respondents were asked whether they had seen vote buying in their neighborhood and whether they had

[19] During the 2008 electoral campaign the Catholic Church's education campaign against vote buying may have enhanced the stigma attached to the practice (*La Prensa*, October 31, 2008).

[20] Field notes (October 2008).

[21] Interview (October 2008).

[22] In contrast, the possibility of getting a job is perceived "as a normal reward for their collaboration during campaigns." Interview (October 2008).

[23] The Nicaraguan Constitution (1985) and the Municipal Law (Law 40, reformed in 1997) established several mechanisms that encourage citizen participation in public affairs (Avendaño 2006). Since his comeback to office in 2006, President Ortega introduced "participatory democracy" reforms by creating the *Consejos de Poder Ciudadano*, which are state-sponsored deliberative neighborhood organizations.

[24] According to *Envío* (October 2008, Report 309), the CPCs have "two faces." First, these organizations recruit and mobilize people who need food stamps or low-interest loans (two key social programs are *Hambre Cero* [Zero Hunger] and *Usura Cero* [Zero Profiteering]). Any member of a CPC has access to housebuilding programs, legal property titles, and employment programs. Second, the government gives power and resources to CPC's leaders in order to monitor the political loyalty of members of the community.

[25] In interviews with various citizens in a poor neighborhood in Managua (October 2008), the Sandinista practice of "helping" poor citizens in obtaining electoral identification documents "practically for free" is perceived by these citizens to be a mechanism to monitor their vote, even though they cannot identify with precision how it operates.

[26] Elections were not held in the seven municipalities in the autonomous North Atlantic Region, which was still recovering from the 2007 Hurricane Felix and postponed elections until January 2009. Thus, the two autonomous regions on the Atlantic coast (representing 10% of the Nicaraguan population) were excluded from the sample.

[27] For a full description of the survey design as well as for Spanish and English versions of the questionnaire, go to the online supporting information. Randomization occurred at the individual level.

been targeted themselves using wording nearly identical to the treatment item on the list experiment in order to allow for a direct comparison of the different items.[28] Follow-up questions asked those who responded affirmatively to either of the direct questions about which parties engaged in the distribution of gifts, the kinds of material incentives dispensed, and whether the gifts had an influence on their vote.

To test existing predictions about party targeting, we collected information about the respondent's socioeconomic status (i.e., income, education, gender, age, and area of residency; e.g., Calvo and Murillo 2004; Kitschelt and Wilkinson 2007),[29] partisanship (e.g., Cox and McCubbins 1986; Dixit and Londregan 1996; Nichter 2008; Stokes 2005), beliefs about party monitoring of vote choice (e.g., Stokes 2005), and proximity to institutions that may facilitate the distribution of goods or alter perceptions of monitoring capacities.

These last two sets of items require some additional elaboration, as they have generally not been subject to empirical testing, even though they are important in theoretical models. Vote buying is characterized by a commitment problem requiring the development of a deeply penetrating political machine to monitor compliance (Kitschelt 2000; Stokes 2005). That said, there is some disagreement on the importance of monitoring in the politics of vote buying. Nichter (2008) argues that given the existence of the secret ballot and the ensuing impossibility of reliable monitoring, parties target their own supporters in an effort to get out the vote, as opposed to buying off indifferent or opposition voters. Some scholars go as far as to suggest that it is this inability to solve the commitment problem that makes vote buying a very ineffective form of electioneering and therefore a rare phenomenon (e.g., Lehoucq 2007). Only by combining a reliable measurement of the aggregate levels of vote buying with an appropriate operationalization of monitoring can these rival explanations be adjudicated. Although the ethnographic literature has described the workings of effective monitoring machines (e.g., Auyero 2001) and formal models have incorporated monitoring as an important parameter (e.g., Stokes 2005), the survey-based quantitative literature has not consistently measured monitoring.

The item on party monitoring asked whether respondents believed that the government or parties could find out how people in their community voted. Whether respondents had received social welfare benefits was asked, with the intuition that being in the government's records increases the probability of both harassment and monitoring.[30] We also asked whether their neighborhood had received investments in public works in the last six months, with the expectation that those areas where the state's presence is higher or where the incumbent party invests more resources may be more susceptible to clientelistic practices. Finally, we inquired about the frequency of involvement in CPCs. Participation in these local deliberative bodies could raise the visibility of participants in the eyes of the clientelistic machine and increase the latter's ability to monitor the voting behavior of these individuals, making them likely targets of vote buying.

Results from the List Experiment

The first step in the analysis is to estimate the number of people reporting the receipt of a gift or favor through the list experiment.[31] The first column of Table 2 reports

[28] Readers may wonder whether the wording "gifts or favors" adequately captures the type of exchanges that interest vote buying researchers. Follow-up questions to the direct neighborhood and individual questions in our Nicaragua survey mitigate this concern. When asked about the specific gifts or favors given away in their neighborhood, respondents indicate that furniture, animals, food, tools, and construction material constitute 77% of the items delivered, 6% reported parties handing out money, and a miscellaneous group of gifts represents another 5%. Only 12% of the gifts reported could be associated with common campaign giveaways, such as clothing (hats and T-shirts) and souvenirs (banners and backpacks). The proportions remain similar when respondents are asked if they personally received gifts or favors. These data indicate that (a) parties deliver a broad variety of goods targeting particular needs and (b) our question wording has high measurement validity. Furthermore, if social desirability bias is deflating estimates from the direct measures, it is unlikely that those providing misleading answers do so to cover up the receipt of relatively benign campaign giveaways. Thus, the list experiment should help to eliminate the reporting of gifts not usually associated with vote buying.

[29] Income is measured using dummy variables so that respondents for whom we have missing income data (7% of the sample) are not excluded from the analysis and to avoid imposing linearity. "Very poor" indicates that the respondent makes less than $100 USD per month (approximately less than C$3,000 Cordoba Oros). "Poor" signifies that the respondent's income is between $100 and $200 USD per month (between C$3,001 and C$4,250). "Middle/upper" indicates that the respondent makes more than $200 USD (more than C$4,251) per month. Education is operationalized as a four-point ordinal variable, ranging from 0 (less than primary education completed) to 3 (postsecondary education). Gender is a dichotomous variable taking the value of 1 if the respondent is a female. Age is coded as a three-value ordinal variable for people between 18 and 29 years old, between 30 and 49, and older than 50. Residency is coded as a dummy variable taking the value of 1 if the respondent lives in an urban area and 0 if the respondent lives in a rural area.

[30] Depending on how welfare policies are distributed, they can also be used as a form of vote buying by parties. In our pilot testing and responses to the survey, no respondents mentioned welfare programs under the term "vote buying."

[31] The balance between treatment and control groups across relevant demographic variables suggests that the randomization

TABLE 2 Vote Buying, Direct and Unobtrusive Measures

		Direct Items	
	List Experiment	**Individual Gifts**	**Neighborhood Gifts**
Control	2.06 [495]		
Treatment	2.31[500]		
Estimated % receiving gifts	24.34% (5.53)	2.39%*** (0.58)	17.84% (1.80)
N	995	1003	998

Notes: List-experiment control and treatment values are the mean number of items identified by respondents. The numbers of subjects in each condition are in brackets. Linearized standard errors adjusted for the survey design are in parentheses.
***p < .01 for difference between list-experiment proportion and the direct individual proportion. The difference between the list and neighborhood proportions is not statistically significant. Tests are two-tailed difference of proportions tests.

the results of the list experiment. The mean number of electoral activities reported by respondents in the control group with only four options is 2.06, while the mean in the treatment group where subjects had the added choice of "receiving a gift or favor" is 2.31. Random assignment ensures that the difference is due to respondents reporting vote buying. Thus, the estimated percentage of respondents receiving gifts during the election according to the list experiment is 24% (s.e. = 5.5 percentage points).[32]

This number is not only statistically significant, but also very different than when asking respondents about vote buying directly. Only 2.4% of respondents (s.e. = 0.6 percentage points) admitted receiving a gift or favor personally (Table 2, column 2). The nearly 20-point difference is a different order of magnitude altogether (unsurprisingly, the difference between the two estimates is highly significant). The list experiment depicts vote buying to be pervasive, while the direct survey measure used in most quantitative studies suggests the practice is rare.[33] The aggregate results are somewhat better when relying on the neighborhood vote-buying item, which indicates that 17.7% of respondents (s.e. = 1.8 percentage points) answered in the affirmative. The 6.7 percentage point difference between the list experiment and the neighborhood question is not statistically significant ($p = 0.263$).

Examining the list experiment by important subgroups makes it possible to construct a map of which types of people parties target for vote buying. The precision of the list experiment depends on the number of subjects in a particular cell (see Table 3, column 3 for the proportion of the overall sample in each category), so the uncertainty surrounding each subgroup is large, but it is striking how few differences there are across subgroups on key variables (see Table 3, column 4). Independents and members of both parties all received gifts (none of the estimates are significantly different from each other), offering little support for Nichter's (2008) argument that machines mainly target core supporters in an effort to buy turnout. The patterns observed could be evidence that both parties attempt to buy off opposition and independent voters, thus supporting Stokes's (2005) theory. Alternatively, since our fieldwork observation in Nicaragua revealed that two clientelistic machines are competing against each other for votes, parties might engage in gift dispensation to both safeguard their core constituency and lure weakly opposed or independent voters, giving rise to more widespread patterns of distribution.

Contrary to expectations from the broader clientelism literature (e.g., Calvo and Murillo 2004; Kitschelt and Wilkinson 2007), there was no evidence of a negative linear relationship between respondents' socioeconomic status and goods dispensation (e.g., welfare, income, and education). This null finding suggests that either Nicaragua is an exception or vote buying is practiced differently in municipal elections than in relatively high-turnout national elections. Alternatively, it could be suggested that the population is sufficiently poor that gifts or favors would still remain appealing to those with comparatively higher levels of income. The percent of respondents reporting vote buying among the poorest (less than $100 USD per month) and the marginally better off ($100 to $200 USD per month) is very similar. Although

procedure worked properly. See Appendix A in the online supporting information.

[32] For the entire analysis, standard errors are adjusted for the survey design.

[33] Even though the list experiment preceded the direct questions, there is little evidence to support the hypothesis that the question order in the survey instrument affected responses. Those respondents assigned to the treatment list were no more or less likely to report individual vote buying and only marginally more likely to indicate neighborhood vote buying (neither difference reaches conventional levels of statistical significance).

TABLE 3 Reports of Vote Buying in Important Subpopulations

	Subgroup	Proportion of the Sample	List-Experiment Estimate	Individual Estimate	Neighborhood Estimate
Political	Voted	63.4%	22.9%(8.9)**	2.7% (0.8)††	20.0% (2.1)
	Abstained	36.6%	27.5%(10.7)**	1.9% (0.8)††	14.0% (2.1)
	Support FSLN	40.1%	20.9%(10.8)*	4.5% (1.2)	18.2% (2.7)
	Support PLC	20.8%	32.6%(15.6)**	1.0% (0.7)††	24.8% (3.4)
	Independents	37.6%	26.2%(10.2)**	1.1% (0.6)††	13.1% (2.1)
Welfare and Public Works	Welfare	7.5%	10.0%(27.9)	18.9% (5.2)	27.0% (5.2)
	No welfare	92.5%	24.7%(5.8)***	1.1% (0.4)†††	17.1% (1.8)
	Public works	40.8%	22.0%(9.4)**	4.2% (1.1)†	24.4% (2.8)
	No public works	59.2%	24.6%(8.3)***	1.2% (0.6)†††	13.6% (1.8)
Citizen Power Councils	Ever attended	18.3%	48.3%(16.5)***	4.9% (1.8)†††	19.8% (3.8)§
	Never attended	81.7%	19.0%(7.0)***	1.8% (0.6)††	17.4% (1.8)
Monitor	Can monitor	35.7%	49.2%(11.4)***	1.4% (0.6)†††	19.8% (2.7)§§
	Cannot monitor	55.2%	6.3%(8.5)	2.9% (0.8)	16.0% (2.2)
	Don't know	9.1%	40.2%(21.1)*	3.3% (1.8)†	20.9% (4.4)
Income	Very Poor	20.8%	23.1%(15.4)	4.3% (1.4)	16.3% (3.0)
	Poor	46.0%	26.3%(10.2)**	2.4% (0.7)††	17.2% (2.5)
	Middle/Upper	26.2%	14.2%(14.0)	1.5% (0.9)	22.2% (3.1)
	Income No Answer	6.9%	52.1%(25.4)**	0.0% (0.0)††	10.0% (3.9)
Education	No education	9.5%	16.1%(18.0)	3.1% (1.7)	15.1% (4.7)
	Primary	34.0%	20.9%(10.8)*	3.5% (1.1)	15.8% (2.3)
	Secondary	42.2%	26.7%(10.1)**	1.4% (0.6)††	20.0% (2.5)
	University	14.3%	37.4%(19.3)*	2.1% (1.2)†	18.1% (4.2)
Age	18–29	36.1%	7.1%(10.1)	3.3% (1.2)	20.3% (3.3)
	30–49	38.8%	29.7%(10.8)***	1.5% (0.6)†††	17.2% (2.3)
	50 or more	25.1%	41.3%(13.1)***	2.4% (0.9)†††	15.3% (2.3)§
Gender	Female	50.5%	33.2%(10.5)***	2.2% (0.7)†††	15.5% (2.1)§
	Male	49.5%	15.3%(9.0)*	2.6% (0.7)	20.2% (2.1)
Zone	Rural	36.9%	25.5%(8.0)***	3.0% (1.0)†††	18.9% (3.3)
	Urban	63.1%	23.6%(7.5)***	2.1% (0.7)†††	17.2% (2.1)

Notes: Linearized standard errors are in parentheses and are adjusted for the survey design.
* Denotes the statistical significance of the estimated difference between treatment and control groups according to the list experiment (*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$).
† Denotes the statistical significance of the difference between the estimated proportion of vote buying according to the list experiment and the proportion of respondents directly reporting individual gifts († $p<0.1$, †† $p<0.05$, ††† $p<0.01$).
§ Denotes the statistical significance of the difference between the estimated proportion of vote buying according to the list experiment and the proportion of respondents directly reporting vote buying in their neighborhood (§ $p<0.1$, §§ $p<0.05$).

the point estimate for the wealthiest income group (more than $200 USD per month) hints of some drop-off at higher levels of income, the size of the standard errors for the income subgroups suggests that there is no statistical difference between the three groups.[34]

Two differences between subgroups are statistically significant and worthy of note. First, people who believe ballots are secret are very unlikely to report vote buying (6% with s.e. = 8.5 percentage points), while nearly half

[34] Changing the coding of the income categories (i.e., middle or upper class) does not change this conclusion. Point estimates for more restricted ranges of people at the top of the income distribution do not suggest that higher-income respondents are less likely to receive gifts or favors from parties. A final interpretation for the surprising result that vote buying is not more prevalent among the poorest citizens is that the very poor are located in areas that are difficult to reach. As a result, parties choose to target more geographically proximate voters in the urban working class sectors, who in a country like Nicaragua are still sufficiently poor in absolute terms so as to make their votes relatively cheap. However, we do not find strong evidence that vote buying is less prevalent in rural areas. We are indebted to Ana de la O for pointing out this interpretation.

of those respondents with doubts about the secrecy of the ballot report vote buying (s.e. = 9.6 percentage points). The second notable difference is that respondents who ever attended CPC meetings were far more likely (48%) to report vote buying than those who never attended (19%). This difference confirms our observation that the CPCs play an important role in the distribution of clientelistic goods. CPC meetings could be used to transfer goods, but the mechanism could also be persuading individuals that the party can monitor ballots.[35] These findings provide the first systematic piece of evidence in support of scholars who hypothesize that parties either pinpoint people who think monitoring is possible or convince targeted individuals of the party's ability to monitor the vote. Thus, parties can solve the commitment dilemma inherent to vote-buying exchanges during electoral processes that are secret without the use of widespread coercive practices (Auyero 2001; Levitsky 2003; Stokes 2005). In the presence of well-articulated clientelistic machines, theories of vote-buying dynamics should not rule out the ability of parties to monitor or credibly threaten noncompliant voters (cf. Nichter 2008).

Analyzing the subgroups using the two obtrusive measures (individual and neighborhood levels) highlights the degree to which direct measures can understate the incidence of vote buying. In nearly every category the individual-level question estimates only a small fraction of the clientelistic exchanges detected by the list experiment (see Table 3, column 5).

The neighborhood measure fares a little better in terms of the relative magnitudes of the proportions but consistently produces estimates below those generated by the list experiment (Table 3, column 6), suggesting that Brusco, Nazareno, and Stokes's (2004) rough guess that the true level of vote buying may fall into the range bounded by the individual and neighborhood items may still understate levels of vote buying, at least in the Nicaraguan context. One may be tempted to conclude that asking about vote buying in neighborhoods is a viable alternative for overcoming social desirability bias than the question focused on the individual. However, not only are there validity questions associated with using the neighborhood item as a proxy, but the neighborhood question also fails to detect differences between subgroups suggested by the list experiment. The difference in CPC meeting attendance, certainty about ballot secrecy, and the positive age association are not present when using the neighborhood question. Thus, it is not clear that asking about vote buying in neighborhoods yields superior estimates to the individual-level question.[36]

The Reliability of Models Using Traditional Obtrusive Measures

If measurement error in the individual and neighborhood vote-buying measures is truly random, then only the estimates of the average rate of vote buying will be biased and researchers can successfully model whom parties target for gifts and favors. The deviations from the list experiment in Tables 2 and 3 suggest the measurement error is probably correlated with important observed and unobserved causes of clientelistic relationships. To evaluate this claim more rigorously, we conducted multivariate models predicting vote buying using the individual, neighborhood, and list-experiment measures as dependent variables (Table 4). Since the individual and neighborhood items are binary, Models 1–4 are logistic regressions. In order to generate comparable estimates with the list experiment, ordinary least squares regressions were estimated using the list-experiment counts as the dependent variable (Models 5 and 6).[37] In addition to the independent variables included in the other models, in Models 5 and 6 we included a dummy variable indicating the list-experiment condition along with interactions between the list-experiment condition and all the independent variables. These interactions test the magnitude of

[35] CPCs play a strategic role in monitoring political loyalty. According to Anderson and Dodd (2009), CPCs are parallel local institutions created and financed by Ortega, and overseen by his wife, that try to undermine the independence of local governments by delivering goods and services conditioned on political support for the president. Scholars have warned that the government uses these organizations politically and that under the pretext of fighting poverty, the Sandinista administration is creating a "captive electorate," jeopardizing citizen autonomy by means of political monitoring (e.g., Marti I Puig 2009).

[36] The validity of the neighborhood measure is further called into question when direct individual-level vote buying and list-experiment estimates are tabulated by responses to the neighborhood question. Nearly every individual reporting that she received a gift or favor also reported vote buying in the neighborhood (only 0.5% did not). However, among people reporting vote buying in the neighborhood, only 53% received a gift themselves according to the list experiment. That is, many of these people were reporting on the behavior of neighbors and not themselves. Among those people claiming that vote buying did not occur in their neighborhood, nearly 18% received a gift from a party. Further, responses to the neighborhood-level question provide direct evidence that respondents sought to strategically implicate the opposing party in vote buying. PLC sympathizers overwhelmingly (85%) pointed to the FSLN as the party giving out gifts in their neighborhood, while only 11% of FSLN sympathizers pointed toward the PLC.

[37] Results are virtually identical when using count models (poisson and negative binomial regressions). For ease of interpretation, OLS estimates are reported.

TABLE 4 Comparing Vote-Buying Models Using Direct Measures and the List Experiment

	Individual		Neighborhood		List Experiment	
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**
Supporter of FSLN (0–1)	1.398**	0.935	0.328	0.187	−0.012	−0.053
	(0.64)	(0.734)	(0.216)	(0.222)	(0.166)	(0.173)
Supporter of PLC (0–1)	−0.144	−0.303	0.750***	0.746***	0.065	0.018
	(0.925)	(1.04)	(0.243)	(0.254)	(0.191)	(0.204)
Very Poor (0–1)	1.174	1.215*	0.479	0.483	−0.259	−0.214
	(0.737)	(0.711)	(0.49)	(0.518)	(0.342)	(0.331)
Poor (0–1)	0.702	1.043	0.511	0.584	−0.275	−0.174
	(0.691)	(0.656)	(0.467)	(0.49)	(0.306)	(0.288)
Middle/Upper Income (0–1)			0.791	0.862*	−0.393	−0.205
			(0.478)	(0.509)	(0.299)	(0.288)
Education (0–3)	−0.327	−0.51	0.041	0.049	0.152*	0.181*
	(0.354)	(0.414)	(0.137)	(0.13)	(0.087)	(0.092)
Age (1–3)	−0.366	−0.432	−0.151	−0.136	0.208**	0.250**
	(0.395)	(0.432)	(0.135)	(0.139)	(0.103)	(0.108)
Female (0–1)	−0.304	−0.723	−0.317*	−0.349**	0.177	0.176
	(0.361)	(0.435)	(0.162)	(0.166)	(0.165)	(0.163)
Monitor (yes or DK, 0–1)		−0.578		0.303		0.323**
		(0.467)		(0.209)		(0.143)
Welfare (0–1)		2.917***		0.402		−0.051
		(0.444)		(0.316)		(0.306)
Public Works (0–1)		0.562		0.677***		−0.036
		(0.558)		(0.2)		(0.135)
Citizen Power Council (0–1)		0.621		0.107		0.287
		(0.459)		(0.232)		(0.197)
Urban (0–1)		0.656		−0.158		−0.187
		(0.609)		(0.248)		(0.129)
Constant	−3.861**	−4.581***	−2.033***	−2.459***	1.687***	1.760***
	(1.497)	(1.648)	(0.611)	(0.667)	(0.318)	(0.325)
N	1003	993	998	989	995	986
Pseudo/Adjusted R^2	0.086	0.281	0.025	0.051	0.043	0.069

Notes: *p < 0.1, **p < 0.05, ***p < 0.01.
Models 1–4 are logistic regressions. For income, "no answer/refused" is the excluded category, except for Models 1–2, which also exclude the middle/upper-income group, since zero respondents who refused the income item admitted to vote buying at the individual level, thereby necessitating the exclusion of two categories for estimation purposes. Models 5–6 are OLS regressions with the list-experiment count as the dependent variable. For ease of interpretation, all coefficients for Models 5–6 are interactions between the independent variable and the list-experiment condition (the treatment), since these provide the most comparable estimates. The coefficients for the main independent variables, which predict the number of other items on the list, are omitted but are available upon request.

adding the vote-buying item to the list for each independent variable; that is, the coefficients for these variables can be interpreted as comparable to the independent variables in Models 1–4 (Coutts and Jann forthcoming, 23; Holbrook and Krosnick 2010, 53–55). For ease of comparison, only the interacted independent variables are displayed, while the noninteracted independent variables are omitted.[38]

[38] These latter variables predict the mean number of list items aside from the vote-buying item, so these coefficients are less central to the analysis. A common misconception is that list-experiment

Models 1, 3, and 5 use basic explanatory variables of vote buying, similar to the models used in Stokes (2005). Models 2, 4, and 6 use a more complete set of covariates the literature suggests should matter.[39] The results from Model 1 strongly suggest that vote buying is rare but primarily targets FSLN supporters. Adding the controls in Model 2 muddies the picture somewhat, but the reduced point estimates and increased standard errors are probably due to collinearity between support for FSLN, poverty, participation in welfare programs, and attending CPC meetings. The ultimate conclusion reached using the individual-level measure of vote buying would be that vote buying primarily targets poor Sandinista sympathizers.

Shifting to the neighborhood measure of clientelistic behavior in Model 3 almost directly contradicts the individual-level regression in Model 1 since PLC supporters are more likely to report vote-buying activity. It also appears that wealthier neighborhoods and males are marginally more likely to be targeted by vote-buying campaigns, whereas the individual-level model hinted that poorer people received gifts or favors in exchange for votes. Adding the covariates in Model 4 does little to change the point estimates from Model 3, and in contrast to Model 2, the public works dummy is now highly significant, and the welfare coefficient is much smaller and not significant.

Not only do the individual and neighborhood models lead to contradictory results, but the models using list-experiment data also paint a very different picture of the dispensation of gifts in Nicaragua. In contrast to the direct models, both Models 5 and 6 suggest that gifts are given to all types of partisans and that income is not very predictive of the receipt of gifts. Furthermore, both Models 5 and 6 point toward age as an important positive factor in vote buying, while the direct models produced nonsignificant negative coefficients. Finally, education generates a positive coefficient that is marginally significant in Models 5 and 6, which contrasts with the negative coefficients in Models 1 and 2 and positive coefficients in Model 4 (none significant). While this marginally significant result is unexpected, it may reflect the fact that, controlling for other factors, the more educated are more likely to be involved in politics and therefore more likely to be exposed to political tactics such as vote buying. The difference between the list experiment and direct question could be due to increased sensitivity to social desirability pressures with regard to elections by the more educated.[40]

Theoretically, one of the most important findings in Model 6 is that doubt about the secrecy of the ballot is a very strong predictor of vote buying; on average over 32% of those who think that their vote can be monitored or express doubts about ballot secrecy received a gift or favor. In stark contrast, the direct models produced nonsignificant results, with Model 2 producing a *negative* coefficient and Model 4 a *positive* coefficient for the monitor variable. Although the coefficient for the CPC attendance variable is substantively large and consistent with field research, its collinearity with several other variables in the model probably accounts for its lack of statistical significance in Model 6. While both Models 2 and 4 produce similarly positive nonsignificant results for the CPC variable, only in the individual-level model is the coefficient substantively large (0.6 logits versus 0.1 logits).

Consequently, researchers should be very cautious when constructing models relying on obtrusive measures of vote buying. Not only do the models underestimate the extent to which vote buying occurs, but also the results are highly dependent on model specification. Signs flipped for important variables such as party affiliation, income, and whether voters think they can be monitored. The models using direct measures also failed to uncover factors that the list experiment, theory, and fieldwork suggest are important. Thus, the measurement error from social desirability bias in obtrusive measures of vote buying does not appear to be purely random and biases results.

Discussion

This article has shown that list experiments offer a technique that solves some bias issues by offering

estimates cannot be used in multivariate analyses, since the researcher does not know which particular individuals respond to which items on the list (e.g., Kuklinski et al. 1997). While it is true that researchers cannot make such individual-level inferences, subgroup-level inferences with controls are possible. Thus, the interaction coefficients in Table 4 represent the group-level averages (similar to Table 2) while at the same time controlling for the overlap among groups.

[39] In order to avoid dropping respondents who refused to answer the income question since such respondents are also likely to edit their answers regarding the sensitive vote-buying items (see Table 3), income dummy variables are included. In Models 3–6, those who refused to answer the income question serve as the excluded baseline category, while in Models 1–2 the middle/upper-income dummy was also excluded, since zero respondents who refused the income item indicated personally receiving a gift, thereby complicating estimation.

[40] An alternative explanation is that more educated respondents feel they should report greater amounts of political participation, so it is possible that the list experiment is biased for educated respondents. The existing literature on list experiments provides no evidence for this thesis, but it is possible.

respondents a high degree of anonymity when answering questions about vote buying. However, the list experiment is unlikely to purge all social desirability bias from responses and the point estimate is likely to constitute a lower bound. Vote buying may be such a sensitive topic that some respondents may be reluctant to "tell the truth" even when provided a forum where it is literally impossible to determine which individuals report vote buying.[41] This greater sensitivity to stigma is likely to exist in contexts where social sanctions, political violence, or legal punishment are possible outcomes from admissions of vote buying.

This inability to identify particular persons admitting to stigmatized behaviors does not prevent researchers from conducting subgroup analysis, even using multivariate techniques. Within each subgroup, respondents are randomly assigned to treatment and control lists, so researchers can understand the *average* response from categories of people. Such estimation techniques will be statistically inefficient, but they will have lessened social desirability bias and allow researchers to map the contours of vote buying. The greater efficiency gained by using direct questions about individual- and neighborhood-level vote buying comes at the cost of underestimating aggregate-level gift dispensation and possibly misleading inferences concerning the targeting of such goods and favors. This bias-efficiency trade-off suggests that researchers desiring accurate models of whom campaigns target with vote buying should ideally conduct list experiments on very large samples.

Nicaragua is the second poorest country in Latin America and political parties across the ideological spectrum have recently demonstrated high levels of vote-buying activities. Given these characteristics, we are confident that the extent of vote buying is clearly underestimated by direct measures and that the list experiment provides a more accurate assessment at the aggregate level. However, our results should not be taken to imply that vote buying is ubiquitous in Latin America. Low reported levels of vote buying using traditional surveys may reflect a genuine lack of illicit campaign activity. Our point is that social desirability bias is an alternative explanation that is not easily dismissed.

In order to gain greater confidence in the external validity of the results yielded by the list experiment in Nicaragua, more explicit comparative research using list experiments is necessary in the developing world. One line of future research is to identify the conditions that increase or decrease the impact of social desirability bias. Our article shows that the difference in reporting between obtrusive and unobtrusive measures of vote buying is statistically significant among females, older voters, better-educated individuals, those living in rural and urban areas, and middle-income voters. This evidence suggests that poorer voters may be more inclined to regard vote buying as "politics as usual," as an inherent part of their social and political problem-solving networks (Auyero 2001). As a result, the poor may be less inclined to lie about receiving gifts or favors during campaigns. The nature and shape of the stigma associated with vote buying is a topic worth understanding in greater depth since it will help to guide attempts to curb the practice.

Our research uncovered two key mechanisms for vote-buying linkages in the Nicaraguan context: (1) voters' perception that parties can monitor their vote and (2) voters' participation or proximity to state-sponsored community organizations like the CPCs. The latter mechanism could be a subtype of the first one since these institutionalized interactions between voters and the state via the councils can offer politicians an infrastructure of surveillance different from that offered by partisan electoral machines. The literature relying on quantitative analyses of survey data has more often than not incorporated successful monitoring efforts in the formal models that inspire that research, but it has rarely tested these assumptions systematically. In this article, we offer systematic evidence that shows that these theoretical intuitions are correct and should be at the forefront of future research on vote buying in Latin America and elsewhere.

[41] The exceptions to this rule are respondents who report the highest possible value in the treatment condition. For this reason, low-incidence categories should be included in the control list.

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