

Vote Buying and Social Desirability Bias: Experimental Evidence from Nicaragua

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Abstract: Qualitative studies of vote buying generally find the practice to be common in many countries, but quantitative studies using surveys often find little evidence of vote buying. We argue that social desirability bias accounts for this empirical discrepancy and employ a survey-based list experiment to minimize the problem. We conducted a nationally representative survey after the 2008 Nicaraguan municipal elections where we asked about vote buying behavior by campaigns using a list experiment and the questions traditionally used by studies of vote buying. Our list experiment estimated that 24% of registered voters in Nicaragua were offered a gift or service in exchange for their vote, whereas only 2% reported the behavior when asked directly. We demonstrate that the social desirability bias is non-random and that 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.

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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 non-random, then empirical results about the dynamics of vote buying derived from surveys are on a shaky foundation. This paper uses a survey experiment to minimize social desirability bias, describe who campaigns targeted with vote buying in an election, and demonstrate that this measurement error is non-random.

Although some authors have pointed to potential benefits of vote buying and clientelism in institutionalizing political systems (e.g. Huntington 1968; Philipson and Snyder 1996), the academic consensus is that the exchange of gifts and favors for votes has deleterious consequences for democracy. Vote buying (and clientelism more generally) shortens the time horizons of public policies 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

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).

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, each demonstrating both important strengths and weaknesses. 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 mobilization 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 macro-strategy.

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 percent 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). This 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 (Stokes 2005, 321 fn.20; Brusco, Nazareno and Stokes 2004, 69-72), 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 percent 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 non-random and correlated with politically important variables. This finding implies that analyses employing direct measures are likely to result in inaccurate results. Our survey also finds that political campaigns avoid targeting people who are convinced of ballot secrecy, which suggests that campaigns can target their vote buying to overcome commitment problems.

Response Bias and Vote Buying

When respondents fear expressing socially undesirable attitudes or participating in stigmatized 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 (Kuklinski, Cobb and Gilens 1997; Kuklinski et al. 1997; Berinsky 1999), religious attitudes and behaviors (Pressor and Stinson 1998; Kane, Craig and Wald 2004), sexual behavior (Tourangeau and Smith 1999), drug use (Tourangeau and Smith 1999; Bachman and O'Malley 1981) and voter turnout (Silver, Anderson, and Abramson 1986).²

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 (e.g. 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

² 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.

incidence. Consequently, even though qualitative evidence suggests that vote buying is pervasive in many developing countries, social desirability bias 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.³

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 et al. (1994, 157-168), 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, p. 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 non-systematic (i.e. random) and constant across all relevant subgroups. Measurement error that systematically varies across subgroups would bias inferences (King et al. 1994, 155-157). 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 2000), they may

³ 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.

under-report 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.⁴

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 non-random 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

⁴ This bias could also run in the opposite direction. Strong party supporters may be more reluctant to admit that they received gifts for fear of making their party look unfavorable in the eyes of the interviewer, so they may systematically under-report the level of goods that they receive at a greater rate than do the weakly opposed. Given this systematic social desirability bias, traditional analysis would lead to the incorrect conclusion that parties mainly target weakly opposed citizens rather than core supporters as part of a vote buying strategy.

⁵ Stokes and coauthors also attempt to reduce the inferential problems associated with this response bias by estimating models that use alternative clientelism items—such as asking whether the respondent has or would turn to a party operative for help—as dependent variables. While this might improve inferences, such items are less direct measures of the particular variable in question—vote buying—and assume that the same people who turn to parties for help also are given gifts in return for votes. While this might be true, this is an empirical question that is hard to test given the high likelihood of bias in the measurement of these variables.

measurement error in neighborhood estimates of vote buying behavior is correlated with politically relevant variables and can lead to biased inferences as well.

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 and attitudes toward affirmative action (Kuklinski, Cobb, and Gilens 1997; Kuklinski et al. 1997), attitudes toward presidential candidates with varying descriptive characteristics (Streb, Burrell, Frederick, and Genovese 2008; Kane, Craig, and Wald 2004; Heerwig and McCabe 2009), attitudes toward the extension of suffrage in Lebanon (Corstange 2009), 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 were listed and read to respondents:

⁶ NOTE TO THE REVIEWER: Given the newness of list experiments to comparative politics, we have included a detailed reviewers' appendix discussing the validity of the technique and associated strengths and weaknesses. We complement this with the results of a validation experiment conducted in Uruguay and Honduras (See Reviewer's Appendices A, B and C). Due to space considerations and its prevalence in other areas of the discipline, we decided not to include this more detailed methodological discussion in the text of the paper. However, we will happily include the validation study as an appendix.

- 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 was shown and read a fifth category, placed in the third response position:

- they gave you a gift or did you a favor⁷

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 under-report 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 occurs, 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

⁷ The wording in Spanish was the following: “*Le voy a entregar una tarjeta donde se le van a mencionar varias actividades, y quisiera que me señale si fueron realizadas por los candidatos y activistas durante la última campaña electoral. Por favor, no me diga cuáles sino solamente CUÁNTAS.*” The response categories were, “*Colocaron carteles/afiches de campaña en su barrio/ciudad,*” “*Visitaron su hogar,*” “*Le hicieron un regalo o favor,*” “*Transmitieron publicidad de campaña por televisión o radio,*” and “*Lo/la amenazaron para que votara por ellos.*” 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 5 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.

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 percent 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 sub-groups of the overall sample (e.g. partisan, socio-economic, 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.

An Empirical Test

The Nicaraguan Case

The central aim of this paper 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 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

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 1985, through competitive elections, Ortega became President for the period 1985-1990, and more recently, in 2006, he was re-elected.

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

Although the 2008 municipal elections were not heavily contested in all parts of the country, 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.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 (1939-1979), Nicaraguans’ experienced the establishment of a patrimonial state in which the distinction between public and partisan realms was blurred.¹³ This longstanding tradition of using state resources to oil political machines enables the capture of

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

¹⁰ After the 2006 Presidential Election, and with the return of Montealegre to the PLC, the other rightist party, 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 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.

state bureaucracies to reward political allies and to funnel resources to the government's electoral base in contemporary Nicaraguan politics (Marenco 2004).¹⁴

The culture of political corruption can manifest itself in explicit agreements among political parties. For example, a 2000 bipartisan deal between PLC's Alemán and FSLN's Ortega, known as *El Pacto*, reformed the electoral laws to give the two dominant parties an advantage over their smaller competitors and served as a mutual guarantee of impunity in future cases of uncovered corruption.¹⁵ Dye and Close (2004) understand the primary aim of *El Pacto* to be improving the main parties' access to state resources for the distribution of clientelistic goods and for nurturing their political bases.

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 post-ballot forms of electoral corruption. Second, vote buying could be a logical extension of goods and services provided in non-electoral time periods. The vote buying immediately prior to the election might

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 *El Pacto* lowered the threshold for winning the Presidency to 40% (or 35% if the distance between the two frontrunners is more than 5 points). The new rules of the game paved the way for an alternation in power in 2006, when the FSLN won the presidency with a plurality in the first round; before *El Pacto* this plurality would not have been enough to avoid a run-off election (Close and Deonandan 2004; Marti I Puig 2008)..

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,¹⁶ enforcement is non-existent and vote buying has become a common mobilization 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 sub-national 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.¹⁷ In this respect, Nicaraguan politics is different from political practices in countries where vote buying is

¹⁶ 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, 19th, 2000), Title XIV, Unique Chapter on Electoral Crimes, Article 174; translation is ours)

¹⁷ 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.

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 found in research on the lasting effect of the Sandinista revolution on political attitudes and behaviors challenges the claim of widespread vote buying, indicating that the revolutionary movement that overthrew Somoza created a context unfavorable to these 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 non-revolutionary 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). This trend may be truer at the national level than at the local level (Anderson and Dodd 2009). Since our study was conducted during a municipal election, the discovery of vote buying would contribute empirical evidence to the ongoing debate on the quality of democracy at the local level in Nicaragua (see Anderson and Dodd 2010; Greene 2010). Given this grassroots nature of the Nicaraguan political culture, these authors would also 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).¹⁸ The first question directly asked about the likelihood that survey respondents would admit to receiving a gift.¹⁹ The second question sought to measure the stigma associated with vote buying by asking about the acceptability of a hypothetical patron-client exchange.²⁰

<Table 1 about here>

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 percent 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 percent thought it would be very or somewhat likely that a person would admit to selling their vote on a survey. Results are similar when respondents were asked to evaluate the behavior of a hypothetical

¹⁸ The omnibus, face-to-face survey was conducted by *Borge y Asociados* from September 28-October 8, 2010, and included 1008 respondents. The survey utilized a multistage random sample with 84 final sampling points (segments), including 12 respondents per segment. Sampling proceeded as follows: The sampling frame consisted of the electoral registry, with primary sampling units chosen proportionate to the size of voting centers within department – municipalities. Within municipalities, random selection proceeded by electoral centers, census tracks, and census blocks, with final sampling points (segments or blocks) containing 12 respondents. Households and respondents within households were chosen randomly in such a way that ensured gender balance. The AAPOR response rate 3 was 57%.

¹⁹ “If a person received a gift in exchange for his/her vote in an electoral campaign, how likely or unlikely would it be that this person would admit to having received a gift in exchange for his/her vote if they were asked about it on a survey? Would it be very likely, somewhat likely, not very likely, or not at all likely?” In Spanish: “*Si una persona recibió un regalo a cambio de su voto en la elección pasada, ¿qué tan probable o improbable sería que esa persona admita haber recibido un regalo a cambio de su voto si se le pregunta en una encuesta? ¿Sería muy probable, algo probable, no muy probable o nada probable?*”

²⁰ “Imagine a man that lives with his wife and two children in [this locality]. During an electoral campaign, a member of a party offered him 300 Cordoba Oros [Approximately \$15 USD] to vote for the party. The man accepted the money and voted as he was asked. In your opinion, was the behavior of this man totally acceptable, acceptable, understandable by unacceptable, unacceptable or totally unacceptable?” In Spanish: “*Imagínese a un hombre que vive con su esposa y sus dos hijos en (esta localidad). Durante la campaña electoral, un miembro de un partido le ofreció 300 Córdoba Oros para que vote por un partido. El hombre aceptó el dinero y votó como se lo pidieron. En su opinión, el comportamiento de ese hombre fue totalmente aceptable, aceptable, entendible pero inaceptable, inaceptable, totalmente inaceptable.*”

patron-client exchange. Over 75 percent of respondents thought that the hypothetical exchange was unacceptable or totally unacceptable, while only 8 percent thought that the situation was acceptable or totally acceptable. These two items demonstrate that there is a strong normative stigma attached to vote buying in the context of surveys conducted in Nicaragua. This finding suggests that social desirability bias can be strong even in a country in which clientelism is a deeply embedded practice.²¹

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 22nd 2008 visited the *Asentamiento Hugo Chávez Frías*, one of the many shantytowns surrounding Managua, and delivered precisely nine wheel chairs to the nine handicapped people in the neighborhood.²² 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.²³ These kinds of goods are clearly considered part of a political transaction. According to a Nicaraguan social researcher, "construction materials, cattle, money,

²¹ 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 31st 2008).

²² Field notes, October 2008.

²³ Interview (October 2008).

and medicine are traditionally the most frequent goods delivered in electoral campaigns in exchange for votes.”²⁴

More recently, the Sandinistas have taken advantage of government-sponsored (and Sandinista controlled) *Consejos de Poder Ciudadano* (Citizen Power Councils, CPC) for clientelistic purposes.²⁵ 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).²⁶ 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 perceive 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 non-compliance by gift recipients less likely. Armed with knowledgeable local brokers, political campaigns may be able to preferentially target individuals with doubts regarding vote secrecy and not waste resources on people who are convinced of the party’s inability to monitor ballots cast.²⁷

²⁴ In contrast, the possibility of getting a job is perceived “as a normal reward for their collaboration during campaigns.” Interview (October 2008).

²⁵ 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. They are modeled on similar organizations promoted by Venezuelan President Hugo Chavez. However, political opponents consider them as the re-edition of the repressive and control organizations that existed during the first Sandinista Government (1979-1990).

²⁶ According to *Envío* (October, 2008; Report 309), the CPCs have “two faces.” First, these organizations recruit and mobilize people that 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 house building 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.

²⁷ 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.

Survey

We conducted a nationally representative face-to-face survey between November 25th to December 8th, 2008, two weeks after the municipal elections.²⁸ 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.²⁹ The AAPOR response rate 3 for the survey conducted by *Borge y Asociados* was 77 percent and resulted in 1008 respondents. Randomization for the list experiment occurred at the individual level (Reviewer's Appendix B contains the text and question order from the survey).

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 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.³⁰ Follow up questions asked those who responded

²⁸ 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 of 2009. Thus, the two autonomous regions on the Atlantic coast (representing 10% of the Nicaraguan population) were excluded from the sample.

²⁹ For this survey, sampling proceeded in the same way as in the survey described above. See ft. 18.

³⁰ 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 (discussed below) 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 percent of the items delivered, 6 percent reported parties handing out money, and a miscellaneous group of gifts represents another 5 percent. Only 12 percent 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. This data indicates that: a) parties deliver a broad variety of goods targeting particular needs; 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

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)³¹ (e.g. Kitschelt and Wilkinson 2007; Calvo and Murillo 2004), 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 (Stokes 2005; Kitschelt 2000). 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

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.

³¹ Income is measured using dummy variables so that respondents for whom we have missing income data (7 percent 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 primary education incomplete) to 3 (post secondary 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.

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.³² 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 CPC. 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

³² 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."

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.³³ The first column of Table 2 reports 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 assures 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 percent (s.e.=5.5 percentage points).³⁴

<Table 2 about here>

This number is not only statistically significant, but very different than when asking respondents about vote buying directly. Only 2.4 percent 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.³⁵ The aggregate results are somewhat better when relying on the neighborhood vote buying item, which indicates that 17.7 percent 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).

³³ The balance between treatment and control groups across relevant demographic variables suggests that the randomization procedure worked properly. See Appendix A for more information.

³⁴ For the entire analysis, standard errors are linearized and adjusted for clustering in the survey design.

³⁵ 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).

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 (2008) 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.

<Table 3 about here>

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 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.³⁶

Three 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 percent with s.e.=8.5 percentage points), while nearly half 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 percent) to report vote buying than those who never attended (19 percent). 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.³⁷ 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

³⁶ Changing the coding of the income categories in order to better capture who would be considered middle class or upper class does not change the conclusion, as 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.

³⁷ 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).

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 deem it implausible that parties can actually monitor or credibly threaten non-compliant voters (cf. Nichter 2008). Finally, a strong positive relationship between age and the dispensation of gifts is evident in the list experiment data and nonexistent in the other data, suggesting that older Nicaraguans are more likely to be targeted than their younger counterparts.

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' (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.³⁸

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 who 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).³⁹ 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

³⁸ 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 percent did not). However, among people reporting vote buying in the neighborhood, only 53 percent 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 percent 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 percent) pointed to the FSLN as the party giving out gifts in their neighborhood, while only 11% of FSLN sympathizers pointed toward the PLC. Thus, social desirability bias may not be completely purged by asking about the neighborhood rather than the individual, and overall the neighborhood item serves as a poor proxy.

³⁹ Results are virtually identical when using count models (poisson and negative binomial regressions). For ease of interpretation, OLS estimates are reported.

the magnitude of 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 main effects of 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 non-interacted independent variables are omitted.⁴⁰

<Table 4 about here>

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. 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.

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 is probably due to collinearity between support for FSLN, poverty, participation in welfare programs, and attending CPC meetings. The

⁴⁰ 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. The full regression results are available upon request. A common misconception is that list experiment 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. See Holbrook and Krosnick (2010, 53-55) for a more detailed explanation.

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 non-significant 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 regards to elections by the more educated.⁴¹

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 percent 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 non-significant 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 non-significant 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 under-estimate the extent to which vote buying occurs, but 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 field work 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.

⁴¹ 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.

Discussion

This paper has shown that list experiments offer a technique that solves some bias issues by offering 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.⁴² 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 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 who 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

⁴² 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.

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.⁴³

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 paper 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) voter’s perception that parties can monitor their vote and 2) voter’s participation or

⁴³ Our survey in Nicaragua attempted to measure goods and services offered by parties immediately prior to the election in exchange for votes. The survey question fielded would not inform researchers of gifts provided at other times of the year. Similarly, other forms of corruption – electoral and otherwise – are not measured by our survey (though list could be used to answer such questions). The broader culture of corruption may be correlated with our measure of vote buying in Nicaragua, but further studies would need to be conducted to directly measure these activities.

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 ethnographic literature on clientelism has described the workings of monitoring strategies and has documented the way citizens perceive them (e.g. Auyero 2001). 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 has rarely tested these assumptions systematically. In this paper 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. Research on monitoring would help researchers to address crucial questions about the relevance of trust and reciprocity in a one-shot political exchange (Kitschelt 2000) and the threat of sanctions in long term clientelistic interactions (Magaloni 2006).

Unfortunately, unobtrusive survey techniques like the list experiment cannot be used to answer all questions regarding vote buying. “Does vote buying change how people vote?” is one of the most relevant puzzles in the literature on non-programmatic mobilization (Schaffer 2007). Answering this question would imply providing evidence that the dispensation of material goods in exchange for the vote is capable of distorting a voter’s preferences, leading him or her to cast the ballot in a way that s/he would not have done otherwise. However, survey experiments cannot adequately address the topic since only the question wording or response categories are being randomly manipulated. Such techniques could be used to make respondents comfortable to report the receipt of such gifts or providing reasons for their vote choices, but the researcher would be assuming that survey respondents can accurately infer and report their reasons for

behavior, which can be a heroic assumption in campaign settings (Gerber and Green 2000). The best way to estimate the effect of vote buying is to randomly manipulate the practice itself. Given the logistical, legal and ethical hurdles, randomized field experiments of vote buying are unlikely to be conducted in the near future. That said, clever researchers may be able to apply the principles of randomized experiments to closely related behaviors. For instance, experiments have been conducted on turnout buying (Panagopoulos 2009) and the clientelistic content in stump speeches (Wantchekon 2003). However, much as traditional surveys are invaluable tools in understanding campaign dynamics and strategies, list experiments can help to provide such insight into stigmatized political practices.

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[March 17 2010]

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%
Don't Know / No Answer	8.8%	Totally Unacceptable	36.50%
N	1008		1008

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	24.44 % (5.52)	2.39 %*** (0.58)	17.83 % (1.8)
N	995	1003	998

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.

Table 3: Reports of Vote Buying in Important Sub-Populations

	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	33.6%	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)***	1.1% (0.6) ^{†††}	13.6% (1.8)
Citizen Power Councils	Ever attended	18.3%	48.3% (16.5)***	4.9% (1.7) ^{†††}	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% (4.6)
	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	14.2%	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)

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 (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$).
 † 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 († $p < 0.1$, †† $p < 0.05$, ††† $p < 0.001$).
 § 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 (§ $p < 0.1$, §§ $p < 0.05$).

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** (0.64)	0.935 (0.734)	0.328 (0.216)	0.187 (0.222)	-0.012 (0.166)	-0.053 (0.173)
Supporter of PLC (0-1)	-0.144 (0.925)	-0.303 (1.04)	0.750*** (0.243)	0.746*** (0.254)	0.065 (0.191)	0.018 (0.204)
Very Poor (0-1)	1.174 (0.737)	1.215* (0.711)	0.479 (0.49)	0.483 (0.518)	-0.259 (0.342)	-0.214 (0.331)
Poor (0-1)	0.702 (0.691)	1.043 (0.656)	0.511 (0.467)	0.584 (0.49)	-0.275 (0.306)	-0.174 (0.288)
Middle/Upper Income (0-1)			0.791 (0.478)	0.862* (0.509)	-0.393 (0.299)	-0.205 (0.288)
Education (0-3)	-0.327 (0.354)	-0.51 (0.414)	0.041 (0.137)	0.049 (0.13)	0.152* (0.087)	0.181* (0.092)
Age (1-3)	-0.366 (0.395)	-0.432 (0.432)	-0.151 (0.135)	-0.136 (0.139)	0.208** (0.103)	0.250** (0.108)
Female (0-1)	-0.304 (0.361)	-0.723 (0.435)	-0.317* (0.162)	-0.349** (0.166)	0.177 (0.165)	0.176 (0.163)
Monitor (yes or DK, 0-1)		-0.578 (0.467)		0.303 (0.209)		0.323** (0.143)
Welfare (0-1)		2.917*** (0.444)		0.402 (0.316)		-0.051 (0.306)
Public Works (0-1)		0.562 (0.558)		0.677*** (0.2)		-0.036 (0.135)
Citizen Power Council (0-1)		0.621 (0.459)		0.107 (0.232)		0.287 (0.197)
Urban (0-1)		0.656 (0.609)		-0.158 (0.248)		-0.187 (0.129)
Constant	-3.861** (1.497)	-4.581*** (1.648)	2.033*** (0.611)	-2.459*** (0.667)	1.687*** (0.318)	1.760*** (0.325)
N	1003	993	998	989	995	986
Pseudo/Adjusted R²	0.086	0.281	0.025	0.051	0.027	0.043

* 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 excludes the middle/upper income group, since 0 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.

Reviewer's Appendix A: Randomization Analysis

The randomization analysis indicates that in the overall the treatment and control groups are well balanced across demographics. The first two pairs of columns in Table A report the number of observations and the means of covariates. The T-test in Table A shows that the means of the both groups are indistinguishable from each other. Table B reports the results of individual regressions of each covariate on receiving the treatment, and the p-values indicate that none of the covariates is a predictor of the treatment.

Figure A. Balance between Treatment and Control groups

Variable	Control group (C)		Treatment group (T)		Difference of means C - T	T-test H ₀ : diff = 0		
	Obs.	Mean	Obs.	Mean		H _a : diff<0	H _a : diff not=0	H _a : diff>0
Very Poor	508	0.071	500	0.068	0.069	0.571	0.858	0.429
Poor	508	0.209	500	0.208	0.001	0.510	0.979	0.490
Middle- Upper Income	508	0.469	500	0.452	0.017	0.700	0.560	0.300
High Income	508	0.252	500	0.272	-0.020	0.235	0.470	0.765
Education	508	1.602	500	1.622	-0.017	0.356	0.712	0.644
Age	508	1.882	500	1.898	-0.016	0.371	0.742	0.629
Female	508	0.506	500	0.504	0.002	0.524	0.952	0.476
Urban	508	0.623	500	0.632	-0.002	0.473	0.946	0.527

Figure B. Logistic regressions on Treatment *

	Logit Coefficient	Standard Error	p-value
Very Poor	-0.044	0.188	0.814
Poor	-0.004	0.149	0.979
Middle-Upper Income	-0.066	0.117	0.571
High Income	0.104	0.135	0.446
Education	0.028	0.069	0.691
Age	0.027	0.078	0.732
Female	-0.008	0.037	0.835
Urban	0.009	0.026	0.735

* Note: The intercepts are not reported

Reviewer's Appendix B: Survey Instrument

English Version

1. Are you a beneficiary of a welfare program?

Yes=1 No=2 DK=8 NA=9

2. Could you tell me if your community has benefited from a public works program such as roads, clinics, water and sanitation infrastructure, in the last 6 months?

Yes=1 No=2 DK=8 NA=9

3.1) I am 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.

- they put up campaign posters or signs in your neighborhood
- they visited your home
- they gave you a gift or did you a favor
- they placed campaign advertisements on television or radio
- they threatened you to vote for them

[Mark the number:] 0 1 2 3 4 5 DK=8 NA=9

3.2) I am 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.

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

[Mark the number:] 0 1 2 3 4 5 DK=8 NA=9

4. During the electoral campaign, did you see people from the parties or candidates giving gifts or favors in your neighborhood?

Yes=1 No=2 [Go to 9] DK=8 [Go to 9] NA=9 [Go to 9]

5. Could you please tell me what they gave?

[Record up to three]

6. Could you please tell me when did they give the gifts? [Mark all the responses]

1. After the election.
2. In the last week before the election.
3. In the last month before the election.
4. In the last six months before the election.
5. In the last year before the election

DK=8 NA=9

7. Could you please tell me which parties gave out these gifts? [Mark all the responses]

1. Alianza Partido Liberal Constitucionalista

2. Frente Sandinista de Liberación Nacional

3. Alianza Liberal Nicaragüense

4. Partido de la Resistencia Nicaragüense

5. Alternativa por el Cambio

6. Others: _____ [Write down response] DK=8 NA=9

8. Do you think that these gifts influenced the vote of the people who received them?

Yes=1 No=2 DK=8 NA=9 /____/

9. During the electoral campaign, did you receive a gift or favor from a political party or candidate?

Yes=1 No=2 [Go to 14] DK=8 [Go to 14] NA=9 [Go to 14]

10. Could you tell me what kind of gift you received?

[Write down up to three responses]

11. Could you tell me when you received these gifts? [Mark all the answers]

1. After the election.

2. In the last week before the election.

3. In the last month before the election.

4. In the last six months before the election.

5. In the last year before the election

DK=8 NA=9

12. What party gave you these gifts? [Mark all the answers]

1. Alianza Partido Liberal Constitucionalista

2. Frente Sandinista de Liberación Nacional

3. Alianza Liberal Nicaragüense

4. Partido de la Resistencia Nicaragüense

5. Alternativa por el Cambio

6. Others: _____ [Write down answer] DK=8 NA=9

13. Could you please tell me if these gifts influenced your vote?

Yes=1 No=2 I did not vote=3 DK=8 NA=9

14. Do you think that the government or political parties can find out for whom people in your community voted?

Yes=1 No=2 DK=8 NA=9

15. Could you please tell me how frequently you attend meetings of the Citizen Power Councils?

Very frequently=1 Sometimes=2 Almost Never=3 Never=4 DK=8 NA=9 /____/

16. Do you have a family member or know someone who was arrested or assassinated during the “civil war”?

No = 0 Insurrection War 1975-1979 =1 Civil War 1979–1987 = 2
Both = 3 DK= 8 NA = 9

Please answer yes or no to the following questions:

	SI	NO	NS	NC
17. Could you please tell me if in the last month, your community has been victim of harassment or violence by the police or any government official?	1	2	8	9
18. Could you please tell me if in the last month, you were a victim of harassment or violence by the police or any government official?	1	2	8	9
19. Could you please tell me if you are a member of any social movement?	1	2	8	9
20. Could you please tell me if you participated in any protest in the last year?	1	2	8	9

21. Which of these groups represents the greatest threat against the health of your community?

1. Gangs 2. Political parties 3. Social movements 4. The police 5. The government
Others: _____ [Write down response] DK= 8 NA=9

31- With which party do you currently sympathize?

Frente Sandinista de Liberación Nacional=1 Partido Liberal Constitucionalista=2
Alianza Liberal Nicaragüense=3 Movimiento de Renovación Sandinista=4
Other _____ DK/NA = 99

32- For which party did you vote for mayor in this municipality in the last election?

PLC=01 FSLN=02 Other= _____ DK/NA=99

Please tell me if you are agree or disagree with each of the following phrases:

	Agree	Disagree	DK/NA
P33. The elections for mayor were free and clean	1	2	9
P34. The Supreme Electoral Council organized these elections well	1	2	9

To wrap up we would like to ask you a few personal questions

200. [Write down the gender of the respondent]. Male: 1 Female: 2 /___/

201. How old are you? _____ /___/___/

202. What was your last full year of education?

No education: 00 Primary School : 1 2 3 4 5 6
High School: 1 2 3 4 5 6 University: 1 2 3 4 5 6
/___/___/

203. Considering your salary and other sources of income, how much does your family earn per month?

_____ /___/___/___/___/___/

204. [If the respondent does not answer to 203] How much does your family earn per month?
Up to C\$ 1,500= 1 Between C\$ 1,501 and C\$ 3,000= 2 Between C\$ 3,001 and C\$ 4,250=3
Between C\$ 4,251 and C\$ 8,500= 4 Between C\$ 8,501 and C\$ 12,750= 5 Between C\$ 12,751 and
C\$ 17,000= 6 Between C\$ 17,001 or more=7 DK/DN =9

Spanish Version

1. ¿Es Ud. beneficiario/a de algún programa de ayuda comunitaria?
Si=1 No=2 NS=8 NC=9

2. ¿Podría decirme si su comunidad fue beneficiada con algún tipo de obra pública como por ejemplo
carreteras, dispensarios o agua potable en los últimos 6 meses?
Si=1 No=2 NS=8 NC=9

3.1 Le voy a entregar una tarjeta donde se le van a mencionar varias actividades, y quisiera que me señale
si fueron realizadas por los candidatos y activistas durante la ultima campaña electoral. Por favor, no me
diga cuáles sino solamente CUÁNTAS.

-Colocaron carteles/afiches de campaña en su barrio/ciudad

-Visitaron su hogar

-Le hicieron un regalo o favor

-Transmitieron publicidad de campaña por televisión o radio

-Lo/la amenazaron para que votara por ellos

[Marcar el número de respuestas:] 0 1 2 3 4 5 NS=8 NC=9

3.2 Le voy a entregar una tarjeta donde se le van a mencionar varias actividades, y quisiera que me señale
si fueron realizadas por los candidatos y activistas durante la ultima campaña electoral. Por favor, no me
diga cuáles sino solamente CUÁNTAS.

- Colocaron carteles/afiches de campaña en su barrio/ciudad

- Visitaron su hogar

- Transmitieron publicidad de campaña por televisión o radio

- Lo/la amenazaron para que votara por ellos

[Marcar el número de respuestas:] 0 1 2 3 4 NS=8 NC=9

4. Durante la campaña electoral, ¿vio a gente de los partidos o a políticos repartiendo regalos o favores en
su barrio?

Si=1 No =2 [Saltar a 9] NS=8 [Saltar a 9] NC =9 [Saltar a 9]

5. ¿Podría decirme que es lo que repartieron?

[Registrar hasta tres respuestas]

6. ¿Podría decirme cuando los repartieron? [Marcar todas las respuestas]

Después de la elección. =1

En la última semana antes de la elección. =2

En el último mes antes de la elección=3.

En los últimos seis meses antes de la elección=4.
En el último año antes de la elección=5

NS=8 NC=9

7. ¿Podría decirme que partidos hicieron estos regalos o favores? [Marcar todas las respuestas]

Alianza Partido Liberal Constitucionalista =01

Frente Sandinista de Liberación Nacional=02

Alianza Liberal Nicaragüense =03

Partido de la Resistencia Nicaragüense =04

Alternativa por el Cambio=05

Otros: _____ [Escribir respuesta] NS=88 NC=99

8. ¿Cree Ud. que estos regalos o favores influenciaron el voto de las personas que los recibieron?

Si=1 No=2 NS=8 NC=9

9. Durante la campaña electoral, ¿recibió Ud. algún regalo o favor de parte de algún partido o candidato?

Si=1 No=2 [Saltar a 14] NS=8 [Saltar a 14] NC=9 [Saltar a 14]

10. ¿Podría decirme que es lo que recibió?

[Registrar hasta tres respuestas]

11. ¿Podría decirme cuando los repartieron? [Marcar todas las respuestas]

Después de la elección. =1

En la última semana antes de la elección. =2

En el último mes antes de la elección=3.

En los últimos seis meses antes de la elección=4.

En el último año antes de la elección=5

NS=8 NC=9

12. ¿Qué partido le hizo estos regalos o favores? [Marcar todas las respuestas]

Alianza Partido Liberal Constitucionalista =01

Frente Sandinista de Liberación Nacional=02

Alianza Liberal Nicaragüense =03

Partido de la Resistencia Nicaragüense =04

Alternativa por el Cambio=05

Otros: _____ [Escribir respuesta] NS=88 NC=99

13. ¿Podría decirme si estos regalos o favores influenciaron su voto?

Si=1 No=2 No voté=3 NS=8 NC=9

14. ¿Cree Ud. que el gobierno o los partidos pueden descubrir por quien votó alguien en su barrio?

Si=1 No=2 NS=8 NC=9

15. ¿Podría decirme cuán frecuentemente asiste Ud. a reuniones de algún Consejo de Poder Ciudadano?

Muy frecuentemente=1 Algunas veces=2 Pocas Veces=3 Nunca=4 NS= 8 NC=9

16. ¿Tiene algún familiar o conocido que haya sido arrestado o asesinado durante la “guerra civil”?

No = 0 Guerra de Insurrección 1975-1979 =1 Guerra Civil 1979–1987 = 2

Ambas = 3 NS = 8 NC = 9 /____/

Por favor conteste sí o no a cada una de las siguientes preguntas

	SI	NO	NS	NC
17. ¿Podría decirme si en el último mes su barrio o comunidad ha sido víctima de acoso o violencia por parte de la policía o de algún otro oficial del gobierno?	1	2	8	9
18. ¿Podría decirme si en el último mes Ud. ha sido víctima de acoso o violencia por parte de la policía o de algún otro oficial del gobierno?	1	2	8	9
19. ¿Podría decirme si es Ud. miembro de algún movimiento social?	1	2	8	9
20. ¿Podría decirme si ha participado de alguna protesta durante el último año?	1	2	8	9

21. ¿Cuál de estos grupos representa la mayor amenaza contra el bienestar de su comunidad?

1. Pandillas 2. Partidos Políticos 3. Movimientos Sociales 4. La Policía 5. El Gobierno

Otros: _____ [Escribir respuesta] NS= 8 NC=9

31- ¿Con cuál Partido Político simpatiza usted actualmente?

Frente Sandinista de Liberación Nacional=1 Partido Liberal Constitucionalista=2

Alianza Liberal Nicaragüense=3 Movimiento de Renovación Sandinista=4

Otro _____ NS/NR = 99

32-¿Por cuál partido votó para Alcalde de este municipio en las elecciones pasadas?

PLC=01 FSLN=02 Otro=_____ NS/NR=99

Dígame si usted está de acuerdo o en desacuerdo con cada una de las siguientes frases:

	Acuerdo	Desacuerdo	Ns/Nr
P33. Las elecciones para Alcalde fueron libres y limpias	1	2	9
P34. El Consejo Supremo Electoral organizó bien estas elecciones	1	2	9

Para finalizar le vamos a hacer unas preguntas personales.

200. Anote el sexo del entrevistado. Masculino: 1 Femenino: 2

201. ¿Cuál es su edad en años cumplidos? _____

202. ¿Cuál es el último año de estudios que usted aprobó?

Sin escolaridad: 00 Primaria : 1 2 3 4 5 6

Secundaria: 1 2 3 4 5 6 Universidad: 1 2 3 4 5 6

203. Sumando sus salarios y otros ingresos, ¿Cuánto recibe su familia aproximadamente por mes?

Si no responde la P203 ¿Cuánto recibe su familia aproximadamente por mes?

204. Hasta C\$ 1,500= 1 De C\$ 1,501 a C\$ 3,000= 2 De C\$ 3,001 a C\$ 4,250=3

De C\$ 4,251 a C\$ 8,500= 4 De C\$ 8,501 a C\$ 12,750= 5 De C\$ 12,751 a C\$ 17,000= 6

De C\$ 17,001 a más=7 Ns/Nr =9

Reviewer's Appendix C: Validation of the list experiment: evidence from Uruguay

While this is the first attempt to use a list experiment to gauge levels of vote buying, scholars have used the technique across a variety of subjects since the 1980s, with political scientists increasingly using the list experiment in recent years. Political scientists have successfully used the list experiment to study racism and attitudes toward affirmative action (Kuklinski, Cobb, and Gilens 1997; Kuklinski, Sniderman, Knight, Piazza, Tetlock, Lawrence, and Mellers 1997), attitudes toward female presidential candidates (Streb, Burrell, Frederick, and Genovese 2008), attitudes toward Jewish presidential candidates (Kane, Craig, and Wald 2004), attitudes toward African American presidential candidates (Heerwig and McCabe 2009), multicultural attitudes in the Netherlands (Sniderman and Hagendoorn 2007), self reported media consumption (Prior 2009), attitudes toward the extension of suffrage in Lebanon (Corstange 2009), and self reported voter turnout (Holbrook and Krosnick 2010). Scholars working in sociology, studying business ethics, and public health have also made use of the technique.⁴⁴ While not all studies deploying list experiments have found significant differences between estimates from direct measures and those derived from the list experiment, Holbrook and Krosnick (2010) report that of the 48 such comparisons that they identified, 63 percent resulted in estimates significantly different from the direct measures in the expected direction. Further, other studies have consistently shown that the list experiment outperforms the randomized response technique as an unobtrusive measurement device, as respondents find the format easier to understand, trust it at higher levels, answer it more quickly, and are less likely to refuse to answer the question (Hubbard, Casper, and Lessler 1989; Coutts and Jann 2008).

Although the list experiment has demonstrated notable success across a number of applications, scholars have highlighted a number of (potential and real) weaknesses worth discussing in some detail. First, the indirect nature of the technique, combined with the need to split the sample in half, results in significant reductions in efficiency of the estimates as compared to direct items, necessitating large sample sizes or other techniques to reduce the variance of items on the list (e.g. Droitcour et al. 1991; Tsuchiya et al. 2007). Second, as noted above, inferences can only be extended to the subgroup through stratified difference of means tests or multivariate interaction analysis, rather than the individual level, although a number of scholars are currently attempting to develop multivariate techniques to derive estimates at the individual level (e.g. Corstange 2009; Glynn 2010; Imai 2010). Third, ceiling effects can distort estimates if all of the nonsensitive items are applicable to the respondent in addition to the

⁴⁴ Studies outside of political science usually refer to the list experiment as the item count or unmatched item count technique. Scholars have used it to study illegal drug use (Miller 1984; Miller Harrel, and Cisin 1986; Droitcour, Caspar, Hubbard, Parsely, Visscher, and Ezzati 1991; Biemer and Brown 2005), unethical workplace behavior (Dalton, Wimbush, and Daily 1994; Wimbush and Dalton 1997), sexual behavior (LaBrie and Earlywine 2000), hate crime victimization (Rayburn, Earlywine, and Davison 2003a, 2003b), shoplifting (Tsuchiya, Hirai, and Ono 2007), eating disorders (Anderson, Simmons, Milnes, and Earlywine 2007), and AIDS (Ahart and Sackett 2004).

sensitive item. In the face of such a situation, anonymity is lost since an indication of all response categories shows the interviewer that the respondent has engaged in the sensitive behavior (e.g. Kuklinski et al. 1997). As a result, the list used in this study included the item about being threatened by parties or candidates. Fourth, scholars have shown that estimation at the subgroup level can at times produce nonsensical negative estimates, generally attributable to either ceiling effects or failures of randomization as sample sizes are further reduced for such analyses (e.g. Kuklinski et al. 1997; Streb et al. 2008). As a result, estimates based on small sample sizes should be viewed with caution.

The question format itself may have an effect. Studies have found that respondents tend to *underestimate* items in list experiments compared to direct questioning, and this underestimation may be positively related to the length of the list (Tsuchiya et al. 2007; Flavin and Keane 2009). What this finding implies, though, is that estimates produced by the list experiment will tend to be biased downwards, suggesting that list experiment estimates should be regarded as minimally valid estimates, with “true” levels of the sensitive attitude or behavior probably higher.

Finally, there is the possibility that the inclusion of an additional category may increase the number of reported items regardless of content (e.g. the addition of a 5th category and not reports of vote buying drive the experimentally detected difference). While this concern is understandable, two pieces of evidence suggest that this does not occur. First, scholars have shown that list experiments examining attitudes or behaviors that are not expected to be subject to social desirability pressures do not result in significantly different estimates from direct measures (e.g. giving blood, Tsuchiya et al. 2007). Second, Tsuchiya et al. (2007) demonstrate that the length of the lists is not correlated with the size of the estimates of the experimental item, suggesting that differing list lengths between treatment and control conditions do not produce artifactual estimates. Thus, the existing evidence suggests that list experiments generally yield accurate estimates of a behavior, or slightly underestimate incidence.

To enhance our confidence in the measurement validity of the list experiment we designed an experiment, unrelated to the issue of vote buying, to test whether the “artificial inflation” hypothesis is true in Latin America. The follow up test was fielded in Honduras and Uruguay on nationally representative probability samples with 1008 and 900 respondents respectively.⁴⁵ The survey in Honduras was conducted by *Borge & Asociados* one month after the November presidential elections in 2009. The survey in Uruguay was conducted by *Equipos Mori* two weeks after the run-off presidential election held in December 2009. Although in terms of levels of development--and in particular with regard to education levels, which may condition respondents’ ability to understand complex survey questions--Uruguay is very different from

⁴⁵ Additional methodological attributes of the surveys are available upon request.

Nicaragua, its neighbor Honduras is very similar.⁴⁶ If the artificial inflation hypothesis can be discarded with evidence from these two very different settings, our confidence in the list experiment technique will be boosted.

The purpose of this validation experiment is to show that the treatment group does not artificially inflate the average number of responses. We asked respondents about their ways of participating in politics during the previous electoral campaign. We randomly assigned respondents to three groups (one control and two treatment groups). The first treatment group includes an extra response option, which we expect very few respondents to count (“I ran for office”). The second treatment group includes an additional response option, which we expect most people to count (“I was aware that the elections were taking place”⁴⁷). None of the items is expected to be subject to social desirability bias.

The design of the experiment is as follows:

We are interested in knowing the various ways in which people get involved in politics. I will show you a list of political activities and I would like you to tell me HOW MANY of these activities you were involved in during the last electoral campaign. Please, do not tell me which one, but HOW MANY.

For the control group in Uruguay, the following political activities were listed:

- I volunteered for the campaign of one of the parties
- I attended a rally
- I tried to persuade a friend to vote for my candidate
- I picked a fight with someone over a candidate

The control group in Honduras received a different set of baseline items.

- I voted for a candidate
- I participated in a rally
- I discussed the election with someone
- I saw or read something about the election in the news

Treatment items remained the same for both countries and were placed in the third position. The first treatment group included the following additional political activity:

- I ran for office

The second treatment group included the following additional item:

⁴⁶ Estimates for 2003 indicate that the literacy rate in Nicaragua is 67.5%. According to the 2001 census, the literacy rate in Honduras is 80%. Estimates for 2003 indicate that the literacy rate in Uruguay is 98%. Source: CIA Country Factbook.

⁴⁷ Over 90% of the electorate participated in the run-off election in Uruguay election. Approximately 50% participated in the Honduran presidential election.

- I was aware that the elections were taking place⁴⁸

To gain confidence in the validity of the list experiment technique, the first treatment group should be no different from the control group, whereas the difference between the second treatment group and the control group should be of an average of approximately 1 additional item. If the former expectation does not hold and the difference in means is positive and significant, the artificial inflation hypothesis will receive strong support. By contrast, if the latter expectation does not hold and the difference in means is lower than 1, we may conclude that the list experiment offers conservative estimates, and by no means fabricates unreasonably large differences.

The results obtained in both surveys are promising. In the case of Uruguay, for the first treatment group, the difference in means is .093, with a standard error of .09 and hence not significant at any conventional statistical threshold. For the second treatment group, the difference in means is .38, with a standard error of .09 and hence highly significant. In the case of Honduras, for the first treatment group, there was essentially no difference in means (.0008), with a standard error of 0.12). For the second treatment group, the difference in means is 0.59 with a standard error of 0.12 and hence highly different from both zero (i.e., it is detecting an effect in the correct direction) and one (i.e., it is underestimating the true prevalence of electoral awareness). Thus, these results suggest that the list experiment technique offers a conservative estimate. Hence the results give us reasons to believe that the vote buying list experiment is not artificially inflating aggregate levels of vote buying in Nicaragua. If anything our technique offers a lower bound estimate, which is demonstrably different from that offered by obtrusive measures.

⁴⁸ The wording in Spanish was the following: “Nos interesa saber cómo se involucran las personas en política. Voy a mostrale una lista de actividades políticas y quisiera que me diga cuántas de estas actividades realizó usted durante la última campaña. No me diga cuáles, sólo CUÁNTAS”. The baseline response categories for Uruguay were: “Participé como voluntario para la campaña de uno de los partidos,” “Participé en una movilización,” “Intenté convencer a un amigo de que votara por mi candidato,” and “Tuve una pelea con alguien sobre un candidato.” The baseline items for Honduras were “Voté por algún candidato,” “Participé en una movilización,” “Discutí acerca de la elección con alguien,” “Vi o leí algo acerca de la elección en las noticias.” The treatment items were “Participé como candidato” and “Estaba al tanto de que las elecciones se iban a llevar a cabo,” respectively.