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 using 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 then demonstrate that the social desirability bias is non-random and that the analysis based on traditional obtrusive measures of vote buying is unreliable. We also provide systematic evidence that shows the importance of monitoring strategies by parties in determining who is targeted for vote buying.

Clientelistic electoral linkages are characterized by a transaction of political favors in which politicians offer immediate material incentives to citizens or groups in exchange for electoral support.[1] Vote buying, which is a more particularized form of clientelism involving the exchange of goods for votes at the individual level (Stokes 2007), has generated numerous ethnographies and surveys to measure its incidence and test related hypotheses. While ethnographies routinely find vote buying to be pervasive in the developing world (e.g., Auyero 2001), surveys routinely 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 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), 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, destroys the possibility of exercising democratic rights in a leveled and competitive playing field. It is therefore imperative not only to

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[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).

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 remains based on qualitative case studies that employ ethnographic techniques to study 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 electoral 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 probably undergirding vote buying in specific contexts, this qualitative literature is limited by its inability to measure the extent of vote buying and systematically test competing theories about party strategy.

In response to this disadvantage, some scholars have used mass surveys to study vote buying at the individual level. In stark contrast to ethnographic studies, those employing survey data often find that small percentages of respondents report receiving a good or favor from a political party when asked about it directly. 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). Similarly, Transparency International (2004) reports that 7 percent of the population was targeted with vote buying during the 2001 elections in Brazil (76). The Brazilian finding is astonishing given that scholars single out vote buying as the primary currency of power in that country (e.g. Hagopian 1996; Mainwaring 1999; Ames 2002). Most surveys

reported vote buying rates in the single digits, with those reporting double-digit rates restricted to Southeast Asian democracies. These types of findings leave many scholars optimistic about the level 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 use questions asking respondents whether they have received a gift or favor from a political party or candidate in exchange for their vote. 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 at the individual level,

advancing the clientelism research agenda will be difficult.

To assess the validity of these arguments, we employed an unobtrusive measurement technique (a list experiment) to determine the degree to which social desirability bias affects direct vote buying items and how such bias is distributed across relevant variables. We tested our technique using a survey of 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 the 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 examinations of group level data also suggest that 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. The data also demonstrate that items assessing vote buying in respondents' neighborhoods are also plagued with bias and uncertainty over respondents' motivations, thereby suggesting that such items are not viable alternatives for reducing social desirability bias.

The paper begins by arguing that social desirability bias is highly likely when using standard obtrusive items that ask about vote buying at the individual and neighborhood levels. We then propose our alternative measurement technique and describe its strengths and weaknesses in overcoming measurement problems. The next section briefly describes the Nicaraguan political setting and the survey we conducted to assess vote buying in the country. The results of the list experiment are reported, followed by a subgroup analysis in which we present the first systematic piece of evidence showing that effective monitoring by electoral machines is associated with higher levels of vote buying. This supports Stokes's (2005) argument about the perversion of electoral accountability in clientelistic exchanges. The

subgroup analysis is then used as a baseline to illustrate how multivariate analyses relying on traditional measures of vote buying can be biased. The paper concludes by placing these findings in a broader context and suggesting directions for future research.

Response Bias and Vote Buying

Social desirability bias in surveys has been found to be important in the United States (e.g. Bradburn et al 1978; DeMaio 1984; Nadeau and Niemi 1995) and in cross-cultural studies (Johnson and Van de Vijver 2003). When respondents fear expressing socially undesirable attitudes or participating in stigmatized or illegal behavior, social desirability bias can occur. Some respondents misrepresent themselves to present 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). 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.

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. Thus, asking directly about receiving a gift in exchange for a vote may exhibit social desirability bias and underreport its incidence. In an illuminating footnote, Stokes (2005) notes the high potential for such bias in her study of vote buying in Argentina:

In contrast, the main problem with the survey approach used here is that people may be reluctant to acknowledge receiving handouts, in the Argentine case probably as much because of the implication that they are poor enough to sell their votes as out of concern about the illegality or immorality of their actions. It is probably evidence of this reluctance that only 7% of our sample acknowledged having received goods, whereas 44% said goods were distributed in their neighborhood, 39% could mention exactly what items were distributed, and 35% could name the party that gave them out. (321, fn. 20)

In a previous work (Brusco, Nazareno and Stokes 2004, 69-72), Stokes and her coauthors 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. 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.

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 nonsystematic (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

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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 party supporters are more reluctant to admit that they received gifts for fear of making their party look unfavorable in the eyes of the interviewer, they may systematically underreport the level of goods that they receive at a greater rate than do the weakly opposed. Given this systematic social desirability bias, the survey data would wrongly suggest a statistically significant relationship between weakly opposed citizens and vote buying even though no such relationship exists. Conversely, strong supporters might see gift dispensation by their party as a commendable component of clientelistic "problem solving networks" and hence under-report vote buying to a lower extent than weak supporters (Auyero 2000). In the latter case, systematic social desirability bias would lead to the wrong conclusion that parties mainly target their core constituency as part of a turnout buying strategy.

Alternative measurement items used to supplement direct items are also subject to similar patterns of error and probably introduce other sources of error. The main alternative to the direct individual item is asking about vote buying more indirectly by inquiring about vote buying in the respondent's neighborhood.[2] However, 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 such individual level vote buying. Response bias can result if supporters of a given party report vote buying operations by rival organizations in their

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[2] 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.

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 just how public vote buying is and not its frequency.

For example, neighborhood measures may overestimate vote buying in urban areas and

underestimate the frequency in rural areas where population density is lower and therefore so is

the probability of a clientelistic exchange being observed by other citizens. Thus, it is entirely

plausible that measurement error in 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 produce more valid estimates of vote buying that reduce error caused by social

desirability bias, 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 subjects subject to social desirability bias, including

racism and attitudes toward affirmative action (Kuklinski. Cobb, and Gilens 1997; Kuklinski,

Sniderman, Knight, Piazza, Tetlock, Lawrence, and Mellers 1997), attitudes toward presidential

candidates with varying descriptive characteristics (Streb, Burrell, Frederick, and Genovese

2008; Kane, Craig, and Wald 2004; 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).[3]

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:[4]

> 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:

- 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 a fifth category, placed in the third response position:

- they gave you a gift or did you a favor[5]

The question does not ask respondents to tell 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

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[3] Given the newness of list experiments to comparative politics, we have included a detailed 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 Appendix 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.

[4] Response options were also read to respondents.

[5] 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 ultima 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, Sniderman, Knight, Piazza, Tetlock, Lawrence, and Mellers 1997).

interviewer cannot ascertain which activities the respondent indicates. Because respondents intuitively understand this anonymity, social desirability pressures should be reduced, providing less incentive to underreport vote buying.

Since respondents were randomly assigned to the treatment and control groups, the two groups will be identical on both observable and unobservable characteristics, on average.[6] 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, on average. Systematic differences in the means provide a point estimate on the number of people reporting vote buying activity. For example, if the average number of items indicated by the control group is 2.10 and the average number of items indicated by the treatment group is 2.35, then we can conclude that 25 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 also be determined by the absolute difference between list experiment estimates and estimates derived by both direct individual and neighborhood items. Thus, the list experiment can provide more valid subgroup level estimates of vote buying and can also demonstrate the degree to which measurement error is systematic or not.[7]

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[6] Appendix A contains the randomization checks for our experiment.
[7] Although a number of scholars are currently working to develop techniques to utilize list experiments to make individual level inferences (e.g. Corstange 2009, Glynn 2009), questions about the validity of such techniques (e.g. Flavin and Keane 2009) suggest that group level inferences remain the state of the art for list experiments.

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 flawed, and 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 decade, 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), is currently the primary political force in the country.[8] The FSLN controls the presidency and maintains a firm support base around 40% (see Appendix Table A1[9]) with electoral strongholds in poor urban areas. Since the late 1990s the *Partido Liberal Constitucionalista* (PLC), originally a faction derived from the Somocista authoritarian regime, has positioned itself as the primary center-right opposition to the Sandinistas by attracting support from the middle-classes, small businesses and poor voters in rural areas (Guzmán and Pinto 2008).[10] The rightist parties when considered together have generally held more than half of the vote share (around 55%, see Appendix Table A1), but in the 2006 Presidential elections votes were spread across several parties.[11]

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[8] The party is headed by Daniel Ortega, who was a member of the five-person *Junta Nacional de Reconstrucción* that took office when dictator Somoza was overthrown by the FSLN 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, was re-elected.

[9] Presidential and parliamentary elections are held simultaneously every five years; while municipal elections every four.

[10] The PLC governed Nicaragua for two consecutive periods, presided by Arnoldo Alemán (1997-2002), and Enrique Bolaños (2002-2007)

[11] After the 2006 Presidential Election, and with the return of Montealegre to the PLC, ALN has lost its political appeal. In the 2008 municipal elections, ALN obtained only 2.1% of the valid votes.

While the literature on the actual incidence of vote buying in Nicaragua is limited, the literature that does exist suggests that vote buying has become an important component of electoral competition in the country.[12] Guzman and Pinto (2008) argue that Nicaraguan parties across the ideological spectrum engage in clientelistic practices both in and out of power. Parties on the left and right have resources (or can expect to have them when they eventually access the state) to develop clientelistic networks.[13] In the particular context of the 2008 campaign both sides could engage in vote buying since FSLN had access to government resources and PLC possessed strong ties to business interests and still controlled some sub-national offices. In addition, both leftist and rightist parties have low skill core constituencies that allow them to engage successfully and efficiently in vote buying strategies alongside or in detriment of programmatic ones because of the elevated relative value that inexpensive gifts have for these voters.[14] In this respect Nicaraguan politics is different from political practices in countries where vote buying is usually studied because two, rather than one, well-articulated machines extensively practice vote buying.

These features of Nicaraguan politics lead us to expect high aggregate levels of vote buying in the country. Fieldwork conducted during the campaign for the November 9th, 2008 municipal elections further supports this view. Although the 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 Leon, led the major parties to

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[12] Interviews with local experts and practitioners conducted by one of the authors further confirms this consensus.

[13] Weak states are prone to predatory practices by incumbent parties (Shefter 1994). According to the World Bank´s indicator of government effectiveness, which measures the quality of public services and the civil service, Nicaragua ranks 21st among 25 other Latin American countries. Nicaragua has a score of -0.667. Chile ranks 1st with a score of 1.268 and Haiti ranks 25th with a score of -1.408 (World Bank 2006).

[14] For a similar argument based on the Argentine case, see Calvo and Murillo (2004). Nicaragua is the second poorest country in Latin America. 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.

use every tool at their disposal to secure victory.[15] 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 strategy employed by parties to mobilize support. One of the

authors documented the well-articulated political brokerage networks that manage highly

personalized information about the needs of their potential clients on behalf of the candidate.

The result is that political parties tailor goods and services in exchange for votes not only to

particular communities but even individuals. The Sandinistas, for example, take advantage of

government-sponsored (and Sandinista controlled) *Consejos de Poder Ciudadano* (Citizen

Power Councils) for these purposes.[17] The distribution of goods occurring through these

councils clearly is not part of an official welfare program.

In addition, 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, especially in poor areas, perceive that their votes can be monitored by the FSLN and by

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[15] According to the electoral council's provisional results, the Sandinistas won 94 of the 146 municipalities, including Managua, the capital city of the country. 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 Day 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 case did not advance and the Sandinista-controlled electoral council proclaimed the incumbent party as the winner of the elections. In January 2009, several civil society groups presented 100,000 signatures to the CSE demanding a recount of votes. As of October 2009, the state had still not released official election results. Currently, the official website makes no mention of the election. See http://www.cse.gob.ni.

[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] 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).

the State intelligence Service. As part of these monitoring efforts, the Sandinista party "helps" poor citizens to get their electoral identification documents "practically for free." This kind of "social service" is perceived by these citizens to be a mechanism to monitor their vote, even though they cannot identify the precise way in which it operates.[18]

The visit of Alexis Arguello, the Sandinista candidate for mayor of Managua, on October 22nd to the *Asentamiento Hugo Chávez Frías*, one of the many shantytowns surrounding Managua, is an excellent illustration of this characterization of the workings of clientelistic networks. Arguello delivered precisely nine wheel chairs to the nine handicapped people in the neighborhood. According to Nicaraguan political analysts interviewed, the Conservatives' networks operate in a similar fashion. The PLC has built deeply rooted clientelistic networks in the rural areas of the country. In the words of a multilateral agency officer, "if you go to *el campo* (rural areas) and ask people who gave them that chicken or even that cow, they wouldn't have a problem saying they were gifts from Alemán[19] or Liberales."[20] These kinds of goods are clearly considered part of a political transaction. According to a Nicaraguan social researcher, "construction materials, animals, money, and medicine are traditionally the most frequent goods delivered in electoral campaigns in exchange for votes."[21] If obtrusive measures significantly underestimate vote buying in this context, where it is expected to be a widely spread practice and where some local observers even expect vote buying's status as "politics as usual" (at least in

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[18] Interviews with various citizens in a poor neighborhood of Managua (October 2008).

[19] Arnoldo Alemán, leader of the *Liberales,* was Nicaraguan President from 1997 to 2002.

[20] Interview (October 2008).

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

some parts of the country) to trump social desirability bias, then these measures are likely to be more problematic in countries where vote buying is less common.[22]

Ethnographic fieldwork and interviews with experts therefore suggest that vote buying in Nicaragua operates in a similar fashion to what is described in other larger and wealthier countries like Mexico and Argentina (e.g., Magaloni et. al. 2007; Stokes 2005; Auyero 2001). It is worth noticing, however, that work done on the lasting impact of the Sandinista revolution on political culture and individual political behavior challenges this claim, indicating that the revolutionary experience may have created a context unfavorable to clientelistic relationships (e.g. Anderson and Dodd 2005, 2009; Booth and Richard 2006). 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.[23] In contrast to most post-revolutionary societies, Nicaraguans and their political institutions have demonstrated less authoritarianism than their neighbors and have tended to be more politically engaged and tied to political parties (Booth and Richard 2006). While there certainly is evidence that the revolution left a lasting impact on the country, Booth and Richard (2006) also argue that this distinctiveness has probably decayed rapidly over time, as the differences between Nicaragua and its non-revolutionary neighbors in electoral behavior have largely diminished. Similarly, Anderson and Dodd (2009) write that

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[22] The fact that vote buying may constitute "politics as usual" in Nicaragua suggests that social desirability bias should not be as much of a concern, but there are still many reasons to expect some level of bias in response to direct vote buying questions. A stigma against vote buying may exist in Nicaragua due to the practice's illegality, its indication that the respondent is poor enough to value vote buying goods (Stokes 2005), or its perceived violation of democratic norms. Regarding this latter point, Nicaraguan respondents to the 2008 Americas Barometer survey demonstrate relatively high levels of commitment to democracy as an abstract idea as well as to basic democratic norms of contestation and inclusiveness (Pérez & Seligson 2008, 15-19). Given the context of the survey, we would expect such adherence to democratic norms to induce socially desirable behavior among respondents with regard to the direct vote buying items, even if the extent of this bias were lower than in contexts in which the behavior is more stigmatized.

[23] As a result, Nicaragua has surprised experts on democratization by the quick rate at which democracy consolidated in the country following the tumultuous conflict of the 1970s-1980s (e.g. Anderson and Dodd 2005).

recent years have seen an increasing authoritarian regression at the national level, although they report a contrary trend at the local level.

In order to adjudicate between these partially diverging assessments of Nicaraguan politics, and to go beyond the scope of events witnessed during fieldwork, the aggregate incidence of vote buying must be accurately measured. The next section begins to take on this task by detailing the survey we conducted to test our hypotheses about vote buying and social desirability bias.

Survey

We conducted a nationally representative survey one week after the municipal elections held in November 2008. The survey randomly selected voting centers (i.e., precincts are the primary sampling unit) across the nation and then randomly selected registered voters within these voting centers.[24] Appendix B contains the text and question order from the survey. The response rate for the survey conducted by *Borge y Asociados* was 60% and resulted in 1008 respondents.

In addition to the list experiment described previously, the survey instrument also included a number of other items used in the empirical analysis. To compare and contrast the different techniques for detecting vote buying, respondents were asked whether they had seen vote buying in their neighborhood and whether they had been targeted themselves using wording nearly identical to the treatment item on the list experiment in order to allow for a direct

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[24] The 2006 Nicaraguan voter registry was used as the sampling frame. For the list experiment, assignment to treatment and control occurred at the individual level.

comparison of the different items.[25] Follow up questions asked those who responded

affirmatively to either of the direct questions about which parties engaged in the distribution of

gifts, the kinds of material incentives dispensed, and whether the gifts had an impact on vote

outcomes.

We collected additional information to test existing predictions about party targeting

including questions about the respondent's socioeconomic status (e.g., income, education,

gender, age and area of residency) (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

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[25] 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 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.

of reliable monitoring, parties target their own supporters in an effort to get out the vote, as opposed to buying-off indifferent or opposition voters. Some scholars go as far as to suggest that it is this inability to solve the commitment problem that makes vote buying a very ineffective form of electioneering and therefore a rare phenomenon (e.g., Lehoucq 2007). Only by combining a reliable measurement of the aggregate levels of vote buying with an appropriate operationalization of monitoring can these rival explanations be adjudicated. Although the ethnographic literature has described the workings of effective monitoring machines (e.g. Auyero 2001) and formal models have incorporated monitoring as an important parameter (e.g., Stokes 2005), the survey-based quantitative literature has not consistently measured monitoring.

The item on party monitoring asked whether respondents believed that the government or parties could find out for whom they had 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. 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 to be more susceptible to clientelistic practices. Finally, we inquired about the frequency of involvement in *Consejos de Poder Ciudadano*. 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

This section presents the results of the list experiment for the overall sample and theoretically important subgroups. We compare the results to findings from direct questions about respondents and their neighborhoods. The analysis then demonstrates that relying on obtrusive questions not only leads to biased estimates on the frequency of vote buying, but also inconsistent estimates of the type of person targeted by parties.

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.[26] The first column of Table 1 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.=7.2 percentage points).

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. = 0.4 percentage points) admitted receiving a gift or favor personally (Table 1, 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. The aggregate results are somewhat better when relying on the neighborhood vote buying item, which indicates that 17.7 percent of respondents (s.e.=1.2 percentage points) answered in the

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[26] The balance between treatment and control groups across relevant variables is reasonably high, suggesting that the randomization procedure worked properly. See Appendix A for more information.

affirmative. The 6.7 percentage point difference between the list experiment and the

neighborhood question is not statistically significant (p=0.357). Thus, while the direct individual

measure greatly underestimates the degree of vote buying, the neighborhood measure may

provide a better aggregate estimate of vote buying.

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 2, 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 2,

column 4). Independents and members of both parties all received gifts, 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 in

Nicaragua two clientelistic machines are competing against each other for votes, parties might

engage in gift dispensation to both safeguard their core constituency and lure weakly opposed or

independent voters, giving rise to more widespread patterns of distribution.

Contrary to expectations from the broader clientelism literature (e.g., Calvo and Murillo

2004; Kitschelt and Wilkinson 2007), there was no evidence that parties preferentially targeted

the poor for vote buying (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 and our measure of income groups sufficiently coarse that a

difference cannot be observed. The percent of the poorest reporting vote buying in the list

experiment is not much smaller than the percent reported by the middle-income group. The former has larger standard errors, which render it non-significant, but this is probably due to the fact that the poorest group is half the size of its middle-income counterpart.[27]

Two differences between subgroups are statistically significant and worthy of note. First, people who believe ballots are secret are very unlikely to report vote buying (6 percent with s.e.=1.0 percentage points), while nearly half of those respondents with doubt about the secrecy of the ballot report vote buying (s.e. = 1.1 percentage points). The second notable difference is that respondents who ever attended *Consejos de Poder Ciudadano* meetings were far more likely (48 percent) to report vote buying than those who never attended (19 percent). This difference strongly suggests that *Consejos de Poder Ciudadano* play an important role in the distribution of clientelistic goods. *Consejos de Poder Ciudadano* meetings could be used to transfer goods, but the mechanism could also be persuading individuals that the party can monitor ballots. These findings provide the first systematic piece of evidence in support of scholars who hypothesize that parties either pinpoint people who think monitoring is possible or convince targeted individuals of the party's ability to monitor the vote. Thus, parties can solve the commitment dilemma inherent to vote buying exchanges during electoral processes that are secret without the use of widespread coercive practices (Auyero 2001; Levitsky 2003; Stokes 2005). In the presence of well-articulated clientelistic machines theories of vote-buying dynamics should not deem it implausible that parties can actually monitor or credibly threaten non-compliant voters (cf. Nichter 2008).

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[27] Yet another interpretation for the surprising result that vote buying is more prevalent among the middle-class is that poor people are located in areas that are difficult to reach. As a result, parties choose to target more geographically proximate voters in the urban middle 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 evidence that vote buying is less prevalent in rural areas. We are indebted to Ana de la O for pointing out this interpretation.

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 2, column 5).

The neighborhood measure fares a little better, but consistently produces estimates below those generated by the list experiment (Table 2, 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, the neighborhood question fails to detect differences between subgroups suggested by the list-experiment. Neither the difference in *Consejos de Poder Ciudadano* meeting attendance nor certainty about ballot secrecy is 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.

The validity of the neighborhood measure is further called into question when direct individual-level vote-buying and list experiment estimates are reported by responses to the neighborhood question (Table 3). Nearly every individual reporting that she received a gift or favor in exchange for her vote also reported vote buying in the neighborhood. Thus, the neighborhood question measures a facet of individual-level vote buying. Unfortunately, the list experiment suggests that the measurement error in two directions is substantial. 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. Thus, social desirability bias may

not be completely purged by asking about the neighborhood rather than the individual.[28]

Assessing competing models: obtrusive versus unobtrusive 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. However, it is possible

that full models with relevant control variables can adequately address some of these concerns.

To evaluate this claim, Table 4 presents four logistic regression models of vote buying using the

direct individual and neighborhood questions as the dependent variables. Models 1 and 3 use

basic explanatory variables of vote buying, similar to the models used in Stokes (2005). Models

2 and 4 use a more complete set of covariates the literature suggests should matter. The goal of

the analysis is to evaluate the robustness of these models and measure the conclusions against the

baseline established by the list experiment in Table 2.

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 could be due to collinearity between

support for FSLN, participation in welfare programs, and attending *Consejos de Poder*

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[28] Responses to the individual-level question provide direct evidence that respondents sought to strategically
implicate the opposing party in vote buying in their neighborhood. In particular, PLC sympathizers overwhelmingly
(67 percent) pointed to the FSLN as the party giving out gifts in their neighborhood. In contrast only 11% of FSLN
sympathizers point toward the PLC as giving out gifts in their neighborhood.

Ciudadano meetings. The ultimate conclusion reached using the individual-level measure of vote buying would be that vote buying primarily targets Sandinista sympathizers. However, independents and PLC supporters both reported significant amounts of vote buying through the list experiment (Table 2, top panel, column 4), so models 1 and 2 could mislead an investigator as to the types of people actually targeted by vote buying campaigns.

Shifting to the neighborhood measure of clientelistic behavior in Model 3 almost directly contradicts the individual-level model since PLC supporters are more likely to report vote buying activity. It also appears that wealthier neighborhoods are targeted by vote buying campaigns, whereas the individual-level model hinted that poorer people received gifts or favors in exchange for votes. The other significant predictors of vote buying, age and gender, have point estimates that are close to those found using the individual-level measure. Adding the covariates in Model 4 does little to change the point estimates from Model 3. Since people from across the partisan and economic spectrum in Nicaragua were targeted for vote buying, these results are also misleading.

Adding to the concern, the coefficients for a few key variables change signs across models. The list experiment demonstrates that people convinced of ballot secrecy were not targeted for vote buying and only those people unsure of a party's ability to monitor votes were targeted. The individual gift models find that people who thought votes could be monitored were *less* likely to report the receipt of gifts. In contrast, the neighborhood gift models estimate that people who thought their vote could be monitored were *more* likely to have reported vote buying in their neighborhood. Without knowing the true relationship from the list experiment, a researcher would have little reason to select one model over the other, but diametrically opposing conclusions would be reached.

It is also interesting to note that neither model supports the view that *Consejos de Poder Ciudadano* are an important mechanism for targeting and distributing clientelistic goods or favors. Fieldwork and the list experiment identified *Consejos de Poder Ciudadano* as an important mechanism by which the FSLN distributes goods. In the individual model, the sign is in the correct direction but the coefficient is not significant. This lack of significance could be due to collinearity with supporting the FSLN. This explanation does not hold for the neighborhood gift models, however, since model 4 does not show that FSLN supporters are targeted for vote buying. Moreover, collinearity should only affect statistical significance, but the point estimate (-0.03 logits) is essentially zero. So, the neighborhood model misses a key part of the vote buying network in Nicaragua.

Thus, 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 ballots are secret. The models also failed to uncover known vote buying mechanisms. As such, the measurement error from social desirability bias in obtrusive measures of vote buying does not appear to be purely random and possibly biases results.

Conclusion

Standard obtrusive survey measures can dramatically understate the extent of vote buying. Correctly estimating aggregate levels of vote buying is important to assess the incidence of this form of electioneering in the developing world. Given that vote buying is a socially and politically stigmatized activity that can lead respondents to underreport it in ways that

systematically bias our measurement of the phenomenon, the error inherent in deflated measures also impedes a proper assessment of the political dynamics involved in the dispensation of goods and favors during campaigns. This paper has shown that list experiments offer a technique that solves some of these bias issues by offering respondents a high degree of anonymity. 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.

The limitations of the list experiment are readily apparent (Corstange 2009, 49). The respondent is correct in assuming that researchers cannot determine whether he or she personally received a gift or favor. Rather, only means across different groups can be compared. Thus, the information from the list experiment cannot be used in regression models to control for individual attributes. In this way, the ability to predict who parties target for vote buying is limited. We feel this limitation is more than compensated by obtaining less biased estimates of vote buying activity at large. The list experiment does allow for subgroup analysis, so the broad contours of vote buying can be mapped. We have shown that the neighborhood measure, which underestimates aggregate levels of vote buying to a lesser extent than the individual measure and can be used in regression analysis, is also subject to bias. Moreover, the neighborhood indicator presents obvious limitations when trying to assess targeting at the individual level.

Nicaragua is the second poorest country in Latin America and political parties throughout the ideological spectrum have recently demonstrated higher levels of vote buying activities. Given these characteristics, we are confident 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. The comparison of subgroups using the unobtrusive measure of vote buying also leads to

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theoretically relevant conclusions about the political dynamics of gift dispensation. Although we have no conclusive answer in relation to the partisan determinants of clientelism, the results suggest that there are two central mechanisms for clientelistic linkages in the Nicaraguan context: 1) voter's perception that parties can monitor their vote and 2) voter's participation or proximity to state sponsored community organizations like the *Consejos de Poder Ciudadano*. 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.

In order to gain greater confidence in the external validity of the results yielded by the list experiment in Nicaragua, more explicit comparative research is necessary in the developing world. One obvious 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. In addition, it is imperative for scholars to study in greater depth the mechanisms of gift dispensation and parties' monitoring strategies, which we show are a crucial component in determining who is targeted by machines. Although one of the survey items we use to gauge these processes explicitly asked respondents to evaluate the secrecy of the ballot, the importance of *Citizen Power Councils* suggests that monitoring is not just about spreading the belief that somehow parties can find out what people do in the privacy of the polling booth. It is important to study the mechanisms that allow parties to evaluate both private and public activities that evidence a citizen's commitment to an electoral effort through his or her participation in political networks of favors. The cases studied should offer variation along important political variables like levels of polarization between parties, number of competing clientelistic machines, and levels of state corporatism and patronage that allow incumbents to reinforce the traditional monitoring practices of partisan machines. These explanatory factors might capture differences in the intensity and extensity of vote-buying efforts as well as in the resources available for sophisticated monitoring.

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Table 1. Vote Buying, Direct and Unobtrusive measures

| | List Experiment | Direct Items | |
		Individual Gifts	Neighborhood Gifts
Control	2.06 [495]		
Treatment	2.31 [500]		
Estimated % Receiving Gifts	24.3% (7.2)	2.4%*** (0.4)	17.7% (1.2)
N	995	1,003	998

List experiment control and treatment values are the mean number of items identified by respondents.

The number of subjects in each condition are in brackets.

Standard errors 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.

Table 2: Reports of Vote Buying in Important sub-populations

Group	Sub-group	Proportion of the sample	List Experiment	Individual gifts	Neighborhood gifts
Political	Voted	63.4%	22.9% (0.09) ***	2.7% (0.01) ††	21.0% (0.02)
	Abstained	37.0%	27.5% (0.12) **	2.0% (0.01) ††	14.0% (0.02)
	Support FSLN	40.1%	20.9% (0.11) *	4.5% (0.01)	18.2% (0.02)
	Support PLC	20.8%	32.6% (0.15) **	1.0% (0.01) ††	24.8% (0.03)
	Independents	37.6%	26.2% (0.12) *	1.1% (0.01) ††	13.1% (0.02)
Welfare and Public Works	Welfare	7.5%	10.0% (0.32)	18.9% (0.05)	27.0 % (0.05)
	No welfare	92.5%	24.7% (0.07) ***	1% (0.00) †††	17.1% (0.01)
	Public works	40.8%	22.0% (0.11) **	4.2% (0.01)	24.4% (0.02)
	No public works	59.2%	24.6% (0.09) ***	1.2% (0.00) ††	13.6% (0.01)
Citizen Power Councils	Ever attended	18.3%	48.2% (0.16) ***	4.9% (0.01) †††	19.8% (0.03) §
	Never attended	81.7%	19.0% (0.08) **	1.8% (0.01) ††	17.4% (0.01)
Monitor	Can monitor	35.7%	49.2% (0.13) ***	1.4% (0.01) †††	19.8% (0.02) §§
	Cannot monitor	55.2%	6.3% (0.1)	2.9% (0.01)	16.0% (0.02)
	Don't know	9.1%	40.2% (0.20) *	3.3% (0.02) †	21.0% (0.05)
Income	Less than $100	22.4%	23.1% (0.16)	4.3% (0.01)	16.3% (0.03)
	Between $100-200	49.5%	26.3% (0.10) **	2.4% (0.01) ††	17.2% (0.02)
	More than $200	28.1%	14.2% (0.15)	1.5% (0.01)	22.2%(0.03)
Education	No education	9.5%	16.1% (0.24)	3.1% (0.02)	15.1% (0.04)
	Primary	34.0%	21.0% (0.12) *	3.5% (0.01)	15.8%(0.02)
	High School	42.2%	26.7% (0.11) **	1.4% (0.01) ††	20.0% (0.02)
	College	14.3%	37.4% (0.18) **	2.1% (0.01) ††	18.1% (0.03)
Age	18-29	36.1%	7.1% (0.12)	3.3% (0.01)	20.0% (0.02)
	30-49	38.8%	30.0% (0.11) **	1.5% (0.01) ††	17.1% (0.02)
	50 or more	25.1%	41.3% (0.15) ***	2.4% (0.01) †††	15.0% (0.02) §
Gender	Female	50.5%	33.3% (0.10) ***	2.2% (0.01) ††	15.5% (0.02) §
	Male	49.5%	15.2% (0.10)	2.6 (0.01)	20.2% (0.02)
Zone	Rural	36.9%	25.5% (0.12) **	3.0% (0.01) †	18.9% (0. 2)
	Urban	63.1%	23.6% (0.09) *	2.1% (0.01) †	17.2% (0.1)

Standard errors in parentheses.

* 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 3: Comparing Neighborhood Gifts to Individual Gifts and the List Experiment

		Individual Gifts	List Experiment
		11.3%	53.3%
	Yes [17.8%]	(2.3)	(18.0)
Neighborhood Gifts		0.5%	18.1%
	No [82.1%]	(0.2)	(7.8)
	N	993	990

The percentages of the sample indicating the incidence of neighborhood gifts are in brackets. Standard errors associated with list experiment estimates are in parentheses.

Table 4. The instability of traditional estimates of vote buying

Dependent Variable	Model 1 Individual gift	Model 2 Individual gift	Model 3 Neighborhood gift	Model 4 Neighborhood gift
Support FSLN	1.377**	1.467*	0.313	0.246
	(0.561)	(0.792)	(0.204)	(0.229)
Support PLC	-0.172	0.332	0.760***	0.750***
	(0.873)	(1.052)	(0.225)	(0.246)
Income *(1-3)*	-0.455	-0.419	0.169	0.175
	(0.310)	(0.355)	(0.126)	(0.137)
Education *(0-3)*	-0.370	-0.454	0.019	0.065
	(0.290)	(0.348)	(0.115)	(0.128)
Age *(1-3)*	-0.362	-0.360	-0.205*	-0.069
	(0.296)	(0.346)	(0.119)	(0.129)
Female	-0.299	-0.280	-0.357**	-0.410**
	(0.422)	(0.494)	(0.173)	(0.186)
Monitor		-0.742		0.327*
		(0.560)		(0.186)
Welfare		2.797***		0.451
		(0.534)		(0.324)
Public works		0.323		0.648***
		(0.536)		(0.190)
Attended Citizen Power Councils		0.606		0.063
		(0.534)		(0.246)
Urban		0.550		-0.196
		(0.550)		(0.203)
Constant	-2.175**	-3.486***	-1.643***	-2.313***
	(1.069)	(1.311)	(0.443)	(0.494)
Observations	933	844	928	840
Pseudo R^2	0.080	0.274	0.024	0.046

Coefficients are from logistic regression.
Standard errors in parentheses.
* p<0.1, ** p<0.05, *** p<0.01

Appendices

Table A1. Elections Results in Presidential Elections. 2001 - 2006

Political Party	Abbrev.	2001	2006
Frente Sandinista de Liberacion Nacional	FSLN	46.3	38.0
Alianza Liberal Nicaraguense	ALN	**	28.3
Partido Liberal Constitucional	PLC	56.3	27.1
Movimiento de Renovacion Sandinista	MRS	d.p.*	6.3
Alianza por el Cambio	AC	d.p.*	0.3
Partido Conservador	PC	1.4	d.p.*

Source: *Consejo Supremo Electoral*.

*Did not participate.

** The ALN was founded in 2005.

Appendix A: Randomization Analysis

The randomization analysis indicates that in the overall the treatment and control groups are well balanced in all covariates. The T-test in Figure A shows that, in general, the means of the both groups are indistinguishable from each other. Figure B reports the results of individual regressions of each covariate on Treatment, and none of the p-values indicate than any covariate is a predictor of the treatment.

Figure A. Balance between Treatment and Control groups

Variable	Control group Obs.	Control group Mean	Treatment group Obs.	Treatment group Mean	Difference of means	T-test H_o: diff = 0 H_a: diff<0	H_a: diff not=0	H_a: diff>0
Neighborhood gift	493	0.160	497	0.197	-0.037	0.065	0.130	0.935
Individual gift	492	1.974	498	1.980	-0.006	0.254	0.508	0.746
Vote	495	0.642	500	0.632	0.010	0.634	0.733	0.366
Party ID	495	0.901	500	0.826	0.075	0.933	0.134	0.067
Welfare	495	0.061	500	0.084	-0.023	0.077	0.155	0.923
Public work	492	0.413	494	0.395	0.018	0.716	0.568	0.284
Council attendance	493	0.166	499	0.192	-0.026	0.143	0.285	0.857
Monitor	495	0.505	500	0.572	-0.067	0.053	0.107	0.947
Income	460	2.050	466	2.069	-0.019	0.345	0.689	0.655
Education	495	1.604	500	1.622	-0.018	0.369	0.737	0.631
Age	495	1.877	500	1.898	-0.021	0.333	0.665	0.668
Female	495	0.497	500	0.496	0.001	0.512	0.976	0.488
Urban	495	0.628	500	0.632	-0.004	0.452	0.904	0.548

Figure B. Logistic regressions on Treatment

	Logit Coefficient	Standard Error	p-value
Neighborhood gift	0.252	0.167	0.130
Individual gift	0.281	0.426	0.509
Vote	-0.045	0.132	0.732
Party ID	-0.121	0.081	0.134
Welfare	0.352	0.248	0.156
Public work	-0.074	0.130	0.568
Council attendance	0.177	0.166	0.285
Monitor	0.157	0.097	0.107
Income	0.037	0.093	0.689
Education	0.025	0.075	0.737
Age	0.036	0.082	0.665
Female	-0.004	0.127	0.976
Urban	0.016	0.131	0.903

Note: The intercepts are not reported

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 you 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=88NC=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

Appendix C: Validation of the list experiment: evidence from Uruguay and Honduras

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.[29] 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, 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 2009). Third, ceiling effects can distort estimates if all of the nonsensitive items are applicable to the respondent in addition to the sensitive item. In the face of such a situation, anonymity is lost since an indication of all

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[29] 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; Wibush 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 Earleywine 2007), and AIDS (Ahart and Sackett 2004).

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 under estimate 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. [30] 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

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[30] Additional methodological attributes of the surveys are available upon request.

Nicaragua, its neighbor Honduras is very similar.[31] 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"[32]). 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:

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[31] 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.

[32] 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 place[33]

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 is 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.

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[33] 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 eleción 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.