

Still for Sale: The Micro-Dynamics of Vote Selling in the United States, Evidence From a List Experiment

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Abstract

In nineteenth-century United States politics, vote buying was commonplace. Nowadays, vote buying seems to have declined. The quantitative empirical literature emphasizes vote buying, ignoring the micro-dynamics of vote selling. We seem to know that vote buyers can no longer afford this strategy; however, we do not know what American voters would do if offered the chance to sell their vote. Would they sell, and at what price, or would they consistently opt out of vote selling? A novel experimental dataset representative at the national level comprises 1,479 U.S. voters who participated in an online list experiment in 2016, and the results are striking: Approximately 25% would sell their vote for a minimum payment of \$418. Democrats and Liberals are more likely to sell, while education or income levels do not seem to impact the likelihood of vote selling.

Keywords— vote buying; vote selling; clientelism; list experiments; United States

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I. VOTE SELLERS AND VOTE BUYERS

Prior research on clientelism usually focuses on whether parties have attempted to buy votes (Vicente and Wantchekon 2009; Vicente 2014; Rueda 2015, 2017; Reynolds 1980; Nichter 2014; Kiewiet de Jonge 2015; Finan and Schechter 2012; González-Ocantos, Kiewiet de Jonge, and Nickerson 2014; Diaz-Cayeros, Estévez, and Magaloni 2012; Brusco, Nazareno, and Stokes 2004). Unfortunately, while this is an important question, it overlooks the conditions under which citizens would sell their vote. In fact, Nichter and Peress (2017) explain that studies continue to view clientelism typically as a top-down process, generally overlooking citizens’ demands. Since several questions pertaining to vote sellers remain unanswered, a bottom-up reconceptualization is necessary. For instance: *What would voters do if offered the chance to sell their vote? Would they sell it? And at what price?*¹

To illustrate the issue at hand, Figure 1 shows responses of U.S. citizens asked whether a candidate or a member of a political party has offered something in exchange for their vote, completely ignoring voters’ preferences. The figure begs the question of whether survey respondents who answered “never” *would* still be willing to sell their votes.

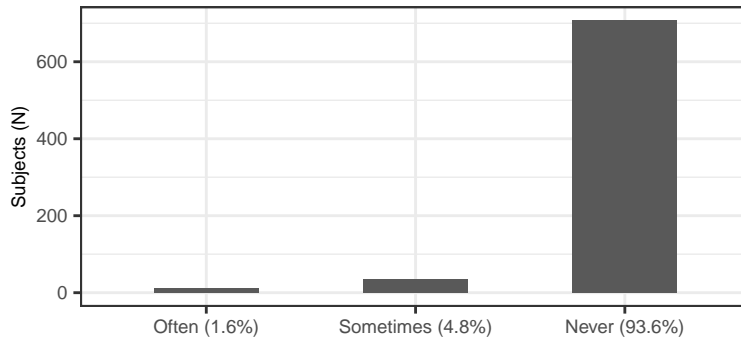


Figure 1: Frequency of Clientelism in the United States (2010).

Note: Figure shows the frequency of survey respondents, $N = 755$.

Source: *LAPOP*, 2010 wave for the United States. Question is *clien1*: “In recent years and thinking about election campaigns, has a candidate or someone from a political party offered you something, like a favor, food, or any other benefit or object in return for your vote or support? Has this happened often, sometimes, or never?”

It seems that whether studies focus on vote buying or vote selling depends partly on methodological rather than theoretical decisions.² On the one hand, historical and/or ethnographically based contributions describe clientelist transactions from the point of view of voters, focusing on

the conditions that make vote selling most likely (Posada-Carbó 1996; Sabato 2001; Auyero 2000; Szwarberg 2013; Borges 2019). On the other hand, statistical, survey, and/or experimentally based work mostly explores issues related to vote buying. For example, using a field experiment in Benin, Wantchekon (2003) stresses the role of incumbency on vote buying. Jensen and Justesen (2014, 227) focus on the impact of “poverty on vote buying,” while Khemani (2015, 84) shows that “vote buying in poor democracies is associated with lower [public] investments.” Hence, and except for several important quantitative studies (Corstange 2012; Imai, Park, and Greene 2015; Nichter and Peress 2017; Hicken et al. 2015, 2018; Michael and Thachil 2018), the emphasis of statistical studies remains on studying vote buying. Importantly, other statistically based studies have explored attitudes toward vote buying (Bratton 2008; Weitz-Shapiro 2012). They suggest that a strong stigma is attached to vote buying, which might make voters unwilling to sell their vote. For instance, González-Ocantos, Kiewiet de Jonge, and Nickerson (2014, 208) designed a list experiment to study attitudes toward vote buying in Latin America. They conclude that most respondents find vote buying “unacceptable when provided with a hypothetical example.”

While the quantitative literature has advanced several important avenues of research, it has overlooked many important questions. The wording of the Latin American Public Opinion Project (LAPOP) question illustrates part of the issue. By focusing on vote buying, it gives the falsely optimistic impression that U.S. voters systematically “oppose” vote buying, “thus” rarely engaging in clientelism (as Figure 1 strongly suggests). Furthermore, most quantitative studies were conducted primarily in developing countries, seriously narrowing the scope of our inferences. In part, this is because the clientelism literature usually focuses on realized behaviors only—that is, actual clientelist transactions. Unfortunately, by ignoring attitudes of potential vote sellers, particularly when it comes to the willingness to sell, selection bias seriously threatens causal inferences.

This paper makes both methodological and substantive contributions to the literature by leveraging a list experiment on hypothetical vote selling in a consolidated democracy. We believe that studying hypothetical behaviors—such as the willingness to sell—is a valuable exercise. Geddes (1990, 131) explains the well-known selection issues of studying “only cases that have achieved the outcome of interest.” Hence, if we are interested in understanding the micro-dynamics of clientelism—particularly as a supply-and-demand issue—we should incorporate the preferences of both sellers and buyers, potential and/or actual. Since the focus of this paper is on the willingness to sell, we believe that we can also learn from *unrealized* clientelist transactions. Following the lead

of González-Ocantos, Kiewiet de Jonge, and Nickerson (2014), this paper presents experimental evidence of hypothetical vote selling in the United States.

In 2016, a novel dataset representative at the national level was collected. A total of 1,479 U.S. voters participated in a list experiment between March 2 and March 6. This experiment made possible both the identification of the demographic factors that would make U.S. voters more likely to sell their vote, and at what price, and the investigation of whether they would systematically lie about selling their vote. The results are striking. The data suggest that a sizable portion of U.S. voters are willing to sell their vote (approximately 25%), would sell it for at least \$418, and would systematically lie about it (approximately 8%). Given that these data are representative at the national level (i.e., this is not a convenient sample), these findings are surprising. Democrats and Liberals are systematically more likely to sell than Republicans. Education and income levels do not seem to have a systematic impact on the willingness to sell.

While this paper essentially describes the phenomenon, it leaves for future research further consideration of the causes of hypothetical vote selling in the United States. Ultimately, this paper attempts to bring voters back into the quantitative study of clientelism, particularly by studying their willingness to sell.

II. THE UNITED STATES AS A CASE

At first, many advanced democracies were clientelist political systems. For instance, Stokes et al. (2013, 200) explain that in the nineteenth-century United States, “vote buying was commonplace” and “the major urban political institution in the late nineteenth century” (Erie 1990, 2). In Chicago, New York City, Newark, and other large American cities, votes were exchanged for “cash, food, alcohol, health care, poverty relief, and myriad other benefits” (Stokes et al. 2013, 200). The street price of the right to vote freely was low. Benseal explains that “[voters] handed in a party ticket in return for a shot of whiskey, a pair of boots, or a small amount of money” (227). In general, students of American political development have analyzed vote buying in detail, confirming both its early development and its generalized practice (Benseal 2004; Campbell 2005).³

However, vote buying currently seems to have declined considerably, for two competing reasons. Stokes et al. (2013, 201) show that industrialization drove up the electorate’s median income, making vote buying more expensive for party machines. However, Kitschelt and Wilkinson (2006, 320)

disregard the industrialization hypothesis, focusing on the lower levels of “[s]tate involvement in the public sector.”

Regardless, clientelist linkages are now rare. [Figure 1](#) suggests that 93.6% of U.S. respondents have never received a clientelist offer from a political party. While only a very small percentage (4.8%) report receiving such an offer from a political party, we do not know whether survey respondents *would* sell their votes. This paper presents systematic evidence that they would. Consequently, the counterintuitive results presented in this paper make our descriptive efforts worth pursuing. Representing the United States as a “crucial case,” both the narrative and the findings follow a “least-likely” design approach. As Levy (2008, 12) explains, “[i]nferential leverage from a least likely case is enhanced if our theoretical priors for the leading alternative explanation make it a most likely case for that theory.” The vote-buying literature mostly considers developing countries and describes vote sellers as poor (Weitz-Shapiro 2014, 12), uneducated (González-Ocantos, Kiewiet de Jonge, and Nickerson 2014), and undemocratic (Carlin and Moseley 2015). Thus, previous literature implies that the willingness to sell votes in the United States should be low, making it a difficult case study on vote selling.

The evidence that this paper presents may be associated with a probable erosion of American democracy.⁴ In a highly controversial pair of articles, Foa and Mounk (2016, 7) document a deep “crisis of democratic legitimacy [that] extends across a [...] wider set of indicators” in the United States. They find that 26% of millennials declare that it is “unimportant” in a democracy for people to “choose their leaders in free elections” (Foa and Mounk (2016, 10), and Foa and Mounk (2017)). These findings raise many (unanswered) questions regarding the actual value that American electoral institutions hold for citizens, possibly undermining the legitimacy of the integrity of voting. Is voting unimportant enough to lead U.S. citizens to sell their votes if offered the possibility?

[The next section](#) gives a historical account of vote buying and vote selling in the United States. The section also attempts to situate both within a historical context. It particularly shows how vote buying and vote selling transitioned from their status as an important institution in American elections to a scarcely practiced electoral method. [The following section](#) explains the experimental design. Immediately [thereafter](#), the paper presents the statistical analyses of the experimental data. The last [section](#) offers some working hypotheses and possible lines for future research.

III. VOTE SELLING AND PATRONAGE IN THE UNITED STATES: A BRIEF HISTORICAL ACCOUNT

While all U.S. states made bribery of voters illegal early in U.S. history, these laws were purposely ignored. Well before the Gilded Age (1877-1896), several norms aimed to prohibit bribery, clientelism, and patronage. For instance, as early as 1725, the New Jersey legislature had already outlawed many electoral malpractices (Bensel 2004, 59). However, these restrictions were systematically bypassed. To circumvent property qualifications, for instance, office-seekers (and their supporters) commonly bought “freeholds for landless men in return for their vote” (Campbell 2005, 6), a practice known as “fagot voting.” Since it was a coercive bribe, after “the election, the land was simply returned to the original owner” (p. 6).

Weak institutions, poor bureaucracies, and bad-quality record-keeping helped to foster electoral malpractice.⁵ First, most states did not have actual registration laws, making voter eligibility difficult to determine (Argersinger 1985, 672). Historians frequently report that judges at polling places had a hard time determining not only the age of the potential voter,⁶ but also whether the prospective voter was a U.S. citizen, especially in cases that involved newly naturalized immigrants with strong foreign accents (Bensel 2004, 20). Consequently, it was often up to the judge’s discretion whether to let prospective voters cast a ballot. Since judges were party appointees (Argersinger 1985, 672), their discretionary powers were systematically used to shape electoral outcomes.

Low literacy levels also helped to sustain vote selling in the United States. For example, in Kentucky and Missouri, the law required voters to verbally announce their choices at the polling places, instead of using party tickets (Bensel 2004, 54). Of course, the *viva voce* method was convenient for party workers who usually swarmed around the polling places. However, the ticket system eventually supplanted this method.

The “party strip” or “unofficial” ballot system also permitted all sorts of fraudulent election practices. The parties themselves produced party tickets. Since tickets varied by size and color, it made “the voter’s choice of party a public act and rendered voters susceptible to various forms of intimidation and influence while facilitating vote buying” (Argersinger 1985, 672). Similarly, Rusk (1970, 1221) explains that distinctive ticket colors and shapes “assured instant recognition of the ballot by the voters [and] party workers.” Reynolds and McCormick (1986, 836) present similar evidence. Consequently, party workers hired to monitor the voting window (Argersinger 1985, 672)

had ample opportunity to punish or reward voters accordingly.

The ticket system required very strong party machines, which, in turn, required considerable economic resources to make the system work. However, political machines were oiled not only with money. On the one hand, many “ticket peddlers” (Argersinger 1985, p. 672) were volunteers (Bensel 2004, 17), saving some of the costs needed to maintain the machine. Most of these volunteers “enjoyed the patronage of elected party officials by holding government jobs, drawing public pensions, servicing government contracts, or enjoying special licensing privileges” (p. 17). On the other hand, political appointees “from janitor to secretary of state” and some corporations donated annually part of their salaries and revenues (Reynolds 1980, p. 197). Thus, parties amassed huge amounts of money.

With all these resources flooding the polls on election day, voting was truly an interesting spectacle. On that day, party agents would offer voters plenty of liquor as an incentive to vote the party ticket. Hence, “the street or square outside the voting window frequently became a kind of alcoholic festival in which many men were clearly and spectacularly drunk [to the point that] some could not remember whether or not they had voted” (Bensel 2004, 20). Even before the Gilded Age, American elections were engineered according to these “principles.” When running for the Virginia House, a young George Washington “spent nearly 40 pounds—a considerable sum for the day—on gallons of rum, wine, brandy, and beer; all used to win over the votes of his neighbors” (Campbell 2005, 5).⁷

The Australian ballot system significantly reduced the frequency of most of this malpractice (Rusk 1970, 1221). However, as vote selling and vote buying were so embedded in what was considered normal, the immediate effect of the Australian system was to reduce turnout (Reynolds and McCormick 1986, 851).

Today, the *modus operandi* of clientelism has changed, and both the frequency of vote buying/selling and the importance of party machines have declined. Scholars have pointed out that “party machines are a thing of the past” (Stokes et al. 2013, 230). However, some contemporary accounts remain of vote buying and selling in American elections. For instance, Campbell (2005, 243-244) explains how a Democratic leader in Logan County, West Virginia, accepted \$35,000 in cash to support Senator Kennedy. As the Democratic leader explained, “this money was for one purpose: ‘We bought votes with it [...] that’s the way real politics works.’ ” Other examples are the famous primary election in March 1972 in Chicago (p. 262) and the elections in the coal-rich Appalachian

Mountains during the 1980s (Campbell 2005, p. 275). Similarly, non-academic sources find that during the 2010 elections, “selling votes [was a] common type of election fraud” (Fahrenthold 2012). Others find that “[v]ote-buying is extremely common in *developed* [...] countries” (Leight, Pande, and Ralston 2016, 1). If vote buying is “a thing of the past,” why do we still see it? How common is vote selling? The next two sections attempt to quantify—in an unbiased way—the willingness to sell votes among a representative sample of U.S. voters.

IV. EXPERIMENTAL DESIGN

The study of individual preferences depends on truthful answers. However, under certain circumstances, individuals might not want to answer truthfully, due to social pressure. For instance, to avoid having the interviewer judge them, individuals might not want to reveal having done something illegal, such as selling one’s vote. Failing to consider this systematic source of bias will pose threats to causal inference.

Since list experiments administer two lists of items (one to the control group, one to the treated group), list experiments are well suited to eliciting truthful answers (Blair 2015). Both lists look identical (e.g., each containing the same three items); however, the treatment list traditionally includes a fourth item, the sensitive item related to some socially condemned behavior. Respondents are asked how many items on the list they would endorse, not which ones. For instance, if an experimental subject answers “2,” the interviewer will not know whether that number includes the sensitive item. Consequently, if the survey respondent wants to endorse the sensitive item, the answer will be “masked” by the other items in the list. This concealment makes this technique suitable for studying socially condemned behaviors, such as vote buying (Corstange 2008; González-Ocantos et al. 2012; Corstange 2012; Blair and Imai 2012), drug use (Druckman et al. 2015), sexual preferences (LaBrie and Earleywine 2000), and attitudes toward race (Kuklinski et al. 1997; Redlawsk, Tolbert, and Franko 2010).

Given that both lists are assigned randomly, the mean number of nonsensitive activities that respondents endorse should be equal across the two lists. However, if there are any differences in means between the two groups, the differences should be attributed only to the presence of the sensitive item.

Blair and Imai (2012) and Imai, Park, and Greene (2015) provide a statistical framework to

analyze list data efficiently.⁸ They formalize two assumptions, namely, that there are (1) “no design effects” (i.e., the inclusion of a sensitive item has no effect on respondents’ answers to control items), and (2) “no liars” (i.e., respondents give truthful answers for the sensitive item). When the two assumptions hold and the item counts for types $y = 0$ and $y = 4$ are fully observed,⁹ experimental subjects with item-count types $y = 1$, $y = 2$, and $y = 3$ can be inferred using multivariate techniques that allow for inferring who answered “yes” to the sensitive item. In addition, the statistical analyses permit studying the relationship between preferences over the sensitive item (i.e., vote-selling) and an individual’s characteristics, such as income and party identification. Also, the design includes a “direct” question on the sensitive item, also making possible an estimation of the amount of social-desirability bias.

Collected in 2016, the data ($N=1,479$) are representative at the national level.¹⁰ **Figure A1** shows the geographical distribution of survey respondents, grouped by party identification. The experiment was framed as a study about crime in the United States, not as a study about vote selling.¹¹ While pretesting the study, it was decided that the experiment needed to mask a very serious felony (selling one’s vote) among other equally serious felonies (such as stealing) and other less serious crimes (such as speeding or downloading music illegally from the Internet). Otherwise, the vote-selling item would have stood out among the other items, making it seem totally negative and undoable, and/or making the true purpose of the study obvious.

Before splitting the subject pool into the subjects’ respective experimental conditions, participants were asked to read an excerpt describing four illegal activities (including vote selling).¹² All were formatted as news pieces. The idea was to explain “vote selling” to “newsreaders.”

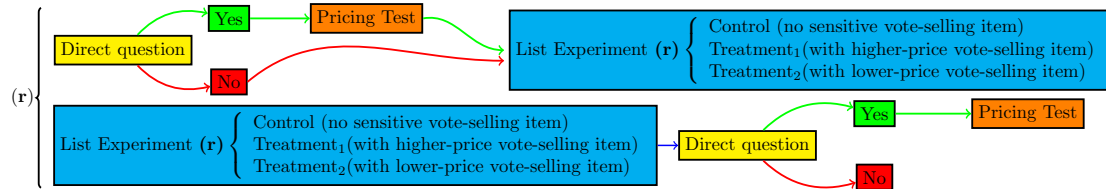


Figure 2: Experimental Flow of the List Design.

Note: This figure shows the flow of the list experiment. Notice that (1) the order in which experimental subjects answered both the direct question and the list experiment was randomized; (2) there are two treatments, one with a selling price of \$100 (“low”) and one with a selling price of \$500 (“high”).

As **Figure 2** suggests, to prevent possible priming effects,¹³ the order in which experimental

subjects answered the direct question¹⁴ and the list experiment were randomly assigned. To be sure, all subjects answered both the direct question and the list experiment. To further prevent the possibility of biased answers when asking the direct question to individuals in the treated group, the direct question stated that the hypothetical possibility of doing one of the illegal things mentioned previously in the excerpt would be randomly assigned. However, all participants were directly asked whether they would be interested in selling their vote. Direct answers were then used to estimate the proportion of “liars.”

As a follow-up, subjects answering “yes” to the direct question answered a pricing test that asked them to indirectly put a price on their votes. Following standard practice in marketing research, participants slid a handle indicating which price was considered “too cheap” for one’s vote. The slide ranged from \$0 to \$1,000, in one-dollar increments. The idea was to capture the respondent’s willingness to sell. The pricing test particularly measures the lowest bound at which the participant would perceive the least economic benefit that was still enough to make selling his/her vote interesting. Moving forward, the list experiment contemplated one control and two possible treatments, each with different vote-selling prices. Since pricing a vote is difficult and based on several pretests, an arbitrarily low (\$100) or an arbitrarily high (\$500) price appeared. Participants randomly assigned to the treatment condition answered only one of these two treatments. More than hard-pricing tests, these two treatments account for possible different elasticities that might have interacted with individual socioeconomic backgrounds, mainly defined by income.¹⁵

Subjects randomly assigned to the control condition answered the following question:

Now, you will have to type HOW MANY, if any, of the following illegal activities you might engage in, assuming you would not go to jail.

- (1) steal an iPod from a large department store
- (2) speed on the highway because you're late for work/school
- (3) download your favorite music from the internet illegally

Type in HOW MANY (NOT WHICH), if any, of these things you would do.

In turn, subjects randomly assigned to the treatment condition answered one of the following two treatments. Subjects assigned to the “low” price condition read the same vignette, but the next line

was added in the third place: (3) **sell your vote to a candidate for \$100**. Those assigned to the “high” price condition read the following line: (3) **sell your vote to a candidate for \$500**.¹⁶

Figure 3 shows the distinct frequencies of participants declaring how many (if any) illegal things they would do. Notice that the figure shows the total number of items, not which ones. For instance, a frequency of “3” does not mean the frequency of the third item, but describes the total number of individuals answering that they would do three of the illegal activities described in the vignette.¹⁷ The order of the items was not randomized, to avoid violating the stable unit treatment value assumption (SUTVA)¹⁸

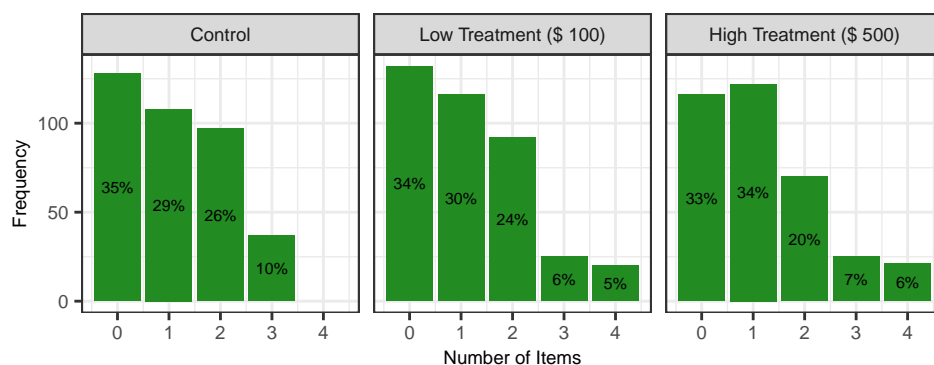


Figure 3: Frequency and Percentages of Subjects Declaring How Many (if any) Illegal Things They Would Do.

Note: Notice that the X-axis denotes the number of items, not which ones. Percentages show proportions per condition.

Showing that the probability of being assigned to any condition is not associated with individual covariates is important. Table 1 shows a multinomial logistic model. The dependent variable is the treatment condition (high treatment, low treatment, and control). The independent variables are observable characteristics captured by a short questionnaire included in the study. Four variables were used: income, education, party identification, and political ideology. These were the same set of variables used when estimating likely vote sellers (below). Conveniently, the base category in the multinomial logistic regression is the control condition. The coefficients in the table are all zeros (and statistically nonsignificant). Consequently, these results show no observable differences between the “high” treatment condition and the control group. The same applies to the “low” condition.¹⁹

The paper acknowledges that considerable friction and transaction costs in the real world might

	High	Low
Ideology	0.019 (0.068)	-0.031 (0.067)
Party Id.	-0.125 (0.083)	0.022 (0.080)
Income	-0.021 (0.022)	0.006 (0.021)
Education	0.049 (0.048)	-0.008 (0.047)
AIC	2449.471	2449.471
BIC	2499.583	2499.583
Log Likelihood	-1214.736	-1214.736

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Reference category is control condition. Intercept was excluded from the table. N = 1,479.

Table 1: Covariate Balance: Multinomial Logistic Regression for Both Treatment Conditions.

Note: The table shows a multinomial logistic regression. The dependent variable is the treatment condition (high, low, control). In both models, the base category is the control condition. The independent variables are observable characteristics captured by a short questionnaire included in the study. This set of covariates is the same as the one used in the statistical analyses of the list experiment. Since all estimated coefficients are close to zero and statistically nonsignificant, we can safely assume that the randomization mechanism worked as expected, i.e., there are no observable differences across the different treatment conditions.

mean that creating a market for vote selling would not be easy. For instance, party identification might increase (or decrease) the cost of selling one’s vote, presumably preventing (or fostering) the transaction. If the party of both sellers and buyers should match, fostering vote selling might represent a win-win situation for both. This experimental design does not consider blocking on party identification, as that might have increased considerably the number of cells.

V. STATISTICAL ANALYSES

I. Would U.S. Citizens Sell Their Vote?

Table 2 shows a simple difference in means analysis between each treated group and the control group. On average, the control group would do 1.116 things on the list. Subjects treated under the “low” condition (\$100) would do 1.182 things on the list, while subjects in the “high” condition (\$500) would do 1.189 things.

Three important points characterize this bivariate analysis. First, the mean differences between treated groups (i.e., “low” and “high” treatments) are statistically zero, implying that neither treatment should introduce design bias into the experiment. Second, while treated subjects do have

Condition	Mean	Difference with Control Condition	Confidence Intervals	t	df	p-value
Low (\$100)	1.182	1.182 - 1.116 = 6.6%	[-9%, 22%]	0.846	748	0.398
High (\$500)	1.189	1.189 - 1.116 = 7.3%	[-8%, 23%]	0.913	700	0.361

Table 2: Differences in Means between Treatments (high and low) and the Control Group.

Note: *Table 2 shows two-tailed t-tests between each experimental treated unit (“low” and “high” conditions) and the control group. The table shows that $0.066 \times 100 = 6.6\%$ of subjects would sell their vote under the “low” condition, while $0.073 \times 100 = 7.3\%$ of subjects would sell their vote under the “high” condition. Also, 95% confidence intervals are shown. It is evident that they are quite wide and not statistically significant.*

slightly higher means when compared to the control group (indicating some vote-selling propensity), these differences are not statistically significant. Third, while not statistically significant, $0.066 \times 100 = 6.6\%$ of subjects would sell their vote under the “low” condition, while $0.073 \times 100 = 7.3\%$ of subjects would sell their vote under the “high” condition. While these estimations score substantially under what is found through the multivariate approach used in this study, as shown below, they are also highly inefficient.

Bivariate calculations are statistically inefficient; hence, the data should be analyzed using multivariate techniques instead. Following the advice of Blair and Imai (2012) and Blair (2015), we took a statistical multivariate approach.²⁰ Exploiting the “low” and “high” treatments, we estimated two identical statistical models. In both models, the outcome variable is the item count of things that subjects would do. The idea is to estimate what we cannot observe (i.e., vote selling), using information that we do observe (i.e., socioeconomic and political variables captured by the questionnaire). The model considers the most common covariates studied in the vote buying literature (Calvo and Murillo 2004; Stokes 2005; Kitschelt and Wilkinson 2006; Nazareno, Brusco, and Stokes 2008; Weitz-Shapiro 2012; González-Ocantos, Kiewiet de Jonge, and Nickerson 2014; Oliveros 2016; Bahamonde 2018)—that is, income, education, party identification, and political ideology.

Leveraging this multivariate approach makes estimating the proportion of hypothetical vote sellers possible. For both the “low” and “high” treatments, Figure 4 shows the proportions of declared vote sellers (“Direct Question”), predicted vote sellers (“List Experiment”), and the difference between the two (“Social Desirability”).²¹ Substantively, the figure suggests that after combining the estimates of the “low” and “high” treatments, approximately 25% of the nationally representative sample would be willing to sell their vote.²² While a considerable proportion answered the direct question

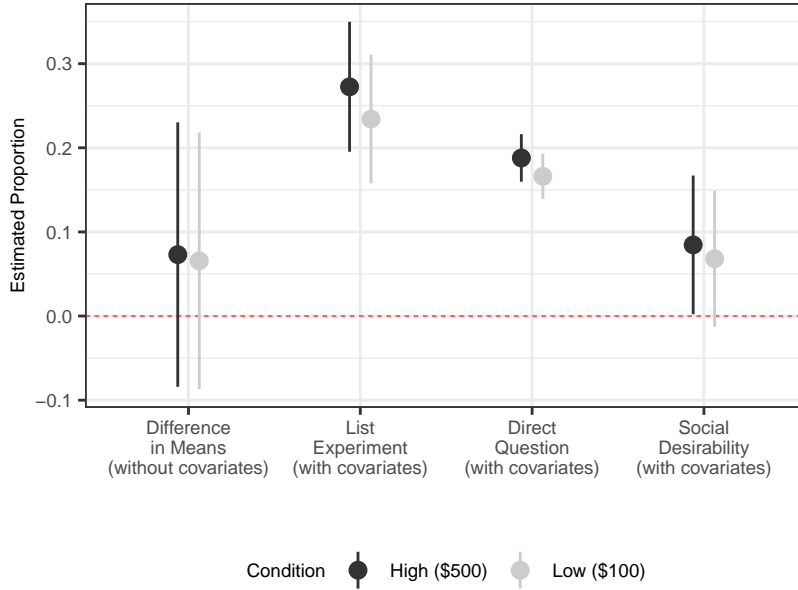


Figure 4: List Experiment Data: Declared and Predicted Vote-Sellers.

Note: The figure summarizes [Table 2](#) by showing the simple difference in means (without covariates). It also shows the proportion of declared (“Direct Question”) and predicted (“List Experiment”) hypothetical vote sellers, and the difference (“Social Desirability”). The three sets of main estimates were obtained via a multivariate procedure (including covariates). Combining both “low” and “high” treatments, 25% would be willing to sell their votes. And of those who answered affirmatively when asked directly (18%) an estimated additional 8% lied about it. “Liars” answer the direct question negatively, but they are likely sellers. The figure shows 95% confidence intervals. There are two arbitrarily “low” and “high” vote-selling prices. The reason for having both was to control for possible price elasticities. The figure suggests some small differences that are not statistically significant. Consequently, these arbitrary pricing decisions do not threaten the experimental design.

affirmatively (18%),²³ the analyses still suggest that survey respondents systematically underreported their true answers—that is, approximately 8% of the nationally representative sample would have lied.²⁴

The difference-in-means approach in [Table 2](#) suggests that between 6.6% and 7.3% would be willing to sell their votes. However, the multivariate approach in [Figure 4](#) suggests that 25% would be willing to do so. While at first these differences might seem huge, they are not. As the literature suggests, multivariate approaches to analyzing list-experiment data are far more efficient (Blair and Imai 2012; Blair 2015). Within the framework of regression analysis, the difference-in-means approach is just a bivariate lineal model.²⁵ Instead, the multivariate approach is also a lineal

model, but it incorporates covariates. We claim that due to the multivariate’s greater efficiency than that of the difference-in-means approach, the former is a far better approach than the latter. One way of showing the efficiency of a statistical model is by examining its standard errors (King 1986, 676): The worse the data’s fit is, the greater the standard errors are, the more imprecise the model is, and the wider are the confidence intervals. Considering the statistical uncertainty of both methods (depicted in Figure 4), it is easy to see that the multivariate approach is far more efficient than the difference-in-means approach. Since it uses more information when fitting the data (the covariates), it gives more precise estimates (narrower confidence intervals). Furthermore, going beyond efficiency issues, the estimates of both methods are statistically indistinguishable. Since the confidence intervals of both approaches overlap, it is not possible to say that the estimated 7.3% and 6.6% are “smaller” than the estimated 25%.²⁶

Moving forward, the estimated proportion of vote sellers—“List Experiment” in Figure 4—is calculated using information from subjects with fully observable preferences, i.e., subjects with an item count of 0 or 4. We know that the former would not do anything, and the latter would do all things mentioned in the list (including the sensitive item). Using the identified covariates (income, education, party identification, and political ideology), a model is fitted to predict all subjects with 0’s and 4’s on the left-hand side. Using this information makes obtaining individual-level vote-selling predictions possible, i.e., participants who would do 1, 2, or 3 things on the list (shown in Figure A2 in the Appendix). Then, these individual-level predictions are compared with the direct question that all experimental subjects answered. If a subject is a predicted vote seller but answers the direct question negatively, it is inferred that due to concerns of social desirability, she might have chosen to lie.

II. What is the Price for which U.S. Citizens Would Sell Their Vote?

Participants were also asked to declare which price they considered “too cheap” for their vote. The intention was to capture the respondent’s willingness to sell. The test measures the lowest bound at which participants would perceive the least possible economic benefit but enough to make them sell. Since it is the lowest threshold, the understanding is that a higher price will still be economically attractive.

The results indicate that the average survey respondent would sell his/her vote for \$418 (N = 189), a very expensive price. These results are not unrealistic. While the selling price is very high, it

matches what others have found. Bahamonde (2018, 52) finds that clientelist political parties in Brazil do target affluent voters at considerably higher prices. Part of the argument is that higher levels of economic development not only raise personal income, but also shift the broker’s vote-buying capacity upward.²⁷ That is, higher income does not necessarily stop vote buying; it just makes it more expensive.²⁸

Stokes et al. (2013) analyze the (im)possibility of expensive vote selling. Industrialization has driven up the median income of the electorate, increasing the selling price while turning vote buying into an increasingly expensive strategy for winning elections. Thus, from the demand-side (parties), vote buying is no longer an efficient mass strategy for party machines. Evidently, with the selling price so expensive, political parties cannot catch up with the supply-side, making vote buying in the United States a rare event (as Figure 1 suggests). This situation has forced party machines to turn to other, less prohibitively costly alternatives. Thus, these results suggest that from the supply-side (i.e., voters), the vote is still up for sale, only for a very high price that party machines cannot afford.

Since the pricing test is based on the direct question, its results require a word of caution. The list experiment does suggest that some respondents lied when directly asked if they would sell their vote. Consequently, we should expect the pricing test to be biased to some degree. Also, only a small proportion of respondents answered the direct question affirmatively. In addition, prices are the product of supply-and-demand dynamics. In this context, prices result from the interaction between parties (buyers) and voters (sellers). This research design observes only the sellers’ side. Hence, we limit our inferences even more by thinking about these results as only suggestive of some willingness to sell. Hence, more than acting as definitive and final pricing tests, these findings do seem to suggest that the vote-selling price is high enough to deter political parties from engaging in vote selling. Finally, future research should design and conduct more complex studies where the design incorporates supply-and-demand dynamics.

III. Who are the Most-Likely Vote Sellers?

The proportion of likely vote sellers was estimated using a multivariate approach. The variables used were the most common explanatory factors studied in the clientelism literature. Ultimately, this procedure allows for profiling participants into likely vote sellers. Figure 5 shows estimated vote-selling probabilities at different levels of all variables used in the multivariate approach.

The analyses suggest that Democrats and Liberals are more likely to sell. These findings are in

line with research that studies the different constitutive values of Liberals and Conservatives. Political psychologists have found that compared with Conservatives, Liberals construct their moral systems primarily upon narrower psychological foundations. Particularly, Liberals consider less important both the authority/respect and the purity/sanctity dyads (Graham, Haidt, and Nosek 2009, 1029). This might lead Liberals to engage more frequently in behaviors that might be considered “wrong,” such as vote selling. In fact, Gray, Schein, and Ward (2014, 7) explain that Conservatives “see impure violations as relatively more wrong.”

Unlike the conventional wisdom (Kitschelt 2000; Calvo and Murillo 2004; Weitz-Shapiro 2012; Carlin, Singer, and Zechmeister 2015), Figure 5 shows that education and income levels do not make vote selling more likely. Poverty has long been associated with vote selling. Brusco, Nazareno, and Stokes (2004), Stokes et al. (2013) and Nazareno, Brusco, and Stokes (2008) explain that since the poor derive more utility from immediate transfers relative to returns associated with future (and uncertain) policy packages, clientelist political parties only target the poor. For instance, Weitz-Shapiro (2014, 12) explains that “[a]lmost *universally*, scholars of clientelism treat and analyze [this] practice as an exchange between politicians and their poor clients.”²⁹ The evidence presented in this paper aligns with that of others who have recently questioned the importance of this canonical predictor. Szwarcberg (2013) “challenges the assumption [that brokers] will always distribute goods to low-income voters in exchange for electoral support,” while González-Ocantos et al. (2012) and Holland and Palmer-Rubin (2015) find that income had little or no effect on vote buying.³⁰ Notably, Bahamonde (2018) explains that brokers target individuals when they are identifiable and groups when brokers need to rely on the spillover effects of clientelism. Both mechanisms occur regardless of individual levels of income.

There do seem to be important substantive differences between the “low” and “high” vote-selling treatments. That is, factors that heavily determine economic status (income and education) seem to be more elastic to marginal increments in the buying price. As Figure 5 shows, low-income and less-educated individuals are willing to sell their votes in a similar proportion to wealthier and more-educated respondents. However, poorer and uneducated individuals are more willing to sell their votes, conditional on higher prices. This might indicate that for them, behaving illegally is worthwhile but only when the payoff is “large enough.” These results are in line with those of experimental and applied economists who argue that “risk aversion decreases as one rises above the poverty level and decreases significantly for the very wealthy” (Riley and Chow 1992, 32). In other

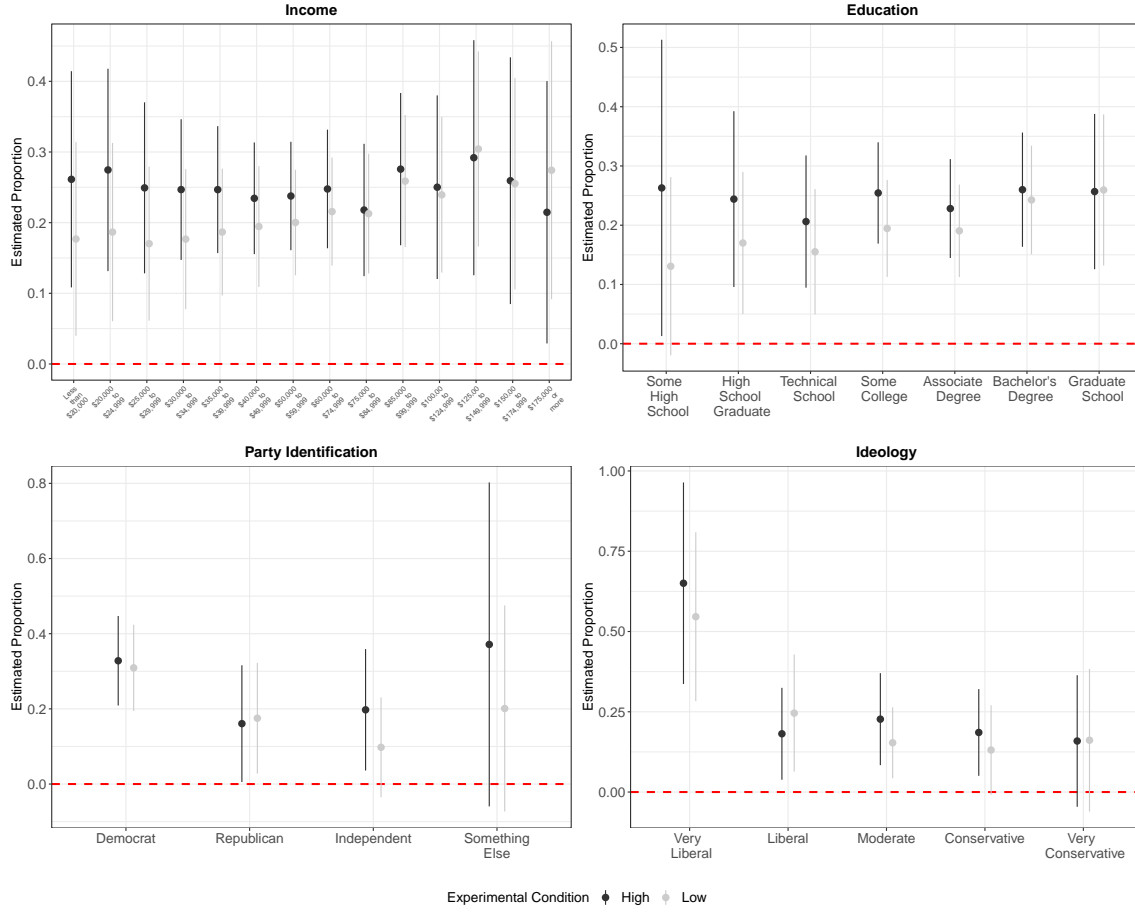


Figure 5: List Experiment: Covariates Used to Estimate Likely Vote-Sellers.

Note: These variables were used in the multivariate statistical model to estimate individual-level probabilities of vote-selling. The figure shows the predicted probabilities and their corresponding 95% confidence intervals for income, education, party identification, and ideology. Since the vote-selling prices were set arbitrarily, the reason for two experimental conditions (“low” and “high”) was to control for possible price elasticities. While there are some perceptible changes, they are not statistically significant. Consequently, these arbitrary decisions do not threaten the identification strategy.

words, less-educated and low-income individuals, who are more fragile and precarious, tend to avoid risks and, hence, illegal activities. On the contrary, higher-income and more-educated individuals seem unaffected by the different stimuli and sell their vote in the same proportion, regardless of the price. For instance, highly educated individuals (graduate school level) sell their vote in the same proportion, under both the “low” (26%) and “high” (26%) conditions.

VI. GENERAL DISCUSSION

Two conflicting pictures emerge. On the one hand, leaving aside concerns about social desirability bias, we “know”—using non-experimental data—that most people have never been offered the possibility to sell their vote (as per [Figure 1](#)). On the other hand, the results presented here strongly suggest that they *would*. While buyers (e.g., parties) are not buying, a large proportion of latent vote sellers is willing to sell their vote.

While vote buying/selling in the United States was commonplace during the nineteenth century, higher median incomes have increased the cost of this strategy as a feasible tool to win elections, in turn, making vote buying rare in the United States. The paper confirms this hypothesis by suggesting that an important estimated proportion of U.S. voters—25%—is very much willing to sell their vote, but for an estimated very expensive price—\$418. Overall, these results are striking, and the author is not aware of any other experimental design in which subjects in an industrialized democracy are asked whether they would sell their votes, and, moreover, which produces positive results. The paper began by establishing the tension between supply and demand sides within a clientelist relationship and noting that qualitative research usually focuses on vote selling, while quantitative studies usually focuses on vote buying. Furthermore, most of the literature concentrates its efforts on studying developing countries, mostly paying attention to realized clientelist transactions. As discussed, both aspects pose threats of selection bias to our inferences. This paper tries to fill these gaps by studying hypothetical vote selling via an experimental design implemented in an advanced democracy.

While the paper is rather descriptive, the author believes that the exercise was worth pursuing. The experimental evidence of a large critical mass willing to sell their votes in a developed country is novel. It is hoped that the paper sets the stage for future research and encourages other scholars to field the experimental design presented here in a comparative setting, to include both developed and developing countries. Future research should also consider different values placed on different

offices.³¹ It is reasonable to think that presidential, Senate, House, state-legislature, mayoral, and city-council elections produce different incentives and constraints regarding buying and selling votes. Also, future research should consider blocking party identification—for example, designing a more complex experiment, in which not only the price varies but also the vote-selling treatment is partisan.

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VII. APPENDIX

I. Experimental Manipulations and Vignettes

Distractor Paragraph. The next paragraph was used to distract subjects from the main purpose of the study, and also to define vote selling.

Washington, D.C.- A department store downtown had a robbery incident last week, reporting several missing iPods from their inventory. Authorities also inform that a group of local residents are trying to ``sell'' their votes to political candidates ahead of a local election for city council. Residents approached some of the candidates running for office and offered to vote for that candidate in return for monetary compensation. In a different subject matter, the local police station released a report on driving habits and behaviors in the Capitol district last week. Finally, cyber-crime has become an increasingly serious issue in the area in the past few year.

Direct Question. All subjects read the next paragraph, and then *all* answered the direct question:

Now you will be entered into a random lottery for the opportunity to do ONE of the illegal things you just read before. This means that you might be randomly offered to hypothetically do ANY of the activities mentioned before.

After a random assignment, you have been selected for the opportunity to hypothetically sell your vote. This means that you will have the hypothetical opportunity to accept money from a candidate for your vote. Would you be willing to accept the offer, assuming you would not go to jail? By selecting ``Yes,`` you could earn up to \$1,000.

II. Testing for Design Effects

Table A1: *Test for List Experiment Design Effects.*

Respondent Types	<i>Low Condition</i>		<i>High Condition</i>	
	<i>Estimate</i>	<i>Standard Error</i>	<i>Estimate</i>	<i>Standard Error</i>
(y = 0, t = 1)	0.0031	0.0346	0.0183	0.0351
(y = 1, t = 1)	-0.0063	0.0349	-0.0345	0.0353
(y = 2, t = 1)	0.0169	0.0226	0.0299	0.0237
(y = 3, t = 1)	0.0519	0.0113	0.0593	0.0126
(y = 0, t = 0)	0.3429	0.0242	0.3277	0.0249
(y = 1, t = 0)	0.2982	0.0347	0.3264	0.0351
(y = 2, t = 0)	0.2453	0.0299	0.2322	0.0307
(y = 3, t = 0)	0.0481	0.0193	0.0407	0.02

Note: Since the Bonferroni-corrected p-values of the *low* (0.8567) and *high* (0.3298) conditions are above the specified alpha (0.05), I fail to reject the null of no design effects.

III. Geographical Distribution of Survey Respondents

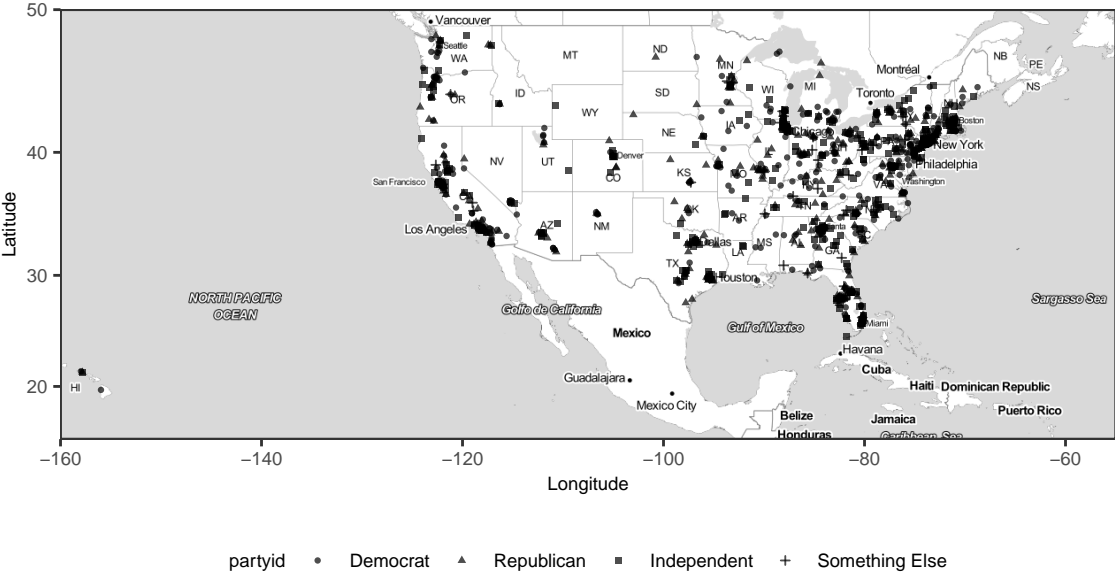


Figure A1: Geographical Distribution of Survey Respondents by Party Identification.

IV. Individual Predictions

The vertical axis of **Figure A2** shows the estimated probabilities of the entire experimental sample, sorted across the horizontal axis. The figure is relevant as it openly shows the amount of uncertainty of the statistical estimates. Ultimately, these individual-specific predictions will be used to profile likely vote sellers.

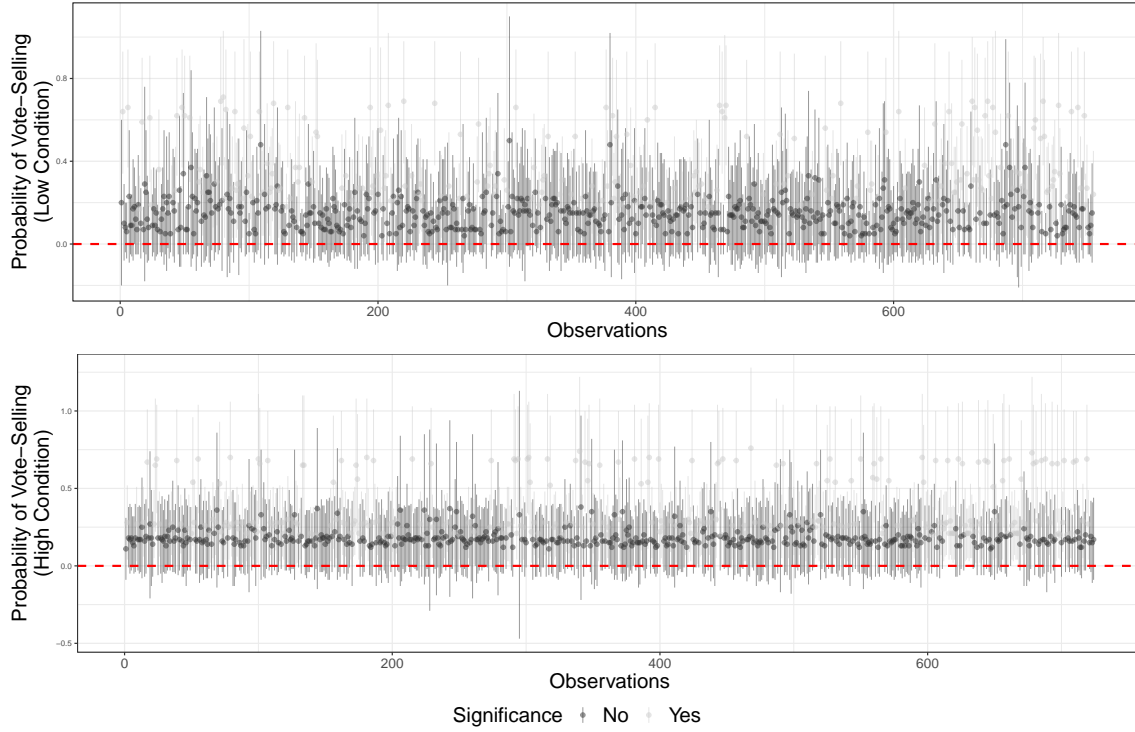


Figure A2: Individual Estimated Probabilities of Vote-Selling.

Note: Figure shows the individual probabilities of vote-selling ($N = 1,479$) under the “low” and “high” conditions. After fitting the model, and following the advice of Blair and Imai (2012) and Imai, Park, and Greene (2015), individual probabilities of vote-selling under the “low” and “high” conditions were estimated. The figure also shows 95% confidence intervals.

NOTES

1. It is important to note that clientelism as a practice involves more than just buying or selling votes. Other goods might be involved in the clientelist transaction—for instance, public jobs or public infrastructure, e.g., see for example Dixit and Londregan (1996), Calvo and Murillo (2004), and Khemani (2015). However, this paper’s focus is on just vote buying and vote selling.

2. I thank one of the anonymous reviewers for this comment.

3. For the British case during the Victorian era see Kam (2017).

4. Relatedly, see Levitsky and Ziblatt (2018).

5. The U.S. Bureau of the Census did not exist. Consequently, it was relatively easy to invent names, “repeat,” or use any other subterfuge to “stuff the ballot box.” In fact, “a St. Louis politician admitted registry fraud but argued that there was no proof that the names he copied into the registry were of real people and, therefore, no crime had been committed” (Argersinger 1985, 680).

6. Judges used as a rough proxy whether the prospective voter had the ability to grow a beard (Bensel 2004, 20).

7. \$1,250 in 2017 U.S. dollars. Conversion based on Williamson (2018).

8. While list experiments are common, researchers unfortunately “[utilize] only a difference in means estimator, and [do] not provide a measure of the sensitive item for each respondent” (Glynn 2013, 159).

9. For a hypothetical treatment list of four items.

10. *Research Now SSI* collected the data between March 2 and March 6. Survey respondents belong to the online panel owned and administered by SSI. Notice of IRB exemption Protocol #E16-292 is kept on file at the Office of Research and Regulatory Affairs of Rutgers University.

11. To isolate the risks and costs associated with engaging in any illegal activity, the next phrase was included: “assuming you would not go to jail.”

12. See [Appendix](#) for wording.

13. Blair and Imai (2012, 54) explain that asking the direct question to individuals in the treated group might bias the results.

14. See Appendix for wording.

15. Holland and Palmer-Rubin (2015, 1189) explain that “the poor are thought to be more susceptible to vote buying.”

16. Since one of the two sentences was added, item (3) `download your favorite music from the internet illegally` was moved to the fourth place.

17. The experimental design passes the standard tests for design effects (floor and ceiling effects). See Table A1.

18. Morton and Williams (2010, 98) explain that the treatment should be invariant or “stable.”

19. I thank the anonymous reviewer at *Acta Politica* for this suggestion.

20. The R package `list` was used (Blair 2015). The estimation method used was the “ml” and the maximum number of iterations was 200,000. The remaining arguments of the package were left at their default values.

21. Since the estimated quantities do not vary across the different treatments (“low” and “high”), it is reasonable to think that there are no specific concerns associated with the (arbitrarily) chosen prices.

22. This number was calculated averaging over the “high” (27%) and “low” (23%) estimates.

23. This number was calculated averaging over the “high” (19%) and “low” (17%) estimates.

24. This number was calculated averaging over the “high” (8%) and “low” (7%) estimates.

25. With just a constant 1 on the right-hand side of the equation.

26. I thank the two anonymous reviewers of *Acta Politica* for stimulating this discussion.

27. Similarly, see Abramo and Speck (2001, 14). For the Philippine case, see Schaffer (2004).

28. In fact, there is some anecdotal evidence suggesting that a broker purchased one man’s vote for \$800 during the 2010 elections in eastern Kentucky (Shawn 2012, 6).

29. My emphasis.

30. Relatedly, González-Ocantos, Kiewiet de Jonge, and Nickerson (2014, 205) and Corstange (2012, 494)

also find very weak results for education in Peru and Nicaragua, and in Lebanon, respectively.

31. I owe this point to Christopher Chambers-Ju.