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

Hector Bahamonde

*Assistant Professor • Instituto de Ciencias Sociales • O′Higgins University

hector.bahamonde@uoh.cl

www.HectorBahamonde.com

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Abstract

In nineteenth-century United States politics, vote-buying was commonplace. Nowadays, vote-buying seems to have declined. Yet, 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 votes. Would they sell their votes (and at what price), or would they consistently opt out of vote-selling? Exploiting a novel experimental dataset representative at the national level, 1,479 U.S. voters participated in an online list experiment in 2016, and the results are striking: Approximately 25% would sell their votes for $418. Democrats and Liberals are systematically more likely to sell, while education levels and income do not seem to impact vote-selling.

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

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Abstract

In nineteenth-century United States politics, vote-buying was commonplace. Nowadays, vote-buying seems to have declined. Yet, 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 votes. Would they sell their votes (and at what price), or would they consistently opt out of vote-selling? Exploiting a novel experimental dataset representative at the national level, 1,479 U.S. voters participated in an online list experiment in 2016, and the results are striking: Approximately 25% would sell their votes for $418. Democrats and Liberals are systematically more likely to sell, while education levels and income do not seem to impact vote-selling.

I. Vote-Sellers and Vote-Buyers

Prior research on this subject 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). While this is an important question, it unfortunately overlooks the issue of whether citizens would sell their votes. In fact, Nichter and Peress (2017) explain that studies continue to view clientelism typically as a top-down process, generally overlooking citizens' demands. A bottom-up reconceptualization is necessary since several questions pertaining to vote-sellers remain unanswered. For instance: *What would voters do if offered the chance to sell their votes? Would they sell their votes (and at what price), or would they consistently opt out of vote-selling?*[1]

To illustrate the issue at hand, Figure 1 shows responses of U.S. citizens when asked whether a candidate or a member of a political party has offered something in exchange for their votes, completely ignoring voters' preferences. The figure begs the question of whether survey respondents who answered "never" would still be willing to sell their votes.
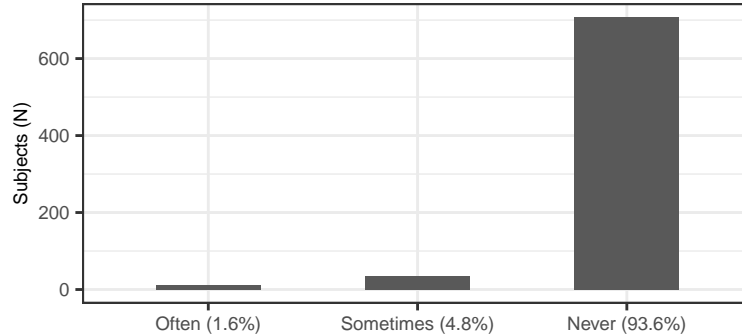
Figure 1: Frequency of Clientelism in the United States (2010).
> **Note**: *Figure shows the frequency of survey respondents, N = 755.*
> **Source**: *LAPOP, 2010 wave for the United States. Question is* `clien1`: *"In recent years and thinking about election campaigns, has a candidate or someone from 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?"*

This note proposes that the clientelism literature should "bring the voters back in." Despite a solid vote-buying literature, we lack a proper body of vote-selling research. A unified framework

that stresses the preferences and incentives of both sellers and buyers is missing. Thus, this paper's analytical contribution is tackling this problem as a story of demand and supply, by explicitly considering the preferences of those who have the votes (voters), and those who demand them (parties). The current demand-side bias in the literature gives an incomplete picture; overlooking the supply-side gives the falsely optimistic impression that U.S. voters systematically oppose vote-buying and, "thus," almost never engage in clientelism (as Figure 1 strongly suggests).

Moreover, whether studies focus on vote-buying or vote-selling has depended, in part, on epistemological rather than theoretical decisions. In general, ethnographers and historians have focused on the supply-side, emphasizing vote-selling (see, for example, Posada-Carbó (1996), Sabato (2001), Auyero (2000), Szwarcberg (2013), and Borges (2019)).[2] Unfortunately, except for several important quantitative studies (see, for instance, Hicken et al. (2015, 2018), Corstange (2012), and Nichter and Peress (2017)), the emphasis remains on studying vote-buying.

Most quantitative contributions to vote-selling are generally formal/theoretical rather than empirical (Vicente and Wantchekon 2009). Additionally, most empirical studies have been conducted in developing countries. For instance, González-Ocantos, Kiewiet de Jonge, and Nickerson (2014) designed a list experiment to study hypothetical vote-buying in Latin America. Thus, previous quantitative empirical studies do not offer satisfactory answers to broader questions about vote-selling outside of the developing world.

This paper makes a methodological contribution, shedding light on these issues by leveraging a list experiment in a consolidated democracy. Given that the paper focuses on eliciting truthful preferences (regardless of whether the behavior is hypothetical), list experiments seem the ideal strategy to pursue.

In 2016, a novel dataset that was 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. Thanks to this experiment, it became possible to identify the demographic factors that would make U.S. voters more likely to sell their votes and at what price, and whether they would systematically lie about selling their votes.

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 at an ideal price of $730, 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

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more likely to sell than Republicans. Education and income levels do not seem to have a systematic impact on vote-selling.

While this paper essentially describes the phenomenon, it leaves for future research further considerations about the causes of hypothetical vote-selling in the United States. Ultimately, this paper is an attempt to bridge the gap between the supply-side and the demand-side, i.e., vote-sellers and vote-buyers, by reporting unprecedented high levels of hypothetical vote-selling.

II. The United States as a Case

At first, many advanced democracies were clientelistic 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. Bensel 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" (Stokes et al. (2013, 227)). In general, students of American political development have analyzed vote-buying in detail, confirming both its early development and its generalized practice (Bensel 2004; Campbell 2005).[3]

However, vote-buying currently seems to have declined considerably, for two competing reasons. Stokes et al. (2013, 201) have shown 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. In fact, Figure 1 suggests that 93.6% of U.S. respondents have never received a clientelistic 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 hard 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 citizens give to American electoral institutions, 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 of the 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 a number of 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 a number of electoral malpractices.[5] First and foremost, 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. And, since party workers were hired to monitor the voting window (Argersinger 1985, 672), this gave 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" (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 part of their salaries on a yearly basis (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 their 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 [up 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 these malpractices (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 lower turnout levels (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/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 in exchange for supporting 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 (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 in 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 exactly the same (e.g., each containing the three same items); however, the treatment list includes (traditionally) 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 towards 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, since the design included a "direct" question on the sensitive item, it was also possible to estimate the amount of social-desirability bias.

The data (N=1,479) were collected in 2016 and are representative at the national level.[10]

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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 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 from the Internet). Otherwise, the vote-selling item would have stood out among the other items, making the vote-selling item seem totally negative and undoable, and/or making the true purpose of the study obvious.

Before splitting the subject pool into their respective experimental conditions, participants were asked to read an excerpt in which four illegal activities were described (including vote-selling).[12] All were formatted as news pieces. The idea was to explain "vote-selling" to "newsreaders."
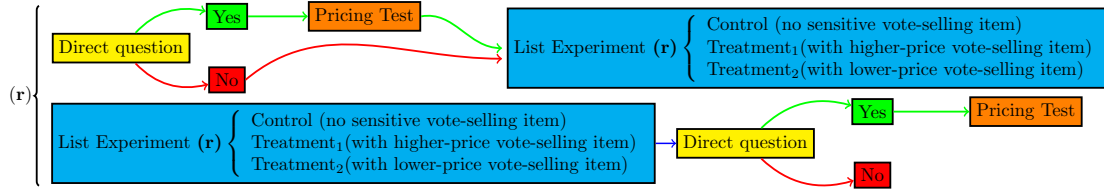
Figure 2: Experimental Flow of the List Design.
Note: *This figure shows the flow of the list experiment. Notice that (1) the order in which experimental subjects answered both the direct question and the list experiment was randomized; (2) there are two treatments, one with a selling price of $100 ("low") and one with a selling price of $500 ("high").*

To prevent possible priming effects[13] (and as Figure 2 suggests), the order in which experimental subjects answered the direct question[14] 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 to do 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 votes. Direct answers were then used to estimate the proportion of "liars."

Subjects answering "yes" to the direct question answered, as a follow-up, a pricing test that asked them to indirectly put a price on their votes. Following standard practice in marketing research, participants slid two handles, one indicating which price was considered "too cheap," and another indicating which price was considered "too expensive" for one's vote (Figure 3). Both sliders ranged

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from \$0 to \$1,000, in one-dollar increments. The intersection of the "too cheap" and "too high" empirical distributions was used to estimate the optimal selling price for a vote (Figure 6).

Which of the following prices (in USD \$) would you consider...

0   200   400   600   800   1000

too cheap for your vote?

too expensive for your vote?

Figure 3: Pricing Test.

Note: As Figure 2 shows, all participants slid both handles, one indicating which price was considered "too cheap," one indicating which price was considered "too high" for one's vote. The intersection of the "too cheap" and "too high" empirical distributions was used to estimate the optimal selling price for a vote.

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 a number of pretests, it was decided to consider both an arbitrarily low (\$100) or an arbitrarily high (\$500) price. 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.
```

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In turn, subjects randomly assigned to the treatment condition answered one of the following two treatments. Subjects assigned to the "low" price condition read exactly 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 4 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 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, or "SUTVA."[18]
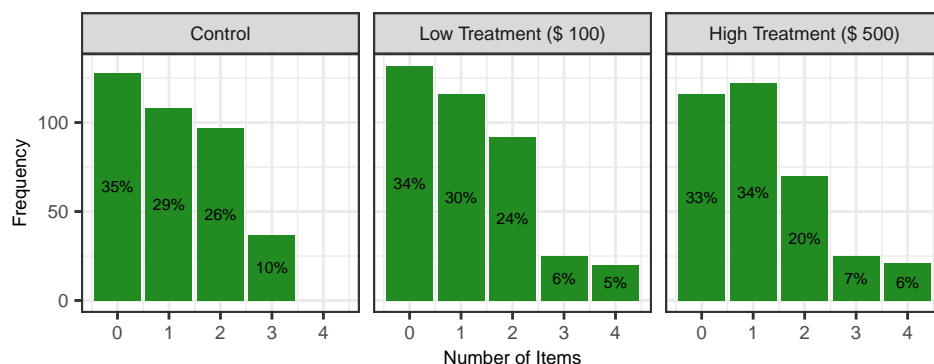
Figure 4: **Frequency and Percentages of Subjects Declaring How Many (if any) Illegal Things They Would Do**.
Note: *Notice that the X-axis denotes the number of items, not which ones. Percentages show proportions per condition.*

It is important to show that the probability of being assigned to any condition is not associated with individual covariates. 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 are 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 close to zero (and statistically non-significant). Consequently, these results show that there are not

	High	Low
Ideology	0.019	−0.031
	(0.068)	(0.067)
Party Id.	−0.125	0.022
	(0.083)	(0.080)
Income	−0.021	0.006
	(0.022)	(0.021)
Education	0.049	−0.008
	(0.048)	(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.*

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 mean that actually 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. For instance, if the party of both sellers and buyers should match, that might represent a win-win situation for both by fostering vote-selling. This experimental design does not consider blocking on party identification, as that might have increased the number of cells considerably.

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 on the list. Subjects treated under the "low" condition ($100) would do 1.182 on the list, while subjects in the "high" condition ($500) would do 1.189 things.

There are three important points regarding this bivariate analysis. First, the mean differences between treated groups ("high" and "low" treatments) is statistically zero. That implies that neither treatment should introduce design bias into the experiment. Second, while treated subjects do

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

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 ("high" and "low" conditions) and the control group. The table shows that $0.073 \times 100 = 7.3\%$ of subjects would sell their votes under the "high" condition, while $0.066 \times 100 = 6.6\%$ of subjects would sell their votes under the "low" condition. Also, 95% confidence intervals are shown. It is evident they are quite wide, and they are not statistically significant at conventional levels.*

have slightly higher means when compared to the control group (i.e., indicating some propensity to sell their votes), these differences are not statistically significant. Third, while not statistically significant, $0.073 \times 100 = 7.3\%$ of subjects would sell their votes under the "high" condition, while $0.066 \times 100 = 6.6\%$ of subjects would sell their votes under the "low" condition. As shown below, these figures score way under what is found in this experimental design.

However, in this context, bivariate calculations are statistically inefficient, and the data should be analyzed using multivariate techniques instead. Following the advice of Blair and Imai (2012) and Blair et al. (2016), a statistical multivariate approach was taken.[20] Exploiting the "high" and "low" treatments, two identical statistical models were estimated. 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 (vote-selling), using information we do observe (socioeconomic and political variables captured in the questionnaire). Thus, 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 5 shows the proportions of declared vote-sellers ("Direct Question") and 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 affirmatively (18%),[23] the analyses still suggest that survey respondents systematically
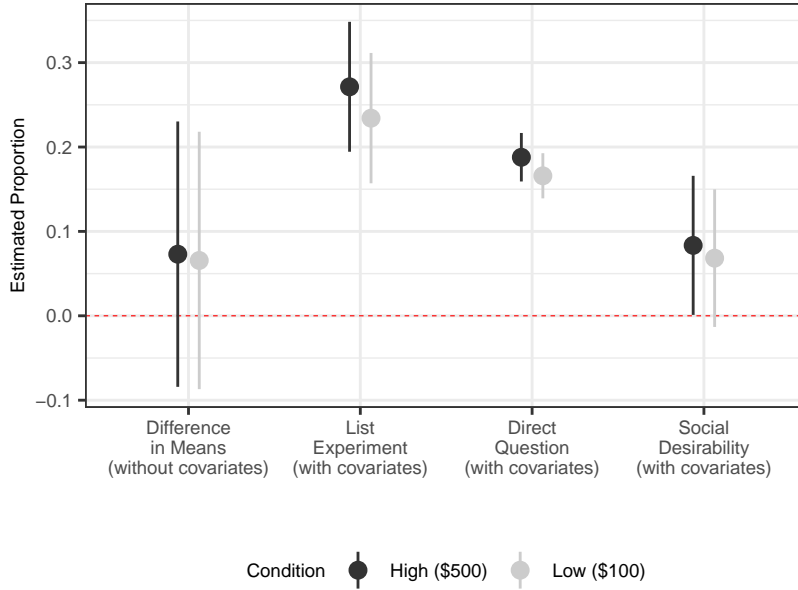
Figure 5: List Experiment Data: Declared and Predicted Vote-Sellers.
> **Note**: *The figure summarizes Table 2 by showing simple difference in means (without covariates). It also shows the proportion of declared ("Direct Question") and predicted ("List Experiment") hypothetical vote-sellers, and its difference ("Social Desirability"). The three sets of main estimates are parametric (i.e. estimated via a multivariate procedure). Combining both "high" and "low" treatments, 25% would be willing to sell their votes. And of those who answered affirmatively when asked directly (18%) there is an estimated additional 8% who lied about it. "Liars" answer the direct question negatively, but in reality, they are likely sellers. The figure shows 95% confidence intervals. There are two arbitrarily "high" and "low" vote-selling prices. The reason for having both was to control for possible price elasticities. The figure suggests some small differences. However, they are not statistically significant. Consequently, these arbitrary pricing decisions do not threaten the experimental design.*

underreported their true answers: approximately 8% of the nationally representative sample would have lied.[24]

The difference in means approach in Table 2 suggests that between 7.3% and 6.6% would be willing to sell their votes. However, the multivariate approach in Figure 5 suggests that 25% would be willing to do so. While at first these differences might seem huge, they actually are not. As the literature suggests, multivariate approaches to analyzing list experiment data are far more efficient (Blair and Imai 2012; Blair et al. 2016). Within the framework of regression analysis, the difference in means approach is just a bivariate lineal model.[25] Instead, the multivariate approach incorporates covariates. One way of showing the efficiency of a statistical model is by examining its

standard errors (King 1986, 676): the worse fitting the data, the bigger the standard errors, the more imprecise the model (and the wider the confidence intervals). Figure 5 shows the estimations and their corresponding confidence intervals. Considering the statistical uncertainty of both methods, it is easy to see that the multivariate approach is far more efficient than the difference in means approach. Going beyond efficiency issues, the estimates of both methods are statistically indistinguishable from each other. In fact, 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]

The estimated proportion of vote-sellers—"List Experiment" in Figure 5—is calculated using information from subjects with fully observable preferences, i.e., subjects with an item count of 0 or 4. For the former, we know they would not do anything, while for the latter, we know they 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, it is possible to obtain individual-level vote-selling predictions, 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 answered by all experimental subjects. If a subject is predicted to be a vote-seller, but answers the direct question negatively, it is inferred that due to concerns of social desirability, s/he might have chosen to lie.

II.   What is the Price for which U.S. Citizens Would Sell their Votes?

Participants were also asked to declare which price they considered "too cheap" and which was "too expensive" for their votes.[27] With these two pieces of information, it was possible to construct two supply curves: the "too cheap" curve, representing the lower boundary (mean = $418) and the "too expensive" curve, representing the upper boundary (mean = $744). Substantively, the ideal selling price is located where the curves intersect.

The results shown in Figure 6 indicate that the average survey respondent would sell his/her vote for $730, 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 non-poor voters at considerably higher prices. Part of the argument is that higher levels of economic development not only raise personal incomes, but also shift the broker's vote-buying capacities upward.[28] That is, higher incomes do not necessarily stop vote-buying but
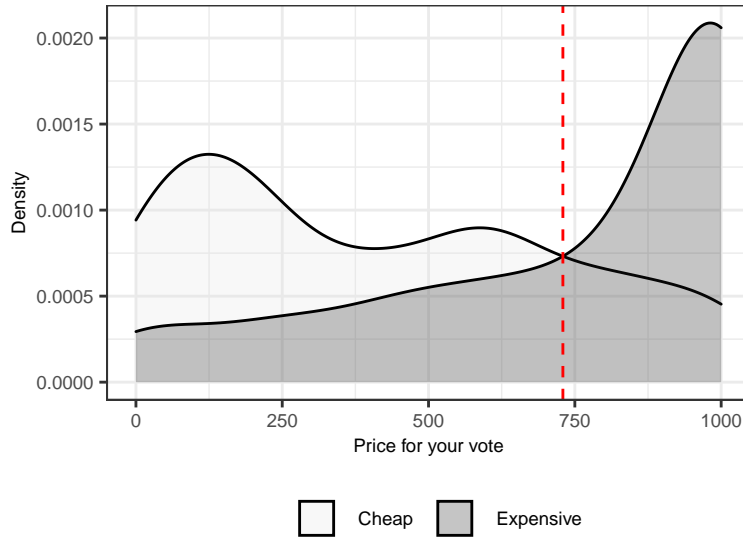
Figure 6: Pricing Test: Ideal Selling Price.
> **Note**: *Subjects who answered "yes" to the direct question (N = 189) were asked to price their votes via a pricing test (see Figure 3). This figure shows the empirical distributions of the "too cheap" and "too expensive" answers. The intersection of these two supply curves (the vertical dashed line) represents the estimated optimal selling price. The data suggest that the right price for one's vote is $730.*

do shift its price higher.[29]

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 and turning vote-buying into an increasingly expensive strategy for winning votes. 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 for sale, only for a very high price that party machines cannot really afford.

Moving forward, the pricing test having been constructed based on the direct question requires a word of caution. The list experiment does suggest that some respondents lied when directly asked if they would sell their votes. Consequently, we should expect the ideal vote-selling price to be

biased to some degree as well. A small proportion of likely vote-sellers answered the direct question affirmatively, but we do not know how many of those who answered the direct question negatively (i.e., that they would not sell) would, in fact, sell their votes. However, 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 more frequently in vote-selling.

III.   Who are the Most-Likely Vote-Sellers?

The proportion of likely vote-sellers was estimated using a multivariate statistical model. The variables were the most common explanatory factors in the clientelism literature. Conditional on observable characteristics, this procedure ultimately allows for profiling participants into likely vote-sellers. Figure 7 shows estimated vote-selling probabilities at different levels of all variables used in the model.

The analyses suggest that Democrats and Liberals are systematically 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 (Calvo and Murillo 2004; Weitz-Shapiro 2012; Kitschelt 2000; Carlin, Singer, and Zechmeister 2015), Figure 7 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, clientelistic 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."[30] 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
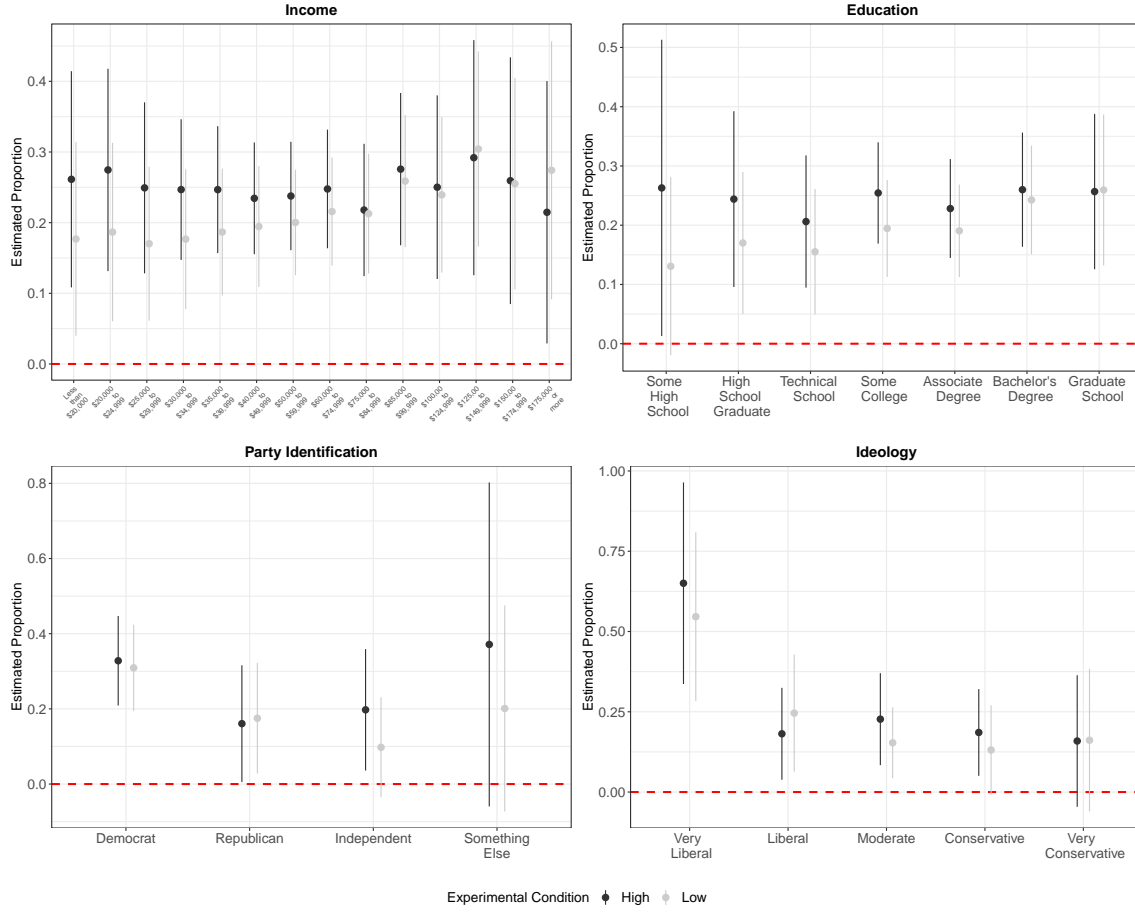
Figure 7: List Experiment: Covariates Used to Estimate Likely Vote-Sellers.
Note: *These variables were used in the multivariate statistical model to estimate individual-level probabilities of vote-selling. The figure shows the predicted probabilities and their corresponding 95% confidence intervals for income, education, party identification, and ideology. Since the vote-selling prices were set arbitrarily, the reason for two experimental conditions ("high" and "low") 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.*

Holland and Palmer-Rubin ([2015](#)) find that income had little or no effect on vote-buying.[31] 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 "high" and "low" vote-selling treatments. That is, factors that heavily determine economic status (income and education) seem to be more elastic to the vote-buying price. As [Figure 7](#) 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, but conditional on the higher price. 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 words, less-educated and low-income individuals, who are more fragile and precarious, tend to avoid risks and, hence, illegal activities. However, a discrete increase in the payoff makes the immediate monetary transfer more attractive, reducing risk aversion. On the contrary, higher-income and more-educated individuals do not seem to be affected 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 "high" (25.6801%) and "low" (25.9469%) conditions.

VI. General Discussion

There seem to be two conflicting pictures. On the one hand, and 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 (see [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 votes, but for an estimated very high price—$730. 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, positive results are produced.

The paper began by establishing the tension between supply and demand sides within a clientelistic relationship. In order to study the micro-dynamics of clientelism, more hypothetical questions should be fielded. If clientelism is conceptualized as a transaction between party machines and citizens, studying only realized transactions should produce only partial answers. Geddes (1990, 131) explains the well-known problems of studying "only cases that have achieved the outcome of interest." On the contrary, questions involving hypothetical scenarios can potentially shed light on unrealized transactions. Notably, the work of González-Ocantos, Kiewiet de Jonge, and Nickerson (2014) constitutes one of the few examples of the study of *hypothetical* behaviors in the vote-buying literature. Following their lead, the paper presented evidence of vote-selling in the United States.

While the paper is rather descriptive in nature, 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.[32] 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 what vote-selling was.

```
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 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	Low Condition		High Condition	
	Estimate	*Standard Error*	*Estimate*	*Standard Error*
(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 broken by Party Identification.*

IV. Covariate Balance

```
## Error in grid_arrange_shared_legend(cov.balance.plot.income, cov.balance.plot.education,
:   objeto 'cov.balance.plot.income' no encontrado
```

V. 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.
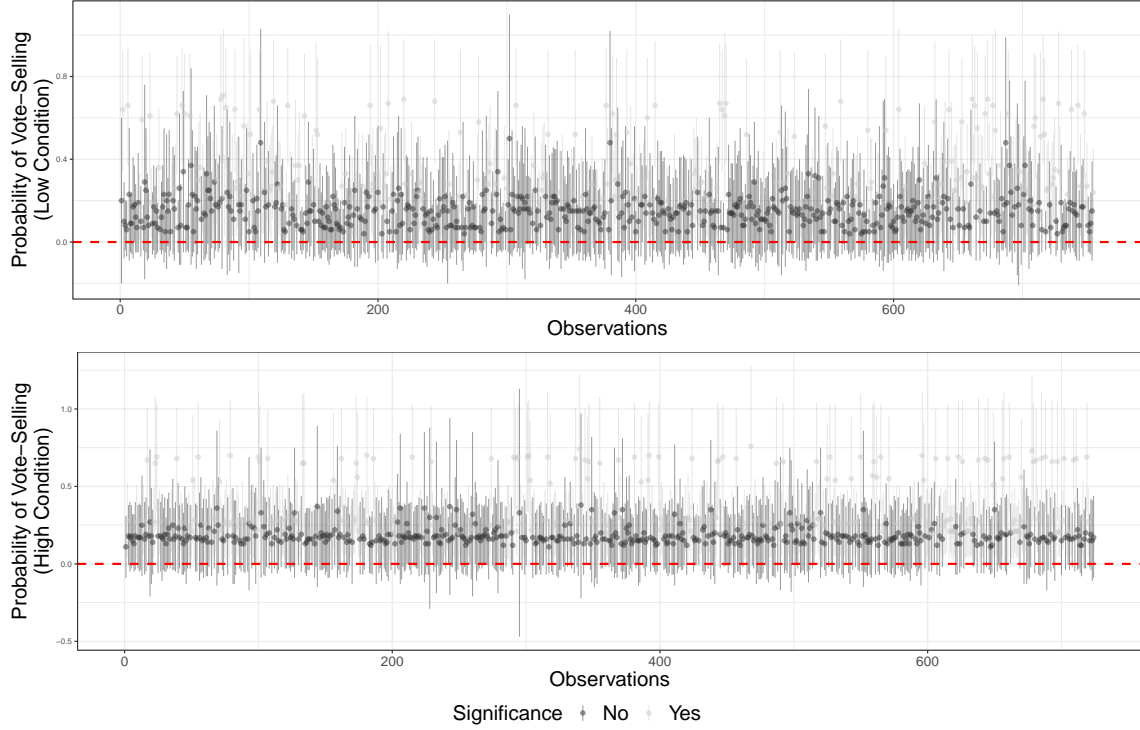
Figure A2: Individual Estimated Probabilities of Vote-Selling.
    Note: *Figure shows the individual probabilities of vote-selling (N = 1,479) under the "low" and "high" conditions. After fitting the model, and following the advice of Blair and Imai (2012) and Imai, Park, and Greene (2015), individual probabilities of vote-selling under the "low" and "high" conditions were estimated. A total of 0 estimations are significant (both conditions). 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 (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. The data were collected by *Research Now SSI* 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 to the anonymous reviewer at *Acta Politica* for this suggestion.

20. The `R` package `list` was used (Blair et al. 2016). 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 ("high" and "low"), it is then reasonable to think that there are not 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. Since there is no other way of knowing what "cheap" and "expensive" mean without mentioning directly what specific good is being considered, it was necessary to ask survey respondents directly how much they would sell their votes for. Only 189 individuals would sell their votes when asked directly.

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

29. 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).

30. My emphasis.

31. 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.

32. I owe this point to Chris Chambers-Ju.