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

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January 13, 2020

Abstract

In nineteenth-century United States politics, vote buying was commonplace. Nowadays, vote-buying seems to have declined. Yet, the 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 $730. Democrats and liberals are systematically more likely to sell while education levels and income do not seem to impact on vote-selling.

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

I. Vote-Sellers and Vote-Buyers

Prior research usually focuses on whether parties have attempted to buy votes (Vicente 2014; Vicente and Wantchekon 2009; Rueda 2017, 2015; 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, unfortunately, it 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. This reconceptualization is necessary since several questions remain unanswered—and worryingly, most pertain to vote-sellers. 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]

Moreover, whether studies have focused on vote-buying or vote-selling has in part depended on epistemological decisions rather than theoretical ones. In general, ethnographers and historians have focused on the supply side putting their emphasis on vote-selling (see for example Posada-Carbó (1996), Sabato (2001), Auyero (2000), Szwarcberg (2013), and Borges (2019)).[2] Unfortunately, except for a number of 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.

To illustrate the issue at hand, Figure 1 shows responses of U.S. citizens to whether a candidate or a member of a political party has offered something in exchange for people's votes, completely ignoring voters' preferences. The figure begs the question if survey respondents answering "never" would still be willing to sell their votes.

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

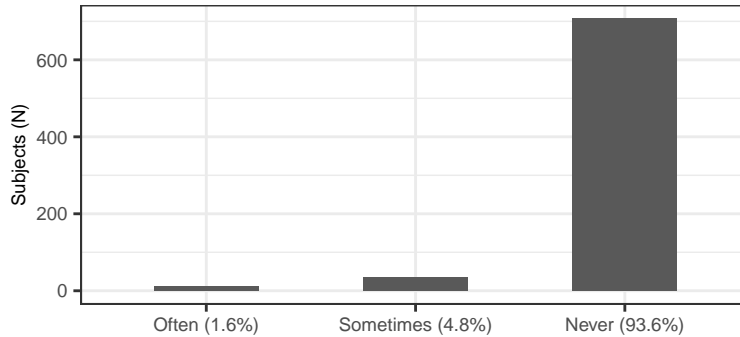2. I thank one of the reviewers for this comment.

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." There is a solid vote-buying literature, but we lack a proper vote-selling body of research. A unified framework that stresses the preferences and incentives of both sellers and buyers is missing. Thus, this paper's analytical contribution is to tackle this problem as a story of demand and supply, by explicitly considering the preferences of those who have the votes (voters), and that which demands them (parties). The current demand-side bias in the literature gives an incomplete picture because 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).

Most 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 empirical studies do not offer satisfactory answers to questions about vote-selling.

A methodological contribution of this paper is to shed some light on these issues by leveraging a list experiment in a consolidated democracy.[3] Given that the paper focuses on eliciting truthful preferences (regardless of whether the behavior is hypothetical), list experiments seem the ideal strategy to pursue.

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3. But see Levitsky and Ziblatt (2018).

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 2nd and March 6th. Thanks to this experiment, it was possible to identify the demographic factors that would make U.S. voters more likely to sell their votes, 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 optimal price of $730, and would systematically lie about it (approximately 8%). Given that these data are representative at the national level—and that 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 vote-selling.

While this paper essentially describes the phenomena, 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

Many advanced democracies were the first clientelistic political systems. For instance, Stokes et al. (2013, 200) explain that in the nineteenth-century United States, "vote buying was commonplace," and that it was "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" (in 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).[4]

Nowadays vote-buying seems to have declined considerably for two competing explanations. First, 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,

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

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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 to have received some kind of clientelistic offer from a political party, we do not know whether survey respondents would sell their votes. This paper shows systematic evidence that they would. Describing a social phenomenon—such as the existence of high levels of willingness to sell—is still a valuable exercise *per se*. However, it is more so if done in a "crucial case" design framework, specifically, a "least-likely" design. 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 (which mostly considers developing countries) describes vote-sellers as poor (Weitz-Shapiro 2014, 12), uneducated (González-Ocantos, Kiewiet de Jonge, and Nickerson 2014), and undemocratic (Carlin and Moseley 2015). Previous literature, then, informs us that the willingness to sell votes in the United States should be low, making it a hard case for vote-selling. Thus, the counterintuitive results presented in this paper make our efforts worth pursuing.

In fact, in a highly controversial pair of articles, Foa and Mounk (2016, 7) document a deep "crisis of democratic legitimacy [which] 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 a number of (unanswered) questions regarding the actual value citizens give to American electoral institutions, possibly undermining the legitimacy of the integrity of voting. Is voting unimportant enough to make U.S. citizens sell their votes, if offered the possibility?

The next section gives an historical account of vote-buying and vote-selling in the United States. The section is also an effort to situate both within a historical context. It particularly shows how vote-buying and vote-selling transitioned from important institutions in American elections, to scarcely practiced electoral methods. The following section explains the measurement, experimental strategies, and empirical findings. 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), a number of 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, it was common for office-seekers (and their supporters) to buy "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" (6).

Weak institutions, poor bureaucracies, and bad-quality record-keeping[5] helped to foster a number of electoral malpractices. 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, often times it was at 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 helped to sustain vote-selling in the United States as well. In places like Kentucky and Missouri, voters were required by law to verbally announce their choices at the polling places, instead of using party tickets (Bensel 2004, 54). The *viva voce* method, of course, was convenient for party workers who usually swarmed around the polling places. Eventually, however, this method was supplanted with the ticket system.

The "party strip" or "unofficial" ballot system permitted all sorts of fraudulent election practices, too. Party tickets were produced by the parties themselves. 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

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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).

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 it work. Political machines were not only oiled with money, however. On the one hand, many "ticket peddlers" (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" (17). On the other, political appointees, "from janitor to secretary of state," and some corporations too, donated part of their salaries on a yearly basis (Reynolds 1980, 197). Parties, then, amassed huge amounts of money.

With all these resources flooding the polls on election day, voting was truly an interesting spectacle. On election 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). American elections, even before the Gilded Age, 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 lowered the frequency of most of these malpractices significantly (Rusk 1970, 1221). However, as vote-selling and vote-buying were so embedded into what was considered normal, the immediate effect of the Australian system was to lower turnout levels (Reynolds and Mccormick 1986, 851).

The *modus operandi* of clientelism has changed today, 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, there are still some contemporary accounts of vote-buying/selling in American elections. For instance, Campbell (2005, 243-244)

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

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 (Campbell 2005, 262) or the elections at the coal-rich Appalachian mountains during the 1980s (275). Similarly, non-academic sources find that during the 2010 elections, "selling votes [was] 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, then? The next section attempts 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, there might be circumstances under which individuals might not want to answer truthfully due to social pressure. For instance, to avoid being judged by the interviewer, individuals might not want to reveal that they have done something illegal, like selling one's vote. If this systematic source of bias is not considered, it 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 elicit truthful answers (Blair 2015). Both lists look exactly the same (say, each one containing the three same items), however the treatment list includes (traditionally) a fourth item, which is 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 to study socially condemned behaviors, such as vote-buying (González-Ocantos et al. 2012; Hicken et al. 2018; Corstange 2012, 2008; Blair and Imai 2012), drug use (Druckman et al. 2015), sexual preferences (LaBrie and Earleywine 2000), attitudes towards race (Kuklinski et al. 1997; Redlawsk, Tolbert, and Franko 2010), among others.

Methodologically, given that both lists are assigned at random, the mean number of nonsensitive

activities that respondents endorsed 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.

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).[8] Blair and Imai (2012) and Imai, Park, and Greene (2015) provide a statistical framework to more efficiently analyze list data. They formalize two assumptions, namely, 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 given that the item count 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.

Using these statistical methods allows for inferring who answered "yes" to the sensitive item. Second, the statistical analyses permitted studying the relationship between preferences over the sensitive item (i.e. vote-selling) and an individual's characteristics, such as income, party identification, among others. Third, since a "direct" question over the sensitive item was included in the design, 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] Figure A1 shows the geographical distribution of survey respondents broken by party identification.

The experiment was framed as a study about crime in the United States, not a study about vote-selling.[11] While pre-testing 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 stand out among the other items, making the vote-selling item seem totally negative and undoable, and/or making it obvious the true purpose of the study.

Before splitting the subject pool into their respective experimental conditions, participants were

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8. Still difference in means are provided in the next section.

9. For an hypothetical treatment list of four items.

10. The data were collected by *Research Now SSI* between March 2*nd* and March 6*th*. Survey respondents belong to the online panel owned and administered by SSI. Notice of IRB exemption Protocol #E16-292 is kept in 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 sentence was included: "assuming you would not go to jail."

asked to read an excerpt[12] in which four illegal activities were described (including vote-selling). All were formatted as pieces of news. The idea was to explain to "newsreaders" what "vote-selling" was.
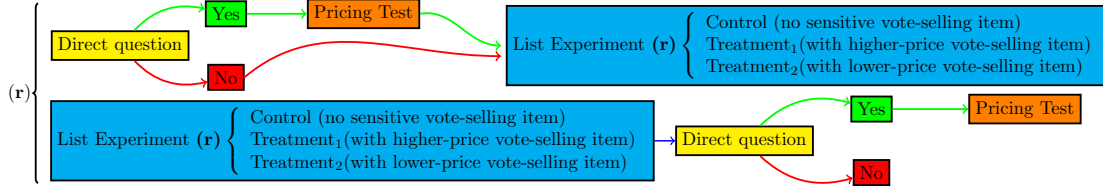

Figure 2: Experimental Flow of the List Design.
Note: *This figure shows the flow of the list experiment. Notice that (1) the order in which experimental subjects answered both the direct question and the list experiment was randomized, (2) there are two treatments, one with a selling price of $100 ("low")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

To prevent possible priming effects,[13] and as suggested in Figure 2, the order in which experimental subjects answered the direct question,[14] and the list experiment, was 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 there would be assigned, at random, the hypothetical possibility to do one of the illegal things mentioned previously in the excerpt. 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 in which they were asked to put a price on their votes. Subjects who answered "no" to the direct question, were asked a follow-up question asking them whether they were sure they did not want to sell 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 from $0 to $1,000, in $1 increments. The intersection of the "too cheap" and "too high" empirical distributions was used to estimate the optimal selling price for a vote (Figure 8).

Moving forward, the list experiment contemplated one control, and two possible treatments,

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12. See the 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.
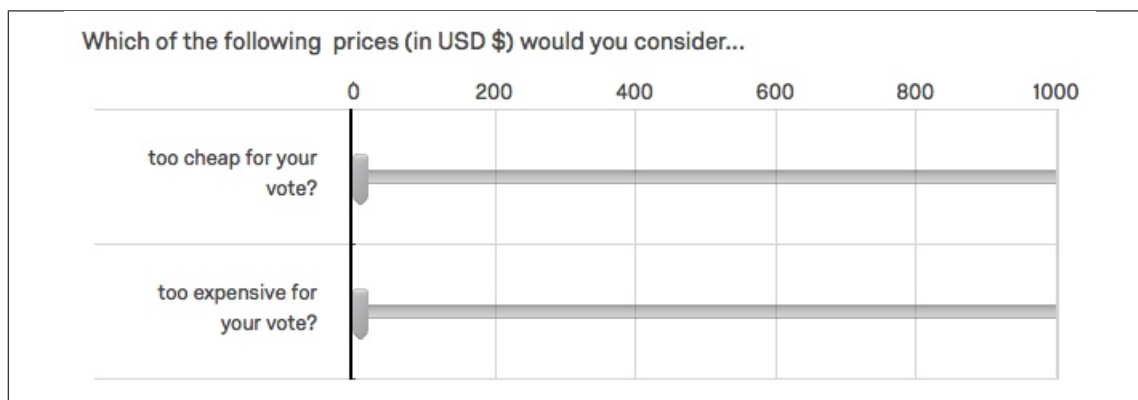14. See the appendix for wording.

Figure 3: Pricing Test.
Note: *As shown in Figure 2, 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.*

each with different vote-selling prices. Since it is difficult to price a vote, 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, only answered one of these two treatments. More than hard pricing tests, these two treatments account for possible different elasticities that might have interacted with individual socio-economic backgrounds, mainly income.[15]

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

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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 the exact same vignette,

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

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 participants 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 so as not to violate the stable unit treatment value assumption, or "SUTVA."[18]
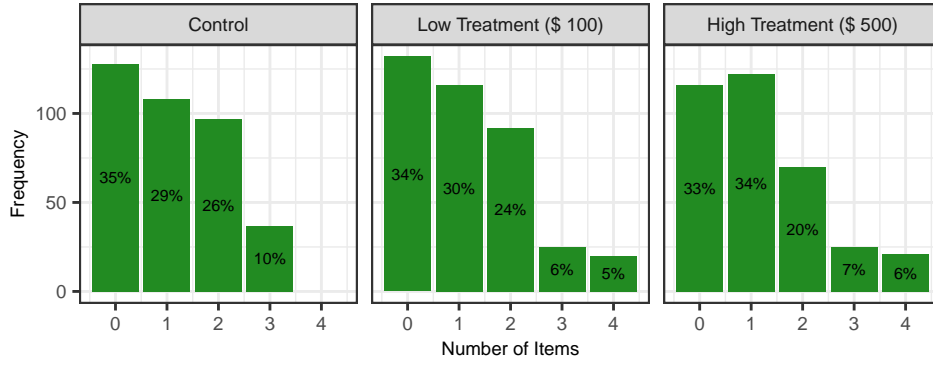

Figure 4: **Frequency and Percentages of Subjects Declaring How Many (if any) Illegal Things They Would Do**.
Note: *Notice that the X-axis denotes the number of items, not which ones. Percentages show proportions per condition.*

Besides going through the experimental portion of the study, participants also answering a small questioner. A number of questions were covered, including income, education, party identification and political ideology. These answers will be used to construct a statistical multivariate model to estimate likely vote-sellers.

As with every experimental study, it is important to show that all experimental regimes (both treatments and control) were truly randomly assigned to subjects. The idea is to show that the probability of being in any of the experimental regimes—the propensity score—is not associated

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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."

to individual covariates. Figure 5 shows the density of the propensity score for both treatments. The propensity score is computed by estimating a GLM, where the outcome variable is whether the experimental subject is treated or not—this process was performed for both treatments. The model cite is estimated as a function of observable covariates. Four variables were used (income, education, party identification and political ideology). This will be the same exact set of variables used when estimating likely vote-sellers below. In simple, the figure shows that, conditional on observables, the probabilities of being assigned to either condition are indistinguishable from each other. That is, the observable characteristics of the experimental sample are not correlated with the assign mechanism.[19]

The paper acknowledges that there is considerable friction and transaction costs in the real world that 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 coincide, that might represent a win-win situation for both, presumably fostering vote-selling. This experimental design does not consider blocking on party identification, as that might have increased the number of cells considerably.[20]

V.   STATISTICAL ANALYSES

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

Table 1 shows a simple difference in means analysis between each treated group and the control group. In average, the control group would do 1.12 of the list. Subjects treated under the "low" condition ($100) would do 1.18 of the list, while subjects in the "high" condition ($500) would do 1.19 things.

There are three important points regarding this bivariate analysis. First, the mean differences among *treated* groups ("high" and "low" treatments) is statistically zero. That implies that both treatments should not introduce design bias into the experiment. Second, while treated subjects do

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19. Additionally, Figure A2 in the Appendix section shows the frequency of every experimental regime per every level of income, education, party identification and political ideology. The figure shows frequencies on the x-axis, while the y-axis shows the levels of every variable. Since the three conditions (displayed in dots) are grouped together per every variable level, the figure strongly suggest that randomization was achieved as expected.

20. To $3 \times 2 \times 3 = 18$ cells: Republican/Democrat/Independent vote-selling treatments, High/Low vote-selling prices, Republican/Democrat/Independent party identifications. Such experiment is not only much more expensive, but statistically more complex.
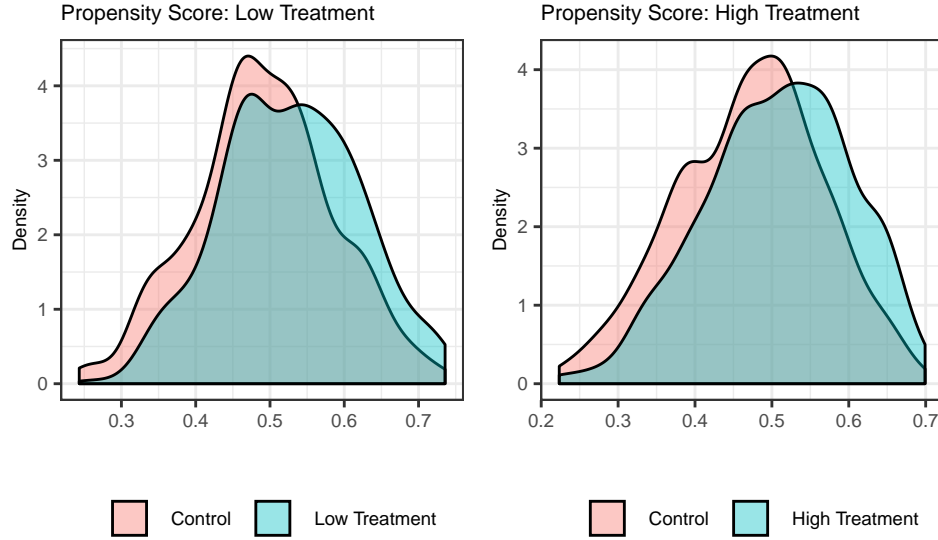
Figure 5: The Propensity Score: The Probability of being Assigned to Treatment.
Note: *The figure shows the densities of the propensity score, which is the probability of being assigned to treatment. The propensity score is computed by estimating a GLM, where the outcome variable is whether the experimental subject is treated or not. The model is estimated as a function of observable covariates. Four variables were used (income, education, party identification and political ideology). This is the same exact set of variables used when estimating likely vote-sellers when employing the multivariate approach below. In simple, the figure shows that, conditional on observables, the probabilities of being assigned to either condition are indistinguishable from each other. That is, the observable characteristics of the experimental sample are not correlated with the mechanism assignment.*

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 (and 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 below to what it is found in this experimental design.

However, these kinds of 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), the list data were analyzed using a statistical multivariate approach.[21] Exploiting the "high" and "low" treatments, two identical statistical models were estimated. In both models,

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21. 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.

Condition	Mean	Difference with Control Condition	t	df	p-value
High ($500)	1.189	1.189 - 1.116 = 0.073	0.913	700	0.36
Low ($100)	1.182	1.182 - 1.116 = 0.066	0.846	748	0.4

Table 1: Differences in Means between Treatments (high and low) and the Control Group. **Note**: *Table shows two-tailed t-tests between each experimental treated unit ("high" and "low" conditions) and the control group. Stars denote statistical significance.*

the outcome variable is the item count of things subjects would do (as previously described in the experimental design section). The estimation method was performed via maximum likelihood. The idea behind these kinds of multivariate analyses is to estimate what we cannot observe (vote-selling), using information we do observe (socio-economic and political variables captured in the experimental questioner). 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.

By leveraging this multivariate approach it is possible to estimate the proportion of hypothetical vote-sellers. For both the "low" and "high" treatments, Figure 6 shows the proportion of declared vote-sellers ("Direct Question") and predicted vote-sellers ("List Experiment"), and its difference ("Social Desirability"). 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 under-reported their true answers: approximately 8% of the nationally representative sample would have lied about it.[24]

The *estimated* proportion of vote-sellers (shown in "List Experiment" in Figure 6) is calculated using information from subjects with fully-observables preferences—subjects with an item count of 0 or 4.[25] 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. Using the mentioned covariates (income, education, party

22. This number was calculated averaging over the "high" (27%) and "low" (23%) conditions.
23. This number was calculated averaging over the "high" (19%) and "low" (17%) conditions.
24. This number was calculated averaging over the "high" (8%) and "low" (7%) conditions. The "low" condition is barely non-significant, and hence it does not alter the substantive results.
25. Importantly, the figure also shows confidence intervals, which represent the amount of uncertainty of our estimated quantities of interest (i.e., likely sellers and liars). When 95% of the predicted distribution do not touch the zero line, we may expect to replicate these results 95% of the times.
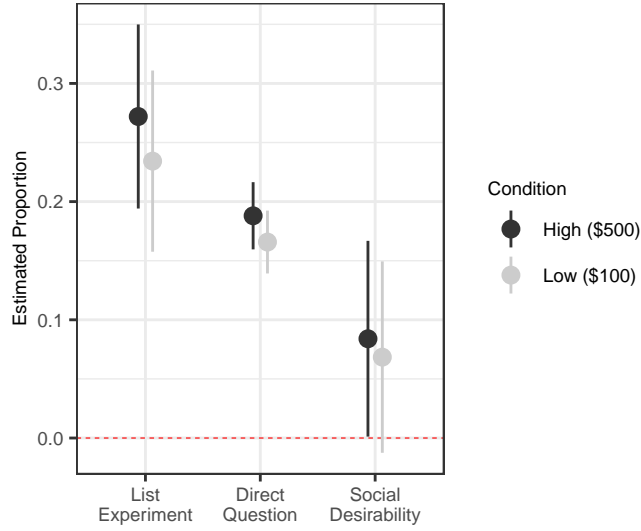
Figure 6: List Experiment Data: Declared and Predicted Vote-Sellers.

> **Note**: *The figure shows the proportion of declared ('Direct Question') and predicted ('List Experiment') hypothetical vote-sellers, and its difference ('Social Desirability'). Combining both 'high' and 'low' treatments, 25% would be willing to sell their votes. And of the ones who answered affirmatively when asked directly (18%) there is an estimated additional 8% who lied about it. 'Liars' answer negatively the direct question, 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. As the figure suggests, there are some small differences. However, they are not statistically significant. Consequently, these arbitrary pricing decisions do not threaten the experimental design.*

identification, and political ideology), we can fit a model to predict all subjects with all these 0's and 4's on the left-hand-side. Then, using this information, we can estimate likely-sellers, i.e. subject who would do 1, 2 or 3 things of the list. Then, the individual vote-selling predictions—shown in Figure 7—are compared with the direct question which is answered by all experimental subjects. If a subject is predicted to be a vote-seller, but answers negatively the direct question, we infer that due to concerns of social desirability s/he might have chosen to lie. Finally, once all these subject-specific probabilities are computed, grand proportions are calculated and displayed in Figure 6.

For reasons of internal validity, it is important to note in Figure 6 that 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. Otherwise,

the estimated treatment effects would have been a function of the design itself.

The vertical axis of Figure 7 shows the estimated probabilities of the entire experimental sample which is 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.
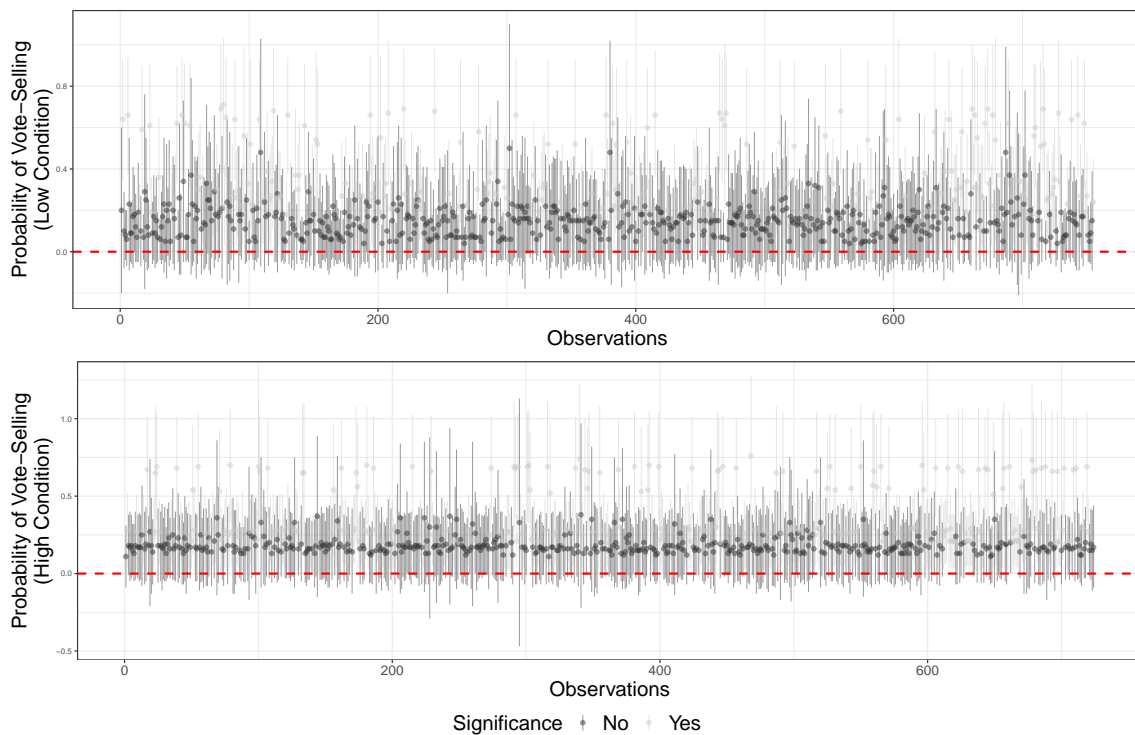

Figure 7: Individual Estimated Probabilities of Vote-Selling.
Note: *Figure shows the individual probabilities of vote-selling (N = 1,479) under the "low" and "high" conditions. After fitting the model, and following the advice of Blair and Imai (2012) and Imai, Park, and Greene (2015), individual probabilities of vote-selling under the "low" and "high" conditions were estimated. A total of 501 estimations are significant (both conditions). The figure also shows 95% confidence intervals.*

II. What is the Price U.S. Citizens would Sell their Votes? Where Supply and Demand Meet.

What would be the tipping point for vote-sellers? As shown in Figure 2, a pricing test was conducted in addition to the list experiment. Subjects were directed to declare which price was considered "too

cheap" and which was "too expensive."[26] With these two pieces of information, it was possible to construct two supply curves. The "too cheap" curve, representing the lower bound (mean = $418) and the "too expensive" curve, representing the upper bound (mean = $744). Substantively, the optimal selling price is located where both curves intersect. Figure 8 indicates that the average survey respondent would sell his/her vote for $730.
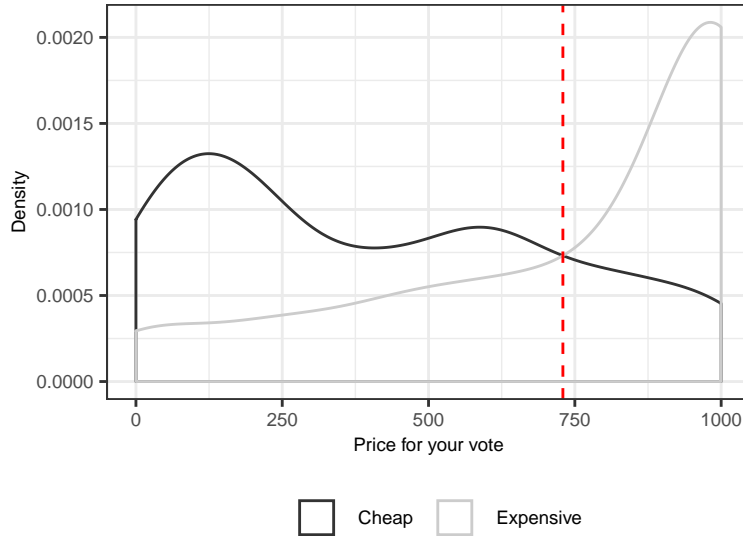

Figure 8: Pricing Test: Ideal Selling Price.
Note: *Subjects who answered "yes" to the direct question (N = 189) were asked to price their votes via a pricing test (see Figure 3). This figure shows the empirical distributions of the "too cheap" and "too expensive" answers. The intersection of these two 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.*

These results are not unrealistic. While the selling price is really high, it matches with 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 shifts the broker's vote-buying capacities upwards."[27] In turn, Shawn (2012, 6) noted that a broker purchased one man's vote for $800 during the 2010 elections in eastern Kentucky. Thus, higher incomes make vote-buying more expensive

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26. 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.

27. Similarly, see Schaffer (2004) for the Philippine, and Abramo and Speck (2001, 14) for the Brazilian cases.

(and more rare).

However, moving beyond anecdotal evidence, these results align with Stokes et al. (2013). From the demand-side, vote-buying is no longer an efficient strategy for party machines. Industrialization has driven up the median income of the electorate, increasing the selling price, turning vote-buying into an increasingly expensive strategy to win votes. Evidently, with the selling price so expensive, the demand-side (i.e. parties) are unable to catch up with the supply-side, making vote buying in the United States a rare event. This situation has forced party machines to turn to other, less prohibitively costly alternatives. Thus, these results confirm that from the supply-side (i.e. voters) the vote is still for sale, only for a very high price—a price that party machines cannot really afford.

Since the pricing test was constructed based on the direct question, a word of caution is needed. The list experiment does suggest that respondents lied when directly asked if they would sell their votes. Consequently, we should expect the optimal vote-selling price to be biased in some degree. Not only a small proportion of likely vote-sellers answered the direct question affirmatively, but also, we do not know how many of the ones who answered the direct question negatively (that they would not sell) would in fact sell their votes. However, more than being definitive and final pricing tests, these findings do seem to suggest that the vote-selling price seems to be high enough to deter political parties of engaging more frequently in vote-selling.

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

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 liberals, when compared with conservatives, 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."

Education and income levels do not seem to have a systematic impact on vote-selling. Interestingly, poverty has long been associated with vote-selling (Calvo and Murillo 2004; Weitz-Shapiro 2012; Kitschelt 2000; Carlin, Singer, and Zechmeister 2015). Brusco, Nazareno, and Stokes (2004), Stokes et al. (2013) and Nazareno, Brusco, and Stokes (2008) explain that since the poor derive more utility
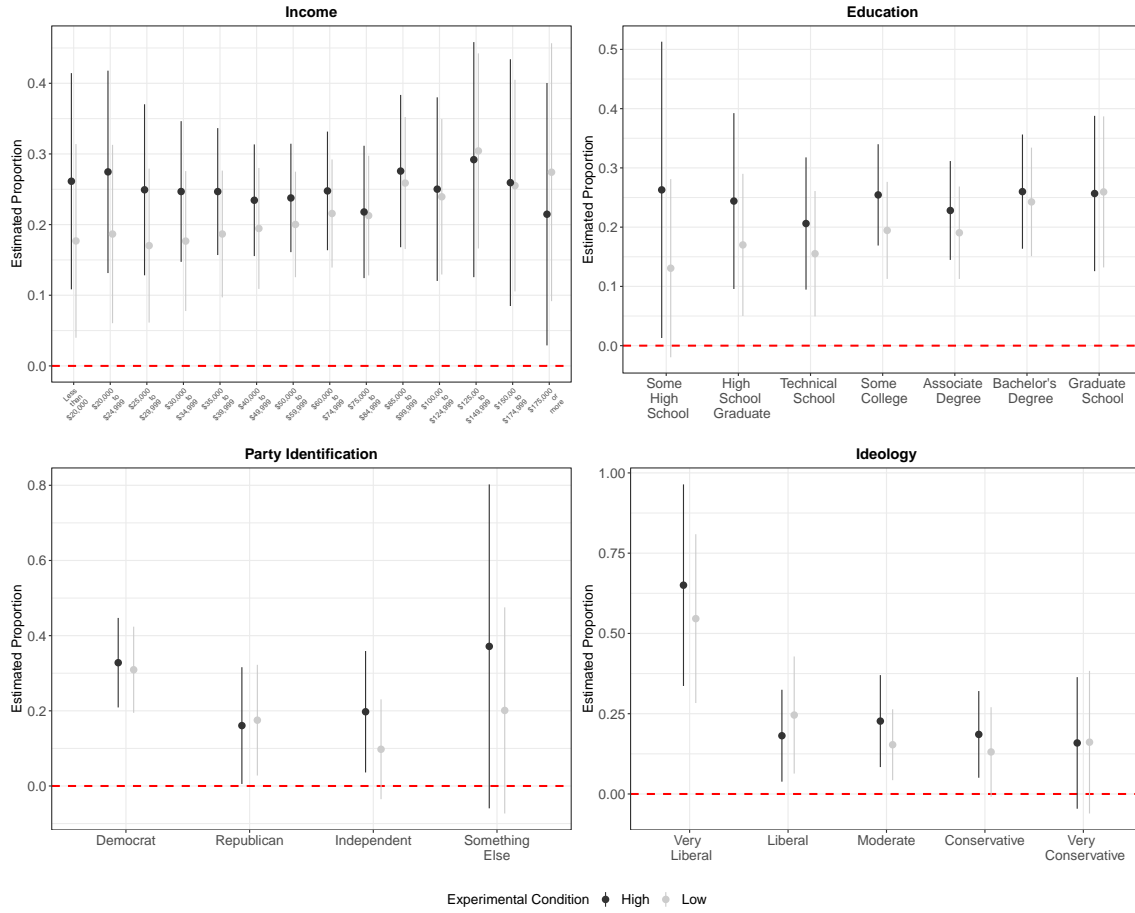
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Figure 9: List Experiment: Predicting Vote-Selling. *Note: After fitting the model on the list experiment data (see **??**), this figure shows the predicted probabilities and their corresponding 95% confidence intervals for: income, education, party 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.*

from immediate transfers relative to returns associated with future (and risky) 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." However, this canonical predictor has recently been questioned. 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. In fact, Bahamonde (2018) advances an argument for why brokers would also target non-poor individuals.

While the differences between the two treatments are not statistically significant—which in fact confirms that the arbitrarily low and high treatments were well-chosen—there seems to be a substantive pattern regarding these two treatments. Factors that heavily determine economic status (income and education), seem to be more elastic to the buying price of the vote: even when poor individuals do not seem to sell more (when compared with wealthier individuals), there does seem to be important within-group differences between the treatments. Particularly, low-income and less educated individuals are willing to sell their vote (just like the rest), but more so under the high-price condition. This might indicate that, for them, it is worthwhile to behave illegally, but only when the payoff is "large enough." These results are in line with experimental and applied economists who argue that "risk aversion decreases as one raises 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, lessening risk aversion.

VI. General Discussion

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

While vote-buying/selling in the United States was commonplace during the 19th 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, find positive results.

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." Questions involving hypothetical scenarios, on the contrary, can potentially shed light on unrealized transactions. Notably, González-Ocantos, Kiewiet de Jonge, and Nickerson (2014) constitutes one of the few examples in 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 rather worth pursuing. The experimental evidence of a large critical mass willing to sell their votes in a developed country was novel. Hopefully, the paper sets the stage for future research and encourages other scholars to field the experimental designs presented in this paper in a comparative setting, such that both developed and developing countries are included. Future research should also consider different values placed on different offices.[28] It is reasonable to think that presidential, Senate, House, state legislature, mayor, and city council elections, follow different incentives and constraints to buy and sell votes. And, future research should consider blocking on party identification. For example, designing a more complex experiment in which not only the price varies (like it does in the presented design), but also the vote-selling treatment is partisan.

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

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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.*

	Low Condition		High Condition	
Respondent Types	*Estimate*	*Standard Error*	*Estimate*	*Standard Error*
(y = 0, t = 1)	0	0.03	0.02	0.04
(y = 1, t = 1)	-0.01	0.03	-0.03	0.04
(y = 2, t = 1)	0.02	0.02	0.03	0.02
(y = 3, t = 1)	0.05	0.01	0.06	0.01
(y = 0, t = 0)	0.34	0.02	0.33	0.02
(y = 1, t = 0)	0.3	0.03	0.33	0.04
(y = 2, t = 0)	0.25	0.03	0.23	0.03
(y = 3, t = 0)	0.05	0.02	0.04	0.02

Note: Since the Bonferroni-corrected p-values of the *low* (0.86) and *high* (0.33) conditions are above the specified alpha (0.05), I fail to reject the null of no design effects.
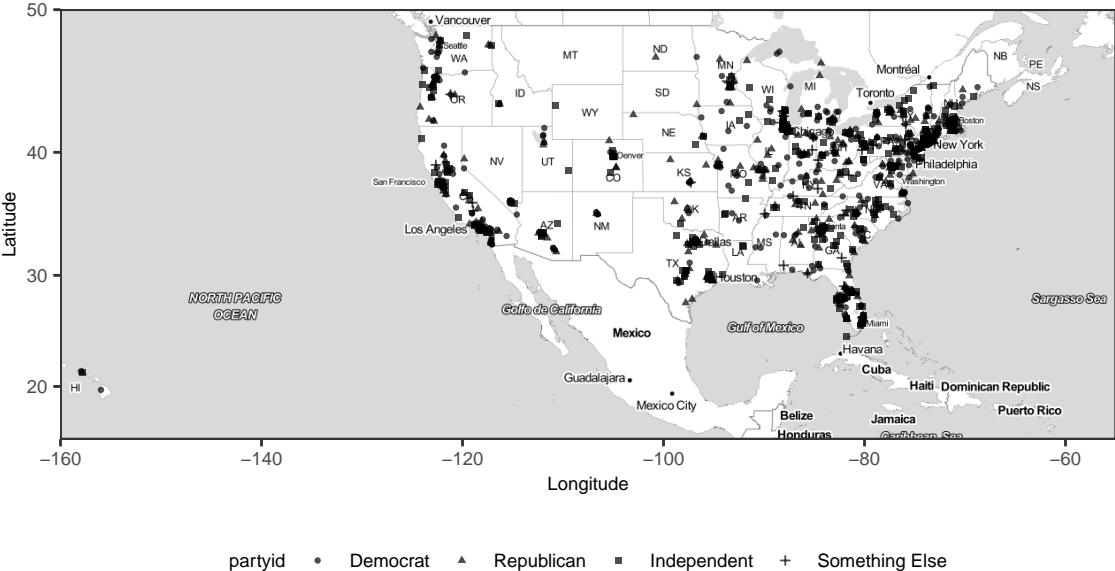
III. Geographical Distribution of Survey Respondents

Figure A1: *Geographical Distribution of Survey Respondents broken by Party Identification.*
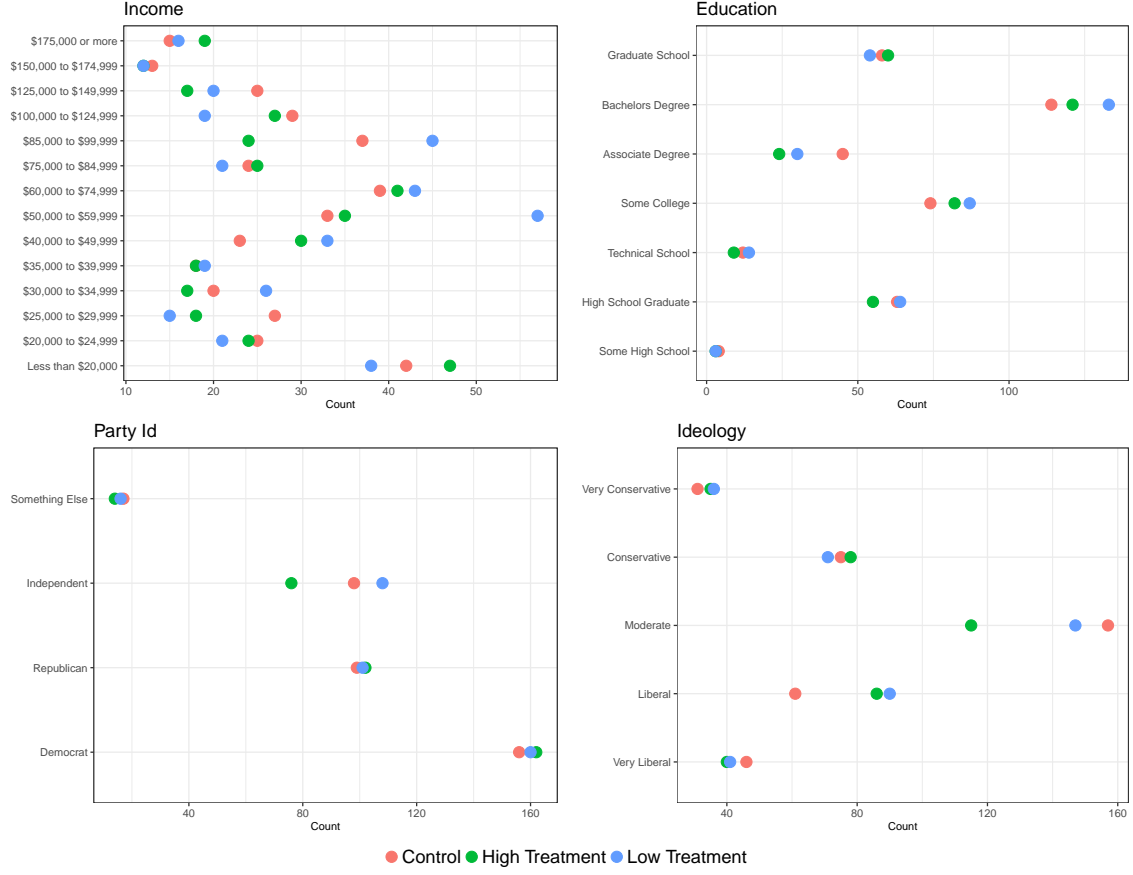
IV. Covariate Balance

Figure A2: Empirical Covariate Balance of the Variables Used to Estimate Likely Vote-Sellers.
Note: *The figure shows the frequency of every experimental regime per every level of income, education, party identification and political ideology. The figure shows frequencies on the x-axis, while the y-axis shows the levels of every variable. Since the three conditions (displayed in dots) are in general grouped together per every variable level, the figure strongly suggest that randomization was achieved as expected.*