Fraudulent Votes, Voter Identification, and the 2012 US General Election
evidence from a survey list experiment

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Abstract

State legislatures around the United States have entertained–and passed–laws requiring voters to present various forms of state-issued identification in order to cast ballots. Proponents of such laws argue that they protect the integrity of the electoral process, sometimes claiming that fraudulent voting is widespread. Opponents argue that widespread or systematic electoral fraud is virtually non-existent; whatever does exist is minor, insufficient to swing an election, and not worth imposing additional burdens on voters to address. They claim that voter ID laws are little more than thinly veiled attempts to disenfranchise the poor, elderly, and racial and ethnic minorities. Surprisingly no one has undertaken to actually measure the incidence of voter fraud in the United States using rigorous survey methodologies. This paper reports the results of a YouGov/Polimetrix survey list experiment fielded immediately after the 2012 US general election designed to measure the prevalence of two specific types of voter fraud: repeat/fraudulent ballot casting and vote buying. We find no evidence of fraudulent vote casting or vote buying, even in the states most contested in the Presidential campaign. We also find that states with strict voter ID laws are no different from others in the (non) existence of fraudulent voting. Based on this evidence, strict voter ID requirements address a problem that did not exist in the 2012 US election.

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> "What's your response to the proposition advanced by the proponents of photo ID that the reason there have not been discovered instances of and prosecution of voter impersonation at the polls is because it's a difficult or nearly impossible crime to detect?" –Wisconsin Executive Assistant Attorney General Steven P. Means[1]

1 Voter fraud in US elections

How much voter fraud exists? The extent of fraud is both difficult to measure and central to debates about the need for voter identification laws.[2] If vote fraud is common, it can undermine confidence in the electoral process; at worst, if fraud can alter outcomes, it calls into question the foundations of democratic governance altogether. If it is rare, requiring voters to show specific forms of identification can disenfranchise voters who may not have easy access to a qualifying form of ID. Stories of electoral corruption remain a centerpiece of American political lore, with visions of Tammany ward heelers herding voters through the polls multiple times, Landslide Lyndon winning his 1948 Senate runoff with the help of ballot box stuffing by friendly election officials, or Daley operatives allegedly dumping thousands of Nixon ballots into the Chicago River to deliver Illinois to Kennedy in 1960.[3]

Voter ID prevents a very specific form of voter fraud: casting a ballot in another persons name, either a different validly registered voter or a fictional and fraudulently registered name. Both involve an individual casting an invalid vote by pretending to be someone else, and would be prevented by requiring voters to provide proof of identity. Other forms of voter

[1] *Milwaukee Branch of the NAACP et al. versus Scott Walker et al.* (2012)

[2] Here, we use voter ID to mean a requirement that voters show a government-issued identification (usually a drivers license or photo ID issued by a DMV) when presenting at a polling place.

[3] The first and second examples are true. Congress investigated the extent of Tammany Halls corruption of the electoral process after the Civil War (US Congress, 1869) and there is compelling evidence that Johnsons 1948 win was the result of fraud. In 1960, there are indications of dishonest vote tabulation in Chicago, though not of a scale that changed the outcome (Kallina, 1985). Even so, much of the corruption lore is likely incorrect, despite confirmed cases of fraud. In a time when fraud was said to be rampant, "claims of widespread corruption were grounded almost entirely in sweeping, highly emotional allegations backed by anecdotes and little systematic investigation or evidence . . . what is most striking is not how many but how few documented cases of electoral fraud can be found" (Keyssar, 2000:159).

fraud are not affected by voter ID requirements: double voting (casting a ballot in multiple jurisdictions by someone otherwise eligible to vote, or voting both by absentee ballot and on election day), absentee ballot fraud, voting by felons, or fraud committed by election officials or with the cooperation of poll workers.

As important as voter fraud is to this issue, there is strong disagreement about how often it actually occurs. Voter ID proponents insist that fraud is widespread, in large part because it is easy to commit and extremely difficult to detect. In a close election even a handful of fraudulent votes could change the result, an abhorrent possibility that warrants strong security measures as a preventive. Critics of ID counter that there is little evidence that vote fraud occurs with any frequency, and that there are many mechanisms in place that both deter and detect it. Minnite (2010:ch. 6) and Hasen (2012:ch. 3) go further, arguing that voter ID advocates have vastly exaggerated the scope of fraud in an effort to politicize the issue and justify restrictive policies that disenfranchise many people (who, coincidentally or otherwise, are more likely than not to be Democrats).

Voter ID advocates almost invariably adopt three strategies when arguing about vote fraud, both of which overstate the actual occurrence of the specific type of voter fraud voter impersonation that an ID requirement would prevent. The first is to conflate a number of illegal election practices, including many that have nothing to do with voter fraud that would be prevented by ID requirements, and insist that all prove the need for strict identification requirements. A variation on this theme is counting the inevitable human errors that are common in election administration recording incorrect names, marking down the wrong person as voting, data entry errors as evidence of widespread voter impersonation. A second is to assert that any examples of fraud are only the tip of the iceberg that proves electoral corruption is widespread; the claim is that fraud is so easy to commit and so difficult to detect that authorities can only catch a fraction of the offenders. A third is to rely on unproven claims easy to make, far more difficult to authenticate as definitive proof of

endemic fraud. A few examples show these rhetorical strategies.

Von Spakovsky (2012:2), a vocal advocate of voter ID, cites a 1984 New York City grand jury report as evidence of "extensive voter registration and voter impersonation fraud in primary elections in Brooklyn between 1968 and 1982 that affected races for the U.S. Congress and the New York State Senate and Assembly." The report cites egregious instances of party and election officials filing fraudulent registration forms, voting in the name of fictitiously registered people (as well as the dead), and multiple voting. Von Spakovsky argues that even though no one was prosecuted in this scandal, "it demonstrates that voter impersonation is a real problem and one that is nearly impossible for election officials to detect given the weak tools usually at their disposal" 2012:7. Yet after reviewing the grand jury report Hasen (2012:63) found that "[m]ost of the fraud had nothing to do with voter impersonation, and that which did involved the collusion of election workers something a voter identification law could not stop."

After the 2010 election in South Carolina, the state Attorney General reported that 207 dead people had voted, a claim that if true would constitute a classic case of voter impersonation. But further investigation showed that of the 207, nearly all were the result of clerical errors by poll workers, erroneous matching against death records, or a voter dying after returning an absentee ballot. Once these errors were corrected, only ten cases remained, and there "was insufficient information in the record to make a determination" about whether any crime had occurred (Minnite, 2013:100). She concludes, "in 95 percent of all cases of so-called cemetery voting alleged in the 2010 midterm election in South Carolina, human error accounts for nearly all of what the states highest law enforcement official had informed the U.S. Department of Justice was fraud."

Even government commissions and agencies can rely on flawed logic. The Carter-Baker Commission on Electoral Reform claimed that "both [multiple voting] and[fraud] occur, and it could still affect the outcome of a close election" (National Commission on Federal

Election Reform, 2005:18). As evidence, it cited a Milwaukee Police Department report of multiple voting and excess ballots in the 2004 presidential election as "clear evidence of fraud" (National Commission on Federal Election Reform, 2005:4). Subsequent investigations of the allegations in that report found that the instances of double voting involved people with similar names, or parents and children with the same names (the "birthday problem," see McDonald and Levitt (2008)), and the excess ballot numbers and suspect registrations were due to inadequate administrative practices and human error rather than fraud (Minnite, 2010:106). No one was arrested or indicted as a result of the investigation.

Virtually every scholar who has studied voter impersonation fraud has concluded that it is vanishingly rare, and certainly nowhere near the numbers necessary to have an effect on any election (Bailey, 2008; Hasen, 2012; Hood and Gillespie, 2012; Minnite, 2010, 2013). To give one idea of the scale: a review of allegations in the 2008 and 2010 elections in Texas found only four complaints of voter impersonation, out of more than 13 million votes cast, and it is not clear whether any of the complaints actually led to a prosecution (Minnite, 2013:101).[4]

One reason voter impersonation fraud does not occur more often is that it is a crime that makes little sense, because the benefits are nearly nonexistent while the costs can be enormous. In Wisconsin, voter fraud is a felony that can result in a $10,000 fine and $3\frac{1}{2}$ years in prison. While the costs are high, the potential benefit is miniscule. The likelihood that a handful of fraudulent votes would change a result is nearly zero, and any organized effort to cast a significant number would increase the risk of detection and almost certainly require the cooperation of election officials. The penalties for committing voter impersonation fraud are so vastly disproportionate to any plausible benefit that it is a wholly irrational act.

Proponents of voter ID are unconvinced by this, and often see the lack of evidence of voter

[4]By contrast, the 2000 presidential election almost certainly was altered by poor ballot design in Palm Beach County, which resulted in at least 2,000 voters who intended to vote for Al Gore and Joe Lieberman casting their ballots for Pat Buchanan by mistake (Wand et al., 2001).

fraud as proof that the crime is nearly impossible to detect because it is easy to commit and leaves no evidence behind. The small number of investigations and convictions says nothing, in this view, about the true rate of voter fraud.

Measuring the extent of voter impersonation is extremely difficult. It is not something that we can directly observe, apart from criminal investigations and prosecutions. Voters would be unlikely to advertise the fact that they voted illegally,[5] and those who commit fraud have every incentive to conceal it.

Common methods for determining the prevalence of election irregularities rely on reported incidents, prosecutions, and convictions (Alvarez and Boehkme, 2008; Bailey, 2008; Kiewiet et al., 2008; Minnite, 2010); survey data (Alvarez and Hall, 2008); and election forensics using statistical tools to look for anomalous patterns (Alvarez and Katz, 2008; Hood and Gillespie, 2012; Mebane, 2008). These analyses typically show few indications of fraud, but focus on the full range of possible types–including official manipulation of results, corrupt voting machines software, and human error–that are not affected by voter ID.

Here we apply a more direct method of estimating how many people commit voter impersonation. It improves over existing survey research, which focuses on public confidence in the electoral process and expressed concerns about fraud (Alvarez and Hall, 2008), or the relationship between turnout and confidence (Ansolabehere and Persily, 2008).

2 Methods

Measuring the prevalence of sensitive or illegal behaviors using surveys is clearly challenging, as respondents will often give inaccurate answers when asked direct questions. Yet recent years have seen resurgence in the use of a powerful tool for just this purpose: the list

[5]Except when they do. A Marquette University student bragged on an ABC national news broadcast that he had voted four times in the 2000 presidential election. He later admitted that he lied in order to show how, and it turned out that he had not voted at all (Minnite, 2007:13). He was, however, charged with five counts of selling fake IDs.

experiment. List experiments have been used to great effect in the study of a variety of sensitive topics, including voter fraud and election irregularities in Nicaragua and Russia (Frye, Reuter and Szakonyi, 2012; Gilens, Sniderman and Kuklinski, 1998; Gingerich, 2010; Glynn, 2013; Gonzalez-Ocantos et al., 2012; Holbrook and Krosnick, 2010).

Survey list experiments provide a way of eliciting information about sensitive, illegal, or socially undesirable behaviors and opinions. Respondents are presented with a list of items and are asked how many (as opposed to which) of these items pertain to the respondent. To measure the prevalence of the behavior of interest, respondents are randomly split in to two groups, with the treatment group seeing a list including one additional item that describes the sensitive quality of interest. The difference in the mean number of items reported between treatment and control groups is then an estimate of the prevalence of the item in question in the larger population. List experiments generally elicit more truthful answers around sensitive topics since there is no way to infer whether any individual respondent has the quality of interest unless they intentionally choose the most extreme category. Recent methodological advances have expanded the set of analysis tools available for list experiments beyond simple difference-in-means calculations (Blair and Imai, 2012; Glynn, 2013; Imai, 2011).

2.1 the list experiment

We conducted our list experiments using a YouGov internet survey of 1000 US citizens aged 18 and over. The survey was in the field December 15-17, 2012. Respondents were selected from YouGov's opt-in Internet panel using sample matching. A random sample (stratified by age, gender, race, education, and region) was selected from the 2010 American Community Study. Voter registration was imputed from the November 2010 Current Population Survey Registration and Voting Supplement. Religion, political interest, minor party identification, and non-placement on an ideology scale, were imputed from the 2008 Pew Religion in Amer-

ican Life Survey. The sample was weighted using propensity scores based on age, gender, race, education, news interest, voter registration, and non-placement on an ideology scale. The weights range from 0.2 to 8, with a mean of one and a standard deviation of 1.21.

We conducted two list experiments concurrently. While treatment status was randomly assigned during administration of the Web questionnaire, we follow the advice of Glynn (2013), and let the treatment group for one question serve as the control for the other and vice versa. The first experiment is designed to address the question of fraudulent vote casting. Election fraud can occur in a variety of ways but, to our thinking, the only form of fraud that voter ID laws can possibly address is repeated voting by a single individual and/or casting a ballot under a false name. To that end we constructed the list experiment described in Table 1. Control items 1-3 are innocuous ways individuals may participate in the electoral process.[6] Control item (4) was included specifically to reduce the risk of any possible "ceiling effects" in the survey. Nevertheless we still observed thirteen respondents in the control group (2.5%) and twelve respondents in the treatment group (2.5%) claiming to have participated in the maximum four and five activities, respectively. We take up this issue below after presenting our core findings. Our list experiment is designed to capture the prevalence of voter impersonation at the polls, *not* the number of fraudulent votes cast.

The second list experiment addresses the possibility of "vote buying," i.e., offering voters specific and personal goods or services in exchange for their support. Voter ID laws are unlikely to address vote buying (if it exists), but concern with vote buying does resonate with some claims made in the aftermath of the election, including comments by Mitt Romney and Paul Ryan.[7] More importantly, our vote buying list experiment closely mimics that described in Gonzalez-Ocantos et al. (2012). Gonzalez-Ocantos et al. (2012) demonstrate

[6]While we refer to items here by the numbers in tables 1 and 2 item positions were randomized in the actual survey administration.

[7]Romney famously attributed Obama's victory to "big gifts" given to particular constituencies (though he later spoke of these gifts in terms of policy). Ryan attributed the victory to "surprising" turnout, "especially in urban areas."

Table 1: Repeat/fraudulent voting list experiment

Prompt:	"Here are some things that you might have done during the election this past November. HOW MANY of these activities you were involved in around this election?"
1	"I attended a rally sponsored by a political party or candidate"
2	"I put up a sign, poster, or sticker on my personal property"
3	"I saw or read something about the election in the news"
4	"I got into a physical fight about the election"
Treatment	"I cast a ballot under a name that was not my own"

that their instrument successfully recovers significant evidence of vote buying where it is expected to have occurred. Using a similar sample size to our own (their $N = 995$), their list experiment finds an estimated 24% of voters in the 2008 Nicaraguan municipal elections were offered gifts in exchange for their vote.[8] Any non finding associated with this question here is therefore better viewed as evidence of the rarity of this behavior in the population rather than simply evidence of a poor survey experimental instrument.

Table 2 presents the details of the vote buying list experiment. Again, control item four is included to reduce problems with ceiling effects. Even with this item included we observe seven respondents (1.4%) in the treatment group and 18 respondents (3.7%) in the control group answering with the maximum of five and four items, respectively.[9]

Using list experiments to infer the prevalence of a sensitive behavior in a population requires that we assume the inclusion of the sensitive item in the treatment condition has no effect on respondents' evaluation of the control items. Blair and Imai (2012) refer to this as a survey "design effect" and propose an hypothesis test to examine whether there is evidence against the null of no effect. Applying the test to our data we calculate $p-$values 0.27 for the voter fraud experiment and 0.55 for the vote buying experiment, failing to reject the null.

[8]Only 2% admitted to this when asked directly.

[9]Gonzalez-Ocantos et al. (2012) find that 26 respondents in the treatment group admitted to all five items, a much larger ceiling effect than we observe in our treatment group.

Table 2: Vote buying list experiment

Prompt:	"Here are some things that political candidates or activists may have done in your area during the election this past November. HOW MANY of these activities did you experience?"
1	"They put up campaign posters or signs in your neighborhood or city"
2	"They visited your home"
3	"They placed campaign advertisements on television or radio"
4	"They threatened you with some harm if you didnt vote for them"
Treatment	"They gave you a gift or did you a favor"

3 Findings

3.1 Headline results

Before turning to a multivariate regression analysis we present our basic set of results using simple difference-in-means tests and visual displays of the response distributions.

Figure 1 displays the unweighted item count frequencies for the treatment groups (those seeing lists with five items) and control groups (those seeing lists with four items) for both the voter fraud and vote buying questions. Visual inspection of the distributions does not show any obvious reason to conclude that the treatment and control groups differ in any systematic way. The treatment groups are certainly not shifted into the higher item counts as they would be if vote buying or voter fraud were prevalent.

Figure 2 presents our headline results. This figure displays the difference between the treatment and control groups in the mean number of items reported, along with the associated 95% confidence intervals. We report results both with and without survey weights. Regardless of whether we weight responses there is no evidence consistent with the systematic prevalence of either fraudulent voting or vote buying. In fact the difference in the mean number of items for the vote fraud treatment using unweighted data is slightly *negative*, similarly for the weighted difference in means for the vote buying question. The notion that

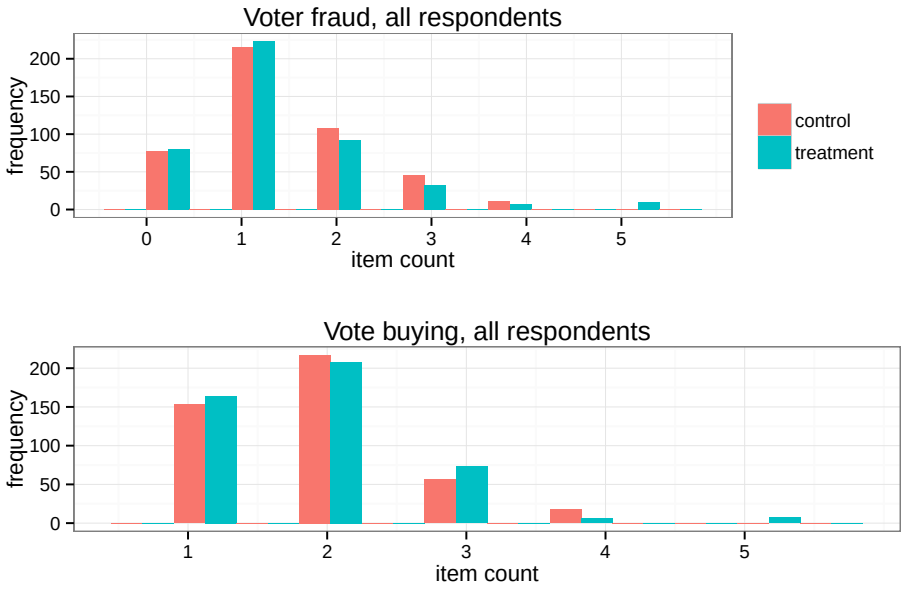

Figure 1: Frequency distribution of number of chosen items to the list experiment questions by treatment status (unweighted data).

voter fraud is a widespread behavior is totally contradicted by these data.

3.2 Multivariate analysis

3.2.1 Response distributions broken out by relevant subgroups

The analysis in the previous section averaged over a national sample. It seems reasonable to imagine that fraudulent voting or vote buying might be more prevalent in some places than others. Public voices in the voter ID debates have made claims that fraudulent voting is more prevalent among some populations. We note that even if this were the case we should still see a difference in means in the national sample insofar as fraudulent voter behavior is essentially absent in the rest of the country and the prevalence of the control items is uncorrelated with any incidence of voter fraud. Nevertheless we investigate this further.

We first look at simple frequency distributions across several partitions of the data that

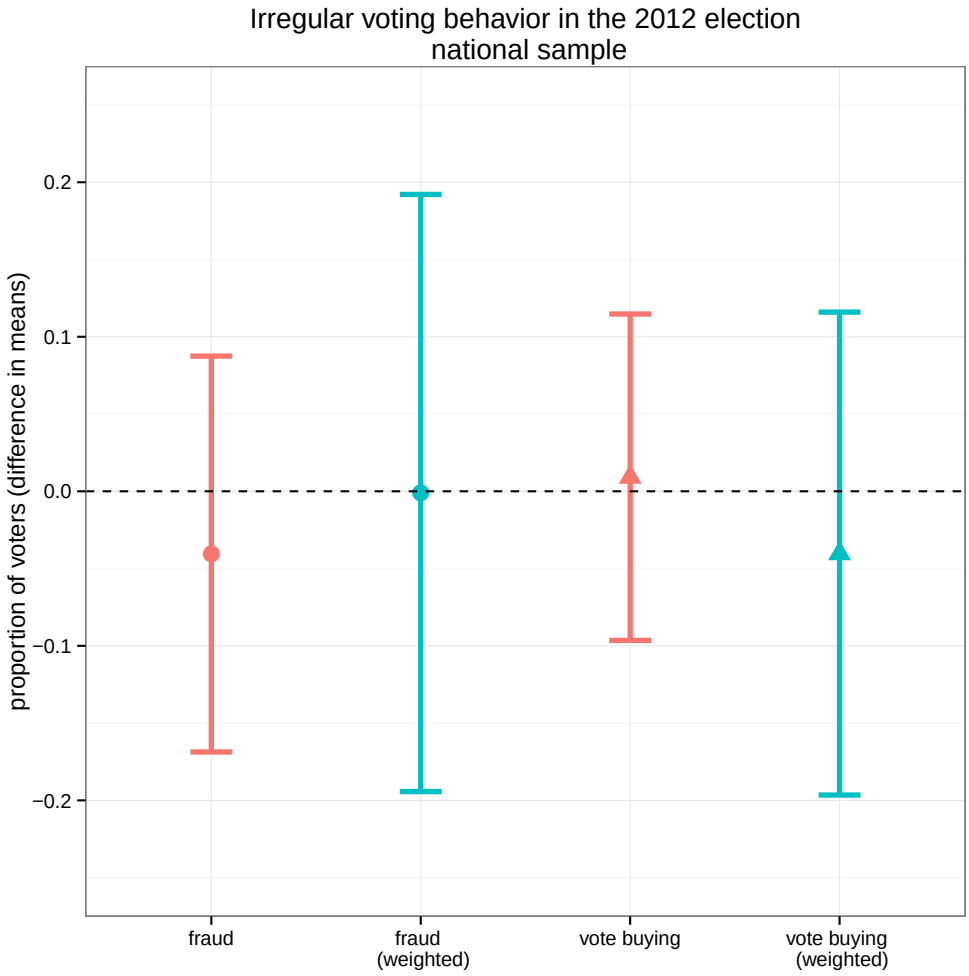

Figure 2: Weighted and unweighted differences in the mean number of list experiment items chosen between treatment and control groups, from a national internet sample fielded December 15-17, 2012. Vertical bars represent 95% confidence intervals. There is no evidence that voters systematically cast fraudulent ballots. Nor is there evidence of attempts to bribe voters.

might be relevant. Most obviously, the incentives to engage in voter fraud or vote buying are stronger in states where the election is closest. To that end we compare contested states to uncontested states. We define a contested state as one in which –those where the margin of victory for the major party presidential candidate was less than 7%: Colorado, Florida, Iowa, Nevada, New Hampshire, North Carolina, Ohio, Pennsylvania, Virginia, and Wisconsin. This

comparison is visible in the top rows of figures 3 and 4. There is no clear evidence that the treatment and control distributions differ, regardless of whether the respondent comes from a contested state. If anything the voter fraud *control* group in contested states seems to have more of its respondents in the higher categories. For vote buying there is no obvious difference between the treatment and control groups in either set of states.

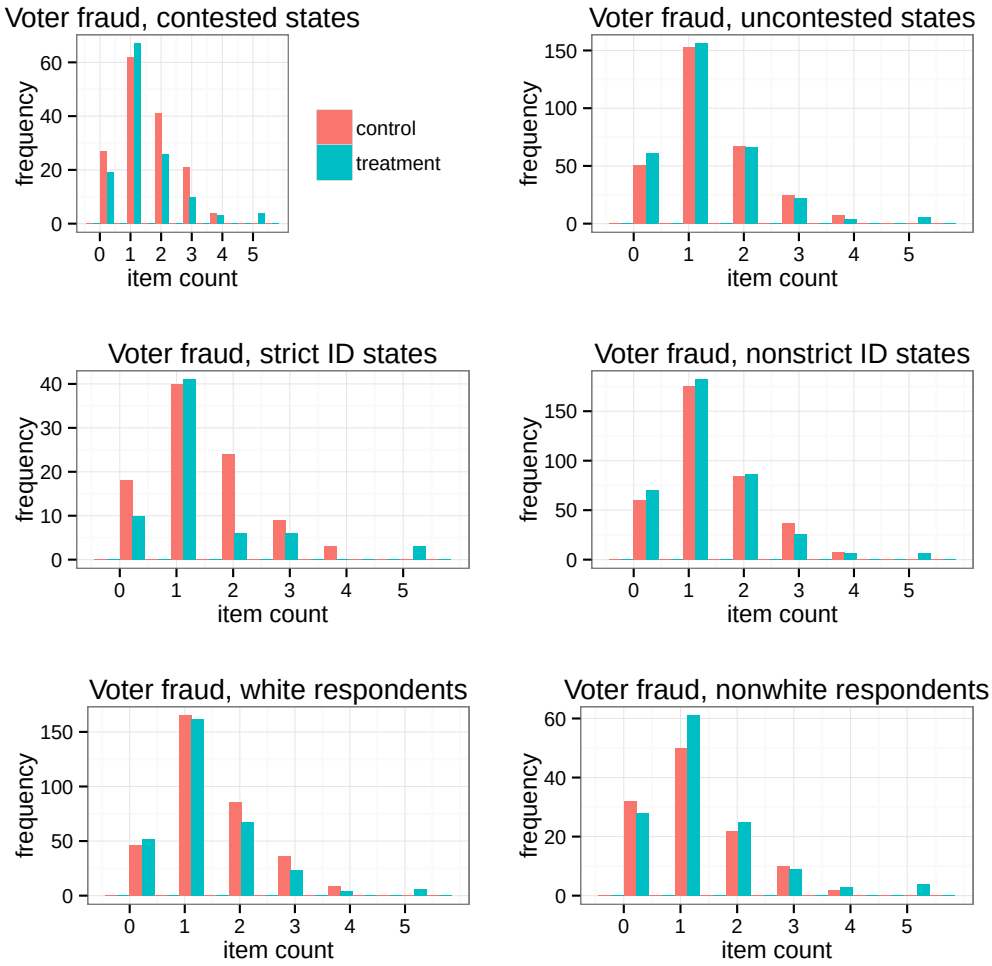

Figure 3: Frequency distribution of number of chosen items to the fraudulent vote list experiment question by treatment status and covariates (unweighted data).

Another possibility is that voter fraud (and possibly vote buying) will be more prevalent where it is easiest–those states that lack strict voter ID laws. In other words, if voter ID laws

are effective in reducing or eliminating fraudulent voting then we should seen noticeably lower levels of fraudulent voting in states with those laws. We rely on the coding of state voter ID laws developed by the National Conference of State Legislatures (National Conference of State Legislatures, 2013). They code state voter ID laws as "strict photo ID", "photo ID", "strict non-photo ID", and "no ID." "Strict" states have require that a voter without the required ID cast a provisional ballot that is kept separate from other ballots and not counted unless the voter returns with the necessary identification within a fixed time frame. We split the data on a strict/non-strict ID basis based on whether the respondent comes from a state that the NCSL reports as having a strict ID law in force for the 2012 election. These states are Arizona, Georgia, Indiana, Kansas, Ohio, Tennessee, and Virginia [double check this list]. The second rows of figures 3 and 4 display the distribution of responses broken out by treatment status and whether the respondent was in a strict voter ID state. Again, there is no obvious visual evidence of fraudulent voting or vote buying in the non-strict ID states. It is not clear that the distribution is any different in strict ID states.

Finally, we cannot ignore the racial overtones of the voter ID controversy. We divide respondents based on self-reported racial identification into white and non-white and display the distribution of responses by treatment status in the bottom rows of figures 3 and 4. There is no clearly visible difference between the treatment and control distributions regardless of race.

3.2.2 ICT regression

Recently published theoretical results (Glynn, 2013; Imai, 2011) and statistical tools (Blair and Imai, 2012) enable rigorous examination of the associations between covariates and voter fraud and vote buying. We fit Item Count Technique (ICT) regression models using the maximum likelihood estimator described in Blair and Imai (2012); Imai (2011).[10] These

[10] All models were fit in 64-bit $\mathcal{R}$ 2.15.1 for Mac OSX using the `list` library.

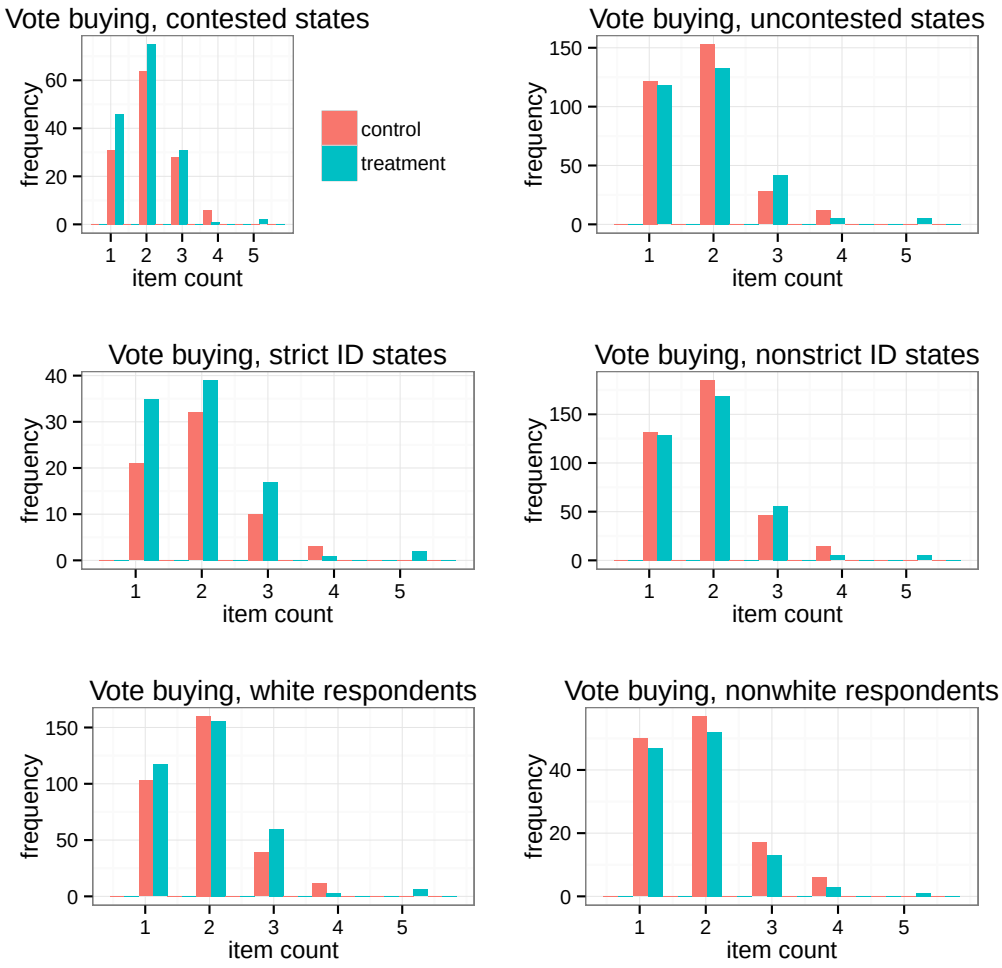

Figure 4: Frequency distribution of number of chosen items to the vote buying list experiment questions by treatment status and covariates (unweighted data).

models use a mixture approach that allows us to describe how different covariates affect responses to both the control items and probability of answering the sensitive treatment item affirmatively; see Imai (2011) and Blair and Imai (2012) for technical details. In all models the dependent variable is the number of items the respondent reported. All models are fit to unweighted data, but we adjust model predictions using weights below.

We account for race, the competitiveness of the election (contested states), and whether the state has a strict voter ID law in place using the variables just described. If there is

meaningful voter fraud taking place we would expect respondents in contested states to be more likely to answer affirmatively for the sensitive items in both the voter fraud and vote buying questions. If strict voter ID laws have the effect of dampening voter impersonation we should observe a reduced probability of reporting voter fraud in strict ID states; it is not clear what, if any, effect voter ID laws would have on vote buying.

The control items on both the voter fraud and vote buying questions represent standard ways in which voters can pay attention to and participate in politics. The voluminous voter behavior literature has established that education and income are strong predictors of political knowledge and participation. Women are generally less likely to report opinions on surveys. We include an indicator variable for gender (female) and an indicator for whether the respondent has attended college or beyond. Because respondents are less likely to report household income information, reducing our sample size, we report models with and without the household income variable included. We expect household income to be negatively correlated with both fraudulent voting and vote buying since the time spent in casting multiple ballots is more expensive for wealthier individuals and the marginal value of income for the poor is higher, making their votes cheaper to buy. We interact the household income variable with the contested state indicator under the hypothesis that the incentives to offer gifts for votes is higher in contested states. Finally, we include a variable indicating whether a respondent self identified as "conservative" or "very conservative." We are agnostic about how this might affect the propensity to respond to the various items on the survey, but the belief in the existence of voter fraud tends to be higher among conservatives (cite).

We report coefficient estimates and standard errors in Table 3. The first two columns report results for the models fit to the voter fraud responses, with the second column including the household income variable and the interaction term. The second pair of columns do the same for the vote buying responses. The top half of the table reports coefficient estimates (and standard errors) describing the effect of a covariate on the probability of answering

affirmatively to the sensitive list experiment item (the "treatment" option). The bottom half of the table describes the effect of a covariate on answering affirmatively to the control items in the list experiments.

We highlight several findings in these models. First, looking at the control items, we see results consistent with existing knowledge of voter behavior. People in contested states, white voters, and those with a college education report significantly more political involvement, as captured in the control items in both the voter fraud and vote buying list experiments. Female respondents are less likely to be involved. Those with higher household incomes answer affirmatively to more control items in the voter fraud list, but in the vote buying list there is no statistically meaningful relationship to be seen. Political conservatism and the presence of strict voter ID laws has no relationship with affirmative answers to the control items. That we replicate replicate well-known relationships from prior research with our survey increases our confidence in the instrument.

Turning to results for the sensitive items, the results are noteworthy for the lack of any systematic relationships. Being in a contested state has a positive but statistically insignificant relationship with the voter fraud item and a *negative* relationship with vote buying. The sign on the strict voter ID coefficient is positive in columns 1, 3, and 4, the opposite of what we would expect. In all four columns the coefficient is insignificant. Gender, race, conservatism and education are all insignificant predictors of affirmative responses to the sensitive items for both voter fraud and vote buying. The only coefficients for the sensitive items that even approach conventional statistical significance are the interaction terms between income and contested state, but these, too, are in the opposite direction of what we would expect: they are *positive*, implying that richer respondents–not poorer–are more likely to respond affirmatively to fraudulent voting and vote buying. In short, we see no evidence of any clear relationship between our covariates and either vote buying or voter fraud. Several of these coefficients estimates are opposite what we would expect under any

Table 3: ICT regression models for list experiments on fraudulent voting and vote buying

	Fraud (base)	Fraud (inc)	Vote buying (base)	Vote buying (inc.)
sentitive: Intercept	−2.30	−14.56	−2.31	−1.57
	(0.92)	(1120.99)	(1.31)	(1.43)
sentitive: strict voter ID	0.57	−0.79	0.32	1.12
	(0.69)	(1.19)	(0.95)	(1.17)
sentitive: contested state	0.39	−1.21	−0.85	−8.32
	(0.63)	(1.37)	(0.91)	(4.77)
sentitive: income		−0.19		−0.01
		(0.20)		(0.22)
sentitive: contested × income		0.51		1.08
		(0.28)		(0.58)
sentitive: white	−0.91	−0.77	0.81	0.38
	(0.63)	(0.80)	(1.12)	(1.25)
sentitive: female	0.89	14.00	−0.78	−0.91
	(0.74)	(1120.99)	(0.89)	(1.01)
sentitive: conservative	0.44	0.40	−0.77	−1.75
	(0.66)	(0.83)	(0.98)	(1.33)
sentitive: college degree	−0.06	−1.23	−0.13	−0.24
	(0.63)	(0.98)	(0.83)	(1.03)
control: Intercept	−1.07	−1.19	−0.27	−0.37
	(0.10)	(0.12)	(0.10)	(0.13)
control: strict voter ID	0.01	−0.05	0.01	−0.07
	(0.10)	(0.11)	(0.09)	(0.11)
control: contested state	0.23	0.18	0.23	0.19
	(0.08)	(0.16)	(0.08)	(0.16)
control: income		0.04		0.02
		(0.02)		(0.02)
control: contested × income		0.01		0.01
		(0.03)		(0.03)
control: white	0.19	0.17	0.04	0.04
	(0.09)	(0.09)	(0.08)	(0.09)
control: female	−0.29	−0.30	−0.14	−0.09
	(0.08)	(0.08)	(0.08)	(0.08)
control: conservative	0.10	0.04	−0.02	−0.02
	(0.08)	(0.08)	(0.08)	(0.09)
control: college	0.32	0.26	0.17	0.11
	(0.08)	(0.08)	(0.07)	(0.09)
N	995	852	904	773
log likelihood	−1356	−1144	−1128	−960

reasonable understanding of systematic voter fraud.

To better highlight the insight provided by the statistical models, we generate the estimated prevalence of the sensitive behaviors in the larger population, along with statements of our uncertainty around these estimates (± 2 standards errors). In making these calculations we incorporate survey weights. Estimates derived from each of the four models in table 3 are displayed in figure 5. In all four models the point estimates are positive (as they must be under the ICT-ML model) but the uncertainty around these estimates is considerable. The confidence bands overlap 0 by a large margin in all cases. We are unable to reject the hypothesis that there is, in fact, no fraudulent voting or vote buying in the population.

3.2.3 lower bound estimates & floor/ceiling effects

All the evidence presented so far give us little reason to believe that there was any systematic voter impersonation or vote buying in the 2012 US election. But the closeness of the election in several states raises the possibility that even a very small level of voter fraud, systematically directed at one candidate, could have been enough. Indeed Obama's margin of victory in Florida was 0.9% or 74,309 votes. We might be tempted to view the 2.5% of respondents in the fraudulent voting treatment condition who claimed the maximum of five items as an estimated lower bound on the prevalence of fraudulent voting. We think this is not the correct interpretation here for several reasons.

First, examining the broader survey behavior of the twelve respondents who claimed the maximum of five in the treatment condition for the voter fraud question we find the following:

- Recall that those in the treatment group for the voter fraud question were in the control group for the vote buying question. Eight of the twelve respondents who chose "5" in the voter fraud question also went on to chose the maximum possible (four) in for the vote buying question.[11] Another two skipped the vote buying question altogether.

[11] Only thirteen respondents in the vote buying control group chose the maximum.

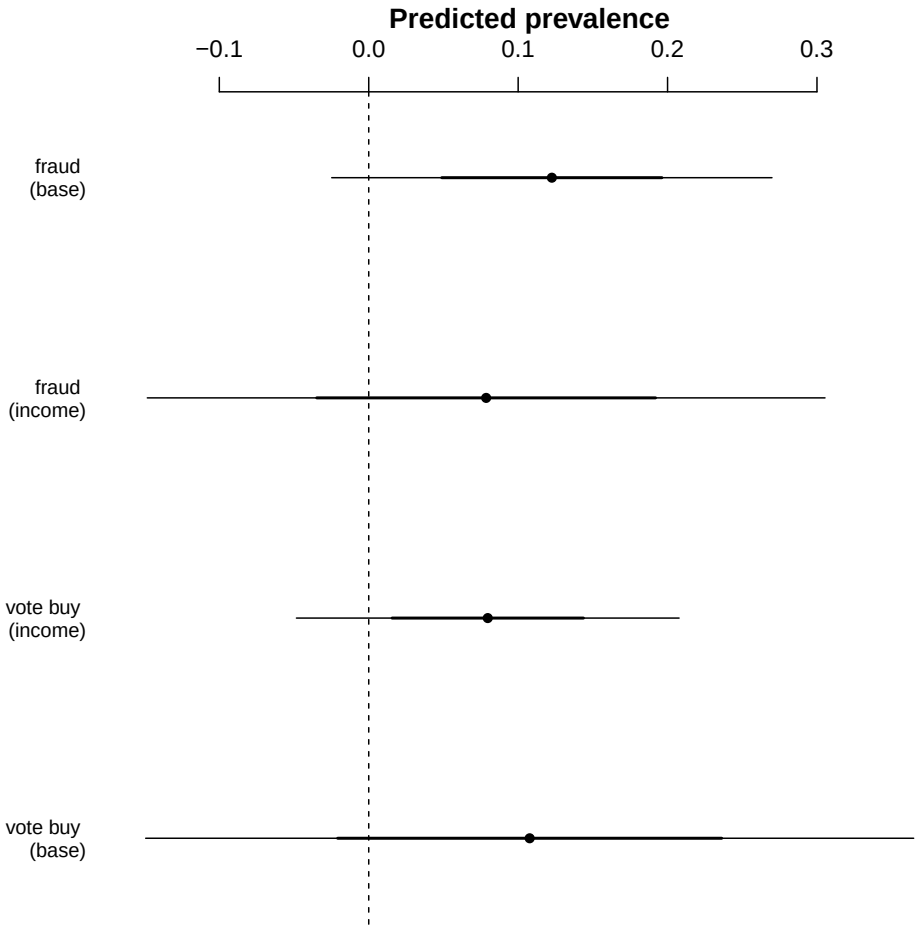

Figure 5: Predicted population prevalence of the sensitive behavior from the models in Table 3 using survey weights. There is no evidence that voters systematically cast fraudulent ballots. Nor is there evidence of attempts to bribe voters. Error bars are ±2 standard errors.

- Survey completion times for all twelve of these individuals was below average and eight of the twelve completed the survey at about the median time or faster.

- Looking at batteries of questions with ten or more consecutive questions following the same response pattern (there were three such batteries on the survey), we see eight different individuals who simply selected the same response for all the questions in the

battery in at least one of the batteries.[12]

- The proportion of respondents choosing the maximum number of items is nearly identical for the treatment and control groups for the fraud question. Those choosing the maximum number in the control condition displayed similar straight-line selection behavior as those in the treatment group.

In other words, most of those choosing the maximum value in the list experiments, whether in the treatment or control groups appear to be rushing to complete the survey as fast as possible, not revealing actual behaviors. If we omit the eight individuals reporting "5" but clearly rushing to finish the survey then the (unweighted) lower bound on the prevalence of casting a fraudulent vote falls under 1%.

Second, the uncertainty around this estimated lower bound, captured by the confidence intervals in figure 5, are wide and clearly cover zero, leading us to conclude that this small number of individuals responding at the maximum level is simply an artifact of the survey measurement process rather than any useful estimate of a lower bound.

Finally, we note that re-estimating the base models for voter fraud and vote buying but allowing for ceiling effects does not materially alter any of the findings presented in table 3 and figure 5. The models presented in table 3 are preferred to models allowing for ceiling effects on a BIC basis.

4 Conclusion

This paper presented what is, to our knowledge, the first and only attempt to estimate, nationwide, the levels of fraudulent voting and vote buying in a major US election. We employed a survey list experiment, including one that has been shown to work in other

[12]Five of the thirteen respondents in the control group that reported the maximum of four also exhibited the straight line choice behavior. In a random sample of twenty respondents from the voter fraud treatment group only two exhibited any straight line choice behavior.

contexts, to better elicit truthful reports of fraudulent voting. Happily, we find no evidence of systematic voter fraud or vote buying in this election. We find this particularly encouraging given the closeness and high stakes of the election along with the amount of money spent by candidates, parties, and "dark money" organizations.

There are limitations to what we can conclude from our findings. First, our findings are necessarily limited to the prevalence of voters casting fraudulent ballots, not the number of fraudulent ballots cast. In principle a tiny number of people could have cast many thousands of fraudulent ballots, but we view this as unlikely, not least because casting in-person ballots, fraudulent or otherwise, is time intensive. Second, our survey has limited statistical power. We cannot reject the null that the amount of fraudulent voting is 0% but nor can we reject the null that the amount of fraudulent voting is 1%. Nevertheless none of our secondary analysis was in any way consistent with the existence of systematic voter fraud: places and people who where we would most expect to see fraudulent voting appear no different from the rest of the country. Future work may consider running similar studies with much larger sample sizes.

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