Alien Abduction and Voter Impersonation in 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 argue that such laws protect the integrity of the electoral process, sometimes claiming that fraudulent voting is widespread. We report the results of a survey list experiment fielded immediately after the 2012 US general election designed to measure the prevalence of one specific type of voter fraud most relevant to voter ID laws: voter impersonation. We find no evidence of voter impersonation, even in the states most contested in the Presidential campaign. We also find that states with strict voter ID laws and states with same-day voter registration are no different from others in the (non) existence of voter impersonation. To address possible "lower bound" problems with our conclusions we run both parallel and subsequent experiments to calibrate our findings. These ancillary list experiments indicate that the lower bound on the population reporting voter impersonation is nearly identical with the proportion of the population reporting abduction by extraterrestrials. Based on this evidence, strict voter ID requirements address a problem that did not exist in the 2012 US election. Effort designed to improve American election infrastructure and security would be better directed toward other initiatives.

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> "I'm always concerned about voter fraud...which is why I think we need to do a point or two better than where we think we need to be, to overcome it." –Republican National Committee chairman Reince Priebus[1]

> "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[2]

Activists and policy makers have sought strict voter identification laws in numerous states in recent years.[3] Proponents of these laws claim that the integrity of American elections is at stake, sometimes alleging that voter impersonation is widespread and sufficient to have altered election outcomes.

Opponents of strict voter identification requirements counter that there is no evidence of widespread or systematic voter impersonation in US elections. They argue that existing proposals do little to make elections more secure but do impose significant additional burdens that fall disproportionately on female, poor, elderly, immigrant, and racial minority voters.

This is a high-stakes policy question. If voter 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. Furthermore, reckless or unfounded claims of fraudulent elections have the potential to poison an already polarized political discourse. Finally, the focus on one specific type of election fraud–voter impersonation–can distract from problems with election security in other domains, such as ballot design, hardware/software security, or absentee voting.

[1] Marley and Bergquist (2012)

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

[3] 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.

The extent of fraudulent voting is central to debates about the need for voter identification laws. But the prevalence of fraudulent voting, as with any illegal or largely private matter, is difficult to measure. Existing studies, relying largely on documented criminal prosecutions and investigations of apparent irregularities, turn up very little evidence of fraud. Critics argue that this is unsurprising because casting fraudulent votes is easy and largely undetectable without strict photo ID requirements. To that end, we present the results of the first application of survey list experiments to the question of voter impersonation in American elections. List experiments are a commonly used social scientific tool for measuring the prevalence of illegal or undesirable attributes in a population. In the context of electoral fraud, list experiments have been successfully used in locations as diverse as Lebanon, Russia and Nicaragua. They present our best tool for detecting fraudulent voting in the United States.

To summarize our findings: using a nationally representative Internet sample we find no significant indicators of voter impersonation in the 2012 US general election. We find no evidence of voter impersonation or vote buying in contested states or among low income voters, subsets where vote fraud is alleged to be most common. Most importantly from a policy perspective, we find no difference between states with and without same day voter registration (where fraud is again alleged to be easiest) and no difference between states with and without strict voter ID requirements (where it should be hardest).

The little evidence we do have pointing toward voter impersonation appears to be driven by a small number of respondents rushing through the survey. To address this "lower bound" issue we ran additional list experiments, both concurrently and subsequently, to validate our sample and calibrate our survey instrument. First, we ran another list experiment concurrently in December 2012 using a survey instrument that had successfully detected vote-buying in Nicaraguan elections. We find no evidence of vote buying in the 2012 US election. Second, we fielded four survey list experiments in September 2013, two of which

repeated the November 2012 experiments (voter impersonation and vote buying) and two additional list experiments. Findings for voter impersonation and vote buying in the second wave mirror those from November 2012. In the first of the new experiments we use our sample to successfully estimate the prevalence of an illegal/undesirable behavior that is known to be common: sending text messages while driving. Our results are consistent with other estimates. The second new experiment presented respondents with the opportunity to admit to something believed not to occur: abduction by extraterrestrials. We find that the lower bound of the population admitting to voter impersonation is the same as that admitting to alien abduction, leading us to conclude that any lower bound estimate for voter impersonation is largely the result of respondent error rather than a true self-report of behavior.

These findings come with two caveats. First, it is very difficult to empirically demonstrate the non-existence of a phenomenon. Our survey has limited statistical power: we cannot reject the null of no fraudulent voting but nor are we able to reject the null of other small values. Second, we look only at the type of electoral irregularity directly relevant to the arguments of voter ID advocates. We cannot comment on other possibilities directly.

The next section briefly reviews the existing studies of fraudulent voting in US elections. Section 2 describes our survey list experiment. Section 3 presents our findings. We first present our basic "headline" results. We then subject those results to a additional statistical scrutiny. We conclude with some methodological observations about list experiments and some recommendations about where resources are better spent in running secure elections that maximize the ability for voters to participate.

1 Voter fraud in US elections

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.[4]

Voter ID, however, addresses a one specific form of voter fraud: casting a ballot in another person's 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; both would be prevented by requiring voters to provide proof of identity at registration and ballot casting. Other forms of voter 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. Nearly all verified cases of voter fraud fall in to these latter categories.

As important as voter impersonation is to this issue, there is strong disagreement about how often it actually occurs. Voter ID proponents insist that fraud is widespread 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, a possibility that warrants security measures as a preventive. Critics of voter ID counter that there is little evidence that vote fraud occurs

[4]The first and second examples are true. Congress investigated the extent of Tammany Hall's corruption of the electoral process after the Civil War (US Congress, 1869) and there is compelling evidence that Johnson's 1948 win was the result of fraud (Caro, 1991:302-17). 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 exaggerated, despite confirmed cases of fraud. In the 19th Century, 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)

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 to support Democrats. Public gaffes by Republican legislators in Pennsylvania[5] and South Carolina[6] along with statements by a disgraced former Florida Republican party chairman[7] only served to reinforce this perception.

The assertions made by proponents of voter ID fall in to four categories. The first ignores the question of whether there is any in-person voter fraud and argues that strict voter ID requirements are necessary to ensure a secure election process. The remaining three categories involve overstating the known occurrence of the specific type of voter fraud–voter impersonation–that an ID requirement would prevent. Claims in the second category cite irregular voting behaviors unaffected by voter ID requirement–voting by disenfranchised felons or voting both absentee and in person–as evidence that voter ID restrictions are needed. A variation on this theme is counting the inevitable human errors in election administration–recording incorrect names, marking down the wrong person as voting, or data entry errors–as evidence of widespread voter impersonation. Claims in the third category insist that any examples of voter impersonation are only the tip of the iceberg, proving electoral corruption is widespread. The claim, as illustrated by quotation from the Wisconsin Assistant Attorney General at the beginning of the paper, is that fraud is so easy to commit and so difficult to detect that authorities can only catch a fraction of the offenders. Fourth, claims of impersonation are offered with no substantiation–easy to make, far more difficult to authenticate–as

[5]Pennsylvania House Majority Leader Mike Turzai said "Voter ID, which is gonna allow Governor Romney to win the state of Pennsylvania, done." (Cernetich, 2012)

[6]State Rep. Alan Clemmons, author of the state's voter ID law, testified in federal court to responding positively to racist emails sent by supporters of the voter ID bill. (Cohen, 2012).

[7]"The Republican Party, the strategists, the consultants, they firmly believe that early voting is bad for Republican Party candidates, (Kam and Lantigua, 2012)

definitive proof of endemic fraud. A few examples follow.

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 (2012:7) 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." 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). Minnite 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."

Government commissions and agencies can also jump to conclusions. 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). However, 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.

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. Virtually all the major scholarship on voter impersonation fraud–based largely on specific allegations and criminal investigations–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). 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).

Christensen and Schultz (2013), in a clever twist, develop a new method relying votes

that are highly unusual based on a voter's past and future voting behavior as a way of identifying voter impersonation. Their approach requires that analysts identify specific electoral jurisdictions of interest ex ante. It is also quite data intensive: they need data at the individual voter level, typically from voter registration rolls, and requires data for several sequential elections. As a result their approach scales poorly to the national level but has the virtue of identifying specific jurisdictions, races, or even votes where identity fraud was particularly likely. Our survey list experiments are relatively quick and inexpensive to run and do not rely on assumptions about the probable location of voter impersonation. We view out approach as complementary to theirs and, like us, they find no evidence of voter impersonation in elections where it was not already known to have occurred.

Opponents of voter ID argue that voter impersonation makes little sense. From the perspective of someone attempting to steal an election, using impersonators of registered voters is time consuming, expensive, and scales poorly. From the perspective of a voter, impersonating someone else at the polls is a crime that makes little sense: the costs in time and effort are non-trivial, and the existing criminal penalties are steep. In the United States Code voter impersonation or vote buying/selling in federal elections is subject to up to five years in prison and a $10,000 fine for each count.[8] The likelihood that a handful of fraudulent votes would change an election 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 steep that any individual benefit offered to a impersonator would have to be significant.

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

[8]Title 42 U.S. Code, Chapter 20, Subchapter I-A §1973i (c).

in the absence of specific allegations of fraud is even more difficult to detect. Accordingly, we apply a method that does not rely on ex ante allegations of specific incidents of fraud to estimate 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. Such systematic underreporting of opinions or actions believed to be objectionable is referred to as "social desirability bias." In recent years, especially since the advent of computer-mediated surveys and representative Internet samples, we have seen resurgence in the use of a powerful tool for just this purpose: the list experiment.[9]

Survey list experiments provide a way of eliciting information about sensitive, illegal, or socially undesirable behaviors and opinions that people would be unlikely to admit to if asked directly. In list experiments survey respondents are presented with a list of items and are asked *how many* (as opposed to which) of these items pertain to them. Since respondents only report a number there is no way to infer whether a specific individual admits to the sensitive item unless she intentionally choose the maximum possible, and even then it is questionable to believe this admission, as we will show below. To measure the prevalence of the sensitive item respondents are randomly split in to two groups. The control group sees a list with a set of innocuous items on it. The treatment group sees the same list, with the addition of one additional item describing the sensitive behavior of interest. Assuming

[9]List experiments are sometimes referred to as the "item count" or "unmatched count technique." (**?**)

randomization worked appropriately, the only difference, on average, between the treatment and control groups is the number of items on the lists they see. The difference in the average number of items reported by members of the treatment and control groups is then an estimate of the prevalence of the sensitive item in the larger population.

List experiments have been shown to elicit more truthful answers in such circumstances. They have been used to great effect in the study of a variety of sensitive topics, including racial attitudes (Gilens, Sniderman and Kuklinski, 1998), self-reported voter turnout (Holbrook and Krosnick, 2010), and voter fraud/election irregularities in Lebanon (Corstange, 2012), Nicaragua (Gonzalez-Ocantos et al., 2012) and Russia (Frye, Reuter and Szakonyi, 2014).[10].

2.1 the core list experiment

We conducted our list experiments using a YouGov internet survey of 1000 US citizens aged 18 and over.[11] The survey was in the field December 15-17, 2012. Respondents were selected from YouGov's opt-in Internet panel using sample matching.[12]

We conducted two list experiments concurrently. The primary question investigates voter impersonation while the second investigates vote buying–offering voters specific and personal goods or services in exchange for their vote. During administration of the Web questionnaire respondents were randomly assigned to two groups, A and B, with equal probability. In order

[10]Also see Gingerich (2010); Glynn (2013) for further reviews of additional applications.

[11]We do not restrict our survey to registered voters or those who reported voting in the election because there is no reason to believe that voter impersonation would be restricted to these populations. We could imagine that voter impersonation might be more common among those who would not self-reporting having voted.

[12]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 American 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.

to avoid potential issues with with priming or contamination[13] we let group A serve as the control group for B and vice versa.[14] Members of group A see a list with five items on the first list experiment question and four on the second question. Members of group B played the reverse roles, seeing four items for the first question and five on the second.

We note that there was a programming error in implementing the vote buying list experiment. The web questionnaire inadvertently failed to present respondents the option of answering "0." Instead respondents saw only the possible responses of $\{1, 2, 3, 4\}$ (for control) and $\{1, 2, 3, 4, 5\}$ (treatment). Since both treatment and control groups suffered from this problem we can still compare the mean number of items chosen across groups for this question, but we cannot use the ICT tools discussed below to analyze this question. The voter impersonation question of primary interest did *not* suffer from this problem.

Although election fraud can occur in a variety of ways we focus on casting a ballot under a false name as the only form of fraud that voter ID laws can possibly address. We constructed the list experiment described in Table 1 to address this question of voter impersonation. Control items 1-3 are innocuous ways individuals may participate in the electoral process.[15] 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. Note that our list experiment is designed to capture the prevalence of voter impersonation at the polls, *not* the number of fraudulent votes cast.[16]

[13]For example, contamination would occur if seeing the sensitive item for the first list experiment affected the response to the sensitive item on the second list experiment, were a respondent to be assigned to the treatment group for both.

[14]While our list experiment is not a strictly a "double list experiment" (Droitcour et al., 2004; Glynn, 2013) we would expect responses to the treatment items for voter impersonation and vote buying to be positively correlated.

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

[16]Here is where we believe the survey experiment and "orphan"/low propensity vote methodology of Christensen and Schultz (2013) will be particularly complementary.

Table 1: Voter impersonation 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"

To mitigate concerns that our (non-)findings are due to poorly worded survey instrument we conducted the second list experiment on vote buying using a survey instrument that closely mimics one that has already proven successful elsewhere. Our vote buying list experiment follows that described in Gonzalez-Ocantos et al. (2012). Using a similar sample size to our own (their $N = 995$), the Gonzalez-Ocantos et al. (2012) list experiment finds an estimated 24% of voters in the 2008 Nicaraguan municipal elections were offered gifts in exchange for their vote.[17] 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.[18]

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

[18] 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 didn't 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. Since in expectation the only difference between the treatment and control conditions is the presence of one additional item on the list, a difference in means provides an estimate of the population-level prevalence of the behavior in question. For example, a difference in means of 0.20 in the voter impersonation list experiment would lead us to infer that 20% of the US adult population engaged in voter impersonation in the last election.

Figure 1 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 voter impersonation or vote buying. In fact the difference in means for the impersonation treatment using unweighted data is actually less than zero, as it is for the weighted difference in means for the vote buying question. The notion that voter impersonation is a widespread behavior is totally contradicted by these data.

horizontal

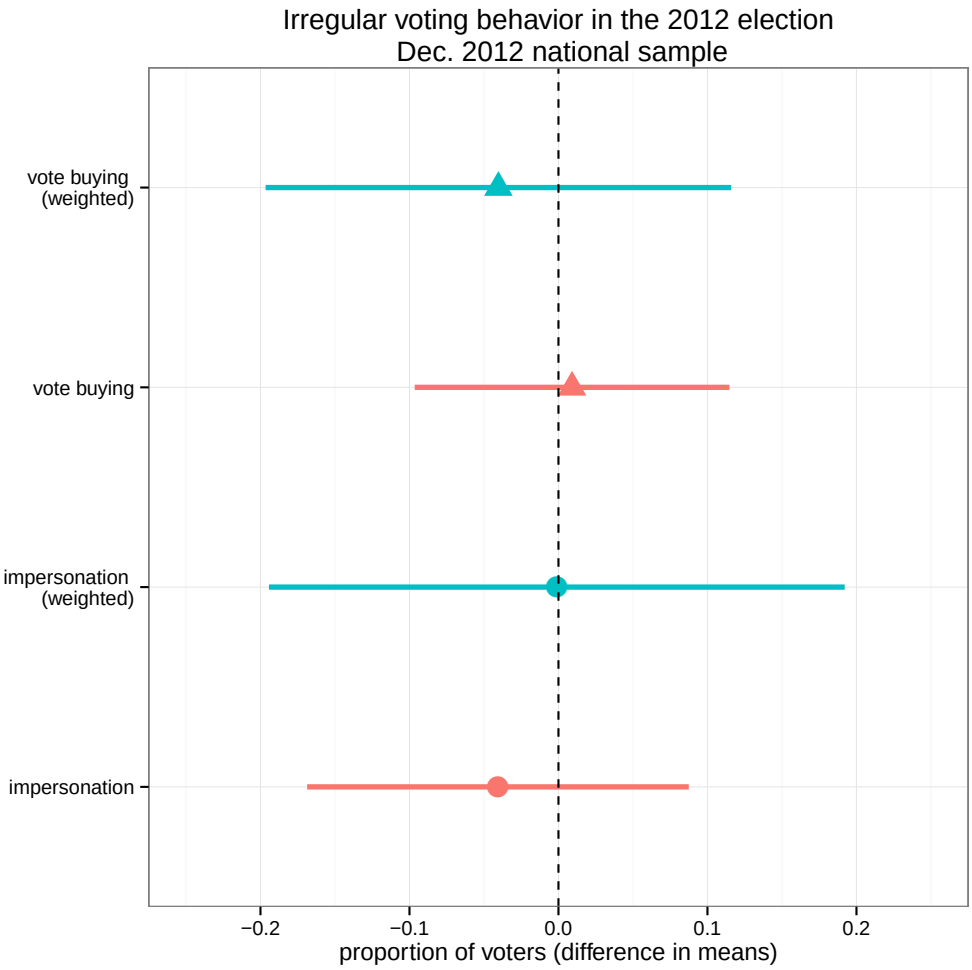


Figure 1: Weighted and unweighted differences in the mean number of list experiment items chosen between treatment and control groups, from a national internet sample fielded December 15-17, 2012. Horizontal bars represent 95% confidence intervals. These estimates are inconsistent with the hypothesis that voter impersonation or vote buying are widespread behaviors.

3.2 Multivariate analysis

3.2.1 Differences in means by relevant subgroups

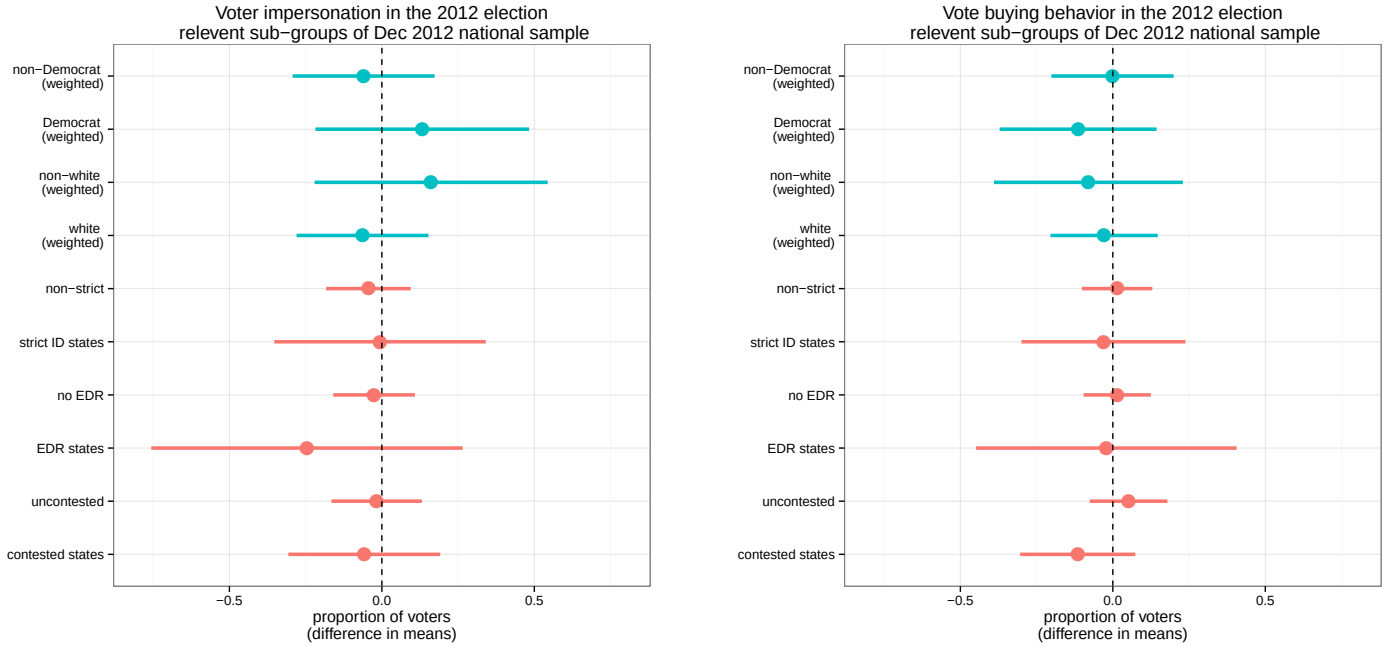
The analysis in the previous section averaged over a nationally representative sample. It seems reasonable to imagine that voter impersonation or vote buying might be more prevalent

among some subpopulations than in others.

We first look at the difference between treatment and control groups in the mean number of items chosen across several partitions of the data that might be relevant.[19] Most obviously, the incentives to engage in voter impersonation or vote buying are stronger in states where the election is closest. To that end we compare respondents in contested states to those in uncontested states. We define contested states as 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; these were the states media reports referred to as swing states in the run up to election day. This comparison is visible in the first two bars from the bottom in figures 2(a) and 2(b). The first bar represents the difference in the mean number of items chosen between treatment and control groups in the contested states, along with the 95% confidence interval. The second bar displays the same information for respondents from uncontested states. In neither case is there evidence that would lead us to reject the null hypothesis of no irregular voting behavior. Again the confidence intervals overlap 0 by a large margin. Similarly for vote buying there is no meaningful difference between the treatment and control groups in either contested or uncontested states.

Some policy makers and activists have claimed that election day registration (EDR), in which voters can register and cast ballots on election day, enables fraud. We therefore compare the differences between treatment and control groups in states with EDR in 2012 (Idaho, Iowa, Maine, Minnesota, Montana, New Hampshire, Wisconsin, and Wyoming) to those without. These results are displayed in the second pair of horizontal bars from the bottom of the figure. Again there is no evidence that would lead us to conclude that there

[19]We use unweighted data when comparing across state-level variables since our survey weights represent national level population weights, not weights appropriate to the populations composing the various subsets of states here. When partitioning on individual-level characteristics, however, we do report weighted data. The non-findings reported here do not change if we were to use national-level population weights.



(a) voter impersonation (b) vote buying

Figure 2: Differences between treatment and control groups in the mean number of items chosen for the vote buying list experiment across relevant partitions of the data. Horizontal bars represent 95% confidence intervals.

is meaningful voter impersonation or vote buying in either EDR or non-EDR states

Another possibility is that voter impersonation (and possibly vote buying) will be more prevalent in states that lack strict voter ID laws. If voter impersonation is common and preventable with voter ID laws 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 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.[20] These states are Arizona, Georgia, Indiana, Kansas, Ohio, Tennessee, and Virginia. The third pair of bars in figures 2(a) and 2(b) display the difference in means between treatment and control groups depending on whether a respondent was in a strict voter ID state. Again, there is no evidence that would lead us to conclude that there is systematic voter impersonation or vote buying non-strict ID states. Strict ID states appear no different in this regard.

We cannot ignore the racial and partisan overtones of the voter ID controversy. We divide respondents based on self-reported racial identification into white and non-white and display the difference in means between treatment and control across these two groups. These results are also displayed near the top of figures 2(a) and 2(b). There is no clearly visible difference between the treatment and control distributions regardless of race. When we explicitly look for evidence of any partisan difference, comparing self-identified Democrats with non-Democrats in the top pair of bars we continue to see no significant differences between the groups.

3.2.2 ICT regression

The standard analysis of list experiments uses the simple difference-in-means estimates we just reported. But recently developed theoretical results (Glynn, 2013; Imai, 2011) and statistical tools (Blair and Imai, 2012) let us say more. Specifically, the Item Count Technique (ICT) regression uses the number of items the respondent reported as the dependent variable, but exploits aspects of the data combined with some testable assumptions to construct multivariate regression models.[21] These models allow us to simultaneously estimate how different covariates relate to both the control items and the probability of answering affirmatively to

[20] Substantive interpretation of findings are similar if we instead use photo ID or strict photo ID states instead.

[21] The primary assumption in play is whether responses to the control items are affected by the presence of the treatment item, referred to as the assumption of no *design effect*. Applying the test described in Blair and Imai (2012) to our data we calculate $p-$values 0.27 for the voter impersonation experiment and 0.55 for the vote buying experiment, failing to reject the null of no design effect.

a greater number of the sensitive items. the sensitive treatment items while accounting for the effects of other variables. An added benefit of the ICT regressions in this case is that we can use the control items to evaluate whether our survey replicates common findings in the American voter behavior literature. We fit ICT regression models using the maximum likelihood estimator described in Blair and Imai (2012); Imai (2011).[22] For computational ease all models are fit to unweighted data, but we adjust model predictions using weights below.

The maximum likelihood estimator that we employ for the ICT regression is based on the beta-binomial likelihood. We estimate two sets of regression coefficients for each covariate, one set that describes the relationship between a covariate and the probability of answering affirmatively for the treatment item, conditional on being in the treatment group and a second set that governs the *average* probability of answering affirmatively to the control items. Coefficient estimates in the former set allow us to investigate whether voter impersonation is taking place in places or among populations where it is most expected, adjusting for the other variables in the model. Coefficients for the latter set are frequently ignored as uninteresting or nuisance parameters. In this case, however, they are worth examining because all the control items in Table 1 represent different forms of political participation or attention. A positive coefficient implies that a covariate is associated with more affirmative answers among the control items and therefore a higher level of political involvement around the 2012 election. We can include covariates representing well-established findings about American political participation in order to check whether our survey is working appropriately.

As covariates we include race, the competitiveness of the election (contested states), whether the state has EDR, and whether a strict voter ID law was in place using the variables described in the previous subsection. If there is meaningful voter fraud taking place we would expect respondents in contested states to be more likely to answer affirmatively for

[22] All models were fit in $\mathcal{R}$ 3.0.0 using the `list` library (Blair and Imai, 2010).

the treatment item. If strict voter ID laws have the effect of dampening voter impersonation we should observe a reduced probability of reporting the treatment item for respondents in states with strict ID states, conditional on the other covariates in the model.

Some have claimed that absentee and mail voting are particularly prone to voter fraud. We want to account for significant cross-state differences in the availability and use of absentee ballots. We use the data reported by the United States Election Assistance Commission (2013) to calculate civilian absentee ballots transmitted as percent of total ballots actually cast.[23]

We also include the several demographic controls based on existing findings about political participation. Education and income are well-established and strong predictors of political knowledge and participation so we include reported household income and an indicator for whether the respondent has attended college. Women are generally less participatory in politics (Burns, Schlozman and Verba, 2001), even though they are more likely to vote (United States Census Bureau, 2013), so include an indicator variable for gender (female). 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 (Ansolabehere and Persily, 2008).

We report coefficient estimates and standard errors in Table 3. As usual, respondents are less likely to answer income questions, reducing our sample size and inducing quasi-complete separation in the gender variable. We therefore report two models. The first excludes the household income variable. The second is a model including household income with missing values imputed.[24] The top half of the table reports coefficient estimates (and standard

[23]Note that this value could exceed 100%, as it does in Washington state where all voting is conducted by mail. In this situation the state sent out more ballots than were ultimately cast. Results are substantively identical if we omit Washington and Oregon respondents from the analysis.

[24]We use Amelia II (Honaker, King and Blackwell, 2011) for R to impute missing values. Reported parameter estimates and standard errors are the result of averaging over 20 imputed datasets.

errors) describing the effect of a covariate on the probability of answering affirmatively to the treatment item for the list experiment. The bottom half of the table describes the effect of a covariate on answering affirmatively to more of the control items in the list experiments.

We highlight several findings in these models. First, results for the control items are consistent with existing knowledge of voter behavior. Specifically, we find that respondents in contested states, those with higher household incomes, and those with a college education report significantly more political involvement, as captured in the control items in the voter impersonation list experiment. Female respondents are less likely to report being involved in political activities. Political conservatism and the presence of strict voter ID laws has no relationship with affirmative answers to the control items. That we replicate well-known relationships from prior research with our survey increases our confidence in the instrument.

Turning to results for the treatment 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 impersonation item. The sign on the strict voter ID coefficient is also both insignificant and unstable. Gender, race, conservatism, education, and household income are all insignificant predictors of affirmative responses to the treatment item. In short, we see no evidence of any clear relationship between our covariates and voter impersonation. Several of these coefficients estimates are opposite what we would expect under any reasonable understanding of systematic fraudulent voting to swing an election.

To better interpret the statistical models, we calculate the predicted prevalence of voter impersonation 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 both models in table 3 are displayed in Figure 3. Unlike the difference-in-means estimator, both estimates are positive here, as they must be under the ICT-ML model, but the uncertainty around these estimates is considerable with the confidence bands overlapping 0 by large margins. We are unable to reject the hypothesis

Table 3: ICT regression models for list experiments on voter impersonation, Dec. 2012 national sample. Model 2 is the average across models fit to 20 imputed datasets.

	Model 1	Model 2
treatment: (Intercept)	−2.27*	−2.24*
	(0.94)	(1.04)
treatment: strict voter ID	0.33	0.50
	(0.77)	(0.78)
treatment: EDR state	−0.45	−0.21
	(1.35)	(1.31)
treatment: absentee	0.01	0.01
	(0.01)	(0.01)
treatment: contested state	0.64	0.46
	(0.75)	(0.74)
treatment: white	−0.98	−0.88
	(0.62)	(0.63)
treatment: female	0.78	0.81
	(0.77)	(0.77)
treatment: conservative	0.34	0.39
	(0.73)	(0.71)
treatment: college degree	−0.19	−0.01
	(0.66)	(0.73)
treatment: household income		−0.04
		(0.13)
control: (Intercept)	−1.06*	−1.24*
	(0.11)	(0.13)
control: strict voter ID	0.06	0.03
	(0.11)	(0.11)
control: EDR state	0.26	0.24
	(0.14)	(0.14)
control: absentee	0.00	−0.00
	(0.00)	(0.00)
control: contested state	0.19*	0.20*
	(0.09)	(0.09)
control: white	0.19	0.16
	(0.10)	(0.09)
control: female	−0.29*	−0.24*
	(0.08)	(0.08)
control: conservative	0.11	0.09
	(0.08)	(0.08)
control: college degree	0.33*	0.22*
	(0.08)	(0.09)
control: household income		0.05*
		(0.01)
Num. obs.	995	1000
BIC	2832	2846

*$p < 0.05$

that there is, in fact, no voter impersonation in the population.

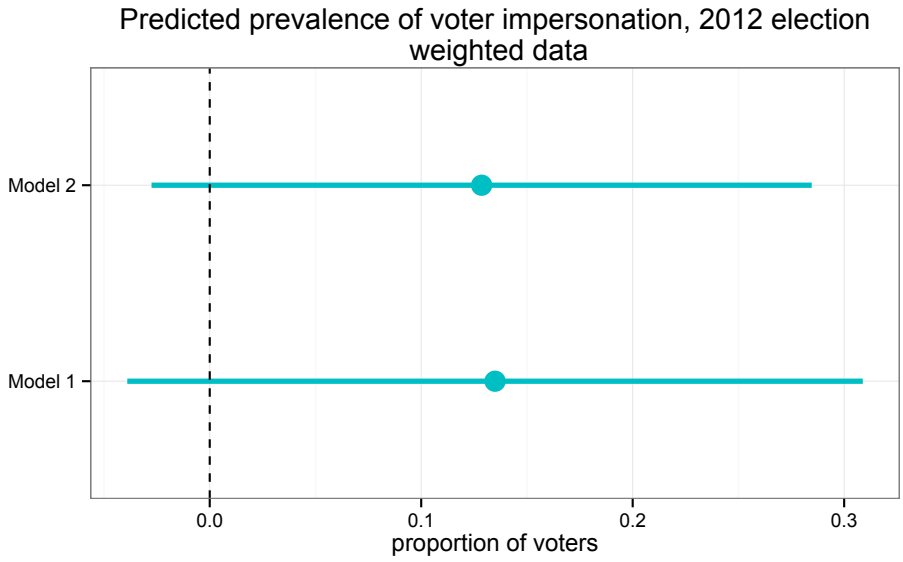


Figure 3: Predicted population prevalence of voter impersonation in the 2012 US election, calculated from the models in Table 3 using survey weights. Error bars are ±2 standard errors.

3.2.3 Interpretation of lower bound estimates

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. Our point estimates of the frequency of impersonation are nonzero, with 12 respondents (about 2.5% of the sample) in the voter impersonation treatment claiming the maximum number of items (5). Indeed these respondents factor notably in identification assumptions of the ICT-ML model (Imai, 2011:410). We might be tempted to view this 2.5% as an estimated lower bound on the prevalence of voter impersonation. However, we think that respondent error, rather than an admission of

fraud, is the more likely explanation 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:

- Eight of the twelve respondents who chose "5" in the voter fraud question also went on to chose the maximum possible (four) for the vote buying question.[25] Another two skipped the vote buying question altogether.

- Survey completion times for these twelve individuals was below the sample 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 made straight line choices, selecting the same response for all the questions in the battery in at least one of the batteries.[26]

- 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%.

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

[26] 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.

3.3 Second wave survey

To further investigate this lower bound issue we returned to the field in September 2013 with a new set of 3000 respondents from the YouGov panel, all adult US residents. If our conjecture that the lower bound observed in the December 2012 survey is in fact an artifact of respondent error inherent in the sample or internet survey process then we should recover a similar lower bound if we repeat the survey. We should also find a similar lower bound value if we present the same subjects with the opportunity to confess to an event believed to be impossible. In other words, we use this second wave to evaluate how noisy our lower bound estimates really are.

To do this our second wave survey consisted of four list experiments, detailed in tables 4-7. Questions A and B are designed to replicate the findings from the December 2102 survey. Note that the wording changed slightly for both the voter impersonation and vote buying questions. This was done to address some concerns about the possible stigma around the "physical fight" response in the December 2012 voter impersonation list experiment. In the voter impersonation question for the second wave we replaced the "physical fight" option with another unlikely but less stigmatizing activity: attending a fundraiser. In the second wave vote buying question we clarified that the actions were taken by "political candidates or activists" as well as making the vote buying transaction more explicit. These minors changes prove to be non-consequential as our findings in the second wave mirror those from December 2012, notwithstanding the passage of ten months.

In Question C (table 6) we are explicitly attempting to demonstrate that our list experiment procedure can recover a population value that is known to be positive and far from 0. In this question the treatment item is whether the respondent sent or read an SMS message while driving. The remaining items in the list were either innocuous or designed to avoid ceiling (travel) or floor effects. For calibration purposes, Madden and Rainie (2010) report the results of a 2010 survey in which they estimate that 27% of US adults have sent

Table 4: Question A–voter impersonation list experiment (September 2013 wave)

Prompt:	"Here are some things you might have done during the election this past November. HOW MANY of these activities were you 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 attended a political fundraising event for a candidate in my home town."
Treatment	"I cast a ballot under a name that was not my own."

Table 5: Question B–vote buying list experiment (September 2013 wave)

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 things happened to you?"
1	"Political candidates or activists put up campaign posters or signs in your neighborhood or city."
2	"Political candidates or activists visited your home."
3	"Political candidates or activists placed campaign advertisements on television or radio."
4	"Political candidates or activists personally threatened you."
Treatment	"Political candidates or activists offered you money or a gift for your vote."

or read a text message while driving. Naumann (2011) reports the results of a large 2011 cross-national survey which estimates that about 31% of U.S. drivers aged 18-64 years had sent an SMS while driving in the last 30 days. Among the eight countries in the survey the reported texting-while-driving rates varied between 15% and 31%.

In Question D (table 7) represents a form of placebo test. In this list experiment we present respondents with an event deemed to be impossible, ridiculous, or vanishingly rare, depending on your cosmological beliefs: alien abduction.[27] We expect that if respondents

[27]We admit to choosing this treatment wording partly for rhetorical purposes. But we pre-tested this question wording against and alternative in which the treatment item was "I won more than a million

Table 6: Question C–common illegal/undesirable behavior list experiment (September 2013 wave)

Prompt:	"Here are some things that you might have done during the past 30 days. HOW MANY did you do?"
1	"I travelled to a foreign country"
2	"I flossed my teeth"
3	"I littered in a public place"
4	"I celebrated my birthday"
Treatment	"I read or wrote a text (SMS) message while driving"

were paying attention and answering truthfully then the lower bound estimate for this question should be 0. The remainder of the items on the list are meant to mitigate ceiling (IRS auditing) and floor (telemarketer call) effects.

Table 7: Question D–impossible event list experiment (September 2013 wave)

Prompt:	"Here are some things that may have happened to you during the past twelve months. HOW MANY of these events happened to you?"
1	"I was asked to serve on a jury"
2	"I was called by a telemarketer"
3	"I was audited by the IRS (Internal Revenue Service)"
4	"An airline cancelled my flight reservation"
Treatment	"I was abducted by extraterrestrials (aliens from another planet)."

In implementing the second survey wave we randomly split respondents into two groups. Those in group 1 saw the full five-item lists for questions A and D while seeing only the four item lists for questions B and C, with those in group 2 saw the converse. Note that respondents seeing the voter impersonation item were also exposed to the alien abduction item. Within each question the list items were presented in random order. Questions (A and B) and (C and D) were given to subjects in pairs, separated by a series of distractor questions but we also randomized which pair of questions the subjects saw first.

dollars in the lottery." Respondent behavior was indistinguishable between the two.

3.3.1 Texting and driving

Unfortunately for our confidence in road safety, our survey experiment works as expected; we find that texting while driving is a prevalent behavior. Based on the weighted difference-in-means between the treatment and control groups we find that about 24% of Americans adults sent or read at least one SMS while driving in the last 30 days. While this is lower than the 2011 CDC report and the 2010 Pew estimate, these other estimates are well within the 95% confidence bounds from our survey.[28] Our survey experiment is able to recover evidence of an undesirable behavior where it is known to exist.

3.3.2 Replication of December 2012 survey findings

We are able to successfully replicate the December 2012 findings with the new survey wave. In Figure 4 we report both the weighed and unweighed difference-in-means for the voter impersonation and vote buying questions from the September 2013 survey. The open triangles/squares represent the point estimates from December 2012 for comparison. All of the point estimates from December 2012 are well within the 95% confidence band from the new survey.

3.3.3 Extra-terrestrials and voter impersonation

Table 4 also reports the difference-in-means (weighted and unweighted) for our key calibration exercise: the alien abduction question. The most striking thing here is that the point estimate for alien abduction *exceeds* that for voter impersonation and is about the same as that for vote buying.

We can use the alien abduction question to calibrate our lower bound estimates. Table 8 compares the proportions of respondents in the treatment conditions choosing "5" across all the list experiments we ran. We see that a naive reading of these responses would lead us

[28]The confidence interval is [11%, 36%]. With unweighted data the 95% CI is [15%, 30%].

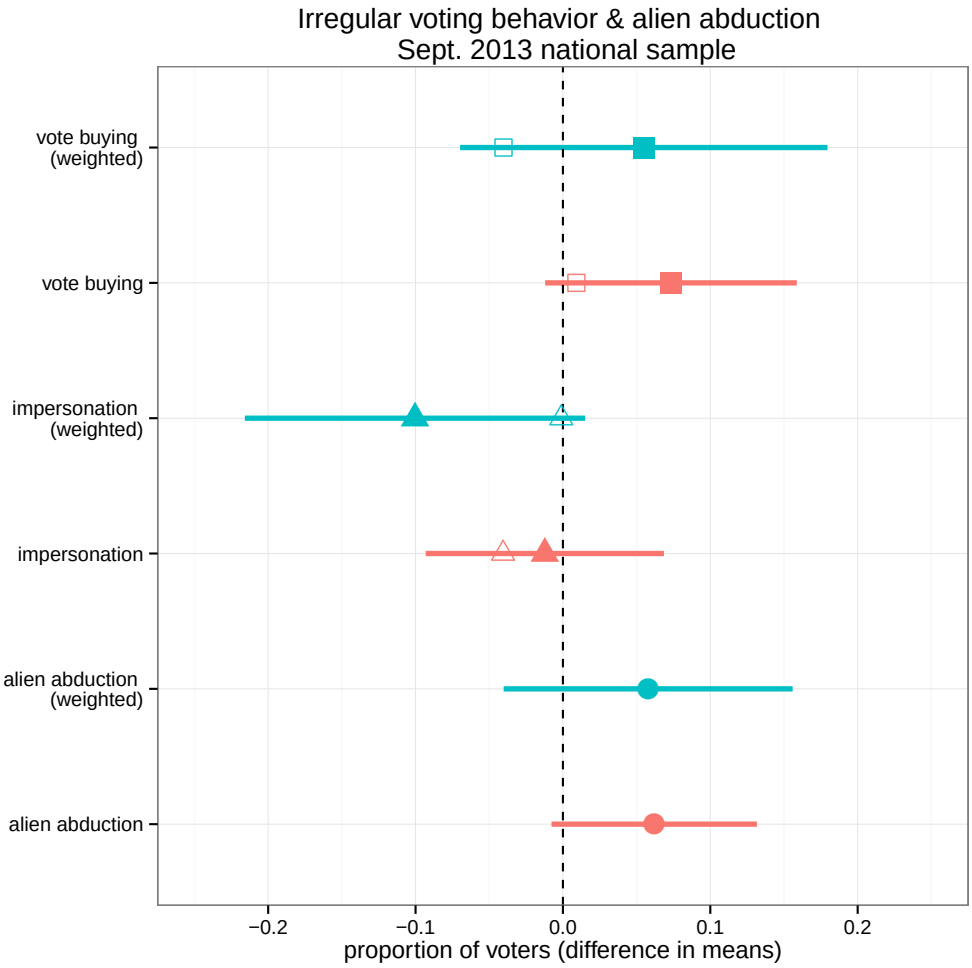


Figure 4: Differences between treatment and control groups in the mean number of items chosen for the September 2013 alien abduction, voter impersonation, and vote buying list experiments. Horizontal bars represent 95% confidence intervals. Hollow points identify estimates from the December 2012 version of the same list experiments.

to conclude that 2.4% of these respondents effectively confessed to alien abduction. This is nearly identical to the 2.5-2.7% we observe for voter impersonation and the 2% choosing "5" in the vote buying questions. Nine (20%) of the 41 respondents choosing "5" in the voter impersonation question also chose "5" for the alien abduction question. All of these proportions are substantially lower than the 3.3% choosing "5" for the texting-while-driving question. The implication here is that if one accepts that 2.5% is a valid lower bound for the

prevalence of voter impersonation in the 2012 election then one must also accept that about 2.5% of the adult US population–about 6 million people–believe that they were abducted by extra-terrestrials in the last year. If this were true then voter impersonation would be the least of our worries.

More seriously, these findings lead to the conclusion that there is a noisy lower bound in these list experiments in the neighborhood of 2.5%, something to be cautious of when describing the data and making claims. Given this noise at the lower end, the difference-in-means seem to be a better reflection of behavior. On this basis we see no evidence of voter impersonation or vote buying in the 2012 election.

Table 8: Evaluating the lower bound on the treatment item across several list experiments. The lower bound for voter impersonation and vote buying is nearly identical to that for alien abduction.

	Wave	% treated choosing "5"	treated N
Voter impersonation	Dec. 2012	2.5%	486
Voter impersonation	Sept. 2013	2.7%	1528
Vote buying	Dec. 2012	1.4%	514
Vote buying	Sept. 2013	2.4%	1472
Alien abduction	Sept. 2013	2.4%	1528
texting while driving	Sept. 2013	3.3%	1472

4 Conclusion

To our knowledge we have presented the first attempt to estimate, nationwide, the levels of voter impersonation and vote buying in a major US election. We employed a survey list experiment, including one that has been shown to work in other contexts, to better elicit truthful reports of irregular voting behavior. We find no evidence of systematic voter impersonation 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.

We also investigated the lower bounds on voter impersonation implied by the list experiment. In a second survey experiment–using a believed-impossible event as calibration–we find that about as many people admit to alien abduction as admit to voter impersonation, reinforcing our conclusions from the earlier survey.

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 only has so much 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, in several instances the difference in means between the treatment and control groups is actually negative. 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. Ramping up the sample size to gain additional statistical power is likely to be prohibitively expensive; our estimates imply that a sample of over 260,000 would be needed in order to discern a difference of of 1% between the treatment and control groups.[29]

Our findings have both methodological and policy implications. The relative performance of various list experiment analysis tools in the presence of measurement error has not, to our knowledge, been formally investigated. We are pursing this angle in other work. From a policy perspective, our findings are broadly consistent with the claims made by opponents of stricter voter ID laws: voter impersonation was not a serious problem in the 2012 election.

[29] Using a two-sample t-test and assuming $\alpha = 0.05$, power of 0.8 and using the sample standard deviations from the December 2012 voter impersonation list experiment. If we were to use the double list experiment and recommendations described in Glynn (2013) we would still need a sample in excess of 130,000.

Those concerned with the security of the American electoral system would do better to focus the attention and resources of our legislatures and voting administrators on issues of equal access, secure and verifiable voting technology, transparent ballot design, and timely and consistent data reporting. Indeed, an open social scientific question is the possible effect that unfounded allegations of election malfeasance have on voter participation and electoral legitimacy.

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