The Effect of Voter ID Laws on Beliefs in Electoral Integrity

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Abstract

Many Americans believe elections are riddled with fraud and other improprieties. Can government policy make more voters believe elections were contested fairly? I bring new evidence to this question by testing the effect of voter ID laws on perceptions of electoral integrity. I improve upon past research by using four waves of survey data with a difference in differences design, examining how opinion changes over time in states that newly adopt a voter ID law compared to states that do not while accounting for average levels of fraud belief in states as well as national trends. My results suggest that voter ID laws are effective in decreasing beliefs in fraud, and there is little evidence that the results are dependent on partisanship.

1 Introduction

Widespread belief in electoral integrity is a bedrock of democracy. However, many Americans report being skeptical about essential parts of the electoral process. For instance, an August 2016 Gallup Survey reported that 36% of Americans view voter fraud as a "major problem", and just 29% thought it was not a problem at all (McCarthy, 2016).[1] As polarization between Democrats and Republicans continues and the stakes of elections increase, it is important that all Americans believe elections are contested fairly (Norris, 2017).

One proposed solution to improve the integrity of American elections are voter ID laws. When states do not have an ID law, the only form of identity verification at the polling place occurs when a voters' signature signature is compared to one recorded at the time of registration.[2] When an ID law is put in place, voters are asked to show some kind of ID before voting, a far more salient and reliable way of ensuring a voter is who they claim to be. Supporters of ID laws claim that this change will cause voters to feel more confident in the integrity of elections.

In this paper, I examine the effects of ID laws on Americans' beliefs in electoral integrity to put the arguments of ID law supporters to the test. While it is possible that voter ID laws improve perceptions of elections, there also also reasons to believe the laws would not cause any changes, cause improvements only among some groups, or even create a backfire effect.

Much of the existing academic literature suggests voter ID laws would not effect public opinion about electoral integrity. Factors strongly associated with perceptions of electoral integrity include a disposition towards conspiratorial thinking, immigrant resentment, racial resentment, level of educational achievement, and partisanship (Sances and Stewart, 2015; Wilson and King-Meadows, 2016; Edelson et al., 2017; Udani and Kimball, 2018; Norris, Garnett and Grömping, 2020). Since these factors are not easily changed by any kind of government policy and are unrelated to the quality of election administration, prospects for policy improving perceptions of electoral integrity would seem bleak.

Another possibility is that voter ID laws only improve perceptions of electoral integrity among Republi-

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[1]These high numbers are not just a 2016 phenomena. According to the Survey of the Performance of American Elections, belief in voter fraud increased just .08 units from 2008 to 2016 on a 1-4 scale, and was actually lower than both 2014 and 2012.

[2]The exact procedures can differ slightly by state, and some newly registered voters in non-voter ID states may asked for ID as required by the Help America Vote Act.

cans, while causing no change or even hurting perceptions among Democrats. The potential security benefits of ID laws come with a critical cost–not all eligible voters (and particularly minority voters who typically vote for Democrats) have the identification the laws require. To many Democrats, these laws undermine democratic principals by making it more difficult for people to vote. Moreover, Democrats claim that voter fraud is virtually nonexistent, and that voter ID laws are a pretext to suppress Democratic turnout (Wang, 2012). Republicans dismiss these concerns, and are close to unanimously supporting voter ID laws.[3] The competing claims of ID law supporters and opponents could cause Republicans to believe voter fraud is less frequent while leaving Democrats unaffected, or even cause Democrats to have less trust in the electoral process.

A third possibility is that ID laws perversely worsen beliefs in electoral integrity among all Americans. To justify ID laws, their supporters often argue that voter fraud is rampant. When ID laws are off the agenda, it is likely that voter fraud is less discussed, less salient issue. Research has found that there is more media coverage mentioning "voter fraud" in states that have passed ID laws, and that media coverage of voter fraud is associated with worsened beliefs in electoral integrity (Fogarty et al., 2015; Udani, Kimball and Fogarty, 2018). Even if ID laws partially assuage people's fear of fraud, the process of passing them could cause people to update their beliefs about the frequency of fraud so much via increased media coverage that the laws have an overall negative effect.

Thus far, the empirical literature on voter ID laws has found scant evidence that the laws reduce fraud (Cantoni and Pons, 2019)[4] or decrease turnout (Highton, 2017). However, that does not preclude voter ID laws from affecting attitudes. Does voting under a voter ID law cause attitudes about American electoral integrity to improve? Or could the laws targeting of Democratic voters who are less likely to have ID cause opinion to polarize, making Republicans more confident in the fairness of American elections while leaving Democrats unaffected? Or could increasing the salience of voter fraud by passing an ID law cause people to believe fraud is more common?

The answers to these questions are important to test whether government policy can cause broad-based changes in Americans' beliefs about the fairness of elections in the face of widespread skepticism. The existing

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[3]For evidence that Republicans support for ID laws are rooted in worries about fraud, see Kane (2017).
[4]Baseline evidence of voter fraud is extremely scant (Minnite, 2010).

research on the connection between electoral administration and attitudes has been mixed, and generally requires strong assumptions to be interpreted causally (Herrnson et al., 2008; Hall, Monson and Patterson, 2009; Claassen et al., 2013; Bowler et al., 2015; King and Barnes, 2018; Udani, Kimball and Fogarty, 2018; Flavin and Shufeldt, 2019).[5] Many policies that are widely regarded by experts as important components of electoral integrity, like paper audits of votes, are unlikely to be recognized by most voters and thus unlikely to produce widespread attitude changes. Voter ID laws are unique in creating a highly salient change to the electoral process. If ID laws cannot improve Americans' beliefs in electoral integrity, it may mean that these beliefs are out of reach of government policies.

The relationship between ID laws and perceptions of electoral integrity is also important to the legal status of voter ID laws. In *Crawford v Marion County Election Board*, the Supreme Court acknowledged that there was no evidence of voter fraud in the state of Indiana. However, the majority ruled that Indiana's photo identification laws were justifiable in part because of the state's interest in "protecting public confidence in elections (Stevens, 2007)." Practically, this ruling means that voter ID laws do not need to be effective at actually stopping fraud to be constitutional. Rather, the laws need to be effective at reducing the perceived existence of fraud.

Perceptions of electoral integrity have also been linked to citizen participation. If people think elections are rigged, there is little use in participating.[6] While it is difficult to precisely isolate the effect of beliefs of electoral integrity on turnout, some research has found that more positive perceptions of integrity is associated with higher turnout (Alvarez, Hall and Llewellyn, 2008; Birch, 2010).

Existing published research has found that on average, voter ID laws do not affect attitudes (Persily and Ansolabehere, 2008; Bowler et al., 2015; Bowler and Donovan, 2016; Stewart III, Ansolabehere and Persily, 2016; King, 2017). However, to infer a causal effect, these studies must assume that differences in opinion between states that have voter ID laws and states that don't are solely attributable to the ID law after controlling for potentially confounding variables.[7] I improve upon this literature by utilizing four waves of the Survey of the Performance of American Elections with a difference in differences research design. Rather

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[5]It's possible that the types of people who are less worried about voter fraud and less conspiratorially minded live in places that have better electoral administration.

[6]This was also cited by the Supreme Court as a justification of Voter ID law's constitutionality.

[7]The one exception is a contemporaneous working paper by Cantoni and Pons, which uses the same difference in difference methodology and data source as I do but finds null effects. I discuss the differences between by paper and theirs in the methodology section and comprehensively in the Appendix.

than assuming any state-level differences of opinion are solely a result of ID laws after applying controls, this research design permits causal identification if the trends (but not necessarily levels) in statewide opinion would be the same in absence of Voter ID laws. By tracking differences in opinion before and after a state adopts an ID law and comparing these differences to the trends in states that don't change their election laws, I have a higher probability of isolating the effect of ID laws from other factors than the previous literature.

My results indicate that Voter ID laws do cause improvements in attitudes about American electoral integrity. Registered voters in states that implemented a voter ID law are less likely to believe people cast multiple ballots or impersonate others when voting compared to states that did not implement a Voter ID law. Despite the polarized nature of ID laws at the elite level, there is little evidence that Republicans respond differently than Democrats. In terms of magnitudes, the effect of adopting an ID law reduces beliefs in voter impersonation as much as going from having a high school diploma to a 4-year college degree, or about 20% of one standard deviation. While not a panacea for Americans' low beliefs in electoral integrity, ID laws do seem to cause a substantial improvement.

One important limitation of my analysis is that I cannot detect if Americans become more worried about voter suppression in states with a voter ID law. Belief in ballot integrity is just one dimension of electoral fairness (Norris, Cameron and Wynter, 2018). It is possible that people (especially Democrats) believe that Voter ID laws decrease fraud but also causes rightful voters will be deterred or turned away at the polls.

While questions about voter suppression and related concepts were not asked consistently over time, I do have data on support for Voter ID laws. Overall, registered voters in states that adopt an ID law are not significantly more or less supportive compared to those who do not adopt one. However, there is evidence that the group most likely to fear voter suppression from voter ID laws, Democrats, become slightly more supportive compared to Republicans. This finding is inconsistent with a world in which passing a voter ID law causes large increases in worries about disenfranchisement from Democrats.

Importantly, my research cannot tell whether the general existence of ID laws has improved Americans' perceptions of electoral integrity. It is likely that the raucous partisan debate over ID laws has made at least some Americans more skeptical of the fairness of American elections. However, the marginal effect of a state passing an ID law does appear to make Americans more confident that voter impersonation and other forms

of fraud are rare. The fact that beliefs about electoral integrity positively respond to electoral policies is encouraging for other electoral reforms.

In the sections that follow, I first review the survey data that I use and show the variation in the adoption of Voter ID laws across states in the period I study. I then explain the difference in difference methodology and the various controls I use to probe for differential trends between treated and untreated states. Third, I show the main results and give a brief account of possible mechanisms and heterogeneous treatment effects. I also explain what could be causing my results to differ from the previous literature. Finally, I conclude by discussing policy implications. While my results represent the first empirical evidence for a concrete benefit of ID laws, there are many outstanding issues with the implementation of ID laws. A productive path for future research would be to examine how other policies without potential negative effects can improve perceptions of electoral integrity.

2   Data

To assess Americans' confidence in elections, I use data from the Survey of the Performance of American Elections (SPAE) 2008, 2012, 2014, and 2016 waves.[8] The SPAE was administered by YouGov/Polimetrix via the internet except for a small portion of the 2008 survey, which was conducted by telephone. Because the SPAE's main purpose is examining the experience of voters on election day, the sampling frame is of registered voters.

The survey contains a variety of questions related to Americans' experience at the polls and, most relevant to this project, their opinions about how frequent different types of fraudulent election activities are in their communities. Questions about confidence your vote was counted correctly, illegal voting, ballot tampering, and voter impersonation were asked in every wave, and are the focus of my analysis. Questions about confidence in your county, state, and the nation's vote were asked in 2012-2016, as well as multiple voting, absentee ballot fraud, and fraud by elected officials. Exact question wordings are in the Appendix.

The SPAE does not sample like a traditional national survey. Rather, it gets representative samples of registered voters from each state of n=200, with each state represented equally.[9] While the relatively small

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[8]This is the entirety of the SPAE data.

[9]The 2008 and 2014 waves provide over-samples of certain states, which I down-weight to be equal to all other states in the
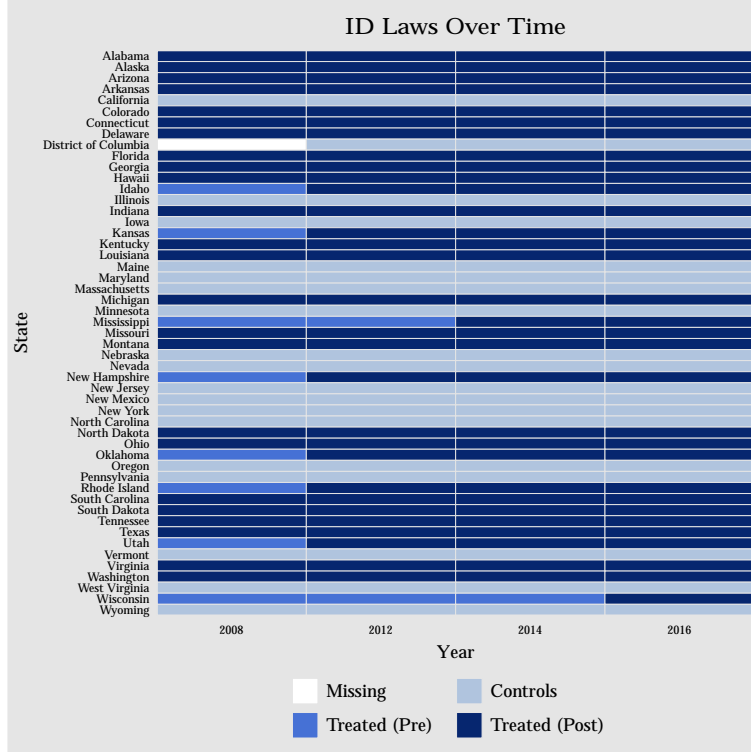
Figure 1: States with voter ID laws over time

number of waves is a significant shortcoming of this data, since voter ID laws vary at the state level, this sampling strategy is ideal for estimating the causal effects of ID laws.

To categorize voter ID laws, I use data from the National Conference of State Legislators, who provided data on states' voter ID laws from 2000-2018. The National Conference of State Legislators is a common source of data for classifying ID laws in this literature (Rocha and Matsubayashi, 2014; Highton, 2017; Biggers and Hanmer, 2017; Hajnal, Lajevardi and Nielson, 2017). Figure 1 shows what states have adopted voter ID laws during the time of my survey data. Eight states switch treatment status, mostly between the 2008 and 2012 survey waves. Therefore I concentrate my analysis on questions asked on all four waves of the SPAE survey.

regressions as to not arbitrarily up-weight certain states that got the larger sample.

3   Methodology

To assess the impact of Voter ID laws on attitudes, I use a series of two-way fixed effects models. The baseline model shown in equation 1. Y represents the answers to various questions about perceived electoral security. The β Voter ID law is a dummy variable for if a state has a voter ID law. The ξ represents state fixed effects and the λ represents year fixed effects, with the ϵ representing the error term. The i subscripts represent variation at the state level and t subscripts represent variation over time. Standard errors are clustered at the state level.

$$y_{it} = \beta \text{ Voter ID Law }_{it} + \xi_i + \lambda_t + \epsilon_{it} \tag{1}$$

For the estimate of β to be causal, trends in opinion change over time must be the same between ID adopting states and non-adopters in all respects besides what is caused by implementing the voter ID law.[10] This "parallel trends" assumption differs from previous research that uses data only from one time period. For this prior research to identify a causal effect, it must assume that states that have an ID law and states that do not would, after applying controls, have identical opinions besides the change that is caused by implementing the ID law, often referred to as "selection on observables" assumption. By switching the assumption from levels to trends, a difference in difference model is more likely to capture the causal effect of ID laws.

I make a variety of additions to the difference in difference model shown in equation 1 to probe the robustness of the parallel trends assumption. First, I add a variety of demographic control variables, including education, family income, length of residence, age, and gender. This helps assure that any incidental changes in types of people taking the survey are not driving the results. Second, I add year by party ID fixed effects, which account for national level "sore loser" effects (Sances and Stewart, 2015). If Republicans reported less confidence in electoral integrity compared to Democrats in 2012 than 2016 because their candidate for President lost in 2012 but won in 2016, this control variable would absorb that effect. Third, to capture state-level "sore loser" effects (Sances and Stewart, 2015), I control for respondents state-level presidential

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[10]I also must assume that there are no spillover effects and, given the rolling cross-section rather than a panel that re-interviews the same people over time, that migration between states is unrelated to the adoption of voter ID laws.

and senate results and interaction with partisanship. Specifically, I add dummy variables for close democratic presidential victory and loss, as well as close senate victory and loss, using a 10 percentage point margin of victory in the two-party vote share. Each term is then interacted with party identification. Forth, I add controls for partisan control of government (Democratic control, Republican control, or divided government) as well as it's interaction with partisanship, as partisan control of government is correlated with the adoption of voter ID laws (Biggers and Hanmer, 2017) and could also lead to sore loser effects. Finally, I add state by (continuous) year fixed effects that relax the parallel-trends assumption by allowing states to have different linear trends. In this specification, the ID law coefficient identifies attitude changes over and above each states' linear trend.

The voter ID treatment variable is simply a dummy variable indicating if a state has or has implemented a voter ID law for the November general election immediately preceding the SPAE survey. Not all voter ID laws are the same. In particular, research on the effects of ID laws on turnout (e.g. Grimmer et al., 2018) has focused on strict voter ID laws that require voters without ID to use a provisional ballot and take extra steps after election day to have it counted (Underhill, 2019). Other laws are less onerous and allow voters without ID to cast ballots that count without follow up action on the part of the voter. The distinctions between strict and non-strict laws and the specific types of ID allowed are important to voters who may lack the required identification to vote. Therefore, it makes sense that work on identifying turnout effects focuses on stricter ID laws.[11] However, the vast majority of voters have the required identification to vote under any type of ID law (Barreto et al., 2019). To these voters, the salient difference in their electoral experience is getting asked for ID rather than simply stating and signing their name in the poll book, not the procedures they must go through if they did not have ID or the specific types of ID required. The vast majority of voters in ID law states show a driver's license or other type of state issued ID, and less than 1% show just a document with their name and address, the type of ID that would not be allowed in all states.

One important caveat about my research design is that it only identifies the marginal effect of a state adopting a voter ID law in the current political context. Many counterfactuals outside of this political

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[11]While I discuss them comprehensively in the Appendix, this foreshadows the difference between my paper and Cantoni and Pons (Cantoni and Pons, 2019). They concentrate on strict ID laws as their primary focus is identifying turnout effects. This seemingly small difference in practice means we use almost a completely different set of state-year pairs for identification, since more states switch to having a non-strict ID law and states that do switch to a strict ID law usually have non-strict ID laws first.

context (but still important and interesting) are outside of the scope of this project. For instance, the polarized debate over voter ID laws may have caused a general decline in voter confidence across all states.

Part of only identifying effects in the current political context is that my research design identifies everything associated with adopting an ID law. The adoption of voter ID laws is usually quite contentious, with Republican politicians almost unanimously support the laws while Democrats oppose them (Biggers and Hanmer, 2017). After a law is implemented, there are public information campaigns to inform voters of the ID requirements (Hopkins et al., 2017) and mobilization pushes by Democrats (Valentino and Neuner, 2017; Neiheisel and Horner, 2019) to counter any negative turnout effects. The upshot of this process is that my estimates should not be taken solely as the effect of increasing the proportion of voters asked for identification. [12] Rather, they capture everything associated with the passage of an ID law.

4 Results

I begin by showing the effects of voter ID laws on attitudes about electoral integrity. I focus on the four questions about electoral integrity that are asked on all four waves of the SPAE in which eight different states newly adopted an ID law. The question most related to ID laws asks about the perceived frequency of voter impersonation in your community. Requiring voters to show ID presents a clear barrier to voter impersonation as instead of just needing to state a name at the polling place, you are also required to show some kind of identification. Another question asks about multiple voting in your community. People committing voter fraud via impersonation are also likely voting more than once, which makes multiple voting also related to ID laws. The last two questions asked on all the surveys ask about ballot tampering in your community and confidence that your vote was counted as intended. These questions are more about the ballot counting stage of elections rather than the casting of votes, and thus may be unaffected by ID laws. However, to the extent feelings of electoral security spill over onto other domains, these questions might be effected as well.

All questions have a four-point scale, with response options ranging from "almost never" to "very common". Higher numbers refer to more belief in fraud (very common is coded as 4, almost never as 1). Exact question

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[12]While I do estimate the effect of ID laws on the proportion of voters asked for ID (table A18), I cannot use this as a true first stage in a two-stage least-squares setup as people who were not asked for ID may also be effected by ID laws.

wordings can be found in the Appendix. The estimates can be interpreted as causal if states that adopted an ID law would of stayed on the same opinion trend as states that did not change their ID law during this period.

Table 1 shows the results for each question. Belief in the frequency of both voter impersonation and ballot tampering shows sharp decreases of .15 and .16 points, respectively. Belief in ballot tampering also appears to decreases somewhat. Finally, the belief that your vote was counted correctly does not change at all.

	(1) Multiple Voting	(2) Ballot Tampering	(3) Voter Impersonation	(4) Confidence in your vote
ID law	-0.16***	-0.09*	-0.15***	-0.00
	(0.05)	(0.03)	(0.04)	(0.03)
R^2	0.03	0.03	0.04	0.02
N	41,624	40,867	41,150	45,664

Table 1. Voter ID Law Effects

Notes: $*p < 0.05, **p < 0.01, ***p < 0.005$. All models include state and year fixed effects with standard errors clustered at the state level. Data from Survey of Elections 2008-16.

4.1 Robustness

Robustness checks for the results in 1 are shown in the Appendix, which shows five different specifications for each question. The first column for each question shows the standard difference in difference estimate with state and year fixed effects. The next column adds year by party ID fixed effects, controlling for national level sore loser effects. The third column adds close election result variables and their party ID interactions, which control for state-level sore loser effects. The fourth column adds a vector of demographic controls. Finally, the last column adds linear state time trends.

Across columns 1-4, the results are very stable in all the tables. The last column, with state-year time trends, stands out from the rest, showing somewhat stronger responses to the adoption of voter ID laws.[13] This is suggestive that states that adopted voter ID laws were on a trajectory slightly more negative than states that did not adopt voter ID laws, and that assuming trends are parallel could understate the effect of these laws. However, with only 4 years with which to estimate the time trends, these results cannot be

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[13]The exception is results for confidence your vote was counted correctly, for which null results are stable across the board.

taken as definitive.

I also combine the four questions about ballot integrity asked in each survey into an index.[14] While I believe some of these questions are unlikely to be affected by the passage of ID laws, a skeptic may view this as post hoc rationalization. The index uses all the data available and also has the benefit of increasing precision. I calculate the index using the methodology in (Kling, Liebman and Katz, 2007), adding all outcomes and subtracting the mean from states that did not adopt an ID law then dividing by the standard deviation of the same group. Results are show in appendix table A5. Across all the specifications, beliefs in electoral integrity increase roughly 25% of one standard deviation and are significant at the p < .005 level.

For completeness, I estimate models for outcomes questions available only from 2012-2016 in appendix tables A10 to A15. Since only two states (Mississippi and Wisconsin) newly adopt an ID law in this period, the results should be interpreted cautiously.[15] Nevertheless, the results are largely consistent with ID laws causing improvements in beliefs around electoral integrity.

One important drawback of my research design is the small number of states that change treatment states (8) and years (4) that I can utilize, given the SPAE only fielded 4 waves between 2008-2016. While I always cluster my standard errors at the state level, the small number of time periods and clusters can still result in overconfident estimates (Esarey and Menger, 2019). I address this by using randomization inference, which relaxes the parametric assumptions of the traditional standard errors and is the most conservative approach in the literature (Conley and Taber, 2011; Mackinnon and Webb, 2018).

The randomization inference procedure is as follows. I randomly assign states to "treatment" while preserving the same treatment structure that actually occurred. That is, six states are randomly assigned to switch to being treated in 2012, one in 2014, and one in 2016. I then run the same difference in difference model and save the results using the states randomly chosen to be treated. I then repeat this procedure (re-randomizing treatment status) 1000 times. Finally, I compare the results observed with the true treatment status to the results of the randomly assigned treatments. If the estimated coefficient is more extreme than the 2.5th or 97.5th percentile of the distribution of randomly assigned treatments, the two-tailed p-value is less than .05 and statistically significant at conventional levels.

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[14]The four questions scale together well with an α of .85.

[15]With such a short panel and number of units changing treatment status, standard errors will be biased downward despite clustering at the state level.

Results are show in figure A.1. The red bars indicate the 2.5th and 97.5th percentile, while the dotted gray line represents the coefficient estimate from the true treatment data. Both the voter impersonation and multiple voting results continue to meet conventional levels of statistical significance under this procedure. The ballot tampering result, which was the more marginal and less theoretically connected to voter ID laws, is no longer statistically significant at the $p < .05$ level.[16]

Another more recent concern with difference in difference research designs that make use of staggered treatment timing is the weighting of different treatment units as well as identification when treatment effects are heterogeneous (Athey and Imbens, 2018; Goodman-Bacon, 2018; Strezhnev, 2018). Since most of the states in my sample change treatment status between 2008 and 2012, I simply re-estimate the difference in difference model using just these "switching" states, dropping the states (Wisconsin and Mississippi) that switch treatment status later. Results are shown in table A6, and are very similar the the main estimates.

While the large number of states that switch treatment between 2008 and 2012 easily allows a robustness check around differential timing, it makes it difficult to probe for differential trends between treated and untreated states. With most states switching treatment after only one time period, there is no way to test for a preexisting opinion trend in these states before treatment. A pre-trend test with only 2 states with 4 time periods is not precise enough to be informative.

However, another way to test the plausibility of the parallel trends assumption is to see if treated states are evolving similarly to untreated states for outcomes that should be unaffected by the treatment. In this context, I can test whether states that newly adopted a voter ID law are changing their opinions about electoral reforms unrelated to voter ID differently than non-ID states. If states that adopt voter ID laws are decreasing their beliefs in fraud for reasons unrelated to ID laws, it is likely they are also changing their beliefs about election reform proposals. As validation of this, within states, higher beliefs in voter fraud are strongly negatively related to support for electoral reform. [17]

Table A7 shows the results testing how the adopting of voter ID laws causes changes in support for unrelated election reforms. I construct an index of the election reform questions asked on every wave of

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[16]I obtain similar results (voter impersonation and multiple voting robust, ballot tampering not robust) when re-running each regression dropping one treated state. This indicates that the ballot tampering result is highly dependent on the effect estimate on a few states, and removing it makes most of the effect go away.

[17]Specifically, in a regression of the voter fraud combo index on support for election reform, after partially out state and year fixed effects, a 1 unit increase in the voter fraud index is correlated with a .16 (se=.02, p value = 0.007) decease in the election reform index.

the SPAE using the (Kling, Liebman and Katz, 2007) methodology.[18] Across all specifications, adopting an ID law causes a small, statistically insignificant increase in support for election reform. This increases confidence that the changes in beliefs about voter fraud shown in table 1 are caused by ID laws,rather than heterogeneous opinion trends between states.

To further probe the robustness of the parallel trends assumption, I combine a matching estimator with the difference in differences design. Rather than comparing all states that don't change their ID law policy to states that do, I select specific comparison states that had a similar likelihood of adopting an ID law and therefore may be more likely to share a parallel trend with the treated states in absence of treatment.

The specific procedure is as follows. First, I take the mean of a set of covariates for states without ID laws in 2008.[19] I then use these covariates to create propensity scores that predict subsequent treatment(adopting an ID law) and construct matched pairs.[20] In regressions, rather than using state and year fixed effects, I use matched pair-by-year fixed effects, so identification only comes from the within pairs. Results are shown in table A8 and are very similar to the main results.

Another concern is that effects could be driven by changes in question wording between the 2008 and 2012 waves. While subtle differences in question wording can cause different response patterns, the change in response patterns would have to be different in states that adopt an ID law compared to those that don't. I further show in Appendix table A9 that the state-level correlation across years for states that do not switch treatment is not lower for questions that changed wording than questions that did, even after accounting for average changes for a specific question and average changes between years[21].

4.2 Heterogeneous effects by Party Identification

When a state passes a voter ID law, does everyone become more confident in the integrity of elections, or is the improvement limited to Republicans, the strongest supporters of these laws? Estimates from the previous section average over the opinion change of all registered voters in states that adopted ID laws. However, if

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[18]The specific questions are support for Internet voting, mail voting, auto registration at age 18, election day registration, and making election day a holiday. All questions are binary support / not support, which was the form they were asked in 2008. The 2012-2016 questions were recoded from a 4 point strongly support to strongly oppose scale.

[19]Demographic controls include length of residence, family income, age, gender, education, and party identification. State-level covariates include indicators for state government control (Democratic, Republican, or divided) and Democratic Party vote share in 2008.

[20]Specifically, nearest neighbor matching without replacement using `psmatch2` in Stata.

[21]Though this estimate is imprecise, it is actually positively signed.

opinion movement is solely concentrated among Republicans, the prospect of ID laws causing broad-based improvements in perceptions of electoral integrity would be severely constrained. It's even possible that Democrats become more skeptical of electoral integrity because of a voter ID law while Republican attitudes improve.

There are several reasons to expect heterogeneous effects by political party. At the elite level, support for ID laws is strongly polarized, with Republicans almost uniformly supporting the laws and Democrats opposing them (Hicks, McKee and Smith, 2016). Democratic politicians claim that ID laws are a pretext to decrease minority turnout and that voter fraud is virtually non-existent, while Republicans claim that voter ID laws are necessary to stop elections from being stolen. Voters often take cues about electoral reforms from elites (Bowler and Donovan, 2013), so these contrasting partisan elite messages could produce different effects by party. Options about voter ID laws have polarized over time, especially among people who follow the news closely, which is consistent with people following elite cues as the parties positions have become clearer (Stewart III, Ansolabehere and Persily, 2016). And some previous research has identified a polarization effect of ID laws on attitudes towards electoral integrity, finding that Republicans attitudes improve, Democrats attitudes worsen (Bowler and Donovan, 2016).[22]

On the other hand, there are reasons to believe ID laws would have broad-based effects. At the mass level, voter ID laws are generally popular. Even though there is a significant gap in support for ID laws between Republicans and Democrats that has grown over time (Stewart III, Ansolabehere and Persily, 2016; Gronke et al., 2019), the laws are still generally popular even among Democrats. For instance, an August 2016 Gallup poll found that 63% of Democrats and 95% of Republicans favored a photo ID requirement (McCarthy, 2016).[23] Furthermore, even if some Democrats worry that ID laws disenfranchise some voters, they may also believe it reduces the frequency of fraud.[24]

To test the difference in responses between Democrats and Republicans, I estimate models that interact partisanship and having a voter ID law. Identification comes from comparing the gap between Democrats and Republicans belief in electoral integrity in states that implement ID laws to states without a policy change

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[22]While this research is limited by using data only from one year, it also controls for the being asked for ID, which makes it hard to interpret exactly what remaining variation the ID law coefficient is identifying (Acharya, Blackwell and Sen, 2016).

[23]Figure C.2 plots the average level of ID law support among Republicans and Democrats over time in the SPAE data.

[24]I discuss the potential for ID laws to cause an increase in worries about disenfranchisement in the next section.

before and after the ID law was implemented. Results are shown in table 2. For each model, independent takes on the base term for the interaction, and all the controls (political results, year by party ID, and demographics) are included but not shown. The "P-Value Party Difference" row indicates the p-value for a test for a significant difference between the Democratic and Republican interaction coefficients.[25] Across all four response options, the point estimates imply that Republicans have a stronger attitude response (reducing their beliefs in voter fraud and other activities more). However, there is no evidence of attitude polarization (a positive effect on Republicans and a negative effect on Democrats), as the point estimates for Democrats do not come close to swamping out the effect of he main voter ID term. Finally, while the absolute value for the difference in interaction effects between Republicans and Democrats are somewhat noteworthy (e.g. .09 points for multiple voting, .08 points for voter impersonation, and .07 points for confidence in your vote) I am not able to reject the null hypothesis that the responses are equal (shown in the p values for party difference row).[26] Overall, the evidence suggests that voter ID laws are slightly more effective at improving beliefs about electoral integrity among Republicans, is is also effective at improving the beliefs of Democrats.

Table 2. Voter ID Law Effects: Party ID Interactions

	(1) Multiple Voting	(2) Ballot Tampering	(3) Voter Impersonation	(4) Confidence in your vote
ID law	-0.16*	-0.10	-0.13*	0.02
	(0.06)	(0.05)	(0.05)	(0.05)
ID Law X Dem	0.02	0.04	-0.01	0.02
	(0.05)	(0.05)	(0.05)	(0.05)
ID Law X Rep	-0.07	-0.01	-0.09	-0.05
	(0.06)	(0.06)	(0.06)	(0.03)
P Value Party Difference	0.10	0.24	0.16	0.08
R^2	0.14	0.09	0.15	0.07
N	41,325	40,552	40,834	45,311

Notes: $*p < 0.05, **p < 0.01, ***p < 0.005$. All models include state year, and party ID by year fixed effects with standard errors clustered at the state level. Independents as the ommitted party ID catagory. Data from Survey of Elections 2008-16.

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[25] The party ID coefficients include those who lean towards one of the parties, and independents refer to "pure" independents.
[26] While the point estimates imply that Democrats options are less responsive to the adoption of ID laws than independents (the omitted group for the interaction), for multiple voting and ballot tampering, a test of the joint significance of the ID law and ID law X Dem coefficients retains significance at the .05 level.

4.3   Attitudes towards ID laws

One important drawback to my results thus far is that they cannot speak to Democrats allegations that voter ID laws prevent rightfully eligible voters from voting (in particular minority voters) and help Republicans win elections. To my knowledge, no data has been collected that can directly assess whether people believe that the frequency with which rightful voters have been deterred from voting increases after a state passes a voter ID law.[27] Without this data, it's impossible to do a fully comprehensive assessment of whether voter ID laws improve Americans' perceptions of their electoral process or not.

However, I do have data on support for voter ID laws over time, which can partially address these concerns. If adopting a voter ID law causes some people to become more concerned with voter suppression, their support for voter ID laws would likely decrease.[28] I can test this by re-estimating the difference in difference models with support for voter ID laws as an outcome.[29]

Table 3 shows the results in column 1 and 2. On average, living in a state that passes a voter ID law makes people marginally more supportive of voter ID laws than states that never passed a law (though these differences are not statistically significant). This would provide some evidence that people do not worry that voter ID laws suppresses votes. However, this assumes that people base their support for voter ID laws on democratic principals. Perhaps Republicans increase their support for voter ID laws upon learning that voter ID laws suppress Democratic votes, while Democrats get less supportive, roughly canceling each-other out.[30]

To speak to this concern, I estimate models where party ID is interacted with the ID law coefficient. Strikingly, passing an ID law actually causes Democrats to become more supportive of ID laws, by a statistically significant 5-6 percentage points. If anything, it is the Republicans that become less supportive,[31] and the difference between Republicans and Democrats responses is statistically significant.

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[27]In fact, to my knowledge this has not been tested in a selection on observables context either

[28]Of course, some people believe that a voter ID law suppresses rightful voters even before personally experiencing an ID law in their state and may be unaffected by an ID law passing in their state since they were already at the bottom end of the scale. However, my data shows that voter ID laws are quite popular, and there is plenty of room for people to become less supportive of ID laws. Figure C.2 shows that republicans almost unanimously support ID laws, and despite some attitude polarization between 2008 and 2016, a majority of Democrats support the laws as well.

[29]Support for voter ID laws is asked in each SPAE wave, but in 2008 it is dichotomous while in 2012-2016 it is put on a point scale of strongly support to strongly oppose. I dichotomize the 2012-2016 data to make the scale the same for all the years of data

[30]Ceiling effects for Republicans, who have high support of ID laws even in non-ID law states, limit this possibility.

[31]Given Republicans high baseline levels of support, this may be driven by ceiling effects.

This result is hard to reconcile with a model in which Democrats worry more about voter suppression after a voter ID law is passed, as worries about voter suppression should not translate to greater support of voter ID laws. However, I must again draw attention to the fact that my research design estimates the marginal impact of passing and implementing a voter ID law on attitudes, not the existence of voter ID laws themselves. The existence of voter ID laws may have made Democrats more worried about voter suppression and less confident in the fairness of American democracy.

Table 3. Voter ID Law Effects on attitudes toward Voter ID Laws

	(1)	(2)	(3)	(4)
ID Law	0.03	0.03	0.02	0.02
	(0.02)	(0.01)	(0.02)	(0.02)
ID Law X Rep			-0.02	-0.03
			(0.02)	(0.02)
ID Law X Dem			0.03	0.04
			(0.03)	(0.02)
P-Value Party Difference			0.04	0.01
Joint P Value Democrats			0.03	0.02
Joint P Value Republicans			0.43	0.31
Demographic Controls		X		X
Party ID Results Controls		X		X
R^2	0.02	0.18	0.15	0.18
N	45,659	45,339	45,526	45,339

Notes: $*p < 0.05, **p < 0.01, ***p < 0.005$. All models include state and year fixed effects with standard errors clustered at the state level. Data from Survey of Elections 2008-16. Year-PID fixed effects interact year coefficient with the party ID coefficent. PID Results controls include indicators for close democratic presidential win and loss (at the state level) as well as close democratic senate win and loss. Each indicator is also interacted with party ID. Demographic controls include length of residence, family income, gender, age, education, race, and party ID.

4.4 Reconciling with Previous Literature

Why do my results differ from the previous literature? Regression with a single cross section and a panel with state and year fixed effects are very different specifications that can yield different results for a variety of reasons. Nevertheless, I have probed a few different reasons why my findings differ from previous research.

One potentially parsimonious explanation is that states that pass voter ID laws are different than states that do not pass ID laws even before the law was passed. Specifically, if states that pass ID laws have higher levels of fraud belief than states that no not pass ID laws even before ID laws are passed, analysis using only one year of data could mistakenly find no effect of ID laws. The effect of the ID law would just bring the

levels of fraud belief in line with other states that have not adopted ID laws.

This dynamic explains some contrasting results in the literature on voter ID and turnout. Even before an ID law was passed and after controlling for a host of other variables, ID law states have lower voter turnout than non-ID law states (Grimmer et al., 2018). In table A16 of the Appendix, I do the same kind of analysis but found that states that went on to adopt ID laws and states that have never had an ID law did not have significantly different attitudes around electoral integrity.

The explanation that better fits the data is that states that switch ID laws in the 2008-2016 period are different than states that have had an ID law since before 2008. When restricting the sample of states to "switchers," analyzing the data in the cross-section without state fixed effects yields estimates roughly comparable to my difference in difference models (see table A17 in the Appendix).

4.4.1 Why are the effects of early and late ID law adopters different

This result (late adopting states being different from early adopting states) is consistent with several different explanations. First, the effect of an ID law for states that switched in 2008-2016 may be different than states that switched earlier. This can (partially) be tested by examining the heterogeneity in ID law effects in the 2008-2016 period. One way of thinking about potential heterogeneous responses by different demographic groups, and groups that are less responsive to voter ID laws maybe over-represented in states that passed ID laws earlier. To speak to this potential, I ran regressions where the different demographic variables (as well as party ID) predicted having an ID law before 2008. There were no substantively significant differences, which helps rule out a heterogeneous effects by demographics based explanation.[32]

Another way of thinking about heterogeneity is using each state as a unit of analysis, and examine how effect sizes vary between states. It's possible my results are driven by a fraction of states with very strong effects, while other states see no improvements in attitudes. This would be suggestive that states' experiences are highly varied, and that states that adopted ID laws before 2008 may have not experienced attitude changes. To speak to this second form of heterogeneity, I re-estimate equation 1 but disaggregate

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[32]I run models comparing the demographics of states that had an ID law before 2008 with all other states and just with states that adopted an ID law between 2008 and 2016. For the comparison to all states, there are no significant differences. For the comparison to states that adopted a law between 2008 and 016, there is a 1.3% (p=.05) higher probability (that these states had residents that have lived there 2-4 years rather than less than 2 years.

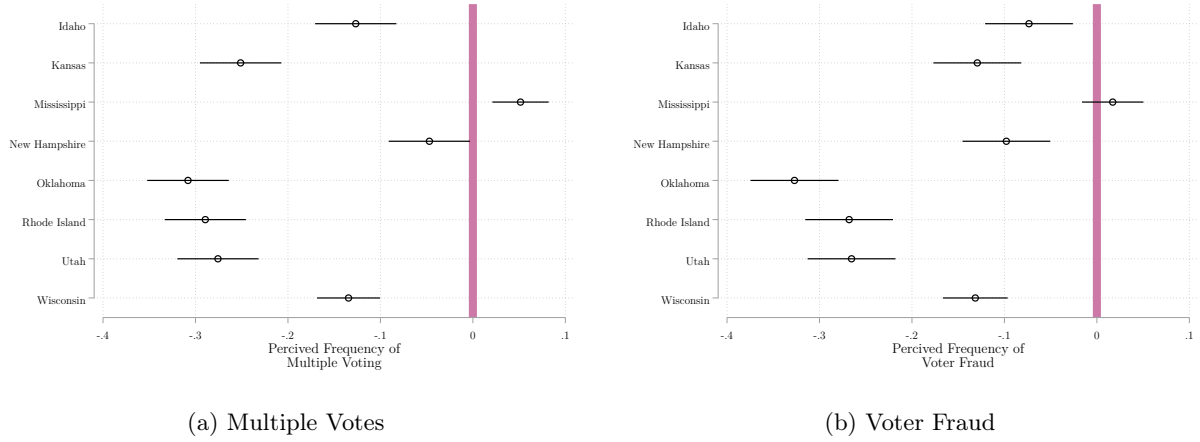(a) Multiple Votes   (b) Voter Fraud

Figure 2: Treatment effects by state

the treatment variable for each individual state that adopted a voter ID law in the 2008-2016 time period. The results are shown in 2. While the effects do differ between states, all but one are signed in a same direction of attitude improvement.[33] The results do not provide support for the possibility that states before 2008 did not show effects because effects are highly variable.

A second possibility is that states that switched before 2008 had a higher belief in fraud before the ID law was implemented, and the ID law brought them back to a baseline level comparable to other states. This would mean that states that initially passed ID laws were more selected on the basis of their worries about voter fraud, but states that passed ID laws more recently are similar to states that did not pass an ID law. It is potentially plausible that the earliest states to pass voter ID laws had the highest worries about voter fraud, but this can only be tested with more data from the pre 2008 period.

Third, it is possible that short and long-run effects differ, and that in the long run the effect of ID laws on attitudes dissipates. This is partially testable with the data at hand, as I can test if the effect of passing an ID law between the first two waves of survey data (2008 and 2012) fades out in the 2014 and 2016 waves. I drop the states that adopted ID laws after 2012 (Wisconsin and Mississippi) and run a model with each post treatment time disaggregated into its own coefficient. Results are shown in figure 3.[34] While some of the coefficients are marginally less negative in later years, there is at best very weak evidence for fade-out over time.

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[33]Even with clustering at the state level, standard errors will be underestimated with 4 periods and giving each individual state a different slope, which serves to exagerate the hetrogenity.

[34]I use the 2008 mean among states that adopted ID laws to show average opinion in 2008 (the base term in the regression) and subtract the 2012-2016 coefficient estimates to make the graph.

(a) Multiple Votes   (b) Voter Fraud
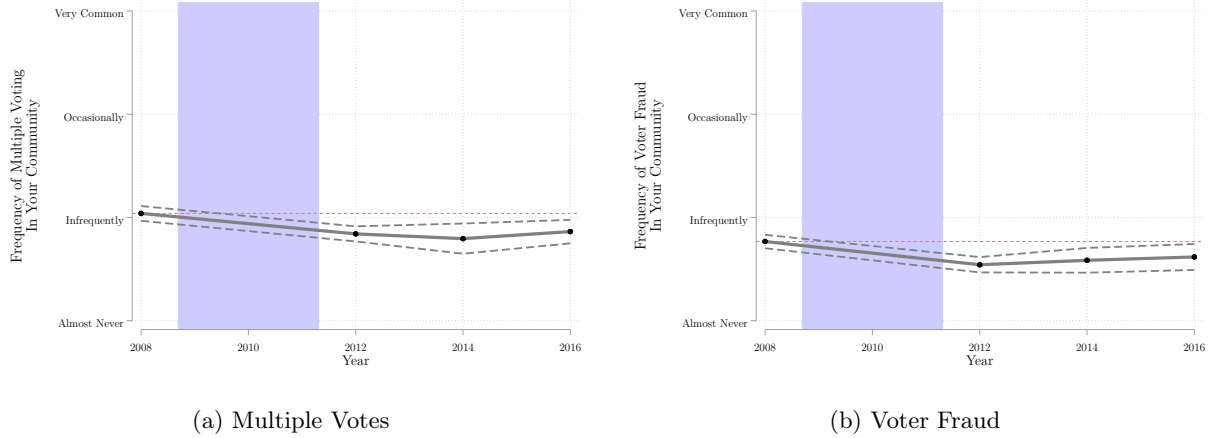
Figure 3: Treatment effects over time

Given that we have both pre and post-treatment data for the eight states that switched, we can be more confident in their responses to an ID law than for states that we only have data for after they implemented an ID law. While there are several explanations for why states that adopted an ID law earlier would not have a detectable ID law effect, my tests have provided evidence against the heterogeneity in responses or fade out over time explanations.

5 Conclusion

My results suggest that voter ID laws cause Americans to be more confident in the integrity of American elections. Specifically, registered voters believe that voter impersonation and multiple voting are more rare in their communities after their state passes a voter ID law. Unlike previous research, I compare states before and after a law passes to other states that don't have such a change with a difference and differences design, and thus can be more confident the changes I observe were due to the ID law. Past published literature that found no effect of ID laws had data on only one year, and relied on controlling for all relevant determinants of state level opinions.

This effect of ID laws is sizable (one-fifth of one standard deviation, roughly comparable to the difference in opinions between registered voters with high school diploma versus those with a 4-year college degree) and there is no evidence it is limited to Republicans. While I do not have data on belief in voter suppression, I show that living in a state that passes voter ID laws appears to make Democrats, the group most likely to

worry about voter suppression, significantly more supportive of ID laws generally. While it is not decisive, this result is hard to reconcile with a model by which implementing an ID law increases worries about voter suppression among Democrats.

These results come with several limitations. Most importantly, I only have four waves of survey data. With most states adopting ID laws between the first and second wave, I am unable to to show that states that go on to pass an ID law are on a similar trend with states that do not pass a law. This "pre-trend" test would would provide important evidence for or against the parallel trends assumption, which is needed for my results to be interpreted causally.

Second, I did not differentiate between different types of voter ID laws, some of which are more strict than others. The amount of survey data and number of law changes does not to test the effects of the different types of laws with meaningful statistical precision. Finally, the SPAE only samples registered voters, thus, I cannot speak to effects on people not registered to vote.

What are the implication of my results for policy? On one hand, the (previously unidentified) positive effect on attitudes around electoral integrity does add a new empirical justification for voter ID laws. On the other hand, there remain several troubling normative issues around the implementation of ID laws.

First, there is no evidence ID laws stop fraud (Cantoni and Pons, 2019).[35] Designing government policy to solve only perceptions of problems may be justified, but it certainly makes the case for a policy weaker. Another issue is the potential for ID law to suppress voter turnout. Current empirical research on ID laws shows little evidence for a disenfranchising effect (Highton, 2017), but the theoretical case for at least *some* voters being deterred by a strict ID law is strong. Some fraction of voters do not have the ID required by these laws. Even if the cost of obtaining an ID is low, it is certainly not zero, which in turn will lead to a non-zero deterrence effect (Decrescenzo and Mayer, 2019).

Third, my design only identifies the marginal effect of a state passing a voter ID law compared to states that do not pass a law on registered voters. The introduction of ID laws more generally, the associated focus on voter fraud to justify them, and, for at least some politicians, the true purpose of disenfranchising minority Democrats, has almost certainly caused some Americans beliefs about electoral integrity to worsen.

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[35]At the baseline, documented evidence of fraud is extremely rare (Minnite, 2010), and the fraud that does occur would not be stopped by ID laws (Gronke et al., 2008).

Voter ID laws are not the only inputs that may change Americans' perceptions of electoral integrity. An important contribution of this article is showing that government policy can cause perceptions of electoral integrity to improve. Much of the other research that has focused on the determinants of attitudes towards electoral integrity has focused on things that are not easily changed by government policy, like media coverage of voter fraud (Udani, Kimball and Fogarty, 2018), immigrant resentment (Udani and Kimball, 2018), and unfavorable election results (Sances and Stewart, 2015). And research on election administration's effect on perceptions of electoral integrity, which is more changeable, has both mixed results and requires strong assumptions to be interpreted causally (Herrnson et al., 2008; Hall, Monson and Patterson, 2009; Claassen et al., 2013; Bowler et al., 2015; King and Barnes, 2018; Udani, Kimball and Fogarty, 2018; Flavin and Shufeldt, 2019). A productive path for future research would be to examine how other policies may improve attitudes, especially policies that do not make it more difficult for some people to vote.[36]

Another productive path for future research would be to test whether strict and non-strict ID laws have similar effects. My results grouped all ID laws together with a single binary variable. I argued that different types of ID laws likely have the same attitudinal effects on voters, who are not aware of the intricate details of what types of IDs are allowed or the rules about how votes from people without IDs votes are counted, but are cognizant they were asked for ID rather than just signing their name. The upshot of this dynamic is that less strict ID laws, where the state takes steps to verify voters' identity rather than putting the burden on voters, could produce the same attitude change as strict laws without the potential disenfranchising effects. The current publicly available survey data and law changes do not allow differentiating all the different types of ID laws with meaningful statistical precision. However, future research with more data could help differentiate the effects of different kinds of ID laws.

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[36]One example is work examining perceptions of ballot secrecy. Although the U.S has had a secret ballot for over a century, 25% of Americans report not believing their ballot choices are kept secret (Gerber, Huber, Doherty and Dowling, 2013). In a field experiment that mailed information to registered voters about protections of ballot secrecy, previous non-voters turnout increased 3%, a stronger effect than traditional get-out-the-vote treatments (Gerber, Huber, Doherty, Dowling and Hill, 2013).

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Appendices

A   Robustness

<p align="center">Table A1. Voter ID Law Effects on belief in Multiple Votes</p>

	(1)	(2)	(3)	(4)	(5)
ID law	-0.17***	-0.17***	-0.18***	-0.18***	-0.26***
	(0.05)	(0.04)	(0.05)	(0.05)	(0.05)
Year-PID Fixed Effects		X	X	X	X
PID Results Controls			X	X	X
Demographic Controls				X	X
State Time Trends					X
R^2	0.03	0.12	0.12	0.14	0.14
N	41,624	41,502	41,502	41,325	41,325

Notes: $*p < 0.05, **p < 0.01, ***p < 0.005$. All models include state and year fixed effects with standard errors clustered at the state level. Data from Survey of Elections 2008-16. Year-PID fixed effects interact year coefficient with the party ID coefficent. PID Results controls include indicators for close democratic presidential win and loss (at the state level) as well as close democratic senate win and loss. They also incude indicators for partisian control of government. Each indicator is also interacted with party ID. Demographic controls include length of residence, family income, gender, age, education, race, and party ID. State time trends interacts state fixed effect with a continuous time trend for each wave of the survey.

<p align="center">Table A2. Voter ID Law Effects on belief in Ballot Tampering</p>

	(1)	(2)	(3)	(4)	(5)
ID law	-0.09**	-0.09**	-0.10*	-0.09*	-0.17***
	(0.03)	(0.03)	(0.04)	(0.04)	(0.06)
Year-PID Fixed Effects		X	X	X	X
PID Results Controls			X	X	X
Demographic Controls				X	X
State Time Trends					X
R^2	0.03	0.05	0.06	0.09	0.09
N	40,867	40,737	40,737	40,552	40,552

Notes: $*p < 0.05, **p < 0.01, ***p < 0.005$. All models include state and year fixed effects with standard errors clustered at the state level. Data from Survey of Elections 2008-16. Year-PID fixed effects interact year coefficient with the party ID coefficent. PID Results controls include indicators for close democratic presidential win and loss (at the state level) as well as close democratic senate win and loss. They also incude indicators for partisian control of government. Each indicator is also interacted with party ID. Demographic controls include length of residence, family income, gender, age, education, race, and party ID. State time trends interacts state fixed effect with a continuous time trend for each wave of the survey.

Table A3. Voter ID Law Effects on belief in Voter Impersenation

	(1)	(2)	(3)	(4)	(5)
ID law	-0.15***	-0.17***	-0.18***	-0.17***	-0.27***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Year-PID Fixed Effects		X	X	X	X
PID Results Controls			X	X	X
Demographic Controls				X	X
State Time Trends					X
R^2	0.04	0.12	0.13	0.15	0.15
N	41,150	41,023	41,023	40,834	40,834

Notes: $*p < 0.05, **p < 0.01, ***p < 0.005$. All models include state and year fixed effects with standard errors clustered at the state level. Data from Survey of Elections 2008-16. Year-PID fixed effects interact year coefficient with the party ID coefficient. PID Results controls include indicators for close democratic presidential win and loss (at the state level) as well as close democratic senate win and loss. They also incude indicators for partisian control of government. Each indicator is also interacted with party ID. Demographic controls include length of residence, family income, gender, age, education, race, and party ID. State time trends interacts state fixed effect with a continuous time trend for each wave of the survey.

Table A4. Voter ID Law Effects on belief in Confidence Personal Vote

	(1)	(2)	(3)	(4)	(5)
ID law	-0.00	0.00	0.00	0.00	-0.02
	(0.04)	(0.04)	(0.03)	(0.04)	(0.05)
Year-PID Fixed Effects		X	X	X	X
PID Results Controls			X	X	X
Demographic Controls				X	X
State Time Trends					X
R^2	0.02	0.04	0.05	0.07	0.08
N	45,664	45,498	45,498	45,311	45,311

Notes: $*p < 0.05, **p < 0.01, ***p < 0.005$. All models include state and year fixed effects with standard errors clustered at the state level. Data from Survey of Elections 2008-16. Year-PID fixed effects interact year coefficient with the party ID coefficient. PID Results controls include indicators for close democratic presidential win and loss (at the state level) as well as close democratic senate win and loss. They also incude indicators for partisian control of government. Each indicator is also interacted with party ID. Demographic controls include length of residence, family income, gender, age, education, race, and party ID. State time trends interacts state fixed effect with a continuous time trend for each wave of the survey.

Table A5. Voter ID Law Effects on belief in Fraud Index

	(1)	(2)	(3)	(4)	(5)
ID law	-0.13***	-0.13***	-0.14***	-0.13**	-0.24***
	(0.04)	(0.04)	(0.05)	(0.05)	(0.04)
Year-PID Fixed Effects		X	X	X	X
PID Results Controls		X	X	X	X
Demographic Controls			X	X	X
State Time Trends					X
R^2	0.05	0.13	0.14	0.16	0.17
N	32,993	32,900	32,900	32,773	32,773

Notes: $*p < 0.05, **p < 0.01, ***p < 0.005$. All models include state and year fixed effects with standard errors clustered at the state level. Data from Survey of Elections 2008-16. Year-PID fixed effects interact year coefficient with the party ID coefficent. PID Results controls include indicators for close democratic presidential win and loss (at the state level) as well as close democratic senate win and loss. They also incude indicators for partisian control of government. Each indicator is also interacted with party ID. Demographic controls include length of residence, family income, gender, age, education, race, and party ID. State time trends interacts state fixed effect with a continuous time trend for each wave of the survey.

Table A6. No Differential Timing

	(1) Multiple Voting	(2) Ballot Tampering	(3) Voter Impersonation	(4) Confidence in your vote
ID law	-0.21***	-0.10*	-0.19***	-0.01
	(0.05)	(0.04)	(0.05)	(0.04)
R^2	0.03	0.03	0.04	0.02
N	40,174	39,442	39,723	44,072

Notes: $*p < 0.05, **p < 0.01, ***p < 0.005$. All models contain state and year fixed effects and drop Mississippi and Wisconsin from the sample, who switch treatment outside of the 2008-2012 window.

Table A7. Voter ID Law Effects on Election Reform Index

	(1)	(2)	(3)	(4)	(5)
ID Law	0.02	0.04	0.04	0.05	0.02
	(0.04)	(0.03)	(0.03)	(0.03)	(0.02)
Year-PID Fixed Effects		X	X	X	X
PID Results Controls			X	X	X
Demographic Controls				X	X
State Time Trends					X
R^2	0.04	0.23	0.23	0.27	0.28
N	50,940	50,807	50,807	50,623	50,623

Notes: $*p < 0.05, **p < 0.01, ***p < 0.005$. All models include state and year fixed effects with standard errors clustered at the state level. Data from Survey of Elections 2008-16. Year-PID fixed effects interact year coefficient with the party ID coefficent. PID Results controls include indicators for close democratic presidential win and loss (at the state level) as well as close democratic senate win and loss. They also incude indicators for partisian control of government. Each indicator is also interacted with party ID. Demographic controls include length of residence, family income, gender, age, education, race, and party ID. State time trends interacts state fixed effect with a continuous time trend for each wave of the survey.

Table A8. Voter ID Law Effects: Robustness to Matching

	(1) Multiple Voting	(2) Ballot Tampering	(3) Voter Impersonation	(4) Confidence in your vote
ID law	-0.14*	-0.09	-0.16*	-0.02
	(0.05)	(0.06)	(0.06)	(0.02)
R^2	0.02	0.02	0.02	0.02
N	12,028	11,792	11,877	13,352

Notes: $*p < 0.05, **p < 0.01, ***p < 0.005$. All models incude fixed effects for matched pairs of states unique for each year. Standard errors are clustered at the state level.

Table A9. Robustness to Question Wording Changes

	(1)	(2)
Question wording change	0.14	0.10
	(0.15)	(0.10)
Year Fixed Effects		X
Question Fixed Effects		X
R^2	0.08	0.89
N	12	12

Notes: $*p < 0.05, **p < 0.01, ***p < 0.005$. Data comes from correlations in responses to questions at the state-year level. Year fixed effects control for the average level of correlation of outcomes across all questions between particular years, question fixed effects control for the average level of correlation for a specific question across all years.

Table A10. Voter ID Law Effects on county vote confidence

	(1)	(2)	(3)	(4)	(5)
ID law	0.07*	0.08	0.08	0.08	0.01
	(0.03)	(0.04)	(0.04)	(0.05)	(0.24)
Year-PID Fixed Effects		X	X	X	X
PID Results Controls			X	X	X
Demographic Controls				X	X
State Time Trends					X
R^2	0.02	0.05	0.06	0.08	0.09
N	38,838	38,795	38,795	38,668	38,668

Notes: $*p < 0.05, **p < 0.01, ***p < 0.005$. All models include state and year fixed effects with standard errors clustered at the state level. Data from Survey of Elections 2008-16. Year-PID fixed effects interact year coefficient with the party ID coefficent. PID Results controls include indicators for close democratic presidential win and loss (at the state level) as well as close democratic senate win and loss. Each indicator is also interacted with party ID. Demographic controls include length of residence, family income, gender, age, education, race, and party ID. State time trends interacts state fixed effect with a continuous time trend for each wave of the survey.

Table A11. Voter ID Law Effects on state vote confidence

	(1)	(2)	(3)	(4)	(5)
ID law	-0.04	-0.05	-0.05	-0.05	-0.06
	(0.02)	(0.03)	(0.04)	(0.05)	(0.16)
Year-PID Fixed Effects		X	X	X	X
PID Results Controls			X	X	X
Demographic Controls				X	X
State Time Trends					X
R^2	0.04	0.06	0.09	0.10	0.11
N	38,804	38,760	38,760	38,631	38,631

Notes: $*p < 0.05, **p < 0.01, ***p < 0.005$. All models include state and year fixed effects with standard errors clustered at the state level. Data from Survey of Elections 2008-16. Year-PID fixed effects interact year coefficient with the party ID coefficent. PID Results controls include indicators for close democratic presidential win and loss (at the state level) as well as close democratic senate win and loss. Each indicator is also interacted with party ID. Demographic controls include length of residence, family income, gender, age, education, race, and party ID. State time trends interacts state fixed effect with a continuous time trend for each wave of the survey.

Table A12. Voter ID Law Effects on national vote confidence

	(1)	(2)	(3)	(4)	(5)
ID law	-0.12	-0.13	-0.14*	-0.14*	-0.27*
	(0.10)	(0.08)	(0.07)	(0.06)	(0.11)
Year-PID Fixed Effects		X	X	X	X
PID Results Controls			X	X	X
Demographic Controls				X	X
State Time Trends					X
R^2	0.02	0.09	0.09	0.10	0.11
N	38,540	38,496	38,496	38,369	38,369

Notes: $*p < 0.05, **p < 0.01, ***p < 0.005$. All models include state and year fixed effects with standard errors clustered at the state level. Data from Survey of Elections 2008-16. Year-PID fixed effects interact year coefficient with the party ID coefficent. PID Results controls include indicators for close democratic presidential win and loss (at the state level) as well as close democratic senate win and loss. Each indicator is also interacted with party ID. Demographic controls include length of residence, family income, gender, age, education, race, and party ID. State time trends interacts state fixed effect with a continuous time trend for each wave of the survey.

Table A13. Voter ID Law Effects on belief in non citizen voting

	(1)	(2)	(3)	(4)	(5)
ID law	-0.25***	-0.29***	-0.28***	-0.27***	-0.50***
	(0.03)	(0.02)	(0.02)	(0.03)	(0.11)
Year-PID Fixed Effects		X	X	X	X
PID Results Controls			X	X	X
Demographic Controls				X	X
State Time Trends					X
R^2	0.03	0.18	0.18	0.20	0.20
N	31,648	31,612	31,612	31,514	31,514

Notes: $*p < 0.05, **p < 0.01, ***p < 0.005$. All models include state and year fixed effects with standard errors clustered at the state level. Data from Survey of Elections 2008-16. Year-PID fixed effects interact year coefficient with the party ID coefficent. PID Results controls include indicators for close democratic presidential win and loss (at the state level) as well as close democratic senate win and loss. Each indicator is also interacted with party ID. Demographic controls include length of residence, family income, gender, age, education, race, and party ID. State time trends interacts state fixed effect with a continuous time trend for each wave of the survey.

Table A14. Voter ID Law Effects on belief in abentee ballot fraud

	(1)	(2)	(3)	(4)	(5)
ID law	-0.14***	-0.14***	-0.15***	-0.15***	-0.24
	(0.01)	(0.02)	(0.02)	(0.02)	(0.16)
Year-PID Fixed Effects		X	X	X	X
PID Results Controls			X	X	X
Demographic Controls				X	X
State Time Trends					X
R^2	0.03	0.11	0.11	0.13	0.14
N	30,215	30,181	30,181	30,084	30,084

Notes: $*p < 0.05, **p < 0.01, ***p < 0.005$. All models include state and year fixed effects with standard errors clustered at the state level. Data from Survey of Elections 2008-16. Year-PID fixed effects interact year coefficient with the party ID coefficent. PID Results controls include indicators for close democratic presidential win and loss (at the state level) as well as close democratic senate win and loss. Each indicator is also interacted with party ID. Demographic controls include length of residence, family income, gender, age, education, race, and party ID. State time trends interacts state fixed effect with a continuous time trend for each wave of the survey.

Table A15. Voter ID Law Effects on belief in elected officals fraud

	(1)	(2)	(3)	(4)	(5)
ID law	-0.13	-0.12	-0.13	-0.14**	-0.44***
	(0.08)	(0.07)	(0.07)	(0.05)	(0.13)
Year-PID Fixed Effects		X	X	X	X
PID Results Controls			X	X	X
Demographic Controls				X	X
State Time Trends					X
R^2	0.03	0.04	0.05	0.10	0.10
N	30,844	30,808	30,808	30,709	30,709

Notes: $*p < 0.05, **p < 0.01, ***p < 0.005$. All models include state and year fixed effects with standard errors clustered at the state level. Data from Survey of Elections 2008-16. Year-PID fixed effects interact year coefficient with the party ID coefficent. PID Results controls include indicators for close democratic presidential win and loss (at the state level) as well as close democratic senate win and loss. Each indicator is also interacted with party ID. Demographic controls include length of residence, family income, gender, age, education, race, and party ID. State time trends interacts state fixed effect with a continuous time trend for each wave of the survey.
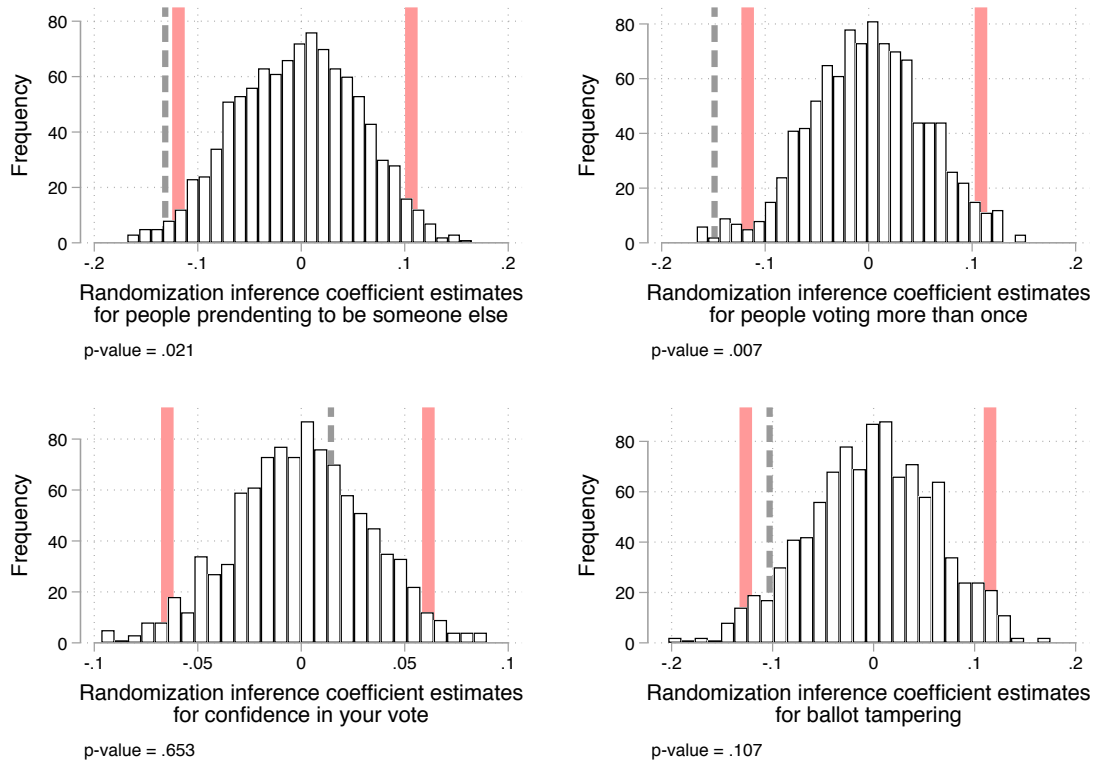
Figure A.1: Histograms show the distribution of randomization inference coefficients. The grey dotted line represents the coefficient estimated on the real data. Red lines represent the 2.5th and 97.5th percentile of observed randomization inference coefficients.

B   Reconciling Results with Previous Literature

Table A16. Voter ID Law Effects: Placebo Test

	(1) Multiple Voting	(2) Ballot Tampering	(3) Voter Impersonation	(4) Confidence in your vote
Pre-ID-Law	0.03	0.01	-0.00	-0.03
	(0.06)	(0.06)	(0.07)	(0.03)
R^2	0.12	0.06	0.12	0.05
N	17,369	17,066	17,114	19,157

Notes: $*p < 0.05, **p < 0.01, ***p < 0.005$. States-years that do have ID laws are excluded. All regressions include controls for length of residence, family income, age, gender, education, party ID, and year. Data from Survey of Elections 2008-16.

Table A17. Voter ID Law Effects: Only States That Change Treatment

	(1) Multiple Voting	(2) Ballot Tampering	(3) Voter Impersonation	(4) Confidence in your vote
ID Law	-0.16*	-0.13	-0.18*	-0.03
	(0.07)	(0.07)	(0.07)	(0.03)
R^2	0.13	0.06	0.13	0.04
N	20,684	20,346	20,419	22,846

Notes: $*p < 0.05, **p < 0.01, ***p < 0.005$. States that always have ID laws are excluded. All regressions include controls for length of residence, family income, age, gender, education, party ID, and year. Data from Survey of Elections 2008-16.
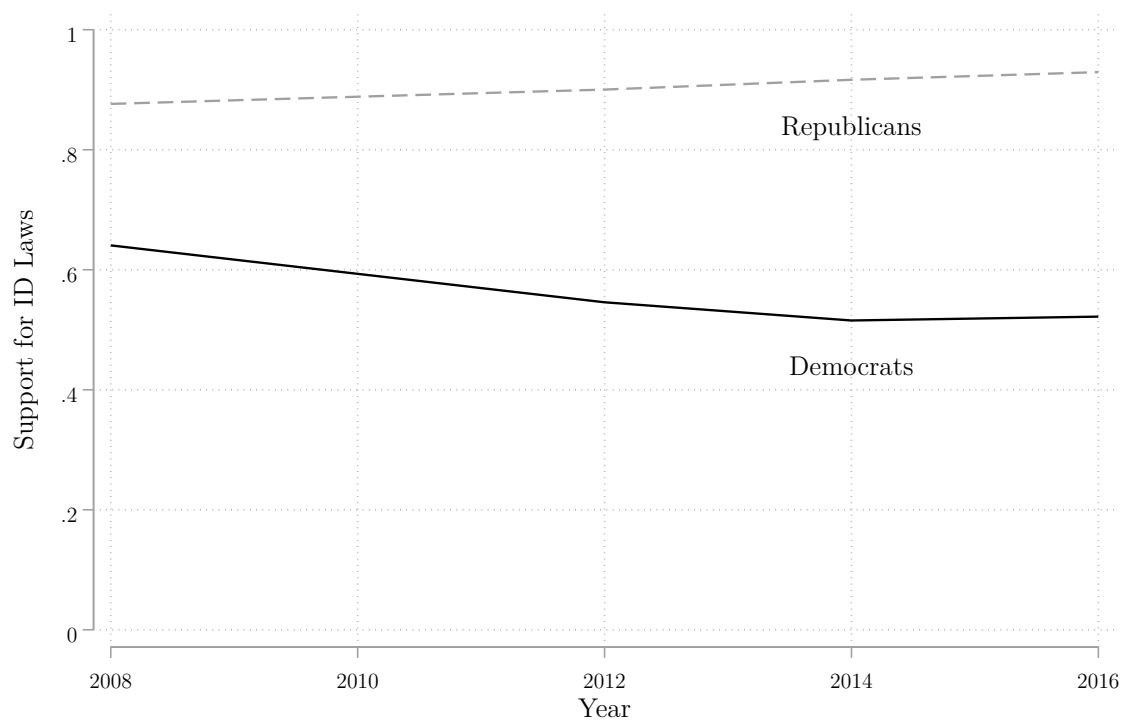
C   Opinion on Voter ID Laws Over Time

Figure C.2: Support for Voter ID Laws by party (grouping leaners) over time. Scale from 2012-2016 converted to binary support not support from four point strongly support, somewhat support, etc.

D Comparison to Cantoni and Pons 2019

The largest difference between my preferred specification and the specification of Cantoni and Pons is the definition of treatment.[37] I define treatment as any type of ID law, while their article considers only states with a strict ID law to be treated. In the Appendix to their article, they consider an alternative definition of strict photo ID laws only, which only compounds the issues discussed below.

Defining treatment as strict ID laws only causes several issues. First, it inappropriately uses states that switch from no ID law to a non-strict ID law as control units. If states that adopt a non-strict ID law have any effect on attitudes, this would violate the parallel trends assumption and bias effects towards the null hypothesis. Second, it inappropriately assigns treatment to states that switch from a non-strict ID law to a strict ID law. If states that switch from a non-strict to strict ID law have a smaller effect (or no effect) on attitudes compared to states that switch from no ID law to a strict ID law, this specification will also bias their results towards the null hypothesis.

If these issues only applied to one or two states, it would be unlikely to be consequential. But in practice, the number of affected states is quite substantial, and it drives the differences between our results. Specifically, Cantoni and Pons have seven states that switch treatment status in the SPAE that identification is based on. Four of those states had a non-strict ID law before switching to a strict-ID law (biasing towards a null hypothesis), so only 3 of the treated states in Cantoni and Pons are "treated" in my specifications. Moreover, five control group states in the Cantoni and Pons specification switched from having no law to having a non-strict ID law (also biasing towards the null hypothesis), which I count as treated.

Theoretical justification for my treatment definition can be found in the methodology section of the main text. Put briefly, voters are unlikely to know the exact rules surrounding different types of ID laws, but the difference between getting asked for ID or not likely is very salient. Here, I provide statistical justification for my choice of treatment by showing evidence of the laws "first-stage" effect on the probability in-person voters are asked to show ID under different definitions of treatment in table A18. Going from a no ID law to a non-strict ID law has a large effect on voter's probability of being asked for ID. Because the Cantoni and Pons treatment group include states that go from non-strict to strict laws (people should be asked for ID equally in these two types of states) and do not account for states that switch from no law to strict laws, their specification results in a much weaker first stage.

A potential problem with my specification is that I do not account for states that switched between different types of ID laws (they are always counted as treated, with no differentiation when switching from strict to non-strict laws). In table A19, I flexibly control for these changes (with separate coefficients for

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[37]They choose to dichotomize the outcome variable while I keep the full scale and have a slightly different setup of controls and weights, but these choices make little difference. They otherwise use the same data and research design.

each unique change) rather than grouping them all under the same binary "ID law" label. The results are largely unchanged.

Table A18. Effect of different ID law definitions on the probability asked for ID

	(1)	(2)	(3)
Any ID law indicator	0.62***		
	(0.04)		
Strict ID law indicator		0.31**	
		(0.11)	
Strict Photo ID law indicator			0.31**
			(0.11)
R^2	0.46	0.42	0.42
N	35,119	35,119	35,119

Notes: $*p < 0.05, **p < 0.01, ***p < 0.005$. All models include state and year fixed effects with standard errors clustered at the state level. Data from Survey of Elections 2008-16.

Table A19. Robustness to non-binary ID law changes

	(1) Multiple Voting	(2) Ballot Tampering	(3) Voter Impersonation	(4) Confidence in your vote
ID law	-0.17***	-0.09**	-0.15***	-0.00
	(0.05)	(0.03)	(0.04)	(0.03)
R^2	0.03	0.03	0.04	0.02
N	41,624	40,867	41,150	45,664

Notes: $*p < 0.05, **p < 0.01, ***p < 0.005$. All models include state and year fixed effects with standard errors clustered at the state level. Data from Survey of Elections 2008-16. All models include unique coefficnets (not shown) for each non-binary ID law change, for instance, changing from non-strict non-photo law to a non-strict photo law.

E   Survey Questions

Voter Confidence

- 2008-2016: How confident are you that your vote in the General Election was counted as you intended?

- 2012-2016: Think about vote counting throughout your county or city, and not just your own personal situation. How confident are you that votes in your county or city were counted as voters intended?

- 2012-2016: Now, think about vote counting throughout (your state here). How confident are you that votes in (your state here) were counted as voters intended?

- 2012-2016: Finally, think about vote counting throughout the country. How confident are you that votes **nationwide** were counted as voters intended?

Illegal voting

- 2008: It is illegal to vote more than once in an election or to vote if not a U.S. citizen. How frequently do you think this occurs in your community?

- 2012-2016: It is illegal to vote more than once in an election. How frequently do you think this occurs in your community?

Ballot Tampering

- 2008: Another form of fraud occurs when votes are stolen or tampered with. How frequently do you think this occurs in your community?

- 2012-2016: People stealing or tampering with ballots that have been voted

Voter Fraud

- 2008: It is illegal for a person to claim to be another person, who is registered to vote, and to cast that person's vote. How often do you think this occurs in your community?

- 2012-2016: People pretending to be someone else when going to vote

Multiple Voting

- 2012-2016: People voting more than once in an election

Absentee fraud

- 2012-2016: People voting an absentee ballot intended for another person

Elected Officials Fraud

- 2012-2016: Officials changing the reported vote count in a way that is not a true reflection of the ballots that were actually counted

Electoral fairness ANES

- 2004: Officials changing the reported vote count in a way that is not a true reflection of the ballots that were actually counted

- 2012, 2016: Officials changing the reported vote count in a way that is not a true reflection of the ballots that were actually counted