

Does Voter ID Improve Public Confidence in the Integrity of Elections?

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Abstract:

State voter identification laws involve a potential tradeoff between the ease of voting and public perceptions of the integrity of elections. While several prior studies have examined the effects of voter ID laws on turnout, relatively few have investigated the effects of such laws on public confidence in the integrity of elections. These earlier efforts do not support the hypothesis that voter ID laws significantly improve common proxies for confidence in the integrity of elections. This study introduces a new survey instrument that directly measures confidence in the integrity of elections and employs this measure to estimate the within-state effects of changes in strict voter ID laws on confidence in the integrity of elections. In contrast to the findings of previous studies, strict voter ID requirements are shown to have a substantively large and statistically significant impact on the probability that citizens express confidence (and especially high confidence) in the integrity of elections.

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1. Introduction

The policy debate over voter identification requirements has long recognized a potential tradeoff between the ease of voting and public confidence in the integrity of elections (e.g., Ansolabehere and Persily 2008). In its 2008 *Crawford* decision,¹ the Supreme Court cemented the importance of weighing any burdens created by identification requirements against not only deterrence of voter fraud, but also the salutary effects of voter identification laws on public perceptions of the electoral process (Ansolabehere 2009).² Emanating from the wake of *Crawford* have been repeated waves of voter identification reforms and voting rights litigation in the states, as well as numerous scholarly studies of state voter identification requirements.³ Yet despite the Court elevating perceptions of the integrity of elections, systematic and direct empirical evidence that state voter identification (ID) laws improve public confidence in the electoral process has proved elusive (Ansolabehere and Persily 2008; Bowler and Donovan 2016; Stewart et al. 2016; Cantoni and Pons 2021).

This is something of a puzzle, as opinion surveys consistently find that voter identification laws garner majority support, although this support tends to be polarized with Republicans viewing voter ID laws most favorably (e.g., Alvarez et al. 2011; Atkeson et al 2014; Stewart et al 2016). Moreover, survey evidence also suggests that the public believes that identification requirements reduce illegal voting. For example, Stewart et al. (2016) report that over 70% of survey respondents agree that voter ID laws reduce voter fraud; and Coll (2024) finds that knowledge of election security institutions reduces perceptions of voter fraud and increases confidence in elections (also, see Endres and Panagopoulos 2021). Recent evidence from the 2022 Survey of the Performance of American Elections finds 36% of respondents believe that voter impersonation is occasional or very common, and that state voter ID requirements remain among the most popular election administration reforms (Stewart 2023). Taken together, these findings provide a strong empirical foundation for the conceptual link from voter identification requirements to confidence in the integrity of elections.

And yet, extant studies do not find much support for a *direct* empirical link between the presence of state ID laws and public confidence in the electoral process. This may be because measures of voter confidence are in general noisy and influenced by partisanship (Gronke 2014; Beaulieu 2014; Sances and Stewart 2015); or because commonly employed

¹ *Crawford v. Marion County Election Board*, [553 U.S.](#) 181 (2008).

² Justice Stevens writes in the majority opinion of *Crawford*: “Public confidence in the integrity of the electoral process has independent significance, because it encourages citizen participation in the electoral process” (quoted in Gronke 2014).

³ See Hasen (2022) on the increase in election litigation in the U.S. over the last 25 years.

proxies for confidence in elections miss the mark. The concept of election integrity is typically defined in broad terms, such as the existence of “free and fair” elections that is a “foundational component of political freedom” and a necessary condition for effective democracy.⁴ This definition implicates both access to voting and the security of election administration, so public confidence in the integrity elections serves as a barometer for the health of democracy. But prior empirical studies have mostly focused on public confidence in the accuracy of vote counts, which may be an overly narrow proxy for election integrity.⁵ Moreover, some studies have not controlled for the effects of partisanship on perceptions of election integrity (e.g., Cantoni and Pons 2021). Consequently, noisy or narrow proxies for confidence in the integrity of elections alone may be the reason for the puzzling absence of evidence.

But the absence of evidence for a direct connection between state voter ID laws and public confidence in elections may also be attributable to the fact that the general public is not well-informed about the details of state voter ID laws (Stewart et al. 2016, Henninger et al. 2021, and Coll 2024). For example, just half of respondents in the Cooperative Election Study can correctly answer whether their state has a photo ID requirement for voting (see Table A2). Public ignorance about voter identification requirements may then attenuate the observable impact of state voter ID laws on public perceptions in general, including confidence in elections.

Yet another potential source of confounding may occur if states that adopt voter ID requirements have lower public confidence in election integrity prior to instituting such policy changes. However, to date, only Cantoni and Pons (2021) examine within-state changes in voter ID on confidence in elections. These authors use a difference-in-difference model to analyze the effects of strict ID laws on several proxies for confidence in elections (including accuracy of vote-counting) from the Survey of the Performance of American Elections (SPAЕ) in 2008 and 2012-2016, with some proxy measures only available beginning in 2012.

Overall, Cantoni and Pons (2021) report point estimates that are consistent with small improvements in perceptions of election integrity, but these estimates are statistically insignificant. This is perhaps not surprising given the spotty time coverage for their

⁴ Per the Freedom House webpage on Election Integrity ([www. https://freedomhouse.org/issues/election-integrity](https://freedomhouse.org/issues/election-integrity); last viewed November 1st, 2024).

⁵ Gronke (2014) reviews the recent history of popular and scholarly usage of public confidence in elections, noting both the increased frequency of references to this concept after the controversial presidential election in 2000, and the tendency of scholars to focus on the accuracy of vote counting as a convenient proxy for the integrity of elections (also, see Alvarez et al. 2008; Atkeson and Saunders 2007; Atkeson et al 2015; and Carter et al 2024).

dependent variables, but these authors also do not include controls for partisan influences on public perceptions in their study.⁶ This is potentially an important omission, since it is well-established that many individuals view politics through a partisan lens, so that public opinion on trust in government, approval of officeholders, confidence in elections, and the like are largely determined by party preferences (e.g., Sances and Stewart 2015; Richardson and Milyo 2016; VanderMolen and Milyo 2016; Primo and Milyo 2020). Moreover, voter turnout and not confidence in election integrity is the focus of the analysis in Cantoni and Pons (2021), so they do not explore whether voter ID laws have differential effects on perceptions of election integrity by race/ethnicity or political party.

This study introduces a novel survey-based measure of election integrity for the purpose of analyzing the effects of state voter identification laws on public perceptions of the integrity of elections. Since 2008, the Missouri team module of the Cooperative Election Study (CES) has directly queried respondents regarding confidence in the honesty and integrity of their state elections. These data are analyzed via mixed-level difference-in-differences regressions to identify the treatment effect of state voter identification laws on respondents' confidence in the integrity of elections, controlling for other relevant factors including partisan preferences, as well as investigating possible differential effects by race/ethnicity or political party.

This exercise provides the first direct and systematic evidence that state voter identification requirements significantly improve public confidence in the integrity of elections. In addition, corroborating evidence is offered in the form of both placebo tests and by demonstrating that state voter ID implementation increases public awareness of voter ID requirements. The findings of these analyses also indicate that while the influence of partisanship is an important determinant of confidence in elections, the omission of this factor alone does not greatly impact the estimated impact of strict ID laws. Rather it appears that employing a direct measure of confidence in the integrity of elections from the CES and examining a longer time-period than Cantoni and Pons (2021) is what matters more for the novel support for strict voter ID requirements presented here. Given this, an important caveat is in order regarding the size of the CES team survey modules and hence the limited number of survey respondents in treated states (see below).

2. Voter ID and the Integrity of Elections

The public debate over voter ID has been highly contentious and polarized, so much so that even interchanges among public intellectuals and scholars are not always genteel.

⁶ In contrast, Ferroni and Milyo (2025) examine additional years of the SPAE and find that higher “costs-of-voting” in state elections significantly reduce perceptions of illegal voting and vote fraud (although they also observe no significant effect on vote confidence in vote counting).

Proponents of ID requirements argue that such laws are common-sense precautions against voter fraud that impose little real burden on voters (e.g. Fund and Von Spakovsky (2012)). Opponents of voter ID argue that the frequency of voter fraud is greatly overstated and that ID requirements unduly burden the right to vote, especially for disadvantaged minorities (e.g., Hasen 2012). Given these diametrically opposed views, it has proven a short step to impugning the motives of opposing advocates as either condoning election fraud or harboring racist intentions. And unfortunately, the social science literature on voter ID has not offered much in the way of definitive evidence for draining the fever swamp that has mired the policy debate over voter identification.

For example, there has been a great deal of scholarly attention to the effects of ID requirements on voter turnout and disparities in voter turnout. But in addition to mixed findings, investigations of this ilk have struggled with methodological challenges (e.g., see the discussions in Erickson and Minnite 2009; Highton 2017; Valentino and Neuner 2017; Hajnal et al 2017; Grimmer et al. 2018; and Pryor et al 2019). However, in contrast to earlier studies, Cantoni and Pons (2021) conduct the most comprehensive and methodologically sophisticated study of voter ID laws to date. These authors access administrative data from state voter files from 2008-2018 to examine 1.6 billion individual-level turnout decisions using more appropriate statistical tools for causal inference. Cantoni and Pons (2021) report “no negative effect on registration or turnout, overall or for any group defined by race, gender, age, or party affiliation.”

And while surely not the last word on the impact of voter ID on turnout, even these null findings are open to a variety of interpretations. It may be the case that anger over ID requirements motivates some citizens to vote despite an increased burden (Valentino and Neuner 2017; and Endres and Panagopoulos 2023), or voter ID requirements may prompt counter-mobilization efforts on the part of candidates, parties, or voting rights advocacy groups (Cantoni and Pons 2021; and Raze 2022). On the other hand, voter ID requirements may both prevent illegal votes from being cast and counted (reducing observed turnout) and improve public faith in elections resulting in greater voter participation.⁷ But in either scenario, the net effect of voter ID on turnout is ambiguous, albeit via pathways that would seem to have very different implications for voter confidence in the integrity of the electoral process.

For the most part, scholars have given hypotheses suggesting salutary effects of voter ID short shrift. This is in part because evidence of widespread voter fraud is notoriously

⁷ For example, field experiments show that information about voter ID laws can increase voter turnout (Citrin et al 2014; and Endres and Panagopoulos 2021); this is consistent with Justice Stevens’ hypothesis in *Crawford* quoted in fn. 2.

sparse (e.g., Minnite 2010; Hood and Gillespie 2012; Christensen and Schultz 2014; and Ahlquist et al 2014). Of course, the dearth of such evidence by itself doesn't mean that no illegal voting is occurring, since fraud of this is extremely difficult to observe (United States Government Accountability Office 2014). However, it does preclude any large-*n* statistical analysis of the treatment effect of voter ID requirements on the incidence of illegal voting. Of course, even absent systematic evidence that state ID laws prevent illegal voting, it is possible that such laws improve public perceptions of the integrity of elections.

As noted above, only a few studies attempt to directly assess whether state ID requirements improve perceptions of the integrity of elections, and none of these find much support for this hypothesis (Ansolabehere and Persily 2008; Stewart et al 2016; and Bowler and Donovan 2016; and Cantoni and Pons 2021). However, all these studies employ narrow “vote counting” measures as a proxy for the integrity of elections; and most examine only cross-sectional evidence which cannot identify the treatment effect of state ID laws. For example, if public perceptions of the integrity of elections are lower in states that eventually adopt voter ID reforms, then it is quite possible that the cross-sectional correlation between voter ID and election integrity will not reflect the true causal relationship. For this reason, it is necessary to examine the impact of changes in state laws on changes in perceptions of election integrity.

Only Cantoni and Pons (2021) conduct an evaluation study of state voter ID laws on election integrity by examining the difference-in-differences over time and across states, but they also do not find any statistically significant impact of strict ID laws on several proxies for public confidence in the integrity of elections. However, as noted above, these authors examine SPAE data for only a limited time period, do not control for the political party of respondents (or interactions of party and year to control for “winner effects”), and do not explore the potential differential impacts of strict voter ID on the perceptions of focal sub-groups of respondents (e.g. See Fraga and Miller 2022). The analysis below addresses each of these concerns.

3. Data and Methods

This study employs individual-level survey data from the Cooperative Election Study (CES) for even numbered years from 2008-2022. The CES is comprised of a “common content” module administered to all respondents and multiple “team” modules administered to subsamples of 1,000 respondents. The CES is designed to be nationally representative in both the common content and the team modules.⁸ In even years, the CES includes both a pre-election wave administered in September/October, and a post-election wave

⁸ All analyses reported in this study employ survey weights; however, the 2008 CES did not include team-specific weights, so general survey weights are used for observations in that year.

administered to the same participants shortly after the November elections (albeit with some drop-off).

The University of Missouri team module of the CES has included the following post-election wave question: “How confident are you in the honesty and integrity of elections in your state?”⁹ Responses were recorded on a seven-point scale ranging from “No Confidence” to “Great Confidence”; half of the responses are in the top three categories (“high confidence”) and about a quarter are in the bottom three response categories (“low confidence”).¹⁰

Figure 1 describes the percentage of respondents with high or low confidence in their state elections over time (the middle category is omitted). Given the alarmist rhetoric often heard in popular discussions of the state of democracy in America, it is perhaps surprising that public perceptions of the integrity of state elections have improved slightly in recent years (there is a significant trend of +1.3% per election year in the percent responding with high confidence ($p < .01$)). This improvement in public confidence in state elections is also observed for adherents of both major parties, albeit more so for Democrats (see Figure 2). Expressions of high confidence increase by 1.5% per election year for Democrats, versus half that for Republicans ($p < .01$). Figure 2 also indicates a clear partisan “pulse” to changes in confidence levels, with Democrats responding more positively in election years that were favorable for Democrats, and Republicans responding more positively in years that were more favorable for Republicans. The notable exception is 2016, when confidence in the integrity of elections improved similarly for Democrats and Republicans coincident with the improbable victory of Donald Trump over Hillary Clinton, and despite prominent claims of “rigged” elections and foreign election interference. These patterns underscore the potential importance of controlling not just time effects, but differential time effects by party when analyzing public perceptions about the integrity of elections.

Challenges to Analyzing Voter ID: Definitions, Timing, and Sample Sizes

The National Conference of State Legislatures (NCSL) classifies state voter identification requirements as either “strict” or “non-strict”. In states with “strict ID” laws voters must provide documentary evidence of their identity as a necessary condition for casting a valid ballot (including provisional ballots). In contrast, some states do not request any external documentation and instead permit voters to sign in or verbally confirm their identity (e.g., California and Hawaii). Other states may request documents for voters to cast a regular

⁹ In total, there are 6,852 observations from the eight election-year surveys (2008-2022); self-reported non-citizens are excluded from the analysis. In 2014, the integrity of elections question appeared in the pre-election wave; however, all regression analyses include controls for year and party-by-year.

¹⁰ The histogram for all responses is reported in Figure A1 in the Appendix.

ballot but also permit voters to either sign a waiver or allow provisional ballots to be counted without additional documents confirming identification (e.g., this sort of “non-strict” voter identification is employed in Florida, Missouri, and Rhode Island). The NCSL also tracks whether voters need to show identification with a current photograph (i.e., “photo ID”). Again, this requirement may be “strict” or “non-strict,” depending on whether there is some generally available work-around to the photo ID requirement.

Some caution is in order when utilizing the NCSL as a source. For example, after Missouri adopted a new voter ID law in 2022, the NCSL initially classified Missouri as a strict ID state before subsequently correcting the classification to “non-strict.”¹¹ Also, in contrast to the NCSL, some studies ignore optional waivers and affidavits at the polls, or signature-matching for counting provisional ballots, as reasons to classify a state ID law as “non-strict” (e.g., Harden and Campos 2023). However, this creates an inconsistency in defining strict ID requirements, since several states use signature requirements for voters to cast regular ballots (e.g., California, Illinois, New York, and Pennsylvania), but these are typically not categorized as states with strict ID requirements. For the sake of consistency, this study employs the NCSL definition and treats only requirements that voters produce external documentation for their (provisional) ballot to be counted as a reason to classify that state as having a strict voter ID law. Another complication comes from the fact that actual implementation of voter ID laws may be delayed or prevented due to legal challenges (see especially, Harden and Campos 2023). However, this study follows Cantoni and Pons (2021) by focusing on the effects of the actual implementation of strict ID laws, as defined by the NCSL.

Table 1 lists the states with strict ID laws (and strict photo ID laws) during the study period, 2008-2022. However, four of these states have strict ID laws throughout the entire time-period (Arizona, Georgia, Indiana, and Ohio). This means that these states do not contribute to the identification of the effects of strict ID laws in difference-in-differences analyses (since changes in laws are not observed for these states). So unfortunately for the purposes of this analysis, only nine states ever adopt a new strict ID law within this time frame – Arkansas, Kansas, Mississippi, North Dakota, Tennessee, Texas, Virginia, Wisconsin, and Wyoming, although there are a total of 13 changes in strict ID laws during this time period (Texas and Virginia switch “on” and “off”, while North Dakota switches on, off, and on again). The limited number of states that adopt strict voter ID laws makes it

¹¹ As of this writing, the NCSL also provides inconsistent information regarding New Hampshire; the state is included in a map and list of states with strict photo ID in 2024, but the detailed description of the law indicate both a waiver process at the polls and that voters casting provisional ballots are not required to produce additional documentation (see: <https://www.ncsl.org/elections-and-campaigns/voter-id>; last viewed July 25, 2025).

difficult to identify statistically significant effects of such laws, especially for outcomes that are only available in surveys with limited time spans like the CES (or SPAE).

A final challenge for estimating treatment effects using the CES comes from the fact that team modules in the CES include only 1,000 observations per year (with the geographic distribution of these correlated with state populations). As a result, the number of raw observations in each state and year available for identifying the treatment effect of strict ID laws is also limited (see Table A1). In total, 1,184 respondents are in states that experience a change in strict voter ID laws (or about 17% of the sample examined in this study), while just over 35% of these respondents were in states with strict ID requirements in place at the time. The limited size of the overall and treated sample sizes in the CES work against finding statistically significant effects and raises the concern that one or two large states in the treated pool (e.g. Texas or Virginia) may drive any findings. These concerns are addressed in subsequent regression analyses by checking the robustness of estimates to several model variants, including models that drop each treated state in turn.

A First Look at Voter ID and Perceptions of the Integrity of Elections

Before turning to formal regression analysis, Figure 3 provides an exploratory first look at the association between strict voter ID requirements and confidence in the integrity of elections. Survey respondents are grouped into three mutually exclusive categories: i) those in states that have not yet implemented a strict ID law during the study period (“Before” or “not-yet-treated”); ii) those in states that have implemented a strict ID law (“Strict ID” or “treated”); and iii) all other respondents (“Other” includes both “never-treated” and states that remove strict ID requirements). The bars in Figure 3 show the weighted percent of respondents indicating high or low confidence for each of these three groupings.

States that eventually adopt strict ID laws have significantly lower rates of high confidence prior to the implementation of strict ID compared to other states ($p < .01$). However, those same states realize a significant *increase* in *high* confidence after the implementation of strict ID laws ($p < .01$). Moreover, states that adopt strict ID laws also exhibit a significant *decrease* in the percentage of respondents reporting *low* confidence in the integrity of elections ($p < .01$). Consequently, there is associational evidence that strict ID laws may indeed improve public confidence in the integrity of elections.

The findings illustrated in Figure 3 also underscore the importance of analyzing *within state changes* in perceptions of the integrity of elections, as failure to do so may confound the identification of the potential causal effects of strict ID requirements. For this reason, all

subsequent estimation models examine within-state changes both strict ID laws and public perceptions of the integrity of elections.

Regression Analyses

The treatment effects of strict ID laws are identified by estimating various difference-in-differences regression models using weighted least squares (employing survey weights). The primary dependent variable is a binary indicator for high confidence (or low confidence) in the integrity of state elections and the independent variable of interest is an indicator for the presence of a strict voter ID requirement in the state.¹² Other dependent variables examined include the full 7-point scale for confidence in election integrity, as well as self-reported and verified vote, interest in public affairs, belief or knowledge regarding voter ID restrictions, and trust in federal or state government. Because this study employs mixed-level analyses of the effects of state laws on individual-level outcomes, standard errors are corrected for clustering at the state-level (Primo, Jacobsmeier, and Milyo 2006).

All regression models include indicators for state and year, and the preferred model specification also controls for other relevant state-level and individual-level attributes. State-level control variables include other state election administration laws as categorized by the NCSL,¹³ as well as an indicator for whether there is a statewide election for governor or the U.S. Senate. Following Primo and Milyo (2020) and other recent studies of public perceptions of political institutions, the main specification also controls for party-by-year effects, unified party control of state government, and party concordance (i.e., whether a respondent affiliates with the same or opposite party in control of state government).¹⁴ The individual-level controls in these regression models include indicators for age, education, race and Hispanic ethnicity, and sex. Descriptive statistics for all variables employed in this study are listed in Table A2.

Three assumptions are implicit in the canonical differences-in-differences (DiD) model: no anticipation, parallel trends, and homogeneous treatment effects. In the context examined here, no anticipation means that any impact on confidence does not begin in advance of a change in voter ID requirements, while the parallel trend assumption requires that any

¹² The linear probability model is widely employed in policy evaluation for ease of interpretation of the estimated coefficients, including the year and state effects (e.g. Primo and Milyo 2020; Cantoni and Pons 2021).

¹³ These state election administration laws are: No Excuse Absentee Voting; All-Mail Voting; Automatic Voter Registration; Early Voting; and Same Day Registration (or no voter registration, in the case of North Dakota).

¹⁴ Data on party control of state offices is also from the NCSL; however, the nonpartisan and unicameral legislature in Nebraska is treated as under Republican control throughout the time period examined here (on the effective Republican majority in Nebraska's Senate, see Masket and Shor 2015; Shor and McCarty 2011; and Janousek, et al 2025).

difference in perceptions of the integrity of elections between the treated and control states is constant prior to the treatment. Homogeneous treatment effects means that strict ID laws have the same impact across states or other groups. All of these maintained assumptions are checked in the next section via several falsification and robustness tests, as well as by testing for differential effects by party or minority race and Hispanic ethnicity.

The methodological approach in this study hews closely to that in Cantoni and Pons (2021); this includes employing a standard two-way fixed effects model (i.e., year and state fixed effects) as the main specification for estimating the within-state effects of changes in voter ID laws. Under the maintained assumptions of no anticipation, parallel trends and homogeneous effects, this workhorse model will yield unbiased estimates of treatment effects when treatments are implemented simultaneously. But when policy changes occur at different times in different states (or “staggered”), as is the case with strict voter ID laws, the standard two-way fixed effect (TWFE) approach may yield biased estimates of treatment effects (for recent review, see Baker, Larker, and Wang 2022). In response, several new methods have been proposed to address potential biases in staggered difference-in-difference models, although user-friendly procedures for implementing these in standard statistical packages (e.g., Stata) continue to evolve, so constitute something of a moving target.

This study employs two recently developed procedures that are not susceptible to potential bias from staggered implementation of policies. The Two-Stage DID model (DID2S) proposed by Gardener (2022) is easily implemented in Stata using all treated states, including those that switch status from treated to untreated. This estimator uses observations from untreated state and not-yet-treated states to form a counterfactual predicted outcome in the treated states; the treatment effect is then estimated by the difference between the actual and counterfactual outcomes in treated states. This estimator can be implemented in Stata with the command DID2S; however, one drawback is that this procedure cannot be restricted to compare only treated states to the “never-treated states.”

For this reason, I also employ the estimation procedure described in Woolridge (2021); this estimator can be implemented with the Stata command JWDID and provides flexibility to compare treated states to only never treated states, or to both never treated and not-yet treated states. A disadvantage to this procedure is that states that switch status from untreated to treated and back again are dropped from the analysis (exacerbating the problem of small numbers of respondents in treated states discussed above). However, the resulting estimates from these staggered DiD models are very similar to those obtained

by TWFE; this mitigates any concerns regarding potential bias from staggered implementation of strict voter ID requirements.

3. Results

Table 2 reports the estimated coefficients for the basic TWFE regression model with four different sets of control variables. The first panel shows results when the dependent variable is the indicator for high confidence, while the second shows results for low confidence. The four columns in Table 2 correspond to a progression of control variables, from a sparse model with only state and year fixed effects to the full model used as the preferred specification throughout this study. The complete regression results for this preferred specification are reported in Table A3, for both high and low confidence, as well as for the full 7-point confidence scale.

Strict voter ID requirements have a significant effect on confidence by either measure, but estimates are larger and more precise for high confidence.¹⁵ Moreover, the magnitudes of these estimates are substantively significant, even with the full set of controls. For example, strict voter ID increases the probability that an individual indicates high confidence by 14 percentage points, which is a 25% increase relative to the mean (and a 35% increase relative to the mean in treated states prior to adopting strict ID). Likewise, the probability that a respondent indicates low confidence decreases by 8 percentage points, or about 30% relative to the mean. Across all the specifications listed in Table 2, the estimated effects of strict voter ID imply an improvement in confidence equivalent to around 20%-30% of the respective standard deviations in these outcome variables.

Looking across the columns in Table 2, the addition of control variables for party effects improves the explanatory power of these models and has a moderating impact on the estimated effect of strict voter ID on confidence. This suggests that the absence of significant findings in Cantoni and Pons (2021) mentioned above is not driven by their omission of controls for party effects. Nevertheless, political partisanship is demonstrated to be an important lens with which respondents gauge confidence in their state elections. For example, party concordance (i.e., being of the same party that controls state government) increases the probability of high confidence by about 10 percentage points (see Table A3).¹⁶

¹⁵ Results are qualitatively similar when employing the 7-point confidence scale; however, this specification imposes homogeneous effects of strict voter ID at the high and low ends of the confidence scale, which is contradicted by the findings in Table 2.

¹⁶ Given the inclusion of party-by-year interactions, the overall effect of party identification is not identified in these models. However, dropping this interaction, Democrats have significantly higher confidence than Republicans, and Republicans show no difference in confidence relative to independents and non-partisans.

In contrast to strict ID requirements, other state election institutions --- no excuse absentee voting, all mail voting, automatic registration, early voting, and same day registration --- have effects on public confidence that are small and imprecisely estimated (Table A3). In fact, the coefficients on these convenience voting measures are neither individually nor jointly significant. Thus strict voter ID stands out as the only election administration institution that has a meaningful impact on public confidence in the honesty and integrity of elections.

Pathways and Placebos

Table 3 provides corroborating evidence for the findings above by presenting estimates of the effects of strict voter ID on several other outcome variables of interest that are also available in the CES. All models employ the same specification as that in column 4 of Table 2 (i.e., with the full set of controls) and are restricted to observations with non-missing values for confidence in election integrity. This permits a comparison of outcomes across the same set of respondents (albeit omitting observations with missing values for the alternative outcome variables).

The first two columns of Table 3 show the effect of strict voter ID on whether a respondent voted. “Verified vote” refers to whether a respondent in the CES is matched to a voter in state voter files, while “self-reported vote” is simply whether a respondent claims to have voted in the post-election wave of the CES. Consistent with Cantoni and Pons (2021), there is no significant effect of strict ID on overall turnout sourced from state voter files, although the point estimate here is both large and positive. Self-reported turnout increases by a statistically significant 5 percentage points. This increase in self-reported voting may indicate an increase in actual turnout, consistent with Justice Stevens’s hypothesis that confidence increases voter turnout, or may be an artifact of increased misreporting attributable to greater salience of elections in states that adopt strict voter ID. For example, these states may realize increased news coverage and informational communications from election officials, or even countermobilizing efforts from political groups opposed to voter ID requirements (e.g., Cantoni and Pons 2021; Raze 2022).

The next two columns of Table 3 show the estimated effects of strict voter ID on whether a respondent believes their state has a photo ID requirement (column 3), or whether they are able to correctly identify whether or not their state has photo ID requirements of any sort (column 4). The survey questions that generate these indicators are also part of the Missouri team module of the CES since 2010, although they are about photo ID, not specifically strict ID requirements. Even so, these do offer some evidence about public awareness of voter ID requirements in the wake of states adopting strict ID laws. In both cases, there is a significant positive effect of strict voter ID laws on awareness that is

similar in magnitude across these measures, and a bit larger in magnitude compared to the increase in high confidence in election integrity.

The findings in columns 1-4 of Table 3 suggest that passage of strict ID requirements may well increase salience and awareness of ID requirements at the margin, in turn increasing confidence in elections among these respondents. Consequently, even though the public is not well-informed about state election administration institutions in general, these findings offer a plausible pathway from strict ID laws to increased confidence in the integrity of elections.¹⁷

The final three columns of Table 3 examine outcomes that are not expected to be greatly affected by state ID requirements, so provide something like placebo tests. First, implementation of strict ID laws may increase the salience of elections or knowledge of voter ID, but it would be a stretch for this to have a more general impact on public interest in all manner of political events and public affairs. The Common Content of the CES includes a 4-point measure of respondents' interest in following political events and public affairs. This outcome is examined in column 5 of Table 3; strict ID requirements indeed have a relatively small, positive, and statistically insignificant impact on general interest in public affairs.

State implementation of strict voter ID is arguably even less likely to have an overall effect on trust in government. The Missouri team module of the CES includes standard 4-point measures of trust in state or federal government in every year examined here.¹⁸ Again, neither of these outcomes is significantly impacted by state ID requirements (columns 6 and 7 of Table 3). Admittedly, these placebo tests are modest in that there may be some spillover effect from confidence in state elections to interest in public affairs or trust in government, but it would be disconcerting if the observed effects for these alternative outcomes were larger or had the opposite sign as the estimated effect of strict voter ID on confidence.

Staggered Difference in Differences

As noted above, the simple TWFE difference-in-difference model may yield biased estimates when treatment implementation is staggered (i.e., occurring at different times in different states), as is the case with state voter ID laws. Consequently, the preferred model

¹⁷ Consistent with this pathway, Milyo (2025) shows approval of several election administration institutions (including photo ID) is higher among CES respondents that believe their state has that institution.

¹⁸ Respondents are asked "How much of the time do you think you can trust (federal or state) government to do the right thing? Responses range from 1= hardly ever to 4 = just about always.

specifications (with the full set of control variables) are re-estimated using estimation procedures that are not biased when treatment implementation is staggered (see Table 4).

Unlike TWFE, states that are always treated (i.e., AZ, GA, IN, and OH) are dropped from the analysis when employing either Gardener's (2021) two-stage procedure or Woolridge's (2021) procedure (DID2S and JWDID, respectively). And while these states do not contribute to the identification of the treatment effect in a difference-in-difference model, their inclusion may impact estimated coefficients on control variables, which in turn may be correlated with treatment effects. Therefore, to confirm that omitting the "always treated" states does not greatly impact the estimated treatment effect of strict voter ID, the first two columns of Table 4 compare TWFE estimates with and without the "always treated" states.

The results for the two-stage model (DID2S) are presented in columns 3 and 4 of Table 4. Comparing estimates across similar models in columns 2 and 3, the effects of strict voter ID are slightly larger (in absolute value) when estimated via the two-stage procedure versus TWFE. In contrast to these estimators, the JWDID procedure cannot include states that switch status, so to provide a baseline for comparison, the two-stage model is also estimated without "switchers" in column 4 of Table 4. Omitting these three states increases the size of the estimate for high confidence a bit, but the standard errors are larger for both high confidence and low confidence. In comparison, the Woolridge estimator produces much sharper estimates with larger effects (in absolute value) on both high and low confidence, and these estimates are all statistically significant ($p < .01$). An additional advantage of the JWDID procedure, is that the treatment effect can be identified using only never treated states (as opposed to including not-yet-treated); however, this has no additional impact on the coefficients of interest (column 6 of Table 4).

The findings reported in Table 4 allay concerns about potential bias due to staggered implementation of strict voter ID requirements. If anything, the alternative estimators yield sharper results, but at the cost of exacerbating concerns about sample size. For this reason, all subsequent analyses take a more conservative approach and employ the standard TWFE model. However, it remains to be shown that the implicit assumptions of the difference-in-difference model are not violated.

Falsification Tests and Robustness Checks

Table A4 in the appendix reports the results of several falsification tests. In no case are the implicit assumptions of "no pre-trends" and "no anticipatory effects" rejected for the high confidence model. Moreover, the assumption that there is no difference in confidence in the two elections prior versus post implementation is easily rejected for both models.

However, for the low confidence model, the assumption that there are no anticipatory effects in the two elections prior to implementation of strict voter ID is marginally rejected ($p > .10$). There is also some indication of marginally significant pre-trends over the three or four elections prior to implementation of strict voter ID ($p < .10$) for the low confidence model. Nevertheless, for completeness all subsequent findings are shown for both the high confidence and low confidence models. Even so, less weight should be ascribed to the interpretation of results from the low confidence model versus the high confidence model due to the results of these falsification tests.

To check whether the estimated effects of strict ID laws obtained via the TWFE model are driven by influential observations in particular states, the preferred model specification from column 4 of Table 2 is re-estimated after dropping one treated state sequentially. The results of this sensitivity analysis are reported in Table A5. For either model, dropping treated states sequentially from the analysis does not dramatically change the estimated effects (all estimates are within .03 of the original estimates). However, relative to the size of the treatment effect, the low confidence model exhibits more sensitivity, and in some cases statistical significance is quashed, as well. This underscores the caveat to soft-pedal findings from the low confidence model.

Differential Effects of Strict ID Laws

The models estimated above impose homogenous effects of strict ID laws across different states or groups. In this section, that assumption is relaxed to test for differential effects of strict *photo* ID, or differential effects of strict (photo) ID by party, race, or ethnicity.

There isn't much scope for identifying a difference between the effects of strict ID requirements and strict *photo* ID requirements, as most of the observed changes in ID requirements involve strict photo ID (see Table 1).¹⁹ Table 5 reports the results of regressions identical to those in column 4 of Table 2, but for different types of strict ID laws. The null hypothesis that strict non-photo and strict photo requirements have identical effects cannot be rejected in either the high confidence or low confidence specifications. In addition, replacing the indicator for strict ID with one for strict photo ID has little impact on the coefficients of interest. Again, this shouldn't be surprising given the limited number of changes in state laws overall. Consequently, subsequent analyses of differential effects by party or race continue to focus strict ID requirements (results for strict photo ID are reported in the Appendix).

Given the polarized nature of the public debate over voter ID, together with the importance of party and party concordance as determinants of public confidence in the integrity of

¹⁹ The indicators for these two laws are highly correlated ($r = .75$) in the mixed-level data.

elections, it is reasonable to conjecture that Republicans may respond more positively to the implementation of strict ID than Democrats. The regression results shown in Table 6 provide a formal test of this conjecture. The results in column 1 indicate that there is no significant differential effect of strict ID on high confidence in the integrity of elections for Democrats or Republicans. However, Republicans do have a more muted reaction to strict ID when it comes to reporting low confidence. But for either model specification, these findings are contrary to the initial conjecture above (and likewise for strict photo ID, see Table A6).

Finally, given that opponents of voter ID requirements claim that such laws suppress turnout among racial and ethnic minorities, a second reasonable conjecture is that minorities may respond less favorably to strict ID laws. However, the results reported in Table 7 indicate that this is not the case for Black or Hispanic respondents, although other minority respondents do respond significantly less favorably to strict ID laws (compared to non-minority respondents) in the high confidence model. Even so, the null hypothesis of a zero net effect of strict voter ID on other minority respondents cannot be rejected at conventional levels of statistical significance. For the low confidence model, there are no significant differential effects of strict ID for any minority groups (column 2 of Table 7). Similar differentials are examined in Table A7, but for strict photo ID requirements. Black respondents are less favorably impacted by strict photo ID (albeit with no significant net effect on confidence), but Hispanic respondents react *more* favorably with respect to confidence in the integrity of elections (and significantly so for the high confidence model).

4. Discussion

This is the first study to demonstrate that strict voter ID requirements have any sort of substantive and statistically significant impact on public perceptions of election integrity. In contrast to earlier studies, the survey instrument utilized here directly measures confidence in the honesty and integrity of their state elections. Furthermore, this is the first study to employ difference-in-difference models while also controlling for the fact that Americans view the integrity of elections through a partisan lens.

The manifestation of salutary effects of voter identification on confidence in election integrity is obscured in simple cross-sectional analyses, since states that adopt strict voter ID laws tend to start out with lower public confidence. But after the implementation of strict ID laws, the probability that individuals in these states report high confidence increases by a statistically significant 14 percentage points ($p < .01$). This estimate is substantively large and robust to multiple falsification and sensitivity tests.

The effects of strict voter ID requirements on high confidence are also robust to several alternative model specifications, including those examining differential effects on subgroups of interest. For example, the beneficial effect of strict voter ID requirements on election integrity is not significantly lower for Black or Hispanic individuals, or Democrats. And while strict photo ID does have differential effects of opposite signs for Black and Hispanic respondents, in no case is there evidence of a statistically significant detrimental net effect of strict ID requirements for any subgroup by party, race or Hispanic ethnicity.

The findings for individuals reporting low confidence offer weaker support to the hypothesis that strict voter ID laws improve perceptions of election integrity. While the probability that treated individuals report low confidence in election integrity is estimated to decrease by 8 percentage points, this effect is less precisely estimated and more suspect considering the underwhelming falsification and sensitivity tests for the low confidence model.

The analysis here largely follows the methodological approach of Cantoni and Pons (2021) by estimating the treatment effect of state voter ID laws in TWFE and difference-in-differences models. However, unlike those authors, this study includes controls for partisan influences on perceptions of the efficacy of political institutions. The analysis here also examines a longer time-period and investigates potential differential effects of strict ID laws on focal subgroups.

And in contrast to the previous literature, this study employs a novel survey instrument that directly measures confidence in the integrity of elections. Previous studies rely on proxy measures such as perceptions of the correctness of vote counts. This is an important difference, since election integrity implicates both access and security, while vote counting measures do not. Further, in the context of voter ID, the relevant concern about illegal voting is not that the ballots which have been cast are counted incorrectly, but that some voters should not have been permitted to cast ballots in the first place. However, further investigation of the sensitivity of public perceptions of election integrity to the framing or wording of survey questions is warranted. To the extent that perceptions of the integrity of elections are highly malleable or context-dependent, this would cast some doubt on whether survey responses regarding such perceptions reflect stable or meaningful convictions (for a related discussion in the context of campaign finance regulations, see Primo and Milyo 2020).

This study has also offered corroborating evidence on the pathway by which state voter ID requirements impact public confidence in elections, by demonstrating an increase in the salience of elections and awareness of ID requirements after a state adopts strict voter ID. However, to the extent that much of the public remains ignorant about voter ID laws, this may obscure the potential impact of such reforms. First, because some respondents may

believe their state already has voter ID when it does not (and vice versa), the observable treatment effects may be attenuated. Moreover, even among relatively sophisticated respondents, delays in implementation of voter ID laws may blur the distinction in the minds of respondents between whether a state has voter ID requirements or not.

This suggests that public education campaigns about the presence of strict voter ID requirements in states with such laws may also increase confidence in the integrity of state elections (also see Citrin et al 2014). Future work might test this pathway by comparing the extent of media coverage of voter ID laws across states as a potential measure of treatment intensity.

Finally, an important limitation of this study is that the number of states that have adopted (or removed) strict voter ID requirements during the study period is limited. Going forward, it would be useful to examine the issues discussed here with additional years of state-level variation in strict voter ID requirements and to the extent feasible, additional years of survey data. Given the novel findings of this study, some re-examination of findings in earlier studies that employ counting-based proxies for public confidence in the integrity of elections may be warranted (e.g., Classen et al 2013; Clark 2021; and Suttman-Lea et al 2023).

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Figure 1: Confidence in the Integrity of Elections in Your State

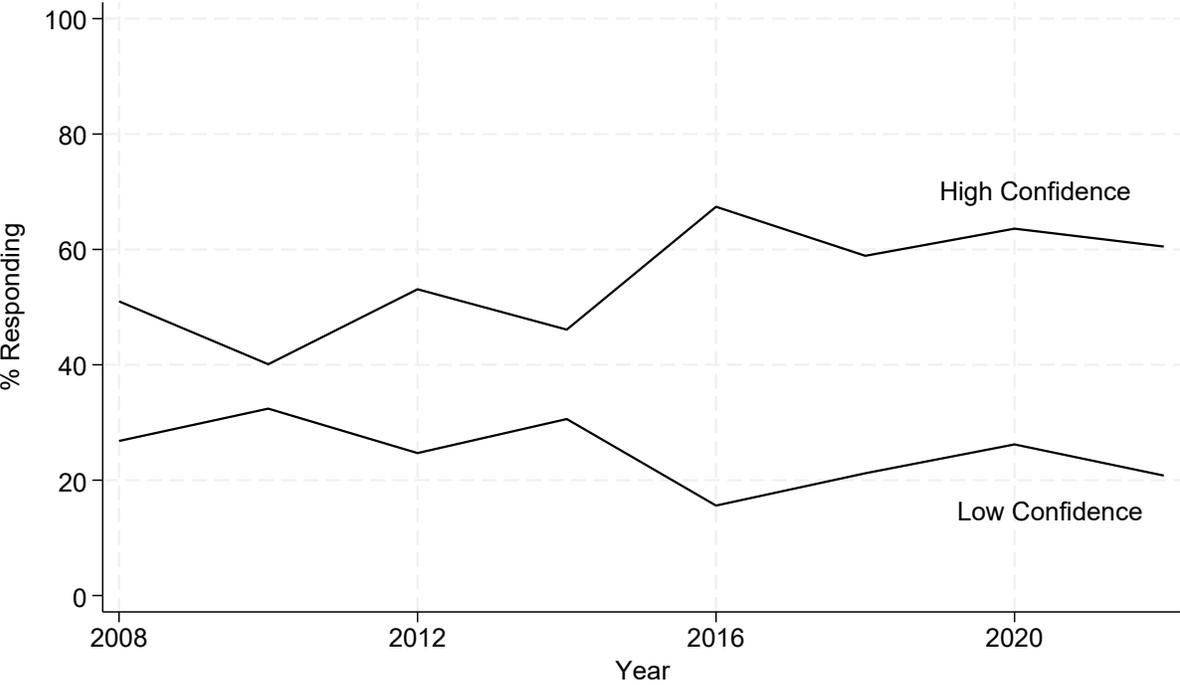


Figure 2: Confidence in the Integrity of Elections by Party

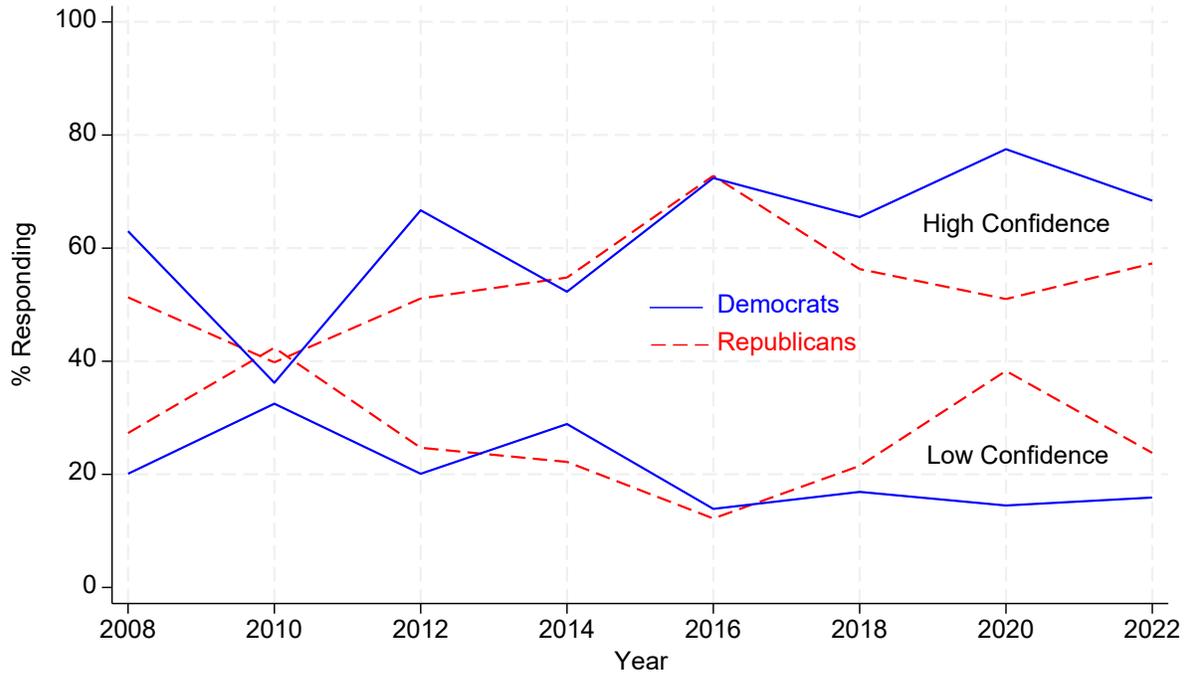
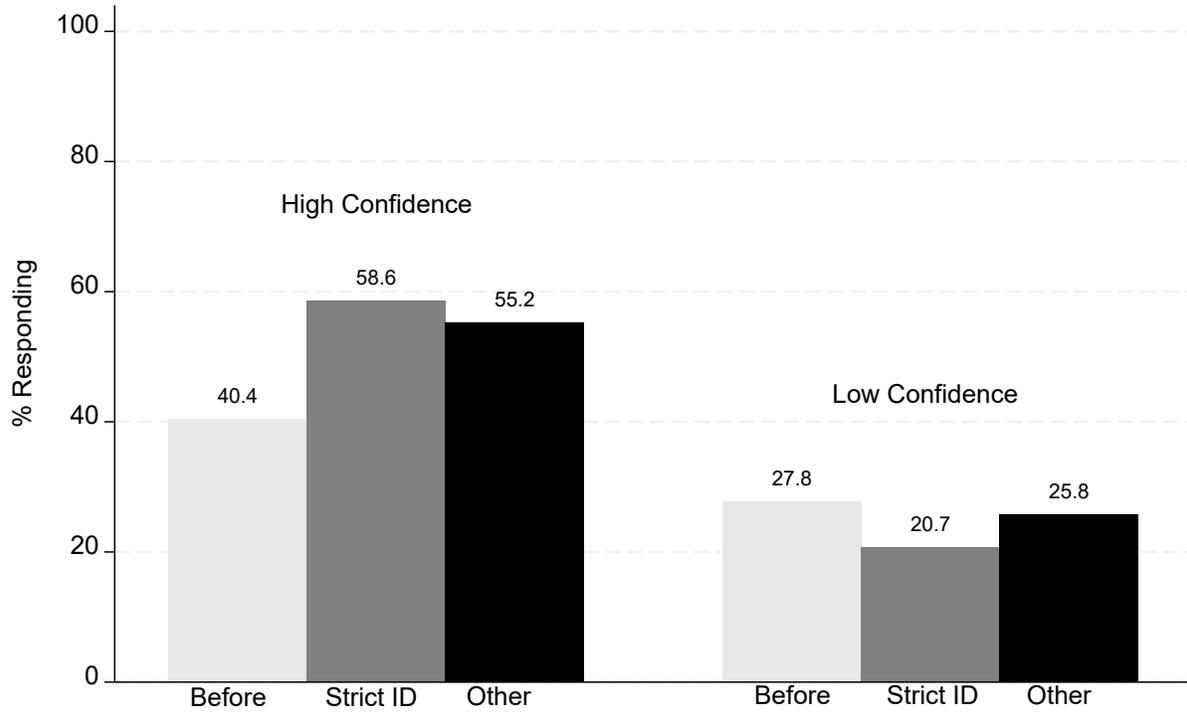


Figure 3: Confidence in the Integrity of Elections by Strict ID Status



Notes: “Strict ID” indicates respondents in a state with a strict voter ID requirement in effect. “Before” indicates respondents in states prior to implementing Strict ID (i.e., “not-yet-treated”), while “Other” indicates all other respondents. All differences are statistically significant for high confidence ($p < .01$); for low confidence, the difference between Before and Strict ID is also significant ($p < .01$).

Table 1: States with Strict Voter ID Requirements

Year	States
2008	AZ, GA*, IN*, OH
2010	AZ, GA*, IN*, OH
2012	AZ, GA*, IN*, KS*, OH, TN*, VA
2014	AZ, GA*, IN*, KS*, MS*, ND, OH, TN*, TX*, VA*
2016	AZ, GA*, IN*, KS*, MS*, OH, TN*, VA*, WI*
2018	AZ, GA*, IN*, KS*, MS*, ND, OH, TN*, VA*, WI*
2020	AZ, GA*, IN*, KS*, MS*, ND, OH, TN*, WI*
2022	AR*, AZ, GA*, IN*, KS*, MS*, ND, OH, TN*, WI*, WY

Notes: * Strict Photo ID; source is CP and NCSL

Table 2: Effects of Strict Voter ID on Confidence in Elections

<i>Panel One: High Confidence</i> (Mean = .56; S.D. = .50)				
	(1)	(2)	(3)	(4)
Strict ID Law	.18*** (.05)	.16*** (.04)	.16*** (.04)	.14*** (.04)
R-squared	.05	.09	.07	.11
<i>Panel Two: Low Confidence</i> (Mean = .26; S.D. = .44)				
	(1)	(2)	(3)	(4)
Strict ID Law	-.09** (.04)	-.08* (.04)	-.09** (.04)	-.08* (.04)
R-squared	.03	.07	.04	.08
Included Controls:				
State & Year Effects	Yes	Yes	Yes	Yes
Party Variables		Yes		Yes
Other Individual-Level Variables			Yes	Yes
Other State-Level Variables			Yes	Yes

Notes: *p<.10; **p<.05; and ***p<.01. Standard errors corrected for clustering at the state level are reported in parentheses. Party controls include: year X party, unified party control of state government, and party concordance. Individual controls include: age, education, Hispanic ethnicity, race, and sex. State controls include: indicators for other election administration laws and concurrent statewide elections. See Table A2 for a description of all control variables.

Table 3: Effects of Strict Voter ID on Other Outcomes

	Vote (Verified) (1)	Vote (Self-Report) (2)	Photo ID (Belief) (3)	Photo ID (Knowledge) (4)	Interest in Public Affairs (5)	Trust in Federal Gov't (6)	Trust in State Gov't (7)
Mean (S.D.)	.49 (.50)	.85 (.35)	.48 (.50)	.50 (.50)	3.3 (.92)	1.8 (.73)	2.0 (.72)
Strict ID Law	.08 (.06)	.05** (.02)	.18*** (.06)	.19* (.10)	.08 (.07)	.03 (.08)	.02 (.09)
Observations	6,852	6,385	5,091	5,091	6,723	6,815	6,797

Notes: * $p < .10$; ** $p < .05$; and *** $p < .01$. Cell entries in the second row are estimated coefficients from separate regressions with standard errors corrected for clustering at the state level in parentheses; all models include the full set of controls employed in column 4 of Table 2.

Table 4: Alternative Difference-in-Difference Estimates

<i>Panel One: High Confidence</i>						
	(1)	(2)	(3)	(4)	(5)	(6)
Strict ID Law	.14*** (.04)	.14*** (.04)	.15*** (.05)	.17** (.08)	.17*** (.03)	.17*** (.03)
<i>Panel Two: Low Confidence</i>						
	(1)	(2)	(3)	(4)	(5)	(6)
Strict ID Law	-.08* (.04)	-.07* (.04)	-.10** (.05)	-.10 (.08)	-.11*** (.03)	-.11*** (.03)
Estimation Method and Model Restrictions:						
	TWFE	TWFE	DID2S	DID2S	JWDID	JWDID
Omit “Always” Treated (AZ, GA, IN, OH)	No	Yes	Yes	Yes	Yes	Yes
Omit “Switchers” (ND, TX, VA)	No	No	No	Yes	Yes	Yes
Treatment identified using only “Never Treated” states:	No	No	No	No	No	Yes

Notes: *p<.10; **p<.05; and ***p<.01. Standard errors corrected for clustering at the state level are reported in parentheses.

All models include the full set of controls employed in column 4 of Table 2.

Table 5: Differential Effects of Strict Photo ID

	High Confidence		Low Confidence	
	(1)	(2)	(3)	(4)
Strict Photo ID Law	.14*** (.04)	.13*** (.05)	-.08* (.04)	-.06* (-.04)
Strict Non-Photo ID Law	.12** (.05)		-.11 (.07)	

Notes: * $p < .10$; ** $p < .05$; and *** $p < .01$. Standard errors corrected for clustering at the state level are reported in parentheses. All models include the full set of controls employed in column 4 of Table 2. For the models in columns (1) and (3) above, the null hypothesis that the estimated coefficients on strict photo ID and strict non-photo ID are equal cannot be rejected.

Table 6: Differential Effects of Strict ID Laws by Party

	High Confidence (1)	Low Confidence (2)
Strict ID Law	.15*** (.053)	-.11** (.04)
Democrat X Strict ID Law	.00 (.05)	.02 (.03)
Republican X Strict ID Law	-.02 (.05)	.09** (.03)
Joint Significance for Differential Effects	No	Yes*

Notes: *p<.10; **p<.05; and ***p<.01. Standard errors corrected for clustering at the state level are reported in parentheses. All models include the full set of controls employed in column 4 of Table 2.

Table 7: Differential Effects of Strict ID Laws by Race and Hispanic Ethnicity

	High Confidence (1)	Low Confidence (2)
Strict ID Law	.16*** (.04)	-.08* (.04)
Black X Strict ID Law	-.07 (.06)	-.00 (.05)
Hispanic X Strict ID Law	.03 (.10)	-.01 (.08)
Other Race X Strict ID Law	-.17* (.09)	.08 (.09)
Joint Significance for Differential Effects	No	No

Notes: *p<.10; **p<.05; and ***p<.01. Standard errors corrected for clustering at the state level are reported in parentheses. All models include the full set of controls employed in column 4 of Table 2.

Figure A1: Confidence in the Integrity of Elections in Your State

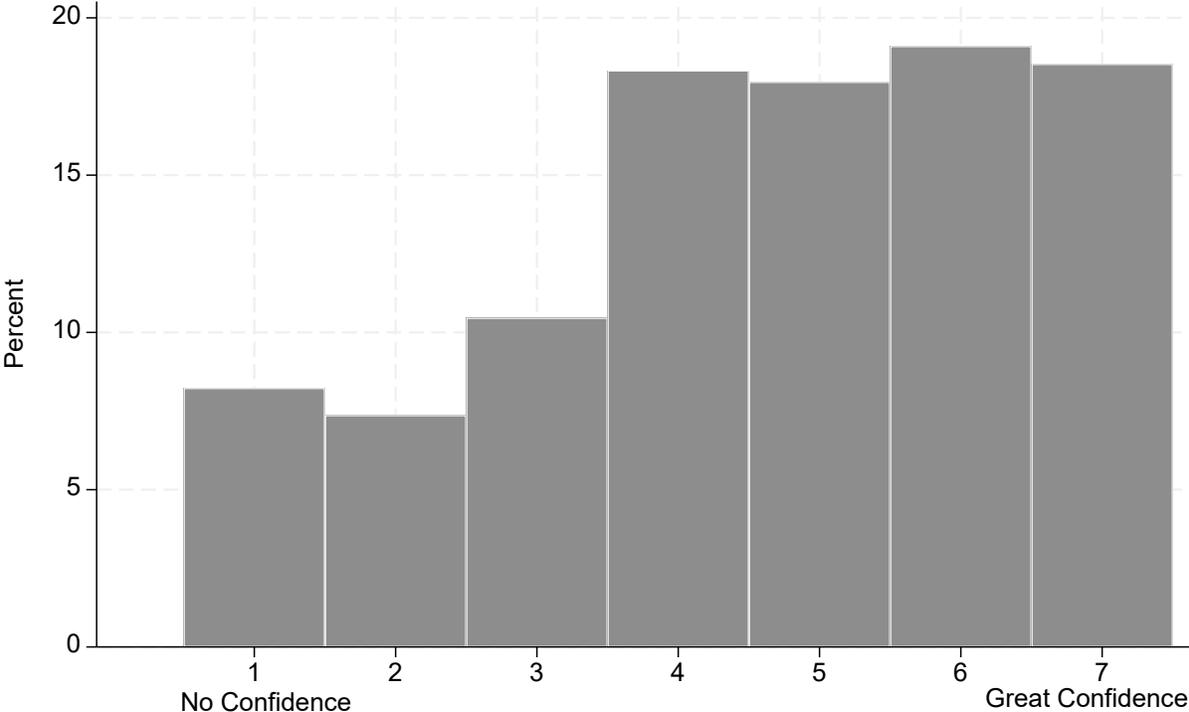


Table A1: Respondents in States with Changes in Strict Voter ID, 2008-2022

State	2008	2010	2012	2014	2016	2018	2020	2022	OFF	ON
AR	10	10	10	9	5	7	9	8	60	8
KS	18	14	13	11	6	8	4	9	32	51
MS	10	10	4	5	5	5	7	6	24	28
ND	2	0	2	5	2	2	2	1	6	10
TN	20	10	16	12	10	20	19	15	30	92
TX	63	52	48	64	60	68	74	73	438	64
VA	24	16	26	25	22	21	20	23	83	94
WI	23	15	20	18	18	18	16	18	76	70
WY	3	4	3	2	4	2	0	0	18	0
All States with Changes in Strict Voter ID:									767	417

Notes: Cell entries report the number of respondents in the CES with non-missing responses for confidence in election integrity; bold face entries indicate the state has implemented a strict voter ID requirement (i.e., the law is “on”).

Table A2: Means and Standard Deviations for All Variables

<i>Confidence Variables</i>		<i>Party Variables</i>	
High Confidence	.55 (.50)	Democrat	.34 (.47)
Low Confidence	.25 (.43)	Republican	.28 (.45)
Confidence Scale (1-7)	4.6 (1.8)	Unified Democratic State Gov.	.24 (.43)
		Unified Republican State Gov.	.41 (.49)
<i>Other Outcome Variables</i>		Party Concordance (-1, 0, 1)	.05 (.64)
Verified Vote	.49 (.50)		
Self-Reported Vote	.85 (.35)		
Interest in Public Affairs	3.3 (.92)	<i>Other Individual-Level Variables</i>	
Belief that State has Photo ID	.48 (.50)	Female	.53 (.50)
Knowledge of State Photo ID	.50 (.50)	Black	.10 (.30)
Trust in Federal Government	1.8 (.73)	Hispanic	.09 (.28)
Trust in Federal Government	2.0 (.72)	Other Race	.07 (.25)
		Less than High School	.08 (.26)
<i>State-Level Variables</i>		High School	.29 (.45)
Strict ID Law	.19 (.39)	Some College	.33 (.47)
Strict Photo ID Law	.11 (.32)	Post-Graduate	.11 (.31)
No Excuse Absentee	.59 (.49)	Age 18-24	.09 (.28)
All Mail Voting	.07 (.25)	Age 25-34	.16 (.36)
Automatic Registration	.13 (.34)	Age 35-44	.17 (.37)
Early Voting	.75 (.43)	Age 45-54	.16 (.37)
Same Day or No Registration	.21 (.41)	Age 55-64	.21 (.41)
Concurrent State Election	.81 (.39)	Age 75+	.06 (.24)

Notes: All variables are binary indicator variables except: Confidence Scale(1 = no confidence; 7=great confidence); Interest in Public Affairs (1= hardly at all; 7 = most of the time); Trust in Government (1=hardly ever; 4=just about always) and Party Concordance (-1 = respondent is opposite part of unified state government; 1= respondent is same party as unified state government; and 0 = otherwise). The omitted categories for the individual-level variables are: Male, White, College, and Age 65-74.

Table A3: Effects of Strict Voter ID on Confidence in Elections

	High Confidence (1)	Low Confidence (2)	Confidence Scale (3)
Strict ID Law	.14*** (.040)	-.08* (.04)	.33** (.15)
No Excuse Absentee	.01 (.06)	.00 (.05)	.05 (.21)
All Mail Voting	.03 (.04)	-.02 (.03)	.14 (.16)
Automatic Registration	-.00 (.03)	.02 (.04)	-.04 (.14)
Early Voting	-.05 (.04)	.02 (.03)	-.07 (.16)
Same Day or No Registration	.01 (.05)	-.01 (.04)	.10 (.18)
Concurrent State Election	-.02 (.02)	.00 (.03)	-.04 (.10)
Unified Democratic State Government	-.02 (.03)	.00 (.02)	-.06 (.11)
Unified Republican State Government	.03 (.02)	-.01 (.03)	.11 (.09)
Party Concordance with State Government	.10*** (.01)	-.10*** (.01)	.46*** (.06)
Female	-.08*** (.02)	.02 (.01)	-.21*** (.07)
Black	-.02 (.03)	-.03 (.02)	.01 (.09)
Hispanic	-.05 (.04)	.02 (.03)	-.11 (.14)
Other Race	-.05* (.03)	.04 (.03)	-.15 (.13)
Less than High School	-.17*** (.03)	.09* (.05)	-.54*** (.17)
High School	-.07*** (.03)	-.00 (.02)	-.21*** (.08)
Some College	-.06*** (.01)	.03* (.02)	-.18** (.07)
Post-Graduate	.018 (.02)	-.00 (.02)	.05 (.08)
Age 18-24	-.01 (.03)	-.10*** (.030)	.08 (.13)
Age 25-34	-.06	-.07**	-.12

	(.04)	(.03)	(.10)
Age 35-44	-.03 (.03)	-.07** (.03)	.01 (.11)
Age 45-54	-.02 (.03)	-.01 (.03)	-.08 (.11)
Age 55-64	-.03 (.03)	-.00 (.02)	-.11 (.11)
Age 75+	-.00 (.03)	-.02 (.03)	-.03 (.12)
State and Year Indicators	Yes	Yes	Yes
(Party X Year) Indicators	Yes	Yes	Yes
R-Squared	.11	.08	.12

Notes: *p<.10; **p<.05; and ***p<.01. Standard errors corrected for clustering at the state level are reported in parentheses.

Table A4: Falsification Tests

<i>Panel One: Anticipation</i>		
	High Confidence (1)	Low Confidence (2)
Indicator for One Election Prior to Strict ID Implementation	-.08 (.08)	-.08 (.06)
Indicator for One or Two Elections Prior to Strict ID Implementation	.04 (.04)	-.07* (.04)
Pre = Post for Two Elections Before & After	.09*** (.03)	-.07** (.03)
<i>Panel Two: Parallel Trends</i>		
	High Confidence (1)	Low Confidence (2)
Pre-Treatment Trend in Strict ID States Over Three Prior Elections	-.01 (.04)	-.03* (.01)
Pre-Treatment Trend in Strict ID States Over Four Prior Elections	-.00 (.03)	-.02* (.01)
Pre-Treatment Trend in Strict ID States Over Five Prior Elections	-.03 (.04)	-.01 (.01)

Notes: *p<.10; **p<.05; and ***p<.01. Standard errors corrected for clustering at the state level are reported in parentheses. All models include the same controls as the models in column 4 of Table 2. Cell entries in boldface indicate failure for a falsification test.

Table A5: Sensitivity to Exclusion of Treated States

Estimated Effects of Strict ID Laws with the following States Excluded:	High Confidence (1)	Low Confidence (2)
None	.14*** (.04)	-.08* (.04)
Arkansas	.15*** (.04)	-.08* (.04)
Kansas	.12*** (.05)	-.05 (.04)
Mississippi	.16*** (.04)	-.09* (.04)
North Dakota	.15*** (.04)	-.08** (.04)
Tennessee	.12*** (.04)	-.06 (.04)
Texas	.12** (.05)	-.08 (.06)
Virginia	.15*** (.05)	-.07 (.05)
Wisconsin	.17*** (.05)	-.11*** (.04)

Notes: *p<.10; **p<.05; and ***p<.01. Standard errors corrected for clustering at the state level are reported in parentheses. All models include the full set of controls listed in column 4 of Table 2.

Table A6: Differential Effects of Strict Photo ID Laws by Party

	High Confidence (1)	Low Confidence (2)
Strict Photo ID Law	.16** (.06)	-.08 (.05)
Democrat X Strict Photo ID Law	-.01 (.07)	-.01 (.04)
Republican X Strict Photo ID Law	-.06 (.05)	.05 (.05)
Joint Significance for Differential Effects	No	No

Notes: *p<.10; **p<.05; and ***p<.01. Standard errors corrected for clustering at the state level are reported in parentheses. All models include the full set of controls listed in column 4 of Table 2.

Table A7: Differential Effects of Strict Photo ID Laws by Race and Hispanic Ethnicity

	High Confidence (1)	Low Confidence (2)
Strict Photo ID Law	.13*** (.04)	-.05 (.04)
Black X Strict Photo ID Law	-.13 (.08)	-.03 (.05)
Hispanic X Strict Photo ID Law	.21** (.10)	-.15 (.05)
Other Race X Strict Photo ID Law	-.03 (.12)	-.07 (.08)
Joint Significance for Differential Effects	Yes***	Yes*

Notes: *p<.10; **p<.05; and ***p<.01. Standard errors corrected for clustering at the state level are reported in parentheses. All models include the full set of controls listed in column 4 of Table 2.