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STRICT ID LAWS DON'T STOP VOTERS: EVIDENCE FROM A U.S. NATIONWIDE PANEL, 2008–2018*

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Abstract

U.S. states increasingly require identification to vote – an ostensive attempt to deter fraud that prompts complaints of selective disenfranchisement. Using a difference-in-differences design on a 1.6-billion-observations panel dataset, 2008–2018, we find that the laws have no negative effect on registration or turnout, overall or for any group defined by race, gender, age, or party affiliation. These results hold through a large number of specifications. Our most demanding specification controls for state, year, and voter fixed effects, along with state and voter time-varying controls. Based on this specification, we obtain point estimates of -0.1 percentage point for effects both on overall registration and turnout (with 95 percent confidence intervals of [-2.3; 2.1pp] and [-3.0; 2.8pp], respectively), and +1.4pp for the effect on the turnout of non-white voters relative to whites (with a 95 percent confidence interval of [-0.5; 3.2pp]). The lack of negative impact on voter turnout cannot be attributed to voters' reaction against the laws, measured by campaign contributions and self-reported political engagement. However, the likelihood that non-white voters were contacted by a campaign increases by 4.7 percentage points, suggesting that parties' mobilization might have offset modest effects of the laws on the participation of ethnic minorities. Finally, strict ID requirements have no effect on fraud – actual or perceived. Overall, our findings suggest that efforts to improve elections may be better directed at other reforms. *JEL* codes: D72.

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I. INTRODUCTION

A tension exists in democracies between safeguarding the integrity of the vote and ensuring broad participation. Electoral fraud – which takes the form of stuffing ballot boxes, buying or intimidating voters, or impersonating citizens who are deceased, absentee, or no longer in residence – was prevalent in the early decades of Western democracies (e.g., Garrigou, 1992; Lehoucq, 2003; Stokes et al., 2013) and is still widespread in developing democracies today (e.g., Collier and Vicente, 2012). Combating such fraud is critical to build citizen confidence in election results and consolidate democratic regimes (Diamond, 1999; Berman et al., 2019). However, rules pursuing those objectives can also weaken democracy if they keep eligible citizens away from the polling booth. Compounding the matter, legislators have an incentive to push for restrictions if citizens enfranchised by flexible rules will likely vote for rival parties – or oppose restrictions if that will widen their base.

This paper presents empirical evidence on the consequences of strict ID laws in the context of the United States, where the debate on control versus enfranchisement is particularly heated. Between 2006 and 2018, 11 states, mostly with Republican majorities, adopted strict voter identification measures (Hicks et al., 2015).[1] Strict ID laws require voters to present an accepted form of identification document before voting. Voters who fail to do so can cast a provisional ballot but their vote will not be counted unless they present proper ID to election officials within the next few days. In contrast, all other states allow people without ID to vote. They either have a non-strict ID law requesting voters to show an ID but allowing those without it to cast a regular ballot, typically by signing an affidavit; check voters' identity by asking them to sign the poll book and verifying their signature; or simply ask voters for their name and check it against a list of eligible citizens.

The effects of strict ID laws on overall participation are ex-ante ambiguous. While these laws create additional costs for people without ID, those who want to vote can acquire it before the election, and it is unclear what share of non-ID-holders would vote otherwise: groups of voters less likely to hold an ID include Blacks and Hispanics, the young, voters older than 70, and poorer and less educated voters (Barreto et al., 2009; Stewart, 2013; Ansolabehere and Hersh, 2017), who have long shown lower propensity to vote than other groups (Wolfinger and Rosenstone, 1980; Verba et al., 1995; Schlozman et al., 2012; Fraga, 2018). Moreover, some citizens may become more likely to vote if the laws enhance their confidence in the fairness of the election.

Using a nationwide individual-level panel dataset, 2008–2018, and a difference-in-differences (DD) design, we find that strict ID laws have no significant negative effect on registration or turnout,

[1] These states are Arizona, Georgia, Indiana, Kansas, Mississippi, North Dakota, Ohio, Tennessee, Texas, Virginia, and Wisconsin. North Dakota and Texas are the only states that experienced a reversal: both states adopted a strict ID law in 2014, and both laws were struck down by federal courts in 2016. In 2018, North Dakota re-instituted a strict, non-photo ID law.

overall or for any subgroup defined by age, gender, race, or party affiliation. These results hold through a large number of specifications and robustness checks. Our most demanding specification controls for state, year, and voter fixed effects, along with state and voter time-varying controls. Based on this specification, and considering the lower bound of the 95-percent confidence interval, we can rule out that strict ID laws reduce aggregate registration and turnout by more than 2.3 and 3.0 percentage points. Focusing on voters living in adjacent counties across state borders, we can further rule out that the laws reduce their participation by more than 0.5 percentage points.

Most importantly, given the complaints of selective disenfranchisement, strict ID requirements do not decrease the participation of ethnic minorities relative to whites. The lower bound of the 95-percent confidence interval from our voter fixed effects regression rules out that the laws decrease non-white turnout (relative to white) by more than 0.5 percentage points. Focusing specifically on Black voters, we can rule out that strict ID laws reduce their turnout by more than 1.3 percentage points, relative to white, and by more than 3.1 percentage points in total.

Strict ID laws' overall effects do not increase over time, they remain close to zero and non-significant whether the election is a midterm or presidential election, and whether the laws are the more restrictive type that stipulate photo IDs. Our identification assumption is that treated states (which adopted a strict ID law between 2008 and 2018) would have experienced the same changes in turnout as other states, absent the treatment. We find that voters in treated states did have different turnout levels prior to the laws, but they did not show different participation *trends* than others, lending support for our identification strategy. Finally, in line with the lack of negative effect on the participation of any subgroup of voters, strict ID laws do not affect the relative vote share of Democratic and Republican candidates either.

These results contrast with the large participation effects of other dimensions of election administration: voter registration laws (Rosenstone and Wolfinger, 1978; Braconnier et al., 2017), convenience voting (Gerber et al., 2013a; Hodler et al., 2015; Kaplan and Yuan, 2019), voting technology (Fujiwara, 2015), and distance to polling station (Cantoni, 2020). It could be that our null findings reflect two mutually opposing forces: the laws' negative effect on participation versus a reaction of voters against a threat to their right to vote (Citrin et al., 2014; Biggers and Smith, 2018). We do not find evidence of such backlash on the part of voters. Strict ID laws have no significant effect on total campaign contributions, measured using administrative records from Bonica (2018), or on an index of voter activity aggregating people's self-reported having donated to a candidate, the amount donated, their having attended a political meeting, put up a campaign sign, and volunteered for a campaign, all measured using the Cooperative Congressional Election Study surveys. However, the laws increase the likelihood that non-white voters report being contacted by a campaign by 4.7 percentage points, suggesting that parties and candidates who fear they might lose votes as a result of strict ID requirements mobilize their supporters around this issue. These mobilization efforts might have offset small direct negative effects on the participation of ethnic minorities.

In a 2017 review of the literature, Highton notes that contemporary concerns and controversies about voter identification requirements date back to the adoption of Indiana and Georgia's strict ID laws in 2005, but he finds only limited evidence about the effect of this type of laws on turnout (Highton, 2017). Early studies based on cross-state comparisons were unable to isolate the effect of strict ID laws (which, again, are characterized by the fact that they prevent citizens without identification from voting) due to the relative recency of these laws and to the slow increase in the number of states enforcing them. Instead, these studies focused on other types of voter identification requirements or, to address the issue of the then low number of states enforcing strict ID laws, pooled together strict ID laws with other methods of voter identification. Estimates ranged from negative effects, overall or specifically for ethnic minorities (de Alth, 2009; Vercellotti and Andersen, 2009), to null (Muhlhausen and Sikich, 2007; Mycoff et al., 2009; Rocha and Matsubayashi, 2014) or even positive effects (Larocca and Klemanski, 2011). Alvarez et al. (2008, 2011) are the first to estimate the effects of strict ID laws specifically. They find a voter turnout difference of two percentage points between states with strict laws and states simply verifying voters' name. However, this difference is imprecisely estimated since the most recent data analyzed in the study are from 2006, the first general election in which strict ID laws were ever implemented. Using similar data, Erikson and Minnite (2009) conclude that the effect of strict ID laws is not significantly different from zero. Government Accountability Office (2014) finds excess average turnout declines of up to 3.2 percentage points in two states that implemented strict photo ID laws between 2008 and 2012, compared to states that did not change their voter identification requirements, and larger drops among Blacks than among whites and Hispanics. Pryor et al. (2019) and Hajnal et al. (2017) use data going until 2014, and they respectively report negative turnout effects of strict ID laws across all races, and disproportionately large and negative effects on the participation of Blacks and Hispanics.

We improve on this literature in three critical ways. First, existing estimates rely on state-level turnout aggregates, which make estimating heterogeneous effects by voter characteristics difficult, or on national surveys, which have limited representativeness and accuracy. National surveys' samples can fail to reflect state voting populations; voters' likelihood to respond can differ across groups; and their turnout data are based on self-reports, which are untrustworthy (Silver et al., 1986; Ansolabehere and Hersh, 2012), or they use validation procedures which vary across states and over time (Grimmer et al., 2018). By contrast, we use administrative records of individual registration and turnout. Our data, collected by the political data vendor Catalist, combine official voter registration and turnout records from all states and cover the near universe of U.S. voting-age individuals, 2008–2018, resulting in a total of more than 1.6 billion observations. This comprehensive individual-level dataset enables us to accurately measure the effects of strict ID laws for different subgroups, which is critical given the concern of differential negative impact on ethnic minorities. In addition, the fact that the data follow individuals over time allows us to test the robustness of the results to specifications controlling for voter fixed effects and estimating the laws' impact out of individuals who faced them for some but not all years.

Differently from the rest of the literature, Hood and Bullock (2012) and Esposito et al. (2019) use individual-level administrative data and difference-in-differences designs like we do. They find that the participation of voters without photo ID decreased relative to voters with ID following the implementation of new voter identification requirements in Georgia and Rhode Island in 2008 and 2014, respectively. However, unlike our analysis, these studies are each restricted to a unique state. Since all individuals in their sample experienced the new law in the post period, these papers' estimates correspond to the differential effects of the law for people without photo ID. But people with ID may also be affected by changes in voter identification requirements, as discussed in Section II.2. Therefore, the *relative* decline in the participation of voters without ID reported in these papers is consistent with *overall* negative, null, or even positive turnout effects of the law change. By contrast with Hood and Bullock (2012) and Esposito et al. (2019), our estimates compare turnout changes in states which adopted a strict ID law with states which did not and, therefore, they capture total, not differential effects. On the other hand, unlike these papers, our data do not allow us to distinguish people who were initially with or without ID.

Second, except for Esposito et al. (2019), prior research has examined the effects of ID laws using samples of registered citizens only, neglecting possible effects on voter registration (citizens who expect not to be able to vote may not register in the first place), and possibly obtaining downward biased estimates of the laws' effects on turnout (if citizens deterred from registering and absent from the sample have a low propensity to vote). By contrast, Catalist data include unregistered voters, allowing us to measure effects on both registration and turnout.

Third, previous papers have used unconvincing or untestable identification assumptions, such as cross-sectional regressions or DD regressions with only two cross-sections. We use the full length of our panel to show parallel pre-trends and bring support for the identification assumption underlying our design; we demonstrate the robustness of our estimates to alternative specifications including state and voter controls, linear state time trends (or state-by-year fixed effects, for heterogeneous effects), and voter fixed effects; and we show that our results hold when comparing voters in contiguous county-pairs straddling a state border, which further enhances the causal credibility of our estimates. This alternative estimation strategy requires restricting the sample to adjacent counties in neighboring states and including county-pair-by-year fixed effects. It is only possible because our dataset provides the location of each individual and contains a sufficiently large number of people living in these counties, thanks to its near-universal coverage of the U.S. voting-age population. We also show that our results remain very similar using novel estimators proposed by de Chaisemartin and D'Haultfœuille (2020a) and Sun and Abraham (2020) to address possible shortcomings of two-way fixed effects estimators. Finally, while the control group of our main regressions includes all states without strict ID laws, we also estimate specifications distinguishing all types of identification requirements. These regressions allow to compare strict ID laws to non-strict laws, thus isolating the effect of the one characteristic of strict laws that is most susceptible of raising voting costs: requiring voters to show an ID to be able to vote. Again, we find effects which are close to null and not statistically significant.

Other studies also based on administrative data consider non-strict ID law states, which request but do not require voters to present an ID and record ballots cast without identification. These studies use counts of people voting without ID to estimate how many voters would be disenfranchised by a shift to a strict ID law (Henninger et al., 2020; Hoekstra and Koppa, 2019). While ingenious, this method may severely overestimate the effects of strict laws. Many of the people voting without identification under a non-strict law actually have a valid ID (Henninger et al., 2020) and would bring it to the polls if required, and some of those without ID could acquire one before the election. Beyond the approximations required to estimate the direct effects of strict laws, descriptive analyses of the prevalence of voting without identification suffer from a second important limitation: they do not take into account indirect effects that may result from increased trust in the electoral process, anger against the laws, countermobilization efforts, and other mechanisms discussed in Section II.2. In contrast, we estimate the net overall effect of strict ID laws and we exploit variation from all states which have adopted them.

Furthermore, we give evidence on both sides of the debate: while most existing research has focused on the effects of strict ID laws on participation, we also measure their effects on voter fraud – the laws' ostensive target. Research has shown that interventions such as deploying observers (Ichino and Schündeln, 2012) or informing voters (Vicente, 2014) can successfully reduce fraud in contexts where it is prevalent. Even if fraud is much more limited in the United States, the extensive attention paid to existing cases could make any reduction consequential. We use two datasets listing cases of voter fraud: one by the Heritage Foundation, a conservative think tank, and another one by News21, a more liberal initiative. We find no significant negative effect in either dataset. Irrespective of any effect on fraud, the very existence of stricter controls at polling places could be perceived as an improvement in election administration and increase voter confidence (Norris, 2004; Atkeson and Saunders, 2007). Stewart et al. (2016) uses the Survey of the Performance of American Elections to show that perceived occurrence of different types of fraud is similar in states with and without strict ID laws. Using the same survey, our DD estimates show no significant impact on this outcome. In addition, we use the American National Election Studies surveys to measure the laws' impact on citizens' belief that elections were fair. Again, we find no significant effect.

Our finding that voter ID laws have null effects is particularly salient in the United States, given the country's history of balancing the threat of fraud against the promise of enfranchisement. Well into the 19th century, political parties took advantage of the lack of control over the identity of people coming to vote. They hired large groups of "repeaters," who walked from one polling place to another and voted over and over again (Converse, 1972). After 1890, many states addressed widespread fraud by requiring citizens to prove their identity and eligibility and sign a register before voting. Registration laws reduced voter impersonation, as voters' signatures could be verified on Election Day, and the registers were frequently purged of nonresidents and the deceased. However, they also created an additional burden for eligible voters, which has prevented many from participating in elections ever since (Nickerson, 2015). Conversely, voting by mail, early voting, and other forms of convenience voting, which have become more widespread since the turn of the century, facilitate participation (e.g., Gerber et al., 2013a) but are more susceptible to fraud than in-person voting on Election Day (Gronke et al., 2008).

Over the last decade, strict ID laws have become one of the country's most polarizing issues (Hasen, 2012): they are supported by a large majority of the overall population, but with a growing gap between Republicans and Democrats (Stewart et al., 2016). Advocates and opponents of these laws disagree both on their benefits and costs.

On benefits, advocates insist that electoral fraud still exists today – about one third of Americans believe it is widespread (Kobach, 2011; Richman et al., 2014). They argue that strict ID laws are required to deter voter impersonation, double-voting, and non-citizen voting, and to boost public confidence in the integrity of elections (von Spakovsky, 2012). Opponents argue that voter fraud, extremely rare, results from individual cases of initiative or error rather than a coordinated effort (Minnite, 2010; Cottrell et al., 2018). On costs, advocates of strict laws argue that they impose only a minor burden on voters, as proof of identification is also required for other activities, like cashing a check. They point to the fact that most other Western democracies also require voters to show identification (Commission on Federal Election Reform, 2005). Opponents observe that, unlike other countries, the United States does not require its citizens to hold a national ID card, (Schaffer and Wang, 2009), and as a result 5 to 19 percent of eligible voters (depending on the state) lack any accepted form of identification (Government Accountability Office, 2014; Ansolabehere and Hersh, 2017). They see these laws as a deliberate and politically motivated attempt to disenfranchise minorities, akin to the poll taxes, literacy tests, and other Jim Crow legislation prevalent before the 1965 Voting Rights Act (Rocha and Matsubayashi, 2014). The laws are enforced more stringently against Blacks and Hispanics (Atkeson et al., 2014; White et al., 2015), who favor the Democratic Party and are less likely to hold an ID in the first place.

Our results suggest that efforts both to safeguard electoral integrity and enfranchise more voters may be better served through other reforms.

The remainder of the paper is organized as follows. Section II summarizes the history of strict ID laws and outlines the main mechanisms through which these laws may affect participation and other outcomes. Section III provides more information on Catalist's voter-level panel data and the other datasets we use. Section IV presents the empirical specifications and results. Section V concludes.

II. RESEARCH SETTING

II.1. History of Strict ID Laws

In the U.S., laws requiring voters to present a document verifying their identity are relatively recent. In 1950, South Carolina became the first state to request – but not require – voters present an ID at the polls. By 2000, 14 states had adopted a similar law, under both Democratic and Republican majorities, without generating much discussion. New voter identification requirements were adopted as part of election-reform efforts following the disputed 2000 presidential election and the ensuing anxiety on electoral integrity (Minnite, 2012). In 2002, Congress passed the Help America Vote Act, which prescribed that first-time voters who registered by mail show identification at the polling place, but refrained from establishing uniform ID requirements for other voters (Ansolabehere, 2008). In 2005, the bipartisan Commission on Federal Election Reform recommended the adoption, at the federal level, of a photo voter-ID card (Carter-Baker Commission, 2005). Soon afterwards, Georgia and Indiana became the first states to require a photo ID at the polls. In 2008, the Supreme Court upheld the constitutionality of Indiana's law in *Crawford v. Marion County*, thereby paving the way for the implementation of similarly restrictive ID laws in other states, mostly by Republican-controlled legislatures (Hicks et al., 2015; Biggers and Hanmer, 2017).

Following the National Conference of State Legislatures (NCSL), we distinguish between two main categories of ID laws: strict and non-strict. In states with non-strict laws, voters are asked to show an ID, but are still allowed to vote without identification. For their ballot to be counted, voters without ID simply need to sign an affidavit identifying themselves (in most states) or have their signature checked against the voter registration record. In contrast, strict ID laws (such as Georgia and Indiana's current laws) require all voters to show an ID. People without one may cast a provisional ballot, but this ballot will only be counted if they return within a few days to the polling place, election board, or county election office to show an accepted form of identification. In other words, citizens without ID are prevented from voting.[2] Strict ID laws further differ by the

[2]The distinction between states requesting vs. requiring an ID is generally straightforward. However, one state is at the limit between these two categories: Alabama. The NCSL classifies Alabama's ID law as non-strict because

type of ID they consider valid. While some accept a wide range of documents, including utility bills or bank statements, most require a document bearing a photo, such as a driver's license, state-issued ID card, or U.S. passport, and are therefore referred to as strict photo ID laws. Online Appendix Table A.1 details the requirements associated with each strict ID law enforced in at least one general election.

Due to their restrictive nature, strict ID laws are very controversial and they have come under immense scrutiny by state and federal courts, as well as by the U.S. Supreme Court. In addition to its 2008 judgment ruling Indiana's strict ID law as constitutional, the Supreme Court effectively upheld a federal court's ruling that Wisconsin's strict ID law was constitutional when it rejected a challenge to this law in 2015. By contrast, in 2017, it declined to hear an appeal to a federal court's striking down a strict law adopted but not implemented by North Carolina, thereby allowing the federal court's decision to stand. Beyond courtrooms, strict ID laws have generated heated partisan debates and received large media coverage and public interest.

States without any ID law do not request, let alone require, any identification document. They verify voters' identity in either of the two following ways. Some states ask voters to sign the poll book or an affidavit of vote eligibility and, in some cases, ask poll workers to verify that this signature matches the one on file. Others simply check voters' name (and, sometimes, other personal information such as voters' address) against a list of eligible citizens.[3]

Online Appendix Figures A.1 and A.2 plot the overall distribution of the four types of voter identification requirements (strict ID law, non-strict law, signature, and checking voters' name) as well as the requirements enforced in each state and general election since 2004. The most important shift in this period is the implementation of strict ID laws by a growing number of states and the simultaneous decline in the number of states with non-documentary ID requirements.

II.2. Conceptual Framework

Strict ID laws are commonly hypothesized to have negative turnout effects by increasing the cost of voting (Highton, 2017), which is a low-benefit activity (Downs, 1957; Riker and Ordeshook, 1968). However, other indirect mechanisms make the overall effects of the laws ex-ante ambiguous.

To the extent that strict ID laws decrease participation by preventing eligible citizens without ID from voting, minority voters and other groups who are less likely to have an ID should be the most impacted. However, this effect will be reduced if people without ID are willing to spend the

people without ID can vote if they are identified by two election officials. It remains that voters without ID who are not identified by election officials are prevented from voting. For that reason, some studies which otherwise follow the NCSL classification count Alabama as a strict ID law state (e.g., Highton, 2017; Kuk et al., 2020). Relabeling Alabama's law as strict would not affect our results, since we control for state fixed effects and Alabama's request to show identification dates back to 2003 (i.e., before our sample period).

[3] See https://www.ncsl.org/research/elections-and-campaigns/voter-verification-without-id-documents.aspx. Accessed: January 15, 2021.

time (and, sometimes, the money) required to obtain an ID or if their propensity to vote is low even absent any ID requirement.

Beyond administrative costs, strict ID laws also create information costs for all voters. Whether or not they have an ID, all voters need to be aware that a new law was implemented and they need to learn which forms of identification are accepted. If they are unaware of the ID requirement, voters who possess a valid ID may not bring it to the polling station. In that case, they will be asked to return with the document for their vote to be counted, and only a subset of voters will do so. Others may wrongly believe their ID is not accepted and thus refrain from even trying to vote.

Several forces may reduce these costs or mitigate their effects. First, states implementing strict ID laws may conduct educational campaigns to inform voters and they may facilitate the acquisition of state-issued IDs (e.g., Hopkins et al., 2017; Bright and Lynch, 2017). Second, Democratic candidates and interest groups opposing strict ID laws may respond strategically by conducting outreach information programs and helping people obtain proper identification (Citrin et al., 2014; Neiheisel and Horner, 2019). In addition, they may use the laws as an argument to mobilize their entire base, including voters who are not personally affected (Endres and Panagopoulos, 2018). Third, media coverage asserting that the goal of the laws is to disenfranchise some citizens may cause anger among voters who feel their group or their party is targeted, thus increasing turnout among these voters (Valentino and Neuner, 2017; Smith et al., 2020).

On net, the effects of the laws on Democratic turnout may be null or even positive if these different responses are sufficiently strong. Differences across groups of voters in the strength of the mechanisms through which strict ID laws affect turnout might generate heterogeneous effects. In addition, these effects may change over time. Early declines in participation may subside as voters learn about the laws, or negative effects may appear after a few years if countermobilization weakens gradually.

On the opposite side of the aisle, Republican voters may become more likely to vote if the laws increase their confidence in election integrity (Endres and Panagopoulos, 2018) and if enhanced trust in elections, in turn, boosts participation. The decision of the Supreme Court in *Crawford v. Marion County* draws the latter connection when it asserts that perceptions of voter fraud depress turnout, but we are not aware of any empirical evidence establishing this relationship. An experiment by Gerber et al. (2013b) studies beliefs on ballot secrecy, not voter fraud, and shows that improving these beliefs causes participation to increase. It is possible that other policies also affect turnout if they improve trust in elections.

Finally, the participation of Democrats and Republicans may endogenously adjust to the expected level of participation of the other side, a mechanism highlighted for instance in group rule-utilitarian models by Coate and Conlin (2004) and Feddersen and Sandroni (2006). Such strategic response may amplify the aforementioned effects, whether they are positive or negative. For instance, Republicans may be less likely to vote if they expect the laws to reduce the participation of Democrats and infer that the number of votes required to obtain a plurality is now lower.

Beyond voter turnout, the laws may also affect vote shares and election outcomes, if they have different overall effects on the participation of Democratic- and Republican-leaning voters. Moreover, strict ID laws have become such a politicized issue that some voters in implementing states may change the orientation of their vote if, on this particular issue, they disagree with the party they usually vote for. Substantial impacts on voter fraud are perhaps less likely, given the low baseline level of fraud (Minnite, 2010).

We estimate the impact of strict ID laws on these different outcomes (participation, vote shares, and voter fraud), and we unpack net effects on participation by examining subsets of voters defined by race or party affiliation, studying changes in effect size over time, and checking whether the laws generated backlash or countermobilization efforts.

III. DATA

III.1. Catalist Voter-Level Panel Data

We measure voter turnout and registration using a novel individual-level panel dataset collected by Catalist, a U.S. company that provides data and data-related services to progressive organizations and has a long history of collaborating with academics (e.g., Nickerson and Rogers, 2014; Hersh and Nall, 2016). The panel covers the near universe of the U.S. voting-eligible population in the 2008, 2010, 2012, 2014, 2016, and 2018 presidential and midterm elections, resulting in a total of about 1.6 billion observations.

For each voter-election, the data report state and county of residence, registration status, voter turnout, and party affiliation (in the 30 states in which it is available). The data also contain age, race, and gender. These demographic characteristics are available for nearly all voters and have been shown to be very reliable (Fraga, 2016, 2018). In eight states – Alabama, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, and Tennessee – Catalist uses self-reports of race that come directly from the voter rolls. For unregistered voters in these eight states and all voters in other states, Catalist estimates race using voters' full names, socio-demographic information about their census block groups or tracts of residence, and, where available, self-reported race from commercial and nonprofit databases. According to Fraga (2018), the average accuracy of Catalist's proprietary race model is very high (93.1 percent), with race-specific accuracy of 77.1, 79.8, and 97.8 percent for Black, Hispanic, and white voters, respectively.[4] Next to race, the Catalist

[4]These estimates indicate the fraction of 2016 CCES respondents matched to Catalist registration records with 90 percent match confidence or greater and self-identifying with the indicated racial/ethnic group who have the same race/ethnicity listed in the Catalist database.

data contain a categorical variable for the degree of confidence in a voter's race estimate (featuring five possible values: "highly likely," "likely," "possibly," "uncoded," and "no code assigned"). For example, Catalist predicts some voters' races with a relatively higher degree of confidence when they reside in racially homogeneous areas or when they carry racially distinctive names (Hersh, 2015). Online Appendix Table A.11 shows that race-specific impact estimates remain very close to those of Table III if we restrict the sample to voters whose race is estimated with highest confidence. This indicates potential race misclassification is unlikely to bias our results.

Catalist's data on registered voters primarily come from official voter registration and turnout records from all states. In addition, about 55 million unregistered voters are covered thanks to three different data sources. First, Catalist keeps track of voters present in past voter files and absent from the most recent one. Second, it identifies unregistered voters using information from data aggregation firms (so-called "commercial data") and customer files of retailers and direct marketing companies. Finally, unregistered voters include individuals who moved to a state without registering, according to commercial data or USPS National Change of Address data (NCOALink®).

Despite Catalist's efforts and multiple data sources, coverage of the unregistered population is likely incomplete: Jackman and Spahn (2018) estimate that at least 11 percent of the adult citizenry – and a disproportionate share of minority voters – do not appear in commercial voter lists like Catalist's. This generates the following risk. Suppose some voters only register absent strict ID laws. We will observe all these marginal registrants in states without ID requirements – as the data cover the universe of the registered population – but might only observe a subset of them in states with ID requirements – as they would not register in these states and coverage of the unregistered population is incomplete. Under this scenario, our estimated registration effects would be biased upward as we would underestimate the share of unregistered voters in state-years with strict ID laws. Reassuringly, Online Appendix Table A.3 shows that the probability of voters appearing in or disappearing from the Catalist data is (conditionally) orthogonal to the presence of strict ID laws. Specifications controlling for voter fixed effects further assuage this concern since they estimate the effects out of individuals who faced a strict ID law for some but not all years. These individuals are present in our sample before the implementation of the law, reducing the risk of sample selection bias.

Another potential issue is that some unregistered individuals in Catalist data may be ineligible to vote. Yet, it seems implausible that the implementation of strict ID laws correlates systematically with the presence of ineligible voters in the data. In addition, Table I and Online Appendix Table A.12 show that our results hold when we restrict attention to registered voters, all of whom should be voting-eligible individuals. Furthermore, Online Appendix Figure A.3 plots the relationship between total state-by-year headcounts in the Catalist data and estimates of the citizen voting-age population from the United States Census Bureau. The nearly perfect linear correlation between the two variables shown in the figure ($R^2 = 0.986$) indicates that variations in headcounts in the Catalist data across states and years nearly perfectly mirror underlying fluctuations in the citizen voting-age population, thus alleviating concerns that our data do not adequately reflect the population of interest.

Further details on the Catalist panel data are given in Online Appendix Section 1.2.

III.2. Data on Mobilization and Campaign Contributions

Measures of campaign contact and voter engagement come from the 2006—2018 post-electoral Cooperative Congressional Election Study (CCES) surveys. We use questions on whether the interviewee was contacted by a campaign, donated to a candidate or campaign (and how much she contributed), attended a political meeting, posted a campaign sign, or volunteered for a campaign.[5] We also construct a summary index of voter activity, defined to be the equally weighted average of the z-scores of its components. An important caveat is that survey data on campaign activities may suffer from misreporting, due for instance to social desirability bias or misremembering. Misreporting would bias our estimates if its prevalence changes differentially across treated and control states following the implementation of strict ID laws.

Information on state-level campaign contributions is from Bonica (2018)'s Database on Ideology, Money in Politics, and Elections (DIME), version 3.0. The data contain all political contributions recorded by the Federal Election Commission, 2004–2018. We compute the total dollar-value contributed by residents of each state in each election cycle, normalize it by the state population in that election year, and take the log, to reduce the impact of outlier states like New York.

Data on total expenditures and campaign-related expenditures by candidates running to the U.S. House of Representatives, 2004–2018, are also based on records from the Federal Election Commission, and compiled by the Center for Responsive Politics. We also obtained data on estimated TV ad expenditures spanning most down-ballot, state, and federal electoral races held in 2004 and 2008–2018 from the Wisconsin Advertising Project and the Wesleyan Media Project.[6] Similarly as for total contributions, we also measure total expenditures, campaign-related expenditures, and TV ad expenditures in logs after normalizing by the state population.

[5]For all survey data we use, exact questions are detailed in Online Appendix Section 1.3. Beyond questions on campaign contact and voter engagement, we also use the CCES surveys to check the robustness of the effects on turnout estimated with the Catalist data. These results are shown in Online Appendix Tables A.13 and A.14, and discussed in Section IV.2.

[6]See `https://elections.wisc.edu/wisconsin-advertising-project/` and `https://mediaproject.wesleyan.edu/`, both accessed January 15, 2021. Estimated expenditures on TV ads for down-ballot races are available for the 2010–2018 elections, while expenditures for congressional, gubernatorial, and presidential races are available starting from 2004. To focus on general elections (instead of primaries), we restrict attention to TV ad expenditures occurring in even-numbered years from June onwards.

III.3. *Voter Fraud*

Measuring voter fraud represents a challenge, as federal and state agencies vary in the extent to which they collect and share information on it (Government Accountability Office, 2014).

We found two datasets covering reported cases of voter fraud. The first is by News21, an investigative project funded by the Carnegie Corporation and the John S. and James L. Knight Foundation. For the project, 24 students from 11 U.S. universities submitted more than 2,000 public-records requests and combed through nearly 5,000 court documents, official records, and media reports about voter fraud. The result is a collection of 2,068 cases of suspected voter fraud reported from 2000 through 2012. The database is admittedly incomplete, as the research team received partial or no responses from several states, and even replying jurisdictions may have failed to include some cases.[7] The second dataset, by the Heritage Foundation, includes 1,277 proven cases. Again, the Foundation's website indicates that this database is non-exhaustive.[8]

We define two outcomes separately in either dataset: the number of fraud cases documented in each state-year per 100,000 residents, and the number of cases potentially preventable by strict identification requirements.[9] We restrict attention to cases of fraud reported in or after 2004, the last election year before the implementation of the country's first strict ID law.

In both datasets, the summaries are typically insufficient to reconstruct the election year the alleged fraud took place. We thus take the reported years as given. We assign records with odd years (i.e., years in which no general election took place) to the previous year's treatment status and covariates.

Despite their limitations, these two datasets allow us to propose the first estimates of the effect of strict ID laws on voter fraud.

III.4. *Surveys on Perceived Election Integrity*

To assess if strict identification laws alter the perceived integrity of the electoral process, we use the 2004, 2012, and 2016 waves of the American National Election Studies (ANES) survey and the 2008–2016 waves of the Survey of the Performance of American Elections (SPAE). From the ANES, we construct a dummy identifying respondents who think the past election was very fair or fair. From the SPAE, we construct separate dummy outcomes for whether the respondent believes the following frauds happen commonly or occasionally: pretending to be another voter, casting multiple votes, non-citizens casting a ballot, casting an absentee ballot intended for another person,

[7] Further details on News21 are available here: `https://votingrights.news21.com/article/election-fraud-explainer/` Accessed: March 5, 2020.

[8] See `https://www.heritage.org/voterfraud`. Accessed: March 5, 2020.

[9] We classify voter impersonation, duplicate voting, false registrations, and ineligible voting as preventable frauds. Other categories are buying votes, altering the vote counts, fraudulent use or application of absentee ballots, illegal assistance at the polls, and intimidation.

officials changing the vote counts, stealing or tampering with ballots. As with voter activity, we construct a standardized index of perceived election integrity based on the individual voter-fraud outcomes.

III.5. Calendars of Voter Identification Requirements, Election Laws, and State Party Control

We identify the type of voter identification requirement enforced in each state-year based on information provided by the NCSL. We also use the NCSL, together with data from Biggers and Hanmer (2015), to construct the following state-level covariates. We build state-by-year indicators for the availability of no-excuse absentee voting, early voting, all-mail voting, and Election-Day registration. Partisan control of the state legislature is identified by three dummies indicating whether the state legislature was controlled by Republicans, Democrats, or its control was split among the two main parties.[10] Similarly, the party affiliation of the governor can take three possible values, Democratic, Republican, and independent.[11]

IV. RESULTS

IV.1. Impact on Turnout

We first estimate the average impact of strict ID laws on all voters with DD specifications of the following form:

$$Y_{ist} = \beta ID_{st} + X_{ist}^{'}\gamma + \alpha_s + \delta_t + \mu_{ist}, \tag{1}$$

where Y_{ist} is a dummy equal to 1 if individual i in state s voted in election year t, ID_{st} is a dummy for whether the state used a strict ID law in that year, X_{ist} is a vector of individual and state controls, α_s are state fixed effects, and δ_t election year fixed effects. Our individual controls include both time-invariant (gender as well as race-by-state fixed effects) and time-varying covariates (age as well as race-by-year fixed effects). All our state controls are time dependent (partisan control of the state legislature, governor's party, and other election administration rules affecting turnout: no-excuse absentee voting, early voting, same-day registration, and all-mail voting). Since the treatment varies at the state-year level, we follow Bertrand et al. (2004) and conservatively cluster standard errors by state.[12]

[10] We include Nebraska's non-partisan state legislature in the final category.

[11] We include the District of Columbia in the final category.

[12] Online Appendix Tables A.32–A.36 and A.37–A.41 show that the state-clustered asymptotic p-values of Tables I–V's coefficients are very close both to their wild cluster bootstrap counterparts (Cameron et al., 2008) and to the randomization inference p-values based either on t-statistics or on regression coefficients (MacKinnon and Webb, 2020).

The coefficient of interest, β, measures the difference in average participation between states with and without strict ID laws (henceforth, treated and control states), conditional on controls. This represents the causal impact of the laws under the assumption that treated and control states were on parallel trends, so that year-to-year turnout changes in control states correspond to the counterfactual evolution in treated states, had they not implemented the law.

The results from equation [1] are presented in Table I. Panel A restricts the sample to registered citizens, following the existing literature. Using a specification with state and election-year fixed effects but without any other control, we obtain an effect close to null and not statistically significant (column (1)). Angrist and Pischke (2015) suggest that credible DD estimates should be robust to the inclusion or omission of covariates and linear state time trends. Accordingly, we test the robustness of our result to three additional specifications.

Namely, our second specification includes individual and state controls. Our third specification also adds state time trends, to allow treated and control states to be on differential linear trajectories. While controlling for state time trends relaxes our identification assumption, it also decreases the precision and accuracy of the estimates for at least two reasons. First and most importantly, using linear time trends in DD specifications is a source of bias. Neumark et al. (2014), Meer and West (2016), and Goodman-Bacon (2019) note that with time-varying treatment effects, linear time trends tend to absorb part of the effect of interest (i.e., to "overfit"), thus leading to attenuation bias. Goodman-Bacon (2019) also points that controlling for time trends implicitly over-weights observations at the end of the panel, adding another source of bias (of a-priori unknown direction and magnitude). Second, controlling for linear trends reduces the available treatment variation, making resulting estimates less precise than un-detrended ones. These caveats mean that results obtained using the third specification should be interpreted with caution. Our fourth and most demanding specification includes voter fixed effects. While identification continues to rely on states that changed voter identification requirements, this specification estimates the impact using only within-individual variation, out of voters who faced a strict ID law for some but not all years (because they experienced a change in their state's law or because they moved across states with different voter identification requirements and their state of origin or destination is one of the states which adopted a strict ID law after 2008). Corresponding estimates are unaffected by the possibility that strict ID laws changed people's likelihood to appear in the Catalist sample, which is otherwise a possible source of bias as discussed in Section III.1. We find no significant effect in any of these alternative specifications (columns (2) through (4)).

In Panel B, we use the same specifications as in Panel A but include both registered and unregistered individuals in the sample, which the existing literature has typically failed to do. This is important, first, because effects on the turnout of registered citizens shown in Panel A miss possible effects on registration: while strict ID laws do not change registration requirements, citizens who expect not to be able to vote might decide not to register in the first place, and citizens who stop voting are more likely to be purged from voter rolls. In addition, restricting the sample to registered voters might lead us to underestimate the laws' true effects on turnout if they decrease registration of citizens with lower propensity to vote than the average registrant. In other words, the estimated null effect on registered voters' turnout could reflect two negative effects: decreased registration (leading to increased turnout of registered citizens, if those deterred from registering have low propensity to vote) and decreased turnout of voters whose registration is unaffected. The inclusion of both registered and unregistered individuals in Panel B addresses both issues. The results reported in this panel are thus our main estimates of the effects of strict ID laws on overall participation.

Panel B considers two outcomes: unconditional turnout (equal to 1 if the individual is registered and votes, and 0 otherwise), in columns (1)–(4), and registration, in columns (5)–(8). The effects of strict ID laws on both outcomes are close to null and point estimates are not statistically significant in any specification. Based on our most demanding specification controlling for state, year, and voter fixed effects, along with state and voter controls, and considering the lower bound of the 95-percent confidence interval, we can rule out that strict ID laws reduce aggregate registration and turnout by more than 2.3 and 3.0 percentage points, respectively (columns (4) and (8)). The precision of our estimates is comparable across specifications.

[Table I about here]

In Online Appendix Table A.4, we implement an alternative strategy based on Dube et al. (2010). We restrict our sample to adjacent counties in neighboring states to compare voters in contiguous county-pairs straddling a state border. Focusing on voters living in adjacent counties across state borders (and controlling for county-pair-by-year fixed effects) further enhances the causal credibility of our estimates. In this table as well as in the remaining analysis on turnout, we use unconditional turnout on the full sample as our outcome, unless specified otherwise. Again, we find no effect of strict ID laws on turnout. Considering the lower bound of the 95-percent confidence interval, we can rule out that strict ID laws reduce overall turnout by more than 0.5 percentage points.

Table II, Panel A, shows the robustness of the null result to different data. Specifically, instead of using individual-level turnout data, we use McDonald's aggregate state-level estimates, whose denominator for turnout excludes non-citizens and ineligible felons (McDonald and Popkin, 2001; McDonald, 2002, 2010). Since the share of ineligible voters fluctuates wildly across states and over time, McDonald's turnout estimates are considered more reliable than alternative measures using the Census Bureau voting-age (or citizen voting-age) population, and are widely used (e.g., Leighley and Nagler, 2013; Burden, 2014; Taylor et al., 2015; Fraga, 2018). We use McDonald's data for

2004–2018, since 2004 is the last year before Arizona, Indiana, and Ohio became the first states in the country to implement a strict ID law.[13] Also this strategy confirms the null result. Similarly, we do not find any significant effect on aggregate state-level registration rates, 2008–2018, computed as counts of registered voters in the Catalist data divided by McDonald's figures for the voting-age or voting-eligible population (Online Appendix Table A.6).

[Table II about here]

While regressions with time and state fixed effects in the form of equation [1] are widely used, a recent literature documents possible shortcomings of these two-way fixed effects specifications (Borusyak and Jaravel, 2017; Goodman-Bacon, 2019; de Chaisemartin and D'Haultfœuille, 2020a; Sun and Abraham, 2020; Callaway and Sant'Anna, 2020). In particular, de Chaisemartin and D'Haultfœuille (2020a) show that the underlying estimator can be written as a weighted sum of the average treatment effects in each state and period, with some possibly negative weights. When treatment effects vary over time or across states, negative weights may result in a negative estimate even if all the average treatment effects are positive. Reassuringly, using de Chaisemartin and D'Haultfœuille (2020a)'s *twowayfeweights* Stata command, we find that less than one third of the weights are negative and that their sum is only 0.087. Furthermore, Online Appendix Table A.7, Panel A (resp. A.8, Panel A) checks the robustness of the results obtained with the Catalist data (resp. the McDonald's aggregate state-level turnout estimates) to alternative estimators proposed by de Chaisemartin and D'Haultfœuille (2020a) and Sun and Abraham (2020). Columns (1) and (2) report the estimated effects in the first election after the implementation of strict ID laws, and columns (3) and (4) the aggregate effects across all elections post implementation. The point estimates are very close in magnitude to our baseline estimates, and none of them is statistically significant.[14]

[13] As shown in Online Appendix Table A.5, we obtain very similar results when using the voting-age population instead of the voting-eligible population as denominator (Panel A, columns (5) through (8)) or when using McDonald's turnout data for 2008–2018, the period corresponding to the Catalist sample, instead of 2004–2018 (Panel B).

[14] We use the Stata *did_multiplegt* command to compute de Chaisemartin and D'Haultfœuille (2020a)'s estimator and run a linear regression interacting relative year fixed effects with cohort fixed effects to compute the estimator by Sun and Abraham (2020). Our design includes three cohorts, each designating a group of states which first implemented their strict ID law in the same year: 2012, 2014, and 2016. Cohort-specific relative year fixed effects are then aggregated using weights which correspond to the share of observations of that relative year that fall in that cohort. Sun and Abraham (2020)'s method does not provide a clear way to aggregate relative year fixed effects across years, so we only show the effects in the first election after implementation of the law. We compare the estimates obtained with these two estimators to two sets of estimates obtained with the two-way fixed effects estimator: estimates based on the full sample, and estimates obtained after dropping always-treated states and transforming our data into a staggered design, where states always remain treated after they first adopted a strict ID law. To do so, we recode the reversals that took place in North Dakota and Texas by assigning positive treatments to the corresponding years. Indeed, negative weights which arise with the two-way fixed effects estimator are only on always-treated states, and both de Chaisemartin and D'Haultfœuille (2020a) and Sun and Abraham (2020)'s estimators drop always-treated states. In addition, Sun and Abraham (2020) focus on staggered designs, and thus require the aforementioned transformation. In contrast,

Finally, to corroborate the validity of the parallel-trend assumption, we plot estimates of β_τ's from the following leads-and-lags regression:

$$Y_{ist} = \sum_\tau \beta_\tau ID^\tau_{st} + X^{'}_{ist}\gamma + \alpha_s + \delta_t + \mu_{ist}, \tag{2}$$

where ID^τ_{st} is a dummy equal to 1 if election year t occurs τ elections after state s first implemented its strict ID law. τ ranges between -4 and +3. The β_τ's measure the difference in participation between treated and control states before ($\tau < 0$) or after ($\tau \geq 0$) the first implementation of the law, conditional on controls. All coefficients are normalized relative to the last pre-treatment election ($\tau = -1$).

Figure I shows that turnout does not change differentially in treated states *after* the first implementation of the law, consistent with the estimates in Table I. Corroborating our identification strategy, we also find no evidence of differential trends *before* implementation: though strict ID laws are not randomly assigned to states (Online Appendix Table A.2 shows slightly lower turnout level in treated states), their implementation does not correlate with differential pre-trends in turnout.[15]

[Figure I about here]

IV.2. Heterogeneity Analysis

The null effects of strict ID laws on overall registration and turnout could potentially mask negative effects on minorities (who are less likely to possess an accepted ID) and positive effects on whites, or differences along other dimensions. To assess treatment impact heterogeneity, we estimate regressions of the following form:

$$Y_{ist} = ID_{st} \times Z^{'}_{ist}\lambda + Z^{'}_{ist}\eta + X^{'}_{ist}\gamma + \alpha_s + \delta_t + \mu_{ist}, \tag{3}$$

where Z_{ist} is the vector of characteristics along which we allow for heterogeneity in the treatment effects. Since this specification does not include ID_{st} uninteracted, the coefficients on the inter-

de Chaisemartin and D'Haultfœuille (2020a)'s estimator of the effect immediately following the change in treatment applies to any two-way fixed effects regressions, not only to those with staggered adoption, so the corresponding estimates use the untransformed data. The *did_multiplegt* command collapses data at the cell level (i.e., by state-year) and computes bootstrap standard errors by resampling entire clusters (states). The command can accommodate covariates, which are averaged at the cell level. However, due to the state-level bootstrap resampling, including a large number of controls may cause some bootstrap replications to run regressions with more covariates than observations. To avoid this issue, when using *did_multiplegt*, we only include state-level controls (i.e., we do not include the voter-level controls race-by-year, race-by-state, age ventile, and gender fixed effects). To ensure comparability across methods, all other estimates in the table similarly control for state-level covariates, but not for voter-level ones.

[15] Online Appendix Figure A.4 reports event-study graphs based on McDonald's turnout data, 2008–2018. The resulting plots are remarkably similar to the main event-study graph based on the individual-level Catalist data (Figure I).

actions between ID_{st} and Z_{ist} directly indicate the effects of strict ID laws on the corresponding groups. In addition, we test for heterogeneous effects across groups.

Table III reports the results for the main dimension of heterogeneity: race. We use the same specifications as in Table I, with two differences. First, all specifications control for race-by-year and race-by-state fixed effects, to ensure that the interaction between ID_{st} and race dummies is not biased by race-specific shocks occurring in a given year (across all states) or in a given state (across all years). Second, in column (4), we control for state-by-year fixed effects instead of state time trends, thereby using a triple-difference framework. The inclusion of state-by-year fixed effects allows us to account for a larger set of possible confounders. It precludes estimating the overall effect of the laws, which varies at this level, but not differential effects by race.

As shown in Panel A, in all specifications the point estimates are close to null for whites and positive but statistically non-significant for non-whites. We cannot reject the null of identical effects on both groups. Considering the lower bounds of the 95-percent confidence intervals of the differential effects estimated using our voter fixed effects specification (column (5)), we can reject that strict ID laws decrease non-white turnout (relative to white turnout) by more than 0.5 percentage points. Various other policies and institutions have been shown to induce substantially larger differential turnout effects. For example, Cantoni (2020) estimates that the disproportionate effect of distance to polling location widens the turnout gap between whites and non-whites by 1.6 to 4.0 percentage points, depending on the election; White (2019) shows that receiving a short jail sentence causes Black turnout to drop in the next election by approximately 13 percentage points, with small and non-significant effects on white turnout; and Fraga (2016) reports that increasing the within-district share of a race group from 10 to 50 percent would raise Black and Hispanic general election turnout by 9.3 to 6.4 percentage points, respectively, while the predicted effect on white turnout is 0.6 percentage point.

In Panel B, we allow the effects to differ by detailed race. Surprisingly, we find a large, positive, and significant effect on Hispanics. The sign and magnitude of this effect are robust across specifications. The estimated difference relative to whites is 2.6 to 3.2 percentage points, depending on the specification. The next subsection discusses one possible mechanism underlying this effect. Instead, we do not find any significant direct or differential effect of the laws on Blacks and on voters of other races. The bottom line is that strict ID laws did not decrease the participation of any race group.

[Table III about here]

The validity of this result relies on the assumption that turnout trends were parallel between treated and control states for each race, which is supported by the lack of differential pre-trends in race-specific event studies plotted in Figure II.

[Figure II about here]

Estimates obtained when restricting attention to voters in adjacent counties across state borders yield the consistent conclusion that strict ID laws did not decrease the participation of any race group (Online Appendix Table A.4, columns (2)–(5)). In Online Appendix Table A.9, we also test the robustness of the race heterogeneity results to state-by-race-level regressions. Specifically, we collapse the data by race-state-years, counting ballots cast by voters of different races. We then construct two outcomes: the natural logarithm of ballots cast and total ballots cast divided by estimates of the citizen voting-age population based on U.S. Census Data in a given race-state-year. Point estimates and resulting patterns of race heterogeneity are very similar to those reported in Table III.[16] Finally, Panels B through E of Online Appendix Tables A.7 and A.8 show the robustness of the race-heterogeneity results to using de Chaisemartin and D'Haultfœuille (2020a) and Sun and Abraham (2020)'s estimators.

A possible concern is that our estimates might miss actual effects of strict ID laws on the participation of Black voters or other ethnic minorities due to the miscategorization of some of these voters' race. Because many campaigns use data similar to ours, minority voters who may be miscategorized in our data may also be less likely to be targeted by campaigns and, thus, more negatively affected by strict ID laws. However, Online Appendix Tables A.11 and A.12 show the robustness of our race-heterogeneity results to restricting the sample to voters whose race is estimated with highest confidence and to registered voters, respectively. (Table A.12 uses the turnout of the registered voters as outcome, as in Table I, Panel A.) Furthermore, Online Appendix Tables A.13 and A.14 measure the effects of strict ID laws, overall and separately by race, using the CCES self-reported turnout data. Despite the limited representativeness and accuracy of national surveys, discussed in the Introduction, one strength of the CCES is that it includes self-reported race. Reassuringly, our null results are robust to using this alternative source of data.

Online Appendix Table A.15 explores treatment impact heterogeneity along other individual characteristics. We find that the laws did not negatively affect the participation of any group of voters defined by age, gender, or party affiliation.[17] This makes it unlikely that the laws changed electoral outcomes. We test this prediction in Table II, Panel B, and find that strict ID laws did not affect the two-party Democratic vote share in elections from 2004 to 2018. In this panel, we pool results from presidential and U.S. House elections. Units of observation are thus state-years, for presidential elections, and congressional district-years, for U.S. House elections. All

[16] Online Appendix Table A.10 replicates Online Appendix Table A.9 for voter registration (instead of voter turnout). We construct again two outcomes for each race group: the natural logarithm of registered voters and the number of registered voters divided by the citizen voting-age population. The race-specific point estimates are generally non-significant and we do not find any significant differential effect of strict ID laws on minority voters, compared to whites.

[17] Party affiliation is only available for two treated states (Arizona and Kansas), one of which is always treated over our sample period (Arizona). Corresponding estimates should thus be interpreted with caution.

point estimates are positive but lower than 1 percentage point and not statistically significant. As shown in Online Appendix Table A.16, the results remain close to null and non-significant when we consider congressional and presidential elections separately.

IV.3. Effects Due to Specific Components of the Laws or Specific Contexts

We now do one last step to challenge our result that strict ID laws have null effects on participation: we test whether specific components of the laws or contextual factors are associated with larger effects.

First, we isolate the effect of *requiring* an ID from the effect of *requesting* one. As discussed in Section II.1, the distinctive feature of strict ID laws is that they require voters to show an ID, meaning that people without proper ID are prevented from voting. In contrast, non-strict laws request voters to show an ID but they allow those without ID to vote, typically by signing an affidavit of identity. While our regressions so far have included all states without a strict ID law in the control group, we isolate the effect of requiring an ID by comparing strict ID laws to non-strict laws, in a specification distinguishing between all four types of voter identification requirements: requiring an ID, requesting an ID, requiring voters to sign the poll book or an affidavit, and checking their name against a list of eligible citizens. Formally, we run a regression in the form of equation [1], in which we replace the dummy ID_{st} with three dummies, respectively for non-strict law, requiring a signature, or simply asking to state one's name.[18] This regression allows us to run pairwise comparisons between states with strict ID law (the default group) and any of the three other types of requirements. An important caveat is that when multiple treatment effects are estimated at once, the coefficient on each treatment is contaminated by a weighted sum of the effects of the other treatments in each state and period, with weights summing to 0 (de Chaisemartin and D'Haultfœuille, 2020b). Unfortunately, the novel estimators proposed by de Chaisemartin and D'Haultfœuille (2020a) and Sun and Abraham (2020) to improve on the two-way fixed effects estimator do not address this specific issue and they cannot be readily used to estimate the effects of multiple treatments. Therefore, the results of this model may be biased, and they should be interpreted with caution.

We report the results obtained with the Catalist data and McDonald's aggregate turnout data in Online Appendix Tables A.17 and A.18, respectively. The sign on the non-strict ID law dummy is generally negative, indicating that strict ID laws have a modest positive effect compared to non-strict laws, but the point estimates are small, and they are non-significant in all specifications, overall and for whites and non-whites considered separately. In comparison to states with strict ID

[18] Colorado (2014–2018), Oregon (throughout our sample years), and Washington state (2012–2018) implemented all-mail voting. Since voters in all-mail states must sign ballot return envelopes for their votes to be counted, we classify all-mail state-years as "signature." All results are substantively unaffected by alternative classifications of voter identification requirements in these state-years.

laws, voter turnout tends to be higher when voters are required to sign the poll book, and lower when they are only asked to state their name, but these differences are generally not statistically significant. The first difference dampens and the second increases when the sample is expanded to also include the 2004 and 2006 elections (Online Appendix Table A.18). Importantly, the effect of strict ID laws, whether measured against non-strict laws, requiring a signature, or asking to state one's name, is never significantly different across whites and non-whites (Online Appendix Table A.17, Panel B).

Second, strict ID laws requiring photo identification (like a driver's license or a state-issued identification card) could affect participation more negatively than those also allowing non-photo IDs (like a bank statement or utility bill). However, we do not find support for this hypothesis: all results are substantively identical using strict photo ID laws as treatment (Online Appendix Figures A.5 and A.6 and Tables A.24 through A.28). Out of 30 coefficients shown in Online Appendix Tables A.24 and A.26, only one is negative and significant (at the 10 percent level). It corresponds to the overall effect of strict photo ID laws on registration, in the specification controlling for state time trends, which is the least reliable as discussed in Section IV.1.

Third, the effects of strict ID laws could also vary over time: they could be largest immediately following implementation, if people are confused by the new rules, or escalate later, if the laws become more stringently enforced.[19] Alternatively, the effects might vary with election type: they might be larger in presidential elections, if these attract more voters unlikely to have an ID (Burden, 2018), or in midterms, if these elections' lower salience makes the administrative cost of acquiring an ID more prohibitive. However, we find no evidence of differential effects along any of these dimensions (Online Appendix Table A.19). If anything, the overall and race-specific event studies show more *positive* (although generally non-significant) effects on turnout in later elections (Figures I and II).

IV.4. Mobilization Against the Laws

The null average effect of strict ID laws on participation and the positive effect on Hispanics could result from the combination of a direct negative effect of the new requirements imposed by the laws, on one hand, and mobilization against them, on the other.

First, parties and candidates who fear they might lose votes as a result of the laws might mobilize their supporters around this issue and they might help voters without an ID acquiring one

[19] Relatedly, in North Dakota and Texas, where strict ID laws were implemented and later repealed, the effects of the laws may persist even after they were abandoned (Grimmer and Yoder, 2021). To account for this possibility, Online Appendix Figures A.7 and A.8 and Tables A.29 through A.31 replace the treatment dummy ID_{st}, equal to one if state s used a strict ID law in year t, with the dummy $\widetilde{ID}_{st}$, equal to one if the state used a strict ID law in that year or in any year before. The results leave our conclusion unchanged: strict ID laws have no negative effect on registration or turnout, overall or for any race.

(Citrin et al., 2014; Neiheisel and Horner, 2019). A large body of evidence shows that get-out-the-vote campaigns can have large participation effects (Gerber and Green, 2000, 2015), including among disenfranchised members of ethnic minorities (Garcia Bedolla and Michelson, 2012; Pons and Liegey, 2019), and that information and administrative help provided in person to voters can help them overcome obstacles to voting such as registration requirements (Nickerson, 2015; Braconnier et al., 2017). While we do not measure the extent to which electoral campaigns specifically refer to the laws or provide assistance to obtain acceptable ID, people's self-reported likelihood to be contacted by a campaign, in the CCES post-election survey data, is a good proxy for campaign intensity. We report the effects of strict ID laws on this outcome in Table IV, columns (1) and (2).

Second, even absent party mobilization, voters belonging to groups least likely to have an ID might perceive these laws as an attempt to deprive them of their rights, and become more likely to vote and engage politically as a result (Valentino and Neuner, 2017). Biggers and Smith (2018) report large effects on turnout of being threatened to be purged from voter rolls, particularly for Hispanics, and explain it based on psychological reactance theory (Brehm, 1966). According to this theory, a threat to a right (here, the right to vote) can enhance its perceived value and lead individuals to take steps to protect it even if they rarely used it previously. We do not have data on feelings associated with strict ID laws, but can estimate their effects on forms of political engagement beyond voting. After each election, the CCES surveys record whether people attended political meetings, posted a campaign sign, volunteered for a campaign, donated to a candidate or a campaign, and how much they contributed. We report effects on a standardized index aggregating these five variables in Table IV, columns (3) and (4), and on the individual outcomes in Online Appendix Table A.20. Finally, we measure effects on total campaign contributions by state and election year using official data from the Federal Election Commission collected by Bonica (2018) (Table IV, columns (5) and (6)).

[Table IV about here]

Panel A of Table IV shows the average effect of strict ID laws on these outcomes for all voters. We find no significant overall impact on any variable, whether we only control for year and state fixed effects or also include state controls and, for individual-level outcomes, voter controls.

Panel B explores treatment impact heterogeneity along race. The effect on the CCES index of voter activity is small and non-significant for both whites and non-whites. As shown in Online Appendix Table A.20, Panel B, we only find a positive and significant effect (at the 10 percent level) for non-whites on one out of five components of the index (i.e., volunteered for a campaign, in column (9)). For this outcome, the differential effect on non-whites compared to whites is significant at the 5 and 10 percent levels in the specifications with and without state and voter controls, respectively. But overall, we do not find any systematic evidence that individual reaction against the laws alleviated direct negative effects.

Instead, we do observe a large and positive effect on campaign contact among non-white voters. The laws increased the likelihood that these voters were contacted by a campaign by 4.7 percentage points, which is significant at the 5 percent level (column (1)). This effect is of similar magnitude and significant at the 1 percent level when including state and voter controls (column (2)). White voters were not more likely to be contacted by campaigns, differently than non-whites, leading to a differential effect of 4.1 percentage points. This differential effect remains significant (at the 5 percent level) and of almost identical magnitude when using strict photo ID laws as treatment (Online Appendix Table A.27).[20]

This result should be interpreted with caution since it is based on self-reported survey data and voters may misremember whether or not they were contacted during the campaign. In addition, even if the increase in campaign contact is real, parties might have targeted a subset of non-white voters unlikely to increase their participation as a result of being contacted. Our data do not allow us to directly measure the consequences of increased party mobilization for voter participation. However, we can check whether increases in the likelihood to be contacted by a campaign and in participation are observed for the same groups of voters. Interestingly, as shown in Online Appendix Table A.22, Panel B, columns (1) and (2), the effect on campaign contact is particularly strong (around 5 percentage points) among Hispanics, who also showed a positive effect on participation, suggesting that the former impact could contribute to explain the latter. The effect on campaign contact is less precisely estimated but also large and positive for the residual race category and it is smaller and non-significant for Blacks, whose participation was not affected by strict ID laws.[21]

Overall, these patterns bring suggestive indirect evidence that the increase in campaign contact was consequential, but they do not allow us to estimate the magnitude of plausible downstream effects on voter turnout. For this, we turn to the existing get-out-the-vote literature. In their review of a large number of experiments conducted in the U.S., Gerber and Green (2015) report that it takes about fifteen canvassing contacts to generate one vote among voters whose baseline propensity to vote lies between 30 and 50 percent. The average turnout of non-white voters in the sample was

[20] Ideally, we would have liked to corroborate this result based on survey responses with data from political parties or from the Federal Election Commission. Unfortunately, we were not able to find administrative data isolating expenditures and activities specifically related to field campaigns, let alone a breakdown of such data by the race of targeted voters. Online Appendix Table A.21 shows effects on coarser outcomes measured at the state-year level: total expenditures and total campaign-related expenditures (encompassing the following expenditure categories: "Campaign data and technology," "Campaign events and activities," "Campaign mailings and materials," "Campaign strategy and communications consulting," and "Polling and surveys") by candidates running to the U.S. House of Representatives, from the Center for Responsive Politics; and TV ad expenditures spanning down-ballot, state, and federal candidates from the Wisconsin Advertising Project and the Wesleyan Media Project. The point estimates are generally positive but modest, and none of them reaches statistical significance.

[21] The effect on the CCES index of voter activity is non-significant for any race, in any specification, except for Blacks, in the specification without state and voter controls (column (3)), where it is positive and marginally significant. When adding these controls, the effect is no longer statistically significant (column (4)).

within this range, as shown in Table III, Panel A, column (1). Therefore, taken at face value, the increase in campaign contact might have increased the participation of non-white voters by about 0.31 percentage points (4.7 percentage points divided by 15). In other words, mobilization against strict ID laws might have offset direct negative effects on the participation of ethnic minorities of about one third of a percentage point.

IV.5. Voter Fraud and Perception of Fraud

Finally, we explore the effects of strict ID laws on voter fraud and beliefs on election integrity. Studies of crime face a well-known challenge: increases in crime statistics can reflect changes in both the number of committed and reported crimes, and many treatments can have both direct and reporting effects (e.g., Bhuller et al., 2013; Draca et al., 2018). Similarly, strict ID laws might affect both the actual number of fraud cases and the likelihood that they get detected and reported. Other limitations inherent to the data available to us and discussed in Section II compound this issue. With these caveats in mind, we report the effects on the extent of fraud in Table V. We consider both the total number of cases (columns (1)–(2) and (5)–(6)) and the subset of cases belonging to categories more directly addressed by strict ID requirements (columns (3)–(4) and (7)–(8)), as described in Section III.3. The total number of cases reported in both the News21 and Heritage Foundation datasets is very low, corroborating existing studies (Minnite, 2010; Cottrell et al., 2018): 0.08 and 0.02 cases per year per 100,000 residents, respectively. About one third (0.03) and one half (0.01) of these cases were directly addressed by the laws. We do not find any significant negative effect of the laws on either outcome in either dataset.

The lack of effect on detected fraud does not preclude effects on voters' beliefs on election integrity. However, using SPAE data, we find the laws had no significant effect on the perceived occurrence of voter impersonation, multiple voting, and non-citizen voting (columns (11)–(16)). The effect on an index aggregating these outcomes (along with the other outcomes reported in Online Appendix Table A.23) is small and non-significant (columns (9)–(10)). Similarly, the laws did not significantly affect citizens' belief that the election was fair, recorded in the ANES (columns (17)–(18)).

[Table V about here]

V. CONCLUSION

For all the heated debates around strict voter ID laws, our analysis of their effects obtains mostly null results. First, the fears that strict ID requirements would disenfranchise disadvantaged populations have not materialized. Using the largest individual-level dataset ever assembled to study voter participation, we do not find any negative effect on overall turnout and registration rates or on any group defined by race, age, gender, or party affiliation. Close to null turnout effects are robust to the choice of the DD specification and to a large number of robustness checks. While we cannot entirely rule out the interpretation that this null result may be due to voters reacting against laws they felt could disenfranchise them, we do not find any effect on campaign contributions or on other forms of political engagement different than voting. However, we find a 4.7 percentage points increase in the fraction of non-white voters contacted by parties, bringing some support for the alternative interpretation that parties responded to the laws by mobilizing their supporters around them. It remains that based on existing estimates of the impact of campaign contact, these mobilization efforts might only have offset direct negative effects on the participation of ethnic minorities of about one third of a percentage point.

Second, contrary to the argument used by the Supreme Court in the 2008 case *Crawford v. Marion County* to uphold the constitutionality of one of the early strict ID laws, we find no significant impact on fraud or public confidence in election integrity. This result weakens the case for adopting such laws in the first place.

Because states adopted strict ID laws only 4 to 14 years ago, our results should be interpreted with caution: we find negative participation effects neither in the first election after the adoption of the laws nor in following ones, but cannot rule out that such effects will arise in the future. Enforcement of the laws already varies across locations and could very well become more stringent over time, especially if polarization on the issue increases. Partisan mobilization against the laws could also weaken over time. So we do not see our results as the last word on this matter – quite the opposite, we hope that they will provide guidance on the types of data and empirical strategies others can use to analyze the longer-run effects of the laws in a few years. For now, there is a real need to improve the administration of U.S. elections, including voting technology, and increase faith in elections (Alvarez et al., 2012), but strict ID laws are unlikely to do that. At the same time, low and unequal participation represent real threats to democracy (e.g., Meltzer and Richard, 1981; Miller, 2008; Cascio and Washington, 2014; Fujiwara, 2015) – but these may be more effectively addressed by reducing other barriers to voting, such as voter registration costs (Braconnier et al., 2017) or long travel and waiting time in areas with low polling station density (Cantoni, 2020).

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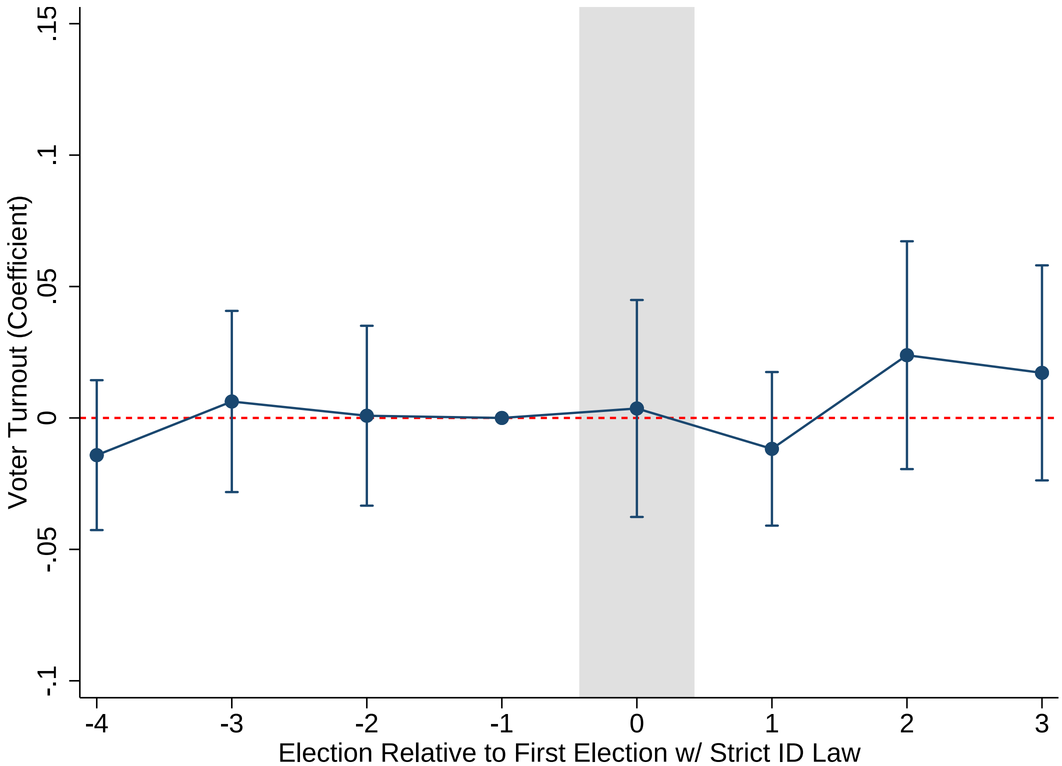
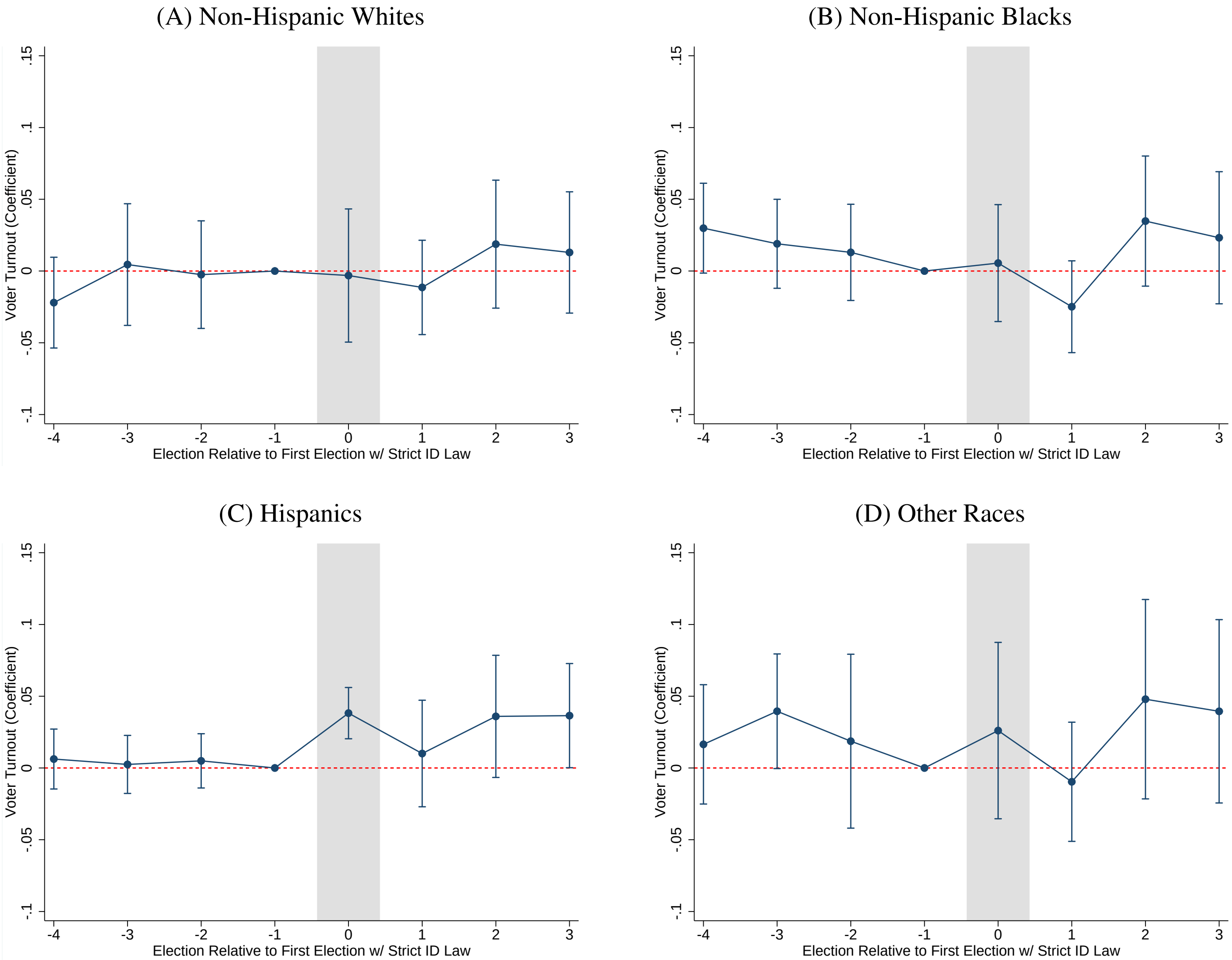
Figure I: Event-Study Graph of the Turnout Effects of Strict ID Laws



The figure plots event-study estimates and 95-percent confidence intervals from a regression (in the form of equation [2]) run on all registered and unregistered voters. The sample includes treated and control states. To avoid picking up variation from 2016 North Dakota, 2016 Texas, and 2018 Texas (which, unlike 2014 and 2018 North Dakota and 2014 Texas, did not enforce a strict law), we define $ID_{ND,2016}^{\tau=1} = ID_{TX,2016}^{\tau=1} = ID_{TX,2018}^{\tau=2} = 0$.

Figure II: Event-Study Graphs of the Turnout Effects of Strict ID Laws by Race



Each panel plots event-study estimates and 95-percent confidence intervals from a separate regression (in the form of equation [2]) run on all registered and unregistered voters of a given race. The sample includes treated and control states. To avoid picking up variation from 2016 North Dakota, 2016 Texas, and 2018 Texas (which, unlike 2014 and 2018 North Dakota and 2014 Texas, did not enforce a strict law), we define $ID^{\tau=1}_{ND,2016} = ID^{\tau=1}_{TX,2016} = ID^{\tau=2}_{TX,2018} = 0$.

Table I: Turnout and Registration Effects of Strict ID Laws

	Outcome:							
	1(Voted)				1(Registered)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A. Only Registered Voters							
1(Strict ID Law)	-.001	-.001	-.011	-.008	-	-	-	-
	(.013)	(.011)	(.019)	(.017)				
Outcome Mean	.620	.620	.620	.620				
	Panel B. Registered and Unregistered Voters							
1(Strict ID Law)	-.007	-.001	-.008	-.001	-.015	-.004	-.008	-.001
	(.015)	(.012)	(.014)	(.014)	(.012)	(.011)	(.007)	(.011)
Outcome Mean	.428	.428	.428	.428	.686	.686	.686	.686
Year FEs	✓	✓	✓	✓	✓	✓	✓	✓
State FEs	✓	✓	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓	✓	✓		✓	✓	✓
State Linear Trends			✓				✓	
Voter FEs				✓				✓

Notes. Each cell reports estimates from a separate regression run on the Catalist data. The sample for Panels A and B consists of, respectively, registered voters and both registered and unregistered voters. The sample size in the two panels is 1,100,864,799 and 1,604,600,607, respectively. State controls are dummies for the availability of no-excuse absentee voting, early in-person voting, all-mail voting, and Election-Day registration, along with indicators for the partisan composition of the state legislature and the governor's party as of Election Day. Voter controls are gender, dummies for the voter's age ventile (defined in the full panel data and including an additional dummy for voters with missing age information), and dummies for whether the voter is Black, Hispanic, or of other non-white, non-Hispanic (or unknown) race, along with interactions of these race dummies with states and years. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).

*** $p < .01$, ** $p < .05$, * $p < .10$

Table II: Effects of Strict ID Laws on Aggregate Outcomes

	(1)	(2)	(3)	(4)
	Panel A. Ballots Cast/VEP (McDonald's Data)			
1(Strict ID Law)	.006	.006	.001	.002
	(.012)	(.013)	(.012)	(.014)
Outcome Mean	.528	.528	.517	.517
N	408	408	408	408
Year FEs	✓	✓	✓	✓
State FEs	✓	✓	✓	✓
State-Year Controls		✓	✓	✓
VEP Weights			✓	✓
State Linear Trends				✓
	Panel B. Democratic 2-Party Vote Share			
1(Strict ID Law)	.001	.009	.005	-
	(.020)	(.017)	(.010)	-
Outcome Mean	.520	.520	.520	-
N	3,684	3,684	3,684	-
Year FEs	✓	✓	✓	
State FEs	✓	✓	✓	
State-Year Controls		✓	✓	
State Linear Trends			✓	

Notes. Panel A reports estimated turnout effects based on McDonald's state turnout data, 2004-2018 (2004 is the last year before strict ID laws were ever implemented). Turnout is defined as the ratio between ballots cast for the highest office on the ballot and the voting-eligible population (VEP) in a given state-year. Panel B reports estimated effects on the Democratic 2-party vote share based on constituency-level election results, 2004-2018, collected by the MIT Election Data and Science Lab. The sample in Panel B pools together congressional and presidential elections; units of observation are state-years (or DC) or congressional district-years. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).

*** $p < .01$, ** $p < .05$, * $p < .10$

Table III: Turnout Effects of Strict ID Laws by Race

	Outcome: 1(Voted)				
	Outcome Mean	Impact Estimates			
	(1)	(2)	(3)	(4)	(5)
		Panel A. Whites vs. Non-Whites			
1(Strict ID Law)×White	.458	-.006	-.003		-.005
		(.015)	(.014)		(.016)
1(Strict ID Law)×non-White	.340	.006	.006		.009
		(.014)	(.010)		(.012)
$\beta^{nonwhite} - \beta^{white}$		.013	.010	.007	.014
		(.008)	(.007)	(.007)	(.009)
		Panel B. By Detailed Race			
1(Strict ID Law)×White	.458	-.006	-.003		-.005
		(.015)	(.014)		(.016)
1(Strict ID Law)×Hispanic	.295	.025 *	.022 ***		.026 **
		(.015)	(.008)		(.010)
1(Strict ID Law)×Black	.380	-.009	-.006		-.004
		(.014)	(.013)		(.014)
1(Strict ID Law)×Other Race	.330	.013	.007		.008
		(.028)	(.022)		(.024)
$\beta^{hispanic} - \beta^{white}$		.032 ***	.026 **	.026 ***	.030 **
		(.011)	(.011)	(.006)	(.014)
$\beta^{black} - \beta^{white}$		-.003	-.003	-.003	.001
		(.008)	(.006)	(.006)	(.007)
$\beta^{other} - \beta^{white}$		.019	.010	-.001	.013
		(.016)	(.010)	(.006)	(.011)
Race-by-Year FEs		✓	✓	✓	✓
Race-by-State FEs		✓	✓	✓	✓
State & Voter Controls			✓	✓	✓
State-by-Year FEs				✓	
Voter FEs					✓

Notes. The sample (N = 1,604,600,607) consists of both registered and unregistered voters. See notes to Table I for details on the controls. Column (1) reports mean turnout in the interacting category. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).
*** p < .01, ** p < .05, * p < .10

Table IV: Effects of Strict ID Laws on CCES Campaign Contact, Voter Activity, and DIME Campaign Contributions

	Was Contacted by Campaign		Index of Voter Activity		Contributions ln($1/100k residents)	
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A. Average Effect			
1(Strict ID Law)	.015	.014	-.002	-.008	.024	.031
	(.020)	(.019)	(.016)	(.016)	(.102)	(.103)
Year & State FEs	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓		✓		✓
			Panel B. Whites vs. Non-Whites			
1(Strict ID Law)×White	.006	.004	-.003	-.011		
	(.021)	(.020)	(.017)	(.016)		
1(Strict ID Law)×non-White	.047 **	.046 ***	.002	.001		
	(.019)	(.016)	(.015)	(.014)		
$\beta^{nonwhite} - \beta^{white}$	.041 **	.042 ***	.005	.011		
	(.016)	(.015)	(.011)	(.010)		
Race-by-Year FEs	✓	✓	✓	✓	✓	✓
Race-by-State FEs	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓		✓		✓
Outcome Mean	.640	.640	.000	.000	14.682	14.682
N	221,926	221,926	308,704	308,704	408	408

Notes. The voter-level outcome for columns (1)-(2) is a dummy for whether a CCES survey respondent reported being contacted by a campaign in the last general election. The voter-level outcome for columns (3)-(4) is a summary index (i.e., sum of z-scores of individual components) of five variables measuring voter engagement in the last general election and recorded in the CCES data: whether people attended political meetings, posted a campaign sign, volunteered for a campaign, donated to a candidate or a campaign, and how much they contributed. The outcome for columns (5)-(6) is the log of political contributions to candidates and parties by state-year per 100k residents, 2004-2018. For a description of state controls, see the notes to Table I. Voter controls in columns (1)-(4) are education, gender, income, and race-by-year and race-by-state fixed effects. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).
*** p < .01, ** p < .05, * p < .10

Table V: Effects of Strict ID Laws on Reported and Perceived Frequency of Voter Fraud

	News21 Frauds/100k Residents		News21 Preventable Frauds/100k Residents		Heritage Frauds/100k Residents		Heritage Preventable Frauds/100k Residents	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1(Strict ID Law)	.045	.025	.014	.001	.009	.006	.013 **	.011
	(.113)	(.109)	(.046)	(.050)	(.007)	(.008)	(.006)	(.007)
Year & State FEs	✓	✓	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓		✓		✓		✓
Outcome Mean	.078	.078	.033	.033	.020	.020	.013	.013
N	459	459	459	459	765	765	765	765

	SPAE Perceived Fraud Index		SPAE Voter Impersonation		SPAE Multiple Voting		SPAE Non-Citizen Voting		ANES Fair Election	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
1(Strict ID Law)	.003	.007	-.004	-.002	-.009	-.013	-.020	-.024	.008	.020
	(.030)	(.029)	(.017)	(.015)	(.023)	(.022)	(.024)	(.024)	(.045)	(.038)
Year & State FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓		✓		✓		✓		✓
Outcome Mean	.000	.000	.210	.210	.209	.209	.275	.275	.698	.698
N	42,600	42,385	42,488	42,277	30,534	30,424	30,533	30,423	11,396	11,396

Notes. Regressions in columns (1)-(4) are at the state-year level and their sample includes both even (i.e., general election) and odd years. The News21 and Heritage data cover, respectively, the 2004-2012 and 2004-2018 years. Preventable frauds include voter impersonation, duplicate voting, false registration, and ineligible voting. The outcome for columns (9)-(10), described in the text, is constructed by normalizing and aggregating SPAE responses used as outcomes in columns (11)-(16) and in Online Appendix Table A.23. The outcomes for columns (11)-(16) are dummies for whether SPAE survey respondents perceive different types of fraud as happening frequently or occasionally. The outcome for columns (17)-(18) is a dummy for whether ANES survey respondents agree the last election was "very fair" or "fair" (ANES 2004) or whether they agree ballots were counted fairly "very often" or "fairly often" (ANES 2012), "all of the time" or "most of the time" (ANES 2016). Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).

*** $p < .01$, ** $p < .05$, * $p < .10$

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STRICT ID LAWS DON'T STOP VOTERS: EVIDENCE FROM A U.S. NATIONWIDE PANEL, 2008–2018

ONLINE APPENDIX

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1. ONLINE APPENDIX

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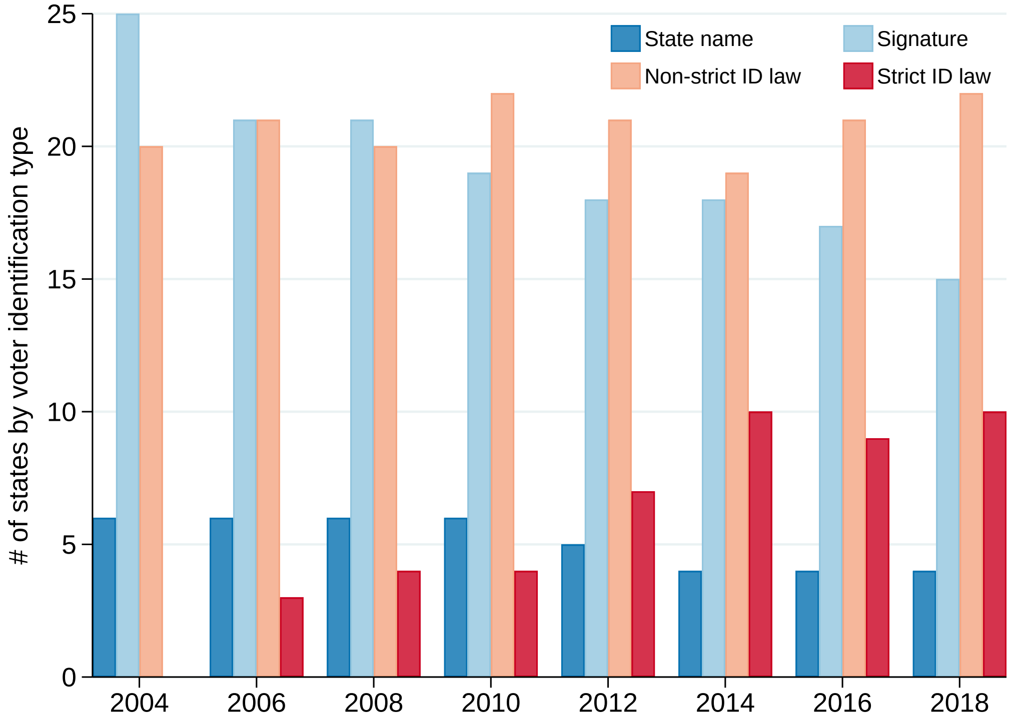
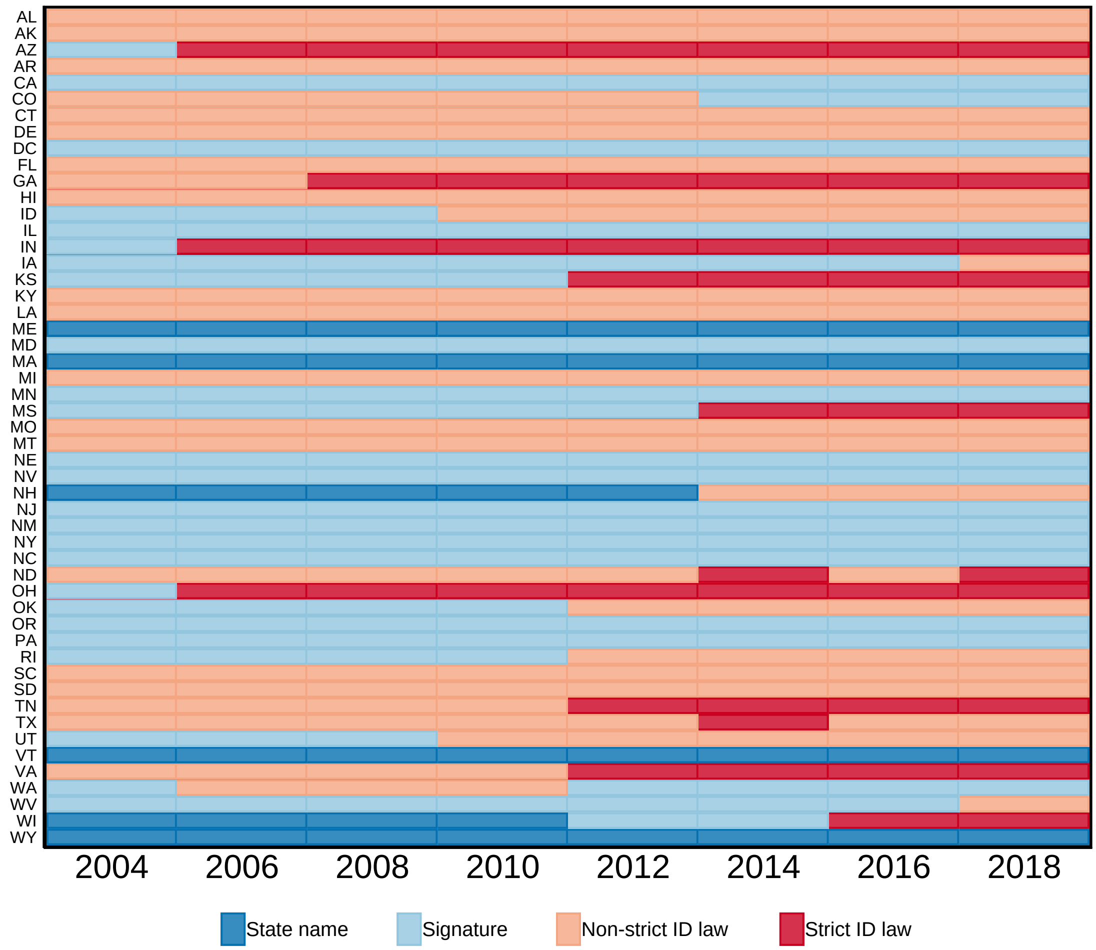
1.1. Voter Identification Requirements Across States and Over Time

Figure A.1: Number of States by Type of Voter Identification Requirement and Year



The figure plots the number of states implementing different forms of voter identification requirements in each general election, 2004–2018.

Figure A.2: Voter Identification Requirements by State and Year



The color of each cell represents the type of voter identification requirement implemented by a certain state (y-axis) in a given year (x-axis).

Table A.1: Description of Strict ID Laws

State	Acceptable Forms of ID	Voters Without ID	Years	Changes Over Time
Arizona §16-579(A)	One of the following forms of photo or non-photo ID: valid AZ driver's license; valid AZ non-driver ID; tribal enrollment card or other form of tribal ID; valid U.S. federal, state, or local government-issued ID; utility bill dated within 90 days of the election; bank or credit union statement dated within 90 days of the election; valid AZ vehicle registration; Indian census card; property tax statement; vehicle insurance card; recorder's certificate	An elector who does not provide the required ID shall receive a provisional ballot. Provisional ballots are counted only if the elector provides ID to the county recorder by 5 pm on the fifth business day after a general election that includes an election for federal office, or by 5 pm on the third business day after any other election.	2006-2018	
Georgia §21-2-417	One of the following forms of photo ID (if the ID doesn't contain the voter's signature, an additional ID with the voter's signature is required): GA driver's license, even if expired; ID card issued by the state of GA of the federal government; free voter ID card issued by the state or county; U.S. passport; Valid employee ID card containing a photograph from any branch, department, agency, or entity of the U.S. Government, Georgia, or any county, municipality, board, authority or other entity of this state; valid U.S. military ID card; valid tribal photo ID	A voter without one of the acceptable forms of photo ID can vote on a provisional ballot. He or she will have up to three days after the election to present appropriate photo ID at the county registrar's office in order for the provisional ballot to be counted.	2008-2018	
Indiana §3-5-2-40.5, 3-10-1-7.2 and 3-11-8-25.1	Specific forms of ID are not listed in statute. Photo ID must be issued by the state of IN or the U.S. government and must show the following: name of individual to whom it was issued, which must conform to the individual's registration record; photo of the person to whom it was issued; expiration date (if it is expired, it must have an expiration date after the most recent general election; military IDs are exempted from the requirement that ID bear an expiration date); must be issued by the United States or the state of IN	Voters who are unable or decline to produce proof of ID may vote a provisional ballot. The ballot is counted only if (1) the voter returns to the election board by noon on the Monday after the election and: (A) produces proof of ID; or (B) executes an affidavit stating that the voter cannot obtain proof of ID, because the voter: (i) is indigent; or (ii) has a religious objection to being photographed; and (2) the voter has not been challenged or required to vote a provisional ballot for any other reason.	2006-2018	
Kansas §25-2908, 25-1122, 25-3002, and 8-1324(g)(2)	One of the following forms of valid photo ID (expired documents are valid if the beared is 65 or older): driver's license issued by KS or another state; state ID card; government-issued concealed carry handgun or weapon license; U.S. passport; employee badge or ID document issued by a government office or agency; military ID; student ID issued by an accredited post-secondary institution in KS; government-issued public asssistance ID card	A voter who is unable or refuses to provide current and valid ID may vote a provisional ballot. To have his or her ballot counted, the voter must provide a valid form of ID to the county election officer in person or provide a copy by mail or electronic means before the meeting of the county board of canvassers	2012-2018	
Mississippi §23-15-563	One of the following forms of photo ID: a driver's license; a photo ID card issued by a branch, department, or entity of the State of Mississippi; a U.S. passport; a government employee ID card; a firearms license; a student photo ID issued by an accredited MS university, college, or community/junior college; a U.S. military ID; a tribal photo ID; any other photo ID issued by any branch, department, agency, or entity of the U.S. government, or any state government; a MS voter ID card	An individual without ID can cast an affidavit ballot which will be counted if the individual returns to the appropriate circuit clerk within five days after the election and shows government-issued photo ID. Voters with a religious objection to being photographed may vote an affidavit ballot, which will be counted if the voter returns to the appropriate circuit clerk within five days after the election and executes an affidavit that the religious exemption applies.	2014-2018	
North Dakota §16.1-05-07	Photo or non-photo ID must include: legal name; current residential street address in ND; and date of birth. The following forms of ID are acceptable: a driver's license; ID card issues by the ND department of transportation; ID issued by tribal government to a tribal member residing in the state. If an individual's valid form of ID does not include the required information or the information is not current, the ID must be supplemented by one of the following that provides the missing or outdated information: current utility bill; current bank statement; check issued by a federal, state or local government; paycheck; or document issued by a federal, state or local government.	If an individual is not able to show a valid form of ID but asserts qualifications as an elector in the precinct in which the individual desires to vote, the individual may mark a ballot that must be securely set aside in a sealed envelope designed by the secretary of state. After the ballot is set aside, the individual may show a valid form of ID to either a polling place election board member if the individual returns to the polling place before the polls close, or to an employee of the office of the election official responsible for the administration of the election before the meeting of the canvassing board occurring on the sixth day after the election. Each ballot set aside under this subsection must be presented to the members of the canvassing board for proper inclusion or exclusion from the tally. The state's ID requirement has partial exemptions for residents of long-term care facilities, uniformed service member or immediate family member, state residents temporarily living outside the U.S., and individuals with a disability that prevents them from traveling away from home.	2014 and 2018	In 2016, a federal judged ordered that voters without ID be given the option to cast a regular ballot after signing an affidavit. In 2017, HB 1369 was enacted, bringing the state back to the strict category.

(Continues)

Table A.1: Description of Strict ID Laws (cont.)

State	Acceptable Forms of ID	Voters Without ID	Years	Changes Over Time
Ohio §3503.16(B)(1)(a) and 3505.18(A)(1)	One of the following forms of photo or non-photo ID: current and valid photo ID, defined as a document that shows the individual's name and current address, includes a photograph, includes an expiration date that has not passed, and was issued by the U.S. government or the state of OH; current utility bill; current bank statement; current government check, paycheck or other government document.	A voter who has but declines to provide ID may cast a provisional ballot upon providing a social security number or the last four digits of a social security number. A voter who has neither ID nor a social security number may execute an affidavit to that effect and vote a provisional ballot. A voter who declines to sign the affidavit may still vote a provisional ballot. Voters who cast a provisional ballot because they did not provide acceptable proof of identity must appear in person at the board of elections to provide such proof within the 10 days immediately following Election Day.	2006-2018	
Tennessee §2-7-112(c)	One of the following forms of photo ID: TN driver's license; valid photo ID card issued by the state of TN; valid photo ID license issued by TN Dept. of Safety; valid U.S. passport; valid U.S. military ID with photo; TN handgun carry permit with photo.	If a voter is unable to present the proper evidence of ID, then the voter will be entitled to vote by provisional ballot. The provisional ballot will only be counted if the voter provides the proper evidence of ID to the administrator of elections or the administrator's designee by the close of business on the second business day after the election. However, "A voter who is indigent and unable to obtain proof of ID without payment of a fee or who has a religious objection to being photographed shall be required to execute an affidavit of identity on a form provided by the county election commission and then shall be allowed to vote." §2-7-112(f)	2012-2018	
Texas 2011 SB 14	One of the following forms of photo ID: a Texas driver's license or personal ID card; a Texas election ID certificate; a Texas concealed handgun permit; a U.S military photo ID; a U.S. citizenship certificate containing the person's photograph; or a U.S. passport or passport card. Each form of ID had to be current or expired only within the last 60 days from presentation, with the exception of citizenship certificates (which do not expire).	If ID was not presented, the voter could vote a provisional ballot. For her or his provisional ballot to be counted, the voter had to return within 6 days to the county voting registrar to show ID or sign an affidavit attesting to a religious objection or that no ID is available due to a natural disaster.	2014	
Virginia §24.2-643(B)	One of the following forms of photo ID: valid United States passport; valid Virginia driver's license or ID card; valid Virginia DMV issued Veteran's ID card; valid tribal enrollment or other tribal ID issued by one of 11 tribes recognized by the Commonwealth of Virginia; valid student ID card from within Virginia if it includes a photo; any other ID card issued by a government agency of the Commonwealth, one of its political subdivisions, or the United States; employee ID card containing a photograph of the voter and issued by an employer of the voter in the ordinary course of the employer's business	Any voter who does not show one of the forms of ID specified in this subsection shall be offered a provisional ballot marked ID-ONLY that requires no follow-up action by the registrar or electoral board other than matching submitted ID documents from the voter for the electoral board to make a determination on whether to count the ballot. In order to have his or her ballot counted, the voter must submit a copy of one of the forms of ID to the electoral board by facsimile, electronic mail, in-person submission, or timely United States Postal Service or commercial mail delivery, to be received by the electoral board no later than noon on the third day after the election.	2012-2018	In 2012, the VA requirement was strict, non-photo. 2013 HB 1337 created the strict-photo requirement. VA strict ID law was repealed in 2020.
Wisconsin §5.02(6m) and 6.79(2)(a)	One of the following forms of photo ID: WI driver's license; ID card issued by a U.S. uniformed service; WI non-driver ID; U.S. Passport; certificate of naturalization issued not more than 2 years before the election; ID card issued by a federally recognized -Indian tribe in WI; student ID card with a signature, an issue date, and an expiration date no later than 2 years after the election; a photo ID card provided by the Veteran's Health Administration. All of the above must include a photo and a name that conforms to the poll list. If the ID presented is not proof of residence, the elector shall also present proof of residence.	An elector who appears to vote at a polling place and does not have statutory ID shall be offered the opportunity to vote a provisional ballot. An elector who votes a provisional ballot may furnish statutory ID to the election inspectors before the polls close or to the municipal clerk no later than 4pm on the Friday following Election Day.	2016-2018	

Notes. This table describes every strict ID law enforced in at least one general election, 2004-2018. The main source of this table is the NCSL Voter ID Laws website (https://www.ncsl.org/research/elections-and-campaigns/voter-id.aspx), which we accessed on January 26, 2017, and on November 5, 2018. We supplemented this information with a chronology of voter ID laws, 2000-2014, which we received from the NCSL on October 30, 2014, and with information on Texas 2014 strict voter ID law, which we obtained directly from the text of Texas 2011 SB 14. According to NCSL's chronology of voter ID laws, "Indiana (P.L. 109/SB 483) – created a strict photo ID requirement; implemented in 2008 after being cleared by U.S. Supreme Court)." However, Alvarez (2008), Alvarez (2011), and court documents (e.g., https://jenner.com/system/assets/assets/4162/original/Crawford_Merits.pdf?1319825362, accessed: January 18, 2021) indicate Indiana's strict ID requirement was already enforced in the 2006 election. We therefore deviate from the NCSL chronology of ID laws and consider 2006 (instead of 2008) the first general election in which Indiana's strict ID requirement was implemented.

1.2. *Additional Details on the Catalist Data*

Over time, Catalist continually updates its database to incorporate new state voter files as well as commercial data refreshes, and it identifies deceased voters based on the Social Security Death Master File (SSDMF) datasets. Catalist also identifies people changing addresses based on NCOA records and by systematically comparing voter lists and commercial records of different states. Catalist gives each person a unique ID, invariant across years and files. Data matching procedures are run to ascertain potential matches across files. For example, if a voter registered with the first name "Tom," but commercial records include an individual called "Thomas" with the same last name, address, and sociodemographic characteristics, Catalist will recognize that it is the same individual and reconcile the two sources of information (Ansolabehere and Hersh, 2014).

The information Catalist shares with its clients usually stems from a cross-sectional "live file," containing the present-day address and information and the full voter turnout history of every individual who ever appeared in its database. Since 2008, however, Catalist has also been saving "historical files": snapshots of its live file as of the date of each biennial nationwide election.[1]

We received six historical files, corresponding to the 2008, 2010, 2012, 2014, 2016, and 2018 nationwide elections, and matched them with the current live file. The live file constitutes our source of longitudinal information on voter turnout and the historical files our source of longitudinal information on voters' residence.

For each election, the historical files we received from Catalist report voters' state and county of residence at that time, a flag for whether the voter was deceased,[2] registration status,[3] party affiliation (for voters registered in the 30 states in which it is available), an indicator for permanent absentee status, and a flag for "best state."[4] From the Catalist live file, we received the following variables: full turnout history, the state where the voter cast her ballot in each general election in our sample, if any, age, race, source of race information, and gender.

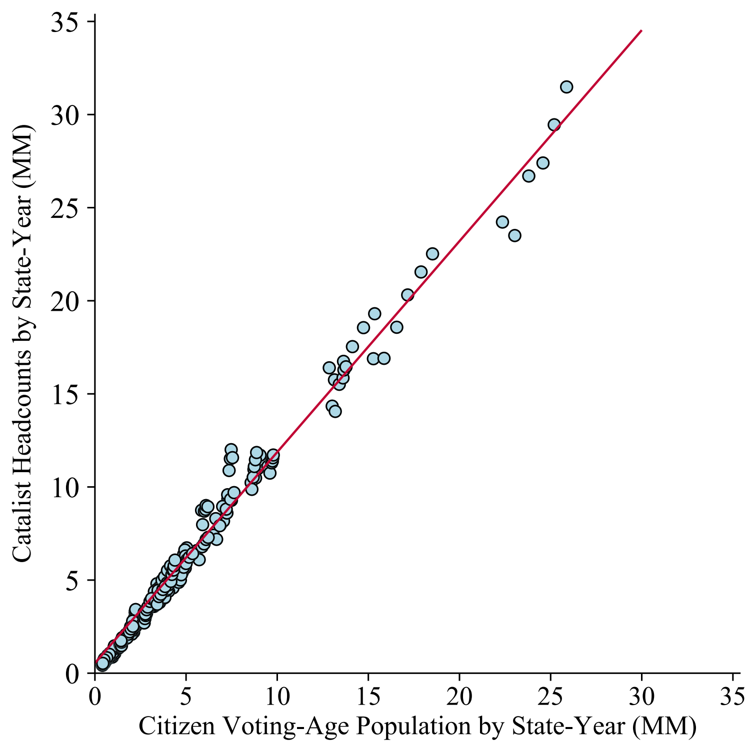
[1]Since it takes two to five months after Election Day for election administrators to process and give Catalist individual-level voter turnout information, historical files are copies of the live file as of two to five months after the corresponding Election Day. For instance, the 2008 historical file was saved between January and March 2009.

[2]Voters are flagged as deceased when they appear in the SSDMF or are reported as deceased in commercial records.

[3]Voter registration features five possible values: A, I, D, M, or U. "A" and "I" denote voters appearing on a state registration file with "active" or "inactive" registration status, respectively. "D" flags "dropped" individuals who appeared on past state voter files, but not in the most recent one. "M" indicates "moved, unregistered" voters who, according to NCOA or commercial data, moved into the state, but did not re-register in that state. "U" are voters whose status is "unregistered": they do not appear on current or past voter files but are known to reside in the state.

[4]When a voter is observed moving across states, Catalist creates a new record, and updates the original record (e.g., recoding the voter's registration status from "active" to "dropped") instead of erasing it. Consequently, the Catalist database is uniquely identified by voter ID *and* state. After using voter ID and state to match the historical files with the live file, we use the "best-state" flag to deduplicate on voter ID. Specifically, we deduplicate the matched historical files using the following lexicographic rules: we privilege the record corresponding to the state where a voter voted, if any; then records flagged as "best state"; then we use voter registration, privileging voter registration statuses in this order: "A", "M", "U", "I", and "D"; then we privilege the record with the oldest registration date; finally, among residual duplicates, we keep a reproducibly random record.

Figure A.3: Catalist Headcounts vs. Citizen Voting-Age Population



The figure plots state-by-year headcounts in the Catalist data (y-axis) against estimates of the citizen voting-age population based on U.S. Census Data (x-axis). The red line represents the best linear fit, weighting by Catalist headcounts.

Table A.2: Summary Statistics

	Control States		Treated States		All States	
	Catalist	Census	Catalist	Census	Catalist	Census
	(1)	(2)	(3)	(4)	(5)	(6)
Female	.527	.514	.530	.513	.528	.514
White	.740	.705	.741	.699	.740	.703
Hispanic	.093	.110	.095	.113	.093	.111
Black	.111	.116	.130	.147	.116	.124
Other race	.056	.070	.034	.041	.050	.062
Age:						
Missing values	.092	-	.109	-	.096	-
Mean	49.0	47.1	48.5	46.4	48.8	46.9
Std. dev.	18.3	-	18.0	-	18.2	-
Voted	.434	-	.410	-	.428	-
Registered	.688	-	.681	-	.686	-
Party registration:						
Living in a party registration state	.730	-	.105	-	.558	-
…and registered as Democrat	.213	-	.021	-	.160	-
…and registered as Republican	.147	-	.027	-	.114	-
…and registered as unaffiliated	.123	-	.019	-	.095	-
…and registered for a third party	.018	-	.005	-	.014	-
N	1,163,102,934	240	441,497,673	66	1,604,600,607	306

Notes. Treated states are defined as states that enforced a strict ID law in the sample years (2008-2018). State-years are the units of observations in columns (2), (4), and (6). Here, the proportion of females and age come from 2008, 2010, 2012, 2014, 2016, and 2018 "1-year" ACS data. In the same columns, state-by-year race shares for the adult population come from the National Cancer Institute (2008) and the United States Census Bureau (for all other years). These shares are then weighted by the estimated fraction of adult population holding U.S. citizenship in the corresponding race-year-state. Estimated citizenship ratios come from "1-year" ACS data.

1.3. Details on ANES, SPAE, and CCES Survey Outcomes

The survey questions used to construct the SPAE-based outcomes are as follows:

- Voter impersonation: q38 (SPAE 2008), q29c (2012), Q37C (2014), Q37C (2016).
- Multiple voting: q29a (2012), Q37A (2014), Q37A (2016).
- Non-citizen voting: q29d (2012), Q37D (2014), Q37D (2016).
- Absentee ballot fraud: q29e (2012), Q37E (2014), Q37E (2016).
- Officials changing vote tallies: q29f (2012), Q37F (2014), Q37F (2016).
- Votes stealing: q37 (2008), q29b (2012), Q37B (2014), Q37B (2016).

The SPAE survey was not administered in 2010. There were also no questions on multiple voting, non-citizen voting, absentee ballot fraud, and officials changing vote counts in 2008.

For the ANES-based outcome on whether the past election was fair, we use the following post-election survey waves and questions: V045042 (2004), electintpo_countfair (2012), V162219 (2016). The question wording changed slightly across years. In 2004, the question was generically whether the 2004 presidential election was fair. In 2012 and 2016, voters were asked whether votes were counted fairly.

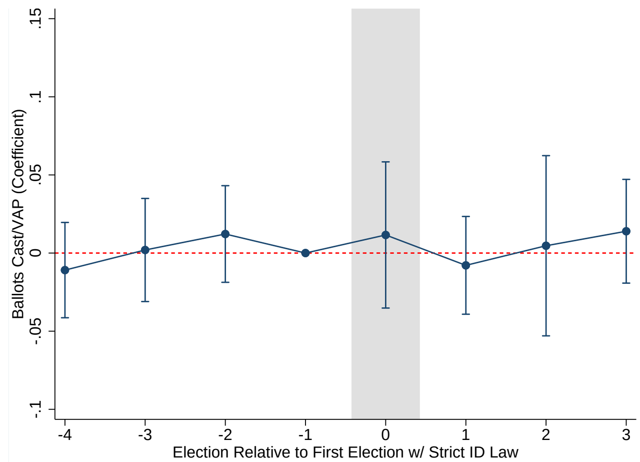
CCES dummy outcomes are based on the following years and survey questions (omitted years correspond to years in which the relevant survey question was not asked):

- Voter was contacted by a campaign: v4065 (2006), CC425a (2010), CC425a (2012), CC425a (2014), CC16_425a (2016).
- Donated to a candidate or campaign: v4062 (2006), CC415_6 (2008),CC417a_4 (2010), CC417a_4 (2012), CC417a_4 (2014), CC16_417a_4 (2016), CC18_417a_6 (2018).
- Amount donated (equal to 0 for people who answered no to the "Donated to a candidate or campaign" question): CC416b (2008), CC417c (2010), CC417c (2012), CC417c (2014), CC16_417c (2016), CC18_417c (2018).
- Attended a local political meeting: CC415_1 (2008), CC417a_1 (2010), CC417a_1 (2012), CC417a_1 (2014), CC16_417a_1 (2016), CC18_417a_1 (2018).
- Posted a campaign sign: CC415_3 (2008), CC417a_2 (2010), CC417a_2 (2012), CC417a_2 (2014), CC16_417a_2 (2016), CC18_417a_2 (2018).
- Volunteered for a campaign: CC415_4 (2008), CC417a_3 (2010), CC417a_3 (2012), CC417a_3 (2014), CC16_417a_3 (2016), CC18_417a_3 (2018).

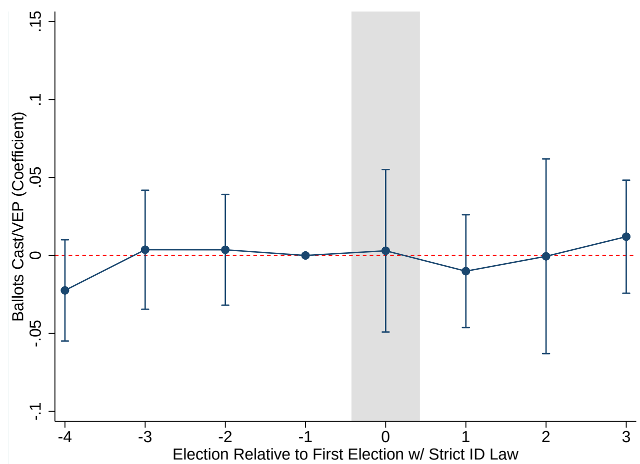
1.4. *Additional Results*

Figure A.4: Event-Study Graphs of the Turnout Effects of Strict ID Laws – McDonald's State Turnout Data

(A) Ballots Cast/Voting-Age Population



(B) Ballots Cast/Voting-Eligible Population



Each panel plots event-study estimates and 95-percent confidence intervals from a separate regression (in the form of equation [2]) run on McDonald's state turnout data, 2008–2018. The outcomes for Panels A and B are total ballots cast divided by, respectively, the voting-age and voting-eligible population in the state-year. The underlying regressions include state controls and are weighted by voting-age (top panel) or voting-eligible (bottom panel) population. To avoid picking up variation from 2016 North Dakota, 2016 Texas, and 2018 Texas (which, unlike 2014 and 2018 North Dakota and 2014 Texas, did not enforce a strict law), we define $ID^{\tau=1}_{ND,2016} = ID^{\tau=1}_{TX,2016} = ID^{\tau=2}_{TX,2018} = 0$.

Table A.3: Effects of Strict ID Laws on the Probability of Appearing in and Disappearing from the Catalist Data

	(1)	(2)	(3)	(4)
	Panel A. Appearing in the Sample			
1(Strict ID Law)	.009	.008	.030 **	.014
	(.016)	(.018)	(.012)	(.012)
Outcome Mean	.096	.096	.096	.096
	Panel B. Disappearing from the Sample			
1(Strict ID Law)	.004	-.001	.002	.002
	(.006)	(.005)	(.004)	(.009)
Outcome Mean	.062	.062	.062	.062
Year FEs	✓	✓	✓	✓
State FEs	✓	✓	✓	✓
State & Voter Controls		✓	✓	✓
State Linear Trends			✓	
Voter FEs				✓

Notes. The outcome for Panel A is a dummy indicating the first election in which a voter (previously not in the Catalist data) appears in the data. The outcome for Panel B is a dummy indicating the last election before a voter disappears from the data. The samples for panels A and B exclude, respectively, the 2008 and 2018 elections. N in the two panels is 1,358,011,521 and 1,309,156,053, respectively. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).

*** p < .01, ** p < .05, * p < .10

Table A.4: Turnout Effects of Strict ID Laws – Adjacent County-Pair Estimates

	Outcome: 1(Voted)				
	All Races	Whites	Blacks	Hispanics	Other
	(1)	(2)	(3)	(4)	(5)
1(Strict ID Law)	.015	.013	.018	.009	.044 **
	(.010)	(.009)	(.015)	(.016)	(.019)
Year FEs	✓	✓	✓	✓	✓
State FEs	✓	✓	✓	✓	✓
State & Voter Controls	✓	✓	✓	✓	✓
County-Pair-by-Year FEs	✓	✓	✓	✓	✓

Notes. The table restricts the sample to adjacent counties in neighboring states, in order to compare voters in contiguous county-pairs straddling a state border, following Dube et al. (2010)'s strategy. All specifications control for county-pair-by-year fixed effects. The sample consists of both registered and unregistered voters. The sample size is: 1,225,013,504 (column (1)), 934,444,329 (column (2)), 153,007,372 (column (3)), 87,590,854 (column (4)), and 49,970,949 (column (5)). See notes to Table I for details on the controls. Standard errors are two-way clustered by states (all the 48 states of the continental U.S. plus D.C.) and border segments (107 border segments).

*** p < .01, ** p < .05, * p < .10

Table A.5: Turnout Effects of Strict ID Laws – McDonald's State Turnout Data

	Outcome: Ballots Cast/VEP				Outcome: Ballots Cast/VAP			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A. 2004-2018 Elections				
1(Strict ID Law)	.006	.006	.001	.002	.005	.005	.001	.006
	(.012)	(.013)	(.012)	(.014)	(.011)	(.012)	(.011)	(.011)
Outcome Mean	.528	.528	.517	.517	.492	.492	.468	.468
N	408	408	408	408	408	408	408	408
				Panel B. 2008-2018 Elections				
1(Strict ID Law)	-.002	-.004	-.006	-.007	-.003	-.004	-.002	.003
	(.016)	(.017)	(.014)	(.020)	(.014)	(.015)	(.012)	(.017)
Outcome Mean	.529	.529	.519	.519	.493	.493	.470	.470
N	306	306	306	306	306	306	306	306
Year FEs	✓	✓	✓	✓	✓	✓	✓	✓
State FEs	✓	✓	✓	✓	✓	✓	✓	✓
State-Year Controls		✓	✓	✓		✓	✓	✓
VEP/VAP Weights			✓	✓			✓	✓
State Linear Trends				✓				✓

Notes. The table reports estimated turnout effects based on McDonald's state turnout data. Panels A and B include, respectively, election years 2004-2018 (2004 is the last year before strict ID laws were ever implemented) and 2008-2018 (i.e., matching the Catalist years). VEP and VAP stand for Voting-Eligible and Voting-Age Population, respectively. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).
*** $p < .01$, ** $p < .05$, * $p < .10$

Table A.6: Registration Effects of Strict ID Laws – McDonald's Registration Denominators

	Outcome: Registered Voters/VEP				Outcome: Registered Voters/VAP			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1(Strict ID Law)	-.015	-.013	-.012	.002	-.015	-.013	-.013	.0002
	(.012)	(.012)	(.012)	(.005)	(.011)	(.011)	(.011)	(.0058)
Outcome Mean	.834	.834	.835	.835	.777	.777	.755	.755
N	306	306	306	306	306	306	306	306
Year FEs	✓	✓	✓	✓	✓	✓	✓	✓
State FEs	✓	✓	✓	✓	✓	✓	✓	✓
State-Year Controls		✓	✓	✓		✓	✓	✓
VEP/VAP Weights			✓	✓			✓	✓
State Linear Trends				✓				✓

Notes. The table reports impact estimates on voter registration. The unit of observation is a state-year, 2008-2018. For each state-year, we compute registration rates as counts of registered voters in the Catalist data divided by the voting-eligible (VEP, columns (1)-(4)) or voting-age population (VAP, columns (5)-(8)), which we obtain from McDonald's state turnout data. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).

*** $p < .01$, ** $p < .05$, * $p < .10$

Table A.7: Robustness of Two-Way Fixed Effects (TWFE) Estimates to Alternative Estimators

	Outcome: 1(Voted)			
	First Election with Strict Voter ID Law		All Elections with Strict Voter ID Law	
	No Controls	Controls	No Controls	Controls
	(1)	(2)	(3)	(4)
Panel A. All Voters				
TWFE estimate	-.002	-.003	-.007	-.009
	(.022)	(.022)	(.015)	(.015)
TWFE estimate w/ staggered design and dropping always-treated states	-.003	-.005	.003	-.0003
	(.022)	(.022)	(.013)	(.013)
Estimate w/ De Chaisemartin and D'Haultfœuille (2020)'s method	-.0003	-.002	.010	.009
	(.027)	(.027)	(.019)	(.018)
Estimate w/ Sun and Abraham (2020)'s method	-.001	-.002	-	-
	(.022)	(.021)		
Panel B. Whites				
TWFE estimate	-.005	-.007	-.006	-.009
	(.024)	(.024)	(.015)	(.015)
TWFE estimate w/ staggered design and dropping always-treated states	-.007	-.009	-.002	-.005
	(.025)	(.025)	(.013)	(.013)
Estimate w/ De Chaisemartin and D'Haultfœuille (2020)'s method	-.002	-.003	.008	.007
	(.027)	(.027)	(.020)	(.019)
Estimate w/ Sun and Abraham (2020)'s method	-.004	-.005	-	-
	(.024)	(.024)		
Panel C. Hispanics				
TWFE estimate	.033 ***	.033 ***	.025 *	.021 **
	(.010)	(.009)	(.015)	(.009)
TWFE estimate w/ staggered design and dropping always-treated states	.034 ***	.033 ***	.026 *	.020 **
	(.010)	(.009)	(.014)	(.008)
Estimate w/ De Chaisemartin and D'Haultfœuille (2020)'s method	.028	.022	.027	.024
	(.032)	(.030)	(.027)	(.020)
Estimate w/ Sun and Abraham (2020)'s method	.035 ***	.035 ***	-	-
	(.009)	(.008)		
Panel D. Blacks				
TWFE estimate	.001	.002	-.009	-.009
	(.019)	(.021)	(.014)	(.016)
TWFE estimate w/ staggered design and dropping always-treated states	.001	.002	-.005	-.002
	(.019)	(.022)	(.012)	(.014)
Estimate w/ De Chaisemartin and D'Haultfœuille (2020)'s method	.004	.002	.005	.015
	(.025)	(.028)	(.016)	(.021)
Estimate w/ Sun and Abraham (2020)'s method	.002	.005	-	-
	(.020)	(.023)		
Panel E. Other Races				
TWFE estimate	.027	.021	.013	-.003
	(.034)	(.031)	(.028)	(.021)
TWFE estimate w/ staggered design and dropping always-treated states	.028	.021	.032 *	.017
	(.034)	(.031)	(.019)	(.013)
Estimate w/ De Chaisemartin and D'Haultfœuille (2020)'s method	.025	.014	.044	.045
	(.047)	(.048)	(.035)	(.036)
Estimate w/ Sun and Abraham (2020)'s method	.030	.024	-	-
	(.033)	(.029)		

Notes. The table explores robustness of our turnout estimates to alternative estimators. Each panel corresponds to a different sample of voters. Each cell reports estimates from a different method and specification. In columns (1) and (2) (resp. (3) and (4)), "TWFE estimate" refers to estimates of β_τ=0 from equation [2] (resp. estimates of β from equation [1]). "TWFE estimate w/ staggered design and dropping always-treated states" refers to analogous estimates obtained after dropping the four states that have strict ID laws throughout the sample period (i.e., AZ, GA, IN, OH) and transforming strict ID laws into an absorbing state (i.e., we assign positive treatment to 2016 ND and to 2016 and 2018 TX). In columns (1) and (2), "Estimate w/ Sun and Abraham (2020)'s method" refers to the estimated β_τ=0 from Interacted Weighted (IW) specifications suggested by Sun and Abraham (2020). To compute estimates based on Sun and Abraham (2020)'s method in columns (1) and (2) and those based on De Chaisemartin and D'Haultfœuille (2020)'s method in columns (3) and (4), we drop always-treated states and make strict ID laws an absorbing state. The controls used in columns (2) and (4) are the state-level controls described in the notes to Table I. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).
*** p < .01, ** p < .05, * p < .10

Table A.8: Robustness of Two-Way Fixed Effects (TWFE) Estimates to Alternative Estimators – Aggregate Turnout Data

	Outcome: 1(Voted)			
	First Election with Strict Voter ID Law		All Elections with Strict Voter ID Law	
	No Controls	Controls	No Controls	Controls
	(1)	(2)	(3)	(4)
Panel A. McDonald's Turnout (Ballots Cast/VEP)				
TWFE estimate	.003	.003	-.003	-.006
	(.025)	(.026)	(.013)	(.014)
TWFE estimate w/ staggered design and dropping always-treated states	.002	.002	.009	.007
	(.026)	(.026)	(.013)	(.014)
Estimate w/ De Chaisemartin and D'Haultfœuille (2020)'s method	.009	.009	.014	.014
	(.028)	(.028)	(.022)	(.019)
Estimate w/ Sun and Abraham (2020)'s method	.004	.004	-	-
	(.022)	(.022)		
Panel B. Whites (Ballots Cast/Citizen Population 18+)				
TWFE estimate	-.003	-.004	-.007	-.009
	(.030)	(.031)	(.015)	(.016)
TWFE estimate w/ staggered design and dropping always-treated states	-.004	-.006	.007	.004
	(.031)	(.031)	(.015)	(.015)
Estimate w/ De Chaisemartin and D'Haultfœuille (2020)'s method	-.001	-.002	.013	.010
	(.031)	(.031)	(.022)	(.019)
Estimate w/ Sun and Abraham (2020)'s method	-.002	-.003	-	-
	(.027)	(.028)		
Panel C. Hispanics (Ballots Cast/Citizen Population 18+)				
TWFE estimate	.039 ***	.039 ***	.026	.023 *
	(.014)	(.014)	(.016)	(.013)
TWFE estimate w/ staggered design and dropping always-treated states	.040 ***	.041 ***	.038 ***	.036 ***
	(.014)	(.014)	(.008)	(.008)
Estimate w/ De Chaisemartin and D'Haultfœuille (2020)'s method	.019	.016	.037	.036
	(.034)	(.033)	(.027)	(.026)
Estimate w/ Sun and Abraham (2020)'s method	.044 ***	.045 ***	-	-
	(.013)	(.013)		
Panel D. Blacks (Ballots Cast/Citizen Population 18+)				
TWFE estimate	.008	.011	-.003	-.001
	(.022)	(.023)	(.017)	(.018)
TWFE estimate w/ staggered design and dropping always-treated states	.007	.011	-.0001	.007
	(.023)	(.023)	(.0150)	(.018)
Estimate w/ De Chaisemartin and D'Haultfœuille (2020)'s method	.015	.018	.010	.022
	(.031)	(.029)	(.028)	(.029)
Estimate w/ Sun and Abraham (2020)'s method	.008	.013	-	-
	(.019)	(.019)		
Panel E. Other Races (Ballots Cast/Citizen Population 18+)				
TWFE estimate	-.003	-.004	-.021	-.026
	(.056)	(.057)	(.028)	(.027)
TWFE estimate w/ staggered design and dropping always-treated states	-.002	-.003	.010	.007
	(.055)	(.056)	(.012)	(.013)
Estimate w/ De Chaisemartin and D'Haultfœuille (2020)'s method	-.023	-.026	.021	.025
	(.065)	(.071)	(.037)	(.037)
Estimate w/ Sun and Abraham (2020)'s method	.002	.003	-	-
	(.043)	(.044)		

Notes. The table replicates Online Appendix Table A.7 using alternative outcomes and race-by-state-level data. The outcome for Panel A is estimated turnout based on McDonald's data, 2008-2018, using VEP as denominator. In Panels B-E, the outcome is counts of voters of a given race who turned out in a state-year divided by counts of citizens 18 or older in the same race-state-year. See notes to Online Appendix Table A.9 for details on the construction of this outcome. Regressions in Panel A (resp. Panels B-E) are weighted by VEP (resp. total citizen population 18+ in a race-state-year). Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).
*** p < .01, ** p < .05, * p < .10

Table A.9: Turnout Effects of Strict ID Laws by Race – Race-by-State-Level Analyses

	Votes Cast/Citizen Population 18+				Ln(Votes Cast)			
	Outcome Mean	Impact Estimates			Outcome Mean	Impact Estimates		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A. Whites vs. Non-Whites				
1(Strict ID Law)×White	.585	-.007	-.009		13.95	-.019	-.026	
	(.110)	(.015)	(.016)		(1.01)	(.037)	(.038)	
1(Strict ID Law)×non-White	.271	.005	.003		10.35	-.003	-.012	
	(.143)	(.014)	(.013)		(1.96)	(.051)	(.048)	
$\beta^{nonwhite} - \beta^{white}$		.012	.012	.004		.016	.014	.013
		(.012)	(.012)	(.012)		(.027)	(.024)	(.026)
				Panel B. By Detailed Race				
1(Strict ID Law)×White	.585	-.007	-.009		13.95	-.019	-.026	
	(.110)	(.015)	(.016)		(1.01)	(.037)	(.039)	
1(Strict ID Law)×Hispanic	.220	.026	.024 *		10.10	.044	.033	
	(.112)	(.016)	(.013)		(1.88)	(.065)	(.055)	
1(Strict ID Law)×Black	.348	-.003	-.004		10.84	-.041	-.047	
	(.141)	(.017)	(.018)		(2.25)	(.046)	(.047)	
1(Strict ID Law)×Other Race	.246	-.021	-.026		10.12	.008	-.010	
	(.141)	(.028)	(.027)		(1.61)	(.106)	(.102)	
$\beta^{hispanic} - \beta^{white}$		.033 **	.033 **	.021 **		.063	.059	.073 **
		(.016)	(.015)	(.010)		(.045)	(.039)	(.032)
$\beta^{black} - \beta^{white}$		.004	.004	.002		-.022	-.021	-.021
		(.015)	(.015)	(.017)		(.029)	(.028)	(.032)
$\beta^{other} - \beta^{white}$		-.015	-.017	-.030 *		.027	.016	-.009
		(.019)	(.018)	(.016)		(.073)	(.068)	(.055)
Population Weights		✓	✓	✓		✓	✓	✓
Race-by-Year FEs		✓	✓	✓		✓	✓	✓
Race-by-State FEs		✓	✓	✓		✓	✓	✓
State Controls			✓	✓		✓	✓	✓
State-by-Year FEs				✓			✓	

Notes. This table reports estimates from regressions run at the race-by-state level. Columns (1) and (5) report mean outcomes in the interacting category. In columns (1)-(4), the outcome is counts of voters of a given race who turned out in a state-year divided by counts of citizens 18 or older in the same race-state-year. Headcounts by state, year, age, and race are from the National Cancer Institute (for 2008) and the United States Census Bureau (for all other years). These headcounts are then multiplied by the share of adult population holding citizenship in the corresponding state-year-race cell, which we estimate using "1-year" ACS data. The outcome for columns (5)-(8) is the natural logarithm of voters who turned out in a given race-state-year. In each regression, the total number of observations is 1,224; that is, four races (i.e., non-Hispanic white, Hispanic, non-Hispanic Black, other race) times six elections times 50 states plus DC. All regressions are weighted by total citizen population 18+ in a race-state-year. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).

*** p < .01, ** p < .05, * p < .10

Table A.10: Registration Effects of Strict ID Laws by Race – Race-by-State-Level Analyses

	Registered Voters/Citizen Population 18+				Ln(Registered Voters)			
	Outcome Mean	Impact Estimates			Outcome Mean	Impact Estimates		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A. Whites vs. Non-Whites				
1(Strict ID Law)×White	.885	-.012	-.012		14.38	-.011	-.012	
	(.082)	(.012)	(.012)		(1.01)	(.016)	(.016)	
1(Strict ID Law)×non-White	.533	-.011	-.012		11.09	-.012	-.016	
	(.211)	(.012)	(.012)		(1.93)	(.021)	(.020)	
$\beta^{nonwhite} - \beta^{white}$		.001	.0002	-.007		-.001	-.004	-.012
		(.007)	(.0070)	(.009)		(.012)	(.011)	(.018)
				Panel B. By Detailed Race				
1(Strict ID Law)×White	.885	-.012	-.012		14.38	-.011	-.012	
	(.082)	(.012)	(.012)		(1.01)	(.016)	(.016)	
1(Strict ID Law)×Hispanic	.466	.0001	-.001		10.92	.008	.003	
	(.173)	(.0059)	(.006)		(1.85)	(.019)	(.019)	
1(Strict ID Law)×Black	.652	-.016	-.017		11.51	-.031	-.034	
	(.183)	(.021)	(.021)		(2.23)	(.029)	(.030)	
1(Strict ID Law)×Other Race	.480	-.023 **	-.023 **		10.84	.001	-.004	
	(.222)	(.010)	(.009)		(1.58)	(.030)	(.027)	
$\beta^{hispanic} - \beta^{white}$		.012	.011	.002		.019	.015	.007
		(.009)	(.009)	(.004)		(.017)	(.017)	(.023)
$\beta^{black} - \beta^{white}$		-.004	-.005	-.011		-.020	-.022	-.027
		(.016)	(.016)	(.018)		(.022)	(.022)	(.027)
$\beta^{other} - \beta^{white}$		-.011	-.010	-.016		.011	.008	-.005
		(.011)	(.011)	(.010)		(.017)	(.015)	(.019)
Population Weights		✓	✓	✓		✓	✓	✓
Race-by-Year FEs		✓	✓	✓		✓	✓	✓
Race-by-State FEs		✓	✓	✓		✓	✓	✓
State Controls			✓	✓		✓	✓	✓
State-by-Year FEs				✓			✓	

Notes. This table reports estimates from regressions run at the race-by-state level. Columns (1) and (5) report mean outcomes in the interacting category. In columns (1)-(4), the outcome is counts of voters of a given race who were registered in a state-year divided by counts of citizens 18 or older in the same race-state-year. Headcounts by state, year, age, and race are from the National Cancer Institute (for 2008) and the United States Census Bureau (for all other years). These headcounts are then multiplied by the share of adult population holding citizenship in the corresponding state-year-race cell, which we estimate using "1-year" ACS data. The outcome for columns (5)-(8) is the natural logarithm of voters who were registered in a given race-state-year. In each regression, the total number of observations is 1,224; that is, four races (i.e., non-Hispanic white, Hispanic, non-Hispanic Black, other race) times six elections times 50 states plus DC. All regressions are weighted by total citizen population 18+ in a race-state-year. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).

*** p < .01, ** p < .05, * p < .10

Table A.11: Turnout Effects of Strict ID Laws by Race – Voters Whose Race is Estimated with Highest Confidence

	Outcome: 1(Voted)				
	Outcome Mean	Impact Estimates			
	(1)	(2)	(3)	(4)	(5)
		Panel A. Whites vs. Non-Whites			
1(Strict ID Law)×White	.479	-.009	-.003		-.005
		(.013)	(.012)		(.014)
1(Strict ID Law)×non-White	.354	-.001	.002		.006
		(.011)	(.010)		(.011)
$\beta^{nonwhite}$ - β^{white}		.007	.005	.008	.011
		(.011)	(.011)	(.011)	(.015)
		Panel B. By Detailed Race			
1(Strict ID Law)×White	.479	-.009	-.003		-.005
		(.013)	(.012)		(.014)
1(Strict ID Law)×Hispanic	.264	.020 **	.019 *		.027 **
		(.009)	(.010)		(.012)
1(Strict ID Law)×Black	.412	-.018	-.011		-.009
		(.013)	(.012)		(.013)
1(Strict ID Law)×Other Race	.313	.029	.026 *		.023
		(.022)	(.014)		(.018)
$\beta^{hispanic}$ - β^{white}		.028 **	.022	.030 *	.032 *
		(.012)	(.015)	(.016)	(.018)
β^{black} - β^{white}		-.009	-.008	-.006	-.004
		(.010)	(.009)	(.007)	(.010)
β^{other} - β^{white}		.038 **	.029 ***	.018	.028 **
		(.015)	(.010)	(.016)	(.013)
Race-by-Year FEs		✓	✓	✓	✓
Race-by-State FEs		✓	✓	✓	✓
State & Voter Controls			✓	✓	✓
State-by-Year FEs				✓	
Voter FEs					✓

Notes. The table replicates Table III restricting the sample to voters whose race is estimated by Catalist with highest confidence. N = 1,049,125,957. Column (1) reports mean turnout in the interacting category. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).
*** p < .01, ** p < .05, * p < .10

Table A.12: Turnout Effects of Strict ID Laws by Race – Registered Voters Only

	Outcome: 1(Voted)				
	Outcome Mean	Impact Estimates			
	(1)	(2)	(3)	(4)	(5)
		Panel A. Whites vs. Non-Whites			
1(Strict ID Law)×White	.654	-.002	-.006		-.014
		(.012)	(.012)		(.019)
1(Strict ID Law)×non-White	.517	.016	.015		.011
		(.014)	(.011)		(.016)
$\beta^{nonwhite} - \beta^{white}$		.019	.021	.015	.025
		(.013)	(.013)	(.010)	(.015)
		Panel B. By Detailed Race			
1(Strict ID Law)×White	.654	-.002	-.006		-.014
		(.012)	(.012)		(.019)
1(Strict ID Law)×Hispanic	.478	.051 **	.050 ***		.044 **
		(.022)	(.017)		(.019)
1(Strict ID Law)×Black	.542	-.006	-.007		-.010
		(.010)	(.010)		(.017)
1(Strict ID Law)×Other Race	.523	.019	.014		.008
		(.028)	(.025)		(.032)
$\beta^{hispanic} - \beta^{white}$		.054 **	.056 ***	.048 ***	.058 **
		(.020)	(.019)	(.008)	(.023)
$\beta^{black} - \beta^{white}$		-.004	-.001	-.001	.004
		(.008)	(.007)	(.008)	(.010)
$\beta^{other} - \beta^{white}$		.021	.020	.006	.022
		(.018)	(.015)	(.009)	(.018)
Race-by-Year FEs		✓	✓	✓	✓
Race-by-State FEs		✓	✓	✓	✓
State & Voter Controls			✓	✓	✓
State-by-Year FEs				✓	
Voter FEs					✓

Notes. The table replicates Table III restricting the sample to registered voters. N = 1,100,864,799. Column (1) reports mean turnout in the interacting category. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.). *** $p < .01$, ** $p < .05$, * $p < .10$

Table A.13: Turnout Effects of Strict ID Laws – CCES Self-Reported Turnout

	Outcome: 1(Voted)		
	(1)	(2)	(3)
1(Strict ID Law)	.004	.002	.001
	(.009)	(.008)	(.009)
Outcome Mean	.880	.880	.880
N	282,650	282,650	282,650
Year FEs	✓	✓	✓
State FEs	✓	✓	✓
State & Voter Controls		✓	✓
State Linear Trends			✓

Notes. This table reports impact estimates on CCES self-reported turnout, 2006-2018. For a description of state controls, see the notes to Table I. Voter controls are education, gender, income, and race-by-year and race-by-state fixed effects. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).
*** $p < .01$, ** $p < .05$, * $p < .10$

Table A.14: Turnout Effects of Strict ID Laws by Race – CCES Self-Reported Turnout

	Outcome: 1(Voted)		
	(1)	(2)	(3)
	Panel A: Whites vs. Non-Whites		
1(Strict ID Law)×White	.006	.002	
	(.009)	(.008)	
1(Strict ID Law)×non-White	.006	.005	
	(.012)	(.011)	
$\beta^{nonwhite}$ - β^{white}	.0003	.004	-.003
	(.0090)	(.008)	(.007)
	Panel B: By Detailed Race		
1(Strict ID Law)×White	.006	.002	
	(.009)	(.008)	
1(Strict ID Law)×Hispanic	.002	-.002	
	(.014)	(.014)	
1(Strict ID Law)×Black	-.001	-.001	
	(.018)	(.017)	
1(Strict ID Law)×Other Race	.025 *	.027 *	
	(.015)	(.014)	
$\beta^{hispanic}$ - β^{white}	-.004	-.004	-.005
	(.012)	(.011)	(.012)
β^{black} - β^{white}	-.007	-.003	-.013
	(.017)	(.016)	(.013)
β^{other} - β^{white}	.019 *	.025 **	.021 *
	(.011)	(.012)	(.012)
Outcome Mean	.880	.880	.880
N	282,650	282,650	282,650
Race-by-Year FEs	✓	✓	✓
Race-by-State FEs	✓	✓	✓
State & Voter Controls		✓	✓
State-by-Year FEs			✓

Notes. This table reports race-specific impact estimates on CCES self-reported turnout, 2006-2018. For a description of state controls, see the notes to Table I. Voter controls are education, gender, income, and race-by-year and race-by-state fixed effects. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).
*** p < .01, ** p < .05, * p < .10

Table A.15: Turnout Effects of Strict ID Laws by Gender, Age, and Party Affiliation

	Outcome: 1(Voted)				
	Outcome Mean	Impact Estimates			
	(1)	(2)	(3)	(4)	(5)
		Panel A. By Gender			
1(Strict ID Law)×Male	.431	-.005	.0004	-.007	.0001
		(.014)	(.0123)	(.014)	(.0142)
1(Strict ID Law)×Female	.437	-.008	-.003	-.009	-.002
		(.015)	(.013)	(.015)	(.015)
		Panel B. By Age			
1(Strict ID Law)×1(age < 35)	.347	-.001	.0001	-.007	.012
		(.017)	(.0169)	(.019)	(.017)
1(Strict ID Law)×1(35 <= age < 60)	.475	-.003	-.003	-.009	-.003
		(.016)	(.016)	(.018)	(.016)
1(Strict ID Law)×1(60 <= age)	.587	-.0003	-.001	-.006	-.003
		(.0137)	(.013)	(.014)	(.014)
		Panel C. By Party			
1(Strict ID Law)×Republican	.705	-.004	-.001	.018 **	.009
		(.011)	(.008)	(.009)	(.010)
1(Strict ID Law)×Democrat	.640	.021 *	.021 **	.039 **	.019 *
		(.012)	(.009)	(.009)	(.010)
1(Strict ID Law)×Other	.204	-.008	-.003	.015 *	.007
		(.009)	(.007)	(.008)	(.008)
Group-Specific Year FEs		✓	✓	✓	✓
Group-Specific State FEs		✓	✓	✓	✓
State & Voter Controls			✓	✓	✓
State Linear Trends				✓	
Voter FEs					✓

Notes. The table reports estimated heterogeneous effects by gender, age, and party affiliation. All samples include both registered and unregistered voters. Samples for Panels A and B exclude voters with missing gender and age, respectively. The sample in Panel C is restricted to the 30 states that record voters' partisan affiliation. Every regression includes year- and state-specific fixed effects for the interacting characteristic (e.g., female in Panel A). Column (1) reports mean turnout in the interacting category. Standard errors clustered at the state level are reported in parentheses (51 clusters in Panels A and B and 30 clusters in Panel C).
*** p < .01, ** p < .05, * p < .10

Table A.16: Effects of Strict ID Laws on Democratic 2-Party Vote Share

	Outcome: Democratic 2-Party Vote Share			
	(1)	(2)	(3)	(4)
	Panel A. U.S. House of Representatives Elections			
1(Strict ID Law)	.0003	.009	.005	.011
	(.0203)	(.018)	(.011)	(.019)
Outcome Mean	.522	.522	.522	.522
N	3,480	3,480	3,480	3,480
	Panel B. U.S. Presidential Elections			
1(Strict ID Law)	-.002	.001	-.007	-
	(.011)	(.012)	(.022)	-
Outcome Mean	.493	.493	.493	
N	204	204	204	
Year FEs	✓	✓	✓	✓
State FEs	✓	✓	✓	✓
State-Year Controls		✓	✓	✓
State Linear Trends			✓	
District FEs				✓

Notes. The table reports estimated effects on the Democratic 2-party vote share based on constituency-level election results collected by the MIT Election Data and Science Lab. The data cover the 2004-2018 general elections, 2004 being the last year before strict ID laws were ever implemented. Panels A and B explore, respectively, effects on U.S. House of Representatives and Presidential elections. In each year, units of observations in Panels A and B are, respectively, the 435 congressional districts and the 50 states plus DC. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).

*** $p < .01$, ** $p < .05$, * $p < .10$

Table A.17: Turnout Effects of Other Forms of Voter Identification Requirements

	Outcome: 1(Voted)			
	(1)	(2)	(3)	(4)
	Panel A. Average Effects			
1(State Name)	.007	-.004	-.015	.004
	(.013)	(.012)	(.034)	(.013)
1(Signature)	.030 *	.023	.028	.021
	(.015)	(.014)	(.017)	(.017)
1(Non-Strict ID Law)	-.002	-.006	.001	-.006
	(.014)	(.013)	(.014)	(.014)
	Panel B. Whites vs. Non-Whites			
1(State Name)×White	.004	-.004	-.013	.005
	(.013)	(.013)	(.034)	(.014)
1(State Name)×non-White	.004	-.001	-.007	.008
	(.015)	(.013)	(.036)	(.014)
1(Signature)×White	.027 *	.024 *	.030	.023
	(.015)	(.014)	(.018)	(.018)
1(Signature)×non-White	.021	.017	.023	.015
	(.013)	(.014)	(.018)	(.017)
1(Non-Strict ID Law)×White	-.003	-.004	.003	-.003
	(.015)	(.014)	(.017)	(.016)
1(Non-Strict ID Law)×non-White	-.013	-.011	-.005	-.014
	(.014)	(.011)	(.010)	(.012)
$\beta^{\text{state name/non-white}} - \beta^{\text{state name/white}}$	.0004	.003	.006	.003
	(.0101)	(.009)	(.006)	(.008)
$\beta^{\text{signature/non-white}} - \beta^{\text{signature/white}}$	-.006	-.007	-.007	-.008
	(.007)	(.008)	(.008)	(.009)
$\beta^{\text{non-strict/non-white}} - \beta^{\text{non-strict/white}}$	-.011	-.008	-.008	-.012
	(.008)	(.007)	(.010)	(.009)
Outcome Mean	.428	.428	.428	.428
Race-by-Year FEs	✓	✓	✓	✓
Race-by-State FEs	✓	✓	✓	✓
State & Voter Controls		✓	✓	✓
State-by-Year FEs			✓	
Voter FEs				✓

Notes. The table reports estimated turnout effects based on the Catalist data (N = 1,604,600,607), where the treatments are different, mutually exclusive ways in which states identify voters at the polls. Strict ID laws are the omitted category. Panel A reports average effects. Panel B explores treatment heterogeneity across white and non-white voters. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).

*** $p < .01$, ** $p < .05$, * $p < .10$

Table A.18: Turnout Effects of Other Forms of Voter Identification Requirements – McDonald's State Turnout Data

	Outcome: Ballots Cast/VEP				Outcome: Ballots Cast/VAP			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A. 2004-2018 Elections				
1(State Name)	-.025 **	-.020	-.008	-.047 *	-.021 *	-.018	-.006	-.045 *
	(.012)	(.013)	(.013)	(.027)	(.011)	(.012)	(.012)	(.023)
1(Signature)	.005	.006	.016	.006	.006	.006	.017	.005
	(.011)	(.012)	(.012)	(.017)	(.011)	(.011)	(.011)	(.014)
1(Non-Strict ID Law)	-.013	-.012	-.008	-.008	-.011	-.010	-.009	-.013
	(.013)	(.014)	(.014)	(.014)	(.012)	(.012)	(.012)	(.010)
Outcome Mean	.528	.528	.517	.517	.492	.492	.468	.468
N	408	408	408	408	408	408	408	408
				Panel B. 2008-2018 Elections				
1(State Name)	-.016	-.008	-.008	-.013	-.012	-.006	-.007	-.025
	(.013)	(.014)	(.013)	(.052)	(.012)	(.012)	(.012)	(.045)
1(Signature)	.014	.018	.031 *	.037	.014	.018	.029 *	.027
	(.016)	(.017)	(.016)	(.024)	(.014)	(.016)	(.015)	(.020)
1(Non-Strict ID Law)	-.003	-.003	-.0004	-.002	-.003	-.003	-.005	-.011
	(.015)	(.015)	(.0120)	(.018)	(.013)	(.013)	(.010)	(.014)
Outcome Mean	.529	.529	.519	.519	.493	.493	.470	.470
N	306	306	306	306	306	306	306	306
Year FEs	✓	✓	✓	✓	✓	✓	✓	✓
State FEs	✓	✓	✓	✓	✓	✓	✓	✓
State-Year Controls		✓	✓	✓		✓	✓	✓
VEP/VAP Weights			✓	✓			✓	✓
State Linear Trends				✓				✓

Notes. The table reports estimated turnout effects based on McDonald's state turnout data, where the treatments are different, mutually exclusive ways in which states identify voters at the polls. Strict ID laws are the omitted category. Panels A and B include, respectively, election years 2004-2018 (2004 is the last year before strict ID laws were ever implemented) and 2008-2018 (i.e., matching the Catalist years). VEP and VAP stand for Voting-Eligible and Voting-Age Population, respectively. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).

*** p < .01, ** p < .05, * p < .10

Table A.19: Turnout Effects of Strict ID Laws by Election Timing

	Outcome: 1(Voted)				
	Outcome Mean	Impact Estimates			
	(1)	(2)	(3)	(4)	(5)
		Panel A. Presidential vs. Midterm			
1(Strict Law)×Presidential	.498	.002	.009	-.001	.006
		(.017)	(.015)	(.015)	(.018)
1(Strict Law)×Midterm	.358	-.012	-.006	-.012	-.005
		(.014)	(.011)	(.013)	(.013)
		Panel B. First Election vs. Following Ones			
1(Strict Law)×Following Elections	.414	-.007	.002	-.019	.002
		(.014)	(.011)	(.019)	(.012)
1(Strict Law)×First Election	.360	-.007	-.003	-.008	-.003
		(.015)	(.013)	(.014)	(.016)
Year FEs		✓	✓	✓	✓
State FEs		✓	✓	✓	✓
State & Voter Controls			✓	✓	✓
State Linear Trends				✓	
Voter FEs					✓

Notes. The sample includes registered and unregistered voters. Panel A explores heterogeneous effects in presidential vs. midterm elections, while Panel B compares effects in the election that immediately follows the laws' implementation and in following elections. Column (1) reports mean turnout in the interacting category. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).
*** $p < .01$, ** $p < .05$, * $p < .10$

Table A.20: Effects of Strict ID Laws on CCES Voter Activities

	Donated to a Candidate or Campaign		Amount Donated		Attended Political Meetings		Posted a Campaign Sign		Volunteered for a Campaign	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
					Panel A. Average Effect					
1(Strict Law)	.007	-.0001	4.6	5.1	-.005	-.008 *	-.013	-.017	.005	.002
	(.010)	(.0099)	(22.5)	(22.1)	(.005)	(.004)	(.016)	(.015)	(.008)	(.008)
Year and State FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓		✓		✓		✓		✓
					Panel B. Whites vs. Non-Whites					
1(Strict ID Law)×White	.006	-.001	7.7	6.0	-.005	-.009 *	-.016	-.019	.003	-.0007
	(.011)	(.011)	(20.1)	(19.9)	(.005)	(.005)	(.016)	(.016)	(.009)	(.0082)
1(Strict ID Law)×non-White	.007	.004	1.4	2.2	-.007	-.007	-.007	-.007	.013 *	.012
	(.015)	(.014)	(56.3)	(54.5)	(.010)	(.009)	(.013)	(.013)	(.007)	(.007)
$\beta^{nonwhite}$ - β^{white}	.001	.006	-6.3	-3.8	-.002	.002	.009	.012	.010 *	.012 **
	(.015)	(.014)	(53.3)	(52.6)	(.010)	(.010)	(.008)	(.008)	(.005)	(.005)
Race-by-Year FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Race-by-State FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓		✓		✓		✓		✓
Outcome Mean	.270	.270	116.937	116.937	.147	.147	.209	.209	.082	.082
N	302,496	302,496	272,283	272,283	272,283	272,283	272,283	272,283	272,283	272,283

Notes. The table reports estimated effects on the CCES campaign engagement variables used to construct the summary index of voter activity used as outcome in Table IV, columns (3) and (4). Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).
*** p < .01, ** p < .05, * p < .10

Table A.21: Effects of Strict ID Laws on Campaign Expenditures

	Total Expenditures ln($1/100k residents)		Campaign-Related Expenditures ln($1/100k residents)		TV Ad Expenditures ln($1/100k residents)	
	(1)	(2)	(3)	(4)	(5)	(6)
1(Strict ID Law)	.045	.061	.107	.043	-.067	.106
	(.100)	(.098)	(.146)	(.137)	(.390)	(.381)
Year & State FEs	✓	✓	✓	✓	✓	✓
State Controls		✓		✓		✓
Outcome Mean	12.489	12.489	9.946	9.946	13.280	13.280
N	408	408	408	408	357	357

Notes. The table reports estimates from state-level regressions. Regressions in columns (1)-(4) are based on expenditures data for candidates to the House of Representatives from the Center of Responsive Politics for 2004-2018. Regressions in columns (5)-(6) are based on data from the Wisconsin Advertising Project and the Wesleyan Media Project and cover all elections, 2004-2018, but 2006. The outcome for columns (1)-(2) is the log of total expenditures of candidates running to the House of Representatives, per 100k residents. The outcome for columns (3)-(4) is the log of campaign-related expenditures of candidates running to the House of Representatives, per 100k residents. The outcome for columns (5)-(6) is the estimated total in-state TV ad expenditures across down-ballot, gubernatorial, congressional, and presidential candidates. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).

*** p < .01, ** p < .05, * p < .10

Table A.22: Effects of Strict ID Laws on CCES Campaign Contact and CCES Voter Activity by Detailed Race

	Was Contacted by Campaign		Index of Voter Activity	
	(1)	(2)	(3)	(4)
	Panel A. Whites vs. Non-Whites			
1(Strict ID Law)×White	.006	.004	-.003	-.011
	(.021)	(.020)	(.017)	(.016)
1(Strict ID Law)×non-White	.047 **	.046 ***	.002	.001
	(.019)	(.016)	(.015)	(.014)
$\beta^{nonwhite} - \beta^{white}$	.041 **	.042 ***	.005	.011
	(.016)	(.015)	(.011)	(.010)
	Panel B. By Detailed Race			
1(Strict ID Law)×White	.006	.004	-.003	-.011
	(.021)	(.020)	(.017)	(.016)
1(Strict ID Law)×Hispanic	.049 ***	.047 ***	-.017	-.025
	(.017)	(.015)	(.024)	(.025)
1(Strict ID Law)×Black	.033	.030	.028 *	.026
	(.026)	(.026)	(.016)	(.017)
1(Strict ID Law)×Other Race	.072 ***	.079 ***	-.032	-.027
	(.026)	(.025)	(.033)	(.028)
$\beta^{hispanic} - \beta^{white}$	.044 **	.043 **	-.014	-.015
	(.020)	(.020)	(.024)	(.023)
$\beta^{black} - \beta^{white}$	.028	.026	.031 *	.037 **
	(.022)	(.020)	(.015)	(.017)
$\beta^{other} - \beta^{white}$	.067 **	.075 ***	-.029	-.017
	(.026)	(.027)	(.026)	(.021)
Race-by-Year FEs	✓	✓	✓	✓
Race-by-State FEs	✓	✓	✓	✓
State & Voter Controls		✓		✓
Outcome Mean	.640	.640	.000	.000
N	221,926	221,926	308,704	308,704

Notes. This table reports impact estimates on CCES campaign contact and CCES voter activities across white and non-white voters (Panel A) and separately by detailed race (Panel B). Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).

*** $p < .01$, ** $p < .05$, * $p < .10$

Table A.23: Effects of Strict ID Laws on Non-Preventable Frauds

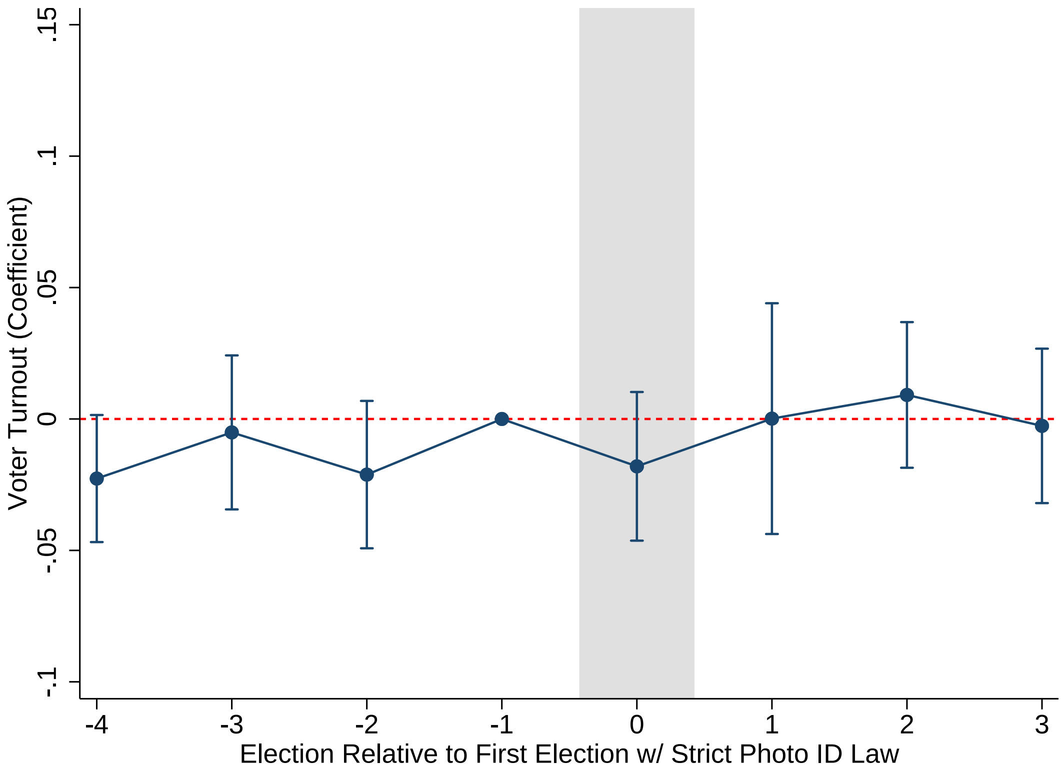
	People Cast Other Voters' Absentee Ballots		Officials Change Vote Counts		People Steal/Tamper with Ballots	
	(1)	(2)	(3)	(4)	(5)	(6)
1(Strict ID Law)	.008	.003	.014	.012	.001	.005
	(.023)	(.023)	(.014)	(.014)	(.015)	(.015)
Year & State FEs	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓		✓		✓
Outcome Mean	.261	.261	.190	.190	.188	.189
N	30,535	30,424	30,539	30,429	42,518	42,307

Notes. The table reports estimated effects on the SPAE measures of perceived electoral integrity used to construct the summary index of perceived fraud used as outcome in Table V, columns (9) and (10), and not already reported as outcomes in that table. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).

*** $p < .01$, ** $p < .05$, * $p < .10$

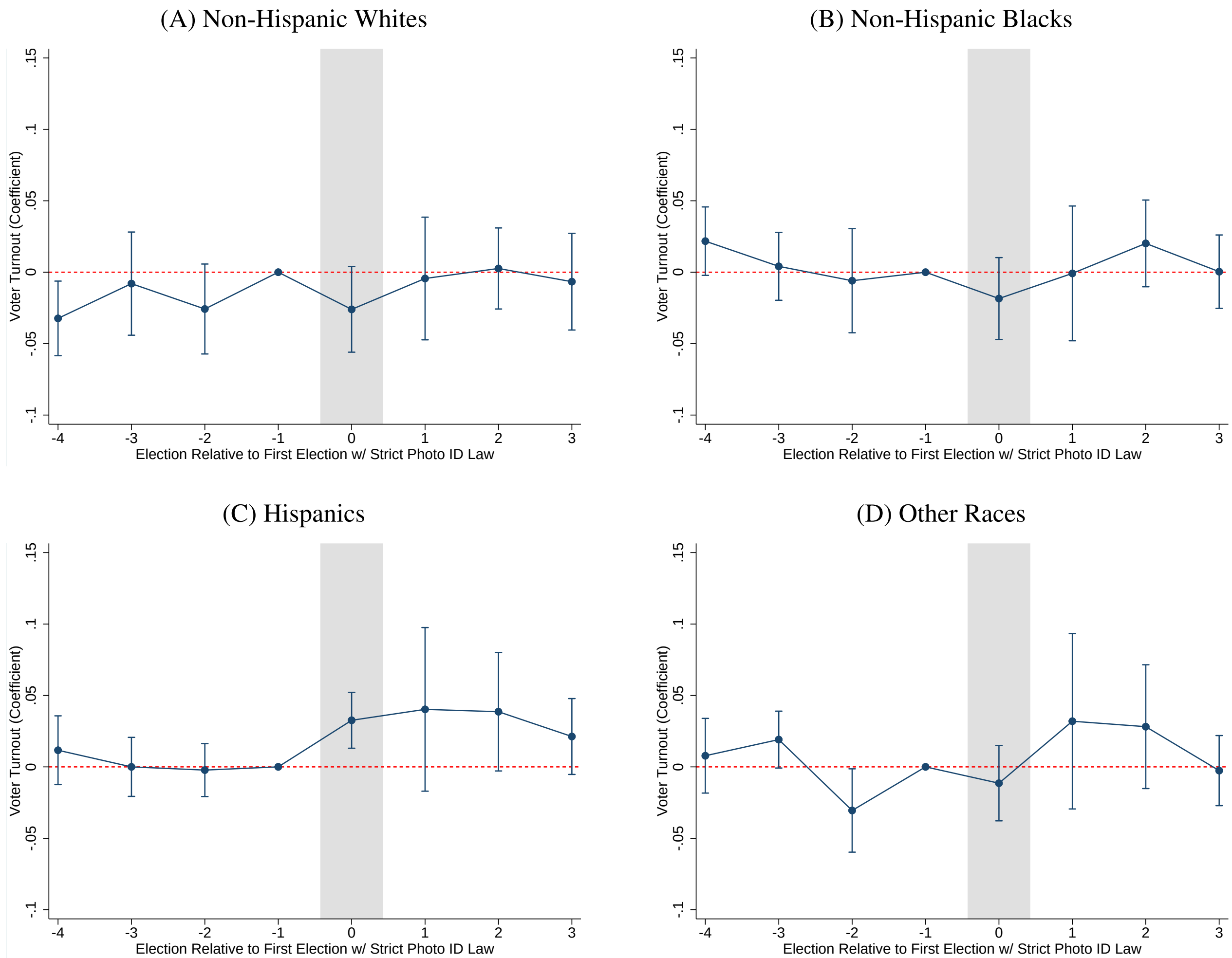
1.5. Effects of Strict Photo ID Laws

Figure A.5: Event-Study Graph of the Turnout Effects of Strict Photo ID Laws



The figure replicates Figure I using strict photo (instead of strict) ID laws as treatment. The underlying regression controls for a dummy identifying state-years with strict, non-photo ID laws.

Figure A.6: Event-Study Graphs of the Turnout Effects of Strict Photo ID Laws by Race



The figure replicates Figure II using strict photo (instead of strict) ID laws as treatment. The underlying regressions control for a dummy identifying state-years with strict, non-photo ID laws.

Table A.24: Turnout and Registration Effects of Strict Photo ID Laws

	Outcome:							
	1(Voted)				1(Registered)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A. Only Registered Voters							
1(Strict Photo ID Law)	-.004	-.004	-.021	-.013	-	-	-	-
	(.011)	(.009)	(.017)	(.015)				
Outcome Mean	.620	.620	.620	.620				
	Panel B. Registered and Unregistered Voters							
1(Strict Photo ID Law)	-.010	-.004	-.017	-.004	-.016	-.005	-.011 *	-.001
	(.013)	(.011)	(.011)	(.012)	(.012)	(.011)	(.006)	(.011)
Outcome Mean	.428	.428	.428	.428	.686	.686	.686	.686
Year FEs	✓	✓	✓	✓	✓	✓	✓	✓
State FEs	✓	✓	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓	✓	✓		✓	✓	✓
State Linear Trends			✓				✓	
Voter FEs				✓				✓

Notes. This table replicates Table I using strict photo (instead of strict) ID laws as treatment. To avoid pooling together control states and state-years with strict, non-photo laws, all regressions in this table control for a dummy identifying state-years with strict, non-photo ID laws. These state-years are 2012 Virginia, 2014 and 2018 North Dakota, as well as 2008-2018 Arizona and Ohio, which implemented a strict, non-photo ID law throughout the sample period. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).

*** $p < .01$, ** $p < .05$, * $p < .10$

Table A.25: Effects of Strict Photo ID Laws on Aggregate Outcomes

	(1)	(2)	(3)	(4)
	Panel A. Ballots Cast/VEP (McDonald's Data)			
1(Strict Photo ID Law)	-.001	-.002	-.002	-.010
	(.012)	(.014)	(.013)	(.014)
Outcome Mean	.528	.528	.517	.517
N	408	408	408	408
Year FEs	✓	✓	✓	✓
State FEs	✓	✓	✓	✓
State-Year Controls		✓	✓	✓
VEP Weights			✓	✓
State Linear Trends				✓
	Panel B. Democratic 2-Party Vote Share			
1(Strict Photo ID Law)	.00015	.008	.0003	-
	(.02095)	(.019)	(.0129)	-
Outcome Mean	.520	.520	.520	-
N	3,684	3,684	3,684	-
Year FEs	✓	✓	✓	
State FEs	✓	✓	✓	
State-Year Controls		✓	✓	
State Linear Trends			✓	

Notes. This table replicates Table II using strict photo (instead of strict) ID laws as treatment. Similarly to Online Appendix Table A.24, all regressions control for a dummy identifying state-years with strict non-photo ID laws. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).

*** $p < .01$, ** $p < .05$, * $p < .10$

Table A.26: Turnout Effects of Strict Photo ID Laws by Race

	Outcome: 1(Voted)				
	Outcome Mean	Impact Estimates			
	(1)	(2)	(3)	(4)	(5)
		Panel A. Whites vs. Non-Whites			
1(Strict Photo ID Law)×White	.458	-.010	-.007		-.008
		(.014)	(.012)		(.014)
1(Strict Photo ID Law)×non-White	.340	.004	.004		.006
		(.013)	(.009)		(.010)
$\beta^{nonwhite}$ - β^{white}		.014 *	.010	.007	.015
		(.008)	(.008)	(.007)	(.010)
		Panel B. By Detailed Race			
1(Strict Photo ID Law)×White	.458	-.010	-.006		-.008
		(.014)	(.012)		(.014)
1(Strict Photo ID Law)×Hispanic	.295	.024 *	.022 ***		.025 **
		(.014)	(.008)		(.010)
1(Strict Photo ID Law)×Black	.380	-.012	-.009		-.007
		(.013)	(.011)		(.011)
1(Strict Photo ID Law)×Other Race	.330	.008	.003		.003
		(.026)	(.019)		(.020)
$\beta^{hispanic}$ - β^{white}		.034 ***	.028 ***	.026 ***	.033 **
		(.011)	(.010)	(.006)	(.013)
β^{black} - β^{white}		-.002	-.002	-.003	.001
		(.008)	(.006)	(.006)	(.007)
β^{other} - β^{white}		.018	.009	-.002	.011
		(.015)	(.009)	(.006)	(.010)
Race-by-Year FEs		✓	✓	✓	✓
Race-by-State FEs		✓	✓	✓	✓
State & Voter Controls			✓	✓	✓
State-by-Year FEs				✓	
Voter FEs					✓

Notes. This table replicates Table III using strict photo (instead of strict) ID laws as treatment. Column (1) reports mean turnout in the interacting category. Similarly to Online Appendix Table A.24, all regressions control for a dummy identifying state-years with strict non-photo ID laws, along with its interactions with a non-white voter dummy (Panel A) or with dummies for detailed race categories (Panel B). Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).

*** $p < .01$, ** $p < .05$, * $p < .10$

Table A.27: Effects of Strict Photo ID Laws on CCES Campaign Contact, Voter Activity, and DIME Campaign Contributions

	Was Contacted by Campaign		Index of Voter Activity		Contributions ln($1k/100k residents)	
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A. Average Effect			
1(Strict Photo ID Law)	.008	.007	-.005	-.011	.001	.014
	(.019)	(.018)	(.016)	(.016)	(.126)	(.125)
Year & State FEs	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓		✓		✓
			Panel B. Whites vs. Non-Whites			
1(Strict Photo ID Law)×White	-.002	-.004	-.006	-.013		
	(.020)	(.019)	(.018)	(.017)		
1(Strict Photo ID Law)×non-White	.040 **	.039 **	-.0002	-.002		
	(.017)	(.015)	(.0151)	(.014)		
$\beta^{nonwhite}$ - β^{white}	.042 **	.042 ***	.006	.012		
	(.017)	(.015)	(.012)	(.011)		
Race-by-Year FEs	✓	✓	✓	✓	✓	✓
Race-by-State FEs	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓		✓		✓
Outcome Mean	.640	.640	.000	.000	14.682	14.682
N	221,926	221,926	308,704	308,704	408	408

Notes. This table replicates Table IV using strict photo (instead of strict) ID laws as treatment. Similarly to Online Appendix Table A.24, all regressions control for a dummy identifying state-years with strict non-photo ID laws, along with its interaction with a non-white voter dummy (in Panel B). Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).
*** p < .01, ** p < .05, * p < .10

Table A.28: Effects of Strict Photo ID Laws on Reported and Perceived Frequency of Voter Fraud

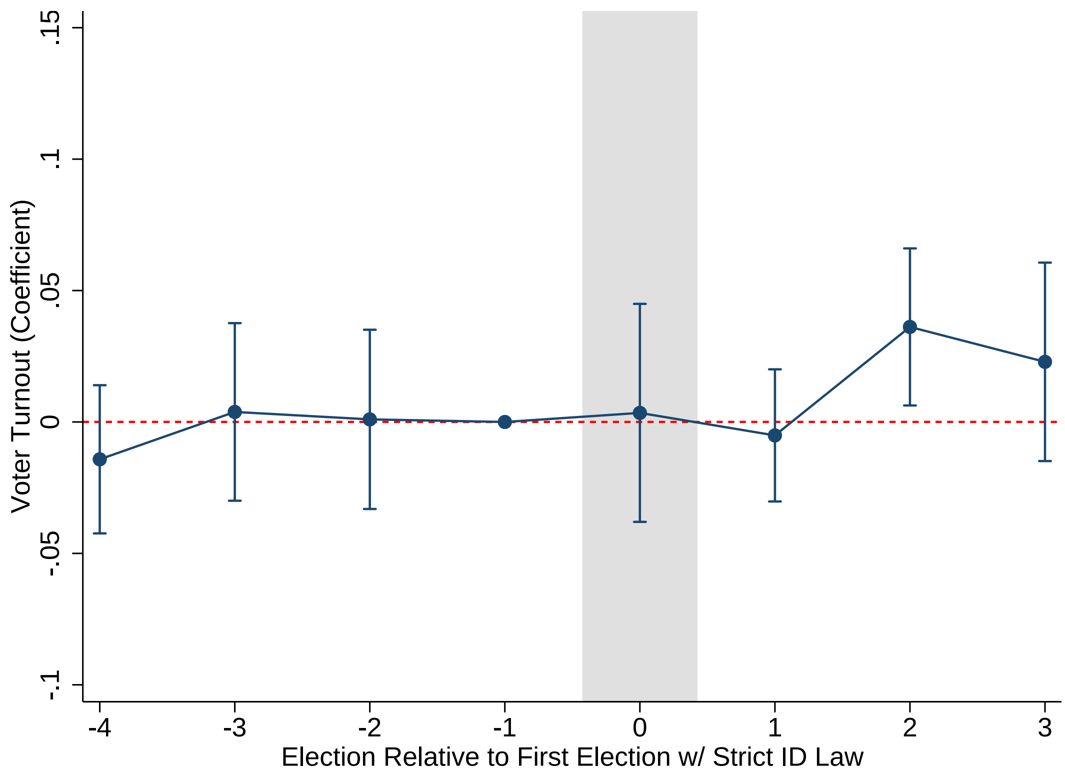
	News21 Frauds/100k Residents		News21 Preventable Frauds/100k Residents		Heritage Frauds/100k Residents		Heritage Preventable Frauds/100k Residents	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1(Strict Photo ID Law)	.070	.049	.025	.011	.005	.001	.011	.007
	(.182)	(.176)	(.074)	(.079)	(.009)	(.008)	(.007)	(.008)
Year & State FEs	✓	✓	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓		✓		✓		✓
Outcome Mean	.078	.078	.033	.033	.020	.020	.013	.013
N	459	459	459	459	765	765	765	765

	SPAE Perceived Fraud Index		SPAE Voter Impersonation		SPAE Multiple Voting		SPAE Non-Citizen Voting		ANES Fair Election	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
1(Strict Photo ID Law)	.003	.008	-.005	-.003	-.008	-.009	-.026	-.030	.018	.031
	(.034)	(.033)	(.019)	(.017)	(.026)	(.026)	(.024)	(.024)	(.049)	(.041)
Year & State FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓		✓		✓		✓		✓
Outcome Mean	.000	.000	.210	.210	.209	.209	.275	.275	.698	.698
N	42,600	42,385	42,488	42,277	30,534	30,424	30,533	30,423	11,396	11,396

Notes. This table replicates Table V using strict photo (instead of strict) ID laws as treatment. As in Online Appendix Table A.24, all regressions in this table control for a dummy identifying state-years with strict non-photo ID laws. Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).
*** p < .01, ** p < .05, * p < .10

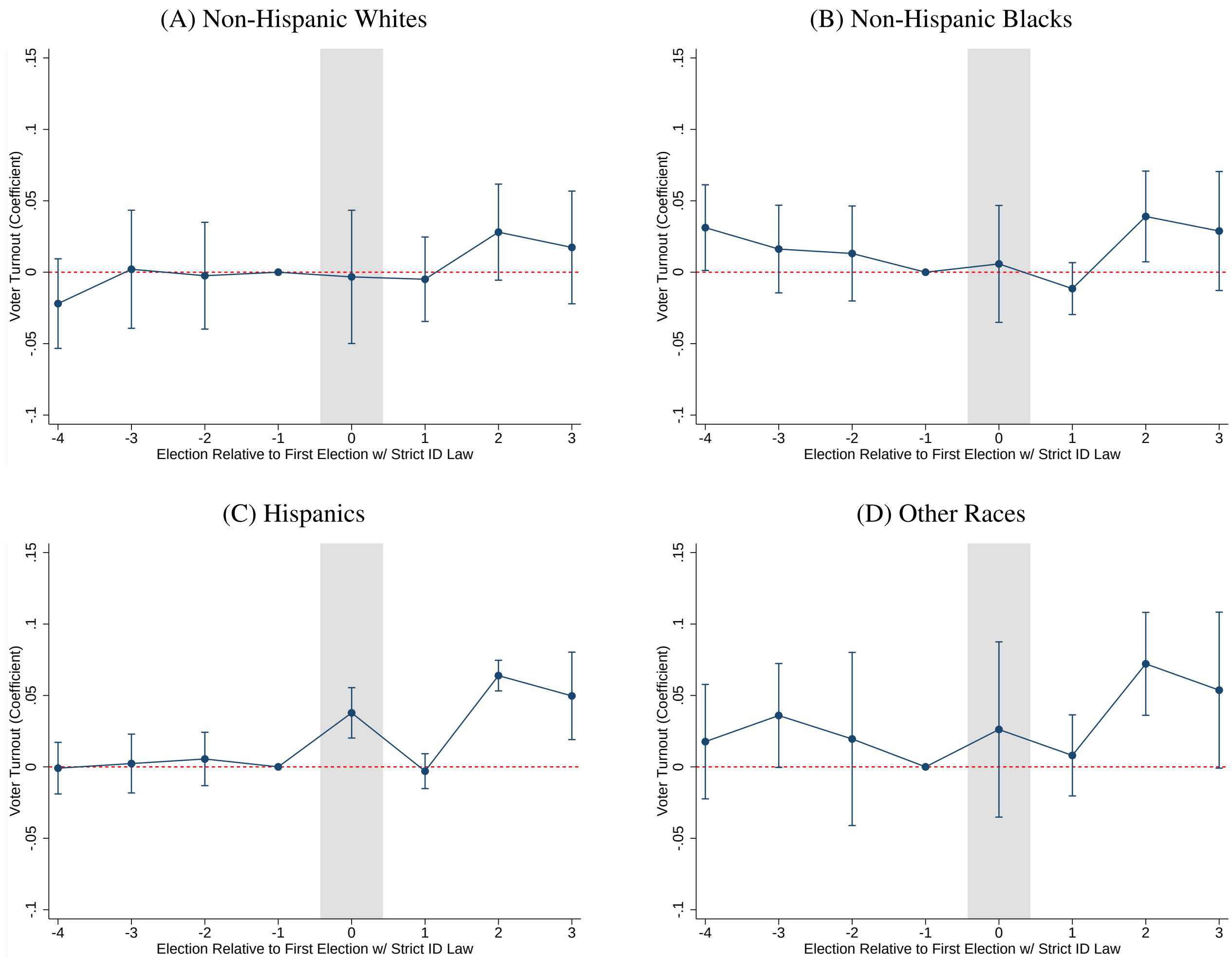
1.6. Effects of Strict ID Laws After Transforming Into a Staggered Design

Figure A.7: Event-Study Graph of the Turnout Effects of Strict ID Laws – Staggered Design



The figure replicates Figure I leaving $ID^{\tau=1}_{ND,2016} = ID^{\tau=1}_{TX,2016} = ID^{\tau=2}_{TX,2018} = 1$, instead of setting them equal to 0.

Figure A.8: Event-Study Graphs of the Turnout Effects of Strict ID Laws by Race – Staggered Design



The figure replicates Figure II leaving $ID^{\tau=1}_{ND,2016} = ID^{\tau=1}_{TX,2016} = ID^{\tau=2}_{TX,2018} = 1$, instead of setting them equal to 0.

Table A.29: Turnout and Registration Effects of Strict ID Laws – Staggered Design

	Outcome:							
	1(Voted)				1(Registered)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A. Only Registered Voters							
1(Strict ID Law)	.013	.014	-.018	.015	-	-	-	-
	(.009)	(.010)	(.027)	(.018)				
Outcome Mean	.620	.620	.620	.620				
	Panel B. Registered and Unregistered Voters							
1(Strict ID Law)	.006	.010	-.010	.012	-.011	-.002	.002	.004
	(.013)	(.010)	(.020)	(.013)	(.011)	(.009)	(.007)	(.009)
Outcome Mean	.428	.428	.428	.427	.686	.686	.686	.686
Year FEs	✓	✓	✓	✓	✓	✓	✓	✓
State FEs	✓	✓	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓	✓	✓		✓	✓	✓
State Linear Trends			✓				✓	
Voter FEs				✓				✓

Notes. The table replicates Table I after transforming strict ID laws into an absorbing state (i.e., we assign positive treatment to 2016 ND and to 2016 and 2018 TX). Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).
*** $p < .01$, ** $p < .05$, * $p < .10$

Table A.30: Effects of Strict ID Laws on Aggregate Outcomes – Staggered Design

	(1)	(2)	(3)	(4)
	Panel A. Ballots Cast/VEP (McDonald's Data)			
1(Strict ID Law)	.008	.009	.009	.011
	(.011)	(.012)	(.011)	(.015)
Outcome Mean	.528	.528	.517	.517
N	408	408	408	408
Year FEs	✓	✓	✓	✓
State FEs	✓	✓	✓	✓
State-Year Controls		✓	✓	✓
VEP Weights			✓	✓
State Linear Trends				✓
	Panel B. Democratic 2-Party Vote Share			
1(Strict ID Law)	.006	.013	.015	-
	(.019)	(.016)	(.017)	-
Outcome Mean	.520	.520	.520	-
N	3,684	3,684	3,684	-
Year FEs	✓	✓	✓	
State FEs	✓	✓	✓	
State-Year Controls		✓	✓	
State Linear Trends			✓	

Notes. The table replicates Table II after transforming strict ID laws into an absorbing state (i.e., we assign positive treatment to 2016 ND and to 2016 and 2018 TX). Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.).
*** $p < .01$, ** $p < .05$, * $p < .10$

Table A.31: Turnout Effects of Strict ID Laws by Race – Staggered Design

	Outcome: 1(Voted)				
	Outcome Mean	Impact Estimates			
	(1)	(2)	(3)	(4)	(5)
		Panel A. Whites vs. Non-Whites			
1(Strict ID Law)×White	.458	.001	.006		.007
		(.013)	(.011)		(.013)
1(Strict ID Law)×non-White	.340	.014	.019		.025 **
		(.012)	(.008)		(.011)
$\beta^{nonwhite} - \beta^{white}$		.012 *	.012 **	.006	.018 **
		(.007)	(.006)	(.005)	(.008)
		Panel B. By Detailed Race			
1(Strict ID Law)×White	.458	.001	.007		.007
		(.013)	(.011)		(.013)
1(Strict ID Law)×Hispanic	.295	.026 *	.035 ***		.044 ***
		(.013)	(.006)		(.007)
1(Strict ID Law)×Black	.380	-.004	.001		.005
		(.012)	(.010)		(.012)
1(Strict ID Law)×Other Race	.330	.030	.027 **		.031 **
		(.019)	(.011)		(.013)
$\beta^{hispanic} - \beta^{white}$		.025 **	.028 ***	.019 ***	.037 ***
		(.011)	(.008)	(.004)	(.010)
$\beta^{black} - \beta^{white}$		-.006	-.006	-.005	-.002
		(.006)	(.005)	(.005)	(.006)
$\beta^{other} - \beta^{white}$		.028 **	.020 ***	.005	.024 ***
		(.012)	(.005)	(.004)	(.006)
Race-by-Year FEs		✓	✓	✓	✓
Race-by-State FEs		✓	✓	✓	✓
State & Voter Controls			✓	✓	✓
State-by-Year FEs				✓	
Voter FEs					✓

Notes. The table replicates Table III after transforming strict ID laws into an absorbing state (i.e., we assign positive treatment to 2016 ND and to 2016 and 2018 TX). Standard errors clustered at the state level are reported in parentheses (51 clusters: all 50 states plus D.C.). *** p < .01, ** p < .05, * p < .10

1.7. *Wild Bootstrap P-Values*

Table A.32: Turnout and Registration Effects of Strict ID Laws: Asymptotic vs. Wild Bootstrap P-Values

	Outcome:							
	1(Voted)				1(Registered)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A. Only Registered Voters							
1(Strict ID Law)	-.001	-.001	-.011	-.008	-	-	-	-
	[.942]	[.928]	[.580]	[.654]				
	{.950}	{.934}	{.674}	{.683}				
Outcome Mean	.620	.620	.620	.620				
	Panel B. Registered and Unregistered Voters							
1(Strict ID Law)	-.007	-.001	-.008	-.001	-.015	-.004	-.008	-.001
	[.628]	[.941]	[.565]	[.931]	[.215]	[.692]	[.248]	[.939]
	{.690}	{.945}	{.693}	{.931}	{.293}	{.721}	{.543}	{.922}
Outcome Mean	.428	.428	.428	.428	.686	.686	.686	.686
Year FEs	✓	✓	✓	✓	✓	✓	✓	✓
State FEs	✓	✓	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓	✓	✓		✓	✓	✓
State Linear Trends			✓				✓	
Voter FEs				✓				✓

Notes. This table reports the same point estimates as Table I. State-clustered asymptotic p-values are reported in brackets. Wild bootstrap state-clustered p-values are reported in braces. Bootstrap p-values are based on Webb weights and 999 repetitions, where this number was chosen following Davidson and MacKinnon (2000) to ensure that the significance level times the sum of the number of bootstraps and one is an integer. To account for the possibility of having too few treated clusters, we follow MacKinnon and Webb (2020) and assign bootstrap weights at a finer level (i.e., by counties) than the level of clustering of the standard errors (i.e., by states). Bootstrap p-values are computed using the Stata *boottest* command (Roodman et al., 2019). For computational reasons, bootstrap p-values in voter FEs specifications rely on the Frisch-Waugh-Lovell theorem. From both the treatment and the outcome, we first partial out voter FEs and the full set of controls used in columns (2) and (6). We then compute bootstrap p-values using the residualized outcome and treatment.

Table A.33: Effects of Strict ID Laws on Aggregate Outcomes: Asymptotic vs. Wild Bootstrap P-Values

	(1)	(2)	(3)	(4)
	Panel A. Ballots Cast/VEP (McDonald's Data)			
1(Strict ID Law)	.006	.006	.001	.002
	[.599]	[.649]	[.965]	[.902]
	{.598}	{.664}	{.964}	{.896}
Outcome Mean	.528	.528	.517	.517
N	408	408	408	408
Year FEs	✓	✓	✓	✓
State FEs	✓	✓	✓	✓
State-Year Controls		✓	✓	✓
VEP Weights			✓	✓
State Linear Trends				✓
	Panel B. Democratic 2-Party Vote Share			
1(Strict ID Law)	.001	.009	.005	-
	[.978]	[.626]	[.626]	-
	{.977}	{.619}	{.657}	-
Outcome Mean	.520	.520	.520	-
N	3,684	3,684	3,684	-
Year FEs	✓	✓	✓	
State FEs	✓	✓	✓	
State-Year Controls		✓	✓	
State Linear Trends			✓	

Notes. This table reports the same point estimates as Table II. State-clustered asymptotic p-values are reported in brackets. Wild bootstrap state-clustered p-values are reported in braces. Bootstrap p-values are based on Webb weights and 999 repetitions, where this number was chosen following Davidson and MacKinnon (2000) to ensure that the significance level times the sum of the number of bootstraps and one is an integer. Bootstrap p-values are computed using the Stata *boottest* command (Roodman et al., 2019).

Table A.34: Turnout Effects of Strict ID Laws by Race: Asymptotic vs. Wild Bootstrap P-Values

	Outcome: 1(Voted)				
	Outcome Mean	Impact Estimates			
	(1)	(2)	(3)	(4)	(5)
		Panel A. Whites vs. Non-Whites			
1(Strict ID Law)×White	.458	-.006	-.003		-.005
		[.664]	[.807]		[.768]
		{.719}	{.849}		{.784}
1(Strict ID Law)×non-White	.340	.006	.006		.009
		[.653]	[.554]		[.450]
		{.697}	{.598}		{.397}
$\beta^{nonwhite} - \beta^{white}$		.013	.010	.007	.014
		[.108]	[.202]	[.353]	[.152]
		{.202}	{.320}	{.432}	{.124}
		Panel B. By Detailed Race			
1(Strict ID Law)×White	.458	-.006	-.003		-.005
		[.664]	[.808]		[.768]
		{.719}	{.849}		{.784}
1(Strict ID Law)×Hispanic	.295	.025	.022		.026
		[.091]	[.006]		[.014]
		{.260}	{.077}		{.009}
1(Strict ID Law)×Black	.380	-.009	-.006		-.004
		[.524]	[.639]		[.798]
		{.548}	{.636}		{.804}
1(Strict ID Law)×Other	.330	.013	.007		.008
		[.653]	[.750]		[.746]
		{.820}	{.868}		{.841}
$\beta^{hispanic} - \beta^{white}$		.032	.026	.026	.030
		[.007]	[.021]	[.000]	[.034]
		{.075}	{.167}	{.038}	{.075}
$\beta^{black} - \beta^{white}$		-.003	-.003	-.003	.001
		[.745]	[.682]	[.614]	[.868]
		{.770}	{.683}	{.656}	{.861}
$\beta^{other} - \beta^{white}$		.019	.010	-.001	.013
		[.237]	[.314]	[.805]	[.267]
		{.424}	{.495}	{.859}	{.298}
Race-by-Year FEs		✓	✓	✓	✓
Race-by-State FEs		✓	✓	✓	✓
State & Voter Controls			✓	✓	✓
State-by-Year FEs				✓	
Voter FEs					✓

Notes. This table reports the same point estimates as Table III. State-clustered asymptotic p-values are reported in brackets. Wild bootstrap state-clustered p-values are reported in braces. See notes to Online Appendix Table A.32 for details on the bootstrap procedure. Column (1) reports mean turnout in the interacting category.

Table A.35: Effects of Strict ID Laws on CCES Campaign Contact, Voter Activity, and DIME Campaign Contributions: Asymptotic vs. Wild Bootstrap P-Values

	Was Contacted by Campaign		Index of Voter Activity		Contributions ln($1k/100k residents)	
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A. Average Effect			
1(Strict ID Law)	.015	.014	-.002	-.008	.024	.031
	[.457]	[.456]	[.911]	[.602]	[.818]	[.767]
	{.482}	{.526}	{.951}	{.835}	{.807}	{.759}
Year & State FEs	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓		✓		✓
			Panel B. Whites vs. Non-Whites			
1(Strict ID Law)×White	.006	.004	-.003	-.011		
	[.781]	[.845]	[.853]	[.515]		
	{.791}	{.836}	{.932}	{.773}		
1(Strict ID Law)×non-White	.047	.046	.002	.001		
	[.016]	[.007]	[.895]	[.967]		
	{.133}	{.110}	{.919}	{.986}		
$\beta^{nonwhite}$ - β^{white}	.041	.042	.005	.011		
	[.014]	[.007]	[.646]	[.268]		
	{.039}	{.030}	{.653}	{.324}		
Race-by-Year FEs	✓	✓	✓	✓	✓	✓
Race-by-State FEs	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓		✓		✓
Outcome Mean	.640	.640	.000	.000	14.682	14.682
N	221,926	221,926	308,704	308,704	408	408

Notes. This table reports the same point estimates as Table IV. State-clustered asymptotic p-values are reported in brackets. Wild bootstrap state-clustered p-values are reported in braces. Bootstrap p-values are based on Webb weights and 999 repetitions, where this number was chosen following Davidson and MacKinnon (2000) to ensure that the significance level times the sum of the number of bootstraps and one is an integer. Bootstrap p-values are computed using the Stata *boottest* command (Roodman et al., 2019).

Table A.36: Effects of Strict ID Laws on Reported and Perceived Frequency of Voter Fraud: Asymptotic vs. Wild Bootstrap P-Values

	News21 Frauds/100k Residents		News21 Preventable Frauds/100k Residents		Heritage Frauds/100k Residents		Heritage Preventable Frauds/100k Residents	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1(Strict ID Law)	.045	.025	.014	.001	.009	.006	.013	.011
	[.688]	[.818]	[.760]	[.978]	[.215]	[.437]	[.046]	[.156]
	{.614}	{.736}	{.647}	{.966}	{.234}	{.411}	{.052}	{.165}
Year and State FEs	✓	✓	✓	✓	✓	✓	✓	✓
State Controls		✓		✓		✓		✓
Outcome Mean	.078	.078	.033	.033	.020	.020	.013	.013
N	459	459	459	459	765	765	765	765

	SPAE Perceived Fraud Index		SPAE Voter Impersonation		SPAE Multiple Voting		SPAE Non-Citizen Voting		ANES Fair Election	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
1(Strict ID Law)	.003	.007	-.004	-.002	-.009	-.013	-.020	-.024	.008	.020
	[.917]	[.822]	[.811]	[.881]	[.699]	[.550]	[.418]	[.335]	[.856]	[.590]
	{.926}	{.838}	{.830}	{.881}	{.732}	{.614}	{.473}	{.432}	{.888}	{.757}
Year & State FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓		✓		✓		✓		✓
Outcome Mean	.000	.000	.210	.210	.209	.209	.275	.275	.698	.698
N	42,600	42,385	42,488	42,277	30,534	30,424	30,533	30,423	11,396	11,396

Notes. This table reports the same point estimates as Table V. State-clustered asymptotic p-values are reported in brackets. Wild bootstrap state-clustered p-values are reported in braces. Bootstrap p-values are based on Webb weights and 999 repetitions, where this number was chosen following Davidson and MacKinnon (2000) to ensure that the significance level times the sum of the number of bootstraps and one is an integer. Bootstrap p-values are computed using the Stata *boottest* command (Roodman et al., 2019).

1.8. Randomization Inference P-Values

Table A.37: Turnout and Registration Effects of Strict ID Laws: Asymptotic vs. Randomization Inference P-Values

	Outcome:							
	1(Voted)				1(Registered)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A. Only Registered Voters							
1(Strict ID Law)	-.001	-.001	-.011	-.008	-	-	-	-
	[.942]	[.928]	[.580]	[.661]				
	{.965}	{.953}	{.713}	{.731}				
	<.969>	<.965>	<.732>	<.695>				
Outcome Mean	.620	.620	.620	.620				
	Panel B. Registered and Unregistered Voters							
1(Strict ID Law)	-.007	-.001	-.008	-.001	-.015	-.004	-.008	-.001
	[.628]	[.941]	[.565]	[.923]	[.215]	[.692]	[.248]	[.907]
	{.731}	{.955}	{.704}	{.934}	{.355}	{.773}	{.392}	{.920}
	<.691>	<.947>	<.706>	<.919>	<.339>	<.755>	<.319>	<.910>
Outcome Mean	.428	.428	.428	.428	.686	.686	.686	.686
Year FEs	✓	✓	✓	✓	✓	✓	✓	✓
State FEs	✓	✓	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓	✓	✓		✓	✓	✓
State Linear Trends			✓				✓	
Voter FEs				✓				✓

Notes. This table reports the same point estimates as Table I, except for voter fixed effects impact estimates (columns (4) and (8)) that, for computational reasons, come from regressions run on a random 1 percent sample of voters from the Catalist data. State-clustered asymptotic p-values are reported in brackets. Randomization inference (RI) p-values based on t-statistics and regression coefficients are reported in braces and angle brackets, respectively. RI p-values are based on 999 permutations of the treatment and are computed using the Stata *ritest* command (Hess, 2017).

Table A.38: Effects of Strict ID Laws on Aggregate Outcomes: Asymptotic vs. Randomization Inference P-Values

	(1)	(2)	(3)	(4)
	Panel A. Ballots Cast/VEP (McDonald's Data)			
1(Strict ID Law)	.006	.006	.001	.002
	[.599]	[.649]	[.965]	[.902]
	{.606}	{.661}	{.967}	{.912}
	<.526>	<.547>	<.962>	<.918>
Outcome Mean	.528	.528	.517	.517
N	408	408	408	408
Year FEs	✓	✓	✓	✓
State FEs	✓	✓	✓	✓
State-Year Controls		✓	✓	✓
VEP Weights			✓	✓
State Linear Trends				✓
	Panel B. Democratic 2-Party Vote Share			
1(Strict ID Law)	.001	.009	.005	-
	[.978]	[.626]	[.626]	-
	{.980}	{.664}	{.693}	-
	<.973>	<.586>	<.759>	
Outcome Mean	.520	.520	.520	-
N	3,684	3,684	3,684	-
Year FEs	✓	✓	✓	
State FEs	✓	✓	✓	
State-Year Controls		✓	✓	
State Linear Trends			✓	

Notes. This table reports the same point estimates as Table II. State-clustered asymptotic p-values are reported in brackets. Randomization inference (RI) p-values based on t-statistics and regression coefficients are reported in braces and angle brackets, respectively. RI p-values are based on 999 permutations of the treatment and are computed using the Stata *ritest* command (Hess, 2017).

Table A.39: Turnout Effects of Strict ID Laws by Race: Asymptotic vs. Randomization Inference P-Values

	Outcome: 1(Voted)				
	Outcome Mean	Impact Estimates			
	(1)	(2)	(3)	(4)	(5)
		Panel A. Whites vs. Non-Whites			
1(Strict ID Law)×White	.458	-.006	-.003		-.005
		[.664]	[.807]		[.767]
		{.769}	{.877}		{.803}
		<.703>	<.829>		<.740>
1(Strict ID Law)×non-White	.340	.006	.006		.009
		[.653]	[.554]		[.452]
		{.795}	{.730}		{.622}
		<.791>	<.745>		<.663>
$\beta^{nonwhite}$ - β^{white}		.013	.010	.007	.013
		[.108]	[.202]	[.353]	[.176]
		{.262}	{.334}	{.520}	{.274}
		<.277>	<.330>	<.312>	<.212>
		Panel B. By Detailed Race			
1(Strict ID Law)×White	.458	-.006	-.003		-.005
		[.664]	[.808]		[.767]
		{.769}	{.877}		{.803}
		<.703>	<.829>		<.742>
1(Strict ID Law)×Hispanic	.295	.025	.022		.027
		[.091]	[.006]		[.008]
		{.350}	{.169}		{.119}
		<.404>	<.349>		<.284>
1(Strict ID Law)×Black	.380	-.009	-.006		-.005
		[.524]	[.639]		[.737]
		{.670}	{.740}		{.780}
		<.623>	<.717>		<.773>
1(Strict ID Law)×Other	.330	.013	.007		.007
		[.653]	[.750]		[.776]
		{.853}	{.867}		{.852}
		<.783>	<.754>		<.754>
$\beta^{hispanic}$ - β^{white}		.032	.026	.026	.032
		[.007]	[.021]	[.000]	[.037]
		{.118}	{.122}	{.045}	{.122}
		<.141>	<.102>	<.024>	<.059>
β^{black} - β^{white}		-.003	-.003	-.003	.0001
		[.745]	[.682]	[.614]	[.987]
		{.810}	{.751}	{.704}	{.992}
		<.815>	<.795>	<.687>	<.992>
β^{other} - β^{white}		.019	.010	-.001	.012
		[.237]	[.314]	[.805]	[.323]
		{.577}	{.504}	{.866}	{.418}
		<.499>	<.452>	<.874>	<.354>
Race-by-Year FEs		✓	✓	✓	✓
Race-by-State FEs		✓	✓	✓	✓
State & Voter Controls			✓	✓	✓
State-by-Year FEs				✓	
Voter FEs					✓

Notes. This table reports the same point estimates as Table III, except for voter fixed effects impact estimates (column (5)) that, for computational reasons, come from regressions run on a random 1 percent sample of voters from the Catalist data. State-clustered asymptotic p-values are reported in brackets. Randomization inference (RI) p-values based on t-statistics and regression coefficients are reported in braces and angle brackets, respectively. RI p-values are based on 999 permutations of the treatment and are computed using the Stata *ritest* command (Hess, 2017). Column (1) reports mean turnout in the interacting category.

Table A.40: Effects of Strict ID Laws on CCES Campaign Contact, Voter Activity, and DIME Campaign Contributions: Asymptotic vs. Randomization Inference P-Values

	Was Contacted by Campaign		Index of Voter Activity		Contributions ln($1k/100k residents)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Average Effect						
1(Strict ID Law)	.015	.014	-.002	-.008	.024	.031
	[.457]	[.456]	[.911]	[.602]	[.818]	[.767]
	{.578}	{.583}	{.931}	{.696}	{.809}	{.777}
	<.478>	<.499>	<.891>	<.509>	<.788>	<.746>
Year & State FEs	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓		✓		✓
Panel B. Whites vs. Non-Whites						
1(Strict ID Law)×White	.006	.004	-.003	-.011		
	[.781]	[.845]	[.853]	[.515]		
	{.835}	{.883}	{.869}	{.592}		
	<.776>	<.857>	<.803>	<.408>		
1(Strict ID Law)×non-White	.047	.046	.002	.001		
	[.016]	[.007]	[.895]	[.967]		
	{.138}	{.097}	{.916}	{.967}		
	<.138>	<.132>	<.914>	<.967>		
$\beta^{nonwhite} - \beta^{white}$	.041	.042	.005	.011		
	[.014]	[.007]	[.646]	[.268]		
	{.094}	{.079}	{.687}	{.387}		
	<.068>	<.056>	<.770>	<.541>		
Race-by-Year FEs	✓	✓	✓	✓	✓	✓
Race-by-State FEs	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓		✓		✓
Outcome Mean	.640	.640	.000	.000	14.682	14.682
N	221,926	221,926	308,704	308,704	408	408

Notes. This table reports the same point estimates as Table IV. State-clustered asymptotic p-values are reported in brackets. Randomization inference (RI) p-values based on t-statistics and regression coefficients are reported in braces and angle brackets, respectively. RI p-values are based on 999 permutations of the treatment and are computed using the Stata *ritest* command (Hess, 2017).

Table A.41: Effects of Strict ID Laws on Reported and Perceived Frequency of Voter Fraud: Asymptotic vs. Randomization Inference P-Values

	News21 Frauds/100k Residents		News21 Preventable Frauds/100k Residents		Heritage Frauds/100k Residents		Heritage Preventable Frauds/100k Residents			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
1(Strict ID Law)	.045	.025	.014	.001	.009	.006	.013	.011		
	[.688]	[.818]	[.760]	[.978]	[.215]	[.437]	[.046]	[.156]		
	{.786}	{.859}	{.861}	{.983}	{.232}	{.466}	{.084}	{.211}		
	<.521>	<.706>	<.720>	<.964>	<.337>	<.538>	<.090>	<.156>		
Year and State FEs	✓	✓	✓	✓	✓	✓	✓	✓		
State Controls		✓		✓		✓		✓		
Outcome Mean	.078	.078	.033	.033	.020	.020	.013	.013		
N	459	459	459	459	765	765	765	765		
	SPAE Perceived Fraud Index		SPAE Voter Impersonation		SPAE Multiple Voting		SPAE Non-Citizen Voting		ANES Fair Election	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
1(Strict ID Law)	.003	.007	-.004	-.002	-.009	-.013	-.020	-.024	.008	.020
	[.917]	[.822]	[.811]	[.881]	[.699]	[.550]	[.418]	[.335]	[.856]	[.590]
	{.920}	{.831}	{.832}	{.902}	{.766}	{.666}	{.525}	{.481}	{.890}	{.691}
	<.925>	<.820>	<.804>	<.888>	<.653>	<.515>	<.322>	<.245>	<.870>	<.639>
Year & State FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓		✓		✓		✓		✓
Outcome Mean	.000	.000	.210	.210	.209	.209	.275	.275	.698	.698
N	42,600	42,385	42,488	42,277	30,534	30,424	30,533	30,423	11,396	11,396

Notes. This table reports the same point estimates as Table V. State-clustered asymptotic p-values are reported in brackets. Randomization inference (RI) p-values based on t-statistics and regression coefficients are reported in braces and angle brackets, respectively. RI p-values are based on 999 permutations of the treatment and are computed using the Stata *ritest* command (Hess, 2017).