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Enrico Cantoni
Vincent Pons

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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 and 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 5.4 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.

Enrico Cantoni
University of Bologna
Department of Economics
Piazza Scaravilli 2, Bologna
Italy
enrico.cantoni@unibo.it

Vincent Pons
Harvard Business School
Morgan Hall 289
Soldiers Field
Boston, MA 02163
and NBER
vpons@hbs.edu

1 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; 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] These 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 be rejected unless they present proper ID to election officials within the next few days. Other states either do not request identification or allow voters without ID to sign an affidavit and cast a regular ballot.

The effects of these measures on overall participation are ex-ante ambiguous. While strict ID 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, the elderly, and poorer and less educated voters (Stewart, 2013; Ansolabehere and Hersh, 2017), who have long shown relatively low propensity to vote (Wolfinger and Rosenstone, 1980; Verba et al., 1995; Schlozman et al., 2012). Moreover, some citizens may become more likely to vote if the laws enhance their confidence in the fairness of the election, similarly to the participation boost of improving beliefs about ballot secrecy (Gerber et al., 2013b).

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, overall or for any subgroup defined by age, gender, race, or party affiliation. These results hold

[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.

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 2.0 and 2.7 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 1.3 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 0.3 percentage points.

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. 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 (2015), 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 were contacted by a campaign by 5.4 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.

Previous studies comparing turnout in states with and without voter ID laws have found either no effect (e.g., Mycoff et al., 2009; Erikson and Minnite, 2009; Highton, 2017; Pryor et al., 2019) or negative effects of up to 4 percentage points on overall participation or on the participation of Blacks and Hispanics (e.g., Alvarez et al., 2011; Government Accountability Office, 2014; Hajnal et al., 2017; Highton, 2017).[2] 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.

Second, 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.

Finally, 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

[2]Other studies use surveys or administrative records to directly count people prevented from voting due to lack of valid identification, and find small numbers (e.g., Ansolabehere, 2009; Henninger et al., 2018). However, administrative counts of people who go to the polls and cannot vote for lack of ID exclude voters deterred from even trying. Estimates based on survey responses might similarly be biased downwards, if non-voters underreport lacking a valid ID as the reason for choosing not to vote, or upwards, if those without an ID overreport their desire to vote.

population.

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 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. Our DD estimates use the same survey to 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 2 provides more information on Catalist's voter-level panel data and the other datasets we use. Section 3 presents the empirical specifications and results. Section 4 concludes.

2 Data

2.1 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., Hersh and Nall, 2016; Nickerson and Rogers, 2014). 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.[3] Next to race, the Catalist 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). Appendix Table A5 shows that race-specific impact estimates remain very close to those of Table 3 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, Appendix Table A2 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 effect 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

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

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, Tables 1 and A6 show that our results hold when we restrict attention to registered voters, all of whom should be voting-eligible individuals.

Further details on the Catalist panel data are given in Appendix A.1.

2.2 Data on Mobilization and Campaign Contributions

Measures of campaign contact and voter engagement come from the 2008—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.[4] We also construct a summary index of voter activity, defined to be the equally weighted average of the z-scores of its components.

Information on state-level campaign contributions is from Bonica (2015)'s Database on Ideology, Money in Politics, and Elections (DIME), version 2.2. The data contain all political contributions recorded by the Federal Elections Commission, 2004–2014. 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.

2.3 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.[5] The second dataset, by the Heritage Foundation, includes 1,277 proven cases. Again, the Foundation's website indicates that this database is non-exhaustive.[6]

We define two outcomes separately in either dataset: the number of fraud cases documented

[4]For all survey data we use, exact questions are detailed in Appendix A.2.

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

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

in each state-year per 100,000 residents, and the number of cases potentially preventable by strict identification requirements.[7] 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.

2.4 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, 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.

2.5 Calendars of Voter ID Laws, Election Laws, and State Party Control

We use the National Conference of State Legislatures (NCSL) to identify the type of ID law enforced in each state-year. Following recent literature (e.g., Hajnal et al., 2017), our main treatment is the presence of strict ID laws. Appendix Tables A14–A18 show that all results are substantively identical using strict-photo ID laws as treatment.

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.[8] Similarly,

[7] 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.

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

the party affiliation of the governor can take three possible values, Democratic, Republican, and independent.[9]

3 Results

3.1 Impact on turnout

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

$$Y_{ist} = \beta ID_{st} + X'_{ist}\gamma + \alpha_s + \delta_t + \mu_{ist}, \qquad (1)$$

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.[10]

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 1. 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

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

[10] Appendix Tables A19–A23 show that the state-clustered asymptotic p-values of Tables 1–5's coefficients are very close to their wild cluster bootstrap counterparts (Esarey and Menger, 2017).

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 and hence estimates the impact using 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 between treated and control states).[11] 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 2.1. 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 aggregate 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

[11] Due to the large sample size, the number of included covariates, and the architecture of Stata's fixed-effects routines, it is computationally very costly to estimate state-clustered standard errors in voter fixed-effects specifications. Thus, standard errors for these specifications come from bivariate regressions of residualized outcomes on residualized treatments with state-clustered standard errors. They do not account for the degrees of freedom lost by partialling out the covariates and voter fixed effects, and are therefore underestimated. This works *against* finding the null result which we obtain under this and other specifications.

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 2.0 and 2.7 percentage points, respectively (columns 4 and 8). The precision of our estimates is comparable across specifications

[Table 1 about here]

In Appendix Table A3, 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.[12] 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 aggregate turnout by more than 1.3 percentage points.

Table 2, 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). 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. We use McDonald's data for 2004–2018, since 2004 is the last year before Arizona and Ohio became the first states in the country to implement a strict ID law.[13] Also this strategy confirms the null result.

[Table 2 about here]

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

$$Y_{ist} = \sum_{\tau} \beta_\tau ID_{st}^{\tau} + X_{ist}^{'}\gamma + \alpha_s + \delta_t + \mu_{ist}, \tag{2}$$

where ID_{st}^{τ} 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

[12] As in the specification controlling for voter fixed effects, we partial out county-pair-by-year fixed effects and voter and state controls from the outcome and treatment, and run bivariate regressions of the residualized outcome on the residualized treatment. Again, this leads us to slightly underestimate the standard errors.

[13] As shown in Appendix Table A4, 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).

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 1 shows that turnout does not change differentially in treated states *after* the first implementation of the law, consistent with the estimates in Table 1. Corroborating our identification strategy, we also find no evidence of differential trends *before* implementation: though strict ID laws are not randomly assigned to states (Appendix Table A1 shows slightly lower turnout level in treated states), their implementation does not correlate with differential pre-trends in turnout.[14]

[Figure 1 about here]

3.2 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 interactions 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 3 reports the results for the main dimension of heterogeneity: race. We use the same specifications as in Table 1, 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

[14] Appendix Figure A1 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 1).

that strict ID laws decrease non-white turnout (relative to white turnout) by more than 0.3 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 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 .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 3 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 2.

[Figure 2 about here]

Estimates obtained when restricting attention to voters in adjacent counties across state borders yield the consistent conclusion that the strict ID laws did not decrease the participation of any race group (Table A3, columns 2–5). Appendix Tables A5 and A6 further 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 A6 uses the turnout of the registered voters as outcome, as in Table 1, Panel A). Finally, in Appendix Table A7, we 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 3.

Appendix Table A8 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.[15] This makes it unlikely that the laws changed electoral outcomes. We test this prediction in Table 2, 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 point estimates are positive but lower than 1 percentage point and not statistically significant. As shown in Appendix Table A9, the results remain close to null and non-ignificant when we consider congressional and presidential elections separately.

Finally, we test whether specific components of the laws or contextual factors are associated with larger effects. 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 (Appendix Figures A2 and A3 and Tables A14 through A18). Out of 30 coefficients shown in Appendix Tables A14 and A16, 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 3.1. 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. 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 (Appendix Table A10). If anything, the overall and race-specific event studies show more *positive* (although generally non-significant) effects on turnout in later elections (Figures 1 and 2).

3.3 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 (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

[15] Party affiliation is only available for one of the treated states. Corresponding estimates should thus be interpreted with caution.

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 4, 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 4, columns 3 and 4, and on the individual outcomes in Appendix Table A11. Finally, we measure effects on total campaign contributions by state and election year using official data from the Federal Election Commission collected by Bonica (2015) (Table 4, columns 5 and 6).

[Table 4 about here]

Panel A of Table 4 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 Appendix Table A11, 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 5.4 percentage points, which is significant at the 1 percent level (column 1). This effect is robust in significance and magnitude to the inclusion of 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 5.1 percentage points. This differential effect remains significant (at the 1 percent level) and of almost identical magnitude when using strict-photo ID laws as treatment (Appendix Table A17).

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 test 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 Appendix Table A12, Panel B, columns 1 and 2, the effect on campaign contact is particularly strong (6.0 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 the strict ID laws.[16]

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 within this range, as shown in Table 3, 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 0.34 percentage points (5.1 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.

3.4 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

[16]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). When adding these controls, the effect is no longer statistically significant (column 4).

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 2 compound this issue. With these caveats in mind, we report the effects on the extent of fraud in Table 5. 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 2.3. 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 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 Appendix Table A13) 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 5 about here]

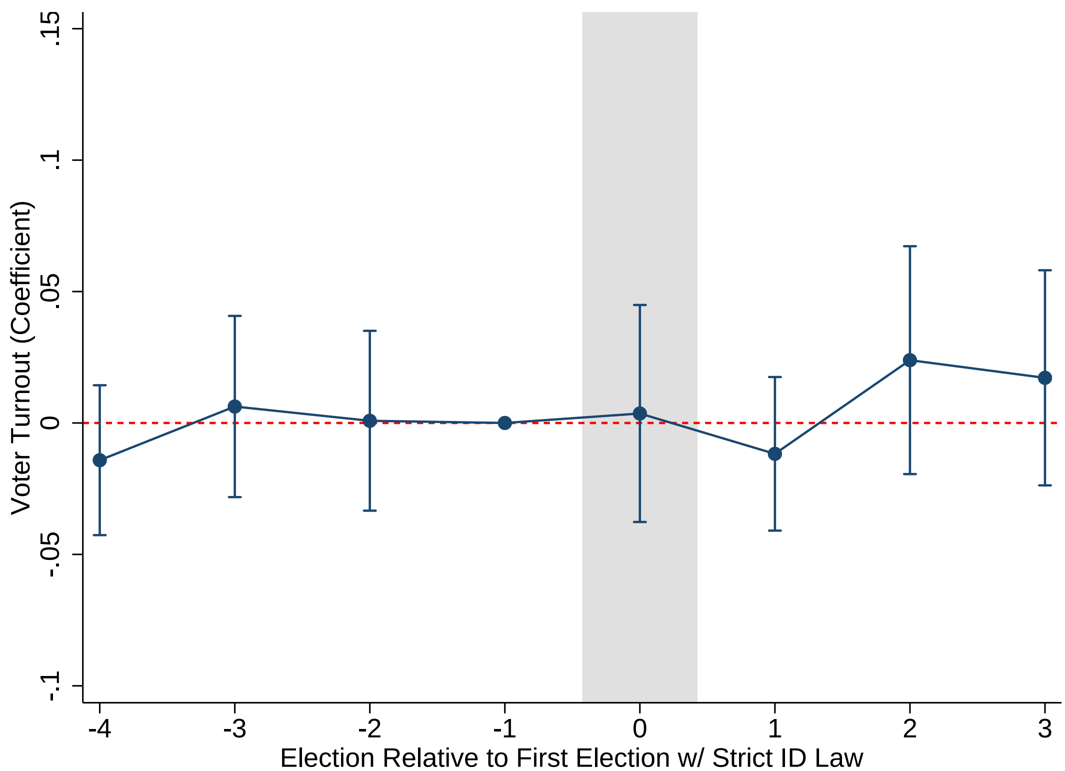
4 Conclusion

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 5.4 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.

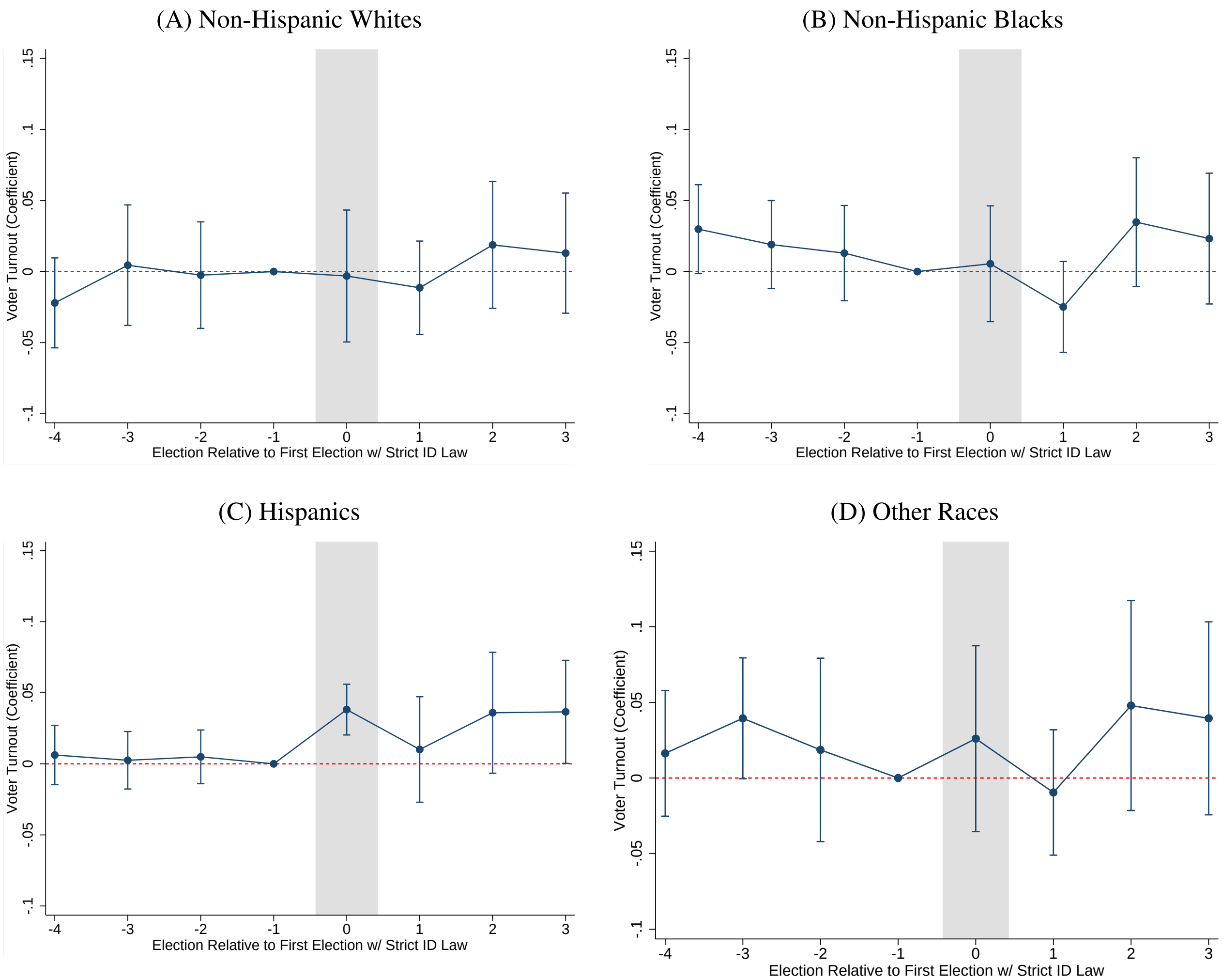
Because states adopted strict ID laws only 4 to 14 years ago, our results should be interpreted with caution: we find negative participation effects neither in the first election after the adoption of the laws nor in following ones, but cannot rule out that such effects will arise in the future. Enforcement 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 – 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).

Figure 1: Event-Study Graph of the Turnout Effect of Strict ID Laws



Notes: 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 $ID^{\tau=1}_{ND,2016} = ID^{\tau=1}_{TX,2016} = ID^{\tau=2}_{TX,2018} = 0$.

Figure 2: Event-Study Graphs of the Turnout Effect of Strict ID Laws by Race



Notes: 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 1: Turnout and Registration Effects of Strict ID Laws

	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)	(.015)				
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)	(.013)	(.012)	(.011)	(.007)	(.010)
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 1,100,864,771 and 1,604,600,687, 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. For computational reasons, voter FEs specifications rely on 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 run a simple bivariate regression of the residualized outcome on the residualized treatment. Standard errors clustered at the state level in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 2: 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	.0001	.001
	(.012)	(.013)	(.0118)	(.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)	.0004	.009	.006	-
	(.0189)	(.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 Michael McDonald's state turnout data, 2004-2018 (2004 is the last year before Arizona and Ohio became the first states in the country to implement a strict ID law). 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 in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 3: 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)		(.014)
1(Strict ID Law)×non-White	.340	.006	.006		.009
		(.014)	(.010)		(.010)
$\beta^{nonwhite} - \beta^{white}$		.013	.009	.007	.014
		(.008)	(.007)	(.007)	(.008)
		Panel B. By Detailed Race			
1(Strict ID Law)×White	.458	-.006	-.003		-.005
		(.015)	(.014)		(.014)
1(Strict ID Law)×Hispanic	.295	.025 *	.022 ***		.026 ***
		(.015)	(.008)		(.009)
1(Strict ID Law)×Black	.380	-.009	-.006		-.004
		(.014)	(.013)		(.012)
1(Strict ID Law)×Other Race	.330	.012	.007		.008
		(.028)	(.022)		(.021)
$\beta^{hispanic} - \beta^{white}$		.032 ***	.026 **	.026 ***	.030 **
		(.011)	(.011)	(.006)	(.012)
$\beta^{black} - \beta^{white}$		-.003	-.003	-.003	.001
		(.008)	(.006)	(.006)	(.006)
$\beta^{other} - \beta^{white}$		.019	.010	-.001	.013
		(.016)	(.010)	(.006)	(.010)
Race-by-Year FEs		✓	✓	✓	✓
Race-by-State FEs		✓	✓	✓	✓
State & Voter Controls			✓	✓	✓
State-by-Year FEs				✓	
Voter FEs					✓

Notes: The sample (N = 1,604,600,687) consists of both registered and unregistered voters. See notes to Table 1 for details on the controls. Column 1 reports mean turnout in the interacting category. Standard errors clustered at the state level in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 4: 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($1k/100k residents)	
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A. Average Effect			
1(Strict ID Law)	.015	.012	-.002	-.009	.039	.057
	(.019)	(.018)	(.015)	(.014)	(.123)	(.114)
Year & State FEs	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓		✓		✓
			Panel B. Whites vs. Non-Whites			
1(Strict ID Law)×White	.003	.0001	-.005	-.012		
	(.020)	(.0186)	(.016)	(.015)		
1(Strict ID Law)×non-White	.054 ***	.051 ***	.004	.0004		
	(.019)	(.016)	(.014)	(.0139)		
$\beta^{nonwhite}$ - β^{white}	.051 ***	.051 ***	.008	.013		
	(.018)	(.017)	(.011)	(.009)		
Race-by-Year FEs	✓	✓	✓	✓	✓	✓
Race-by-State FEs	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓		✓		✓
Outcome Mean	.640	.640	.000	.000	14.548	14.548
N	221,926	221,926	308,704	308,704	306	306

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, 2004-2014. For a description of state controls, see the notes to Table 1. 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 in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 5: 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	.015	.004	.001	-.003	.005	.003
	(.106)	(.101)	(.044)	(.046)	(.009)	(.010)	(.009)	(.011)
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	.018
	(.030)	(.029)	(.017)	(.015)	(.023)	(.022)	(.024)	(.025)	(.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	11,397	11,397

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 Appendix Table A13. 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 in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

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A Appendix for Online Publication

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A.1 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.[17]

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,[18] registration status,[19] 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."[20] 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.

[17] 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.

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

[19] 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.

[20] 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.

Table A1: 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	.741	.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	-	.104	-	.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	1,163,102,994	240	441,497,693	66	1,604,600,687	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 US citizenship in the corresponding race-year-state. Estimated citizenship ratios come from "1-year" ACS data.

A.2 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.

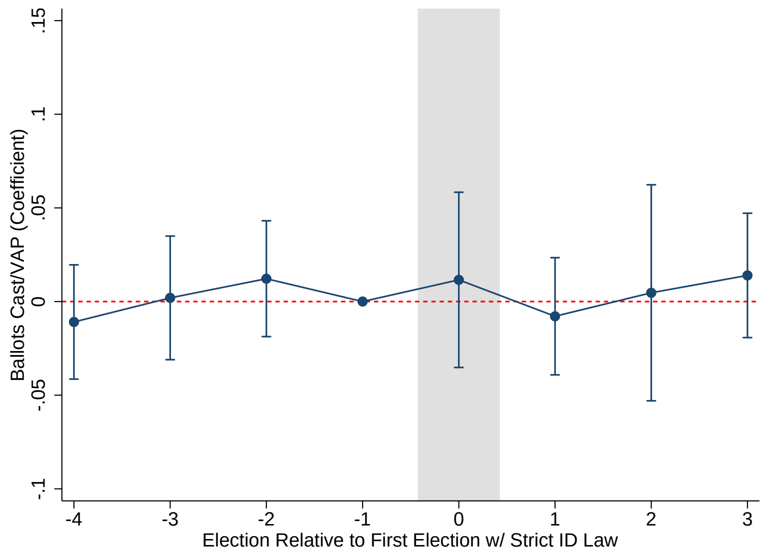
CCES dummy outcomes are based on the following years and survey questions (omitted years correspond to years in which the relevant survey question was not asked):

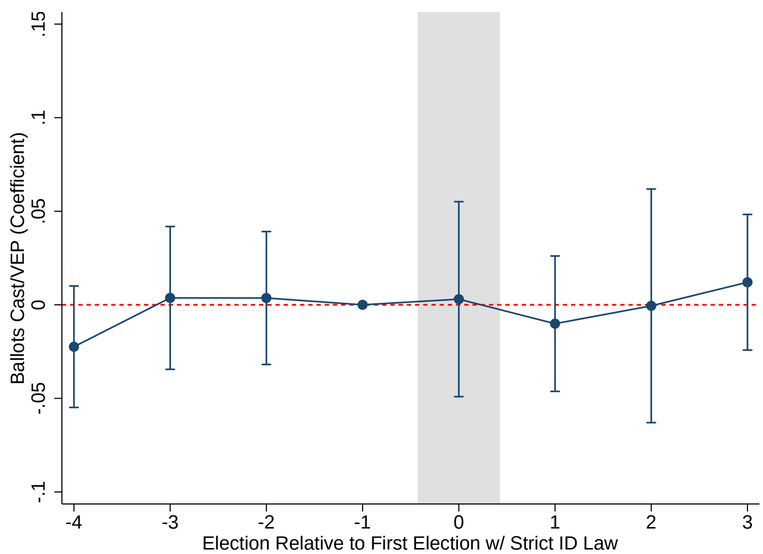
- Voter was contacted by a campaign: v4065 (2006), CC425a (2010), CC425a (2012), CC425a (2014), CC16_425a (2016).
- Donated to a candidate or campaign: v4062 (2006), CC415_6 (2008),CC417a_4 (2010), CC417a_4 (2012), CC417a_4 (2014), CC16_417a_4 (2016), CC18_417a_6 (2018).
- Amount donated (equal to 0 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).

A.3 Additional Results

Figure A1: Event-Study Graphs of the Turnout Effect of Strict ID Laws – Michael McDonald's State Turnout Data

(A) Ballots Cast/Voting-Age Population





(B) Ballots Cast/Voting-Eligible Population

Notes: Each panel plots event-study estimates and 95-percent confidence intervals from a separate regression (in the form of equation [2]) run on Michael 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 A2: Effects of Strict ID Laws on 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)	(.011)
Outcome Mean	.096	.096	.096	.096
	Panel B. Disappearing from the Sample			
1(Strict ID Law)	.004	-.001	.002	.002
	(.006)	(.005)	(.004)	(.008)
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 1,358,011,608 and 1,309,156,087, respectively.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table A3: Turnout Effect 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)	.017	.016	.006	.024	.051 ***
	(.015)	(.014)	(.015)	(.019)	(.013)
Year FEs	✓	✓	✓	✓	✓
State FEs	✓	✓	✓	✓	✓
State & Voter Controls	✓	✓	✓	✓	✓
County-Pair-by-Year FEs	✓	✓	✓	✓	✓

Notes: The table reports estimates from specifications run on registered and unregistered voters living in adjacent counties across state borders based on Dube et al. (2010)'s strategy. The sample size is: 1,225,049,077 (column 1), 934,530,439 (column 2), 152,990,940 (column 3), 87,570,867 (column 4), and 49,956,831 (column 5). Standard errors are two-way clustered by states and border segments.

*** p < 0.01, ** p < 0.05, * p < 0.10

Table A4: Turnout Effect of Strict ID Laws – Michael McDonald's State Turnout

	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	.0001	.001	.005	.005	.001	.006
	(.012)	(.013)	(.0118)	(.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 Michael McDonald's state turnout data. Panels A and B include, respectively, election years 2004-2018 (2004 is the last year before Arizona and Ohio became the first states in the country to implement a strict ID law) 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 in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table A5: 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)		(.013)
1(Strict ID Law)×non-White	.354	-.001	.002		.006
		(.011)	(.010)		(.010)
$\beta^{nonwhite} - \beta^{white}$		.007	.005	.008	.011
		(.011)	(.011)	(.011)	(.013)
		Panel B. By Detailed Race			
1(Strict ID Law)×White	.479	-.009	-.003		-.005
		(.013)	(.012)		(.013)
1(Strict ID Law)×Hispanic	.264	.020 **	.019 *		.027 ***
		(.009)	(.010)		(.010)
1(Strict ID Law)×Black	.412	-.018	-.011		-.009
		(.013)	(.012)		(.011)
1(Strict ID Law)×Other	.313	.029	.026 *		.023
		(.022)	(.014)		(.015)
$\beta^{hispanic} - \beta^{white}$		.028 **	.022	.030 *	.032 **
		(.012)	(.015)	(.016)	(.016)
$\beta^{black} - \beta^{white}$		-.009	-.008	-.006	-.004
		(.010)	(.009)	(.007)	(.009)
$\beta^{other} - \beta^{white}$		.038 **	.029 ***	.018	.028 **
		(.015)	(.010)	(.016)	(.011)
Race-by-Year FEs		✓	✓	✓	✓
Race-by-State FEs		✓	✓	✓	✓
State & Voter Controls			✓	✓	✓
State-by-Year FEs				✓	
Voter FEs					✓

Notes: The table replicates Table 3 restricting the sample to voters whose race is estimated by Catalist with high confidence. N = 1,049,126,053. Column 1 reports mean turnout in the interacting category. Standard errors clustered at the state level in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table A6: 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)		(.017)
1(Strict ID Law)×non-White	.517	.016	.015		.011
		(.014)	(.011)		(.014)
$\beta^{nonwhite}$ - β^{white}		.019	.021	.015	.024 *
		(.013)	(.013)	(.010)	(.013)
		Panel B. By Detailed Race			
1(Strict ID Law)×White	.654	-.002	-.006		-.014
		(.012)	(.012)		(.017)
1(Strict ID Law)×Hispanic	.478	.051 **	.050 ***		.044 ***
		(.022)	(.017)		(.017)
1(Strict ID Law)×Black	.542	-.006	-.007		-.010
		(.010)	(.010)		(.015)
1(Strict ID Law)×Other Race	.523	.019	.014		.008
		(.028)	(.025)		(.028)
$\beta^{hispanic}$ - β^{white}		.054 ***	.056 ***	.048 ***	.058 ***
		(.020)	(.019)	(.008)	(.020)
β^{black} - β^{white}		-.004	-.001	-.001	.004
		(.008)	(.007)	(.008)	(.009)
β^{other} - β^{white}		.021	.020	.006	.022
		(.018)	(.015)	(.009)	(.016)
Race-by-Year FEs		✓	✓	✓	✓
Race-by-State FEs		✓	✓	✓	✓
State & Voter Controls			✓	✓	✓
State-by-Year FEs				✓	
Voter FEs					✓

Notes: The table replicates Table 3 restricting the sample to registered voters. N = 1,100,864,771. Column 1 reports mean turnout in the interacting category. Standard errors clustered at the state level in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table A7: 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	.003		.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	-.0045		10.84	-.041	-.047	
	(.141)	(.017)	(.0175)		(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 **	.032 **	.021 **		.063	.059	.073 **
		(.016)	(.015)	(.010)		(.045)	(.039)	(.032)
$\beta^{black} - \beta^{white}$		.004	.004	.001		-.022	-.021	-.021
		(.015)	(.015)	(.017)		(.029)	(.028)	(.032)
$\beta^{other} - \beta^{white}$		-.015	-.017	-.030 *		.027	.016	-.009
		(.019)	(.018)	(.016)		(.074)	(.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 in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table A8: 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)	(.0125)
1(Strict ID Law)×Female	.437	-.008	-.003	-.009	-.002
		(.015)	(.013)	(.015)	(.013)
		Panel B. By Age			
1(Strict ID Law)×1(age < 35)	.348	-.001	.0002	-.007	.012
		(.017)	(.0169)	(.019)	(.015)
1(Strict ID Law)×1(35 <= age < 60)	.475	-.003	-.003	-.009	-.003
		(.016)	(.016)	(.018)	(.014)
1(Strict ID Law)×1(60 <= age)	.587	-.0003	-.001	-.006	-.003
		(.0137)	(.013)	(.014)	(.012)
		Panel C. By Party			
1(Strict ID Law)×Republican	.705	-.004	-.001	.018 **	.009
		(.011)	(.008)	(.009)	(.008)
1(Strict ID Law)×Democrat	.640	.021 *	.021 **	.039 **	.019 **
		(.012)	(.009)	(.009)	(.009)
1(Strict ID Law)×Other	.204	-.008	-.003	.015 *	.007
		(.009)	(.007)	(.008)	(.007)
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 in parentheses.
*** p < 0.01, ** p < 0.05, * p < 0.10

Table A9: 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)	.0002	.009	.007	.011
	(.0196)	(.017)	(.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 Arizona and Ohio became the first states in the country to implement a strict ID law. 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 in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table A10: 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)	(.016)
1(Strict Law)×Midterm	.358	-.012	-.006	-.012	-.005
		(.014)	(.011)	(.013)	(.011)
		Panel B. First Election vs. Following Ones			
1(Strict Law)×Following Elections	.414	-.007	.002	-.019	.002
		(.014)	(.011)	(.019)	(.010)
1(Strict Law)×First Election	.360	-.007	-.003	-.008	-.003
		(.015)	(.013)	(.014)	(.014)
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 in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table A11: 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)	.008	-.0002	4.6	5.1	-.005	-.008 *	-.013	-.017	.005	.002
	(.009)	(.0092)	(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	-.002	7.7	6.0	-.005	-.009 *	-.016	-.019	.003	-.0007
	(.010)	(.010)	(20.1)	(19.9)	(.005)	(.005)	(.016)	(.016)	(.009)	(.0082)
1(Strict ID Law)×non-White	.010	.006	1.4	2.2	-.007	-.007	-.007	-.007	.013 *	.012
	(.014)	(.013)	(56.3)	(54.5)	(.010)	(.009)	(.013)	(.013)	(.007)	(.007)
$\beta^{nonwhite} - \beta^{white}$	.005	.007	-6.3	-3.8	-.002	.002	.009	.012	.010 *	.012 **
	(.015)	(.013)	(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 4, columns 3 and 4. Standard errors clustered at the state level in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table A12: Turnout 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	.003	.0001	-.005	-.012
	(.020)	(.0186)	(.016)	(.015)
1(Strict ID Law)×non-White	.054 ***	.051 ***	.004	.000
	(.019)	(.016)	(.014)	(.014)
$\beta^{nonwhite} - \beta^{white}$	.051 ***	.051 ***	.008	.013
	(.018)	(.017)	(.011)	(.009)
	Panel B. By Detailed Race			
1(Strict ID Law)×White	.003	.0001	-.005	-.012
	(.020)	(.0186)	(.016)	(.015)
1(Strict ID Law)×Hispanic	.060 ***	.055 ***	-.017	-.028
	(.019)	(.016)	(.023)	(.024)
1(Strict ID Law)×Black	.039	.034	.031 **	.027
	(.025)	(.025)	(.015)	(.017)
1(Strict ID Law)×Other Race	.078 ***	.083 ***	-.030	-.026
	(.026)	(.025)	(.032)	(.027)
$\beta^{hispanic} - \beta^{white}$	.057 **	.054 **	-.012	-.016
	(.025)	(.024)	(.023)	(.022)
$\beta^{black} - \beta^{white}$	.036	.034	.035 **	.039 **
	(.022)	(.021)	(.015)	(.016)
$\beta^{other} - \beta^{white}$	.075 ***	.083 ***	-.026	-.013
	(.026)	(.028)	(.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 in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

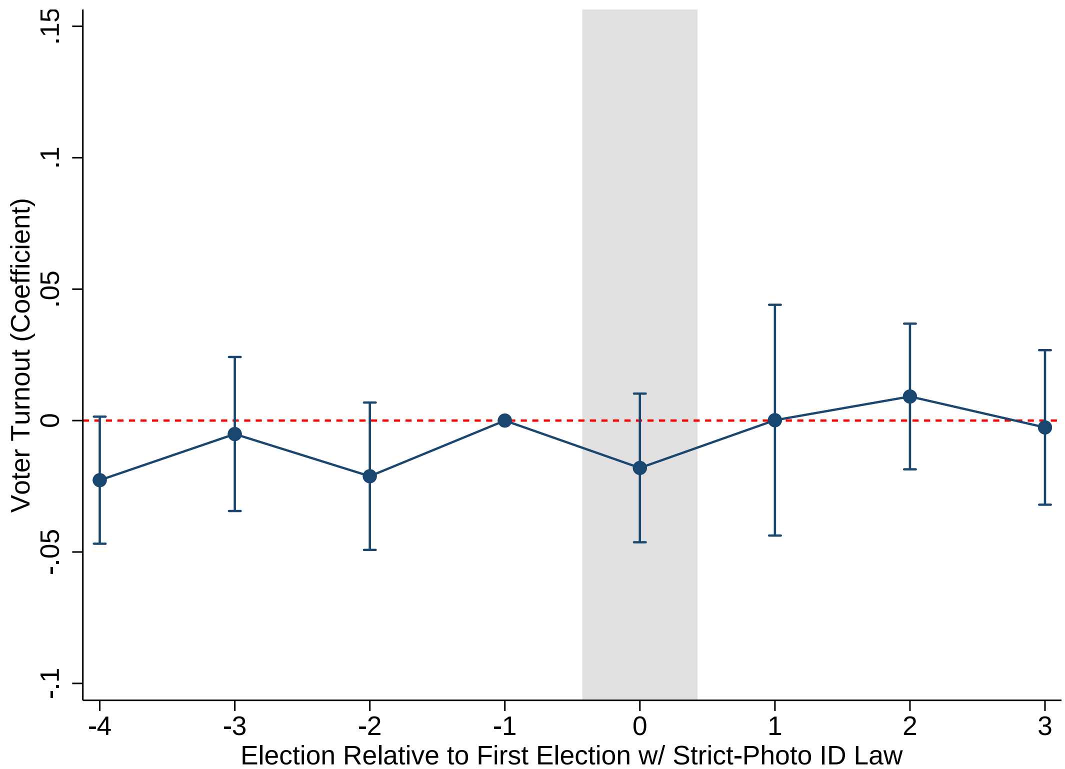
Table A13: Effects of Strict ID Laws on Non-Preventable Frauds

	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	.011	.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 Table 5's summary index and not already reported as outcomes in that table. Standard errors clustered at the state level in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

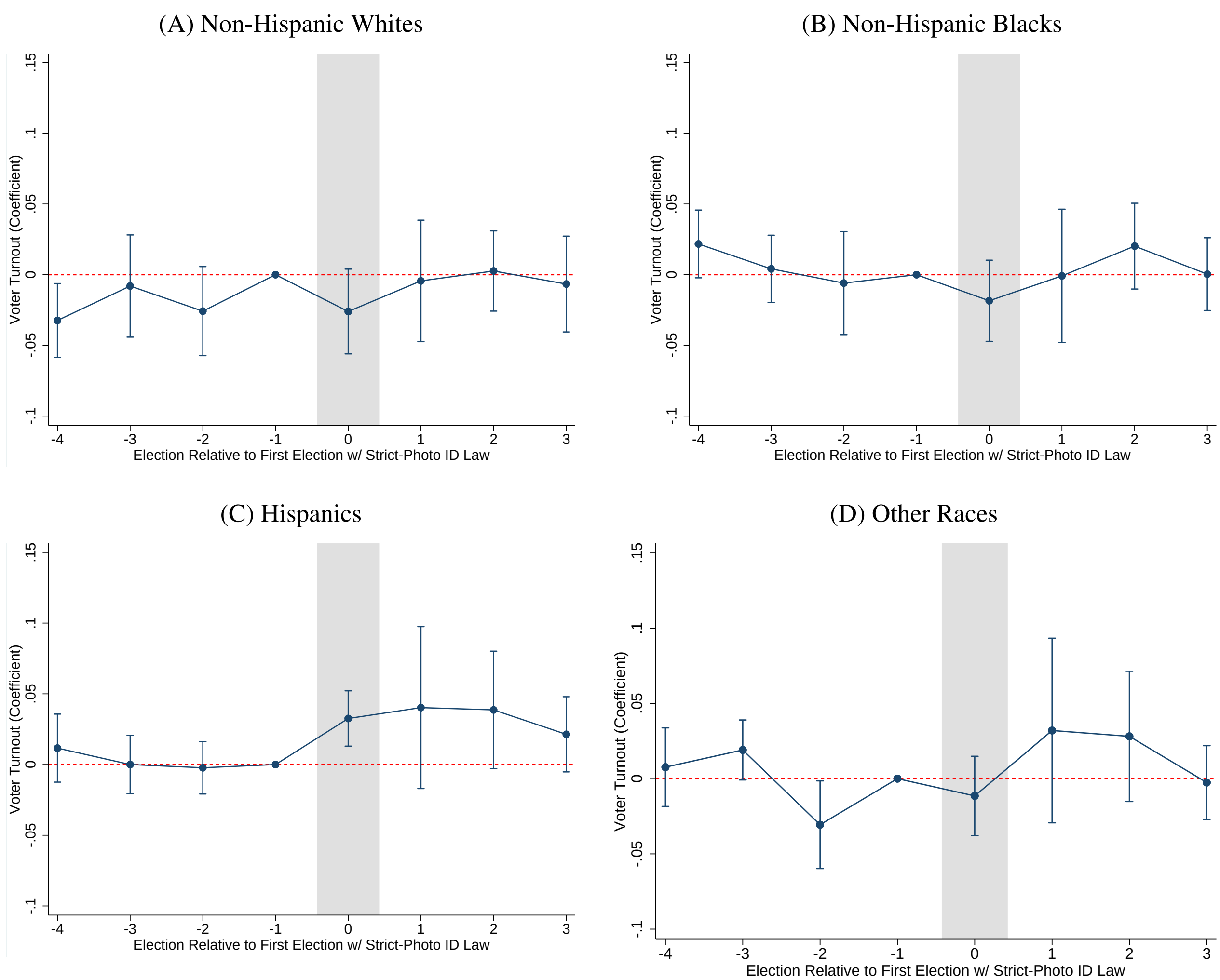
A.4 Effects of Strict-Photo ID Laws

Figure A2: Event-Study Graph of the Turnout Effect of Strict-Photo ID Laws



Notes: The figure replicates Figure 1 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 A3: Event-Study Graphs of the Turnout Effect of Strict-Photo ID Laws by Race



Notes: The figure replicates Figure 2 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 A14: 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)	(.013)				
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)	(.011)	(.012)	(.011)	(.006)	(.009)
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 1 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 in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table A15: 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)	-.0003	-.002	-.003	-.011
	(.0120)	(.013)	(.012)	(.013)
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)	.00003	.009	.002	-
	(.02020)	(.018)	(.013)	-
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 2 using strict-photo (instead of strict) ID laws as treatment. Similarly to Table A14, all regressions control for a dummy identifying state-years with strict, non-photo ID laws. Standard errors clustered at the state level in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table A16: 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	-.006		-.008
		(.014)	(.012)		(.012)
1(Strict-Photo ID Law)×non-White	.340	.004	.004		.006
		(.013)	(.009)		(.009)
$\beta^{nonwhite} - \beta^{white}$		.014 *	.010	.007	.015
		(.008)	(.008)	(.007)	(.015)
		Panel B. By Detailed Race			
1(Strict-Photo ID Law)×White	.458	-.010	-.006		-.008
		(.014)	(.012)		(.012)
1(Strict-Photo ID Law)×Hispanic	.295	.024 *	.022 ***		.025 **
		(.014)	(.008)		(.009)
1(Strict-Photo ID Law)×Black	.380	-.012	-.009		-.007
		(.012)	(.011)		(.010)
1(Strict-Photo ID Law)×Other Race	.330	.008	.003		.003
		(.026)	(.019)		(.018)
$\beta^{hispanic} - \beta^{white}$		.034 ***	.028 ***	.026 ***	.033 **
		(.011)	(.010)	(.006)	(.012)
$\beta^{black} - \beta^{white}$		-.002	-.002	-.003	.001
		(.008)	(.006)	(.006)	(.006)
$\beta^{other} - \beta^{white}$		.018	.009	-.002	.011
		(.015)	(.009)	(.006)	(.009)
Race-by-Year FEs		✓	✓	✓	✓
Race-by-State FEs		✓	✓	✓	✓
State & Voter Controls			✓	✓	✓
State-by-Year FEs				✓	
Voter FEs					✓

Notes: This table replicates Table 3 using strict-photo (instead of strict) ID laws as treatment. Column 1 reports mean turnout in the interacting category. Similarly to Table A14, 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 in parentheses.
*** p < 0.01, ** p < 0.05, * p < 0.10

Table A17: 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)	.009	.005	-.005	-.012	.027	.049
	(.018)	(.017)	(.015)	(.015)	(.146)	(.132)
Year & State FEs	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓		✓		✓
			Panel B. Whites vs. Non-Whites			
1(Strict-Photo ID Law)×White	-.004	-.007	-.008	-.015		
	(.019)	(.018)	(.016)	(.016)		
1(Strict-Photo ID Law)×non-White	.048 ***	.044 ***	.002	-.002		
	(.018)	(.015)	(.014)	(.014)		
$\beta^{nonwhite} - \beta^{white}$	.051 ***	.051 ***	.009	.013		
	(.019)	(.017)	(.011)	(.010)		
Race-by-Year FEs	✓	✓	✓	✓	✓	✓
Race-by-State FEs	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓		✓		✓
Outcome Mean	.640	.640	.000	.000	14.548	14.548
N	221,926	221,926	308,704	308,704	306	306

Notes: This table replicates Table 4 using strict-photo (instead of strict) ID laws as treatment. Similarly to Table A14, all regressions control for a dummy identifying state-years with strict, non-photo ID laws, along with its interactions with detailed race categories (in Panel B). Standard errors clustered at the state level in parentheses.

*** p < 0.01, ** p < 0.05, * p < 0.10

Table A18: 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)	.067	.046	.025	.013	-.005	-.009	.001	-.003		
	(.166)	(.157)	(.068)	(.071)	(.010)	(.011)	(.011)	(.012)		
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	-.010	-.026	-.030	.018	.029
	(.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	11,397	11,397

Notes: This table replicates Table 5 using strict-photo (instead of strict) ID laws as treatment. As in Table A14, 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 in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

A.5 Wild Bootstrap P-Values

Table A19: 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]	[.929]	[.580]	[.610]				
	{.944}	{.930}	{.642}	{.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]	[.942]	[.565]	[.921]	[.215]	[.693]	[.248]	[.931]
	{.675}	{.937}	{.649}	{.931}	{.282}	{.703}	{.495}	{.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 1. 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 (2018) 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 Stata *boottest* routine (Roodman et al., 2018).

Table A20: Effects of Strict-Photo 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	.0001	.001
	[.587]	[.643]	[.993]	[.955]
	{.583}	{.655}	{.988}	{.963}
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)	.0004	.009	.006	-
	[.982]	[.594]	[.532]	-
	{.979}	{.602}	{.558}	-
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 2. 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.

Table A21: 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
		[.665]	[.808]		[.737]
		{.711}	{.838}		{.784}
1(Strict ID Law)×non-White	.340	.006	.006		.009
		[.654]	[.555]		[.391]
		{.665}	{.569}		{.400}
$\beta^{nonwhite} - \beta^{white}$		.013	.009	.007	.014
		[.109]	[.203]	[.355]	[.104]
		{.093}	{.221}	{.406}	{.125}
		Panel B. By Detailed Race			
1(Strict ID Law)×White	.458	-.006	-.003		-.005
		[.665]	[.809]		[.737]
		{.711}	{.837}		{.784}
1(Strict ID Law)×Hispanic	.295	.025	.022		.026
		[.091]	[.006]		[.006]
		{.056}	{.010}		{.009}
1(Strict ID Law)×Black	.380	-.009	-.006		-.004
		[.522]	[.639]		[.770]
		{.543}	{.641}		{.802}
1(Strict ID Law)×Other	.330	.012	.007		.008
		[.654]	[.750]		[.713]
		{.786}	{.869}		{.841}
$\beta^{hispanic} - \beta^{white}$		.032	.026	.026	.030
		[.007]	[.021]	[.000]	[.016]
		{.010}	{.060}	{.023}	{.075}
$\beta^{black} - \beta^{white}$		-.003	-.003	-.003	.001
		[.742]	[.679]	[.612]	[.853]
		{.766}	{.693}	{.651}	{.864}
$\beta^{other} - \beta^{white}$		.019	.010	-.001	.013
		[.238]	[.317]	[.800]	[.209]
		{.351}	{.447}	{.836}	{.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 3. State-clustered asymptotic p-values are reported in brackets. Wild bootstrap state-clustered p-values are reported in braces. See notes to Table A19 for details on the bootstrap procedure. Column 1 reports mean turnout in the interacting category.

Table A22: 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	.012	-.002	-.009	.039	.057
	[.424]	[.499]	[.893]	[.516]	[.751]	[.617]
	{.457}	{.554}	{.946}	{.763}	{.761}	{.596}
Year & State FEs	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓		✓		✓
			Panel B. Whites vs. Non-Whites			
1(Strict ID Law)×White	.003	.0001	-.005	-.012		
	[.867]	[.9959]	[.776]	[.419]		
	{.880}	{.9920}	{.885}	{.652}		
1(Strict ID Law)×non-White	.054	.051	.004	.0004		
	[.006]	[.003]	[.787]	[.9783]		
	{.078}	{.066}	{.821}	{.9880}		
$\beta^{nonwhite} - \beta^{white}$	.051	.051	.008	.013		
	[.008]	[.004]	[.432]	[.189]		
	{.017}	{.012}	{.430}	{.225}		
Race-by-Year FEs	✓	✓	✓	✓	✓	✓
Race-by-State FEs	✓	✓	✓	✓	✓	✓
State & Voter Controls		✓		✓		✓
Outcome Mean	.640	.640	.000	.000	14.548	14.548
N	221,926	221,926	308,704	308,704	306	306

Notes: This table reports the same point estimates as Table 4. 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.

Table A23: 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	.015	.004	.001	-.003	.005	.003
	[.673]	[.803]	[.737]	[.938]	[.943]	[.801]	[.577]	[.802]
	{.589}	{.709}	{.626}	{.907}	{.944}	{.826}	{.587}	{.809}
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	.018
	[.917]	[.821]	[.811]	[.882]	[.699]	[.551]	[.418]	[.344]	[.857]	[.637]
	{.926}	{.834}	{.830}	{.883}	{.732}	{.614}	{.473}	{.435}	{.888}	{.798}
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	11,397	11,397

Notes: This table reports the same point estimates as Table 5. 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.