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Francesco Maria Esposito, Diego Focanti, and Justine S. Hastings
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ABSTRACT

We study the effect of photo ID laws on voting using a difference-in-differences estimation approach around Rhode Island's implementation of a photo ID law. We employ anonymized administrative data to measure the law's impact by comparing voting behavior among those with drivers' licenses versus those without, before versus after the law. Turnout, registration, and voting conditional on registration fell for those without licenses after the law passed. We do not find evidence that people proactively obtained licenses in anticipation of the law, nor do we find that they substituted towards mail ballots which do not require a photo ID.

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1 Introduction

Voter ID laws require a person to show some form of identification, often a photo ID, to be able to vote at the polls in an election. Proponents view these laws as potential tools to prevent voter fraud, while opponents claim that such laws may disenfranchise voters. Eighteen states in the US require voters to show a valid photo ID at the polls. In Rhode Island, in July 2011, State Law 17-19-24.2 came into effect and established the requirement of showing a photo ID to cast a ballot at the polls starting on January 1st, 2014.

The impact of such laws is open to debate. US courts recently struck down voter ID requirements in North Carolina[1] and Texas[2], while they have upheld them in Virginia[3]. A recent literature has measured the impact of Voter ID laws and found mixed results. Many researchers have relied on aggregated data on voter turnout (Hopkins et al. 2017) or surveys with self-reported data on past voting behavior (Alvarez et al. 2008). More recently, Hood and Bullock (2012) use administrative data on registered voters linked to DMV records to measure the impact of Georgia's voter ID law on voting among registered voters. In general, these papers tend to find small negative effects of voter ID requirements on turnout and produce mixed results when looking at subgroups such as minority voters.

We use anonymized administrative records from the State of Rhode Island housed in a secure facility. Personally identifiable information has been removed from the data and replaced with anonymous identifiers that make it possible for approved researchers to analyze records associated with the same individual while preserving anonymity. We use these data to study the impact of the state's photo ID requirement on turnout (measured as a fraction of the voting age population), registration, and voting conditional on registration (total votes as a fraction of registered voters) using a difference-in-differences approach. We measure how voting changed after the law for those without licenses versus those with licenses. Individuals with and without licenses, as well as those who are not registered to vote, exist in our data. This allows us to measure the impact of the law on turnout and registration, in addition to voting conditional on registration. Rich demographic information allows us to examine heterogeneous effects by key socioeconomic factors.[4]

We compare elections before to elections after the law came into effect. We measure the impact of the law on turnout in midterm elections using the 2010 and 2014 elections, and we measure the impact in

[1]North Carolina State Conference of the NAACP et al. v McCrory et al., US Court of Appeals for the 4th Circuit No. 16-1468 (2016).

[2]Veasey at al. v Abbot et al., US District Court, Southern District of Texas, Corpus Christi Division. Case 2:13-cv-00193 (2017).

[3]Lee, Brescia and Democratic PArty of Virginia v Virginia State Board of Elections et al. US Court of Appeals for the 4th Circuit No. 16-1605 (2016).

[4]Note, we do not observe other valid forms of ID for voting, in particular State IDs. This omission should understate our measured impacts of the law as some people with valid state ID's will be counted in the "treated group" - i.e. those without a Photo ID .

national elections using the 2012 and 2016 elections.[5] We use either Double LASSO (Belloni et al. 2014) or propensity score reweighting (Barsky et al. 2002; Imbens 2004) to flexibly control for observable factors correlated with driver's license status and the outcome variables of interest.

We find a significant decline in turnout, registration, and voting conditional on registration (for more vulnerable groups of voters) in presidential elections after the law was implemented, and demonstrate that these results are robust across specifications. We obtain smaller and mixed results in midterm elections, with no significant impact on turnout in our preferred specification. In presidential elections, our difference-in-differences estimates suggest that the law led to a decline in overall registration of 7.6 percentage points, no decline in voting conditional on registration, and a decline in turnout of 2.7 percentage points. These estimates imply that overall votes declined by 0.42 percentage points as a result of the law[6].

We address several potential threats to identification. First, the photo ID law passed in 2011, allowing people two and a half years to proactively obtain a license in anticipation of the law. If those without licenses who vote regularly were more likely to obtain a license in response to the law, we may find that voting decreased after the law for those without licenses because likely voters responded to the law by obtaining licenses, rather than because the law decreased the likelihood that an individual without a license would register or vote. We show that those without licenses in the pre-period were no more likely to get a license before the law came into effect if they had a history of voting versus did not. We use data on whether someone had a license in July 2011 (when the law was passed) as an instrument for whether they had a license at the time of each election, and demonstrate that our results are robust. Selection bias due to pro-active license acquisition in anticipation of the law is not driving our results.

Second, because we are comparing events that occur four years apart, the composition of eligible voters and their characteristics may have changed over time in ways that are correlated with both their driver's license status and voting behavior. To control for unobserved differences in individuals, we estimate an individual fixed effects specification. This within-estimator is only identified using individuals who remained in the sample for the entire period and whose license status has changed. We find similar, or even larger, effects in this specification.

Third, because we only observe one post-law election for midterm elections and presidential elections, we may be concerned that there are coincident changes in other factors which influence voting and differentially impact those with or without licenses. To address this concern, we conduct a series of placebo tests. We run our difference-in-differences model, but use as dependent variables factors which should not have been

[5] We add data from the 2006 and 2008 elections to the analysis as a robustness check that supports our main results.

[6] Approximately 464,000 people voted in the 2016 election in Rhode Island according to the State of Rhode Island Board of Elections. Available at https://www.ri.gov/election/results/2016/general_election/

affected by the photo ID law such as enrollment in social safety-net programs or having a child. If there are no differential, potentially confounding underlying trends in our data, we would expect to find zero impact of the law on these placebo outcomes. We estimate statistically significant but economically small effects.

To test whether these small changes in other observables drive our results, we measure the potential bias from differential demographic trends by performing a bounding exercise. We replace each individual's actual voting behavior with the behavior that is predicted by their observables. We estimate our difference-in-differences model with this predicted voting behavior as the dependent variable. Any change in this outcome attributable to the law would mechanically come from only differential changes in demographics rather than in voting behavior. We find near-zero or positive coefficients on the impact of the law, implying that our results are not driven by differential trends in demographics or social program enrollment changes around the time of the law.

Next, we use the Current Population Survey data and voter registration files from Allegheny County (PA) to test whether confounding events associated with the 2016 election may influence our results. Using the CPS we estimate our model separately for Rhode Island and the pool of Connecticut and Massachusetts, two neighboring states that did not enact a similar reform in our period of analysis. We do not find evidence of spurious effects in the neighboring states. We also re-estimate our voter-level model from Rhode Island using Allegheny County data. We similarly find no evidence of spurious effects.

We conduct additional analyses to extend our main results. First, we estimate the model by socio-economic status, minority status, age group, gender, and party affiliation. For presidential elections, we find the largest decreases in turnout (when measured as a percentage of their baseline turnout) among young and low SES voters. For midterm elections, we find mixed results with insignificant effects on turnout for most subgroups.

Second, we examine differential impacts of the law by precinct, and their correlation with precinct political characteristics. We estimate our main specification separately for the state legislative districts whose representatives and senators voted for or against the photo ID law. We observe larger effects in districts where both representatives and senators voted against the law. These happen to be districts with a larger fraction of people without a driver's license and a lower baseline turnout. This suggests legislators forecast, and considered the potential effects on their constituencies when voting on the law.

Finally, we measure the extent to which voters responded proactively to the law by using a mail ballot. Our estimates would represent a lower bound on the law's impact if many voters without photo IDs responded to the law by casting votes by mail ballot instead of at the polls. We estimate our difference-in-differences model only on the subsample of people who voted, and we use voting by mail as the dependent variable.

We find no significant overall impact on the likelihood that a registered voter chooses to cast their ballot by mail. We do observe significant results for a few subgroups, but not all are significant in both IV and OLS specifications. We conclude that mail ballots did not appear to be an effective substitute for voting at the polls for those without photo IDs.

We add to a recent literature measuring the impact of voter identification laws on voting behavior. Few other studies have attempted to estimate effects of voter ID laws on turnout using a difference-in-differences framework.[7] Closest to ours is Hood and Bullock (2012) who used linked DMV and voting administrative data from the state of Georgia and also employed a difference-in-differences approach to measure the impact of the law in Georgia on voting among registered voters. They find that, after the photo ID law was enacted, registered voters without a driver's license became 6.5 percentage points less likely to vote compared to those with a license. This implies that turnout conditional on registration declined by 0.4 percentage points. A limitation of this paper is that it only observes registered voters, so they cannot measure how the photo ID requirement may impact the decision to register. Moreover, they can only observe a small set of covariates, which limits their ability to examine impacts on subgroups and validate the difference-in-differences approach using placebo tests. They also do not observe other aspects of voting behavior, such as mail and provisional ballots.

Alvarez et al (2008) uses the voter supplement of the Current Population Survey to estimate a difference-in-differences model comparing states that passed voter ID laws with states that did not. Their results are mixed and they only find effects of up to two percentage points in turnout in the states that enacted the strictest photo ID laws. However, CPS data is susceptible to overreporting of turnout (Hur and Achen, 2013) which could lead to an underestimation of the effects.

Hopkins et al (2017) use precinct-level data from Virginia to estimate the relationship between precinct-level demographics and the prevalence of provisional ballots due to lack of photo ID. They find that this is higher in precincts with a larger fraction of voters without a driver's license. They also find that turnout is actually higher in these precincts, which they attribute to a targeted Department of Elections mailing campaign. However, the aggregate nature of the data and the lack of information on ID status does not allow the authors to identify changes in turnout for the population that is affected by the law.

[7] Other studies use smaller scale surveys to ask whether people from different socioeconomic or ethnic groups are more or less likely to have a valid ID to vote. Examples of this are Barreto et al's (2009) phone survey in Indiana and Stewart's (2017) nationwide survey. These studies show that african-americans and hispanics are significantly less likely to possess a valid ID. Similar studies approach the question of whether ID requirements are enforced differentially at the polls using similar surveys. Atkeson et al (2010) use a letter survey in New Mexico and show that hispanic and male voters are more likely to be asked for an ID at the polls. Cobb et al (2012) did an exit poll in the 2008 election in Boston and showed that african-american and hispanic voters were also more likely to be asked for an ID at the polls.

2 Background

Voter ID laws have existed in some form in the US since 1950. The Commission on Federal Election Reform (also known as the Carter-Baker Commission) was created following the 2004 presidential election and, in 2005, issued a series of recommendations that included uniform requirements of photo identification at the polls. At the same time, some states independently established photo identification (ID) requirements, while many others did not. Georgia and Indiana became the first states to pass a strict[8] photo ID requirement for voting in 2005 (NCSL, 2017a), with several states following suit. Currently eighteen states require a photo ID at the polls (see table A.1 for details).

In July 2011, Rhode Island passed State Law 17-19-24.2 which required showing a photo ID at the polls starting in January 2014. When a person who claims to be an eligible voter is unable to show one of the accepted forms of ID, the poll worker is supposed to give them a provisional ballot.[9] These ballots are kept in a separate bag and attached to a form that the voter signs, in which the poll clerk must include the reason why the voter received it. In the 2016 election in Rhode Island, 735 voters received a provisional ballot due to lack of photo ID, which amounts to 0.17% of all ballots cast at the polls (see table A.2). These provisional ballots are submitted to each municipality's local board of canvassers (LBC) for review. The law mandates that the LBC must check the signature on the provisional ballot form against the voter's signature on their voter registration form. Only if their signatures match, the provisional ballot is opened and counted. In the 2016 election, 85.7% of the provisional ballots given due to lack of a photo ID were counted.

Rhode Island's photo ID law may be considered more lenient than similar laws in other states. For instance, while states like Georgia and Indiana only accept a state-issued form of ID, Rhode Island accepts a broader list of forms of photo identification that includes IDs issued by educational institutions in the US and identification cards issued by the state and federal governments. The Secretary of State also offers a free Voter ID Card that can be obtained by showing a proof of residence such as a utility bill or a bank statement. In addition, voters who receive a provisional ballot due to lack of ID in Rhode Island do not need to take any further steps to verify their identity and have their vote counted.[10] This is different in states like Georgia, Indiana or Virginia, where the voter is required to return to an election office with proof of identity at a later date, or their vote is not counted (NCSL, 2017b).

[8] A "strict" photo ID requirement is defined by NCSL as one were the voter who receives a provisional ballot due to the lack of a valid ID must take further steps to have their provisional ballot counted, such as providing their board of elections with a proof of identitiy at a later date.

[9] It is possible that not all poll workers know of or abide by this rule. They may instead turn voters without an ID away without giving them a provisional ballot.

[10] The identity of these voters is verified directly by the Local Board of Canvassers, as described in the previous paragraph. A very small number of voters need to meet additional requirements under federal law to have their provisional ballot counted.

Another relevant aspect of the elections system are mail ballot rules. Because the identity of a voter who uses a mail ballot cannot be checked in person, alternative verification is required. The voter who wishes to vote by mail first fills out and signs a mail ballot application. This signature and the voter's registration information are verified against the corresponding registration form by the Local Board of Canvassers. If those signatures and the voter registration information match, the voter receives a mail ballot and an oath envelope. The voter must also sign this oath evelope where their mail ballot is placed, and this signature must be either notarized or witnessed by two people[11]. When the complete ballot is returned, the Board matches the signature in the oath envelope to the signature in the original application before opening and counting the ballot. This means mail ballots allow voters without a valid photo ID to have their identity verified and cast a vote. This is possible because voters do not need to provide an excuse to vote by mail in Rhode Island.[12] We will measure whether voters without a driver's license are more likely to substitute in favor of a mail ballot as a result of the photo ID law.

3 Data

To measure the impact of the photo ID law on turnout, we use Rhode Island state administrative records housed in a secure facility. Personally identifiable information has been removed from the data and replaced with anonymous identifiers that make it possible for researchers with approved access to join and analyze records associated with the same individual while preserving anonymity. The database contains administrative records from many major government programs, including employment records from payroll taxes, participation in social safety-net programs, indicators for whether the anonymized record is associated with a driver's license, records on interactions with the criminal justice system, as well as voter registration and indicators for past voting history. The data do not indicate if a person has a State Photo ID instead of a driver's license. Voting data is available since 2006, and most other datasets in the database go back further.

Thus, the database contains anonymized records on all individuals who work in Rhode Island, have a driver's license or registration in Rhode Island, are registered to vote or voted in Rhode Island, or have interacted with any social services program including giving birth at a hospital, receiving cash transfer programs, state-sponsored health insurance enrollment, or public education. Figure 1 plots the distribution of individuals by age in the database versus the 2010 US Census for Rhode Island.[13] The database contains

[11] Voters who are absent from the state due to military service or living overseas do not face this requirement.

[12] While nineteen other states require voters to present an excuse (such as working abroad or being physically impaired), most do not impose such restrictions. Sevreal years ago Rhode Island also required this, but in 2011 the state changed the mail ballot law to allow voters to simply state in their mail ballot form that they cannot be at the polls on the day of the election without attesting to a specific reason as to why. This amounts in practice to establishing "no excuse" mail voting.

[13] Figure A.1 replicates the same plot but adding a line for the distribution by age of only active people in the database.

approximately 18% more people than the Census does. The difference is largest for individuals in their 20's and 30's. A main reason for this difference is that, while our database dentifies all individuals who interact with government services, it does not have information on when people leave Rhode Island.

For example, if an individual registers to vote in Rhode Island, the Rhode Island database would record them as living in Rhode Island, but if they moved, they would not be removed from the voter registration records and therefore from our database until they are found to have moved out of state in a National Change of Address update that is done in odd-numbered years, or if they are sent official election mail that is returned as undeliverable and they do not vote in one of the subsequent two general elections, or if the state of Rhode Island is notified that the voter registered in another state. The Rhode Island database and the Census count numbers are the most different for young adults, the age group with the most geographic mobility, and closest for older adults who are typically less geographically mobile.

We use social program enrollments to construct individual-level measures of low SES. We consider an individual to be low SES if they have been enrolled in either SNAP, TANF, Medicaid, Supplemental Social Security Income, Childcare Assistance Program, or General Public Assistance at any point since January 2004. We also use these data to construct placebo tests of whether differential changes in social program enrollment or demographics may impact our estimates. Our identification strategy requires that there are no differential changes in factors that could affect voting behavior between those with driver's licenses and those without which are coincident with the photo ID law.

While the Rhode Island database population count is higher than the census', we will obtain unbiased estimates of the law's impact on voting as long as this difference is not a function of factors which change differentially between those with and without driver's licenses around the time of the photo ID law. In Section 5.1, we test for such differential changes in key measures of social program enrollment and demographic characteristics. We find that all changes are small in magnitude and we demonstrate that they do not drive our main results.

Finally, the Rhode Island data on voting includes several features not available in prior research. In particular, we are able to measure not only dates of registration and voting but also who requested a mail ballot form, to what address it was sent, what reason the voter gave for requesting it, and whether they submitted the mail ballot. We also observe who received a provisional ballot, for what reason, and whether each provisional ballot was accepted or not by the local board of canvassers.

3.1 Descriptive Statistics

Table 1 shows registration and turnout rates in the different subpopulations of interest, as well as the percentage of people in each group with a valid driver's license as of November 2012. Turnout is measured as votes divided by the total population of voting age. Overall, 67% of people of voting age in our data are registered to vote and 45% voted in the 2012 presidential election. These numbers are a slight underestimation of the real values because, while we overestimate the population of voting age (as shown in Figure 1), we are not overestimating either the number of people in the voter registry or the number of people who voted. Both registration and turnout rates increase with age and socioeconomic status, and are higher for whites relative to minorities (black or hispanic).

We also observe that 77% of voting-age individuals have a driver's license, which means that the no-license group is slightly over a fifth of the voting age population. Democrats, minorities and elderly voters (over the age of 65) have the lowest proportions of people with a driver's license, while males and whites have the highest.

4 Empirical Model

Our main goal is to estimate the effect of the photo ID law on turnout, which can be decomposed into the effect on registration, and the effect on voting conditional on registration. [14]

We use a difference-in-differences research design to estimate the impact of the law on turnout (measured as a fraction of the total voting age population in the Rhode Island database), registration, and voting conditional on registration (that is, total votes as a fraction of all registered voters). We assume that voters who have a valid driver's license at the time of an election are not affected by the requirement of showing a photo ID at the polls (and therefore are a suitable "control group" conditional on observable characteristics). We examine how our three measures of voting behavior change for those who lack a valid driver's license, relative to those who have one, before versus after the law. While this is not the only type of ID a voter can show at the polls, it is the most prevalent one. We do not observe State ID cards.[15] Thus, our results may be

[14]Intuitively,

$$Pr(Voted) = Pr(Registered) \cdot Pr(Voted|Registered)$$

Taking a partial derivative with respect to the photo ID law, we obtain the following:

$$\frac{\partial Pr(Voted)}{\partial(IDLaw)} = \frac{\partial Pr(Reg.)}{\partial(IDLaw)} \cdot Pr(Voted|Reg.) + Pr(Reg.) \cdot \frac{\partial Pr(Voted|Reg.)}{\partial(IDLaw)}$$

[15]A literature review from the Government Accountability Office (GAO, 2014) places estimates of the percentage of either eligible or registered voters who have any valid photo ID for voting in the 80%-95% range. At the same time, a report from the US

biased towards zero since we count people with valid State ID's among those without any photo ID to show at the polls.

The main equation we estimate is the following:

$$Y_{it} = \alpha_0 + \alpha_1 \cdot Post_t + \alpha_2 \cdot NoLicense_{it} + \alpha_3 \cdot Post_t \cdot NoLicense_{it} + X'_{it}\beta + \epsilon_{it} \tag{1}$$

where Y_{it} is either turnout, registration to vote, or voting conditional on registration, and X_{it} is a vector of controls selected by Double LASSO (Belloni et al 2014) from a set that includes a third degree polynomial in age, dummies on sex, race, incarceration, and participation in social programs (e.g. SNAP, TANF, Medicaid, SSI, CCAP and GPA), and all possible interaction terms.[16]

The coefficient of interest is α_3, and the identification assumption is that, conditional on X_{it}, there are no underlying trends that differentially affect the outcomes of those without licenses versus those with licenses. We estimate regressions separately for midterm and presidential elections since their average turnout is very different. We use the 2010 midterm and 2012 presidential elections as our pre-reform periods and the 2014 midterm and 2016 presidential elections as the post-reform periods. We test our results including data for 2006 and 2008 in the robustness checks section. However, due to how infrequent elections are, we are unable to estimate our results with more than one post-reform period for each type of election. As a robustness check to Double LASSO-selected controls, we use propensity score reweighting (Barsky et al. 2002; Imbens 2004). We compute weights in two different ways: by selecting the covariates that form the predicted propensity score using traditional research intuition, and by selecting the covariates using Double LASSO to form the propensity score. Our results are robust to all these alternative specifications.[17]

Because people may endogenously get a driver's license as a result of the photo ID requirement to vote, we include specifications in which we instrument for $NoLicense_{it}$ with not having a driver's license at the time the law was passed (July 2011) . To do this we estimate equation 1 instrumenting for $NoLicense_{it}$ with $NoLicense_{i,2011}$, and $Post_t \cdot NoLicense_{it}$ with $Post_t \cdot NoLicense_{i,2011}$. Hence, we estimate the

Federal Highway Administration (USDOT, 2017) shows that 85% of the driving age population in the US have a driver's license. Therefore, there does not appear to be a big difference between the fraction of adults who have a driver's license and the fraction of adults who have any kind of valid photo ID.

[16]Having a high-dimension of data and an unknown functional form poses problems for inference due to variable selection based on instinct alone, p-hacking, and overfitting. To overcome these challenges, new machine-learning algorithms have been developed to allow researchers to systematically select (based on predetermined penalty functions) the control variables to include which maximize predictive fit, thus avoiding p-hacking and overfitting. One such machine learning algorithm is the Least Absolute Shrinkage and Selection Operator (LASSO). LASSO is a regression method that selects variables by penalizing the size of their coefficients. LASSO is a systematic method to select variables that provide the strongest predictive fit by shrinking the coefficients on weak explanatory variable coefficients towards zero (Tibshirani 1996; Hastie et al. 2009; Belloni et al. 2014). See appendix B for a list of the controls selected by LASSO in our estimation.

[17]These results are shown in tables A.12 and A.13.

following two first-stage equations:

$$Post_t \cdot NoLicense_{it} = \gamma_0 + \gamma_1 NoLicense_{i,2011} + \gamma_2 \cdot Post_t \cdot NoLicense_{i,2011} + X_{it}^{'}\gamma + v_{it} \quad (2)$$

$$NoLicense_{it} = \delta_0 + \delta_1 \cdot NoLicense_{i,2011} + \delta_2 \cdot Post_t \cdot NoLicense_{i,2011} + X_{it}^{'}\delta + u_{it} \quad (3)$$

The validity of the instrument requires that an individual's probability of registering to vote or voting is only affected by their driver's license status in 2011 through the effect this has on their driver's license status at the time of each election.

Additionally, to check for changes in the composition of our population over time, we estimate equation 1 with individual-level fixed-effects. In this specification, the impact of the law is identified within voter, and only people who remain in the dataset for both the pre- and post-reform elections contribute to the result.

Finally, to look at heterogeneous effects of the law, we estimate equation 1 for the subpopulations of registered voters, focusing on SES, age, gender, minority status, and political party affiliation.

5 Results

We estimate equation 1 for the whole population using turnout, registration, and voting conditional on registration, as the outcome variable of interest, Y_{it}. We find significant impacts of the law on the first two outcomes but not on voting conditional on registration. The IV estimates in column 2 of Table 2 suggest that the law led to a decline in turnout of 2.7 percentage points, a decline in registration of 7.6 percentage points, and no decline in voting conditional on registration. These results suggest that photo ID requirements can have a significant effect on the decision of whether or not someone registers to vote. This implies that previous studies which only considered registered voters may have underestimated the effects of these requirements.

Table 2 also shows we do not find significantly negative impacts of the law on turnout in midterm elections. While we find a decline of 5.1 percentage points in registration, we estimate a relative increase of 4.1 percentage points in voting conditional on registration. The total impact on turnout is not statistically different from zero.

5.1 Robustness Checks

The results in column 1 may be biased if likely voters obtained licenses in anticipation of the law. Recall that our difference-in-differences estimator relies on the assumption that, around the time the photo ID law passed, there were no other factors which impacted the voting behavior of people without licenses versus those with licenses. One way this assumption may fail is if those without licenses who vote regularly were more likely to obtain a license in response to the law. The photo ID law passed in 2011, allowing people two and a half years to proactively obtain a license in anticipation of the law. If likely voters without photo ID's responded to the photo ID law by getting ID's at a greater rate than those without ID's who were unlikely voters, we may find that turnout decreased after the law for those without licenses because likely voters responded to the law by obtaining licenses, rather than because the law decreased the likelihood that an individual without a license would register or vote.

We use whether someone had a license in July 2011 as an instrument for whether they had a license at the time of each election. Column 2 of Table 2 shows these results. While there are small changes in the point estimates, the main results remain largely unchanged. The effect of 4.7 percentage points on voting in presidential elections (using the OLS) is reduced to 2.7 percentage points, while we do not find a significant effect on turnout in midterm elections.[18] In Table A.3, we show that, among the people who did not have a driver's license at the time of the 2012 presidential election, only 6.4% of those who voted and 8.9% of those who did not vote obtained a driver's license by the time of the 2016 presidential election.

While in the previous tests we considered the issue of endogenous selection in driver's license status post-law, we now control for unobservable differences in individuals by estimating an individual fixed effects specification. This within-estimator is only identified using individuals who remained in the sample for the entire period and whose license status has changed. Table A.4 shows the difference in the means of our observables between the full sample and the group we use in the exercise. While these differences are statistically significant, they are economically small, meaning we do not end up with a fundamentally different sample of voters. Results for the FE specification are in column 3 of Table 2. We find a slightly larger effect of 6.5 percentage points on turnout, which is explained by a larger effect on voting conditional on registration. From this, we conclude that our results cannot be explained by changes in the composition of the voting age population over time. We also find a negative effect of 1.7 percentage points on turnout in midterm elections when we used individual fixed effects.

Next, we conduct a series of placebo tests to examine if other socio-economic factors were changing differentially around the time of the election between the license and the no-license groups. We run our

[18]Tables A.5 and A.6 show the main coefficients of the first stage regressions and their F-statistics.

difference-in-differences model using as dependent variables factors which should not have been affected by the photo ID law such as enrollment in social safety-net programs, being incarcerated or having a child. If there are no differential, potentially confounding underlying trends in our data, we would expect to find zero impact of the law on these placebo outcomes. Table 3 shows these results. Columns 1 presents the relevant coefficients from table 2 and columns 2-6 report the placebo results. While we do find statistically significant coefficients, they are economically small. The placebo effects on TANF and UI enrollments are close to half a percentage point, and the effects on having a child, SNAP, and incarceration are even smaller.

Given the statistical significance of these results, we test whether these small changes in other observables drive our results with a bounding exercise. We replace each individual's actual voting behavior with the behavior that is predicted by their observables in a linear regression. We estimate our difference-in-differences model with this predicted voting behavior as the dependent variable. Any change in this outcome attributable to the law would mechanically come from only differential changes in demographics rather than in voting behavior, so our desired outcome would be to find coefficients statistically equal to zero. A negative coefficient would mean that underlying changes in observables are making us overestimate our estimated effects, while a positive coefficient might imply that we are under-estimating the effects of the law. Table 4 shows the results of this exercise, which we performed for the OLS, IV, and FE specifications.

We find near-zero or positive coefficients on the impact of the law, implying that our results are driven by changes in voting behavior resulting from the law, and not from differential trends in demographics coincident with the law. Put together, these tests confirm that the voting law did not change coincidentally with other factors which could impact voting differentially for those with or without licenses.

Our next test is related to the issue that we only include one election pre and post reform.[19] Unfortunately, given how infrequent elections are and that this reform is active since 2014, we are unable to incorporate data for more post-reform years. We did include data for the 2006 midterm and 2008 presidential elections and show these results in Tables A.7 and A.8. In these tables, we see that the estimated effect on turnout is similar to that on Table 2 with a larger effect on registration. [20]

Finally, because we only have data for one state and one post-reform election cycle, we are concerned that confounding events associated with the 2016 election may influence our results. The Rhode Island

[19] Also, in tables A.12 and A.13, we resport the results of estimating our model using propensity score rewieghting instead of Double LASSO. In table A.9, we test whether clustering standard errors would change our conclusions. Since "treatment" depends on driver's license status and its enforcement occurs at the precinct level (Abadie et al 2017), we would like to cluster at the level of precinct by driver's license status. However, since we can only assign precincts to registered voters, we test this clustering only in the equations where voting conditional on registration is the dependent variable. While we obtain larger standard errors, the change is small and our results remain statistically significant. Also, note that we only perform this exercise for presidential elections because the 2012 redistricting causes precincts to be different between the 2010 and 2014 midterm elections.

[20] Figures A.3-A.2 show how turnout, for different subgroups, evolved over time. Observations are weighted according to the probability of having a driver's license.

database is not available in other states to measure if states with similar voting patterns to Rhode Island experience the same measurable impacts on voting among those without licenses in the absence of a photo ID law implementation. However, we can take two approaches to examine if our findings are indeed specific to Rhode Island when the ID law was passed.

First, we can employ the Current Popolation Survey's Voting and Registration Supplement to compare voting patterns between Rhode Island and similar states (here, Connecticut and Massachusetts). This supplement is part of the November survey in even years and it includes questions of whether the respondent voted and whether they were registered to vote at the time of the election. These questions are only asked to people who are of voting age and US citizens.

One limitation of these data is that voting is self-reported (Hur and Achen, 2013). While in the Rhode Island database 59.8% of registered voters voted in the 2016 election, which closely matches official results, this number goes up to 89.2% in the CPS. If over reporting is constant over time across people with and without licenses, we can still use the the CPS data to compare the impact of the law in Rhode Island relative to its neighboring states. A second limitation of the CPS data is sample size. For reference, there are only 851 reported eligible voters in the CPS for the 2016 election in Rhode Island. A third limitation is the lack of information on driver's licenses or any other type of photo ID. We use Rhode Island data to predict the probability that a person has a driver's license based on their set of observable characteristics available both in the CPS and in the Rhode Island Database. We use LASSO to generate the prediction, and use the resulting coefficients to construct the probability that a respondent to the CPS has a driver's license. We then convert this predicted probability to a binary variable equal to one if the individual is above the 80th percentile in the distribution of the predicted probability of not having a license.[21]

With these data, we estimate equation 1 separately for Rhode Island, and for the pool of Connecticut and Massachusetts. The results are reported in Table A.10. The point estimates imply a decrease in voting and registration for Rhode Island in presidential elections, but no such result in the neighboring states. However, given the small sample sizes, the standard errors are large and the point estimates are not statistically significant for any state.

Second, we obtained voting data from Allegheny County in Pennsylvania, a county with similar sized metropolitan center and demographics to RI in a state that was willing to share voter records for academic research. These data do not include an indicator for individuals having a driver's license, but do include their address, age, gender, party affiliation, and race if they choose to report it (about 50% of individuals

[21] This percentile is chosen to approximate the fraction of people without a license in the Rhode Island data. Our conclusions are robust to using the 75th and 85th percentiles as well.

report race). We use block group level census data on income, education, employment status, commuting time to work, and car ownership among others, together with individual demographics and party affiliation in the RI to predict who has a driver's license using LASSO. We then use this prediction to test if we find similar results in RI when we use the predicted probability of having a driver's license instead of the actual driver's license indicator. We confirm that we find similar results. Next, we use the same prediction model to predict who has a driver's license in Allegheny County, and run the usual difference-in-differences model with voting conditional on registration as outcome variable to check whether we get analogous results where no voter ID law was implemented. We do not find similar results in Allegheny County.

In particular, results from the above set of regressions are presented in Table A.11. The two rows refer respectively to RI and Allegheny County. The first column reports the result of the difference-in-differences model using the actual driver's license indicator in RI. The second column replaces the actual indicator for driver's license with the predicted probability of having one. The third column trasforms the predicted probability in a predicted indicator using a threshold which is selected with ROC analysis. While in RI the coefficient estimated using the actual indicator is -.071, the one estimated using the predicted one is -.051. In PA instead, the coefficient obtained using the predicted indicator falls down to -.012. Results using the predicted probabilities also show that the coefficient estimated in PA is about 15 times smaller compared to the one estimated in RI. Standard errors clearly show how precise our estimates are.

Thus we do not find impacts in our PA placebo that we find in RI, nor do we find similar effects in MA and CT using CPS data. Both tests give us confidence that the main findings of this paper are most likely driven by the voter ID law, rather than a spurious correlation with specifics of the 2016 election and subpopulations less likely to have a driver's license.

6 Extensions

6.1 Heterogeneity Across Subpopulations

A major aspect of the debate on photo ID laws is whether certain populations are affected more than others because they lack valid photo IDs. We estimate our IV model separately for a number of subpopulations of interest. Figures 2-7 plot our estimates for presidential and midterm elections respectively. In these figures, the darker gray diamond shows the baseline mean of the dependent variable for each group[22], and the lighter gray square describes that average plus our estimate of the impact of the law with its confidence interval. We

[22]This baseline is the average of the variable in the pre-reform period. As explained in section 3.1, this turnout is slightly understimated due to our overestimation of the population of voting age.

estimate the model by socio-economic status, minority status, age group, gender, and party affiliation. These results are shown in the subsequent columns of each figure.[23] For presidential elections, the largest impacts are in low SES voters and the younger age groups. These are also the groups with the lowest baseline turnout rates. For midterm elections, we find mixed results with insignificant impacts for most subgroups.

In Tables A.14 and A.15 we also repeat our bounding exercise from section 5.1 for all subgroups. Just like in Table 4, we mostly observe zero or even slightly positive coefficients, meaning that, if anything, we may be slightly underestimating the effects of the law. The exception to this is the group of young voters. This is the group where we found the largest coefficient in Figure 2. This IV coefficient is larger (in absolute value) than the OLS coefficient, suggesting that moving from not having a license to having one may be negatively correlated with the likelihood of voting in this age group, rather than positively correlated. We find some evidence that this may be the case. Table A.3 shows that, in the general population, 6.4% of people without a license who voted in 2012 had a license by the 2016 election and 8.9% of people who did not vote and did not have a license in 2012 had a license in 2016. This difference is much larger for the people who are 22-25 years old in 2016. Within this group, 22% of those without a license who voted in 2012 had a license by the 2016 election and 37% of those who did not vote and did not have a license in 2012 had a license in 2016. Therefore, not only getting a new driver's license is much more common in young people but it is negatively correlated with having voted in 2012. While this may seem counterintuitive, given that having a license is positively correlated with voting, one possible explanation is that young people who are interested in voting are also more likely to get a driver's license even before they are eligible to vote[24] and those who only acquire their first license after age 18 are less likely to vote. Thus, the negative coefficients we observe in the first row of Table A.14 can partly explain why the effect of the law seems larger for young voters in Figure 2.

6.2 Heterogeneity Across Precincts

We examine heterogeneous impacts of the law across precincts to examine if the impact of the law varies with measures of local implementation of photo ID laws. In our data, we observe when poll workers gave a voter a provisional ballot, for what reason they decided to do this, and what final decision the LBC made on each provisional ballot. We use the fraction of voters in a given precinct who are given a provisional ballot, and the fraction of those ballots that are accepted by the LBC. We use this as potential measures of how rigorously voter requirements are administered locally. Ideally we want to observe whether poll workers

[23] We do not include voters younger than 22 in our IV specification because those who were younger than 22 in 2016 were not eligible for a driver's license at the time the photo ID law passed. Therefore our instrumental variable is not available for them.

[24] Rhode Island residents can apply for a limited provisional license by age 16.

turned voters away from the polls when they did not produce a valid photo ID. However, we cannot distinguish a person who showed up at the polls and was denied a ballot by the officials from the individual who simply chose to stay home.[25] We use the fraction of voters using a provisional ballot and the fraction of ballots accepted conditional on local demographic characteristics as a potential indirect measure of photo ID law implementation.

Table A.2 shows an increase over time in the fraction of voters at the polls (that is, excluding mail ballots) that received a provisional ballot from 0.28% in 2010 to 0.95% in 2016. Voters who received a provisional ballot due to the lack of photo ID were just 18% of all provisional ballots. The main reason for a provisional ballot is not being in the voter list for the precinct, which typically occurs either because the individual went to the wrong precinct or because they did not register to vote before the deadline for that election. Because of this, and the fact that a precinct is a relatively small unit, we extend this part of the analysis to all voter requirements, not just the photo ID law, and we aggregate precincts to the representative district level. Because a redistricting changed electoral precincts and districts in the state before the 2012 election, we only conduct this exercise for presidential elections when all districts remain unchanged.[26]

Our main results are reported in Figures 8 and 9. In these graphs, the vertical axis shows the coefficient of estimating equation 1 on voting conditional on registration in each district using our instrumental variables specification, with its confidence interval. The horizontal axis shows the fraction of provisional ballots given or accepted in each district, respectively. We conduct this exercise only on the group of registered voters using voter conditional on registration as the dependent variable. We do this because electoral precincts are assigned only to registered voters. We fit a linear prediction of our estimated effects of the law on our measures of law administration rigidity, where districts are weighted by their population size. The negative slope of this line in Figure 8 indicates that we do observe a stronger impact of the photo ID law in districts where voters are more likely to receive provisional ballots. We do observe also a slightly negative correlation in Figure 9 where the horizontal axis variable is the fraction of provisional ballots accepted. Thus, the effects of the photo ID law seem to be correlated with the rigidity with which poll workers apply the voting rules or with our measure of LBCs rigidity.

To check whether difference in voters across precincts drive these results, we first regress our measures of implementation on the observable characteristics of voters in that district and we compute the residuals of that regression. Then, we repeat our plots using the residualized measure of strictness. Figures A.7 and A.8

[25]According to their training, poll workers are instructed that they must give a provisional ballot to any person who states they are allowed to vote in their precinct. While there were reported anecdotal evidence that poll workers may have turn away some voters, these instances are not directly observable in our data.

[26]Figures A.5 and A.6 show substantial variation in the prevalence of provisional ballots and their acceptance rate across precincts. This is the variation we exploit in this exercise.

show these results. The fitted line in Figure A.7 is slightly flatter than the one in Figure 8, suggesting that the overall relationship is partially driven by observable differences.

6.3 State Legislators, Party Affiliation, and the Photo ID Law

The photo ID law was passed in Rhode Island's state legislature by a margin of 54-21 in the House of Representatives and by a margin of 28-6 in the Senate. We separate electoral districts in four groups according to whether both the corresponding representative and senator voted for or against the photo ID law, and we estimate equation 1 in each of these groups separately.

Figure 10 shows that districts where both legislators voted against the law had a larger fraction of people without a driver's license, a lower baseline turnout, and display an effect on the turnout of registered voters more than twice as large as the effect in districts where both legislators voted in favor of the law. This suggests legislators may have considered the potential effects on their constituencies when voting on the law.

Our final exercise consists of examining whether the previous results are correlated with the political affiliation of voters in each district. In Table 5, we conduct a counterfactual exercise where we compute the turnout among registered voters and dividing our sample by party affiliation and votes of state representatives and senators.[27] Focusing on presidential elections to compare our results with Figure 10, we can see that we once again observe larger effects in districts where representatives and senators voted against the law, and also that those differences in turnout are concentrated in effects on the turnout of registered Democrats.

6.4 Mail Ballots

Because the identity of a voter who uses a mail ballot cannot be checked in person, alternative verification is required. The voter who wishes to vote by mail first fills out and signs a mail ballot application. This signature and the voter's registration information are verified against the corresponding registration form by the Local Board of Canvassers. If those signatures and the voter regsitration information match, the voter receives a mail ballot and an oath envelope. The voter must also sign this oath evelope where their mail ballot is placed, and this signature must be either notarized or witnessed by two people[28]. When the complete ballot is returned, the Board matches the signature in the oath envelope to the signature in the original application before opening and counting the ballot. This means the mail ballot could allow voters without a valid photo ID to cast a vote and verify their identity in this alternative way. Thus registrants can vote by mail ballot without a photo ID. Furthermore, Rhode Island voters can use mail ballots without the need to prove they

[27] In Table A.16 we also compare predicted and actual turnout by party affiliation and legislator votes.

[28] Voters who are absent from the state due to military service or living overseas do not face this requirement.

are unable to show up at the polls.

If voters without a photo ID understood that they could alternatively vote by mail ballot as described above, we would expect that (i) more mail ballots were requested to addresses in Rhode Island, and (ii) that voters without a driver's license became relatively more likely to use a mail ballot without a specific excuse for not being able to vote at the polls. To test whether this is the case, we obtained detailed mail ballot data since 2012, which means we can only compare the 2012 and 2016 presidential elections. In these data, we do observe a strong increase in the use of mail ballots over time (60% more mail ballots were received in 2016 vs 2012, see table A.17). Almost all of this increase is explained by voters who requested the mail ballot be sent to an address in Rhode Island. Also, most of the increase is explained by voters who chose the "no excuse" option to vote by mail. However, Figure A.9 shows the increase in the use of mail ballots is large for both voters with and without driver's licenses.

To test whether our no-license and license groups behaved differently in this regard, we estimate our difference-in-differences model only on the subsample of people who voted, and we use voting by mail as the dependent variable. Figure 11 shows the results. We find a small overall impact on the likelihood that a registered voter chooses to cast their ballot by mail. We do observe significant results for a few subgroups, but not all are significant in both IV and OLS specifications. While the point estimate for young voters is not statistically significant in our IV specification, it is very similar in size and statistically significant in the OLS specification. We conclude that mail ballots did not appear to be an effective substitute for voting at the polls for those without photo IDs.

7 Conclusion

This paper provides new empirical evidence on the effects of photo ID laws on turnout, registration, and voting conditional on registration. In all presidential election specifications, we find a significant negative effect of the photo ID requirement on the probability that people without driver's licenses vote and the probability that they register to vote. This suggests previous studies that only used data on registered voters may have understated the effect of photo ID requirements.

We do not find compelling evidence that voters responded to the photo ID requirement by either proactively obtaining driver's licenses, except potentially among very young voters. We also do not find that people without photo IDs used mail ballots as a substitute for voting at the polls.

Finally, we do find evidence that heterogeneity in the estimated effect of the law is correlated with the rigidity in which voter requirements are applied in local precincts. We also show the behavior of state legislators is consistent with an anticipation of the effects of the law on their own constituencies.

These results contribute to the understanding of a relevant and currently open U.S. policy debate, and suggests subgroups for which more outreach and education could help minimize any undesired impacts of voter photo ID requirements while preserving the integrity of the electoral process.

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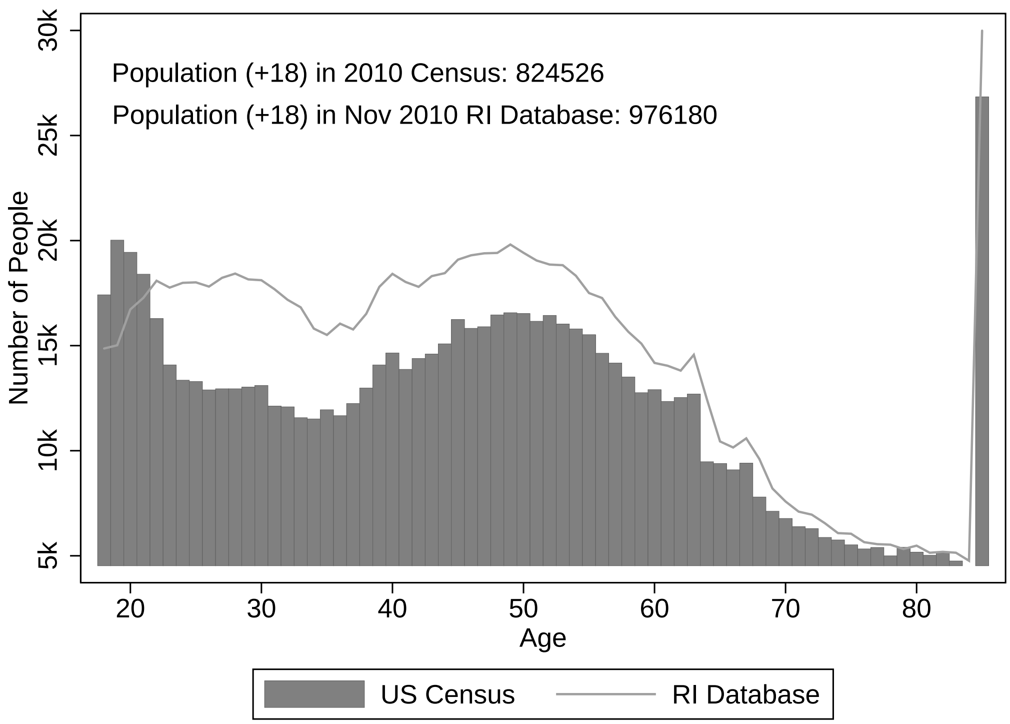
Figure 1: State Population by Age (18+)



Notes: The population of age 85 and over is collapsed.

Table 1: Descriptive Statistics of Voters in 2012

	% Registered	% Voted	% with Driver's License
All Voters	0.67	0.45	0.77
Young (22-25)	0.60	0.29	0.75
Adult (26-40)	0.58	0.31	0.74
Adult (41-64)	0.69	0.52	0.82
Elder (65+)	0.79	0.60	0.72
Low SES	0.50	0.30	0.74
High SES	0.75	0.53	0.79
Minority	0.47	0.28	0.68
White	0.64	0.43	0.84
Female	0.69	0.49	0.78
Male	0.63	0.42	0.81
Democrats	-	0.87	0.62
Republicans	-	0.75	0.86
Independents	-	0.64	0.81

Notes: In columns 1-3 we report respectively the fraction of people registerd to vote, the fraction of people that voted, and the fraction of people having a driving license. The fraction of people that voted is computed over the entire voting age population. These three fractions are reported for each subgroup of voters which we include in our analysis. We refer to the 2012 presidential election year.

Table 2: Estimated Impact of the Photo ID Law

	(1)	(2)	(3)
	OLS	IV	Fixed Effects
	Presidential Elections		
Turnout	-0.047	-0.027	-0.065
	(0.001)	(0.002)	(0.001)
Registration	-0.080	-0.076	-0.048
	(0.001)	(0.002)	(0.001)
Voted$\|$Reg	-0.017	0.001	-0.050
	(0.002)	(0.002)	(0.002)
	Midterm Elections		
Turnout	0.005	0.003	-0.017
	(0.001)	(0.002)	(0.001)
Registration	-0.055	-0.051	-0.033
	(0.001)	(0.002)	(0.001)
Voted$\|$Reg	0.038	0.041	-0.009
	(0.002)	(0.002)	(0.002)

Notes: The table reports the α_3 coefficient in equation 1 for different model specifications in presidential and midterm elections. The dependent variable is either being registered or voting. In column 2, having a driver's license at the time of the election is instrumented by having a license in July 2011. In column 3 we use individual fixed effects. Controls are selected using Double LASSO. Robust standard errors are reported in parenthesis.

Table 3: Estimated Placebo Effects of Photo ID Law

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable					
	Turnout	SNAP	TANF	UI	Incarceration	Child
	Presidential Elections (*Panel* A)					
OLS	-0.0470	-0.0003	-0.0039	0.0069	-0.0001	-0.0023
	(0.0014)	(0.0010)	(0.0003)	(0.0004)	(0.0000)	(0.0004)
	[0.2115]	[0.1641]	[0.0111]	[0.0106]	[0.0001]	[0.0097]
IV	-0.0275	0.0348	-0.0023	0.0115	0.0000	-0.0016
	(0.0020)	(0.0014)	(0.0005)	(0.0004)	(0.0000)	(0.0005)
	[0.2038]	[0.1667]	[0.0099]	[0.0055]	[0.0000]	[0.0062]
Fixed Effects	-0.0645	-0.0059	-0.0039	0.0127	-0.0002	0.0008
	(0.0012)	(0.0008)	(0.0004)	(0.0004)	(0.0000)	(0.0004)
	[0.2368]	[0.1765]	[0.0116]	[0.0054]	[0.0001]	[0.0079]
	Midterm Elections (*Panel* B)					
OLS	0.0051	-0.0016	-0.0039	0.0105	-0.0000	-0.0038
	(0.0013)	(0.0010)	(0.0004)	(0.0005)	(0.0000)	(0.0004)
	[0.1422]	[0.1601]	[0.0143]	[0.0160]	[0.0002]	[0.0104]
IV	0.0032	0.0146	-0.0033	0.0165	-0.0001	-0.0043
	(0.0016)	(0.0013)	(0.0005)	(0.0005)	(0.0000)	(0.0005)
	[0.1562]	[0.1653]	[0.0140]	[0.0104]	[0.0001]	[0.0094]
Fixed Effects	-0.0166	-0.0087	-0.0045	0.0222	-0.0002	-0.0002
	(0.0011)	(0.0009)	(0.0005)	(0.0005)	(0.0000)	(0.0005)
	[0.1691]	[0.1676]	[0.0141]	[0.0082]	[0.0001]	[0.0086]

Notes: We run our difference-in-differences specification using different dependent variables which should have not been affected by the photo ID law. We show theα_3 coefficient in equation 1. Controls are selected using Double LASSO. Robust standard errors are reported in parenthesis. Averages of the dependent variable among voters belonging to the "treatment" group are reported in brackets. The dependent variable in column 5 is being incarcerated at the time of an election. The dependent variable in column 6 is having a child born during the year of the election.

Table 4: Estimated Impact of the Photo ID Law on Predicted Voting

	(1)	(2)	(3)
	OLS	IV	Fixed Effects
	Presidential Elections		
Turnout	0.0038	-0.0012	0.0007
	(0.0002)	(0.0003)	(0.0000)
Registration	0.0059	0.0004	0.0009
	(0.0002)	(0.0003)	(0.0000)
Voted|Reg	0.0028	-0.0011	-0.0010
	(0.0001)	(0.0002)	(0.0001)
	Midterm Elections		
Turnout	0.0012	-0.0029	0.0032
	(0.0002)	(0.0003)	(0.0000)
Registration	0.0032	-0.0013	0.0035
	(0.0002)	(0.0003)	(0.0001)
Voted|Reg	0.0035	-0.0007	0.0010
	(0.0002)	(0.0002)	(0.0000)

Notes: The table reports the α_3 coefficient in equation 1 for different model specifications in presidential and midterm elections. The dependent variable is either predicted registration or predicted voting from an OLS regression of actual registration or actual voting on voters observable characteristics. In column 2, having a driver's license at the time of the election is instrumented by having a license in July 2011. In column 3 we use individual fixed effects. Controls are selected using Double LASSO. Robust standard errors are reported in parenthesis.

Figure 2: Turnout Estimates in Presidential Elections

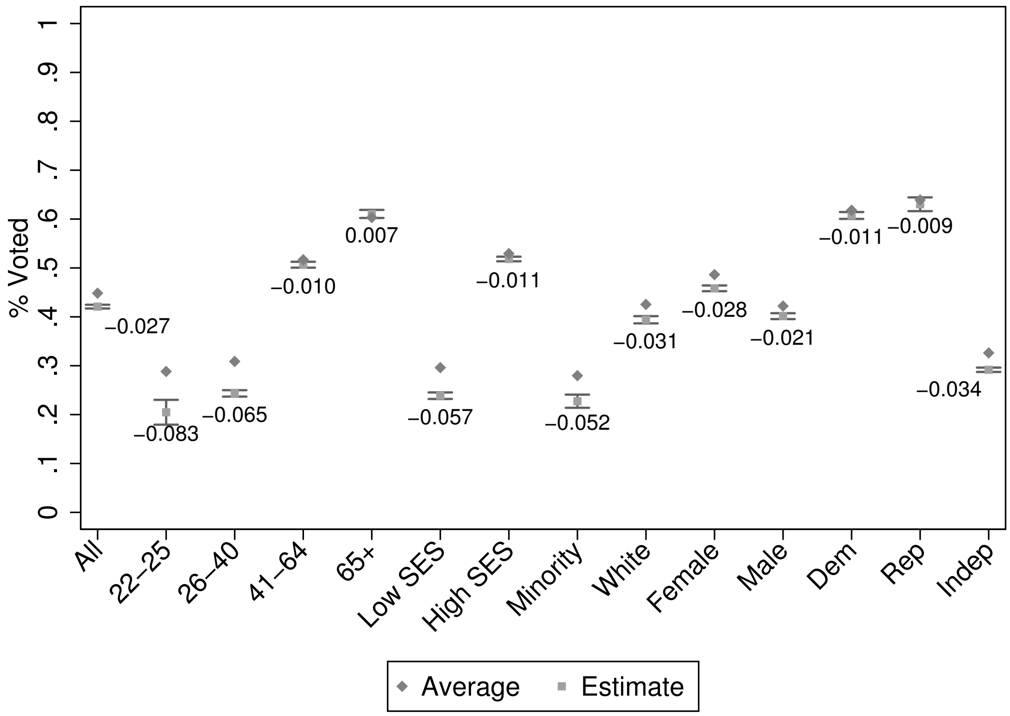


Figure 3: Registration Estimates in Presidential Elections

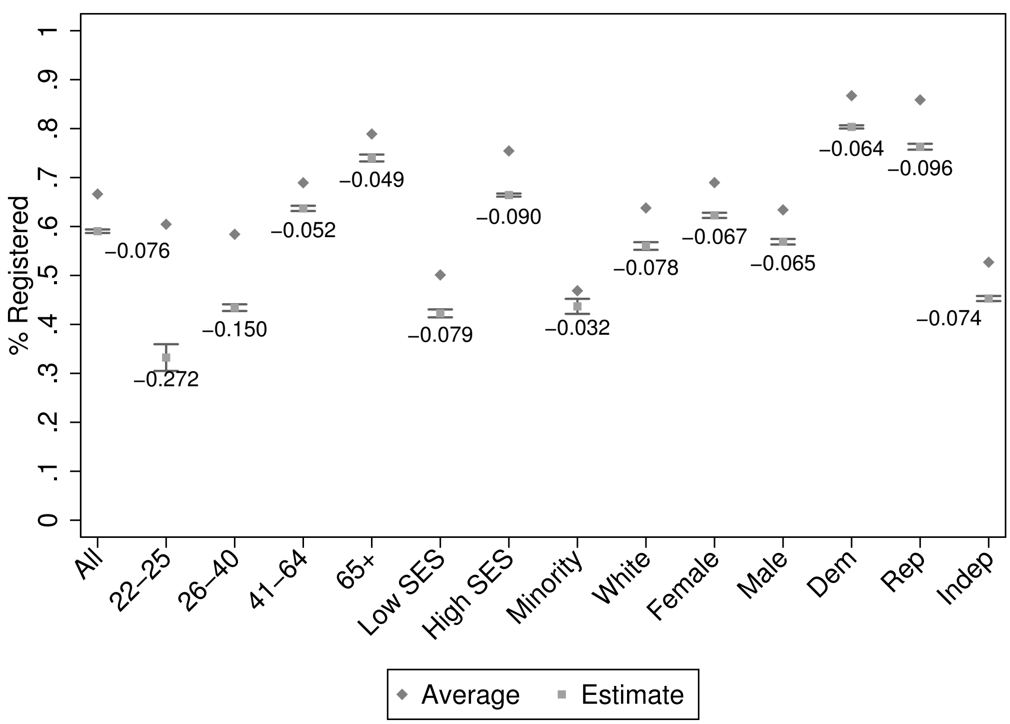


Figure 4: Voting Conditional on Registration Estimates in Presidential Elections

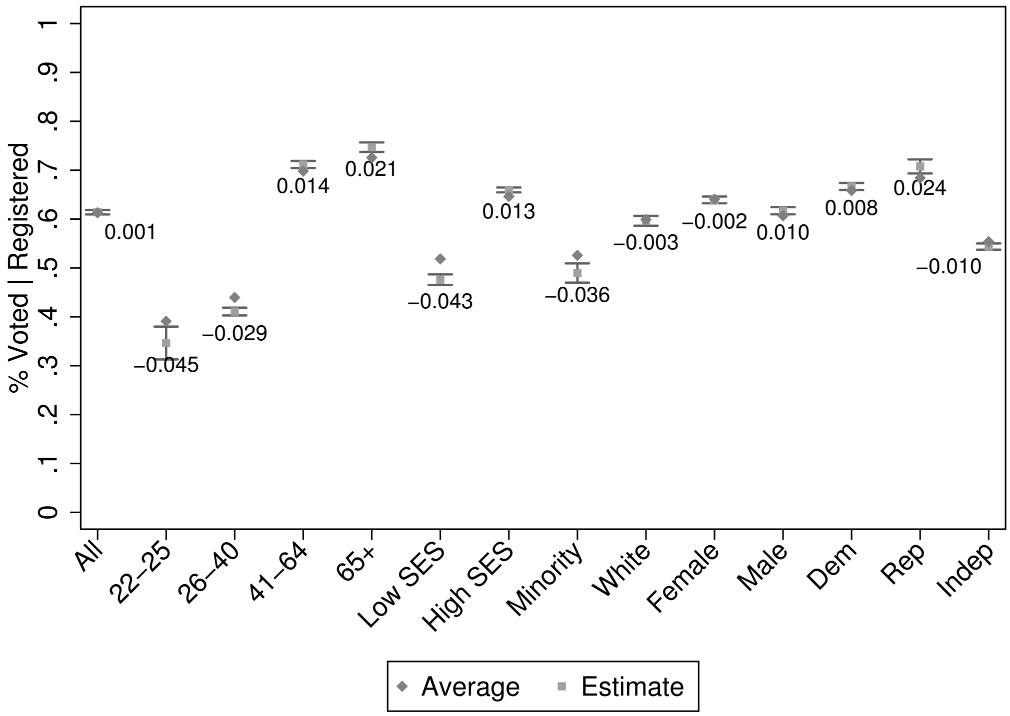


Figure 5: Turnout Estimates in Midterm Elections

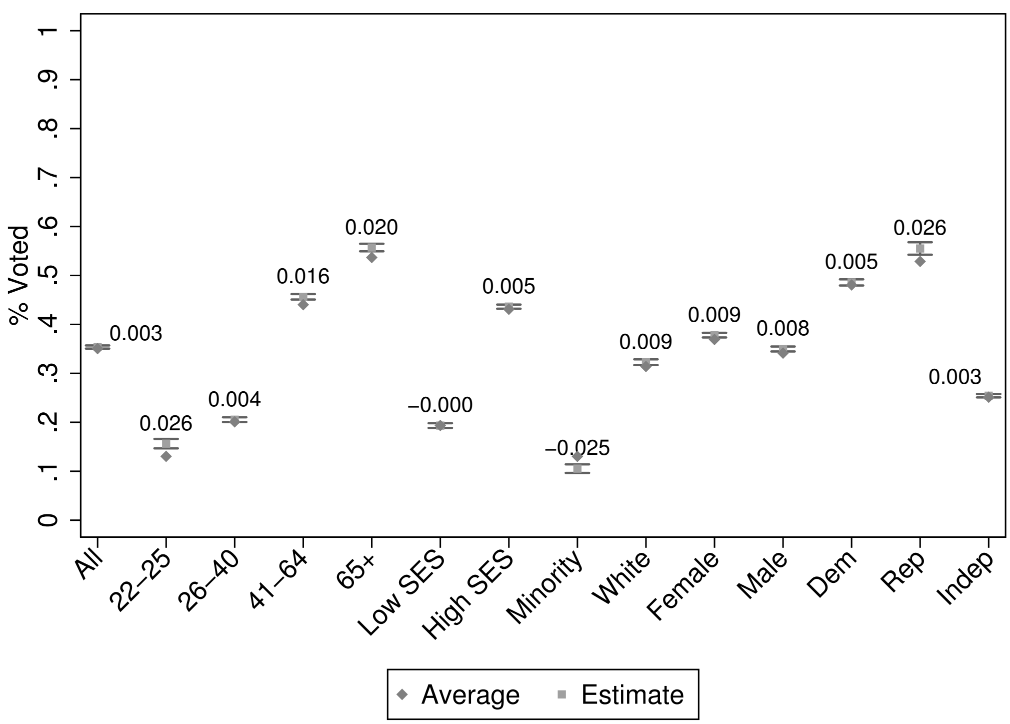


Figure 6: Registration Estimates in Midterm Elections

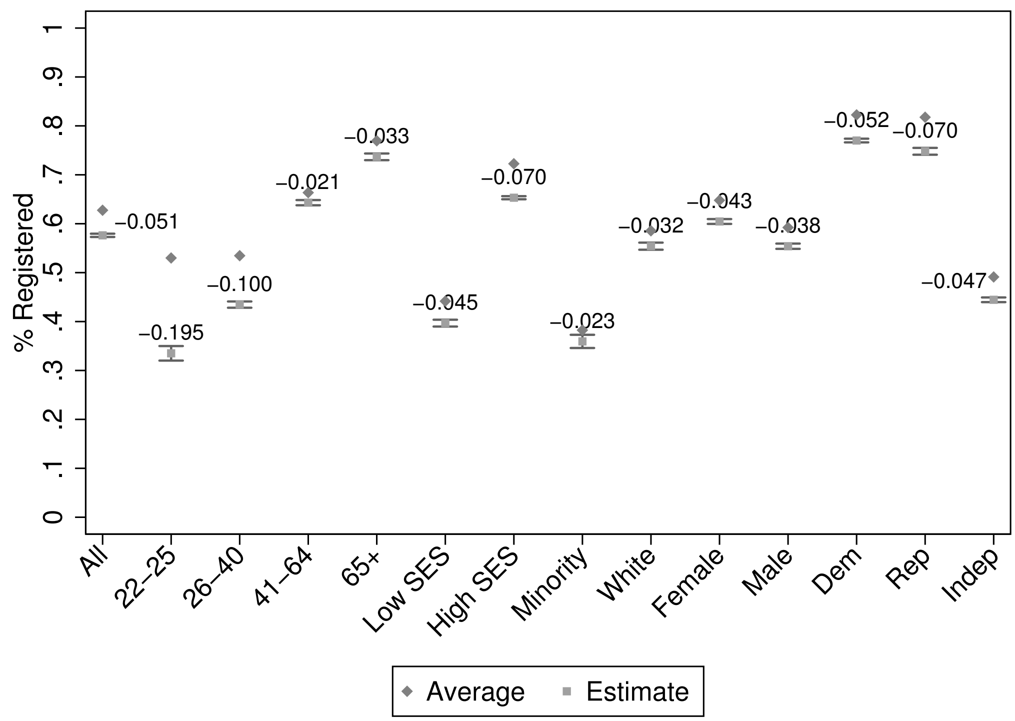


Figure 7: Voting Conditional on Registration Estimates in Midterm Elections

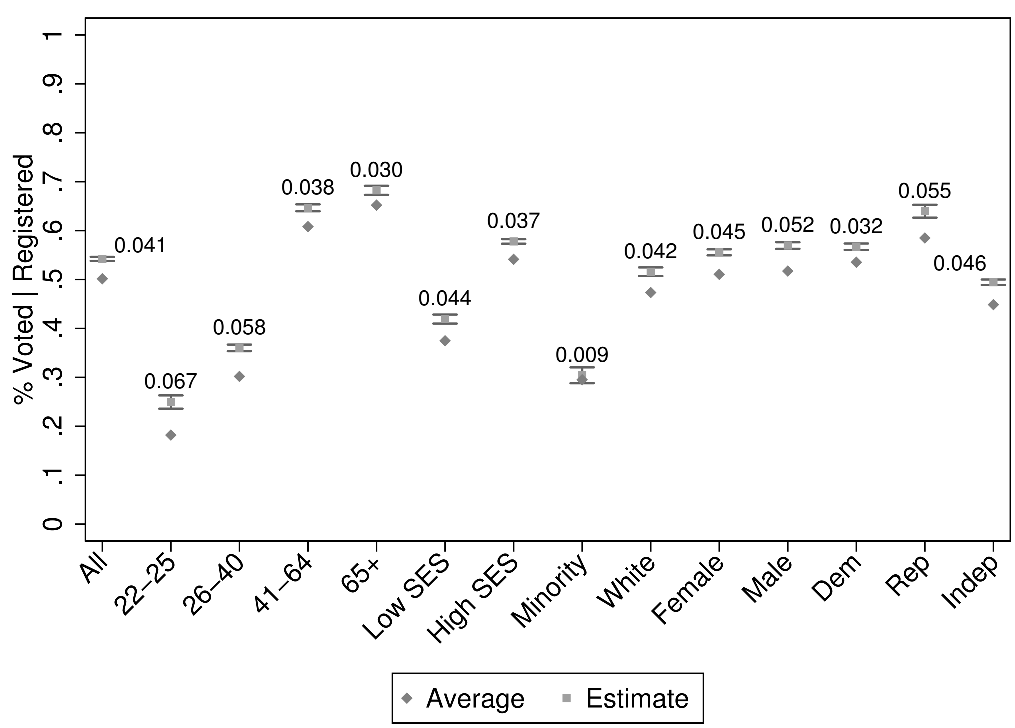
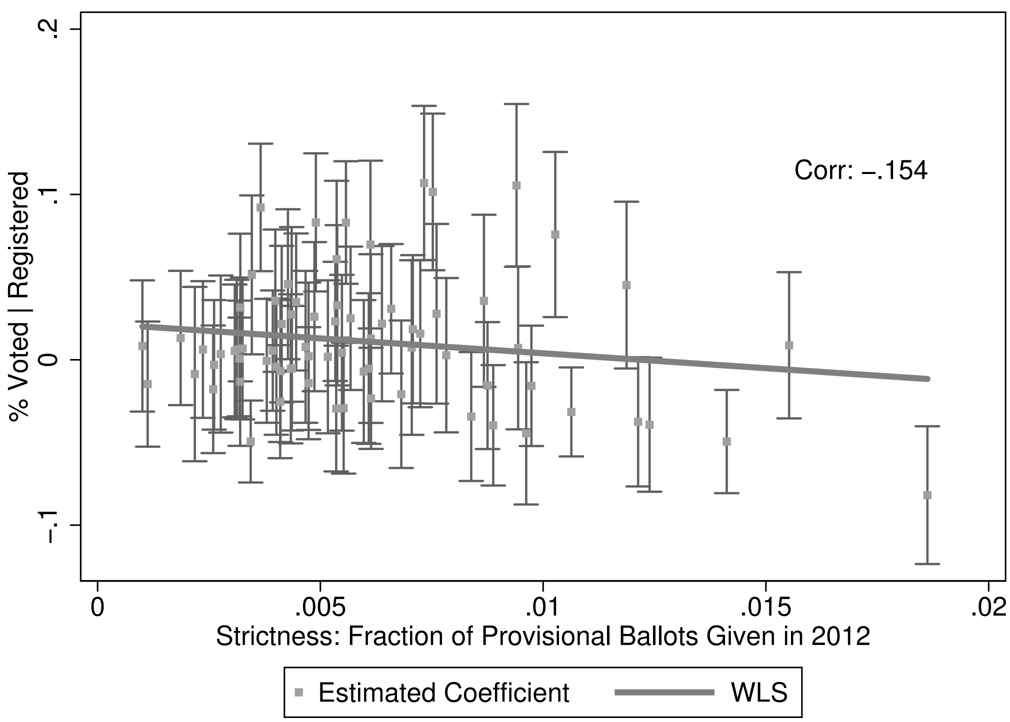
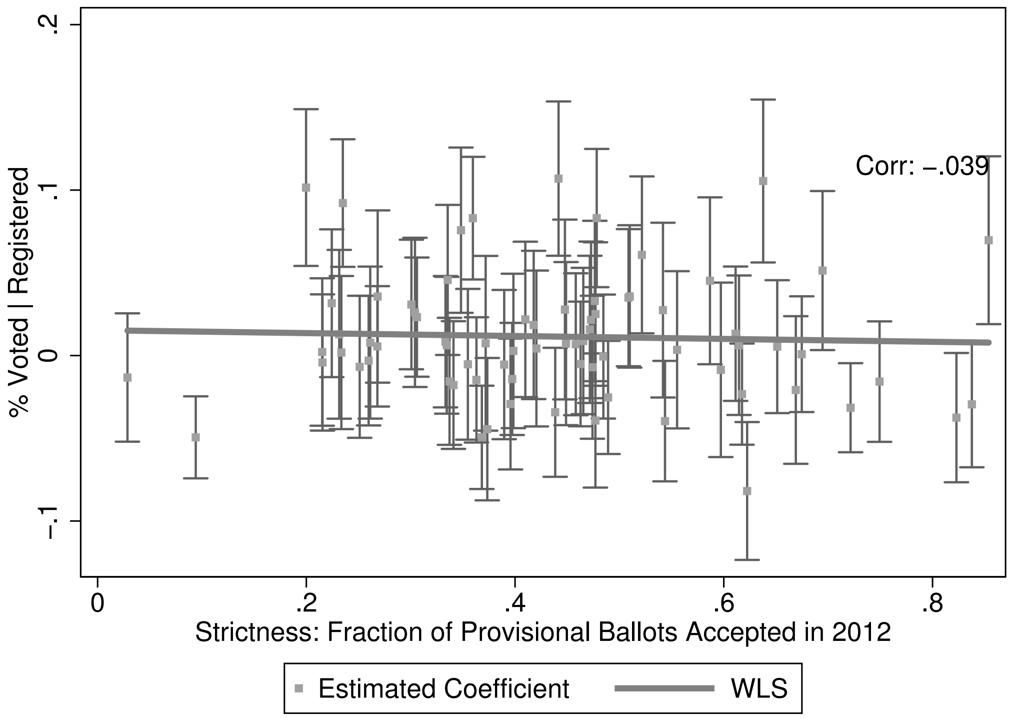


Figure 8: Correlation Between Impact of Photo ID Law and Provisional Ballots Given
by Congressional District



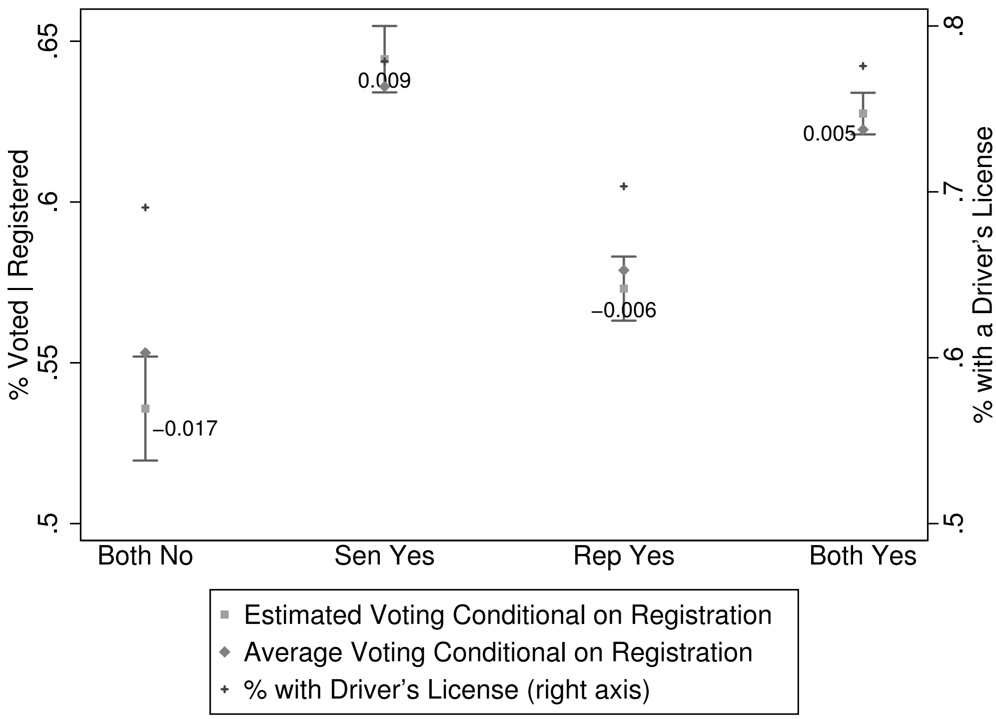
Notes: This graph shows the correlation (weighted by district population) between district strictness and the estimated effect of the law at the district level. The coefficients plotted correspond to α_3 in equation 1. We run IV regressions by district with controls selected by Double LASSO. In all these regressions the dependent variable is voting conditional on registration. This graph drops 1 case for which the fraction of provisional ballots given is higher than 5%. The number of districts is 75.

Figure 9: Correlation Between Impact of Photo ID Law and Provisional Ballots Accepted
by Congressional District



Notes: This graph shows the correlation (weighted by district population) between district strictness and the estimated effect of the law at the district level. The coefficients plotted correspond to α_3 in equation 1. We run IV regressions by district with controls selected by Double LASSO. In all these regressions the dependent variable is voting conditional on registration. This graph drops 1 case for which the fraction of provisional ballots given is higher than 5%. The number of districts is 75.

Figure 10: Impact of Photo ID Law by State Legislator Votes on the Law
in Presidential Elections



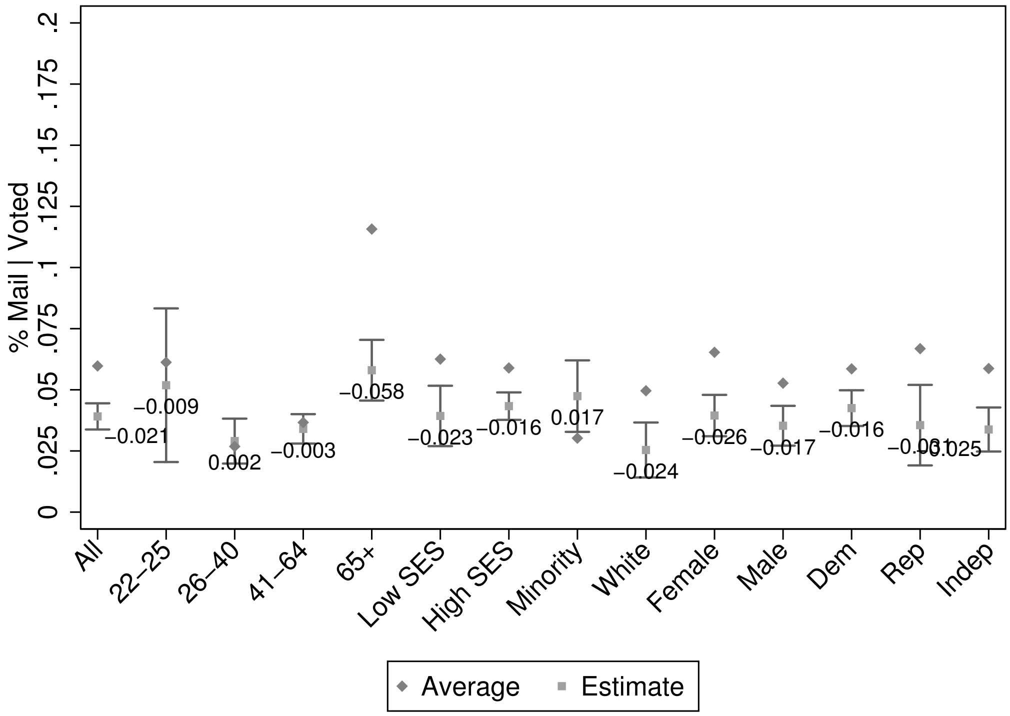
Notes: This graph shows how the estimated impact of the law varied depending whether district legislators voted in favor of the law or not. Precincts are grouped depending on how the senator and the representative voted. We run IV regressions by these four precinct groups with controls selected by Double LASSO. In all these regressions the dependent variable is voting conditional on registration. The number of precincts in which both legislators voted against the law are 31. The number of precincts in which only the senator was in favor of the law are 89. The number of precincts in which only the representative was in favor of the law are 80. Finally, the number of precincts in which both legislators voted in favor of the law are 221.

Table 5: Counterfacual Analysis on Voting Conditional on Registration by Party Affiliation

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Registered Democrats			Registered Republicans			Independents		
		Predicted	Counterf.	Difference	Predicted	Counterf.	Difference	Predicted	Counterf.	Difference
Districts where:		2016 Election (*Panel A*)								
Representative Voted	Yes	0.628	0.626	0.002	0.673	0.668	0.005	0.531	0.534	-0.003
	No	0.624	0.621	0.002	0.690	0.685	0.006	0.538	0.541	-0.003
Senator Voted	Yes	0.645	0.641	0.004	0.691	0.686	0.005	0.553	0.555	-0.002
	No	0.581	0.583	-0.002	0.614	0.610	0.004	0.466	0.471	-0.005
All		0.627	0.624	0.002	0.677	0.672	0.005	0.533	0.536	-0.003
		2014 Election (*Panel* B)								
Representative Voted	Yes	0.486	0.477	0.008	0.533	0.522	0.012	0.382	0.371	0.011
	No	0.489	0.482	0.007	0.561	0.551	0.009	0.402	0.392	0.011
Senator Voted	Yes	0.503	0.495	0.008	0.556	0.547	0.009	0.407	0.396	0.010
	No	0.445	0.436	0.008	0.471	0.451	0.020	0.325	0.312	0.013
All		0.486	0.478	0.008	0.541	0.530	0.011	0.388	0.377	0.011

Notes: The table reports predicted voting conditional on registration, an estimated counterfactual without the photo ID requirement, and the difference between the two. Rows split the sample according to whether state representatives or senators voted for or against the photo ID law. Predictions come from coefficients estimated with IV regressions. The counterfactual is the predicted value when $\hat{\alpha}_3$ is set to zero. Panel A shows the counterfactual results for 2016 (presidential elections). Panel B shows the counterfactual results for 2014 (midterm elections). Controls are selected using Double LASSO.

Figure 11: Impact of Photo ID Law on Mail Ballots in Presidential Elections



A Online Appendix: Tables and Figures

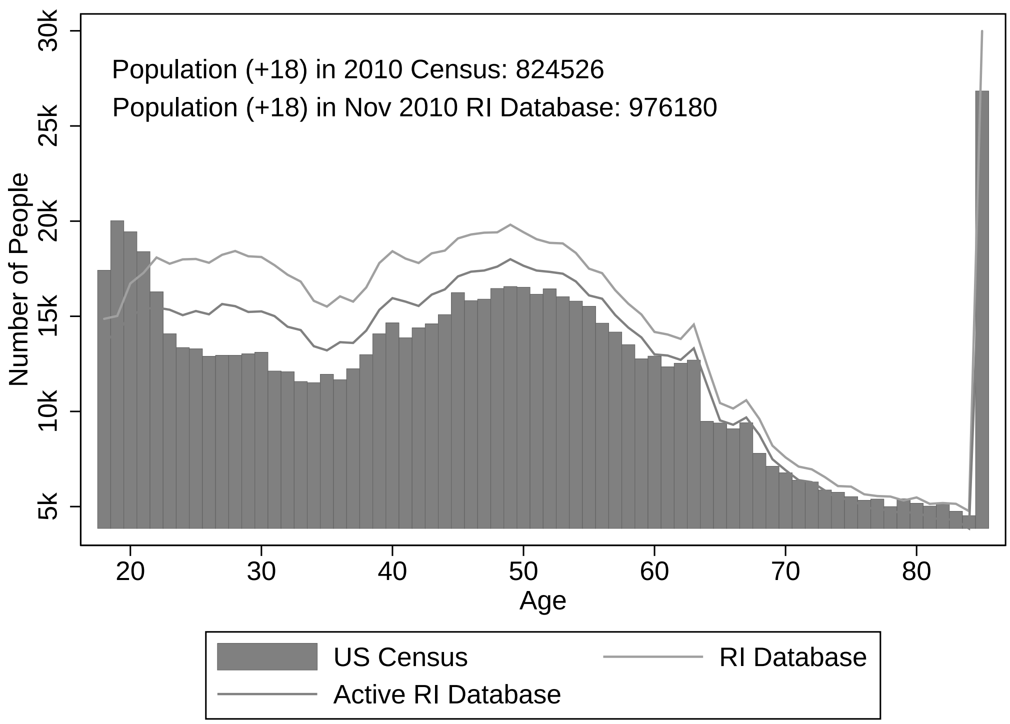
Table A.1: State ID Law Comparison

State	Voter ID Required	Require Further Proof	Year of Current Law	Vote by Mail
Alabama	Photo ID	No	2003	Excuse Required
Alaska	Non-photo ID	No	1980	No Excuse
Arizona	Non-photo ID	Yes	2004	No Excuse
Arkansas	Photo ID	No	2017	Excuse Required
California	No ID required	—	—	No Excuse
Colorado	Non-photo ID	No	2003	Everyone
Connecticut	Non-photo ID	No	2012	Excuse Required
Delaware	Non-photo ID	No	1996	Excuse Required
D.C.	No ID required	—	—	No Excuse
Florida	Photo ID	No	1977	No Excuse
Georgia	Photo ID	Yes	2005	No Excuse
Hawaii	Non-photo ID	No	1970	No Excuse
Idaho	Photo ID	No	2010	No Excuse
Illinois	No ID required	—	—	No Excuse
Indiana	Photo ID	Yes	2005	Excuse Required
Iowa	No ID required	—	—	No Excuse
Kansas	Photo ID	Yes	2012	No Excuse
Kentucky	Non-photo ID	No	1988	Excuse Required
Louisiana	Photo ID	No	2011	Excuse Required
Maine	No ID required	—	—	No Excuse
Maryland	No ID required	—	—	No Excuse
Massachusetts	No ID required	—	—	Excuse Required
Michigan	Photo ID	No	2016	Excuse Required
Minnesota	No ID required	—	—	No Excuse
Mississippi	Photo ID	Yes	2011	Excuse Required
Missouri	Non-photo ID	No	2002	Excuse Required
Montana	Non-photo ID	No	2003	No Excuse
Nebraska	No ID required	—	—	No Excuse

Nevada	No ID required	—	—	No Excuse
New Hampshire	Non-photo ID	No	2012	Excuse Required
New Jersey	No ID required	—	—	No Excuse
New Mexico	No ID required	—	—	No Excuse
New York	No ID required	—	—	Excuse Required
North Carolina	No ID required	—	—	No Excuse
North Dakota	Non-photo ID	Yes	2017	No Excuse
Ohio	Non-photo ID	Yes	2006	No Excuse
Oklahoma	Photo ID	No	2010	No Excuse
Oregon	No ID required	—	—	Everyone
Pennsylvania	No ID required	—	—	Excuse Required
Rhode Island	**Photo ID**	**No**	**2011**	**No Excuse**
South Carolina	Non-photo ID	No	2011	Excuse Required
South Dakota	Photo ID	No	2003	No Excuse
Tennessee	Photo ID	Yes	2013	Excuse Required
Texas	Photo ID	No	2017	Excuse Required
Utah	Non-photo ID	No	2009	No Excuse
Vermont	No ID required	—	—	No Excuse
Virginia	Photo ID	Yes	2013	Excuse Required
Washington	No ID required	No	2008	Everyone
West Virginia	No ID required	—	—	Excuse Required
Wisconsin	Photo ID	Yes	2016	No Excuse
Wyoming	No ID required	—	—	No Excuse

Sources: National Council of State Legislatures (NCSL), and state government websites.
Notes: A state is classified as “requiring further proof” if the voter who receives a provisional ballot due to the lack of a valid ID must take further steps to have their vote counted, such as providing proof of identity to their board of elections at a later date.
The year indicated in the third column is when the ID law that is currently in effect was either passed or last amended.
Voting by mail requires an excuse when a voter must sign an affidavit and/or provide further evidence that they are physically imparied or unable to be at the polls during the election. Colorado and Washington conduct their elections entirely by mail.
Iowa and West Virginia passed voter ID laws that will take effect in 2018.
Missouri established a photo ID requirement starting in 2017.
In North Carolina and Pennsylvania, photo ID laws have been struck down in court and are currently not in effect.
North Dakota’s ID law is enacted but currently blocked by a court decision.
Washington is usually qualified as a no ID required state because the entire states votes by mail as the norm. However, each district opens one voting booth and there is a non-strict non-photo ID requirement for those who use them. In the 2016 presidential election, 12,576 voters out of 3.36 million used the voting booths.

Figure A.1: State Population by Age (18+)



Notes: The population of age 85 and over is collapsed. We have added one line with respect to 1 of active people in the RI Database. We define people active if, in a 12 months window around November 2010, they have been enrolled in any social program or have registered a car at the DMV, or had a child, or have been incarcerated, or had a valid driver's license, or received a wage in RI. Social programs considered are: Medicaid, SNAP, TANF, TDI, UI, Supplemental SSI, GPA, and CCAP.

Figure A.2: Turnout by Election and Gender

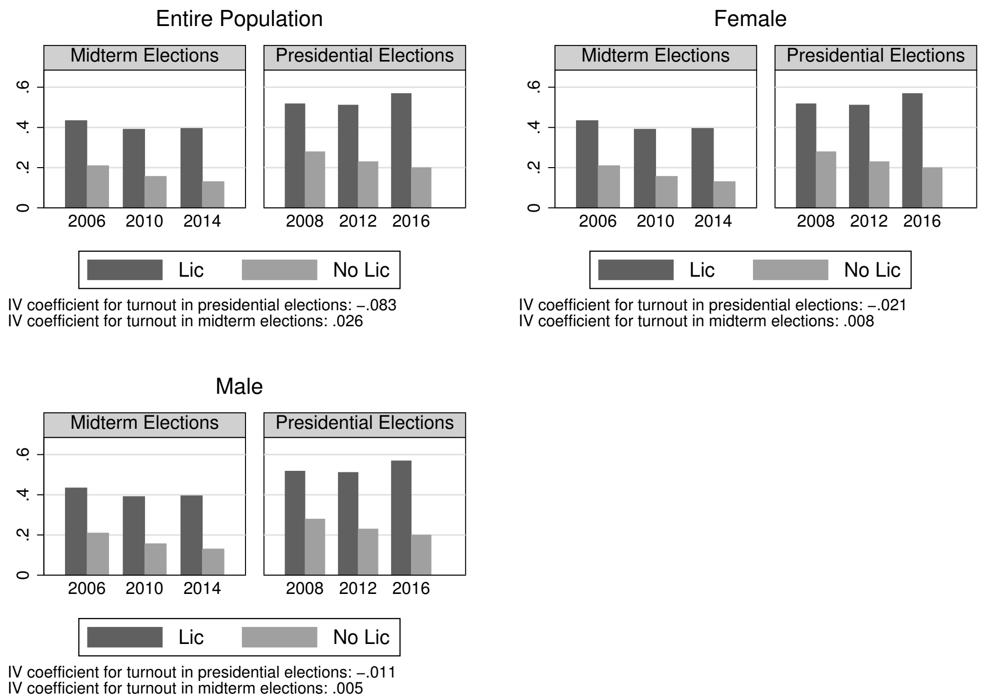


Figure A.3: Turnout by Election and Age

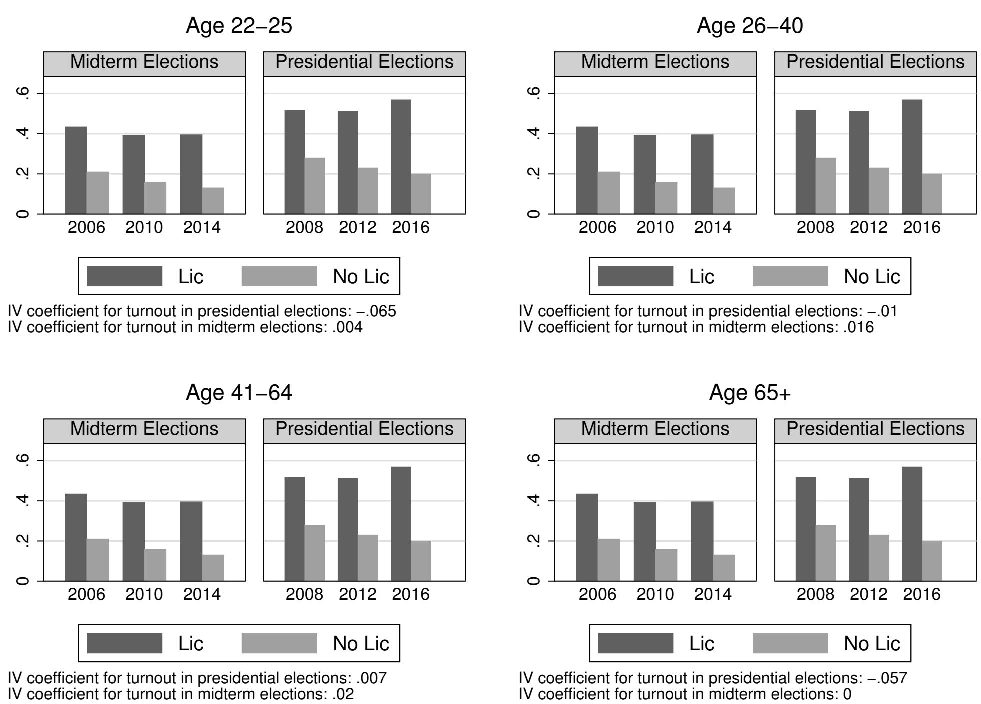


Figure A.4: Turnout by Election, SES, and Race Subgroups

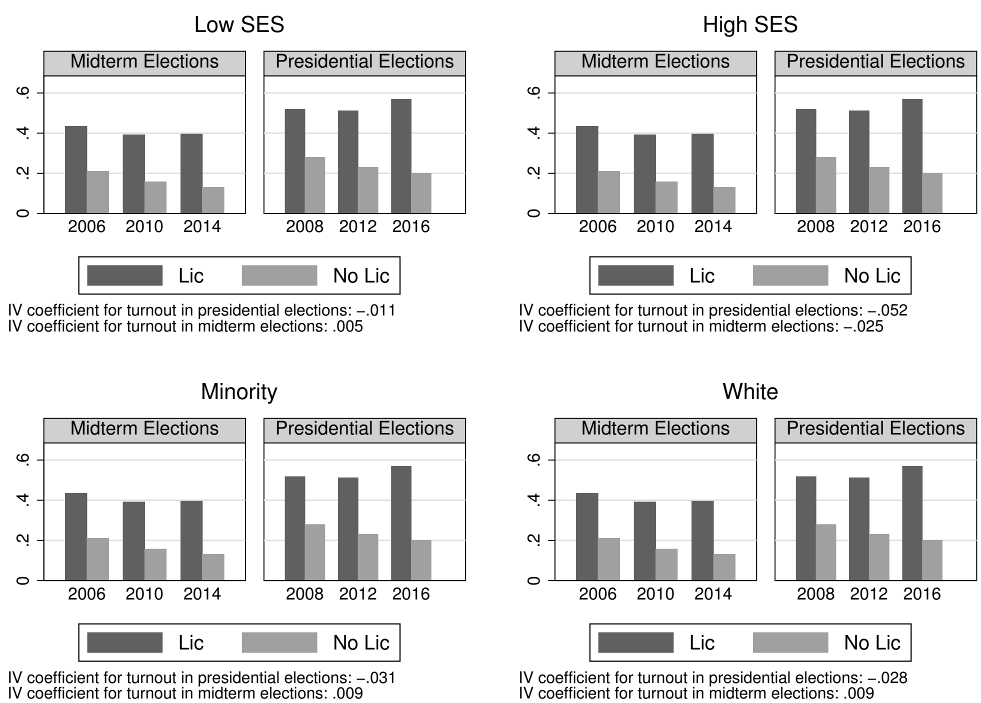


Table A.2: Provisional Ballots by Election Year and Category

		Election Year			
		2010	2012	2014	2016
(1)	**Provisional Ballots Given:**	926	2272	2046	4092
(2)	No Category Selected	0	0	38	298
(3)	No ID Presented	5	92	401	735
(4)	Not on the Precinct Voting List	907	2094	1524	2904
(5)	Voter Identity Challenged	2	9	11	15
(6)	Already Voted by Mail	12	77	72	140
(7)	**% Provisional Ballots:**	0.28%	0.53%	0.65%	0.95%
(8)	No Category Selected	0.00%	0.00%	0.01%	0.07%
(9)	No ID Presented	0.00%	0.02%	0.13%	0.17%
(10)	Not on the Precinct Voting List	0.27%	0.49%	0.48%	0.67%
(11)	Voter Identity Challenged	0.00%	0.00%	0.00%	0.00%
(12)	Already Voted by Mail	0.00%	0.02%	0.02%	0.03%
(13)	**% Accepted Provisional Ballots:**	59.61%	45.55%	51.71%	50.71%
(14)	No Category Selected	0.00%	0.00%	76.32%	67.45%
(15)	No ID Presented	80.00%	79.35%	96.01%	85.71%
(16)	Not on the Precinct Voting List	58.88%	42.12%	37.47%	38.02%
(17)	Voter Identity Challenged	100.00%	100.00%	36.36%	53.33%
(18)	Already Voted by Mail	100.00%	92.21%	95.83%	94.29%

Notes: We report the number of provisional ballots by election year and by category. The columns refer respectively to the four general elections held from 2010 to 2016. Row 1 reports the total number of provisional ballots given. The poll worker is required to specify the reason why the voter is casting a provisional ballot. Rows 2-6 report the number of provisional ballots by category. Rows 7-12 report provisional ballots as a percentage of all valid votes. Rows 13-18 report the percentage of provisional ballots accepted by the corresponding Local Board of Canvassers.

Table A.3: Fraction of People who Obtained a Driver's License after Photo ID Law

Presidential Elections - Population Without a License in 2012		
	% with License in 2016	% with License and Voted in 2016
People who voted in 2012	0.0637	0.0432
People who did not vote in 2012	0.0886	0.0187
Midterm Elections - Population Without a License in 2010		
	% with License in 2014	% with License and Voted in 2014
People who voted in 2010	0.0368	0.0233
Perople who did not vote in 2010	0.0901	0.0099

Notes: Table shows the percentage of people with a valid drivers license at the time of an election after the photo ID law was enacted, and the percentage of people who had a driver's license and voted in the same election. In each panel, the first row only includes people who voted without a license before the photo ID law was enacted, and the second row only includes people who did notv have a driver's license at the time of that election.

Table A.4: Characteristics of Voters across OLS and Fixed Effects Specifications

	Presidential Elections			
	OLS	Fixed Effects	Difference	P-value
Identifying Voters	1112731	830175	282556	-
% with Driver's License	0.755	0.789	-0.034	0.00
% Registered	0.704	0.727	-0.023	0.00
% Voted	0.446	0.481	-0.035	0.00
% Female	0.489	0.493	-0.004	0.00
% Minority	0.088	0.084	0.004	0.00
% Low SES	0.352	0.349	0.003	0.00
% Democrats	0.313	0.326	-0.013	0.00
% Republicans	0.092	0.096	-0.004	0.00
% Independents	0.595	0.578	0.017	0.00
Average Age	47.19	48.24	-1.05	0.00

Notes: We do not report means for the groups of Male, White, and High SES voters because their complements are already included in the table. We also report average age instead of percentages of people in each age group.

Table A.5: First Stage Results for IV Regressions in Presidential Elections

	(1) All	(2) Young	(3) 26-40	(4) 41-64	(5) Elderly	(6) Low SES	(7) High SES	(8) Minority	(9) White	(10) Female	(11) Male	(12) Dem	(13) Rep	(14) Indep
	Turnout (*Panel* A)													
IV Coeff	-0.027	-0.083	-0.065	-0.010	0.007	-0.057	-0.011	-0.052	-0.031	-0.028	-0.021	-0.011	-0.009	-0.034
	(0.002)	(0.013)	(0.003)	(0.003)	(0.004)	(0.003)	(0.002)	(0.007)	(0.004)	(0.003)	(0.003)	(0.004)	(0.007)	(0.002)
No License * Post	0.828	0.448	0.757	0.898	0.888	0.708	0.906	0.672	0.787	0.812	0.810	0.844	0.862	0.813
(1st Stage n.1)	(0.001)	(0.006)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)	(0.003)	(0.001)
F-statistic	131085	471	35338	136722	145502	18161	346137	6432	26275	45389	28323	73594	21919	65499
No License	0.920	0.869	0.911	0.957	0.928	0.877	0.958	0.873	0.907	0.920	0.928	0.921	0.934	0.917
(1st Stage n.2)	(0.001)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
F-statistic	438973	11054	120721	397314	228571	74003	688672	21330	101781	177719	105702	200565	67313	246030
	Registration (*Panel* B)													
IV Coeff	-0.076	-0.272	-0.150	-0.052	-0.049	-0.079	-0.090	-0.032	-0.078	-0.067	-0.065	-0.064	-0.096	-0.074
	(0.002)	(0.014)	(0.003)	(0.003)	(0.004)	(0.004)	(0.002)	(0.008)	(0.004)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)
No License * Post	0.828	0.447	0.757	0.898	0.892	0.708	0.906	0.672	0.787	0.812	0.810	0.843	0.862	0.813
(1st Stage n.1)	(0.001)	(0.006)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)	(0.003)	(0.001)
F-statistic	130816	473	37466	154691	137761	19229	346137	6343	29502	42954	28298	81726	25953	67476
No License	0.920	0.868	0.911	0.958	0.901	0.877	0.958	0.867	0.914	0.928	0.928	0.927	0.937	0.917
(1st Stage n.2)	(0.001)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
F-statistic	438705	11606	125089	441591	235005	78356	688672	21146	109767	165033	105373	216656	77224	251434
	Voting|Registration(*Panel* C)													
IV Coeff	0.001	-0.045	-0.029	0.014	0.021	-0.043	0.013	-0.036	-0.003	-0.002	0.010	0.008	0.024	-0.010
	(0.002)	(0.017)	(0.004)	(0.004)	(0.005)	(0.005)	(0.003)	(0.010)	(0.005)	(0.004)	(0.004)	(0.004)	(0.007)	(0.003)
No License * Post	0.844	0.463	0.779	0.909	0.884	0.705	0.900	0.680	0.795	0.831	0.829	0.845	0.864	0.838
(1st Stage n.1)	(0.001)	(0.007)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.004)	(0.002)	(0.001)	(0.002)	(0.001)	(0.003)	(0.001)
F-statistic	140260	404	44256	158066	90217	8807	324742	-	22619	40715	29400	70428	23388	66174
No License	0.925	0.854	0.899	0.961	0.931	0.868	0.952	0.872	0.915	0.926	0.931	0.924	0.935	0.924
(1st Stage n.2)	(0.001)	(0.003)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
F-statistic	378082	11465	85337	402964	161194	32864	564217	-	85058	148975	104216	178126	64903	184248

Notes: Each panel corresponds to one of our dependent variables and each column to a different subpopulation. In each panel, we first report the second stage coefficient on the interaction between $Post_t$ and $NoLicense_{it}$. This is α_3 in equation 1. Then, we report the main coefficient of interest in each of the first stage regressions. These are the coefficient on $Post_t \cdot NoLicense_{2011}$ when the dependent variable is $Post_t \cdot NoLicense_{it}$ (γ_2 in equation 2) and the coefficient on $NoLicense_{i,2011}$ when the dependent variable is $NoLicense_{it}$(δ_1 in equation 3). We also show the F statistics for all first stage equations. Some of them are missing because the robust var-cov matrix is not of full rank. Robust standard errors are reported in parenthesis.

Table A.6: First Stage Results for IV Regressions in Midterm Elections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	All	Young	26-40	41-64	Elderly	Low SES	High SES	Minority	White	Female	Male	Dem	Rep	Indep
	Turnout (*Panel* A)													
IV Coeff	0.003	0.026	0.004	0.016	0.020	-0.000	0.005	-0.025	0.009	0.009	0.008	0.005	0.026	0.003
	(0.002)	(0.005)	(0.002)	(0.003)	(0.004)	(0.002)	(0.002)	(0.004)	(0.003)	(0.002)	(0.003)	(0.003)	(0.006)	(0.002)
No License * Post	0.882	0.700	0.851	0.932	0.919	0.803	0.934	0.774	0.854	0.871	0.868	0.890	0.903	0.874
(1st Stage n.1)	(0.001)	(0.004)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
F-statistic	232001	2791	72304	242242	203351	26471	534013	-	46835	76663	43671	129577	36763	-
No License	0.935	0.918	0.928	0.954	0.924	0.927	0.944	0.935	0.932	0.937	0.935	0.934	0.922	0.937
(1st Stage n.2)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
F-statistic	702999	28691	210769	711097	305867	78015	1194187	-	128628	149249	88508	381105	90766	-
	Registration (*Panel* B)													
IV Coeff	-0.051	-0.195	-0.100	-0.021	-0.033	-0.045	-0.070	-0.023	-0.032	-0.043	-0.038	-0.052	-0.070	-0.047
	(0.002)	(0.008)	(0.003)	(0.003)	(0.003)	(0.004)	(0.002)	(0.007)	(0.004)	(0.003)	(0.003)	(0.002)	(0.004)	(0.002)
No License * Post	0.882	0.700	0.851	0.932	0.921	0.803	0.934	0.774	0.854	0.871	0.868	0.889	0.902	0.874
(1st Stage n.1)	(0.001)	(0.004)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
F-statistic	242983	2711	76547	263458	210397	30863	591910	-	41961	81429	44930	127347	48575	134851
No License	0.937	0.918	0.928	0.955	0.910	0.927	0.944	0.936	0.935	0.937	0.937	0.937	0.924	0.937
(1st Stage n.2)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
F-statistic	764852	28221	228151	771149	308301	90916	1313821	-	117609	158406	92817	372126	119925	413216
	Voting|Registration (*Panel* C)													
IV Coeff	0.041	0.067	0.058	0.038	0.030	0.044	0.037	0.009	0.042	0.045	0.052	0.032	0.055	0.046
	(0.002)	(0.007)	(0.003)	(0.004)	(0.005)	(0.005)	(0.002)	(0.008)	(0.005)	(0.003)	(0.003)	(0.003)	(0.007)	(0.003)
No License * Post	0.893	0.735	0.856	0.937	0.914	0.801	0.927	0.776	0.863	0.885	0.881	0.894	0.904	0.889
(1st Stage n.1)	(0.001)	(0.004)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.004)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
F-statistic	237590	3082	129061	223996	131865	19731	528816	-	34507	83236	58491	139324	48848	146786
No License	0.929	0.892	0.916	0.950	0.919	0.903	0.941	0.932	0.924	0.930	0.926	0.934	0.921	0.926
(1st Stage n.2)	(0.001)	(0.003)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
F-statistic	662207	31597	287037	597394	195080	43124	1124062	-	84842	138051	97380	413244	115154	395629

Notes: Each panel corresponds to one of our dependent variables and each column to a different subpopulation. In each panel, we first report the second stage coefficient on the interaction between $Post_t$ and $NoLicense_{it}$. This is α_3 in equation 1. Then, we report the main coefficient of interest in each of the first stage regressions. These are the coefficient on $Post_t \cdot NoLicense_{2011}$ when the dependent variable is $Post_t \cdot NoLicense_{it}$ (γ_2 in equation 2) and the coefficient on $NoLicense_{i,2011}$when the dependent variable is $NoLicense_{it}$(δ_1 in equation 3). We also show the F statistics for all first stage equations. Some of them are missing because the robust var-cov matrix is not of full rank. Robust standard errors are reported in parenthesis.

Table A.7: Estimated Impact of Photo ID Law Using Years 2006-2016 in Presidential Elections

	(1)	(2)	(3)
		IV	
	Turnout	Registration	Vote|Reg
All Voters	-0.045	-0.119	0.007
	(0.002)	(0.002)	(0.002)
Young (22-25)	-0.082	-0.366	-0.012
	(0.013)	(0.014)	(0.017)
Adult (26-40)	-0.068	-0.213	0.019
	(0.003)	(0.003)	(0.004)
Adult (41-64)	-0.019	-0.074	0.019
	(0.003)	(0.002)	(0.003)
Elderly (65+)	-0.009	-0.068	0.011
	(0.004)	(0.003)	(0.005)
Low SES	-0.053	-0.113	-0.013
	(0.003)	(0.004)	(0.005)
High SES	-0.042	-0.136	0.010
	(0.002)	(0.001)	(0.002)
Minority	-0.064	-0.073	-0.013
	(0.006)	(0.007)	(0.009)
White	-0.025	-0.109	0.030
	(0.004)	(0.004)	(0.005)
Female	-0.039	-0.105	0.008
	(0.003)	(0.002)	(0.003)
Male	-0.035	-0.112	0.027
	(0.003)	(0.003)	(0.004)
Democratics	-0.035	-0.094	-0.001
	(0.003)	(0.002)	(0.003)
Republicans	-0.024	-0.132	0.026
	(0.007)	(0.003)	(0.007)
Independents	-0.043	-0.121	0.012
	(0.002)	(0.002)	(0.003)

Notes: The table reports the α_3 coefficient in equation 1 for the IV specification by voters subgroups in presidential elections. The dependent variable is either being registered or voting. Controls are selected using Double LASSO. Robust standard errors are reported in parenthesis.

Table A.8: Estimated Impact of Photo ID Law Using Years 2006-2016 in Midterm Elections

	(1)	(2)	(3)
		IV	
	Turnout	Registration	Vote|Reg
All Voters	-0.001	-0.102	0.071
	(0.002)	(0.002)	(0.002)
Young (22-25)	0.023	-0.307	0.122
	(0.005)	(0.007)	(0.007)
Adult (26-40)	0.007	-0.171	0.124
	(0.002)	(0.003)	(0.003)
Adult (41-64)	0.009	-0.052	0.052
	(0.003)	(0.002)	(0.003)
Elderly (65+)	0.001	-0.049	0.016
	(0.004)	(0.003)	(0.004)
Low SES	0.006	-0.082	0.079
	(0.002)	(0.003)	(0.004)
High SES	-0.007	-0.125	0.064
	(0.002)	(0.002)	(0.002)
Minority	-0.025	-0.073	0.050
	(0.004)	(0.006)	(0.008)
White	0.021	-0.075	0.090
	(0.003)	(0.003)	(0.004)
Female	0.010	-0.088	0.078
	(0.002)	(0.002)	(0.003)
Male	0.006	-0.096	0.092
	(0.002)	(0.003)	(0.003)
Democratics	-0.005	-0.088	0.046
	(0.003)	(0.002)	(0.003)
Republicans	0.018	-0.114	0.075
	(0.006)	(0.003)	(0.006)
Independents	0.007	-0.100	0.094
	(0.002)	(0.002)	(0.003)

Notes: The table reports the α_3 coefficient in equation 1 for the IV specification by voters subgroups in midterm elections. The dependent variable is either being registered or voting. Controls are selected using Double LASSO. Robust standard errors are reported in parenthesis.

Table A.9: Clustering Standard Errors

	(1)	(2)
	IV	
SE:	Robust	Clustered
All Voters	0.001	0.001
	(0.002)	(0.004)
Young (22-25)	-0.045	-0.045
	(0.017)	(0.019)
Adult (26-40)	-0.029	-0.029
	(0.004)	(0.006)
Adult (41-64)	0.014	0.014
	(0.004)	(0.004)
Elderly (65+)	0.021	0.021
	(0.005)	(0.005)
Low Income	-0.043	-0.043
	(0.005)	(0.006)
High Income	0.013	0.013
	(0.003)	(0.004)
Minority	-0.036	-0.036
	(0.010)	(0.010)
White	-0.003	-0.003
	(0.005)	(0.005)
Female	-0.002	-0.002
	(0.004)	(0.004)
Male	0.010	0.010
	(0.004)	(0.004)
Democrats	0.008	0.008
	(0.004)	(0.005)
Republicans	0.024	0.024
	(0.007)	(0.007)
Independents	-0.010	-0.010
	(0.003)	(0.004)

Notes: The table reports the α_3 coefficient in equation 1 for different model specifications in presidential and midterm elections. The dependent variable is voting conditional on registration. Column 1 shows robust standard errors while column 2 shows standard errors clustered at the precinct by driver's license status level. Hence, the number of clusters is equal to twice the number of precincts.

Table A.10: Estimated Impact of the Photo ID Law Using CPS Data

	(1) Rhode Island	(2) Connecticut and Mass.
	Presidential Elections	
Turnout	-0.048	-0.052
	(0.049)	(0.032)
Registration	-0.051	-0.050
	(0.041)	(0.028)
Voted$\vert$Reg	-0.009	-0.007
	(0.040)	(0.024)
	Midterm Elections	
Turnout	-0.026	0.033
	(0.054)	(0.037)
Registration	-0.033	0.044
	(0.047)	(0.033)
Voted$\vert$Reg	-0.026	-0.003
	(0.059)	(0.040)

Notes: The table reports the α_3 coefficient in equation 1 using CPS data. The dependent variable is either being registered or voting. Column 1 includes only Rhode Island and column 2 pools Connecticut and Massachusetts. Robust standard errors are reported in parenthesis.

Table A.11: Estimated Impact of the Photo ID Law Using Out Of State Data

	(1) Actual Driver's License Indicator	(2) Predicted Driver's License	(3) Predicted Driver's License Indicator
	Presidential Elections		
Rhode Island	-0.071	-0.209	-0.051
	(0.003)	(0.015)	(0.003)
Allegheny County (PA)	-	-0.013	-0.012
	-	(0.006)	(0.001)

Notes: The table reports the α_3 coefficient in equation 1 using Rhode Island and Allegheny County (Pennsylvania) data. We restrict the analysis to the subsample of voters for which we know their race since this is a main predictor for driver's license. The dependent variable is voting conditional on registration. Column 1 uses the actual driver's license indicator. Column 2 uses the predicted driver's license variable when including predictors selected by LASSO. Column 3 uses instead an optimal threshold to convert the predicted driver's license variable in a predicted driver's license indicator. The threshold is selected using ROC analysis. All controls in equation 1 are also selected by LASSO and include individual demograohics, party affiliation, and several block group variables as well as their interactions. Robust standard errors are reported in parenthesis.

Table A.12: Robustness Checks Using Additional Specifications in Presidential Elections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	OLS			Propensity Score with LASSO			Propensity Score with Logit		
	Turnout	Reg	Vote|Reg	Turnout	Reg	Vote|Reg	Turnout	Reg	Vote|Reg
All Voters	-0.047	-0.080	-0.017	-0.060	-0.087	-0.014	-0.048	-0.086	-0.021
	(0.001)	(0.001)	(0.002)	(0.009)	(0.008)	(0.008)	(0.004)	(0.004)	(0.004)
Young (22-25)	-0.045	-0.136	-0.025	-0.051	-0.094	-0.058	-0.052	-0.120	-0.062
	(0.005)	(0.005)	(0.006)	(0.010)	(0.009)	(0.018)	(0.009)	(0.009)	(0.013)
Adult (26-40)	-0.056	-0.130	-0.032	-0.038	-0.124	-0.047	-0.047	-0.124	-0.032
	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)	(0.012)	(0.006)	(0.006)	(0.008)
Adult (41-64)	-0.042	-0.072	-0.012	-0.047	-0.074	-0.005	-0.042	-0.080	-0.008
	(0.002)	(0.002)	(0.003)	(0.013)	(0.013)	(0.006)	(0.006)	(0.005)	(0.005)
Elderly (65+)	-0.029	-0.040	-0.010	-0.030	-0.042	-0.009	-0.031	-0.046	-0.017
	(0.003)	(0.003)	(0.004)	(0.006)	(0.005)	(0.005)	(0.011)	(0.009)	(0.009)
Low SES	-0.039	-0.046	-0.043	-0.031	-0.034	-0.034	-0.027	-0.038	-0.038
	(0.002)	(0.003)	(0.004)	(0.002)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)
High SES	-0.054	-0.101	-0.015	-0.075	-0.134	0.003	-0.068	-0.120	-0.032
	(0.002)	(0.002)	(0.002)	(0.011)	(0.010)	(0.016)	(0.007)	(0.006)	(0.006)
Minority	-0.037	-0.004	-0.026	-0.037	-0.003	-0.027	-0.035	0.005	-0.038
	(0.004)	(0.005)	(0.007)	(0.004)	(0.005)	(0.007)	(0.005)	(0.005)	(0.007)
White	-0.053	-0.061	-0.027	-0.058	-0.060	-0.033	-0.058	-0.061	-0.039
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)
Female	-0.042	-0.052	-0.023	-0.041	-0.049	-0.028	-0.044	-0.062	-0.026
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
Male	-0.048	-0.053	-0.020	-0.051	-0.057	-0.027	-0.049	-0.064	-0.022
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
Democratic	-0.057	-0.049	-0.023	-0.044	-0.049	-0.016	-0.058	-0.059	-0.022
	(0.003)	(0.001)	(0.003)	(0.013)	(0.004)	(0.009)	(0.006)	(0.003)	(0.006)
Republicans	-0.056	-0.080	-0.007	-0.048	-0.093	-0.009	-0.062	-0.089	-0.014
	(0.005)	(0.003)	(0.005)	(0.012)	(0.018)	(0.010)	(0.010)	(0.007)	(0.010)
Independents	-0.036	-0.082	-0.012	-0.037	-0.078	-0.019	-0.032	-0.079	-0.018
	(0.002)	(0.002)	(0.002)	(0.009)	(0.009)	(0.011)	(0.006)	(0.006)	(0.007)

Notes: The table reports the α_3 coefficient in equation 1 for different model specifications by voter subgroups in presidential elections. In columns 1-3 we run OLS regressions. In columns 4-9 we use a propensity score reweighting. While in columns 4-6 we predict probabilities of having a driver's license using LASSO, in columns 7-9 we use logit. Since logit does not convergence for all subgrops of voters, in columns 7-9 we reduced the number of variables included considering only levels of demographics instead of their interactions as well. The weights are equal to 1 for voters without a driver's license and $\frac{\hat{p}}{1-\hat{p}}$ for voters with a driver's license. Controls are selected using Double LASSO. Robust standard errors are reported in parenthesis.

Table A.13: Robustness Checks Using Additional Specifications in Midterm Elections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		OLS			Propensity Score with LASSO			Propensity Score with Logit	
	Turnout	Reg	Vote|Reg	Turnout	Reg	Vote|Reg	Turnout	Reg	Vote|Reg
All Voters	0.005	-0.055	0.038	-0.014	-0.049	0.043	-0.006	-0.065	0.027
	(0.001)	(0.001)	(0.002)	(0.006)	(0.004)	(0.004)	(0.005)	(0.004)	(0.007)
Young (22-25)	0.006	-0.125	0.028	-0.012	-0.074	0.056	0.015	-0.119	0.039
	(0.003)	(0.005)	(0.005)	(0.024)	(0.023)	(0.014)	(0.008)	(0.010)	(0.014)
Adult (26-40)	-0.000	-0.091	0.040	-0.021	-0.070	0.014	-0.003	-0.082	0.021
	(0.002)	(0.003)	(0.003)	(0.011)	(0.011)	(0.019)	(0.006)	(0.006)	(0.009)
Adult (41-64)	0.004	-0.048	0.032	0.012	-0.023	0.026	-0.004	-0.058	0.032
	(0.002)	(0.002)	(0.003)	(0.017)	(0.017)	(0.008)	(0.006)	(0.005)	(0.006)
Elderly (65+)	-0.002	-0.023	0.005	0.002	-0.022	0.005	0.010	-0.031	0.012
	(0.003)	(0.003)	(0.004)	(0.006)	(0.004)	(0.006)	(0.014)	(0.011)	(0.012)
Low SES	0.011	-0.022	0.036	0.015	-0.013	0.035	0.017	-0.019	0.023
	(0.002)	(0.003)	(0.004)	(0.002)	(0.003)	(0.004)	(0.002)	(0.003)	(0.004)
High SES	-0.004	-0.070	0.030	-0.020	-0.074	0.025	-0.025	-0.082	0.011
	(0.002)	(0.002)	(0.002)	(0.007)	(0.007)	(0.008)	(0.007)	(0.006)	(0.007)
Minority	-0.007	-0.007	0.030	-0.008	-0.011	0.032	-0.008	0.000	0.021
	(0.003)	(0.005)	(0.006)	(0.003)	(0.005)	(0.007)	(0.004)	(0.006)	(0.008)
White	0.010	-0.027	0.044	0.002	-0.026	0.033	0.000	-0.034	0.028
	(0.002)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.006)
Female	0.015	-0.033	0.041	0.009	-0.037	0.033	-0.002	-0.048	0.023
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)
Male	0.004	-0.035	0.039	-0.003	-0.042	0.031	-0.008	-0.050	0.030
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)
Democratic	-0.007	-0.042	0.022	-0.012	-0.034	0.013	-0.018	-0.046	0.012
	(0.003)	(0.002)	(0.003)	(0.008)	(0.005)	(0.007)	(0.007)	(0.004)	(0.008)
Republicans	0.005	-0.064	0.039	-0.000	-0.065	0.023	-0.018	-0.069	0.024
	(0.005)	(0.003)	(0.005)	(0.035)	(0.012)	(0.011)	(0.013)	(0.010)	(0.014)
Independents	0.012	-0.056	0.052	0.002	-0.056	0.043	0.007	-0.060	0.049
	(0.001)	(0.002)	(0.002)	(0.004)	(0.004)	(0.043)	(0.007)	(0.007)	(0.013)

Notes: The table reports the α_3 coefficient in equation 1 for different model specifications by voter subgroups in midterm elections. In columns 1-3 we run OLS regressions. In columns 4-9 we use a propensity score reweighting. While in columns 4-6 we predict probabilities of having a driver's license using LASSO, in columns 7-9 we use logit. Since logit does not convergence for all subgrops of voters, in columns 7-9 we reduced the number of variables included considering only levels of demographics instead of their interactions as well. The weights are equal to 1 for voters without a driver's license and $\frac{\hat{p}}{1-\hat{p}}$ for voters with a driver's license. Controls are selected using Double LASSO. Robust standard errors are reported in parenthesis.

Table A.14: Estimated Impact of the Photo ID Law on Predicted Voting by Subgroups Presidential Elections

	(1)	(2)	(3)
		IV	
Dep. Var.:	Turnout	Registration	Vote|Reg
Young (22-25)	-0.055	-0.063	-0.031
	(0.002)	(0.003)	(0.002)
Adult (26-40)	-0.002	-0.006	-0.003
	(0.000)	(0.001)	(0.000)
Adult (41-64)	0.002	0.004	0.001
	(0.001)	(0.001)	(0.000)
Elderly (65+)	0.007	0.006	0.010
	(0.001)	(0.001)	(0.001)
Low SES	0.006	0.015	-0.001
	(0.000)	(0.000)	(0.000)
High SES	-0.000	-0.002	0.001
	(0.000)	(0.000)	(0.000)
Minority	0.002	0.007	0.003
	(0.000)	(0.001)	(0.000)
White	-0.003	0.004	-0.009
	(0.001)	(0.001)	(0.001)
Female	-0.002	0.001	-0.002
	(0.001)	(0.001)	(0.000)
Male	-0.004	-0.001	-0.003
	(0.001)	(0.001)	(0.000)
Democratics	0.003	0.004	0.000
	(0.000)	(0.000)	(0.000)
Republicans	0.001	0.001	-0.001
	(0.001)	(0.000)	(0.001)
Independents	-0.004	-0.003	-0.002
	(0.000)	(0.001)	(0.000)

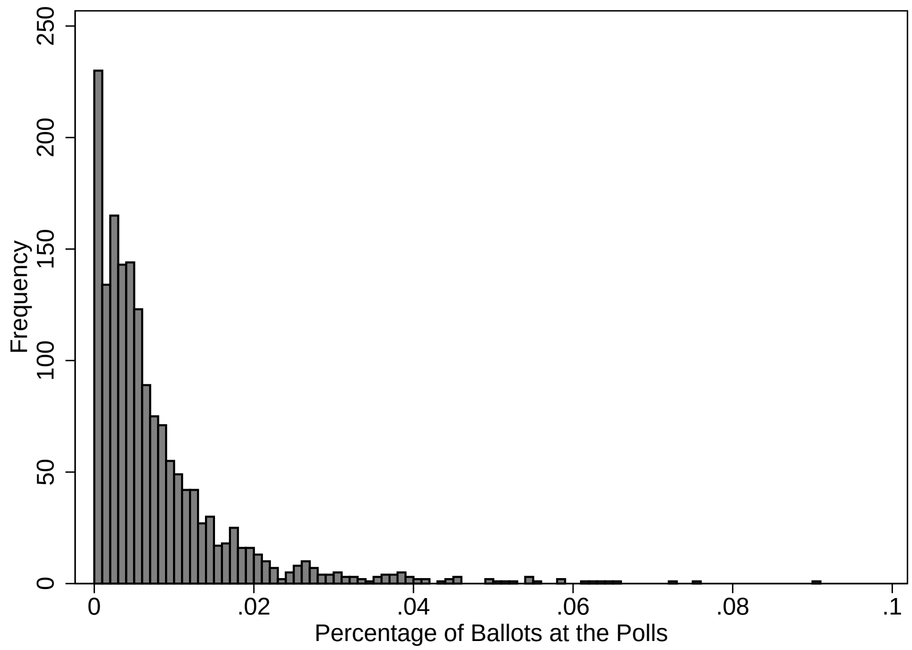
Notes: The depdendent variable is the predicted probability of voting or registering to vote. We report the α_3 coefficient in equation 1 for the IV specification by voters subgroups in presidential elections. Controls are selected using Double LASSO. Robust standard errors are reported in parenthesis.

Table A.15: Estimated Impact of the Photo ID Law on Predicted Voting by Subgroups in Midterm Elections

	(1)	(2)	(3)
		IV	
Dep. Var.:	Turnout	Registration	Vote|Reg
Young (22-25)	-0.006	-0.030	0.001
	(0.001)	(0.002)	(0.001)
Adult (26-40)	0.000	0.001	-0.000
	(0.000)	(0.000)	(0.000)
Adult (41-64)	-0.003	0.003	-0.005
	(0.001)	(0.001)	(0.000)
Elderly (65+)	-0.005	-0.007	0.002
	(0.001)	(0.001)	(0.001)
Low SES	-0.006	-0.003	-0.005
	(0.000)	(0.000)	(0.000)
High SES	-0.000	-0.002	0.002
	(0.000)	(0.000)	(0.000)
Minority	-0.001	0.014	-0.009
	(0.000)	(0.001)	(0.000)
White	-0.011	-0.008	-0.011
	(0.001)	(0.001)	(0.001)
Female	-0.007	-0.004	-0.004
	(0.000)	(0.000)	(0.000)
Male	-0.002	0.000	-0.001
	(0.000)	(0.001)	(0.000)
Democratics	0.001	0.003	0.000
	(0.000)	(0.000)	(0.000)
Republicans	0.000	-0.000	-0.001
	(0.001)	(0.000)	(0.001)
Independents	-0.005	-0.002	-0.002
	(0.000)	(0.001)	(0.000)

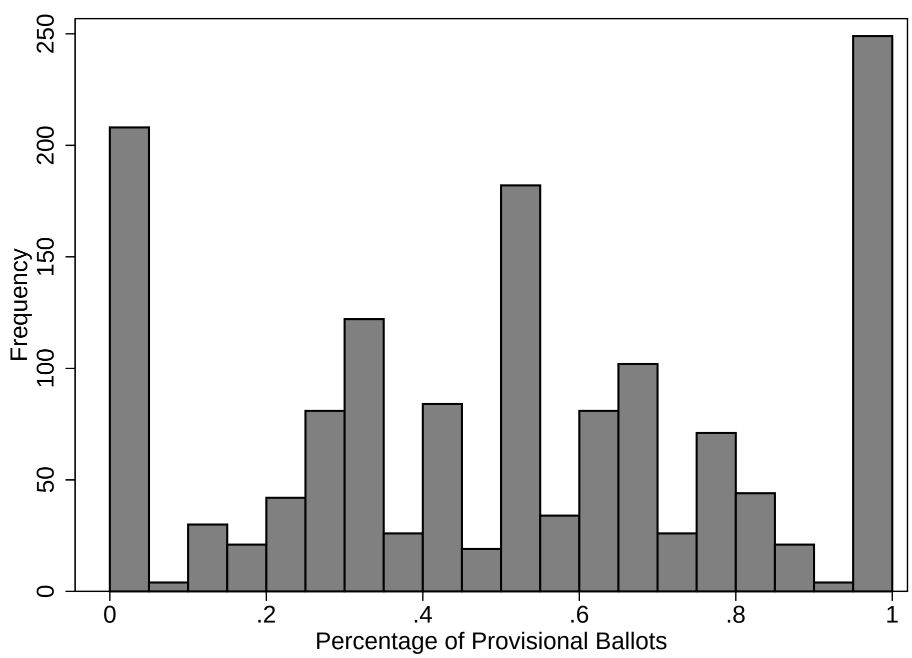
Notes: The depdendent variable is the predicted probability of voting or registering to vote. We report the α_3 coefficient in equation 1 for the IV specification by voters subgroups in midterm elections. Controls are selected using Double LASSO. Robust standard errors are reported in parenthesis.

Figure A.5: Provisional Ballots Given by Precinct



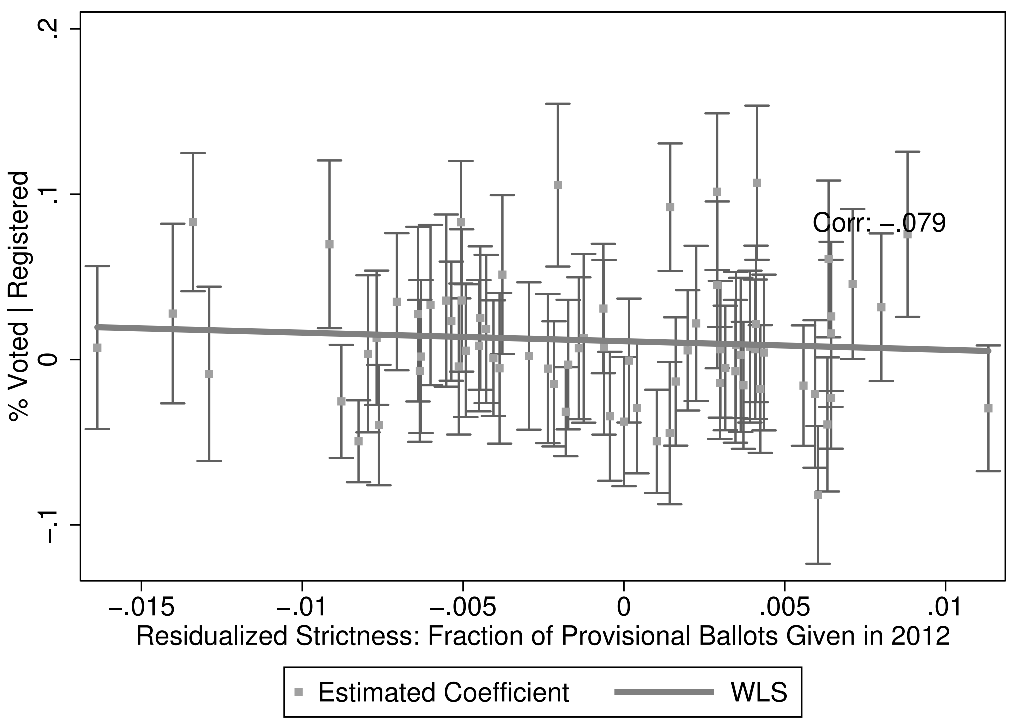
Notes: This graph shows the distribution by precinct of the fraction of provisional ballots given. We drop 5 precincts where the fraction of provisional ballots is higher than 10%. The bin width is 0.1%.

Figure A.6: Provisional Ballots Accepted by Precinct



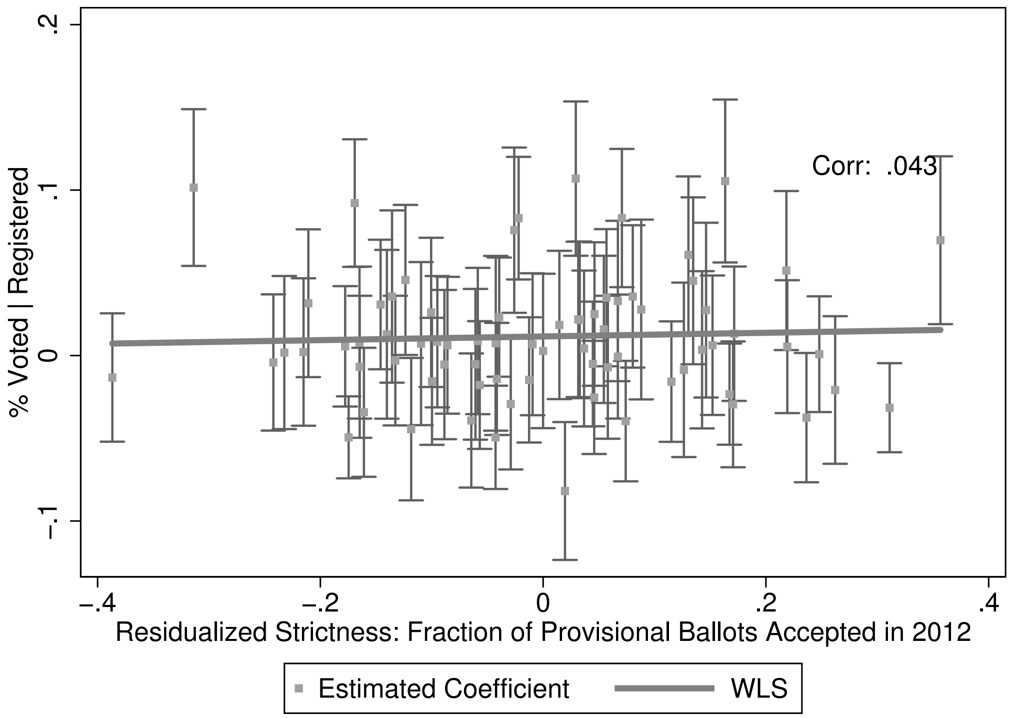
Notes: This graph shows the distribution by precinct of the fraction of provisional ballots accepted. We drop 5 precincts where the fraction of provisional ballots is higher than 10%. The bin width is 5%.

Figure A.7: Correlation Between Impact of Photo ID Law and Residualized Provisional Ballots Given
by Congressional District



Notes: This graph shows the correlation (weighted by district population) between residualized district strictness and the estimated effect of the law at the district level. The coefficients plotted correspond to α_3 in equation 1. We run IV regressions by district with controls selected by Double LASSO. In all these regressions the dependent variable is voting conditional on registration. This graph drops 1 case for which the fraction of provisional ballots given is higher than 5%. The number of districts is 75.

Figure A.8: Correlation Between Impact of Photo ID Law and Residualized Provisional Ballots Accepted by Congressional District



Notes: This graph shows the correlation (weighted by district population) between residualized district strictness and the estimated effect of the law at the district level. The coefficients plotted correspond to α_3 in equation 1. We run IV regressions by district with controls selected by Double LASSO. In all these regressions the dependent variable is voting conditional on registration. This graph drops 1 case for which the fraction of provisional ballots given is higher than 5%. The number of districts is 75.

Table A.16: Predicted and Average Voting Conditional on Registration by Party Affiliation

		(1)	(2)	(3)	(4)	(5)	(6)
		Registered Democrats		Registered Republicans		Registered Independents	
		Predicted	Actual	Predicted	Actual	Predicted	Actual
Districts where:				2016 Election (*Panel A*)			
Representative Voted	Yes	0.628	0.638	0.673	0.676	0.531	0.543
	No	0.624	0.634	0.690	0.689	0.538	0.548
Senator Voted	Yes	0.645	0.652	0.691	0.693	0.553	0.563
	No	0.581	0.600	0.614	0.619	0.466	0.484
All		0.627	0.637	0.677	0.680	0.533	0.544
				2014 Election (*Panel B*)			
Representative Voted	Yes	0.486	0.485	0.533	0.528	0.382	0.382
	No	0.489	0.488	0.561	0.553	0.402	0.402
Senator Voted	Yes	0.503	0.502	0.556	0.550	0.407	0.405
	No	0.445	0.447	0.471	0.466	0.325	0.328
All		0.486	0.486	0.541	0.535	0.388	0.388

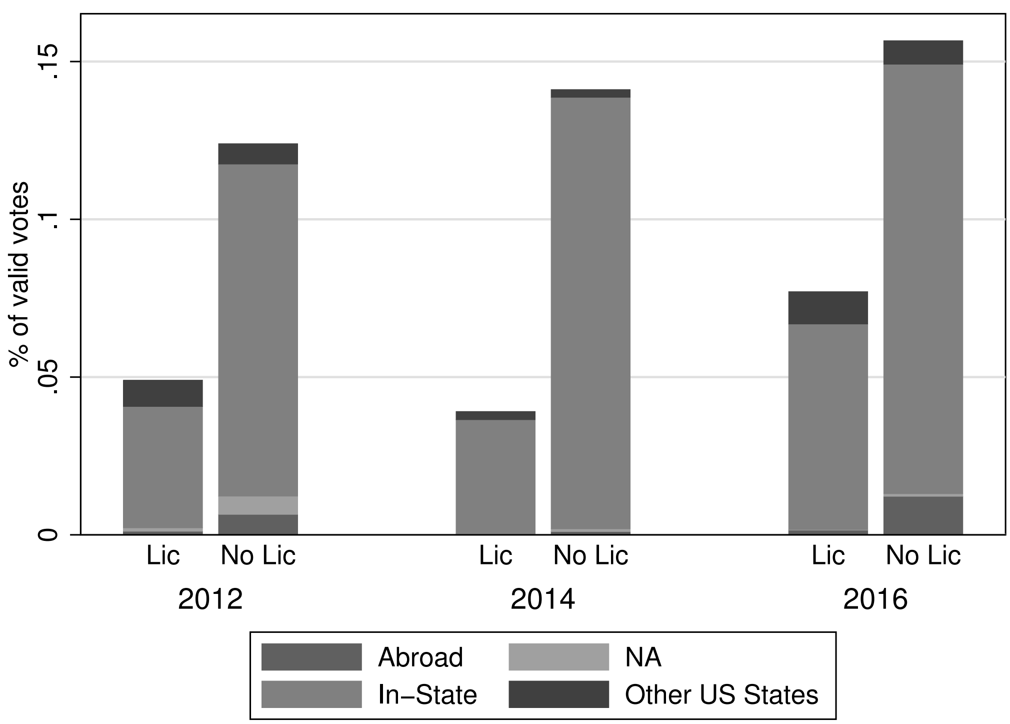
Notes: The table reports predicted and actual voting conditional on registration. Rows split the sample according to whether state representatives or senators voted for or against the photo ID law. Predictions come from coefficients estimated with IV regressions. The counterfactual is the predicted value when $\hat{\alpha}_3$ is set to zero. Panel A shows the counterfactual results for 2016 (presidential elections). Panel B shows the counterfactual results for 2014 (midterm elections). Controls are selected using Double LASSO.

Table A.17: Mail Ballots by Election Year, Address and Category

		Election Year		
		2012	2014	2016
(1)	Total Forms Sent	28837	18878	45062
(2)	Total Ballots Received	26081	16571	41911
(3)	Emergency Ballots	5253	4685	14520
(4)	From In-State	20952	15547	35497
(5)	From Other States	3697	894	4945
(6)	From Abroad	786	96	1345
(7)	From Impaired Voters	4850	3249	6237
(8)	From Confined Voters	2429	1850	2824
(9)	From Absent Voters	1317	189	1917
(10)	No Excuse	17485	11283	30933

Notes: We report the number of mail votes by election year, by address and by category. The columns refer respectively to the three general elections held from 2012 to 2016. Row 1 is the total number of mail ballots requested to the Board of Election. Row 2 is the number of mail ballots casted by voters. Row 3 are mail ballots requested within 20 days of the election. Rows 4-6 are respectively mail ballots sent to either in-state addresses, other US states, or abroad. We exclude from rows 4-6 124 mail ballots for which address data are inconsistent or incomplete. Rows 7-10 classify mail ballots by the reason given by the voter who requested it. In 7 the voter is incapacitated or impaired to go to the polls; in 8 the voter is confined to a health care institution; in 9 the voter is absent due to employment or military service; and in 10 the voter is unable to go to the polls on election day without further excuse.

Figure A.9: Mail Ballots by Year, Address and License Status



B Online Appendix: Controls Selected by LASSO

In this paper, we use the Least Absolute Shrinkage and Selection Operator (LASSO) to deal with the high dimensionality of our data. LASSO is a regression method that selects variables by penalizing the size of their coefficients. LASSO is a systematic method to select variables that provide the strongest predictive fit by shrinking the coefficients on weak explanatory variable coefficients towards zero (Tibshirani 1996; Hastie et al. 2009; Belloni et al. 2014). In all our specifications, LASSO picks from a set that includes the following: a third degree polynomial in age, dummies on sex, race, incarceration, and participation in social programs (e.g. SNAP, TANF, Medicaid, SSI, CCAP and GPA), and all possible interaction terms.

The set of controls selected by Double LASSO for the estimation of Table 2 with turnout as the outcome variable are:

- Age * Incarcerated
- Age * Hispanic
- Age * Race Null
- Age * Black
- Incarcerated
- Low SES
- Age * Low SES
- Female * Race Other
- Black * Low SES
- Hispanic * Low SES
- Male
- Male * Black
- Low SES * Incarcerated
- Male * Race Null
- Male * Low SES
- Race Null * Low SES
- Race Other
- Male * Hispanic
- Race Other * Low SES
- Age
- Age * Female
- Age * Male
- Black
- Low SES
- Female
- Female * Hispanic
- Hispanic
- Race Null