The Impact of Voter ID Laws on Turnout: Evidence from Texas

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Abstract

Debate continues over the advantages and drawbacks of voter ID laws. In this paper, I consider the impact of one of the strictest such laws, enacted just prior to the 2014 election in Texas. To identify effects, I use a difference-in-differences approach comparing turnout for people with and without permissible photo IDs over time. I do so using individual-level data matching voter history and state-issued IDs, accounting for ID issue date. Results show that registered voters who had not been issued a photo ID were no more or less likely to vote in the 2014 midterm election. In the 2016 presidential election, voters lacking ID were 7.6 percentage points less likely to turn out. I find no evidence of differential effects for minority groups.

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1 Introduction

Voting is fundamental to the democratic process, with elections directly affecting representation and policy. Ideally, election outcomes are accurate and fair, representing the preferences of the voting population with all individuals having equal access and ability to participate. However, voter turnout in the U.S. consistently ranks below other countries in the OECD as a proportion of the eligible population. Major policies like the Motor Voter Act and the Help America Vote Act have tried to address this by increasing access to voter registration or improving election administration and accessibility.

Recently, a number of states have enacted voter ID laws requiring individuals to present certain kinds of ID as proof of identification in order to cast a ballot. While proponents of such laws claim they boost election accuracy by reducing fraud, there are eligible citizens who may lack the necessary documentation to easily vote. In a simple economic decision-making model, this represents an increased cost on participation. Furthermore, some argue that these laws are discriminatory in nature, with a disparate impact on young and minority voters.

In this paper, I explore the impact of voter ID laws on turnout in the context of Texas 2011 S.B. 14 which at the time was one of the strictest such laws in the nation. To identify effects, I compare turnout of registered voters with and without permissible IDs over time using a difference-in-differences approach. The benefit of this design is that it allows me to distinguish the effects of the Texas law from other time-varying factors. I do so with a novel dataset matching individual-level voting history 2000-2016 to administrative records of state-issued driver licenses and identification cards. This data allows me to focus on the small subpopulation lacking ID rather than estimating effects in aggregate.

Although previous papers have studied how voter ID laws affect turnout, most of these suffer from data or methodological shortcomings such as self-reported turnout data, pre-post comparisons, or estimating state-level effects.[1] This literature largely finds a negative

[1]Self-reported turnout data has been found to be differentially biased, pre-post comparisons cannot account

impact on turnout in general but mixed results for subgroups of the population (e.g. G.A.O., 2015; Grimmer et al., 2018; Hajnal et al., 2017; Hood and Bullock, 2012). In a recent study, Cantoni and Pons (2019) compare turnout across states using individual-level voter history records and identify effects based on the timing of voter ID law changes at the state level. By measuring the impact of turnout in affected states relative to those without ID requirements, the advantage of this approach is that it can pick up suppressive effects of such laws that potentially affect even those individuals who do have proper ID. They find that photo ID laws negatively impact registered voters by 1.4 percentage points, but this estimate is not statistically significant. However, you would expect these laws to affect only the small subpopulation lacking proper ID, so the issue may be difficult to evaluate at the state level.

In the paper most similar to mine, Esposito et al. (2019) are better able to focus on this subpopulation of voters lacking proper identification. The authors use the 2011 voter ID law in Rhode Island and identify treatment effects by comparing voters with and without a state driver license before and after the law, conditional on observables. They estimate a decrease in turnout for registered voters without a photo ID of 0.9 percentage points in the 2014 midterm election with a stronger decrease of 5.0 percentage points in the 2016 presidential election. However, it is difficult to evaluate this paper since there is no clear evidence of the common trends identifying assumption of the differences-in-differences framework, so there are lingering concerns that the treatment and control groups in this context may be trending differently in a way that could bias estimated effects.

Another recent paper also compares voters with and without ID in the context of a 2016 North Carolina voter ID law (Grimmer and Yoder, 2020). The authors find that the law resulted in a 1 percentage point decrease in turnout in the primary election for voters lacking ID, and a 2.6 p.p. decrease in the general election in the same year. Importantly, the law was suspended and not actually in effect for the general election, which suggests that voters lack current information. However, since their data only identifies voters that currently have

for other time-varying factors, especially since elections are infrequent events, and state-level effects only pick up results that can be estimated in aggregate.

driver licenses and therefore after the voter ID law was passed, there is some concern that treatment is endogenous.

The voter ID literature asks an important question but offers no clear causal answer. I contribute to existing work by measuring the impact of voter ID laws between individuals with and without ID in the relatively strict Texas context. This is where an impact on turnout seems most likely, and also utilizes a population that is large and diverse. I present evidence of the identifying assumption for the difference-in-differences framework and demonstrate my results are robust to alternative specifications.

There are several reasons why the Texas case is an ideal context to evaluate the impact of voter ID laws. At the time it was passed in 2011, Texas S.B. 14 was one of the strictest voter ID laws in the country, requiring voters to present one of only seven permissible forms of photo identification to be able to vote. These included a state driver license, state identification card, state election identification certificate, state handgun license, federal military photo ID, federal passport, or federal photo citizenship certificate. Importantly, the absentee voting rules in Texas are relatively strict and did not change as the result of S.B. 14, and Texas does not otherwise allow voting by mail - limiting alternative methods that do not require voters to present ID. After passage, the Texas law was held up in the court system until just three weeks prior to the 2014 election, when the law first went into legal effect with little prior warning. After further court challenges, the law was relaxed by court order about two months prior to the 2016 election.[2] Additionally, as of the 2010 Census, Texas has a population of 25 million, which is 11.8 percent black and 37.6 percent Hispanic. The timing and strictness of the law and the demographics of the population in Texas make it an almost ideal context to look for the impact of voter ID laws.

I study the impact of the Texas voter ID law on turnout at the individual level using a unique dataset with voting history as well as information on which individuals do and do not have permissible ID. I created this dataset by first obtaining data on registered voters

[2]As of 2016, the law allows individuals without a prerequisite photo ID to instead sign a "Reasonable Impediment Declaration" and present alternative proof of identity.

and their voting history from the Texas Secretary of State and matching it to data from the Texas Department of Public Safety (DPS) which lists all individuals that have ever been issued a Texas driver license or identification card as well as when the ID was first obtained. Using the merged data, I then compare a treatment group of individuals without state-issued photo ID to a control group of those who should not have been affected by the Texas law. I compare these groups before and after the law over the period from 2000 to 2016. I estimate effects for each post-treatment year (2014 and 2016) separately, since different types of voters may participate in midterm versus presidential elections.

Results show that registered voters who had not been issued a photo ID were no more or less likely to vote in the 2014 midterm election. However, in the 2016 presidential election, this group of voters was 7.6 percentage points less likely to turn out. These results are robust to a variety of alternative model specifications. Since voting also affects other outcomes like representation, government funding, and policy, these findings carry great economic significance.

2 Background

The surge of state voter ID laws can be traced back to the Help America Vote Act of 2002 (HAVA), passed following the controversial ballot issues surrounding the presidential election of 2000. Among other more primary objectives,[3] HAVA required voters who had not previously done so to present identification in order to vote in a federal election. Many states have individually imposed stricter ID requirements than the HAVA minimums, sometimes asking voters to present a photo ID in order to cast a ballot in every election.

As of the 2018 federal election, seventeen states require a photo ID to vote, and many of these laws have already been through the court system. One early such law was passed in Indiana and immediately challenged in court. However, the U.S. Supreme Court upheld

[3](1) Replacing punchcard voting methods that were problematic in the 2000 election, (2) establishing the Election Assistance Commission, and (3) standardizing administration requirements for elections across states.

the constitutionality of that law in 2008. Following this ruling, more states began passing stricter photo ID requirements. Other states have had their own voter ID laws challenged with mixed results. Another important legal change was when the Supreme Court suspended a part of the 1965 Voting Rights Act which mandated that certain jurisdictions with a history of racial prejudice in voting receive preclearance by the federal government before they are allowed to change their election procedures.

In the Texas case, a strict voter ID law was passed in May 2011, challenged and temporarily suspended in court, but then cleared on October 14, 2014, just three weeks before the general election on November 4 of that same year. The law in its original form was one of the strictest in the nation, requiring voters to show one of seven permissible forms of ID at the polls to have their vote counted. These included a driver license, identification card, handgun license, election identification certificate, U.S. passport, citizenship certificate, or military ID. Without one of these, a person could not vote in Texas in the November 2014 election. There were only two exceptions. The first, absentee voting, requires a voter to be actually absent from their registered county on the date of the election (and during the entire period of early voting) and the absentee ballot must be requested no later than nine days in advance of the election. The second, a provisional ballot, allows a voter to participate in the election but the ballot is only counted if one of the seven permissible forms of ID is presented in person within two weeks of the election. The law was relaxed by court order about two months prior to the 2016 election and these changes have since been signed into law. In 2016, voters without one of the prerequisite forms of photo ID could sign a "Reasonable Impediment Declaration" and present alternative proof of identity. In doing so, the voter must attest to the specific reason they are impeded from procuring a photo ID and does so under penalty of prosecution for perjury.

3 Data

The data to compare voting turnout for individuals with and without permissible photo ID for this project comes from two separate sources. First is the voter history file from the Texas Secretary of State Elections Division, which contains a snapshot of all voters registered to vote in Texas as of August 2018 as well as their voting history. Second is a list all individuals who have ever been issued a photo ID from the Texas Department of Public Safety (DPS). This is a snapshot from December 2018 and includes all current and expired driver licenses or identification cards, which represent two forms of photo ID allowed under the Texas law.[4]

The voter history data contains each individual's name, current address, date of birth, gender, registration date, county, precinct, and voter status as well as a Hispanic surname flag. It also includes historical data for turnout and voting method for each federal election, i.e. the even year general elections from 1996 through 2016. Voting method details whether an individual voted on election day, during early voting, by absentee ballot, or provisionally. The outcome of interest for this study is whether an individual voted or not which I define as a simple binary variable equal to 1 if an individual voted in a given election by any method and a 0 if they did not.

The state ID data contains similar variables for name, current address, and date of birth so that individuals can be matched with their voting record. The data also contains variables indicating the type of ID (driver license or identification card) as well as when it was first issued. This is allows me to control for possible selection into obtaining a photo ID as a result of the passage of the Texas law and ensures that I am not assigning individuals to the treatment or control groups endogenously.

I merge the ID data to the voter file using four variables: date of birth, last name, first name, and 5-digit zip code. These identifiers are unique in the registered voter data but

[4]The data does not include two other state-issued forms of photo ID - handgun licenses or election identification certificates. Applying for a handgun license requires a driver license or identification card. A total of 879 election identification certificates were issued as of the end of 2016 for the entire state of Texas according to Jones et al. (2017).

not in the ID data, which is expected as individuals can obtain multiple IDs from Texas Dept. of Public Safety during their lifetime. With an exact merge on these four variables, I match 78% of my main analysis sample to an ID record, which is in line with estimates of ID possession from other sources (Esposito et al., 2019; G.A.O., 2015; Barreto et al., 2009).

The resulting merged dataset is trimmed to include only individuals who were registered by the 2000 election on November 7. I do this for several reasons. First, allowing individuals to enter the sample over time means that the composition of the treatment and control groups could be changing differentially in each election year. Conditioning my main analysis sample on registration by a fixed start date eliminates this possibility.[5] Second, while I do observe each individual's most recent registration date, I do not know when a person originally registered to vote. For some individuals I observe voting history before their registration date and for most others I do not. (According to the Elections Division office, this is likely due to the individual moving to a different county and thus having a voting history prior to their new registration date.) Setting a fixed start date therefore reduces the number assumptions needed to evaluate the impact of the law. Finally, some young voters might enter the sample before having acquired a driver license, meaning they would be classified as treated when they are perhaps more similar to the control group, and I wish to avoid this issue. The resulting, conditioned sample has 4.8 million individuals and 9 election years of voting data from 2000-2016. Descriptive statistics are shown in Table 1.

I assign treatment according to a fixed starting point to control for possible selection into obtaining a photo ID as a result of the passage of the Texas law. For much of my analysis, this is the 2000 election on November 7, the same date used as the registration cutoff. If a voter in my sample has ever been issued a driver license or identification card by the Texas Dept. of Public Safety at any point up to this start date, I assign them to the control group, designated $noID_i = 0$. This is most often a person who has never had a driver license or identification card, but is sometimes an individual who did posses an ID but was first issued

[5] In the next section I discuss my identification strategy which controls for a linear time trend. Allowing voters to enter the sample over time could further be contributing to this differential trend.

one after the start date. Alternatively, it is possible that I do not observe an ID because the match between the voter and identification data failed. This should be uncommon because my match rate is comparable to other sources, but even if this is true for some individuals in the data it would attenuate my estimated effect size because there are voters in the treatment group who do have ID and can legally vote. Another concern is that I cannot see if an ID is active or expired so there are potentially individuals in my control group that do *not* have a valid photo ID. This is a limitation of my data. However, this would also attenuate my estimates, so my estimated treatment effect should still serve as a lower bound for the actual effect size in magnitude.

4 Research Design

I implement a difference-in-differences design to estimate the impact of the Texas law. I compare the control group of individuals who have been issued a driver license or identification card to the treatment group of those without these forms of photo ID. Legally, the control group should be unaffected by the law. And while those registered voters without a state-issued photo ID could still have a permissible form of federal identification such as a passport, they are more likely to be affected by the Texas law. Some have argued that voter ID laws could have a more general suppressive effect even for individuals with valid identification, or that there are binding costs relating to renewing or replacing lost IDs. This paper is unable to address such concerns, but if true, they would attenuate my estimates and the results would represent a lower bound of the magnitude of the full treatment effect.

The standard diff-in-diff model assumes that the treatment and control groups will trend similarly over time. In this context, however, the noID group diverges from the control group according to a pre-existing linear trend over time (see Figure A.1). Additionally, the difference in turnout between the noID and control groups oscillates between presidential and midterm elections, suggesting that these groups turn out differently according to election type. Therefore my main analysis controls for this linear time trend and election-type-by-ID-status differences. The identifying assumption of this model is that the change in outcomes of the treatment group would have continued along this same linear trend relative to the change in outcomes of the control group in the absence of treatment. I operationalize this design by estimating the following equation:

$$voted_{it} = \alpha_i + \alpha_t + \gamma \cdot trend_{noID,t} + \delta \cdot prez_t \cdot noID_i + \beta_1 \cdot noID_i \cdot D_{2014} + \beta_2 \cdot noID_i \cdot D_{2016} + \epsilon_{it} \quad (1)$$

where $voted_{it} \in \{0, 1\}$ indicates if individual i voted in election year t (by any method). I include individual and time fixed effects, α_i and α_t, a linear time trend $trend_{noID,t}$ that controls for differential trends between the treatment and control groups, and an election-type-by-ID-status fixed effect $prez_t \cdot noID_i$ to allow the treatment and control groups to behave differently in presidential elections versus midterms. The treatment variable $noID_i$ is equal to 1 if i was without a state-issued photo ID by the start date and is 0 otherwise. Finally, I estimate the treatment effect separately for each year in the post-treatment period with D_{2014} and D_{2016} since there are likely different types of voters participating in midterm versus presidential elections. I conservatively cluster standard errors at the county level to allow for correlation of the error terms within an area, since different counties may have different demographics, poll workers, or perhaps even voter ID law enforcement severity, although this should be similar state-wide.

5 Results

The difference-in-differences design relies on the common trends assumption. Figure 1 shows that the control and treatment groups follow the same pattern in the pre-treatment period when I control for a linear time trend between them as well as an election-type-by-ID-status fixed effect.[6] The identifying assumption of the following analysis is that the change in outcomes of the treatment group would have continued along this same linear trend relative to the change in outcomes of the control group in the absence of treatment. The point of this figure is to show that the dynamic treatment effect estimates over the pre-treatment period are close to zero, demonstrating evidence that the treatment group does appear to follow the same trend relative to the control group prior to treatment.

It is immediately apparent that 2006 appears off of the pre-existing trend. In Table 2, I present evidence that this year is inconsistent with the rest of the administrative data. Counts of votes cast by voting method over time show impossibly low numbers of absentee and early voting participation (1 and 0 votes cast by these methods in 2006, respectively). The Texas Secretary of State website gives totals for 2006 of early voting and mail voting of 1,001,298 and 73,526, respectively, so my administrative data is clearly incorrect. While it is possible these data are miscoded as election day votes, the low number of votes and participation rate in that year relative to the other data lead to the plausible explanation that some large portion of voting history data was accidentally dropped from that year. Unfortunately, this is how the data appears on the Elections Division system. This is a concern if data is missing in a manner correlated with treatment, and it is clear from the "Early voted" variable in Table 1 that this is the case. Therefore I operate under the assumption that the point estimate for 2006 on the dynamic treatment effect graph in Figure 1 is an inaccuracy. In my main analysis, I opt to omit the year entirely to avoid biasing treatment estimates. This is also to minimize any assumptions that become necessary when imputing new values for 2006 voting history outcomes, an exercise that I explore later in this section.

As my main result, I estimate a statistically significant, negative effect from the Texas voter ID law in the 2016 presidential election year, but there is no significant change for the 2014 midterm. This is consistent with the idea that there are different types of voters that

[6]Figure A.1 shows the dynamic treatment effect before controlling for the linear time trend and election type, and thus why these controls are necessary.

participate in midterm versus presidential elections, and that the voter ID law appears to affect these groups differently. The primary specification omits data from 2006 and includes the linear time trend, an election-type-by-ID-status fixed effect, and individual and year fixed effects. Results are shown in column 1 of Table 3. For the 2014 midterm election, a registered voter was no more or less likely to vote, while in the 2016 presidential election, I estimate that a registered voter was 7.6 percentage points less likely to participate. The latter result is significant at the 99% confidence level, while the 2014 results can rule out an increase greater than 1.7 percentage points or a decrease of more than 2.1 percentage points at the 95% confidence level. From outcome means taken from the conditioned sample, these estimates correspond to no change for the 2014 midterm and a decrease in turnout of 11% for the 2016 presidential election.

One unusual feature of these data is that voting propensity changes with age, and this change occurs differentially with treatment. On average, individuals vote more often as they age, and this change is different for those classified as noID vs. hasID. Indeed there is evidence of this in my data as well as in Hood and Bullock (2012). One might therefore be concerned that the estimated treatment effect is biased by such patterns, so I add age-by-ID fixed effects to the model in column 2 of Table 3. Controlling for differential age trends by ID ownership gives point estimates which are similar in magnitude and significance.

Another concern is that an individual's decision to vote is affected by the area where they do so. For example, the type workers at the polling station, the way these workers were trained, or the underlying demographics of that region could vary from one locale to the next, and even over time. The individual fixed effect would partially control for this except that individuals move and I only observe their location at a single point in time as of August 2018. Therefore, I add a set of voting precinct-by-year fixed effects in column 3 of Table 3. Estimated effects of the voter ID law are still close to zero in 2014 and remain negative and statistically significant in 2016.

For robustness, I explore alternative specifications in Table 4. The first column is identical to the main specification, while the second column instead assigns treatment later, allowing individuals to obtain a photo ID up until the 2010 election on November 2. Column 3 further allows individuals to enter the sample until the same 2010 election, so the data is no longer conditioned on being registered to vote in 2000. Estimates in both of these columns are similar in magnitude and significance to my primary specification.

I include the previously omitted 2006 year observations in column 4 of Table 4, and as expected, this positive value in the pre-treatment period pushes estimates downwards. As an alternative, I predict voting turnout for the 2006 election using a logistic regression with all available variables, and excluding the treatment variable, observations from 2006, and observations from the two post-treatment years. The variables include a dummy for presidential elections, a set of dummies for each year of age at election by ID status, dummies for gender, Hispanic surname, and each county, variables for time, time^2, and time^3, and a set of dummies indicating each individual's own voting history. I then use the predicted values in the same model from equation 1, and results are shown in column 5 of Table 4. Finally, column 6 uses only observations from presidential election years, which does not include 2006 at all. Across these columns, results are similar to my primary specification in magnitude and significance, suggesting that the 2006 anomaly is not a significant source of bias.

6 Discussion

Since the ability to vote is connected to important economics outcomes through representation, government funding, and policy, a major concern with voter ID laws like the one in Texas is that they can adversely affect vulnerable groups. Unfortunately, my data does not identify race like some other voter registries do. Instead, I predict race using name and address and historical census data (cite). I then analyze the impact of the voter ID law for each group separately by race. However, I caution against strong interpretation of these results due to the predicted nature of the race variable as well as the fact that the identifying assumption would still have to hold for each subgroup individually, and in some cases the visual evidence of this is tenuous. I estimate the effect of the Texas voter ID law on groups by predicted race in Table 5. Figure **??** shows the dynamic treatment effect over time on voters lacking ID within each subgroup.

In summary, voter ID laws have become more prevalent in the U.S. without clear evidence of their impact on turnout. While advocates claim it boosts election accuracy, while others caution an additional cost to voting. Since voting affects outcomes like representation, government funding, and policy, laws affecting turnout carry great economic significance. I study the effect of a strict case of one such law in Texas and identify its impact on the small portion of the population lacking proper identification required to vote. I do so with data created by matching individual-level data on state-issued driver licenses and identification cards to voter history 2000-2016. I identify effects by comparing people with and without permissible photo ID over time. Results show that registered voters who had not been issued a photo ID were no more or less likely to vote in the 2014 midterm election. In the 2016 presidential election, however, this group of voters was 7.6 percentage points less likely to turn out. These results are robust to the inclusion of control variables and a variety of alternative specifications.

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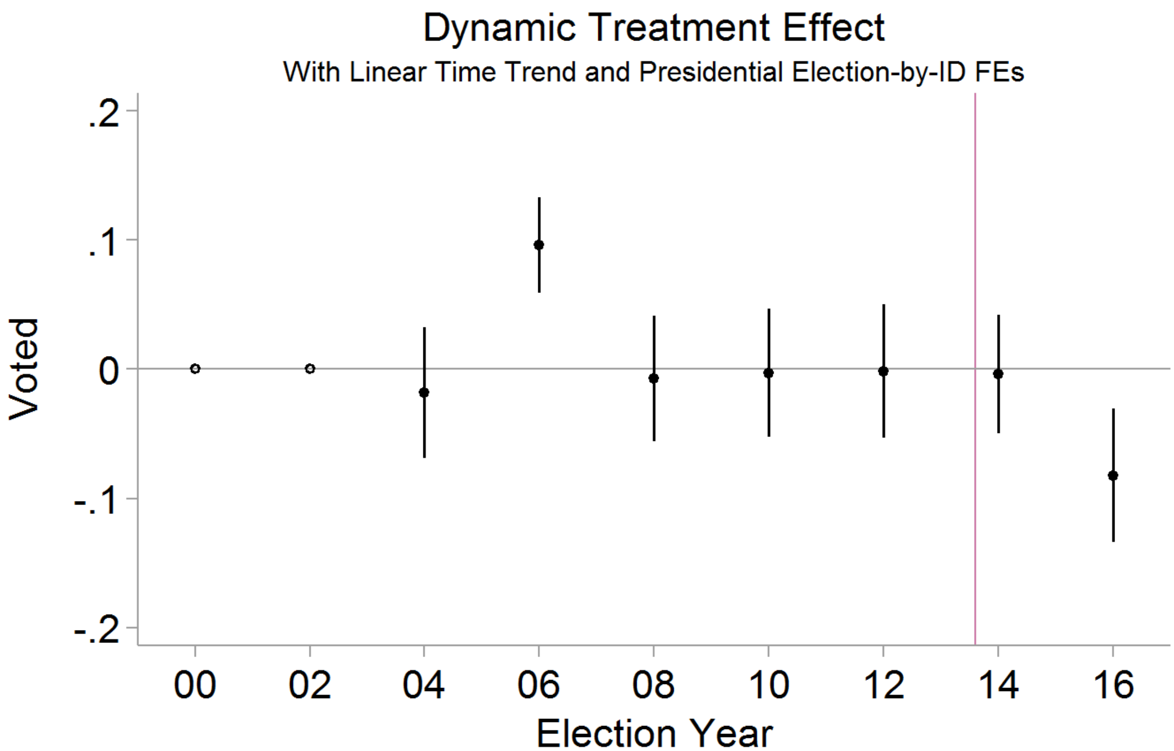
Figure 1: Evidence of Common Trends



This is a conditioned sample that includes individuals voters who were registered to vote by the time of the 2000 election on Nov. 7. Treatment (noID) is assigned to those individuals who had not been issued a driver license or identification card in Texas by the same start date.

Table 1: Summary Statistics

	All	HasID	NoID
NoID (Treated)	0.24	0.00	1.00
DPS match	0.78	1.00	0.08
Female	0.55	0.55	0.58
Hispanic	0.23	0.22	0.28
Age at election	53.24	52.83	54.55
Voted	0.56	0.61	0.41
Early voted (EV)	0.26	0.29	0.17
Individuals	4,765,221	3,628,527	1,136,694
Observations	42,886,989	32,656,743	10,230,246

This is a conditioned sample that includes individuals voters who were registered to vote by the time of the 2000 election on Nov. 7. Treatment (noID) is assigned to those individuals who had not been issued a driver license or identification card in Texas by the same start date.

Table 2: Total Votes by Voting Method by Year

	2000	2002	2004	2006	2008	2010	2012	2014	2016
Absentee	10,559	19,665	33,148	1	68,420	79,892	129,635	231,480	278,208
Election Day	1,999,194	1,613,967	2,363,747	989,916	1,152,043	1,141,595	1,098,803	966,722	715,382
Early Voting	436,249	304,596	986,060	0	2,378,686	1,347,485	2,243,439	1,204,339	2,349,614
Provisional	0	0	0	0	14	39	82	248	757
Total	2,446,002	1,938,228	3,382,955	989,917	3,599,163	2,569,011	3,471,959	2,402,789	3,343,961

There are no reported early voters and only one absentee voter in 2006, which is clearly inconsistent with the other years. This is a problem because early voting is correlated with treatment (see Table 1, "Early voted" variable). Thus I exclude 2006 from the main analysis in Table 3 but show my results are robust to alternative solutions to this problem in Table 4.

Table 3: Main Treatment Effect Estimates, Omitting 2006

	(1)	(2)	(3)
$noID \times 2014$	-0.0021	0.0008	0.0009
	(0.0097)	(0.0097)	(0.0063)
$noID \times 2016$	-0.0756***	-0.0709***	-0.0827***
	(0.0182)	(0.0179)	(0.0136)
Observations	38,121,768	38,121,768	38,121,768
Voted mean (midterm)	.4834	.4834	.4834
Voted mean (presidential)	.6818	.6818	.6818
Individual & year FEs	Y	Y	Y
Pres. election-by-ID FEs	Y	Y	Y
Age-by-ID FEs		Y	Y
Precinct-by-year FEs			Y

This is a conditioned sample that includes individuals voters who were registered to vote by the time of the 2000 election on Nov. 7. Treatment (noID) is assigned to those individuals who had not been issued a driver license or identification card in Texas by the same start date. All columns include a linear time trend for hasID vs. noID, an election-type-by-ID-status fixed effect, and individual and year fixed effects. Columns 2 and 3 add an age-by-year fixed effect and column 3 further includes a voting precinct-by-year fixed effect. This table presents estimates omitting data for 2006, where a large number of observations appear to be missing from the underlying dataset in a manner correlated with treatment. Standard errors in parentheses are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 4: Robustness to Alternative Specifications

	(1) Baseline	(2) ID ('10)	(3) Registered ('10)	(4) Include '06	(5) Predict '06	(6) Only Pres.
$noID \times 2014$	0.0009	0.0016	0.0081	-0.0253***	0.0096	
	(0.0063)	(0.0063)	(0.0058)	(0.0062)	(0.0070)	
$noID \times 2016$	-0.0827***	-0.0888***	-0.0789***	-0.0819***	-0.0833***	-0.0822***
	(0.0136)	(0.0128)	(0.0105)	(0.0136)	(0.0136)	(0.0108)
Observations	38,121,768	38,121,768	70,137,738	42,886,989	42,886,989	23,826,105
Voted mean (midterm)	.4834	.4834	.3798	.4145	.4976	.
Voted mean (presidential)	.6818	.6818	.5753	.6818	.6818	.6818
Individual & year FEs	Y	Y	Y	Y	Y	Y
Pres. election-by-ID FEs	Y	Y	Y	Y	Y	-
Age-by-ID FEs	Y	Y	Y	Y	Y	Y
Precinct-by-year FEs	Y	Y	Y	Y	Y	Y

This is a conditioned sample that includes individuals voters who were registered to vote by the time of the 2000 election on Nov. 7. Treatment (noID) is assigned to those individuals who had not been issued a driver license or identification card in Texas by the same start date. All columns include a linear time trend for hasID vs. noID, an election-type-by-ID-status fixed effect, and individual and year fixed effects. Column 1 presents the main results from Table 3, column 2 alternatively assigns treatment (noID) as of the 2010 election on Nov. 2, column 3 allows individuals to enter the sample up until the 2010 election (with the same treatment status also as of 2010), column 4 uses the previously omitted (but inconsistent) data for 2006, column 5 uses predicted values for 2006, and column 6 presents results using only presidential elections during the same period. Standard errors in parentheses are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

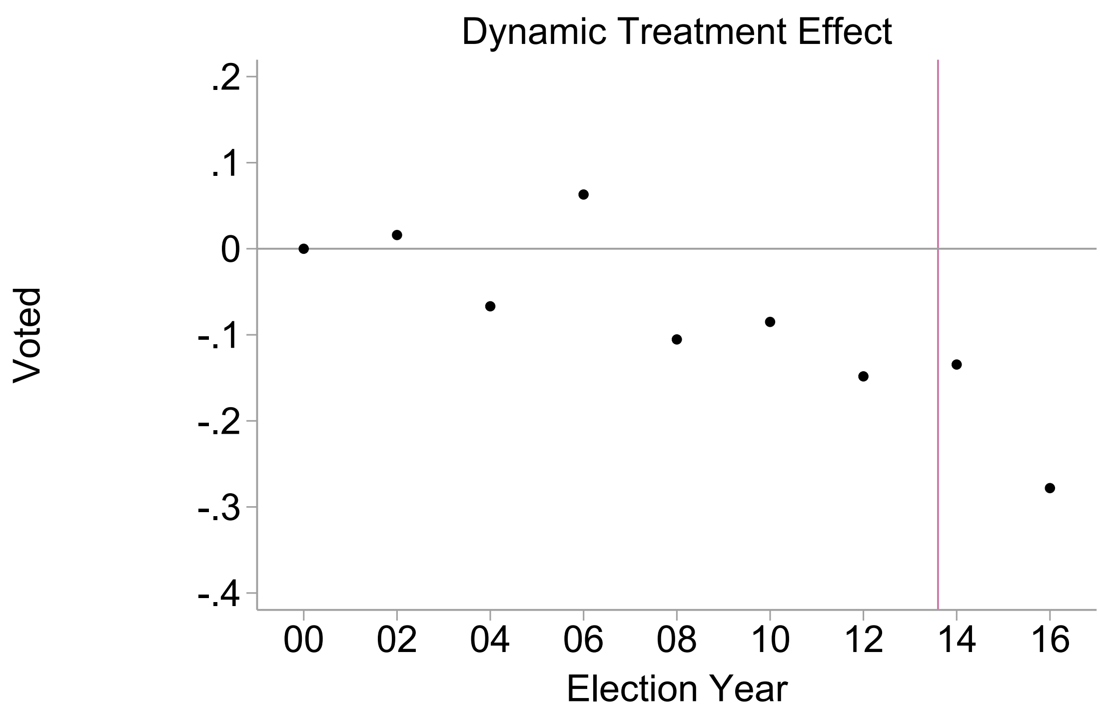
Table 5: Population Subgroups

	(1) White	(2) Black	(3) Hispanic	(4) Other
$noID \times 2014$	-0.0164***	0.0051	0.0330***	0.0439***
	(0.0055)	(0.0085)	(0.0089)	(0.0082)
$noID \times 2016$	-0.1145***	-0.0402	-0.0379***	-0.0056
	(0.0116)	(0.0272)	(0.0107)	(0.0191)
Observations	24,862,768	3,614,512	8,965,144	679,344
Voted mean (midterm)	.5504	.4607	.3186	.3251
Voted mean (presidential)	.745	.6659	.5215	.5665
Individual & year FEs	Y	Y	Y	Y
Pres. election-by-ID FEs	Y	Y	Y	Y
Age-by-ID FEs	Y	Y	Y	Y
Precinct-by-year FEs	Y	Y	Y	Y

This is a conditioned sample that includes individuals voters who were registered to vote by the time of the 2000 election on Nov. 7. Treatment (noID) is assigned to those individuals who had not been issued a driver license or identification card in Texas by the same start date. This table presents estimates omitting data for 2006, where a large number of observations appear to be missing from the underlying dataset in a manner correlated with treatment. Standard errors in parentheses are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

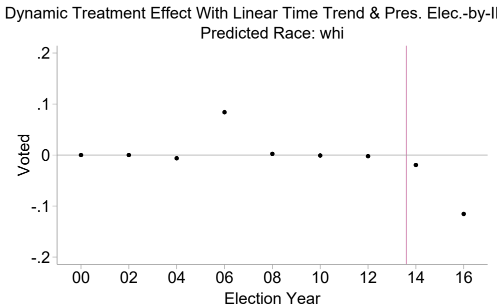
A Appendix

Figure A.1

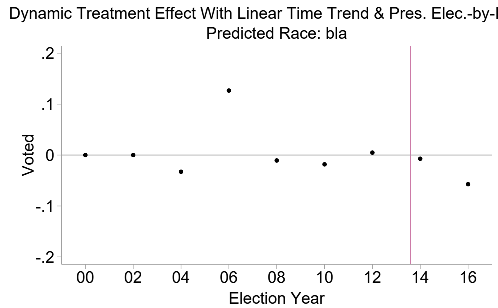


This is a conditioned sample that includes individuals voters who were registered to vote by the time of the 2000 election on Nov. 7. Treatment (noID) is assigned to those individuals who had not been issued a driver license or identification card in Texas by the same start date.

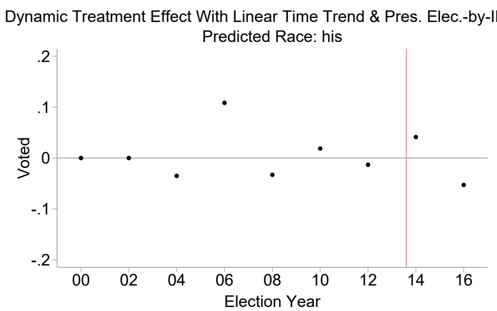
Figure A.2



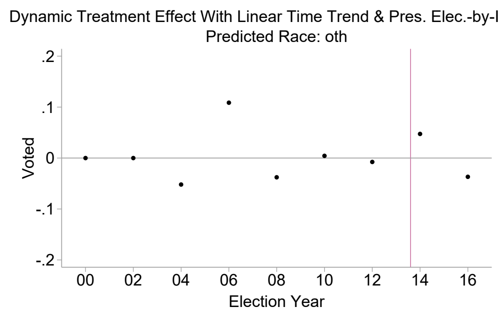
(a) Picture 1



(b) Picture 2



(c) Picture 3



(d) Picture 4