Comment on "Voter Identification Laws and the Suppression of Minority Votes"*

Justin Grimmer[†] Eitan Hersh[‡] Marc Meredith[§]

Jonathan Mummolo[¶] Clayton Nall[‖]

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Abstract

To study how voter identification laws affect participation in elections, Hajnal, Lajevardi and Nielson (2017) examines validated turnout data in five national surveys conducted between 2006 and 2014. The study concludes that strict ID laws cause a large turnout decline among minorities, especially Latinos. Here, we show that the results of this paper are a product of large data inaccuracies, that the evidence does not support the stated conclusion, and that model specifications produce highly variable results. When errors in the analysis are corrected, one can recover positive, negative, or null estimates of the effect of voter ID laws on turnout. Our findings underscore that no definitive relationship between strict voter ID laws and turnout can be established from the validated CCES data. Our analysis highlights more general problems with the way empirical evidence is assembled and reported, but it also offers useful suggestions on the appropriate evidence sources for research on election administration.

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[†]Associate Professor, Department of Political Science, Stanford University

[‡]Assistant Professor, Department of Political Science, Yale University

[§]Associate Professor, Department of Political Science, University of Pennsylvania

[¶]Ph.D. candidate, Department of Political Science, Stanford University

[‖]Assistant Professor, Department of Political Science, Stanford University

Measuring voter identification (ID) laws' effect on voter turnout is a crucial public policy question. Hajnal, Lajevardi and Nielson (2017) (HLN hereafter) provides the most recent attempt to measure this effect, using validated individual-level turnout data from five Cooperative Congressional Election Studies (CCES) surveys conducted between 2006 and 2014. HLN conclude that strict voter ID laws cause a large turnout decline in certain racial and ethnic minority groups, including Latinos, who "are 10 [percentage points] less likely to turn out in general elections in states with strict ID laws than in states without strict ID regulations, all else equal (HLN, p. 6)".[1]

Strict voter ID laws may reduce turnout, particularly among minorities, but the evidence presented in HLN does not constitute reliable information documenting such a relationship. HLN's measures of turnout often substantially differ from official state turnout. Further, the core analysis in HLN, a series of cross-sectional regressions, does not credibly isolate the causal effect of voter ID laws because of the presence of unobserved differences between states with and without these laws. A placebo test of HLN's model shows a statistically significant relationship between *future* implementation of voter ID laws and turnout, an indication of omitted variable bias. Finally, HLN's difference-in-differences approach, which is better equipped to address this problem, is incorrectly interpreted in the text of the paper. If taken at face value, the findings suggest that strict voter ID laws *increase* turnout across all racial groups. Correcting data errors and adjusting modeling choices yields null and highly uncertain estimates. In short, the data and research design in HLN leave us unable to draw actionable conclusions about the impact of voter ID laws on turnout.

Measuring Voter Turnout. HLN's evidence for the effect of voter ID laws on turnout by racial group comes from CCES surveys. After people take the survey, a third-party attempts to match respondents to a database maintained by the company Catalist, which

[1]HLN also examines the relationship between voter ID laws and the turnout rate among Democrats and Republicans. Here, we focus on the effect of voter ID laws and minority turnout because of its relevance under the Voting Rights Act.

combines data from voter registration files and consumer databases. For respondents who are matched to the database, indicators are created noting whether the respondent had a record of voting in primary and general elections that year in the database.

These measurement errors in turnout raise the potential of both inefficiency and bias. Even if these laws affect turnout, measurement error may add so much noise to the data that we fail to detect any effects (inefficiency). And once the data are aggregated by state, if measurement errors are correlated with the adoption of voter ID laws, we will recover the wrong treatment effect estimates (bias). Figure 1 shows that CCES estimates of state-level turnout deviate substantially from the truth, and also shows substantial variation in the measure, raising the specter of inefficiency. In the 2006 CCES, the estimated turnout rate was 10 points below actual turnout in 15 states, most of which showed practically zero turnout according to the CCES.[2] Virginia had almost no validated voters in 2008, as well.[3] Given the error in the 2006 study, that year is not suitable for use in the analysis, nor are Virginia's records from 2008.[4] But HLN include these data in their analysis.

A close look at how mis-measurement affects treated and untreated states suggests that bias is a problem as well. Consider turnout in Tennessee, which implemented strict voter ID between the 2010 and 2012 general elections, and in Kentucky, which had no strict ID law in that period. As Table A.3 in our appendix shows, turnout increased in Tennessee, even more so than in Kentucky according to CCES data while in reality the opposite is true: turnout decreased in Tennessee more so than in Kentucky, over this time period.[5]

Figure A.1 in our appendix shows the problems identified in Kentucky and Tennessee

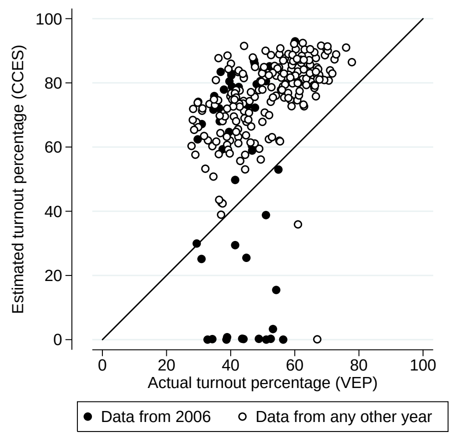
[2]In the appendix, Table A.1 and Table A.2 report turnout rates by state-year in general and primary elections, respectively.

[3]Due to a state policy in Virginia that was in effect in 2006 and 2008, CCES vendors did not have access to vote history in that state.

[4]Based on Table A.2, we also exclude primary turnout data from Louisiana and Virginia in all years.

[5]The CCES data presented under the subheading "Sample weights, drop unmatched/unregistered" correspond to the CCES data presented in Figure 1. They show turnout increased by about 15 and 10 percentage points in Tennessee and Kentucky, respectively. Actual turnout data reveals that turnout dropped by about 5 percent in Tennessee, while remaining roughly flat in Kentucky.

Figure 1: Plotting Actual vs CCES Estimates of General Election Turnout Percentage



Note: CCES turnout percentage constructed using sample weights, dropping respondents who self-classify as being unregistered, and dropping respondents who do not match to the voter file. We do this to be consistent with the coding in HLN Table 1. Actual turnout percentage calculated by dividing the number of ballots cast for the highest office on the ballot in a state-year by the estimated voting-eligible population (VEP), as provided by the United States Election Project.

are also present in the broader population of states. Virginia, which implemented voter ID between the 2010 and 2012 elections, is the biggest problem, being measured as having a 90 percentage point and 70 percentage point increase in turnout from 2008 to 2012 and 2006 to 2014, respectively. Figure A.1 reveals a number of less dramatic issues as well. Sometimes the CCES indicates that turnout increased by more than 10 percentage points, despite it decreasing in reality, while in other cases it shows that turnout decreased despite an actual increase. One reason why turnout trends in the CCES may not match reality is that Table A.4 shows that the share of CCES respondents who failed to match to Catalist's database changed over time, increasing from about 10 percent in 2008 and 2010 to about 20 and 30 percent in 2012 and 2014, respectively. HLN drop these respondents from all analyses.

The measurement error in year-to-year turnout in the CCES data makes studying the effect of voter ID laws difficult. Erikson and Minnite (2009) detail the statistical power issues researchers generally face when trying to estimate the effect of voter ID policies on turnout. Noisy measures of turnout only will exacerbate this problem, even if that measurement error is unrelated to voter ID laws. Table A.4 shows that changes in the CCES vote validation procedures over time affect racial groups differently, with the number of unmatched minorities increasing over time relative to the number of whites, an especially serious concern given HLN's focus on voter ID laws' effects across racial subgroups.[6]

Omitted Variable Bias. The main findings in HLN use a series of cross-sectional logit regressions to test whether strict voter ID laws suppress turnout, and whether those effects are more pronounced among racial and ethnic minorities. The key explanatory variable equals 1 if a state has a strict ID law in place in a given election year. This variable is also interacted with indicators for respondents' racial group to measure heterogeneous effects.

The main weakness of this approach is well-known: states that did and did not adopt voter ID laws systematically differ on unobservable dimensions that also affect turnout. The authors address this by including a host of variables meant to control for these confounding factors. To regard the estimates produced by these models as causal, we must assume that the authors have included all such relevant control variables in their models.

A placebo test helps to assess the plausibility of this assumption. If the model eliminated omitted variable bias, results will not indicate that voter ID laws affected turnout in the period *before* they were enacted. Stated differently, conditional on these covariates, states that do and do not adopt these laws should have roughly the same turnout levels *in the absence of voter ID laws.*

This is not the case. Table A.5 in our appendix presents estimates from nearly the same specification that HLN report in Table 1, Column 1. There are two main differences. First, we do not include states that already implemented strict voter ID in the analysis. Second,

[6] These patterns do not imply that CCES data are generally problematic, but that care must be taken when using these data, particularly when estimating determinants of longitudinal turnout rates within groups.

our treatment variable is an indicator for whether the state will implement a strict voter ID law by 2014.[7] The interpretation of this treatment variable is the difference in turnout in states *that have not yet implemented strict voter ID laws* relative to states *which never implemented such a law*, after adjusting for the individual-level and state-level variables that HLN include in their analysis. The results presented in Table A.5 suggest that turnout is about 6 percentage points lower in places that will adopt a strict ID law, and there is little detectable heterogeneity in this relationship for whites, blacks, and Latinos. This is a strong indication that the main HLN model does not overcome omitted variable bias.

Misinterpretation. The failure of the placebo test highlights the need for a different analytic approach. HLN acknowledges this concern by reporting a supplementary model (HLN Appendix Table A9) with state and year fixed effects (i.e., a difference-in-differences estimator). HLN notes that this is "among the most rigorous ways to examine panel data."

HLN claims that the results of this fixed-effects analysis tell "essentially the same story as our other analysis.... Racial and ethnic minorities...are especially hurt by strict voter identification laws." This description is inaccurate. The estimates reported in HLN Table A9 imply that voter ID laws *increased* turnout across all racial and ethnic groups, though the increase was less pronounced for Hispanics than for whites, a finding we also regard as implausible for reasons discussed below.[8] As Table A.6 in our appendix shows, this fixed-effects model estimates that the laws increased turnout among white, African American, Latino, Asian American and mixed race voters by 10.9, 10.4, 6.5, 12.5 and 8.3 percentage points in general elections, respectively. The laws' positive turnout effects for Latinos only look low compared to the large positive effects estimated for the other groups. These results

[7] We also omit 2006 data due to the problems cited above, and 2014 data because there are no states that will implement a strict voter ID by 2014 that remain in the sample. By defining the treatment this way we necessarily drop the indicator for being in the first year of a strict voter ID law.

[8] In contrast to the other models in the paper, we replicated the results in Table A9 using OLS regression, no survey weights, and without clustering the standard errors. HLN provided replication code for their appendix, but the estimated model from that code does not produce the estimates reported in Table A9.

deviate substantially from other published findings of a treatment effect of zero, or close to it (Citrin, Green and Levy 2014) and voter ID laws increasing turnout is a conclusion with different legal implications than the one advanced in HLN.[9]

In addition to Table A9, HLN Figure 4 presents simple bivariate difference-in-differences models, comparing changes in turnout (2010 to 2014) in just three of the states that implemented strict ID laws between these years to the changes in turnout in the other states. HLN reports that voter ID laws increase the turnout *gap* between whites and other groups without presenting whether voter ID laws actually suppress turnout.[10] The results of our replication show no consistent evidence of suppressed turnout. Table A.2 in our appendix shows that the large white-minority gaps reported in HLN Figure 4 are driven by increased white turnout in Mississippi, North Dakota, and Texas, not by a drop in minority turnout.

Additional Errors. HLN contains additional data processing and modeling errors. First, without explanation, HLN includes an indicator of whether a state had a strict voter ID law *and* a separate indicator of whether the state was in its first year with this strict ID law. With this second variable included, the correct interpretation of their estimates is not the effect of ID laws on turnout, but the effect in years other than the first year. The interactions with racial groups are even harder to interpret since they are not also interacted with the law-in-first-year indicator.[11]

[9]In an email exchange HLN assert that the model in the appendix is mistakenly missing three key covariates: Republican control of the state house, state senate, and governor's office. The authors provided additional replication code in support of this claim. This new replication code differs from the original code and model in several respects. First, we replicated the original coefficients and standard errors in Table A9 using a linear regression with unclustered standard errors and without using weights. The new code uses a logit regression, survey weights, and clusters the standard errors at the state level. While including Republican control of political office adjusts the coefficients, the other major data errors remain and the adjustment is sensitive to the included covariates.

[10]Note: In replicating these results, we recovered different effects than those reported in Figure 4 and accompanying text. In an email exchange, the authors stated they had miscalculated the effects for Asian Americans and those with mixed race backgrounds.

[11]The first year indicator contains some coding errors. Table A.1 shows that HLN code "First year of strict law" in Arizona occurring in 2014, even though it is coded as having a strict ID law since 2006. HLN also never code "First year of strict law" in Virginia, even though Virginia implements a strict ID law in 2011 according to the HLN coding.

Second, there are a number of inconsistencies in model specifications. HLN cluster their standard errors at the state level in their main analysis, but they fail to do so in their appendix analysis[12] Based on our replications, it also appears that sampling weights only were used Table 1, but not Figure 4 or Table A9. For the analyses reported in Table 1 and Table A9, but not Figure 4, HLN exclude about 8% of respondents based on their self-reported registration status.[13] Because the decision of whether to register could also be affected by a strict voter ID law, it seems more appropriate to keep these respondents in the sample. Finally, HLN code six states as implementing voter ID between 2010 and 2014 when constructing Table 1 and Table A9, but then only consider three of them when performing the analysis that appears in Figure 4.[14]

Finally, for all of the analyses contained in the paper, HLN drop respondents who do not match to a voter file. Whereas all confirmed voters are theoretically listed in publicly available voter files, only some non-voters appear in databases like Catalist's. Pertinent here, Latino and Black voters are much more likely to be unlisted than whites (Jackman and Spahn 2017). Conditional on being a non-voter, appearing in a commercial database is likely correlated with having a photo ID. Thus, best practice would dictate that voters who do not match to a Catalist profile should have been treated as non-voters as recommended by Ansolabehere and Hersh (2012).

Adjustments to HLN's Model. To assess whether measurement errors and modeling choices affect results, Figure 2 presents the implied treatment effect estimates according to the fixed-effects model in Table A9 in HLN, as well as alternative estimates after addressing

[12] Standard errors need to be clustered by state because all respondents in a state are affected by the same voter ID law. There are many state-level attributes, like political culture, that affect the turnout calculus of all individuals in a given state. And in any given election year, the turnout decisions of individuals in a state may respond similarly to time variant phenomena, like whether the candidates are appealing.

[13] We are unsure how this variable was coded because no details are provided.

[14] An additional concern is that in HLN's models of primary election turnout control for competitiveness using a measure of general election competitiveness rather than primary competitiveness. If the model is meant to mirror the general election model, it should include a control for primary competitiveness, which is important given the dynamics of presidential primaries over this period.

the concerns outlined above.[15] For clarity and brevity, we focus on effects among white and Hispanic voters only.[16] The effect for whites is positive, but only statistically significant in primaries. The effect for Latinos is sometimes positive, sometimes negative, and generally not significant. Our 95% confidence intervals are generally 8 to 10 percentage points wide, consistent with Erikson and Minnite's (2009) observation that models of this sort are underpowered to adjudicate between plausible effect sizes of voter ID policy.[17]

We find similar patterns when we examine the robustness of the results presented in HLN's Figure 4.[18] In no specification do we find that primary or general turnout significantly declined between 2010 and 2014 among Hispanics or Blacks in states that implemented a strict voter ID law in the interim, and in many the point estimate is positive. There are some specifications that suggest that white turnout increased, particularly in primary elections. But we suspect that this is largely due to flawed data, as actual returns indicate that overall turnout declined in these states relative to the rest of the country.

In sum, the survey data HLN utilized contain immense measurement error in the dependent variable that is non-constant over time, making it unsuitable for this research question. The cross-sectional regressions that comprise the central analysis in the study fail to adequately correct for omitted variable bias. The difference-in-differences model yields results that, if taken as true, would refute the claim that voter ID laws suppress turnout. Finally, our attempts to address measurement and specification issues still fail to produce the ro-

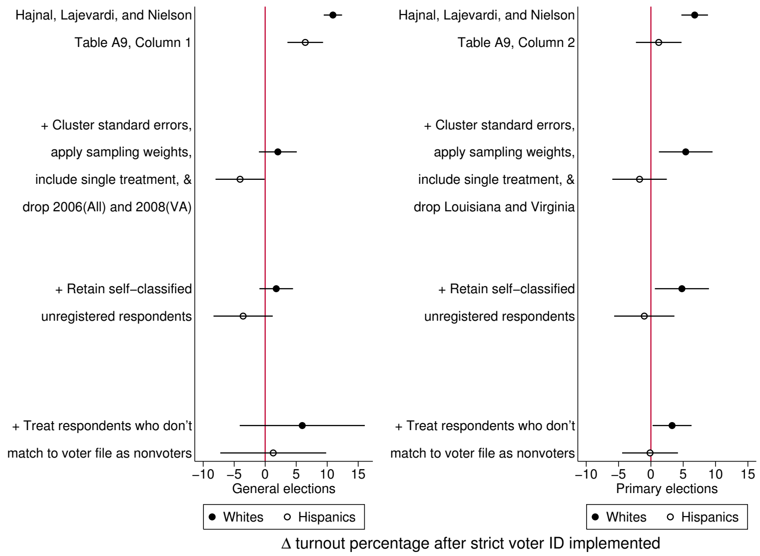
[15]In our appendix, we present a similar figure (Figure A.3) and table (Table A.9) that examines the robustness of the pooled cross-sectional results presented in HLN's Table 1. We find that the negative association between a strict photo ID law and minority turnout attenuates but remains as these errors are corrected.

[16]Results for all racial groups are presented in Table A.7 (general elections) and Table A.8 (primary elections) in our appendix.

[17]And these confidence intervals are only considering uncertainty due to sampling error, and not model specification. While we maintained HLN's statistical model for comparability, this would not be the statistical model we would estimate if starting anew. Further, we note again that increases in voter turnout for minority groups would make a voting rights claim very difficult. Therefore the direction of the effect, and not just the effect size relative to whites, is essential for understanding the effect of the policy.

[18]See Figure A.4, Table A.10, and Table A.11 for more details.

Figure 2: Sensitivity of Estimates from Models with State Fixed Effects to Alternative Specifications



Note: Bars represent 95% confidence intervals. Models are cumulative (e.g., we are also retaining self-classified unregistered respondents in model in which we treat respondents who do not match to voter file as nonvoters). See Table A.7 (left) and Table A.8 (right) in our appendix for more details on the models used to produce these estimates.

bust results required to support public policy recommendations. Using these data and this research design, we can draw no firm conclusions about the effect of strict voter ID laws.

Implications for Future Research. Under new disciplinary norms about data-sharing, post-publication review is an increasingly important part of political science research. Thanks to the publication of replication code and data, we were able to replicate most of the findings in HLN. We hope that our contribution here is seen as it is intended: as a natural continuation of the abbreviated and incomplete nature of the current peer-review process. We hope that the findings herein contribute to an ongoing research agenda dedicated to improving knowledge and data sources surrounding our electoral institutions.

While augmented national survey data have useful applications, they have limited use in this context. The CCES survey is not designed to be representative of small populations like those lacking photo IDs, and many of the discrepancies we identify are due to substantial year-to-year differences in measurement and record linkage. Future research on voter ID laws and turnout should look elsewhere. Given the existing evidence, researchers should turn to data that allow more precision than one can obtain from a national survey sample. Such measures could include linking voter databases to records of ID holders (Ansolabehere and Hersh 2016), or custom-sampling surveys of individuals affected by voter ID laws. While strategies like these may require partnerships with governments and financial resources, such investments are commensurate with the importance of research on electoral institutions.

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1 Appendix

Table A.1: Estimated CCES General Election Turnout by State and Year

State	2006	2008	2010	2012	2014
Alabama	59.3	74.6	55.7	74.7	62.1
	(3.1)	(3.2)	(3.2)	(3.8)	(4.1)
	N = 314	N = 316	N = 557	N = 575	N = 406
Alaska	80.5	81.5	62.5	87.0	82.2
	(5.3)	(5.6)	(7.8)	(4.8)	(7.2)
	N = 82	N = 62	N = 117	N = 101	N = 73
Arizona	.8	75.4	69.5	88.7	73.4
	(.4)	(2.3)	(2.2)	(1.4)	(2.3)
	N = 467	N = 668	N = 1308	N = 1161	N = 945
Arkansas	0	74.1	68.1	82.0	86.0
	(0)	(3.4)	(3.7)	(3.1)	(2.2)
	N = 194	N = 337	N = 412	N = 399	N = 299
California	82.3	83.5	74.4	84.8	74.1
	(1.0)	(1.0)	(1.1)	(1.0)	(1.1)
	N = 2095	N = 2201	N = 4503	N = 3788	N = 3333
Colorado	86.6	83.9	70.7	90.4	85.3
	(2.1)	(2.3)	(2.5)	(1.4)	(2.1)
	N = 376	N = 450	N = 901	N = 841	N = 691
Connecticut	60.4	75.8	74.3	76.1	83.4
	(3.8)	(2.8)	(2.7)	(2.8)	(2.2)
	N = 215	N = 371	N = 656	N = 473	N = 397
Delaware	78.5	82.4	75.6	87.1	60.3
	(5.1)	(5.0)	(4.8)	(3.2)	(5.6)
	N = 84	N = 104	N = 190	N = 192	N = 132
Florida	80.5	78.4	64.7	84.2	77.6
	(1.2)	(1.4)	(1.3)	(1.3)	(1.3)
	N = 1593	N = 1804	N = 3785	N = 3008	N = 2497
Georgia	74.4	81.2	62.0	80.6	69.6
	(1.8)	(1.9)	(2.1)	(2.2)	(2.4)
	N = 812	N = 718	N = 1489	N = 1345	N = 1038
Hawaii	77.9	77.7	75.8	91.5	87.7
	(6.1)	(5.8)	(5.1)	(3.3)	(4.8)
	N = 64	N = 62	N = 144	N = 135	N = 105
Idaho	73.0	86.2	65.6	86.6	84.3
	(4.1)	(3.2)	(4.4)	(3.6)	(3.7)
	N = 173	N = 148	N = 246	N = 275	N = 161
Illinois	82.9	81.3	63.2	84.2	76.8
	(1.4)	(1.8)	(1.7)	(1.5)	(1.6)
	N = 1074	N = 991	N = 2149	N = 1602	N = 1478
Indiana	68.0	85.5	42.7	88.9	60.3
	(2.2)	(2.1)	(2.3)	(1.7)	(2.5)
	N = 623	N = 631	N = 1035	N = 824	N = 767
Iowa	79.6	88.6	67.9	90.0	83.0
	(3.0)	(2.1)	(3.2)	(1.9)	(3.1)
	N = 255	N = 391	N = 528	N = 517	N = 382
Kansas	.3	86.2	68.0	87.6	83.9
	(.3)	(2.5)	(3.5)	(1.9)	(2.9)
	N = 345	N = 355	N = 488	N = 555	N = 335
Kentucky	78.8	76.8	61.2	77.9	71.2
	(2.6)	(2.6)	(3.0)	(2.8)	(3.1)
	N = 335	N = 392	N = 658	N = 667	N = 459
Louisiana	62.4	80.0	60.7	82.3	73.5
	(3.5)	(3.0)	(3.4)	(2.8)	(3.9)
	N = 251	N = 331	N = 551	N = 541	N = 373
Maine	15.5	80.7	62.0	91.6	82.5
	(3.2)	(3.3)	(5.1)	(1.9)	(4.2)
	N = 167	N = 216	N = 308	N = 330	N = 209
Maryland	58.9	82.2	66.4	87.7	77.8
	(2.5)	(2.7)	(2.7)	(1.6)	(2.5)
	N = 500	N = 431	N = 859	N = 826	N = 625
Massachusetts	.3	82.6	59.5	79.3	81.5
	(.3)	(2.1)	(2.9)	(1.9)	(2.0)
	N = 268	N = 470	N = 903	N = 887	N = 718
Michigan	85.2	80.9	53.0	85.6	73.5
	(1.3)	(1.9)	(2.0)	(1.4)	(1.9)
	N = 1054	N = 925	N = 1664	N = 1451	N = 1227
Minnesota	92.9	86.5	61.8	91.0	84.9
	(1.4)	(2.3)	(3.1)	(1.1)	(1.7)
	N = 469	N = 515	N = 804	N = 823	N = 709
Mississippi	30.0	35.9	38.9	79.8	57.6
	(4.4)	(3.6)	(4.5)	(4.1)	(4.8)
	N = 132	N = 235	N = 342	N = 347	N = 249
Missouri	83.8	82.5	57.6	88.4	63.4
	(1.8)	(2.0)	(2.4)	(1.5)	(2.7)
	N = 582	N = 731	N = 1100	N = 969	N = 726

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Table A.1 – continued from previous page

State	2006	2008	2010	2012	2014
Montana	0 (0) N = 91	79.1 (3.8) N = 164	61.1 (8.4) N = 136	92.4 (2.2) N = 200	87.9 (3.0) N = 134
Nebraska	72.3 (4.9) N = 129	72.7 (4.3) N = 207	42.4 (6.1) N = 139	90.5 (2.0) N = 455	74.8 (3.7) N = 260
Nevada	83.4 (2.7) N = 262	81.9 (2.7) N = 345	76.8 (3.1) N = 534	87.0 (2.0) N = 517	67.8 (4.2) N = 378
New Hampshire	29.5 (5.3) N = 100	82.9 (3.3) N = 192	70.7 (4.7) N = 303	91.4 (1.8) N = 284	85.0 (3.0) N = 187
New Jersey	64.7 (2.3) N = 567	81.2 (2.1) N = 718	43.5 (2.4) N = 1237	77.5 (1.8) N = 1125	71.3 (2.1) N = 926
New Mexico	78.7 (3.3) N = 220	79.9 (3.2) N = 222	72.6 (4.6) N = 363	84.5 (2.8) N = 357	80.9 (3.6) N = 270
New York	75.9 (1.5) N = 1180	72.7 (1.6) N = 1418	61.7 (1.6) N = 2402	83.1 (1.2) N = 2109	68.4 (1.6) N = 1866
North Carolina	67.2 (2.2) N = 661	84.0 (1.6) N = 807	59.2 (2.2) N = 1290	85.6 (1.3) N = 1341	72.6 (2.0) N = 1085
North Dakota	25.5 (17.5) N = 8	73.2 (6.7) N = 83	61.4 (8.2) N = 101	92.2 (3.6) N = 71	82.8 (5.3) N = 67
Ohio	85.9 (1.3) N = 1084	84.8 (1.4) N = 1168	67.9 (1.8) N = 2117	87.1 (1.3) N = 1638	73.1 (1.8) N = 1546
Oklahoma	72.1 (3.6) N = 245	81.6 (3.0) N = 369	63.2 (3.8) N = 466	80.5 (2.7) N = 506	66.2 (4.6) N = 306
Oregon	.3 (.2) N = 498	81.0 (2.6) N = 504	78.6 (2.9) N = 689	90.4 (1.4) N = 945	90.0 (1.3) N = 684
Pennsylvania	81.9 (1.4) N = 1094	79.3 (1.4) N = 1563	64.7 (1.6) N = 2292	86.8 (1.3) N = 1725	74.6 (1.4) N = 1663
Rhode Island	38.8 (6.5) N = 72	87.2 (4.7) N = 88	63.7 (6.7) N = 167	89.0 (3.5) N = 195	75.5 (5.6) N = 125
South Carolina	71.6 (2.9) N = 335	75.3 (2.7) N = 370	58.0 (3.3) N = 573	78.9 (2.6) N = 720	74.8 (2.6) N = 512
South Dakota	88.2 (3.6) N = 88	83.0 (4.0) N = 115	63.1 (8.3) N = 132	88.7 (3.2) N = 131	69.0 (8.0) N = 97
Tennessee	49.8 (2.7) N = 428	79.5 (2.2) N = 550	50.8 (2.8) N = 833	82.4 (2.4) N = 836	65.4 (3.0) N = 647
Texas	25.1 (1.1) N = 1923	76.0 (1.3) N = 1733	53.3 (1.4) N = 3208	80.3 (1.5) N = 2746	71.9 (1.6) N = 2199
Utah	.2 (.2) N = 226	77.8 (3.8) N = 232	57.8 (4.4) N = 302	90.7 (1.7) N = 410	73.8 (3.3) N = 281
Vermont	53.0 (7.9) N = 50	84.3 (4.0) N = 91	56.1 (9.0) N = 82	87.5 (5.2) N = 122	72.0 (6.2) N = 84
Virginia	.2 (.2) N = 492	.1 (.1) N = 671	N = 0	89.5 (1.3) N = 1212	69.8 (2.5) N = 897
Washington	87.0 (1.5) N = 782	83.5 (2.1) N = 731	75.4 (2.2) N = 1153	90.5 (1.5) N = 1168	74.8 (2.4) N = 885
West Virginia	0 (0) N = 196	77.9 (3.1) N = 214	64.3 (4.8) N = 272	77.1 (4.5) N = 271	72.0 (4.2) N = 224
Wisconsin	3.3 (2.6) N = 30	87.3 (1.6) N = 584	69.9 (2.6) N = 900	88.9 (1.8) N = 933	82.9 (2.1) N = 771
Wyoming	0 (0) N = 54	87.2 (5.1) N = 47	68.5 (11.4) N = 73	81.6 (8.4) N = 105	88.5 (4.6) N = 57

Note: Turnout Measured as Hajnal, Lajevardi, and Nielson do in Table 1: using sample weights, dropping respondents who self-classify as being unregistered, and dropping respondents who do not match to a voter file record. Dark grey cells denote state-years coded as being the first year of a strict voter ID law. Light grey cells denote state-years coded as having a strict voter ID law, but it is not the first year of the law. Standard errors reported in parentheses.

Table A.2: Estimated CCES Primary Election Turnout by State and Year

State	2008	2010	2012	2014
Alabama	52.6 (3.4) N = 331	43.3 (3.0) N = 562	34.7 (3.3) N = 575	40.3 (4.2) N = 406
Alaska	67.6 (6.3) N = 67	57.1 (7.6) N = 117	48.0 (6.6) N = 101	71.3 (8.9) N = 73
Arizona	50.3 (2.4) N = 715	47.4 (2.1) N = 1331	49.7 (2.4) N = 1161	54.0 (2.5) N = 945
Arkansas	51.5 (3.5) N = 343	34.2 (3.3) N = 414	42.2 (4.8) N = 399	38.0 (4.1) N = 299
California	66.3 (1.3) N = 2275	56.0 (1.2) N = 4608	54.8 (1.4) N = 3788	54.1 (1.3) N = 3333
Colorado	29.4 (2.5) N = 471	41.8 (2.5) N = 925	28.6 (2.2) N = 841	37.3 (2.6) N = 691
Connecticut	29.9 (2.5) N = 398	32.2 (2.7) N = 671	26.2 (2.8) N = 473	16.4 (2.7) N = 397
Delaware	44.2 (5.2) N = 107	40.5 (5.8) N = 193	27.2 (4.1) N = 192	15.8 (3.7) N = 132
Florida	49.0 (1.4) N = 1883	40.9 (1.2) N = 3910	42.9 (1.5) N = 3008	40.3 (1.5) N = 2497
Georgia	54.1 (2.3) N = 742	34.7 (1.9) N = 1519	36.6 (2.2) N = 1345	34.1 (2.3) N = 1038
Hawaii	42.6 (6.9) N = 71	58.7 (6.5) N = 146	69.2 (6.1) N = 135	73.9 (6.2) N = 105
Idaho	34.0 (5.0) N = 155	33.6 (5.0) N = 252	39.1 (4.4) N = 275	45.1 (5.8) N = 161
Illinois	51.3 (2.0) N = 1016	38.7 (1.6) N = 2202	42.7 (1.8) N = 1602	37.2 (1.8) N = 1478
Indiana	60.4 (2.6) N = 650	34.7 (2.1) N = 1047	41.7 (2.7) N = 824	31.6 (2.2) N = 767
Iowa	21.0 (2.1) N = 398	35.0 (3.1) N = 537	15.1 (1.8) N = 517	22.8 (2.8) N = 382
Kansas	37.3 (3.1) N = 363	41.9 (3.4) N = 496	41.4 (3.0) N = 555	46.8 (3.8) N = 335
Kentucky	48.5 (2.9) N = 398	46.6 (2.9) N = 658	23.2 (2.4) N = 667	43.8 (3.5) N = 459
Louisiana	34.0 (3.0) N = 346	44.2 (3.2) N = 566	22.4 (2.9) N = 541	0 (0) N = 373
Maine	26.5 (3.0) N = 223	43.4 (4.5) N = 311	24.7 (3.6) N = 330	23.6 (3.7) N = 209
Maryland	46.6 (2.9) N = 444	36.4 (2.5) N = 890	32.4 (2.3) N = 826	39.8 (2.6) N = 625
Massachusetts	50.3 (2.7) N = 488	29.1 (2.1) N = 913	36.5 (2.2) N = 887	39.6 (2.5) N = 718
Michigan	45.3 (2.0) N = 949	33.1 (1.7) N = 1677	46.9 (1.9) N = 1451	41.2 (2.0) N = 1227
Minnesota	26.6 (2.1) N = 537	28.6 (2.2) N = 825	26.1 (2.1) N = 823	31.3 (2.3) N = 709
Mississippi	39.4 (3.6) N = 246	6.5 (1.7) N = 348	38.3 (4.9) N = 347	34.6 (4.6) N = 249
Missouri	60.8 (2.3) N = 750	37.7 (2.2) N = 1108	46.9 (2.5) N = 969	47.2 (2.7) N = 726
Montana	59.4 (4.7) N = 170	40.5 (8.9) N = 142	59.3 (5.1) N = 200	61.6 (5.6) N = 134

Continued on next page

Table A.2 – continued from previous page

State	2008	2010	2012	2014
Nebraska	40.1 (4.0) N = 215	23.9 (4.5) N = 141	42.6 (3.5) N = 455	49.2 (4.2) N = 260
Nevada	24.3 (2.7) N = 362	42.6 (3.2) N = 555	32.6 (3.4) N = 517	33.5 (3.8) N = 378
New Hampshire	73.6 (4.0) N = 198	39.9 (4.3) N = 308	58.7 (5.0) N = 284	37.6 (4.4) N = 187
New Jersey	48.1 (2.3) N = 748	14.7 (1.4) N = 1275	21.2 (1.7) N = 1125	21.1 (1.9) N = 926
New Mexico	43.2 (3.9) N = 228	32.6 (3.5) N = 377	33.4 (4.3) N = 357	33.5 (5.3) N = 270
New York	38.9 (1.5) N = 1494	20.4 (1.2) N = 2482	9.9 (.9) N = 2109	21.7 (1.5) N = 1866
North Carolina	51.4 (2.2) N = 824	24.5 (1.7) N = 1332	55.5 (2.1) N = 1341	31.6 (1.9) N = 1085
North Dakota	40.1 (7.0) N = 87	36.9 (6.5) N = 103	76.2 (5.5) N = 71	42.2 (7.7) N = 67
Ohio	62.5 (1.8) N = 1194	41.3 (1.6) N = 2144	40.9 (1.7) N = 1638	39.6 (1.9) N = 1546
Oklahoma	56.6 (3.3) N = 383	40.8 (3.6) N = 483	44.0 (4.0) N = 506	40.5 (4.1) N = 306
Oregon	58.8 (2.8) N = 518	56.5 (3.1) N = 705	57.5 (2.6) N = 945	60.7 (2.6) N = 684
Pennsylvania	48.9 (1.5) N = 1606	41.6 (1.5) N = 2324	39.9 (1.7) N = 1725	34.8 (1.6) N = 1663
Rhode Island	45.5 (6.9) N = 92	24.0 (3.9) N = 176	35.9 (5.2) N = 195	34.2 (6.3) N = 125
South Carolina	46.0 (3.2) N = 380	34.6 (3.0) N = 589	37.7 (3.0) N = 720	38.5 (3.3) N = 512
South Dakota	45.2 (5.4) N = 119	23.5 (5.5) N = 136	29.5 (6.1) N = 131	43.8 (7.8) N = 97
Tennessee	49.4 (2.6) N = 563	37.0 (2.6) N = 848	44.3 (2.8) N = 836	43.7 (3.0) N = 647
Texas	52.1 (1.5) N = 1794	31.4 (1.2) N = 3282	31.7 (1.5) N = 2746	34.7 (1.6) N = 2199
Utah	44.9 (3.7) N = 243	27.7 (3.6) N = 321	34.8 (3.5) N = 410	18.9 (2.7) N = 281
Vermont	37.2 (5.2) N = 97	31.2 (7.6) N = 85	33.7 (7.2) N = 122	10.6 (3.8) N = 84
Virginia	.5 (.2) N = 695	N = 0	20.0 (1.7) N = 1212	5.9 (.9) N = 897
Washington	62.5 (2.3) N = 754	60.9 (2.3) N = 1165	60.8 (2.5) N = 1168	51.5 (2.4) N = 885
West Virginia	58.3 (4.1) N = 215	39.6 (4.5) N = 275	46.9 (5.1) N = 271	44.5 (5.5) N = 224
Wisconsin	62.3 (2.3) N = 594	39.4 (2.4) N = 927	56.4 (2.5) N = 933	38.0 (2.4) N = 771
Wyoming	43.2 (7.7) N = 51	60.3 (8.9) N = 76	55.4 (7.4) N = 105	72.1 (7.2) N = 57

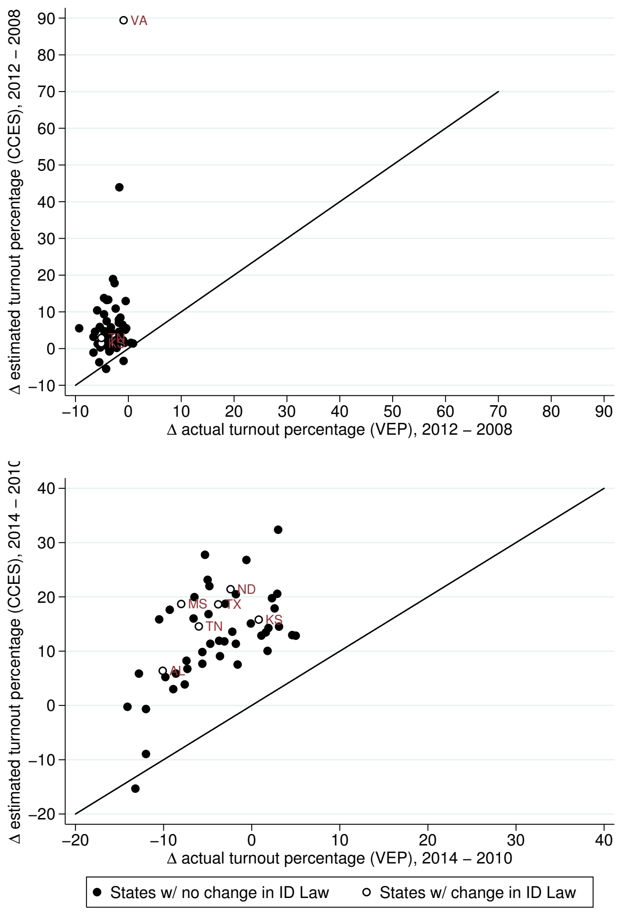
Note: Turnout Measured as Hajnal, Lajevardi, and Nielson do in Table 1: using sample weights, dropping respondents who self-classify as being unregistered, and dropping respondents who do not match to a voter file record. Dark grey cells denote state-years coded as being the first year of a strict voter ID law. Light grey cells denote state-years coded as having a strict voter ID law, but it is not the first year of the law. Standard errors reported in parentheses.

Table A.3: Comparing Actual and CCES Estimates of Turnout in Tennessee and Kentucky

	Turnout In:					**Δ Turnout:**	
	2006	2008	2010	2012	2014	2012-2008	2014-2010
Actual Turnout:							
Voting-eligible population:							
Tennessee	42.1	57.4	35.1	52.4	29.8	-5	-5.3
Kentucky	44.2	59	44.3	56.2	44.9	-2.7	.6
Difference	-2	-1.6	-9.3	-3.9	-15.1	-2.3	-5.9
Registered voters:							
Tennessee	50	66.3	41.3	61.9	36	-4.5	-5.4
Kentucky	49.5	63.9	49.1	59.8	46.4	-4.2	-2.8
Difference	.4	2.4	-7.8	2.1	-10.4	-.3	-2.6
Estimated Turnout (CCES):							
No sample weights, drop unmatched:							
Tennessee	48	74.2	60.9	80.4	60.4	6.2	-.5
	(2.3)	(1.8)	(1.6)	(1.3)	(1.8)	(2.2)	(2.5)
Kentucky	77.5	72.6	66	76.3	65.1	3.7	-.8
	(2.2)	(2.2)	(1.8)	(1.6)	(2.1)	(2.7)	(2.8)
Difference	-29.5	1.6	-5.1	4.1	-4.7	2.5	.4
	(3.2)	(2.8)	(2.4)	(2.1)	(2.8)	(3.5)	(3.7)
Bias	-27.5	3.2	4.2	8	10.4	4.8	6.2
Sample weights, drop unmatched/unregistered:							
Tennessee	49.8	79.5	50.8	82.4	65.4	2.9	14.6
	(2.7)	(2.2)	(2.8)	(2.4)	(3)	(3.3)	(4.1)
Kentucky	78.8	76.8	61.2	77.9	71.2	1	10
	(2.6)	(2.6)	(3)	(2.8)	(3.1)	(3.9)	(4.3)
Difference	-29.1	2.6	-10.4	4.5	-5.9	1.9	4.5
	(3.8)	(3.4)	(4.1)	(3.7)	(4.4)	(5.1)	(6)
Bias	-27	4.2	-1.1	8.4	9.3	4.2	10.4
Sample weights, unmatched are nonvoters:							
Tennessee	30.5	62.1	40.7	60.2	44.9	-1.9	4.2
	(2)	(2.6)	(2.5)	(2.5)	(2.4)	(3.6)	(3.4)
Kentucky	55.7	60.3	48.7	56.8	46.2	-3.5	-2.5
	(2.6)	(3)	(2.8)	(3)	(2.7)	(4.2)	(3.9)
Difference	-25.2	1.8	-8	3.4	-1.3	1.6	6.7
	(3.3)	(3.9)	(3.7)	(3.9)	(3.6)	(5.6)	(5.2)
Bias	-23.2	3.4	1.3	7.2	13.9	3.9	12.6

Note: Bias is defined as the estimated difference in the CCES minus the actual difference in the voting-age population. Actual turnout data come from reports collected from the Kentucky and Tennessee Secretary of State's websites.

Figure A.1: Plotting Changes in Actual and CCES-Estimates-of Turnout



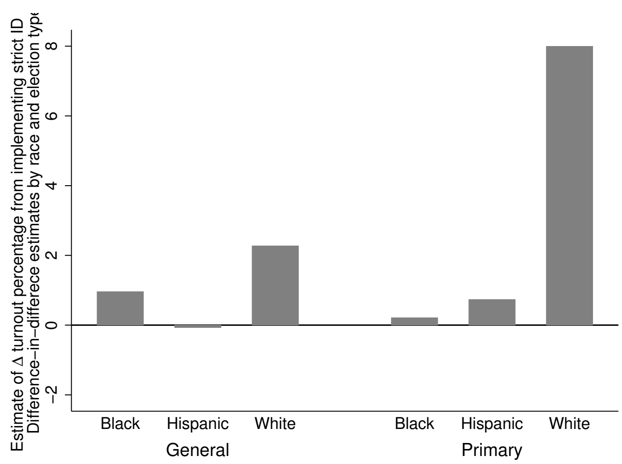
Note: CCES turnout percentage constructed using sample weights, dropping respondents who self-classify as being unregistered, and dropping respondents who do not match to the voter file. We do this to be consistent with how Hajnal, Lajevardi, and Nielson code turnout in Table 1. Actual turnout percentage calculated by dividing the number of ballots cast for the highest office on the ballot in a state-year by the estimated voting-eligible population, as provided by the United States Election Project

Table A.4: Percentage of CCES Respondents Who Do Not Match a Voter Registration Record by Race and Year

	Year of Survey:				
Racial Group	2006	2008	2010	2012	2014
All	31.7	11.2	9.7	20.5	29.9
White	29.9	10	7.5	17.7	26.7
Black	38.3	12.9	20.1	24.3	37.1
Hispanic	35.3	15.9	14.5	31.7	42.4
Asian	25.3	16	9.6	41.5	51.7
Native American	27.9	11.9	13.7	23.5	29.4
Mixed	37.2	19.1	12.7	23	34
Other	35.9	16.4	12.6	25.4	27.6
Middle Eastern	44.6	40.7	4.1	59.5	33.9

Note: Observations weighted by sample weight.

Figure A.2: Increasing Group Turnout Percentage Implied by Hajnal, Lajevardi, and Nielson, Figure 4



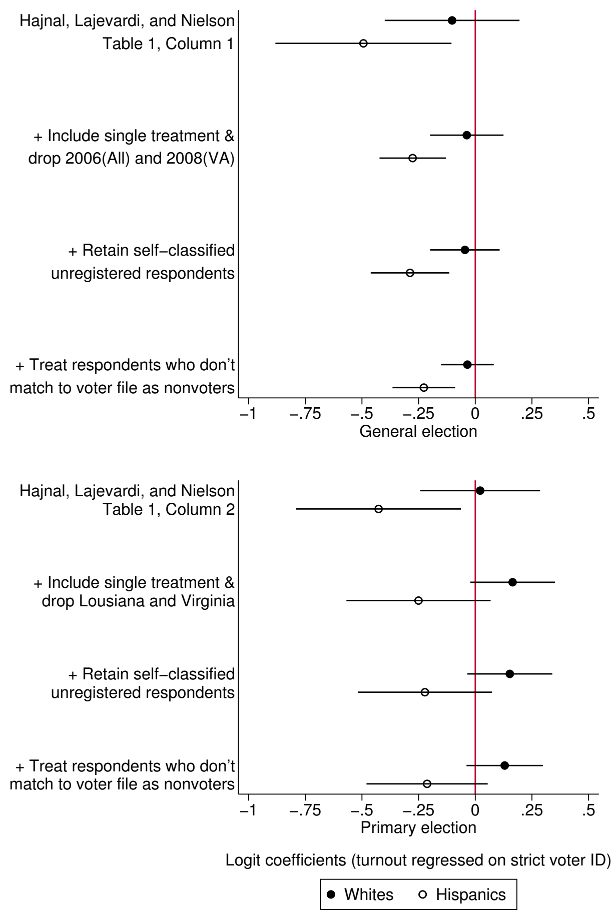
Note: This graph plots the difference-in-differences that underlie the difference-in-difference-in-difference graphed in Figure 4 of Hajnal, Lajevardi, and Nielson. This analysis does not use sample weights, keeps respondents in the sample who self classify as being unregistered, and drops respondents who do not match to a voter file record.

Table A.5: Relationship Between Future Implementation of Strict Voter ID and Turnout

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	General Elections:						Primary Elections:					
Include respondents who self-classify as unregistered	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Include unmatched respondents as non-voters	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes
Number of Observations	93,652	93,652	99,864	99,864	114,230	114,230	93,989	93,989	100,379	100,379	112,553	112,553
Future Strict Voter ID State	-0.368	-0.385	-0.344	-0.356	-0.253	-0.258	-0.070	-0.073	-0.090	-0.091	-0.084	-0.080
	(0.117)	(0.141)	(0.092)	(0.116)	(0.077)	(0.097)	(0.200)	(0.208)	(0.189)	(0.199)	(0.169)	(0.178)
Black X		0.057		0.016		-0.004		0.101		0.101		0.066
Future Strict Voter ID State		(0.134)		(0.142)		(0.122)		(0.117)		(0.126)		(0.120)
Hispanic X		0.077		0.050		0.088		-0.103		-0.132		-0.084
Future Strict Voter ID State		(0.108)		(0.118)		(0.097)		(0.103)		(0.088)		(0.085)
Asian X		0.398		0.670		0.409		-0.008		0.040		-0.086
Future Strict Voter ID State		(0.505)		(0.382)		(0.348)		(0.205)		(0.183)		(0.179)
Mixed Race X		-0.219		-0.263		-0.406		-0.832		-0.882		-0.945
Future Strict Voter ID State		(0.141)		(0.128)		(0.103)		(0.118)		(0.141)		(0.124)

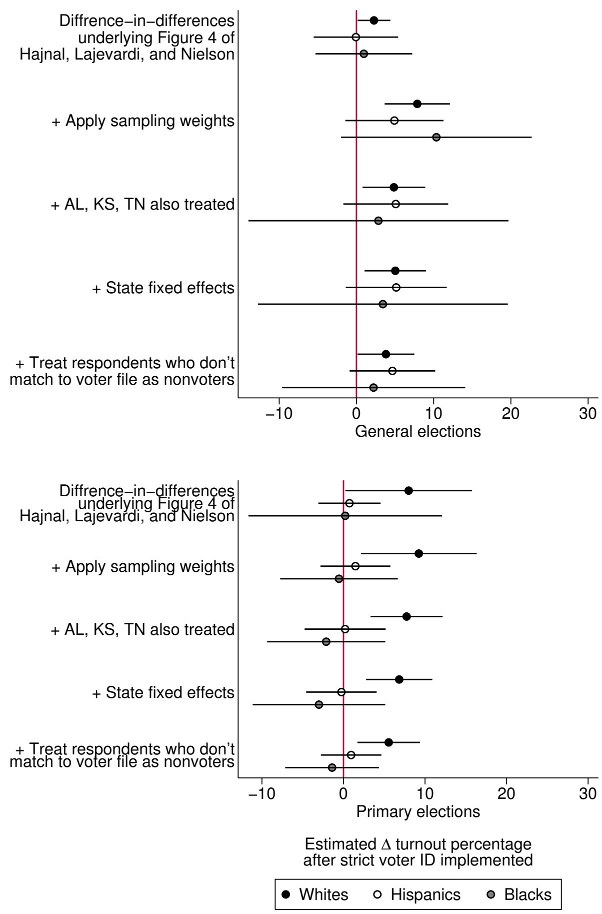
Note: Sample include all respondents in 2008. 2010, and 2012, except those from states that already implemented strict voter ID. Regressions also include all control variables listed in Table 1 of Table 1 of Hajnal, Lajevardi, and Nielson. Observations weighted by sample weights and standard errors clustered by state are reported in parentheses.

Figure A.3: Sensitivity of Estimates from Models Excluding State Fixed Effects to Alternative Specifications



Note: More details on the models producing these estimates can be found in Table A.9 in the Appendix.

Figure A.4: Sensitivity of Difference-in-Difference Models Using 2010 and 2014 Data to Alternative Specifications



Note: More details on the models producing these estimates can be found in Table A.10 (top panel) and Table A.11 (bottom panel) in our appendix.

Table A.6: Estimated Group Turnout Percentage Implied by Hajnal, Lajevardi, and Nielson, Figure A9

Racial Group	General Election	Primary Election
White/Other	10.9	6.8
	[9.4, 12.4]	[4.7, 8.8]
Black	10.4	2.5
	[8.4, 12.4]	[-.1, 5]
Hispanic	6.5	1.2
	[3.6, 9.3]	[-2.3, 4.7]
Asian	12.5	6.6
	[5.7, 19.4]	[-1.4, 14.7]
Mixed Race	8.3	3.1
	[3.8, 12.8]	[-2.3, 8.5]

Note: Point estimates represent the change in turnout following the implementation of a strict voter ID law for a given racial group and election type. 95% confidence intervals presented in brackets.

Table A.7: Alternative Specifications of General Election Turnout Models Including State Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cluster Standard Errors by State	No	Yes	Yes	Yes	Yes	Yes	Yes
Exclude First Year of Strict ID Law	No	No	Yes	Yes	Yes	Yes	Yes
Exclude 2006 and 2008-VA Data	No	No	No	Yes	Yes	Yes	Yes
Apply Sampling Weights	No	No	No	No	Yes	Yes	Yes
Include respondents who self-classify as unregistered	No	No	No	No	No	Yes	Yes
Include unmatched respondents as non-voters	No	No	No	No	No	No	Yes
Number of Observations	167,524	167,524	167,524	144,044	143,916	153,620	190,732
Strict Voter ID State	0.109	0.109	0.115	0.011	0.020	0.018	0.060
	(0.008)	(0.147)	(0.094)	(0.010)	(0.015)	(0.013)	(0.050)
Black X	-0.005	-0.005	-0.005	-0.006	-0.033	-0.024	-0.019
Strict Voter ID State	(0.008)	(0.016)	(0.017)	(0.012)	(0.019)	(0.019)	(0.018)
Hispanic X	-0.045	-0.045	-0.044	-0.045	-0.061	-0.053	-0.047
Strict Voter ID State	(0.013)	(0.017)	(0.018)	(0.022)	(0.022)	(0.026)	(0.024)
Asian X	0.016	0.016	0.016	-0.022	-0.035	-0.009	-0.043
Strict Voter ID State	(0.034)	(0.040)	(0.040)	(0.034)	(0.040)	(0.055)	(0.033)
Mixed Race X	-0.026	-0.026	-0.026	-0.026	-0.025	-0.042	-0.024
Strict Voter ID State	(0.022)	(0.033)	(0.034)	(0.034)	(0.030)	(0.047)	(0.040)

Note: All models include all other variables included in Table A9, Column 1 in Hajnal, Lajevardi, and Nielson. Result in Column 1 replicate this model exactly.

Table A.8: Alternative Specifications of Primary Election Turnout Models Including State Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cluster Standard Error by State	No	Yes	Yes	Yes	Yes	Yes	Yes
Exclude First Year of Strict ID Law	No	No	Yes	Yes	Yes	Yes	Yes
Exclude 2006 and 2008-VA Data	No	No	No	Yes	Yes	Yes	Yes
Apply Sampling Weights	No	No	No	No	Yes	Yes	Yes
Include respondents who self-classify as unregistered	No	No	No	No	No	Yes	Yes
Include unmatched respondents as non-voters	No	No	No	No	No	No	Yes
Number of Observations	146,683	146,683	146,683	142,254	142,119	151,886	184,261
Strict Voter ID State	0.068	0.068	0.078	0.035	0.054	0.048	0.033
	(0.010)	(0.065)	(0.043)	(0.022)	(0.021)	(0.021)	(0.015)
Black X	-0.043	-0.043	-0.044	-0.050	-0.069	-0.061	-0.047
Strict Voter ID State	(0.010)	(0.022)	(0.022)	(0.021)	(0.026)	(0.026)	(0.021)
Hispanic X	-0.056	-0.056	-0.055	-0.064	-0.071	-0.058	-0.034
Strict Voter ID State	(0.016)	(0.022)	(0.022)	(0.021)	(0.027)	(0.029)	(0.028)
Asian X	-0.001	-0.001	-0.001	-0.031	-0.084	-0.048	-0.024
Strict Voter ID State	(0.040)	(0.044)	(0.044)	(0.041)	(0.042)	(0.036)	(0.029)
Mixed Race X	-0.037	-0.037	-0.037	-0.049	-0.050	-0.057	-0.047
Strict Voter ID State	(0.026)	(0.035)	(0.036)	(0.037)	(0.034)	(0.030)	(0.025)

Note: All models include all other variables included in Table A9, Column 2 in Hajnal, Lajevardi, and Nielson. Result in Column 1 replicate this model exactly.

Table A.9: Alternative Specifications of Models Excluding State Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent Variable	**General Election Turnout**					**Primary Election Turnout**				
Exclude First Year of Strict ID Law	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Exclude 2006 and 2008-VA Data	No	No	Yes	Yes	Yes	No	No	No	No	No
Exclude Louisiana and Virginia Data	No	No	No	No	No	No	No	No	Yes	Yes
Include respondents who self-classify as unregistered	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Include unmatched respondents as non-voters	No	No	No	No	Yes	No	No	No	No	Yes
Number of Observations	167,396	167,396	143,916	153,620	190,732	146,548	146,548	142,119	151,886	184,261
Strict Voter ID State	-0.102	-0.057	-0.037	-0.045	-0.035	0.022	0.097	0.165	0.152	0.130
	(0.148)	(0.128)	(0.081)	(0.076)	(0.058)	(0.132)	(0.112)	(0.093)	(0.093)	(0.084)
Black X	-0.112	-0.102	-0.161	-0.125	-0.104	-0.397	-0.385	-0.384	-0.365	-0.341
Strict Voter ID State	(0.102)	(0.102)	(0.106)	(0.103)	(0.085)	(0.116)	(0.117)	(0.113)	(0.117)	(0.112)
Hispanic X	-0.391	-0.333	-0.239	-0.242	-0.192	-0.448	-0.360	-0.415	-0.375	-0.342
Strict Voter ID State	(0.119)	(0.163)	(0.102)	(0.121)	(0.092)	(0.121)	(0.130)	(0.120)	(0.119)	(0.106)
Asian X	-0.219	-0.195	-0.172	-0.067	-0.345	-0.637	-0.603	-0.687	-0.452	-0.606
Strict Voter ID State	(0.210)	(0.204)	(0.200)	(0.272)	(0.196)	(0.250)	(0.251)	(0.257)	(0.217)	(0.211)
Mixed Race X	-0.225	-0.212	-0.116	-0.225	-0.122	-0.309	-0.290	-0.290	-0.314	-0.324
Strict Voter ID State	(0.144)	(0.151)	(0.163)	(0.222)	(0.182)	(0.181)	(0.185)	(0.193)	(0.161)	(0.148)

Note: All models include all other variables included in Table 1, Columns 1 and 2 in Hajnal, Lajevardi, and Nielson. Results in Column 1 replicate Table 1, Column 1 exactly and results in Column 6, replicate Table, Column 2 exactly. Observations weighted by sample weights and standard errors clustered by state are reported in parentheses.

Table A.10: Alternative Specifications of Difference-in-Difference-in-Difference General Election Turnout Models

	(1)	(2)	(3)	(4)	(5)
Apply Sampling Weights	No	Yes	Yes	Yes	Yes
Include AL, KS, and TN as States Implementing Strict Voter ID (2010 -2014)	No	No	Yes	Yes	Yes
Include State Fixed Effects	No	No	No	Yes	Yes
Include unmatched respondents as non-voters	No	No	No	No	Yes
Observations	80,406	80,286	80,286	80,286	103,996
State Implemented Strict Voter ID (2010 - 2014)	-0.053	-0.087	-0.085		
	(0.018)	(0.035)	(0.028)		
Year == 2014	-0.023	0.159	0.159	0.159	0.004
	(0.010)	(0.012)	(0.013)	(0.013)	(0.015)
State Implemented Strict Voter ID (2010 - 2014) X Year == 2014	0.023	0.079	0.049	0.050	0.038
	(0.011)	(0.021)	(0.020)	(0.020)	(0.018)
Hispanic Respondent	-0.248	-0.278	-0.282	-0.315	-0.310
	(0.014)	(0.015)	(0.016)	(0.014)	(0.012)
State Implemented Strict Voter ID (2010 - 2014) X Hispanic Respondent	-0.023	0.027	0.033	0.043	0.043
	(0.021)	(0.034)	(0.027)	(0.019)	(0.017)
Hispanic Respondent X Year == 2014	0.001	0.021	0.020	0.020	0.009
	(0.022)	(0.028)	(0.028)	(0.026)	(0.021)
State Implemented Strict Voter ID (2010 - 2014) X Hispanic Respondent X Year == 2014	-0.023	-0.030	0.002	0.001	0.008
	(0.022)	(0.032)	(0.035)	(0.034)	(0.026)
State Implemented Strict Voter ID (2010 - 2014) X Black Respondent	-0.182	-0.179	-0.177	-0.174	-0.212
	(0.011)	(0.016)	(0.017)	(0.016)	(0.013)
State Implemented Strict Voter ID (2010 - 2014) X Black Respondent	-0.012	-0.058	-0.049	-0.045	-0.039
	(0.024)	(0.046)	(0.044)	(0.045)	(0.033)
Black Respondent X Year == 2014	0.034	-0.013	-0.007	0.000	0.032
	(0.010)	(0.011)	(0.010)	(0.010)	(0.011)
State Implemented Strict Voter ID (2010 - 2014) X Black Respondent X Year == 2014	-0.013	0.025	-0.020	-0.016	-0.016
	(0.029)	(0.077)	(0.076)	(0.072)	(0.056)

Note: Column 1 replicates the results presented in Figure 4 in Hajnal, Lajevardi, and Nielson. All regressions include self-classified unregistered respondents and drop all respondents who do not identify as white, Hispanic, or black. Standard errors clustered by state are reported in parentheses.

Table A.11: Alternative Specifications of Difference-in-Difference-in-Difference Primary Election Turnout Models

	(1)	(2)	(3)	(4)	(5)
Apply Sampling Weights	No	Yes	Yes	Yes	Yes
Include AL, KS, and TN as States Implementing Strict Voter ID (2010 -2014)	No	No	Yes	Yes	Yes
Include State Fixed Effects	No	No	No	Yes	Yes
Include unmatched respondents as non-voters	No	No	No	No	Yes
Observations	81,407	81,281	81,281	81,281	103,996
State Implemented Strict Voter ID (2010 - 2014)	-0.069	-0.078	-0.042		
	(0.047)	(0.040)	(0.031)		
Year == 2014	-0.100	0.010	0.008	0.017	-0.062
	(0.015)	(0.013)	(0.013)	(0.011)	(0.010)
State Implemented Strict Voter ID (2010 - 2014) X Year == 2014	0.080	0.092	0.077	0.068	0.055
	(0.039)	(0.035)	(0.022)	(0.020)	(0.019)
Hispanic Respondent	-0.233	-0.214	-0.215	-0.266	-0.249
	(0.012)	(0.014)	(0.014)	(0.026)	(0.023)
State Implemented Strict Voter ID (2010 - 2014) X Hispanic Respondent	0.005	0.037	0.009	0.071	0.063
	(0.040)	(0.036)	(0.025)	(0.030)	(0.027)
Hispanic Respondent X Year == 2014	0.075	0.081	0.086	0.084	0.070
	(0.021)	(0.023)	(0.023)	(0.019)	(0.014)
State Implemented Strict Voter ID (2010 - 2014) X Hispanic Respondent X Year == 2014	-0.073	-0.078	-0.075	-0.071	-0.046
	(0.036)	(0.038)	(0.033)	(0.030)	(0.028)
State Implemented Strict Voter ID (2010 - 2014) X Black Respondent	-0.208	-0.171	-0.170	-0.161	-0.167
	(0.014)	(0.016)	(0.016)	(0.016)	(0.015)
State Implemented Strict Voter ID (2010 - 2014) X Black Respondent	-0.020	-0.009	-0.012	-0.022	-0.022
	(0.017)	(0.023)	(0.023)	(0.020)	(0.019)
Black Respondent X Year == 2014	0.099	0.042	0.046	0.062	0.071
	(0.013)	(0.018)	(0.018)	(0.018)	(0.014)
State Implemented Strict Voter ID (2010 - 2014) X Black Respondent X Year == 2014	-0.078	-0.098	-0.099	-0.098	-0.069
	(0.024)	(0.018)	(0.027)	(0.028)	(0.019)

Note: Column 1 replicates the results presented in Figure 4 in Hajnal, Lajevardi, and Nielson. All regressions include self-classified unregistered respondents and drop all respondents who do not identify as white, Hispanic, or black. Standard errors clustered by state are reported in parentheses.