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

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Abstract

Widespread concern that voter identification laws suppress turnout among racial and ethnic minorities has made empirical evaluations of these laws crucial. But problems with administrative records and survey data impede such evaluations. We replicate and extend Hajnal, Lajevardi and Nielson (2017), which reports that voter ID laws decrease turnout among minorities, using validated turnout data from five national surveys conducted between 2006 and 2014. We show that the results of the paper are a product of data inaccuracies; the presented evidence does not support the stated conclusion; and alternative model specifications produce highly variable results. When errors are corrected, one can recover positive, negative, or null estimates of the effect of voter ID laws on turnout, precluding firm conclusions. We highlight more general problems with available data for research on election administration and we identify more appropriate data sources for research on state voting laws' effects.

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Requiring individuals to show photo identification in order to vote has the potential to curtail voting rights and tilt election outcomes by suppressing voter turnout. But isolating the effect of voter ID laws on turnout from other causes has proved challenging (Highton 2017). States that implement voter ID laws are different from those that do not implement the laws. Even within states, the effect of the laws is hard to isolate because 85 to 95 percent of the national voting-eligible population possesses valid photo identification, [1] so those with ID dominate over-time comparisons of state-level turnout. Surveys can help researchers study the turnout decisions of those most at-risk of being affected by voter ID, but survey-based analyses of voter ID laws have their own challenges. Common national surveys are typically unrepresentative of state voting populations, and may be insufficiently powered to study the subgroups believed to be more affected by voter ID laws (Stoker and Bowers 2002). And low-SES citizens, who are most affected by voter ID laws, are less likely to be registered to vote and respond to surveys (Jackman and Spahn 2017), introducing selection bias.

The problems of using survey data to assess the effect of voter ID laws are evident in a recent article on this subject, Hajnal, Lajevardi and Nielson (2017) (HLN hereafter). HLN assesses voter ID using individual-level validated turnout data from five online Cooperative Congressional Election Studies (CCES) surveys, 2006-2014. HLN concludes that strict voter ID laws cause a large turnout decline among minorities, including among 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" (368).[2] HLN implies that voter ID laws represent a major impediment to voting with a disparate racial impact.

In this article, we report analyses demonstrating that the conclusions reported by HLN are unsupported. HLN use survey data to approximate state-level turnout rates, a technique

[1] See "Issues Related to State Voter Identification Laws." 2014. GAO-14-634, U.S. Government Accountability Office; Ansolabehere and Hersh (2016).

[2] HLN also examine the relationship between voter ID laws and Democratic and Republican turnout rates. Here, we focus on minority turnout because of its relevance under the Voting Rights Act.

we show to be fraught with measurement error due to survey nonresponse bias and variation in vote validation procedures across states and over time. HLN's CCES-based turnout measures, combined with a coding decision about respondents who could not be matched to voter files, produce turnout estimates that differ substantially from official ones.

Using a placebo test that models turnout in years prior to the enactment of voter ID laws, we show that the core analysis in HLN, a series of cross-sectional regressions, does not adequately account for unobserved baseline differences between states with and without these laws. In a supplementary analysis, HLN include a difference-in-differences (DID) model to estimate within-state changes in turnout, a better technique for removing omitted variable bias. This additional analysis asks too much of the CCES data, which is designed to produce nationally representative samples each election year, not samples representative over time within states. In fact, changes in CCES turnout data over time within states bear little relationship to actual turnout changes within states. After addressing errors of specification and interpretation in the DID model, we find that no consistent relationship between voter ID laws and turnout can be established using the HLN CCES data.

Use of National Surveys for State Research

The CCES is widely used in analysis of individual-level voting behavior. The CCES seems like a promising resource for the study of voter ID laws because it includes self-reported racial and ethnic identifiers, variables absent from most voter files. But the CCES data are poorly suited to estimate state-level turnout for several reasons. First, even large nationally representative surveys have few respondents from smaller states, let alone minority groups from within these states.[3] Unless a survey is oversampling citizens from small states and minority populations, many state-level turnout estimates, particularly for minorities, will be extremely noisy. Second, Jackman and Spahn (2017) find that many markers of

[3]For example, 493 of the 56,635 respondents on the 2014 CCES were from Kansas, only 17 and 24 of whom are black and Hispanic, respectively.

socioeconomic status positively associate with an individual being absent both from voter registrations rolls and consumer databases. The kind of person who lacks an ID is unlikely to be accurately represented in the opt-in online CCES study.

Third, over-time comparisons of validated voters in the CCES are problematic because the criteria used to link survey respondents to registration records have changed over time and vary across states. Table A.4 shows that the percentage of respondents who fail to match to the voter registration database increased from about 10 percent in 2010 to 30 percent in 2014. The change in the number of unmatched Hispanics is even starker, increasing from 15 to 42 percent over the same time period. The inconsistency in the CCES vote validation process is relevant to the analysis of voter ID because it generates time-correlated measurement error in turnout estimates.

These features of the CCES data, as well as several coding decisions in HLN, make HLN's turnout measures poor proxies for actual turnout. To demonstrate this, Figure 1 reports a cross-sectional analysis comparing "implied" turnout rates in HLN—the rates estimated for each state-year when using HLN's coding decisions—to actual state-level turnout rates as reported by official sources. While this figure measures overall statewide turnout, note that the problems we identify here likely would be magnified if we were able to compare actual and estimated turnout by racial group. We cannot do so because few states report turnout by race.

Figure 1 (panel 1) shows that HLN's estimates of state-year turnout often deviate substantially from the truth. If the CCES state-level turnout data were accurate, we should expect only small deviations from the 45-degree line. In most state-years, the HLN data overstate the share of the voters by about 25 percentage points, while in 15 states, HLN's rates are about 10 points below actual turnout.[4] Many cases in which turnout is severely

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

underestimated are from jurisdictions that were not properly validated. Many jurisdictions were not validated with turnout in the 2006 CCES. Virginia was not validated until 2012.[5] Respondents who claimed to have voted in such jurisdictions were coded as not matching to the database, and hence dropped, while those who claim not to have voted remained in the sample. As a consequence, HLN's analysis assumes a turnout rate of close to zero percent. Given the limitations of the vote validation, we contend that neither 2006 data anywhere, nor Virginia's records from 2008, should be included in any over-time analysis.[6]

As the upper-right panel shows, once the 2006 data and Virginia 2008 data are excluded, HLN almost always substantially overestimate turnout in a state-year. One potential reason for this overestimation is because HLN drop observations that fail to match to the voter registration database. This contrasts with Ansolabehere and Hersh's (2012) recommendation that unmatched respondents be coded as non-voters. Being unregistered is the most likely reason why a respondent would fail to match. The bottom left panel of Figure 1 shows that when respondents who fail to match to the voter database are treated as non-voters rather than dropped, CCES estimates of turnout more closely match actual turnout. One way to assess the improvement is to compare the R^2 when CCES estimates of state-level turnout are regressed on actual turnout. We find that the R^2 increases from 0.36 to 0.58 when we code the unmatched as non-voters.[7] The R^2 further increases to 0.69 when we weight observations by the inverse of the sampling variance of CCES turnout in the state, suggesting that small sample sizes limit the ability of the CCES to estimate turnout in smaller states.[8]

The CCES data might be salvageable here if errors were consistent within each state. Unfortunately, as the bottom right panel of Figure 1 shows, within-state changes in turnout

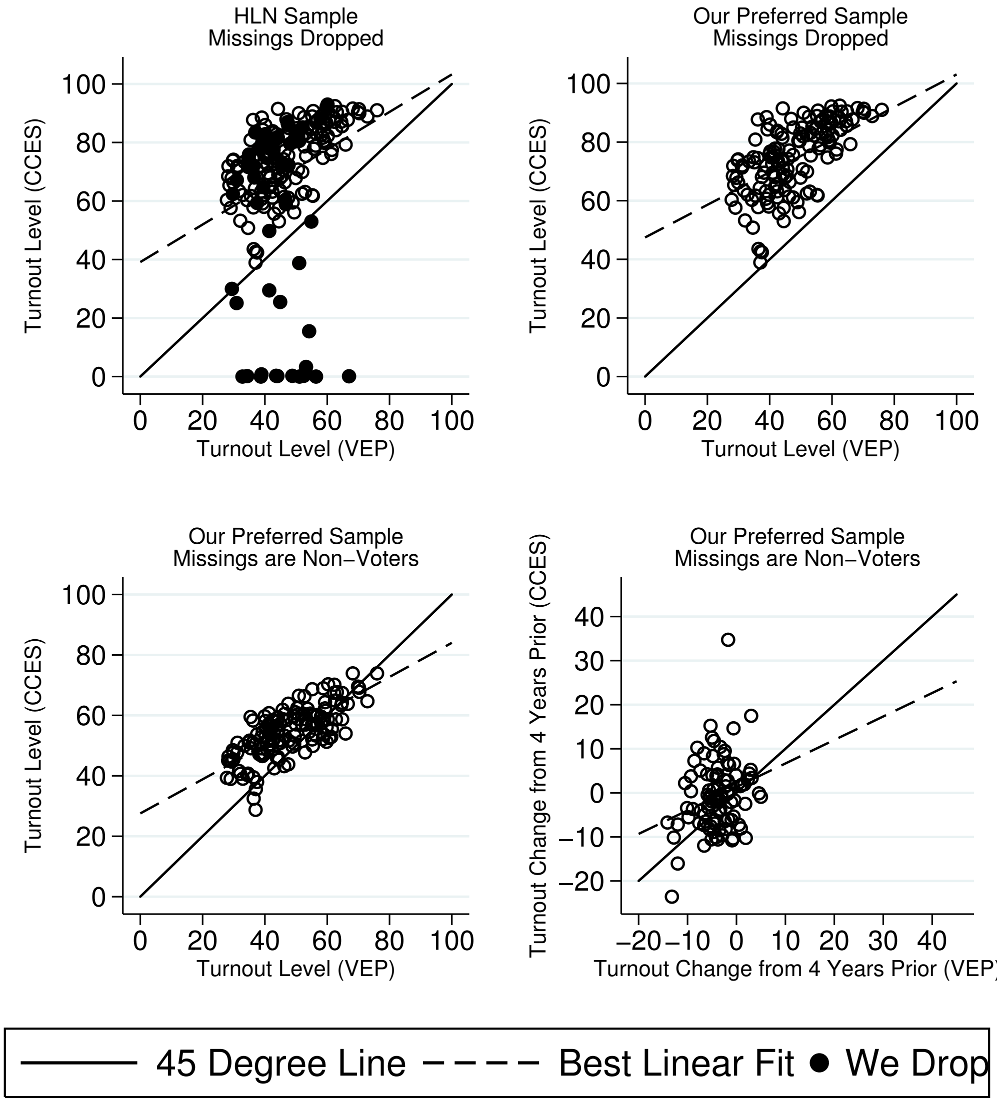
[5]Due to a state policy in Virginia that was in effect through 2010, CCES vendors did not have access to vote history in that state. HLN correctly code Virginia's turnout as missing in 2010, but code nearly all Virginia CCES respondents as non-voters in 2008.

[6]We also exclude primary election data from Louisiana and Virginia for all years based on inconsistencies highlighted in Table A.2.

[7]In addition, the mean-squared error declines from 9.0 to 5.8.

[8]In addition, the mean-squared error declines from 5.8 to 4.9.

Figure 1: Measurement Error in HLN's State-Level Turnout Estimates



Note: HLN turnout percentage is calculated to be consistent with how turnout is coded in HLN Table 1, meaning that we apply sample weights, drop respondents who self-classify as being unregistered, and drop respondents who do not match to a voter file record. Actual turnout percentage is 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.

as measured in the CCES have little relationship to within-state changes in turnout according to official records. The R^2 is less than 0.15 when we regress the change in CCES turnout between elections on the actual change in turnout between elections, (dropping bad data, coding unmatched as missing, and weighting by the inverse of the sampling variance).[9] This means the overwhelming share of the within-state variation in turnout in the CCES is noise.

No definitive source exists on turnout by race by state and year; however, Figure A.2 in the Appendix shows weak relationships between the racial gaps estimated in the CCES and the Current Population Survey (CPS), a common resource in the study of race and turnout. For Hispanics, there is an insignificant negative relationship between the racial gap in the CCES and CPS in a state-year. In contrast, there is a positive association between the difference in white and black turnout in the CPS and the CCES. These findings are consistent with the claim that the sample issues in the CCES are magnified when looking at racial heterogeneity in turnout within a state.

While the CCES is an important resource for individual-level turnout research (e.g., Fraga 2016) it is problematic when repurposed to make state-level inferences or inferences about small groups (Stoker and Bowers 2002). The data are particularly problematic when the analysis requires the use of state fixed-effects to reduce concerns of omitted variable bias, because the small sample within states makes within-state comparison noisy. The survey data and coding decisions used in HLN inject substantial error into state-level estimates of voter turnout. While this error can be reduced with alternative coding decisions, a substantial amount of error is unavoidable with these data.

Estimating Voter ID Laws' Effects on Turnout

Imperfect data do not preclude a useful study, and social scientists often rightly choose to analyze such data rather than surrender an inquiry altogether. In light of this, we now

[9] Figure A.1 separates the within state change between the presidential elections in 2008 and 2012 and the midterm elections in 2010 and 2014, and shows there is a stronger relationship between CCES estimates and actual turnout change for the later than the former.

replicate and extend the analysis in HLN. We highlight and attempt to correct specification and interpretation errors in HLN. Our goal is to assess whether improving the estimation procedures can yield meaningful and reliable estimates of voter ID laws' effect. We find no clear evidence about the effects of voter ID laws.

Cross-sectional comparisons. A central concern in the study of voter ID laws' impact is omitted variable bias: states that did and did not adopt voter ID laws systematically differ on unobservable dimensions that also affect turnout. To address the systematic differences, HLN presents a series of cross-sectional regressions that include a host of variables meant to account for confounding factors. In these regressions, an indicator variable for existence of a strict ID law in a state in each year is interacted with the respondent race/ethnicity. The main weakness of this approach is clearly acknowledged in HLN: the causal effect of voter ID laws is identified only if all relevant confounders are assumed included in the models.

We report results of a placebo test meant to assess the plausibility of this assumption by applying the HLN cross-sectional regression models to turnout in the period before ID laws were enacted. Table A.5 in our appendix presents estimates from this placebo test using nearly the same specification that HLN report in their Table 1, Column 1.[10] The interpretation of the coefficient on the voter ID treatment variable is voter ID laws' effect before their adoption in states *that had not yet implemented strict voter ID laws* relative to states *which never implemented such a law*, after adjusting for the same individual-level and state-level variables used in HLN. The results presented in Table A.5 suggest that voter ID laws "caused" turnout to be lower at baseline in states where they had yet to be adopted. The failure of the placebo test implies that HLN's cross-sectional regressions fail to account

[10]There are two main differences. First, we do not include states that previously implemented strict voter ID. Second, our treatment variable is an indicator for whether the state will implement a strict voter ID law by 2014. We also omit 2006 data due to the data problems cited above, and 2014 data because, after applying the above restrictions, no states that implemented a voter ID law by 2014 remain in the sample. By defining the treatment this way we necessarily drop the authors' indicator variable for a state being in the first year of its voter ID law.

for baseline differences across states.

Within-state analyses If cross-state comparisons are vulnerable to unobserved confounders, perhaps a within-state analysis could yield more accurate estimates of a causal effect. That's why HLN report a supplementary model (HLN Appendix Table A9) with state and year fixed effects (i.e., a difference-in-differences (DID) estimator) meant to address this issue.[11] The main text of HLN notes that this is "among the most rigorous ways to examine panel data," and 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," (p.375).

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.[12] 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 are only *relatively* lower compared to the large positive effects estimated for the other groups. Compared to most turnout effects reported in prior work, these effects are also implausibly large (Citrin, Green and Levy 2014).

In addition to Table A9, HLN Figure 4 presents estimates from simple bivariate difference-

[11] In an email exchange Hajnal, Lajevardi and Nielson asserted 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, this is the result of the included covariates removing Virginia from the analysis. Even if we stipulate to this design, we still find that the reported effect estimates are sensitive to the model specification, coding decisions, and research design.

[12] 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 in order to replicate the published results. HLN provided replication code for their appendix, but the estimated model from that code does not produce the estimates reported in Table A9.

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 demonstrating that voter ID laws generally suppress turnout.[13] Our replication produces no consistent evidence of suppressed turnout. Figure A.3 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.

Importantly, the difference between a law that suppresses turnout for minorities versus one that increases turnout for minorities but does so less than for whites is very important for voting rights claims, since claims under Section 2 of the VRA are focused on laws resulting in the "denial or abridgement of the right...to vote on account of race or color."

Improved analysis, inconclusive results. HLN contains additional data processing and modeling errors which we attempt to correct in order to determine whether an improved analysis leads to more robust results. Without explanation, HLN includes in their DID model 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 after the first year of implementation. In this model, the interactions with racial groups are harder to interpret since they are not also interacted with the "first year"

[13]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.

indicator.[14] There are also a number of inconsistencies in model specifications.[15][16][17][18]

Figure 2 presents the treatment effect estimates implied by the data and fixed-effects model in HLN Table A9, as well as alternative estimates after we address the modeling and specification concerns. For clarity and brevity, we focus on effects among white and Hispanic voters only.[19] 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 the previous observation that models of this sort are underpowered to adjudicate between plausible effect sizes of voter ID policy (Erikson and Minnite 2009).[20]

We find similar patterns when we examine the robustness of the results presented in HLN's Figure 4.[21] In no specification do we find that primary or general turnout significantly

[14]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 codedin their data as having a strict ID law since 2006. HLN also never code "First year of strict law" in Virginia, even though Virginia implemented a strict ID law in 2011, according to the HLN data. Research provides no clear suggestions on the direction of a "new law effect." When a law is first implemented, people must adjust to the law and obtain IDs, additionally depressing turnout, but such laws also often induce a counter mobilization that can be strongest in the first years after passage Valentino and Neuner (2016).

[15]For example, HLN reports standard errors clustered at the state level in the main analysis, but not in the appendix analysis. Standard errors need to be clustered by state because all respondents in a state are affected by the same voter ID law, and failing to cluster would likely exaggerate the statistical precision of subsequent estimates. Many state-level attributes 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.

[16]Based on our replications, it also appears that sampling weights were only used in 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. 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.

[17]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.

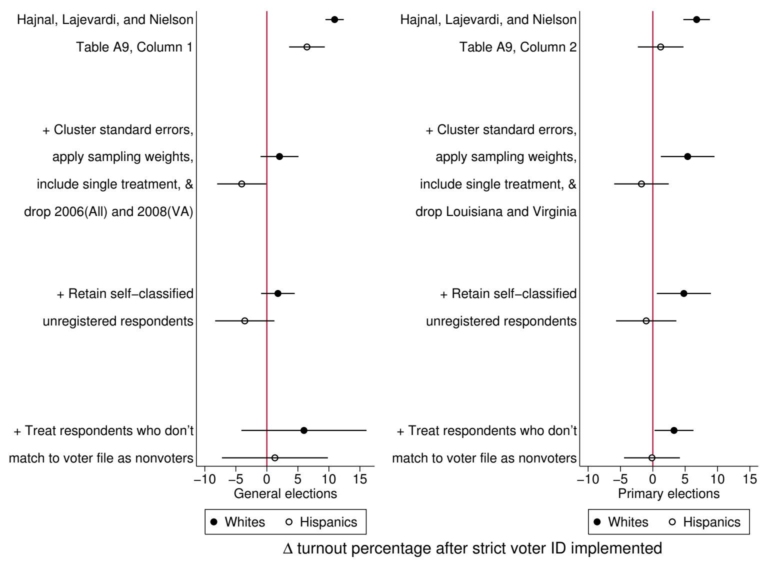
[18]An additional concern is that in HLN's models of primary election turnout control for competitiveness using a measure of general election competitiveness rather than primary competitiveness. If the model is meant to mirror the general election model, it should include a control for primary competitiveness,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

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

[20]In addition, these confidence intervals do not account for uncertainty in model specification and multiple testing. We maintain HLN's statistical model for comparability.

[21]See Figure A.5, 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.

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. Several specifications suggest that white turnout increased, particularly in primary elections. But we suspect that this is largely due to the data errors we identified, as actual returns indicate that overall turnout declined in these states relative to the rest of the country.[22]

Implications for Future Research

Our analysis shows that national surveys are ill-suited for estimating the effect of state

[22] In our appendix, Figure A.4 and Table A.9 present our tests of 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. While this replication is consistent with HLN's initial findings, we do not find it credible since our previous analysis shows the vulnerability of the pooled cross-sectional to omitted variable bias.

elections laws on voter turnout. While augmented national survey data have useful applications, they have limited use in this context. The CCES survey used in HLN is not representative of hard-to-reach populations (such as people lacking photo IDs), and many of the discrepancies we identify are due to substantial year-to-year differences in measurement and record linkage. These data errors are sufficiently pervasive—across states and over time—that standard techniques cannot recover plausible effect estimates.

Our results may explain why the published results in HLN deviate substantially from other published findings of a treatment effect of zero, or close to it (Citrin, Green and Levy 2014; Highton 2017). 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 actually *refute* the claim that voter ID laws suppress turnout. Finally, our attempts to address measurement and specification issues still fail to produce the robust results required to support public policy recommendations. Using these data and this research design, we can draw no firm conclusions about the turnout effects of strict voter ID laws.

Problems specific to the CCES have been discussed here, but similar problems are sure to appear in the context of any survey constructed to be representative at the national level. One key implication of our work is that distributors of survey data should provide additional guidance to researchers. The CCES does not presently offer users clear enough guidelines for how to use features like validated vote history, including how to deal with over-time variation in the vote-validation procedures and in data quality. Given the existing evidence, researchers should turn to data that allow more precision than surveys offer. Such measures could include voter databases linked 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 more financial investments and partnerships with governments, the stakes are high enough to warrant additional investment.

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

Continued on next page

Table A.1 − continued from previous page

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

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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: Relationship Between CCCS - Actual Turnout and State-Level Variables in HLN

	(1)	(2)	(3)	(4)	(5)	(6)
Exclude 2006 and 2008-VA Data	No	No	Yes	Yes	Yes	Yes
Include unmatched respondents as non-voters	No	No	No	No	Yes	Yes
State Fixed Effects	No	Yes	No	Yes	No	Yes
Number of Observations	248	248	197	197	197	197
Strict photo voter ID law (1 = yes)	-1.34	9.33	1.71	2.45	1.09	1.60
	(2.81)	(11.32)	(1.46)	(3.17)	(1.34)	(3.28)
Senate election year (1 = yes)	-1.78	0.23	1.30	1.83	1.34	1.46
	(2.21)	(2.33)	(0.63)	(0.79)	(0.75)	(0.89)
Gubernatorial election year (1 = yes)	4.14	2.47	3.09	1.86	2.38	1.51
	(2.66)	(2.88)	(1.03)	(1.29)	(1.02)	(1.15)
Registration deadline (# days)	0.16	-0.21	0.15	-0.02	0.16	-0.00
	(0.13)	(0.32)	(0.08)	(0.08)	(0.07)	(0.10)
Early in-person voting (1 = Yes)	7.20	20.11	4.94	9.59	4.82	4.48
	(3.72)	(14.04)	(1.80)	(2.04)	(1.90)	(2.48)
Vote-by-mail state (1 = Year)	3.66	-9.90	5.04	5.76	5.44	1.55
	(6.46)	(9.64)	(1.76)	(2.34)	(1.97)	(2.63)
No-excuse absentee voting (1 = Yes)	-4.23	-19.11	-1.10	-1.84	-2.17	0.66
	(3.22)	(8.10)	(1.36)	(1.28)	(1.73)	(1.37)
Margin in most recent presidential election	2.97	-4.59	22.95	20.02	12.05	19.15
	(13.41)	(31.28)	(6.47)	(13.61)	(4.66)	(14.64)

Note: All specification include year fixed effects. Robust standard errors clustered by state reported in parentheses.

Figure A.1: Measurement Error Within States over Time

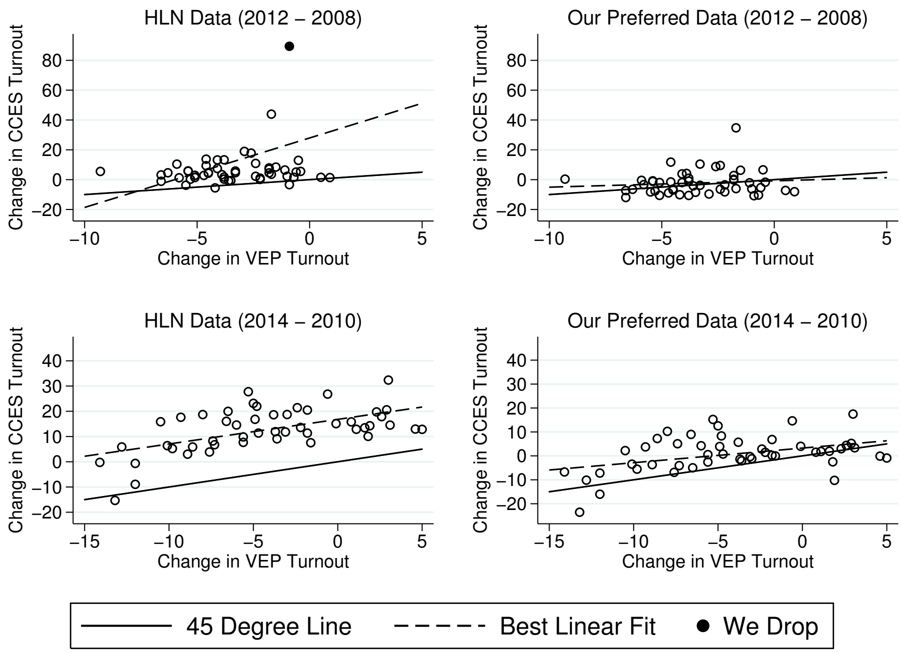
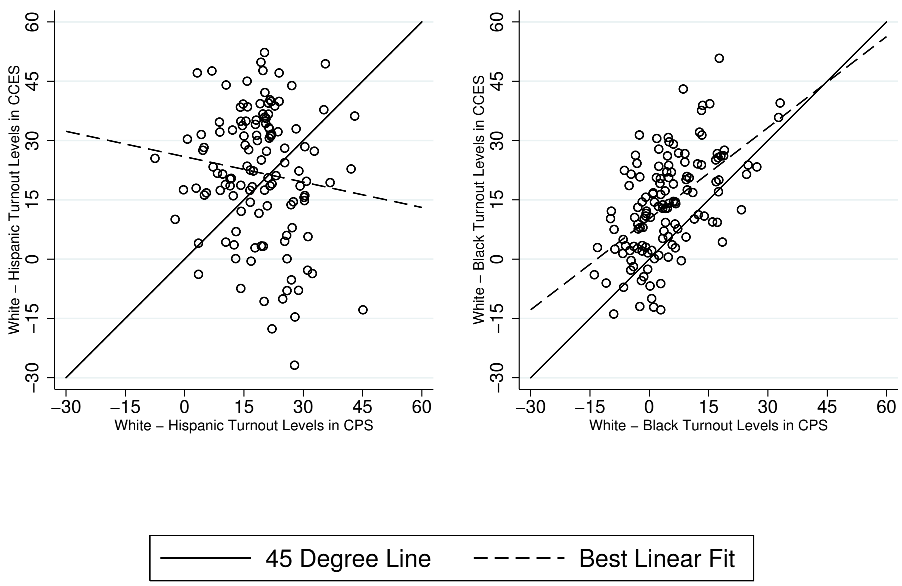


Table A.4: Percentage of CCES Respondents Who Do Not Match a Voter Registration Record by Race and Year

	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: Comparing Racial Gaps in the CPS and CCES



Note: CPS turnout by race constructed from the P20 detailed tables found at `https://www.census.gov/topics/public-sector/voting.html`. White, Hispanic, and black turnout is taken from "White non-Hispanic alone", "Hispanic (of any race)", and "Black alone or in combination" rows, respectively. The CPS only report turnout rates when a sufficient population of a minority group resides in a state. This figure include 125 and 132 state-year observations in which a turnout rate was reported Hispanics and blacks, respectively.

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)

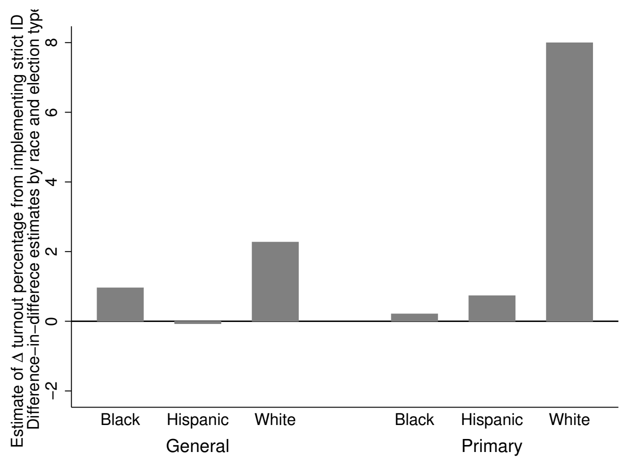
Note: Sample include all respondents in 2008. 2010, and 2012, except those from states that already implemented strict voter ID. Regressions also include all control variables listed in Table 1 of Table 1 of Hajnal, Lajevardi, and Nielson. Observations weighted by sample weights and standard errors clustered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

Table A.6: Estimated Group Turnout Percentage Implied by HLN, 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.

Figure A.3: Increasing Group Turnout Percentage Implied by HLN, 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.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)

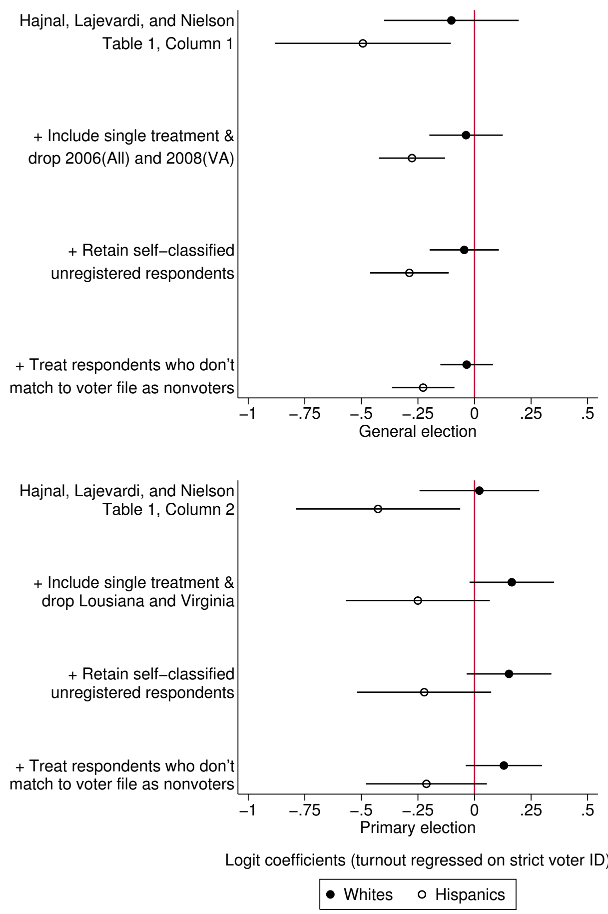
Note: All models include all other variables included in Table A9, Column 1 in Hajnal, Lajevardi, and Nielson. Result in Column 1 replicate this model exactly.

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.

Figure A.4: Sensitivity of Estimates from Models Excluding State Fixed Effects to Alternative Specifications



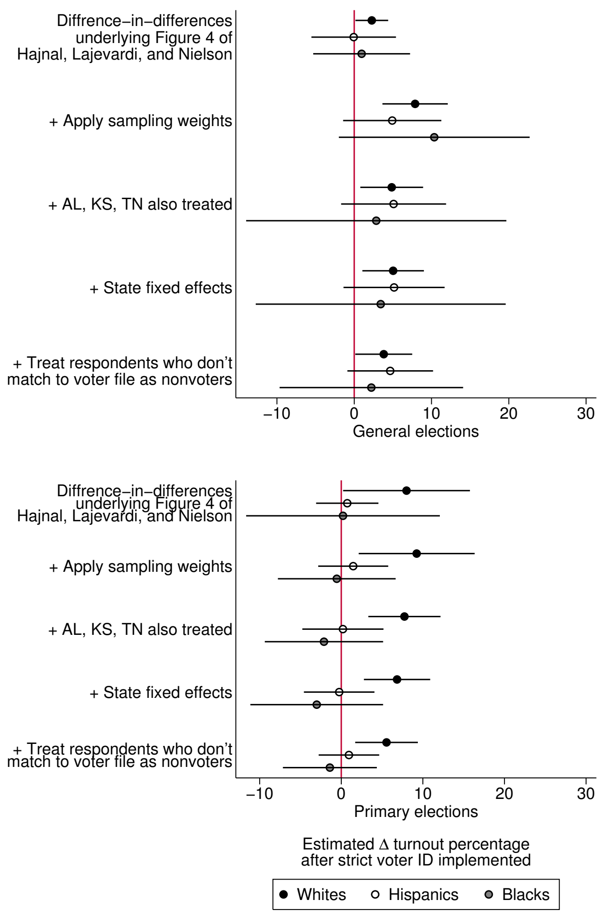
Note: More details on the models producing these estimates can be found in Table A.9 in the Appendix.

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.

Figure A.5: 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.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.