

Modeling Problems in the Voter ID-Voter Turnout Debate

Abstract

In April 2008, the U.S. Supreme Court upheld Indiana's controversial new voter identification law. Adopted in 2005, the law requires voters to show a current, government-issued photo identification. Opponents worry the new rules will place an undue burden on the voting rights of elderly, low income and minority voters, disputing the need for these laws. Nevertheless, over the last five years, new voter ID requirements have been adopted on party line votes in more than a dozen states. Stimulated by the pressing policy debate, recent scientific research on the turnout question is largely inconclusive: different datasets, measurement rules and statistical models produce different and contradictory findings. Voter identification requirements do not appear to affect voter turnout in a straightforward way, and the complexity of electoral rules and how they influence voting behavior make statistical outcomes quite sensitive to research designs. We see problems with existing designs. Our paper evaluates recent research and investigates the strength of the statistical arguments used to support findings. We use difference-in-difference techniques and Current Population Survey data to test for an impact in the states, comparing turnout in the 2002 and 2006 midterm elections. Our findings suggest that the data are not up to the task of making a compelling statistical argument.

Robert S. Erikson
Columbia University

Lorraine C. Minnite
Barnard College

Paper Prepared for the Eighth Annual Conference on State Politics and Policy
Elections and Representative Democracy in the States
Temple University
Philadelphia, Pennsylvania
May 30 – 31, 2008

Introduction

In a widely reported story of the 2008 primary in Indiana, twelve elderly nuns were turned away from their resident convent polling place by a fellow sister because they failed to comply with the state's new voter identification (ID) law (Hastings 2008a; 2008b; Gordon 2008; Martelle 2008). The week before, the Supreme Court upheld the controversial law which compels citizens in Indiana to show a current government-issued photo ID in order to vote.¹ As voter registration surged in anticipation of the primary (Jacobs and Burns 2008; Voter Registration Numbers 2008), voting rights advocates worried that new or vulnerable voters would not have their votes counted for failure to present the appropriate ID. In the end, however, despite record turnout, there were few official reports of instances of vote denial (Indiana Secretary of State 2008), leading defenders of stricter voter ID laws feeling vindicated (Hastings 2008c). And yet, important questions remain, prompted by concerns like those expressed by the (*Muncie, IN*) *Star Press* three days after the primary:

While only 20 provisional ballots were cast in Tuesday's election – and not all of them because of a lack of ID – it is unknown how many were turned away from the polls by inexperienced polls (sic) workers, but there is anecdotal evidence it happened. [One disabled] veteran, for example, wasn't given a provisional ballot in Precinct 23 until a mob of voters outside demanded it, going so far as to ask a Democratic party official to come to the polling place ("Indiana Voter ID Law Disenfranchised Some" 2008) .

This vignette from the Hoosier State presents a puzzle for both the courts that may hear future voter ID disputes, and for social science upon which lawyers, judges and advocates in voting rights cases often rely. Do voter ID laws deter voting? Do the data and instruments we have allow us to detect what may be only marginal negative influences on voting from a single voting rule? Courts need to know in order to better evaluate the nature of the "burden" the rules

may inflict on the right to vote. The problem is the silence in the available data. Until the current controversy, political scientists did not investigate the relationship between ID requirements and voting, and for good reason: six years ago only 11 states required all voters to present documentary proof of their identity before casting a ballot at the polls (Electionline.org 2006). That number has since more than doubled to 24 (Project Vote 2007). At the same time, while these laws are rhetorically defended as anti-fraud and voter confidence measures, none of the legislative sponsors of voter ID bills have made any credible showing of voter fraud to justify the need for more ballot security.² Troubling to many legal scholars, courts upholding voter ID laws show little sensitivity to these important empirical lapses and the questions they raise about whether voter fraud is indeed a problem or if voter ID laws will depress voting. This strongly suggests that going forward, the debate over voter ID will be difficult to settle as a simple matter of fact.

To be generous, we could conclude that politicians have tightened voter ID laws on the faith that they are, as Indiana election officials put it, only “a party-neutral, good-government reform...” (Brief of State Respondents 2007, 37). Unfortunately, the politics surrounding the statehouse slugfest over the voter ID issue suggests something else. Of the ten voter ID bills introduced by Republican state legislators between 2005 and 2007, 95.3 percent of the 1,222 Republicans, but just 2.1 percent of the 796 Democrats voting on these measures supported them (Brief of Amici Curiae 2007, 28). Given the long history of partisan maneuvers to win elections by excluding certain voters under the guise of “good government” election reform (Kousser 1974; Piven and Cloward 2000), the effects of voter ID laws on voting deserve much more scientific scrutiny. In the vacuum of evidence, the perception of a party advantage in tightening up voter ID requirements drives the debate. The question is whether the data we have are up to

the task of finding what may be a needle – e.g., 12 elderly nuns in South Bend, Indiana – in a haystack.

If not, at least history and common sense provide good reasons to believe that stricter requirements for proving identity and eligibility at the polls could disproportionately burden the poorest, most vulnerable and least educated Americans (Brief of Amici Curiae 2007). To help us better understand the trade-offs, this paper first evaluates the findings of recent efforts to address some of the gaps in our knowledge on the relationship between voter ID rules and turnout. The results of these efforts, none yet published in peer reviewed journals, are largely inconclusive: different datasets, measurement rules and statistical models produce different and contradictory findings. Voter ID requirements do not affect voter turnout in a straightforward way and the complexity of electoral rules and how they influence voting behavior make statistical outcomes quite sensitive to research designs. Next, to advance the debate by addressing what we see as problems with current research designs, we analyze the strength of the statistical arguments used to support findings, and offer an alternative approach. We conclude with some thoughts for future directions in research.

Literature Review

Researchers analyzing whether voter ID laws influence turnout have approached the question in three ways. Several studies construct statistical models to test for relationships between the “stringency” or restrictiveness of voter ID requirements and voter turnout levels, looking for any disproportionate effects among different groups of voters (Lott 2006; Eagleton Institute 2006; Vercellotti and Anderson 2006; Mulhausen and Sikich 2007; Mycoff et al. 2007; Alvarez et al. 2007; Milyo 2007; Logan and Darrah 2008). Others conduct surveys or match lists

to estimate the proportion of the electorate that lacks ID and to examine whether the possession of ID varies among groups (Brace 2005; Pawasarat 2005; Brennan Center 2006; Hood and Bullock 2007; Barreto et al. 2007a; 2007b). A third approach uses survey data to assess attitudes among voters toward stricter voter ID (Pastor et al. 2008; Ansolabehere and Persily 2008). There are two different assumptions tested using in this last approach. The first considers the strength of public support for voter ID as a rationale behind these laws. The second argument frames voter ID laws as a remedy for a general lack of confidence in electoral administration, hypothesizing that as public confidence increases so too will turnout.

Our paper is only concerned with the first approach to the question of voter ID laws and turnout effects, and with statistical models using Current Population Survey (CPS) data to measure turnout (Eagleton Institute 2006; Vercellotti and Anderson 2006; Mulhausen and Sikich 2007; Alvarez et al. 2007).

The statistical work on voter ID laws clusters around research commissioned in 2005 by the U.S. Elections Assistance Commission and performed by the Eagleton Institute of Politics at Rutgers University and the Moritz College of Law at Ohio State University (and hereafter referred to as the “Eagleton Institute” study). Eagleton Institute researchers looked for statistical relationships between the stringency of voter ID laws and turnout in the 2004 presidential election using two different datasets. The first is a file with election and demographic variables aggregated at the county level, the other, an extract of voters from the Current Population Survey and November Supplement which asks questions about voter participation in the general election.

Statistical tests for causal relationships require the conversion of information into quantities or measures. This usually presents challenges for the empirical researcher.

Measurement error in statistical modeling distorts the results and undermines the analysis. Given the complexity of voter ID rules in the different states, the Eagleton Institute researchers faced challenges constructing their measures of ID law stringency. The least stringent ID rules in place simply require voters to state or sign their names, where in some states, signatures then are matched to those on file in the poll books. The more stringent ID laws on the books for the 2004 election required voters to present some kind of documentary evidence of their identity to poll workers, with the most restrictive of these laws limiting the range of acceptable documents to only those issued by a government, and/or including a current photograph and address of the voter. However, with the notable exception of Indiana, even those states with the most restrictive requirements allow voters to vote a regular rather than provisional ballot without the required ID if the voter signs an identity affidavit. The states thus provide exceptions to the rules which in turn complicates the data reduction involved in statistical analysis.

To address this problem, the Eagleton Institute researchers constructed two variables for the stringency of ID rules, one, a maximum scale that captures the range of rules for the most voters could be asked to do or show at the polls, the other, a minimum scale that measures the least voters would be required to do or show to vote. Both the minimum and maximum measures are constructed as five-point scales: the maximum scale measures stringency in the following way: ‘state name,’ ‘sign name,’ ‘match signature,’ ‘provide ID,’ ‘provide photo ID’. The minimum scale is similar: ‘state name,’ ‘sign name,’ ‘match signature,’ ‘provide ID,’ ‘sign an identity affidavit.’ Both scales were constructed and tested in two ways, as continuous variables and as a set of discrete dichotomous variables for each level of stringency.

The Eagleton Institute researchers then constructed other variables to control for demographic factors (i.e., age, race, education and income) and political context (i.e., a

competitive election), factors known to influence voter turnout. At the county level, the statistical evidence showing a negative causal relationship between the stringency of a state's voter ID requirements and voter turnout is very weak.³

The evidence is a little stronger when the analysis is performed using individual level data. Probit regression models show negative effects on turnout for all levels of ID requirements beyond stating one's name, with the exception of match signature requirements (which are not statistically significant). The Eagleton Institute researchers note, "these effects translated into reduced probabilities of voting of about 3 to 4 percent for the entire sample, with larger differences for specific subgroups" (Eagleton Institute 2006, 29). The group whose turnout appears to suffer most in these models is Latinos.⁴ The predicted probability of Latino voters casting a ballot in states requiring a non-photo ID is 10 percentage points lower than it is for Latinos in states only requiring voters state their names. The non-photo ID requirement is most consistently associated with reduced probabilities of voting for all racial groups in states that require more than stating one's name to affirm identity at the polls.

There are, however, potential problems with this study's design. In its continuous form, the minimum ID variable may be mis-specified, which may explain why tests of correlations suggested to the researchers that the relationship between ID and turnout may not be linear (Eagleton Institute 2006, 23). It is not clear why signing an identity affidavit is at the top of the scale, considered more difficult to meet than showing a non-photo ID. The Eagleton Institute researchers hypothesize that by signing an affidavit, the voter is made more aware of the criminal penalties for misrepresentation of one eligibility to vote and for illegal voting. They argue that meeting the threshold by signing an identity affidavit is the equivalent of a being able to provide a photo ID.⁵

A stronger argument may be made for dividing the states into just two groups, those with rules that require voters only to attest to their eligibility, versus those whose rules require voters substantiate their eligibility by bringing identity documents (with or without photos, with or without current address information and expiration dates, etc.) to the polls. The difference between those voters with the ability and where-with-all to bring identity documents to the polls and those without that capacity is likely larger than the difference between voters able to sign their names versus those able to sign and match their names to a signature on file. It may also be larger than the difference between those voters only able to bring non-photo ID to the polls and those who possess a photo ID. For voters, it stands to reason that rules mandating they produce identity documents at the polls may be the tipping point between voting and not voting. Being able to bring the documents to the polls implies being able to acquire them in the first place, which itself may necessitate levels of education and literacy not required to vote.⁶ For these reasons, having to show any identity documents to cast a polling place ballot may be the single most important ID criteria relevant to the decision to make or not make the trip.

Logan and Darrah test this hypothesis with pooled CPS turnout data for five election years (1996, 1998, 2000, 2002, and 2004). Instead of using a categorical variable to capture the different voter ID regimes in the states, they collapse the categories into just two, whether or not a state requests documentary evidence at the polls. Using logistic regression, and controlling for a range of individual and contextual predictors, they estimate the odds of registering and voting for different racial groups. For whites, living in a state with a voter ID requirement over the 1996-2004 period has a positive effect on the likelihood of registering (increasing the odds by 15 percent), but not for racial minorities. This boost in registration is all but canceled out by the depressing effect of voter ID on white voting rates, reducing the odds of voting among this group

by some 10 percent. For blacks and Latinos living in states with an ID requirement, the odds of voting are even worse, by 14 percent for blacks and 20 percent for Latinos. There is no statistical significant effect on voting among Asians. Therefore, for three of four racial groups, requesting documentary proof of eligibility to vote reduces the odds of voting. Logan and Darrah found no effects on the elderly (70 years of age or more) or on naturalized citizens.

The most prominent test of the Eagleton Institute study's findings can be found in a paper by Muhlhausen and Sikich (2007) of the conservative policy think tank, the Heritage Foundation. Once Muhlhausen and Sikich make what they contest are corrections and improvements to the Eagleton Institute models, the statistical significance of the negative relationship between ID stringency and turnout in the individual level data disappears.

Muhlhausen and Sikich argue the Eagleton Institute study is flawed in two important ways. First, they make good arguments for a variety of corrections to the coding of variables, including race and income, and point out the miscoding of a couple of states on the ID requirements.⁷ Second, they criticize the Eagleton researchers' use of a one-tailed test of statistical significance, and by implication, the hypothesis that the only relationship between voter ID and turnout is a negative one.

When Muhlhausen and Sikich re-run the Eagleton maximum requirements model correcting for the mis-coding of Arizona and Indiana,⁸ the statistical significance of Eagleton's finding that requiring a photo ID may have a slightly negative impact on turnout disappears. However, it returns when Muhlhausen and Sikich correct for the misclassification of race and income, add a homeownership variable and revise the residency requirement variable from six to 12 months. A small, negative but statistically significant influence of requiring the voter sign an identity affidavit in lieu of a photo ID is obtained in the corrected model, as well. This suggests

that the identity affidavit is as burdensome as requiring a photo ID and challenges our hypothesis that the critical distinction is between requiring voters attest to their eligibility versus substantiate it providing a document to a poll worker. But we do not feel Muhlhausen and Sikich's findings here are in any way definitive.⁹

Muhlhausen and Sikich raise important questions about the Eagleton Institute study's findings, pointing out flaws with the coding and design of the statistical analysis, but then they add some problems of their own. Both studies, in fact, suffer from specification and measurement problems.

For example, Muhlhausen and Sikich suggest the relationship between ID requirements and turnout could vary in a positive direction, but they offer no rationale for this thesis.¹⁰ We have yet to see any credible research or evidence supporting the notion that distrust in the security afforded by the strictest voter ID laws is a viable explanation for non-voting. Given what political scientists know about the determinants of non-voting, it seems unlikely voters stay home because they worry about voter fraud.¹¹ On the other hand, implausibility itself is not a reason to exclude this hypothesis from the analysis since there could be other reasons why turnout might increase in the face of stricter ID laws.

Muhlhausen and Sikich cloud their own findings, however, by adding variables like additional measures of marriage status to the Eagleton Institute models without any reason, except perhaps because adding them de-stabilizes the Eagleton Institute regression equations. Without a good rationale for these modeling decisions, the Muhlhausen and Sikich's model re-specification is unreliable.

Finally, Muhlhausen and Sikich mislead in their conclusions when they ignore an important finding from their own statistical analysis. They sum up their findings on whether

there could be differential impacts of ID laws on minority voters by pointing out that “minority respondents in states that required *photo* ID are just as likely to report voting as are minority respondents from states that only required voters to say their name (emphasis added)” (Muhlhausen and Sikich 2007, 22). This is correct but misleading because Muhlhausen and Sikich do find statistically significant relationships between ID and turnout. Controlling for a host of potentially influential variables, in states that require some form of a non-photo ID to vote, Muhlhausen and Sikich find that blacks and Latinos are less likely to report voting than blacks and Latinos in states where all that is required of registered voters is that they give their names to poll workers. It’s difficult to judge the degree to which the suggested corrections and alternative modeling offered by Muhlhausen and Sikich improve the Eagleton Institute study’s models. Some of their analyses of the shortcomings of the Eagleton Institute study’s models are sensible and reasonable, others deserve more theoretical justification. However, even with all the adjustments Muhlhausen and Sikich still find what the Eagleton Institute researchers found when it comes to a racially discriminatory effect of documentary proof of ID requirements. They, like the Eagleton Institute researchers find a statistically significant and negative influence on black and Latino turnout when voters must produce documentary proof of identity at the polls. The effect appears small, but its persistence even after all the model tinkering is noteworthy.

Alvarez et al. (2007) offer yet another variation on a theme and the most statistically sophisticated treatment of the voter ID-voter turnout modeling problem to date. They examine state-level turnout models for two presidential elections, 2000 and 2004, and models of turnout based on individual-level data for the four federal elections held between 2000 and 2006. They make further refinements to the measurement of state voter ID laws, expanding the five-level

variables developed by the Eagleton researchers by adding two categories that distinguish whether a particular form of ID is required or only requested. The Eagleton researchers handled this problem by creating two scales, one measuring increasingly burdensome levels of voter identity verification up to requiring a photo ID, the other the minimum ID the state could require if a voter was unable to comply. The approach by Alvarez et al., raises questions about whether ‘requested ID’ is relevant or properly coded if failing to produce it does not prevent an eligible voter from casting a regular ballot (because it can be superseded by some other form of verification of the voter’s identity, i.e., an identity affidavit). If this is the case, there could be problems with the way Alvarez et al. have measured the ID variable.

The authors test two hypotheses: 1) states with increasingly stringent requirements are more likely to experience a reduction in turnout among registered voters; and 2) ID requirements will have a negative impact on turnout among black and Latino registered voters compared to whites. Through the use of a multilevel binary logit model they attempt to provide a solution to some of the problems that have plagued studies analyzing the effects of electoral rules on voter turnout. Their multi-year modeling allows them to address the impact of changes in voter ID laws over time and across states, an improvement over tests that only examine effects in any one election like the Eagleton study and its replication by Muhlhausen and Sikich.¹²

As Alvarez et al. explain, multilevel (hierarchical) models allow researchers to examine variation across space and time and are especially well suited for cases where independent variables influence each other and/or are embedded in hierarchical categories (Keder and Shively 2005). Moreover, multilevel models allow the researcher to estimate the effects of independent variables where circumstances vary, as is the case with state ID requirements across the nation over time. Furthermore, as Alvarez et al. note, the nature of their data is hierarchical, varying at

the individual (i.e., educational achievement) and state (i.e., different laws and multiple forms of implementation) levels (Jusko and Shively 2005).¹³

A second design innovation in the Alvarez et al. study is the way they use a random effects model to reduce sparse data problems, and “shrink” or mathematically transform the effects of different voter ID regimes on turnout to fit a linear model (rather than assuming ID requirements have an additive effect on turnout).

Alvarez et al. first conduct an aggregate analysis for the two presidential elections in 2000 and 2004, using election returns and demographic statistics from various government sources aggregated at the state level. They estimate the impact of state ID laws on turnout among registered voters and conclude that the effects of voter ID laws on turnout are insignificant. At the individual level of analysis, Alvarez et al. employ three models to compare the effects of state ID requirements on voter participation. In the first model, the voter ID explanatory variable is not assumed to have increasing values. In the second model, the variable has a linear effect, that is, the value of the variable increases in accordance with the stringency of state ID laws. The third approach combines the first two and computes a weighted average to allow the data to suggest a relationship to the dependent variable. From this analysis, they conclude that voter ID requirements have a linear effect on turnout. This is one of the most interesting findings in the study. That is, increasingly stringent requirements, for example, from ‘match signature,’ to ‘show photo ID,’ to ‘require photo ID,’ have an increasingly negative impact on turnout across different categories of voters.¹⁴ The authors pick up a small negative effect for the strictest forms of ID requirements. Their efforts to examine whether these effects are more pronounced for minority voters produce little evidence of that, but their models here may be flawed by lumping together all “non-white” groups.¹⁵ Moreover, like the other studies

reviewed so far, this one should but does not use a fixed effects model to control for with-in state ID rules changes over time.

The Challenge

We return to the questions at hand. Do voter ID laws suppress turnout? Is their effect particularly severe among certain disadvantaged groups whose erasure from the electorate could tilt the partisan outcome? As social scientists can we document the effect from analyzing the usual turnout data, such as from the CPS?

Let us accept the first two claims, while admitting that the effects must be small, a finding of the research reviewed above. For the sake of argument let us pull some numbers out of the hat as generous conjectures about the short-term effects of a draconian voter ID law. First, assume that when a state goes from no ID required to the demand for a government issued photo ID, something like two percent of the registered electorate becomes disenfranchised. Of this two percent, three out of four would have voted, which (we assume) is the same rate as those with the required photo IDs. Thus, of the original electorate, 98 percent show up to vote displaying their IDs, while two percent either are intimidated by the law to stay home or are refused when they show up at the polls. Let us also assume that if they could vote, our suddenly ineligible voters would split one-sidedly as 80 percent Democratic versus 20 percent Republican. Before disfranchisement, our missing two percent would add $.02 \times .80$ to the Democratic vote or $.016$. This is $.06$ above what they would contributed if they split a neutral 50-50. Now, if, say, the 98 percent with their photo IDs split as evenly as 49.5 percent Democratic and 50.5 percent Republican, our missing voters could make the difference if they voted ($.98 \times .495 + .02 \times 80 = .4851 + .016 = .5011$).

If these numbers are approximations of what politicians believe, then on partisan grounds alone, the battle is worth waging in terms of whether voter ID requirements can be a decisive factor in close elections. (In effect, our hypothetical numbers would mean that the decisive partisan threshold for the Democratic party goes from 50 percent to 50.5 percent of the two-party vote.) Given our fake numbers, many would see a normative imperative as well, with keeping voters enfranchised outweighing the possible phantom of voter fraud.

But our question here is different. If two percent of the eligible electorate go missing due to voter-ID disfranchisement, are our instruments truly capable of detecting it? In asking these questions we must be wary not only of false negatives (as when researchers claim they find evidence that ID laws have no effect) but also of false positives (as when researchers claim they find convincing evidence that voter ID laws do matter).

If we can estimate the effect of voter ID laws with evidence available today, the best data source would seem to be the U.S. Census's post-election turnout surveys – the Current Population Survey's voter supplements collected every other November. CPS respondents are asked whether they are registered and, if registered, whether they voted in the recent national election. Pooled over several election years, the CPS survey contains literally hundreds of thousands of respondents for analysis.

Here, we analyze the CPS data for 2006 (the most recent election) and 2002 (the most recent midterm election comparable to 2006). Our methodology will be a reliance on the basic technique of difference-in-differences, where we ask whether the change in the dependent variable varies as a function of the change in the treatment. Our dependent variable is the turnout among registered voters, estimated in the CPS. Our treatment or independent variable is the presence or absence of new laws restricting turnout enacted between the 2002 and 2006

elections. For possible controls we have the characteristics of the individual voters in the CPS survey. For units, the appropriate level is the set of 50 states plus the District of Columbia. Thus, while using a large survey with multiple thousands of respondents, this data is ultimately collapsed to aggregates involving large state samples of voters.

We do not claim that our methodology is the only one worthy for this task or even the best. But it does illustrate how the task of estimating the effects of voter ID laws is truly daunting. The handicaps are obvious. We start with the expectation that any effect is small as we search for a possible missing two percent of the registered electorate. And even though we can observe treatments in the form of new voter ID laws enacted between 2002 and 2006, these are mostly mild innovations – not the widespread demand for photo-IDs. Here, the expected effect is even smaller. In addition, we have the handicaps that come with working with voter surveys. Although this tendency may be minimal in the context of the non-political CPS survey, people do lie to pollsters, exaggerating their voting histories. Perhaps the biggest hurdle of all, we must ask whether the undocumented voters who are otherwise eligible and registered are fully represented in even well-run census surveys?

Finally, despite the fact that CPS surveys include thousands of respondents, the effective number of cases is not in terms of survey respondents but in terms of states that generate the treatments of voter ID policy. This is a central lesson of this paper. Now, having listed the arguments against finding anything, let us turn to the data.

Research Design

At one level, our goal is to estimate the possible effects of voter ID laws by means of a difference-of-difference test applied to 2002 and 2006 voter participation data. Difference in

difference analysis simply is the current econometric term for comparing change scores for different treatment groups. Especially in a non-experimental setting, it is helpful to control for additional sources of variation in the dependent variable. The more controls, the less the concern about spurious relationships. And the more the extraneous sources of variance are controlled, the more similar are the treatment groups in terms of the independent variable of interest. Limiting the unexplained variance facilitates the statistical power of the comparisons across treatment groups. With group level treatments, it is important to take into account that the clustering of the group level effects. The appropriate degrees of freedom for estimating the standard error of the group treatment effect is the number of groups, not the number of subjects (e.g., potential voters) across groups. At the same time, gains can be made by controlling for individual characteristics (such as the demographic traits of CPS respondents). The classic statement is by Moulton (1986, 1990); see also Donald and Lang (2007).

Specifically, we ask: did state-level voter participation change between these two midterm elections as a function of changes in the states' voter ID legislation? The idea is simple. The independent variable is change in legislation between the two elections. The dependent variable is change between the same two elections as a measure of voter participation among registered voters. If voter ID laws suppress turnout, the relationship should be negative: increased demands for voter ID should be associated with lower participation.

At a second level, our goal is to tell a cautionary tale, illustrating the limitations of our statistical enterprise! On the one hand, we argue for the quality of our statistical modeling. It is arguably subject to little bias and approaches the limit in how much information can be wrung from the data. On the other hand, we show that the errors in our estimates are inherently large, so that the search for small effects of voter registration legislation must be inconclusive. We

argue that one cannot yet say much about the effect of voter ID laws from studying voting participation data in the states.

Our study measures voter participation in 2002 and 2006 as the participation rate of each state's samples in the November Voter Participation supplements. With over 64,000 registered voters in each survey, the CPS provides state estimates based on more than 1,000 respondents per state. We use the CPS rather than official turnout numbers because of concerns about uneven purging of the registration rolls in the state. Whereas turnout as a percentage of the theoretically eligible is readily available from official sources at the state level (subject to some concerns about who should be included in the eligible voter denominator), the turnout rate as a function of official registration figures is more problematical.

A second reason for using the CPS – and one that motivated Alvarez et al., for example – is that the CPS survey offers controls for some individual characteristics of the state electorate. Alvarez et al., model respondents as the unit; we see states as the proper unit, but with the potential for using individual-level analysis to adjust state estimates.

The measure of legislation is the ordering of eight types of requirements for voting at the polls. Borrowed from Alvarez et al. (2007), these are, in order of increasing stringency:

0. Voter must state his/her name
1. Voter must sign his/her name in a poll-book
2. Voter must sign his/her name in a poll book and it must match a signature on file
3. Voter is requested to present proof of ID or voter registration card
4. Voter must present proof of ID or voter registration card
5. Voter must present proof of ID and his/her signature must match the signature on the ID provided

6. Voter is requested to present photo ID
7. Voter is required to present photo ID.

There are further variations, and some increments may be more severe than others. Only two states went to level 7 by 2006. One (Indiana) required a government-issued photo ID while the other (Florida) was less strict about the source. In our analysis we measure change either as the net change in the numerical value (0-7) or the presence or absence of an increase in severity. When perusing details of the data, one might keep a special eye on the two “7” states, Florida and Indiana.

The main measure of voter participation is the observed voting rate among CPS registrants. We supplement this with an adjusted rate as the mean state residual from the prediction from a set of individual-level characteristics of the respondent – based on age, education, income, race, gender, and marital status. These controls (constructed similarly but not exactly as here) play a central role in Alvarez et al.’s individual-level analysis.

Our state-level data set is displayed in Table 1. The basis for the residual measures of turnout levels is the set of individual-level equations shown in Table 2. Each respondent obtains a predicted turnout probability based on Table 2’s equations. The adjusted state turnout level then is the deviation of the observed turnout in the state sample from that predicted by Table 2’s demographic characteristics. We also observed and estimated demography-adjusted turnout levels for three subgroups: college educated with BA degrees or higher (who presumably are little affected), those with no more than a high school diploma, and grade school educated without a high school degree (who presumably are most subject to any deterrent effects of voter ID legislation).

A Wrong Path

We could proceed, misguidedly, by pursuing a cross-sectional analysis. We might even be tempted into using our 64,000-plus respondents as our units rather than our 51 states. It is worthwhile considering how we would be led astray.

Suppose we add year-specific state scores on the eight-point index of voter ID legislation to supplement the existing variables in the individual-level variable equations shown in Table 2. (The details are not shown.) The results are negative as theory would suggest. Unadjusted, the standard errors for net change in legislation produce absolute t-values of greater than 6. In other words voter ID legislation is “significant” at better than the .001 level. But apart from important and obvious endogeneity concerns that arise, we should be concerned that the reported significance level assumes the relevant degrees of freedom based on 64,000-plus cases rather than based on a modest set of 51 states. Indeed, suppose we “mistakenly” substitute the 2002 legislation numbers for the 2006 values in the 2006 equation. The t-value actually zooms to an absolute value of 11.6! Further, suppose we substitute into the model a series of randomly drawn state-level variables from a normal distribution and observe their reported significance levels based on their unadjusted standard errors. We would find the coefficients outside the bounds of the designated significance level far more often than expected.

We can readily correct the exaggerated t-values by substituting clustered standard errors where the standard errors are clustered by state. The resultant standard errors for legislative change inflate to their proper values based on the fact the relevant degrees of freedom are based on 51 states rather than 64,000-plus individual responses. The precision of the estimates for individual characteristics hold when clustering by states, but the state-level legislation variable becomes non-significant.

The intuition for this result is not always obvious. If state turnout levels varied solely based on the legislation measure (plus the individual characteristics in the equation), there would be no problem. But of course that is not true. The correct degrees of freedom is based on the number of states, not respondents (see Primo et al. 2007). If our cross-sectional exercise is conducted at the state level, with level of voter ID law severity predicting state turnout rates, the cross-sectional relationship for neither 2002 nor 2006 is “significant.”

Difference in Differences

Working with change over time alleviates the endogeneity problem. Potentially it also increases the efficiency of the estimates. This is because potentially there is less un-modeled variation in states’ change in turnout than in their level of turnout. At the same time, since the turnout estimates contain sampling error, this source of error will double when examining change scores.

The variances of the various potential dependent variables are shown in Table 3. Indeed, change scores have less variance (but only slightly so) than levels of turnout. Adjusting the state samples for sample demography also offers a slight decline in the variance to explain. The less the variance, the less will be the uncontrolled variance to be explained.

Still, the gains from the lesser variance are slight. One might be surprised that adjusting for individual characteristics of the state samples contributes so little. After all, the usual suspects – age, education, income, race, gender, marital status – all matter at the individual level. But many of them, especially gender, marital status, and age, only vary marginally at best when accounting for state-to-state differences. Moreover the state samples are sufficiently large that

adding individual-level controls contributes little. For these reason the gain from residualizing is modest.

Table 4 presents the coefficients and standard errors for the effect of change in legislation utilizing the difference-in-difference analysis. Change is measured two ways, as net change in the state score, 2002-2006, and as the presence or absence of any increase in severity. The results are shown for all voters plus three segments based on education. Results are presented with and without the adjustment for sample demographics.

Some of the results are displayed graphically in Figures 1-5. In appearance, these graphs support the hypothesis of a depressing effect on turnout. They show scatterplots overlaid with regression lines. Figures 1 and 2 show the pattern when generalizing to all registered voters. We see that whether using observed (Figure 1) or adjusted (Figure 2) turnout estimates, as one goes from low to high scores on the voter ID law index, and expected turnout declines by about the two percent. This pattern is in the range one might expect and seeming support for the suppression hypothesis.

The problem, however, is that these estimates are decidedly not significant. None of the estimates for all voters or even for the “target” non-high school educated group are close to being statistically significant. The rough pattern is that as laws become severe turnout declines but slightly. The significance levels (in the .50 range) tell us that if the null hypothesis were true (no effect), the observed pattern would just as likely be a slight increase as decrease.

One further test might offer hope of a better resolution. We observe that change in legislation has as close to zero “effect” as possible for the college educated, especially when adjusted for individual characteristics. This of course is consistent with theory, since college-educated citizens should not be easily deterred by voter ID laws. We could perform a difference-

in-differences-in difference analysis comparing the states' change among possibly vulnerable non-high school graduates compared to the change among the states' college educated. In other words, we ask whether with an increase in law severity turnout among the high school educated declines more relative to turnout among the college educated. The answer again is a pattern that is decidedly not significant. See Figures 3-5 for the data display.

Discussion

On the one hand we can observe average turnout "effects" that mimic the plausible complaint of critics. The average estimate is that going from lax to severe voter ID requirements is associated with a few percentage points less in the voting rate, as found by the Eagleton Institute study (2006), Muhlhausen and Sikich (2007), and Alvarez et al. (2007). Moreover, this decline seems contained mainly among the least educated. But that is not the lesson here. The pattern as described is not close to statistical significance. This is true even if we control for the demographic characteristics of the respondents in the CPS state surveys.

We obtain this inconclusive result because state turnout varies considerably apart from the variables of our analysis. One can see this from Figures 1-5. The observations are considerably dispersed around the regression line. Our imagination might tell us that shifts in voter turnout (especially among registered) vary little from state to state. If that were the case, the observations would be around the regression lines, and we would be claiming estimates of effects that are statistically significant.

Our conclusions are in contrast to the claims of Alvarez et al. (2007) in their analysis of CPS voter participation data. We obtain estimated "effects" of similar magnitude to theirs. Yet we differ in our reports of the precision of our estimates. Whereas we see our results as

decidedly non-significant, Alvarez et al. report tight ranges to their coefficients that suggest otherwise. We stand by our interpretation that the evidence is far too shaky to stake a claim of discovery. Alvarez et al. offer few details regarding the nuts and bolts of their Bayesian methodology applied to the problem. The challenge for them is to show reasons for statistical confidence where in our view there exists none.

The moral is simple. We should be wary of claims – from all sides of the controversy – regarding turnout effects from voter ID laws based on current CPS data. The effects may be there as claimed. By all tests there is nothing to suggest otherwise. But the data are not up to the task of making a compelling statistical argument.

Conclusions

It should be evident that our sympathies lie with the plaintiffs in the Voter ID cases. Yet we see the existing science regarding vote suppression as incomplete and inconclusive. This is not because of any reason to doubt the suppression effect but rather because the data that has been analyzed to-date do not allow a conclusive test.

What can be done to boost the empirical analysis of the problem? Of course we can count on additional elections and additional states enforcing strict voter ID laws. Thus there will be more and better data. Beyond that, we suggest a more detailed analysis not of survey data, but of aggregate data within and between states. Here is one difference-in-difference-in-difference design: suppose we observe a decline in the voting rate in disadvantaged precincts of a strict-enforcement state such as Indiana relative to the voting rate of advantaged precincts within the state. This would be evidence that the poor are voting less relative to the rich, but is this because of the voter ID law? A test would be whether the decline is present only in states with

new voter ID laws and not in states that fail to enact them. And then, even if there is an effect, the test will work only if changes in the rich-poor voting gap are rare in the absence of newly enacted voter ID laws. This is where the CPS data fails as a venue. State differences in respondent turnout (and change in turnout) are too vast for the voter ID law effect to be measured with sufficient precision. Conceivably this problem can be alleviated by using within-state aggregate voting returns, which whatever their demerits, are free of the noise from survey sampling error.

	State	2002 Law	2006 Law	2002 Voting Rate		2006 Voting Rate	
				Observed	Adjusted ^a	Observed	Adjusted ^a
1	AL	1	4	67.5	.3	68.0	-.9
2	AK	4	4	75.8	7.4	75.4	4.6
3	AZ	3	4	72.3	1.5	75.6	3.3
4	AR	3	4	72.6	4.4	69.8	0
5	CA	1	1	69.4	-.6	77.2	4.0
6	CO	1	4	75.0	5.0	76.9	3.9
7	CT	4	4	67.5	-4.4	74.7	.6
8	DE	5	4	65.8	-4.5	68.3	-3.8
9	DC	1	1	70.1	1.3	68.4	-2.9
10	FL	5	7	73.1	3.1	68.8	-4.4
11	GA	4	4	65.0	-1.7	68.5	-1.6
12	HA	3	6	85.4	10.7	79.4	3.9
13	ID	1	1	74.9	5.7	79.6	8.0
14	IL	2	2	69.4	-.1	69.5	-2.3
15	IN	1	7	65.5	-4.0	70.0	-1.1
16	IA	1	1	70.4	1.9	71.6	.5
17	KS	1	1	72.7	1.8	70.9	-2.0
18	KY	4	4	67.6	.6	68.2	-.3
19	LA	4	6	67.0	-.1	57.0	-12.6
20	ME	0	0	71.5	2.7	74.1	3.2
21	MD	1	4	76.8	4.4	79.8	6.3
22	MA	3	3	73.1	2.1	77.3	3.4
23	MI	1	1	69.6	1.4	78.8	8.2
24	MN	1	1	84.8	16.3	83.7	11.1
25	MS	1	1	61.0	-5.7	61.9	-5.7
26	MO	5	4	71.5	2.7	74.0	3.5
27	MT	1	4	77.6	7.1	85.3	14.8
28	NE	1	1	65.1	-3.9	74.9	2.7
29	NV	2	2	75.4	4.4	76.7	3.3
30	NH	0	0	77.1	5.7	70.3	-3.8
31	NJ	2	2	65.8	-6.3	70.6	-4.5
32	NM	1	4	75.2	7.4	78.0	6.1
33	NY	2	2	65.5	-4.2	67.8	-4.7
34	NC	0	0	69.2	-.7	59.1	-11.5
35	ND	0	4	68.7	2.1	66.2	-2.4
36	OH	2	4	66.5	-2.5	75.2	5.2
37	OK	1	1	72.5	3.6	67.3	-2.6
38	OR	2	2	79.1	9.6	83.8	11.4
39	PA	2	2	68.1	-1.7	73.7	1.6
40	RI	0	0	75.1	4.4	81.1	8.8

41	SC	5	4	68.5	-.1	70.0	-.2
42	SD	0	6	87.4	19.0	81.2	11.1
43	TN	2	4	73.3	5.0	72.0	1.5
44	TX	4	4	61.4	-5.4	58.2	-11.7
45	UT	0	0	68.0	-.4	65.8	-5.2
46	VT	0	0	75.0	4.8	79.2	6.8
47	VA	4	4	59.0	-12.1	72.2	-.7
48	WA	1	4	72.2	2.0	77.2	4.1
49	WV	1	1	61.2	-6.8	59.8	-8.8
50	WI	3	3	72.8	2.9	80.9	9.3
51	WY	0	0	82.6	13.9	79.0	7.7

^aAdjusted state means are mean deviations of observed turnout in the sample from predicted turnout from individual demographic variables. See Table 2.

Table 2. Logit Equations Predicting Voting Among Registered in CPS Surveys				
	2002		2006	
	coefficient	std. error	coefficient	std. error
Age	0.0584	0.0031	0.0534	0.0032
Age squared	-0.0003	0.0000	-0.0002	0.0000
Female	-0.0358	0.0186	-0.0523	0.0178
Married	0.1527	0.0208	0.2740	0.0201
White	-0.0757	0.0257	-0.1756	0.0255
No HS Degree	-1.1978	0.0369	-1.1981	0.0350
HS Degree only	-0.5886	0.0224	-0.5405	0.0216
Income ^a	0.0538	0.0030	0.0469	0.0030
Income Missing	0.5972	0.0407	0.4878	0.0396
Intercept	-1.2246	0.0866	-1.2611	0.0834
Zavoina-McKelvey Pseudo R squared	.15		.14	
N	67,174		64,251	
a. Income is measured as the income intervals in the CPS codebook.				
All coefficients are significant at .001 except Female and White (2006).				

Table 3. Standard Deviations of State Voting Rates from CPS Surveys						
	2002		2006		2006 minus 2002	
	Observed	Adjusted	Observed	Adjusted	Observed	Adjusted
All	6.1	5.8	6.7	6.1	5.0	5.1
Grade School ^a	7.6	7.8	9.5	9.2	8.4	8.2
High School ^b	6.2	6.1	7.1	6.7	6.0	5.8
College Graduate	6.5	6.2	5.5	3.3	5.2	5.1
N=51 (states plus DC)						
^a No High school degree						
^b High School degree but no BA						
Adjusted standard deviations equal the standard deviations of the state mean of Xobs.-exp., where observed X is the turnout (1 or 0) and expected X is the turnout expected based on respondent individual characteristics from Table 2.						

Table 4. Estimated Effects of Voter ID Laws on Turnout Among Registered						
Independent Variable = Net Change Score in Voter ID Legislation						
	Dependent Variable=Change in Observed Voting Rate			Dependent Variable =Change in Adjusted Voting Rate		
	coefficient	std. err.	p-value	coefficient	std. err.	p-value
All	-0.45	0.44	.31	-0.38	0.44	.40
Grade School ^a	-0.60	0.73	.42	-0.42	0.72	.57
High School ^b	-0.59	0.51	.26	-0.54	0.68	.29
College Graduate	-0.11	0.45	.82	-0.02	0.04	.97
Grade School. Minus College	-0.49	0.66	.47	-0.44	0.65	.51
Independent Variable = Presence or Absence of Increase in Voter ID Legislation (0 or 1)						
	coefficient	std. err.	p-value	coefficient	std. err.	p-value
All	-1.8	1.5	.25	-1.5	1.5	.34
Grade School ^a	-3.0	2.5	.45	-1.3	2.6	6.2
High School ^b	-1.8	1.8	.31	-1.7	1.7	3.3
College Graduate	-1.7	1.6	.29	-1.2	1.5	.46
N=51 (states plus DC)						
^a No High school degree						
^b High School degree but no BA						
Adjusted standard deviations equal the standard deviations of the state mean of Xobs.-exp., where observed X is the turnout (1 or 0) and expected X is the turnout expected based on respondent individual characteristics from Table 2.						

Figure 1. Change in Voter Turnout by Change in Legislation
 All Cases, Observed State Turnout Data

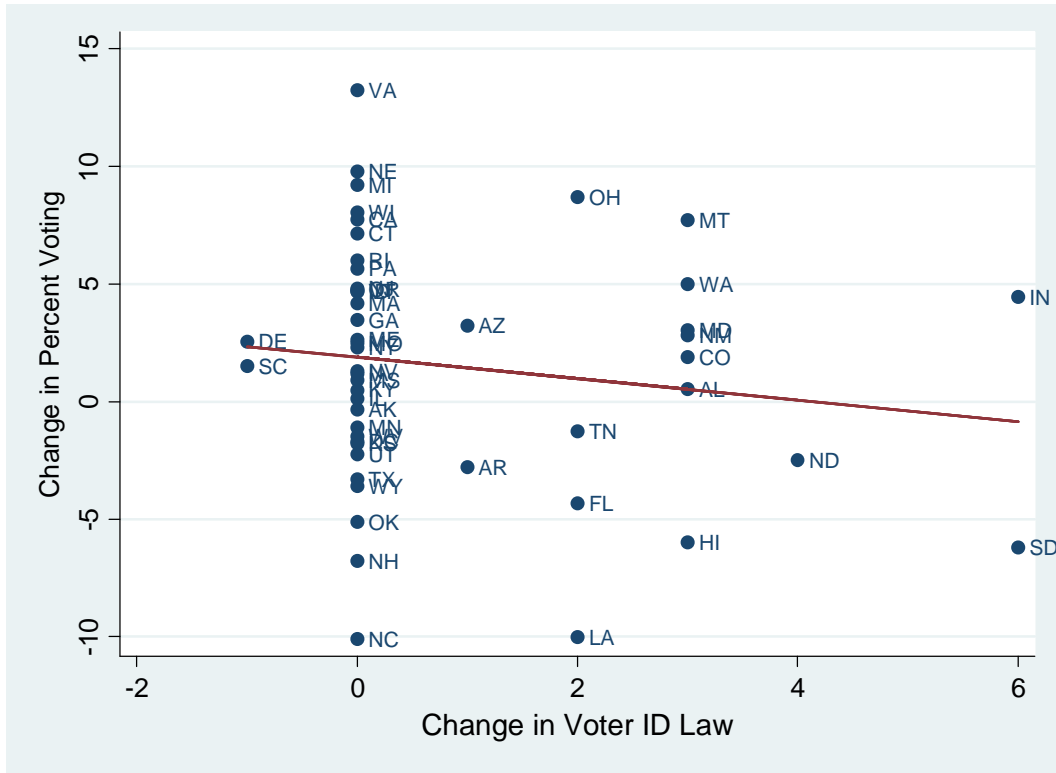


Figure 2. Change in Voter Turnout by Change in Legislation
 All Cases, Adjusted for Demographic Characteristics of Individual CPS Respondents

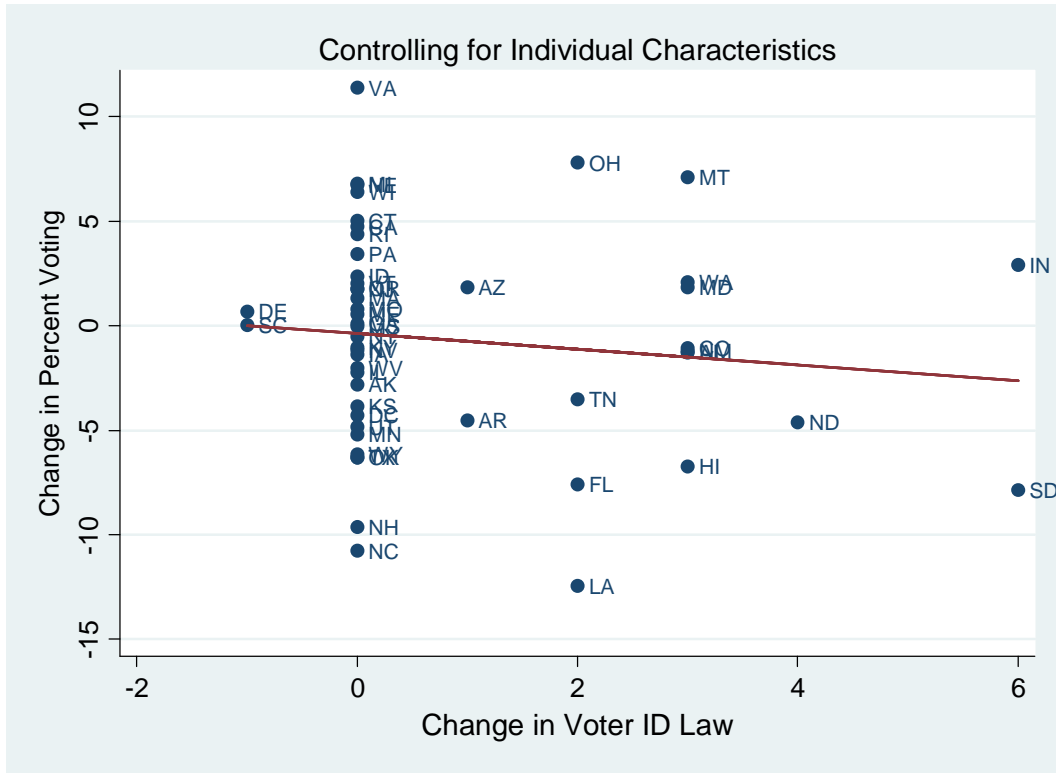


Figure 3. Change in Voter Turnout by Change in Legislation
 Non-High School Graduates Adjusted for Demographic Characteristics of Individual CPS Respondents

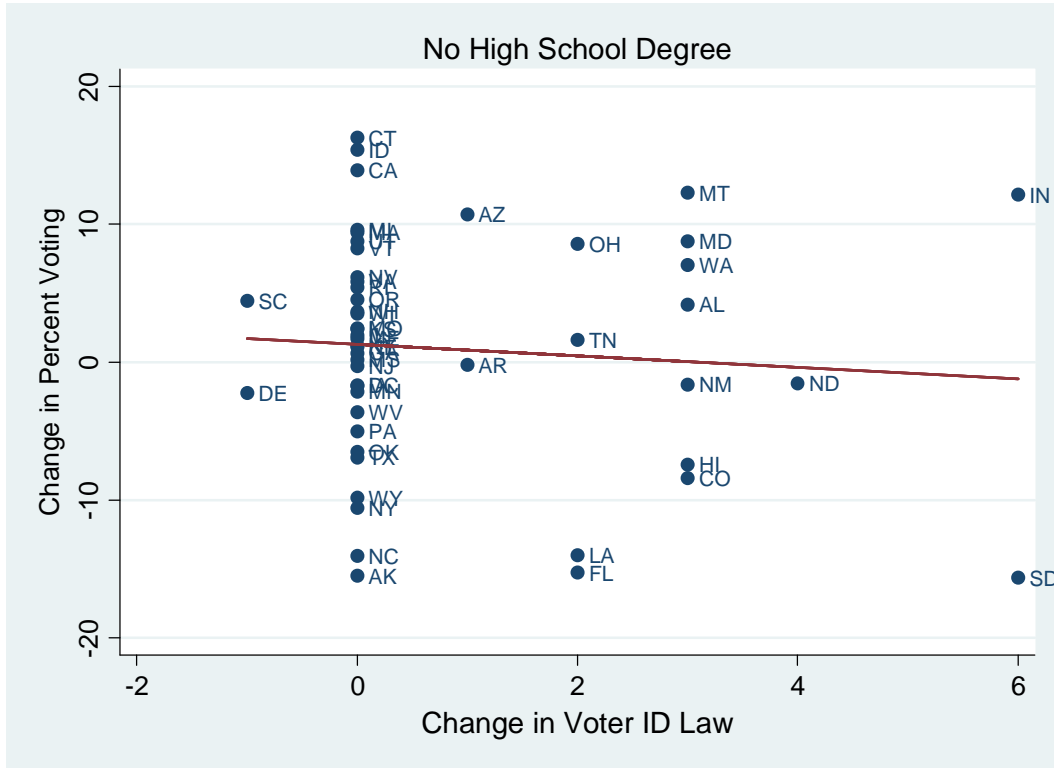


Figure 4. Change in Voter Turnout by Change in Legislation.
 College Graduates Adjusted for Demographic Characteristics of Individual CPS Respondents

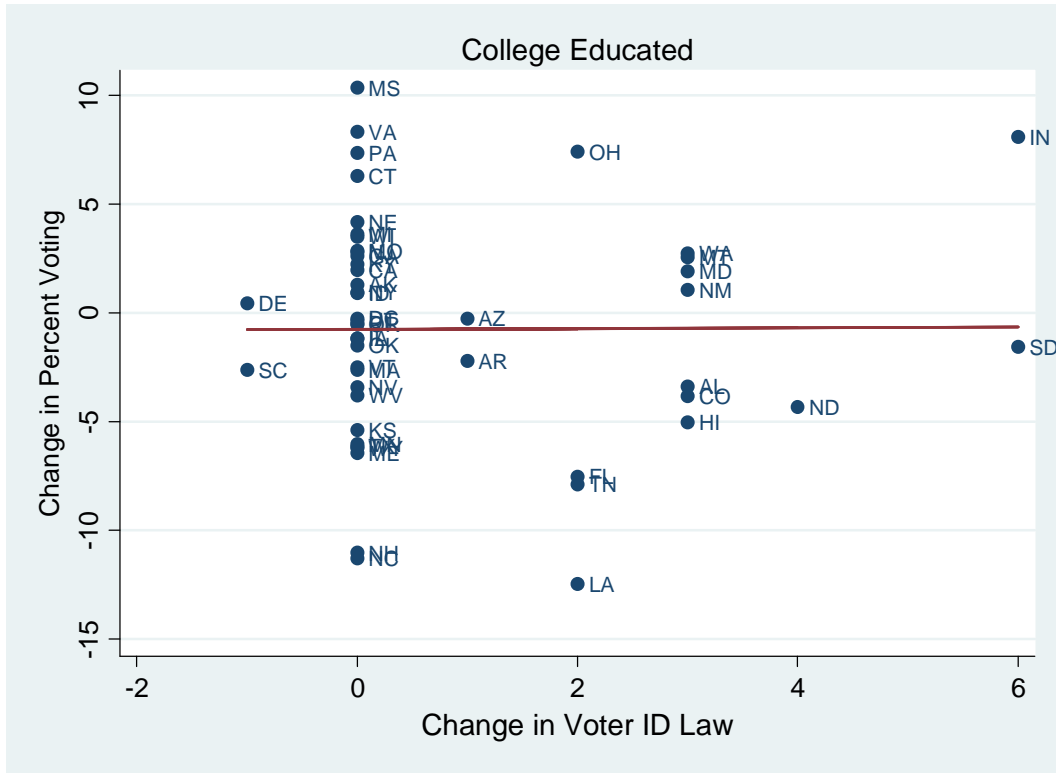
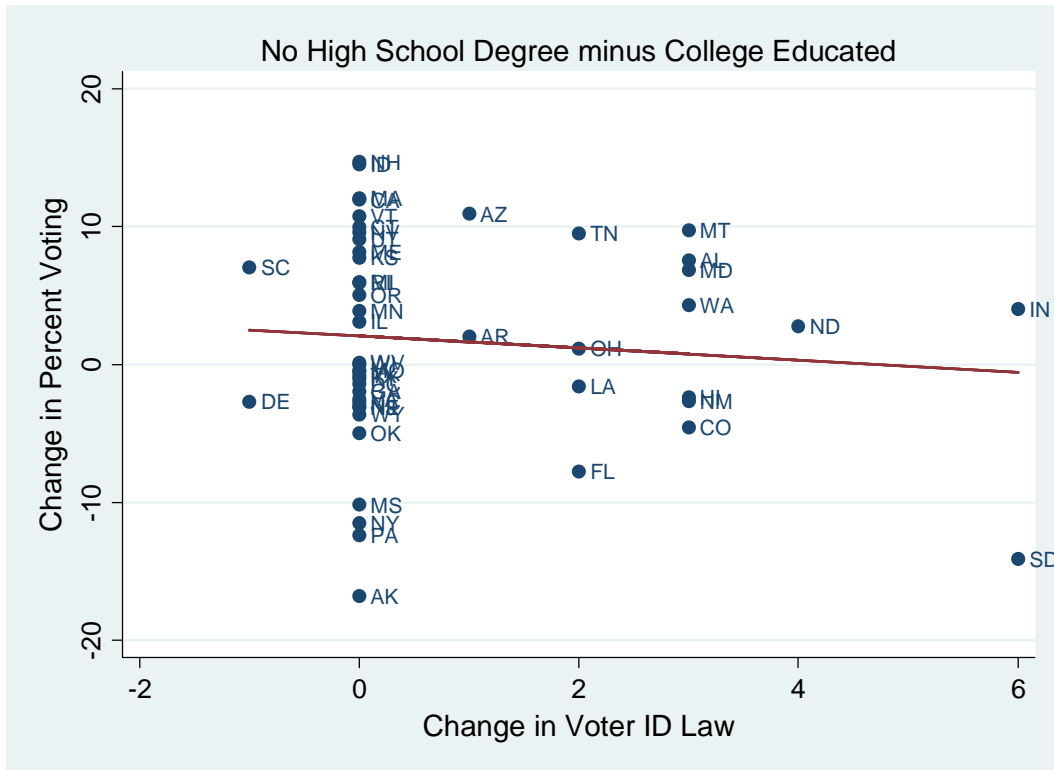


Figure 5. Difference of Difference of Difference Analysis



1. *Crawford et al. v. Marion County Election Board et al.* slip op. 07-021 (U.S. 2008).
2. For findings strongly suggesting that incidences of voter fraud are rare in American elections today, see Minnite and Callahan (2003); and Minnite (2007a; 2007b).
3. At the county level, holding the controls at their means and using the maximum scale of ID requirements, those requirements beyond stating one's name, not including requiring a photo ID produced an independent negative effect on turnout. The photo ID requirement, however, can not be measured very well because the five states that required a photo ID to vote in 2004 also allowed voters to substitute an affidavit. When the Eagleton Institute researchers looked at turnout using the minimum scale to account for the fact that no state in 2004 prevented a voter lacking the requisite photo ID from casting a regular ballot, they found no statistically significant effects of the various levels of ID required relative to the lowest level requirement – stating one's name.
4. In exit polls conducted in California, New Mexico and Washington, Barreto et al. (2007a) found that racial minorities were less likely than whites to have at least a driver's license and one other form of identification of the kind often required in states with voter ID laws. Other studies have found that blacks, in particular, are less likely than whites to possess valid driver's licenses and other forms of official identification like birth certificates and passports (Pawasarat 2005; Brennan Center 2006; Barreto et al. 2007b).
5. There are other potential problems in the coding of the Eagleton Institute study's data. For example, their measures of electoral competitiveness are derived from post-election results: those states in which the margin of victory for the winning presidential, gubernatorial or Senate candidate was five percent or less are classified as competitive or "battleground" states. But if we want to hypothesize an effect on turnout, what matters is the perception of competitiveness in the media or among voters before the election. The status of a state as competitive is one that should be derived from pre-election information, not retrospectively from the actual outcomes of electoral contests. Without knowing how the states were coded on these measures, it is difficult to know whether the substitution of pre-election for post-election information on electoral competitiveness would change any of the study's outcomes.
6. In fact, survey and list matching research on whether the types of ID required to vote in some states suggests proportionally small but large in absolute numbers of Americans currently lack these documents (Brace 2005; Pawasarat 2005; Brennan Center 2006; Hood and Bullock 2007; Barreto et al. 2007a; 2007b; Pastor et al. 2008; Ansolabehere and Persily 2008).
7. At the time of the Eagleton Institute study five states required some form of ID but provided other options to allow voters without ID to vote. Arizona and Indiana both recently tightened their polling place ID requirements but had not implemented them by the time of the 2004 election. On their maximum scale, the Eagleton Institute researchers coded these two states as if they had. In addition, the Eagleton researchers coded American Indians, Alaskan Natives, Hawaiian/Pacific Islanders, and those reporting multiple racial/ethnic identities as white. With income, the Eagleton Institute researchers used an ordinal variable measuring income in 16

income ranges as if the differences between the coded income ranges (i.e., 1, 2, 3...) were of the same magnitude, one equal unit, as the differences between individual income levels. Because income is measured in equal dollar units, and because the income ranges aggregated in the CPS are not equally divided, a one unit change in the variable does not clearly tell us what happens to turnout as income increases. In other words, the Eagleton Institute researchers may have erred when they treated an ordinal variable as if it were an interval-ratio. As Muhlhausen and Sikich point out, this can lead to estimation problems and is potentially hazardous for statistical modeling of turnout because of the importance of income in predicting voting.

8. Models 6 and 7 were run with the recoded states, which they describe in the text as Arizona and *Illinois* (see Muhlhausen and Sikich 2007, 17). We are presuming they mean Arizona and Indiana.

9. Muhlhausen and Sikich identify five other states besides Arizona and Indiana that could have been mis-classified by the Eagleton researchers on the minimum requirements scale. Connecticut, Delaware, Georgia, South Dakota and Virginia are all coded by the Eagleton researchers as requiring a non-photo ID on that scale when in fact those states allowed voters unable to produce ID to cast regular ballots by signing an identity affidavit. As best we can determine, Muhlhausen and Sikich did not re-run the corrected Eagleton model with these revisions to the minimum requirements scale. The miscoding of the states on both scales may explain some of the instability in the statistical significance of the photo ID variable influencing the probability of voting.

10. Conservative supporters of stricter laws have argued for the symbolic value of requiring voters to produce proof of their identity at the polls. They've suggested, following the lead of the Carter-Baker Commission, that restrictive ID requirements will improve perceptions of the electoral system and increase voting by enhancing security measures to safeguard the vote. They assert without any evidence that voters worry that their votes will be canceled out by illegal votes (Federal Commission on Election Reform 2005, 18, 23). Ansolabehere and Persily (2008) find no support for this theory.

11. It may, in fact, be the case that Republican partisans or Southerners are more concerned about voter fraud and that this bears on their decision to vote (see Mycoff et al. 2007; and Ansolabehere 2008), but in general, this concern does not show up in studies of non-voting.

12. The U.S. Election Assistance Commission explained its decision not to adopt the Eagleton Institute study (which it had commissioned) in part because it examined turnout in only one year.

13. Jusko and Shively use a two-step multilevel approach to examine why turnout declines in relation to the number of parties on the ballot.

14. This is the case as long as linearity is not being forced by the way the authors have constructed the independent variable.

15. Muhlhausen and Sikich (2007) found a negative effect of non-photo ID rules on black and Latino voting behavior, but not for Asians. Barreto et al. (2007a), found that unlike black and Latino voters, Asian American voters were less likely to possess a drivers license than whites. These findings suggest there could be important differences among the major non-white groups bearing on the question of voter ID and voting behavior.

Reference List

- Alvarez, R. Michael, Delia Bailey and Jonathan Katz. 2007. The Effect of Voter Identification Laws on Turnout. CalTech/MIT Voting Technology Project Working Paper #57. October.
- Ansolabehere, Stephen. 2008. Access Versus Integrity in Voter Identification Requirements. *N.Y.U. Annual Survey of American Law*. 63. Forthcoming.
- Ansolabehere, Stephen and Nathaniel Persily. 2008. Vote Fraud in the Eye of the Beholder: The Role of Public Opinion in the Challenge to Voter Identification Requirements. *Harvard Law Review*. 121: 1738-74.
- Barreto, Matt A., Stephen A. Nuño and Gabriel R. Sanchez. 2007a. Voter ID Requirements and the Disenfranchisements of Latino, Black and Asian Voters. Paper prepared for the Annual Meeting of the American Political Science Association, Chicago, Illinois. 30 August – 2 September.
- . 2007b. The Disproportionate Impact of Indiana Voter ID Requirements on the Electorate. Working Paper, Washington Institute for the Study of Ethnicity and Race. 8 November.
- Brace, Kimball W. 2005. Report on the Matching of Voter Registration and Driver's License Files in Indiana Democratic Party et al. v. Todd Rokita et. al. Court Case. Submitted with Affidavit of Kimball W. Brace. Brief in Support of Motion for Summary Judgement. Exhibit 5. September.
- Brennan Center for Justice and New York University Law School. 2006. Citizens Without Proof: A Survey of Americans' Possession of Documentary Proof of Citizenship and Photo Identification. Voting Rights & Elections Series. November. Available at http://www.brennancenter.org/page/-/d/download_file_39242.pdf.
- Brief of Amici Curiae of Historians and Other Scholars in Support of Petitioners. *Crawford et al. v. Marion County Election Board et al.* Nos. 07-21, 07-25 (U.S., 2007).
- Brief of State Respondents. *Crawford et al. v. Marion County Election Board et al.* Nos. 07-21, 07-25 (U.S., 2007).
- Donald, Stephen G. and Kevin Lang. 2007. Inference with Difference-in-Differences and Other Panel Data. *Review of Economics and Statistics*. 89: 221-233.
- Eagleton Institute of Politics, Rutgers, The State University of New Jersey and The Moritz College of Law, The Ohio State University. 2006. Report to the U.S. Election Commission on Best Practices to Improve Voter Identification

- Requirements, Pursuant to Help America Vote Act of 2002, Public Law 107-252. 28 June.
- Electionline.org. 2006. Election Reform: What's Changed, What Hasn't and Why, 2000-2006. February. Available at http://www.pewtrusts.org/uploadedFiles/wwwpewtrustsorg/Reports/Election_reform/electionline_022006.pdf.
- Federal Commission on Election Reform. 2005. *Building Confidence in U.S. Elections: Report of the Commission on Federal Election Reform*. September.
- Gordon, Greg. 2008. With No Photo IDs, Nuns Denied Ballots in Indiana. McClatchy Washington Bureau. 6 May.
- Hastings, Deborah. 2008a. Saint Mary's Nuns Told: No Photo IDs, No Vote. *South Bend Tribune*. 7 May.
- Hastings, Deborah. 2008b. Indiana Nuns Lacking ID Denied at Poll by Fellow Sister. *Associated Press*. 6 May.
- Hastings, Deborah. 2008c. Indiana's Primary Turnout High, Despite Photo ID Law. *Associated Press*. 8 May.
- Hood, M.V. III and Charles S. Bullock, III. 2007. Worth A Thousand Words? An Analysis of Georgia's Voter Identification Statute. Paper prepared for the Southwestern Political Science Association, Albuquerque, New Mexico. 15-17 March.
- Jacobs, Lawrence R. and Melanie Burns. 2008. The Big Mobilization: Increased Voter Registration in 2008. Report Prepared by the Center for the Study of Politics and Government, Humphrey Institute of Public Affairs, University of Minnesota. 5 May. Available at http://www.politicsandgovernance.org/reports/2008/The_Big_Mobilization.pdf.
- Jusko, Karen Long and W. Phillips Shively. 2005. Applying a Two-Step Strategy to the Analysis of Cross-National Public Opinion Data. *Political Analysis*. 13(4): 327-344.
- Kedar, Orit and W. Phillips Shively. 2005. Introduction to the Special Issue. *Political Analysis*. 13(4): 297-300.
- Kousser, J. Morgan. 1974. *The Shaping of Southern Politics: Suffrage Restriction and the Establishment of the One-Party South, 1880-1910*. New Haven: Yale University Press.

- Logan, John R. and Jennifer Darrah. 2008. The Suppressive Effects of Voter ID Requirements on Naturalization and Political Participation. Report of the American Communities Project, Brown University. 2 January.
- Lott, John. 2006. Evidence of Voter Fraud and the Impact That Regulations to Reduce Fraud Have on Voter Participation Rates. Working Paper. University of Maryland.
- Martelle, Scott. Nuns Are Turned Away From Indiana Polls Under Voter ID Law. *Los Angeles Times*. 7 May.
- Milyo, Jeffrey. 2007. The Effects of Photographic Identification on Voter Turnout in Indiana: A County-Level Analysis. Report 10-2007. University of Missouri/Columbia, Institute of Public Policy.
- Minnite, Lori and David Callahan. 2003. Securing the Vote: An Analysis of Voter Fraud. New York: Demos, A Network for Ideas & Action.
- Minnite, Lorraine. 2007a. The Politics of Voter Fraud. A Report to Project Vote. March.
- . 2007b. Election Day Registration: A Study of Voter Fraud Allegations and Findings on Voter Roll Security – A Preliminary Report. New York: Demos, A Network for Ideas & Action.
- Moulton, Brent R. 1986. Random Group Effects and the Precision of Regression Estimates. *Journal of Econometrics*. 32: 385-397.
- . 1990. An Illustration of a Pitfall in Estimating the Effects of Aggregate Variables in Micro Units. *Review of Economics and Statistics*. 72: 334-338.
- Muhlhausen, David B. and Keri Weber Sikich. 2007. New Analysis Shows Voter Identification Laws Do Not Reduce Turnout. A Report of The Heritage Center for Data Analysis. CDA07-04. 10 September.
- Mycoff, Jason D., Michael W. Wagner and David C. Wilson. 2007. The Effect of Voter Identification Laws on Aggregate and Individual Level Turnout. Paper prepared for the Annual Meeting of the American Political Science Association, Chicago, Illinois. 30 August – 2 September.
- Pastor, Robert, Robert Santos, Alison Prevost, and Vassia Gueorguieva. 2008. Voter IDs Are Not the Problem: A Survey of Three States. Center for Democracy and Election Management, American University. 9 January.
- Pawarsarat, John. 2005. The Driver License Status of the Voting Age Population in Wisconsin. Employment and Training Institute, University of Wisconsin-Milwaukee. June.

- Piven, Frances Fox and Richard A. Cloward. 2000. *Why Americans Still Don't Vote and Why Politicians Want It That Way*. New York: Beacon Press.
- Primo, David M., Matthew I. Jacobsmeier, and Jeffrey Milyo. 2007. Estimating the Impact of State Policies and Institutions with Mixed-Level Data. *State Politics and Policy Quarterly*. 7: 446-459.
- Project Vote. 2007. Restrictive Voter ID Requirements. Issues in Election Administration: Policy Brief Number 8. 23 March. Available at http://projectvote.org/fileadmin/ProjectVote/Policy_Briefs/Project_Vote_Policy_Brief_8_Voter_ID.pdf.
- Vercelloti, Timothy and David Anderson. 2006. Protecting the Franchise, or Restricting It? The Effects of Voter ID Requirements on Turnout. Paper prepared for the Annual Meeting of the American Political Science Association, Philadelphia, Pennsylvania. 31 August – 3 September 3.
- Voter Registration Numbers Increase By the Thousands. 2008. *(Indiana) Carroll County Comet*. 16 April, 5A.