Election Laws and Voter Turnout Among the Registered: What Causes What?

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Abstract
Scholars make causal claims about the vote-boosting power of laws designed to increase turnout, citing evidence from regression analyses that show that generous voting laws are related to high turnout. Yet one must be skeptical of contamination from endogeneity in this relationship. The skeptic's argument is: states with a culture of participation pass legislation designed to encourage voting. With participatory states being the cause of pro-turnout legislation, the causal direction is reversed from what is normally supposed. We take the skeptic's argument seriously and use sensitivity tests to evaluate claims that turnout is influenced by pro-turnout legislation (and vise versa). Specifically, we apply a "zero covariance restrictions" assumption and estimate the effect of turnout on legislation via two-stage least squares, with state demographic variables as instruments. Then, assuming that there are no unobserved variables that affect both turnout and legislation, we can back out the reverse effect of legislation on the vote. The 2SLS analysis shows that the turnout-legislation effect fully accounts for the turnout-legislation covariance, leaving no room for legislation to effect turnout.

Numerous studies have examined the effectiveness of legislation designed to increase voter turnout. Some studies approach the matter based on temporal evidence, for example examining the change in turnout following legislative reforms (e.g, Erikson and Minnitte, 2009). Many studies, however, are based on cross-sectional evidence, where states are compared at a single point in time (e.g. Wolfinger, Highton, and Mullin, 2005). Typically, cross-sectional studies compare survey respondents' reported voter turnout as a function of the degree to which their state's laws encourage voting. These cross-sectional studies have a powerful appeal from their often massive numbers of survey respondents, combined with statistical controls for respondents' individual characteristics.

The large *N*s and controls might seem to reassure against threats to causal inference. But these seeming safeguards can be deceptive. Whereas the number of respondents can be in the multiple thousands, the number of causing units—the states—is slim, <51. In short, whereas the observations are individuals, the causation operates at a different level—states. One risk for researchers that is now known (and sometimes easily remedied) is that their standard errors can be over-confident. Here we address a potentially even more serious threat—the problem of reverse causality.

Threats to Inference

Conventional cross-sectional studies must make the assumption that the key independent variable—enabling legislation—is uncorrelated with unmeasured causes of the dependent variable—voter turnout. A bit of thought should indicate why this assumption is unlikely to be true. States do not choose legislation randomly, which introduces two potentially serious sources of bias. The first is that unmeasured causes of turnout can conceivably affect reform legislation as well, thus contaminating the estimated effect of reforms. Even when aggregated to the state level, the analyst's list of respondent control variables cannot account for much variation in state voting rates. In one formulation, the degree to which a state has a participatory "culture" could determine both the diligence of its citizens regarding voting and the state's propensity to pass laws to encourage more voting.

The second potential source of bias is that states' propensities to pass legislation to encourage turnout are themselves directly affected by the states' citizens' propensities to vote. One possibility is a negative effect, where low turnout encourages legislatures to design mechanisms to stimulate voting, and thus estimates obtained using cross-sectional variation of the effect of such laws are dampened. Conversely, high turnout might encourage legislators to enact reforms to encourage even more turnout, thus biasing estimates of the laws' effect upward. In short, there may be simultaneous causation—a reciprocal causal relationship between voting and legislation that encourages voting. Either a positive or a negative effect would contaminate the correlation evidence regarding a state's legislation and voting rate, even with controls for the states' survey respondents.

To solve this problem, let us examine the causal model underlying our discussion. Figure 1 depicts the causal relationship that one must assume in order to estimate β, the effect of laws (X) on turnout (Y), using a regression analysis on cross-sectional data. In particular, there are no confounding variables that affect both laws and turnout outside of those measured in W. Additionally, there is no reciprocal relationship whereby turnout causes laws.

Figure 2 depicts a more complicated set of relationships. Here, there may be some correlation between the unmeasured causes of laws, u, and the unmeasured causes of turnout, v. In other words, W (observed) does not contain all of the confounding factors that influence both X and Y. There may also be some reciprocal causation between Y and X, captured by γ, as discussed above. What are we to do? We could find an instrument that affects laws but not voting directly. This would allow us to identify β despite these other two complications. But, what that instrument would be eludes us.

However, note that there are of a number of variables (Z's) that affect voting but perhaps do not directly affect legislation. These are the demographic variables the analyst uses as controls in the individual-level analysis. With these variables as instruments, we can estimate the effect of voter turnout (Y) on laws (X): the net indirect effect of Z's on X is, by the model, the product of the effects of Z's on Y and the effect of Y on X (γ in the graph).

While this is nice, we really want the reverse effect of X on Y (β). But knowing (estimating) γ, we can back out an estimate of β if we are willing to make one additional assumption. That assumption is that there are indeed no unmeasured confounding variables, i.e., the covariance between u and v is zero. Figure 3 depicts this relationship. This assumption may or may not be reasonable, but this is nonetheless the assumption one must always make in order to run any typical regression.[1] Thus, while we cannot solve the problem of unmeasured confounders, we can solve the problem of reciprocal causation by finding a plausible instrument (Z) for the effect of turnout (Y) on laws (X).

With an estimate of γ in place, the formula for β can be easily be estimated as:

$$\beta = \frac{r-\gamma}{1-r\gamma}$$

where r=the correlation between X and Y, and for simplicity, the variables are assumed to be standardized (both standard deviations equal 1.0).

The econometric literature (e.g., Hausman, Newey, and Taylor, 1987) refers to this as estimation via the zero-covariance restriction. The leverage is the assumption that every non-observable cause of Y is unrelated to X directly. This is nothing more than the

[1] The implications of violating this assumption are discussed below.

mandated assumption of OLS, that all omitted causes of Y are unrelated to X. In other words, if our worry is simultaneity, but have an estimate of the reverse effect γ, we can back out an estimate of β. All that is further required is the usual assumption that omitted causes of Y are related to X.

Another way of looking at this is as if the confounding effect biasing the result is entirely via reciprocal causation. Effects of outside variables, observed or not, are proportional in their effects on X and Y. And this is due to their influencing Y (turnout in our example) and then X (laws in our example).

Data and Analysis

For our analysis, we make use of a data set that has been analyzed several times in the pages of *State Politics and Policy* (Wolfinger, Mullin, and Highton 2005; Primo, Jacobsmeier, and Milyo 2007; and Erikson, Pinto, Rader 2010). This is the data on reported turnout among registrants in the 2000 Voter Supplement to the US Census's Current Population Survey (CPS). Each analysis is based on an equation predicting the vote or not-vote decision based on census demographics (the controls) plus a slew of reform laws that are meant to encourage people who are already registered to vote to do so. While turnout among registered voters is typically high, there is still significant state-to-state variation, as shown in Figure 4. In 2000, 92 percent of Delaware's registered voters voted, while only 72 percent of Texas's did. The various laws intended to increase these numbers are time off work for private employees on election day, time off work for public employees, mailed polling place information, mailed sample ballots, early morning voting hours, and late evening voting hours.

Wolfinger et al. show that most reforms are statistically significant using conventional standard errors. Primo et al. and Erikson et al., however, show that Wolfinger et als' standard errors are overconfident. Wolfinger et al's standard errors are too small and reported significance levels too large when compared to the more appropriate "clustered' standard errors (Primo et al.) and to the standard errors produced by randomization tests (Erikson et al.).

Thus, one worry is overconfidence in the results of what might otherwise be a properly specified equation. In the present paper, our concern is bias from simultaneity. Respondents from states with liberal laws might be more (less) likely to vote not because of the laws' effects, but rather because the high voting rate in some states induces their legislatures to pass more (less) liberal laws.

Individual level analysis

Table 1 contains two probit equations. Equation 1 predicts the participation decision from standard demographic variables, plus the state laws in question. Equation 2 predicts solely from demographics. These are similar equations as in the previously cited work except that, to keep the example simple, we ignore possible interaction effects between law and citizen type. We use equation 1 to get a first estimate of the laws' relative effects, adjusting for the measurable demographic variables. That is, we sum the β^*X coefficients for the various laws to create a law index. We use the predictions from equation 2 (specifically the predicted probit index) to obtain a composite measure of demographic-based propensity to vote.

The next step is to aggregate both the law index and demographic composite to the state level. We simply record the means for each state in the large CPS sample. The laws based prediction is the measure of X, the net contribution of state laws to voting, once registered. The demographic based measure is Z, the composite measure of demographically-induced voting in the state. The idea then is that Z (demographics) influences Y (now, voter turnout within the state sample of registrants) but not X (laws) directly.

Before turning to the aggregate-level analysis, however, let us take a close look at the individual-level table, particularly column 1. As discussed in Primo et al. (2007) and Erikson et al. (2010), the clustered standard errors do not justify much confidence in the individual law effects. Yet when we look at the effects collectively, we see that they are collectively significant (p=.002) even when standard errors are clustered by states. We observe this result from an equation (not shown) where individual vote choice is a function of the demographic variables plus our law index created from the estimated sum of state effects from column 1.

Thus we might have confidence in the overall substantive significance of legislation to encourage voting among the registered. For example, using the summary index of state laws, the difference between the effect of the least reformed state (Georgia) and the most reformed (Louisiana) is .33 in the units of the probit equation. This is roughly equivalent to the effect of moving from one of the education categories to the next highest rung—for instance from high school dropout to high school graduate, or from "some college" to completed college. All might agree that if this result holds up, the effect of legislation to lure registered voters to the polls is substantively significant.

But for the result to hold up, it must be unshaken by serious concern about endogeneity. If laws to encourage post-registration voting positively influence the post-registration voting rate, we must be concerned by omitted variables (e.g., "culture") that could account for the statistical relationship. And, we must be particularly concerned that the voting rate itself influences the legislation. In particular, a high voting rate might induce state legislatures to liberalize post-registration voting hurdles. To find out, we turn to the aggregate-level analysis of our 42 states.[2]

Aggregate level analysis

[2] Following Wolfinger, et al, we exclude states that allowed same-day registration (Idaho, Maine, Minnesota, New Hampshire, Wisconsin, and Wyoming), states that do not require registration (North Dakota), and states with mail-in voting (Oregon).

Next we switch to the aggregate analysis of data where the units are the 42 available states. We measure turnout among registered voters from the means in the 2000 Current Population Survey. As a first cut, we can estimate the regression of vote turnout on the composite laws index based on the sum of law effects from Table 1. We obtain:

$$\mathit{Turnout} = \underset{(0.07)}{0.83} + \underset{(0.07)}{0.22}\,\mathit{Laws} \qquad \text{Adj. } R^2\text{=0.196}$$

where turnout is measured as a proportion, and standard errors are in parentheses. Seemingly, our legislation can explain a fifth of the variance in turnout among registered. Figure 5 presents the bivariate relationship between laws and the turnout.

Next, we recreate something akin to our original individual-level equation by regressing post-registration turnout rates on both the composite laws index and the demographic predictions aggregated to the state level. We obtain the following equation:[3]

$$\mathit{Turnout} = \underset{(0.08)}{0.40} + \underset{(0.09)}{0.40}\,\mathit{Demographics} + \underset{(0.06)}{0.12}\,\mathit{Laws} \qquad \text{Adj. } R^2\text{=0.495}$$

The coefficient for the law index plunges and is now barely statistically significant, with a *p*-value of .047. Still, the range of the aggregated laws index is 0.33, suggesting that the difference between the most stringent and the most liberal post-registration laws is about 4 percent of the vote (.0.12 x .33 = 0.04). Can laws can make a difference of adding 4 percentage points to voting among registered?

We should note that demographics are the predominant predictor of turnout. By itself, the demographic index explains 45 percent of voter turnout. And demographics are related to laws. Suppose we regress the state level predictions from the demographic index onto the laws index. We obtain the picture shown in Figure 6, which is similar in shape to Figure 5, above, when turnout among the registered is the dependent variable.[4]

The problem with the picture in Figure 6 is this: Laws do not determine demographics. As a causal statement claiming to show the effects of legislation, Figure 6 is obviously spurious. To the extent Figure 6 represents a causal argument, it must be that demography affects laws. (Below we argue that this effect is indirect.)

[3] See the appendix for a state-level regression including the components of *Demographics* and *Laws*.
[4] That is, we first regress turnout on demographics, and use the prediction equation as the *Y*-axis variable in Figure 6.

We would like to see how much laws can explain the residual portion of state turnout that is *not* determined by demographics.[5] This unmeasured portion includes omitted variables about the state populations we might want to measure but cannot from the CPS (e.g., religious data) plus other state level variables that we might call "culture."

Suppose then, we relate the non-demographic portion of turnout on laws. We get Figure 7. Here, the relationship is a bit more ragged and if we compute the *p*-value on the underlying regression coefficient, it is not quite significant (p=.06). And so, we should worry that the unmeasured causes of turnout (the presumed *Y* variable) actually could be causing turnout.

Now we estimate the effect of laws on turnout using our zero-covariance assumption. Our working assumption is that the effects of demographic characteristics on laws is indirect, via turnout itself. In other words, the assumption is that demographic variables influence turnout, but do not affect legislation, except by influencing turnout.

First we obtain an estimate of the effect of turnout on laws from the 2SLS equation. That is, we predict turnout from demographics, using the predicted turnout as the independent variable in the equation predicting laws. With an estimate of the effect of turnout on laws, we back out the estimate of central interest—the reverse effect of laws on turnout.

Table 2 presents the results from this analysis along with the comparable OLS regressions that do not take into account simultaneity.[6] Column 1 shows the basic model. As discussed above, an OLS regression of turnout on our laws index and demographic composite variable suggests that registered voters in states with laws that encourage registered voters to vote actually do vote more often than voters in other states. However, our zero-covariance restriction analysis shows that this positive association is due almost entirely to the reverse causal mechanism—states with high turnout are more likely to pass laws encouraging turnout than are states with low turnout. The coefficient for the effect of laws on turnout, 0.12, plunges to -0.05 and loses statistical significance once reciprocal causation is accounted for. The coefficient of the effect of turnout on laws, estimated by using demographics as an instrument for turnout, is positive and statistically significant.[7]

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[5] That is, for Figure 7 the *Y*-axis variable is turnout minus the prediction from the demographic equation, used in Figure 6.

[6] The zero-covariance results were obtained using EQS structural equation modeling software.

[7] Recall that we collapse each of the demographic variables into one composite variable by regressing individual level turnout on individual demographic characteristics, taking the predicted (probit index) turnout from that equation, and aggregating it to the state level. We do this because it makes the zero-covariance analysis less computationally intense. However, to test the exogeneity of the demographics instrument, we used the set of separate state-level demographic variables as instruments for turnout and performed a

The next columns display results from analyses similar to the basic model but with additional control variables included to make the zero-covariance restriction more plausible. For example, a state's political liberalism may correlate both with its level of turnout and with its set of turnout-encouraging laws. Column 2 displays the results from an analysis that includes state Democratic vote for president in 2000 as a measure of liberalism. That variable itself turns out to predict neither laws nor turnout, and its inclusion does not change the substantive message from the basic analysis—the positive association between laws and turnout is due entirely to turnout causing laws and not the other way around.

Column 3 displays two sets of zero covariance analyses. One includes indicators for southern states, battleground states, and states that had concurrent senatorial or gubanatorial elections as additional control variables. The other uses battleground and concurrent elections as additional instruments for turnout.[8] Again, in both instances, we find that the positive association between laws and turnout we see from the naïve OLS regression disappears once we take into account reverse causality.

Finally, one might think that the political culture of a state might simultaneously cause both turnout in that state and laws that encourage turnout. Column 4 displays the results from an analysis that includes Elazar's (1984) measures of state political culture.[9] Elazar defined three categories of states. Moralistic culture values citizen participation and views government as a force for public good, and so one might expect that moralistic states would have both higher turnout and laws that encourage turnout. Traditionalistic culture sees government as a vehicle for maintaining law and order through elite control and little citizen participation, and so one might expect traditionalistic states to have both low turnout and fewer laws encouraging turnout. Individualistic culture is somewhere between—it sees government as one of many instruments for turning citizen responding to citizen demands and so neither values nor minimizes the importance of participation. (For more, see, e.g., King 1994.)

In our analysis, we include indicators for moralistic and individualistic states. Interestingly, moralistic and individualistic states do have higher turnout than traditionalistic states (controlling, of course, for demography) but do not seem to be more likely to pass laws encouraging turnout (independent of turnout). Either way, as before, we find that states with high turnout tend to pass laws encouraging turnout but not the other way around.

Sargan test for overidentification. We cannot reject the null hypothesis that the set of instruments is valid (p=.47).

[8] A Sargan test for overidentification does not reject the null hypothesis that this set of instruments is valid (p=.25).

[9] Alaska and Hawaii are excluded from this analysis.

Thus, regardless of specification, we consistently find that the positive association between state-level turnout and state laws that encourage registered voters to vote seems to be due to the fact that states that already have high turnout are more likely to pass laws to encourage turnout. Why would this be the case? One explanation could be that high turnout literally causes state legislatures to be more likely to pass laws encouraging even further success in getting people out to vote. Another could be that some states simply value participation in a way that is not captured by the Elazar culture measures. If this is so, then perhaps there is some unobserved cultural force that moves both laws and turnout. That is, the zero covariance assumption (and, of course, the standard OLS assumption) is violated. However, this is even worse news for laws meant to encourage turnout among the registered because it means that our modest and statistically insignificant estimates are too high. If there is indeed an omitted variable that is positively associated with both laws and turnout, then our estimate will be biased upward even after accounting for reverse causality.

Discussion and Conclusions

This paper has challenged the argument that laws designed to boost post-registration voting turnout actually have their intended effect. We assume that the variables that affect turnout—measured and unmeasured—influence laws to increase turnout, and do so in proportion to their impact on turnout. With this assumption, the state-level correlation between turnout levels and laws intended to boost turnout can entirely be explained by the effect of turnout on laws. The leverage is the assumption that both observed and unobserved variables that affect turnout also affect laws in proportion to their turnout effects.

Of course these results are presented with a note of caution. Like any analysis using instrumental variables, the results are conditional on the quality of the underlying theory. We believe that our assumptions of zero covariance of disturbances plus the instrumental variable specification is superior to the assumptions of zero covariances of disturbances plus zero effect of turnout upon laws, which would be required for OLS.

There are further reasons for caution. We are not only dealing with a censored sample of only 42 states; most of the missing states are states with “same day” registration, a reform that indeed may produce its intended effect (Fenster, 1994; Burden and Neiheisal, 2012). Moreover, it should be stressed that we are examining the effects of post-registration laws on turnout among registered, rather than the effects of laws to ease registration costs on registration and voting turnout. It is quite possible that manipulating registration effects on turnout is easier than luring actual registrants to the polls.

Our final point is to reiterate that the best way to analyze the effects of laws on turnout is by utilizing time. When laws change, does turnout increase? A demonstration that turnout behavior changes (or not) following changes in the law is the best way of determining whether vote-inducing legislation works.

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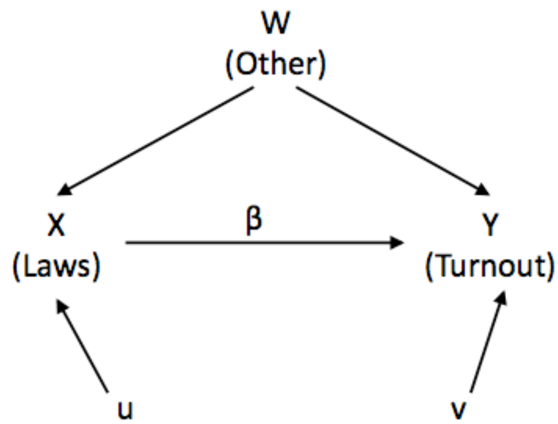

Figure 1: Typical Model

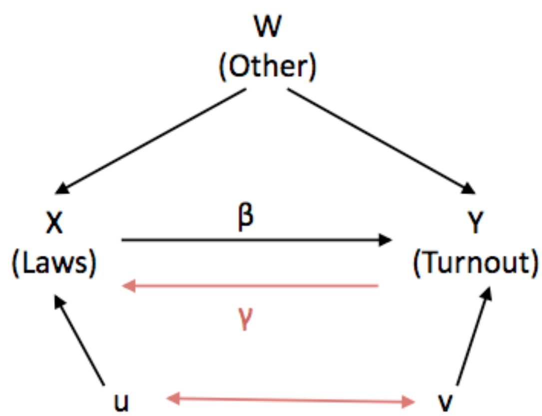

Figure 2: Potential Problems

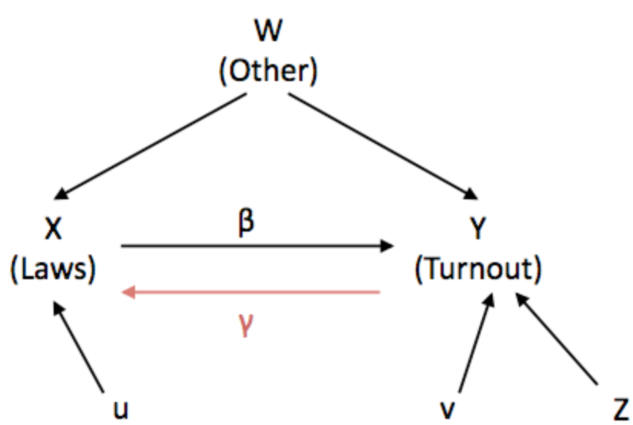

Figure 3: Zero-covariance Restriction Solution

Figure 4: Turnout of Registered in 2000

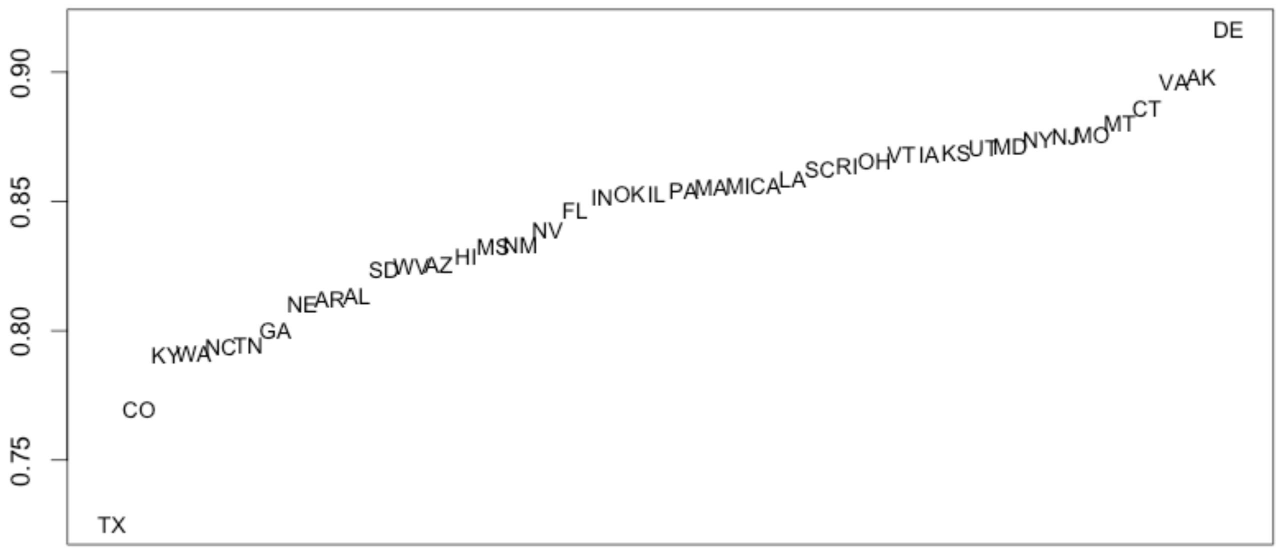

Figure 5: Postregistration Laws and Turnout of Registered in 2000

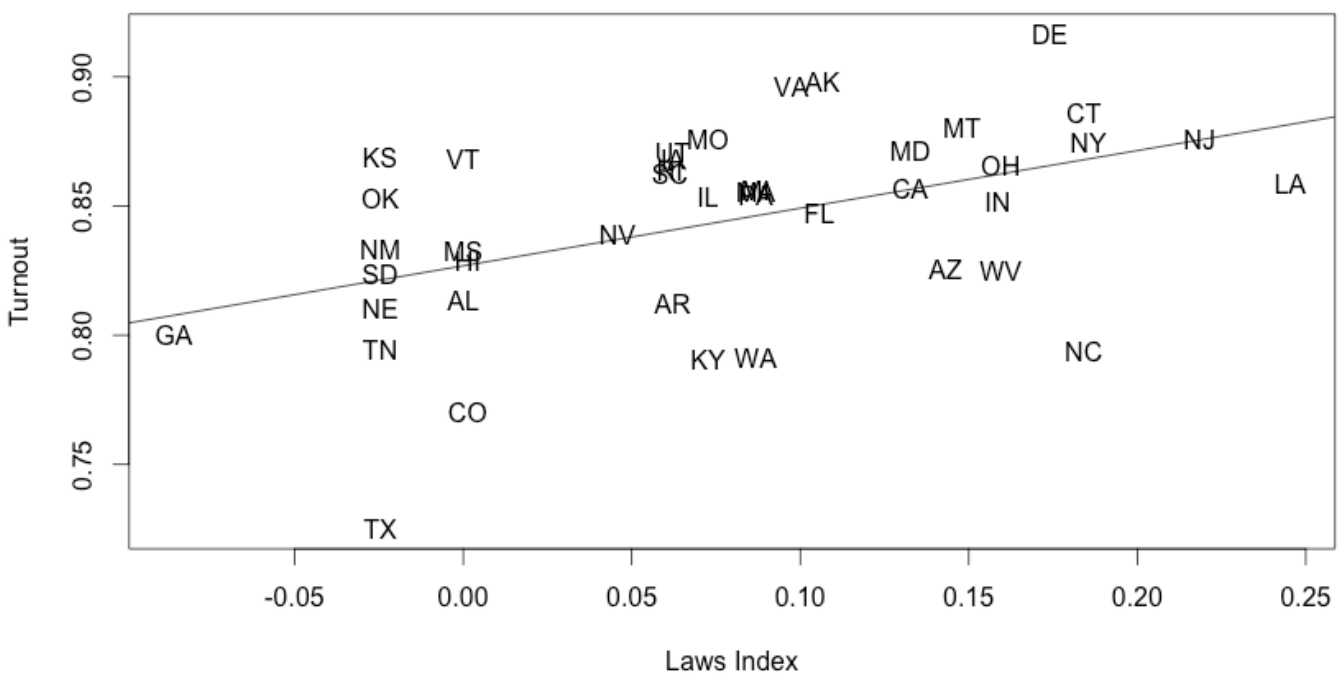

Figure 6: Postregistration Laws and Demographic Turnout in 2000

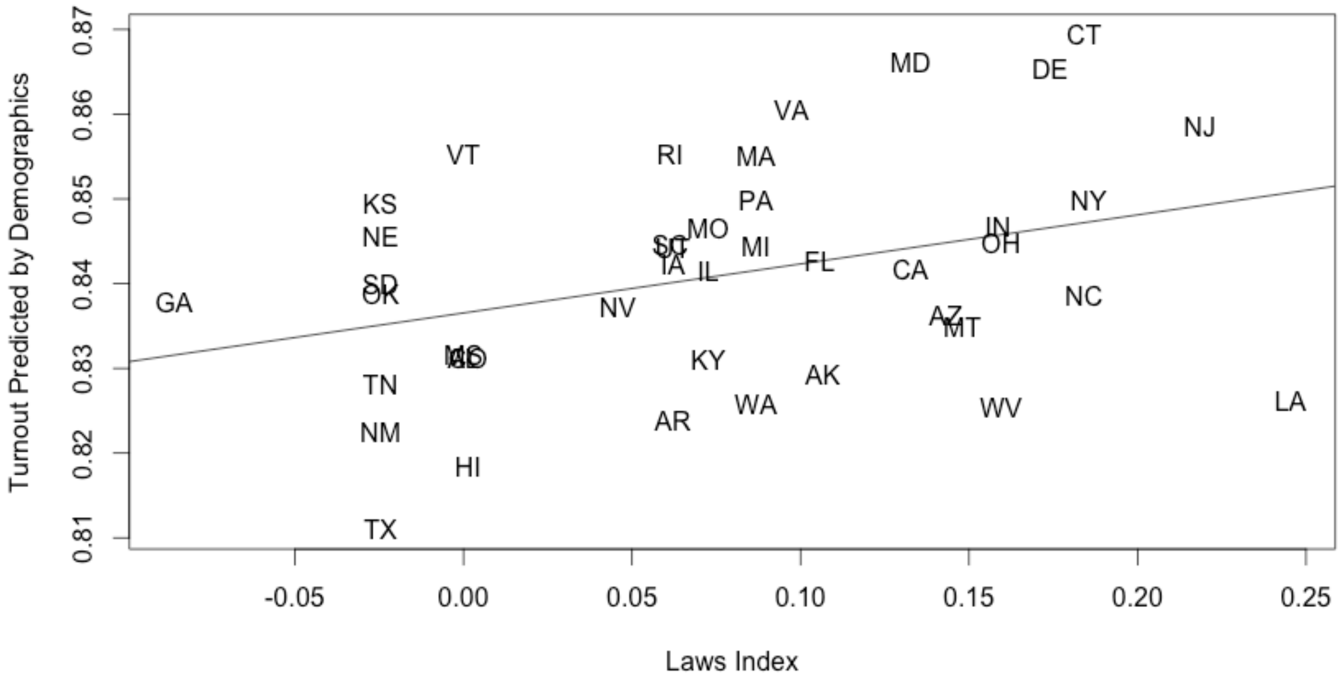

Figure 7: Postregistration Laws and Residual Turnout in 2000

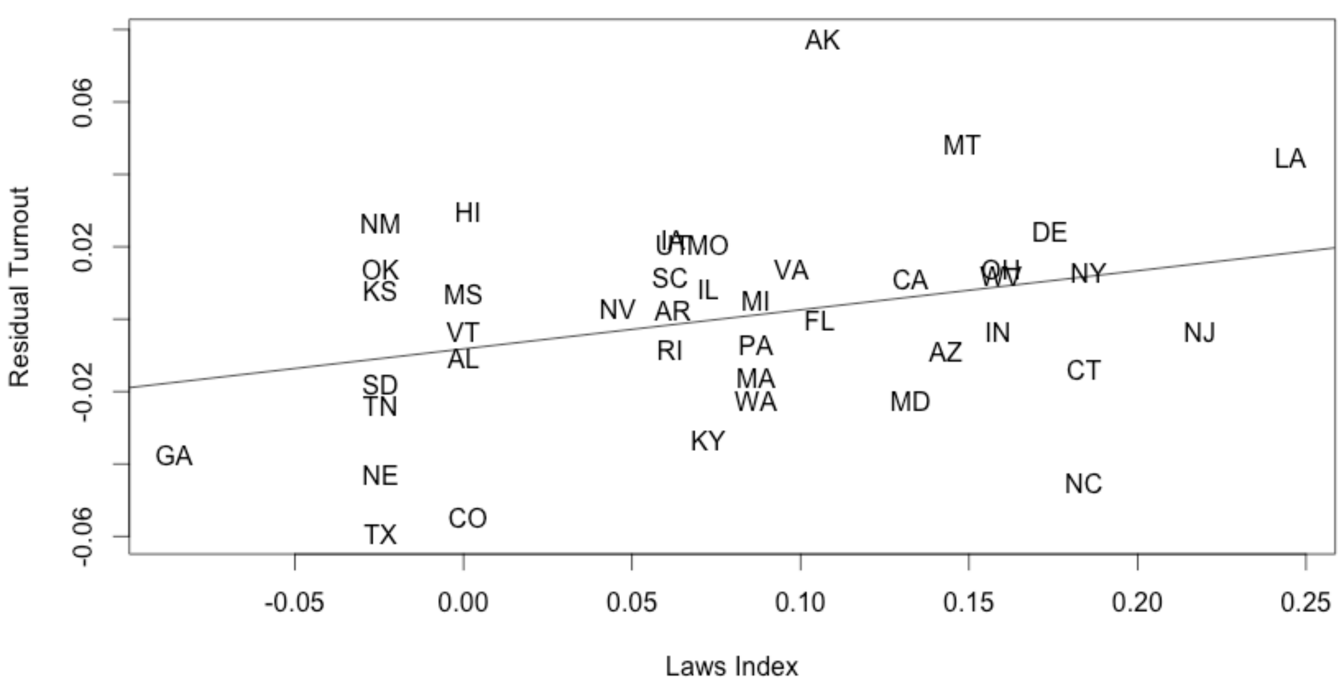

Table 1: Probit Predicting Turnout Among the Registered, 2000

Time off for Private Employees	-0.09*	
	(0.03)	
Time off for Public Employees	0.06	
	(0.06)	
Mailed Polling Info	0.03	
	(0.05)	
Mailed Sample Ballot	0.04	
	(0.06)	
Early Voting	0.10	
	(0.07)	
Late Voting	0.09*	
	(0.04)	
Age	0.05*	0.05*
	(0.00)	(0.00)
Age^2	-0.04*	-0.04*
	(0.00)	(0.00)
Employed	0.06*	0.06*
	(0.02)	(0.02)
High School Degree	0.36*	0.37*
	(0.02)	(0.03)
Some College	0.60*	0.60*
	(0.04)	(0.03)
College Degree	0.91*	0.92*
	(0.04)	(0.03)
Income	0.14*	0.16*
	(0.03)	(0.02)
1-2 Years at Current Address	0.22*	0.22*
	(0.04)	(0.03)
2+ Years at Current Address	0.40*	0.40*
	(0.03)	(0.03)
Latino	-0.10	-0.11*
	(0.07)	(0.03)
Black	0.17*	0.16*
	(0.04)	(0.03)
Asian	-0.28*	-0.27*
	(0.06)	(0.06)
Constant	-1.37*	-1.28*
	(0.10)	(0.06)
N	44859	44859
McKelvey and Zavoina R^2	0.16	0.15

Standard Errors Clustered by State

* indicates significance at the 95% confidence level or higher
^ indicates significance at the 90-95% confidence level

Table 2: Predicting State Level Turnout Corrected for Reverse Causality

	Column 1			Column 2			Column 3					Column 4		
	OLS	Zero Cov		OLS	Zero Cov		OLS	Zero Cov		Zero Cov		OLS	Zero Cov	
DV	Turnout	Turnout	Law Index	Turnout	Turnout	Law Index	Turnout	Turnout	Law Index	Turnout	Law Index	Turnout	Turnout	Law Index
Law Index	0.12*	**-0.05**	---	0.12*	**-0.01**	---	0.11^	**-0.05**	---	**-0.05**	---	0.12*	**0.05**	---
	(0.06)	**(0.10)**	---	(0.06)	**(0.09)**	---	(0.06)	**(0.09)**	---	**(0.08)**	---	(0.06)	**(0.05)**	---
Turnout	---	---	1.16*	---	---	0.94*	---	---	1.10*	---	1.10*	---	---	0.69*
	---	---	(0.43)	---	---	(0.41)	---	---	(0.37)	---	(0.36)	---	---	(0.33)
Demographics	0.40*	0.49*	*instrument*	0.42*	0.48*	*instrument*	0.45*	0.54*	*instrument*	0.54*	*instrument*	0.44*	0.58*	*instrument*
	(0.08)	(0.10)	*for turnout*	(0.09)	(0.09)	*for turnout*	(0.10)	(0.09)	*for turnout*	(0.09)	*for turnout*	(0.09)	(0.05)	*for turnout*
Democratic	---	---	---	-0.03	-0.01	0.17	-0.05	-0.04	0.10	-0.04	0.17	-0.04	-0.13*	0.24
Vote for	---	---	---	(0.05)	(0.05)	(0.12)	(0.06)	(0.05)	(0.12)	(0.05)	(0.12)	(0.06)	(0.03)	(0.14)
South	---	---	---	---	---	---	-1.25	-1.17	1.36	-1.18	1.37	---	---	---
	---	---	---	---	---	---	(1.00)	(0.95)	(2.41)	(0.95)	(2.46)	---	---	---
Battleground	---	---	---	---	---	---	1.21	1.87*	2.37	1.87*	*instrument*	---	---	---
	---	---	---	---	---	---	(0.95)	(0.94)	(2.24)	(0.88)	*for turnout*	---	---	---
Concurrent Sen	---	---	---	---	---	---	-1.02	-0.83	2.21	-0.83	*instrument*	---	---	---
or Gub Election	---	---	---	---	---	---	(0.98)	(0.93)	(2.33)	(0.93)	*for turnout*	---	---	---
Moralistic	---	---	---	---	---	---	---	---	---	---	---	1.34	2.41*	-3.13
Culture	---	---	---	---	---	---	---	---	---	---	---	(1.03)	(0.64)	(2.62)
Individualistic	---	---	---	---	---	---	---	---	---	---	---	1.12	4.37*	-0.84
Culture	---	---	---	---	---	---	---	---	---	---	---	(1.05)	(0.59)	(2.72)
N	42			42			42					40		

* indicates significance at the 95% confidence level or higher
^ indicates significance at the 90-95% confidence level

Appendix: OLS Predicting State Level Turnout Among the Registered, 2000

Time off for Private Employees	-0.01
	(0.01)
Time off for Public Employees	0.02
	(0.01)
Mailed Polling Info	-0.02
	(0.02)
Mailed Sample Ballot	0.02
	(0.02)
Early Voting	0.00
	(0.01)
Late Voting	0.01
	(0.01)
Age	0.00
	(0.00)
Employed	-0.27
	(0.17)
High School Degree	0.56*
	(0.26)
Some College	0.90*
	(0.26)
College Degree	1.00*
	(0.28)
Income	-0.11
	(0.12)
1-2 Years at Current Address	0.75
	(0.48)
2+ Years at Current Address	0.79*
	(0.28)
Latino	-0.06
	(0.08)
Black	0.15*
	(0.07)
Asian	-0.03
	(0.05)
Constant	-0.38
	(0.29)
N	42
Adjusted R^2	0.53

* indicates significance at the 95% confidence level or higher
^ indicates significance at the 90-95% confidence level

Laws Index	0.12*
	(0.06)
Predicted Turnout from Demographics	0.40*
	(0.08)
Constant	0.40*
	(0.09)
N	42
Adjusted R^2	0.49