How State Electoral Institutions Influence the
Electoral Participation of Young Citizens

Julianna Pacheco & Eric Plutzer

Department of Political Science
219 Pond Laboratory
Penn State University
University Park, PA 16802

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State policies play a major role in fostering or hindering electoral participation. In particular, political scientists have focused on three different types of legislation that have important impacts on overall turnout levels: laws affecting the ease of voter registration, laws affecting enfranchisement, and laws that affect the likelihood that already registered citizens will, in fact, cast votes.

<u>Ease of Registration</u>

The single most important – and studied – factor concerns state laws related to the voter registration process. According to the November, 2004, Current Population Survey, 89% percent of those who were registered to vote actually cast ballots. A nearly identical percentage (86%) of registered voters reported voting in the 2004 National Election Study. These percentages are somewhat higher than those recorded in previous elections. In the somewhat lower turnout election of 1996, the CPS estimates a turnout rate of 82% among registered citizens (US Census Bureau 2005, Table A-6).

Thus, being registered explains the lion's share of individual level electoral participation in presidential contests, and state laws concerning registration have been prominent in voter turnout research. Rosenstone and Wolfinger's (1978) early analysis of data from the Current Population Survey suggested that the nationwide adoption of Election Day registration and allowing evening or weekend registration would have increased voter turnout in the 1972 election by over 8%. A more sophisticated analysis by Rosenstone and Hansen of nine presidential elections concluded that for every ten

days before an election that registration closes, turnout falls by about a point (1993, 130-131 and 207).

Some contrary results, however, have emerged in the literature. Hill and Leighley (1999) find that their (interactive) ease of registration measure has no impact on *aggregate* turnout in the 1980 and 1982 elections. Lloyd (2001) finds that early closing dates have no negative impact on a sample of recent movers and those who had been registered over a long time span. The absence of aggregate state effects could be due to countervailing factors that are accounted for by controls in individual level analyses or due to the fact that state electorates are composed of individuals who were socialized under varying election rules. The contradiction between Lloyd's results and others can be reconciled if the impact of registration laws seen in the aggregate is felt most strongly among younger citizens and more weakly (and gradually) by older citizens. Highton (2004, 509) suggests precisely this in his review essay on registration and voting. However, no systematic analysis of registration laws impact on the political socialization of young citizens has been undertaken. By analyzing a longitudinal survey of a single cohort, we will ensure that all respondents were socialized under the laws that are coded to create the independent variables.

Felon Disenfranchisement

A second important area of state election policy lies in the criteria for having the right to vote. In particular, felon disenfranchisement laws have been estimated to deny the right to vote to 2.8 million citizens (McDonald and Popkin 2001), 3.9 million citizens (Fellner and Mauer 1998), and as many as 4.7 million citizens (Manza and Uggen 2004). There are two rather large unknowns about the overall impact of felon

disenfranchisement laws. One is how many of the disenfranchised would actually vote if they had the legal opportunity. The second is whether these laws have spillover effects that lower the turnout of those who are not strictly covered by the laws. The first question follows from the fact that those convicted of crimes tend to be drawn from low-turnout social groups – those with little education and unstable families, for example. In addition, the experience of being arrested may diminish employment opportunities and otherwise alienate young citizens from networks that might promote civic involvement. In the most thorough analysis to date, Manza and Uggen (2004) estimate that the turnout rate of disenfranchised ex-felons would be only about 20-25% had they regained the right to vote after they completed their sentences. This would only raise the national turnout rate by about a percent or so, but this increase is distributed unevenly across states and is sufficiently large to have changed the outcomes of several statewide elections, both for office holders and presidential electors (Manza and Uggen 2004). For our sample, this possibility is not easily tested. Few adolescents are tried and convicted of adult felonies, and even fewer end up in prison (minors comprised about one fifth of one percent of the total prison population in 2004). Lacking data on the probation or parole status of sample members, we are unable to address these direct effects.

The second question is whether there are indirect spinoff effects as a result of felon disenfranchisement laws. One indirect effect is that the general knowledge that some former felons are disenfranchised leads those who are in fact eligible to *believe* that they do not have the right to vote (Uggen and Manza 2004). Of those who completed all five waves of the NELS, 5% who report at least one brush with the law. The experience of being arrested might interact with the severity of state laws if severe laws lead *people*

to believe that they are disenfranchised. A second indirect impact may be ecological: if the roughly four million disenfranchised citizens are drawn disproportionately from some communities, those contexts will have lower adult turnout and fewer civically active role models than would otherwise be the case. This would suggest that restrictive state enfranchisement laws might have disproportionate impact on African American youth, since blacks are far more likely to have felony convictions than whites.   Finally, a main affect of being arrested, regardless of state law, would suggest either spurious linkages (the types of adolescents who get arrested have low scores on unmeasured traits that promote turnout) or indirect effects through unmeasured pathways such as social isolation; but in either case, state laws would be independent of this effect.

We should note that it is very possible that none of these factors are very relevant to the youngest citizens. Disenfranchisement laws can have a substantial impact only *among those who would have otherwise voted*. Young citizens from disadvantaged backgrounds – those most likely to be arrested in the first place – are not likely to have established a pattern of regular participation anyway. So the impact of these laws might not be seen until they are older and have made a transition from non-voter to habitual voter, a transition that occurs at much older ages for citizens of disadvantaged background (Plutzer 2002).

<u>Post-Registration Policies</u>

Voting is a two-step process (Timpone 1998). Recently Wolfinger, Highton and Mullin (2005) have directed our attention to state policies that come into play only after citizens are registered to vote. They show that state *post-registration* laws can increase the probability that registrants will vote by having longer polling hours (which increase

turnout among the registered by 2-5 points), and by mailing information (sample ballots or reminders about the location of the polling place) to registrants. Mailing such information had a small, net, impact on most voters but increased turnout by 3-4 points for those registrants who had not completed high school.

Should we expect to see such impacts among citizens eligible to vote in the first election or two? It might be argued that few citizens 18-24 have severe time constraints because many are not simultaneously working a full time job and keeping family responsibilities. Thus, in the absence of detailed theory or prior literature, we suspect that this effect might be muted. On the other hand, those who have never voted before and recently registered for the first time might be especially helped by a postcard reminder explaining the location of their polling place and reminding them of the election date, or by sample ballots that might reinforce their knowledge of the candidates, ballot issues, and so on. Thus, the effect of these reforms on young citizens might be stronger than that seen in older citizens of comparable education.

Data and Methods

We use the National Education Longitudinal Survey, 1988-2000 (NELS). The NELS is produced and distributed by the National Center for Education Statistics (NCES). The spring 1988 NELS baseline survey is a nationally representative sample of eighth-graders attending 1,052 schools, both public and private, across the fifty states and the District of Columbia. The completion rate for the initial wave was 93% (Curtin et al. 2002, 195). A random subset of the respondents was selected for follow-up interviews in 1990, 1992, 1994, and 2000 and 79% of those students selected for follow-up completed the entire panel (Curtin et al. 2002, 205). In addition to surveying the students, NCES

also surveyed one of the child's parents in 1988 (87% response rate) and again in 1992 (with a 92% retention rate).

Dependent Variables. We employ two different dependent variables in this analysis. The first is whether respondents were registered to vote in the spring of 1994, roughly two years after their cohort graduated from high school. Seventy percent of the sample reported being registered at that time, when they were roughly 20 years old.

Among those registered in 1994, we examine reported turnout in the 1996 presidential election, four years after their cohort graduated from high school when respondents were roughly 22 years old (respondents were asked retrospectively about their voter turnout in the 1996 presidential election during the 2000 survey). Of those registered to vote in 1994, 69% reported turning out in the 1996 presidential election, yielding an overall reported turnout rate of 57%.

State-Level Independent Variables. Our key independent variables measure state policies affecting ease of registration, felon disenfranchisement, and ease of voting for registrants. To measure ease of registration, we employ the index developed by Hill and Leighley (1993, 1999). This interactive index is the product of two components: one is an index of barriers to registration and ranges from one to six. The second is the number of days before the election that the registration period closes. This ranges from zero (for states with Election Day registration) to 60 days, with a mean in our sample of 25.9. We rescale the interactive index to range from 0 to 1 to ease statistical interpretation.

Our data on felon disenfranchisement laws were collected by Manza and Uggen. As Manza and Uggen show, states vary in the number of electoral restrictions imposed on felons. Some states have no restrictions, some disenfranchise inmates only, some

disenfranchise inmates and parolees, some disenfranchise inmates, parolees and

probationers, and some disenfranchise prisoners, parolees, probationers, inmates, and all

ex-felons. All but two states disenfranchise felons in prison and none of our respondents

were in prison. Hence, our disenfranchisement variable is the sum of all other restrictions

in a state for 1994; the index is highly reliable (Cronbach's alpha = .85) and is

standardized to have a mean of zero and standard deviation of one.

The measures of post-registration laws are those reported by Wolfinger, Highton

and Mullin (2005) and are measured to coincide with the 2000 election. In some cases,

state laws may have been different in the 1996 election but we think that these will

provide a fairly accurate measure of the climate in 1996. We create an additive scale of

post-registration laws based on whether a state has extended voting hours and whether a

state mails ballot or polling information to registrants.[1] This scale ranges from zero to

four with a mean value of 1.38.

Because political competition is positively related to turnout (Rosenstone and

Hansen 1993) we include a measure of state political competition for the 1996

presidential election. State political competition is measured based on the number of

votes obtained by the two parties during the 1996 election as reported by David Leip on

his website. High values represent a state in which the 1996 election was highly

competitive where low values represent a state in which the election was lopsided. This

variable also ranges from 0 to 1 to ease interpretation.

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[1] We do not include whether states allow time off from work in our measure because
most of our respondents would still be in school during the 1996 presidential election.
Indeed the work measures are unrelated to voter turnout among registrants ($r = -.005$).

All of the state level variables were merged with the state of residence as reported by respondents in 1994. In the event that an individual's state of residence in 1994 was missing (as is the case with several respondents who did not attend post-secondary education), the state level measures were merged using the 1992 state as reported by respondents. By using the 1992 state residence, we were able to recover 89% of the missing cases (*N*= 1,635 recovered).

Individual-Level Independent Variables. Prior work has suggested that state laws may interact with whether young citizens live at home and with their education level. In particular, research suggests that registration barriers may have a more negative impact on respondents who are living on their own; respondents who live with their parents may already have the resources to overcome certain registration barriers. We also expect both registration barriers and post-registration laws to have a larger impact on the less educated. In addition, our exploration of the impact of enfranchisement laws requires that we measure reports of criminal activity and arrests because these laws may only affect those youths who have had run-ins with the law.

We measure whether respondents are living with their parents during the 1994 survey as reported by respondents by a dummy variable (1=living with parents). Roughly 49% of our respondents were still living with their parents in 1994. Unfortunately, because there was no survey conducted in 1996, it is impossible to know exactly where respondents were living during the 1996 election, and many of those living at home were no doubt on their own two years later (by 2000, only 11% were living with their parents), so this is the best measure available.

We measure education in two ways in order to capture attainment prior to the 1994 registration variable and the 1996 presidential election. To capture variance prior to the registration in 1994, we use a dummy variable, which indicates a respondent's dropout status based on school records and self-reports. Dropouts who left before what would be their senior year (1992) and who never returned to high school to obtain a diploma or GED were coded as one.

To capture attainment after high school but before the 1996 election, we use a detailed set of monthly status questions from the 1994 interview to measure educational attendance during the 1993-1994 academic year– what would be the sophomore year of college if a student enrolled in college directly after high school. This measure is based on status reports during September, October, and November of 1993 and February, March and April of 1994. A student who reported full-time attendance at a four-year college or university during all six months received a score of 100%. A student, who reported half time attendance during all six months, or full time attendance for three months, would receive a score of 50%. Students attending less than half time are scored as 25% for that particular month. Thus the scale ranges from 0% to 100%. We created a similar score for attendance at two-year colleges.

Our measure of whether respondents were arrested during adolescence is based on self reports during the 1990 and 1992 surveys, what would be 10th and 12th grade, respectively. We create a dummy variable in which one indicates that a respondent reported being arrested in either survey. Five percent of the adolescents reported being arrested prior to 1994 ($N = 609$).

Additional Independent Variables. We attempt to account for all other major factors that can influence the registration and turnout of young citizens. We include measures of gender (female=1), race (white is the omitted category compared with African Americans and Hispanics, regardless of race), parental family income in 1987 (values ranging from 1 for no income to 15 for $200,000 or more), parent's highest degree earned (1-6), number of residential moves between 1988 and 1992, and church attendance in 1992. Because the home political environment is one of the most important influences on registration and turnout (Verba et al. 2005), we include a measure of political discussion within the home (never, sometimes, often) in 12th grade as reported by both students and parents.

Results

We use logistic regression to assess the impact of the key independent variables on registration.[2] To account for clustering within schools, models are reported with Huber-White robust standard errors. Table 1 reports our initial model containing our ease of registration index and summary of disenfranchisement restrictions. We see that the difficulty of registration index has a large (and significant) impact on registration. The odds of registering are three times higher in states with Election Day registration compared to the most restrictive state, and a standard deviation shift in state policy would increase the odds of registration by more than 30% for the average young citizen. In terms of probabilities, a one standard deviation shift of a state's registration laws would result in a 5% increase in registration rates among its youngest citizens. If the *average* state adopted Election Day registration, youth registration rates would increase by about

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[2] Our models are estimated using Stata 9, and our simulations of marginal effects of each variable on the probability of voting are calculated using Stata's prchange utility.

12%. We explored a variety of interactions suggested in the literature – difficulty of registration interacted with education (all three measures) and living at home but none achieved conventional levels of significance (results not reported). In addition, we examined the interaction of the difficulty of registration with geographic mobility. Easy registration should help habitual voters re-register if they move. But since few young voters are registering for the first time, the barriers to registration appear to be invariant to how often they've moved in the past.

In contrast, the felon enfranchisement laws appear to have no effect at all. They have no impact in general, nor do they interact with the experience of being arrested (results not reported). However, as shown in Model 2, black youths living in states with the most restrictive felon enfranchisement laws had somewhat lower registration (the odds of registration were roughly 25% lower) – an effect significant in a one tailed test. This small effect is intriguing but general, as we have no way of discerning if this is an actual spillover effect of the enfranchisement rules or a historical legacy of the fact that many of these laws were enacted as part of broader efforts to limit the rights and opportunities of black citizens (Manza and Uggen 2004).

[Table 1 about here]

We now move on to examining voting. Following the methodology of Wolfinger, Highton and Mullin (2005), we restrict our analysis to those registered two years prior to the 1996 presidential election. Results in Table 2 show that our measure of post registration laws is significant and positively related to youth voter turnout among registrants. The estimate suggests that for each additional "best practice" adopted by a state, youth turnout would increase by 2%, and were the modal state (having one positive

post-registration feature) were to adopt two additional ones, turnout among registered youth in that state would increase by about 4%.

[Table 2 about Here]

Neither felon disenfranchisement laws nor 1996 state political competition matter for turnout among young registrants. While being arrested was not related to registration, it is negatively related to turnout among those who are registered to vote. Those arrested once during high school but who were nevertheless registered are estimated to have turnout rates 9% lower than other young citizens. We also see that registrants who lived at home in 1994 have a higher likelihood of turning out in the 1996 election compared to those registrants who lived on their won. Finally, registrants who dropped out of high school and never returned to receive a diploma are significantly less likely to turnout compared with those who completed their HS degree, with turnout 16% lower than similar youth who completed high school.

As with our model of registration, we explored a number of interactions suggested by some previous studies -- the post registration laws by all three measures of education and living at home as well as disenfranchisement with being arrested--as suggested by the literature, but all failed to reach statistical significance (results not reported).

Discussion

State laws play a major role in determining turnout but these policies differentially impact citizens at various stages in the life cycle. Felon enfranchisement policies reduce rates of turnout only if they impact citizens who would have otherwise voted and do not seem to have any impact upon such a young sample. Laws that make registration relatively easy or difficult make a substantial difference for young people,

almost all of whom were registering for the first time in the time frame of our analysis.
The effects of these laws were large and relatively uniform across major groups. We
estimated that if young citizens living in typical states were to instead be in states with
the easiest registration requirements, their probability of being registered would increase
by twelve points. If 70% of these new registrants were to vote, this would yield a net
increase of 8.4%. Because those who vote in their first eligible election are very likely to
vote in subsequent elections, this increase could diffuse through the age structure, raising
turnout rates of successive cohorts.

Of course, it is possible that those coaxed into registration by low barriers might
include a disproportionate number of apathetic voters, thereby lowering the turnout rate
of those already registered. Some evidence suggests that this is why reforms such as
motor-voter failed to raise turnout as much as expected. If this were true, we would
expect that if we add the difficulty of registration measure to our second model, we
should see that ease of registration should be associated with modestly lower turnout.
We investigated this but found a small, non-significant, effect in the wrong direction.
This study suggests that state policy reforms aimed at registering younger citizens might
fare better than motor voter.

We also found that post-registration policies had modest effects on turnout.
Longer hours and mailed reminders matter to young citizens just as they do to older ones.
If a typical state adopted Election Day registration *and* three post-registration reforms,
youth turnout would increase by about 9%. This small additional gain is because small
gains in post-registration are discounted by the non-registration rate. This study suggests

that the largest gains will come from efforts to increase registration, rather than the turnout of those young citizens who are already registered.

We were unable, however, to replicate several of the interactions reported in other studies, particularly those related to education. More research is necessary to definitively account for this inconsistency across studies. However, one possibility derives from the fact that educational attainment is strongly correlated with birth cohort, with older citizens less likely to have graduated from high school and also more likely to have been socialized in an era of more restrictive registration and post-registration laws. The interactions estimated in cross-sectional studies might very well be picking up the effect of the liberalization of voting rules on those raised in the most restrictive era. If so, it would require the analysis of multiple cohorts over time – something not possible with longitudinal studies of a single cohort or cross-sectional designs such as those used by the NES and CPS.

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Table 1. Logistic Regression Slopes on Voter Registration in 1994

Independent Variable	1 B	2 B
Registration Barriers	-1.08 **	-1.08 **
	(.18)	(.18)
Felon Disenfranchisement	.04	.06
	(.04)	(.04)
Felon Disenfranchisement*Black		-.30 *
		(.18)
Dropped out of HS, Never Returned	-.50 **	-.49 **
	(.17)	(.17)
Arrested at Least Once	-.15	-.16
	(.15)	(.15)
Lived at Home 1994	-.004	-.003
	(.07)	(.07)
Individual Level Controls		
Parental Education	.04	.03
	(.04)	(.04)
Family Income in 1987	.03	.03
	(.02)	(.02)
Non-Hispanic Black	.21	.30 *
	(.18)	(.17)
Hispanic, regardless of race	-.10	-.10
	(.12)	(.12)
Female	-.41 **	-.41 **
	(.08)	(.08)
Number of Residential Moves	-.11 *	-.11 *
	(.06)	(.06)
Sophomore Status 2 Yr. College	.01 **	.01 **
	(.001)	(.001)
Sophomore Status 4 Yr. College	.01 **	.01 **
	(.001)	(.001)
Church Attendance 1990	.01	.01
	(.02)	(.02)
Home Political Discussion	.11 **	.11 **
	(.05)	(.05)
Constant	1.32 **	1.31 **
	(.24)	(.24)
Peusdo R^2	.06	.06
N	8619	8619

Note: Dependent variable is a voter registration in 1994. *p<.05, **p<.01 with one-tailed tests. Standard errors in parentheses

Table 2. Logistic Regression Slopes on Voter Turnout in 1996 Presidential Election Among Registrants in 1994

Independent Variable	1 B
Post Registration Laws	.10 *
	(.05)
Felon Disenfranchisement	-.05
	(.06)
1996 State Political Competition	.04
	(.46)
Dropped out of HS, Never Returned	-.70 **
	(.24)
Arrested at Least Once	-.41 *
	(.19)
Lived at Home 1994	.21 **
	(.09)
Individual Level Controls	
Parental Education	.07
	(.04)
Family Income in 1987	.04 *
	(.02)
Non-Hispanic Black	.28
	(.19)
Hispanic, regardless of race	.13
	(.16)
Female	.24 **
	(.08)
Number of Residential Moves	-.03
	(.05)
Sophomore Status 2 Yr. College	.01 **
	(.00)
Sophomore Status 4 Yr. College	.003 **
	(.001)
Church Attendance 1990	.11 **
	(.02)
Home Political Discussion	.29 **
	(.05)
Constant	-.65
	(.47)
Peusdo R^2	.06
N	5527

Note: Dependent variable is a voter turnout in 1996. *p<.05, **p<.01 with one-tailed tests. Standard errors in parentheses