Electoral Laws and Turnout, 1972-2008

In this paper we examine the impact of electoral laws on overall turnout, and class bias in the electorate. Using turnout in each state in each year we use cross sectional time series analysis to estimate the impact of electoral reforms on turnout, with particular attention to the discriminatory impact of legal changes on persons at different segments of the income distribution. This gives us much more powerful estimates of these effects than were previously available. We do not suffer from the problems of cross-sectional analyses which rely on the assumption that the choice of electoral regime is independent of the likelihood of voting. And by using all presidential elections from 1972 thru 2008 we have much more statistical power than has been provided by previous analyses simply looking at difference of means tests across two elections. We consider the impact of: the number of days prior to election day that registration closes; the availability of election day registration; the availability of no-excuse early voting, and the availability of no-fault absentee voting. We show the net effects of these reforms on the probability of individuals voting, and we calculate the effect on at-risk votes and show that more poorly educated voters are not necessarily those more likely to take advantage of electoral reforms.

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1 Introduction

In this paper we examine the impact of electoral laws on turnout, and on class bias in the electorate. We look at the impact of changes in registration laws on who votes, with particular attention to the discriminatory impact of legal changes on persons at different segments of the income distribution.

The United States is unique among modern democracies in the burden it puts on citizens to vote. In most states citizens must register anywhere from 10 to 30 days before the actual date of the election. And until very recently, almost all citizens had to vote in person on election day. The last 30 years has seen a revolution in restrictions on registration and voting. In assorted states citizens may vote well before the election, they may vote at 'alternative voting cites', or they may vote without going to any polling place by sending in an absentee ballot. States have also eased the registration process through the National Voter Registration Act (NVRA). NVRA mandated that states make available the opportunity to register to vote wherever drivers licenses were issued (thus its common name - Motor Voter), and wherever states provide public benefits.

Whether registration laws place unequal burdens on citizens at different levels of socio economic status has been a central focus of work on voter turnout since Wolfinger and Rosenstone's seminal 1980 work *Who Votes?*. Wolfinger and Rosenstone concluded that assorted registration requirements placed a higher burden on poorly educated voters. That finding has been examined many times since then (Nagler 1991, Brians & Grofman 1999). The publication of *Who Votes?* was followed by the National Voter Registration Act (NVRA), intended to address the burden of registration identified in *Who Votes?*.

The NVRA required states to provide the opportunity to register to vote to citizens at all state agencies dispensing public benefits, including state agencies providing drivers

licenses. The later provision is the best known of the bill. But it often obscures the potentially politically powerful provision of requiring states to provide voter registration assistance in agencies providing aid to poor people. NVRA provided an opt-out mechanism for states who did not wish to provide such registration assistance for voters: they could adopt election day registration (EDR). Only three states (Idaho, New Hampshire, and Wyoming) chose to avoid the registration requirements imposed by NVRA by adopting election day registration, joining the three states which had adopted election day registration in the early 1970s (Minnesota, Maine, and Wisconsin).

In addition to reforms mandated by NVRA, many states have relaxed absentee voting requirements since 1972 – effectively providing absentee voting on demand (or 'no-fault' absentee voting). In many states, as many as one-third of ballots are cast via absentee voting. And several states have experimented with early voting, extending the period when voters can cast ballots up to 45 days prior to the election. And some states have allowed voting at 'alternative voting' cites.

These reforms have generated a substantial scholarly literature. A series of articles looks at the impact of NVRA (Franklin & Grier 1997, Knack 1993, Knack 1995). Another series looks at the impact of EDR (Knack 2001, Fenster 1994, Knack & White 2000, Highton 1997, Brians & Grofman 1999). And several articles have looked at the impact of early voting (Neeley & Richardson 2001, Stein & Garciamonet 1997, Stein 1998), and at the relaxation of requirements for absentee voting. Wolfinger, Highton, and Mullin (2005) look at post-registration behavior of states and the impact of state policies on turnout.

Brown and Wedeking (2006) look at the impact of NVRA on *registration*, as well as turnout. Hill (2003) looks at the impact of NVRA across demographic groups, but only uses two elections: 1996 and 1998. Knack (1995) looked at registration laws across states that existed prior to the implementation of NVRA to examine the effect of provisions that

NVRA mandated.

Fitzgerald (2005) examined the impact of election day registration using a pooled cross-sectional time-series analysis. Her work is closest in design to our paper. The primary difference is that Fitzgerald only looked at the impact of reforms on aggregate turnout, not turnout of specific demographic sub-groups. We are interested in how the impact of reforms varies across demographic groups.

Fenster (1994) estimated the effect EDR had on aggregate turnout in the three early adopters — Maine, Wisconsin, and Minnesota – to be about five percentage points by comparing the change in turnout in those states to the change in turnout in other states pre-adoption and post-adoption. Knack (2001) estimated the change for the second wave of states – Idaho, New Hampshire, and Wyoming, to be about three percentage points using a similar technique. Brians and Groffman (1999) look at the impact of EDR using turnout by demographic groups by state using a least-squares dummy variable analysis. But they only utilize two years of data: 1972 and 1992.

2 Election Reforms We Examine

We look at the the impact of the following variables on aggregate turnout (and for EDR and Closing Days, on turnout by demographic group):

Closing Period : Number of days before the election that registration closes.

DMV Registration: Indicates that citizens are asked if they want to register to vote when applying for or renewing drivers license.

No-Fault Absentee Voting: Indicates that citizens can request an absentee ballot without a reason.

Election Day Registration: Provision for persons to register and vote on election day.

No-Excuse In-Person Early Voting: Indicates that citizens can cast a ballot in-person prior to election day.

Early Voting Period: The length of the Early Voting period, in days.

We have data on the availability of these voting and registration lawsfor each state for each year from 1972 thru 2008.[1] Aggregate turnout is based on data from the Congressional Research Service and from Mike McDonald. For our estimate of turnout for each demographic group in each state for each presidential election since 1972 we use the Census Bureau's Current Population Survey (CPS) to estimate these quantities. The CPS is a monthly survey of approximately 50,000 households conducted by the Census Bureau, generating 90,000 observations of individuals.[2] The sample size is large enough to guarantee enough respondents per state to generate meaningful estimates of turnout for each demographic group. Since we are working with the raw data, we can aggregate turnout by state for any group we choose.[3]

Our demographic categories of interest include income, education, age, race, and gender. We measure income by the respondents' position in the income distribution: each respondent is assigned to an income quintile based on their reported income. Each respondent is placed in one of four education categories: less than high school, high school graduate, some college, or college graduate.

Table 1 lists the rate at which states adopted three major reforms to the registration process: election day registration, availability of registration at department of motor vehicles, and changes to the number of days before an election that registration closed. Between 1972 and 1976 three states adopted election day registration, and this basically remained constant until three additional states adopted EDR between 1992 and 1996 to avoid the 'standard' ways to meet the obligations imposed by the NVRA. Then between 2004 and

2008 an additional three states adopted EDR. Prior to the 1980s only 3 states provided any sort of voter registration when citizens applied for drivers licenses. Between 1980 and 1992 this number rose to 18, and basically became universal (if one includes the EDR states) with the passage of NVRA. Contrary to some public perception, there has *not* been a substantial change in the requirement of most states for voters to register well before election day. In 1972 28 states required registration 30 days or more before election day. This number did drop to 13 by 2008. However, as we can see in the next column of Table 1, this occurs mostly because of the removal of outliers. In 1972 33 states required registration 25 or more days prior to the election. By 2008 this number had only dropped to 25. So much of the reform consisted in moving states to a registration deadline of within 30 days of the election, but not to a deadline of closer than 25 days.

[**Table 1 Here**]

Table 2 gives the number of states allowing two forms of alternative voting: no-fault absentee voting, and early voting. Here we see drastic change. In 1972 only three states allowed no-fault absentee voting. By 2008 over half of the states allow no-fault absentee-voting. And whereas in 1972 *no* state allowed early voting, by 2008 31 states had some form of early voting. Note that both of these reforms do nothing to help persons who are not registered.

[**Table 2 Here**]

3   Bivariate Data Analysis

Before proceeding to a multivariate analysis we first perform several straight-forward difference of means tests to examine the impact of several reforms. To get an initial estimate of the effectiveness of these reforms, we computed turnout for each state for 1972-2008 using the Census Bureau's Current Population Survey. We aggregated the raw data by demographic

sub-groups to get estimates of turnout by income, education, age, and marital status. If election day registration really raises turnout, then we should see a larger increase in turnout from pre-EDR adoption (i.e., 1972) to post-EDR adoption (i.e., 1976 thru 2008) for the three states that adopted EDR prior to 1976 than for the states that did not adopt EDR. Since the CPS did not include a state variable in 1976, we compare 1972 turnout to turnout in the post-adoption period of 1980-2008. To eliminate contamination by the second-wave and third-wave states, we drop them from this part of the analysis. Table 3 thus gives turnout for two groups of states, for two periods for each group. First, notice that the EDR states had substantially higher turnout than the non-EDR states even prior to adoption of EDR. But what matters here is that adoption of EDR *increased* this advantage even more. The key columns are the increases in turnout from 1972 to the post-adoption period of 1980-2008. If EDR is effective, we should see larger increases in the first column of one-time increases in turnout (the column for the EDR states) than for the second column of one-time increases in turnout (the column for the non-EDR states). That is exactly what we see. Whereas since 1980 the EDR states have had turnout on average 5.0 percentage points higher than their 1972 turnout, the non-EDR states have seen their turnout increase by only 0.9 percentage points since 1972.[4] This difference between the increase for the EDR states and the non-EDR states is the net effect of adoption of EDR on turnout, and is reported in column 7 ('Net EDR Effect') of Table 3.

[**Table 3 Here**]

A central question of any electoral law reform is whether or not it has differential impact across different types of citizens. Having concluded that EDR does in fact raise turnout, the question remains as to *who* is voting more? Since we have measures of turnout of different demographic groups for each state in each election, we can determine this. Rows 2 thru 6 in Table 3 give the change in turnout for citizens in different income groups. EDR increased the turnout of the second and third income quintiles by 5.6 percentage points

and 4.8 percentage points respectively, while only increasing turnout for the bottom income quintile group by 0.9 percentage points. And EDR increased the turnout of the top two income quintile groups by 2.4 percentage points and 3.1 percentage points, respectively. So it appears that EDR has almost twice as large an impact on the second and third income quintiles as on the top two income quintiles. And the smallest effect is for those in the bottom income quintile. Thus if one were trying to infer the policy impact of EDR adoption, these results show that adoption of EDR should lead to an increase in the representation of voters in the second and third quintiles relative to the fifth quintile; while potentially actually harming the representation of voters in the very bottom quintile.

However, these it is important to realize that these are results about *groups* of voters, not inferences about individuals. The 8th column of the table gives the impact of EDR on the 'at risk' population of citizens among any group: those who were not voting prior to the adoption of EDR. In 1972 aggregate reported turnout was 71.7% in EDR states, thus 28.3% of citizens were not voting. These are the citizens who were 'at risk' to be converted to voters by EDR. Since EDR had a net impact of 4.1 percentage points on turnout, the effect on at-risk voters was that 4.1 out of 28.3 were 'converted' to voters by EDR. Thus the effect on at-risk voters was 14.5%. In other words, between one out of six and one out of seven non-voters was converted to voting via EDR. If we look at the last column of Table 3 we can see that the increased turnout effect of EDR for quintiles 2 and 3 over quintiles 4 and 5 is *not* because any individual person in income quintiles 2 or 3 are more likely to take advantage of EDR than any individual person in income quintile 4 and 5. In fact citizens in quintile 5 were substantially more likely to take advantage of EDR (23.5%) than were citizens in either quintile 2 or 3 (14.7% and 17.3%, respectively).

For citizens with different levels of education, we see the largest effect for the middle two groups: aggregate turnout of citizens with a high school degree or some college goes up by 7.0 and 4.4 percentage points respectively, while turnout of those with college degrees

or more actually decreases by 1.3 percentage points. And note that for the least educated, those with less than a high school degree, aggregate turnout only rose 2.9 percentage points. Comparing the at-risk effect, we see huge differences between this bottom group and the two middle groups. Whereas 7.5% of the bottom group become voters thru EDR, fully 24.2% and 27.0% of previously non-voting members of the middle education group become voters. This result directly refutes previous assertions that people with low levels of education do not vote because the process is too confusing for them. Here we see that a change designged to make registration easier is more likely to be taken avantage of by non-voters with a high school education than by non-voters without a high school education.

In contrast to the relatively small variation in the effect of EDR we see across socio-economic classes, we see stark differences across age groups: younger citizens receive a much greater benefit from EDR than do older voters. Whereas the aggregate turnout of 18 to 24 year olds rose 12.1 percentage points based on adoption of EDR, we cannot even observe a positive effect for those age 61 thru 75, and the effects are decreasing as we look at older citizens.[5]

Table 4 performs a similar analysis for the wave II EDR states (Idaho, New Hampshire, and Wyoming). These states adopted EDR between 1992 and 1996. Here the results are much less impressive. There has in fact been no increase in turnout in the 3 elections since adoption of EDR compared to turnout in the 3 elections prior to adoption of EDR for these states, whereas the non-EDR states had an increase in turnout of 0.4 percentage points. Of course these are small states. And while these states were adopting EDR the non-EDR states were following the mandates of NVRA and adopting other reforms intended to increase turnout. Thus the correct inference here is not that EDR had no effect for these states, but that it did not have a substantially larger effect than did the adoption of NVRA provisions in the other states.[6]

[**Table 4 Here**]

While we have yet to perform a detailed analysis of the Census Bureau data for the EDR Wave III states (Iowa, Montana, and North Carolina), we did perform the same difference in difference calculation using aggregate turnout, based on the 'Vote for Highest Office' values reported by Michael McDonald.[7] The 3 Wave III EDR states went from 57.7% turnout pre-2008 to 64.7%, for an increase of 7 percentage points; whereas the 41 non-EDR states went from a pre-2008 mean turnout of 53.3% to a 2008 mean turnout of 57.6%, for an increase of 4.3 percentage points. Thus the net effect of EDR on the Wave III states appears to have been 2.7 percentage points.

We next turn to an analysis of two other voting reforms: early voting and absentee voting. As we documented above in Table 2, states adopted these reforms over an extended period. The adoption of election reforms by so many states over such a long period makes it reasonably straightforward to determine the impact of the reforms. We utilized two different measurement strategies to determine their net impact in a bivarate setting. First, for each state that adopted no-fault absentee voting between the years 1976 and 2008, we looked at the change in turnout for the state from the election immediately preceeding adoption of no-fault absentee voting to the election immediately following adoption of no-fault absentee voting.[8] To control for any nationwide changes in turnout between elections, we also compute the matching change in turnout between the same election years for the set of states that did not chance their no-fault absentee voting laws between the relevant elections. Thus in the election of 1980, we compare the change in turnout of the 3 states that adoped no-fault absentee voting between 1976 and 1980 to the change in turnout of the 47 states that did not alter their absentee voting laws between 1976 and 1980. Table 5 gives these results. For 1980, the states that adopted no-fault absentee voting had an average increase in turnout 0.2 percentage points, while the 47 states which did not adopt no-fault absentee voting between 1976 and 1980 had an average *decrease* in turnout of 0.4 percentage points. Thus the difference between them, or the estimate of the impact of adoption of no-fault absentee

voting, was 0.6 percentage points. We then compute a weighted average of these comparisons for each election from 1976 to 2008, and estimate the impact of adoption of no-fault absentee voting to be 1.4 percentage points.[9]

[Table 5 Here]

We performed an identical analysis for the adoption of no-excuse early voting by the states. The results are reported in Table 6. Here the weighted average of the effects over the years is only a 0.4 percentage point difference in turnout between states adopting no-excuse early voting and the comparison set of states. Thus these suggest that the states adopting no-fault absentee voting and no-excuse early voting had a small increase in turnout relative to those states that did not.

[Table 6 Here]

While the comparisons we report above are perfectly valid, we note that they are limited in that they are only utilizing one pair of elections per state that adopted a reform: the election immediately prior to reform, and the election immediately after the reform. Since either of those elections may have been unusual for the state, we might ask a more comprehensive question: if we compare all elections from 1972 to the year prior to adoption, to all elections from the year of adoption to 2008 – was the increase in turnout higher for those states that adopted no-fault absentee voting (or early voting) than for those states that did not adopt no-fault absentee voting (or early voting) in the entire period? Thus for each state that adopted no-fault absentee voting, we calculated average turnout for the state for each election since 1972 *prior* to adoption of no-fault absentee voting, and for each election *since* adoption of no-fault absentee voting up to 2008. We look at the change in average turnout for the state between the two periods – pre-adoption versus post-adoption – and compare it to the change in turnout *averaged over those same two periods* for the set of states which never adopted no-fault absentee voting. This the most comprehensive analysis

we are aware of of simply comparing the turnout of states adopting no-fault absentee voting before and after adoption. And we performed an identical analysis for states that adopted early voting.

Here we get slightly different answers. Over the entire period from 1972 to 2008, the states that adopted no-fault absentee voting during this period had an average turnout increase of 1.6 percentage points, considering all pre-adoption elections versus all post-adoption elections. However, over the same period the control group of states also had a turnout increase of 1.6 percentage points when we compare the same turnout in the same time periods. And for early voting, the comparable figures are a 1.3 percentage point increase for the states that adopted no-excuse early voting, but a 3.4 percentage point increase over the same time period for states that did not adopt no-excuse early voting. Thus while the two methods of comparison give slightly different answers, it is clear that it has *not* been the case that states that adopted either no-fault absente voting or early voting had substantial increases in turnout. At best, turnout in the no-fault Absentee Voting states went up by 1.6 percentage points relative to the states that did not adopt no-fault absentee voting, which suggests that the overwhelming majority of absentee voting that is observed is being done by voters who would have voted in the absence of the availability of absentee voting.

So while we have shown that states adopting no-fault absentee voting and no-excuse early voting did *not* experience an increase in turnout relative to states that did not adopt these reforms – this does not answer the counterfactual question as to what would have happened had these states *not* adopted no-fault absentee voting or no-excuse early voting? While it is tempting to infer that no-fault absentee voting and no-excuse early voting do not raise turnout, it may be the case that the set of states adopting them were not random – and that other factors were at work to depress turnout in those states, thus masking the impact of the reforms. The simple conjecture is that it is possible that the states adopting these reforms did so in order to combat other trends negatively affecting turnout. Or, in a less

nefarious conjecture, it is possible that the states adopting these reforms also were states that had less competitive elections post-reform, leading to lower turnout. In the following section we perform a cross-sectional time-series analysis to control for observable characteristics of the states such as demographic characteristics, other elections laws, and competitiveness of elections, that were changing over this period.

4   Multivariable Data Analysis

The difference of means test for the 3 EDR states seems very clean statistically. The only comparision concerns an intervention, and unless one thinks that other attributes of the states in question were changing simultaneously with the switch to EDR, the comparion over time is capturing the effect of EDR. However, the adoption of early voting laws and absentee voting laws, as well as changes in days to registration closing, takes place over a much longer time frame. And it is possible that such adoptions are not independent of changes in other state characteristics likely to affect turnout. Thus to accurately guage the impact of early voting and absentee voting, we estimate a multivariate model of turnout, and of turnout by different demographic groups.

We estimate a model of the following form, where s, t, and d index state and time respectively (we also estimate the model by demographic groups, here we suppress the d subscript):

$$
\begin{aligned}
\mathbf{T}_{s,t} \;=\;\; & \beta_0 \;+\; \beta_1 \mathbf{T}_{s,t-1} \\
& +\; \beta_2\, \mathbf{DaysToClosing}_{s,t} \;\;+\;\; \beta_3\, \mathbf{EDR}_{s,t} \\
& +\; \beta_{23}\, (\mathbf{EDR}_{s,t} * \mathbf{DaysToClosing}_{s,t}) \\
& +\; \beta_4\, \mathbf{NoFaultAbsentee}_{s,t} \\
& +\; \beta_5\, \mathbf{EarlyVoting}_{s,t} \;\;+\;\; \beta_6\, \mathbf{EVPeriod}_{s,t} \\
& +\; \beta_7\, \mathbf{EducDUMMIES}_{s,t} \;\;+\;\; \beta_8\, \mathbf{AgeDUMMIES}_{s,t} \\
& +\; \beta_9\, \mathbf{MeanIncome}_{s,t} \;\;+\;\; \beta_{10}\, \mathbf{PresMargin}_{s,t} \\
& +\; \beta_{11}\, \mathbf{SenMargin}_{s,t} \;\;+\;\; \beta_{12}\, \mathbf{GovMargin}_{s,t} \\
& +\; \Gamma\, (\mathbf{State-Dummies_s}) \\
& +\; \Psi\, (\mathbf{Year-Dummies_t}) \\
& +\; \epsilon_{s,t}
\end{aligned}
$$

We can examine the impact of assorted electoral reforms on aggregate turnout and on turnout of different demographic groups. We are able to take advantage of the over time variation in the introduction of various reforms across states – thus making use of the states as true laboratories of democracy. Using the estimated model parameters we can compute, among other quantities:

- The impact of changing the close of registration from 30 days prior to an election to 10 days prior to an election on turnout of: high-school educated voters, college educated voters, the bottom quintile of the income distribution, the top quintile of the income distribution, etc.

- The impact of a state adopting no-fault absentee voting on the turnout of: high-school educated voters, college educated voters, the bottom quintile of the income distribution, the top quintile of the income distribution, etc.

- The impact of a state adopting election day registration on turnout of: high-school educated voters, college educated voters, the bottom quintile of the income distribution, the top quintile of the income distribution, etc.

However, in this paper we concentrate primarily on the aggregate level results.

We estimated the model above using data for turnout in the 50 states from 1972 to 2008.[10] We measured turnout as the proportion of the voting age population casting votes for the highest office. Since turnout is bounded between 0 and 1, we used the log-odds ratio of turnout as the dependent variable. And we compute panel corrected standard errors. In Table 7 we report the parameter estimates for this model (we do not report the estimated coefficients for the year or state fixed effects). Because of the inclusion of several interactive terms, and the nature of the model with log-odds of turnout as the dependent variable, we do not focus on interpreting these coefficients, but instead focus on the first differences we present later. However, note that the coefficients generally have the expected sign. And note that the specification we use implies that the impact of Early Voting is contingent on the length of the early voting period, and that the impact of EDR depends upon the number of days to closing. According to the estimates in Table 7, an early voting period of 28 days is required for Early Voting to increase turnout. Also note that while we found in Table 5 that states that adopted no-fault Absentee Voting did not see an increase in turnout relative to states that did not adopt no-fault Absentee Voting, in the multivariate model we can see that when controlling for state demographics, closeness of elections, and other institutional rules – no fault Absentee Voting has a statistically significant positive impact on turnout.

[**Table 7 Here**]

To guage the magnitude of the effects of the institutional reforms, we estimated the long-run impact on turnout in a state with a 50% turnout rate if the state adopted a reform, and all other variables were held constant. Thus we incorporate both the immediate impact of the change in each institutional reform on turnout, as well as the long run impact picked up through the lagged log-odds turnout term in the model. These first differences are presented in Table 8. According to our estimates, adoption of No Fault Absentee Voting leads to a 2.7 percentage point increase in turnout. No Excuse Early Voting with a 45 day

voting period would lead to a 2.8 percentage point increase. And a 10 day decrease in the length of the registration closing period would lead to a 0.8 percentage point increase in turnout (for states without EDR). We estimate that adoption of EDR in a state with a 15 day registration closing period would lead to a 3.4 percentage point increase in turnout.

5   Results By Income

We also estimated the same model dissagregated by income. The data used for this is based on Current Population Survey data, and thus excludes 1976 when the Census Bureau does not include a state identifier for all respondents in the data. In Table 9 we present the results just for the coefficient of Days to Closing, broken down by income. While we see variation across the income groups, the estimates are not precise enough to make an inference about how the true parameter varies across the groups.

6   Concluding Thoughts

While the research design presented here has the potential to take advantage of much available data on turnout reforms, we must still face the limitations of that data. There have only been eight presidential elections where we can estimate turnout for different demographic groups for each state. Thus estimating the impact of the electoral reforms whose adoption has been relatively diffuse over time appears difficult. And doing so in a multivariate setting on different demographic groups appears even more difficult.

However, we think that the robust estimation of the impact of EDR across different demographic groups is a major contribution of this paper.[11] We also think that considering the impact of electoral reforms on the 'at-risk' population – the set of *non*-voters – is a

major modification to prior consideration of the impact of electoral reforms. In order to understand the impact of electoral reforms it is important to develop an understanding of the behavioral mechanism through which they operate. Previous work has inferred that larger *net* effects on probability of turnout of people with low levels of education means that such people have a harder time dealing with registration barriers than higher educated people. However, the result we present suggests that it is a different reason that accounts for the lower turnout of poorly educated people relative to higher educated people: since more non-voting higher-educated people become voters when the restriction is lifted than do non-voting lower-educated people.

We also note that we finally provide evidence to show that the relationship between the length of the registration closing period and turnout is *not* merely an artifact of high-turnout states choosing short registration closing periods, but that this is a true causal effect. By isolating the effect of the change in days to closing over time we are able to remove the caveat that all cross-sectional studies of this relationship must bear.

We also believe it is a major contribution to show that when the effect of no-fault absentee voting is considered in a multivariate model, it appears to have an impact on turnout. So to rush to a judgement that no-fault absentee voting does not cause some increase in turnout seems premature. However, we think that our bivariate analysis shows that it is simply a statement of fact that states that have adopted no-fault absentee voting have *not* to this date seen their turnout increase substantially relative to states that did not adopt no-fault absentee-voting.

We also think that our estimates of the impact of No Fault Absentee Voting and No Excuse Early Voting are a major contribution to understanding their effect. And we think the difference in the impact of EDR in the wave I, wave II, and wave III states suggests that variation in implementation of EDR, as well as variation in the strategic use of EDR by the

parties and candidates, can determine how effective EDR is at increasing turnout.

Notes

1. Much of this data collection was facilitated by a grant from Pew Charitable Trust's Making Voting Work, and carried out in collaboration with Daniel Tokaji of Ohio State University, and Nathan Cemenska.

2. We refer to these 90,000 observations as respondents, even though in fact a single individual in each household is used as an informant for other adults in the household.

3. However, there is no state identifier in the CPS for 1976.

4. We also compared turnout for EDR and non-EDR states for the period 1964-1972 to 1976-2004 using actual turnout rather than reported turnout. The calculated impact of EDR is almost identical: EDR states had a decrease of 1 percentage point over this period, but non-EDR states had a decrease of 5.6 percentage points. Again, the inference is that adoption of EDR was worth an additional 4.6 percentage points of voter turnout. [This calculation has not yet been updated to 2008.] We note that our unit of analysis here is *the state*, thus we average over states – not over population.

5. Though note that 1972 was the first election following adoption of the 26^{th}th amendment giving 18 year olds the right to vote, so the comparisons at the bottom of the age distribution should be interpreted with caution.

6. We also note that these three states appear to have had relatively high registration rates prior to adoption of EDR. Thus the impact of adoption of EDR could be mitigated, though we would not expect it to be zero. See Rigby and Springer (2007) for a discussion of the impact of existing registration rates on the effect of electoral reforms. [Author's note: This table has not been updated to include 2008 data.]

7. Date retrieved from http://elections.gmu.edu/voter_turnout.htm.

8. Since our initial year of data is 1972, we do not know if states with no-fault absentee voting in 1972 also had it in 1968, so we can not use turnout in 1972 as a measure of the impact of adoption of no-fault absentee voting. Also note that we are only considering elections in presidential years. So a state that adopted no-fault absentee voting in 1979, the election preceeding adoption would be 1976, not 1978.

9. We weight each year by the number of states adopting no-fault absentee voting for that year.

10. As noted above, the CPS does not include a state identifier for 1976, so for estimates using CPS data we omit 1976.

11. Brians and Grofman look at the impact of EDR on different demographic groups, but they only include 1972 and 1992.

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Table 1: Adoption of Registration Reforms

		Number of States w/		
	EDR	**DMV Reg**	**Closing GE 30 days**	**Closing GE 25 days**
1972	0	2	28	33
1976	3	2	23	32
1980	3	3	22	31
1984	3	5	20	29
1988	3	8	20	29
1992	3	18	19	31
1996	6	50	15	31
2000	6	50	14	30
2004	6	50	14	28
2008	9	50	13	25

Entries are number of states in any given year with the indicated registration provision.

Source for closing-days is generally reading of state statute and codes where available, secondary sources for some state-years.

Table 2: Adoption of Voting Reforms

	Number of States with:		
	No-Fault Absentee Voting	In-Person Early Voting	Election Day Registration
1972	2	5	0
1976	3	6	3
1980	6	7	3
1984	6	7	3
1988	6	9	3
1992	12	11	3
1996	16	14	6
2000	22	22	6
2004	24	27	6
2008	27	31	9

Entries are number of states in the row year with the indicated voting provision.

Data on Absentee Voting and Early Voting coded from state statutes. Implementation may vary by state.

Table 3: Over-Time Impact of Election Day Registration (Wave I)

	EDR Wave I States			non-EDR States			Net EDR Effect[a]	At Risk Effect[b]
	1972	1980-2008	Increase: 1972 to 1980-2008	1972	1980-2008	Increase: 1972 to 1980-2008		
Aggregate	71.7	76.7	5.0	66.0	67.0	0.9	4.1	14.5
Income - Q1	63.2	62.3	-0.9	53.2	51.5	-1.8	0.9	2.4
Income - Q2	61.9	71.3	9.4	57.6	60.7	3.8	5.6	14.7
Income - Q3	72.3	77.3	5.0	66.8	67.0	0.2	4.8	17.3
Income - Q4	79.8	83.8	4.0	72.6	74.2	1.6	2.4	11.9
Income - Q5	86.8	88.0	1.9	82.4	81.2	-1.2	3.1	23.5
Educ - LT HS	61.3	57.2	-4.0	53.6	46.7	-6.9	2.9	7.5
Educ - HS	71.1	72.0	0.9	67.5	61.3	-6.1	7.0	24.2
Educ - Some Coll	83.7	82.4	-1.3	78.5	72.9	-5.7	4.4	27.0
Educ - Coll Plus	93.8	92.3	-1.6	86.9	86.6	-0.3	-1.3	–
Age 18-24	54.6	60.5	5.9	52.6	46.4	-6.2	12.1	26.7
Age 25-30	67.7	69.0	1.3	62.2	56.1	-6.1	7.4	22.9
Age 46-60	78.1	83.3	5.1	73.7	75.8	2.1	3.0	13.7
Age 61-75	78.7	82.2	5.6	71.7	78.9	7.2	-1.6	–
Single	63.6	65.9	2.4	57.0	56.2	-0.8	3.1	8.8
Married	74.9	82.9	8.1	69.8	73.5	3.7	4.7	17.5

Entries are reported Turnout from the Current Population Survey, or increase in reported turnout, for states that adopted EDR between 1972 and 1976, or for states that have never adopted EDR.

[a]The post-1972 change in turnout for the 3 EDR Wave I states minus the post-1972 change in turnout for the non-EDR states.

[b]The percentage of non-voters converted to voters based on net effect of EDR.

The Wave I EDR states are: Maine, Minnesota, and Wisconsin.

Table 4: Over-Time Impact of Election Day Registration (Wave II)

	EDR Wave II States			non-EDR States			
	1972-1992	1996-2004	Increase: 1972-1992 1996-2004	1972-1996	1996-2004	Increase: 1972-1992 1996-2004	Net EDR Effect
Aggregate	68.8	68.8	0	66.1	65.5	0.4	-.4
Income - Q1	54.9	51.6	-3.3	51.5	50.0	-1.5	-1.8
Income - Q2	64.5	62.9	-1.6	59.4	60.4	1.0	-2.6
Income - Q3	68.5	70.4	1.9	66.1	67.0	0.9	-1.0
Income - Q4	76.2	75.7	-0.5	73.1	73.9	0.8	-1.3
Income - Q5	77.8	84.7	6.9	80.9	81.4	0.5	6.4
Educ - LT HS	58.4	48.2	-6.2	53.7	46.9	-6.2	0
Educ - HS	73.0	61.1	-11.9	67.6	61.1	-6.5	-5.4
Educ - Some Coll	79.3	73.7	-5.6	79.0	72.5	-7.5	1.9
Educ - Coll Plus	85.7	89.7	4.0	86.9	86.3	-0.6	4.6
Age 18-24	58.2	42.5	-15.7	52.7	45.1	-7.6	-8.1
Age 25-30	56.6	57.6	1.0	62.6	55.0	-7.6	8.6
Age 46-60	83.0	79.6	-3.4	73.7	75.4	1.7	-5.1
Age 61-75	86.3	81.4	-5.9	71.8	78.4	6.6	-12.5
Single	66.3	56.0	-10.3	57.0	56.2	-0.8	-9.5
Married	72.6	75.2	2.6	69.9	72.8	2.9	-0.3

Entries are reported Turnout, or increase in reported turnout, for states that adopted EDR between 1992 and 1996 (following adoption of NVRA), or for states that have never adopted EDR.

Columns for 'Prior to 1992' include elections from 1972, 1980, 1984, 1988, and 1992.

Columns for 'Since 1996' include elections from 1996, 2000, and 2004.

The Wave II EDR states are: Idaho, New Hampshire, and Wyoming.

Table 5: Impact of Adoption of No-Fault Absentee Voting

| Year[a] | **Change in Aggregate Turnout** | | |
	States Adopting No-Fault Absentee-Voting[b]	**Other States**[c]	**Diff**[d]
1976 (1)	-3.3	-1.0	-2.3
1980 (3)	0.2	-0.4	0.6
1984 (0)	–	–	–
1988 (1)	-2.1	-2.3	0.2
1992 (6)	5.8	4.8	1.0
1996 (5)	-5.3	-7.2	1.9
2000 (6)	3.5	1.7	1.8
2004 (2)	8.8	5.4	3.4
2008 (3)	2.3	0.9	1.4
Weighted Average:			1.4
Pre-Post Comparison[e]	1.6	1.6	0.0

[a] Numbers in parentheses are the number of states adopting no-fault absentee voting in the row-year.

[b] Entries are the average change in actual turnout from the previous presidential election for those states adopting no-fault absentee voting between the previous presidential election and the election of the row-year.

[c] Entries are the average change in actual turnout from the previous presidential election for those states which had no change in their absentee voting laws between the two elections.

[d] Entries are the difference in change in turnout experienced by states adopting no-fault absentee voting between elections, and those states which did not change absentee voting laws between elections.

[e] See text for explanation of pre-post comparison involving all election results from 1972-2008.

Table 6: Impact of Adoption of No-Excuse Early Voting

Year[a]	Change in Aggregate Turnout		
	States Adopting No-Excuse Early-Voting[b]	Other States[c]	Diff[d]
1976 (1)	-9.1	-0.9	-8.2
1980 (1)	2.1	-0.4	2.5
1984 (0)	–	–	–
1988 (2)	-1.9	-2.3	0.4
1992 (2)	7.3	4.9	2.4
1996 (3)	-4.8	-7.2	2.4
2000 (8)	2.0	1.9	0.1
2004 (5)	6.7	5.4	1.3
2008 (4)	0.0	1.1	-1.1
Weighted Average:			0.4
Pre-Post Comparison[e]	1.3	3.4	-2.1

[a] Numbers in parentheses are the number of states adopting no-excuse early voting in the row-year.

[b] Entries are the average change in actual turnout from the previous presidential election for those states adopting no-excuse early voting between the previous presidential election and the election of the row-year.

[c] Entries are the average change in actual turnout from the previous presidential election for those states which had no change in their early voting laws between the two elections.

[d] Entries are the difference in change in turnout experienced by states adopting no-excuse early voting between elections, and those states which did not change early voting laws between elections.

[e] See text for explanation of pre-post comparison involving all election results from 1972-2008.

Table 7: Multivariate Model of Turnout by State-Year, 1972-2008
Dependent Variable: Log-Odds of Turnout for Highest Office (VAP)

	Coeficient	t-statistic
Log-Odds Turnout (t-1)	0.537***	(6.898)
Days to Closing	-0.00149*	(-1.708)
No Excuse Early Voting	-0.0795***	(-2.785)
Early Voting Period	0.00291**	(2.288)
No Fault Absentee Voting	0.0499***	(3.329)
EDR	0.00747	(0.920)
EDR * Days-to-Closing	0.00370**	(2.128)
DMV Registration	0.00492	(0.243)
State Per-Capita Income	-6.29e-06	(-0.896)
Prop Citizens Age 25-30	0.0955	(0.149)
Prop Citizens Age 31-45	0.762	(1.538)
Prop Citizens Age 46-60	0.539	(1.014)
Prop Citizens Age 61-75	0.578	(1.108)
Prop Citizens Age 76-89	0.691	(1.081)
Prop Citizens hsgrad	0.601**	(2.444)
Prop Citizens somecoll	0.0749	(0.235)
Prop Citizens collplus	0.827***	(2.657)
Closeness of Pres Election	0.00396	(0.744)
Closeness of Gov Election gov_close	0.0532	(1.397)
Closeness of Sen Election sen_close	-0.000100	(-0.156)
Gov Election (0/1)	0.0467	(1.623)
Senate Election (0/1)	0.0142**	(2.215)
Constant	-0.651	(-1.254)
Observations	450	
R^2	0.934	

*** p<0.01, ** p<0.05, * p<0.1

Cross-Sectional Time Series estimates, panel-corrected standard errors.

Table 8: Effects of Legal Change on Aggregate Turnout

Institutional Measure	Long-Run Effect of Adopting Institutional Measure on Turnout[a]
No Fault Absentee Voting	2.7
Early Voting (45 day period)	2.8
Days to Closing (10 day decrease, no EDR)	0.8
EDR (Days to Closing = 15)	3.4

[a] Cell entries are the estimated long-run percentage point increase in turnout caused by adoption of the indicated institutional measure.

Table 9: Pooled Cross-Sectional Time Series Estimates
Coefficients of Days to Closing for Different Income Groups
Dependent Variable: Log-Odds of State Turnout

	Coefficients of Days to Closing					
	Agg	**Q1**	**Q2**	**Q3**	**Q4**	**Q5**
Days to Closing	-0.0030	-0.0022	0.0011	-0.0043	0.0005	-0.0043
	(2.05)	(0.79)	(0.47)	(1.90)	(0.21)	(-1.54)

t-stats in parenthesis
Data source is Current Population Survey.