

# Election Day Registration's Effect on U.S. Voter Turnout\*

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*Objective.* Early voter registration deadlines make voting more difficult for many American citizens. In an attempt to facilitate voting, several U.S. states now permit registration on election day, at the height of the campaign. This article examines the turnout effects of adopting election day registration (EDR) and other smaller reductions in closing dates. *Methods.* Primarily using the Current Population Study (1972–1996), we estimate the turnout advantage of EDR for citizens having low, middle, and high socioeconomic status. *Results.* The elimination of closing dates, through EDR, is predicted to produce about a 7-percentage-point turnout boost in the average state. Those having a high school education and middle incomes are expected to see the largest turnout gains, with the less educated and poorer citizens doing almost as well. No evidence is found to link the implementation of EDR to subsequent changes in the electorate's partisan balance. *Conclusions.* Even the most dramatic easing of voter registration costs has a modest effect on the total number of voters and little impact on the long-standing skew toward greater representation of those having higher status in the voting electorate of the United States.

Among modern democracies, U.S. voter registration provisions require a nearly unique degree of individual citizen responsibility, encumbering Americans with greater turnout costs (e.g., Wolfinger, Glass, and Squire, 1990:562–63). In states having typical voter registration rules, for example, citizens must register to vote up to a month before election day. These institutional preregistration requirements are thought to particularly disadvantage America's voter participation vis-à-vis other industrialized democracies (Powell, 1986; Jackman, 1987).

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Since the 1970s, six states have adopted rules allowing residents to register on election day. Interest in reducing the lag time between registration deadlines and voting continues, with the California Assembly approving election day registration (EDR) in June 1999. Although EDR was threatened with a veto by the state's governor, interest in allowing registration at the height of a campaign remains high, typically tied to efforts to increase voter participation (Gledhill, 1999).

Although reducing potential voters' costs by easing voter registration rules seems like a reasonable way to increase turnout, earlier research has often lacked the data to address this relationship comprehensively. This article takes advantage of recent data offering twice as many EDR states to study, and we examine the characteristics of those more likely to vote when EDR is implemented. Our analysis finds promise in EDR's ability to increase turnout, with modest consequences for the composition of the voting electorate, and finds no evidence of changes to the preexisting partisan balance.

Cumbersome voter registration systems' association with nonvoting was noted early in the twentieth century by Merriam and Gosnell (1924) in their classic book *Non-Voting*. Wolfinger and Rosenstone's (1980) *Who Votes?* identifies closing date (i.e., the last day to register before an election) as the legal restriction having the single largest impact on voter turnout. Using cross-sectional Census Bureau survey data, Wolfinger and Rosenstone (1980:88) predict that U.S. presidential election turnout would be 9 percentage points higher if not for early closing dates. Because the first state (Minnesota) to adopt EDR did so in 1973 (Smolka, 1977) and Wolfinger and Rosenstone (1980) are relying primarily on 1972 data, they are not able to evaluate separately the impact of EDR on turnout. Three states (Maine, Minnesota, and Wisconsin) adopted EDR for all general elections between 1973 and 1976 and maintained it through the 1992 election. Addressing EDR's impact in a single election, Squire, Wolfinger, and Glass (1987) report that states that allowed citizens to register and vote on election day had higher aggregate turnout in the 1980 presidential election than did other states with more restrictive registration laws.<sup>1</sup>

The intuition underlying the assumption that shorter closing dates (i.e., ability to register closer to the election day) should increase turnout is quite simple. Allowing voter registration closer to the climax of an electoral campaign should reduce peripheral voters' costs, thereby increasing turnout. The most extreme form of reduced closing date, EDR, entirely eliminates closing date restrictions and substantially reduces registration costs by allowing voter registration when the election becomes almost impossible to ignore. EDR, though, should be distinguished from simply a "zero days"

<sup>1</sup> Squire, Wolfinger, and Glass observe that in a highly mobile society such as ours, EDR facilitates registration for voters who have recently moved. Institutional costs, including individual voter registration, are the principal factors reducing turnout among movers (Brians, 1997a).

closing date. It hardly seems reasonable to posit that even allowing registration as late as *one* day in advance would equal the turnout effects of permitting citizens to register and vote in a single trip. We hesitate to follow several earlier works' assumption of linearity by implicitly treating the change from one-day advance registration to EDR as equal to a change from 30-day advance registration to a 29-day advance registration (e.g., Rosenstone and Hansen, 1993; Mitchell and Wlezien, 1995; Rhine, 1995).<sup>2</sup>

Although the turnout implications of EDR and closing date rules are the subject of a number of studies, these research findings have been constricted by the data and methodology employed. Earlier voter registration and turnout studies fall into three general categories. First, some studies utilize cross-sectional survey designs that may mask selection bias effects (e.g., Kelley, Ayres, and Bowen, 1967; Kim, Petrocik, and Enoksen, 1975; Rosenstone and Wolfinger, 1978; Wolfinger and Rosenstone, 1980; Glass, Squire, and Wolfinger, 1984; Squire, Wolfinger, and Glass, 1987; Teixeira, 1992; Highton, 1997; Timpone, 1998; Highton and Wolfinger, 1998). For example, if a state already has relatively high turnout before liberalizing its registration laws, then subsequent observations of its high turnout may be misattributed to easing the costs of registration. Second, other research uses pooled cross-sectional data that often does not specifically model state-level variation (e.g., Rosenstone and Hansen, 1993; Mitchell and Wlezien, 1995). When pooled cross-sections fail to (statistically) control for each state's unique history and qualities, this design also risks inadvertently ascribing preexisting state characteristics (e.g., high turnout) to other variables. Third, cross-sectional or longitudinal aggregate studies using aggregate data (e.g., Fleury, 1992; Fenster, 1994; Rhine, 1995; Knack, 1995; King and Wambeam, 1996; Franklin and Grier, 1997) are sharply limited in their capacity to control for citizens' individual characteristics known to influence turnout (e.g., education, income) without suffering from an ecological fallacy.

### Research Design

In order to capitalize on the existing turnout and methods literature, this article will (1) employ a longitudinal research design, (2) simultaneously model several prominent competing voter registration provisions, (3) distinguish closing date reductions from EDR, and (4) use a multivariate model,

<sup>2</sup>Although Highton and Wolfinger (1998:88) acknowledge our finding regarding the discontinuity between EDR and a one-day closing date, they utilize a different approach. In lieu of modeling the two concepts separately within a single equation, they use a single "square root of closing date" measure. The theoretical justification of this single, new variable is unknown, but Highton and Wolfinger mention that they obtained the same empirical results using this combined variable or two separate dummies and that the combined variable preserves parsimony.

explicitly controlling for both election year and citizens' state of residence, to permit the analysis of *which* citizens benefit most from EDR. Not to overstate the expected benefits of easier registration, we assume that voters face multiple barriers to turnout: informational, motivational, and procedural. Thus, reducing the costs of registration should have only a modest impact on turnout if other costs (e.g., information or motivation) exceed the perceived benefit of voting. This reasoning is consistent with the expectations of a public choice model emphasizing the multiple sources of voters' costs (see Brians and Grofman, 1999). In sum, we expect a greater turnout increase from EDR than from reduced closing dates, but even with registration available on voting day, not all eligible voters will turn out.

To avoid mistaking preexisting turnout conditions with those produced by changes in particular voter registration laws, we propose a simple natural experiment (Cook and Campbell, 1979). Put simply, our pseudoexperimental analysis compares observed voter turnout before and after registration laws are changed. In contrast to most previous studies, this design explicitly models turnout *change* following a quasi-experimental treatment (i.e., registration law changes).<sup>3</sup>

Our natural experiment studying EDR is facilitated by the nearly simultaneous institution of EDR in several U.S. states at two points in time. Three states (Maine, Minnesota, and Wisconsin) adopted this reform between 1973 and 1976. Additionally, between 1992 and 1996 three more states implemented EDR (Idaho, New Hampshire, and Wyoming). Thus, a comparison of these two groups of states' turnout in presidential elections from 1972 to 1996 with that of states not having EDR should highlight EDR's turnout effects. A preliminary study comparing 1992 to 1996 turnout in the new EDR states (i.e., Idaho, New Hampshire, and Wyoming) found that these states maintained their turnout rank versus other states (Knack and White, 1998).<sup>4</sup> North Dakota is omitted from all analyses because it neither had voter registration provisions during the time period we are examining nor in any way changed its registration laws.

Because this is a natural experiment, we were not able to independently manipulate which states would experience the treatment (i.e., EDR) and which states would constitute the control group. In this case, we find that the EDR states are somewhat more rural and less populous than many U.S. states. Judging from concerns about possible voter fraud associated with EDR raised in the recent debate in California, it seems likely that a large, urban state adopting EDR would include additional security provisions

<sup>3</sup> Although examining aggregate changes, Fenster (1994) and Rhine (1995) also model turnout change following the adoption of new voter registration regulations.

<sup>4</sup> Additionally, based on Current Population Survey data aggregated at the state level, Knack and White report smaller turnout declines in the new EDR states for those having lower socioeconomic status, younger citizens, and the more mobile than for their counterparts in a group of control states.

(e.g., requiring photo identification at the polls, etc.). Still, the group of states currently using EDR represents a variety of regions, with at least two states (Minnesota and Wisconsin) having sizable urban centers.

Turnout of people in every state in the presidential elections spanning 1972 to 1996 is well represented in the Current Population Survey (CPS), a high-quality survey sampling the voting and registration behavior of people from every state in the nation.<sup>5</sup> State identifiers for the 1976 data are not available, but that year's absence is ameliorated by the presence of five subsequent elections. The CPS's very large sample size—ranging from about 90,000 to over 180,000 respondents per year—yields adequate cases to examine registration laws' impact on turnout in every state. Although the CPS is not permitted to query respondents' political behavior beyond voting and registration questions, the U.S. Census Bureau's *Current Population Survey: Voter Supplement* contains excellent data on several key turnout predictors gathered from respondents in each EDR state. The CPS reports respondents' educational attainment, income, age, employment, marital status, gender, and race—variables whose association with turnout has been long established (Wolfinger and Rosenstone, 1980). Unlike the National Election Study, which samples only a subset of U.S. states, the CPS conducts interviews in every U.S. state and, thus, every EDR state. The Appendix describes the variable coding in more detail.

We enhance these data by adding contextual data on state-level variables. A popular reform in recent years, motor voter registration, has been actively employed in several states for a number of years. It has particularly gained favor among voting rights advocacy groups, who see it as having the potential to reach many currently apolitical citizens administratively. Although explicitly studied elsewhere (Brians, 1998), motor voter's effects will be controlled in this analysis. Additionally, political competitiveness has long been theoretically associated with increased turnout (Downs, 1957). In light of some recently emerging empirical links (Hill and Leighley, 1993; Hanks and Grofman, 1998), changing levels of competition and electoral closeness could intervene in our model if the changes occur coincident to changes in registration laws. The well-known Ranney Index, which operationalizes state legislature dominance by a single party, is probably of less value when considering turnout in national elections (King, 1989). Therefore, this analysis employs a competitiveness measure derived from the Democratic Party vote share in each state for each of the presidential elections (1972–1996). The Appendix describes these computations in more detail.

<sup>5</sup>These six U.S. presidential elections spanning two decades are comparable, modern, high-salience presidential elections. Presidential elections are particularly useful to a study comparing states, since they are less susceptible to campaign-specific or state election-specific effects that might be displayed only in a certain state or region.

## **Data Analysis**

Although previous studies (e.g., Fenster, 1994) found that subsequent turnout increases for states adopting EDR, this literature generally offers less insight as to which citizens benefit from EDR's implementation. We attempt to fill this gap, using logistic dummy variable regression to analyze the CPS survey data gathered from 1972 to 1996.<sup>6</sup> Using this multivariate model employing individual-level Census Bureau survey data, we find that even after controlling for many variables associated with voting, EDR exerts a strong and positive influence on turnout.<sup>7</sup> Closing date's negative coefficient indicates that by reducing the number of days to register before an election, states increased the probability that their citizens would vote. The other variables represented in Table 1 yield unsurprising results. As expected, the dummy variables for each election year following 1972 have negative coefficients, reflecting the U.S. turnout decline from 1972 to 1996. The demographic variables age, education, income, employment, marital status, race (black), and female gender are all significantly associated with higher turnout.<sup>8</sup>

<sup>6</sup>A variant of Least-Squares Dummy Variable regression, this estimation technique has been shown to perform well in comparison to other longitudinal multivariate approaches, with two limitations: it can consume tremendous degrees of freedom (i.e., a dummy variable for each state, except one), and it yields dummy variables of unknown substantive interpretation (Stimson, 1985, 922–23). Here, we merge several massive surveys, obviating the first concern. Additionally, since the year and state dummies are intended principally as control variables, the latter restriction poses a trivial concern. Although this technique's use is not wholly uncontroversial (e.g., Erikson, 1995a, 1995b; Radcliff, 1995), its use is well suited to this case. Stimson (1985:926) demonstrated that place (i.e., state) and time (i.e., year) control variables largely remove autocorrelation from the equation, provided that time does not dominate the data (see also Nagel and McNulty, 1996:782). This presents little danger here, as our data provide more than 500,000 valid cases across only six time points.

<sup>7</sup>Although this article examines voter registration laws' effects, we focus on effects measured in turnout percentages rather than registration percentages. Our rationale for focusing on turnout mirrors a broader interest in the operation of the political system. Just as citizens' responsibility to register themselves shoulders some of the blame for comparatively low U.S. voter turnout, politicians seek to remedy low turnout—not just low registration—through easing registration rules. If institutional rules change and no concomitant turnout increase occurs, then the registration or voting rule change may logically be deemed ineffective in its ultimate goal of enhancing turnout. Furthermore, the variables that empirically predict registration are largely the same factors predicting higher turnout (Erikson, 1981).

<sup>8</sup>In addition to the primary coefficients of interest reported in Table 1, we tested numerous interactions for registration laws, education, and income, as suggested by Nagler (1991). Separately capturing each interaction may help to guard against misinterpreting the logistic function's increased sensitivity to small changes near 0.5 (probability) as actual changes in voting probability. We present the noninteractive model in Table 1, because our experimental analyses including more than 100 additional variables failed to substantively alter the findings presented here. Home ownership, a factor typically associated with greater turnout, is not among the control variables because data on respondents' living quarters is not available in all of the surveys (1972–1996).

TABLE 1

Logistic Regression Predicting Election Day Registration's  
Influence on U.S. Turnout: 1972 to 1996

	<i>b</i>	<i>SE</i>
Election day registration	.0924***	.0246
Closing date	-.0043***	.0013
Motor voter (active)	.0825***	.0144
Motor voter (passive)	.0605***	.0140
Age in years	.0783***	.0010
Age squared	-.0004***	.000001
Education (dummies)		
9–12 years	.7738***	.0108
13–14 years	1.5588***	.0132
15–16 years	2.0028***	.0147
17 and over	2.3610***	.0197
Income (dummies)		
2nd quartile	.2750***	.0087
3rd quartile	.5399***	.0092
Highest quartile	.8416***	.0103
Employed	.1657***	.0075
Marital status (married)	.3272***	.0070
Gender (male)	-.1496***	.0064
Race (black)	.2880***	.0102
Political competitiveness	.4405***	.0070
1980 (dummy)	-.3422***	.0127
1984 (dummy)	-.2529***	.0120
1988 (dummy)	-.5149***	.0136
1992 (dummy)	-.2702***	.0153
1996 (dummy)	-.6533***	.0181
Constant	-3.6209***	.0762
Correctly predicted	71.53%	
-2 log-likelihood	625,768	
Number of cases	522,747	

SOURCE: Data are from the 1972, 1980, 1984, 1988, 1992, and 1996 Current Population Surveys.

NOTE: Variable coding descriptions may be found in the Appendix, with additional detail available in Brians, 1997b. The dichotomous control variable coefficients for each state have been omitted from this table but are available at <[www.majbill.vt.edu/polisci/brians](http://www.majbill.vt.edu/polisci/brians)>.

\* $p < .05$ , \*\* $p < .01$ , \*\*\* $p < .001$ , two-tailed significance.

Since all of the registration variables of interest examined in Table 1 are statistically significant, a more nuanced view of each law's contribution to overall turnout should help clarify their *relative* contributions. To present the turnout effect of EDR and changes in closing date more plainly, we computed turnout probabilities from the logistic regression coefficients. In Table 2 the average turnout under EDR is predicted at 59 percent, whereas it is only 53 percent with a typical 30-day closing date—a 6-percentage-

point advantage. These average turnout predictions are based on values for employed, married, white, median-age, male citizens living in average U.S. states. Although turnout inches higher as closing dates shorten, voter turnout still remains higher with the adoption of EDR than with even very short closing dates.

TABLE 2  
Predicted Voter Turnout by Registration Deadline

	Overall Turnout	Low SES	Middle SES	High SES
Election day registration	59%	23%	50%	84%
Closing date before election:				
15 days	55%	21%	46%	82%
30 days	53%	20%	45%	81%

SOURCE: These figures were calculated from the logistic regression coefficients presented in Table 1 and are based on these laws' effects in the 1970s, 1980s, and 1990s as experienced by employed, married, male, median-age residents of an average state. Low socioeconomic status (SES) includes those having less than a high school education and the lowest quartile of income; middle SES is defined as having a high school education and middle income; high-SES individuals possess a college education (four years) and the highest quartile of income. The equation used to calculate predicted turnout was derived from an equation in Liao, 1994 (12).

$$\text{prob}(y = 1) = \frac{1}{1 + e^{-\left(\sum_{K=1}^K b_K x_K\right)}}$$

where *y* is voting, *e* is the natural log, and *b* and *x* are each of the *K* logistic coefficients and independent variable values, respectively.

The effect of EDR on individual turnout is not felt equally by those across the socioeconomic spectrum. By a small margin, EDR has its greatest impact on the turnout of middle-class voters. The three right-hand columns in Table 2 compare voting levels for citizens in low, medium, and high socioeconomic status (SES) groups. Those having medium SES, classified as having earned a high school education and middle income, report a turnout that is about 5 percentage points higher under EDR than under a 30-day closing date, and low-SES citizens experience a 3-percentage-point boost. The positive effects of EDR on those of middle SES should be proportionally stronger, since those having a high school education make up about one-third of the U.S. voting age population. High-SES citizens with a college education and top-quartile income realize only a 3-percentage-point turnout advantage under EDR. This smaller effect is hardly surprising, since a substantial majority within this group are already voters.

Does making voter registration easier with EDR influence the partisan balance? Unfortunately, individual-level party identification data from each



EDR state are not available, but an indirect way to address this question is to compare the proportion of a state's vote given to Democrats versus Republicans before and after EDR. In an analysis comparing the two-party vote share in presidential elections since 1972, we found that the 1980–1996 EDR states were about 5 percentage points more Democratic than other states before EDR, and this margin was maintained through 1996. There was virtually no partisan difference between the newer (1996) EDR states and the rest of the states, before or after changes in registration laws. Thus, these aggregate data yield no evidence that easier voter registration produces a Democratic or Republican electoral boon.

### **Conclusions and Discussion**

Considering a commonsense cost-benefit analysis, it has long been hypothesized that easing voter registration requirements should increase turnout. We posit that statistically visible turnout increases should develop only when the registration procedure changes yield substantial enough cost reductions to move a significant number of people over all of the threshold barriers to voting. This research examines a natural experiment in which treatments (registration law changes) have been applied to voters in some states while those in other states experienced only minor changes in registration laws. Citizens were exposed to (1) modest changes in closing dates or (2) a virtual elimination of closing dates with EDR. Distinguishing EDR, which requires only a single trip for voting and registration, from other closing dates that still necessitate advance registration is supported both theoretically and empirically.

This article's three principal findings, although partly confirming previous research, also offer a challenge to some of the conventional wisdom on voter registration. First, in line with previous studies, we found that where EDR was adopted, average turnout increased (by about 4 percentage points) and has stayed higher than in the rest of the United States. Second, there is a weaker relationship between reduced closing dates and greater turnout, once the effects of EDR are clearly specified. Third, the middle class reaps the greatest turnout benefits from EDR.

In appraising the importance of the modest and enduring turnout boost produced by EDR, one must not forget that the states implementing EDR are not a random sample of U.S. states. They had higher than average turnout to begin with, and are often rural, smaller-population states. It is far from certain what turnout consequences EDR might produce in urban, large-population states. Additionally, turnout gains offered by EDR may be smaller today in light of the implementation of the National Voter Registration Act of 1993 (particularly, motor voter registration provisions) and the fact that by 1996 many states had already reduced their closing dates to fewer than 30 days. Still, many other states share a demographic composition similar to that of the states enacting EDR in the 1970s and 1990s, and

in 1996 18 states had 30-day or longer closing dates, whereas 38 states had 15-day or longer closing dates.

Although it is difficult to anticipate the consequences of drawing current nonvoters into the electorate, the socioeconomically differential turnout effects of EDR and the partisan balance data provide some clues. With the middle class accruing the greatest turnout benefits from EDR, coupled with this group's large size, there is little reason to expect a disproportional electoral gain for either political party or any particular policy agenda. Similarly, the insubstantial changes in partisan balance following the adoption of EDR suggest that both hopes and fears of a Democratic or Republican windfall from easier registration are misplaced.

## **Appendix: Variables and Data**

### ***Variable Coding (Current Population Survey)***

*Closing Date:* Coded in days for each state for 1972 through 1996.

*Election Day Registration:* Represents a *change* in EDR, because no state requiring registration had this system in 1972. Maine, Minnesota, and Wisconsin coded 1 from 1976 to 1996, Oregon coded 1 for 1980 and 1984, and New Hampshire, Wyoming, and Idaho coded 1 for 1996. All other states and all other years were coded to 0.

*Active Motor Voter/Passive Motor Voter:* Based on interviews with voter registration activists and Mitchell and Wlezien (1996), states in each year between 1972 and 1996 were identified as having adopted active motor voter registration.

*Age in Years:* Respondent's reported age in years.

*Age Squared:* Respondent's reported age in years, squared.

*Education:* Years of education coded as a series of dummy variables:

<i>Label</i>	<i>Years of Education</i>
Grade school	1–8 years
High school	9–12 years
Some college	13 and 14 years
Four-year degree	15 and 16 years (including diploma)
Graduate work	17 or more years

*Family Income (quartiles):* To minimize the confounding effects of inflation or other possible time-dependent income covariates, family income was divided into national quartiles for each of the survey years.

*Employed:* A dummy variable coded 1 for currently employed and 0 for all others.

*Marital Status*: A dummy variable coded 1 for married and living together and 0 for living apart.

*Gender (male)*: A dummy variable coded 1 for male and 0 for female.

*Race (black)*: A dummy variable coded 1 for African American and 0 for other races.

*Political Competitiveness*: A variable ranging from 0 to 1 calculated from the percentage voting Democratic in each presidential election for each state. Please see the "Contextual Data" section below for coding details.

### ***Current Population Survey***

The Voter Supplement Files for 1972, 1980, 1984, 1988, 1992, and 1996 (ICPSR 0060, 7875, 8457, 9318, 6365, and 2205) were originally collected and prepared by the U.S. Dept. of Commerce, Bureau of the Census (1974, 1981, 1986, 1994, 1998). Neither the collector of the original data nor the Inter-university Consortium for Political and Social Research (ICPSR) bear any responsibility for the analyses or interpretations presented here.

### ***Contextual Data***

Information on EDR and registration laws' actual implementation was obtained from Smolka (1977), Mitchell and Wlezien's (1996) data set (i.e., ICPSR 01102), interviews with many election officials, and JoAnne Chasnow of Human SERVE.

The competition variable for each state in each election year was computed using party voting data drawn from *Congressional Quarterly's Presidential Elections, 1789–1996* (1997). For each state and for each of the five elections, we divided the Democratic presidential candidate's vote by the total presidential vote in that state:

$$Competition(x) = (1 - (\frac{|(DempresVote) - .5|}{.5})), \quad (A.1)$$

where  $x$  is a given state and *DempresVote* is the Democratic Party candidate's share of the two-party vote in that state. Thus, 1.00 is perfectly competitive, whereas a score of 0 would indicate that either the Republican or Democratic candidate received all of the votes. This calculation was performed for each state and in each year.

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