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Abstract

The number of states with election-day registration (EDR) of voters doubled in the early 1990s, providing a new opportunity to estimate the turnout impact of EDR. Because of some important and neglected features of the "first wave" of EDR states, adopting EDR in the early 1970s, there is good reason to expect this "second wave" to generate larger estimates of EDR's turnout impact. Controlling for other factors, new EDR programs are associated with a turnout increase of about 6 percentage points in the midterm elections (1990 to 1994), and 3 percentage points in the presidential elections (1992 to 1996). Contrary to expectations, these estimates from the "second wave" of EDR states do not exceed those generated by studies of the "first wave" of EDR adoption.

1. Background

America's low and declining voter turnout is one of the most intensively-studied issues in political science. Comparisons with other rich democracies inevitably lay much of the blame for America's low turnout on our uniquely burdensome registration requirements (Lijphart, 1997; Powell, 1986). Researchers emphasizing the impact of registration obstacles argue that "those who are registered vote" overwhelmingly (Squire, Wolfinger & Glass, 1987: 47; also see Erickson, 1981).

Efforts to reform voter registration in the last few decades have focused on several alternative programs. In the early 1970s, about 20 states adopted mail-in registration programs. In part because there was no discernible impact on turnout, the spread of mail-in programs stalled and reformers turned their attention to an alternative that seemed to be more effective.

The apparent success of election-day registration (EDR) in getting more voters to the polls in Minnesota and Wisconsin helped convince President Carter to make it the centerpiece of his program to increase voting participation. Despite large Democratic majorities in both houses of Congress, he failed to get a national program of election-day registration passed in 1977.

Thereafter two independent efforts took shape, one pushing the concept later known as "agency registration" targeted toward registering the poor through public assistance agencies, and the other advocating "motor voter", the creation of Richard Austin, Michigan's Secretary of State, implemented in that state in 1975 and targeted mainly at registering the young and residentially mobile. These two efforts eventually merged, and with the election of a Democratic president, the National Voter Registration Act was signed into law in the summer of 1993. The bulk of new registrants under the NVRA are registering via its "motor voter" provisions, but it also mandated registration at public assistance and other government offices, mail-in registration with no witnessing requirements, and restrictions on purging registrants for failure to vote.

Studies of variants of "motor voter" that were adopted by some states prior to NVRA's passage forecast significant, if modest, turnout impacts (Knack, 1995; Rhine, 1995). Because driver's license cycles last 3 to 5 years in most states, motor voter cannot be judged on the basis of one election. Nevertheless, the record low turnout in 1996--the first election following implementation of NVRA--understandably deepens the suspicions of many analysts that many Americans simply aren't interested in voting, however easy it becomes. Turnout in North Dakota, which does not even have voter registration, fell to 56% of the voting-age population in 1996.

The impact of NVRA cannot be fully analyzed until motor voter programs are fully mature, by the 2000 election. Even then, with dramatically liberalized registration procedures in all states, disentangling the impact of the NVRA from other factors will be difficult.

Paradoxically, the NVRA has provided the best opportunity in a generation to analyze the effects of election-day registration. States could escape NVRA mandates only if they had universal election-day registration at the polls prior to the 1994 elections. Idaho, New Hampshire, and Wyoming took Congress up on this offer, all implementing EDR prior to the 1994 election. These states constitute a second wave of states adopting EDR, with Maine, Minnesota, Oregon and Wisconsin all having adopted it in the mid-1970s.ⁱ Election-day registration at the polls eliminates registration as a separate step in voting, so it provides an upper-bound estimate of the eventual impact of NVRA reforms--which although drastically reducing the inconvenience of registering to vote, will not register everybody, leaving some otherwise-eligible citizens without the option of going to the polls when they wake up on election day.ⁱⁱ

The impact of EDR is immediate and permanent (Fenster, 1994), unlike "motor voter," making its effects easier to estimate. The task of estimating its impact is further simplified by the

fact that it was implemented by all three “new” EDR states prior to the 1994 election, while NVRA states began implementing motor voter and other provisions only in early 1995 (or later). Thus, turnout change between the 1990 and 1994 midterm elections provides a relatively clean test of the EDR's impact.

Because EDR was implemented in these three states between the 1992 and 1996 presidential elections, those elections provide a second new test, but one complicated somewhat by NVRA implementation in most states between those two election years. Comparisons using the 1996 elections necessitate the use of a much smaller “control group” of states, because all but a very few states implemented substantial reforms between the 1992 and 1996 elections. Turnout effects will be explored here by examining changes in state turnout figures between 1990 and 1994, and between 1992 and 1996, using appropriate control groups.

Because of the time-series dimension to the analysis, numerous other factors influencing turnout are implicitly held constant in these comparisons (Fenster, 1994). However, multivariate tests of turnout change are also conducted, so that statistical controls as well as research design controls are employed.

The impact of EDR on turnout is not particularly controversial: the best studies, reviewed below, have produced fairly consistent estimates. All of these estimates, however, have been dependent on a very small number of states adopting (or dropping) EDR over time; similar estimates produced by additional states would contribute to resolving this generalizability issue. Moreover, all of the previous studies have neglected important idiosyncratic features of these states' EDR programs and registration histories, described in the next section. The NVRA-induced adoption of EDR by Idaho, New Hampshire, and Wyoming nearly doubles the number of “natural experiments” for evaluating its turnout effects.

2. EDR: The First Wave

This section briefly reviews evidence on the first wave, and examines the four states adopting EDR in the early 1970s more closely. Table 1 shows the national turnout rate for the 1972 and 1976 elections, and the rates--and state rankings, in parentheses--for the four states adopting EDR between those two elections. All four states ranked in the top half of states even in 1972; with EDR all improved their rankings in 1976. Turnout increase in the EDR states averaged 2.4 percentage points, in the face of a 1.6-point decline nationally, suggesting that the adoption of EDR increased turnout by about 4 percentage points.ⁱⁱⁱ

There is good reason to believe that estimates produced by this first EDR wave understate what EDR's impact on turnout would be in most states. In Minnesota and Wisconsin, about one third of eligible persons lived in counties that did not even have voter registration prior to adopting EDR (Smolka, 1977). Thus, EDR's turnout effects should be about 50% greater if adopted in states in which all voters were previously required to register.

Of the four first wave EDR states, only Minnesota and Wisconsin adopted universal election-day registration at the polls. In Oregon, election-day registrants had to register at another location before going to the polls. The same was and still is true for larger urban centers in Maine,^{iv} although these apparently account for less than half of the state's population. Deleting Oregon from the EDR group yields a mean turnout increase in 1976 of 3.4 percentage points, producing an estimated turnout impact of about 5 percentage points.

Table 2 summarizes turnout change in the first midterm election held with EDR in each of the four states. Three of the four--all but Minnesota--rose in the turnout rankings.

Oregon's abandonment of EDR in 1985 provides a further test. Its turnout rank fell from

8th in 1984 to 13th in 1988, but rose to 8th again in 1992. Its turnout rank in midterm elections actually rose from 8th in 1982 to 5th in 1986, remaining there in 1990. Oregon's experience thus suggests that EDR is largely ineffective unless voters can register at the polls.

The 5-point estimate for EDR's turnout impact suggested by Table 1 is consistent with estimates obtained in more formal analyses. Using data from several elections prior and subsequent to EDR implementation, rather than just the immediately prior and following elections, Fenster (1994) concludes that EDR caused a once-and-for-all turnout rise in Maine, Minnesota and Wisconsin of about 5 percentage points.^v Multivariate analyses by Rhine (1995) using state-level data, and by Wolfinger and Rosenstone (1980), Teixeira (1992), and Mitchell and Wlezian (1995) using survey data, produce estimates ranging from 3 to 6 percentage points.^{vi}

None of these studies account for the fact that EDR could not have had any effect on one third of Minnesota and Wisconsin residents, who were not required to register at all before the advent of EDR. Likewise, none account for the fact that a sizeable fraction of Maine voters cannot register at the polls on election day. Most of these studies--by classifying Oregon as EDR--do not account for the fact that no voters could register at the polls on election day in that state.

3. The Second Wave: Evidence from Midterm Elections

Because the three new EDR states required all residents to register previously and adopted state-wide election-day registration at the polls, a natural hypothesis is that turnout may increase by more in this second EDR wave than in the earlier wave. In fact, estimates detailed below range from about 3 to 6 percentage points -- remarkably similar to those generated from the first wave of EDR adoption.

As shown in Table 3, turnout increases in the second wave states between 1990 and 1994

averaged 5.8 percentage points, while turnout in the remaining 48 states rose by an average of 2.0 points. This difference suggests a turnout impact of about 3.8 percentage points, although the difference in mean turnout changes between the new EDR group and the other 48 states is not significant at conventional levels ($p=.31$, two-tailed test). Two of the three states (ID and WY) substantially improved their turnout rankings among the states.

Although implementation of NVRA-mandated programs began only in early 1995, 11 states had adopted effective "motor voter" programs prior to NVRA passage, with 8 of these new enough to influence turnout differences between 1990 and 1994.^{vii} Subtracting these 8 states creates a control group of 40 states with no major registration reforms between 1990 and 1994. As shown in Table 3, this 40-state group actually experienced a slightly larger turnout increase (2.2 percentage points) than the 48-state group (2.0 points). Based on a comparison with the 40-state control group, new EDR programs provided a turnout edge of about 3.6 points ($5.8 - 2.2$). The difference in average turnout changes between the new EDR group and the 40-state control group is not significant ($p=.37$).

Even in these 40 "non-reform" states, there were minor reforms in some, such as allowing mail-in registration, or making registration forms available on request at various government offices. Although mail-in and "passive" versions of agency and motor voter programs appear to have little effect on turnout (e.g., Knack, 1995), it is useful to examine a more pure four-state control group: the three "old" EDR states and North Dakota (which has long had no registration requirement). While there have been some reforms even in these states (most notably, motor voter in Maine since 1990), they cannot possibly have much of an impact, as most voters in Maine, and all voters in the other three states, can simply show up at the polls on election day to register and vote all at one time and in one place.

Turnout fell marginally (by an average 0.4 points) between 1990 and 1994 in this four-state control group, as shown in Table 3. Based on a comparison with this group, new EDR programs appear to have provided a turnout edge of about 6.2 percentage points ($5.8 + 0.4$). Unlike the other comparisons, the difference in turnout changes between these two groups is statistically significant, despite the very small sample sizes ($p=.04$).

Idaho and New Hampshire had governor's races on the ballot in 1994 but not in 1990, possibly accounting in part for their relative improvement. But Wyoming exhibited the largest turnout increase among the 3 new EDR states, and it had Senate and governor's contests in 1990 as well as 1994.

The multiple regression analysis reported in Table 4 controls for these and other possibly confounding influences on turnout change. The dependent variable is the percentage point change in state turnout between 1990 and 1994, with positive numbers representing increases and negative numbers representing declines. Independent variables control for ballot effects, possible "regression to the mean" effects, demographic changes, and other registration innovations.

Part of any election-to-election change in turnout could simply reflect a "regression to the mean" effect in which states with unusually high or low turnout levels in the previous election move back toward their norms. The negative and significant coefficient on 1990 turnout in Table 4 is consistent with this hypothesis: on average, each 3 percentage-point increment in the level of turnout in 1990 is associated with a 1-point drop between the 1990 and 1994 elections.

The Senate dummy is coded +1 for states with a Senate contest in 1994 but not in 1990, and -1 for states with a contest in 1990 but not in 1994; all other states are coded 0. The regression coefficient thus estimates the turnout impact of adding a Senate contest to a given state's ballot. Table 2 indicates that a Senate race increases turnout by 2 percentage points. A similar dummy

for governor's races produces a large negative but insignificant coefficient.^{viii}

Neither of the demographic controls -- change in per capita income in thousands of dollars, and change in percent African American -- come close to being significant.^{ix}

Other things equal, states with "new" motor voter programs -- those implemented for less than 5 years prior to the 1994 election -- experienced a nearly 3 percentage-point drop in turnout, rather than the expected increase. However, this change is not statistically significant.

Finally and most importantly, new EDR programs are associated in Table 4 with a significant turnout increase, of just under 6 percentage points, in the midterm elections.^x This estimate, coupled with a slightly smaller one generated by the 1992-96 presidential elections (as shown in the following section), provides evidence remarkably consistent with that produced by analyses of the turnout impact of the first wave of EDR adoption.

4. The Second Wave: Evidence from Presidential Elections

Most states undertook major reforms associated with NVRA implementation between 1992 and 1996. Despite those reforms, turnout fell by an average of 6.7 percentage points in the 48 states without new EDR programs (see Table 5). Turnout fell by an average 5.2 points in the new EDR states; the 1.5 point difference in these declines is not significant ($p=.31$). The new EDR states -- indeed, all 50 states and DC -- suffered declining turnout, but maintained (ID) or improved (NH and WY) their turnout rankings among the states.

Because most of the 48 states without new EDR programs implemented major new reforms between the 1992 and 1996 elections, two smaller control groups are used to provide more appropriate benchmarks for evaluating EDR. A 14-state control group includes first-wave EDR states (ME, MN, WI), the state with no registration (ND), the sole state subject to NVRA not

implementing it by the 1996 election (VT), and states with active motor voter programs in effect prior to 1992 (AZ, CO, DC, MI, MT, NC, NV, OR, and WA). Even many of these "old" motor voter states implemented other NVRA-mandated reforms -- such as mail-in and agency registration -- after 1992. Thus, a second, smaller group of five non-reforming states is comprised of the three "old" EDR states, North Dakota, and Vermont.

Turnout decline in the 14-state group averaged 8.1 points, compared to the new-EDR group's 5.2 point drop. This difference suggests a turnout impact for EDR adoption of about 2.9 percentage points, and is statistically significant ($p=.08$, or $.04$ for one-tailed test).

Turnout decline in the 5-state control group averaged 9.6 percentage points. The 4.4-point difference between that decline and the new EDR group's 5.2-point drop is statistically significant ($p=.03$).

The multiple regression analysis of Table 6 produces a smaller, but statistically significant, estimate of 3 percentage points for EDR's turnout-enhancing impact. This regression is closely analogous to that of Table 4, but with turnout change from 1992 to 1996 as the dependent variable, instead of 1990-94 turnout change.

Other than the smaller EDR coefficient, results in Table 6 differ from those of Table 4 in several ways. Both Senate and gubernatorial races significantly increase presidential-year turnout, but by very little, with point estimates of less than a single percentage point. The "regression-to-the-mean" effect is only about half as strong as for the midterm elections.

Dummies are included for two groups of states with alternative reforms to EDR: (1) states implementing motor voter in 1991 or earlier, for which NVRA mandates are likely to have little impact on 1992-96 turnout change, and (2) states implementing NVRA in 1992 or later, whether voluntarily or because of NVRA mandates. Turnout should have risen somewhat in this latter

group of states, although most of them did not complete a driver's license renewal cycle before the 1996 election. The omitted category of states, coded 0 for these two dummies and for the new EDR dummy, is comprised of the 5 non-reform states (ME, MN, ND, VT, WI). As hypothesized, turnout declines the least for the new EDR states, then for the new motor voter states, then for the old motor voter states, and declines most for the omitted category of non-reformers, controlling for other variables. The only statistically significant difference between non-reformers and reformers is for the new EDR states, however, as neither motor voter coefficient is significant.

5. Conclusion

This study investigates the effects of EDR on overall turnout, and on turnout rates for different groups, by comparing turnout changes for the 3 new EDR states and control groups of no-reform states. Previous estimates of EDR's impact on turnout have been based on only a few states, and typically have ignored idiosyncracies of registration law in these states. Many Minnesota and Wisconsin residents were not required to register even before the advent of EDR. Voters in Oregon and in parts of Maine could register on election day, but not at the polls. All four states were historically high turnout states, subjecting any cross-sectional estimates to omitted variable bias. Despite these characteristics of first-wave states, estimates of EDR's turnout impact generated by the second wave of EDR implementation are remarkably similar to those produced by studies of the earlier wave, varying from about 3 to 6 percentage points.

While perhaps reassuring from an empirical researcher's point of view, these findings must be disappointing to reform advocates who hoped for much larger increases in turnout from the NVRA. Because most but not all eligible persons will eventually be registered by motor voter and other programs in the NVRA states, one can interpret EDR's impact as an upper-bound

estimate of NVRA's turnout impact. Turnout in the 1996 election certainly provides no reason to suspect that 3-6 percentage points is too modest a prediction. While not a full test of brand-new motor voter programs, the experience of 1996 suggests that most new registrants will not vote.

A final cautionary note is that all of the EDR states, from both waves, tended to have above-average turnout rates even prior to their adoption of EDR. Reforms in lower-turnout states may eventually prove to have larger percentage-point impacts, simply because they are starting from a lower base.

Endnotes

i. Oregon repealed EDR by initiative in 1985, in response to a religious cult's takeover of the town of Antelope (renamed "Rajneesh") and threatened takeover of the Wasco County government.

The Rajneeshees recruited hundreds of street people from around the country to vote in the local elections held on November 6, 1984. (See "Guru Recruits Drifters; Takeover by Outsiders Feared," Washington Post, Sept. 24, 1984, page A1.) Ohio adopted EDR in early 1977, only to overturn it via a constitutional amendment ratified by voters in November 1977, without ever trying it in a national election year.

ii. On the other hand, the names of motor voter registrants (but not election-day registrants) appear on lists that can be obtained by parties and used in get-out-the-vote efforts. Voter registration lists identify the party affiliations of registrants in about half of the states, including ME, NH and OR but not ID, MN, WI or WY among the EDR states.

iii. The timing of Senate contests is a possible explanation for the relative showings of Oregon and Wisconsin. Oregon had a Senate race in 1972 but not in 1976, which might have contributed to the turnout decline in that state. Wisconsin, which had the largest turnout increase from 1972 to 1976, had no Senate contest on the ballot in 1972 but had one in 1976. In any event, the turnout effects of Senate races appears to be modest (Knack, 1995; Rhine, 1995; Cox & Munger, 1989; Boyd, 1981). Senate and gubernatorial races are controlled for explicitly in multivariate tests below.

iv. Maine, unlike other current EDR states, is ineligible for a waiver from NVRA mandates because it does not have universal election-day registration at the polls.

v. Ideally, the turnout effects of the second wave of EDR eventually could be tested in a similar manner. At a minimum, replication of Fenster's method for the second EDR wave would require waiting until several more elections have occurred. Even then, disentangling EDR's effects from that of the NVRA would be highly problematic.

vi. Fenster did not include Oregon as an EDR state. Rhine (1995) and Brians (1997) classify Oregon as having EDR from 1976 through 1986. Statutes indicate that the state legislature instituted a day-before-election-day deadline in 1985, in effect in the 1986 election, during which voters approved an initiative restoring the pre-1976 20-day deadline (Crocker, 1996; also see Oregon Revised Statutes, 247.025). Fenster uses a simple EDR/non-EDR dichotomy. All of the other studies cited estimate EDR's effects using the variable "closing date" which varies from 0 (for EDR states) up to 30 or more days. Brians (1997) confirms the intuitive expectation that the impact of closing date is highly non-linear: closing the registration rolls 30 days prior to election day is estimated to reduce turnout by little more than a 30-day closing date. The effects of instituting EDR will vary slightly by state, however, as previous closing dates differ.

vii. These were DC, HI, MT, NC, NV, OR, TX, and WA. Programs in AZ, CO, and MI had been in effect for a full driver's license cycle by the 1990 election. "Passive" programs that require applicants for drivers' licenses to ask for registration forms are not classified here as motor voter, nor are nominally "active" programs in NJ, OH and WV which apparently were not fully implemented.

viii. Evidence on gubernatorial contests on a state's ballot varies widely. They are associated with a turnout increase of just under 2 percentage points in Rhine (1995) and Knack (1995), who use state-level data from the 1970s through 1992 and control for fixed state effects. When presidential years are separated from midterm years, this positive effect surprisingly is found to hold only for the presidential years (Knack, 1995). Cox and Munger (1989) find small (about 1 percentage point) and only marginally significant effects (p values of about .10 for two-tailed tests) for Senate and gubernatorial races, using congressional-district level data for the 1982 election. Boyd (1981), perhaps the best-known study of election-calendar effects, finds that concurrent gubernatorial elections raise a state's turnout by nearly 6 percentage points while Senate contests have no effect. However, he uses survey data (from the 1980 National Election Studies) representing only one election and only about 30 states.

ix. State-level data on other demographic variables, such as educational attainment, age and residential mobility, are unavailable for non-Census years.

x. This estimate is not sensitive to outliers, as found by using alternative estimation procedures that weight outliers less heavily than OLS. Robust regression and median regression, respectively, generate EDR coefficients of 6.0 and 7.6 (with standard errors of 2.5 and 2.0).

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Table 1

Turnout Change, 1972 to 1976

	1972	1976	Change	Senate
US	55.2	53.6	-1.6	--
4 EDR	63.4	65.8	+2.4	--
3 EDR	63.8	67.2	+3.4	--
ME	60.3 (21)	63.7 (6)	+3.4	72, 76
MN	68.7 (3)	71.5 (1)	+2.8	72, 76
OR	62.1 (16)	61.3 (11)	-0.8	72
WI	62.5 (12)	66.5 (4)	+4.0	76

Table 2

Turnout Change in Early EDR States

	Before	After	Change	National change	Senate	Governor
ME 1970-74	54.1 (13)	50.3 (8)	-3.8	-8.4	70	70, 74
MN 1970-74	60.7 (5)	47.8 (12)	-12.9	-8.4	70	70, 74
WI 1974-78	39.2 (33)	46.0 (11)	+6.8	-1.0	74	74
OR 1974-78	48.7 (10)	50.4 (4)	+1.7	-1.0	74, 78	74, 78
OR 1984-88	61.8 (8)	57.3 (13)	-4.5	-3.0	84	
OR 1982-86	53.2 (8)	52.4 (5)	-0.8	-3.4	86	82, 86

Table 3

Turnout Change and EDR, 1990 to 1994

	1990	1994	Change	Senate	Governor
48 state control	39.6	41.6	+2.0	--	--
40 state control	39.4	41.6	+2.2	--	--
4 state control	50.3	49.9	-0.4	--	--
3 new EDR states	43.3	49.1	+5.8	--	--
ID	45.4 (15)	51.5 (8)	+6.1	90	90, 94
NH	34.3 (39)	37 (40)	+2.7	90	90, 94
WY	50.2 (10)	58.8 (2)	+8.6	90, 94	90, 94

Table 4

Multiple Regression Analysis:

Change in Turnout Rates, 1990 to 1994

Independent variable	coefficient	std. error
Intercept	15.19	5.13
Turnout 1990**	-0.34	0.09
Senate race**	2.00	0.75
Governor's race	-4.16	2.72
Change in per capita income	0.48	1.74
Change in percent black pop.	-0.90	2.15
New motor voter state	-2.75	2.35
New EDR state*	5.91	2.47

NOTE: $R^2 = .34$ and adjusted $R^2 = .23$. Sample size is 51. Mean of dependent variable is 2.17. Standard errors are corrected for heteroskedasticity using White's (1980) method.
 *Significant at .05, 2-tailed test; **Significant at .01, 2-tailed test.

Table 5
 Turnout Change and EDR, 1992 to 1996

	1992	1996	Change	Senate	Governor
48 state control	57.5	50.8	-6.7	--	--
14 state control	61.9	53.8	-8.1		
5 state control	69.5	59.9	-9.6	--	--
3 new EDR states	63.1	57.9	-5.2	--	--
ID	64.3 (11)	57.1 (11)	-7.2	92, 96	
NH	64.2 (12)	57.3 (9)	-6.9	92, 96	92, 96
WY	61.0 (19)	59.4 (5)	-1.6	96	

Table 6

Multiple Regression Analysis:

Change in Turnout Rates, 1992 to 1996

	coefficient	std. error
Intercept	2.10	3.56
Turnout 1992**	-0.17	0.05
Senate race*	0.86	0.38
Governor's race*	0.86	0.42
Change in per capita income	0.10	0.24
Change in percent black pop.	0.14	0.40
Pre-1992 motor voter state	0.32	0.99
New motor voter state	0.94	0.95
New EDR state*	2.92	1.24

NOTE: $R^2 = .42$ and adjusted $R^2 = .31$. Sample size is 51. Mean of dependent variable is -6.63. Standard errors are corrected for heteroskedasticity using White's (1980) method.

*Significant at .05, 2-tailed test; **Significant at .01, 2-tailed test.

Biographical paragraph:

Stephen Knack's previous work on the impact of voter registration procedures has been published in the Journal of Politics, PS, American Politics Quarterly and other journals.