RESEARCH NOTES

Does "Motor Voter" Work? Evidence from State-Level Data

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The National Voter Registration Act of 1993 mandates "motor voter" programs in all states prior to the 1996 presidential election. Using state-level registration and turnout data over the 1976–1992 period, this study finds that motor voter programs already implemented in many states have significantly increased participation rates. A duration-based specification of motor voter is employed, to account for the fact that driver's license renewal cycles last up to six years or even more in some states. Dummy-variable specifications are shown to underestimate the eventual impact of motor voter. Models include state dummy variables to control for long-standing differences in participation rates across states that otherwise bias coefficient estimates for registration closing date and other variables. In contrast to motor voter, other provisions required by the NVRA—including mail-in and agency-based registration, and limitations on the purging of voter rolls—show little evidence of effectiveness in the states where they have already been implemented.

INTRODUCTION

The post-1960 decline in presidential-year voting turnout—partially reversed in the 1992 election—led to increased support among lawmakers for further registration reforms designed to stimulate greater participation. These efforts culminated in passage of the National Voter Registration Act (NVRA), signed into law by President Clinton on May 20, 1993.[1] The NVRA mandates, for all states currently without election-day voter registration, the establishment of mail-in and agency-based registration programs, and eliminates the purging of registrants solely for nonvoting. The key feature of the NVRA, however, is the "motor voter" provision for registering driver's license applicants, which is widely expected to have the

Financial support was provided by the Center for Study of Public Choice at George Mason University. Registration and turnout data were provided by Elections Data Services, Inc. JoAnne Chasnow, associate director for the 100% VOTE project of Human Serve, supplied helpful information on the adoption and administration of agency and motor voter programs in various states. Analyses and interpretation of the data, as well as any remaining errors, are the sole responsibility of the author.

[1]See Public Law 103-31, 103d Congress.

THE JOURNAL OF POLITICS, Vol. 57, No. 3, August 1995, Pp. 796–811

greatest potential for adding new names to the voter rolls. The NVRA requires that "each State shall include a voter registration application form for elections for Federal office as part of an application for a State motor vehicle driver's license." A single combined form may be used; where two forms are used, the voter application form must be automatically provided with the driver's license application, and may require only a minimal amount of information from the applicant.

This article analyzes state-level registration and turnout over the 1976–1992 period, finding strong evidence that the motor voter programs currently in effect in many states have succeeded in raising registration and turnout levels—contradicting other studies using inadequate methodology (Crocker 1990; Calvert and Gilchrist 1993a, 1993b). Other provisions required by the NVRA such as mail-in and agency-based registration, and limits on purging, show little evidence of effectiveness in the states where they have already been implemented.

Data and Methodology

An analysis of motor voter and mail-in programs over the 1972–1988 period by Crocker (1990) found mixed results: motor voter states had somewhat higher registration and turnout than non-motor voter states, but presidential-year turnout continued to decline in a majority of motor voter states following implementation of the programs. Mail-in provisions were found to be associated with *reduced* registration and turnout, in either a with/without comparison of states or a before-and-after analysis of states adopting mail-in registration. Crocker acknowledges that a more rigorous test of these electoral innovations would require controlling for other variables that affect state-level voter participation rates.[2]

For example, states of the Old Confederacy have long had substantially below-average voting rates. Because North Carolina was the only southern state with motor voter during the period covered by Crocker's analysis,[3] a simple comparison across groups of states with and without motor voter may greatly exaggerate its apparent impact.

A simple before-and-after comparison of participation rates in states adopting new programs contains similar flaws to the simple comparisons across groups of states. As Crocker (1990) notes, his before-and-after analysis showing a turnout decline in states adopting motor voter "assumes that no other major changes, which can affect registration and turnout, are taking place at the same time" (7).

[2]Crocker's study was intended for a lay readership of congresspersons and their staffs.

[3]North Carolina adopted and implemented motor voter prior to the 1984 election, but thereafter the program was virtually abandoned under the state's Republican administration, as few driver's license applicants were asked by clerks if they wished to register (no question was included on the application form). Since the 1988 election, North Carolina reinstituted an "active" program and Texas became the second former Confederate state to adopt one.

The present study adopts Crocker's basic approach of estimating the future impact of NVRA-mandated registration procedures on voter participation, making use of the fact that most of these provisions have already been widely—but not universally—implemented by state law. This study, not being constrained methodologically—as was Crocker's—by the requirement of being comprehensible to our elected officials, employs multivariate techniques to test the effects of motor voter, mail-in, agency, and no-purging laws on participation rates. State-level data on registration and turnout rates for the period 1976–1992, collected by Elections Data Services, are used. With nine national elections held over this period, there are nine observations on each state for both registration and turnout.[4]

Pooled time-series cross-section models are employed, with state and year dummy variables. These "least-squares dummy variables" (LSDV) or "fixed effects" models are more appropriate for present purposes than either cross-sectional analyses of single elections or pooled models without unit and time dummies.

In tests of a single cross-section of data, registration law coefficients may be biased by state-specific factors that influence turnout levels *and* the passage of relaxed registration requirements. Registration closing date is illustrative of these endogeneity problems in cross-sectional turnout studies. Registration and turnout are almost invariably shown in cross-sectional analyses to be strongly and inversely correlated with the number of days before election day that the registration rolls are closed: the sooner in October (or November) the deadline for registering is, the fewer registrants (and voters) there are.[5] Crocker (1989, 12) notes that turnout in states permitting registration on election day (i.e., a closing date of zero) exceeded that in states without election-day registration by 12 to 14 percentage points in each of the four presidential elections between 1976 and 1988. However, as Crocker points out, "the particular states with election day voter registration systems have always been higher turnout states" and turnout in fact *declined* in these states as a group following the adoption of election-day registration. State-level voter participation and the adoption of liberal registration practices are apparently *both* related to some underlying and difficult-to-quantify cause, such as a strong "participatory culture."[6] The closing date coefficient is therefore biased in simple cross-sectional analyses, by capturing part of the impact of the unmeasured under-

[4]The District of Columbia is included as a state. Elections Data Services does not provide registration data for North Dakota and Wisconsin, as the former and some rural counties in the latter do not require registration. These states are omitted in analyses of registration, yielding an N of 441 ($51 \times 9 - 9 \times 2$). North Dakota is also omitted from turnout analyses, as several key independent variables—mail-in, motor voter, etc.—are not meaningful for a state without registration. Louisiana had no general elections in 1978 or 1982, yielding an N of 198 ($51 \times 4 - 4 - 2$) for midterm turnout, and 250 ($51 \times 5 - 5$) for presidential turnout analyses.

[5]E.g., see Wolfinger and Rosenstone (1980), and Teixeira (1992), which employ survey data on turnout; Kelley, Ayres, and Bowen (1967) and Calvert and Gilchrist (1993b) obtain a similar finding for closing date with city-level registration data and state-level data, respectively.

[6]States closing voter registration books earlier tend to have lower mail-in response rates for the 1990 U.S. Census, lower church membership rates, and higher crime rates. All of these variables are strongly

lying variable. The inclusion of state dummy variables in a panel model nets out these state-specific unmeasured influences, leaving the registration-law coefficients to pick up only the true impact of the provision itself, rather than adding in the influence of unmeasured variables.

This problem of biased estimates due to correlation of regressors with omitted variables applies not only to single cross-sections, but to pooled time-series cross-sections without unit and time dummies. By treating N units observed over T periods as $N \times T$ independent cases, these models essentially consist of a single cross-section in which N is artificially inflated with the effect of improving t values. If the (ignored) unit effects are in fact significant—i.e., the units have separate intercepts—the assumption of $N \times T$ independent cases yields observed heteroscedasticity and autocorrelation, even if we start with data that are homoscedastic and nonautocorrelated. Forcing all observations to fit a common intercept will lead to uniformly positive residuals for high-turnout (for example) states, uniformly negative residuals for low-turnout states, and large error variance for both groups. Only for medium-turnout states will there be low error variance and little autocorrelation among residuals (Stimson 1985).

By estimating state intercepts, the fixed effects model used here avoids these problems of introducing heteroscedasticity and autocorrelation. Potential disadvantages of fixed effects are (1) loss of efficiency, as many degrees of freedom are lost in estimating unit and time dummies, and (2) attenuation of total variance, as between-unit variance is assigned to the fixed effects, so that variable estimates are informed only by within-unit variance (Stimson 1985). Note that both of these disadvantages make it more rather than less difficult to find a statistically significant impact for independent variables of interest, such as motor voter laws. For variables that change relatively little over the period examined, such as registration closing date and purging provisions, the lack of substantial within-unit variance makes fixed effects perhaps too strong a test (although not changing the fact that purely cross-sectional tests are too weak). For motor voter and mail-in, however, there is substantial within-unit variance as numerous states adopted these provisions over the period under examination (see appendix B).[7]

Control variables in the model include: (1) demographic characteristics (percent high school graduates among adults, percent living at current address less than five years, per capita income in 1976 dollars, and percent unemployed), election calendar indicators (dummies for the presence of a Senate or gubernatorial race on the ballot, and the proportion of the state's congressional seats contested by both major parties on election day) and, for equations including only presidential-year data, the absolute vote margin separating the top two candidates in the state.[8] Registration-

correlated with registration and turnout at the state level (crime negatively, Census response and church membership positively).

[7]See Cook and Tauchen (1984) and Knack (1993b) for additional applications of fixed effects models.

[8]See appendix A for more complete definitions and data sources.

law variables include a dummy variable for election-day registration, a dummy for states closing the books at least 28 days before election day,[9] and another for states that do not purge registrants solely for failure to vote in previous elections.[10]

This analysis follows Crocker's in differentiating "active" motor voter programs from "passive" programs, and extends this distinction to mail-in programs. Previous studies treating mail-in as an either/or proposition (e.g., Wolfinger and Rosenstone 1980; Mitchell and Wlezian 1989; Crocker 1990) overlook potentially important differences in the administration of mail-in registration systems across states. For example, the forms are available at post offices, libraries, and elsewhere in certain states, but not in others, and the programs are more heavily publicized in certain states than in others. Many states require mail-in forms to be notarized and/or witnessed. "Active" mail-in programs are here defined as those requiring neither the witnessing nor the notarization of mailed registration forms, while "passive" programs require either or both. The choice of this characteristic to differentiate active from passive programs is dictated in part by data availability: there is much less information available on how widely the various states distribute mail-in forms than there is regarding witnessing and notarization requirements by states. The distinction can also be defended on grounds of policy relevance, however, as NVRA backers believed anything more than self-attestation would substantially weaken the effectiveness of mail-in. They succeeded in including a stipulation that mail-in forms "may not include any requirement for notarization or other formal authentication," such as witnessing (Public Law 103-31, One Hundred Third Congress), despite strong objections among opponents in Congress concerning the added potential for fraud.

"Active" motor voter states are defined as those in which applicants for driver's licenses are asked either by employees, or via an item on the application form, if they wish to register to vote. This definition is consistent with the version of motor voter required by the NVRA. "Passive" motor voter programs are defined as those in which, at best, forms are merely available on tables or countertops or upon specific request by patrons. These programs are coded for the states over the 1976–1992 period using information provided by the 100% VOTE project of Human Serve (an organization advocating and assisting states with registration reform), Congres-

[9]The usual practice of specifying closing date as a continuous variable ignores significant nonlinearities: the difference between 0 (election-day registration) and 1 for the interval-scale variable "closing date" should have a far greater impact on participation than any other 1-unit increase, as it entails an extra trip to register as opposed to simply an earlier trip.

[10]Certain registration provisions such as evening and Saturday registration, regular registration office hours, and allowing deputy registrars are not included in the model due to lack of sufficiently reliable data covering the full period considered here. These provisions appear to have changed relatively little since 1976, so that fixed state effects will capture most of their impacts (which appear to be minor in cross-sectional studies; e.g., see Teixeira 1992, 122–23). Residency requirements were drastically reduced in the early 1970s, have changed very little since 1976, and apparently have little remaining impact on participation following their reduction (Conway 1981).

sional hearings reports, and state election administrators.[11] As with mail-in programs, the expectation is that registration in states with active programs, controlling for other influences, exceeds that in passive states, which should in turn show higher rates than states with no programs at all.

In addition to adopting the active-passive distinction employed by Crocker (1990), this study introduces a "duration" rather than a dummy specification of motor voter programs. In contrast to other registration provisions, not everyone in a state is immediately influenced by motor voter. Driver's license renewal cycles last up to six years or more in some states. Thus, the impact on registration and turnout should increase with time, at least for the first few elections following the implementation of the program. "Motor duration" is defined as the number of consecutive elections, presidential and midterm, including the "current" election, for which an active motor voter program has been in effect. For example, Michigan implemented motor voter in 1975 and is coded 1 for 1976, 2 for 1978, etc. Beyond some point, say, six or eight years, all but new driver's license applicants will have been exposed to motor voter, and its impact will stabilize. The square of "motor duration" is included in regressions to capture this expected levelling-off pattern.[12]

Agency-based registration programs can in principal be categorized as "active" or "passive" just as with motor voter. However, there were no agency-based programs in which all patrons were asked by employees or on application forms if they wished to register until Minnesota adopted such a program prior to the 1988 election, and there were still very few as of the 1992 election. Because of this lack of variation in the data, separate variables for active and passive agency-based programs cannot be reliably tested, and a single variable will thus be included to represent all agency-based programs.

Effects of NVRA Variables on Participation Rates

The effects of NVRA provisions on both registration and turnout rates are examined here. Registration laws influence turnout only indirectly through their impact on the decision to register. From a statistical point of view, registration is thus the preferred dependent variable.[13] As turnout is one step removed from the

[11] Codings differ in some instances from Crocker's (1990), as 100% VOTE documented several errors in his classifications.

[12] The log of "motor duration" might appear a preferable specification, as the squared term allows the impact to turn *downward* past some point, rather than merely levelling off. However, for the majority of state-year observations in the sample, the value of "motor duration" is zero, the log of which is undefined. Studies of political violence have handled this problem by substituting an arbitrary small number for the zeros, but results are found to be sensitive to the size of the number chosen (Wang et al. 1993).

[13] Some advocates of the NVRA have predicted that registration rates would exceed 90% or even 95% with motor voter, mail-in, and agency registration fully implemented. These forecasts assume that virtually everyone reached by these programs will choose to register; empirical analyses such as this one

registration decision, there is more potential for noise to be introduced in analyzing the effects of registration laws if turnout is instead used as the dependent variable (Cook and Tauchen 1984). Turnout may nevertheless be a useful check on results obtained from looking at registration rates, because of the possibility that state dummy variables and purge laws fail to fully capture differences in the proportion of "deadwood" on the registration rolls in state-level data. Deadwood registrants—those who are deceased or who have moved and may be registered elsewhere, whose names nevertheless remain on the books—comprise on average 10% to 15% of the names on the official state registration rolls, with this percentage believed to vary widely across the states (Piven and Cloward 1989, 582). Estimating effects on turnout directly as well as on registration is also advisable because it is not clear that finding sizable effects on registration rates would imply anything about turnout effects: people registering via motor voter who would otherwise not register won't necessarily show up at the polls on election day. Effects on turnout cannot be estimated simply through multiplying the registration increase by the percentage of registrants who currently vote, as *marginal* registrants may vote at rates substantially below those of the *average* registrant (for evidence from California, see Cain and McCue 1985). The District of Columbia found that nearly 60% of registrants who had registered by traditional means voted in the 1990 elections compared with only 30% turnout among those who had registered via motor voter.

Table 1 presents results for state-level registration rates covering the nine elections occurring over the 1976–1992 period, and for the presidential-year and midterm subsets of these nine elections. The dependent variable is registrants as a percentage of a state's voting age population (VAP).[14] Observations are weighted by the square root of VAP. Table 2 reports analogous regression results for turnout.

Among the demographic variables, education is positively correlated with registration and turnout, but significantly so only for turnout. Mobility significantly depresses registration as anticipated, and significantly depresses presidential-year turnout. Income is never significant, while unemployment shows some evidence of stimulating participation in midterm years.

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would thus only need to examine turnout effects, as the only interesting question would then be how many of the new registrants actually show up at the polls. Anecdotal and statistical evidence indicates, however, that many Americans fail to register not merely because of the inconvenience, but also out of fear that their registration information will be used for juror selection and other nonelectoral purposes (Knack 1992, 1993b). For citizens intent on avoiding jury duty, making it easier to register via motor voter or other programs will obviously be ineffectual until the use of registration lists for juror selection purposes is abandoned. Near-universal registration following passage of the NVRA cannot be assumed, therefore, as long as many state and federal courts choose jurors primarily from voter registration rolls.

[14]Results using the logistic transformation of registration (and turnout), which confines predicted rates to the 0%–100% interval, are nearly identical in terms of both statistical significance and quantitative importance (unsurprisingly, given that very few turnout observations exceed 70%, and even for registration only a couple of observations exceed 90%). Untransformed participation rates are analyzed here so that estimated coefficients can be directly interpreted as percentage impacts.

TABLE 1

STATE-LEVEL REGISTRATION, 1976–1992
FIXED EFFECTS MODEL

	Elections		
	All	Presidential Year	Midterm
Variable	Parameter Estimate (standard error)		
Per capita income	8.562	13.865	6.863
	(5.177)	(7.991)	(7.461)
Unemployment	0.369**	0.228	0.785**
	(0.139)	(0.206)	(0.215)
% with diploma	0.220	0.217	0.190
	(0.131)	(0.182)	(0.206)
% mobile	−0.874**	−1.056**	−0.675
	(0.238)	(0.336)	(0.360)
Senate race	−0.119	−0.351	0.478
	(0.297)	(0.448)	(0.449)
Governor race	0.870*	2.368	−4.285
	(0.397)	(1.680)	(3.668)
% congressional contests	0.158	0.288	0.601
	(1.403)	(2.309)	(2.023)
Election-day reg.	6.498*	6.742	7.662
	(2.726)	(3.918)	(4.001)
Early closing of reg.	−0.779	−0.684	0.817
	(1.052)	(1.449)	(1.884)
No purging for nonvoting	−1.685	−0.742	−1.718
	(1.108)	(1.732)	(1.642)
Agency registration	−2.240**	−2.595**	−1.683
	(0.624)	(0.885)	(0.986)
Mail-in reg. (active)	2.644	6.465**	−2.146
	(1.458)	(1.979)	(2.623)
Mail-in reg. (passive)	−0.263	0.870	−1.284
	(1.179)	(1.559)	(2.014)
Motor voter duration	3.108**	2.919**	3.108**
	(0.504)	(0.682)	(0.932)
Motor voter duration squared	−0.238**	−0.218**	−0.240*
	(0.057)	(0.077)	(0.104)
Motor voter (passive)	2.022**	3.052**	0.764
	(0.563)	(0.831)	(0.870)
Presidential vote margin		0.600	
		(0.850)	
N	441	245	196
R^2	.91	.91	.93

A * (**) indicates significance at .05 (.01) for a two-tailed test. Coefficients for state and year dummies are not shown.

TABLE 2

STATE-LEVEL TURNOUT, 1976–1992
FIXED EFFECTS MODEL

	Elections		
	All	Presidential Year	Midterm
Variable	Parameter Estimate (standard error)		
Per capita income	−5.127	6.604	−14.446
	(5.874)	(4.095)	(9.784)
Unemployment	0.213	0.102	0.517
	(0.153)	(0.105)	(0.271)
% with diploma	0.485**	0.393**	0.610*
	(0.144)	(0.093)	(0.257)
% mobile	−0.198	−0.402*	0.047
	(0.266)	(0.173)	(0.460)
Senate race	0.566	0.275	1.662**
	(0.329)	(0.230)	(0.572)
Governor race	1.762**	2.045*	−5.443
	(0.444)	(0.868)	(4.681)
% congressional contests	4.096**	0.424	5.264*
	(1.549)	(1.193)	(2.546)
Election-day reg.	−2.149	−1.805	−2.035
	(2.993)	(1.989)	(4.971)
Early closing of reg.	0.290	0.066	4.106
	(1.231)	(0.748)	(3.022)
No purging for nonvoting	0.328	−1.089	1.579
	(1.120)	(0.803)	(1.867)
Agency registration	−0.003	−0.562	0.777
	(0.695)	(0.456)	(1.260)
Mail-in reg. (active)	0.591	1.991*	−0.767
	(1.626)	(1.021)	(3.348)
Mail-in reg. (passive)	0.173	0.761	0.137
	(1.315)	(0.805)	(2.565)
Motor voter duration	1.646**	0.790*	3.475**
	(0.561)	(0.352)	(1.199)
Motor voter duration squared	−0.174**	−0.060	−0.438**
	(0.064)	(0.040)	(0.133)
Motor voter (passive)	0.617	0.625	1.521
	(0.625)	(0.427)	(1.101)
Presidential vote margin		−0.910*	
		(0.430)	
N	448	250	198
R^2	.92	.96	.86

A * (**) indicates significance at .05 (.01) for a two-tailed test. Coefficients for state and year dummies are not shown.

Among election calendar variables, congressional and Senate races stimulate additional turnout in midterm elections only. More unexpectedly, gubernatorial races stimulate added turnout only in presidential years. Closeness of the presidential race at the state level is significant but the quantitative impact is small: a vote margin of more than one million (far exceeding the VAP of many states) is needed to reduce turnout by one percentage point.[15]

State dummy coefficients are not shown in tables due to space limitations.[16] Year dummies in presidential-year turnout models appear to suggest a downward trend—but one that was rudely interrupted by the 1992 election:

Year	*B*	*t*
1992	0 [base year]	
1988	−4.1	9.49
1984	1.2	1.53
1980	2.1	1.88
1976	5.6	3.32

This unexplained four-point turnout rise between 1992 and 1988 cannot be attributed to the spread of motor voter, or to higher unemployment or a closer election in 1992, because these variables are controlled for in the model.[17]

Agency-based registration is surprisingly associated with lower registration rates, while no-purge laws show no effects. Mail-in programs appear to be effective in increasing registration and turnout, but only the ones requiring no notarization or witnessing, and only for presidential elections.[18]

"Active" motor voter programs exhibit consistently positive and significant impacts on both registration and turnout, with the marginal effects diminishing over

[15]The square of the state-level presidential vote margin proved insignificant and was dropped from the equation reported. Controlling for voting age population, under the hypothesis that a given vote margin might imply (at least psychologically) a closer election in a larger state than in a small one, also failed to improve the performance of the margin variable.

[16]The 10 highest state effects for presidential-year turnout are, in order, MN, SD, WI, ID, ME, MT, OR, UT, IA, and LA. The 10 lowest are, from smallest to largest, DC, HI, NY, SC, PA, MD, NC, GA, CA, and TX.

[17]See Knack (1993a) for evidence regarding the role of Ross Perot's candidacy in the 1992 turnout rise.

[18]In pooled models that are otherwise identical but fail to control for state effects, active mail-in programs are associated with significantly *lower* registration and turnout. Mail-in programs were categorized here as "active" or "passive" according to whether or not notarization and/or witnessing was required. Insufficient information exists as to the relative availability of mail-in forms across the states. In an attempt to measure the distribution of these forms, mail-in provisions were interacted with agency registration, on the assumption that the forms are more easily obtained in states with registration forms accessible in various government offices serving the public. The NVRA, in fact, requires that agencies providing public assistance and disability services distribute such forms. These tests, however, found no enhanced impact of mail-in associated with this proxy for availability of forms.

TABLE 3

CUMULATIVE IMPACT OF "ACTIVE" MOTOR VOTER

# of elections	Registration: All Elections	Turnout: All Elections	Turnout: Presidential	Turnout: Midterm
1	2.87	1.47	0.73	3.04
2	5.26	2.60	1.34	5.20
3	7.18	3.37	1.83	6.48
4	8.62	3.80	2.20	6.89
5	9.59	3.88	2.45	
6	10.08		2.58	
7	10.09		2.59	
Dummy variable estimate	3.75 [$t = 4.21$]	2.40 [2.50]	0.97 [1.60]	6.01 [2.73]

time as hypothesized.[19] These findings strongly contradict those of previous studies (Crocker 1990; Calvert and Gilchrist 1993a, 1993b), which miscoded states in some cases and neglected the age of the programs. Table 3 indicates how the impact of "active" motor voter on participation rates cumulates over time, to the point where the estimated effects turn downward.[20]

As expected, the percentage-point impact on registration and turnout is generally found to reach its maximum by about the fifth election following implementation. Employing a simple dummy variable specification for motor voter, as in other studies, underestimates the eventual impact of these programs, due to their newness in many states (table 3). For registration, the dummy variable substitute for "motor duration" and "motor duration-squared" indicates a mere 3.75 percentage-point impact on registration, as opposed to the 10 points indicated by the duration specification. For turnout for all nine elections, and for the subset of presidential elections, the dummy version also understates the efficacy of motor voter.

While even the duration-based estimates are perhaps disappointingly modest for the more optimistic advocates of registration reform, they reject the not uncommon view that participation is insensitive to how convenient the process of registering is. They also reject the skeptical view that most registrants signed up via

[19]Iowa's motor voter program registers only first-time driver's license applicants, not renewals. Recoding Iowa as a "passive" motor voter state does not increase the estimated impact of "active" motor voter, however, in turnout or registration regressions (on the other hand, the coefficients on "passive" motor voter rise somewhat).

[20]Estimated cumulative impacts for six or more elections are largely out-of-sample extrapolations, as Michigan was the sole "active" motor voter state until before the 1984 election. In any event, while there are theoretical reasons to expect the marginal effects to diminish, there is no reason to expect them to turn negative. Estimates for the fourth or fifth election are thus likely the most appropriate for gauging the performance of motor voter.

motor voter will not bother to turn out on election day; these estimates suggest that almost one-half of them will.

An unexpected result is the larger estimated impact on midterm than on presidential-year turnout (table 2 and table 3). On the surface, this finding may appear plausible, as there is a larger pool of nonvoters in midterm elections that may be affected by a new stimulus to vote. However, motor voter affects turnout only through registration; if these new registrants are at all similar to other registrants, they can be expected to vote more frequently in presidential than in midterm elections. No explanation is offered here for the inability to confirm these expectations.

The effects of "passive" motor voter—which requires at least a minimal degree of initiative on the part of would-be registrants—are, unsurprisingly, relatively modest, with no significant impact on turnout. A duration-based specification for passive motor voter does not substantially raise its estimated impact on participation, unlike the case for "active" motor voter.

"Active" motor voter states tend to also have mail-in programs: 54% of the state-year observations with active motor voter had mail-in registration, either active or passive, compared with 43% of states without active motor voter (difference is significant at .06 for two-tailed test). Any potential effects of mail-in thus may be obscured by motor voter; where motor voter is registering large numbers of people, mail-in may add little to the registration rolls. Regression coefficients for mail-in therefore may underestimate its effects in a non-motor voter state. However, tests of a motor voter/mail-in interaction term show no support for such a conjecture.

Is "motor voter" as effective as election-day registration? Calvert and Gilchrist (1993a, 1993b) conclude there is no contest: election-day registration is the far superior mechanism. Their comparison is faulty on several grounds, however:

1. They stress that about one-seventh of Minnesota's voters register on election day, implying that this figure can be taken as an estimate of the provision's impact. This conclusion rests on the dubious assumption that none of these voters would have had the foresight to register earlier if it had been necessary.
2. They rely on estimates from state-level cross-sectional analyses, which neglect the fact that the election-day registration states—which, incidentally, are all nonsouthern, have high Census response rates, high church memberships, and low crime rates—*always* had high participation rates.
3. Their estimates—largely borrowed from Crocker (1990)—for motor voter's effects are dependent on miscodings and on neglecting the important duration dimension of motor voter.

The fixed-effects estimates of the registration and turnout effects of closing date presented here are based on very little within-state variation, and thus are not the product of a fair test. An informal estimate of closing date's impact can be obtained by comparing the 6–8 percentage point average turnout lead experienced by

Wisconsin, Minnesota, and Maine before adopting election-day registration with the 12–14 point average advantage subsequent to implementation. This six-point difference is not far above the estimated effects of motor voter obtained here (table 3). Moreover, Smolka's (1977, cited in Teixeira 1992) case-study approach to analyzing the effects of the reform in Minnesota and Wisconsin estimated a much smaller impact of the introduction of election-day registration, of no more than two percentage points on turnout.

Teixeira (1992) notes, in attempting to forecast the impact of motor voter, that election-day registration and motor voter should be close substitutes for each other in enhancing turnout: with either reform, no trip to a registration office is necessary; to vote, only a single trip to the polls on election day is required. From this perspective, a state's choice between election-day registration and adoption of the NVRA provisions may thus be largely a matter of perceived administrative costs and potential for fraud under each alternative.

Conclusion

If the experience of states choosing "motor voter" registration programs can be generalized to states it is imposed on, the registration rolls will be substantially augmented in the coming years. Of greater importance, of course, is the projection that nearly one half of these added registrants will actually vote. Where the active version of motor voter mandated by the NVRA has already been implemented, registration and turnout were significantly higher over the 1976–1992 period than they would otherwise have been. The potential impact on voter participation of the various other NVRA provisions appears to be relatively minor, although available data do not permit a full test of "active" agency registration.

Most states require renewal of a driver's license only every three to five years, or even longer for some states. Because of this lengthy driver's license cycle, motor voter programs are distinguished by their age in this study. Evidence is found that the maximum impact of motor voter on registration rates is not felt until at least several elections subsequent to its implementation.

Estimates from this study, while free of some of the endogeneity problems plaguing other studies, are not wholly free of "self-selection" bias. Motor voter—as well as other reforms—may be *more* effective when mandated in the states that have not yet implemented them than in states already adopting them on their own, because the latter had somewhat smaller pools of nonregistrants and nonvoters to begin with. Maine, Minnesota, Oregon, and Washington have long been high-participation states; all have adopted motor voter, while very few (low-turnout) southern states have. On the other hand, states choosing motor voter or mail-in registration on their own may implement it more effectively than states having it forced upon them, possibly leading to larger impacts in the former than in the latter.

A further issue receiving little or no attention, even in the more careful voter participation studies (e.g., Teixeira 1992), is that of interaction among registration provisions. In states with election-day or motor voter registration, for instance, mail-in may be less effective, because it is less needed: any potential effects from mail-in are "crowded out" by more powerful programs. While no significant interaction effects among motor voter, mail-in, agency, and election-day registration were found in this study, one must remain cautious about assuming that estimates obtained from simple linear specifications using data from past elections—for which many states had either mail-in, or motor voter, for example, but not both—can be used to forecast participation rates for future elections—for which all states without election-day registration will have motor voter, and mail-in, and agency, and purging reform.

Manuscript submitted 11 April 1994
Final manuscript received 27 September 1994

APPENDIX A

VARIABLE DEFINITIONS AND DATA SOURCES

% with diploma: Percent of people 25 and older who are high school graduates. Source: Bureau of the Census.

% mobile: Percent of people aged 5 and older who have lived at current residence less than 5 years. Source: Bureau of the Census. For elections in non-Census years, % mobile and % with diploma are estimated via linear interpolation. For example, % diploma for 1988 = 1/5(% diploma 1990) + 4/5(% diploma 1980).

Per capita income (in logs). Source: Survey of Current Business, Bureau of Economic Analysis.

% unemployed: percent of state's labor force unemployed, average for year. Source: Local Area Unemployment Statistics, Bureau of Labor Statistics.

Vote margin: difference in absolute vote totals for top two presidential candidates in state. Source: *America Votes.*

Senate race: dummy variable indicating presence of Senate race on ballot in state. Source: *America Votes.*

Governor's race: dummy variable indicating presence of gubernatorial race on state ballot. Source: *America Votes.*

% congressional contests: proportion of state's congressional seats contested by both major parties on election day (the sole case of an independent winning is coded as being contested). Source: *America Votes.*

Registration law variable definitions and data sources are described in the text.

APPENDIX B

INTRODUCTION OF REGISTRATION PROVISIONS BY STATE

Date indicates first election in sample period that provision was in effect.

State	Motor Voter	Mail-in	Agency	State	Motor Voter	Mail-in	Agency
AK	1984	1976		NC	1984*,		
AZ	1984*	1992	1992		1986,		
CA	1988	1976*			1992*		
CO	1986*			NH		1976	
CT	1990	1988	1990	NJ	1990,		
DC	1990*	1976*			1992*		
DE		1976*		NM	1992		
HI	1992	1992*		NV	1988*	1992	
IA	1988,	1976*	1988	NY	1992	1976*	1988
	1990*			OH	1978,	1984*	1988
ID	1992				1984*		
IL	1990			OR	1992*	1976*	
KS		1976*		PA	1982	1976*	
KY		1976*		RI	1990		1992
LA	1990			SC		1988	
MD	1988	1976*	1990	SD		1976	
ME		1976,		TN		1976	
		1992*		TX	1992*	1976*	1986
MI	1976*			UT		1976*	
MN	1988*	1976*	1988	VT	1986	1990*	
MO		1992		WA	1984,		1984
MS	1992	1992			1992*		
MT	1992*	1976		WI		1976	
NB		1986		WV	1992*	1986	

A * indicates active versions of motor voter and mail-in. North Carolina's motor voter program lapsed under a Republican governor from 1986–1990. States with no motor voter, mail-in, or agency programs over the entire 1976–1992 period include AK, AR, FL, GA, IN, MA, OK, VA, and WY. North Dakota has no registration requirement.

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