Election-day registration and turnout inequality

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Abstract

Many researchers blame voter registration requirements for inequalities in turnout rates across various groups in American society. The number of states with election-day registration (EDR) of voters doubled between the 1990 and 1994 elections, providing a unique opportunity to examine its impact on turnout inequality across demographic groups. The adoption of EDR is found to be associated with large and significant improvements in the turnout rates of young persons relative to older persons, and of recent movers relative to nonmovers. Turnout inequality by income class also declines with EDR adoption, but not by a significant amount in multivariate tests. The adoption of EDR does not improve equality of representation across educational levels.

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1. Introduction

Voter turnout rates in the U.S. differ widely among demographic groups. Low voting participation among the young, mobile, less educated, and poor suggests to many researchers and activists that voter registration requirements decrease turnout disproportionately for certain demographic groups (Wolfinger and Rosenstone, 1980; Piven and Cloward, 1988). This belief dates as far back as the early 1800s, when attempts to initiate voter registration systems in New York and Philadelphia were met with accusations that the rich would benefit at the expense of the poor (Harris, 1929: 67-70). Historians of voting in the U.S. generally agree that registration requirements were instituted at least as much "to shape the social character of the eligible electorate" as to control fraud (Kleppner, 1982: 9). In countries where registration is not required or is automatic, correlations between socioeconomic status (or age) and turnout tend to be weaker or absent altogether (Kleppner, 1982; Powell, 1986).

Wolfinger and Rosenstone (1980: 8) suggest that socioeconomic status provides "political resources" which enhance individuals' ability to "bear the cost of voting" including "clerical procedures" and "bureaucratic requirements". Piven and Cloward (1988: 178) argue that even after poll taxes and literacy tests were eliminated, the remaining administrative tasks required to register constituted de facto income tests (as registration offices were often open only during working hours) and education tests ("since it was not easy to discover the location" of offices).

This study analyzes the impact of election-day registration (EDR) on inequality of turnout rates among different groups, taking advantage of a unique opportunity provided by the recent "second wave" of EDR adoption. States were exempted from implementing "motor voter" and other programs mandated by the National Voter Registration Act of 1993 only if they had no

registration requirement (North Dakota) or if they had adopted universal election-day registration at the polls, prior to the 1994 elections. Idaho, New Hampshire, and Wyoming took advantage of this exemption, all implementing EDR prior to the 1994 election. These states constitute a second wave of states adopting EDR, with Maine, Minnesota, and Wisconsin all having adopted it in the mid-1970s.[1]

Results provide strong indication that EDR adoption improves the turnout of the young relative to older persons, and of the residentially mobile relative to non-movers. Turnout inequality by income and, especially, education level, appears to be more resistant to registration reform. Participation by "new" voters -- young persons, and movers who are new to voting in their jurisdiction--appears to be enhanced by EDR, but participation by socially disadvantaged persons--in terms of education and income -- is not, according to our findings.

2. Previous Literature

The evidence on whether EDR *augments* the electorate is remarkably clear and consistent. Studies finding positive and significant turnout impacts are too numerous to list. Estimates of the size of this impact vary, but the most convincing studies -- those incorporating a time-series dimension -- produce estimates of three (Rhine, 1995) to five (Fenster, 1994) percentage points.[2] The turnout impact of the "second wave" of EDR states appears to be very

[1]Oregon also adopted EDR in the early 1970s, but repealed it by initiative in 1985. Ohio adopted EDR in early 1977, but overturned it via constitutional amendment ratified by voters in November 1977, without ever being implemented.

[2]In cross-sectional studies, it is difficult to control for the fact that turnout rates in the EDR states were substantially above the national average even before they adopted EDR. Although some cross-sectional estimates are similar to that of Fenster (1994), for example Wolfinger and Rosenstone (1980) and Teixeira (1992), others are

consistent with evidence from the "first wave." Controlling for other factors influencing turnout change, turnout in the new EDR states increased by nearly 6 percentage points between 1990 and 1994, and by 3 points between 1992 and 1996 (Knack, forthcoming).

Evidence on whether EDR and other major registration reforms *alter* the electorate's demographic or partisan makeup is less consistent, but no less important. The demographic composition of the electorate potentially has policy consequences. For example, states in which the poor are better represented at the polls have been found to offer more generous welfare benefits (Hill and Leighley, 1992).

Wolfinger and Rosenstone (1980), Teixeira (1992), and Mitchell and Wlezian (1995) all conclude that later registration deadlines improve participation more for low-turnout groups such as the less educated. As Nagler (1991) explains, however, this result is effectively ensured by the methodology employed in their simulations. The use of probit or logit assumes that the marginal effects of any independent variable are maximized when the predicted value for the dependent variable is .5 (50%). Because turnout proportions in CPS and NES surveys typically far exceed .5 for presidential elections, and education, age and income are all strongly correlated with turnout, subjects with predicted values for turnout of about .5 will be mostly younger, less educated, and lower income than the average subject. When Nagler (1991, 1994) explicitly tests for interaction effects, by using multiplicative terms or splitting the sample into high-education and low-education groups, he finds that the impact of stricter closing dates does not vary

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much higher, such as those cited by Lijphart (1997: 7).

significantly with education.[3] Nagler (1991) uses the 1972 CPS data file to replicate the results of Wolfinger and Rosenstone (1980), but the methodological point also applies to Teixeira (1992) and to Mitchell and Wlezian (1995), calling into question their findings based on pooled data using later CPS surveys as well as 1972 data.

Calvert and Gilchrist (1993) also find that EDR does not lead to a more representative electorate, using county-level data for Minnesota from several elections between 1984 and 1992. The proportion of registrants in each county who registered at the polls on election day was (1) uncorrelated with the vote share of Democratic candidates, (2) *positively* correlated with the percentage of adults with college degrees, and (3) *negatively* associated with the percentage of families under the poverty line.

More recently, a few studies have provided some reason to believe that EDR, and registration liberalization more generally, marginally improves the representativeness of the electorate. Highton (1997) divides his sample (CPS data pooled from 1980 and 1992) into EDR (including North Dakota) and non-EDR groups, and compares the impact of education on turnout across the two subsamples.[4] He finds that the positive association of education (and age, and residential stability) with turnout is substantially stronger for subjects who do not reside in EDR states -- as one would expect if registration costs act as a stronger deterrent for the less educated.

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[3]Using a more general estimator for dichotomous dependent variables, Nagler (1994) finds that respondents with a probability of voting of about 40% in presidential elections are the most sensitive to changes in any independent variable. Because most of these respondents have below-average education levels, low education will appear to increase the impact of changes in registration closing dates. But, this pattern is observed "because of the greater sensitivity of poorly educated individuals to *any* stimuli" and "*not* because of a peculiar link between education and ability to register early" (p. 251).

[4]This procedure is analytically similar to splitting the sample by education levels and examining the impact of EDR, as Nagler (1991) did. Both methods are similar to testing for interaction effects in combined samples.

However, less than one-third of the effects of education go away when EDR is held constant, indicating that a strong educational bias would remain in the American electorate even if EDR were the rule in all states.[5]

Aggregating CPS data from 1984 and 1986 to the state level, Jackson et al. (1998) examine the influence of closing date and "elite party liberalism" on turnout by the rich (family incomes over $50,000), the poor (under $12,500), the less educated (no high school diploma), and the highly educated (4-plus years of college). Stricter closing dates are found to deter registration by nearly twice as much for the poor and less educated samples as for the rich and better educated.

Highton and Wolfinger (1998) conduct a simple comparison of turnout by demographic category in Colorado before and after its adoption of motor voter in mid 1985. Relative to the rest of the country, turnout among under-30 voters -- but not in other age groups -- rose markedly in Colorado between 1984 and 1988. The turnout increase in Colorado was concentrated in the middle rather than lower educational categories, however -- a pattern also discovered by Nagler (1991) with respect to closing date.

3. Data and Research Design

We analyze turnout inequality by age, education, income, and residential mobility using state-level aggregates from the Current Population Survey's (CPS) Voter Supplement Files. Inequality by education and income are measures of the "class bias" of the electorate with

[5] Avey (1989) argues that the association between socioeconomic status indicators and participation is an artifact of party mobilization and issue advocacy.

potentially important implications for policy. Age and mobility are primarily of interest because the young and recent movers are notorious low-turnout groups. However, lack of representation by the young also has potential policy consequences, for example in the areas of educational or Social Security policies.[6]

The general hypothesis to be tested is that inequality will fall more in the new EDR states than in states with weaker or no reforms, using data from the 1990 and 1994 midterm elections. If EDR influences the composition of the electorate, we would hypothesize that the second wave of EDR states should show disproportionate increases in turnout by the traditionally underrepresented groups.

Unlike the cross-sectional tests discussed above, this method implicitly controls for state-specific and time-invariant factors influencing the relative turnout rates of demographic groups. It examines *changes* in relative turnout, as a function of *changes* in registration requirements. If, for example, states adopting EDR already had rates of turnout equality higher than the national average prior to adopting EDR, cross-sectional tests might attribute turnout equality to EDR -- even though the "effect" temporally precedes the "cause." The design employed here is not subject to this problem.

The "second wave" of EDR adoption provides a unique opportunity for conducting such tests, using CPS data for 1990 and 1994. State identifiers were not included in the 1976 CPS, precluding a similar study of the "first wave" of EDR adoption. The mass implementation of NVRA provisions beginning in early 1995 makes comparisons for 1992 and 1996 problematic,

[6]The issue identified most strongly with the "Rock the Vote" campaign to encourage registration and voting among the young in 1992 was censorship in popular music.

as the impact of new EDR programs is difficult to disentangle from reforms occurring in most other states between those two elections.

Our primary tests examine changes in the relative turnout of different demographic categories between 1990 and 1994 as computed from CPS data. We also briefly discuss evidence from comparisons of the 1992 and 1996 elections, keeping in mind the above caveat.

Although the major purpose of the monthly CPS is to estimate unemployment rates, items relevant to other issues are often included. In November of each election year, respondents are asked whether they were registered to vote, and whether they voted. This "Voter Supplement File" provides individual-level data on education, income, age, and length of residence as well as voting participation. Typically about 50,000 households are represented, and turnout is ascertained for more than one adult within the household, where possible. The surveys include a relatively large and representative sample from each state (including DC). By contrast, the NES is designed to be representative only at the national level, and includes respondents from only about one-third of the states in any given year.

The total sample size of adults for which turnout is determined in the CPS varies from about 800 in some of the smaller states to 7,000 or more in the largest states. Turnout rates aggregated at the state level from the CPS represent estimates, with error margins that vary with sample sizes in the usual way. From the standpoint of state-level analyses of these data, the fact that the sample size is limited in each state introduces random measurement error. The difference across states in CPS sample sizes is also a possible source of heteroskedasticity, as the absolute value of regression residuals for the states may vary inversely with their sample sizes. For this reason, regression results reported below include standard errors based on White's

(1980) heteroskedastic-consistent variance-covariance matrix. We also replicated each regression using weighted least squares, with the CPS sample sizes as the weight variable, with estimated coefficients very similar to the OLS estimates reported in tables below. This finding indicates that our results are produced by measurement error associated with smaller sample sizes in some states. Finally, we replicated the regressions using "robust regression" techniques which downweight outlying observations, to ensure that a few extreme values are not driving our results. This method also yielded results very similar to those reported below.

4. Results

Although implementation of NVRA-mandated programs began only in early 1995, quite a few states had adopted effective "motor voter" programs prior to NVRA passage, with eight of these new enough to influence turnout differences between 1990 and 1994.[7] Deleting these eight states leaves a 40-state control group with no major registration reforms over the period.

Even in these 40 states, there were minor reforms in some, such as allowing mail-in registration, or making registration forms available to the public 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 advisable to examine a second and more pure 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

[7]These were DC, HI, MT, NC, NV, OR, TX, and WA. Programs in AZ, CO, and MI had already been in effect for a full driver's license cycle by the 1990 election, so motor voter should not influence changes in turnout inequality between 1990 and 1994 in those states. States with "passive" programs in which applicants are not asked if they wished to register are not classified here as motor voter states.

(most notably, motor voter in Maine since 1990 and in Minnesota since 1988), they cannot possibly have much of an impact, as most voters in Maine and all voters in the other states already could simply show up at the polls on election day to register and vote all at one time and in one place.

The dependent variables analyzed are measures of turnout inequality, by age, residential mobility, education, and income.[8] For example, turnout inequality by age is constructed as the ratio of the turnout rates for under-30 respondents and for those over 30 years old, expressed as a percentage.[9] If turnout rates for the two groups are equal, this measure will equal 100%. Values less than 100% imply underrepresentation by the young; values exceeding 100% would imply underrepresentation by persons over 30. Inequality by income category is measured by the turnout rate for CPS respondents living in households earning less than $15,000, as a percentage of the turnout rate for over-$30,000 earners. Inequality by education is defined as the turnout rate for respondents without a high school diploma, as a percentage of the rate for high school graduates. A final inequality-of-representation variable is defined as the turnout rate for movers--those living at their current address for less than one year--as a percentage of the rate for nonmovers, those living at their current address for two years or more.[10]

Table 1 summarizes changes between 1990 and 1994 in these turnout inequality ratios in

[8]The extremely small number of minorities included in the CPS sample for the new EDR states, which is largely a function of the racial homogeneity of these states, precludes analyzing racial inequalities.

[9]Similar measures of turnout inequality have been used by Wolfinger and Rosenstone (1980), Hill and Leighley (1992), and Jackson et al. (1998), among others.

[10]No important findings are changed if the income and mobility measures are (like inequality by age and education) defined using mutually exclusive categories, namely households earning under and over $30,000, and residence of less and more than two years.

the three new EDR states. The adoption of EDR appears to have been a spectacular success in equalizing turnout rates between 1990 and 1994. All three new EDR states ranked very low among the states in turnout equality in 1990, especially by age and income, but rose dramatically in the state rankings in 1994 (rankings are shown in parentheses in the table). For example, in relative turnout by the young in 1990 Idaho was ranked 41st, New Hampshire 43rd, and Wyoming 39th. They improved in 1994 to 14th, 24th, and 7th respectively. Of the 12 changes in rank on equality of representation summarized in Table 1, all but one are improvements in rank. (For comparison, Table 3 shows the mean and standard deviation of each of the four turnout inequality measures, among all states for each election year.)

Evidence from the 1992 and 1996 elections is less impressive. Equality rankings for the new EDR states tended to improve in 1996, as shown in Table 2, but by less than in 1994. In Table 2, only 8 of the 12 shifts in rankings represent improvements, compared to 11 of 12 shifts in Table 1. Equality by income and education improved substantially for New Hampshire, but worsened quite a bit in Idaho. Equality by age improved in all three states, only marginally for New Hampshire, but Idaho and Wyoming leapfrogged over 28 and 30 states, respectively. A likely reason for the more mixed results on new EDR programs and turnout inequality obtained from comparisons of 1992 and 1996 is, as discussed above, the confounding effects of the implementation of NVRA provisions that took place in most other states during the period between those two elections. Nevertheless, the results reported in Table 2 on balance support the proposition that election day registration enhances turnout equality.

Table 4 examines the impact of new EDR programs in the 1994 election in more detail.

This table shows the mean change in each of the four inequality measures, for the group of three new EDR states, and for two control groups: the 40-state group and the four-state group. The relative turnout of the poor improved by an average of nearly 11 percentage points in the new EDR states, and by less than 1 point in the control group (difference statistically significant at .02 for two-tailed test).

Relative turnout for the less educated worsened in 1994 for the 40-state control group by more than 5 points, compared to a rise of almost 1 point in the new EDR group. This difference is not significant at conventional levels, however.

The relative turnout of the young and movers was far lower in the new EDR states than in the 40-state group in 1990. In 1994, after implementing EDR, these gaps reversed, favoring the new EDR states. Turnout equality by age improved nearly 13 points in the new EDR group, and fell by about 2 points in the control group (difference significant at .02). The relative turnout of movers increased by more than 14 points in the new EDR states, and increased by less than 1 point in the control group (difference significant at .03).

In the four-state control group (comprised of the four states with either old EDR programs or no registration), changes in turnout inequality by income and education were very small, and not significantly different from changes in the new EDR group. Turnout rates for the young and movers in the four-state group fell, however, from more then three-fifths of the turnout rates of older persons and nonmovers in 1990 to just over half in 1994. In the new EDR group, relative turnout for the young and movers rose from far below one half to more than one half the rate of older persons and nonmovers. The enormous differences in turnout inequality by age and mobility between these two groups of states in 1990 were erased in 1994. Even with the

small sizes of each group, differences in turnout inequality changes between the groups are statistically significant for age ($p = .002$) and mobility ($p = .05$).

It is conceivable that the drastic rise of the new EDR states in the equality rankings in 1994 is attributable in part to other factors, including "regression-to-the-mean" effects, and the distinct partisan composition of the states in the experimental group relative to the control group. Regarding regression-to-the-mean, the states in the experimental group had very low rankings in 1990; these rankings may have been unusually low for these three states in 1990, with part of their improvement in 1994 simply representing a return to their norms. Alternatively, our findings could be a product of spurious correlation associated with the fact that all three states in the experimental group are Republican-leaning while the control group pools Democratic-leaning and Republican-leaning states. Perhaps turnout inequality by age, income, or education changed in certain ways for *all* Republican-leaning states in the "Republican revolution" election of 1994, whether or not they had adopted EDR since the 1990 election.

Multivariate analyses can provide a check against the possibility that the improvements in equality of turnout by age, income, and mobility status are merely the spurious products of such effects. Table 5 reports on two sets of tests; the dependent variables in both are the percentage point changes, between 1990 and 1994, in the four turnout inequality measures. (Summary statistics for all variables in these regressions are shown in Table 6). In the first set, these are regressed on two dummy variables representing the new EDR and old EDR (including ND) states, and on the 1990 levels of the turnout inequality measures. The second set of regressions adds a control for Republican party identification, operationalized as the percentage of the Voter News Services exit poll survey respondents in 1992 who identify themselves as Republicans

(computed as a percentage of the sum of all party identifiers, Democrats plus Republicans, with independents and others discarded).

In the first set of tests reported in Table 5 (those excluding the controls for partisan identification), if there is a tendency for states placing low in the equality rankings to rise, independently of any effects of registration reform, the coefficients on the 1990 levels will be negative. They prove to be negative and highly significant in all four cases. Controlling for these regression-to-the-mean effects, the estimated equality-enhancing impact of EDR declines somewhat, compared to estimates from Table 4, in which EDR adoption appeared to increase the relative turnout of the young and mobile by 14 percentage points or more. In Table 5, the corresponding estimates are about 9 percentage points. While part of the dramatic rise in equality of voting by age and mobility status in the new EDR states thus appears to be a regression-to-the mean effect, EDR still has a large and significant effect.

The effect of EDR on turnout inequality by income also shrinks somewhat when the 1990 level is controlled for, and EDR's remaining 6.4-point impact is not significant at conventional levels. The estimated effect of EDR on turnout equality by educational attainment, which was not significant in Table 4, shrinks further. Surprisingly, the old EDR states (ME, MN, ND, and WI), in principle the purest control group, significantly gain on the omitted category of states (the remaining 44) in equality by educational attainment, controlling for the 1990 level of inequality. In the case of equality by income, age, and mobility status in Table 5, the old EDR states do not differ at all from the omitted category of states.[11]

[11]In results not shown in tables, we attempted to differentiate among the 44 states in the omitted category, by creating separate dummies for the eight "new" and three "old" motor voter states (listed in footnote 5). In every

The addition of the control for partisan identification (alternatively operationalized, with similar results, as the percentage of the two-party vote received by Bush in the 1992 presidential election) does not weaken any of the equality-enhancing effects of new EDR indicated by the first set of regressions in Table 5. Inclusion of the party variable in fact increases the new EDR coefficient, in the case of inequality by age, from 9 to nearly 14 percentage points (equal to the impact of new EDR on inequality by age in the difference-of-means tests in Table 2). The party variable is itself significant in the case of inequality by age: states that are more Republican-leaning had less of an improvement than other states in the relative turnout of the young in 1994.

6. Conclusion

Previous analyses of the turnout inequality effects of EDR, and of earlier registration deadlines more generally, have relied entirely on cross-sectional variation. Taking advantage of a recent "second wave" of EDR adoption, we analyze *changes* over time in turnout inequality at the state level. Changes in turnout rates for different groups between 1990 and 1994 provide strong evidence that EDR disproportionately helps electorally disadvantaged groups.

Our analysis indicates that the adoption of EDR appears to have dramatically improved equality of representation by age and mobility status in 1994. Squire, Wolfinger, and Glass (1987) had previously argued that easy registration procedures such as EDR would increase turnout disproportionately for recent movers, but their tests, based on cross-sectional data for

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case, coefficients were very small and t-ratios never exceeded 0.6, indicating no important information is lost by grouping the 11 pre-NVRA motor voter states with the 33 other non-EDR states

1980, were not conclusive. Evidence presented here strongly confirms their argument. Moreover, the notable improvements in the relative turnout of the young and mobile in the new EDR states were not specific to any one of the three states: all three rose markedly in the state rankings in 1994. These improvements thus cannot be attributed to the presence on any state's ballot of an issue particularly salient to the young or to movers.[12]

Compared to our results on age and mobility, evidence is weaker with respect to lower-income earners and (especially) less-educated persons. These effects are not only somewhat more modest, but they also are not statistically significant when we add controls for the 1990 level of relative turnout. These results are consistent with Nagler's (1991) results on earlier registration deadlines using cross-sectional data.

These disparate impacts of registration reform on the varied types of turnout inequality have implications for those who support or oppose reform in pursuit or fear of partisan or ideological advantage or disadvantage. Although scholarship (e.g., Teixeira (1992) and Wolfinger and Rosenstone (1980)) indicates that such hopes and fears are unlikely to be realized, those supporting reforms in the hopes of improving participation among the poor and less educated (groups reasonably, albeit over-simplistically, associated with liberal and Democratic party causes) may be disappointed by our findings. Reform appears to benefit primarily the young and mobile, who do not lean as strongly as the poor and less educated toward Democratic candidates or liberal causes. Our results do not substantiate the claim that pre-NVRA

[12]However, generalizability questions cannot be entirely resolved with the available data, and EDR (or registration liberalization more generally) may have different effects if adopted in other states than in these three small and relatively rural states with low minority populations.

administrative barriers to registering acted as "de facto income and literacy tests" (Piven and Cloward, 1989: 584-85). From a longer-run and national perspective, the elimination of literacy tests and poll taxes in the 1960s, the dramatic liberalization of residency requirements in the 1970s, and more recent reforms have not been associated with a decline in the class bias in the American electorate, as one would expect if Piven and Cloward were right.[13]

Nevertheless, improving turnout and the relative representation of the young is surely a worthwhile accomplishment of EDR, given the difficulty of achieving each of these goals. Making the electorate more democratic does not necessarily mean making it more Democratic.

[13]Leighley and Nagler (1992) find no noticeable trend in the class bias in turnout between 1964 and 1988.

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Table 1
Change in Relative Turnout, in New EDR States, 1990-94

	Year	ID	NH	WY
Under $15,000/ over $30,000	1994	66.4 (41)	82.2 (3)	70.4 (28)
	1990	64.5 (38)	56.4 (48)	63.2 (37)
No high school graduate/ high school graduate	1994	62.6 (22)	52.4 (45)	62.2 (29)
	1990	62.8 (32)	54.0 (49)	59.7 (41)
Age < 30/ over 30	1994	54.5 (14)	50.2 (24)	59.8 (7)
	1990	41.3 (41)	41.1 (43)	43.6 (39)
Reside < 1 year/ reside > 2 years	1994	49.6 (18)	44.9 (26)	56.4 (6)
	1990	44.3 (24)	29.8 (48)	33.7 (42)

Values in table show turnout of underrepresented group, as ratio of turnout of "over"represented group. In parentheses are state rankings.

Table 2
Change in Relative Turnout, in New EDR States, 1992-96

	Year	ID	NH	WY
Under $15,000/ over $30,000	1996	70.2 (44)	88.1 (2)	74.2 (35)
	1992	81.4 (3)	68.6 (32)	70.3 (28)
No high school graduate/ high school graduate	1996	57.9 (43)	72.2 (9)	65.0 (22)
	1992	70.5 (16)	57.4 (48)	66.3 (33)
Age < 30/ over 30	1996	65.9 (20)	52.9 (46)	65.5 (21)
	1992	63.3 (48)	63.3 (47)	56.2 (51)
Reside < 1 year/ reside > 2 years	1996	48.9 (51)	59.9 (32)	63.9 (22)
	1992	64.0 (37)	59.5 (48)	60.3 (46)

Values in table show turnout of underrepresented group, as ratio of turnout of "over"represented group. In parentheses are state rankings.

Table 3
Means and Standard Deviations (in parentheses) in Relative Turnout Measures Among All States

Turnout Inequality Measure	1990	1994	1992	1996
Income	68.8 (9.6)	70.5 (8.9)	70.3 (7.6)	76.7 (6.3)
Education	69.4 (10.1)	64.3 (10.8)	67.6 (7.0)	64.1 (8.1)
Age	50.2 (8.9)	49.1 (9.2)	73.8 (7.9)	63.2 (7.2)
Mobility	44.2 (10.5)	45.4 (8.3)	70.2 (9.1)	63.1 (8.0)

Table 4
Change in Relative Turnout, 1990-94 (CPS)
Differences in Means

	Year	.New EDR (3)	No reform (40)	Old EDR (4)	New EDR minus No reform	New EDR minus Old EDR
Under $15,000/ over $30,000	1994	71.7	71.0	73.5	+0.7	-1.8
	1990	60.8	70.2	74.6	-9.4	-13.8
	change	+10.9	+0.8	-1.1	+10.1 (p=.02)	+12.0 (p=.17)
No high school graduate/ high school graduate	1994	60.3	65.3	76.9	-5.1	-16.6
	1990	59.4	70.3	77.7	-11.1	-18.3
	change	+0.9	-5.0	-0.8	+5.9 (p=.26)	+1.7 (p=.74)
Age < 30/ over 30	1994	54.8	48.8	53.3	+5.8	+1.5
	1990	42.0	50.5	63.8	-7.1	-21.8
	change	+12.8	-1.7	-10.5	+14.5 (p=.02)	+23.3 (p=.002)
Reside < 1 year/reside > 1 year	1994	50.3	44.8	51.4	+5.7	-1.1
	1990	35.9	44.2	61.2	-8.8	-25.3
	change	+14.4	+0.6	-9.8	+13.8 (p=.03)	+24.2 (p=.05)

Statistical significance of differences in means is shown by p values, in parentheses.

Table 5
Turnout Ratio Regressions (CPS)
Dependent variable: change in turnout ratio, 1990-94

	Dependent variable: change in turnout ratio, 1990-94							
	Income ratio	Education ratio	Age ratio	Mobility Ratio	Income ratio	Education ratio	Age ratio	Mobility ratio
Intercept	29.24** (8.35)	22.36* (9.28)	31.40** (9.98)	28.32** (5.25)	28.68** (11.60)	34.46** (11.33)	51.71** (13.13)	29.22** (7.76)
1990 ratio	-0.41** (0.12)	-0.41** (0.13)	-0.66** (0.20)	-0.63** (0.11)	-0.41** (0.12)	-0.38** (0.11)	-0.72** (0.19)	-0.63** (0.11)
New EDR	6.39 (6.02)	2.82 (2.92)	9.02** (2.92)	8.48** (3.34)	6.19 (6.27)	7.50 (3.75)	13.72** (3.53)	8.73* (3.98
Old EDR	-0.02 (2.22)	8.58* (3.88)	-0.01 (3.45)	0.11 (3.29)	-0.06 (2.31)	9.33* (4.38)	2.13 (3.36)	0.19 (3.35)
Republican ID					0.01 (0.16)	-0.31 (0.17)	-0.37* (0.16)	-0.02 (0.14)
R^2	.29	.22	.38	.50	.29	.30	.47	.50
mean, dep. var.	1.64	-5.14	-1.04	1.18	1.64	-5.14	-1.04	1.18

Standard errors are in parentheses. A * (**) attached to parameter estimates indicates significance at .05 (.01) for 2-tailed test. Sample size is 51 in all regressions.

Table 6
Descriptive Statistics for Variables in Table 5 Regressions

Variable	Mean	Stand. Dev.	Min.	Max.
Change in turnout ratio: income	1.64	8.26	-17.85	25.80
Change in turnout ratio: education	-5.14	9.52	-20.10	22.59
Change in turnout ratio: age	-1.04	10.91	-20.65	29.45
Change in turnout ratio: mobility	1.18	10.24	-29.92	22.66
1990 ratio: income	68.82	9.60	45.26	88.75
1990 ratio: education	69.40	10.13	45.93	89.06
1990 ratio: age	50.19	8.92	34.99	77.19
1990 ratio: mobility	44.21	10.49	26.55	82.27
New EDR	.059	.238	0	1
Old EDR	.078	.272	0	1
Republican ID	47.22	9.56	9.75	66.86

N = 51 for all variables.