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# People Who Have Their Tickets But Do Not Use Them

## “Motor Voter,” Registration, and Turnout Revisited

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We reexamine the effects of the National Voter Registration Act (NVRA) legislation on aggregate participation rates in the states. We approach NVRA as working directly on registration, and only indirectly on turnout, and we argue that the most appropriate way to evaluate NVRA is to examine how it translates increased registration into turnout. Using Federal Election Commission/Electoral Assistance Commission and census data, we find that an unanticipated impact of NVRA has been to alter the long-standing empirical relationship between registration and turnout. Further analyses suggest this failure to transmit registration into turnout may, ironically, be the result of another success of NVRA—an increase in the relative equality of registrants. By encouraging lower income citizens to register, NVRA has helped create a pool of registered citizens less likely to vote. These findings have implications for discussions of NVRA and for the participation literature that views registration as a primary catalyst for turnout.

**Keywords:** *election reform; motor voter; political participation; voting; NVRA; turnout*

**A**lmost all theorists and observers of democracy hold that a healthy electoral system is synonymous with high levels of citizen participation

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(e.g., Verba & Nie, 1972; Verba, Scholzman, & Brady, 1995).<sup>1</sup> On election day in a healthy system, citizens are motivated to go to the polls and most do so, with high levels of voter turnout creating an electorate representative of the citizenry at large. Given the importance of citizen participation for determining democratic well-being, it is not surprising that discussions of electoral reform often center on the need to increase voter turnout. This is certainly true in the United States, where voter participation routinely lags well behind other Western democracies (Jackman, 1987; Lijphart, 1997; Powell, 1986), and quadrennial worries about the state of citizen involvement in democratic governance are the norm.

One recent attempt to increase citizen involvement in the electoral process is the National Voter Registration Act (NVRA). Enacted in 1993, the NVRA was based on the premise that the most likely way to increase voter turnout is to lower the barriers that stand between would-be voters and the polls. The most important of these barriers is voter registration. Thus, the NVRA, most commonly called “motor voter” because of its most prominent component, was passed to try and ease the cost or burden of registration.<sup>2</sup> Rather than leaving it to citizens to track down registration information for themselves, NVRA federally mandates that states offer registration opportunities in combination with other functions, such as registering while renewing a drivers’ license or applying for public assistance benefits.<sup>3</sup>

This link between registration and turnout has certainly not been lost among the scholarly community. Indeed, a large volume of literature documents the negative impact of registration barriers on election day participation (e.g., Kim, Petrocik, & Enokson, 1975; Wolfinger & Rosenstone, 1980). Squire, Wolfinger, and Glass (1987) found that failing to register resulted in roughly one third of the eligible electorate’s being unable to vote. From a comparative perspective, Powell (1986) found that the United States has much more stringent registration requirements than most other Western democracies, resulting in a 14% reduction in voter turnout. More recent work by Hill and Leighley (1996) confirms the general relationship between barriers to registration and voter turnout. This relationship is stated most plainly by Erikson (1981) when he answers the question “Why do people vote? Because they are registered.” The upshot of this research, as it relates to NVRA, is seemingly clear: Make registration easier and increased turnout will follow.

## PARTICIPATION AS A TWO-STAGE PROCESS

Yet, the relationship between registration laws and turnout may not be quite so simple. A central point of more recent work on voter participation has been to revisit the oft-stated but underappreciated truism that voting is a two-stage process where first, citizens must register, then those who have registered *may* vote (Brown, Jackson, & Wright, 1999; Jackson, 1996; Timpono, 1998). In this two-stage conceptualization, registration tends to reflect long-term citizen characteristics and political system forces, whereas turnout among those who have made this commitment is more a reaction to the candidates and campaigns at hand (Jackson, 1996; Timpono, 1998). In other words, although the two behaviors are clearly related, they are in fact different, and subject to different influences. Aggregate studies of state-level variation in registration and turnout reflect these individual-level results (Brown et al., 1999).

This line of analysis is suggestive of other factors that may lead us to question the registration-begets-turnout logic that undergirds NVRA and much of the literature on registration barriers and turnout. For example, Brown et al., (1999) find that the strength of the relationship between aggregate rates of registration and turnout varies by income. For wealthier citizens (again, in the aggregate), the effect of registration is almost dispositive: There is almost a one-to-one correspondence between registration rates and turnout rates among states' wealthier citizens. This effect gets smaller as income drops, so that although still strong, the relationship between registration and turnout rates among the states' poorer citizens is less impressive (Brown et al., 1999, p. 475). We suggest that this difference in effect across income levels may lend important insight into examinations of the impact of NVRA on voter turnout, as it reveals that the empirical relationship between registration and turnout may not be as consistent as suggested by previous literature.

In addition, Knack (1995) indicates that reducing the burden of registration may actually result in a weaker motivation to vote. Highton (1997) concurs that the real issue is indeed motivation to engage in electoral politics. If people are not interested in voting anyway (i.e., not interested enough to overcome the registration barrier), then removing registration as an obstacle to voting will have little effect. Martinez and Hill (1999, p. 308) make the point perhaps most directly when they suggest it is likely that a significant portion of the individuals who overcome the registration hurdle are already predisposed to voting, whereas the unregistered are not. With regard to NVRA, this represents an important distinction, as factors that reduce the likelihood of voting in the pre-NVRA period still stand as psychological

obstacles to participation after NVRA's enactment and its impact on registration.

We believe that this general line of inquiry—that of participation as a two-stage process where each stage is responsive to different influences—is fundamentally important to understanding the probability that NVRA and other attempts to lower registration requirements will succeed in their overall goal of increasing turnout. By actively targeting those who are less likely to register to begin with (poorer, less educated, etc.), NVRA differentially increases registration among those groups relative to wealthier and better educated groups—indeed, this is the goal of the legislation. Yet, when viewed from the perspective of voting as a two-stage process, where different factors motivate individuals to participate at each stage, it is a mistake to assume that increased registration leads automatically to increased voting.

In this article, we draw from this premise and seek to help clarify some of the issues surrounding NVRA and similar programs as they relate to the registration turnout relationship and suggest that recognizing participation as a two-stage process has important implications for how we model the effects of NVRA on state-level patterns of participation. Previous research in this general area has produced somewhat lukewarm and mixed results. Rhine (1995, 1996) and Highton (1997) find that reduced registration costs have a positive impact on turnout or in reducing turnout declines (Knack, 1999). Highton and Wolfinger (1998) confirm Rhine's findings that turnout is higher in election-day registration states but go on to conclude that these effects are not likely to translate well to individuals who have little or no interest in the political system. Mitchell and Wlezien (1995) confirm some effects for liberalizing registration laws on increases in both registration and turnout. Yet, at the same time, Mitchell and Wlezien predict only modest effects for NVRA, pointing out that reducing the cost of voting is only part of the problem and that the people NVRA is designed to help (poor and less educated) are generally less motivated to vote anyway. Finally, in analyses designed specifically to examine the effects of NVRA, Knack (1995, 1999) and Martinez and Hill (1999) find somewhat contradictory results. Knack (1995), in evaluating the effects of early state efforts at motor voter provisions, does find some effects for these previously implemented state programs. In subsequent work, Knack (1999) found that registration reform substantially slowed the drop in turnout from 1992 to 1996. Alternatively, Martinez and Hill, although finding evidence of registration increases associated with national NVRA implementation, find no evidence to suggest that this increased registration translated into increases in turnout (or reduced turnout declines).

One important issue here is that although most studies focus on the impact of NVRA on turnout directly, NVRA was actually designed to boost *registration* in anticipation of boosting turnout; the direct impact of NVRA is on registration only. By helping people over the registration hurdle at the time they are applying for or renewing their drivers' licenses or public assistance benefits, motor voter seeks to increase registration. It is assumed that once this first stage is met, increased turnout will result, but the turnout effect is indirect. We believe this is an important nuance missing from the current literature and suggest here that it is important to model the impacts of NVRA in conjunction with its underlying logic of voter participation as a two-stage process if we are to provide adequate insight into the program's successes and failures.

Perhaps more important, we suggest that the impact of NVRA may go well beyond its direct effect on participation. We propose that an unintended consequence of NVRA is that it has altered the nature of the relationship between registration and turnout. Whereas previous research documented the effect of registration on turnout to be a relative truism, this research is based on what is in effect a biased sample. That is, the exceedingly strong link between registration and turnout is strong precisely because it has been examined among those most likely to do both. Although NVRA seeks to increase turnout by increasing registration, it does so by targeting those groups of citizens *least* likely to do both. It is plausible, therefore, that NVRA is creating a different pool of registered voters, citizens who for whatever reason are unwilling or unable to overcome the registration hurdle. In essence, the pool of registered voters who might actually make it to the polls on election day is now diluted. Previous research would suggest this is because these citizens lack the long-term commitment or ties to the political system that would have compelled them to register in the first place. The analogy we use here is one of the professional sports team trying to increase attendance. Various promotions that supply low-cost or even free tickets may give people the means to attend a game, but it does not necessarily increase their incentive to do so. Simply holding a ticket need not imply that a person is interested in attending the event.

As such, whereas previous analyses have focused almost exclusively on turnout, we believe the most appropriate way to examine the effects of NVRA on participation is in terms of the relationship between registration and turnout. We believe this path will provide greater insight not just into the successes or failures of NVRA but the reasons behind them as well. Finally, we will highlight that an unexpected effect of NVRA has been a fundamental change in the importance of registration as a predictor of turnout in the

American states. Has NVRA created a pool of individuals who have their tickets but have no desire to go to the game?

## **NVRA and Registration in the American States**

Beginning from the premise of participation as a two-stage process, we believe that more careful attention needs to be focused on assessment of the direct effects of NVRA reforms. Recall that although the ultimate goal of NVRA was to improve voter turnout, the mechanism by which this was to be accomplished was through increased citizen registration. Yet, although most studies acknowledge this causal pathway, and many have noted an increase in registration rates after NVRA, there have been few systematic attempts to link increased registration rates to motor voter legislation (Highton & Wolfinger, 1998; Martinez & Hill, 1999; Rhine, 1996).<sup>4</sup> This first step is vital, for without solid evidence that NVRA is responsible for changes in registration, we cannot attribute changes in actual voter turnout to the federal mandate.

Martinez and Hill (1999) offer initial evidence of NVRA-related increases in registration rates by showing descriptive statistics of aggregate registration rates over time. We acknowledge this here and attempt to gain more leverage on the role of motor voter in increasing state registration rates by modeling registration in presidential elections over time (1980-2000, 1980-2004). In recognition of the ongoing discussion regarding the relative accuracy and validity of different sources of registration and turnout data, we use three sets of data from two different sources.<sup>5</sup> Our first source is published reports from the Federal Election Commission (FEC; now the Electoral Assistance Commission [EAC]) for the years 1980-2000.<sup>6</sup> To these we add two additional sets of state registration and turnout rates, both taken from the Bureau of the Census (1980-2004). The first is the common measure deriving participation rates by using the voting-age population in the denominator. In recognition of potential difficulties with these data, namely, that participation rates may be artificially deflated by including individuals unable to vote in the denominator of calculation (i.e., noncitizens), the Census Bureau now also publishes data using the number of citizens in the state as the denominator. We will report results from all three data sources for each of the analyses to follow.<sup>7</sup>

Prior to adoption of NVRA in 1993, and full implementation in 1996, several states had already adopted their own versions of motor voter policies. Still others were in the process of limited implementation of their own poli-

cies, whereas others had none. Following Martinez and Hill (1999), we assume that the greatest effects of NVRA will be in those states whose programs were most affected by the federal mandate—that is, those states with little or no implementation prior to 1993.<sup>8</sup> We incorporate these state differences into our analysis by employing an interactive model. Specifically, we include two dummy variables categorizing states by previous level of motor voter implementation (scored 1 for low and 0 for none, respectively) and interact these with a dummy variable differentiating the election years 1996-2004 from those prior. Our rationale behind this modeling scheme is straightforward. To the extent that motor voter policies implemented prior to the federal mandate had an effect, we should expect registration rates to be higher in states with high levels of prior implementation, all things being equal. Given the coding of this variable described above, we should therefore expect the prior implementation dummy coefficient to be negative; registration rates should be lower in states with low or no prior implementation. After full implementation dictated by the federal mandate, however, we should expect a change in this relationship. That is, we should expect the largest effects of motor voter to be seen in those states where implementation was lowest prior to the federal mandate. Interacting the prior implementation level variables with a dummy denoting pre- and post-NVRA (coded 1 if the election year was 1996, 2000, 2004 and 0 if 1980, 1984, 1988, 1992) allows us to discern these changing effects. More important, it allows us to link these effects to full NVRA implementation.

Finally, as controls we include many of the usual suspects in the registration and turnout literature: education (percentage with college degree), registration closing date, a provision for mail registration in the state, a regional dummy variable for the South, age (percentage 65 or older), and the extent of a state's metropolitan population.<sup>9</sup> In addition, we include the presence of senatorial or gubernatorial races in the state on the possibility that these additional races might serve as an impetus for participation.<sup>10</sup> To allow for the possibility that states with same-day registration provisions might have higher registration levels, we incorporate a dummy variable to denote these states.<sup>11</sup> Finally, in recognition that registration levels are highly correlated within states from one presidential election to the next, we include a lag of registration rate to account for prior levels of registration.<sup>12</sup>

Table 1 shows the results and provides some evidence that NVRA did have the desired effect on state registration rates. Beginning with the FEC data, of the controls only closing date performs as expected, with restrictive closing dates reducing registration rates. This is not necessarily surprising given the importance of closing date to determining registration and the

**Table 1**  
**National Voter Registration Act (NVRA) and State Registration Rates (1980-2000/2004)**

Variable	FEC 1980-2000	Census (VAP) 1980-2004	Census (Citizens) 1980-2004
Lagged registration	.8083*** (.0522)	.7276*** (.0452)	.6032*** (.0473)
Prior NVRA low	-.0174* (.0129)	-.0044 (.0098)	-.0038 (.01)
Prior NVRA none	-.0333*** (.0108)	-.0172*** (.0077)	-.0178** (.0079)
Low Prior NVRA × Post-1992	.0481** (.0217)	-.0076 (.0134)	-.006 (.0139)
No Prior NVRA × Post-1992	.0477** (.0181)	.0185** (.111)	.0205** (.0113)
Closing date	-.0012** (.0005)	-.0004* (.00027)	-.0007** (.0003)
Mail registration	-.0009 (.0067)	.0087** (.0043)	.0118** (.0044)
Same-day registration	-.0134 (.0148)	.001 (.01)	-.0018 (.0096)
% college education	.00007 (.001)	.0015** (.0047)	.0026*** (.0005)
Age (% 65+)	.0007 (.0018)	.001 (.001)	.001 (.0009)
South	.002 (.0085)	.0059 (.0047)	.0052 (.0051)
Metro population	-.00007 (.0002)	-.0004*** (.0001)	-.00025** (.0001)
Senate race	-.0126 (.0078)	-.0048 (.0045)	-.0046 (.0046)
Governor race	.0084 (.0071)	.0042 (.0046)	.001 (.005)
Post-1992 dummy	-.0345** (.0174)	-.04*** (.01)	-.0557*** (.01)
Constant	.205** (.061)	.203*** (.041)	.2828*** (.0443)
$n^a$	240	288	288
Adjusted $R^2$	.791	.74	.653

Note: FEC = Federal Election Commission; VAP = voting-age population. Entries are unstandardized regression coefficients, robust standard errors in parentheses.

$n$ ,  $n = 240$  and 288 because of the presence of the lagged dependent variable and because North Dakota and Wisconsin are excluded. North Dakota has no registration provision and the FEC does not have registration data on Wisconsin. In addition, the FEC has not published turnout figures for 2004. Hence, we refrain from including the 2004 registration data for the FEC equation in order to remain consistent throughout our analyses. Note that the results (not shown) do not change when we include the 2004 registration data.

\* $p < .10$ , one-tailed. \*\* $p < .05$ , one-tailed. \*\*\* $p < .01$ , one-tailed.

inclusion of the lag. In the equations using census data, mail registration provisions emerge as significant, as do education and metropolitan population.

Our primary concern, however, is with the previous implementation dummies and their interactions, and here we see evidence that NVRA did work to increase registration rates where we would expect it most—in those states with little or no previous implementation. Note first the negative coefficients for the previous implementation dummies. As expected, these indicate that overall registration rates in states that previously had minimal or no motor voter implementation prior to 1996 had significantly lower registration rates. Yet, the interaction term indicates that after full NVRA implementation, the slope of previous implementation shifts upward. Indeed, the shift is strong enough that the previous effect is now reversed (FEC data:  $-.0333 + .0477 = .0144$ ). This effect is consistent across both the FEC and census data and indicates that the effect of NVRA on registration rates was greatest in those states where there was no previous implementation of state motor voter programs.

It is interesting that we see that the post-1992 dummy is also significant and negatively signed. In this modeling configuration, this coefficient taps the impact of states with previously high levels of NVRA implementation and thus indicates a decline in registration in these states after the 1992 election. This may indicate a possible limited long-term capacity for NVRA programs as they compete with other factors working to reduce participation. It is also quite plausible that these apparent drops in registration would have been worse without the early implementation of NVRA-type programs.

Overall, we believe the analyses illustrate that the first part of the two-stage process seems to have worked; NVRA did have a positive impact on state registration rates in those states where we would expect it to have its greatest effect. Although this may seem self-evident to some, documenting this effect is no idle exercise. For it suggests that if previous research has documented minimal effects for NVRA on turnout, then we need to examine why increased registration has not translated into increased voting.

### **NVRA and Turnout in the American States**

Having found systematic evidence of a relationship between NVRA and higher registration rates, we turn now to turnout. We begin by examining the direct effect of NVRA on state turnout rates and then move to the indirect effect of NVRA through its impact on registration.

Our initial approach is to draw from recent work looking at the effect of NVRA on turnout, though in this instance we use a more expansive time frame. Following the logic outlined earlier, we agree with Martinez and Hill (1999) that if NVRA is to show an impact, it will be among those states that had little or no implementation of state-level motor voter policies prior to the national mandate. Consistent with this logic and with our previous analyses, we model this by including dummy variables for states that fall into these lesser categories of NVRA implementation. To gauge the change in turnout in these states over the pre- and post-NVRA period, we include interactions as in the registration analysis. Finally, in addition to the control variables used in the registration model, we add a variable designed to tap an additional short-term force: competition in the presidential race.<sup>13</sup> We operationalize this variable as the difference between the winner's and loser's vote share. A negative coefficient on this variable would thus indicate that a close presidential election works to stimulate turnout.

Table 2 shows the results of these analyses. The controls tend to perform as expected, with the results being largely consistent across all three data sets. Closing date and mail registration work to decrease and increase turnout, respectively, though same-day registration has little effect. Education and age also exert a very strong positive influence on state turnout rates. Most important, the dummies and interactions designed to parse out the effects of NVRA perform according to expectations—that of little or no effect for NVRA. The significant dummy for states with no previous implementation indicates that these states have lower levels of turnout than states with low or full previous NVRA implementation—this is to be expected. Yet, as opposed to the registration analyses, the interaction terms are not significant. This indicates no subsequent increase in turnout with the advent of NVRA. Thus, although NVRA has spurred registration in these states, the subsequent turnout effect does not seem to have materialized. When controlling for common sources of participation and previous levels of turnout, NVRA appears to have had little direct impact on aggregate turnout across the states. Again, these results are consistent across all three data sets.

Note also the coefficient for the post-1992 dummy. In the context of these interactions, this variable captures the turnout effects in states that had previously implemented NVRA-type programs at a high level. The significant negative coefficient provides further evidence that NVRA has had little positive effect on turnout, with turnout slipping even in those states that have had motor voter provisions in place for some time. If motor voter were indeed effective at increasing (or at least reducing the decline in) turnout, we would expect this coefficient to, at worst, be nonsignificant. Yet, in spite of the

**Table 2**  
**National Voter Registration Act (NVRA) and State Turnout Rates (1980-2000/2004)**

Variable	FEC 1980-2000	Census (VAP) 1980-2004	Census (Citizens) 1980-2004
Lagged turnout	.5621*** (.0461)	.5091*** (.05)	.359*** (.052)
Prior NVRA low	-.0077 (.0134)	.0009 (.011)	-.0034 (.0118)
Prior NVRA none	-.0206** (.0104)	-.0161** (.0083)	-.0158** (.0089)
Low Prior NVRA × Post-1992	-.0022 (.0174)	-.0206 (.0163)	-.0196 (.0168)
No Prior NVRA × Post-1992	.0151 (.0138)	.013 (.013)	.0149 (.0137)
Closing date	-.0006** (.0003)	-.000** (.0003)	-.0008** (.0003)
Mail registration	.0036 (.005)	.012** (.0052)	.0138** (.0054)
Same-day registration	.0152 (.01)	.0103 (.0109)	-.0089 (.0133)
% college education	.0057*** (.0007)	.0044*** (.0007)	.0055*** (.0007)
Age (% 65+)	.0039** (.0007)	.0029** (.0013)	.0027** (.0007)
South	-.0043 (.007)	.00001 (.006)	-.0087 (.0069)
Metro population	-.0007*** (.0001)	-.0008*** (.0001)	-.0005*** (.0001)
Senate race	-.0084* (.0056)	-.0072* (.0053)	-.0057 (.0057)
Governor race	.009* (.0057)	.0065 (.0059)	.0015 (.0063)
Presidential competition	-.0003* (.0002)	-.0003 (.0003)	-.0003 (.0003)
Post-1992 dummy	-.0749*** (.0138)	-.0667*** (.0118)	-.0836*** (.011)
Constant	.183** (.039)	.268*** (.042)	.3566*** (.0447)
$n^a$	240	288	288
Adjusted $R^2$	.751	.633	.585

Note: FEC = Federal Election Commission; VAP = voting-age population. Entries are unstandardized regression coefficients, robust standard errors in parentheses.

a.  $n = 240$  and 288 because of the presence of the lagged dependent variable and because North Dakota and Wisconsin are excluded. North Dakota has no registration provision and the FEC does not have registration data on Wisconsin. In addition, the FEC has not published turnout figures for 2004. Hence, we refrain from including the 2004 registration data for the FEC equation in order to remain consistent throughout our analyses. Note that the results (not shown) do not change when we include the 2004 registration data.

\* $p < .10$ , one-tailed. \*\* $p < .05$ , one-tailed. \*\*\* $p < .01$ , one-tailed.

efforts of motor voter-type legislation, overall turnout rates are still lower in the post-1992 period, even when including the relatively high-stimulus 2004 election.

## **NVRA, Registration, and Turnout in the American States**

The rationale behind NVRA is straightforward and well supported by the scholarly literature. Because of the strong tendency for registration to translate into turnout, policies that reduce registration barriers should yield higher turnout rates. Yet, although the direct effect of NVRA seems to be having some effect, the second-stage benefits do not appear to have been realized. If NVRA has led to increased registration but not increased turnout, as the above evidence suggests, then perhaps one reason for this might be that something has compromised the well-documented relationship between registration and turnout.

We reiterate here that in our estimation, the best way to understand the impact of NVRA on turnout is by making modeling decisions consistent with viewing participation as a two-step process. Thus far, we have examined each step separately, because doing so helps to isolate what is happening in the two different stages. In linking these stages, we hypothesize that encouraging a large influx of new registrants, those drawn (by design) from segments of society shown in the past to be the least willing to register or vote, has fundamentally altered the relationship between the two stages of participation. That is, after the 1992 presidential election and full implementation of NVRA, we expect the relationship between registration and turnout to be attenuated.

Turnout is modeled over the last six or seven (depending on the data source) presidential elections as a function of both long-term and campaign-specific factors: a lag accounting for previous presidential election turnout, state education, age, metropolitan population density, a southern dummy, presidential competition, and the presence of senatorial and gubernatorial races. The dummies for previous motor voter implementation are retained to account for the possibility of hypothesized effects in states that were slow in implementing earlier versions of NVRA.

It is important to note we explicitly model the effect of registration on turnout by adding registration rates to the equation. This approach is much more in keeping with recognizing participation as a two-stage process, and it allows us to examine changes in the registration-turnout relationship.<sup>14</sup> We model this hypothesized change in a fashion similar to our previous analyses,

using interaction terms to account for the intervention of full NVRA implementation. To account for this hypothesized NVRA influence on the effect between registration and turnout, we interact registration rates with the post-NVRA dummy variable. Our expectation is that the coefficient for this variable will be negative, indicating a weakening in the effect of registration on turnout after full NVRA implementation.<sup>15, 16</sup>

Results for the full turnout model are reported in Table 3. Control variables perform largely according to expectations. States with same-day registration, more college graduates, older populations, less population density, and the presence of a gubernatorial race tend to have higher turnout rates. Here, we also note an effect for southern dummy, with southern states tending toward lower participation levels. Although the control variable coefficients are consistent across all data sets, the southern dummy effects are particularly strong for the census data equations.

Most important for evaluating NVRA, though, are the coefficients for state registration rates and the interaction term of registration with the post-1992 dummy. In this context, registration should be interpreted as the effect of registration rates on turnout prior to the 1996 election and full NVRA implementation. Taken by itself, the coefficient provides support for the familiar relationship between registration and turnout. As in previous studies, states with high levels of registration also tend to have higher participation on election day. Yet, as the negative coefficient on the interactive term indicates, this relationship is seriously attenuated in the elections following full implementation of NVRA. To be sure, registration still exerts a positive effect on turnout across the states (FEC:  $.3073 - .0939 = .2134$ ), but the slope shift represents a substantial reduction in the strength of this relationship. The results using the census data differ somewhat, largely because registration has a much larger influence on turnout rates in these data. Indeed, the impact of registration on turnout dwarfs that of previous turnout levels, which are negative in the face of this impressive relationship. Clearly, registration is still a dominant factor in producing higher turnout rates. Yet, although less impressive, the overall pattern of a decline in the registration-turnout relationship emerges in these data as well. The impact of registration on turnout, although still strong, decreases in the full NVRA implementation period.

Overall, the results lend considerable support to our central thesis. Examining three measures of turnout and registration from two different data sources, we find that NVRA appears to have expanded the pool of registered citizens by targeting those least likely to both register and vote. The apparent failure of NVRA to increase turnout is at least partly a result of its direct and

**Table 3**  
**National Voter Registration Act (NVRA), Registration, and State Turnout Rates (1980-2000/2004)**

Variable	FEC 1980-2000	Census (VAP) 1980-2004	Census (Citizens) 1980-2004
Lagged turnout	.3502*** (.0475)	-.0694** (.0322)	-.0882** (.028)
Registration rates	.3073*** (.0383)	.9182*** (.0362)	.9341*** (.0378)
Registration × Post-1992 interaction	-.0939*** (.0072)	-.0525*** (.0062)	-.0521*** (.0063)
Prior NVRA – low	-.009	-.0055 (.0042)	-.0049 (.0044)
Prior NVRA – none	-.0062 (.0055)	-.003 (.0037)	-.0029 (.0038)
Same-day registration	.0213** (.0115)	.0138** (.0069)	.014** (.0071)
% college education	.0061*** (.005)	.0029*** (.0004)	.003*** (.0004)
Age (% 65+)	.0043** (.0011)	.0018** (.0007)	.0018** (.0008)
South	-.0095* (.0063)	-.0208*** (.0037)	-.0232*** (.0039)
Metro population	-.0005** (.0001)	-.0001 (.0001)	-.00006 (.00009)
Senate race	-.0044 (.0048)	-.0013 (.0032)	-.0013 (.0033)
Governor race	.0092** (.0051)	.0038 (.0034)	.0037 (.0035)
Presidential competition	-.0002 (.0002)	.00007 (.0001)	.00007 (.0002)
Constant	.0128 (.0315)	-.0435** (.0222)	-.0467** (.025)
$n$	240	288	288
Adjusted $R^2$	.818	.888	.874

Note: FEC = Federal Election Commission; VAP = voting-age population. Entries are unstandardized regression coefficients, robust standard errors in parentheses.

$n = 240$  and 288 because of the presence of the lagged dependent variable and because North Dakota and Wisconsin are excluded. North Dakota has no registration provision and the FEC does not have registration data on Wisconsin. In addition, the FEC has not published turnout figures for 2004. Hence, we refrain from including the 2004 registration data for the FEC equation in order to remain consistent throughout our analyses. Note that the results (not shown) do not change when we include the 2004 registration data.

\* $p < .10$ , one-tailed. \*\* $p < .05$ , one-tailed. \*\*\* $p < .01$ , one-tailed.

intended effect on registration. The result is an attenuation of the empirical linkage between registration and turnout.

## **NVRA, Turnout, and Registration Equality**

Thus far, we have documented evidence indicating that NVRA has attained its preliminary goal—increased registration. In addition, we find that an unintended consequence of this is a modification of the causal relationship between registration and turnout. It is this attenuation of the registration effect that has kept NVRA from achieving its ultimate goal of increased turnout. In this final section, we try to link these changes in registration and turnout together by addressing another primary goal of NVRA—increasing the degree of equality in the registered population.

Recall that although the overall goal of NVRA is to increase turnout, it specifically seeks to do so by targeting segments of the population that are most likely to be disenfranchised—the poor, less educated, and so forth. If these groups could be reached at a time when engaging in other, more immediately relevant, activities (i.e., renewing driver's license, applying for public assistance benefits), then the registration barrier could be removed and increased registration would result in increased turnout and greater equality in the voting population. Yet, we posit that it is precisely this increased equality in registration that has led to the lack of a turnout effect and suggest that this is an unintended consequence of the success of NVRA in registering those it has targeted.

Recent work by Lloyd (2001) bolsters our confidence in this hypothesis. In his individual-level analysis, Lloyd finds that registration laws are not uniformly the primary force in keeping people at home on election day. Rather, it is a lack of interest in participation. Simply registering the disinterested may not increase turnout if these newly registered individuals are not already inclined to participate. By seeking out groups that are less likely to participate and making it easy for them to register, NVRA has increased the pool of voters that is legally *qualified* to participate on election day. In doing so, it has also modified the pool to include more individuals that in all likelihood are less *inclined* to participate.

We try to get an empirical handle on this supposition by examining the effect of changes in the relative equality of the registrant pool on turnout among the registered. Based on those developed by Rosenstone and Hanson (1993, p. 293), we utilize two measures of registrant equality: education and income. Each measure is created by first comparing the relative share of the

states' populations at high and low education (income) levels to their relative share of registrants. These ratios are then compared to create an overall equality score (termed EQ by Rosenstone and Hanson), as follows:

$$EQ = \text{Ratio}_b / \text{Ratio}_t$$

where

$\text{Ratio}_b$  = the representation ratio for the group at the bottom of the distribution;  
 $\text{Ratio}_t$  = the representation ratio for the group at the top of the distribution.

where

$\text{Ratio}_b$  and  $\text{Ratio}_t = \text{Participants}_b / \text{Population}_b$  and  $\text{Participants}_t / \text{Population}_t$ , respectively.

Thus, the EQ measure first looks to see the degree to which each group is overrepresented or underrepresented among the pool of registrants by comparing the group's share of the participants to its share of the population as a whole. A ratio of 1.0 indicates that the group is evenly represented—its share of the participants equals its share of the population. A ratio greater than 1.0 means the group is overrepresented among registrants, and a ratio of less than 1.0 means the group is underrepresented among registrants, again, relative to its size in the overall state population. By combining these separate representation ratios into a single construct, the EQ measure allows for the assessment of the degree of inequality across groups.<sup>17</sup>

For our analyses, we examine education and income inequality by comparing the representation ratios of the groups at the ends of each distribution. Thus, for education we compare those with less than a high school degree to those with a college degree or more. For income, we compare the lowest and highest quintiles of the income distribution. To create the representation ratios, we use data from the 1992 and 2000 *Current Population Surveys* (CPS; <http://www.bls.census.gov/cps/cpsmain.htm>).<sup>18</sup> In addition to its normal battery of questions, in November of even-numbered years the CPS asks respondents whether they were registered and whether they voted. As such, these surveys provide a unique opportunity to develop state-level participation measures with demographic breakdowns. The 1992 and 2000 surveys, for example, each include data on the registration status, turnout behavior, and sociodemographic characteristics of between 82,000 and almost 100,000 voting-age citizens drawn randomly from the civilian, noninstitutionalized population of the United States. The large size of the survey sam-

ples allows aggregation of respondents by state to develop state level of registration and turnout rates (see Hill & Leighley, 1992, pp. 353-354; Wolfinger & Rosenstone, 1980).<sup>19</sup>

Our approach to determining the effect of increases in registrant equality on turnout is to model turnout among registered voters in 2000 as a function of the change in registrant equality from 1992 (the last presidential election prior to full NVRA implementation) to 2000. In this way, we allow for one full postimplementation election cycle to be completed. We examine turnout among the registered (as opposed to among voting-age population or citizens as in the previous analyses) because this group most closely fits the relationship of interest. NVRA is designed to increase registration under the assumption that these newly enfranchised will turn out on election day. Our hypothesis is that the unintended effect of increasing registration in the states is to dilute the pool of likely voters among the registered. We expect, therefore, that increases in the income and educational equality of registered voters will actually have a negative effect on turnout among the registered.<sup>20</sup>

We examine this idea by adding equality variables created to tap change in registrant equality between 1992 and 2000. Change in equality is created by simply subtracting the income and education equality variables for 1992 from their corresponding measures in 2000. A positive change figure therefore represents that the degree of lower income (education) equality has improved between 1992 and 2000.<sup>21</sup> Most other components of the model remain intact. As in the previous analyses, the dummies for previous motor voter implementation are retained to account for the possibility of hypothesized effects in states that were slow in implementing earlier versions of NVRA. Controls for demographic and short-term political stimuli are also included. Finally, we add a lag of registered turnout in 1996.

The results for this model of turnout among registered voters in the 2000 presidential elections are shown in Table 4. Obviously, the lag exerts the strongest effect. Yet, even in the face of the lag, several variables also emerge as significant influences (although some only marginally so) on turnout among the registered, with same-day registration and presidential competition being the strongest among the controls.

Most important for our purposes, the effect of increased income equality among registrants has the expected negative effect, though education equality does not.<sup>22</sup> As the share of registrants with the lowest incomes (relative to those in the highest quintile) increased between 1992 and 2000, turnout among the registered decreased accordingly. The effect here is strong, with a unit increase in income equality leading to turnout decrease of 0.407 percentage points. To illustrate, Arkansas had a rather large increase in income equality of 9.5 points. With this increase we might expect an associated drop

**Table 4**  
**2000 Registration, Turnout, and Registration**  
**Equality Turnout Among Registered Voters**

Lagged turnout (1996)	.8898***	(.1344)
Income equality change (2000-1992)	-.4069**	(.2014)
Education equality change (2000-1992)	.1245	(.1044)
Prior NVRA—low	-.015	(.0251)
Prior NVRA—none	-.0138	(.0238)
Same-day registration	.0439*	(.0289)
% college education	.0008	(.0019)
Age (% 65+)	-.0056	(.0049)
South	-.003	(.0194)
Metropolitan population	-.00001	(.0005)
Senate race dummy	.0126	(.0187)
Governor race dummy	-.0077	(.0187)
Presidential competition	-.0013*	(.0009)
Constant	.1462**	(.0867)
<i>n</i> <sup>a</sup>	49	
Adjusted <i>R</i> <sup>2</sup>	.622	

Note: NVRA = National Voter Registration Act. Entries are unstandardized regression coefficients, robust standard errors in parentheses.

a. *n* = 49 because North Dakota, with no registration, is excluded.

\**p* < .10, one-tailed. \*\**p* < .05, one-tailed. \*\*\**p* < .01, one-tailed.

in turnout among the registered of 3.87 percentage points.<sup>23</sup> Thus, as expected, we find that increases in income equality among the registered—one of the fundamental mechanisms through which NVRA proposed to increase turnout—is actually associated with declines in turnout among the registered from 1992 to 2000. By achieving its goal of increased registration, the pool of likely voters appears to have been diluted. The empirical relationship between registration and turnout has changed as a result.

## DISCUSSION AND CONCLUSIONS

Previous studies examining the relationship between registration and turnout were clear; eliminate registration barriers and increased turnout will follow. Yet, there is a relatively small segment of the turnout literature that stresses a counterpoint—that registration and turning out to vote are two separate if inherently linked processes. What motivates participation in one arena may not necessarily foster participation in the other. In this article, we draw upon this important distinction to construct a framework with which to

view the impact of motor voter on the registration-turnout relationship. In doing so, we hope to provide further insight into the nature of this relationship, by suggesting that an unintended consequence of the NVRA has been to alter the nature of the linkage between registration and turnout. This alteration is instructive to our understanding of registration as a predictor of turnout in the American states. The evidence here suggests that NVRA has helped to increase the number of individuals who are registered but also have little desire to vote. In other words, the state registrant pools increasingly comprise individuals who have their tickets but still find little motivation to attend the big game.

Our approach and analyses provide some advantages over previous research. First, our data are two or three full presidential election cycles removed from the mandated implementation of motor voter. Many previous analyses of motor voter remained inconclusive or incomplete because only one election cycle had elapsed. With our data several full election cycles removed, and most driver license renewals due within 8 years, the effects of motor voter should now be more visible. As a result, our analysis can be thought of as the first test after registration has completely changed. We do not know of any previous NVRA study that has incorporated more than one electoral cycle. Second, our data span election years from 1980 to 2000 or 2004 across the American states, granting our analyses additional leverage by accounting for normal registration and turnout fluctuation. In addition, we also provide an equity analysis derived from CPS data aggregated to the state level, thereby allowing us to determine if motor voter was actually successful in one of its other original intentions—reducing the equity gap among voters.

Most important, our approach represents a conceptual improvement in the examination of NVRA by linking it directly to success in *both* registration and turnout and the linkage between these two types of participation. NVRA legislation was premised on the stable, positive relationship between registration and turnout. To study its effects, we must examine its impact on this relationship itself. Thus, to the extent that any scholarly consensus previously existed expressing doubt about the success of reforms, we examine the NVRA under a new lens that suggests it was both successful and unsuccessful. Ironically, the success of NVRA in one area (equity) is at least partly responsible for its lack of success in its primary goal (turnout).

Our analyses highlight several important findings for turnout in the American states. First, we demonstrate how registration rates have been influenced by the NVRA, with the largest registration gains found in states that had no prior implementation of NVRA. This NVRA effect on registration is quite robust and withstands stiff tests from numerous controls as well as several sources of participation data. In addition, we provide support for recent anal-

yses finding little direct evidence of NVRA effects on increased turnout in the states.

More important, we have attempted to explain these disparate findings by examining how NVRA implementation has altered the way aggregate registration influences turnout rates across the states. Although still an important influence, registration now has a weaker effect on turnout than it did before the mandated implementation of motor voter. This finding stands as an important caveat to the turnout literature that suggests registration be treated as a crucial mobilizing force.

Finally, we attempt to tie this unintended consequence of NVRA to one of its other stated objectives—an increase in the relative equality of the registrant pool. Finding that as registrants with the lowest incomes (relative to the highest) increased from 1992 to 2000, turnout among the registered decreased, we provide some evidence into the unintended impacts of NVRA. Although motor voter may have reduced the equity gap in terms of registration, it provided no impetus for the newly registered to head to the polls on election day.

These core findings fit nicely with previous work citing the importance of mobilizing forces on turnout and add to the rising strain of participation literature that views registration and turnout as inherently related yet politically distinct acts. In the end, we are able to account for the mechanism driving some of the mixed results found in the literature evaluating NVRA. Perhaps more important, we are able to speak to the larger issue of how changes in the political environment might influence the important connection that exists between patterns of registration and turnout in the American states.

What does all of this mean for the future of voting reforms and examinations of electoral participation? The underlying point is simple—granting access to the voting booth may not be the panacea that results in people flocking to the polls. More broadly, we reiterate our point reflected on at the outset regarding high levels of participation being synonymous with a healthy electoral system. Reforms such as motor voter, although perhaps a key ingredient for resurrecting the health of our electoral system, are not likely to be the ultimate cure. Although we do not doubt that many individuals have taken advantage of the easy registration at the motor vehicle office, we also believe that NVRA's lukewarm results for turnout stem at least in part from a lack of proper accounting for the two-step process that exists between registration and voting. Registration and voting are not the same act, and removing the hurdle of registration will not automatically result in higher turnout. Similar reforms that seek to increase access but do little to increase interest or efficacy will thus likely result in similar disappointments. We want to be clear on this point: Normatively speaking, we are not advocating that it is undesirable to remove registration barriers. Rather, we believe that these structural

changes will continue to have a minimal effect until more is done to engage citizens in the process. Giving away free tickets to the game will not increase attendance if fans have little motivation to sit in the stands.

## Appendix Data and Sources

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*Registration:* Percentage of the state voting-age population that is registered.

Source: Federal Election Commission (FEC; <http://www.fec.gov/>). The passage of the Help America Vote Act 2002 created the Electoral Assistance Commission (EAC), which is now responsible for collecting and housing election statistics that were previously delegated to the FEC. In addition, for the EAC registration figures, we use the "percent active" rather than the regular percentage of registered voters. See Note 6 for a more detailed reason. For census data, the first measure is the percentage of a state's voting-age population that is registered. The second indicator is the percentage of the state's citizens that are registered.

*Turnout:* Percentage of the state voting-age population that voted in presidential elections, 1980-2000. Source: FEC. Note that 2004 turnout figures have not yet been published by the EAC. For the census data, respectively, the measures are the percentage of the states' voting-age population and the percentage of the states' citizens for 1980-2004.

*Turnout of registered:* Percentage of the registered state population that votes. Source: FEC.

*Income equality change:* Ratio of the population falling in the lowest income quintile / ratio of the population falling in the highest income quintile. Equation from Rosenstone and Hansen (1993, pp. 291-296). Data from 1992 and 2000 *Current Population Surveys* (<http://www.bls.census.gov/cps/cpsmain.htm>).

*Education equality change:* Ratio of the population with less than a high school degree / ratio of the population with a college degree or more. Equation from Rosenstone and Hansen (1993, pp. 291-296). Data from 1992 and 2000 *Current Population Surveys*.

*Prior NVRA implementation:* Coded 0 for states with a high level of prior implementation, coded 1 for low level, and 1 for states with no previous level of NVRA implementation, respectively (for list of states, see Note 8). From Martinez and Hill (1999), developed by Highton and Wolfinger (1995).

*Closing date:* Number of days between registration deadline and election day. Source: FEC.

*Mail registration provision:* Coded 1 if that state has a provision to register by mail. Source: FEC.

*Same-day registration:* Coded 1 if that state had a same-day registration provision. Source: Center for the Study of the American Electorate ([http://election04.ssr.c.org/research/csae\\_2004\\_final\\_report.pdf](http://election04.ssr.c.org/research/csae_2004_final_report.pdf)).

*Age*: Percentage of the state age 65 or older. Source: *Statistical Abstract of the United States* (various years; <http://www.census.gov/statab/www/>).

*Metropolitan population*: Percentage of the state living in a metropolitan area. Source: *Statistical Abstract of the United States* (various years).

*Percent college education*: Percentage of state with a college education. Source: *Statistical Abstract of the United States* (various years).

*State competition (presidential race)*: Absolute margin of victory in state presidential contest. Source: *Statistical Abstract of the United States*.

*Post-1992*: Coded 1 for years after 1992; 0 otherwise.

*South*: Coded 1 for the 11 states of the old Confederacy; 0 otherwise.

*Senate race*: Coded 1 if there was a Senate race; 0 otherwise.

*Governor race*: Coded 1 if there was a gubernatorial race; 0 otherwise.

## Notes

1. See Schumpeter (1950) and Berelson, Lazarsfeld, and McPhee (1954) for alternative arguments to this point.

2. Motor voter is just one component of the National Voter Registration Act (NVRA). However, we focus specifically on motor voter in this article because it is the only component that has attracted a great deal of attention in the scholarly literature and is one of the most widely used components by the general public.

3. Some states had previously engaged in similar attempts to increase participation among certain portions of the electorate. The federal mandate was designed to bring all states to an even playing field. The differences in compliance with the NVRA will be commented on later in the analysis section.

4. See Knack (1995) and Calvert (1996) for articles that take a more systematic look at the relationship between NVRA and registration.

5. The source of election statistics, along with their relative validity and reliability, is currently the topic of an important debate in the scholarly literature. We do not wish to rehash this debate or take sides on it here. Rather, we wish to acknowledge that it exists and note that we use different sources of data in an attempt to account for the various arguments and increase the robustness of our results. For more on these issues, see the spirited debate between Piven and Cloward (1989, 1990), Gans (1990), and Bennett (1990a, 1990b).

6. For the Federal Election Commission (FEC) registration data (now collected by the Electoral Assistance Commission [EAC]), we use the “percent active” rather than the percentage registered of the voting-age population. We do this for important reasons. First, and more fundamentally important for the validity of our analysis, the NVRA made it extremely difficult (if not impossible) for states to clean completely the “deadwood” from the voter registration rolls. Thus, NVRA fundamentally changed the way states keep and maintain registration statistics, making registration figures for the pre-NVRA period incomparable to post-NVRA periods. Recognizing this issue, the EAC has recently begun publishing the “percent active registered,” taking advantage of an NVRA feature that allows states to put people on inactive lists rather than remove them from the rolls altogether. Thus, a plausible figure is still attainable by simply subtracting the number of inactive people from the total registered population. Using the percent active as our registration statistic (for post-1992 elections) transforms the FEC data to make them very comparable

to how they were compiled prior to the implementation of the NVRA. In addition, we use two alternative data sets (census), thus providing a robust test that extends beyond issues concerning data sources.

7. See McDonald and Popkin (2001) for a more complete discussion of issues related to using voting-age population in the denominator of turnout calculations.

8. We use Martinez and Hill's (1999) modification of the classification of premandate motor voter implementation developed by Highton and Wolfinger (1995). We further modify this classification scheme to code each state by the individual year in which it began motor voter implementation to account for the level of previous implementation across the entire time frame. Thus, states are given a previous implementation coding for each year in which they have motor voter. In 1992, for example, the states are coded as follows: Colorado, Hawaii, Michigan, Minnesota, Montana, North Carolina, Nevada, Ohio, Oregon, and Washington are coded as high previous implementation states. Idaho, Maine, New Hampshire, Wisconsin, and Wyoming are added to this group because they have same-day registration, as is North Dakota, with no registration. States with a low level of implementation in 1992 were Alaska, Arizona, Illinois, Maryland, New Jersey, New Mexico, New York, Rhode Island, Tennessee, Utah, Vermont, and West Virginia. States with no implementation in 1992 were Alabama, Arkansas, California, Connecticut, Delaware, Florida, Georgia, Indiana, Iowa, Kansas, Kentucky, Louisiana, Massachusetts, Mississippi, Missouri, Nebraska, Oklahoma, Pennsylvania, South Carolina, South Dakota, Texas, and Virginia.

9. We do not include income because of its high correlation with education (.88). Substituting income for education in all analyses gives similar results. See the appendix for details on operationalizations and data sources for all variables.

10. Following previous literature, we attempted to include a measure of residential mobility in the registration equations, but a lack of comparable data for all states during the time frame made this untenable. Test models including residential mobility showed it to have a significant effect on registration but has no impact on the other relationships in the model.

11 Six states fall into the category of same-day registration: Idaho (1994), Maine (1973), Minnesota (1974), New Hampshire (1996), Wisconsin (1971), and Wyoming (1994), with dates of implementation in parentheses. Finally, North Dakota is excluded from all analyses involving registration, as this state has no registration provision.

12. Note that in addition to being theoretically relevant, the inclusion of the lag, along with the use of panel-corrected "robust" standard errors, also helps alleviate difficulties created by the pooled nature of the data (Beck & Katz, 1995, 1996). In addition, we run all models using Stata's random effects estimator for cross-sectional time-series models (xtreg). All models conform to those reported here.

13. Presidential competition is not included in the registration equation because we view this as short-term influence on participation and theoretically more appropriately linked to turnout.

14. With registration rates directly in the model, we drop closing date and mail registration, which have only an indirect impact on turnout through their relationship to registration (though we note that including these variables does not change the overall pattern of results). We continue to include a dummy for same-day registration states because this provision, although technically fostering registration, serves as a vehicle for allowing greater participation on election day.

15. To assess changes in the impact of registration on turnout, we are forced to exclude the post-1992 dummy as a separate variable in this equation because of its overwhelming correlation with our variable of primary interest: the Registration  $\times$  Post-92 interaction ( $r = .99$ ). Dropping the variable does not affect the theoretical point of the model, however, as the reason for the high correlation is because the Registration  $\times$  Post-92 interaction data for each state are exactly the same as those for registration in the post-1992 period. That is, the interaction variable accurately

taps the theoretical notion of the changed impact of registration on turnout, and it does this whether the post-1992 dummy is included separately or not. The only potential negative impact of dropping the post-1992 dummy is that this variable may be picking up some other factor in the political environment that has systematically served to reduce turnout after 1992. We can think of no such theoretically plausible possibility that is not adequately accounted for by the pooled nature of the data and the inclusion of lagged turnout in the model. Finally, although including all component parts of interaction terms is convention, it is not necessary unless the components are theoretically relevant in their own right. Palmer and Whitten (2003) argue that including all components of the interactive term is a “cookbook” recommendation whose sole reasoning is “to prevent inexperienced analysts from attributing a fixed effect to an interaction effect. But there is no algebraic reason why the components of interactive terms have to be included separately” (p. 143).

16. We also drop the interactions that were used in previous analyses to parse out the effects of states that previously had little or no previous NVRA implementation. We do this because we are now interested in the registration-turnout effects for all states, and because previous analyses provide little reason to believe that states with previously implemented NVRA provisions are immune from these effects. Indeed, there is evidence to suggest that the impact of registration on turnout has changed for all states, not just those that prior to the national legislation had minimal or no motor voter provisions. Thus, the inclusion of these additional interactions to parse out differential state effects is unnecessary.

17. See Rosenstone and Hanson (1993, pp. 291-296) for a more in-depth discussion on the creation of the equality (EQ) measure and its properties.

18. Note that we use 2000 data for these analyses requiring individual-level data to create the equity variables because at the time of this writing only the state-level aggregate census data are available for 2004.

19. When disaggregated by state, these data can be treated as state samples, with sample sizes ranging from 797 to 6,397 and an average of 1,915 in 1992, and 850 to 5,835 and an average of 1,632 in 2000. Registered voters range from 585 to 4,679 and an average of 1,440 in 1992, and 450 to 3,897 and an average of 1,148 in 2000.

20. We also explored the possibility of including an equality variable related to age in the model, under the assumption that NVRA might be attracting young individuals (ages 18-24) who are less inclined to vote. It is interesting that we find that the relative age equality between younger voters (ages 18-24) and those more likely to vote (ages 25-67) has actually decreased from 1992 to 2000. A full exploration of this outcome is beyond the scope of the present article. We note, however, that when age equality is included in the model, it is insignificant and does not alter the other coefficients.

21. In no cases are the lower education or income classes overrepresented relative to their wealthier and better educated counterparts. Thus, we do not need to worry about a situation where an increase in lower tier representation actually signifies an underrepresentation of the higher tier groups.

22. We were originally concerned that there might be a high degree of collinearity between the income and education equity variables, but they only correlate at .41. Therefore, we decided to leave the variables as separate components rather than combine them into an index. Education equity change correlates at a very low  $-.06$  with overall levels of education.

23. In fact, turnout among the registered did drop significantly (12.9 percentage points) in Arkansas between 1992 and 2000. The above results indicate that increases in income equality played some part in this decline.

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