What if Everyone Voted? Simulating the Impact of Increased Turnout in Senate Elections

Jack Citrin University of California, Berkeley
Eric Schickler University of California, Berkeley
John Sides University of California, Berkeley

The conventional wisdom among journalists and politicians is that higher turnout would benefit Democrats, although extant scholarly research suggests otherwise. We adopt a new approach to assessing the partisan impact of higher turnout. We use state-level exit polls and Census data to estimate the partisan preferences of nonvoters in Senate elections and then simulate the outcome of these elections under universal turnout. While nonvoters are generally more Democratic than voters, the dearth of close races means that very few election outcomes would have changed had everyone voted. Other scenarios—full turnout among registered voters, equal turnout rates for whites and African-Americans, and equal turnout rates across income groups—generate similar results: although Democrats fare better in each scenario, few outcomes would have changed. However, the gap between voters and nonvoters' partisan preference varies considerably across states and across years, suggesting that this "partisan differential" warrants further examination.

It is commonplace to bemoan the low level of turnout in American elections. Many believe that this signifies public disinterest in and disenchantment from political life. Others worry that officials elected by a declining segment of eligible voters will lack legitimacy. There is thus a plethora of ostensibly nonpartisan, civic-minded proposals intended to increase voter turnout. Yet despite the consensual rhetorical support for increased turnout, support for specific reforms tends to break down along party lines. The conventional wisdom is that since nonvoters in America are drawn disproportionately from the poor, the working class, and ethnic minorities—groups that tend to support Democrats—higher turnout would produce more Democratic votes.[1] And this would have policy consequences. Given the ideological differences between the major political parties, more Democrats in office should lead to increased support for income redistribution and an expanded welfare state, policies most conservatives deplore. Based on this reasoning, Republican legislators should be more likely than Democrats to resist measures designed to boost turnout, such as the National Voter Registration (a.k.a. "motor voter") Act. This presumption that higher turnout helps Democrats is perhaps most widely held among journalists and practicing politicians, but prominent scholars such as Lijphart (1997) and Piven and Cloward (1988) share it. For example, emphasizing the relationship between social class and turnout, Lijphart advocates compulsory voting as the route to greater economic and social equality.

Much empirical research, however, suggests that increased turnout would *not* necessarily benefit the Democrats. DeNardo (1980) was among the first to espouse this view. He argues that the relationship between turnout and the partisan distribution of the vote is complex. He distinguishes between the "composition effect" of higher turnout, which helps the Democrats by drawing lower socioeconomic status voters to the polls, and the "defection effect," which has a less consistent influence

Jack Citrin is Professor of Political Science, University of California, Berkeley, Berkeley, CA 94720 (gojack@uclink4.berkeley.edu). Eric Schickler is Professor of Political Science, University of California, Berkeley, Berkeley, CA 94720 (schick@socrates.berkeley.edu). John Sides is a Ph.D. candidate in the Department of Political Science, University of California, Berkeley (jsides@uclink4.berkeley.edu).

We thank Adam Berinsky, Ben Highton, Ray Wolfinger, Claudine Gay, Rob Van Houweling, Norman Nie, Richard Brody, and the reviewers of the article for helpful comments. Kathryn Pearson and Jocelyn Kiley provided valuable research assistance. A previous version of this article was presented at the 2001 Annual Meeting of the American Political Science Association.

[1]This is not a new idea. In a 1943 article, George Gallup blamed the Democrats' seat losses in the 1942 midterm election on "absenteeism" among union workers involved in war production (*Washington Post*, 12 April 1943).

American Journal of Political Science, Vol. 47, No. 1, January 2003, Pp. 75–90

 ISSN 0092-5853

and sometimes can benefit Republicans. DeNardo argues that higher turnout brings an increasing number of "peripheral" voters to the polls. Relative to the "core" electorate, peripheral voters have weaker party identifications. As a result, rates of partisan defection should increase with the level of turnout. Higher turnout will therefore tend to benefit the minority party in a particular jurisdiction. When the majority party is the Democrats and turnout rises, the defection effect counteracts and sometimes outweighs the impact of the changing social composition of the voting public.

Congressional election data from 1938 to 1966 illustrate this point: in districts dominated by Democrats, the relationship between turnout and the Democratic vote is actually *negative*. A further nuance to these data is temporal: in the 1960s, the relationship between turnout and the partisan division of the vote weakened considerably. DeNardo hypothesizes that voters' partisan predilections became less pronounced at this time and therefore the "core" shrank as the periphery grew. There is thus reason to suspect that the partisan implications of turnout vary not only across spatial contexts but also over time.

Studies of voting patterns at other levels of office tell a similar story. For example, when Democrats dominated the South in the first part of the twentieth century, higher turnout in presidential elections in the South helped Republicans (Tucker and Vedlitz 1986; see also DeNardo 1986 and Nagel and McNulty 2000). But once the partisan composition of the South changed, so did the direction of this relationship. Erikson (1995) finds that in the post-Jim Crow era, higher turnout in the South typically reflects the enfranchisement and subsequent mobilization of black voters and thus tends to benefit Democratic presidential candidates. Nagel and McNulty (1996) find similar trends at the senatorial and gubernatorial levels, where again there is no overall relationship between turnout and partisan advantage. In the South, however, higher turnout had in the past helped Republicans, although in the 1990s this relationship appeared to have reversed as the Democratic advantage in the South continued to wane. Taken as a whole, the research using aggregate data suggests a complex relationship between turnout and outcomes. This relationship varies over time and across electoral contexts, and thus there is no constant, linear association between turnout and the Democratic vote.

Another line of research relies on individual-level analyses and also discredits the presumed linkage between higher turnout and more Democratic votes (Wolfinger and Rosenstone 1980; Bennett and Resnick 1990; Gant and Lyons 1993; Highton and Wolfinger 2001). These studies survey the public to compare the issue positions and candidate preferences of voters and nonvoters. Typically, the few differences that do emerge are too small and inconsistent for compulsory voting to transform the political agenda or have drastic electoral consequences. A quote from Highton and Wolfinger is illustrative: "Simply put, voters' preferences differ minimally from those of all citizens; outcomes would not change if everyone voted" (2001, 179).

Given these aggregate and individual-level findings, it is perhaps unsurprising that scholars examining specific electoral reforms find that they do not have significant partisan consequences. For example, the "motor voter" legislation of 1993 tends to register younger voters without strong partisan leanings in either direction (Highton and Wolfinger 1998; Knack and White 1998). In states where voters are allowed to vote up to three weeks prior to the election, "early" voters are stronger partisans than election-day voters, but not disproportionately more Democratic or Republican (Stein 1998). Oregon's vote-by-mail system does little to change the voting behavior of "resource-poor" constituencies typically thought to prefer Democratic candidates (Berinsky, Burns, and Traugott 2001).[2]

Reviewing this literature, Grofman, Owen, and Collet (1999) identify three distinct questions regarding the partisan effects of higher turnout. First, are elections with higher levels of turnout also elections where Democrats do better? The aggregate-level studies cited above focus on this issue. Second, are low turnout voters more likely to vote Democratic than high turnout voters? The individual-level studies primarily address this issue. Third, if turnout had increased in a given election would Democrats have done better? Grofman et al. argue that answers to the first two questions cannot settle the third. This article therefore employs a new empirical strategy to estimate the partisan consequences of alternative turnout rates in specific elections.

One novel feature of our methodology is to employ individual-level data about the determinants of electoral choice among known voters to estimate the preferences of nonvoters. This enables us first to compare voters and nonvoters to determine if their partisan preferences differ. We then "add" these nonvoters, or subsets of them, to the active electorate, "as if" they had voted. This exercise generates a simple calculation of the partisan consequences of more voting. The main focus of this article is the case of full turnout, following Lijphart's (1997) policy prescription of compulsory voting. This scenario should furnish the most favorable test for the proposition that

[2] Nevertheless, there is some evidence that in high-turnout elections, the electorate is more representative of the voting-age population as a whole (Rosenstone and Hansen 1993; Hill 1994; Jackson, Brown and Wright 1998).

more voting would produce more Democratic winners.[3] More briefly, we also simulate outcomes under the alternative scenarios of full turnout among registered voters, equal turnout rates for whites and African-Americans, and equal turnout rates among different income groups.

A second novel feature of this analysis is that we calculate the effects of full turnout separately for a large set of elections. The specific electoral contests examined are the races for the U.S. Senate in 1994, 1996, and 1998. These data have several advantages. First, Senate races are numerous, allowing us to examine a large number of cases and thereby providing greater confidence in the robustness of the results than would be appropriate had we focused on just a few presidential elections. Second, unlike presidential elections, which are complicated by the dynamics of the Electoral College, Senate elections are straightforward in the sense that the candidate with the most votes always wins. This makes the partisan consequences of increased turnout transparent. Third, the widely varying political contexts of Senate elections enable us to consider whether the partisan effects of increased turnout vary across time and place, as previous research suggests. Finally, by concentrating on elections in the 1990s, it is possible to reassess the role of higher turnout during a new electoral era characterized by increased party polarization in the aftermath of the partisan realignment in the South.

This analysis allows us to explore a distinction between two potential effects of increasing turnout. One concerns the *distribution* of the vote. Our numerous state-level simulations show that there are often meaningful differences between voters and nonvoters: in the majority of cases, nonvoters tend to be more Democratic, sometimes substantially so. However, the balance of partisan preferences among nonvoters does fluctuate significantly across states and over time, and there are instances in which nonvoters are actually more Republican than voters. Previous studies using national data fail to emphasize that the magnitude and direction of the "partisan differential" between nonvoters and voters varies. This variation is a central focus of this analysis, and a later section of the article investigates how the partisan differential is related to a state's partisan history and specific campaign dynamics.

A different kind of effect concerns the *outcomes* of elections. In an important sense, elections are about tipping points, not average effects (Uggen and Manza 2000, 9). In a closely contested race, even a small difference between voters and nonvoters nonetheless could be critical were turnout to increase. In reality, there were relatively few competitive Senate elections between 1994 and 1998, and this severely limited the potential effects of increased turnout on the outcomes of the contests. Moreover, in the few cases in which our simulations find that higher turnout would have changed the result of a Senate race, the Democratic candidate was not always the beneficiary. When and how turnout affects the distribution of the vote and electoral outcomes, not whether it does, are the pertinent research questions.

[3] See Jackman (1999) on compulsory voting in Australia and Elkins (2000) on compulsory voting in Brazil.

Calculating the Effects of Full Turnout

The methodology employed here requires a large enough sample of eligible voters within each state to make reasonable inferences. The best available data comes from the November Voter Supplement that the Census Bureau conducts every election year as part of the Current Population Survey (CPS). This survey asks respondents whether they voted in the recent election. Its sample size of nearly 90,000 generates at least 900 respondents in every state, and usually more. This enables us to consider each state and each race separately, rather than assuming that the effects of increased turnout would be constant across races (see Wattenberg and Brians 2002).

Second, we need an estimate of how nonvoters would have voted. This task is complicated because the CPS does not include questions about vote choice or other political preferences. While another potential data source, the 1988–1992 Senate Election Studies, does have a rich battery of such questions, these surveys lack enough cases in each state to provide valid state-level estimates. One could pool Senate Election Study respondents across states and construct only one equation for a given election year or for a given region in that election year (see Uggen and Manza 2000), but this would undoubtedly obscure important variation across states. Another problem with the Senate Election Studies is that over-reports of turnout are substantially more frequent than in the CPS. For the 1988–92 elections, the Senate Election Studies overestimate turnout by an average of 25.5 percentage points. By comparison, for the 1994–98 elections, the CPS overestimates turnout by only 12.4 percentage points.

We therefore rely on the Voter News Services (VNS) exit polls conducted in individual states on Election Day to generate models of electoral choice. The exit polls have large enough state samples to generate reasonable estimates of vote choice based on the demographic variables

included in the CPS.[4] And since exit polls by definition sample only voters, these estimates are not contaminated by over-reports of turnout.[5] We marry the VNS data with the CPS data in the following fashion:

1. Estimate a vote choice equation for each Senate race using the relevant exit poll.
2. Take the coefficients from each equation and use them to construct a predicted vote for CPS respondents on a state-by-state basis.
3. Compare the predicted aggregate vote choice of voters and nonvoters in the CPS to determine whether the outcome of the race would have changed had all the nonvoters or a specific subset of nonvoters actually gone to the polls.

One problem with this approach is that the predictors are limited to the demographic variables common to both the VNS and CPS. As noted above, the CPS includes no political variables, and thus the vote equations cannot include potent predictors such as party identification. Moreover, the demographic variables consistently available in the VNS exit polls include only age, race, gender, income, and (almost always) education. Marital status, employment status, and union membership are occasionally available, and we incorporate them whenever possible. While one could argue that these sociological predictors produce a rather underspecified equation, we believe that the consequences are not serious, theoretically or empirically. Recall that the claim that higher turnout would help Democrats rests precisely on the presumed impact of enhanced participation in particular demographic categories. As Bartels has noted: "working politicians seem to think about electoral mobilization and conversion primarily in terms of identifiable social groups" (1998, 56).[6] Moreover, the intention of this analysis is not to explain individual-level vote choice, but to examine aggregate differences between voters and nonvoters. As discussed below, the predicted two-party vote distribution from these exit poll equations correlates very highly with the actual results of the Senate elections we study. Adding a variable like party identification to the model does not produce a significantly better aggregate prediction. Thus even these thinly specified models perform adequately.

A second problem is that, as mentioned above, the CPS itself is not immune from over-reports that potentially blur the estimated differences between voters and nonvoters. Previous research suggests that the typical person who falsely claims to have voted is relatively well-educated (Silver, Anderson, and Abramson 1986). The question here is whether those who erroneously report voting will bias our estimates of the choices of voters and nonvoters, respectively. One salutary piece of evidence is that education itself does not have a consistently strong relationship to Senate vote choice. In the 64 cases where education could be included as a predictor, it had a statistically significant ($p < .05$) relationship to vote choice in only 27 (42 percent). Moreover, even the significant relationships frequently were confined to just one of the dummy variables employed to measure education, suggesting no regularity in these effects. Furthermore, the observed relationships varied in direction: at times increasing education was associated with a Democratic vote and at other times with a Republican vote. Finally, the effects of education on vote choice generally were weaker than those of income or race. So we conclude that the over-reporting of turnout in the CPS should not skew the results of our simulations towards either of the parties.

A more crucial assumption built into our analysis is that the relationship between the demographic variables used as predictors and vote choice is the same for both voters and nonvoters.[7] This is what DeNardo (1980) objects to in distinguishing (albeit only theoretically) core and peripheral voters and arguing that peripheral voters

[4] Westlye (1991) argues that cross-sectional surveys based on cluster sampling—like the ordinary National Election Study—are inappropriate for studying state-level elections. Any survey based on cluster sampling will not necessarily contain a representative sample of any geographic subunit, such as congressional districts or states (see Stoker and Bowers 2001). This is yet another reason to rely on state-level surveys like the VNS exit polls.

[5] One problem that exit polls may have is selection or nonresponse bias, which would occur if exit polls systematically underrepresented certain kinds of voters (see Brehm 1993; Berinksy 1999, 2002). In prior work on nonresponse bias, Berinsky (1999) argues that, when confronted with issues subject to social desirability pressures (e.g., racial issues), nonrespondents secretly have an opinion distinct from that of actual respondents, such that combining the two produces a very different distribution of opinion in the aggregate. However, it seems unlikely that social desirability would systematically affect reports of vote choice. In the exit polls we analyze here, the two-party breakdown of the vote among VNS respondents correlates quite highly with the actual outcome of the election ($r = .982$). While the VNS polls do overestimate the Democratic candidate's share of the vote more often than they overestimate the Republican's, the consequences for our analysis are not significant. The difference between the actual outcome and the VNS poll marginals is not correlated with the key variables we analyze below, such as the "partisan differential" among nonvoters. Furthermore, it is not evident that the slight tendency for conservative nonresponse would bias our demographic model of the vote.

[6] Along these lines, one reason that demographics are typically included as controls in vote equations even when issues and partisanship are also present is that demographics are thought to be surrogates for attitudinal variables and thus mitigate the risks of excluded variable bias.

[7] This analysis also assumes that had nonvoters turned out, they would have cast invalid or third-party ballots at the same rate as actual voters.

are more prone to partisan defection.[8] And, indeed, in the combined 1988, 1990, and 1992 Senate Election Studies, self-reported nonvoters preferred the candidate opposite to their own party identification 33 percent of the time. Self-reported voters did so only 25 percent of the time.

However, there are other reasons to think that this assumption is useful for present purposes. The key question is how the process of mobilization affects the so-called "peripheral" voters. One likely mechanism for drawing less interested citizens to the polls is the partisan efforts of the political parties and candidates. Another is communication among friends and neighbors, who tend to be similar in background and who, if politically active, are likely to be exemplars of the dominant connection between social background and party identification. In this regard, one of the major findings of the campaign effects literature, dating back to Lazarsfeld et al. (1948), is that campaigns "activate" the electorate's underlying political predispositions, leading them to a vote choice consonant with those predispositions. If a habitual nonvoter became interested enough in an election to vote, or was mobilized directly by a party or candidate, it is quite plausible that a similar activation process would take place. It may be, as DeNardo argues, that the "periphery" has weaker predispositions than the "core" *a priori*. But the periphery is not devoid of predispositions. Ragsdale and Rusk (1993) argue that nonvoters are themselves somewhat heterogeneous. Some subtypes of nonvoters are actually relatively educated and informed, but merely indifferent to a particular electoral contest. This position, that there is diversity within the periphery, is consonant with another strand of research that has tracked the voting behavior of a set of citizens over multiple elections (Sigelman et al. 1985; Sigelman and Jewell 1986). These scholars find no simple dichotomy between "core" and "periphery." Many citizens move in and out of the electorate in a fairly haphazard fashion. As such, people who are mobilized to vote in any given election may possess relatively enduring political predispositions that when activated by the campaign would lead them to behave quite similarly to habitual voters.[9]

In addition, the scholarly and political debates about the effects of higher turnout focus largely on the causal role of sociological variables. The conventional wisdom is that racial and class differences in turnout lead to "biased" outcomes, in the sense of reducing the Democratic vote. The assumption that nonvoters who are brought to the polls would behave similarly to their sociological counterparts among habitual voters furnishes the most favorable test for the hypothesis that higher turnout benefits the Democratic party. For these reasons, we believe that the advantages of the proposed simulation overcome the shortcomings just listed and that our methodology provides a novel way of identifying the partisan effects of increased turnout.

Estimating Vote Choice in Senate Elections

The first step in the simulation is to model vote choice in each of the 91 Senate races with available data.[10] Drawing on the appropriate VNS exit poll, we estimate an equation in which the dependent variable, coded 1 for a Democratic vote choice and 0 for a Republican vote choice, is a function of age, race, income, and gender, as well as education, marital status, and union membership where available. Each of the predictors is measured by a dummy variable or a series of dummy variables. (See the appendix for a discussion of the specification of the equations and measurement of each covariate.)

For purposes of illustration, Table 1 presents the VNS models for four selected races. Clearly, the effects of these demographic predictors vary in direction, magnitude, and statistical significance across these four races. For example, the likelihood of voting Democratic increases with age in Virginia's 1994 race, but nowhere else. In California in 1998, the coefficients for age, though statistically insignificant, suggest the opposite relationship. While higher income is associated with a declining probability of voting Democratic in California, Illinois, and South Carolina, it has the opposite, albeit statistically insignificant, effect in Virginia. The Virginia race is also noteworthy in that education is strongly associated with a vote for the Democrat, Charles Robb. This may derive from Oliver North's distinctive brand of social conservatism. Gender is significant in races where there were women candidates—Barbara Boxer in California and Carol Moseley-Braun in Illinois—and in the Virginia race, which could again indicate an election-specific reaction to North. The effect

[8]There is some empirical evidence for heterogeneity in what predicts vote choice, deriving from such factors as political sophistication (e.g., Rivers 1988; Sniderman, Glaser, and Griffin 1990).

[9]See Wattenberg and Brians (2002) for an analysis of midterm legislative elections using a similar assumption.

[10]The total number of Senate races in this time period is 104. VNS conducted no exit polls in several elections: in 1994, Vermont, Rhode Island, Indiana, North Dakota, West Virginia, Tennessee, Mississippi, Utah, and Hawaii; in 1996, Kansas and Oregon; and in 1998, Alaska and Hawaii. Most of these elections were uncompetitive. In only two cases—the 1994 Vermont race and the 1996 Oregon special election—was the margin less than 55–45.

TABLE 1 Logit Models of Vote Choice in Selected Senate Races

	California (1998)	Illinois (1998)	South Carolina (1998)	Virginia (1994)
Age 30–44	−.04	.10	−.47#	.22
Age 45–59	−.07	.05	.06	.63**
Age 60 and above	−.22	−.17	.17	.83**
$15–30,000	−.17	.11	−.14	.05
$30–50,000	−.45*	−.19	−.29	.25
$50–75,000	−.68**	−.56	−.61	.24
$75,000 and above	−.68**	−.38	−.39	.20
HS diploma	−.04	.77#	−1.26**	.25
Some college	−.17	.77#	−1.10**	.38
College degree	−.19	.66	−1.33**	.87**
Advanced degree	.34	1.18**	−.37	1.30***
Female	.43***	.50***	.23	.45**
Latino	1.03***	.58	1.52	1.05#
Black	1.82***	3.01***	3.40***	3.12***
Asian	.13	.80#	−1.24	.30
Other	.27	.14	1.78	1.05
Union	.35**	.42**	–	–
Veteran	–	–	–	−.18
Constant	.30	−1.42**	.88	−1.52***
Log-likelihood	2655.3	1287.2	973.6	1304.4
χ^2	218.0***	254.8***	311.5***	280.8***
PRE	.099	.204	.380	.194
N	2117	1097	900	1335

Table entries are logit coefficients with their associated levels of significance. The dependent variable is coded 1 for a Democratic vote choice and 0 for a Republican vote choice. The excluded categories are: age (under 30); income (less than $15,000); education (no high school degree); and race (white). The results for Virginia (1994) also include dummy variables for marital and employment status, though these coefficients are not displayed.

Source: Voter News Service exit polls.

***p < .001; **p < .01; *p < .05; #p < .10 (two-tailed).

of race also varies. Though being black is always associated with voting Democratic, this relationship is much stronger in Illinois, where a black candidate (Moseley-Braun) was running, and in both Southern states, where there is a large black population and a more salient party cleavage on racial issues. These results demonstrate that the structure of vote choice (i.e., the effects of particular demographic attributes) varies across electoral contexts, and thus underscore the benefits of using state-by-state VNS data rather than a pooled regional or national estimate.[11]

[11]The percent reduction in error, or PRE, at the bottom of each column indicates in each case that these models could do better. However, we were pleased to see values as large as they were in the case of Illinois, South Carolina, and Virginia, given the paucity of variables available to us. In general, this "sociological" model of voting produces PREs between .05 and .40.

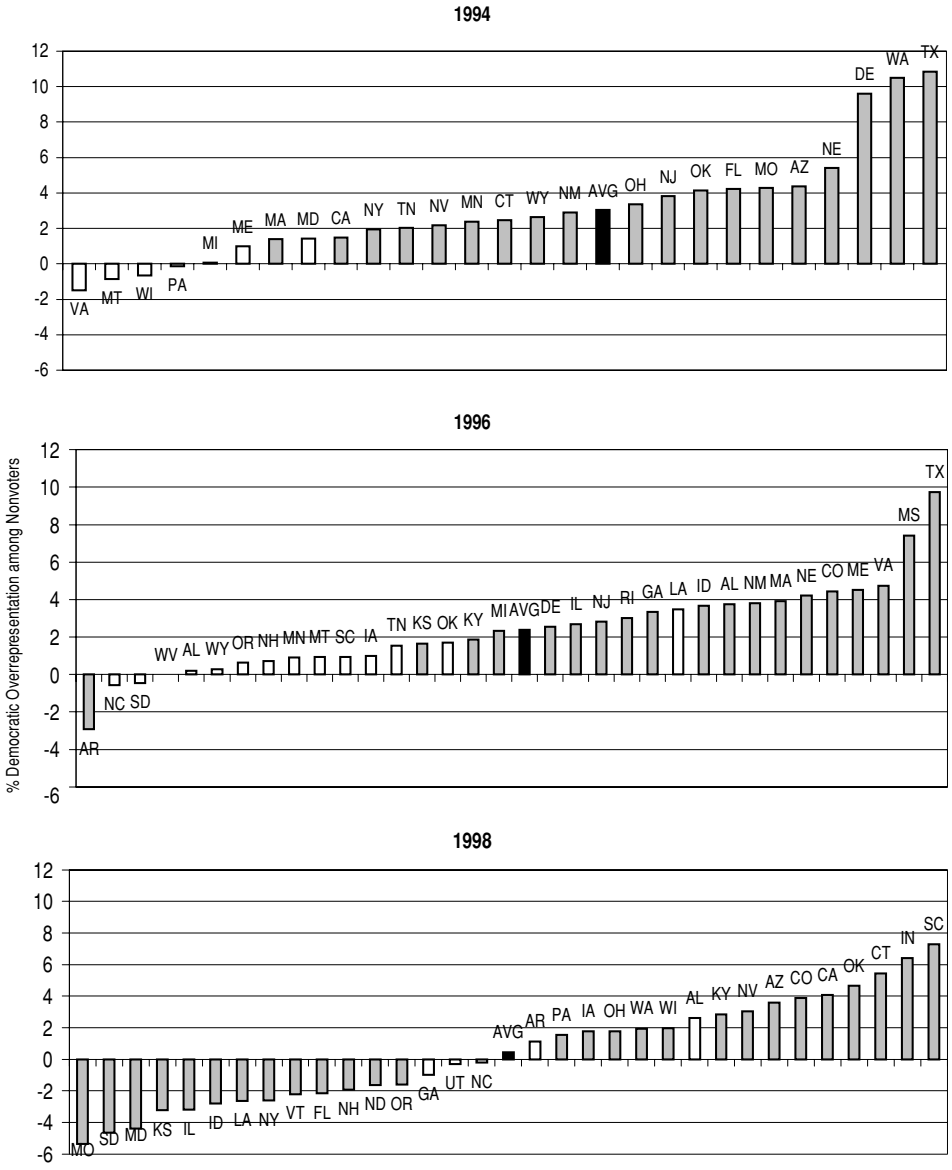
"Partisan Differential": Comparing Voters and Nonvoters

Turning now to the CPS data, we use the estimated coefficients from the exit poll equations to calculate the predicted probability of a Democratic vote choice for each respondent in each state.[12] A first question is whether voters and nonvoters differ in their partisan preference. We compare the mean probability of a Democratic vote choice among voters to the mean among nonvoters.

Figure 1 presents the percent of nonvoters estimated to support the Democratic candidate minus the percent of voters estimated to support the Democratic candidate.

[12]This is calculated in the usual fashion for logit equations: P(vote Democratic) = $\exp(X\beta)/1 + \exp(X\beta)$, where $X\beta$ is the sum of each variable multiplied by its coefficient, plus the constant.

FIGURE 1 Difference Between Estimated Vote Preference of Voters and Nonvoters



Note: Gray bars signify that the difference between voters and nonvoters is significant at $p < .05$.

Positive numbers indicate that nonvoters are more Democratic than voters. (Bars in gray indicate that the difference between voters and nonvoters is statistically significant at the .05 level.) As the graphs in Figure 1 demonstrate, nonvoters were more Democratic than voters in 74 percent of the races examined (67 of 91). On average, nonvoters were 1.9 percentage points more Democratic than voters and in 38 of these 91 races they were at least 2.5 percentage points more Democratic. In several races, the difference between voters and nonvoters is actually quite large—e.g., more than 10 percentage points in Texas and Washington in 1994. By contrast, nonvoters were at least 2.5 percentage points more Republican than voters in just nine races.

The observed variation across years also conforms to the standard accounts of political commentators. In 1994, nonvoters were, on average, 3.05 points more Democratic than voters (treating each race as the unit of observation, SD = 3.20). In that year, the Christian Coalition and other conservative groups made a special effort to mobilize support for Republican candidates. The partisan differential

between voters and nonvoters was almost as large in 1996 (2.39; SD = 2.40), notwithstanding the overall rise of voting in a presidential election year. In 1998, however, the gap was just .44 (SD = 3.39), indicating that nonvoters were, on average, only slightly more Democratic than voters.[13] Of the 24 races in which nonvoters were more Republican than voters, 16 occurred in 1998, suggesting that the major get-out-the-vote drive launched by unions and other Democratic-allied groups at that time paid off.[14] The state of Washington provides a clear illustration: in 1994, nonvoters were 10.5 points more Democratic; in 1998, they were only 1.9 points more Democratic. That a swing of this magnitude took place *within* a single state over only four years suggests that campaign contexts affect the difference in the partisan composition of voters and nonvoters.

In sum, while nonvoters generally are, as a group, more Democratic than voters, the differences across elections confirm DeNardo's point that the conventional wisdom is not ironclad. There are cases where nonvoters are more Republican than voters. That said, in most Senate races the conventional wisdom holds: nonvoters are more Democratic than are voters. At times, the gaps can be quite large. The next task is to determine whether this "partisan differential" affects election outcomes.

Simulating the Effects of Full Turnout on Electoral Outcomes

To estimate the partisan effects of turnout with the utmost validity, the VNS models should generate predictions very close to the actual outcomes of these Senate races. Fortunately, this was largely the case: the correlation between the VNS prediction and the outcome was over .99 in each of the three election years.[15] The next imperative is to find the best measure of the behavior of voters, to which we can then combine the predicted behavior of nonvoters. We used the actual distribution of the vote as the measure of the voters' behavior. To this we add the estimated choice of the CPS nonvoters, weighting by the actual level of turnout in the election. (As before, the preference of nonvoters is measured by the mean of the predicted probability among nonvoters in the CPS.) Since the actual outcome is obviously the best available estimate of what voters did, we do not need to rely on the predicted Democratic vote share among CPS voters.[16] In addition, we can reasonably assume that the CPS nonvoters did in fact abstain in the actual election.

An example illustrates how to compute the outcome under the counterfactual condition of universal turnout. In California's 1994 Senate race, Feinstein received 51 percent of the two-party vote. Thirty-seven percent of those eligible turned out in that election. Our coefficient estimates indicate that Feinstein would have won 50.3 percent of the two-party vote among nonvoters. The expected vote share for Feinstein under full turnout thus becomes: $(51.0) \times (.37) + (50.3) \times (.63) = 50.5$.

Under full turnout, Democrats typically do better, as one would expect given the partisan differentials displayed in Figure 1. The Democratic candidate's percent of the vote increases by an average of 1.5 percentage points in 1994, 1.3 points in 1996, and .15 points in 1998. The variation across years corresponds to that in Figure 1: the Democrats would have benefited more from universal turnout in years when Republicans were apparently more mobilized, such as 1994. These average Democratic gains do conceal some notable variation. There are races in which the Democratic candidate does significantly better under full turnout. For example, in the 1996 Texas Senate race, the simulation suggests that the vote share of insurgent Democrat Victor Morales would have increased by five percentage points, producing a virtual dead heat with incumbent Republican Phil Gramm. A handful of other races that initially had fairly healthy GOP margins become extremely close in this simulation, including the 1996 contest between Joseph Brennan and Susan Collins in Maine, as well as the 1996 Virginia Senate race between Mark Warner and John Warner. In a few other cases, the

[13] These yearly averages are statistically different at conventional levels ($F = 6.1$, $p < .01$). We replicated the analysis reported in Figure 1 using the actual outcome, rather than CPS voters, as the comparison group for CPS nonvoters. This produced similar results: the two measures of partisan differential were correlated at .83, and the means in each election year were virtually identical.

[14] Our evidence obviously does not establish that the Christian Coalition was especially active in 1994 or that labor was more engaged in 1998. However, the much smaller pro-Republican turnout bias in 1998 at least suggests that Democrats' get-out-the-vote efforts that year were more successful than in the preceding two elections.

[15] Adding party identification to these equations, while certainly improving the models' performance at the individual level, did very little to improve the prediction at the aggregate level.

[16] The predicted Democratic vote among CPS voters is, as one would expect, extremely close to the actual outcome. The only differences appear to be due primarily to sampling variability and imprecision in our model of the vote. Thus, we find that the percent Democratic among CPS voters was symmetrically distributed around the actual outcome, rather than being skewed in either the Democratic or Republican direction. An alternative way to do our simulation would be to simply combine CPS voters and nonvoters, without bringing in the actual outcome. This generates similar results to those reported below, but does sacrifice information and thus leads to different predictions in the very small number of cases in which our estimate for CPS voters departs from the actual outcome.

Table 2 Simulated Effects of Full Turnout in Senate Races

Year	State	Candidates	Actual Outcome	CPS Results		Simulation (full turnout)
				Nonvoters	Voters	
1994	Delaware	Oberly (D)	43.2	54.3	44.7	50.2
		Roth (R)	56.8	45.7	55.3	49.8
1994	Virginia	Robb (D)	51.5	47.4	48.9	49.0
		North (R)	48.5	52.6	51.1	51.0
1994	Washington	Sims (D)	44.3	55.1	44.7	50.5
		Gorton (R)	55.7	44.9	55.3	49.5
1998	Kentucky	Baesler (D)	49.7	52.7	49.9	51.6
		Bunning (R)	50.3	47.3	50.1	48.4

simulations indicate that Republicans would have benefited from full turnout. For example, Democrat Charles Schumer's margin of victory over incumbent Republican Alfonse D'Amato in 1998 becomes exceedingly small.

The next question is whether these shifts in the distribution of the vote under universal turnout are sufficient to change actual election outcomes. The answer is only rarely. In this simulation, four of the 91 races would have had a different winner if everyone eligible had voted. Table 2 presents data regarding these four races, including the actual outcome, the estimated preference of CPS voters and nonvoters, and the simulated outcome under full turnout. Three races would have switched from the Republican to the Democratic column: the 1994 race in Washington between Republican Slade Gorton and Democrat Ron Sims; the 1994 race in Delaware between Republican William Roth and Democrat Charles Oberly; and the 1998 race in Kentucky between Democrat Scotty Baesler and Republican Jim Bunning.[17] The race that flips in the opposite direction is the 1994 Virginia contest between Democrat Charles Robb and Republican Oliver North. North's victory in this simulation likely derives from the unusually strong positive impact of education on Democratic vote choice. Education routinely predicts turnout but not necessarily vote choice. In this case, virtually all of the best demographic predictors of high turnout—education, age, and income—were positively related to Democratic vote choice (see Table 1). As a result, full turnout would in essence have mobilized a substantial number of North supporters.[18]

The reason full turnout changes so few election outcomes is that, like elections for the House of Representatives, most Senate elections also are subject to vanishing (or at least scarce) marginals (see Mayhew 1974).[19] If one defines "competitive," somewhat arbitrarily but generously, as a race where the two-party division of the vote is 55–45 or closer, only 31 percent of Senate races in 1994, 1996, and 1998 qualify (33 of 104). While candidates and their campaign managers always run scared and worry about how surges in turnout might cause an upset, in reality many races are so lopsided that even the most thorough mobilization of nonvoters would probably not change the outcome. For example, in 1998 Republican Frank Murkowski of Alaska won nearly 80 percent of the vote. Even if every man, woman, child, and caribou from Juneau to Prudhoe Bay had voted, his victory would have remained a *fait accompli*. Skeptics of the supposed partisan effects of higher turnout tend to focus on similarities among voters and nonvoters, but the structure of electoral competition is arguably equally important. Indeed, even if nonvoters and voters were quite politically

[17] The Kentucky result is consistent with contemporary media coverage of that election. In the *New York Times* of 29 October 1998, R.W. Apple wrote, "Here in the hills of eastern Kentucky . . . the politicians are talking about a 'turnout election.'" That the Sims-Gorton outcome flips is also consonant with reporting on that race. Susan Gilmore of the *Seattle Times* made this point in an election post-mortem: "Sims did better among less-dependable voters" (9 November 1994).

[18] A nuance of this race was the presence of another Republican, J. Marshall Coleman, who lost in the primary to North but ran as an independent in the general election and garnered 11.5 percent of the vote. The analysis presented in Table 2 ignored third-party candidates. However, we conducted additional simulations that incorporated Coleman and obtained the same pattern of results: nonvoters were more pro-North and slightly more pro-Coleman, but always less favorable toward Robb than were voters. When we conducted the full turnout simulation with all three candidates, North still wins a plurality of the vote.

[19] It is true that Senate elections are more competitive than House elections (Abramowitz and Segal 1992), although the incumbency advantage in the Senate has grown notably since 1950 (Highton 2000).

distinct, increased turnout will rarely flip the outcome of one-sided races.

Of course, even small gains can have enormous political consequences; in the evenly divided 107th Senate, a Baesler-for-Bunning switch would have given Democrats a slim majority even before the defection of James Jeffords of Vermont. It is also possible that by making certain Republican leads much narrower, full turnout would force Republicans to spend more money in these races, indirectly helping Democratic candidates elsewhere.

To summarize, the exact number of outcomes changed by our simulation is less important than the more general pattern of results showing that there are significant differences between voters and nonvoters in many Senate races. Nonvoters generally are more Democratic than voters, though these differences vary across races. However, the structure of electoral competition—i.e., the dearth of extremely close elections—means that even full turnout would likely change only a handful of outcomes. While sampling variability, potential equation error, and the vagaries of mobilization and candidate strategy mean that one cannot be absolutely certain that a given outcome would have changed under universal turnout, the basic story is likely to be robust.[20]

Alternative Simulations: Registration, Race, and Income

Given that compulsory voting is nowhere on the political horizon, it is worth projecting the partisan effects of smaller increases in voter turnout. We consider three such scenarios in this section: universal turnout among registered voters, equal turnout rates for African-Americans and whites, and equal turnout rates across income groups.[21]

The first of these simulations was the most straightforward to conduct, as the CPS asks respondents if they were registered. We simply took the actual outcome of the race and then added in those CPS nonvoters who said they were registered to vote.

For the analysis assuming equal turnout rates among blacks and whites, we first determined whether, in each state, a higher proportion of blacks or whites in the CPS reported voting. We then added in nonvoters from the lower turnout group until its participation rate equaled the higher turnout group's rate. In the majority of cases (78 percent), this meant adding in additional African-American voters.[22] For example, if the reported turnout rate among whites was 40 percent and the rate among blacks was 35 percent, we added in the number of African-American nonvoters required to make the black turnout rate 40 percent. The additional black voters were identified by estimating a model of turnout in each state using all of the available CPS demographic variables.[23] We then computed predicted probabilities of turnout for all respondents. We added in those nonvoting blacks with the highest predicted probability of voting, on the assumption that if turnout increased these would be the most likely

[20]Two alternative ways of calculating the effects of full turnout demonstrate that, while the distribution of the vote always shifts in the same direction as before, this shift is not always enough to change specific outcomes. First, we replicated the simulation for each race in which the outcome changed using the lower bound (or, for the Virginia race, the upper bound) of the 95 percent confidence interval around the percent of nonvoters favoring the Democrat. Under this conservative test, two races whose outcomes changed originally do not change: Roth wins the 1994 Delaware race, and Robb wins the 1994 Virginia race. Second, we replicated the full turnout simulation by calculating the behavior of nonvoters differently—as the actual outcome of the race plus the partisan differential (i.e., the difference between CPS voters and nonvoters) in that race. The simulated outcome thus equals:

$$(\text{actual outcome} \times \text{turnout}) + [(\text{actual outcome} + \text{partisan differential}) \times (1 - \text{turnout})]$$

This version assumes that the estimate of the nonvoters' partisan preference may be inaccurate because of equation or sampling error, but that such error affects voters and nonvoters similarly, such that the difference between voters and nonvoters in the CPS is estimated accurately. Of the four races that switched using the simulation described in the text (see Table 2), the 1994 Delaware and Virginia races do not switch under this alternative version. The 1996 Texas race, which became extremely close in the original simulation, switches in this alternative version.

[21]To do so, we computed the following equation:

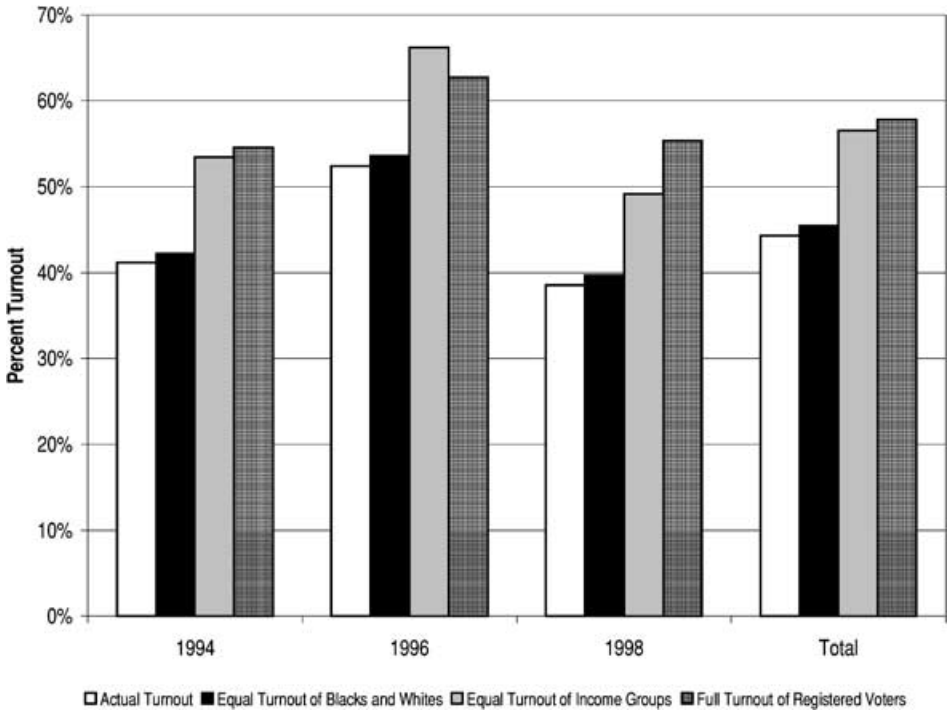
$$(\text{actual Democratic vote share}) \times t + (\text{estimated Democratic vote share among new voters}) \times (1 - t)$$

In this equation, t = (actual turnout)/(new turnout under simulation). New turnout was calculated as: (actual turnout rate) × (CPS turnout rate under simulation/CPS turnout before simulation). In other words, the actual turnout rate is increased by a factor greater than 1, which is derived from the simulated increase in turnout within the CPS. We then use this "new turnout" essentially to weight the Democratic votes shares, both actual and among the new voters. The weighted average of these two vote shares becomes our simulated estimate.

[22]In ten cases, there were an inadequate number of blacks in a given state's sample to do this simulation. These were: in 1994, Maine and Montana; in 1996, Idaho, Maine, and South Dakota; and in 1998, Idaho, New Hampshire, North Dakota, Pennsylvania, and Vermont. In the 1996 Alabama race, blacks and whites turned out at the same rate, making this simulation moot. Thus, we examine only 80 of 91 races in this simulation. Of those 80 races, 62 featured higher white turnout than black turnout.

[23]This model was specified according to past practice (e.g., Wolfinger and Rosenstone 1980). It contained these variables: age (number of years), age squared, education, income, marital status, gender, race, employment status, and union membership, as well as dummy variables for being a student and for being a government employee.

FIGURE 2 Turnout Under Various Simulations



additional voters.[24] Next, we used our earlier equation predicting vote choice to estimate the mean Democratic vote share among these new, hypothetical voters. The final step was to take the actual election outcome and project the impact of adding these voters. This allows us to estimate how election outcomes would differ if there were no racial bias in turnout rates.

The income simulation was conducted in essentially the same manner. The only important difference is that there were five income categories in the CPS. As a result, we raised the turnout rate of each income group to the turnout rate of the group with the highest reported participation level. With only 13 exceptions, this meant raising the four lower income groups to the turnout rate of the highest income group.[25] This analysis allows us to estimate how election results would differ if there were no economic bias in turnout. Admittedly, eliminating such bias is unlikely to happen anytime soon but the simulation is still illuminating given scholarly attention to unequal participation across income groups (e.g., Piven and Cloward 1988; Verba, Schlozman, and Brady 1995).

Figure 2 provides a sense of how much overall turnout would rise under each of the aforementioned counterfactual situations.[26] Equalizing white and black turnout does relatively little on average in any year, in part because a large number of states have small black populations. However, mobilizing all registered voters or equalizing turnout among income groups does make a difference. Under these circumstances, turnout generally increases by between ten and twenty percentage points. In the absolute, turnout in the registration and income simulations reaches a level that is certainly quite high compared to current levels, but is not entirely implausible. Thus, relative to the universal turnout simulation, these other simulations provide a more modest test of the effects of increased turnout.

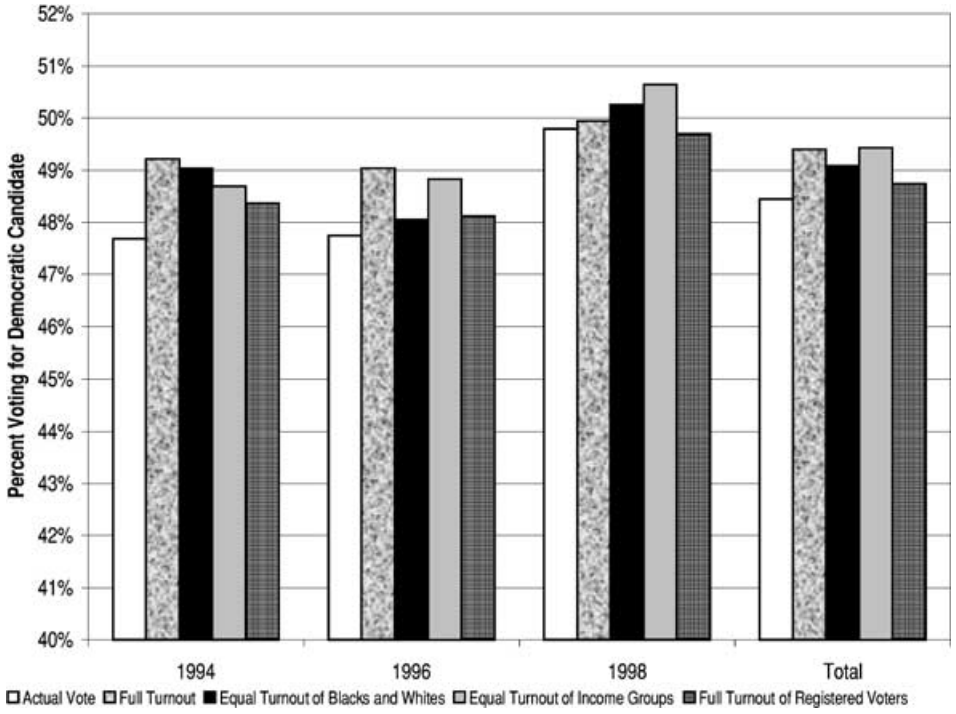
Does increasing turnout in these scenarios actually help Democratic candidates? Figure 3 presents the average

[24]In general, the black nonvoters "mobilized" in this simulation were quite representative of black nonvoters as a whole in terms of their predicted partisan preference. This is not surprising, given that blacks typically support Democratic candidates at such high rates. When one instead takes a random sample of black nonvoters and adds them to the projected electorate, the results are virtually identical to those reported.

[25]In twelve cases, the second-highest income group had the highest turnout: in 1994, CT, MO, OK, AZ; in 1996, ME, TN, and CO; and in 1998, WI, IA, ND, LA, and ID. In one case, Wyoming in 1994, the third-highest income group had the highest turnout. As in the race simulation, the nonvoters mobilized in this simulation were again representative of nonvoters in their income group as a whole.

[26]The raw data on which Figures 2–4 are based can be found in Table A-1. Turnout under these simulations was calculated as described in footnote 20.

FIGURE 3 Effect of Simulations on Democratic Vote



Democratic vote share in each simulation. In nearly every case, these simulations help the Democrats—at times nearly as much as or more than full turnout itself. The effects are somewhat larger in 1994 than in other years, perhaps reflecting the relatively poor showing Democrats made in turning out their voters in that election. These increases average one-and-a-half percentage points at most. In some states, of course, the increases are larger. For example, the income simulation would have increased the Democratic share of the vote by 3 percentage points or more in eight of these 91 races.

Do any of these three simulations produce actual changes in the outcome of races? As in the case of the universal turnout simulation, a reversal of the result almost never occurred in the Senate elections we studied. Adding registered, black, or lower-income nonvoters to the electorate produced a different winner only in the extremely close 1998 Baesler-Bunning race in Kentucky, which the full turnout simulation also flipped. Our results suggest that the Democrat Baesler would have won 50.5 percent of the vote if all registered voters had turned out, and 50.4 percent if turnout had been equal across income groups.[27] The race simulation did not change the outcome here, though it did narrow Baesler's margin of defeat slightly. In sum, these alternative simulations had effects very similar to the full turnout simulation: a higher Democratic share of the vote in most instances but only rare changes in the outcome of any given contest. The structure of competition (or lack thereof) mitigates the partisan consequences of increased turnout.

Explaining Variations in "Partisan Differential"

A consistent finding of our diverse simulations is variation across states and over time in the degree to which nonvoters are more Democratic than voters (see Figure 1). What explains the variation in the partisan differential between voters and nonvoters? Table 3 presents the relationship in each year between the partisan differential and a range of potential explanatory variables. As the first row of the table indicates, higher turnout in Senate

[27]The standard errors on these simulated results for Baesler are large enough that the predicted vote share cannot be statistically distinguished from 50 percent. However, since Baesler wins consistently in every simulation we conducted, it seems likely that increased turnout would have had an impact on the outcome of this race.

Table 3 Variation in Democratic Overrepresentation among Nonvoters

	N	1994	1996	1998	Total
Correlation of Democratic overrepresentation among nonvoters and:					
Actual overall turnout in the race	91	−.203	−.140	−.201	.005
Democratic margin of victory/defeat	91	−.296	−.292	−.145	−.233*
Democratic percentage of presidential vote since 1980	91	−.183	−.153	−.090	−.137
Democratic advantage in spending (logged)	91	−.184	−.213	−.126	−.099
Percent Democratic overrepresentation among nonvoters by:					
Democrat incumbent running	35	1.8	1.6	.1	1.0
Republican incumbent running	29	6.9	3.3	.3	2.5
Open seat	26	2.8	2.2	1.9	2.3
Northeast	18	2.9	2.9	.1	2.1
South	29	3.5	2.4	.8	2.1
Midwest	22	2.5	1.8	−.7	.1
West	22	3.3	2.5	1.5	2.4

*$p < .05$

elections is negatively associated with the partisan differential, meaning that nonvoters are less disproportionately Democratic in high-turnout races—a finding that is consistent with the conventional political wisdom that, on average, higher turnout helps Democrats. Democratic overrepresentation among nonvoters is also reduced as that party's share of the vote increases, although the opposite is also probably true: as Democrats are mobilized to vote, Democratic candidates' share of the vote should increase. A related finding, reported in the third row of the table, is that Democratic overrepresentation among nonvoters is reduced as the party's share of the presidential vote in *previous* elections increases. In states where the "normal vote" is more favorable to the Democrats, their relative advantage among nonvoters declines. One interpretation of this relationship is that in traditionally Democratic states, the party organization does a better job of mobilizing its supporters and thus nonvoters in these states are less disproportionately Democratic. A cautionary note is that the magnitude of the correlations is modest in each case and, as the table indicates, there are so few cases that most of the correlations are not statistically significant.

Table 3 also suggests that some campaign-specific variables may be associated with the partisan differential. Not surprisingly, since one effect of campaign spending is to mobilize one's supporters, a Democratic advantage in campaign spending tends to reduce Democratic overrepresentation among nonvoters. Furthermore, Democratic overrepresentation among nonvoters is lowest when a Democratic incumbent is running and highest when a GOP incumbent is running, with the bias in open seat races somewhere in between.[28] This hints at another kind of mobilization effect: Democrats go to the polls when their party's candidate is attractive, well-known, or likely to win, as in the case of an established incumbent. Put another way, Democrats in Massachusetts are more likely to turn out for Ted Kennedy than Democrats in Mississippi are to turn out for (or against) Trent Lott, an unattractive (to them) incumbent.

Also noteworthy is the stability in Democratic overrepresentation among nonvoters across regions. The partisan differential is virtually identical in the South, Northeast, and West. The sole exception is the Midwest, where Democrats have apparently done a better job of turning out their supporters, and African-Americans specifically. Of the 18 races where black turnout exceeded white turnout, eight were in the Midwest—Ohio (twice), Illinois (twice), Michigan (twice), Indiana, and Minnesota.[29]

Ultimately, a pooled time-series cross-sectional analysis encompassing more years and additional indicators of campaign contexts should better illuminate the sources of the varied partisan effects of increased turnout. For example, Southwell (1996) suggests that the working class is

[28]The relationship between incumbency and Democratic overrepresentation among nonvoters is statistically significant in 1994, but falls short of statistical significance in 1996 and 1998. For the entire set of races, the relationship falls just short of statistical significance ($p = .13$).

[29]It is also possible that union strength plays a role. However, we examined union turnout by region in the CPS during these years and did not find that it was any higher in the Midwest.

more mobilized when economic concerns are salient factors in the election. If this were true, then higher turnout would have been more likely to help the Democrats when the cleavage between the parties centered on issues related to economic liberalism and conservatism rather than on the racial and cultural issues that splintered the New Deal coalition. Still, the results reported here point to the significance of mobilization processes, candidate quality, and state political history in determining whether and how much increased turnout would affect the partisan division of the vote.[30]

In this regard, the method we have developed allows for additional simulations. For example, we could construct more realistic scenarios where turnout increases in the aggregate or among certain groups by small amounts, say, 5 or 10 percent. That said, our results to date suggest that small increases would likely have even milder partisan consequences. We could expand the analysis to other contests, such as gubernatorial elections, state-level presidential elections, and votes on ballot measures. If the presidential election of 2000 taught us anything, it is that tiny changes in the distribution of the popular vote (and thus small differences in turnout) can make an enormous difference to the nation's politics.

Conclusion

This article outlines a new methodology for projecting the consequences of higher turnout for American party politics. By simulating the outcomes of Senate elections under alternative turnout scenarios, we revisited the proposition that increasing the level of voter participation would benefit Democratic candidates and, by implication, promote government policies aimed at benefiting ethnic minorities, low-income, and working-class citizens.

The initial conclusion of our simulations is that there are indeed meaningful differences in the partisan leanings of voters and nonvoters. But while nonvoters usually are more Democratic than voters, there are exceptions to this tendency, a finding that departs from both the conventional wisdom that more electoral participation always helps the Democrats and previous scholarship maintaining that universal turnout would make no political difference.

A second conclusion is that the partisan effects of higher turnout vary across states, time, and electoral contexts in ways that make political sense but that have not been closely attended to in the literature on political participation. Specifically, our simulations suggest that the interplay between a state's political history, the quality of candidates in a specific election, and organizational factors affecting the mobilization of voters help account for variation in the partisan effects of increased turnout.

Our third main point is that despite the impact of higher turnout on the partisan distribution of the vote, the structure of electoral competition dampens the potential effect on who wins or loses. The lack of competitiveness of most Senate (and House) elections means that there are unlikely to be many races where even universal turnout would change the outcome. Even under these conditions, however, increasing turnout might have indirect effects on the overall results nationwide. It could conceivably render certain races more competitive and thereby force parties to shift the allocation of campaign resources. In addition, candidates might be motivated to craft different messages. For example, they might emphasize ethnicity- or class-based appeals more strongly if they believed that compulsory voting would increase the number of minority and low-income voters. And this reframing of an election conceivably could modify the relationships between social background and voting behavior.

The simulations presented here assumed that nonvoters would have the same candidate preferences as voters with the same demographic characteristics; in other words, the periphery would behave exactly as the core. Future research might relax this assumption, though we should first probe further to determine whether the preferences of nonvoters and occasional voters are more influenced by short-term forces than are those of chronic participants. Such short-term forces, according to DeNardo, on balance are likely to benefit the minority party. Yet if a state's entrenched political culture or overall climate of opinion also influence voting, then newly minted voters may be most affected by these prevailing norms and thus may be predisposed to favor the majority party in the state. Our own exploration suggests that the degree and direction of potential partisan defection among peripheral voters depend on local circumstances and campaign dynamics.

Finally, our analysis indicates that that the direction and strength of the relationships among social background, turnout, and party affiliation are variable, not constant. The magnitude of class differences in turnout waxes and wanes, even within the United States. So does the extent of class voting. The methodology developed here, in which we analyze multiple races under a variety of assumptions regarding turnout levels, is a fruitful approach for identifying if and when increased turnout has political consequences.

[30]Other work that emphasizes the role of mobilization includes Rosenstone and Hansen (1993), Gerber and Green (2000), Shaw, de la Garza, and Lee (2000), and Leighley (2001).

TABLE A-1 Effects of Simulations

	1994 (N = 26)	1996 (N = 33)	1998 (N = 32)	Total (N = 91)
Turnout under various simulations				
Actual turnout	41.1	52.4	38.5	44.3
Full turnout of registered voters	54.6	62.8	55.3	57.8
Equal turnout of whites and blacks	42.1	53.6	39.7	45.4
Equal turnout of all income groups	53.4	66.2	49.1	56.5
Percent vote for the Democratic candidate				
Actual outcome	47.7	47.7	49.8	48.4
Full turnout	49.2	49.0	49.9	49.4
Full turnout registered voters	49.0	48.0	50.3	49.1
Equal turnout of whites and blacks	48.7	48.8	50.6	49.4
Equal turnout of income groups	48.4	48.1	49.7	48.7

Table entries are the numbers presented in Figures 2–3.

Appendix

In estimating a model for vote choice in each exit poll, we took advantage of every demographic predictor for which there data was available in both the VNS and CPS. Every model we estimated contains age, gender, race, and income. The following exit polls from 1996 did not include an education variable: AK, AL, AR, DE, ID, IA, KS, KY, LA, MA, MS, MT, NE, NM, OK, RI, SC, SD, VA, WV, WY. Neither did these 1998 polls: KS, LA, ND, SD, UT, VT. In every other race we examine, the exit poll did include education. The other variables were included much more sporadically. Marital and employment status were available for most 1994 polls (AZ, CA, CT, FL, MA, MD, ME, MI, MN, MO, NY, OH, TN, TX, VA, WI). Marital status was available for Georgia and Kentucky in 1998 as well. Union membership was available in these polls: in 1994, New York and Ohio; in 1996, Michigan; and in 1998, California, Illinois, Michigan, and Pennsylvania. For the 1994 Virginia race, a variable capturing veteran status was also available. It seemed to have theoretical merit given that Oliver North was the Republican candidate in that race (ultimately, the coefficient was correctly signed—i.e., veterans were more likely to support North—but statistically insignificant).

We operationalize variables like age and education as a series of dummies to capture any nonlinearities that might be present, and thus to maximize the predictive power of these imperfect equations. The categories for each variable are as follows: age: under 30, 30–44, 45–59, 60 and above; race: white, black, Latino, Asian, other; income: less than $15,000, $15–30,000, $30–50,000, $50–75,000, and $75,000 and above; education: no high school degree, high school degree, some college, college degree, advanced degree; gender: 1 for females and 0 for males; union membership: 1 if a union member and 0 otherwise; employment status: 1 if employed and 0 otherwise; and marital status: 1 if married and 0 otherwise.

References

Abramowitz, Alan I., and Jeffrey A. Segal. 1992. *Senate Elections*. Ann Arbor: University of Michigan Press.

Bartels, Larry M. 1998. "Where the Ducks Are: Voting Power in a Party System." In *Politicians and Party Politics*, ed. John Geer. Baltimore: Johns Hopkins University Press, 43–79.

Bennett, Stephen Earl, and David Resnick. 1990. "The Implications of Non-Voting for Democracy in the United States." *American Journal of Political Science* 34(3):771–802.

Berinsky, Adam J. 1999. "The Two Faces of Public Opinion." *American Journal of Political Science* 43(4):1209–30.

Berinsky, Adam J. 2002. "Silent Voices: Social Welfare Policy Opinions and Political Equality in America." *American Journal of Political Science* 46(2):276–87.

Berinsky, Adam J., Nancy Burns, and Michael W. Traugott. 2001. "Who Votes by Mail: A Dynamic Model of the Individual-Level Consequences of Voting-by-Mail Systems." *Public Opinion Quarterly* 65(2):178–97.

Brehm, John. 1993. *The Phantom Respondents: Opinion Surveys and Political Representation*. Ann Arbor: University of Michigan Press.

DeNardo, James. 1980. "Turnout and the Vote: The Joke's on the Democrats." *American Political Science Review* 74(2):406–20.

DeNardo, James. 1986. "Does Heavy Turnout Help Democrats in Presidential Elections?" *American Political Science Review* 80(4):1298–304.

Elkins, Zachary. 2000. "Quem Iria Votar? Conhecendo as Conseqüências do Voto Obrigatório no Brasil." *Opinão Pública* 6(1):111–37.

Erikson, Robert. 1995. "State Turnout and Presidential Voting: A Closer Look." *American Politics Quarterly* 23(4):387–96.

Gant, Michael M., and William Lyons. 1993. "Democratic Theory, Nonvoting, and Public Policy: The 1972–88 Presidential Elections." *American Politics Quarterly* 21(2):185–204.

Gerber, Alan S., and Donald P. Green. 2000. "The Effect of Canvassing, Telephone Calls, and Direct Mail on Voter Turnout: A Field Experiment." *American Political Science Review* 94(3):653–63.

Grofman, Bernard, Guillermo Owen, and Christian Collet. 1999. "Rethinking the Partisan Effects of Higher Turnout: So What's the Question?" *Public Choice* 99(3):357–76.

Highton, Benjamin. 2000. "Senate Elections in the United States, 1920–94." *British Journal of Political Science* 30(3):483–506.

Highton, Benjamin, and Raymond E. Wolfinger. 1998. "Estimating the Effects of the National Voter Registration Act of 1993." *Political Behavior* 20(2):79–104.

Highton, Benjamin, and Raymond E. Wolfinger. 2001. "The Political Implications of Higher Turnout." *British Journal of Political Science* 31(1):179–223.

Hill, Kim Quaile. 1994. *Democracy in the Fifty States*. Lincoln: University of Nebraska Press.

Jackman, Simon. 1999. "Noncompulsory Voting in Australia? What Surveys Can (and Can't) Tell Us." *Electoral Studies* 18(1):29–48.

Jackson, Robert A., Robert D. Brown, and Gerald C. Wright. 1998. "Registration, Turnout, and the Electoral Representativeness of U.S. State Electorates." *American Politics Quarterly* 26(3):259–87.

Knack, Stephen, and James White. 1998. "Did States' Motor Voter Programs Help the Democrats?" *American Politics Quarterly* 26(3):344–65.

Lazarsfeld, Paul F., Bernard Berelson, and Hazel Gaudet. 1948. *The People's Choice: How the Voter Makes Up His Mind in a Presidential Campaign*. New York: Columbia University Press.

Leighley, Jan E. 2001. *Strength in Numbers? The Political Mobilization of Racial and Ethnic Minorities*. Princeton: Princeton University Press.

Lijphart, Arend. 1997. "Unequal Participation: Democracy's Unresolved Dilemma." *American Political Science Review* 91(1):1–14.

Mayhew, David. 1974. "Congressional Elections: The Case of the Vanishing Marginals." *Polity* 6(2):295–317.

Nagel, Jack H., and John E. McNulty. 1996. "Partisan Effects of Voter Turnout in Senatorial and Gubernatorial Elections." *American Political Science Review* 90(4):780–93.

Nagel, Jack H., and John E. McNulty. 2000. "Partisan Effects of Voter Turnout in Presidential Elections." *American Politics Quarterly* 28(3):408–29.

Piven, Frances Fox, and Richard A. Cloward. 1988. *Why Americans Don't Vote*. New York: Pantheon Books.

Ragsdale, Lyn, and Jerrold G. Rusk. 1993. "Who Are Nonvoters? Profiles from the 1990 Senate Elections." *American Journal of Political Science* 37(3):721–46.

Rivers, Douglas. 1988. "Heterogeneity in Models of Electoral Choice." *American Journal of Political Science* 32(3):737–57.

Rosenstone, Steven J., and John Mark Hansen. 1993. *Mobilization, Participation, and Democracy in America*. New York: MacMillan.

Shaw, Daron, Rodolfo O. de la Garza, and Jongho Lee. 2000. "Examining Latino Turnout in 1996: A Three-State, Validated Survey Approach." *American Journal of Political Science* 44(2):332–40.

Sigelman, Lee, Philip W. Roeder, Malcom E. Jewell, and Michael A. Baer. 1985. "Voting and Nonvoting: A Multi-Election Perspective." *American Journal of Political Science* 29(4):749–65.

Sigelman, Lee, and Malcolm E. Jewell. 1986. "From Core to Periphery: A Note on the Imagery of Concentric Electorates." *Journal of Politics* 48(2):440–49.

Silver, Brian D., Barbara A. Anderson, and Paul R. Abramson. 1986. "Who Overreports Voting?" *American Political Science Review* 80(2):613–24.

Sniderman, Paul M, James M. Glaser, and Robert Griffin. 1990. "Information and Electoral Choice." In *Information and Democratic Processes*, ed. John A. Ferejohn and James H. Kuklinski. Chicago: University of Illinois Press, 117–35.

Southwell, Priscilla L. 1996. "Economic Salience and Differential Abstention in Presidential Elections." *American Politics Quarterly* 24(2):221–36.

Stein, Robert M. 1998. "Early Voting." *Public Opinion Quarterly* 62(1):57–69.

Stoker, Laura, and Jake Bowers. 2002. "Designing Multi-level Studies: Sampling Voters and Electoral Contexts." *Electoral Studies* 21(2):235–67.

Tucker, Harvey J., and Arnold Vedlitz. 1986. "Does Heavy Turnout Help Democrats in Presidential Elections?" *American Political Science Review* 87(4):1291–98.

Uggen, Christopher, and Jeff Manza. 2000. "The Political Consequences of Felon Disfranchisement Laws in the United States." Presented at the Annual Meeting of the American Sociological Association, Washington DC.

Verba, Sidney, Kay Lehman Schlozman, and Henry E. Brady. 1995. *Voice and Equality: Civic Voluntarism in American Politics*. Cambridge: Harvard University Press.

Wattenberg, Martin P., and Craig Leonard Brians. 2002. "Partisan Turnout Bias in Midterm Legislative Elections." *Legislative Studies Quarterly* 27(3):407–421.

Westlye, Mark Christopher. 1991. *Senate Elections and Campaign Intensity*. Baltimore: Johns Hopkins University Press.

Wolfinger, Raymond, and Stephen J. Rosenstone. 1980. *Who Votes?* New Haven: Yale University Press.