Are Voters Better Represented?

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Studies of political participation and representation often contend that elected officials respond more to the preferences of voters than those of nonvoters, but seldom test this claim. This is a critical assumption because if true, biases in who participates will lead to biased representation. Office holders might respond disproportionately to voters' preferences because voters tend to select like-minded representatives, voters tend to communicate their preferences more, and only voters can reelect representatives. We find that voter preferences predict the aggregate roll-call behavior of Senators while nonvoter preferences do not. We also present evidence supporting the three explanations advanced to account for the preferential treatment of voters.

Students of political participation and representation have long been concerned about the unrepresentative character of the U.S. electorate. Study after study has shown that as groups, voters and nonvoters differ in a variety of socioeconomic ways (e.g., Rosenstone and Hansen 1993; Verba and Nie 1972; Verba, Schlozman, and Brady 1995; Wolfinger and Rosenstone 1980). Many observers worry that since different socioeconomic groups generally hold different political opinions and support different political parties, elected officials disproportionately represent those who participate, leading to policies biased against nonvoters, i.e., minorities, the poor, and the less educated (e.g., Lijphart 1997). In short, they fear that skewed participation leads to skewed government action.[1] For some, the common claim that "unequal participation spells unequal influence" raises significant questions about political equality in the United States (Lijphart 1997, 1; Verba 2003), contributes to the gap between the haves and have-nots, and calls for substantial changes in electoral laws, such as motor voter legislation and even compulsory voting (e.g., Lijphart 1997; Piven and Cloward 1988).

[1] For example, Dahl argues "if a group is inactive . . . the normal American system does not necessarily provide it with a checkpoint anywhere in the process" (1956, 138). Burnham put it more bluntly: "if you don't vote, you don't count" (1987, 99). Piven and Cloward contend that socioeconomic bias in turnout is so politically powerful that "the distinctively harsh development of industrial capitalism in the United States during the twentieth century can be attributed partly to it" (1988, xi).

Although many *claim* that government policies respond more to citizens who participate in various aspects of the political process than to those who do not, relatively few have *tested* this assertion (Martin 2003). Fewer still examine whether the specific decisions of elected officials are more responsive to participants, leaving open the question of exactly how participatory inequalities lead to biased policies. Surely such a common claim, one that suggests deep biases in the nature of representation in American politics, deserves closer empirical scrutiny.

We examine whether elected officials actually do pay more heed to voters. Specifically, we test whether U.S. Senators' roll-call voting responds more to the views of voters than nonvoters. We find Senators to be consistently more responsive to voters when making roll-call decisions; in fact, as a group, Senators are not responsive to variation in nonvoters' preferences at all. We also uncover preliminary evidence that this is so because voters select like-minded Senators, voters are more likely to communicate their desires to their Senators, and only voters can reelect Senators.

Background

Although most discussions linking political participation and representation do not test the assumption that elected officials respond disproportionately to voters' preferences, they offer three reasons why officials might do so. First, voters' preferences may influence officials' actions *because voters select like-minded representatives* (e.g., Erikson 1990; Miller and Stokes 1963, 50), which we call the *selection hypothesis*. Since an office holder was elected, presumably a majority of the electorate holds views relatively close to the official, which may not be true for nonvoters. As Lijphart (1997, 4) argues, "who votes, and who doesn't has important consequences for who gets elected and for the content of public policies." Many studies have examined the first part of this hypothesis, testing whether turnout patterns advantage the Republican Party (e.g., Citrin, Schickler, and Sides 2003; DeNardo 1980; Highton and Wolfinger 2001; Nagel and McNulty 1996; Tucker, Vedlitz, and DeNardo 1986), but these studies seldom empirically connect electoral outcomes to the decisions elected officials ultimately make, leaving the second part of the hypothesis largely untested (but see Hill and Leighley 1992).

Second, voters might be advantaged *because they are more likely to communicate their opinions to policy makers* (e.g., Miller and Stokes 1963, 54–55). We call this the *communication hypothesis*. Elected officials will have a hard time representing the opinions of individuals who do not communicate their views. As Verba (2003, 663) put it, "political activity is the means by which citizens make their needs and preferences known to governing elites and induce them to be responsive," so "equal activity is crucial for equal consideration" (see also Verba, Schlozman, and Brady 1995, 163–64). Although voting itself does not convey much information about citizens' preferences, voters are more likely to engage

in other activities that convey more content (e.g., Verba and Nie 1972). Thus, officials may respond more to voters because they are more likely to communicate their preferences by other avenues.

Notably, according to these two hypotheses, voters can be better represented even if office holders do not know who votes or what voters' preferences are. If voters select relatively like-minded officials who simply vote their personal preferences, voters' preferences will be better represented. Similarly, if officials respond to the constituent voices they hear and those voices are drawn mostly from the ranks of voters, voters' preferences will be advantaged. Thus, voters can be better represented even if this is not what elected officials intend.

A third claim, which we label the *reelection hypothesis*, maintains that elected officials intentionally focus on voters' preferences (Bartels 1998; Fiorina 1974). In introducing his model of representatives' roll-call decisions, Fiorina (1974, 31) argues "we believe that constituents' preferences are reflected in a representative's voting (if at all) primarily through his concern for his electoral survival." Since representatives whose voting records do not match their constituents' preferences tend to suffer at the polls (e.g., Canes-Wrone, Brady, and Cogan 2002), reelection-minded representatives must cast their ballots with an eye toward their constituency. As Fiorina points out, when a district includes groups with divergent preferences, the reelection-oriented representative will choose to vote with the group that will most enhance his or her reelection prospects. Because only voters can change reelection chances, election-minded officials will focus on voter preferences when they make policy decisions. Of course, office holders do not want to arouse a previously inactive opposition, but as Bartels (1998, 45) reminds us, "vote-maximizing politicians must care more, other things being equal, about the views of regular voters than about the views of people who seldom or never get to the polls." Key (1949, 527) made the same claim, flatly stating, "the blunt truth is that politicians and officials are under no compulsion to pay much heed to classes and groups of citizens that do not vote."

Unlike the communication and selection hypotheses, the reelection hypothesis requires that elected officials know who voters are and what they desire in order to respond to them. To gauge voter opinion, they may rely on public opinion polls, especially those of "likely voters" or actual voters, as in exit polls. Prior studies indirectly support the reelection hypothesis, finding that officials are especially responsive to their reelection constituency (Bullock and Brady 1983; Fenno 1978). Although related, these studies do not speak directly to our question because they examine governmental responsiveness to subsets of the public rather than voters generally (however, we will pay special attention to Senators' responsiveness to their copartisans).

In sum, many studies point to reasons why officials might respond more to voters' preferences, *but few show this actually happens*. As Bartels put it, differences in turnout among various groups "are seldom explicitly related to any observed or potential impact they may have upon the strategic decisions of candidates or the policy outcomes produced by the electoral process" (1998, 45). To

be sure, some studies have uncovered connections between participation and representation. Verba and Nie (1972) show that local elected officials' and political leaders' policy priorities were more often consistent with the priorities of the politically active. However, they did not specifically compare leaders' responsiveness to voters versus nonvoters and also focused on leaders' attitudes rather than their behavior. An important line of research on race and representation implies that elected officials' decisions are less likely to reflect the preferences of groups of citizens that vote at lower rates, particularly African Americans (e.g., Tate 2003). As Guinier argues, "few disagree that blacks continue to be underrepresented in federal, state, and local government" (1994, 8). If African Americans, who vote at lower rates than whites, are less well represented, this implies that voters may be better represented overall. By examining whether Senators favor the politically active, we may contribute to this literature by providing empirical evidence of one mechanism by which minorities are underrepresented—they may be underrepresented because they vote less often. Although the race and representation studies offer suggestive evidence that voters are better represented than nonvoters, they do not examine the relative representation of voters and nonvoters per se, which is our present task.

Still other studies have linked turnout rates of various groups and governmental policy outputs, generally finding that policies favor groups that tend to vote. For instance, states with higher socioeconomic disparities between voters and nonvoters tend to adopt redistributive policies that benefit the wealthy (Hill and Leighley 1992); Southern localities provided better public services to blacks after the Voting Rights Act (Keech 1968); areas within congressional districts that vote at higher rates receive disproportionate outlays of federal discretionary moneys (Martin 2003); and senior citizens' political participation affects Social Security policies (Campbell 2003). Although these studies show a connection between voters' preferences and governmental policies, they tend to "black box" the mechanism by which voter preferences are translated into voter-friendly policies.[2] We uncover the box and examine this process by looking at the connection between voters' preferences and one aspect of the policymaking process, Senators' roll-call votes.

Data and Method

We model Senators' roll-call behavior as a function of voter opinion, nonvoter opinion, and Senator partisanship. We use state-level opinions as measured in the General Social Survey (GSS) from 1974 to 2002 to model Senators' roll-call behavior over the same period (93rd to 107th Congresses). If Senators respond

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[2] But see Bullock (1981), who showed that enfranchisement of Southern blacks moderated the roll-call conservatism of Southern legislators, and Campbell (2003), who explored whether legislators representing states with more politically active senior citizen populations were more likely to reverse their votes on the Medicare Catastrophic Coverage Act.

more to voter opinion, coefficients for voter opinion should exceed those for non-voter opinion. We also control for Senators' partisanship, which gives us a sense of how responsive Senators are to voters over and above the partisan electoral consequences of participatory inequalities. Beyond electing Republicans or Democrats, do voters' preferences have a greater effect on Senator roll-call decisions?

Dependent Variable

We measure the *aggregate* voting behavior of Senators using DW-NOMINATE coordinates, which range continuously from −1 to 1 (McCarty, Poole, and Rosenthal 1997).[3] Studies of congressional roll-call voting have frequently employed these and alternative versions of NOMINATE scores as dependent variables (e.g., Ansolabehere, Snyder, and Stewart 2001; Jenkins 1999). As a practical matter, NOMINATE coordinates are highly correlated with alternative, summary measures of legislator roll-call behavior such as interest group ratings and Heckman-Snyder scores (Burden, Caldeira, and Groseclose 2000).

Explanatory Variables and Controls

We model these scores as a function of voters' and nonvoters' general political ideologies and Senators' party affiliations, using an indicator for **Republican** affiliation. Finding measures of constituency opinion is a difficult task for studies of representation (see e.g., Achen 1978; Erikson 1978). These studies require reliable and valid measures of district- or state-level opinion, and such measures are hard to come by for two reasons now familiar to representation scholars. First, most national surveys sample far too few respondents in most states to render reliable measures of state opinion. Second, most national surveys are designed to draw samples representative of the nation, rather than states. As a result, estimates of state-level preferences drawn from national surveys are plagued with measurement error, attenuating estimated relationships between constituency opinion and legislator behavior (Achen 1978; Erikson 1978). The National Election Studies' 1988–92 Senate Election Study (SES) was designed to overcome these difficulties by providing relatively large samples drawn to be representative of states (the three waves provide average state samples of 185 respondents). Although these studies have proven a rich source for representation studies (e.g., Erikson 1990), the state samples fall short of the extra demands of our study. Reliability coefficients (Jones and Norrander 1996) suggest that these samples provide reasonably reliable estimates of state-level ideology for voters (r = .74),

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[3] Senators receive a DW-NOMINATE score for each Congress in which they served, but their *lifetime* roll-call record influences their score for each Congress. To capture Senators' lifetime mean roll-call behavior, we averaged each Senator's scores for all terms served. Modeling scores from each year separately generates similar results.

but not for nonvoters (r < .50).[4] Although analyses with the SES data generated results similar to those we report below, we searched for more reliable measures.

In the absence of single surveys that draw reliable state-level samples, one strategy is to pool samples over time. Erikson, Wright, and McIver (1993, hereafter EWM) adopted this approach, pooling *New York Times*/CBS surveys from 1976 to 1988 for measures of state ideology and partisanship These pooled samples provide reliable, stable, and valid measures of state ideology. Unfortunately, most of the surveys they used did not ask whether respondents voted, so we cannot use their data to test whether voters are better represented.

We adopted their approach, however, and pooled the GSS over time. The GSS provides estimates of state voter and nonvoter opinion in 44 states.[5] Pooling over time generates state-level samples averaging 829 respondents.[6] To measure **Voter Ideology** and **Nonvoter Ideology**, we took the mean ideological self-placement of a state's voters (those who reported having turned out in the most recent presidential election) and nonvoters (see the appendix for question wording). Although this approach dilutes the sample size in each state to an average of 539 voters and 290 nonvoters, the samples remain much larger than those used in previous studies of representation.[7]

The GSS measures prove reasonably reliable, stable, and valid. Although the GSS does not draw state-level samples, Brace et al. (2002) found that its state samples correspond with state population characteristics and produce highly reliable measures of state opinion. In fact, reliability coefficients for voter ideology and nonvoter ideology are .88 and .63, respectively. Since the estimated relationship between unreliable measures and the dependent variable will be attenuated and the measure of nonvoter ideology is less reliable, evidence that Senators respond more to voters may be the result of measurement error. To be sure measurement error is not driving our results, we give nonvoter ideology an advantage, explicitly accounting for its error wherever possible in our models, but assuming that voter ideology is perfectly measured.[8] If voter ideology continues to be more closely related to Senators' voting behavior, we can be reasonably sure that Senators really are more responsive to voters.

Pooling over time raises the issue of stability, which EWM explored. Following their example, we split the studies into earlier and later halves, and found that the state ideology measure correlated at .64 across the two halves.[9] Although

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[4] The coefficient ranges from 0 to 1. Jones and Norrander (1996, 302) classify values over .7 as "highly reliable" and between .6 and .7 as "moderately reliable."

[5] The GSS does not sample from Hawaii, Idaho, Maine, Nebraska, Nevada, and New Mexico.

[6] These samples range from 79 (AK) to 3,627 (CA).

[7] SES state samples averaged 185. Miller and Stokes' (1963) data averaged 13 per district.

[8] One potential reason for the lower reliability of nonvoter opinion is that surveys may undersurvey nonvoters, deflating the sample size of nonvoters, which depresses the reliability coefficient.

[9] We split the surveys into 1973–88 and 1989–2002 halves. The latter half is a bit shorter, but because the later sample sizes were a bit larger, the two halves have roughly the same number of respondents.

the measures were strongly related, this correlation is a good bit below EWM's reported .86 correlation for their ideology measure and points to some change over time. EWM argue that we can assess the amount of real change over time as opposed to change due to sampling variation by dividing the observed correlation by the square root of the product of the reliabilities from each period. This adjusted correlation for the GSS measure is a more respectable .79.

Even at this relatively high correlation, it is clear that some real change occurred over this period. As EWM point out, this presents a tradeoff between consistency and stability. Since the SES provides more stable but less reliable measures, we opted in favor of consistency, as did EWM when faced with the same tradeoff in their measure of state partisanship.[10] In the end, this decision may not have been consequential, since our results using GSS and SES measures are quite similar. Furthermore, we estimated our models with both halves of the data separately and found results similar to those with fully pooled measures, and some of the analyses we report below pooled over much shorter periods, which enhances the stability of the data.

Finally, the GSS ideology measure appears quite valid. Our state ideologies correlate with each state's Republican vote margin in the 2000 presidential race at .74. Most tellingly, our state ideology measure correlates with EWM's at .81. In sum, the GSS provides reasonably reliable, stable, and valid measures. We recognize, of course, that the measures are imperfect and so throughout our analyses we take steps to assess the implications of these imperfections and to account for them in the estimates as much as possible.

One potential confounding factor in our analysis is the tendency of survey respondents to overreport voting. Some who did not actually vote say they voted ("false voters"). This may not affect our results much since the error in independent variables induced by overreporting only leads to minor biases in models of turnout and candidate choice (e.g., Cassel and Sigelman 2001; Sigelman 1982). However, to the extent that false voters differed from actual voters, including them in the measure of voter ideology introduces error in the variable, attenuating the estimate of Senators' responsiveness, biasing our analysis toward null findings. More importantly, if false voters were ideologically similar to actual voters, evidence that voters were better represented may be misleading. If false voters were similar to voters, the estimated relationship between voter opinion and Senators' roll-call voting would be unaffected, because the mean opinion for a state's voters would be relatively unchanged if the false voters were removed. However, the estimated relationship between nonvoters and Senator behavior would be attenuated, with the actual relationship more like that between voters and Senators. Evidence that false voters tend to be highly educated and gener-

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[10] EWM's partisanship measure had a reliability of .81 (.86 purged of sampling error), and just .48 for the 11 states with the largest sample size. They chose to "break the tie" in favor of consistency (1993, 39).

ally most likely to vote suggests that false voters may indeed be similar to voters (Silver, Anderson, and Abramson 1986).

Unfortunately, we cannot determine whether false voters in the GSS were similar to actual voters because the GSS does not validate turnout. However, since the National Election Studies (NES) validated turnout from 1976 to 1980 and from 1984 to 1990, we can examine false voters' ideological tendencies in other contexts. These seven studies show that false voters are generally not ideological carbon copies of actual voters. They are sometimes similar to voters, but are often appreciably different (see the appendix). In some instances, false voters were more like nonvoters than voters. Since these studies give no conclusive reason to expect false voters to be ideological clones of actual voters, the likely consequence of overreporting is added noise in estimates of voter and nonvoter ideology, attenuating their relationship with Senators' voting. At worst, if false voters really were similar to voters in our data, since they tend to be most likely to vote (Silver, Anderson, and Abramson 1986), our results would suggest that Senators disproportionately respond to *likely* voters. This adjusted inference continues to point to considerable biases in representation to the extent that the group of actual and likely voters differs from nonvoters. Thus, at worst, our results sustain the significant implications we outline in our conclusion.

Results

We proceed in three steps. First, we measure the differences between voter and nonvoter ideology by state. We then investigate the responsiveness of Senators' roll-call decisions to voter and nonvoter ideology. Finally, we examine the common explanations offered to account for officials' greater responsiveness to voter preferences.

Voter and Nonvoter Preferences

If the preferences of voters and nonvoters do not differ, Senators cannot respond differentially to voters and nonvoters. Prior studies generally find relatively small differences in preferences (e.g., Verba, Schlozman, and Brady 1995; Wolfinger and Rosenstone 1980), but some find more substantial differences between voters' and nonvoters' party preferences, demographic characteristics, and policy preferences (Bennett and Resnick 1990; Citrin, Schickler, and Sides 2003; Verba 2003; Verba and Nie 1972). Most of these studies present national-level comparisons, but since we are interested in comparing voters and nonvoters in states, we follow Citrin, Schickler, and Sides (2003) and disaggregate voter and nonvoter ideology to the state level.

Figure 1 compares voter and nonvoter ideology by state, graphing mean ideological differences between voters and nonvoters. Ideology is scaled so that conservative positions are higher, so positive values result from voters being more conservative than nonvoters. Moving from the national to state level reveals many

FIGURE 1

Ideological Bias in State Electorates (1973–2002)
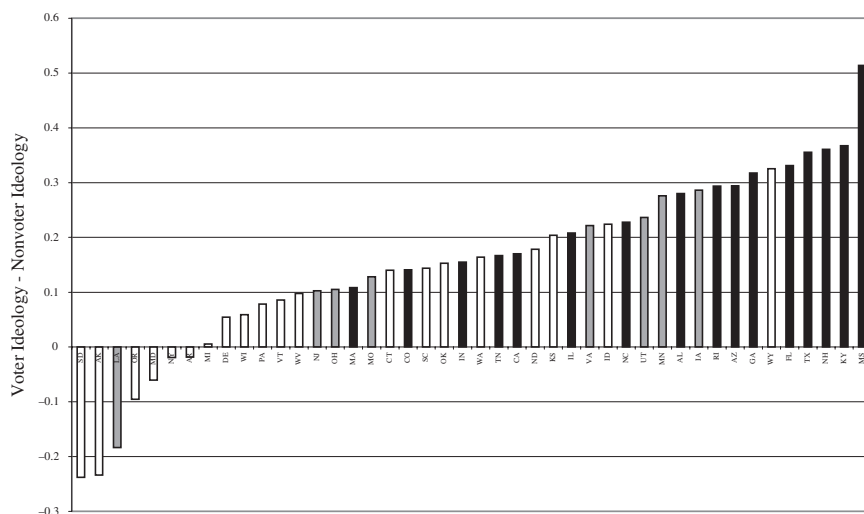
Columns reflect the mean ideology of voters minus the mean ideology of nonvoters. Positive values represent conservative bias, negative values liberal bias. Based on difference of means tests, black columns denote $p < .05$, gray columns $p < .10$.

important differences. Voters and nonvoters significantly differ in 24 of 44 states ($p < .10$), with voters almost always more conservative.[11] In many states there is a quarter-point difference between mean voter and nonvoter ideology on the 7-point scale. To put this in perspective, the difference between the ideological average of all Massachusetts (3.92) and Florida (4.19) residents in the sample is only slightly larger.

This state-level variation begs for explanation. Citrin, Schickler, and Sides (2003) suggest that it might be explained by a state's political history, candidate quality in each state's races, and mobilization effects specific to each election. Because our data are pooled across elections, Figure 1 suggests that although election-specific mobilization is a likely cause of the direction and extent of ideological bias in the electorate, historical and environmental factors may induce a constant bias. For instance, in eight of the 11 states of the former Confederacy voters are more conservative than nonvoters. However, this is not our principal

[11] As some have emphasized, perhaps the opinions of nonvoters would change if they became voters (e.g., Lijphart 1997). However, the argument usually runs that nonvoters would become more liberal if they voted, which would make them even more different from voters than we observe.

TABLE 1

Effect of Voter and Nonvoter Ideology on Senator Roll-Call Voting

	(1)	(2)
Voter Ideology	.69*	—
	[.09]	
Nonvoter Ideology	−.08	—
	[.09]	
Voter Ideology—Nonvoter Ideology	—	.47*
		[.09]
Republican	.64*	.65*
	[.02]	[.03]
Constant	−2.9*	−.42*
	[.31]	[.02]
R^2	.78	.72
N	248	248

Cell entries are OLS estimates. Bracketed entries are standard errors.
* denotes $p < .05$.

question, so we leave the task of explaining ideological bias in state electorates to future research. The important point is that voter and nonvoter preferences differ in a majority of states, usually in a conservative direction.

Voters and Roll Calls

The heart of our analysis assesses whether Senators respond more to the preferences of voters than nonvoters when casting roll-call votes. We first estimated the effects of state voter and nonvoter ideology on the average first-dimension DW-NOMINATE score of the 248 Senators who served at any point during the 93rd to 107th Congresses, controlling for Senators' party affiliation. Variables are coded with conservative values higher, so positive coefficients indicate positive responsiveness. Because state ideology is the same for all Senators who represent(ed) the same state, we experimented with clustering the estimations by state, but found this not to be consequential for the results.

As Table 1 shows, the voter ideology parameter estimate is positive and significant, while the nonvoter ideology parameter estimate is essentially zero. We also regressed Senator DW-NOMINATE scores on the difference between state voter and nonvoter ideology, or the extent to which voters are more conservative than nonvoters (as represented in Figure 1), which purges the model of any collinearity between voter and nonvoter ideology ($r = .65$). We find that Senators were responsive to the degree of ideological bias in the electorate (column 2); in states where voters are more conservative than nonvoters, Senators tend to be more conservative. Not surprisingly, accounting for the lower reliability of nonvoter ideology by using an errors-in-variables estimator to reanalyze the model

in column 1 merely exacerbates nonvoter ideology's inverse relationship with Senator voting behavior (the parameter estimate was −.21 with a standard error of .26). Based on these results, we conclude that our results are not simply due to better measurement of voter ideology.[12]

We turn now to the magnitude of these effects.[13] We simulated the effect of shifting each of the variables in Table 1, column 1 from their 25th to 75th percentile values on Senators' DW-NOMINATE scores, controlling for the mean effect of each of the remaining factors.[14] These simulations indicate that such a change in voter ideology shifts Senators' DW-NOMINATE scores about .18 points. This is much less than the effect of being a Republican, which is estimated to shift Senators' scores by about .65 on the 2-point scale. If constituents want a more liberal Senator, their best strategy is to elect a Democrat. This is hardly surprising. However, even beyond the dramatic effect of party, voter ideology substantially affects the ways Senators cast roll-call votes. As a concrete example, in the 103rd Senate, such a shift in the ideology of Connecticut voters would change Joseph Lieberman's (D-CT) DW-NOMINATE score to a position more conservative than that of 15 additional Democrats, including Sam Nunn (D-GA), and in Massachusetts would have caused Edward Kennedy (D-MA) to vote more conservatively than Bill Bradley (D-NJ). Certainly Senators' party affiliations matter more, but voter preferences have an additional, substantial influence on Senators' votes.

These results suggest that Senators are disproportionately responsive to voters. However, extant research points to two alternative explanations for these results. First, several studies show that the actions of elected officials can affect the attitudes of citizens, especially on salient issues that divide the major parties (e.g., Hill and Hurley 1999; Hurley and Hill 2003). If constituent attitudes respond to Senators' voting behavior, our findings may merely indicate that voters pay more

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[12] When including voter or nonvoter ideology separately in models and controlling for Senator party, both voter and nonvoter ideology predict DW-NOMINATE scores, but voter ideology's coefficient was about half again as large as the nonvoter coefficient. Even when we corrected for unreliability in the nonvoter ideology measure using errors-in-variables regression, the voter ideology coefficient was larger than that for nonvoter ideology. As an additional robustness check, we replicated the analyses in columns 1 and 2 using measures of voter and nonvoter ideology from the SES to model roll-call behavior in the Congress immediately following each survey wave (1988 data for the 101st Senate, 1990 for the 102nd, and 1992 for the 103rd). Again, voter ideology predicts roll-call behavior, but nonvoter ideology does not, and Senators responded to their state electorate's ideological bias in two of the three Congresses (see additional analyses). In addition, we examined the connection between Senators' votes on specific measures and (non)voters' opinions in that policy domain by analyzing 24 roll-call votes from the 104th to 108th Congresses the National Right to Life Committee designated as "Key Votes" on abortion and four additional votes from the 101st to 103rd Congresses that *Congressional Quarterly* designated as "Key Votes" relating to abortion. Senators' votes responded to voters' abortion opinions in 11 of the 28 votes, but never responded to nonvoter opinion (see additional analyses).

[13] Marginal effects were simulated using CLARIFY (Tomz, Wittenberg, and King 2003).

[14] The 25th percentile value of voter ideology is 4.07, while its 75th percentile value is 4.33. The 25th percentile value of nonvoter ideology is 3.91, while its 75th percentile value is 4.13.

attention to their Senators' activities and are persuaded to change their ideological perspective to match that of their Senators to a greater degree than are nonvoters. While we have not located a study that shows citizens' *ideologies* (as opposed to their more specific issue positions) are affected by elite behavior, to be certain that constituent ideology precedes Senators' votes, in unreported analyses we used pre-1990 GSS data to model the average DW-NOMINATE scores of the 142 Senators who served from 1990 to 2002 (see additional analyses). As an added advantage, this model examines the effect of aggregating the GSS over a shorter period of time. In this reduced sample, the voter ideology parameter estimate still greatly exceeded the nonvoter ideology estimate.

We further attempted to exclude the possibility that our results capture the effect of Senators' votes on citizens' preferences by focusing on Senators' first-term voting. We used constituent opinion measured before Senators cast a single roll-call vote to model votes from Senators' first two years in the chamber. This design purges the model of reciprocal effects since Senators' first-term votes cannot affect their constituents' ideologies prior to their election. Specifically, we modeled the first-term DW-NOMINATE scores of the 91 Senators who served a first term between 1986 and 2000, using GSS state voter and nonvoter ideology measures pooled only to the year each Senator was elected.[15] The estimates show that first-term Senators' roll-call decisions better reflect the preferences of voters than nonvoters. Thus, we can be confident that the reciprocal influence from Senators to constituency opinion is not driving our results.

A second, alternative interpretation of the results in Table 1 is that they reflect Senators' attentiveness to their copartisan constituents, who may be more likely to be mobilized in elections (e.g., Bullock and Brady 1983; Powell 1982). Since members of Congress are especially responsive to the copartisans in their state or district (e.g., Bullock and Brady 1983; Fiorina 1974; Hurley and Hill 2003) and the group of voters may be comprised disproportionately of copartisans, our results may just reflect Senators' disproportionate responsiveness to copartisans rather than to voters per se. We test this possibility two ways, first asking whether copartisan voters are advantaged over copartisan nonvoters. In unreported analyses, we found that even among each Senator's copartisans, voters are better represented (see additional analyses). In fact, like nonvoters at large, Senators do not respond to their nonvoting constituents at all, even when they identify with the Senators' party. Second, we modeled copartisan and voter ideology together to see whether what appears to be responsiveness to voters is really just responsiveness to copartisans. Although copartisan ideology is strongly related to

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[15] Due to the "overlapping generations" nature of the DW-NOMINATE procedure, a roll-call score in a single congressional term is influenced by roll-call behavior in subsequent terms. In this way, there is a potential that our first-term roll-call measures are "contaminated" by later voting behavior. If Senators are more responsive to recent public opinion than to past opinion, contamination by later roll-call behavior will tend to dilute Senators responsiveness in earlier periods, introducing error into our measure of first-term roll-call decisions and thus inefficiency into our models of these decisions.

Senator voting scores, voter ideology remains significantly related to Senator voting scores as well. Again, we can be fairly sure our results are not spurious.[16]

Testing Explanations for Voter Overrepresentation

The results thus far indicate that voters are better represented than nonvoters. This finding on its own provides novel support for the broadly held but generally unsubstantiated claim that government responds disproportionately to the politically active. It also holds important normative implications, which we discuss below. As important as the finding is on its own, ultimately we want to know *why* voters are advantaged. In this section, we examine the three reasons offered to explain why voter preferences are advantaged as just demonstrated.

We begin with the selection hypothesis, which argues that Senators' ideologies mediate the effect of voter ideology on Senators' roll-call voting. That is, voters elect Senators who share their policy views, so when Senators vote their own preferences, they represent their voters' preferences. Testing this claim requires a measure of **Senator Ideology**. Since these measures are notoriously susceptible to various forms of endogeneity, we rely on five different Senator ideology measures brought together by Burden, Caldeira, and Groseclose (2000). These measures attempt to untangle Senators' personal ideology from their roll-call votes in different ways, so common results across the measures will indicate some real mediating effects, not just endogenous relationships. The measures include mean citizen placements of their Senators on a 7-point ideology scale (SES 1988–92); Senator self-placements obtained in a 1982 CBS/*New York Times* survey; modified ADA scores produced by Levitt (1996) which "are designed to be purged of non-ideological influences" (Burden, Caldeira, and Groseclose 2000, 240); *Roll Call*'s 1993 "Completely Unofficial Ideological Spectrum of the U.S. Senate;" and a campaign-based measure designed by Hill, Hanna, and Shafqat (1997). Since each measure was compiled at a different point in time, one or more of the measures may be unavailable for some Senators in the Congresses we analyze.

If Senators' ideologies mediate the relationship between voter ideology and Senators' roll-call decisions, three conditions must hold: first, voter ideology must be related to Senator ideology; second, Senator ideology must be related to Senators' roll-call votes; third, when including Senator ideology in the model of Senators' votes, the effect of voter ideology must decrease (e.g., Baron and Kenney 1986). We first test whether voter ideology predicts Senators' ideology, controlling for nonvoter ideology and Senators' party affiliations. We continue to

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[16] A reviewer suggested that we estimate the models in Table 1 including various controls for demographic characteristics that correlate with turnout (race, income, and age). It may be that Senators overrepresent whites, older citizens, and constituents with higher incomes, which would mean that Senators will also overrepresent voters. However, even with these controls, voters remained significantly better represented (see additional analyses at http://www.journalofpolitics.org).

TABLE 2

Voter Ideology, Senator Ideology, and Senator Roll-Call Voting

	SES	CBS/NYT	Levitt	Roll Call	HHS
Voter Ideology	.41*	.34*	.14	.29*	.13	.38*
	[.12]	[.13]	[.13]	[.14]	[.12]	[.11]
Nonvoter Ideology	.08	−.09	−.1	−.06	−.06	−.18
	[.12]	[.13]	[.12]	[.10]	[.10]	[.10]
Republican	1.14*	.77*	.67*	.67*	.51*	.72*
	[.04]	[.07]	[.06]	[.04]	[.06]	[.05]
Senator Ideology	—	.30*	.08*	.07*	.15*	.05*
		[.06]	[.01]	[.01]	[.01]	[.01]
Constant	−2.67*	−2.88*	−1.07*	−1.19*	−1.30*	−1.38*
	[.41]	[.43]	[.41]	[.55]	[.36]	[.37]
R^2	.88	.89	.92	.97	.95	.93
N	148	82	57	50	61	80

Cell entries are OLS estimates. Bracketed entries are standard errors. *denotes $p < .05$.
Dependent variable is Senators' DW-NOMINATE scores.

measure voter and nonvoter opinion using GSS data, averaged over the period preceding each Congress we analyze. Specifically, we averaged the DW-NOMI-NATE scores of all Senators who served in the 101st to the 107th Senates and estimated five models, one for each measure of Senator ideology. In four of the five cases, voter ideology predicted Senator ideology ($p < .05$), but nonvoter ideology never predicted Senator ideology (see additional analyses at http://www.journalofpolitics.org).

Next, using the same group of Senators, we estimated six models of Senator DW-NOMINATE scores. The first column in Table 2 reports a baseline model of Senators' DW-NOMINATE scores regressed on state voter ideology, nonvoter ideology, and Senators' own party affiliations. The next five columns present results controlling additionally for each of the five measures of Senator ideology. The results indicate that the choice of relatively like-minded Senators is one of the important avenues by which voter ideology affects Senator roll-call voting. As expected, every measure of Senator ideology is significantly related to Senators' voting behavior. In addition, using four of the five measures, the voter ideology parameter estimate declines rather substantially, in two cases to statistical insignificance. Although electing like-minded Senators may not completely account for the relationship between voter ideology and Senator roll-call behavior, it seems to account for some of it.

Our evaluation of the communication hypothesis follows similar logic. Since voters are more likely than nonvoters to participate in politics in other ways, we expect the preferences of "communicators" to reflect those of voters more than nonvoters.[17] We also expect these communicators' views to be especially well rep-

[17] The correlation of voter ideology and communicator ideology is .59.

TABLE 3

Voters, "Communicators," and Senator Roll-Call Voting, 101st–103rd Congresses

Dependent Variable	Communicator Ideology	DW-NOMINATE	DW-NOMINATE	DW-NOMINATE (Errors-in-Variables)
Voter Ideology	.62*	.44*	.30*	.16
	[.18]	[.12]	[.12]	[.13]
Nonvoter Ideology	−.19	−.02	.02	.06
	[.19]	[.12]	[.12]	[.11]
Republican	—	.97*	.98*	.98*
		[.04]	[.04]	[.04]
Communicator Ideology	—	—	.22*	.43*
			[.06]	[.12]
Constant	−.53*	−2.36*	−2.91*	−3.42*
	[.63]	[.43]	[.43]	[.47]
R^2	.13	.85	.87	.89
N	102	102	102	102

Cell entries are OLS estimates unless noted. Bracketed entries are standard errors. * denotes $p < .05$.

resented, for reasons provided above. It follows from the hypothesis that incorporating communicators' ideologies into the model should diminish the estimated effect of voter ideology and state voter ideology should predict state communicator ideology.[18] We use the SES only to measure **Communicator Ideology**, the mean ideology of respondents who had some contact with a Senator in their state, either by personally meeting a Senator, contacting his or her office, or talking with a member of the Senator's staff.[19] As we would expect given the small sample size, the communicator ideology measure has fairly low reliability ($r = .58$), which we will take into account in the estimation. To best match the SES time period, we use ideology measures from before the 101st Congress to model Senators' mean DW-NOMINATE scores from the 101st to 103rd Congresses.

Table 3 shows considerable support for the communication hypothesis. First, as seen in column 1, voter ideology is closely linked to communicator ideology,

[18] Unfortunately, we cannot evaluate whether Senators respond more to voters' ideologies because voting *itself* communicates information concerning voters' preferences.

[19] About 85% of these respondents reported voting (85, 82, and 88% in 1988, 1990, and 1992, respectively). To boost sample sizes, we included respondents who contacted either of their Senators in the communicator ideology measure for both of the state's Senators. It may be that Senators share information about the opinions they are hearing back home (Schiller 2000).

TABLE 4

Moderating Effect of Senator Reelection Status, 101st–107th Senates

	Stood for Reelection	Not Up/Retired
Voter Ideology	.69*	.59*
	[.13]	[.13]
Nonvoter Ideology	.11	−.01
	[.15]	[.14]
Republican	1.03*	1.15*
	[.04]	[.04]
Constant	−3.95*	−3.10*
	[.76]	[.59]
R^2	.88	.88
N	174	450

Cell entries are OLS estimates. Bracketed entries are robust standard errors. * denotes $p < .05$.

as expected. Second, communicator ideology is strongly related to Senators' roll-call votes, as seen in columns 3 and 4. Of course, when controlling for error in the communicator ideology measure (column 4), its parameter estimate increases quite a bit, but even without this correction, communicator ideology is significantly related to Senators' votes. Third, compared to our baseline model that does not control for communicator ideology (column 2), the effect of voter ideology declines substantially when we account for communicator ideology. In fact, when we account for error in the communicator ideology variable, the voter ideology parameter is about a third its original size and its p value rises to .21. We conclude from this that another reason voters are better represented is their tendency to communicate their views to public officials.

Finally, we adopt a different approach to evaluate the reelection hypothesis, which maintains that elected officials are disproportionately responsive to voters because only voters can reelect them. If Senators respond to voters' opinions due to reelection concerns, Senators up for reelection at the end of a Congress should be especially responsive to voter opinion when they cast roll calls (e.g., Levitt 1996). For each of the 101st to 107th Congresses, we identified whether each Senator was scheduled to stand for reelection or whether their term was not complete or they retired. We then estimated two models encompassing all seven Congresses—one for Senators who stood for reelection at the end of a Congress and one for those who did not. Because Senators may be observed multiple times in the data, we clustered the estimations on the individual Senator. The results of these models indicate that voters are somewhat better represented during terms when their Senator plans to stand for reelection. The voter ideology coefficient for Senators standing for reelection is almost 20% larger than the estimate for Senators not standing for reelection. Although it would be a mistake to make too

much of these results, given that the 95% confidence intervals of the voter ideology estimates overlap, reelection concerns seem to increase Senators' responsiveness to voters.[20]

Conclusion

These results provide empirical support for the common claim that government represents voters more than nonvoters. To recapitulate, we found substantial evidence that even though Senators may not know with certainty who votes and what their preferences are, their patterns of roll-call voting respond to voters' opinions, but not to nonvoters' opinions. We also showed that Senators' better representation of voters is not simply a reflection of voters' greater attention to Senators' decisions or their identification with their Senator's political party. Finally, we uncovered suggestive evidence that this link between voters and Senators results from voters' selection of relatively like-minded representatives, their greater rates of communicating preferences via other forms of participation, and Senators' desire for reelection. Thus, Senators may be more responsive to voters' preferences both because they purposely focus on voters and due to indirect influences operating even when Senators do not know who voters are and what they want. However, we do not claim we have entirely explained this phenomenon. More research is required to specify the precise mechanisms by which voters' preferences become advantaged in governmental decision making.[21]

Although our analyses cannot fully explain the processes by which voters are better represented, they point strongly toward the conclusion that Senators do in fact respond more to voters' preferences than nonvoters'. This finding has important implications for our understanding of American politics and raises significant normative issues. On the explanatory side, our study points to three underappreciated features of the participation-representation link in American politics. First, these analyses advance existing evidence that government rewards those who vote (e.g., Bullock 1981; Hill and Leighley 1992; Keech 1968; Martin 2003). As many have assumed, those segments of the public that do not vote appear, as a group, to have little influence on legislators' roll-call voting, opening the path to biases in legislator behavior and ultimately public policy. Where previous studies generally analyze policy outcomes, our focus on roll-call voting explores another aspect of representation, looking at an important feature of the policymaking process. Our examination of legislators' behavior and three possible links between participation and representation moves us a step closer to com-

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[20] Since our main interest here is comparing the voter ideology coefficient for Senators seeking reelection to that for those not seeking reelection, nonvoter ideology is less important analytically. In fact, when controlling for measurement error in the nonvoter ideology measure, the voter ideology measure continues to be about 20% larger for Senators running for reelection.

[21] For example, Senators may be more responsive to the preferences of voters because within states voters' opinions are more homogeneous (e.g., McCrone and Kuklinski 1979).

prehending the paths from political participation to government action. Understanding those paths ultimately may point to ways of encouraging more equal representation.

Next, we found that representational biases cut in a conservative direction. Voters are more conservative than nonvoters, and voters are better represented by elected officials. Contrary to the conclusions of national-level analyses (e.g., Bennett and Resnick 1990; Highton and Wolfinger 2001), our results based on state-level differences between voters and nonvoters suggest that increases in turnout may lead to greater policy liberalism.

Third, much empirical study of inequalities in turnout focuses on whether these inequalities benefit the Republican Party. Our analyses point to this question's significance by showing the dramatic difference party affiliation makes for roll-call behavior, but we move past this question. The electorate's ideological biases affect Senators' votes even beyond their partisan effects. Whether or not a conservatively biased electorate can help elect a Republican, it seems to move Senators' roll-call votes in a conservative direction. Thus, to comprehend the impact of turnout inequalities, we must not focus solely on partisan electoral effects.

Finally, these results relate to the vigorous debate about whether voting is rational (e.g., Aldrich 1993). Without taking a side in this debate, we simply note that finding representational rewards for voting provides a benefit of voting the debate has tended to overlook. While an individual voter may have only a miniscule impact on who is elected, if elected officials monitor the opinions principally of voters, then voting has inherent representational rewards.

On the normative side, these results are an invitation to consider a host of questions about whether elected officials' focus on voters is a cause for concern. Simply put, we find that Senators do not respond to all their constituents equally. Ought elected officials strive to represent *all* their constituents (Verba 2003)? This question is related to deeply contested issues of descriptive representation and the winner-take-all electoral system—issues of whether a white male can or should even try to represent the African-American women in his district or whether a victorious Democrat is obligated or able to represent non-Democratic constituents (e.g., Mansbridge 1999). To what extent can officials represent nonvoters? To what extent should they? Some might argue that demands for equal representation require at least some activity in the political realm (recall the civics class dictum that "if you don't vote, you can't complain"). Others may claim that since voters are better informed (e.g., Bennett and Resnick 1990), their views should be overrepresented. Still others might applaud an official who represents those that for whatever reason do not or cannot vote. We propose no answer to these questions, but our results point to the necessity of wrestling with them, suggesting as they do that "[o]nly the rare politician would pass up the blandishments of the active to champion the cause of those who never take part" (Rosenstone and Hansen 1993, 247).

If indeed we expect officials to respond more to the politically nonactive, can legislators' incentives be altered to encourage this? Doing so may prove difficult,

since the effects of biased turnout seem in part indirect. Further, if somehow officials responded equally to voters and nonvoters, this might actually reduce incentives to vote. Seen in this light, current inequalities in representation may provide an incentive to participate in politics.

A final implication of our analysis stems from the knowledge that voters tend to enjoy higher incomes and levels of educational attainment than nonvoters and whites tend to vote at higher rates than do minority groups. Given this, our results imply that whites and those with the highest incomes and levels of education are best represented. Future work should continue to grapple with the difficult questions of whether the representational disparities we unearth are ultimately due to socioeconomic differences, differences in political activity, or the characteristics of representatives themselves. It seems to us, however, that political participation must play an important role in the overall story of disparities in representation. If poorer Americans or minority groups are underrepresented, it makes sense that their lower rates of participation may be an important causal factor in their lesser ability to garner the benefits of representation.

Students of democratic politics have long studied the extent of public influence on elected officials. Our study joins previous analyses in finding that government often responds to public opinion (e.g., Miller and Stokes 1963; Page and Shapiro 1983; Stimson, MacKuen, and Erikson 1995). However, this responsiveness is much greater for those who pull the lever.

Appendix

General Social Survey Question Wording

Ideology: We hear a lot of talk these days about liberals and conservatives. I'm going to show you a 7-point scale on which the political views that people might hold are arranged from extremely liberal—point 1—to extremely conservative—point 7. Where would you place yourself on this scale?

Ideological Comparison of Validated Voters, False Reporters, and Nonvoters

Figure A1 presents the mean ideology of validated voters, false voters, and nonvoters from seven NES. The figure reveals no obvious pattern of relationship between false voters and validated voters. In some years, they are similar, but in many they are quite different. Thus, there is no reason to expect false voters to mirror actual voters, but they might. We acknowledge that our results may point to the conclusion that Senators respond to likely rather than actual voters.

FIGURE A1

Ideology of Voters, False Voters, and Nonvoters
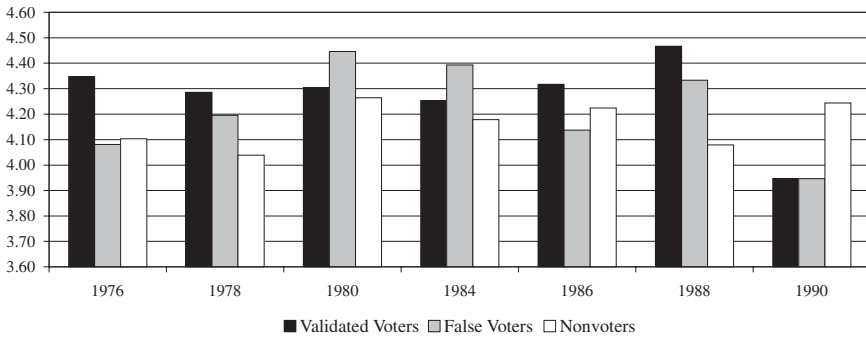
Validated Voters ■   False Voters ▨   Nonvoters □

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