If Everyone Had Voted, Would Bubba and Dubya Have Won?

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Adapting and refining the approach used in earlier work on Senate elections, we simulate the impact of universal turnout on each presidential election from 1992 to 2004 and find little evidence that increased turnout would systematically transform partisan competition or policy outcomes. A state-level analysis of exit polls and the Current Population Survey reveals considerable variability in the gap separating voters and nonvoters. In most cases, nonvoters are just slightly more Democratic than voters. However, a handful of states, such as Texas, consistently feature a large "partisan differential," in which nonvoters come disproportionately from demographic groups that are more Democratic than voters. We find that universal turnout may well have tipped an extremely close election—such as that of 2000 or even 2004—into the Democratic column. But the partisan differential is generally small enough that universal turnout would only change the outcome of an already close contest rather than leading to a wholesale transformation of competitive dynamics.

Raymond Wolfinger's seminal research has established with elegance and precision the demographic and institutional bases of voter turnout in the United States. With these results in hand, Wolfinger turned to the significant "so what?" question, probing the implications of higher levels of participation for electoral outcomes and subsequent

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policy making. Wolfinger and Rosenstone (1980) and Highton and Wolfinger (2001) provocatively conclude that outcomes in recent American presidential elections would not have changed if everyone had voted and that, as a whole, the preferences of nonvoters are well represented by the opinions of voters (Highton and Wolfinger 2001, 179, 192).

This conclusion challenges much conventional wisdom. Because the poor and ethnic minorities are less likely to vote and because the Democrats have been the favored party among these groups, there is a pervasive belief that higher levels of turnout would favor the Democrats. Democratic politicians, prodded by their newly mobilized constituents, would then adopt bold policies that would reduce economic inequality (Lijphart 1997). For leftist dreamers, compulsory voting would mean a permanent Democratic majority that ultimately could bring social and economic democracy to America. But leaving aside such a utopian scenario, at a minimum there is evidence that the preferences of representatives better correspond to those of voters than nonvoters (Griffin and Newman 2005) and that states with greater lower-class turnout have more generous welfare policies (Hill, Leighley, and Hinton-Anderson 1995), suggesting that less than full turnout does have consequences for the quality of representation and the content of public policy in the United States.

Studying the implications of higher turnout entails a "what if" analysis that necessarily involves assumptions about how nonvoters would behave. Wolfinger and Rosenstone's skeptical take on the implications of higher turnout rests on survey research regarding the gap between the opinions of nonvoters and voters. Highton and Wolfinger replicate the analysis and extend it to respond to the argument that with universal turnout, the content of electoral campaigns would change to engage the hitherto neglected priorities of nonvoters. They show that despite the class differences between voters and nonvoters, the "grievances and aspirations" of the two groups are very similar and that the poorer, more heavily minority nonvoting group is, if anything, less class conscious (Highton and Wolfinger 2001, 187-88).[1] This raises doubts about whether low-income nonvoters would always be motivated by economic concerns and therefore consistently cast a pocketbook vote for the Democrats.

Relying on aggregate data rather than public opinion polling, DeNardo (1980) echoes these arguments. He confirms that the electoral advantage of higher turnout for Democrats in congressional races is neither large nor universal. This advantage depends not just on the strength of the party–class linkage but also on the election-specific factors that cause peripheral voters to defect. On the assumption that nonvoters are less likely to be strong partisans, higher turnout would hurt Democratic candidates whenever short-run forces favor the Republicans because defection rates would be higher among their newly mobilized partisans (DeNardo 1980; Martinez and Gill 2005).

The main conclusion of research on the relationship between turnout and electoral outcomes in congressional (DeNardo 1980; Wattenberg and Brians 2002; Wuffle and Collet 1997), Senate (Citrin, Schickler, and Sides 2003; Nagel and McNulty 1996), and presidential elections (Brunell and DiNardo 2004; DeNardo 1980; Highton and

1. There is also little evidence of differences among voters and nonvoters within particular ethnic groups, though African Americans are something of an exception (Ellcessor and Leighley 2001).

Wolfinger 2001; Martinez and Gill 2005; Nagel and McNulty 2000) is that the impact of higher turnout is both variable and usually small. In most cases, Democrats gain from higher turnout, and even a small shift in the partisan distribution of the vote can change the result in close elections. Nevertheless, in most of the American national elections analyzed in the studies cited here, even universal turnout evidently would not have produced a different winner.[2]

The purpose of this paper is to extend this research agenda by considering the implications of compulsory voting in the last four presidential elections. Because the 1992, 2000, and 2004 elections were so close, the tabloid version of our research question is, "If everyone had voted, would Bill Clinton and George W. Bush have gone to the White House?" On a loftier analytical plane, our primary concerns are to estimate, state by state, the *partisan differential* between voters and nonvoters and to explain variation in this differential. With the estimated party differential value in hand, we can calculate the effect of higher turnout on the actual Electoral College results. The inclusion of four elections permits comparisons across states and within states over time.

We find that, on average, nonvoters were slightly more Democratic than voters in each of the four elections examined. Some states—such as Texas and Colorado—have consistently large partisan differentials. Others, such as Pennsylvania and New York, have only very modest differences between voters and nonvoters. Our state-by-state estimates suggest that Bill Clinton likely would have won a handful of additional Southern states in 1992 and 1996 if there had been universal turnout, increasing his Electoral College margin. Our estimates suggest that there is a reasonably high probability that Al Gore and John Kerry would have won under universal turnout, but both elections still would have been extremely close. This suggests that although universal turnout might well tip very close elections in the Democrats' favor, the electoral landscape would not be transformed. And, of course, the impact of higher but less than universal turnout would depend on which voters were mobilized in a particular contest.

Methods and Data

Our approach (see Citrin, Schickler, and Sides 2003) first requires a large enough sample of eligible voters within each state to make reasonable inferences. The best available data come from the November Voter Supplement that the U.S. Census Bureau conducts every election year as part of the Current Population Survey (CPS). This survey asks respondents whether they voted in the most recent election and contains large

2. Hajnal and Trounstine (2005) show that in the context of city elections—in which turnout tends to be extremely low and the minority population relatively large—increased turnout would make a more substantial difference to minority representation. It is also worth noting that other sorts of participatory biases, such as the larger financial contributions of wealthy citizens, may have a greater impact on politicians' behavior and political outcomes (Gilens 2005; Verba, Schlozman, and Brady 1995). But when it comes to the question of the impact of increased turnout in statewide and national elections, the evidence suggests that even universal turnout would make only a modest difference. There is, however, some evidence that higher turnout benefits left-of-center parties in Western Europe and in postcommunist countries (Bohrer, Pacek, and Radcliff 2000; Pacek and Radcliff 1995).

samples in every state.[3] This enables us to consider each state separately rather than assuming that the effects of increased turnout would be constant across states.[4]

Second, we need an estimate of how nonvoters would have voted. Because the CPS does not include questions about vote choice or partisanship, we 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. And because exit polls, by definition, sample only voters, these estimates are not contaminated by inflated self-reports of turnout.[5] We link the VNS data with the CPS data in the following fashion:

1. Estimate a vote choice equation for the presidential race in each state 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. We include only respondents that are citizens age 18 and over. We assume that the parameters of voter choice would have been the same among nonvoters if they had voted. Citrin, Schickler, and Sides (2003) provide reasons why this assumption is plausible. (Others who simulate the consequences of increased turnout—notably Martinez and Gill [2005]—make this same assumption.)
3. Compare the predicted aggregate vote choice of voters and nonvoters in the CPS to determine whether the outcome of the race in that state would have changed had all the nonvoters actually gone to the polls.

The first step in the simulation is to model presidential vote choice in each race. 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.) One complication is

3. For example, the 2004 CPS had about 104,000 respondents in total, and the median sample size across states was 1,700.

4. Moreover, the CPS estimate of turnout is also more accurate than that of the NES or other "political" surveys. Though the CPS estimate suffers from the problem of overreported turnout, it is substantially closer to the true level than is the NES estimate (see Citrin, Schickler, and Sides 2003).

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 Berinsky 1999, 2002; Brehm 1993). If such underrepresentation had partisan consequences—for example, if conservatives were less likely to answer exit polls than liberals—it would complicate our analysis. In the exit polls we analyze here, the breakdown of the vote among VNS respondents correlates quite highly with the actual outcome of the election ($r \approx 0.99$ in all years). However, the VNS polls do overestimate the Democratic candidate's share of the vote more often than they overestimate the Republican's—a tendency that became somewhat notorious on election day in 2004. However, in the analysis that follows, we employ the VNS sample weights, which mitigate any such bias. For example, using these weights, the average difference between the 2004 VNS state-level marginals and the state-level outcomes is essentially zero (by contrast, the unweighted marginals are, on average, about 3 points more favorable to Kerry). Moreover, the difference between the actual outcome and the VNS poll marginals is not correlated with the key quantities we analyze later, such as the "partisan differential" among nonvoters. For example, in 2004, the correlation between the error in the VNS and the partisan differential is $r = -0.16$ ($p = .26$). Furthermore, it is not evident that the slight tendency for conservative nonresponse would bias our demographic model of the vote.

how to handle third-party and independent candidates, especially in 1992 and 1996, when Ross Perot received about 19% and 8% of the vote, respectively. For those years, we estimate two separate sets of logit models in the VNS. In the first, the dependent variable is coded 1 for Clinton and 0 for Perot and Bush/Dole. The second dependent variable is coded 1 for Clinton and Perot, and 0 for Bush/Dole. The probability of voting for Clinton falls directly out of the first model.[6] The probability of voting for Perot is equal to the probability of voting for Clinton (from Model 1) minus the probability of voting for Clinton or Perot (from Model 2). The probability of voting for Bush/Dole is just 1 minus the probability of voting for Clinton or Perot (from Model 2).[7]

In calculating the predicted probability of a Democratic vote choice for each respondent in each state, we need to take into account the uncertainty that characterizes the coefficient estimates from the VNS vote choice model. Therefore, for each CPS respondent, we create 1,000 simulated predicted probabilities. We treat each coefficient estimate from the VNS model as a normally distributed random variable, with a mean equal to the point estimate from the VNS model and a standard deviation equal to the standard error for that point estimate. We then "draw" a value for each coefficient and calculated the predicted probability, repeating this procedure 1,000 times. The Democratic share among voters and nonvoters is simply the mean of predicted probabilities across the simulations within each state. These 1,000 iterations enable us to calculate bootstrapped standard errors for these estimates. We can then calculate whether any difference between voters and nonvoters is statistically distinguishable from 0. In calculating how full turnout would have affected the outcome of each state's election, these standard errors provide estimates of whether the percentage voting for, say, John Kerry, under full universal is statistically distinguishable from 50%. Moreover, we can also compute the percentage of the simulations in which each candidate is the "winner" under the full-turnout scenario. This allows us to consider alternative thresholds for concluding that a state's outcome would have changed under full turnout.[8]

The advantages of this methodology are several. First, it allows the determinants of vote choice to vary across states. As in our earlier work on Senate elections, this first-stage analysis reveals considerable variation in the determinants of vote choice across states. For example, in 2004, the effect of being wealthy—using our dummy variable specification, the effect of making $75,000 or more versus making less than $15,000—is positively

6. These probabilities are 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.

7. As a robustness check, we replicated this exercise using a multinomial logit model in the 1992 VNS (with Bush as the base category). The results were virtually identical. As a further check, we estimated a predicted probability for each VNS respondent using the logit models described in the text and then averaged those probabilities to the state level. In 1992, those state-level predictions correlated with the actual vote share at these levels: Clinton ($r = 0.99$), Perot ($r = 0.99$), Bush ($r = 0.99$). In 1996, the corresponding correlations were Clinton ($r = 0.99$), Perot ($r = 0.97$), Dole ($r = 0.99$). Similarly high correlations were obtained in 2000 and 2004.

8. Citrin, Schickler, and Sides (2003) take account of underlying uncertainty in the estimates of nonvoters' preferences in a more casual fashion (see p. 84 n. 20). We believe this simulation approach is a significant improvement. A next improvement to the simulation would be to take account of covariation among the coefficients from the VNS models in generating the simulated predicted probabilities.

(though never significantly) associated with a Democratic vote choice in 18 states, negatively and insignificantly associated with a Democratic vote choice in 19 states, and negatively and significantly associated with a Democratic vote choice in 14 states. Simply put, there is a great deal of variation in the direction and magnitude of income's effect (see also Gelman 2008). A similar point can be made about many other variables in these models. Such variability in the structure of vote choice across states reinforces the argument for using the VNS rather than relying solely on NES (National Election Studies) data, as in Brunell and DiNardo (2004) and Martinez and Gill (2005).

This method allows us to estimate the preferences of nonvoters and to simulate the effects of full turnout on a state-by-state basis. Given the centrality of the Electoral College to presidential campaign dynamics, we believe that the ability to analyze state-level results using exit polls and the CPS is crucial. Though employing a survey such as the NES does expand the set of available covariates related to turnout and vote choice (see Martinez and Gill 2005), it limits scholars to a single national estimate of the influence of higher turnout and makes it impossible to take into account the dynamics of the Electoral College. Knowing that, under full turnout, Democrats would have gained two points in the popular vote in 2000 or 2004 tells us little about the ultimate outcome of interest absent information about the geographic distribution of the Democratic gains. We believe that a state-level approach more than makes up for the lack of attitudinal variables in the CPS.[9]

The "Partisan Differential": Comparing Voters and Nonvoters

A first question is whether voters and nonvoters differ in their partisan preference. To calculate this "partisan differential," we simply subtract the mean probability of a Democratic vote choice among voters from the mean among nonvoters across the simulations in each state. Thus, positive values indicate that nonvoters were more "Democratic" than voters, as the conventional wisdom suggests.

Figures 1 and 2 summarize the partisan differential in two different ways. Figure 1 presents kernel density plots of the partisan differential across the 50 states and the District of Columbia for each election, with vertical lines representing the median value in each year. Figure 2 presents the partisan differential year by year for each state, with vertical lines around each estimate to represent the 95% confidence interval.[10] Two central findings stand out from these figures. First, on average, nonvoters were more Democratic in each of the elections. In Figure 1, the median partisan differential (treating the state as the unit of analysis) ranges from a low of 1.3 percentage points in 1996 to a high of 2.6 in 1992. In fact, with the exception of 1996, the median values of the partisan differential are remarkably stable. Among the 204 state–year combinations captured in Figure 1 (51 states × 4 elections), 167 (or 82%) have a positive partisan differential, and

9. Furthermore, reliance on simple demographic correlates of the vote—such as income, race, gender, and education—addresses most directly the claim that the class bias in turnout is itself the key obstacle to progressive victories.

10. We do not present the results for the District of Columbia simply to make Figure 2 symmetrical.

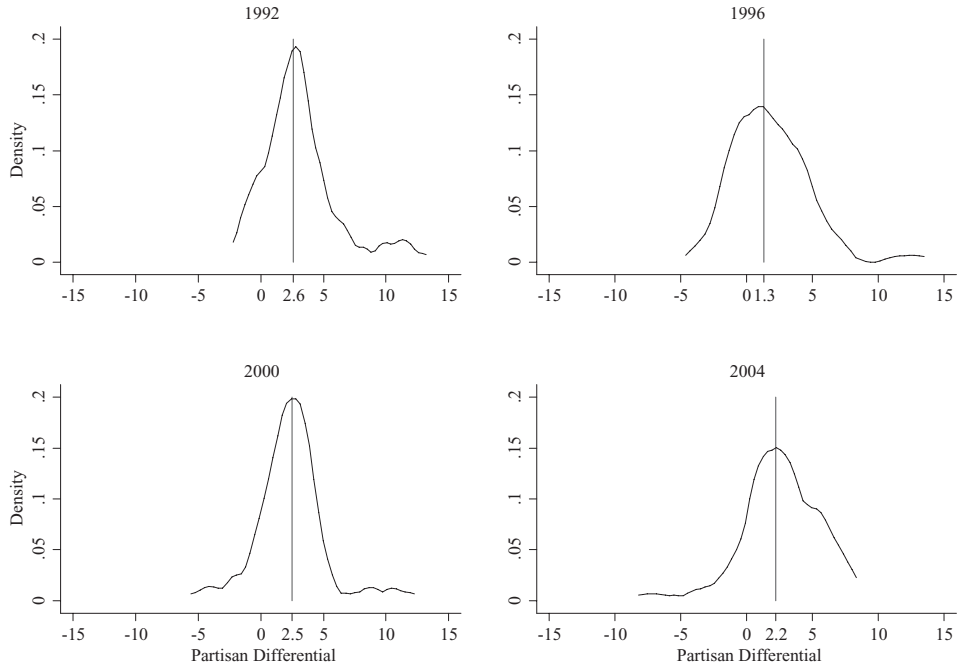

FIGURE 1. Kernel Density Plots of the Partisan Differential, by Year
Note: Each figure is a kernel density plot of the partisan differential, where the unit of analysis is the state. The vertical line indicates the median value.

126 of those 167 are statistically distinguishable from 0. As we found in our previous analysis of Senate races, there is merit to the argument that nonvoters tend to be more Democratic.[11]

Second, there is considerable variation in the partisan differential—variation across years, across states, and within states over time. The median values in Figure 1 suggest some variation over time. The variation across states is evident in both figures. While the average partisan differential is always positive, the partisan differential in each state is not always positive. For example, the partisan differential is estimated to be negative in 16 states in 1996, suggesting that in those cases, nonvoters were actually more Republican. (In 4 of these 16 states, the negative partisan differential is statistically distinguishable from zero.) Some of this variation across states appears to reflect chronic differences. For example, the partisan differential in Texas is consistently large and positive, ranging from a low of 6.0 percentage points in 2004 to a high of 12.4 in 1996. By contrast, the partisan differential in Pennsylvania is consistently quite low, ranging from −0.06 percentage point in 1992 to 2.1 in 2000. However, as Figure 2 shows, there is also variation within

11. Our results also suggest a "Perot bias" among nonvoters. When we calculate this same partisan differential for Perot, the median values are 1.4 in 1992 and 1.6 in 1996. This suggests that nonvoters may also be more predisposed than voters to prefer (at least some) third-party candidates.

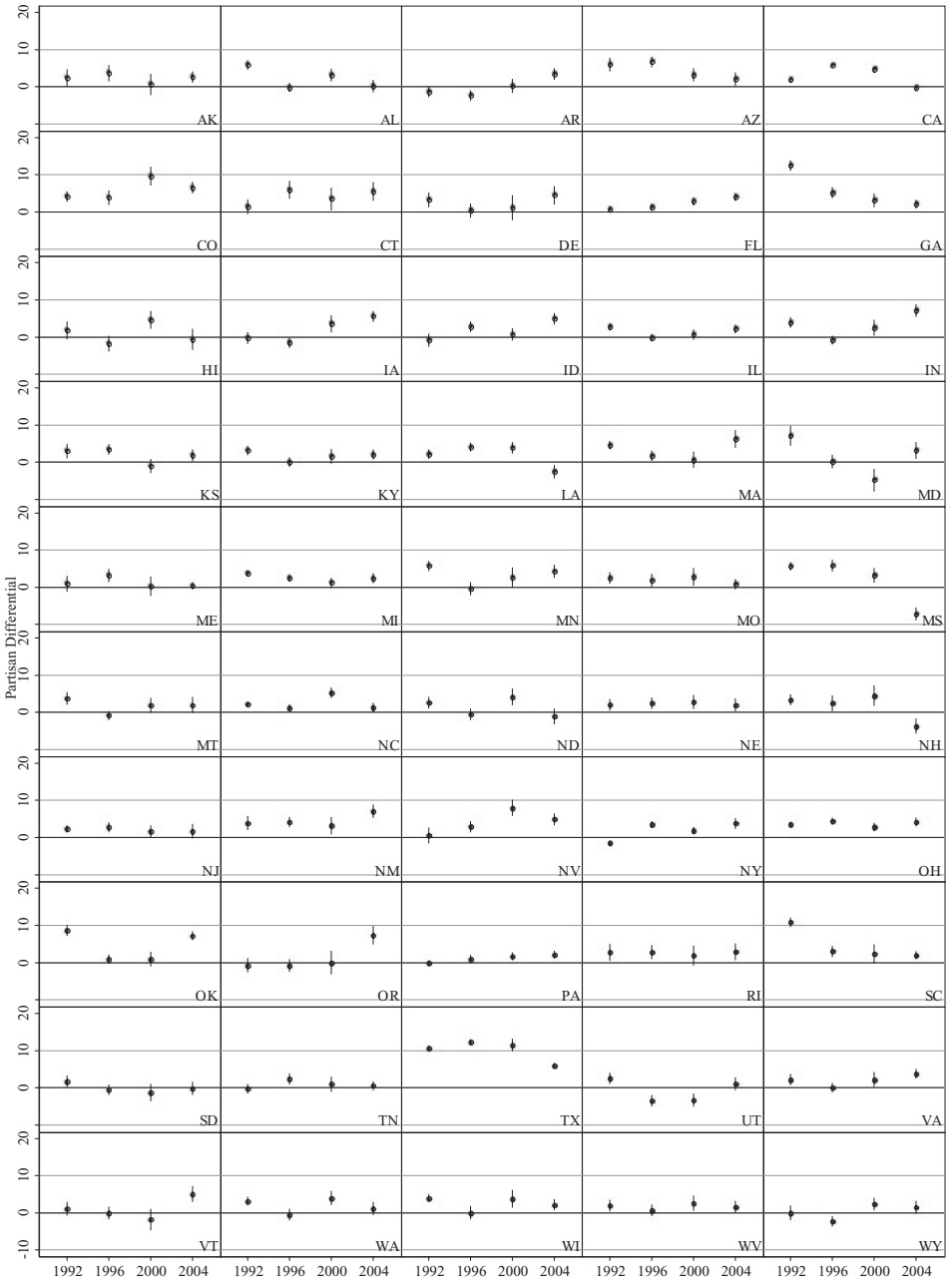

FIGURE 2. Partisan Differential 1992-2004, by State

Note: The point estimates are the partisan differential in that state year. The vertical lines are 95% confidence intervals.

states over time, suggesting that the partisan differential may derive not only from fairly stable features, such as a state's demographic composition, but also from election-specific forces.

These findings confirm the conclusions in our previous analysis of the partisan differential in Senate elections: Nonvoters do tend to be more Democratic, but this tendency varies in notable ways across time and space. Below we present some preliminary analysis that seeks to account for some of this variation. In general, this variability across states suggests that the impact of universal turnout on presidential election outcomes cannot be estimated absent close attention to state-level dynamics.

Simulating the Effects of Full Turnout on Electoral Outcomes

To estimate the effects of universal turnout on state-level outcomes, we combine our estimate of the partisan differential and the actual outcome of the election in each state.[12] We use the actual outcome because it is obviously the best available estimate of what voters did, and thus we do not need to rely on the predicted Democratic vote share among CPS voters.[13] In projecting the behavior of nonvoters, we rely on the partisan differential as the most defensible estimate. We weight the outcome and the partisan differential by the actual turnout in this race.[14] Thus, the projected outcome in each state j equals

$$\mathrm{SIM}_{ij} = (\mathrm{outcome}_j \times \mathrm{turnout}_j) + [(\mathrm{outcome}_j + \mathrm{PD}_{ij}) \times (1 - \mathrm{turnout}_j)],$$

where PD_{ij} is the partisan difference between voters and nonvoters in state j in the ith run of the simulation. The best estimate of the outcome is simply the mean of the 1,000 values for SIM_{ij}. Furthermore, we compute the proportion of simulations in which each candidate wins each state.

This version assumes that the estimate of the nonvoters' partisan preference may be slightly 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 and thus best captures how adding in nonvoters would change the results. Notice that a large partisan differential is unlikely to shift outcomes when the turnout is already high—so that (1− turnout) is small—or when the original outcome is lopsided.

12. To carry out this full-turnout simulation with the utmost validity, the VNS models should generate predictions very close to the actual outcomes of the presidential race in each state. Fortunately, this was largely the case: The correlation between the state-level VNS predictions and outcomes was over .99 in each election year (see footnote 6).

13. The predicted Democratic vote among CPS voters is, as one would expect, extremely close to the actual outcome. The only differences appear to be attributable primarily to sampling variability and imprecision in our model of the vote. Thus, we find that the percent Democratic among CPS voters is relatively symmetrically distributed around the actual outcome and not consistently skewed in either the Democratic or Republican direction. An alternative simulation would combine CPS voters and nonvoters without drawing on 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.

14. We use turnout of the voting-age population.

We also replicate the full-turnout simulation by simply taking the original outcome, adding in the estimated choice of CPS nonvoters and weighting by the actual level of turnout in the election. The preference of nonvoters is measured by the mean of the predicted probability among nonvoters in the CPS. Thus, we have,

$$\text{SIM2}_{ij} = (\text{outcome}_j \times \text{turnout}_j) + [(\text{nonvoters}_{ij}) \times (1 - \text{turnout}_j)]$$

Our earlier work focused primarily on this model and used the simulation using the partisan differential as a robustness check (see Citrin, Schickler, and Sides 2003). However, we now believe that it makes more sense to use the partisan differential rather than the mean predicted probability in projecting likely outcomes. By using the partisan differential, we are in effect saying, given what we know about the determinants of vote choice from the VNS models, how do nonvoters differ from actual voters? If our model of vote choice is imperfect, the estimates will be unaffected so long as the errors in the model affect nonvoters and voters in similar ways. While the two simulations produce broadly similar results, there are a handful of noteworthy differences in 2000 and 2004. We focus our discussion on the simulations using the partisan differential (SIM_{ij}) but also note any differences that emerge using the alternative approach (SIM2_{ij}).

In Table 1, we present the states where we estimate that full turnout would have changed the outcome. This table includes both the actual vote totals and the means of the simulations in each state. Table 2 presents the Electoral College votes that each candidate won in reality and would have won given the new state outcomes presented in Table 1. In 1992, universal turnout is estimated to generate Clinton victories in four states that George H. W. Bush won: Arizona, North Carolina, South Carolina, and Texas.[15] No states flipped from the GOP to the Democratic column. The resulting Electoral College margin for Clinton increases from 370-168 to an even more convincing 432-106. In 1996, once again, four states switch to the Democrats under full turnout: Colorado, Georgia, Mississippi, and Texas.[16] The prevalence of Southern states on this list makes sense: Democrats targeted their mobilization efforts in swing states outside the South. But in these good Democratic years—with a Southerner at the top of the ticket—these states may have been within Democratic reach given a full mobilization of the electorate.

The 2000 and 2004 elections present somewhat more complicated cases given the closeness of the actual outcome. In 2000, our preferred simulation method suggests that universal turnout would have led to Democratic victories in three states carried by George W. Bush: Florida, New Hampshire, and Nevada. Each of these victories is estimated to be quite narrow (see Table 1), but the Florida and Nevada switches holds in all of the 1,000 simulations, and the New Hampshire switch occurs in 97.6%.[17] The

15. This holds in each of the 1,000 iterations. The results are similar in the second simulation (SIM2), except that Florida also flips to the Democrats.

16. The Mississippi result holds in 994 of 1,000 iterations. The other states flip in each of the 1,000 iterations. In the second simulation, Georgia, Mississippi, and Texas consistently flip to the Democrats, but Colorado goes Democratic in just 33% of the iterations.

17. Though the mean of the simulated outcome does not "change hands," Colorado switches to the Democrats in 43% of the iterations, while Oregon goes from Gore to Bush in 36%.

TABLE 1
Results of Full Turnout Simulation

		Actual Outcome			*Simulated Outcome*		
	Type of Switch	*Democrat*	*Republican*	*Perot*	*Democrat*	*Republican*	*Perot*
1992							
AZ	R→D	37.0%	38.9%	24.1%	39.7%	35.9%	24.4%
NC	R→D	42.7	43.5	13.7	43.8	42.9	13.3
SC	R→D	40.1	48.3	11.6	45.9	42.3	11.7
TX	R→D	37.2	40.7	22.1	42.3	34.8	23.0
1996							
CO	R→D	45.9	47.3	6.8	47.6	45.5	6.9
GA	R→D	46.2	47.4	6.4	49.1	42.2	8.7
MS	R→D	44.5	49.6	5.9	47.6	46.1	6.3
TX	R→D	44.1	49.1	6.8	51.0	42.2	6.8
2000							
FL	R→D	50.0	50.0		51.4	48.6	
NH	R→D	49.3	50.7		51.1	48.9	
NV	R→D	48.1	51.9		52.8	47.2	
2004							
CO	R→D	47.6	52.4		50.1	49.9	
IA	R→D	49.7	50.3		51.4	48.6	
NH	D→R	50.7	49.3		49.5	50.5	
NM	R→D	49.6	50.4		52.8	47.2	
NV	R→D	48.7	51.3		51.1	48.9	
OH	R→D	48.9	51.1		50.4	49.6	

Note: Cell entries under "simulated outcomes" are means of the 1,000 iterations of the first simulation discussed in the text (SIM).

results are more favorable to Bush in our alternative simulation: While New Hampshire and Nevada go Democratic, Bush wins Oregon and holds Florida. Thus, the bottom line is a projected Gore win in the first simulation (by a 299-239 Electoral College margin) and a narrow Bush win in the second (270-267). Because the partisan differential in Florida suggests that nonvoters were 2.8 percentage points more Democratic than voters, thus making it seem likely that universal turnout would have tipped the state to Gore, the first simulation appears more persuasive. But it is also striking that an extremely close election still would have been close under the assumption of universal turnout, with the winner determined by narrow victories in only a handful of states.

The 2004 election is much the same story. In our preferred simulation, five states where Bush won by slim margins switch to Kerry: Colorado, Iowa, New Mexico, Nevada, and, perhaps most strikingly, Ohio. Kerry's narrow 49%-51% loss in Ohio becomes, under full turnout, an even more narrow victory (50.4%-49.6%). In most of these states, Kerry wins in the vast majority (98%-100%) of the simulations. In Colorado, he wins 61% of the simulations. By contrast, New Hampshire tilts from the Kerry column to Bush in the full-turnout simulation.[18] Taken together, this suggests a Kerry victory, with

18. This holds in 92% of the iterations.

TABLE 2
Actual and Simulated Electoral College Outcomes

	Actual Electoral College Outcome	*Simulated Electoral College Outcome*
1992		
Clinton	370	432
Bush	168	106
Perot	0	0
1996		
Clinton	379	439
Bush	159	99
Perot	0	0
2000		
Bush	271	239
Gore	266	299
2004		
Bush	286	245
Kerry	252[†]	293

Note: The simulated outcome is calculated taking into account the states that would have switched under full turnout, as presented in Table 1.
[†] In 2004, one elector from Minnesota voted for John Edwards for both vice president and president. This vote is counted as Kerry's for the purposes of this table.

a 295-243 Electoral College margin. Even without Colorado, where his victory was somewhat more in question based on our simulations, Kerry still wins by a 286-252 margin—with the result dependent on a very tight Ohio contest.[19] Once again, universal turnout tends to offer a small boost to the Democrats in some highly contested states. But much like in 2000, the 2004 race still would have hinged on the votes of a small fraction of the electorate in certain battleground states.

Explaining the Partisan Differential, or What's the Matter with Texas?

Our analysis to this point has begged important questions: What factors underlie the partisan differential? Why does it vary across states and over time? At this stage, as in Citrin, Schickler, and Sides (2003), we hazard only preliminary answers to these questions. In general, we hypothesize that the partisan composition of nonvoters derives first from stable attributes of a state's voting population, such as the prominence of groups that tend to vote at lower rates—the less educated, those with lower incomes, and nonwhites. We also suspect that the partisan differential derives from election-specific

19. The second simulation suggests a different outcome. Kerry wins two states, Iowa and New Mexico, but Bush wins three—not only New Hampshire but also Pennsylvania and Wisconsin. This results in a 310-228 Electoral College margin for Bush. Although, for the reasons delineated earlier, we believe the first simulation provides a more reasonable assessment of the consequences of full turnout, it is important to note that the predicted outcomes vary with the slightly different assumptions in the models.

forces—notably, characteristics of the candidates and efforts by the candidates, parties, and others to mobilize like-minded groups of voters.[20] A simple hypothesis is that in more competitive states, get-out-the-vote efforts tend to benefit the Democratic Party more than the Republican Party. Thus, in competitive states, the partisan differential—that is, the Democratic "bias" among nonvoters—should decrease.

To examine how a state's sociodemographic composition and its level of partisan competition affect the partisan differential, we estimate a simple regression model in which the partisan differential is the dependent variable. The independent variables are the mean levels of education and income; the proportion of the state's population composed by blacks, Latinos, and Asians, respectively; the proportion of the population that belongs to a union; and the winning candidate's margin of victory.[21] The last measure captures the partisan competition in the state: As the margin of victory increases, the partisan differential should also increase. With regard to the demographic measures, we expect that the partisan differential will exhibit a greater Democratic "bias" among nonvoters in states with lower mean levels of education and income and in states with larger nonwhite populations.[22]

In Table 3, we present the results of this model, estimated separately by year. We find little role for electoral competitiveness, as the winner's margin of victory is never significant.[23] Education, income, and union membership also have negligible effects. If anything, the partisan differential is mainly affected by the nonwhite population in a state. In 1992, the proportion of blacks is positive and statistically significant; states with higher black populations also had a greater Democratic bias among nonvoters. In 1996, 2000, and 2004, the proportion Latino has this same effect.

We can thus say with some certainty that the partisan differential depends on the ethnic composition of the state. This helps account for why Texas manifests such a large partisan differential. It has a large proportion of Latinos (approximately 27% of voting-age citizens in the 2004 CPS). Only California and New Mexico have Latino populations that are comparable in size. Moreover, as Figure 3 indicates, in each of these four elections, Latinos in Texas were simultaneously less likely to vote than white or blacks, as measured by the CPS, but also disproportionately Democratic when they did vote, as measured by the VNS exit poll.

This finding requires two caveats, however. First, the effect of the Latino proportion on the partisan differential is not constant across years. There is some evidence in Table 3

20. That election-specific forces matter is evident in the way our estimates of the "presidential" partisan differential correspond to our estimates of the partisan differential in Senate races (Citrin, Schickler, and Sides 2003). At this point, we have computed both sets of estimates for the 1992, 1996, and 2000 elections. The correlations between these two measures of the differential, treating states as the units of analysis, are 0.63 in 1992, 0.81 in 1996, and 0.67 in 2000. That these correlations are significantly greater than 0 suggests that state attributes matter; that these correlations are far from perfect signals the relevance of election-specific forces.

21. The demographic measures are computed from the Current Population Surveys in each state, as these provide more up-do-date measures than the decennial census. Mean income and education are simply the averages of five-category scales.

22. Adding a dummy variable for Southern states to this model did not produce a statistically significant result and did not change the other results appreciably.

23. We also investigated the possibility that the margin of victory affects the absolute value of the partisan differential, under the assumption that competitive states simply have less bias (whether Democratic or Republican) among nonvoters. There was, however, no significant relationship.

TABLE 3
Regression Models of the Partisan Differential

	1992	*1996*	*2000*	*2004*
Winner's margin of victory	–0.104	–0.02	–0.021	0.001
	(0.052)	(0.040)	(0.030)	(0.034)
Mean level of education	0.024	–0.053	–0.024	0.047
	(0.046)	(0.038)	(0.046)	(0.041)
Mean level of income	–0.002	0.03	–0.002	0.001
	(0.032)	(0.022)	(0.022)	(0.025)
Proportion black	0.148**	0.053	0.03	–0.047
	(0.053)	(0.037)	(0.035)	(0.039)
Proportion Latino	0.057	0.215**	0.142**	0.100*
	(0.061)	(0.046)	(0.045)	(0.045)
Proportion Asian	–0.005	–0.037	0.047	–0.085
	(0.048)	(0.037)	(0.039)	(0.048)
Proportion union member	–0.127	0.037	–0.092	0.148
	(0.556)	(0.449)	(0.456)	(0.532)
Constant	–0.026	0.059	0.084	–0.106
	(0.078)	(0.070)	(0.090)	(0.072)
Adjusted R^2	0.10	0.29	0.13	0.11
N	51	51	51	51

Note: Cell entries are ordinary least squares regression coefficients, with estimated standard errors in parentheses.
* $p < .05$; ** $p < .01$ (two-tailed).

that it declined in more recent elections, from b = 0.215 in 1996 to b = 0.100 in 2004. A similar trend is evident in Texas: As Figure 3 shows, the proportion of Latinos voting Democratic declined from 69% in 2000 to 53% in 2004. Thus, demographic features such as ethnicity and socioeconomic class will affect the partisan differential only when those features are themselves strongly associated with vote choice—a point made by Martinez and Gill (2005) as well, as they found that the decreasing relationship between increased turnout and Democratic electoral success from 1960 to 2000 arose in part because socioeconomic class is less strongly associated with vote choice.

A second caveat takes on this question: Then what's the matter with California? Like Texas, California has a large Latino population that is simultaneously less likely to vote than whites or blacks but also predisposed to vote Democratic (data not shown). Why does Texas manifest such a large partisan differential (an average of 10.1 in 1992-2004), whereas California (an average of 3.0) does not? One speculative answer is that the health of the Democratic and Republican parties in these states is crucial. At the statewide level, the Democratic Party is much more successful in California than in Texas, where Democratic victories in major races have been nonexistent since Ann Richards lost the 1994 gubernatorial race to George W. Bush. A healthy Democratic Party likely generates interest among Latinos and helps mobilize them on election day.[24] Indeed, in

24. If Texas is any indicator, the health of the party may matter more than the presence of an attractive candidate. In 1996, when the Senate race featured incumbent Republican Phil Gramm and Democrat Victor

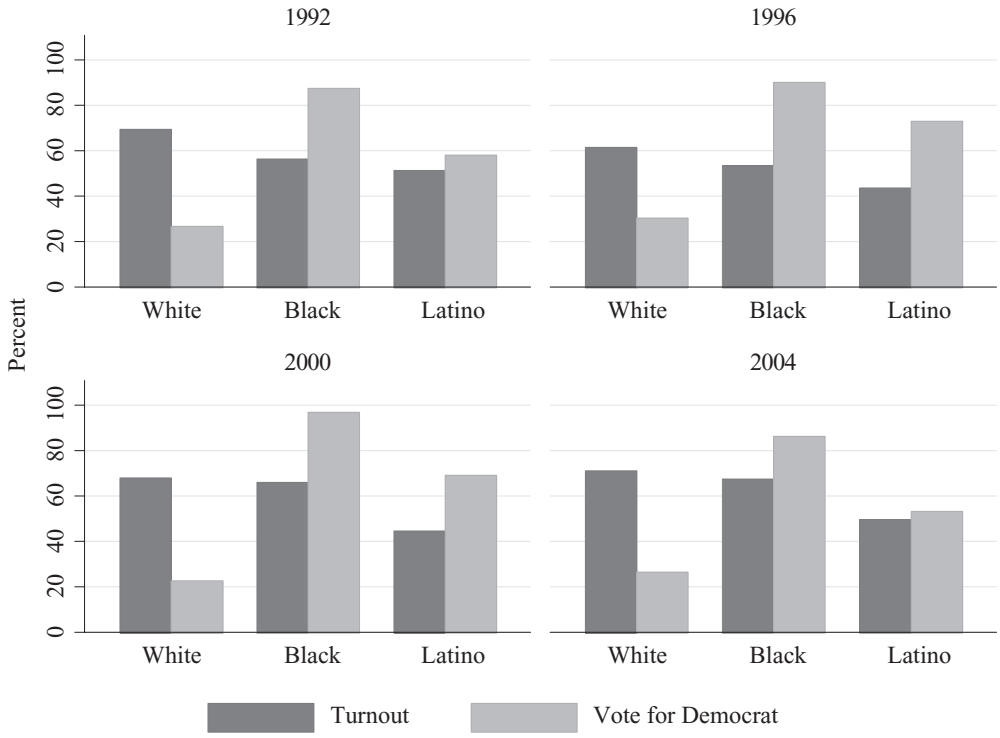

FIGURE 3. Voter Turnout and Vote Choice in Texas (1992-2004), by Race
Source: Current Population Survey (turnout) and VNS exit polls (vote choice).

1992-2004, Latino turnout in California was always higher than in Texas (e.g., in 2004, 56% versus 50%). Party strength is another factor to consider in future analyses.[25]

Conclusion

Our research is heavily indebted to Wolfinger's pioneering work on voter turnout. Methodologically, we have followed Wolfinger and Rosenstone (1980) in using the Census Bureau's CPS data to study political participation. Their innovation is essential to the state-by-state analysis at the heart of this paper and our previous work on Senate elections. Our substantive question also builds on Wolfinger, Rosenstone, and Highton's

Morales, the partisan differential was scarcely any lower (an estimated 9.8), even though one might expect Morales to have attracted Latinos to the polls. Of course, Morales was himself a long-shot candidate who traveled the state in his dilapidated pickup truck. Perhaps a more viable candidate would have had greater success in countering the low turnout among Latinos.

25. It is also worth noting that the level of turnout itself is typically negatively correlated with the partisan differential: States with higher turnout levels tend to have lower partisan differentials. While the correlation is essentially 0 in 2004 ($r = -0.02$), it ranges from −0.23 to −0.33 in the other years examined (1992 and 2000 are significant at $p < .05$).

investigations of the political implications of higher levels of turnout. In characteristic Wolfinger fashion, their research put conventional wisdom to an empirical test and with sophistication and clarity reached a different and more grounded conclusion.

This paper also followed Wolfinger's lead in asking whether compulsory voting (or universal turnout) would consistently produce Democratic victories in American national elections and therefore result in more egalitarian public policies. Our results generally confirm Wolfinger and Highton's conclusion that although full turnout usually would help Democratic presidential candidates, the net gain would be both variable and numerically too small to change the outcome except in very close elections. The state-by-state analysis pursued here emphasizes the variability across time and space in the partisan differential resulting from universal turnout. Democrats typically gain, but there are exceptions. More generally, the states manifest quite stable differences in the size of the party differential, with Texas at the high end of the continuum.

Martinez and Gill (2005) correctly point out that the predicted leftward push from compulsory voting assumes that contemporary nonvoters tend to come from lower-income groups and that class voting is both pervasive and motivated by redistributive sentiments. The state-by-state analysis of VNS exit polls conducted here shows that the effect of income on vote choice varies in magnitude and direction. The same is true of other factors such as education and ethnicity. The broader point is that any pro-Democratic effect of universal turnout is contingent on a strong relationship between vote choice and ethnicity, class, and so on. If those relationships were to weaken over time or in particular elections—for example, Bush's strong showing among Latinos in Texas in 2004—then the consequences of higher turnout would be more complex. Moreover, the partisan choice of newly mobilized voters depends on the salient issues of an election. As Wolfinger and Highton show, lower socioeconomic status nonvoters are often more conservative than the general public on key social issues. If the election turns on those issues, then the partisan implications of higher turnout will not necessarily favorable to the Democratic Party.

Wolfinger and Highton emphasize that the implications of higher turnout depend on the degree to which the preferences of voters and nonvoters diverge. This obviously varies across electorates. A recent op-ed by Norman Ornstein (2006) proposed mandatory voting as a solution to the intense polarization in American politics. Ornstein argued that nonvoters are more centrist than voters, in part, because parties find it cost-effective to mobilize their ideological base. Ornstein's image of the moderate nonvoter more closely resembles Wolfinger and Highton's portrayal of the less opinionated abstainer than Lipjhart's left-leaning variant. Though our analysis deals only with general elections, it is true that universal turnout has a greater potential to affect outcomes in races in which the level of turnout is quite low, as is usually true in primary elections. An interesting avenue for future analysis is how and whether increased turnout would actually affect primary outcomes, which depends crucially on whether the decision to vote is correlated with key determinants of candidate choice.

The present paper analyzed the presidential elections from 1992 to 2004. Using a new approach to account for the uncertainty inherent in estimating what nonvoters might do, we found evidence of a pro-Democratic partisan differential, suggesting that

the extremely close elections of 2000 and 2004 likely would have been decided differently if everyone eligible had voted. In later research, we intend to simulate the outcomes of these elections under alternative scenarios of higher but less than full turnout, a more realistic circumstance at present. As an example, what would be the impact of a 10% across-the-board increase in Latino voting?

Even if universal turnout could change the outcomes of close presidential elections, this does not necessarily mean that compulsory voting would transform American public policy. Al Gore's policy agenda doubtless would have differed from George W. Bush's, but given the resistance to change built into the American political system, particularly when the party division in Congress is so close, it is unlikely that a durable shift in policy to the left would have occurred. In the event of a narrow Gore victory in 2000, compulsory voting would not have precluded a shift back toward the Republicans in 2002 or 2004: The partisan differentials we have estimated simply are not large enough to override the impact of election-specific factors such as the state of the economy, international events, or candidate appeal.

Wolfinger opened up a domain of research in which exciting prospects remain. Among the important tasks are explaining variations in the partisan differential, examining how turnout levels affect the nature of the issues in campaigns, and learning more about the differences in political outlook between voters and nonvoters. Our own research suggests that it is important to look beyond the impact of class differences to the role of race, ethnicity, and age, factors that are linked both to turnout and to party identification.

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Appendix: Model Specification in the VNS Exit Polls

For each state in the 1992-2004 VNS exit polls, our model includes dummy variables for age (25-29, 30-39, 40-44, 45-49, 50-59, 60-64, and 65 and over, with 18-24 excluded), income ($15,000-$30,000, $30,000-$50,000, $50,000-$75,000, and $75,000 and over, with under $15,000 excluded), race (black, Latino, Asian, and other race, with white excluded), and gender. Where available, we included dummy variables for education (high school diploma, some college, college graduate, advanced degree, with no high school diploma excluded), whether the respondent was married, whether the respondent or someone in the respondent's family was a union member, and, in 1992 only, whether the respondent was a veteran. We summarize the states that lacked measures of education, marital status, union membership, and veteran status.

TABLE A-1
States Lacking Certain Variables in VNS Exit Polls

Variable	*1992*	*1996*	*2000*	*2004*
Education	AL, AK, AZ, AR, CO, DE, DC, HI, ID, IN, IA, KS, LA, ME, MA, MN, MS, MT, NE, NV, NH, ND, OK, RI, SC, SD, TN, UT, VT, VA, WA, WV, WI, WY	AL, AK, AZ, AR, DE, DC, HI, ID, IN, IA, KS, KY, LA, MD, MS, MT, NE, NV, NM, ND, OK, RI, SC, SD, UT, VT, VA, WV, WY	AL, AK, HI, ID, KS, LA, MS, OK, SD, UT, WY	AK, AL, AR, CT, DC, DE, GA, HI, ID, IL, IN, KS, KY, MA, MD, ME, MS, MT, ND, NE, OK, RI, SC, TN, TX, UT, VA, VT, WY
Marital status	AK, AR, DE, DC, HI, ID, IN, IA, KS, ME, MI, MS, MT, NE, NV, NC, ND, OH, OK, RI, SD, UT, VT, WV, WY	AL, AK, AZ, AR, CO, CT, DE, DC, FL, GA, HI, ID, IL, IN, IA, KS, KY, LA, ME, MD, MA, MI, MN, MS, MO, MT, NE, NV, NH, NJ, NM, NY, NC, ND, OH, OK, OR, RI, SC, SD, TN, TX, UT, VT, VA, WA, WV, WI, WY	AL, AK, AZ, AR, CO, CT, DC, HI, ID, IN, IA, KS, LA, ME, MA, MS, MT, NE, NH, NM, NC, ND, OK, RI, SC, SD, TN, UT, VT, WV, WY	AK, AL, AZ, CA, CT, DC, DE, FL, GA, HI, ID, IL, KS, KY, LA, MA, MD, ME, MS, MT, NC, ND, NE, NY, OK, RI, SC, SD, TN, TX, UT, VA, WA, WY
Union membership	AK, AR, DE, DC, HI, ID, IN, IA, KS, ME, MS, MT, NE, NV, ND, OK, RI, SD, UT, VT, WV, WY	AL, AK, AZ, AR, CA, CO, CT, DE, DC, FL, GA, HI, ID, IN, IA, KS, KY, LA, ME, MD, MA, MN, MS, MO, MT, NE, NV, NH, NJ, NM, NY, NC, ND, OK, OR, RI, SC, SD, TN, TX, UT, VT, VA, WA, WV, WY	AL, AK, AZ, AR, CO, CT, DE, DC, FL, GA, HI, ID, IN, IA, KS, KY, LA, ME, MA, MS, MT, NE, NH, NM, NC, ND, OK, SC, SD, TN, UT, VT, VA, WY	AK, AL, AR, AZ, CO, CT, DC, DE, FL, GA, HI, IA, ID, IN, KS, KY, LA, MA, ME, MS, MT, NC, ND, NE, NH, NM, OK, SC, SD, TN, TX, UT, VA, VT, WY
Veteran status	AK, AR, DE, DC, HI, ID, IN, IA, KS, ME, MS, MT, NE, NV, ND, OK, RI, SD, UT, VT, WV, WY	n/a	n/a	n/a