Do Democrats and Republicans Pay the Same Price for Seats in U.S. State Lower House Elections? An Analysis of "Cheap Seats" in Forty-four States

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We apply Campbell's "Cheap Seats" approach to measuring partisan bias in U.S. House races to elections in forty-four state lower houses from 1968 to 1999. We find that using partisan voter turnout differences as the basis for calculating partisan bias reveals generally pro-Democratic Party biases and that in many states the size of these biases is growing. States with a large number of contested seats and with a large number of marginal districts had higher levels of turnout bias than their counterparts in the 1970s. Partisan turnout bias may, on some occasions, affect party control of the house. We discuss possible efforts to alleviate these biases.

The alleged existence of partisan bias in legislative elections has been the subject of increased scrutiny by scholars of Congress, U.S. state legislatures, and non-U.S. legislatures in recent years. How election results are translated into party seats is of major importance in any discussion of legislative elections. Concerns about the basic fairness of legislative elections call into question the legitimacy of representative democracy. However, among scholars there is still no consensus on an operational definition of partisan bias. This paper is an application of one measure of partisan bias—James E. Campbell's "cheap seats" definition—to U.S. state legislative lower house elections (Campbell 1996).

The Campbell approach is relevant in that turnout differences across legislative districts may result in one party receiving more seats than it should perhaps receive given that party's total statewide vote. Not only might one party's majority in the legislature be diminished, thus making it more difficult to achieve legislative victories, but in competitive states such a bias could conceivably cost a party a legislative majority. Recently in the state of Texas, for example, Democrats first in the lower house and later in the upper house fled the state (to Oklahoma first, no less, and then to New

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Mexico), in order to deny Republicans in the Texas legislature a quorum. This flight to deny a quorum delayed the Republicans from pushing through a pro-Republican congressional redistricting plan that would challenge the Federal court-ordered plan produced when the legislature was unable to create its own plan in the immediate aftermath of the 2000 census. Why did Texas Republicans feel the need to create a plan that was so heavily partisan? The effort was produced by their belief that Texas Democrats had won the last round of redistricting in the 1990s; even after the Democrats complied with the Bush, Sr. Justice Department mandate to maximize the number of majority-minority districts in the state, Republicans could win only 15 of 32 U.S. House seats in the 2002 general elections while winning dozens of consecutive statewide races over the course of several years (Jackson 2003; Lublin 1997). Texas may not be the only competitive state in which this turnout bias exists.

Believing that a study is warranted by academic interest as well as by current headlines, we offer the following plan for our paper. First, the literature on partisan bias will be summarized. Second, partisan bias measures will be generated for most of the fifty U.S. state lower houses from 1968 to 1999 using the Campbell method. Finally, we will observe any trends in partisan bias that may be noticeable, and put forth possible reasons for variation in partisan bias across states and over time. However, the primary focus of our article is to apply the turnout-based model of bias to state legislatures for the first time. Thus, we see our work as a measurement paper first and foremost, and we hope to explain more fully variation in partisan bias in state lower houses in future research.

We do not suggest that the Campbell method is the only legitimate approach to measuring partisan bias; we simply note that the Campbell approach is provocative, and that it has only been applied to U.S. House elections. Believing that turnout-based bias is a real phenomenon that has not received the treatment it is due in the literature, we apply the Campbell method to U.S. state lower houses to seek to determine if the results are similar to those for the U.S. House and to attempt to understand how and why the Campbell method might produce different measures of bias than other approaches.

Previous Operationalizations of Partisan Bias

Following Gary King and Robert Browning (1987), several researchers have sought to differentiate partisan bias and partisan representational form (or responsiveness) in legislative elections. King and Browning suggest that the rate of seat change given a change in partisan voting (representational form) is not the same as partisan bias. Further, along with Tufte (1973) and

Grofman (1983) they claim that the relationship between seats and votes could be variable. They calculate partisan bias using a logit model that measures the rate of partisan seat change given a specified partisan vote change, and that identifies asymmetry in the seats-votes curve such that at a given percentage of the vote one party would win a higher percentage of seats than would the other party at the same vote percentage. It is this asymmetry that is King and Browning's measure of partisan bias.

The logit model is one way to measure what Grofman, Koetzle, and Brunell (1997) have referred to as distributional bias, that bias which simply reflects the "differences in the distribution of party voting strength across constituencies" (Brunell 1999). Another technique used to measure distributional bias—which has become more popular than the logit model since the mid-1990s—is the use of JudgeIt (Gelman and King 1994a, 1994b), which requires the researcher to regress district-level election results on independent variables from the district to produce a deviation from the "normal vote" of previous years. Either the logit model or JudgeIt have been used to measure distributional bias in a wide range of settings: congressional elections (Brunell 1999; Gelman and King 1990, 1994b); U.S. state legislative elections (Browning and King 1987; Campagna 1991; Gelman and King 1994a; Gryski, Reed, and Elliott 1990; Niemi and Jackman 1991); the electoral college (Garand and Parent 1991); presidential primaries and caucuses (Ansolabahere and King 1990; Geer 1986); and elections in non-U.S. multiparty democracies (King 1990).

It should also be noted that Grofman, Koetzle, and Brunell (1997) have argued that distributional bias is not the only source of bias in elections, and that many of the early efforts to operationalize partisan bias measured only distributional bias. Rather, they suggest that malapportionment and partisan differences in voter turnout present distinct biases in the seats-votes relationship. Aside from any election results that stem from the distribution of partisans in different proportions across a geographical region or district, if seats are allocated based on differences in population across constituencies then malapportionment bias can result. Thus, the electoral college, for example, is subject to malapportionment bias. Additionally, partisan differences in seats won due to the fact that one party's voters turn out to vote at higher rates than the other party's voters can produce turnout bias. James Campbell (1996) was the first to measure turnout bias in a systematic fashion, applying his method to U.S. House races.

Partisan Bias as "Cheap Seats"

Campbell agrees with Tufte (1973) and King and Browning (1987) that a distinction should be drawn between representational form (or responsive-

ness) and partisan bias (1996, 87-88), but Campbell is concerned with partisan bias due to party differentials in voter turnout: "Responsiveness, the rate by which votes translate into seats, is a function of a party wasting votes in losing causes. In the single-member-district, plurality-rule system, the party with the minority of the vote generally wastes a larger portion of its votes in losing causes. As a consequence, the party with a majority of the vote wins a larger share of seats than its vote share and the responsiveness of the electoral system is greater than one." Responsiveness is symmetric, in that any majority party would gain the same level of over-representation. But partisan bias implies an asymmetry in the seats-votes relationship, whereby one party would win a greater number of seats than the opposing party even if the same number of votes were cast for each. According to Campbell (1996, 90),

> [P]artisan bias depends on the distribution of unwasted rather than wasted votes. . . . A system can be biased in favor of a party by reducing the mean number of unwasted votes per victory for that party and raising the mean number of unwasted votes per victory for the opposing party. That is, bias is generally produced by one party winning relatively cheap seats.

For Campbell, party differentials in unwasted votes for one party's winning candidates relative to those of the opposing party is the key to understanding partisan turnout bias. Thus, Campbell argues that district votes rather than district vote percentages are the starting point for understanding partisan bias.

Campbell suggests that U.S. House races—with seats apportioned according to population rather than to votes (as in the U.S. Senate)—tends to favor Democrats since successful Democratic candidates win with fewer votes than successful Republican candidates. Democrats, in essence, win what Campbell refers to as the "cheap seats." According to Campbell, the ability of Democrats to win low-turnout House districts from 1954 to 1992 resulted in fewer unwasted Democratic votes, and contributed to party bias.

On another methodological score, like Butler (1951), Campbell believes that one election is sufficient for generalizing about an election system. Thus, Campbell rejects the historical approach of using a number of elections to calculate seat-votes curves (King and Browning 1987; Niemi and Jackman 1991; Rae 1967; Tufte 1973) or the JudgeIt approach (Gelman and King 1994b), which also involves the use of more than one year of data. The Campbell method also involves the use of actual election data rather than simulations based on uniform partisan swing (Backstrom, Robbins, and Eller 1978; Butler 1951; Campagna 1991; Campagna and Grofman 1990; Garand and Parent 1991; Gelman and King 1990; Scarrow 1981, 1982, 1983).

Campbell suggests that the researcher merely examine the number of votes cast for each party's candidates across the state and calculate the size of the vote won by successful candidates from each party. Then, a comparison is made between the average number of votes of Democratic winners and Republican winners. In the case of the Democrats (one could use the Republicans, of course), the researcher then calculates the total seats in the legislature that Democrats would have won if they had won seats at the same vote totals as had all winners. Next, the percentage of seats Democrats would have won, if they had won seats at the same vote totals for all winners, is subtracted from the percentage of seats actually won by Democrats. Finally, this figure is subtracted from .50 to produce Campbell's measure of partisan bias.

Campbell contends that using mean district votes or mean district vote percentages to operationalize party vote in an election masks the effects of turnout differences on seat allocation. In this respect, he agrees with Tufte (1973) and Grofman, Koetzle, and Brunell (1997) that differential voter turnout across districts is one element of partisan bias. The difference between Campbell and Grofman and company concerns how best to measure this bias.

Grofman and his associates start with a measure of distributional bias based on a simulation produced by JudgeIt. They then separately calculate turnout bias by comparing Democratic vote share with a weighted average share based on "the turnout in each district" and malapportionment bias by comparing Democratic vote share with a weighted average share based on "the population of each district (Brunell 1999)." They claim their methodology produces three distinct measures of bias and that the Campbell approach, albeit a good attempt to measure an elusive concept, is contaminated by not isolating turnout bias from distributional and malapportionment bias. However, their claims regarding the Campbell measure are not conclusive; indeed, Brunell refers to Campbell's "cheap seats" argument as "compelling" and suggests from the results of his own study of the U.S. House that "Campbell's cheap-seats hypothesis is reaffirmed" (Brunell 1999, 323-24).

Campbell's "cheap seats" method would appear to be as applicable to U.S. state legislative elections as to congressional elections. Thus, the present study is an attempt to measure bias in lower house U.S. state legislative elections since 1968. We simply ask whether the Campbell method identifies a pro-Democratic bias in state legislative elections as it does in congressional elections. We also attempt to make conjectures about other factors that might enhance or mitigate these partisan bias effects, with the hopes of stimulating debate and future research efforts that concern representation in state legislatures.

Data and Methods

The data used to calculate partisan bias come from two sources. First, we use the Inter-University Consortium for Political and Social Research (ICPSR) state legislative elections data set for the period of 1968-1989. Second, we have collected primary source data from each state for lower house state legislative general elections for the 1990-1999 period. These district-level data permit us to study lower house state legislative elections for most of the states during the 1968-1999 period.

Six states are excluded from the analysis for various reasons. Louisiana is not included because their blanket primary often serves as the general election in that state's election system. Nebraska is excluded because of the nonpartisan nature of elections in that state. Vermont is not included because the data from that state were not included in the original ICPSR data set. Arkansas, Florida, and Oklahoma are excluded because for most years vote totals for a significant number of losing candidates are not provided in the ICPSR data set. For the same reason as with Arkansas, Florida, and Oklahoma, data from Hawaii are not included after 1986. New Hampshire has election returns from 1968-1986 collected in the original ICPSR data set; however, elections from 1988-1998 are excluded because of the large number of districts in that state's lower house and the time-consuming nature of producing data that are in a useable format. Finally, districts in which a third-party candidate wins the seat are not included.

The magnitude and direction of partisan bias due to turnout differentials is measured by analyzing all elections in 42 states from 1968 to 1999 and from Hawaii and New Hampshire from 1968-86. The first step in the process is to determine whether the vote totals for winning candidates differ by party in a given election in a state. If there are differences, then bias exists in favor of the party whose candidates on average win with the fewer votes. Second, one can operationalize partisan bias in a given election by determining the percentage of seats won by a particular party if they had won 50% of the votes won by all winners (Campbell 1996, 109). For example, if Democrats win 55% of seats at the same time they win 50% of the votes won by winners, then there is a 5% bias toward the Democrats. Likewise, if Democrats win only 48% of the seats and 50% of the votes won by winning candidates, then there is a 2% bias toward Republicans.[1]

There are a number of points that should be made concerning the use of data in the "cheap seats" measure of partisan bias. For example, some researchers exclude multimember districts from state legislative election studies (Basehart and Comer 1991; Gelman and King 1994a; Holbrook and Tidmarch 1991). Other scholars include multimember districts and develop elaborate means of matching one party candidate with another to calculate vote percentages for each race (Jewell 1982; Niemi and Jackman 1991;

Niemi, Jackman, and Winsky 1991; Van Dunk and Weber 1997). The Campbell method allows the inclusion of multimember districts since only each winner's vote totals are utilized; therefore, no matching system is necessary.

In the present study, the decision was made to include uncontested elections. Campbell (1996) asserts that inclusion of such districts might inflate the measure of Democratic bias since Democrats usually are unopposed more frequently in U.S. House races (until the early 1990s) and unopposed candidacies dramatically reduce voter turnout in an election. We argue that particularly in closely contested states, the drawing of district boundaries that might cause one party to be so heavily favored by the demographics and party identification of the constituencies to keep the opposition party from contesting the seat could have an effect on the partisan bias in the state (Lublin 2004, 113-15). We should also add that uncontested lower house state legislative elections have increased during the time period under study and hence their exclusion could have an impact on the measures of partisan bias for each state. We believe the arguments in favor of including uncontested districts are greater than those for excluding them. In fact, rather than excluding them, we seek to understand their impact on partisan bias.

Floterial districts also pose a minor problem in the Campbell formulation. Floterial districts are similar to "bonus districts," in that the vote totals from a specified number of "regular districts" are simply summed to produce another seat, thus inflating the seat margin for the party that performs well in those specified districts. Floterial districts were used to allocate seats in a number of early election years during the 1968-1999 period in Georgia, Idaho, Mississippi, New Jersey, Tennessee, and Virginia. The extremely large vote totals accumulated in floterial district seats can skew mean winning vote totals. However, again, we wish to err on the side of including districts rather than excluding them, since every district does contribute to a seat in the house and is part of the process of translating votes into seats. We simply note that in the handful of floterial districts found only in a few years in a very small number of states, there will be a disproportionate impact of those districts on party bias in those state-years. As discussion of the results below will indicate, none of these states with floterial districts tend to produce outliers or strongly unintuitive results.

The Distribution of Partisan Bias Values in 1968-1999 Elections

Table 1 reports the mean distribution of partisan bias values for each state lower house chamber in the analysis for the years 1968 to 1999 as calculated with the "cheap seats" method.[2] Table 1 presents bias measures in the form of actual percentages of bias, given that Democrats won 50% of the

Table 1. Mean or Aggregate Partisan Turnout Bias, by State, 1968-1999

Alabama	1.85%	Montana	2.19%
Alaska	3.35	Nevada	6.37
Arizona	16.39	New Hampshire*	4.61
California	4.94	New Jersey	7.31
Colorado	7.04	New Mexico	2.03
Connecticut	5.03	New York	6.78
Delaware	4.13	North Carolina	1.38
Georgia	4.56	North Dakota	7.60
Hawaii*	0.31	Ohio	3.42
Idaho	3.79	Oregon	-0.54
Illinois	0.55	Pennsylvania	1.32
Indiana	5.97	Rhode Island	0.22
Iowa	1.07	South Carolina	1.74
Kansas	6.29	South Dakota	6.81
Kentucky	2.25	Tennessee	2.54
Maine	-3.79	Texas	4.33
Maryland	-0.30	Utah	7.33
Massachusetts	0.96	Virginia	2.18
Michigan	2.98	Washington	0.26
Minnesota	1.95	West Virginia	0.62
Mississippi	0.23	Wisconsin	1.47
Missouri	3.35	Wyoming	-1.56

*Includes data from only 1968-1986

vote. Positive figures indicate a Democratic bias, and negative figures illustrate a Republican bias.

As would be expected, there are Democratic biases in 40 of the 44 states. In most cases, however, these Democratic biases are not large. Especially large Democratic biases are found in Arizona, Colorado, New Jersey, North Dakota, and Utah. The four exceptions to the rule of Democratic bias are Maine, Oregon, Wyoming, and Maryland, which have mean levels of turnout bias that benefit Republicans.

A number of studies have documented the fact that Democratic voters or those most prone by socioeconomic factors to vote Democratic turn out to vote at lower levels than Republican voters or would-be Republican voters (Beck and Sorauf 1992; Campbell et al. 1964; Flanigan and Zingale 1987; Oppenheimer 1989). Weber (2000) has found that the ratio between high and low turnout calculations is the highest in lower houses where population

diversity exists in the legislative districts. Our findings in Table 1 illustrate that Democratic districts tend to have lower levels of turnout than Republican districts in state lower house elections.

The year-by-year data reveal fluctuations from one election to the next in partisan turnout bias. For several states, Maine and North Dakota being the best examples, the high average measures of partisan bias are a consequence of having high bias measures for a small number of years in the time-series. But for Wyoming, there are consistently pro-Republican biases from 1968 to 1990, which then become very large Democratic biases from 1994-98. We will speculate later in the paper about the variation in partisan bias measures by state.

Examining Party Bias in Elections Over Time

We next present the data for the lower house state legislative chambers in the 44 states in Table 2 for four different time periods. The time periods selected roughly approximate the periods when different districting plans were in use in each state: 1968-1970, 1972-1980, 1982-1990, and 1992-1999.

Several generalizations can be made from the data presented in Table 2. First, the vast majority of states illustrate trends toward more Democratic bias over time. In fact, for the data set as a whole, there is a Pearson's r of .143 ($n = 659$, $p < .01$) between time (as measured by the year of the election) and a growing Democratic bias (as measured by positive levels of bias). The pro-Democratic trend is not perfectly linear for all states, and it only results in very high levels of bias in Arizona, New Jersey, Nevada, and Wyoming. The trend toward higher levels of bias in Arizona and Nevada could partly be due to the presence of growing numbers of Latino voters. Majority-Latino districts have been referred to by one set of researchers as "modern-day rotten boroughs," often producing Democratic officeholders with very low vote totals (Lublin and Voss 2003).

Southern states tend to be characterized by very low rates of bias in the early years followed by years of Democratic biases. One could speculate that low turnout among all voters (including Republicans) in the early years may have contributed to these low levels of bias. Then, in the South, as Republicans became viable opponents of the Democrats in the later years, perhaps Republican turnout increased while Democratic turnout remained low, thus creating pro-Democratic biases. It may also be that race-based redistricting in the 1980s and 1990s resulted in more partisan bias as Democratic candidates in majority-minority districts were not seriously challenged, thus decreasing turnout and contributing to partisan bias toward the Democratic Party. When only the eight former Confederate states are analyzed, there is a Pearson's r of .521 ($n = 110$, $p < .01$) between the election year and positive

Table 2. Partisan Bias by Electoral Time Periods for Forty-four States

State	1968-1970	1972-1980	1982-1990	1992-1999
Alabama	-0.0%	0.2%	1.9%	4.2%
Alaska	-1.3	2.9	6.8	2.0
Arizona	4.1	16.9	16.8	21.3
California	6.0	3.7	6.8	3.7
Colorado	8.5	7.0	6.0	7.7
Connecticut	4.7	4.7	4.9	5.8
Delaware	9.4	6.4	2.2	1.1
Georgia	1.1	5.4	6.5	8.3
Hawaii	2.9	-1.1	0.9	N/A
Idaho	1.2	5.4	4.2	2.6
Illinois	-1.2	-0.5	1.8	1.2
Indiana	2.7	6.4	7.7	4.9
Iowa	1.0	1.9	-0.1	1.5
Kansas	6.2	5.5	5.5	8.4
Kentucky	N/A	1.8	2.5	2.4
Maine	-19.1	-6.0	0.7	1.1
Maryland	-1.9	-0.6	0.3	-0.2
Massachusetts	-1.5	0.7	2.1	1.2
Michigan	0.8	3.8	3.1	2.9
Minnesota	N/A	1.9	0.9	3.3
Mississippi	N/A	-0.1	0.1	0.6
Missouri	5.2	2.9	2.8	3.7
Montana	0.8	1.2	2.6	3.6
Nevada	-2.6	2.9	6.8	14.7
New Hampshire	-2.3	5.8	7.3	N/A
New Jersey	18.2	1.7	5.7	13.6
New Mexico	1.3	1.9	1.9	2.8
New York	10.5	7.1	5.9	5.6
North Carolina	1.8	0.3	1.1	2.7
North Dakota	28.1	9.8	0.7	3.3
Ohio	5.9	3.3	2.4	3.7
Oregon	-5.1	1.4	-0.2	-1.1
Pennsylvania	0.8	1.5	0.8	2.0
Rhode Island	-0.7	0.0	0.9	0.1
South Carolina	0.8	0.9	1.3	3.9
South Dakota	17.5	8.2	3.9	3.4
Tennessee	1.2	2.1	1.4	5.2
Texas	1.1	1.2	4.9	9.2
Utah	2.8	7.8	8.3	7.9
Virginia	7.8	0.4	1.5	4.0
Washington	-0.4	1.2	-0.1	-0.2
West Virginia	5.9	2.4	-1.6	-1.4
Wisconsin	1.9	1.6	-0.2	3.3
Wyoming	-6.5	-3.8	-8.7	12.6

(or pro-Democratic Party) bias.[3] In fact, all eight former Confederate states in the analysis exhibit their strongest levels of pro-Democratic biases in the 1990s.[4]

A few non-southern states—Missouri, South Dakota, and Delaware—show the opposite trend, a move toward less Democratic bias. Nonetheless, in none of these states do we find a decade characterized by average levels of bias favoring the Republican Party. Thus, the most remarkable observation from Tables 1 and 2 is the relative lack of Republican turnout bias during most decades. Perhaps the state of Wyoming merits further consideration in future research, as in every election before 1992, the state exhibited Republican bias; after 1990, every election was characterized by Democratic bias.

Discussion and Suggestions for Future Research

One might inquire as to whether plans adopted by neutral redistricting commissions show less partisan bias than plans adopted by the legislatures themselves. Currently, according to the National Conference of State Legislatures, seven states use neutral commissions to create and adopt state house districting plans. Five of these seven states have relatively low average partisan bias scores in Table 1. Hawaii, Pennsylvania, and Washington have scores less than 1.5%, while Montana has a score of 2.19% and Missouri has a score of 3.35%. However, the other two states—Colorado (7.04%) and New Jersey (7.31%)—have scores that are among the highest on the average partisan bias measures. A similar pattern is revealed for these seven states in the annual data. The data are not conclusive, but since 19 states where the legislature adopted the lower house plans have average partisan bias scores that are lower than the average in Missouri, it would appear that having a neutral redistricting commission create and adopt plans is not the key to understanding turnout-based partisan bias differences across states.

An analysis of partisan bias based on how strongly each party contends with the other party for seats in a state is suggested by the work of Van Dunk and Weber (1997), who have calculated marginality and contested races for 49 state lower houses. Specifically, Van Dunk and Weber have calculated the percentage of lower-house races that were won by more (and less) than 60% of the vote, and the percentage of seats that were contested in each state from 1968 to 1989. Why might one conclude that marginality and contested seats could have an effect on turnout bias?

It seems intuitive that turnout differentials are more likely to affect election results in seats that are closely contested. If there are many competitive seats in a state, then the ability of one party to win with relatively low turnout compared to the vote totals won by winners of the other party should have its maximum impact. It may also be the case that the dominant party in

the legislature would have an incentive to draw district lines in such a way as to maximize bias based on turnout if seats are hotly contested, if such gerrymandering could influence turnout differences by party. One would expect, therefore, to see higher levels of partisan bias in states in which there are more marginal seats. Likewise, a large number of uncontested elections would likely decrease turnout for both parties. But if seats are contested more frequently, then turnout variation by party is more likely to be felt in the translation of votes to seats. If so, one could expect to find higher levels of partisan bias in states in which there are a relatively large number of contested seats.

The best we can do without additional data to test these hypotheses is to draw from Van Dunk and Weber (1997). They present the mean percentage of marginal seat races and contested races in state upper and lower house elections in the 1970s and 1980s. Admittedly, the data are not perfect for our analysis since the decadal data lump together state upper house measures with those from the lower houses of each state. However, Van Dunk and Weber also include upper and lower house figures separately in the aggregate; the aggregate figures, which are not broken down by decade, indicate that bias measures for the two houses in each state are quite similar, and the greatest variation appears to be state-by-state rather than house-by-house differences in the same state. Using their data, we report regression results for the relationships between marginal seats and contested seats and turnout bias in Table 3. It should be noted that for this part of the analysis, we use absolute numbers in our measures of partisan bias. Thus, we are interested in the size, rather than the partisan direction, of bias at this point.

The Pearson's r correlation (not reported in Table 3) between state marginal seats in the 1970s and state partisan bias measured in absolute numbers is .329 ($p < .05$), and the correlation between state contested seats in the 1970s and state partisan bias is .299 ($p < .05$). While these correlations are not exceptionally large, they are in the direction one might expect and are statistically significant. The findings for the 1980s are weaker, however, as the marginality-bias correlation is only .022 (n.s.) and the contested seats-bias correlation is only .142 (n.s.). Thus, although the 1980s relationships appear to be in the correct direction, they are very weak. As reported in Table 3, when one regresses mean state partisan bias in absolute terms by decade on mean state marginal seat percentages and mean state uncontested seat percentages by decade, neither independent variable is statistically significant, although marginal seat percentages in the 1970s does have a t-ratio of 1.234 and does come close to the $p < .10$ level if a one-tailed test is used.[5] The conclusion to draw from this exercise is that competition, however defined, may well have been related to partisan turnout bias in the 1970s, but probably was not related to turnout bias in the 1980s. The 1970s findings are provocative, however, and would perhaps point to a pooled cross-sectional,

Table 3. OLS Estimates of the Effects of Contested Seats and Marginal Seats on Partisan Turnout Bias in State Lower Houses, 1972-1980 and 1982-1990

Independent Variables	Model 1: 1972-1980	Model 2: 1982-1990
% Contested Seats	2.587	3.211
	(3.249)	(3.539)
% Marginal Seats	5.258	-2.645
	(4.262)	(5.593)
Constant	-0.436	2.009
	(1.950)	(1.975)
N	44	44
R^2	.122	.020

Note: Coefficients for independent variables do not reach commonly accepted levels of statistical significance. Standard errors for coefficients are in parentheses.

time series analysis design for future research to answer the question of how competitive elections affect turnout bias more definitively.

One additional point about southern states should be mentioned. Southern states, as expected, tend to have fewer contested seats, have more safe seats, and have low levels of partisan bias. Shouldn't the creation of majority-minority districts produce the low turnout, automatic Democratic districts at the state level that mirror those congressional districts described by so many scholars, and thus contribute to high levels of Democratic bias? Perhaps, but the push to achieve majority representation had not reached fruition during most of the 1968-89 period of time of the Van Dunk and Weber study. However, the year-by-year results for each state throughout the time series indicate that states such as Alabama, Tennessee, Texas, and Virginia have seen sharply rising rates of partisan bias since the mid-1980s. We speculate that majority-minority redistricting may be part of the explanation for this rising tide of Democratic bias in the South since the mid-1980s.

Implications of the Findings

Partisan gerrymandering to influence legislative elections has been a staple of U.S. politics from the earliest decades of the Republic. We would argue that partisan gerrymandering is largely based on providing winning margins for the dominant legislative party through manipulation of what Grofman and associates call distributional bias. It may be true that partisan attempts to gerrymander could perhaps affect partisan voter turnout in a

differential manner, but we believe the drawing of district lines is focused primarily on winning plurality vote margins through packing, stacking, and incumbent displacement (which includes pitting an incumbent from the minority party against a fellow incumbent from the same party) (Butler and Cain 1992).

Similarly, since the federal courts have set a more liberal maximum population deviation standard of 10% for state house and senate redistricting plans as opposed to the nearly zero population deviation standard for congressional districts,[6] it is possible to have a form of malapportionment bias in state legislative elections. In fact, most states in the 2000 redistricting cycle did utilize population deviations of between 5% and 10%. Therefore, we must acknowledge that malapportionment bias may be a reality in state lower-house races if the resulting overrepresentation of some constituents has a partisan pattern. But it should be pointed out the federal courts can intervene to prevent such a partisan effort to exploit malapportionment bias, particularly when such an attempt disregards traditional principles of redistricting. In Georgia, for example, the redistricting plans for both the state house and senate in 2001 were successfully challenged in federal court, where the federal district court found the plans unfairly tended to pit Republican incumbents against one another but did not do the same for Democratic incumbents; drew districts that were not compact; and used the technique of point contiguity. The federal district court's decision in the Georgia case was ultimately affirmed by the U.S. Supreme Court.[7] We believe turnout bias not only exists, but may in fact be the form of bias least likely to be recognized or remedied by legislatures or the courts (Campaign Legal Center 2003; National Conference of State Legislatures 2005).[8]

Is there any possibility of minimizing the effect of partisan turnout bias while allowing parties the leeway they have been granted historically to draw district lines to maximize their ability to win votes based on the distribution of partisan voters in the districts? As seats are currently allocated according to votes, based on the "one [person], one vote" principle, which is interpreted to mean that legislative districts should be drawn based on equal population, Democrats would likely benefit from a type of bias *even if distributional bias could be alleviated through "fair" redistricting procedures*. Some might argue that turnout bias does not matter in the long run, as Democrats will always win low turnout districts, and that the net gain or loss in state legislative seats might not always be consequential. We argue that in competitive states such as Indiana, turnout bias may indeed be consequential, as party control of the house may hinge on a handful of seats. Additionally, we argue that in the best of all possible worlds, *both distributional bias and turnout bias should be minimized* as much as possible. We do not see minimizing these two forms of bias as being necessarily mutually exclusive

endeavors. How, then, could a remedy for partisan turnout bias be fashioned?

Remedies for Turnout Bias

In attempting to find remedies for partisan turnout bias, we rely on suggestions made by Campbell (1996). One strategy would involve drawing district lines based on actual votes rather than the general population of potential voters. As it is, prisoners, illegal aliens, and children are included in the "equal population of districts" requirement; thus, citizens who live in districts with many of these nonvoters benefit from the cheap seats. Whether such a remedy could be applied through judicial interpretation or state constitutional amendment, it would require a dramatic redefinition of "one person-one vote."

Second, a reduction in the number of state lower house districts might reduce the level of partisan bias in some state plans. The size of the U.S. House has varied throughout the nation's history and was finally limited to its present size by statute, rather than by constitutional amendment, in 1911. Since the size of many state lower house chambers is provided for in state constitutions, this would be a more considerable task in the states. Reducing the size of state legislatures in the South might also make it more difficult to craft majority-minority districts. Nonetheless, good candidates for this remedy would be Georgia, with 180 seats; New Hampshire, with 400 seats; and North Dakota, with 98 seats, all of which have relatively large numbers of seats and large levels of turnout bias. The small populations of New Hampshire and North Dakota make it even more plausible to suggest a reduction in seat size for the lower houses of those states, without undue loss of representation.

A third remedy involves using voter turnout as well as the more traditional demographic and partisan factors to draw district lines. State legislatures could still adhere to the equal population requirement but simply take into account precinct turnout differences in drawing the boundaries. Such a remedy also has the added benefit of producing more diverse, heterogeneous districts since lower socioeconomic regions would have to be included with neighboring higher socioeconomic areas to satisfy both the equal population and the "similar turnout" requirements. This strategy, would, however, certainly pose a challenge to the practice of drawing majority-minority districts.

The fourth remedy is simply to increase voter turnout. If turnout is increased, particularly in low turnout districts, then the level of partisan bias should decrease. This remedy could involve easing voter registration requirements, expanding voting hours or allowing more than one day to vote, expanding the use of mail-in or absentee ballots, or finding alternative methods to encourage people to vote. Whether such attempts would be

successful is another question, but increased voter turnout appears to be a less problematic and politically charged remedy to partisan turnout bias than other possibilities we have mentioned.

NOTES

[1]We offer the 1998 election for state legislative lower house seats in Texas as an example. We note that the mean vote of Texas Democratic winners (DWINMEAN) in 1998 was 13,913.33. All winning candidates earned a total of 2,604,091 votes; dividing this figure by two, each major party in an unbiased system should have expended a total of 1,302,045.5 votes (labeled UNWASTE for "unwasted votes"). Dividing the number of unwasted votes that each party should have won in an unbiased system by the average number of votes for Democratic winners produces 93.583 (DSEATS). Dividing this number (93.583) by the number of seats won by major party candidates (in this case, equal to the number of contested seats—150), produces 0.6239. This 62.39% (DPERCENT) represents the percentage of seats that would have been won by the Democrats if they had won 50% of the votes won by all winners. Finally, partisan bias (DBIAS) was calculated by subtracting 0.50 from DPERCENT, reflecting a pro-Democratic Party bias of 12.39%.

[2]The annual bias values for each state will gladly be provided to interested readers by the authors.

[3]We recoded the odd-numbered years for Mississippi and Virginia to even-numbered years to minimize any undue influences two states with odd-numbered election years might have on an eight-state analysis. For example, Mississippi's election years of 1971, 1975, 1979, etc., were recoded as 1972, 1976, and 1980 only for purposes of this portion of the analysis.

[4]For Virginia, there was a bias of 7.57% in 1969, only one case in the 1960s. After that, partisan bias in Virginia tended to fluctuate close to a Democratic bias of 1.0% until the late 1980s. But Virginia had biases of 6.52%, 2.19%, and 6.89% in the elections of 1993, 1995, and 1997, respectively.

[5]It should be noted that the correlations between the two independent variables are .634 in the 1970s and .642 in the 1980s, suggesting that collinearity could be a problem in the regression runs. Since neither independent variable is correlated highly with partisan bias in the 1980s, the issue of collinearity is irrelevant in the 1980s regression equation. However, in the 1970s, it could well be the case that both independent variables are related to bias, but the strong relationship between the two causal variables masks these relationships. It should be noted that a state-by-state breakdown of above-average and below-average marginality and above-average and below-average contested seat percentages into a two-by-two matrix shows that there are many states that score in the top half on one variable and the bottom half of the other, suggesting that the two variables are somewhat independent. Nonetheless, perhaps a better way of dealing with the issue in the future is to create an index of competition using the two independent variables and simply note that more competition resulted in higher levels of turnout bias in the 1970s.

[6]See *Allen v. Pataki*, 308 F. Supp. 2d 346 (S.D. N.Y. 2004) and *Marylanders for Fair Representation, Inc. v. Schaefer*, 849 F. Supp. 1022 (D. Md. 1994).

[7]*Cox v. Larios*, 124 S. Ct. 2806 (2004).

[8]Some might argue the U.S. Supreme Court's ruling in *Vieth v. Jubelirer*, 541 U.S. 267 (2004) overturns *Davis v. Bandemer* and precludes federal courts from hearing redistricting cases. It would appear, however, that only a conservative plurality in *Vieth* argued

that redistricting is a nonjusticiable "political" act. In fact, although Justice Kennedy concurred with the majority, his basis for siding with the majority was his belief that the Court has not presented "manageable standards" for pursuing redistricting claims. Justice Kennedy suggested the Court should fashion a set of standards based on the First Amendment's guarantee of free political association.

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