Assessing the Sample Quality of the 1988 Senate Election Study: A Response to Wright

A Technical Report Submitted to the NES Board of Overseers

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1 Introduction

This technical report discusses some hints for users of the 1988 National Elections Study's Senate Election Study (NES/SES). [1] It is organized into four parts. First, I discuss the quality of the 1988 Senate dataset. I show how the sample can be weighted in order to obtain national estimates. These means are compared to ones obtained from the NES pre-post study and the Census. Second, I discuss a potentially serious problem discovered by Gerald Wright — misreport of Senate votes. I present a quick summary of Wright's results and critique his suggested causes. Third, I show how to correct the vote variable, and demonstrate that the consequence of misreporting is much less severe than Wright suggests. Finally, I discuss the problems comparing structural coefficients obtained from very different surveys (specifically, exit polls and the NES/SES survey).

2 How Good is the NES/SES Sample?

The 1978 National Election Study caused a huge increase in the number of papers, articles, and books devoted to Congressional elections. It coincided with a renewed interest in how a member of Congress represents his district,

[1] The data utilized in this paper were made available by the Inter-University Consortium for Political and Social Research. Neither the collector of the original data nor the consortium bears any responsibility for the analyses or interpretations presented here. I would like to thank John Brehm particularly for help and guidance. I am indebted to Graham Kalton, Donald Kinder, Steven Rosenstone, Mike Traugott, Santa Traugott, and Gerald Wright for advice and assistance. They are of course absolved of responsibility as well.

learns norms in Washington, participates in committees, and gets reelected. One result of all this activity, however, was that "congressional" elections came to mean "House elections." Few, if any, introductory political science texts and edited volumes on Congress had anything to say about Senate elections.

Furthermore, what we thought we did know about the Senate turned out to be erroneous due to the way the 1978 NES sample was drawn. Senate challengers were *ceteris paribus*, more visible than House challengers. (c.f. Abramowitz, 1980; Hinckley, 1980; Mann and Wolfinger, 1980) As Westlye showed, sample bias negated the *ceteris paribus* rule: large states were overrepresented in the sample. These same states also tend to have intensely contested Senate seats. In low profile Senate races, challengers are almost as invisible as their House counterparts. (Westlye, 1983, 1987)

Ten years later, the 1988 NES/SES should stimulate a parallel surge in Senate elections research. The sample scheme incorportates states as a strata. This is appropriate, since any study of Senate elections has to take the kind of state explicitly into account in the analysis. The NES has proposed companion studies in 1990 and 1992. This will allow scholars to answer numerous questions relating to representation, the dynamics of the Senate term, and citizen information about sitting and running Senators (just to name a few).

It is important that political scientists do not rush headlong to examine the NES/SES data without an understanding of its limitations. Like any survey, the NES/SES is not without fault. Response rate was very low (43%, compared to 61.9% for the 1984 Rolling Cross Section (also a phone survey) and 70.5% for the 1988 National Election Study). The state samples are far from equal (the final sample size is 3145; the average state sample size is 62.9, ranging from 89 in Maine to 32 in Hawaii). [2] The sample size of voters is smaller still: the average number of presidential voters is 39.68, ranging from 63 in Maine to 23 in Rhode Island and Hawaii. In the most populous state, California, only 26 voters were interviewed. (The average number of Senate voters per state is smaller still, 38.1, and gets as low as 17 in Hawaii.) [3]

Small sample sizes result in inefficient point estimates. An illustration of this problem is shown in Table 1. The table entries show how much error is made when estimating Senate election outcomes using the NES/SES. On average, the percent of votes going to the Republican Senate candidate is 8.8% different from the actual outcome. Misses range as high as 23%. The winner is incorrectly identified in five states (NB, MT, NV, NM, and WA). Estimates for the Presidential race are about as accutate — the average error in Bush's vote percentage is 7.4%; the winner is incorrectly identified in 11 states. Nonetheless,

[2] The cause of the low response rate seems to lie with Market Opinion Research. According to the NES/SES codebook, the MOR staff was not diligent in converting initial refusals.

[3] Comparing response rates across states would be fascinating for what it might reveal about state political cultures, but is impossible. The NES did not release response rates by state. Hopefully the 1990 and 1992 legs of the NES/SES will release more information on the administration for the survey, as they have for the 1988 pre-post election study.

all this does not mean that the NES/SES design was flawed. If political scientists are moving toward a general model of the electoral process, our models should be estimated across states and regions, i.e. state samples should be pooled anyway. The NES/SES is well-designed for this purpose.

Table 1: Errors in Senate Election Predictions By State

State	Miss[1]	State	Miss
Wyoming	-.02	Nebraska	.09
Texas	-.02	Florida	.10
Connecticut	-.02	Hawaii	-.10
New York	.02	West Virginia	.10
Maine	-.02	Rhode Island	.11
California	-.03	Michigan	-.12
Ohio	-.03	Tennessee	-.12
Virginia	-.03	Mississippi	.12
Maryland	-.03	Nevada	.14
New Jersey	-.03	Vermont	.14
Washington	-.04	Pennsylvania	.14
Delaware	.04	Arizona	-.17
Montana	-.04	Massachussets	-.17
Utah	.05	New Mexico	-.20
Wisconsin	-.06	Missouri	.20
Minnesota	.09	Indiana	.23
North Dakota	-.09		

Average "Miss" for Senate Vote Totals = .088

Average "Miss" for Presidential Vote Totals = .074

[1] "Miss" is the proportion of the vote going to the Republican Senatorial candidate (estimated from the NES/SES) - the actual proportion of Republican votes.

2.1 Using NES/SES as a National Survey

Why not take pooling to its logical conclusion — use the NES/SES as a national survey? The NES/SES contains many of the same questions as the NES, but was a telephone survey, so survey methodologists can explore mode effects across the surveys. Voting and public opinion researchers can compare their models across two different surveys. The larger sample size allows more efficient estimates of subgroup opinions.

Scholars taking this path need to make some minor adjustments to the data. Because the Senate study was intended to provide representative samples of the states, weights should be applied when data from a number of states are pooled.

The example I present here is for national estimates. When looking at smaller aggregates (such as the South), the population of the smaller aggregate should be substituted for "National Population" below.

The procedure is simple. Equation 1 accounts for differential probability of selection due to variations in state population:

$$Weight = \frac{Pop.State_i}{(\sum_{i=1}^{50} Pop.State_i)/50} \tag{1}$$

$$Weight = \frac{50 * PopState_i}{NationalPopulation}$$

Note that the voting age population should be used (since only citizens of voting age are eligible respondents), and that DC needs to be taken out of the national total. Estimates are available the Census Current Population Series P-25, No. 1019, Jan. 1988, "Projections of the Population of Voting Age for States: November 1988"

The mean value for *Weight* is not yet 1.0, because the weight is predicated on equal sample sizes in the states, a goal which was badly missed. Equation 2 multiplies the weight by the ratio of the average sample size to each state's sample size:

$$FinalWeight = Weight * \frac{Avg.n}{State_i n} \tag{2}$$

The average state sample is 62.9. For Maine, the weight is multiplied by 62.9/83, for Hawaii, the weight is multiplied by 62.9/32, and so on for the 50 states.

The first two columns of Table 2 show that weighting the data has a negligible impact. A change in the mean of .1 qualifies as a large effect; most changes are in the second decimal point. On demographic measures, where one might expect weighting to have the largest influence (since it forces equal state sample sizes and groups such as Blacks and Hispanics are far from equally distributed across states), weighting brings the NES/SES estimates marginally closer to Census figures. Even with weighting, blacks are underrepresented by almost 50% in the NES/SES.

Table 2: Assorted Comparisons for NES/SES Data

NES Variable	Senate Unweighted	Senate Weighted	NES	Census
Political Interest	2.65	2.60	2.94	
Watch the News	5.00	4.99	4.95	
Read Newspaper	4.26	4.32	3.94	
Party ID	3.03	3.01	2.83	
Ideology	4.47	4.48	4.37	
Reagan Job Approval	2.43	2.44	2.72	
Bush FT	58.54	59.43	60.57	
Dukakis FT	47.43	48.57	56.76	
Bush Ideology	5.23	5.34	5.11	
Dukakis Ideology	3.11	3.08	3.24	
Reagan Ideology	5.45	5.55	5.19	
Democrat Ideology	3.42	3.37	3.33	
Republican Ideology	5.17	5.27	5.19	
Turnout †	72.7%	71.4%	69.2%	57.4%
Vote (1=Bush)	58.2%	57.7%	52.9%	54.0%
Federal Spending:				
Environment	1.46	1.43		
Schools	1.47	1.47		
Social Security	1.45	1.45	1.44	
Food Stamps	2.15	2.11	2.10	
Contra Aid	2.51	2.47	2.48	
Unemployment	1.83	1.80		
Star Wars	2.35	2.35	2.31	
Elderly	1.25	1.23		
AIDS	1.40	1.37	1.33	
Childcare	1.49	1.46		
War on Drugs	1.34	1.31		
Defense	2.23	2.22		
Medical Care	1.38	1.35		
Discrimination	1.78	1.74		
Foreign Imports	2.39	2.40	2.31	
Union (1=yes)	14.9%	15.2%	19.2%	
Age	42.49	42.38	45.01	40.5
Education	12.01	12.11	12.54	12.6

Table 2: Assorted Comparisons for NES/SES Data

NES Variable	Senate Unweighted	Senate Weighted	NES	Census
Race (1=Black)	6.6%	8.3%	13.3%	11.2%
Hispanic (1=yes)	1.6%	1.8%	1.7%	7.1%

† The comparison with the Census is somewhat problematic since the Census includes "don't know" as not voted.

Coding Notes: Missing values are not included in any of the variables, except for: Reagan job approval, "don't know" = 3 (middle category); Party ID, "Other, Apolitical" = 3 (Independent); all federal spending items, "Cut Out Entirely" = 3 (Decreased). Variable ranges are as follows. Political Interest: 1 (high interest) - 5 (low interest). Watch the News and Newspaper Reading: number of Days. Party Id runs from Strong Democrat (0) to Strong Republican (6). Bush and Dukakis FT (feeling thermometers): 100 points, 0="cold" 100="warm." Self-identified and ascribed ideology items run from Strong Liberal (1) to Strong Conservative (7). Turnout is a dummy variable, where 1=voted in the presidential election. Vote is also a dummy, with 1=vote for Bush (3rd party votes are excluded). Feeling thermometers: 100 point scale, where 0="cold" 100="warm." Self-identified and ascribed ideology items run from Strong Liberal (1) to Strong Conservative (7).

How well do national estimates derived from the 1988 Senate Study match up with those from the Census and the 1988 NES? The match is good, especially between the NES/SES and the NES (compare columns one and two to three). Weighting the Senate data moves the means closer to NES estimates (with a few exceptions — four of the ideology measures, opinion on foreign imports, and age). The NES/SES sample appears slightly more politically attuned — they express greater interest in politics and read newspapers more often. In line with Hurley's (1989) finding, the NES/SES sample is distinctly more conservative than the NES sample — more Republican, conservative, and approving of Reagan's job performance. Senate study respondents rate Dukakis more liberal but also rank Reagan and Bush more conservative. They express significantly "cooler" feelings toward Dukakis, while being only marginally cooler toward Bush. Both studies overestimate turnout (10-15%). Again in line with Hurley, the NES/SES sample overestimates Bush's vote, both compared to the NES and to the actual election result.

On demographic measures, the NES/SES sample is less unionized, younger, less well educated, less Black, and less Hispanic than the NES sample. Compared to the Census (column 4), both samples dramatically undercount Hispanics. The Senate study provides worse estimates of education levels, and percent Black; the NES study provides worse estimates of age.

The NES/SES can be used as a national survey. Users may want to do this in order to check models estimated in the NES, or to take advantage of the larger sample size. Weighting is strictly required when producing descriptive statistics; generally, the effects are small enough that weighting is not necessary. Still,

the weighted Senate data looks more like the unweighted Senate data than it does the NES data. Add to this a more conservative pattern of responses in the NES/SES, and this indicates that researchers should be careful when comparing these two datasets. Unanticipated differences may be altering sample estimates (for example, survey mode, questionnaire design, and aspects of administration, such as how easily missing responses were accepted). My recommendation to the National Election Study would be to concentrate on improving response rates among Blacks and Hispanics (assuming this is the cause of the underrepresentation of these groups) and to neither encourage nor discourage the use of weights.

3 Wright's Problem: Overreport of Senate Votes

Even if the NES/SES *sample* is well-designed for the study of Senate elections, Gerald Wright (1989) has questioned the quality of the *data*. He discovered that the pattern of misses in Senate outcomes shown in Table 1 is not random. There is an bias in vote reports towards overreporting voting for the winner. His analysis suggests that voters increasingly err in favor of the winner as the gap between the election date and interview date widens. This effect interacts with the margin of victory (i.e. the larger the margin, the larger the overreport).[4] The consequence, he maintains, is biased coefficients in voting models — the influence of presidential vote on Senate vote is underestimated while candidate-based effects are overestimated. I question Wright's conclusions about the cause and result of misreporting vote. The purported cause has a huge logical hole in it — why should respondents overreport only their Senate vote and not their Presidential or House choices? The effect of overreports — bias in structural coefficients — is not properly established. First, when the source of error is included as an additional regressor, the resulting coefficient estimates do not look at all like Wright suggests they should. Second, the bias is demonstrated by comparing equations estimated with two different datasets — the NES/SES and network exit polls. Nowhere does Wright present an argument about why exit polls should be a standard against which we should measure academic surveys.[5]

[4] This is all explained more fully below. If you must jump ahead, look at equations 3 and 5.

[5] To be fair, the argument is implied, though not explicit: since they are conducted on election day, exit polls are free of time-based errors. As I discuss below, there are many other differences between academic and exit polls besides interview date. Any of these could cause differences in regression coefficients.

3.1 A Review of the Problem [6]

Sample surveys attempt to measure as accurately as possible the attitudes, opinions, and demographic characteristics of their target population. The NES/SES measures, among other things, Senate vote. Ideally, the correlation between vote choices reported by survey respondents and actual election outcomes should be 1.0, disregarding error due to sampling variance. At the state level, this implies that the expected value of a survey estimate of Republican vote percentage is the actual vote percentage. Variation across states in the proportion of respondents who said they voted Republican ought to bear a 1:1 relationship with the actual variation in Republican vote percentages. Let RV_n = mean percentage vote going to the Republican in state n (coded 0 for Democrat and 1 for Republican) and AV_n = the actual percentage Republican Senate vote in state n. If we regress reported vote on actual vote, both measured at the state level, the slope of the regression line ought to be 45° (hence $b_1 = 1$) and there should be no constant over or underreporting (hence the intercept, b_0, should be 0). All this is presented in equation 3.

$$RV_n = b_0 + b_1 AV_n + e \qquad (3)$$
$$If\ E(RV_n) = AV_n\ then \quad E(b_0) = 0\ and\ E(b_1) = 1.0$$

In actual fact, this relationship deviates substantially from expectations — some degree of overreporting for the winner is going on. Figure 1 is a plot of the actual percentage of votes received by Republican Senate candidates against the mean percentage Republican Senate vote in the NES/SES (aggregated by state) with the expected 45° line added for reference. The distribution is tilted, with reported vote increasing faster than actual vote. The estimated slope and intercept, obtained through OLS regression, are reported in Table 3, column 1. Is there overreporting? Based on these results, when actual vote is greater than 65.56, my prediction of reported vote is over 100%. More NES/SES respondents are saying they voted for the winner than actually did, thereby inflating winner vote totals on both ends of the scale. Curiously, the same effect is not apparent for presidential vote (see column 2 of Table 3). The state-state variation in NES/SES presidential election vote percentages matches closely actual percentages.

The same result is obtained when this relationship is estimated at the individual level. Now I am regressing the vote report of the ith repondent (1=Republican) in the nth state against the actual vote in the nth state. [7]

$$RV_{in} = b_0 + b_1 AV_n + e \qquad (4)$$

[6] This section is a summary of Wright (1989) with some additional analyses of my own.

[7] Because of heteroskedasticity due to the dichotomous dependent variable, the individual level regressions are estimated using weighted least squares (Hanushek and Jackson, 1977, p.180-184)

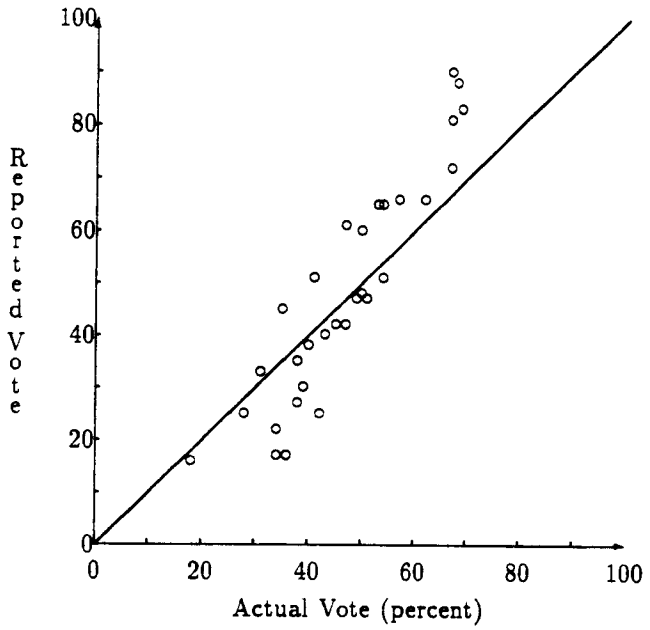

Figure 1: Overreporting Senate Votes

Column 1 of Table 4 indicates that individual respondents report voting for the winner 1.3 times as often as they ought to, if vote reports were unbiased. The effect is not limited to the NES/SES. NES post-election data reveal the same problem (compare columns 1 and 2 of Table 4). Clearly, something is amiss in how Senate votes are being reported.

Wright examines a number of possible causes of overreports — bandwagon (constant bias in favor of the winner), a spiral of silence (respondents fear reporting minority opinions [in this case vote for a losing candidate]), survey instrument (a pro-incumbent bias due to the frequency of queries about incumbents in the survey), and time (learn the winner "in order to know which way to misreport"). Operationalizing and testing each of these alternatives, Wright settles on a combination of effects: the spiral of silence and time:

> The results are consistent with the time effects hypothesis in which there is a spiral, is not of silence, then away from admitting losing votes for Senate candidates. (1989, p. 9-10)

Wright establishes this result by searching for the combination of variables which reduces a and b_1 to their desired values. This occurs when two variables are

Table 3: State Level Evidence – Overreport of Senate Votes

Aggregated Data (States)		
Coefficient	Senate	Presidency
	NES/SES	NES/SES
Constant b_0	-.236	.091
(se)	(.054)	(.119)
Actual Vote b_1	1.529	.892
(se)	(.110)	(.217)
N =	33	50
R^2 =	.849	.245
Standard Error =	.085	.085

Table Notes: Both equations were estimated via ordinary least squares regression. The dependent variable in both cases is the reported vote for the Republican Senate candidate, aggregated to the state level. The independent variable is the actual Republican vote percentage, taken from the Congressional Quarterly. The substantive results are the same when the Presidential equation is estimated only for states holding Senate elections.

added: *Days* and *Days* $*$ *ActualVote*. [8]

$$RV = b_0 + b_1 AV + b_2 Days + b_3 Days * AV + e \quad (5)$$

Voters err in their reports of their Senate vote as the gap between the election and the interview date increases; the size of the error increases in proportion to the size of the winning margin (hence the interaction term in equation 5).[9]

As Wright admits, it is one thing to point to a methodological fault and cry "Error, error!" It is another to show that the error is something to be concerned about. Is this a worrisome situation? Wright thinks it is — overreporting biases coefficients in other equations. He compares the results from regression equations predicting partisan Senate choice using Senate election study data to those using ABC and CBS exit polls. In Wright's words:

> The conclusion from our estimation of the partisan choice model is that coattail effects are much larger in the exit polls; local fac-

[8] *Days* = how long after the election the interview took place, or (Date of interview - Nov. 8).

[9] The coefficient estimates from this equations were obtained via weighted least squares:

RV	=	.022	$+1.055(AV)$	$-.008(Days)$	$+.015(Days * AV)$
		(.088)	(.205)	(.002)	(.006)
N	=	1258	$R^2 = .156$	$SE = .316$	

Table 4: Invidual Level Evidence — Overreport of Senate Votes

Individual Level Data						
Coefficient	Senate		Presidency		House	
	NES/SES	NES	NES/SES	NES	NES/SES	NES
Constant b_0	-.157	-.264	.063	.062	-.011	- .056
(se)	(.035)	(.067)	(.114)	(.163)	(.001)	(.020)
Actual Vote b_1	1.386	1.506	.937	.863	1.007	1.126
(se)	(.075)	(.145)	(.204)	(.304)	(.003)	(.041)
N =	1258	842	1984	1209	1702	1054
Standard Error =	1.03	.465	.491	.498	.045	.114

Table Notes: All equations were estimated via weighted least squares. The dependent variable in both cases is the reported Senate, Presidential, or House vote (where 0 = Democrat and 1 = Republican). The independent variable is the actual Republican vote percentage, taken from the Congressional Quarterly. This variable is identical to the one used in the aggregate analysis, where election results are attached to the respondent record.

> tors, incumbency and candidate spending, are substantially larger in the NES/SES data. This suggests the possibility that the systematic bias in reported vote may work to underestimate the effects of national forces and to overestimate the impact of incumbency/candidate variables in Senate elections. (1989, p. 14)

The relative importance of national and incumbency variables is at the center of academic debate about Congressional elections. If Wright is correct, this is surely a damning critique of the survey method used by the National Election Study. In fact, Wright closes with a call for reorientation of voting surveys, asking them to emulate exit polls: "...the ideal would be huge election-day polls so that we could tap voter attitudes and reports of behavior before they are contaminated." (p. 18) Is this harsh conclusion warranted?

4 Other Conflicting Evidence

I think it is not. My initial question that I would ask, as Wright did himself, is whether overreporting occurs in other races (Wright only looks at the presidency; I add the House). The answer is that it does not. As shown in Wright's paper (and confirmed in columns 3 through 4 of Table 4), individual reports of Presidential votes do not suffer from overreporting. House votes appear to suffer from minor overreports, but nowhere as severe as the Senate (columns 5 and 6). This evidence does not fit at all with Wright's suggested cause. Wright explains the time effect this way:

> In the days that pass between the election and the interview, the respondent is exposed to post-election coverage and to social interactions which provide information and pressures that can affect recall and ... overall image of the election. (p. 7)

If information about the election outcome is influencing respondents to misreport the vote, surely the presidential results should be more prone to error than the Senate, not less so. The amount of news coverage on the presidential outcome is intense, far outweighing Senate coverage. If "abandonment of the loser" is what is occurring, I certainly would expect Dukakis voters to join in the fun. Any suggested cause would have to explain why only Senate results are affected. I do not think Wright's account does this. [10]

Furthermore, Republican vote percentage here is simply a way to get at the real issue — was there overreporting in favor of the *winner*. Suppose we take winner's vote percentage as the variable of interest. The content of Wright's results ought to be unchanged. At the aggregate level, the bias remains. When the analysis is performed at the individual level, however, the bias disappears (see Table 5). This result is both striking and confusing. By reexpressing the dependent and independent variables in line with the theoretical model, the pattern of results change dramatically, but only at one level of analysis.

Table 5: Overreport of Senate "Winner's" Votes?

Coefficient	Winner, Aggregated Data	Winner, Individual Level
Constant b_0	-.157	-.021
(se)	(.115)	(.081)
Actual Vote b_1	1.35	1.12
(se)	(.188)	(.125)
N =	33	1258
Standard Error =	.09	.44

Table Notes: The aggregate level equation was estimated via OLS, the individual level equation via WLS. The dependent variable in both cases is the vote for the Senate winner (at the individual level, 1 = voted for the winner, 0 = voted for the loser; in the first equation, these reports are aggregated to the state level). The independent variable is the actual winner's vote percentage, taken from the Congressional Quarterly. Both these variables are simple tranformations of the variables from the previous table.

[10] I wondered whether we might be witnessing an outlier effect — perhaps overreports were concentrated in a few unique states. This failed to pan out. We already saw misses by state (shown above, in Table 1). If anyone can tell me what features Indiana,Missouri, New Mexico, Massachusets, and Arizona (the five largest) share in common, I'd be interested in hearing it. I took these five states out of the dataset and ran the aggregate and individual regressions — the bias is still pronounced.

4.1 Correcting for Overreports

Nonetheless, Wright has identified a problem. Is there any way to correct it, beyond his suggestion of switching to huge surveys on election day? I think there is a less drastic alternative. Wright's proposed representation of the bias problem was shown above (equation 5). When descriptive statistics on Senate votes are desired, coefficient estimates from this equation can be used to take out the time-induced bias. Rearrangement of the equation, moving AV to the left hand side, results in equation 6, with $\hat{RV}$ substituted for AV. When multivariate analysis is used, the date variables should be added as additional regressors.

$$\hat{RV} = \frac{RV}{b_1 + b_3 * Days} - \frac{b_0}{b_1 + b_3 * Days} - \frac{b_2 * Days}{b_1 + b_3 * Days} \quad (6)$$

Figure 2 is a reproduction of Figure 1, this time plotting corrected Senate vote (aggregated to the state level) against actual Senate vote. The cloud of points reveals no evident bias. The new variable, when reexpressed as percent voting for the winner, has a mean closer to the true value (see below). The correction works.

Winner's Vote Percentage:

	Actual	NES/SES (corrected)	NES/SES (uncorrected)
Mean	60.9	61.5	66.0

There is a more fundamental issue here, however. Does the bias really effect the coefficients in voting models – do we really have to worry about this? Wright implicitly claims that exit poll data are "correct" and that NES/SES data are "wrong." If so, the coefficients from a vote equation, once the additional regressors are added, ought to look like those obtained from exit poll data. [11]

Instead, the coefficients are virtually identical (see Table 6). Adding additional regressors to account for the bias makes no difference in the results. The conclusions concerning the relative impact of presidential preference and candidate factors remain the same. The overreporting bias that Wright discovered does not alter our substantive conclusions about Senate voting.

[11] Table 5 in Wright compares vote models estimated using NES/SES data and exit poll data. Part of that table is reproduced here (all variables are described in the notes to Table 6). The coefficients on presidential vote are much larger in the exit poll equations while the coefficients on candidate based measures (incumbency and spending) are much smaller. CBS exit poll data shows similar patterns.

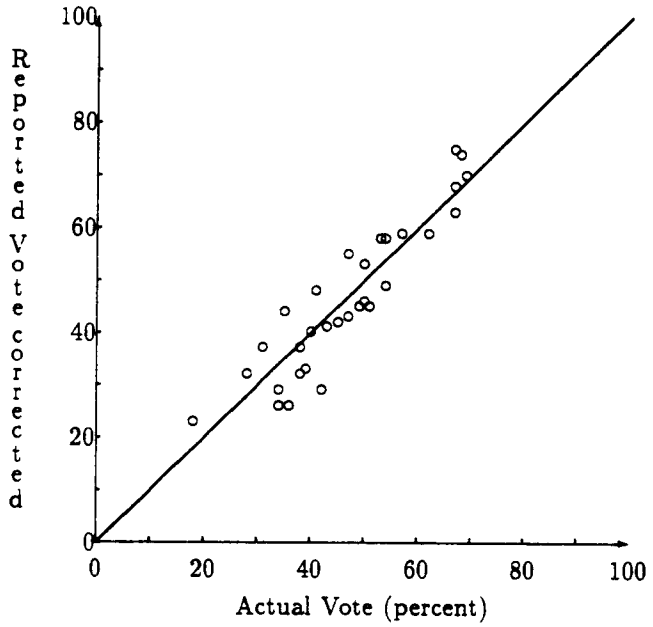

Figure 2: Fixing the Bias in Vote Reports

Some other difference between the ABC and CBS exit polls and the NES/SES besides date of interview (and resulting bias in vote reports) must be causing the differences in coefficients reported by Wright. Unfortunately, there is no obvious reason for the differences Wright observed. I cannot believe that overreporting is the culprit until a series of alternative hypotheses are eliminated. What is the demographic makeup of the CBS and ABC exit poll sample — are the exit poll and NES/SES samples comparable? If the effect of overreporting is to bias regression coefficients, then regression models for races where overreporting is *not* a problem, e.g. the presidential race, should look the same across these studies. Is this true? (Wright does not report comparable analyses for the House and Presidency.) Most important, why does time affect the Senate and not the House or Presidency?

Wright Vote Equations		
	NES/SES	ABC News
Party ID	.509	.528
Pres. Vote	.742	1.20
Log Dem	-.371	-.271
Log Rep	.301	.256
Dem Inc	-.368	-.117
Rep Inc	.015	-.011
constant	-.226	-.590
R^2 =	.475	.529
N =	1258	63,855

Table 6: Vote Equations: Without and With Bias Variables

Variables	Wright Model	Adding Regressors
Party ID	.341 (.061)	.340 (.061)
Presidential Vote	.789 (.121)	.791 (.121)
Log Democ. Spending	-.371 (.053)	-.364 (.053)
Log Repub. Spending	.326 (.045)	.316 (.047)
Democr. Incumbent	-.377 (.123)	-.381 (.124)
Republ. Incumbent	-.079 (.133)	-.088 (.134)
Date	—	-.041 (.026)
Date * Actual Vote	—	.091 (.055)
Constant	.108 (.319)	.121 (.312)
Maddala R^2 =	.311	.313
Log-likelihood =	-788.41	-788.41
Perc. correctly predicted.	72.2	72.6
N of cases	1138	1138

Table Notes: Because the dependent variable - partisan vote choice (0=Democrat and 1=Republican) - is a dichotomy, these are all probit estimates. Party identification is a three point scale, with Strong Democrat through Independent leaning Democrat coded into 1, pure Independents coded into 2, and Independent leaning Republican to Strong Republican coded 3. Presidential vote is a dichotomy (0=Dukakis, 1=Bush). Democratic and Republican spending are logged per capita measures: $\ln((Campaignspending)/statepopulation)$. Democratic and Republican incumbency are dummies. Asymptotic standard errors are in parentheses. These estimates differ in minor ways from those reported by Wright, due to a different coding of partisanship and a different handling of missing data (his N = 1258).

5 Comparing Exit Polls with Academic Surveys

What we are seeing here, I think, is the danger in comparing two very different polls. Is there good reason to suppose that exit polls are better measures of voter opinion than the NES/SES poll? Plissner and Mitofsky (1982) have likened participation in an exit poll to voting a second time. In an important way, they are correct: the exit poll becomes part of the single activity of voting. This improves the reliability of exit poll data. Every respondent to an exit poll is a voter. The exit poll minimizes the effect of history, be it contamination from post-election coverage, social interactions, rationalizations, bandwagons (since the winner is not yet known), or simple forgetting.

There are reasons to suspect exit polls. Some problems relate to administration. There is some attempt to convert initial refusals, but nothing comparable to the multiple callbacks a well-run academic poll will employ (this can cause serious bias in political measures, particularly those relating to participation,

see e.g. Brehm, 1990). News deadlines mean there is little time to adjust to problems which might arise during interviewing. Other concerns relate to the sample. Sampling within strata is done in proportion to either the total number of votes case in some "base year" or in proportion to current voter registration. (Levy, 1983; Mitofsky and Waksberg, 1989) Large variations in turnout relative to either figure would result in unequal selection probabilities. News organizations differ in their treatment of non-response: some instruct the interviewer to take the next available voter, thus converting and every kth voter skip pattern to an every $k+1$ pattern. (Levy, 1983) Finally, Mitofsky and Waksberg notice a slight but persistent overreporting bias in favor of Democrats. (1989, p. 16-17)

Exit polls provide highly reliable measures of election results. Response rates (around 75%) are comparable to academic surveys, and are higher than many telephone polls such as the NES/SES. Wright is joined by Levy (1983) when he claims that exit polls measure voters' attitudes, opinions, and actual vote choice better than academic polls such as the National Election Study.

Nonetheless, academic polls should not be replaced by huge election day polls. The central purposes of an exit poll and an academic poll like the NES/SES diverge. An exit poll provides a snapshot of voter opinions as they leave the polling booth. It allows news organizations, and the public, to learn the outcome of the election quickly. It provides limited information about the reasons behind the vote. What is gains in speed and sample size it loses in breadth and sample reliability. The NES/SES survey instrument ran, on average, 35 minutes. It asked questions about sitting Senators not up for reelection, contacts with Senators and House members, political interest, media use, reactions to prominent political figures, and much more. The sample is carefully drawn, and great effort is expended to complete each interview. [12]

Where the exit poll questionnaire is driven by the need to construct a compelling account on television news and next morning's byline, the academic poll is driven by questions and issues that concern the scholarly community. At a 1985 meeting of the Midwest Political Science Association I attended, the 1984 Rolling Cross Section was criticized because it did not ask the "right" questions about the 1984 campaign. Academics criticized the media polls because they asked *only* the "right" questions and little else. Neither situation is ideal.

6 Future Directions

I can think of two possible ways to fit Wright's account in with the pattern of empirical results. One cause of overreports could be the survey instrument. This account draws on Wright's story about post-election coverage. Over time, the name of the Senate winner receives prominent play in the media while the loser leaves the public stage. Over time, then, information about the winner, partic-

[12] Admittedly, the NES/SES fell short in this area. Hopefully this will be improved upon in 1990, and response rates can return to the 70% level reached in 1984

ularly the name, is readily accessible in people's minds, while the loser's name becomes less and less available (particularly declining relative to the winner). In this scenario, any information provided within a survey which stimulates recall of the winner's name might result in misreports. The NES provides just this kind of stimulus, and only for House and Senate races: for these contests, the candidate names are provided. In our attempts to simulate the polling booth, we may be unintentionally biasing vote reports. This accounts for the appearance of overreporting only for the Senate and House. The weaker effect for House voting is a result of the heavier coverage given to the Senate elections. Time comes into play because the relative availability of the winner's versus the loser's name increases as time passes.

A second possibility relies on a model of the process of misreporting. [13] There are three variables which determine the scope of misreporting: confusion, exaggeration, and the election result. Voters may be confused about who they voted for. This would vary based on the interest in the election and the cognitive effort the voter put into the decision. Voters also can exaggerate in favor of the winner due to social pressures (desire to be with the winner, conform to the majority's choice, etc.). Finally, the possibility of misreports is affected by the probability that a respondent could have voted for the loser. This is determined, of course, by the actual election result.

To start, I assume that misreporting m, for the ith individual, is affected only by confusion and exaggeration. Therefore, the probability of misreports is equal to the joint probability of confusion and exaggeration:

$$Pr(m_i) = Pr(Conf) + Pr(Exag) - Pr(Conf) * Pr(Exag)$$

Next, I assume that overreporting operates in one direction, toward the winner. You can only misreport if you voted for the loser:

$$Pr(m_i) = Pr(Loser) * (Pr(Conf) + Pr(Exag) - Pr(Conf) * Pr(Exag))$$

Wright's account includes only the first two factors: confusion (forgetting the vote) and exaggeration (as a result of time, media coverage, and associated social pressures). The probability of voting for the loser is left out, yet plays a critical role. In the extreme case, voters cannot misreport voting for the winner when a candidate ran unopposed. More practically, the distribution of winning percentages by state assumes a relatively normal shape for the Presidency and the Senate, i.e. there are not many blowouts. The House distribution is trimodal — a fair number of close races and a large number of Democratic and Republican blowouts. For blowouts, the likelihood that you could have voted for the loser is low; therefore, the likelihood of misreporting is also low. [14]

I combine this observation with conjectural mean values on the other variables in Table 7. For this illustration, I have used three values in the cell entries,

[13] John Brehm gracefully clarified my thoughts in this section.

[14] For discussion of these distributions, see Jacobson, 1987 or 1989

Table 7: A Speculative Model of Misreporting

	Pr(Confusion)	Pr(Exagg)	Pr(Vote Loser)	Pr(Overreport)
Presidency †	.25	.75	.5	.59
Senate ††	.5	.5	.5	.625
House †††	.75	.25	.25	.30

For illustration, I have used three values, low (.25), medium (.5) and high (.75) in the cell entries.

† For the presidential race, I assume that voters were most interested in this race. Therefore I suppose that *confusion* is low. I assume that *exaggeration* is high because media coverage is heaviest. Finally, the mean percentage vote won by Dukakis was 45%, with few blowouts – *loser vote* is scored medium.

†† Voters are generally less interested in Senate races than in the presidential race, but more so that in House contests. I score likelihood of *confusion* medium. Media coverage of the Senate results also falls in between the Presidency and House(Westlye, 1987); I score *exaggeration* medium. Finally, the distribution of Senate loser vote percentage centers on 39%, with few blowouts – I score *loser votes* medium.

††† For the House, I score *confusion* high, since voter interest and involvement in these races has traditionally been low. I score *exaggeration* low as well, since there is little coverage of the outcome, certainly nothing rivaling the Presidential or Senate results. Finally, I score *loser vote* low since there are many lopsided House elections.

low (.25), medium (.5) and high (.75). These particular values are arbitrary. The ranking of the institutions, however, is not. A good argument can be made for each cell value (the logic for cell entries is contained in the table notes).

Though purely speculative, the analysis targets Senate voting as most likely to suffer from overreports. On many dimensions, the Senate falls in between the House and the Presidency — citizen interest, campaign intensity, media coverage, institutional visibility (this is one reason why studying Senate elections holds so much promise for improving our understanding of electoral behavior). Moderately high citizen interest (resulting in less confusion) combined with moderately high media coverage (increasing the potential for exaggeration) results, ironically, in the highest probability of overreports. Less to its credit, the analysis indicates that the Presidential race should suffer from overreports more than the House, a result not supported by the empirical findings (though individual level results on this point were inconclusive). Also, the predicted probability of overreporting Presidential vote is not much smaller than Senate vote, whereas the observed difference is large. Obviously I could jiggle with number to make the results come out cleaner, but that would obscure rather than clarify. Suffice it to say, the next step is to insert real values into this table.

This model of overreporting implicates both systemic and individual level influences. It suggests where researchers might profitably look for causes of misreporting. Individual level variables such as education and political interest

determine the likelihood of confusion; attentiveness to campaign coverage and group affiliations will effect exaggeration. At the same time, the content of post-election coverage and post-election discussion in a community will determine, in part, the degree of exaggeration. To measure voting for the loser, we could even disaggregate Senate and Presidential outcomes to smaller areas — what was the margin in county A or congressional district B? Finally, most of the causes suggested here can be estimated using data already available — levels of political interest, education, and media usage, and group affiliation can be obtained from survey data; election results are publicly available. The missing component is the content of post-election coverage. The model and attached table suggest a more complex model of misreporting could be worthwhile to pursue.

7 Conclusion

The 1988 Senate study is an invaluable resource for congressional and electoral scholars. I would feel more comfortable with an improved response rates, but the comparisons to the NES show that, nationally at least, the Senate study is accurate. On some measures, it does better than the NES when compared to the Census. Highest on my list of priorities would be to improve the non-response conversion rate and improve Black and Hispanic response rates. In most respects, the Senate study performs neither better nor worse than the NES. Multivariate relationships can be compared across the datasets.

The most serious problem with the study is the small samples of voters and the errors in vote reports first discovered by Wright. Just as the 1984 Rolling Cross Section was criticized for the small sample sizes in each week, the Senate study seems vulnerable to criticism for small state n's. More work needs to be done on the reasons for the overreport of Senate outcomes. Although I have suggested a correction for the error here, I cannot think of any good reason for the overreports which implicates the Senate alone. I do not agree with Wright, however, when he suggests overreporting is biasing regression coefficients, and when he recommends large, election-day polls as a solution to the problem.

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