

SOURCES OF PRO-INCUMBENT BIAS
IN N.E.S. SURVEY ESTIMATES FOR U.S. HOUSE RACES
SINCE 1978: A Second Look

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INTRODUCTION: NES and the Pro-Incumbent Bias

For more than forty years the Institute for Social Research has been conducting its National Election Studies (NES), large-scale biennial national surveys whose purpose it is to learn what we can about voting behavior in American national elections. Since the program's inception the surveys have grown in length, scope, complexity and sophistication as issues of sampling, instrument design, interview techniques, validity of methods, and reliability of data have been dealt with.

During most of this period, survey estimates of voting percentages for national office have closely matched aggregate estimates, a fact which has made the wide range of social science practitioners who regularly use those data comfortable that responses for other variables are similarly representative or

unbiased. Starting with the 1978 NES, however, a shift occurred that began to call into question the reliability of survey estimates of the most basic variable -- for whom did the respondent vote?

Eubank and Gow, and Question Order Priming

In two articles published in the American Journal of Political Science in 1983, John Eubank and David Gow brought to our attention their disturbing findings that, in the NES surveys for 1978, 1980 and 1982, survey estimates of percentage vote for incumbent in contested House races, is substantially higher than the actual vote figures.¹ This was true when NES estimates were compared both with figures for the whole country, and from only NES sample districts.

As can be seen in Table 1, from 1970 through 1976 the difference between reported and actual vote for incumbents was relatively small, ranging from -1.4% in 1974 (where reported was actually somewhat lower than actual vote) to 4.2% in 1972. In 1978, however, that figure skyrocketed to 11.5%, followed by 5.5% in 1980 and 9.1% in 1982. It appeared that bias of some kind had entered into the survey measurement, starting in 1978.

Eubank and Gow concluded that the newly discovered bias was the result of a new and extensive battery of questions about the incumbent having been added to the survey instrument, positioned before the vote questions. These questions, in which the

1 Eubank, John, and David Gow. 1983. The Pro-Incumbent Bias in the 1978 and 1980 National Election Studies. American Journal of Political Science 27 (February 1983): 122-139.

respondent was repeatedly reminded who the incumbent was and asked to remember things about him or her, were hypothesized by Eubank and Gow to act as a priming mechanism that made respondents more likely to report having voted for the incumbent. This argument, because of its intuitive nature and due to the coincidence of the timing of the inclusion of the new questions with the increase in bias, gained widespread attention, if not acceptance, and calls were made to change the question order.

The question order was in fact changed for the 1984 survey, where the voting question was now placed before any questions about the incumbent; from 1986 on, the voting question was asked after the campaign contact questions (in which both the incumbent and the challenger are given equal weight) but before the questions specifically about the incumbent. These changes make possible a very strong test of Eubank and Gow's hypothesis: if they were right, then the bias should have returned in 1984 and subsequently to pre-1978 levels. On the other hand, if the bias were to remain at high levels in spite of the 1984 question order change, that could be taken as almost proof positive that the bias was not a result of question order priming at all.

A look at the numbers in our extension of Eubank and Gow's data shows that the latter is indeed the case. Using Eubank and Gow's own methods (see notes for Table 1) we found that pro-incumbent bias remained at high levels even after the changes in question order. The numbers for 1984 through 1988 respectively are 7.28%, 7.99%, and 9.63% (a post-1978 high!).² It seems very

2 Results from Senate races are similarly damaging to Eubank and Gow's argument. The 1978 Senate races also showed a level of pro-incumbent bias somewhat lower than that in the

clear that if we want to get at the true causes for the bias we must look elsewhere.

What we are looking for, then, is some new factor that will explain both the large increase in pro-incumbent bias that started in 1978 and the fact that the bias remained even after the question order was changed to remove the possibility of priming. If question order per se is no longer the prime suspect we need to look for another source of the bias. A suspect is at hand, in the form of the new "mock ballot card" that was also added to the survey instrument in 1978.

Table 1. Pro-Incumbent Bias in House Races, 1970-1988

Year	N	Survey Estimate	Actual Vote	Difference
1970	535	67.7	65.8	1.9
1972	970	69.5	65.3	4.2
1974*	826	63.0	64.4	-1.4
1976*	1058	69.6	67.0	2.6
1978	761	78.8	67.3	11.5
1980	693	72.4	66.9	5.5
1982	527	71.3	62.2	9.1
1984	962	72.9	65.6	7.3
1986	701	77.3	69.3	8.0
1988	751	78.7	69.1	9.6

*N's and estimates for these years are weighted. Figures from 1970 through 1982 are from Eubank and Gow's two articles. The numbers from 1984 through 1986 are from research conducted in 1990 by Joel Bloom and Peter Muhlberger using NES data. "Actual Vote" is for survey districts only.³

House, and in 1980, after the Senate incumbent questions were removed entirely that bias remained unchanged.

- 3 We included only voters in contested races with an incumbent. Prior to 1978 there was no "type of race" variable included in the survey data, so Eubank and Gow used their own criteria during that period. Last year, that variable was retroactively added for all years before 1978.

The Mock Ballot Card as Source of Pro-Incumbent Bias

The battery of questions on Congressional incumbents was not the only major change in the NES survey to be initiated in 1978. The survey that year was a marked effort to dig deeper into questions about House races that had largely been ignored in previous surveys. One change that was made that year was to conduct sampling by congressional districts rather than SMSA's so as to facilitate analysis at the district level, but there is no reason to think that could have caused the bias, and like the question order, that practice has since been changed as well.

By far the most important change in the survey that year was one that came in response to concerns over an entirely different issue. A great deal of research over the years has demonstrated very thoroughly that citizens are much better able to recognize the name of a candidate when presented with alternatives than they are to recall a name on their own. And yet, the single most important question on the NES surveys, "who did you vote for for

In our attempts to duplicate their findings we had varied success. For 1978-1982, since we were using the same screening (type of race) variables our N's and our survey estimate of vote are virtually identical. Prior to 1978, where we used different standards for filtering cases, both the N's and the estimates vary a good deal more. As for the "actual vote" figures for survey districts, Eubank and Gow's articles did not inform us as to how their figures were derived -- whether they counted each district equally, weighted them by number of respondents per district, or by number of total votes in the district. Our figures are de facto weighted by number of respondents since a district's actual vote is counted one time for each respondent in the survey from that district. Actual vote figures came from ISR's "Candidate Name" data base, but were altered somewhat when we found some discrepancies in several cases and inaccurate figures for New York State. Our changes led to a net increase in incumbent's vote of approximately 0.5%.

X office?" had always required recall. This was problematic for two reasons: first, that noted above, that relying on recall might increase the chances of misreport; second, that the voting choice as presented in the survey was nothing like the real act of voting, which is theoretically one of recognition rather than recall, (to the extent that it is not simply done by partisan identification).

To rectify this situation, starting in 1978, NES surveys have used a mock ballot card with the names and parties of all candidates from the respondent's district. This was supposed to both make the survey process more similar to the actual experience of voting, and, by simplifying the tasks involved, make more accurate reporting possible. Yet, ironically, the introduction of the mock ballot card coincided not with greater accuracy but with greater bias in the voting reports: it may well be asked why this is the case.

To begin to understand this puzzle it will be helpful if we can determine among which group of voters the "misreport" of vote in favor of incumbents is occurring. We may reasonably surmise that voters identifying with the incumbent's party would have little or no reason to defect to the challenger, but that challenger partisans might indeed be fairly likely to vote for the incumbent. Thus we would expect to see relatively high defection rates among challenger partisans, but strong partisan voting among incumbent partisans. In addition, while challenger partisans who actually voted for the challenger might misreport by claiming to have voted for the incumbent, due to bandwagon effect, there is no similar argument that would seem to lead a

loyal incumbent partisan to claim a vote for the challenger. Thus, it would seem that any bias in vote reporting in favor of incumbents would very likely be concentrated among challenger partisans.

Table 2, below, shows several interesting patterns that, when combined with the above assumptions, provide some interesting insight into the sources of pro-incumbent survey bias. The first thing that is immediately evident is that among incumbent partisans there is astonishing stability over this period of more than thirty years. While there is some variation

Table 2. Percentage Reported Vote for Incumbent among Incumbent and Challenger Partisans.

Year	Incumbent Partisans		Challenger Partisans	
	N	%	N	%
1956	528	93.6	344	15.1
1958	401	93.0	299	16.1
1960	521	91.6	381	18.6
1964	435	89.2	315	24.8
1966	296	91.2	205	33.7
1968	361	87.0	297	32.3
1970	290	93.8	222	30.6
1972	542	90.0	347	37.2
1974	402	88.6	306	35.0
1976	555	89.5	449	38.4
1978	408	95.6	276	53.3
1980	375	90.4	273	48.4
1982	293	91.7	195	35.9
1984	516	91.7	377	47.2
1986	418	93.8	242	48.8
1988	428	97.0	265	49.1

Figures from 1956-1968 and 1972 are from Eubank and Gow's Table 1 (Feb. 1983). Figures for all other years are from NES, and Bloom and Muhlberger's research. Again, only contested races with incumbents appear, and this time only voters who expressed partisan identification were included (independent leaners were included as partisans). Again, numbers for 1974 and 1976 are weighted.

from year to year, in no case did incumbent partisans report voting for the incumbent by less than 88.6% (1974) and in no case was it more than 97.0% (1988), with the typical figure in the low 90% range. And there is no pattern of increase or decrease over the period included in the table.

The second fact that is clearly evident in the numbers of Table 2, is that the exact opposite is true of reported votes for challenger partisans. The data for challenger partisans seems to divide itself into three highly distinct periods: 1) a period of low but gradually rising defection from 15.1% in 1956 to 24.8% in 1964 and then 33.7% in 1966. 2) Then defection rates remain highly stable in this intermediate period between 1966 and 1976, a period marked by only minor fluctuations. 3) Finally, beginning in 1978 we see an abrupt increase in defections to 53.3%, followed by a stabilization at a high level of defection in the high 40% range (with the singular exception of 1982). Thus, we have a period of low but gradually rising defection, a period of stable mid-level defection, then an abrupt increase, followed by a stabilization at that high level. Indeed, between 1984 and 1988 there is virtually no variation at all.

What can we learn from these figures? First of all, if the much-noted decline in partisanship really has occurred during this period, it seems to have taken place exclusively among challenger partisans -- incumbent partisans seems to be as loyal as ever. Secondly, because prior increases in defection among challenger partisans occurred quite gradually, the very sudden jump in 1978 does seem to lend itself toward interpretation as

some sort of aberration, rather than as the result of some natural change in the political environment.

Indeed, while the change between 1956 and 1966 has the look of a secular phenomenon which can easily be explained by the decline in partisanship during the period, the 1978 blip defies such attempts at explanation. For one, all measures of partisanship showed gradual decline during the fifties, sixties and early seventies; but the same measures show a leveling off from the late seventies on.⁴ By all accounts it looks as if Americans have found a level neither of partisanship nor of non-partisanship, but perhaps of semi-partisanship with which they are quite comfortable. In addition, a quick look back at Table 1 will show that incumbent's actually did not increase their percentage of vote during this time. Thus, we should expect stability in defection rates, not the tremendous increase that actually happened.

Assuming that we can dismiss any theories to the effect that some factor in American politics arose in 1978 that suddenly made challenger partisans into liars, then, we can safely look to the survey itself as the unwitting instrument of that change. As previously stated, we are looking for a major change in the survey itself: one that first appeared in 1978 and remained until the present. Eubank and Gow thought they had found the culprit in a purported priming effect from new questions about the incumbent, but this theory was disproved when changes in the

4 See The Changing American Voter and Martin Wattenberg's The Decline of American Political Parties 1952-1988. (1990, Harvard University Press).

question order did not result in a return to prior levels of low bias.

The introduction of the mock ballot card, on the other hand meets all the above requirements -- it is a major change, it first appeared in 1978, and it is still in place. The only thing left to do now, before throwing the matter open for future research, is to present an argument as to why the ballot card might lead some people who really voted for the challenger to say that they voted for the incumbent.

The most simple and intuitive explanation is that the ballot cards, in addition to facilitating recall of the respondent's vote, may have also exacerbate the bandwagon effect among the least politically aware group of challenger partisans. First of all, it should be remembered that NES surveys are generally conducted anywhere from a week or two to several weeks after the elections. Since House races are of relatively low salience to most voters, it is hard to imagine that a month or two after election day everyone would clearly remember for whom they voted.

By relying on their own recollection prior to 1978 challenger partisan voters in this category who voted for the challenger, when forced to recall their vote would probably have reported a partisan vote unless they had heard a lot about the incumbent since the election (and since, as we have seen, nearly 70% of challenger partisans voted their partisanship prior to 1978, this is a large group, as well of the one of greatest interest to understanding pro-incumbent misreport). On the other hand, presented with a mock ballot card the respondent would only need to have been exposed to a small amount of information about

the incumbent as winner to trigger the bandwagon effect, either through genuine forgetfulness ("I've heard of this guy, so he must be the one I voted for") or through desire to be with a winner, remembering that in races they enter incumbents win around 96% of the time.

This attempt at explanation is by no means intended to apply to all voters, or even all low-salience, low-information voters. The point is simply that, 1) some of the less politically aware challenger partisans who did vote for the challenger may have the need to look for cues (or clues) to help them remember for whom they voted; 2) the cues available both before and since 1978 are primarily partisanship and media exposure; 3) prior to the introduction of the ballot card the need for recall made partisanship the strongest cue; 4) with the ballot card, it now became easy for the respondent to recognize the incumbent (read winner), kicking into action the bandwagon effect. Thus, while the cues remain the same, the introduction of the ballot card fundamentally altered their relative importance: partisanship is still the most important cue, but now large numbers of challenger partisans who voted their partisanship are responding to the other cue of incumbency/winner/bandwagon effect.

Conclusions and Implications

In summation, the pro-incumbent bias in NES House surveys cannot be explained by a priming effect of incumbent-related questions as Eubank and Gow hypothesized. We propose that the

bias was caused instead by the use of the mock ballot card, whose purpose, ironically, was to reduce error in reporting.

The mock ballot card was the only major change in the survey that both was initiated in 1978 and has remained in place since then. Finally, we proposed a mechanism by which the ballot card might cause some voters to falsely report having voted for the incumbent, due to a relative weakening of the partisanship cue and a relative strengthening of the bandwagon effect.

An interesting, and easily testable corollary to this hypothesis is that this effect, would be stronger in states which have the mechanism of straight partisan voting in which there is the option to vote a straight party ticket by pulling a single lever. This mechanism would first of all presumably lead to lower defection rates among both incumbent and challenger partisans, and particularly among the groups of voters of most interest here theoretically -- the low information voter looking to make his or her decision easier. Secondly, challenger partisans in that latter group, and who never even pulled a lever specifically for the House race, would be the most likely to rely on cues both internal (partisanship) and external (media, etc.) to aid them in their recall or recognition of their vote. Thus, two factors in states with straight-ticket voting -- higher levels of partisan voting, and less real consciousness of that voting -- would seem to make challenger partisans in those states more likely to misreport voting for the incumbent than in states where a separate lever must be pushed for each place on the ballot.

Another topic of interest to this discussion is the question of why we are even presuming an effect of incumbency. Eubank and Gow presumed it because their argument was based on the impact of questions in the survey about the incumbent. In testing their hypothesis, which entailed duplicating their methods, we were thus in effect stuck looking at the issue in terms of incumbency, even though as pointed out earlier, it seems evident now that incumbency is really just a surrogate for the real issue, of winning. And a quick examination of NES figures based on vote for winner shows that the pro-winner bias is, not surprisingly, virtually indistinguishable from pro-incumbent bias. One easy test, then, of whether the key variable is really incumbency or winning, would be to look only at open seats and see if there is still a pro-winner bias (although this would not be entirely fair given the possible intervention of the lower margins of victory in open races).

A more sophisticated test would involve an examination of the reasons we would expect an incumbency effect -- mainly the incumbent's development over time of a reputation in his or her district. At the individual level, the key here is the length of time the incumbent and the voter have existed as a pair. This incumbent/voter pairing is affected not only by turnover in elections, but also by voter mobility in and out of the district, and changes in district boundaries due to redistricting. With this in mind, an incumbency-effect model would require incumbency effect to increase with the duration of the incumbent/voter pair. If it could be shown, then, that pro-incumbent bias is greater in cases of long-term pairing than in short-term this would be a

strong argument in favor of incumbency as a source of the error (although it is quite possible that both incumbency and winning contribute).