

If Turnout Is So Low, Why Do So Many People Say They Vote?

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Aggregate survey estimates of voter participation rates generally exceed actual voter turnout rates, and sometimes by quite a lot. Figures 1 and 2 show that turnout estimates of both the National Election Studies Series and the Census Bureau Current Population Surveys always exceed the actual proportion of ballots cast by Americans who are eligible to vote in Presidential and midterm elections. For example, while 60% of voting eligible population of the United States actually cast ballots in the 2004 Presidential election, 77% of National Election Study respondents and 64% of CPS citizen respondents said that they had voted. Several factors that contribute to the inflated reports of voter participation in surveys are well understood in the social science literature, including *measurement error*, *panel attrition*, *panel conditioning*, and *non-random sampling errors (due to biases in contacting and responses)*. However, these have generally been addressed one at a time. This essay will highlight the disparate causes of inflated survey reports of participation, and use various NES data to provide an assessment of the magnitude of the various effects that result in survey overestimates.

Figures 1 and 2 about here

Measurement Error

The most obvious factor contributing to survey overestimates of turnout is *measurement error*, or more specifically *over-reporting* (false positive responses) by people who actually failed to vote. (Abramson and Claggett 1984; Bernstein, Chadha, and Montjoy 2000; Cassel 2003) A number of factors may influence non-voters to tell survey interviewers that they voted, including *social desirability* (due to embarrassment of admitting a violation of an expected norm of participating in the political process, see Karp and Brockington 2005), faulty *memory* (especially in the case of a ritual voter who may have forgotten that an unusual event prevented him or her from casting a ballot in the last election), or *misinterpretation* of the question. Casual observers of this

literature often assume that this measurement error is the only source of the survey overestimates.

How much of the survey overestimates of turnout are due to nonvoters misreporting participation? The best (but still imperfect) estimates are from the NES Voter Validation Studies, conducted for nine election years (including five presidential election years) between 1964 and 1988. The procedures for validation evolved over this twenty-four year period, but in general, NES interviewers attempted to locate and record the official registration and voter participation records in county election offices. Over-reports of voting are indicated when the official records indicate that a respondent who reported voting was either not registered or was registered and had failed to cast a ballot. Traugott (1989, 9) calculates a “confirmed” NES voter turnout estimate by reclassifying self-reported voters as nonvoters when official records indicate that they did not vote, but accepting self-reports of voting as valid when official records could not be found. Table 1 shows the proportion of the NES respondents who misreport voting (and the proportion of the NES overestimate that could be accounted for by misreporting) increased in presidential elections over the 24 year time period.¹

Table 1 about here

The NES voter validation studies provide many useful insights about the patterns of misreporting by respondents, including analyses that have shown that over-reporters are more similar to confirmed voters than to confirmed nonvoters in terms of political interest and most demographic variables (Silver, Anderson, and Abramson 1986; Traugott 1989), except race: Blacks are more likely to misreport voting, probably reflecting heightened norms among a group that fought hard and suffered much in the struggle to achieve the right to vote (Abramson and Claggett 1984). Nevertheless, it is important to recall that the voter validation data themselves are

tainted by unreliability (Traugott, Traugott, and Presser 1992) and measurement error (Presser, Traugott, and Traugott 1990).

One of the most direct approaches to addressing the problem of measurement error in surveys is to change the survey instrument itself. Traditionally, NES respondents were asked

In talking to people about elections, we often find that a lot of people were not able to vote because they weren't registered, they were sick, or they didn't have time.

How about you - did you vote in the elections this November?

Changes in the frame in which this question is posed might be expected to prompt respondents to think more about their actual behavior and provide more accurate reports, but survey experiments that have altered the frame by introducing other questions or changing the question order generally have had insignificant effects on the reported levels of turnout. In separate 1989 Maryland samples, asking the location of a respondent's polling place and asking about the respondent's voting history prior to the turnout question had no significant effect on the level of reported voting in the survey (Presser 1990). In 1984, NES moved the registration and turnout questions from near the end to the post-election survey to nearer to the beginning, but neither that change nor mode of interview (face-to-face or telephone) had any significant effect on misreporting as compared to previous presidential elections (Presser, Traugott, and Traugott 1990, 3; see also Table 1).

However, changes in wording on the turnout question itself have shown an effect. Beginning in 2000, NES introduced a revised question which offered several "excuses" for not voting. (For an extensive discussion, see Duff *et al.* 2004). Respondents were asked which of the

following statements “best describes you”:

1. I did not vote (in the election this November)
2. I thought about voting this time but didn't
3. I usually vote but didn't this time
4. I am sure I voted

Providing social support for some reasons that people might fail to vote (in the second and third response categories in the revised question), might be expected to reduce overreporting. Duff *et al.* (2004) analyze the question wording experiment in the 2002 NES, in which half the respondents were asked the traditional turnout question, and half were asked the revised question, and I replicate that analysis using data from the 2004 NES question experiment. Table 2 shows that the revised question reduced self-reported turnout by eight points (from 64.9% to 56.9%) in 2002, and by seven points (from 80.0% to 72.6%) in 2004, accounting for 31.7% to 37.6% of the NES overestimates, as compared to the VEP rate.² If, as was the case in the 1980s, overreporting accounts for 70% to 75% of the total survey overestimate, the change in the question wording by itself would not eliminate the survey overreport, but might reduce it by about half.

Table 2 about here

Panel Attrition

Each NES presidential election year survey between 1952 and 2004 (plus the 2002 midterm) is technically a panel study, in which the NES staff attempts to interview a national probability sample of respondents about their attitudes, beliefs, and intentions *before* the presidential election, and re-interview them about their attitudes, beliefs, and voting behavior *after*

the election. In addition, NES conducted multi-election panel studies in 1956-58-60, in 1972-74-76, in 1990-92, and in 2000-02-04, so some post-election respondents in 1960, 1976, and 2000 were being interviewed for the fifth time (pre- and post- in both presidential election years in the study, plus the post-election interview in the midterm year). Panel studies provide invaluable data for analyses of individual level change, but also introduce the possibility of testing effects on sample estimates. (Bartels 2000; Martinez 2003). One of those testing effects takes the form of panel attrition, in which significant numbers of pre-election respondents are not re-interviewed despite the surveyors' best efforts, either because of barriers to survey administration (such as the respondent moving between interview waves), mortality, or the respondent's choice not to participate in the post-election interview. Again, we might expect that factors that would make it difficult for survey organizations to successfully re-contact respondents in an earlier wave (such as changing addresses) would also negatively affect the likelihood of those respondents casting a vote. Moreover, some respondents with little interest in or knowledge about politics who politely acquiesced to one interview might be even less likely to agree to enduring a second wave of questions about topics that they find boring or worse. Based on data from the 2004 American National Election Study, Table 3 illustrates the fact that re-interview rates are related to political interest and recall of previous voting behavior within a single pre-post election study.

Table 3 about here

While this evidence suggests that differential panel attrition accounts for some of the survey overestimate of turnout, we do not know exactly how much, simply because we do not have information about the voting behavior of the missing post-election respondents. But we can get some sense of the magnitude of the effect of panel attrition through a simulation. First, I estimate a

logit model of self-reported vote participation as a function of pre-election and interview form variables for respondents who participated in both waves of the panel. Then, I derive imputed probabilities of voting for the missing post-election respondents by applying the logit function to the sum of the cross-products of the estimated coefficients and the reported values on the pre-election and administration variables. (For the respondents who did participate in the post-election wave, self-reported voters have a probability of one and self-reported non-voters have a probability of zero.) Table 4 shows the estimated model for respondents in both the standard and revised versions of the turnout question. As expected, reported turnout was significantly higher on both questions among respondents who expressed higher levels of campaign interest, caring about the outcome of the election, education, and partisan strength. On the standard version alone, loquaciousness on the candidate likes and dislikes questions and age were also significant (one-tailed test).

Table 4 about here

Table 5 reports the effects of panel attrition on self-reported turnout, by comparing the self-reported turnout rates of post-election respondents only to the estimated turnout rates when the imputed probabilities of post-election “dropouts” are included. Two methodological caveats are in order. First, the form of the interview variable (V044001) indicated the random assignment of respondents to the pre- or post-election patriotism questions and to the standard or experimental version of the turnout question. Obviously, post-election “dropouts” were not assigned to either turnout question, so I randomly assigned them to one of those conditions for the imputation, while retaining information about whether they were asked the patriotism questions in the pre-election wave. Second, since NES did not assign post-election weights to post-election “dropouts”, the

turnout figures in Table 5 are calculated using the pre-election weights (causing some minor differences with the data reported in Table 2).

Table 5 about here

The effects of panel attrition are evident in both the standard and revised question formats, but they are much weaker than the effect of the question wording itself. Overall, I estimate that if the “dropouts” had participated in the post-election wave of the 2004 survey, the NES overestimate of turnout would have been reduced by less than half a percent (from 76.5% to 76.1%). The effects of panel attrition were greater in the standard question format than in the revised question format, but in both formats, the effects of panel attrition pale in comparison to the effects of the question wording.

Panel conditioning

On the other hand, testing effects might also spur participation. The pre-election waves of questions about respondents’ thoughts and feelings about parties, candidates, and issues may motivate some people who might have otherwise abstained to cast votes. The NES study manager for many years noted a “disconcerting number of instances in which respondents spontaneously mention to interviewers that the whole process of being interviewed has certainly made them more aware of and interested in politics, and they have made a certain effort to ‘study up’.” (Traugott 1989, 4n.)

While participation in pre-election surveys is tiring and boring to some apolitical respondents, “conversations at random” (Converse and Schuman 1974) about political issues, values, and personalities might stimulate others’ interests in the electoral process.. The pre-election survey prompts respondents to think about what they know and believe about politics for

over an hour, and that might carry over to generate interest and convert some abstainers into voters in the current election. The NES study manager for many years noted a “disconcerting number of instances in which respondents spontaneously mention to interviewers that the whole process of being interviewed has certainly made them more aware of and interested in politics, and they have made a certain effort to ‘study up’.” (Traugott 1989, 4n.)

Assessment of how *much* panel conditioning affects reported turnout rates requires a comparison of post-election reported participation (adjusted for panel attrition) from respondents who were interviewed prior to the election *and* from respondents who were not. Because all NES post-election respondents also were interviewed prior to the election, we lack data that would provide us with the leverage to estimate the effects of panel conditioning within a single pre-post election study.

However, NES multi-election panel studies can provide some insight on the potential effects of long-panel participation on reported turnout levels. The respondents in the last multi-election panel (covering the 2000, 2002, and 2004 elections) could have participated in as many as five waves (2000 pre and post, 2002 pre and post, and 2004 post), any one of which might have stimulated participation. Recognizing that potential, NES drew a fresh sample of respondents for the traditional pre-post election study in 2004, so a comparison the reported participation rates of the panel respondents and the “fresh” cross-section respondents will provide a rough estimate of the effects of panel conditioning.

I adjusted the self-reported turnout rates for the 2004 panel respondents for panel attrition using the imputation procedure described above, this time with predictor variables from the 2000 wave of the 2000-02-04 panel (as shown in Table 6), again randomly assigning panel dropouts to

one of the question forms and using the pre-2000 wave weights (as panel dropouts did not have 2004 weights assigned). With the controls for question wording and panel attrition, the difference between the adjusted self-reported turnout rates of the long panel respondents and the fresh 2004 cross-section respondents (shown in Table 7) is the estimated effect of panel conditioning. Overall, that effect is about six percent (the difference between 82.0% and 76.1%). The effect was a little larger in the revised question condition than in the standard question condition, perhaps reflecting a ceiling effect in the latter.

Tables 6 and 7 about here

On the whole, these estimated panel conditioning effects are substantial, but inferences about their generalizability should be guarded. It is important to remember that the comparison group (the fresh-cross section of respondents in the 2004 study) itself was part of a pre-post panel, and there is no way to tell how much participation in the pre-election survey might have prompted interest in the campaign that would result in higher turnout among the post-election respondents who did not drop out. Thus, the effects that we see in Table 7 are the difference between the effects of conditioning over a long-panel and those over a standard pre-post panel. *A priori*, there is no way to know whether conditioning effects are marginally increasing, marginally decreasing, or linear over multiple waves of a panel survey, but we do know from Table 7 that they can be substantial with respect to self-reported turnout.³

Non-random sampling errors

Non-random sampling errors might also partially account for surveys' overestimates of turnout rates. Part of that is reflected in voter-nonvoter differences in *contact rates*, as we might expect that NES, the Census Bureau, and other survey organizations would find voters more easily

than non-voters. Voters, after all, are more likely to have stable addresses and contact information that would enable survey researchers to establish contact with potential respondents. We also might expect differential *response rates* (Brehm 1993, Burden 2000) among voters and non-voters. As a group, nonvoters might be more hesitant than voters to participate in an extended “conversation at random” that poses repeated questions on subjects about which they have more disdain and suspicion than interest or knowledge. Analyses of the estimated impact of these factors on survey overestimates of turnout will be forthcoming in a later version of this paper.

Summing up

The National Election Studies surveys are an invaluable research base for the scholarly community in the electoral studies field, yet it is important for scholars and students to recognize the limits of their utility. This essay has highlighted several different sources of non-random error which affect NES’s estimates of voting participation.

Except for the spike in 1996, the NES self-reported turnout rate has been between 16.8% and 18.8% higher than the proportion of the voting eligible population who cast ballots in presidential elections since 1972. A complete decomposition of the overestimate in any given year is impossible, as no single election study year has all of the research design components that would facilitate an estimate of each distinct effect. But, the accumulated evidence over the time series provides some sense of the magnitude of the various effects. Based on evidence from the voter validation studies, misreporting appears to have been in the range of 3.5% to 5.2% between 1976 and 1988, accounting for between 20% and 30% of the total overestimate in those years. Question wording experiments in the 2002 and 2004 NES studies show that a revised question which provides “excuses for non-voting” reduces the aggregate overreport by 7.4% to 8.0%,⁴ accounting

for 31-38% of the overestimate (using the traditional question) in those years. In contrast, differential panel attrition between voters and non-voters appears to have only a small effect (0.4%), about 2% of the gross the NES overestimate in 2004. The effects of panel conditioning in a single pre-post election study are harder to estimate, but the conditioning effects of participating in a long-panel study appear to be on the order of 6%, or equivalent to about 36% of the gross overestimate of turnout in 2004. Thus, a roughly 6.9% overestimate,⁵ or about 40% of the gross overestimate in 2004 is still unaccounted for, and is likely due a combination of misreporting on the revised question, and non-random sampling errors (differential contact and response).

Other studies have provided much more complete analyses of which respondents are most likely to be affected by some of these processes. Overreporting in general appears to be more prevalent among non-voters who share the demographic and attitudinal characteristics of most voters, and among Blacks for whom the long hard-fought struggle to obtain the franchise may make “yes, I voted” an especially socially desirable response (Abramson and Claggett 1984). In contrast, the effects of the question wording change introduced 2000 seems to have had the most effect on those least likely to vote, that is “among respondents who are young, have low levels of education, have low incomes, do not own homes, are new to their community, do not care much about the outcome of the House election, have low levels of political knowledge.” (Duff *et al.* 2004, 10) Bartels (2000, 9) found that panel attrition effects were inconsistent with respect to demographic variables between 1992 and 1996, though there is some evidence that a combination of panel attrition and conditioning produce higher levels of political knowledge in later waves of a panel survey. Once a complete decomposition of the gross overreporting of turnout is complete, it would be valuable to extend that analysis to demographic subpopulations. That would enable us to

get a more nuanced portrait of the differences between voters and non-voters, and provide clues on how best to increase the signal-to-noise ratio in our field's most utilized and valued data source.

Table 1: VEP turnout, NES self-reported turnout, and NES confirmed turnout rates

	<u>1964</u>	<u>1976</u>	<u>1980</u>	<u>1984</u>	<u>1988</u>
VEP turnout rate	62.8	54.8	54.2	55.2	52.8
NES self-reported turnout	77.7	71.6	71.4	73.6	70.0
VEP - NES self-report	14.9	16.8	17.2	18.4	17.2
NES “Confirmed” turnout	75.9	68.0	67.0	69.1	64.8
VEP - NES “Confirmed”	13.1	13.2	12.8	13.9	12.0
Misreporting (self-report - confirmed)	1.8	3.6	4.4	4.5	5.2
Proportion of overestimate due to misreporting	12.1%	21.4%	25.6%	24.5%	30.2%

Source: Traugott (1989).

Table 2: VEP turnout and NES self-reported turnout:
traditional and revised questions, 2002 and 2004

	<u>2002</u>	<u>2004</u>
VEP turnout rate	39.5	60.3
NES self-reported turnout (traditional question)	64.9	80.0
VEP - NES self-report (traditional question)	25.4	19.7
NES self-reported turnout (revised question)	56.9	72.6
VEP - NES self-report (traditional question)	17.4	12.3
Effect of question wording change	8.0	7.4
Proportion of overestimate (based on traditional question) accounted for by change in question wording	31.7%	37.6%

Source: VEP from McDonald (2005). NES 2002 reports from Duff *et al.* (2004). NES 2004 data calculated by author.

Table 3: 2004 Post-election re-interview rate by
Pre-election reported campaign interest and 2000 vote recall

	<u>Campaign Interest</u>		
	<u>Very Much</u>	<u>Somewhat</u>	<u>Not Much</u>
Re-interviewed	89.0%	87.9%	83.0%
Not re-interviewed	11.0%	12.1%	17.0%
	483	529	200
	<u>Recall of 2000 vote</u>		
	<u>Yes, voted</u>	<u>No, did not vote</u>	<u>Don't Know</u>
Re-interviewed	89.6%	83.9%	83.3%
Not re-interviewed	10.4%	16.1%	16.7%
	786	415	12

Source: 2004 American National Election Study (weighted by v040101)

Table 4: Logit model of self-reported voter turnout in NES 2004

	Standard question		Revised question	
	Coefficient	z-score	Coefficient	z-score
(Intercept)	-5.055	-4.531	-4.224	-4.221
Female	0.181	0.631	0.176	0.725
Campaign Interest	0.348	3.094	0.308	3.111
Care about Outcome	0.613	3.316	0.375	2.311
Church attendance	0.102	1.126	0.087	1.095
Education	0.911	4.530	0.373	2.376
Partisan Strength	0.380	2.622	0.410	3.196
Log of number of candidate likes and dislikes	0.519	2.278	0.279	1.449
Age	0.069	1.737	0.052	1.427
Age squared	-0.001	-1.627	-0.000	-1.047
Form of Interview	-0.094	-0.662	0.055	0.468
AIC	375.31		482.09	
Number of Cases	527		516	

Source: 2004 American National Election Study

Table 5: Estimated effects of panel attrition on turnout, NES 2004

	Standard		Revised		Total	
	Post-election responses only	Include imputed probs.	Post-election responses only	Include imputed probs.	Post-election responses only	Include imputed probs.
Voted	79.9%	79.2%	73.1%	72.9%	76.5%	76.1%
Not Voted	20.0%	20.8%	26.9%	27.1%	23.5%	23.9%
Number of Cases	537	613	529	599	1066	1212

Source: 2004 American National Election Study

Table 6: Logit model of self-reported voter turnout in 2004
(NES 2000-04 panel)

	Standard question		Revised question	
	Coefficient	z-score	Coefficient	z-score
(Intercept)	-3.181	-2.778	-3.088	-2.929
Female	-0.720	-1.771	0.554	1.562
Campaign Interest in 2000	0.506	3.310	0.136	0.933
Care about Outcome in 2000	0.178	0.761	0.339	1.522
Church attendance in 2000	0.090	0.800	0.021	0.189
Education in 2000	-0.348	-0.560	0.177	0.305
Partisan Strength in 2000	0.253	1.331	0.286	1.616
Log of number of candidate likes and dislikes (2000 election)	0.145	0.506	0.436	1.772
Age in 2000	0.735	2.180	0.428	1.400
Age squared	0.000	0.685	0.000	1.693
AIC	240.48		249.14	
Number of Cases	428		388	

Source: 2000-02-04 American National Election Study

Table 7: Estimated effects of Long-Panel Conditioning on Self-Reported Turnout,
NES 2004

	Standard		Revised		Total	
	Panel adj.	Cross section adj.	Panel adj.	Cross section adj.	Panel adj.	Cross section adj.
Voted	83.1%	79.2%	80.9%	72.9%	82.0%	76.1%
Not Voted	17.9%	20.8%	19.1%	27.1%	18.0%	23.9%
Number of Cases	901	613	906	599	1190	1212

Source: estimated from 2000-02-04 American National Election Study and 2004 American National Election Study

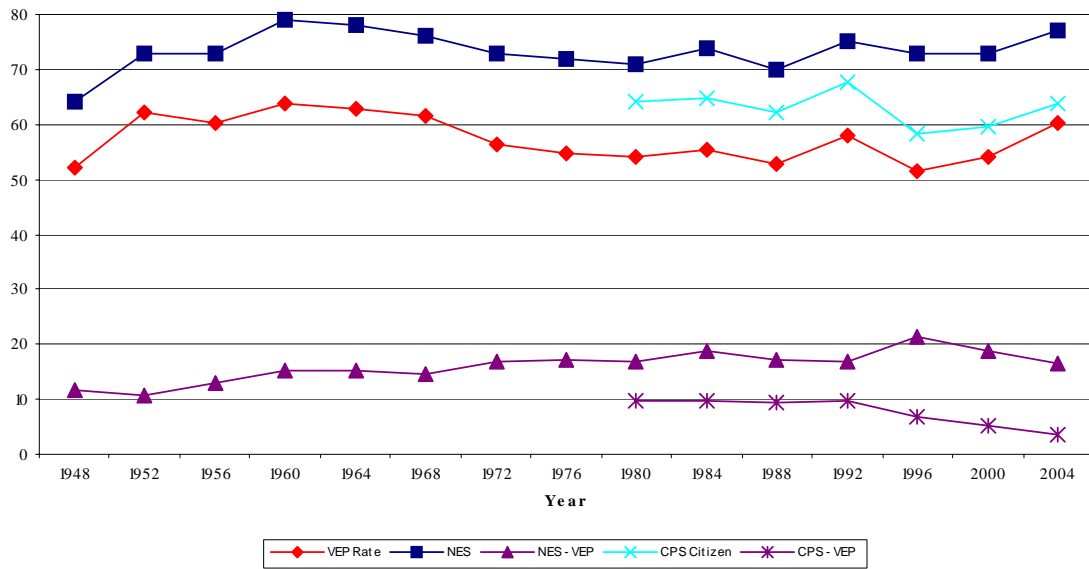


Figure 1: Actual (VEP) and Survey (NES and CPS) reports of turnout in Presidential Election Years. (Sources: VEP (McDonald 2005), American National Election Studies (2005), and U.S. Census Bureau (2005)).

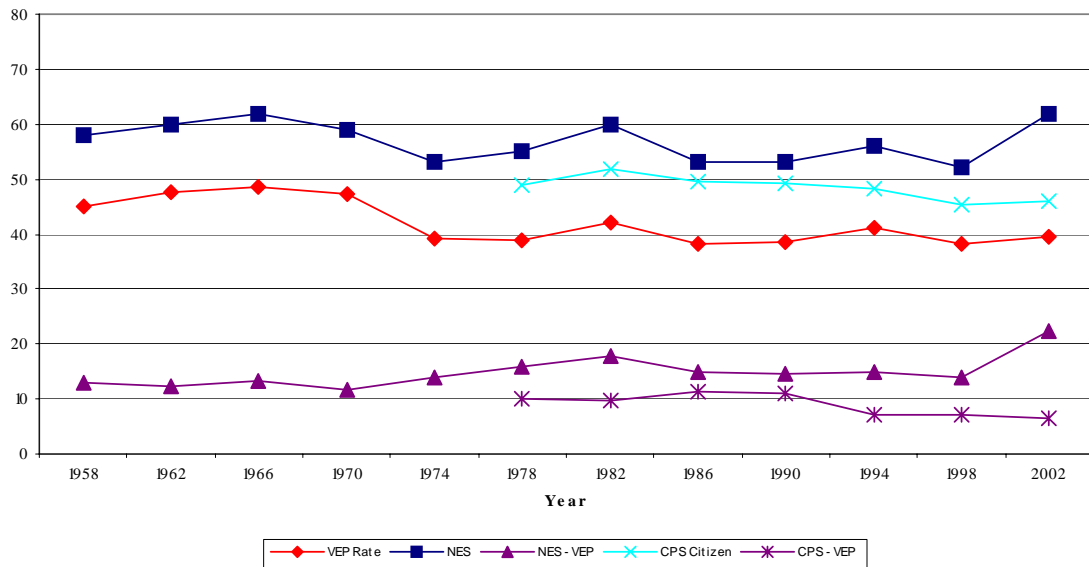


Figure 2: Actual (VEP) and Survey (NES and CPS) reports of turnout in Mid-term Election Years. (Sources: VEP (McDonald 2005), American National Election Studies (2005), and U.S. Census Bureau (2005)).

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Endnotes

1. Traugott (1989) compares the NES turnout rates to the “aggregate” rate, calculated as the ratio of votes cast for President to the size of the voting age population. The voting-eligible population (VEP) data provided by McDonald (2005; see also McDonald and Popkin 2001) is closer to the NES sampling frame, and suggest that the NES overestimate of turnout is lower than previously feared. (See Martinez 2003.)
2. Like Traugott (1989), Duff *et al.*(2004) use the VAP turnout rate as the baseline.
3. A better design to estimate the panel conditioning effects of a single pre-post election study would be to interview a fresh cross-section of respondents only in the post-election wave, and compare the panel-attrition adjusted reported turnout of the pre-post sample to the self-report in the post-only sample.
4. Since the difference between the reported turnout rates using the standard and the revised questions represents a “floor” (as misreporting is still possible and likely with the revised question), these results suggest that misreporting has continued to rise since the last voter validation study in 1988.
5. Calculated at the gross overestimate in 2004 (16.7) minus half the effect of the question wording change (3.7) minus estimated panel attrition (0.2) minus estimated panel conditioning (5.9).