How Good is This Excuse?:
Correcting the Over-reporting of Voter Turnout in the 2002 National Election Study

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Abstract

Utilizing studies which validate voter turnout, previous researchers have been able to identify a strong tendency for individuals to report voting when they in fact did not. In this paper, we assess the effectiveness of a new turnout question on reducing voter over-reporting in the National Election Study. Providing respondents with socially acceptable excuses for not voting, we find that this alternate question significantly reduces over-reporting of turnout in the 2002 National Election Study by about 8 percentage points. We also find that this reduction occurs primarily among those who are the least engaged with politics.

Introduction

For scholars interested in American elections, there is probably no more important question asked of American National Election Study (NES) respondents than the one that inquires if they voted in the previous election. Knowing who votes is in itself a central question of political science, and is also a crucial piece of information for scholars using the NES to study other questions regarding American politics. Unfortunately, each year, a portion of the people who did not vote in the previous election will report to the NES that they did. This over-report can be inferred simply by comparing the reported turnout of the NES sample to the turnout of the population at large. It has also been confirmed in various studies that have used voter rolls to verify the turnout of survey respondents.

The two factors most often identified as driving the over-reporting of voting are problems with recall and the social desirability of saying one voted (particularly in the midst of answering a number of questions about politics). The traditional NES turnout question attempts to mitigate these two factors with its introductory script. The question reads:

> *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?*
>
> 1. *Yes, voted.*
> 5. *No, didn't vote.*

Acknowledging socially acceptable reasons for not voting might make it easer for the respondents to say they did not vote. It is also possible that the respondents will be reminded that they were indeed sick, or especially busy this year.

In 2000, the NES introduced a new version of the turnout question. This version had an identical introductory script, but offers the respondents a chance to seize onto one of the socially acceptable excuses in giving their response. After the introductory script, respondents were

asked “Which of the following statements best describes you” and were read the following response options:

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*

This new version did seem to be successful in reducing the size of the over-report of turnout in 2000, if only by a small percentage (see Table 2 below). But it was difficult to determine whether the new version was affecting different social groups in different ways, and therefore might be introducing new bias into the study of voting. The 2002 NES offers an opportunity to investigate that question because it used an experimental design to randomly assign half its respondents to the new version of the turnout question and half to the standard version. In this paper, we take advantage of the opportunities offered by the experimental design to assess the effectiveness of this new version of the turnout question on reducing the rate of over-reporting. We find that it does indeed seem to mitigate the problem of overestimating the turnout rate, by about 8 percentage points ($p = 0.002$). We offer further analysis in an effort to determine if this new version of the question affects different groups of respondents in different ways. In particular, we investigate the extent to which the new version might systematically filter out certain groups that tend to over-report and not others. When examined against the validated vote results presented by Belli et al. (2001) this analysis provides insight into the bias created by the new turnout question. We conclude with an analysis of the potential consequences of the new question wording on our understanding of the precursors to voter turnout.

Vote Over-reporting

The over-reporting of voting in surveys is a well recognized problem among survey researchers and those interested in understanding voting behavior. Although the size of the survey over-report calculated from validation studies is dependent on how we measure both actual turnout and misreporting (see Silver, Anderson and Abramson 1986; Martinez 2003; McDonald 2003), survey over-reporting tends to range from 8 to 14 percentage points (Belli, Traugott, and Beckmann 2001) and by some measures has extended beyond 20 points in recent elections (see Burden 2000; Martinez 2003; McDonald 2003; and Table 2 below). Such high levels of over-reporting can have important consequences for research attempting to explain voter turnout, especially if there is something systematic about who is likely to over-report. At the very least, the tendency for individuals to report voting when they did not greatly distorts the accuracy of survey estimates of voter turnout.

Attempts to understand better the source of this problem have resulted in two distinct approaches to assessing vote over-reporting: voter validation studies and survey experiments. While each of these approaches addresses the questions of which respondents tend to over-report and why they do so, voter validation studies have met with much more success than the experiments in identifying who over-reports. Yet, survey question experiments, if properly designed, hold promise for building the over-report solution into the survey instrument itself, and avoiding the expensive and not always accurate validation process (see Presser, Traugott, and Traugott 1990).

Validation Studies

By comparing respondents' self-reported turnout from surveys to official voting records, researchers have sought to assess the accuracy of responses to questions measuring individual voter turnout (Clausen 1968; Traugott and Katosh 1979). Although these validation studies have proven to be a useful tool in identifying both the extent of vote over-reporting and its demographic and attitudinal correlates, as Table 1 summarizes, there is a fair amount of variability in the results across studies. Variability in the size of the over-report can be traced back to the way researchers have chosen to measure vote over-reporting. Some researchers calculate vote over-reporting by contrasting over-reporters with all other non-voters, while others contrast them either with validated voters or with admitted non-voters. Each of these comparisons results in slightly different estimates of vote over-reporting (for a review see Belli et al. 2001).

[Table 1 About Here]

While these differences in the approach of validation studies have also produced a long and varied list of attitudinal and social determinants of the turnout over-report, the race of the respondent stands out as the most consistent predictor of vote over-reporting. Validation studies indicate that African Americans are anywhere from two to three times more likely to over-report voting than white Americans (Abramson and Claggett 1984). The most common explanation for this finding is that the historical struggle for the right to vote is so salient to black respondents that they experience a higher than usual internal pressure to report having voted. Additionally, over-reporters tend to be more similar to validated voters than to non-voters in their attitudes and beliefs about politics (Belli et al. 2001). These results provide some evidence for the idea that over-reporters are those individuals who face the strongest internal and societal pressures to

produce socially desirable responses. In other words, blacks, the more politically efficacious, and those who care more about politics are more likely to be aware of the norm of voting and see voting as tool for social change.

While validation studies have provided us with an important means of assessing over-reporting, these studies, of course, fall short of solving the over-report problem within the survey context. The expense alone of the validation process—in both time and money—provides incentive to seek an alternative means of addressing survey respondents' tendency to inform researchers that they voted when they did not. Moreover, there is evidence of complications for validation studies stemming from reliance on official state records that may be unreliable in a troubling, systematic way. Notably, Presser, Traugott and Traugott (1990) find significant racial and regional differences in the accessibility of voting records used in validation studies; voting records for blacks and southerners are significantly harder to locate than records for other Americans.[1]

Experimental Studies

As an alternative to validation studies, researchers have sought ways to rectify the survey over-report problem by manipulating the survey itself. These studies have explored variations in the survey conditions and the wording and answer choices of the turnout question meant to reduce over-reporting by targeting the respondents' likely incentive to provide socially desirable responses or ability to recall accurately their behavior.

Such experiments, however, have not been entirely successful at reducing misrepresentations of actual behavior. Presser (1990) found that manipulating whether or not the respondent was asked to report earlier instances of political participation did not reduce over-

[1] As states move toward state-wide electronic voter registration systems some of these problems will be alleviated.

reporting, nor did preceding the turnout question with an item asking information that only a person who voted would know. Abelson, Loftus, and Greenwald (1992) report the results of three survey based experiments, each of which fails. Perhaps their failure to find significant results was due to their decision to limit their analysis to citizens whose registration could be validated. That is, by overcoming the hurdle of registration (Wolfinger and Rosenstone 1980) the citizens in their studies have performed at least one political act, thus suggesting some attachment to the political system and an awareness of its norms. As we discuss later, this explanation is supported by our results. Belli, Traugott, and Rosenstone (1994) find that encouraging respondents to scrutinize the sources of their memories also had little effect on vote over-reporting. Operating under the idea that over-reporting is the result of confusion among respondents about the election in which they actually voted (the election in question or a previous election), Belli et al. (1994) find that exposing respondents to either the standard turnout question or a version of the question which encouraged them to think harder about having voted, along with additional answer choices (the same as used in the 2000 and 2002 NES) resulted in no statistically significant differences in vote over-reporting. As was the case with the work by Abelson et al. (1992), we believe Belli et al.'s (1994) reliance on validated registrants may have greatly limited their ability to reduce over-reporting. Utilizing experimental techniques designed to reduce social desirability pressures, by assuring respondents of the anonymity of their responses, Holbrook and Krosnick (2003) also failed to find evidence of reduced vote over-reporting.

However, there is an example of success. Recently, Belli et al. (1999) tackled the problem of over-reporting by treating social desirability and source confusion as forces that might act together. With both an introductory text designed to reduce memory failures and

answer choices (the same as used in the 2000 and 2002 NES) that are more socially acceptable than a simple "no," they find that over-reporting was reduced by about 9 percentage points. What remains unclear is what the separate effects of the enhanced introductory text and the additional answer choices are. Our results speak to this question since the NES changed their response options, but left their introductory text as it was.

Hypotheses

Our hypotheses are guided by recent work that demonstrates the difficulty of isolating the separate effects of social desirability and source monitoring (Belli et al. 1994, Bell et al. 1999) and recognition of the failures of previous survey based approaches to the reduction of over-reporting.

According to the social desirability hypothesis those who have not voted but who feel pressure to place themselves among voters, should be more likely to fall into one of the new answer choice categories. These pressures should be concentrated among those who are most interested, engaged, and have the most to gain from the system (Silver et al. 1986). Of course, if the pressures are sufficiently strong, much more than the addition of two socially acceptable excuses will be necessary to overcome the urge to falsely report having voted.

Similarly, those who have voted in the past but cannot accurately separate recent behavior from previous behavior should find solace in the new answer choices which allow them to count themselves as a "usual" voter. Because people in this category are likely to have high levels of resources and be interested in politics, if the new answer choices help reduce source confusion we again would expect to see that those most likely to vote are attracted to the new answer choices. However, without the expanded introductory text employed to jog respondents'

memories (see Belli et al. 1994; and Belli et al. 1999) the answer choices fit more closely with a social desirability explanation. Evidence to suggest the answer choices led to a reduction in source confusion would be most apparent if over-reporting is lowest among those who were interviewed long after the election, when memories are more likely to fade.

Results

Recent Rates of Over-reporting

The relevance of the 8 point drop in turnout that resulted from the new NES question can be seen by placing it in the context of several recent elections. Consider first the turnout rate in the three most recent midterm elections (see Table 2).[2] While turnout across these elections was quite similar, differing by less than 3 percentage points, turnout over-reporting using the standard NES questions varied from 16 to 28 points, a swing four times larger than the fluctuation in turnout. The largest rate of over-reporting was found in 2002 among those who were asked the standard NES question. Even with the additional answer choices, the rate of over-reporting in 2002 (20 points) was higher than in both 1994 and 1998. Though explaining the increase in over-reporting that results from the standard NES question in 2002 is beyond the scope of this paper, it is possible that the pressure to perform one's civic duty after the 2000 election and the events of September 11 played a role.[3]

[Table 2 About Here]

As noted previously, the new answer choices were also included in the 2000 NES. Because all respondents in 2000 received the new version of the turnout question we cannot

[2] We use official returns from the Federal Election Commission with the Voting Age Population (VAP) as the base. Calculations using the Voting Eligible Population (see McDonald and Popkin 2001) show turnout to be about 3 points higher than the VAP estimates.

[3] For a lively debate on the factors leading to the rise in over-reporting over time see Burden (2000, 2003), McDonald (2003), and Martinez (2003).

isolate the effect of the additional answer choices on reducing the rate of over-reporting. However, in comparison to 1996, the results in Table 2 suggest that the new question might have had a small effect, as over-reporting was 3 points lower in 2000, despite the fact that the election being asked about was the most closely contested and obsessively discussed in decades, and thus social desirability pressures were likely intensified for respondents.[4]

Over-reporting by Individual Characteristics

That the overall rate of over-reporting was reduced is important on its own, especially in light of the various attempts that have failed. But for those interested in understanding political behavior, the degree to which over-reporting varies across individuals is a more central concern. As previously noted, though the literature has produced conflicting accounts of who over-reports, most agree that over-reporting is not constant across groups of people with different demographic and attitudinal characteristics.

Although we do not have access to validation data for respondents to the 2002 NES, by virtue of the experimental design we can compare turnout across versions of the question and individual characteristics. Table 3 presents the self-reported turnout rate across the two versions of the turnout question, the difference in turnout between the control (standard NES question wording) and treatment (new question wording) conditions, and the p value for this difference by demographic and attitudinal characteristics.

[Table 3 About Here]

While there are some differences in turnout across conditions for those most likely to vote, the most striking result found in Table 3 is that the largest differences tend to be for those

[4] A caveat is necessary here. As Burden (2000, 2003), McDonald (2003), and Martinez (2003) note, the 1996 NES suffers from problems with the sample (see also The National Election Studies 1996). We utilized the weight provided by the NES, V960005B, to adjust for sampling error.

least likely to vote. That is, some of the largest differences and those which are statistically significant appear 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,[5] and do not score highly on the efficacy scales. For example, among those who reported that they did not care at all who wins the House election the rate of self-reported turnout was 31 points higher (and statistically significant at $p = 0.013$) among those asked the standard NES question, while the difference was 11 points for those who said they cared "very much" who wins. What is more, turnout in the treatment condition among those who did not care at all was a mere 11%. Worse yet, for proponents of high turnout, only 5% of those who were given the lowest political knowledge ratings said they voted when asked the new NES question. Once again, the gap in turnout was over 30 points across conditions ($p = 0.048$). The incredibly low rates of turnout for some groups when they are asked the new question reveals that for scholars and reformers who treat low turnout as a problem, matters are far worse than previously thought.

Across several categories the results suggest that when asked the new NES question members of certain groups that we would expect to feel the strongest pressures to over-report do confess to not voting. With respect to age, the 30 point turnout gap across conditions among 60 – 64 year olds rivals the gap for the two youngest groups. A similar pattern appears for income, where the difference in self-reported turnout among respondents in the $50,000 – $65,000 range is on par with the difference among the least well off financially. However, the differences

[5] Preferably, the respondents' knowledge would be assessed through questions that tap factual information about politics. These sorts of questions were not included in the 2002 NES. Therefore, we use the interviewer's assessment of the respondent's knowledge. Although factual questions were included on the 2000 NES, using 2000 knowledge would mean losing all cases from the 2002 fresh cross-section.

among those traditionally thought to have high probabilities of voting do not fall into a discernable pattern.

One of the most regular findings of previous research is that non-whites are more likely over-report. The evidence in Table 3 suggests little difference in the effect of the new answer choices between whites and non-whites. It seems that the pressure among non-whites to conform to the idea that voting is a mechanism to improve their standing (see Belli et al. 2001), or prove they are part of society (or citizens) and not outsiders keeps many from falling into the new answer choices.

The results with respect to the length of time between the election and the interview are also noteworthy. Respondents who were interviewed more than 2 weeks after the election had a turnout rate 17 points lower in the treatment condition; while those interviewed within the first week had a turnout rate 11 points lower in the treatment condition. It is important to note, however, that the difference between the 17 point reduction and 11 point reduction is not statistically significant. Thus, the new answer choices may help reduce over-reporting among respondents who might have trouble remembering whether or not they voted in the most recent election, but it is difficult to say this definitively.

In sum, the additional answer choices do not seem to be enough to reduce turnout reports among those who feel the strongest pressure to produce a socially acceptable response or those who are likely to have voted in the past but who have murky memories that confuse behavior in the current election and previous elections. Given that these types of individuals are not widely affected by the new response options, it is not surprising that survey based experiments on validated registrants (Abelson et al. 1992; Belli et al. 1994), a group with a mix of resources, attitudes, and previous behavior that would predict voting, did not reduce over-reporting. For

those who are least likely to care about the election and whose demographic characteristics suggest they probably vote infrequently, if at all, appearances seem to be enough; i.e. these voters do not feel sufficient pressure to report the most desirable response and more willingly latch onto the new answer choices. Not only do the results suggest that the new answer choices cannot fully overcome the social desirability pressure or constraints on memory but these factors appear more prevalent among the least engaged than previously thought. Perhaps the most consequential result is that the levels of turnout found among citizens with low levels of resources and little attachment to politics point to a more severe degree of electoral inequality.

Comparison of Traits Across Answer Choices

Having investigated the types of citizens who are less likely to falsely report when they can report non-voting while still appearing to be civic minded, we now turn to a comparison of individuals across answer choice categories. That is, we examine who says "*I thought about voting this time but didn't*" and who says "*I usually vote but didn't this time*" in comparison to reported voters and those who chose the traditional did not vote response (*"No, I didn't vote"* or *"I did not vote"* in the standard and new NES questions respectively). The results are presented in Table 4a.

[Table 4a About Here]

The first three rows show the number and percentage of respondents who selected into each answer choice across the traditional and experimental conditions of the 2002 NES turnout question. While the traditional "Did not vote" category decreased to twelve percent, the two newly introduced excuse options drew just over thirty percent of the respondents, with 14% reporting that they "thought about voting but didn't" and 17% selecting the "usually vote but

didn't this time" option. Had these respondents been given the traditional question without the excuse options, most of them would have chosen the simple "Did not vote" category, but at the same time, it is evident that a significant fraction of them would have chosen to falsely report having voted.

Table 4a, when considered in light of previous research on voting, offers some cause for caution for scholars who study voting using the new turnout question. In the remainder of this table we present the mean values for a number of independent variables traditionally associated with turnout across the response categories in the control and experimental conditions. Previous research using these demographic and attitudinal variables to compare validated voters, over-reporters, and admitted non-voters suggests that the respondents most likely to over-report will most closely resemble actual voters (Belli et al. 2001). Therefore, it would be reassuring to find a similar pattern of results when comparing the new intermediate categories of "thought about voting" and "usually vote but didn't this time" to those who report voting versus those who are sure they did not vote. In other words, it would be reassuring to find that respondents who seize upon one or both of the "excuse" categories (the "thought" and "usually" categories) are more similar to definite voters than to definite non-voters in terms of their demographic and attitudinal characteristics. If this is not the case, it suggests that the new question might be introducing systematic bias as it peels of a certain segment of over-reporters, and leaves another group of demographically and attitudinally distinct over-reporters untouched. Unfortunately, to the extent that it is possible to discern a pattern in table four, it follows the pattern that raises caution flags.

What makes it difficult to interpret the results in tables 4a and 4b is that the respondents who fall into the two "excuse" categories are a mix of respondents who would have claimed to vote if asked the traditional question, and those who would have admitted to not voting even if

they were not offered an excuse. It would be convenient to assume that those in the former category would fall predominantly into the "usually vote" category and the later group would fall mostly into the "thought about voting" category. However, we can not make that assumption with any confidence. Therefore, we scrutinize respondents in both categories to determine to the extent to which they resemble those who report voting and not voting when asked the traditional question. While our findings certainly raise concerns about systematic bias in who is affected by the new version of the turnout question, this ambiguity makes it difficult to demonstrate definitively the degree to which bias is introduced.

The one category where Belli et al. (2001) found over-reporters to be more similar to admitted non-voters than to validated voters was age. Table 4a reports a similar result with the new NES question. Belli et al. found that younger respondents are more likely to over-report, and we found that younger respondents are also more likely to opt for the new excuse options. In this category, then, the experimental question offers an encouraging result. Young voters seem particularly attracted to the "thought about voting" category. It makes intuitive sense that the youngest of voters do not latch onto the excuse that they "usually" vote, since they have had fewer opportunities to vote in their lifetimes.

When examining a number of other variables associated with turnout we find a different pattern, one that does not parallel the findings of Belli et al. (2001). Those researchers found that in the categories of education, caring about the election, efficacy, interest in the election, partisan strength, and knowledge, over-reporters "are significantly closer to validated voters than to admitted non-voters" (Belli et al. 2001, 489). Turning to the 2002 NES and comparing the respondents who seized upon the new excuse categories to those who report voting and those

who chose the simple "did not vote" category, the "excuse" respondents more closely resemble those who report they simply did not vote, in both the control and experimental conditions.

For example, non-voters who "thought about voting," when compared to those who report voting in the control condition, have significantly lower levels of education, less interest in the election, care less about the outcome, have lower levels of efficacy, and less political knowledge. (Table 4b presents the mean differences and results from tests for statistical significance across the respondents in each category of the experimental and control groups). When the same "thought about voting" respondents are compared to the group who answered the traditional turnout question by saying they did not vote, the differences in the means are smaller, and none of the differences are statistically significant.

[Table 4b About Here]

A similar pattern emerges even when comparing those non-voters who said they "usually vote" to respondents asked the traditional turnout question. The "usually" group is significantly different from reported voters in their partisan strength, their interest in the election, caring who wins, and internal efficacy. When compared to non-voters in the control group, however, the "usually" group is only statistically distinguishable on one of these measures, internal efficacy.

This general pattern repeats itself when one looks at only those respondents who were asked the new version of the NES question. Comparing respondents in the excuse categories to those who said they are sure they voted produces a host of statistically significant differences. That is, self-reported voters are older, have higher levels of education, have larger incomes, are more likely to own homes, have greater interest in the campaign, care more who wins, and are more knowledgeable about politics than those who fall into the excuse categories. Comparing the excuse respondents to those who simply say they did not vote, however, reveals very few

differences that rise to conventional levels of statistical significance. The one notable exception to this rule is that those non-voters who say they "usually vote" are more similar to voters than they are to simple (no excuse) non-voters in levels of internal efficacy.

When comparing respondents across the excuse options we see little to distinguish those who select the "thought about voting" option from those who select the "usually vote" option. As previously noted, younger voters should be less likely to report that they "usually vote;" from Table 4b we see that the average age among respondents who "usually vote" is 8 years older ($p = .037$) than those who "thought about voting." The only other statistically significant difference across these groups relates to their internal efficacy, with "usual" voters scoring slightly higher on the internal efficacy score ($p = 0.052$).

In one way the results from Table 4a and Table 4b are reassuring and intuitive. In general, the respondents who seize upon either of the two excuses offered by the new turnout question fall somewhere in the middle of those who report they voted and those who say they did not on most attitudinal and demographic variables associated with turnout. This is what previous research would lead us to expect and it implies that the new version of the turnout question is doing just what it is designed to do: inducing a segment of the respondents who might have falsely reported voting in the previous election to instead report that they had not voted.

The cause for concern lies in the particular segment of respondents who seem to be affected by the new version of the question. The results in Table 4a and Table 4b, especially when viewed in light of the findings of previous research involving verified voting data, imply that the new question tends to pick off the easiest cases. The new question seems to disproportionately affect those false-reporters whose demographic and attitudinal characteristics are most distinct from habitual voters. These results demonstrate a clear need for additional

research on ways to reduce vote over-reporting among citizens with high levels of resources and attitudes that favor voting.

Understanding Turnout

The above analysis has shown that the reduction in over-reporting was driven primarily by individuals who differ in systematic ways from self-reported voters. We now investigate the extent to which this creates a bias for our understanding of the effect of traditional demographic and attitudinal variables on turnout. The number of model specifications used to predict turnout is too large to summarize here; for the sake of simplicity, our turnout model is based on the specification devised by Rosenstone and Hansen (1993).

[Table 5 About Here]

Table 5 presents the results for a model, run separately for those asked the standard NES question and those asked the new version, that includes demographic and attitudinal variables along with a variable to indicate whether or not the respondent had been contacted by a political party about voting.[6] The two columns on the left represent the coefficients and p-values when the model is run using the standard question. The two columns on the right represent the coefficients and p-values when the model is run using the experimental question.

Across both conditions, education, age, caring who wins the election, partisan strength, being contacted by a party, and residency in the south are significant predictors of turnout. For education, age, and caring who wins, the size of the coefficient was larger for the experimental question; in the case of caring who wins the election, the size of the coefficient more than doubled.

[6] We obtain similar results when all respondents are pooled and we include interactions between the predictor variables and a dummy variable indicating whether the respondent received the new turnout question or the standard question.

Several differences in terms of sign and statistical significance are also apparent. In the treatment condition, the sign on the income coefficient is positive but the effect cannot be distinguished from zero. Surprisingly, the effect of income is negative and statistically significant in the control condition. Unfortunately, we do not have an explanation for this result.

Home ownership and external efficacy are not significant predictors for the traditional question but are significant when modeling the experimental question. For external efficacy, the magnitude of the change in the coefficient is actually very small, but the variable nonetheless is significant at the $p<.10$ level when modeling the experimental question. The change in the coefficient for home ownership across the two models is much larger – it more than doubles.

Although it is not straightforward to test the differences in the probit estimates from the two groups, we compared the marginal effects of the independent variables across conditions.[7] Our tests revealed that only two of the independent variables have different effects across conditions: respondent income and the degree to which the respondent cares who wins. In light of our earlier results, the estimated effects for "care who wins" deserve special attention. Our computation shows that caring who wins an election rather than not caring, holding other variables at their means or medians, will increase the probability of reporting having voted by 3 percentage points in the standard question, and 13 points in the experimental question. Though this is a sizeable difference, on the whole, the new NES question does not do much to revise our knowledge of the effect of various demographic and attitudinal variables on turnout.

[7] We computed the marginal effect of each independent variable, holding the other variables at their means (cardinal variables) or their medians (dummy variables). The "delta method" provides standard errors of fitted probabilities, and we checked whether the estimated effects in one condition fell within the confidence interval of their counterparts in the other condition. See Herron (1999) for more details.

Conclusion

Ultimately, we believe this excuse is quite good, and that it would be best to include the new response categories on future versions of the NES survey and other post-election surveys. The new question basically works in that it significantly reduces over-reporting, and furthermore does so without the inclusion of a lengthy and cumbersome introductory script. For most scholars who make use of NES respondents' answers to the turnout question this is a significant improvement that is unlikely to introduce overly bothersome complications. The new question does not get perfect information, but what it does seem to do is reduce precisely the false reports of voting in the data that are likely to be the most disharmonious and the most likely to obscure the overall theme. Over-reporters who are very dissimilar to habitual voters seize on the excuses, over-reporters who are very similar to habitual voters (or who are usual voters who simply failed to vote in the most recent election) stick to their guns.

What over-reporting the new turnout question does remove will allow researchers to highlight better and explore what is undoubtedly among the most significant findings regarding who votes and who does not vote in the U.S. – that those Americans who lack resources, education, and the confidence that they can understand politics and participate meaningfully vote at astonishingly low rates. The new question drives home just how rarely the least well off, interested, and informed go to the polls, even as it continues to exaggerate participation among the well off, the interested, and the informed.

However, for researchers who want to understand not merely who typically votes, but to dig deeper into the question of which occasional, and nearly habitual voters mobilize or de-mobilize in particular elections, in response to particular sorts of campaigns, or despite particular obstacles et cetera, the new question does not offer much new hope. The same can be said for

those scholars interested in the group of citizens that resembles habitual voters in attitudes and demographics and that nonetheless does not vote. NES data for both these groups of respondents will continue to be problematic, even with the new question.

To the extent that the over-reporting of voting is a result of social desirability, our analysis seems to indicate that a sense of the social desirability of being a voter is very widespread. As we reported in Table 3, when asked the traditional question, even those who say they "did not care at all" who won the election, and those who have the lowest levels of political knowledge, 40% or more report that they voted. But our analysis seems to show that this widespread social desirability effect is very deep in some spots and very shallow in others. The new NES response categories seem to be very good at mopping up the shallow spots – for example among the aforementioned groups (those who do not care who wins and have very little political knowledge), the reported turnout drops dramatically (more than 75%) with the new question. Since the factors we expected to be associated with social desirability were not good predictors of whether people wound up in the new "excuse" categories it does not seem that the new question does well where the sense of social desirability runs deep. Previous research has indicated that one place the social desirability of voting runs deep is in the black community. Our analysis indicates that black NES respondents were not very likely to seize on the new "excuse" categories.

Inclusion of the new response categories are probably a cost effective substitute for validation studies, which represent a time consuming and often problematic approach to finding out which respondents *really* voted. However, we believe it would be worthwhile to combine the new question with a validation study at least once, especially as states implement electronic voter files that will make validation less costly. Our analysis leads us to predict that the likely

result would be similar to the findings of Belli et al. (2001). When using the new turnout question, there will be fewer over-reporters, but those who remain will resemble confirmed voters much more than admitted non-voters. With the new question this pattern should be even more pronounced. The analysis we have presented in this paper strongly indicates that this would be the case, but using the new question in combination with a validation study is the best way to achieve greater certainty.

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Tables

Table 1. Previous Research on Vote Over-reporting

	Method	Data	% Over-report	Who Over-reports
Traugott and Katosh (1979)	Vote Validation	NES (1976)	14[a]	Young, non-white, low income, Democrats, first time participants in an election survey and support Carter.
Sigelman (1982)	Vote Validation	NES (1976)	23[b]	Non-Whites
Abramson and Claggett (1984)	Vote Validation	NES (1964, 1976, 1978, 1980)	9 to 46[e]	Blacks
Hill and Hurley (1984)	Vote Validation	NES (1976)		Highly educated, interested in politics, high levels of efficacy regarding voting and blacks.
Silver, Anderson and Abramson (1986)	Vote Validation	NES (1964, 1976, 1980)	22 to 31[c]	Highly educated (reduced magnitude of race over-report).
Anderson and Silver (1986)	Vote Validation	NES (1980)	9 to 14[c]	Highly educated and those most supportive of civic norms (reduced magnitude of race over-report).
Presser (1990)	Survey Experiment	1989 Survey of Maryland Residents	Inconclusive/ No Reduction	
Abelson, Loftus, and Greenwald (1992)	Survey Experiment	1987, 1989 NES Pilot, 1988-89 Seatte Study	Inconclusive/ No Reduction	
Belli, Traugott, and Rosenstone (1994)	Survey Experiment	1994 Survey of Ann Arbor and Ypsilanti Michigan Registered Voters	Inconclusive/ No Reduction	
Belli et al. (1999)	Survey Experiment	1996 Survey of Consumer Attitudes and 1996 Oregon Study	9	Respondents interviewed later in the survey interviewing process.
Belli, Traugott and Beckmann (2001)	Vote Validation	NES (1964, 1978, 1980, 1984, 1986, 1988, 1990)	8 to 14[a]	Over-reporters are between validated voters and admitted non-voters in their age. Over-reporters also tend to be men, non-whites and those respondents interviewed later in the survey interviewing process.
Bernstein, Chadha, Montjoy (2001)	Vote Validation	NES (1980, 1984, 1986, 1988)	23 to 32[d]	Racial minorities (blacks and Latinos) living in areas with high concentrations of same race minoties, and whites living among high concentrations of blacks or Latinos.
Holbrook and Krosnick (2003)	List/Randomized Response Survey Experiment	Knowledge Networks (national random sample) 2002	Inconclusive/ No Reduction	

Notes:
a Percent of all respondents who misreported whether they voted.
b Percent of those who claimed to have voted but did not.
c Percent of actual non-voters who clamed to have voted.
d Percent of actual non-voters who clamed to have voted (as estimated by racial group 23% for whites and Latinos and 32% for blacks non-voters clamed they voted but did not.
e Percent of those who claimed to have voted but did not (broken down by race for 1964, 1976, 1978, 1980 e.g., 1978 21% of whites said they voted but did not compared to 46% of blacks in this year).

Table 2. Official and NES Turnout Rates 1994 - 2002

Year	Official, based on VAP	NES Standard Question	NES New Question	NES Over-report, Standard Question	NES Over-report, New Question
1994	**38.78%**	**55.69%**	**NA**	**17**	**NA**
1996	**49.08%**	**71.81%**	**NA**	**23**	**NA**
1998	**36.39%**	**52.14%**	**NA**	**16**	**NA**
2000	**51.30%**	**NA**	**72.07%**	**NA**	**21**
2002	**37.05%**	**64.93%**	**56.87%**	**28**	**20**

Notes:

The Official turnout rate uses total votes cast for the highest office as reported by the FEC.

All results from the NES are weighted.

NA indicates that the result is not applicable due to the absence of the question version for given years.

Table 3. Turnout Across Conditions by Demographic and Attitudinal Factors, 2002 NES (weighted)

Demographics				
Age	**Control**	**Treatment**	**Difference**	**p value**
18-24	0.34	0.16	0.18	0.141
25-30	0.57	0.30	0.27	0.012
31-39	0.57	0.61	-0.04	0.505
40-49	0.70	0.65	0.04	0.414
50-59	0.80	0.71	0.09	0.062
60-64	0.84	0.54	0.30	0.001
65 and up	0.82	0.75	0.07	0.134
Education				
0-8 Years	0.60	0.54	0.06	0.732
9-11 Years	0.49	0.27	0.21	0.091
High School Grad	0.59	0.45	0.14	0.007
1-3 College No Degree	0.58	0.57	0.01	0.900
Junior College Grad	0.63	0.60	0.03	0.696
College Grad	0.87	0.82	0.05	0.220
Advanced Degree	0.87	0.83	0.05	0.399
Income				
$0 -$14,999	0.52	0.33	0.19	0.038
$15,000-$34,999	0.61	0.47	0.14	0.017
$35,000-$49,999	0.64	0.54	0.10	0.149
$50,000-$64,999	0.73	0.53	0.20	0.006
$65,000-$84,999	0.70	0.75	-0.04	0.495
more than $84,999	0.70	0.70	0.00	0.971
Race				
Non-White	0.59	0.48	0.11	0.063
White	0.66	0.60	0.07	0.021
Sex				
Female	0.59	0.52	0.07	0.055
Male	0.73	0.62	0.11	0.005
Homeownership				
Non-Homeowners	0.50	0.36	0.15	0.014
Homeowners	0.70	0.64	0.06	0.040
Years in the Community				
Less than 1 Year	0.61	0.39	0.22	0.108
1-2 Years	0.55	0.48	0.08	0.446
3-5 Years	0.52	0.48	0.04	0.653
6 Years or Longer	0.68	0.60	0.08	0.005
Interview Week				
First Week	0.73	0.61	0.11	0.000
Second Week	0.45	0.51	-0.06	0.273
Three or Later Weeks	0.66	0.49	0.17	0.029

Table 3. continued

Attitudes				
Partisan Strength	**Control**	**Treatment**	**Difference**	**p value**
Independent	0.42	0.30	0.12	0.261
Weak	0.60	0.55	0.05	0.166
Strong	0.86	0.70	0.16	0.000
Campaign Interest				
Not Much Interested	0.25	0.16	0.09	0.137
Somewhat	0.65	0.59	0.07	0.075
Very Much Interested	0.92	0.83	0.09	0.003
Care Who Wins House Election				
Not at all	0.42	0.11	0.31	0.013
Not very much	0.53	0.33	0.20	0.000
Pretty Much	0.63	0.67	-0.04	0.374
Very Much	0.86	0.75	0.11	0.004
Information Level (Interviewer assessment)				
Low	0.39	0.05	0.34	0.048
2	0.44	0.22	0.22	0.033
3	0.57	0.48	0.09	0.080
4	0.73	0.68	0.05	0.241
High	0.73	0.71	0.02	0.718
External Efficacy				
Least Efficacious	0.57	0.38	0.19	0.005
0.25	0.45	0.51	-0.06	0.554
0.5	0.60	0.52	0.08	0.154
0.75	0.67	0.66	0.01	0.906
Most Efficacious	0.74	0.68	0.06	0.120
Internal Efficacy				
Least Efficacious	0.42	0.16	0.26	0.045
0.17	0.24	0.18	0.05	0.724
0.33	0.49	0.46	0.04	0.530
0.50	0.54	0.45	0.09	0.177
0.67	0.72	0.71	0.01	0.886
0.83	0.89	0.74	0.15	0.009
Most Efficacious	0.94	0.84	0.10	0.016

Notes:

Control refers to the standard NES turnout question.

Treatment refers to the new NES turnout question with additional answer choices.

Table 4a. Mean Values of Predictors by Experimental Condition and Voting Outcome

	Control Group		Experimental Group				
	Did Not Vote	Voted	Did Not Vote	Thought About Voting But Did Not	Usually Vote But Did Not	Sure Voted	Total
Number of Cases[1]	172	500	61	53	96	462	1344
Weighted Number of Cases	236	436	79	97	114	382	1344
Weighted Percentages	35.07%	64.93%	11.72%	14.37%	17.03%	56.87%	
Demographics							
Age	37.59	47.99	45.05	31.87	40.06	48.55	44.32
Education	3.44	4.19	3.16	3.37	3.78	4.48	3.99
Income	3.30	3.68	2.85	3.24	3.31	3.96	3.58
% White	0.74	0.79	0.77	0.59	0.76	0.79	0.77
% South	0.44	0.34	0.40	0.36	0.41	0.34	0.37
% Male	0.32	0.46	0.43	0.43	0.38	0.51	0.44
% Own Home	0.64	0.80	0.75	0.56	0.54	0.83	0.74
Political Characteristics							
Party ID	2.90	2.99	2.61	2.46	2.81	3.06	2.92
Partisan Strength	0.98	1.34	1.16	1.14	1.07	1.34	1.23
Interest	1.65	2.31	1.61	1.64	1.87	2.33	2.08
Care Who Wins	2.55	3.03	2.41	2.41	2.69	3.16	2.87
Int. Efficacy Scale	0.44	0.65	0.36	0.41	0.54	0.66	0.57
Ext. Efficacy Scale	0.53	0.65	0.36	0.48	0.59	0.68	0.60
Knowledge (IWer Assess)	3.39	3.82	3.00	3.26	3.57	3.96	3.67
Contextual Variables							
Interview Day	8.25	7.01	7.12	9.23	8.62	6.97	7.52
Interview Week	1.64	1.44	1.53	1.65	1.60	1.45	1.51

[1] The numbers represent the cases of the dependent variable before conditioning on the independent variables: due to item non-response, the N is smaller in some cells.

Table 4b. Mean Differences and Tests of Significance from Table 4a.

	Thought - Did Not Vote (control)	Thought - Voted (control)	Usually - Did Not Vote (control)	Usually - Voted (control)	Usually - Thought	Sure Voted - Thought	Sure Voted - Usually	Thought - Did Not Vote (exp)	Usually - Did Not Vote (exp)
Age	**-5.722**	**-16.121**	**2.466**	**-7.933**	**8.188**	**16.685**	**8.497**	**-13.178**	**-4.99**
Education	**-0.076**	**-0.826**	**0.332**	**-0.418**	**0.408**	**1.111**	**0.703**	**0.211**	**0.619**
Income	**-0.055**	**-0.435**	**0.011**	**-0.369**	**0.066**	**0.719**	**0.653**	**0.442**	**0.523**
% White	**-0.146**	**-0.201**	**0.017**	**-0.039**	**0.163**	**0.200**	**0.038**	**-0.175**	**-0.025**
% South	**-0.085**	**0.022**	**-0.034**	**0.074**	**0.052**	**-0.017**	**-0.069**	**0.036**	**0.016**
% Male	**0.109**	**-0.033**	**0.061**	**-0.081**	**-0.047**	**0.076**	**0.123**	**0.004**	**-0.044**
% Own Home	**-0.072**	**-0.241**	**-0.093**	**-0.261**	**-0.021**	**0.271**	**0.291**	**-0.187**	**-0.208**
PID	**-0.437**	**-0.533**	**-0.082**	**-0.177**	**0.355**	**0.607**	**0.251**	**-0.149**	**0.21**
Partisan Strength	**0.164**	**-0.198**	**0.089**	**-0.273**	**-0.075**	**0.200**	**0.275**	**-0.012**	**-0.087**
Interest	**-0.007**	**-0.671**	**0.219**	**-0.445**	**0.226**	**0.686**	**0.460**	**0.031**	**0.258**
Care Who Wins	**-0.137**	**-0.617**	**0.141**	**-0.339**	**0.277**	**0.752**	**0.475**	**0.004**	**0.282**
Int. Efficacy Scale	**-0.030**	**-0.233**	**0.093**	**-0.110**	**0.123**	**0.246**	**0.123**	**0.052**	**0.175**
Ext. Efficacy Scale	**-0.053**	**-0.175**	**0.061**	**-0.061**	**0.114**	**0.198**	**0.085**	**0.115**	**0.229**
Knowledge	**-0.132**	**-0.561**	**0.171**	**-0.258**	**0.304**	**0.697**	**0.394**	**0.261**	**0.565**
Interview Day	**0.979**	**2.221**	**0.369**	**1.611**	**-0.610**	**-2.265**	**-1.654**	**2.109**	**1.499**
Interview Week	**0.003**	**0.209**	**-0.045**	**0.160**	**-0.049**	**-0.198**	**-0.149**	**0.121**	**0.072**

Note: Significant differences ($p < 0.05$), using the Bonferroni test procedure, are highlighted.

Table 5. Modeling Turnout by Condition (Demographics and Attitudes) (Source: 2002 NES (weighted))

Independent Variables	Control coeff.	control p value	Treatment coeff.	treatment p value
Income	-0.099	0.036	0.053	0.245
Education	0.187	0.000	0.248	0.000
Unemployed	-0.304	0.325	-0.272	0.468
Age	0.048	0.029	0.080	0.000
Age Squared	0.000	0.102	-0.001	0.004
Years in the Community	0.021	0.786	0.022	0.784
Church Attendance	0.034	0.511	-0.002	0.968
Home Ownership	0.160	0.315	0.388	0.015
Border South	-0.035	0.905	0.367	0.146
Southern State	-0.304	0.041	-0.271	0.061
Race – Black	0.043	0.840	-0.020	0.928
Registration Date	-0.002	0.783	-0.001	0.850
External Efficacy	0.263	0.131	0.287	0.081
Partisan Strength	0.687	0.000	0.369	0.002
Care Who Wins	0.144	0.085	0.422	0.000
Contacted by Party	0.751	0.000	0.551	0.000
Constant	-2.968	0.000	-5.376	0.000
Number of Cases	589		595	
Log Likelihood	-287.05		-289.45	

Variable Coding:

Age: in years 18-99.

Black: 1 = black only, 0 = all else.

Border South: 1 = lives in a border state (KY, MO, MD, OK, WV), 0 = all else.

Care Who Wins: 1 = Not at all, 2 = Not very much, 3 = Pretty Much, 4 = Very Much.

Church Attendance: 1 = attends church, 0 = does not.

Contacted by Party: 1 = was contacted by a political party, 0 = was not.

Education: 1 = 0-8 Years, 2 = 9-11 Years, 3 = High School Grad, 4 = 1-3 College No Degree, 5 = Junior College Grad, 6 = College Grad, 7 = Advanced Degree.

External Efficacy Scale: ranges from 0 to 1 (average of answers to 2 questions).

Home Ownership: 1 = owns a home, 0 = does not.

Income: 1 = $0 -$14,999, 2 = $15,000-$34,999, 3 = $35,000-$49,999, 4 = $50,000-$64,999, 5 = $65,000-$84,999, 6 = more than $84,999.

Interest: 1 = Not much interested in the campaign, 2 = Somewhat, 3 = Very Much Interested.

Internal Efficacy Scale: ranges from 0 to 1 (average of answers to 3 questions).

Interview Day: ranges from 1 – 31 (number of days after the election that the interview was conducted).

Interview Week: 1 = within the first week, 2 = within the second week, 3 = within the third or fourth weeks (number of weeks after the election that the interview was conducted).

Knowledge (Interviewer Assessment): ranges from 1 to 5 (1 = low, 5 = high).

Male: 1= male, 0 = female.

Party ID: 0 = strong democrat, 1 = weak democrat, 2 = independent-democrat, 3 = independent-independent, 4 = independent-republican, 5 = weak republican, 6 = strong republican.

Partisan Strength: 0 = independent, 1 = weak partisan, 2 = strong partisan.

Registration Date: number of days prior to the election that registration ends (0-30 days).

Southern State: 1 = lives in the South (NES coding), 0 = all else.

Trust Government Scale: ranges from 0 to 1 (average of answers to 4 questions).
White: 1 = white only, 0 = all else.

Years in the Community: 0 = less than 1 year, 1 = 1-2 yrs, 2 = 3-5 yrs, 3 = 6 or more.