
MODELING MISREPORTS IN SELF-REPORTED VOTE CHOICE DATA*

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Abstract: Whereas scholars have extensively studied over-reports of voter turnout in surveys, they have devoted much less energy to survey misreports of vote choice. However, each phenomenon poses a potential dilemma for unbiased statistical inference, and an accurate understanding of why people vote for whom they do is also obviously central to our understanding of representative democracy. The first part of this study documents the extent to which misreport of vote choice in congressional elections persists across the past two decades of major academic voting surveys in the United States. The second part assesses several alternative strategies for estimating vote choice models with survey data in which there is clear evidence of over-report for the winner. The partial observability model (aka the split population model), which has recently gained prominence in political science, would appear to provide a compelling estimation-based “fix.” However, our results reveal that a simpler approach of specifying as covariates factors that influence the probability of misreport (e.g., the number of days between the election and the survey interview) may be preferable to the partial observability model in most circumstances. We consider both simulated data and data from the 1992 NES and SES for in-depth analysis. Regarding data available for past elections, estimation-based strategies provide the only feasible response in terms of addressing bias in vote choice models.

A small literature (e.g., Wright 1990, 1992, 1993; Carsey and Jackson 2001) has raised an issue that should trouble those who use post-election survey data (e.g., the National Election Studies (NES)) to study vote choice – frequently, more respondents report voting for the winner than actually did. Measurement error staring us in the face regarding a seemingly straightforward behavior such as vote choice raises concerns about the data products of the survey enterprise. However, do these vote misreports present simply a superficial nuisance, or do they underlie more fundamental problems for consumers of these data? If respondents' over-reports of votes for the winner occurred randomly, then these misreports would not lead to biased coefficient estimates for covariates of interest in models of vote choice. However, this misreport bias appears to be more than a harmless artifact as it pertains to models of vote choice. Wright (1993, p. 292) summarizes that “such distortions...accentuate the apparent explanatory power of any variables that are highly correlated with the source of the biased response.” In Wright’s (1990, 1992) models of Senate vote choice based on the 1988 NES/Senate Election Study (SES) data, the distortion resulted in artificial inflation of the influence of candidate variables (e.g., incumbency) and deflation of the influence of national forces (e.g., the coattail effect that accompanies presidential vote choice).

These findings present a fundamental problem for students of voting. For example, debate regarding the relative influence of the aforementioned factors on congressional voting has motivated the research agenda of many congressional elections scholars. What confidence should we place in much of the extant research on vote choice in light of Wright’s findings, and are there pro-active steps that we can take in response? Unfortunately, the research community has not provided a satisfying response – via either statistical adjustment at the point of estimation or, ideally, the collection of unbiased data at the outset.

Whereas scholars have extensively studied the over-report of voter turnout in survey data (for recent statements, see Ansolabehere and Hersh 2012 and Hanmer, Banks, and White 2014), they have devoted much less energy to misreport of vote choice. The often greater level of turnout over-report and the availability of validated turnout data for various sets of survey respondents likely have made turnout over-report the more obvious, and perhaps more tractable, object of study. However, each phenomenon – turnout over-report and vote choice misreport – poses a potential dilemma for unbiased statistical inference, and an accurate understanding of why people vote for whom they do is central to our understanding of representative democracy.

This study does two major things. First, it documents the extent to which misreport of vote choice persists across the past two decades of major academic voting surveys in the United States. Second, it assesses several alternative strategies for estimating vote choice models with survey data in which there is clear evidence of over-report for the winner. The partial observability model (aka the split population model), which has recently gained prominence in political science, would appear to provide a compelling estimation-based “fix.”¹ However, our results reveal that a simpler approach of specifying as covariates factors that influence the probability of misreport (e.g., the number of days between the election and the survey interview) may be preferable to the partial observability model in most circumstances. We consider both simulated data and data from the 1992 NES and SES for in-depth analysis. Regarding data that are available for past elections, estimation-based strategies provide the only feasible response in terms of addressing bias in vote choice models.

¹ The most well-known political science applications of the partial observability model appear in studies of IMF agreements and of trade and conflict (Przeworski and Vreeland 2000 and 2002, Vreeland 2003, Stone 2008, and Xiang 2010).

EVIDENCE OF PRO-WINNER MISREPORT, 1988-2008

Does misreport of vote choice persist across the past two decades of post-election NES voter surveys?² To answer this question, we adopt Wright's (1993) method of estimating the accuracy of respondents' reported votes, focusing on U.S. House and Senate races. Wright simply regresses Vote Difference, which is the difference between the individual-level self-reported vote (1 = Republican, 0 = Democrat) and the Republican's share of the two-party vote (*Rep. Vote*), on the two party vote (*Rep. Vote*) using ordinary least squares.

$$\text{Wright's Model: } \text{Vote Difference} = \beta_{cons} + \beta_{\text{Rep.Vote}} \text{Rep. Vote} + \epsilon.$$

If respondents' vote reports are unbiased, then mean reported vote for any particular contest ought to equal the actual aggregate vote in that race, plus or minus random sampling error. If there is no pro-winner misreport of vote choice, then the estimate of the coefficient $\beta_{\text{Rep.Vote}}$ in Wright's model will be near zero. Wright finds positive slopes for most races (i.e., U.S. House, Senate, gubernatorial, and presidential) for most of the years (1952-1988) of the (Cumulative) NES that he analyzes, indicating a systematic tendency for voters to over-report support for the winner.

In this study, we analyze those U.S. House and Senate races where a major party (Democratic or Republican) victor faced a major party opponent. Assessing misreport of U.S. House and Senate vote choice across the 1988-2008 (Cumulative) NES and the 1988-1992 SES, Figure 1 presents the estimates and 90% confidence intervals for $\beta_{\text{Rep.Vote}}$ from 1988-2008 (excluding 2006) for Senate, House, and competitive House elections. We define competitive

² The 2006 NES did not include vote choice questions for U.S. House and Senate elections. We also assessed vote misreport for the U.S. House and Senate in the 2006 and 2008 Cooperative Congressional Election Study (CCES) data and found no evidence of over-report for the winner.

House elections as those in which the winner received less than 80% of the two-party vote.³ Extending Wright's findings, we find a great deal of evidence of over-report for the winner across the 1990s, with the largest positive coefficients emerging for the 1990 midterm elections. The evidence of over-report becomes somewhat spottier across the more recent elections of the 2000s.

A positive coefficient reveals the percent by which the survey data over-estimate the winner's margin of victory that year (a negative coefficient suggests an under-estimate). For example, the coefficient of 0.49 for the House in the 1992 NES suggests an exaggeration of the winning margin by almost 50 percent. To illustrate the magnitude of this effect, if a candidate receives 60 percent of the actual two-party vote, a 50 percent over-report for the winner would lead to a post-election survey estimate of the winner receiving 65 percent. The most severe misreport estimate appears for the Senate in the 1990 NES, which suggests an exaggeration of winning margins of almost 85 percent for that office that year. Although the problem appears to have diminished somewhat, it clearly has not disappeared, as the quite sizable positive Senate coefficient for the 2008 NES manifests.

³ In the context of this study, "competitive" simply refers to not extremely lop-sided. The concern is that extreme values on *Rep. Vote* may exert undue leverage on the estimated slope (see Wright 1993). Furthermore, relatively few respondents can misreport for the winner in these contests.

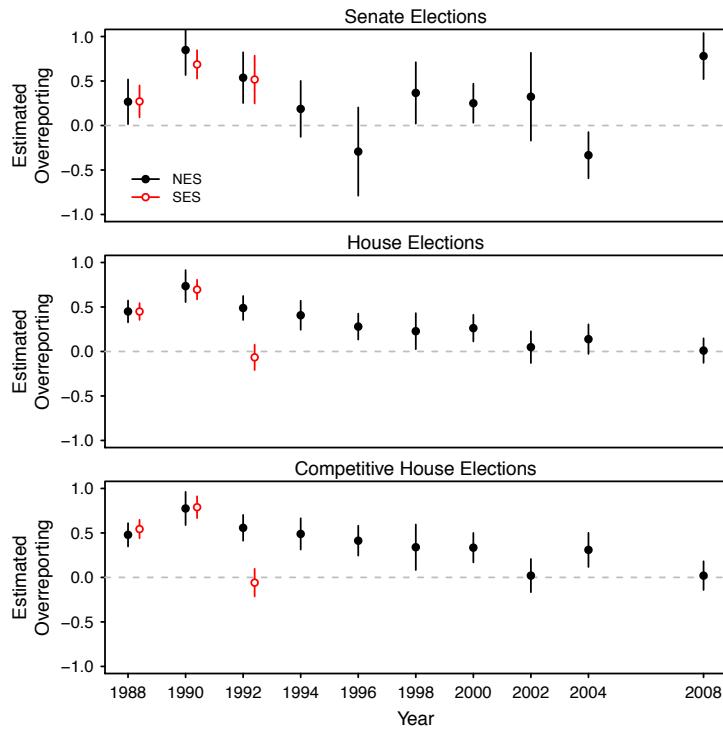


Figure 1: The estimated amount of misreporting in Senate, House, and competitive House elections from 1988 to 2008. We define competitive elections as those in which the winner received less than 80% of the two-party vote. We use Wright's (1993) approach (discussed in the text above) to estimate the amount of misreporting. The point estimates and 90% confidence intervals above are for the parameter $\beta_{\text{Rep.Vote}}$ in Wright's model.

DATE OF INTERVIEW AND INCUMBENCY EFFECTS: SOURCES OF MISREPORT?

Existing studies demonstrate that Election Day exit polls conducted by the media do not have a pro-winner bias (Wright 1990; Carsey and Jackson 2001). The 1992 Voter Research and Surveys (VRS) 50 state exit polls on which we rely later in this study again reveal no bias.⁴ The absence of pro-winner misreporting in exit polls suggests that the source of the bias in the NES and the SES is likely respondents' exposure to the knowledge of who won and the subsequent media coverage of that outcome. This raises the possibility that the amount of elapsed time between Election Day and the survey interview may be related to the level of misreporting. More elapsed time provides respondents more exposure to post-election information and more temporal distance between their actual vote and their recall of that vote.⁵

Another hypothesis about the source of misreport relates to incumbency. Eubank and Gow (1983; Gow and Eubank 1984; Eubank 1985) discuss a pro-incumbent bias in reports of vote choice in U.S. House elections, and Mattei (1998) also finds evidence of greater misreport in House elections with incumbent winners. Their argument is that incumbents are more familiar to voters than are winners of open-seat races. It is this greater familiarity that creates a misreport bias that only appears to be a pro-winner bias when all winners (incumbents and first-time winners) are analyzed together.

⁴ For example, the misreport coefficient is a substantively negligible 0.02 for a 1992 Senate exit poll model that invokes sample weights, and an even more minuscule -0.01 for the unweighted version of the same model.

⁵ Of course, across an extended period of time, post-election information dwindles and such factors as party identification likely begin to dominate faulty memory recall. However, these post-election survey interviews were conducted within the couple of months following Election Day.

To evaluate these two hypotheses, we expand the base model by adding an indicator for whether the race was an open-seat contest (*Open*), a measure of the time between Election Day and the survey interview (*Date*), and multiplicative interactions between these two variables and the actual level of support received by the Republican candidate (*Rep. Vote*), yielding an interactive model.

Interactive Model:

$$\begin{aligned} \text{Vote Difference} = & \beta_{\text{cons}} + \beta_{\text{Rep. Vote}} \text{Rep. Vote} + \beta_{\text{Open Seat}} \text{Open Seat} \\ & + \beta_{\text{Rep. Vote} \times \text{Open Seat}} \text{Rep. Vote} \times \text{Open Seat} + \beta_{\text{Date}} \text{Date} \\ & + \beta_{\text{Rep. Vote} \times \text{Date}} \text{Rep. Vote} \times \text{Date} + \varepsilon. \end{aligned}$$

The coefficient $\beta_{\text{Rep. Vote} \times \text{Open Seat}}$ assesses whether the race being an open seat contest conditions the level of misreport. A negative coefficient indicates that the over-report phenomenon is primarily a pro-incumbent (as opposed to a pro-winner) phenomenon. Figure 2 shows the estimates and 90% confidence intervals for $\beta_{\text{Rep. Vote} \times \text{Open Seat}}$ in Senate, House, and competitive House elections across time. Seventy-three percent of the coefficients operating on the *Open seat* interaction are negative, suggesting that the over-report for the winner may tend to be more prevalent in contests with incumbents. However, only three of these negative coefficients achieve statistical significance. Furthermore, three of the positive coefficients operating on this interaction (in the NES Senate data for 1990 and 1992 and the SES House data for 1992) also achieve statistical significance. The evidence indicates, overall, that the pro-winner misreport phenomenon is not simply a by-product of most winners being incumbents (which is especially the typical outcome in House contests).

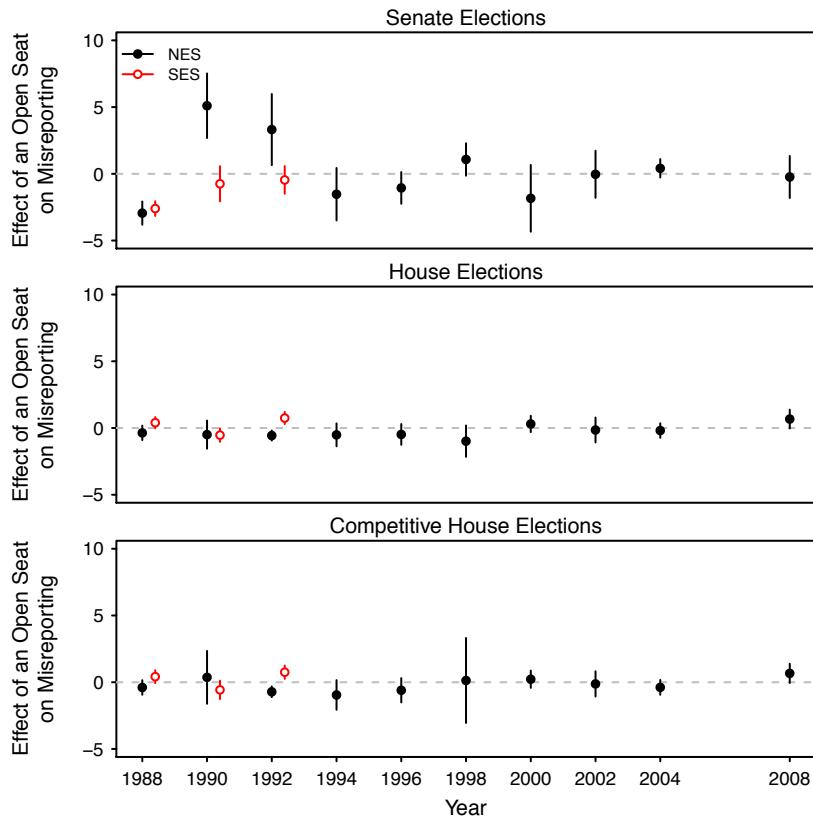


Figure 2: This figure shows the effect of an open seat on misreporting, assessing whether misreport for the winner might better be described as misreport for the incumbent. Notice that the pro-winner misreport phenomenon is not simply a by-product of most winners being incumbents. The point estimates and 90% confidence intervals above are for the parameter $\beta_{\text{Rep.Vote} \times \text{Open Seat}}$ in the Interactive Model.

The coefficient $\beta_{\text{Rep.Vote} \times \text{Date}}$ assesses whether the length of time between the vote and the interview conditions the level of misreport. A positive coefficient indicates that, on average, the over-report bias becomes more severe in interviews conducted more days after Election Day. Figure 3 presents the estimated coefficients and the 90% confidence intervals across time for Senate, House, and competitive House contests. We find that almost 70 percent of the estimates of $\beta_{\text{Rep.Vote} \times \text{Date}}$ are positive, which suggests that the over-report bias may tend to worsen the further from Election Day the survey is administered. Also, the three significant interactions are

all positive.⁶ The conditioning effect of time is not as universally crisp as one would hope in terms of supporting a generalized account of the misreport process. However, for some offices in some years (e.g., the 1992 and 1994 House results for the NES), its role is demonstrable.

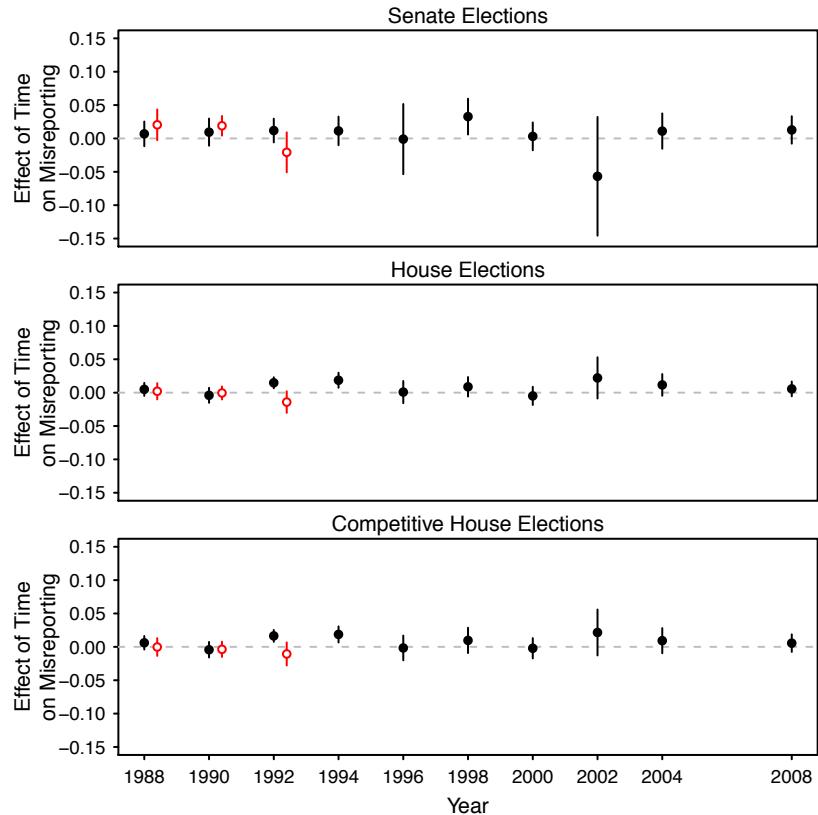


Figure 3: This figure shows the effect of time on misreporting, assessing whether survey respondents are more likely to misreport as the days since the election increase. The evidence for this effect is strongest in the 1992 and 1994 NES data for House elections. The point estimates and 90% confidence intervals above are for the parameter $\beta_{\text{Rep. Vote} \times \text{Date}}$ in the Interactive Model.

⁶ Surprisingly, we do not reproduce Wright's (1990) finding of a significant conditioning effect for *Date* in the 1988 SES Senate data. Also, the clearest evidence that the date of the interview fundamentally conditioned the level of over-report emerges from the 1992 and 1994 NES House results – indeed, the base coefficient operating on *Rep. Vote* in these models becomes small and insignificant.

MODELING THE MISREPORT PROCESS

Almost no voting studies that rely on survey data assess the possibility of misreport of vote choice, let alone attempt to account for it when present. The status quo approach is a collective “burying of heads in the sand.” Although this approach of simply ignoring the (potential) problem has ease on its side, the prospect of biased inference looms. First, we suggest that researchers assess whether misreport is present in their data. The follow-up question is what to do if it appears. We outline and evaluate four modeling responses.

In modeling the misreport process, we would like a model that can represent the following relationships, which we refer to as the guiding assumptions:

1. The probability of reporting a vote for the Republican candidate either increases or does not change across time (i.e., in the days and months immediately following Election Day) in districts won by a Republican candidate.
2. The probability of reporting a vote for the Democratic candidate either increases or does not change across time in districts won by a Democratic candidate.
3. To identify the model, assume that there is no over-report for the losing candidate and no over-report on Election Day.⁷

We are willing to allow our conclusions to rest on these assumptions. We offer several potential modeling strategies, each of which (except for the naïve approach) can represent the relationships outlined above. But, of course, other assumptions are necessary. These other assumptions define the precise model.

We consider several estimation strategies, each of which has its own strengths and weaknesses.

⁷ Again, exit poll data are not associated with misreport bias (see fn. 2).

1. *The Naive Model* (i.e., *the Status Quo*). Ignore the errors in the data. Estimate a simple logistic regression model and include covariates thought to influence actual vote choice.
Note that this model makes no attempt to model or adjust for misreports.
2. *The Simple Model*. Simply include time as a covariate. Estimate a simple logistic regression model and include covariates thought to influence vote choice as well as covariates thought to influence the probability of misreporting.⁸ Notice that this model is entirely consistent with the guiding assumptions. Unless the researcher wants to make further assumptions based on a more detailed understanding of the process, this model is quite plausible.
3. *The Interaction Model*. Include time as a covariate, but also interact time with the key explanatory variables. Estimate a simple logistic regression model and include covariates thought to influence vote choice as well as covariates thought to influence the probability of misreporting. Also, interact all covariates thought to influence misreport with the key explanatory variables (e.g., presidential coattails and/or incumbency). Again, unless the researcher wishes to make further assumptions, this model is consistent with the guiding assumptions.
4. *The Partial Observability Model*. Thoughtfully model the process. Based on the best theoretical intuition available, directly and thoughtfully model the process, sacrificing model simplicity as needed. While the other approaches are useful because they are mathematically convenient and easy to implement, a thoughtful model can offer an easier interpretation *once the model is estimated*.

The first three strategies are already in the hands of applied researchers, and recent work (e.g.,

⁸ Gronke (1992) and Wright (1992) had an exchange regarding the usefulness and plausibility of this approach.

Braumoeller 2003 and Beger et al. 2011) makes the fourth increasingly accessible. Nonetheless, our problem presents several novel challenges, so we derive a new model that differs slightly from previous partial observability models.

A PARTIAL OBSERVABILITY MODEL OF MISREPORT

A partial observability model (e.g., Poirier 1980) is a form of a finite mixture model (e.g., Imai and Tingley 2012) and a split-population model (e.g., Svolik 2008), which researchers use to capture causal complexity (Braumoeller 2003), although sometimes a need to address measurement error motivates a partial observability model (Beger et al. 2011; see also Feinstein 1990). In fact, Beger et al. (2011) advocate researchers' consideration of a partial observability model to assess behaviors that are subject to misreport bias when measured via a survey question. Although these types of model come in many varieties across which the details vary, we are interested in a model of binary outcomes in which an event can occur as a result of one of two causal processes. Each process is imagined to influence the probability of a latent event. Depending on the exact substantive application, the researcher might assume that the observed outcome variable Y takes on a value of one if and only if both latent events occur, or the researcher might assume that the observed outcome takes on a value of one if either latent event occurs. Whereas models of directly observed outcomes can be evaluated using test sets or future observations, partial observability models are more difficult to evaluate because of the inferences they are designed to make. In this study, we assess the performance of a partial observability model when the sets of variables thought to influence each outcome ((1) misreport of vote choice and (2) vote choice itself) do not overlap.

We develop our model of misreport around the idea that survey respondents increasingly

misreport voting for the winner over time.⁹ We begin by assuming three types of individuals: (1) those who report correctly, (2) those who misreport for the Republican candidate, and (3) those who misreport for the Democratic candidate. At each point in time, a fixed fraction of the population, if interviewed, would report their vote correctly, denoted by $\text{Pr}(\text{Accurate})$. If a Republican won the election, then the remaining fraction of the population would over-report for the Republican, denoted by $\text{Pr}(\text{Misreport}_R)$. Similarly, in a district won by a Democrat, $\text{Pr}(\text{Misreport}_D)$ denotes the proportion misreporting for the Democrat.

To develop our model, we assume that $\text{Pr}(\text{Misreport}_i)$ grows over time if party i wins, but remains at zero otherwise. Thus, we assume that over-report for the winner increases across time and that over-report for the loser does not occur. Particularly, we assume that honest reporters become misreporters at a fixed rate a as time t increases continuously. This notion can be represented by the simple differential equation

$$\frac{d\text{Pr}(\text{Misreport})}{dt} = a[1 - \text{Pr}(\text{Misreport})],$$

where $0 \leq a \leq 1$. Integrating, we obtain

$$\text{Pr}(\text{Misreport}) = a[1 - \text{Pr}(\text{Misreport})]t + C.$$

Note that the constant term C must be zero, since we are assuming that $\text{Pr}(\text{Misreport}) = 0$ when $t = 0$ (Election Day). It turns out to be particularly convenient to divide both sides by $1 - \text{Pr}(\text{Misreport})$ and then take logs, giving

$$\log\left(\frac{\text{Pr}(\text{Misreport})}{1-\text{Pr}(\text{Misreport})}\right) = \log(at),$$

⁹ We focus on days since election for two primary reasons: 1) this variable has been a principal explanation in the small, existing literature on misreport of vote choice, as well in the much larger literature on turnout over-report (e.g., Belli, Traugott, and Beckmann 2001; Stocke 2007; Stocke and Stark 2007; Selb and Munzert 2011) and 2) we have empirical evidence of this variable fundamentally conditioning misreport in several of our cross-sections. It is also worth noting that researchers could adapt our model of misreport to account for other covariates that influence misreport likelihood (e.g., incumbency).

which can be rewritten as

$$\text{logit}(\Pr(\text{Misreport})) = \log(a) + \log(t)$$

by expanding the log.

Incorporating these ideas into a full model of misreport, we obtain the identity for the probability of a respondent reporting voting for the Republican candidate,

$$\Pr(\text{Self-Report}_R) = \Pr(\text{Vote}_R) \Pr(\text{Accurate Response}) + \Pr(\text{Misreport}_R) - \Pr(\text{Misreport}_D),$$

where

$$\Pr(\text{Accurate}) = 1 - \Pr(\text{Misreport}_R) - \Pr(\text{Misreport}_D),$$

and model the probability of voting for the Republican using the usual logistic regression formulation

$$\Pr(\text{Vote}_R) = \text{logit}^{-1}(X\beta).$$

In our applications below, we use the following covariates to predict vote choice

$$X\beta = \beta_0 + \beta_1 ID_R + \beta_2 ID_D + \beta_3 Inc_R + \beta_4 Inc_D + \beta_5 Vote_{Bush} + \beta_6 Vote_{Clinton}.^{10}$$

The probability of misreporting for the Republican and for the Democrat are given by

$$\Pr(\text{Misreport}_R) = \frac{e^{Z_R\gamma}}{1+e^{Z_R\gamma}+e^{Z_D\delta}}$$

and

¹⁰ Illustrating with a Senate vote choice model for 1992, we adopt a simple, parsimonious specification that can be estimated with both post-election survey data and exit poll data. Relying on a series of dummy variables, it specifies respondent party identification (Republican and Democrat, with Independent/Other (as the suppressed category)), respondent presidential vote (Bush vote and Clinton vote, with Perot vote (as the suppressed category)) to gauge presidential coattail effects, and state-level contextual variables that capture incumbency effects (Republican incumbent and Democratic incumbent, with open seat (as the suppressed category)). For the midterm vote choice models, we specified presidential vote choice in the prior presidential election, although a dichotomous variable for respondents' presidential approval is also available for consideration. In addition to Wright (1990, 1992) and Gronke (1992), numerous other studies adopt a similar specification (e.g., Hendry, Jackson, and Mondak 2009; Jacobson 2009; Jacobson 2013).

$$\Pr(\text{Misreport}_D) = \frac{e^{Z_D\delta}}{1+e^{Z_R\gamma}+e^{Z_D\delta}},$$

respectively, where $Z_R\gamma = \log(a) + \log(t \times \text{Republican Winner})$ and $Z_D\delta = \log(\delta) + \log(t \times \text{Democratic Winner})$.¹¹

$$Z_R\gamma = \log(\gamma) + \log(t \times \text{Republican Winner})$$

$$Z_D\delta = \log(\delta) + \log(t \times \text{Democratic Winner})$$

To estimate the model, we use MCMC sampling, evaluating convergence with Gelman and Rubin's (1992) \hat{R} statistic. The model converges after about 500 iterations, and we use 2,000 samples after the burn-in to make inferences.

The question of immediate substantive interest for applied researchers is whether the theoretically appealing partial observability estimator provides a compelling response to biased vote choice data – or do the simpler estimation responses compete effectively in terms of the quality of their inferences? By comparing inferences from NES and SES data, which are based on interview responses drawn throughout the period of the several months following Election Day, to those from exit poll data, we hope to gain insight into the accuracy of the inferences of the partial observability model.¹²

¹¹ Notice that when $t = 0$, the log function forces the probability of misreport to zero. Further, when there is a Democratic winner, the probability of misreporting for a Republican is zero. When there is a Republican winner, the probability of misreporting for a Democrat is zero.

¹² Although misreports of vote choice motivate our present consideration of a partial observability model, we believe that our ideas generalize beyond this context and offer additional insights into what researchers can and cannot learn from partial observability models.

EVALUATING THE MODELS USING KNOWN DATA- GENERATING PROCESSES: THREE SIMULATION STUDIES

Before considering observational survey data, we conduct three simulation studies to evaluate the various modeling strategies when the true data-generating process (DGP) is known. In each simulation study, we generate 50 fake data sets, and for each data set, estimate an arbitrary effect (first-difference) and confidence interval using each approach. Our goals are (1) to illustrate that misreport biases coefficients and (2) to assess whether either the more complicated partial observability model or a simpler approach reduces this bias. Across the simulation studies, the DGP varies as follows:

1. Only the partial observability model can represent the true DGP. Our theoretical intuitions are right. This is the best case scenario for the partial observability model.
2. No model considered can represent the true DGP. Our theoretical intuitions are wrong. In this situation, which model performs best depends on the exact DGP. Despite the plausibility, theoretical appeal, and seeming complexity of the partial observability model, it might be outperformed when it cannot represent the true DGP.
3. There is no misreport. All models can represent the true DGP. Hopefully, none of the approaches outlined above affects the inferences. We would hope that a model would adjust our inferences when misreport is present, but leave the inferences unchanged in the absence of misreport.

SIMULATION STUDY #1: THE PARTIAL OBSERVABILITY MODEL CAN ACCURATELY REPRESENT THE TRUE DGP

In the first simulation study, the thoughtful partial observability model can accurately represent the true DGP. That is, the DGP is actually given by the partial observability model developed

above. In this case, the partial observability model should provide the best inferences, and the naïve approach should be biased. However, the relative performances of the simple model and the interaction model are unknown. The effect of interest is the change in the probability of an individual actually voting for a Republican candidate, as an arbitrary binary variable X changes from zero to one. We set the parameters so that the true effect is about 0.1.

1. As expected, the naïve model is biased. It actually estimates a negative effect of -0.01, on average, with a standard deviation of 0.02. Thus, the naïve model underestimates the effect by about 11 percentage points.
2. The simple model adjusts for much of the bias found with the naïve approach, estimating an effect of 0.07, on average, with a standard deviation of 0.02. Thus, the simple approach, although it does not capture the exact DGP, is still useful in correcting for misreports, removing about 70% of the bias.
3. The interaction model also removes much of the bias, estimating an effect of 0.06, on average. However, the standard deviation of the estimates is 0.05—more than twice as large as that of the other approaches. While the interaction model and the simple model provide similar reductions in bias, the simple model is much more efficient and therefore likely preferable to the interaction model.
4. The partial observability model is unbiased, getting the effect correct, on average. It is slightly less efficient than the simple model, with a standard deviation of 0.03, but if researchers know that the partial observability model correctly represents the actual DGP, then that model performs the best.

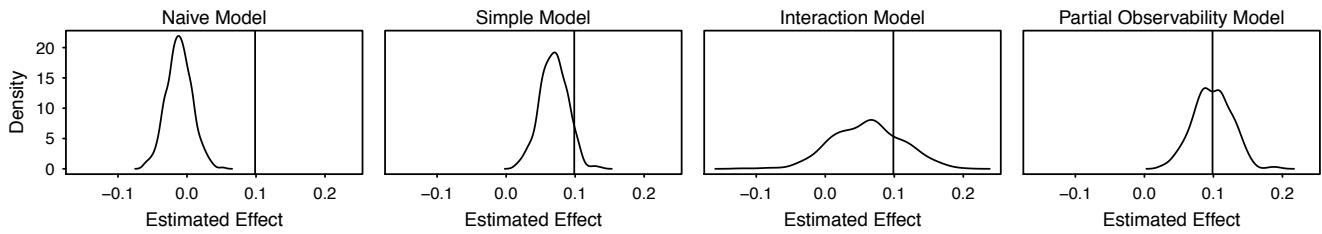


Figure 4: Density plots showing the variance in the estimates across 400 simulations using the four alternative approaches to modeling bias due to misreports. In this case, the partial observability model is used to generate the data, so it provides the best inferences. However, notice that the simple model and interaction model are able to remove most of the bias with a more accessible modeling strategy.

Simulation Study #2: No Model Can Accurately Represent the Actual Relationship

In the second simulation study, we evaluate the models' performance when none of the approaches we consider exactly captures the DGP. In this simulation, the probability of misreport for the winner is given by $\Pr(\text{Misreport}) = 0.5 \left(\frac{t}{50}\right)^3$ (t ranges from 0 to 50 in our simulations). Misreporting grows quickly in the first few days after the election, but then slows. Other than this seemingly innocuous change, the DGPs are identical. While none of our statistical models exactly captures this DGP, this process is consistent with the three guiding assumptions that we laid out earlier and is just as substantively plausible as a DGP given exactly by the partial observability model. Because only the probability of misreporting changes, the correct inference is identical to that of the previous simulation.

Figure 5 shows estimates from 400 simulated data sets.

1. Notice first that the naïve model *overestimates* the actual effect in this simulation, causing the researcher to overstate claims about the key explanatory variable. The average estimate is 0.14, overestimating the effect by about 40%. The standard deviation of the estimates is 0.02.
2. The simple model *slightly underestimates* the effect. The average estimate using this

approach is 0.08, underestimating the effect by about 0.02 on average. The standard deviation of the estimates is 0.02. Notice that the simple model improves substantially (as before) on the naïve approach.

3. The interaction model also performs well, on average, producing an average estimate of 0.12, an overestimate of about 0.02. Notice, though, that the standard deviation of these estimates is 0.12—much larger than that of the others. In this situation, the interaction approach certainly sacrifices efficiency for any gains from reducing bias.
4. The partial observability model is severely biased upward and does little to correct the bias in the naïve model. The partial observability model produces an average estimate of 0.14, about the same as the naïve approach. The standard deviation of these estimates is 0.02.

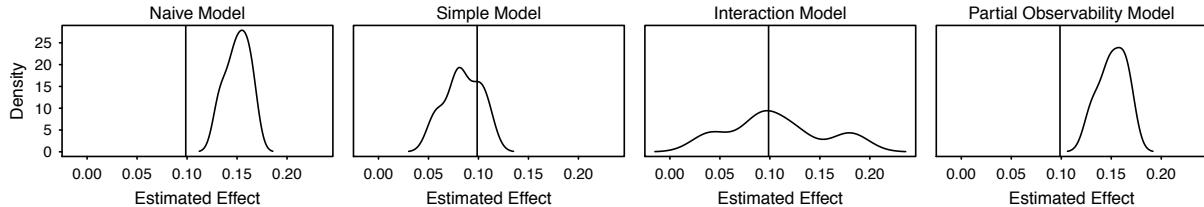


Figure 5: Density plots showing the variance in the estimates across 400 simulations using the four alternative approaches to modeling bias due to misreports. None of the models exactly matches the true data-generating process, though the process is substantively reasonable. In this case, the partial observability model provides a nice match to a particular theory about how the process might work, but this approach actually makes the inferences worse. Notice, though, that the simple model and interaction model are able to remove most of the bias with a more accessible modeling strategy.

SIMULATION STUDY #3: THERE IS NO MISREPORTING

In the third simulation study, we examine the behavior of the model when there is no misreporting, that is, when the naïve model is correct. In addition to accounting for misreporting when it is present, we would prefer our models not point toward and incorrectly adjust for misreporting when none is present. Thus, we would expect that each approach would offer

unbiased estimates of the effect of interest, especially since the all approaches can represent a scenario in which there is no misreporting. Figure 6 presents the estimates from the 400 simulated data sets.

1. Notice first that the naïve model, which is the correct model in this simulation, offers a relatively unbiased and efficient estimate of the effect of interest. The average estimate across the 400 simulated data sets is 0.1 for the naïve model—exactly on target—with a standard deviation of 0.2.
2. The simple model performs comparably well. Since time is not related to self-reported vote choice, this model includes one too many variables, but the estimate is comparable to the naïve model with an average estimate of 0.1 and a standard deviation of 0.2.
3. Even though the interaction model offers a relatively unbiased estimate of the effect of interest, it is much less efficient than the other three approaches. The average estimate across the 400 simulated data sets is 0.2, but the standard deviation jumps to 0.5, more than double the others.
4. The performance of the partial observability model is comparable to the simple model and naïve model. Although it includes a more theoretically-nuanced structure and also includes one variable too many, the average estimate is also 0.1 across the 400 simulated data sets with a standard deviation of 0.2.

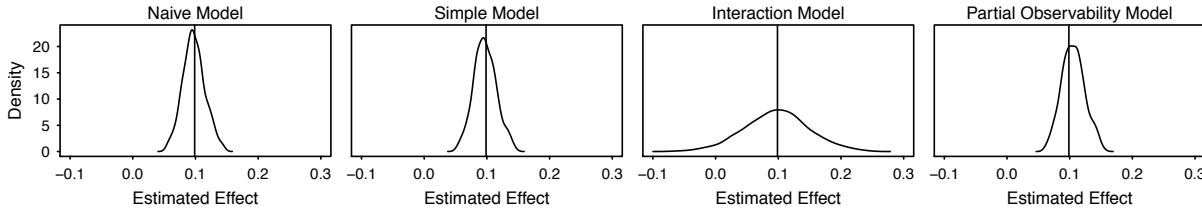


Figure 6: Density plots showing the variance in the estimates across 400 simulations using the four alternative approaches to modeling bias due to misreports. In this situation, there is no misreporting, so each approach should offer an unbiased estimate of the true effect. Notice that the simple model and the partial observability model offer unbiased estimates and efficiency similar to the naïve model.

Although the partial observability model has a certain theoretical appeal, the less appealing, but easier-to-implement, simple model performs better on an equally plausible DGP. Further, even if the partial observability model captures the true DGP, the simple approach removes most of the bias. Unless a researcher has strong reasons to believe that the partial observability model provides a better fit to the actual DGP than does the simple model, the simple approach emerges as a plausible way to proceed based on these simulations.

ESTIMATING THE EFFECTS OF PRESIDENTIAL COATTAILS AND INCUMBENCY IN THE PRESENCE OF OVER-REPORT: THE 1992 NES AND SES

To test these ideas on real data, we estimate the effects of presidential coattails and incumbency on the probability of voting for the Republican U.S. House candidate using the 1992 NES data and on the probability of voting for the Republican U.S. Senate candidate using the 1992 SES data. Our analysis focuses on these data because evidence presented in Figure 3 suggests that time has an important impact on House vote choice in these NES data, but little impact on Senate vote choice in these SES data – however, Figure 1 reveals that over-reporting is present in both

data sets.¹³ Thus, these data provide a nice test case for our models. We should expect our models to give us some improvement in the NES data, but little improvement in the SES data, since we rely on time to model misreports. However, in neither case should modeling misreport worsen estimates.

As did Wright (1990, 1992) and Carsey and Jackson (2001), we treat exit poll estimates as relatively unbiased markers for comparison. If ignoring misreports leads to estimates in the NES and SES data that differ from those based on the exit poll data, then we assume that misreports are biasing the estimates. To the extent that a modeling strategy pushes the estimates closer to the exit poll estimates, we assume that the modeling strategy is effective.

We first examine the estimates of the presidential coattails effect, presented in Figure 7. In both the NES and the SES data, the naïve approach (ignoring misreports) leads to an estimated effect of about 0.4 — that is, voting for Bush rather than Clinton increases the probability of voting for the Republican candidate by 0.4.¹⁴ However, the exit poll data suggest an effect closer to 0.6. Thus, we conclude that the misreports inherent in the NES and the SES data lead to a substantial downward bias in the estimated effect of presidential coattails.¹⁵ But can modeling the misreport remove some of the bias? No particular approach stood out as clearly removing a substantial amount of the bias. Indeed, each attempt to model the bias leads to estimates closer to the naïve estimate than to the exit poll estimate.

¹³ Incumbency also conditioned over-report in the 1992 NES House data. Obviously, the baseline vote choice model already specifies incumbency status variables.

¹⁴ Of course, obtaining first differences requires setting all covariates at specific values. Unless otherwise specified, we make predictions for a Republican who voted for Bush in a district with no incumbent.

¹⁵ Our findings regarding the directions of the bias in the presidential coattail and the incumbency estimates are consistent with those of Wright (1990, 1992).

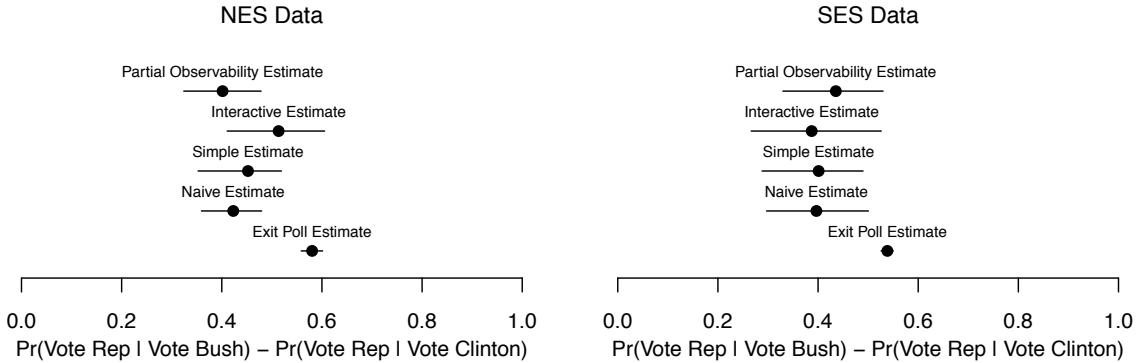


Figure 7: This figure compares the inferences about the effect of presidential coattails from each of our four proposed models of vote choice. The exit poll estimate represents the approximately correct inference. Notice that no approach among the four performs especially well, but the partial observability model does not substantially outperform the alternatives. In the NES data, the partial observability model performs worse than the naïve approach that ignores misreports altogether.

Now we turn to the estimated effects of incumbency. We discuss the effects of Republican incumbency and of Democratic incumbency separately because the models do not require the results to be similar, though they do closely mirror each other in our findings. Figure 8 presents the estimated effects of Republican incumbency. For the NES data, the naïve approach suggests a fairly large effect of about 0.1, while the exit poll data suggest a smaller effect of 0.03. Thus, we again have evidence that misreport seems to influence the estimates. But do the other models help us more in this situation? Notice first that the partial observability model produces an estimate almost identical to that of the naïve model. However, both the simple model and the interactive model reduce the bias in the estimates. In this situation, there is evidence of misreport, and it seems to increase over time, but the partial observability model does not capture this. The simpler alternatives do. In the SES, the estimates from the naïve approach and the exit poll data agree quite closely, so there is little room for improvement. However, it is important that a model not worsen the estimates in this situation, and none does.

We now turn to the effects of Democratic incumbency, presented in Figure 8. The naïve

approach suggests a large effect of nearly -0.2, while the exit poll data suggest a much smaller effect, nearly zero. The partial observability model, again, does little to improve on the naïve model estimate. However, both the simple approach and the interactive approach reduce the bias by over half. For the SES data, we see a small bias that gives our estimate the wrong sign, and no approach seems to work particularly well.

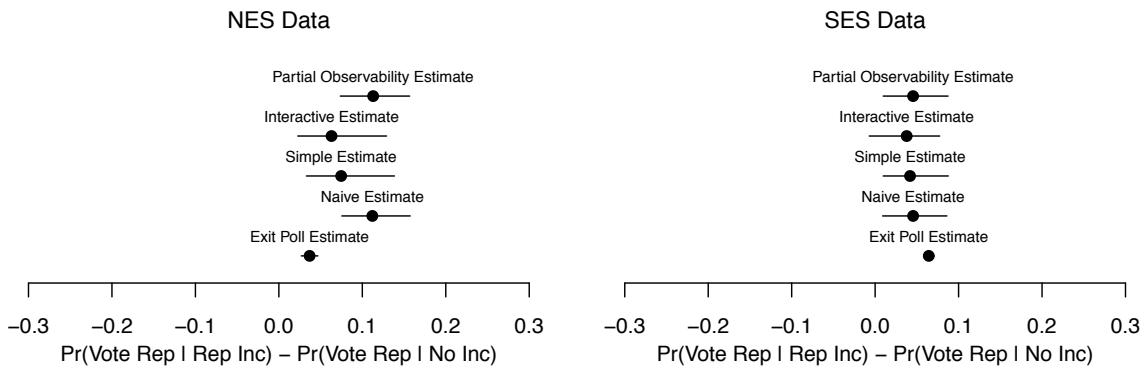


Figure 8: This figure compares the inferences about the effect of Republican incumbency from each of our four proposed models of vote choice. The exit poll estimate represents the approximately correct inference. Considering the NES data, notice that the partial observability model performs the worst among the three models that attempt to correct for misreports. There is little bias to correct in the SES data.

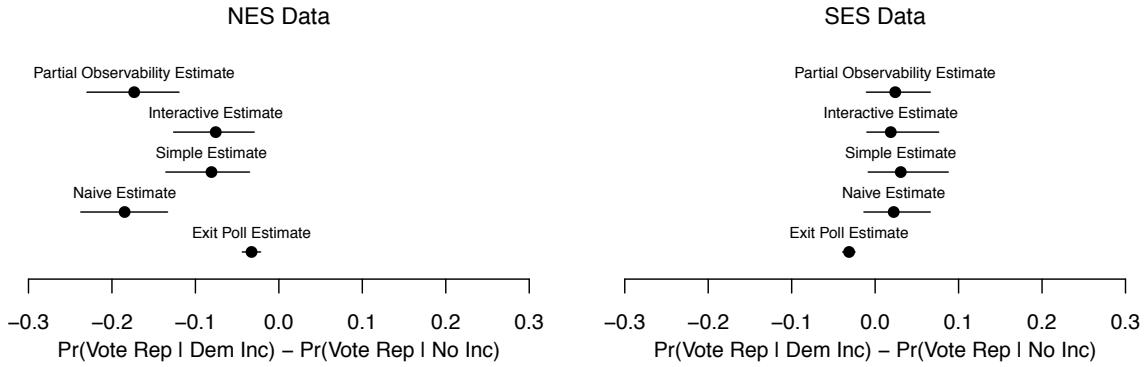


Figure 9: This figure compares the inferences about the effect of Democratic incumbency from each of our four proposed models of vote choice. The exit poll estimate represents the approximately correct inference. Considering the NES data, notice that the partial observability model performs the worst among the three models that attempt to correct for misreports. While there is some bias in the SES data, all four approaches yield similarly biased inferences.

Conclusions

We will conclude with observations on dealing specifically with vote misreport, as well as some observations regarding the partial observability model more generally. First, the best option for dealing with vote misreport is to collect quality data.¹⁶ For example, if misreport worsens as time passes between Election Day and the survey interview, collecting vote choice information as temporally close as possible to the vote seems advisable. Wright suggests that the ideal may be huge Election Day polls that “tap reports of behavior before they are contaminated by news of victors and post-hoc rationalizations of the election” (Wright 1990, p. 560). Of course, exit polls *are* huge Election Day polls. Gronke (1992, p. 123) also recognizes that exit polls minimize “the effect of history, be it contamination from post-election coverage, social interactions, rationalizations, bandwagons (since the winner is not yet known), or simple forgetting.” However, a common concern among scholars about exit polls is that they do not contain the breadth and depth of questions that academic surveys provide. In response to this concern, Wright (1993, p. 313) advocates for “short Election Day interviews, accompanied by longer pre- or post-election questionnaires that gather the larger volume of less time-sensitive data.” Implementing this suggestion would likely be a logistical nightmare and quite expensive, and, not surprisingly, those who administer and finance academic surveys have not taken up Wright’s call.¹⁷

¹⁶ In addition, designers of surveys must be cognizant of instrumentation effects. For example, since 1978 the NES has incorporated a ballot-style question format that presents respondents with the names of U.S. House candidates when they answer the House vote choice question. This change appears to have produced systematic bias in favor of over-reports for victors and incumbent winners in particular (Wright 1993; Box-Steffensmeier, Jacobson, and Grant 2000).

¹⁷ We can take some solace in the fact that the CCES, the most visible, new major source of academic survey data on voting behavior, does not appear to be associated with an over-report for the winner bias -- perhaps in part because the data collection takes place in a more compressed two week time period.

We suggest that scholars do not dismiss exit poll data when assessing vote choice. Most importantly, these data are unbiased. Exit poll data indeed accommodate only “simple,” parsimonious model specifications of the type we employ in this analysis. Yet, in terms of comparing the relative influence of variables on electoral behaviors (e.g., voter turnout and vote choice) in a meaningful fashion, there is much to recommend this type of specification. Across the past decades, the conventional approach of electoral scholars has been to specify in multivariate models an increasingly larger basket of (relatively) stable background factors (e.g., demographics and party identification) *and* more proximate, short-term attitudinal variables. However, Gelman and Hill (2007, pp. 190-94) warn against making inferences based on models that specify intervening or mediating variables, which short-term attitudinal measures clearly are in this set-up. The concern is “nonignorability—systematic differences between groups defined conditional on the post-treatment intermediate outcome” (Gelman and Hill, p. 193). Rather than those few scholars who incorporate exit poll data being on the defensive and habitually apologizing for doing “the best they could” with them, at this point some onus rests on the members of the larger community to defend and justify the status quo approach as they assess vote choice with frequently biased post-election survey data and questionably expansive model specifications.

In terms of selecting a model for assessing vote choice in the presence of over-report for the winner, although the partial observability model has an appealing intuition, simpler approaches are often just as reasonable and work easier. Our simulations show that when the partial observability model can represent the actual relationship, it works extremely well in removing bias. However, simpler approaches that specify as covariates variables thought to influence misreport work nearly as well. Simpler approaches can work better when the partial

observability model cannot represent the actual relationship, which undoubtedly is the norm. The analysis of the NES, SES, and exit poll data suggests a similar conclusion. Although theoretically appealing, the more complex partial observability estimator often fails to adjust the estimates, and simpler approaches often outperform it. We do not want to stake out an indefensible position that analysts should always rely on a simpler approach or always use a partial observability model. However, we would argue that a simpler approach often provides a plausible and acceptable modeling strategy, and that intuitive appeal and model complexity do not always deliver accurate substantive conclusions.¹⁸

¹⁸ Providing another assessment of the performance of the partial observability model, we have conducted a study that analyzes self-reported voter turnout in the 1984-1990 NES via a partial observability model that attempts to account for over-reports (of voter turnout). Importantly, *validated* voter turnout data are available for these years of the NES to produce the (relatively) unbiased markers for comparison. Again, these voter turnout results do not reveal that the partial observability inferences are superior to those from simpler models, including a naïve model. In fact, the partial observability inferences are frequently inferior in this application as well. What distinguishes both our vote choice results in the current study and these voter turnout results from those of the other applications (of which we are aware) of the partial observability model is that we are able to bring to bear “approximately true” data for comparison—exit poll data for vote choice and the validated data for voter turnout.

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Appendix I: Data Acknowledgements:

We thank Josh Kimrey for his research assistance in collecting the aggregate data on U.S. Senate elections. We thank Gary Jacobson for providing us with the data on U.S. House election returns and Jerry Wright for providing various datasets from his misreport analyses.

We downloaded the following datasets from the ICPSR:

American National Election Studies. American National Election Studies (ANES) Cumulative Data File, 1948-2008. ICPSR08475-v14. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2011-12-05. doi:10.3886/ICPSR08475.v14.

Miller, Warren E., Donald R. Kinder, Steven J. Rosenstone, and the National Election Studies. AMERICAN NATIONAL ELECTION STUDY: POOLED SENATE ELECTION STUDY, 1988, 1990, 1992 [Computer file], 3rd ICPSR version. Ann Arbor, MI: University of Michigan, Center for Political Studies [producer], 1999. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2005.

Voter Research and Surveys. VOTER RESEARCH AND SURVEYS GENERAL ELECTION EXIT POLLS, 1992 (Computer file). New York City, NY: Voter Research and Surveys (producer), 1992. 2nd release. Ann Arbor, MI: Inter-university Consortium for Political and Social Research (distributor), 1993.

Appendix II: Complete Estimates for Wright's Model

Table 1. (Pro-winner) Misreport Coefficients for U.S. House and Senate in the National Election Studies (NES) and the Senate Election Studies (SES), 1988-2008

	Senate	N	House	N	House (.20-.80)	N
NES						
1988	.267* (1.72)	841	.449*** (6.72)	798	.479*** (6.53)	772
1990	.848*** (5.68)	440	.735*** (7.01)	597	.775*** (7.11)	584
1992	.538*** (3.34)	962	.489*** (6.52)	1292	.558*** (6.87)	1250
1994	.187 (0.96)	781	.407*** (4.36)	835	.489*** (4.98)	814
1996	-.293 (-0.97)	608	.280*** (3.12)	991	.413*** (3.87)	939
1998	.366* (1.67)	415	.228** (2.03)	458	.339** (2.21)	430
2000	.250* (1.90)	732	.262*** (3.02)	810	.335*** (3.46)	779
2002	.324 (0.93)	380	.048 (0.40)	624	.021 (0.16)	610
2004	-.334** (-1.96)	515	.139 (1.28)	631	.309** (2.46)	578
2008	.780*** (4.35)	801	.010 (0.11)	1160	.020 (0.17)	1091
SES						
1988	.271** (1.98)	1224	.449*** (5.98)	1582	.544*** (6.75)	1523
1990	.687*** (5.00)	1347	.696*** (7.70)	1547	.789*** (7.48)	1503
1992	.517*** (3.08)	1158	-.066 (-0.58)	1222	-.059 (-0.47)	1197

Note: Misreport coefficient estimates based on the linear regression model:

Vote Difference = β_{cons} + $\beta_{Rep.Vote}Rep.$ Vote + ϵ . Estimates calculated for contests with two major party candidates, using sample weights with significance levels based on robust standard errors. Third column of coefficients excludes lopsided House races in which the Republican candidate received either less than 20 percent or more than 80 percent of the two-party vote. T-values in parentheses. *p < .1, **p < .05, ***p < .01 (two-tailed).

Appendix III: Complete Estimates for the Interactive Model

Table 2. Incumbency and Time of Interview Effects on (Pro-winner) Misreport for U.S. House and Senate in the NES and SES, 1988-2008

	(Actual) Vote	Open Seat	Vote x Open Seat	Date	Vote x Date	Constant	N
NES Senate							
1988	.482** (2.01)	1.49*** (5.25)	-2.95*** (-5.56)	-.0015 (-0.28)	.0069 (0.63)	-.285** (-2.47)	841
1990	.667*** (2.69)	-3.30*** (-3.75)	5.10*** (3.76)	-.0014 (-0.25)	.0094 (0.81)	-.444*** (-3.64)	440
1992	.167 (0.53)	-1.57** (-2.07)	3.32** (2.04)	-.0065 (-1.40)	.012 (1.27)	-.099 (-0.62)	962
1994	.050 (0.18)	.871 (1.18)	-1.53 (-1.17)	-.0060 (-0.80)	.011 (0.83)	-.013 (-0.09)	781
1996	.013 (0.02)	.448 (1.19)	-1.06 (-1.42)	.0039 (0.26)	-.0010 (-0.03)	-.049 (-0.18)	608
1998	-.238 (-0.66)	-.366 (-1.04)	1.08 (1.50)	-.013* (-1.73)	.033** (2.07)	.0017 (0.01)	415
2000	.220 (0.81)	.860 (1.05)	-1.84 (-1.03)	-.0024 (-0.38)	.0031 (0.23)	-.129 (-0.99)	732
2002	.682 (1.16)	.063 (0.09)	-.035 (-0.03)	.030 (0.90)	-.057 (-0.86)	-.366 (-1.24)	380
2004	-.527 (-1.74)	-.224 (-1.06)	.411 (0.94)	-.0072 (-0.87)	.011 (0.65)	.275* (1.88)	515
2008	.391 (1.04)	.055 (0.13)	-.236 (-0.23)	-.0026 (-0.32)	.013 (0.87)	-.283 (-1.44)	801
SES Senate							
1988	.062 (0.14)	1.32*** (6.24)	-2.60*** (-6.83)	-.013 (-1.60)	.020 (1.32)	.063 (0.27)	1224
1990	.293 (0.86)	.412 (1.24)	-.747 (-1.32)	-.0098 (-1.51)	.019 (1.43)	-.126 (-0.76)	1347
1992	.752** (2.48)	.123 (0.29)	-.457 (-0.51)	.011 (1.16)	-.021 (-1.11)	-.381** (-2.47)	1158
NES House							
1988	.392*** (3.22)	.163 (0.94)	-.364 (-1.11)	-.0017 (-0.54)	.0049 (0.82)	-.174*** (2.94)	797
1990	.783*** (4.65)	.321 (0.80)	-.495 (-0.63)	.0027 (0.86)	-.0040 (-0.56)	-.411*** (-6.34)	597
1992	.180 (1.21)	.311*** (3.02)	-.563** (-2.49)	-.0070*** (-3.50)	.015*** (3.54)	-.129* (-1.86)	1292
1994	.154 (1.04)	.243 (0.79)	-.512 (-0.92)	-.010*** (-3.14)	.019*** (3.36)	-.070 (-0.86)	835
1996	.278* (1.80)	.0082 (0.04)	-.481 (-1.04)	-.00029 (-0.05)	.00085 (0.09)	-.101 (-1.21)	991
1998	.128 (0.69)	.530 (1.30)	-.985 (-1.17)	-.00044 (-0.14)	.0088 (1.26)	-.151* (-1.70)	458
2000	.326** (2.09)	-.164 (-0.71)	.304 (0.80)	.0048 (1.28)	-.0048 (-0.62)	-.212*** (-2.89)	810
2002	-.116 (-0.62)	.225 (0.73)	-.148 (-0.27)	-.0077 (-0.68)	.022 (1.20)	.037 (0.34)	624
2004	.013 (0.07)	.126 (0.65)	-.189 (-0.50)	-.0038 (-0.73)	.012 (1.15)	-.036 (-0.39)	631
2008	-.116 (-0.71)	-.283 (-1.13)	.673 (1.36)	-.0025 (-0.71)	.0056 (0.77)	.021 (0.27)	1160
SES House							
1988	.360 (1.28)	-.155 (-0.98)	.399 (1.34)	-.00067 (-0.13)	.0021 (0.21)	-.207 (-1.46)	1573
1990	.726*** (3.95)	.238 (1.50)	-.531 (-1.59)	.0022 (0.54)	-.00046 (-0.06)	-.353*** (-3.92)	1547
1992	.034 (0.17)	-.310** (-2.50)	.748*** (2.83)	.0044 (0.75)	-.014 (-1.14)	-.013 (-0.13)	1222

Note: Misreport coefficient estimates based on the interactive regression model:

Vote Difference =

$\beta_{\text{cons}} + \beta_{\text{Rep.Vote}} \text{Rep. Vote} + \beta_{\text{Open Seat}} \text{Open Seat} + \beta_{\text{Rep.Vote} \times \text{Open Seat}} \text{Rep. Vote} \times \text{Open Seat} + \beta_{\text{Date}} \text{Date} + \beta_{\text{Rep.Vote} \times \text{Date}} \text{Rep. Vote} \times \text{Date} + \varepsilon$. Estimates calculated using sample weights with significance levels based on robust standard errors. T-values in parentheses. *p < .1, **p < .05, *** p < .01 (two-tailed).