MODELING MISREPORTS IN SELF-REPORTED VOTE CHOICE DATA*

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Abstract: 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. Especially in regards to those data that are available for past elections, estimation-based strategies provide the only feasible, albeit less than optimal, 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 a troubling issue for 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

* We thank participants at the 2013 Southern Political Science Association Annual Conference, and James Garand in particular, for helpful comments. The data and code necessary to replicate these results and implement the methods we discuss are available at github.com/carlislerainey/misreports.

more fundamental problems for consumers of these data? If a respondent reporting a vote for the winner, when the respondent actually voted for the loser, occurs randomly, for example, it would not lead to biased coefficient estimates on 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 rather 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.

This study does two major things. First, it documents the extent to which misreport 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". 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. Especially in regards to those data that are available for past elections, estimation-based strategies provide the only feasible, albeit less than optimal, response in terms of addressing bias in vote choice models.

EVIDENCE OF PRO-WINNER MISREPORT, 1988-2008

Does misreport of vote choice persist across the past two decades of post-election NES voter surveys?[1] 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 House elections as those in which the winner received less than 80% of the two-party vote.[2] 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.

[1] 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.

[2] In the context of this study, c*ompetitive* 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.

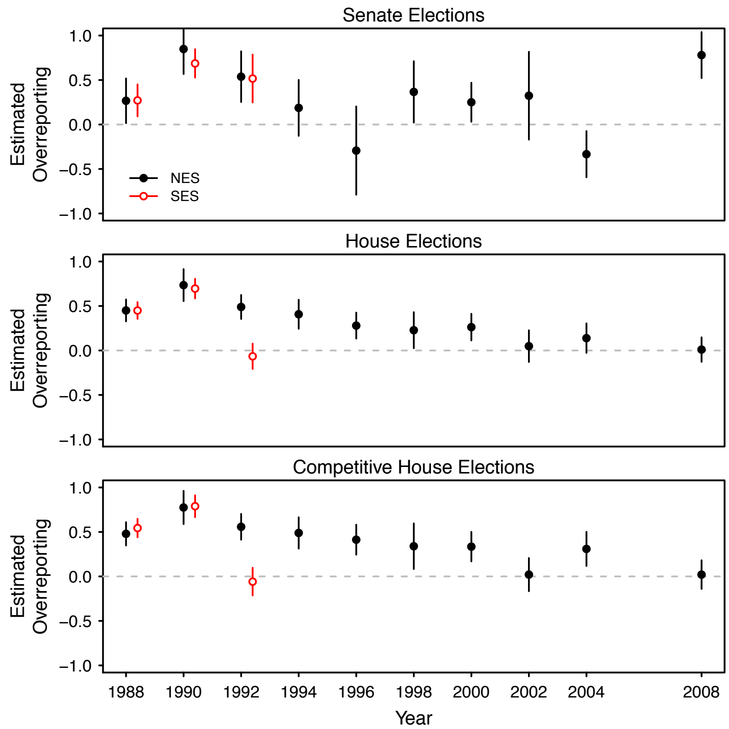

Figure 1: The estimated amount of misreporting in Senate, House, and competitive House elections from 1988 to 2008. We define competitive elections as those in which the winner received less than 80% of the two-party vote. We use Wright's (1993) approach (discussed in the text above) to estimate the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

Each positive coefficient reveals the percent by which the survey data over-estimate (or under-estimate if the coefficient is negative) the winner's margin of victory that year. 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.

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.[3] 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.[4]

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

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[3] The misreport coefficient for these data is a substantively negligible 0.02.
[4] Of course, over 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.

bias that only appears to be a pro-winner bias when all winners (incumbents and first-time winners) are analyzed together.

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 the interactive model

$$\begin{aligned} &\text{Interactive Model:} \\ \text{Vote Difference} = {} & \beta_{\text{cons}} + \beta_{\text{Rep. Vote}}\text{Rep. Vote} + \beta_{\text{Open Seat}}\text{Open Seat} \\ & + \beta_{\text{Rep. Vote} \times \text{Open Seat}}\text{Rep. Vote} \times \text{Open Seat} + \beta_{\text{Date}}\text{Date} \\ & + \beta_{\text{Rep.Vote} \times \text{Date}}\text{Rep. Vote} \times \text{Date} + \epsilon\,. \end{aligned}$$

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).

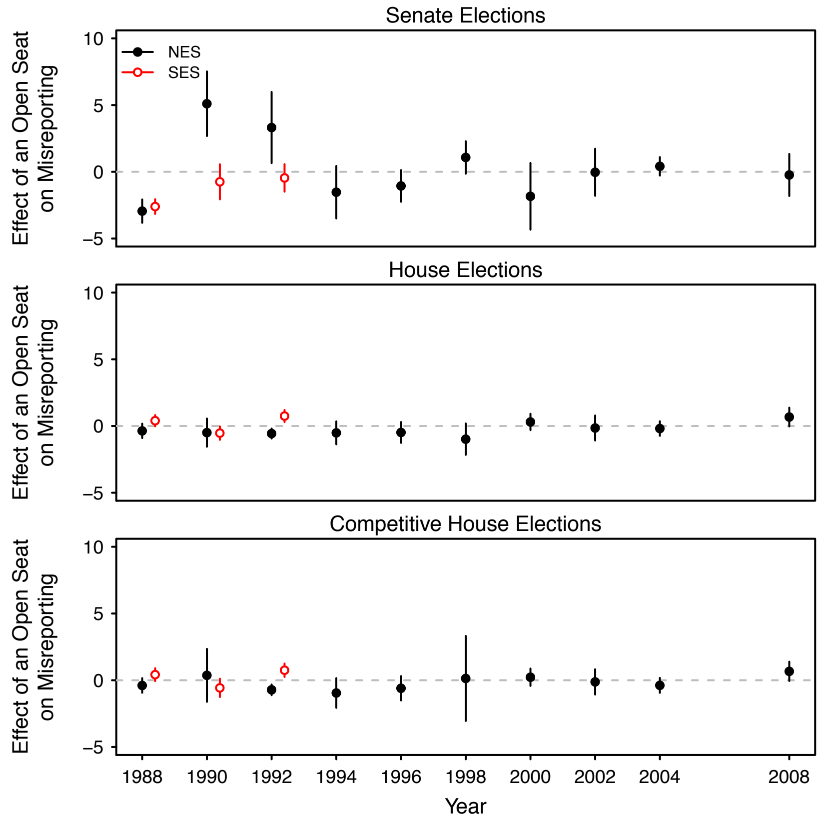

Figure 2: This figures shows the effect of an open seat on misreporting, assessing whether misreport for the winner might better be described as misreport for the incumbent. Notice that the pro-winner misreport phenomenon is not simply a by-product of most winners being incumbents. The point estimates 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.[5] 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.

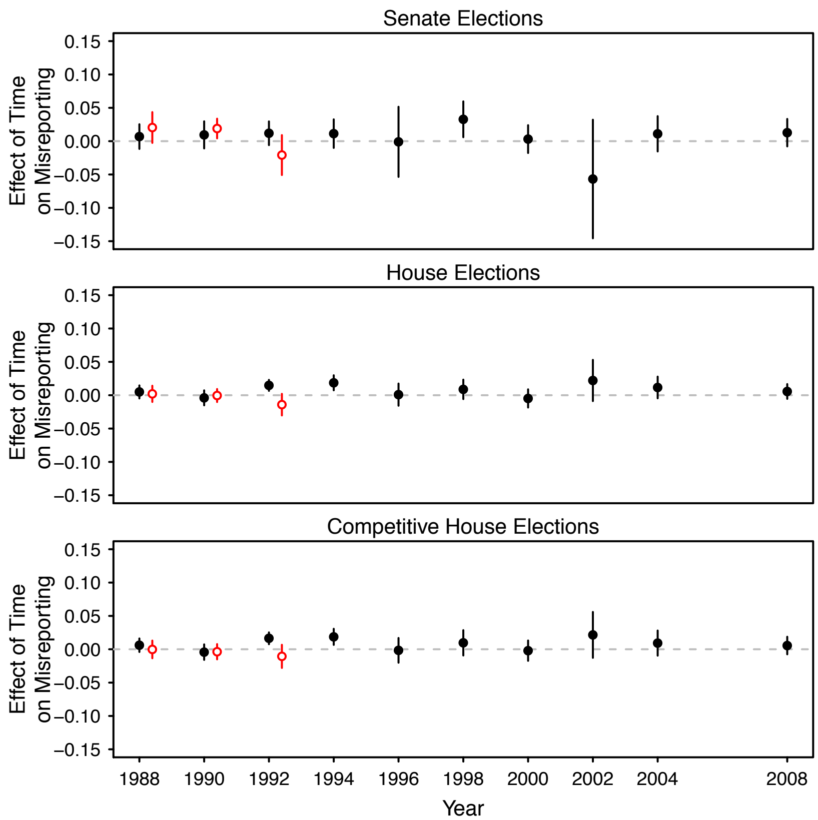

Figure 3: This figures shows the effect of time on misreporting, assessing whether survey respondents are more likely to misreport as the days since the election increase. The evidence for this effect is strongest in the 1992 and 1994 NES data for House elections. The point estimates and 90% confidence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

[5] Surprisingly, we do not reproduce Wright's (1990) finding of a significant conditioning effect for *Date of interview* 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

Partial observability models (e.g., Poirier 1980) are a form of finite mixture model (e.g., Imai and Tingley 2012) and split-population model (e.g., Svolik 2008) that researchers use to capture causal complexity (Braumoeller 2003), though sometimes these models are motivated to address measurement error (Beger et al. 2011). In fact, Beger et al. (2011) advocate incorporation of partial observability models when researchers assess behaviors that are subject to misreport bias when measured via a survey question. Although these models 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. Two examples illustrate:[6]

1. Przeworski and Vreeland (2002) argue that bilateral cooperation between two nation states occurs if and only if both are willing to cooperate. Thus, the observed outcome, cooperation, occurs as a result of two distinct processes. One process influences the willingness of the first state to cooperate. The second process influences the willingness of the second state to cooperate. Cooperation occurs, however, if and only if both states are willing to cooperate.
2. In Rainey and Jackson (2014), we suggest that respondents self-report turning out to vote if they actually turned out to vote or if they feel sufficient social pressure to report having voted. Thus, we model two latent processes, one influencing actual turnout and the other influencing whether the respondent feels sufficient social pressure to misreport voting. If either of these two events occurs, then respondents report turning out to vote.

[6] Additional applications in political science include Feinstein (1990), Przeworski and Vreeland (2000), Vreeland (2003), Stone (2008), and Xiang (2010).

While 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. Previously, we have evaluated a partial observability model in a setting in which the researcher believes that a key explanatory variable influences both latent outcomes (Rainey and Jackson 2014). In this study, we are able to assess the performance of a partial observability model when the sets of variables thought to influence each outcome do not overlap The question of immediate interest for applied researchers is whether the theoretically appealing partial observability estimator provides a compelling response to biased vote choice data – or do 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 the election, to those from Election Day exit poll data, we hope to gain insight into the accuracy of the inferences of the partial observability model. However, we do believe that our ideas generalize beyond the context of misreports of vote choice and offer additional insights into what researchers can and cannot learn from partial observability models.

In modeling the misreport process, we would like models 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 models, 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.

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

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

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 probabilities 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 (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.

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

A PARTIAL OBSERVABILITY MODEL OF MISREPORT

We develop our model of misreport around the idea that survey respondents increasingly misreport voting for the winner over time. 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 Pr(Accurate). If a Republican won the election, then the remaining fraction of the population would over-report for the Republican, denoted by $\Pr(\text{Misreport}_R)$. Similarly, in a district won by a Democrat, the proportion misreporting for the Democrat is denoted by $\Pr(\text{Misreport}_D)$.

To develop our model, we assume that $\Pr(\text{Misreport}_i)$ 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

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

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

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

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

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

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,

$$\Pr\left(\text{Self-Report}_{\text{R}}\right) = \Pr(\text{Vote}_{\text{R}}) \Pr(\text{Accurate Response}) + \Pr(\text{Misreport}_{\text{R}}) - \Pr(\text{Misreport}_{\text{D}}) ,$$

where

$$\Pr(\text{Accurate}) = 1 - \Pr(\text{Misreport}_{\text{R}}) - \Pr(\text{Misreport}_{\text{D}}) ,$$

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

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

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

$$X\beta = \beta_0 + \beta_1 ID_R + \beta_2 ID_D + \beta_3 Inc_R + \beta_4 Inc_D + \beta_5 Vote_{Bush} + \beta_6 Vote_{Clinton} .$$ [9]

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

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

and

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

respectively, where $Z_R\gamma = \log(a) + \log(t \times \text{Republican Winner})$ and $Z_R\gamma = \log(\delta) + \log(t \times \text{Democratic Winner})$.[10]

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[9] 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, Democrat, or Independent/Other (as the suppressed category)), respondent presidential vote (Bush vote, Clinton vote, or Perot vote (as the suppressed category)) to gauge presidential coattail effects, and state-level contextual variables that capture incumbency effects (Republican incumbent, Democratic incumbent, or 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).

[10] 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.

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

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

To estimate the model, we use MCMC sampling, evaluating convergence with Gelman and Rubin's (1990) $\hat{R}$ statistic. The model converges after about 500 iterations, and we use 2,000 samples after the burn-in to make inferences.

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

To evaluate the various modeling strategies when the true data-generating process (DGP) is known, we conduct three simulation studies. In each simulation study, we generate 400 fake data sets, and for each data set, we 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 this bias can be reduced using either the more complicated partial observability model or a simpler approach. 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 our models 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. Figure 4 shows the results from the 400 simulated data sets.

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 a 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.

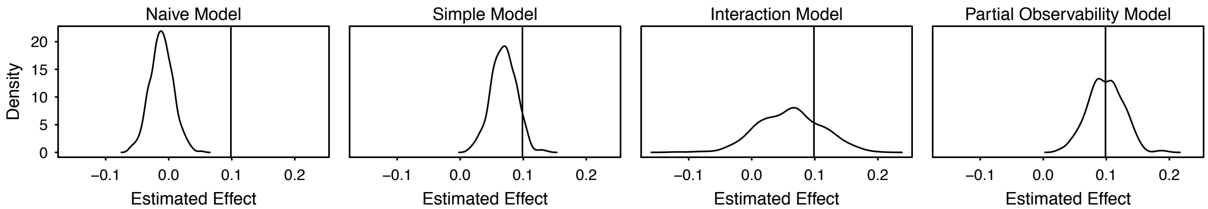

Figure 4: Density plots showing the variance in the estimates across 400 simulations using the four alternative approaches to modeling bias due to misreports. In this case, the partial observability model is used to generate the data, so it provides the best inferences. However, notice that the simple 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 $\Pr(Misreport) = 0.5\left(\frac{t}{50}\right)^3$ (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 our 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 *over*estimates 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.

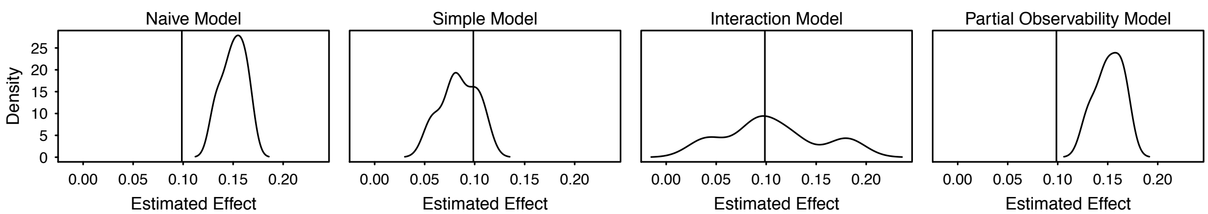

Figure 5: Density plots showing the variance in the estimates across 400 simulations using the four alternative approaches to modeling bias due to misreports. None of the models exactly matches the true data-generating process, though the process is substantively reasonable. In this case, the partial 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 an efficient estimate of the effect of interest. The average estimate across the 400 simulated data sets is 0.1 for the naïve mode—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.

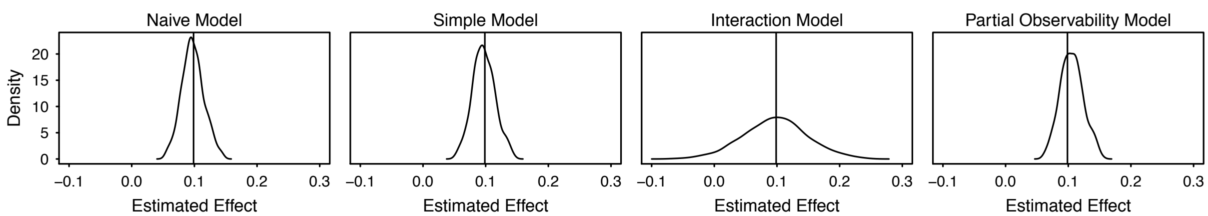

Figure 6: Density plots showing the variance in the estimates across 400 simulations using the four alternative approaches to modeling bias due to misreports. In this situation, there is no misreporting, so each approach should offer an unbiased estimate of the true effect. Notice that the simple model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

WHAT WE LEARN FROM THE SIMULATIONS

While 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 the 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 stands as a plausible way to proceed.

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. 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.[11] 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.[12] But can modeling the misreport remove some of the bias? No particular approach stood out as clearly removing a

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

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

substantial amount of the bias. Indeed, each attempt to model the bias leads to estimates much closer to the naïve estimate than to the exit poll estimate.

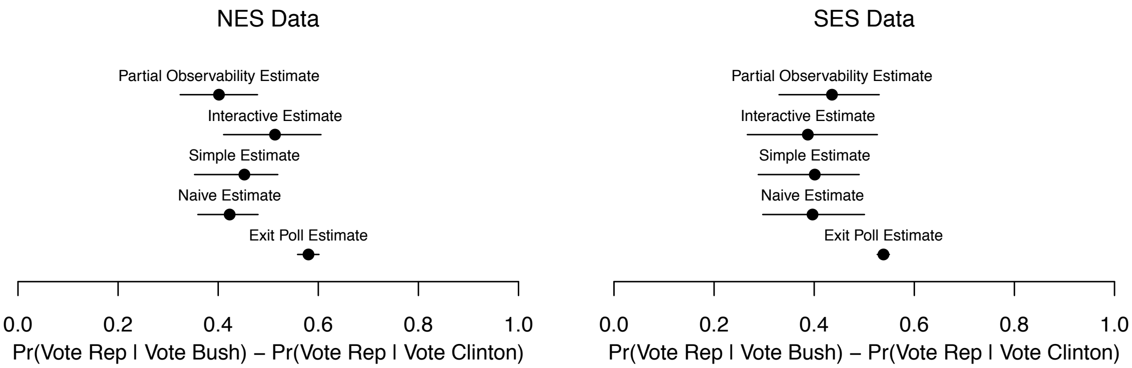

Figure 7: This figure compares the inferences about the effect of presidential coattails from each of our four proposed models of vote choice. The exit poll estimate represents the approximately correct inference. Notice that no approach among the four performs especially well, but the partial observability 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.

In light of the simulation studies and example applications, how should applied researchers model the effects of time passage in order to improve their estimates in models of vote choice? Our studies suggest that plausible representations of the theoretical process (e.g., the simple and partial observability approach) might decrease or increase the bias due to misreporting (see, for example, the left panel of Figure 7). In this situation, it is important that researchers summarize the variation in their results across models and demonstrate that their key conclusions are robust to most plausible alterative specifications. In light of our results, we recommend that researchers choose between the simple and partial observability model using theoretical guidance, model fit criteria (e.g., AIC/BIC), or both, and present their main results using that model. However, researchers should be prepared to demonstrate that their results are robust to the other approach.

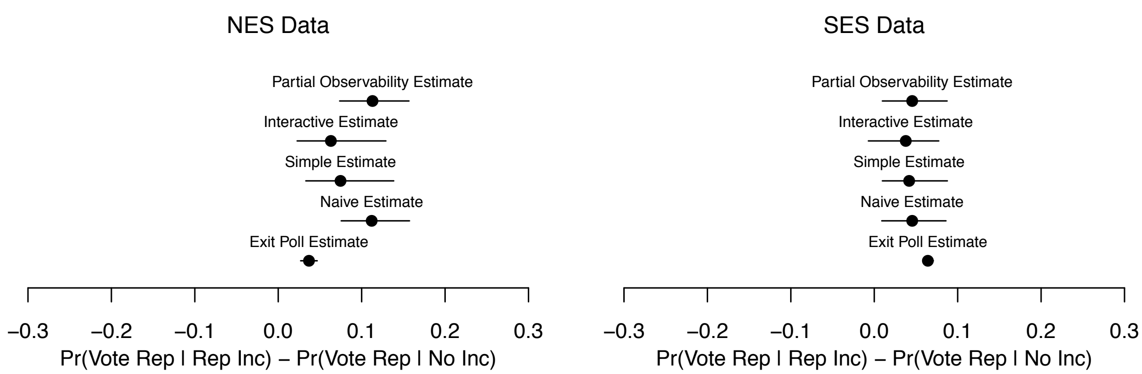

Figure 8: This figure compares the inferences about the effect of Republican incumbency from each of our four proposed models of vote choice. The exit poll estimate represents the approximately correct inference. Considering the NES data, notice that the partial observability model performs the worst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

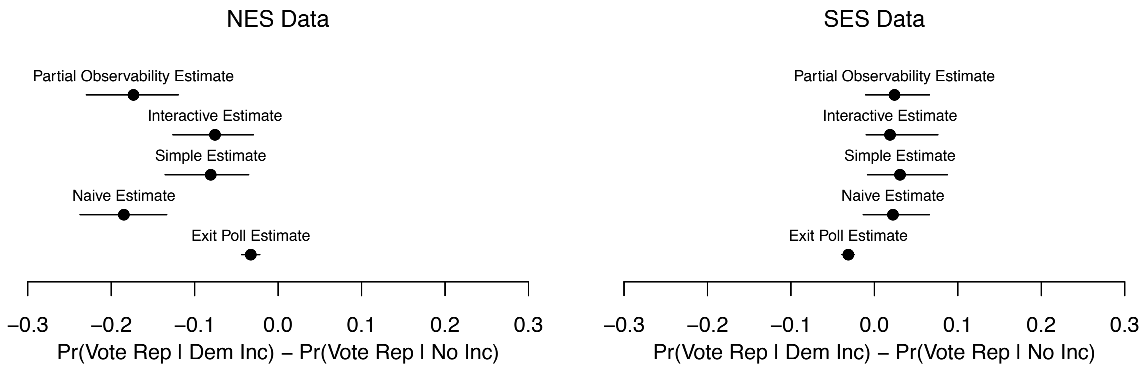

Figure 9: This figure compares the inferences about the effect of Democratic incumbency from each of our four proposed models of vote choice. The exit poll estimate represents the approximately correct inference. Considering the NES data, notice that the partial observability model performs the worst 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 observations regarding the partial observability model more generally. First, the best option for dealing with vote misreport is to collect better data.[13] 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,

[13] 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).

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.[14]

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, while the partial observability model has an appealing intuition, simpler approaches are often just as reasonable and easier to implement. 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

[14] 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.

misreport work nearly as well. Simpler approaches can work better, if the partial observability model cannot represent the actual relationship. The analysis of the NES, SES, and exit poll data suggests a similar conclusion. The theoretically appealing, but more complex, partial observability estimator often fails to adjust the estimates, and is often outperformed by the simpler approaches. 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.[15]

[15] Providing another assessment of the performance of the partial observability model, we have undertaken a similar analysis that relies on the validated voter turnout data in the 1984-1990 NES. As introduced briefly in the Modeling the Misreport Process section above, this application (Rainey and Jackson 2014) is an assessment of self-reported voter turnout in the 1984-1990 NES via a partial observability model that attempts to account for over-reports (of voter turnout) and for which *validated* voter turnout data are also available in 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:

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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		House		House (.20-.80)	
NES		N		N		N
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: $Vote_{dif} = a + b_1(Vote_{actual}) + e$. 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 of Interview	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 regression model: $Vote_{dif} = a + b_1(Vote_{actual}) + b_2(\text{Open seat}) + b_3(Vote_{actual} \times \text{Open seat}) + b_4(\text{Date of interview}) + b_5(Vote_{actual} \times \text{Date of interview}) + e$. 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).