Voter Overrepresentation, Vote Misreporting, and Turnout Bias in Postelection Surveys

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ABSTRACT

Figures from postelection surveys often grossly overestimate election turnout. Two distinct phenomena are responsible for this gap: overrepresentation of actual voters and vote misreporting by actual nonvoters among survey respondents. Previous accounts of turnout bias are inconclusive in that they fail to separate between the two (see Burden, 2000; 2003; Martinez, 2003; and McDonald, 2003). In this paper, we decompose turnout bias into its constituent parts, assess their empirical prevalence and heterogeneity using an extensive collection of 47 vote validation studies (VVS), and employ meta regression techniques to account for cross-study differences. Our results indicate that both election and survey characteristics differentially affect the components of turnout bias. To validate, we predict bias components and compound turnout bias in 'fresh' postelection surveys for which VVS data are unavailable. We conclude with a discussion of the threats and potentials of our findings for survey-based comparative electoral research.

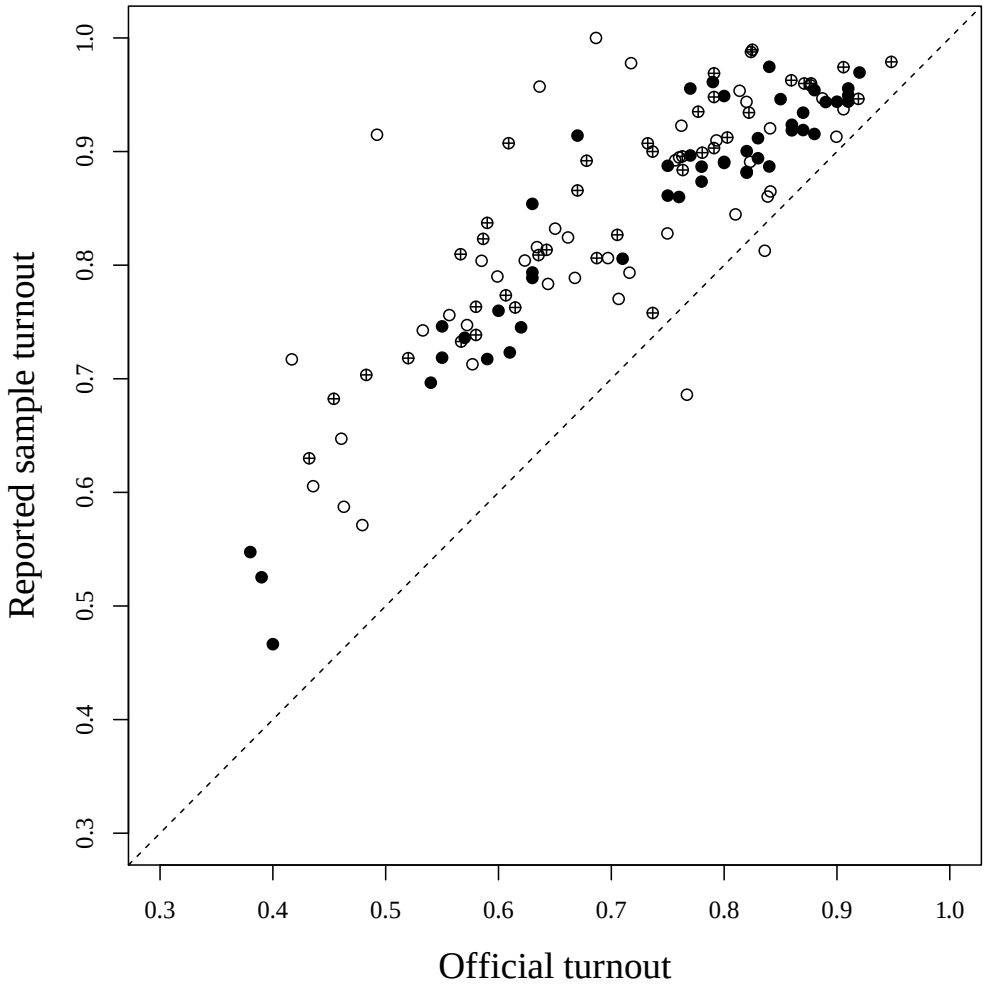
1. INTRODUCTION

Rarely do survey data confront reality as bluntly as when comparing estimates of voter turnout from postelection polls to official statistics. Survey-based figures commonly exceed official turnout by large margins, a phenomenon frequently dubbed 'vote overreporting' in the pertinent literature. Figure 1 plots sample turnout rates from 128 postelection studies in 43 countries versus official election turnout. All but two surveys in fact overreport turnout,[1] the average difference between sample and official turnout is 14%, and the maximum observed discrepancy amounts to a staggering 42% at the 2005 national elections in Albania. Vote overreporting thus certainly ranks among the big annoyances of survey-based electoral research, as it threatens both the general credibility of survey data and the validity of conclusions drawn from studies of individual political behavior and attitudes (Bernstein, Chadha and Montjoy, 2001; Brehm, 1993; Cassel, 2003; Hill and Hurley, 1984; Jones, 2008; Karp and Brockington, 2005; Katosh and Traugott, 1981; Presser and Traugott, 1992; Sigelman, 1982; Tittle and Hill, 1967).

While amply used in the literature, the term vote overreporting seems a misnomer, as it muddles two distinct causes of turnout bias: *over*representation of actual voters due to the disproportionate self-selection of politically involved citizens into realized election survey samples (e.g., Brehm, 1993; Jackman, 1999; Voogt and Saris, 2003), and vote mis*reporting* by actual nonvoters among survey respondents due to pressures of social desirability and other psychological processes (e.g., Belli et al., 1999; Presser, 1990). Therefore, it is surveys, not just respondents, that overstate electoral participation. Terminological quibbles aside, it is important to keep these two concepts separate, first, since the methodological issues arising from each differ – selection bias in the former (see Achen, 1986; Heckman, 1979), and misclassification bias in the latter case (see Hausman, Abrevaya and Scott-Morton, 1998) – and thus call for different strategies when modeling, for instance, individual turnout propensities using survey data (e.g., Brehm, 1993; Brehm, 1999; Deufel and Kedar, 2010; Katz and Katz, 2010). Second, voter overrepresentation and vote misreporting may be linked through common factors, though in inconsistent ways – a possibility that renders any attempt to understand the phenomenon 'from above' essentially hopeless (Tourangeau, Groves and Red-

[1] The most conspicuous case of vote *under*reporting (the circle markedly below the diagonal line in Figure 1) occurs with the 2008 parliamentary election in Belarus which, according to official observers, clearly fell short of OSCE commitments for democratic elections (see OSCE, 2008). It is a matter of speculation whether such massive vote underreporting may not be a consequence of polished official turnout figures and / or an expression of protest by actual voters among survey respondents.

FIGURE 1: Reported sample turnout rates from 128 postelection surveys versus official turnout. Data are taken from Modules 1 to 3 of the *Comparative Study of Electoral Systems (CSES)*, and from a collection of election surveys for which vote validation studies (VVS) are available. For more detailed information, see the Appendix to this paper.



line, 2010).

To illustrate, consider the lively exchange between Burden (2000; 2003), McDonald (2003) and Martinez (2003) on why turnout bias in the *American National Election Studies (ANES)* has more than doubled over the past decades, from 12% in the 1950s to 25% in the 1990s. Burden (2000) attributes rising turnout bias in ANES surveys to declining response rates that supposedly exacerbate the overrepresentation of voters. To empirically support his conjecture, he regresses (compound) turnout bias in 11 ANES surveys on response rates and indeed finds a significantly negative relationship. McDonald (2003) criticizes Burden's (2000) analysis on measurement as well as theoretical grounds, and suggests that declining official turnout is the chief culprit since, with decreased turnout, there is a higher share of nonvoters among survey respondents 'at risk' of misreporting their true voting behavior. Though equipped with a more appropriate estimate of official turnout, McDonald still considers compound turnout bias, as does Martinez (2003) who makes voter overrepresentation due to panel conditioning responsible for some particularly pronounced discrepancies between sample and official turnout. As plausible as these arguments may appear, there is simply no way to judge their empirical merits unless one examines the components of turnout bias separately. For instance, it may well be that increased response rates reduce voter overrepresentation yet at the same time, also draw more embarrassed nonvoters into the sample who are more likely to misreport (Tourangeau and Yan, 2007). Likewise, higher turnout evidently reduces the population of nonvoters at risk of misreporting, but may also put up social desirability pressures, so that the individual propensity to misreport among the (fewer) nonvoters participating in election surveys increases (see Karp and Brockington, 2005). Looking at compound turnout bias alone simply will not provide any conclusive answer.

Vote validation studies (VVS) that examine official records to verify self-reported votes in election surveys offer invaluable information about both sources of bias (for an overview, see Traugott, 2008). As of yet, however, such studies have almost exclusively been utilized in isolation to determine the magnitude and *individual* correlates of misreporting at a given election[2]

[2] Obviously, VVS are silent about individual correlates of voter overrepresentation, as such studies only allow for observing respondents', but not nonrespondents' characteristics. Other paradata generated as a by-product of the survey data collection process (e.g., contact histories, interviewer observations of should-be respondents refusing to participate in the survey) are needed to determine individual correlates of voter overrepresentation (e.g., Brehm, 1993; Stoop, 2005). However, as we demonstrate in the subsequent section, VVS data can be used to gauge at least the magnitude of voter overrepresentation in a study.

– an approach ill-suited for shedding light on the puzzling observation that turnout bias varies so widely across elections and surveys within a single country (as observed by Burden, McDonald and Martinez in the ANES case), but also across countries. Reconsidering Figure 1, the standard deviation of the differences between reported and official turnout rates across election studies is as high as 7.5%. Accounting for this enormous scatter is the primary aim of the present study. To this end, we first formally decompose turnout bias in postelection surveys into its constituent parts, and then assess their empirical prevalence and heterogeneity across 47 VVS from six countries (marked with solid circles in Figure 1). Next, we identify election and survey characteristics that may account for eventual cross-study differences, and test their predictive value using Bayesian meta regression techniques that synthesize information from all the VVS. Subsequently, we will reassemble the components, and use our and alternative models' estimates to predict turnout bias in a 'fresh' collection of postelection surveys for which the relevant election- and study-level features are known, but vote validation data are unavailable (marked with crossed circles in Figure 1). The final section concludes with a discussion of the threats and potentials of our findings for survey-based electoral research, with a particular emphasis on comparative approaches.

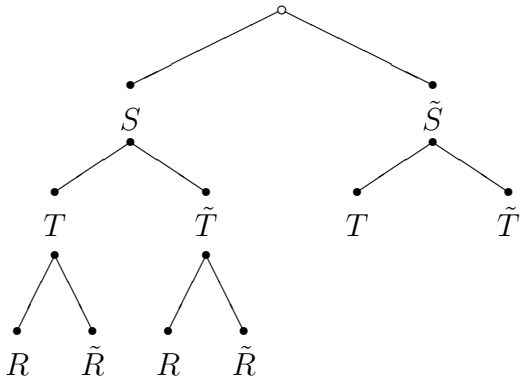
2. DECOMPOSING TURNOUT BIAS

In this section, we delineate our central concepts and their linkages. In doing so, we heavily draw on Jackman's (1999) treatment of nonresponse and misreporting bias in survey estimates of proportions, and Manski's (1995, ch. 4) approach to identification in response-based samples. By way of illustration, we initially set up the problem as a simple model of nested sets (see Figure 2). The first ply divides the *original survey sample* (as opposed to the *realized sample*) into two strata: the share of individual sampling units that are actually interviewed (i.e., the respondents), S, and the share of nonrespondents, $\tilde{S} = 1-S$, who cannot be reached during the field period or, probably more importantly, refuse to participate in the survey. Among both strata, there is a substratum of actual voters, T, and nonvoters, $\tilde{T} = 1 - T$. Finally, actual voters and nonvoters among survey respondents each divide into a subsubstratum that report that they voted, R, and that admittedly abstained from the voting booth, $\tilde{R} = 1 - R$. Evidently, R is the *reported sample turnout* rate, and *turnout bias*, B, can be written as

$$B = R - P, \tag{1}$$

where P is the population parameter of interest, that is, the *official turnout*

FIGURE 2: Survey participation S, true voting behavior T, and reported voting behavior R: a simple model of nested strata.



rate. In the discussion to follow, we assume that (1) official turnout is appropriately measured in terms of the voting-eligible population (VEP) which also constitutes the sampling frame of most postelection surveys (see Holbrook and Heidbreder, 2010; Martinez, 2003), and that (2) survey estimates of turnout are corrected for potential deviations of the sampling process from simple random sampling (e.g., by using design weights), so that the expectation of the survey estimate will correspond to the population parameter if (a) either the realized sample equates the original sample, or nonvoters have the same propensity to participate in election surveys as voters, and (b) respondents correctly report their true voting behavior.

It is immediately obvious from our exposition that there are two kinds of misreporting in postelection surveys, both of which can be observed from vote validation data: nonvoters who report that they voted (which we will dub *type I misreporting*),

$$M_1 = \frac{R \cap (\tilde{T}|S)}{\tilde{T}|S} \tag{2}$$

and actual voters who erroneously indicate that they abstained from the election (*type II misreporting*),

$$M_2 = \frac{\tilde{R} \cap (T|S)}{T|S}. \tag{3}$$

Equipped with these definitions, we may ask how *true sample turnout*, $T|S$, and reported turnout, R, are linked. It is clear that, absent misreporting, $R = T|S$. On the other hand, with type I and type II misreporting rates

included, the reported sample turnout rate can be expressed as

$$R = (T|S) + M_1 \times (\tilde{T}|S) - M_2 \times (T|S) \tag{4}$$

Thus, vote misreporting among nonvoters may be partly counteracted by vote misreporting among voters in producing the reported sample turnout rate. Yet, albeit both types of misreporting may originate from common processes such as memory failure (e.g., Belli et al., 1999; Stocké and Stark, 2007), we would nevertheless expect the share of misreporting nonvoters, M_1, by far to outweigh misreporting by actual voters, M_2, primarily due to social desirability pressures (e.g., Tourangeau, Groves and Redline, 2010). For this reason, Jackman (1999) precludes type II misreporting in his model by assumption, while we leave the matter of how prevalent both types are to further empirical scrutiny.

Turning now to the definition of voter overrepresentation, we may next ask under what condition the true sample turnout rate, $T|S$, would approach the population parameter, P. Random sampling guarantees that the true turnout rate in the original sample, T, equals the population parameter, P, in the limit. However, the same will hold true for $T|S$ only if $T|S$ equals $T|\tilde{S}$, since P is composed of turnout rates among respondents and nonrespondents,

$$P = (T|S) \times S + (T|\tilde{S}) \times (1 - S). \tag{5}$$

This assumption seems quite heroic, since intuition and previous findings suggest that electoral participation and survey participation are products of common underlying causes (e.g., Brehm, 1993). If so, we would expect actual voters to be overrepresented in realized election survey samples, that is, $T|S > T|\tilde{S}$. *Voter overrepresentation*, O, may thus be defined in terms of the ratio of true voter proportions among respondents and nonrespondents,[3]

$$O = \frac{T|S}{T|\tilde{S}}, \tag{6}$$

which would assume values greater than unity if voters were indeed overrepresented among survey respondents. While both $T|S$ and $T|\tilde{S}$ are theoretical quantities that are invisible from postelection surveys alone, we may profit from VVS to directly observe $T|S$, and once again exploit the random sampling assumption to infer $T|\tilde{S}$ (see Manski, 1995, ch. 4). In particular, it follows from Equation 5 that

[3]In contrast, Jackman (1999) specifies voter misrepresentation in terms of the underrepresentation of nonvoters in realized survey samples.

$$T|\tilde{S} = \frac{P - (T|S) \times S}{\tilde{S}}. \tag{7}$$

Thus, Equation 6 can be rewritten (and voter overrepresentation be identified) as

$$O = \frac{(T|S) \times \tilde{S}}{P - (T|S) \times S}. \tag{8}$$

Having the three components of turnout bias written out that way, we may solve Equation 8 for $T|S$ and substitute this into Equation 4, so that the reported sample turnout, R, can be re-expressed as

$$\begin{aligned} R &= \frac{O \times P}{(O-1) \times S + 1} + M_1 \times \left(1 - \frac{O \times P}{(O-1) \times S + 1}\right) \\ &\quad - M_2 \times \frac{O \times P}{(O-1) \times S + 1} \\ &= \frac{(M_1 \times O - M_1) \times S + (1 - M_1 - M_2) \times O \times P + M_1}{(O-1) \times S + 1}. \end{aligned} \tag{9}$$

Finally, by substituting R in Equation 1 by the right-hand side of Equation 9, turnout bias, B, can be decomposed as

$$B = \frac{(M_1 \times O - M_1) \times S + (1 - M_1 - M_2) \times O \times P + M_1}{(O-1) \times S + 1} - P. \tag{10}$$

Note that this model of turnout bias naturally accounts for some logical constraints which are, perhaps, of minor empirical interest but nevertheless need to be taken into consideration when investigating the phenomenon. For instance, if official turnout, P, approaches 1, the share of nonvoters at risk of misreporting their voting behavior, M_1, converges toward 0 (see McDonald, 2003), and the voter ratio among respondents versus nonrespondents draws near 1. Compound turnout bias, B, in turn, can never exceed $1 - P$, as 1 constitutes the upper bound for reported sample turnout, R. That way, our formulation allows us to focus on the empirically relevant relationships between election and study characteristics on the one hand, and the components of turnout bias on the other.

3. VOTE VALIDATION STUDIES: A SYSTEMATIC REVIEW

In the previous section, we have presented definitions of turnout bias components that lend themselves to operationalization using data from vote val-

idation studies. Though indispensable for our purposes, VVS are relatively rarely conducted due to data privacy and economic constraints. Nevertheless, we were able to gather data from 47 such studies carried out in six countries: Ireland (2 studies), Norway (12), New Zealand (5), Sweden (16), the United Kingdom (4) and the United States (8). Most of these studies pertain to national elections (parliamentary or presidential), though four Norwegian studies were implemented subsequent to local elections.[4]

In this section, we will use Bayesian methods to combine study estimates of vote misreporting and voter overrepresentation, and to assess the degree of heterogeneity in these estimates across VVS. Bayesian methods offer a unified modeling framework for synthesizing information from different studies, to adjust for covariates through regression models (see section 4), and to make out-of-sample predictions for other studies (see section 5; Warn, Thompson and Spiegelhalter, 2002).

In notational terms, we index studies with $j = 1, 2, \ldots J$, where $J = 47$ is the number of election studies attended with a VVS component. Furthermore, we denote realized sample sizes N_j, with N_{1j} indicating the numbers of self-reported nonvoters, and N_{2j} the numbers self-reported voters in study j. We designate $\tilde{N}_j$ to represent the number of nonrespondents per study (i.e., the difference between original and realized sample sizes). As opposed to our previous treatment of misreporting and overrepresentation, we now also consider the fact that sample proportions (and ratios of proportions) are subject to sampling error, and denote the underlying true misreporting parameters, in line with Jackman's (1999) notation, η_{1j} and η_{2j}, and the overrepresentation parameter ω_j. All the parameters are modeled on the log scale to rid their bounds and to make them approximately normally distributed. Accordingly, the log proportion of nonvoters in study j who misreport their true voting behavior, M_{1j}, is assumed to follow a normal distribution with mean $\log(\eta_{1j})$ and known variances representing study-specific sampling error, $\hat{\sigma}^2_{M_{1j}}$,

$$\log(M_{1j}) \sim \mathrm{N}\left(\log(\eta_{1j}), \hat{\sigma}^2_{M_{1j}}\right). \tag{11}$$

The Taylor approximation may be used to estimate the variance of $\log(M_{1j})$ from the sample data (e.g., Cornell and Mulrow, 1999):[5]

[4]See the Appendix for more detailed information.

[5]Alternatively, we could model the binomial outcome data underlying M_{1j} (and the other summary measures) directly, thereby evading the assumption that $\hat{\sigma}^2_{\eta_{1j}}$ is known, although in fact it has to be estimated from the data (see Warn, Thompson and Spiegelhalter, 2002). However, given our relatively large sample sizes, differences in results should be (and, as we found in supplementary analyses, actually are) minuscule (see Thompson,

$$\hat{\sigma}^2_{M_{1j}} = \frac{1 - M_{1j}}{M_{1j}} \times \frac{1}{N_{1j}}. \tag{12}$$

Having accounted for the within-study variability of $\log(M_{1j})$ this way, $\log(\eta_{1j})$ is modeled as a function of a constant term (so to speak the grand propensity of nonvoters to misreport their voting behavior across studies), β_0, and a study-specific deviation from that grand propensity, u_j, for which we assume a normal distribution with mean zero and variance σ^2_u,

$$\begin{aligned} \log(\eta_{1j}) =& \beta_0 + u_j \\ u_j \sim& \mathrm{N}(0, \sigma^2_u). \end{aligned} \tag{13}$$

An analogous model of type II misreporting is set up by replacing observed and estimated shares of misreporting nonvoters, M_{1j} and η_{1j}, by the respective proportions of misreporting voters, M_{2j} and η_{2j}, and substituting N_{1j} with the number of self-reported voters, N_{2j}. To model voter overrepresentation, we replace M_{1j} and η_{1j} by O_j and ω_j, and use the Taylor approximation for the variance of the log ratio of proportions instead of the formula given in Equation 12 (see Cornell and Mulrow, 1999):

$$\hat{\sigma}^2_{O_j} = \frac{1}{N_j \times (T_j|S_j) \times (\tilde{T}_j|S_j)} + \frac{1}{\tilde{N}_j \times (T_j|\tilde{S}_j) \times (\tilde{T}_j|\tilde{S}_j)}, \tag{14}$$

where N_j is the number of respondents in the realized sample and $\tilde{N}_j$ is the number of nonrespondents.

Inference proceeds via Markov chain Monte Carlo (MCMC) methods to compute a joint posterior density for all the model parameters. We use WinBUGS to run the required computations (Lunn et al., 2000). Since the posterior distribution is produced by simulation, we can make direct inferences for the parameters of interest (i.e., η_{1j}, η_{2j}, and ω_j), instead of their log transforms. Bayesian methods require the specification of prior distributions on the model parameters. We use uninformative priors to let the data determine the parameters. In particular, we use normal priors with mean 0 and large variance for β_0, and vague uniform priors within a range of (0, 10) for the cross-study standard deviation, σ_u. To monitor convergence, we set up three chains with randomly chosen starting values for the parameters,

Turner and Warn, 2001). Therefore we maintain the computationally less demanding summary statistic models which proved to converge more steadily, particularly when it came to the more complex models in the preceding section. Additional results are available on request.

each with 20,000 iterations, of which we discard the first 10,000 before summarizing the parameters' posterior probabilities (up to 100,000 of 200,000 iterations with the more complex models below).

Figure 3 presents medians of the posterior densities for η_{1j}, η_{2j}, and ω_j plus their 90% credible intervals. Considering first type I misreporting, we can see from the last line in column 1 that, on average,[6] a remarkable proportion of about 0.28 of the nonvoters wrongly indicate that they voted. This proportion is highly variable across VVS, ranging from approximately 0.10 in the 1970 Swedish national election study to an astonishing 0.68 in the 1985 Norwegian Storting election survey. The cross-study standard deviation of these proportions is estimated 0.13. Expectedly, the results pertaining to type II misreporting are far less alerting. On average, less than 1% of the voters misreported their actual voting behavior, an estimate not significantly distinguishable from 0. After all, a maximum estimated percentage of about 3.4 occurs in the ANES midterm study 1990, perhaps making it worthwile to model the determinants of cross-study differences in η_{2j} as well. Finally, the voter overrepresentation parameter is, on average, 1.5, that is, there are 1.5 voters in the realized sample per voter among nonrespondents. There is not a single study in our review where voters did not disproportionately self-select into survey samples, i.e., all ω_j-estimates significantly exceed 1. And again, overrepresentation ratios are highly scattered across studies, with an estimated standard deviation of 0.4. Voter overrepresentation is strongest in the 1999 Norwegian local election study, with three times as many voters among respondents than among nonrespondents.

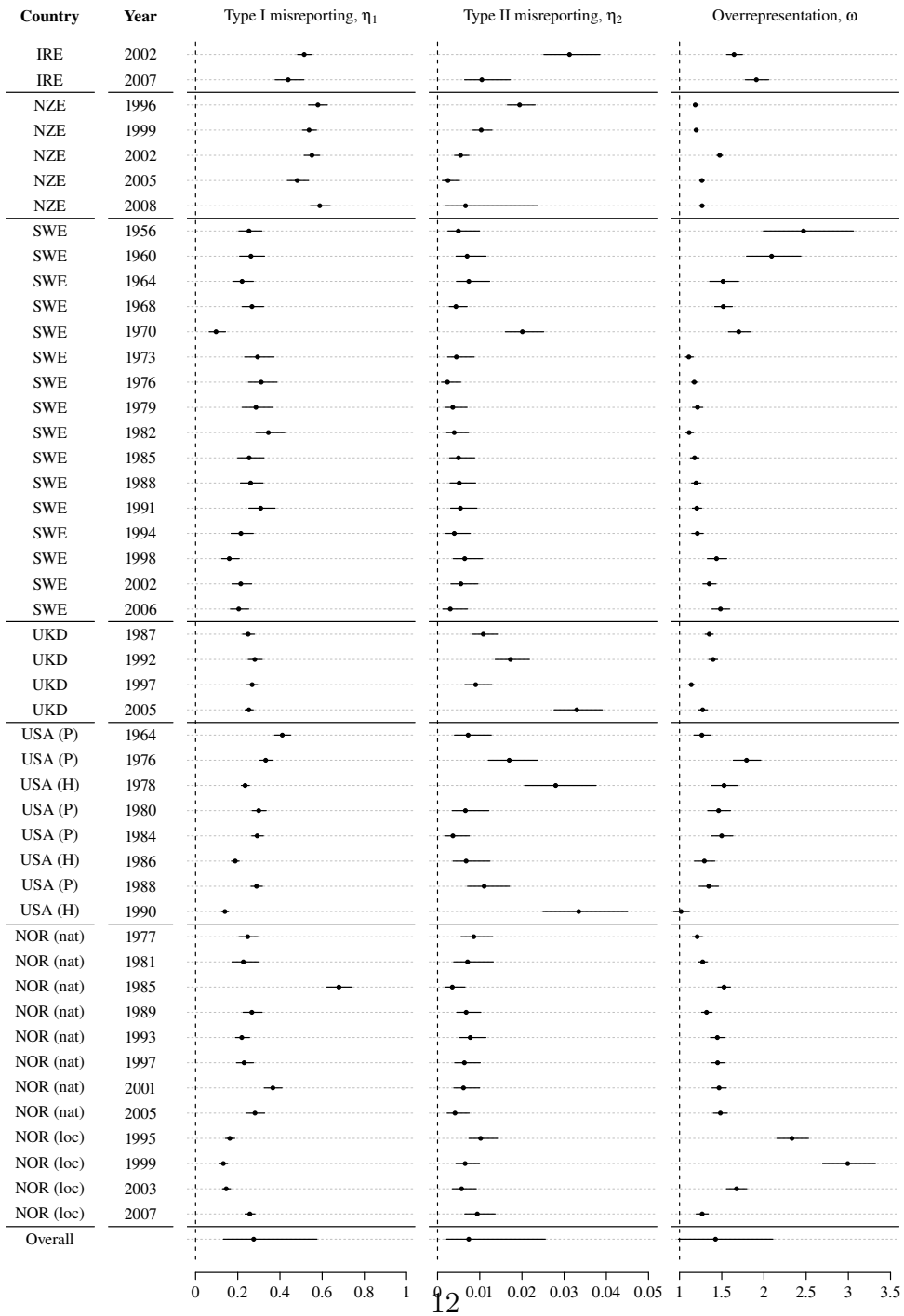
In sum, all the studies considered are affected by vote misreporting among nonvoters (but not necessarily among voters) and voter overrepresentation, although to varying degrees.

4. DETERMINANTS OF OVERREPRESENTATION AND MISREPORTING: META REGRESSION ANALYSES

Which election and study features may be responsible for the considerable cross-study variation in misreporting rates and overrepresentation ratios previously observed? Some authors suggest that the actual turnout rate, P, is the prime suspect. Actual voter turnout may affect the extent of turnout bias in several ways. First, the share of nonvoters at risk of misreporting their true behavior converges to 0 as official turnout rates approach 1 (see McDonald, 2003), a logical constraint that our model formulation in Equa-

[6] Overall predictions are based on the posterior density of β_0 with cross-study variance σ_u^2.

FIGURE 3: Vote misreporting by actual nonvoters (η_1) and voters (η_2), and voter overrepresentation (ω) in 47 vote validation studies: medians of the bias estimates' posterior densities (points) and their 90% credible intervals (lines).



tion 10 already takes care of. In a similar vein, the voter ratio among respondents and nonrespondents converges to 1. Under full turnout, there are simply no nonvoters to misreport their behavior, or to be underrepresented in the realized sample. The relatively low magnitudes of turnout bias to be observed in Figure 1 for surveys from high-turnout elections are indicative of these limiting conditions. More interestingly, actual turnout and components of turnout bias may be linked through psychological processes. In particular, Karp and Brockington (2005) demonstrate in their inventive analysis of 23 VVS from five countries that higher effective election turnout increases the propensity to misreporting among actual nonvoters participating in election surveys. Accordingly, actual (or anticipated) turnout sets the 'descriptive social norm' that prescribes how socially desirable the act of voting is (see Gerber and Rogers, 2009). Another related election characteristic pertains to the level of elections. It is frequently argued that citizens and political elites ascribe higher importance to national elections than to races at lower federal levels, and to presidential as compared to parliamentary elections (e.g. Norris, 1997). One may therefore reason whether higher-level and executive elections induce higher social pressures for nonvoters to misreport their behavior that are not wholly being captured by the fact that the former usually generate higher turnout levels. Indeed, Burden (2000), Martinez (2003) and McDonald (2003) all observe that turnout bias is more pronounced in ANES surveys on presidential than on midterm elections. In the same vein, Karp and Brockington (2005) find varying misreporting rates among nonvoters of 19, 22, and 26% in Swedish local, regional, and national elections, respectively. To capture eventual qualitative differences in bias components across electoral levels, we code local elections (indicated with 'loc' for Norway in Figure 3) and House elections ('H' as opposed to 'P' for presidential elections in the U.S. case) as second-order elections, SOE. Thus, 7 out of the 47 VVS under investigation pertain to second-order elections.

On the other hand, study characteristics may account for differences in voter overrepresentation and misreporting. As mentioned above, Burden (2000) hypothesizes that voter overrepresentation decreases with higher response rates, RR. Implicit in this conjecture is the notion of a 'continuum of resistance' (see Keeter et al., 2000), i.e., that nonvoters are generally more hesitant to participate in election surveys, and that increased response rates primarily result from the survey institutions' success to overcome the nonvoters' inhibitions to agree to be interviewed. At the same time, however, smaller overrepresentation bias might turn out to be a dearly purchased success when higher response rates draw more embarrassed nonvoters into the realized sample who are more likely then to misreport (Tourangeau and Yan, 2007). Response rates vary from 0.40 to 0.95 in our collection of VVS,

with a mean of 0.72. We also code a variable that covers the proportion of post-election interviews that were preceded by a pre-election *Panel* wave. As Martinez (2003) argues, the mere fact of being interviewed prior to the election may remind respondents of reasons why they might want to vote, and thus motivate some respondents to turn out in the election who may have abstained otherwise (also see Granberg and Holmberg, 1991; Smith, Gerber and Orlich, 2003; Yalch, 1976). Therefore, panel effects may be expected to increase voter overrepresentation by nonvoter conversion. 33 of our 47 VVS include panel components, but only in three studies all the respondents were interviewed prior to the election, which makes for an average proportion of 0.37 being potentially affected by such panel conditioning.[7]

To model type I misreporting (and, analogously, type II misreporting and voter overrepresentation) as a function of covariates, we simply add these variables to Equation 13. At this stage, we also include a country-level random effect, $v_{k(j)}$, to account for unobserved cultural or institutional differences (or temporally stable study and election characteristics) that potentially affect the turnout bias components,

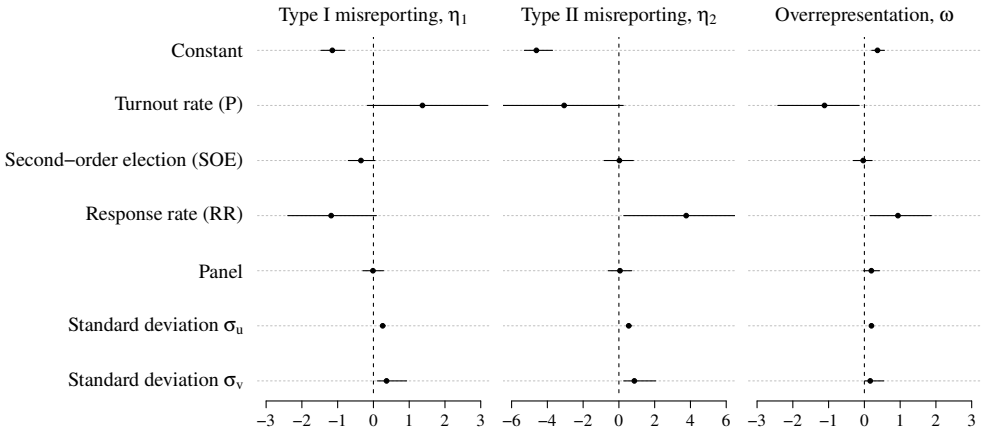
$$\begin{aligned} \log(\eta_{1j}) &= \beta_0 + \beta_1 P_j + \beta_2 SOE_j + \beta_3 RR_j + \beta_4 Panel_j + u_j + v_{k(j)}, \quad (15) \\ u_j &\sim \mathrm{N}(0, \sigma_u^2), \\ v_{k(j)} &\sim \mathrm{N}(0, \sigma_v^2). \end{aligned}$$

Figure 4 reports the corresponding results for the three models. As expected, higher actual turnout has an ambivalent effect on the bias components that any study of compound turnout bias simply sweeps under the carpet. While increased turnout alleviates voter overrepresentation (see McDonald, 2003), it tends to exacerbate the problem of misreporting among nonvoters, probably by putting up social desirability pressures (see Karp and Brockington, 2005). Interestingly, higher turnout also tends to reduce the portion of actual voters who mistakenly report that they abstained from the election. An ad hoc explanation of this finding may be that, with increased turnout, actual voters unable to recall their true voting behavior more and more consider voting as the default behavior and, in case of doubt, opt for

[7] To be sure, there are other factors that may also impinge on the bias components, for example, the interview mode (see Locander, Sudman and Bradburn, 1976; Tourangeau and Smith, 1996), the time passed between election day and interview date (see Belli et al., 1999; Stocké, 2007), or the success rates in VVS (see Traugott, Traugott and Presser, 1992). However, given the limited number of VVS that informs our study, we will focus our empirical analysis on the four determinants prominently discussed by Burden (2000; 2003), Martinez (2003) and McDonald (2003). Preliminary analyses suggest that the factors just mentioned have relatively little predictive power. Detailed results are available on request.

the perceived standard. Actual turnout being equal, we also tend to observe less misreporting by nonvoters in second-order, i.e., local and midterm elections, which supports the reasoning that normative standards and related social pressures vary qualitatively across types of elections. In contrast to the expectations, our results indicate that response rate is *positively* related to voter overrepresentation, and *negatively* to misreporting among nonvoters. At the same time, increased response rates seemingly come along with higher misreporting rates among voters. While we have difficulties to come up with a plausible ad hoc explanation for these findings, it is clear that Burden's (2000) and others' speculations of relevant differences between respondents and nonrespondents automatically vanishing with increasing response rates seem overly optimistic (also see Groves, 2006; Groves and Peytcheva, 2008). Finally, and in line with Martinez (2003), we find that voter overrepresentation increases with higher shares of respondents being interviewed in a pre-election panel wave, while misreporting rates remain unaffected.

FIGURE 4: Bayesian meta regression results: medians of the parameter estimates' posterior densities (points) and their 90% credible intervals (lines).



Hardly surprising, the factors included in our models cannot fully account for the observed cross-study variability in voter overrepresentation and vote misreporting. Both study- and country-specific random effects, u_j and $v_{k(j)}$, still vary considerably across studies, as indicated by the estimates of σ_u and σ_v. This suggests that other, as yet unmodeled, study, election and country characteristics affect the components of turnout bias. It is indeed a general problem of meta analytical approaches to completely identify possible

patterns of predictor variables, since (a) the number of studies that inform such analyses is usually limited (and so are the degrees of freedom), and (b) the potential for measuring relevant covariates heavily depends on the least common set of information the underlying studies document. For the time being, a hierarchical model specification including both study and country random effects therefore seems appropriate.

5. COMPOUND TURNOUT BIAS: OUT-OF-SAMPLE PREDICTIONS

How far does our model get us in understanding the phenomenon of turnout bias in general, that is, in studies other than the 47 VVS closely investigated in the preceding section? To answer this question, we draw on a collection of 39 'fresh' postelection studies from 18 countries included in Modules 1 to 3 of the *Comparative Study of Electoral Systems* for which the election- and study level covariates previously used to predict bias components are known, but for which vote validation data is unavailable.[8] In a first step, we combine covariate information from the selected CSES surveys with the posterior densities of our parameter estimates (including v_k in the case of the U.S., which is the only country for which both VVS and independent CSES data are available) to compute study-specific estimates of η_{1j}, η_{2j} and ω_j, which are then reassembled following Equation 10 to yield out-of-sample predictions for turnout bias contingent on the studies' covariate values.

As benchmarks, we also fit the compound turnout bias models according to Burden (2000), McDonald (2003) and Martinez (2003) to the 47 VVS, and use these parameter estimates to predict turnout bias in the CSES surveys.[9]

[8] In order to avoid irregularities as those encountered in the introductory section (see Footnote 1), we focus our analysis on countries with a flawless democratic record, as indicated by Polity IV values of 10 (see `http://www.systemicpeace.org/polity/polity4.htm`). Out-of-sample predictions for a wider collection of countries including those with lower Polity values came out less precisely, although the relative fit of alternative models (see below) was reconfirmed. Detailed results are available on request.

[9] The Burden model is a simple bivariate linear regression of turnout bias on survey response rates, which yields parameter estimates [standard errors] of $\hat{B}_j = 0.335[0.046] - 0.301[0.063]RR_j$, with a root mean squared error (RMSE) of 0.047. The McDonald model includes both response rates and official turnout, and yields $\hat{B}_j = 0.438[0.036] - 0.224[0.046]RR_j - 0.223[0.033]P_j$, RMSE = 0.033, with our VVS data. Finally, Martinez' model specification includes response rates, a second-order election dummy and proportions of respondents having been interviewed in a pre-election panel wave, $\hat{B} = 0.338[0.047] - 0.305[0.062]RR_j - 0.029[0.023]Panel_j + 0.025[0.019]SOE_j$, RMSE = 0.046. Some of these estimates are substantially different from those found by Burden, McDonald and Martinez on a sample of 11 *ANES* surveys. However, for comparative purposes, we consider it more appropriate to refit their models using the VVS data from which our own model's estimates derive.

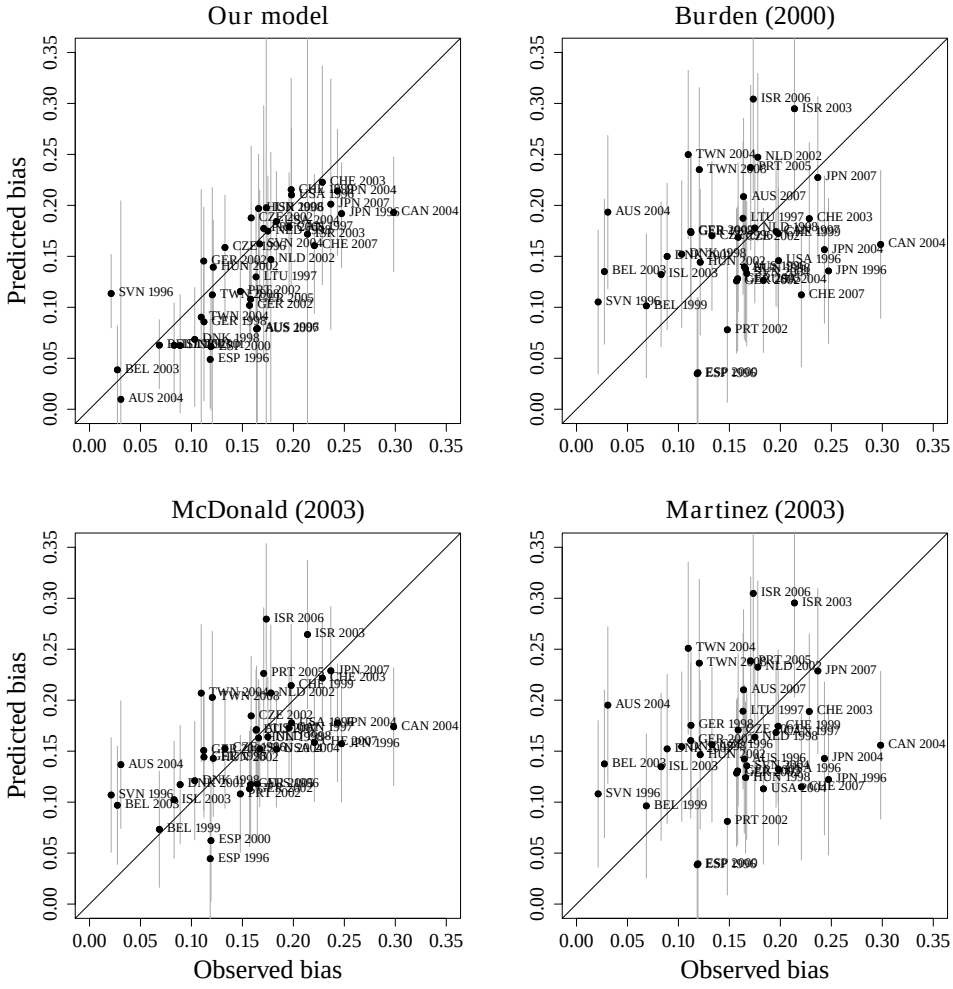
In validation terms, one important criterion will be the distance between observed turnout bias values and their estimates expressed as the empirical root mean squared error (RMSE) of (the median of) the estimates' posterior probabilities. Clearly, smaller RMSEs indicate better point estimates. As to these estimates' uncertainty, we calculate 90% Bayesian credible intervals from the highest posterior density regions that can be immediately interpreted in terms of the probability that the true value of the estimated parameter is inside a given interval. A second validation criterion is then the *coverage probability* of the credible intervals, that is, the proportion of the time that the intervals actually contain the observed value of interest. The actual coverage probability should approximate the nominal level of 90% as closely as possible. A final validation criterion bears on the logical constraints of turnout bias discussed in section 2. In particular, turnout bias, B_j, cannot exceed $1 - P_j$, since 1 constitutes the upper bound for reported sample turnout, R (see Equation 1). Thus, we count the relative number of times that $\hat{B}_j > 1 - P_j$, which should, of course, be zero with theoretically sound estimators.

Figure 5 pits predicted against observed turnout bias in the 39 selected CSES studies. Predictions are based on the four alternative models: the compound turnout bias models according to Burden (2000), McDonald (2003) and Martinez (2003) as well as our own. Eyeballing suggests that our model and McDonald's model based on official turnout and response rates fit the observed turnout bias data reasonably well, while Burden's response rate based model and Martinez' model including response rates and the second-order election and panel variable perform considerably worse. On closer examination, our model outperforms the McDonald and the other models along all the validation criteria (see Table 1). The RMSE for our model is just 0.043 and – by construction – it always yields estimates within the logically possible limits. In contrast, the other models yield out-of-bound predictions in 3 to 9 out of 39 cases, and produce RMSEs ranging from 0.054 for the McDonald model to 0.076 for Martinez'. The coverage probabilities are relatively far off their nominal level of 90% for all the models, indicating that all the predictors are overconfident. Nevertheless, our model comes closest to the nominal level, with a coverage probability of 77%. Much more importantly though, our model is the only one to give disaggregate insight into how election and study characteristics relate to the components of turnout bias.

6. DISCUSSION

In this study, we have demonstrated that turnout bias and its components vary tremendously across studies, elections and countries, and that part of

FIGURE 5: Observed versus predicted turnout bias: out-of-sample predictions from our model, and from model specifications according to Burden (2000), McDonald (2003) and Martinez (2003). Lines indicate 90% credible intervals.



	Our model	Burden (2000)	McDonald (2003)	Martinez (2003)
RMSE	0.043	0.074	0.054	0.076
Coverage Probability	0.769	0.692	0.718	0.692
No. of cases with Bhat > 1- P	0.000	0.231	0.077	0.231

TABLE 1: Empirical root mean squared errors (RMSE), coverage probabilities and out-of-bounds predictions (proportion $\hat{B}_j > 1 - P_j$) for four alternative models of turnout bias.

this heterogeneity emanates in a predictable manner from specific election features (actual turnout rates, types of election) and survey characteristics (response rates, panel vs. cross-sectional designs). So why should we bother? It is well known for the single-study case that misreporting and overrepresentation can considerably bias substantive findings (see Brehm, 1993; Cassel, 2003; Hill and Hurley, 1984; Jones, 2008; Karp and Brockington, 2005; Katosh and Traugott, 1981; Presser and Traugott, 1992; Sigelman, 1982; Tittle and Hill, 1967). For example, Bernstein, Chadha and Montjoy (2001, 41) in their comparison of voting behavior models based on self-reported turnout versus official records conclude that the use of reported vote "substantially distorts standard multivariate explanations of voting, increasing the apparent importance of independent variables that are related in the same direction to both overreporting and voting, while sharply decreasing the importance of independent variables related in opposing directions to those two variables. Those distortions can be severe enough to cause researchers to miss support for some hypotheses or falsely accept support for others." Thus far, however, the problem is less frequently appreciated in comparative work. Indeed, Karp and Brockington (2005, 838) are among the few to explicitly recognize that "comparisons between models of turnout that rely on reported and validated votes suggests that contextual influences on overreporting affect some explanatory variables at different rates, and that the participatory benefits of certain types of electoral arrangements might be somewhat overstated by studies that rely on individual level data."

Previous strategies to deal with the problem include, though with limited success, ex ante efforts to reduce vote misreporting by improved survey instruments and data collection procedures (e.g., Abelson, Loftus and Greenwald, 1992; Belli et al., 1999; Belli, Moore and VanHoewyk, 2006; Box-Steffensmeier, Jacobson and Grant, 2000; Duff et al., 2007; Gerber and Rogers, 2009; Holbrook and Krosnick, 2010b; Holbrook and Krosnick, 2010a; Presser, 1990; Silver, Anderson and Abramson, 1986), or to reduce self-selection via increased survey response rates (e.g., Arzheimer and Klein, 1999; Brehm, 1994; Keeter et al., 2000; Schmeets, 2010). More promising ex post attempts seek to identify demographic correlates of survey participation (e.g., Berinsky, 2004; Brehm, 1993; Sigelman, 1982; Voogt and Saris, 2003) and vote misreporting (e.g Abramson and Claggett, 1986; Cassel, 2004; Clausen, 1968; McDonald, 2007; Silver, Anderson and Abramson, 1986; Traugott, Traugott and Presser, 1992) using auxiliary data, and to use this information either to poststratify survey samples to population controls (e.g., Berinsky, 2006), or to simultaneously model overrepresentation and misreporting *and* the outcome of substantive interest in order to adjust for potential biases (e.g., Brehm, 1993; Deufel and Kedar, 2010; Katz and

Katz, 2010). The problem with such estimation solutions is that the required individual-level information – usually drawn from VVS and paradata generated as by-products of the survey data collection process (see Footnote 2) – will be unavailable in many circumstances. What we will usually know, however, is exactly the contextual information we have used to model the turnout bias components. Some comparative approaches directly employ this information to correct for turnout bias. For example, Brockington (2004; 2009) and Chen (2011) in their comparative studies of individual turnout adjust their dependent variables for overreporting relative to the country-specific gap between reported and official turnout. In his more elaborate model-based approach, Brehm (1999) uses U.S. state-specific survey response rates as proxies of voter overrepresentation, and includes this information in the first stage of a two-step sample selection model of individual voter turnout. However, as we have seen in the previous section, official turnout, response rate and other election and study features relate to the components of turnout bias in very specific ways, and at times even oppositely affect different components. From our perspective, it therefore seems more sensitive not to use contextual information directly to try to correct for turnout bias, but to use these data (together with our parameter estimates) to first predict study-specific vote misreporting rates and voter overrepresentation ratios, and then to employ the predicted bias components in estimation solutions that correct for sample selection (e.g., Brehm, 1993; Brehm, 1999) and misclassification problems (e.g., Deufel and Kedar, 2010; Katz and Katz, 2010).

APPENDIX

VOTE VALIDATION STUDIES

Data sources

All in all, data from 47 election studies with validated information about voting behavior could be gathered for six countries. Table 2 reports where they can be found in the web.

TABLE 2: Data sources

Country	Web site
Ireland	`http://www.tcd.ie/ines/`
Norway	`http://nsddata.nsd.uib.no/`
New Zealand	`http://www.nzes.org/`
Sweden	`http://www.snd.gu.se/`
United Kingdom	`http://www.esds.ac.uk/findingData/bes.asp`
USA	`http://www.electionstudies.org/`

Variable codings

Official turnout. Information on official turnout was taken from the International Institute for Democracy and Electoral Assistance (International IDEA) website[10]. For the Norway local elections, information was taken from the Statistics Norway webpage[11]. We used reconciled voting-age population (VAP) turnout for the U.S. elections as suggested and delivered by McDonald (2003).[12] For all other studies, we used the figures for voter turnout defined as the percentage of registered voters who actually voted.

Reported turnout. Table 3 reports the variables used to gain information about voting behavior. Only retrospective questions for the latest elections were used. Refusal of answer and the "Don't know" option were

[10] `http://www.idea.int/vt`

[11] `http://www.ssb.no/valgaktuelt/2001/tab-2001-08-13-02.html`

[12] `http://elections.gmu.edu/NES_Bias_Table.xls`.

coded as missing.

Validated turnout. All studies report information on the result of a vote validation procedure. Conditions for validation varied among the countries: In Ireland, official marked registers of electors are available for inspection up to six-months after an election (Marsh and Sinnott, 2008, 499). Concerning the other studies, Karp and Brockington (2005, 828) summarize: "In Sweden and Norway, the official voting records are centralized with the census, making it relatively easy and cost effective to validate respondent behavior. In both countries these records are nearly complete and accurate (Granberg and Holmberg, 1991; Waldahl and Aardal, 2000). In New Zealand, official records of whether a person has voted are held by the local registrar of electors and are available for public inspection. Voter registration is compulsory and the Electoral Enrolment Centre (EEC) compiles and maintains the electoral rolls. These records were made available to the New Zealand Election Study (NZES) which used the list to draw its post-election survey sample (Vowles et al., 1998; Vowles et al., 2002). Similarly, in Britain, the sample for the BES consists of persons who are on the electoral registers. Official records are compiled by the presiding officer at every polling station and later deposited with the Clerk of the Crown and are available for public inspection. In the United States, in contrast, voting records are kept locally making the validation process more difficult and more prone to error (see Presser, Traugott and Traugott, 1990)." Cases which were not clearly ascertainable were coded as missing.

Survey response rate. Information on the response rate was taken from documentation reports. We tried to ascertain that the measure of response rate follows the same standard, for which we took the AAPOR (2011) definition for "Response Rate 5", as this seemed to be the definition most commonly used (this also applies to the sample of CSES studies used later). Accordingly, the response rate "is the number of complete interviews divided by the number of interviews (complete plus partial) plus the number of non-interviews (refusal and break-off plus non-contacts plus others)", whereby cases of unknown eligibility are not regarded. However, we could not assure the use of this definition in some cases where the response outcomes were not fully itemized.

TABLE 3: Question wording for reported turnout variables in vote validation studies

Country	Year	Variable name	(Translated) Question	Answer categories
Ireland	2002	v0072	As you may know, many people did not vote in the recent general election. How about you? Did you vote in the general election in May 2002?	A I voted in the election B I did not vote in election C I thought about voting but didn't D I usually vote but didn't
	2007	v0072a	As you may know, many people did not vote in the recent general election. How about you? Did you vote in the general election in May 2007?	A Yes, voted B No, did not vote
New Zealand	1996	pp96vt	Some people didn't cast a party or an electorate vote in the election. Did you?	A Did not cast a party vote B Did cast a party vote
	1999, 2002, 2005, 2008	vot99p, wvot02p, yvot05p, zvt08p	Thinking now of the **party vote**, which party did you vote for in the 1999 [2002,2005,2008] election?	A Did not vote for a party B Another party C-G [names of parties]
Norway	1977, 1981	v255, var301	Did you vote in the election?	A Yes B No
	1985, 1989, 1993, 1997, 2001, 2005	v382, v326, v195, v165, v377, v220	Did you vote in the election this fall?	A Yes B No
	1995	v005	Did you vote in the local elections on September 11?	A Yes B No
	1999, 2003, 2007	v012, v022, v013	Did you vote in the local elections this year?	A Yes B No
Sweden	1956, 1960, 1964, 1968	v186, v147, v142, v429	Did you vote in the election this year?	A Yes B No
	1970, 1973, 1976, 1979, 1982, 1985, 1988, 1991, 1994, 1998, 2002, 2006	v161, v179, v227, v206, v169, v205, v181, v220, v251, v228, v302, v236	Did you vote in the election this year?	A Yes B Yes, but not in all elections C No, did not vote
United Kingdom	1987, 1992, 1997	v6a, v7a, voted	Talking to people about the general election, we have found that a lot of people didn't manage to vote. How about you - did you manage to vote in the general election?	A Yes, voted B No
	2005	bq12a	Talking with people about the general election on May 5th, we have found that a lot of people didn't manage to vote. How about you, did you manage to vote in the general election?	A Yes, voted B No
USA	1964	VCF9151	In talking to people about the election we find that a lot of people weren't able to vote because they weren't registered or they were sick or they just didn't have time. How about you, did you vote this time, or did something keep you from voting?	A No, did not vote B Yes, voted
	1976	VCF9151	In talking to people about the election we often find that a lot of people weren't able to vote because they weren't registered or they were sick or they just didn't have time. How about you, did you vote in the elections this fall?	A No, did not vote B Yes, voted
	1978, 1980, 1984, 1986, 1988, 1990	VCF9151	In talking to people about the election we often find that a lot of people weren't able to vote because they weren't registered or they were sick or they just didn't have time. How about you, did you vote in the elections this November?	A No, did not vote B Yes, voted

Design weights

Sampling weights which correct for differences in the probability of selection due to the survey design were only used in cases where they were not committed with population weights or similar non-design weights. Design weights are available for New Zealand (2008) and Norway (1995, 1999, 2003, 2007).

TABLE 4: Vote validation studies: Descriptives

	Country	Year	N	Turnout (off.)	Turnout (rep.)	Turnout (val.)	SOE	RR	Panel
1	Ireland	2002	2488	0.63	0.85	0.75	0	0.6	1
2	Ireland	2007	1087	0.67	0.91	0.86	0	0.54	0.89
3	New Zealand	1996	4988	0.88	0.95	0.93	0	0.63	0
4	New Zealand	1999	5969	0.85	0.96	0.92	0	0.64	0
5	New Zealand	2002	5768	0.77	0.95	0.91	0	0.52	0
6	New Zealand	2005	3740	0.8	0.95	0.88	0	0.46	0
7	New Zealand	2008	3042	0.79	0.96	0.89	0	0.4	0
8	Sweden	1956	1131	0.8	0.89	0.86	0	0.95	1
9	Sweden	1960	1436	0.86	0.92	0.91	0	0.91	0.53
10	Sweden	1964	1432	0.84	0.89	0.86	0	0.92	1
11	Sweden	1968	2928	0.89	0.94	0.93	0	0.88	0.98
12	Sweden	1970	2761	0.88	0.92	0.93	0	0.87	0.54
13	Sweden	1973	1415	0.91	0.94	0.93	0	0.82	1
14	Sweden	1976	2620	0.92	0.97	0.96	0	0.74	0.55
15	Sweden	1979	1665	0.91	0.96	0.94	0	0.81	1
16	Sweden	1982	1704	0.91	0.95	0.93	0	0.82	1
17	Sweden	1985	1704	0.9	0.94	0.93	0	0.78	1
18	Sweden	1988	1663	0.86	0.92	0.9	0	0.75	1
19	Sweden	1991	1558	0.87	0.93	0.91	0	0.73	0.93
20	Sweden	1994	1508	0.87	0.92	0.9	0	0.8	0.97
21	Sweden	1998	1484	0.82	0.88	0.87	0	0.82	0.97
22	Sweden	2002	1575	0.8	0.89	0.87	0	0.7	1
23	Sweden	2006	1706	0.82	0.9	0.88	0	0.8	1
24	United Kingdom	1987	3826	0.75	0.86	0.83	0	0.64	1
25	United Kingdom	1992	3528	0.78	0.87	0.85	0	0.73	1
26	United Kingdom	1997	3615	0.71	0.81	0.74	0	0.62	1
27	United Kingdom	2005	4130	0.61	0.75	0.69	0	0.65	0.23
28	USA	1964	1450	0.63	0.79	0.66	0	0.81	1
29	USA	1976	1909	0.55	0.75	0.64	0	0.7	1
30	USA	1978	2304	0.38	0.55	0.43	1	0.69	1
31	USA	1980	1408	0.55	0.72	0.6	0	0.72	1
32	USA	1984	1989	0.57	0.74	0.63	0	0.72	0.49
33	USA	1986	2176	0.39	0.53	0.42	1	0.68	1
34	USA	1988	1775	0.54	0.7	0.58	0	0.71	1
35	USA	1990	1980	0.4	0.47	0.4	1	0.71	1
36	NOR	1977	1730	0.83	0.89	0.87	0	0.73	0
37	NOR	1981	1591	0.82	0.88	0.86	0	0.8	1
38	NOR	1985	2180	0.84	0.97	0.93	0	0.73	0.36
39	NOR	1989	2193	0.83	0.91	0.89	0	0.73	0.45
40	NOR	1993	2193	0.76	0.86	0.83	0	0.73	0.45
41	NOR	1997	2055	0.78	0.89	0.86	0	0.7	0.45
42	NOR	2001	2052	0.75	0.89	0.83	0	0.7	0.44
43	NOR	2005	2012	0.77	0.9	0.86	0	0.68	0.43
44	NOR	1995	3036	0.63	0.79	0.76	1	0.71	0
45	NOR	1999	3191	0.6	0.76	0.74	1	0.74	0.49
46	NOR	2003	2737	0.59	0.73	0.69	1	0.69	0.61
47	NOR	2007	26391	0.62	0.76	0.68	1	0.67	0.39

CSES DATA

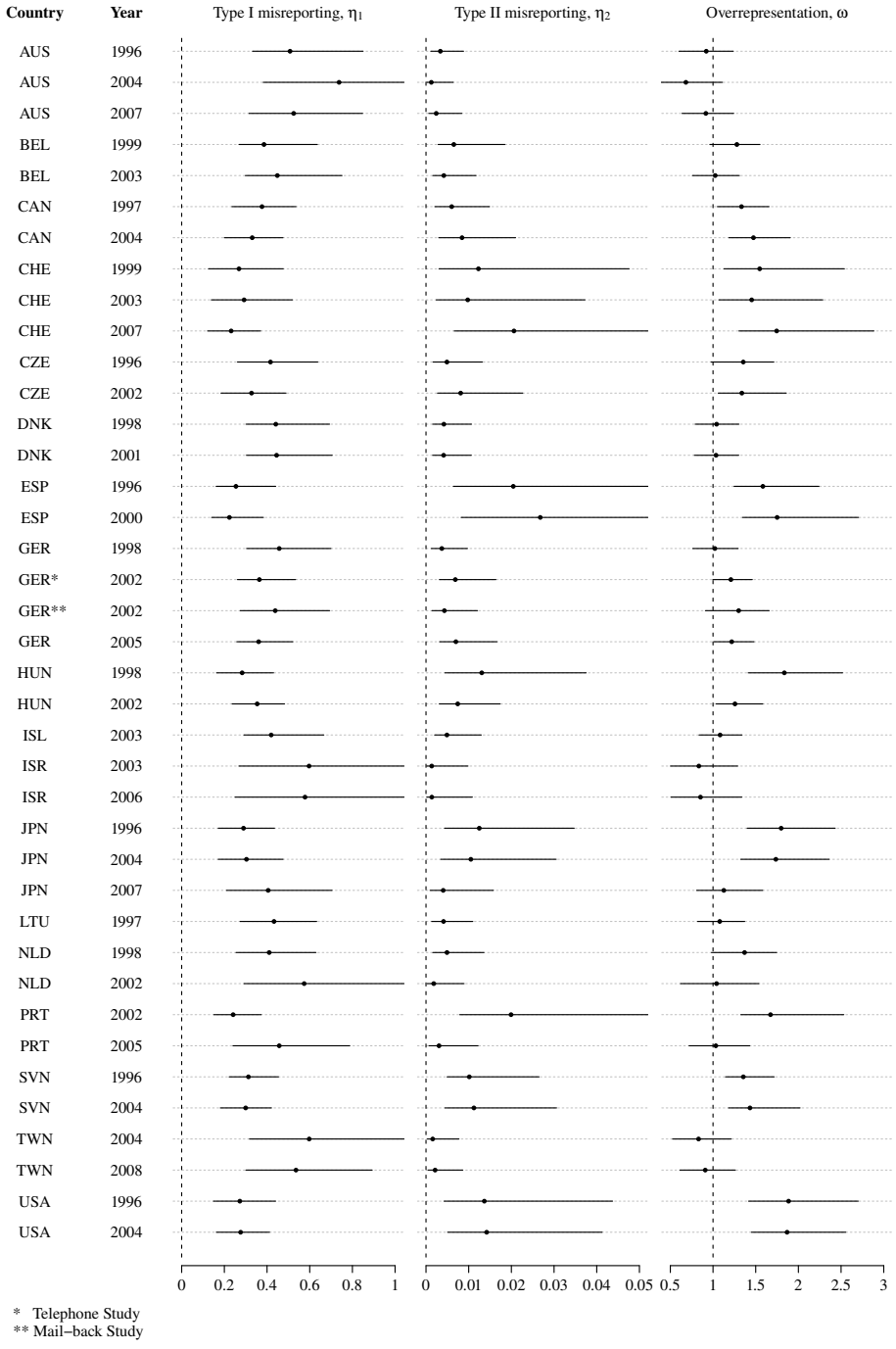
To date, CSES provides 93 studies from 1996 to 2008 in three modules. We predicted turnout bias for those studies for which election and study level covariates used to predict bias components are known (i.e., official turnout, reported turnout, response rate, and panel design) and for countries with

Polity IV values of 10 (see Footnote 8 in the paper). Data from the three CSES modules is available from `http://www.cses.org` after a registration process. The study archive also provides design reports which served as a basis for further information used in the analysis. Concerning the figures of official turnout we refer to Netscher (2010), who provides corrected values. Table 5 gives an overview over all variables used.

TABLE 5: CSES data: Descriptives

	Country	Label	Year	N	Turnout (off.)	Turnout (rep.)	RR	SOE	Panel
1	Australia	AUS	1996	1798	0.83	0.99	0.62	0	0
2	Australia	AUS	2004	1769	0.95	0.98	0.45	0	0
3	Australia	AUS	2007	1873	0.82	0.99	0.40	0	0
4	Belgium	BEL	1999	2179	0.91	0.97	0.74	0	0.5
5	Belgium	BEL	2003	2225	0.92	0.95	0.63	0	0
6	Canada	CAN	1997	1851	0.67	0.87	0.51	0	0.5
7	Canada	CAN	2004	1674	0.61	0.91	0.55	0	0.5
8	Switzerland	CHE	1999	2048	0.43	0.63	0.52	0	0
9	Switzerland	CHE	2003	1418	0.45	0.68	0.47	0	0
10	Switzerland	CHE	2007	3164	0.48	0.70	0.71	0	0
11	Czech Republic	CZE	1996	1229	0.76	0.90	0.52	0	1
12	Czech Republic	CZE	2002	948	0.58	0.74	0.53	0	0
13	Denmark	DNK	1998	2001	0.86	0.96	0.58	0	0
14	Denmark	DNK	2001	2026	0.87	0.96	0.58	0	0
15	Spain	ESP	1996	1212	0.78	0.90	0.95	0	0
16	Spain	ESP	2000	1208	0.69	0.81	0.95	0	0
17	Germany	GER	1998	2019	0.82	0.93	0.51	0	0
18	Germany	GER	2002	2000	0.79	0.95	0.66	0	0
19	Germany	GER	2002	1023	0.79	0.90	0.51	0	1
20	Germany	GER	2005	2018	0.78	0.94	0.66	0	0
21	Hungary	HUN	1998	1525	0.57	0.73	0.63	0	1
22	Hungary	HUN	2002	1200	0.71	0.83	0.61	0	0
23	Iceland	ISL	2003	1446	0.88	0.96	0.65	0	0
24	Israel	ISR	2003	1212	0.68	0.89	0.13	0	0
25	Israel	ISR	2006	1200	0.64	0.81	0.10	0	0
26	Japan	JPN	1996	1327	0.59	0.84	0.63	0	1
27	Japan	JPN	2004	1977	0.57	0.81	0.57	0	1
28	Japan	JPN	2007	1373	0.59	0.82	0.34	0	0
29	Lithuania	LTU	1997	1009	0.74	0.9	0.47	0	0
30	Netherlands	NLD	1998	2101	0.73	0.91	0.50	0	1
31	Netherlands	NLD	2002	1574	0.79	0.97	0.28	0	1
32	Portugal	PRT	2002	1303	0.61	0.76	0.81	0	0
33	Portugal	PRT	2005	2801	0.64	0.81	0.31	0	0
34	Slovenia	SVN	1996	2031	0.74	0.76	0.73	0	0
35	Slovenia	SVN	2004	1002	0.61	0.77	0.64	0	0
36	Taiwan	TWN	2004	1823	0.80	0.91	0.27	0	0
37	Taiwan	TWN	2008	1905	0.76	0.88	0.32	0	0
38	United States	USA	1996	1534	0.52	0.72	0.60	0	1
39	United States	USA	2004	1066	0.58	0.76	0.66	0	1

FIGURE 6: Predicted bias components (vote misreporting by actual nonvoters, η_1, and voters, η_2, and voter overrepresentation, ω) for 39 CSES studies: medians of the bias estimates' posterior densities (points) and their 90% credible intervals (lines).



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