Public Opinion Quarterly, Vol. 73, No. 1, Spring 2009, pp. 130–143

THE IMMENSELY INFLATED NEWS AUDIENCE: ASSESSING BIAS IN SELF-REPORTED NEWS EXPOSURE

MARKUS PRIOR

Abstract Many studies of media effects use self-reported news exposure as their key independent variable without establishing its validity. Motivated by anecdotal evidence that people's reports of their own media use can differ considerably from independent assessments, this study examines systematically the accuracy of survey-based self-reports of news exposure. I compare survey estimates to Nielsen estimates, which do not rely on self-reports. Results show severe overreporting of news exposure. Survey estimates of network news exposure follow trends in Nielsen ratings relatively well, but exaggerate exposure by a factor of 3 on average and as much as eightfold for some demographics. It follows that apparent media effects may arise not because of differences in exposure, but because of unknown differences in the accuracy of reporting exposure.

How much and what kind of news people watch matters, according to the very large literature on media effects. Exposure to political information is thought to influence how much people know about politics, how they feel and think about politics, and whether they participate in politics. Although other factors, such as attention during exposure (Chaffee and Schleuder 1986; Chang and Krosnick 2002), may condition the effect of political messages, the causal chain starts with exposure, and exposure appears to be consequential even when media users pay little attention (Krugman and Hartley 1970; Zukin and Snyder 1984).

The large majority of research into media effects relies on self-reported exposure. Yet, there is evidence to doubt the validity of these self-reports. Several studies show differences between frequency reports, time diary entries, and direct observation of media users (Bechtel, Achepohl, and Akers 1972; Robinson 1985; Papper, Holmes, and Popovich 2004). In one study, for example, 35 percent of the respondents reported listening to NPR, while Arbitron

MARKUS PRIOR is with the Woodrow Wilson School and the Department of Politics at Princeton University, Princeton, NJ 08544-1013, USA. Address correspondence to Markus Prior; e-mail: mprior@princeton.edu.

doi:10.1093/poq/nfp002 Advance Access publication March 18, 2009

ratings suggested that only 6 percent of the population did so (Price and Zaller 1993).

Self-reports of many nonpolitical behaviors are also known to be biased. Americans overreport ATM withdrawals (Burton and Blair 1991) and church attendance (Hadaway, Marler, and Chaves 1993, 1998; Presser and Stinson 1998). Soccer players overreport how often they head the ball (Rutherford and Fernie 2005). And either men overreport the number of their sex partners, or women underreport it (Brown and Sinclair 1999).

Considering the prominence of self-reported news exposure in public opinion research, the lack of validation despite these warning signals is troubling. This article reports a systematic validation of self-reported news exposure against an independent benchmark, audience ratings. I assess the extent of overreporting in self-reported exposure to evening network news by comparing survey estimates to Nielsen estimates. I then determine if we can treat reporting errors as random (and therefore relatively benign) by comparing overreporting in different demographic groups. Finally, I discuss the threat invalid self-reports pose for survey-based studies that use news exposure as independent and dependent variables.

Method and Data

Survey methodologists have developed a model that specifies what respondents do when they answer a survey question about the frequency of their past behavior (for different versions of the model, see Schwarz 1999; Tourangeau, Rips, and Rasinski 2000; Schwarz and Oyserman 2001). According to this model, respondents have to (1) understand the question, (2) recall the relevant behavior, (3) estimate the frequency of the relevant behavior, (4) map the frequency onto the response alternatives, and (5) report either their candid answer or a socially desirable answer. Self-reports of news exposure can go awry at any of the stages in the five-step model.

The purpose of this study is to assess the magnitude of the total response error by comparing survey estimates of news audiences to estimates collected by Nielsen Media Research. I use national Nielsen ratings measured by "people meters," the company's technology to monitor television viewing in a random sample of U.S. households. During the period examined in this study, its sample consisted of about 5,000 households. In those households, each television set is attached to a meter and household members indicate the beginning and end of their viewing by pushing a button.[1] The biggest advantage of Nielsen's people meter is that household-level data are not confounded by incomplete recall,

1. Nielsen estimates use poststratification weights to match the Current Population Survey on a number of demographic dimensions, including age, gender, race/ethnicity, and education.

flawed estimation, or social desirability biases because television viewing is recorded automatically.[2]

The main source of survey data for this study is the National Annenberg Election Survey (NAES) 2000 (Romer et al. 2004). The NAES was conducted as a rolling cross-sectional design (see Johnston and Brady 2002) and produced independent daily random samples of U.S. residents 18 and older for the entire year 2000.[3] Throughout the year, the 2000 NAES asked respondents:

> How many days in the past week did you watch the national network news on TV—by national news, I mean Peter Jennings on ABC, Dan Rather on CBS, Tom Brokaw on NBC, Fox News or UPN News?

This question is not as clear as it could be because it refers to "Fox News" and UPN News. Neither the Fox broadcast network nor UPN have national news programs. The mention of "Fox News" might invite respondents to report cable instead of network news exposure. The American National Election Study (ANES) used a more precise question. A comparison between NAES and ANES yields no statistical differences for the period in which both studies overlapped in 2000, suggesting that respondents understood the NAES question as referring to network news exposure.[4,5]

To compare survey self-reports and Nielsen estimates, I divide responses to the NAES question by 7 and use the average of respondents' scores to calculate the expected daily network news audience. For example, a respondent who reported watching network news on seven days in the "past week" had a 1.0 probability of watching each day of the week before the interview. A respondent who reported two days of exposure had a 2 in 7 ($\approx$.29) probability of watching each day. If the sample consisted of just these two respondents, the average respondent's probability of watching the news on a given day in the "past week"

2. Although I consider people meter data more accurate than self-reports, they, too, are estimates with both random and systematic errors (see, e.g., Napoli 2003, pp. 71–95). The accuracy of household estimates based on people meters still depends on the quality of the viewer sample. Although even person-level Nielsen data are unlikely to suffer from social desirability bias or systematic memory error, they are not without measurement error, and the quality of the sample is difficult to evaluate independently without more information about the company's data collection (see Milavsky 1992).

3. The AAPOR 4 response rate for the NAES was 31 percent (Romer et al. 2004, p. 15). The sample is weighted to adjust for the number of adults and the number of phone lines in the household and to match CPS distributions of race, hispanicity, age, sex, and education.

4. In its pre-election survey (conducted between September 5 and November 6), the ANES asked respondents, "How many days in the past week did you watch the national network news on TV?" The weighted mean response was 3.19 with a standard deviation of 2.80. For NAES interviews conducted in the same period, the weighted mean was 3.06 with a standard deviation of 2.66.

5. Chang and Krosnick (2002) have recently argued that asking about news exposure in a "typical week" is a more valid measure of exposure than the "past week" question when the goal is to measure habitual news exposure. The "typical week" question produces even higher estimates of national news exposure (Price 1993).

would be (1.0 + .29)/2 = .645. This implies an average daily news audience for that week of 64 percent of the population. For each daily sample, I average across all respondents and aggregate the daily estimates to generate weekly averages. Multiplying these percentage estimates by the voting-age population in 2000 produces survey-based estimates of the daily network news audience (averaged by week).

These survey estimates can be compared to Nielsen estimates of the network news audience. For each network news program, Nielsen measures the average and the total audience. The former refers to the average audience per minute, while the latter counts everyone who watched at least some portion of the program. If all news viewers watched the entire newscast, average and total audience would be the same. The precise equivalent to the survey estimate is the total audience for the three network newscasts. This is different from the sum of the total audiences for each of the three newscasts because a viewer may watch a portion of two or three nightly newscasts. While Nielsen could calculate the daily total network news audience, it does not report this quantity.

The following analysis uses weekly averages of the average weekday audience for the ABC, CBS, and NBC evening newscasts in 2000 (measured in number of viewers[6]). Because this measure counts viewers only in proportion to the portion of the newscast they watched, it underestimates the total weekday audience. But it also misses weekend viewing, which is lower than weekday viewing. These two biases thus work in opposite directions. In the appendix, I present analyses using alternative Nielsen measures to evaluate the magnitude of the distortion caused by these biases. Even analyses using alternative measures support the same substantive conclusions regarding overreporting of news exposure.

Results

The top graph in figure 1 compares survey estimates and Nielsen estimates of the nightly news audience in the year 2000. The survey estimates vastly overstate the size of the network news audience. According to Nielsen, between 30 and 35 million people watched the nightly news on an average weekday. Based on NAES self-reports, that number is between 85 and 110 million for most of the year.

6. Viewer statistics used here refer to persons 2 and older. The comparison between Nielsen and the NAES is therefore conservative because estimates of overreporting would be higher if viewers between 2 and 17 could be excluded from the Nielsen estimate. Approximately 5 percent of weekday network news viewers were between 2 and 17 in 2000 according to monthly data, which would generate an average overreport factor of 3.4 instead of the 3.2 based on the data in figure 1.

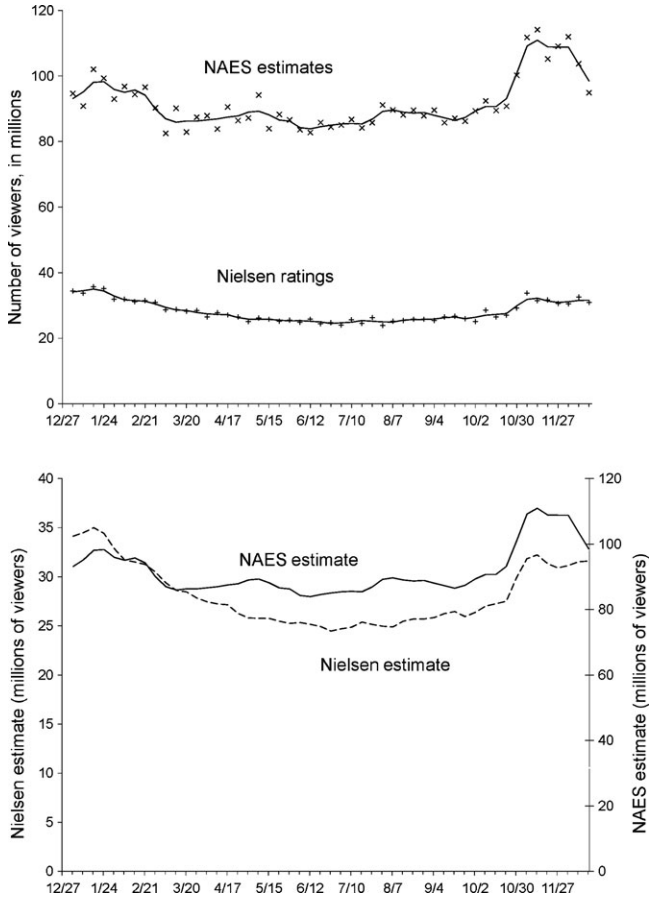

Figure 1. The Network News Audience in 2000. (Top) Average Daily Network News Audience (Same Scale). (Bottom) Average Daily Network News Audience (Different Scales). Nielsen data show the combined daily average audience for ABC, CBS, and NBC nightly news (weekly averages, Monday to Friday). Trend lines are generated using locally weighted regression on time with a bandwidth of .1.

The NAES estimates also vary more than Nielsen estimates. The OLS regression equation for their relationship (with standard errors in parentheses) is

$$\text{News Audience}_{\text{NAES}} = \underset{(7.2)}{43.7} + \underset{(.3)}{1.7}\ \text{News Audience}_{\text{Nielsen}}.$$

A one-percentage point increase in the Nielsen audience corresponds to a larger, 1.7-point increase in the self-reported audience. If the relationship

between the two aggregate measures were in fact linear, the intercept would imply that 43.7 percent of Americans report watching the news even when Nielsen estimates that nobody does.

In the bottom graph in figure 1, the two estimates are plotted on different scales so that the NAES scale is contracted three times more than the Nielsen scale. Deflated by a factor of 3, the survey-based estimates mirror the Nielsen trend quite closely. According to both series, news exposure increases by almost a quarter between the late summer and the month of the election. Yet, over the course of the entire year, Nielsen estimates drop considerably more steeply than the NAES estimates. News audiences are generally higher in the winter months. Survey-based estimates do not reflect the full extent of this regularity.

Failure by Nielsen panelists to sign in when they watch the news does not explain the mismatch between surveys and Nielsen estimates. Even in single-member households, where all viewing is recorded automatically without signing in, overreporting was considerable (average overreport factor of 2.6; see below for details).

The severe inflation of survey-based estimates of regular news exposure appears to have little to do with sampling problems or the design of the survey. Even though NAES and ANES used different sampling procedures and asked different questions about network news exposure in 2000, the difference between the survey marginals was not statistically significant, as noted in footnote 4.[7]

Individual Differences in Overreporting

It is critical to realize that the overreporting observed in figure 1 cannot be constant across respondents. By definition, those who report no news exposure at all do not overreport. Similarly, respondents who in fact watched the maximum number of newscasts cannot inflate their self-reports (Zaller 2002, pp. 311–13). With average overreporting by a factor of 3, even respondents who watch somewhat less than the maximum cannot overreport to the same extent as infrequent viewers. It follows that infrequent viewers overreport by a factor considerably greater than 3.[8]

To examine directly if some people overreport more than others, I use Nielsen's *National Audience Demographics Report*, which reports monthly ratings disaggregated by gender, age, region of the country, and several other

7. Respondents in the 2000 ANES were randomly assigned to be interviewed by phone or in person. Average news exposure was 3.13 days for in-person interviews and 3.26 days for phone interviews (t [1800] = .97, n.s.). Chi-square tests for the two distributions did not indicate statistically significant differences either.

8. One explanation might be that for many of those who watch the news a few days per week, network news exposure is an irregular behavior, which is overreported more often (Schwarz and Oyserman 2001).

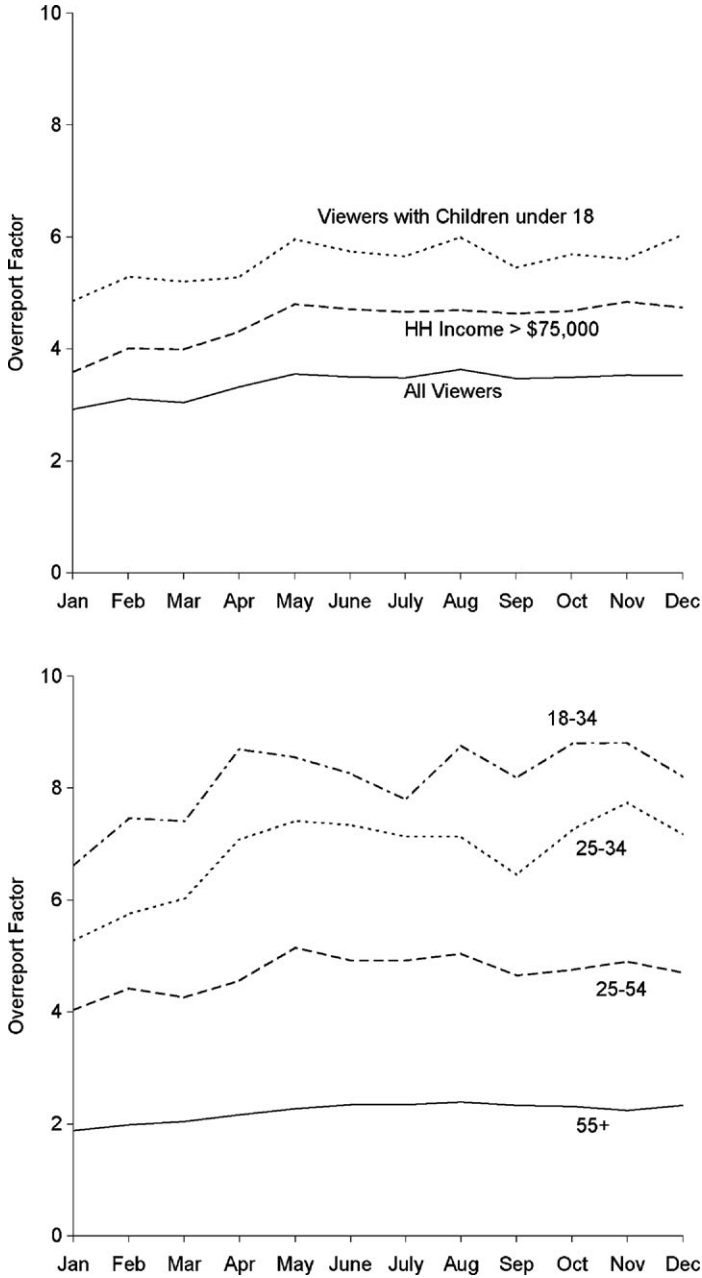

Figure 2. Self-Report Inflation, by Demographic Characteristics of the Respondent. (Top) By Income and Presence of Children in the Household. (Bottom) By Age.

attributes. Most of these attributes are assessed in the NAES, making it possible to compare Nielsen estimates and survey estimates for subpopulations. I calculate the extent of overreporting in each available subgroup by dividing the survey estimate by the Nielsen estimate.[9] The solid line in the top panel in figure 2 shows the average overreport factor for the voting-age population as a whole, already known to be about 3.

The other lines in the top panel indicate the monthly self-report inflation for two demographic groups which display higher levels of overreporting, viewers in households with yearly incomes over $75,000 and viewers in households with children. Average self-reports in these two groups are, respectively, 4.5 and 5.5 times higher than corresponding Nielsen estimates. The bottom panel in figure 2 shows self-report inflation in different age groups. Of all the categories assessed by Nielsen, age is the demographic attribute that reveals the greatest variation in the extent of overreporting. Among people between 18 and 34, the overreport factor exceeds 8. According to Nielsen estimates, less than 5 percent of them watch the network news on an average weekday. Yet, in the NAES, about 35 percent in this age group reported exposure. Older Americans overreport the least. But even the self-reports of people 55 and older are still twice as high as Nielsen estimates for this age group.

Some demographic attributes are not related to the extent of overreporting. Men are only slightly more likely to overreport than women. Overreporting is only marginally higher in cable households. It does not differ by region of the country.

Conclusion

Survey-based estimates of network news viewing were on average three times as high as Nielsen estimates in 2000 and up to eight times as high in some demographic subgroups. Over the course of the year, average overreporting was relatively stable. This is consistent with the observed stability in self-reported exposure to regularly available news (e.g., Price 1993). Yet this indication of measurement reliability must not be confused with validity. In fact, "[o]verreport bias can. . . hide the damage it does behind exaggerated reliability estimates" (Zaller 2002, p. 313). Self-reports of regular news exposure are reliable measures of how much news people think they watch. As measures of people's actual news exposure, they lack validity. This casts doubt on studies of media effects and audience behavior that treat self-reports as measures of actual exposure.

9. To keep this part of the analysis parallel to the data presented in figure 1, I use Monday to Friday ratings to calculate self-report inflation in different subpopulations. Using Monday to Sunday ratings instead would increase inflation estimates and subgroup differences in those estimates slightly.

If respondents made random mistakes in their reports of news exposure, analyses of media effects could statistically account for measurement error (Allen 1981; Bartels 1993). Even if all respondents overestimated their media use to the same extent, exposure measures would still distinguish heavily exposed from lightly exposed respondents. Yet, this analysis shows that both assumptions are false for network news exposure as measured in the NAES or the ANES. It would be prudent not to make them reflexively for other exposure measures either.

Individual differences in self-report inflation imply that the news exposure scale examined here is not comparable across respondents. Results indicate that the average respondent over 55 who reports "3 days" of news exposure has most likely watched more newscasts than the average respondent under 35 who reports "5 days" of exposure. When self-reported news exposure is the dependent variable, greater overreporting among younger respondents would lead to an underestimate of the relationship between age and network news exposure. Ostensible effects of news exposure as an independent variable could in fact be age effects. (For a formal demonstration of distortions that can result when survey respondents interpret the same scale differently, see Brady 1986.) More generally, apparent media effects may arise not because of differences in exposure, but because of differences in the accuracy of reporting exposure.

Although statistical analysis can correct for known individual differences in overreporting, it is not possible to check for many of the most plausible correlates of overreporting. Turnout validation studies show that more educated and more politically involved respondents are more likely to overreport turnout and other forms of political participation (Presser 1984; Volgy and Schwarz 1984; Silver, Anderson, and Abramson 1986; Belli, Traugott, and Beckmann 2001), but Nielsen does not publish audience ratings by political interest or education, so it remains unknown if these respondents also overstate news exposure more than others. In the absence of more information about respondents for whom we have behavioral data, effective corrections for overreporting are not feasible. This makes it even more important to understand why some people overstate their news exposure. The most prominent explanation for turnout overreporting is social desirability bias (Presser 1990; Belli et al. 1999; Bernstein, Chadha, and Montjoy 2001; Holbrook and Krosnick 2005). In a separate study (Prior, forthcoming), I found no evidence that social desirability also inflates self-reports of news exposure. Instead, errors appear to occur at the estimation stage as respondents cannot recall all instances of news exposure and use estimation rules with an upward bias (such as, "I find politics interesting, so I must watch news more often than I can remember right now.")

Scholars would do well to assess media effects with research designs that do not rely on self-reported exposure at all. Perhaps the most widely accepted alternative is to move from exposure to message reception (measured by general "political awareness") as the key independent variable (Zaller 1992; Price and

Zaller 1993). Although an important innovation in the study of media effects, this approach does not help us study the effects of exposure. Price and Zaller (1993, p. 135) themselves do not view political awareness as a measure of media exposure, but as a summary of "being exposed to a news story, attending to it, comprehending it, and remembering it." In fact, respondents could have been exposed to the story through interpersonal channels, not the mass media. Zaller (1996) acknowledges that political awareness "can be used to measure media exposure only to the extent that all media function as 'common carriers' of roughly the same information." This assumption is increasingly implausible (see, e.g., Prior 2007).

Other methods to study media effects do not use survey measures at all. One of the justifications for both laboratory and field experiments in political science is precisely a concern about the validity of self-reports. Cross-sectional or temporal variation in advertising volume and content can be exploited to study media effects without reliance on self-reports. Likewise, the effect of media coverage can be studied by drawing on variation in newspaper content. In other research areas, where alternative designs are not readily available, the inability to accurately measure news exposure in surveys is more damaging. Nothing, however, could be as damaging as a research approach that rests entirely on a variable that stubbornly defies validation.

Appendix: Nielsen's Measures of the Network News Audience

In this appendix, I calculate both an upper bound and a lower bound for the estimated size of the network news audience according to Nielsen data. The upper bound estimate (based on separate total weekday audience estimates for the three network newscasts) exceeds the average audience calculation presented here by less than 5 million viewers. The lower bound (based on average audiences for the seven-day week) is about 5 million viewers below the average weekday audience. The two biases thus roughly neutralize each other.

The average weekday audience for the ABC, CBS, and NBC evening newscasts, the measure used in figures 1 and 2, differs in two respects from the measure implied by the NAES survey questions. First, the measure represents the average per-minute audience for the three newscasts, thus counting viewers in proportion to the portion of the newscast they watched. The survey question asks how many days the respondents watched the newscast and thus includes even professed viewers who did not watch an entire newscast at full weight. The precise equivalent to the survey estimate is the total audience for the three network newscasts, which is not available to me. However, at the household level, I can compare the average audience to the sum of the total audiences for each of the three newscasts. This measure overstates the size of the news

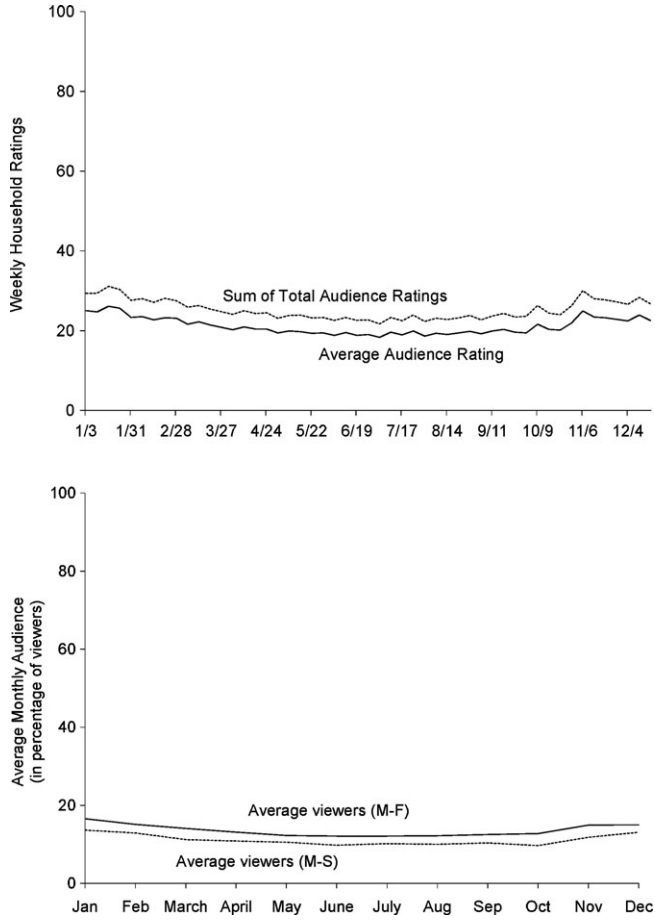

Figure A1. Alternative Nielsen Measures of the Network News Audience. (Top) Total versus Average Household Audience. (Bottom) Weekday versus Seven-Day Average Audience.

audience because it counts households more than once if they watch portions of more than one newscast on the same day. Because of this potential double and triple counting, the sum of total audiences provides an upper bound of the total audience. The top panel in figure A1 compares it to the average (household) audience. The graph shows that the sum of total audiences is consistently higher than the average audience, but that the difference is relatively small. On average, the upper bound exceeds the average audience by about 3.9 rating points (or just under 20 percent). The equivalent difference among viewers should be smaller because different household members may have watched

different newscasts (which would not affect the average household audience, but would be picked up by the average person audience). Even under the worst-case assumptions—using the sum of the three total audiences and making no adjustments for the difference between household- and viewer-level estimates—the upper bound implies only an additional 5 million viewers, clearly not enough to change the interpretation of figure 1. The overreport factor is still 2.7 for the upper bound estimate, compared to 3.2 for my best estimate.

Whereas the difference between total and average audience leads to a modest underestimate in the Nielsen estimates in figure 1, the exclusion of weekends actually inflates the estimates. As the bottom graph in figure A1 shows, the audience estimates for Monday through Sunday are about 18 percent lower than the weekday estimates, a difference of roughly 5 million viewers. (For this comparison, I did obtain person-level data, but only in monthly aggregation.) The overestimate due to using weekday estimates in figure 1 thus more or less cancels the underestimate due to using average rather than total audience numbers. If anything, the bias arising from excluding weekends is larger, thus making the estimated average overreporting factor of 3 a conservative estimate.

References

Allen, Richard L. 1981. "The Reliability and Stability of News Exposure." *Communication Research* 8:233–56.

Bartels, Larry M. 1993. "Messages Received: The Political Impact of Media Exposure." *American Political Science Review* 87:267–85.

Bechtel, Robert, Clark Achepohl, and Roger Akers. 1972. "Correlates between Observed Behavior and Questionnaire Responses in Television Viewing." In *Television and Social Behavior, Vol. 4: Television in Day-to-Day Life*, eds. Eli A. Rubinstein, George A. Comstock, and John P. Murray, pp. 274–344. Washington, D.C.: U.S. Government Printing Office.

Belli, Robert F., Michael W. Traugott, and Matthew N. Beckmann. 2001. "What Leads to Voting Overreports? Contrasts of Overreporters to Validated Voters and Admitted Nonvoters in the American National Election Studies." *Journal of Official Statistics* 17:479–98.

Belli, Robert F., Michael W. Traugott, Margaret Young, and Katherine A. McGonagle. 1999. "Reducing Vote Overreporting in Surveys: Social Desirability, Memory Failure, and Source Monitoring." *Public Opinion Quarterly* 63:90–108.

Bernstein, Robert, Anita Chadha, and Robert Montjoy. 2001. "Overreporting Voting: Why It Happens and Why It Matters." *Public Opinion Quarterly* 65:22–44.

Brady, Henry E. 1986. "The Perils of Survey Research: Inter-Personally Incomparable Responses." *Political Methodology* 11:269–91.

Brown, Norman R., and Robert C. Sinclair. 1999. "Estimating Number of Lifetime Sexual Partners: Men and Women Do It Differently." *Journal of Sex Research* 36:292–7.

Burton, Scot, and Edward Blair. 1991. "Task Conditions, Response Formulation Processes, and Response Accuracy for Behavioral Frequency Questions in Surveys." *Public Opinion Quarterly* 55:50–79.

Chaffee, Steven H., and Joan Schleuder. 1986. "Measurement and Effects of Attention to Media News." *Human Communication Research* 13:76–107.

Chang, LinChiat, and Jon A. Krosnick. 2002. "Measuring the Frequency of Regular Behaviors: Comparing the 'Typical Week' to the 'Past Week'." *Sociological Methodology* 33:55–80.

Hadaway, C. Kirk, Penny Long Marler, and Mark Chaves. 1993. "What the Polls Don't Show: A Closer Look at U.S. Church Attendance." *American Sociological Review* 58:741–52.

Hadaway, C. Kirk, Penny Long Marler, and Mark Chaves. 1998. "Overreporting Church Attendance in America: Evidence That Demands the Same Verdict." *American Sociological Review* 63:122–30.

Holbrook, Allyson L., and Jon A. Krosnick. 2005. Social Desirability Bias in Voter Turnout Reports: Tests Using the Item Count and Randomized Response Technique. Unpublished manuscript. University of Illinois at Chicago.

Johnston, Richard, and Henry E. Brady. 2002. "The Rolling Cross-Section Design." *Electoral Studies* 21:283–95.

Krugman, Herbert E., and Eugene L. Hartley. 1970. "Passive Learning from Television." *Public Opinion Quarterly* 34:184–90.

Milavsky, J. Ronald. 1992. "How Good Is the A.C. Nielsen People-Meter System? A Review of the Report by the Committee on Nationwide Television Audience Measurement." *Public Opinion Quarterly* 56:102–15.

Napoli, Philip M. 2003. *Audience Economics: Media Institutions and the Audience Marketplace*. New York: Columbia University Press.

Papper, Robert A., Michael E. Holmes, and Mark N. Popovich. 2004. "Middletown Media Studies: Media Multitasking and How Much People Really Use the Media." *International Digital Media and Arts Association Journal* 1:9–50.

Presser, Stanley. 1984. "Is Inaccuracy on Factual Survey Items Item-Specific or Respondent-Specific?" *Public Opinion Quarterly* 48:344–55.

Presser, Stanley. 1990. "Can Changes in Context Reduce Vote Overreporting in Surveys?" *Public Opinion Quarterly* 54:586–93.

Presser, Stanley, and Linda Stinson. 1998. "Data Collection Mode and Social Desirability Bias in Self-Reported Religious Attendance." *American Sociological Review* 63:137–45.

Price, Vincent. 1993. "The Impact of Varying Reference Periods in Survey Questions about Media Use." *Journalism Quarterly* 70:615–27.

Price, Vincent, and John Zaller. 1993. "Who Gets the News? Alternative Measures of News Reception and Their Implications for Research." *Public Opinion Quarterly* 57:133–64.

Prior, Markus. 2007. *Post-Broadcast Democracy: How Media Choice Increases Inequality in Political Involvement and Polarizes Elections*. New York: Cambridge University Press.

Prior, Markus. Forthcoming. "Improving Media Effects Research through Better Measurement of News Exposure." *Journal of Politics*.

Robinson, John P. 1985. "The Validity and Reliability of Diaries versus Alternative Time Use Measures." In *Time, Goods, and Well-Being*, eds. F. Thomas Juster and Frank P. Stafford, pp. 33–62. Ann Arbor: University of Michigan Survey Research Center, Institute for Social Research.

Romer, Daniel, Kate Kenski, Paul Waldman, Christopher Adasiewicz, and Kathleen Hall Jamieson. 2004. *Capturing Campaign Dynamics*. New York: Oxford University Press.

Rutherford, Andrew, and Gordon Fernie. 2005. "The Accuracy of Footballers' Frequency Estimates of Their Own Football Heading." *Applied Cognitive Psychology* 19(4):477–87.

Schwarz, Norbert. 1999. "Self-Reports: How the Questions Shape the Answers." *American Psychologist* 54:93–105.

Schwarz, Norbert, and Daphna Oyserman. 2001. "Asking Questions about Behavior: Cognition, Communication and Questionnaire Construction." *American Journal of Evaluation* 22:127–60.

Silver, Brian D., Barbara A. Anderson, and Paul R. Abramson. 1986. "Who Overreports Voting?" *American Political Science Review* 80:613–24.

Tourangeau, Roger, Lance J. Rips, and Kenneth Rasinski. 2000. *The Psychology of Survey Response*. New York: Cambridge University Press.

Volgy, Thomas J., and John E. Schwarz. 1984. "Misreporting and Vicarious Political Participation at the Local Level." *Public Opinion Quarterly* 48:757–65.

Zaller, John. 1992. *The Nature and Origins of Mass Opinion*. New York: Cambridge University Press.

Zaller, John. 1996. *Analysis of News Exposure Items from the 1995 Pilot*. Ann Arbor, MI: Institute for Social Research.

Zaller, John. 2002. "The Statistical Power of Election Studies to Detect Media Exposure Effects in Political Campaigns." *Electoral Studies* 21:297–329.

Zukin, Cliff, and Robin Snyder. 1984. "Passive Learning: When the Media Environment Is the Message." *Public Opinion Quarterly* 48:629–38.