

[A]ttendance figures from Episcopal parishes are far below what would be expected if self-defined Episcopalians attended church in the numbers they claim. Based on Gallup surveys and other poll data, about 35 percent of Episcopalians say they attended church during the last seven days. If 2.5 percent of Americans claim to be Episcopalian and 35 percent of Episcopalians attend worship, total attendance during an average week should exceed 2 million. . . . Instead, average weekly attendance was less than 900,000 in 1991. (Pp. 742-43)

But in this instance, it is plain beyond argument that the surveys overreported the population of Episcopalians more than their church attendance rate. In 1991, 2.11 percent of GSS respondents reported themselves to be Episcopalian (Davis and Smith 1991). Applying this percentage to the 1991 national population gives us 5.3 million Episcopalians.¹ Using HMC's 2.5 percent rate gives us 6.1 million Episcopalians. Both of these wildly inflated estimates contain more phantoms than people. The Episcopal church counts its members with reasonable care, and the official count for 1991 was 2,474,625 (Protestant Episcopal Church 1992).² Thus, the average weekly attendance of "less than 900,000" cited by HMC closely matches the 35-percent attendance rate reported by Gallup surveys and other poll data.

This does not explain why the proportion of GSS respondents identifying themselves as Episcopalians so much exceeds the actual proportion of Episcopalians in the population, but that interesting question can be left for another day.

Meanwhile, until better evidence is presented, the verdict on any statement that "church attendance in the United States is overreported" must be "Not Proven."

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¹ Because the Episcopal Church baptizes infants and counts all baptized persons as members, HMC decided that "our estimates of self-identified Episcopalians pertain to the entire population rather than adults" (p. 742, note 3).

² Confirmed by Bruce Woodcock, General Convention Office, October 1996.

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Comment on Hadaway, Marler, and Chaves, ASR, December 1993

WHAT CHURCH OFFICIALS' REPORTS DON'T SHOW: ANOTHER LOOK AT CHURCH ATTENDANCE DATA*

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Hadaway, Marler, and Chaves (1993, henceforward HMC) conclude that, when asked in surveys about attending religious services, Americans overstate their actual weekly church attendance rates by a factor of two. They base their conclusion on administrative estimates they received from churches in one county in Ohio and on published data for 18 Roman Catholic dioceses.¹ From these data sources they speculate that church attendance may be falling, despite no evidence of change among Protestants in any of the survey records and evidence that the

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¹ Chaves and Cavendish (1994) include more dioceses in their subsequent analysis on Catholics' church attendance rates.

decrease in attendance among Catholics was confined to the years immediately following *Humae Vitae*, the birth control encyclical in 1968 (Hout and Greeley 1987).

We question the reliability of HMC's "actual counts." We present survey-based evidence on the validity and reliability of self-reported church attendance, reject the conjecture that bias in self-reports has increased in recent years, and conclude that survey reports exaggerate church attendance rates by a factor closer to 1.1 than 2.0.

OBTAINING "ACTUAL COUNTS" FOR CHURCH ATTENDANCE

There is ample reason to believe that Americans may overstate or understate the frequency of certain behaviors to suit their values or the perceived values of others. Surely they overstate their election participation (Leighley and Negler 1992), and they probably understate their income and criminal activity. Differences in misreporting might hint at important clues about behavior, as when men overstate their sexual adventures or women understate them (Smith 1992).² Americans probably also exaggerate their religious observance in surveys about attending church services, although, as HMC concede, such exaggeration at a minimum indicates that Americans value church attendance. The question is, how much does exaggeration inflate survey-based estimates of church attendance?

HMC use what they call "actual counts" of Protestant church attendance in Ashtabula County, Ohio to estimate how much surveys exaggerate church attendance. They draw together an amalgam of data from a variety of sources:

Average attendance figures were obtained through denominational yearbooks, telephone interviews, letters, and church visits. We requested membership totals, definitions of membership, and average attendance estimates (including young children who were not in the worship service) from each church in the county. Average attendance counts were received from 137 Protestant churches. For the remaining 22 (mostly small) Protestant

churches, attendance was estimated using the number of cars in the parking lots or actually counting persons attending Sunday services in February and March 1992. (P. 744)

These "actual counts" for Protestant churches are not direct observations by social scientists but quasi-official estimates provided by pastors and secretaries. HMC do not evaluate the quality of these estimates, and although they regard them as more reliable than the reports of adult survey respondents, they do not give evidence to support that assessment. Their own direct head counts may be superior to the survey data, yet they do not tell us what fraction of the total data set these direct observations amount to. Nor do they explain how they calculated their "actual count" of 13,080 people in attendance at services from these various bits of data.³

For Catholic Church attendance, HMC use official Catholic head counts from 18 dioceses. These data vary greatly from diocese to diocese. The attendance rate for the diocese with the highest reported attendance was twice that reported by the lowest (see fig. 1, p. 747),⁴ exhibiting substantially more place-to-place variation than surveys obtain. In the General Social Survey (GSS) (Davis and Smith 1994) variation in attendance among census regions spans a range of 1.6 to 1; variation among cities of different sizes ranges from 1.3 to 1.⁵ Generally, error-rid-

³At one point HMC speculate that their estimate may be too high. Without amplification or rationale, they simply state, "This total probably overestimates [church] attendance in Ashtabula County" (p. 744).

⁴This included Omaha, which HMC dismiss as "too high." Surprisingly, in their conclusions HMC refer to this wide variation in the Catholic church attendance data as "uniform."

⁵The GSS codes regions by the nine census regions. Our calculations show that the East North Central, West North Central, and West South Central regions have the highest attendance rates among Catholics: Distributions imply a probability of Catholics attending church on any given Sunday of 62 percent. The Pacific region has the lowest attendance among Catholics: A distribution implies a probability of attending on any given Sunday of 38 percent. This range represents a ratio of 1.6 to 1. Similar calculations by size of place (using the GSS size variable labeled

² However, when "opportunity structure" is taken into account, men and women agree on the proportions of marital infidelities (Greeley 1994).

den data vary more widely than clean data; thus without corroborating evidence that church attendance varies across dioceses as widely as 2 to 1, this place-to-place variation in the official Catholic data implies that there is more error in the official data than in the survey data.⁶

Therefore, a closer look at the data HMC use shows the data to be neither as direct nor as reliable as the term "actual count" implies. We can see how a study linking head counts with surveys could yield reasonable estimates of "actual" attendance, but neither the Ashtabula County data nor the data from the 18 Catholic dioceses make that link.

HOW IMPORTANT IS SOCIAL DESIRABILITY?

We acknowledge that errors probably occur in self-reports of church attendance, and that these errors probably stem from the respondents' perceptions that interviewers expect them to say that they attend church (a so-called "socially desirable" response). However, we doubt that people exaggerate so much that reported attendance is actually twice as high as their actual attendance. Lacking a study that links head counts with survey responses, can we say anything about how much socially desirable misreporting could bias survey estimates of church attendance?

The survey record gives us two internal checks that might help: (1) We can compare self-reports with reports others make on the same population (under the assumption that the others feel less social pressure), and (2) we can try to isolate those respondents who are unlikely to exaggerate their attendance because religious practice is not socially desirable in their reference group.

Treated carefully, married peoples' responses about their spouses' church attendance can give us estimates of married

peoples' behavior that are independent of what married people say about themselves. In the GSS, respondents are asked how often they attend religious services. In the 1986–1989 GSS, married people also were asked how often their spouses attend: Data on what married women say about their husbands are independent from data on what married men report about themselves, because the "husbands" that the married women report on are not the married men in the sample. Similarly, data on what married men say about their wives are independent from data on what married women report about themselves. As Smith (1985) spells out: "For married couples, the self-reports of the randomly selected respondents and of respondents' spouses represent the same universe (married people) and therefore the distributions should be the same" (p. 2). This provides an excellent check on the accuracy of self-reports, because while one person's report might be contaminated by that of his or her own spouse, it cannot be contaminated by a report made by somebody else's spouse.

In the 1986–1989 GSS, married people report virtually the same attendance for their spouses as married persons of the opposite sex report for themselves. Thus, 37 percent of married women reported attending church weekly themselves, and 35 percent of the men reported weekly church attendance for their wives.⁷ Similarly, 28 percent of the married men reported that they attended church weekly, and 30 percent of the married women reported weekly attendance for their husbands.⁸

If social desirability is such a potent force, why did it not lead the men to exaggerate their own attendance enough to close the gap with their wives' reports? Perhaps because the self-report question came first and a man had no way of knowing that the question about his wife was coming up. But why then

"XNORCSIZ") show that towns with less than 2,500 population have the highest church attendance rate (61 percent). Cities with over 250,000 population have the lowest attendance rate (46 percent). The city-size ratio is 1.3 to 1.

⁶ The observed variance is the sum of true variance plus error variance (assuming that errors are independent of true values). Thus error-laden data typically has more variance than cleaner data.

⁷ The 1986–1989 GSS has data on respondents' and spouses' attendance at religious services for 1,659 married women and 1,489 married men. Therefore, the 95-percent confidence interval for the difference between the wives' reports of their husbands' attendance and the married men's reports of their own attendance is ± 3.4 percentage points.

⁸ This difference also has a 95-percent confidence interval of ± 3.4 percentage points.

did social desirability not lead the women to exaggerate their husbands' attendance enough to close the gender gap? The sequence of questions made it possible for a woman to avoid giving the interviewer an opportunity to think less of her husband, nevertheless, 31 percent of wives said that their husbands go to church or synagogue less than they do.

HMC themselves provide another suggestion about where to look for evidence of the influence of social desirability on self-reports:

If survey respondents view regular church attendance as normative or view infrequent church attendance as deviant, they may be inclined to overreport their attendance. This tendency may be greater for persons who think they "ought" to attend church. (P. 749)

In other words, the degree of exaggeration should vary by some sort of normative "climate." Who would be *least* likely to view regular church attendance as normative or infrequent church attendance as deviant? We suggest "intellectuals." At least since the time of Voltaire, religious practice has been suspect in intellectual circles. Indeed in the environments in which intellectuals live and work, regular church-going might be viewed as deviant and nonattendance as normative. Furthermore, intellectuals are skeptical—by both disposition and training—and therefore are less susceptible (if not immune) to distortions like "social desirability."⁹

As our second internal check, then, we examine men and women who have completed at least 20 years of education or those who can legitimately be classified as "intellectuals" on the basis of their occupation. We use the pooled 1972–1993 GSS (N = 29,171) for this analysis because the GSS question on church attendance records more detail than does the Gallup question. To make our results comparable to HMC's, we recalibrated the GSS response categories to reflect the implied probability of attending church in any given week.¹⁰

⁹ At least we expect this to be true with respect to some behaviors like attending Jewish or Christian religious services. On the other hand, intellectuals' normative climate probably encourages exaggeration when it comes to voting (Leighley and Nagler 1992).

¹⁰ The GSS categories (with implicit probabili-

Starting with education, then, we find that 403 respondents have 20 or more years of education (1.3 percent of the sample). These "intellectuals" report church attendance that corresponds to 40.5 percent attending in any given week; their rate is not significantly different from the 43.0 percent rate in the rest of the population. If the true rates are the same and the observed rates differ only because those with less than 20 years of schooling hide their nonattendance by exaggerating, then reported rates are only about 2.5 percent points too high. This suggests a minimal desirability bias in the church attendance question.¹¹

Turning to occupation, we divided the detailed occupational titles in the 1970 census scheme into "skeptical" professionals (such as "life and physical scientists," "social scientists," "university teachers," and "writers, artists, and entertainers"); "nonskeptical" professionals (all other professionals); and all nonprofessionals.¹² Our skeptical professionals comprise a pretty exclusive group: 669 cases (2.6 percent of the GSS sample) fell into the skeptical category; 3,296 respondents fell into the nonskeptical professional

ties in parentheses) are: "never" (0), "less than once a year" (.01), "once or twice a year" (.02), "several times a year" (.05), "about once a month" (.23), "two or three times a month" (.58), "nearly every week" (.85), "every week" (.99), and "several times a week" (.99). In this way we generate for each respondent an implicit estimate of the probability of her or his having attended church "in the last seven days," which is the object of the Gallup question.

¹¹ The 95-percent confidence interval around this point estimate of 2.5 percent overlaps 0 (i.e., this estimate is not statistically different from 0 at the $p < .05$ level). A sample of more than 29,000 is usually said to have a margin of error much smaller than ± 2.5 percentage points. However, here we are taking the difference between two percentages, and one percentage is based on only 403 cases.

¹² The 1970 occupational codes for "skeptical" professionals are 42–54, 91–96, 102–140, 175–194. The codes for "nonskeptical" professionals are 1–40, 55–90, 100, 101, 141–174, and 195. Analyses with more detailed occupational categories offered more occupational contrasts, but they did not differ in the two crucial contrasts—skeptical professions versus other professions and skeptical professions versus all other occupations. So we report the simpler results here.

category. Skeptical professionals also tend to be areligious: 18 percent have no religious preference (compared with 9 percent of non-skeptical professionals and 7 percent of non-professionals).

Skeptical professionals do in fact report lower church attendance rates than the other groups: Their attendance responses imply that 38.0 percent will attend services in any given week, compared with 42.2 percent of nonprofessionals (difference not significant) and 49.1 percent of nonskeptical professionals (significant difference, $p < .05$). The quantitative implication of these differences for estimating the influence of social desirability depends on which baseline we choose. If we believe that all differences among occupations are due to different rates of misreporting (i.e., that occupation has no "real" effect on attendance), then the difference between attendance reported by skeptical professionals and that reported by everyone else reflects a combination of sampling error and the consequences of social desirability. That difference is 5 percent ($43\% - 38\% = 5\%$). By this reasoning, surveys overstate church attendance by 5 percentage points, or a ratio of 1.1 to 1. Moreover, in such an analysis the group most likely to be susceptible to the influence of social desirability would be the nonskeptical professionals—the upper-middle class—who think people ought to go to church even if they themselves do not attend every week. If all of this difference between the skeptical professionals and the nonskeptical professionals is due to the influence of social desirability rather than at least partly to real behavioral differences—an extreme premise—then the appropriate difference is between the reported attendance of skeptical and non-skeptical professionals: $49\% - 38\%$, or an 11-percentage-point error. Even this significant difference is less than one-third as large as the reporting error estimated by HMC.

Persons in the skeptical professions are older, less fundamentalist, and less frequently from the South than are other adults. Any of these variables might contribute to the gross differences we report. Controlling for the effects of age, denomination, and region by ordinary least squares regression, we find no net difference between church attendance reported by skeptical pro-

fessionals and nonprofessionals, and a significant net difference between skeptical professionals and nonskeptical professionals of 6 percentage points (details available from the authors).

We do not strongly insist that our multivariate results be accepted as the best estimate of bias due to social desirability. We are content, rather, to concede to HMC the 5-percentage-point gross difference between skeptics and everyone else—a 1.1 to 1.0 exaggeration, not 2 to 1. Even the somewhat dubious attribution of all of the difference in church attendance between skeptical professionals and nonskeptical professionals to exaggeration results in an estimate of 1.3 to 1.0. From our estimates, survey data on attendance at religious services is much more useful than HMC's analysis makes it appear.

IS SOCIALLY DESIRABLE EXAGGERATION INCREASING?

HMC speculate that the influence of social desirability might have increased enough to sustain the levels of *reported* church attendance when *actual* church attendance has declined. Two points foster doubts about their argument.

(1) It is odd that at some point in the supposed decline of actual church attendance the desirability of misreporting attendance does not begin to decline as well. One would think that as new generations come into the populations and consider the actual church attendance they observe around them, reporting the same level of church attendance as their predecessors did would seem slightly foolish. Furthermore, it might be altogether impossible for them to know what level of attendance they are "expected" to report, because without consulting the Gallup or GSS reports they can only observe actual rates of attendance.

(2) What if people simply remember their own past behavior and misreport their current behavior in proportion to what they used to do, hiding their declining participation by reporting their former, higher rate? This conjecture is inconsistent with the tendency of reported church attendance to increase, not decrease, with age (Hout and Greeley 1987; Stolzenberg, Blair-Loy, and Waite 1994). The overall population stays in balance be-

cause later life-cycle increases in attendance are offset by the influx of young people who attend church with the (in)frequency characteristic of youth (Firebaugh and Harley 1991). This pattern is not inconsistent with constant social desirability effects, but to factor in *increasing* social desirability we must believe that each successive cohort manages to emulate precisely the behavior claimed, but not practiced, by the cohort before it.

After 20 years of denying the steady survey record, the scholarly community finally recognized that surveys showed no evidence of the long-anticipated decline in church attendance: Secularization was about to be swept into the dustbin of failed social science theories. Yet HMC say they expect their results to "stimulate the ongoing debate over secularization" (p. 751). Without evidence, they speculate that a "true" decline in attendance at religious services is masked in survey self-reports because "the gap between what people do and what they say they do increased" (p. 750). Supposedly secularized Americans have increased their rate of lying to pollsters, not enough to make attendance seem universal, but enough to keep the reported rate of attendance constant at around 40 percent.

Yet HMC present no evidence that exaggeration is increasing. Nor can we find any. Our 1987 conclusions about trends in religious practice still hold as our most accurate and defensible statement on the subject (Hout and Greeley 1987):

We could find no evidence for religious secularization as measured by attendance at religious services in the United States over the past half century. The downward trend in church attendance in the United States during the late 1960s and early 1970s was strictly a Catholic phenomenon. American Catholics reduced their participation in religious services by one third between 1968 and 1975, while Protestants and Jewish participation did not change [theirs]. (P. 341)

CONCLUSIONS

Our results call into question HMC's conclusions about exaggerated church attendance rates. Our data on spousal reports of attendance at religious services suggest no bias in survey data. Our data on church attendance

rates for "intellectuals" (persons with 20 years or more of education for those who are in "skeptical" professional occupations) suggest that the ratio of actual attendance to that reported in surveys is closer to 1.1 to 1 than 2 to 1. We suspect that part, if not all, of this discrepancy stems from how HMC relied on pastors, secretaries, and other church officials for their "actual counts." Neither their data nor the survey record support the conjecture that church attendance rates in the United States has fallen in recent years.

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 Comment on Hadaway, Marler, and Chaves,
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WHEN SURVEYS LIE AND PEOPLE TELL THE TRUTH: HOW SURVEYS OVERSAMPLE CHURCH ATTENDERS*

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Hadaway, Marler, and Chaves (1993, henceforward HMC) argue that actual church attendance in the United States is only half the level reported by surveys. If this is true, surveys may misreport other behaviors and attitudes by similar margins. Unfortunately, such errors are difficult to detect because we have few reliable behavioral counts to compare with survey estimates (and we have no direct access to people's attitudes). Moreover, if social desirability causes gaps between what happens and what

surveys report, this would presumably bias correlations between socially desirable behaviors, and researchers would have difficulty distinguishing real correlations from spurious ones.

Thus, the church attendance gap observed by HMC has implications far beyond the sociology of religion. Previously I provided a comprehensive examination of this gap, and concluded that little of it appears to be caused by social desirability bias (Woodberry 1997a). In this study, I suggest that about 29 percent of Americans attend church or synagogue on an average week (i.e., adjusted head counts and reduced survey estimates meet at slightly under 29-percent attendance). However, in this comment I restrict my attention to how surveys oversample church attenders.

OVERSAMPLING CHURCH ATTENDERS

Most surveys oversample church-goers because they are easier to contact and more cooperative than non-church-goers. Regular attenders are easier to contact because people with nine-to-five jobs, married couples, families with children, and families in which the wife is a homemaker or works part time all tend to be more religiously active (Woodberry 1997b). Referrals probably accentuate this bias because easy-to-contact family members (e.g., homemakers) often tell researchers when difficult-to-contact members (e.g., husbands with busy schedules) are likely to be home. Thus, in the 1988–1992 National Election Study (NES) (Miller and NES 1995), when we regress the probability of attending church on the number of calls needed to contact respondents (variable "V9123"), the coefficient is negative and highly significant ($b = -.01$, S.E. = .002, $p = .000$, with a range of 1 to 33).¹ For each additional call needed to contact a respondent, respondents are typically one percentage-point less likely to have attended

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¹ The NES asks respondents how often they attend religious services. I recoded their responses into the literal probability of attending church during an average week: "every week" = 1, "almost every week" = $36/52 = .69$, "once or twice a month" = $18/52 = .39$, "A few times a year" = $4/52 = .08$.