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*Reply to Caplow, Hout and Greeley, and Woodberry*

**OVERREPORTING CHURCH  
ATTENDANCE IN AMERICA:  
EVIDENCE THAT DEMANDS THE  
SAME VERDICT\***

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In "What the Polls Don't Show: A Closer Look at U.S. Church Attendance" (Hadaway, Marler, and Chaves 1993), we presented evidence that weekly church attendance in the United States is substantially below the 40-percent level reported by most social surveys and public opinion polls. We also concluded that the overreporting of church attendance by survey respondents explains a major portion of the "gap" between attendance counts and poll-based estimates. Our findings are questioned by three critical comments. Woodberry (1998) agrees with us that survey-based attendance rates are inflated, but claims that response bias accounts for most of the inflation. Caplow (1998) and Hout and Greeley (1998) argue that survey-based rates are not substantially inflated, so there is no inflation to be explained. These

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three papers criticize our research methods, offer “internal checks” allegedly pointing to different conclusions, and suggest that we greatly exaggerate the degree of overreporting on social surveys. Bolstered by new evidence from continuing research on church attendance, we show here that their criticisms are groundless, their survey-based “internal checks” are not persuasive, and that church attendance is indeed overreported by many Americans. The essence of our response is simple: *Every* relevant piece of available evidence points to a substantial gap between self-reported and actual weekly church attendance in the United States. We also address the issue of change over time in church attendance rates and church attendance overreporting.

### RESPONDING TO CRITICISMS

The three comments direct four criticisms at our research. According to the authors: (1) Our estimate of the relevant Protestant and Catholic populations may be too large, resulting in a deflated church attendance rate (Caplow 1998); (2) our counts and our surveys may not represent the same time periods, and so should not be trusted (Caplow 1998); (3) our count data rely on local church leaders, and thus may underestimate actual attendance (Caplow 1998; Hout and Greeley 1998); and (4) the surveys we used as reference points were methodologically flawed, resulting in survey-based attendance rates that are too high (Woodberry 1998). We respond to each of these points in turn.

#### *Population Estimates*

Caplow suggests that we should not use “wildly inflated estimates [that] contain more phantoms than people” (p. 113) to estimate the Protestant and Catholic populations when we calculate the count-based attendance rate. He would prefer that we use Protestant and Catholic church membership as the denominator for the count-based atten-

dance rate and the larger population of self-identified Protestants and Catholics as the denominator for the survey-based rate. We find this suggestion odd. Because our count-based attendance measure was intended to be an external validation of the poll-based rates, it makes sense to use the *same* denominator in both cases.<sup>1</sup> It would be possible, although methodologically difficult, to obtain survey-based and count-based church attendance rates for registered church members only. The gap would remain, however, because using church members as the denominator would increase *both* the poll-based and count-based attendance rates. Thus, Caplow’s point that there are more self-identified Catholics than appear on official lists of registered Catholics (and his truism that using registered Catholics in the denominator would generate a higher attendance rate than if we used self-identified Catholics) is irrelevant.

#### *Time Periods*

Caplow also asserts that the lack of a direct connection in time between our count data and survey comparisons is problematic. It is true that the survey data and count data were not gathered during the same week; limitations of personnel and resources prevented that kind of data-gathering. Nevertheless, we maintain that the research strategy employed, and the resulting findings, fairly represent attendance claims versus attendance counts. As described in our original article, both the Protestant data and the bulk of the Catholic data were collected during the fall months, widely known to be “middling” in the seasonal variation of church attendance. We did not poll or count during Advent or Lent, but we also did not poll or count during August. The attendance rate surely would be higher during the holiday seasons and lower in the summer, but so what? Our concern is with the *gap* between self-reported attendance and actual attendance counts, and we see no reason—and Caplow offers no reason—to worry that such a gap is generated by the particular season during which the data were collected. Perhaps more convincing, Marler and Hadaway (1997) conducted a test (described in greater detail below) in a single church where the survey

<sup>1</sup> The Protestant and Catholic church attendance rates yielded by surveys always are based on the respondents who self-identify as “Protestant” or “Catholic”—whether or not they are formal members of a church.

and attendance counts were conducted during the same week: Overreporting of church and Sunday school attendance were found in this church as well.

### *Count Data*

Both Caplow and Hout and Greeley suggest that our count data are unreliable and must somehow understate actual attendance. Caplow states that our judgment that the count-based rates, if anything, still overestimate weekly attendance is “merely bias” (p. 112). Hout and Greeley assert that we “do not evaluate the quality of [our] estimates” (p. 114). Both of these statements are false. We, in fact, paid close attention to assessing the quality both of our Protestant and our Catholic head-count data, and as reported in our original article, when it was necessary to estimate counts we went to great lengths to ensure that the counts *overestimated* attendance. Furthermore, we evaluated the counting procedures used in each Catholic diocese and declined to report count data gathered from more than a dozen dioceses because the data were of questionable quality.

Hout and Greeley do not trust the ability of church officials to maintain records on average weekly attendance or to estimate attendance on a typical day of worship; they accept only direct counts by social scientists. Anticipating this reaction, Hadaway and Marler (1997b) conducted two tests of the attendance gap among Roman Catholics by directly counting attendance at all weekend masses in all Roman Catholic parishes in Ashtabula County, Ohio and Oxford County, Ontario (Canada). In both counties poll-based estimates of Roman Catholic attendance were more than twice as high as the aggregate number of persons we counted attending mass. So whether we rely on church officials or our own direct counting, the gap between attendance counts and poll-based measures of attendance remains.

Readers should note that neither Caplow nor Hout and Greeley provide *any evidence* in support of their suspicions of our count data.<sup>2</sup> They merely impugn the quality of

these data without engaging in the hard work of evaluating that quality. Furthermore, they assume that most church leaders seriously *undercount* attendance in their own churches. If surveys overstate church attendance by a factor of only 1.1, as Hout and Greeley suggest, this implies that church officials consistently undercount attendance by one third or more—so that, for example, a minister with a Sunday morning congregation of 300 systematically counts only 200 (or fewer) persons attending. We believe this is extremely unlikely, and Hout and Greeley provide neither data to support their belief nor a plausible explanation for why church officials might undercount (or underestimate) to this extent.

### *Response Bias*

Unlike Caplow and Hout and Greeley, Woodberry agrees with us that surveys yield inflated attendance rates (by at least 10 percentage points). He suggests that public opinion polls systematically overstate church attendance rates because frequent church attenders are easier for pollsters to reach and more likely to respond when contacted. In particular, Woodberry questions results from telephone surveys that limit the number of call attempts. Pursuing the possibility that such response bias favors church attenders strikes us as a sensible direction for research. Indeed, we made the same point in our original article, where we noted that if “less religiously active persons are underrepresented in survey results, then self-reported church attendance would be artificially high” (p. 748).

We do not believe, however, that Woodberry’s research adds much information concerning the magnitude of a possible bias produced by survey nonresponse and interviewing procedures. His principal finding is that a 1996 national telephone survey that limited call attempts to four yielded a church attendance rate that was 15 percentage points higher than the 1996 General Social Survey (GSS) rate, using an identical question. He uses this finding to suggest, arbitrarily, that

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suggestion that this implies more error in our data does not follow. It is likely that survey data have been obscuring the true amount of variation in regional and urban/rural church attendance.

<sup>2</sup> Hout and Greeley’s observation that there is more variation by geographical area in our Catholic count data than in survey data is true; but their

sampling issues are responsible for “over half” of the discrepancy we reported between count-based and survey-derived attendance rates. There is no warrant for this conclusion. The 1996 GSS yields a weekly church attendance rate of 37.6 percent (using Woodberry’s translation of GSS response categories into a weekly rate). His survey, based on methodology that was less than ideal, yielded an attendance rate of 52.5 percent.<sup>3</sup> There is, indeed, a 14.9 percentage point difference between the two rates, but we fail to see how that difference is relevant to the issue at hand. No survey that we used as a reference point found an attendance rate as high as that found by Woodberry’s Southern Focus Poll (SFP). The attendance rate in Gallup data was between 40 and 42 percent from 1991 to 1993, and the attendance rate we discovered in Ashtabula County, Ohio in 1992 was only 37.2 percent (using a telephone survey with eight callbacks). The GSS produced a particularly low attendance rate in 1996, but in 1991 to 1994 the GSS-based attendance rate fluctuated between 39 and 42 percent.

In short, attendance rates from the Gallup surveys, the GSS, and our own Ashtabula County survey were similar and are well below the attendance rate Woodberry reports from his SFP survey. This similarity undermines Woodberry’s claim that inflated church attendance rates are produced largely by response bias and sampling problems in telephone surveys. The key point, however, is that the *count-based* rates we reported are well below rates generated by *all* extant surveys—telephone or face-to-face, four callbacks or eight—that directly ask respondents about attending religious services. This fact, obscured in Woodberry’s discussion, strongly suggests that reasons other than response bias account for much of the gap between count-based and survey-based attendance rates. Thus, although Woodberry’s suggestion that response bias is one factor contributing to inflated attendance rates is a sensible one, and although his result contributes to the growing body of evidence showing that poll-based attendance rates are inflated, he provides no empirical basis for es-

<sup>3</sup> This 52.5 percent rate was verified by Woodberry (personal communication).

timating the *magnitude* of such a response-bias effect in the commonly cited national surveys that we address.<sup>4</sup>

#### ASSESSING THE (IR)RELEVANCE OF “INTERNAL CHECKS”

Hout and Greeley use survey evidence to argue in favor of a much smaller gap than the one we discovered. This evidence consists of “internal checks” that employ self-reports of church attendance to determine the existence and extent of overreporting as a function of social desirability. In one such “check” Hout and Greeley ignore an alternative interpretation, and in the other they rely on an unsubstantiated assumption that “intellectuals” are immune to social desirability effects. In our view, neither check is an adequate test for the presence of a gap between self-reported and actual church attendance.

First, Hout and Greeley report no difference between self-reported church attendance rates for male and female respondents, on the one hand, and respondent reports of their spouses’ attendance, on the other. But this finding shows only that whatever dynamics govern self-reported church attendance also govern how a person reports their spouse’s attendance. It seems quite probable that social desirability bias regarding oneself also would carry over to reports about intimates. Perhaps more important is the likelihood that a mechanism *other than social desirability* is responsible for the bulk of the gap between reported and actual

<sup>4</sup> Woodberry also calls attention to the fact that the GSS and the NES (National Election Study) both produce slightly lower estimates of the self-identified Catholic population (about 2 percentage points) than did the National Survey of Religious Identification (NSRI) or the Gallup polls. The magnitude of this difference is trivial, however, and is statistically significant only when the GSS and NES sample sizes are increased by pooling multiple years of data. Moreover, Woodberry’s argument about response bias does not explain why the polls find differing proportions of Catholics and not differing proportions of other more frequently attending groups, such as Baptists. Thus, he has no basis for claiming that the pooled GSS figure for Catholics is more accurate than the figure obtained by the NSRI or Gallup.

church attendance.<sup>5</sup> We refer here to the possibility that the gap is generated by a cognitive process in which respondents interpret the survey question about church attendance to mean something like, "What is your usual pattern of church/synagogue attendance?" or "Are you a good Christian/Jew?" Below, we present evidence supporting the existence of this cognitive mechanism. Here, we simply note that it provides an interpretation of the similarity in spousal results that Hout and Greeley do not consider: Respondents similarly overreport both their own and their spouse's church attendance because the same mechanism—some combination of social desirability, question interpretation, and identity consistency—drives both responses.

Hout and Greeley's evidence concerning "intellectuals" or "skeptical professionals," and their interpretation of differences in self-reported attendance rates between educational and occupational groups as a measure of a social desirability effect, are even less compelling. Their analysis and interpretation rest on the assumption that highly educated people and those in certain occupations are immune to social pressures that affect everyone else who responds to survey researchers. Accordingly, Hout and Greeley treat the self-reports of "intellectuals" as "true" and argue that the small difference between intellectuals and everyone else implies a minimal exaggeration of attendance.

They offer no support, however, for the assumption that the "intellectual elite" are "less susceptible" to distorting their own behavior when talking to a survey researcher. Moreover, Hout and Greeley's operationalization of the "skeptical professional" category confuses the issue. Along with physicists, sociology teachers, and other plausible "skeptics," Hout and Greeley include the following occupations: agricultural scientists; urban and regional planners; health specialist teachers; business and commerce teachers; coaches and physical education teachers; home economics teachers; theology teachers; trade, industrial, and technical teachers; ath-

letes and kindred workers; public relations men [sic] and publicity writers; and radio and television announcers. This is the hodgepodge of "intellectuals" whose "disposition and training" makes them "skeptical . . . and therefore . . . less susceptible (if not immune) to distortions like 'social desirability'" (p. 116)? Indeed, existing empirical evidence points in the *opposite* direction. As Hout and Greeley note, those with more education are *more* likely than those with less education to inaccurately claim to have voted in a recent election. Also, scholars have observed very substantial overreporting of attendance at art museums, live theatrical performances, operas, orchestra concerts, and ballet (Netzer 1992:202–206), venues frequented, of course, much more often by highly educated professionals than by others.

In the end, both of Hout and Greeley's analyses are unpersuasive because self-reports are not compared to some external criterion. If a comparison of survey subgroups yields a difference in self-reported attendance rates, it is impossible to know how much of this difference is produced by real behavioral differences and how much is produced by differing tendencies to overreport certain behaviors. Their conclusion, on the basis of these "internal checks," that weekly church attendance is overreported "by a factor closer to 1.1 than 2.0" (p. 114), is not credible.

### NEW EVIDENCE THAT DEMANDS THE SAME VERDICT

While we believe that the foregoing discussion sufficiently defends the conclusion of our original article, we are able to further strengthen that conclusion by reporting several pieces of new evidence that directly bear on the question at hand.

First, while our original article reported data from 18 Catholic dioceses, Chaves and Cavendish (1994) gathered data from 48 dioceses (including the original 18), containing approximately 38 percent of the U.S. Catholic population. These data represent virtually *all* of the high-quality Catholic data that exist. The result remains the same: The aggregate weekly church attendance rate from these 48 dioceses is 26.7 percent of the Catholic population.

<sup>5</sup> In our original paper we mentioned social desirability as only one possible mechanism among several that might produce the attendance gap (pp. 748–49).

Second, while our original article relied only on aggregate data, we now have individual-level data that yield the same result. Marler and Hadaway (1997) counted attendees at worship, obtained Sunday school records, and interviewed a sample of 300 adult members regarding their church and Sunday School attendance at a large evangelical Protestant church on a single Sunday in 1996. Members were polled by phone beginning Sunday afternoon and continuing until the following Saturday. Using a membership directory it was possible to identify the name of each respondent who said they attended Sunday school and then check that name against the record of actual attendees. Out of 181 adult respondents who said they attended Sunday school on March 10, 1996, only 115 actually did so. The self-reported attendance rate was 60 percent of adult enrollment; the actual rate was 38 percent. The poll-based estimate of Sunday school attendance thus represents an overreporting of more than 20 percentage points—a gap that is consistent with the church attendance gap found among evangelical Protestants in Ashtabula County, Ohio.<sup>6</sup> Who misstates their church attendance? Most were persons who say they normally attend church every week—the very people most likely to think of themselves as active church members. This is exactly the sort of study—one that links head counts and survey respondents at the individual level—that Hout and Greeley say would be more persuasive to them.

Third, recent tests of alternative ways to measure self-reported church attendance support our conclusion that traditional survey questions generate substantial overreporting. In the first two studies that followed the appearance of our article, experimental items generated attendance rates equivalent to traditional items (Belli, Traugott, and Rosenstone 1995; Princeton Religious Research Center 1994). It now appears, however, that these new items produced no reduction in the church attendance rate because they attempted to address only possible errors from

<sup>6</sup> In response to Caplow's suggestion that changing the denominator would change the gap: Note the higher attendance rates when church members (rather than affiliates) are used as the denominator and the consistency of the resulting gap.

telescoping and social desirability. More recent work has recognized the likelihood that overreporting is generated by the combination of a respondent's desire to report *truthfully* his or her identity as a religious, church-going person and the perception that the attendance question is really about this identity rather than about *actual attendance*.<sup>7</sup> Following this logic, Presser and Stinson (1996; 1998) examined data from a survey that asked respondents to list their activities of the previous 24 hours. For respondents interviewed on Mondays, they found that only 27 percent reported attending religious services.<sup>8</sup> Clearly, this result is much closer to our estimate of actual weekly attendance than it is to traditional estimates. Furthermore, Smith (1998) reports similar results from a recent GSS question-wording experiment.

A comparable question-wording effect also was found in Great Britain, where the typical Gallup attendance item asks how respondents spent their time on the previous Sunday rather than if they had attended "church or synagogue in the last 7 days." "Going to church" is given as an option, along with "watching television," "taking a walk," and a number of other activities. In 1993, using this item only 14 percent of the British population said that they went to church (Princeton Religion Research Center 1993).

<sup>7</sup> This phenomenon was dramatically illustrated at the 1994 Society for the Scientific Study of Religion session devoted to our article. One of the commentators, a retired official of the National Council of Churches of Christ, acknowledged that she would say she attended services last week even if it was not true. She went on to say that she would not consider her answer to be a lie, but instead an affirmation of her involvement in and commitment to the church.

<sup>8</sup> That this study is limited to Sunday worship attendance is not a serious problem. We know from several studies that the vast majority of self-reported worship attendance takes place on Sundays. In a survey of Ashtabula County residents, only 3 percent of the total sample attended church on a day other than Sunday. If this rate is similar to the national level of non-Sunday church attendance, the percent of respondents who said they attended church in the Presser and Stinson (1996; 1998) study would rise to approximately 30 percent—still far below the rate regularly yielded by traditional surveys.

In a poll conducted in September 1996, the traditional Gallup U.S.-wording of the attendance question—asking about church attendance in the last seven days—was included on a Gallup U.K. face-to-face omnibus poll in Britain. It produced a self-reported attendance rate of 21 percent (Hadaway and Marler 1997a).

We consider these research findings to be among the most exciting developments to result from the publication of our article. We say this because they clearly point the way to more valid survey-based estimates of church attendance. More generally, these survey results further bolster the overwhelming empirical evidence supporting of our original conclusion: Weekly attendance at religious services in the United States is substantially lower than conventionally reported.

### THE QUESTION OF HISTORICAL CHANGE

Hout and Greeley focus part of their comment on the issue of historical change in church attendance. Two points are relevant here. First, in our original article, we claimed empirical support for one proposition and one proposition only: "We estimate that church attendance rates for Protestants and Catholics are, in fact, approximately one-half the generally accepted level" (abstract, p. 741). We did *not* claim that the attendance gap had increased over time. We addressed the question of historical change only as an interesting speculative issue that was raised by the demonstration of the contemporary gap. Indeed, the fact of a contemporary gap reopens a historical question that may have been prematurely closed by too much confidence in trends shown by self-reports.

Second, the only data we are aware of that permits a direct assessment of the historical question suggests, in fact, that the gap between self-reported and actual church attendance rates has increased over time. In our original article we compared the survey-based and the count-based attendance rates for Catholics in San Francisco (both data sources collected in 1972) and found that the 95-percent confidence intervals for the two rates overlapped (p. 747). In other cities, based on more recent data, the rates were widely separated. Does this mean that

the gap is now wider in San Francisco, or that the gap has always been smaller there than in other places? To answer this question Marler, Hadaway, and Curtis (1997) conducted a survey of community residents in the three-county area conterminous with the Archdiocese of San Francisco in 1996 and compared the self-reported attendance rate of Roman Catholics to the annual fall attendance count conducted by all parishes in the archdiocese. The results are compelling. The 1972 poll-based attendance rate represented a 58 percent overreporting of attendance, but by 1996 overreporting had increased to 115 percent. During this 24-year period, aggregate attendance at mass declined, but the population of Roman Catholics increased slightly, as did the self-reported rate of church attendance among Roman Catholic affiliates. The net result was an increasing attendance gap.

San Francisco does not represent all of America, and we do not consider the above test to definitively establish a historical change in the overreporting of worship attendance. Still, this finding, along with supporting evidence in Britain (Hadaway and Marler 1997a) and among American youth (Presser and Stinson 1998), suggests that a historical decline in actual church attendance may have occurred, even though self-reported rates of attendance have been remarkably stable. If church attendance is overreported and if the degree of overreporting varies by denominational type and by question wording, it follows that overreporting is influenced by social processes. And if overreporting is so influenced, there is no theoretical reason to dismiss the possibility that such processes might change over time.

### CONCLUSION

Despite our critics' claims, we see no reason to revise our earlier finding that actual weekly U.S. church attendance is substantially lower than previously reported: Their direct criticisms are not compelling, and their "internal checks" are not persuasive. Most importantly, *every extant piece* of relevant empirical evidence supports our original conclusion. Weekly attendance at religious services in the United States is closer to one person in four than to two in five, and

conventional survey items produce a substantial overreporting of church attendance.

This lower observed weekly rate and the gap between self-reports and actual attendance are social facts that should not be ignored. These facts also raise important empirical and theoretical questions about American religion: To what extent do these findings challenge the conventional wisdom that Americans are a very religious people? What specific social and cultural factors influence the relationship between behavior and self-perceptions of behavior? What might be learned about social change and symbolic identities (political, racial/ethnic, religious, etc.) by investigating this relationship? We believe that the answers to these questions lie in comparative and historical analysis. We should not presume, for instance, that overreporting church attendance is a peculiarly American phenomenon. As we have shown, overreporting occurs in Canada and Great Britain, and it likely occurs elsewhere. Nor should we presume that overreporting is limited to the 1990s. The gap between reported and actual attendance is a result of social processes whose magnitude is likely to vary across time and space. Investigating this variation and its correlates may lead to increased understanding of religion's cultural and social significance.

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