Commentary: Understanding religious involvement and mortality risk in the United States: comment on Bagiella, Hong, and Sloan

Robert A Hummer

In high profile academic outlets and the popular press, Bagiella, Hong, and Sloan1 (hereafter BHS, based on the authorship of the article in this issue) have been very outspoken critics of the scientific literature on religion and health/mortality, as well as the implications that such literature may or may not have for the practice of health care and medicine in the United States.2–4 While praising a very limited number of empirical studies in the area, including mine, that have shown a protective relationship between public religious involvement and mortality, their essays have strongly critiqued much of the methodological work in the religion–health area and they have used such critiques as part of their rationale for efforts to keep religion out of medical and health care practice. Seemingly, their critiques were written without doing any of their own previous empirical work in the area. This article, then, is a very interesting foray for this group into such empirical work. Using data from four US communities, they investigate the relationship between self-reports of public religious attendance and subsequent mortality risk, first using pooled data from the four sites and then investigating each separate site. They find, as others have, that frequent religious attendance is related to lower overall adult mortality risk in the United States, that the baseline relationship is reduced with the inclusion of a large set of control variables, and that the relationship varies somewhat across the study sites. None of this is a surprise and, in and of itself, is a welcome addition to the literature. The technical aspects of the statistical analyses also seem to be mechanically well done. There are, however, several very important critiques that should be taken into account when placing this article in the context of this literature.

First, and most important, BHS demonstrate very little understanding of what religious attendance might mean within the context of US society and within the context of these four communities for the mortality risk of individuals. Indeed, one can read the article and, with the exception of a few very general statements, forget that the key predictor they are examining has anything to do with religion at all. What does religious attendance mean for individuals? Why might it be associated with mortality risk? What are the key confounders and mediators of the relationship? There are a number of excellent classic and contemporary works conceptualizing religious attendance as a social phenomena, detailing measures of religion and what they mean, and laying out the behavioural, psychological, social, and health mechanisms by which religious involvement might work to influence mortality.5–10 None of this literature is referenced, nor did it have any impact on what BHS apparently thought about religious attendance, the religion–mortality relationship, or the possible confounders or mediators of this relationship. As a result, their interpretation of the findings and conclusions can be seriously questioned. For example, they state that, ‘At the New Haven and Iowa sites, although the (attendance) effect was in the same direction (as found in the Duke and East Boston sites), it did not reach statistical significance.’ While this is true in the more complete models that were specified, this is simply untrue in the less complete models (see their Table 5). In other words, their models in the Iowa and New Haven sites were able to statistically eliminate the religious attendance effect with control for physical health, smoking, mental health, and social involvement. This does not mean, however, that religious attendance is unrelated to mortality risk. What it does mean is that the control variables that were included were either successful confounders or successful mediators of the religion–mortality relationship; however, they provide no theoretical guidance for helping readers understand this important change in the attendance coefficients—only instead, concluding that there was no association in two of the four sites. Using this same logic, their education, social involvement, and depression variables also display no association with mortality in the pooled cohorts (see their Table 3). A large number of social epidemiologists, sociologists, psychologists, and demographers who study mortality would have serious misgivings about such conclusions. What must be remembered here is that religious attendance should, indeed, display no association with mortality if the complete set of confounding and mediating variables that drive the overall relationship are included in the statistical models. However, they simply do not consider which controls they include to be probable confounding variables or mediating variables; they do not place the religiosity–mortality relationship in any kind of theoretical framework. This key omission leads to after-the-fact speculations in the conclusion that are simply ad hoc.

Second, the paper’s starting point is what BHS describe as inconsistent findings and they return to this theme in the conclusion. Their introduction describes findings from a number of studies that show that public religious attendance is related to an overall lower mortality risk (as they also find),
but with differing magnitudes by age, gender, race, geographic area (as they also find), and cause of death. Considering public religious involvement as a social phenomenon, rather than as a dose of medicine that is supposed to work the same for everyone in all places, makes such differential associations both extremely interesting and much more complex than BHS seem to seriously consider. The key point here is that public religious involvement should not be expected to work in the same way for mortality risk in different geographic contexts (or for different age, gender, race/ethnic, educational, and other demographic, social, and geographic groups); context is critical.

Third, if the intent of the BHS study is to aid in their well-publicized efforts to keep religious influences out of the practice of medicine, at least based on findings like their own or well-received studies in the religion–mortality area, I am in agreement. A recently published review by myself and several of my colleagues reflects such sentiment. Based on findings in the religious attendance–mortality literature, implications for the practice of health care and medicine are limited. Other portions of the religion–health literature—that deal much more directly with religion’s possible impacts within clinical settings and with which I am much less involved or an expert on—would seem to be much more relevant in terms of possible implications for health care and medical practice. At the same time, the work by BHS in this issue, as well as related religion–mortality studies within this literature, absolutely beg for a greater understanding of the population-based relationship between religious involvement and mortality for a number of reasons, most importantly including: (i) the development of better social and epidemiological theories of health and mortality; (ii) the better understanding of how demographic and social contexts have impacts on health and mortality patterns.

In conclusion, the BHS article is interesting because it comes from outspoken critics in this area of study, but includes interpretations of the data and conclusions that seem to be based on: (i) a lack of understanding of religious attendance as a social phenomena in the United States; (ii) no theoretical guidance; (iii) questionable interpretations of what their regression coefficients might mean; (iv) little concern for how contexts are important for mortality patterns at the population level; and (v) an agenda that seems to be based on keeping religion out of medicine and health care but is trying to do so using social science and social epidemiology research findings that do not really touch upon religion as an aspect of medical or health care. This fascinating area of scientific inquiry deserves better treatment. It is time to put aside other agendas and work towards scientifically better understanding, with appropriate theoretical guidance, why religious involvement seems to have a beneficial association with mortality risk in the United States.

Acknowledgements
I thank the National Science Foundation for financial support in this area of research (grant #SES-0243189) and Christopher G. Ellison for comments on an earlier draft.

References