Dr. Hall bases his analysis on observational data that are of questionable validity. The fact that churchgoers live longer than people who do not attend church may very well have nothing to do with churchgoing but may result from uncontrolled confounding.

Observational studies that showed a benefit of exogenous estrogens in postmenopausal women were debunked by the Women's Health Initiative Randomized Controlled Trial.[1] The accepted explanation for the discrepancy between the observational findings and the experimental results is that women who took estrogens were systematically different from non-users in ways which resulted in improved outcomes (eg, reduced coronary disease).

Barrett-Connor referred to this as the "healthy user effect."[2] The inability to control for the healthy user effect resulted in the biased findings of many observational studies of estrogen use.

Similarly, churchgoers are systematically different from non-churchgoers in ways that are difficult to measure but are likely to result in improved health outcomes that may have nothing to do with churchgoing. Churchgoers are more likely to be employed, have intact families, and are less likely to be homebound by illness or disability. Until the healthy attender effect can be controlled for, it is unwise to attempt to make any inferences about the effect that churchgoing has on health.

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The above letters were referred to the author of the article in question, who offers the following reply.

To the Editor: I would first like to thank Drs. Denberg, Larimore, Mann, and Millard for their perceptive comments. I had hoped my article would spark thoughtful debate among both the "proponents" and "opponents" in this continuing conversation, and judging by these letters, my efforts have been rewarded. I am also grateful that the editors have seen fit to continue the conversation in print.

Before addressing particular comments, it is important to restate that my article was written to make a specific, limited, rhetorical argument against those such as Drs. Sloan and Bagiella who would dismiss the association between religious attendance and longer life as so small as to be clinically irrelevant.[1] I did not collect any new data, but simply reframed existing data using life table analyses to present the findings in a more intuitive metric. As such, it was written to anticipate the objections of a skeptical audience, including some of the peer reviewers. The admittedly dramatic and playful title was chosen to hook readers into the sustained argument of the text, and such rhetorical strategy is not without precedent within professional literature.

Both Drs. Millard and Denberg note the problem of confounding. My article did not specifically describe the statistical controls used for confounding variables because these details are set forth in the methods sections of McCullough's meta-analysis[2] and the primary studies contained therein. As with any meta-analysis, the controls were not identical between data samples, but most of the underlying studies met or exceeded standards for epidemiologic research as they controlled for age, race, income, marital status, smoking, alcohol consumption, employment, baseline health (physical, functional, and mental), social support, employment, and exercise. One elegant study even controlled for what Dr. Millard calls the "healthy user effect" by factoring into the logistic regression data regarding each subject's physical capacity to actually attend religious services (or whether they were homebound).[3] Drs. Millard and Denberg are correct in noting that prospective, cohort studies cannot establish causality, but a careful study of the underlying data demonstrates that the association between religious attendance and longer life cannot be dismissed as mere confounding.

Dr. Denberg asks for a "plausible, scientific hypothesis" to explain the noted association. Such hypotheses have been offered throughout the literature, but were beyond the scope of the limited argument of this article and would have been unnecessary duplication of other work. The hypotheses are still works in progress, but there is growing consensus that the observed associations are mediated by a complex network of behavior, social support, practices of coping and worldview that manifest physical changes through some form of psychoneuroimmunological mechanism.[4–7] Dr. Denberg is correct in noting that these specific mediating pathways can and should be the focus of further research. However, it remains an empirical question as to whether or not such practices can be sustained or even studied "effectively" when divorced from the religious contexts that give them meaning. The data suggests that religious belief and practice are in some way uniquely influential in shaping and sustaining practices relevant to health. In other words, the social support engendered by religious attendance may be uniquely relevant to mortality even after controlling for non-religious forms of social support, and therefore, future research should be aimed at understanding how specifically religious forms of social support are unique. In fact, some have argued that mediating variables like social support should no longer be treated as confounders, but as unique pathways through which the multidimensional construct of religious belief and practice mediates observed associations.[4,8,9] There may be secular analogues for many of the proposed mediating pathways, but as Drs. Larimore and Mann contend, religious communities remain profoundly influential for many patients, and the specific nature of this influence is a relevant topic for study.

Drs. Mann and Larimore are enthusiastic about pursuing the kind of empirical research that could demonstrate the effectiveness of religious interventions, and they take issue with my reservations regarding such an instrumental approach to religious belief and practice. I am sympathetic to many of their points, and there is much about which we agree. However, as I have argued elsewhere, I am convinced that meaningful findings in this field of research will depend increasingly on the capacity to conceptualize and operationalize "religion" in more sophisticated ways than are currently modeled.[10] I suspect this will require abandoning attempts to study "religion-in-general" in favor of measuring the associations specific to particular religions or spiritualities. I also suspect that such research will begin to describe the ways that particular religious traditions are both "adaptive" and "maladaptive" from the perspective of biomedical science—"take up your cross and follow me" isn't a promise to live long and prosper.

The difficult work of better conceptualizing religion is all the more important given the legitimate concerns of those like Dr. Denberg who criticize the way that ideology drives the national debate regarding controversial issues like "intelligent design" and prayer. However, as I have noted elsewhere, ideological assertion is found on both sides of these controversies.[11] To categorically neglect data regarding one phenomenon (religion) because the data are controversial may betray a secular ideology that is anything but neutral. The best traditions of scientific inquiry seek to minimize the influence of ideology (both secular and sacred) in favor of dispassionate assessment of data, and this is what I have tried to do in my article. After all, as Dr. Denberg notes, it was a peer-reviewed, blinded trial of intercessory "prayer" that has most discredited the instrumental use of prayer as a medical intervention.

I fear that Drs. Mann and Larimore miss my point when I suggest that researchers approach religious belief and practice as a demographic factor. I agree that religion is often perceived by faithful people as "therapeutic" in many and various ways. I also agree that moral complexity does not, in itself, preclude physicians from engaging their patients regarding religion, and I have written elsewhere about how physicians might appropriately engage this aspect of patient care.[12] I even acknowledge that it is theoretically possible to design a randomized intervention through which physicians could encourage patients to attend religious services in hopes of improving some aspect of health (though I am not convinced such a study would or could yield meaningful findings). However, there are formidable philosophical, scientific and ethical challenges to "using" religion as a therapy,[10,13] and even if those hurdles are cleared, research may demonstrate that "religion" is not effective. Yet even if future research demonstrates that religious belief and practice cannot (technically) or should not (ethically) be manipulated as a therapeutic agent, I suspect religion will remain a relevant factor in the delivery of health care because like other demographic factors, specific religious traditions may be associated with patterns of disease, health, and value that can guide medical decision making. These aspects of religious belief and practice will remain relevant to health professionals regardless of the more contested issue of whether or not religion "works."

Even though many faithful people perceive their religion to be "therapeutic" in the layman's sense of the word, most religious traditions (or at least the Christian traditions with which I am most familiar) do not understand themselves to constitute a "therapy" in the technical, scientific sense of the word, and therefore, I am reluctant to shoe-horn "religion" into the existing medical model of therapy. From the Christian perspective, the call to discipleship is an invitation to a *relationship* with God. "Seeking a relationship with Christ" is not necessarily the same thing as "attending church," and even if the earnest desire for longer life leads someone to seek a relationship with God, if the nature of the relationship remains only a transactional exchange for better health, it falls short—idolatrously so—of the kind of relationship to which the Christian God calls all people. This does not mean that Christians should not seek God's healing. Rather, it simply means that it is inadequate to conceptualize "religion" merely as a therapy, and that any "use" of religious practice for merely therapeutic ends is likely an idolatrous expression of that religion. I agree that it is "not the place of the medical community to determine what is or is not idolatrous," but if scientists are to study religion, some degree of theological sophistication is required to avoid distorting the subject of study.[13]

Drs. Mann and Larimore argue that "experienced clinicians should encourage positive spiritual interventions to interested patients," suggesting that it is "incumbent on [physicians] to provide [spiritual] treatment to. . . improve health." Although there is substantial data describing associations between "religion" and health, I think it is premature to make so sanguine a recommendation. To my knowledge, aside from the problematic studies of intercessory prayer, there is only one published trial of a "spiritual intervention" where a secular cognitive behavioral therapy (CBT) for depression was compared with a form of CBT adapted to the idiom and metaphor of the Bible.[14] The data for confident *intervention* is simply not yet available.

In conclusion, I share many of the concerns voiced by Drs. Denberg and Millard, but quite apart from the therapeutic manipulation of religious belief and practice, I couldn't agree more with Drs. Mann and Larimore about the urgent need for physicians to explore ways through which they can provide or facilitate the spiritual support and comfort on which much of the hope, and perhaps even the health, of our patients may depend. It is toward this end that I wrote my paper.

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