professions for ignoring their spiritual needs. In addition, professional associations and educational institutions are beginning to provide learners and clinicians information on how to incorporate spirituality and practice. Furthermore, anecdotal evidence indicates that clinicians having received training in providing spiritual or religious interventions in clinical care find it immediately helpful and do apply it to their practice without self-reported evidence of harm.[8]

Therefore, the medical community has an obligation to take the observed relationships seriously and to carefully consider their implications for clinical practice and public health. Our view of the evidence to date is that trained or experienced clinicians should encourage positive spiritual interventions to interested patients (easily determined with any of several simple spiritual assessments) and that there is no evidence that such interventions are, in general, harmful.

Further, unless or until there is evidence of harm from a clinician's provision of either basic spiritual care or a spiritually sensitive practice, interested clinicians and systems should learn to assess their patients' spiritual health and to provide indicated and desired spiritual intervention. Clinicians and health care systems should not, without compelling data to the contrary, deprive their patients of the spiritual support and comfort on which their hope, health, and well-being may hinge.

The possibility of integrating religious/spiritual interventions into medical practice should not be dismissed without a thorough and open discussion about all the issues involved, and without more rigorous research about the potential benefits and/or harms of such interventions.

Joshua R. Mann
University of South Carolina
School of Medicine, Columbia
Walter Larimore
University of Colorado Health
Sciences Center, Denver

References

1. Hall DE. Religious attendance: more cost-effective than lipitor? J Am Board Fam Med 2006;19:103–9.
2. Hollins S. Spirituality and religion: exploring the relationship. Nurs Manag (Harrow) 2005;12:22–6.
3. Puchalski C. Spirituality in health: the role of spirituality in critical care. Crit Care Clin 2004;20:487–504.
4. Larson DB, Larson SS, Koenig HG. Mortality and religion/spirituality: a brief review of the research. Ann Pharmacother 2002;36:1090–8.
5. McCullough ME, Hoyt WT, Larson DB, Koenig HG, Thoresen C. Religious involvement and mortality: a meta-analytic review. Health Psychol 2000;19:211–22.
6. Larimore WL, Parker M, Crowther M. Should clinicians incorporate positive spirituality into their practices? What does the evidence say [review]? Ann Behav Med 2002;24:69–73.
7. Larimore WL. Providing basic spiritual care for patients: should it be the exclusive domain of pastoral professionals? Am Fam Physician 2001;63:36, 38–40.
8. Larimore WL, Parker M, Crowther M. Should clinicians incorporate positive spirituality into their practices? What does the evidence say [review]? Ann Behav Med 2002;24:69–73.

Religious Attendance: More Cost-Effective Than Lipitor?

To the Editor: The title of this piece is unnecessarily provocative and inappropriate for a scientific medical journal. The study is not, as the author actually acknowledges, a proper econometric analysis. The title is not only deceptive in this regard, but it also suggests—which the author himself disavows—that religious attendance could potentially substitute for cholesterol-lowering drugs or other medical interventions.

The study mentions, but does not address as directly and prominently as it should, the importance of confounding: the idea that people who attend religious services regularly may also be more likely to do other things that benefit their health, such as get regular exercise, eat well, enjoy social support, see doctors, and adhere to medications. Alternatively, healthier people may be more likely than those who are less healthy to attend religious services on a regular basis. Nothing more than a quick nod to these possibilities—which, prima facie, have a more directly causal relationship to health—encourages the interpretation, especially by unsophisticated readers, that religious belief or attendance at religious services is *itself* responsible for health benefits.

A study such as this should, but does not, present a plausible *scientific* (read: mechanistic) hypothesis as to why religious attendance, per se, has positive health effects. If confounding variables are more important, then *these* should be the focus of further research. Simply reporting an association (the veracity of which I do not doubt) and then calling for "further research" is trivial and unworthy of publication in this journal.

Ultimately, I am concerned that this kind of "research" is part of a larger, troubling trend in American society to bring religion closer to politics and to enhance the "scientific credibility" of concepts such as Intelligent Design and the healing power of prayer at a distance (recently discredited in a large clinical trial, by the way). Certainly, religious beliefs are valuable to those who hold them, but scientific studies of the potential health benefits of religion need to go beyond the mere reporting of associations.

I believe your decision to publish this article, and to publish it under the title you did, was regrettable.

Tom Denberg, MD, PhD
Assistant Professor of Medicine,
Division of General Internal Medicine
University of Colorado Health
Sciences Center, Denver

Reference

1. Hall DE. Religious attendance: more cost-effective than lipitor? 2006;19:103–9.

J Am Board Fam Med: first published as 10.3122/jabfm.19.4.430 on 29 June 2006. Downloaded from http://www.jabfm.org/ on 6 October 2026 by guest. Protected by copyright.

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.

Peter S. Millard, MD, PhD
Family Practice Residency Program
Eastern Maine Medical Center
Bangor, ME

References

1. The Women's Health Initiative Steering Committee. Effects of conjugated equine estrogen in postmenopausal women with hysterectomy: the Women's Health Initiative Randomized Controlled Trial. JAMA 2004;291:1701–12.
2. Barrett-Connor E, Bush TL. Estrogen and coronary heart disease in women. JAMA 1991;265:1861–7.

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.

J Am Board Fam Med: first published as 10.3122/jabfm.19.4.430 on 29 June 2006. Downloaded from http://www.jabfm.org/ on 6 October 2026 by guest. Protected by copyright.