

FIELD ANALYSIS OF THE LITERATURE ON RELIGION, SPIRITUALITY, AND HEALTH

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INTRODUCTION

While the literature on religion, spirituality, and health has improved since our first review in 1999, a great deal remains to be accomplished. This review addresses current problems in the field and makes recommendations for activities worth pursuing, not worth pursuing, and about an important clarification of the aims of such research.

CURRENT PROBLEMS IN THE FIELD

Paucity of Evidence

The most thorough review of the empirical evidence is that of Powell et al. in 2003 ⁽¹⁾. This review is vastly superior to the voluminous but highly questionable *Handbook of Religion and Health* by Koenig et al. ⁽²⁾. We have shown, for example, that the *Handbook* overestimates by a considerable margin the number of studies that support the proposition that religious involvement is associated with beneficial health outcomes ⁽³⁾.

In the Powell et al. review, 9 hypotheses about the connection between religion and health were evaluated. Powell et al. concluded that only in the case of studies of attendance at religious services and mortality was the evidence persuasive. In all other cases – that religion or spirituality protects against cardiovascular disease, against cancer mortality, that deeply religious people are protected against death, that religion or spirituality protects against disability, that religion or spirituality slows the progression of cancer, that people who use religion to cope with difficulties live longer, that religion or spirituality improves recovery from acute illness, and that being prayed for improves physical recovery from acute illness – the evidence was at best equivocal.

It is true that studies of religious attendance and mortality are the strongest of the lot but even so, there are significant problems with them. These problems include self-selection ^(4,5), residual confounding ⁽⁶⁾, measurement error in the self-report of attendance ^(7,8), and data dredging ⁽⁹⁾. Most of these problems characterize the field as a whole, too.

Heterogeneity of Findings

Even among studies that are well conducted and show effects of attendance on mortality, there is considerable heterogeneity in the findings. For example, Hummer et al. found that after adjusting for confounders and covariates including functional status and social connection, frequency of religious attendance was inversely associated with mortality in a study of over 21,000 subjects ⁽¹⁰⁾. However, the protective effect was entirely absent for patients with cancer and only marginally significant for patients with

heart disease, the two diseases that account for the bulk of deaths in the US. Omen and Reed found that in a community sample of 1931 affluent, largely white adults over age 55 in Marin County, California⁽¹¹⁾, religious attendance was associated with reduced mortality in multivariate model (RR = 0.76, 95% CI = 0.62, 0.94), an effect seen for both men and women. However, when they used a different measure of attendance and added individual items representing various types of social engagement, the risk ratio rose to 0.81 (95% CI = 0.81, 1.00) and the model retained museum or art gallery attendance (RR=0.81, 95% CI = 0.63, 1.04) as a marginally significant effect. In contrast to the main finding of Oman and Reed, the Tecumseh Community Health⁽¹²⁾ and Alameda County studies⁽¹³⁾, frequency of attendance at religious services was inversely associated to mortality but after control for all relevant covariates, this relationship held only for women. In contrast, for men but not women in the Tecumseh study, frequency of attendance at meetings of voluntary organizations was associated with reduced mortality while religious attendance was not⁽¹²⁾. In a study by Schoenbach⁽¹⁴⁾, the effect of religious attendance on mortality was seen primarily for white men only. In the Duke cohort (N=3968) of the EPESE study, the effect of religious attendance was significant in the multivariate model, but in the full model, the effect for men achieved only marginal significance (RR=0.83, 95% CI 0.69-1.00)⁽¹⁵⁾. In the New Haven EPESE cohort, no such association was found⁽¹⁶⁾ but more generally, social and productive activities were associated with reduced mortality⁽¹⁷⁾.

Residual Confounding

A broader problem with the religious attendance literature is the inability of studies conducted to date to tease apart attendance from more general efforts to remain active in one's community. While this is difficult, it is not in principle impossible and evidence from several community studies suggests that this distinction is important. For example, in the New Haven cohort of the EPESE study, attendance at religious services was not associated with future mortality but engagement in community and social activities was, suggesting that the broad category of remaining active in older age more generally is the operative factor. Such engagement could mean attendance at religious services but could also include volunteering at schools or literacy programs, going to the public library, or working in a community center.

Some studies, while showing associations between religious attendance and reduced mortality, do so from the perspective that religious attendance is one of many indices of social engagement. For example, in a cohort of 15,938 subjects, age 55 or older, enrolled in the National Health Interview Survey, attendance at religious services in the past two weeks was inversely related to mortality but so were attending shows, movies, and concerts, socializing with friends and neighbors, visits with relatives, and volunteerism⁽¹⁸⁾. Not surprisingly, analysis of a subset of these data restricted to participants age 70 years and over showed the same findings⁽¹⁹⁾.

Measurement issues

One of the central concerns of this literature is a lack of specificity of religion and spirituality, the putative “independent” variables in these studies. A similar, although not identical problem arises for the outcome variables. No scientific inquiry can proceed without rigorous specification of the variables in question. The difficulty in clearly defining independent and dependent variables no doubt contributes significantly to the paucity of evidence described above.

One would assume that religion, in contrast to spirituality, is easier to specify and although this may be so in principle, in practice difficulties have arisen

Independent Variables

In the literature examining the health correlates of religious involvement, there are almost as many different definitions of religious involvement as there are studies. A great many studies have examined self-reported attendance at religious services as an index of involvement. Other have assessed self-reported prayer, reading the Bible or other religious texts, listening to religious radio, or watching religious TV. Others studies have addressed denominational differences, e.g., Christians vs. Jews, Protestants vs. Catholics, Seventh Day Adventists vs. residents of New York City. Still others have operationalized religiosity as the degree of orthodoxy within a given religion, e.g., orthodox vs. reform vs. nonreligious Jews.

Such diversity of definitions of religiosity have an advantage and a disadvantage. From a measurement perspective, if these different ways of operationalizing religiosity resulted in consistent results, we would be increasingly confident that they all tap into an underlying construct of religiosity. In the absence of such consistent findings – the current state of the evidence – such definitional inconsistencies lead more to confusion than clarity. This may be why studies of religious attendance and mortality – in which both the independent and dependent variables are relatively unambiguous – are the strongest of the studies on religion and health.

But even in this case, there are significant measurement problems. Presser and Stinson have demonstrated a significant self-presentation bias in studies of religious attendance and mortality that employ interview methods, either in person or by phone⁽⁸⁾. They suggest that during such interviews, specific questions about church attendance are understood by many participants as asking whether or not the participants are good Christians. In the interpersonal setting of the interview, questions about religious attendance - “how often do you attend religious services? More than once/week? Once/week? Once or twice/month? – engage a self-presentation bias that leads participants inflate their estimates of church attendance.

The alternative method of data collection – time use estimation - asks participants about the activities they engaged in during the past week with no reference to any particular activity. For example, participants are asked what they did at 9 am on last

Monday?10 am on Monday?... and so on throughout the entire week, including Saturday and Sunday, when participants could report attendance at religious services. Presser and Stinson showed that time use estimation leads to a substantial reduction in self-reported attendance at religious services compared to interview methods that ask specifically about how often the participants attended services. Hadaway et al. have similarly demonstrated such overestimation^(7, 20).

The problem is worse still with definitions of spirituality. Recognizing this lack of clarity, Larimore et al. attempt to provide some guidance, endorsing what they refer to as “positive spirituality,” which “involves a developing and internalized personal relationship with the sacred or transcendent”⁽²¹⁾ (p. 71). Positive spirituality, to be encouraged by the physician, is characterized by “honesty, self-control, love, joy, peace, hope, patience, generosity, forgiveness, thankfulness, kindness, gentleness, goodness, faithfulness, understanding, and compassion” (p. 71) as means toward better mental and physical health⁽²¹⁾. These values are virtues to be sure but we have no evidence that they are associated with better health. And questions should arise about whether it is the business of physicians to promote them, regardless of their merits as a whole. Suggesting that it is the business of physicians to make recommendations about the values that their patients hold represents an arrogant and unwarranted extension of the role of a doctor.

In contrast to this list of virtues, Miller and Thoresen, in a review article, report that in popular usage, “spirituality” is distinguished from material reality and as such, refers to the transcendent, something beyond the self⁽²²⁾. Anandarajah and Hight agree. They assert that “world’s great wisdom traditions suggest that some of the most important aspects of spirituality lie in the sense of connection and inner strength, comfort, love, and peace that individuals derive from their relationship with self, others, nature, and the transcendent”⁽²³⁾ (p. 87).

According to a draft report of a consensus panel on spirituality in medical education, spirituality transcends rituals, dogmas, institutions, and religions. It refers to the striving for meaning, growth, development, transcendental experience, and ultimate hope that keeps humans going. This definition agrees in general with that of Anandarajah and Hight. Studies that use the popular FACIT-SWB (for spiritual well-being) instrument generally are consistent with this definition of spirituality. The FACIT-SWB operationalizes spirituality as consisting of two factors: a sense of meaning, peace, and purpose in life and faith⁽²⁴⁾.

The “spiritual index of well-being”⁽²⁵⁾ takes an entirely different view of the term. This index is a 12-item scale consisting of two subscales assessing self-efficacy and “life scheme” with the latter reflecting a sense of self-directedness. This is spirituality as Horatio Alger, the great American striving for accomplishment. In the SWIB, there is nothing about transcendence, nothing about other directedness. Spiritual well-being consists in knowing what you want and believing that you can get it.

Because definitions of spirituality that appear in the literature are more diverse and less well established than definitions of religiosity, it is extremely difficult to draw conclusions about whether spirituality is associated with health outcomes.

Outcome Variables

In the case of the outcome variables in the literature on religion and spirituality and medicine, the problem is not that the variables are poorly defined but rather that they vary widely from study to study.

Again, the Powell et al. review is informative. In the 9 hypotheses they identify, the following outcome variables appear: mortality, protection against CVD, cancer mortality, disability, cancer progression, and recovery from acute illness. The *Handbook of Religion and Health* has a great many chapters, each devoted to a different outcome variable. Among those variables are hypertension, cardiovascular disease, cerebrovascular disease, cancer, disability, pain, health behavior, immune system dysfunction, depression, suicide, marital instability, delinquency, substance abuse, and schizophrenia. And within each of these chapter headings, there are multiple outcome variables.

And within each of these categories, there are multiple variants. For example, the chapter on cardiovascular disease cites studies that examine improved functioning, adherence to treatment, and diminished health concerns a year after cardiac transplantation, length of stay in ICUs, length of stay in the hospital, pain medication required, arrhythmic events, blood pressure, functional status, disability, and blood lipids.

This variety of outcome variables results in part from the different interests of investigators. Nevertheless, the enormous variety makes it difficult to come to general conclusions about associations with religiosity. What are we to say if, for example, one study showed that attendance at religious services was associated with lower blood pressure while another failed to show any relationship between frequency of prayer and blood lipids?

The Sharpshooter's Fallacy and Related Problems

In many cases, individual studies will measure many of these variables. A problem produced by this strategy is the likelihood that if enough outcome variable are measured, one certainly will achieve statistical significance. This is the problem of multiple comparisons that arises from the failure to adjust the level of significance for the number of statistical tests conducted ⁽⁶⁾. Physicist Robert Park has referred to this as the "sharpshooter's fallacy": the sharpshooter empties the six-gun into the side of the barn and *then* draws the bullseye. An excellent example of this problem is provided by a study of Koenig and colleagues in which well over 100 outcome variables were measured ⁽²⁶⁾. Such a strategy guarantees that some of the variables collected will achieve a level of statistical significance unless alpha levels are adjusted downward.

Such analytic behavior falls well short of what is methodologically acceptable for hypothesis testing. The only appropriate stance to take regarding such fishing expeditions is that they are exploratory investigations: any associations that achieve a 0.05 level of significance should then be tested as hypotheses in *new datasets* with appropriate control for multiple comparisons.

A related problem, not often easy to detect, is that in the large datasets very often used in these studies, it is possible to cut the data in a great many ways before conducting analyses. So for example, Helm et al. reported that among a sample of the elderly who were not functionally disabled, private religious behavior, e.g., reading the Bible, watching religious TV, prayer, was associated with reduced mortality⁽²⁷⁾. Because in the entire sample, no such relationship between private religious behavior and mortality existed, one must wonder why the authors decided that dichotomizing the data on functional status was crucial. More likely, they cut the dataset in multiple ways, e.g., by sex, race, education, etc. until a “significant” finding emerged. The absurd conclusion they drew – that private religious behavior protects only those who engage in it for a lifetime - is consistent with this view. After all, how did Helm and his colleagues know that those already disabled did not also have such a habit of private religious behavior?

Reliance on datasets designed for other purposes

Many of these problems arise directly from the use of large datasets that contain information on religious activities and beliefs and health variables but were designed for other purposes. This encourages a practice referred to as “data-dredging,” in which investigators, now aided by the availability of powerful computers, conduct analysis after analysis until something “emerges.”

Failure to adequately consider ethical and theological issues

At least three significant ethical problems arise in connection with attempts to link religious activities to health outcomes: manipulation, invasion of privacy, and causing harm.

Manipulation Health professionals even in these days of consumer advocacy retain influence over their patients by virtue of their medical expertise. This threat to patient autonomy was raised most recently by Cassell in the *New England Journal of Medicine*⁽²⁸⁾. When doctors depart from areas of established expertise to promote a non-medical agenda, they abuse their status as professionals and violate the implicit norms of the physician patient relationship. Some physicians apparently believe that they should inquire into the patient’s spiritual life in the service of making recommendations that link religious practice with better health outcomes. Is it really appropriate, as Matthews et al.⁽²⁹⁾ recommend, for a physician to ask patients what he or she can do to support their faith or religious commitment?

Privacy A second ethical consideration involves the limits of medical intervention. If religious or spiritual factors were shown convincingly to be related to health outcomes,

they then would join such factors as socio-economic status and marital status⁽³⁰⁾, already well established as significantly associated with health. While physicians may choose to engage patients in discussions of these matters to better understand them, we would consider it unacceptable for a physician to counsel a single patient to marry because the data show that marriage is associated with lower mortality⁽³⁰⁾. This is because we generally regard financial and marital matters as private and personal, not the business of medicine, even if they have health implications. There is an important difference between “taking into account” marital, financial, or religious factors and “taking them on” as the objects of interventions.

Causing Harm A third ethical problem concerns the possibility of actually doing harm. Linking religious activities and better health outcomes can be actively harmful to patients, who already must confront age-old folk wisdom that illness is due to their own moral failure⁽³¹⁾. Within any individual religion, are the more devout adherents “better” people, more deserving of health than others? If evidence showed health advantages of some religious denominations over others, should physicians be guided by this evidence to counsel conversion? Attempts to link religious and spiritual activities to health are reminiscent of the now discredited research suggesting that different ethnic groups show differing levels of moral probity, intelligence, or other measures of social worth⁽³¹⁾. Because all of us, devout or profane, ultimately will succumb to illness, we should avoid the additional burden of guilt for moral failure to those whose physical health fails before our own.

THE AIMS OF RESEARCH ON RELIGION AND HEALTH

Beyond the empirical issues, there is a broader issue to be addressed: given the significant place that religion holds in the US and the substantial ethical issues that arise in connection with religion and health, what, precisely, is the larger objective of studies that seek to examine connections between religious practices and health? In other areas of biomedical research, studies, either epidemiological or experimental, attempt to illuminate underlying pathophysiological mechanisms in a way that leads to development of new treatments. To take the example of studies that repeatedly show relationships between depression and heart disease, the aim of most researchers is to identify the pathophysiological mechanisms so that new interventions can be developed. Such interventions might involve treatment of depression or treating mechanisms in the causal pathway.

If this analogy seems inappropriate, consider that Koenig et al. have commented that if religious beliefs and activities really help the patient to be physically or mentally healthier then “this finding has major implications for our struggling health care system” (p. 5⁽²⁾). Others suggest that by implementing religious practices in medicine, management of chronic disease may be improved⁽³²⁾ and health care costs can be reduced⁽³³⁾. Harris et al. recommend introducing prayer into medical practice⁽³⁴⁾. Larimore et al. encourage physicians to bring religion and spirituality into their clinical practices⁽²¹⁾.

In the case of depression and heart disease, this approach makes sense and the path to therapeutic interventions is plausible. It is far from clear that this is the case with religious involvement and its putative health effects. The analogy to treating depression – treating insufficient religious devotion, were this shown by the epidemiological literature to be associated with poor health – is highly problematic. While physicians can treat depression with pharmacotherapy or psychotherapy, not only is there no parallel treatment for low religious devotion but it would be ethically impermissible for physicians to make recommendations that their patients engage in religious behavior of one sort or another.

Alternatively, again following the case of depression and heart disease, a physician might attempt to intervene at the level of the mechanism in the causal pathway, e.g., enhanced platelet reactivity or reduced autonomic regulation of the cardiovascular system, to reduce the risk associated with heart disease, without addressing the depression at all. In a parallel fashion, if there were solid evidence that religious devotion were associated with lower risk of a specific disease and the intervening mechanisms were clearly understood, a physician might intervene at the level of these mechanisms without making recommendations about religious beliefs or behavior. On the surface, this might be ethically permissible but religious involvement would be reduced to a marker rather than a mechanism.

WHAT TO DO? WHAT NOT TO DO?

Areas of research not worth pursuing

Neuroimaging Studies

Recently, Andrew Newberg, a neurologist at the University of Pennsylvania, has conducted neuroimaging studies of meditation and demonstrated that this practice leads to differences in regional cerebral blood flow. In itself, there is nothing objectionable or, for that matter, very interesting about this finding since there are blood flow differences in the brain that correspond to virtually all human activity including writing about, or reading about, research studies on religion and health.

What *is* objectionable about this is the implication that there is something special the religious experience because it has neurophysiological underpinnings. According to his website, Dr. Newberg claims that neuroscience can elucidate the nature of mystical experiences, their importance in human evolution, and why the abiding need for a concept of God is imperative for the survival of the human species (<http://www.andrewnewberg.com/default.asp>).

Identifying areas of the brain that light up during prayer or meditation has little value and almost certainly is not worth the resources such studies consume. Not only are they wasteful but they trivialize the religious experience by suggesting that it is nothing more than increased activity of a region of the brain.

Assessing the impact of distant, intercessory prayer

As indicated above, most studies of religion, spirituality, and health are observational in nature and as such, cannot control exposure to the religious activities or attitudes thought to be associated with health benefits. In such studies, confounding and self-selection become significant problems.

In contrast to these observational studies, research on the impact of distant, intercessory prayer (IP) permits random assignment to treatment conditions and double blind assessment of outcomes. Unfortunately, these advantages over epidemiological studies of religion and health are more apparent than real. Significant problems characterize all aspects of these studies. At the level of the treatment variable, the inability to understand the characteristics of prayer make it impossible to determine with certainty the degree of exposure to the putative therapeutic agent, a problem which does not exist in randomized controlled trials. At the level of the outcome variables, there is a different type of uncertainty: the inability of IP researchers to specify the outcomes likely to be influenced by IP leads to a shotgun approach that violates standards of statistical analysis. Finally, the absence of a persuasive mechanism linking IP to outcomes has led to assertions about the revolutionary nature of the IP “findings” that are greatly overstated and fail to appreciate the nature of true scientific revolutions.

These limitations demonstrate that further study of IP is not justified.

Studying Mechanistic Pathways

Any theory about how religion and spirituality might influence health should specify the intervening pathways. However, presenting a laundry list of potential physiological mediators, e.g., IL6, elevated BP, increased behavioral sanctions against risk behavior, is not sufficient.

Even a sophisticated list of potential mechanisms is premature. Efforts to understand mechanisms must wait until there is solid evidence of an association between religious involvement and health outcomes. As Powell et al. have demonstrated, only in the case of religious attendance and mortality does the evidence reach this level. And even in this case, as discussed above, serious questions arise.

What is worth pursuing?

The most obvious direction for this literature is to conduct a relatively definitive study of what appears to be the strongest findings to date: the link between attendance at religious services and mortality. As an observational study, such a project can never be free of the potential biases of self-selection and confounding.

But a new, well-designed study can do more to address these concerns than previous studies that have relied on existing databases designed for other purposes entirely. Thus, a study designed from the start to investigate this matter can address the concern about self-presentation bias associated with interview methods that Presser and Stinson⁽⁸⁾ have identified by employing time use estimation. It could address the potential confound associated with a more general inclination to engage in socially productive, community activities suggested by Cohen⁽³⁵⁾. It could address the issue of assessing the differences in the availability of religious and community resources to participants as a way of teasing apart whether reduced mortality is associated with a more general interest in social engagement or a more specific interest in religious involvement. Such a study has another advantage: an unambiguous outcome variable.

CONCLUSIONS

Even a well-conducted study is only a means for understanding the elements of religious involvement that promote health. Because of the substantial ethical problems identified above, it can never be the basis for active introduction of religious activities in clinical medicine.

A parallel activity must address the general aims of such an inquiry and more generally, of studies that attempt to determine the health correlates of religious involvement. Explicit recognition of the limits of such studies, regardless of their findings, is required. These limits pertain to activities of medical clinicians and relate to the bioethical imperatives associated, among other things, with threats to the religious freedom of patients. That is, the principle of patient autonomy requires that clinicians recognize the power of their role as medical expert and assiduously avoid coercive or manipulative actions related to religious activities. This restriction also is related to a recognition that physicians lack expertise in religious matters and that the clinical setting is one in which they are required to limit their attempts to influence patients to the medical matters in which they possess expertise. Recommending an antibiotic for pneumonia is not manipulative in this setting; recommending, either explicitly or implicitly, that a patient attend religious services is. Recognition of the bioethical principle of nonmaleficence requires that in the clinical setting, physicians avoid causing harm.

And although not specifically a bioethical principle, a dose of humility is required. Increasingly, physicians are called upon to consider the importance of spirituality in the lives of their patients. Physicians not only are encouraged to probe deeply into the spiritual lives of their patients but also to function as arbiters of appropriate and inappropriate spiritual beliefs, e.g.,^(21, 23, 36). As such, the roles of physician and clergy become conflated and physicians are asked to become guides to the spiritual lives of their patients, an astonishingly arrogant assumption of responsibilities for which they have no training whatsoever. The bitter irony of this stance is that as physicians choose to spend time exploring matters of spirituality with their patients, an activity for which they have

no qualifications, they will have even less time to attend to basic recommendations about disease prevention, which is not only within their domain of expertise and within the practical limits of their interactions with patients but also is their responsibility.

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