GUEST EDITORIAL

Religion, Aging, and Health: Current Status and Future Prospects

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An impressive body of research indicates that elderly people who are involved in religion tend to enjoy better physical and mental health than older adults who are not as religious (Koenig, 1995; Levin, 1996). The articles in this issue by Idler and Kasl (1997a, 1997b) provide convincing evidence of this point. Their research indicates that increased church attendance is associated with better physical functioning in late life. This work is compelling because it is based on a large-scale random probability sample and a prospective study design.

Idler and Kasl's (1997a, 1997b) research is part of an apparent surge of interest in religion, health, and aging. The purpose of this editorial is to comment on this trend and to speculate on where it may be headed.

Evidence of increased interest in religion and health comes from at least three sources. First, although hard to document, there appears to be an increase in the number of scholarly papers and grant applications concerning this subject submitted to mainstream journals and major funding sources. Second, journals devoted solely to religion and aging have emerged (e.g., the Journal of Religious Gerontology), and special issues in existing journals have recently been devoted to this topic (e.g., Journal of Aging Studies). Third, major funding sources have sponsored committees designed to promote a better understanding of religion and health. For example, I chair a standing committee that is supported by the Fetzer Foundation and the National Institute on Aging (NIA). This group has been charged with developing a comprehensive set of religion measures to be used in surveys of health across the life course. The Templeton Foundation is currently supporting a similar effort.

As Levin (1994) pointed out, the recent “discovery” of religion is a paradox because the topic is certainly not new in sociology and psychology. Durkheim (1915) and James (1902) devoted entire volumes to the subject. Moreover, highly respected specialty journals have been around for quite some time (e.g., Journal for the Scientific Study of Religion). Viewed from this perspective, the recent surge of interest actually represents greater acceptance of religion among those who have typically shied away from the subject. Although the reasons for this are hard to pin down, three factors may be involved. First, as medical costs continue to skyrocket, there appears to be a greater willingness to consider the role played by nontraditional factors in health. Evidence for this may be found in the recent establishment of the Office of Alternative Medicine in the National Institutes of Health. Research on religion and spirituality represents an important focus of the activities in this Office (Dossey, 1993). Second, a growing level of sophistication and quality in studies of religion and health makes the topic harder to ignore. Previously, empirical work on religion and health has been described as “dreadful” (Levin, 1994), but as the articles by Idler and Kasl (1997a, 1997b) reveal, this is changing rapidly. Finally, there is mounting evidence that people may become more religious as they grow older (Futterman and Koenig, 1995). While it is always difficult to disentangle age, period, and cohort effects, data from a series of Gallup surveys suggest that differences between younger and older adults have persisted for nearly 40 years (Futterman and Koenig, 1995).

As we stand poised on a new era of research on religion, it is important to reflect on the course this work may take, and identify problems that should be addressed along the way. The remainder of this editorial is concerned with the challenges facing basic, as well as applied, researchers.

Issues in Basic Research

While the quality of basic social and psychological research on religion and health is improving, we need to do better. In particular, more attention should be devoted to the theories that are devised and the measures used to evaluate them. Many studies demonstrate that there is a statistically significant relationship between various dimensions of religion and health, but we do not have a well-developed and intuitively pleasing sense of why these relationships exist. There are at least four ways to deal with this problem. First, researchers may benefit from revisiting classic theoretical work on religion. In addition to Durkheim (1915), lesser known theoretical papers may provide valuable insight as well. For example, Simmel’s (1905) discussion of social relationships and religion is as relevant today as it was nearly a century ago.
Second, better theoretical perspectives may be developed by assessing whether an explicitly religious framework may add to our understanding of existing perspectives on aging that are cast largely in a secular framework. For example, Erikson's (1959) widely cited, eighth stage of development in the life cycle would appear to lend itself well to this approach (see Fowler, 1981). The same is true of Butler's (1974) research on the life review. At the core of both perspectives is the notion that deriving a sense of meaning and coherence is a key developmental task in late life. Since one purpose of religion is to infuse life with a sense of meaning, it may provide an important way to reach this developmental goal. Similar insights may be obtained by looking for ways to adapt religion to emerging research on primary and secondary control (Schulz and Heckhausen, 1989), as well as Antonovsky's (1987) sense of coherence construct.

Third, we need to develop theories that have a uniquely gerontological focus. All too often, investigators merely test propositions that have been formulated with younger adults in mind. We need to know more about how various dimensions of religion change over the life course. Koenig's (1994) work on a mature religious faith represents a step in the right direction. A mature faith refers to a qualitatively different belief system that is richer, and more deeply infused in an elder's life style, than beliefs held by a younger person. We need to know how a mature faith develops, why it is related to health, and why some elders develop a mature faith while others do not.

Fourth, a causal modeling perspective may help to improve theoretical work on religion. In reading some studies, one is left with the impression that the goal is to find the one dimension of religion that best predicts health. This is typically accomplished by pitting one domain against another in an effort to see which explains the most variance in health. There are better ways to approach the problem. A more useful strategy is to focus on the interrelationships among multiple dimensions of religiousness within a causal modeling framework. In particular, conceptual schemas are needed that link more distal religious constructs (e.g., church attendance) with more proximal religious factors. For example, the following conceptual sequence may provide a useful way of relating church attendance with health: (1) Elders who attend church frequently get more religious support than older adults who do not go to religious services often; (2) one function of religious support is to transmit and reinforce religious coping responses (Pargament et al., 1988); (3) elders who adopt religious coping strategies deal more effectively with stressful life events than older adults who do not rely on religion. Estimating the direct, indirect, and total effects that operate through this sequence should provide a better sense of the process linking religion with health.

Sound measurement goes hand-in-hand with well-articulated theory. Unfortunately, there have been many problems with the measurement of religion in later life. Two are especially troublesome. First, many investigators rely solely on either denominational preference or the frequency of church attendance to assess religion (Sherrill, Larson, and Greenwold, 1993). However, there is much more to religion than this. Researchers must identify the key dimensions of religion and devise high-quality measures for each domain. The Fetzer/NIA committee mentioned above has made important progress in this area. In particular, this group has identified 10 key dimensions of religion, and they are now in the process of testing and developing survey measures to assess each domain.

The second major challenge in the measurement of religion follows directly from one of the theoretical problems discussed above. Many of the measures used in studies of older adults were developed for younger people. Little effort has been made to consider whether they are appropriate for elderly people, especially those from culturally diverse groups (Chatters and Taylor, 1994). For example, there is virtually no research on the measurement of religion among older Muslims or elderly people who practice traditional Eastern religions. As our society becomes more culturally diverse, rigorous qualitative work is needed urgently to fill this gap in the knowledge base.

**Applied Research on Religion**

If religion really has a beneficial effect on health in late life, then the goal of research in this field should ultimately be to inform intervention design. Researchers may find they are in a unique position to do so because, for decades, religious institutions have been a conduit for the delivery of health-related services to elderly people. This assistance has typically taken one of two forms. First, as Conrad (1990) pointed out, religious organizations frequently offer what are largely secular health services, such as nutrition programs or programs designed to promote physical activity. In contrast, they also provide other services that are more explicitly religious in nature. For example, as Worthington and his colleagues indicated, there has been a phenomenal growth in religious counseling services (Worthington, Kurusu, and McCullough, 1996). They reported, for example, that during a two-year period (1993–1995), membership in the American Association of Christian Counselors grew from 2,000 to more than 16,000 people. Given the current trajectory of research on religion and aging, it is likely that investigators will have their greatest opportunity to inform the second type of service (i.e., explicitly religious programs).

The ethical and practical challenges that confront the development of explicitly religious health services are formidable. Church and state are separated by law. Given this legal mandate, it is not clear whether it would be appropriate for NIA to fund research that will be used to promote interventions with explicitly religious content. Problems also arise over how to best deliver this type of service. Should physicians be trained to provide religious counseling or should clergy become a formal part of the treatment team? Would this be possible in state-supported medical facilities? Is it appropriate to question patients about their religious background, and would some be offended if they were offered programs with an explicitly religious content?

This is obviously not the place to address these thorny ethical issues. These matters are best left to medical ethicists. Even so, researchers working in the field must be cognizant of the fact that the work they do may someday con-
tribute to interventions that have an explicitly religious content. However, if religion is a focal point in the lives of many elders, and if it has salubrious effects on health, then it is difficult to see how it can be ignored in social gerontology. The benefits of further work should far outweigh the costs. Few substantive domains hold out the promise of improving the health of our aging population while at the same time reuniting us with our intellectual past.

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