
Richard J. Contrada and Ellen L. Idler
Rutgers, The State University of New Jersey

Tanya M. Goyal
Duke University Medical Center

Corinne Cather
Massachusetts General Hospital and Harvard Medical School

Luba Rafalson
Rutgers, The State University of New Jersey

Tyrone J. Krause
University of Medicine and Dentistry of New Jersey—Robert Wood Johnson Medical School

In this reply to K. E. Freedland’s (2004) comments on R. J. Contrada et al. (2004), it is shown that the statistical issues he raised, and his preferred interpretation of the findings, were adequately addressed in the original article. It is argued that methodological limitations also were fully characterized and do not differ in kind from those of biomedical studies. Other issues discussed include the merits of focusing on distal versus proximal causation, plausibility of explanatory mechanisms for health effects of religious involvement, and potential practical applications that do not require manipulation of religious involvement. The article is concluded by commenting on subtle aspects of discourse that may unnecessarily polarize discussions of possible physical health effects of religious involvement.

Key words: religion, surgery, health, biopsychosocial model

In his commentary, Freedland (2004) has discussed our study (Contrada et al., 2004) of religious involvement and surgical outcomes, with some points extending to the larger literature on religion and health and to biopsychosocial research in general. We find his views quite interesting and are grateful for this opportunity to respond. In doing so, we address Freedland’s substantive points as well as subtler issues concerning philosophy of science and methods of discourse.

Freedland’s overall conclusion—that our findings will have little impact on coronary artery bypass graft (CABG) surgery research—might be viewed as a safe bet rather than a subject for debate. Our study was, after all, a single, small-scale, observational investigation, published in a journal of health psychology, not surgery, and addressed to health psychologists, the vast majority of whom are not even indirectly involved in surgery. However, Freedland brings to this issue considerable expertise and experience, so his arguments and opinions warrant close examination.

One of Freedland’s underlying premises appears to be that religious involvement cannot readily be modified, which, in his view, deprives the findings of practical value. We agree with the premise but not with the implication: Certainly, manipulation of religious beliefs is both practically and ethically problematic, but in this regard, any difference between those beliefs and other psychosocial factors is hard to see. Many variables of interest to health psychologists, such as personality, expectations, and even mood states, not to mention age, gender, race/ethnicity, and marriage, are not so readily altered, when they can be altered at all, yet they are central to health research. When direct manipulation is impossible or unwise, these factors might be put to other uses. For example, measures of religious involvement, along with other psychosocial variables, might help to identify high-risk patients, who could then undergo additional biomedical testing and interventions on factors that can be manipulated (e.g., support, information, and coping skills). In addition, religious involvement might be integrated within interventions without itself being targeted for change. Along with other personal values (e.g., sense of duty to family), it might be recruited to motivate specific behaviors, such as symptom monitoring, use of postoperative treatments, and participation in rehabilitative procedures. This proposition raises interesting questions: Can religious and other personal motives help surgery patients to endure and accept pain and facilitate their mobility, ambulation, and resumption of daily living activities? Are religious patients more attentive to medical staff recommendations? Are they seen as easier to deal with? These questions

Richard J. Contrada and Luba Rafalson, Department of Psychology, Rutgers, The State University of New Jersey; Ellen L. Idler, Department of Sociology, Rutgers, The State University of New Jersey; Tanya M. Goyal, Department of Psychiatry and Behavioral Sciences, Duke University Medical Center, Corinne Cather, Department of Psychiatry, Massachusetts General Hospital, Boston, and Department of Psychiatry, Harvard Medical School; Tyrone J. Krause, Department of Surgery, University of Medicine and Dentistry of New Jersey—Robert Wood Johnson Medical School.

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Correspondence concerning this article should be addressed to Richard J. Contrada, Department of Psychology, Rutgers, The State University of New Jersey, 53 Avenue E, Piscataway, NJ 08854-8040. E-mail: contrada@rci.rutgers.edu
have yet to be adequately addressed. The answers may reveal a number of ways that religion and other psychosocial factors can have practical value for promoting adaptation to major surgery without their being directly manipulable.

It is important to note that Freedland’s projections regarding impact were confined to the findings for religious beliefs. They did not pertain to the null effect for prayer or the apparent negative impact of religious attendance. Nor did he comment on the association of depressive symptoms with greater length of stay (LOS) or on data indicating that effects of religious beliefs and attendance are more pronounced in women than in men. The gender differences are particularly noteworthy in light of recent increases in the proportion of female CABG patients and given a consistent gender difference in post-CABG LOS that has not been explained by biomedical factors or variations between hospitals (Butterworth et al., 2000). As to the null and negative findings, we see them as just as worthy of attention as our positive findings. They may raise useful questions about the disparate effects of different aspects of religion and help to correct public misperceptions and encourage patients to invest in appropriate coping strategies.

The first of four factors that Freedland believes will limit the impact of our findings for religious beliefs concerns statistical analysis. We begin by correcting an erroneous inference some readers might draw from Freedland’s comments, which is that we failed to report LOS analyses that controlled for all biomedical predictors, including postsurgical complications. We did, indeed, report just that analysis (Contrada et al., 2004, p. 229). This regression model, including all predictors, served as the final step in mediational analysis. Thus, when Freedland stated that “adjusting for complications would have eliminated the effect of religious beliefs on LOS” (p. 239), he need not have used the conditional tense. We performed that analysis and the result, as reported in the article, was just as Freedland indicated. We then discussed the conventional interpretation as well as others that involve neither mediation nor any effects of religious beliefs on complications or LOS. Freedland’s assertions notwithstanding, we did not “omit such an important covariate” or “conclude that an effect has been found . . . [when] covariate adjustment abolishes it” (p. 239).

Freedland stated that “because it can transform negative findings into positive ones” (p. 240), medical researchers may view mediational analysis as a form of statistical “alchemy” (p. 240), and he reported that he was unable to locate an application of these techniques in CABG research. Path analysis, the statistical approach from which mediational analysis is derived, was originated by Sewell Wright (1921), a biologist, was later introduced to social sciences by O. D. Duncan (1966), a sociologist, and today is used across a spectrum of disciplines ranging from economics to behavioral genetics. The standard citation for statistical mediational analysis in psychology is Baron and Kenny (1986), an article published almost two decades ago in the Journal of Personality and Social Psychology; in social and health psychology the practice is neither innovative nor controversial. Moreover, key elements of our mediational analysis were used in a recent study in which pre- and postoperative predictors of LOS were entered hierarchically in successive steps of regression analysis rather than into a single, simultaneous model (Rosen et al., 1999), and predictors are commonly entered into logistic regression models in a hierarchical manner in the study of post-CABG atrial fibrillation (AF; e.g., Zaman et al., 2000). So, full-blown path analysis may yet find its way into the regular armamentarium of CABG surgery researchers. As for Freedland’s alchemy metaphor, it is misleading if taken to describe science as being concerned simply with whether findings are positive or negative. Hypothesis testing is a matter of distinguishing among true associations, null associations, false negatives, and false positives. By ignoring mediational pathways that may link multiple predictors to outcomes, one risks (Type II) inferential errors in which discoveries go unnoticed.

The second obstacle discussed by Freedland concerns the use of a complications index and analysis of AF. Here we find some common ground. We certainly would like to have had a sample large enough to permit individual modeling of complications using a more comprehensive set of biomedical and psychosocial predictors, including some complications not observed in our study that are generally low in prevalence but nonetheless important. We agree that generalizability of our findings may be limited by use of a convenience sample and that for these reasons the results should be viewed guardedly. These factors are described in the article, and their probable role as methodological limitations are acknowledged, although it should be noted that methodological implications of small studies can bias toward Type II as well as Type I errors. In any event, these issues are best addressed through replication. Toward that end, our group is currently engaged in a larger study of CABG outcomes that will permit more definitive examination of the role of religion and other psychosocial factors.

The third major point discussed by Freedland concerns explanatory mechanisms. He discounted both psychophysiological and behavioral explanations for our findings. He also dismissed the import of these sorts of mechanisms if they did operate, because they consign religious beliefs to a “distal” role of little relevance to more proximal determinants of surgical outcomes of interest to health care professionals. We agree that mechanism-focused research on religion is at an early stage but disagree with the suggestion that there is no theoretical basis for the existence of mechanisms linking religion to physical health outcomes. We view as well established the general case for psychophysiological and behavioral mechanisms of interplay between social, psychological, and biomedical phenomena (Baum & Poslusny, 1999) that serves as the foundation for biopsychosocial research (Engel, 1977). Religious beliefs and practices and ties to a religious community are quite plausibly viewed as factors that may influence disease-promoting psychophysiological processes (Masters, Hill, & Kircher, 2002; Seeman, Dubin, & Seeman, 2003). And, going beyond mere plausibility, we think available evidence quite clearly and directly documents associations between religious involvement and behavioral mechanisms of interplay between social, psychological, and biomedical phenomena that influence cardiovascular mortality risk, such as smoking (Strawbridge, Cohen, Shema, & Kaplan, 1997). In contrast with studies of intercessory prayer (Byrd, 1988; Harris et al., 1999; see comments by Thomson, 1996), ours was not a study in which there was no scientific mechanism available for the interpretation of positive results.

Regarding Freedland’s discounting of distal as opposed to proximal causation, we see this as a false choice: Explanation of any phenomenon requires identification and integration of both. We believe causal inquiry may profitably begin either with a focus on distal influences, as in the epidemiologic tradition, or with a focus on proximal, mechanistic ones, as in the research on pathogenic processes. Ultimately, the gap between the two must be bridged with evidence regarding explanatory processes (Anderson, 1998),
a point with which we expect Freedland would agree. Viewing the proximal–distal distinction in a somewhat different way, we can vigorously assert, after having completed hundreds of interviews, that the patients for whom religious beliefs and practices are important do not see these aspects of their life as at all distal from the stressful event they are undergoing. Indeed, a primary impetus for the study was our and our physician colleagues’ observations of the importance of religious issues for medical patients. So, in one sense, to say that religious beliefs are distal from surgical outcomes is obvious, even tautological, because their effects must be mediated by biomedical processes. But at the same time, religion may be seen as a prominent, self-selected coping mechanism though which many patients attempt to adapt to the stressful experience of surgery.

Freedland’s fourth concern was that established risk factors did not predict AF or other complications in our study. This, he suggested, raises the question of whether religious beliefs truly predicted outcomes independently of biomedical risk factors and whether our patient sample differs from others. Freedland’s comments here would be misleading if taken to indicate that there is a large number of well-established risk factors for postoperative AF that were uncontrolled in our study. Regarding post-CABG AF, Zaman et al. (2000) commented that “only increased age has consistently been associated with AF after CABG” (p. 1403). A similar conclusion was reached by Hravnak et al. (2001): “Increased age is typically the only patient characteristic consistently identified as a risk factor for new-onset . . . [atrial fibrillation] following . . . [standard] CABG” (p. 1494). In light of these assessments, our inability to identify predictors of AF or of other complications, other than age, is neither surprising nor unusual, especially given modest sample size. Here we reiterate Freedland’s call for further research involving larger, more representative samples modeling complications individually. It follows from points already discussed that we would find further associations linking religion and other psychosocial factors to surgical complications to be of interest and potential practical value even if they were fully mediated by (and therefore not statistically independent of) other, established risk factors.

Freedland commented on secular trends in which scientific advances and policy changes have reduced surgical complications and LOS following CABG. His point, that any benefits of identifying psychosocial factors such as religious beliefs would “pale in comparison” (p. 241) with these developments, might be misinterpreted as an indication that there remains little unexplained variance in short-term CABG surgery outcomes. This is not the case. In their sample of 3,605 Medicare patients who underwent CABG in 1992–1993, Rosen et al. (1999) reported a multiple correlation squared of .14 for a model predicting LOS from a large set of demographic and biomedical factors. Similarly, Peterson et al. (2002) reported a multiple correlation squared of .14 in a multivariate analysis of post-CABG LOS that involved over two dozen predictors and nearly 500,000 patients who underwent surgery in 1997–2001. Thus, we agree that the impact of advances in surgical technique and patient care may exceed further improvements that might come from the study of psychosocial factors. And we agree that the opportunity for psychosocial factors to influence outcomes of CABG surgery is constrained by the effects of factors such as age, surgical procedures, and intensive monitoring and medical management of CABG patients. Nonetheless, we believe there remains considerable opportunity to identify additional risk factors and mechanisms, including possible psychosocial influences, particularly as the CABG population expands to include more women and the elderly and as research increases emphasis on quality-of-life outcomes.

Freedland made additional comments regarding the origins of worthy hypotheses and the bases of researchers’ decisions regarding the topics they investigate. He accurately stated that hypotheses regarding the effects of religious involvement on surgical outcomes “did not spring from the research literature on complications of CABG surgery” (p. 240). We assume this is not to be taken as a general prescription against hypotheses that are generated in one area being applied to another. We became interested in the possible effects of religious involvement on CABG surgery outcomes on review of epidemiologic findings involving all-cause mortality and particularly evidence linking religiousness to circulatory disease deaths, the most prevalent cause of mortality (Hummer, Rogers, Nam, & Ellison, 1999; Kark et al., 1996). As one way to explicate these findings, we initiated research on religion and the course and outcome of coronary disease and its treatment.

Thus, Freedland was correct to infer that the study was driven by interest in the independent variable, as we would expect to be the case for many Health Psychology articles, but the dependent variable was not selected arbitrarily, and of course, both independent and dependent variables require consideration. Our interest is in extending research on health effects of religious involvement, a psychosocial construct, to a new biomedical domain, major surgery. Moreover, our psychosocial interests also extend to choice of outcomes, such that, in ongoing work, we are examining depressive symptoms, quality of life, and other “soft” indicators of relevance to CABG patients. We would not expect the typical surgical research team to take up this same set of questions. Our group is multidisciplinary in representing psychology, psychosocial epidemiology, and surgery, and this is reflected in our selection of research questions and methods. The surgeon on our team was as interested in moving this project forward as were the rest of us; many of today’s younger surgeons are much more than technicians focused on organs and not patients. Surgeons with concerns beyond the surgical suite will combine both medical and psychosocial interests. Our choices surely reflect bias in the sense of preference or inclination in selection of research topic but we hope not in the sense of unfair prejudice in analysis, reporting, or evaluation of findings.

A final set of comments concern some subtle aspects of discourse. Without denying Freedland the latitude in expression that is his right as an author, we see instances in which choice of a word or phrase may have had the unnecessary effect of polarizing readers’ reactions. One is the title of his commentary, which could be taken to frame this as a debate about causal effects or even divine intervention—neither of which were claimed in our article. Another is his broad-brush, self-fulfilling characterization of “most” studies of religion and health as “controversial,” implying that, as a group, studies on this subject fail to meet scientific standards.

Beyond phraseology, Freedland applied evaluative criteria so strictly as to construct straw men. We referred earlier to his projection regarding the small likelihood of a single, modestly sized, observational study having a “durable impact on surgical outcomes research” (p. 245). Elsewhere, Freedland asked whether
“these new findings [will] persuade medical and surgical researchers that religious factors have a major role [italics added] in the outcomes of CABG surgery” (p. 239), even though we mention no such possibility. Later, Freedland described as paradoxical the proposition that religious beliefs might promote health in individuals who eventually require CABG surgery, as though risk factors typically operate in an all-or-none fashion and as if undergoing elective CABG were an ultimate negative health outcome.

To extend the thought with which Freedland began his commentary, we expect that readers with strong positions pro or con regarding possible health effects of religion will find support in our study, because we report mixed results, and a reader’s preexisting beliefs may influence attention and interpretation. The title of Freedland’s commentary reminds us of Albert Einstein’s (1930) observation that “the mysterious . . . is the source of all true art and science” (p. 194). Unexplained mysteries are and should be a magnet for research, whether the solution has an immediate practical application or not. It is often difficult to anticipate future applications of research not directed at intervention, a view expressed by Neal E. Miller (1992). Although he was a champion of science aimed at improving the human condition, Miller nonetheless argued against an exclusive, application-centered focus “concentrating all research on the immediately obvious goals” (p. 849). It is the usual practice of commentators to encourage more research on the subject of a new study, but some recommend a specific conceptual or methodological course correction, or even try to justify a moratorium. On this, Freedland appears to be of two minds. He found merit in our “well-designed, methodologically sophisticated study of an interesting biopsychosocial phenomenon” (p. 239) and encouraged efforts to see whether our findings can be “replicated in representative samples” (p. 241). But elsewhere, both in content and tone, he appears to be calling for less research on the health effects of religion. Our view is that this research question is worth asking and that the answers will be worth knowing.

References


