Most reviewers have covered a highly heterogeneous group of studies, including studies conducted on nonhuman targets, on human subjects aware of the intended intervention, and on human subjects kept blind as to whether or not they were being treated. Abbot (2000), for example, reviewed 22 studies involving randomized clinical trials, which were almost evenly split between distant healing studies and studies involving contact between the healer and the patient. The results of the studies were also almost evenly split, with 10 showing a significant positive effect (five of them involving distant-healing and five direct contact). Moreover, there did not seem to be a relationship between the methodological quality of the study and the results. Because of these mixed results, and because of the "significant heterogeneity" in the studies with regard to healing method used, medical condition treated, outcome measure, and control intervention, Abbot decided—as have most other reviewers and researchers—that no firm conclusion can yet be drawn, but that there has been enough positive evidence from studies of good quality to warrant further and better research on faith healing.[11]

11. I mentioned earlier that few medical journals would publish results of faith or spiritual healing studies, regardless of their quality, until recent years. When the prestigious *JAMA (Journal of the American Medical Association)* finally broke this ban in its pages, it did so in an unusual and revealing way. Specifically, in 1998 the editors of *JAMA* published a science-fair project of a 9-year-old 4th-grader who herself had designed and carried out an experiment to test the practice of Therapeutic Touch (TT), and in particular "whether TT practitioners can actually perceive a 'human energy field'" (Rosa, Rosa, Sarner, & Barrett, 1998, p. 1005). Finding that the "practitioners were unable to detect the investigator's 'energy fields,'" the authors concluded that this study provided "unrefuted [sic] evidence that the claims of TT are groundless and that further professional use is unjustified" (p. 1005).

However, as 12 letters to the editor about this paper unanimously pointed out, such a sweeping conclusion was premature, biased, and irresponsible. One clinician summed up the study as "simpleminded, methodologically flawed, and irrelevant" (Freinkel, 1998, p. 1905). Several others described some of its serious methodological flaws. Others recognized that the authors failed to make the important distinction between the efficacy of the method and the theoretical underpinning proposed by practitioners: "The definitive test of a healing practice is whether healing takes place, not whether the practitioners have a flawless grasp of the natural forces at work" (Lee, 1998, p. 1905). It is remarkable—but unfortunately not uncommon—that the editors of this major journal would publish a paper that sweepingly dismisses a whole complex and controversial phenomenon solely on the basis of one small and highly flawed study—especially since they have not, to my knowledge, published a review or research paper with more moderate and reliable conclusions. One can only conclude that this affair reflects a deep-seated bias on the part of the editors, where "one would expect medical professionals to be more concerned with whether real healing takes place" (Lee, 1998, p. 1906). It is not difficult to guess what would have been the fate of a paper submitted by similar authors, with similarly flawed methodology and conclusions, if the results had been positive and not in accord with editorial bias. For more responsible reviews of TT, see Astin, Harkness, and Ernst (2000), Peters (1999), and Wardell and Weymouth (2004).