

The MANTRA II study

We applaud Mitchell Krucoff and colleagues (July 16, p 211)¹ and *The Lancet* editors² for publishing an extensive, well controlled, multisite study on prayer and healing. The Article and accompanying Editorial note that prayer had no significant effect on outcome in 748 heart patients.^{1,2} Study of the intangible is challenging, and the researchers should be recognised for their insight and courage.

However, the results and discussion sections omit one point that could substantially alter the conclusions. The problem derives from the use of inferential statistics and hypothesis testing. In experimental studies, outcome measures between groups are compared; if the differences are significant, the null hypothesis is rejected and the alternative hypothesis tentatively accepted: the intervention is effective.³⁻⁵ The researcher must statistically test whether the results could have been obtained by chance.⁴ The α value is usually set at 0.05: could this result occur by chance less than five out of every 100 tries?^{3,5}

In the prayer study, the null hypothesis would be: "prayer has no effect on healing". If significant differences are found, we reject the null. But what if the difference is not significant? The null hypothesis has not been shown to be true. The researcher should fail to reject the null, and conclude that all possibilities remain.^{3,5} But Krucoff and colleagues did something different: they accepted the null hypothesis, concluding that prayer had no significant effect. They omitted the conclusion: "therefore, all possibilities remain".

This study was not designed to show that prayer has no effect on healing in cardiac patients. The finding of significance required simultaneous alignment of numerous assumptions for an effect to be found. For example: is there a placebo or Hawthorne effect from participation in a prayer study? Did those praying know how to pray effectively, and receive enough information to con-

nect the patient with a source of transcendence? Does researcher commitment to the study of prayer result in altered outcomes? Are there differences among types of prayer (eg, is emotional, heartfelt prayer overstimulating and detrimental to cardiac patients)? Is prayer so pervasive among cardiac staff and patients that a non-prayer condition is not discernable from a treatment condition? Do cardiac medical interventions have enough variability in outcome to measure such a subtle intervention? Is prayer clinically significant only within the outcome measure (eg, could intervention of a transcendent power result in death)? Lack of significant differences could have resulted from any of these, each of which might have confounded Krucoff and colleagues' conclusions.

Let us, as pioneers of science, continue to search for new ways to explore and understand the full gamut of human experience. Caution is indicated as we attempt to use the yardstick of statistics to measure the expression of infinity.

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- 1 Krucoff MW, Crater SW, Gallup D, et al. Music, imagery, touch, and prayer as adjuncts to interventional cardiac care: the Monitoring and Actualisation of Noetic Trainings (MANTRA) II randomised study. *Lancet* 2005; **366**: 211-17.
- 2 The Lancet. Mantra II: measuring the unmeasurable? *Lancet* 2005; **366**: 178.
- 3 Kerlinger FN, Lee FN. Foundations of behavioral research, 4th edn. Fort Worth: Harcourt College Publishers, 2000.
- 4 Onwuegbuzie AJ, Levin JR. Without supporting statistical evidence, where would reported measures of substantive importance lead? To no good effect. *J Mod Appl Stat Meth* 2003; **2**: 133-51.
- 5 Shadish WR, Cook TD, Campbell DT. Experimental and quasi-experimental designs for generalized causal inference. Boston: Houghton Mifflin, 2002.

The almost uniformly negative findings from the MANTRA II study, reported by Mitchell Krucoff and colleagues,¹ are far less surprising than the emphasis of

the report and accompanying Editorial.² In MANTRA II, 748 patients with coronary artery disease undergoing percutaneous coronary intervention or elective catheterisation were randomly assigned distant intercessory prayer (IP); combined music, imagery, and touch (MIT) therapy; IP plus MIT; or standard care.

Despite the entirely negative findings for the IP intervention, both the paper and Editorial enthusiastically speculate about prayer to the virtual exclusion of an examination of the one promising outcome: the reduced rate of mortality in the MIT group at 6-month follow-up. Although this finding—the only one among a great many comparisons to achieve significance—is likely to be due to chance, it still could be explored. But almost all of the discussion section of the paper is about distant prayer.

This is all the more surprising because no biologically plausible mechanism exists for distant prayer, from intercessors around the world and at substantial distances from the patients, whereas a plausible mechanism might be advanced for MIT treatment delivered at the bedside. Indeed, some studies have suggested that interventions that promote arousal reduction³ or distraction⁴ can have physiological effects. However, nothing in our contemporary scientific views of the universe or consciousness can account for how the "healing intentions" or prayers of distant intercessors could possibly influence the wellbeing of patients even nearby let alone at a great distance. These differences indicate the inappropriateness of conflating prayer and MIT.

Krucoff and colleagues and *The Lancet* question whether the prayers of different religious denominations might have different, and superior, effects. It is hard to imagine a more troubling recommendation. Even if there were a plausible mechanism to account for possible effects of distant prayer, do we really want to test denominational differences in its effi-

