

Harris et al have convincing arguments favoring their interpretation of their data over mine. They might point to the fact that more people believe in prayer than in my clairvoyant and telepathic powers. There were times, however, that everyone believed that the earth was flat, and everyone was wrong. Which will it be in this study—prayer, telepathy, or a summary statistic of uncertain validity? I am willing to reveal that I will settle for chance.

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1. Harris WS, Gowda M, Kolb JW, et al. A randomized, controlled trial of the effects of remote, intercessory prayer on outcomes in patients admitted to the coronary care unit. *Arch Intern Med.* 1999;159:2273-2278.
2. Hays WL. *Statistics.* 2nd ed. London, England: Holt Rinehart & Winston Inc; 1973.

P Value Out of Control

As suggested by Harris et al,¹ effective remote, intercessory prayer could be explained by one of two mechanisms. It might represent a miracle: the intervention of God in the physical world by a supernatural force in ways that are incompatible with natural law. It might also represent a form of telekinesis: the movement (healing) of an object (human body) at a distance (remotely) with thought or will (prayer) by an unknown natural force. Miracle or telekinesis has never been shown to exist by credible, replicable scientific experimentation.

Harris et al state that their purpose is not to speculate on mechanisms, but rather to convey results. This approach seems to miss the heart of the issue. It is the very improbability of the mechanism that raises doubts concerning the validity of the results. Goodman² has cautioned against overreliance on *P* values in assessing the efficacy of studies. He emphasizes that *P* values must be evaluated within the context of the prestudy probability of efficacy. For years, skeptics have warned that extraordinary claims require extraordinary proof. This is another way of stating Goodman's theme that results that are inconsistent with a well-validated scientific precedent (low prestudy probability of efficacy) require a higher burden of proof (lower *P* value). Within this context, the study of Harris et al actually suggests that remote, intercessory prayer has no effect on outcome.

Harris et al draw an analogy between their study and James Lind's scurvy trials. If Lind's studies had been subjected to statistical analysis, I suggest that the *P* value would have been far more impressive. Such a *P* value would have probably justified a reevaluation of the then current theories regarding the mechanism of scurvy. However, Harris et al are not merely testing the efficacy of a medication. On the basis of a *P* value of .04, Harris and his colleagues are suggesting the need to reassess 500 years of scientific advancement in our understanding of how the physical world is organized.

As science has advanced, we have actually become more confident that the earth is round, that lemons cure scurvy, that no miraculous forces suspend natural law, and that unknown forces do not move objects from a dis-

tance. Rather than doubting the fundamental nature of the scientific worldview, shouldn't we be questioning the meaning of a *P* value of .04? Is it not more likely that the results of the study conducted by Harris et al have occurred by chance (1 in 25) or by bias rather than postulating a mechanism that requires a seminal paradigm shift in physics? Do not their results suggest the need to reassess our statistical methods for judging efficacy rather than the need to reassess the fundamental theories of science?

The study by Harris et al is a wonderful example of a *P* value out of context and out of control. It is out of context because of the failure to properly adjust for mechanistic improbabilities. It is out of control because of its propensity to encourage much pseudoscientific mischief.

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1. Harris WS, Gowda M, Kolb JW, et al. A randomized, controlled trial of the effects of remote, intercessory prayer on outcomes in patients admitted to the coronary care unit. *Arch Intern Med.* 1999;159:2273-2278.
2. Goodman SN. Toward evidence-based medical statistics, 2: the Bayes factor. *Ann Intern Med.* 1999;130:1005-1013.

No Effect of Intercessory Prayer Has Been Proven

In the recent article by Harris et al,¹ the effects of remote, intercessory prayer on the medical course of patients in the coronary care unit (CCU) had borderline statistical significance at best. Of 40 measures (35 Mid America Heart Institute–Cardiac Care Unit [MAHI-CCU] score components, the weighted and unweighted overall MAHI-CCU scores, length of CCU stay, length of hospital stay, and Byrd score), 2 were significant ($P < .05$). One in 20 is classically what one would expect to be significant by chance; the 2 significant measures reported by Harris et al were the overall MAHI-CCU scores—essentially the same thing.

Statistical significance is not the only way to look at the value of a treatment, however. One can calculate the effect or the number of people one would need to pray for to produce an improvement. It is appropriate, of course, to keep in mind the confidence interval (CI) of these estimates. The unweighted MAHI-CCU score counted the patients' treatments and new diagnoses. With an estimated difference of 0.30 fewer such events for patients in the prayer group (2.7 vs 3.0), the number needed to treat is 3.33. One would have to pray for 3.33 CCU patients to prevent 1 such event (95% CI, 1.7-41.3) or for 10 patients to produce an event-free course (95% CI, 5.2-123.8). Concerns about capitalizing on chance might lead us to acknowledge a wider CI. If we adjust our α value by the Bonferroni procedure (divide the α level selected by the number of measures tested [$.05/40 = 0.00125$]), then the 99.875% CI for the differences is -0.16 to 0.76 , which corresponds to a CI for the number needed to treat of 1.3 to -6.4 .² That is, it is possible that an adverse CCU event may be prevented for every 1.3 people prayed for; on the other hand, it is also pos-