God in the CCU?

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The day of publication of the July 1988 issue of the *Southern Medical Journal* must have been one Hell of a busy news day, literally. Apparently none of the major media in the world could manage to find room to include this little item: a scientific study, published that day in that journal, indicates that God exists, and that he interceded in the recovery of a group of coronary care unit patients! Both Paul Harvey and Charles Osgood publicized this study in their radio commentaries in early 1989, so, despite the delay, word of this startling development has by now reached the heartland of America.

In his article entitled "Positive Therapeutic Effects of Intercessory Prayer in a Coronary Care Unit Population," Randolph C. Byrd, M.D., a San Francisco cardiologist, endeavored to answer these questions: (1) Does intercessory prayer (IP) to the Judeo-Christian God have any effect on a CCU patient's medical condition and recovery? (2) How are these effects manifested, if present?

The study took place between August 1982 and May 1983, when 393 patients signed informed-consent papers upon admission to the San Francisco General Hospital CCU, and were entered into a prospective double-blind, randomized study. (The remaining fifty-seven patients admitted during this period cited various reasons for refusing to participate.) A computer-generated list randomly assigned patients to either the IP group or the control group, and neither they, nor the CCU doctors and staff, nor Randolf Byrd were aware of which patients were assigned to which group.

Intercessors chosen to pray for the IP-group patients were " 'born again' Christians (according to the Gospel of John 3:3) with an active Christian life as manifested by daily devotional prayer and active Christian fellowship with a local church." Each IP patient "was assigned to three to seven intercessors. . . . The [IP] was done outside of the hospital daily until the patient was discharged . . . each intercessor was asked to pray daily for a rapid recovery and for prevention of complications and death."

The IP group consisted of 192 patients, and the control group of 201. Analyses revealed no significant statistical differences between the health of the two groups upon admission. "Thus it was concluded that the two groups were statistically inseparable and that results from the analysis of the effects of [IP] would be valid." The mean age of the IP patients was two years younger than that of the control patients, a difference deemed statistically insignificant.

Each patient's hospital course was given a severity score of "good," intermediate," or "bad," based upon the degree of morbidity experienced by the patient. In addition, twenty-six categories of "New Problems, Diagnoses, and Therapeutic Events After Entry" were measured, and tested for statistical significance between the groups. These included such things as congestive heart failure, diuretics, hypotension, intubation/ventilation, pneumonia, and so on.

The results of the study, as reported by Byrd, employing "multivariant [*sic*] analysis of the data using [these twenty-six] variables . . . revealed a significant difference (P less than .0001) between the two groups based on events that occurred after entry into the study. Fewer patients in the prayer group required ventilatory support, antibiotics, or duretics."[1] In addition, using the "good/intermediate/bad" severity score, "A bad hospital course was observed in 14% of the prayer group vs. 22% of the controls . . . chi-square analysis of these data gave a P value of less than .01" (that is, a less than 1 percent probability that chance alone could account for the difference).

In his introductory abstract, which was also reproduced in the "Domestic Abstracts" section of the *Journal of the American Medical Association* on January 20, 1989, Byrd concludes that the "data suggest that [IP] . . . has a beneficial therapeutic effect in patients admitted to a CCU." In the final paragraph of his article, Byrd says, "based on test data there *seemed* to be an effect [from IP], and that effect was *presumed* to be beneficial" (emphasis added).

But what are those of lesser faith—or of other faiths—to make of this miraculous claim for the efficacy of prayer? Has the Judeo-Christian God been shown to exist, and to intervene in the hospital course of patients?

The most striking flaw in this study's methodology is one forthrightly acknowledged by Byrd. "It was assumed that some of the patients in both groups would be prayed for by the people not associated with the study; this was not controlled for. . . . Therefore, 'pure' groups were not attained in this study." In other words, the focus of the study—prayer—was "not controlled for," except that three to seven intercessors were assigned to pray daily for each patient in the IP group, and none were assigned to the controls. Thus, although unlikely, it is nevertheless theoretically possible that the control group received as many prayers as did the IP group, if not more.

If "intercessory prayer" was not controlled, except that each IP patient was assumed to have received somewhere between X+3 and X+7 prayers daily, as opposed to X+0 for the control patients, what are we to conclude? That God is conditioned in a Pavlovian manner to automatically respond to the side with the greater number of troops, even though the assigned intercessors had no emotional ties to their patients, and even though the IP patients were otherwise no more worthy of healing as a group than were the controls? Does God not know that the side with fewer troops is in just as much need of assistance? Where is the evidence of his omniscience and compassion?

And what can be said about the evidence for God's omnipotence? It is true, assuming that Byrd's data are valid, that in the IP group, 5 percent fewer patients needed diuretics, 7 percent fewer needed antibiotics, 6 percent fewer needed respiratory intubation and/or ventilation, 6 percent fewer developed pneumonia, and 5 percent fewer suffered cardiopulmonary arrest. But no significant differences were found among the other twenty categories, including mortality, despite explicit prayers "for prevention of . . . death." And, reports Byrd, "Even though for [the six seemingly significant] variables the P values were less than .05, *they could not be considered statistically significant* because of the large number of variables examined. I used two methods to overcome this statistical limitation . . . [the] severity score, and multivariant [*sic*] analysis" (emphasis added).

But was this lack of significance truly "overcome"? One must note the interrelationships among these six categories: for instance, the development of congestive heart failure automatically leads to the need

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for diuretics; the development of pneumonia automatically requires the use of antibiotics; and the development of either would likely increase the risk of developing the other, of requiring intubation or ventilation, and of suffering cardiopulmonary arrest. Thus, the development of any single complication may automatically lead to a cascade of other complications and therapeutic interventions that cannot be considered independent events, rendering the significance of Byrd's data highly doubtful.

In addition to the twenty-six categories previously described, three further variables were tracked during the study and tested for significance: "Days in CCU after entry," "Days in hospital after entry," and "Number of discharge medications." *No* significant differences between the prayer and control groups were found, despite explicit prayers for "a rapid recovery." Are we thus to conclude from all of the data derived in this study that although God *may* reflexively respond to the will of the majority, his manifestations are so marginal as to approach insignificance?

Consider a hypothetical study (containing similarly flawed methodology) allegedly demonstrating the beneficial effects of reading periodicals on the course of CCU patients: Patients in the test group are given anywhere from three to seven periodicals daily by people associated with the study; patients in the control group are given none. (Okay so far.) Patients in both groups are allowed to have family and friends bring them periodicals, in a manner "not controlled for." Differences of several percent in six interrelated categories are noted (comparable to the "significance" of Byrd's data), with no significance found in twenty-three other variables measured. I cannot imagine such an article surviving the rigorous screening process of any authoritative medical journal.

The religious nature of Byrd's hypothesis may have been the attraction for the *Southern Medical Journal,* which is published in Birmingham, Alabama, in the heart of the Bible Belt. I assumed that the five-year gap between his study's conclusion (1983) and its publication indicates that a number of other journals had been approached prior to *SMJ,* but had failed to appreciate the historic nature of Byrd's alleged findings. Byrd graciously responded to my inquiry on this point, informing me that he had received two prior rejections, which he called "the academic average."

Perhaps the other two journals subscribe to the generally accepted axiom of science that extraordinary claims (particularly miraculous ones) require proportionately extraordinary proof. This is not to say that studies purporting to demonstrate evidence of supernatural events ought not be published, as long as a journal's minimum standards of acceptability are met. *Nature* has published several such studies, but has historically accompanied them with statements expressing editorial reservations.[2] In contrast, Byrd's *SMJ* article was accompanied by a "commentary" entitled "Religion in Healing," whose author says, "The paper by Dr. Byrd answers a question that has long been asked: Does prayer make a difference? His data say that it does."[3]

Three previous scientific/medical studies on the efficacy of prayer were briefly reviewed in Byrd's paper. We are informed that Galton's 1872 article, one of the first on record, on "found no salutory effects."[4] In Joyce and Welldon's 1965 study of rheumatics, the prayer group fared better in the first half, "but in the second half *the control group* did better" (emphasis added).[5] And in 1969, Collip's findings regarding prayer and leukemia "did not reach significance."[6]

Byrd obviously believes that his study has succeeded where others have failed. But are the data obtained in his study—in which prayer was admittedly "not controlled for"—sufficient to suggest the existence of the omnicient, omnipotent Judeo-Christian God, and the efficacy of intercessory prayer on CCU patients? Or is it much more likely that what we have here is akin to the findings of the Shroud of Turin Research Project (STURP), in which scientists blinded by faith concluded, erroneously, that the shroud was authentic? In his report, Byrd notes that "How God acted in this situation is unknown." But I suspect it was with smoke and mirrors.

Notes

1. The correct term is "multivariate."
2. See, for instance, Targ and Puthoff's paper on "remote viewing," 251:602–7, 1974, and Benveniste et al. on "high dilution," 333:816–818, 1988.
3. The author of the commentary also cited several books and articles that have reported positive effects of faith and personal prayer (as distinguished from intercessory prayer) upon conditions that clearly have large psychosomatic components. Also appearing in the same issue was a study of the beneficial effects of faith in dealing with stress of traumatic injury.
4. Galton, F. "Statistical inquiries into the efficacy of prayer," *Fortnightly Review,* 12:125–135, 1872.
5. Joyce, C. R. B., and R. M. C. Welldon, "The efficacy of prayer: A double-blind clinical trial," *Journal of Chronic Disease,* 18:367–377, 1965.
6. Collipp, P. J. "The efficacy of prayer: Triple blind study." *Medical Times,* 97:201–204, 1969. ●

Author's corrections of publisher's errors:

1) Phrase on p. 44, column 3, paragraph 2 should read:

"prayed for by people not associated with the study."

2) Sentence on p. 44, col. 3, par. 4 should read:

"It is true, assuming that Byrd's data are valid, that in the IP group, 5 percent fewer patients needed diuretics, 7 percent fewer needed antibiotics, 6 percent fewer needed respiratory intubation and/or ventilation, 6 percent fewer developed congestive heart failure, 5 percent fewer developed pneumonia, and 5 percent fewer suffered cardiopulmonary arrest."

3) Sentence on p. 45, col. 2, par. 2 should read:

"We are informed that Galton's 1872 article, one of the first on record on 'the effects of prayer in the clergy, found no salutory effects.'"