therefore, I cannot comment on the possible effects of the dose revision.

In summary, the new dose estimates—if correct—do *not* strengthen the argument that there is "no" safe "level of exposure to radiation."

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References and Notes

1. The estimated mortality rates for leukemia and AMN in the 0 rad dose group are $4.4 \times 10^{-5} \pm 0.8 \times 10^{-5}$ and $2.3 \times 10^{-3} \pm 0.06 \times 10^{-3}$ per person-year respectively. The same rates for the 1- to 9-rad dose group are $4.8 \times 10^{-5} \pm 1.3 \times 10^{-5}$ and $2.17 \times 10^{-3} \pm 0.09 \times 10^{-3}$ per person-year. This calculation assumes a rate constant in time. The estimated crude probabilities for these end points at a dose of 0 rad are 28/29,943 = $9.0 \times 10^{-4} \pm 2.0 \times 10^{-4}$ and 1,460/29,943 = $4.9 \times 10^{-2} \pm 0.1 \times 10^{-2}$. The same probabilities for the 1- to 9-rad dose group are 14/13,796 = $1.0 \times 10^{-3} \pm 0.3 \times 10^{-3}$ and 634/13,796 = $4.6 \times 10^{-2} \pm 0.2 \times 10^{-2}$. The corresponding net probability estimates for leukemia were presented by P. G. Groer, T. Ishimaru, M. Ichimaru, Y. Yasunaga, and J. Brodsky at the Radiation Research Society Meeting, Minneapolis, Minn., in June 1981. Net probability estimates for AMN can be obtained by a life-table analysis. The data are from G. W. Beebe, H. Kato, C. E. Land, *Life Span Study Report 8* (TRI-77, Radiation Effects Research Foundation, Hiroshima, Japan, 1977). Errors are expressed as ± 1 standard deviation.
2. W. E. Loewe and E. Mendelsohn, *Health Phys.*, in press.
3. The estimated mortality rates for leukemia in the 0.1- to 9-, and 10- to 49-rad T65 dose groups are $7.1 \times 10^{-5} \pm 2.7 \times 10^{-5}$, $7.4 \times 10^{-5} \pm 2.0 \times 10^{-5}$, and $5.1 \times 10^{-5} \pm 2.0 \times 10^{-5}$, respectively. The estimated crude probabilities of dying from leukemia for the same groups are 7/4004 = $1.7 \times 10^{-3} \pm 0.6 \times 10^{-3}$, 13/7140 = $1.8 \times 10^{-3} \pm 0.5 \times 10^{-3}$, and $1.2 \times 10^{-3} \pm 0.5 \times 10^{-3}$. The results for the corresponding net probability estimates were presented in the paper cited in (*1*). All estimates for leukemia in Nagasaki use data which incorporate the changes in group sizes due to the relocation of the hypocenter in this city.

Boojums

For the first appearance of the term "boojum" (Research News, 19 June, p. 1378), and for that matter, "snark," in scientific literature (well, social science), please refer to the presidential address given by Frank Beach to the Division of Experimental Psychology, American Psychological Association, in 1949. The title of that speech was "The snark was a boojum." The speech was reprinted in, among other publications, *Readings in Animal Behavior* (T. McGill, Ed., Holt, Rinehart and Winston, New York, 1965).

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Mermin need not worry about Anderson's getting credit for introducing the term "boojum" into the scientific literature. Priority goes to noted psychologist Frank Beach, whose article "The snark was a boojum" appeared in the *American Psychologist* in 1950 (vol. 5, p. 115). Beach cautioned American comparative psychologists against excessive reliance on white rats as their subject of study, lest in virtually exclusively hunting that snark, their own research specialty would "softly and suddenly vanish away." Beach may well have been right. According to the June 1981 issue of the *American Psychologist*, there is a serious move afoot to change the title of the major American outlet for the kind of work to which Beach himself so eminently contributed—the *Journal of Comparative and Physiological Psychology*. The proposed new title eliminates "comparative" entirely and substitutes modern synonyms for "physiological." Although in Beach's use, boojum was not a scientific construct, as it seems to be in Mermin's, Beach should be acknowledged for finding in Lewis Carroll's boojum an apt metaphor for the state of an entire scientific discipline.

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With reference to the article "Let us now praise famous boojums," I would like to point out that boojum has been the common name for *Idria columnaris* Kellogg since 1922 (*1*). The boojum has been described as a bizarre plant found primarily in Baja California, and the term boojum has been given as the common name for *Idria columnaris* Kellogg in at least two publications in scientific journals (*2*). It would appear that physicists are not the only scientists who read Lewis Carroll.

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References

1. R. R. Humphrey, *The Boojum and Its Home* (Univ. of Arizona Press, Tucson, 1974).
2. ———, *Cact. Succ. J.* **42**, 209 (1970); J. Henrickson, *Aliso* **7**, 439 (1972).

Effectiveness of Psychotherapy

I fear that Dawes' perfectly reasonable point (Letters, 29 May, p. 986) regarding the skill of the therapist as a factor in the experimental interaction between psychotherapeutic treatment and patient will obscure the point Strupp was making in the passage quoted by Kolata (Research News, 24 April, p. 432). In the passage quoted, Strupp was warning against too great a reliance on the experimental design applicable to the evalua-

tion of drug treatments, where clear knowledge of the ingredients of the treatment are usually available beforehand.

One of the important features of the National Institute of Mental Health program is that it starts with two psychological treatments of depression that are more explicitly specified than most. Yet embedded in these specifications are approximately 20 hours of interaction between two persons in which there is great room for variations according to specific characteristics of patient and therapist and the surrounding circumstances. Not only that, but the treatments constitute a roughly sequenced package of therapist interventions, such as tasks, explanations, and inferences. Prior research has encouraged the belief that these interventions provide positive treatment effects and justify the ambitious follow-up program. It is important to emphasize that this program includes plans to search for evidence that all the explicit components of the treatment are important to the changes sought and that inadvertent accompaniments are not major factors.

From all the controversy over the effectiveness of psychotherapy, it should be clear that, even given that the evidence is positive, the effects are not great or certain. This tells us that whatever knowledge we have is very crude and inexact. Our treatment packages may contain actions that prove to be useless rituals or, even worse, rituals that block or undo the very effects we seek. Research designs should be directed beyond the goal of evaluation of any treatment package toward the kind of understanding that can provide a basis for increasing the power, certainty, and safety of psychotherapeutic treatments. The dangers in the analogy to chemotherapies are that it fosters unrealistic public expectations and inappropriate research questions and designs.

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Correction

In an article titled "How safe is Bendectin?" (31 Oct. 1980, p. 518), it was incorrectly reported that William McBride of Sydney, Australia, was paid $5000 a day to testify as an expert witness in a court case involving allegations that Bendectin caused birth defects in a Florida child named David Mekdeci. McBride was not paid for certain testimony. Rather, he was compensated for time away from his Australian practice at a rate of approximately $1116 a day so that he could appear as an expert witness on behalf of the Mekdeci family. He was also reimbursed for his travel expenses to and from Australia. *Science* regrets the error.