Letters

Hatch on NCI Hearings

After reading Marjorie Sun's article (News and Comment, 19 June, p. 1366) on the 2 June oversight hearing by the Senate Labor and Human Resources Committee on the National Cancer Institute (NCI), I felt she had covered a different hearing from the one I chaired. To correct misconceptions among readers who may judge by that article, I offer these facts.

The article says the hearing covered old ground, "citing cases of contract abuse that have already been well investigated by the government." What was omitted is as follows:

Since 1978, the General Accounting Office (GAO) and the Inspector General of the Department of Health and Human Services (HHS) have each produced three reports, detailing general practices and specific instances where NCI contracting procedures were slipshod and wasteful. Despite having such findings consistently confirmed, implementation of recommended reforms by NCI was found lacking by HHS and GAO. Impressive-sounding changes were outlined repeatedly but not implemented, a commonly encountered federal syndrome familiar to Washington reporters. Because we ascertained that little had been done, the committee highlighted its evidence to guarantee vigorous NCI efforts to implement substantive change promptly.

Sun's article touches briefly upon the roles of the Eppley Institute and Tracor-Jitco Inc. in the bioassay program. Omitted is the following: $1.1 million in federal funds was to be recovered by NCI through civil action; NCI had not made serious efforts to do so over several years. With Tracor-Jitco, the article ignored hard information that a $64 million program was in serious disarray, had been so for some time, and that NCI was doing nothing. Our original lead even came from a *Science* article ["NCI bioassays yield a trail of blunders" (News and Comment, 22 June 1979, p. 1287)].

Regarding the Marc Straus case, the article ignores vital committee data:

1) When Straus was asked to resign from Boston University Hospital, NCI was fully and promptly informed, yet no investigation was ordered for 2 years.

2) A memorandum of understanding between the Food and Drug Administration (FDA) and NCI calling for prompt, specific notification of any drug situation, such as emerged in the Straus case, was violated. NCI did not inform FDA in a timely manner, to FDA's dismay.

3) NCI's central operating principle is peer review. The director did not inform the appropriate peer review group about the Straus case when it considered Straus's ultimately successful attempt to reenter NCI's grant mainstream.

4) Only when the Boston *Globe* publicized the case in July 1980 was a federal inquiry hurriedly ordered.

The committee is concerned not only about waste of federal funds but about medical ethics. We are dealing with people, not laboratory mice.

Sun argues that this is just one case. We aired other NCI conflict-of-interest situations at the hearing, but they were not included in the article. One involved a physician playing a key role in approving a large grant to an organization with which he had strong professional ties. Another involved a physician working on approval of an NCI grant. Once approved, he applied for that grant, amounting to several million dollars. We had much more.

Each case was amply documented because we operate on a rule of two sources. I hope these added facts will reach *Science* readers to correct any misconceptions left by the original article.

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A-Bomb Radiation Doses

The article on the revised dose estimates for the atomic bomb survivors (News and Comment, 22 May, p. 900) gives an interesting account of the recent efforts to solve this important and complex dosimetry problem. I disagree, however, with the opinions of some experts quoted in the article that the revised dose estimates for Hiroshima and Nagasaki—if correct—will increase low-dose risk estimates for gamma radiation. My reasons are the following:

1) There is *no* statistically significant difference in the estimated mortality rate and the crude or net probability of dying from leukemia or all malignant neoplasms (AMN) between the Hiroshima dose group with total T65 doses of from 1 to 9 rads (all ages at the time of the bombing and both sexes) and the 0 rad dose group at *any time* after 1950 (*1*).

2) In Hiroshima, the revised gamma-ray doses in rads are greater than the T65 gamma doses by between 30 percent to 100 percent, depending on the dose range (*2*). All "new" total dose estimates are also higher (*2*). For example, the new total mean dose for the Hiroshima 1- to 9-rad group mentioned above is 9.9 rads (*2*). The corresponding T65 dose was only 3.7 rads. It is not mentioned in the article that the new gamma-ray and total dose estimates are higher.

Points 1) and 2) above imply that in Hiroshima there is no significant effect at a gamma-ray dose more than twice as large as the mean T65 dose in the range of 1 to 9 rads. The gamma-ray dose in this instance is also practically identical with the total dose, since the new neutron dose is only 0.047 rad (*2*). In Hiroshima, gamma radiation in the range of 1 to 9 rads (which is the relevant range for the current controversy about effects of low levels of radiation), therefore, appears to be *safer* than before. Thus, a plot of the estimated probability or rate of dying from leukemia or AMN resembles a hockey stick with no significant effect along the blade that grew in length by virtue of the dose revision. This low-dose point for leukemia and AMN in Hiroshima—the best data point—is in violation of the "linear equation" between dose and "ill effects."

3) In Nagasaki, the new gamma-ray doses and total doses—again in rads—*decreased* (*2*). But this decrease *does not increase* the low-dose gamma-ray risk estimates for leukemia in this city, since there is no statistically significant difference in the estimated mortality rates and the crude or net probability of dying from leukemia between the 0 dose group and the groups with T65 doses between 1 to 9, 10 to 49, and 50 to 99 rads (*3*). Thus, the blade of the hockey stick for leukemia in Nagasaki extends from 0 to 99 rads. Data for AMN in Nagasaki which incorporate the change in the location of the hypocenter were not available to me;

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-