Proof of the Pudding

The appointment of Science Officers to serve at several of our embassies (see page 1561, this issue) prompts us to take another look at the role of science in the Department of State. The revival of the science office, which began nearly a year ago with the appointment of Wallace R. Brode as Science Adviser to the Secretary of State [Science 127, 175 (24 Jan. 1958)], indicated that the Department recognized the need for a mechanism to ensure that scientific factors would be taken into account in decisions affecting foreign policy and that contact between foreign and American scientists needed to be facilitated.

The general pattern for the organization of the Science Adviser's Office is now clear. At the base is a Washington staff of which the principal officers, in addition to Brode, are as follows: Deputy Science Adviser L. H. Farinholt, who was formerly professor of chemistry and director of the chemistry laboratories at Columbia University and science attaché in London in 1954; Assistant Science Adviser Mary E. Corning, physical chemist, who was formerly with the National Bureau of Standards; Raymond L. Zwemer, zoologist, who was for three years chief of the Division of International Cooperation for Scientific Research at UNESCO in Paris; and Assistant to the Science Adviser Walter M. Rudolph, who has been in the science program since its beginnings in 1947.

The Science Officers (who appear to be equivalent to the attachés under the earlier program) will serve for two years and will be backed up by Deputy Science Officers, who will serve similar but overlapping terms to provide for continuity. These officers will be assigned only to certain major and centrally located countries, but they will be expected to keep abreast of developments that bear on foreign policy in neighboring countries. Provision is being made for carrying out similar functions in countries beyond the purview of the Science Officers by the designation of foreign service officers, who have the requisite familiarity with science, to cooperate with the science office.

This is the formal structure. How it will work in practice will depend less upon its table of organization than upon a number of unspecified and informal relations. However favorable the predisposition of the old-line foreign service officers may be to the newly appointed Science Officers, the latter will still have to work out their precise role in the embassies and gain an accepted place. So also, the Science Adviser's office will have to maintain and improve its working relations with such other branches within the Department as the International Cooperation Administration and the Technical Cooperation Administration and with outside agencies such as the National Science Foundation, the National Academy—National Research Council, and the Killian Committee.

The new office has its work cut out for it. Not the least of its tasks will be that of avoiding the gradual attrition that afflicted its predecessor, an attrition which was the more readily brought about by the short-term appointments of scientists; when their terms came to an end, no successors were appointed. The new office has a greater assurance of continuity in that its Washington base is permanently staffed, but the Science Officers are still vulnerable by virtue of their limited terms of appointment. Perhaps the best remedy is to establish career appointments for at least some of the Science Officers, who would thus become scientist-diplomats. A permanent cadre of this kind would give greater continuity of experience and increase the chances that the work would be maintained when the political winds blow cold.—G.DuS.
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(Continued from page 1534)

are made. On the contrary, my conclusions are not based on the results at these low levels. They stem from the extrapolation of tumor data from mice through cats and dogs to man, and from comparisons of radium and strontium-90 toxicity in mouse and man.

In point 2 Moos objects that the experimental design is inadequate to demonstrate a threshold dose. That is certainly true. However, the design is adequate for the intended purposes of the experiment—namely, to examine the effects of a range of doses and to investigate the shape of the dose-response curve.

One consistent difficulty in assessing the fallout situation is exemplified in point 3 of Moos’s letter. The first sentence is one with which any intelligent person could agree whole-heartedly. The second sentence is a consequence of the charged, emotional approach so often apparent in discussions of the hazards of fallout. This attitude has unconsciously influenced many interpretations of radiobiological data. We need honest, objective, unemotional evaluations of the experimental results, which can then be applied to problems of world-wide contamination. It is very important that concern over these problems not be permitted to distort the appraisal of the experimental results.

Moos has suggested that I temper my conclusions. None of the animal data have produced linear dose-response curves. The obvious conclusion is not “that a linear dose-effect relation is less probable than a nonlinear relation” but that the relationship probably is not linear. Regarding the evidence for a threshold, I agree that the only justified conclusion at this time is that a threshold might exist. I so stated in my article.

Roth’s reaction to the opening paragraph of “Mice, men, and fallout” beautifully illustrates one of the primary reasons for that article’s having been written. Too many of us expect the distinguished authority in some specialized field to be an unquestioned authority in all fields.

It has been objected that not enough animals were used to predict events that might happen one time in a few hundred thousand. This is certainly true. If Roth will reread the third paragraph of the article in question, he will find that my objective was not to test such frequencies but to examine the two major assumptions upon which the previous predictions of damage from fallout have been based. The 960 mice provided dose-response curves with characteristics contrary to these two assumptions. That is, they are not linear, and they suggest that a minimum dose must be exceeded before the response is manifest. Consequently, extrapolations along straight lines from effects at moderate or high doses to no effect at no dose are unwarr-
Roth calls attention to the very short life span of the mouse contrasted to that of man. This difference, along with the great dissimilarity in size, is the main obstacle to transferring mouse data directly to man. He will note, in the tentative extrapolations given in Fig. 5 of the article, that both of these factors have been taken into account.

Apparent Dunn and Dobzhansky feel that my article should have encompassed all of radiobiology. On the contrary, it was deliberately limited to one small aspect of this subject—namely, considerations of the methods that have been used and that can be used to predict the consequences to exposed individuals of low levels of radiation. A discussion of heritable damage was not pertinent, and I expressly stated that the exposed generation only would be considered. If one announces that he is going to investigate the effects of temperature upon mitosis, for example, should he be accused of "willful neglect" if he does not include the effects of temperature upon gene mutation? I have had no experimental experience with radiation genetics, and it would be presumptuous for me to pose as an authority on that subject. I am confident that the geneticists themselves will eventually be able to tell us whether the linear relationship between gene mutation and exposure holds at doses lower than 25 roentgens.

Dunn and Dobzhansky say that I have assumed "that the main danger of radiations in man's environment lies in their effects on the individuals exposed." There is no basis in my article for this statement. The sentences they quoted were not intended to justify the omission of a discussion of genetic consequences, as they suggest. These quotations merely describe the kind of changes that are most apparent and most easily measured in exposed animals.

I agree wholeheartedly that the data I presented have no bearing on the problem of radiation exposure and germinal mutations. It also was not my purpose to discuss somatic mutations or possible mechanisms of carcinogenesis. Why should a reader be surprised that these subjects were not covered? I also did not include any mention of the effect of radiation upon the sexual behavior of Paramecium.

It is difficult to understand how two distinguished scientists could so misread my paper that they should accuse me not only of ignorance of the distinction between somatic and germinal radiation damage but also of "neglect of elementary methods of critical examination of evidence." My main thesis was that most
predictions of the effect of fallout on tumors and life shortening have been based on very scanty evidence and unsupported assumptions. I proposed alternative methods of prediction that use information from animal experiments as well as available human data. I am forced regretfully to conclude that the fallout problem elicits such an emotional response that many otherwise sagacious and objective scientists lose their ability to read accurately and think clearly.

MIRIAM P. FINKEL
Argonne National Laboratory,
Lemont, Illinois

**Forthcoming Events**

**January**


22-23. Mathematical Assoc. of America, 42nd annual, Philadelphia, Pa. (H. M. Gehman, MAA, Univ. of Buffalo, Buffalo 14, N.Y.)


26-29. Institute of the Aeronautical Sciences, 27th annual, New York, N.Y. (IAS, 2 E. 64 St., New York 21.)

26-30. Writing and Publication in Industry, conf. and workshops, Brooklyn 1, N.Y. (T. L. Donahue, Writing and Publication Conf., Polytechnic Inst. of Brooklyn, 333 Jay St., Brooklyn 1.)

27-30. Society of Plastics Engineers, Inc. 15th annual tech. conf., New York, N.Y. (L. A. Bernhard, SPE, 65 Prospect St., Stamford, Conn.)


29-31. Western Soc. for Clinical Research, 12th annual, Carmel-by-the-Sea, Calif. (W. N. Valentine, Office of the Secretary, Univ. of California Medical Center, Department of Medicine, Los Angeles 24.)

**February**


3-5. Reinforced Plastics Conf., 14th, Chicago, Ill. (Soc. of Plastics Industry, Inc., 250 Park Ave., New York 17.)

6-7. American College of Radiology, Chicago, Ill. (W. C. Stronach, 20 N. Wacker Dr., Chicago 6.)

9-11. American Acad. of Allergy, Chicago, Ill. (B. Rose, Royal Victoria Hospital, Montreal, P.Q., Canada.)

9-11. Nature of Coal, symp., Bihar, India. (Director, Central Fuel Research Inst., P. O. Fuel Research Inst., Dhanbad District, Bihar.)


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