President's Science Adviser

When the Editor of *Science* recommends [*Science* **142**, 1025 (22 Nov. 1963)] that the several staff roles of "the President's Science Adviser" be assigned to different men in order to bring about "a less arrogant mode of operation," some readers may need to be reminded that an editorial in *Science* does not represent the views of the AAAS, nor of its Council or Board, but only those of its author.

That particular editorial seems mistaken to at least one member of the AAAS Board, the undersigned, who would not (any more than the Editor) pretend to speak for anyone but himself.

The charge that the Science Adviser wields too much power and authority misunderstands the nature of the Executive Office of the President.

There is no question that the Science Adviser, wearing each of his several hats, wields influence—as adviser to the President, as chairman of PSAC, as chairman of the Federal Council, and as director of OST. But influence is neither power nor authority: the Science Adviser can do nothing of importance by virtue of power vested in him by law. On any important issue on which he advises—especially on the budget for research and development—his advice is checked for the President by the competing advice from other members of the Executive Office, such as the Budget Director and the Special Assistant for National Security Affairs. Even more important, his advice, like that of PSAC, bears on the activities entrusted by law to Cabinet members and agency heads, who also have access to the President, and have scientific advisers of their own. It would be a delusion of scientific grandeur to think that the scientists in the Executive Office have undue power in that political competition.

If scientists have begun to have a role of usefulness to the President, and of influence within the administration on problems which affect the scientific community, it is largely because they have learned that service in a confidential staff capacity does not license them to undertake independent political action.

If you should look on the several staff units and committees which the Science Adviser tries to coordinate as independent political agencies, you would be warranted in proposing to break them apart. But such a course would force them into the kind of jurisdictional rivalry that would destroy the usefulness of all of them to the President, especially in linking his policies with those of the departments and agencies. If the scientific community wishes to undermine its status within the Executive Office, this would be the way to go about it.

DON K. PRICE
Harvard University

Ethical Code for Scientists?

Lawrence Cranberg [*Science* **141**, 1242 (1963)] makes the point that scientists have been dilatory, as compared to engineers, in doing more than merely talk about formal codes of ethics for their professions. I am not in a position to equate the efforts of the various professions in making clear the relationship of their work to society. However, I wish to point out that one group of scientists, the Society for Social Responsibility in Science, has taken its social responsibilities seriously.

Each scientist, in becoming a member of this society, agrees: "(1) to foresee, insofar as possible, the results of his professional work, (2) to assume personal moral responsibility for the consequences of this work, not delegating this responsibility to his employer, (3) to put his own efforts only into that work which he feels will be of lasting benefit to mankind, and (4) to share his scientific knowledge, and such ethical judgments as are based upon it, with government and laymen in order that they may intelligently use the tools which science provides."

This is, in effect, a code of ethics, which, as Cranberg says, is much needed today.

W. E. GRAHAM
*973 Woodmere Drive,
Westfield, New Jersey*

However worthy the SSRS may be, its concern with a limited, special range of ethical problems and its existence apart from the main body of professional scientific organizations only emphasize the disparities which exist between scientists and other occupational groups with respect to ethical education and regulation. These disparities remain to be justified or eliminated.

LAWRENCE CRANBERG
*Department of Physics,
University of Virginia,
Charlottesville*

On the Rewards of Tenacity

The dedication of Polanyi to proving the validity of his theory [M. Polanyi, "The potential theory of adsorption," *Science* **141**, 1010 (1963)] must have derived from a firm conviction of its validity and not, as he modestly says, from ignorance of developments which were extant at the time of his first publications. The emerging verification after a half century of frustration must indeed be a rewarding experience. The doctor must be well endowed with what might be called a philosophical tenacity to his convictions in the face of almost overwhelming opposition.

One might also conclude from this 50-year disputation that scientists, discipline notwithstanding, are still very human. We have our heroes and popularly accepted theories. The orthodoxy and dissent of which Dr. Polanyi speaks are perhaps the virtues in which the scientific community can take its greatest pride.

GEORGE A. LOUIS
*8301 East Mariposa Drive,
Scottsdale, Arizona*

Sex Attractant of the American Cockroach

In a report published in *Science* (*1*), Jacobson, Beroza, and Yamamoto claimed to have isolated and identified the sex attractant of the female American cockroach. This claim has received wide publicity in the public and scientific press (*2*) and has been reiterated in another, more recent paper in *Science* (*3*). Since the claim of these investigators can be supported neither by the evidence which they advance nor by our own knowledge of the behavior of the substance, we feel obliged to point out that identification of the attractant cannot be considered to be accomplished. On the contrary, it seems to us that the available evidence shows that the proposed compound could not in fact be the attractant.

Jacobson *et al.* (*1*) have stated that "much larger amounts of fairly pure attractant were obtained by passing air continuously over . . . virgin females in metal containers," according to Yamamoto's procedure (*4*), than were obtained by the paper method which we described (*5*).

This claim has been tested by quantitative bioassay (*6*) and found invalid. The attractant from two homogeneous groups of virgin females was collected by the two methods, and, in two different experiments, the material collected from paper was shown, on the basis of yield per roach per day, to be three to five times as potent as the material collected by passing air over the females. Comparison of these results with the claim of Jacobson *et al.* shows how far from actuality the estimated amounts of attractant can be in the absence of a reliable bioassay.

Jacobson *et al.* claim to have isolated "12.2 mg of the pure attractant," but no evidence of purity of this product is given except for a gas chromatogram obtained "under conditions identical with those reported by Wharton *et al.*," which "gave a single peak with an elution time of 6 minutes as contrasted with 105 or 145 minutes reported by them." The retention times we reported (*5*) are typical of *many* chromatograms performed on *many* columns, and the corresponding peaks have been repeatedly demonstrated, by trapping and subsequent quantitative bioassay, to be those of the attractant. There can be no doubt that, under the conditions we reported, the attractant is eluted from a column coated with 5 percent Apiezon M in from 105 to 145 minutes. Moreover, no components of any of our preparations, which were trapped from the chromatographic column up until the appearance of the attractant peak, have ever been found to be active. On the other hand, it is significant that Jacobson *et al.* appear not to have trapped the substance responsible for the peak they observed at 6 minutes, or to have subjected their preparation to a quantitative bioassay, as we do ours (*6*). They have shown no proof that the substance responsible for the peak at 6 minutes and the attractant are identical. In view of the extreme sensitivity of the male cockroach to the attractant, it is readily conceivable that their preparation contained only a minute quantity of attractant, which could not be observed on the chromatogram, and that the material responsible for the peak at 6 minutes was a major, inactive component. In such circumstances it would also be unlikely that any of the other procedures used for characterizing the substance would yield evidence of the presence of the attractant as a trace component of the major substance isolated.

Jacobson *et al.* have shown no proof of correspondence betweeen the attractant and the structure they propose. There is also considerable doubt in our minds that the structure itself can validly be deduced from the data they present. For example, their assignment of a 12.5-μ band to an isopropylidene group is at variance with the reference they cite (*7*), which states, rather, that for a fully substituted ethylene (such as their proposed structure is), "no band can arise from hydrogen deformations about the double bond." Moreover, the assignment of the splitting at 7.25 μ to isopropyl in the hydrogenated derivative appears arbitrary, since with respect to the structure proposed, the assignment could as well have been made to the gem-dimethyl groups on the ring (*8*).

We also note the fortuitous circumstances which led to their identifying the hydrogenated derivative of the product they isolated with the material they synthesized. We would expect, rather, that the preparative methods used would yield different *cis-trans* isomers, or at least a mixture of the isomers, and we would expect these to have different infrared spectra and to differ in gas-chromatographic behavior.

The effort of various investigators to identify natural insect attractants and to use them for the control of various pests is a highly commendable scientific endeavor. Great caution must be exercised, however, to avoid premature claims of success.

D. R. A. WHARTON
E. D. BLACK
C. MERRITT, JR.
U.S. Army Natick Laboratories, Natick, Massachusetts

References

1. M. Jacobson, M. Beroza, R. T. Yamamoto, *Science* **139**, 48 (1963).
2. See, for example, *Time* (18 Jan. 1963); *New York Times* (18 Aug. 1963); *Chem. Eng. News* (21 Jan. 1963).
3. M. Jacobson and M. Beroza, *Science* **140**, 1367 (1963).
4. R. Yamamoto, *J. Econ. Entomol.* **56**, 119 (1963).
5. D. R. A. Wharton, E. D. Black, C. Merritt, Jr., M. L. Wharton, M. Bazinet, J. T. Walsh, *Science* **137**, 1062 (1962).
6. D. R. A. Wharton, G. L. Miller, M. L. Wharton, *J. Gen. Physiol.* **37**, 461 (1954); *ibid.*, p. 471.
7. L. J. Bellamy, *The Infra-red Spectra of Complex Molecules* (Wiley, New York, ed. 2, 1958), pp. 51, 52.
8. A. Weissberger, in *Chemical Applications of Spectroscopy*, W. West, Ed. (Interscience, New York, 1956), vol. 9, p. 337.

While we have no wish to engage in polemics with Wharton and his group concerning the sex attractant of the American cockroach, we should like to point out the following facts in reply to their "invalidation" of our claims.

1) Wharton's quantitative bioassay method is based on the use of "wing lift as the single criterion of response." Our bioassay tests with more than 100 organic compounds of various classes, including several substances that are repellent to male American cockroaches, show that a number of materials will elicit wing-raising in these insects. Our bioassay procedure therefore requires, as a positive response, no less than a complete behavioral cycle in the male—namely, intense excitement, wing-raising, *and* attempts to copulate with one another. Of the many compounds tested, only our natural attractant evokes this complete response.

2) The purity of our 12.2 mg of attractant is shown by its emergence, from each of three separate gas-chromatographic column packings, as a single peak that caused a complete sexual response in males as it emerged from the column. The columns used and the conditions were as follows: (i) 0.63 cm × 0.61 m stainless steel column; 20-percent silicone grease on Chromosorb P; column temperature, 100°C; helium flow rate, 72 ml/min (retention time, 2.5 minutes); (ii) 0.63 cm × 1.37 m stainless steel column; 20-percent Craig polyester succinate on firebrick; column temperature, 150°C, helium flow rate, 75 ml/min (retention time, 13.5 minutes); (iii) 0.63 cm × 1.2 m glass column, 5-percent Apiezon M on 80 to 100-mesh Chromosorb W; column temperature, 130°C, argon flow rate, 110 ml/min (retention time, 6 minutes). The column and conditions described under (iii) are those described by Wharton *et al.* as giving a retention time of 105 or 145 minutes; in our experiment the attractant, emerging after 6 minutes, was collected by means of a dry ice cooled receiver and was the only eluant present, despite the fact that chromatography was allowed to proceed for 180 minutes.

3) Of the insect sex attractants investigated by us in these laboratories, that of the American cockroach is by far the most potent, eliciting a positive response in males at a concentration of 10^{-14} μg; this is equivalent to 30 molecules. Were the attractant "a trace component," its activity at this level would doubtless be impossible; rather, this extreme potency suggests purity. Moreover, the attractiveness of our pure material has recently been independently substantiated by Boeckh

et al. (*1*), using an electrophysiological technique.

4) We agree with Wharton *et al.* that our assignment of a band at 12.5 μ to an isopropylidene group was an unfortunate one. The assignment was based on the absence of this band from the infrared spectrum of the hydrogenated attractant. Gas chromatography of the hydrogenated attractant and the synthetic saturated product on packings different from those described has indeed shown the presence of several peaks, identical in the two preparations.

In spite of the marked instability of our cockroach sex attractant, we are attempting to synthesize this material. In the final analysis, only synthesis can serve as the decisive factor.

MARTIN JACOBSON
MORTON BEROZA
Entomology Research Division, U.S. Department of Agriculture, Beltsville, Maryland

References

1. J. Boeckh, E. Priesner, D. Schneider, M. Jacobson, *Science* **141**, 716 (1963).

Biological Mechanisms of Aging

In a recent very stimulating paper [*Science* **141**, 686 (1963)] Curtis has discussed biological aging processes and concluded, quite justly, that somatic mutations appear to have a primary importance in reducing longevity of living creatures. I think, however, that he has been unjust in holding up the "wear and tear" hypothesis of Selyle, Comfort, and others as antithetical to the "somatic mutation" hypothesis which he espouses so effectively.

While Curtis has shown conclusively that certain stresses, even when applied systematically and repeatedly, do not accelerate aging, he has *not* shown that a wide variety of such stresses (such as are the burden of every living thing) will not do so. As he points out, certain stresses cause irremediable damage to some organ or tissue and thus make eventual failure at that point relatively likely. The beauty of radiation as an experimental stress is that it is general, striking all sensitive tissues at once. But from the experiments reported by Curtis we cannot say that application of a number of specific chemical and disease stresses will not bring about a general loss of viability similar to that caused by radiation. The advance Curtis has made is to suggest (implicitly) that the wear-and-tear hypothesis *is commuted to* the somatic-mutation hypothesis by the recognition that viability-reducing stresses are those which are mutagenic in certain susceptible tissues: those in which mitosis is slow. The two hypotheses are more alike than they first seemed.

Experiments like those of Curtis's group are suggested, in which mice during their laboratory lifetimes are subjected to a carefully selected *range* of stresses. Like Selye, I would include various types of psychological stress, in addition to numerous chemicals and diseases, because such stresses are notorious as "imbalancers" of function in many of the organs now shown to be most susceptible to weakening by somatic mutation.

RICHARD O. WHIPPLE
Department of Chemistry, University of Singapore, Singapore, Malaysia

I think Whipple's suggestion is excellent and am inclined to agree that if just the right combination of stresses were found it would shorten the life span. But the question would then be: What does this have to do with natural aging? It is apparent there is abundant room for future research.

HOWARD J. CURTIS
Department of Biology, Brookhaven National Laboratory, Upton, New York

Blood of Anthropoid Apes

Readers using the report by Wiener and Moor-Jankowski on blood groups in apes and baboons [*Science* **142**, 3588 (4 Oct. 1963)] will probably also be interested in a neglected paper by Yvan Bereznay [*Bull. Soc. Roy. Zool. Anvers No. 10* (1959)]. Bereznay gives detailed data on blood groups, chemistry, counts, various measurements, and immunoelectrophoretic patterns for nine chimpanzees, six gorillas, and two orang-utans. A finding in some contrast to that of Wiener and Moor-Jankowski concerns the blood groups of gorillas: Bereznay reports type O in four *Gorilla gorilla beringei* and one *G. g. gorilla*, type A in one *G. g. beringei*.

GEORGE B. RABB
Chicago Zoological Park, Brookfield, Illinois