Letters

Transplantation of Tissues

In the Research News report by Thomas H. Maugh II, "Tissue cultures: Transplantation without immune suppression" (7 Sept., p. 929), it is stated that "At least one other investigator in the United States and three in Europe have also duplicated some of his [W. T. Summerlin's] results. . . ."

In my laboratory we have successfully transplanted cultured skin of DBA/2 mice onto normal C57BL/6 mouse recipients. These two strains of mice are strongly histoincompatible. One (DBA/2) has a tan fur color; the other (C57BL/6) is black. When fresh skin grafts were used, the black mice rejected DBA skin in 9 to 11 days.

We have performed six series of experiments, using 10 to 15 recipient black mice for each series. In the first four series we used the classical technique of Billingham, Brent, and Medawar for positioning the graft and dressing the wound after grafting. In none was there a permanent take of the allogeneic graft.

In the last two series, in which we made some technical adjustments suggested to us by Summerlin, five black mice are carrying patches of white hair 60 to 75 days after transplantation.

To us, this is confirmation that failure in attempts to duplicate Summerlin's results can be ascribed to simple technical difficulties.

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Nuclear Waste Disposal at Hanford

In "Radiation spill at Hanford: The anatomy of an accident" (News and Comment, 24 Aug., p. 728), Robert Gillette implies that there was a discrepancy between my statement on storage tank leakage at the Hanford Reservation in the 1959 congressional hearings on nuclear waste disposal and a 1968 report (*1*) by the General Accounting Office (GAO).

The GAO report of 1968, which I have just read for the first time, does indeed appear to contradict my 1959 testimony. The GAO correctly reported material prepared by a contractor and accepted by the Atomic Energy Commission (AEC). A more careful study by the present contractor resulted in the correction of some imprecise terminology in the GAO report and some new estimates of the volumes believed to have leaked. The relevant discrepancy developed from the poor terminology.

My 1959 testimony before the Joint Committee on Atomic Energy was intended as a broad overview of the Hanford situation, with a brief reference to the life of the storage tanks. This was amplified in response to questions from members of the committee. My answers were correct and forthright. In particular, I acknowledged that suspicious occurrences had led to investigations of whether nuclear wastes had in fact leaked. No leak had been found up to that time.

My testimony (*2*) was strengthened by a more technical account by R. E. Tomlinson (*3*). That account clearly identifies three off-standard events, in 1956, 1957, and 1958, that might lead one to suspect a leak. The event at issue occurred in 1958 and involved tank 113-SX. The tank liner bulged, and the radioactive waste was pumped out in case a leak had occurred. The liner went back to about its original position. Although every effort was made at that time to find a leak, none was detected.

After the 1959 hearings, leak tests continued on tank 113-SX. No escaped waste was detected as late as August 1962. In late 1962, the tank was filled with saturated salt solution for test purposes. This liquid certainly leaked and drove some of the 1958 residual radioactive material into the detection laterals below the tank. It was then conservatively assumed that the leak had originated in 1958 (although it could have been one of the self-sealing leaks described in the GAO report). This leak was later carelessly described in the GAO report as a "leak detected in 1958."

For the record, the estimated leakage has been corrected from 35,000 gallons to 15,000, and most of this was the nonradioactive salt solution. Also, the 113-SX tank is only one of several for which estimated date of leak and volume leaked have been revised. In fact, the 1956 occurrence reported by Tomlinson is now believed to have involved a small leak that same year.

It is important to Tomlinson and me that our testimony of 1959 be recognized as valid. More important is the recognition that both the AEC and its contractor voluntarily presented the facts to the public as soon as security permitted it. Those who believe they invented environmental concern in the 1960's overlook such steps as the initiation of environmental programs at Hanford before the first reactors operated in 1944, and the creation of the Columbia River Advisory Group as early as 1949 to keep state pollution officers aware of Hanford waste management efforts. Most important, the many fine scientists and engineers who worked at Hanford from 1944 until the reference year of 1959 sacrificed peer recognition of their findings because of data classification. They should not be further hurt by innuendos that at any time they were asked, by the Manhattan District, the AEC, or their contractors, to report other than their best technical interpretations of their work.

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References

1. Comptroller General of the United States, *Observations Concerning the Management of High-Level Radioactive Waste Material* (Report to the Joint Committee on Atomic Energy No. B-164052, General Accounting Office, Washington, D.C., 1968).
2. U.S. Congress Special Subcommittee on Radiation of the Joint Committee on Atomic Energy, *Industrial Radioactive Waste Disposal* (86th Congr., 1st sess., 1959), vol. 1, p. 159.
3. ———, *ibid.*, p. 282.

Insect Control

Three reports ["Insect control (I): Use of pheromones" (Research News, 24 Aug., p. 736) and "Insect control (II): Hormones and viruses" (Research News, 31 Aug., p. 833), both by Jean L. Marx, and "Insect viruses: A new class of pesticides" (News and Comment, 7 Sept., p. 925) by Nicholas

Wade] were interesting to read and took me back to my boyhood, when I enjoyed collecting insects.

In New Hampshire in the summer of 1919 the sugar maples were completely defoliated by the rosy maple moth, *Dryocampa rubicunda*. In 1920 the maples were normal. In the maple forests, my joy, as a collector of beetles, was great because the woods were swarming with *Calosoma frigidum* eating the larvae of the moth. At home in New Jersey, I visited a friend on his farm where potatoes were being dug. The insecticides 50 years ago were not as effective as they are now; that potato field was swarming with *Calosoma calidum*, which had been feeding on larvae of the potato beetle, *Leptinotarsa decemlineata*.

I am not an organic gardener, but in my vegetable patch I cultivate the lazy way with a minimum of plowing and some mulch of leaves and shredded sticks. I have not sprayed or dusted for several years. The corn ear worms and bean beetles are there, but their damage is small.

I suppose my farmer friends are right in saying that it is not economically practical to rely on natural predators for control. I am a chemist and so should probably not speculate, but I wonder what would result if a *Calosoma*, full of eggs, could be kept alive in cold storage to be released when pest larvae were hatching. Clausen (*1*) names many insects, some with unrestricted feeding habits, which might be treated this way. Could not some of these be used to control the gypsy moth?

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References

1. C. P. Clausen, *Entomophagous Insects* (McGraw-Hill, New York, 1940).

University Cooperation with Industry

The spirited discussion by G. D. Cody, W. D. Compton, and R. Roy (Letters, 31 Aug., p. 800) of Roy's article (1 Dec. 1972, p. 955) on university-industry interaction patterns prompts me to mention our experience at Carnegie-Mellon University, where the Processing Research Institute (PRI) was organized with a grant from the RANN (Research Applied to National Needs) program of the National Science Foundation (NSF). PRI benefits from what Cody calls a "troika," in which government funds are used as a "catalyst" to bring industry and the university together in a meaningful way. During the 1972–73 academic year, PRI cooperated with 14 companies in projects having an annual value of $500,000. Approximately 60 percent of the funds were provided by industry.

A key feature of PRI is a 2-year Master of Engineering degree program which provides for a diversified, broad curriculum. PRI attracts problem-oriented graduate students who consider their industry-sponsored project to be a vital part of their education. Aspects of our experience that we think are critical for successful industry-university interaction are (i) an identifiable organization on campus that interacts with industry—at Carnegie-Mellon, the PRI; (ii) a broad base of disciplinary support—in our case, from the departments of chemical engineering, mechanical engineering, and metallurgy and materials science; (iii) sufficient faculty of acknowledged competence who are willing to enthusiastically support this type of activity; and (iv) encouragement and support from the university administration.

As Cody has indicated some doubt concerning the feasibility of an effective industry-university partnership, it is important to mention some of the benefits to the university we have observed in a brief span of time: (i) the development of a problem-oriented graduate program that parallels the traditional discipline-oriented programs; (ii) a broadened outlook on the part of the faculty; (iii) an increased interaction between the three cooperating departments; and (iv) increased support of the graduate program through industry-sponsored projects. From the point of view of industry, the opportunity to provide a positive input to graduate education, especially in the development of new approaches to problem-solving, is gratifying. Representatives of industry who visit our campus seem to benefit from the broad view that our faculty takes of their disciplines, which has led to some unusual solutions to industrial problems.

Finally, one of the objectives of the NSF grant is to experiment with different forms of industry-university interaction. We invite comments and suggestions.

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