Letters

Exposure to Alkyl Mercury

As one who has investigated the chemistry of mercury compounds for the past 40 years, personally and with a number of Ph.D. candidates, I was taken aback by the technical comment, "Precautions with alkyl mercury" (21 May, p. 872).

Evidently my students and I live charmed lives. In 1931, because of a cracked flask under reflux, I was exposed for 3 hours to inhalation of dibutyl mercury. The dosage was sufficient to cause loosening of my teeth; subsequently I lost two teeth because of infection but otherwise suffered no ill effects. In 1934, I was exposed for 2 days to dimethyl mercury, which was being repeatedly distilled at atmospheric pressure, until I realized the composition of my by-product. Again I suffered no damage.

In 1940, when Canadian scientists were commissioned to make life unpleasant for Hitler's Germans, I decided to test dimethyl mercury as a lethal gas. I devised an economical manufacturing process (aluminum carbide with aqueous mercuric chloride). In collaboration with the University of Toronto, School of Hygiene, I allocated 500 grams for use in vapor exposure to 25 rats in a suitable enclosure (108 cubic feet) at a dosage of 25 grams per day.

The atmosphere of the enclosure was only changed once each day, for feeding and observation prior to the introduction of a new dose. The behavior of the rats during the administration of a new dose was of interest. They loved it! They would gather about the inlet like hogs at the feeding trough, day after day, until the 500 grams was used up. The experiment was concluded after we were unable to cause the death of any rats or to observe any abnormal symptom or behavior.

In 1968–70, my technician (a man subject to allergic reactions from many chemicals) determined the temperature coefficient of the electric moment of dimethyl mercury under conditions in which he could not avoid exposure to the vapor. He was unaffected visibly during the 200 days of exposure.

My students and I were similarly "careless" about manipulation of alkylmercuric salts. Except for occasional skin rashes, no physiological abnormalities occurred. We may have been encouraged in our "carelessness" by the knowledge that organomercurials (maximum dosage is more than a gram per day when given intravenously) were essentially the only diuretics used for therapy prior to 1952 and are still used.

Perhaps the toxicity of alkylmercurials is a matter of human idiosyncrasy. In 1931, following the "mercury scare" of that period, in an experiment supervised by Gilman at Iowa State University, it was demonstrated that as an individual I eliminated mercury at the rate that I absorbed it. Perhaps the students who worked under my direction (and whose present positions attest that they have not suffered debilitating effects) were attracted to the organomercurial field because they were equally immune.

If this was the fact, then attention should be devoted to a study of the idiosyncrasy rather than to outright condemnation of mercury. Many citizens besides myself enjoy eating tuna and swordfish steak. Some citizens earn their living by harvesting fish. Also, the impending protein deficiency of the growing world population indicates that we must depend, more and more, on fish as a source of this essential nutrient. In these circumstances can we tolerate condemnation of mercury by dedicated (and some less dedicated) zealots unless they can prove that the derivatives of this element are deleterious to the majority of the human race? I have yet to see scientific evidence to support the limit of 0.5 part per million set for mercury in foodstuffs.

I am not impressed by the clinical references extending back to 1940 cited by Klein and Herman. The side effects of mercurials used as drugs (and not within the parts per million limit) have been known for some centuries, certainly longer than the side effects of modern pharmaceuticals. Why, then, have we not heard these gory details earlier?

During the mercury scare that occurred about 1930, the studies made by the U.S. Department of Agriculture placed lettuce at the top of the list of herbiferous mercury accumulators. Since antimercury zealots must have a target, I suggest that they attack lettuce, which may be a less critical human requirement than fish.

Beneath all of this scientific small talk is an underlying social principle. What is the cutoff ratio at which the few must be favored at the expense of the many? In the case of penicillin, the ratio of 1 to 1000 was enough to persuade people, via their doctors, that penicillin should continue as a controlled therapy, despite the 0.01 percent mortality rate from violent allergic reaction.

I am not advocating detriment to the few for the advantage of the many. I do claim that governmental regulatory bodies are also part of the citizenry. Such bodies have a responsibility (all too frequently lacking) to ascertain whether an arbitrary regulation redounds to the detriment of the many for the sake of the few. If these bodies discover that such a situation exists, then it is their further responsibility to find ways to protect the few without detriment to the many.

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Need for Graduate Education

It may well be true that fewer Ph.D.'s will be needed in the next decade as teachers in our colleges and universities (see 9 April, p. 139). To conclude from this that the extended training of gifted and highly motivated young people interested in scholarly careers should therefore be curtailed shows, however, a lack of imagination. Such people are needed as never before, not only in the natural sciences, but in all fields of learning.

Those of us who appreciate how little men yet know and how much there is to find out should not throw up our hands and agree with the cost accountants and some of the politicians that graduate education should be curtailed. Rather we ought to begin now

to provide the means for employment of more scholars in careers of research.

Solutions to the cancer problem, to the still larger medical problem of heart disease, or to the many problems of pollution associated with a high standard of living are not going to be found by untrained individuals. The great advances in biology in recent years may be directly correlated with the support given by certain foundations and by the National Institutes of Health, support that made it possible for universities to maintain men committed in large part to research. It was this kind of support, for example, that resulted in a vaccine against polio and thereby saved millions of dollars that would otherwise have had to be spent for hospitalization and therapy of people crippled in youth by this disease. It is this kind of support that now permits some to contemplate a "final push" against cancer.

Rather than curtail training programs, we should work for future increases in job opportunities. One way to do this is to establish graduate universities and research institutions affiliated with them. The latter might be centered on practical problems but could approach these problems with a long-range view and in an interdisciplinary way. The Woods Hole Oceanographic Institution and its new graduate school of oceanography affiliated with the Massachusetts Institute of Technology provide an excellent working example.

It is already clear that such research and training centers are needed in diverse fields. They have to be staffed with scientists who have an equivalent of the Ph.D. A sharp curtailment in training of Ph.D.'s now will leave us short of the young people we will need.

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Breath Test Machines

In his letter (2 July) criticizing my report on alcohol breath tests (2 Apr., p. 57) Edwards raises three questions explicitly or implicitly. These are: (i) Are the data presented correct? (ii) Was the report really necessary in view of the fact that the defects described were "well known"? and (iii) Whose problem is alcholism and whose problem is the correct calibration of an instrument used by traffic police?

Neither Edwards nor anyone else has questioned the accuracy of my data. I stated in my report that "There have been occasional warnings about the inaccuracy of Breathalyzer or other alveolar gas tests . . . but these are rare, generally nonquantitative, and inaccurate." To date, I have found no evidence to indicate that this statement was incorrect. Edwards does not cite any previous paper in which the Breathalyzer or any similar instrument was properly calibrated. In addition to the disputed time factor, still inaccurately stated by Edwards, other errors in usage of breath test machines needed to be corrected. For example, it has been assumed that "true reactions with alcohol in expired breath from other than alveolar air (eructation, regurgitation, vomiting) will, of course, vitiate the breath alcohol results, but can be detected by observation of the test subject, and [italics mine] *prevented by having the subject rinse out his mouth with water*" (*1*). This assumption, also, was demonstrated in my report to be false.

Alveolar gas tests are also used in scientific laboratories. It was for this purpose that a Breathalyzer, with its book of instructions (omitting mention of time factors), was sold to the NASA Langley Research Center for use in a study of alcohol effects upon human motor, sensory, and mental performance. Convinced that, in principle, this instrument could not perform as claimed by the manufacturers, I suggested to Maraman, of the Langley center, that we should test the accuracy of the instrument. As indicated in reference 7 of my report, I presented these data to a plenary session of the 16th International Symposium on the Prevention and Treatment of Alcoholism at Lausanne, Switzerland, in June 1970. None of the assembled (scientific, medical, and legal) experts hinted to me, in the lively discussion that followed, that they were old hat. Indeed, I was persuaded by my colleagues at this meeting to give wider circulation to these data. After my report was published in *Science*, M. V. Stack, of the Medical Research Council Dental Research Unit in Bristol, England, called to my attention that he had reported to a meeting in 1957 some of the difficulties with residual mouth alcohol in usage of the Breathalyzer. Unfortunately this information (apparently not published in a journal) was to be found neither in the instruction book issued with the Breathalyzer 11 years later, nor in recent reviews of this subject.

I had and have no interest in pro-