LETTERS

Vinyl Chloride and DDT: Environmental Effects

The report of the AAAS Committee on Scientific Freedom and Responsibility, abbreviated as an article by John T. Edsall (16 May, p. 687), requires comment. The section entitled "*The case of data suppression concerning the carcinogenicity of vinyl chloride*" (p. 690) contains omissions and errors that are inconsistent with the committee's concern for scientific freedom and responsibility. Since so much pertinent information has been omitted concerning this matter, readers may be misled into believing the report.

Edsall states that, until May 1970, "there had apparently been no research in the plastics industry concerning the possible hazards of vinyl chloride. . . ." This is untrue. In fact, the basic published data on vinyl chloride toxicity came from American industry (*1, 2*). The information in (*2*) had been publicly disclosed and presented before their publication dates. Furthermore, the investigations of Viola and of Maltoni have been supported by European industries.

Edsall correctly states that Manufacturing Chemists Association (MCA) member companies entered into an agreement with the European sponsors of Maltoni's work not to reveal *preliminary* Italian data. The agreement was not dissimilar to those commonly made between American sponsors (government, industry, and academic) and research institutions that new data remain the property of the institution and that final results be disclosed through proper scientific channels when a study is completed and evaluated. There was no intention of withholding adverse data. Under MCA rules, all data developed under its auspices must be promptly reported publicly. Without exception, this has been done with all data on vinyl chloride.

U.S. government scientists knew of Maltoni's preliminary results early in 1973 but considered them to be as preliminary as did U.S. industry. At the Second International Symposium on Cancer Detection and Prevention in Bologna, Italy, 9 to 12 April 1973, Maltoni showed a table of preliminary results linking vinyl chloride with cancer formation (which was not published until 1974) (*3*). Umberto Saffiotti, associate director of carcinogenesis at the National Cancer Institute, was present at the meeting and subsequently discussed the results with Maltoni at his Italian laboratory (*4*). Saffiotti felt that the work should be included in "a well documented report" to be assessed by a review panel of the World Health Organization's International Agency for Research on Cancer (of which the United States is a member). This panel also considered Maltoni's data to be preliminary at that time and "decided to postpone compilation of the definitive review to the next meeting of the committee" (*4*).

After a period in which epidemiological and animal studies were being established, MCA, on 17 April 1973, asked the Europeans to jointly make presentations to U.S. and European government authorities. The Europeans readily agreed, and on 15 June 1973, MCA called the National Institute for Occupational Safety and Health (NIOSH) and requested an opportunity to bring new information to that agency's attention. The appointment was set by NIOSH for 17 July. Five European and American industry representatives presented to NIOSH the information that tumors had been found in animals exposed to vinyl chloride concentrations below those that Viola had reported caused cancer in rats. At that time, no human cancer cases had been reported; therefore, no special significance could be attached to the types of tumors observed by Maltoni.

The precautionary label suggested by MCA and referred to by Edsall was illustrated in MCA's *Chemical Safety Data Sheet* (SD56) on vinyl chloride, revised in April 1972. Section 10.1 on "Health hazards" refers to Viola's published studies. The *Data Sheet* states, "Recent research studies reported from Italy indicate that repeated, long-term high level exposures of rats to vinyl chloride monomer vapor can result in the development of malignant tumors. However, many years of industrial experience with human exposures to concentrations frequently far above current standards have not demonstrated any carcinogenicity to humans." Thus the statement by Edsall that MCA ". . . appears to have deliberately deceived NIOSH regarding the true facts" is inaccurate and misleading.

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3. C. Maltoni, *Excerpta Med. Int. Congr. Ser. No. 275* (1974), p. 4.
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John T. Edsall errs in crediting DDT with halting a cholera epidemic in Naples during World War II. A National Academy of Sciences committee stated in 1970 (*1*) that "DDT has prevented 500 million deaths due to malaria that would otherwise have been inevitable." However, the Naples miracle involved typhus rather than cholera or malaria.

It is unfortunate that Edsall selected an undergraduate student's term paper (*2*) as "a useful short history of the use of DDT," for several well-informed scientists have written articles and books on DDT which would be more appropriate sources for AAAS articles. He also refers to "the long persistence of DDT in soil," perhaps being unaware that Stringer *et al.* (*3*) found the half-life of DDT to be only 3 years in orchard soil after 17 successive years of heavy spraying. Edsall cites DDT's "progressive concentration in food chains" as having "led to the killing of great numbers of certain birds and fish"; however, the lack of food chain magnification, especially in marine environments, has been pointed out by Moriarty (*4*), Hamelink *et al.* (*5*), Harvey (*6*), and in two committee draft reports of the National Academy of Sciences (*7*).

While Edsall did not recommend halting DDT use for malaria control, some "environmentalists" are again seeking to halt all DDT exports from the United States. It is encouraging to see no such proposals in the AAAS committee report.

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I am glad to note that, as Clark's letter points out, there was research by American industry on the toxicity of vinyl chloride, beginning several years before 1970. In this work, however, what have proved to be the major hazards were not discovered; if they had been, the later story would have been very different. I am obliged also for Clark's correction regarding the precautionary label, which did in fact make reference to malignant tumors in rats pro-

duced by "repeated long-term high level exposures." The attitude expressed in Clark's letter, however, illustrates the major point that caused concern to the Committee on Scientific Freedom and Responsibility. The extremely ominous findings in the animal studies did not trigger any major alarm until cases of cancer in factory workers exposed to vinyl chloride began to be reported. This has been in the past the general attitude regarding such problems; I believe that it is now out of date. New reagents introduced into industry on a large scale should be regarded as dangerous until proved safe. Precautions to protect workers from inhaling or absorbing them should be built in from the very start, rather than being superimposed later, after damage has become apparent. Carcinogenic substances usually take years to produce cancers; we should not wait for the cancers to appear in human subjects before taking action. Fortunately the work of Ames *et al.* (*1*) and others has shown that carcinogens are generally mutagens. Screening of chemicals for mutagenicity can be carried out fairly rapidly. With the techniques available today we can thus often obtain an early warning of danger, but negative results on mutagenicity tests should not be cause for complacency. Highly toxic substances may be nonmutagenic.

I thank Edwards and others for correcting the erroneous statement that DDT brought a halt to a cholera epidemic. It was of course a typhus epidemic that was halted by DDT. How this error slipped through I do not know; the error is in any case mine, not that of the committee. This point will be corrected in all reprints.

One can add to the references cited by Edwards a recent review on insecticides by Jukes (*2*) which contains a vigorous defense of the value and safety of DDT. Some of the environmental damage that has been charged to DDT was probably unreal or greatly exaggerated; some was probably due to other substances, such as the polychlorinated biphenyls (PCB's), which most analysts long failed to distinguish from DDT and its breakdown products. Nevertheless the official decision to ban DDT for nearly all uses *in the United States* was not arbitrary or capricious; it was taken after prolonged hearings, with presentation of a profusion of evidence by both sides. The case for using DDT in countries where malaria is a serious problem is far stronger. To my knowledge there is no adequate substitute for the spraying of interior walls of houses with DDT solutions as an essential element in a malaria control program. I would oppose any policy of denying such countries the ability to purchase and use DDT for these purposes. The need of the future, however, is to develop less persistent and more specific pesticides that can be directed against a narrow range of target species, while doing little or no damage to others.

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Energy Equivalents

The recent interest in alternative energy sources—in particular, energy derived from solid wastes—has prompted what may be described as a new parlor game of quoting energy equivalents of the new sources. For example, the potential of a new fuel is frequently quoted as the equivalent of so many barrels of oil, implying that the new fuel could be so converted. The potential for deriving some form of energy from solid wastes expressed this way clearly ignores the laws of thermodynamics. To avoid error and confusion, I propose a new vocabulary.

I suggest that the product of the given mass of fuel, M, the heat of combustion, ΔH_c, and the conversion factor, F, that is commonly used to express the energy value in barrels of oil (or some similar newspeak unit) be termed the *arithmetic equivalence*, $M\Delta H_cF$, of the new source. This term could serve to alert audiences that scaling of new sources in such units does not mean, nor imply, that the new source could physically or chemically be converted to the form described by the unit.

Besides being used as a fuel directly (for example, in incineration) solid wastes may be converted to some other form by mechanical, chemical, or biological means to produce a unit of new fuel with a higher heat of combustion, $\Delta H'_c$. The conversion process will use energy, and the law of conversion of mass-energy dictates $M\Delta H_c \geq M'\Delta H'_c$ where M' is the mass of product fuel. Therefore, the *conversion equivalence* of feedstock M can be defined as $M'\Delta H'_cF$. Alternatively, the conversion equivalence may be defined as the arithmetic equivalence less the energy input and losses of the particular process. The two definitions of conversion equivalence differ slightly, particularly in ease of computation. However, the differences in value may not be great.

The new fuel source may be used as a supplement to, or substitute for, a commonly used fossil fuel, with or without passing through a conversion process. Examples would be raising steam in an incinerator using raw refuse, or in a suspension boiler designed for processed refuse-derived fuel, or using the fuel gas from pyrolysis of solid wastes. In a given application, the new form of fuel may operate with the same, greater, or lower efficiency than the fuel it is replacing. Thus the *substitution equivalence* is defined as the amount of fuel in the new form that must be used to replace the conventional fuel in a specific application. The substitution equivalence should be used as the basis for energy policy planning and not the arithmetic equivalence, as seemingly is done now.

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Misplaced Research

I wish to draw attention to the fact that Banting and Best worked at the University of Toronto and not at the University of Ontario, as indicated in Thomas H. Maugh's article on diabetes (Research News, 30 May, p. 920). A University of Ontario is nonexistent.

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Sounds of Science

This autumn quarter I will conduct, for the second time, a seminar entitled "Songs about science." I would appreciate donations of lyrics or music relating in any way to any area of science in its broadest sense. If you have taped or recorded material which you could reproduce, or if you want to sing or play some science songs, I will be pleased to send blank cassette tapes. There is relatively little professionally recorded music relating to science. Most science songs are written and performed by amateurs (usually students and scientists); few of these are ever published or recorded and, in general, are rapidly lost. Frequently these amateur works provide penetrating commentaries on science and scientists. Thus I feel that this rare art form should be collected, preserved, and studied.

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