Letters

Decay Time of DDT

In their article "Systems studies of DDT transport" (30 Oct., p. 503) Harrison *et al.* gave a fine description of a rather detailed mathematical model for predicting the ecological transport of DDT. However, due to lack of data, they make some assumptions on page 506 in order to arrive at estimates of the eventual decay time for DDT at various trophic levels. An important assumption in this analysis is that the organisms at each trophic level retain all DDT ingested. This assumption leads to rather long estimates of the decay times for organisms in the higher trophic levels.

We have arrived at a shorter estimate of the decay time, which we feel is somewhat more realistic, by using 1964 data on application rates of DDT and average levels of DDT in various foodstuffs. We have used the same type of mathematical formulation, that is, systems of simultaneous, ordinary differential equations) to develop a model for DDT transport. However, we have used existing data to develop our model rather than theoretically developing a model that requires data which do not exist. By assuming steady-state conditions (losses from the ecosystem balance the application rates) for 1964, our model shows that degradation of DDT in soil is the long-term factor controlling decay time. According to the data of Edwards and others (*1*), this decay time is on the order of 7 years, which corresponds to a 98.2 percent reduction of DDT (if all applications were halted) in about 27 years. On the basis of this steady-state assumption (which assumes ingested DDT is not indefinitely retained), all other decay times are faster and do not affect the long-term decay after approximately 5 years from the date of halting the DDT application.

The actual, long-term decay time for DDT may be somewhere between the 7 years we have estimated and the values of up to 100 years given by Harrison *et al.* Although metabolism of DDT is not extremely rapid, conversion of DDT into more polar metabolites and eventually into DDA, the excreted metabolite, does proceed in a number of species (*2*). Known intermediary metabolites of DDT such as DDMU [1-chloro-2,2-bis(*p*-chlorophenyl)ethylene], DDD, and DDE occur in all samples of environmentally acquired pesticide residues (*3*). Together these data support the hypothesis of an active metabolism and excretion of DDT in the environment by man, birds, insects, and fish (*2*). We would be more inclined to believe the 7-year decay time because it is based on actual data (although the interpretation may be questioned) and because the metabolism and excretion data conflict with the assumption which Harrison *et al.* make about complete retention of DDT.

SANFORD G. BLOOM
Battelle Memorial Institute,
505 King Avenue,
Columbus, Ohio 43201

D. B. MENZEL
Ross Laboratories,
625 Cleveland Avenue,
Columbus, Ohio 43215

References

1. C. A. Edwards, *Residue Rev.* **13**, 83 (1966); W. D. Guenzi and W. E. Beard, *Soil Sci. Soc. Amer. Proc.* **32**, 522 (1968); W. H. Ko and J. L. Lockwood, *Can. J. Microbiol.* **14**, 1069 (1968).
2. W. J. Hayes, Jr., W. F. Durham, C. J. Cueto, Jr., *J. Amer. Med. Ass.* **162**, 890 (1956); M. B. Abou-Donia and D. B. Menzel, *Biochem. Pharmacol.* **17**, 2143 (1968); D. B. Menzel, S. M. Smith, R. Miskus, W. M. Hoskins, *J. Econ. Entomol.* **54**, 9 (1961); G. L. Greer and U. Paim, *Fish. Res. Board Can. J.* **25**, 2321 (1968).
3. R. W. Risebrough, D. B. Menzel, D. S. Martin, Jr., H. S. Olcott, *Nature* **216**, 589 (1967).

I protest the publication of "Systems studies of DDT transport," an apparently refereed scientific article. I suppose we should be thankful that six professors and their lawyer are so concerned for the environment, but I cannot really believe that their end is properly served by "snowing" a public official or by the publication of such nonsense. The body of the article delivers very little on the subject promised by the title or the introduction. Instead, unsupported assertions are decorated with a flurry of jargon and largely irrelevant mathematics to produce propositions contrary to first principles and common sense. The "conclusions" given would not be objectionable as a statement of faith, but receive very little support from the rest of the article. If this is representative of the material presented in Wisconsin, a Boffey treatment in News and Comment would have been far more appropriate.

CLARE A. STEWART, JR.
407 Brentwood Drive, Carrcroft,
Wilmington, Delaware 19803

The approach we have taken in the development of our model has been based upon the physical principle of the "conservation of mass." The real importance of this type of approach is that the parameters significant to the system are decided by the theory associated with the principle. Models developed in this way can point to the absence of critical data and help to structure data acquisition programs.

Our "first-approximation" model (Eq. 13) assumed that DDT is a highly persistent substance and that it is neither metabolized nor excreted. However, an assumption that DDT is "lost" from any trophic level by *fixed* rates of metabolism or excretion would lead to a prediction of reduced equilibrium levels of DDT concentrations with no change in predicted response time. If any metabolism or excretion of DDT is functionally related to the concentration of the pesticide in a trophic level, then it is possible for the time constant, and thus the response time, of the system to be reduced.

There appears to be some misunderstanding as to our use of time constants for predictive purposes. We did not seek to estimate decay times for DDT at various trophic levels but, rather, the times required for a complete response (attainment of new equilibrium levels) to sustained inputs of DDT. In essence, we predicted that if all applications of DDT were to cease, concentrations of DDT in the higher trophic levels would continue to rise for some time due to the dynamics of the system.

It is true that different assumptions lead to different predictions and, as time passes, we would expect new in-

formation to become available which would permit more precise predictions of DDT flows and storages in ecosystems. However, with regard to assumptions, we would be interested in learning the reasons for Bloom's and Menzel's assumption that, in 1964, the losses of DDT from the ecosystem balanced the application rates.

For the benefit of those who may wish to make use of our equations, we would like to point out two errors in the equations as they were printed. In the first term of Eq. 9, $\dot{m}_i$ should be m_i. In Eqs. 13, 14, and 15, $\dot{m}_{i-1}$ should properly be $\dot{m}_{i-1,i}$.

H. L. HARRISON, O. L. LOUCKS
J. W. MITCHELL, C. R. TRACY
College of Engineering,
University of Wisconsin,
Madison 53705

Enlightened Employment Tactics

We commend Boris Magasanik for his letter (19 Feb.) encouraging qualified women to apply for positions in his department. He is correct in surmising that women are often discouraged from seeking positions in academic departments of excellent reputation in the certain knowledge that they will not be considered or, if considered, will be placed at extreme disadvantage relative to male competitors. It would be helpful if other department chairmen would similarly state their willingness to consider qualified candidates regardless of sex, by publishing statements in *Science* or elsewhere.

Recruitment for posts at this level is customarily informal and not pursued by public advertisement. Thus chairmen should also tell their colleagues of their willingness to consider applicants regardless of sex. Often when a woman allows her candidacy to be known via a third party, she is at a great disadvantage if this third party is ambivalent about women filling such posts. If more employers would publicize their intent to hire on the basis of qualifications alone, it would create a climate in which women would be encouraged to apply, directly or indirectly, and their colleagues would feel more free to recommend them.

LORETTA LEIVE
MAXINE F. SINGER
National Institute of Arthritis and Metabolic Diseases, Bethesda, Maryland 20014

M.I.T. Confrontation

Southwick's News and Comment article "Visitors ask M.I.T. faculty to renounce military research" (15 Jan., p. 156) describes demonstrations carried out in front of M.I.T. by members of the New York branch of Scientists and Engineers for Social and Political Action (SESPA). The article contains little news and rather biased comment. It is absurd to say that M.I.T. is probably the most important military research center in the United States. Seymour Melman is quoted as saying that M.I.T. has done nothing about the problems of conversion. In fact, M.I.T. and its faculty have been leaders in turning science to societal problems.

The demonstration was carried out, and knowingly so, at a time when M.I.T. was not in regular session. There was no notice of the event. According to the article, the Union of Concerned Scientists (UCS), of which I serve as chairman, was the "main target of the demonstrations." If so, it was an injudicious choice. It is far easier to confront your potential allies than the Pentagon. It is also far less productive. Several points in the article should be clarified and amplified:

I have no idea how many UCS members would work on a laser ABM, but I was prompted by the quote to inquire of a few nearby members of scientific persuasion. One conditional "yes" is worth noting. The member allowed that he might conceivably work on the laser ABM, but only on very cloudy days with air heavy with smog.

UCS has taken no position on the SESPA pledge not to participate in war research. We do not intend to. This point became clear when Melman, at his request, addressed an open meeting of UCS on 16 October 1970. The UCS members present held to the view that signing such a pledge is a matter for personal conscience, not collective intimidation.

There are circumstances when some of us would work on weaponry. We are convinced that now is not such a time. We devote our energies and our talents so that the time may never come.

The principal discussions of the 16 October meeting concerned the tactics used by SESPA in its confrontation with workers at the Riverside Research Institute in New York, an effort which Melman hoped we would emulate in Boston. The UCS members rejected those tactics which went beyond the picketing of the laboratory to the