derlie the discrepancy concerning proactive facilitation.

It might also be argued that the drug was operating on consolidation processes. Since it is generally assumed that consolidation is a progressive stabilization of a memory trace over time, our data suggest that this labile phase of memory lasts for at least 5 days. That there was a trend toward facilitation in the LD(1) group supports this contention. The same trend, however, appeared in the LD(5) group. Thus we would have to postulate not only that the labile phase persists for at least 5 days, but also that there is equipotentiality of the memory trace throughout this time period. These assumptions are not supported by previous investigations, which found no evidence of retrograde facilitation of memory when the drug was administered at intervals beyond several hours after training (1–3). Furthermore, the consolidation argument can be vitiated since neither the LD(1) nor the LD(5) group differed significantly from the control group in this study.

It is conceivable that the observed facilitation could be attributable to performance factors unrelated to memory. For instance, the repeated drug administration could have increased motivational, arousal, or attentional levels. Further, activity levels as well as sensitivity to the white and dark alleys could have been altered. If, however, any of these variables were responsible for the reported enhancement, it is likely that the naive groups given strychnine would have been similarly facilitated.

Since strychnine was facilitating only to animals that had received prior exposure to the maze, the most reasonable interpretation of these results is that strychnine influenced the long-term store of memory. There are several ways in which strychnine could have produced the observed enhancement. The drug could have strengthened the informational representation of the training experience or could have retarded the decay of that representation from the long-term store (prevention of forgetting). Alternatively, strychnine may have increased the accessibility of information in the long-term memory store (enhancement of retrieval processes). At present, these alternatives cannot readily be distinguished. It should be noted that some investigations of facilitation of memory have employed repeated administrations of the analeptic compound over many days.

The facilitation observed in some of these studies may be due in part, therefore, to the enhancement of the long-term store of memory. Finally, the most significant finding of this research is that the long-term store of memory is dynamic and susceptible to pharmacological manipulation.

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7. The LD_{50} of intraperitoneally injected strychnine sulfate for C57BL/6 female mice (60 to 90 days old) maintained in our animal colony is 2.5 mg/kg.
8. Latency on the first retention day varied significantly among groups ($F = 4.76$, d.f. = 5,100, $P < .01$). All of the groups that received strychnine displayed shorter latencies than the control group; however, latencies did not correlate with either error measure and, consequently, were not further analyzed.
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Atmospheric Circulation of DDT

In their article "DDT in the biosphere: Where does it go?" Woodwell et al. (1) present an admirable review of knowledge about the storage and transport of DDT [1,1,1-trichloro-2,2-bis(p-chlorophenyl)ethane] and a mathematical model to describe its movements. They conclude that DDT is ultimately stored in the depths of the ocean after evaporation into the atmosphere, transferal from the atmosphere to the ocean primarily by rainfall, and subsequent movement downward in the ocean, until dispersed.

The model as presented necessarily requires many assumptions, and the validity of their conclusions must be questioned on the basis that measurements of atmospheric concentrations of DDT, as judged by the one reference given on the subject (2), are completely incompatible with the requirements of the model. In order that DDT be transported as proposed, Woodwell et al. calculate that a worldwide circulation of DDT components, largely as vapor, must be present at a concentration of 80 ng per cubic meter of air, with a mean residence time of 4 years. These estimates are of course approximate, and local deviations could be expected with higher concentrations in the vicinity of areas where DDT is being applied and lower values in areas where cleaning of the atmosphere is occurring. Stanley et al. (2), in a survey of atmospheric concentrations, found *maximum* values at various locations in the range of 10 to 2000 ng of total DDT components per cubic meter of air, apparently largely particulate in nature and related to local applications. The background level, however, can hardly be more than the 2 ng/m³ observed repeatedly at one location (Dothan, Alabama) where significant agricultural applications are reported, or the 10 ng/m³ maximum observed at the Salt Lake City site. In about 20 percent of the measurements of Stanley et al., the concentration of DDT was less than the detectable limit of 0.1 ng/m³ (p,p'-DDT). If there is a long-term, worldwide circulation, it must be at a level which is not more than 10 percent, perhaps much less than 1 percent, of the predicted value. Even at this level of accuracy, the results could perhaps be considered a tribute to the general analysis made by Woodwell and

his co-workers, but the results are far from sufficient to justify the conclusion that most of the DDT ever present is being transported to the ocean as proposed. From the rest of the arguments given, it would be necessary to conclude that this is at most a small part of the answer to the question of where the DDT eventually goes. If, as they suggest, an appreciable fraction of the pesticide applied is initially evaporated into the air, then the low concentration observed requires that the mean residence time in the atmosphere is too short to permit one to speak of a worldwide circulation. Woodwell's earlier analogy (3) to the dispersal of pollen, where only a small fraction of the total material is dispersed on a global scale, would seem far more appropriate. The remaining material must either be destroyed in the atmosphere in some manner or be rapidly returned to earth to be destroyed, stored, or transported in some manner not accounted for in the model as presented.

At this point in time, it would be unreasonable to question that the use of DDT has done a great deal of good, protecting men and animals from insect-borne diseases and permitting increased production of food, or that it has also done harm, mostly by virtue of its ability to reduce the insect population, which is an important level in the food chain of wildlife, and also by virtue of its direct toxicity to fish and other species. These disadvantages are shared to a greater or lesser degree by any form of insect control. It is quite a different matter, however, to assert that the accumulation of DDT with continued usage is "causing spectacular declines in populations" of various species. If there is serious factual evidence that this is so, it would be appropriate for the authors to cite it separately from the many items of speculation, suggestion, and opinion which were provided (4).

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the same opinions but no evidence with any obvious bearing on this issue.
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Stewart's assumption that any complex analysis such as the one we presented on DDT is open to further interpretation is of course correct, and his questioning of the magnitude of aerial transport is appropriate.

The decay and transfer rates that we used in our analysis were derived from the literature as explained in detail in the article. The set of rate constants that appeared most reasonable to us resulted in a prediction of a peak DDT concentration in the atmosphere of about 80 ng per cubic meter of air in 1966. The data of Stanley *et al.* (1) appear to support our analysis in that concentrations of DDT in samples of air of nine U.S. cities in 1967–1968 commonly fell in the range from 1 to 100 ng of DDT residues per cubic meter of air, as we mentioned in our article. This span was obtained not from the table of maxima as Stewart asserts, but from a consideration of all of the data presented. Stanley *et al.* moreover found p,p'-DDT and o,p'-DDT in all localities and considered "typical" concentrations of DDT residues in air to be 10 to 30 ng/m³. Their report contains no explicit basis for the assertion of Stewart that "In about 20 percent of the measurements . . . the concentration of DDT was less than the detectable limit of 0.1 ng/m³." The data from Dothan, Alabama, which Stewart prefers, include only concentrations of p,p'-DDT, which may be from one-half to two-thirds of the total DDT residues as judged from other data presented. Fourteen selected analyses were presented for Dothan, spanning a range from 1.3 to 7.2 ng of p,p'-DDT per cubic meter of air. These might reasonably be interpreted as indicating a range of 2 to 12 ng of total residues per cubic meter of air, in partial support of Stewart's argument that "background levels" were lower than predicted.

The rates in greatest question in our model are the rate of transfer of DDT from the troposphere to the oceans and the rate of decay of DDT in the troposphere. Time constants (τ) of several months to 1 year for the transfer of DDT from the troposphere to the oceans and of up to 2 years for the decay of DDT in the troposphere, if used in conjunction with a combined

time constant for loss from the land by decay and vaporization of 3.6 years, give concentrations in the troposphere in 1966 in the range from a few to about 15 ng/m³. This modification scarcely invalidates the model; it shows its usefulness.

The fact of worldwide aerial transport of DDT residues seems hardly open to serious question. For the aerial worldwide pathway to be trivial as Stewart and others seem to wish, a small fraction of the DDT used would have to find its way into the atmosphere and its residence time in the atmosphere would have to be days as opposed to weeks, months, or years. This conclusion follows both from our analysis and from data on the rate of movement of radioactive fallout, which have shown that air parcels move around the world in mid-latitudes in 3 weeks to 1 month. Small radioactive fallout particles in the troposphere have a half-time of residence of 2 to 6 weeks (2). A careful reading of our article and the literature we cited should convince Stewart and others that the evidence at present strongly favors the idea that aerial transport is the major mechanism of worldwide transport for DDT residues. Nonetheless, there is an extraordinarily large gap here in our knowledge of the biosphere which should be embarrassing both to the purveyors of pesticides and other toxins and to those who chart the course of science.

Stewart's final comment on the effects of DDT seems to have been covered adequately in his own first two paragraphs. Our documentation of effects was more substantial than he acknowledges, and we refer him to the citations included in the article.

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