

tile anesthetics thus is required to provide a degree of exposure of the liver analogous to that accomplished after a single injection of phenobarbital.

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*To the Editor:*—Some statements by Liebman concerning the review article demand comment. In the first place, it is indeed lamentable that Liebman's work was not included in the review. He has, however, plenty of company, rather eminent company at that, and so should not be perturbed. Some things had to be left out; otherwise the review would have resembled a telephone directory.

Second, Liebman criticizes the hypothesis of Daniel (*Biochem. Pharmacol.* 12: 795, 1963) that an ethylene linkage may be formed transiently as an intermediate during trichloroethylene metabolism. Liebman criticizes this hypothesis because the ethylene compound has not been isolated. In its place he proposes the hypothesis that either an epoxide or glycol (or both) might be intermediate products prior to formation of the first stable product he could isolate. Since neither the epoxide nor glycol has been isolated either, Liebman's theory is an attractive hypothesis, as attractive as Daniel's; however, it is merely that, for it has never been demonstrated.

The next point Liebman makes is absolutely correct. The review should have emphasized that intramolecular translocation of chlorine atoms occurs during trichloroethylene but not during chloral hydrate metabolism.

Liebman's final comment is obscure in its relevancy to the subject under discussion: the ability of methoxyflurane to produce enzyme induction. Of course duration of exposure may be of importance. So? It is not at all surprising that Liebman's work on trichloroethylene induction was not quoted. It appeared eight months after the review had been finished.

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