out the essential points. They are not made superfluous by the historical part of the *Exposition du Système du Monde*, in which Laplace had given a more continuous narrative of the same history, treated with less technical detail and a greater display of eloquence; they still deserve to be consulted for any deeper study. Their first-hand assessment of the extremely active and fertile period extending from Newton's *Principia* to Laplace's own researches is particularly valuable. The reprint is made, not from the original edition of 1825, but from the typographically neater and more correct edition of Laplace's collected works, sponsored by the Académie des Sciences (1882). It is not only a useful addition to Bowditch's translation; it will also afford those many collectors and librarians whose set of the original work lacks the fifth volume a welcome opportunity of completing it.

L. ROSENFELD

Correspondence

DDT in Milk

SIR,—In the biased review on DDT by T. H. Jukes (*Nature*, **225**, 301; 1970), I was personally attacked and misquoted. The fact is that I was invited to the DDT hearing in Madison, Wisconsin, in May 1969 by the Assistant Attorney General of the State of Wisconsin, Robert McConnell, who was the public intervenor in the case.

The presence of DDT in human milk was first reported in 1951. The general public was, however, not told about DDT in human milk until I and others in 1968 and 1969 pointed it out. It was then also pointed out that breast-fed babies ingest more DDT compounds than the acceptable daily intake (ADI), and that the ADI was set without considerations to the long-term effects of DDT compounds on the metabolism of endogenous and xenobiotic compounds as well as to the carcinogenic action in mice of low DDT doses.

The review by Jukes is not even modest—it is meagre. He apparently thinks that possible negative effects of DDT only are acute toxicity symptoms—exactly as the arguments of those who have a vested financial interest that DDT is used indiscriminately—completely disregarding the possible late, long-term effects.

We know now some of the late effects of DDT compounds on wildlife; partly from multi-generational studies. Man has not yet been exposed to the organochlorine pesticides for a full lifetime. As we have very few anamnestic data, we will probably never be able to acquire the epidemiological variables which are necessary for an evaluation of the possible actual hazards.

The use of DDT and other organochlorine compounds has been so ingeniously advocated that we no longer have any proper controls. This is a sufficient reason to ban these almost persistent compounds completely. We may argue about the time to complete the de-escalation, be it one year, two or at most five years, but no other discussions are necessary.

Yours faithfully,

GÖRAN LÖFROTH

University of Stockholm,
Stockholm, Sweden.

Enzymes in Crystals

SIR,—In response to the recent article by your Molecular Biology Correspondent on "Enzymes in Crystals" (*Nature*, **225**, 225; 1970), we would like to clarify his statement concerning the "pardonable satisfaction" with which Professor Lipscomb is said to "contradict with confidence" the amino-acid sequence of carboxypeptidase A as deduced in our laboratory (Bradshaw *et al.*, *Proc. US Nat. Acad. Sci.*, **63**, 1389; 1969). While the interpretation of the X-ray data by Professor Lipscomb and co-workers (Lipscomb *et al.*, *Proc. US Nat. Acad. Sci.*, **64**, 28; 1969) may contradict three points in the chemical sequence, we, in turn, take a certain degree of satisfaction in being able to supply unequivocal chemical evidence in support of our reported assignments.

It has been established that there are two forms of the enzyme differing at three sites in the polypeptide chain (Pétra *et al.*, *Biochemistry*, **8**, 2762; 1969). Professor Lipscomb believes—solely on the basis of X-ray assignments—that the enzyme used in his studies was of the valine type (Pétra and Neurath, *Biochemistry*, **8**, 2466; 1969). Taking this report at face value, we have prepared pure valine enzyme, by chromatographic means, in order to avoid the allotype controversy. An analysis of the tryptic peptides was conducted specifically to re-examine the residue in position 151. In agreement with our previous report, we have found this residue to be phenylalanine, whereas Professor Lipscomb and co-workers interpreted it to be tryptophan. Although re-examination of the other two sites (residues 93 and 245) in genetically pure enzymes has not been made, there is no question that in the pooled enzyme preparations used in the original studies the chemical identification is correct. At worst, these discrepancies may be due to allotypic variations not previously uncovered.

We do not enjoy a polemic of the relative merits of X-ray and chemical methods for the determination of the structure of an enzyme since, in the final analysis, a combination of both of these methods is the most expeditious approach to the total determination of structure. We feel compelled, however, to set the record straight, to avoid misinterpretation of the report of your Molecular Biology Correspondent.

Yours faithfully,

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and
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Our Molecular Biology Correspondent writes:

It was certainly not my intention to whip up from the ringside a contest between the sequencers and the crystallographers. And to be fair, Lipscomb and his colleagues did firmly make the point that sequencing by X-ray structure analysis is not on, and probably never will be. It is interesting, just the same, to see how many errors in sequences have been uncovered by crystallographic studies, and indeed how few sequences since insulin have proved correct in every detail—which proves merely that sequencing is still an arduous trade. Professor Neurath's interesting letter shows that there can be other problems than the purely chemical to make life hard for sequencers and crystallographers alike. At the same time there is clearly a conflict still to be resolved between the chemical and crystallographic data. Meanwhile, to set the record straight, Professor Neurath's satisfaction is no whit less pardonable than that of Lipscomb *et al.*

Sternglass's Assumptions

SIR,—Lindop and Rotblat state in their article in *Nature* (**224**, 1257; 1969) that they see no reason why the logarithm of infant mortality should decrease linearly with time; in fact, such a law follows from the simplest of hypotheses. The probability S of an infant surviving a given year is