Periodic extinctions undermined

Last year's fashion for explaining a supposed 16-million-year periodicity in mass extinctions of species has been made to seem a little spurious. Defining a mass extinction is a more urgent need.

THOSE who, last year, were enthusiastically speculating on the mechanism that might have been responsible for mass extinctions of living things at intervals of 26 million years will be given uncomfortable second thoughts by the article by Antoni Hoffman on p. 659 of this issue. For on the face of things, Hoffman has undermined the assumption on which all the excitement was based, the belief that there is a 26 million year periodicity to be explained. But human nature being what it is, it seems unlikely that the enthusiasts for catastrophism will now abandon their quest.

What Hoffman has done is to re-examine the palaeontological data on which rests the supposition that there is, or at least has been, a regularity in the timing of mass extinctions. The data consist of a catalogue of disappearances from the fossil record of families of marine animals compiled by J.J. Sepkoski and published, during 1982 and 1983, in the journal put out by the Milwaukee Public Museum. The first analysis of this material appeared in February 1984, in a paper by D.M. Raup and J.J. Sepkoski (*Proc. natn. Acad. Sci., U.S.A.* **81,** 801-5; 1984). It advanced the conclusion that mass extinctions of marine animals have taken place at times in the geological past separated by intervals of 26 million years or an integral multiple thereof.

That Sepkoski's original data may not be robust enough to support this dramatic conclusion has always been a possibility, as Raup and Sepkoski themselves acknowledged. The most obvious difficulty is the unavoidable uncertainty in assigning absolute ages to the successive geological stages recognizable only by the fossil assemblages that define them. For the past 100 million years, including the Cretaceous/Tertiary transition at 65 million years, the dating of the stages is reasonably good, with the result that most geological chronologies are consistent during that period, but disagreement is rife for earlier times. Raup and Sepkoski gave reasons, in their paper, for adopting the chronology due to W.B. Harland; the first column (labelled **a**) in Hoffman's Fig. 1 shows how the times of mass extinctions would have been changed if they had followed another equally plausible chronology.

The nub of Hoffman's argument is, however, more subtle. Because palaeontological stages are defined by their fossil assemblages, each must differ from its predecessor and its successor by the appearance or the extinction of at least one family. So, Hoffman argues, distinguishing between a normal stage and one that qualifies as one of mass extinction requires the consistent application of some objective criterion. He would prefer a criterion based on the rate or probability of extinction per unit time (say a million years) rather than one based on counting the number of family extinctions in a stage, which he says is biased towards periodicity.

So much is evident in the work of Raup and Sepkoski, where it is a necessary condition for associating a particular stage with a mass extinction that the number of families extinguished should be greater than in the preceding and succeeding stages. The effect of that is to exclude the possibility that consecutive stages could be associated with mass extinction, which is bound to prejudice the analysis towards a periodic interpretation. Indeed, Hoffman argues, if the probabilities of extinction in successive stages are entirely independent, simply looking for peaks in the record of extinctions implies a one-in-four chance that any stage will be recognized as a time of mass extinction and, given the exclusion of the possibility that consecutive stages may be thus described, the analysis is certain to yield the conclusion that, on the average, extinction peaks occur every four stages. This, says Hoffman, is the origin of Raup and Sepkoski's periodicity.

The chief effect of this argument will be to emphasize how tricky is the concept of mass extinction. To be fair, many of the pitfalls were acknowledged by Raup and Sepkoski. The plain truth is that the disappearance of one or several families of genera from the fossil record cannot be counted as proof of a mass extinction, in the ordinary meaning of that term, without much more detailed scrutiny of the data than mere enumeration. How abrupt was the transition? Did all species disappear simultaneously, or can something be learned of the nature of the extinction from the sequence in which they disappeared? And since there are many possible causes of the extinction of species, genera and even whole families, ranging from evolutionary competition and climate change to external influences such as are involved in cometary or other impact, what can be said about the mechanism? The need to give at least partial answers to these questions should now be plain.

Where does this cautionary tale leave the explanations constructed to account for periodic extinctions, particularly for that which seems to have carried off the dinosaurs at the Cretaceous/Tertiary boundary? One conclusion that stands out from Hoffman's argument is what he calls the Maestrichtian stage at the end of the Cretaceous was, by all the different criteria applied, an extinction period.

Thanks to the work of the Alvarezes, father and son, and their associates, this is also recognizable by the world-wide presence of material rich in iridium at the physical boundary between the Cretaceous and the Tertiary. The most compelling evidence that this material came from the impact of some extraterrestrial object with the Earth is the finding of cracks caused by explosion stresses in a high-pressure phase of quartz associated with the iridium layer (B. F. Bohor *et al. Science* **224**, 867–8; 1984). The case for this impact is strong (and would be even stronger if there were some trace of where it may have happened), but cautious people will insist that there is as yet only circumstantial evidence that the impact caused the extinction.

By contrast, the explanations of periodicity in the pattern of extinctions assume a somewhat academic character, which is not the same as saying they are uninteresting. The most plausible source of material hitting the surface of the Earth is the distant ring of comets called the Oort cloud, from which material may be ejected towards the inner Solar System either by the perturbations of an unseen companion of the Sun (called Nemesis) or by perturbations of the Solar System caused by its periodic passage through the plane of the Galaxy.

Last year's rush of explanation has indeed thrown light on questions such as the stability of the Oort cloud, and of distant (and hypothetical) elements of the Solar System. But the case for Nemesis has now wilted. And those who are attached to the notion that extinctions are caused by the oscillations of the Solar System above and below the plane of the Galaxy, among whom Clube and Napier (see *Nature* **283,** 455; 1979) seem to be the first, might now usefully turn the problem around, and ask what happens to the Solar System on these excursions through the galactic plane.

John Maddox

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