Letters

Periodicity of Extinctions

While discarding my criticisms (*1*) of Raup and Sepkoski's hypothesis of 26-million-year periodicity of mass extinctions (*2*), Roger Lewin (Research News, 16 Aug., p. 640) does not consider the main point I raised (and in fact emphasized in the title of my paper). I indicated that each definition of mass extinction is arbitrary and demonstrated that, depending on the extinction metric and geological time scale employed in the analysis of family-level data on the marine record, a very wide variety of geological stages appear as mass extinctions. Yet the definition of mass extinction under which the support for the periodicity hypothesis has been found runs counter to the common usage of the term. It accepts all peaks of extinction intensity, and not only truly major events (like Late Permian or Late Cretaceous), as mass extinctions. Why should we wish to accept this particular—counterintuitive—definition rather than any other? For instance, one of the definitions employed by Raup and Sepkoski in their earlier papers.

It would be inappropriate to repeat here the arguments I made elsewhere, but the list of misrepresentations and omissions in Lewin's biased account is frustratingly long.

1) The effect of data culling by Raup and Sepkoski is not only to introduce incompatibility of the early and late events in the record, but first of all to arbitrarily relegate some peaks of extinction (Guadelupian, Pliocene) beyond the scope of the analysis, and to shift others from one stage to another (Middle and Late Eocene, Middle and Late Miocene).

2) Lewin writes that "even when the data set is maintained intact the 26-million-year signal still emerges, though less sharply," yet gives no reference to the work he cites. I am unaware of such results.

3) In any event, Lewin might also note that if my arguments on the effect of definition and time scale are correct, this is exactly the expected result! Thus, it obviously cannot invalidate my arguments.

4) I do not argue, as Lewin suggests, that the alleged 26-million-year signal comes through because of uncertainties in the absolute dating of the geological time scale, but rather because the approximately constant time-interval duration and the definition of mass extinctions as all peaks of the curve bring about the pattern analyzed by Raup and Sepkoski. Thus, Lewin knocks down a straw man instead of the actual proposition.

5) There is more to the problem of stage duration than Lewin's remark that "many [stages] are in the region of 6 to 7 million [years in duration]." Sixty percent of the stages, which cover jointly 70 percent of the total time interval, analyzed for periodicity in mass extinctions are assumed to range from 5 to 7 million years. If these stages are assumed to vary in duration within the limits of analytic errors on their dating, the appearance of periodicity disappears.

6) Nowhere do I argue that any single stage has a 0.25 probability of standing out as a "major extinction," but merely that it has such a chance of being a peak, that is, having a greater (however slightly) proportion of extinct families than the adjacent stages.

7) Contrary to Lewin's assertion, I do not argue that a "clear 26-million-year cycle" is likely to emerge from a "random distribution of extinctions between stages." I only indicate that a pseudo-periodic pattern is likely to occur which, given the statistical noise in the data, may be indistinguishable from such a periodicity. Lewin does not mention the article by Kitchell and Peña [published in *Science* (*3*) and cited in my paper] which shows that such an outcome is quite likely indeed.

8) The corollary of Lewin's consideration is that in Raup and Sepkoski's analysis, "random distribution was the null hypothesis, which was statistically rejected." Thus, he implies, the random walk model I suggest was refuted at the outset. Lewin does not note that there are different random distributions. The random distribution analyzed by Raup and Sepkoski was generated within the constraints of their definition of mass extinction and geological time scale. The only sentence in their original paper possibly relevant to my argument about the random walk nature of their pattern reads: "The time series of extinction data shows somewhat fewer peaks than would be predicted were it a random walk" (*2*, p. 803). Given the random walk I suggest, this statement is incorrect because the actual number of peaks is greater, not smaller, than predicted.

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References

1. A. Hoffman, *Nature (London)* **315**, 659 (1985).
2. D. M. Raup and J. J. Sepkoski, *Proc. Natl. Acad. Sci. U.S.A.* **81**, 801 (1984).
3. J. A. Kitchell and D. Peña, *Science* **226**, 689 (1984).

It would be fruitless to repeat my criticisms, which I stand by as a fair and accurate assessment of Hoffman's paper. Interested readers will wish to scrutinize all the literature very carefully.

—ROGER LEWIN

Destroyers by the Pound?

Daniel E. Koshland, Jr.'s, recent contribution to particle economics (Editorial, 2 Aug., p. 429) is impressive. However, certain problems that he raises about the pricing of a destroyer have, if memory serves, been addressed by Russell Baker, who observed that, over time, everything in the United States converges to a fixed price of $3.22 per pound. Bread and peanut butter are still a little below this figure, but rising fast, while computers and automobiles are falling to this level from above the line. The increasing size and weight of a fully outfitted modern destroyer as a function of commission date gives the budget advocate a distribution of costs to choose from, all of which, however, could be normalized.

Further, it seems reasonable to suggest that the Department of Defense use $3.22 per pound as a figure of merit in their procurement process. If the cost of any item should exceed this figure by more than an order of magnitude, then a routine check could be made to assess the efficacy of the purchase.

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Erratum: The last sentence of the report "Crassulacean acid metabolism in the strangler *Clusia rosea* Jacq." (6 Sept., p. 969) was incorrectly printed. It should have read, "The 1985 expedition was led by I. P. Ting and J. Hann, with L. Blose, R. E. Bonning, J. C. Broyles, D. W. Stewart, and J. A. Zabilski as participants."