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The Case for Extraterrestrial Causes of Extinction [and Discussion]

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The case for extraterrestrial causes of extinction

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The dramatic increase in our knowledge of large-body impacts that have occurred in Earth's history has led to strong arguments for the plausibility of meteorite impact as a cause of extinction. Proof of causation is often hampered, however, by our inability to demonstrate the synchronism of specific impacts and extinctions. A central problem is range truncation: the last reported occurrences of fossil taxa generally underestimate the true times of extinction.

Range truncation, because of gaps in sedimentation, lack of preservation, or lack of discovery, can make sudden extinctions appear gradual and gradual extinctions appear sudden. Also, stepwise extinction may appear as an artefact of range truncation. These effects are demonstrated by experiments performed on data from field collections of Cretaceous ammonites from Zumaya (Spain). The challenge for future research is to develop a new calculus for treating biostratigraphic data so that fossils can provide more accurate assessments of the timing of extinctions.

INTRODUCTION

Ever since Alvarez *et al.* (1980) reported anomalously high iridium concentrations at the Cretaceous–Tertiary (K–T) boundary, intense controversy has surrounded the question of a large-body impact (asteroid or comet) as the primary cause of the terminal Cretaceous mass extinction. Although the K–T problem and iridium anomalies still attract considerable attention, the investigations have broadened to include other geochemical and geophysical evidence of impact and other extinctions in the Phanerozoic. This has stimulated, in turn, new research on the nature of the mass extinctions and on other possible extinction mechanisms, including the effects of sea-level- and climate change and of episodes of intense volcanism.

The question of impact as a possible cause of mass extinction has become extraordinarily complex. Several hundred research papers have been published on the subject since 1980, and commentaries number in the thousands. The literature has thus become unmanageably large, so that a comprehensive review is impossible. More important, the subject involves so many separate scientific disciplines – from palaeontology to astrophysics – that no one individual is competent to judge the merits of all the arguments and counterarguments. Because of this, I refrain from trying to summarize the evidence for the extraterrestrial interpretation of specific extinctions. Instead, the reader is referred to recent reviews by Alvarez (1987) and Hallam (1987); these papers summarize large segments of the basic evidence.

Despite the problems just mentioned, one can easily make a case for the plausibility of large-body impact as a cause of mass extinction.

PLAUSIBILITY ARGUMENTS

Virtually all palaeontologists agree that there have been intervals during the Phanerozoic of unusually high extinction rates. Although the mass extinctions of the late Ordovician, late

Devonian, late Permian, late Triassic and terminal Cretaceous are undoubtedly the largest such events, several of the lesser extinctions are more intense and pervasive than can be explained simply by chance variation in background extinction. Furthermore, these events (or intervals) generally affect organisms over broad geographical areas and encompass many habitats and modes of life. Differences among extinction events exist but are remarkably minor (see Raup & Boyajian (1988) for documentation).

The pervasiveness of the larger extinction events makes it difficult to sustain explanatory mechanisms based on purely biological factors, such as interspecific competition, predation and the like. On the other hand, relatively rapid changes in the physical environment do offer plausible, albeit not necessarily compelling, candidates to explain mass extinctions.

Although an extinction event need not be truly worldwide to produce a significant peak in the Phanerozoic extinction record, a disturbance of substantial geographical extent seems to be required. Among the many ways of producing large-scale environmental perturbation, impacts by asteroids and comets must be considered among the plausible candidates for two reasons: (1) large-body impacts have been common throughout Phanerozoic time; and (2) large impacts have pronounced global effects on atmospheric chemistry, insolation and other relevant environmental factors.

The impact rate over the past 600 million years is reasonably well known. Knowledge of this phenomenon, and its historical record, has increased tremendously in the past three decades with the advent of satellite observations of ancient impact structures, the discovery of geophysical, mineralogical and geochemical indicators of impact, and the increased knowledge of asteroids now in Earth-crossing orbits. Contrary to the conventional wisdom of earlier decades, bombardment of the earth by large objects was not limited to the early Precambrian but rather, has continued at a moderate level to the present day.

Current estimates of the Phanerozoic flux of large impacting objects call for an average of one object of 10 km diameter or larger every 100 million years, and of one object of 1 km or larger every 200000 to one million years (Shoemaker *et al.* 1988)

Although much excellent work on the environmental effects of large-body impact has been done since 1980, many questions remain to be answered. All workers agree, however, that the physical consequences of an impact of one of the larger objects would be severe, many orders of magnitude greater than the effects of a simultaneous detonation of all nuclear bombs and warheads now in existence. Much attention has been given to the debris cloud that would be produced by a large impact and this cloud could indeed be an important element. But recent work on the effects on atmospheric chemistry suggest yet more severe environmental consequences (Prinn & Fegley 1988; Crutzen 1987).

The foregoing discussion is not intended to argue for impact as a better explanation of mass extinction than any other. Its purpose is merely to show that a causal link between impact and extinction is entirely plausible and that this link should be evaluated equally with other explanations. Furthermore, the impact explanation is eminently testable. Although some of the evidence for large-body impact at the K–T boundary is challengeable, such as the putative microtectites and some of the trace element and shocked-quartz data, geology has at its disposal several virtually ironclad indicators of impact. The verification of more than 100 large craters through impact melts, high-pressure minerals (stishovite and coesite) and shatter cones argues for the feasibility of testing proposed extinction–impact pairs (see Grieve 1987). Also, where impacts melts are available, extremely accurate radiometric dating is straightforward.

Stratigraphic problems

In the controversies surrounding the search for causes of mass extinction, palaeontologists are continually being asked to provide data on the details of particular extinction events. This has been especially true for the Cretaceous–Tertiary transition, but it applies throughout the Phanerozoic. Common questions include the following.

1. Are the extinctions simultaneous (sudden) or are they spread out over an extended time (gradual)?
2. If the extinctions appear to be gradual overall, is the pattern punctuated by smaller events: the simultaneous extinction of small groups of species?
3. Does a stratigraphic gap between the last occurrence of a group of fossils and a major boundary indicate that the organisms died out before the boundary event?

If mass extinctions are gradual, extending over a considerable time, some of the proposed causes of extinction are not tenable, such as a single comet or asteroid impact. On the other hand, if it can be shown that many species died out in a relatively short time, the idea of a single impact becomes viable, albeit not proven. If mass extinctions are punctuated by 'steps', then proposals of extended comet showers are plausible (Hut *et al.* 1987). Thus stratigraphic data become vitally important in the evaluation of extinction mechanisms.

It has been customary for palaeontologists to accept the first and last occurrences of a species (or larger group) as a literal record of origination and extinction, even though it is widely recognized that the observed ranges of fossils are often truncated by failure of preservation or lack of discovery. As is shown by examples in this paper, range truncation can make a gradual extinction appear sudden or a sudden extinction appear gradual. Until palaeontologists develop the methodology needed to cope with this problem, the contributions of stratigraphic data to our knowledge of mass extinction will be severely limited.

In the sections that follow, I illustrate the problems just mentioned by developing a case study from data on occurrences of late Cretaceous ammonites at Zumaya in northeastern Spain. Most of the phenomena I describe are well known to palaeontologists and stratigraphers, but the formulation may help to focus attention on those situations most in need of new methods of analysis.

Zumaya ammonites

Figure 1 shows the ranges of 21 ammonite lineages in the 200 metres below the K–T boundary at Zumaya. The data were provided by P. D. Ward (personal communication, April 1988) and are the result of exhaustive collecting and taxonomic work over several years by Ward, in conjunction with J. Wiedmann, J. Mount, and W. J. Kennedy. The data are an update of a similar chart published by Ward *et al.* (1986) and are based on extensive new collections, taxonomic revision, and re-identification. Only material actually in Ward's possession or field identifications made with certainty by Ward are included. Although these studies have been extensive, new collecting will undoubtedly modify details of the record. Future additions and corrections are not relevant to my purpose here, however, because the value of the Zumaya dataset is that it typifies problems common throughout the stratigraphic record.

The 21 lineages in figure 1 are considered by Ward to be separate evolutionary lines, although preservation does not allow specific or subspecific identification in all cases. The

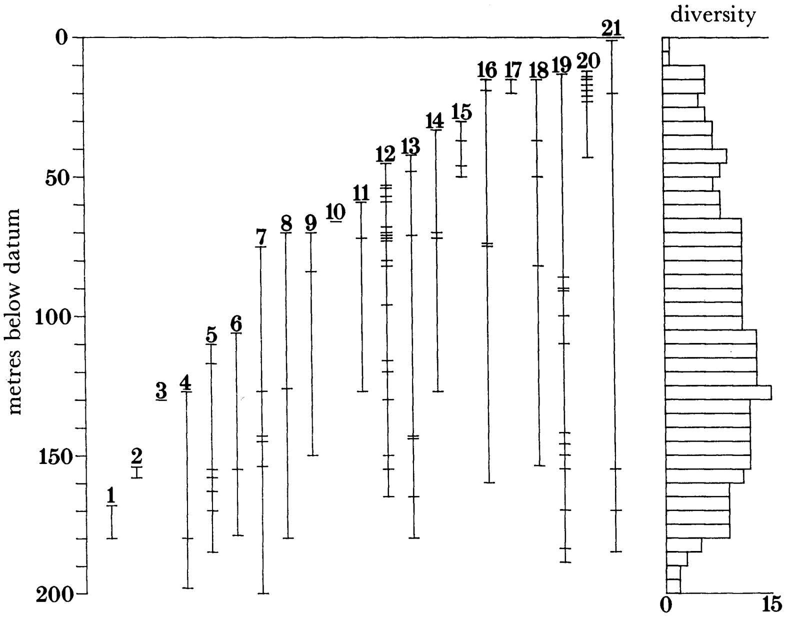

FIGURE 1. Stratigraphic ranges for 21 ammonite lineages at Zumaya, Spain (data provided by P. D. Ward, April 1988). The lineages are defined in table 1. The vertical scale is in metres below the Cretaceous–Teriary boundary at Zumaya. Fossil occurrences are indicated by horizontal tick marks. The histogram on the right shows the changing number of lineages through the section.

TABLE 1. AMMONITE LINEAGES RECOGNIZED BY WARD IN THE 200 m BELOW THE CRETACEOUS–TERTIARY BOUNDARY AT ZUMAYA, SPAIN

(Lineages are numbered in order of last occurrence (see figure 1 for range chart).)

1. *Baculites anceps*
2. *Hauericeras rembda*
3. *Desmophyllites larteti*
4. *Pachydiscus epiplectus*
5. *Pseudophyllites indra*
6. *Phylloptychocerus sipho*
7. *Pachydiscus neubergicus*
8. *Anagaudryceras* cf. *A. politissimum*
9. *Hypophylloceras surya*
10. *Fresvillia constricta*
11. *Pachydiscus jacquoti*
12. *Glyptoxoceras subcompressum*
13. *Diplomoceras cylindraceum*
14. *Pachydiscus gollevillensis*
15. *Hoploscaphites constrictus crassus*
16. *Saghalinites* sp.
17. *Vertebrites kayei*
18. *Phyllopachyceras forbesianum*
19. *Baculites* sp.
20. *Anapachydiscus fresvillensis*
21. *Neophylloceras ramosum*

identifications, as provided by Ward, are given in table 1. For convenience, the lineages are arranged in order of last occurrence and numbered accordingly.

In all, 150 ammonite specimens were identified. Of these, 44 specimens are redundant in the sense that other specimens of the same lineage were found at the same horizon. This leaves 106 occurrences, if an ‘occurrence’ is defined as one or more specimens of a lineage at a horizon. The 106 occurrences are indicated by the horizontal ticks in figure 1.

The histogram on the right in figure 1 summarizes lineage diversity through the 200 m section. Diversity increases from near zero at the base to a maximum of 15 coexisting lineages at the 125–130 m level. Diversity then declines to zero as the K–T boundary is approached. The low diversity near the base of the section is due to lithofacies: below 200 m, the rocks are dominantly turbidites and contain few ammonites (P. D. Ward, personal communication, 1988). Of greatest interest, of course, is the long upward decline in number of lineages towards the K–T boundary. This decline has been used by several authors as evidence that the ammonites were not suddenly and simultaneously eliminated by the K–T boundary (Ward *et al.* 1986; Wiedmann 1987; but see Ward & MacLeod (1988) for a different view).

Although the extinction pattern at Zumaya is highly relevant to the question of the Cretaceous mass extinction, the dataset also provides a good testing ground for more general aspects of the analysis of stratigraphic range charts. After considering some of these, I return to the question of the gradual extinction of ammonites at Zumaya.

THE HIATUS EFFECT: GRADUAL EXTINCTIONS APPEAR SUDDEN

It is well known that an erosional unconformity, a gap in sedimentation, or a failure of fossil preservation can simulate a sudden extinction. This can be illustrated with the Zumaya range chart by performing a simple experiment: ignore all fossil occurences for an arbitrary thickness of sediment. This is done in figure 2 using an algorithm that eliminates all fossil occurrences between 25 m and 125 m. The remaining ranges are shown as vertical lines connecting the first and last occurrences that survived the experiment (that is, those above 25 m or below 125 m). The lineages have been rearranged to match the new sequence of last occurrences, but the numbering used earlier is maintained so that lineages can be identified. Also, a histogram of last occurrences (apparent extinctions) is shown on the right.

Figure 2 has several interesting features. The ranges of some lineages, such as 1 (on the left) and 21 (on the right) are unaffected by the artificial hiatus because their actual first and last occurrences lie outside the hiatus interval. The ranges of most other lineages are profoundly

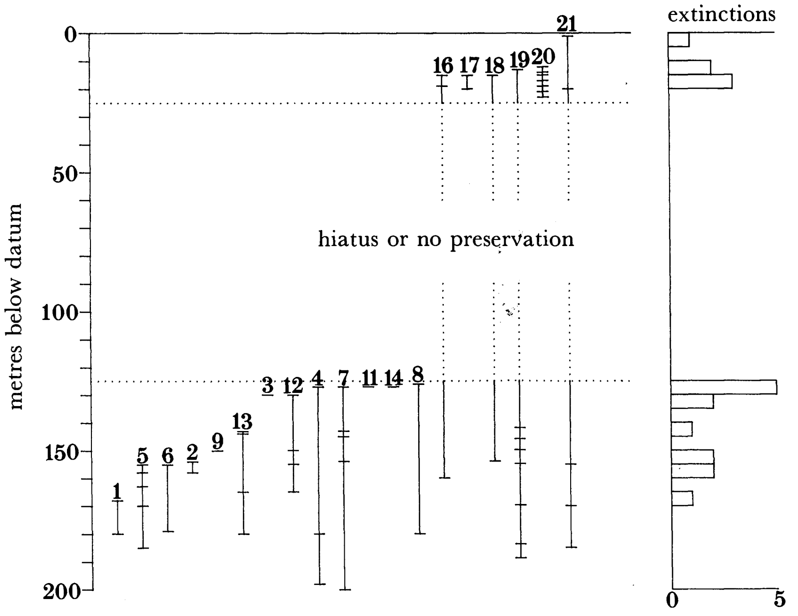

FIGURE 2. Experiment with the effect of imposing a preservational gap in the Zumaya range chart. The pattern is that which would be produced if all fossil occurrences between the 25 and 125 m levels were ignored. Dotted lines indicate Lazarus taxa: those taxa that disappear at the hiatus but reappear above the hiatus. Note that the hiatus produces the appearance of a sudden extinction at the 125 m level even though the full dataset (figure 1) contains no such event.

affected, however, by the loss of information in the 25–125 m interval. For example, the top of lineage 5 drops from 110 m to 155 m because of the loss of the two occurrences in the hiatus interval.

The main effect of the range truncation is to produce a concentration of apparent extinctions close to the base of the hiatus interval (lineages 3, 12, 4, 7, 11, 14 and 8). This is reflected in the histogram of extinctions. Thus we have the appearance of a substantial extinction event at about 125 m even though we know from figure 1 that no such event occurred.

If the hiatus (or interval of no preservation) were not recognized, the pattern in figure 2 could be interpreted as a sudden extinction. In this particular case, the presence of several Lazarus taxa (see Jablonski 1986; McGowran 1986) provides evidence of the hiatus: lineages 16, 18, 19 and 21 are present below the hiatus and reappear above the hiatus.

The pattern produced by the hiatus experiment mimics the real-world situation of brachiopods in the K–T section at Nye Klov (Denmark). In range charts published by Surlyk & Johansen (1984), the brachipods show a gap in occurrence (documented by Lazarus taxa), immediately above the K–T boundary, and it is impossible to determine whether the termination of species at the K–T boundary is real or merely an artefact of range truncation (see Raup (1987) for discussion). The proposition of sudden and simultaneous extinction is plausible but no more so than the opposing proposition that extinction was gradual throughout the first few metres of the Tertiary.

A substantial gap in the fossil record does not necessarily produce a pattern simulating sudden extinction. In repeated simulations of the type shown in figure 2, spurious extinction events are common but not universal: the outcome in each case depends on the vagaries of the distribution of occurrences above and below the specified hiatus interval.

The Signor–Lipps effect: sudden extinctions appear gradual

In an important paper, Signor & Lipps (1982) analysed the consequences of limited sampling of the fossil record. In particular, they noted that range truncation often leads to a backward smearing of extinction events and this was later labelled the Signor–Lipps effect (Raup 1986). The work of Signor & Lipps was largely model based and relied primarily on monte-carlo simulations. It is appropriate to explore the phenomenon further by experiments on the Zumaya ammonite dataset.

Figure 3 shows the consequences of imposing a complete and instantaneous extinction of all Zumaya ammonites at the 100 m level. It is assumed, for the experiment, that all ammonites died out at the 100 m level and consequently, all occurrences above this level are ignored. The real occurrences below 100 m are used to construct the range chart as it would appear under these circumstances. The 100 m level was chosen, with reference to the raw data in figure 1, because there was high diversity and no extinction at this level.

The question for this experiment is: what would the preserved record of a sudden annihilation of all lineages look like? The striking result is that the extinctions appear to be gradual, as shown by the histogram of diversity on the right side of figure 3.

The reason that a sudden extinction produced a gradual decline in diversity is obvious from the original data. The stratigraphic tops of all lineages that had originally extended above the extinction horizon (100 m in this case) were truncated back to their last pre-extinction occurrence.

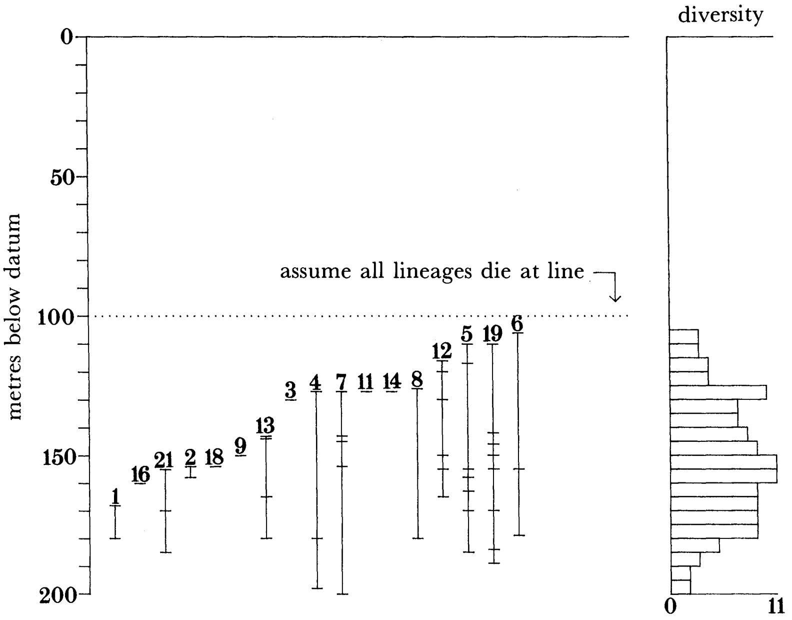

Figure 3. Experiment showing the effect of eliminating all fossil occurrences above the 100 m level, simulating the sudden extinction of all lineages at that level. Diversity declines gradually toward the extinction horizon even though the extinction is 'known' to have been sudden. Also, a spurious extinction 'step' appears at the 125 m level.

This experiment shows another kind of artefact that calls for special attention. Note that at about the 125 m level, six lineages terminate almost together (3, 4, 7, 11, 14 and 8). There is nothing in the full dataset to suggest a stepwise extinction and the 'event' is 25 m below the imposed extinction. The step is an indirect result of the 100 m extinction because all higher occurrences of four of the six lineages happen to be above the 100 m level (see figure 1).

It is clear from this experiment that a true extinction in the fossil record may produce a spurious extinction lower in the section. In repeated simulations of this type, with extinctions imposed at different horizons, spurious stepwise extinctions are extremely common.

Discussion

The experiments with the Zumaya dataset have several clear implications, as follows:

(1) a hiatus in fossil preservation often produces the appearance of a sudden extinction where none exists;

(2) extinctions, known to be sudden, often appear to be gradual;

(3) stepwise extinction is often an artefact of the placement of fossil occurrences.

The spurious stepwise extinctions produced by the experiments have further implications. These non-events usually occur at horizons having natural concentrations of fossils rather than at times of true extinction. Given a true extinction event, ranges are truncated back to the next lower horizon of fossil concentration. Only if the fossil concentration happens to coincide with the extinction, is the stepped event an indicator of the extinction. In the general case, a true stepwise extinction event is unlikely to be preserved as such; rather, it is most likely to be smeared backwards in time because of variable range truncation among the involved lineages.

Generality of the Zumaya results

The experimental results presented here are caused by the fact that the stratigraphic ranges at Zumaya are based on discontinuous and irregular sequences of fossil occurrences. To the extent that this condition is common throughout the fossil record, the same patterns will be found regardless of the age or kind of organisms used.

The distortions found in the Zumaya experiments can be avoided only if fossil preservation is virtually continuous. It follows that palaeontological situations more continuous than Zumaya are less likely to distort true patterns of extinction. Conversely, situations with less continuous preservation are more likely to distort true extinctions. The two extremes may be found typically in marine microfossils and terrestrial vertebrates, respectively. Thus an observed pattern of extinction has more credibility if the underlying fossil record is nearly continuous. It should be noted, however, that a nearly complete fossil record does not protect against the tendency for an unrecognized hiatus to produce a spurious extinction event (figure 2). In fact, the more continuous the fossil record, the sharper the extinction produced by a hiatus will be, as long as species are dying out during the hiatus interval.

Towards a new calculus

The foregoing discussion is non-rigorous in the sense that it documents tendencies and typical outcomes: it does not provide the means to assess the likelihood that a given extinction is what it appears to be. In the case of the late Cretaceous disappearance of ammonites at Zumaya, it is important to be able to deduce whether the extinctions at or below the K–T boundary were actually gradual or merely an artefact of the distribution of last occurrences.

The past few years have seen a dramatic increase in the quantity and quality of research on the general problem of evaluating truncated stratigraphic ranges and many of these studies are germane to the Zumaya problem. Especially important is the classic paper by Paul (1982), which presents a preliminary formulation of a calculus for estimating the uncertainty in the placement of first and last occurrences. Follow-up studies include those of Strauss & Sadler (1987) and Springer & Lilje (1988).

Several of the methods used by Paul and others could be applied to the Zumaya case to estimate the probability that a given extinction pattern seen in the range charts is real. Furthermore, the experimental mode used here with the Zumaya data makes it possible to test methodologies by asking whether an imposed extinction event (such as in figure 3) can be reconstructed from the experimental results. But most of the existing statistical methods are directed at problems of time correlation between stratigraphic sections and thus are designed to answer somewhat different questions. For this reason, I will describe a different approach to the problem of assessing extinction patterns. This will involve bootstrapped simulations of what the preserved record should look like under varying conditions of extinction timing.

Simulation of sudden extinction

Starting from the horizon of maximum diversity in the full range chart for Zumaya (15 lineages at 130 m in figure 1), let us assume, as an extreme case, that all lineages actually survived up to the K–T boundary (0 m level) but were killed simultaneously at the boundary. In a preserved record, the ranges of each of these lineages will be truncated backwards in time by an amount typical of the gaps between fossil occurrences at Zumaya. The full range chart (figure 1) contains 85 gaps between fossil occurrences (ticks) and the length of each can be measured. A gap (amount of range truncation) can be assigned to each of the lineages and a picture of probable diversity decline can be constructed. This can then be compared with the diversity decline actually observed to evaluate the efficacy of the model of sudden extinction.

If the occurrences of ammonites at Zumaya were randomly distributed, we could assume an exponential distribution of gap sizes, following Paul, and the ideal case of simultaneous extinction could be modelled by a simple calculation. But the occurrences at Zumaya are not randomly distributed, there being more small gaps than would be predicted by a random model. Therefore, a boostrap technique using the gaps observed in the full Zumaya range chart (figure 1) is preferable.

For each lineage, a gap size is chosen at random (with replacement) from the 85 gaps observed at Zumaya. The gap selected is then used to define a distance (down) to the top of the preserved range of that lineage. When applied to all lineages, this procedure produces one possible pattern of declining diversity. The simulation is repeated many times to build a general picture of diversity decline under the condition of sudden extinction. The result is shown in figure 4: the vertically ruled pattern marked 'sudden' contains 90 % of the outcomes of 200 simulations. Thus, if all lineages actually lived up to the K–T boundary and all went extinct at that level, the apparent decline in diversity is expected to lie within the indicated band.

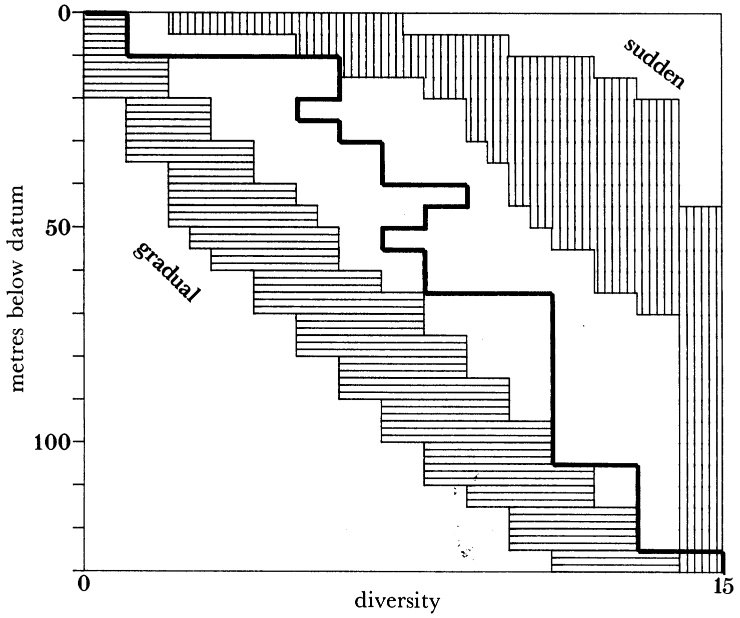

FIGURE 4. Simulation of the pattern of diversity decline expected under two experimental conditions, one representing sudden extinction (vertical ruling) and the other representing gradual extinction (horizontal ruling). The actual diversity decline, from figure 1, is shown as the solid line. The position of the solid line between the simulation bands indicates that ammonite extinction below the K–T boundary at Zumaya was a combination of the sudden and gradaul modes.

Simulation of gradual extinction

The procedure is the same except that a linear decline in actual diversity is postulated. The lineages are assumed to go extinct, one by one, at a uniform rate from the 130 m level to the

K–T boundary. Random picks from the Zumaya gap distribution are then used to further truncate the ranges. The resulting distribution, based on 200 simulations, is shown as the horizontally ruled pattern marked 'gradual' in figure 4.

Evaluation of results

The two alternative models, sudden and gradual extinction, predict patterns of diversity decline that are virtually non-overlapping. The solid line in figure 4 is the track of diversity decline observed in the full range chart (figure 1) and this falls between the two extreme models.

We must conclude that the upper part of the Zumaya section is best explained by a mixture of sudden and gradual extinction. The basic shape of the diversity decline is that of sudden extinction but the position of the curve is to the left of the prediction for sudden extinction and to the right of that for gradual extinction. This suggests that the extinctions were spasmodic, but it does not tell us whether they were concentrated at the K–T boundary.

New field collections made since April 1987, at other localities in the Bay of Biscay Basin demonstrate that the ammonites persisted at high diversity to within a metre of the K–T boundary (Ward & MacLeod 1988). Thus the suddenness of the ammonite extinction is established regionally even though the situation at Zumaya remains uncertain.

CONCLUSION

The case for or against extraterrestrial causes of extinction will not be made firm until we have a clearer picture of the timing of extinctions in the stratigraphic record. Although existing methods of quantitative biostratigraphy work well for some aspects of stratigraphic correlation, new approaches are needed to work with the extinction problem. Although the method used to develop figure 4 has obvious limitations, it does suggest a direction for future research. The method should yield more nearly definitive results when applied to larger datasets: more lineages over longer time spans.

This research was supported by NASA grant NAG 2-237. Also, the study benefited substantially from discussions with colleagues, including J. J. Sepkoski, Jr, David Jablonski, George Boyajian, Charles Marshall, and Mike Foote. Special thanks are due Peter D. Ward for providing his field data on the occurrences of ammonites at Zumaya.

REFERENCES

Alvarez, L. W. 1987 Mass extinctions caused by large bolide impacts. *Physics to-day*, July, pp. 24–33.

Alvarez, L. W., Alvarez, W., Asaro, F. & Michel, H. V. 1980 Extraterrestrial cause for the Cretaceous–Tertiary extinction. *Science, Wash.* **208**, 1095–1108.

Crutzen, P. J. 1987 Acid rain at the K/T boundary. *Nature, Lond.* **330**, 108–109.

Grieve, R. A. F. 1987 Terrestrial impact structures. *A. Rev. Earth planet. Sci.* **15**, 245–270.

Hallam, A. 1987 End-Cretaceous mass extinction event: argument for terrestrial causation. *Science, Wash.* **238**, 1237–1242.

Hut, P., Alvarez, W., Elder, W. P., Hansen, T., Kauffman, E. G., Keller, G., Shoemaker, E. M. & Weissman, P. R. 1987 Comet showers as a cause of mass extinction. *Nature, Lond.* **239**, 118–126.

Jablonski, D. 1986 Causes and consequences of mass extinctions: a comparative approach. In *Dynamics of extinction* (ed. D. K. Elliott), pp. 183–229. New York: John Wiley.

McGowran, B. 1986 Beyond classical stratigraphy. *J. Petrol. Explor. Soc. Aust.*, September 1987, 28–41.

Paul, C. R. C. 1982 The adequacy of the fossil record. In *Problems of phylogenetic reconstruction* (ed. K. A. Joysey & A. E. Friday), pp. 75–117. London: Academic Press.

Prinn, R. G. & Fegley, B. Jr 1988 Biospheric traumas caused by large impacts and predicted relics in the sedimentary record. *Contr. lun. planet. Inst.* **673**, 145.

Raup, D. M. 1986 Biological extinction in earth history. *Science, Wash.* **231**, 1528–1533.

Raup, D. M. 1987 Mass extinction: a commentary. *Palaeontology* **30**, 1–13.

Raup, D. M. & Boyajian, G. E. 1988 Patterns of generic extinction in the fossil record. *Paleobiology* **14**, 109–125.

Shoemaker, E. M., Shoemaker, C. S. & Wolfe, R. F. 1988 Asteroid and comet flux in the neighborhood of the earth. *Contr. lun. planet. Inst.* **673**, 174–176.

Signor, P. W. III & Lipps, J. H. 1982 Sampling bias, gradual extinction patterns, and catastrophes in the fossil record. In *Geological implications of impacts of large asteroids and comets on the Earth* (ed. L. T. Silver & P. H. Schultz), pp. 291–296. Boulder, Colorado: Geological Society of America.

Springer, M. & Lilje, A. 1988 Biostratigraphy and gap analysis: the expected sequence of biostratigraphic events. *J. Geol.* **96**, 228–236.

Strauss, D. & Sadler, P. M. 1987 Confidence intervals for the ends of local taxon ranges. *Tech. Rep. Univ. Calif. (Riverside) Dep. Statist.* **158**, 1–21.

Surlyk, F. & Johansen, M. B. 1984 End-Cretaceous brachiopod extinctions in the Chalk of Denmark. *Science, Wash.* **223**, 1174–1177.

Ward, P. D. & MacLeod, K. 1988 Macrofossil extinction patterns at Bay of Biscay Cretaceous–Tertiary boundary sections. *Contr. lun. planet. Inst.* **673**, 206–207.

Ward, P. D., Wiedmann, J. & Mount, J. F. 1986 Maastrichtian molluscan biostratigraphy and extinction patterns in a Cretaceous/Tertiary boundary section exposed at Zumaya, Spain. *Geology* **14**, 899–903.

Weidmann, J. 1987 The K/T boundary section of Zumaya (Guipuzcoa). In *Field-guide excursion to the K/T boundary at Zumaya and Biarritz* (ed. M. A. Lamolda), pp. 17–23. Leioa, Vizcaya: Jornadas de Paleontologia.

Discussion

P. A. SABINE (19 *Beaufort Road, London, U.K.*). Professor Raup's suggestion that no single individual could present a case for or against an extraterrestrial cause of extinction may be refuted for the Stevns Klint, Denmark, K–T locality by the extraordinarily careful stratigraphical and chemical work done, especially by Hansen and his colleagues of Copenhagen. Recognition of carbon-stained and non-stained white Bryozoa, and ^{13}C measurements by these workers contradicts the hypothesis that the carbon black originated from a giant forest fire started by an impacting bolide (Hansen *et al.* 1987). The carbon-black isotope values are unrelated to meteoritic material but are consistent with a terrestrial origin.

It is highly relevant that the carbon black occurs in the 3.5 m (*ca.* 50000 years) below and before the boundary clay with its Ir anomaly. The Ir, with large values (at a maximum of 185 ng g^{-1}, *ca.* 45 times the world average of 4 ng g^{-1}), was found to be closely associated with the carbon. It was concluded that the iridium-carrying phase was the carbon black itself. The occurrence recalls the strong affinity of gold and carbonaceous material in the Witwatersrand. Iridium is known (as fluoride) in volcanic gases and a likely explanation is that Ir from such a source was adsorbed on to the carbon black.

There is a general point that this sort of investigation does show the value to be obtained by very careful, precise work when it is based on innovative ideas, and, even more, the necessity to do it. This research provides a powerful comment on Professor Raup's initial suggestion and I ask whether he would accept that in this case the bolide hypothesis has strong evidence against it.

Reference

Hansen, H. J., Rasmussen, K. L., Gwozdz, R. & Kunzendorf, H. 1987 *Bull. geol. Soc. Denm.* **36**, 305–314.

D. M. RAUP. Dr Sabine has suggested that the Hansen group in Copenhagen has 'strong evidence' against the bolide hypothesis of mass extinction, and that this case argues against my suggestion that the problem of mass extinction by extraterrestrial causes is beyond the expertise of any one individual. I will stand with my earlier statement and suggest that the Hansen research is a good illustration of my point.

The Hansen study is important, of course, and is well known in the scientific community. And I agree that it is an excellent example of what can be accomplished with 'very careful, precise work'. Dr Hansen presented his findings and interpretations at the Global Catastrophes in Earth History meeting (Snowbird, Utah) in October 1988, and it was debated fully by the assembled geologists and geochemists, but with no clear resolution. The lack of consensus was, I think, because the subject is extraordinarily complex and because any evaluation of the conclusions requires expert knowledge of Cretaceous palaeontology and biostratigraphy, the biogeochemistry of carbon, the geochemistry of iridium and other trace elements, stable isotopes, diagenetic processes in a complex environment, forest fires and their products, and chemistry of meteorites.

Although I have no doubt that the Hansen team has many (or perhaps all) of the necessary ingredients, any synthesis of the problem made by a single individual must perforce be based largely on hearsay, or to put it another way, on deference to expert colleagues. And this was my major point: unlike many other research problems one deals with on a daily basis, the K–T problem is beyond the resources of any one of us. For the practising palaeontologist or geochemist, this can be both discouraging and exciting. On the one hand, no one is fully competent to evaluate even their own research results, but on the other hand, they are privileged to work in a multidisciplinary milieu one rarely experiences.

P. ASHMOLE (*Department of Zoology, University of Edinburgh, U.K.*). The experiments Professor Raup has described seem to show that one can say very little about the temporal pattern of extinction of a group if the fossils are scarce. I suppose I am right in thinking that the problems would be much less severe if the fossils were, say, an order of magnitude more abundant. Does Professor Raup think that in analysing episodes of extinction one should simply accept that useful data can be obtained only from taxa with abundant fossils in the strata concerned?

D. M. RAUP. Should we ignore scarce fossils when dealing with extinction questions like those at the K–T boundary? This is an interesting and important question. In the Zumaya experiments described in my paper, I showed that it can be impossible to distinguish between sudden and gradual extinction because of limitations in sampling and preservation. Yet the Zumaya case is not atypical of the fossil record: it is better than many (especially terrestrial vertebrate sections) but worse than many others (microfossil sequences, in particular). Should researchers wishing to work with the details of mass extinction convert to micropalaeontology? Or should they simply work harder to find more fossils in sparsely fossiliferous rocks and search for better methods of treating the available data? I don't know.

P. W. KING (*Department of Biology, University College London, U.K.*). It has been suggested that astronomers and physicists should learn more about palaeontology, or even that they should not interfere at all in it. However, I think that it is necessary to teach the palaeontologists a little bit of astronomy.

When the *Apollo* astronauts went to the Moon, they did not just go there for the greater glory of the United States of America: they also performed a lot of useful science. For example, they collected a lot of rocks, which were brought back and, among other things, dated by radioistropic methods. It was found that most of the lunar rock and soil samples were older than almost any rocks found on the Earth, and that for the second half of its life the Moon has been almost totally geologically inactive. However, it has been struck during this time by large bodies, up to asteroidal sizes, which have produced some of the large craters (although most of these are much older). *Apollo 12* astronauts sampled some material from a ray, which comprises ejected material, associated with the crater Copernicus. This is about 100 km in diameter and would have been formed by an object about 10 km in diameter. This material was dated to be about 900 million years old. Copernicus is thus Pre-Cambrian. It is a very well-preserved crater. Nevertheless, there are two craters of similar size on the near-side of the Moon which are, from photographic evidence, even better preserved and less eroded by very small impacts, and are of Phanerozoic age in all probability (I do not know if there are other craters this large and well preserved on the far side of the Moon, but there may well be). If one considers therefore that the Moon has been struck in Phanerozoic times by, say, two objects around 10 km diameter it follows that the Earth, which has a surface area of about 14 times as large, should have been struck by around 28 such asteroidal bodies, of which one or two can reasonably be expected to have been rather larger than the 10 km minimum I am setting. In fact, the greater gravity of the Earth would have a focusing effect and the number of such impacts expected may be slightly over 28. There is no way that the Earth can have avoided being hit.

I assume that such a 10 km diameter body hit the Earth at, for example, the Cretaceous–Tertiary boundary. 'Back of envelope' calculations reveal that, if it is assumed only one third of the bulk of the impacting object is comminuted to fine dust, which enters the atmosphere and remains in suspension long enough to be uniformly deposited worldwide, then each square metre of the surface of the globe will be covered by two kilograms of dust which would make a layer one or two millimetres thick. (I have assumed the remaining asteroidal material would either enter the body of the Earth or escape into outer space. In reality much of the material excavated from the crater by the impact – terrestrial material – would also enter the atmosphere, but it is difficult to calculate how much.) Even the quantity I calculate, which is a very conservative estimate, is sufficient that, when suspended in the upper air as dust, it would cause a great deal of obstruction to the light of the Sun, with, if continued for any length of time, consequent climatic changes. It is not reasonable to believe that an event of this severity would have no measurable effect on life at all.

I am convinced that large impacts are not uncommon in Phanerozoic times. The evidence for a large impact at the K–T boundary (the Iridium and other heavy-metal anomalies, the shock metamorphism of quartz grains, etc.) is, to my mind, overwhelming. I am much less convinced of the likely scale of the environmental effects, although simple physics shows us they must have been severe. Many enthusiasts have proposed a variety of extreme scenarios, and the evidence brought forward in support of them from computer modelling has been confusing and even contradictory. I think that a great deal more work needs to be done in this field, modelling likely effects, climatic and other, which experimental evidence brought to bear from many disciplines of science. The survival, apparently almost unaffected, of many groups across the K–T boundary places a loose upper bound on the scale of the environmental dislocation and may, I think, rule out some of the most extreme models.

In conclusion, I find the evidence of periodicity in mass extinctions to be weak at best, but of the reality of impacting bolides causing environmental dislocation I have no doubt.

The input of physicists to palaeontology has been most fruitful, as it has in other branches of geology, for without methods developed from physics how could absolute dates of rocks be obtained?

D. M. Raup. I am in complete agreement with the suggestion that our ignorance of the environmental effects of large-body impact is the weak link in the extraterrestrial interpretation of the Cretaceous mass extinction. We have no personal experience with impacts of the size postulated (fortunately!) so the environmental consequences of the impact can only be inferred by extrapolation from small-scale laboratory experiments or by numerical modelling. My understanding is that the uncertainties in both approaches are formidable. And if the physical consequences of a large impact are in doubt, what chance is there of making precise predictions about biological effects and, in turn, confirming or denying these predictions in the fossil record?

Despite this rather dismal view, there is hope from another direction, using the following logic. It is known, as Mr King has pointed out, that the Earth has been hit by many large meteorites during the Phanerozoic. And the geological, geophysical and geochemical techniques for recognizing these events have vastly improved. Within a few years, there should be as good a chronology of Phanerozoic impacts as there now is for extinctions. Armed with these data, it should be straightforward to show whether there is a statistically valid association between large impacts and large extinctions.

A. W. Wolfendale, F.R.S. (*Department of Physics, University of Durham, U.K.*). Concerning the title of Professor Raup's lecture 'The case for extraterrestrial causes of extinction', would it be true to say that there has been a gradual extinction of 'the case'? Specifically, does Professor Raup consider that the periodicity in the extinction record is now less secure than hitherto?

D. M. Raup. Professor Wolfendale has asked whether there has been a gradual extinction of the case for extraterrestrial causes of extinction. From my vantage point, the opposite is true. Eight years ago, there was only the iridium evidence and that was based on only three localities. Furthermore, the iridium data were open to a plethora of alternative interpretations. By now, the K–T iridium anomaly has been confirmed at scores of localities worldwide and many (although not all) of the alternative interpretations have been ruled out by most people working in that subject. In addition, we have very strong evidence from shocked minerals and from osmium isotopes. The missing crater is still a problem to some workers, but there are several good candidates being investigated actively.

Also, the past eight years have seen a vast increase in our knowledge of the details of the terminal Cretaceous extinctions, thanks largely to the field work and laboratory analyses stimulated by the debate. Most (although not all) of the new discoveries have served to shorten the time span of the extinctions and to move it closer to the presumed time of impact.

So, whether or not the extraterrestrial hypothesis is ultimately proven or disproven, I think a gradual strengthening of the case is clear.

Professor Wolfendale asked also about my confidence in the claim of periodicity in the extinction record. This question is entirely separate from that of extraterrestrial causes.

Periodicity, as a description of the extinction record, has been found by several palaeontologists over several decades. Periodicity is involved in the debate over extraterrestrial influences for the sole reason that several astronomers proposed mechanisms in the solar system or galaxy to explain the periodicity, and some of the proposals have used comet or asteroid impact. Thus the connection is not a primary one. Periodicity in the fossil record may turn out to be correct but not driven by extraterrestrial phenomena, or meteorite impact may cause extinction yet have nothing to do with periodicity, or some combination of these may obtain.

The strength of the periodicty proposal itself depends largely on which statistical analyses one finds most convincing. In the past five years, the published papers (and authors) analysing the extinction data are about evenly split on the question. My own view, as a participant, is that the case is extremely strong in the late Mesozoic and Cainozoic but weak or absent in the Palaeozoic.