the top of the boundary clay in the Madrid area just as they do at the other sites we have studied. A 1- to 3-m-thick crevasse splay sandstone forms a prominent tabular ledge a few centimeters above the coal.

Other sites in Colorado. The kaolinitic clay bed at the K/T boundary has been traced along a 2-km stretch of outcrop in Purgatoire Canyon to a point about 1.2 km west of Madrid, where it disappears under valley alluvium. The boundary bed has been identified in several measured stratigraphic sections, mostly along the south side of the valley (*17*). Except for a new site recently discovered in a road cut in Road Canyon, about 20 km to the north, no other localities have been found in the Raton Basin. The boundary clay appears to represent a single layer that extends throughout a wide area in the Raton Basin; yet only a few sites have been found along hundreds of kilometers of outcrop of the lower zone of the Raton Formation in both the Colorado and New Mexico portions of the basin.

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References and Notes

1. L. W. Alvarez, W. Alvarez, F. Asaro, H. V. Michel, *Science* **208**, 1095 (1980); W. Alvarez, L. W. Alvarez, F. Asaro, H. V. Michel, *Geol. Soc. Am. Spec. Pap. 190* (1982), p. 305.
2. C. J. Orth *et al.*, *Science* **214**, 1341 (1981).
3. ———, *Geol. Soc. Am. Spec. Pap. 190* (1982), p. 423.
4. C. L. Pillmore, *N. Mex. Geol. Soc. Guideb. Field Conf.* **27**, 227 (1976).
5. ——— and J. O. Maberry, *ibid.*, p. 191; S. M. Tur and R. M. Flores, *Mt. Geol.* **19** (No. 2), 25 (1982); V. L. Leighton, thesis, Colorado State University (1980).
6. W. T. Lee, *U.S. Geol. Surv. Prof. Pap. 101* (1917), p. 1.
7. R. M. Flores, unpublished data; S. Strum, R. M. Flores, V. V. Cavaroc, *Geol. Soc. Am. Abstr. with Programs* (Southeastern Section) (1983), vol. 15 (No. 2), p. 89.
8. R. M. Pollastro, C. L. Pillmore, R. H. Tschudy, C. J. Orth, J. S. Gilmore, in Clay Minerals Society 32nd Annual Clay Minerals Conference Program and Abstracts (1983), p. 82. B. F. Bohor, in Clay Minerals Society 32nd Annual Clay Minerals Conference Program and Abstracts (1983), p. 48; ———, E. E. Foord, P. J. Modreski, D. M. Triplehorn, in preparation. B. F. Bohor *et al.* reported the presence and positive identification of diaplectic (shock-metamorphosed) quartz in the boundary clay at the Brownie Butte locality, Hell Creek area, Montana, which supports an origin as altered asteroid-impact ejecta. We observed similar quartz grains in the Raton Basin boundary clay.
9. R. W. Brown, *U.S. Geol. Surv. Prof. Pap. 375* (1962), p. 24.
10. R. H. Tschudy, in *Cretaceous and Tertiary Rocks of the Southern Colorado Plateau*, J. E. Fassett, Ed. (Four Corners Geological Society Memoir, Durango, Colo., 1973), p. 131.
11. ———, *Geol. Soc. Am. Spec. Pap. 127* (1970), p. 65.
12. J. S. Gilmore, C. J. Orth, J. D. Knight, C. L. Pillmore, R. S. Tschudy, *Nature (London)* **307**, 224 (1984).
13. R. H. Tschudy, C. L. Pillmore, C. J. Orth, J. S. Gilmore, in preparation.
14. A. T. Cross, unpublished data.
15. I. A. Williamson, *Min. Mag.* **122**, 203 (1970); B. F. Bohor, R. M. Pollastro, R. E. Phillips, Clay Minerals Society 15th Annual Meeting (1978) (Abstr.) p. 47; D. A. Spears and R. Kanaris-Sotiriou, *Sedimentology* **26**, 407 (1979).
16. B. F. Bohor and C. L. Pillmore, *N.M. Geol. Soc. Field Conf. Guideb.* **27**, 177 (1976).
17. R. G. Wells, unpublished data.
18. We thank R. Flores, R. Fleming, and R. Pollastro for critical reviews of the manuscript and V. Sable for helpful comments and assistance. C.J.O., J.S.G., and J.D.K. thank the Department of Energy for support.

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The End of the Cretaceous: Sharp Boundary or Gradual Transition?

Abstract. *Evidence indicates that the Cretaceous-Tertiary boundary is very sharp, and, within the limits of resolution, it is apparently synchronous at the various boundary localities. Arguments to the contrary, particularly those of Officer and Drake, are shown to be invalid.*

Two papers critical of the impact theory of the Cretaceous-Tertiary (K/T) extinction (*1, 2*) have recently appeared in *Science* (*3, 4*). These two publications, when coupled with similarly oriented articles in the *American Scientist* and *Geochimica et Cosmochimica Acta* (*5*), might lead a nonspecialist reader to conclude that the impact theory was encountering serious difficulties. If these articles were left unchallenged, it would appear that the points they raise have falsified the theory, which is not the case. If we simply answered the specific points raised, it would appear that this was a case of two conflicting but strongly supported hypotheses, where experts could reasonably disagree; this also is incorrect. We think it is important, therefore, to examine these critical papers in the context of wider developments within the rapidly advancing field of research on mass extinctions. We will then proceed to a detailed commentary on the paper by Officer and Drake (*4*).

One might ask how the impact theory is holding up, and whether the initial resistance it encountered is tapering off. In the 3 years since it was formally presented (*1*), the impact explanation for the terminal-Cretaceous extinctions has survived several critical tests and attempts at falsification, and a number of its critical predictions have been confirmed (*6, 7*). With the minor modifications that have been necessary, it should be considered a viable theory.

Furthermore, many of the "flaws" that were pointed out in the early days of the theory have now been understood and are acknowledged by critics to be of no relevance. A notable example concerns our original proposal (*1*) that dust spread by the impact would cause darkness sufficient to suppress photosynthesis for several years. Our calculations were based on data from the 1883 Krakatoa eruption. Subsequently calculations showed that darkness estimates should not be based on the Krakatoa model and that dust would be spread not by atmospheric circulation, as with Krakatoa, but along ballistic trajectories outside the atmosphere (*8*). These corrections to the original theory remove several objections and explain well the plankton extinction (*9*). This aspect of the evolution of the theory has been discussed in detail (*6*).

The degree to which evidence is building up in favor of the impact theory is not apparent from the paper of Officer and Drake (*4*). They do not mention the Cretaceous-Tertiary Extinction Conference (K-TEC) in Ottawa in May 1981, at which the theory was discussed in great detail, with much new supporting evidence reported. The complete transcript of the K-TEC Conference has been published (*10*). They also fail to mention the October 1981, Snowbird Conference on "Large-body impacts and terrestrial evolution: geological, climatological, and biological implications," even though Drake attended the meeting and has even published a criticism of some of the papers presented there (*11*). A collection of 48 articles based on that conference is available (*2*), and many were summarized earlier in a volume of abstracts (*12*); not one of these contributions was cited by Officer and Drake. The interested reader can thus find an extensive literature that contradicts the impression of a theory in disarray.

Officer and Drake (*4*) have addressed the question of whether the K/T boundary is abrupt and shows evidence of having resulted from the impact of a large extraterrestrial object or whether it is a gradual transition unrelated to an impact. They favor the latter interpretation, as shown by the title of their article: "The Cretaceous-Tertiary transition." A review by scientists who have not been active in the field might have been valuable if it had been balanced, but unfortunately Officer and Drake use a strange double standard, in which they apply keen scrutiny to evidence favoring the impact theory—as, of course, they should—but uncritically accept any results, no matter how flawed, that contradict it. They fail to mention most of the data that support the theory. Instead, they fix their attention on a few cases that can be made to look like contradictions. Generally these cases involve stratigraphic sections that are difficult to interpret because of such obvious problems as depositional gaps and drilling disturbances. In one case, Deep Sea Drilling Project (DSDP) site 384, they base their conclusion on paleomagnetic data that were specifically labeled as questionable by the investigators who made the measurements (*13*).

Much of their paper is devoted to consideration of the evidence from various geographic localities bearing on (i) δt, the duration of the K/T change in various faunal and floral groups and (ii) T, the absolute age in the LaBrecque-Kent-Cande magnetic polarity time scale (*14*) corresponding to the moment of an abrupt K/T faunal or floral change or to the midpoint of a gradual change.

They conclude that δt ranges from 10×10^3 to 100×10^3 years for marine plankton, and that macrofossil groups from Denmark (the best sections available) show little or no K/T change. As they note, varying response times suggest extinctions in response to environmental modifications; we would agree, noting that a major impact would produce important environmental changes, and that instantaneous extinction in all groups everywhere is not a necessary corollary of the impact theory. Hsü *et al.* (*15*) have addressed the question of environmental changes that could be triggered by an impact and that could, in turn, cause extinctions spread over 10^4 to 10^5 years. This consideration invalidates Officer and Drake's use of δt as a test of the impact theory. Even so, it is worth pointing out three flaws in their evaluation of δt. (i) Reworking and bioturbation commonly smear out abrupt events. (ii) There is also a misunderstanding of the foraminiferal data of Smit on Caravaca, in southern Spain, the best stratigraphic record extensively described in the literature. Smit and Hertogen (*16*) evaluated the duration of the mass extinctions as about 200 years; Officer and Drake use the longer interval before the appearance of typically Tertiary forms, citing the transition as 6×10^3 to 25×10^3 years. (iii) Their conclusion that macrofossil assemblages in Denmark show little change across the boundary is not in accord with the relevant literature (*17*); the published data in fact show sharp changes in several groups, precisely at the iridium anomaly (*18*).

On the basis of magnetic stratigraphy, Officer and Drake conclude that the time of extinction, T, is diachronous in various sites by as much as 840×10^3 years,

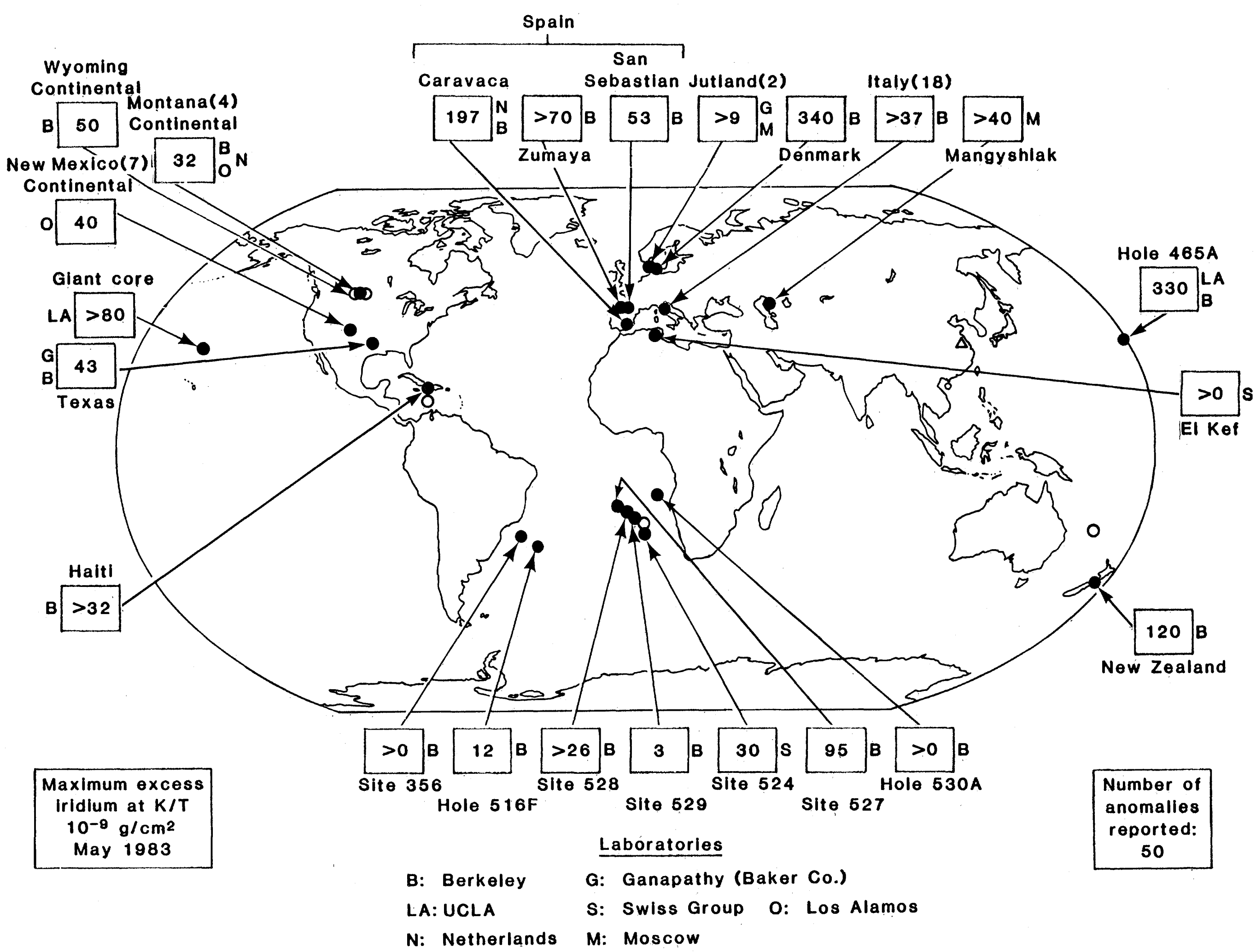

Fig. 1. Localities where an iridium anomaly has been found at the Cretaceous-Tertiary boundary by researchers at one or more of the seven laboratories active in this work (*33*). Values are given in terms of the excess iridium integrated beneath the curve of concentration (in parts per billion) as a function of depth. Sites marked > 0 have anomalous iridium but not enough measured points to define the shape of the curve.

falling in three different polarity zones, and is thus in contradiction to abrupt extinction scenarios, including the impact hypothesis. This is the most important point in their article, on which most of their case rests. However, their conclusion is absolutely unwarranted in view of currently available data. Because of undetectable variations in sedimentation rate, differences between boundary localities in the calculated level within a particular polarity zone are not significant. What is critical, as Officer and Drake recognize, is that, if boundaries could be shown to occur in different polarity zones, they would certainly be diachronous. Their case therefore rests on three sites which they maintain have K/T boundaries in zones younger than reversed (R) polarity zone 29R. This zone is known to contain four K/T boundaries that they cite (Gubbio, Belluno, Red Deer Valley, and DSDP 524), as well as three others that they omit (*19*). The three apparent discrepancies are the K/T boundaries in DSDP 384 and the San Juan Basin, which they say are in the younger normal (N) zone 29N, and in eastern Montana, which they say falls in the still younger zone 28R.

Officer and Drake's discussion of DSDP site 384 is seriously misleading. They note that in 1975 this site was considered to have the best available biostratigraphic record across the K/T boundary in pelagic facies; that was true at the time, but no one currently considers this to be a particularly outstanding boundary section. They fail to mention that, even when the core was first taken, it was seen to be recognizably affected by drilling disturbance and possibly also by burrowing (*20*, pp. 116, 124, and 139). Next, they use the magnetostratigraphic results of Larson and Opdyke (*13*) as evidence that the K/T boundary lies in polarity zone 29N (that is, younger than the K/T boundary that elsewhere falls in zone 29R), despite the warning in the last paragraph of Larson and Opdyke's paper that these results are not reliable. The rapidly varying values of inclination in the vicinity of the boundary (*13*, figure 2, core 13, section 3) are a clear signal to anyone familiar with paleomagnetism that these results are likely to be overprinted and therefore unreliable, and this is emphasized by question marks in the figure. It is now well known that, before the deployment of the hydraulic piston corer on the *Glomar Challenger* in 1979, drilling disturbance was a common and serious problem that destroyed the paleomagnetic usefulness of most DSDP cores (*21*). Site 384 is clearly an example of this problem; it must be rejected as providing evidence for a diachronous K/T boundary.

Officer and Drake accept the original interpretation of Butler, Lindsay, and co-workers (*22*) that dinosaur extinction in the San Juan Basin of New Mexico occurred in zone 29N. They reference but ignore the serious questions raised by several other workers. This procedure can be contrasted with the painstaking research done by Butler to test the validity of the San Juan paleomagnetic data (*23, 24*). Careful studies of magnetic mineralogy (*24*) have shown that one normal zone too many was originally inferred in the San Juan Basin, because of the presence of a very resistant post-depositional normal-field overprint. When this is corrected, the San Juan Basin dinosaur extinction is seen to fall in zone 29R, as predicted by the impact hypothesis. As independent evidence in support of this revised correlation, Butler and Lindsay note that it removes a formerly perplexing overlap in the temporal ranges of Paleocene land mammal ages (*24*).

Officer and Drake's assignment of the Montana boundary to zone 28R rather than the expected zone 29R is unsupported and unconvincing. All they say is (*4*, p. 1388), "The magnetic stratigraphy observations cover only a short interval of geologic time, but on the basis of faunal correlations with the San Juan Basin, we infer that the reversed interval is 28R. . . ." The people who have done the biostratigraphic and paleomagnetic work are far more cautious, and their conclusions must take precedence over the unexplained "faunal correlations" of Officer and Drake. The conclusions of the original researchers are as follows (*25*, p. 159):

> We stress that until the current controversy regarding correlation of the magnetic polarity sequence in the San Juan Basin is resolved, or other pertinent data become available, the magnetic zones recorded in these terrestrial sections in Alberta, Montana, and New Mexico cannot be securely correlated with the magnetic polarity time scale.

In fact, new pertinent data have subsequently become available. As noted above, the problems with polarity correlation of land mammal ages have been resolved (*24*). In addition, an iridium anomaly has been found within the reversed-polarity interval in eastern Montana in which dinosaurs become extinct (*26*). Although the search for additional iridium anomalies is not complete, it appears that zone 29R can be distinguished from nearby reversed zones by the presence of the anomaly and that the extinctions in eastern Montana do fall in zone 29R.

If it appears that we are trying to explain away some uncomfortable contradictions, we would note that the iridi-

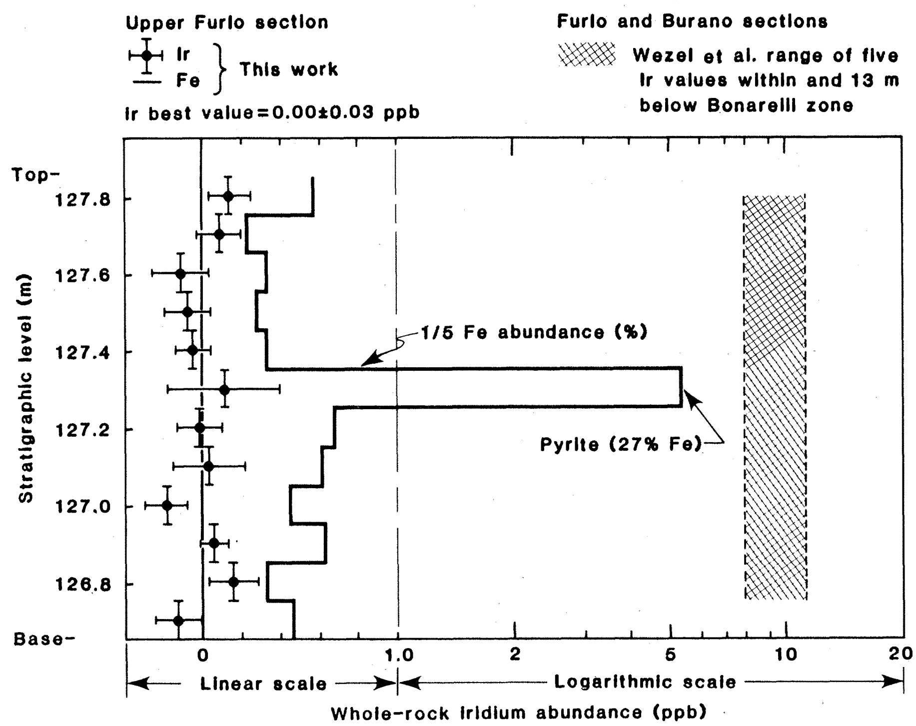

Fig. 2. Iridium measurements on the Bonarelli level from the upper road section at Furlo, Italy (*34*). An iridium anomaly has been reported from this locality (*30*), which is near the Cenomanian-Turonian boundary. The results of present measurements, made on samples that completely cover the interval, did not indicate the presence of excess iridium. These results suggest that the measurements made in the earlier study (*30*) were affected by contamination.

um anomaly has been found in 50 sections worldwide (Fig. 1). Most of these sections show clear relationships that are in accord with the impact theory. Officer and Drake have chosen to base their case on the few sites where stratigraphic complications make the interpretation ambiguous.

In summary, the portion of Officer and Drake's paper that evaluates T and δt contains numerous errors and misunderstandings. This is the most critical part of their argument, but it is simply wrong and cannot be taken as evidence against the impact hypothesis. We have space to comment on only a few of the other points made in their paper.

Officer and Drake make reference to the work of Rampino and Reynolds (*3*), who found that various K/T boundary clays are "neither mineralogically exotic nor distinct from clays above and below the boundary" (*4*, p. 1390). This conclusion is in disagreement with the findings of Kastner (*27*) and Bohor (*28*). It also disagrees with Rampino and Reynolds' own findings on the section at Nye Kløv, Denmark, where they report pure smectite in the boundary layer and different clay minerals above and below. They consider this smectite layer to be unimportant, because they interpret it as an altered volcanic ash. Both Kastner (*27*) and Bohor (*28*) have noted that the clay minerals of the K/T boundary layer could be produced by alteration of impact-melt glass, as well as by alteration of volcanic ash. Furthermore, by looking only at < 2 μm clays, Rampino and Reynolds discarded the boundary spherules (*29*), which are not only exotic but apparently unique to the K/T boundary.

Officer and Drake also cite Wezel *et al.* (*30*), who have reported iridium anomalies both above and below the K/T boundary in some Italian sections, notably in the 1-m black, cherty shale called the Bonarelli level, about 240 m below the K/T boundary. We have been concerned about this report, because Wezel's group have also published strange micropaleontological results (*31*) that later were shown to be due to contamination (*32*), and contamination is all too easy in chemical analytical work at the parts-per-billion level. To test the results of Wezel *et al.*, we have analyzed 12 independently collected samples that completely cover the Bonarelli level at the site where Wezel's group reported an iridium anomaly. The results are shown in Fig. 2, and we conclude that there is no evidence for an iridium anomaly at the Bonarelli level.

The last paragraph of Officer and Drake's article seems to be a plea for a return to the time before the iridium anomaly was discovered, when almost any speculation on the K/T extinction was acceptable. This idea is pleasantly nostalgic, but there is by now a large amount of detailed astronomical, geological, paleontological, chemical, and physical information which supports the impact theory. Much interesting work remains to be done in order to understand the evolutionary consequences of the impact on different biologic groups, but the time for unbridled speculation is past.

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References and Notes

1. L. W. Alvarez, W. Alvarez, F. Asaro, H. V. Michel, *Science* **208**, 1095 (1980).
2. L. T. Silver and P. H. Schultz, Eds., *Geol. Soc. Am. Spec. Pap. 190* (1982).
3. M. R. Rampino and R. C. Reynolds, *Science* **219**, 495 (1983).
4. C. B. Officer and C. L. Drake, *ibid.*, p. 1383.
5. J. D. Archibald and W. A. Clemens, *Am. Sci.* **70**, 377 (1982); M. L. Keith, *Geochim. Cosmochim. Acta* **46**, 2621 (1982).
6. L. W. Alvarez, *Proc. Natl. Acad. Sci. U.S.A.* **80**, 627 (1983).
7. R. Ganapathy, *Science* **216**, 885 (1982); W. Alvarez, F. Asaro, H. V. Michel, L. W. Alvarez, *ibid.*, p. 886.
8. O. B. Toon *et al.*, in (*2*), p. 187; E. M. Jones and J. W. Kodis, *ibid.*, p. 175.
9. D. H. Milne and C. P. McKay, *ibid.*, p. 297.
10. D. A. Russell and G. A. Rice, Eds., *Syllogeus* (1982), vol. 39.
11. C. L. Drake, *Geology* **10**, 127 (1982).
12. *Lunar Planet. Inst. Contrib. 449* (1981).
13. P. A. Larson and N. D. Opdyke, *Initial Rep. Deep Sea Drill. Proj.* **43**, 785 (1979).
14. J. L. LaBrecque, D. V. Kent, S. C. Cande, *Geology* **5**, 330 (1977).
15. K. J. Hsü, J. A. McKenzie, Q. X. He, in (*2*), p. 353.
16. J. Smit and J. Hertogen, *Nature (London)* **285**, 198 (1980).
17. T. Birkelund and R. G. Bromley, Eds., *Cretaceous-Tertiary Boundary Events* (University of Copenhagen, Copenhagen, 1979), vol. 1.
18. W. Alvarez *et al.*, *Science* **223**, 1135 (1984).
19. H. R. Thierstein, in (*2*), p. 385.
20. B. E. Tucholke, P. R. Vogt *et al.*, *Initial Rep. Deep Sea Drill. Proj.* **43**, 791 (1979).
21. J. LaBrecque, *Eos* **64**, 219 (1983); L. S. Chan and W. Alvarez, *Rev. Geophys. Space Phys.* **21**, 620 (1983).
22. R. F. Butler, E. H. Lindsay, L. L. Jacobs, N. M. Johnson, *Nature (London)* **267**, 318 (1977); E. H. Lindsay *et al.*, *Geology* **6**, 425 (1978); *Am. J. Sci.* **281**, 390 (1981).
23. R. F. Butler, *J. Geophys. Res.* **87**, 7843 (1982).
24. ______ and E. H. Lindsay, in preparation; R. F. Butler, personal communication.
25. J. D. Archibald, R. F. Butler, E. H. Lindsay, W. A. Clemens, L. Dingus, *Geology* **10**, 153 (1982).
26. Data of W. Clemens, F. Asaro, H. V. Michel, W. Alvarez, and L. W. Alvarez [see (*6*), figure 10]; data of B. F. Bohor, C. J. Orth, and J. S. Gilmore [see (*28*)].
27. M. Kastner, quoted by F. Asaro, *Syllogeus* **39**, 8 (1982); M. Kastner, F. Asaro, H. V. Michel, W. Alvarez, abstract for Meeting on Glass in Planetary and Geological Phenomena (Alfred University, Alfred, N.Y., 1983).
28. B. F. Bohor, *Clay Miner. Soc. Ann. Mtg. Progr.* **20**, 48 (1983).
29. J. Smit and G. Klaver, *Nature (London)* **292**, 47 (1981); J. C. Varekamp and E. Thomas, in (*2*), p. 461; A. Montanari, W. Alvarez, F. Asaro, H. V. Michel, L. W. Alvarez, *Geology* **11**, 668 (1983).
30. F. C. Wezel, S. Vannucci, R. Vannucci, *C.R. Acad. Sci.* **293**, 837 (1981); F. C. Wezel, *Nature (London)* **294**, 248 (1981).
31. R. Coccioni, *Ateneo Parmense Acta Nat.* **14**, 223 (1978); F. C. Wezel, *ibid.* **15**, 243 (1979).
32. I. Premoli Silva and H. Luterbacher, *Riv. Ital. Paleontol. Stratigr.* **84**, 667 (1978); W. Alvarez and W. Lowrie, *Nature (London)* **294**, 246 (1981).
33. W. Alvarez, L. W. Alvarez, F. Asaro, and H. V. Michel [in (*2*), p. 305] list 32 K/T sites with iridium, reported by seven different laboratories. As of May 1983, 50 K/T sites and seven Eocene-Oligocene sites are known to have anomalous iridium concentrations.
34. W. Alvarez and W. Lowrie, *Geol. Soc. Am. Bull.*, in press.
35. This work was supported by NSF grant EAR-81-15858. We thank D. M. Raup for a critical review.

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Mass Spawning in Tropical Reef Corals

Abstract. *Synchronous multispecific spawning by a total of 32 coral species occurred a few nights after late spring full moons in 1981 and 1982 at three locations on the Great Barrier Reef, Australia. The data invalidate the generalization that most corals have internally fertilized, brooded planula larvae. In every species observed, gametes were released; external fertilization and development then followed. The developmental rates of externally fertilized eggs and longevities of planulae indicate that planulae may be dispersed between reefs.*

It has been widely accepted that most scleractinian corals are viviparous, often releasing larvae intermittently throughout the year (*1–3*). This view is supported by studies of a few species that release planula larvae in the laboratory (*1, 4–10*). Recent studies have shown that some corals are not viviparous, but spawn gametes during brief annual spawning periods (*11–18*). To determine the typical mode and timing of sexual reproduction in corals, we studied gametogenesis and spawning in a large number of hermatypic coral species from the central Great Barrier Reef Province.

Studies were undertaken on nearshore fringing reefs at Magnetic Island and Orpheus Island, and on a midshelf platform reef, Big Broadhurst Reef (Table 1). We observed gamete release in 23 species in situ and in the laboratory. In nine other species, spawning was inferred from the disappearance of mature gametes in sequential samples, or from the presence of gametes in aquaria or plankton mesh bags placed over corals in