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Asteroid Extinction Hypothesis

A distinguishing feature of the asteroid impact hypothesis presented by Alvarez *et al.* (6 June, p. 1095) for the end-of-Cretaceous biotic crisis is that it is based on direct physical evidence: the distribution of iridium in several Cretaceous-Tertiary sedimentary sections. I draw attention to other evidence which suggests that (i) a high Ir concentration may not be uniquely associated with an extraordinary extraterrestrial event and (ii) the impact of a large asteroid in any case is not likely to have had the dire consequences to life on the earth that they propose.

Crucial to their argument for an asteroid impact at the Cretaceous-Tertiary boundary is the interpretation of the Ir concentration in the boundary clays as anomalously high. Background levels of Ir in modern deep-sea sediments are usually on the order of 0.3 part per billion and can be attributed to the influx of meteoritic dust (*1*). However, much higher values can occur, even in Pleistocene sediments. For example, Crocket and Kuo (*2*) reported Ir abundances of 0.11 to 0.71 ppb from nine levels in deep-sea sediment core Eltanin 21-17. The $CaCO_3$ content of the analyzed bulk samples ranged from 83.0 to 94.9 percent, so that if the bulk Ir contents are recalculated as per weight of insoluble residue, as done by Alvarez *et al.*, they would range from 0.83 to 7.6 ppb, with an average of 2.0 ppb in this core. I suggest that the significance of the 9.1-ppb Ir content in the Gubbio boundary clay, on which the asteroid impact hypothesis largely rests, is open to question in light of the comparable concentration range of Ir in deep-sea sediments of Quaternary age (*3*), a time period for which neither a large asteroid impact nor massive extinctions have been suggested.

But why should "abnormally" high Ir contents occur at the level of the Cretaceous-Tertiary boundary in stratigraphic sections worldwide? As Alvarez *et al.* and others point out, the Cretaceous-Tertiary boundary is often associated with a hiatus, an interval of essentially nondeposition or erosion, even in pelagic marine sediments. Under a steady influx of Ir-bearing meteoritic material onto the earth's surface, Ir would be concentrated in sediment either by a reduced input of terrestrially derived sediments or by their preferential removal by bottom current activity. The latter mechanism seems plausible when one considers that the extraterrestrial material in deep-sea sediments typically occurs as spherules from tens of micrometers to several hundred micrometers in size (*4*)—far in excess of particle sizes of clays (< 2 micrometers) such as those at the Cretaceous-Tertiary boundary in the Italian and Danish sections. The relatively larger size combined with a generally higher density of the cosmic spherules would tend to cause segregation of the meteoritic material from clays in the presence of currents, leading to a highly heterogeneous distribution of Ir in sediments. Thus, the factor of 10 difference in Ir concentration between the Fiskeler in the Danish section and the Gubbio boundary clay could be accounted for. Moreover, Alvarez *et al.* acknowledge the variation in thickness of the Fiskeler, from a few centimeters to as much as 35 centimeters locally (*5*), which can be interpreted as due to local sedimentary control by bottom current activity.

Clearly more data on the distribution and abundance of Ir in sediments are needed to establish whether high Ir concentrations are uniquely associated with extraordinary extraterrestrial events, or more generally occur locally, perhaps associated with sedimentary conditions which may not coincide in time with biotic crises. But regardless of the eventual outcome of these researches, it is of interest to reconsider the proposed effects of a large asteroid impact.

Alvarez *et al.* consider the effects of the historic eruption of Krakatoa and extrapolate them by a factor of ~ 10^3 to suggest that the amount of material injected into the stratosphere from the impact of a 10-kilometer asteroid would effectively shut out sunlight for several years, suppressing photosynthesis and causing the collapse of most food chains. However, the 1883 eruption of Krakatoa was small in comparison to other volcanic eruptions in the geologic record, whose effects may be more comparable to those suggested for a large asteroid impact.

A well-known example of the remains of a very large volcano is the Toba caldera in Sumatra, which measures approximately 100 by 35 km; by comparison, the caldera of Krakatoa is only a few kilometers in diameter. A tephra layer several centimeters thick and correlated to the Toba eruption can be found in deep-sea sediment cores more than 2500 km distant from the source, whereas no distinguishable deep-sea tephra deposit from Krakatoa has been found in sediment cores as close as 200 km from the vent (*6*).

But it is difficult to compare these eruptions on the basis of the volume of ejecta, which has been calculated in dif-

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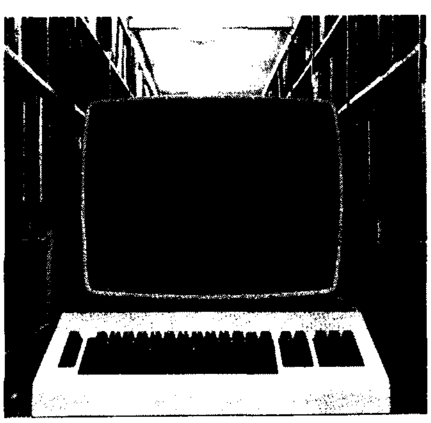

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ferent ways for Toba (*6*, *7*) and Krakatoa (*8*). A more consistent way to estimate their relative magnitude is to compare the volumes of the calderas and assume that caldera volume is proportional to amount of ejecta put into the atmosphere. The volume of the Toba caldera is estimated to be 2000 km^3 (*7*), and that of the Krakatoa caldera to be 5 km^3 (*9*). The expected sunlight attenuation for Toba, calculated with the same assumptions and values used by Alvarez *et al.* but with the effect of Toba 400 times that of Krakatoa, comes to $\exp(-12) \simeq 10^{-5}$. This attenuation factor is not nearly as large as the one postulated by Alvarez *et al.* for the asteroid impact. However, it appears to be more than sufficient to suppress photosynthesis and could presumably have led to at least some of the consequences life on the earth suffered at the end of the Cretaceous according to asteroid impact hypothesis. The pertinent point is that the eruption of Toba occurred 75,000 years ago (*10*), a time that has yet to be noted for massive extinctions or other extraordinary effects on life. Moreover, there is little reason to believe that the magnitude of the Toba eruption was exceptional; even larger explosive volcanic eruptions probably occurred over geological time.

The two principal points raised here—the first concerning the uniqueness of the association of high Ir concentrations in sediment with large asteroid impacts and the second regarding the proposed effects of such an asteroid impact on life on the earth—are independent of each other. That an asteroid impact occurred at the time of the Cretaceous-Tertiary boundary may in time be substantiated by further geochemical work and stratigraphic studies. However, the lack of evidence for serious consequences to global life from large volcanic eruptions, which may approach the ejecta volume postulated for a large asteroid impact, suggests that the cause of the massive extinctions is not closely related to a drastic reduction in sunlight alone, and an alternative mechanism should be sought [for example, (*11*)].

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Alvarez *et al.* have studied the elemental composition of the materials that are known to have lain at the earth's surface at the time of the Cretaceous-Tertiary extinction and have concluded from the high abundance of iridium that an extraterrestrial event was involved, probably the impact of an earth-crossing asteroid. They suggest that the extinction itself was due to a complete cessation of photosynthesis caused by the obliteration of sunlight by a stratospheric dust layer that persisted for a few years.

The case made by Alvarez *et al.* for an asteroidal impact is a compelling one. Their argument that the immediate effect on the biosphere was the cessation of photosynthesis is less strong. The possibility that the extinction was due to collapse of the photosynthetic food chain was suggested by Crutzen and Reid (*1*) and expanded on by Reid *et al.* (*2*) as one of the potential consequences of a nearby supernova explosion. In the cases considered, however, the magnitude of the reduction in sunlight was on the order of 10 percent at most, in contrast to the reduction by a factor of 10^7 (approximately 10 percent of full moonlight) suggested by Alvarez *et al.* Such a scenario would cause a total collapse of photosynthesis; it would also have global climatic consequences that might place an even more severe strain on the biosphere.

The decay of the climate system following the extinction of sunlight was investigated by Hunt (*3*), using a general-circulation model of the atmosphere. His calculations ran for only 50 days after the extinction, at which time the atmosphere still retained weakened jet streams and meridional temperature gradients. After the 2- or 3-year stratospheric residence time for the dust layer, these climatically important features would presumably have essentially disappeared. The scenario suggested by Alvarez *et al.*, however, is not equivalent to extinguishing the sun, since the earth would be blanketed by a hot dust layer capable of producing a powerful greenhouse effect, perhaps analogous to that existing at present on Venus. The radiative balance of the earth under these conditions would depend on such factors as the optical properties of the dust layer in the infrared and its albedo in the visible, but there is little doubt that the climate system would be thrown into a vastly different mode from that of today or that of the Late Cretaceous. The global blanket of dust would largely eliminate the latitudinal temperature gradients that are ultimately responsible for weather. Since the vertical temperature structure of the atmosphere would revert toward stability, convective activity would cease, thereby removing one source of precipitation. Removal of the horizontal temperature gradients would also remove baroclinic instability, thereby eliminating synoptic-scale weather systems, the other major source of precipitation. Extreme drought conditions would thus prevail globally, and the land plants might die from lack of water before they would otherwise die from lack of sunlight.

Under these extreme conditions, the problem becomes not one of explaining the extinction of half the genera living at the time (*4*), but one of explaining the survival of the other half. It is more likely that the actual scenario was considerably less extreme than that proposed by Alvarez *et al.* The larger particles thrown up by the impact would settle out within a relatively short time, and the residue of fine particles may have been relatively transparent. The fact that the clay layer which presumably resulted from settling of the debris has been seen at locations as far apart as Europe and New Zealand might seem to argue against this, since a fairly long residence time is thought to be required to disperse material in the atmosphere over global distances. This argument is based on observations of the normal atmosphere, however, and it is possible that substantial amounts of debris could have been distributed globally within a short time after the impact, through material thrown into ballistic orbits by the enormous kinetic energy release or by the transient atmospheric disturbance created by the passage of the asteroid.

The small fraction of material remaining in the stratosphere would produce effects similar to those proposed (*2*) as accompanying the production of NO_2 in the stratosphere by a nearby supernova—that is, a reduction in global temperature by several degrees, a tendency toward drought conditions, and a reduction in photosynthesis that would have its most severe impact on the large herbivorous land animals and their predators. The

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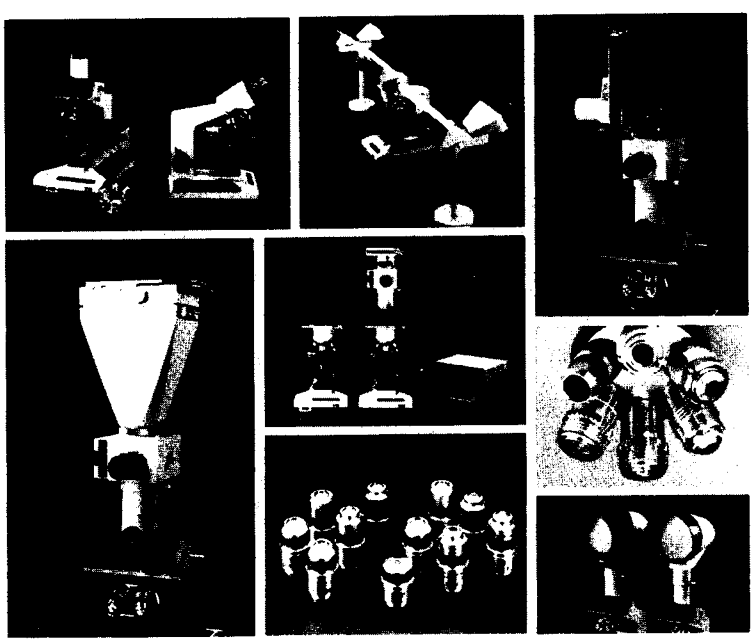

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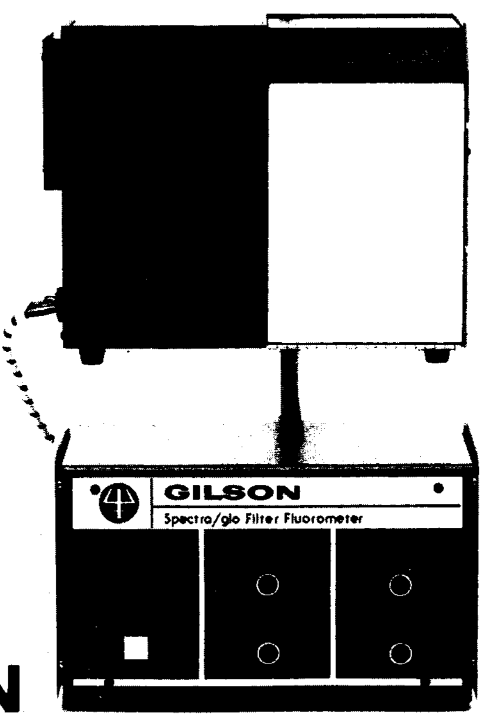

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collapse of the ocean food chain may have been a result of reduced photosynthesis, but it may also have been triggered by overturning of ocean water and crustal fracture due to an oceanic impact.

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On page 1099 of their article, Alvarez *et al.* state: "Iridium has been detected (*45*) in a warm spring on Mount Hood in northern California. . . ." I have not been able to check reference 45 to determine whether an obscure Mount Hood actually exists in northern California. However, many years of living in Oregon and California lead me to believe there is only one Mount Hood, immediately south of the Columbia River and due east of Portland, Oregon.

For many years California sought to claim Crater Lake. Is this now an effort to leapfrog Crater Lake in a move to capture Mount St. Helens, which has superceded Mount Lassen as the most recently active volcano in the conterminous United States?

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The foregoing letters are among the most thought-provoking of the many we have seen in the past several months. The first of Kent's two points is that ocean mechanisms such as bottom current activity could give rise to the high concentrations of Ir we observed at Gubbio, without involving an asteroid collision. While this might be true for a single location, we were unable to devise such a theory to explain the worldwide distribution of this level of Ir, the much higher levels we observed in Denmark, and the still higher integrated amounts we have subsequently seen in a deep-sea core from the north central Pacific. Many other problems arise if one invokes concentration of Ir by bottom currents; we have discussed this in a widely circulated report that is still available from us (*1*). But the strongest argument against Kent's first suggestion comes from our recent unpublished observation (in collaboration with D. A. Russell) of a distinct Ir enrichment in a nonmarine stratigraphic sequence in Montana, at a level above the highest dinosaur remains and below the lowest Tertiary mammal fauna. This observation seems to eliminate from serious consideration extinction theories that involve the ocean in substantive ways, such as those proposed by Kent and by Gartner (*2*).

The subtitle of our article was "Experimental results and theoretical interpretation." We feel that the experimental results we published on anomalous Ir concentrations worldwide, plus the Montana results, plus Ganapathy's additional analysis (*3*) of the Danish Cretaceous-Tertiary boundary material, all confirm our primary theoretical conclusion—that the Cretaceous-Tertiary extinction was initiated by the collision of a large asteroid with the earth, 65 million years ago. Ganapathy found that the relative abundances of several noble metals in the Danish boundary layer matched those in chondritic meteorites and departed in important ways from such measured ratios in all known terrestrial samples. Our independent unpublished measurements (*4*) involve fewer elements than Ganapathy's, but they agree even more closely with chondritic meteoritic abundances than did his. So we see no escape from the conclusion that a large asteroid hit the earth at exactly the end of the Cretaceous period.

Both Kent and Reid correctly point out that we have not proved the asteroid impact led to the extinction through the "dust-screening" mechanism we favored in our article. Since none of us has had any experience in atmospheric modeling, we are delighted to see that Reid is becoming involved in the problem and will ask the correct questions about what would happen after a large asteroid struck the earth, either in the ocean or on the land. We are familiar with Emiliani's suggestion (*5*) that the dust cloud we postulated might lead to a "greenhouse effect" that would cause death by hyperthermia, and we appreciate the strength of his arguments. But others have suggested that "turning off the light" would lead to a drastic lowering of global temperatures, thereby freezing living things to death. We understand the delicate balance between the optical and infrared properties of the dust particles that could give rise to large temperature variations in either direction, so we await the results of computer studies that may lead to a definite answer to this question. Kent estimates that the Toba eruption would have ejected about 400 times as much material as Krakatoa did, close to

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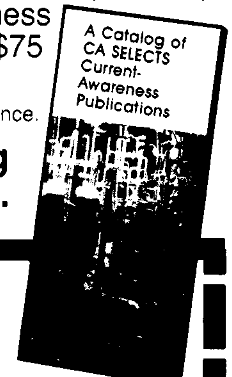

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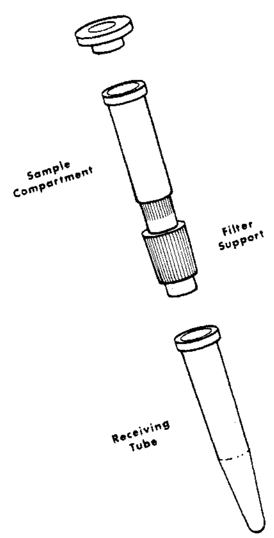

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our estimate of 1000 times Krakatoa for the impact event, although Toba did not produce extinctions. This consideration may make it possible to place a lower limit on the size of extinction-producing events, and we note that if objects substantially smaller than the one we postulate were capable of producing mass extinctions, these would occur more frequently, because of the steep decrease of numbers of asteroids with increasing size. We are prepared to abandon our "darkness hypothesis" when and if some other proposed killing mechanism is shown to fit the geological record better than it does. But we feel our major theoretical conclusion—that the worldwide boundary layer contains the remnants of a large asteroid that somehow triggered the extinction—now rests on a very solid experimental base.

To apologize to Brown and to others who are better proofreaders than we are, we have all stayed after school and written on the blackboard 100 times, "Mt. Hood is in Oregon."

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Erratum: In the report "Mutagenicity of fly ash particles in *Paramecium*" by J. Smith-Sonneborn *et al.* (9 Jan., p. 180), Tables 1 and 2 are printed incorrectly. Significance lines are missing from both tables and "uninduced" should read "induced" in the sixth, eighth, and ninth entries of the first column in Table 1. The tables are reprinted below as they should have appeared.

Table 1. Mutagenic effect of fly ash and heat-treated fly ash in *Paramecium*. Values not connected by the same line are significantly different from each other (Wilcoxon matched-pairs signed-ranks test, $\alpha = .05$). The data from six experiments were pooled since the control values for autogamous progeny were not significantly different. Cerophyl is the ryegrass extract used for cultivation of *Paramecium*. Induced S-9 is the Ames liver microsome fraction from rats receiving Arochlor 1254 (polychlorinated biphenyl) to activate the enzymes for conversion of promutagens to mutagenic form; uninduced S-9 is from rats receiving corn oil only (the vehicle for the Arochlor). Glass beads (1 to 3 μm) suspended in either induced or uninduced S-9 were used as a negative control for nonnutritive particles. Kaolinite was also used in one experiment, and the results were the same as those for the glass beads. Benzo[*a*]pyrene was the positive control for mutagenicity requiring induced S-9. The initial concentration of suspended fly ash was 535 μg/ml. The average number of affected progeny from treated parent cells was 20 percent higher than the average number of affected control progeny. Since one mutation would theoretically yield only 4 affected progeny in 16 autogamous progeny from a treated parent cell (*6*), the percentages, though low, reflect significant damage.

Substance	Lethal and detrimental cells (%)	Number of progeny examined
Cerophyl	1.01 ± 0.21	1568
Glass beads + uninduced S-9	1.36 ± 0.33	1904
Glass beads + induced S-9	1.41 ± 0.42	1888
Benzo[*a*]pyrene + uninduced S-9	3.2 ± 0.39	1440
Fly ash + uninduced S-9	3.7 ± 0.69	2992
Heated fly ash + induced S-9	3.9 ± 1.6	1728
Heated fly ash + uninduced S-9	4.6 ± 1.3	1280
Fly ash + induced S-9	9.3 ± 1.7	1296
Benzo[*a*]pyrene + induced S-9	12.5 ± 5.8	1664

Table 2. Mutagenicity of heat-treated fly ash extracted with HCl or DMSO. Values not connected by the same line are significantly different from each other (pairwise comparisons of proportions, $P < .05$). The concentration of fly ash particles suspended in uninduced S-9 was 1068 μg/ml. The higher than usual value for mutagenicity in the controls can be attributed to the considerable age of the clone used here [micronuclear damage increases with age (*12*)].

Substance	Lethal cells (%)	Number of progeny examined
Glass beads	3.14 ± 0.33	624
HCl-extracted particles	4.77 ± 1.25	960
DMSO-extracted particles	8.21 ± 2.00	656
Unextracted particles	14.09 ± 5.29	896