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Geology

Experimental evidence that an asteroid impact led to the extinction of many species 65 million years ago[a]

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Contributed by Luis W. Alvarez, October 12, 1982

I used to be able to say just about everything I know about this subject in an hour. I could develop it in historical order in a standard-length lecture, but things have moved so rapidly in the last 2 years that it is quite impossible to follow that scheme anymore. Therefore, I am going to have to concentrate on the present state of our theory that an asteroid hit the earth 65 million years ago and wiped out large numbers of species, both on the land and in the ocean.

I think the first two points—that the asteroid hit, and that the impact triggered the extinction of much of the life in the sea—are no longer debatable points. Nearly everybody now believes them. But there are always some dissenters. I understand that there is even one famous American geologist who does not yet believe in plate tectonics and continental drift. We now have a very high percentage of people in the relevant fields who accept these two points. Of course, science is not decided by a vote, but it has been interesting to watch the consensus develop.

The third point, that the impact of the asteroid had something to do with the extinction of the dinosaurs and of the land flora, is still very much open to debate, although I believe that it very definitely did. But I will tell you about some of our friendly critics who think not. I will concentrate on a series of events that has led to a great strengthening of the theory. In physics, theories are declared to be strong theories if they explain a lot of previously unexplained observations and, even more importantly, if they make lots of predictions that are verified and if they meet all the tough scientific challenges that are advanced to disprove them. In that process, they emerge stronger than before.

So, I am going to tell you of a number of predictions that our theory has made; almost without exception they have been verified. And I will tell you of several serious questions and doubts that have been raised concerning the validity of the theory. People have telephoned with facts and figures to throw the theory into disarray, and written articles with the same intent, but in every case the theory has withstood these challenges. I will therefore concentrate on those things that show the theory to be a strong one, but I will not neglect a few "loose ends."

Instead of using the historical approach, which has been my custom up till now, I am going to start by following the cub reporter's checklist that he learned in journalism school. Every story should contain Who, What, When, Where, and Why.

First of all, Who? The original "Who" were the Berkeley group (Fig. 1). Let me introduce my colleagues, shown in alphabetical order, in our first major publication.[b] The second one is my son, Walter, who is a professor of geology at Berkeley. Frank Asaro and Helen Michel are nuclear chemists at the Lawrence Berkeley Laboratory. All of us have been involved in every aspect of the problem, since the earliest days. I have even been out looking at some rocks in Italy—a new experience for me. Helen Michel has collected rock samples in Montana, where there are dinosaur fossils. Her husband tripped over a previously undiscovered Triceratops (horned dinosaur) skull on one occasion. So, we have not been a group of people each working in his own little compartment, but rather we have all thought deeply about all phases of the subject.

When we sent the paper (1) to *Science*, Philip Ableson, its editor, had two comments. In the first place, it was too long. He could not publish it unless we cut it in half. It still turned out to be pretty long. Second, Phil said, "I have published quite a few papers on the cause of Cretaceous-Tertiary extinction in the last few years, so at least $n - 1$ of them have to be wrong." But in spite of that, he published ours, and we are most appreciative.

Since we first presented our results at three geological meetings (2–4) starting in early 1979, about 12 other groups have entered the field. The latest one to be heard from is a Russian group.

Now, the "What" category. We have very strong evidence that an asteroid (Fig. 2) hit the earth 65 million years ago at a velocity in the range of 25 kilometers per second. You may wonder how we got this picture of an asteroid that hit the earth 65 million years ago. Actually, this is a picture of Phobos, the larger of the two moons of Mars. It was taken by the Mars Orbiter, and I was surprised to see that it was pocked with craters. I had always imagined "our asteroid" as being a nice smooth, round thing that ran into the earth, but of course it must have been bumped into by many, many smaller asteroids and meteorites, so this is what it undoubtedly looked like. Phobos is actually twice the size of "our" 10-kilometer-diameter asteroid, but otherwise it looks exactly the same. From the color of Phobos NASA found that it is probably a carbonaceous chondrite, and we have very strong evidence that the asteroid that hit the earth was also of carbonaceous chondritic composition.

When the asteroid hit, it threw up a great cloud of dust that quickly encircled the globe. It is now seen worldwide, typically as a clay layer a few centimeters thick in which we see a relatively high concentration of the element iridium—this element is very abundant in meteorites, and presumably in asteroids, but is very rare on earth. The evidence that we have is largely from chemical analyses of the material in this clay layer. In fact, meteoritic iridium content is more than that of crustal material by nearly a factor of 10^4. So, if something does hit the earth from outside, you can detect it because of this great enhancement. Iridium is depleted in the earth's crust, relative to normal solar system material, because when the earth heated up and the molten iron sank to form the core it "scrubbed out" the platinum group elements in an alloying process and took them "downstairs." (We now use the trick of heating our rock samples with molten iron, to concentrate the iridium, and thereby gain greatly in signal-to-noise ratio.)

Abbreviations: C-T, Cretaceous-Tertiary; P-T, Permian-Triassic.

[a] Presented at the annual meeting of the National Academy of Sciences, Apr. 28, 1982, Washington, DC.

[b] Alvarez, L. W., Alvarez, W., Asaro, F. & Michel, H. V. (1979) Lawrence Berkeley Laboratory Report 9666.

EXTRATERRESTRIAL CAUSE FOR THE CRETACEOUS-TERTIARY EXTINCTION: EXPERIMENT AND THEORY

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FIG. 1. Title page of Lawrence Berkeley Laboratory Report 9666.

We come to "When" on the checklist. There are two time scales for the "When." The first one is the geological time scale (Table 1), which I now know the way I know the table of fundamental particles. Note that the 570-million-year time span from the beginning of the Cambrian up to now is called "Phanerozoic time"—that is, when there are easily observed fossils in the rocks. Phanerozoic time is divided into three eras: the Paleozoic, or old animals; the Mesozoic, or middle animals; and the Cenozoic, or recent animals. The fact that geologists characterize their rocks by the fossils that are in them shows us the close interrelationship between geology and paleontology.

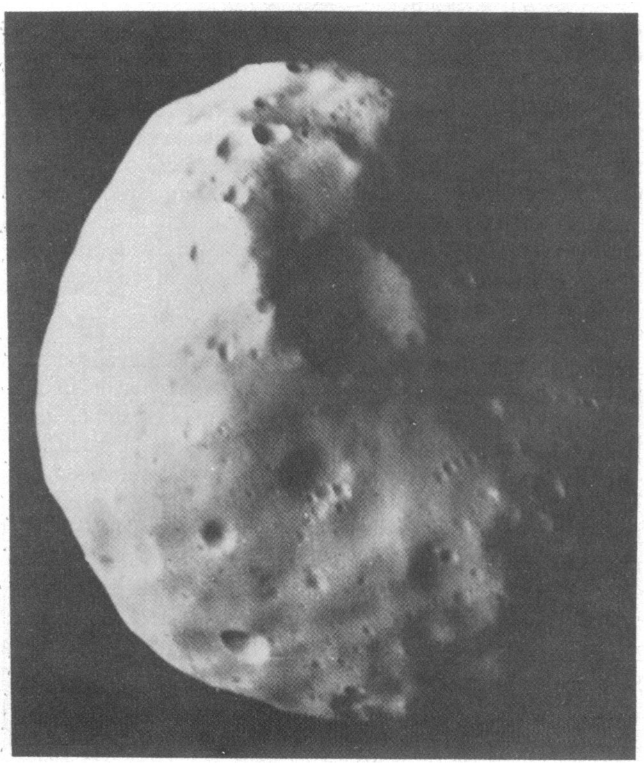

FIG. 2. Phobos, a satellite of Mars. (Photo courtesy of National Aeronautics and Space Administration.)

Table 1. Geological time chart of the Phanerozoic eon

Era	Periods or system*	Epochs or series†	Time since beginning, yr
Cenozoic	Quaternary	Recent	
		Pleistocene	1,000,000
	Tertiary	Pliocene	12,000,000
		Miocene	30,000,000
		Oligocene	40,000,000
		Eocene and Paleocene	65,000,000
Mesozoic	Cretaceous		120,000,000
	Jurassic		155,000,000
	Triassic		225,000,000
Paleozoic	Permian		250,000,000
	Carboniferous (Pennsylvanian; Mississippian)		300,000,000
	Devonian		350,000,000
	Silurian		390,000,000
	Ordovician		480,000,000
	Cambrian		570,000,000

* Period of time or system of rock.
† Epochs of time or series of rock.

I am going to concentrate most of my attention on what could be called the Mesozoic-Cenozoic boundary, but everyone calls it the "Cretaceous-Tertiary (C-T) boundary." It is 65 million years old. I will also talk briefly about the Permian-Triassic (P-T) boundary. That is when there was another major extinction. I should say that there have been five major extinctions in Phanerozoic time (5). I will also say something about the boundary between the Eocene and the Oligocene which is at about 34 million years ago and was accompanied by a less-severe extinction event.

Raup and Sepkowski (6) recently published a definitive article on the five major extinctions, from which I have used a plot of the number of extinctions at the family level, per million years, against time (Fig. 3). Such a graph makes me feel right at home because for a good many years I was called a "bump hunter"—a particle physicist who looks for "resonances" or peaks that stick out above a distribution of background points. Fig. 3 shows that there *is* a substantial background of extinctions; individual families are going extinct all the time, for natural reasons quite unconnected with the events that have triggered the five "major extinctions." And those who criticize our asteroid theory of the C-T extinctions have known about this background for much longer than I have. But I think that on many occasions, they have, as we would say in physics, confused some background events with events that really belong to the peak. I mention this because I believe that such a confusion has contributed to the present controversy concerning the validity of the asteroid hypothesis. When we point to a number of species that went extinct precisely at the iridium layer, our critics commonly discount those extinctions by pointing to other species that were obviously "on the way out," just before the asteroid hit. That is what I call "confusing the background with the peak events," and if I did not direct attention to this graph, you might find those arguments against our theory more persuasive than the evidence warrants.

The second time scale is the present time scale and is concerned with the discovery of iridium enhancements in the geological record and with their interpretation in terms of an asteroid impact. We started our search 5 years ago. We saw our first iridium "spike" 4 years ago. We were looking for iridium but, it turns out, for the wrong reason. The first time we saw

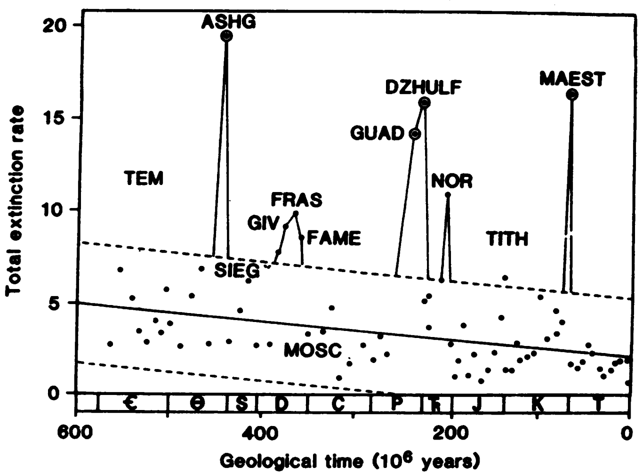

FIG. 3. Total extinction rate (extinctions per million years) through time for families of marine invertebrates and vertebrates. The plot shows five mass extinctions: late in the Ordovician (ASHG), Devonian (GIV-FRAS-FAME), Permian (GUAD-DZHULF), Triassic (NOR), and Cretaceous (MAEST) periods. The late Devonian extinction event is noticeable but not statistically significant. [From Raup and Sepkowski (6), by permission of the American Association for the Advancement of Science, copyright 1982].

the iridium enhancement we did not have a sufficiently complete set of rock samples, so Walter went back to Gubbio, Italy, and collected the set whose analysis makes up the points shown in Fig. 4. We plotted that curve, 3 years ago, and showed it at a number of geological meetings. This is an unusual diagram, with time plotted upward, in a linear mode in the middle section and in a logarithmic mode in the top and bottom sections. The iridium concentration, which has been fairly constant for 350 meters below the C-T boundary, increases sharply, by a factor of about 30, in the 1-cm clay layer and then decreases as one goes into the earliest Tertiary limestones. For the rest of the 50 m above the boundary, the iridium concentration is at the background level seen in the late Cretaceous limestones.

This is the very large signal that we explained as being due to the impact of an extraterrestrial object. If I were following the historical approach, I would give you our original justification for that conclusion. But instead I will later give you more recent data that show beyond any question that the clay layer contains "undifferentiated" solar system material, with a composition that matches that of carbonaceous chondrites with surprising accuracy. Our first thought was that the material came from a supernova because some paleontologists believed at that time that a nearby supernova was responsible for triggering the C-T extinction. But we soon found that the clay was too similar to solar system material to be from a supernova. I sent a letter to Malvin Ruderman, a physicist friend of mine and one of the key exponents of the supernova theory, explaining why we could no longer accept his theory. He wrote back a very short letter saying, "Dear Luie: You are right, and we were wrong. Congratulations. Sincerely, Mal." That is something that made me very proud to be a physicist, because a physicist can react instantaneously when you give him some evidence that destroys a theory that he previously had believed. But that is not true in all branches of science, as I am finding out.

So, 3 years ago we had this graph and this theory. We wrote it up, and it was published in *Science* (1). Now, a little more on "When." Since our original work, there have been three conferences on the subject, because it is such a rapidly evolving field. The first conference (7) was held about 1 year ago in Ottawa under the sponsorship of the National Museums of Canada; about 25 people were there, people who study meteorites, impact craters, geology, paleontology, and quite a range of subjects, and we had a very good 3-day meeting.

Last fall there was a 4-day meeting at Snowbird, Utah. It was sponsored by the Lunar and Planetary Institute and by the National Academy of Sciences. One hundred and ten people attended that meeting, which lasted 4 days. They came from more fields than you can imagine, including atmospheric modeling, impact dynamics, chemistry, physics, asteroids, and, of course, geology and paleontology. We had a very good exchange of views, and almost everyone in this new field had a chance to meet "all the players." More recently, there was a day-long seminar (8) on this subject at the 1982 meeting of the American Association for the Advancement of Science.

Now, the "Where." The iridium enhancement was first seen near a little town called Gubbio, which is in the north central part of Italy. It is directly north of Rome and directly east of Siena in the Apennines. The rocks there were laid down as limestone on the bottom of the ocean from 185–30 million years ago, and then a few million years ago they were raised up in the mountain-building process. They were then eroded by running water, and fortunately engineers built roads up through the canyons so that someone like me, an armchair geologist, could get there in comfort. I found that I could get out of the car, wield a geologist's hammer to break a new surface of the rock, and look at the little creatures that lived there and see how they changed with time.

It is really dramatic to observe the little things called foraminifers (order Foraminifera) which are shelled creatures about 1 mm in diameter (Fig. 5*b*). You can see them with a hand lens literally by the thousands, right up to the boundary, at apparently constant intensity, and then, without warning, they are gone, right at the clay layer. It was really a catastrophe. They were suddenly wiped out. The only foraminifers that escaped extinction were the tiny species *Globigerina eugubina* that can be seen in the thin section, above the boundary line in the same figure.

Fig. 6 shows what the rocks look like. This layer was deposited 65 million years ago, and it is seen many places worldwide. We took it upon ourselves to analyze the layer by neutron activation analysis, looking particularly for iridium. You have already seen the iridium enhancement, which surprised us so greatly when we first saw it in 1978.

The limestone in this region is about 95% calcium carbonate and about 5% clay. The calcium carbonate comes from the shells of the little animals that live in the ocean and fall to the bottom when they die. The clay is washed down from the continents and carried out to sea by river currents. The two components fall to the ocean floor, where they are compacted to form the limestone.

It was generally assumed, before we did our work, that the clay in the layer was of the same origin as the clay in the limestone, but that turns out not to be the case. After we had seen the iridium in the layer and concluded that it came from an asteroidal impact, we made our first prediction—that the gross chemical composition of the clay layer would be substantially different from that of the clay in the Tertiary and Cretaceous limestones above and below the layer and that these latter two clays would be essentially identical. We published measurements in our *Science* paper that showed that this first prediction was verified.

Our second prediction was that the iridium enhancement would be seen worldwide. At that time we had only seen it in one place in Italy, in a valley near Gubbio. We knew that the extinctions were worldwide. So, we guessed and predicted that the iridium would be seen worldwide and in fact it is. Before

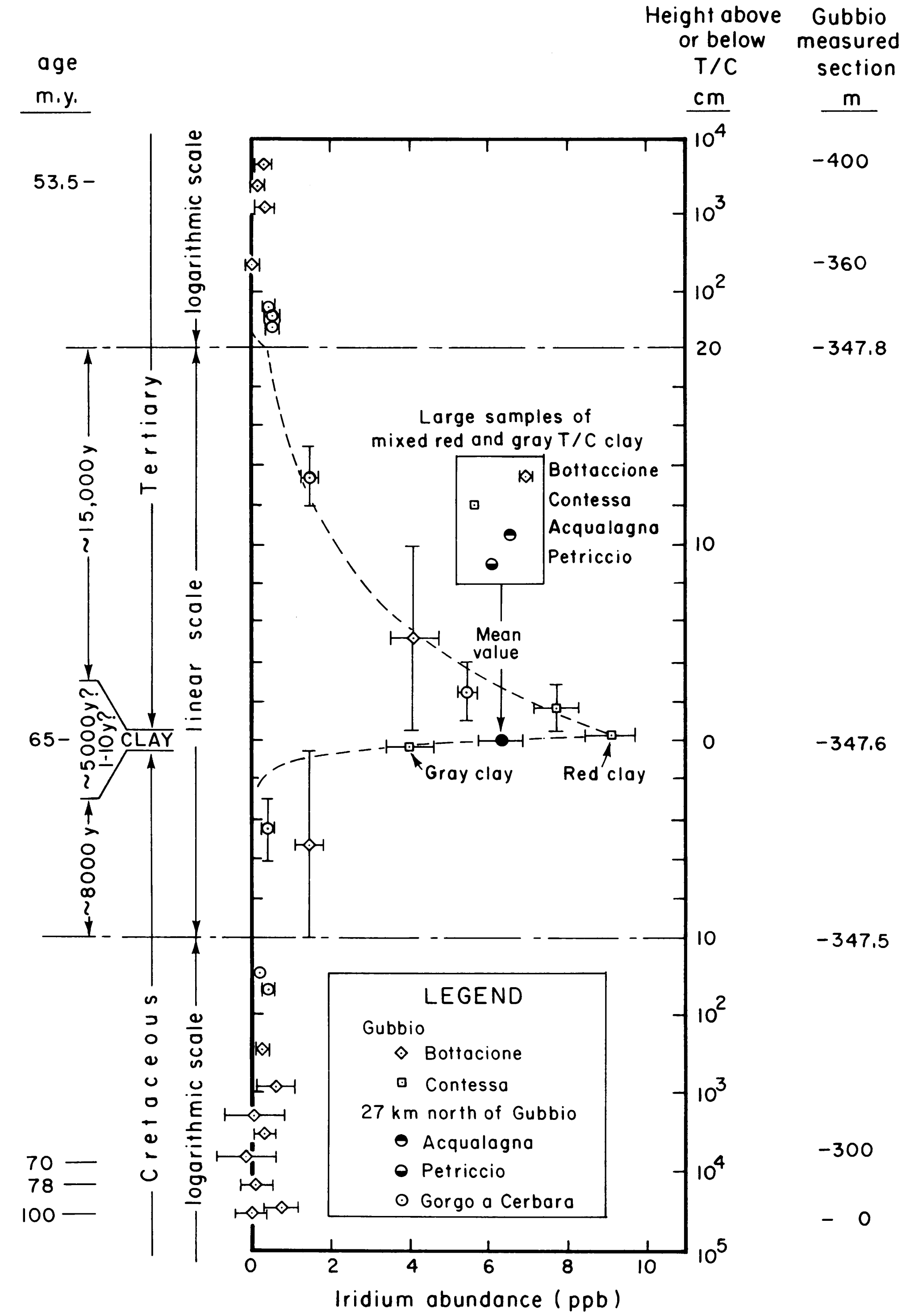

FIG. 4. Iridium abundance per unit weight of acid-insoluble (2 M HNO_3) residues from Italian limestones near the C-T boundary. Error bars on abundances are SD for counting radioactivity. Error bars on stratigraphic positions indicate the stratigraphic thickness of the samples. The dashed line is an "eyeball exponential fit" to the data.

we published our 1979 paper, we had samples from Denmark, which Walter collected, and also some from New Zealand that Dale Russell was kind enough to give us. Both of those showed a nice iridium enhancement. Both enhancements were bigger than the one we saw in Gubbio. In fact, as shown in Fig. 7, we first discovered the iridium in nearly the hardest place to find it, where the iridium concentration was quite small compared to most places. The number indicated for each site is the measured iridium, in nanograms per square centimeter, at that location. This is of course the area under the curve of the type in Fig. 4, times the density of the rock.

At the present time, there are more than 36 locations where the iridium has been found. With one exception the iridium has been found every place that has been thoroughly looked at by our laboratory. Whenever a paleontologist says, "This is the C-T boundary," one of the groups now looking for iridium collects some rock samples and finds the iridium enhancement by using neutron activation analysis. The one place where this is not true

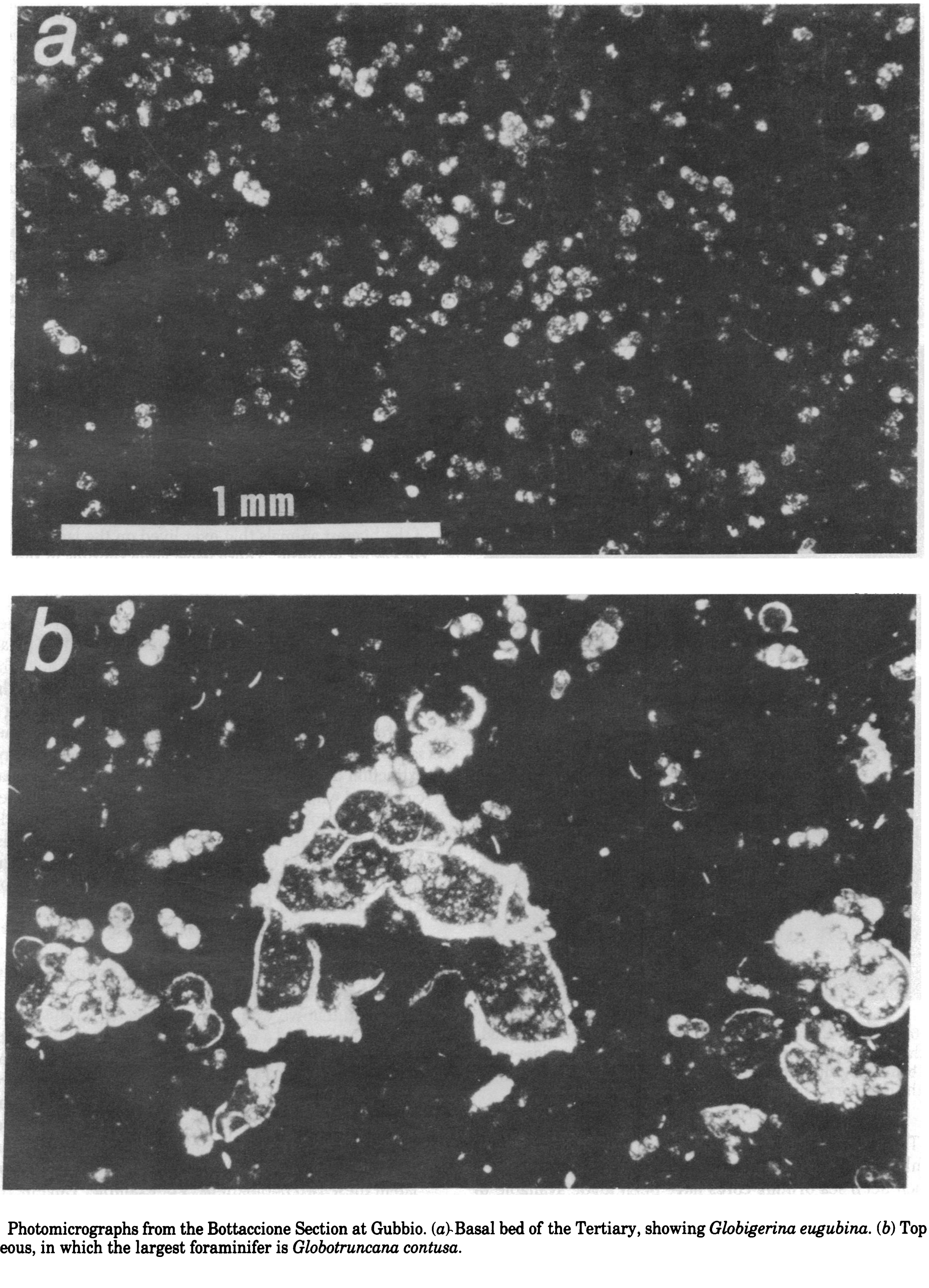

FIG. 5. Photomicrographs from the Bottaccione Section at Gubbio. (*a*) Basal bed of the Tertiary, showing *Globigerina eugubina*. (*b*) Top bed of the Cretaceous, in which the largest foraminifer is *Globotruncana contusa*.

is in Montana. We have two sites in Montana where there are abundant dinosaur fossils. But it is not so easy to pick out the C-T boundary, and there is no obvious clay layer. (The clay layer is seen in nearly all the marine deposits.) In one of these Montana sites, we have iridium, but we have not found it at the other site, even after two summers of sample collecting. So, it is almost correct to say that iridium has been found at every identified C-T site that anyone has looked at. In all of the pelagic or ocean-based sites, the iridium was laid down on the ocean floor 65 million years ago, and it has been found, in all our studies, within 10 cm and often within 1–2 cm of the place where the paleontologists said we should look.

I think it is interesting that, after seeing the iridium at one site in Italy, we predicted that it would be seen worldwide at

FIG. 6. (*Upper*) L.W.A. (left) and W.A. pointing to the C-T boundary in the Bottaccione Gorge near Gubbio, Italy. (*Lower*) Close-up of the C-T boundary, with a coin (similar to a U.S. quarter) indicating the size of the boundary.

the C-T boundary, and Fig. 7 shows that that prediction has been fulfilled. You will see that there are sites in both oceans for which deep sea drilling cores have been made available to us, and to other groups. The largest amount of iridium (in the north central Pacific) is 330 ng/cm^2. As a physicist, I had expected that, when we got a map like this, we would be able to draw lines connecting places with equal iridium values and then we would be able to mark the center, as on a contour map and say, "This is where the asteroid hit." But that is not the way things work in the much more complicated world of geology.

Now, we come to "Why," the last item on the checklist. Why did we study this problem in the first place? I do not really have to explain that to an audience of this kind. If I did, I would probably use George Mallory's famous response as to why he tried to climb Mount Everest, "Because it is there." But if I wanted to get more serious, I would say that, a few years ago, the four of us suddenly realized that we combined in one group a wide range of scientific capabilities, and that we could use these to shed some light on what was really one of the greatest mysteries in science—the sudden extinction of the dinosaurs. How many species or genera went out, 65 million years ago? I get a different set of numbers from every paleontologist I talk to, but everyone agrees that it was simply a terrible catastrophe. Most of the life on the earth was killed off; about half of all the genera disappeared completely, never to be seen again.

At this point some comment about the disappearance of the dinosaurs is in order. They were reptiles, and the land reptiles went out in a really catastrophic way. In all, there were several orders of reptiles that disappeared completely, including giant marine reptiles. Normally, one talks about an extinction at the species level. The passenger pigeon disappeared in the last century. The condors are probably going out soon. Each is a species extinction. Above that we have a genus or many genera; above that comes the family; above that is the order and, for fauna, the only higher taxa are class and phylum. Thus, an extinction that suddenly wiped out several orders was a spectacular catastrophe—not to be attributed to some ordinary environmental change, as some of my friends believe. Dinosaurs were some of the biggest animals that ever lived on the land, *Tyrannosaurus rex* for example. There were large reptiles in the seas—the plesiosaurs. There were large reptiles that were flying around in the air—the pterosaurs. All disappeared suddenly, never to be seen again. I simply do not understand why some paleontologists—who are really the people that told us all about the extinctions and without whose efforts we would never have seen any dinosaurs in museums—now seem to deny that there ever was a catastrophic extinction. When we come along and say, "Here is how we think the extinction took place," some of them say, "What extinction? We don't think there was any sudden extinction at all. The dinosaurs just died away for reasons unconnected with your asteroid." So my biggest surprise was that many paleontologists (including some very good friends) did not accept our ideas. This is not true of all paleontologists; some have clasped us to their bosoms and think we have a great idea.

To this overview of the situation I should just add one point. Dinosaurs did last for nearly 140 million years from the early Mesozoic, which is sometimes called the age of reptiles, and we believe that had it not been for the asteroid impact, they would still be the dominant creatures on the earth. We would not be sitting here. At least we would not look as we do; it has been suggested that we would have distinctly reptilian features.

Now I must add a few odd facts that do not fit into the checklist that I started out with. One is that "earth orbit crossing asteroids" are studied by two groups of people. One group looks at them as astronomical objects by using Schmidt cameras on whose photographic plates the asteroids appear as streaks moving relative to the background stars. The other group studies craters, either on the moon or on the earth. There is some overlap in these two populations. For example, Eugene Shoemaker is an expert in both of these fields.

All of these people agree that there is a power law relationship between the mean time to collision of an asteroid of a given size and its diameter: the mean time to collision is roughly proportional to the square of the diameter of the object. These two groups of people also agree on the absolute numbers. What they say is that an object 10 kilometers in diameter should hit the earth every 100 million years, on the average. If you drop the size by a factor of 10, to 1 kilometer, then you drop the mean time to collision by a factor of 100, to 1 million years. If you go down to 100-meter objects, these hit the earth about every

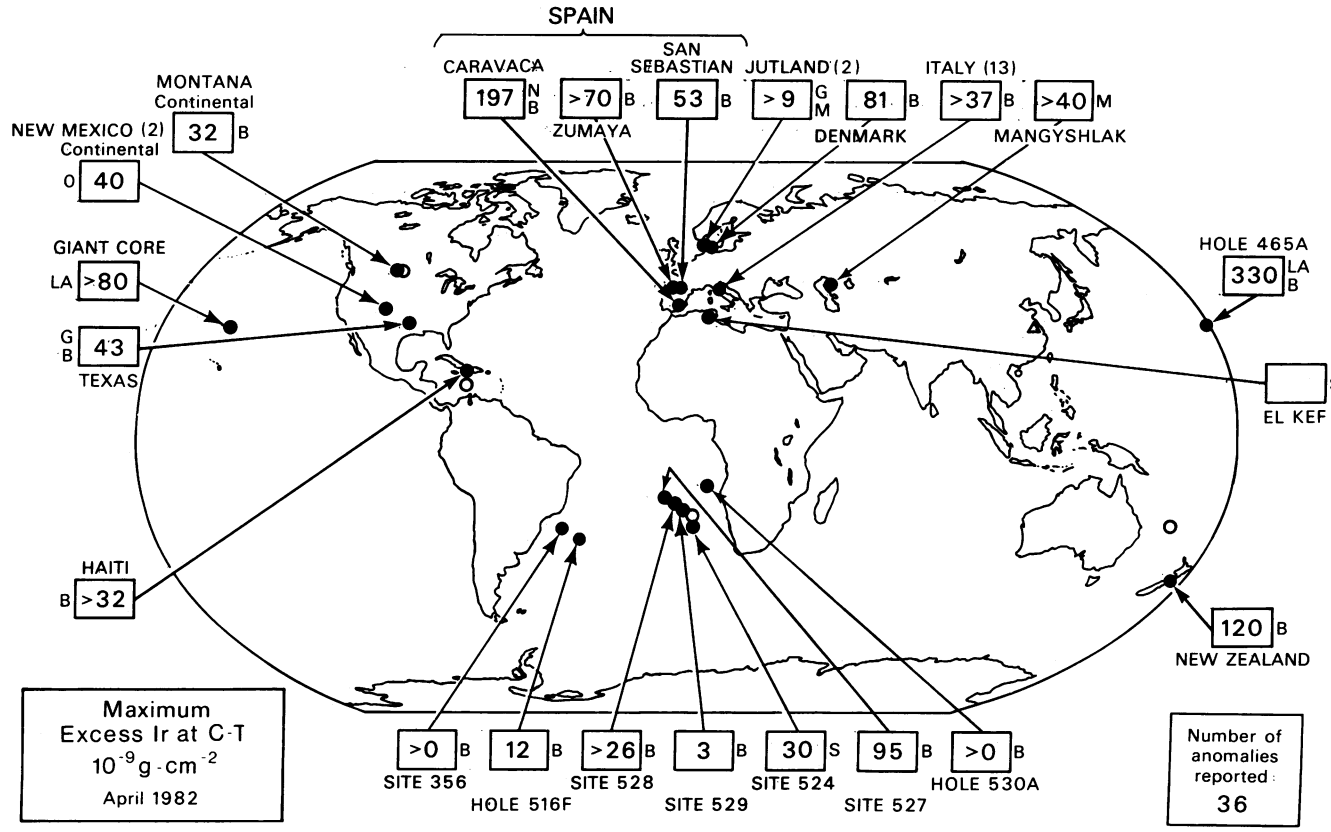

FIG. 7. Map of the world with locations of iridium anomalies. Laboratories: B, Berkeley; LA, University of California at Los Angeles; O, Los Alamos; G, Ganapathy (Baker, Co.); S, Swiss group; N, Netherlands; M, Moscow.

10,000 years. That power law goes over an enormous range of sizes. It has been verified on the moon, where you can see very small craters. On the earth, the little craters have been eroded away or the objects burned up in the atmosphere, so you can only see the evidence of the big ones.

There have been five major extinctions in the last 570 million years, and our third prediction was that all of these would turn out to be caused by the same mechanism, an asteroid collision. That is one prediction that has not turned out to be true, but it does have an element of truth. We have only looked at one other of the five major extinctions, the P-T. It is hard to sample, because the best sites are in China. Frank Press, working through our National Academy and the Chinese Academy, helped us get one of the two sets of samples of P-T rocks that we have analyzed. There is a clay layer between the limestone-like rocks at the P-T boundary. We felt sure that there would be lots of iridium there. But there is not any that we can find.

However, we are very intrigued by the existence of that layer, whose basic chemistry is quite different from that of the rocks above and below it. The fact that it exists was not widely known until quite recently; Walter learned about it less than 3 years ago. Our present best guess is that it is of volcanic origin, but it might be consistent with the idea that the layer was laid down by a cometary impact. Comets can go much faster than asteroids and, in fact, can have 50 times the specific energy. So a comet could throw the same amount of dust into the atmosphere and do the same damage, while bringing in only 1% as much iridium. That factor of 100 comes from the square of the increased impact speed times perhaps a factor of 2 because a comet is typically half composed of ice. That is simply one possible working hypothesis. There is no proof for it. But if it does turn out to be true, then we will know that the C-T extinction was due to an asteroid, and not a comet, as some of our friends are calling it. At this point, I think the distinction is of no importance; the important conclusion is that a large chunk of undifferentiated solar system material hit the earth 65 million years ago and triggered a major extinction.

Although our prediction was not confirmed in the P-T case, it did lead to another case in which there is a coincidence between an iridium layer and an extinction, although not one of the five major ones. Some people say, "I'll bet there are lots of iridium layers all over the place, so there is no reason to say that the oceanic and terrestrial iridium layers are synchronous." But in my view, that is an exercise in grasping for straws because it turns out that there are very few iridium layers. No one has yet made a systematic search through all of geological time, but two groups have systematically searched a total of 23 million years of sediments and found not a single iridium enhancement in this randomly selected 4% of Phanerozoic time. One group, led by Frank Kyte and John Wasson of the University of California at Los Angeles, has searched through the lowest 15 million years of the Tertiary limestones; and the other group, led by Carl Orth of Los Alamos, has searched through 8 million years of the late Devonian.

We found a very definite iridium enhancement in the Caribbean Sea, at the Eocene-Oligocene boundary (9, c), 35 million years ago, and it was independently found by R. Ganapathy (10, d) of the Baker Chemical Company. Both of our groups looked

[c] Asaro, F., Alvarez, L. W., Alvarez, W. & Michel, H. V., Conference on Large Body Impacts and Terrestrial Evolution: Geological, Climatological, and Biological Implications, Snowbird, Utah, Oct. 19–22, 1981, p. 2 (abstr.).

[d] Ganapathy, R., Conference on Large Body Impacts and Terrestrial Evolution: Geological, Climatological, and Biological Implications, Snowbird, Utah, Oct. 19–22, 1981.

there because that boundary coincided with a known layer of microtektites and with a lesser extinction event. That was very exciting to us because, shortly before we did this work, Billy Glass, a leading expert on microtektites, and his collaborators, had shown that these microtektites—part of the "North American strewn tektite field"—extended more than halfway around the world (11). And here again, "everybody" (all but one person) believes that tektites are due to the impact of large meteorites (or small asteroids) on the surface of the earth. Also, Billy Glass points out that, at the tektite "horizon," there was an extinction of several species of Radiolaria, much like the foraminifers I talked about earlier, but their chemistry is siliceous rather than calcareous. [Note Added in Proof: In collaboration with Billy Glass, we have recently found three new and quite substantial iridium enhancements at the Eocene-Oligocene tektite horizon in deep sea drilling cores from the Gulf of Mexico, the central Pacific, and the Indian Ocean.]

So, we have several different bits of evidence that tie impacts to extinctions. We have the iridium layer at the tektite layer and we have the extinction of the radiolarians at that same time. Although we did not find any iridium at the P-T boundary, we did find another iridium enhancement coincident with an extinction, and at the present time there are only two known stratigraphic levels where there is a sudden excess of iridium that is seen in more than one location. In regard to the connection between extinctions and impacts, the theory seems to be holding up very well on that score, and the third prediction can be considered to have been partially confirmed. Asteroid impacts have produced more than one extinction but not all five of the major ones.

Prediction number four is that there should be an iridium enhancement at the C-T boundary, on the continents as well as on the sea floor. A lot of people were saying, 2 years ago, that the reason we found iridium in the sea floor deposits was that some change in ocean chemistry, 65 million years ago, precipitated out the iridium that was dissolved in the ocean. We had given two arguments in our paper as to why that was not so, but we could not prove it conclusively. We asked one of the national funding agencies for money to search for iridium in Montana, alongside the dinosaurs, and one of the peer reviews that came back said, in effect, "These guys would be wasting their time and your money if they did this job, because the iridium came out of the ocean and therefore won't be seen in continental sites." Fortunately we were able to do it anyway; we went up to Montana and looked for iridium.

But before we got our first iridium there, Carl Orth from Los Alamos and his colleagues discovered that there was iridium at a continental site in New Mexico (12). I think this was a very important discovery, and I want to show you Carl Orth's curves.

They drilled a hole in the Raton Basin, in New Mexico, and subjected the rocks to neutron activation analysis with a higher sensitivity than anyone else has attained. In Fig. 8, the scale of iridium abundance is logarithmic. The iridium suddenly went up by a factor of 300 precisely where the paleontologists told them to look. That was a very exciting thing because it showed that the iridium did not come out of the ocean. It was deposited on the continents, as well as on the ocean floor, as called for by our prediction number four. So the Los Alamos discovery added great strength to our theory, as far as some of its critics were concerned. We were not surprised because we thought the arguments we had given against an oceanic source of the iridium were quite valid.

Just a few days before I saw Carl Orth's preprint, which he kindly sent me, I had read a paper (13) by Leo Hickey, who is a paleobotanist in Washington and who has been one of our most vocal critics. He is also a very good friend. Walter went to graduate school with him and they have been close personal friends ever since. His paper in *Nature* was entitled "Land Plant Evidence Compatible with Gradual, not Catastrophic, Change at the End of the Cretaceous." He wrote this paper after seeing all the evidence that we presented, and I could not find anything in it that made me feel that he was ignoring our evidence; he was just looking at a different data base and coming to a different conclusion.

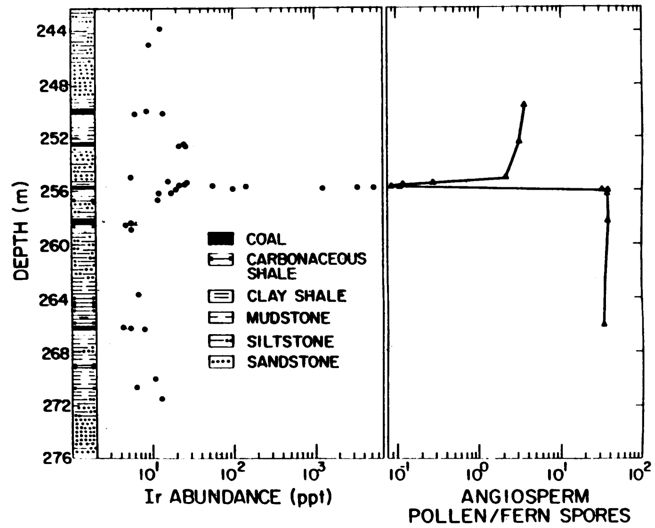

FIG. 8. Iridium abundances (●) and ratios of angiosperm pollen to fern spores (▲) as a function of core depth and lithology. The surface Ir density is ≈40 ng/cm^2. (Reprinted from Los Alamos report LA-UR-81-2579.)

His abstract ends with this sentence: "However, I report here that the geographically uneven and generally moderate levels of extinction and diversity change in the land flora, together with the nonsynchroneity of the plant and dinosaur extinction, contradict hypotheses that a catastrophe caused terrestrial extinctions." So, his considered opinion after studying all the evidence and looking at what he saw in the plant record convinced him that we were wrong. He says quite clearly that there was no effect of a catastrophe on the plants. And I had no evidence that directly contradicted his conclusions.

So, you can imagine my excitement when I saw Carl Orth's data as plotted in the right-hand side of Fig. 8, showing the number of pollen grains per cm^3 plotted against stratigraphic height, and normalized to the fern spore count. The interesting thing is that the pollen count drops by a factor of 300, in precise coincidence with the iridium enhancement. I must say that this looks to me like a catastrophe. In fact, several pollen types disappeared from the record at this point. Fig. 9 shows that the resolution of the pollen fall-off is undoubtedly limited by the sample thickness, 2 cm. The drop-off of a factor of 300 occurs from one rock sample to the next, and my guess is that the discontinuity is even more precipitous than this graph shows. This is by far the sharpest resolution that paleobotanists have ever seen, as far as I can learn, and it is not surprising that it has been missed in the past, just because of its sharpness. (And it confirms prediction number five in our paper—there would be an extinction of plants in coincidence with the iridium layer, on the land.)

To show that the missing of a "sharp spike" is not peculiar to paleobotany, let me remind the physicists in the audience how the psi meson was discovered at Stanford several years ago. The SLAC-SPEAR electron-positron colliding ring had been operating for some time, without anything "very interesting" being found. It was exploration of new territory, and the physi-

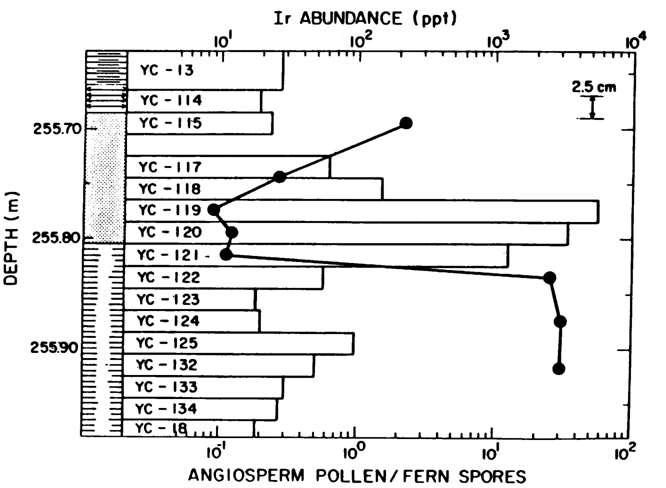

FIG. 9. Expanded view of Fig. 8 in the vicinity of the iridium anomaly and the pollen break. The Ir abundances are given by the histogram; the angiosperm pollen/fern spore ratios are shown by the solid circles. The lithologic symbols are the same as in Fig. 8 except for coal, which is shown by stipple. (Reprinted from Los Alamos report LA-UR-81-2579.)

cists were looking for enhancements in the counting rate ("bump hunting") by stopping every 100 MeV—equivalent in paleontology to taking a sample every meter. They were unhappily coming to the rather firm conclusion that there were no new "resonances" in this energy region; such resonances were expected to be more than 100 MeV wide. But as a result of some excellent detective work, with attention paid to the slimmest of clues, the SLAC-LBL group looked between a pair of 100-MeV "milestones" and discovered the extremely narrow psi resonance that sent the counting rate up by more than a factor of 100, within the space of 1 MeV and within an observing time interval of 2 hours. The important point I want to make is that, after those 2 hours of excitement at Stanford, no one ever said again that there was nothing interesting going on in that wide energy range. The psi "bump" from then on was a part of the lore of physics.

Hickey has behaved quite differently with respect to the "narrow spike" discovered by Carl Orth; he ignored it. At the annual AAAS meeting (8), some months after the Snowbird conference, he repeated the conclusions of his *Nature* paper, that the pollen spectra showed no evidence for a catastrophe, and said, "Every pollen spectrum that has come in since this chart was plotted tends to corroborate these data." However, the narrow "glitch" in the pollen spectrum (Figs. 8 and 9) contradicts the idea that the plants did not notice the asteroid impact. (My own guess is that, before long, this graph will be reproduced in every textbook on geology and paleontology.)

I consider Orth's important paper to be a confirmation of three separate predictions or deductions we made in our *Science* paper. Prediction number four was that the iridium would be found on the continents as well as on the ocean floor. Prediction number five was that the plants would suffer simultaneous extinctions, just as the animal life had. And prediction number six was that the iridium did not come from a supernova.

Hickey asserted (8) the plant and dinosaur extinctions were "nonsynchronous" but I think I will soon convince you that he was wrong in that.

Science has published, in the last year, three separate reports on the state of the asteroid theory. They were all written by Richard Kerr. The first one (14) entitled "Asteroid Theory of Extinction Strengthened," reported interviews with people who thought that we were wrong for a number of reasons. I thought the strangest reason was that we found *too much* iridium in the Danish clay layer. Several experts on cratering were quoted as saying that we should not have found nearly that much iridium because, when the asteroid hits, the material going up into the stratosphere should be not only that of the asteroid but also crust material equivalent to 1,000 to 10,000 times the mass of the asteroid. Richard Grieve of the Department of Energy, Mines and Resources, of Canada was quoted as favoring the figure 1,000; Tom Ahrens of California Institute of Technology was said to prefer 10,000. We had used a dilution of 1:60 in our paper, a factor we had gotten from Richard Grieve by telephone a few months earlier. So we were surprised by Kerr's *Science* report. It turned out later that both of these gentlemen's remarks had been misinterpreted. They both had said that the material close to the crater would be diluted by these very large factors, and that ties in well with what we know about Meteor Crater in Arizona—there is very little meteoroid material close to the crater. But both men agreed that the material that was sent up high and would be spread worldwide would be diluted 1:20 to 1:100, in line with what we observed. So, that was a major challenge which the theory met and, in the process, came out stronger. Everybody now agrees that the iridium concentrations we find are consistent with the asteroid impact hypothesis.

Another report by Kerr in *Science* (15), entitled "Impact Looks Real, the Catastrophe Smaller," came after the Snowbird meeting in November 1981 and indicated that a consensus had formed in favor of the asteroid theory. There we had come up against a really serious challenge, involving good science, in which the new numbers were in serious disagreement with the corresponding ones we had used in our *Science* paper. We had said that the time for the dust to fall out of the stratosphere was about 3 years, which gave it time to spread slowly across the equator; winds would spread it very rapidly across all longitudes, near its original latitude. We based our numbers on the observations we found recorded in a thick volume published by the Royal Society (16) soon after the volcanic explosion of the island of Krakatoa in the Dutch East Indies, in 1883. But at Snowbird, Brian Toon,[e] of the National Aeronautics and Space Administration (Ames, IA), said the dust would fall out in 3–6 months, so our mechanism for getting it from one hemisphere to the other would not work. We therefore were in very serious trouble, except for one comforting fact—we had already seen the iridium layer worldwide, so we knew there had to be a transport mechanism.

How did the Royal Society go wrong, almost a hundred years ago, and how did we recover from that mistake? Professor Stokes, of Stokes law fame, measured the size of the dust particles by the angular diameter of their diffraction rings, and calculated the time of fallout to be 2–2.5 years, in agreement with the duration of dusty sunsets that were seen worldwide. We took his word for it. We said we thought "our" (much more copious) dust would stay up about 3 years. But, more recently, dust has been found to fall out much more quickly than that because the dust particles grow by accretion and, as Stokes equation predicts, fall faster. So, after Krakatoa, the "dusty sunsets" were at first made by the dust, but it fell out in 3–6 months. Unknown to Professor Stokes, the job of making the sunsets dusty was smoothly taken over by the much finer aerosols that accompany *volcanic* eruptions but not impact explosions. They did their

[e] Toon, O. B., Pollack, J. B., Ackerman, T. P., Turco, R. P., McKay, C. P. & Liu, M. S., Conference on Large Body Impacts and Terrestrial Evolution: Geological, Climatological, and Biological Implications, Snowbird, Utah, Oct. 19–22, 1981.

work for the next 2 years, but Toon correctly pointed out that we could not use such aerosols to keep the sky dark, 65 million years ago.

We knew there had to be a mechanism to get the dust spread worldwide, but our original idea that it was spread through the stratosphere went down the drain. It takes more than 1 year for material suspended in the atmosphere to move from the northern hemisphere to the southern hemisphere. The Russian hydrogen bomb tests in the 1950s made a lot of carbon-14, and that was observed to move from the northern hemisphere to the southern in about 1 year. So, if the dust fell out in 3–6 months, it could not get from one hemisphere to the other. But we had already found it in both hemispheres. So something was wrong.

Fortunately, the next day at the Snowbird Conference, two groups reported that the material got spread not by stratospheric winds but by either of two much faster mechanisms. Jones and Kodis[f] from Los Alamos, showed that the material actually went into ballistic orbits and was spread worldwide in a matter of hours. We had known, of course (from a calculation on the back of an envelope), that there was enough energy brought in by the asteroid to put the observed material into ballistic orbit, but we could not think of a detailed mechanism that would accomplish that feat. We did not see how you could get the little particles up through the atmosphere, but people at Los Alamos and Pasadena used very large computers and ran a simulation of an asteroid coming down and hitting the earth. It turned out that convective vertical winds in the fireball did the job. They analyzed a cylindrical asteroid coming downward vertically; the symmetry introduced in this way simplified the calculations. Both groups showed that when the asteroid hit it would distribute the material worldwide, as we saw it distributed very rapidly, and that it would be diluted by between 20 and 100 times its incoming weight, also as we had seen.

So, all of a sudden, everything was in great shape. The computers did not know that we were in trouble, but they got us out of it very nicely. It turned out that Ahrens and O'Keefe,[g] who did their work in Pasadena, were actually wired in by a special line to the Berkeley computer. That computer was down in the basement of our building, cranking away on this problem of great interest to us, and we did not know it.

Now, for a couple of other odd facts. Miriam Kastner (17, 18) of the Scripps Institute of Oceanography, has shown that the boundary clay layer in Denmark was a glass 65 million years ago as a result of a volcanic or impact eruption. Smit and Klaver (19, 20) found, in Spain and Tunisia, large numbers of very unusual, tiny "sanidine spherules" embedded in a very narrow iridium-bearing clay layer at the C-T boundary. Smit (20) argued from these that the layer is either of impact or volcanic origin and, because the relative abundances of the rare elements match that of carbonaceous chondrites, but not that of crustal or mantle material, he concluded that it is of impact origin. These two separate observations confirm our implied prediction number seven. Alesandro Montanari, a student of Walter's, has also found these same unusual spherules in the Italian clay.

Smit has shown that the sanidine-bearing layer in Spain, where he does his work, is about 1 mm thick, which shows that it was deposited in a period of 50 to 300 years (or less). So, paleontologists now have a time marker which is seen worldwide, and which we now know, from geological observations, to be laid down in an exceedingly short time. From the computer simulations, which I happen to believe, we know that the layer was laid down even much faster. The so-called hydrodynamic computer programs used in these computer simulations are like the ones used to design nuclear weapons; they involve temperatures, pressures, and material velocities much higher than those found under normal conditions, and they are known to do their tasks with great precision. A typical computer run involves many billions of numerical calculations. So far as I know, such great computing power has never before been brought to bear on problems of interest to paleontologists.

Now, as far as killing mechanisms are concerned, we had trouble finding our first killing mechanism. We had to discard all culprits but the asteroid. So finally, we said, "Okay, let us accept the fact that the material that we see worldwide had to fall down through the atmosphere. We now see that it is a few centimeters thick. Let us take that material and distribute it in the atmosphere in any kind of particles and with any spacing that you can imagine. It is going to be very, very opaque." We originally thought the sky would be black for 3 years. Now, the number is 3–6 months, and the scenario that we came up with was that the darkness would stop photosynthesis, all the little phytoplankton on the surface of the ocean would die and fall to the bottom, and the food chain for the larger animals in the sea would be disrupted. On the land, the plants would also die. Herbivores would die of starvation, and carnivores would die because they would not find anything to eat. That was just the first of several killing scenarios. I am confident that it is the only one we need to explain the catastrophic extinctions in the oceans. The lack of sunlight will quickly kill the phytoplankton in the surface layers and, when that base of the food chain is eliminated, most of the life in the sea is doomed to a relatively quick death.[h] Thierstein, a paleontologist who specializes in microplankton, is comfortable with this scenario,[i] and stated "Darkness is a very good mechanism that could account for the pattern we have (15)." In fact, the micropaleontologists, most of whom like the asteroid impact theory, are much happier with the 3–6 months of darkness than they were with the original, longer interval.

Historically, the second one (21) is due to Cesare Emiliani, who is a paleontologist, E. B. Kraus, who is an atmospheric modeler, and Gene Shoemaker, to whom I have already referred. They believe that a greenhouse effect caused by the asteroid hitting the ocean and sending up an enormous amount of water vapor would heat the atmosphere and the environment up by as much as 10°C. That does not seem like very much to me, but they assure us that it would kill a great number of the land animals, particularly near the equator, where the fauna are living close to the maximum tolerable temperature.

Then, Toon and his colleagues[e] came up with a third killing mechanism. They reported that their computer simulations show that it first would be very cold for several months. The temperature would go down to about −18°C for 6–9 months. That would wipe out most of the animals that did not know how to hibernate.

Recently, a fourth killing scenario has come to light. This one,

[f] Jones, E. M. & Kodis, J. W., Los Alamos Report LA-UR-81-3495 and Conference on Large Body Impacts and Terrestrial Evolution: Geological, Climatological, and Biological Implications, Snowbird, Utah, Oct. 19–22, 1981.

[g] O'Keefe, J. D. & Ahrens, T. J., Conference on Large Body Impacts and Terrestrial Evolution: Geological, Climatological, and Biological Implications, Snowbird, Utah, Oct. 19–22, 1981.

[h] Milne, D. H., McKay, C., Conference on Large Body Impacts and Terrestrial Evolution: Geological, Climatological, and Biological Implications, Snowbird, Utah, Oct. 19–22, 1981.

[i] Thierstein, H. R., Conference on Large Body Impacts and Terrestrial Evolution: Geological, Climatological, and Biological Implications, Snowbird, Utah, Oct. 19–22, 1981.

from Professors Lewis and co-workers,[j,k] is that the enormous amount of radiant energy in the rising fireball would go through the atmosphere and fix nitrogen to make enormous amounts of nitrogen oxides. It would make acid rain, the rain would fall into the ocean, and the calcium carbonate-based foraminifers would dissolve in the acidified water. I think the chances are that all four of these scenarios had some part in the various extinctions, and it is going to be a life's work for some people to untangle all these things.

Let me now tell you just how much energy was released when the asteroid hit. A trivial calculation shows that it released an energy of about 100 million megatons. A 1-megaton bomb is a big bomb. This is 10^8 of those. The worst nuclear scenario I have ever heard considered is when all 50,000 bombs that we and the Russians own go off pretty much at the same time. The energy released in that case would be less than what we got in the asteroid impact by a factor of about 10^{-4}. So, this asteroid impact was the greatest catastrophe in the history of the earth, of which we have any record, and in fact we have a very good record of it.

I will now comment in some detail on the contrary views of the C-T extinction that have been expressed in print, and in many lectures, by my good friend William Clemens, professor of paleontology at Berkeley, who is certainly the most vocal critic of our work. We have a nice arrangement with Bill. For the past 12 weeks, seven or eight of us have spent every Tuesday morning sitting around a table in his conference room—four members of our group, Bill Clemens, one or two of his students, and Dale Russell, who is on sabbatical leave at Berkeley. Dale is a vertebrate paleontologist whose specialty is the study of dinosaurs. He agrees with us that the dinosaurs were wiped out suddenly as a direct result of the asteroid impact, and he further believes that, had the asteroid not hit the earth 65 million years ago, the mammals could not have evolved the way they did. But he believes that intelligent "humanoids" would have evolved in the class of reptiles. He and one of his colleagues are responsible for a set of pictures that purport to show what these two-legged, upright-walking, intelligent creatures might have looked like. And they might have formed their own National Academy and be discussing what would happen to them when one of the asteroids they see in their telescopes hit the earth.

Our little group has sat around the table for 3 hours each time and debated our differences and tried to get to understand how the other person was thinking. I do not think this has happened very often across disciplinary lines in science. It is a really good way to settle arguments, even though we still have some pretty serious disagreements. But fortunately, we have remained friends throughout our long period of disagreement.

We are indebted to Bill for getting us samples from Montana that show an iridium enhancement in rocks that are close to dinosaur fossils. Carl Orth's group in New Mexico found the first iridium at a continental site, but there were no dinosaurs around there. Bill Clemens collected samples for us in his favorite hunting grounds at Hell Creek in Montana, one of the greatest sites for finding dinosaurs. Frank Asaro and Helen Michel found a large enhancement of iridium, and that is the first experimental evidence that ties the asteroid impact to the extinction of the dinosaurs (Fig. 10). I had given a number of talks to physics department colloquia entitled "Asteroids and Dinosaurs," before we had any direct connection between the asteroid impact and the dinosaur extinction. You might consider that to be one

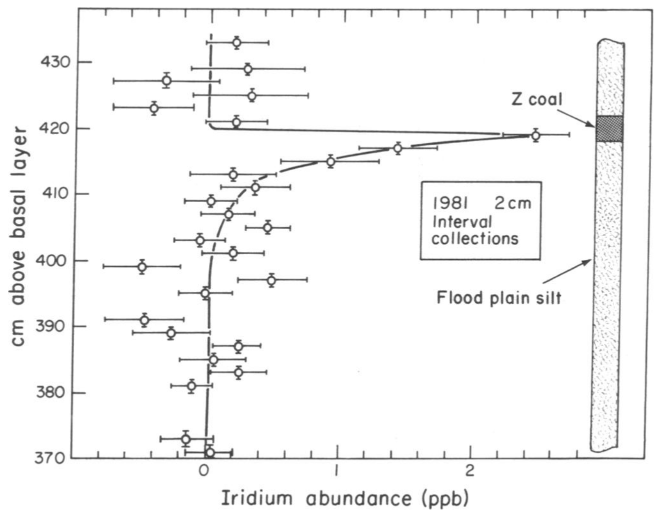

FIG. 10. Iridium abundance at Iridium Hill section, Hell Creek, Montana, showing the Z coal.

of our major predictions—that the asteroid impact led directly to the dinosaur extinction. I think the connection is now extraordinarily well established, but will try to explain why Bill Clemens does not agree with that conclusion, and then will explain why I think his arguments do not stand up under close scrutiny.

Fig. 11 is Bill Clemens' slide that he has used in a great many talks, and I am indebted to him for letting me use it. He uses this to show that we are wrong in associating the dinosaur extinction with the asteroid impact. The iridium was found in what is called the (basal) "lower Z coal." This coal layer is seen over wide areas in Montana; on this diagram it is shown at the 4.2-meter level. Bill says that the 0.8-meter level is the highest at which he has seen dinosaur bone and he frequently refers to this as the stratigraphic level at which "the dinosaurs became extinct." [In a recent article (22) with Archibald he says that his student, Lowell Dingus, has seen some dinosaur fossils above the Z coal layer.] Because this is our main point of contention, I will spend some time explaining our differing views concerning the significance of that "highest bone."

Two other features of this very important slide are also worthy

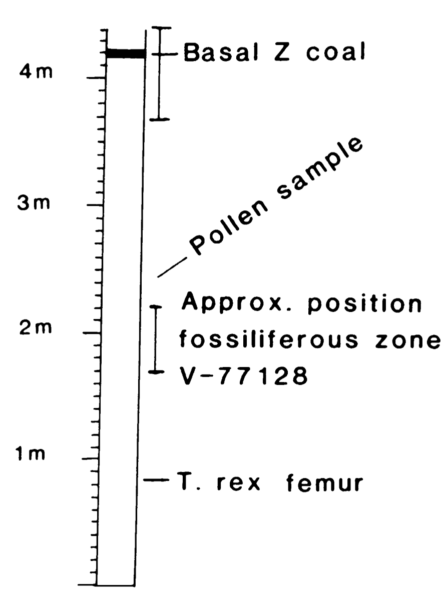

FIG. 11. Stratigraphic section in Hell Creek, Montana.

[j] Lewis, J. S. & Watkins, G. H., "Chemical Consequences of Major Impact Events on Earth," preprint, May 18, 1981.

[k] Hartman, H. & Lewis, J. S., "Cretaceous Extinctions: Effects of Acidification of the Surface Layer of the Oceans," preprint, July 23, 1981.

of notice, the pollen sample and the fossiliferous zone which Bill usually refers to as a site which produces Paleocene mammal fossils. I will not speak further of the pollen which does not seem to bother Bill nearly as much as the Paleocene (or early Tertiary) mammal fossils do. (These appear at the 2-meter level.) Bill's main interest is in early mammals, rather than in dinosaurs, and he thinks that such mammals have no business being below the iridium layer, if that layer really defines the C-T boundary, which he doubts is the case on the continents—although he is apparently able to accept it in the oceanic sequences. During many of our private discussions, I took the position that evolution does not move fast enough to make the appearance of Paleocene mammal fossils below the iridium layer troublesome to our theory—that the dinosaurs were reproducing at a fairly constant rate, over millions of years, and were suddenly wiped out as a result of the asteroid impact. Paleontologists have never before had such a worldwide sharply defined "horizon" as is furnished by the iridium layer, except for those special cases that happen to coincide with a paleomagnetic reversal. So my argument (in a field in which I have no credentials) was that there was no previous evidence that the Paleocene mammals did not originate 20,000 years before the Paleocene period started—at the C-T boundary.

Now let us look at the time scale that applies to Fig. 11. In all our long discussions of this figure, the sedimentation rate was assumed by everyone to be about 1 meter in 9,000 years. (That average rate comes from the known time between the magnetic reversals that are observed in the Montana sections.) So Bill Clemens is impressed by the fact that the dinosaurs became extinct "long before" the iridium layer was deposited—a difference in height, on this figure, of 3.4 meters or 30,000 years. But the usual description, by paleontologists, of an extinction that took place in the course of 1 million years, is that it "happened rapidly." To someone like me, who is new to the field, it is confusing to hear from the same people, that 1 million years is a "short time," and 30,000 years is a "long time."

In addition to this strange confusion in time scales, I have heard Bill Clemens, and other paleontologists as well, say that the dinosaurs did not disappear suddenly but were declining in population and diversity, all over the world, for a million years or so, before they finally became extinct, near the C-T boundary. First of all, I should say that I have looked closely at a lot of data that bear on this alleged "decline," and I agree with Dale Russell that they do not stand up under careful examination. In the last of our 12 seminars to which I have referred, Bill Clemens presented a table of dinosaur fossils that showed that neither the population nor the diversity of dinosaurs had changed appreciably in the 20 meters below the Z coal layer. (At least, that is what the data said to me and to Dale Russell, and Bill Clemens did not attempt to use them to prove otherwise.) Bill's table appropriately showed only "articulated dinosaur fossils," meaning samples of at least two bones in nearly their normal relationship, or a single bone so large that one could be sure that it had not been shifted from its original site, by running water, etc. There were 17 fossils in the sample, extending downward from the Z coal to a distance of 18.3 meters. This corresponds very nearly to a time interval of 165,000 years. The average spacing was 1.1 meters per dinosaur, and anyone who is used to looking at truly random samples of objects would say, "There is no indication that the population from which this sample was taken was declining as it approached the Z coal layer." It looks extraordinarily uniform to me, even though there is a non-statistically significant *increase* in the number of fossils in the top 30 feet compared to the bottom 30 feet—10 to 7.

I will return to a more detailed discussion of these matters because they are the ones that cause me to come to conclusions quite different from those drawn by Bill Clemens. And I will show that, if Bill Clemens is correct in his "decline hypothesis," it destroys his argument that the eventual extinction of the dinosaurs came before the asteroid impact occurred.

I will now address what I consider to be a serious error in the way Bill Clemens analyzes his data. The field of data analysis is one in which I have had a lot of experience—in contrast to my inexperience in paleontology—so I will offer this criticism without apology. The "*T. rex* femur" that appears at the 0.8-meter level is considered by Bill Clemens to mark the time at which the dinosaurs went extinct. I have "called him" on this point so many times in our little seminars, that I am sure I am not being unfair to him when I say that he really believes that the dinosaurs went extinct 3.4 meters before the iridium layer was deposited, or close to 30,000 years earlier [and in his recent article (22), he made this point several times]. The various members of our group have come up with at least four different ways of demonstrating that the proper point to mark the disappearance of the dinosaurs—based on Bill's "highest observed fossil"—is measured in meters (rather than decimeters or decameters) above the "highest bone." (The fact that we proposed several new ways of demonstrating that assertion, on several succeeding Tuesday meetings, is the best proof I can offer that we had not convinced Bill by our earlier arguments. Each week the proposer of the new explanation would say, ahead of time, "I'll bet this one will convince Bill Clemens.")

The easiest way to show what the problem is, and why it is not important in the marine deposits, is to state it in its simplest form. We will assume that some fossils—for example, foraminifers or dinosaurs—are seen in an exposed cliff face, with an average vertical spacing equal to L meters. (If we look at a section of the face of the cliff only half as wide, the appropriate value of L will be twice as large.) The hypothesis we are testing is that the creatures whose fossils we are observing were reproducing at a substantially constant rate until they were suddenly eliminated as the result of some catastrophic event. We have used four separate methods to show that the most probable location of the true "extinction layer" is exactly L meters above the highest observed fossil in that section. The four methods are (*i*) analytical, (*ii*) use of computer-generated plots of randomly occurring "fossils" but with a known cut-off level not indicated on the plot, (*iii*) the Monte Carlo random number method, and (*iv*) an analogy based on locating the United States–Canada border by observing (*a*) the home of the most northerly U.S. citizen and (*b*) the home of the northernmost U.S. Congressman. You may enjoy developing this analogy—it works quite well.

The second method corresponds most closely to what one finds in the field—a collection of fossils extending left and right, to the edges of the page, but with no fossils in the upper part of the diagram, above some unmarked line that was at a different height on each page. We passed out dozens of these plots, which were generated on the computer by Walter's student Kevin Stewart at one of our seminars, and asked each participant to guess where the computer had located the "sharp cut-off." In some of these plots, the computer was instructed to weight the surviving fossil population differently in various lithological layers. We did this because Dale Russell's experience as a dinosaur fossil hunter has taught him that there is a larger chance of finding fossils in some formations, such as sandstones, than there is in siltstones or mudstones. So the computer-generated fossil plots corresponded as closely as we could make them to a real field situation. When the "key" was revealed, it was clear that no one had done a good job in locating the iridium layer, but those of us who believed the analytical theorem—that one should pick a point that is above the highest fossil by an amount equal to the average spacing, L—did better than the paleontologists, who have been taught for most of their professional lives to take most seriously the levels corresponding to the "first appearance" and the "last appearance" of any species. The difference between those two levels is called the "range" of the species, and it is accepted that all species do (or will) become extinct at some level.

I believe the reason for the wider acceptance, among paleontologists, of the idea that the asteroid impact led to the extinction of the foraminifers is that the average spacing L between their fossils in limestone that crosses the C-T boundary can be a small fraction of a millimeter. The boundary clay has a lower boundary that is definable to only somewhat less than 1 mm, so the coincidence between the iridium layer and the "highest foraminifer" is "perfect," and so "everybody" believes in the causal relationship between the asteroid and the extinction.

In the case of the dinosaur fossils, the average spacing is unknown, but in Bill Clemens' table it is slightly more than 1 meter. If we took it to be exactly 1 meter, and independent of lithological factors, the analytical expression for the chance that the iridium layer appeared at least 3.4 meters above the highest fossil is $p = e^{-3.4} = 0.033$. On the other hand, if the average spacing were 2 meters or 0.5 meter, the probabilities that the iridium layer is where it is are $e^{-1.7}$ or $e^{-6.8}$, equal to 0.183 and 0.0011, respectively. We will soon see that all of these probabilities are larger than the exceedingly small probability that Bill Clemens is forced to accept, when he says that the dinosaurs became extinct, for some unspecified reason unconnected with the asteroid impact he has accepted, about 30,000 years before that impact took place.

It is easy to calculate the probability that the dinosaurs, which had dominated the earth for nearly all of the Mesozoic era—from about 200 million years ago—would become extinct just 30,000 years before any arbitrarily chosen time marker—for example, the asteroid impact; that probability is the ratio of those two times, or 1.5×10^{-4}. As I just said, that is smaller than any of the probabilities we can construct from the "gap" data, and it suffers further from its completely *ad hoc* nature—there is nothing in the history of the earth that can be connected with this extraordinarily coincidental "extinction." On the other hand, our preferred scenario is tied solidly to a well-documented catastrophe that is the most severe event of which we have any record. I really cannot conceal my amazement that some paleontologists prefer to think that the dinosaurs, which had survived all sorts of severe environmental changes and flourished for 140 million years, would suddenly, and for no specified reason, disappear from the face of the earth (to say nothing of the giant reptiles in the oceans and air) in a period measured in tens of thousands of years. I think that if I had spent most of my life studying these admirable and hardy creatures, I would have more respect for their tenacity and would argue that they could survive almost any trauma except the worst one that has ever been recorded on earth—the impact of the C-T asteroid.

Because I mentioned the Monte Carlo method of demonstrating that one needs to add L to the height of the highest fossil to locate the most probable position of the iridium layer, I show (Table 2) the results of 20 computer-generated dinosaur fossil sequences. Each set was constructed by a random number generator which positioned 50 dinosaur fossils randomly in a stratigraphic height of 100 meters. L is 2.0 meters in all 20 sections. The sharp cut-off at the top is always located at 0 meters and you can see where the highest fossil is located in each section. You also see that, if you assume that the cut-off is at the highest bone, you guess wrong, on the average, but just by L meters. But if you add L meters to the highest bone in each section, then your average estimate of the position of the cut-off in the 20 cases is just right.

Table 2. Computer-generated "highest dinosaur": Monte Carlo table of the highest fossil in 20 random sequences of 50 fossils, each having a density of 50 fossils per 100 meters

Sample	Level, meters	
	Z	$Z + L$
1	−3.201	−1.201
2	−3.063	−1.063
3	−0.521	+1.479
4	−0.396	+1.604
5	−0.097	+1.903
6	−5.408	−3.408
7	−2.930	−0.930
8	−0.649	+1.351
9	−3.747	−1.747
10	−1.097	+0.903
11	−0.109	+1.891
12	−0.244	+1.756
13	−1.501	+0.499
14	−0.680	+1.320
15	−1.896	+0.104
16	−4.330	−2.330
17	−2.903	−0.903
18	−3.681	−1.681
19	−4.112	−2.112
20	−1.665	+0.335
Mean	−2.112	−0.112

L = 2 meters; zero elevation corresponds to true extinction.

I said above that I would point out the trouble Bill Clemens would be in if the "gradual decline" of the dinosaurs turned out to be real—which I continue to doubt. The trouble comes from the fact that the value of L that one must add to the height of the "last observed dinosaur," to locate the "most probable height" of the extinction level, is not the *average* value of L observed in some collecting site but the much larger value of L associated with the smaller (declined) population near the time that the "highest fossil" was laid down. Because the probabilities of observing "gaps" ($>G$) between the highest fossil and the iridium layer (assuming it caused the extinction) are equal to $e^{-G/L}$, we see that Bill does not have a statistically significant experimental gap to explain if he really believes in his "decline hypothesis." (The larger L is compared to G, the closer $e^{-G/L}$ approaches unity.)

Two questions that I frequently hear are, "Where did the asteroid hit?" and "How is the theory being accepted these days?" The answer to the first is that we do not know. No crater of the correct size (100–150 km in diameter) and age is known on the earth, with the possible exception of the Deccan Traps region on the Indian subcontinent. Fred Whipple's (23) interesting suggestion that the asteroid hit the mid-Atlantic ridge, between Greenland and Norway, and led to the formation of Iceland unfortunately is wrong because paleomagnetic evidence shows that there was no such ridge at the end of the Cretaceous period—Greenland and Norway had not yet separated. We may never see the crater, because 20% of what was the earth's crust 65 million years ago has since been subducted below the continents. So there is a 20% chance that the crater has disappeared forever, but there is also a finite chance that it still exists on some part of the ocean floor that has not been mapped with sufficient resolution to show it. Many geologists have written to suggest possible impact sites, and each one has looked pretty exciting at first glance. But all of them have had to be discarded, for one reason or another.

I conclude by addressing the question concerning the acceptance of the theory. Almost everyone now believes that a 10-km-diameter asteroid (or comet or meteorite) hit the earth 65 million years ago and wiped out most of the life in the sea. When we first said that the extinctions were caused by an asteroid, we had no information on the detailed composition of the asteroid and, in fact, no one had ever had a chance to analyze an asteroid. But if we had been a little more adventurous, we would have made an eighth prediction—that we would eventually prove that the asteroid had a composition essentially identical to that of the most common solar system debris we know, the carbonaceous chondritic meteorite. We always assumed that it *did* have that composition, but it did not occur to us that there would be a way to prove it. This was first done by Ganapathy (24) who found that the ratios of platinum group elements in the Danish boundary layer corresponded roughly to values in carbonaceous chondritic meteorites. We then measured the Pt/Ir and Au/Ir ratios in Danish and Spanish boundary clays with high precision, and they agreed almost perfectly with type I carbonaceous chondrites. They did not resemble at all crustal or mantle material from anywhere on the earth. The measured ratios also agreed with ratios in two other kinds of chondritic meteorites but not in iron meteorites.

Since Archibald, Clemens, and Hickey all assert that the extinctions were not synchronous—the land plant extinctions, and the land animal (e.g., dinosaur) extinctions—let me end the technical part of my talk with arguments that I find overwhelmingly convincing as to their precise synchroneity.

The Orth graphs, plus the rarity of iridium layers, show that the oceanic and land plant extinctions were synchronous to better than 5 cm, or appreciably less than 1,000 years. No data have been presented by any of the three authors I just mentioned that attempt to challenge that conclusion. But they do challenge the simultaneity of the land floral and faunal extinctions, based on the 3-meter "gap" between the "highest dinosaur" and the pollen changes. I cannot think of anything to add to the set of four arguments I have given to show that the "gap" has no experimental significance.

In trying to decide whether we or our critics are correct in our deductions, I suggest comparison of two models. The first is ours, which says the asteroid was responsible for the iridium layer found by Orth in New Mexico and for the ones that we and others have found over the earth and in oceanic sections, and that anyone, using a hand lens, can see was synchronous with the oceanic extinctions. Our model says these two were synchronous to within a few years, so one doesn't need to calculate a probability—the theory simply predicts what we see—simultaneously within the resolution of the observations.

But, if we take the Archibald, Clemens, and Hickey position that the asteroid had nothing to do with the land floral extinctions, then the observed time coincidence of the two events is purely a matter of luck, which can be expressed as a probability. The numerator is the very generous 1,000 years I have assigned, and the denominator should be the average time between "spikes" such as the dip in the pollen density. Because I have not heard of other spikes of this nature, I will use for this average time what I think of as the "characteristic species time" or 1 million years. So the probability that the observed simultaneity is due to pure luck unrelated to an asteroid impact is about 10^{-3}. In physics, we do not treat seriously theories with such low *a priori* probabilities. (But if you look closely at the writings of Archibald, Clemens, and Hickey, you find that they do not really have a viable competing theory—one that explains some reasonable fraction of the observational data. I think it is correct to say that their theory is that our theory is wrong!)

The simultaneity of the C-T extinctions in the oceans and on the land can also be demonstrated by a completely different argument, that depends only on foraminifers, dinosaurs, and iridium. Let us look at what our group concluded after finding iridium layers in Italy, Denmark, and New Zealand and deducing that these layers resulted from an asteroid impact. With the exception of Walter, none of the members of our group knew anything about the extinctions of the land animals. But we were forced to say that there would be an iridium layer seen in continental sites, precisely at the C-T boundary, as defined by the paleontologists. And this prediction relates to dinosaur extinctions on all continents, so we should see iridium layers just above the highest dinosaurs in Western North America, Argentina, France, Spain, and Mongolia. (We have not yet looked at the foreign locations, but I remind you that we did not pick the site to examine; that was a random selection.) Three of us knew nothing about Montana dinosaurs or the lower Z coal layer. But if we had known what Dave and Bill now say about that layer, we would have predicted (number nine) that the iridium enhancement would be found in the lower Z coal layer. (Here is what they say about that layer: "This coal came to represent the Cretaceous-Tertiary boundary in Montana, *because* remains *of dinosaurs* had not been found above it" [emphasis added].) Note that this sentence does not mention pollen or mammals. So, with no knowledge whatsoever about dinosaurs, we predicted that there should be an iridium enhancement at the (unknown to us) C-T boundary, which Dave and Bill could have told us was in the Z coal layer, and when we looked there, there it was! (Fig. 10). Actually, we first looked in the region of Bill Clemens' favored place, 3 meters below the Z coal layer, and found no "signal." We then worked our way, slowly upward, 10 cm at a time, until we found the enhancement.

If you believe the asteroid theory, as we do, then there is nothing surprising about this—that is just where the iridium *had* to be. But if you take the point of view of our paleontologist critics—that the asteroid impact had nothing to do with the dinosaur extinction—then you can calculate the probability that we were simply lucky in that prediction. In this case, the numerator is the thickness of the Z coal layer, or about 4 cm, which we can again approximate as less than 1,000 years. The denominator is again undetermined but certainly in the range of millions of years. So my estimate of the probability that we were "lucky," even though our theory was quite invalid, is about 10^{-3}. And in case you think I'm simply repeating an old argument, I'll remind you that the numerator, 5 cm, in the first probability came from a comparison of the two halves of the Orth graph (Fig. 8), whereas the nearly same value for the numerator in the second probability calculation came from the measured thickness of the lower Z coal layer, and our discovery of the iridium enhancement at its base (Fig. 10). So the two sets of measurements are quite independent, and the rules of statistics say that we should multiply the two probabilities, to get an obviously absurd chance of the two sets of observations being due to luck; $p = 10^{-6}$. It is also interesting that we did not have to calculate the probability that the iridium layer was in coincidence with the extinction of the foraminifers; that probability has, for its numerator, a distance closer to 1 mm in several places that are widely distributed over the globe.

I hope these exercises will show you why, as an experimentalist, I am convinced that the three extinctions in question were simultaneous—the oceanic extinction, the land floral extinction, and the land faunal extinction.

And before I leave the matter of probabilities, let me remind you that above I calculated the probability that the dinosaurs, which appeared on earth about 200 million years ago, would suddenly become extinct within about 3 meters, or about 30,000 years of some arbitrarily chosen time marker. (We did the calculation on the assumption that the time marker was the time of the asteroid impact. But if the asteroid had nothing to do with the dinosaur extinction, as our critics believe, then there is no reason to use the asteroid impact as the "arbitrary time marker"—it could in fact be any arbitrarily assigned time.) And, as I showed earlier, the probability that this happened "by luck" was about 1.5×10^{-4}. When I wrote the first draft of this paper, I treated this probability as independent of the other two—its numerator is 30,000 years rather than 1,000 years, and its denominator is 200 million years rather than 1 million years. So I multiplied the three probabilities together, to yield an overall probability that all three observations happened by luck—assuming that the asteroid impact had no relationship to either of the land extinctions. But that is probably "overkill," for two reasons: (*i*) I should not use Clemen's erroneous conclusion that the 30,000 year "gap" is significant, to cast further doubt on his gradualistic theory; and (*ii*) the 4-cm limit of error between the Z coal layer and the iridium layer, and the 3-meter interval between the Z coal layer and the "highest dinosaur" are not completely independent; both involve the location of the Z coal layer. But I think that a factor of 10^6 "working against" the Archibald–Clemens theory is impressive enough.

I conclude this talk with a brief discussion of how a theory is "proved." We all know that theories cannot be proved; they can only be disproved, as Newton's theory of gravitation was disproved by the observations that led to the acceptance of Einstein's theory of gravitation. So let me change my words and ask how theories come to be accepted. Here the classic example is the Copernican heliocentric theory that displaced the Ptolemaic geocentric theory. It became accepted not because Galileo saw the phases of Venus, as most of us believe, but simply because the heliocentric theory easily passed a long series of tests to which it was subjected, whereas to pass those same tests, the geocentric theories had to become more and more contrived. (That is why I've spent so much time telling you of the many tests and predictions that the asteroid theory has "passed.")

And finally, if you feel that I have been too hard on my paleontologist friends and have given the impression that physicists always wear white hats, let me remind you of a time when our greatest physicist, Lord Kelvin, wore a black hat and seriously impeded progress in the earth sciences. We all know that he declared—with no "ifs," "ands," or "buts"—that the geological time scale was all wrong; he was absolutely sure that the sun could not have been shining for more than about 30 million years, using the energy of gravitational collapse.

But most of us do not know that the first man to suggest the answer to this serious problem was Thomas C. Chamberlin (25), a geologist at my alma mater, the University of Chicago. He said that, since the sun had obviously been shining for a much longer time, there must be an as yet undiscovered source of energy in the atoms that make up the sun! And on this occasion, when the tables were turned, the physicists, who had been dragging their heels for a long time, eventually discovered "atomic energy" for themselves (and even convinced everyone that it was "their baby"), and then went on to explain in detail just where the sun's energy comes from.

Every science has much to learn from its sister sciences, and I look forward to the continuation of our cross-disciplinary Tuesday morning sessions.

APPENDIX

The recent article by Archibald and Clemens (22), "Late Cretaceous Extinctions," is contemporaneous with my talk. Bill Clemens and I had earlier discussed all the points I made in my talk and almost all those made in his new article, so it might be useful for the reader—who might be trying to decide which point of view to adopt—if I comment on a few places where we obviously disagree.

It would make this printed version much too long if I were to address all the points in the article with which I disagree. I will concentrate on the alternative theories that Archibald and Clemens discussed in some detail. They mention explicitly only two such theories, and both can be quickly dismissed. The first—the supernova theory—is not consistent with Orth's limit on the plutonium-244 near a continental boundary; he found less than 10^{-4} of the amount called for by the theory. Furthermore, that theory has already been abandoned by its three chief proponents, Mal Ruderman in physics, Dale Russell in paleontology, and Wallace Tucker in astrophysics.

The second theory is Steve Gartner's "Arctic Spillover Model." This was an acceptable theory when it was proposed, several years ago, but it is no longer acceptable because it offers no reasonable explanation for the iridium layer in the ocean sediments and no possible explanation for the iridium layers found at continental sites. I'm really quite puzzled to see that in 1982, two knowledgeable paleontologists would show such a lack of appreciation for the scientific method as to offer as their only two alternative theories to that of the asteroid, a couple of outmoded theories. One can't use the excuse that when they were proposed, neither could be falsified. The facts of the matter are that as of today, both of them are as dead as the phlogiston theory of chemistry, and I have not heard a serious suggestion in place of the asteroid theory. (But of course that situation has no bearing on whether or not the asteroid theory is correct.)

On the last page of their article, they speak of several vaguely defined noncatastrophic theories, but then they apparently (and I believe correctly) dismiss such theories by saying, "Looking back, it seems unlikely that gradual processes could have caused the extinctions that occurred at the end of the Cretaceous." This evaluation seems to be in good accord with a statement that appears near the beginning of the article, "From today's perspective, the extinction of the dinosaurs some 65 million years ago appears to have occurred almost literally overnight."

After reading this article at least six or eight times, I came away with the feeling that they are emphasizing four main points. First, in field paleontology, it is terribly difficult to make meaningful measurements that tell very much about what happened 65 million years ago. I agree completely with this point, and my admiration for the observations that my newfound friends have made is enormous. But as you can tell, that admiration does not extend to some of the conclusions they draw from those observations.

Their second point is that the dinosaurs disappeared about 3 meters (approximately 30,000 years) below the C-T boundary. They state this conclusion, explicitly, on four of the eight pages of their article, and it is the point that comes through loudest and clearest. (And you can see that even after trying in four different ways to convince Bill that such a gap has no significance, we really "struck out.")

Their third point is expressed in this way in the article's final sentence, "At present, the admittedly limited, but growing store of data indicates that the biotic changes that occurred before, at, and following the C-T transition were cumulative and gradual and not the result of a single catastrophic event." Again, this point is made on at least four of the eight pages.

Their fourth point is not stated explicitly, but it comes through quite clearly—they do not take seriously the idea that the asteroid impact (if it in fact really occurred, and they never say that they believe that) had anything to do with the extinction of the dinosaurs. There is not a single indication that they take seriously any of the many properties of the iridium layer that I discussed above and which lead me to conclude that the asteroid *did* trigger the dinosaur extinction. (You can be sure that before I make such a sweeping statement, I have carefully read and reread what Dave and Bill said about the iridium layer, each of the 13 times they mentioned the word "iridium.")

It seems to me that their article is in no way responsive to the wealth of data that I have presented in this talk, and with which Dave and Bill are intimately familiar. If George Mallory of Everest fame were still alive, I think he would say, "Gentlemen, you should take the iridium layer seriously—it is there!"

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