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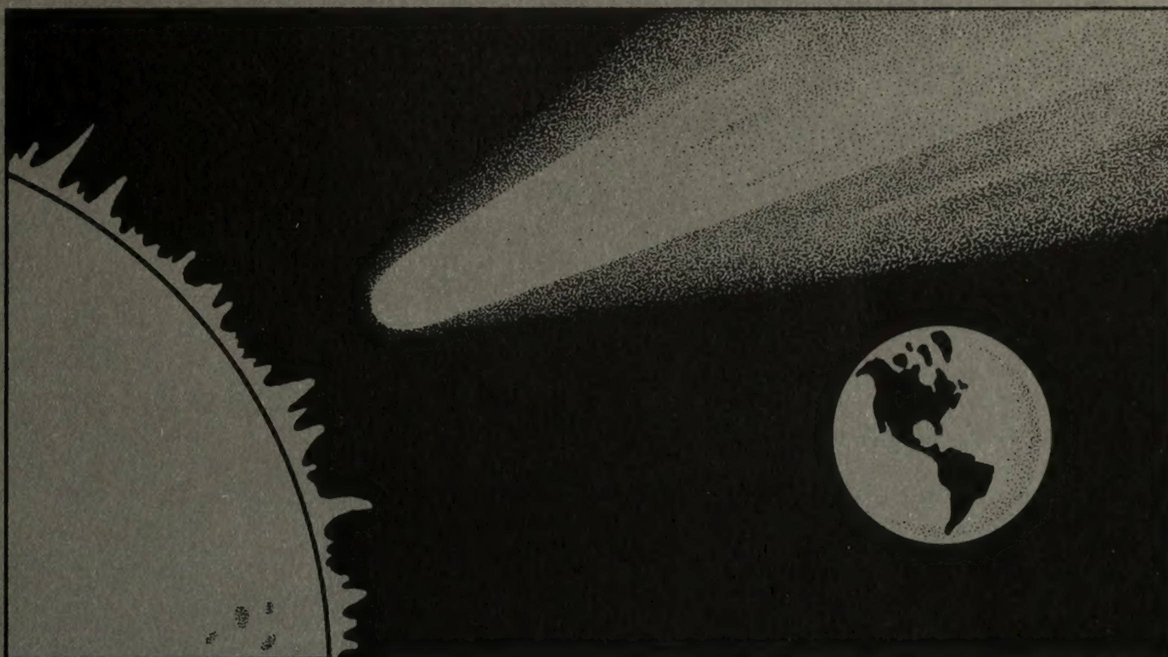

K-TEC II

Cretaceous-Tertiary Extinctions and Possible Terrestrial and Extraterrestrial Causes

No. 39 1982

Syllogeus

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K-TEC II

CRETACEOUS-TERTIARY EXTINCTIONS AND POSSIBLE TERRESTRIAL AND EXTRATERRESTRIAL CAUSES

by the K-TEC* group

Proceedings of the workshop held in Ottawa, Canada

19-20 May 1981

Edited by D.A. Russell and G. Rice

Sponsored by the Paleobiology Division, National Museum of Natural Sciences (Canada) and by the Herzberg Institute of Astrophysics, National Research Council of Canada.

*L. Alvarez, W. Alvarez, F. Asaro, *P. Béland*, K. Brasch, S.V.M. Clube, D. Dilcher, *P. Feldman*, R.A.F. Grieve, I. Halliday, K.J. Hsü, *D.M. Jarzen*, J.F. Jaworski, F.T. Kyte, A.E. Litherland, D.M. McLean, H. Michel, W.M. Napier, *K.A. Pirozynski*, J.C. Rucklidge, *D.A. Russell*, J. Smit, H.R. Thierstein, *W. Tucker* (italicized names indicate participants in the first K-TEC workshop, 16-17 November 1976).

Syllogeus No. 39

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Participants in the K-TEC II workshop:

Standing, from left to right; V. Clube, I. Halliday, P. Feldman, W. Napier, D. McLean, W. Tucker, K. Pirozynski, A. Litherland, G. Rice, J. Jaworski, D. Jarzen, J. Rucklidge, L. Alvarez, D. Russell, W. Alvarez, J. Smit, D. Dilcher, *Stenonychosaurus inequalis*. Seated, from left to right: F. Asaro, K. Brasch, K. Hsü, F. Kyte, H. Michel, R. Grieve, P. Béland, H. Thierstein.

Contents

PREFACE

It is a pleasure to present herewith the proceedings of the second workshop on *Cretaceous-Tertiary extinctions and possible terrestrial and extraterrestrial causes* sponsored by the Paleobiology Division, National Museum of Natural Sciences (Canada) and by the Herzberg Institute of Astrophysics, National Research Council of Canada. The first workshop, held four and one-half years previously, received the acronym "K-TEC" after Cretaceous-Tertiary Environmental Change (K being the geological symbol for Cretaceous) from the pen of Pierre Béland. There were only eleven of us then, and all but two were supported by Canadian institutions. The original workshop was both enjoyable and stimulating, — and we were grateful to find that some of the concepts we considered figured in the subsequent debate on the extinction of the dinosaurs. One of the outstanding needs perceived at the time was for chemical studies of the boundary horizon. This need is now being filled so vigourously that it appears that paleontology must yield pride of place to trace element studies in resolving the nature of the extinctions.

Included within the volume resulting from the original workshop were 16 pages summarizing discussions between participants. Reactions to this small section were so favourable that its impact probably exceeded that of the remainder of the volume. For this reason the proceedings of the second workshop consist almost entirely of a condensation of the two-day dialogue between workers in different disciplines approaching a common problem from different theoretical points of view. It represents an alternative to the general practice of publishing formal presentations with minimal or no reactions from other participants in a colloquium. A few contributed abstracts follow the discussions. The prestige of our host institutions is not responsible for the academic talent which made itself available for these symposia. Scholars have obviously attracted other, comparably distinguished scholars. However, all of us were keenly aware of the absence of colleagues who have made important contributions to understanding the problem of mass extinctions. We acknowledge our gratitude to them, and wish them continued success in their researches. I must also personally underscore my gratitude to Dr. Paul Feldman of the H.I.A. for his wise counsel and generous consideration during the months when our workshop was taking form.

D.A. Russell

PREFACE

Je suis heureux de présenter le compte rendu du deuxième atelier consacré aux *Extinctions du crétacé-tertiaire et aux phénomènes divers, terrestres et extraterrestres, susceptibles de les avoir provoquées.* L'atelier était parrainé par la Division de paléobiologie du Musée national des sciences naturelles (Canada) et par l'Institut Herzberg d'astrophysique du Conseil national de recherches du Canada. Le premier atelier, tenu il y a quatre ans et demi, avait été surnommé "K-TEC" pour "Cretaceous-Tertiary Environmental Change" (K est le symbole du Crétacé en géologie) par M. Pierre Béland. Nous n'étions qu'onze à y participer, tous envoyés par des établissements canadiens à deux exceptions près. Nous avions trouvé l'expérience aussi agréable que stimulante — et nous avons constaté avec plaisir que certains des concepts étudiés avaient été repris à l'occasion du débat subséquent sur la disparition des dinosaures. Nous avions reconnus, entre autres besoins impérieux, la nécessité d'étudier la composition chimique de l'horizon limite des extinctions. Ce besoin a été comblé avec tant d'ardeur qu'il semble maintenant que la paléobiologie doive s'incliner devant les études d'éléments-traces pour ce qui est d'expliquer la nature des disparitions.

Les travaux du premier atelier furent consignés dans un ouvrage qui comprenait seize pages de résumé des discussions. Si l'on en juge par l'accueil qui lui fut réservé, cette brève section a sans doute en plus d'impact que le reste de l'ouvrage. Forts de cette réussite, nous publions surtout, dans le compte rendu du deuxième atelier, des résumés des entretiens qu'ont eu pendant deux jours les spécialistes de diverses disciplines, qui abordaient un problème commun sous divers angles théoriques. Nous avons préféré cette solution à l'usage courant qui consiste à publier les communications sans mentionner les réactions des autres participants. Le compte rendu des débats est suivi de quelques résumés soumis par les auteurs. La compétence des chercheurs participants ne doit rien au prestige des établissements grâce auxquels la rencontre a pu avoir lieu. De toute évidence, les chercheurs distingués se sont attirés les uns les autres. Nous étions toutefois conscients de l'absence de collègues qui ont grandement contribué à éclaircir le problème des extinctions massives. Nous tenons à leur exprimer notre respect et à leur souhaiter de fructueux travaux. Pour ma part, je désire remercier en particulier M. Paul Feldman, de l'Institut Herzberg, qui m'a prodigué de précieux conseils et accordé généreusement son attention tout au long des mois de préparation de l'atelier.

D.A. Russell

Asaro: The basic framework upon which the physical science studies of the Cretaceous-Tertiary (K-T) boundary are based has been provided by paleontological and geological evidence which defines the location of the extinctions in the stratigraphic record. The iridium anomaly was initially found associated with the Cretaceous-Tertiary boundary in Italy, and was predicted to arise from sources which were extraterrestrial (Alvarez *et al.* 1979A) but within the Solar System (Alvarez *et al.* 1979B). The anomaly was also predicted to be world-wide in occurrence and hypothesized to be the result of a 10 kilometer type 1 carbonaceous chondrite asteroid impacting the Earth. The impact caused an explosion equivalent to ~ 10^8 megatons of TNT and the resulting dust cloud enveloped the Earth, spread the iridium, shut off the sunlight, stopped photosynthesis and was responsible for the K-T extinctions (Alvarez *et al.* 1980).

The iridium anomaly associated with the K-T boundary has, up to the present time, been found in 17 out of 18 localities in which it was carefully sought and the K-T boundary was thought to be intact. This includes a site in the Pacific which Dr. Kyte mentioned to me this morning. This work has been done by six different laboratories (Smit and Hertogen (1980), Kyte *et al.* (1980), Ganapathy *et al.* (1981), Hsü *et al.* (1981), Orth *et al.* (1981), Alvarez *et al.* (1980, 1982), and Michel *et al.* (1981)). The work by the Berkeley group is presently directed to determine if the iridium is truly distributed world-wide, if the iridium anomaly is of an extraterrestrial origin, if this extra-terrestrial iridium has an extraordinary origin (i.e. it is not due to the normal ablation of meteorites), and if iridium can be found associated with other extinctions in the past.

With respect to the extraterrestrial nature of the iridium anomaly, Ganapathy (1980) measured nine elements, including several noble elements, and found a pattern which he interpreted as that of type 1 carbonaceous chondrites. Later Kyte, Zhou and Wasson (1980) and we in Berkeley (Asaro *et al.* 1980) also made measurements on the same section (Stevns Klint, Denmark). All three investigations interpreted the data as evidence of the impact of extraterrestrial material but drew slightly different conclusions on the exact form of the object. A plot (Fig. 1) may demonstrate the significance of abundance ratios of noble elements, especially Au/Ir and Pt/Ir for source attributions. Although platinum and iridium are both noble elements, their ratio can be altered as a result of thermal

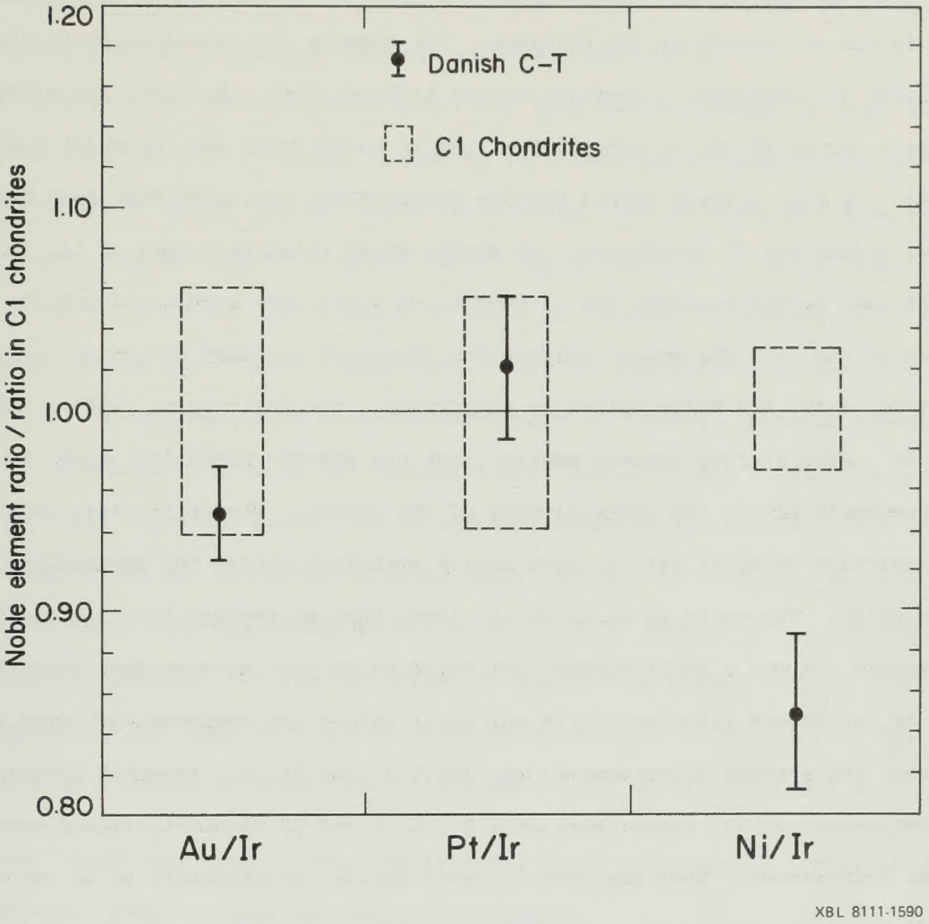

FIGURE 1: *Plot showing the significance of abundance ratios of noble elements (Au/Ir, Pt/Ir, Ni/Ir) at the Cretaceous-Tertiary boundary, Stevns Klint, Denmark, compared to their ratios in C1 chondrites.*

mantle melts, platinum will go into the lower melting fractions, and iridium remains behind in the higher melting fraction. The higher the partial melting of the mantle the lower is the enrichment of the platinum. For example, in nickel sulphide flows which occurred in considerable quantity during Archaean time, the Pt/Ir and Au/Ir ratios are perhaps a factor of two or more higher than in chondrites, and the Ni/Ir ratios are even higher. In Fig. 1 these ratios for the Stevns Klint K-T layer have been divided by the average values for C1 chondrites. An object whose relative values of Ir, Pt, Au and Ni were the same as the averages for C1 chondrites would have a value of one for each ratio shown in Fig. 1. The boxes indicate the estimated standard deviations in the mean for the Pt/Ir, Au/Ir and Ni/Ir ratios in chondrites. The Au/Ir ratio, with a standard deviation of ~ 2½%, is five percent smaller than the average chondrite value, but is easily encompassed within the uncertainties of the latter. The Pt/Ir ratio, with a standard deviation of about 3½%, is also easily contained within the uncertainty in the chondritic value. The ratio of Ni/Ir is 15% lower than an average for C1 chondrites. Nickel, however, is not a noble element and would be subject to many more chemical effects. If the Stevns Klint K-T Pt/Ir and Au/Ir ratios are compared with data for C2 or H chondrites, the average disagreements are 11% (C2) and 5% (H), somewhat greater than the C1 disagreement of 3%, comparisons with L, LL, E and C3 chondrite values exhibit more significant differences. Thus the noble element ratios, as precisely as we can measure (between 2½ and 3½%), agree very well with the expectations for C1 chondrites and somewhat less well for other chondrites. Terrestrial and iron meteoritic materials usually have considerably different values.

Next we wished to determine if this extraterrestrial material was of an extraordinary origin. Dr. Miriam Kastner from the Scripps Institution of Oceanography studied the clay mineralogy of the Danish boundary layer which averages about one centimeter in thickness (Alvarez *et al.* 1982). The clay component was found to be a nearly pure smectite with very little indication of other types of clays, such as the mixed layer clays. According to Dr. Kastner, such a clay would only occur as an alteration product of an eruptive ash or glass. She could not distinguish between ash from an asteroidal impact or a volcano. In contrast, the clay component in limestone above and below the boundary are primarily smectite/illite mixed layer clays, which are detrital and derive from the continents. The Danish boundary clay was therefore deposited *in situ* and is extraordinary in the sense that it is not composed of detrital clays. As the boundary clay is not detrital, its high iridium content is not due to a lag deposit resulting from a cessation of calcium carbonate deposition.

With respect to the repeatability, our team sent a student to China to collect samples from the Permo-Triassic boundary, with the very generous co-operation of the Government of the Peoples' Republic of China. The samples came from Nanking and supplement earlier ones from this boundary region which we received from Dr. John Utting of Petro-Canada and in which we found no iridium. A boundary clay occurs between Permian and Triassic limestones, which is composed of montmorillonite according to information we received previously (J. Utting, personal communication 1980). Chemical studies of this boundary clay indicate that it differs greatly from the material above and below. It is strongly depleted in strontium, and enriched in thorium, yttrium and zirconium (Giauque 1981), but not in iridium. We initially measured whole rock abundances with a sensitivity of about 0.2 or 0.3 PPB and detected no iridium. We are now trying to improve the sensitivity of the iridium measurement. So, this is where we stand at the moment; we are looking for the iridium anomaly in a number of other extinctions and so far we haven't found any.

L. Alvarez: Would you compare your results with those of Ganapathy (1980) and Kyte, Zhou and Wasson (1980) to show the spread in values?

Asaro: The noble element abundance measurements are difficult to do with high precision, particularly that of platinum. Ehmann and Gillum (1972) stated that measurements for gold and platinum could be carried out at a rate of 15 per working day. We didn't do 15 per day. To do one measurement of the platinum required one week. Much of the time was spent on determining how well the experiment was carried out and how accurate the value was. Ganapathy (1980) and Kyte and his co-workers (1980) have found values for gold, platinum and iridium abundances in the Stevns Klint K-T boundary layer that are off scale, below the graph.

Kyte: I am surprised that you have plotted such large ranges for the Cl abundances. The Cls do not vary at all except for sampling and analytical error (Kallemeyn and Wasson 1981).

Asaro: The standard deviation in the C1 Au/Ir ratios was 19%, with a standard deviation of the mean of 6.6%. The individual errors in the platinum measurements appeared to be about 10%. The standard deviation of the mean was taken as 6% for the Pt/Ir ratio. The abundances in the meteorites are an order of magnitude higher than in the Cretaceous-Tertiary boundary layers under study and the techniques that work with meteorites are not necessarily the best for K-T studies.

Kyte: Your plot illustrates the fact that trace element analyses always present problems in terms of interlaboratory comparison. There is no range of nickel concentration in the C1's. Also, in some Danish layers a significant fractionation exists particularly with regard to nickel and chromium, relative to C1 abundances. Nickel is sometimes depleted by as much as a factor of 3, not 20% as plotted in Fig. 1. The question remains open as to the compositon of any proposed extraterrestrial source material.

Asaro: I was very concerned about the discrepancy in the nickel abundance of Stevns Klint boundary clay as measured in different laboratories and re-checked our secondary standard (standard pottery) against standards from the United States National Bureau of Standards. The value we obtained agreed closely with the previous calibration. In addition, an x-ray flourescence calibration of our secondary standard (Giauque *et al.* 1973) agreed within 1% with the first calibration, so our secondary standard is probably accurately known. In each irradiation we use a secondary standard that contains the element that is being measured so that nickel was compared to a standard with nickel of known abundance.

Kyte: Using data from Deep Sea Drilling Project (D.S.D.P.) site 465A, the three highest iridium samples indicate a nickel to iridium ratio of about 7,000 whereas the C1 value is on the order of 23,000, or different by a factor of over three. Too much could be made of this discrepancy, but I do think that not enough data are available to assign the composition of the extraterrestrial source to a single type of meteoritic object. There is good evidence of chondritic material, although it may be difficult to rule out an iron projectile. More analyses are needed.

Asaro: Nickel is not a noble element and the deviation is not unexpected in site 465A.

In the pyrite-bearing fraction the Ni/Ir ratio is higher, and in the calcareous phase, which contains most of the iridium, it's lower than in chondrites (Michel *et al.* 1981). We have measured the platinum to gold to iridium ratios in the calcareous phase and find they are roughly consistent with chondritic values. The experiment is difficult and the errors are large. However, at Stevns Klint, Caravaca and site 465A measurements of platinum, gold and iridium are all consistent with type 1 carbonaceous chondrite ratios. Nickel shows great variations from one deposit to another.

Kyte: Platinum is certainly more difficult to analyze radiochemically than gold and iridium. However, in one of our 465A samples (Kyte, unpublished data) a gold to iridium ratio was measured which was 2½ times the Cl value. Fractionation has occurred and your finding a precisely Cl ratio may well be fortuitous; a sample just happened to fall within a very large possible range of sample concentrations.

L. Alvarez: Our original conclusion that an asteroid or a meteorite hit the Earth was based on evidence from different sources. When Ganapathy (1980) first measured what we call a fingerprint of relative elemental abundances, he drew attention to meteoritic material in the Cretaceous-Tertiary boundary in the title of his paper. Although his points are off the graph (Fig. 1), Ganapathy concluded that the material was chondritic.

Kyte: The source was probably chondritic.

L. Alvarez: Everyone, as one of my friends used to say in a discussion like this, seems to be "... in violent agreement". The analytical errors are everywhere, but different kinds of evidence suggests that a chondritic meteorite hit the Earth 65 million years ago.

Kyte: It is important in constraining any model to correctly determine the source composition. One of the main factors in an impact is the amount of energy involved, which is directly related to the mass of accreted material. Iridium concentrations in chondrites vary by a factor of three to four (Mason 1971), and if carbonaceous chondrites are cometary material, ice may also be present. A very large range of masses becomes possible because of this uncertainty.

L. Alvarez: The velocity of impact can also vary be a factor of seven, increasing the energy by a factor of 50, adding cometary ice produces a factor of 100 in the amount of material that could be thrown up.

Kyte: A possible iron meteroid source increases the range of possible concentrations to perhaps a factor of 30.

Hsü: Comets have been described as dirty snowballs. What is the dirt or the solid phase in the core of the comet?

Halliday: The spectrum produced by the dirt from cometary fragments as they come into the atmosphere is amazingly similar to the shock tube spectrum of a chondritic meteorite. The abundances in Solar System materials from which both comets and meteorites are derived are fairly similar in spite of fractionating processes. Qualitatively, cometary and meteor spectra show no suggestion that they are dramatically different.

Hsü: Are cometary cores similar to carbonaceous chondrites?

Halliday: Carbonaceous chondrites are the most primitive of the meteorites, or objects which reach the ground. Cometary meteors are fragile dynamically, and break up at very low pressures in the upper atmosphere. We would think of them as an extension of the meteorite series beyond the carbonaceous chondrites into perhaps more primitive materials.

Kyte: Extraterrestrial dust collected in the stratosphere provides the best evidence for comet compositions (Brownlee *et al.* 1977). The particles range in size from 5 to 50 microns and largely consist of fine-grained aggregates of chondritic composition. In some respects they appear to be more primitive than carbonaceous chondrites.

Hsü: What does more primitive mean?

Kyte: Carbonaceous chondritic material that has been least altered (Fraundorf 1981), by, for example, the action of liquid water, is most primitive. Carbonaceous chondrites have sulphate veins and clay minerals which indicate aqueous alteration. The very fine grain size of these dust particles is such that if they were ever in the presence of liquid water their high surface free energy would result in rapid alteration.

These are very primitive materials, having a high carbon content and perhaps originally a high ice content. The main point is that a large fraction of the dust in the stratosphere is apparently carbonaceous chondritic, or something very similar, in compositon.

Rucklidge: It was mentioned that the accelerator measurements were off figure 1 in the upper direction. Our method of making these measurements is in the early stages yet and the problem is one of calibration. This method was developed by a group from the universities of Toronto and Rochester, and General Ionex Corporation, for direct measurement of carbon 14 using a tandem accelerator as a mass spectrometer, and can be used for the ultrasensitive detection of other elements. Platinum and iridium have the fortunate property of forming negative ions very readily, a necessary condition for these elements to be analyzed by this method. Our results for iridum from the boundary clay at Stevns Klint, Denmark, confirm the measurements published by Alvarez *et al.* (1980). Platinum can be more easily measured than iridium, contrary to the case with neutron activation, and we have been able to make platinum to iridium comparisons. The problem of covering a larger mass range, to embrace the lighter platinum group elements, such as rhodium and palladium, is more difficult. The sample is in a pressed powder form, and a beam of cesium ions is directed at it. Ions sputter off the surface and as time proceeds the cesium beam erodes a hole. If the material is not homogeneous then the signal varies. We discovered that the platinum signal varied with time quite dramatically and this could be correlated with the presence of pyrite in the sample. Assuming that the high values corresponded with the presence of pyrite, we prepared a heavy mineral separate using heavy liquids to extract the pyrite from the samples. The heavy and light fractions were compared, and it was found that platinum and iridium were enriched in heavy fractions relative to light fractions. There appears to be heterogeneous distribution in these materials in the Danish boundary clay. A sample was taken of a pyritiferous Ordovician shale from Oshawa, Ontario. The pyrite from this shale yielded about three parts per billion of iridium. In a second series of experiments an improved ion source was used, in which the cesium, which had formerly flooded the sample, was changed to focus onto a spot of about 200 microns in diameter and became, in effect, a microprobe. In the second series of experiments it transpired that a heavy metal bearing phase might or might not be hit, and extremely low or considerably higher values could be encountered. Three samples were taken from D.S.D.P. site 465, two of which were pyritiferous and one of which was a white ooze just below the pyritiferous layer. There was a factor of approximately 20 between the pyritiferous and ooze samples, which is approximately what one would expect. The pyritiferous samples, however, yielded concentrations of only 0.04 PPB of platinum and 0.06 PPB of iridium in one, and the elements were not detected in the other. We estimate the detection limit for iridium and platinum to be approximately 1 PPT, on the basis of a 15 minute measurement. The ratios vary considerably I must admit, and lie quite a bit above points shown on Fig. 1. One other experiment was made by a student of Geoffrey Norris, Silvana de Gasparis. She was interested in the possibility that the noble metals might in fact be associated with the pollens and other organic materials, and prepared an organic extract from some of the Danish clays. The platinum and iridium contents of this extract were well over a part per million. The extraction had been performed with nitric acid, and the noble metals may have been concentrated in the organic residue. Samples were subsequently prepared in which extreme care was taken to eliminate by physical means all the metallic phases, and only minute amounts of organic material were extracted. These also showed significantly high platinum and iridium content. Now this is interesting in the light of an abstract by a group at Los Alamos (Orth *et al.* 1981), where they have reported an iridium anomaly in New Mexico in association with coal seams at the Cretaceous-Tertiary boundary. Geologists are familiar with the fact that coal seams provide a reducing environment in which heavy metals are often concentrated. Uranium in fact has been mined from coal seams, and a possible geochemical affinity exists which should not be overlooked in examining this problem.

Asaro: They have located the boundary using palynofloral evidence to within one meter, and the iridium anomaly was in that meter. The maximum concentration of iridium is two to four parts per billion and the enhancement over the background is about a factor of 100. The iridium was concentrated chemically and sensitivities are comparable to your work.

Kyte: Our recent work indicates that a reducing environment may not be required to produce an iridium horizon. We found it in an abyssal clay in the mid-Pacific, which is typically a rather oxidized environment. Preliminary data suggest an iridium content of about 80 nannograms per cm^2 or more in that horizon (Corliss and Hollister 1979).

Hsü: Dr. Kastner noted that unusual clay minerals are present in the boundary layers in Denmark and at D.S.D.P. site 465. In our boundary layer in the Southern Hemisphere core, the iridium concentration is not as high as in the Northern Hemisphere, reaching a maximum of only 3.3 PPB (Hsü *et al.* ms.). The anomaly may be obscured by a detrital dilution effect, because most of the clay minerals are terrestrial detritus. Smectite is present but it is not unusually abundant. I would like to pose the problem on how this iridium anomaly should be expressed. You show it in nannograms per square centimeter on your map (Fig. 2).

Asaro: Yes.

Hsü: This would solve the problem of vertical disturbance like bioturbation. However problems would remain in the case of lateral transport through slumping, turbidity currents or bottom currents. The boundary layer is thicker in some areas because of resedimentation. It might be worthwhile to prepare a map showing the maximum concentration as well as the total excess iridium.

W. Alvarez: Our map (Fig. 2) shows excess iridium in nannograms per square centimeter in all of the localities for which we have information. It contains work by ourselves, and data published by Smit and Hertogen (1980), Kyte (*et al.* 1980), and Ganapathy (1980). Hsü and Kräyenbühl's data (personal communication) are also included, as well as new information from New Mexico (Orth *et al.* 1981) and from the Brazos River section in Texas (Ganapathy and Gardner, in press). Another new point is based on samples from Haiti given to us by Florentine Maurrasse at Florida International University. There are two northern Spanish samples, and Smit's section.

L. Alvarez: The largest value of 520 is from the thickest layer, and may be taken as evidence of lateral transport of material. The concentration is similar to that in other sites.

Hsü: What is the concentration in the North Pacific core?

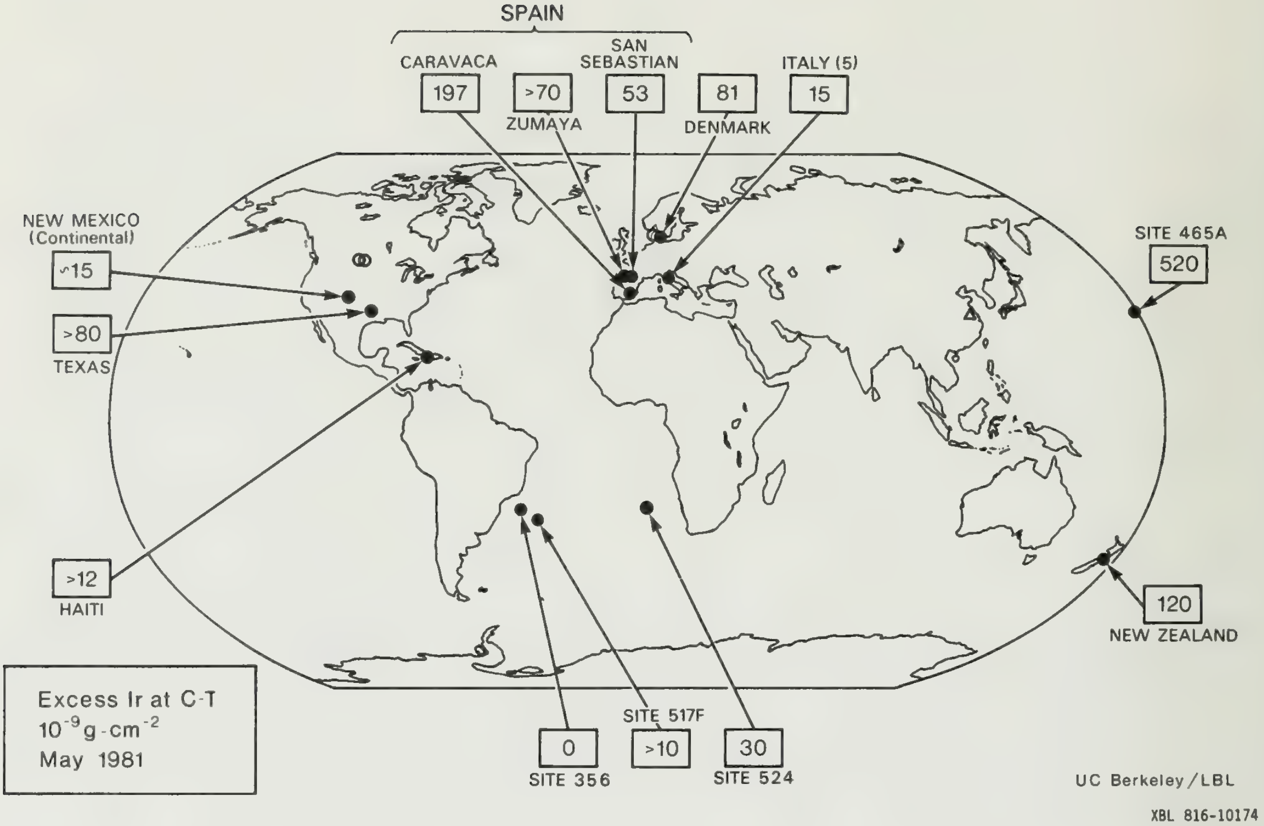

FIGURE 2: *Excess Ir in nannograms per square centimeter in Cretaceous-Tertiary boundary samples from United States, Haiti, New Zealand, Spain, Italy, Denmark and several oceanic sites.*

Asaro: In the calcareous part it's about 9 PPB, and in the pyritic part it's considerably higher.

Kyte: We measured 16 ng/g, but in addition to pyrite the clay content was also very high, with the total insoluble residue approaching 35% (Kyte *et al.* 1980).

Asaro: We found that the material containing the pyrite in D.S.D.P. site 465A is a magnesium aluminum silicate. Maximum pyrite concentrations are about 25%, but the rare earth content is extremely low (Michel *et al.* 1981). A significant cerium depletion indicates the material was deposited in place. It is not as depleted as are the normal calcareous materials so another component may be present. An analogous situation exists in the low abundances of slightly different rare earths in the acid insoluble fraction in the Danish boundary layer. These contrast with montmorillonite deposits in general where rare earths are considerably higher. We have tried to interpret the low rare earth content in terms of an impact of an asteroid in the ocean, with a large component of mantle material in the resulting ejecta. So far we are not getting good quantitive agreement with this model.

Hsü: What would be the distribution pattern of ejecta if it is deposited from the atmosphere as fallout? Would the pattern resemble that of fallout from nuclear explosions?

L. Alvarez: The atmosphere would be very different from conditions following an atomic bomb test. Many cubic kilometers of vapourized water would be injected into the atmosphere. Turbulence and currents would be like nothing I could possibly guess. Someone who is good at atmospheric modelling should examine this carefully. It should not be explored by analogy. Even Mt. St. Helens did not perturb the atmosphere appreciably. Only a very small amount of material was added to the atmosphere, and the atmosphere remained very well behaved.

Grieve: We cannot make analogies with volcanic explosions. On penetration, a bolide would create a hole in the atmosphere, which would disturb atmospheric circulation patterns.

L. Alvarez: As the atmosphere rushes back into the vacuum one can't imagine the consequences to the weather.

Smit: Among the rare earth elements, was only this cerium depletion found in the Danish section?

Asaro: Cerium is slightly depleted with respect to the other rare earths in the acid insoluble residue of the Stevns Klint boundary clay, but it is in no way comparable to the depletion in the pyrite-containing material of D.S.D.P. site 465A. The rare earths as a group are in rather low abundance in the Danish acid-insoluble boundary residue, as they also are in the site 465A pyrite-bearing silicate.

Smit: Using neutron analyses in whole rock, we (cf. Smit and Hertogen 1980, unpublished data) found a series of significant depletions of five rare earth elements only in the basal millimeters of the Danish boundary clay section. We found a similar depletion in the Caravaca section (Spain) and in the Le Kef section (Tunisia), in the basal millimeters of the boundary clay. At the level of the highest nickel, chromium and iridium abundances, we also found significant rare earth element depletions. We devised a model where an asteroid plummets into the ocean, and is mixed with oceanic basalts to produce the correlation between the highest iridium enhancements and the rare earth element depletions. In four sections, the highest iridium concentrations are associated with a significant rare earth element depletion. About 600 PPM of sulphur have been detected.

Kyte: An oceanic impact should not be modelled with only basalts in the ejecta. There would also be a significant mantle component which would probably be enriched in elements such as nickel and chromium. Yet there is barely enough nickel and chromium present to support a chondritic projectile.

Smit: In the Caravaca section we detected levels of 3,000 PPM nickel, which is an order of magnitude higher than in the Danish sections. The nickel-iridium correlation is probably better in that section than in Denmark, and conditions were not anoxic. An abundant benthic fauna is present, unlike in Denmark. The only correlation we see is between a rare earth depletion and an iridium enhancement. The latter amounts to 45 PPB at Caravaca, or about as much as in the Pacific cores.

W. Alvarez: In the Gubbio section, the highest iridium levels occur in the upper half of the boundary clay, which is bright red.

Hsü: The iridium anomaly at D.S.D.P. site 524 in the South Atlantic also occurs in a well-oxidized, hematite-rich clay without pyrite concentrations. By the way, Katharina Perch-Nielsen, a nannofossil specialist, believes that the Cretaceous-Tertiary sequence is incomplete at D.S.D.P. site 356, and that the absence of iridium at this site could be explained by removal of sediment at the boundary, on the Rio Grande Rise.

Kyte: The possibility of using concentrations, as opposed to net iridium per square centimeter, could be useful in estimating the distribution of the horizon around the world and calculating the total mass of material accreted. However, these concentrations should be reported free of indigenous sediment. For example, if 44 PPB iridium occurs in the Caravaca section, which is is turn 50% carbonate, a value of about 88 PPB or more should be reported.

L. Alvarez: That's why we used nannograms per square centimeter.

Hsü: It can also easily be expressed, for comparative purposes, in terms of concentration in the insoluble residue, as Kräyenbühl (Hsü *et al.*, ms.) did.

McLean: The possible volcanic origin of the smectite has been raised. At the time of the marine extinctions one of the greatest episodes of volcanism in all Earth history began. The Deccan Traps volcanism in India was initiated 65 million years ago and lasted up to 60 million years ago. Is it possible that the smectite originated as a result of Deccan Traps volcanism?

Grieve: The Deccan Traps are basalts, and basalts are not usually associated with large scale pyroclastic eruptions.

McLean: India during the period of the Deccan Traps volcanism was located near the equator where oceanic currents could carry volcanic materials into each hemisphere. A lot of submarine volcanism occurred at the same time.

Hsü: But Deccan volcanism spanned several million years, and the smectite rich layer is only a few millimeters thick.

McLean: A key issue here is, how much time does that boundary clay really represent? A very short amount of time? Or is it a lag deposit representing quite a long interval.

W. Alvarez: The Deccan Traps cover a very large area in the northwestern part of India and they are about 1,000 m thick at their maximum on the west coast. One of the problems in studying Deccan Traps stratigraphy is that outcrops are poor and usually covered with a lot of vegetation. However, sections along the coast near Bombay cover only two magnetic polarity zones. The lower half is reversely polarized and the upper half is normally polarized. Volcanism thus began in a reversed polarity zone, and in the Italian section the extinction occurs in a reverse polarity zone. The best available work on correlating polarity zones to the eastern side suggests that below the reversed zone volcanism occurred in a normal polarity zone, and below that another reversed polarity zone occurred. These correlations are not well established, but appear valid. There are thus at least two and probably four polarity zones involved in the volcanism of the Deccan Traps. This many polarity zones cannot be missing in the Italian section. The reversal sequence at Gubbio matches the sea floor spreading magnetic anomalies well, and there are no missing reversal zones. The Deccan Traps clearly represent much more time than can possibly be missing in the Italian sections.

McLean: The extinctions may not have spanned five million years. Perhaps they occurred over a much shorter interval, during the earlier phases of volcanism. Could some of the smectite be of volcanic origin?

Asaro: Kastner stated that she could not distinguish between a volcanic and an asteroid impact origin for the material. If it were derived from the mantle, I would expect it to be enriched in platinum with respect to the iridium. The material from the Danish boundary layer does not appear to be enriched in platinum with respect to iridium.

McLean: Morgan (1972A, 1972B) has suggested that Deccan volcanism is a result of mantle plume activity, with the plumes originating near the base of the mantle. Relatively large concentrations of the noble elements would be expected to occur in the Earth's core. And if in fact the Deccan Volcanism does represent mantle plume activity, the plumes would provide a conduit to bring noble elements from Earth's core to Earth's surface, where they would be distributed geographically.

Kyte: One problem in deriving the noble metals from the core is that, during crystalization, iridium strongly fractionates into the first solid, so that the inner core probably contains most of the iridium below the mantle.

McLean: Elsasser and his colleagues (1979) suggest that circulation may even involve the outer core itself. They visualize whole mantle circulation.

Kyte: The partition coefficient between solid and liquid for iridium is nevertheless so high that the solid inner core probably contains 90% or more of the whole core's iridium. Also, because of this fractional crystalization, it seems highly unlikely that chondritic abundances would occur in the outer liquid core.

Hsü: Many have suggested that the boundary clay layers are lag deposits representing a long interval of geologic time. The Gubbio paleomagnetic work is excellent, and an amplified section at D.S.D.P. site 524, essentially confirms its validity. Another objective of deep sea drilling in our leg was to investigate the chronology of sea-floor-spreading anomalies and determine if spreading rates were linear. Using precision stratigraphy, the linear rates were confirmed (Hsü *et al.*, in press). Within this framework the negative interval of sea-floor Anomaly 29, during which the extinctions occurred, could not represent more than a half million years.

McLean: A half million years is the figure I use.

Hsü: The extinction horizon represents only one centimeter out of the five or 10 meters of the clay contained in sediments deposited during Anomaly 29 time. The time required for its deposition is on the order of a few hundred to a few thousand years. Magnetic stratigraphy restricts the time available to a geological instant.

McLean: In Denmark, Bromley (1979) shows that this boundary clay is sitting on top of an unconformity. We don't know how much upper Cretaceous is missing before the early Tertiary boundary clay was deposited. In some cases this boundary clay was apparently deposited upon a surface that was even lithified before clay deposition began.

Thierstein: We are discussing high resolution stratigraphy in sequences which may or may not be complete. We can search for sections where we think, according to partly subjective evolutionary and phylogenetic models, the succession is complete. The phlogenetically youngest planktic foraminifera and calcareous nannofossils that have ever been found in any late Cretaceous section should be present as well as the most primitive foraminifera and lowest diversity coccolith assemblages that have been found of earliest Tertiary age. In addition, extinctions and first appearances below and above the bounday can be used to define zonal subdivisions. In Gubbio, Italy, for example, only foraminiferal zones can be recognized (Premoli Silva 1977). In most of the deep-sea drilling sites, it is possible to recognize coccolith zones as well. In several deep-sea sections, equivalent foram zones near the boundary are two to three times thicker than the same zones in the Gubbio section. Over 90% of the planktic foraminifera and the coccolithophorids became extinct at the boundary itself. In most of the sections not disturbed by preservational changes in carbonate fossils or by slumps and turbidites, the carbonate content of the earliest Tertiary sediments is very much reduced. Large sedimentation rate changes across the boundary must, therefore, be expected. Some microfossil assemblages in different sections do not resemble each other and micropaleontologists cannot agree whether differential preservation or evolutionary patterns are responsible for the dissimilarities. For instance, the *G. eugubina* Zone, which is the earliest foram zone of the Tertiary, contains a very primitive *Globigerina* type foram. At Gubbio the zone is about 40 centimeters thick. At D.S.D.P. site 356, where the iridium anomaly has not been found, the zone is about 50 centimeters thick. In Tunisia, in the El Kef section, Dr. Katharina Perch-Nielsen (in press) notes that the zone is nearly three meters thick, and at site 465A it is more than two meters thick as well. Very high resolution stratigraphic studies may not elucidate evolutionary patterns very close to the extinction horizon. It may be more useful to compare biostratigraphic events further removed from the Cretaceous-Tertiary boundary such as the base of the latest Cretaceous foram and coccolith zones or the first appearance of *Cruciplacolithus tenuis* and *Chiasmolithus danicus* which occur above the extinction horizon. However, the section may appear to be complete over a time span of one or maybe one-half million years, but allowing for differential sedimentation rates within that interval, several tens of thousands or several hundreds of thousands of years could be accommodated in a clay layer. Only comparisons between sections can shed light on whether one section is more complete than another or whether redeposition or volcanic inflow has occurred in individual sections.

Hsü: In Denmark, the boundary layer or Fish Clay is a hard ground, which is a little harder and more slowly sedimented than adjacent layers. However, according to Perch-Nielsen (1979), all of the nannofossil zones are present there. Correlations suggest that this hard ground was of very short duration. Late Quaternary hard grounds in the Mediterranean Sea have been radiometrically dated to have been formed within a few thousand years (N. Shackleton, personal communication). Thus the presence of hard ground at the Cretaceous-Tertiary contact cannot account for the high iridium concentration in Denmark. The time represented by the hard ground in the Danish section must be within a minor portion of the Anomaly 29 negative-polarity interval.

Thierstein: Unfortunately, the thickness of the latest Cretaceous foram and coccolith zones in Denmark are not accurately known, but they must exceed 60 meters (Stenestad 1979). This is much thicker than in any deep-sea section. Sedimentation-rates were very high during the latest Maastrichtian and were also high during Danian time. The boundary clay layer could still represent a significant amount of time, although the mineralogy argues against it.

Rucklidge: A new section has been exposed in Jutland at Nye Kløv and I was able to obtain a few samples of marl from Dr. Hans Jørgen Hansen in Copenhagen. Our measurements indicate they are extremely low in both platinum and iridium. No anomaly is indicated.

Smit: Were your samples taken from the correct marl? I have a difference of opinion concerning the identification of the boundary clay as published in the Copenhagen symposium. The true boundary in my opinion was one or two meters below the horizon indicated.

Kyte: Were the samples from the lower marl?

Rucklidge: I believe so.

Kyte: Hansen sent us a few of those samples too, but they did not resemble the Fish Clay at Stevns Klint.

Hsü: In Denmark there are hard grounds suggesting bottom current activities, as well as a variably thick boundary clay. Denmark was evidently an area of strong bottom currents and materials were removed from some areas and concentrated in others.

Thierstein: Paleontological investigations based on forams and coccoliths indicate that the Jutland sections are more complete than the section at Stevns Klint.

Asaro: Ganapathy *et al.* (1981) have reported an iridium anomaly in Jutland. We have separated out the dense mineral fraction in the Danish boundary clay and in the pyrite-containing material in D.S.D.P. site 465A. The iridium is not concentrated in the heaviest fraction containing the pyrite. We also did a density-fractionation study on the Danish boundary material and many of the elements, such as iron, selenium and antimony, follow the dense fraction, but not iridium. In site 465A the iridium was not depleted in the pyrite-containing phase, but since that phase is so small it contains only a small amount of the total iridium present.

Rucklidge: Do you have any comparable figures for the platinum?

Asaro: We have made only one measurement of platinum in site 465A, from the whole rock. We have no fractionation or heavy mineral information.

Dilcher: We have discussed the composition of the Cretaceous-Tertiary bounday clay layer in some detail. Have there been any comparable analyses of volcanic materials, or deep mantle, shallow mantle or even deeper core materials which span the Cretaceous-Tertiary boundary?

Asaro: The lavas that are most similar to the mantle in composition are the komatiites. These were large nickel sulphide flows that occurred in Archaean times (Arndt *et al.* 1979). The platinum to iridium ratio (Naldrett and Duke 1980) is a factor of 1.7 to 1.8 higher than the Cl chondrite value for the classic komatiites. The Au/Ir ratios were at least a factor 1.9 higher than the Cl values. There is an ultrabasic nodule (Kaminskiy *et al.* 1974) which has a roughly chondritic Pt/Ir ratio (1.8). Other ratios, which we have not considered here (e.g. Pd/Ir), do not occur in chondritic proportions.

Kyte: Were these xenoliths?

Asaro: Yes.

Kyte: Probably the best approximations of mantle composition can be determined from xenoliths in basalts. The source of the noble metals in the upper mantle is believed to be from late stage bombardment following segregation of the core (Chou 1978, Arculus and Delano 1981). Lunar evidence indicates that this occurred up to four billion years ago. Most estimates for the composition of the upper mantle include about 1% chondritic abundances for noble metals. Estimates of platinum to iridium ratios are probably within a factor of four of the chondritic abundances. However, these concentrations are significantly lower than those in the Cretaceous-Tertiary boundary clay. A mantle source for the latter would seem unlikely.

Dilcher: As a paleobiologist, could I conclude that the noble metals in the boundary clay would likely not have a volcanic source?

Kyte: The mantle is an unlikely place to find noble metals in such a high concentration. Even a plume from the core seems a rather unlikely possibility because one would expect the outer liquid core of the Earth to be highly fractionated relative to its initial composition.

McLean: Platinum group elements in the boundary clay have been acted upon by the chemistry of seawater. Some of them are far more mobile than they were previously thought to have been. Sodium-iridium-chlorine compounds have been made in the laboratory that are soluble in water and it is reasonable to assume that they might exist in nature. Platinum and iridium also have different calcophilic characteristics, and the ratios in the boundary clay may not be comprehensible unless one can address the chemistry of the oceans at that time. Some platinum group elements seem to go into solution with organic acid compounds and the problem of what was distributed here and there is difficult to come to grips with. Platinum-group concentrations are often cited from basic rocks. Are these derived from sulphide phases?

Kyte: Most measurements of basic rocks were carried out on bulk samples. With respect to Cretaceous-Tertiary sediments, we noted the ubiquitous presence of pyrite. Subsequently, however, the iridium enhancement has been identified in oxidizing environments.

Smit: Apart from that, one would expect the ratios to deviate from the chondritic ratio due to the effects of oceanic environments. A pattern should emerge which deviates from the chondritic pattern Ganapathy (1980) described.

McLean: Considering that the Earth is made up of accreted Solar System material, why would one not expect that some areas within the Earth would not approximate extraterrestrial ratios.

Asaro: It is not likely that one would obtain volcanic material from the upper mantle which would have these ratios. It is difficult to obtain material which would produce the proper absolute amount of iridium. Then marine processes and other effects would be required to change the platinum and the gold to chondritic ratios within a few percent. This is a very low probability situation. We have obtained consistent measurements from two other places besides Denmark. Uniform, world-wide conditions of marine chemistry and weathering would therefore be necessary to produce similar ratios.

McLean: Håkansson and Hanson (1979) have suggested that a world-wide clay event or calcium carbonate dissolution event may have occurred. Chamley and Robert (1979) suggest that the clays indicate a warmer, more aggressive climate. If in fact this boundary clay represents a lag deposit over a relatively long period of time, of 50 to 100 thousand years, then extraterrestrial meteoritic dust would accumulate as a concentrate in the clay. Barker and Anders (1968) discuss this point. The time of deposition of the boundary clay must be known with better precision.

Kyte: Barker and Anders (1968) studied iridium and osmium concentrations in abyssal clays which were accumulating at rates on the order of one millimeter per thousand years. The concentrations were on the order of 100 times lower than in some Cretaceous-Tertiary boundary sediments.

McLean: If the boundary clay is a lag deposit, could part of it be extraterrestrial dust?

Kyte: If the modern flux rate is assumed to be normal, and if this rate was valid 65 million years ago, the boundary clay would represent on the order of a million years of accumulation.

Thierstein: It would take only 18,000 years to deposit all the iridium present in the Gubbio bounday clay at present day iridium accumulation rates in the deep sea, if one assumes:

- today's average accumulation rate of iridium in deep-sea sediments $R_T = 3.5 \times 10^{-10}$ g cm^{-2} ka^{-1} (Crocket and Kuo 1979, p. 840)

- the insoluble fraction of the 1 cm thick boundary clay at Gubbio is 44% (CF = 0.44) and contains an average of 6.5 ppb of iridium (ppbIR) (Alvarez *et al.* 1980, p. 1097 and 1100).

- the sedimentation rate (S_G) in the Gubbio G-magnetic zone is 1.28 cm ka^{-1} (Kent 1977, p. 771)

- the specific gravity of clay $\rho_c = 2.7$ g cm^{-3} (Keller and Douglas 1980), and the porosity of clay $\emptyset = 0.2$ (Pettijohn 1975),

then the total iridium contained in 1 cm^3 of boundary layer:

$$\Sigma IR = 1 \times CF \times \rho_c \times (1-\emptyset) \times ppbIR = 6.2 \times 10^{-9} \text{ g}$$

and the time required to accumulate that amount of Ir at present day rates

$$T = \frac{\Sigma IR}{R_T} = 17.7 \text{ ka}$$

Hsü: Then why are comparable iridium concentrations not found in the red clays in the north Pacific cores? Sedimentation rates there are about 0.2 millimeters per thousand years. At this rate, economically exploitable quantities of iridium should occur within 100 thousand years.

Thierstein: The problem is to keep away all other terrestrial, detrital material from the Gubbio clay.

Kyte: If deposition stopped at Gubbio, would it not have continued elsewhere?

Thierstein: Not necessarily. These calculations assume steady state mechanisms. Most of the oceanic biomass is today recirculated in the photic zone. A very small amount of it reaches the sea floor an even smaller amount is incorporated into sediments. If one assumes, and I have not done these calculations, that iridium and some of the other metals are fractionated and enriched even to a small extent in the biomass, and if it were not transported to the ocean floor by fecal pellets of grazing organisms as it is today, iridium could become enriched in the ocean. When the first grazers reappeared and began wrapping the biomass up and transporting it to the ocean floor, perhaps the organics might be recirculated, but the heavy metals might not be. There was no steady-state mechanism at the Cretaceous-Tertiary boundary and the biochemical cycling must have been interrupted because much of the biomass in the photic zone out in the oceans was exterminated.

Hsü: This brings us back to the definition of a rare or catastrophic event, e.g. that a major perturbance of the steady state produced an iridium anomaly. Why did this particular disturbance of the steady state occur at that particular time? This cannot be understood by involving gradualistic processes.

Thierstein: The point is, could the iridium enrichment in deep-sea sediments at the boundary be a consequence of the biotic extinctions rather than a cause, as has been proposed by K.M. Towe (personal communication).

Feldman: Thierstein said earlier that some 18,000 years might have been all that was required to obtain the iridium enhancement from accretionary accumulation of extraterrestrial dust. This would require the Solar System to have passed through the darkest of the dark interstellar gas and dust clouds, with molecular hydrogen number densities $n(H_2) \sim 10^5$ cm^{-3} and a standard dust-to-gas ratio $n(\text{grains})/n(H_2) \approx 8 \times 10^{-13}$ (cf. Lang 1980). In this extreme case 10 ngm cm^{-2} of iridium could be accreted on time scales between 8,000 and 100,000 years. Thus, a very unusual kind of accretionary scenario is required to produce the kinds of abundances found in the boundary clay on a time scale of ~ 20,000 years. In the original paper by Alvarez *et al.*, (1980) the ratio of the iridium 191 to 193 isotopes came out very close to the Solar-System value. Has further work been done on other sections that would shed more light on this?

Asaro: We have only made that measurement on the iridium from the Gubbio boundary layer.

L. Alvarez: I might explain why. When Frank Asaro and Helen Michel first measured the iridium ratios, they found values that differed from Solar System values by 5% and we suspected we had evidence of a supernova. They measured it again and found it was 5% off in the opposite direction, alerting them to the fact that there was a serious systematic error in the measurements. The error was traced to two quite separate effects. One was geometric and the samples had to be positioned at the same distance from the counter within a few thousandths of a centimeter. The other was that the samples had to be bombarded in the reactor in the same place. Because two samples cannot occupy the same position, two standard samples of normal iridium were placed three or four millimeters apart with the unknown halfway between. If an average of the two standards was taken and compared to the sample, then the ratio could be determined to within 0.2 to 0.3 of a percent. I have rarely seen an experiment that was more difficult or required more insight to explain the systematic errors. Soon after, Smit and Hertogen reported that the osmium ratios were the same to within a tenth of a percent. We were uncertain how to accept that, simply because one can be off by 5% and not even know anything was wrong. It is an exceedingly difficult experiment. One of the isotopes has an 18 hour half-life, requiring work around the clock for a couple of days.

Smit: Most of the work was done by Hertogen, who notes that the osmium isotopes 184 and 190 have relatively long half-lives.

L. Alvarez: The difficulty arises not from the half-lives but from the fact that the gamma rays come out in different proportions and in coincidence. The counter may pick up two simultaneous gamma rays and will then add the energies together as if they were one. This effect leads to the loss of a gamma ray count, and it is very sensitive to the geometry. So the samples were finally located on little sapphire balls to within about a thousandth of a centimeter. I have asked a number of very good physicists what could make the apparent isotopic ratios change so rapidly with source-counter distance, and not one of them could explain it, until given some broad hints. The answer showed up accidentally in this technique, which had never been used before. Because Hertogen didn't mention all the problems he went through to obtain his number, we think he was probably lucky.

Smit: You repeated the experiment twice with more or less the same results?

L. Alvarez: It was repeated, and had it come out 5% off in the same direction we would have published it as evidence of a supernova.

Asaro: Perhaps the osmium ratios are not as difficult as the iridium.

L. Alvarez: We know they are somewhat easier, as we have examined the gamma ray spectra, but not an order of magnitude easier.

Smit: Hertogen decided not to use the iridium isotopes, preferring radiochemically purified samples of osmium.

L. Alvarez: This brand new effect was discovered by Asaro and Michel as they performed the experiments, and has not been described in the literature where Hertogen and others could have been aware of its existence. It would be interesting to remeasure the osmium ratios.

Feldman: Were the osmium and iridium measurements on the same sample, from the same section?

L. Alvarez: No.

Feldman: This is an important point to pursue, in order to be certain that the material is of Solar-System origin.

L. Alvarez: There seems to be no information on how big a variation one could expect from different supernovas.

Tucker: The idea that all these heavy elements originated from supernovas is suspect. Most attempts to reproduce the patterns have failed, and the only attempt that has succeeded, reproduces the pattern of abundances in a red giant (Cowan *et al.*, in press). A red giant of about one solar mass is a very common star, and one would expect that the interstellar material was similar to the Solar System material. Fluctuations of two would not be expected because mixing scales are now on the order of two parsecs rather than hundreds of parsecs. It may not be useful to pursue these experiments in view of their difficulty and the uncertain value of the results.

Asaro: With respect to a possible lag in the sedimentation of the boundary clay, there is a way of looking at this which might be helpful. In Denmark, we have detected 31.5 PPB of iridium in an approximately 1-cm-thick K-T layer. The clay is 44.5% non-calcareous, yielding between 65 and 70 PPB of iridium per unit of clay. Dr. Kastner has studied the clay mineralogy and states that 90% or more is authigenic, that is, not due to the normal deposition of detrital clay. This implies that the abundance of iridium as a function of normal detrital clay deposition is at least 700 PPB, or about the chondritic abundance. The amount of time needed to deposit 5 mm of meteoritic material is something like 10 million years. All detrital clay deposition must cease and the result would be fairly pure meteoritic material mixed in with the authigenic montmorillonite that Dr. Kastner found.

McLean: How does this observation translate to the boundary clays in other sections; are they similar or does a different scenario apply to each one?

Asaro: To a good approximation, a different scenario applies to each one, in terms of the major element abundances. In New Zealand, the boundary sediments are not clay; they are limonite which is a hydrous iron oxide. We do not have this comparable information for other sections. In Gubbio for example, there is a very considerable detrital component in the boundary material as indicated by Dr. Kastner's work, and high rare earth abundances.

Smit: In the Caravaca section, there is no difference between clay content below and above the boundary, and the boundary clay. Only the basal millimeter of the 10 centimeter-thick boundary clay contains some smectite and is different in clay composition from the remainder of the clay. The latter also contains a significant amount of iridium.

Asaro: A similar situation occurs in D.S.D.P. site 465A. A considerable clay component is spread out over some 50 centimeters and the iridium is also spread out. It is a detrital clay and at this point we do not have an adequate explanation for the phenomena.

Hsü: It is interesting that rare earth elements are depleted in the boundary clay in contrast to conditions in plume basalts which are enriched in rare earths. Oceanic basalts are usually depleted in rare earth elements.

Asaro: The rare earth abundances are low in some K-T boundary materials we have measured, but they are not as low as in chondritic materials.

Hsü: Are they depleted with respect to the terrestrial average?

Asaro: Yes. The depletion I spoke about concerns cerium, which is chemically different from the other rare earths. It is deposited in manganese nodules, leaving a deficiency of cerium in seawater. A large cerium depletion in the acid soluble components in clay in D.S.D.P. site 465A indicates it was laid down out of the seawater.

Hsü: The question of whether the elemental abundances could be volcanic could be resolved by comparing the boundary clay rare earth data with a tremendous amount of data from the Deep Sea Drilling Project on the mantle plume basalts.

Brasch: Is it possible that more than one catastrophic event occurred, such as successive impacts of different types of bolides or perhaps a chain of volcanic events following an initial impact?

McLean: A hot spot under the Earth's surface is where one of the deep origin mantle plumes comes to the surface, and Morgan (1981) suggests that such an area will be inundated by flood basalts. A hot spot is present for tens of millions of years before the volcanism occurs, which might mitigate against a meteoritic impact causing the plume. As a lithosphere plate migrates over a hot spot, it is domed up and fissured, and lavas emerge from the fissures. Flood basalts are not characteristically explosive in nature. I know of no explosive cratering associated with the Deccan volcanism. Where is the crater of a large Cretaceous-Tertiary bolide today? Ejecta from large craters are not associated with large quantities of iridium and osmium. It would appear that some of the large earth-crossing asteroids which have impacted did not produce large amounts of siderophile ejecta.

Grieve: Not all, but some have. The impact melts in about 10 craters have identifiable siderophile enrichments, often at about one to two percent in C1 abundances.

Kyte: Some impact melt rocks in Clearwater East contain up to 7% chondritic abundances (Palme *et al.* 1979).

Hsü: Are impact melts composed largely of terrestrial materials?

Grieve: The impact melt is made up of melted target rocks, which are contaminated by material from the projectile. The nature of the impacting body can be identified through the contaminants.

Hsü: Therefore, the reason why iridium anomalies do not occur more frequently in sediments is because the ejecta from the impact of solid objects does not have a high iridium content.

Kyte: Impact events, in general, should produce ejecta that is only slightly enriched in iridium.

Hsü: The bolide, then, disintegrated in the air, and this is why an iridium-enriched layer occurred.

Kyte: We suggested the possibility that a cometary object was disrupted tidally within the Roche limit, and broke into several fragments which then disintegrated in the atmosphere. The material was not diluted by large amounts of crater ejecta. This could lead to an accretion event that would span several months or perhaps several years. Another possibility would be an enriched source, such as an iron meteoroid. However, the chromium enrichment in the boundary horizon might be significant enough to rule out an iron meteoroid.

L. Alvarez: A large asteroid would probably not be broken by tidal forces as it passes by.

Kyte: It is a complicated problem. Öpik (1972) suggested that if the proto-Moon had passed within a few Earth radii of the Earth it would have been disrupted by tidal forces.

Halliday: The broken cometary fragments will continue in essentially the same orbit. If the nodes of the orbits are near the Earth's orbit, then the probability is high that they will collide with the Earth within a few tens or hundreds of thousands of years.

W. Alvarez: There was a news article in Science (Kerr 1981) recently about new evidence that asteroids may possess satellites of their own, or that two similar-sized asteroids may tumble about each other. Would this allow a binary set to impact simultaneously, an otherwise extremely improbable event?

Halliday: This was considered many years ago for the Clearwater Lakes. An astronomer from the former Dominion Observatory investigated this and found that the configuration would be stable against planetary perturbations, but the objects would diverge in the last few hours before impact.

W. Alvarez: Apparently there is tenuous observational evidence, based on star occultations, of binary asteroids.

Halliday: Several have been reported and are still debated.

L. Alvarez: Several have been seen in the same neighbourhood by independent observers. Some (Kyte *et al.* 1980) have suggested that the biggest problem with the asteroid theory was that there was too much iridium in the boundary layer. We used the number Richard Grieve supplied to us that an asteroid would be diluted by 60 or 70 times its own weight of crustal material. In order to further reduce the amount of crustal material ejected, it could be postulated that the asteroid impacted in the ocean and some reasonable fraction of its energy was dissipated by vapourizing water. What is your best estimate to date?

Grieve: Massive impact events are poorly understood. Our knowledge of crater mechanics is largely derived from small-scale experiments, such as TNT or nuclear explosions. These are very small in comparison to major impacts. Various computer codes, which are very expensive, can model the flow field, excavation process, and amount of material excavated by craters up to a few kilometers in size. For the 3.8 km Brent crater, the dilution factor of the volume of material excavated relative to the volume of the projectile is modelled to be of the order of 3-400 (Grieve and Cintala, in press). The amount of material excavated depends on initial conditions, i.e. for a given projectile size, the nature of the target, the density of the projectile, and its velocity. We have been attempting to model large impact events, such as the 40 million-year-old, 100-km-sized Popigai crater in the Soviet Union. In that case it seems that the flow field changed with time. In small impacts gravity does not act upon the flow field, and the calculations are simpler. In larger structures it appears that dilution factors may approach the thousands. With the exception of E. Clearwater, impact melts concentrated in the centre of the crater, rarely contain more than or one or two percent of C1 abundances (Palme *et al.* in press and references cited therein). In the few instances where we have examined ejecta thrown from the crater, siderophile enrichments were not detected. Perhaps a real problem exists in the case of dilution effects.

L. Alvarez: If, using the asteroid model, it is difficult to account for the iridium anomaly in Denmark, then the difficulties are exacerbated by a cometary model for two separate reasons. Firstly, a comet is half ice and secondly, it impacts at higher velocities. Iridium concentrations could thus be reduced by a factor of 100. An asteroid would be preferable.

Kyte: There can be different sources of cometary material. There are long-period comets which go beyond Pluto, and there are short-period comets. Also, some Apollo asteroids, may be extinct comets. In order for the tidal disruption and accretion model to work, the object has to occupy a nearly circular orbit approximating that of the Earth. A low encounter velocity is required.

L. Alvarez: Such an object could be called an asteroid.

Kyte: The asteroid belt is generally believed to be the source of ordinary chondrites, while the volatile-rich comets may be a source of carbonaceous chondritic material.

L. Alvarez: What appeared to be a disagreement between our two groups is really not all that severe, now that we have had a chance to discuss these matters with Dr. Grieve.

Feldman: Aggarwal and Oberbeck (1974) discussed the effects of the Roche limit on a solid body. Break-up occurs for a body approaching either the Earth or the Sun only if the tensile strength of the body is less than of the order of 10^6 dynes cm^{-2}, which is comparable to that of glacial ice, assuming the incoming body is about 10 kilometers in diameter. Allowing for equal amounts of icy volatiles and iridium-bearing material in the cometary nucleus, its size would be somewhat larger than 10 kilometers. Break-up might then occur just outside the Earth's atmosphere or the Sun's photosphere. In the more attractive case of a comet breaking up as it passed the Sun the dispersed material might be accreted by the Earth over a number of its yearly orbits. This could yield a series of iridium anomalies. An asteroid approaching the Earth would not break up. The lowest estimate that I've seen for the tensile strength of asteroidal rock, cited by Aggarwal and Oberbeck, is of the order of 10^7 dynes cm^{-2}.

Kyte: Break up can occur at virtually any velocity. A very low encounter velocity would allow the Earth to capture a significant amount of material at one point in time, rather than sweeping it up on another pass 10,000 years later. In a low velocity encounter, break-up would be followed by an accretionary event, in which case objects in elliptical orbits, involved in a 3-body problem which includes the Moon, would probably be accreted over a period of several months.

K. Hsü: Two craters which are about 65 million years old occur in the southern part of the Soviet Union. The craters contain impact melts and breccia with Cretaceous fossils, over which were deposited marine sediments of earliest Tertiary or Danian age. The Kamensk crater is about 25 kilometers in diameter, and the Gusev crater to the northeast is 3 kilometers in diameter (Masailis 1976). Is anyone aware of additional information pertaining to these craters?

Grieve: Craters are being located at a rate of one or two per year in the Soviet Union. Another crater 60 kilometers in diameter, Kara, has been dated at 60 ± 5 million years (Masailis *et al.* 1980).

Hsü: Many such craters could form after a cometary breakup.

Grieve: A comet is a low density body and may not penetrate deeply into the target. The amount of material excavated would be proportionally less.

Feldman: The Tunguska event in 1908 is now identified as a fragment of the periodic comet Encke (Kresák 1978). Spherules found by Glass (1969) in the Tunguska dust show an anomalously high potassium-to-sodium ratio, but not as high as in those found by Dr. Smit. Was the ratio you found about a hundred to one?

Smit: Yes. In the level of the highest iridium concentration, at the very base of the clay layer at Caravaca, we found thousands of tiny spherules consisting of sanidine. They did not form authigenically on the sea floor, but must have cooled from temperatures of about 600 to 700 degrees centigrade. Only the occurrence in Caravaca is known, where they occur at a density of about 500 per cubic centimeter. We suggested they were derived from an iron meteorite, one specimen of which is known to contain large single crystals of potassium feldspar with a similar potassium-sodium ratio. I was not aware of the Tunguska comet fragment containing a high amount of potassium. If comets are not fractionated how can high potassium concentrations occur in them?

Halliday: Is it reasonable to think of this as a differentiation in the atmosphere, when the material became molten?

Kyte: Potassium is one of the more volatile elements in meteoritic material and would be likely to vapourize. It also might be likely to condense rather rapidly from a vapour.

Smit: We were surprised by the enormous amount of spherules in the clay layer at Caravaca, where they form a nearly continuous surface. Perhaps this is because the site was situated in a strewn field.

W. Alvarez: Were the Tunguska spherules found precisely at the site?

Halliday: They were distributed by the wind, and carried from the southeast to northwest of the site.

Kyte: Did the Tunguska spherules contain chondritic material?

Halliday: Analyses are only available for the major elements, not for the trace elements.

Kyte: We have found a horizon in a core from the Antarctic basin a little over two million years old. It has an iridium anomaly (Kyte *et al.* 1981) comparable to that at the Cretaceous-Tertiary boundary, and is also full of chondritic debris, apparently from an atmospheric explosion or oceanic impact of an object. The mineral chemistry and texture is similar to chondritic ablation spherules normally found in deep-sea sediments.

Halliday: Different kinds of spherules were found at Tunguska, including magnetite ones, silicate ones, and occasionally even a silicate one with a smaller magnetite one inside, all with an average diameter of 80 to 100 microns.

L. Alvarez: I am interested in hearing suggestions on what should be done to demonstrate more fully the validity of the asteroid impact hypothesis. Our resources are limited, and should be concentrated on the task of obtaining that kind of data which would be most useful in making a convincing case.

Kyte: I think there was a large accretionary event at the end of Cretaceous time. Several models on how this took place have been suggested, including an asteroid impact, tidal disruption and accretion of numerous small bodies, and accretion of cosmic dust clouds. Methods should be found to test these hypotheses. Concerning the asteroid impact model, one test would be that both meteoritic components and ejecta components should have been distributed on a world-wide scale. Does the ejecta component exist in all the sites that have been investigated and is it of relatively uniform composition? For example, if there was an oceanic impact, a large fraction of mantle material should occur in the ejecta and be correlated from location to location. If the meteoritic signal is diluted by only normal terrigenous sediments, then perhaps something like the interstellar dust cloud or the tidal-disruption model should be favoured.

McLean: We must entertain alternatives and not fix upon one model too quickly. The boundary clay, if it is a lag deposit resting on top of a Cretaceous unconformity, might account for the range truncations below it and the illusion of a catastrophe. A world-wide regression of the seas occurred at the end of the Cretaceous, which would have created unconformities in continental sections, and removed the terminal Cretaceous record there. Hiatuses are known to occur at the Cretaceous-Tertiary boundary in continental sequences. If the boundary occurred 65 million years ago it coincided with the onset of the Deccan Traps. The extinctions were most severe in this region of the world. It could also be postulated that acid volcanic gases were injected into the atmosphere and passed into shallow oceanic waters through gaseous exchange where calcite-producing organisms became extinct. In the marine realm a carbonate dissolution event is known to have coincided with the Cretaceous-Tertiary boundary. Loss of record could again create the illusion of simultaneous extinctions. The paleontological and stratigraphical record in the vicinity of the range truncations, and the length of time represented by the boundary clay are two questions which should be more carefully considered.

Smit: Dinoflagellates have a turnover rate comparable to that of planktic foraminifera. If range terminations of the planktic foraminifera occur, similar range terminations should be evident for dinoflagellates, which is not the case.

McLean: Dinoflagellates have organic walls and would continue to settle down. In fact they are quite concentrated in this boundary clay.

Thierstein: In general terms, the geochemical cycles of rare and noble elements should also be examined more carefully. How much of that material is dissolved in oceanic waters or stored in marine and terrestrial biomass? Another problem is identifying the mechanism that produced the significant extinctions of the calcareous marine phytoplankton. These organisms have high turnover rates and high regeneration rates. The only survivors with an adequate record prior to the Cretaceous-Tertiary bounday among the calcareous phytoplankton are *Braarudosphaera* and *Thoracosphaera*, and they are considered to be cysts. Dinoflagellates can encyst themselves, and survive for many months on the ocean floor at a depth of over 200 meters and then resurface and reproduce (Williams 1978). They can survive adverse environments in the surface. Could the asteroid impact model provide an effective mechanism to eliminate the phytoplankton, upon which many organisms higher in the food chain depend? Such a mechanism would be the screening out of light, although living calcareous phytoplankton is known to survive for months at light intensities of less than 1% of that available at the ocean surface (Blankley 1971). It is physically difficult to imagine a stratospheric dust cloud that would screen out 99% of the currently incident light for extended periods of time (Thierstein, in press). At the densities required, preliminary calculations suggest that it is impossible to keep enough material in the stratosphere for more than a few weeks. Atmospheric modelling would be useful. Another possibility which I have favoured, and which I still think is conceivable, would be a significant salinity change in oceanic surface waters. The model is unfortunately virtually untestable because of the very rapid exchange rates required between a freshened ocean basin and open ocean environments.

Hsü: Volcanic sources have recognizable signatures in the rare earth elements. Would it be possible to compare the rare earth elements of the boundary layer more systematically with the various materials from basalts?

Asaro: We have analyzed all of our samples for their rare earth abundances and have in addition many analyses of akalic, oceanic and tholiitic basalts made as part of a geochemical programme. The rare earth patterns in the acid-insoluble residues and whole-rock samples from the K-T boundary regions are different from the basalts.

W. Alvarez: Are the extinctions synchronous all over the world, which one would expect if they were the result of a sudden event like an impact? This would seem to be the case in the marine environment. There are two sections in Italy in which the extinction occurs in a particular magnetic polarity zone. In site D.S.D.P. 524 in the South Atlantic, the extinctions occur in the same polarity interval. A subsequent leg in the South Atlantic reveals the same relationship. The extinctions also occur in the same polarity interval at Caravaca. Problems remain in the two terrestrial sequences, containing dinosaurian remains, which have so far been studied. In the San Juan Basin section in northwestern New Mexico, which was studied by Butler, a well substantiated gap in the section occurs right at the boundary. A channelled unconformity with conglomerates seven meters thick is located at this level (Baltz *et al.* 1966). One cannot, therefore, be certain of the correlation of the polarity zones. The other section is in the Red Deer Valley, in Alberta and was studied by Lerbekmo, Evans and Baadsgaard (1979). In this section it would appear that the dinosaurs became extinct at the same time as did the marine microfauna, but to a paleomagnetist the reversals do not seem very clean. I have had many discussions with Butler, who carried out the work in the San Juan Basin, and we have come to the conclusion that with the data available from the land sequences the correlations remain uncertain. Terrestrial sequences should be sought in which good paleomegnetic stratigraphic studies can be done. A thick enough sequence is required so that a characteristic fingerprint of long and short, normal and reversed polarity zones can be obtained which will permit confident correlations.

Rucklidge: Background abundances of trace elements should also convincingly be established, so that similar lithological samples from different levels in the stratigraphic column can be compared. Pyrite from an Ordovician shale in Ontario contained three PPB of iridium in it, which is greater than the anomaly quoted in some of the boundary layers.

Russell: I have compiled data pertaining to genera of organisms that have been identified in strata which were deposited during the last 5 to 10 million years of the Cretaceous and the first 5 to 10 million years of the Tertiary (Table 1). The numbers represent a state of the art and certain serious sampling inhomogeneities. They should not be interpreted in the same sense as the numbers which were being considered earlier. They constitute the paleontological basis for recognizing an event in the fossil record which is quite interesting.

Hsü: The timing of the extinction should be defined as closely as possible. One of the best sections for paleomagnetic stratigraphy, which is not affected by facies changes or other complications, is the D.S.D.P. site 524 in the South Atlantic (Fig. 3). One of our major objectives was to measure the interval of time represented by the polarity intervals. The core passed through 200 meters of Paleocene strata and then 100 meters of Cretaceous sediments, all of which were undisturbed. Coring was nearly continuous. The extinction took place in the Anomaly 29 reversed interval, as in the Gubbio section. The correspondence between site 524 and the Gubbio section in other polarity intervals is good. The thickness of the polarity intervals in the sediments is correlated with the width of the sea-floor-spreading magnetic anomaly patterns in the South Atlantic so that age limits can be obtained for various polarity intervals. Our data confirm the estimates of Kent, and Anomaly 29 reversed interval lasted about 480 thousand years (Hsü *et al.*, in press).

Sedimentation rates for the four intervals identified were of order of 30 to 25 meters per million years, or about 3 centimeters per thousand years. Time resolutions of the equivalent of one centimeter's deposition could be approximated. Suggestions of an extraterrestrial event were reflected in the vertical carbon and oxygen isotope excursions. The stable isotopes of carbon are ^{13}C and ^{12}C and are usually expressed in delta ^{13}C in PPT. A shift of minus 3 parts per thousand occurred

TABLE 1. *NUMBER OF GENERA OF FOSSIL ORGANISMS CURRENTLY RECOGNIZED AS HAVING LIVED PRIOR TO AND FOLLOWING THE TERMINAL CRETACEOUS EXTINCTIONS.**

Fresh-water organisms			
charophytes (algae)	19	11	
cartilagenous fishes	4	2	
bony fishes	11	7	
amphibians	10	12	
reptiles	13	17	
	57	49	86%
Terrestrial organisms (including fresh-water organisms)			
charophytes (algae)	19	11	
higher plants	100	90	
snails	16	18	
bivalves	10	7	
cartilagenous fishes	4	2	
bony fishes	11	7	
amphibians	10	12	
reptiles	58	25	
mammals	24	(11) 30	
	252	202	80%
Floating marine microorganisms			
acritarchs	28	10	
coccoliths	43	4	
dinoflagellates	95	90	
diatoms	10	10	
silicoflagellates	4	3	
radiolarians	63	63	
foraminifers	18	3	
ostracods	79	40	
	340	223	66%
Bottom-dwelling marine organisms			
calcareous algae	47	47	
sponges	261	81	
foraminifers	95	93	
corals	87	31	
bryozoans	337	204	
brachiopods	28	22	
snails	300	135	
bivalves (with 52 gen. rudists)	381	129	
barnacles	32	24	
malacostracans	69	52	
sea lilies	16	16	
echinoids	150	69	
asteroids	36	35	
	1839	938	51%
Swimming marine organisms			
ammonites	34	0	
nautiloids	10	7	
belemnites	4	0	
cartilagenous fishes	14	10	
bony fishes	38	10	
reptiles	30	2	
	130	29	22%
Overall totals:	2561	1392	54%

* Modified from Russell (1977) after data in Bujak and Williams (1979), Carpenter (1979), Danilchenko (1978), Glezer (1977), Korde (1977A, 1977B), Kues *et al.* (1980), Lupton *et al.* (1980), Molnar (1978), Naidin (1979), Naylor (1978), Rasmussen (1979), Sohl (1960), Van Valen (1978), Wall and Galton (1979), Whetstone (1978) and Weishampel and Jensen (1979).

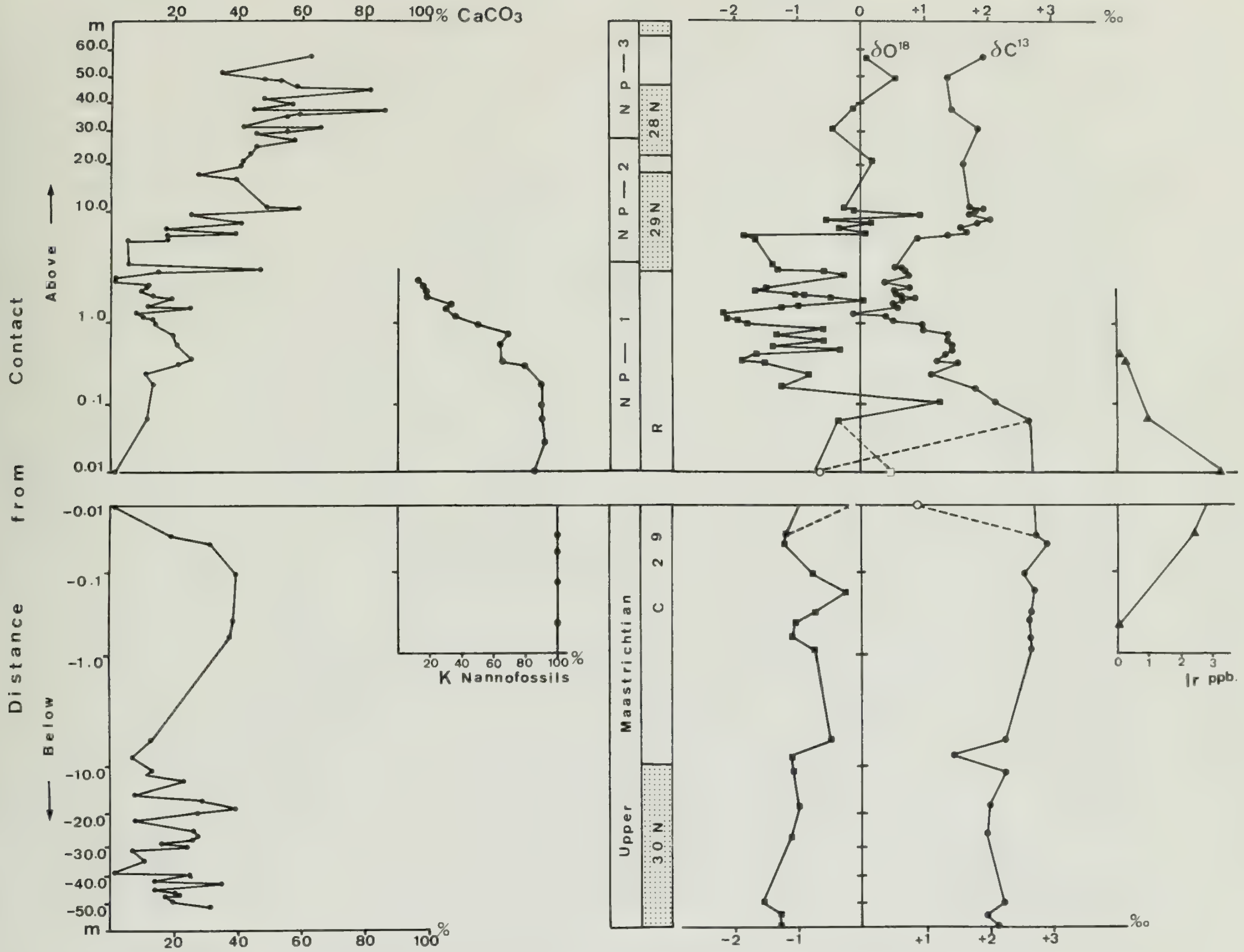

FIGURE 3. *The iridium-spike detected by Urs Krähenbühl indicated the deposition of extraterrestrial fallout several hundred years before the first appearance of the Tertiary "taxa" of nannofossils.*

Work done by Steve Percival and Katharina Perch-Nielsen showed the rapidly declining percentages of Cretaceous "taxa" of nannofossils in the earliest Tertiary sediments deposited during the first 30,000 years after the extraterrestrial event.

Work done by Q.X. He, Judy McKenzie, Hedy Oberhänsli, and Helmut Weissert indicated: 1) the rapid decrease of $CaCO_3$ in latest Cretaceous and earliest Tertiary sediments, signifying perhaps a drastic reduction of biomass and a rapid rise of CCD; 2) oxygen-isotope shift, signifying perhaps an initial cooling of about 5°C, followed by a 10°C rise in temperature; and 3) a systematic carbon-shift, signifying perhaps an increase of dissolved CO_2 in the oceans with an anomalously high proportion of ^{12}C atoms. Two data-points near the contact were based upon isotope analyses of samples containing less than 5% $CaCO_3$, which are not reliable.

The text of the work produced by this diagram is a manuscript submitted to Science, entitled Environmental and evolutionary consequences of terminal Cretaceous event, by K.J. Hsü and 19 co-authors.

across the boundary. This is a very large shift. If all of the biomass on land were placed in the ocean enough ^{12}C would be available to cause a shift of only minus 2 parts per thousand. It took place within a very short time, and certainly less than a few hundred thousand years. We are not certain if this shift took place in less than 300 years or less than 30 thousand years. Simultaneously, a shift occurred in the oxygen 18/16 ratio. Boersma *et al.* (1979) have found a shift in both benthic and planktic organisms, amounting to one to two parts per mille or a thermal warming of four to eight degrees centigrade. The thermal excursion might have had devastating effects on dinosaurs (Hsü 1980). Did it occur immediately or did it build up over a few thousands, or tens of thousands of years? The work by Dr. Alvarez and others indicates, to my satisfaction, that an extraterrestrial event took place and we need to know more about it. It is no longer useful, in my opinion, to question whether or not such an event happened. What stresses caused the biologic catastrophe? Perhaps this can be approached by examining the rate at which various environmental changes took place. A paleontologic boundary must first be recognized on the basis of frequently occurring nannofossil species. About 30 nannofossil species occur in Cretaceous sediments. The first appearance of four Tertiary species defines the base of the Tertiary. Many Cretaceous species are found up to a level which is equivalent to about 40 thousand years after the beginning of the Tertiary. They were either descendents of survivors of the holocaust, or dead organisms which were reworked by sedimentary processes or mixed up to this level through bioturbation. We do not know which alternative is correct. Below the boundary 100% of the species are Cretaceous; by definition there are no Tertiary species. During the first 40 thousand years of the Tertiary many of the microfossils belong to Cretaceous species. In fact the first Tertiary sample contains only 3% Tertiary forms.

The base of the iridium spike, with abundances of 2.4 to 3.3 PPB against a background of less than 0.1 PPB, occurs one centimeter below the paleontologic boundary. Note that the paleontologic boundary is arbitrary, and that the cometary impact may actually have occurred during the last few hundred years, or few thousand years of what we call Cretaceous time. Calcium carbonate concentrations reflect biologic productivity; during the Cretaceous time the $CaCO_3$ content of sediments was about 30%, it then dropped to 2 or 3% following a mass mortality after the cometary impact, and returned to about 30% approximately 100 thousand years later. Again, the decline in the $CaCO_3$ content of the sediments occurred below the earlier paleontological indicators of Tertiary time. A carbon isotope excursion of minus 3 parts per thousand is found and it is about as large as any documented in the extinction interval. The delta ^{13}C declined logarithmically, to a minimum value after 30,000 years and slowly returned to the previous level after about 100 thousand years. There was also an excursion of paleotemperatures as indicated by the oxygen isotope data. However, the trend is more erratic. A decrease of 3-4°C coincided in timing with the iridium spike, followed by an increase of 6-8°C during the next 30,000 years. Conditions then gradually returned to normal as carbon-isotope values returned to normal (Hsü *et al.*, ms.).

We are now considering the possibility that there are two phases in this catastrophe. The first phase was a mass mortality, the biomass in the oceans was destroyed to a great extent. The second phase of mass extinction resulted from environmental consequences of the initial mass mortality. If the oceanic biomass were drastically reduced, carbon dioxide utilization in photosynthesis would also decline sharply. The oceans would become more acid, and a carbonate dissolution event would occur. What brought about the carbon isotope excursion? Rivers deliver carbon atoms to the ocean in the form of dissolved carbonate and dissolved organic carbon, representing an influx of isotopically light carbon. How much time would be required for the observed carbon isotope excursion to occur if the isotopically light carbon were not being deposited in oceanic sediments through interactions with oceanic biota? Various models suggest that this change can be produced in 40 thousand years if an average of one-third of the oceanic biomass were destroyed. From the point of view of step functions, initially on the order of 90% of the biomass could have been eliminated, and near the end the reduction may have been only by 10%, producing the logarithmic change in ^{13}C. The modelling also suggests an increase in CO_2 content of the atmosphere by about 5 to 10 times. The cooling trend, if real, could have resulted from the insulation of sunlight by ejecta from an impact event. The subsequent warming could not be associated with atmospheric heating during the fall of the meteor, because the warming required a process operative for a longer term. We are thus favouring the CO_2 greenhouse model for the warming. Are the data derived from this section exceptional or are they in accordance with a general rule? I suspect they are typical.

Thierstein: The replacement of the Cretaceous by the Tertiary flora and fauna usually occurs over an interval of 75 centimeters to 3 meters. Could that indeed be a transition? One way to approach this question is to examine the evolutionary patterns within the surviving Cretaceous assemblage in basal Tertiary sediments. If there was a gradual deterioration of Cretaceous taxa, abundance flunctuations should be apparent. In the three sections I have examined there is no drastic change in the relative abundances of the Cretaceous taxa above the boundary. This leads me to believe that the Cretaceous fossils are indeed reworked by lateral transport and benthic mixing.

Hsü: In either case, the transition represents about 40 thousand years, at most. We are not discussing normal evolutionary changes. Even if the nannofossils are not reworked, we are "violently agreeing" in the sense that the time scales involve between a few hundred and a few tens of thousands of years. Dr. Perch-Nielsen, one of our co-workers, has studied nannofossils in 10 or 12 European sections and has identified no large paleontologic hiatus. I do think that a terminal Cretaceous catastrophic change in the pelagic environment was real, and the change occurred synchronously over a wide part of the ocean areas. Perhaps the extinction of the dinosaurs occurred a few tens of thousands of years later, because of other environmental effects. A maximum of temperature was attained near Anomaly 29 positive in our core.

Clube: Do you exclude extraterrestrial input of ^{13}C in your modelling, because this could be a very powerful test of meteoritic versus cometary impact?

Hsü: After we defined the trend that I have described, we analyzed additional samples deposited at a time thousands of years after the catastrophic event. We found a ^{13}C anomaly of minus 2.5 to 3 parts per mille. The three points for this anomaly are based on five or six replicate analyses of surface-dwelling organisms, after benthonic organisms were removed from the samples. This could be interpreted as evidence of extraterrestrial input into surface oceanic waters. A very small amount could change the composition of surface waters (Hsü 1980). Unfortunately this is the only site where this has been identified so far. Perhaps it will be found in the Tunisian section.*

Feldman: The interstellar value of the ratio of ^{12}C to ^{13}C is 67 ± 10 (Penzias 1980). What is it on Earth?

Hsü: On Earth 1.11% of the carbon atoms are ^{13}C and 98.89% are ^{12}C. It is possible that carbon is fractionated in comets because ^{12}C may preferentially enter the ice phase. The delta ^{13}C of a comet with all of its water and dry ice might be of the order of minus 15 parts per mille. A body of 10^{18} gm could produce a signal in the surface layers of the ocean, but not within the ocean as a whole.

McLean: The ^{13}C value of volcanic CO_2 and reduced carbon in igneous rocks is quite low, being of the order of minus 7 to minus 30 (Hoefs 1973, Javoy and Pineau 1978). Volcanic CO_2 flooding into the oceans over a long period of time could account for a portion of this drop in ^{13}C values.

Thierstein: An additional biologic complication exists. Individual species of nannoplankton fractionate carbon and oxygen in different proportions. The range is about three per mille in living coccolithophorids (Dudley and Goodney 1979), and the shift across the Cretaceous-Tertiary boundary could in part be due to vital effects caused by the change in the assemblages. A hint of this is provided by the isotopic shift in the benthic foraminifera at D.S.D.P. site 356, which actually become heavier rather than lighter in carbon across the boundary. Several mechanisms could be involved in producing the shifts.

Russell: The swing in carbon isotope balance could have been more profound in the atmosphere, although no stable carbon isotope measurements have been taken in terrestrial boundary sequences. Perhaps this is an area worth looking into. In terrestrial environments of deposition in Montana, Clemens and Archibald (1980) have noted, as I have too, a separation of several meters between the highest articulated

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* A similar ^{13}C excursion was subsequently found in Tunisia and Germany across the Cretaceous-Tertiary boundary (K. Hsü).

dinosaur bones, and the horizon of the palynofloral change usually associated with the Cretaceous-Tertiary boundary. In terms of terrestrial sedimentation rates, this might represent tens of thousands of years.

Jarzen: I have studied two sections in western Canada, where sampling at close intervals has been carried out from the last *Triceratops* remains to the number 1 coal, or Z coal, or "boundary" coal, and then to several meters above the coal. There does not appear to be a drastic reduction or change in the palynofloral content. My efforts have been primarily concerned with the angiosperm component, but the spore and gymnosperm components have been examined as well. The changes that have been observed involve two major groups of pollen forms; one typical of the western interior of North America and Siberia called *Aquilapollenites* and the other typical of eastern North America and Europe called *Normapolles*. These two groups do in fact suffer major extinctions. The remaining angiosperms seem to pass through the transition relatively unaffected. From 10 to 40% of the palynomorph taxa become extinct, depending on the palynologist, the section sampled and perhaps the sampling interval. The change observed in sampling at close intervals may be less than the change observed in sampling at widely-spaced intervals. In both of the sections from Saskatchewan, and in a few other sections from this province, in the last two or three meters of the Cretaceous the preservational quality of the palynomorphs decreases so drastically as to render the identification of some of the palynomorphs impossible. Twenty centimeters below the boundary coal the samples become for all practical purposes barren. The coal itself is often barren, but above that coal an abundant palynoflora is again preserved with many of the same taxa present that were observed below. Why does this drop in preservational quality occur?

McLean: One thing that seems to be causing a lot of confusion is the definition of the Cretaceous-Tertiary boundary in the western interior. Brown (1962) chose the first persistent coal above the highest occurrence of dinosaur bones. That reflects a tectonic change. Leffingwell (1971) identified the Cretaceous-Tertiary contact with a pollen change. This is nearly 20 meters below Brown's boundary coal, and is the same pollen break that Lerbekmo *et al.* (1979) believe is synchronous over Montana, Wyoming and Colorado. The pollen break in Colorado is about 35 meters below volcanic strata that have been dated at about 66 million years. A hiatus is generally present in sequences around the western intermontane basins, and the most continuous sedimentation occurred in the inner parts of the basins that are usually only accessible by the drill bit. Here, there seems to be a sort of gradation of palynofloral changes across the contact.

Hsü: Hickey (in Kerr 1980) has stated that there was a great floral change in northern temperate or arctic regions, but a minor change in lower, more tropical latitudes. What is your impression?

Jarzen: The figures cited by Dr. Hickey imply that perhaps 70 to 80% of the species in western North America became extinct. I do not believe that. The last time I spoke with Dr. Tschudy he felt that the extinctions were less significant than that. The citation is either a misprint or we were misquoted.

Hsü: He quoted you with reference to the extinctions in the tropics.

Jarzen: I don't know about the extent of the extinctions in the tropics.

McLean: Dr. Hickey accepts Leffingwell's (1971) pollen break as the Cretaceous-Tertiary boundary. Leffingwell in turn suspects that hiatus control or missing section is the cause of this pollen break and is due to tectonism.

Russell: In deltaic sections in Montana and North Dakota, Moore (1976) noted some minor scouring near the transition into Tertiary strata. With a possibility of wave activity and flooding of marginal lands as a consequence of an oceanic impact event, perhaps this should be examined more carefully. However, a hiatus has not been detected in the lithostratigraphic sequence in the eastern Fort Peck Reservoir region, where there is an impressive continuum of change in the highest levels of the Hell Creek Formation. The badlands there contain large-scale, cross-bedded riverine sands within a matrix of finer overbank and floodplain deposits. The sands contain debris of different kinds of dinosaurs and a relatively large quantity of bones of smaller vertebrates. The highest level of occurrence of *Triceratops* coincides with a change in the quality of deposition, the visual impression of which is very powerful. Patterns of sedimentation gradually become more laminar, the scale of the cross-bedding decreases and within about five meters the sequence has changed into the laminated sandstones and coals typical of basal Tertiary deposits. There is a feeling of a massive but gradual shifting of gears from Cretaceous to Tertiary depositional patterns.

McLean: These changes may be the result of tectonic factors. It may be well to suspect the possibility of a hiatus until proven otherwise, because a hiatus occurs in so many places. Which datum in terrestrial sediments - the Z coal or the palynofloral break - corresponds to the marine Cretaceous-Tertiary boundary? There may be a significant time differential between the various reference horizons that radiometric dating could resolve.

Hsü: Radiometric dating of rocks of this age are only accurate to within plus or minus one million years. Paleomagnetic resolution is better.

McLean: Lerbekmo and his collaborators (1979), using magnetostratigraphic evidence, view the dinoaurian and marine extinctions as synchronous, while Butler and his colleagues (1977, 1978, 1979) using the same kind of evidence, interpret the dinosaurian extinctions as occurring much later than the marine extinctions.

Hsü: At most, the discrepancy could not exceed one million years. Dating techniques, whether radiometric or magnetostratigraphic, have their limitations. For example, radiometrically datable ash beds have not been located at the boundary in marine sections. Incidentally, where does the iridium anomaly occur in the Montana section?

Asaro: It was on the platinum engagement ring of the person who prepared our samples. We had found an 11-peak iridium anomaly in Montana and became concerned that iridium was so easy to detect. The iridium was found on the aluminum foil used to wrap the samples, and correlated with the order in which the samples were wrapped. The person who wrapped our samples had platinum wedding and engagement rings. We measured the platinum to iridium to gold ratio on the aluminum foils that touched her rings and found they conformed, within experimental counting errors, with the platinum to gold to iridium abundances in the contamination. I must add that we were overjoyed to hear that the Los Alamos group had discovered a continental iridium anomaly in New Mexico. None of the measurements that we have published were affected. Those of you who associate at one time or another with people who are married may be interested to know that if a platinum wedding ring loses 10 percent of its mass in 30 years, the average erosion per minute is about two orders of magnitude higher than our sensitivity for measuring iridium. We re-examined our previously detected gold anomalies and discovered these could be correlated 100% with the gold wedding rings worn by another person.

W. Alvarez: Returning to the paleobotanial record, how does one interpret the conclusion of certin workers that 90% of the palynofloral taxa become extinct in the *Aquilapollenites* province, but there was no change in the flora at the end of the Cretaceous?

Jarzen: *Aquilapollenites* represents perhaps 10% of the total flora in the *Aquilapollenites* province (Oltz 1969). Some 90% of *Aquilapollenites* species are no longer in the Tertiary. One or two species are found in Paleocene strata and some have recorded the genus from the Eocene, so the form did not become extinct. The same is true of the *Normapolles* lineage or group in eastern North America. More than 80 or 90% of the taxa became extinct, but studies by Tschudy (1975) indicate that several Cretaceous genera continue through the boundary and occur as late as the Eocene in North America and Europe. There were normal evolutionary changes through this horizon. If one looks at numbers and at just *Aquilapollenites*, a good case could be made for mass extinctions. If one looks at the entire flora, including angiosperms, gymnosperms, sporophytes, freshwater plants, and dinoflagellates, the extinctions do not appear so great. In Saskatchewan with a sampling interval of 5 to 15 cm, many of the Cretaceous forms seem to survive (Jarzen 1977A). The sediments cannot have been reworked, because one of the forms that almost always occurs in all the samples is a freshwater fern called *Azolla*. A massula or body in *Azolla* encloses the spores and this very delicate structure would be broken by reworking. A degradation of the preservational quality of the pollen, for reasons unknown, does occur, but not mass extinctions.

McLean: Would you comment on the abundance of *Aquilapollenites* before the Cretaceous-Tertiary boundary.

Jarzen: *Aquilapollenites* probably reaches a peak in species diversity during Campanian time, then begins a decline which stabilizes through the lower and middle Maastrichtian, to show another decline at the end of the Maastrichtian.

Hsü: What is the ecological significance of the two groups of plants which did suffer a higher proportion of extinctions?

Jarzen: *Aquilapollenites* was related to the families Santalaceae and Loranthaceae, which are very common today in the tropics where they are parasitic on other angiosperms (Jarzen 1977B). The *Normapolles* group contains lines of many botanical affinities. They are loosely united into a morphologic group because they possess a particular pore structure.

Dilcher: We are speaking about a group, the angiosperms or flowering plants, which have their most probable origin in the lower Cretaceous. They became progressively more important in the Cretaceous and their modernization occurred in the Tertiary. The evolutionary theme is one of continuity, unlike in the case of the dinosaurs. The major groups continue. Species level studies, such as those of Krassilov (1978) on gymnosperms, show evidence of extinctions at the Cretaceous-Tertiary boundary. One group, the cycadeoides, became extinct earlier. The flowering plants carry across time boundaries and there seem to be no breaks in their record. The change that is present on a species level could be a manifestation of the regression of the epicontinental sea in mid-North America, and a mixing of the components of previously distinct floral provinces to the east and west. As an aside, I remember that paleobotanists once denied that fossil plants showed any evidence of continental drift. When plate tectonics became geophysically understandable a few years later, the same paleobotanists demonstrated how paleobotanical evidence supported plate tectonic theory. Are we in a similar situation, where if a good mechanism were provided, data could be found to support it? We can now explain the paleobotanic evidence in various ways, but there is no evidence that demands a catastrophe as an explanation.

Hsü: Why should the northern temperate plants have suffered more than those in the tropics?

Dilcher: Hickey has suggested, in an unpublished manuscript, that a global cooling would affect boreal regions before it would low latitudes.

Feldman: An atmospheric CO_2 increase by a factor of five has been modelled by Manabe and Wetherald (1980), who predict a temperature increase of about 3 to 12°C. This would be accompanied by a decline in rainfall in the 35° to 40° northern latitude zone and the melting of the Arctic but not the Antartic ice cap.

Hsü: There were no ice caps in existence at the end of the Cretaceous. Because the tropics are already saturated with respect to the greenhouse effect, a CO_2 increase would produce a warming in high latitudes but not in the tropics. Hickey's observation is in conformity with this prediction. Incidentally, I would like to point out that the overall Cenozoic cooling trend should not be confused with the thermal perturbations across the Cretaceous-Tertiary boundary.

Dilcher: The structure and size of leaves generally reflect the climate under which the trees grow. During the middle Cretaceous, leaf sizes are what would be expected in a warm deciduous forest. There seems to be a cooling in the Cretaceous, a warming again in the Tertiary until the middle to late Eocene, and then a cooling. Some have suggested that mid-Cretaceous angiosperms mark the advent of deciduousness of flowering plants that had already been in existence for several millions of years. They interpret the leaf record as evidence of the deciduous habit.

Russell: Returning to the marine record, could we hear a résumé of the high resolution stratigraphy at the Caravaca section in southern Spain?

Smit: During the last five years we have attempted to integrate all the data on lithology, paleontology, trace elements, and stable isotopes in a section which is very well constrained in terms of time (Fig. 4, see also Smit 1981). The last planktic foraminiferal biozone (the *Abathomphalus mayaroensis* Zone) of the Cretaceous is about 110 meters thick and represents one to two million years. Sedimentation rates of up to 11 centimeters per thousand years are thus indicated for a Cretaceous marl which is composed essentially of coccoliths and planktic foraminifera, and contains only 1 to 2% material of benthic foraminifera and ostrocods.

The marls contain a clastic component of about 20% detrital clay, and planktic or benthic foraminifera can easily be removed and studied three-dimensionally under a scanning electron microscope. Counts of 200 specimens each were made through the

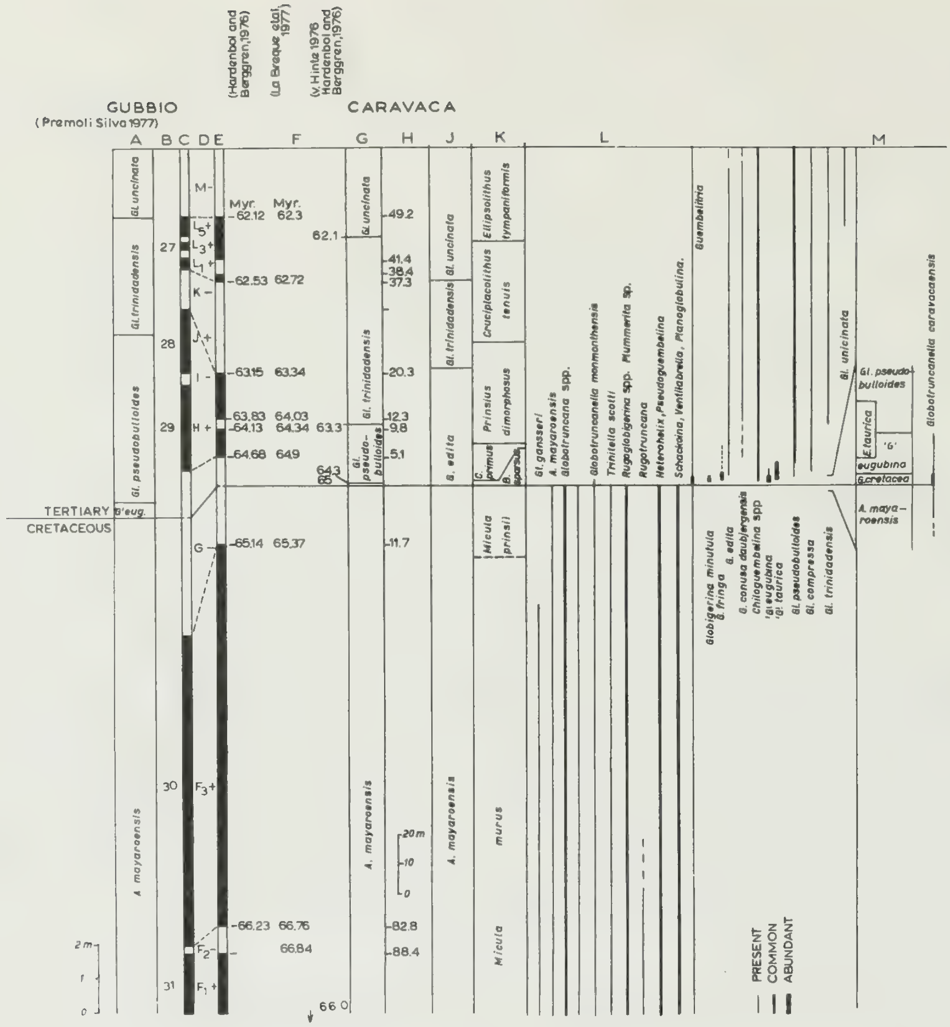

FIGURE 4. *Established magneto- and biozonations of the Barranco del Gredero section, compared with bio- and magnetozonation of Gubbio (Alvarez et al., 1977).*

The stratigraphic range of important planktic species is indicated.

Linear scale is used for the geomagnetic polarity columns of Gubbio and Caravaca. Base anomaly F_1+ and the top of anomaly L_5+ are placed at the same level.

A) Biozonation of the Gubbio section after Premoli Silva (1977).
B) Sea floor geomagnetic anomaly.
C) Reversal pattern of the Gubbio section (Alvarez et al. 1977).
D) Gubbio magnetozones (ibid.).
E) Reversal pattern of the Gredero section (G. Brunsmann, pers. comm.).
F) Numerical ages in millions of years.
G) Biozonation of the Gredero section.
H) Distance in meters from the K-T boundary.
J) Biozonation after v. Hillebrandt (1974).
K) Nannofossil zonation (Romein 1979).
L) Ranges of selected planktic foraminifera.
M) Biozonation in detail around the K-T boundary.

section and virtually no change was noted until the last millimeter of the Cretaceous at the top of the zone. Only one species, *Globotruncana gansseri*, disappeared earlier from the samples, and it was probably a rare species in the remainder of the section. Then, of the 55 species of planktic foraminifera, 50 disappear within this topmost millimeter. Only those species which are very rare in the smaller fractions of the Cretaceous samples are relatively abundant in the boundary clay layer. If it were a question of reworking only smaller species into the boundary clay, one would expect to see relative abundances similar to those in the smaller Cretaceous fraction, which is definitely not the case. On top of the 10 cm clay layer occur the first Paleocene species. None of these five to seven species are in fact represented in the smaller fraction of the upper Cretaceous biota. The lowest 40 cm of the Paleocene marl is facially identical to the Cretaceous marl, and also yields a planktic-benthic ratio of about 0.9 - 0.98 as well as a comparable clay content of about 18.5%. Facies and lithology are thus exactly the same below and above the boundary clay. The oldest or "*Globigerina*" *eugubina* biozone of the Tertiary is 40 cm thick. Coccolith high-resolution biostratigraphy is less precise because of the possibility of reworking. Seventy meters for the first four Paleocene biozones versus 110 meters for the latest Cretaceous biozone probably represents a significant drop in sedimentation rate.

The normal polarity interval 30 was identified, and 16.5 meters of Anomaly 29 negative which extends 11.5 meters into the top of the Cretaceous and 5 m into the Paleocene, well into the *Globigerina pseudobulloides* zone. Thus about 11.5 and 5 meters were deposited in about 480 thousand years, or the duration of 29 negative, as was already pointed out by Dr. Hsü. These 16.5 m, yielding sedimentation rates extrapolated linearly according to the durations of the magnetostratigraphic and biostratigraphic subdivisions, already account for about 500 thousand years. There is thus no evidence of a hiatus in deposition. All of the *Globotruncana* and associated forams become extinct at a very sharply defined level. In the boundary clay and in the *eugubina* zone only one species (*Guembelitria* aff. *cretacea*) remains from the Cretaceous assemblage, a species which occurs today in the Persian Gulf and which was probably living during uppermost Jurrasic time. It is a very hardy, triserial species and as far as I know is the only planktonic foraminifer to survive the extinction interval. This species, which has an abundance of less than one per mille in the upper Cretaceous, begins to bloom in the upper part of the boundary clay layer and reaches its peak at the base of the true Paleocene. This peak is followed by a bloom of the earliest *eugubina*, then the bloom of another planktic species and so on. These nearly monospecific blooms of several species, one after another, occur over an interval of about 15 thousand years, or the estimated duration of the *eugubina* zone. The first "stable" *Globigerina* species, *G. pseudobulloides*, then appears. To recapitulate, a sudden extinction event is followed by a clay layer in which some survivors have been preserved. If all of the clay is detrital, an interval of 5 thousand to 10 thousand years is indicated. Then, a rapid succession of new opportunistic species occurs, which are very small and thin shelled. They are succeeded by a stable fauna which is more or less still living today.

The sediments were analyzed chemically to detect trace element changes (Fig. 5). The upper Cretaceous pattern remains stable to a point 30 millimeters below the clay layer. Nickel, chromium, antimony, selenium, arsenic, cobalt, iridium and osmium abundances suddenly increase at the base of the clay layer, coinciding with a significant decrease of all of the rare earth elements. This peak occurs in the basal millimeter of the 10 centimeter boundary clay layer. Most of these elements decline within a half centimeter to normal Paleocene levels. The iridium peak is very sharp but does not decline so abruptly upwards. Perhaps the trail of the iridium is due to secondary effects and the deposition of reworked material in the upper part of the clay. The rare earth elements correlate well with the insoluble residue content throughout the entire section. Only in the basal millimeters of the clay can a significant decrease of the rare earth elements be detected. We cannot account for the arsenic anomaly, which may be associated with peculiar environmental conditions analogous to those promoting the formation of pyrite. Perhaps 900 PPM of arsenic would be sufficient to constitute a biologic hazard. All of these data are based on whole rock analyses, recalculated on a carbonate free base, without chemical purification. Carbon 12/13 and oxygen 18/16 isotope ratios were drawn from coccolith platelets, with the benthic debris removed. Cretaceous values prevail to a level one half centimeter above the iridium peak, where a shift occurs toward the lighter isotopes both of carbon and oxygen. A return to former values occurs in the remainder of the clay layer. The total oxygen excursion is on the order of 2 parts per mille, representing an increase of about $8^{\circ}C$.

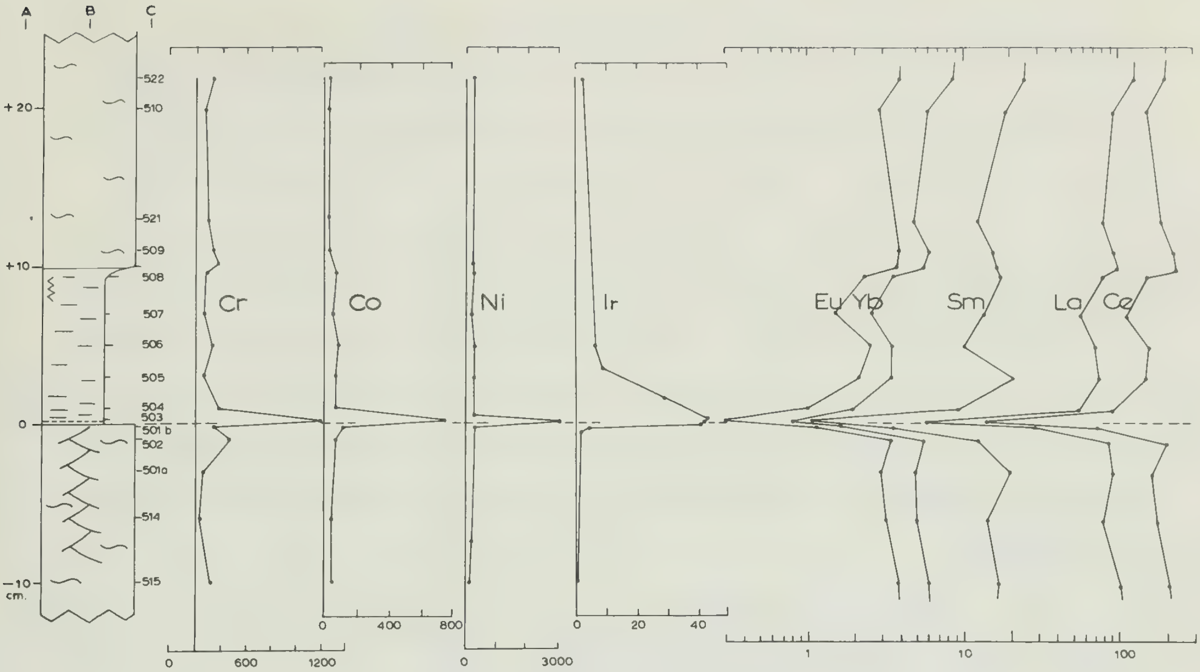

FIGURE 5. *Distribution of 'meteoritic' elements Cr, Co, Ni and Ir, and the REE across the K-T boundary in the Barranco del Gredero section. All plotted on a carbonate free base, the REE on a logarithmic scale. A = distance from K-T boundary in cm, B = lithology, C = sample number,* ≻ *= well-preserved* Zoophycos *burrows,* ≷ *= tectonic distortion (slicken sides).*

Sanidine spherules measuring 0.2 - 0.9 mm are found only in the basal millimeter of the clay layer, associated with the iridium anomaly, and were probably rained in suddenly. They contain inclusions of nickel and chromium which have abundances of about 10,000 PPM, and thus metallic meteoritic particles may also be present within them.

McLean: If *Globigerina eugubina* first occurs at the top of the clay layer, why haven't you drawn the Cretaceous-Tertiary boundary at this level?

Smit: This is a matter of paleontological convention. Strictly speaking, a boundary should be drawn at the first occurrence of new species, so the Tertiary should begin at the top of the boundary clay. However, the Cretaceous ends at the base of the boundary clay.

Russell: We have explored some of the paleontological attributes of one mass extinction. Several other, comparable extinctions are known in the stratigraphic record, and the timing of those events has been a matter for some speculation.

Tucker: When one considers possible mechanisms for mass extinctions, whether they be crater impacts or supernovas, the probability of the occurrence of a mechanism must correspond to the frequency of extinctions in the record. We noted the occurrence of extinctions beginning 542 million years ago, another at 500, another at 435, another at 355, another at 230 and the one we have been discussing at 65 million years ago. The interval between the extinctions is increasing regularly, commensurate with a factor of 21 million years. The elapsed time between the oldest and subsequent extinctions are two times, five times, nine times, 15 times and 23 times 21 million years, within about ± 10%. The interval between one extinction and another plotted as a function of time describes a regular linear curve with the interval between extinctions increasing proportional to elapsed time. This tends to indicate that there is some overlying regularity. The Sun moves around the galaxy and encounters close spiral density waves about every 100 million years. It also bobs up and down out of the galactic plane with a period of between 40 and 80 million years. These time-scales do not resemble the intervals between the extinctions. Neither does the pattern resemble that of random occurrences of supernova outbursts, or random impacts from cometary or Apollo object populations. If the time between extinctions is converted into distances between extinction-producing encounters, the pattern is reminiscent of the distance between planets in the Solar System. The solar nebula must have consisted of rings that were increasingly separated as distance from the Sun increased. Dust and gas, unlike planets, will spiral in under the Porting-Robinson effect. The passage of these rings across the orbit of the Earth might then occur with a regularity resembling the occurrence of the extinctions. The Earth would make a few thousand orbits through the ring during its passage, and the time-scale for an extinction could not be less than a few thousand years. Within any astrophysical system, clouds do tend to break up into rings. The material could have been left over from the primordial nebula or acquired through a passage of the Solar System through an interstellar cloud. The latter is not a very probable event, but could be expected to happen every 300 to 500 million years. During passage through such a cloud, material cannot fall onto the Sun if it has any angular momentum. It must spiral down into a disc and then the disc can accrete onto the Sun in the plane of the Sun's rotation, which coincides with the orbital planes of the planets. In the case of either primary or captured material, the cloud would be located at about a tenth of a light year from the Sun. This is about the area from which comets are derived. The relationship between extinctions and time would imply that the next passage will not be for another 140 million years or so. In this model the clouds are diffuse, the densities just enough to yield an optical thickness of about one. The matter is probably accreted on Earth in the form of interstellar grains, which would be heated to perhaps 10 or 20 thousand degrees and vapourized.

L. Alveraz: From a statistical point of view these numbers, 65, 230, 355, 435, 500 and 542 million years, plot on a straight line, with no indication of a quadratic distribution. There are insufficient numbers to demonstrate non-linearity.

Tucker: The interval between extinctions seems to be increasing linearly with time. This indicates an underlying regularity. Unfortunately, the statistics are based on very few numbers.

L. Alvarez: In the case of an asteroid, a collision could be expected, on the average, every 100 million years with an object about 10 kilometers in diameter. This can be deduced either from the cratering record on the Earth or Moon, on the one hand, or from the number of Apollo asteroids in space. This is consistent with the numbers that you have presented.

McLean: Approximately 10 thousand years ago, there were great mammalian extinctions in the Northern Hemisphere, and 100 million years ago, coinciding with massive submarine volcanism, there was a truncation of ranges of foraminifera in the oceans. The so-called Permo-Triassic extinctions may be an illusion because of gaps in the sections simply due to the destruction of record.

Russell: The causes of one mass extinction may not be the same as those of another. The dates to which Dr. Tucker referred are based on the assumption, which may or may not be true, that the Cretaceous-Tertiary, Permo-Triassic, late Devonian, terminal Ordovician, basal Ordovician, and end of lower Cambrian extinctions are the result of the same kind of processes. Colleagues who are familiar with Paleozoic extinctions (T.E. Bolton, W.H. Fritz, R. Ludvigsen, personal communications) inform me that the extinctions cited appear to be similar in scope and effect.

Dilcher: How would one discriminate between the effects of clouds versus those of asteroids?

L. Alvarez: Dr. Smit has noted that the extinctions occurred within a one millimeter layer that took about 50 years to be deposited. No one has suggested that enough material could be accreted from a cloud in 50 years.

Feldman: That is correct. The densest kinds of clouds in the galaxy are similar to Lynds 134N, with central-core number densities of about 10^5 hydrogen molecules cm^{-3}. The mean free path of gas in these clouds is larger than the accretion radius parameter defined by Bondi and Hoyle (1944). The problem then is a classic one of accretion onto an impact parameter of the Earth's radius. The matter density in clouds of this kind is about 100 parts of gas to one part dust. It is possible to accrete 10 ngm cm^{-2} of iridium from dust grains with chondritic elemental abundances in passing through such a dense interstellar cloud which is approximately one parsec (3.26 light-years) in diameter. The transit time would be about 100 thousand to perhaps 8 thousand years, corresponding to a random relative velocity of ~ 10 km s^{-1} in the former case, and to an overtaking relative velocity (with respect to a spiral arm) of ~ 125 km s^{-1} in the latter case. A density enhancement of the kind suggested by Tucker, resulting from the Sun's increased accretion of material, could occur and contribute to the production of the iridium enhancements observed. However, a substantial interval of time would still be required, which seems incompatible with the stratigraphic data presented by Smit.

A major part of the work I do is on the radio astronomy of interstellar molecules in these dark clouds, the principal constituents of which are CO, HCN, its isomer HNC, and other highly toxic molecular species. The injurious characteristics of these gases are well known. On applying the mass fractions for these gases with respect to molecular hydrogen, and scaling the hydrogen to what would have to be present as dust to produce the iridium enhancement, cyanide and iso-cyanide concentrations in the atmosphere would be about four orders of magnitude lower than those required for a toxic dose. Concentrations of CO would be too low by two orders of magnitude. The possibility of dense interstellar clouds poisoning the biosphere thus seems very slight. I might be inclined to change my mind if the time scale for the enhancements begins to approach a thousand years.

Dilcher: If a catastrophic model is considered, then steady-state conditions no longer apply, at least during the event. How were the estimates of five thousand years and 50 years, with respect to Cretaceous-Tertiary boundary events, calculated?

Smit: The boundary clay is in my opinion detrital, and I assumed a more or less constant influx of hemipelagic clay in this interval. The lowest (Paleocene) sedimentation rate was applied to the clay layer, yielding a maximum figure of 15 thousand years and a more reasonable figure of five to six thousand years. If the hemipelagic component is deposited at the normal Cretaceous rate, only two thousand to two thousand five hundred years would be required. The major accretion event is preserved in one millimeter of the whole 10-centimeter-thick clay layer. Any increase in sedimentation of the detrital component will only reduce the time for deposition of the entire clay layer. Another approach has been made through paleomagnetic evidence. The impact event coincides with only the base of the clay layer in Caravaca and in Denmark. In the Danish clay, after removing the pyrite, the remaining clay fraction in the basal millimeter contained the most depleted rare earth signals. I think my figure of 50 years, or at the most 250 years, describes the maximum duration of the event accurately.

Dilcher: This record is from marine deposits under three thousand meters of ocean water?

Smit: They were fully marine, fully pelagic environments. Spain at that time was well out in the Atlantic Ocean, not within a restricted Tethyan environment.

Hsü: The Spanish deposits are on top of a continental crust; considerations of isostatsy suggest that the sea was not thousands of meters deep there. The Danish section is usually considered to have accumulated at water depths of 500 meters or less.

Smit: The enhancements so far have been identified only in pelagic sections with the exception of the Danish section and a continental section in New Mexico.

McLean: According to some of the definitions of the top of the Cretaceous and of the bottom of the Tertiary, or base of the *Globigerina eugubina* zone, the boundary clay does not fit into any available time era.

Hsü: In the case I was describing, the first appearance of Tertiary nannofossils in that section was below the last appearance of the Cretaceous forms. Thus the "top" of the Cretaceous was above the "bottom" of the Tertiary. But criteria must be selected, and in this instance the bottom of Tertiary has been defined conventionally by a first appearance of Tertiary taxa. Implicitly, this also defines the top of the Cretaceous.

Smit: Was the clay layer missing in your section?

Hsü: The clay layer was present, but from our definition the clay is the top of the Cretaceous. Where to draw the boundary is an arbitrary convention. In fact, according to our definition, the event indicated by the iridium spike occurred during the last few hundred years of the Cretaceous. Perhaps the iridium anomaly will prove to be the best definition of the stratigraphic boundary.

Feldman: Can you explain why the iridium "tails off" through that clay, and the other enhancements do not?

Smit: It is most likely a question of the nobility of the metal iridium. Iridium was the only platinum metal we analyzed through the section. There were two effects, a direct influx from the dust clouds and a secondary influx from the continents where iridium was also deposited. In integrating whole peaks, two effects are combined. Like Dr. Hsü, I prefer to plot only the maximum concentration in any particular section. The clay layer in Caravaca is laminated and there is no evidence of bioturbational mixing. In the transport from the continents through the oceans, oceanic residence times for nickel and cobalt are probably longer than for iridium.

Hsü: Bioturbation of bottom sediments suddenly ceases at the horizon of mass-mortality at D.S.D.P. site 524. The laminated sediments above the boundary clay suggest that the biomass of benthic forams was inadequate to disturb the sediments to the same degree that it had previously.

Smit: Bioturbation does not turn over clays near the Cretaceous-Tertiary boundary, for otherwise sharp enhancement phenomena would be destroyed.

L. Alvarez: This would seem to indicate there was a very sudden killing, affecting benthonic organisms at the same time that the iridium was deposited. The benthonic organisms must have been absent for a significant span of time.

Smit: It is the only evidence available for extinctions of deep-water organisms, for the fossil record does not show significant extinctions of bottom-dwelling organisms.

Kyte: With regard to Dr. Smit's comment on secondary influx from the continents above the initial millimeter, if one millimeter represents the amount of material which is deposited by the accretionary event at Caravaca, and everything else is detrital material that was washed in, then 1% or 2% of the iridium in the entire horizon was deposited by that event. About 50 times more iridium must be derived from other places around the world. If a similar record occurs over 30% of the Earth's surface, then a mass balance problem is created.

Hsü: How long can the iridium dust stay in suspension?

Kyte: It depends on where it is coming from. If it was generated by an impact even I would suspect that it would fall out in a few weeks to a few months.

L. Alvarez: It cannot be transported world-wide through the atmosphere in a few weeks. A world-wide distribution indicates that it stayed up more than a few weeks, unless the major transport was by ballistic trajectories, as tektites are believed to be distributed.

Jaworski: If the material is in the form of a hot vapour, when it contacts an aqueous phase it would most probably start to dissolve. This could allow for a considerable length of time before it was finally deposited. If it entered the biosphere, it would appear in the sedimentary record only with the eventual deposition of the organic material. This could produce the tailing effect seen in the geologic record.

L. Alvarez: Can the mechanism that you are describing distribute the material world-wide?

Jaworski: No. Once it has been distributed through the atmosphere it would fall and come into contact with water. The original global transport would be through the atmosphere.

L. Alvarez: In the present atmosphere, which is not nearly as turbulent as the atmosphere would be after an impact event, it typically takes one year to transport material from the Northern Hemisphere to the Southern. That was discovered from the distribution of carbon 14 that was generated in the extensive Soviet bomb tests.

McLean: Considering the slow sinking time of most clay particles in the oceans, marine circulation would have to be a distributing agent as well.

Thierstein: That depends on what the transport mechanism is. Clay particles that settle in the deep ocean must be transported down through fecal pellet transport at high velocities, because the settling time of a two micrometer glass sphere would be on the order of 50 years. Lateral current in the deep ocean would therefore homogeneously distribute all the coccoliths and the clay minerals throughout the world ocean. The average grain-size of dust particles after the Mount Agung explosion in 1963 was about two micrometers at 20 to 30 km in the stratosphere (Mossop 1964). Junge and others (1961) calculated the settling-rates of particles based on measured grain-size versus altitude distributions of aerosol particles. The relationship is not linear with respect to altitude in the stratosphere. At an altitude of 10 kilometers a two micrometer particle sinks at a velocity of 4×10^{-2} cm s^{-1}. At 20 kilometers altitude the sinking velocity is 6×10^{-2} cm s^{-1}, or 1.5 kilometers per month. There is no allowance for coagulation processes which might result because of high particle densities, and the velocities were calculated according to Stoke's Law. If the average elevation were about 15 kilometers, within ten months virtually all of the particles would have settled below 10 kilometers where they would be rained out within a matter of days. It appears to be impossible to keep considerable concentrations of dust in suspension over extended periods of time at these altitudes.

L. Alvarez: The velocities that you mention are very small compared to the turbulent vertical velocities, which would then be the controlling velocities. The question should be referred to a specialist, but the principle applies to the ocean where particles are held in suspension by currents in spite of the settling effects of Stoke's Law.

Béland: I would like to present a few thoughts in an ecological mood. One is that ecosystems react in the same way to any drastic stress; they become simpler. For example, the effects of overgrazing by elephants in Africa and the effects of exposing a forest in North America to radiation are similar in that a degradation in the structure of the ecosystem takes place. An observer might not be able to tell whether too many elephants were present or a scientist was trying to prove some point. Another thought is that a list of generic extinctions is less meaningful than a list of species extinctions. Another thought is that once a planet has caught that disease that we call life, it is very difficult to cure it. Another thought is that the catastrophe cannot have been too drastic because the new species that appear in the Tertiary were derived from species that survived even though they have not been detected in the fossil record. A catastrophic event was followed by a diversification event. An event that would cut off light sufficiently to reduce phytoplankton production would have to block a very high percentage of light. That leads me to the last thought, which is that production should be considered, not biomass. Other links in the food-chain depend on phytoplankton production, not biomass.

What would happen to phytoplankton if light were severely attenuated? If it were completely cut off for a very long period of time, I think everything would have died, and this did not happen. If light-reduction were drastic enough, the resulting instantaneous stoppage of production would ensure that the remaining cells would be consumed by herbivores so rapidly that, in geological terms, an instantaneous event would occur. Several species in several groups, such as diatoms, thallophytes and xanthophytes, would survive through in encystment. This is common in temperate and high latitudes. How long the cysts would remain viable is not known, but it is probably on the order of two to four years. The stimulus that causes these cells to return into production may not be the return of light but the quality of the water or the temperature.

The timing of the event could be important. For example, the deleterious effects would be immediately felt in the tropics whatever the time of the year. In the high latitudes, if the event were to occur today in the fall, it would probably not matter at all because production normally ceases in the fall. During the Cretaceous the differences between high latitudes and the tropics were not as great as now, but differences between the patterns of production in high latitudes and low latitudes must have existed. The decay of cells would probably produce an increase in bacterial production. Whether this production could be used by other organisms down the food-chain is a matter of speculation. Single bacteria would be too small and groups of bacteria clustered around dead cells would be too large to be filtered out by specialized zooplankton carnivores, and most of these animals would die. Many zooplankters, including fish larvae, rely on light to catch their prey. They are unable to feed in the dark even if food is available. Some zooplankters could survive by effectively encysting. For example, cladocerans have both sexual and parthenogenetic modes of reproduction which they adopt according to prevailing conditions. They also produce durable cyst-like eggs which can survive for months and perhaps for years. It is possible that some ostracods and copepods also produce durable eggs. Further down the food-chain, many pelagic fish feed on zooplankton. Herring and mackerel depend heavily on the bloom in phytoplankton during May and June. They feed heavily, and larvae are recruited into the adult population. If a catastrophe happened in the spring, the effects would be more drastic than in the fall.

Perhaps surprisingly, the larger predators, such as sharks and other large fish, would survive longer, because they could feed on decaying animals or temporarily become dormant. Most of these animals are ectothermic, and in colder water their metabolism would be further depressed. Under such conditions they could survive much longer than terrestrial mammals, for example. The turnover-rate of food in deep-sea bottom-dwelling organisms is very slow and ultimately depends on surface productivity, but with a time lag. There is even a possibility that the rain of dead matter from the surface would sustain the deep-sea benthos for a substantial period of time.

Marine ecosystems could probably survive pretty well if the blockage of light lasted from six to eight months, or perhaps a year. They would probably not survive a blockage of two to two and one-half years. McKay and Milne (1980) estimate that phytoplankton cells would die after two days and zooplankton after about 30 days. The latter may be an overestimate, for most zooplankton would not survive more than a couple of days unless the event happened to coincide with the autumn in high latitudes. A survival of 15 days for small fish is reasonable, as is a survival of 150 to 200 days for large fish. Blocking the light for longer than about eight months would make it very difficult for anything to survive into Tertiary time.

Hsü: Why should any phytoplankton survive at all if the blockage lasted more than two days?

Béland: About two days would be required for the production of phytoplankton to completely stop. Individual cells tend to sink continuously and are brought to the surface by currents where they would probably rapidly be eaten. Most cells would not survive longer than a matter of days.

Asaro: In a private communication to Dr. Milne, Dr. Bernard M. Oliver stated that there would be tails on these distributions. Dr. Milne replied that he agreed, and the question is how low does the density have to become before the population cannot renew itself.

Béland: Some species could survive through encystment.

Brasch: One can envisage a rapid and total darkening halting all photosynthetic activity, which is then followed by a rapid clearing. An extinction of some species occurs, but not others that can survive seasonally adverse conditions. One can also envisage gradual darkening, producing a slow reduction in primary food production, followed by a gradual clearing. In this case some species might be able to adjust to the changing conditions. The link between photoperiodism and reproductive cycles must not be overlooked. If normal photoperiodic changes were interrupted for a period of two or three years, survival might be jeopardized not by a lack of food but by an inability to reproduce. The biosphere is buffered against a number of possible catastrophic scenarios, but only within certain time constraints, which are critical and must be defined.

Béland: Many zooplankters in high latitudes are very well synchronized to reproduce in the spring. This would be the last thing to do in the absence of phytoplankton productivity, but the organisms would probably be unable to switch off that pattern.

Theirstein: In experiments in culture it has been shown that one of the dominant living coccolithophorids, *Emiliania huxleyi* can survive by feeding on dissolved organic matter for longer than four months in complete darkness (Blankley 1971). If the concentration of dissolved organic matter is low, a minimum illumination of about 400 lux, or 0.3% of the flux incident at the surface of the ocean is needed. This is equivalent to light intensities at a depth of 150 meters in clear ocean water. Coccolithophorids are known to live and survive at depths of 200 meters, so that one would have to assume 99% or more of the light was blocked for an extended period of time to produce phytoplankton extinctions.

Béland: Although individual cells might survive up to a year in total darkness, the effects of the blockage of production at the surface on the food chain would be so drastic that it is difficult to believe that blockage of production lasted this long.

Dilcher: What is the record of fish kills at this time?

Russell: According to Danilchenko (1978) the number of recorded fish genera declined from 38 to 10 from terminal Cretaceous (Maastrichtian) to basal Tertiary (Danian) time.

Thierstein: According to Dr. Patricia Doyle (personal communication), the record of the teeth of bathypelagic fishes shows a comparable evolutionary turnover, although the resolution is only within ± 10 million years.

Kyte: The resolution may be somewhat better than that. Doyle (1980) was able to locate the Cretaceous-Tertiary boundary within 20 centimeters and iridium was detected, implying a resolution of about 100 thousand years. However, she had no confidence in distinguishing Danian from Upper Maastrichtian time, because she could not find a change in ichthyoliths across the boundary.

Hsü: Nautiloids and ammonites have a different record. Are there any physiological reasons to think they occupied different habitats?

Thierstein: Only nautiloids can now be studied. *Nautilus pompilius* is known to dive as deep as 500 meters (Stenzel 1957).

McLean: Some plant groups were in existence during Cretaceous-Tertiary transition time that might have had very little storage tissue either in the sporophyte or in the spores. Two of these groups that are also living today are the Equisitales and Osmundales. They seem not to have suffered greatly in the transition, as far as I know. A darkening lasting several years should have devastated those groups.

Pirozynski: Perhaps marine habitats are relatively well buffered against a catastrophe. Land habitats are very vulnerable. Plants are growing systems with symbiotic fungi and bacteria that absolutely depend on them for a constant supply of photosynthates. They are also attacked by pests and diseases that operate within annual life cycles. One would expect that obligate parasites to suffer very greatly if photosynthesis stopped for more than a year. If all the parasites, symbionts and pests were removed a complete floral turnover would take place.

Dilcher: Could poisoning have disrupted food chains more effectively than blockage of photosynthesis?

L. Alvarez: How could the material be transported world-wide without causing a darkening? Two available time-constants are one year to transport material between the Northern and the Southern Hemispheres, and two and one-half years for Krakatoa material to fall out of the stratosphere. Other than atmospheric transport, there seems to be no other way for landing material on both Italy and New Zealand. When one centimeter of clay is suspended in the air, in any particle size distribution, the result is blackness.

Thierstein: In the case of small particles and aerosols, a lot of back-scattering of light can occur. The effect is less in the case of coarse particles. Smog reduces visibility by impairing the transmission of direct light, whereas the light incidence is due to back-scattered light, which is not so effectively blocked.

L. Alvarez: In passing through the equivalent of a one centimeter clay layer composed of one micron particles, the light intensity would not decline exponentially through scattering; it would decline linearly. The extreme cases are exponential attenuation by perfectly absorbing particles and linear attenuation by perfectly reflecting particles. The true situation is obviously intermediate between these extremes, but the suspended layer must certainly have resulted in very severe attenuation of the sunlight. The light must pass through a certain number of grams of material per square centimeter, and it makes no difference whether it is spread out over 100 kilometers or 10 centimeters.

Thierstein: If a glass of water containing suspended coccoliths is evaporated, light will not penetrate a clay layer of coccoliths that is one-half millimeter thick. If the same light is directed into a glass of water with the same amount of coccoliths distributed in suspension through the glass, the bottom of the glass will shine white because of back-scattering.

McLean: Atmospheric particles are not necessarily opaque because light can be transmitted through petrologic thin-sections which are thicker than the particles.

L. Alvarez: Some light will come through to be sure, but, qualitatively, it would be too dark for plants to grow.

Dilcher: It would be useful to document in a rigorous manner that photosynthesis was depressed to 10% or 5% of former levels for a period of a full year. The difference between extinction proportions in terrestrial and marine systems suggests that poisons might have been more important than the total lack of light for photosynthesis. Poisons could be removed by fresh water flowing off of terrestrial systems into marine environments where they might accumulate and produce the most damage to life.

L. Alvarez: Specialists in chemical warfare often experience difficulties because the poisons react with rocks, water or other materials.

Hsü: Hydrogen cyanide could have been injected by 10^{17} grams of impacting cometary material. Part of it would be oxidized, and the remainder within an ice conglomerate would be dissolved in the ocean following an oceanic impact. A poison concentrated in surficial currents would not come in contact with either benthic or terrestrial organisms. Experiments to define lethal concentrations in the seawater for phytoplankton are needed.

Kyte: Nickel sulphide is also toxic to some organisms (Costa and Mollenhauer 1980).

Jaworski: If one particular lethal mechanism is operative, another one by inference is probably operative as well. The high levels of arsenic, osmium and nickel in the very thin boundary layer at Caravaca could easily be dissolved in a surface layer of water in the oceans and be toxic. On land, unless the poisons were directly inhaled, the soil would act as an ion-exchanger and absorb much of these toxic metals before they would be taken up by the roots of plants. This is one way of differentiating between terrestrial plant toxicity and phytoplankton toxicity. The chemical form of the elements is also significant. Some are extremely insoluble, but others, such as oxides and ions that form soluble chloride complexes, are soluble. Another environmentally differentiating mechanism is that, in freshwater, the complexing anions are hydroxide and carbonate, whereas in marine systems the complexing anions are chloride ions. Thus many elements which would normally precipitate in freshwater, would remain in solution in salt water. It would also be useful to obtain data on the levels postulated for some of these elements in the impacting material in order to estimate the resulting concentrations in the biosphere.

Feldman: The platinum group elements themselves are not necessarily nontoxic. Many of the soluble compounds of ruthenium and osmium, such as the chlorides, amino chlorides and volatile oxides, are <u>highly</u> toxic and can elicit physiologic responses at concentrations as low as 10^{-9} gm/ml (National Academy of Sciences, 1977). Assuming a distribution of 10 ngm cm^{-2} of Pt-group elements mixed into the oceans, the resulting concentrations of toxins are four orders of magnitude lower than is required to elicit a physiological response. Two orders of magnitude can be regained by restricting consideration to the shallow waters along the continental margins. However, unless bioconcentration of some of these compounds occurs, their effects would not appear to be significant.

Pirozynski: One might consider that those organisms that survived had a greater tolerance, because they survived.

Hsü: If the poisons were concentrated in surface currents, which are usually only a few hundred meters deep, the missing two orders of magnitude might be found.

Feldman: I had already made that assumption. The concentrations would be, at best, 10^{-11} gm/ml, and 10^{-9} is required, for a physiologic response.

Hsü: Surface currents occupy only a part of the surface of the ocean area. Perhaps the poisons were spread into the tropics. Another order of magnitude can be gained by restricting them to 10% of the ocean surface.

Béland: It should be noted that many bottom-dwelling marine organisms have a phase in their life cycle which is planktonic. They would be vulnerable to a drop in phytoplankton productivity. Freshwater organisms living at that time of the extinctions are not well sampled. Only those terrestrial organisms living in specific environments coinciding with areas of sedimentation can have a fossil record. Conditions in the ocean are more homogeneous, as is the possibility of being preserved in the fossil record.

Clube: Certain points in the approach Napier and I (1979) have adopted might be a source of further discussion. A suspicion has perhaps arisen that the theory we developed was an *ad hoc* astronomical response to the discovery of geochemical anomalies. However, the theory was developed prior to these discoveries and we feel it has a comparatively secure astronomical basis. Catastrophism is a direct consequence, and the evidence that has been discussed here fits into this kind of theoretical framework. To begin, a cratering record is preserved on the inner planets and many Apollo (Earth-crossing) asteroids have been identified. A satisfactory general relationship exists between the number of Apollo asteroids and the cratering-rate of the last three billion years. This relationship was not recognized some 10 to 20 years ago because the number of known Apollos was too low. Discoveries of Apollo asteroids have increased in recent years, however, following the work of Shoemaker and his colleagues (1979). Apollos impinge on planets and produce craters, but more frequently they just miss planets. Close encounters tend to deplete the population, ejecting them from the Solar System altogether; the time-scale is something like 30 million years. However, the average cratering-rate has remained approximately constant over three billion years. There must be an astronomical mechanism, therefore, for feeding the Apollo asteroid population and keeping it in tune with the cratering rate. Where do these Apollos come from? The asteroid belt was once considered a natural source for the Apollo population, when it was believed to be smaller. But there are not many perturbers available that extract the Apollo population from the asteroid belt (Wetherill 1976). Thus, a growing consensus amongst astronomers, views comets as the main source of the Apollo population. Essentially, Apollos derive from the short-period comet population which in turn derives from the spherical distribution of long-period comets, through interaction with Jupiter. With comets identified as the main source, a slight problem still remains. There seems to be a current overabundance of Apollos relative to the long-period comet flux. We conclude there must have been an enhancement in the supply of comets approximately 10 million years ago, producing the current population of Apollo asteroids. It has been known since the turn of this century that the distribution of the perihelia of long-period comets is not isotropic; there are indications of concentrations which are related to the motion of the Sun through the solar surroundings and indeed vague signs of correlation with the Milky Way. Yabushita (1979) asked the question, "How long ago would this sort of distortion of the surroundings have been imposed upon the comet system?" He examined relaxation arguments and suggested something significant happened in relation to the current comet population, again about 10 million years ago. This led us to ask, is there anything in the solar neighbourhood that would cause one to think that something could have happened to the Solar System approximately 10 million years ago?

Over the last few decades astronomers have recognized with increasing confidence the existence of spiral-arms in the solar neighbourhood. Another feature of our

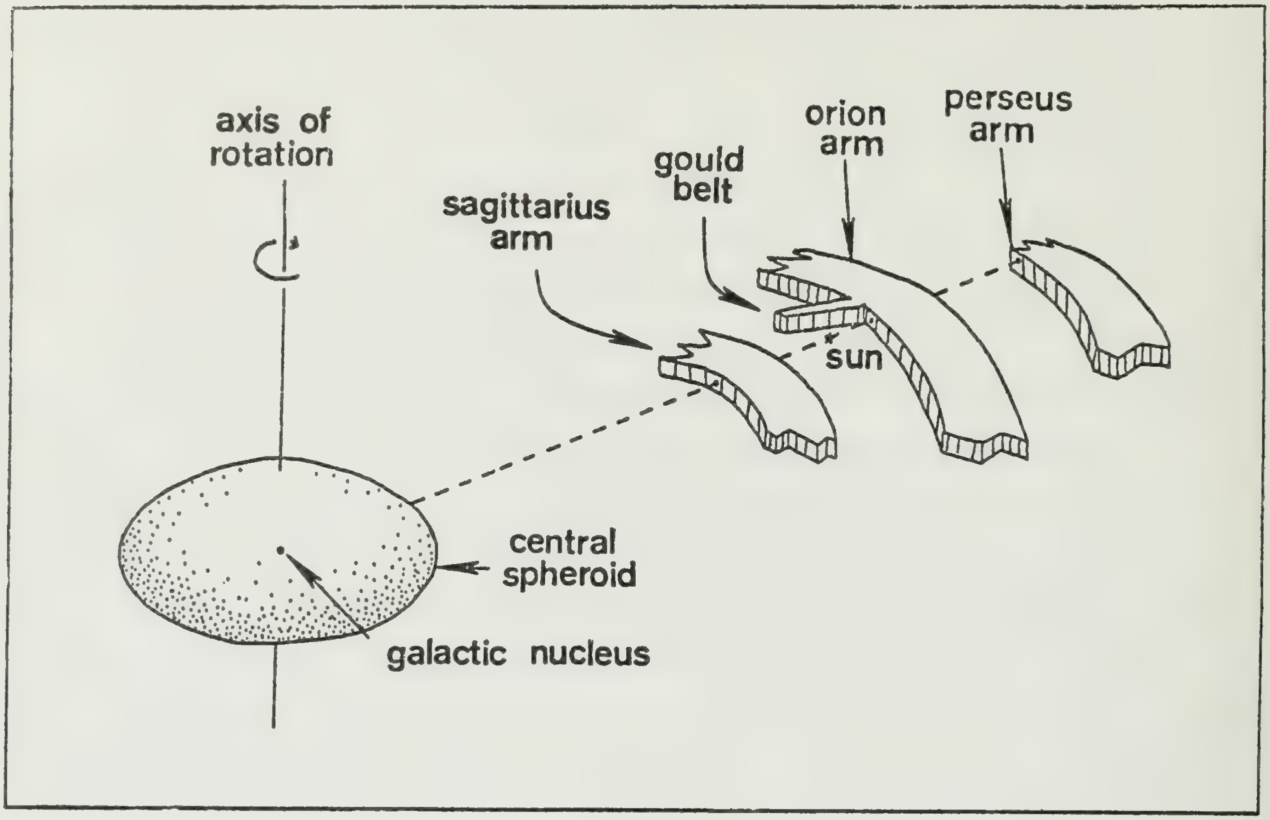

FIGURE 6. *Schematic illustration of Gould's Belt. According to the most recent estimates, the Sun's distance from the Galactic nucleus is ~ 7 kiloparsecs. The Sun is moving away from Gould's Belt in the direction of Galactic rotation at ~ 25 km s^{-1}.*

galactic neighbourhood of very great significance is the presence of some material which has been known for nearly a century as Gould's Belt (Fig. 6). It is a feature containing young stars, large quantities of gas and dust, and all the things that we normally associate with spiral-arms, but it is not in fact part of the spiral-arm system. Many theoretical models are given in the literature as to what caused it, but they are not relevant here; all we need to know is that the feature exists. It is a couple of hundred parsecs from the Sun, and the Sun's motion relative to the feature is on the order of 25 kilometers per second. These numbers are very well determined, and it is possible to conclude that the Solar System passed through Gould's Belt about 10 million years ago. A significant possibility exists that the long-term comets were captured or perturbed as a result of going through Gould's Belt. This is not in accord with the prevailing view that the comets in the Solar System are primordial, and it indicated a need for further research.

Radioastronomers, in surveying the galactic plane over the last few years, have become increasingly aware of the existence of giant molecular clouds. There are many of them, they are very massive and they present to the galactic astronomer an environment for discussing the Oort Cloud that is entirely different from the one in which the Oort Cloud of comets was originally conceived. When the Solar System and its Oort Cloud pass close to a giant molecular cloud the latter is disturbed by tidal action and comets are dispersed into the surroundings. Thus, it is now possible to demonstrate that the Oort Cloud, of necessity, cannot be primordial. If the primordial Oort Cloud is no longer tenable, but comets are present, the probability that they are captured is high. Once comets are captured, many of them fall into the inner part of the Solar System where they can produce the craters and the possibility of episodic catastrophism dictated by the rate at which they are captured from the surroundings. If impacts are responsible for triggering many of the phenomena registered by the geophysical record, the prediction of episodicity is of fundamental importance and one that is provided by no other theory (Clube and Napier, in press). It is clearly important to know whether a catastrophe, such as the one we have been discussing, was caused by the impact of a comet or a meteorite, although from some points of view, the distinction may not be significant. As the Solar System passes a giant molecular cloud, comets would be captured, but what is already present in the Solar System, such as the primordial family of meteorites, would also be perturbed. These objects will be fed into the inner parts of the Solar System at the same time as comets are captured. Thus, both families respond to interaction with giant molecular clouds, and both families are capable of impacting and producing craters. The ratio of comet to meteorite impacts is of importance in our theory.

W. Alvarez: If the Oort Cloud is not primordial, then some meteorite debris from the Antarctic ice sheet should have originated at a different time than did the Solar System. The isotopic ages are all similar, and must then be interpreted either as coincidental or wrong.

Clube: This material is undoubtedly meteoritic and is basically of Solar System origin.

W. Alvarez: Would comet debris never be found on the Earth? Should it always be broken up and dispersed? Some delicate, soft kinds of "meteorites" have certainly been recovered from Antarctic ice.

Clube: I am aware of this, and it is clearly important to decide what compositional aspects would distinguish comets from meteorites.

L. Alvarez: The spherules that were found near Tunguska were probably derived from a comet. If that comet came into the Solar System 100 or 600 million years ago its origin was different from normal Solar System material. The time clocks (for example the uranium isotope ratios) would be set differently than the ones in the normal meteorites. Such discrepancies have not been observed.

Kyte: Analyses of stratospheric dust (Brownlee *et al.* 1977, Fraundorf 1981) suggest that comets are very similar to carbonaceous chondrites, which are all very similar in terms of non-volatile bulk chemistry.

Halliday: These are ablation products. They were derived presumably from cometary material, but the condensate, following the melting process in the atmosphere, could be quite different (Krinov 1966).

L. Alvarez: If the Clube-Napier theory is correct, they should not show the 4.7 billion year age of our Solar System.

Kyte: I don't think anyone has tried to date the spherules from Tunguska. Certainly if a piece of a comet were analyzed, one should be able to prove whether or not it was from the Solar System .

Halliday: Anders *et al.* (1973) has shown, although Wetherill (1971, 1979) does not agree, that carbonaceous chondrites have spent a long time in the regolith of asteroids because they were soaked by solar winds. A comet does not live long enough in the inner Solar System to pick up much solar wind.

Feldman: Is there any possibility that enough cometary dust would be on the surface of the Moon to show an age signal significantly different from four and half billion years?

Halliday: The fraction of Moon-dust that is presumably meteoritic in origin is about one to two percent. Most of it is considered to be cometary in origin (Anders *et al.* 1973).

W. Alvarez: In addition to the differences in radiometric systematics, non-Solar System material would show differences in heavy stable isotopes, such as iridium and osmium, which may be more easily detected using accelerator techniques.

Feldman: It might be more relevant to examine the lighter materials. Radioastronomical techniques can be used to determine ^{13}C to ^{12}C ratios. This ratio differs in the galactic centre, the galactic disc and the Solar System. Radio observations of HCN molecules could determine the ratio in Halley's Comet.

Kyte: There is still a possibility of a sample return from Halley's Comet during its next transit (D.E. Brownlee, personal communication).

L. Alvarez: Experience with accelerators enhances one's appreciation of how difficult it is to inject a particle from outside into a stable orbit. It cannot be accomplished with static fields. Similarly, gravitational potential resembles a hyperbolic hole and it is very difficult to inject something into that hole and make it stay there; if it goes down it should come back out again. If capture is so difficult, how can the Solar System capture comets so efficiently?

Napier: Perhaps I can address this question, as well as whether or not the Oort Cloud is a primordial object. The crucial point is the survivability of the primordial cloud in the presence of tidal forces being exerted by encounters with the giant molecular clouds. The molecular clouds are probably distributed along the spiral-arms of the galaxy. They are certainly associated with dust, and dust is a known spiral-arm tracer. The Solar System would not be able to soar over or under the clouds but would have to pass between them in order to avoid an encounter. The large mass of these clouds, in the range of a 100 thousand to a million solar masses, would produce a strong gravitational focussing, and the actual target-area is rather larger than the geometric target-area. Taking the uncertainties into account, one finds that at intervals of about 100 to 200 million years the Solar System will pass through a giant molecular cloud. Because of the substructure within a cloud it is quite possible for the Solar System to pass completely through it without encountering a dense nebula. Perhaps once within Phanerozoic time, a nebula with densities of order 10^4 or 10^5 gm cm^{-3} has been encountered. I have carried out some numerical modelling, and it turns out that the tidal-effect is very important in the neighbourhood of a dense nebula (Fig. 7). About 33 thousand comet orbits have been calculated, using standard models for the primordial Oort Cloud, and random encounters with these nebulae. In the outer extremities of the cloud the comets are easily swept out of the Solar System, but a dense, inner core may remain. If the Sun passes within 10 parsecs, 5 to 10% of the comets in the long-period region are lost. If the encounter is closer, the effect becomes more drastic, and it is evident that even a single passage through a giant molecular cloud has a dramatic effect upon the primordial Oort Cloud. The cometary orbits are disturbed through three-body encounters, or a tidal effect, basically.

The substructures in the giant molecular clouds are in the size-range of a few parsecs, while the average dimension of the cloud is 20 parsecs. The substructures are well separated and discrete, and facilitate the disruption of a primordial Oort Cloud. After 500 million years in one simulation, we found that we had almost lost the outer regions of the Oort Cloud (Fig. 8). After two thousand five hundred million years there was really nothing left. I think the conclusion is that it is difficult to maintain the primordial Oort Cloud hypothesis with the

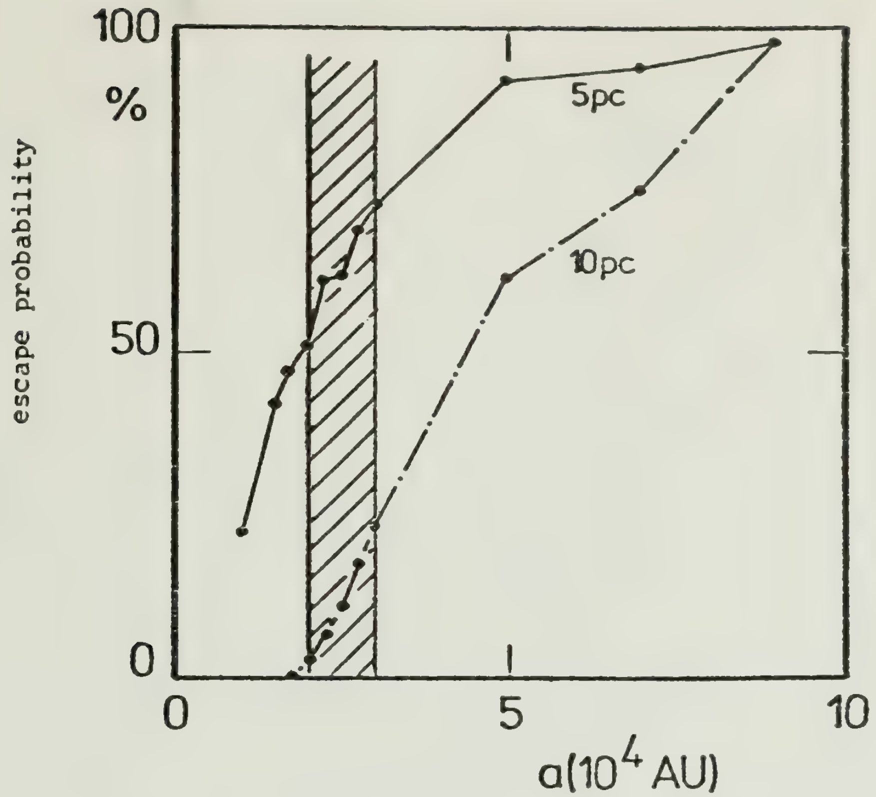

FIGURE 7. *Illustration of the damage done to a comet due to a single encounter with a massive nebula. In this case the nebula has mass 20,000 solar masses, asymptotic approach speed 5 km/sec and impact parameters as shown (1 pc = 3.26 light years). "a" represents the semi-major axes of the comet orbits (in a.u.) which were randomly oriented and uniformly distributed in eccentricity. The hatched area represents the region from which long period comets are observed to arrive.*

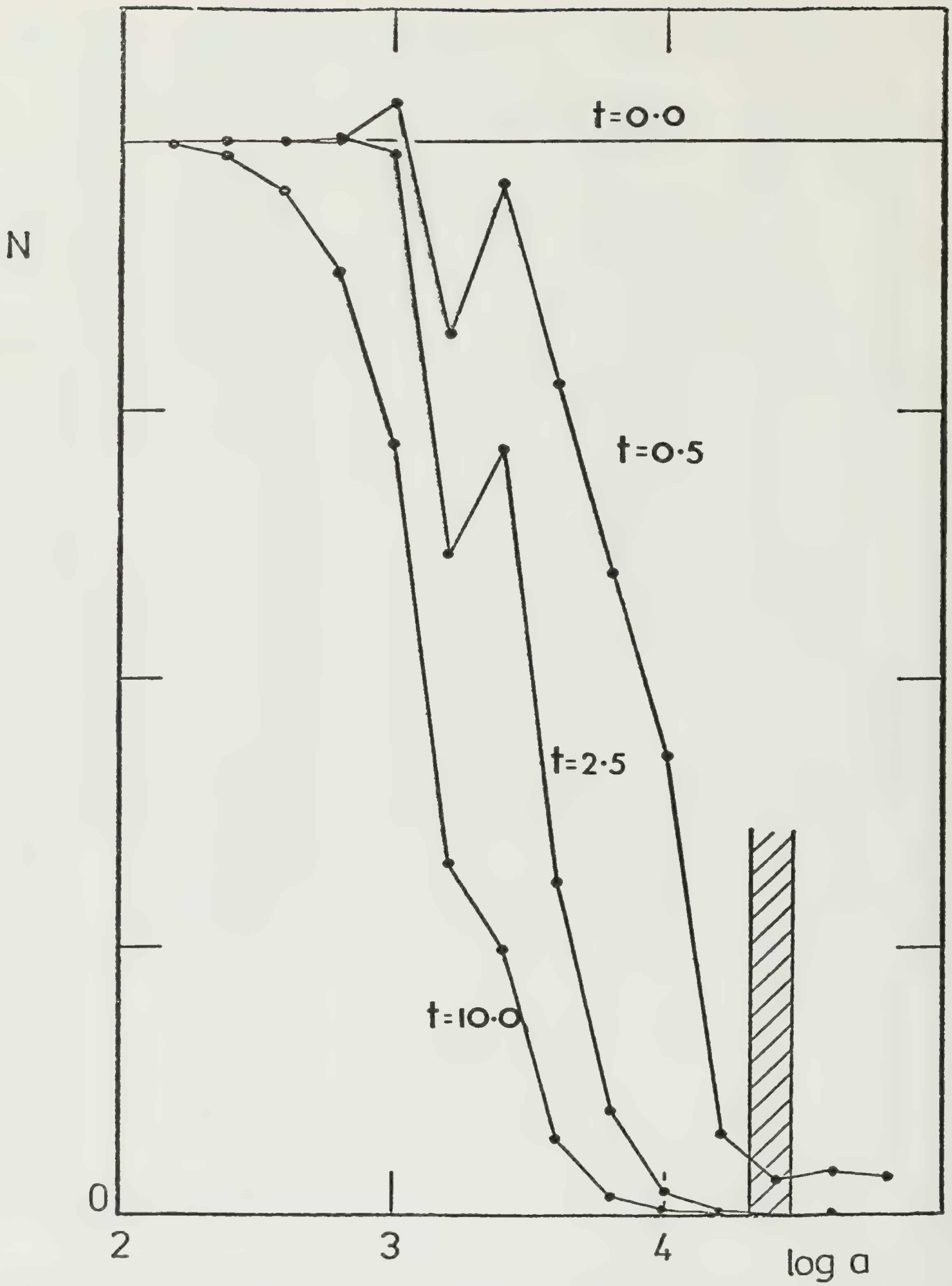

FIGURE 8. *Cumulative effect of massive nebulae on a hypothetical comet cloud initially populated uniformly in log a. Time is measured in billions of years. Any primordial cloud (t ≃ 5) would have long since been dissipated apart from a core a few 10^3 a.u. radius. The long period comets thus orbit in an unstable region, implying recent capture from interstellar space.*

molecular-cloud system as it is currently envisaged.

It then seems advisable to examine how comets could be captured from the interstellar medium. Several processes, from straight-forward sedimentation to Jeans' collapse, could result in the formation of cometary bodies in giant molecular clouds. The figures are crude and the uncertainties are large. Condensates from typical clouds would contain a mean mass of several 10^{17} grams, which corresponds well with the estimates of Hughes and Daniels (1980) for the mean mass of long-period comets. There seems to be a striking resemblance between the chemical composition of comets and that of the interstellar medium. Thus, although growth mechanisms have not been fully developed, there seems to be no great obstacle to this mode of cometary formation. The absence of comets coming in on hyperbolic orbits places an upper limit to the density of field-comets, and it would not be possible to capture field-comets at a sufficient rate to produce a quasi-equilibrium Oort Cloud. They must be captured from dense regions. Assuming about 5% of the mass of molecular clouds is in kilometer-sized planetesimals, the capture rate can be estimated by using simple sphere-of-influence arguments. The figure of 5% is not arbitrary. There is a long-standing problem with depletion of C, N and O in the interstellar medium, which Tinsley and Cameron (1974) and Greenburg (1974) have suggested might be locked up in interstellar objects. With the 5% figure it was found that the entire Oort Cloud could be captured in one passage through a giant molecular cloud. It may be an overstatement, but the Oort Cloud might be viewed as a quasi-equilibrium phenomenon.

The hypothesis is vulnerable from a number of directions. For example, if the small-body population is controlled by the Galaxy rather than by processes internal to the Solar System, then time-scales appropriate to the Galaxy should be sought. One consequence would be a constant cratering-rate, because the decay time of galactic material is long. The rings that Dr. Tucker proposed earlier would be as subject to instability as Oort Cloud comets, but if there is anything in the numerology, then one should look for some sort of galactic explanation. Modulations in the cratering-rate would be expected. Periods of high risk should follow periods of low risk, because of decay times of materials in the Solar System. Discrepancies should be apparent between the known Apollo population and what would be expected according to the cratering-rates. Shoemaker *et al.* (1979) have found a statistically significant two to one increase in the cratering-rate over the last 600 million years against that over the last three billion years, and in turn there seems to be significantly more Earth-crossing asteroids than is consistent with the more recent cratering-rate.

Also of interest would be anomalies in isotopic abundances. Iridium or osmium isotope ratios may not be helpful because r-process environments are poorly understood; so many supernovas have occurred and so much mixing of ejecta has taken place over long time-scales. Whether striking Solar System isotope ratios being detected indicate the Cretaceous-Tertiary boundary material is truly indigenous to the Solar System is not, according to D. Arnett (personal communication), a settled question. However, I would definitely look for variation in the ^{12}C to ^{13}C ratio in strata deposited in temporal proximity to mass extinctions. Finally, the theory predicts impact episodes which may include 10^8 or 10^9 megaton impacts, and these should be associated with three to six biological catastrophes within the Phanerozoic corresponding to spiral-arm passages. McCrea (1981) has reviewed the episodicity question in some detail.

Grieve: The sample of well-dated craters is too small to reveal adequately changes in the cratering-rate. The uncertainty factor is about two during Phanerozoic time.

Napier: We suspect that there was an increase, actually.

Grieve: Curves can be drawn that show a factor of two increase. The lunar record covers the period between four to three million years ago. This record must be fitted to the Phanerozoic terrestrial record, making adjustments for the different gravities of the two planets. Other uncertainties include the cosmic velocity of the objects and how large the gravitational cross-section correction will be. One can draw a line through the points and conclude that the cratering-rate has been constant for the last three billion years. With the uncertainties attached, the data will also support an increase by a factor of two perhaps quite recently in the Phanerozoic. What is the composition of these extra-Solar System cometary bodies? Impact melts match the relative siderophile abundance patterns of known meteorite groups. Some are Cl, others are different types of chondrites, and some are A chondrites or irons. Do these data fit your hypothesis?

Napier: This requires a statistical answer. There will be a mixture of bodies ranging from *bona fidé* interstellar objects to primordial remnants. How many impact melts have been analyzed?

Grieve: Melts from only 10 to 15 craters have been measured. Perhaps, another 50 craters remain to be analyzed. The present sample is small. In some cases siderophile enrichments were not detected in impact melts, for unknown reasons. Possibly the impacting body left a signature that is very like that of terrestrial material. It does not stand out as a geochemical anomaly.

Halliday: There are some very interesting aspects of your theory. In discussing the Apollo objects you may be subscribing to a view which overemphasizes the cometary component. Shoemaker and Wetherill are of one opinion, and I suspect they are on the minority side in this case. The kind of exotic resonance phenonmena with Jupiter that can bring material in from the asteroid belt seems to account for most of the ordinary chondrites arriving in a time which is consistent with their cosmic ray exposure ages. These are usually 20 to 30 million years, and in extreme cases, up to 60. Irons have survived longer in the inner Solar System and have greater exposure ages. Extrapolating down to the small fireballs, which are all Apollo objects, we think with increasing confidence that the material can be identified by how well it penetrates the Earth's atmosphere. Three main classes are identified, which are arriving in just about the abundance that was predicted 15 years ago. The early results from the Prairie Network suggested the rate was much lower, but this was due to an error which gave low densities for the projectiles. Ordinary chondrites and irons are represented in the correct abundance. A second group seems to represent the carbonaceous chondrites. They are much more abundant than the proportion of carbonaceous chondrites in meteorite collections, because the atmosphere is such a severe filter. A third group appears never to penetrate the atmosphere, and these are presumably the cometary components. Many of the objects responsible for terrestrial cratering are associated with definable meteorite categories. Cometary impacts must also occur, but they are probably more difficult to identify.

Napier: A weak point in the work at this state of development is, how many objects are *bona fidé* interstellar and how many are primordial? Dr. Pillinger at Cambridge is developing a technique for measuring the ^{12}C to ^{13}C ratio in Brownlee, or micron-sized particles. It is probable now that the Giotto Project, which is a Halley flyby, will attempt to measure the isotope ratio.

L. Alvarez: This theory is attractive in many ways, but presents one area of difficulty. The isotopic abundance of uranium can be measured without a mass spectrometer. Alpha particle emission in the two different ranges provides a simple measure of the relative abundances of uranium 238 and uranium 235. No uranium from a natural source has ever shown a departure from the expected ratio with one exception. That was a sample of uranium from Gabon, which was found to contain too little uranium 235 in it, leading to the discovery of a fantastic natural reactor that operated a couple of billion years ago when more uranium 235 was present and a homogeneous reactor could operate with natural water. If different natural "factories" make uranium at different times and in different places in the galaxy, then the ratio of uranium isotopes will also differ from region to region. Comets arriving from these different regions of space should not all have a ratio typical of the Solar System.

Napier: One of the problems may be the speculative nature of current nuclear synthesis theories. For example, Rees (1978) has suggested that supermassive, or very massive objects, might generate heavy nuclei in a pregalactic phase: the material is then already present and mixed. The time taken to recycle material in the Galaxy is of the order of a third of the age of the Galaxy. If supernovas were the "factories" and produced a certain isotope ratio which is taken to vary by a factor of two from one supernova to another, then over one-third of the lifetime of the Galaxy several 10^8 "factories" will produce and mix this material. It is possible then that two divided by the square root of 10^8 would be the expected characteristic variations in an average galactic locality.

L. Alvarez: Supernovas would then probably be eliminated as the source of uranium.

Tucker: The consensus was that uranium is made in supernovas, because no other suitable source has been identified. Cowan and his associates (in press) now suggest that the r-process occurs in the cores of red giants. Their populations are 10^{10} instead of 10^8, so the fluctuations are further depressed. If the Solar System passes through a cloud, whether comets are present or not, a very large amount of interstellar material will be swept up. This would depend on the geometry and mechanics of the accretion episode, but as much could be accreted as is observed in this boundary layer. If a catastrophe were not caused by an impact event, could it still occur in terms of shutting off the light?

Kyte: The Napier and Clube hypothesis is testable, as it should produce iridium anomalies in the stratigraphic record. We plan in the near future to construct an iridium profile for the Cenozoic, and should we find a sharp peak at about 10 million years, we will let you know. This hypothesis predicts that we should find a sharp increase, say two or three times, in the flux of cometary material, which should then tail off over a period of several million years. It should be measurable, but an increase on the order of the Cretaceous-Tertiary enrichments should not occur.

W. Alvarez: If the Apollo objects cannot be derived from the asteroid belt, and the Oort Cloud is periodically overturned, where do the iron meteorites come from?

Napier: A different scenario may not be necessary for the iron meteorites; they could be derived from the main-belt asteroids. The transfer rates are adequate for them, but Wetherill (1978) finds that for kilometer-sized objects transfer rates fail by a factor of 10 to 100 in deriving objects from the main belt.

Feldman: I want to point out that a large accretion of dust will probably not occur in passing through a diffuse giant molecular cloud unless there is a direct passage through one of the dense molecular clouds associated with it. Relative to a dense molecular cloud, the size scale of a diffuse cloud is expanded by a factor of 10, but the density scale is decreased by about three orders of magnitude. If the dust-to-gas ratio remains constant, there would be two orders of magnitude less material than has been identified in the boundary layer, assuming chondritic abundances. The short time-scale signature observed in the geologic record does not resemble that expected from a giant molecular cloud, unless there was a close encounter with a very dense condensation, with $n(H_2) \sim 10^{7-8}$ cm^{-3}.

Asaro: With respect to isotopic ratios, was the material in these molecular clouds made at essentially the exact time that the Solar System was made?

Napier: No.

Asaro: If different "factories" made uranium isotopes in the same ratio, but the uranium was made at times that differed significantly compared to the half life of ^{235}U, which is about 700 million years, then identifiable differences in the isotopic ratios would be detectable. If the isotopic ratios of uranium in material captured from such a cloud have a Solar System value, then they must not only have been made in the same way, but the length of time that has subsequently elapsed must also be the same.

Clube: We are discussing heavily-diluted observations of uranium, the interpretation of which is very theory dependent. Large molecular clouds with a different composition from those associated with the origin of the Solar System may well exist. But that raises the question as to whether or not galactic processes are adequately understood at present. Does galactic material remain in one region and undergo continual chemical recycling? Is spiral-arm material being displaced from another part of the Galaxy? These questions are related to how spiral-arms are formed. Are they created by density waves, traversing the intergalactic medium and reprocessing material? Are they periodically produced in some other way with characteristic compositions? We certainly recognize the point you are making, but do not believe the data at the moment provide a satisfactory answer.

Halliday: How high is the temperature in molecular clouds when cometary material is condensing?

Napier: It is usually estimated to be 10 or 20°K.

Halliday: Some condensates in carbonaceous chondrites formed at temperatures of approximately 1600°K.

Clube: Where the condensation takes place may be integrally bound up with a satisfactory theory of Solar-System formation itself.

W. Alvarez: So far no one has located a circular hole anywhere on Earth of the right size and age to be the crater of the impacting asteroid. Some have suggested that the impact might have initiated volcanic activity, and that a large accumulation of volcanic rocks should be sought instead of a crater. Dr. F.W. Whipple postulated in a public television programme that if the impact occurred close to a mid-oceanic ridge, where the lithospheric crust is thin and material is very close to the melting point, then a large quantity of volcanic materials would be released. He examined the mid-oceanic ridges for a large accumulation of volcanics and identified one that seemed to be the correct age called Iceland. The problem is that there was no oceanic crust in the vicinity of Iceland 65 million years ago. Greenland and Norway were in contact until something like 55 or 60 million years ago, according to magnetic anomaly patterns documented by Talwani and Eldholm (1977). The Cretaceous-Tertiary boundary is very close to Anomaly 29. The earliest anomaly in the North Atlantic is Anomaly 24, which is near the Paleocene-Eocene boundary. Seyfert and Sirkin (1979) have considered the possibility that large impacts may trigger hot-spot activity and generate the onset of seafloor spreading. Dr. Seyfert (personal communication) suggests that the Cretaceous-Tertiary object, after some time delay, caused the onset of volcanism in this area. In support of this, he notes that the ages available on volcanic rocks in Scotland and in eastern Greenland do in fact turn out to be very close to the age of the Cretaceous-Tertiary boundary. However, volcanism on the west coast of Greenland is clearly associated with the somewhat earlier openings of a rift between Baffin Island and Greenland. Volcanologists are satisfied on petrological grounds that the lavas from east and west Greenland are related to the same volcanic processes. In west Greenland, ages are known back to 77 million years. Furthermore, a microcontinent called Rockall Bank west of the British Isles has similar volcanic rocks which have ages dating back to 105 million years. This general volcanic province is significantly older than the time of the Cretaceous-Tertiary boundary. That some volcanic ages coincide with the boundary event is probably coincidental.

Clube: Do radiometric ages indicate a number of impacts down the Mid-Atlantic Ridge?

W. Alvarez: There are several volcanic centres which have been related to hot spots in the mantle, extending from Jan Mayen Island in the north to Bouvet Island off the coast of Antarctica. The onsets of volcanism extend from well below the Cretaceous-Tertiary extinction to somewhat after it.

Hsü: Walvis Ridge volcanism commenced about 80 million years ago, and if it had anything to do with an impact it would not be the Cretaceous-Tertiary impact. Some colleagues working in the Scottish Highlands on basaltic ring-dykes with ages of 65 million years half jokingly suggested that they may have something to do with the terminal Cretaceous impact.

W. Alvarez: It is a curious coincidence that there is an extensive volcanic pulse in India, the age of which is essentially indistinguishable from 65 million years.

McLean: There seems to have been a general increase in mantle circulation and opening of the North Atlantic at the time of the Cretaceous-Tertiary extinctions. Iceland has volcanics which are relatively rich in alkalis. However, can these lavas be related to a meteorite impact?

W. Alvarez: These lavas are all much younger than the extinctions.

McLean: Schilling (1973) makes a case that they are the result of plume activity, and related to a general opening of the North Atlantic. I think that we are in agreement that Iceland is not a good choice for the site of meteoritic impact.

W. Alvarez: There is no evidence to indicate anything other than a normal continuation of the process of the opening of the Atlantic. Rockall Bank itself is separated from the British Isles by what appears to have been an aborted rifting attempt. The processes were clearly active before the Cretaceous-Tertiary extinctions.

Hsü: Icelandic volcanism differs from mid-ocean ridge volcanism in that the light rare earth elements are less depleted. They are supposedly derived from mantle sources which were less differentiated than those of other oceanic basalts. Icelandic volcanism has continued to the present.

W. Alvarez: In comparison with the angular momentum of the Earth or the energy transport involved in plate tectonic overturn, an impact is in the level of background noise. However, Dr. Seyfert (personal communication) suggests that the mantle may itself be in a metastable condition, so that meteorite impacts could be triggers of much larger energy transfers.

Hsü: Hot spots do not stay stationary with respect to the plates. If one was created as the result of an impact, its volcanic product would not have come out at the same point of a moving plate during the last 65 million years.

W. Alvarez: Deccan Trap volcanism began as close to the extinctions as time can be measured. One thousand meters of plateau basalts, including many flows, were extruded within no more than four and possibly as few as two polarity zones. At that time these zones were of about a half-million years duration, so that the volcanic episode lasted only one to two million years. One would expect that melts of that sort would be very undifferentiated chemically, as are the Columbia River basalts, but there are substantial amounts of rhyolite and other chemically differentiated volcanic rocks in the Deccan Traps. There may be an incongruity between the time required for differentiation to occur and the duration of the entire episode. Perhaps this is evidence that the material did not simply flood out of a hole suddenly created in the crust. Iceland is not the site of an impact. The Deccan Traps might be, but a strong case cannot be made for it at present. Perhaps the most important conclusion might be that something caused a volcanic-tectonic disturbance about 65 million years ago.

Hsü: Were the Deccan basalts deposited on a flat surface?

W. Alvarez: That would appear to have been the case.

Hsü: Can the occurrence of craters be excluded?

W. Alvarez: The pre-Deccan Trap surface has not been sufficiently well explored. It extends over a larger area than a one- or two-hundred kilometer crater. The 65-million-year level in adjacent oceanic areas has not yet been penetrated by D.S.D.P. cores.

Feldman: Where were the Deccan Traps 65 million years ago?

W. Alvarez: They were probably south of the equator.

Thierstein: How do they compare volumetrically with the Columbia River basalts?

W. Alvarez: I believe they are substantially larger.

McLean: The Deccan Trap lavas are today concentrated in western and central India, and cover about 500 thousand square kilometers. Along the west coast near Bombay they are up to three kilometers thick. Isolated outliers of basalt on the east coast of India suggest that before erosion the lavas probably covered at least a million square kilometers. Pascoe (1964) considers basalts in Baluchistan to be the same age. Reunion, a volcanic island east of Madagascar, is considered to have been the hot spot then. Submarine volcanism occurred in what is now the Arabian Sea. The lavas in western and central India today are only a fraction of the total volcanism of that time. The Brito-Alaskan flows may be younger, but extensive volcanism may be involved in the Cretaceous-Tertiary transition scenario.

W. Alvarez: Some of the volcanic outliers in eastern India are of late Jurassic or early Cretaceous age.

McLean: India was drifting very rapidly at a rate of up to 17 to 18 centimeters per year. Morgan (in press), and Chandrasekharam and Parthasarathy (1978) suggest that convection from under the Indian plate facilitated its rapid drift to the north. Mantle-plume activity may thus have been partly responsible. India does seem to have been domed up, and the lavas emerged from fissure and crack systems. The lavas were quite fluid, and individual flows can be traced for 60 or 70 miles. Eruption was not continuous, nor would I postulate that the extinctions spanned the entire five million year period of Deccan volcanism. Massive outpourings may have coincided with the early stages of this volcanism. Deep origin volcanism could bring large quantities of primordial CO_2 to the surface (Wiley and Huang 1976). Because latitudinal temperature gradients were less, oceanic circulation would have been slower during Cretaceous time than at present. Carbon dioxide would also have been taken up more slowly by the oceans. Volcanic CO_2 would accumulate in the atmosphere and then, by diffusing into the oceanic surficial water, produce a lowering of pH.

Calcareous planktonic organisms indeed were severely affected in the terminal Cretaceous extinctions. Perhaps the siderophile enhancement does reveal information on mantle processes. The plumes may be cited as a potential source of the siderophiles, through which platinum-group elements were transported to the crust where they would be scattered by volcanic exhalations into strata of about the same age as the extinctions. I postulate that the primary source of biotic stress was a global warming. There is a drop in ^{18}O values across the contact that can be taken as a warming signal. Volcanism began approximately contemporaneously with the marine extinctions. There is some evidence that the extinctions of so-called Cretaceous organisms in the marine realm spanned several tens of thousands of years. Magnetostratigraphic techniques enable us to correlate more precisely terrestrial and marine sediments. Butler and others (1977, 1978, 1979), basing their interpretations on these techniques, would have the extinction of the dinosaurs occurring at least a half a million years later than the marine extinction. Perhaps a progressive accumulation of CO_2 in the atmosphere had by then resulted in a warming sufficient to disrupt reproduction among the large reptiles. Late Pleistocene mammalian extinctions may have also occurred as a result of an abrupt postglacial warming and disruption of reproductive physiology. Many modern reptiles in desert areas cannot reproduce during the summer because with the increasing heat, reproductive tissue degenerates. So with a global warming, dinosaurs would progressively experience reproductive problems. Erben and his associates (1979) showed progressive thinning of dinosaurian egg shells from below the boundary up to the Cretaceous-Tertiary contact. Interestingly, during hot summer months the shells of chicken eggs begin to thin until the hens stop laying (Wolfenson *et al.* 1979). The extinctions can be seen as a slow, progressive phenomenon, particularly if the abrupt termination of ranges is due to hiatus control as a consequence of a regression of the seas.

W. Alvarez: The regression is a valid explanation for simultaneous terminations of ranges in shallow water sediments, but at a depth of two thousand meters in the oceans a hiatus need not occur. If there is a gap in the record of deep-sea sediments, it is extremely short. The geochemical anomaly, magnetic stratigraphy, and calculated sedimentation rates are strong evidence that if a break does occur in sedimentation, it is very short. The enormous effect of a regression in shallow water will not be there.

McLean: In the Indian Ocean there are deep water hiatuses that span several tens of millions of years.

Hsü: Yes, but they are not relevant to the problem because they do not coincide with the Cretaceous-Tertiary boundary.

Russell: The biostratigraphy of the dinosaur egg shells in southern France has not been well studied. No one has attempted to examine carefully the distribution of different egg morphotypes through the lithostratigraphic units. Changes have been seen near the boundary; what kind of changes occur lower in the section? The data available are not of the same quality as microfossil data from pelagic carbonate sequences, such as those considered earlier. The surface-volume proportions of dinosaurs have generated some discussion on how much activity they could sustain and still balance their thermal budgets. With regard to a warming-trend through the Cretaceous-Tertiary boundary, it seems likely that in the case of gradual changes refugia might persist in some areas of the globe which would be climatically compatible with the survival of some dinosaurs.

McLean: In the late Pleistocene mammalian extinctions, refugia were created in the general warming of the globe because all areas did not warm equally. With a CO_2 increase, the Northern Hemisphere warms more than the southern, and certain regions of North America and northern Eurasia warm far above the Northern Hemisphere average. With respect to the definition of the Cretaceous-Tertiary boundary, can we draw precision from imprecision? The crux of the issue is that these secondarily-truncated ranges have been defined as the top of the Cretaceous. In a time sense the K-T boundary would have been higher in the section. Then a clay is deposited on this truncated surface, which has nothing to do whatever with the true extinctions at the end of Cretaceous. Let us postulate a variety of ranges of organisms in time whose remains are preserved in shallow-marine sediments. The seas then retreated to their basins, and the sediments were removed through erosion down to the new base level. All that remains in the record are a series of decapitated ranges.

Russell: Is there any place on Earth where we should look for a complete section?

McLean: On the basis of some recent drilling, some zones were apparently discovered which have not been identified before. This would indicate that some of the sections that have been studied are not complete. It is known that a large volume of rock was stripped away after the great terminal Cretaceous regression in many areas.

Smit: Where is the large volume of rock that has been stripped away now? It must be deposited somewhere.

McLean: All along the east coast of North America are great thicknesses of Mesozoic and Cenozoic sediments that came from the erosion of the continent.

L. Alvarez: Dr. Butler's observation that the dinosaurs disappeared in a positive magnetic sequence and the marine organisms disappeared in a negative magnetic sequence clearly implies that these extinctions were not synchronous. This point was addressed in our paper. I recently spoke with Dr. Butler on this important problem. He indicated that he is reviewing the evidence and is no longer certain of his earlier conclusions. I believe that he would say that if he were here.

Asaro: With respect to the enormous amounts of material that are presumed to have been eroded from all of the boundary sections that have been measured, the abundances of the rare earth elements can be measured with high precision and their ratios do vary somewhat from point to point. In one section in Italy the abundances just below and just above the clay layer could not be distinguished from each other using the highest precision techniques available. We could not measure the time that elapsed between the deposition of the sediments sampled, but there could not have been an enormous amount of missing or eroded rock.

McLean: In many, but not all cases, there is a considerable hiatus in terrestrial sediments. In one section on the east coast, Upper Cretaceous strata are absent and Tertiary deposits lie on Lower Cretaceous strata. Thus, in many regions a hiatus of often indeterminate magnitude can be identified in marine sediments. In response to any phenomenon all parts of the world do not change exactly the same way. For example, calcium carbonate dissolution occurs at certain depths, which may change with latitude. It would be very useful if a section could be found across the boundary that could be proven to be complete.

W. Alvarez: Every responsible stratigrapher is well aware of the effects of erosional hiatuses. Anyone who is concerned with the Cretaceous-Tertiary extinctions thinks about it constantly. It is very difficult to imagine any way by which a sequence can be shown to be absolutely complete. It is clear in a number of sections, particularly the one at Caravaca, that if any time is missing it is less than the thousands of years range. This is all we can hope for.

McLean: In some localities in Europe, the end of Cretaceous has been defined by marine extinctions. The base of the Tertiary is defined by the appearance of *G. eugubina*. Between these levels is a time-gap which should be discussed.

Hsü: How are we to define an incomplete section? By the presence of a 10-day hiatus or a million-year hiatus?

Smit: The clay layer at Caravaca was deposited in 2,500 to 10,000 years.

Hsü: According to minimal pelagic sedimentation rates, it is highly unlikely that this clay layer represents more than two million years.

McLean: Was it deposited uniformly from top to bottom, or was it dumped in as a storm deposit with the top few centimeters representing a few millions of years?

W. Alvarez: An argument can be made from paleomagnetic stratigraphy. We know that all of the reversed and normal polarity zones are recorded at Gubbio because the match to the seafloor spreading record is unmistakeable. The duration of each of these polarity zones is known with some degree of accuracy. We also know how thick they are in the section at Gubbio. A rate of sedimentation can therefore be calculated. These rates can vary by factors of three from zone to zone, but not by factors of 10. If the Cretaceous-Tertiary boundary is assumed to represent more and more time, then less time will be represented by sediments above and below it in the reversed magnetic polarity zone that contains the boundary. Sedimentation rates are driven up to approach unacceptable values. Yet the sediments in adjacent polarity zones remain similar, with identical nannofossil matrices containing 5 to 10% foraminifera. There is no change in lithology to suggest that sedimentation rate changes of major magnitude occurred.

L. Alvarez: It has been stated that a general environmental change need not produce uniform sedimentological consequences all over the world. However, the iridium anomaly has a world-wide distribution, with only minor differences between localities.

McLean: It could be interpreted as evidence of volcanism.

Hsü: Where are the data showing trace element similarities between the clay layer and the Deccan volcanics? This question has already been posed, and the materials are shown to have different trace-element chemistries. Unless new data are recovered which prove that the other data were wrong, the question need not be discussed further. What is a large amount of CO_2? How many megatons of CO_2 would be introduced into the atmosphere by Deccan volcanism, and how does this amount compare to the amount of CO_2 already present in the atmosphere? The earth sciences today are more quantitative than in the 19th century, and unless we can speak in terms of numbers our arguments lose their force.

McLean: The linking of volcanism to the Cretaceous-Tertiary transition scenario is new, and the consequences are not yet as well known as they would be if the issue had been under consideration for some time. Has anyone examined the strontium 87 to 86 ratios in the boundary clay? In the Deccan volcanics the ratios are low, but at least some chondritic material has a relatively high strontium 87 to 86 ratio.

Thierstein: We have examined the thicknesses of the biostratigraphic zones in various carbonate sections. The sedimentation-rate in any one section seems to be reasonably constant because the individual biostratigraphic events tend to be spaced in proportion to the sedimentation-rates calculated for the total thickness of the section. Parenthetically, sedimentation-rates would have to be corrected for density and porosity differences between deep-sea and epicontinental sediments in order for the numbers to be comparable. There is no indication of a large or disproportionate gap in the record in any of the sites considered, which are the best ones known. The quality of preservation of coccoliths does not decline across the boundary (Thierstein, in press A). The decrease in the carbonate content of the sediments is presumably due to a decline in the productivity of calcareous phytoplankton. The preservation of coccoliths in many earliest Tertiary (basal Danian) assemblages is actually better than in the lastest Maastrichtian.

McLean: Perhaps pH changes in the ocean depressed phytoplankton productivity.

Thierstein: The preservational patterns we observe in recent coccolith assemblages from deep-sea sediments are not determined by pH in the surface waters, but by the dissolution gradient in the deep-sea. The Cretaceous-Tertiary boundary is not associated with a dissolution event in the Atlantic Ocean, although it may be in the Pacific. However, it is often difficult to distinguish a dissolution event from a diagenetic feature. Dissolution in low carbonate, high organic carbon sediments of middle Cretaceous age has been shown by imprints of coccoliths, which are no longer preserved, in black shales (Thierstein *et al.*, in press). Many of the preservational changes observed in the Mesozoic record are probably diagenetic rather than primary.

McLean: In volcanic eminations,carbon gases are second in abundance to water-vapour. Carbon dioxide could enter the water by simple diffusion over a period of time, independent of movements of the carbonate compensation depth.

Thierstein: Two additional points can be made in reference to changing the CO_2 content of the atmosphere. The first is that living coccolithophorid cultures can be placed in a sea-water medium with a pH as low as five (Wilbur and Watabe 1963). The effect on *Emiliania huxleyi*, for example, is that it will shed its coccoliths and survive naked for as long as two to three years. If the coccolithophores are returned to higher pH environments with nutrients they will produce coccoliths as formerly. A second point is that it is unlikely that much CO_2 can accumulate in the atmosphere without it ultimately entering the ocean. This would produce a global dissolution event in basal Tertiary sediments which is not seen in some sections.

Hsü: In the drilling on the Walvis Ridge, it was very easy to locate the Cretaceous-Tertiary boundary because it coincided with a red clay. Perhaps the change in sedimentation is due to a decrease in coccolith productivity, but there are also indications of increased dissolution. At D.S.D.P. site 356 the red clay is absent, as was one of the nannozones across the Cretaceous-Tertiary boundary. Dr. Perch-Nielsen insists that the zone should have been there, although you disagree. There is a possibility that the zone and the red clay were removed by mechanical erosion. I do not think that you can say that there is no indication of increased $CaCO_3$ dissolution at the end of the Cretaceous.

Thierstein: Most sedimentary sequences show a change in lithology which is related to a carbonate decrease. This can be achieved by decreasing the supply of carbonate from the photic zone or by increased dissolution. The preservation state of individual fossil assemblages often improves across the boundary.

Hsü: If a concentration gradient exists in which the water is greatly undersaturated, sediments tend to dissolve at the water-sediment interface. In most of the D.S.D.P. sites, the Cretaceous *Globotruncana* have undergone a great deal of dissolution. There is thus evidence of increased dissolution in the sea floor at the end of the Cretaceous, but conditions were improving when the Tertiary began.

L. Alvarez: Two separate arguments have been presented that the material in the boundary layer is extra-terrestrial in origin; one in our original paper, and another by Ganapathy (1980) and by Asaro earlier in this conference. I would like to ask Dr. McLean in what way he feels these arguments are invalid.

McLean: I have approached the Cretaceous-Tertiary transition from the point of view of trying to understand a broad spectrum of Earth processes. This issue could be addressed without discussing meteorites or siderophiles at all, but I want to be complete. The dinosaurs can be seen as disappearing in a climatic change which is related to very broad and sweeping mantle processes, which have been in operation throughout geologic time. These mechanisms may provide an alternative source for the siderophilic enhancements. At this stage it is wise to consider as many alternatives as possible, and keep examining the data.

L. Alvarez: Two separate pieces of data have been cited which exclude some material in the boundary clay from being derived from volcanos. In what way is this incorrect?

McLean: The distribution of siderophiles throughout the entire Earth is not well known. Deccan volcanism is real. It began at almost precisely the same time that the marine extinctions began. Fluctuations in the carbon-cycle may have produced the observed paleontological scenario. Basaltic ultrabasics, particularly those from greater depths, are generally rich in siderophiles. If the Deccan volcanism resulted from mantle-plume activity, perhaps the siderophiles have an origin from one or several levels of the mantle. A mantle-plume mechanism is proposed as a terrestrial alternative that is worthy of consideration.

L. Alvarez: Why is a mechanism that has never been observed before to be preferred over one concerning which much information exists and which would produce the observed siderophile signal?

McLean: I cannot say that a meteorite did hit the Earth. I cannot say that a meteorite did not hit the Earth. Its role in the extinctions needs to be explored. The role of massive volcanism needs to be explored. The role of massive flood basalt volcanism in climatic change needs to be explored. The siderophiles are incidental.

Asaro: Do you think it would be useful to carry out chemical analyses of the basalts of the Deccan Traps?

McLean: It could well be if the right ones were studied. In some volcanic episodes the siderophile-enriched materials seem to come up as an early pulse. I have no figures on the platinum-group elements in the Deccans.

Grieve: From the point of view of cratering statistics, the possibility exists that a large impact event could have occurred 60 to 70 million years ago. As far as the geochemical anomalies are concerned, from the patterns of siderophile enrichments in material associated with impact craters, it should not be a matter of concern that the siderophile fingerprint is slightly different from site to site. Projectiles vapourize on impact and fractionation takes place in the vapour, as well as in various chemical interactions with the target materials. Fractionation will be better understood when the projectile and target materials are more accurately known. A projectile of the size proposed will not be affected by the ocean in a physical sense, although water will be thrown into the atmosphere. The volume of material excavated relative to the volume of the projectile varies with impact conditions, but is probably on the order of several hundred to a thousand times. The bulk of that material is thrown out of the crater in ballistic trajectories and forms an ejecta blanket, which is a continuous cover out to two crater radii, although some ejecta may extend out as far as 10. This material originates from a position which is relatively distant from the projectile and contains little or no projectile component. Of more interest is the material that is ejected as the projectile is penetrating the ground. As a projectile enters the ground, the flow-field is partially directly radially into the target but a component is also directed upwards. The latter material contains a large projectile component. At this point the projectile is a super-heated vapour, with a temperature perhaps as high as 10,000°C. The proportion of projectile to target material in the vertical flow-field varies between 0.01 to 1.0 with impact conditions (O'Keefe and Ahrens 1981). This is the material that would reach high levels in the atmosphere, and could be deposited as a global clay layer. Thus there may be no dilution problem with respect to an asteroidal impact model, if only this initial high speed ejecta is considered.

Kyte: We were concerned about this dilution problem, and tried to devise a few alternative methods to obtain the enrichments of 20% which were observed in the Danish section. One hypothesis involved an asteroidal core which will also have chondritic noble metal abundances. In this respect it is difficult to determine whether or not a chondritic or an iron-core object were involved. There appears to be a chromium enrichment in the horizon and chromium is normally depeleted in iron meteorites. If a chromium component were identified, perhaps an iron meteoroid source could be ruled out. Another alternative hypothesis involved an interstellar dust cloud. We came to the conclusion that the siderophile anomaly would be distributed through too great a thickness of sediment, and left that question open. Our third hypothesis involved an accretionary event without an impact. We suggested the possibility of tidal disruption of cometary material and accretion of numerous small fragments, which would be destroyed in the upper atmosphere and produce little or no ejecta. The data do not uniquely constrain a chondritic asteroid impact, but an accretionary event must have occurred.

We have identified an iridium anomaly in an Antarctic core which is only a few million years old. The noble metal carrier in that horizon is apparently chondritic material and there is no evidence of impact ejecta in that horizon. These particles are quite unstable in the marine environment, and only large particles have been recovered. All have severely etched surfaces, and if this horizon were to survive for 65 million years these particles would be completely dissolved. The primary carrier of the noble metals would no longer be recognizable. Perhaps similar material served as the carrier for the Cretaceous-Tertiary material. We are examining some microtectite horizons which are believed to be derived from ejecta, but have not found associated iridium enrichments. It would be useful to further constrain these alternative hypotheses. An advantage of a tidal-disruption model is that the accretionary event would probably last for a period of weeks to months. Virtually all of the energy available would be transmitted to ecosystems, rather than being buried by an impact, so the extinction effect may be enhanced.

Thierstein: I have been wondering about alternative processes to enrich these sediments with noble elements, and would like to pose three questions. This first consideration is whether these enrichments could be a consequence of the mass extinction? If some of these elements are enriched in present-day biomass in similar proportions, then the extermination of that biomass would produce similar enrichments in the sediments. Does any evidence exist to rule out such a possibility?

Rucklidge: In the process of studying the iridium anomalies in the Danish section we made various separations. Silvana de Gasparis prepared some organic extractions and these were measured directly using the tandem accelerator-mass spectrometer method. The organic extractions had extremely high platinum and iridium values, in the parts per million range. We suspected that, in the process of extraction, platinum metals were also concentrated into this organic fraction because of the chemistry involved. Using physical methods for extraction the same type of enrichments were again detected. However, only a small fraction of the total amount of iridium present in the section could be contained in the organic fraction.

Thierstein: In this example the iridium is related to organic material still preserved. If all of the organic material had decayed, the residue would contain the element enrichments. A second kind of consideration is: could the enhancements be related to inorganic processes caused by oxygen deficiency, since noble elements tend to be enriched in sulphides (Wedepohl 1970)? We (Thierstein and Berger 1978) have speculated on the possibility of developing an oxygen-minimum layer below a freshwater lid. There has been a debate concerning an absence of pyrite. However, heavy metals are precipitated from the water-column during anaerobic periods, and buried. When oxygen is recirculated into the water-column, metal sulphides may oxidize, so that an absence of pyrite does not necessarily imply the absence of an anoxic event. The possibility still exists that there was an oxygen deficiency at depth across the Cretaceous-Tertiary boundary. Present-day anoxic environments could thus be examined to see if a mechanism exists whereby noble elements are precipitated from seawater inorganically. The third consideration is: if an asteroid impact provides a mechanism for the extinctions at the Cretaceous-Tertiary boundary, what makes that mechanism so unique? The extinction in my opinion remains unique both in magnitude and in timing, although it is true that less is known of the other mass extinctions. If an asteroid impact has such devastating effects, where are the paleobiologic effects of smaller, but more numerous impacts? This question may in part be answered by finding an iridium anomaly at another time-interval. If the iridium concentration at 2.3 million years occurs in other localities, where are the biotic extinctions? It must be deleterious for some of the phytoplankton if only 60% of the sunlight were blocked.

Asaro: Concerning the first point, could there actually be less organic material associated with the extinction event in the sedimentary record? An extinction would depress production, and reduce the concentration of an element that might be deposited with the organic material.

Thierstein: This is a question concerning global cycles of various elements. How much carbon is used up at any one time in the biotic reservoir, and how much leaves the reservoir into the sediments? We would have to make some assumptions on what the biomass was at that time and what the flux-rates were. For simplicity, perhaps the present-day oceans should be considered as an analogue, the amount of oceanic biomass measured, and if this were destroyed, the trace element signal buried in the sediment should be calculated. If the carbonate decrease across the boundary is due to a productivity decrease rather than a dissolution event, then the carbon that was formerly in the biomass, assuming that the biomass was considerably smaller after the extinction event, must be dissipated. A chemical trace should occur in the sediment.

Russell: The biomass of dinosaurs prior to the extinction event was probably about one one-hundreth that calculated for an impacting asteroid.

Béland: The fact that biomass is not accumulating can simply imply that most of the additional biomass produced in one year by all sources is used up by the biomass itself. Individual growth and births equal total deaths, although production is continuing. The productivity is consumed to be used as kinetic energy or metabolism, and transformed into heat.

Thierstein: This is not quite in agreement with global steady-state model assumptions where the carbon supply into the ocean is held to be approximately equal to the carbon that leaves the ocean reservoir, both in carbonate and in organic carbon. My understanding is that the annual carbon supply is relatively small compared to existing biomass.

Hsü: The annual carbon supply is 8.6×10^{14} grams of carbon atoms and the present ocean has 3.5×10^{19} grams of carbon atoms in dissolved carbonates (Hsü *et al.*, ms.). With respect to your first two considerations, why should either organic or inorganic precipitation of platinum-group metals produce ratios similar to those of chondrites? It would be too much of a coincidence. With respect to your third consideration concerning the uniqueness of the Cretaceous-Tertiary extinction, I suggest that the uniqueness may be explained by assuming cometary instead of asteroid impact. The three agents of stress which have been postulated are dust, poison and heat. Perhaps only a comet would provide enough poison. How do particular groups of organisms differentially react to dust, poison and heat? The record of these groups across the Cretaceous-Tertiary boundary might reveal which stresses were more lethal during the interval of crisis. A cometary poison model might be unique because comets do not impact as often as do stony meteorites. Further, toxic materials in asteroids may be largely buried as a solid body after the impact, while cometary material could more easily be dispersed to some extent by the atmosphere. Heavy metals could form compounds soluble in seawater which could be recycled, and become agents, more toxic than cyanide.

Again, experiments should be carried out on the toxicological resistance of particular groups of organisms and compared to the record of the group through the extinction interval. For example, it is very difficult to locate the most important extinction-interval across the Cretaceous-Tertiary boundary on the basis of some groups of echinoderms. Are they equally resistant to all of these poisons and to agents of physical stress? More attention should be given to the biological patterns of extinction.

L. Alvarez: At the Radiation Laboratory in Berkeley, when large amounts of radioactivity were first available, we became very interested in the problem of surviving radiation. When radiation dose (or biological insult) is plotted against survival, the curve is initially nearly flat, but then declines sharply through the point of a 50% lethal dose (LD50). The curve then flattens out into a very long tail, like that of an exponential curve. When the LD50 is exceeded by a number of probable errors or standard deviations, and then exceeded by twice that, the number of survivors declines by an enormous factor. This may be a key to understanding why the Cretaceous-Tertiary extinctions were so severe. Had the stresses been twice as severe we would not be here to talk about them. For example, a large population of organisms will produce survivors as long as it is not depleted below a thousand members or whatever number is required to reproduce sexually. A controlling factor that decides whether a species survives or not may be whether or not its population, multiplied by the fraction that can survive a given insult, exceeds this critical number of one thousand.

Brasch: The LD50 can be defined quantitatively. However, it is also important to determine consequences to the food-chain. Applying these statistics to a mixed population of interacting species, many can easily survive a given insult, but if a key primary-producer cannot, then the others will ultimately be removed by starvation. Thus, if a global darkness is extended beyond a certain time, then the food-chain is destroyed, LD50s notwithstanding. If the darkness is too short there will be no long-term effect.

The action spectrum for photosynthetic activity under given models of atmospheric darkening should be studied because some photosynthetic organisms will react primarily to red or far-red light, others to green, and so forth.

L. Alvarez: Whether the stress is due to darkness, cyanide or some other agent, the survivorship curves tend to be similar. If the Cretaceous-Tertiary insult were at such a level that 20% of the genera survived, and then this insult were doubled, then nothing would survive. Doubling the size of an asteroid implies changing the diameter only by the cube root of two.

Brasch: In connection with a toxicity model, if we envisage toxins like cyanide and heavy metals such as nickel and osmium, which are severe metabolic poisons, it again becomes difficult to rationalize the heterogeneous extinction pattern observed. At certain levels the common metabolic enzymes of all organisms would be affected.

Jaworski: This may depend on the environment in which an organism resides. The availability of a particular toxin will depend on the chemistry of the aquatic or terrestrial environment. Also even among fish species, differences in sensitivity of two or three orders of magnitude can exist in LD50 to the same toxin in the same water.

Brasch: This is true, but an upper-limit can be assigned nonetheless to the concentration of a given toxin in the oceans if any species survives.

Hsü: In the case of poisons, selectivity can be geographic as well as physiologic. In the marine realm, poisons could be concentrated in surface currents. The areas of equatorial current convergence should thus be peculiarly vulnerable to large-scale poisoning (Hsü 1980). Assuming poisoning, organisms not requiring photosynthesis can be eliminated without invoking starvation. The total darkness does not seem to be flexible enough in its effect to explain the selective extinction.

McLean: Terrestrial plants do not generally seem to have been greatly affected in the extinctions, in contrast to calcareous microplankton in the ocean. How do the effects of a meteoritic impact discriminate between these two groups?

Russell: Terrestrial palynofloras of late Cretaceous age often include on the order of 200 species. Tschudy (1971) has demonstrated a decline to on the order of one-third Cretaceous levels in Montana, and to about one-half in the Mississippi embayment.

McLean: Some of those declines were taking place before the end of the Cretaceous. The event at the Cretaceous-Tertiary contact was of short duration. There was a decline in global temperature, throughout late Cretaceous time, the effects of which can be seen in terrestrial floras.

Jaworski: Perhaps there were some organisms in a decline that were eliminated in the extinction event.

McLean: At the Cretaceous-Tertiary contact calcareous phytoplankton were more severely affected than terrestrial floras.

Hsü: Microplankton could have been destroyed by chemical poisoning. The extinction could alter ocean chemistry to the point of changing the solubility of some heavy metals, increasing the toxic effect. Why were calcareous organisms more affected? Perhaps a change in the CO_2 content in the ocean resulted in a stress environment for organisms which secrete calcareous skeletons.

McLean: How is a poisoning of the water compatible with the generic diversity figures cited in Table 1 of, for diatoms, 10 before and 10 after; for silicoflagellates, 4 before and 3 after; and for radiolarians many of which live in the photic zone, 63 before and 63 after?

Hsü: Diatoms tend to populate cooler waters more densely. Poisons would be concentrated in the equatorial currents.

McLean: Diatoms are caught by the billions in plankton nets off South Carolina.

Hsü: Those areas in which diatoms suffer only partial mortality would provide a source of migrants into disaster areas. Those you mention do not secrete $CaCO_3$.

Jaworski: Each of the groups mentioned has a mineral component in its exoskeleton.

McLean: In diatoms it is opaline silica.

Jaworski: Shrimp and various other organisms can concentrate many elements in the exoskeleton, thus reducing concentrations in the cytoplasm and reducing the possibility of toxicity.

McLean: Does cytoplasm not communicate with the seawater through the pores in diatoms?

Jaworski: There are freshwater algae that are able to survive rather high levels of heavy metal concentration by concentrating the metal in the cell wall.

McLean: How can an organism concentrate material in its wall, when the wall construction is due to protoplasmic activity?

Dilcher: Individual cells can pack the vacuole full of metabolic wastes. If differentially permeable membranes are not denatured by toxins, the organisms (plants) can usually cope with them.

Asaro: Mechanisms of extinction are likely to be discussed in increasing detail during the next few years. It would be very useful to have a detailed list such as the one discussed here (Table 1), but more expanded to show the numbers of species which became extinct, and where they became extinct. For those species which did not become extinct, but whose populations renewed themselves, as a function of geography, the information might be of assistance in locating the impact-site of the object that caused the extinctions.

McLean: To record a species diversity of 40, for example, below the boundary, and 40 above does not clarify the problem if all of the species are different. There is a need for more detail.

Russell: In Table 1, generic turnover across the boundary is often masked, or diversity decreases are obscured, by a rapid post-extinction diversification.

Béland: Assuming that such a list of species were available, and that toxicity levels were determined in related living organisms, how could one be confident that the latter results could be applied to the more ancient species?

Jaworski: It would be necessary to argue by analogy. The chemistry of the aquatic situation at the close of Cretaceous time is not known in sufficient detail. An overriding consideration is that whether at a particular pH, carbonate or chloride concentration, the uptake of toxins by organisms is permitted.

Béland: Complex organisms, such as vertebrates and large molluscs, show very diverse reactions to given products.

Jaworski: Perhaps what was suggested was that two separate groups of organisms be identified that are quite different in their physiological characteristics, and then used as test cases in relation to the biological insults which may have been operative at that time.

Béland: Perhaps some priority should be given to the primary-producers, because of the possible implications on food-chain. The reaction of unicellulars may be more constant than that of the higher invertebrates. For example, forams today and forams living 30 million years ago, belonging to the same taxonomic categories, would probably react to a stress in essentially the same way.

Thierstein: When it became apparent to me that an instantaneous extinction of phytoplankton occurred at the end of the Cretaceous, I was initially disappointed because models of ecologic succession or phyletic gradualism could not be applied to the paleontological phenomena. Knowing that four or five species were present in earliest Tertiary strata, an obvious question was what their ecological preferences were during late Cretaceous time. The data I have assembled on late Cretaceous biogeography may be the most complete available because of sample-sizes including thousands of fossils (Thierstein, in press B). There is a tendency for coccoliths, which are typical of early Tertiary time, to occur in late Cretaceous sediments in higher latitude sites rather than tropical sites, supporting the thesis that whatever lived in high-latitudes in the marine environments was somewhat better adapted to the extinction than forms living in low-latitude environments. The relative abundances of Tertiary taxa in the late Cretaceous sediments, assuming no contamination, are on the order of a tenth to a hundredth or less of a percent, making it impossible to discern an evolutionary pattern such as one of phyletic gradualism. The same applies to the appearance of

Emiliania huxleyi, which is the youngest coccolithophorid living today (Thierstein *et al.* 1977). It appeared in the geographical record about 250 thousand years ago and remained in incredibly low abundances for 80 to 100 thousand years. Then it began to bloom, and this is when it would conventionally be found in the geological record. The point is, with an almost perfect fossil record and large sample sizes, it is not possible to determine what the diversity of the earliest Tertiary phytoplankton was in the latest Cretaceous. Even their point of origin cannot be determined because they are initially so incredibly rare. How can one be sure that all of the species of macrofossils have been recognized with small sample sizes? Even if a list is made on a species level, true changes in diversity may not be apparent.

Hsü: Perhaps high latitudes were a Cretaceous refugium, from which survivors could migrate into new niches after tropical waters were detoxified. There they could bloom vigourously, because the competitors were gone.

Thierstein: Perhaps they were adapted to darkness because of high-latitude seasonality.

Hsü: In any event, evidence concerning biogeographic provinciality may be more important than that relating to diversity changes.

Russell: The *Aquilapollenites* province seems to have suffered the greatest diversity declines, although it occurs in high latitudes. If *Aquilapollenites* were parasitic, it was a boreal parasite, although existing vegetational gradients suggest that epiphytic plants are more common in the tropics.

Dilcher: There is no question that some species extinctions and/or replacements occur across the Cretaceous-Tertiary boundary. But when examined on a generic or family level these changes are masked, so perhaps a closer look at this boundary from several different botanical aspects (e.g. leaves, pollen, wood, fruits and seeds) is justified. Much of the plant evidence is not based on an entire organism. Some parts of angiosperms are more diagnostic than others. There is an ongoing effort to understand the relationships of organs across time boundaries. Techniques such as palynology, detailed leaf analyses and cuticular analyses have helped to resolve these (Dilcher 1974). Crepet (1981) presents an interesting scenario where the earliest flowering plants have generally been assumed to be insect-pollinated, and we see a radiation of wind-pollinated plants following the Cretaceous-Tertiary extinctions. He has proposed that there was strong competition among plants for insect pollinators. This could have been a result of the extinction of insects in terrestrial environments during the terminal Cretaceous event, or as Crepet suggests, the fact that they hadn't radiated or were not abundant. Surviving plants became facultatively, and later obligately, wind-pollinated.

Russell: It is in the quiet air on the forest floor that plants tend to rely more on insect pollination. Would this imply that forest herbs suffered more than emergent trees?

Jarzen: In the tropics, mode of pollination does not depend on an arborescent versus herbaceous form. It depends on the spacing of plants and on wind velocities at the top and in the lower canopy.

McLean: Tectonism was involved in Montana, Wyoming and Colorado, and one would expect insects to migrate in and out of areas in response to changing environmental conditions.

Dilcher: Crepet does not raise the possibility of catastrophic events playing any role in the extinctions. He points out that there is radiation of wind-pollinated forms at this time, perhaps in relationship to migrations of plants into seasonally dry habitats and temperate areas.

Hsü: Terrestrial organisms were beyond the effects of poisoned seawater, but may have been more affected by temperature changes. Temperature changes would in turn affect environmental conditions in polar regions far more than in tropical regions. I can imagine the possibility of latitudinally different extinction patterns in terrestrial and marine organisms.

Jarzen: The possible duration of dormancy in seeds is also a factor to be considered. The forms growing in the western interior of Canada were ecologically more similar to living tropical rather than temperate species. Germination studies of northern plants such as commercial trees and agricultural crops are available, but information on tropical trees is difficult to obtain. Ng (1978) studied germination patterns and timing in 180 species of Malayan trees, or about 4% of the tree-species in Malayan forests. Of these, 65% would germinate within an average of 12 weeks, 7% within 22 to 68 weeks, and 28% within 6 to 27 weeks. Of those germinating within the 22 to 68 week interval, some took as long as 158 weeks, or approximately three years, for germination. Thus a period of darkness lasting up to a year might be compatible with the survival of the seeds of some species but those of others would have become non-viable. If the cloud of dust was circulated more rapidly, perhaps because of turbulence following the impact, and if 10% of the light returned within a one year interval, then many tree-species may have survived. In a rain forest near Leticia, Colombia, I found that the darkest portion of the forest floor received approximately 10% of the light falling on an open area near a stream bank. Plants such as *Selaginella*, mosses and liverworts were growing in the dim light. These measurements were casually made, but they do suggest that many plants could survive under conditions of low illumination for periods of time of up to one year.

Dilcher: I know of no well-described terrestrial sequence across the Cretaceous-Tertiary boundary in a tropical, low-latitude area. Trees in tropical areas experience even temperatures and only small changes in day length. Many of them are evergreens; they can flower at any time, and the seeds usually germinate and grow when dispersed. Plants that live at high latitudes often have deciduous vegetation, experience months of long darkness and have physiological mechanisms to delay seed germination. I would predict that the boundary effects in tropical areas, if the stresses were the result of a dust model, would be more drastic than in high-latitude areas.

Pirozynski: The suite of adaptations that promoted survival may offer a clue as to what happened during the extinction period. For example, the rise of the wind-pollinated amentiferae in forests dates roughly from that time. These systems are adapted to acid soils, and the trees are often deciduous. Both birds and mammals evolved very elaborate systems whereby the young animal eats different food from the adult. These kinds of traits might be conspicuous by their absence in the organisms that were eliminated.

McLean: A marine regression was taking place as well as tectonic changes and volcanism. Perhaps a meteorite impacted as well. All of these factors should be integrated into models which seek to account for the paleontological data.

Feldman: Among these factors is the discovery by Dr. Smit of considerable concentrations of volatiles in the basal millimeter of the Caravaca section. Arsenic, selenium, antimony and other elements can be toxic in sufficient concentrations. The boundary material seems to be impossible to derive from terrestrial rock sources within the very narrow time space of 100 years. Here is therefore another thing that has to be explained in the context of some kind of accretionary event, and which appears to rule out an ordinary meteorite. Can we come to some kind of consensus that, if these data are correct, there is evidence for a cometary object that brought volatile poisons into the terrestrial environment?

Smit: Many would correlate the arsenic with the pyrite horizon within the boundary layers, and a small amount of pyrite is also present in the basal millimeter at Caravaca. These are pyritic residues of probably organic origin. The sulphur content is lower than the arsenic content in the horizon. It is difficult to imagine an oceanic mechanism to deposit only arsenic here. It would be preferable to derive this element from the projectile itself, but this is inconsistent with existing data from meteorites. Perhaps comets contain these arsenic elements, as Dr. Hsü has suggested.

Feldman: It is probably as important for the Berkeley group to deal with this problem, as it is for Dr. McLean to deal with the abundance anomalies of the platinum-group metals.

L. Alvarez: I can assure you that we will consider this problem.

Kyte: These same enrichments occur in the Danish section, but the arsenic to iridium ratio is 150 times chondritic.

Smit: Arsenic is so volatile that it may be present in higher concentrations in live comets than in extinct ones.

Kyte: Of course this is possible; perhaps there may even be sanidine meteorites. But it is difficult to imply the existence of a type of meteorite for which there is no evidence.

Smit: None of the meteoritic hypotheses can account for the sanidine. Only one iron meteorite has been found to contain sanidine, and iron meteorites are depleted in chromium. This is a report of K_2O rich spherules in the Tunguska event (Glass 1969).

Kyte: There is a slight enrichment of selenium and arsenic in the D.S.D.P. site 465A horizon, but not in our abyssal clay section. These elements probably have a terrestrial source.

Jaworski: Is there a compendium of trace-element work on boundary core samples?

Kyte: There are very few data in the literature at present. We published all our data on the Danish section and on two samples from site 465A. The Berkeley group is publishing their data on 465A now, but about a dozen sections have been analyzed for which the data are not yet available in the literature.

Hsü: We also have some data for arsenic and other rare elements, which can be made available.

Smit: It might be significant that the arsenic is only present in those layers which show the highest concentrations of iridium. If these layers are not present, an arsenic anomaly will also be absent, although an iridium anomaly may remain detectable.

Kyte: We found the selenium anomaly to be much sharper near the base than that for arsenic.

Jaworski: Have nickel and vanadium concentrations been measured? These are markers for certain benthic organisms.

Smit: Vanadium correlates perfectly with the detrital clay.

Asaro: We have analyzed a number of Cretaceous-Tertiary boundary samples. Sometimes there are enhancements of many elements and sometimes there are none. In the seven centimeter New Zealand boundary layer, for example, the middle section contains 20 PPB of iridium and approximately 5,000 PPM of arsenic. In the clay immediately above and below, the iridium content declines. It is evident from this pattern that the iridium and other elements are peaking in slightly different horizons. Of the 50 elements examined, the abundances of 30 to 40 do not correlate with those of iridium. A lack of correlation means that discrepancies will occur in the ratios of elemental abundances. The elements are displaced stratigraphically from each other to some extent. In anoxic materials from another age, we detected enormous enhancements of zinc, for example. Many other enhancements were detected in the anoxic sediment sample which correlate approximately with abundances in seawater. There was no enhancement of iron and nickel.

Jaworski: Using iridium as an internal standard, do other elements line up with the maxima of iridium?

Asaro: There is a general correlation, for example, in D.S.D.P. core 465A between the clay elements and the iridium. As mentioned earlier, the Ni/Ir ratio is smaller than in Cl chondrites in the detrital clay, but is larger than in Cl chondrites in the pyrite-bearing silicate (which may be an authigenic clay). However, the patterns in different localities appear to be sensitive to local conditions.

Jaworski: If there was a single event, and the particulate matter did not dissolve, then the maxima and ratios would not vary. If dissolution was occurring in aquatic environments, then the resonance times of the various elements are not similar. Their deposition will not be simultaneous, and the observed differentiation would simply reflect the chemistry that has occurred between the initial entering of the aquatic phase and deposition.

Asaro: In D.S.D.P. site 465A the iridium seems to be associated with the clay and the pyrite but some kind of elemental displacement may be present. Why is the iridium distributed through a broad peak considerably above the level where the boundary might be, and why is a considerable amount of detrital clay distributed over the same region? The Gubbio type of iridium pattern with a fairly well-defined peak occurs in some localities. In others it is spread out over 50 centimeters. Examples of multiple-peaking occur, and it is not known whether the pattern is in the sediments or if it is a function of the coring operation. The patterns are often not as simple as one iridium peak which tails off apparently exponentially.

L. Alvarez: Could the separation depend on the depth of the ocean? At what depth were the sediments in the Caravaca section deposited?

Smit: Our estimates, from the few larger echinoids present in the sediments, range between 400 and 2000 meters.

Hsü: The record in the 465A core may be the result of resedimentation phenomena, in which the peaks were blurred.

Thierstein: There is no evidence for coccolith resedimentation at 465A. The site is in a foram sand, and winnowing may have occurred rather than redeposition.

Hsü: Winnowing implies the movement of bottom materials, and particle-size sorting effects.

Thierstein: The most drastic of the documented extinctions occurred in the photic zone in open-ocean environments, which is one of the areas most vulnerable to the effects of heat, poison or even radiation. One of the best places to escape these effects would be in the lower part of the photic zone. Impact mechanisms imply that these stresses would be distributed globally in the atmosphere and should affect terrestrial organisms much more severely than those in open-marine environments.

Jaworski: Heavy metals do not move very much through the soil column. The reason why we have not destroyed ourselves from sludge containing industrial wastes is because the soil column has a high ion exchange capacity. This is lacking in the water column because there are fewer cation exchangers.

Thierstein: Why did nearshore organisms suffer less than oceanic plankton?

Hsü: Ocean surface currents did not enter shelf areas everywhere.

Smit: Why not combine a slight heating with a slight blocking of sunlight? If the photic zone is restricted to the top 50 or 40 meters, and if the atmosphere is heated to 200°C and some of the heat dissipated into the ocean, the combined effects would be more lethal than either in isolation.

McLean: The reproductive cells of plants can be as vulnerable to heat as those of animals. For example, the fern gametophyte is a small plantlet-bearing antheridia that produce sperm cells and archegonia that produce eggs. The sperm swim in a film of water from an antheridium to an archegonium. Heat over an extended period of time would have caused major reproductive disruptions amongst some of these plants.

W. Alvarez: Cesare Emiliani expressed regret that he would not be able to be present and asked me to summarize a paper authored by himself, Eric Krause and Eugene Shoemaker, which has been accepted by Earth and Planetary Science Letters. Dr. Emiliani finds that many organisms are living now and were living during the Cretaceous very close to their maximum temperature tolerances. An increase of a few degrees would be lethal to many of these organisms. Animals tend to be somewhat closer to their maximum tolerance than plants. Dr. Shoemaker presents a detailed quantitative estimate of what would happen during a large meteor impact into the ocean. Dr. Krause considers the possible atmospheric effects. Immediately after impact in the ocean a moderate temperature increase would take place due to the passage of hot oceanic lithosphere fragments through the air, and adiabatic compression of the atmosphere. The heat content of the rocks thrown into the air could represent up to 20% of the kinetic energy of the bolide. These two effects are on the order of one to two degrees each. The dust-albedo effect is then likely to produce a rather short decline in temperature. Water injected into the stratosphere rains out quickly and carries the dust with it. An enormous amount of water vapour remains in the air, dramatically increasing the greenhouse effect. If approximately two centimeters of precipitable water is injected into the stratosphere, and even one hundredth of a centimeter remains, temperature rises of the order of a few degrees, and up to 10 degrees could be expected. These are evidently conservative estimates.

Hsü: Many factors could affect oxygen isotope composition, and the signals from the deep sea cores may not be as systematic as we would like. However, we did detect a temperature decrease about of 4 or 5 degrees immediately after the impact event. If that signal is real, it may reflect the presence of a dust cloud. The predicted third stage of warming was also suggested to me by Peter Signer (personal communication) who noted that the water vapour from the ocean would increase the greenhouse effect. Our data show an increase of about 10 degrees following the decline of 4 to 5 degrees. Our data do not contradict the predictions of Emiliani and others, but we prefer to assume the greenhouse effects of increased CO_2 in the atmosphere.

Smit: Are there only a few data points for the decrease and subsequent increase in temperature?

Hsü: Yes.

Smit: Many locations have now been identified where there is a thick accumulation of sediment across the Cretaceous-Tertiary boundary, with the same facies above and below the boundary. In all of those basins the sediments are intercalated with turbidites. Yet nowhere in the world, to my knowledge, has a turbidite been deposited exactly on the Cretaceous-Tertiary boundary. An impact in the ocean of the magnitude usually postulated would disturb the whole marine environment by tidal waves. A crater in the ocean some 300 kilometers in diameter would lower sea-level by 6 to 8 meters. Some kind of violent sedimentary influx should be detectable in turbidite basins, such as in Zumaya in northern Spain, where every thousand to two thousand years a turbidite deposit is recorded.

Hsü: The absence of such evidence is in contradiction with a violent oceanic impact. I have never found turbidites at the Cretaceous-Tertiary boundary.

McLean: Tidal waves coming ashore should produce a mixing of marine and terrestrial sediments. Assuming that large amounts of seawater were thrown into the atmosphere, marine fossils should be rained down at the Cretaceous-Tertiary contact throughout terrestrial areas.

Hsü: Two of the three Soviet craters which have been dated at 65 million years measured 25 and 3 kilometers in diameter, and were flooded by seawater after they were formed. The third, in Siberia, was a terrestrial impact which left behind a crater of 60 kilometers. The earliest sediments in those craters might reveal local environmental conditions, and the impact melts might reveal the nature of projectile.

The findings of Soviet researchers will be most interesting in these areas.

W. Alvarez: Oil companies are beginning to find craters by multichannel seismic reflection profiling techniques. A few have been detected in the northern plains (Williston Basin) in the subsurface, which are not of the same age as the Cretaceous-Tertiary boundary, but they do produce oil.

Hsü: A fragmented projectile could produce more than two or three craters. In the Scaglia Rossa near Ancona in Italy, a crater-like feature was recently located in pelagic sediments of Maastrichtian age, and covered with Tertiary sediments.

Smit: It may well not be of the correct age.

Hsü: If a cometary object is disrupted through tidal effects the fragments need not impact simultaneously.

Kyte: Using the tidal disruption model the way we envision, numerous fragments could enter elliptical orbits around the Earth. Öpik (1972) has described a similar mechanism for the possible capture of the Moon. In this, a proto-Moon is tidally disrupted and reformed from fragments elliptically orbiting the Earth with perigee at the point of encounter and apogees extending to a distance of several hundred Earth radii. Only about half of the fragments are actually captured. Cometary objects would probably be initially less scattered, but the gravitational effects of the Moon could hurl them onto the Earth's surface. These scattering effects, combined with Earth's rotation could easily spread the impacts over 100 degrees of latitude. However, as we stated earlier, in order to have a disruption and a capture, a low encounter velocity is implied, meaning that the object must have an orbit similar to that of the Earth. There are a few Apollos which have apogees and perigees within about 10% of the Earth's orbit.

L. Alvarez: What is your capture mechanism?

Kyte: This has been described by Öpik (1972). Perhaps someone familiar with this type of problem could model it and determine its feasibility.

L. Alvarez: The asteroid could conceivably pass through the atmosphere and lose enough velocity to be captured and broken apart. However, it would only decelerate by about 1 g, and the cross-section is inadequate for such a close pass. Direct asteroidal impacts are more likely.

Kyte: Öpik (1966) lists the probability of ejection, collision and tidal encounter with Earth for the comets Encke, Apollo and 1948E. The probability of ejection from the Solar System for these is about 10% or less, the probability of a collision with Earth ranges between 88 and 97%, and the probability of tidal encounter is 100%. If the assumption is made that cometary objects are extremely fragile and if a tidal encounter occurs before a collision, most of the fragments would not be large objects at the time they are finally accreted.

Béland: Returning to biological considerations, northern land plants could accommodate changes in temperature and a darkening of the skies better than low-latitude plants. Deciduous trees, for example, that had lost their leaves might survive an extension of adverse conditions for an additional six months, whereas plants in humid tropical environments could not sustain a comparable stress. Savanna plants, such as those in Africa or Australia, that lose their leaves during the dry season could perhaps also survive a reduction in light for an extended period of time. Did plants growing in high latitudes and in seasonally dry tropical regions actually survive the extinctions better than those in equable environments?

Pollen grains may not reveal the habitat preferences of the parent plant. For example, there are 600 different species of eucalypt trees in Australia living in a wide diversity of habitats. If the pollen grains show close taxonomic resemblances, very little could thereby be deduced about the ecological requirements of different species. It is probably also true that the pollen entering the fossil record is biased towards wind-pollinated plants, and pollen from insect-pollinated plants is not so well represented. Perhaps wind-pollinated plants were highly diversified after the Cretaceous-Tertiary boundary, but what was their status before this time? Would they not always be over-represented in the pollen record? When it is said that the pollen record does not show significant changes in diversity, such a small percentage of the possible spectrum of plants and habitats may be represented that the generalization is without meaning.

Dilcher: The germ line which passes through the male gametophyte (angiosperm pollen) has been the object of extensive selective pressure. Many extant genera and families have been mapped according to the morphology of that cell and, although many extinct types of pollen occur in the fossil record, certain unique forms are found only in particular lines of extant plants today which can be traced back in the fossil record. Pollen grains (wind and a few insect-pollinated forms) are distributed more widely in basins of accumulating sediments than are leaves, wood, fruits or seeds. The pollen record is thus a more accessible part of the fossil plant record.

Béland: This is true. However, consider *Eucalyptus saligna*, a very large tree that grows at the border of rain forests, and another eucalypt that lives as a tiny shrub in very dry areas. Their pollen grains would probably be similar. Applying the analogy to the record of Cretaceous plants, if half of a given type disappears, how would one know that moist habitats were more disturbed than dry ones?

Jarzen: Ecological conclusions cannot be drawn on the basis of some pollen types. For example, pollen of the lily family is abundant in Cretaceous strata. However, modern lilies occur in northern and equatorial latitudes, and the occurrence alone of the family in Cretaceous strata would not provide grounds for environmental speculation. Fortunately other pollen-types can be related to genera or to tribes within families which do have restricted ecological requirements. Among these, *Gunnera*, *Pandanus* and *Nyssa* may be cited. Ecological conclusions are drawn based on the occurrence of these forms.

Graham (1976) demonstrated that in tropical areas insect-pollinated plants are well represented in the pollen record. Outwashing of tropical rains around fresh or brackish water basins carry insect-pollinated pollen in abundance into the sedimentary record. For example the pandans are largely insect-pollinated, at least in those forms that have so far been studied. Yet in some sections in western Canada 68% of the pollen belongs to *Pandanus*, while the wind-pollinated pines and podocarps are poorly represented. We can postulate that this basin was receiving a great outwash of pandan pollen because the pandans were growing in close proximity and were washed right into the section. Palynofossils are often abundantly preserved, and in great morphological diversity. Some can be identified, but many in every sample cannot be accurately identified at present.

Russell: Based on the palynofloral record, what conclusions can be drawn concerning the nature of the stresses on the Cretaceous-Tertiary boundary?

Jarzen: The stresses cannot at present be identified by the pollen. There is a general trend across the boundary towards greater diversity and numbers of pollen typical of wind-pollinated plants, but there are exceptions.

McLean: Are you referring to the boundary coal of Brown, or the horizon of palynofloral change of Leffingwell?

Jarzen: The reference horizon is the Number 1 coal in western Canada.

Béland: Some statements have appeared to the effect that "...but land plants do not show any change across the boundary." This suggestion has not been convincingly supported.

Jarzen: Land plants do not show evidence of drastic extinctions across the Z coal boundary, but they do show evidence of change.

Béland: The number of species represented in the sampled sections is small relative to the perhaps 500 species then in existence in the vicinity of the depositional site. The sampling may be inadequate to reveal important changes in diversity.

Jarzen: Two hundred palynomorph species can be identified in one section. The total number of identified late Cretaceous terrestrial and marine palynomorph species is in the thousands. How many species of dinosaurs occur in one section?

Russell: Would it be useful to examine pollen residues for their trace element content?

Rucklidge: Our method could detect the presence of anomalies of platinum and iridium in this material. Unaltered spores should be analyzed from a different locality as a control because there is always a fear of contamination from other components in the clay, particularly when it is known to contain trace element anomalies.

Béland: If the plants that were producing pollen at the end of the Cretaceous stopped growing and producing pollen for two years, could this be detected?

Hsü: Intervals of two years cannot normally be resolved in the sedimentary record.

Béland: The effects on herbivore populations could nonetheless be catastrophic.

Pirozynski: Parasitic insects and fungi should show similar declines. Pollinating insects would also be unable to survive a two-year halt in flower production. After their extinction, wind would be the sole remaining pollination mechanism.

Béland: Insects are an extremely important component in terrestrial ecosystems, but their fossil record is rudimentary.

Hsü: If the impacting projectile had a significant nitrogen component, perhaps the cosmic nitrogen 15 to 14 ratio could be detected in pollen and spore residues.

W. Alvarez: Would the signal not be overwhelmed by that of atmospheric nitrogen?

Hsü: It might be detectable in remains of aquatic organisms if the nitrogen were mixed directly into the water without passing through the atmosphere.

W. Alvarez: With respect to the problem of the apparent absence of turbidites associated with the Cretaceous-Tertiary boundary, it is my experience in the Italian sections that fossil zones are commonly missing either immediately below or above the extinction horizon. In probably half of the outcrops, the boundary is not preserved. The same seems to be true of deep-sea drilling cores. This would appear to support a violent physical event of some kind in the oceans, but the absence of turbidites is puzzling.

Smit: Nevertheless in the deepest basins neither a hiatus or a turbidite can be found. There is no evidence of any violent sedimentological event. Pelagic sections in Europe and in many deep-sea cores show a significant decrease in sedimentation rates from the Cretaceous to the Paleocene, beginning at the Cretaceous-Tertiary boundary. The detrital component, or the clay which has been derived from the continent, also accumulates less rapidly. At Zumaya, Gubbio and Caravaca, sedimentation rates fall by a factor of 3 to 10, implying a significant decrease in continental rates of erosion.

Continental sections should show changes which reflect a decreased rate of sediment transport to ocean basins.

Russell: This does not seem to have occurred in the northern interior of North America, where, if anything, rates of deposition increase. Certainly the grain-size of clastic sediments increases and lignitic deposits become more abundant above the Cretaceous-Tertiary boundary. It would be interesting to learn if similar changes occur at this time in comparable sedimentary environments in Argentina. Are the changes due to deforestation in adjacent highlands?

Smit: Perhaps it was due to a change in the balance between herbivores and the vegetation. When herbivores are removed on a large scale, such as the removal of elephants from stressed systems in eastern Africa, the vegetative cover becomes heavier.

W. Alvarez: Drs. Emiliani, Krause and Shoemaker note in their manuscript that the damage in the *Aquilapollenites* province surrounding the North Pacific may identify the area of impact of the object. Perhaps the Mediterranean cores were shielded from oceanic disturbance.

Smit: Turbidites might be expected to occur around the North Pacific basin.

Thierstein: Many Pacific cores contain long range hiatuses extending from various horizons in the late Cretaceous into the Paleocene, but in most that I have examined the hiatus terminates at the very base of the Paleocene and the latest Cretaceous is missing.

Hsü: At the Tunguska site the trees were burned within a small area, but if a larger cometary object impacted, would the effects account for the damage observed in boreal terminal Cretaceous floras?

Dilcher: It would be difficult to explain floral changes over the entire boreal zone by the impact of an object in the middle of one of two floral provinces existing at that time. Dr. Hickey suggests that the changes are due to a withdrawal of an interior sea that divided North America, which allowed the two floral provinces to mix and elements within them to compete. This explanation seems to be most plausible. Around the margins of an impact area one would expect reasonable survival rates.

Feldman: Rainfall may be important, as indicated in the manuscript by Dr. Emiliani and his associates, in redepositing atmospheric dust. Is enough known from the palynological record to identify regions of high rainfall? Can a link be established between climatic patterns 65 million years ago and the removal of dust from the atmosphere?

Dilcher: Yes, sedimentation of the material with rain would take place over a geologically very short period of time. No physical evidence of a resulting pattern has been identified in terrestrial strata. I find the most convincing arguments in favour of a catastrophic event at the Cretaceous-Tertiary boundary to be the noble metal fingerprinting and the extinction of marine micro-organisms. Terrestrial evidence is much more ambiguous.

Hsü: Poisons introduced into the air in the vicinity of a mercury mine produce devastating effects on animals and plants in the neighbourhood. The effects could be extrapolated to the scale of a floral province.

Dilcher: During terminal Cretaceous time the western floral province was larger than the boundaries that separated it from the eastern floral province. Its centre may have been devastated but the margins would remain nearly intact. If the latter were also devasted the devastation would have spread right into the eastern province as well. In fact, the eastern province suffered less. A number of plants such as the palms, and ancestral birches and oaks survive. Among the groups becoming extinct in the eastern province is an interesting plant called *Liriophyllum*. A number of mid-Cretaceous plants survive up to the Cretaceous-Tertiary boundary and become extinct. There are extinctions in the eastern province as well.

McLean: Do you refer to the Z coal or the level of palynofloral change as the Cretaceous-Tertiary boundary?

Dilcher: The forms cited occur in eastern North America, but the boundary there could probably be correlated with some horizon of palynofloral change in the western interior.

Hsü: Were the extinct forms insect-pollinated?

Dilcher: Both insect- and wind-pollinated forms became extinct at the boundary.

Russell: When we were in East Africa several years ago, it was interesting to observe how large vertebrates bioturbated alluvial sediments. Along the banks of the Luangwa in Zambia, or around Tendaguru Hill in Tanzania the effects of the passage of elephant through soft substrate were profound. In similar Jurassic deposits at Tendaguru a string of tail vertebrae of a brontosaur was interrupted by a footprint of another dinosaur. One animal was mired upright and died with a forefoot plunged two meters into the substrate. In the Hell Creek Formation in the western interior, the strata also have a massive appearance, and do not become finely laminated until after the dinosaurs disappear from the sedimentary record. The lithologic change may well be linked to the absence of dinosaurs. Fossil leaves are easily recovered from the fissile strata of basal Paleocene age. The absence of lamination in the older strata containing dinosaur bones has discouraged paleobotanists from attempting to recover leaf fossils from the floras which predate the extinctions, to our loss.

McLean: There are always preservational problems, and only small amounts of the pollen that is produced are ever preserved. Many fossil palynomorphs resemble modern ones. However, many living plants produce the same kind of pollen grains, and identifications of fossil pollens are uncertain. Compounding these uncertainties are large gaps in the fossil record near the Cretaceous-Tertiary boundary in the western interior. Yet Nichols and Ott (1978) have not been able to detect major changes in the palynofloral record.

Russell: Within the Hell Creek and Bug Creek areas of eastern Montana where might the large gaps be seen on the Cretaceous-Tertiary boundary?

McLean: What formula should be used to define the Cretaceous-Tertiary contact?

Russell: Use whichever one you like.

McLean: This raises the need for good radiometric dates over a broad area. Using the Z coal as the boundary defines it on the basis of tectonic change. The palynofloral change some 20 meters below probably reflects the presence of a hiatus.

Russell: The deposition of the marine Cannonball Formation in North Dakota spanned at least a large part of the two-million-year duration of the *Globigerina pseudobulloides* zone (Fox and Olsson 1969). The Cannonball is separated by a thin layer of lignite from a bentonite which is at the top of the Hell Creek Formation and the highest occurrence of the *Triceratops* dinosaur fauna. The *pseudobulloides* fauna follows the *eugubina* zone which occurred immediately after a marine extinction and is also in close physical proximity to the highest terrestrial dinosaur fauna in the interior of North America. This biostratigraphic evidence implies simultaneity in the marine and terrestrial extinctions.

McLean: The resolution is inadequate to discriminate short-time intervals.

W. Alvarez: Radiometric dates from 65 million years ago also have plus and minuses associated with them of one to two million years. This is one standard deviation, not the absolute range. Resolution in that part of the time-scale is better with paleomagnetic and micropaleontologic techniques.

W. Tucker: It would be useful to be better informed about other extinctions so statistics for more than one event would be available. Does an extinction event have to happen more than five times in five hundred million years, or does it just have to happen once? What are the time intervals between extinctions? Are these extinctions similar or is there a spectrum of extinctions? Do some happen with much less intensity than others? This information would enable us to examine the cosmic setting with statistical means to see which hypotheses are plausible.

Hsü: Two extinctions were missing from the list you cited earlier. One was at the end of the Triassic about 200 million years ago, when the ammonites almost died out, and the other was at the end of the Eocene about 38 million years ago. These are almost as important as the terminal Devonian and terminal Ordovician extinctions.

Clube: Possible declines in groups of organisms prior to the extinction event have not been discussed extensively, but have been cited as evidence against the kind of astronomical model that we have proposed. However, if one giant asteroid impacted and caused a major extinction event, it would have also been associated with comets and debris from outside the Solar System, or a gathering of lower-mass objects. It is statistically very unlikely that the huge object would be the first one to impact, and an array of earlier impacts of smaller objects would inflict some damage. It would be very interesting to examine the nature of the prolonged declines that occur prior to major events.

Kyte: What kind of time-scale do you expect in the passing through a cloud or collection of such objects?

Clube: Basically one should consider the relaxation times in the Apollo asteroids, or time-scales of several millions of years.

Smit: The paleontological record could accommodate perhaps 10 thousand years but certainly not one million years.

Thierstein: This applies to deep, open-ocean plankton in the Northern Hemisphere. Some paleontologists who work on molluscs, such as Kauffman (1979) and Wiedmann (personal communication), maintain the view that there is a gradual decline in both ammonite and inoceramid diversity through the final 10 to 20 million years of Cretaceous time. The shallow-water evidence indicates that the problem there is the sample control and transgressive - regressive control.

McLean: It was pointed out that *Aquilapollenites* was declining for quite a long time before the end of the Cretaceous. This is why a broad view is necessary. Start many years before the Cretaceous-Tertiary transition, see what the world was doing; then trace the pattern of events up to and through the extinction interval. The events will be much more easily understood than if the extinctions are treated as an isolated event.

Smit: Throughout the history of the ammonites, at any particular time there is roughly a 50% chance they are either in a decrease or in an increase. The trends may have no relation to the boundary.

McLean: When the number of genera are counted through time with histograms, ammonite diversity was stepping down like a staircase.

Smit: This was during late Cretaceous time; at the beginning of the Cretaceous they were increasing.

Russell: In the long view some groups are always waxing and some are always waning. What makes the subject more controversial is that abrupt extinctions occurred in some groups when they were already in a decline. An example are the ichthyosaurs. Their remains occur in terminal Cretaceous deposits, but they are much more abundantly preserved in Triassic and Jurassic strata.

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CONTRIBUTED ABSTRACTS

Episodic Catastrophism

S.V.M. Clube

I think it might be helpful if I say a few words about the origin of the theory of terrestrial catastrophism[1]. There may be a tendency to think the astronomy has grown in an ad hoc way out of the geochemical evidence. This is not so. In fact, for several years now, various problems in galactic evolution have highlighted the importance of testing whether spiral arms should be treated as material bodies or as density waves[2, 3, 4, 5]. It is known for example that spiral arms house giant molecular clouds and these are the natural places for making comets — but in the density wave theory unlike material arm theory, the comets would have to drift away from behind the spiral arms and fill the disc of the galaxy more or less uniformly. Since the Solar System captures comets as it moves through the galaxy and there have for some years been good reasons for believing comets produce Apollo asteroids and Apollo asteroids through impacts produce terrestrial catastrophes, a history of "Episodic catastrophism" (controlled by spiral arm passages) as opposed to "regular catastrophism" (controlled by purely random encounters) was evidently a means of distinguishing whether the galaxy contained material arms or density waves.

The proposition that Solar System comets may be recent captures from interstellar space contradicts present views as to the origin of comets and we were led to review the status of the so-called Oort Cloud. When the primordial Oort Cloud was first postulated[6], the existence of giant molecular clouds was not known, but as Dr. Napier will show[1, 5, 7], encounters with these molecular clouds are very damaging to any primordial Cloud and we now know it is bound to be stripped away. So where do the existing long period comets come from? As it happens, recent calculations had shown[8] that the comets could have been captured into the Solar System ~ 10 Myr ago, and this idea turns out to provide a better explanation of the high mass members of the Apollo asteroid population (as dead comets) than the older idea, already in some difficulty[5], that they are meteorites come from the asteroid belt. So, in the light of the theory already developed, we raised the question whether there is spiral arm material that the Solar System went through ~ 10 Myr ago? And, as it happens, there is: there is in the solar neighbourhood a large conspicuous spiral arm feature known as Gould's Belt which the Solar System entered some 20 Myr ago and emerged from ~ 10 Myr ago. So, mindful of the possibility that impacts may trigger glaciations, and the knowledge that recent terrestrial history through 10-20 Myr has the character of a major ice-age, we came to the conclusion that there was a basis in both fact and theory for proposing "Episodic Catastrophism". Catastrophic extinctions were predicted for large Apollo asteroids with simultaneous deposition of interstellar signatures, and the discovery of such effects at the K-T boundary evidently bears great promise for galactic theory.

I would like to emphasise however, that the theory quite naturally produces meteorites at the low mass end of the Apollo population at the same time as it does dead comets at the high mass end since the former are derived from the remnants of the Solar System primordial population which are gravitationally perturbed into such orbits as a result of passing close to an interstellar cloud. We have tended therefore to take the view that the proof of meteorite[9], comet[10,11], or Apollo asteroid[1] for any particular impact producing extinctions like the K-T is not really a fundamental problem. It is the pattern of impacts that is more fundamental; indeed it is important now to be recognising that impacts are an inevitable and (statistically) predictable part of the astronomical scene and to be asking whether they are primary agents in producing the stratigraphic record generally or whether they are minor perturbations on events that are largely of pure terrestrial origin[12].

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Looking Back on the Tunguska Event

Ian Halliday

When we consider the possible effects of a major impact on the Earth some 65 million years ago it is probably worth spending a few moments to review what is known about the most violent of recent impact events, the Tunguska explosion of June 30, 1908. The popular news media enjoy circulating dramatic stories attributing the Tunguska event to the current scientific (or science fiction) novelty — antimatter, nuclear accidents on alien spacecraft, neutron stars, black holes — so it is important to realize that much concrete knowledge exists. The early research on Tunguska occupies a large portion of the volume "Giant Meteorites" by E.L. Krinov (1966, English edition) an excellent scientific book with the flavour of an adventure story.

Krinov's account contains interviews with eyewitnesses to the 7 a.m. fireball which rivalled the Sun, descriptions of the sounds, the blast of radiation and the pillar of fire and smoke reported by more distant observers. Vague accounts of fallen trees and forest fires were reported by native hunters but it was not until the second decade after the event that the accounts were collected and expeditions organized to this remote area of north-central Siberia.

Dust from the explosion caused two very bright nights over northern Europe which persisted to some degree for two months. Seismic and microbaragraph records from across Europe suggest an energy of 10^{22}-10^{23} ergs for the explosion, which produced an active seismic record for an hour at Irkutsk, 900 kilometers from the desolate site of the event.

Due to the tireless efforts of the meteorite expert, L.A. Kulik, early expeditions to locate the site began in 1927. Great hardships were endured in penetrating the forest with horse-drawn wagon trains but a large area of devastated forest was located and mapped, in which the trees were blown down radially outwards from a central region. Later surveys established the area of this damage as 1600 km^2. The central area coincided with a large swamp in permafrost and Kulik believed the numerous pits were caused by individual meteorite fragments, so a trench was dug to drain one 32-meter diameter pit, but no meteorites could be found. Today it is believed that the swamp was a pre-existing feature and no sizable fragments from the incoming object survived to strike the ground. At least four major expeditions were conducted before the Second World War (which claimed the life of Kulik).

Later expeditions included aerial surveys, geophysical surveys and forest and soil research. Of particular significance are collections of sub-millimeter spherules of magnetite and silicates, first collected in 1930 but extensively mapped by subsequent expeditions. They appear to define an area up to 200 kilometers from the epicentre, over which ablation products were distributed by the wind, concentrated to the northwest of the explosion site. Analysis of some of these particles yields an unexpectedly high ratio of potassium to sodium. There seems to be little doubt that the large mass of the projectile was completely disrupted at a height of about 5 to 7 kilometers and experimental models using an explosion along a line source can produce a very similar pattern of blast damage.

Several attempts have been made to establish the path of the Tunguska meteoroid in order to speculate on its orbit and origin. The possibility that it was a cometary fragment was suggested by Kulik and Astapovich in the U.S.S.R. and by F.J.W. Whipple, an English meteorologist, as early as the years of the first expeditions to the site. This remains the best explanation and recently it received further support in a study by Kresak (1978) who used a recent analysis of the visual observations to conclude that the Tunguska object was probably a fragment from Encke's Comet.

Our knowledge of the chemistry of comets is still far from satisfactory. Some qualitative information is available from cometary spectra and the spectra of shower meteors, whose origin can be attributed to comets, but we are not in a position to say much about the relative abundances of minor constituents in comets vs asteroids. There is still debate as to whether some of the more fragile classes of meteorites may have had their origins in comets, although the fate of the Tunguska object may be an indication of how severe a filter the Earth's atmosphere is for moderately large chunks of cometary matter. There will be differences in the details of the impact mechanism for high-density and low-density projectiles but it is clear from Tunguska that large amounts of material may be spread over great distances by major impact events.

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Evolutionary and Environmental Consequences of a Terminal Cretaceous Event

Kenneth J. Hsü

I started with the premise that the occurrence of a collision event with an extra-terrestrial object is the only viable working hypothesis at the moment to explain the K-T boundary. The questions to be explored are: 1) the nature of the object, and 2) the scenario for selective extinction. During the conference, I presented the work done on deep sea drilling cores from Site 524 in the South Atlantic (30°N, 3°E) by my colleagues Q. He, J. McKenzie, K. Perch-Nielsen, H. Oberhänsli and U. Krähenbühl in Switzerland. The data indicated the possibility of a collision event, which caused a mass-mortality of ocean life, followed by catastrophic environmental stresses which caused the selective extinction of marine calcareous planktons and of dinosaurs.

The K-T boundary at Site 524

The boundary is defined by nannofossil evidence on the basis of first common occurrence of Tertiary forms, such as *Thoracosphaera* spp. even though nannoplankton species which had populated the Cretaceous constitute the bulk of the fossil assemblage (95-50%) in the first 10-centimeter thick sediment above the contact, deposited during the first three or five thousand years after the postulated boundary event. The paleontologically defined boundary falls within a magnetostratigraphic epoch corresponding to the Seafloor Anomaly 29 - reversed, which spanned a time interval less than half a million years.

Mass-Mortality and Dissolution-event

A boundary clay about two centimeters thick, containing less than 5% of $CaCO_3$ is present, straddling the boundary. The clay constituents are apparently detrital, containing little of what could be considered extraterrestrial materials, except an iridium anomaly (3.6 ppb Ir) is present. The decrease of carbonate-content, as has been observed elsewhere, has been interpreted as evidence of drastically reduced fertility of ocean life during and after the terminal Cretaceous event. Dissolution of planktonic foraminifera is remarkable and the $CaCO_3$-content showed a 10-fold decrease, in the uppermost (~ 10 centimeters) of Cretaceous sediments. We believed that the calcite-compensation-level of the terminal Cretaceous ocean rose because the lack of production had minimized the chemical difference between surface and deep-bottom waters.

Carbon-Isotope Excursion

Both the analyses of the bulk and of the fine fraction, consisting almost exclusively of nannofossils, produce a shift of the $^{13}C/^{12}C$ ratio, with maximum $\delta^{13}C$ shifts of about minus three pro mille. A first shift during the first few hundred years after the event may indicate a temporary change in the isotopic compositions of the surface waters when the production of organisms was almost completely suppressed. After returning to the normal Cretaceous value, a second maximum of minus three pro mille shift was built up during some 30 or 50 thousand years, probably because the production and sedimentation or organisms could not keep in pace with the influx from rivers of carbon atoms anomalous rich in ^{12}C.

Oxygen-Isotope Excursion

Analyses of the bulk and fine fractions also revealed drastic changes of oxygen isotopes, indicative of temperature-changes of surface waters. The shift in the boundary clay layer could be interpreted as a signal of 5°C cooling during the first few hundred years. Like the carbon-shift, the oxygen-shift in the earliest Tertiary sediments suggests a temperature rise of about 10°C (after the minimum) during the next 30 or 50 thousand years. While the initial cooling might be related to the insulation of solar energy by the ring of ejecta in the stratosphere, the subsequent warming could be related to the greenhouse effect, either of water molecules thrown into the air by impact, or of increased CO_2 in atmosphere after the exchange between the atmosphere and an ocean with reduced fertility.

Scenario for Terminal Cretaceous Extinction

I am suggesting a two-act drama for the boundary event. The initial collision event was so brief, that its only record was the fallout of the siderophiles (iridium), a carbon isotope shift indicative of a suppressed ocean-productivity and an oxygen shift suggestive of a momentary cooling. The mass-mortality of the oceans, in my view, might have been caused by chemical poisoning, which has been distributed by surface currents and has affected the tropical planktons the most. The consequences of the mass-mortality was a change toward a more acid ocean. The survivors of the "endangered species", mainly marine calcareous planktons, became extinct in this stressed ocean environment within 50 thousand years or so after the catastrophe, while an explosive evolution of the Tertiary species was eventually to occupy the vacated ecological niches. The temperature-rise after the catastrophe may have produced the thermal stress which caused the extinction of the dinosaurs, which had probably become the "endangered species" on land after the holocaust.

This summer an article is to be published by myself with my Swiss colleagues and with my shipmates on Leg 73. I am presenting our joint research as a reporter for the team.

Deccan Volcanism and the Cretaceous-Tertiary Transition Scenario: a Unifying Causal Mechanism

Dewey M. McLean

The Cretaceous-Tertiary (K-T) transition Deccan flood basalt volcanism, one of the greatest outpourings of lavas from the Earth in geological history, was synchronous with, and accounts for via fluctuation of the carbon cycle, the K-T transition: (1) marine $CaCO_3$ dissolution and "clay" events, and the apparently linked extinctions of calcareous microplankton, (2) drops in $\delta^{13}C$ and $\delta^{18}O$ values, and (3) via "greenhouse" conditions, the extinctions of the dinosaurs by heat-infertility linkage. Theorized to represent mantle plume activity originating near Earth's core-mantle boundary, this volcanism provides a mechanism for conveying iridium and osmium from the core (where they presumably exist in cosmical proportions) to be distributed via volcanic exhalations. Carbon gases are among the most abundant volcanic emanations, and possibly reflect degassing of Earth's interior of material trapped during Earth's accretion. Deep origin volcanism spanning the 5 million years of the Deccan volcanism (65 to 60 Myr ago) would have brought vast amounts of carbon gases to Earth's surface. Volcanic CO_2 and reduced carbon in igneous rocks have low $\delta^{13}C$ values ($-7^0/_{00}$ and -19 to $28^0/_{00}$, respectively), accounting for the drop in $\delta^{13}C$ values across the K-T contact. Build up of atmospheric CO_2 would result in "greenhouse" conditions and the drop in $\delta^{18}O$ values (a warming signal) across the K-T contact. Acidic volcanic gases injected into the oceans would cause $CaCO_3$ dissolution; the holding of $CaCO_3$ components in solution would allow accumulation of a concentrated clay layer (e.g. the Danish fish clay) on the Cretaceous erosional (corrosional) surface. Lowered marine pH would also trigger calcareous microplankton extinctions; they were most severe in the Tethyan region in proximity to the Deccan volcanism. "Greenhouse" conditions would have affected reptilian reproduction according to size. Larger organisms have relatively small surface-volume ratios; during warming, organisms above a critical size (10 kilograms) would have retained excessive body heat, causing degeneration of internal germinal tissues in males and, in females, disruption of calcium metabolism and hormonal systems. The abrupt terminations of geological ranges of many taxa at the K-T contact seemingly reflect hiatus control of ranges associated with the shallow marine $CaCO_3$ dissolution event, and terrestrial hiatuses and facies control associated with the terminal Cretaceous regression of epeiric seas off the continents, that have created the illusion of simultaneous global range terminations. In all likelihood, the K-T extinctions spanned hundreds of thousands, if not millions, of years.

Effects of Solar System Passage Through Giant Molecular Clouds

W.M. Napier

It is now known that giant molecular clouds comprise about half of the mass of the interstellar medium. They have masses 10^5-10^6 solar masses and diameters typically 30-300 light years. It is probable that they are concentrated in the spiral arms of the Galaxy; certainly they are associated with dust, which is a spiral arm tracer, and they are seen to lie along the spiral arms of the Andromeda Galaxy. The Solar System passes through spiral arms at 50-60 million year intervals. Approaching a line of GMCs it cannot soar over or under because its orbit is nearly in the galactic plane, and it can only avoid encounter by passing between them; however because of the large masses of these clouds there is a gravitational focussing effect. Putting all these factors together one finds that there is an expectation of one encounter with a GMC every 100-200 million years, that is 3-6 times within the Phanerozoic.

GMCs seem to be largely empty space: they comprise dense nebulae, generally a few light years in diameter and with masses about 10^4 solar masses. During passage through a GMC there is an expectation that the Solar System will pass within 15 light years or less of some such substructure, and strong tidal forces are then exerted across the Oort comet cloud, which is over a light year in diameter. The result is to perturb the orbits of the comets around the Sun to the extent that many of them escape. This has been modelled numerically and some typical results are shown in Fig. 7: we see that even a single passage through a GMC has a dramatic effect on the comet cloud. A typical close encounter will result in the loss of 20-80% of the long period comets attached to the Solar System. In addition, the orbits of the surviving comets are expanded so that escape becomes easier on subsequent encounter. This process has been followed by numerical simulations: one such run is shown in Fig. 8. We see that, within the lifetime of the Solar System, any primordial cloud of comets would have been completely destroyed beyond a few thousand astronomical units from the Sun. And yet the observed long period comets conspicuously arrive from regions 40,000-50,000 AU distant. These comets must have been placed there recently, and so we are led to an interstellar origin, with a capture event having taken place within say the last 10 million years.

Questions to be asked on this theory are whether comets can grow in the interstellar environment, and whether they can be captured from it. It turns out that molecular clouds are crucial in both respects. Because of their high densities and low temperatures ($T \lesssim 20K$) they are ideal places for the growth of large bodies. The mechanism we have studied involves forcing together of dust grains by differential radiation pressure. We find that bodies will coagulate out with mean mass $\bar{m} \sim 10^{18}$ gm (say diameter ~ 10 kilometers) and differential mass distribution $n(m) \propto m^{-1.7}$ or so. Recent observational work indicates that the long-period comets have masses a few 10^{18} gm and $n(m) \propto m^{-1.64\pm0.2}$. The striking resemblance between cometary and interstellar chemical compositions has often been remarked on and follows from this theory. The agreement is spuriously good – the theory is not that accurate – but it does seem that there is at least one mechanism for growing comets in the interstellar medium and there are probably others.

There is a deficiency of heavy elements in the interstellar medium in relation to the predictions of galactic nucleosynthesis theory, and interstellar comets have already been proposed as a means of 'locking up' the missing material. If 5% of the mass of a GMC is in the form of comets, then elementary calculations, with the GMC as a third body, give an approximate capture rate during a close encounter between Sun and GMC. It turns out that 10^9-10^{11} comets can be captured during each encounter, comparable to the number lost: one therefore anticipates a substantial overturn of the Oort cloud comet population during encounter. On this hypothesis the Oort cloud is in a quasi-equilibrium situation with calculated population and dimensions much as observed, whereas on the usual 'primordial' cosmogony these are empirical quantities.

The theory is vulnerable on a number of points: (1) the Solar System cratering flux should be determined by time constants appropriate to the Galaxy rather than the Solar System. In particular we expect fluctuations in the rate by factors three or four to one, or even more, at 100-200 million year intervals on average; (2) we do not expect precise agreement between the observed cratering rate (say over the last 500 million years) and that expected from the current circumterrestrial missile population. Again a discrepancy by a factor up to three or four is expected; (3) we should look for chemical signatures. The (Ir, Os) isotope ratios at the K-T boundary have Solar System values but this may not be critical: an interstellar planetesimal represents a mix of the output from many r-process environments over a wide range of time and space and there is no reason to expect significant variations within the Galaxy. On the other hand $^{12}C/^{13}C$ varies appreciably from place to place in molecular clouds due to fractionation and one should look for departures of several percent from the terrestrial value of 89 at significant boundaries.

Accelerator Mass Spectrometric (AMS) Measurement of Pt and Ir at the Cretaceous-Tertiary Boundary

J.C. Rucklidge et al.

The tandem Van de Graaff accelerator at the University of Rochester has been used as an ultra-sensitive mass spectrometer to measure ultra-low levels of Pt and Ir in samples from the Cretaceous-Tertiary boundary. This method, which offers sensitivity comparable and often superior to neutron activation analysis (NAA) is described in detail in Rucklidge *et al.* (1982) or more generally in Litherland (1981). While still in an early stage of development, this method has advantages which include the use of milligramme size samples without extensive pretreatment, and the ability to determine sub-ppb levels of Pt and Ir in times measured in minutes rather than days or weeks. By this method, the detection of Pt is about 1.6 times more sensitive than Ir, which is in stark contrast to NAA where Ir is about 1000 times more sensitive than Pt. This latter fact is the reason that Ir is the element for which the anomaly is noted in so many analyses of the K-T boundary, while in fact there may be equally valuable information in the Pt data. The work summarized here is the first attempt to apply AMS to a geological problem, and the data presented may not be directly comparable with NAA data from similar samples. This is because the volume of material sampled by the two methods differs by a factor of 10^4 to 10^5. The AMS sample may very well not be representative of the bulk material, and the results will be very sensitive to the particular mineral grains which present themselves to the primary ion beam. Thus a heterogeneous distribution of Pt and Ir within a sample could result in widely disparate readings which would have to be interpreted accordingly.

While a study of Ir in four samples across the boundary at Stevns Klint gives a profile (Fig. 1) which is similar to that found by Alvarez *et al.* (1980) the Pt profile of the same samples (Fig. 2) is extremely erratic, showing large variations in count rate with time. The sample showing the largest variation is also the most pyritiferous, and the changes in Pt count rate may be ascribed to the presence or absence of pyrite grains under the primary ion beam. It would appear from this that Pt is concentrated in the sulphide phase, which might imply that some marine geochemical process has been at work to achieve this. The same erratic behaviour is not apparent in the Ir profiles, but there the count rate was much smaller, and the statistics gave less confidence.

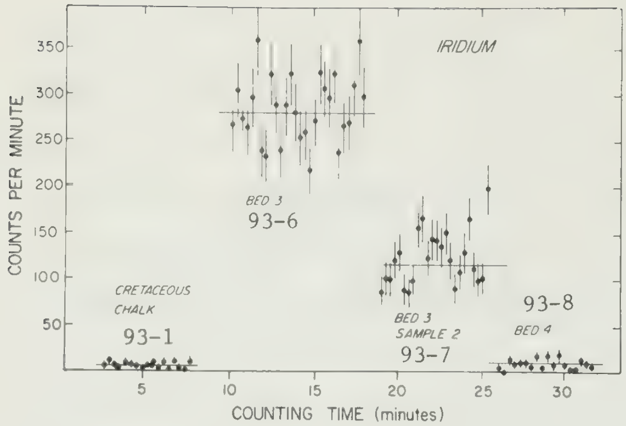

Fig. 1. ^{193}Ir count rate plotted against time while analysing four samples across the Cretaceous-Tertiary Boundary, Stevns Klint, Denmark.

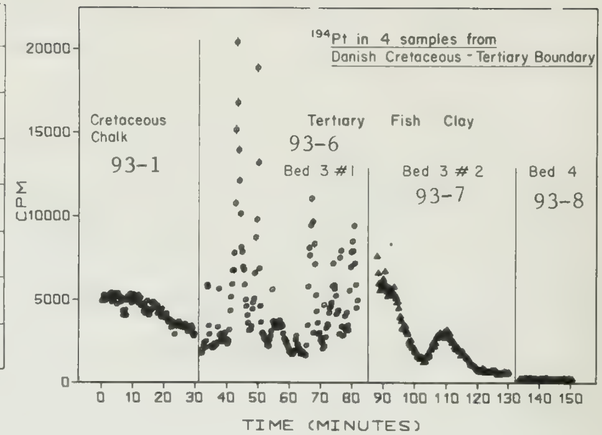

Fig. 2. As for Fig. 1 but for ^{194}Pt.

Table 1

Ir and Pt by AMS, Stevns Klint. Dec 1980

Sample		Ir(ppb)	Pt(ppb)
Tertiary	Bryozoan Lste	0.13	16
	Br'n Clay93-8	2.7	2.3
	92-4	9.3	148
Fish	Bl'k Clay93-7	5.3	10
	92-3	2.7	26
Clay	92-2*	14	163
	Pyrite 92-2+	9.3	87
10-20cm	rich 93-6*	87	452
	Clay 93-6+	12	14
	Grey Clay93-5	2.9	2.6
Cretaceous	Bryozoan Chlk	0.14	0.3

*heavy fraction +light fraction

Table 2

Ir and Pt by AMS, Glendive,Mont.Dec 1980

Sample	Ir(ppb)	Pt(ppb)
0168/9 3"above Z contact	0.07	9
0168/8 at Z contact	0.17	8
0168/7 3"below Z contact	0.13	28

University of Toronto Palynological collection numbers should all be of the form 15nn-n.
The order in the tables reflects the stratigraphic relationships.

Table 3

Ir and Pt by AMS, Danish sections. May 1981

Sample		Ir(ppb)	Pt(ppb)	Enrich*
Stevns Klint				
Section 1	92-6	0.82	7.2	476
	92-5	0.43	3.4	224
Heavy	92-4	109	667	476
fractions	92-3	24	106	52
	92-2	6.3	10	?
Section 2	93-9	4.6	4.8	4650
	93-8	0.63	48	200
Heavy	93-7	0.48	13	253
fractions	93-6	7.8	34	?
	93-5		72	245
Jutland				
Nye Klør (whole rock)		0.17	0.004	476
Kjolby Gard (heavy)			0.96	6250
Kjolby Gard (whole)		0.003	0.05	1

*Enrichment=(Wt rock sample)/(Wt heavy extract)

Table 4

Ir and Pt in Danish organic extracts. May 1981

Sample		Ir(ppb)	Pt(ppb)	Enrich*
93-10	Grey Clay	5.6	883	1626
92-3	Black Clay	3.9	24	348
93-7	Black Clay	4.8	58	90
93-5	Grey Clay	11.3	39	1140

*Enrichment=(Wt rock sample)/(Wt organic extract)

To establish whether concentration of Pt group elements (PGE) in sulphides has taken place, some of the more pyritiferous samples were separated into heavy and light fractions, and analysed individually. The results certainly seem to demonstrate a strong affinity of Pt and Ir for the chalcophile fraction, as can be seen from Tables 1 and 3. The other type of separation which was made was to extract only the organic component, and subject this to analysis for Pt and Ir. In preparing these organic extracts from the Danish Fish Clay, physical methods were used as far as possible to avoid any possibility that PGE could be concentrated chemically. Glauber's salt was used to break the rock down, and the resulting grains were passed through 30 mesh, 150 mesh and 40 micron sieves. The fractions retained by these three sieves were treated with heavy liquid (tetrabromoethane) and the light fractions from this stage were treated with 10% nitric acid and 35% hydrofluoric acid to extract the organic residue. Only milligramme quantities of organic material were obtained, but the analytical method has such sensitivity that these were analysed with ease, and were found to contain impressively large amounts of Pt and Ir. However, when the enrichment factor is taken into account, there is insufficient Pt and Ir in the organic fraction to account for the concentrations in the bulk material. It is interesting to note that the Ir anomaly reported by Orth *et al.* (1981) occurred in a coal seam, so the association of PGE and organic remains may be one which should be investigated in more detail. The analytical data for samples from Stevns Klint, N. Jutland, and Montana are shown in Tables 1 to 4.

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PARTICIPANTS

Luis Alvarez	Lawrence Berkeley Laboratory University of California
Walter Alvarez	Department of Geology and Geophysics University of California
Frank Asaro	Lawrence Berkeley Laboratory University of California
Pierre Béland	Pêches et Océans Québec, P.Q.
Klaus Brasch	Department of Biology Queens University, Kingston
Victor Clube	Royal Observatory, Blackford Hill Scotland
David Dilcher	Departments of Biology and Geology Indiana University
Paul Feldman	Herzberg Institute of Astrophysics National Research Council of Canada
Richard Grieve	Earth Physics Branch Department of Energy, Mines and Resources (Canada)
Ian Halliday	Herzberg Institute of Astrophysics National Research Council of Canada
Kenneth Hsü	Swiss Federal Institute of Technology Zürich
David Jarzen	Paleobiology Division National Museum of Natural Sciences (Canada)
John Jaworski	Environmental Secretariat National Research Council of Canada
Frank Kyte	Institute of Geophysics and Planetary Physics University of California, Los Angeles
Ted Litherland	McLennan Physical Laboratory University of Toronto
Dewey McLean	Department of Geological Sciences Virginia Polytechnic Institute
Helen Michel	Lawrence Berkeley Laboratory University of California
William Napier	Royal Observatory, Blackford Hill Scotland
Kris Pirozynski	Paleobiology Division National Museum of Natural Sciences (Canada)
John Rucklidge	Department of Geology University of Toronto

Dale Russell	Paleobiology Division National Museum of Natural Sciences (Canada)
Jan Smit	Geological Institute University of Amsterdam
Hans Thierstein	Geological Research Division Scripps Institution of Oceanography
Wallace Tucker	Center for Astrophysics Harvard College Observatory, Smithsonian Astrophysical Observatory.

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