

Reply to the comments by A D Shukla and P N Shukla

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The presence of the coccolith *Micula prinsii* is not indicative of Paleocene deposits but of Uppermost Maastrichtian marine deposits (see e.g., Pospichal 1996), so Shukla and Shukla must be misinformed.

As pointed out by Hansen *et al* (2001) the marine K/T boundary occurs right after the disappearance of Upper Maastrichtian planktonic foraminifera and coccoliths. The marine boundary definition is in this connection of interest only, as far as there is presence of two Ir anomalies at El Kef shortly after the extinction level. The presence of Ir-anomalies has no bearing upon the terrestrial boundary as earlier pointed out by us. We also noted that Ir anomalies have been recorded at other levels than the K/T boundary, and that Ir anomalies are not present at all marine K/T boundaries. Thus presence of an Ir-anomaly cannot be used as a boundary criterion and definitely not used as the only criterion for the recognition of the boundary.

The fossil evidence (including ostracods, charophytes and fishes) suggests placement of the Anjar lake section in the Maastrichtian (Bajpai and Prasad 2000) while there is no paleontological indication of the presence of a K/T boundary, on the contrary—there are Maastrichtian fossils above, between and below the three possible boundary levels!

The radiometric dates referred to by Shukla and Shukla (from Shukla *et al* 2001) are here presented in figure 1. The Anjar lake sediments are placed between flow 3 and 4 (and are of reversed polarity). The dates place the basaltic series in the later part of the Maastrichtian or in the earlier part of the Paleocene. Since we consider it unlikely that the dinosaurs persisted into Tertiary time, the Anjar

sediments with their presence of a partly articulate dinosaur skeleton most likely belong to chron 29R.

Taken at face value, the radiometric ages of the two underlying basalt flows (no. 2 and 3) indicate that the flows could well belong to magnetochron 30N, while the suggested placement of flow 1 in a still earlier normal chron is not supported by the data.

If the Anjar sediments with an exposed thickness of around 2 m should contain the K/T boundary the sediments would represent an accumulation period of around 550,000 years (namely the Cretaceous part of magnetochron 29R). The plot of magnetic susceptibility suggests 3 to 4 minor pulses in the lake sediments, which can be translated into 60 to 80 kyr. This also agrees with the intertrappean lake sediments reported by Hansen *et al* (1996) from Daiwal and Ranipur that both have about the same thickness. It seems unlikely that 2 m lake sediments should represent a time interval of 550 kyr. Other lake sequences (such as part of the North Horn Formation in Utah) are demonstrating accumulation rates of 4–6 m per 100 kyr.

Shukla and Shukla mention sediment thickness of up to 6 m. This is only obtainable if the different sections of the lake sediments are projected onto one imaginary vertical section as already mentioned by Hansen *et al* (1999). The thickness of the series from five different sections were recorded by Khadkikar *et al* (1999). No section covers more than 1.8 m. The section of BG 1 was sampled and measured by Mohabey, who noted, that the lower contact to the underlying basalt was not exposed (and it was not exposed at a later visit by Mohabey and Hansen) but Khadkikar *et al* show that Pit 1

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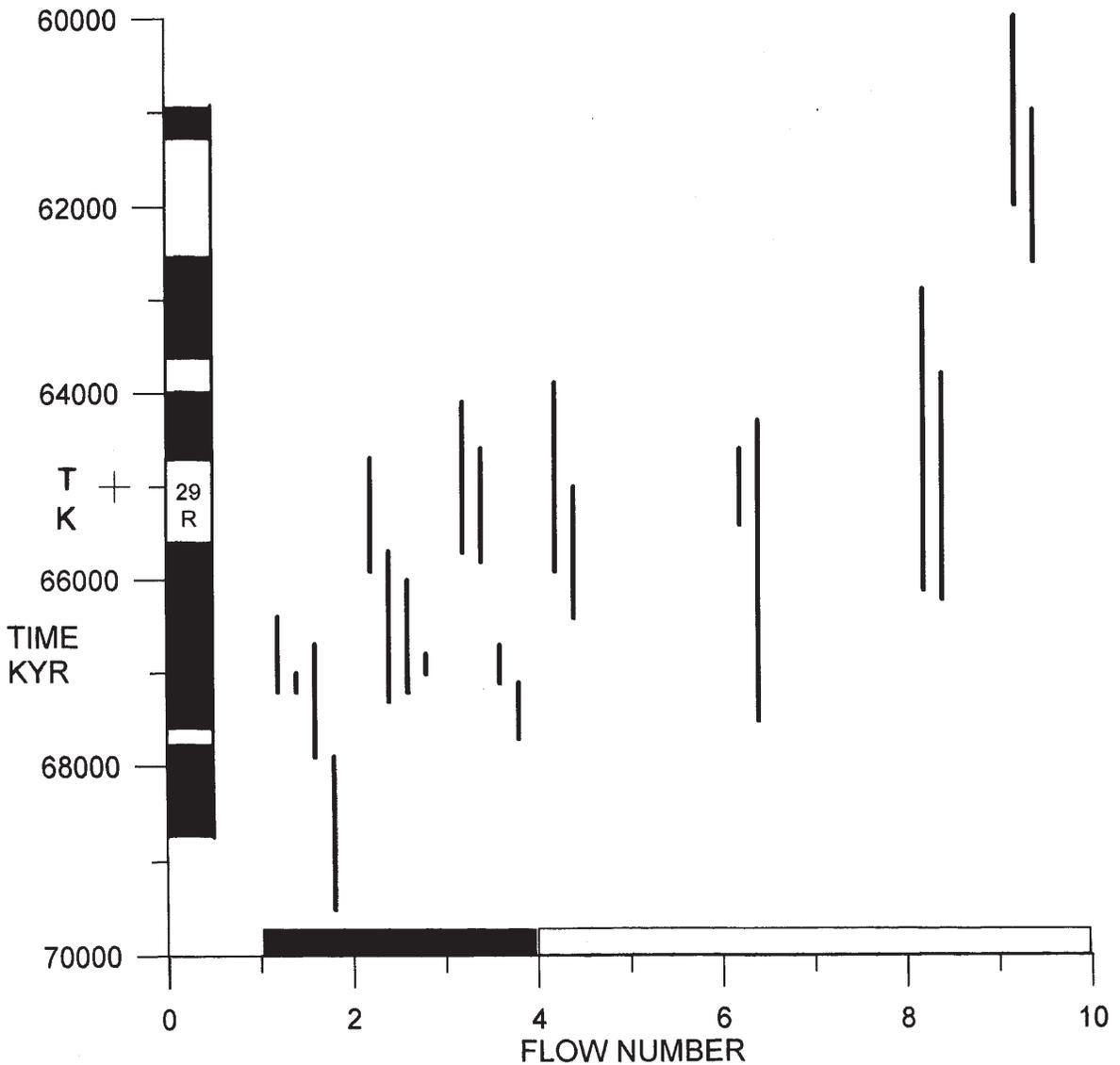


Figure 1. Plot of radiometric dates of basaltic flows from Anjar (data from Shukla *et al* 2001). Magnetic polarity scale from Cande and Kent (1995). Flows 1–3 are of normal polarity while flows 4–9 are of reversed polarity. The reversed Anjar sediments are found between flows 3 and 4.

(the BG 1) now exposes 20 cm of the underlying basalt.

In case the Anjar sediments contained the K/T boundary we should, in consequence of the above, expect a hiatus between the Anjar sediments and the underlying basalt flow 3. There are no reports of a hiatus of this length in the form of e.g., a lateritic bed between flow 3 and 4.

It would appear, that Shukla and Shukla are attempting to convert the marine boundary into a terrestrial event-boundary where various interesting finds are being linked to a marine stratigraphic boundary, but where a series of unexplained occurrences such as fullerenes, Ni-rich spinels (absent from Anjar), microspherules (absent from Anjar) etc. are being used as safe stratigraphic correlation tools. They are not!

The basic difficulty in establishing the presence of a K/T boundary in terrestrial sediments without biostratigraphic control, relates to the lacking precision of radiometric dating and magnetostratigraphy. So far, only two other independent methods are available: Stable carbon isotopes and the magnetic susceptibility pattern.

Shukla and Shukla argue, that magnetic susceptibility of the sediments may be influenced by a wide variety of factors and is of no value for the discussion of presence or absence of a K/T boundary in the Anjar Lake. This is a mistake, since the absolute value of magnetic susceptibility of the sediments is of no interest. Some sediments (such as e.g., the Danish white chalk) are even diamagnetic, but in spite of this, the pattern is recognizable from one locality to the next. It is the pattern (which

we believe to represent orbitally driven climatic changes and thereby precipitation pattern) that forms the basis of our correlations. In our studies of 61 different K/T boundaries covering all continents except Australia and Antarctica, we note that the patterns at identical levels constrained by magnetostratigraphy and biostratigraphy allows correlation with high resolution. We are therefore confident, on the basis of our K/T boundary sections world-wide, that a characteristic pattern is to be expected at the K/T boundary (as reported by Hansen *et al* 1993, 1996, 2001). This pattern is, however, not found in the Anjar sediments.

The black magnetic microspherules mentioned from the K/T boundary are absent at Anjar. They are only found in marine boundary sections (both at the K/T and the P/T) and represent the diagenetic infill of prasinophyte algae (Hansen *et al* 1986, 2000) and are unrelated to impacts.

The negative shift in organic carbon isotopes at the K/T boundary (absent at Anjar) is by our opponents mentioned as caused by world-wide forest fires. Where the shift is present, it is not abrupt, as would be expected from such a scenario. The abrupt shift in carbonate carbon isotopes reported earlier from El Kef is an artifact (Hansen *et al* 1996). The raised amount of organic carbon ascribed to forest fires was shown to start much earlier than the K/T boundary (Hansen *et al* 1987). The negative isotopic excursion starts at the K/T boundary, but does not have an abrupt course.

The presence of C4 plants at the K/T boundary is, at its best, wishful thinking. The Lameta sediments are well established (Brookfield and Sahni 1987; Mohabey *et al* 1993; Tandon *et al* 1995 and Mohabey 1996), as deposited in an alluvial-limnic environment under semi-arid conditions. The floral evidence (gymnosperms/conifers and angiosperms/dicots and monocots) were reported by Mohabey *et al* (1993) and the stable carbon and oxygen isotopes of the Lameta sediments along with dinosaur eggs (Sarkar *et al* 1991) demonstrate that C3 plants flourished under the semiarid conditions and constituted the food for dinosaurs.

The absence in the Anjar lake sequence of a negative organic carbon isotopic anomaly which is present at numerous other marine and terrestrial boundaries is a fact, which can not be explained by a suggestion of presence of organic carbon from plants with a C4 photosynthesis system and is a flaw by Shukla and Shukla since the C4 system was not "invented" by nature until mid Tertiary time (Bocherens *et al* 1993). Expressed briefly: Dinosaurs were not grass-eaters!

Much so-called organic carbon, which should correctly be characterized as non-carbonate car-

bon, originates from forest fires and can be characterized as soot. Non-carbonate carbon from plants (C3 type) registers the isotopic composition of the ambient atmospheric CO₂ and constitutes an independent correlation tool, which, along with the variation pattern in magnetic susceptibility, is a possible correlation tool between marine and terrestrial sequences.

The non-carbonate carbon present in the sediments most often is preserved in the form of charcoal and soot. Both components are graphitic in nature and as such very difficult to alter isotopically. They may be oxidized, but this will lead to their disappearance, which is not the case at Anjar.

In the Chui Hill section the presence of the K/T boundary is indicated by the susceptibility pattern in the Upper Limestone member (cycle zero of Hansen *et al* 1996) along with the development of a negative organic carbon isotopic anomaly. No typical Paleocene fossils occur above the K/T boundary, but the same is true of Maastrichtian fossils. The magnetic susceptibility curve from Chui Hill matches well that from the boundary stratotype at El Kef, Tunisia. The possible absence of fullerenes, micro-diamonds, nano-sized iron oxides, Ni-spinels, microspherules and iridium is of no significance for the placement of the K/T boundary in this and other sections.

The presence of low quartz paramorphs after high quartz with glass inclusions is the ultimate indicator of rhyolitic volcanic ash. We never indicated that the rhyolitic ash beds (each associated with an underlying Ir-enriched, rusty, discontinuous horizon) in the Anjar sequence should stem from the Anjar basalts.

Rhyolites exist in the region as Maithani *et al* (1996) reported from Gujarat (Osham Hills, Dhori Nes and other localities in Saurashtra). However, the ash beds do not necessarily originate from India, since rhyolitic ash may travel enormous distances (2000 km in the case of the rhyolitic ash bed at the P/T boundary in China (Yin *et al* 1992).

Shukla and Shukla mention a series of intertrappean sections studied for their content of Ir. It should be pointed out that Chui Hill, Bergi Canal and Jirabad sections are infratrappean and belong to the Lameta Beds.

It is difficult to understand why Shukla and Shukla are calling upon a current or wave action in connection with a tidal environment to explain the deposition of the "lenticular cherty limestone horizon" above the uppermost Ir-enriched stripe. Our interpretation differs from that of our opponents as it is not a lenticular body but represents a palustrine carbonate flat deposited in emerging shore-line areas of the lake. There is no evidence of a tidal flat and it is not necessary to call upon turbulence in order to precipitate carbonates.

It appears wishful thinking to postulate an initial calm period where the shales bearing the Ir-stripes were deposited which should later change to a turbulent environment. One can not deliberately ignore that the shale in question (original lithology by Tandon *et al*, Anjar Field-workshop, 1996, organized by PRL), is rich in shell hash and weathered Deccan basalt debris pointing to a somewhat agitated environment during deposition.

The comment that we have carried out the work without sedimentological analysis is wrong. The section at pit BG-1 has been sampled at cm scale with lithologging for the purpose of our study and we are well accustomed with the litho- and bio-facies of the section at Anjar.

It would appear that the interpretations by Shukla and Shukla were made in order to explain the presence of Maastrichtian fossils above the iridium stripes with the intent of demonstrating their reworked nature. No amount of reworking can explain the presence of a semi-articulated skeleton of *Titanosaurus indicus* between the two lower Ir-stripes (Mohabey 2001) as well as the bulk concentration of eggshell fragments and ostracodes (Bajpai and Prasad 2000) above the third and topmost Ir-stripe.

Courtillot *et al* (2000) mentioned in connection with the red stripes that "these levels cannot be regarded as continuous horizons: they are not well defined and locally display some splitting." Since the overlying rhyolitic bentonites are well-defined, there does not seem to have been enough agitation to prevent their deposition. In case the red stripes have been deposited as air-fall layers they should also be expected to form continuous horizons like the bentonites. By contrast, an origin from leaching of the bentonites fits well with the discontinuous nature of the stripes and thereby supports the interpretation by Hansen *et al* (op. cit).

In their closing remarks our opponents call upon Courtillot *et al* (2000) as having presented evidence that "amply prove the presence of KTB at Anjar." This involves (according to Courtillot *et al*) a time period of 1–2 myr to deposit the 2 m lake sediments!

The title of our contribution definitely does not need any changes!

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