Extended period of extinctions across the Cretaceous/Tertiary boundary in planktonic foraminifera of continental-shelf sections: Implications for impact and volcanism theories: Discussion and reply

Discussion

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Keller's (1989) interpretation of the Cretaceous/Tertiary (K/T) boundary section at the Brazos River section is significantly weakened by lack of attention to physical stratigraphy and sedimentology. Her discussion of mode and tempo of extinctions in the section is at best incomplete if sedimentology is not considered, and the biostratigraphically defined boundary of this section is largely irrelevant to the question at hand: How much time is represented by the coarse-grained bed and capping mud below that boundary?

The unusual, coarse-grained layer in the Brazos sections has been interpreted as a storm deposit, a turbidite, a mega-tsunami deposit, and a condensed section (see review in Bourgeois and others, 1988). Consider the stratigraphic implications of any one of these interpretations. The first three indicate virtually instantaneous deposition; the last, very slow deposition over thousands of years or more. The upper part of this bed and directly overlying mudstone, as sampled at Brazos 1 (not in the core Keller studied), are rich in iridium (Ir). This capping, Ir-rich sediment could be explained, for example, by settling out, through the water column, of airborne dust from a bolide impact, after a mega-tsunami swept across the shelf. Post-impact or post-tsunami volcanism might also be responsible for the Ir. Alternatively, a condensed section could be rich in cosmic dust. Dust of silt size or finer will settle out much more slowly than sand (two orders of magnitude or more). Thus the tsunami/impact interpretation would assign weeks to months to overlying clay layers; the condensed section interpretation would assign probably thousands of years. Only if the Ir was generated by volcanism *might* the layer represent sedimentation rates similar to bracketing (background) rates.

Without consideration of these processes, Keller (1989) quantifies time in the section by using magnetostratigraphy plus over-all stratigraphic thickness (of Chron C29R) to generate sedimentation rates. This technique is acceptable to determine *average* accumulation rates, but *for background sedimentation rates only.* Its accuracy, especially given the nature of the section, is on the scale of an order of magnitude or worse, and cannot be applied to unusual features within the section (Bourgeois, in press). Keller applies this rate universally to a section that clearly exhibits evidence for episodes of severe erosion and very rapid deposition, as well as very low rates of deposition, the latter within the Paleocene. For example, she assigns a time-span of ~295,000 yr to the erosional surface below the coarse-grained bed. Clearly only one significant figure should be used in this estimation, but Keller has not even presented a valid way to estimate the amount of erosion that took place, particularly because her samples came from a core. Even in outcrops, only the *relief* on this erosional surface can be measured, which gives a *minimum* amount of erosion.

Keller (1989) stated that Bourgeois and others (1988) "suggested that the K/T boundary based on first appearance of Tertiary species is misplaced in the Brazos River section. . . ." This statement is not a true representation of our position and obscures scientific questions at the expense of definitions. The first appearance of Paleocene fossils in the section is typically 20–30 cm above the top of the graded sandstone bed. The intervening mudstone is devoid of macrofossils. If the K/T boundary is formally defined as the first appeareance of Paleocene fossils, then technically, the coarse bed is not "at" the boundary, it is below the boundary. No problem. How much time does this 20- to 30-cm layer represent? How much time would we expect to pass between an impact, its aftermath, and the first appearance of new micro-organisms? The determination of how long in time below the boundary the coarse layer is, and whether or not it is associated with the terminal Cretaceous extinction, depends significantly, albeit not exclusively, on *sedimentological* analyses.

Keller (1989) does not disagree fundamentally with the sedimentological analysis of Bourgeois and others (1988). She attacks, however, a verbal suggestion made at Snowbird II (not in our paper, as she implies, and made originally by Jan Smit, though I later repeated it). The suggestion is that *dwarfed* forms in mudstone *below the boundary* as she defines it (not survivors, as she states in her paper) may be part of the graded tsunami deposit. She bases her attack on six reasons, which will not be completely repeated here due to space limitations. Reason (1) is not true; the bed *is* well sorted. Reason (2) states that the iridium anomaly is sharp—it is not—and Keller (1989) misplaces it. Reason (3) has no importance if the tsunami bed was deposited rapidly; one would still expect a coincidence of the $\delta^{13}C$ anomaly with the biostratigraphic boundary. Reason (4) only misrepresents the verbal suggestion made at Snowbird II (see above). Reasons (5) and (6) have to do with section *above* the boundary and are not relevant to the problem of how much time is represented by that 20–30 cm between the graded sand and the first appearance of Paleocene forms. In addition, T. Hansen's observations (summarized in Bourgeois and others, 1988) that this mudstone layer is essentially barren of *macrofossils* tends to support an interpretation that the layer was deposited relatively rapidly.

I am very familiar with the Brazos River sections, where I have conducted detailed sedimentological analyses. Those analyses are not critically examined in Keller (1989), yet her paleontological interpretations are

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seriously affected by those analyses. Keller's paper is a good illustration of the danger of using one technique, in this case biostratigraphy, without consideration of sedimentology and physical stratigraphy, to answer significant questions about the nature of the K/T boundary.

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Reply

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Bourgeois' critique is predicated on the assumption that an impact-generated tsunami bed at the K/T boundary at Brazos River, Texas, is a fact rather than an interpretation. For instance, with regard to our disagreement in placement of the K/T boundary, she states: "No problem . . . How much time would we expect to pass between an impact, its aftermath, and the first appearance of new micro-organisms?" She then argues that the amount of time is significantly determined by *sedimentological* analysis (her emphasis) and that the "biostratigraphically defined boundary of this section is largely irrelevant to the question at hand." Despite this dismissal of biostratigraphic data, her two major objections to Keller's paper are (1) the placement of the K/T boundary and (2) determination of average sediment-accumulation rates. In addition, she specifically criticizes the illustration of the Brazos-core section in Keller's (1989b) survey paper as inappropriate because Ir analysis was done only at the Brazos-1 section, but she ignores the fact that Keller (1989a) earlier published a detailed faunal analysis of three Brazos sections, including Brazos-1 where the Ir profile was discussed. In the survey paper, Keller (1989b) chose the Brazos-Core as a representative Brazos section because the cored section has a more extended stratigraphic record and is completely comparable in its faunal sequence to Brazos-1 and Brazos-CM (Cottonmouth Creek). In the following, I will address these two major objections.

(1) Bourgeois' critique of Keller's placement of the K/T boundary in the Brazos River sections places major emphasis on the position of the uppermost Ir anomaly as a determining criterion for this boundary. Although the Ir profile of this section is very interesting with its multiple peaks (2 or 3 peaks; Ganapathy and others, 1981; Asaro and others, 1982; Hildebrand and Boynton, 1988), its interpretation is not unequivocal. Such anomalous Ir distributions may imply post-depositional alteration and transport of iridium, multiple impact events, or marine flooding surfaces (Donovan and others, 1988). Even experts apparently do not place much trust in the Brazos River iridium distribution (for example, when asked of his assessment, Frank Asaro publicly called the Brazos-1 iridium data "rubbish" at the Snowbird II Meeting in 1988). Correlating the multiple Ir peak profiles of Ganapathy and others (1981), Asaro and others (1982), and Hildebrand and Boynton (1988) to outcrop samples collected at different times and locations from those collected and studied by Hansen and others (1987), Bourgeois and others (1988) and Keller (1989a, 1989b; Hansen provided samples for Keller's study) compounds the problem because of lateral variability in the thickness of the lithologic units. Jiang and Gartner (1986, p. 234) observed that the Brazos River outcrop is laterally variable even at a distance of 1 m, and the variability and change in thickness vary both downstream and upstream over a distance of 1.5 km. The precise relationship of the Ir peaks to the microfossil datum events can thus be determined only in paired samples. Moreover, the Ir anomaly at Brazos-1 or in any other K/T boundary section does not define the K/T boundary, but it serves as a supplementary geochemical criterion similar to the $\delta^{13}C$ shift, the increase in TOC, and the thin rusty red layer found at the base of the boundary clay, all of which are found in the most complete K/T boundary sections. In the Brazos-1 section, there appears to be a coincidence of these geochemical signatures with the first occurrence of Tertiary species, which strongly indicates a correlative stratigraphic interval to the K/T boundary elsewhere.

The only Brazos River study of microfossils and Ir analyses in either paired samples or samples subsequently taken at the same location was presented by Ganapathy and others (1981) and subsequently by Jiang and Gartner (1986). Their assessment of the K/T boundary based on the first appearance of Tertiary species in relationship to lithology and the iridium distribution therefore seems the most accurate to date. Keller interpreted her placement of the uppermost Ir peak after many discussions with both Gartner and Hansen, who do not agree on this issue. In this controversy, it is instructive to compare the lithologic description of the "tsunami" complex given by Bourgeois (in Bourgeois and others, 1988) with that of Jiang and Gartner (1986, p. 234). Both authors agree that an irregular scoured surface (hiatus of Keller, 1989a, 1989b) is overlain by a coarse glauconitic sand of variable thickness; a dense, hard, rippled calcareous sandstone; a 2- to 3-cm-thick layer of soft, light gray, sandy clay or mudstone; and capped by a 10-cm-thick, light gray, micritic chalk.

Above this micritic chalk, Bourgeois and others' lithologic description diverges from that of Jiang and Gartner (1986). A 10-cm-thick claystone (unit G) above the micritic chalk was interpreted as an impact fallout layer by Bourgeois and others (1988), followed by a 5-cm-thick, sandy bed. In contrast, Jiang and Gartner (1986) did not observe this sandy bed, but Gartner (1990, personal commun.) observed that some discontinuous sandy lenses are present. With regard to the lithologic placement of the K/T boundary, Jiang and Gartner (1986, p. 234) observed, "approximately 15–17 cm above the chalk is an inconspicuous, thin, clayey bed, which probably marks the precise position of the Cretaceous/Tertiary boundary." Gartner (1990, personal commun.) described this layer as a laterally continuous, thin (a few millimeters thick), rusty brown, sandy clay which closely correlates to the position of the upper Ir peak of Ganapathy and others (1981) and the first appearance of Tertiary nannofossils. Asaro and others (1982, p. 238), who collected their samples "within about one meter" of the samples collected by Ganapathy and others (1981), also found two major peaks, "one at about 3–4 cm and the other at about 16–17 cm above the top of the micrite (chalk) ledge," in agreement with the earlier findings of Ganapathy and others (1981). A few-millimeter-thick, rusty layer and Ir anomaly characterizes the K/T boundary and the first occurrence of Tertiary nannofossils and planktic foraminifera in all major boundary sections, including El Kef, Stevns Klint, Caravaca, Agost, and Sopelana (Keller, 1989; Smit, 1982, 1989; Schmitz, 1988). Keller (1989a, 1989b) found the first Tertiary planktonic foraminifera in Brazos-1 in a 10-cm interval channel sample spanning the interval between 10 to 20 cm above the unit described by Hansen and others (1987) as tsunami deposit and within unit I which Hansen and others (1987, p. 233) described as "a return to normal shelf suspension-sedimentation." The first appearance of Tertiary planktonic foraminifers is therefore well within the error margins of the thin, rusty brown, sandy clay described by Jiang and Gartner (1986), which closely correlates to the uppermost Ir peak and first appearance of Tertiary nannofossils. Within the limits of the biostratigraphic data, correlation of the geochemical anomaly, the lithologically distinct, rusty brown layer, and the placement of the K/T boundary based on the first appearance of Tertiary species in the Brazos River sections are all in agreement with the placement of this boundary in sections worldwide where faunal, lithological, and geochemical criteria are used to characterize the Cretaceous-Tertiary boundary interval. The Brazos River biostratigraphic data therefore cannot be lightly dismissed as irrelevant.

It is evident from Bourgeois' critique that her concern over the "lack of attention to physical stratigraphy and sedimentology" that she ascribes to Keller refers to the tsunami hypothesis rather than the data. Bourgeois assumes that the heterogeneous lithologic unit at Brazos-1 represents an impact-generated tsunami deposit and hence must represent the K/T boundary. Incidentally, there is considerable argument among sedimentologists with respect to this interpretation (Donovan and others, 1988). In contrast, Keller considers this lithologic complex as incidental to the determination of the K/T boundary, especially in the presence of the characteristic faunal, geochemical (Ir, $\delta^{13}C$), and lithologic criteria that mark this boundary globally.

It is clear from the detailed faunal discussion by Keller (1989a) and in the summary paper criticized by Bourgeois (1989b) that the characteristic faunal changes begin well above the tsunami deposit. In addition, stable-isotope analysis of the Cretaceous planktonic survivor *Heterohelix globulosa* and the benthic foraminifer *Lenticulina* sp. in the Brazos core (Barrera and Keller, 1990) as mentioned by Keller (1989b) shows the onset of the characteristic K/T boundary $\delta^{13}C$ shift coinciding precisely with the first appearance of Tertiary species in the Brazos core section. This $\delta^{13}C$ shift represents a global environmental change and has been reliably correlated with the K/T boundary worldwide (Zachos and Arthur, 1986). The Brazos core stable-isotope data show a close correlation between the nine species extinctions, about 20 cm above the boundary and with the maximum negative $\delta^{13}C$ values or maximum environmental stress. There are no significant variations in the stable-isotope record below the K/T boundary, including the tsunami deposit. Bourgeois dismisses this critical isotope data, including the $\delta^{13}C$ shift (as well as many other reasons discussed by Keller (1989a, 1989b) as having "no importance if the tsunami bed was deposited rapidly; one would still expect a coincidence of the $\delta^{13}C$ anomaly with the biostratigraphic boundary." I fail to understand this seemingly circular reasoning which is predicated on the assumption that a K/T boundary impact generated the tsunami bed. What if the "tsunami bed" simply represents infilling of incised topography during the lowstand of the latest Maastrichtian sea-level regression (Donovan and others, 1988)?

(2) Bourgeois claims that Keller did not consider the geologically instantaneous time of deposition of the tsunami bed in her estimates of average sediment-accumulation rates. In fact, she implies that Keller assumed constant sedimentation rates through the tsunami bed. Obviously, Bourgeois is unaware of the thorough discussion on this topic presented in Keller (1989a, p. 315–316). Keller's estimate of a hiatus spanning about 295,000 yr is based on the assumption of constant sedmentation rates through Chron 30°N and up to the scoured base of the tsunami bed, as well as on the assumption of constant sedimentation rates above the tsunami bed between the K/T boundary and Chron 29R. The tsunami bed is thus excluded in this estimate of sedimentation rates. Although I agree that the estimated 295,000 years missing at the base of the tsunami bed may be off by an order of magnitude or more as Bourgeois claims, this is indeed a minimum estimate based on a conservatively low sedimentation rate of 0.4 cm/1,000 yr.

Bourgeois also claims that "Keller has not even presented a valid way to estimate the amount of erosion that took place, particularly because her samples came from a core." I fail to understand this argument. Why is a core with good paleomagnetic and faunal data not valid for determining average sediment accumulation rates?

In conclusion, Bourgeois' critique of Keller's (1989a, 1989b) placement of the K/T boundary and her interpretation for the placement of the uppermost Ir anomaly at the top of the tsunami deposit (top of unit G) instead of 16–17 cm above the micritic chalk (unit F) cannot be supported by available data, including the iridium profiles of Ganapathy and others (1981), Asaro and others (1982), and Hildebrand and Boynton (1988). The coincidence of first Tertiary nannofossil and planktonic foraminifera, the onset of the $\delta^{13}C$ shift, the uppermost Ir peak and the "rusty brown, sandy clay" layer in the Brazos-1 section, as also in all the best K/T boundary sections worldwide, indicate that this interval represents the K/T boundary. Bourgeois' critique seems largely directed at reviving the impact/tsunami event hypothesis while disregarding the mounting evidence to the contrary.

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