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Discussion

Reply to 'Chicxulub impact predates K–T boundary: New evidence from Brazos, Texas' Comment by Schulte et al.

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1. Introduction

We appreciate this opportunity for further discussion of the Brazos, Texas, K–T boundary sequences and their timing with respect to the Chicxulub impact. Keller et al. (2007) used a multidisciplinary approach to document the stratigraphy, paleontology, mineralogy and geochemistry of the newly drilled Mullinax-1 core and a new outcrop sequence. Based on this multi-proxy dataset very strong evidence was presented that reveals that the Chicxulub impact predates the K–T mass extinction (Keller et al., 2007). Schulte et al. take issue with this approach and our findings largely because they believe that the Chicxulub impact caused the K–T mass extinction and therefore the K–T boundary must be placed at the impact spherule layer (Schulte et al., 2008-this volume; Schulte et al., 2006; Smit et al., 1996). We welcome this opportunity to clarify misunderstandings, misconceptions and misinterpretations of the K–T record in Texas and elsewhere.

At the heart of our disagreements is the decades old controversy about the cause of the end-Cretaceous mass extinction. Schulte and collaborators have long argued that the sandstone complex, or event deposit, with impact spherules at the base in NE Mexico and Texas mark Chicxulub impact-generated tsunami deposits at the K–T boundary (Schulte et al., 2006; Smit et al., 1996; Smit, 1999; Schulte et al., 2003; Smit and van der Gaast, 2004). Keller and others have documented that these sandstone complexes were deposited over a long time period, that the K–T boundary is above these deposits and the original Chicxulub impact layer is in late Maastrichtian sediments predating the K–T mass extinction by about 300ky (Keller et al., 1997; Keller et al., 2002a; Keller et al., 2003a; Keller et al., 2004;Gale, 2006). With the stratigraphic sequences in Mexico and Texas in direct conflict with the Chicxulub as K–T impact scenario, Schulte and collaborators now consider these sequences as too complex to reveal the K–T and Chicxulub impact history. Instead, they favor condensed deep-sea and terrestrial sections as the ultimate support for the Chicxulub K–T age claim because they juxtapose the spherule layer and early Danian sediments (Martínez-Ruiz et al., 2002; Macleod et al., 2006; Olsson et al., 1997). But the ultimate test for any historical sequence of events lies in the expanded records of continental shelf and slope areas where high sedimentation rates reveal stratigraphic separation and normal sedimentation between events, such as we documented for Texas (Keller et al., 2007). We stand by our published data and interpretations and present new data and graphics to clarify their misconceptions and misrepresentations on the placement of the K–T boundary.

2. Placement of K–T boundary

Schulte et al. claim that the K–T boundary is based on just two criteria: (1) the evidence of an asteroid impact and (2) the mass extinction in planktic foraminifera (Molina et al., 2006). In the early 1990s when the El Kef and Elles sections of Tunisia were studied by the ICS working group (including Keller), for the nomination of the K–T stratotype, the criteria

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for the placement of this boundary included: (1) the lithological break from marl to clay, (2) the 2–3mm oxidized red layer at the base of the boundary clay, (3) maximum Ir anomaly in the red layer, (4) Ni-rich spinels, (5) the negative $\delta^{13}C$ shift, (6) the mass extinction horizon and (7) the first appearance of Danian planktic foraminifera within a few cm of the base of the boundary clay (Keller et al., 1995). Arbitrarily reducing this list to just two criteria, the mass extinction and asteroid impact, does not change the existence or importance of the other defining criteria. In our global analyses of over 100 K–T sequences, the most consistent markers are the mass extinction in planktic foraminifera, the first appearance of Danian species and the $\delta^{13}C$ shift. The $\delta^{13}C$ shift is a global oceanographic signal and therefore provides an independent check on paleontological and impact criteria, which is critical to avoid circular reasoning. One cannot test the hypothesis that the Chicxulub impact caused the K–T mass extinction by defining the impact as the K–T boundary, as Schulte et al. propose.

At Brazos, we placed the K–T boundary at the first Danian species, the $\delta^{13}C$ shift, and the mass extinction (Keller et al., 2007). These criteria fall at the same stratigraphic level 40cm and 80cm above the event deposit in the CMA-B outcrop and Mullinax-1 core, respectively. To avoid further misconceptions we show new paleontological and iridium data for the CMA-B outcrop in Fig. 1. Schulte et al. criticized these K–T criteria as based “exclusively on secondary – at best – and poor stratigraphic markers”, or that the $\delta^{13}C$ shift is “tentative at best and can only be useful in discontinuous sequences or low-resolution pilot studies.” These statements are very puzzling to any paleontologist working on the K–T boundary. Instead, Schulte et al. place the K–T boundary at the base of the event deposit (Schulte et al., this volume, Fig. 1) solely on the basis of reworked Chicxulub impact spherules and the belief that they represent the time of the K–T impact. This is circular reasoning at best. Our study demonstrates that there are no geochemical or paleontological markers, no mass extinction or significant faunal changes at the base of the event deposit (Fig. 1).

2.1. Mass extinction and first Danian species

On p. 352 (Keller et al., 2007) we cautioned that “the mass extinction of all tropical and subtropical planktic foraminifera is diminished in the Brazos region because this species group is extremely rare or absent in the very shallow, low oxygen depositional environment”, which leads to the absence of the sudden mass extinction. Schulte et al. took the phrase “absence of the sudden mass extinction” out of context to argue that if the mass extinction is not as sudden as in open marine tropics, then there is no mass extinction. This simplistic and erroneous view is best dispelled with the new data shown in Fig. 1. Most species survived up to and just across the K–T boundary, similar to the tropics. Their presence in zone P0 may be due to reworking or survivorship. The extinction pattern is gradual rather than sudden and the mass extinction diminished as a result of the lower species diversity in shallow environments. Similar patterns have been documented for other Brazos sections, as well as shallow environments in Denmark, southern Tunisia and Egypt (Keller, 1989; Keller et al., 1993; Keller et al., 1998; Keller, 2002).

Schulte et al. seem fixated on the abrupt mass extinction pattern of condensed deep-sea sections without realizing that this pattern is often due to incomplete records, and does not apply to shallow water environments such as Brazos. Thus, they argue that our biozonation is inappropriate because “Keller et al. do not record the simultaneous extinction of Cretaceous taxa, (therefore) the base of biozone P0 cannot be established based on these data.” At Brazos, as well as El Kef and Elles in Tunisia and complete sections worldwide there is an overlap of Cretaceous species with the evolution of Danian species in zone P0 and even into P1a (Keller et al., 1995; Keller, 1989; Keller et al., 2002b; Keller, 1988) and this overlap is independently confirmed by the $\delta^{13}C$ shift (Fig. 1).

Inexplicably Schulte et al. wrongly assert that no evolutionary first appearances of species of any biotic group coincide with the base of the Danian in expanded continuous sequences. Since only planktic foraminifera evolved immediately in the aftermath of the mass extinction, other biotic groups (e.g., dinocysts and nannofossils) are immaterial to this argument. At the stratotype and co-stratotype sections of El Kef and Elles in Tunisia, the boundary clay (zone P0) is 50cm thick and the first appearances of *Woodringina hornerstownensis*, *Parvularugoglobigerina extensa* and *Globoconusa daubjergensis* occur in the basal 1–10cm (Molina et al., 2006; Keller et al., 1995; Keller et al., 2002b). Only the larger morphotypes of *G. daubjergensis* first appear in zone P1a (Keller, 1988). Therefore, it is ironic that Schulte et al. argue that Keller et al. (2002b) placed the first appearance of *G. daubjergensis* in zone P1a and use this argument to support their placement of the K–T boundary at the base of the event deposit at Brazos-1 (Schulte et al., this volume, Fig. 1).

2.2. K–T $\delta^{13}C$ shift

Schulte et al. extensively criticize our $\delta^{13}C$ data while ignoring the previously published record of the Brazos section (Barrera and Keller, 1990), which would have obviated the need for this discussion. One of their misconceptions is that there is a “strong diagenetic overprint” because the $\delta^{13}C$ curve parallels the calcite content. Barrera and Keller (1990) demonstrated that (1) foraminiferal shell preservation is pristine, and (2) that the gradual $\delta^{13}C$ shift in *Lenticulina* and *H. globulosa* is not due to reworking, but evidence that *H. globulosa* survived the K–T mass extinction. Our data parallels this record. In addition to *Lenticulina*, we also analyzed fine residues (38–63µ), rather than bulk rock, because contrary to their assertion, the fines consist mainly of small planktic foraminifera, whereas bulk rock can be biased by large benthics. The very low negative (− 7‰) values in the coarse grained event deposit and clasts are due to secondary calcite precipitated from isotopically light meteoric water. In contrast to the sudden $\delta^{13}C$ shift in condensed deep-sea sections, the K–T shift is gradual at Brazos due to the expanded

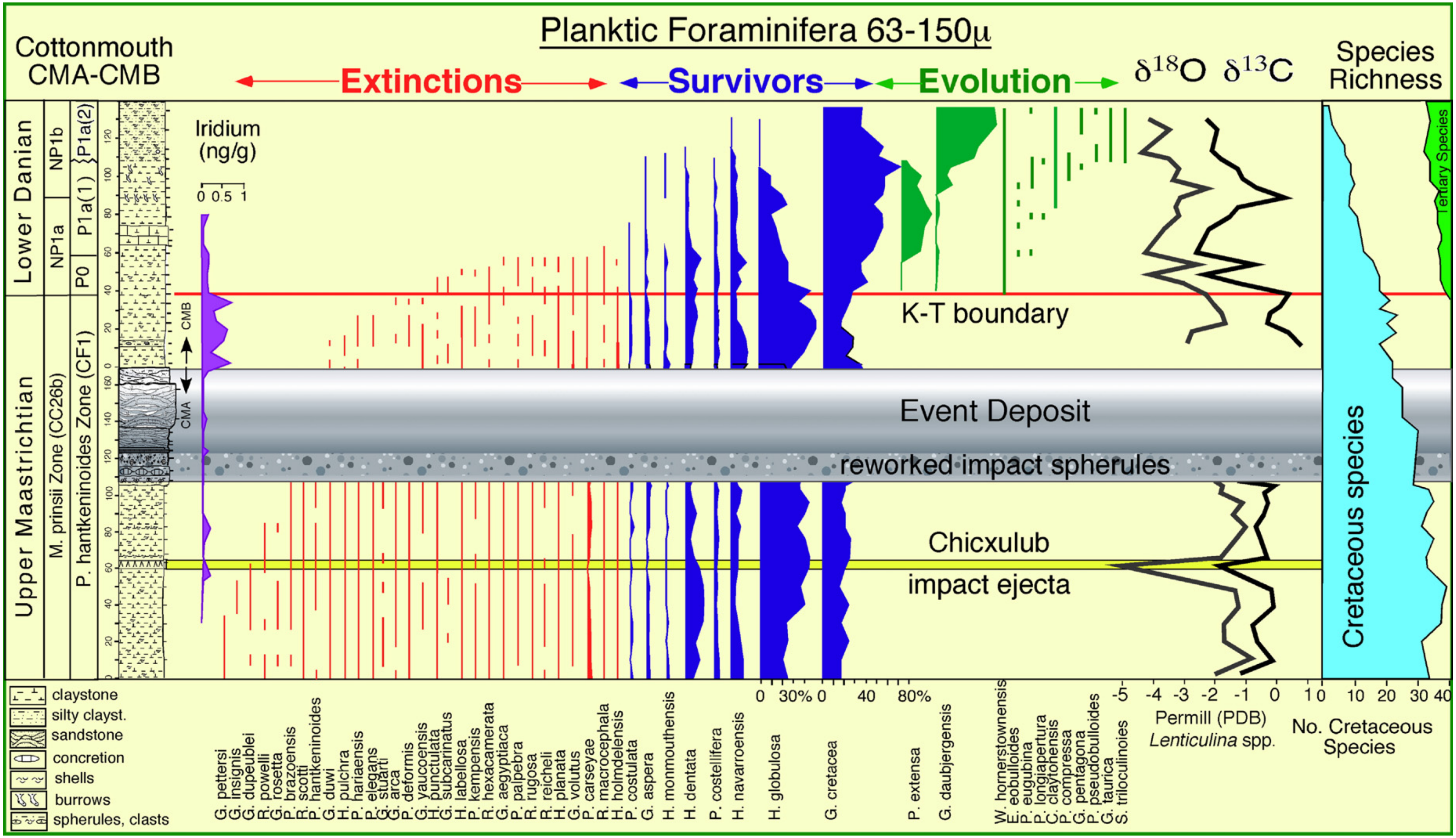

Fig. 1. Stable isotopes, species ranges and relative abundances of planktic foraminifera across the Chicxulub impact layer, event deposit and K–T boundary mass extinction at the Cottonmouth Creek CMA-B section. Note the gradual decrease in species richness beginning in the late Maasrichtian and continuing across the K–T boundary is largely due a shallowing marine environment that excludes deeper dwelling species. The K–T boundary is well marked by the mass extinction, first Danian species and negative carbon shift. Many Cretaceous species present in the 20 cm above the boundary are likely reworked. Ir concentrations do not coincide with the K–T boundary or the Chicxulub impact spherule layer.

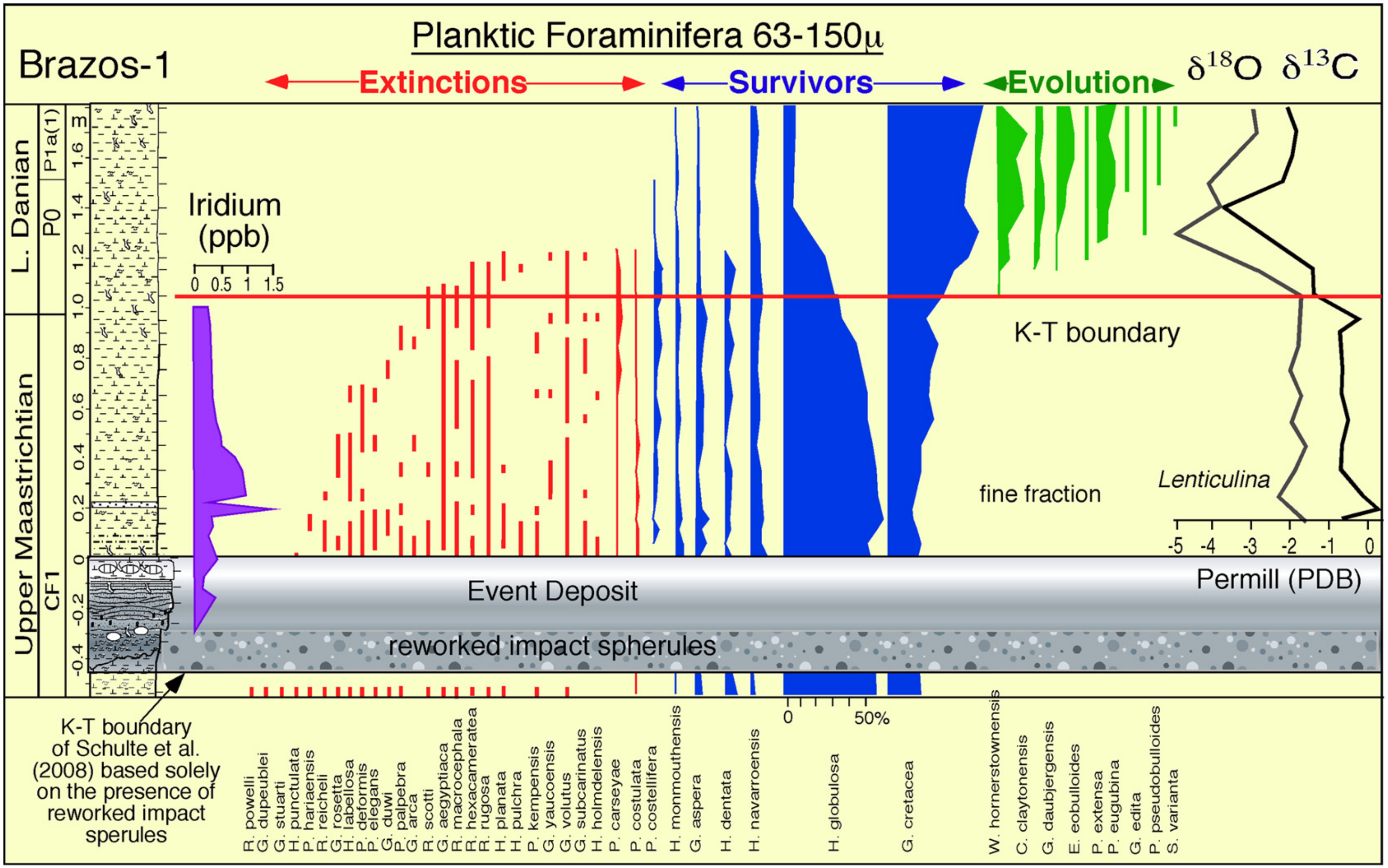

Fig. 2. Stable isotopes, Species ranges and relative abundances of planktic foraminifera across event deposit and K–T boundary at the classic Brazos-1 section. This sequence is similar to CMA-B, except that the interval between the event deposit and the K–T boundary is more complete (1 m compared with 40 cm), and a well-defined Ir anomaly is present. The mass extinction is well marked, but does not coincide with the Ir anomaly or the base of the event deposit with reworked impact spherules.

record, gradually reduced diversity and productivity (Fig. 2) (Keller, 1989; Barrera and Keller, 1990). The same gradual pattern was observed at the similarly shallow Stevns Klint section of Denmark (Keller et al., 1993). The sudden δ^{13}C shift observed in deeper distal sections noted by Schulte et al. is usually the result of condensed sedimentation.

3. Ir anomalies and impact spherules

3.1. Iridium

There is no iridium anomaly or impact spherule layer at the K–T boundary as defined by δ^{13}C and standard micropaleontological critera in the Brazos sections. The three Ir profiles of the classic Brazos-1 outcrop were all done on the same section with just a few meters of lateral exposure and where a discontinuous thin rust-colored sand layer is present. Rocchia et al. (1996) show the maximum Ir anomaly in the 1–2cm below this layer and a second anomaly immediately above, just as we show in Fig. 8 (Keller et al., 2007). Two minor Ir enrichments are present in the sandstone of the event deposit and just above it. Schulte et al. misrepresent our figure by placing the maximum Ir anomaly (Rocchia et al., 1996) above this sand layer in their Fig. 1 and then argue that we misrepresent the data. The lithologs for the Asaro et al. (1982) and Ganapathy et al. (1981) Ir profiles are more sketchy and for this reason we consulted with Tom Yancey (Texas A&M University), who guided their field party and confirmed the positions of the main Ir peaks near the thin sand layer, as well as the smaller peaks below. Schulte et al. (this volume, 2006) re-plotted these three sets of published Ir data in their Fig. 1 based on Hansens et al. 's (1987, 1993) interpretation and inexplicably affixed the misleading label “original data”. We conducted Ir and PGE analyses of several other sequences, and find significant variations in the profiles due to variable erosion. For example, Ir profiles in the CMB-A and Brazos-1 sections where the K–T boundary is 40cm and 100cm above the event deposit are different because of erosion (Figs. 1, 2).

3.2. Separation of Ir anomaly and spherules

In the Brazos sections the reworked spherule unit is at the base of the event deposit and always separated from the two small (0.4–0.6ppb) and the main (1.5ppb) Ir anomalies. Schulte et al. (this volume) creatively explain this separation as rapid fallout of the spherules after the Chicxulub impact, followed by much later settling of the iridium. They thus ignore the evidence of multiple horizons of trace fossils and truncated burrows that indicate deposition of the event beds occurred over a long time period marked by repeated colonization of the ocean floor alternating with storm deposits (Keller et al., 2007; Gale, 2006), as also observed in Mexico (Keller et al., 1997; Ekdale and Stinnesbeck, 1998). In areas where the spherule layer and the Ir anomaly are in close proximity, such as at El Kef, they explain this as the two ejecta layers having merged in simultaneous fallout. This interpretation is the basis for correlating the El Kef Ir anomaly with the spherule unit at the base of the event deposit in Brazos-1 shown in their Fig. 1. Why would the heavier spherules settle out simultaneously with the Ir in distant regions, but “considerably later” in proximal areas? Distance does not make Ir settle faster or spherules settle slower. In addition, the clay, iron, or glauconite spherules at the El Kef section are in no way similar to those from the Chicxulub impact and there is no genetic link.

Our new study of the same Brazos-1 sequence shows that there is no paleontologic, geochemical, lithological or impact justification to place the K–T boundary at the base of the event deposit (Fig. 2). No faunal changes coincide with the Ir anomaly. The K–T boundary is well-marked 1m above the event deposit by the first appearance of Danian species, the negative δ^{13}C shift and the mass extinction. As in the CMA-B section (Fig. 1), most species are present in the 20cm above the boundary either due to reworking or survival.

3.2.1. Reworked spherules in event deposit

We demonstrated that (1) spherules in the event deposits are not the original ejecta fallout — but reworked from an older layer as evident by the lithified clasts with well-preserved spherules, and (2) the original Chicxulub ejecta layer is present in the altered impact glass spherule layer (cheto smectite) 45–60cm below the event deposit (Keller et al., 2007). Schulte et al. misrepresent our data and interpretation with a mock argument asserting that we proposed the clasts were eroded from the cheto smectite clay. Clearly, the clasts originated from a lithified unit of the original spherule layer. This unit was probably deposited in very shallow waters, lithified and subsequently eroded, as suggested by the cracks infilled with spherules that indicate subaerial erosion. This is also indicated by the highly negative (− 7 to − 9‰) δ^{13}C values of the clasts that suggest secondary calcite precipitated from isotopically light meteoric water. It is only in the lithified clasts that spherule preservation is good, which in now way contradicts the clay altered (cheto smectite) original impact spherule layer as Schulte et al. argue (see below). Contrary to their argument, the three upward fining spherule-rich units of the event deposit in Mull-1 show characteristic re-sedimentation and dilution by glauconite, phosphate grains, broken shells and detritus.

4. Yellow clay — original Chicxulub ejecta layer

Schulte et al. claim that without “true Chicxulub ejecta spherules, i.e. round- or drop-shaped spherules with internal cavities and vesicles … a volcanic origin for the yellow clay layer is more plausible.” However, restricting Chicxulub impact ejecta to well-preserved spherules and relegating all altered glass to volcanic origin makes little sense, especially since they argue that the K–T clay, iron and glauconite spherules from the Tethyan realm represent Chicxulub spherules. It is well known that glass alters to clay. Therefore, it should not be surprising that the original spherule layer is weathered into cheto smectite clay minerals under the humid climatic conditions of West Texas, as also observed throughout Mexico, the Chicxulub crater core Yaxcopoil-1, Guatemala and Belize (Keller et al., 2004; Debrabant et al., 1999; Keller et al., 2003b). The presence of ghost spherules transformed into clay minerals in the yellow clay excludes a volcanic (bentonite) origin, as also evident by the absence of characteristic volcanic minerals (e.g. plagioclase, biotite, apatite, amphibole etc). Shocked quartz and Ir have never been observed even in the well-preserved spherule layers, as tacitly acknowledged by Schulte et al. in their Fig. 1. Moreover, the so-called Balcones volcanic province noted by Schulte et al. as possible origin for the yellow clay is much older (70–87Ma) with peak activity during the Campanian (Byerly, 1991; Spencer, 1969) and deposition as alkali basalt to phonolite lava flows, sills, dykes and rare thin bentonite ash and tuffs (Salvador, 1991; Ewing and Caran, 1982).

In contrast, the yellow clay layer at Brazos (CMA-B) is exclusively composed of cheto smectite and mineralogically similar to the clay in the overlying reworked spherule layers, as well as cheto smectite from altered impact glass in the Chicxulub crater breccia and spherule layers from Haiti, Belize and Guatemala (Debrabant et al., 1999; Keller et al., 2003b). In the yellow clay, as well as all of these altered impact glass layers, ESEM and EDX analyses of well-crystallized smectite reveal a webby morphology and show that the major element is a typical Mg-smectite (Si, Al, Mg with minor Fe) characterized by excellent crystallinity and very high intensity of the 001 reflection (Fig. 3). After heating, the 9.6 Å reflection is very reduced compared with ethylen-glycol solvated preparation implying a particular cationic configuration of the interlayer as observed in bentonite (Debrabant et al., 1999; Keller et al., 2003b; Caillère et al., 1982).

Schulte et al. claim that high potassium in the yellow clay is incompatible with pure smectite mineralogy and suggests the presence of a large amount of illite smectite mixed layers. However, XRD analysis shows that all the typical reflections characterizing pure smectite (Moore and Reynolds, 1997) are recognized (001 at 17 Å, 002 at 8.46 Å, 003 at 5.64 Å, 004 at 4.23 Å etc) and no illite mixed-layer peaks are detected. The significant K content is due to the fact that geochemical analysis was not performed on the clay fraction, but on the rare glass relicts found in the yellow clay and spherule layers of the event deposit. Indeed, the same method was used by Schulte et al. (2003) who reported similar high K_20 values (5%–8%) from spherules from NE-Mexico (Schulte et al., 2006; Schulte et al., 2003). The use of $FeO+MgO$, K_2O+Na_2O and CaO ternary diagrams, similar to Schulte et al.'s binary diagrams (Schulte et al., 2003, Fig. 9, p. 130), is therefore appropriate to characterize and correlate spherules, even though some diagenetic overprint occurred. Their critique that we did not correlate our spherule geochemistry with ejecta spherules from the Tethyan realm directly contradicts their own criteria for characteristic Chicxulub impact spherules (see above). Spherules found in the K–T boundary clay in the Tethys largely consist of iron, iron hydroxyde or glauconite and are clearly not of Chicxulub origin.

5. Sequence stratigraphy

Sea-level change was not the major topic of our research report and was only used in the discussion and summary Fig. 9 to illustrate the depositional environment of the Brazos sections (Keller et al., 2007). Yet, Schulte et al. accuse us of violating "well-established sequence stratigraphic concepts," then launch into a lecture on sequence stratigraphy and sea level analysis developed by Baum (one of the authors) and his colleagues at

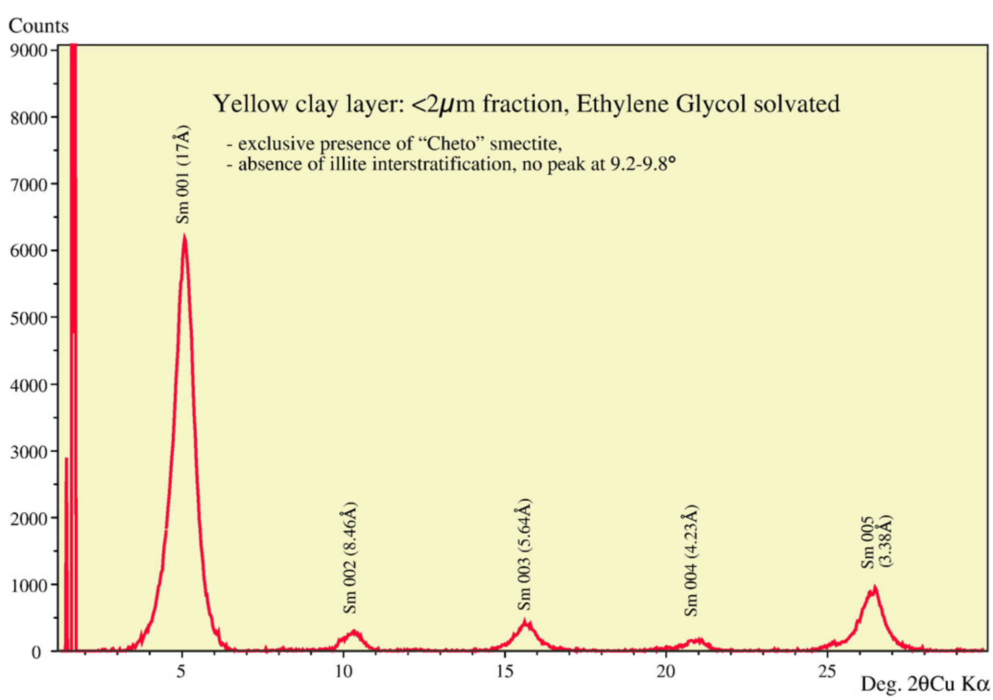

Fig. 3. XRD diffractogram characterizing the yellow clay layer (>2 μm clay fraction, Ethylen–Glycol solvated sample). Note the presence of a single phase of very well-crystallized Cheto Mg–smectite and the absence of reflections indicative of illite–smectite interstratification.

Exxon. They seem shocked that anyone would propose a major sea level fall with a concomitant subaerial unconformity in the late Maastrichtian, followed by a sea level rise through the K–T boundary, yet this has been proposed by various workers (Baum and Vail, 1988; Donovan et al., 1988; Haq et al., 1987; Haq et al., 1988; Loutit et al., 1988) (Baum pers. comm. to Schulte). The geochemical profiles (stable isotopes, TOC) from the Brazos River cores and outcrop are consistent with both relative and eustatic sea level changes in the Brazos sections (Abreu et al., 1998; Baum et al., 1994). They seem singularly transfixed with Fig. 9, centered around incised valleys and the use of the terms transgression and maximum flooding surface (mfs). Concerning the first issue, to our knowledge, none of the developers of sequence stratigraphy ever gave width, length and depth dimensions as criteria for incised valleys. In fact, many of the terms in sequence stratigraphy were purposely "neuter" terms related to geometries and not depositional processes. Moreover, Baum and Vail, (1988) included incised valleys in both the lowstand and transgressive depositional systems, and sometimes incised-valley-fill can be fairly confidently differentiated (Loutit et al., 1988).

Sometimes arguments ensue where two different terms exist for the same thing or two disparate definitions for the same word, such as transgression. According to Neuendorf et al. (2005), transgression can be defined as "spread or extension of the sea over land areas." Jervey (1988) has shown that eustatic sea level is rising in the early highstand, but at a lower rate of rise. With this in mind, the early highstand deposits are transgressing. But has the physical stratigraphic framework at the Brazos localities changed or become "erroneous" because transgression is defined differently by Schulte et al.? We think not.

Nowhere in our Fig. 9 is mfs labeled, but is described in the text as a burrowed omission surface. However, one could presume it to be between TST and HST. Schulte et al. are confused on the meaning/definition of mfs. Baum and Vail, (1988) preferred to use the physical term, surface of maximum starvation, to separate the transgressive and highstand depositional systems and gave criteria for recognition. They understood that, depending on the basin transect, eustatic sea level and paleo-water depths (relative sea level) typically continue to increase above the physically defined mfs (Keller et al., 2007, Fig. 9), before falling to the next unconformity/sequence boundary. Not to add to Schulte et al.'s confusion, except for basin floor fans, onlap occurs throughout a complete depositional sequence.

6. Omission of evidence?

Schulte et al. criticize our Brazos research paper on the basis that it is not a review paper and therefore omitted evidence "in and outside the Gulf of Mexico" that would support their viewpoint. Specifically, they claim "more than 24 recent ODP K–P drillcores all provide strong support for the genetic relationship between the Chicxulub impact event and the worldwide distributed K–P boundary ejecta layer" and conclude "Yet, Keller and her coworkers prefer to keep ignoring nearly all of it." There is a powerful irony in this accusation when they referenced not a single paper of our Chicxulub studies (Schulte et al., this volume). We did not ignore their papers. Over the past 10 years we have written several review papers (Keller et al., 1997; Keller et al., 2003a; Keller, 2005; Keller, 2008) and discussed their evidence and interpretations, as well as numerous research reports on over 45 K–T sequences with impact spherules. As for their claim, there are, in fact, not 24 drill-cores, but only three (Bass River, Blake Nose and Demara Rise (Martínez-Ruiz et al., 2002; Olsson et al., 1997; MacLeod et al., 2006) that juxtapose Chicxulub spherules and Danian sediments. Rather than a genetic link, this juxtaposition appears to be due to condensed sedimentation and erosion as reviewed in Keller (2008). Curiously, for none of these sections has high-resolution quantitative faunal analyses been published, which is necessary to determine how complete the sections are. Until such studies are published, these sections cannot be considered evidence for the "genetic link" claimed by Schulte et al. (this volume). Moreover, the condensed records of deep-sea or terrestrial sections cannot be considered as more complete than the high sedimentation records of continental shelf and slope areas.

7. Conclusions

Schulte et al. conclude that Keller et al. (2007) "have not made any case for Chicxulub as a pre-K–T impact." But they made their case by repeatedly resorting to factual misrepresentations, misinterpretations, out of context quotes, selective use of references, ignoring critical studies and bogus arguments. Amazingly, this was done in the most strident tone and accusations of misuse of biostratigraphy, geochemistry, mineralogy and sequence stratigraphy.

In our reply we have addressed the major issues they raised and provided new data that show the biostratigraphy and mass extinction relative to the Chicxulub impact layer in the Cottonmouth Creek (CMA-B) and Brazos-1 sections and the nature and origin of the yellow clay and spherules of the event deposit. Our detailed multi-disciplinary research results from the Brazos area stand as verifiable body of work and remain factually unchallenged by Schulte et al.'s arguments. The physical and stratigraphic separation, the detailed geochemical, mineralogical and paleontological analyses presented reveal a historical sequence of events that places the Chicxulub impact unequivocally in the late Maastrichtian prior to the K–T mass extinction. The event deposit, where Schulte et al. place the K–T boundary based on reworked impact spherules, is no more than an incised valley filled with eroded sediments during a low sea level. The mass extinction occurred at a considerably later time during a sea level rise and is marked by the global $\delta^{13}C$ shift.

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