

## Interactive comment on "Cenomanian to Coniacian Water-mass Evolution in the Cretaceous Western Interior Seaway of North America and Equatorial Atlantic" by James S. Eldrett et al.

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On behalf of all co-authors we thanks Anonymous Referee #2 for the recommendations to improve the paper. We believe we have satisfactorily addressed all the comments raised. Detailed responses (R1-24) for each comment are provided below.

Anonymous Referee #2 The study by Eldrett et al is a very nice contribution about the mid Cretaceous Western interior seaway and how WIS sediments are influenced by different water masses that are supposed to originate both from the north and from the south. The authors present a large amount of data that makes it sometimes very hard for the reader to follow the arguments because many data are only shown in the supplements but are discussed in the main text in length and are sometimes very

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important for the interpretation of the data. Here, it would be good to have some more information in the main figures (maybe one or two additional figures. Overall, I think that this is a nice contribution that is worth to be published in Climate of the Past. However, there are quite a few points that should be clarified by the authors to increase readibility of the text and the data interpretation. Furthermore, available datasets from the sites studied should be taken into account by the authors to support their statements and interpretation. Therefore, I recommend moderate to major revision. My main points are (in the order they appear in the manuscript):

1) Abstract: why is the abbreviation for the Cretaceous WIS written with a K instead of a C?

R1. This nomenclature for abbreviation follows conventional usuage and previous publications on the topic from the 1990's onwards; either shortening Cretaceous Western Interior Seaway to KWIS or Cretaceous Western Interior Basin to KWIB. As for the K-T boundary event the K is for Cretaceous and T for Tertiary; at the level of system/period, C is usually reserved for the Carboniferous. Within the Cretaceous, at the level of stage, C is used in this paper to abbreviate Cenomanian; if we abbreviated Cretaceous to C it may be confused with the C-T (Cenomanian-Turonian) boundary. The early workers of the Western Interior Seaway did not abbreviate; however to be more concise with fewer words we abbreviate so consistent with other publications.

2) The first paragraph and especially the first sentence of the introduction needs much more references to be included. Same is true for lines 20-30 on page 2.

R2. Our intention for the first sentence was to set the scene; with the detailed references relating to Large Igneous, sea level etc... being stated in lines 20-26. We agree with the reviewer that currently the first sentence reads light on the references, but our concern if inserted is that they will be duplicated in the following sentence. Lines 20-30, page 2. We believe the key literature is referenced here; but will review available literature to assess whether additional references are warranted.

3) line 11 on page 3: make clear that the benthic zone is only a small part of OAE 2.

R3. We agree with the reviewer that the benthic zone as defined by Keller and Pardo (2004) is only a small part of OAE-2; however we also note that the original benthonic zone definition of Eicher and Worstell (1970) is much broader and in northerly KWIS localities spans the entire OAE-2 and post-OAE-2 interval. We have amended the text as follows: "..tethyan water during OAE-2 was interpreted to have cumulated in the abrupt oxygenation of the seafloor as recorded by development and persistent abundance of benthic fauna (i.e. Elderbak and Leckie, 2016). However, this interval of benthic faunal abundance in the KWIS was originally termed the benthonic zone by Eicher and Worstell, (1970) who demonstrated that the benthonic foraminifera zone was best expressed in northerly sections where it spanned the entire Cenomanian-Turonian Bridge Creek Limestone, and is less developed in the central KWIS sections where it is stratigraphically restricted to the uppermost Cenomanian (i.e. beds 68-78 at Rock Canyon, Pueblo, Colorado; Eicher and Worstell, 1970), and where it spans only part of the OAE-2 interval and subsequently termed the Benthic Zone by Keller and Pardo (2004).

4) line 11 on page 4: where is the connection between figures 1 and 2 and the text?

R4. We agree with the reviewers comment. We have deleted "(Figures 1-2)" from the text and inserted reference to the figures more appropriately at the beginning of this section and in the methods. E.g. "The Eagle Ford Gr. was deposited during the Cenomanian to Turonian across the broad Comanche Platform in the southern intersection of the KWIS and northern Gulf of Mexico (Figure 1), "

5) chapter 3.2.2 needs a reference to figure 8

R5. We have inserted reference to Figure 8 as suggested.

6) chapter 3.3: at least present the most important features of the palyno-dataset that are used in the following discussion so that the reader has not to go back to the sup-

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plements every time.

R6. We utilized the supplementary information in order to reduce the size of the manuscript. We would like to defer to the editor whether the palynological results section should be re-instated to the main text.

7) line 25 on page 9: eigen scores are not shown in figures 7 and 11.

R7. This was also identified by reviewer #1; we have corrected.

8) lines 29-31 on page 10: would delete this statement from the text.

R8. We have deleted this comment

9) lines 14-15 on page 11: since there is no increase in pollen and spores, there is no support for an increased hydrological cycle

R9. In the sections from Texas and Demerara Rise there is not an increase in absolute abundance of pollen spores. However; in the Portland-1 core there is a slight increase (2,000- 5,000 c.p.g) so we disagree with the reviewers observation and a discussion is warranted. Regardless of absolute abundance, our data show a pollen assemblage shift to gymnosperm dominance; something that is highly relevant when comparing with other palynological records discussing increased hydrological cycle (e.g. Van Helmond et al. 2014) and subsequent citations.

10) line 19 on same page: what is the indication for climate cooling? Only the PCE is cooler, under background values that are much warmer than before or after OAE2!

R10. We agree with the reviewer that the primary evidence for climate cooling is linked with the PCE interval that elsewhere is associated with a sea surface temperature cooling (e.g. Forster et al. 2007; Van Helmond et al. 2014, 2016); climate cooling and drop in PCO2 (See Jarvis et al. 2011) as well as influx of boreal fauna. The increase in gymnosperms may therefore reflect expansion of conifer forests during this "cool snap". Additional evidence for climate cooling for the Turonian (i.e. what were

background levels in the Turonian) are not well constrained. The persistence of boreal fauna in the sections presented here may indicate the presence of additional cooling episodes throughout the Turonian. Further work is required on this topic.

11) Chapter 4.2.2: why should the tethyan water mass be suboxic-anoxic? This is inferred by the authors at the beginning of this chapter and then used in the following interpretation but it is never shown convincingly to the reader that this is the case. What is the independent evidence for this? Same holds true for the boreal water mass. Are there any other indications other than own data and interpretations? If yes, present them in detail. So far, the main problem with this chapter is that there is no prove that the suggested water masses existed and are characterized by the suggested data in the way they are presented here.

R11. This point was also raised by reviewer #1. In our initial response to reviewer #1 were thought the introduction section was adequate/to the point. However, as both reviewers have raised this issue we have expanded the introduction and detail the independent evidence

12) line 18 on page 12: why is this indicative for an WIS source? Explain and justify in detail. Same with the argument in lines 28-29 with the shift from agglutinated to calcareous forams. Why is this a water mass characteristic and not simply a matter of preservation or changing food availability? Technically I wonder how the foram data were produced. Are the based on the linings in the palynological samples as indicated by the figure headings? I am not aware of a single study that has shown this to work.

R12. This point was also raised by reviewer #1. (comment 20 and response). As per previous response "We agree with the comment of the reviewer and have clarified this sentence. The transition from agglutinated to calcareous foraminifera had been interpreted as reflecting the incursion of carbonate rich tethyan water into the KWIS, which in part is supported by occurrence planktonic foraminifera, ammonites, nannofossils of tethyan influence (see previous sentence). Detailed discussion of the foraminiferal

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assemblages we feel is beyond the scope of this contribution and have also inserted "for detailed discussion of tethyan-boreal foraminiferal distribution within Colorado, see Eicher and Diner, 1989)"

The benthic foram data was produced through two methods that we shall make clearer in the methods and figure captions; i) test linings from the palynological residues; ii) micropalaeontological analyses from sieved residues and thin sections. As previously mentioned, detailed inclusion of the micropalaeontological data we feel is beyond the scope of the paper and we primarily discuss the benthic abundances; inclusion would also lengthen the paper significantly as every sample analysed for palynology has an associated micropalaeontological dataset; as well as ~40 outcrops and we believe this requires a dedicated contribution. The occurrences of benthic foraminifera in the micropaleontological analyses and test linings in palynological assemblages are in general good agreement (as presented in Figure 4; quantified as c.p.g; and Figure 8 as relative abundance) and the method is demonstrated to work in this study. As below (comment 14) we shall also include published benthic foraminiferal data for sites 1260 and 1261, which are also in general good agreement.

13) line 31 on same page: at this point in the succession, there are no benthic forams, so the statement above in lines 28-29 cannot be valid!

R13. We disagree with the observation of the reviewer; there are foraminifera test linings (including for outside the initial 100 count specimens) in almost all samples throughout the Graneros to Hartland (with the exceptions of the 160.07m and 185.53m samples [column GQ in the Portland-1 datafile]). In addition, the statement above in lines 28-29 refers to the transition near the top of the MCE interval that is within the Graneros Shale and thus are also two completely different intervals of the core.

14) lines 32ff on page 13: there are benthic foraminiferal assemblage data available form these sites, how do they compare to the data produced by linings? This would be a good test to show if the presented foram data of this study are of any value.

R13. We agree with the reviewer and shall include benthic foraminifera abundances for Sites 1260 and 1261 in figures 9-10; and are in good agreement with the foram lining data.

15) lines 2-3 on page 14: I am not aware that there are any data e.g. Nd isotopes from these sites that support a boreal influence. Furthermore, the authors state that this water mass should only influence shallow water settings. However, a cold boreal watermass should be denser than warmer waters near the tropics and therefore influencing bottom waters and not surface waters as suggested here.

R15. As we state "The southern expression of this boreal influence is therefore limited in duration and extent". There is a Nd isotope excursion at this horizon and the nature of Nd signal is complex and requires additional localities from the KWIS., We state that our findings "indicating more complex interaction between water-masses and the oxygen minimum zone" and requires further work to resolve. It is an assumption by the reviewer that a boreal watermass would be denser as it would be colder; currently thermal gradients are not well constrained and salinity variations, particularly in the Western Interior Seaway are debated with suggestions of relatively freshwater (although we find no evidence of freshwater algae in our dataset). Our principal data are dinocysts that are mostly indicative of the photic zone so the watermasses are inferred to reflect surface water; however the vertical expression is apparent with sediment-water interface becoming oxygenated during OAE-2 associated with boreal taxa/watermass. Whether this is direct evidence for intermediate or deep water is unclear and we have included a statement in the text to raise this point – see also reviewer comment 22

16) line 35-36 on page 14: nowadays, nobody thinks anymore that the Cretaceous had a equitable climate!

R16. We agree and that is why it is stated ..." than previously thought". In addition, this is an important point to make as not many studies have investigated this topic; the contribution of Gambacorta et al., (2016) is notable and our data is supportive of the

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## proposition.

17) lines 23-24 on page 15: but the red dots are all over the place in figure 15 and quite a few from Demerara Rise even above 3. What is the r2 for these data? Further in this chapter, Mo/TOC is used to say something about a silled basin situation at this site. Why not simply cite the papers that show that there was no sill during that time (e.g. seismic evidence)?

R17. In unrestricted settings such as the Namibian Shelf and the OAE-2 interval presented here, there is a positive but generally weak co-variance between Mo and TOC and so would expect poor r2 values. In addition, preferential enrichment of Mo in euxinic conditions and in particular recycling in the particulate shuttle usually results in a TOC threshold for enhanced Mo enrichment; so the basic relationship is non-linear. We have clarified this in the text and inserted "weak positive co-variance between Mo and U.". Public domain evidence for absence of a sill across the US Gulf Coast is limited and along with the Demerara Rise ambiguous as reflect present-day geometries.

18) last paragraph page 15: wouldn't be the absolute amount of refractory terrestrial organic matter (RTOM) an even more important factor than the T:M ratio alone? The ration could be high even when there is less RTOM and therefore a lower influence on Mo! Since this is not quantified, this is a weak justification and discussion.

R18. We mostly agree with the reviewers comment. The absolute amount of refractory terrestrial organic matter (RTOM) would be more significant than the T:M ratio alone. However; properly quantifying RTOM as far as we are aware is not currently possible. The major component of the organic matter is amorphous organic matter (AOM) and as stated in the text its origin is relatively poorly constrained; some part can be degraded terrestrial and/or marine in origin. Given that we cannot yet adequately identify and quantify the origin and composition of AOM and thus RTOM; the T:M ratio is the best approximation of terrestrially derived palynomorphs (and thus a component of RTOM). The text is slightly amended to include "refractory" and feel this uncertainty

is now adequately captured, "We cannot determine the impact of this observation as the T:M ratio reflects a relatively small proportion of the total refractory organic matter, however further investigation is warranted into the variable origin of the more dominant and relatively unknown component, namely AOM; and whether Mo is preferentially incorporated within different organic matter components".

19) first paragraph on page 16: How do these factors deplete Mo? Please explain the details.

R19. The processes and controls have been included as suggested by the reviewer.

20) lines 8-9 on same page: but isn't that what you are proposing above?

R20. In part we agree, but the actual relationship between refractory organic matter and Mo is not documented by Dickson et al. (2016); as such this is the first integrated data of terrigeneous organic matter and Mo/TOC relations.

21) lines 21-23: check benthic assemblage data for these sites if available and see if there are benthic forams occurring in these intervals. If yes, these are oxygenation events, if not, it was anoxic. This would be an independent proof of the statements made here.

R21. Benthic foraminifera are present (Friedrich et al. 2006, 2011) and reflect repopulation events associated with improved oxygenation. This data will be included in Figures 9-10 (see comment 14) and the following text has been inserted ". This interpretation is also supported by the occurrence of benthic foraminifera during the OAE-2 interval in both organic rich and organic lean sediments from Demerara Rise (Friedrich et al. 2006, 2011); Texas (Lowery et al. 2014; Dodsworth, 2016; this study) and central KWIS (e.g. Eicher and Worstell, 1970; Keller and Pardo, 2004; Elderbeck and Leckie, 2016 and references therein).

22) point 1 in the conclusions: are these water masses be interpreted to be surface and bottom-water masses at once? This has to be clarified in the discussion.

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R22. See comment R15. This has been clarified in the discussion.

23) Figure 2: in part "a" it is hard to figure out the core locations.

R23. Core locations shall be made bigger and unique symbology

24) Figure 3: what are the horizontal red lines?

R24. Horizontal red wavy lines are hiatal surfaces. The captions shall be expanded to better explain the symbology in the figures

kind regards.

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