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Climate of the Past Discussions

Interactive comment on "Dynamic climate-driven controls on the deposition of the Kimmeridge Clay Formation in the Cleveland Basin, Yorkshire, UK" *by* Elizabeth Atar et al.

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This manuscript uses a wide variety of geochemical proxies, which they integrate with sedimentological information, to investigate the depositional environment of the Kimmeridge Clay Formation in the Cleveland Basin. In general, the conclusions they reach are valid and after some moderate revisions I recommend publication of this manuscript.

Important revisions:

1. Section 3 (Materials and methods) should include information about precision and

accuracy, as well as better descriptions of the analytical methods; notably for the analysis of carbon isotopes. Details are provide below.

2. The manuscript is a bit disorganized in places (e.g., interpretations more suited to the discussion are found within the result section, figures are out of order). Details are provided below.

3. The discussion of the d13Corg data is limited and lacking in detail. The authors simply say lighter values indicate more terrestrial organic matter. While this is correct they do not providing references / background information to support this interpretation. In general, Section 5.2 is lacking in appropriate references.

Specific Comments:

Page 1, Line 27 – You talk about "three states that produced a distinct cyclicity" however the paper is primarily divided into two units LVMIs and HVMIs. This is a bit confusing.

Page 3, Lines 27 and 28 – Why are "ocean overturn, salinity/temperature stratification and redox conditions" mentioned in this sentence about organic carbon enrichment? Something does not make sense here.

Page 4, line 13 – The sediments might be thermally immature but 425°C is high and would have undoubtedly affected the sediments. Diagenetic alteration can occur at temperatures below 100°C. Just something to keep in mind especially when looking at the Hg data.

Section 3, General comment – Where is the information about precision and accuracy of the geochemical analyses?

Page 4, line 20 – It is highly unlikely that sedimentation was linear throughout this time period. The changes in climate and resulting changes in sedimentation were simply to drastic. It would be better to use the biostratigraphy to estimate how much time this covers.

Page 4, line 28 – XRF measures major and minor (not trace) elements. For consistency, you should also list which elements were analyzed

Page 4, lines 28 to 29 – More information about how d13Corg was measured is needed. What equipment was used? Were the samples pretreated to remove carbonate before d13C was analyzed? What standards were analyzed?

Page 5, lines 9 to 15 – This discussion about Hg does not belong here. Move to page 8 (First paragraph of the discussion).

Page 5, line 17 – You need to provide more detail on how the boundaries of the HVMIs were determined. I'm requesting this because the lowermost zone is thicker than I think it should be based on carbonate and d13Corg values.

Page 6, lines 2 and 3 – Did you measure grain sized? If not, how are you determining the difference between "medium to coarse mud-size" and "fine to coarse mud-sized"? Also, there is technically no such thing as a "mud-sized grain". Grain size is subdivided into clay, silt and sand . . . mud is a mix of clay and silt, with perhaps a bit of fine sand.

Page 6, lines 11 and 12 – According to what you wrote at the end of page 5 LVMIs are composed of two facies (1 and 6) so facies 4 should be discussed in Section 4.1.2

Page 6, line 16 – What three samples are you referring to in Figure 2? I assume it is those samples identified by arrows but you should make this clear by saying (Fig 2, arrows in CaCO3 plot).

Page 6, lines 20 to 30 – You need to emphasize the differences between Facies 2 and 3. It would help if you referred to Figure 3 more often throughout Section 4.1.

Pages 7 and 8 – Section 4.2 should simply describe the results. For example, LVMIs have lower d13Corg values than the HVMIs (Fig. 2). The interpretation of the d13C data should not occur until the Discussion. Furthermore, a lot of the results are not mentioned (e.g., CaCO3 content in the LVMIs). This section needs to be cleaned up.

Page 7, lines 18 to 20 – This discussion about the d13Corg data needs to move to Section 5. Once you do this, please provide references for this statement "Marine organic matter has a higher d13Corg than terrestrial OM".

Page 8, line 24 – You need to better explain how Hg/TOC data rules out a volcanogenic sediment source. Also, these sediments got rather warm during burial (425°C) . . . would this affect the Hg content?

General comment about Discussion – Section 5.1 merges a lot of information, much of which you haven't discussed yet (e.g., the d13Corg data which is, or should be, discussed in section 5.2). I suggest you swap the order of sections 5.1 and 5.2 (i.e., discuss productivity and organic carbon first).

Page 8, line 30 – You cannot identify what clay minerals are present (i.e., kaolinite vs illite) using just petrography. This requires specialized X-ray diffraction techniques.

Page 10, Section 5.2 – This section might be easier to understand if you divided into subsections as you did for Section 5.3 (i.e., a subsection for LVMIs and HMVIs).

Page 10, line 7 – A brief discussion about what type II and III organic matter is would be helpful. Also, this is the place to discuss how d13Corg can be used to identify terrestrial vs marine organic matter. Can you estimate the end member marine and terrestrial values? If yes, you could estimate the fraction of organic matter that is terrestrial vs marine. I assume you do not have %N data but if you did you could also use Corg/N ratios . . . although not all terrestrial organic matter has high Corg/N ratios.

Page 10, line 11 – So, if the LVMIs are deposited in a more "distal" location relative to the HMVIs . . .. why is terrestrial organic matter higher in the LVMIs?

Page 10, line 23 – It is extremely unlikely that the euphotic zone reached the seafloor if waters depths were greater than 200 meters, which is the approximate limit of the euphotic zone in the modern ocean. That said, light limitation would only be an issue in at higher latitudes (i.e., mid-latitudes to polar regions) where in winter it limits productivity

along with strong storm mixing. Nutrient supply is undoubtedly controlling productivity.

Page 11, lines 1 and 2 – I agree that nutrient availability may have influenced whether the HVMI sediments were TOC-rich or carbonate-rich. In the modern ocean a well-stratified water column, characterized by a stable, warm, nutrient-poor surface layer does favor coccolithophorids over diatoms.

Page 11, line 4 – The discussion about fecal pellets and zooplankton is not critical to the discussion and perhaps distracting. I suggest removing it. If you prefer to keep it then write it in the same way as it is written on page 17and include some references that support the idea that packaging of organic matter into fecal pellets speeds the delivery to the seafloor and aids in preservation. Also, zooplankton were probably around during the formation of both LVMIs and HVMIs . . . but bioturbation of the LVMIs destroyed the fecal pellets in these units. Interestingly, in modern sediments fecal pellets tend to be preserved where you have sediment starvation regardless of whether there is bioturbation or not.

Page 11, lines 12 and 13 – In section 4.2.1 (page 7, line 25) you mention that trace metal enrichments in LVMIs are low (i.e., around 1) which agrees with metal/Al ratios shown in Figure 6. However, here you say there are trace metal enrichments and imply they are large. What I believe you mean to say is that when you look at Mo and U EFs (Fig 8 . . . which should be Fig 7) there is some enrichment indicating the development of reducing conditions after burial. Please clarify.

Figures 7 and 8 – you need to switch the order of Figures 7 and 8 so they match the order they are discussed in the text. Make sure to change the figure numbers in the text as well. Also, the figure caption for Fig 8 (what will be Fig 7) has some errors (e.g., the panels that depict the HVMIs are blue not grey).

Page 11, lines 16 to 24 – this discussion about TOC should be moved to Section 5.2 (i.e., the Productivity and organic matter composition).

Page 11, line 25 - I think you mean HVMIs . . . not LVMIs in the title of section 5.3.2

Page 11, line 28 – technically anoxic refers to porewaters with sulfide and euxinic to a water column (i.e., bottom waters) with sulfide but I know there is some “debate” about these terms.

Page 12, lines 4 and 5 – You still have not discussed Figure 7 and you are now mentioning Figure 9. I would remove this sentence and wait until Section 5.3.4 to discuss it.

Page 12, lines 6 to 9 – I agree, what you are seeing in the KCF looks very similar to the Baltic Sea.

Page 12, line 27 – Do you mean that the high As/Al and Sb/Al ratios within the HVMIs support the idea of higher pyritisation? A better way of saying this is that pyritization probably explains the enrichment of metals known to accumulate with pyrite (e.g., As, Sb and Mo).

Page 13, line 12 to 13 – You need to emphasize that those steep geochemical gradients are driven by changes in oxygen vs sulfide (i.e., redox).

Page 13, line 16 – replace the word “phases” with “particulates” at both the start and end of this line.

Page 13, line 24 – You need to explain the Mn better (e.g., what about the Mn carbonates). I suggest removing this sentence and adding a paragraph about Mn at the end of this section.

Page 14, lines 4 and 5 – remove “with associated enrichments of redox sensitive/sulfide forming trace metals (Fig. 6)” . . . it is not needed here.

Page 14, lines 6 to 8 – Also remove this sentence. It does not read well and you basically say the same thing in the next paragraph. So, now you can merge the second and third paragraphs on this page.

Page 14, line 19 – Can you provide some Mo/U enrichment ratio data for other rock and sediments (not just the Tribovillard et al 2012 data which is also the KCF) to support your conclusion that the Mn-Fe shuttle must have been working during HVMI deposition. A figure similar to your Figure 9 would work. If there are not enough data to do this, you need to say "probably facilitated".

Page 14, lines 26 and 27 – I'm not sure what you mean by "which is in good agreement with our Fe/Al and Mn/Al data discussed above". I'd remove this.

Page 14, line 28 – didn't you say sediment redox conditions in the LVMIs become anoxic (Page 11, line 14)? It cannot be both.

Page 15, line 5 – no need to reference Brumsack (2006) here.

Page 15, line 6 – average shale or UCC?

Page 15, line 15 – I thought that Mn (in the form of carbonate) was enriched in the HVMIs (Page 12, line 1). Perhaps depletion is not the correct word or you need to clarify "depleted in comparison to . . .".

Page 15, line 25 – by "the two intervals of deposition" are you referring to the Black Sea and HVMIs? This is unclear.

Page 16, line 4 – What do you mean by "If at all. . ."? Are you trying to suggest the Baltic is the best modern analogue we have? Right, I see where you say this on line 16. Perhaps you need to move this last paragraph (lines 16 to 21) up to line 4.

Page 16, lines 23 to 16 – You repeat yourself a couple of times. This paragraph needs to be rewritten or just remove it.

Page 16, line 18 – rewrite as follows ". . .fluctuated between LVMI and HVMI deposition."

Page 16, line 10 – I thought you said high TOC concentrations related to the fact it was terrestrial. Where did you say that reducing conditions lead to TOC enrichment in the LVMIs?

Page 17, general comments – do fluctuations in sea level not play a role in the changes we are seeing?

Page 17, line 4 – write as follows "an increase in OM flux rate to the seafloor"

Page 17, line 7 – you mention elevated depositional energies during the during oxygenation events associated with HVMIs but in Figure 3 the erosional features are associated with Facies 2, which I though was the "anoxic" facies. Please clarify.

Page 17, line 17 – you imply that both the LVMI and HVMI units ("the studied section") are similar to what you see in the Baltic Sea. Is this what you mean to say?

Page 17, lines 19 to 24 – you discuss the timescale and mechanism that is mostly likely controlling the change between LVMIs and HVMIs. What about within the HVMIs? These changes must be much more rapid. Any educated guesses about what might be controlling this?

Page 18, line 5 – What data repository will you use?

Spelling/Grammar:

Page 3, Line 24 – the word basins should not be capitalized Page 4, Line 1 – add the word "well" before "exposed" Page 4, line 9 – need a space between core list and (Fig. 1c) Page 7, line 25 (also on page 8, line 15) – add the word "relative" before "to UCC" Page 7, lines 29 and 30 – The value -27.0 should come before -28.8 ‰ Page 12, lines 1 to 9 – some grammar issues in this paragraph Page 12, line 32 – replace "of" with "for" (i.e., the need for caution when using Mo. . .) Page 13, line 22 – proxies not proxy Page 16, line 14 – replace "is" with "are" and spell out Figure (e.g., in the Baltic Sea are plotted on Figure 9.). Page 16, line 23 – remove the "an" before "insight"

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