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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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Summary

This manuscript presents an integrated sedimentological and geochemical dataset through one section of the Kimmeridge Clay Formation (KCF) in the Cleveland Basin (Yorkshire, UK). This manuscript assesses viability of the 'Hadley cell' hypothesis, defined by intensification of wet-dry climatic cycles operating across the Boreal Seaway, as a key control on the distribution of sedimentary facies through the KCF. Six sedimentary facies (1-6) are defined. Two facies associations/packages are defined; Lower Variability Mudstone Invervals (LVMIs) and Higher Variability Mudstone Intervals

(HVMIs). Facies 1 is defined as a 'clastic' mudstone and is therefore considered the siliciclastic end-member. Facies 6 is defined as a carbonate-cemented mudstone (or possibly limestone). LVMIs almost entirely comprise Facies 1 mudstones, whereas Facies 6 is very rare. The authors suggest Facies 6 is primarily diagenetic in origin. Facies 2-5 mudstones are variously organic and pellet-rich (2), coccolith-rich (3), foraminifera-rich (4) and bioclastic (5), and typify HVMIs. LVMIs lack enrichment in redox-sensitive trace metals and are bioturbated. Whereas HVMIs typically lack bioturbation (Facies 5 is an exception) and are enriched in redox-sensitive metals. The authors utilise the distribution of redox-senstivie trace metals as palaeoredox proxies. The authors discuss and compare changing redox conditions between deposition of LVMIs and HVMIs. Related processes of sulphide production, reactive Fe input, organic matter sulphurization and chemocline stability (Fe-Mn shuttling) are considered. The authors compare anoxia in the Cleveland Basin with several modern settings. The authors conclude elevated TOC in HVMIs is best explained by coupling of bottom water anoxia to increased rates of primary productivity and increased water column stratification (freshwater cap). The model for anoxia in the Cleveland Basin is therefore a hybrid of productivity-driven and restriction-driven settings.

General Comments

The dataset underlying this manuscript is high-resolution (n=116, spanning 40 m core) and diverse (47 thin sections, TOC measurements, major and trace element geochemistry via XRF and ICM-MS and δ13Corg). The underlying scientific principles and assumptions are also robust. Aspects of the manuscript itself are robust and do not require significant modification. The comparison with modern settings is useful too. Therefore there are many merits of this research. If seen through to publication, I believe this is a valuable contribution that will be of interest to a wide readership. Despite this, there are several aspects of this manuscript which require significant revision prior to publication. On this basis I recommend acceptance of this manuscript with major revisions.

My main concerns are provided in the section below. I have provided as much detail as possible, in order to help the authors improve the manuscript. I will respond to requests for clarification/discussion on any of these points if needed.

Main Concerns

- This manuscript poorly integrates sedimentological (petrographic) observations with the geochemistry. The authors define 6 sedimentary facies, yet it is very difficult to relate these facies to the geochemistry. This is primarily because the sedimentary logs are defined using 'mudstone', 'marl' and 'limestone' yet this nomenclature is not used in the main text. Therefore this is a problem of consistency between the main text and the log (Fig. 2). The sedimentary log should be defined to the facies scale. Doing so will better link the sedimentological observations with the geochemistry. Doing this may also help delineate more subtle relationships between the geochemistry and particularly Facies 2-5 within HVMIs, which might help support/develop the conclusions of this research. In addition, a bioturbation index (Lazar et al. 2015, as is referenced in this manuscript) should be plotted alongside the logs. In my opinion, for a manuscript that is focussed on bottom water redox conditions, this is very important. I also strongly recommend plotting TOC, carbonate content and δ13Corg alongside some of the other redox-sensitive elements.

- Sedimentology – 1) In my opinion the facies descriptions need much more support. There are too many assertions and not enough descriptions supported by data. I think this manuscript would greatly benefit from more example microphotographs. Perhaps all that is required is some rewording to improve clarity and more detail (including logic for facies definition and ordering) plus one additional figure, which provides evidence for the following; bioturbation, phosphate clasts, faecal pellets, algal mats, pyrite microtextures, depositional processes (normal grading, erosive bases) and authigenic clays. Perhaps this could include scanning electron microphotographs too. (SEM is mentioned in the methods.) 2) Quantification – it would be advantageous if the authors quantify the abundance of all sedimentary components (e.g., carbonate, bioclasts, coc-

coliths, etc.) where possible – even if this is simply done at percentile resolution. (i.e., trace, 25%, 50%, 75% and <> where needed). In some cases it would be useful to quantify grain/cryst diameters (e.g., framboids), even if such observations are approximations;

- Organic matter typing – In my opinion much more emphasis should be given to the δ13Corg data as a proxy for bulk OM type. I am not convinced petrographic observations are sufficiently robust/reliable in order to define bulk OM type (i.e., Type II vs III). In my opinion there are too many assertions rather than observations supported by data (in exactly the same way as the general sedimentological descriptions). Assessment of bulk OM using petrography would need to be supported by multiple annotated microphotographs for each facies. Thus in my opinion, the δ13Corg record should be emphasised and utilised as the primary proxy for OM type. Petrographic observations (at the level of detail presented) are secondary;

- Algal mats – this is very interesting but needs to be supported by evidence – ideally microphotographs and then integrated onto Fig. 10;

- Evidence for sediment starvation & winnowing (e.g., p9, line 10) – this needs to be supported by evidence (i.e., microphotographs). Perhaps it is also appropriate to estimate a mean sediment accumulation rate for this section too. i.e., the fine-grained nature of this section does not necessarily indicate this was a distal setting subject to low sediment accumulation rates;

- Enrichment factors – PAAS or UCC? It is unclear whether the authors calculated enrichment factors using PAAS or UCC, which are not the same. The manuscript body quotes UCC but some of the figure captions quote PAAS. In my opinion, it is preferable to calculate enrichment factors using PAAS, as this allows for comparison with many other black shale studies. PAAS vs. UCC is likely to be particularly important at low EFs (close to 1) when plotted onto the log-log Mo and U EF cross-plot of Tribovillard et al. (2012) Chemical Geology (Fig. 8b in this manuscript). I recommend re calculation of EFs using PAAS if necessary.

- This manuscript lacks detailed comment on sea level. Was sea level stable or fluctuating? If fluctuating, was this via eustacy or a local mechanism? Sea level (but not fluctuation through the section) is mentioned briefly in the geological setting and also briefly mentioned in the discussion - p14 line 23-24 "…concluded that TOC enrichment in the Cleveland Basin occurred during times of transgression.." Yet the authors do not provide the context, or critique, of sea level fluctuation as a potential control on the distribution of sedimentary facies and organic matter through the KCF. It is extremely important the authors give more consideration to the role of sea level fluctuation. This should feature in the geological setting and discussion, and perhaps also in the introduction. Could HVMIs represent transgressive and highstand packages? Therefore couldn't the 'cyclicity' between HVMIs and LVMIs simply relate to sea level fluctuation? i.e., an 'expanding puddle' model of anoxia (e.g., Wignall, 1994, Black Shales). If sea level fluctuation is not considered the key control on the distribution of facies, the authors should explain why this is the case.

- The model for anoxia. (1) In my opinion, I think there is a more subtle signal through each LVMI and into the overlying HVMI. This is particularly clear, in my opinion, when assessing the δ13Corg record (Fig. 2). Can the authors consider my suggestion? My argument is this. Increasingly wet conditions through deposition of each LVMI drives increasing run-off, input of TOM and progradation of a freshwater cap. Ultimately basin stratification approaches a tipping point, which triggers bottom water anoxia, during the extreme wet part of the cycle. Bottom water anoxia drives the 'eutrophication pump' (e.g., Sageman et al., 2003, Chemical Geology), generating a positive feedback in terms of productivity and further expansion of anoxic conditions in bottom waters. The euphotic zone is no longer P-limited. Perhaps somehow these conditions encouraged carbonate productivity oscillation. Then finally, progressive reduction in precipitation as the onset of the 'dry' part of the cycle reduces freshwater input, therefore gradually weakening the pycnocline. Ultimately this process encourages ventilation of bottom

waters. Bottom water ventilation switches off the 'eutrophication pump', reducing productivity, further reducing the OM load to seabed and further promoting ventilation. Then the cycle starts again. At the least, I think the role of the 'eutrophication pump' deserves comment. (2) Bottom water conditions were apparently 'intermittently euxinic' during deposition of organic-rich parts of HVMIs rather than 'permanently euxinic'. The data presented indicates an unstable chemocline (particulate shuttle) and in my opinion does not indicate a strongly stratified and permanently euxinic system. Therefore Fig. 10 and relevant discussion should be revised to reflect this.

- The prose should be improved throughout this manuscript. For example, many sentences are too long and/or poorly structured. I have highlighted some specific cases in the annotated PDF. In many cases this can be resolved quickly, by splitting one long sentence into 2 or 3 shorter sentences. This will help communicate the science. In places the language is too informal and mixes tenses.

Please also see the attached PDF for detailed annotations – the figures, in particular, could be improved substantially.

Joe Emmings, British Geological Survey, 25/03/19

Please also note the supplement to this comment:
https://www.clim-past-discuss.net/cp-2018-172/cp-2018-172-SC1-supplement.pdf

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