We thank Joe Emmings for taking the time to provide detailed comments and alternative interpretations of the dataset. In order to keep this response clear and concise, we have grouped comments from SC1 and the supplementary material into 7 categories. Where comments were repeated or of a similar nature, we address the first comment (but also refer to the latter ones). Comments from SC1 are in blue, comments from the SC1 supplementary information are in purple, our responses are in black text.

1.1 Integration of geochemistry and sedimentology

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.

The sedimentary log is redrawn from the initial work on the core (ca. 1987; documented in IFPEN internal files), which was carried out using hand specimens. The facies model presented in this study was constructed using both hand specimens and thin sections of selected intervals only, so it is impossible to construct a sedimentary log from the latter technique that is directly comparable to the original 1987 log. We wanted to incorporate all the work that was done on this core in our manuscript, and in doing so consider it appropriate to use both the original and our new datasets, even if they are not directly comparable/correlative. To aid interpretation, we will add an appendix that details the assignment of individual thin sections to their respective facies.

Petrographic observations (at the level of detail presented) are secondary

This manuscript is mainly a geochemical study that is supported by petrographic observations, so we regard the balance between both types of observations/data as appropriate.

1.2 Sedimentology

- 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.
- Please add bioturbation record to the logs (Figs. 2, 5, 6) - this is very important (page 6, line 9)
- Please add bioturbation indices (see Lazar et al. 2015) on the logs (page 7, line 4)

We agree that this will aid interpretation and have added it to Fig. 2.

I also strongly recommend plotting TOC, carbonate content and d13Corg alongside some of the other redox-sensitive elements.

While we agree that this could aid interpretation, we want to avoid repetition of data plots in the manuscript to not make already busy figures even busier; we therefore added grey horizontal panels to all depth plots to facilitate comparisons/correlation between figures.

- 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.)
- Algal mats – this is very interesting but needs to be supported by evidence – ideally microphotographs and then integrated onto Fig. 10
- Personally I think this section would benefit from at least one further figure of example microphotographs supporting the features described here. The examples in Fig. 3 are good but in my opinion more support is required. (page 5, line 21)

We agree and have added another figure to the manuscript comprising optical micrographs and SEM images to illustrate the features outlined above.

Evidence for sediment starvation & winnowing (e.g. page 9, line 10) – this needs to be supported by evidence (i.e., microphotographs).

We agree that this issue was not ideally phrased, which was also highlighted by Reviewer 1's comments on the same matter, and we have updated our interpretation in the revised manuscript.

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

We agree. Following Reviewer 1's comments/discussion of calculated sedimentation rates, we have incorporated them into the revised manuscript.

- Quantification – it would be advantageous if the authors quantify the abundance of all sedimentary components (e.g., carbonate, bioclasts, coccoliths, 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.
- Can the authors quantify proportions? What does 'dominated' mean in terms of %? Doing this will add much more value to this manuscript, and will help readers compare this work with their own

Adding approximations of sedimentary components counted in thin sections increases the errors on the dataset. Instead, we have used geochemistry, supported by petrographic observations, to quantify sedimentary components. For example, $CaCO_3$ (measured by LECO) gives a quantitative measurement of carbonate within each sample. However, we have added details on the sizes of individual sedimentary components (e.g. framboids) where we had not done so in the original manuscript.

Organic matter typing – In my opinion much more emphasis should be given to the d13Corg 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). Assssment of bulk OM using petrography would need to be supported by multiple annotated microphotographs for each facies. Thus in my opinion, the d13Corg record should be emphasised and utilised as the primary proxy for OM type.
We agree that geochemical data is required to support petrographic observations determining OM type. We have updated the manuscript to more strongly reflect the relationship between OM and $\delta^{13}C_{org}$ and incorporated published RockEval pyrolysis data to further support our interpretations.

Can the authors comment on the nature of the contacts between LVMIs and HVMIs? For example are the contacts sharp or gradational?
Given the sampling strategy employed (i.e. sampling representative intervals of macroscopically distinguishable lithologies) and the often subtle nature of facies changes, it is not possible to comment on the nature of the boundaries between facies.

Also what is the difference between a mudstone that is 'organic material... rich' (Facies 2) and 'carbonaceous' (Facies 4)
The difference lies in other aspects of the facies, in that Facies 2 contains calcareous pellets and Facies 4 contains agglutinated foraminifers.

(with reference to authigenic kaolinite) Interesting. Is this booky? (page 6, line 7)
Yes, we now include photomicrograph illustrating this.

1.3 Geochemistry

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.
We are aware of the differences between PAAS and UCC. When looking at downcore trends, we normalise our element contents to UCC because it most closely matches the lithogenic background sedimentation at our coring site. PAAS is only used in Fig 8b (Mo-U enrichment factor cross plot), as this facilitates direct comparison to the original data generated by Tribovillard (1994) using PAAS for normalisation. For the comparison to other organic-rich deposits (Fig. 9), we normalise to Average Shale (AS; Wedepohl, 1991) following Brumsack (2006) who published the most comprehensive data base on such deposits. Thus, normalising to UCC, PAAS, and AS each has its own merits depending on the purpose of normalisation. We have updated the manuscript to make this clearer to the reader.

- 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 13Corg 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. (this comment is slightly rephrased on page 10, line 25)
The $\delta^{13}C_{org}$ record through the LVMIs reflects the global carbon isotope curve mapped at multiple sites across the world (Gröcke et al., 2003). We have updated the manuscript to include this. The eutrophication pump was most likely active during formation of the organic-rich intervals (it mostly is during times of bottom water anoxia in shallow water masses), but we do not have the data to judge whether it was the dominant factor controlling organic matter accumulation and/or redox conditions. This would require additional information about salinity stratification, phosphorus speciation, nitrogen concentrations/isotopes.

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.
We have implemented this recommendation in the revised manuscript.

Neither excess Fe or early diagenetic pyrite oxidation preclude sulphurization. We observe exactly the reverse in the Bowland Shale. Paradoxically, sulphurization is triggered by Fe loading in some environments. We have a ms accepted for publication on this topic, available on request, if the authors would like to see this. (I do not think the Bowland Shale model for anoxia is analogous to the KCF, but nonetheless, present research shows sulphurization does not require Fe-limitation and perhaps this can be acknowledged.)
We have updated the manuscript to include recent references on early sulphurisation of OM despite the presence of reactive Fe. While the relationship between reactive Fe availability and the sulphurisation of OM can be more complex than previously assumed, detailed investigation of this matter is beyond the scope of this manuscript.

The authors could assess the viability of normalising to Al, by calculating the coefficient of variation - see Tribovillard et al. (2006)
Normalising to Al is a valid and common approach, and only problematic at very low Al contents. We do not use element/Al ratios in a quantitative sense, as we are looking at patterns and how they change, and so our approach is sufficient for this purpose.

The authors imply Mn enrichment indicates oxygenation - but this is not necessarily the case - Mn can be fixed in sulphide under sulphidic conditions. See Lyons and Severmann 2006 Geochimica et. Cosm. Acta., for example (page 12, line 3).
MnS is extremely rare and in the modern environment is only seen in some very particular deep basins of the Baltic Sea – but even there, the vast majority of Mn is bound as carbonates that

formed following oxygenation of a Mn^{2+} charged water column, and later burial and reduction of the Mn oxides.

Can the authors comment on why KCF Mn is so low compared to the other modern examples? (page 27)
Either there wasn't a lot of Mn^{2+} accumulated in the water column (since the basin was not completely restricted and Mn oxides might have precipitated in oxic shallower environments), or some of the precipitated Mn oxides were reduced fairly close to the sediment-water interface and could diffuse back into the water column, or the oxidation events occurred between fairly short periods of anoxia/euxinia and only little Mn^{2+} was available in the water column for oxidation. Our data do not support any one of these arguments over the others.

1.4 Sea level

- 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 the authors comment on sea level during this period? Both in terms of eustatic sea level and local sea level variation. I am assuming no ice caps..?
- Could the authors provide comment on the record of sea level through this interval? Could this alternation reflect sea level fluctation? (I am assuming not, but for completeness..) (page 11, line 2)

Sequence stratigraphic principles have been applied to the coastal outcrops of Kimmeridge Clay Formation at Boulonnais (France) and applied to coeval sediments in the Wessex and Cleveland Basins (Herbin *et al*., 1995). Organic enrichment does not coincide with fluctuations in sea level so sea level cannot be the primary driver. Furthermore, Powell (2010) states that regional sea level reconstructions in the Cleveland Basin cannot be correlated with the global sea level curve. There is also no evidence for ice at the poles during this time (Dera *et al.*, 2011). We have updated the manuscript to reflect this.

1.5 Prose and grammar

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. E.g. Page 1, lines 24 and 27.
We kept this in mind while revising the manuscript.

In my opinion these sentences would be better placed after the authors introduce the 'Hadley Cell' hypothesis. The logic of the abstract is then 1) the KCF is highly organic-rich 2) an

expanded Hadley Cell is thought to explain the distribution of organic-rich intervals 3) In order to test this hypothesis, we present a petrographic and geochemical dataset...etc.
We have restructured the abstract to accommodate this.

Should this read 'Stable Isotope Facility?' Or even NEIF? (Please check correct nomenclature with Mel Leng.)
We have changed to the BGS stable isotope facility (part of the National Environmental Isotope Facility.

kyrs not kyr
We prefer kyr (without the 's') as this is used widely in the literature. However, will update should the editorial team request it.

Is it acceptable to include references in the abstract? Perhaps just say "It has been recently postulated..."
We have rewritten the abstract to omit references.

I suggest replace 'energies' terminology throughout this manuscript. Perhaps it is better to say "...most likely due to the action of vigorous bottom currents at seafloor..."
We have included a definition of 'depositional energies' to clarify what we refer to.

I prefer use of 'sediment deposition' rather than sedimentation. Sedimentation means sediment formation rather than deposition, which I think is the authors' meaning here.
We opted to use the Oxford Dictionary definition of *sedimentation*: The process of settling or being deposited as a sediment.

should replace with 'reduced ventilation in bottom waters' or something similar -This is because "increased... redox conditions.. " doesn't make sense
We have updated this in the manuscript.

siliciclastic? or calciclastic? definitions required
The manuscript has been updated to use siliciclastic.

1.6 Figures

Palaeolatitude here? (page 19)
We have added palaeolatitude to the figure.

Is 1c sourced from BGS mapping? If so, the authors should check whether to reference DigMapGB-625 or something similar. Mapping data is copyrighted, but at this scale is likely covered by the Open Government License. Please investigate the source of this map and check the BGS website for referencing protocols if appropriate. (page 19)
This is a figure redrawn from the review paper by Powell (2010).

This ref to 1a is not in sequence. Either re-arrange text so that 1a comes first, or re-arrange fig 1 a-c itself
We have done this in the revised manuscript

But this ms does not include any electron microphotographs in the figs? If this is correct, I suggest the authors remove any mention of SEM, or add example electron microphotographs.
We have added a new figure containing SEM images to the revised manuscript.

If this is scaled, does this corner actually represent 100%? Or perhaps 50%? Might be better to plot CaO (without x2) and then scale the axes appropriately (page 22)
We have used a conventional and widely accepted method of plotting this data (e.g. Brumsack, 1989).

Why not add a legend to this plot? (page 22)
We have included a legend on this plot in the revised manuscript.

Does this delineate a water mass? i.e., perhaps a freshwater cap?) If so, why is the chemocline positioned beneath the pycnocline on b and c?
The green layer represents surface productivity.

Do the authors suggest bottom waters were fully oxic? Or perhaps sub-oxic or ferruginous? If so, perhaps this fig. needs modifying. And do the authors suggest sulphide was completely absent in bottom waters? Is this consistent with the trace element record? (page 28)
We propose that HVMIs are characterised by variations between fully oxic and euxinic conditions.

I think the authors need to put double-ended arrows here and perhaps some comment, to show the chemocline was unstable - i.e., 'particulate shuttle' conditions requires chemocline fluctuation down to seabed. i.e., Fig 5b in Algeo and Tribovillard, 2009, Chemical Geology
This is not correct. The particulate shuttle in the Black Sea, for example, is active despite the chemocline being sometimes thousands of meters above the seabed. The main purpose of the shuttle is to get metals adsorbed to Fe/Mn oxides in the oxic part of the water column, then shuttle them down into the anoxic/euxinic part where they are released and taken up into sulphides or organic matter – and importantly, large parts of the Mn gets re-oxidised at the chemocline so it can continue "pumping" trace metals down into the anoxic/euxinic waters.

Grey and black circles are OK but not the easiest to distinguish - I suggest switching to circles and squares, or circles and crosses, or open circles and filled circles
We have changed the grey circles to grey squares.

1.7 Samples, sample information, and sample locations

What is the name of the type section? Where is it located? (the authors could supply coordinates, and/or refer to the BGS memoir for the sheet)
We have included this in the revised manuscript.

Please include mE and mN coordinates for this borehole and projection system (ideally British National Grid), or long & lat with geographic system
We have included this in the revised manuscript.

Please add the ref for this biostrat
We have included this in the revised manuscript.

Colour does not necessarily imply increased TOC - it is also a function of mineralogy, particularly sulphides (pyrite is not necessarily correlated with TOC)
Data from the IFPEN archives report that OC-rich intervals are darker in colour, thus in this study it was appropriate to consider this observation during high resolution sampling.