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Interactive
Comment

Interactive comment on "Comparative organic geochemistry of Indian margin (Arabian Sea) sediments: estuary to continental slope" *by* G. Cowie et al.

Anonymous Referee #2

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Review of Cowie et al "Comparative organic geochemistry of Indian margin (Arabian Sea) sediments: estuary to continental slope"

This is a beautiful data set that is under interpreted and perhaps interpreted incorrectly. Basically, the authors argue that winnowing dominates over an O2 effect but that there might be a slight one one within the OMZ. However, when the data here are compared to other (oxic) margins (something the authors themselves do not do), the loadings of OC relative to grain size are enhanced for all the OMZ sediments but not for the shelf or deep (oxic) sites. This suggests that the interpretations are backwards, and that the O2 effect is present and enhancing OC preservation within the OMZ despite the winnowing delivering different sized materials to different locations. Please see my comments below, which are listed as a function of the flow of the manuscript.

The abstract is mostly qualitative not quantitative. ‘sites across’ not necessary in abstract

Why do you sometimes call the OMZ semi-permanent? (on page 3391 line 15 you call it permanent, as do you in Fig 2). When in recent memory has it been relaxed and oxygenated? Do you wish to use the term OMZ or perhaps think about changing to ODZ for deficiency? Every water mass has a minimum. . .

P3389 L15: I don’t like the use of the etc.

What about Keil and Cowie (1999) which showed a strong correlation with grain size and then enhanced preservation within OMZ sediments relative to the more oxic ones? There are loose but established SA-grain size correlations that the authors could use to evaluate their data in light of OC:SA-type interpretations.

3391 L15: aren’t most silty- or clayey shelf sediment anoxic anyways, regardless of water column conditions? And how does that impact the preservation potential of shelf sediments?

3391 L27: aren’t you jumping from Fig 1 to Fig 3 here? What about Fig 2?

The results section should be eliminated. As it stands this is wasted space. Perhaps rename the Discussion to ‘Results and Discussion’.

The discussion is very focused on the Arabian Sea and almost completely ignores other margins (ODZ-impacted or not). There is a wealth of knowledge available from other margins that could and should be called upon. The references list lacks some references used in the text.

3393 L15: the opening phrase ‘as illustrated in Fug 2’ is poor style. Simply make the factual statement and place (Fig 2) at the end of the sentence. Also eliminate the word ‘selected’. Combine the next paragraph with this one, and eliminate words such as

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‘notably’ and ‘dramatic’ and ‘clearly’ because they imply feelings not facts.

Section 4.1 of the discussion begs for inclusion of Keil and Cowie (1999). I hope to read later about how this region compares with other ODZ sediments for which there has been abundant research (e.g. Peru, Mexico, etc).

Section 4.2 again is dogmatic and uses words such as ‘clearly’. I’d argue that little is ever clear in science. The ‘notable’ lack of a trend in d13C is interpreted as being caused by better ventilation or less hypoxia, but these factors are not discussed and the paragraph ignores the possibility of winnowing delivering differently sourced material to different portions of the margin. What about the loss-and-replacement model of Keil et al (1997) where they used C isotopes and mineral SA to tease apart the dilution of terrigenous material versus replacement with marine material? In the current manuscript the authors have enough data to make somewhat similar calculations. This would help answer the questions posed at the top of page 3397.

Why say that the 13C mixing model is ‘for illustrative purposes’ but then rely on the numbers so much that they make it into the abstract? Why is 80% marine ‘overwhelming’ (which implies to me that the region is special) when most river-dominated systems are 70+% marine (Keil et al 1997 and other papers) and most other margins are also 80+% marine (Burdige 2005).

3397 L5: the inference that the terrestrial C carried by the rivers is within or near the estuary is grossly simplistic and ignores a large literature on C cycling processes in estuaries. I’d guess that most of this material is actually converted to CO2. A good place to start reading up on this topic is Bianchi 2011.

3398 L25: start a new paragraph with the THAA discussion? I am not convinced that the 15N data are useful but I think they should be shown. I suggest that the authors think about removing the discussion of the data (but leaving the data in the figures and tables). 3399 L10: site your own work Cowie and Hedges 1999?

How much of the material on the shelf is distinct debris that is fresh and currently undergoing diagenesis, versus more degraded debris? Have the authors considered the hierarchical roles for grain size and oxygen exposure for oxic and ODZ margins as presented by Arnarson and Keil (2007)?

3399 section 4.3 what exactly does 'low and uniform contribution of marine OM' mean? Fig 1 shows that the values are as high as 7% (much higher than the 'normal' 2.5% loading observed on most margins) and the previous discussion implied that >80% of this material was marine in origin, so what is low and what is uniform about the data? Continuing in this paragraph, the statement is made (line 15) that there is a several-fold difference in wt%OC within and below the OMZ, which argues FOR rather than against an oxygen effect. Ending on line 24; there is a rich wealth of information on hydrodynamic processes and their role in carbon storage on margins, yet the authors completely omit any references that are not for the Arabian Sea. Why? The paragraph ends on 3400 and lacks references. Are we still referring to the Calvert paper?

Throw away the first sentence (line 5) of the first full paragraph on page 3400. This paragraph contains an assertion that the oxygen in the OMZ does not go to zero, and in fact stays about 5uM. There are several published manuscripts that contradict this, none are referenced here. Electrodes on CTDs are notorious for having a zero value in the 2-5uM range. How do the authors know that their O2 data are accurate? Also, this paragraph does not actually add anything to the discussion and could be dropped.

3401 In order to continue the train of thought being developed, the first full paragraph on this page should be about OC and grain size, not about OC and O2.

3401 L5: I disagree with the conclusion of this and the next paragraph and feel that the authors are underutilizing their data and the knowledge that can be gained from other regions. They are relying on one Calvert paper while ignoring their own data (e.g. Fig 9) and the abundant other literature. I suggest that the authors flip their figures around, presenting Fig 9 before Fig 8. Why? Because Fig 9 'clearly' illustrates that the OMZ

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samples are ‘enriched’ in OC relative to other samples within this data set or within the literature. For example, within the lead authors body of work there is a paper (Keil and Cowie 1999) that ‘clearly’ shows OMZ sediments having higher loadings of OM per unit grains size than sediments from outside the OMZ (see their Fig 2). Arnarson and Keil (2007) saw similar things in their comparison of Washington and Mexican margin sediments. In that body of work, the authors argue that the O2 effect allows the winnowing/mineral effect to ‘protect’ more carbon that it would alone. What the new manuscript here offers is an opportunity to ask if that same thing (O2 enhances over the winnowing) is occurring in this location. Figure 9 ‘clearly’ suggests that this is occurring. The authors themselves even labelled that EVERY SINGLE sediment that has an exceptionally low OC loading is either shallow or deep (all the data to the right hand side of Fig 9). To reiterate, every single OMZ sample in this data set, regardless of grain size, falls along a single line with a slope of roughly 0.6 %OC per 10% silt+clay. Similarly, all the samples that are from oxic locations (deep or shelf) fall off that line and are lower. What does this mean relative to ‘normal’ continental margin sediments that are oxic? The line for these types of sediments has a slope of roughly 0.3, or half that of what the authors observe here (see Premuzic et al 1982 for the first and largest data set showing this, but there are many other papers showing the same grain size effect – a quick search on Web of Science found 31 papers relating OC to grain size).

The discussion next about the amino acid data again ignores non-Arabian Sea literature. In particular, there is a wealth of information available about the role of carbonates and opals in ‘driving’ the DI (e.g. Horsfall et al 1997, Keil et al 2000 and others). To me, these amino acid data again suggest an O2 enhanced preservation effect within the OMZ (many of the degradation parameters suggest the OC is ‘less degraded’ than above or below).

Overall, I think the authors have their logic straight but backwards. They argue for winnowing to dominate on this margin and that the O2 effect is essentially minimal. However, their data suggest that even within the OMZ the coarser grained sediments have higher OC loads than they would if they were sorted and delivered under oxic conditions. They don't realize this because they do not compare their data to data from other studies. They undervalue their grain size data and how it can be used to tease apart the importance of the different processes. I believe that the authors would be better suited modifying the text to suggest that while winnowing and sorting effects drive where there will be higher or lower OC contents in general, it is the O2 effect that is enhancing the OC loads within the OMZ.

This boils down, somewhat, to a question of whether wt% OC is the best way to evaluate preservation. The authors may want to re-read Hedges and Keil 1995 or any of the Mayer surface area papers for alternative views on the topic. In essence, when you rely on wt% OC as your factor for determining whether there is enhanced carbon preservation, you must normalize for the grain size effect. The authors here opt to highlight the grain size effect, which acts to apparently limit the actual O2 effect (or any other effect competing with mineral association). To put it another way, if a sediment is coarse and is 'supposed' to have 1%OC under normal oxic conditions but it is 2% within an OMZ, then the O2 effect has essentially doubled the carbon preservation. If another sediment within the OMZ is fine and supposed to be 3% but it is actually 6%, that again indicates that the O2 effect has caused a doubling in carbon preservation. In the current manuscript, the authors see only the difference between 2 and 6% and call that winnowing (they are right), but they then ignore the doubling of carbon due to the O2 effect.

So, which is more important? I strongly suggest that the authors conduct some multiple linear regression or other analyses to tease apart these differences. This data set is super and the potential is here for this paper to be great. The data beg for a multivariate analysis – especially given what is shown in Fig 9, which is essentially identical in shape to Fig 4 of Hedges and Keil (1995). In that article, the authors speculated that the shape of the curve was related to sedimentation rate and dilution of organics with large mineral grains.

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Even if the authors disagree with me about highlighting the O2 effect, it cannot be denied that the paper lacks any statistical approach to quantitatively evaluating the beautiful data set. That is a shame.

The lignin data do not take advantage of the LPVI to sort out potential sources. The high C/V ratios imply either selected degradation of the V component, or unknown sources. Since the authors (and previous studies) have looked at lignin in this region, a revised set of shadings for Fig 5b are called for.

The amino acid data looks roughly correlated with grain size wt%OC in the OMZ, where the Yokosuka transects are sandier, lower in carbon, higher in amino acids and less degraded. To me this makes sense.

Fig 1 should be a larger map to provide a reference for where the spots really are; especially for the readers who are not used to looking at maps of the western edge of India. Given the latitudinal length of the margin, the three sites are not that far apart.

Fig 2 should have a closed box with the legend within the box. The colors should correspond to Fig 1.

Figs 2,4, 6 and 7 could be merged into a multi-panelled image that takes up an entire journal page. That would help the reader visually compare different measurements.

Fig 7 median grain size should be converted to Phi units and the readers would benefit knowing if the distributions were modal and Gaussian or not.

Fig 4: Use a scale break to expand the top 200m of data since that is where all the action is. I'd give that 200m about 1/3 of the y axis.

Interactive comment on Biogeosciences Discuss., 11, 3387, 2014.