Response to Anonymous Referee #2

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 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.

We thank the referee for extremely thorough, and ultimately very helpful, critique of our manuscript. We agree with the large majority (though not all) of the recommendations, and have addressed all comments in a revised MS. Preliminary response to specific comments is provided below.

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

We have conducted a Person correlation analysis across all measured parameters as well as sequential multiple regression analysis for %C_{org} against potential controlling variables. We will include results in the revised abstract. Results indicate that 76% of variance in %C_{org} across the shelf and slope can be explained by two measured parameters, with % silt explaining the largest fraction (45%) followed by DO (31%).

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.

We will use common terminology throughout the text. Our intention was to distinguish seasonal hypoxia that occurs on the shelf from the mid-depth layer of oxygen depleted water, which is comparatively permanent (but shows wholesale fluctuation in the sedimentary record, on ~23ky cycles).

We recognise that OMZ is not a fully accurate or ideal term, but the same could be levelled at ODZ; if mid-depth waters are deficient, then what is the reference - saturation? - essentially all open-ocean water columns would also then have a mid-depth ODZ, just as they have an OMZ. We have used OMZ because it has been, and continues to be, the most commonly used term, not just in the Arabian Sea (by ourselves and many others over the last 3 decades) but in other ocean regions. Further, it is a term that has been used, for example, by Helly and Levin, 2004, in definitions of hypoxia (and thus assigning oxygen levels to the boundaries of the OMZ, as we have done here).

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

Ok

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?

For illustrative purposes, several examples were given of past studies that have come to different conclusion as to the primary controls on organic carbon distribution in the Arabian Sea. The lists were by no means complete, but we agree that this article deserves to be cited along with others in the pro-oxygen camp. We will address this and other associated matters further as part of a Grouped Response (below).

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.

Again we address this, along with findings in the Keil and Cowie 1999 paper, and others, as outlined below.

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?

The referee is quite right that muddy shelf sediments are generally reducing (within mm) regardless of water-column conditions. What sets this shelf apart is that it experiences regular prolonged periods of hypoxia, with associated impacts on bioturbation, ventilation and sediment biogeochemistry (as well as changes in productivity and current regime and spatial shifts from muds to sand). We do not assume that this has an effect on preservation potential. The fluctuation in redox conditions very distinctly distinguishes the nearshore muds (as well as shelf sands) from their counterparts on the slope. This study was an attempt to assess and delineate the effects of this, and other factors, on organic matter distribution.

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

This was an error. Corrected.

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

Agreed.

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.

Please see Grouped Response below.

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 'notably' and 'dramatic' and 'clearly' because they imply feelings not facts.

Corrected.

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).

Please see Grouped Response below.

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.

Please see Grouped Response below.

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).

Our choice to use the term "for illustrative purposes" was because, while we have some evidence from riverine SPOM values (now to be included in Table 1), these values are quite variable and we do not have a fixed terrigenous $\delta^{13}C$ endmember to work with. However, whichever endmember value one uses, the OM in all but the estuarine and nearest-shore sediments is predominantly marine, <u>and the 80% value quoted is a minimum</u>. We did not intend to suggest/ imply that the margin is exceptional, and will modify our statements and relate our findings to those from other margins.

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.

In the sentence immediately following we stated "Alternatively, as the large majority of OM in both riverine and estuarine particulates and in coastal sediments typically is intimately attached to associated mineral surfaces (Hedges and Keil, 1995), the results indicate that terrigenous OM export is greatly outweighed by local autochthonous OM production, and terrigenous OM is presumably remineralised and replaced by marine OM on mineral surfaces (Keil et al., 1997; Mayer et al., 1998)." Thus, we believe that we have reached the same conclusion drawn by the referee. We will clarify further, and cite the suggested paper.

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).

We are in two minds how best to respond to these suggestions. While both parameters can be useful in certain applications, neither the THAA nor the $d^{15}N$ data, alone or in combination with other parameters, are conclusive here as both likely reflect multiple factors (source and diagenetic effects) that cannot be readily resolved. We included them because they have been reported for the Pakistan margin (to which we compare our results) and, for example, the less positive $\delta^{15}N$ values on the Indian margin suggest less pronounced denitrification (hypoxia) than off Pakistan. But neither set of data is central to the main conclusions of the paper. Thus, our inclination is to remove them, both from the table and figures.

3399 L10: site your own work Cowie and Hedges 1999?

We believe that the referee refers to Cowie et al 1999 (Mar. Geol.) (?). While oxygen availability was concluded to be a factor controlling C_{org} distribution (on the Pakistan margin), it was also concluded that it is not necessarily the primary factor. This is basically the same as what we have concluded for the Indian margin in the present study (with additional information). We therefore chose to cite two examples of studies where oxygen availability has been concluded to be the primary control on C_{org} distribution.

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)?

The above are both valid questions. Please see Grouped Response below.

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?

This was an error. The intended statement was "low and uniform contribution of terrigenous OM". We apologise for the confusion. We did not mean to suggest that the amount of marine OM is invariant (with $\%C_{org}$ values up to ~7%), but that the percentage of the OM that is marine is high and relatively uniform.

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?

We did not intend to conclude one way or the other (on this basis) whether the difference in $\%C_{org}$ within vs below the OMZ argue for/against an oxygen effect.

The points we made are that, despite the several-fold differences that do indeed suggest an oxygen effect a) %C_{org} values do not always mirror (i.e. inversely relate to) oxygen (e.g. there is variability in %C_{org} within the OMZ, without parallel variability in oxygen, and %C_{org} values are often maximal in oxygenated/bioturbated sediments at the lower OMZ boundary, e.g. off Pakistan, or even below it e.g. off Oman) and b) diagenetic indices generally indicate only slight differences in degradation state within vs below the OMZ (as per cited references). Our intention was only to illustrate with this comparison of results from past studies of different Arabian Sea margins that the relationship between %C_{org} and oxygen availability is not straightforward, and that some studies – such as Calvert et al 1995 – invoke other factors altogether. Thus, our point is that the controls remain uncertain and a subject of debate. We make this statement more clearly in the revised manuscript, but we do not feel that it is necessary to refer to studies from other potentially comparable settings just to make this relatively simple point.

All statements following the reference to the Calvert et al 1995 paper on line 18, p 3399, were indeed drawn from that paper (hence the lack of further references). We also focused in particular on this paper because it is the most directly comparable to the present study (it is about the Indian margin), and our results in some cases support, but in others contradict, their findings.

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.

We are aware (and will clarify) that many O_2 measurements in OMZs, including our own (in this study and in our past Pakistan margin studies) may be erroneously high. On the other hand, we did not state that the minimum recorded value was “about 5 μM”.

The DO data collected on the Yokosuka transects were obtained not with a CTD but with a carefully cross-calibrated sensor on a submersible, sitting on the sea floor at the time cores were collected. Values recorded within the OMZ were as low as 0.5 μM (see data in Table 1), but all recorded oxygen levels were non-zero. We do not suggest that these were necessarily fully accurate. However, it is notable that, in contrast to the Pakistan margin, where sediments at the core of the OMZ are devoid of burrowing macrofauna and are laminated, benthic fauna were observed across the entire OMZ off India, consistent with non-zero oxygen levels. Also, in contrast to the extreme hypoxia sometimes observed on the shelf, sulfidic waters have not been observed within the OMZ. We chose not to elaborate on the minutiae of O_2 sensor accuracy because, ultimately, it is not important to our analysis (e.g. Fig 8) whether we accurately distinguish DO levels between 0 and 0.5 μM, given that the full DO range extends to >100 μM.

We have condensed and clarified the paragraph in the revised manuscript. But we disagree with the notion that a description of the oxygen conditions across the margin does not add anything – assessment of the role of oxygen as a factor controlling C_{org} distribution is central to the paper, and this paragraph lays the foundation. Thus, the details of how oxygen varies, across the margin and seasonally, are important (especially if we are meant to compare to other margins).

For response to following comments and selected comments above, see “Grouped Response” below:

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 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 grain 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 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 different processes. I believe that the authors would be better 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.

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.

Grouped Response:

We respond to the comments above, and to selected comments identified earlier in the review, because they all fall into three areas of critique:

a) Insufficient quantitative/statistical analysis of the data

b) Incomplete scrutiny of available interpretation potential , or reference to other studies/regions, and
c) Potentially incorrect interpretation regarding the relative importance of hydrodynamic effects vs oxygen exposure as controls on preservation.

We take exception with a few of the details (as specified below), but the referee made many valid and very useful comments and suggestions. We have revised the text and figures, and (somewhat) modified our conclusions, accordingly, and believe that the manuscript has benefited greatly.

a) *Insufficient quantitative/statistical analysis*. Following the referee's suggestions, we performed a Pearson correlation analysis on the full data set (excluding station co-ordinates and metadata, and the estuary stations, for which no DO data were available). Results will be explored in greater detail in the revised MS but, aside from expected (auto) correlations, such as between $\%C_{org}$ and %TN, and between % sand, % silt and % clay, it is notable that the strongest correlation for $\%C_{org}$ was firstly with % silt (positive), followed by that with % sand (negative), and in turn by that with DO (negative). These are entirely consistent with the concept of interplay between hydrodynamic processes and oxygen exposure controlling C_{org} distribution.

Taking this forward as an hypothesis, we conducted sequential multiple regression analysis of $\%C_{org}$ against the variables in Table 1. The results (as R values) indicate that % silt (46%) followed by DO (30%) can account for 76% of the variance in $\%C_{org}$ (with % sand adding another 3% and no other parameter responsible for more than 1%). However, there is uncertainty in DO values for shelf stations (which vary dramatically between seasons, with reported values being from the oxygenated intermonsoon period). In a similar analysis for slope sediments only (>200m), for which DO values are temporally invariant, variance in $\%C_{org}$ accounted for by % silt and DO rose to 84% (50% and 34% respectively).

At face value, this is evidence for the broader relative importance of hydrodynamics over oxygen as a control on $\%C_{org}$ distribution. However, this masks important detail. For example, if one separates the slope stations between those within the OMZ (e.g. 235-1000m) and those below (1000m+), % silt and %DO again account for a large % of the total variance (81%) within the OMZ, but %silt is by far the dominant control (78% vs 3%). The situation below the OMZ is reversed: the sample set is smaller but, while % silt and DO account for 92% of the variance in % C_{org}, DO becomes much more important than % silt (79% vs 13%).

Together, these results strongly support the main conclusions that we drew; that increasing O_2 exposure can explain the decline in $\%C_{org}$ below the OMZ, and enrichment within it (where it occurs), but that major variability in $\%C_{org}$ occurs within the OMZ (where DO is largely invariant), and this is driven by hydrodynamic processes.

In the revised MS we go on to explore these relationships further. Specifically, we plot $\%C_{org}$ and against % silt to illustrate both the strong correlation that exists within the OMZ and that sites above as well as below the OMZ show lower relative values (similar to Fig 9). Most notably, when the difference between observed values and those predicted from the regression line within the OMZ (i.e. the residuals) is plotted against DO, there is a clear negative relationship for the slope stations below the OMZ. This strongly supports the referee's assertion that sediments of a given grain size (or % silt) are generally more enriched in C_{org} within the OMZ than equivalent sediments from more oxygenated sites below; i.e. it is O_2 depletion that leads to C_{org} enrichment . The relationship is less clear on the shelf, possibly because of aforementioned uncertainty of DO values, but also because particularly coarse sediments show anomalously high C_{org} values. Thus, it is less clear whether sands within the OMZ are enriched in OM relative to those on the shelf.

b) *Incomplete scrutiny of available interpretation potential , or reference to other studies/regions*. As per our response to our first review, no other data set from the Arabian Sea (and not that many elsewhere), includes the same combination of parameters applied to estuary and shelf sediments as well as slope sediments. Thus, the scope for meaningful comparison is actually somewhat limited, and we naturally focused more attention on the Calvert et al paper because it is also about the Indian margin and includes sites from the shelf as well as the slope. More importantly, the thorough comparative analysis sought by the referee is precisely what we will present in a MS that is currently in preparation, which will be a comparison of data (some already published, some new) from the Oman, Pakistan and Indian margins of the Arabian Sea and, will include grain size and surface area data as well as the same suite of source and diagenetic indicator biomarkers presented in this study. We will be in a much better position to draw broadly reliable conclusions at that stage.

Above all, we believe strongly that this data set provides important new information, and deserves to be considered, in its own right (as has been the case for most past studies in the Arabian Sea and many other margins), and not purely through comparison.

None the less, we accept that more thorough analysis (e.g. of C_{org} to grain size and surface area relationships,etc) alongside more thorough reference to previous studies, from the Arabian Sea as well as other regions, would enhance our interpretation in several areas. In particular, the revised MS includes more explicit references to particularly relevant studies, such as Keil and Cowie 1999 and Arnarson and Keil 2007 (amongst others).

c) *Potentially incorrect interpretation*. We believe that the referee has misinterpreted our conclusions, as reflected in the statement that "*They argue for winnowing to dominate on this margin and that the O2 effect is essentially minimal".* These were not our conclusions, and we do not believe that our logic is backward or, in fact, fundamentally different from the referee's. We believe that both hydrodynamics and oxygen are important; where we perhaps have a difference in perspective is whether/where one is more important than the other. It is a matter of perspective.

We also did not "*rely on wt% OC as your factor for determining whether there is enhanced carbon preservation"*. At no point did we use %C_{org} <u>in itself</u> as a measure of preservation – we used Fig 8 to show the decline in %C_{org} with increasing DO below the OMZ, but also to illustrate the large range of %C_{org} values at low DO values within the OMZ. The same was done, for example, by Hartnett et al (1998). We relied on diagenetic indices, and cross-margin trends in these, to look for differences in preservation state.

Though we accept that we could perhaps have refined our statements, we did not conclude, or even mean to suggest, that O_2 is unimportant. Its importance is certainly better highlighted by the more thorough and quantitative analysis recommended by the referee, as outlined above and as will be included in the revised MS, but we never concluded that oxygen is unimportant.

Specifically, our final two conclusions were:

- Organic matter enrichment in upper slope sediments is due to a combination of physical processes (winnowing and dilution) on the shelf and progressive decay of OM with increasing oxygen exposure below the OMZ.
- Major variability in sediment OM content within the OMZ is strongly linked to grain size distributions. Thus, while low oxygen exposure may contribute to OM enrichment in the OMZ, hydrodynamic processes are the overriding control.

Further, we also stated elsewhere in the text that the apparent importance of oxygen exposure below the OMZ contradicts the findings of Calvert et al (1995). What is lacking, and will be altered in the revised MS, is a clearer statement that OM enrichment within the OMZ is attributable to O_2 depletion.

However, while OMZ sediments may be enriched in OM relative to sediments of the same grain size distribution below (and in some cases above) the OMZ, oxygen depletion ultimately becomes a side-show within the OMZ. The fact is that sediment OM contents within the OMZ, at very similar DO levels, can vary from a maximum of ~7% to as low as ~1-2%, which is as low as sediments found at 2000m and many found on the shelf. Thus, hydrodynamics are the overriding control on C_{org} **distribution** (<u>not</u> preservation), and this is most evident within the OMZ. The lack of similar variability within the OMZ off Pakistan is due to the relatively quiescent conditions on that margin.

Finally, we note that our data suggest that hydrodynamic processes also are the predominant control on C_{org} distribution across the shelf. If these stations were taken in isolation, this would be particularly evident – %C_{org} varies from <0.3% in offshore relict sands up to 3.8% in nearshore muds, while the same sites experience very similar (seasonally fluctuating) redox conditions. What is then less clear is whether slope sediments contain OM that is better preserved (less degraded) than in the seasonally oxygenated and bioturbated sediments on the shelf. There is some evidence for this (see discussion above), but there is also evidence against. We cannot agree with the referee's claim that "*amino acid data again suggest an O2 enhanced preservation effect within the OMZ (many of the degradation parameters (Fig. 10) suggest the OC is 'less degraded' than above or below)"*. The indices certainly are consistent in showing less degradation in sediments within vs below the OMZ, but the same cannot be said for sediments above the OMZ. The three parameters produce contradictory cross-shelf trends, and comparison to slope sediments is somewhat complicated by the wide range in sediment types. However, to compare "like for like", none of the parameters indicate that OM in the nearshore muds is systematically more degraded than that in corresponding muds on the slope. Thus, while enhanced preservation may contribute to C_{org} enrichment on the slope, it is not evident from these indices.

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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.

We agree that the high C/V values recorded for the Yokosuka North transect are anomalous and indicate either preferential loss of V phenols or inputs by plants with exceptionally high C phenol contents (relative to the plant suite characterised by Hedges and Mann 1979, which is the source of the "nonwoody angiosperm" box shown in Fig. 5b). We included the discussion of the phenol compositions only because there appeared to be a systematic difference in the Yokosuka North sites. The LPVI of Tareq et al 2004 is also based on the Hedges and Mann 1979 plant suite, and thus does not help to resolve this particular matter. Moreover, LPVI values yield the same broader inferences about mixed plant sources that we drew from S/V and C/V ratios.

The phenol yields of essentially all of the slope sediments are extremely low, and border on detection limits for the sandier sediments, and results are correspondingly less reliable. For this reason, and because this analysis is tangential to the main thrust of the paper, we have decided to drop Fig 5b, and associated discussion, from the revised MS.

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.

We agree, and explore whether the limited OM in coarser sediments is "fresher/more reactive" in the revised MS.

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.

The site map will be improved in the revised MS. The sites cover a total distance (N-S) of almost 400km.

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

Ok

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.

We agree that this is a good idea. We made an attempt, but found that the individual panels became too small to see important details. However, we will make an effort to combine figures where possible.

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.

We have altered the grain size units (in Fig 7 and Table 1) as requested, and will provide information on modality etc (as per results from GRADISTAT software).

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.

Good idea.