

Interactive
Comment

***Interactive comment on* “The effect of meter-scale lateral oxygen gradients at the sediment-water interface on selected organic matter based alteration, productivity and temperature proxies” by K. A. Bogus et al.**

Anonymous Referee #1

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General Comment:

This is a clear and concise report on a study of how lipid-based parameters that have previously been found to offer indices of aerobic alteration, export production and sea surface temperature respond to varied depositional redox conditions. The three sets of samples – a cross-margin transect spanning an oxygen minimum zone (OMZ), and meter-scale transects across different methane seeps (within and below the OMZ) – represent a conceptually elegant sampling strategy. It encompasses demonstrated contrasts in SWI oxygen availability/penetration and, with the OMZ seep, a means for

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assessing cross-seep variability with(near) constantbottom-water oxygen levels. The writing and structure are clear, figures and tables are all presented well, and referencing is generally appropriate and thorough.

To varying degree, the results show consistent trends in the various alteration indices across the cross-margin and seep oxygen gradients. On the other hand, it is clear that some export production indices vary quite considerably across both of the small-scale seeps transects, in some cases with contrasting trends relative to changes in oxygen concentration. Finally, the three different GDGT-based temperature indices show no consistent differences across any of the oxygen gradients, suggesting that they are resistant to post-depositional oxidation. However, to varying degree they also produced unrealistic SST values as a result of as-yet-unidentified water-column and/or sedimentary processes. *All of these findings are important, both for the support they provide for the postulated alteration/oxygen indices and for the notes of caution that result for proper interpretation of production and SST indices.*

Specific comment:

Nonetheless, there are some problems that need to be addressed.

Firstly, the authors incorrectly cite a number of papers in support of a statement that “bottom water oxygen concentration is the primary control on OM preservation” across the Pakistan margin (p 11362, l 3-7). It may seem pedantic, but the question of what controls OM distributions across all Arabian Sea margins (and elsewhere) remains a subject of debate. It is perhaps true that Paropkari et al, and more notably van der Weijden et al (which focused on the Pakistan margin) drew the stated conclusion. However, numerous other Arabian Sea studies (by Calvert et al, Pedersen et al, and others) have argued that oxygen is at most a contributing factor, or that it is minor (i.e. a symptom more than a cause). But that is not the main point. Contrary to the authors’ assertion, the other papers that are cited by the authors (Cowie et al, Keil and Cowie and Schulte et al, which also focused on the Pakistan margin) actually concluded (to

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varying degree) that oxygen was a contributing factor but not necessarily the primary factor. More recent studies from the same margin (e.g. Cowie and Levin, 2009 DSR II and references therein), and other Arabian Sea margins, have drawn similarly mixed conclusions.

Either the uncertainty and debate need to be acknowledged and statements altered accordingly, or, more simply, the authors should state what is important and relevant to this study, which is that their OMZ transect and below OMZ seep transect unquestionably represent differences in bottom-water oxygen concentration.

A second problem arises with the methodology. Although method descriptions are otherwise good, we are never told the depth interval(s) used for the surface samples analysed at each site, or even if a common depth interval was used. This needs to be addressed.

Linked to this is a problem with the basic underlying assumptions that surficial samples (whatever depths these cover) represent "freshly deposited material" and that observed trends necessarily reflect rapid response to the oxygen concentrations of overlying waters. While the authors correctly state that turnover of freshly deposited organic matter is typically rapid, what they ignore is that, in addition to acknowledged differences in microbial populations across both the margin and seep transects, there undoubtedly also are major differences in faunal communities. The authors also tacitly assume that sediment accumulation rates and rates and depths of mixing, are either the same at all sites within a given transect, or make no difference.

Both the size and composition of benthic communities, and the extent and depth of bioturbation, vary across the Pakistan margin, with dramatic shifts over short spatial scales across the lower OMZ transition. There are doubtlessly also steep gradients in faunal populations and activity from the centres of methane seeps across bacterial mats to adjacent clam beds to the sediments beyond. Furthermore, there are likely to be considerable changes (across both margin and seep transects) in the relative

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abundances of meio- and macrofauna, and in scavenging epifaunal and infaunal of varied feeding mode. As such, surface sediments across these transects very likely experience very different histories in terms of benthic faunal digestive processes, and not just differences in short-term exposure to oxygen at the SWI.

What is more, in addition to differences in porewater oxygen penetration, there are almost certainly also major differences in sediment accumulation rate between the OMZ core and well below the OMZ, and even across seeps (though it is not clear how sedimentation varies between a seep vent, a bacterial mat and adjacent sediments). Add to this the variation in sediment mixing and irrigation arising from differences in faunal communities and activity, and you end up with surficial sediments, across both margin and seep transects, that may be very different in terms of mixing between surficial and sub-surface horizons, and thus in average age as well as digestive history and extent of oxygen exposure.

In short, the system is not as simple as the authors portray, and any differences in OM composition observed across these transects may be due to faunal as well as aerobic microbial alteration, and to greater age and exposure to oxygen than would be inferred from position at the SWI and instantaneous bottom-water oxygen concentrations or porewater oxygen profiles.

The authors need to acknowledge that observed alteration is a composite effect of microbial and faunal processes, and, because of reworking and sediment mixing, inevitably *more a reflection of differences in overall OM alteration and oxygen exposure time than bottom-water oxygen concentrations.*

Above all, *they need to remove references to “rapid” OM cycling* that they infer from the meter-scale seep transect, because they do not have a timescale on which they can base such statements.

This does not necessarily impact on interpretation drawn from the cross-margin trends, which inherently incorporate effects of contrasting benthos as well oxygen concentra-

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tion. But there is no clear evidence from this data set that, for example, shorter-term changes in bottom-water oxygen concentration would necessarily be recorded in sedimentary records or as differences between sites.

With respect to the export production indices, it would be sensible for discussion to consider what the cross-margin transect data indicate relative to what is actually known, or might be guessed, about cross-margin trends in productivity and export production. It would also be helpful to know how all of these values relate to bulk organic C content. At present, it would seem that differences are assumed to be due only to post-depositional alteration.

Technical details:

There are numerous compound words in the text which, debatably, need hyphenation. Hyphens are included in some cases and not in others, and I think that the authors need advice as to the journal's policy.

The authors use the terms “anoxic”, “suboxic” and “oxic”. Definitions are (more or less) included in the text, but I would take issue with the term “anoxic” when there are clearly non-zero bottom-water oxygen levels at some “anoxic” OMZ-seep sites (given the presence of clamsetc). Notably, the presence of bacterial mats does not actually indicate the extreme of anoxia (i.e. zero oxygen). I think that “hypoxic” might be a better term.

Figure 3 needs definitions of symbols and error bars. Also, oxygen concentrations at the SWI (expressed in μM) are quite different from values (several fold higher at the oxic site) from the CTD-based bottom-water oxygen concentrations for the same sites that are listed (as ml/l) in Table 1.

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In Figure 4 it is not clear what “Replicate to below OMZ seep-1” and “Replicate to below OMZ seep- 2” actually mean. “Replicate to” what?

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