

Interactive comment on “Organic carbon production, mineralization and preservation on the Peruvian margin” by A. W. Dale et al.

Anonymous Referee #2

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General Comments:

This manuscript presents a compilation of sediment geochemical and radiochemical data that is combined with in situ benthic flux measurements and modelling to estimate primary production, water-column and sedimentary C turnover, and sedimentary C burial efficiency across two transects on the Peruvian margin (from shelf to ~1000m depth, at the lower boundary of the OMZ).

The main strengths of the manuscript are the comprehensive, high-quality data sets and the attention that is paid to the modelling approach. This combination is potentially powerful; determinations of carbon burial efficiency are rare enough in themselves, but especially those that include shelf as well as slope stations, and comparisons of benthic and pelagic C turnover for the same site are even rarer. The authors also have done

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a good job of assessing their results in the context of previous results available from both normoxic and other hypoxic margins. The manuscript is generally well written and easy to follow, although there are some problems with structure, language and figures (see below).

The significant findings of this study are not the estimated pelagic turnover rates, which appear surprising given the low mid-water oxygen levels; the authors rightly acknowledge considerable uncertainty here, especially as the calculations rely heavily on modelled estimates rather than direct measurement of pelagic processes. Nor is it surprising to learn that cumulative CBE across these margins is higher than for normoxic margins. The main points raised by this study are the (apparently) low CBE for shelf stations and the high CBE for sites “below” the OMZ.

The weaknesses of the study are, firstly, that it includes a comparison of new data from one transect to previously published data from another but, ultimately, little is made of, or learned from, this comparison; we do not really gain anything that we would not have learned from one transect or the other.

Secondly, while uncertainties and possible errors associated with previous studies (short trap deployment durations, lateral sediment transport etc) are discussed at some length, discussion of uncertainties associated with the approach used here are effectively relegated to the very end of the manuscript, and then are not adequately addressed.

Finally, and most importantly, the discussion ends up being in large part a reworking of existing debate on factors potentially affecting cross-margin organic carbon distribution/preservation, without, in my view, adding significantly to that debate. The section entitled “Significance of O₂” suggests that O₂ concentration is not the appropriate metric by which one should assess O₂ effects on C distribution/preservation, and then ends without any clear conclusion other than a statement that the authors do not wish to repeat discussion to be found in previous studies. But that is exactly what they go

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on to do.

The following two sections, on “Sorptive Preservation” and “Macrofauna” address neither of these factors directly, and do not offer new information, since the necessary studies were not conducted here. Instead, both sections wander. Sorptive effects are said to have been “ruled out” as a factor in a previous cross-OMZ study in the Arabian Sea (Vandewiele et al, 2009), a conclusion that is highly debatable, but there is then a quite laborious discussion of porewater organic matter. The section never actually reaches a conclusion as to the overall role of sorptive processes in explaining observed cross-margin organic matter or carbon burial efficiency distributions. Likewise, the entire discussion of possible macrofauna effects is based on previous studies which generally (e.g. Koho et al 2014) have suggested the importance of macrofauna (through their absence at the OMZ core in the Arabian Sea) but, critically, rarely if ever have actually directly tested this inference. The present study offers no new information on the subject, and instead goes on to discuss cross-OMZ differences in organic matter quality/reactivity found in previous studies (potentially but not necessarily related to the presence/absence of macrofauna?). But these were again not measured in this study and, once again, the section reaches no clear summary statement. We are left with no coherent proposed explanation for the observed cross-margin trends in CBE (contrary to the opening statement of the Conclusions section).

Specific comments:

I suggest a fundamental re-organisation and focusing of the discussion. Firstly, the authors need to fully acknowledge and address up front the uncertainties in their own approach, which I believe actually throw into question whether the calculated CBE values on the shelf or below the OMZ are, in fact, anomalous or even valid.

First and foremost, the authors need to acknowledge that, very similar to the shortcomings of many sediment trap deployments, the DIC flux determinations on which their calculations depend are “snap-shots” that may be fundamentally different from the

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longer-term average of C accumulation/burial represented by the underlying sediment records. The authors belatedly acknowledge the problem of seasonal variability in their discussion of primary production, but only to the extent that the error bars inherent in the model (which itself is acknowledged to poorly represent PP in nearshore waters) are somehow representative of intra-annual variability in PP. This notion is poorly explained and seems decidedly questionable. My guess is that seasonal variability in PP is, in fact, much larger than this. But more importantly, there is an apparent implication that the same variability applies to benthic DIC fluxes. In fact, variability in benthic C turnover (i.e. DIC fluxes) is potentially quite different, due to the impacts of seasonal changes in bottom-water redox conditions. In short, measured DIC fluxes (determined when shelf bottom waters were anoxic) may have been wholly atypical. This needs to be addressed.

The second element of the CBE calculation is C burial rates, which depend on the accuracy of chosen background (buried) C values, but also of estimated sediment accumulation rates. The authors acknowledge uncertainty in the former (e.g. variability in past C burial – such as during the little ice age), but assessment of possible error in accumulation rates is only through the model itself. The authors contend that their calculated accumulation rates are independently validated by peaks in bomb-derived ^{241}Am released from the early 1960s (data not shown). But for most/all of these sites, these peaks would appear within the surface mm-cm, and therefore are of debatable value in providing validation for profiles spanning 10+ cm depth. Notably, bioturbation alone can lead to penetration of relatively short-lived isotopes such as ^{210}Pb , and to depth profiles that appear to indicate sediment accumulation. From personal experience, accumulation rates estimated from ^{210}Pb profiles, even in only moderately bioturbated sediments at sites near OMZ boundaries (laminae are still visible but x-radiography shows clear evidence of burrowing), that are an order or magnitude (or more) greater than those derived from ^{14}C profiles from the same cores (unpublished data). So, while the authors have used a robust modelling approach, there is nonetheless a strong possibility that estimated sediment accumulation rates for sites above and

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below the OMZ, and therefore CBEs, have been overestimated.

Once these uncertainties are acknowledged and findings are modified as required, the discussion should be streamlined so as to focus on data from the present study and findings that directly contribute to the debate over processes contributing to observed C distributions:

Firstly, with respect to the assessment of O₂ as a controlling factor, it needs to be made clear that, by most definitions, the sites referred to as “below the OMZ” in this study are questionable. These sites, despite having bottom-waters that are more oxygenated than those within the OMZ core, do not represent the marked contrast (in terms of redox conditions or benthic communities etc) that would be found at greater depth. In other OMZ studies, these sites might well have been placed within the OMZ (e.g. if a 50 µM limit is set, as per Helly and Levin 2004).

Further, the authors state that bottom-water oxygen concentration is not the appropriate metric, but then (mystifyingly) make no reference at all to the one parameter, oxygen exposure time (OET), that has been shown to provide a valid metric. Were oxygen penetration depths determined for the sites in this study? If not, it should still be possible to estimate, or at least constrain, oxygen penetration depths from other porewater profiles or via modelling. Either way, such estimates, combined with sediment accumulation rates, would permit OETs to be estimated (or at least constrained). This would allow comparison of CBE assessed in relation to OET, and for data from this study to be compared to results from other margins (see multiple papers by Hartnett, Keil, Hedges and others). It would make for a much a meaningful assessment of how oxygen, relative to other factors, contributes to observed cross-margin C distributions. Also, I am fairly certain that such measurements might have shown that, despite more oxygenated bottom waters, the sites “below the OMZ” still have short OETs. If this is the case, one may not need to invoke lateral sediment transport to explain high CBEs at these sites. Notably, previous studies (e.g. Hartnett et al 1998) have demonstrated that, while CBE tends to decrease systematically with increasing OET at longer OET,

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widely variable CBE are common at low OET. Therefore, other factors clearly contribute in determining CBE at low OET, and may contribute to observed differences in CBE between shelf, OMZ and “below”-OMZ sites.

The authors mention that there is cross-margin variability in sediment grain size, but we are never shown the data, and there is no discussion of whether grain size varied down-core (which might itself explain down-core variability in C content). Also, all results from station 8 are ignored because, without further clarification, this station is stated to have been affected by “erosion”. These observations strongly suggest that, as across other shelf-slope transects, hydrodynamics (not just lateral transport, but the multiple influences of bottom-water currents, winnowing, physical reworking and particle sorting) may be a very significant factor in determining C distributions. But this is basically ignored other than vague and indirect discussion of “sorptive preservation” and a belated mention that lateral transport might contribute to high CBE below the OMZ.

My suggestion is that the authors should present ALL relevant data and reframe their discussion to include issues of flow regime, lateral transport, sediment texture and sorptive preservation as part of a more focused and explicit discussion of hydrodynamic impacts on C distributions (lateral and, possibly, downcore). For example, if the authors have grain size data, then there are ways to estimate specific surface area, which would then allow C contents to be assessed in relation to surface area. This would permit a more legitimate and more direct assessment of possible sorptive preservation effects, and for results from this study to be compared to OC:SA results from other margins (normoxic and hypoxic). There is now a substantial literature on this subject. Also, if there are grain size data for intervals down core, it would perhaps help explain some of the variability that is observed in C contents.

I am quite confident that all OC loadings, at sites above, within and below the OMZ, will be found to be greatly in excess of those found in sediments of equivalent grain size (or surface area) on normoxic margins, and there may be no clear positive correlation be-

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tween OC and SA. The excess loadings would provide confirmation of the importance of low O₂ availability as a controlling factor across this margin. Notably, however, a lack of correlation would not “rule out” the contribution of hydrodynamic processes (as previously concluded for the Arabian Sea by Vandewiele et al 2009). Oxygen deficiency may lead to general OC enrichment, but hydrodynamics may still dictate distribution.

Also, the authors need to specifically address the questions of bottom-water flow regime and sediment texture with respect to their calculated benthic fluxes. They have made the unstated assumption in their calculations that all sediments across the margin are (equally) cohesive and unaffected by advective pumping found in more permeable sediments, especially on continental shelves. If, as the authors acknowledge, there is variability in grain size (not shown), and data from one site are rejected because of “erosion”, then it strongly suggests that bottom-water flow regime is a highly relevant issue, which might again throw calculated DIC fluxes (determined in sealed chambers with either no flow or constant flow?) into question.

Without a reshaping of the discussion, to include more thorough analysis of the results and more direct assessment of oxygen and hydrodynamic effects (etc) as factors controlling CBE etc, there is a real question as to whether this manuscript is publishable.

The discussion of macrofauna and cross-margin differences in organic matter quality observed in previous studies, should be greatly reduced or eliminated altogether, as the present study offers no new information on these subjects.

Technical comments: 1. Sentences are frequently started with symbols or acronyms. This is sometimes difficult to follow, and is generally considered poor style. However, spelling out all words at the start of sentences (or in section titles) is not a policy enforced by all journals, and it will be up to the handling editor to decide whether it is BG policy. 2. The authors need to be consistent with their use of compound words. ALL cases where compound words are used as descriptors (i.e. adjectives or adverbs) should be a single word or have a hyphen, but not otherwise. So, it is “bottom-water

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oxygen levels. . .” and “oxygen levels in bottom water (not bottom-water)”. Similarly, “onboard (or on-board) analyses were carried out. . .” but “. . .analyses were carried out on board (not onboard, as is used on multiple occasions). The authors should check for these errors throughout the manuscript, as there are numerous examples. e.g. oxygen-deficient, carbonate-poor etc. 3. In multiple places there is a misuse of the term “rapidly” to describe trends with respect to distance (rather than time). For example, on line 6 of the abstract, “. . .rapidly with water depth” should read “. . .sharply (or steeply, or similar) with depth” as there is no time scale by which to say any change is rapid. 4. P 13072, L28. Predominantly (not predominately) 5. P 13073, L1, Use oxygenated in place of oxygen-containing (less awkward). L 13. Describe the flow regime inside the chambers (stirring rate etc). 6. P 13704. L11. 80 cm-long L12. 15 mL (since this is the unit used elsewhere) L18. Define MUC. Do not bother telling us about BIGO cores that were not used. L27. Delete “gas” after argon. L29. 0.2-um 7. P13705, L2. “ for analysis on shore” (not onshore). Section 3.2. Stated precisions and detection limits for all methods need to be defined. Also, what is meant by an “onboard detection limit”? What difference does being on board make? 8. P13076. L24. 2-cm 9. P13077. L3. Should read “closed-system behavior” L16. It is not necessary to tell the reader that there were no spurious outliers (that were not used). L20. It is not clear why or how corrections were made for hydrogen sulphide. 10. P13080. L21. How was it established that non-local transport rates by burrowing organisms were very low? 11. P13081. L10. dissolution or precipitation. . . L19. In steady state (not steady-state). L25. Discretized is not a word. 12. L29. Invariant (not invariable). 13. P13084. L17. The authors need to be very clear in their terminology when referring to fauna. When they say that “megafauna” were absent during fieldwork, what does this mean? And was this at all sites? And what about meio- and (burrowing) macrofauna? If they are suggesting that all burrowing fauna were absent, how was this established? And what size class do they mean when they say there is intermittent colonization? And at what sites (all?). And how long does this colonization endure? (What is the balance between periods of conditions with/without burrowing organisms?) . This is



currently far too vague. 14. P13085. L1. How was it established that erosion occurs at Station 8 at 12oS? Why does this only occur at this site? Seems rather peculiar, and suggests that hydrodynamics are a factor that needs to be considered explicitly. 15. P13086. L1. Maximal POC contents. (maximum is a noun) L3. Lower OC contents on the shelf are normal, not anomalous. Clarify logic. 16. P13087. L15. Refractory (not refractive, as in refractive index). 17. P13088. L 6. Maximal CBE... L 12. ...and or?. 18. P13089. L11. Start a new paragraph with “The rate at which...”. This sentence currently does not follow from preceding text. L26. “Pooling the data ≥ 101 m” does not make sense. 19. P13090. L 13. Offshore of Peru. (this error also occurs elsewhere in the text, including offshore of Chile on the following page). 20. P13091 and 13092. The paragraphs starting with “Microbial communities...” and “Multi-decadal oscillations...” wander and do not come to clear statements. This applies to the section as a whole...i.e. after describing how turnover rates of C within the water column were estimated, the discussion wanders and no clear summary statement is reached. This needs to be tightened up. 21. P13904 and beyond. Section 5.3. My criticisms of this section are outlined above. The section on O₂ never addresses the concept of oxygen exposure time, and instead wastes considerable text summarising the opposing conclusions of various previous studies, and never really goes anywhere new. Likewise, the section on sorptive preservation never directly or adequately assesses whether this process (or organic-mineral interactions or hydrodynamic processes) might be a contributing factor, and instead wanders, again never reaching a clear statement. The section on macrofauna is even less informative, and should be eliminated altogether. The section on lateral sediment transport needs to be incorporated into a revised analysis of hydrodynamic factors, and reviewed in light of the comments above – i.e. high CBE “below” the OMZ could be explained by OET that is short despite slightly higher bottom-water O₂ levels. 22. Section 5.4 (P 13099). The analysis of uncertainties in the present data set needs to come up front, before or alongside any discussion of uncertainties in other studies/approaches. 23. P 13101. Conclusions and outlook. Ultimately, this becomes a wish list for future research. Very little that is new or of

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substance is derived from the data presented here and, for reasons outlined above, I think that more could have been done on this front, in place of some rather speculative wandering. 24. P13102. L12.in the long term (not long-term). 25. P13102. The plural of gear is gear (in this context). (also found in the caption to Table 1). 26. Figures. ...generally good quality BUT, in many (especially those with multiple panels) symbols, units, legends etc are vanishingly small and impossible to read. These need to be enlarged.

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