Second Review of Doering et al. for BGD

By Patrick Rafter*

Summary

First, I'd like to apologize for the tone of my last review—it reads as a more negative review than I intended. This is an improved manuscript and because I better understand the arguments and assumptions being made, I now have more comments to improve the manuscript. First, I would like to see more elaboration on the origins of Fig. 2, especially since it includes new information / calculations. But most importantly the Discussion section needs a complete overhaul.

As I stated in my 1st review, "I don't think the Discussion section is the location for describing every individual wiggle of the observations… No one wants to read a listing of which way the wiggles are wiggling and when. :)" Unfortunately, the new Discussion section is not significantly changed from the earlier manuscript. I think that much of the current Discussion text can be moved or in many cases removed entirely—it is unnecessary to describe every single wiggle and how they relate to every other wiggle. General statements can be useful and the reader can look at the data themselves. Statistics are even better!

To state this differently, the data and the application of the data is interesting and worthwhile, but the Discussion of the data can be much improved by discussing the results in the Discussion section. First, I would categorize most of the Discussion as unnecessary and / or Results section related text. Second, I would suggest how the interpretation of these results is consistent with theories and previous datasets about changes in ENSO variability over the last 600 years. I suggest a complete rewrite of this section with an emphasis on: (1) why these changes are consistent with ENSO and (2) the consistency of the implied changes in ENSO with other datasets.

Line by line notes:

Line 42: result

47: remove "\"

48: confusing "material from the and…"

62: Shouldn't this isotope effect or fractionation factor be negative if the other isotope effects (for uptake) are negative?

91: wrong tilde

118: this assumption of the depth of upwelled water is somewhat arbitrary, but I think it is ok. You could reference a study that has identified the depth of source waters.

121: It is here in the description of Fig. 2A that I realize that how this figure was made has not been described. Am I wrong in thinking that it uses the new data first shown in this manuscript? If so, it seems like the new data should not be included in the Introduction.

144: At this point I again realize that these figures (Fig. 2A, 2B, and 2C) seemingly are using new data that has yet to be introduced. Furthermore, while I think it is important for the reader to understand this spatial variability, the methods used to create these figures (even if they are from earlier work) should be described.

147: The anaerobic oxidation of ammonia (Anammox) does not directly influence the concentration or isotopic value of nitrate.

164-171: This is a good section.

Section 2.5: I had a difficult time understanding this section and it was the second time I have reviewed this technique. I don't have a specific suggestion for editing this section, but I think the authors should take my difficulty into consideration. For example, could this be more easily explained using an illustration? Or an analogy? I'm simply suggesting that they should consider alternate approach for describing their methods here.

265: there is no Fig. 4E

309: Doesn't this sentence need a callout to Fig. 5? The text confuses me because it is seemingly using the MAR as a proxy for upwelling strength (Fig. 3), but the text already quantifies nutrient supply in Fig. 5. These need to be considered together or the text should only use the Fig. 5 estimates.

317: needs a period instead of ;

317-318: This last part of the sentence is vague. "In phase" is another statistical term that should not be used to describe wiggles that look like they are going up and down at the same time. Statistics can prove me wrong.

328: this sentence is confusing

330: remove lower

336: this is a great introductory paragraph for the Introduction! Gives the reader a good motivation for why the study is worthwhile. The beginning of the Discussion should be used to restate the question being addressed and point to the Results that

improve our understanding. Also, I am of the mind that time should move forward in the narrative of describing a time series. Beginning with the most recent events and moving back in time is awkward.

344: “latter” refers to the sentence above, but can also be misinterpreted / misread as “later” and should be removed.

348: “This was inferred to result in” is awkward. Reword.

355: element

373: more reference to upwelling strength in Fig. 3. The estimate of nutrient supply rate is one of the cool, new things provided by this study. This should be the focus. Furthermore, it should be made clearer in the manuscript whether the new nutrient supply rate estimates were consistent with he sediment MAR. That is a new contribution to the field.

389-390: Once again, the MAR in Fig. 3 are being used to describe a variety of processes that were (presumably better) estimated later in the manuscript in Fig. 5.

393: I though “correlation” was removed from the text?!!

401: There are a couple instances where the coretop and CWP values are used to estimate nitrate and silicate utilization at the surface. But how well do these compare with the observed modern values?

416: Almost all of this is a boring description of how the different proxies or metrics vary, which is not even altogether necessary in a Results section. It certainly does no good in a Discussion section.

417: this reads to me like equating changes in source nitrate d15N with changes in nitrate utilization. It could happen, but need not be related.

424-426: Despite there being no measurements, the reduction in denitrification predicts a lowering of nitrate d15N. Why can’t we use this assumption here?

454: not “changes-in”

460: Another prime example (out of many) of text that belongs in a Results section.

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While it is interesting that these results are mostly consistent with other sedimentary proxy results indicating higher or lower denitrification rates, productivity, etc., the reason we care about this is because of ENSO, right? How does this new data fit within the abundant datasets on ENSO activity over these

timescales? I would think that this would be the prime focus of the Discussion section.

*Why I am signing all my reviews
Full transparency of peer reviews makes reviewers accountable for their work. I say this based on my own experience; my signed reviews are more thoughtful and useful, which leads to better science.