

Interactive comment on “Photic zone changes in the North West Pacific Ocean from MIS 4-5e” by G. E. A. Swann and A. M. Snelling

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Received and published: 17 November 2014

We welcome the positive and constructive comments made by both reviewers and have adjusted the manuscript accordingly as outlined below.

Reviewer 1 (responses in bold)

1. The reason why variations in pCO₂(aq) or $\delta^{13}\text{C}$ -DIC have negligible/minimal impacts on $\delta^{13}\text{C}$ -diatom is not clearly explained. **We have expanded this paragraph to explain this in more detail**
2. My complaint about the paper is that they treat biogenic opal as a better pale-

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oproductivity proxy than biogenic barium (BioBa). This issue was also raised by reviewer 2 and we have revised the text accordingly. **We now provide a more balanced summary of the BioBa record in Section 4.2 and, in line with Jaccard et al. (2009), use BioBa to examine possible changes in organic carbon export in Section 4.2.2. In line with the suggestions of reviewer 2, however, we continue to focus most of our discussion on using the opal record as a proxy of siliceous productivity since this the diatom/siliceous fraction forms the basis of the interpretations made in the manuscript.**

3. Can we assume that the average value of seawater $\delta^{30}\text{Si}$ remained constant during the time interval considered in the study? **We have added some text explaining that we are assuming that modern day values are valid for the past. Whilst the $\delta^{30}\text{Si}$ of silicic acid may have changed over time, it is likely that such changes were small and do not significantly alter our interpretations. This is in light of evidence that the $\delta^{30}\text{Si}$ of silicic acid in the ocean (outside of seasonal biological fluxes) is fairly resilient to change over the timescales relevant to this study (De La Rocha and Bickle 2005 Marine Geology 271: 267-282), except in extreme circumstances linked to major reductions in riverine silicon flux.**
4. The absence of an increase of bioavailable iron supply in WSP can't be judged only from dust proxy. **We acknowledge that it is risky making inferences about the role of iron purely by looking at dust. At the end of this paragraph we have added a statement that due to the lack of records that will give us the necessary information we are unable to account for possible changing fluxes of bioavailable iron from the Okhotsk Sea, winter mixing and other sources already identified earlier in the same paragraph. Similar statements have been added elsewhere in the manuscript.**
5. A weakening of the thermocline must be important for the supply of nutrients and

carbon. **Although the halocline is the most important control, changes in the thermocline may indeed be relevant. However, at this time there is no SST record for the region preventing us from speculating about this. We have added words to this effect at the end of the paragraph.**

Reviewer 2 (responses in bold)

We have made all the changes and suggestions made by the reviewer with the exception of the following points/issues that we wish to highlight below. The comments about the biogenic barium record are covered in our response to Reviewer 1 above.

1. I am not sure I understand how Si(OH)_4 consumption can be above 100% (Fig. 3C). **This is already explained in the manuscript, but we have added to the figure caption to make it clear that changes in consumption/supply are relative to mean conditions in MIS 5e.**
2. Max et al., 2014 (Geology), which the authors may have been unaware of at the time of submission, show clear episodes of cooling (and sea-ice advance) concomitant with the short-term oscillations observed in Greenland ice-cores, in a sediment core from the Bering Sea. While one can certainly not directly compare the two North Pacific records at face value, would it be still be possible to better constrain a maximum upper limit for the volume of freshwater needed the explain the diatom- $(\delta^{18}\text{O})$ excursions? **Existing work has shown that the Bering Sea and subarctic North Pacific Ocean display very different response/changes over the same time interval. Comparisons in this instances are further limited by the very different environments between the site of Max et al (sea-ice margin) and ODP Site 882 [this study] (significantly beyond the sea-ice margin). Max et al. does not provide us with an indication of what the**

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freshwater isotope member for ODP Site 882 might be.... whilst we could “pick” a range of possible end-members to try and establish the volume of freshwater needed to explain the ($\delta^{18}\text{O}_{\text{diatom}}$) excursions, this would be highly speculative and likely of little value given the guesswork that would be involved.

3. The argumentation in this paragraph [Section 4.3] is too speculative and only randomly supported by the available data. I would suggest to remove it altogether as it does not provide critical new insights to the manuscript. **We are unsure what part of Section 4.3 the reviewer finds speculative. Paragraph 1 outlines what the results show in relation to existing work. Paragraph 2 does much the same. We can only assume the reviewer is concerned by our focus on changes that are much smaller than those discussed earlier in the manuscript. We believe that we have been cautious in interpreting these changes to avoid over-interpretation and do not feel that this section is too speculative.**

Additional editor comments (responses in bold)

The text within the green and red shading in Figure 3 does not print clearly. **We have altered the figure accordingly and thank the editor for pointing out this issues.**

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