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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

Anonymous Referee #1

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The authors present new diatom isotope records for reconstructing the paleoceanography of the western subarctic Pacific (WSP) and try to elucidate the changes of photic zone conditions there from MIS 4-5e. This study is important in that the information about silicate and freshwater cycles in the upper ocean of WSP during the time interval is added to what have been obtained by other proxies until now. The paper is well written and the arguments made are well thought out. I recommend publication with minor revision.

My complaint about the paper is that they treat biogenic opal as a better paleoproductivity proxy than biogenic barium (BioBa), and advance their discussion based on their belief. The paper argues that opal peaks during 105-86 ka BP while BioBa doesn't. However, as far as I know, almost all the data of BioBa obtained in several locations

of WSP show the largest peaks during the MIS5e through the MIS 5, while some data of opal represent the maxima during the MIS 5c and the other data do that during the MIS 5e (Brunelle et al., 2010, Jaccard et al, 2005, Narita et al., 2002, Shigemitsu et al., 2007). The spatial variability of opal records during the MIS 5 in WSP is also noticed by them (Lines 4-10 on page 3643). Although several researches point out the dissolution problem of BioBa in anoxic sediments and report the lack of a correlation between BioBa and export production, it seems like BioBa records have more consistency across locations compared to opal (which I think has the dissolution problem in sediments) at least in WSP. Thus, I would like them to interpret their data for the case BioBa is a more credible paleoproductivity proxy than opal in addition to the present discussion.

Specific comments: (1) Lines 9-11 on page 3638: The reason why variations in pCO2(aq) or d13C-DIC have negligible/minimal impacts on d13C-diatom is not clearly explained in the present manuscript. I think they should write the reason here clearly.

(2) Lines 20-24 on page 3638: As mentioned above, the discussion here about the comparison of BioBa and opal should be reconsidered.

(3) When interpreting the data of d30Si-diatom in the study, can we assume that the average value of seawater d30Si remained constant during the time interval considered in the study?

(4) Lines 13-15 on page 3639: The absence of an increase of bioavailable iron supply in WSP can't be judged only from dust proxy because the bioavailable iron supply in this region will be maintained mainly by other sources. Among the sources, the sediments in the Okhotsk Sea and the winter vertical mixing of surface/sub-surface water seem to be more important (Misumi et al., 2011, and Nishioka et al., 2013). Therefore, they should reconsider this sentence.

(5) Lines 10-12 on page 3641: This sentence should also be reconsidered by the above reason.

(6) Lines 27 on page 3641 to 1-3 on page 3642: A weakening of the halocline must be important for the supply of nutrients and carbon. Along with the change, the thermocline might also be changed during the time interval considered here and the change might influence the supply. I would like them to discuss that.

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