

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 #2

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Swann and Snelling present an interesting piece of work addressing the history of the northwest subarctic Pacific biological pump and associated surface ocean nutrient delivery based on the (painstaking) analysis of diatom isotopic ratio of oxygen, carbon and silicon during the time interval MIS5-4. Their results directly corroborate inferences made previously based solely on export production proxies. Interestingly, Swann and Snelling highlight two different biogeochemical regimes characterized by distinct nutrient dynamics.

First, the interval centered on stage 5e is characterized by high export production rates and low relative nutrient uptake, suggesting that the subarctic Pacific may have acted as a source of CO₂ to the atmosphere early into the deglaciation. Second, the time period centered on MIS 5b-c shows high sedimentary opal concentrations coincident

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with generally higher isotopic values possibly indicating a reduced transfer of previously sequestered carbon to the atmosphere. Subsequently, the transition into MIS4 - marking the last glacial inception - suggests a permanent change in nutrient limitation.

The study also highlights the influence of enhanced freshwater supply in modulating the strength of the halocline, which ultimately controls the supply of nutrients to the photic zone. Unfortunately, the data presented in this study does not permit to give additional constraints on the geographical source of freshwater to the open subarctic Pacific. This aspect should certainly be a major focus for further studies.

In essence, I support publication of the manuscript presented here, provided the authors can address the rather short list of comments highlighted below.

p. 2, l. 22 - Technically it is the conjunction of high precipitation and low evaporation rate that maintains the halocline. Same remark applies to the statement p. 13, l. 21.

p. 3, l. 6 – the onset of major NHG reduced the annual mean SSTs. The precision is important since alkenone-derived SST suggest increased summer-fall SSTs after the establishment of the permanent halocline (Haug et al., 05).

p. 3, l. 8 - I'm not sure the upwelling of abyssal water ceased completely at 2.73 Myr. I think it is probably safer to say that it strongly decreased.

p. 8, l. 25 - Barite (BaSO₄) crystals should be reasonably well preserved in ODP882 sediments since sulfate reduction has not been observed in interstitial pore waters. In addition, the absence of authigenic Mo enrichments, only taking place under sulfidic conditions, suggest that barite dissolution was very limited if not totally absent (Jaccard et al., 09). I feel the manuscript should in any case focus on the opal export production record, since the new isotopic data presented here relate to the opal fraction only.

§4.2.1 – I'm not sure I understand how Si(OH)₄ consumption can be above 100% (Fig. 3C).

p. 13, l. 7 - The discrepancies probably also highlight poor stratigraphic control outside

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the deglacial periods, when CaCO₃ is more abundant.

p. 14, l. 5 – Most of the freshwater flux would be concentrated early in the deglaciation when the rate of warming was highest, thereby destabilizing the (small) ice-sheets in the NW Pacific realm. This is further corroborated by high rates of sea-level rise being front-loaded in the deglaciation.

§4.2.3 – 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 to explain the diatom- $\delta^{18}\text{O}$ excursions?

§4.3 – The argumentation in this paragraph 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.

Fig. 4 – The schematic illustration depicted in Fig. 4 is certainly useful to illustrate the argumentation. It is however somewhat confusing as it suggests it represents some sort of basinal transect. I would “straighten” the oblique lateral segments so that the reader does not confuse it with an ocean basin.

Interactive comment on Clim. Past Discuss., 10, 3631, 2014.