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Climate of the Past Discussions

Interactive comment on "Millennial-scale variability of marine productivity and terrigenous matter supply in the western Bering Sea over the past 180 kyr" *by* J.-R. Riethdorf et al.

Anonymous Referee #2

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Riethdorf and colleagues present one of the first high-resolution, well-resolved paleoceanographic study from the Bering Sea covering the full last glacial cycle. The stratigraphy is well resolved and robust, which is a major outcome given the relative lack of well-preserved carbonate microfossils in North Pacific sediments. The records clearly show climate-related oscillations in export production and the delivery of ice-rafted terrigenous material, which are, as expected, anti-phased.

Warm intervals are characterized by generally higher sedimentary concentrations of biogenic material, while cold periods, likely associated with increased sea-ice cover, are characterized by almost pure terrigenous material. The authors also highlight a strong meridional gradient, with higher export production towards the south, suggesting a strong control of temperature and/or sea-ice on surface ocean fertility.

Ultra-high resolution XRF scanning results reveal short-lived, sub-millennial events reminiscent of D/O events recorded in Greenland.

This manuscript is richly documented and will undoubtedly be a very useful contribution to our understanding of the paleoceanographic evolution of the North Pacific realm. I support final publication of this study, provided the authors can address the relatively minor comments listed below.

Major comments – - p. 6162-6163. There is a general confusion about subsurface ventilation across the last glacial termination. There are indeed many signs of increased ventilation in the subarctic Pacific during HS1. However, the records highlighting enhanced ventilation are restricted to the upper 2km of the water column (e.g. Ahagon et al., 03; Okazaki et al., 10; Okazaki et al., 12), possibly associated with increased intermediate water formation related to sea-ice dynamics in the Bering Sea. Deeper records show no signs of increased ventilation until the onset of the B/A (e.g. Galbraith et al., 07; Jaccard & Galbraith, 13; Lund et al., 12). I would urge the authors to clearly distinguish between intermediate vs deep ventilation to avoid any confusion. - The sediments from the North Pacific in general and the Bering Sea in particular are primarily composed of two main components, biogenic opal and detritic material supplied by melting icebergs. As a result, the increase of one of the components will decrease the sedimentary concentration of the secondary component by dilution. Total sediment mass accumulation rates seem to be driven by changes in sedimentation rate, which in turn seem to be largely determined by changes in terrigenous matter supply. Is there any way to figure out which component is driving the sedimentary dilution? It would be useful to have a plot showing total sediment MAR, biogenic MAR and detritic MAR for each of the core.

Minor comments –

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- p. 6137, l. 7: the variable that is recorded in the sediment is export production (and not primary production) - p. 6137, l. 8: "... during intervals of marine isotope stage 5..." - p. p.6138, l.11-13. This sentence is misleading. A less efficient biologically driven drawdown of organic matter corresponds to a more efficient biological pump. Please replace drawdown of organic matter by export of organic carbon. - p. 6139, l. 3-4. The authors have overlooked the study by Shigemitsu et al., 07, Marine Chemistry. The study presents a thorough provenance study based on sedimentary trace metal ratios based on a sedimentary archive from the open western subarctic Pacific. In addition, I wouldn't say that these studies are rare. There are plenty of examples of sedimentological studies addressing past terrigenous matter supply, for example in the Atlantic. There are indeed possibly more sparse in high-latitude regions, although some studies have been overlooked (e.g. VanLaningham et al., 09, EPSL). - p. 6139, l. 20. Replace according by "...reconstructions are thus restricted..." - p. 6129, l. 21. Replace morphological by bathymetric or topographic. - p. 6141, l. 2. It relies to disintegration? - p. 6145, l. 7-8. This sentence is misleading. Accumulation rates are VERY prone to biases due to sediment redistribution/winnowing (see Francois et al., 04, Palaoceanography for a review). - p. 6145, l. 20-21. While I agree that sedimentary CaCO3 concentrations are primarily driven by changes in calcite saturation, one cannot exclude that enhanced CaCO3 production also contributes to the observed patterns. - p. 6145, l. 28-29. Barite crystal formation has been observed in lab experiments (e.g. Ganeshram et al., 03, GCA). - p. 6147, l. 6-8. The basic assumption underlying the use of Babio to infer Pnew is that Babio was not affected by changes in preservation. Babio has been shown to dissolve when sulfate starts to be reduced in porewaters. Are there indications allowing to exclude changes in preservation as the dominant factor controlling the downcore pattern of Babio (i.e. absence of authigenic Mo enrichments, porewater SO4 measurements)? - p. 6149, l. 1. Sources of terrigenous matter would be determined more accurately using trace elements (REE for examples – see Shigemitsu et al., 07). Was this suite of elements quantified via discrete XRF measurements? - p. 6149, l. 8-9. Does this pattern match satellite-derived

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export production estimates? - p. 6155, l. 19-20. I reiterate my above-mentioned comment. There would probably be more to learn considering trace metals showing more variability within endmembers compared to major elements, which show relative homogeneity (as stated in the text). - p. 6159, l. 6. Export rather than primary productivity. - p. 6159, l. 27. more efficient nutrient utilization - p. 6160, l. 3. Suppressed vertical mixing would have decreased export production to an absolute minimum. Vertical mixing was likely strongly reduced, but not suppressed. - p. 6160, l. 5-7. This is subject to controversy. Hayes et al., 11, Paleoceanography and Bradtmiller et al., 09, EPSL for example found no evidence of enhanced glacial export production in the equatorial Pacific. - p. 6160, l. 9. The detritic Fe is likely not bioavailable. Most of the bioavailable Fe observed in the subarctic Pacific originates either from dust or from lateral advection from continental margins (Lam & Bishop, 08, GRL). As a result, the authors cannot use their sedimentary Fe concentrations as evidence to support alleviation of Fe-limitation during cold periods. Nonetheless, there is ample evidence that enhanced bioavailable supply of Fe to the subarctic Pacific during ice ages did not affect export production. - p. 6160, l. 26-28. Interesting conjecture! Surface waters flowing into the Arctic through the shallow Bering Strait are fresh surface water masses. The closure of the Strait would have allowed the fresh surface waters to pool in the Bering Sea, thereby reinforcing vertical stratification. - p. 6161, l. 20-23. High TOC concentrations at the onset of HS1 could also be explained by better preservation under poorly-oxygenated conditions at the sediment-water interface. - p. 6162, l. 7-10. Higher nutrient supply during HS1 would indeed have lowered d15N values, but would also have increased export production, which is at odds with the author's own observations as well as with the data from the literature (Kohfeld & Chase, 12, QSR). Also I am privy of unpublished data, that the authors may be unaware of, that show that the low diatom-bound d15N values observed during HS1 in the subarctic Pacific and the Bering Sea may well be related to contamination by low-d15N radiolarian. - p. 6163, l-25-28. This is an interesting observation! Would there be a way to come up with a (crude) estimate on how much meltwater would be required to explain the divergence from the LR04 stack? - p.

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6164, l-21. It is not clear what is meant by allochtonous nutrient supply.

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