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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.

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We are very grateful to have received such positive reviews on our manuscript "Millennial-scale variability of marine productivity and terrigenous matter supply in the western Bering Sea over the past 180 kyr". We thank Luc Beaufort for the handling of this manuscript and for his encouragement to submit a revised version. The detailled comments and suggestions from the two anonymous referees were all constructive and we consider them of great help for the improvement of our manuscript.

In the following we have addressed all issues raised by the referees. Modifications of the original text were applied and are referred to by page and line numbers of the

Track-Changes-Version, which is submitted along with this response.

The main changes of the revised version include:

1. The now more precise discussion contained in chapter 4.

2. The Yukon River as a potential source of the detrital material.

3. Redrawings of figures 3 and 4.

4. Extended evaluation of the applicability in using Babio to estimate PNew, including a new table on surface sediment analyses in Appendix B.

5. Exclusion of the paragraph on iron limitation.

6. Implementation of most minor comments.

Response to referee #1

Major points:

R1.1) Overall impression. The manuscript has redundant descriptions particularly chapter 4. In addition, discussions are often mixed between the author's original data (western Bering Sea) and data by previous studies (Bering Sea other than western part, Okhotsk Sea, and open subarctic Pacific). As the authors mentioned in the manuscript, paleoceanographic changes in the western Bering Sea showed different features from subarctic Pacific records. I would like to request the authors to emphasize their novel findings and suggestions based on their original data by separating previous studies.

A1.1) We updated chapter 4 and tried to overcome the redundant descriptions in hand with setting up a hopefully more precise discussion. Please be referred to the text of the revised manuscript (see lines 371 ff.).

R1.2) p. 6157, Line 7-9. The authors mentioned that riverine source is insignificant for terrigenous input in the western Bering Sea. However, VanLaningham et al. (2009)

indicated that the significance of Yukon River contribution for detrital materials in the Meiji Drift, western subarctic Pacific. Considering counterclockwise ocean circulation in the Bering Sea, I think that it is not easy to rule out the riverine source (i.e. Yukon River) on the ground of distance. See also Asahara et al. (2012) and Nagashima et al. (2012) showing the Yukon River contribution to the northern Bering slopes. Uncommon findings of drop stone in Shirshov Ridge samples suggest that sea-ice is not a main agent for transportation detrital materials?

A1.2) Unfortunately we overlooked the study of VanLaningham et al. (2009, EPSL 277, 64-72), who argued that glacial sediments in the Meiji Drift consist of a large fraction of terrigenous material delivered from Yukon-Bering Sea sources. The potential cause for the enhanced glacial contribution from these sources was ascribed to (i) higher ocean current velocities, or (ii) increased sediment supply from glacial erosion, as well as (iii) additional influence from a change in circulation patterns due to the closure of the Bering Strait. Notably, they mentioned that ice might be a potential sediment source and could complicate sediment transport pathways.

Asahara et al. (2012, DSR2 61-64, 155-171) analyzed the detrital fractions of surface sediment samples from the eastern Bering and Chukchi seas. They concluded that "the detritus in the eastern Bering Sea mainly consists of two components: the continental material from the Yukon River basin mainly underlain by Mesozoic and Paleozoic rocks in the Alaskan mainland ... and the Aleutian-arc volcanics ... transported northward by the Alaska Coastal and Bering Shelf waters". Nagashima et al. (2012, DSR2 61-64, 145-154) also used surface sediment samples from the eastern Bering Sea shelf to study the contribution of Yukon River-derived detrital material. They suggested that "coarse grains deposited on the continental shelf are reworked relict deposits" and that clay- to silt-sized quartz on the NE continental shelf is modern suspended matter from the Yukon River.

None of these studies involved material from the western Bering Sea. However, we meanwhile generated results to be published in a subsequent manuscript indicating

that POM from the Yukon River contributes to the terrestrial-derived organic matter in sediments from Shirshov Ridge (Riethdorf et al., submitted to Deep-Sea Res. Pt. II). Hence, we agree that terrigenous matter derived from sources drained by the Yukon River was deposited on the eastern Bering Sea shelf and that it was subsequently transported to Shirshov Ridge and to the Meiji Drift. The remaining question is whether this terrigenous matter was transported by ocean/bottom currents and/or sea-ice. This question can not be solved by our study, but sea-ice that formed in the eastern and northeastern Bering Sea is likely to have entrained sediments deposited on the shelf and to have carried and released them over Shirshov Ridge, especially the coarse fraction. Overall, this issue demands further investigation and it demonstrates the need for western Bering Sea provenance analyses and records of ice-rafted debris (IRD).

Dropstones can be transported by both, sea-ice and icebergs. Their appearance as well-rounded pebbles indicates a beach deposit origin. We prefer the view that sea-ice mainly transported the fine fractions (silt, clay), but that dropstones were entrained into newly formed ice as well.

We altered our manuscript taking into consideration the information given above (see the abstract, lines 28-31; chapter 1, lines 73-89; section 4.2.3, lines 634-645, 605-614, and 659-668; chapter 5, lines 938-941).

R1.3) p. 6139, line 21. Change term from “Morphological high” to “topographic high”.

A1.3) done (see line 100).

R1.4) p. 6141, line 17. Definition of “deep intermediate water” is required because this watermass is not common in oceanography.

A1.4) Since in oceanography there is in fact no differentiation between shallow and deep intermediate water levels we changed the formulation toward ’intermediate to deep water levels’ (see line 151). Please see also our response to comment #1 of referee #2.

R1.5) p. 6142, line 20: Brief explanation about oxygen isotope offset between Uvigerina peregrina and U. auberiana are useful. Also, plot δ18O values of two Uvigerina species in fig. 3 separately.

A1.5) Uvigerina δ18O values are in equilibrium with seawater (Shackleton and Hall, 1984). U. auberiana was only used in core 77KL, when U. peregrina was not present. We changed the respective sentence (see lines 179-180). We found no indication for a δ18O offset between both species in core 77KL. In Figure 3 each measurement is now indicated by a symbol, with 'plus' symbols for U. peregrina and 'open triangles' for U. auberiana.

R1.6) p. 6143. P-Mag data qualities should be mentioned because previous study indicated notable effect of sediment diagenesis on magnetic properties in the Bering Sea (Takahashi et al., 2011).

A1.6) The following paragraph is now included in section 3.1.3 (see lines 200-211):

Takahashi et al. (2011) reported on the transformation of oxide magnetic minerals into paramagnetic FeS2 in Bering Sea sediments. They associated the formation of FeS2 with sulfate reduction processes under anaerobic methane oxidation. We performed quality control of the magnetic hysteresis parameters (Day-plot, orthogonal projections of AF demagnetization of NRM, et al.) and the thermomagnetic analyses of the magnetic fraction, which also indicate an influence by sediment diagenesis in core 85KL. Specifically, the thermomagnetic analyses suggest the occurrence of paramagnetic FeS2 in the major ferrimagnetic phase from 880 cm to 1760 cm core depth. We assume that the dissolution of very fine-dispersed almost single-domain magnetic particles and the preservation of coarse pseudo-single-domain and multi-domain particles just led to decreasing RPI values within that depth interval. However, this most probably had no effect on the relative variability of the reconstructed geomagnetic field.

R1.7) p.6145, line 1-5. Core stretching effect by Széréméta et al. (2004) is a case of giant piston coring. In the subarctic Pacific and its marginal seas, lack of Holocene

section is mostly found in the northern slope of the Bering Sea and the Emperor Seamounts. On the other hand, Holocene sections composed of diatomaceous ooze were recovered from the eastern and southern Bering Sea and the Okhotsk Sea. It is hard to say that core stretching effect was a main reason for the lack of Holocene section in the studied sites.

A1.7) The absence of Holocene sediments in cores from the Bering Sea is hardly explained in the literature. We did not state that missing Holocene sediments in cores 85KL and 101KL are the result of core stretching. We suggest that it might be due to a change in sedimentation in favor of diatomaceous ooze, which might have been eroded by, e.g., bottom currents. The hint toward oversampling when using giant piston cores is now included (see lines 250-254).

R1.8) p. 6145, line 25. Matul et al. (2002) is a paper of radiolarian biostratigraphy and not appropriate for a reference of opal production.

A1.8) We changed Matul et al. (2002) to Honjo (1990), which is indeed more appropriate (see line 277).

R1.9) p. 6148, line 4-6. Describe a procedure to separate >63 μm and <63 μm fractions.

A1.9) done (see lines 338-340).

R1.10) p. 6152, line 22-24. Explanation how to estimate PNew is needed (Nürnberg (1995) is in German). Because PNew estimation by Nürnberg (1995) is used Atlantic samples, the authors should indicate this method is applicable for the Bering Sea samples. Ecosystem of the subarctic Pacific including Bering Sea is quietly different from the one in the Atlantic.

A1.10) The formula how to calculate PNew is given in the caption of Figure 7. We now provide a cross reference to that figure in sections 3.3.2 and 4.1.3 (see line 314 and 480). The original reference is indeed in German, but the equation is used in subsequent studies (e.g., Pfeifer et al., 2001, Mar. Geol. 177, 13-24; Nürnberg and Tiedemann, 2004). Especially Nürnberg and Tiedemann (2004) used it for their reconstruction of PNew in the Okhotsk Sea. The ecosystem of the Bering Sea is more comparable to that of the Okhotsk Sea than to that of the Atlantic.

The following considerations are now implemented and some statements were reconsidered (see section 4.1.3, mainly lines 452-455, 480-490, and 505-507):

Springer et al. (1996) provided a map (their figure 2) showing the generalized pattern of primary production (PP) in the Bering Sea. According to that map, our sites lie in the 'oceanic domain' of the Bering Sea, which is characterized by a PP range of 50-100 gC m-2 yr-1 (average of 61 gC m-2 yr-1). During SO201-2 we recovered surface sediment samples in the western Bering Sea in the vicinity of our coring sites using a MultiCorer. Some of these samples were analyzed according to the same procedures described in the manuscript, but the data are unpublished and were not included in the manuscript. Along the north-south transect the conducted XRF bulk analyses showed Babio values in between ~180 ppm (north) to ~1320 ppm (south) (see additional Table B1 in Appendix B). Due to the partly missing Holocene sediments we can not estimate average Holocene ARBulk values for sites 85KL and 101KL. However, at Site 77KL the average ARBulk value is ~4 g cm-2 kyr-1 for the Holocene section, which is similar to that reconstructed for sites 85KL and 101KL during MIS 5.5. Hence, assuming an average Holocene ARBulk value of ~4 g cm-2 kyr-2 at all sites results in PNew estimates ranging between ~3 and 54 gC m-2 yr-1 which translates into a PP-range of ~35-145 gC m-2 yr-1 (see Table B1 in Appendix B). Despite the uncertainties regarding the Holocene ARBulk values (which are likely to be higher toward the northern sites) and in the calculation of PNew , this range is comparable to the modern estimate given by Springer et al. (1996), which would argue in favor of the applicability of our approach.

Referee #2 remarked that using Babio to infer PNew requires that Babio was not affected by changes in preservation that can occur as a result of sulfate reduction and

associated barite dissolution. Unfortunately, we did not perform porewater sulfate measurements to clearly exclude an impact of this process on the Babio records. However, although we can not fully exclude preservation effects, we consider changes in Babio concentrations to mainly reflect variations in Babio accumulation (i.e., export of organic matter) because records of biogenic opal, TOC, and Babio all show a similar temporal evolution with low glacial and increased interglacial concentrations (see Figure R1). This is in agreement with previous studies conducted in the Bering Sea (e.g., Nakatsuka et al., 1995; Brunelle et al., 2007), Okhotsk Sea (e.g., Sato et al., 2002; Narita et al., 2002; Nürnberg and Tiedemann, 2004), and the subarctic NW Pacific (e.g., Kienast et al., 2004).

Certrain trace elements can be used as paleoredox proxies. For example, Mo and U are reported to be sensitive to anoxic conditions, while Cr and Mn reflect suboxic and oxic conditions, respectively (e.g., Calvert and Pedersen, 1993, Mar. Geol. 113, 67-88; Yarincik et al., 2000, Paleoceanography 15, 2, 210-228; Tribovillard et al., 2006, Chem. Geol. 232, 12-32). Mn is available for our cores via XRF core scanning, although count rates of Mn are very low. The records of Mn count rates of all our studied sediment cores consistently show decreased values during the warm intervals (MIS 5.5, 5.3, 5.1, 1), when export production was high (see Figure R1). Assuming that changes in XRF count rates of Mn actually reflect relative variations in porewater dissolved oxygen concentrations this would indicate that barite dissolution due to sulfate reduction was strongest during the warm intervals, which is not in agreement with the higher Babio concentrations and therefore argues against a significant preservation effect. Of course concentration data of Mn (Mo, Cr, and U) would be needed for a more thorough evaluation of this issue.

R1.11) p. 6162, line 20-21. Peak of >63 μm in B/A is suggested to be caused by sudden release of IRD from melting sea-ice. If so, this region was perennial sea-ice covered during H1. It appears that coastal radiolarian species R. boreale and brackish diatoms in B/A section suggest a presence of low salinity surface water, which helps sea-ice formation in winter.

A1.11) This comment is gratefully acknowledged. Our complementing studies (Max et al., 2012; Riethdorf et al., 2013) indicate the presence of sea-ice and weak thermal mixed layer stratification during HS1, but absent or at least significantly reduced sea-ice influence and enhanced thermal mixed layer stratification during the subsequent B/A. It is correct that low salinity surface water would help sea-ice formation in winter. However, our mixed layer salinity reconstruction (Riethdorf et al., 2013) suggests regional differences during Termination I with more sea-ice influence (brine rejection) toward the northern sites. Higher abundances of R. boreale and P. sulcata indicate fresher surface waters during the warm seasons and argue for a shortened sea-ice season during the B/A. Our results, in addition, argue for a reduced intensity of sea-ice rafting during the B/A with respect to HS1. From our results we can not argue if sea-ice was perennially present during HS1 and not anymore during the B/A, but maintained export production (although significantly reduced) and the presence of IP25 suggests, that sea-ice was not perennial but seasonal during HS1.

R1.12) p. 6163, line 19-25. Extremely high coarse fraction during MIS5e is very interesting finding. Nürnberg et al. (2011) did not suggest perennial sea-ice during MIS5e in the Okhotsk Sea. Is this simply explained by enhanced seasonal sea-ice during MIS 5e in the western Bering Sea?

A1.12) We did not suggest perennial sea-ice during MIS 5.5. For late MIS 6 Nürnberg et al. (2011) proposed "a seasonal to perennial, but still mobile ice cover (high IRD supply) in the eastern Okhotsk Sea dominated by drifting icebergs and related particle supply from Kamchatka ... and to a rather seasonal ice coverage in the western sea supplying less IRD". They further stated that this "MIS 6 ice scenario is supported by Wang and Wang (2008, Terr. Atmos. Ocean. Sci. 19, 4, 403-411), who identified an almost entirely ice-covered central Okhotsk Sea during that period based on rare occurrences of diatoms and increasing sea-ice assemblages".

Respective studies are missing for the Bering Sea for MIS 6. We suggest that at our sites sea-ice was perennial during the final phase of MIS 6 and that its sudden melt during Termination II resulted in the recorded coarse fraction maxima (see lines 707-712, 853-856, and 858-859). This would be supported by concurrent maxima in LSR (ARBulk), which are not observed. Notably, the coarse fraction maxima occurred ~3 kyr before the maxima in TOC and biogenic opal. However, this issue might remain equivocal at the moment due to constraints in our age models. Yet, at the moment we see no possibility for a respective improvement of our age models for that timeframe. Records of ice-rafted debris (IRD) would be of great help to further decipher the nature of the coarse fraction maxima.

For clarification: terrigenous matter supply via sea-ice (as well as LSR and ARBulk) seems to have been stronger during the cold intervals (MIS 6, 5.4, 5.2, 4-2) when seasonal contrasts were most probably weaker. Supposedly stronger seasonal contrasts during the warm intervals (MIS 5.5, 5.3, 5.1, 1) resulted in enhanced export production and reduced terrigenous input, most probably because the sea-ice season was significantly shortened and sea-ice rafting was less intense.

Response to referee #2

Major comments

R2.1) 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.

A2.1) These comments are gratefully acknowledged. We now provide more precise statements regarding the ventilation changes (see lines 796-809).

Our initial confusion was related to oceanographic terminology. For the modern situation Talley (2003, J. Phys. Oceanogr. 33, 530-560) defined an 'intermediate depth layer' in the North Pacific in 500-2000 m, but at 24°N Pacific Deep Water (PDW1) is already present from ~1000 to 2500 m (Talley, 2008, Prog. Oceanogr. 78, 257-303). At 50°N in the subarctic North Pacific the core of PDW, which is characterized by low dissolved oxygen concentrations and a potential density ($\sigma\Theta$) surface of 27.8, can be found at ~1700 m (Talley, 2013, Oceanography 26, 1, 80-97). Enhanced ventilation in the subarctic Pacific during HS1 above 2000 m is related to the presence of so-called glacial North Pacific Intermediate Water (GNPIW). Okazaki et al. (2012) already noted that "a water mass extending to 2000 m should not be called an intermediate-water in the sense of physical oceanography (Matsumoto et al., 2002)" which is why they used "the term “GNPIW” to refer to the well-ventilated water mass following Matsumoto et al. (2002)". This is in agreement with Jaccard and Galbraith (2013) who stated that "it seems more appropriate to refer to ventilation that reaches such depths an “expanded North Pacific intermediate water,” following Keigwin (1998) and Matsumoto et al . (2002)".

Accordingly, we recommend to always include a depth range when discussing the extent of (deglacial) ventilation changes.

R2.2) 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.

A2.2) This is correct. We followed the respective suggestions and calculated accumulation rates of siliciclastics (ARSiliciclastics) and biogenic components (sum of TOC, opal, CaCO3; ARBiogenic), which are now included in Figure 4. From this figure it is obvious that changes in ARBulk (LSR) are indeed largely determined from changes in ARSiliciclastics (terrigenous matter supply) and that the sedimentary dilution comes from changes in the concentrations of the biogenic components. These thoughts are now implemented in the text (see section 3.4.1, lines 351-353; section 4.2.1, lines 520-529; chapter 5, lines 932-934). It needs to be stressed, however, that changes in ARBiogenic are strongly affected by the LSR variability due to the low concentrations of the biogenic components. Hence ARBiogenic might be partly overestimated. Moreover, changes in element ratios, like K/Ti, should not be affected by sedimentary dilution. Our records of K/Ti (assumed to reflect changes in the geochemical source) all show a similar evolution resembling that of %Terrigen, which still argues for relatively enhanced terrigenous input during the cold intervals and its reduction during the warm intervals.

One more note: Whether the detritic material in Bering Sea sediments is supplied by sea-ice, icebergs or from ocean currents is under debate. We favor the view that sea-ice and not icebergs acted as the dominant transport agent for sediments recovered from our sites as discussed in section 4.2.3. Please see also our response to comment #2 of referee #1.

Minor comments

R2.3) p. 6137, l. 7: the variable that is recorded in the sediment is export production (and not primary production)

A2.3) changed (see line 25). We applied respective changes in other parts of the text, when necessary.

R2.4) p. 6137, l. 8: ": : : during intervals of marine isotope stage 5: : :"

A2.4) changed (see lines 26-27).

R2.5) 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.

A2.5) done (see line 58-59).

R2.6) 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).

A2.6) Unfortunately we also overlooked the recent studies of Asahara et al. (2012) and Nagashima et al. (2012) as indicated by referee #1. We included these studies and the other comments in the respective paragraph of the revised version of the introduction (see lines 73-89).

R2.7) p. 6139, l. 20. Replace according by ": : :reconstructions are thus restricted: : :"

A2.7) done (see line 99).

R2.8) p. 6129, l. 21. Replace morphological by bathymetric or topographic.

A2.8) done (see line 100).

R2.9) p. 6141, l. 2. It relies to disintegration?

A2.9) Changed for a hopefully clearer expression (see lines 133-136).

R2.10) 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).

A2.10) We deleted the sentence and rewrote the paragraph (see lines 259-263).

R2.11) p. 6145, l. 20-21. While I agree that sedimentary $CaCO_3$ concentrations are primarily driven by changes in calcite saturation, one cannot exclude that enhanced $CaCO_3$ production also contributes to the observed patterns.

A2.11) A respective remark is now included (see lines 272-275 and 425-426).

R2.12) p. 6145, l. 28-29. Barite crystal formation has been observed in lab experiments (e.g. Ganeshram et al., 03, GCA).

A2.12) The respective study is now referenced (see lines 282-284).

R2.13) 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 SO_4 measurements)?

A2.13) Please be referred to our response to comment #10 of referee #1. We did not perform pore water analyses or ICP-MS-based analyses to determine trace element concentrations.

R2.14) 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?

A2.14) Our suite of elements were indeed quantified via discrete XRF measurements of the bulk sediment samples as described in section 3.3.2. We now include respective remarks (see lines 304-308 and 364-365).

R2.15) p. 6149, l. 8-9. Does this pattern match satellite-derived export production estimates?

A2.15) Actually, this pattern does not match satellite-derived estimates of PP and/or export production. SeaWiFS data (Mizobata and Saitoh, 2004, J. Marine Syst. 50, 101-111; Ishizaka and Kameda, submitted), model results (Nakata and Doi, 2006, Environ. Modell. Softw. 21, 204-228) and results of Springer et al. (1996) show the exact opposite pattern. However, our records of ARBiogenic (Figure 4) do not show a similar gradient as observed for the concentrations. Instead, the ranges of ARBiogenic are comparable and values are low at all sites. Hence, the gradient in concentrations might just be the result of decreasing LSR toward site 77KL. One explanation for the low values of ARBiogenic (and PP) might be that our sites are located too far south of the modern high-productivity band of the BSC and that the extent of export production was more or less similar at our sites (in the 'oceanic domain' of Springer et al., 1996).

Accordingly, we changed the respective paragraph (see lines 374-376).

R2.16) 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 end-members compared to major elements, which show relative homogeneity (as stated in the text).

A2.16) Our discrete XRF analyses are not accurate enough for determining trace element concentrations. We strongly encourage subsequent studies to conduct respective ICP-MS-based investigations to validate our results and interpretations.

R2.17) p. 6159, l. 6. Export rather than primary productivity.

A2.17) changed (see line 703).

R2.18) p. 6159, l. 27. more efficient nutrient utilization.

A2.18) changed to 'more efficient nitrate utilization' (see line 727).

R2.19) 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.

A2.19) changed to 'enhanced stratification' (see line 730).

R2.20) 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.

A2.20) We deleted this paragraph since our sites do actually not lie in the HNLC region of the Bering Sea and due to comment #21.

R2.21) 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.

A2.21) This is correct. Accordingly we deleted the respective paragraph.

R2.22) 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.

A2.22) Reassessing previous studies revealed that this was already suggested by Sancetta (1983), which is now referenced to (see lines 754-756).

R2.23) 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.

A2.23) Acknowledged and included (see lines 780-781).

R2.24) p. 6162, l. 7-10. Higher nutrient supply during HS1 would indeed have lowered δ15N 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 δ15N values observed during HS1 in the subarctic Pacific and the Bering Sea may well be related to contamination by low-δ15N radiolarian.

A2.24) This is correct and acknowledged (see lines 793-795). However, we do not wish to go into too much detail here, since reconstructions of nitrate utilization (stratification) are not the primary focus of our study. Most records from the subarctic N Pacific argue for better ventilation above $\sim$2000 m depth during HS1, which is likely to explain the decrease in δ15N when export production remained more or less at the same level as during the preceding LGM. We favor the view that marine productivity was still restricted during HS1 due to temperature limitation and potentially also due to light limitation, which resulted from an expanded sea-ice coverage. This is at least supported by our complementing study of Max et al. (2012) who found IP25 in sediments during HS1 but not during the subsequent B/A.

R2.25) 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?

A2.25) In our opinion this would be too speculative, especially when considering constraints in our age models, which could also explain this offset to LR04. We have only few age control points during MIS 6 and during Termination II, and at the moment we see no way how to improve our age models during that timeframe. A respective remark is now included (see lines 858-859).

R2.26) p. 6164, l-21. It is not clear what is meant by allochtonous nutrient supply.

A2.26) We meant the supply of nitrate from the subsuface nitrate pool into the euphotic zone. The sentence was accordingly changed (see lines 879-881).

Yours sincerely,

Jan-R. Riethdorf

Please also note the supplement to this comment:
http://www.clim-past-discuss.net/8/C3694/2013/cpd-8-C3694-2013-supplement.pdf

Interactive comment on Clim. Past Discuss., 8, 6135, 2012.

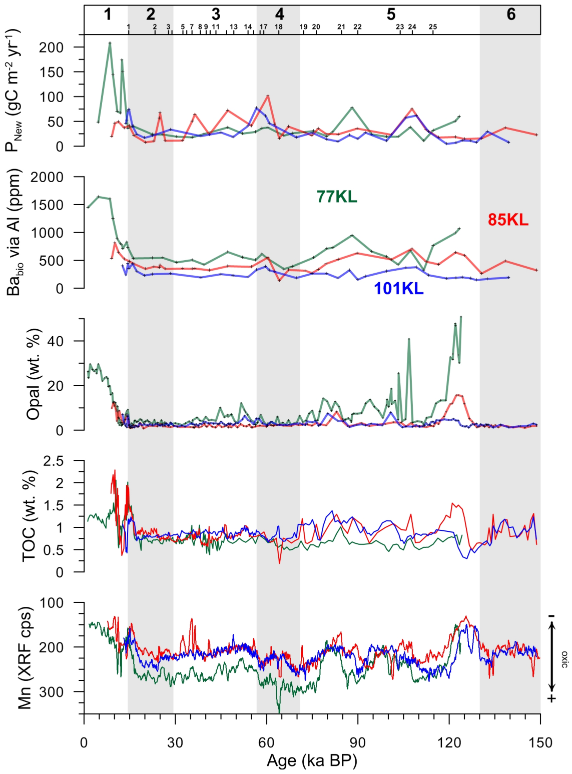

Fig. 1. Figure R1: Records of XRF-based Mn count rates, TOC, biogenic opal, biogenic barium (Babio), and new production (PNew) for sediment cores SO201-2-77KL (green), -85KL (red), and -101KL (blue).