With thanks we received the helpful and detailed comments of anonymous reviewer #2. These helped to significantly improve the quality of the manuscript. The main comments by referee #2 concerns the proposed temperature threshold and the Holocene lamination pattern. The original comments of the reviewer #2 are given in normal letters and our answers in bold/italic.

Major comments: Deglacial SST threshold hypothesis (Section 4.4)

(1) I do not find the lamination-temperature threshold hypothesis convincing. The authors point out that the onset of the Bølling laminations are abrupt, with no sign of layered sediment at that transition. However, the SST increase around this time is gradual and reaches 5.5◦C, not the 6-7◦C threshold stated in P. 2484, Line 16, and Line 19 of the Abstract. In fact, during the Bølling, where the laminations are most well developed, the SST data are virtually all lower than the threshold. And during the "T1-BLU4" and "T1-BLU3" periods, which contain intermittent layered facies, appear to be times of the highest reconstructed SST. Referring to this latter point, the authors later appear to contradict their claim of a SST threshold (p. 2487, Line 18-20), saying that the SST-oxygenation relationship is not a simple linear one.

We acknowledge the concerns of reviewer #2 and rephrased the sections about the temperature threshold and present the concept instead as a suggestion or potential mechanism, which admittedly, cannot be proven with the currently available, significantly lower-resolved SST data based on alkenone Uk37 data from Max et al. (2012) from the Bering Sea region. In addition we changed Fig. 10 in a way, that we only plot SST data from the Shirshov Ridge (Max et al., 2012). These cores have a similar resolution of data points. Further we focus the discussion more on the trends in the SST's. To illustrate potential relative temperature changes we have added calculated mean SST values for each of the laminated and non-laminated time intervals to Fig. 10 based on the Max et al. stacked record, and show average temperature differences of about 1 °C between laminated and non-laminated intervals. We have clarified in the revised version that the suggestion of a SST-threshold-like pattern in the deglacial Bering Sea ultimately needs to be validated by SST-proxydata in much higher temporal and spatial resolution, including data directly form the core site. To this effect, a study is underway in our group that will try to establish (sub)decadal-scale SST records based on site 18-3-6 and other records.

I suggest the authors include the Preboreal time period in Fig 10 (they refer to it as a "warm" time in P. 2484, Line 16, are the SST reconstructions warm, or are they referring to the Greenland temperatures?). I would argue that the Bølling-Allerød appeared to be a time of generally higher SSTs (and generally laminated sediments), but that there is not a clear relationship between SST amplitude and centennial-scale changes in lamination intensity.

Concerning your remark to P. 2484, Line 16: We here refer to the sea surface temperatures, which show a warming in the Preboreal (Fig. 5 in Max et al., 2012). In Fig. 10 of our manuscript we focus on the Bølling-Allerød. This time interval reveals short term changes imprinted in the laminated/layered sediments. Further, most sediment cores from the western Bering Sea used for the establishment of the stacked SST record lack, unfortunately, sufficiently resolved data from the early Holocene (Max et al., 2012). As stated in our previous reply paragraph we think that a relationship between SST trends and lamination occurrence can be generally inferred from Fig. 10 and prefer to keep the updated version of Figure 10 with additional information in the revised manuscript. However, we have modified language in the according discussion paragraph to allude to other factors that can be substantial modifiers to a potential close SST-lamination relationship. Together with the modifications outlined before (see previous reply paragraph) we trust to have modified our line of reasoning in a revised manuscript sufficiently to reflect reviewer #2's concerns about the SST – OMZ relationship on shorter timescales.

(2) P. 2484, Line 19: The authors claim there was a change in "sea ice cover" that shortened the blooming season, but cite no sea ice proxy record to support this claim, or a paper that demonstrates the stated link between sea ice and export productivity. This is the crux of the proposed SST-lamination link, and I think the authors need to support this claim with something! For example, with a figure with supporting data. On P. 2490, Line 26-27, the authors mention smear slides and sea ice diatoms, but show no data—tantalizing and unsatisfying. I am very curious to see some data, since the deglacial diatom assemblage data from the Umnak region (Caissie et al., 2010) may not be representative of the study area, because the slope current may have maintained open water there even in high sea ice times.

Papers with IP25 data, sea surface temperatures and numerous detailed diatom counts including cores SO202-18 are finalized by other lead authors / working groups. Pictures of smear slides and counting results of these slides were included in the revised supplement.

In these slides we see high amounts of* Fragilariopsis cylindrus *(more than 40 %) which is known as a sea ice related diatom in the northern North Pacific today (von Quillfeldt, 2000; Ren et al., 2014).

(3) To me, a big problem with attributing the pattern in productivity changes at this study area to sea ice is that this pattern is seen across the subarctic Pacific, and is not limited to areas with seasonal sea ice (Kohfeld and Chase, 2011). The authors may want to refer to Lam et al (2013, Nature Geosciences, doi: 10.1038/ngeo1873).

We thank the reviewer for this comment and will consider the article of Lam et al (2013) in our discussion. The influence of a succession of deep convection and subsequent meltwater stratification on North Pacific productivity is indeed an important process. Thus, we have added in the revised discussion a paragraph that details the potential influence of a sea ice or meltwater-induced stratification on our observed productivity and lamination record.

(4) The authors carefully show in Fig 6 that the sediment fabric and XRF Ca abundance can be correlated between their study area and SO201-2-114. Therefore, I think it would be reasonable to transfer the age model from SO202-18-3/6 to the SST record from SO201-2-114.

The age model by Max et al (2012) is based on XRF data correlation and color*b correlation to NGRIP stable isotope data. Color*b is a suitable proxy biogenic opal variations and total organic matter of anoxic sediment (Nuernberg & Tiedemann, 2004; Debret et al., 2006; Max et al., 2012). This means that the principles of the establishment of the age model are comparable to our approach as we counted laminae from diatomaceous sediments. The close correlation between high color*b values and lamination occurrence was also shown for nearby core PC23A for the last deglaciation (Kim et al., 2011). We thus think it is suitable to use the age model of Max et al. (2012) which have not such a high resolution compared to our sediment cores, but targeted the onset and termination of the laminated facies in the respective core by AMS 14C dates (Max, pers. communication), comparable to our initial approach of bracketing laminated intervals firstly with 14C dates, then proceeding to the refined age model with laminae counts and analyses.

<u>Holocene SST-lamination pattern (Section 4.5.1)</u>

The authors describe that the deglacial relationship between oxygenation and temperature breaks down in the Holocene in the paragraph that begins on P. 2488. Beginning Line 13 on that page, the proposed effects of an open Bering Strait (strength of gyre circulation, stratification, fluvial input) are described but largely unsupported by citations.

To our knowledge there are relatively few published studies that specifically target the effects of the Bering Strait opening on the development of upper-ocean and mid-depth water masses in the Bering Sea. It is, however, well established that today and in the Holocene fresher, nutrient-rich North Pacific water is transported through the Bering Strait into the Arctic Ocean, which can lead to higher productivity, e.g. in the Chukchi Sea (e.g. Keigwin et al., 2006, Hu et al., 2010). In our paper we therefore entertain the idea of a possible nutrient- and heat-trapping mechanism inside the Bering Sea that amplifies the signal known from the modern setting. We concur with the reviewer that the discussion paragraph would benefit from a better use of the evidence available. We have thus re-formulated the section and include the above-mentioned and additional modern process-oriented papers in our slightly expanded discussion. Indeed, higher resolved paleo-data both in the temporal and lateral domain that would progress our understanding of the Bering Strait influence on the internal paleo-chemical and -physical upper and mid-depth signatures in the Bering-Pacific sector would be highly interesting, a task we plan to pick up on in the future.

<u>Minor comments:</u>

P. 2474, Line 13: I don't follow how the bioturbational feature in Fig 3a could result in a reversal, could the authors expand a little to explain?

Fig. 3a shows the transition between the laminations and the "normal" bioturbated sediment. In the left part of the picture strong effects of bioturbation are visible. It seems that our sample from this core depth was influenced by material brought up by bioturbation. Maybe the text was misleading as we spoke about a "particular bioturbational feature". With that phrase we wanted to point out, that the x-ray reveals strong traces of bioturbation at that depth interval. We have rephrased this in the revision and in addition marked the particular pattern in the figure for a clearer understanding .

P. 2475, Line 4: Forgive me if I missed it, but the authors should say on what length scale they assigned facies type. There is an ambiguous layer (<1 cm of what I'd con-sider "layered facies") in an interval designated "laminated facies" from the Preboreal (Fig 3c, ~414 cm).

Concerning Fig. 3c, 414 cm: The "layer" mentioned is a comparable thicker single laminae. In contrast to the layered sediments the laminae reveal clear boundaries in the x-ray images and also show the alternations of higher and lower Cl counts and Si/Ti ratios in the XRF data (Fig. 8). This enables us to perform laminae countings. As long as we could proceed with countings, we talk about laminated sediments. Maximum thickness of a single laminae can reach up to 0,6 cm and thinnest intervals of layered sediments have a length of 1 cm. The combination of x-ray images and high resolution u-XRF data from the ITRAX scanner has proven to be suitable for laminae countings also for the Lake Suigetsu varve record (Staff et al., 2012). The classification of the sediment has been revised accordingly and a clarification has been added to the definition part within the material and method section.

P. 2475, Line 2: I was surprised to see that the authors don't report an error estimate with each layer count. Ambiguous layers in otherwise well-developed laminations (i.e. Fig 3c, ~414 cm) would result in uncertainty in the layer counting, and the incidence of these ambiguous layers probably vary with depth. I suggest the authors report such uncertainties.

Counting uncertainties of different observers mentioned on P. 2475, Lines 1-3 were amended to include the counting uncertainties for our defined TI-BLU units.

P. 2476, Line 7: I don't think the authors say explicitly how they constructed the composite record. It could be helpful if the authors indicate in Fig 4 which sections of the two cores are in the composite. It's confusing when comparing to Fig 6a–is this the composite (with the portion from SO202-18-6 rescaled?) or the record from SO202- 18-3?

We reconstructed the composite record by correlating the XRF-data (e.g. Ca peaks), the lamination pattern and ash layers between both cores (see Fig. 8). In doing so we were able to transfer the 14C ages of SO202-18-6 onto the depth-based Ca curve of SO202-18-3, shown in Fig. 6. We will clarify the methodology in the revision. In addition, the data provided in the supplement of the final paper and/or through the PANGAEA database will feature a composite depth-depth-age scale record to clarify the correlation.

P. 2477, Line 25: See the recent Katsuki et al (2014, GRL, doi:10.1002/2014GL059509) paper on PC23A.

We also have become aware of this paper, which was not yet published when we submitted our manuscript to CPD and have prepared a brief discussion item for inclusion in the final

manuscript version. However, a recently finalized companion paper to the study submitted here, is taking up and extending the main conclusions of Katsuki et al (2014) with regard to potential forcing mechanisms by using dedicated time series spectral analyses (FFT, Wavelet, B/T and MEM) on laminated sections of our presented cores (Kühn, H., Lembke-Jene, L., Lohmann, G., Esper, O., Gersonde, R., Lamy, F., Arz, H. and R. Tiedemann (in prep.) *Interdecadal Cyclic Variations in Bering Sea Laminations – Constraining the Forcing of Deglacial Export Production Maxima* ***).***

P. 2478, Line 19-20: "...especially at the Younger Dryas-Holocene transition in both cores and at the onset of the Bølling in..." Use "Bølling" instead of "Termination 1a" so to use the same kind of nomenclature for both transitions.

We agree and corrected this in the revised version.

P. 2479, Line 21-26: I would delete these two sentences. At this point, it's premature to refer to a productivity mechanism that links "warm"/"cold" periods to laminations/bioturbation. You haven't made the argument for this mechanism yet (Sec. 4.4). Since this section is about chronology and NGRIP, defer any mention of the mechanism until the next section.

We agree with the reviewer and deleted these sections in the revised manuscript.

P. 2481, Line 21: "88 laminae couplets" In Fig 8, you label this interval with "89".

We corrected this in the revised manuscript.

P. 2483, Line 18: Since the authors have not talked at all yet about SST, nor shown any SST data, I would rephrase this entire paragraph in terms of a "proposed hypothesis" and "testing the relationship between SST and laminated sediments/productivity proxies".

We thank the reviewer for this suggestion and rephrased this section in a way that we are testing for relationship of lamination occurrence and SST's.

P. 2492, Line 6-7: "...we observe millennial-scale changes in the NPIW oxygen concentrations..." Do the authors mean the source intermediate water to this location? Or local intermediate water (affected by local productivity)?

We mean mid-depth Bering Sea waters. These waters ,in turn, are influenced by North Pacific mid-depth source water masses.

Table 3: I think it would be very helpful for the authors to indicate the T1-BLU assignments in this table. I wrote it in myself and referred to it frequently while reading the paper.

We agree and the different TI-BLU intervals are now marked in bold/italic in Table 3 in the revised version of the manuscript.

Figure 1: Typo in annotation of SO201-2-114.

We thank the reviewer for this comment and corrected this in the revised manuscript.

Figure 2: Site U1344 (3173 m) also has deglacial laminations (see the Proceedings volume).

We acknowledge the comment and included Site U1344 in the revised version of Fig. 2. We also agree with Takahashi et al. (2011) that the OMZ development in the Bering Sea is an intriguing and yet unresolved feature of the last deglaciation. The situation becomes even more complicated because unlike Site U1344 deep cores from the INOPEX cruise don't show laminations like those observed during the last deglaciation. Unfortunately a more in-depth analysis of vertical features of the OMZ could not be fitted in this study. This however is something we are planning to do in the future.

Figure 5: "Short-term sedimentation rate maxima..." I can't find any reference to this in the text. It occurred to me that with annual laminations, you could construct extremely high-res estimates of changes in sed rate through time. I would personally find it fascinating to see how it compares to the radiocarbon-based coarse sed rate estimates.

We changed the figure caption and replaced the term "Short-term sedimentation rate maxima…". We further agree with the reviewer that high-res sedimentation rate records would be interesting. This will be done in a separate study as we don't have finished the high-res length measurements of single laminae and microfacies analysis.

Figure 6: The uncorrected 14C ages don't perfectly match the numbers in Table 2–transcription error? You need x-axis labels for panel a. What do the asterisks in panel a mean?

We appreciate the comment of the reviewer. The 14C ages shown in Fig. 6 are correct but the 14C age at 530-532 cm of SO202-18-3 is not shown. We corrected the figure, including the x-axis labels, in the revised version of the manuscript.. 14C Ages marked with asterisk are from core SO202-18-6. This was also clarified.

Figure 8: I suggest you annotate the NGRIP panel with the number of years in each GI climate interval. Include labeling NGRIP with 85 years and SO202-18-3 with 60 y for the early-Allerød cool period. Amazing correlation!

We thank the reviewer for this comment and revised the figure accordingly.

Literature:

Debret, M., M. Desmet, W. Balsam, Y. Copard, P. Francus, and C. Laj.: Spectrophotometer analysis of Holocene sediments from an anoxic fjord: Saanich Inlet, British Columbia, Canada, Marine Geology, 229(1–2), 15–28, doi:10.1016/j.margeo.2006.01.005, 2006.

Hu, A., Meehl, G. A., Otto-Bliesner, B. L., Waelbroeck, C., Han, W., Loutre, M-F., Lambeck, K., Mitrovica, J. X., and Rosenbloom, N.: Influence of Bering Strait flow and North Atlantic circulation on glacial sea-level changes, Nature Geoscience, 3, 118-121, doi:10.1038/ngeo729, 2010.

Keigwin, L. D., Donnelly, J. P., Cook, M. S., Driscoll, N. W., Brigham-Grette, J.: Rapid sea-level rise and Holocene climate in the Chukchi Sea, Geology, 34, 861-864, doi:10.1130/G22712.1, 2006.

Kim, S., Khim, B. K., Uchida, M., Itaki, T., and Tada, R.: Millennial-scale paleoceanographic events and implication for the intermediate-water ventilation in the northern slope area of the Bering Sea duriing the last 71 kyrs, Global And Planetary Change, 79, 89-98, doi:10.1016/j.gloplacha.2011.08.004, 2011.

Lam, P. J., Robinson, L. F., Blusztajn J., Li, C., Cook, M. S., McManus, J. F., Keigwin, L. D.: Transient stratification as the cause of the North Pacific productivity spike during deglaciation, Nature Geoscience, 6, 622-626, doi:10.1038/ngeo1873, 2013.

Max, L., Riethdorf, J.-R., Tiedemann, R., Smirnova, M., Lembke-Jene, L., Fahl, K., Nürnberg, D., Matul, A., and Mollenhauer, G.: Sea surface temperature variability and sea-ice extent in the subarctic northwest Pacific during the past 15,000 years, Paleoceanography, 27, PA3213, doi:10.1029/2012PA002292, 2012.

Nuernberg, D., Tiedemann, R.: Environmental change in the Sea of Okhotsk during the last 1.1 million years, Paleoceanography, doi:10.1029/2004PA001023, 2004.

Ren, J., Gersonde, R., Esper, O., and Sancetta, C.: Diatom distributions in northern North Pacific surface sediments and their relationship to modern environmental variables, Palaeogeography, Palaeoclimatology, Palaeoecology, doi:10.1016/j.palaeo.2014.03.008, 2014.

Staff, R. A., Nakagawa, T., Schlolaut, G., Marshall, M. H., Brauer, A., Lamb, H. F., Bronk Ramsey, C., Bryant, C. L., Brock, F., Kitagawa, H., van der Plicht, J., Payne, R. L., Smith, V. C., Mark, D. F., MacLeod, A., Blockley, S. P. E., Schwenninger, J.-L., Tarasov, P. E., Haraguchi, T., Gotanda K., Yonenobu, H., Yokoyama, Y. & Suigetsu 2006 Project Members.: The multiple chronological techniques applied to the Lake Suigetsu SG06 sediment core, Japan, Boreas, 42(2), 259-266, doi:10.1111/j.1502-3885.2012.00278.x, 2012.

Takahashi, K., Ravelo, A. C., Alvarez Zarikian, C., and Expedition323Scientists: Bering Sea Paleoceanography Expedition 323 of the riserless drilling platform. Victoria, British Columbia (Canada), to Yokohama, Japan; Sites U1339–U1345; 5 July–4 September 2009, in: Proceedings of the Integrated Ocean Drilling Program, Volume 323, Integrated Ocean Drilling Program Management International, Inc., Tokyo, 2011.

Von Quillfeeldt, C. H.: Common diatom species in Arctic spring blooms: Their distribution and abundance, Botanica Marina, 43, 499-516, doi:10.1515/BOT.2000.050, 2000.