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Interactive comment on "Laminated sediments in the Bering Sea reveal atmospheric teleconnections to Greenland climate on millennial to decadal timescales during the last deglaciation" *by* H. Kuehn et al.

Anonymous Referee #2

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This paper presents results from a remarkable sediment core with laminated sediments from during the last deglaciation. The authors show a clear relationship between NGRIP d18O and sediment fabric, and argue compellingly that the laminations are annual couplets throughout the Bolling-Allerod and early Holocene. I think this analysis and the high quality of the data are the strongest parts of the paper.

The authors argue that variations in oxygenation of NPIW and variations in local productivity (controlled by sea-ice extent) together control the OMZ intensity and extent (rather than NPIW ventilation rate). My major comments are about how the authors

could strengthen this latter section of the discussion.

Major comments:

Deglacial SST threshhold hypothesis (Section 4.4)

(1) I do not find the lamination-temperature threshhold hypothesis convincing. The authors point out that the onset of the Bolling 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 threshhold stated in P. 2484, Line 16, and Line 19 of the Abstract. In fact, during the Bolling, where the laminations are most well developed, the SST data are virtually all lower than the threshhold. 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 threshhold (p. 2487, Line 18-20), saying that the SST-oxygenation relationship is not a simple linear one.

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 Bolling-Allerod 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.

(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.

(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).

(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.

Holocene SST-lamination pattern (Section 4.5.1)

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.

Minor comments:

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?

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 consider "layered facies") in an interval designated "laminated facies" from the Preboreal (Fig 3c, ∼414 cm).

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 uncertanties.

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?

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

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

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.

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

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".

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)?

Table 3: I think it would be very helpful for the authors to indicate the T1-BLU assign-

ments in this table. I wrote it in myself and referred to it frequently while reading the paper.

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

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

Figure 5: "Short-term sedientation 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.

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

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-Allerod cool period. Amazing correlation!

Interactive comment on Clim. Past Discuss., 10, 2467, 2014.