

Interactive comment on “Centennial to millennial climate variability in the far northwestern Pacific (off Kamchatka) and its linkage to East Asian monsoon and North Atlantic from the Last Glacial Maximum to the Early Holocene” by Sergey A. Gorbarenko et al.

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General Comments.

This paper presents a very high-resolution record of productivity in the Northwest Pacific for the past 21 kyrs. It is really a remarkable amount of data especially for the discretely sampled analyses (TOC, grain size, etc). The paper attempts to answer

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the question of whether productivity changes in the North Pacific were synchronous or asynchronous from changes in North Atlantic climate. They conclude that increased productivity in the North Pacific is positively correlated with warm intervals in Greenland C1CPD Interactive comment Printer-friendly version Discussion paper and attribute this to “tight atmospheric teleconnections.” There are several major issues with this manuscript that make it difficult to follow the authors’ conclusions. Specifically, issues with English grammar and word choice make it very difficult to understand at times, the age model causes extreme concern, and the assignment of high productivity/warm events appears to be arbitrary for the core studied (41-2). I highlight a few issues with the word choice and organization of the paper below in “Technical Corrections,” however this should not be considered an exhaustive list as I tried to avoid copy-editing as much as possible. It is possible that I have misunderstood how the authors constructed the age model, however it appears that they first radiocarbon dated a mix of benthic and planktonic foraminifera. They then applied a correction of 1400 years to the benthic foraminifera based on the data in Max et al., (2014). They then compared productivity cycles in 41- 2 and 12KL to refine their age model and discarded four of the five radiocarbon ages from core 41-2. Finally, they correlated their productivity cycles to well-dated Chinese subinterstadials. This presents serious concerns for several reasons: RC2: 1. It’s unclear why the authors are using benthic foraminifera at all. Are planktonics not plentiful enough? Why weren’t radiocarbon samples chosen from the CaCO₃ peaks? Surely there should be enough planktic foraminifera when carbonate is high.

Answer. We had used benthic foraminifera because planktonic ones were very rare. There are insignificant carbonate peaks in core 41-2 and carbonate content was low (look Fig. 2).

RC2: 2. It’s unclear at which point the authors apply the ventilation conversion— before or after calibrating—after would be appropriate.

Answer. We applied the ventilation conversion after calibrating.

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RC2: 3. It's unclear why they chose 1400 as a stable ventilation age. One of the main points of Max et al., 2014 is that ventilation changes dramatically and frequently during deglaciation. In that paper it ranges from 160 to more than 2500 years. It might have been more appropriate to use ventilation estimates corresponding to the approximate C2CPD Interactive comment Printer-friendly version Discussion paper calibrated age.

Answer. The age difference between benthic and planktic (B-P) in core 41-2 for depth 120 cm was accepted as 1400 year according to smoothed spline interpolation for NW Pacific deep water ventilation rate after 15 ka (Max et al., 2014, Fig. 5). Bottom water of core 12KL (water depth of 2145 m) and core 41-2 (depth of 1924 m) is most likely originated from N Pacific deep water. AMS 14C age for depth 120 cm by accepting by a B-P age difference nearly 1400 yr equals to 9.12 ka and separated by our the centennial-millennial low productivity/cold event nearly this interval (Fig. 2) closely correlated with NGRIP cooling event of 9.3 ka.

RC2: 4. It's unclear how the productivity cycles are determined in 41-2. The authors state that they're "based on a suite of productivity proxies and PM records" and that they correspond to synchronous changes in these proxies. However, I have examined several of these intervals and cannot find the commonalities between them. As I understand it, the authors are using Ba, Br, Si, b*, TOC, CaCO₃, and chlorin as productivity proxies. Looking first at event 6 from the Last Glacial, I see that some of these proxies are flat during this event, some are fluctuating, and some are high. This pattern continues for all the other productivity events as well. Barium seems to most often be high during productivity events, but there are several peaks in Barium that are not associated with productivity events, why not? Clarifying the determination of these events is essential to all aspects of this manuscript.

Answer. We consider that using suite of productivity proxies instead of 1-3 ones provide more realistic information about productivity changes. Each productivity proxy has own specific response to surface water primary production changes, many papers devote these problem: Dymond et al., 1992 for Barium, Yu et al for CaCO₃ content, Sarin et al.

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et al., for TOC, Gorbarenko et al., 2014 for Si-bio (color “b”) et so on. It is subject for special paper. Different productivity proxies respond to other kinds of productivity effect on content of TOC, chlorin; preservation in sediment of separated proxies, etc. The presentation of the suite of productivity proxies is our advantage. Beside productivity proxies we also used the PM record because the sediment PM reflects the changes in transportation of dust from continent by atmosphere circulation associated with climate changes. Yes, not all proxies change synchronously but their common trends allow us to trace productivity events. Therefore we used term quasi-synchronous. For statistic we calculated a productivity stack of all productivity proxies and reversed PM index to show more clearly (statistically estimated) occurrence of the centennial-millennial productivity events in the NW Pacific which was presented in revised Figs. 3 and 5. Therefore we did some changes in related paragraph of manuscript (lines ...).

RC2: 5. It's unclear how the authors determined which radiocarbon ages to discard. They discarded three benthic ages and the only planktic age measured. The planktic age is probably the strongest part of the age model and it seems to fit in with their correlation to 12KL. Why is it discarded? Why is one benthic age kept, but not others?

Answer. We kept benthic age for depth 120 cm because it is younger age for core 41-2 and this data also good matched with projected age of core 12KL for depth 126 cm. Laying below AMS data of core 41-2 are good matched with projected AMS 14C datum from core 2KL and confirm validity of projection 14C data core 12KL on depth of core 41-2. We do not discard them and show them in Figs 2 and 3. But sediment of core 41-2 has lower carbonate content and do not characterized by such significant CaCO₃ peaks as in core 12KL. That is why we prefer to use 14C data projected from core 12 KL in our final age model for core 41-2, because 14C data for core 12 KL were determined for planktonic foram from depth with strong Ca peaks.

RC2: 6. The final correlation of the productivity events to Chinese subinterstadials is perhaps the most troubling part of this age model. If the age model for 41-2 is tuned to the oxygen isotopes from Chinese caves, then the authors can not claim

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that productivity events in the North Pacific happened synchronously with these sub-interstadials. This is circular logic. In addition, it would be useful to include a discussion that addresses the differences between the myriad productivity proxies. By no means do these records look the same, especially on centennial to millennial scales. Why not?

Answer. In context of critical comments of reviewers 1 and 2 we try more clearly present age model of core 41-2 construction. We had yet explained using of suite productivity proxies in identification of the productivity events in point 4. Then we show how we find that centennial-millennial productivity events in NW Pacific core had occurred synchronously with summer EAM sub Interstadial: “A close time correlation of these NW Pacific productivity increasings/environmental amelioration events with sub-interstadials in summer EAM become apparent after projection of the radiocarbon datum of both cores on the absolute U-Th dated $\delta^{18}\text{O}$ record of the China caves stalagmites (Wang et al., 2008;) over the 20-8 ka BP (Fig. 3)”, lines

Specific Comments

RC2: Section 2.1: It's unclear how tephra was estimated. Was it identified under a microscope? By magnetic susceptibility, some kind of Principal Component Analysis of the XRF scanning? I'm not sure what “semi-quantitative component analysis of this fraction with a total of 12 ranged scales” means.

Answer. In the section of 2 Materials and methods we tell that content of the terrigenous particles, tephra, planktonic and benthic forams and other component in the coarse sediment fraction (CF) was semi -quantitatively estimated under binocular with a total of 12 ranged scales. We used the comparative percentage charts for estimating proportions of sedimentary components (Rothwell, 1989). These results allow us to separate tephra input from IRD one (only terrigenous particles) in the measured CF values (Fig. 2).

RC2: When I first read Section 2.4, I was under the impression that this manuscript

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would present planktic-benthic pairs of radiocarbon dated foraminifera. I needed to examine the table myself to determine that it does not. This should be clarified.

Answer. Table 1 show AMS ^{14}C measurements of the benthic and planktic foraminifera picked in several depth intervals and their calibration.

RC2: In Section 2.6, it's unclear how the terrigenous component of Ba, Br, and Si was determined. Lines 159-160 read, "The terrigenous component, in turn, was calculated from empirical regional (Ba/Al)_{terr} ratios in the sediment core with the lowest Ba-tot contents." What sediment core are you referring to? Where is it located? What is the regional observed value (Ba/Al)_{terr}? If this is an empirical value, it should not be vague.

Answer. The empirical (Ba/Al)_{terr} ratio was estimated for the studied core 41-2 using the technique suggested by Goldberg et al. (2005). The exact value of the ratio was calculated separately for each of data series.

RC2: The age model should come before any discussion of sediment ages in the Results Section, i.e. Sections 3 and 4 should be reversed (and revised accordingly).

Answer. Our age model of core 41-2 was based on the correlation of the productivity events between the cores 41-2 with ones of well dated core 12KL. That is why we need put results with productivity proxies and separated productivity events before an age model section.

RC2: In line 169, the authors posit that they observe high productivity in the middle part of the core; however, productivity proxies here are only slightly higher than the bottom of the core, but not high at all in relation to the full record. In addition, not all of the listed proxies show an increase in productivity during this interval (see for example, Ba). It would be more accurate to say that many of these proxies increased during Termination or just after the glacial. Furthermore in this paragraph, not all proxies decrease between 230 and 190 cm.

Answer. ALL PRODUCTIVITY PROXIES demonstrate higher productivity in interval

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of 230-315 cm, as was stated in line 169, including the Ba-bio as well (Fig. 2). This interval was correlated with BA warming by us, consistently with available for this core AMS 14C data and found earlier for studied region high productivity during BA warming. ALL PRODUCTIVITY PROXIES demonstrate also productivity decrease in interval of 190-230 cm, correlated by our with YD cooling, and following productivity increase from 190 cm was associated with Holocene warming that is also consistent with known regional climate and productivity trend and AMS 14C data.

RC2: In several places, but first on line 175 (later, line 186), the authors associate high productivity with warming, but no sea surface temperature proxies are presented in the paper. It is unclear where this association comes from. If it is only from the association of high productivity with climatically warm periods, i.e. Holocene Thermal Maximum, then the sentence on lines 174-176 contains circular reasoning.

Answer. We had clarify the connection of productivity changes with environment and climate changes on the millennial scale in revised ms and discussed SST changes versus time for core 12 KL (Max et al., 2012).

RC2: On lines 192-196, the authors assert that the percent coarse fraction, magnetic susceptibility, and volcanic glass can be used as a proxy for ice rafted debris, however it is unclear how these were used. Was an index created of the three to track IRD? This should be clarified.

Answer. We had clarified this in revised version of paragraph 2.1 (Coarse fraction measurement). We show that CF and MS records may be used as IRD only with controlling of tephra share input in the CF. When the tephra share is large in CF (Fig. 2), we can't determine IRD input.

RC2: Lines 213-214 indicate that the Tiedemann/Max age model is tuned to the oxygen isotope record from NGRIP, but it is not. In addition, if it was that would cause the main conclusions of this manuscript to employ circular reasoning. This should be clarified so that it is evident that it was a conclusion of Max et al., (2012, 2014) that b^* from 12KL

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correlates to NGRIP even with an independent age model in 12KL.

Answer. Thank you for this comment. We had significantly revised section related to the age model and show how we used AMS 14C datum of core 12KL and correlation of color “b” with NGRIP $\delta^{18}\text{O}$ curve according to Max et al. (2012, 2014), lines .

RC2: Section 5.1 is interesting, but seems lengthy and tangential to the discussion at hand. Four pages and two figures are dedicated to reviewing the relationship between paleoclimate in Greenland, Antarctica, and East Asia without any mention of the core that is the subject of the paper. Likely, some of this information is necessary to back up the idea that the Northwest Pacific acted synchronously with East Asia and Greenland, and out of phase with Antarctica, but it needs to be condensed and better organized.

Answer. It is important comment. We added the productivity proxy records in Fig. 5 in order to test centennial-millennial productivity events in the NW Pacific. Here we present statistically significant stack of productivity for core 41-2. Causal relationship of identified centennial-millennial productivity events in the NW Pacific (yellow bars) with paleoclimate changes in the East Asia, the Greenland and Antarctica and solar irradiance variability for the last 14 kyr was discussed in this section. The productivity records of cores 41-2 and 12KL with the shown productivity events were discussed in Sections 5.1 and 5.2.

RC2: In lines 390-393, the authors state that there is increased sea ice based on their coarse fraction and magnetic susceptibility records, however there is no basis for these claims. Coarse fraction is highest between 18 and 19 ka, not between 15.5 and 17.8 ka. What percent coarse fraction indicates sea ice? I'm not aware of a citation for this from the Pacific or for sea ice specifically, though this is a common indicator of glacial ice rafted debris in the North Atlantic. Also, the coarse fraction presented in Fig. 4 is significantly different from that in Fig. 2. Has Fig. 4 been modified to account for volcanic glass? If so, how was that transformation completed. This should be clearly noted on the figure and in the text.

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Answer. We show trends of CF and MS records in Fig. 4 which reflect IRD trend input in the sediments off Kamchatka up to nearly 10 ka, when tephra share in CF significantly rise and discussed this in revised text. Records of CF and MS are similar in the Figs. 2 and 4; only in Fig. 4 they were presented versus age.

Technical Corrections

RC2: The excessive use of acronyms adds to the reader's difficulty in understanding this manuscript. Line 20: This should read, "occurred synchronously." Line 48: Space missing before Max et al., 2012 reference. Line 88: Please add the word, "the" before "joint Russian-Chinese expedition." Line 109: color b* "correlates well" not "well correlates." Line 114: This should read, "from THE 125-250 um fraction." Lines 182-183: I have no idea what "mechanisms likely similar to established earlier regularities at the orbital-millennial scale" means.

Answer: We add some references in order to show "established earlier regularities" in the revised text and discussed the linkages of the sharp productivity events with climate and environmental changes in the N Pacific and its marginal seas.

RC2: Line 206: I cannot find an explanation of what RPI stands for.

Answer: "Synchronous pattern of RPI variability in the far NW Pacific variability" means that relative paleointensity of magnetic field of Earth change synchronously in the past. Therefore RPI curves, recorded of sediment cores 41-2 and 12KL have to change synchronously versus time and may be used for time correlation.

RC2: Line 293: what is a "smoothed warmer condition"?

Answer: Thank you. We change this sentence in revised text: warming in the Antarctica at 23.6-24.4 kyr was . . .

RC2: Line 573: Is this the full reference for Harada, 2006?

Answer: We modified this reference. This cruise report is available on-line.

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RC2: Table 1: There is no need to include both calendar age in years and calendar age in ka. Note that the date at 306 cm indicates that this foraminifera is 16,016 years old and 14.616 ka old. Which is it?

Answer: Thank you. We delete the column with calendar ages in year. Calendar age of sediment at depth of 306 cm equal to 14.61 ka.

RC2: Figure 2, 3, 4: Please note that the scales for magnetic parameters are reversed.

Answer: Thank you. We note this in revised Fig captures for PM.

RC2: Figure 3: Could this figure be clarified/condensed in any way? It's a bit overwhelming.

Answer. We condense Fig. 3 by showing the productivity stack.

RC2: In addition, some of the lines of correlation are missing in some cores. Is that intentional?

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