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Climate of the Past
Discussions

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.

Anonymous Referee #2

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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 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

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:

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.

2. It's unclear at which point the authors apply the ventilation conversion– before or after calibrating–after would be appropriate.

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

calibrated age.

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 eventsâĂŤwhy not? Clarifying the determination of these events is essential to all aspects of this manuscript.

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?

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 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?

Specific Comments

Section 2.1: It's unclear how tephra was estimated. Was it identified under a micro-

scope? 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.

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

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)ter 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)ter? If this is an empirical value, it should not be vague.

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).

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.

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.

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.

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

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.

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.

Technical Corrections

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.

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

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

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

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?

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

Figure 3: Could this figure be clarified/condensed in any way? It's a bit overwhelming. In addition, some of the lines of correlation are missing in some cores. Is that intentional?

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