Clim. Past Discuss.,
doi:10.5194/cp-2016-105-RC1, 2016

Climate of the Past Discussions
Open Access
EGU

Interactive comment on "Holocene dynamics in the Bering Strait inflow to the Arctic and the Beaufort Gyre circulation based on sedimentary records from the Chukchi Sea" *by* Masanobu Yamamoto et al.

Anonymous Referee #1

Received and published: 7 December 2016

This paper deals with variability of the Beaufort Gyre and Pacific inflow into the Arctic Ocean during the Holocene. The authors used sediment cores from the Chukchi Shelf, and applied ratios of minerals, measured by XRD, as proxies for those two systems. High resolution Holocene time series of Bering Sea inflow and Beaufort Gyre strength are sparse, and study like this are much needed to improve our understanding of Arctic ocean circulation during interglacial time periods.

The ratio of quartz over feldspar (Q/F) as proxy for strength of the Beaufort Gyre is promising and the results from two studied sediment cores show good agreement,

Printer-friendly version

Discussion paper

indicating a gradual decrease during the entire Holocene.

The proxies for Bering Strait inflow (BSI), the ratios of chlorite and kaolinite over illite (CK/I and C/I), however, are more problematic. Although the data from the surface sediments indicates potential for a good proxy of Bering Sea sourced water in the Arctic, the data from the sediment cores do not show any good agreement between them, contrary to what the authors claim.

One of the main problems with this manuscript is the data presented in the supplementary information. It does not always match what is shown in the manuscript figures, and inconsistent data (deglacial time period in 05JPC) seem to be omitted from the discussion. Since it is not clear which data is correct (tables or figures), it is difficult to review the interpretations and discussions suggested by the authors.

The chronology of Core 01A-GC in the late Holocene is based on only 1 radiocarbon date for the last 4000 years, and relies on interpolation from ca. 3.5 ka to present. Based on such minimal age control, any correlation of centennial variability to other records is very difficult.

The description of results is extremely minimal and contains errors.

The discussion jumps directly into a larger context including large scale processes, sea ice feedbacks, solar insolation, complex atmospheric teleconnections to the North Atlantic etc. What is missing however, is a critical evaluation of the data presented in this study. This review does not focus much on the discussion, as there are too many issues in the results that need to be addressed first.

In its current state, I would not recommend publication of the manuscript in Climate of the Past. This study, however, has generated a lot of interesting data and I think a heavily revised version of the manuscript could be more suitable for publication. Now there are too many errors in the manuscript including the misrepresentation of data, inconsistencies between tables and figures, and incorrect description of results. The

Printer-friendly version

Discussion paper

Q/F proxy could be a promising tool and the Holocene reconstruction shows a clear trend in both cores, and millennial variability worth investigating in more detail. I suggest that the authors focus on this more. The other proxies (the BSI reconstruction) do not show agreement between the different cores, and there are problems with the data itself. There is not enough here for such speculative interpretations with complex teleconnections.

Specific comments

- Clarify the relationship between the Aleutian Low pressure center and the Bering Strait inflow. It seems that increased BSI is related to a weak AL (e.g. text in abstract), but sometimes the opposite is described (Page 5, lines 112-117: here a strong AL is linked to increase BSI). Maybe add an overview figure of the mechanisms to make this clear.

- In Section 2.2, a good overview is given of the different mineralogical proxies and ratios used in this study. Kaolinite, however, is missing here. Where does Kaolinite come from? And why is it a good indicator of BSI?

- As mentioned, other studies included muscovite to study BSI. Why is it not included here?

- Section 4 (Results) is extremely short and describes only the mineralogy results of the surface samples and the sediment cores. The chronology of core 01A-GC is not described in the results. Start with a sub-header: 4.1 Chronology of sediment core 01A-GC and describe the dates and the age model.

- The description of the mineral ratio results is poor. Page 12. Line 280-281. Listing of minima and maxima. Besides the many mistakes in these lists (e.g. 6.8 maximum, 2.9 minimum), there seems to be no clear rule for what a maximum or a minimum is (e.g. why are 2.2 and 4.6 ka maxima, but not 3.1?).

- The samples in 05JPC are at approximately half the temporal resolution compared to

the data of 01GC. Therefore, it can be expected that some of the short term variability seen in 01GC is not detectable in 05JPC. The resolution of core 05JPC, however, still provides ca 4-5 data points per 1000 years, and millennial variability should thus still be present. Any long-term trends (i.e. low frequency signals) should be visible in both cores. This is not what the authors state, e.g. Page 13, Lines 288-289: "no significant long-term trend can be recognized, presumably because of relatively low age resolution". Page 12, Line 282: "the millennial-scale variability is unclear because of lower age resolution".

- This also means that both records should show some agreement over longer time scales. This is true for the Q/F data, but much less so for the BSI proxies. This critical discussion is missing in this manuscript.

- Age control of core 01A-GC. The youngest date corresponds to approx. 3 – 3.5 cal ka BP depending on the reservoir age. Yet, the authors have assigned the ages to sediments of the top to modern age. If you simply extrapolate the dates, the top would be somewhere around 2500 cal BP in age. This would change the entire interpretation significantly.

- Why do the authors assume that the top of the gravity core corresponds to present? Is there any data to support this? e.g. Pb210 data or a correlation to a surface core? It is unusual for a gravity core to preserve the sediment surface intact, and so the question is how much is missing? 2500 years?

- Supplementary Table 3 shows all mineralogical data from core HLY0501-05JPC/TC. Apparently the age of this core ranges back to 15 ka BP, but only Holocene data is shown in the manuscript figures. Although the precise timing of the Bering Strait flooding is unknown, it should be somewhere during the deglaciation, based on sea level reconstructions (see also page 17, lines 387-388). This would mean that in the base of this core, there is no connection to the Pacific Ocean and Bering Strait Inflow is non-existent, or greatly diminished at least. Yet, the mineralogical proxies C/I and CK/I

show no such signal, which could mean a couple of things:

o The ages are incorrect, OR

o The proxy does not work as expected, OR

o The Bering Strait gateway was open until at least 15 ka, an important finding in itself.

- Suppl. Table 2: The listed CK/I and C/I values for core 01AGC do not match the data displayed in Figure 3B. Which is correct? The table or the figure? The manuscript text and interpretation seem to be based on the data shown in the figures. If the data in Supplementary Table 2 is correct, then this would change the entire paper.

- Page 16 describes an agreement between chlorite + muscovite in core 06JPC and the CK/I in core 01A-GC. There is no agreement however with 05JPC, which is much closer to 06JPC. Why not?

- There is a mistake in the dinoflagellate sea-ice reconstruction of Core 05JPC, presented in Figure 8C. The data presented (red curve in Fig. 8C) is only that of the piston core, and the trigger core data is overlooked. This also means that the ages at the top of the trigger core are too young. This same mistake was made in Farmer et al. 2011 and in de Vernal et al 2013. Look at McKay et al 2008 (CJES; 10.1139/E08-046) for the original publication and I would suggest to contact Anne de Vernal for the original data from the trigger core. The correct data shows that some of the highest sea ice concentrations occur during the last 1000 years, contradicting what is written in the manuscript, e.g. Page 19-20, Lines 453-454.

Figures and Tables

Figure 1: To avoid overcrowding the map, I suggest to remove the black dots showing the locations of the surface sediment samples. It is enough to show them in Figure 2. The core locations are not clear on the bathymetry. I would suggest to include an insert, zooming in on the core sites.

Figure 2: There are a number of inconsistencies here.

- In panel C (C/I ratio), there are several data points missing compared to the other datasets. Is this a mistake, or is there no data? If there is data to show for (C+K)/I, there should also be data to show for C/I? If this is no mistake, there is probably a technical explanation for this, which can be provided in the methods or results.

- The scale bar for the Q/F ratio in panel A ranges from 4-40, but the ratio axis for the same proxy in panel D ranges from 0.6 – 1.6. Is this a logarithmic scale? If yes, make this clear.

- From Panel E, one can see that there should be a data point with a CK/I ratio around 2.0 at about 63°N. This is not visible in Panel B. Check this carefully, as there may be others?

- At some sites, there are too many data points for this type of plot. An example: In Panel A, at the Mackenzie delta there are a lot of yellow dots, but they are covering up green ones as well. Either, make inserts for those areas, or make the dots smaller?

- Add a panel showing the newly added samples (19?) vs. the existing samples from Kobayashi et al 2016.

Figures 3 and 4. These are two figures of the same proxies on the same timescale, from different cores. Combine these into 1 figure, to show where the records agree, and where they don't.

Figure 4. - Curves A and B: The 1sigma analytical error intervals (as in Figure 3) are missing here. - Curve D: What is the difference between the grey and the black curve?

Figure 6. These figures add little to the existing text in the discussion, and drawing maps of sea ice margins for the entire Arctic Ocean without the supporting data is highly speculative.

Figure 10: Add a and b to name the panels and explain the two scenarios in the caption.

Table 1: - Add a column with mollusk species if that information is available. - Do the calibrated ages represent the mean, or the median? I suggest to list the calibrated 2-sigma age ranges, because the distributions are often not symmetrical. - There is a mistake in the first calibration. 3740 $\pm$ 30 14C years does not correspond to 3709 $\pm$ 41 calibrated years BP using Marine13 and ΔR = 0 years. The others are correct.

Minor remarks

- Page 3, line 51. Change “Being” to “Bering”.

- Page 4, line 86. Change “interact” to “and interacts”.

- Page 9. Divide section 3 in subsections: e.g. 3.1 Coring and Sampling, 3.2 Chronology, 3.3 XRD Mineralogy.

- Page 10. Line 214. Refer to Figure 2 instead of Figure 1.

- Page 11. Line 257. Add “the” add the end of the line.

- Page 12. Line 285. Do you mean Figure 3B?

Interactive comment on Clim. Past Discuss., doi:10.5194/cp-2016-105, 2016.

Printer-friendly version

Discussion paper