

Interactive comment on

“Central Arctic Ocean paleoceanography from ~50 ka to present, on the basis of ostracode faunal assemblages from SWERUS 2014 expedition” by Laura Gemery et al.

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Interactive comment on Clim. Past Discuss., doi:10.5194/cp-2017-22, 2017.

Received and published: 7 April 2017

In the context of global warming and recent Arctic sea ice waning, it is important to understand the natural forcing of past sea ice changes. Here, Gemery and co-authors present a low resolution reconstruction of Central Arctic sea ice changes over the past 50,000 years using ostracode faunal assemblages in two twin cores retrieved in 2014. Although such records are highly necessary, the manuscript suffers from several limitations and flaws that prevent acceptance in its present form. First, the manuscript does not go further than the previous study published by the same group (Cronin et al., 2010) in which conclusions were exactly the same. Central Arctic sea ice was re-constructed in several cores from the Lomonosov Ridge, over the same time period. It was evidenced that “Results suggest intermittently high levels of perennial sea ice in the central Arctic Ocean during Marine Isotope Stage (MIS) 3 (25-45 ka), minimal sea ice during the last deglacial (16-11 ka) and early Holocene thermal maximum (11-5 ka) and increasing sea ice during the mid-to-late Holocene (5-0 ka)”. Similar interpretations are here presented by Gemery and co-authors. The only addition to Cronin et al. (2010) is that “sea-ice cover during the last glacial maximum may have been less extensive at the southern Lomonosov Ridge at our core site (~85.15°N, 152°E) than farther north and towards Greenland”, which is pretty weak.

Authors' reply:

This paper addresses the distribution of key species of benthic Ostracoda and uses them as paleoenvironmental proxies to shed light on benthic community responses to changing ice and ocean conditions during the past 50ka. The core location is in a region of the Arctic unstudied for glacial, deglacial and interglacial paleoceanography and as such, fills an important geographic gap in a region that today is undergoing rapid sea ice decay. Many of the results do corroborate the conclusions of prior studies conducted on other Arctic submarine ridges (Cronin et al., 2010, which focused on a sea ice-dwelling species and Poirier et al. 2012). The new SWERUS core provides evidence for large-scale shifts in ostracode species bathymetric and geographical distributions during rapid climatic transitions. Some evidence suggests that the location of this core may not have been covered by thick ice during the 1st glacial period as long as other sites, but we are cautious to state this, as additional studies, especially radiocarbon dating, would be ideal to support or reject this.

Second, the manuscript is only descriptive and does not present any forcing mechanisms to explain the observed changes in sea ice cover over the past

50,000 years. Why the MIS 3 did not experience perennial sea ice cover when temperatures were globally lower than during the Late Holocene? What is the link between intermittent perennial and seasonally ice-free conditions during MIS3 and HE/DO? What is the impact of lower sea-level during MIS3 on ocean circulation (less to no North Pacific waters), on sea ice formation (mainly on marginal seas if I am right) and sea ice transport off the Arctic Ocean? The new data should be presented and explained in the context of large scale ocean and atmosphere changes over the past 50,000 years. There are plenty of publications from the GIN Seas and Fram Strait to document NADW inflow (marked here by *Krithe* spp. and *Cytheropteron* spp.) and AW outflow (marked here by *Polycope* spp. and *P. caudata*). There is also a wealth of publications from continental peri-Arctic to document atmospheric patterns and their impact on central Arctic sea ice. As such, the very attractive title is misleading.

Authors' reply:

*The reviewer poses excellent questions about what atmospheric and oceanic forcings and feedbacks are at play causing sea ice changes. We have added some explanations about the large-scale forcings affecting/controlling/linking oceanographic changes from published literature but a more thorough discussion is beyond the scope of this paper. The regional variability of changes in the sea-ice regime, especially during rapid climatic events, is not yet well understood due to the low sedimentation rates in the central Arctic (1-2 mm/ka). Our study focused on *A. arcticum* as an indicator for the expansion/contraction of sea ice; other proxies might also be applied to this region (ie, dinoflagellates, IP25).*

Third, results are discussed in “climatic phases” that are not congruent with the ostracode faunal changes. It is more sensible to discuss changes in the four “ostracode zones”. I however do not fully agree on the four zones. Based on faunal changes more periods can be discussed: The K zone, a first increase in *A. arcticum* between 42-35 kyrs BPP, a *P. caudata* peak between 35-27 kyrs BP, a second increase of *A. arcticum* between 25-20 kyrs BP, a second *P. caudata* peak between 20-12 kyrs BP, the C zone and the A zone.

Line 266-271: The shift between *Polycope* spp. and the *Krithe*-*Cytheropteron* group is at 12 kyrs BP not 14.5 kyrs BP. And the *Krithe* gp is less than 10%. Is this small increase significant? Over the deglaciation I see the following sequence: *P. caudata* (20-12 kyrs BP); *Cytheropteron* (12-9 kyrs BP); *Krithe* (10-7 kyrs BP). This is not really discussed.

Line 280: *Krithe* spp. are less than 10%. This is not what I call abundant.

Authors' reply:

We followed Poirier et al., 2012 faunal zonation, as these zones are well established throughout the Arctic Ocean and in the SWERUS 32 cores. Broad deglacial-Holocene faunal changes are discussed and interpreted in Poirier et al. (2012) and further in our paper.

There is no information on why there are so much difference in ostracode abundances and species numbers between the twin cores.

Authors' reply:

The dominant species' faunal trends in 32MC and 32GC are very similar. The difference in ostracode numbers between the two cores is due to getting the larger sediment sample sizes for the multicore and sampling it every centimeter. For the gravity core, we sampled every 2 or 3 cm but within that interval we sampled usually a smaller amount from half the width from the already halved archive half. This difference is commonplace when splicing together records from two types of cores from the same location. It is preferred as piston and gravity coring often does not recover the uppermost sediments, which in the Arctic can pose a huge problem due to low sed. rates. Hence, one augments the gravity core with a multicore. The sampling strategy is described in the Methods section.

Fourth, the "Results" part present description of results mingled with some environmental interpretations. And the "Discussion" part does not present any environmental interpretations nor forcing mechanisms. The structure should be modified accordingly. Lines 307-325: Useless in the paper. Authors should stick to paleoceanographic re-constructions and interpretations.

Authors' reply:

We thank the reviewer for pointing this out and we modified the Results section by instead putting all environmental interpretations in the Discussion section.

We agree, lines 307-325 are tangential, but are relevant to discussion of microfaunal species indicators of ecosystem regime change.

Fifth, the paper oscillates between presenting new sea ice reconstructions (but no explanation of such changes) and validation of *R. mirabilis* to infer past sea ice changes. I would say that these are two different topics and should perhaps be presented in two different papers.

Authors' reply:

**R. mirabilis*' stratigraphic appearance in intermediate depth cores is an important finding; they are distinct microfaunal migrational events in which a species that lives on today's continental shelf is found in intervals in sediment cores that is far outside its usual depth and geographic range. For example, *R. mirabilis* migrations are found not only in 32MC/GC but in PS2185, PS2179, AOS94 28, HLY6, AOS94 8, AOS94 12 that are presented in this paper. In addition to their paleoceanographic and ecological significance, rapid faunal migrations and limited stratigraphic ranges make these useful stratigraphic marker for correlating cores from across the Arctic Ocean.*

Additionally, records of *R. mirabilis* should be described in the "Results" part. They here appear out of the blue at the very end of paper. Lines 328-330: Ostracode species mentioned here are not presented in the results. There is no way to compare and assess what is written. Although it is difficult to assess here because the records are presented in different plots, it seems to me that *R. mirabilis* record in the twin cores are similar to the *Krithe* spp. record with peaks centered at 42-44 kyrs BP and 10-5 ka BP. This contradicts lines 328-333 where authors state that *R. mirabilis* modern distribution mimics *B. aculeata*'s one. This should be expanded. Why these two species share a similar modern distribution

(linked to perennial sea ice) while presenting different down-core records whereby *B. aculeata* is still linked to perennial sea ice while *R. mirabilis* goes together with species tracking less sea ice and NADW influx into central Arctic?

Authors' reply:

*The reviewer makes excellent points and we have reorganized the Results section and removed comparisons with foraminifera such as *B. aculeata*.*

Sixth, the “Chronology” part is not totally clear to me. Data used to estimate the mentioned 3cm offset between the MC and GC cores are not presented. The tuning below 31.5 cm is not presented. It seems that there is only one point with *E. huxleyi* to infer the MIS5. I strongly doubt that the mean reservoir age was constant through time. It should be acknowledges even though this may not have a big impact on the results/interpretations here due to low temporal resolution.

Authors' reply:

*We have clarified these points in the text and specified that the reservoir age was not likely constant through time. We used the dominant ostracode patterns to align the MC and GC and thereby determine the 3cm offset. Chronology beyond 50ka and use of *E. huxleyi* is presented is based on correlation of sediment properties and dates from other nearby cores. Chronology beyond 50 ka is not relevant to this paper, albeit we still present it as supplementary information for the reader.*

Seventh, the “Introduction” is very weak. The scientific issue is not very well presented (only in first and last paragraph). There is not state-of-the art. I suggest to much better highlight the difference to Cronin et al. (2010).

Authors' reply:

We thank the reviewer for the suggestion to fortify the Introduction. We have added and revised this section accordingly.