

We thank the reviewer for their comments and suggestions. We believe that incorporating these comments will lead to an improved manuscript.

*Vermassen et al. present a nice paper on the palaeoceanography of a Greenlandic fjord. In my opinion, the paper is relevant and a good fit for Climate of the Past. However, there are some issues that need addressing and resolving before the paper can be published.*

*1. I think the paper suffers from focusing solely on one proxy (benthic foraminifera), especially since relatively major oceanographic/environmental changes are inferred from the data. Supporting data, for example TIC/TOC, biogenic silica, stable isotopes of oxygen and carbon etc. would greatly strengthen the interpretations, especially those regarding productivity. Although this may be outside the scope of this study, I still find it surprising that such relatively standard sedimentary analyses were omitted here.*

We certainly agree that multi-proxy analysis can add value to sediment core analysis. Unfortunately, due to time constraints as well as limited available sediment, we cannot complete further additional analysis.

*2. One paper – Vermassen et al. (submitted to JQS) is quoted repeatedly, with many of the authors overlapping with the present manuscript; however, this paper is under review and so presumably not available yet. Is the JQS paper focussing on the same core, and if so, what was the reason for publishing it separately rather than having it as one, stronger paper? I think this issue should be addressed, as relevant data from the JQS manuscript may strengthen the current one.*

The quoted article (JQS) has meanwhile been accepted for publication and is in production. The JQS article investigates IRD variations from multiple cores in Upernavik fjord, also including this core. The main reason that the IRD data is not incorporated in this article (Climate of the Past) is that we found that the relationship between IRD and glacier behavior is not as straightforward as is often assumed. Therefore we decided to write a 'proxy-evaluation' article that specifically discusses the relation between IRD and glacier behavior (i.e. the JQS paper), and this is why the IRD is not addressed in this paper. This allows this Climate of the Past paper to focus more clearly on the question 'does/did ocean variability affect glacier behavior'. Nevertheless, I agree that it would still be good to at least also show the IRD data in this article, so I include this now in Fig. 7.

*3. Since your core was collected at 900 m depth, your benthics record bottom water conditions only. You mention planktonic foraminifera in passing, however [section 4.3; lines 11-12]. Although planktonics may be sparse, I still think this is important and should be expanded on, as they may give you a clue as to the validity of your overall interpretations.*

We agree that the way the planktonic species were mentioned was not very clear. Some planktonics were found, but in very low abundances. I expand this section and add the variability in total no. of planktonic foraminifera per gram to Figs. 5 and 7.

*4. The dissolution of calcareous foraminifera also depends on depth – for example, there are almost no calcareous foraminifera in deeper Baffin Bay waters.*

We have added a line about this to the manuscript.

*5. You identify organic linings in your samples and assign them to *Elphidium excavatum*. How confident are you that these linings are those of *E. excavatum*? Did you dissolve specimens of this species to check this? What about the linings of other planispiral species? If you get foraminiferal linings in your samples, you must have more than one type present – what are they? I think the link between linings and a specific*

*foraminiferal species should be demonstrated more clearly, as this forms the basis of your argument regarding dissolution later on [section 5.1], and, especially, the link to Atlantic water inflow.*

We agree that identifying a species solely based on organic lining is too tentative and removed mentioning *E. clavatum* as the main species. We did not intend to present the variability in organic linings as the main argument for deriving an effect of dissolution on the faunal assemblage. Rather, we derive the importance of dissolution based on the the distinct variation between agglutinated/calcareous species and the fact that this has been reported as a very important processes in Arctic waters/fjords. I have rewritten the discussion to make it more clear that the variation in organic linings is not the main argument for the effect of dissolution, but is rather one element that supports our interpretation.

*6. Also regarding linings, the abundance in Fig. 5 are rather on the low side (max. ' 15 linings/g) – is this correct? Does this include all linings or just those of planispiral species? How do you make the leap between dissolution and lining presence, especially in periods that have plenty of calcareous proportions but also the highest rates of linings (e.g., 1920-1960 on Fig. 5)?*

The numbers of linings per gram are correct. Tests that were partly dissolved but still showed enough features to be identified were identified, otherwise they were counted as organic linings (the amount of partly dissolved test was very low tough, most linings showed no remnants of carbonate test). The organic linings were nearly all planispiral, but some unidentified organic tubes were also included in the count number (very few however). For thorough analysis of organic linings the samples should be prepared as playnological slides, but here we use them simply as a (rough) indication of dissolution of the calcareous assemblage.

*7. Inflow of Atlantic waters into Baffin Bay and adjacent regions are inferred in previous palaeo-studies (e.g., Knudsen et al. 2012, Boreas). How similar or dissimilar are the benthic assemblages in these studies compared to the present one.*

The benthic assemblage is very similar to that of Lloyd et al., (2012) and this has now been cited in the text This can be expected since this study is most comparable study available with regard to proximity, investigated time interval, water depth and sample preparation.

*8. The inference of nutrient levels in the fjord [section5.1,line 24onwards] is tentative in my opinion, since you don't have other palaeoproductivity indicators (TOC, d13C) to support this notion. True, there are some species in your record which indicate high flux of organic matter to the seafloor, but this is mostly *N. labradorica*. I think these inferences between dissolution, Atlantic water inflow, and nutrients should be done with greater caution, therefore.*

I agree this interpretation would have indeed be stronger with additional paleo-productivity proxies and the link with productivity is somewhat speculative. I have rewritten this section to incorporate a bit more caution with regard to the interpretation. In general we still believe that this is a point worth suggesting, in particular since these data might confirm recent research that has showed a strong link between Atlantic water inflow and (modern) productivity (Meire et al., 2017) .

*9. This figure is a bit confusing and should be modified for ease of reading. For ' example, are the abundances shown relative or absolute? What are the stippled lines (very faint!)? I suggest adding lines or points to the silhouette graphs so the number of samples/datapoints can be seen more clearly.*

We adjust the figure so it is clear these are absolute abundances. The stippled lines are merely for visual support to compare the different abundances. We add a symbols for data points.

*10. Unless I missed it, a list of all species found, including taxonomic designations, should be added to the paper.*

This list is now present and placed main this the main document (Table 1).