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Interactive comment on "Deglacial to postglacial history of Nares Strait, Northwest Greenland: a marine perspective" *by* Eleanor Georgiadis et al.

Anonymous Referee #1

Received and published: 8 August 2018

Thank you for the opportunity to review Georgiadis and colleagues paper. They present new data, including grain-size, CT, XRF, and radiocarbon, from sediment core AMD14-Kane2b from Kane Basin and discuss implications for the deglaciation of Nares Strait. They infer a major deglacial event, the opening of Nares Strait, from an IRD event and XRF geochemistry. It is a good dataset and is suitable for publication in Climates of the Past. However, there are a few important issues in the discussion and data treatment that should be addressed before this manuscript is accepted for publication.

First, I like to praise how the paper focuses on a detailed description of the core stratigraphy on depth. The inclusion of Table 2 in addition to Figure 5 make it very easy for me, the reader, to understand the stratigraphy of the core and the author's interpretation of that stratigraphy. In my view, the most important take away from this paper is

the clear description of the stratigraphy and I applaud the authors for that.

My biggest issue with the paper is how the authors make statements regarding the meaning of the data and then fit their interpretations to that model. This is particularly true for the XRF data. With the detailed grain-size data set the authors have generated, it would be much more informative to learn about the relationship between sediment geochemistry and grain-size based on Kane Basin data instead of importing conceptual models from vastly different depositional environments. This would make the results of this study much more convincing and help other researchers working in the region. At a minimum, the authors need to be clearer about what is their interpretation and what is supported by data in the results section. I recommend adding a figure showing the relationships between XRF element counts and particle size in the various lithologic units, as this relationship (or lack of relationship) is central to many of the interpretations made by the authors.

I would also like to see the authors expand their discussion to include how their data compare to another marine perspective on the Holocene deglaciation of Nares Strait by Jennings et al. (2011). Although the paper is referenced in the introduction and the discussion, the authors do not address why their age for the opening of Nares Strait is younger. I believe the two observations can be reconciled, but it is worth a discussion by the authors as Jennings et al. present faunal and stable isotope data that clearly show the change in oceanographic conditions with the opening of the strait and have a high quality age constraint above the transition at 8,328-8,528 cal yrs BP ($\Delta R = 335 \pm 85$) based on Neogloboquadrina pachyderma sinistral. I consider these to be more reliable evidence than semi-quantitative bulk-sediment geochemistry and an IRD event layer.

I have included specific comments related to each section and line comments for each section below.

Section/Line comments:

Title: As a suggestion, perhaps include the geographic location of your study site, such as “a marine perspective from Kane Basin,” to draw more attention to your article from researchers working in or interested in Kane Basin.

Abstract: The abstract is fine and includes the major interpretations made by the authors. My only general recommendation, beyond the line comments would be to include qualifiers like around before the dates. For example, changing “at 8.3” to “around 8.3” and “from 7.5 to 1.9” to “from about 7.5 to 1.9.” There is a great deal of uncertainty in your age model and it is always good to indicate that these are just estimates of the events, especially when you are not reporting the uncertainty associated with those event ages (which would be difficult to quantify with a program like CLAM).

Page 1 Line 12: For clarity, perhaps rewrite “that translate into ice sheet configuration in the strait” to “that provide new insight to the ice sheet configurations in Nares Strait.”

Page 1 Line 14: It is not clear, even after reading the paper, what you mean by “unstable sea surface conditions.” It would be helpful to clarify what this means and maybe what data it is based on.

Page 1 Line 20: Change sediment source to sediment geochemistry

Introduction: This is a nice introduction and clearly introduces the problem you are trying to address.

Page 2 Line 4: I recommend starting a new paragraph after discussing the modern observations and beginning the discussion of the Holocene observations.

Page 2 Line 6: I would suggest changing the word admitted. “It is now widely admitted” reads awkward

Page 2 Line 8: You could also include the palaeoceanographic evidence from Hall Basin (Jennings et al., 2011).

Page 2 Line 19: Include the name of the core

Page 2 Line 19: Perhaps also indicate that you are presenting radiocarbon data. For example, “sedimentological, geochemical, and geochronological data. . .”

Regional Settings: This is a nice overview of the regional oceanography and geology. I would just be careful about making the jump from regional geology to geochemical signatures for source regions. While it makes sense that Ca is probably from the carbonate rich regions and the K is from gneiss regions, we don’t have a good sense from your paper (or previous work) what the sub-ice bedrock is or what the geochemical variability of these units (like the siliciclastic sedimentary rocks found throughout the region). I would also include a call out to your Figure 2 when you discuss the regional geology.

Materials and Methods: The CT, grain-size, and XRF methods all seem appropriate and sufficiently described. However, as noted in the line comments, I do not find the conceptual framework for the XRF interpretation convincing and needs further support from data and/or references. The grain-size statement would fit much better in the results section, after a more direct comparison of the ‘grain-size sensitive elements’ to the detailed grain-size record the authors have generated. This would be the first direct observation I am aware of for Kane Basin or Nares Strait and would be a helpful observation for others working in the region.

The choice of ΔR, 240 years, seems reasonable and is similar to the recent Jakobsson et al. (2018) choice of 268 $\pm$ 82 years. I would have liked to see uncertainty included in the calibration of the dates, as the authors acknowledge the pre-bomb ΔR range could have been as high as 335 years, but I doubt that would have changed the overall interpretation, as the uncertainty with respect to the material dated in this study is likely larger than the uncertainty associated with the choice of ΔR (with 35% of the reported ages rejected in the age model). Given this uncertainty, perhaps it would be better to present the chronology in the results section, after discussing the radiocarbon results and specific justification for which ages you accept/reject.

Page 4 Line 5: Change section number from 2 to 3. Likewise, subheading numbers need to be changes from 3.2 to 3.1 and 3.3 to 3.2

Page 4 Line 8: Does u-channel need to be capitalized?

Page 4 Line 8: Define CASQ acronym

Page 4 Line 11: Define INRS acronym

Page 4 Line 27 to Page 5 Line 2: The authors need to do a better job justifying their interpretation of the XRF data. The sentence that begins "The sedimentary rocks from northern Kane Basin and Nares Strait. . ." needs to be supported by data or a reference. I am not convinced that these are the geochemical signatures of each source region otherwise. I also do not think it is convincing based on the low resolution geologic map presented as Figure 2. Additionally, the second sentence that begins "Additionally, heavy elements Ti and Zr reflect the grain size variation. . ." is often true; however, Ti and Zr can also be influenced by provenance changes. The cited references for this statement involve study areas on the Oman Margin, NW Mexican Margins, and Gulf of Cadiz (and a fourth by Correns, from 1954, which is not listed in the reference list)âĂŤnot Kane Basin. I could list other studies from different regions which show Zr variations are not related to lithic particle size, such as Phillips et al. (2014) in the Bay of Bengal. The point I am making is that I would like to see Georgiadis and colleagues make observations based on the data set they have generated from Kane Basin, rather than fitting their data to a preconceived notion. I would also argue that the authors are in a great position to do exactly that, as they have generated a detailed grain-size record to go along with their XRF geochemistry.

Page 5 Line 7: The supplementary figure could be moved to the main text, as the manuscript only has six figures. They are good data–why bury them in a supplement when you don't need to?

Page 5 Line 29: Indicate what settings you used in CLAM 2.2 (i.e. is this a polynomial

fit? Or linear interpolation?)

Page 5 Line 31 to Page 6 Line 3: This paragraph would be a better fit for the results section after a discussion of the radiocarbon results and specific criteria for rejected dates.

Results and Interpretations: As stated earlier, I think the authors do a great job discussing the stratigraphy of the core, the depositional processes that may be important in creation of the deposit, and, for the most part, saving the narrative for the discussion.

Page 6 Line 12: Can you say anything about the thickness of unit 0 given that the acoustic waves are likely attenuated? I think your interpretation is correct, I just don't think you have any evidence for the thickness of the unit.

Page 6 Line 17: Is the evidence for 'The elemental composition of the sediments is largely dominated by Ca throughout the core' based on the XRF data? If so, even after your normalization, semiquantitative count differences between elements don't necessarily mean weight percent differences. I think you would need a more quantitative method and/or a calibration to make that statement.

Page 6 Line 18: Why does K reflect source changes and not particle size changes. This needs to be explained further and supported by data and/or references.

Page 7 Line 4-5: Perhaps clarify that the larger particles are relative to overlying units, as it seems Unit 1B is finer grained than Units 1A and 1C.

Page 7 Line 27-30: Indicate that these are likely sources or your interpreted sources.

Page 8 Line 5: I would change "A significant portion of these sediments derived from eastern Kane Basin gneisses" to something like "Relatively high K counts likely reflect an increase contribution from eastern Kane Basin gneisses."

Page 9 Line 22-25: Like other comments, at this point state the geochemical evidence for your interpretation. The contribution of northern and/or eastern carbonates doesn't increase slightly, Ca counts increase slightly which is likely consistent with an increased contribution of northern and/or eastern Kane Basin carbonates.

Page 9 Line 27: What is Unit E? Do you mean Unit 5?

Page 10 Line 5-6: Make sure that you are discussing the data. There wasn't a change in the delivery of gneiss material. There was a change in K and Ca, which you interpret as a change in provenance. But as you have already discussed, there is also a big change is sediment grain-size.

Page 10 Line 15: The authors indicate that two mollusk shells are younger than expected and claim that they were likely remobilized by bioturbation. In one case this would indicate movement of about 80 cm downward in the core (UGAMS-24308). Is this a reasonable interpretation for a mollusk shell? Are there any references that could be used to support that interpretation? Would the simpler interpretation be to accept the younger dates and attribute the older dates to reworking, as you do for the older part of the age model?

Discussion The review of the terrestrial data and comparison to your core data is good. As stated in the intro, I think this paper is missing a comparison to the other important marine perspective–from Hall Basin. It is also my opinion that many of the interpretations are a bit speculative in the discussion section, such as the influence of the 9.3 ka cold event, but I think, because it is in the discussion only, it is okay. However, it detracts from one of the main and well-supported findings of the paper (in the same section), that your region of Kane Basin was deglaciated by 9 ka and that provides an important constraint for your Figure 6. In this instance and others, you might consider focusing your discussion on the major and best supported findings so that they are the main focus of the reader's attention.

Page 11 Line 16: Not necessary for this paper as it was only available online after you submitted your paper to Climates of the Past, but you may also be interested in a recent cosmogenic study by Reusche et al. (2018) for your future work.

Page 11 Line 24: Do you mean Figure 5?

Page 13 Section 5.3: I do not find the presence of old foraminifera convincing evidence for a Northern Nares Strait sediment source. Obviously, there is something complicated happening with radiocarbon, as 35% of your reported ages are rejected and not used in your age model. Part of your argument may be that these are on mixed benthic forams; however, you have other mixed benthic forams which are clearly too old elsewhere in you core (e.g. SacA-46003) without similar sedimentological indicators. I think this part of the discussion could use a little work and dive a bit more into the uncertainties of your chronology and the constraints from the Hall Basin record (and its uncertainties). Page 13 Line 32 and Page 14 Line 4: You use the term 'gneiss signal.' As I've been critical of in other parts of this review, you present no data that K is a proxy for gneiss or sediment sourced to a specific region.

Conclusions:

Page 15 Line 17: Change section number from 5 to 6.

Page 16 Line 2: Authors claim this is the first non-land view point of the deglaciation of Nares Strait. This is not true, as a northern perspective of the events was documented by Jennings et al. (2011) using a marine sediment core from Hall Basin. The authors need to change this to a southern perspective, or a Kane Basin perspective.

Figures:

Figure 3: In the caption, indicate that the age model is a polynomial, spline, or linear interpolation (whatever you used) and indicate if the shading is the one sigma or two sigma uncertainty of that model fit.

Figure 5: In the caption, indicate that the plotted radiocarbon dates are only the ones you accepted to use in your age-depth model and make a call out to Table 1, so it is clear to the reader that there are other radiocarbon data.

References Cited in Review:

Jakobsson, M., Hogan, K.A., Mayer, L.A., Mix, A., Jennings, A., Stoner, J., Eriksson, B., Jerram, K., Mohammad, R., Pearce, C., Reilly, B., Stranne, C., 2018. The Holocene retreat dynamics and stability of Petermann Glacier in northwest Greenland. Nat. Commun. 9, 2104. https://doi.org/10.1038/s41467-018-04573-2

Jennings, A.E., Sheldon, C., Cronin, T.M., Francus, P., Stoner, J., Andrews, J., 2011. The Holocene history of Nares Strait: Transition from glacial bay to Arctic-Atlantic throughflow. Oceanography 24, 18–33.

Phillips, S.C., Johnson, J.E., Giosan, L., Rose, K., 2014. Monsoon-influenced variation in productivity and lithogenic sediment flux since 110 ka in the offshore Mahanadi Basin, northern Bay of Bengal. Mar. Pet. Geol., Geologic implications of gas hydrates in the offshore of India: Results of the National Gas Hydrate Program Expedition 01 58, 502–525. https://doi.org/10.1016/j.marpetgeo.2014.05.007

Reusche, M.M., Marcott, S.A., Ceperley, E.G., Barth, A.M., Brook, E.J., Mix, A.C., Caffee, M.W., 2018. Early to Late Holocene Surface Exposure Ages From Two Marine-Terminating Outlet Glaciers in Northwest Greenland. Geophys. Res. Lett. 0. https://doi.org/10.1029/2018GL078266

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