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Interactive comment on "Glacial sedimentation, fluxes and erosion rates associated with ice retreat in Petermann Fjord and Nares Strait, NW Greenland" *by* Kelly A. Hogan et al.

Kelly A. Hogan et al.

kelgan@bas.ac.uk

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Below we list our responses to each of the comments and details of the changes made to the manuscript. Reviewers' comments are listed in sequence followed in each case by our responses.

Review: Anonymous Referee #1

This is a well written and illustrated paper that presents new data on glacigenic sediment fluxes and erosion rates associated with the retreat of Petermann Glacier in NW Greenland. The paper is very appropriate to The Cryosphere. However, there are a

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few issues that need to be addressed:

1. You state that this is the first comprehensive investigation of the glacial-sedimentary infill of a major fjord system in Greenland (last para of Introduction and first line of Conclusions). I think you could do with justifying this claim a bit more robustly. For example the lead author has herself published on a similar topic from Disko Bugt/Jakobshavns Isbrae (e.g., Hogan et al., 2012 Marine Geology) and there has been previous work carried out on seismic stratigraphy in East Greenland fjords (Scoresby Sund – e.g., Uenzelmann-Neben, et al. 1991: Quaternary sediments in Scoresby Sund. East Greenland: their distribution from rellection seismic data. In Moller et al. (eds.): The Last Interglacial-Glacial Cycle: Jameson Land and Scoresby Sund, East Greenland). It may be that the present study significantly supersedes this earlier work but more justification/comment on this is required.

We thank the reviewer for this comment and have addressed it by adding text to several places in the text. We have now stated that the difference in this study is the combination of high-resolution acoustic methods with a high survey-line density INSIDE a major Greenland fjord and we have included references to previous geophysical studies of Greenland fjord infill: Uenzelmann-Neben et al., 1991; Andrews et al., 1994; Gilbert et al., 1998, 2002; Ó Cofaigh et al., 2001; Evans et al., 2002. An earlier draft of the paper actually included more information on the unique-ness of this high-resolution survey but that was cut at some stage!

2. Line 132. Explain why a delta-R of 268+/- 82 was chosen.

We have used delta-R of 268+/- 82 directly following the recalibration of terrestrial radiocarbon dates by Jakobsson et al. (2018), which we refer (and cite) to on lines 137-138 of the original manuscript. The reported 1-sigma ranges are those calculated by Jakobsson et al. (2018), available as this paper is from the same authorship group, and were included to give age ranges for the glacial erosion estimates. The delta-R of 268 +/- 82 used by Jakobsson et al. (2018) is based on the five nearest radiocarbon-

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dated shells to Nares Strait as reported in Coulthard et al. (2010), however, because the delta-R is derived from Jakobsson et al (2018) which is cited directly we do not think it necessary to add to the text to address this comment. Coulthard, R. D., Furze, M. F. A., Pieńkowski, A. J., Chantel Nixon, F. & England, J. H. New marine ΔR values for arctic Canada. Quat. Geochronol. 5, 419–434 (2010).

3. I suggest you change Sub-heading 4.2 'Petermann Fjord' to something a bit more informative, particularly as sub-heading 4.1 is 'Seismo-acoustic facies and depositional environments in Petermann Fjord and Nares Strait'.

We agree that the sub-headings could be more informative and have now changed section 4.2 heading to "Glacial marine sediment infill in Petermann Fjord and Nares Strait" (Sections 4.2 and 4.2 were merged in response to R2 comment #10). We believe that this new sub-heading describes the contents of these paragraphs more fully but does not wander into interpretation of the age of the sediments as post-glacial, Holocene or post-LGM, which would contradict comment #5 from this reviewer. All of the infilling sediments were deposited in a glaciomarine environment so we think this new sub-heading is appropriate.

4. Lines 230-233 and Line 316. You state that subglacial tills are deposited as sediment gravity flows. But gravity flows are NOT subglacial tills. For a recent treatment see D.J.A. Evans 2018 – 'Till'. Please change.

We see the confusion in the text regarding these statements and have now clarified it in both places by removing the term subglacial and adding a description of the depositional process (see tracked changes document). The confusion arises because subglacial material was deposited at the grounding line, and some of this subglacial material was remobilized in sediment gravity flows in front of the grounding line. Acoustically, there is no difference/boundary between these facies so they appear continuous in our profiles, however we recognise the different modes of deposition. We thank the reviewer for forcing us to look carefully at these statements!

5. Lines 369-371. You suggest that pre-LGM sediments most likely do not occur citing as support the presence of glacially-sculpted bedrock surfaces and referring to Jakobsson et al. (2018). I think this is pretty thin evidence. For example, pre-LGM sediments could be preserved locally within bedrock depressions and/or be too thin to be resolved by your seismic system(s). At the very least I would suggest the inclusion of a caveat acknowledging this would be appropriate here.

This comment is thought-provoking. We take the point that pre-LGM sediments can be preserved in bedrock depressions but we note that there are few large, deep basins filled with hundreds of meters of sediment in our study area (see Fig. 11a) as is the case with many of the cited examples that include pre-LGM sediments. Indeed, the bathymetry shows a sculpted seafloor for almost all of the fjord area which almost certainly was sculpted by ice flow during the last glacial (see Jakobsson et al., 2018). Furthermore, our calculations only include sediment that is above this "LGM surface" (which could be till or bedrock) meaning that, for the most part, our volume estimates should not include pre-LGM sediments. We do accept that any pre-LGM sediments could be too thin to be resolved in our SBP profiles and this is a potential source of error. However, if there are not many bedrock basins where pre-LGM sediments may be preserved, and/or pre-LGM sediments are thin then, volumetrically, they probably do not make up a large component of the infill and this assumption is appropriate for our study. However, as we acknowledge in the text, there is no way to distinguish these sediments from Holocene post-glacial sediments in our data and that there is no way to quantify what kind of error this might add to our estimates, which is unsatisfying. As such, we have added the caveat that pre-LGM sediments may be contributing to our volumes and that this is a potential source of error. Note that this text has been moved to the methods section as per R2 Comment #13.

6. Lines 372-373. You state that glaciomarine sedimentation seaward of the grounding line has two components the first of which is that of coarse or mixed material delivered to the grounding zone as subglacial deposits. But how is this material actually

deposited? It is glaciomarine sediment deposited seaward of the grounding line, so although it might be delivered to the grounding line subglacially it is not deposited seaward of the grounding line as a subglacial till.

We accept this comment and have modified the sentence to say that the material is delivered to the GL subglacially but deposited seaward of this as gravity flows (see tracked changes document).

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