The authors' replies on the comments are written in red, bold and italics

Interactive comment on "Last Glacial ice-sheet dynamics offshore NE Greenland – a case study from Store Koldewey Trough" by Ingrid Leirvik Olsen et al.

Anonymous Referee #2

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The manuscript provides a multi-method dataset comprising geophysical, sedimentcore and geomorphological data from the little studied area of the NE Greenland continental shelf. Therefore, our understanding of ice sheet history and associated icedynamics and sediment processes in this region is poorly constrained. Therefore, a study on this understudied region is welcome and should garner widespread interest. The disappointing aspect of the study was the lack of chronological constraints on the geomorphological dataset and interpretations even though sediment cores were part of the study. Apart from the middle shelf coverage, the swath bathymetry dataset and the interpretation of it seems to be identical to that published in Laberg et al. 2017, but the sediment-core data, middle shelf geophysics and interpretations are new. The identification of the landforms in swath bathymetric imagery does not appear to be correct. The authors do not make enough use of the sediment core analyses or data, and interpretations need to draw on this data more as well as the literature. The sedimentcore aspect of the study could be expanded as core information in NE Greenland is extremely limited in published work to date. The discussion needs to be developed further and there needs to be a natural flow and emergence of a central argument between paragraphs that uses the geomorphological and sedimentological evidence. At times, there does not appear to be a natural link between paragraphs and some paragraphs appear to be dropped in without reference to previous paragraphs. ***We acknowledge the feedback from the referee and tried to address the issues mentioned in our revision. Please see below for details.***

Section 1-3 Is this paper 'contributing to validation and improvement of numerical models' i.e. will this be examined in this paper based on the data and interpretations presented? If not, then this is a misleading statement and should be altered or removed. I do not see any point in making the observation that "It has been suggested that the northeastern part of the GIS reached the inner or middle parts of the continental shelf during its maximum extent during the last glacial (see Funder et al. 2011 for a review)" as more recent studies of Evans et al. 2009, O Cofaigh et al. 2004, Arndt 2018, Arndt et al. 2015, 2017, Arndt and Evans 2016, and Laberg et al. 2017 show quite clearly that ice went beyond the inner and middle shelf. The authors make this same point so there is no need to repeat an outdated debate. Include Evans et al. 2009, O Cofaigh et al. 2004, Arndt et al. 2015, and Arndt and Evans 2016 in the studies that have indicated ice was much more extensive on the NE Greenland shelf than the original summaries of Funder et al. 1998 and Funder et al. 2011 implied.
We have altered the sentence regarding `validation and improvement of numerical models', emphasizing the need for paleo-reconstructions. Furthermore, we removed the "previously suggested maximum extent of the GIS in NE Greenland" by Funder et al. (2011), replacing it with more updated studies as suggested by the referee.

The authors need to highlight how the swath bathymetric data presented in this paper differs to that presented in Laberg et al. 2017, and then detail how this study is different to that of Laberg et al. 2017. The same data for the outer shelf is presented again and there needs to be a clear statement or discussion differentiating what is published and what is new. I suggest that the authors add a section detailing what is known about the swath bathymetry and sub-bottom profiler data and implications for ice sheet history and sedimentary processes of the Laberg et al. 2017 study.

We rewrote the introduction to chapter "4.2 Submarine landforms", providing information about what part of the data is new, and what has been previously published in Laberg et al. (2017). In the re-submitted version of the manuscript we clarify which part of the data set from Laberg et al. (2017) we have re-interpreted and why.

Section 4.1 The range of analyses performed from geochemistry, sediment grain size, shear strength, etc. are outlined in the paper, but there is no reference to the actual data within the description of the lithofacies o, even in the interpretation of the lithofacies or the discussion. For instance, the 'magnetic susceptibility and Ca/Sum ratio vary between each core, with the highest in HH17-1326 and lowest in HH17-1328. Wet bulk density and shear strength are generally high: : :'. This is vague and does not serve the paper well. There is no subsequent use of much of this detailed data when it comes to the discussion of the glacial history later in the paper. I am still uncertain as to the point of including the magnetic susceptibility, XRF and wet bulk density data in this paper beyond including them for the sake of it. ***We agree and have, therefore, taken out the XRF core scanner and shear strength data in the re-submitted manuscript.***

The interpretation of Facies 3 should explain what is meant by 'open conditions' and explain how the 'outer ice-proximal setting' inferred to be the location of the depositional environment differs from that envisaged for Facies 4. The paper notes the similarity of Facies 2 and 1 apart from the presence of IRD. Does this merely reflect the stochastic behaviour of icebergs rather than anything to do with permanent sea-ice or 'increased influence of drifting ice' in the sense of increased iceberg calving. The differences between the facies is essentially down to the vagaries of iceberg processes. ***We rephrased these paragraphs and hope it is more clearly now (see page 5, lines 4-7, 15-19 and 28-32).***

Section 4.2 I am not convinced that there are MSGL in Figure 4 and 6. The features shown in Figure 5 appear to be lineations rather than MSGL and the description of them only refers to their length as >1.5 km. ***We understand the reviewer`s point. However, we keep our suggestion that the landforms are fragments of/partly buried MSGLs, because the lengths/width ratios exceed 10:1 (cf. Clark, 1993).***

Do sub-bottom profiler records across the GZWs exist in order to rule out that they are bedrock sills? ***We have included the publication by Petersen et al. (2015) showing that there is a thick Neogene sedimentary succession offshore NE Greenland, ruling out bedrock sills.***

Figures 4 and 5 are misleading as the recessional moraines and crevasse squeeze ridges are merged and have the same colour scheme, and it is difficult to distinguish where the crevasse-squeeze ridges are located. ***Corrected.***

I am not convinced that some of the ridges represent a rhombohedral network indicative of crevasse-squeeze ridges. There appears to be little difference between the recession moraines and the crevasse-squeeze ridges apart from slight differences in morphology that might be linked to variations in grounding line processes and behaviour. The CSR appear to have a limited distribution and are not pervasive or widespread implying that the interpretation of 'surging' is unlikely and that they are more likely to be a localised feature maybe related to complex pattern of recessional moraines linked to ice-margin processes during standstill and retreat. Therefore, the idea of surging behaviour may not be correct and that the landform assemblages only record variable rates of grounding ice margin retreat and stabilisation. ***We appreciate the extensive comment of the referee! Based on that, we revisited the data set and changed our interpretations from crevasse-squeeze ridges and multi-keel ploughmarks to saw-tooth moraines. Therefore, we rewrote the part of the result chapter regarding these specific landforms as well as the following discussion chapter.***

If indeed these features are CSR, why do they have to be associated with a surge rather than an advance/acceleration of an ice stream (linked to mass balance) and formation of basal crevasses due to tensile stress and ice break-up as it steps back to a stillstand position? Also, if it's a surge or even a simple readvance/acceleration of an ice stream, why aren't these features more widespread across the trough floor as presumably, a wider area would stagnate? The limited distribution implies a more complex recessional moraine pattern linked to complex ice retreat in some areas. ***See reply to comment regarding rhombohedral network and crevasse-squeeze ridges, above.***

I'm not convinced that the features identified as multi-keel iceberg ploughmarks is correct as they appear identical to the recessional moraines in Figure 4, 5 or 6. How would you even differentiate between a multi-keeled iceberg ploughmarks and the intervening ridges they create from those that are recessional moraines? ***See reply to comment regarding rhombohedral network and crevasse-squeeze ridges, above.***

Section 5 The authors state that "We propose that the Store Koldewey Trough was filled by grounded ice originating from the area presently covered with the Storstrømmen ice stream (Fig. 8A). This implies that the northeastern sector of the GIS reached a thickness allowing the ice stream to flow unrelated to the underlying topography, including the mountain ranges between present day Storstrømmen and Germania Land." This is speculative statement on its own. On what basis or geomorphological evidence are you making this assertion? Why wouldn't Storstrommen have preferentially flowed along and filled Dove Bugt Trough? The authors then go on to note that "An alternative interpretation is that Store Koldewey Trough had a much smaller drainage-basin, limited to Germania Land (Arndt et al., 2015). However, based on our data, including the observations of mega-scale glacial lineations, recessional moraines and grounding zone wedges, we favor the interpretation of Storstrømmen filling Store Koldewey Trough during full glacial conditions based on the volume of ice needed to fill a trough of this magnitude. We propose that the ice sheet thinned and that the underlying topography controlled the direction of ice flow during a late phase of the last glacial, i.e. that the ice flow from the interior of the GIS was directed to Jøkelbugten in the north and Dove Bugt in the south (Fig. 8B)." What is being proposed is speculative. Therefore, the discussion on the topographic and non-topographic controls on ice stream flow pathways, source and development from one to the other needs to be developed further. ***We have elaborated on this topic by providing information on the altitude that the Storstrømmen Ice Stream had to overcome to drain into Store Koldewey Trough (single peaks of 500-900 m), complimented with modelling results of paleo-ice sheet thickness on Germania Land during LGM (1000-1500 m; Fleming and Lambeck (2004) and Heinemann et al. (2014)).***

Why does the retreat of the grounded ice margin have to be 'slow' between stillstands? What evidence is used to support this assertion? There are no radiocarbon dates from the study cores that constrain ice stream retreat, so it is not possible to conclude the relative rate of retreat. Evidence from Antarctica shows that ice streams can abandon their groundling zone very quickly and then retreat at variable rates to the next stabilization point. It is worth exploring the issue of terrain factors (e.g. trough dimensions, trough depth distribution, underlying bed slope, etc.) modulating externally driven ice sheet retreat. The authors should consider the literature on GZW morphology and volume as an indicator of the relative length of time that the grounding line remains stable in one place (e.g. Dowdeswell et al. and Batchelor et al.). The authors need to develop the discussion in terms of what the smaller recessional moraines versus the larger GZWs mean for ice stream retreat rates, length of time of stabilisation and ice margin behaviour during temporary stillstands. For instance, the smaller moraines may be winter advances during stillstand.
These are many good suggestions that we appreciate! The terms "slow" and "episodic" retreat have been introduced by both Ó Cofaigh et al. (2008) and Dowdeswell et al. (2008) discussing styles of ice retreat accompanied with the formation of recessional moraines and grounding zone wedges, respectively. We wish to continue using these terms and have, therefore, rephrased the paragraph, hopefully making our use of terms more clear to the reader. Furthermore, we provide a more in-

depth discussion of the relationship between local trough geometry and locations of the GZWs, as well as the sedimentary environments regarding the different lithofacies.

The authors note that "We interpret the break-up and retreat of the GIS to have happened in two stages; initial retreat by breaking up and calving of grounded ice due to eustatic sea level rise caused by melting of ice at lower latitudes (Lambeck et al., 2014) (Fig. 9: Stage 2) and a second phase of melting driven by ocean warming, possibly due to the onset of inflow of intermediate water masses. The latter is supported by the occurrence of meltwater-channels and laminated sediments interpreted to be a result of excessive meltwater production in the middle and inner parts of the trough". On what basis, evidence or studies are you making this assertion for this region of Greenland, particularly the impact of sea level rise or inflow of intermediate water masses? What intermediate water masses are you referring to? There is no sediment evidence such as iceberg rafted lithofacies recorded in the cores to support iceberg calving and margin retreat due to sea level rise. Meltwater derived sediment facies cannot be used the defining piece of evidence indicating ocean warming retreat as the ice sheet will always produce and discharge meltwater due to the simple fact the ice at the subglacial bed is at pressure melt point. In fact, meltwater sediments will be deposited even when sea levels are rising and causing the ice margin to retreat.
The impact of these external factors will depend on the relative balance between atmospheric warming, precipitation, ocean warming and sea level rise, but stating that sea level rise causes a first stage of retreat is too simplified. For instance, studies in Antarctica show that maximum grounded ice extent in some sectors of the Ross Sea occurred during deglaciation even though there was atmospheric and oceanic warming and sea level rise because precipitation had a more dominate impact on mass balance, but eventually ocean factors dominated to cause retreat. How do you know there are two phases to the retreat of the ice sheet in this region without age constraints? In fact, the geomorphology implies more than two stages to ice sheet retreat. It is also worth noting that ice sheet retreat history is not merely a simple function of sea level rise, ocean warming and atmospheric warming but also due to terrain factors that can modulate ice sheet response and the rate of response to these external factors. The authors note the importance of the terrain for ice sheet retreat but do not really consider the literature that have looked at the impact of terrain factors on ice stream retreat. For example, Stewart et al. 2012 and Livingstone et al. 2012. These studies show the importance of trough width and depth on the rate of ice sheet retreat and try to quantify rates of retreat. ***Following the comments of the referee, we revisited the paragraphs mentioned above, and concluded that additional proxy information is needed to identify the driving forces causing the retreat of the GIS. In consequence, we have removed the paragraph.***

The authors note that "Based on the varying numbers of GZW0s we suggest that retreat/ readvances of the ice streams offshore NE Greenland occurred asynchronously." Whilst I agree that it is possible that ice streams over such a large region as NE Greenland will experience asynchronous behaviour, I am not convinced of the evidence that is presented for this assertion. The data from Norske Trough, Westwind Trough and elsewhere do not provide a complete coverage of the respective areas and it is possible that there may be GZWs that exist, but have yet to be discovered undermining the suggestion that the number of GZWs indicates asynchronous ice stream behaviour. Secondly, without chronological constraints on regional ice stream behaviour during deglaciation or the ages of GZWs then the assertion of asynchronous behaviour is speculative. ***We agree that there might be undiscovered GZWs in Norske Trough and Westwind Trough. We rephrased this paragraph, focusing on the presentations of facts, rather than speculating on differences in deglaciation dynamics between different troughs.***

The authors note that "The present sub-glacial topography of Storstrømmen consists of a reversed bed slope, accompanied by a floating ice tongue (Hill et al., 2018). Thus, a potential future response to increased ocean warming could result in episodes of rapid retreat as the ice front undergoes thinning and/or ice tongue collapse. Such episodes are believed to cause a dynamic response up-glacier, resulting in an accelerated ice flow, contributing directly to sea level rise (Hill et al., 2018)". It is not entirely clear how this statement links, and is relevant, to the previous paragraphs discussing ice sheet behaviour during deglaciation. ***We see the referee`s point and have removed this paragraph.***

The authors equate 'surge' behaviour with an ice stream. Why does the ice stream have to surge rather than simply readvance/accelerate? I am not convinced that the features they describe are crevasse-squeeze ridges but if they are then the section needs to be developed further to explain and justify why the ice stream surges as opposed to accelerate and readvance. The authors also need to explain why the CSR are limited in their spatial extent and distribution within the swath bathymetry dataset and why they have a close association with the GZW and recessional moraines. ***The landforms interpreted as surge-related landforms in the first version of the manuscript have been re-interpreted in the resubmitted version (see above), making the concept of surging irrelevant for the manuscript.***