**Olsen et al.: Last Glacial ice-sheet dynamics offshore NE Greenland – a case study from Store Koldewey Trough**

This manuscript is a considerably improved re-write/re-submission of a previously reviewed manuscript. The data are more clearly presented, the geomorphological interpretations are more logical (both with respect to morphological indicators and interpretations of glaciodynamic contexts/conditions for particular landform types), and the Discussion is better formulated. That said, I still have some concerns about the rigour of data interpretation and justification for some of the elements of the Discussion.

*Sediment cores*
The presentation of core data seems limited. The text reporting is clear and logical but I wonder if more use of valuable core material could have been developed. Was any grain size sorting analysis done? – you have quantitative data on grain size fractions. Why are clast proportions/abundance not quantitatively reported, since you've sieved clasts out as a specific size and so presumably have these data? What are the colour changes related to – organic content? Is 'high' or 'low' density just on relative terms (to the rest of the core/s) or compared to literature typical values? – does 'high' bulk density necessarily mean over-consolidation of tills?

Facies 3 & 4 lack any references to other literature that would guide or support your interpretations (and the interpretations of the other facies are also sparsely referenced). Yet there is a wealth of work on proximal to distal glaciomarine sedimentary characteristics and physical properties. I suggest (all of) your interpretations should be grounded in available literature.

Could facies 3 mark the calving front of an ice shelf (melt out of englacial debris), as often suggested for first emergence of IRD after limited sub-ice shelf sedimentation in Antarctic deglacial/glaciomarine facies succession models? Are there other indicators that IRD would be from freely floating icebergs on an at least partly open ocean?

*Geomorphology*
The lineations are sparse, and I still question the 'mega-scale' interpretation; interpretation of an ice stream footprint shouldn't rest on one ratio, but rather a whole suite of landform and landform assemblage observations. Flutes on a valley glacier forefield can have elongation ratios of >10:1 – it doesn't make them msgls. At best, the evidence for fast flow here is limited to the outer shelf, and the discussion of this system should reflect the distribution of these limited indicators.

GZW A looks no different, morphologically, to the first two larger moraines inland of wedge A (about 15 km and then a further 5 km) – based on their appearance in Figure 5. Why interpret it as a wedge?

I would think there's some value in discussing overall trough morphology (orientation, depth, width, tributaries compared to other troughs along this margin), either as part of the geomorphological results or within the Discussion (part 5.1). This trough seems, from Fig 1, to have an abrupt start and a lack of obvious feeder tributaries – how does this relate to the discussion about the source of ice to the trough, or the flow velocity, sediment flux, erosional vigour?

*Discussion*
The Discussion is better focussed and internally logical, but some passages still are rather underdeveloped. Section 5.2 (dynamics during deglaciation), in particular, comprises a set of paragraphs that clearly fit within this theme but don't really connect to or build on each other, they just appear as discrete ideas. Try to better weave a discussion together from paragraph to paragraph.

The attempt to quantify grounding line landform formation times and retreat rates is a useful addition to the work, though the authors MUST acknowledge the assumptions and caveats implicit in the approaches that they take, and give ranges for their estimates that reflect the uncertainties in the approach; it is difficult to achieve much better than order of magnitude results. The Abstract and Conclusions are too definitive and the Discussion neglects important caveats. The use of any sediment flux makes considerable assumptions about sediment supply, mode of transport, ease of transport, thermal regimes, ice flow velocity. Are the sources you take your upper and lower fluxes from describing glacial systems that would be appropriate analogues for Storstrømmen? How well do these earlier studies really know (measure? infer? with what independent assumptions?) what their sediment fluxes are? Similarly, the interpretation of some types/arrangements of recessional moraines as annually forming is a major and controversial assumption. Annual formation isn't even straightforward to conclude (debate still continues) in classical regions where there are annually-resolved varve chronologies to inform the retreat pattern and rate. It is inappropriate to simply take this assumption without at least acknowledging the debate – and, preferably, discussing the validity of the choices you make.

I don't think that blindly applying conceptual interpretations of retreat *rate* like 'slow', 'rapid' or 'episodic' based on data that is inherently pattern-based rather than chronological, while acknowledging that you lack any chronological constraint, is especially constructive. Wishing to 'continue using the terms' put forward from previous conceptual work isn't a valid reason to do so – are they appropriate, given your data? Can you use your own evidence to evaluate whether these conceptual models are applicable, rather than just 'choosing' to apply them? I think you have more to say from your own data than just adopting a term that doesn't adequately summarise the range of retreat behaviours you see. Regardless of the interpreted retreat *rate* terminology, I would suggest it's more interesting that in a trough system, commonly expected to host well-formed lineations and retreat from wedge to wedge, here you have a record dominated by retreat landforms of various morphologies, marking different grounding line sedimentation processes and/or rates, different grounding line durations, and that overall the magnitude of the retreat events is rather small. The atypical landform assemblage for a trough setting is more interesting to explore than applying a conceptual model of rate that you can't say much about.

**Line edits**

Title: ice-sheet is hyphenated here, but nowhere else in the text.

*Abstract*
p1 line 10-12: re-consider the phrasing here in light of uncertainties/assumptions in calculations (see main comment in Discussion text).

p1 line 13-14: what evidence do you have that ice <u>retreated</u> directly across Store Koldewey Island and Germania Land? You can argue that at the ice sheet's maximum extent, ice was sufficiently thick to flow across this topography, but in a late stage of deglaciation? Isn't it more plausible that the high ground forced flow paths (and retreat) around the topography? The latter is in fact what you conclude. This part of the abstract should be revised.

*Main text*

p1 line 20: "…GIS is presently drained…"

p2 line 14: explain why an absolute chronology is relevant to the previous sentence: chronology enables us to understand rates of change, while absolute ages let us tie retreat events in with external forcing (climate or ocean changes)

p2 line 17-19: do these cosmogenic dates record ice retreat, as in movement of the ice *front* landward of the island, or do they record ice sheet *thinning?*

p3 line 1: it deepens seaward but with bumps and dips along its length – it's not a smooth/steady deepening.

p3 line 5-8: while interesting, it doesn't seem relevant to the rest of the paper that this ice stream has displayed two recent surges.

p3 line 20: suggest ending this section with some sort of motivation statement for the rest of your work, or introductory statement to what you've done that builds on these previous studies that you mention.

p3 line 33: suggest here you comment on which data have already been published in Laberg et al, and which are new (i.e. move from p5 line 36-38). There also is a bit of a methods gap between this paragraph and the next, where you explain GZW volume estimates, despite not having mentioned that you have / how you have recorded GZWs. I think a sentence or so stating that you've mapped landform outlines or crestlines is needed, and perhaps the basis for how you've interpreted the environmental origins and how/why you've re-interpreted some earlier published ideas (e.g. assessment of size, shape, arrangement, sedimentary setting…)

p3 line 35: what do you mean by 'box volumes'? You've assumed a rectangular cross-profile? On what basis? Is this more valid than an asymmetric triangle?

p3 line 38: you could refer to other works that have followed a similar approach…

p4 line 2: you could note that this is a common problem in cold polar waters close to ice sheet grounding lines (relatively little life at the grounding line and low carbonate preservation subsequently (dissolution)) – plenty of other Greenland and Antarctica studies suffer the same limitations

p5 line 6: 'more dominant component AT the expense of…'

p5 line 24: refer to your sand-silt-clay data, specifically, to support that facies 1 is coarser than facies 2… although, looking at these plots, the coarser nature of facies 1 is not particularly convincing.

p5 line 26: suggest you note in the facies 1 and facies 2 observations that these two facies alternate in two of the cores – there may be more than one occurrence of each/either facies in the core succession.

p5 line 35: both Fig 4 and Fig 5 should be referenced here, not only Fig 5. E.g. '…data from the middle (Fig 4) and outer (Fig 5) shelf of…'

p6 line 14: what data did Laberg et al interpret wedges A-D from? Presumably not all multibeam, since you present a new block here? Seismic?

p6 line 17-18: can you give more information on how you calculated these volumes? Did you choose just one cross-profile per wedge, or several, and on what basis? From the multibeam, wedge C looks considerably larger than A, more comparable to B.

p6 line 31-32: a figure and more detailed presentation and discussion is warranted here if there are genuinely two superimposed sets of moraines. I can't see that this is evident from either the multibeam panel or the mapped interpretation of moraines in Fig 5. On what basis do you interpret superimposition? If there is, in fact, one group that sits clearly on top of another, then we must interpret that there's been a readvance: you would have one retreat assemblage buried under another. And in that case, this finding would warrant further discussion.

p7 line 10: describe or explain why you find the sawtooth-like pattern incompatible with your earlier interpretation.

p7 line 14: '…identified along the … sidewalls' sounds like channels are running parallel to the walls of the trough. These simply occur at the periphery of your data coverage, and cut obliquely through other landforms. Suggest you rephrase.

p7 line 37: either break the sentence after the list of lithological units ('… and 1 (Fm(d)). It occurs at all…') or insert 'and' before 'it'.

p8 line 10-13: suggest you switch these two sentences around, it flows more logically from the previous paragraph to begin with the till, and then what rests on top of the till.

p8 line 25: 1000-1500m thick, or high? (i.e. surface altitude or ice thickness?)

p8 line 30: since Storstrømmen is an outlet of the contemporary NEGIS, it sounds strange to talk about this as a 'similar flow feature'. Can you instead emphasise here that the disregard for topography appears (from your results) to be a characteristic of both the palaeo and contemporary NEGIS? And be specific about exactly where this independence from topographic steering occurs within the NEGIS today.

p8 line 31: 'a palaeo-ice stream' – can you be specific, which one?

p8 line 34-38: do Arndt et al propose a more restricted ice margin as well as a limited drainage basin (supply), or do they also suggest shelf-edge glaciation? If they envisage shelf-edge glaciation, then your counter-argument ('we have evidence of shelf-edge glaciation') isn't really sufficient.

p8 line 37: can you be more specific about the volume of ice required, and compare to what Germania Land could sensibly supply?

p8 line 42-45: I don't think these opening sentences really add anything useful.

p9 line 5: the phrasing here makes it sound like Store Koldewey also has a reverse slope. The fact that it doesn't is surely an important reason for any contrast in behaviour. You set up a 'problem' here that isn't really one. I would revise this paragraph as a commentary on the rather uncommon situation of having a seaward dipping trough that has led to a rather stable retreat pattern, supplemented by local trough shallow/narrow points, rather than making this more of a puzzle than it really is.

p9 line 13-14: what do you mean by 'had a more dynamic response to…'? Rephrase to say something direct, this is vague.

p9 line 16: 'local trough geometry' (typo, through)

p9 line 22: where are the other 2 wedges that Batchelor & Dowdeswell find here, and why do you not include those in your reconstructions?

p9 line 20-27: this paragraph doesn't seem to go anywhere. What do you interpret to be the significance in the number of wedges recorded in different troughs? Are the single wedges in Norske & Westwind Troughs at the shelf break? What would be the implications for your work if these formed during the Younger Dryas?

p9 line 35: which West Antarctic ice stream? Or do you mean ice sheet?

p9 line 37: these values use the upper sediment flux rate. Using the lower flux would give you an order of magnitude longer formation time, i.e. 1300, 7400 and 1500 years. Is there enough time available for retreat across this shelf, with those standstill durations? See also main comment about sediment flux assumptions.

p9 line 40: this passage must better reflect the debate about whether recessional moraines can be interpreted as annual or not.

p9 line 50-52: this sentence either could be removed (since you don't put it in the context of your results) or should reflect the vast literature on grounded to open marine (deglacial) sedimentological facies. Picking a random three papers that have studied this succession is rather meaningless.

p9 line 6: masks

p9 line 7-9: Prothro et al (Marine Geology) discuss the distance for rainout of basal debris distal to the grounding line – they find it to be extremely short.

p9 line 25: it is intriguing that the sediment drape only occurs across the inner-middle shelf, and not the outer parts that deglaciated first. Why do you think this is? Is this a supply or a preservation/deposition question?

p9 line 30: what is the significance of facies 1 & 2 alternating in the two outer cores? How does this affect your environmental interpretations here?

p9 line 37: would reduced sea ice not allow more icebergs to access the area? Or, conversely, expanded sea ice would limit access of icebergs and deposition of IRD?

Figure 1: label the profile shown in panel B on the dashed white line in panel A (instead of or as well as writing in the caption).

Figure 5: could you make the colours for sawtooth moraines and iceberg ploughmarks more distinct from one another?