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Interactive comment on "Cryostratigraphy, sedimentology and the late Quaternary evolution of the Zackenberg Delta, Northeast Greenland" *by* Graham L. Gilbert et al.

Anonymous Referee #2

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General Comments: An interesting paper that continues to mine data from a geographical area of focussed research and environmental monitoring spanning several decades. Authors attempt to integrate a wide scope of studies based on coring of two raised delta terraces, linking sedimentology with cryostratigraphy

Specific Comments: Although not necessarily a main focus of the paper, the chronological story, for which their OSL data is applied is somewhat muddled. It has to be acknowledged that the RSL curve for this area is poorly constrained in the immediate period following deglaciation, when ∼half of the total uplift (∼40 m) occurs (at suggested rates of 10 mm/a). The 10.1 calibrated AMS age on bottom-set beds (∼31 m asl) must therefore be recognized simply as a minimum estimate for deglaciation,

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and in the absence of sedimentological evidence where one can trace bottom-sets into foresets and potentially topsets, it is not possible to so precisely ascribed the paleo-sea-level to which they are accordant. Thus, projecting the upward part of the RSL curve on a sample with a wide potential paleo-depth level is problematic (the M. truncata species itself potentially occupies a wide habitat depth). It is not surprising then that OSL ages could be older than this radiocarbon (calibrated) date. It becomes problematic, however, in that potentially this implies that they have foreset/topset beds that are older than what may well be accordant bottomset beds. If the foreset beds are dating 12-13 ka...then this would require and even earlier deglaciation, as the entire period of post-glacial sea level fall between marine limit (70 m asl) and their uppermost delta (38 m asl) would have to occur before the sediments that are forming these two deltas aggrade. Why is incomplete bleaching of sediments that would have been part of a very turbid, potentially short transport distance, not more considered/discussed? Authors mention the inclusion of feldspar ISRL as a check on this, but do not discuss the results within the paper. There is no reason to suspect the C14 ages are wrong, other than perhaps generally small regional variations in marine reservoir correction. What needs to be better qualified by them is the stratigraphic relationship between their OSL ages and those on marine macrofossils.

Facies recognition from core samples only 42mm in diameter is difficult, at best. While detailed architecture and facies descriptions were based off of river-cut bluff exposures at "sections," it would have helped if at least one of the drill holes was established in direct proximity to an existing section in order to better tie the core and section logs together.

Given comparable sedimentology between C1 and C2...why is the moisture content of C1 generally higher, and also, what is the explanation for why there are more significant excursions in higher moisture content and excess ice content? Would this not, in part, reflect lateral variations in sedimentology unresolvable at the core level - that is, it suggests that while both deltas are fairly coarse in nature, and thus would have largely

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been free-draining, that because of lateral variations in sediment texture/sediment homogeneity, may have permitted more effective fluid permeabilities? While I understand the valley-fill history model, I question it's application to an environment that is more akin to broad lowland.

Why is atmospheric moisture-sourced groundwater eliminated as a potential source of the epigenetically formed ground/pore ice? Depending on the configuration of the retreating ice, much of the surrounding terrain could have been subaerially exposed, contributing to regional groundwater tables. In the absence of chemical/isotopic study, I'm uncertain how the authors can simply state the interstitial water was glacial meltwater/marine sourced. Authors also do not discuss the implications of saline permafrost, and the role this may have had on enhanced drainage of coarse sediments as the depressed freezing point may have perpetuated fluid flow through the sediments during permafrost aggradation...would this have implications on cryostratigraphy?

Presumably part of the importance of understanding/reconstructing the cryostratigraphy is to enable some comment about ice content (p10L14 - "understanding of the amount of ground-ice in Arctic valleys) vis a vis potential melt volume/sediment compaction were this to thaw...can this be meaningfully done with your data?

Technical Comments: p3L20 - as previously discussed...the 9.5 ka shell date is a poor constraint on the 70 m marine limit and it's projection to 10.1 ka p6L30...what are the diagnostic sedimentological properties of a "dilute" turbidity current? p7L7 - a lack of IRD clasts does not have to reflect high sedimentation rates and dilution - it could simply be an absence of IRD reflecting lack of calving margins; it could also reflect that the delta currents move debris-laden ice off-shore, so little chance to accumulate. p9L5 - raised deltas would usually be isolated pretty quickly after emergence, so I'm unsure how you see these continuing to aggrade through the Holocene in any kind of fluvial sense...presumably Aeolian activity could be responsible? Table 2 - while I know you can mine the data out of Figs 7/8, it would be helpful if you included a column in this table that showed the FA interp for each of the samples

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