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The Cryosphere Discussions

Interactive comment on "Hydrologic controls on coastal suspended sediment plumes around the Greenland ice sheet" *by* V. W. Chu et al.

Anonymous Referee #3

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This is certainly a very interesting manuscript on a very important topic. We urgently need to know more about the large-scale hydrology of the Greenland Ice Sheet. However, this contribution, while it represents a substantial body of work, is ultimately rather equivocal. It generates data that it doesn't satisfactorily interpret, and more importantly, data that perhaps aren't actually susceptible to useful interpretation. Therefore, although I found the manuscript interesting, I also found it a little frustrating.

The high degree of spatial aggregation employed likely obviates pattern detection, as the authors' recognise, and this in itself might call the approach into question, but this is only one aspect of the problem. Some anticipated relationships are very unlikely to exist anyway, e.g. it is observed that "ice sheet PDD and plume SSC are generally uncoupled, suggesting that spatio-temporal aggregation is not effective for resolving the

well-known temporal limitations of MODIS in narrow fjord environments." I really don't think that this is a resolution/data availability issue: there is no reason at all to expect any melt proxy and SSC to be even approximately linearly related, or even related with time lags. The most obvious issue is that PDDs (or microwave melt extent) are only vague approximators of meltwater output, taking no account of meltwater routing and storage in glacial and proglacial systems. But even setting that aside, it is very well known that suspended sediment transport in glacial meltwater is characterised by hysteresis at multiple temporal scales, which confounds attempts to link runoff and SSC even in small, simple glacier systems. I think this fundamental point has been overlooked, and this paper would benefit considerably from a MUCH greater engagement with the literature on glacial-fluvial sediment transfer.

I wonder if we actually shouldn't expect any relationship between melt/runoff proxies and SSC at all, but just accept that SSC is a convenient, almost binary, label for glacial runoff, with no explanatory power beyond water mass discrimination once it has entered the fjord environment. I would be fairly confident that the spatial scale of this study is too ambitious, and that too little is yet know about the interactions of glacial runoff with fjord waters at any scale to afford useful interpretation of this large data set: certainly this is the case for glaciers with tidewater termini, about which we know frustratingly little in terms of hydrology - this is reflected in the very equivocal discussion of tidewater cases in the manuscript. I think this paper deserves publication as it contains a unique and potentially valuable data set, but only if it is recast as a contribution raising issues with ice-sheet hydrology, plume detection, assessing the potential and providing recommendations to move this intriguing but difficult area of glaciology forward.

Specific comments p2366,l15-16: "SSC allows assessment of long-term conditions" - this sentence is unclear to me, it seems quite vague. What kind of conditions, and what is it about them that SSC reveals? This needs to be clearer as it is an important part of the paper's conclusions.

p2367,l19-29: "meltwater processes are less important to marine-terminating glaciers

than area destabilised calving fronts" - I don't think this is the case. Destablized calving fronts may be implicated in rapid retreats, but meltwater processes are highly likely to contribute towards the perennial fast flow of tidewater glaciers, and there are very interesting, unanswered questions about how and why such glaciers evolve to sustain low-effective pressure drainage systems and how these might expand to other parts of the ice-sheet. Moreover, there is accumulating evidence for more seasonal variation in tidewater glacier velocities than previously assumed.

p2368 first paragraph and throughout the manuscript: 18 references for a couple of fairly basic points. The number of references is excessive: there is too much duplication, and readability is affected. On the other hand, there is surprisingly little reference to the glacial-fluvial sediment transfer literature.

p2369,l20-24: you've got to acknowledge the complicating influence of seasonal sediment supply variations/hysteresis - there shouldn't really be any expectation of a simple/linear discharge-SSC relationship. This influence already showed up in your own assessment of the plume in Kangerlussuaq.

p.2370,l20-22: this is only true when PDDs are used in a well-calibrated temperature-index melt model, not when they are used untransformed as a melt proxy. Given the scale and intentions of the paper, this approximation of runoff is not wholly unreasonable, but it has major limitations, and these should be more fully acknowledged, if not tested. Many factors intervene in the relationship between PDDs and runoff rates: for instance, in East Greenland, air temperatures were fairly high in summer 2003, but overall runoff was low, because thick snowpacks from the previous, high-accumulation winter kept albedos and meltwater retention rates high. It seem that the data presented in this paper can only be interpreted with a much more thorough knowledge and understanding of hydrological variations, and PDD values just don't give enough information to do this.

p.2373,l6: presumably there should be a "distinguish" between "to between" Section

2.2.4: I don't have much faith in this calibration, it is very weakly constrained. As suggested above, I don't think this can realistically provide more than a binary SSC/no SSC function. Moreover, the relationship is unvalidated. I accept that all this is challenging at the spatial scales considered, but I would suggest that we need a better understanding of the various states, processes and relationships concerned (e.g. SSC-spectral reflectance variability in fjord waters) before we can meaningfully interpret patterns at these scales anyway.

p2377,l10-11: related to the above point, if the empirical model agrees with the values in Hasholt (1996), I'm equally skeptical, as the values in that paper are derived from terrestrial rivers rather than fjords. You'd expect higher SSC values in these rivers.

p2378,l7-10: confusing - says regions are "based on" Ohmura and Reeh (1991 - need year in l9), but then they are "considerably different" from O&R91?

The results section in general repeats a lot of the information in Table 1 and could be shortened.

Table 1: "seasonal" suggests <1 year to me, I suggest using less ambiguous terms (seasonal, decadal? Annual, total?)

You could also indicate statistical significance (or not) with italics.

In general, the figures are good, but they'll need to be reproduced in a large size to make sure they're fully readable.

Some of the correlations may be significant, but they're so low that you doubt they have much explanatory power or predictive utility, e.g. p.2383,l9.

p.2383,l19-21: this is worth noting. but the relationship was never likely to be this simple.

p.2384,l7-9: we really know very little about sediment transfer rates from calving glaciers, certainly ones of the size considered here. There's no reason to believe

that they are any different to other glaciers in this respect, although plume detection is obviously more difficult at their margins. I'm not sure much can be read into this correlation, at least not from a sediment transfer process perspective; more likely to be a data artefact.

p.2384,l27: "climatologies" of SSC?

p.2385-2386: a lot of this discussion is quite speculative and somewhat ambivalent, which stems from the use of proxies of debatable effectiveness (see above) and partly from the questionable reflectance-SSC model (also see above): again, the results are difficult to interpret because the data generated are too detached from process understanding at the appropriate scale. This has been a recurring but very important point.

There should be a stand-alone conclusion.

Interactive comment on The Cryosphere Discuss., 5, 2365, 2011.