The Cryosphere Discuss., 5, C1254–C1259, 2011
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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 #2

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General comments: This paper investigates whether remotely sensed observations of suspended sediment concentration (SSC) in Greenlandic fjords can be used as a proxy for glacial meltwater discharge to the ocean. The eventual aim is to provide a method to quantify the amount of surface meltwater which actually reaches the surrounding ocean rather than refreezing in the snowpack or interior of the Greenland Ice Sheet (GrIS).

Observed SSC is compared to positive degree days (PDD) produced by the the Polar MM5 regional climate model, a proxy for surface melting. In the analysis, PDD for individual drainage basins are compared with SSC from a corresponding 100 km by 100 km coastal 'cell' into which meltwater from the particular drainage basin is assumed to flow.

The authors find that in general, high SSC is associated with higher PDD, but they also find that outlet glacier type (i.e. land- or marine-terminating) is an important factor, with a higher proportion of land-terminating outlet glaciers in a particular coastal cell producing higher SSC values.

Using SSC rather than plume area is a novel idea, which should extend the applicability of remote sensing of sediment plumes beyond those which occur in ice-free fjords. The paper is fairly well written, but would benefit from clarification of several sections (see some of the specific comments below), particularly the 'Data and Methods' section. I have several major comments:

1. The authors state in the introduction that 'the amount of meltwater that truly reaches the ocean (rather than refreezing or being retained by the ice sheet) is presently unknown'. However, SSC is subsequently compared with PDD (a proxy for surface melting) which does not account for refreezing. Surely it would be more beneficial to compare SSC with runoff from a model which includes some treatment of refreezing? Otherwise this important process will not be represented in any SSC-derived assessment of meltwater runoff. In other words, the inclusion of refreezing could significantly alter the relationship between PDD and SSC presented in this paper.

2. The variations in SSC associated with the proportions of land- and marine-terminating outlet glaciers should be investigated further. Runoff from a proglacial river will enter a fjord at the surface, whereas the majority of runoff from a marine-terminating glacier is likely to be at depth – up to 500 or 600 m below the ocean surface. Not all the sediment contained within the meltwater will make it to the fjord surface because the buoyant meltwater plume will mix with the ambient fjord water as it ascends. It therefore seems likely that SSC will underestimate actual meltwater runoff for marine-terminating basins.

3. In the light of points (1) and (2), I think it would be useful to include a more thorough discussion of the potential of the MODIS-derived SSC method for estimating GrIS

TCD
5, C1254–C1259, 2011

runoff, including a more detailed treatment of sources of error.

I think that the paper will be suitable for publication in The Cryosphere once these concerns are addressed. Below there are some further specific points for the authors to consider.

Specific comments: Page 2367, line 6: 'velocity speedup' this seems a bit clumsy, consider revising.

Page 2367, line 16: 'also' suggests that you have already mentioned something else which meltwater runoff is linked to, which you have not.

Page 2367, line 22: Drainage of supraglacial lakes is also important because it establishes a link between the surface and bed which can subsequently be exploited by surface meltwater to potentially affect ice flow.

Page 2368, line 10: 'However, its release from the ice sheet edge to the ocean remains largely unstudied, consisting of a handful of modeling efforts. . .' does not make sense. Perhaps: 'However, its release from the ice sheet edge to the ocean remains largely unstudied. Existing research consists of a handful of modeling efforts. . .'

Page 2368, line 18: 'like' is a bit colloquial, how about 'such as' instead?

Page 2370, line 22-23: What exactly is 'its un-cumulative form'? Needs a little explanation.

Page 2371, line9-10: 'total length of the ice sheet edge' is a little confusing, does this mean the length of the basin perimeter?

Page 2372, line 23-25: The authors mention 'Particular difficulty in distinguishing sediment-rich water from melting ice' but do not explain how this difficulty was overcome. This needs to be expanded upon.

Page 2372, line 27-28: 'high band 6 reflectance > band 1 reflectance' could be more succinctly stated using 'band 6 reflectance » band 1 reflectance'.

TCD

5, C1254–C1259, 2011

Page 2373, line 6: Missing word: ‘taking advantage of their higher spatial resolutions to between sediment rich. . .’ should be ‘taking advantage of their higher spatial resolutions to distinguish between sediment rich. . .’

Page 2373, paragraph beginning line 4: It would help to convince the reader of the validity of the MODIS imagery for classification by stating some statistics about the verification.

Page 2373, lines 9-12: This is a clumsy sentence. ’selected to be’ could be ‘limited to those’. Also what exactly is meant by ‘outlet-water interface types’ – I don’t think this phrase is used anywhere else in the paper and should be simplified/clarified. Page 2374, lines 1-26: This section is quite confusing and needs some clarification. I may be misunderstanding this but doesn’t the spatial averaging over the 100 km ‘gridcell’ negate the benefits from having the Regions of Interest (ROIs) < 50 km from the outlet glacier termini? Also the terminology is confusing, for example there are ‘ROIs’, a ‘100 km grid cell’ and then a ‘100 km gridcell ROI’.

Page 2375, line 12: ‘culled for the points’ seems slightly strange wording (and wouldn’t you want to cull the points that did not overlap the ROIs).

Page 2375, line 21: It would be good to see a p-value for this relationship.

Page 2376, line 1: Is there a ‘The’ missing before ‘Outlet’? or should ‘environment provides’ be ‘environments provide’?

Page 2376, line 11: Consider changing ‘and remaining’ to ‘which remain’.

Page 2376, lines 13-15: This sentence is a bit clumsy. E.g. is ‘outlet meltwater source’ the same as ‘outlet-water interface types’ (Page 2373, lines 9-12) – need to be more consistent in the use of terminology.

Page 2376, line 18: ‘categories of outlet types’ could simply be ‘outlet types’.

Page 2376, line 27: ‘edge’ is a bit ambiguous, how about ‘ice sheet margin’ instead?

TCD

5, C1254–C1259, 2011

Page 2376, line 28: If there were lakes between the land-terminating glacier meltwater outlet and the fjord, much of the sediment may have already settled out and the fjord plume would have lower SSC.

Page 2377, lines 8-11: What is the potential error in extrapolating so far from the empirical SSC-reflectance relationship? Indeed there is little mention of errors throughout the paper, despite some being very large – e.g. 55 +/- 63 mg/l (page 2378, line 18). Perhaps a brief section could be added to address this.

Page 2378, lines 9-15: I'm not sure it is necessary to include the details about the naming conventions as there is no direct comparison with the Ohmura and Reeh (1991) data.

Page 2379, lines 3-4: Awkward sentence, need to change.

Page 2379, lines 5-7: another slightly strange sentence, consider revising.

Page 2379, line 17: Not sure it is necessary to mention 'this region encompasses the southwest' in the section about the 'Southwest Region'. Also 'giving way' could be changed to 'contributing to'.

Page 2380, lines 7 and 20: The east and north east cannot both have the lowest mean PDD.

Page 2380, line 22: Should it be a comparison of NL and NM?

Page 2381, line 20: There is a 'w' on the end of 'Southeast'.

Page 2381, line 24: Should mention that it was a strong positive interannual correlation.

Page 2383, line 1: 'lower intensity of PDD' consider changing to 'fewer PDDs'?

Page 2383, line 21: 'less likely' or simply slower?

Page 2384, line 14-15: Successful as compared to what – the ASTER and Landsat verification data?

TCD

5, C1254–C1259, 2011

Page 2384, line 23-24: you cannot be certain that it is due to open water detection problems (although it seems likely) – there may be a plume beneath the sea ice.

Page 2385, line 10: 'Buoyant plumes are most. . .'

Page 2385, line 11: Should also show values for high SSC in parentheses to be consistent.

Figures:

Figures 2 and 3: Both require a north arrow or a latitude and longitude grid and also a scale bar.

Figure 2: It would be helpful to identify the different outlets and resulting plumes on the figure – i.e. a 1, 2 and 3 in some of the Landsat and ASTER close-ups.

Figure 3: It is quite difficult to see the individual basins in the SE and E as the delineation lines are a similar colour to the background.

Figure 4: Add the p-value to the plot.

Figure 5: Need to make it clear that the scaled circles are for part (a) and then to include a smaller legend for part (c).

Figure 6: Add (a) and (b) to the figure caption to clarify the description. Also, it would be better to alter the y-scale for several of the part (b) plots so that the relationships are clear.

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