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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.

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This paper describes a technique to infer the suspended sediment concentration (SSC) in coastal waters using daily MODIS imagery. The method is new and novel in that it utilizes the high temporal resolution and broad coverage of MODIS imagery. In this study, the authors apply the technique to Greenland, having defined 2,800 regions of interest in 230 fjords. Data for each region of interest are aggregated into 100 km x 100 km grid cells - to produce time series of higher temporal resolution. To compute SSC values, the authors use an empirical function derived from comparison of observed SSC and MODIS reflectance.

The aim of the study is to compare the spatial pattern and temporal characteristics of MODIS-derived SSC against surface melting in catchments of the Greenland Ice

Sheet. Positive degree days (PDD) from the Polar MM5 regional climate model are used as a proxy of meltwater production on the ice sheet. The study reports correlations (and lack thereof) between SSC and PDD, when data are averaged yearly and across seasons. The study also shows that SSC inferred from MODIS is generally higher for land-terminating glaciers and river systems compared to marine-terminating glaciers.

The paper is well written and the methods are clearly laid out. The results are interesting and nicely presented, and the discussion is (apart from a few places, see below) relevant and sound.

My main criticisms of the paper are the following four points:

(1) There is very little attempt to tackle the error that inevitably exists in the data. The plots of interannual (Fig. 6) and seasonal (Fig. 8) variability should ideally contain error bars. How big are errors relative to the variability? This needs to be at least described if not shown.

(2) Whereas the problem of distinguishing sediment laden surface water from water containing icebergs etc (the 'melting ice' term) is clearly mentioned, the solution whereby this problem was solved is not clear. Based on higher-resolution imagery, how can the authors be sure that dirty sediment-laden surface water can be distinguished from clean surface water containing patches of brash ice or clumps of small icebergs, i.e. features that doesn't show in a 500 m x 500 m MODIS cell but could influence reflectance? This needs to be clarified

(3) Care and clarification are needed when comparing seasonally co-varying factors statistically. The fact that SSC is seasonal comes from this data source alone. The correlation coefficient between seasonally varying SSC and seasonally varying PDD is not particularly meaningful. It's just autocorrelation. The presentation of statistics needs to be revised slightly. I suggest reducing any discussion where auto-correlation exists and strengthening the discussion on the onset and duration of high SSC vs.

high melt water discharge. The latter may not correlate statistically, but may offer a meaningful interpretation nonetheless.

(4) Land- vs. marine-terminating glaciers: The various depths at which sediment-laden meltwater is injected into a fjord from different glacier types are likely to be very important. For big marine-terminating glaciers, subglacial meltwater may flow into a fjord >500 m below the surface. Although modeling suggests that meltwater plumes from marine-terminating glaciers should rise to the surface (the authors refer to a study by Mugford and Dowdeswell (2011)), not all sediment will be able to follow the meltwater as it rises, broadens and lose velocity. If the plume is linear along the front rather than circular (as in the study cited), the velocity will be much less. This could happen if basal water is found in a distributed rather than channelised system. Also, a layer of freshwater at the surface (which is common in Greenland fjords) could force the plume to lose buoyancy and prevent it from surfacing, i.e. thus not being visible in the MODIS imagery. The paper touches on this aspect, but only briefly. I suggest a more elaborate discussion on this issue.

I recommend publication of the manuscript if the points above can be satisfactorily addressed. Minor points to consider include:

p. 2370 line 21: 'outperformed' is not the right word here. PDD and energy-balance models have their strengths and weaknesses. For a comparison of the two approaches, see Bougamont et al. (GRL, 2007).

p. 2372 line 13: what exactly is meant by 'minimal' cloud cover and 'minimal' atmospheric interference? Be specific.

p. 2372 line 23-25: 'Particular difficulty in distinguishing ....' This problem is subsequently not fully explained. How was the problem ultimately solved?

p. 2373 line 1: 'melting ice'. What exactly is meant by this? I can think of several types of ice that melts and might influence MOPDIS reflection, ranging from brash ice

to patchy sea ice and small icebergs. A better explanation is needed.

p. 2373 line 8-9: 'Melting ice proved difficult to discern ....' Again, this problem is subsequently not fully explained. How was the problem ultimately solved?

p. 2375 line 11: '...80 km north of Ilullisat....' Add name of fjord or at least region.

p. 2379 line 6: If the Jakobshavn drainage basin distorts the mean, why not use the median?

p. 2381 line 3: 'intensities if PDD and SSC'. Does this simply mean values of PDD and SSC?

p. 2381 line 3: 'track each other'. Track? Suggest a different word.

p. 2382 line 3-10: These correlation coefficients are probably just autocorrelation (see comment above).

p. 2383 line 1-4: Again, autocorrelation (see comment above).

p. 2383 line 16-21: Geology. This is very brief and I am not convinced by the analysis. The difference between marine- and land-terminating glaciers is more likely to be related to deep plumes vs. surface plumes (see comment above).

p. 2383 line 24: this correlation is not particularly interesting.

p. 2385 line 1-4: 'calf'. Is this a correct term?

p. 2386 line 5: What exactly is meant by 'fairly anti-correlated'? Be precise.

p. 2386 line 20: Plumes (see comment above).

References: The Mugford and Dowdeswell paper should be the one from 2011, not 2010. I haven't checked the rest of the list.

Table 1: I suggest writing statistically significant correlation coefficients in bold, to guide the eye of the reader. The font is very small! I suspect it is the TCD formatting that

does this. In fully published format, the table will hopefully be clearer.

Figures. The font is very small! I suspect it is the TCD formatting that does this. In fully published format, the figures will hopefully be clearer.

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