We'd like to thank Poul Christoffersen and the two anonymous referees for their thoughtful and detailed reviews of this paper. Their suggestions have been incorporated and have greatly improved the manuscript.

Poul Christoffersen, Referee #1

General comments:

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.

   Interannual and seasonal plots have been added with 95% confidence intervals, and a discussion of uncertainties has been added.

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

   This has been elaborated upon to show that thresholds were chosen conservatively to err on the side of missing sediment-rich water rather than over-sampling open water when distinguishing from melting ice, particularly when the 500 m resolution results in many mixed pixels of both. Statistics on the validation were also added in the results to show an overall accuracy of 79%, and specifically for the open water class, a producer accuracy of 66% and a user accuracy of 82%. In other words, while only 66% of open water pixels (including sediment-rich water) have been correctly identified as open water, 82% of the pixels called open water are truly open water. This conservative estimate of open water areas was deemed adequate for estimation of SSC.

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.

   A clarification has been added to address the autocorrelation and to de-emphasize the use of correlation coefficients, using them to merely state relative differences between regions.

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.

The discussion on marine-terminating glaciers has been greatly expanded, as requested. Particularly, the two issues of sedimentation as the meltwater plume rises, causing a decrease in sediment before reaching the fjord surface, and the presence of freshwater at the surface, preventing a loss of buoyancy, have been added.

Specific comments:

1) 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).

   The wording has been changed and the limitations with PDDs as a melt proxy are further discussed.

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

   This has been clarified to show these were defined using quality flags from the MODIS Quality Assurance layer within each scene.

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

   This has been elaborated upon to show that thresholds were chosen conservatively to err on the side of missing sediment-rich water rather than over-sampling open water. Statistics on the validation were also added in the results to show high user accuracy for open water pixels, the class of interest.

4) 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.

   The explanation has be expanded to show melting ice including all of the above mentioned, and terminology has been changed to include these terms.

5) 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?

(see above)

6) p. 2375 line 11: ‘. . .80 km north of Ilullisat. . ..’ Add name of fjord or at least region.

Added name, Eqip Sermia.

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

The wording has been changed to clarify the point; the mean is similar to the median and is not distorted by Jakobshavn (the regions contains many large basins)..

8) p. 2381 line 3: ‘intensities if PDD and SSC’. Does this simply mean values of PDD and SSC?

Yes, the wording has been changed.

9) p. 2381 line 3: ‘track each other’. Track? Suggest a different word.

The wording has been changed.

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

(see above)

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

(see above)

12) 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).

A clarification has been added to show the limitations of this result and other possible relationships.

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

This correlation and result has been removed.

14) p. 2385 line 1-4: ‘calf’. Is this a correct term?

The wording has been changed.

15) p. 2386 line 5: What exactly is meant by ‘fairly anti-correlated’? Be precise.

Wording has been changed to show a lack of any correlation.

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

Issue is discussed further (see above).

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

Both 2010 and 2011 papers are cited, and both are included in the references.

18) 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.

Statistically significant coefficients have been bolded.

19) 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.

Figures have been submitted in large format, and the small font is due to TCD formatting.

Anonymous Referee #2

General 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, he inclusion of refreezing could significantly alter the relationship between PDD and SSC presented in this paper.

   This issue has been added to the discussion, showing that PDD is not a proxy for runoff and that SSC is useful as a signal of meltwater release but not as a proxy for meltwater or sediment flux.

2) The variations in SSC associated with the proportions of land- and marineterminating 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 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:

1) Page 2367, line 6: ‘velocity speedup’ this seems a bit clumsy, consider revising.

   The wording has been changed.

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

   Corrected.

3) 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.

Added.

4) 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. . .’

   The wording has been changed.

5) Page 2368, line 18: ‘like’ is a bit colloquial, how about ‘such as’ instead?

   The wording has been changed.

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

   The wording has been changed and the limitations with PDDs (untransformed and not used in a temperature-index melt model) as a melt proxy are further discussed.

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

   Wording has been changed to the total horizontal length of the ice sheet edge.

8) 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.

   This has been elaborated upon to show that thresholds were chosen conservatively to err on the side of missing sediment-rich water rather than over-sampling open water. Statistics on the validation were also added in the results to show high user accuracy for open water pixels, the class of interest.

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

   Corrected.

10) 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. . .’

    Corrected.

11) 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.

Statistics on the validation were added in the results to show high overall accuracy as well as high user accuracy for open water pixels, the class of interest.

12) 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.

    The wording has been changed.

13) 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’.

    A clarification has been added to explain that the spatial aggregation over the 100 km gridcells only averages of data within those ROIS representing fjords downstream of glaciers draining the ice sheet, and the terminology has been simplified.

14) 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).

    The wording has been changed.

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

    Added p-value.

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

    Corrected.

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

    The wording has been changed.

18) 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.

    The wording has been changed.

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

The wording has been changed.

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

    The wording has been changed.

21) 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.

    This issue has been added to the discussion on sediment transport processes from both land-terminating glaciers.

22) 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.

    A paragraph has been added to the results to discuss the uncertainties of the empirical model. A paragraph at the end of the discussion has also been added to address the limitations and uncertainties of the data products and analysis overall.

23) 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.

    This section has been clarified, removing the reference to Ohmura and Reeh.

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

    The wording has been changed.

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

    The wording has been changed.

26) 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’.

    The wording has been changed.

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

    Corrected.

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

Corrected.

29) Page 2381, line 20: There is a ‘w’ on the end of ‘Southeast’.

Corrected.

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

Added.

31) Page 2383, line 1: ‘lower intensity of PDD’ consider changing to ‘fewer PDDs’?

The wording has been changed.

32) Page 2383, line 21: ‘less likely’ or simply slower?

The wording has been changed.

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

Yes, compared to the verification data but mostly meant here to represent retrieval for ~80% of the Greenland coast.

34) 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.

The wording has been changed.

35) Page 2385, line 10: ‘Buoyant plumes are most. . .’

Wording has been corrected, as requested.

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

Added.

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

Added.

38) 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.

Added labels.

39) 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.

Colors have been adjusted.

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

Added.

41) 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).

Added a second legend for part (c).

42) 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.

Y-scale altered to fit the individual plots.

Anonymous Referee #3

General comments:

1) 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 imitations 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.

   We agree, and the stated goals and the results/discussion throughout the paper have been tempered to reflect the complex nature of the processes and to provide a better context for the broad-scale results our data and analyses can provide. PDD limitations are discussed, as requested.

2) 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.

   A more in-depth review of glacial-fluvial sediment transfer and hysteresis has been added, and the limitations of SSC are discussed.

3) 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.

   Yes, we agree, see comment for 1) above. SSC limitations are discussed, as requested. This paper's contribution has been recast, as suggested, and a clarification has been added that the goal is to provide a first ever synoptic view of sediment plumes as an indicator of meltwater release, in addition to highlighting the current need for process-scale knowledge of ice sheet hydrology.

Specific comments:

1) 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.

   The wording has been changed to show that SSC allows the assessment of a meltwater signal across different fjord environments.

2) 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.

   This point about meltwater processes in marine-terminating glaciers has been expanded and the wording has been changed to show its importance.

3) 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.

   References have been trimmed down, as requested, and more glacial-fluvial sediment transfer literature has been added.

4) 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.

   Yes, we agree, a more in-depth review of glacial-fluvial sediment transfer and hysteresis has been added, and the limitations of SSC are discussed.

5) 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.

The limitations with PDDs and their purpose in providing a broad-scale proxy for meltwater production are discussed, as requested.

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

   Corrected, as requested.

7) 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.

   We agree that the use of SSC is limited, given the small number of field observations from two locations, and these suggestions are incorporated into the discussion. A paragraph has been added to the results to address uncertainties of the empirical model, also showing that only 5.6% of the data exceed field measurements of SSC.

8) 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.

   A discussion of the empirical model has been added a separate paragraph in the results as well as in the discussion, and this sentence has been revised to reflect that the maximum extracted is well below the . Also, the issue of higher SSCs in rivers than in fjords has been elaborated in the discussion on sediment transport in land-terminating environments.

9) 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?

   This section has been clarified, removing the reference to Ohmura and Reeh.

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

    Results section has been shortened.

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

The term seasonal is kept to indicate correlation of seasonal cycles, though correlations are de-emphasized in discussion due to autocorrelation and used only to indicate relative differences between regions.

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

Statistical significance has been noted with italics.

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

Figures have been submitted in a much larger size.

14) 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.

We agree, and the wording has been changed to de-emphasize correlation coefficients.

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

A clarification has been added to show the limitations of this result and other possible relationships.

16) 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.

The wording has been changed.

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

Added definition for this (averaged across each day-of-year for 10 years).

18) 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.

Results and discussion have been tempered to reflect the broad-scale view of sediment plumes as an indicator of meltwater release. Also, a future work section has been added to the discussion.

19) There should be a stand-alone conclusion.

Added a stand-alone conclusion.