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The Cryosphere Discussions

Interactive comment on "Surface melt magnitude retrieval over Ross Ice Shelf, Antarctica using coupled MODIS near-IR and thermal satellite measurements" *by* D. J. Lampkin and C. C. Karmosky

Anonymous Referee #1

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This manuscript presents a method to observe surface melt. This method is a calibration of MODIS near IR/thermal surface images using results of a 1D snowmelt model. The latter model is driven with a blend of observations and reanalysis products.

Initially, I was pleased to read this article because sentences are well formulated, so the article is easy to read. However, in the end I am quite disappointed. The different content parts are poorly connected and essential questions and content parts are simply neglected.

My major points are:

1. Section 5: In words is explained how the calibration is performed, and a flowchart is given. Indeed, a similar method has been used for Greenland (Lampkin and Peng, 2008), but given the poor quality of elements of the data, just this description is not enough. Include a full, clear and conclusive discussion and evaluation of the model results and the subsequent regression. Now the reader has to trust you that it works.
2. Section 6: The Antarctic summer is short, so it contains only a few 8-day intervals with melt. If you really wants to show that this method works, extent the comparison to several years.
3. Figure 7: Mean LWF_{eff} for certain XPGR melt duration is not very informative. Provide probability distributions of LWF_{eff} for each XPGR melt duration. Then more insight is given about the prediction performance. For example, for most no-melt days XPGR observations, one expects a LWF of zero.
4. Figures 8, 9 and 10: Why do all the sensitivity tests not extend over the whole analysis period?
5. Section 7: Please explain how the relative uncertainties (Table 2) are determined and what they mean. If it is indeed a 2.8% uncertainty on the estimated LWF_{eff} values, thus 0.01% to 0.02% LWF's, than it is too low to be realistic.
6. Section 7: Section 4.3 section is not very clear about the method to get a reasonable SW estimate. I conclude from it that there is significant uncertainty related to it. Please clarify 4.3 and add results of sensitivity tests for SW radiation, including albedo, in section 7.
7. I would assume that the uncertainty on downwelling LW is more than 5%, many models have 20 $\mathrm{W/m^2}$ biases. Figure 9 shows that for 5% deviations, the LWF

changes a factor 2. Surprisingly, that gives a relative error of 1.4%. More importantly and once again, please explain how this error estimate is obtained.

8. Section 8 / Figure 11 and 12: I don't see the added value of the discussion of the meteorological conditions for melt. If you want to investigate why melt events occur on the Ross Ice Shelf, you need more than only wind fields that are only on 2.5 degree resolution.

All in all, this manuscript as it is now does not convince me that the proposed method provides useful results. Therefore, I recommend that the authors provide a major revision of this manuscript.

While rewriting, please keep in mind that the reader can be convinced of the quality of your results when the reader can follow what you did. The given comments provide some guidance for the revision.

Furthermore, I suggest the authors to shorten 'introduction-like' text pieces like the first half of Section 5.

Interactive comment on The Cryosphere Discuss., 3, 1069, 2009.