

## ***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 #3**

Received and published: 11 February 2010

To my humble opinion, the paper could have been submitted as a letter. All the information that the authors provide regarding, for example, the XPGR and other topics is way too much for a paper. It goes without saying that it is important to provide background but there is too much discussion on topics that are ultimately not implying the validity of the method. This distracts the reader from the main paper's topic. Also, some sections of the first part of the paper are very sloppy and the style is not effective.

I am not fully convinced that this approach can really provide valuable information against that derived with microwave data. Also, in terms of spatial resolution,

C576

QUIKSCAT data processed at BYU at 2.25 km can be used for wet/dry snow maps with a spatial resolution higher than that used in the study (4 km).

There is another issue and very important: there is no evidence of a direct validation of some of the results (e.g., grain size on the surface, LWC along the vertical profile, etc.). As long as I understand the inherent difficulties connected with this phase of the work, this is still a major drawback of the whole paper. There is no mention of this in the conclusions. I strongly suggest the author to highlight the fact that the LWCEff is not validated and that the results discussed regard the output of a model.

The overall methodology has some 'obscure' points, such as the variability of grain size due to factors different from melting and the fact that the method is based on empirical relationship. Other more elegant methods could and should be applied, such as classification methods or neural network approaches that aim at generalizing the relationships among the different inputs and outputs. I do not recommend the publication of the paper as long as the above mentioned issues are not addressed.

Page 1070

- The sentence from line 7 through 11 does not make sense. Please rephrase.

Page 1071

- lines 1 and 2. There is no reference here. Also what does it mean 'beyond anything experienced ...'. This is not a technical sentence nor support any results or reports previous work. This sentence should be revised or removed.

Page 1072

- Line 2. How is it clear that surface temperature over Antarctica are increasing ? The above mentioned papers refer to different periods and most of th warming occurs in winter and fall, not in summer. The author should include more details of the mentioned papers (specifying the years over which the trends are estimated) and should also specify how they can relate the more melting that they claim (whose trends are not

C577

statistically significant over the satellite era) is related to the above mentioned trends of surface temperatures

- Line 5. Define the 'effective' LWF.

- Line 18. I am confused. The authors say that the Rayleigh-Jeans approximation for surface emitted radiation is measured by radiometers. It is true that the radiometers measure  $T_b$  which, in the microwave region and at temperatures of the order of that of our planet, can be approximated by emissivity times kinetic temperature (Rayleigh-Jeans). However, Rayleigh-Jeans is the name of the approximation and does not have anything physical. I am not sure what the authors want to say but the way the sentence is now is misleading and should be revised.

Page 1073

- Line 5 – What do you mean for 'important' ? I am not sure that a method can be considered more important than another. This should be rephrased or deleted.

- Line 11. Please specify what you mean when you say 'emissivity alone'. If the authors refer to the fact that the effect of the temperature can be disregarded because of the ratio, then they also have to consider and explain briefly that the two frequencies have different penetration depths and, therefore the term 'emissivity alone' is still an approximation

- Line 13. Insert reference after 'signal decreases.'

- Line 13. The sentence starting with 'A higher ...' is confusing.

- Line 22. I think that the term 'erroneous passive microwave measurements' is not correct. You need to better specify what you want to say.

Page 1074

- the first sentence (beginning in the previous page) should be split in at least two if not three sentences. As it is, it does not make any sense and it is too long.

C578

- Line 8 – When is this approach used ? If this is a citation the reference is missing.

Page 1077

- Line 17. Add a reference for the dependence of infrared reflectance and grain size. Also the authors should mention that other factors are responsible for increase in grain size, beside melting

- Line 20. What is the criterion for the cloud mask ?

Page 1078

- Line 7. Add the link for the NSIDC

Page 1079

- Line 25-26. Higher temperatures are not necessarily only responsible for more surface melting. The time when surface temperatures exceed the melting point is also important. The authors should discuss this point.

Page 1080

- Lines between 17 and 27 seem to be cut and paste from another text. This information has already been provided and this text should be deleted.

Page 1081

- Line 14. Why was a 0.5 diameter used. What is the sensitivity of the method to the initial grain size values ?

- Line 23. Specify what '12z' is.

After then, the authors make a good job in trying to address some of the very important questions regarding the sensitivity and validation of the method.

---

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

C579