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Discussions

Interactive comment on "Remote sensing of sea ice: advances during the DAMOCLES project" *by* G. Heygster et al.

Anonymous Referee #3

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The present manuscript of Heygster and his colleagues aims to introduce and summarize the sea ice remote sensing achievements performed during the Developing Arctic Modeling and Observing Capabilities for Long-term Environmental Studies (DAMOCLES) project. The variety of topics addressed is wide, though they are all essentially related to sea ice and its overlying snow cover, and the number of space-based sensors considered is also impressive. The main topics addressed include but are not limited to:

- microwave emissivity retrievals from both the Advanced Microwave Scanning Radiometer - Earth Observing System (AMSR-E) and the Advanced Microwave Sounding Unit A (AMSU-A);

- microwave emissivity modeling using a coupled snow/ice thermodynamic and emis-

sion model;

- some temperature retrievals;

- snow grain size estimations from the Moderate Resolution Imaging Spectroradiometer (MODIS);

- sea ice drift and deformation calculations from a combination of different sensors including microwave scatterometers and;

- sea ice thickness estimations.

The manuscript could be very interesting, but sadly does not appear really thrilling. Despite the fact that every section addressing a specific scientific topic starts with a brief introduction, both the scientific question and its context as well as the literature could be significantly clarified. Indeed, I believe there will be very few readers with a high level of expertise on every topic addressed in the manuscript; therefore, and because the manuscript is designed as a review, it is important to emphasis the scientific objective and provide sufficient background introduction. The ultimate intention is to provide the readers, of all levels, enough material to really appreciate the importance of all the advances accomplished by the authors during the successful DAMOCLES project.

From my point of view, this manuscript, as it stands, presents (and summarizes) attractive new methods and interesting results, though it would be improved by further emphasizing the advances in sea ice remote sensing with a sufficiently complete and precise description to allow their reproduction. The scientific content is of great interest to the sea ice community and suitable for The Cryosphere journal, in particular in the DAMOCLES special issue. However, it does not yet meet a good level regarding the The Cryosphere Evaluation Criteria (http://www.the-cryosphere.net/review/ms_evaluation_criteria.html). I strongly recommend to specifically distinguish previously published methods/results from new results that have never been evaluated in a peer-reviewed journal. In conclusion, with additional work and better clarification of the new results, this manuscript might be suitable for publication.

In short, there are problems with some methodology descriptions, acronyms, vagueness in model names, equation labels (see for instance p. 63, l. 15 where the reference to Eq. 2 should be to Eq. 3), units (it is missing on p. 64 l. 24), and vocabulary (confusion between measurements and retrievals/estimations). Please, kindly note that I didn't carefully check the references because I gave up on the very first one which is mentioned p. 39, l. 6 and does even not appear in the reference list... It obviously seems that the manuscript was hastily put together.

Please, bare in mind that I tried to help the authors by providing a constructive review. In the following document are some of my suggestions to improve, and also correct, the manuscript.

General comments:

As earlier mentioned, I do believe that a better introduction in every section highlighting the scientific question and providing key references to point toward specific state-of-the-art publications dealing with the question will clarify the contribution of the authors and be appreciated by all readers. From my understanding, the idea of this manuscript is to provide a review of the advances possible thanks to the DAMOCLES project, and a review must present a clear literature review.

Several times, the authors present results without clearly referring to a peer-reviewed paper, I therefore would appreciate that some of these new results be compared to other independent published studies (eg, sea ice temperature and drift). There exist several AMSR-E level 3 products distributed by the National Snow and Ice Data Center (NSIDC) namely (a) the AMSR-E/Aqua Daily L3 12.5 km Brightness Temperature, Sea Ice Concentration, & Snow Depth Polar Grids and (b) the AMSR-E/Aqua Daily L3 6.25 km Sea Ice Drift Polar Grids. There also exists MODIS land surface temperature products which can be used either to provide some material for comparison or to validate the presented results over sea ice. The manuscript will significant benefit from further

comparisons with independent studies.

Sea ice drift calculations are performed using different approaches and sensors. Clarification would be appreciated to know how well those different methods agree to one another, or more interestingly where, when and, if possible, why do they differ?

The reader is too often left in the dark regarding the models used. Please, at a minimum, mention the name of the models considered. This is also valid for the atmospheric reanalysis from the European Center for Medium-Range Weather Forecasts (ECMWF). For instance, was the ERA-40 or ERA-interim data used?

There are several acronyms not explained, though understandable, such as: EU, AMSU and probably additional sensor names. In contrast, the Maximum Cross Correlation (MCC) is explained at least three times. Please, pay more attention to providing us a coherent paper where each author's contribution fits adequately with the entire flow.

On a similar topic, there are frequent unclarity in the manuscript that should be considered before publication. For instance, it is mentioned that an AMSU sensor has been used (see abstract, and p. 42 l. 20+). Were both the AMSU -A and -B units used, or only one?

In several places, the authors referred to something explained or described "above"; however, I often had trouble to find and agree with those references. To mention only one example (p. 43, l. 10+) it is written "This in turn requires knowledge of the sea ice emissivity of first-year ice and multiyear ice which is taken from the method described above." Unfortunately, the method is not described above, only results using the mentioned method are provided, but nothing about the method itself.

Specific comments:

p. 38, l. 15+. This activity deals with snow on land, whereas the main focus of the manuscript is on sea ice. Please, mention this fact.

p. 39, l. 18. This sentence should be reformulated because passive microwave measurements are sensitive to snow temperature, hence depend on the day/night time of observation. I see a problem with the current wording.

p. 40, l. 5+. This is the only time that a reference to the snow/ice interface temperature (shortened to SIIT!) is made. Do snow surface temperature and ice surface temperature refer to the temperature of the same interface? It is important to use the right word for defining the air/snow and snow/ice interface in particular because space-based sensors operating at different wavelengths are sensitive to one interface or the other.

p. 40, l. 25+. It was not clear to me between which component of the Earth climate system and the ocean there are ice mass exchanges.

p. 41, l. 5+. The equation used to define brightness temperature is only valid for an isothermal medium. Therefore, T_em is the physical temperature of the isothermal emitting layer.

p. 41, l. 11. Provide some examples of surface and atmospheric retrieved parameters.

p. 41, l. 13. If a multi-year ice (MYI) concentration algorithm exists, please, provide a peer-reviewed reference describing it.

p. 41, l. 15. I would expect that it is only due to the high microwave sea ice emissivity that it is challenging to retrieve atmospheric properties, and not really from its temporal variability. Most probably a reference is missing here to convince me.

p. 41, l. 22. Add a reference to the statement finishing here.

p. 42, l. 5+. Which reanalysis model has been used and what was the spatial resolution compared to the space-based radiometer footprints?

p. 42, l. 5+. Some important clarifications might be required here. As far as I understand, it is mentioned that for computing the emissivities from the AMSR-E measure-

ments (collected after May 2002), in-situ temperature measurements from a period before launch were used (1998 and 1999). This is surprising and raises questions in particular because the authors mentioned that microwave emissivities have a high temporal variability (p. 41, l. 15). During which time period were the microwave emissivities computed? Does it matter that dramatic thickness changes are observed on the MYI cover?

p. 42, l. 21. Add emissivity: . . . when the higher emissivity values ...

p. 42, l. 22. Were both AMSU -A and -B sensors included in the study?

p. 42, l. 22. I don't understand why and how AMSR-E emissivities can agree with AMSU emissivities since there is no common channels on these radiometers.

p. 42, l. 25+. What does met.no mean?

p. 43, l. 5+. Is there a bare-ice (ie snow-free) assumption?

p. 43, l. 14. I did not find anything about the method itself.

p. 43, l. 15+. Estimates of sea ice concentration from the AMSR-E measurements are computed with the NASA Team 2 algorithm, not the NASA Team algorithm as mentioned. Please, correct and add a reference.

p. 43, l. 15+. Which relationship has been used to convert brightness temperature into a priori surface temperature? Does surface here mean air/snow interface? Was the AMSR-E level 3 ice surface temperature product considered, if not why?

p. 43, l. 20+. A standard deviation is usually an uncertainty, not an error. Please, only consider this comment according to your philosophy.

p. 44, l. 10+. The gradient ratio definition is wrong. Correction is mandatory.

p. 44, l. 10+. It has been previously mentioned in the manuscript (p. 41, l. 8) that T_b=T_em x e (without a single comment of any kind on the assumption of this equation). Here, it is now stated that the correlation between T_b, T_em and e is high! To make this section relevant for publication major improvements are mandatory.

p. 44, l. 15. How high is high? Temperature values are required to support such a statement. Very seriously, at temperatures approaching the melting point snow microwave emissivities theoretically equals one at all frequencies. The correlation coefficient between emissivity should then be extremely good.

p. 44, l. 15+. Sentences l. 16-17 and 22-23 sound contradictory.

p. 44, l. 25+. Emissivity is not only affected by volume scattering but by absorption as well.

p. 45, l. 1+. Quantify the wide range of temperatures and snow depths used.

p. 45, l. 10+. Mention what HIRLAM stands for and provide a reference.

p. 45, l. 14-22. Please, quantify the improvements to prove your confident statement.

p. 46, l. 11. No thermodynamic model was presented in the previous section!

p. 46, l. 10+. Was the thermodynamic model forced by in-situ measurements or other model outputs? What about the input/output temporal resolutions?

p. 46, l. 15+. Was this finding compared to the operational AMSR-E level 3 sea ice temperature product?

p. 47, l. 4. I think “but” can be replace by “it”.

p. 47, l21-22. I don’t think these two sentences make sense here.

p. 48, l. 20. Quantify “admissible accuracy”.

p. 48, l. 20. Quantify “unacceptable error”.

p. 50, l. 1+. In my view, a more scientifically interesting statement than the amount of personnel deployed is the time of the year they have been deployed...

p. 50, l. 5+. The mentioned measurements have been collected on land, more over on top of a hill. What would be the possible contribution of brine over sea ice?

p. 54, l. 22. Please add a reference.

p. 56, l. 5+. I had in mind that the combination QuikSCAT-SSM/I was from the early 2000’s, was this already a DAMOCLES contribution?

p. 60, l. 4. Can ice drift still be estimated with the current AMSR-E scanhead position?

p. 61, l. 15. Please, change measurements to estimates or retrievals.

p. 63, l. 8. Correct units of density.

p. 63, l. 10. I think the density database collected during the SHEBA experiment has been measured and not estimated. Correct if appropriate.

p. 63, l. 15+. Quantify “wide range”.

p. 64, l. 1. Correct the equation number.

p. 64, l. 5+. Correct the formating of the references.

p. 64, l. 10+. Cavalieri and others have a published paper with on this topic: http://dx.doi.org/10.1109/TGRS.2011.2180535. If this peer-reviewed paper contains the desired information, I’d believe it is better to use this source rather than an AGU abstract...

p. 64, l. 24. Please, add units.

p. 65, l. 3. Please, add a reference after “daily fields of snow depth” because the AMSR-E snow depth on sea ice product does not operate on MYI.

p. 66, l. 10+. Mention the link where the DAMOCLES data can be accessed.

p. 66, l. 29. The uncertainty in snow load dramatically contributes to the remaining error in sea ice thickness estimates. This should be mentioned.

Interactive comment on The Cryosphere Discuss., 6, 37, 2012.