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Interactive comment on "The impact of melt ponds on summertime microwave brightness temperatures and sea ice concentrations" *by* S. Kern et al.

Referee #2 (Greg Flato)

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The ability to remotely sense and discriminate between sea-ice, open water and melt ponds is an important topic for model evaluation, process studies and initialization of operation forecast systems. This paper provides a very in-depth development and application of a scheme to estimate aspects of the area fraction of the surface types listed above.

Thank you very much for your careful reading of our paper. We tried to keep the in-depth information in a clearer form in the revised manuscript which we re-wrote completely.

Unfortunately, the paper is very difficult to read. It is so densely packed with abbreviations and acronyms and excess technical detail that it will be unintelligible to most readers. Even if one has a background in this field, the presentation style is a real impediment to effective communication. I would very strongly urge a complete re-write of the manuscript with an eye to simplification, clarification and more organized flow of material.
We took this comment serious and completely re-wrote the manuscript.

We remove almost all of the acronyms to improve readability.

We structured the text much more than before. We start with a re-written introduction, have a data and methods section, where we go through the data sets one by one and have one extra sub-section in the section about MODIS data into which we put all the uncertainty discussion of the MODIS sea-ice parameters.

We completely removed plots and discussions about brightness temperatures and derived parameters (former figures 4 to 6). Instead we follow the comments of reviewer #1 and start with a comparison of AMSR-E and MODIS sea ice concentrations before we motivate why we use MODIS ice-surface fraction for which purpose. In the presentation and discussion of the results we try to keep the information easy and try to focus on the most important issues.

We focused on comparisons between AMSR-E sea-ice concentration and MODIS ice-surface fraction and discuss the results by means of discussing how some of the algorithms work. We add snow property changes to this discussion and give much less weight to the ice type discrimination which we show for completeness only – in form of tables.

We add the time dimension by additionally looking at the temporal evolution of brightness temperatures and contemporary MODIS ice-surface fractions to better understand and interpret our results.

We much more structured both the results as well as the discussion sections.

The same issues arise with the figures which are, like the text, almost impenetrable.
The authors believe that by removing old complex Figures 4-6 the revised manuscript has become easier to follow with less redundancies.

I am convinced that the length of the text and the number of figures could be reduced by half, which would also allow individual figure panels to be increased to a readable size.
We thank the reviewer for this comment. We find it however extremely difficult to cut down the length of the paper substantially without hiding too much important information.
We are showing an inter-comparison of SEVERAL algorithms. This requires showing a number of figures and also giving specific comments to these as they mostly reveal a NUMBER of differences.
In the new Figures 4 and 5, we reduced the number of images per figure by 2, i.e. show results from 6 instead of 8 algorithms. We also don't show figures anymore which are specifically valid for multiyear ice.

This requires careful consideration of what the key messages/ conclusions are, and what is the essential material that must be presented in order to substantiate these. There is no point publishing a paper that no one can or will want to read.
Thank you very much for this thoughtful comment. We agree. Still we find that with the new, completely rewritten manuscript with better structure and flow, clearer motivations of the steps carried out, and a clearer focus on the main issue, we can and should show the number of figures we show in the revised version. Actually, the new manuscript has 8 instead of 12 figures … but we submit 4 figures as supplementary material.

I read and re-read the Conclusions section multiple times and I must say I am still not clear on what the real take-home message is. Certainly there is a lot of detail about uncertainties and their source and the differences between different algorithms. But the last paragraph basically just says melt pond fraction is confounded with open water fraction in summer (something that has been well known since the early days of seaice remote sensing), that users should be aware of this, and that there nothing at the moment that can be done about it. Given all the preceding detail, it is surprising that nothing is said regarding which algorithms are more or less reliable and how a user might make choices when faced with a particular problem or application, or indeed how an 'essential climate variable' might be constructed. The second-last paragraph of the paper seems to provide some commentary on different algorithms, but having read it several times, I still cannot glean any concrete guidance.

We agree with the reviewer.

First of all we hope that the reviewer agrees with us that the material published in the paper warrants to write the conclusion in the form of a "Summary and Conclusions" section.

Secondly, we didn't and still don't see ourselves in the position to tell a user which of the algorithms investigated should or of should not be used and which is best.

One reason is that we look at only ONE summer. It is a case study.

The second one is, that we did not (and still don't) have a consistent set of summer sea-ice tie points to carry out this study and inter-comparison in a FAIR way. At least – and we are happy about this – we were able to use a consistent set of winter sea-ice tie points and thereby were able to mitigate differences between the algorithms by using published tie points which might have been retrieved under special conditions in different sea-ice regions.
As we show and discuss for the two Bootstrap algorithm modes, using the summer sea-ice tie points from the literature does not yield to an overwhelming improvement.

We give some clearer messages now about
a) which algorithm is particularly / not particularly sensitive to melt ponds,
b) how large are over- and under-estimation of the sea ice concentrations for the case where the open-water fraction is from open water between the ice floes and for the case where the open-water fraction is mainly from open water in form of melt ponds on the sea ice,
c) why the algorithms behave differently and
d) which algorithms are potentially better suited for optimization during summer than others.

So, my conclusion is that this paper requires quite a bit of work, and I would recommend major revisions.
The manuscript has been rewritten completely and we thank the reviewer again for pointing us to a number of key issues which we hope to have addressed properly in the revised version of the manuscript.