The Cryosphere Discuss.,
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The Cryosphere Discussions

Interactive comment on "Investigation of spatiotemporal variability of melt pond fraction and its relationship with sea ice extent during 2000–2017 using a new data" *by* Yifan Ding et al.

Anonymous Referee #2

Received and published: 18 December 2019

The manuscript is dedicated to the retrieval of melt pond fraction using remote sensing data. As input data, a higher level MODIS product (surface reflectance at 4 spectral channels) is used together with a set of training in situ data from various sources. The connection between the ground truth data and the observed surface reflectance is established by means of a multi-layer neural network. The obtained dataset is further compared to the existing melt pond fraction product and to the ice extent data, in order to check the prediction skill of the melt pond data in spring relative to the minimum ice extent in autumn, according to the well-known publication of Schroeder et al.The motivation for the presented work is solid and is well-presented. At the time of writing, there are no published long-term melt pond fraction data sets. The existing MODIS

data set (Rösel et al) is not continued, as well as the alternative product from MERIS (Zege et al, stopped due to sensor failure). Therefore, the topic of the publication is certainly up-to-date and a new melt pond fraction data set is of high importance to the scientific community. The topic of the manuscript fits into the scope of the journal as well.

However, the transparency and quality of the presentation is sometimes so poor that it is hard for the reviewer to decipher e.g. which data were used or which method was applied. The main concerns are listed below:

a) The weak points of the manuscript are the network training and the validation. It is far too early to include MPF trend and MPF map analysis before these are sorted out, as well as claim to outperform another retrieval. The provided description of the in situ training data, validation data, and validation results are insufficient and do not allow to assess the performance of the retrieval.

Please provide:

- a detailed description of the training and the validation datasets you use - current description is confusing and hard to understand. For each dataset, the size of the sample, spatial resolution, spatial coverage, temporal coverage, and the method of spatial and temporal collocation should be clearly stated. For each 8-day MODIS composite that you compare, how many days offset to in situ data do you allow? when you do that, do you have any assumptions about the evolution of MPF? how to do you train a neural network for 8-day composites using single day in situ data, and how do you compare those for validation? In a 8-day composite, which is not an 8-day average, you do not really know which day a given pixel stems from - or did you use this information?

- The Webster validation dataset which is your only independent validation dataset is either a typo or just wrong, there is no such dataset in that paper. Please double check.

Line 149-152: You state that the validation data by Webster et al 2015 supposedly

stems from 2000-2014 and has resolution 8 to 25km2. When I look into that manuscript I discover that the study by Webster et al 2015 is a fine approach to classify optical GFL images of 1m resolution, using collocated APLIS 2011 field campaign data - please see Table 1 in Webster et al 2015 for a list of the used data. These data are from 2011 only and have spatial resolution of 1 meter. I cannot find any other data in the manuscript by Webster et al 2015 which stems from 2000-2014 and has resolution 8-25km2.

- a scatter plot with "original MPF" and "retrieved MPF" on the axes, where each data point of the training and validation datasets can be seen, as well as the size of the sample, also for the Webster dataset. Your Fig. 4 cannot be used as the validation plot.

- make sure to use the original MODIS resolution and the finest spatial resolution of in situ data, both datasets also temporally collocated, to ensure a good quality of the comparison. For the transparency, it would be a good idea to provide case studies where you plot e.g. reference aerial values on your retrieved MODIS MPF map and dicsuss the discrepancies.

b) It is not sufficient to train a neural network only for melt ponds disregarding both open water and surface variability - it has been already mentioned by other reviewers and I 100% support this important concern. In the MPF maps (Fig. 10,11,12) the MPF along the ice edge stays constantly at the maximum value of 0.5 throughout the summer, although the FYI cannot hold the maximum pond fraction after melt peak due to the increased ice permeability and pond drainage (Polashenski thesis and other works). From this one can conclude that this high MPF value is rather connected to the low ice concentration at the ice edge and not to the MPF. Certainly, this problem is present not only at the ice edge, just not as clearly visible as at the ice edge. This issue is currently not solved, not discussed and has to be in some way addressed.

c) the structure of the manuscript: should you consider extending the descriptions, discussion, adding new plots and case studies as suggested, then the material from

3.2 onward would be far too much for one publication. You might also need to retrain the neural network for satellite from single days or include ice concentration in the equation, so the trends and MPF maps need to be updated as well. The reviewer suggests that you rethink and reduce the structure and focus on the quality of the research and the methodology first, so that the results that you claim would be clearly supported by your investigations.

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