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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.

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Comments to

Investigation of spatiotemporal variability of melt pond fraction and its relationship with sea ice extent during 2000-2017 using a new data

by

Ding, Y., et al.

Dear authors,

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with great interest I read your interesting paper about a new attempt to apply ANN to MODIS optical / NIR reflectances with the purpose to retrieve melt-pond fraction on sea ice in the Arctic.

I have the following concerns / questions.

I can understand very well that your ultimate goal is to (finally) derive a long melt-pond fraction time series for Arctic sea ice so that you can improve its geophysical interpretation. This is needed and very timely.

However, I find that both, the description of your methodology (see 1) and the description and amount of your evaluation activities (see 2) seems not yet to justify usage and exploitation of the data set the way presented in the current manuscript. I believe, if I were you, then I would kick out almost all figures / discussions dealing with the geophysical interpretation of this (so far) insufficiently evaluated data set because these interpretations could be very misleading. I would suggest to purely focus on the algorithm and results and their evaluation; this would be more than enough stuff to publish.

1) Using ANN requires optimal understanding and preparation of the input data and an accurate description of how you actually applied the ANN. In your case this applies to both the reflectivity data and the melt-pond observations. While you state which MODIS data set you use [which one was used by Rösel et al.?] it is not clear i) which collection this is based on (4,5,6?), and ii) how accurate the cloud masking indeed is for the high-latitude regions. So far I found limited evidence in the documentation of MODIS reflectance data that particularly over high latitudes (in contrast to lower latitudes) the uncertainties / biases due to clouds, cloud shadows and fog have been substantially improved. It would be good to better specify what is meant by "low view angle, absence of clouds [quality flag used?], cloud shadows and aerosol loading" so that other scientists could repeat your analysis. Please also note my comment to Figures 10 and 11.

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Furthermore, I am concerned about your training data sets. Their description is very short and does neither sufficiently explain the different degrees of reliability between ship-based visual observations and air-borne observations nor does it comment on the accuracy of the data. What is called "resolutions" seems to be the coverage of one observation "footprint". The description lacks which additional data are used (sea-ice concentration from these ship- and air-borne observations) and it lacks to give examples which allows the reader to get an impression about the actual kind of data you are using. What seems in addition to be stated insufficiently detailed is how these data are pre-processed to be used in the ANN.

2) Your evaluation and presentation of the results appears to be very global. The only "true" kind of evaluation figure is Figure 4 and if I am not mistaken then there aren't any figures showing inter-comparisons of the actual melt-pond fraction for single 8-day periods with independent data. Wouldn't it therefore be a good idea to i) include map-based inter-comparisons between, e.g. the Istomina et al. data set or the Rösel et al. data set and your results, ideally these come along with scatterplots and/or histograms of the actual distribution of the melt pond fraction; ii) include overlays of airborne (and in-situ) data on both the input MODIS reflectance data and the resulting melt-pond fraction. iii) include an investigation about how melt progress is seen in your data set and how it is seen in the evaluation data sets used - if possible. iv) include a detailed description of how you co-located the different data sets. v) include a detailed description of the accuracy of both your results and the data used for training and evaluation. This could (and should) involve to include information about the sea-ice concentration in Fig. 10 and Fig. 11. One could doubt that your results are independent of the actual sea-ice concentration due to the dominating impact of any open water on the brightness temperatures used for the sea-ice concentration data set you used as sea-ice mask (which was one of the things avoided by Rösel et al. for good reason). It is also not clear to me (and seems not to be described in the methods section overwhelmingly detailed) how the reflectances of open water and melt ponds are unmixed efficiently enough to identify open water as open water and to not identify an actual melt pond as

a certain fraction of open water as well.

- You motivate (in Sect. 2.1) the inclusion of a fourth spectral band with the fact that by this action you are able to better discriminate property changes within the snow pack. While this might be an advantage for the early phase of melt (which you could have explained in more detail) it seems not to be clearly stated how this could improve melt-pond fraction retrieval at a later stage. I guess, one of the main suggestions for improvement in the Rösel et al. paper was motivated by the change in spectral characteristics over the course of the melt season resulting in a different spectral response of melt ponds on MYI compared to melt ponds on FYI. This is where I hoped that your paper would enhance the current state-of-the-art but I have difficulties to see this in the paper yet.

- Lines 120-122: You use a standard sea-ice concentration product as a sea-ice mask. While this is fine, several questions immediately pop up: i) what is meant by "revised NASA Team algorithm (Cavalieri et al., 1996)"? The year of the reference makes clear that it cannot be the enhanced NASA Team algorithm". ii) what are the specifications of this data set in terms of spatial and temporal resolution and how did you pre-process the data to match with the MODIS data? iii) passive microwave concentration have biases during summer as has been discussed, e.g., in Comiso and Kwok in 1996: "Surface and radiative characteristics of the summer Arctic sea ice cover from multi-sensor satellite observations" and in Kern et al. in 2016: "The impact of melt ponds on summertime microwave brightness temperatures and sea-ice concentrations". Doesn't using such sea-ice concentration data sets as sea-ice mask therefore require a more in-depth description of how you used the data and how the expected bias in sea-ice concentration influences your melt-pond retrieval?

- It appears to me that you did not yet adequately cite the MODIS melt-pond fraction data set of Rösel et al. (2012) which you are using in your overall comparison (e.g. Figure 4). Would you mind to check which version of this data set you used and provide the doi and version of it in your reference list? I guess this would help other potential

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users to locate the correct data set.

- Lines 148-153: I checked the Webster et al. [2015] paper. I have serious doubts that this is the correct reference. I found that this paper basically compares a new method to derive melt-pond fraction based on APLIS campaign data and compared the results with SHEBA data. I did not find the mentioned 2000-2014 MPF data set. Here you would appreciate a hint about where to find this potentially very valuable data set.

- Figure 5: This figure states an average (2000-2017) pan-Arctic melt-pond fraction of 10% already in the middle of May. This appears to be too large. While Liu et al. (2015) found a similar evolution they used the old Rösel et al. melt-pond fraction data set which was erroneously high and which has been corrected based on the findings presented in Mäkynen et al. (2014). As you state yourself in the paper, melt onset typically occurs early June and I'd even state the melt onset for the majority of the MYI is in late June / early July which is when you suggest a melt-pond fraction over MYI of 15% already.

- Figures 10 and 11: These are 8-daily estimates of the melt-pond fraction. How come that compared to the Rösel et al. product there are no gaps due to clouds? It appears to be very unlikely that the more recent collection of MODIS data you used does contain less pixels flagged as cloud covered. This gets back to my general concern about the degree of detail in the description of the input data and then also in the results.

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