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The Cryosphere Discussions

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

Anonymous Referee #1

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In this paper Kern et al. discuss the potential impact of surface meltwater on passive microwave retrievals of sea ice concentration. The work is of interest to a broad community because it evaluates a well known, physics-based issue with sea ice concentration retrievals against an independent dataset. The methodology chosen is novel and appears to use the best available data type for comparison (MODIS melt pond retrievals from spectral mixing algorithms). The comparison is therefore useful and likely impactful. The data has issues however, and the potential exists for many other types of noise in the MODIS pond dataset to impact the conclusions the authors present. The authors present many caveats about drawing conclusions based on this comparison (e.g. Page 12 line 23). The reviewer felt the authors could do a better job clarifying

what conclusions remain firm and concrete regardless of all the errors and uncertainties, vs. which ones are on shakier ground. For example, it appears the conclusion that microwave models result in a value that exceeds ISF in summer is solid, but that the connection between ice type and overestimation is very sketchy (we develop this below). The reader could be more clearly provided this information.

The reviewer has significant reservations about the robustness of several of the conclusions reached, due to the substantial uncertainty in the MODIS melt ponds product used. These reservations are made much more pronounced because this work primarily uses gridcells above 85 degrees latitude, where MODIS, which is in a sun synchronous orbit, does not directly overpass. The off-nadir observations of surface reflectance at these high latitudes incorporate higher atmospheric path length and are much more impacted by surface roughness (the high parts of a rough surface are over-represented). Observations in this area are also impacted by low solar zenith angle, resulting in considerable shadowing, which has significantly different impacts on ascending and descending passes of MODIS, due to viewing geometry. Impacts of clouds and particularly cloud shadowing are higher at large off nadir angles as well. Selecting only ice of high concentration also necessarily subsets the MODIS data. The quality of this subset of the MODIS data should be addressed. There are many reasons why the data would not be as robust within a single extreme of the spectral mixing solution, such as near 100% concentration. For example, the spectral space between water and ponds is relatively small, and ocean water (even where not underlain by ice) at the edge of floes has a blue spectral signature more similar to ponds. This is due to both atmospheric distortion and scattering light transmitted through the ice in the upper ocean adjacent to floes. What is the potential that such narrow leads are interpreted as melt pond fraction more commonly in high ice concentration – leading to an import 'noise' in the MODIS pond data that is unique to high ice concentrations?

The reviewer feels the authors have not adequately addressed whether the passive microwave algorithms perform as designed – a critical question for most readers. The key here is that the algorithms ARE NOT designed to produce ISF in summer, even if theoretically that is what they SHOULD see. These products are SIC products. So over-predicting ISF actually may indicate that the products are behaving exactly as designed – and are therefore empirically adapted to overcome the fact that the response should be based on ISF. The reviewer feels the authors must plot MSIC against SIC and evaluate whether the algorithm is actually working for the wrong reasons, rather than stating that the algorithm SHOULD theoretically produce ISF, and because it over-predicts this, it is inaccurate. Discussion and plots of ISF vs SIC can be retained and discussed in great detail, but the paper should not be published without comparison of SIC and MSIC.

The reviewer believes the author's work would have more impact in the future if more time is spent on refining and clarifying its presentation. As an overall impression, the reviewer found the results section quite dense and noted considerable redundancy in some discussion throughout the paper. The reviewer suggests authors try to consolidate statements to their appropriate sections to reduce these redundancies. The reviewer also noted excessive detail in describing some sections of the results that was not paired with relevant analysis – this made some sections a bit rote. Some of these descriptions of the data could be reduced and organized following clear statements about what they show. The reviewer also strongly encourages having an editor go over the text. There are many punctuation errors (particularly dozens of missing commas) and many instances of plural subjects with singular verb conjugations (i.e. the sentence on page 10, line 22-23) and several other odd wordings which may be hard for a non-native English speaker to eliminate.

There are lots of acryonyms being made up in this work (TB, ISF, SIC etc). The casual reader will not read the paper from end to end and/or may have different ideas of what these mean from prior works. As written, a thorough reading is required to find and becoming conversant in all these new acronyms. The reviewer strongly suggests a table of acronyms be created and placed into the document near the beginning. The

reviewer also regularly became confused about the origin of particular data products. Since the key to the entire paper is a comparison of MODIS-derived vs. microwave-derived products, all MODIS derived products should be somehow clearly differentiated from all AMSR-E or SSM/I derived products in the acronyms (ISF, for example is MODIS-derived, but not denoted as such in a manner similar to MSIC). Perhaps all MODIS derived product acronyms would start with 'm'.

Specific comments 1. Several times it is mentioned that brightness changes in the sea ice surface itself may counteract some of the melt pond covering. Please quantify the relative magnitude of brightness changes compared to melt pond flooding. 2. Page 2 line 18. These references are not the most appropriate for describing the physical processes of melt pond formation. Eicken et al., 2004; Polashenski et al., 2012; and Landy et al., 2014 are more focused on physical processes of pond formation. Perovich and Polashenski, 2012 is primarily focused on the evolution of albedo, as is Perovich et al., 2003. Petrich does discuss the connection between snow and pond locations. 3. Page 2 line 22 – "can cover up to 50-90%" this is an un-necessarily sensationalist statement, particularly for a paper which is trying to quantify the TYPICAL impact of ponds on SIC retrieval. It would be more appropriate to discuss the TYPICAL coverage of melt ponds rather than the EXTREME bound. The references here are also not particularly relevant. Eicken is not primarily focused on pond coverage but rather on the processes controlling ponding, papers published by Perovich in 2011 only reference other direct works on melt ponds. Yackel and Barber is appropriate, but only one of many. Also, Landy et al., 2014; Polashenski et al., 2012; Hanesiak and Barber etc. Futher, some of the references here and elsewhere are not found in the reference list (e.g. Perovich et al., 2011) 4. Page 2 line 25 – Albedo values certainly vary, but these albedo values are simply incorrect. Dry snow covered ice has an albedo of about 0.8. Bare, unponded melting FYI has a value of about 0.55 +- 0.1, depending on reference, and MY has 0.6+- 0.1. Melt pond covered ice tends to be lower than 0.5. Ponding therefore does not reduce ice albedo from 0.8 to 0.5, but rather from somewhere in the range 0.45-0.7 to somewhere in the range 0.1 to 0.5. Perovich, 2003 is a good reference for MYI, but does not reflect current state of the literature which increasingly works on FYI. Perovich and Polashenski, 2012 describes FYI ponds, as does Frey et al., 2014. 5. Page 3, line 7 – This discussion of noise is important as the justification for trying to tease out why SIC from passive microwave products might be 'right for the wrong reasons'. It is worded poorly, and hard to follow. The project name and reason for conducting it is also not so important and could be dropped. The key is that the paper is largely about understanding whether the passive microwave products are interpreting changes in pond coverage as a type of noise in the SIC record. 6. Page 3 line 15 "Melt ponds are pools. . . " Redundant. this was already established and could be deleted. 7. Page 3 Line 16 - on penetration depth of passive microwaves. Passive microwaves are emitted from the sea ice and snow – as is correctly stated here. They are attenuated by liquid water. Discussing them as if the 'penetration depth' is limited would be language more appropriate to an active sensor with energetic waves PENETRATING from above. In this case waves EMITTED from below are being attenuated along the path to the sensor (because it goes through water). A novice reader could better understand that the ponds are attenuating a signal from below. 8. Page 5 MODIS data – Pond algorithm is executed using C5 data – several recent papers have suggested that significant uncorrected sensor degradation was present on C5 data (e.g Lyapustin et al., 2014). Though the degradation is only a few percent, it is not the same on all bands, meaning spectrally based algorithms can be significantly impacted. It would be useful to comment on whether this impacts the MODIS melt pond retrieval meaningfully. 9. MODIS data- It appears the locations you are focusing on, with high ice concentration, are all very far north. Here the MODIS data quality is likely to be quite poor because MODIS sun synchronous orbit does not place the sensor over high northern latitudes, and all retrievals are made at high off nadir angles, with low solar zenith angles, through long atmospheric path lengths. Under these conditions the MODIS surface reflectance products are well known to have substantial issues. The authors must at address this. The reviewer feels this is an important enough issue that the authors must examine whether the conclusions about SIC over representation by

passive microwave method apply at lower latitudes where MODIS data is likely better. Such a comparison may reveal that the FYI /MYI differences are not actually cause by ice type. This may require restricting the timeframe evaluated if 100% ice coverage is required. 10. Page 6 Paragraph 3, bias correction – Where does the 3% global addition to the MSIC come from? 11. Page 8 paragraph 3 – Ice age. It should be discussed and understood that the ice age within a 4 year cell is actually mostly less than 4 years, because all leads forming over the 4 year duration that at least some ice remained in the area refroze as younger ice. Many 4+year packs are composed of a large fraction of younger ice. As a result, this may not be a very effective mechanism for avoiding the influence of FYI. If the authors wish to really focus on the MYI/FYI differences they may consider using back trajectories to examine where the MYI was at the end of summer in the previous year, and eliminating MYI originating from areas of low ice concentration (where likely much FYI formed between the MYI floes). 12. Page 9 line 14. Morassutti and LeDrew primarily show that depth of the melt pond is not causally related to spectral response in visible wavelengths, but rather related to the underlying ice properties. . . so the reference should be after the first clause, and the second should be deleted. Deeper ponds on MYI actually appear spectrally similar to shallow, early season ponds on FYI- again because the predominant factor is underlying ice properties. 13. Uncertainties of MODIS sets – this section would benefit from a summary/concluding statement. In total, adding up all these errors in the MODIS sets, do they or do they not have the potential to alter the fundamental conclusions of this paper. 14. Line 23-25. This sentence would be better stated "Passive microwave emissions from the sea ice are attenuated within a path length of several mm of water, at the frequencies. . . hence in theory, melt ponds fully attenuate microwave emissions. . . and appear the same as leads.' Discussion of penetration again may confuse the novice reader into thinking this is some type of active sensing. Same comment applies to top of page 15 in section 4.1 15. Page 11 line 11 – these other factors impacting brightness temperature are very important and they are brought up repeatedly but never really addressed. This redundancy should be eliminated and a more complete discussion of them should be included. What is the range of expected impacts from surface wetting on TB? For example- can the authors show that these are secondary in magnitude to the pond impacts? The reviewer is left concerned that the increasing brightness temperature of wet and metamorphosing snow could offset pond reductions in TB – leaving SIC algorithms right for the wrong reasons. 16. Page 12 last paragraph and page 13 first paragraph. Here would be a good place to quantify the TB change associated with these other changes. 17. Section 4. This section is dense and challenging to get through for all but the most intrepid reader. Reviewer suggests it will have more impact if presented more concisely, perhaps with several tables displaying results as a matrix of algorithm with over/under prediction. 18. Why not plot MSIC against SIC – perhaps the algorithm is actually working for the wrong reasons. 19. Page 17 Line 1- what gives the authors confidence that the range of bias in ISF does not exceed 10%, particularly given the small subset of the MODIS Melt pond data and extreme northern latitudes investigated? The reviewer is not convinced of this level of accuracy. 20. Page 19 Paragraph 2. This discussion about other factors strongly argues that this analysis should include investigation of smaller areas separately, so that impacts of melt timing can be considered. Last sentence of this paragraph is very long. The reviewer feels that such an analysis could greatly strengthen this work. 21. Section 4.3 Paragraph 1 and 2. This would seem to indicate that the NASA Team algorithm is behaving CORRECTLY at its stated purpose – observing SIC. Perhaps for the wrong reasons, but nevertheless, the NASA team algorithm does not target ISF as the authors reason it should, it targets SIC. It is not accurate, therefore to state that ISF is overestimated by NASA_Team algorithm, because this is not what the algorithm purports to produce. A note of this must be made here. Also, how does the team algorithm work? Does it effectively include an empirical correction that is handling a presumed melt pond fraction? 22. Section 4.4. This section is un-enlightening. The authors describe the plots but fail to discuss what these results mean or why they occur. 23. Page 22 line 28 – yes, but what would this reduction do to actual SIC – the parameter that the algorithm is designed to retrieve. 24. Page 23 line 21 – this reviewer is not convinced that the

MODIS ISF and MPF have this low of a bias under the extreme circumstances of high latitude, high off nadir angle, low solar zenith angle in the study region. 25. Page 23 line 24 – The reviewer finds this statement to be theoretically accurate but poorly informed. While the addition of more channels could theoretically increase the number of surface types discriminated, the spectral signature of FY ponds + MY ponds as well as Bare and Snow covered sea ice overlap considerably. Additional channels are extremely unlikely to add orthogonal information in this system and further differentiation is unlikely. 26. Page 24 line 3 – this reviewer is not convinced that the ice type relationship comes entirely from the microwave side of the data. MODIS ISF is also likely to be impacted by ice type, both due to changes in the spectral character of MY ponds vs FY ponds and due to the MY ponds commonly being more deeply recessed into the ice surface, and therefore less visible at high of nadir angles of MODIS. Also, see next comment. 27. Page 25 Line 29 – this is a very large majority of the data concentrated in MYI. This weakens the conclusions based on ice type considerably. Further, all the FYI is at a lower latitude, where the geometry of the satellite sensors is quite different. The reviewer feels that view geometry must be eliminated as a cause for the FYI MYI discrepancies if the authors are to retain discussion of FYI and MYI 28. Page 26 Line 11-15 – this is approximately the 5th time these other factors are mentioned in the paper. A considerable redundancy. Further, the reviewer finds none of the discussions of these other factors sufficiently quantitative for the reader to assess whether they are so large in magnitude as to alter the paper's fundamental conclusions. 29. Page 27 Line 14 – this statement is the thesis of the paper. The reviewer does not believe it has been adequately supported yet, because the authors seem to be ignoring that the SIC retrieval algorithms are calibrated for SIC retrieval, NOT ISF – even though, theoretically, ISF is the response they see. Melt ponds SHOULD be interpreted as open water based on theory. The data however, actually does not indicate that the algorithms ARE interpreting the ponds as open water. The over estimation of ISF means that the value produced is actually closer to SIC. This would mean that the algorithms are NOT interpreting the ponds as open water. Further, the statement below noting that the current

SIC algorithms produce SIC in winter and ISF in summer is also theoretically true but in practice unsupported by the data presented. The values do not represent ISF well. They are too high. They appear likely to represent SIC better. (Though such a comparison needs to be made) The reviewer believes the authors actually understand this distinction, but does not feel this has been clearly communicated to the reader yet.
30. Page 26 lines 27-33 – Further comparison is needed here between MSIC and SIC. Perhaps the algorithm has empirically corrected for MPF impact, for all the wrong reasons. An empirical algorithm which did this would likely produce above-100% values for 100% ice cover, non ponded. (ISF =100) Since these would be truncated to 100% by the user, this would not result in a SIC error in practice. Over estimation could also be correct if this were a case of 100% ice cover with 30% ponds, because the algorithms are supposed to be retrieving SIC. In this case it appears likely that MODIS would retrieve an ISF =70% while several of the algorithms would find SIC = 100%. Again both could be correct at their design function, even if theory says the microwave derived SIC should be seeing something else.

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