The Cryosphere Discuss., 9, C837–C857, 2015
www.the-cryosphere-discuss.net/9/C837/2015/

The Cryosphere Discussions

Open Access

TCD

9, C837–C857, 2015

Interactive Comment

Interactive comment on "Satellite passive microwave measurements of sea ice concentration: an optimal algorithm and challenges" *by* N. Ivanova et al.

N. Ivanova et al.

natalia.ivanova@nersc.no

Received and published: 21 May 2015

The authors would like to thank Dr. J. C. Comiso for the valuable comments that helped improving quality of the paper.

We will address the comments point-by-point (the answers are marked by A).

General Comments:

1. The primary objective of this study is to evaluate the performance of several sea ice concentration algorithms, identify the strengths and weaknesses of a selected few and come up with an optimal hybrid algorithm that takes advantage of the techniques used

in the higher performing versions. First, the authors selected 13 from 30 algorithms and evaluated the merits of each based on statistical and sensitivity analysis in conjunction with a set of validation data. The hybrid algorithm as put together by the authors may be an improvement over some of the other algorithms but they fail to properly provide a convincing evidence that what they ended up with is indeed the optimal and most accurate algorithm. Also, although the criteria used for choosing the hybrid algorithm are reasonable they are not exhaustive enough to take into consideration some of the weaknesses of the techniques they decided to implement.

A: The authors agree that it would be too ambitious to say that the outcome of this study is an optimal and most accurate algorithm, but this is indeed the impression the manuscript gives. There is obviously still potential for development in passive microwave algorithms. In the revised version of the manuscript we alter the focus: we emphasize that it does not aim at developing an optimal algorithm but rather identify the need for it and investigate some of the criteria that should be employed. We will adjust the title, abstract and conclusions accordingly, as well as where relevant in the main text of the paper.

2. They even failed to test other algorithms properly or at least use them as they are normally implemented for the production of sea ice data sets.

A: Please see the detailed answer to the Technical Corrections below where we explain the reason for testing the Bootstrap Algorithm in its two modes. For all the other algorithms their original versions were implemented. However, the RRDP tie-points were used instead of the original ones and no weather filters were applied. This was done to achieve a fair comparison of the algorithms. Please understand that what we aimed to do here in the framework of the ESA-CCI Sea Ice ECV project is a novel and fair way to inter-compare different retrieval techniques without (sometimes) subjective tuning to tie-points or application of (too) general filters.

3. Furthermore, the authors failed to show how they handle other parts of the ocean

where the algorithm does not work properly. Since it is a global algorithm and meant for climate studies, the authors should demonstrate that they are not retrieving sea ice in areas where they are not supposed to be found. In particular, strongly disturbed areas in the open seas as may be caused by strong winds and bad weather and coastal areas contaminated by land could have signatures similar to those of ice covered ocean. They tried to address the first but there is no demonstration that their technique really work everywhere.

A: The validation dataset locations in Arctic and Antarctic for open water are shown in the figures 1 and 2 of the paper, it covers different areas, including the areas where there normally should not be any ice (blue squares in the figures' left panels). This dataset only for the shown years (2007 and 2008) contains about 30 000 data points, which we consider to be sufficient, bearing in mind such extensive validation datasets have not been produced and used before for validation of sea ice concentration algorithms. The other years are covered less, approximately 4 000 data points per year, except the SMMR period with about 1000 points per year, but the full dataset extends from 1978 to 2011. We are confident that these locations represent the full amplitude of weather influence on measured brightness temperatures and hence retrieved sea ice concentrations. The reviewer could perhaps take into account that the present paper does not aim to "sell" the algorithm and to provide a complete set of validation results. These have to and will be addressed in another paper. The present paper basically deals with the challenge to select the most suitable combination of algorithms for a long-term climate sea ice concentration data set. These details will be added to the revised manuscript (Sect. 3.2).

4. A good land mask is also needed to exclude land areas that may change with time due to iceberg calving or surging.

A: The authors do agree that for production of a final SIC dataset it is important to implement a good land mask and correction of pixels closely located to land. The land mask should take into account the fact that different algorithms use different passive microwave channels with different footprint size. Implementation and application of such masks and corrections would solve this concern. However, production of a final SIC dataset is out of scope of this paper. For the consistent validation exercise completed here, such areas (in the vicinity of land) were not selected for the validation and evaluation of the algorithms. The primary focus was on 0 % and 100 % sea ice concentration (turned out to be 15 % and 75 % for the reasons mentioned in the paper) in open waters. Therefore contamination of SIC estimates by land has no effect on the results. The authors wish to underline that they are well aware of the problem the reviewer is mentioning. The reviewer might be pleased to learn that it is planned to include the approach published by Maass and Kaleschke (2010) into the production chain of the next version of the SICCI SIC product. This method allows correcting for land contamination independently of frequency used. As implementation of this approach has been planned since the beginning of the SICCI project we did not find it necessary to evaluate the different algorithms also for their capability (or incapability) to retrieve accurate SIC adjacent to land. The reviewer is mentioning land area changes due to iceberg calving - so primarily the Southern Hemisphere. The authors are also aware of this problem. An annually or even monthly revised outline of the ice shelf and glacier borders would be a target solution here. But it is beyond the scope of this paper to find the optimal solution for these problems because this is something outside the SIC retrieval approach and more similar to the problem with land contamination. Hence for the same reason as stated above we don't find it appropriate to discuss this issue in the context of this paper. This will be mentioned in the discussion section of the revised manuscript.

5. They correctly indicate that there are large errors in areas of meltponding and over thin ice regions but a real solution to the problem was not presented.

A: The manuscript may not offer solutions for such well-known problems as melt ponds or thin ice, but its merit would be in revealing more information about the causes of these problems and presenting a new approach to address them. What has been

TCD
9, C837–C857, 2015

done in the current study with respect to melt ponding on sea ice, and could be valued, is the approach that resulted in the data shown in Figure 4. Here we had to use another data source (MODIS) that is more capable of characterizing melt ponds on ice surface. For the solution of the melt-pond issue we would suggest that one could either use visible data and/or accept that passive microwave measurements interpret melt ponds as open water. Another aspect of the study that should contribute to developing of an optimal method is the use of thin ice thickness in evaluating the algorithms in presence of thin ice (Figure 5). This identification of the sensitivity of different algorithms is new information. These would probably be valued more if viewed as an endeavor to shed more light on a few long-standing difficulties in the way of developing a generic algorithm rather than offering an "optimal algorithm".

6. The scientific merit of this study is good and well founded and the creation of a robust algorithm that is acceptable to everybody would be highly desirable. However, the paper needs to be revised extensively as indicated below before its publication. First of all, the authors should be commended for pursuing this noteworthy project. Since the launch of SMMR, there has been some progress in making refinements to the algorithms but the same techniques are basically made leading to just minor improvements in the accuracy of the retrievals. It is not until now that an attempt is being made to evaluate the various existing algorithms and come up with a hybrid version that could better than any of the existing ones. The question is: how well did they succeed in coming up with such an optimal version? I find it disappointing that there are no comparison of real products. Ideally, the authors should give examples of products that demonstrate problems with existing algorithms. They should then show that their hybrid version eliminates or at least minimizes such problems. This should be done for various seasons and both hemispheres. They should also show some time series of ice extents and ice areas and demonstrate how the new technique provides significantly improvements in accuracy and reliability.

A: The text of the paper will be substantially changed in order to clarify the points raised by Dr. Comiso and, as we pointed out above, we will re-formulate the main impression the paper gives from "optimal algorithm" to what it was aiming at originally, namely to inter-compare and validate different algorithms using a reference dataset (which is public and free for everyone to use). The hybrid algorithm has according to these criteria some (minor) improvements relative to the original Bootstrap algorithm but is in essence very similar. When it comes to comparing real products, we find this to be out of scope of this particular study because this would mean evaluation of all the processing steps involved in production of a SIC dataset. To mention only some, these would be land-mask and land spillover correction, gridding, ocean-masks (climatologies of ice extent are often used to dismiss OW areas far away from ice). While all these evaluations would be very important, it would have been impossible to cover in one paper. Also, validation of time series of area and extent (and making a conclusion on how much improvement is achieved by using the hybrid algorithm) would require accurate daily validation maps for the length of the required time period, which do not exist yet. The novelty of this study is the use of a limited but very accurate reference datasets (the RRDP) and addressing some of the major problems, common to all algorithms, and inter-compare these algorithms in a transparent and objective way. Our attempt of being objective can be seen in our efforts to keep algorithms like the ASI and the NT2 in the loop even though they cut off SIC at 100% or 102%. We constructed artificial 75% and 15% sea ice concentration datasets to evaluate potential biases across ALL algorithms considered. We tackle known problematic areas such as thin ice and melt ponds. For the first time we can now visualize - using real ice thickness information - how different algorithms are biased towards too low SIC values over thin ice. For the first time we visualize how different algorithms fail to provide a physically reasonable estimate of the net ice surface fraction during summer conditions. Maybe the reviewer could see that this goes beyond showing time series or maps of sea ice concentration (anomalies) of different algorithms with different tie-points applied to different sets of brightness temperatures using different weather filters.

7. In making the evaluations, the authors did not do a good job in their analysis of the

various algorithms. For example, they separated the Bootstrap Algorithm as has been described in literature into two algorithms: one using the 18V versus the 37V set, which they call CV, and the other using the 37H versus the 37V set, which they call P. The two sets needs to be combined and are usually used to complement each other with the P-set utilized mainly in highly consolidated area where ice can be retrieved at a high accuracy (using this set). The CV set is then used for the rest of the data to take care of areas where the P-set does not do a good job such as in ice cover areas affected by layering in the snow and ice cover. Separating the two sets in an algorithm would compromise the overall accuracy of the retrieval.

A: Please see the detailed answer to the Technical Corrections below where we provide the justification for testing the Bootstrap Algorithm in its two modes. We found that even though this algorithm showed very good performance, it was somewhat better, if we used Bristol over areas of consolidated ice instead of Bootstrap P, while keeping Bootstrap F for lower concentrations. This point will be added to the discussion section.

8. Their assessments of atmospheric and emissivity effects is also not so accurate. The scatter plots show that the data points in the consolidated ice region form a well defined cluster that are basically confined along a line that is then used as a reference or “tie points” for 100% sea ice. With a few exceptions, the effect of different weather conditions and different surface emissivity of sea ice is to cause the data points to move along this line. Hence, the accuracy is not altered as long as the tie point for ice is estimated properly. The other issue is in the use of stability through statistical analysis as the key criteria for validation. Stability may not be a good measure in many cases since a poor retrieval of sea ice cover can be consistently wrong. There should be a direct comparison with real data on sea ice concentration in two dimension to illustrate that the algorithm captures the spatial distribution of sea ice properly. I saw an earlier data set using the recommended technique and I find sea ice concentrations north of Greenland that are less than 95% in winter or substantially less than other parts of the Arctic basin.

A: The presented RRDP exercise shows that varying emissivity does not only generate variations along the line but also perpendicular to the line (as do some atmospheric effects). These effects are in fact the main reason for algorithm uncertainty, and in our dynamic tie-points approach we use this variability to estimate the uncertainty. Earlier papers on ice emissivity, such as (Cavalieri 1994), show exactly that some ice types (or mixtures of ice types) have emissivities that differ from the 'ice line'. It is correct that stability can be systematically wrong, which is the reason why we use a reference dataset that is distributed all over the Arctic (and the Southern Ocean). Since this study is devoted to algorithm inter-comparison, the prototype dataset, which is the one the reviewer is referring to in the last sentence of his comment above, should not be included into the discussion. The authors stress again that the present paper is not about the validation of the SICCI SIC retrieval algorithm but about the challenging steps to decide which hybrid of which algorithms could have the best performance and why.

9. Finally, they failed to provide solutions to basic requirements of a good sea ice concentration climate data set. One requirement is a land/ocean mask that would separate land covered areas which are not of interest from the ocean region which is partly covered by sea ice. Such mask should take into consideration the different requirements of different sensors which usually have different resolutions. Another requirement is a technique that takes into account of land contamination in ocean pixels. In this case, the contamination of pixels near coastal areas by land causes the algorithm to estimate non-zero ice concentrations in such areas where sea ice is not expected (e.g., coast of Spain). Some visual comparisons of actual ice concentration maps would also be useful. The impacts of not taking care of these requirements can be more serious than some of the issues, including the atmospheric effects, that the authors are so worried about.

A: The aim of this paper was to document the algorithms' skills rather then a final dataset quality assessment. The difference is that the dataset production chain con-

TCD

9, C837–C857, 2015

tains several implementation and processing steps, which we do not aim to address here. Such steps can be for example, use of climatological masks, correcting land contamination effects and gridding from swath to daily maps. This study is devoted to a systematic evaluation of the algorithms. For this purpose a limited but very accurate reference dataset (the RRDP) was built. Therefore we do not show inter-comparison of maps. We will make this point clear in the revised manuscript.

10. A third requirement which they actually tried to address is that of an open ocean mask or weather filter. They use RTM for this purpose and indicate improvements in the distribution of the open water data. However, they should demonstrate that they are consistent in removing all erroneous data with their technique and also ensure that they are not deleting data (e.g., 15% to 30%) that is used to define the ice edge.

A: The concept of RTM correction was introduced in order to avoid removing ice. The drawback of this approach compared to weather filter is that it does not remove all atmosphere over the ocean, which leaves some noise that cannot be corrected for (cloud liquid water, and some from wind speed and water vapor). We will provide more explanations to make relevant sections (3.5, 4.4 and 5.5) clear in the revised manuscript.

Specific Comments:

p. 1272, line 6: I agree that the uncertainties in the summer are high but they are primarily caused by surface melt and meltponding. Large errors at the ice edge do not happen only in summer but in other seasons as well and they are basically caused by variations in the emissivity of new ice and the effect of side lobes that causes a smearing of ice edge location as the satellite crosses the ice/ocean boundary from different directions.

A: The authors agree that this formulation is not clear enough in the text. The message was that the uncertainties are large in summer and at the ice edge, but in the explanation of the reasons that follows it is not very clear which are more relevant to

TCD
9, C837–C857, 2015

each of these situations. For example, atmospheric contribution and wind roughening are more of a problem for low and intermediate SIC values, while emissivity variations meant in this particular context are relevant for consolidated ice areas. The summer issues (surface melt) are addressed in more detail later in the Introduction (p. 1273, line 23). We do not address smearing and footprint mismatch uncertainties in this paper because this would more naturally belong to a paper on production of a final dataset, where all the uncertainty components should be discussed. Note however that the passive microwave data used in the evaluation were footprint matched. The text will be re-formulated in the revised version of the paper.

p. 1272, line 21: In consolidated regions in the Arctic, the accuracy in the retrieval that takes into account spatial variations in emissivity and temperature is about 2.5% (see, Comiso, 2009, Vol. 29, p. 203, J. Remote sensing of Japan).

A: This work will be cited in the Introduction.

p. 1272, line 28: The statement that starts with “The apparent. . .” is incorrect. Kwok (2002) did not make an assessment of emissivity fluctuations in the Arctic – such assessments were done by others including Comiso (1983) and Eppler (1992). It is hard to tell which one is secondary and which one is primary. It is more accurate to say that for retrieved concentrations higher than 97%, the actual percentage of open water may range from 0 to 3% because of uncertainties in the 100% ice tie point.

A: Wrong citation was inserted after this statement; it should be Andersen et al 2007 instead of Kwok 2002 (which is cited earlier in the text). Will be corrected in the revised manuscript.

p. 1273, line 4: The impact of water vapor and cloud liquid water is to change the effective emissivity of the surface. Such effect is already included in the determination of “tie points” for sea ice and water.

A: This is correct, the effect is included; especially when the tie-points are sampled in various areas they should cover various local weather conditions. However, it is still an averaged value that is used in an algorithm (except ECICE which works with distributions) when calculating SIC. This gives one value for each tie-point per day. There will be variation of real Tbs around this value, and part of them is explained by the mentioned atmospheric effects that deviate from that average value. The atmospheric correction suggested in this study decreases this deviation (not for cloud liquid water though, which is explained in the text).

p. 1273, line 6: Wind effects on surface water signature is not as much within the ice pack as in the open seas. In the open seas, weather filter or ocean mask is normally used. Within the pack, the change is less significant but is included in the estimate of the ocean tie-point.

A: The effect is indeed less significant within the ice pack, mainly because one would expect much smaller fetch for wind to work in the openings/leads in consolidated ice. However, for the areas of low sea ice concentration or open water (where ocean mask is not applied) the weather filters remove also part of actual ice, and not only false ice retrievals, as we show in the Figure 6. Therefore, we emphasize the importance of this effect and suggest applying atmospheric correction. Development of the existing weather filters to solve this issue could be an alternative solution. It could be questioned whether the wind effect which is included in the estimate of the ocean tie-point is the valid one to be used within the sea ice cover. The ocean tie-point is estimated for open water well away from the ice edge. Hence the fetch is long enough to provide the full spectrum of waves and foam coverage. Inside the sea ice cover the same wind speed will cause a different set of water surface modulation with potentially a different wave spectrum and less foam and hence a different radiometric signature compared to the open ocean.

p. 1273, line 29: Meltponding is indeed a big issue but note that it is a problem for only two months. For this period a special algorithm needs to be designed to improve ability to obtain more accurate results.

TCD

9, C837–C857, 2015

Interactive Comment

A: We agree that development of a new algorithm (for example, based on optical measurements) would be beneficial to support passive microwave measurements in summer months. We will add this point to the discussion section.

p. 1274, line 7: Thin ice is a problem because the microwave emissivity changes with thickness and there are two basic types, namely, nilas and pancakes the signature of which are also different. Effects on heat fluxes are also different. There needs to be a means to identify thin ice unambiguously to be able to utilize any thickness algorithm from passive microwave data.

A: This is a valuable remark, however we would like to keep this paragraph unaltered in terms of amount of detail, since it was not the purpose of this study to retrieve sea ice thickness from passive microwave data. We merely assessed SIC over areas where we identified the fact of presence of thin ice from SMOS and SAR.

p. 1275, line 18: The Bootstrap algorithm should not be split into two since it takes advantage of both polarization mode and the frequency mode. The frequency mode is relatively stable but it has problems including more sensitivity to temperature and emissivity than the polarization mode. On the other hand, the polarization mode does a better job in highly concentrated (near 100%) sea ice cover.

A: Please see our detailed answer to the Technical Corrections.

p. 1283, lines 15-20: There should be a demonstration that the use of RTM for the ocean mask or weather filter works everywhere. Using a model to generate geophysical product is not a reliable technique especially if the atmospheric parameters needed as input by the model also comes from other models or historical data.

A: The result of RTM correction shown in the Figure 7 of the paper was obtained using the locations shown in Fig. 1 here.

We assume these locations cover different weather types (for some it is more common to have storms and strong winds, and some are typically more quiet). Total amount of points sampled in these locations amounts to 2320 and covers whole year of 2008, SSM/I. The improvement due to the RTM correction shown in the Figure 7 of the paper is an average measure for all these samples – we show that the standard deviation of SIC obtained from the algorithms becomes significantly smaller after the correction. Please note that some of these points were only used in summer, since there is ice at these locations during winter. This explanation will be added to the text of the paper.

p. 1284, lines 7-11: It is a mistake to consider only 15% and 75% cases. Most of the pixels within the pack have ICs close to 100%. Ability to detect the high concentration data effectively is very important.

A: Yes, the high concentration areas are important on their own, and accurate SIC retrievals for such areas would be much appreciated in a number of applications. In this study we aimed at inter-comparison of as many as possible of the main available algorithms (or groups of algorithms), which includes NASA Team2, ECICE and ASI. These algorithms though could not be added to the experiment for 100% SIC for the reasons explained in the paper. Therefore we made such choice – a tradeoff – to use 75% and include all the algorithms but thus miss the opportunity to address areas of SIC close to 100%. However this seems like a fine trade-off because an algorithm inter-comparison study focused particularly on high SIC has already been published (Andersen et al 2007). Please see also our answer to the Technical Corrections for more details.

p. 1287, line 10-14: Is it true that the NASA team IC does not go beyond 100%? If so, the ice tie point used is not correct and the estimated IC would be an underestimate of the real IC. The high IC for CalVal is in part caused by the high variability of the emissivity of summer ice and also to take into account the expected bias due to meltponding. The error gets significantly reduced in August when the surface starts to become dry and the emissivity becomes more stable.

A: No, it is not true that NT does not go beyond 100%. Actually if NT did not go beyond 100% the tie-point would be underestimated by our criterion (NT>95%) and the actual ice concentration would then be overestimated. In winter most of the data points that have NT>95% will actually have SIC very close to 100% (99-100%) since very little open water exists during winter. During other parts of the year (especially during summer) the average SIC for NT>95% might well be slightly lower than 100% (perhaps 97-98%) and our tie-point may cause a small overestimation of some ice concentrations by up to 3% in those periods. We consider this an acceptable possible bias (unknown) and a significant improvement over having a bias of up to 30% or larger. The high SIC during summer for CalVal (>130% in some locations) is due to changes in emissivity as well as changes in effective temperature. We do not believe it is the correct approach to handle melt ponds by 'overestimation' of the ice in between the melt ponds to make them look like ice. This will only provide the 'desired' result at one melt pond fraction and will still overestimate the ice concentration where the MPF is less than expected, and underestimate the ice concentration where the MPF is larger than expected.

p. 1288, lines 5-20: None of the existing algorithms does a good job on thin ice. Within the pack, thin ice forms in leads and polynyas and they are usually narrow and not easily resolved by the passive microwave sensors (especially SSM/I). The fraction of thin ice in most cases are usually relatively small and not much to worry about. Where it counts would be in large coastal and deep ocean polynyas where the open water or thin ice is represented by a significant number of pixels. In these cases, ability to identify them in the ice concentration maps (because of the bias) is actually an advantage since they are areas where heat fluxes are significantly different. Producing an ice concentration map that treats thin ice (including grease ice) on an equal footing as the thicker ice types would produce maps that are mainly 100% within the ice pack. A newly formed lead within the pack normally freeze within hours and would not be represented by such a map and an important information would be lost.

A: The thin ice we relate to in this study is newly formed ice in fall, but yes, large polynyas are of relevance as well. It can be important to be able to distinguish this ice as ice and not areas of open water because ice formation is an indicator of starting freezing season with all the relevant processes. For example, increased ocean salinity, or terminated wind energy transmission to the ocean. However, we agree that with passive microwave standard algorithms there is no way to distinguish thin ice from low concentration ice. More over, if areas of thin ice are interpreted as reduced concentration we should say so. This issue is similar to melt ponds in a way that there is no simple solution, and one should be aware of the limitation, which we demonstrate by the Figure 5. In general, it can be of interest to distinguish leads with open water from the ones with thin ice. For example, if a lead is wide enough to be affected by wind and provoke ocean convection; or for studying of brine rejection effects on the ocean stratification. But such division should be very hard to achieve by passive microwave methods alone. The authors suggest that in case of thin ice it might again be required to rely on data fusion techniques and instead of using only microwave radiometry to include independent data which permit discrimination between thin and thick ice and hence provide the desired information where an apparently (too) low SIC is caused by actual lower ice concentration or where it is caused by thin ice or perhaps even both. What is new here is that we manage to quantify the effect and thus allow sea ice modelers with a thickness distribution to assimilate ice concentration data in a more proper way.

p. 1280, lines 1-20: Losing <30% ice concentration is not acceptable and also, the authors must demonstrate for sure that there are no residuals. The other techniques used by other algorithms (e.g., NT2 and Bootstrap for AMSR data) are probably more effective and should be examined.

A: We did investigate the traditional weather filters (as used by the NT2 and Bootstrap algorithms) (see Figure 6) and found that they remove ice sometimes up to 30%. We agree that this is normally unacceptable and therefore we decided NOT to use these filters. Instead we decided to perform atmospheric correction of the measured Tbs using reanalysis atmospheric data (ERA Interim). This procedure reduces the atmospheric noise considerably but does not remove it completely. There will therefore be some residual atmospheric noise over the ocean. We argue that this noise is more acceptable in an ice concentration algorithm than the removal of ice, but agree that this is debatable and for some applications the removal of ice may be preferable. We did investigate the performance of NT2 at low concentrations and the 'weather correction' of this algorithm turned out to not perform very well (see e.g. Figure 3). Relevant sections on the weather filters and atmospheric correction will be made clearer in the text (Sect. 3.5, 4.4 and 5.5).

Technical Corrections: The Bootstrap Algorithm should be implemented as designed. Both P (37H and 37V) and CV (18V and 37V) techniques should be utilized in concert as described by the author especially when making the comparisons with other techniques.

A: The authors understand the concern regarding testing the two modes of the Bootstrap Algorithm separately, and would like to clarify this issue in more details, which they hope will justify their choice. They also admit that this point is not explained very well in the current version of the paper. This will be addressed properly in the updated version.

Here we offer a step-by-step procedure of the decision-making:

1. Since accurate intermediate SIC reference data are not available we have created validation datasets at 0% and 100%.

2. We validate SIC obtained by the algorithms using the obtained validation datasets for 0% and 100% and find out that some of the algorithms are hard to validate at these values because they cut-off the SIC at 0% and 100% (NASA Team2, ECICE), are affected by a combination of large bias and nonlinearity at high SIC (ASI). These effects cut part of standard deviation (see examples in Fig. 2 and Table 1 here: SIC100%, NASA Team 2 and ASI), while we aim at evaluating the full variability around these

reference values (0 and 100%). We implement the algorithms (except these 3) without cut-offs, allowing thus SIC values below 0% and above 100% as well. In order to be able to include these three algorithms in the inter-comparison, we produce artificial datasets (the procedure is described in the paper) of SIC 15% and 75%, and used them instead of 0% and 100% datasets respectively. We find that the algorithms' performance at 15% is representative of that of 0%, and so is 75% to 100%. Therefore we show only the 15% and 75%. By "representative" here we mean that the algorithms' ranking does not change significantly (Tables 1 and 2 in the supplement) even though the absolute values of standard deviations are different. We only show Northern Hemisphere here because the Bootstrap P scheme is originally used in this hemisphere (Comiso 1995).

3. The Polarization scheme (mode) of the original Bootstrap algorithm is applied only when Tb19V is above the AD line (ice line) minus 5K, that is when

Tb19V - (t1a+sad*Tb37V - 5) > 0, (1)

where t1a and sad are intercept and slope of the ice line (please see [Comiso 1995] for details). Otherwise the Frequency mode is applied. The threshold defined by this line can be converted to a SIC value, which amounts to values shown in the Table 3 (supplement) as obtained from our RRDP tie-points set. Both of our test datasets, 15% and 75% SIC, are well below these values, and therefore only Frequency mode would be chosen by the original Bootstrap scheme. However, we show the Bootstrap Polarization mode in the paper anyway.

4. Thus, we did not show in the paper the tests of Bootstrap P for what it is originally meant – near 100% SIC. We show this test here (Figure 3), and it is indicating that Bootstrap P performs quite well, but Bristol showed somewhat lower standard deviations and therefore was selected for the hybrid algorithm. Please note that the 100% SIC reference dataset may still have some small fraction of residual open water. This however, does not jeopardize our use of the minimum standard deviation as a mea-

sure of algorithm performance, since we are only looking for the relative differences between algorithms.

References

Cavalieri, D. J.: A microwave technique for mapping thin sea ice, J. Geophys. Res., vol. 99, no. C6, 12561–12572, 1994. Comiso, J. C.: SSM/I Sea Ice Concentrations Using the Bootstrap Algorithm, NASA Reference Publication 1380, NASA Center for Aerospace Information, 800 Elkridge Landing Road, Linthicum Heights, MD 21090-2934, (301) 62 1-0390, 1995. Maass, N., and L. Kaleschke: Improving passive microwave sea ice concentration algorithms for coastal areas: applications to the Baltic Sea, Tellus, vol. 62A, 393-410, 2010.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/9/C837/2015/tcd-9-C837-2015-supplement.pdf

---

Interactive comment on The Cryosphere Discuss., 9, 1269, 2015.

TCD
9, C837–C857, 2015

Interactive Comment

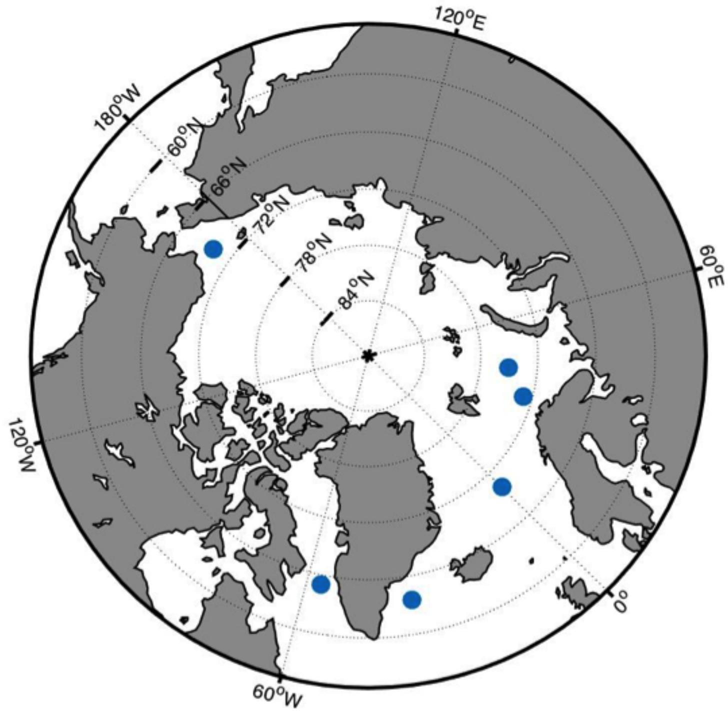

Fig. 1. Figure 1. Locations where the RTM correction was tested (Figure 7 of the paper).

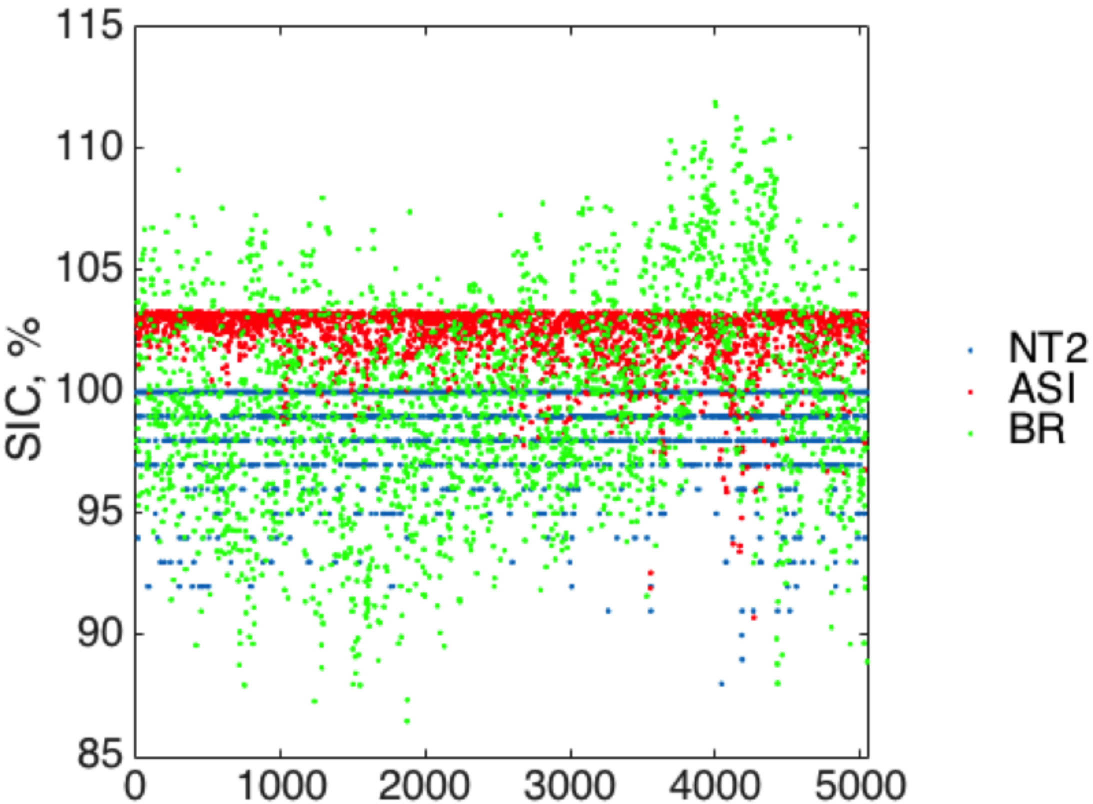

Fig. 2. Figure 2. SIC obtained by NT2, ASI and BR algorithms (BR is shown for reference) from the Tbs over areas of SIC 100%, SSM/I, 2008, winter.

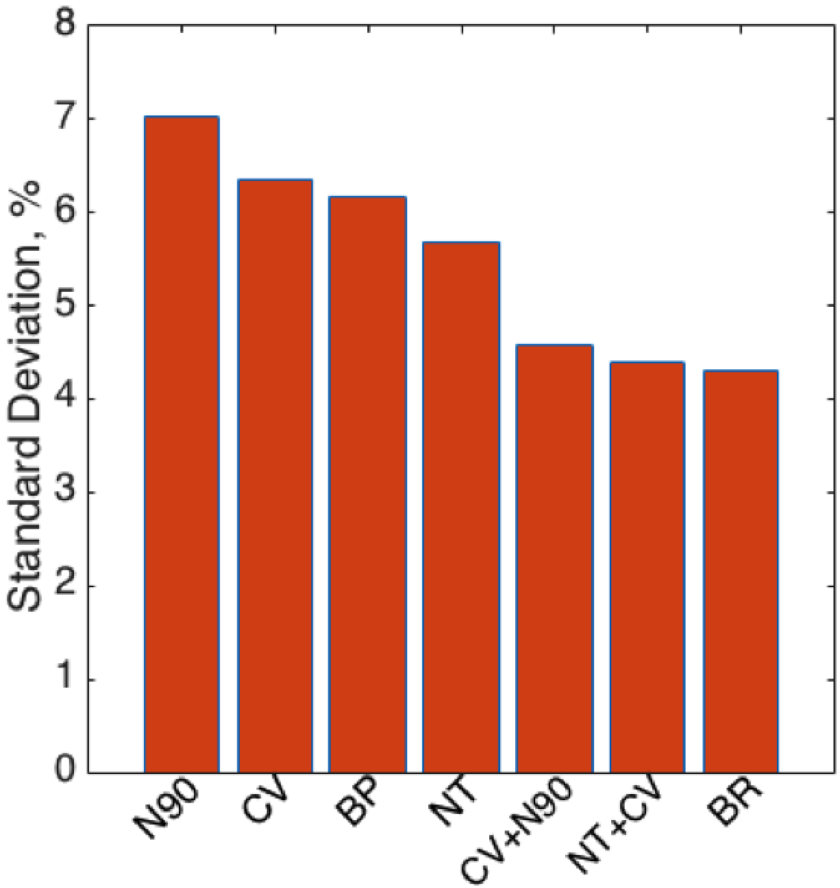

Fig. 3. Figure 3. Standard deviations from SIC 100% validation dataset: average 2007 – 2011, SSM/I and AMSR-E, winter, Northern Hemisphere.