The Cryosphere Discuss., 9, C443–C449, 2015
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The Cryosphere Discussions
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Interactive comment on "Satellite passive microwave measurements of sea ice concentration: an optimal algorithm and challenges" *by* N. Ivanova et al.

JC Comiso (Referee)

josefino.c.comiso@nasa.gov

Received and published: 17 April 2015

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

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.

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.

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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 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.

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.

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).

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.

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.

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.

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.

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.

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.

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.

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.

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.

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.

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.

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.

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