The Cryosphere Discuss., 9, C1124–C1132, 2015
www.the-cryosphere-discuss.net/9/C1124/2015/

The Cryosphere
Discussions
Open Access

Interactive comment on "The benefit of using sea ice concentration satellite data products with uncertainty estimates in summer sea ice data assimilation" *by* Q. Yang et al.

Anonymous Referee #2

Received and published: 2 July 2015

Review of

The benefit of using sea ice concentration satellite data products with uncertainty estimates in summer sea ice data assimilation

by

Yang, Q., et al.

This paper uses the MITgcm sea ice model to forecast the Arctic sea ice cover during summer (June to August) in terms of the evolution of its concentration and its thickness. For this purpose the MITgcm is assimilated using LSEIK with two different sea ice

concentration data sets. These do have uncertainty estimates. Different realizations of uncertainties are tested: two constant onces and to varying ones. The influence of using these for forecasting Arctic Ocean is investigated for concentration and thickness.

The paper is an important contribution to current knowledge and the paper is a good example for the usage of sea ice products WITH uncertainty estimates. Most of the paper is clearly written and well to understand already. A number of things and questions deserve more attention in my eyes, though, because of which I recommend to carry out some major revisions before acceptance of the manuscript for "The Cryosphere".

I detail my general comments in the following paragraphs. These will be followed by a number of other detailed comments before I will close the review with some hints towards typos etc.

General comments:

While the description of the methodolgy is fine - in my eyes - as far as it concerns the model and LSEIK some important questions and motivations remain open for the observational data sets. a) I have difficulties to understand why the authors compare a coarse-resolution (25 km) but newer sensor sea ice concentration (SIC) data set (AMSR-E SICCI) with a finer resolution (10 km) but older sensor SIC data set (SSM/I OSISAF). AMSR-E offers finer spatial resolution than SSM/I and I guess the producers of the AMSR-E SICCI data set had a good reason for keeping the grid resolution of this data set similar to the SSM/I SICCI data set. On the other hand I doubt that the 10 km grid resolution offered by SSM/I OSISAF is a "real" resolution because footprint size and sampling of SSM/I data usually allows for 25 km grid resolution if using the lower frequency (19 and 37 GHz) channels. I recommend the authors to motivate their choice a bit better and to also discuss whether the different grid resolution and different actual resolution of the different products might have had an influence on the results.

b) I am wondering why the authors did not also use the uncertainty information provided by the OSI-SAF SIC data set. Perhaps the uncertainty retrieval is the same for OSI-

SAF and SICCI and therefore it is sufficient to look at SICCI only?

c) The authors use an NSIDC SIC product - presumably based on the NT2 algorithm - to evaluate their model and assimilation results. While this is a fair approach to use the reader might miss some information about the why this product was chosen, what would have been the alternatives, whether it is important to have alternatives (at all), and what are the potential difficulties with the NSIDC product used. Currently the authors are risking that they rate the quality of their results with regard to SIC to a SIC data set which uncertainty and which bias are unknown.

d) The authors use BGEP ULS data to get a view of the sea ice thickness (SIT). I am missing two things in their investigation of that data set. First I would have liked to see more discussion about the large different in the spatial representativity of the SIT data from ULS compared to the model. Secondly, the authors used the SICCI SIC to convert the SIT into a sea ice volume to more easily inter-compare it with the model data. Why did they use SICCI? Why didn't they use OSI-SAF and how the results looked like with OSI-SAF (and its finer grid resolution)? Wouldn't it be more reasonable - from the point of view of that you are evaluating the impact SICCI SIC has in the model - to multiply the BGEP SIT data with the NT2 data against which you also reference the SIC model results? Currently, one might argue that sea ice volume as computed from BGEP data and model output in terms of SIT are not independent because both use SICCI SIC.

In the following I abbreviate page with P and line with L

Detailed comments: P2544, L22-26: I am wondering whether only the economic opportunities are driving this research. I would have thought that maybe seasonal weather forecast, climate model and also ordinary weather model data might be influenced by the changes we witness. Maybe the authors could be a bit broader here and motivate their study also from the science point of view. In this context I am wondering about the "risks" you mention (which are these?) and in particular about how these are "managed"?

P2545, L1: The authors wrote that data assimilation (DA) is important for accurate sea ice prediction because it is important to have a realistic initial state. Is this really the case? I would have thought that model assimilation with observations data is rather a tool to "push" or "keep" the model close(r) to reality ... i.e. not the initial state is important (this is the case with any model, right?) but the potential to continuously "supervise" where the model goes.

P2545 L9-10: I am wondering whether the authors also looked aside their own work a bit and maybe found other literature. how about Schweiger, A., Lindsay, R., Zhang, J., Steele, M., Stern, H., Kwok, R., 2011. Uncertainty in modeled Arctic sea ice volume. J. Geophys. Res. 116, C00D06?

Also, in L10, the authors write "efforts". Which efforts are meant here? Are you referring to the previous studies mentioned above? Or other studies?

P2545 L20: Here one could add a line that at the time of writing these two, SICCI and OSISAF, are the only two algorithms or products which come with a physically based sea ice retrieval uncertainty information - instead of an estimate of the spatio-temporal variation of the SIC within a certain grid area and time window which is a measure of the variation of the SIC due to actual SIC changes and due to artificial SIC changes implied due to algorithms' deficiencies to work under certain weather and/or surface property conditions.

P2546, L5: I would fine it helpful to fine a half-sentence saying that the motivation for using the LSEIK is given in the following section.

P2546, L5 and L22: One time the authors write LSEIK, the other time they write SEIK. If there is a difference between those and if the authors wish to highlight this then it does not become clear from the paper currently. If this is the same and/or if the authors mean LSEIK all the time, then they might want to change the name accordingly.

P2547, L19: "grid points" ... I would recommend to write "model grid cells".

P2547, L27: Can the authors please check whether the SICCI data set is avaliable on polar-stereographic grid? I doubt so. Also see my general comment a).

P2548, L1: "revised algorithm merging method" ... this remains unclear as long as the reader does not know that two (or more?) algorithms are combined in a hybrid approach.

P2548, L3: As the authors write "total standard error" they might want to also explain how this total error is composed. This is in a way also needed later in the paper in the discussion.

P2548 L7: "grid spacing" ... which grid is used here? EASE2?

P2548, L7-9: See my general comment c). Also the statement that the NSIDC SIC is independent from the SICCI and OSI-SAF data sets is not entirely true because SSM/IS is the successor of SSM/I and hence share the same channels and smae viewing geometry.

P2548, L13-15: I am wondering whether the relvatively old paper of Melling et al. is a proper reference for the BGEP moorings. How about in addition: Krishfield, R. and Proshutinsky, A.: BGOS ULS Data Processing Procedure Report, http://www.whoi.edu/fileserver.do?id=85684pt=2p=100409, Woods Hole Oceanographic Institute, 2006. This is at least a more recent reference.

Also, I am wondering whether the paper by Nguyen et al. is suited as a reference about how to convert ULS draft to thickness. I guess, when the authors look into that paper more carefully they will find a proper reference which can be used for this.

Finally: Did you use the conversion factor of 1.1 regardless of ice type? If so, you might want to discuss this in the discussion section.

P2548, L25: What do the authors intend to say with this last sentence? How do the authors define "open water"? When I look at the SIC maps provided later in the paper then I see SIC as low as 50

P2549, L2: What do the authors mean by one standard deviation? Does the model treat the constant uncertainty estimate of 0.25 as one standard deviation?

P2549, L4: What do the authors mean by "representation error"? Does this refer to the smearing uncertainty which is given separately in the SICCI SIC product as well?

P2549, L10-13: See my general comment b). In addition to that what happened in these to cases with grid cells where the total error was below the given thresholds. Were these not used for the model assimilation or were these used but with the uncertainty set to the respective value?

P2549, L14-16: The SICCI SIC product has flags where the SIC stems from interpolation and/or has been set to 0 or flagged invalid for various reasons. How about flagged grid cells not belonging to the north pole disc? Did you use this data as well? Did you also choose an uncertainty of 0.3 here? As you show later the total error in the area around the north pole disc is quite small, usually below 0.05. How does the assimilation cope with the jump in uncertainty from < 0.05 to 0.3? Another question regarding the SICCI SIC data set is whether the authors used the "regular" SIC product or whether they also tried to use the off-range SIC?

P2549, L20-23: This reads as if large retrieval errors (only) can be expected at SIC < 0.05. This is a quite small value and I am wondering how this value is justified. I can only speak from the remote sensing side and would say that elevated uncertainties start to pop up at considerably higher SIC than just 0.05. Also it appears to me that SIC = 0.05 is within the (total) retrieval uncertainty at this SIC range?!

P2550, L2: "... from, on average, 0.24 ... 0.11, respectively. ..."

P2550, L11: "time and space dependent" ... how about writing "the full range of"?

P2550, L12: Here the discussion of results in Figure 2 seems to be done. What explains the maximum RMSE at the end of June followed by a net decrease in RMSE into August for all but LSEIK-3?

P2550, L16: See comment d)

P2551, L5: At the end of this paragraph I ask myself: Why? Why does NOT using the full range in SIC uncertainty seemingly result in better results with regard to SIT? Where are the uncertainties of the range 0.01 - 0.1 located? Can we used the location of these grid cells as a potential explanation?

P2552, L1: The authors write that "the satellite based concentration estimates are known to underestimate the sea ice cover". I would add that you talk about microwave radiometry here. I would add references here which underline your statements written? For which algorithms is this statement valid? Is this a general phenomenon for ALL algorithms or are there better or worse algorithms to compute SIC? Maybe the SICCI reports can help or the paper by Rösel et al., in IGARSS 2012.

Also, one important comment at the end of this paragraph ending on P2552, L2: The uncertainties provided with the SIC data are a physically based retrieval uncertainty and NOT an estimate of the / a potential bias. A potential over- or under-estimation of SICCI SIC during summer is hence NOT reflected in the uncertainty estimate given.

P2552, L13-15: I agree to what you write here. However, these results have to be seen against what is used to convert draft –> thickness –> volume. The authors should always keep in mind what their reference data are and how well these are known. So, compared to NSIDC NT2 SIC, LSEIK-3 is better than LSEIK-4. Compared to BGEP SIT LSEIK-4 is a bit better than LSEIK-3.

P2553, L3: I would not use the term "under-estimation" here because as I wrote before the SICCI SIC retrieval uncertainty does not include potential biases and is not meant to do so.

P2553, L7: Having read the discussion I see the following 4 points missing: 1) The largest part of the SICCI SIC total error comprises smearing uncertainty outside areas of compact sea ice. this could / should be discussed. 2) See my comment d) 3) ULS

SIT and its uncertainty are not that clear to me. Also see my comment d). How would the SIT curve from ULS whould be like?

P2553, L10-12: Please rewrite sentence "While the ... distributors."

P2553, L13: "better estimates" ... implies how accurate the reference is. It is important to tell what is the reference.

P2553, L19: To me it is not clear what is meant by "mismatch"

P2553, L22: Again the under-etimation of uncertainties used here.

P2553, L24: How do you know? What is the basis for the knowledge?

And further: What makes you so sure that the mismatch does not occur during winter? Did you check it out by yourself?

In general: What would you expect using different assimilation processes?

Typos, etc.:

P2544, L12: "... uses improved ..."

P2544, L12: "atmosphere weather" ... maybe the authors wish to be a bit more specific here? Or they simply write "weather"?

P2548, L1: "tunes" –> "computes"

P2548, L7: "... NSIDC ice concentration ..."

P2548, L11: "Experiment Program" –> "Exploration Project"

P2548, L21: "... are some differences between ..."

P2548, L22: "... both data sets show ..."

P2548, L23: "heavy" is maybe not the correct term here. Do you mean thick pack ice or closed pack ice or something alike?

P2553, L 15: "... uses improved algorithms ..."

P2553, L26: "...melting. Fully resolving ..."

References: I did not find Losa et al., 2012, and Nerger et al, 2006, cited in the text somewhere.

Figure 1: I guess the small symbols deserve an a bit larger font, and thicker lines and also some more possibilities to act.

Interactive comment on The Cryosphere Discuss., 9, 2543, 2015.