The Cryosphere Discuss., 7, C3003–C3008, 2014
www.the-cryosphere-discuss.net/7/C3003/2014/

The Cryosphere
Discussions
Open Access

Interactive comment on "SMOS derived sea ice thickness: algorithm baseline, product specifications and initial verification" *by* X. Tian-Kunze et al.

Anonymous Referee #1

Received and published: 16 January 2014

This is a well written and thought-out paper. Rarely does one find a complicated algorithm explained clearly enough so that most steps could be followed and results replicated. Highly commendable from that perspective! The paper extends prior work on the retrieval of ice thickness from SMOS by addressing several simplifications a prior semi-empirical approach makes. These improvements include the explicit treatment of temperature and salinity variability on SMOS brightness temperature and the non-uniform sea ice thickness distribution which introduced sea ice thickness retrieval errors. The authors take a well thought out approach to constrain these variables using simple physical models and ancillary data from model and reanalysis. They examine the sensitivity of results to assumptions and assess the uncertainty of the overall re-

sults. Validation for several cases studies utilizes numerical sea ice reanalyses, in situ and air craft observations, and MODIS derived ice thickness maps. I think this paper presents a significant step forward by establishing a framework on how this innovative sensor might be used to enrich the data base of ice thickness retrievals. I believe this should be seen as a first (or second) step and further validation and improvements of approach will likely follow. While the presented validation studies provide some indication of improved sea ice thickness characteristics, I am not entirely convinced that these retrievals at this point allow an examination of sea ice thickness variability (monthly, inter-annual, spatial) or whether some of improvements rather adjust the retrievals to a growth curve that is closer to observations. The paper does not provide statistics on the explained variance or skill of the retrievals. A more comprehensive validation study should follow once more correlative data have been compiled - if they don't exist already. Some of the uncertainty analysis needs a little better explanation too. Nonetheless, the authors are reasonably careful not to over-hype their results. I suggest that minor revisions should let this paper go forward.

Detailed comments.

P5737, 9... uncertainties related to snow thickness assumptions could be mentioned here too. (as with pretty much all the thickness retrievals. A statement to this effect might be made at the end of the review.

P5738, a brief review of the physical principles behind the TB variation would be useful. How does sea ice thickness relate to the emissivity of sea ice at 1.4 GHz? How does temperature affect both the emissivity and the total brightness temperature?

P5739, 8 "thermal equilibrium"

Please explain what is meant by that. I assume you mean that the ice has no heat capacity and thus ice surface temperatures are at "equilibrium" with the surface heat balance? Some words on why this is a good assumption for the ice thicknesses you are targeting would be good.

P5739, 19, deformation

Deformation isn't the only reason that we have different ice thicknesses I think?

P5379, 24 tends to represent

Variations in retrieved ice thickness tend to be dominated by the thin ice, right? The footprint TB and retrieval "represents" all ice types. . .. Minor quibble.

P5741, 7, up 85 Deg,. . .

I assume the orbit configuration leaves a hole north of 85 or is the coverage just reduced. Southern boundary of daily coverage might be useful, polar is a bit vague

P 5744 SSS

Is a weekly climatology constructed from the MIT model? Not inter-annual variation? Please clarify time step and that it is not time varying as "climatology" implies. Also, some discussion that using SSS from a model in which SSS responds to sea ice thickness variations, may lead to some circularity if SMOS retrievals are used to compare to model. The paper doesn't show a comparison with this particular model, but SSS errors could be correlated across models. Not sure if this is something significant but a caveat might be inserted.

P5746, 13 shortwave

How does shortwave radiation effect the retrieval of ice thickness from MODIS? Are channels with significant visible light used? This seems odd.

P5746, 15 does not include snow layer

What is the effect of snow on the 1.4 Ghz emission? If there is none or insignificant, then this should be stated. I understand the effect of the snow layer on the thermodynamic temperature but that seems irrelevant with respect to the radiative emission model.

P5747, 9 observation data

Observation data of salinity or does this mean that this salinity gives the best retrieval results?

P5747, 24 varies between -5 and -10. . .

This needs a reference and also seems wrong at least for thick ice. Timescale and space scale of that variation would make it more meaningful.

P5748, 7, partly caused by the insulation

So the emissivity is also effected by the snow layer (see prior comment).. Again some quantification or statement why you don't worry about this further, would be helpful.

P 5748, 24 Cs, Ce calculated from Reanalysis

Anyone who hasn't read the Maykut 1986 paper will scratch their head over this one. This approach seems kind of awkward and needs some explanation. I am not quite sure why you aren't using the JRA-25 radiative fluxes. The assumption of constant clouds is likely to set up very strange surface energy balance solutions since the surface air and ice surface temperature in winter respond quickly to any variations in clouds and downwelling longwave radiation. On a clear day, the air temperature will be lower and this will reduce the downwelling longwave in the parameterization, but not fully, because you keep your clouds constant. The net effect will be a low bias in the downwelling longwave, which will result in a lower surface temperature that you would expect (If I got this right). I am not sure how much of a difference this will make but the authors may want to give this some thought and have future implementations of the algorithm use a consistent set of forcing variables from the reanalysis (maybe they have tried and ran into other issues?)

P 5750, 11 optimal accuracy

What is meant by that

P 5752, 17 Sea ice thickness uncertainties

I could use a more detailed explanation on how the overall uncertainty was established. Is this based on a Monte Carlo simulation or how is the error propagation dealt with. Some discussion on the correlation between errors is needed. The implicit assumption seems to be that those errors are uncorrelated and therefore scale by SQRT(N). While errors in TB are possibly that way (are they? Is this a random sampling of the different incidence angles or other TB errors), I doubt very much that errors in Ta, errors in assumptions about fluxes, snow, thickness distribution are. That should at least be discussed. A thorough assessment of the correlation length scales of those errors would probably be difficult.

P5754, 24 eq 11

A note that max(d) isn't dmax would be helpful. I had my brain in a knot until I realized that. Also some discussion of whether this integration includes the open water category (I think it should) should be linked to the prior discussion of the sensitivity to ice concentration and the 100% assumptions. Those points are clearly related. There is also a much richer data base from which sigma (and its validity) could be derived and tested. I am a bit doubtful that this adjustment will produce anything but push SMOS ice thickness just closer to a climatological mean. The fact that MODIS comparisons don't use II* suggests that application of sigma comes with problems. That choice b.t.w. needs some explanation.

Page 5755,1 Comparisons

This section could benefit from some analysis of variability? What is the correlation between retrieved and model ice thickness after taking out the seasonal cycle. Ideally you would want to do this with observations. [Are there really not more observations available that this? What about the previously IceBridge data used in the study?] Are you retrieving "variability" or are you adjusting the seasonal cycle to something more realistic? It would also be interesting to see what is "retrieved" from the SMOS bright-

ness temperatures and what is "modeled" based on the linear model. One could run a thermodynamic ice growth model using the same input or compare with a simple freezing degree, ice growth calculation. It would be nice to highlight the information that comes from SMOS here. I think the more convincing validation data come from the MODIS comparisons. The match in spatial variability makes a good argument. Some quantification of that match (spatial correlations) might be useful.

Figures:

Figure 1 needs some more explanation or can be cut as far as I am concerned. Fig 11-13.. PIOMAS and TOPAZ could use different colors. EMT could use a different color.. I had a hard time finding it at all.

Interactive comment on The Cryosphere Discuss., 7, 5735, 2013.