The Cryosphere Discuss., 6, C1280–C1287, 2012
www.the-cryosphere-discuss.net/6/C1280/2012/

The Cryosphere Discussions

Interactive comment on "A method for sea ice thickness and concentration analysis based on SAR data and a thermodynamic model" *by* J. Karvonen et al.

Anonymous Referee #2

Received and published: 27 August 2012

General Comments: In general I really like the idea of the paper. It is for sure a brilliant idea to try to enhance operational sea ice charting with the aid of numerical modelling of sea ice (in this case only thermodynamic instead of dynamic-thermodynamic) and analysis of sea ice cover and thickness dynamics based on SAR data. I have the feeling that the authors have a very good data set at hand to do this study: Quite an amount of SAR data, CIS ice charts, helicopter-borne sea ice thickness (SIT) measurements, and a robust thermodynamic (TD) model to simulate the SIT ... and all this for two quite different sea ice winters. I have my problems, though, to really find the added value in the novel approach this paper is trying to discuss. This is for sure partly related to my limited skills to understand what the authors try to figure out of and interpret into the

SAR features they are describing. However, even without being an expert in this, my impression is that a number of things need to be written more clearly and discussed more critically before I, personally, would speak about an added value. I will come back to these points in the specific comments which are ordered chronologically, not by their degree of importance.

Specific Comments: Section 1, page 1873, line 16-24: I would add that the discrimination of sea ice from open water is also complicated by ambiguities in the SAR signature of sea ice in comparison to the SAR signature of open water which is a function of the size of the openings and the wind forcing.

Section 1, page 1874, lines 14-15: Why did you use ECMWF? I am sure that there is a Canadian weather forecast model which has perhaps an even finer spatial resolution and which would give better results. What makes usage of the ECMWF model so desirable here?

Section 2, page 1875, lines 15-17: These ice charts are a major source against which you compare your results. Therefore I recommend to tell a bit more about these charts, in particular a) temporal sampling (how often are these released?), b) is there a fixed release time?, c) how much do these charts actually rely on RADARSAT SAR data and are as such not independent from the data you use in your approach?, d) how "good" are these charts in terms of i) total ice concentration, ii) sea ice thickness distribution, iii) ice type distribution? This is a crucial part because you do not just use these maps for a qualitative, visual comparison but you also try to calculate sea ice thickness from these. In this context: I am a bit puzzled about how ice thickness values are assigned to the different egg codes in Table 1; in particular I do not get code 6 to 9: There is 1st year ice with 50 cm thickness. There is THIN 1st year ice with 70 cm thickness. There are TWO more 1st year ice THIN classes also with 70 cm thickness ... before the thickness then jumps to 120 cm for code "1.". I also have the feeling that the thickness values given here mark the upper boundary of what is usually used. My understanding of sea ice thickness ranges for nilas, grey ice and grey-white to white ice are: 2-10 cm, 10-15 cm, and 15-30 cm, respectively.

Section 2, page 1875, line 25-27: What you write for the estimation of the sea ice thickness from the CIS charts reads to me like: If I have a 100% sea ice covered segment of which 50% is nilas, 30% ice grey ice and 20% is grey-white ice then the thickness value of nilas is used: 10 cm, although one might want to use 0.5 * 10 cm + 0.3 * 15 cm + 0.2 * 30 cm = 15.5 cm which is an about 50% underestimation of your estimate compared to the "real" conditions as given in the CIS ice charts. This might not be relevant because usually one segment does not comprise too many different ice types and thicknesses. I simply recommend to let the reader know about this.

Section 2, page 1876, lines 6-9 and Figure 3: Just for my understanding: You always divide by the total number of grid cells in your investigation area (shown in Figure 1) - no matter whether these are ice covered or not? I understand that you are aiming for a basin-scale average sea ice thickness but maybe you can again stress in the text that you are doing this. Also, the Figure 3 speaks about extent and area, both. Please be more specific about what you show here: Did you sum up the areas of all segments containing some sea ice (no matter what the total concentration is) or did you weight that estimate with the actual total ice concentration?

Section 3, page 1878, lines 10-24: In line 12 you write "comparatively high" ... compared to what? In this paragraph you give information about correlation and bias for air temperature and wind speed between the two mentioned models. I am fine with this, would have seen though also information about standard deviations and/or RMSD to learn a bit more about this comparison. More importantly, however, how good is the Canadian model compared to ground based observations. I guess you can add a reference or two regarding this to justify that this comparison makes sense. Finally you might want to add a comment that you did not look for agreement in wind direction because neither HIGHTSI nor other components of your approach need this parameter.

Section 3, page 1879, lines 18-28 and Figure 4: I'd be careful with this interpretation

and I would maybe come to the opposite conclusion that in 2002/03 the agreement between model and CIS is rather good (same timing of maximum sea ice thickness, similar decay, similar features during growth phase) while the agreement for 2008/09 is less good (different timing of maximum sea ice thickness, retarded decay) but this is subjective. Furthermore: The HIGHTSI are basin-scale averages over all 3111 grid cells. The CIS sea ice thickness estimates as well? Finally: Why does the modeled sea ice thickness start at 2 cm in 2002/03 instead of 0 cm like in 2008/09?

Section 4, page 1881, lines 16-19: You perform an incidence angle influence correction. Such an approach makes only sense (to my humble opinion) if there is basically one (known) type of sea ice in the image and/or the change of radar backscatter with incidence angle is the same for the different sea ice types involved. Is this the case? Also, you might want to motivate that you need to do this to have a smooth SAR image mosaic comprising SAR images from ascending and descending overpasses with different look directions. Finally, you might want to mention to which incidence angle you did this correction.

Section 4, page 1881, lines 19-22: Can you please specify a) in which way these mosaics will be used later on - for the sea ice drift analysis? - for the sea ice concentration estimate? - for the novel approach to estimate the sea ice thickness? In any case it would be very useful if you could specify the time periods from which these moasics are generated? Are these SAR images acquired during one day, two days ... more? Yes, I know you speak about daily mosaics but I wonder whether you had enough images available to cover the whole region in an efficient way.

Section 4, page 1882, lines 1-7: You talk about the segmentation ... I have kind of two contradictory questions: a) can you give the lower bound for the segment size you used? Is there a physical justification for the choice of this? And, you used an upper bound of 100 pixels ... is there a physical reasoning for this choice?

Section 4.3 and Section 5, in general: I am sure that this method works as you are describing it. However, I have real difficulties to connect the mentioned features, mean over features and feature distributions to real physical processes and parameters. I wonder whether the authors would consider to perhaps add a schematic illustration which maybe also includes examples of the features they are speaking about so that also non-experts in SAR-based sea ice parameter classification can understand the strengths and weaknesses of your approach. To me it reads like a black box.

Section 5, page 1886, lines 23-24 and first lines on page 1887: I don't understand how HIGHTSI sea ice thickness is redistributed on the basis of several image features ... I would understand if you would talk about sea ice drift ... but which features do you mean here and which physical meaning do these have? Also, what you write here reads like that the approach requires kind of a spin-up phase (two weeks) before first sea ice thickness estimates can be retrieved. Is this correct? Finally you talk about a "mean relative amount of edges" which I don't get the physical meaning of.

Section 6, page 1888, lines 12-18: After this long writing about how the method works ... this is the main result and you only spend 7 lines. Isn't this a bit too less? You say that according to Figures 7-10 the sea ice concentration obtained with your approach and the one taken from the CIS ice charts agree rather well. First of all, this is just Figures 7 and 8 and secondly, my subjective opinion of these figures is that sea ice concentrations are mostly overestimated by your approach, for some cases dramatically, in a way that I would not trust that approach at all. Figures 9 and 10 are about the obtained sea ice thickness distribution. Discussion of these figures is essentially missing. I strongly recommend to discuss these figures and tell the reader what is to be seen here and also try to give the reader a feeling what is realistic and what not. How real is the thickness distribution from the CIS ice charts? It seems like sea ice thickness values between 20-30 cm and 70 cm are completely missing. Is this real? Concerning the statement made earlier that 2002/03 was a heavy and 2008/09 was a light ice year: this cannot be seen in the CIS ice chart based sea ice thickness distributions. There are big discrepancies between the HIGHTSI sea ice thickness distribution

and the CIS ice chart sea ice thickness distribution. Can all these be addressed to the missing dynamics in the HIGHTSI model? According to your approach, March 05 2009 sees a widespread sea ice thickness of close to 90 cm ... which in that extent and magnitude did not occur in 2002/03. ... there are a number of inconsistencies which I believe are worth to be discussed before you start to argument that the sea ice drift product resolution could be one reason for the different sea ice thickness estimates.

Section 6.2, page 1889, lines 2-15: My knowledge of the EM technique as applied by Haas et al., Hendricks et al., and Prinsenberg et al. is that it measures the total thickness of sea ice plus snow. How is this handled here? You are relatively vague about when exactly the flights with the EM took place ... Wouldn't it be more reasonable to compare flight legs of one day with sea ice thickness results of the same day and do the same for the EM flight on another day instead of putting all EM data of one period into one pot and try to read something meaningful out of the full suite of data?

Section 6.2, page 1889, lines 16-24, Figure 11 HERE you FINALLY give a very important information: that you used EM-Bird data in your approach to estimate the sea ice thickness from the SAR data, to interprete the features and/or to give these features some physical meaning. I'd really appreciate to see this information much earlier in the paper. At the end of this paragaph you speak about registration inaccuracies between EM and SAR data ... I do not understand this since you are working with SAR mosaics and with the full set of EM-Bird data. Perhaps a more careful description of what has been done for this comparison and also for using EM-Bird data in the approach itself would be helpful. Finally, the lines in Figure 11 seem to be color coded ... do the colors have a certain meaning? Would it be possible to provide either dates at the flight lines or denote them in a way that one get to know which flight took place on which day?

Conclusions: I have no comments on the conclusions because I think that these might change substantially after the paper has been discussed and modified.

Technical Corrections / Comments: Section 1, page 1874, line 2: Typo: Fichefet instead of Fischefet

Section 2, page 1875, lines8-9: What is the source of the RADARSAT-1/2 SAR data and which form were these used? CEOS-Format? Calibrated? Which production level? Received from MDA or from the Canadian Ice Service?

Section 2, page 1875, line 10-15: I would specify the actual size of the images (500 km), give the spatial resolution (pixel size and grid resolution) and also the temporal sampling. It seems like the temporal sampling is much denser for 2008/09 than for 2002/03.

Section 2, page 1875, line 15: I would break the paragraph here after the description of the SAR properties.

Section 2, page 1876, lines 10-17 and Figure 3: I recommend to use the same scaling for x- and y-axes. If you prefer not to do so, I recommend to specifically mention in the figure caption that the scales are different between a) and b). In the text I recommend to add the julian day for end of March and mid March so that the reader more easily finds this in the figure. I would also recommend to mention that the sea ice extent was 2.5 times as large in 2002/03 compared to 2008/09 and the sea ice thickness even 3 times as thick.

Section 3, page 1879, line 9: add "(see Section 4)" behind "HIGHTSI model." In general in this paragraph: Are there any other sources of oceanic heat? What about the St. Lawrence river? How about the salinity? How saline are the waters in this estuary?

Section 3, page 1879, line 21: CIS gives 24 cm, HIGHTSI 16 cm ... since CIS is the baseline I would speak of around 35% underestimation instead of 50%.

Section 4, page 1880, line 17: "... experiment and will be discussed in the subsequent sections."

Section 4, page 1880, line 19: "... HIGHTSI model (see above) and ..."

Section 4, page 1881, line 10: "typically": How was this in your case? Was the calibration coeffient given in the data files? Where did you take this information from in case that not?

Section 4, page 1882, line 14: Can you explain what you mean by "edged" areas of even show an example?

Section 4, page 1882, lines 10-28 and page 1883, lines 8-15 This all reads as if you are using individual image pairs ... but later on I learned that also the ice drift analysis was done on the mosaics ... could perhaps stress this a bit more, perhaps by in line 27/28 speak about " ...W x W pixels of 500 m resolution (see section 4.1). We used W = 16, the window size is therefore 8 km x 8 km. To compute ..." And then you of course need to clarify what you write on page 1883, lines 8-15 about co-registration, down-sampling and so on ...?!?

Section 4, page 1884, end of section 4.3: Even though results of the ice drift algorithm can be found in Karvonen 2012 a I would find it quite instructive to see ONE example in this work as well. Also, you might want to tell the reader what the temporal sampling of the ice drift product is, whether this sampling is stable in time; I can imagine that you may have gaps in the sea ice drift maps ... in space and time ...

Section 6, page 1888, line 18: "...reasons for this could be ..."

Figure 5, page 1906: I suggest to a) note in the caption that this is the same grid and area as shown in Figure 1 in order to understand why some Islands are connected with each other, and b) provide a dB scale.

Figure 12, page 1913: It is hard to see the blue line representing HIGHTSI results. Perhaps you can choose a different color?

Interactive comment on The Cryosphere Discuss., 6, 1871, 2012.