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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 #1

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The paper presents results of a series of new algorithms to derive sea ice thickness and concentration in the Gulf of St Lawrence from a combination of thermodynamic modeling, and ice drift tracking and image processing by means of SAR data. Results are compared to traditional retrievals from Canadian Ice Service (CIS) ice charts and simple thermodynamic modeling. The comparison shows the superior performance of the new system, which is evaluated by means of coincident airborne ice thickness surveys. The manuscript is interesting and results are promising, and methods suitable, such that the presented material is certainly worth publishing. However, I have some reservations with regards to the presentation of the material, in particular the description of the algorithms and the exploitation of the results, which may require some major

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revisions. Although the presentation of the new algorithms for ice drift, concentration, and thickness retrieval from SAR imagery includes some detail (section 4.2, 4.3, and 5), it is still difficult to comprehend and lacks proper examples for better illustration and demonstration. This may be partially due to the fact that reference is made to other recent publications by Karvonen et al. (2012a and b) where the algorithms may be explained and evaluated in more detail. Although the description of algorithms in the present manuscript should be self-contained and not require the readers to look for those other publications, I would suggest to remove formal detail from these sections and rather include a better, more concise verbal description of the algorithms, and then refer to the other Karvonen papers for more mathematical detail. Or, to improve the description of algorithms in the present manuscript and additionally include examples for better illustration, which will significantly extend the scope of the paper but improve its readability and clarity. On the other hand, the paper falls short of the exploitation of the results and a careful evaluation of the new system's performance. If the description of the algorithms would be made more concise, there would be room to spend more effort on a more careful analysis of the comparison with CIS and ice thickness data and a more thorough evaluation of capabilities and deficiencies of the algorithms. Below are more specific comments to underline these points and suggestions for improvements.

Specific comments (excluding many typos which should be carefully checked for) Abstract: mention if these algorithms are applied automatically or require manual interaction?

Introduction P1872, l23: replace height with thickness P1873, l4-5: give examples, e.g. assimilation? L 11: check name of Johannessen L 17-20: Give more detail what you mean that this is a challenge for higher than 85% concentration. Otherwise this statement is unclear and quite surprising L 29-P1874, L1: unclear; what does crystal structure have to do with thermodynamics? L 6ff: I think it would be important to mention and delineate the RGPS system, which attempted to do similar things, e.g. calculate the thickness of leads by means of a thermodynamic model.

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Chapter 2 P1875, l22 ff: replace partial with fractional? P 1876, l3, and elsewhere throughout the paper: Check if average is an appropriate word, or if “mean” would be better L 13-14L how can the rates of ice thickness change and ice concentration change be compared? Different units? This needs to be formulated differently and maybe more qualitatively

Chapter 3.1 P 1877, l16: There is very much detail on the model on the one hand. However, on the other hand specific information is missing, e.g. what values exactly were used for ocean heat flux?

Chapter 3.2 Why did you not use EC weather station observational data instead of reanalysis data? That would seem more appropriate. L20: how are winter seasons defined? What period?

Chapter 3.3 Replace experiment with simulation or run? L7: do you mean resume or maintain? L9 ff: the calculation of thickness change is unclear; how is this done? Is the model applied to the mean grid cell thickness, or only to the thickness of the ice fraction? How is the problem addressed that thin ice grows more rapidly than thick ice; Please clarify this paragraph L 9 ff: mention that this will be described below? L 23 ff: what do you want to say? Is the amount of snow or winter air temperatures more important for ice thickness in a given winter?

Chapter 4.1 Is this information really required? L4-6: Is this step mentioned in the right order? Also why is it mentioned at all as this linear scaling will change the calibrated backscatter values and make them incomparable? L17-18: can the same incidence corrections be applied on the GSL as in the Baltic, given that the water and ice salinities could be quite different? Anyways you should be careful with the transferability of results from the Baltic to elsewhere, and discuss this uncertainty somewhere in the paper.

Chapter 4.2 This section is long and unclear. Be more specific and concise. Provide a better summary and otherwise clearly refer to Karvonen; Or: include examples to

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illustrate the processing. P1882, l 22: pixel's 8-PIXEL neighborhood Eq. 4: The introduction of this equation is unclear. Also, what are k and l ? L 10 ff: does the down sampling preserve edges? How does this affect the results? L20-21: unclear Eq. 5: is total deformation an established parameter or is this just defined by you? How is it being used? Eq. 6: unclear; describe in words what the ratio is and means. What are f_i , t_0 ?

Chapter 4.3 Should this be presented before the thickness retrieval? Again, this chapter is very detailed but still difficult to understand and would benefit from some illustrating examples. Also a careful evaluation of the quality of the concentration retrievals would be beneficial. Please show some examples or shorten chapter significantly. P1884, L19: what are the units of T ? dB? P1886, l3-4: why did you choose these values for T , and how do variations affect the results? L5-6: Why 50 % and not 15% or 85% as commonly done with SSM/I ice concentration? L8-9: What changes in the melting period and how does it affect the results? L13-14: do you mean the wavelength of water (ripple) waves?

Chapter 5 P1886, l19: define kinematic ice features. Should be done already earlier and in the context of eq. 5 L 23-24: where exactly is this shown in Figure 6? Text and figure should use same language L 24: how is ice thickness redistributed? Do you mean the ice is advected or do you mean that the thickness distribution is changed? P1887 ff: this is pretty incomprehensible and should be illustrated with some figures showing examples. L 9: do you mean spatial distribution or frequency distribution?

Chapter 6.1 P1888, l 17: HOW WELL? Here and in the following it would be nice if some more quantitative and differentiated statements about the quality of the results could be given. E.g. you could present the pixel data in a scatter plot which would much better show how good the agreement is. What explains some of the obvious disagreements of the results?

Chapter 6.2 There has been a Prinsenberget al paper in Annals of Glaciology (?)

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which showed some EM measurements of thickness distribution changes before and after deformation events in the GSL. That study should be cited and maybe more carefully considered. L 16-24: What were the mean thicknesses anyways? Are the disagreements large or small, relatively (you could state them as percent deviation from the mean); are results from 2003 better because the data sets are not independent? P1890, l6-9: Summarize some of the most relevant/significant/interesting results in words rather than just pointing to the tables and figures. L 15: overestimated more: by how much??

Conclusions P1891, l 18-19: How useful are the SAR drift fields anyways? Are you getting trajectories for longer time periods, or is the data set only piecewise continuous with large time gaps in the drift and deformation fields? L 26-30: but all these methods suffer from the same issue in narrow straits: low resolution P 1892, l13: Radarsat and sentinel are not SAR instruments – check sentence

References Check carefully. For example, there is a typo in line 26 on p 1893

Table 2 caption: spell out OW and IF Fig 3 caption: explain what min and max thickness refers to (i.e. in complete study area?); Use same axes scales (at least for x axis); can you indicate months on x axis for better readability? Fig 7: add dates into panels; Also the agreement in the second row is not good at all. Why? Fig 10: what are green areas? Not visible from color scale Fig 11: What are the colors of the tracks? Hardly readable anyways; make lines thicker Fig 12: Are these spatial profiles or time series? Scale?

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

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