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Interactive comment on "Comparison of automatic segmentation of full polarimetric SAR sea ice images with manually drawn ice charts" *by* M.-A. N. Moen et al.

Anonymous Referee #2

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General comments

This is a well-written paper that presents an interesting compilation of data, combining SAR polarimetry, ship observations, airborne ice thickness measurements and manual classifications by ice chart analysts. The three aims of the paper and the broader justification for this work are sound and highly relevant with increasing ship traffic in the Arctic and the need for more accurate ice charts. Also, extracting quantitative information about ice type and ice morphology from SAR polarimetry is an important goal in of itself. Hence, the type of comprehensive analysis advanced in this paper is both timely and relevant.

Unfortunately, the manuscript falls short of its declared goals in a number of respects that are outlined in detail below. These points need to be addressed in revising the paper. In its present form the study is interesting but runs the danger of doing a disservice to both ice charting and polarimetric SAR analysis by failing to establish a coherent, transparent framework for the comparison between the manual and automated classification approaches.

(1) There is lack of clarity with respect to the comparison between the automated classification of the SAR data and the manual classification carried out by ice chart analysts. In the title of the paper and the introduction (and other parts of the text) it is implied that the study compares ice charts by two analysts with polarimetric data segmented objectively. As pointed out by the authors on p. 2602, ice charts are generated using a multitude of data sources; here specific reference is made to SAR data and "available optical data".

However, in the description of methods it is unclear whether the analysts relied on remote sensing data other than the Radarsat SAR data. At the same time, they utilized ship-based observations and photographs, information which may only rarely be available for standard classification in the context of production of operational ice charts.

Since the paper outlines in the introduction that the research is motivated by an improvement of operational ice charts, more detail needs to be provided on the manual segmentation process and data sources used. For example, if "available optical data" were used as in standard charting operations (such as AVHRR, MODIS or DMSP OLS) then the type of data and their spatial resolution and extent need to be clearly stated. What about thermal IR data, which will be highly effective in distinguishing ice types at the surface temperatures observed in the region. If such data was used, how did cloudiness impact their use across the scene? Also, the level of experience of the two analysts needs to be clearly stated as well.

If on the other hand, the analysts did not generate a standard ice chart but simply used

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their expertise in manually segmenting the SAR scene by relying on surface based observations and photographs, then this needs to be made clear. If such an approach was taken, then the title and key sections of the paper would need to be revised to avoid any confusion of a manual classification with the generation of an actual ice chart.

At present section 4 of the paper and the discussion are difficult to evaluate since critical information is missing. Some wording in Section 4 (e.g., on p. 2609) or Section 5 (p. 2612) implies that in fact the analysts prepared standard ice charts. However, if that is the case, then much of the discussion and key conclusions of the paper are of limited value with respect to aims #1 and #2 outlined on p. 2599. Specifically, the paper does not make it clear what the polarimetric classification criteria are compared against in the way of datasets entering into the manual classification. Moreover, the match between the different charts cannot be compared rigorously without a more detailed evaluation of the optimal number of classes required to describe the observed ice types.

(2) The number of predefined classes is a critical element of the comparison between manual and automated classification. Since the ice chart analysts appeared to have had access to a much broader range of data, they can be expected to arrive at a larger number of classes than the fully automated algorithm. However, to limit the number of classes in the automated analysis to 5 based on a subjective approach is problematic. Rather, in this type of unsupervised classification ideally objective criteria need to be employed to determine the optimal number of classes. Such criteria can be based on measures of covariance, as determined from a principal components analysis. Alternatively, the number of classes could have been constrained based on expert judgement by the ice analysts to match their evaluation. Lacking such detail, it is difficult to assess the significance of the differences between the three independent segmentation approaches discussed in the paper.

(3) The discussion of the range of different polarimetric parameters for different ice classes and open water on p. 2613 and 2614 assumes that there is in fact a com-

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paratively narrow, unique set of parameters that describes each of these different ice types. However, neither for open water and nor for thicker first-year ice is this likely to be the case. As is well established (and discussed in some of the papers referenced by authors), backscatter signatures over open water depend on wind speed, fetch and wind direction relative to the viewing geometry. For first-year ice, it is the type, orientation and distribution of deformation features (ridges, ice rubble, rafting etc.) that will determine SAR backscatter signatures. These aspects may not be reflected in differences between the statistics of different ice thickness distributions (and likely also won't show up in visible/thermal IR satellite imagery). This raises the question as to whether the findings from this study can be interpreted in more general terms without a more detailed analysis of the ice morphology or the wave spectrum developed over open water.

(4) The introduction of the paper nicely made a case for improving automated approaches that can support the generation of ice charts for a range of applications. The conclusions need to be more specific in regards to steps that need to be taken for such improvements to take effect. For example, in several places the paper emphasizes how the ice analysts were not familiar with polarimetric SAR products. Does that mean that a simple training course in the interpretation of polarimetric SAR data can vastly improve the quality of operational ice charts? If so, what steps need to be taken towards that goal? Along the same lines, to what an extent can one extrapolate from the current study to conditions that are much more representative of typical shipping operations in the Arctic in summer and fall? At that time, warm, wet ice surfaces may mask many of the differences between ice types observed in late winter and early spring.

Specific comments

P. 2596 - l. 13-14: This sentence is vague and sounds more like an advertisement than a conclusion from the paper. Please be more specific (and appended directly to previous sentence in abstract without paragraph break). - l. 16: "Arctic sea ice cover" is more specific than "Arctic ice cover" unless you are also referring to lake and river

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ice. - l. 19: Increasing human activities such as shipping are driven as much by other factors such as global economics or geopolitics as by changes in the ice cover, please reword.

p. 2597 - l. 25: “processes”

p. 2598 - l 8ff: I assume you are referring exclusively to SAR data here since there are a number of studies that have compared ice charts based on visible range and IR range imagery and passive microwave products to other types of validation data. Please be specific. If you are referring to broader validation efforts then these other studies (authored, e.g., by researchers at the CIS or the NIC in the US) need to be referenced.

p. 2599 - l. 8: “relative kurtosis” in the context as used here is cryptic, please explain briefly – i.e., does this refer to the fourth moment of a distribution of a polarimetric variable derived over a specific region or is this jargon for something else?

- p 2600 - l. 5: The Pauli colour coding scheme needs to be explained in the figure caption as well, or better yet, the three RGB channels need to be shown in the figure legend along with the corresponding channel combination.

p. 2601 - l. 14: “acquiring”

p. 2603 - l. 16: What is the rationale for a 21x21 scattering vector matrix? Is this driven by the desired effective resolution or chosen for other (computational?) reasons? Would one expect to see similar classification results for a much larger or smaller number of scattering vectors?

p. 2604 - equation 3 and l. 10ff: The text is a bit confusing here (e.g., please clarify “heavy tail” in terms of the pdf at several standard deviations away from the mean). Please also explain how specifically RK relates to the true kurtosis of a Gaussian distribution and what you mean by “non-Gaussianity”. Since RK is always non-zero and positive I’m not entirely sure I see how this relates to the true kurtosis.

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p. 2606 l. 19: “fourteen”

p. 2607 - l. 2: Please use standard nomenclature when referring to what appears to be a rank-order filter.

p. 2612 -l. 7 ff: The reference to “the ice analysts have used too many classes” makes little sense and points to a key flaw in the analysis. If the ice analysts drew on the variety of data sources hinted at further up in the paper, then the segmentation into 7 or 6 classes is probably very well justified. However, comparing two sets of classifications based on different premises and datasets requires further work to attribute discrepancies between different classifications.

p. 2618 -l. 32: correct spelling of Nghiem

Interactive comment on The Cryosphere Discuss., 7, 2595, 2013.