The Cryosphere Discuss.,
doi:10.5194/tc-2016-21-RC2, 2016

The Cryosphere Discussions
Open Access
EGU

Interactive comment on "Near Real Time Arctic sea ice thickness and volume from CryoSat-2" *by* R. L. Tilling et al.

Anonymous Referee #2

Received and published: 18 March 2016

This article presents near-real time Arctic sea ice thickness and volume estimates from CryoSat-2 data. The availability of a NRT data product is a great achievement which could have wide benefit. Much of the article describes the differences between the NRT product and regular product, and uncertainties for the NRT product are also determined. The paper also discusses sea ice volume determination, this type of product is much more of interest for the comparison of current conditions to long-term climate records rather than a tool for shipping, oil, or other resource extraction uses. I believe a product such as this, and particularly the associated uncertainties, require a much more detailed treatment than what has presently been done. Ingestion and comparison to models also requires that the data biases and uncertainties be well known and described. But as detailed below, I believe that the uncertainties in the data are larger than were presented in the paper due oversimplification of errors as well as the

Printer-friendly version

Discussion paper

possible exclusion of key uncertainty factors.

In several areas of the text the mathematical operations performed on the data need to be explicitly written out as otherwise it is unclear exactly how some of the calculations were done. One example on this is that it is unclear whether a correction for the slower speed of light in snow has been applied to the calculation of freeboard. It is stated in Tilling et al., 2015 “A correction is applied to each freeboard measurement to account for the attenuation of the radar pulse as it passes through any snow cover on sea ice, where snow depth is based on a climatology.” But this sentence is confusing as it could also apply to attenuation of energy through the snow, which in itself would not necessarily impact the freeboard determination. If this factor is applied, and whether it was applied in the determination of sea ice thickness and volume uncertainty, is not clear in the text.

It is also unclear how freeboard retrieval errors would propagate into the uncertainty calculations. Tilling et al., 2015 state that an interpolation is done between ocean surface elevation measurements to determine freeboard. The interpolation procedure was not explicitly stated but needs to be done so here. Any such interpolation would change the correlation length of the errors in the assessment and needs to be considered.

Further detailed comments are outlined below:

P2L25: The need for model ingestion is mentioned. But it should be considered that many models which ingest data have trouble with gridded mean sea ice thickness data and prefer to work with swath level data because sea ice thickness in modern models is represented as a distribution rather than a mean value. It would be more useful to provide the point to point measurements of freeboard (the actual measurement made by CryoSat-2) which could be more easily ingested in a model.

P4: The mathematical expression for determination of sea ice thickness error needs to be written out. Was the uncertainty due to the lower speed of light in snow considered in the error estimates?

P4L27: The mathematical expression for the circular operator needs to be written out as it is unclear how this was applied to the data.

P3L19: The reference to Kwok et al., 2009 is confusing here as the paper does not describe the use of CryoSat-2 data.

P4L5: Which geophysical corrections are often missing in the data? They should be listed.

P4L16-17: How is snow from the Warren climatology applied beyond areas of the central Arctic? The reasons for this were mentioned clearly in the other review. I think this is a critical part of the manuscript as this could have a large impact on first year ice areas outside of the central Arctic basin.

P4L17-18: The specific densities for sea ice and water need to be written out.

P4L26: If a 1 km grid can be provided, why not also provide the swath level freeboard data which is of similar resolution?

P4L37: Given the extrapolations of the Warren climatology outside of the central Arctic, as well as the modified version over first year ice, I would question these snow depth uncertainty estimates as they have been quite modified from their original source.

P4L42-44: The statement that the large number of freeboard measurements negates the uncertainty rests on the assumption that the errors are uncorrelated in space and time. This seems highly unlikely given that the retrieval method does not account for factors such as changing snow conditions as shown by Ricker et al., 2015.

Ricker, R., Hendricks, S., Perovich, D. K., Helm, V., & Gerdes, R. (2015). Impact of snow accumulation on CryoSatâĂŘ2 range retrievals over Arctic sea ice: An observational approach with buoy data. Geophysical Research Letters, 42(11), 4447-4455.

P5L1-7: The method for determining volume uncertainties is unclear and should be written out mathematically to fully describe the procedure. Also, over what range is

Printer-friendly version

Discussion paper

each parameter adjusted to calculate the rate of change?

P5 second paragraph: I think this estimate of error is a gross simplification of the uncertainties and is not accurate. For the snow depth term, it was already acknowledged that there are large differences over first year and multi-year ice which are unrelated to synoptic scale meteorology but is rather related to the timing of snow fall events and ice freeze-up. Sea ice density would also similarly be unrelated to synoptic scale meteorology particularly as the values used in the study are based on first year and multi-year ice types. I would therefore not consider the 2000 km decorrelation length to be accurate. Have you looked at other data to determine the decorrelation length for these parameters?

The last sentence in this paragraph is not accurate as there is likely residual error in the sea surface height estimate since there is a need to interpolate over data gaps due to the varying number of lead points available. The interpolation procedure needs to be written out so that the correlation length of errors in the sea ice thickness can be better understood and taken into account.

Figure 2a: There appear to be negative ice thickness values in the distribution, I'm guessing this is due to uncertainties in the freeboard retrieval but some explanation on this is in order.

A map of the differences with the final data compared to the NRT also needs to be shown. This will reveal whether regional differences are present.

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-21, 2016.