The Cryosphere Discuss., 7, C2549–C2552, 2013
www.the-cryosphere-discuss.net/7/C2549/2013/

The Cryosphere Discussions

Open Access

TCD

7, C2549–C2552, 2013

Interactive Comment

Interactive comment on "Uncertainties in Arctic sea ice thickness and volume: new estimates and implications for trends" *by* M. Zygmuntowska et al.

Anonymous Referee #2

Received and published: 27 November 2013

This is an interesting paper which outlines uncertainty estimates in Arctic sea ice thickness and volume from altimetry data, ICESat and CryoSat-2 in particular. A discussion of uncertainties in the hydrostatic balance equation is undertaken and estimates of the uncertainties for the dominant parameters are considered. A Monte Carlo approach is then used to estimate the basin-wide uncertainty in the mean sea ice thickness and volume, and their associated trends. The most compelling conclusion is that the loss of ice volume from ICESat and CryoSat-2 may not have been as substantial as previously reported.

The overall scope of the paper is quite good, it touches on a very important problem in estimating uncertainty. The data sets and methods seem to be suitable for the task. I think this could be a valuable contribution to the literature, but first would like to see a number of points clarified. I think there also needs to be some additional consideration of the uncertainty estimates which may affect the stated conclusions somewhat.

My main takeaway from the paper is that it points to unresolved biases in the retrieval of freeboard from both the ICESat and CryoSat-2 missions. The choice of different density values then allows for these biases to be mitigated when comparing to independent data sets (e.g. ICESat was compared to submarine and mooring data, CryoSat-2 was compared to the EM bird, mooring data, and IceBridge). But from a geophysical standpoint, these biases need to be addressed in the retrieval algorithms themselves. Thus, I would not conclude from the analysis done in this study that the ice loss between ICESat and CryoSat-2 may have been less dramatic than previously reported since the mean thickness estimates do compare well with independent data sets. But rather, that there are unresolved issues in the current retrieval methods . There are also likely higher uncertainties in the estimated trends, which this study shows quite nicely.

In terms of determining uncertainties in the trend, I think a point that needs to be addressed is separating regional/spatial and interannual variability. If the uncertainty in sea ice thickness due to spatial variability for a parameter (density, snow depth, freeboard) is large it, but unbiased, then the uncertainty for the mean thickness and volume (and their trends) will be small since the hydrostatic balance equation is linear and the number of measurements are large. This is what was shown in Kwok et al., 2009 and was discussed in the paper. A key example of this can be shown with one of the snow depth data sets used in this study. For AMSR-E measurements, Brucker and Markus, 2013 found the snow depth retrieval method has a negligible bias, which suggests that if enough data points are averaged together then the error is negligible for uncertainty in the trend. Warren et al., 1999 also provide an estimate for interannual variability of snow depth and snow density which could be used in the study to separate spatial and interannual variability contributions. The main unknown would then be in the ice density, which as pointed out here, there is little data to ascertain an estimate of the spatial and interannual variability.

Lastly, in comparing the JPL data to the Yi and Zwally data set it needs to be acknowledged that substantially different freeboard retrieval methods are used, and this likely contributes to some of the differences seen. Major differences including the identification of leads between the methods, and the JPL data includes two different bias corrections. Note also that very different snow depth retrieval methods are used as well, with the JPL data using modeled snow depth data which is quite different than the climatology.

Specific comments

P. 5055, L10-15: The elevation accuracy is estimated to be 15 cm. The footprint size and ellipticity varied each campaign, but was nominally a 70 m circle. The surface was sampled every 172 m.

P. 5055, L 21: Only the freeboard retrieval is described in Zwally et al., 2002. The freeboard retrieval method is described on the NSIDC website.

P. 5058, L16-20: In what way was the data hole filled using the surrounding percentage of multi-year ice? This suggests a weighting scheme, but it is not clear. It would be best to write this out mathematically.

P. 5060, L1: Reference for the density value of 916 kg/m3 is needed.

P. 5062, L5: The mean density is 990 kg/m3? This does not seem correct. Perhaps 890 kg/m3?

P. 5069, L20-25: I'm still unclear where the absolute uncertainty comes from. Is this due to expected interannual variability in the data? Expected biases in the data? Or is it the combined impact of random uncertainty of each of the parameters mentioned for each 25 km data grid cell?

I would also expect uncertainties in the sea ice freeboard to contribute a substantial portion of the uncertainty because a 1 cm uncertainty in freeboard gives $\sim$10 cm uncertainty in thickness.

P. 5071 L15-20: I would suggest discussing these uncertainties also from the perspective of interannual variability, in addition to treatment as a bias.

Section 5.3, the conclusion that ice loss between ICESat and CryoSat-2 may have been less dramatic than previously reported, or that there was even an ice gain is not fully supportable with the analysis that has been done. It suggests instead that there are unresolved instrumental biases in the freeboard retrieval methods and that variations in the ice density and snow depth data may have been used to mitigate these biases. As an examination of instrumental freeboard biases and uncertainty was not done in this study it is difficult to state this conclusion with much confidence. Admittedly, it is stated at the end of the section, but it is quite a prominent statement in the abstract.

Interactive comment on The Cryosphere Discuss., 7, 5051, 2013.