

We would like to thank the anonymous reviewer for the very useful comments and suggestions which help us improve the quality of our paper. Before replying to the more specific points raised by the reviewer we wish to clarify a few general points.

The first concern raised by the reviewer deals with our statement regarding the change in sea ice volume from ICESat to CryoSat-2 periods. He argues that based on the analysis done in our study we cannot conclude 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 there are rather unresolved biases in the current freeboard retrieval methods.

It is true, that sea ice thickness estimates from ICESat and CryoSat-2 have been evaluated and agree well with independent in-situ data. It is however also important to stress that these validation data used are limited both in space and time. For CryoSat the evaluation of multi-year-ice has been done with EM and ICEBridge data in April only in two consecutive years. For seasonal ice, and ICESat the evaluation was done with upward looking sonars on moorings in different years. Therefore estimates agree well at point wise selected locations but not necessarily the mean thickness.

Besides this spatial and temporal limitation, it is actually surprising that the altimeter estimates agree well with the validation data. As the retrieval methods are so different one can expect a discrepancy in the thickness estimates. Measurements of sea ice draft can be biased positively due to overestimating ridges while CryoSat-2 with its coarse resolution is rather underestimating ridges. Calculation of sea ice thickness with IceBridge data in turn was done using different values for sea ice density and snow depth that should result in other values than estimated from CryoSat (see Figure 3a).

As suggested by the reviewer, we will add some information about the possible biases in the freeboard retrievals in the discussion section, as it is a very important point. However, we still believe that our statement about the sea ice loss is a good compromise between the quantitative result on changes in sea ice volume, as requested by the first reviewer, and to neglect our quantitative results completely. We also think that this statement is very careful and in agreement with our results.

The reviewer is further asking for a separation between spatial/regional and inter annual variabilities. If the spatial variability of the different parameters is large but unbiased, the uncertainty of sea ice thickness may be small. The reviewer is arguing that using AMSR-E and its spatial variability and additionally the climatology from Warren one could separate between those two effects.

This is indeed true and could be done, but only over first-year-ice where AMSR-E snow depth data are available. However the goal of our study was to analyse existing methods, algorithms and data sets that are widely used by the community and the resulting differences and unresolved biases. These large scale biases are the primary sources of uncertainty. Random spatial variability should be addressed in

the future but beyond the scope of our study.

The reviewer is further pointing out that the two datasets used are very different in their freeboard retrievals. The substantial difference between the two datasets is the method to identify leads and corrections made for the reflectivity. We were not able to compare the freeboard retrievals directly, but compared the thickness estimates using the same density of sea ice (Figure 3). Using the same densities of 925 kg/m^3 the results are relatively close (black line, D2). The corrections included in the JPL dataset increase the freeboard while the snow accumulation product used seems to give a higher snow depth than in the modified Warren climatology (see figure Kwok 2009, Figure 5 b), this lowers the thickness estimates. For the total mean sea ice thickness the two effects cancel each other and can therefore not explain the difference in sea ice volume seen in Figure 8.

Below we answer the more specific questions and comments from the reviewer:

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.

→ This is correct and we will include this information in our paper.

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

→ This is correct and we will include this information in our paper.

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.

→ We fitted a 3rd order polynom through the values of sea ice thickness and multi year ice fraction in the grid cells 2 degrees around the hole. To fill the data hole we used the multi year ice fraction around the hole as a proxy for sea ice thickness. This method is only a rough estimate and we found it to be more robust if the coefficients are selected separately for each period. Therefore it is not written out mathematically in the manuscript.

P. 5060, L1: Reference for the density value of 916 kg/m^3 is needed.

→ It is close to the values from Laxon et al. 2003 and Kurtz et al. 2011 of 915 kg/m^3 , and the values reported by Alexandrov 2010 of 916.7 kg/m^3 .

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

→ This is a typo and the correct value is indeed 890 kg/m³.

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.

→ The total uncertainties have been calculated with the Monte-Carlo approach varying all parameters simultaneously according to the PDFs described in Figure 2 and Section 3.4. The given uncertainty of sea ice volume is the standard deviation of the calculated PDF for the sea ice volume.

For the snow depth the PDFs follow the Warren climatology, the mean in the PDFs is the mean over the Arctic Ocean and the standard deviation is the inter-annual variability. In this sense the uncertainty calculated is a result of interannual variability, which is a consequence of the climatology used. As stated by the reviewer and also in our paper the AMSR-E snow depth measurements which contain no bias could be used to reduce our uncertainty. The dataset however is only available over first-year-ice, while over multi-year-ice no data is available. For this reason the AMSR-E snow depth data set has never been used for sea ice thickness estimates and in our study we mainly used the Warren climatology.

For the sea ice density there is no consensus in the community which value is the right one, and different groups use different values (e.g. AWI & Laxon et al. for CryoSat: 915 kg/m³ and 882 kg/m³; ICESat, JPL 925 kg/m³; ICESat and ICEBridge, Kurtz et al. 915 kg/m³; Laxon et al. EnviSat 915 kg/m³). To each value there is an uncertainty, this is then an uncorrelated error (and is considered in the standard approach of error propagation) but this does not cover the whole magnitude of uncertainty.

For the area the uncertainty results from the different algorithms and the way they use different frequencies, polarizations, tie points and how sensitive they are to surface and weather filters.

The uncertainties in sea ice volume calculated in Figure 8 and 9 are therefore uncertainties due to different assumptions on geophysical parameters in the community. Our knowledge of these parameters is still limited, and the use of the different parameters results in biases that we describe in our paper. A random natural variability of the parameters, and an analysis of the causes, should be included in the future. But, considering the lack of knowledge at this particular point, we believe that the random natural variability is of less concern.

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 10 cm uncertainty in thickness.

→ The freeboard does indeed also contribute, and different methods to identify leads can clearly give an influence on the estimated freeboard (see e.g. Connor 2013, Armitage and Davidson 2013). We believe however that this analysis is beyond the scope of our study, because we primarily focused on the geophysical 'uncertainties', and less on the instrumental 'errors' as described in section 3.4. In particular for CryoSat is much work required before any conclusion can be made. We will however revise our statement about the freeboard 'uncertainty' and how it contributed only a few centimeters to the total uncertainty in sea ice thickness. This simplification was too general and will revise our statement in the discussion section to clarify this point.

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

→ This has been addressed, see discussion above.

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

→ This has been addressed, see discussion above.

References:

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