

Interactive comment on “The relation between sea ice thickness and freeboard in the Arctic” by V. Alexandrov et al.

K. Giles (Referee)

k.giles@cpom.ucl.ac.uk

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General Comments

This paper brings together in-situ measurements of sea ice to evaluate the uncertainties in the parameters used to convert satellite measurements of sea ice freeboard to thickness. The paper assesses the contribution from the uncertainty in ice density to the total ice thickness uncertainty. This is a useful exercise and suitable for publication, providing some corrections are made to the manuscript. These are detailed below, but the two most serious issues that need to be addressed are:

1) It is not clear how the uncertainty in the ice density is calculated (see comments below P647, L10)

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2) The motivation of this paper is to better constrain the uncertainty in satellite derived sea ice thickness measurements, as described in the abstract. However, the paper contains multiple references to constant freeboard/thickness ratios that are not used by the majority of papers that use altimeter measurements of freeboard to compute thickness (e.g. Laxon, Giles, Kwok, Zwally etc.). Constant freeboard to thickness ratios are not used due to the sensitivity of the ratio to the changes in the snow depth and density, which are accounted for when equation 4 is used, taking estimates of the snow depth and density from climatology, for example. All references to this ratio must be removed and the paper should focus on evaluating the ice density uncertainty and how it affects the total uncertainty in ice thickness using equ.5.

The paper also includes a number of grammatical errors that should be corrected.

Specific Comments

P642, L16: The value given for the freeboard uncertainty is 0.05. Where does this number come from? I suggest taking the FB uncertainty of 0.03 m from Giles et al., (2007).

P642, L21: Replace CryoSat with CryoSat-2 for consistency though out the paper. The authors should check for other occurrences of this.

P643, L5: CryoSat-2's objective is to measure trends in sea ice thickness. The uncertainty on a point measurement of sea ice thickness as described here is expected to be greater than the error in the trend.

P643, L6: CryoSat-2 was launched in April 2010.

P643, L13: Beaven (1995) shows that under dry snow conditions the radar penetrates to the snow ice interface. Therefore the use of this reference here is miss-leading as it implies that Beaven's experiment shows an uncertainty in this assumption.

P643, L14-15: The study by Giles and Hvidegaard (2006), used data from April and May in the Fram Strait, where it is likely that changes in temperature could have resulted

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in changes to the snow pack, effecting the penetration of the radar. CryoSat-2 will only estimate changes in the winter (defined as October to March) ice thickness to avoid possible issues with increases in temperature changing the dielectric properties of the snow pack. Therefore, it is not true that changes to the snow pack are not taken into consideration.

P643, L26: Kwok (see Kwok & Cunningham, 2008) calculates the snow depth using a combination of climatology and snow precipitation data. This method is not mentioned here.

P645. L10. Remove 'yardstick'

P645, L10: Does the fact that the survey area is being used as a runway affect the measurements of snow depth?

P645: Equation 1: The use of constant ratios to determine sea ice thickness is not relevant to satellite altimetry as neither the IceSat or CryoSat-2 scientists use this method. This comment also applies to equations 8 – 11 on p650.

P646. L21: Do you mean the 'brine content' of the air bubbles?

P646. What is infiltrated snow ice? If you mean snow ice caused by sea water flooding then note that this is only relevant to Antarctic sea ice and therefore not relevant to the CryoSat-2 mission aims.

P647, L10: Which measurements have been used to calculate the mean density of FY ice? (Sever data?) How has the uncertainty been calculated? i.e. is it the standard deviation of the individual points in the 'large' area? If so how do they know that the ice is in hydrostatic equilibrium at those points? Or have they divided the data into many large areas, which are assumed to be in hydrostatic equilibrium, averaged the large areas and taken the standard deviation of those large areas?

P647, L12: Can they be more specific as how large a "large area" is?

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P649. L1: The paper by Tonboe et al. 2009 in Cryosphere has not been published yet, due to unaddressed issues raised by the reviewers. Please use published values for the error in freeboard (e.g. Kwok, Giles, etc.)

P650: Equation 11: Neither Laxon et al., (2003) nor Giles et al., (2007, 2008) used this equation to estimate sea ice thickness. The equation written in this form assumes that the snow depth and density are constant. Laxon and Giles do not make this assumption. N.B. comparison with Equ 6. shows that it is the choice of snow depth & density that is causing Equ. 11 to overestimate the ice thickness.

P652, Conclusions: The authors could recommend that different densities for FY and MY ice should be used routinely to calculate sea ice thickness from satellite measurements of freeboard (both in laser and radar measurements).

Figure 1. The labels on the figures are too small.

Figure 2: the caption could contain the data sources. Do all the data sources estimate density in the same way? If not, how much of the measured interval is due to the error in the measurement techniques and how much is natural variability?

Figure 3: This is an interesting plot showing how the relative contribution of the errors change as a function of freeboard. It would be easier to read if the colours of the contributions from the errors were the same for FY and MY ice e.g. total error is currently green for FY ice and blue for MY ice, and the freeboard error is blue for FY and green for MY ice. The authors could produce the same plot for laser altimetry, using equation 4 in Giles et al., 2007.

Figure 4: The letters a)-e) in the caption do not relate to the figure

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