

## ***Interactive comment on “SMOS derived sea ice thickness: algorithm baseline, product specifications and initial verification” by X. Tian-Kunze et al.***

**Anonymous Referee #2**

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The manuscript “SMOS derived sea ice thickness: algorithm baseline, product specifications and initial verification” by Tian-Kunze et al. describes the current status of algorithms developed at the University of Hamburg to retrieve thin sea ice thickness from SMOS measurements. A major part of the manuscript describes and validates an enhancement of the first SMOS sea ice thickness algorithm by Kaleschke et al. (2012). The new algorithm (algorithm II) accounts for variations in ice temperature and salinity and in all cases shows a better performance than algorithm I. In general algorithm II is clearly described (see my attached comments for some clarification requests) and sufficiently validated provided for a first description of the algorithm (I assume more extensive SMOS ice thickness validation is future work). Validations show that algorithm

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II is underestimating the mean sea ice thickness in the SMOS footprint but not as much as algorithm I and also shows more realistic ice thickness variability. This is found to be caused by the single-layer constant ice thickness assumption in algorithms I and II.

To overcome this shortcoming as a second step a sub-footprint-size ice thickness distribution is added. This correction is not constant but depends on the ice thickness, T and S. By using this addition the SMOS ice thickness comes closer to the thickness of two models and EM-Bird ice thickness. Also the Arctic-wide ice thickness distribution looks more realistic. This correction on average doubles the SMOS ice thickness and has a bigger effect than the change from algorithm I to II. However, the discussion of this algorithm change is comparable short and leaves some open questions. It should for example be discussed what statistical quantity of the sub-footprint ice thickness distribution one can expect SMOS to measure. Should it be closer to the modal or mean ice thickness? These topics are sometimes mentioned in half sentence but never focused on (e.g. the comparison to MODIS ice thickness is done with the modal thickness, which assumes that also SMOS algorithm II represents the mode). SMOS ice thicknesses saturate somewhere between 0.5 and 1 m (depending on temperature and salinity). It is my understanding that the SMOS derived ice thickness would be nearly the same if the footprint is covered by 90% 30cm and 10% 1m thick ice (mean 37cm) than if the footprint is covered by 90% 30cm and 10% 3m thick ice (mean 57cm). It is an inherent incapability of the SMOS measurement that it cannot distinguish these two cases. Adding a correction factor like done here can help on average. However, on a single footprint base the SMOS ice thickness can still be wrong or maybe better not representing the mean ice thickness. Such issues should be discussed more critically. The limitations of the SMOS measurements should be more clearly mentioned. Applying the correction factor is probably the best one can do if one is interested in the mean ice thickness but it is not representing the real sea ice thickness distribution within the footprint (which is unknown). Also that this correction factor was derived from measurements done mainly over thicker ice (IceBridge) should be discussed more critical. Find more comments in the attached document.

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Overall the manuscript describes a significant improvement over the first version of the SMOS ice thickness algorithm. It is clearly written and provides sufficient details to follow the processing of the SMOS sea ice thickness product distributed by the University of Hamburg. The presented comparisons to EM-Bird and MODIS derived ice thicknesses shows a good agreement within the expected uncertainties. However, a more critical discussion about the limitations of the SMOS measurements and the proposed inclusion of an external (not measured) ice thickness distribution (algorithm II\*) is needed. Find more comments and other issues discussed in the attached document. If these reservations are solved I recommend the manuscript for publication in The Cryosphere.

Please also note the supplement to this comment:

<http://www.the-cryosphere-discuss.net/7/C3206/2014/tcd-7-C3206-2014-supplement.pdf>

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Interactive comment on The Cryosphere Discuss., 7, 5735, 2013.

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