We thank the two reviewers and the editor for their effort in re-reviewing the revised manuscript and their valuable comments and suggestions. Below you will find our answers in blue. In summary, we only changed the title and gave answers to the questions. Furthermore, we added a DOI to the Zenodo archive in the section code and data availability.

Thank you to the authors for revising the manuscript according to the guidelines of the two reviewers. There remain some outstanding comments, but these are minor. I summarise them here:

1) The revised title sounds a little unnatural now; the original was perhaps better.

Agreed, we changed the title back.

2) A discussion on the effect of motion on the ULS validation was promised in the response but not found in the revised manuscript.

The effect of ice motion was discussed in Behrendt et al. (2013). We cannot correct this error because ice drift information from satellites is not available with the required high sampling rate. However, this error is certainly small compared to other sources of error, in particular the large discrepancy in the sampling footprints (SMOS 35-40 km vs USL 6-8 m). Furthermore, Behrendt et al. (2013) state that the errors are considered smaller over longer periods of time, such as daily or monthly averages. We would therefore prefer not to discuss this effect further because it is already discussed in detail in the references.

Behrendt et al. (2013): *A problem with the presented ULS data is the lack of ice drift information. Contrary to the measurements reported by Melling et al. (1995), the AWI ULS instruments were deployed without acoustic Doppler current profilers (ADCP). Their data can therefore not be converted into space-referenced data. Ice draft distributions of time-referenced data may contain peaks different from the distributions of space-referenced data (Melling et al., 1995). This sampling problem is induced by the character of the ice drift. If, by chance, only thick ice classes are present when the ice drift is slow, these classes will be more common in the draft statistics. The differences between the two distributions are expected to decrease in daily or even more in monthly mean sea ice drafts. However, the conversion of the observations into regular intervals of space would eliminate the problem.*

3) Line 181: 'the algorithm' can be changed to 'the presented algorithm'

Unfortunately, we can not find this term in Line 181.

4) Chapter 7.4: The authors mentioned that warm air intrusions cause an unrealistic ice thickness estimation. How about the influence of cold air advection? Does the estimated sea ice thickness become overestimated?

We mention only the warm air intrusions because this effect is indeed only in one direction over thick ice because of the non-linearity. For the case of thick ice, which is thicker than the maximum retrieval thickness, warming has an effect on the result because it reduces the inferred maximum retrieval thickness. Cooling (over thick ice) would have no effect on the resulting thickness because the actual thickness is already thicker than the maximum. Over thin ice, however, the effect of ice temperature is much smaller and in both directions. The idea of the algorithm is to correct for ice temperature changes by considering the atmospheric temperature variation in the thermodynamic and emission model. Actually, these variations are taken into account when determining the ice thickness, unfortunately only for the range of validity of the assumptions. The general error associated with uncertain ice temperatures is already discussed in Tian-Kunze et al. (2014). Therefore, we consider the description in Section 7.4. sufficient to describe the current limitation.