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Interactive comment on "Satellite-based sea ice thickness changes in the Laptev Sea from 2002 to 2017: Comparison to mooring observations" *by* Hans Jakob Belter et al.

Anonymous Referee #3

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In this paper the authors validate the sea ice thickness (SIT) climate data record (CDR) produced from the Envisat and Cryosat-2 radar altimeter data in the frame of the ESA Sea Ice Climate Change Initiative Phase 2 project. The authors compare the sea ice draft estimated from the sea ice freeboard and thickness retrievals with the draft measurements of the sonar-based ULS and ADCP in the Laptev Sea over the period 2003 to 2016. It is found that the satellite-derived draft is overestimated (underestimated) over thin (thick) ice, but the performance of the monthly gridded CCI-2 SIT CDR is stable over time. Analysis of the daily mean sea ice draft time series derived from along-track Cryosat-2 data shown that it is in agreement rather with the modal sea ice draft derived from ULS measurements that is attributed to inability of radar altimetry to

reproduce changes in the SIT caused by sea ice dynamics.

The paper provides extension of the SIT products' validation activity to the seas of the Russian Arctic, and is of interest in terms of interpretation and generation of the SIT datasets derived from satellite radar altimetry. The paper worth publication, but several following comments should be considered for improving the manuscript.

Specific comments: Line 12: This phrase does not fully correspond to the results presented in the paper. Overestimation (underestimation) of sea ice draft for thin ice below 0.7 m (thick ice above 1.3 m) is indicated from comparison of the mean values, but not with respect to the modal draft. Line 40: Authors could also note that in (Kern et al., 2018) the airborne Operational Ice Bridge data were used for validation of the satellite product as well. Section 2.1: I guess that open water was excluded from the sonar-based measurements. If so, please, mention it in the text. Line 101: The phrase 'bottom track mode measurements of surface and error velocity' sounds not clear. Although paper by Belter et al. (2019b) will, I guess, describe details of the methodology, some clarifications on what is, e.g., 'error velocity' would be helpful. Line 125: This way of estimating draft uncertainty is applicable if SIT uncertainty accounts for the sources of freeboard uncertainty. If so, please, mention it in the text. From the other side, since the authors do not use this draft uncertainty further in the analysis, it is not clear what it was estimated for. Line 127: It is not clear how authors calculate weighted mean values. Possibly this weighting account for the distance between grid center and mooring location? If so, please clarify it. Line 133: As a frequency of the orbit tracks that pass over the mooring sites the authors specify 'four overflights'. However Envisat and Cryosat-2 have different orbit inclinations and this frequency should be different for these missions. Section 3: I suggest the authors to change structure of this section: to combine sections 3.2 and 3.3 in one section 3.2 with the title, for example, 'Validation of CCI-2 products', and with the subsections '3.2.1 Gridded CCI-2 sea ice draft'(currently section 3.2), '3.2.2 Orbit CCI-2 sea ice draft' (currently section 3.3.1), and '3.3.3 Intercomparison of CCI-2 and merged CS2SMOS drafts' (currently section

3.3.2). Then the accordingly revised text from the first paragraph of the current section 3.3 could be moved to the beginning of new section 3.2 Line 228: I guess that this enhanced underestimation of thick ice by CS2SMOS data is observed because for some bins corresponding to thick ice the CS2SMOS product is the only available data (as I can see from Figure 5). It means that for these bins CS2SMOS product is generated only from the SMOS measurements. If so, this could be explained in the text. Line 285: The reference (Paul et al., 2018) is not appropriate here. Paul et al., 2018 do not provide regional estimates of the differences between SIT derived from ENVISAT and CS2 data. Line 315: The indicated trends are small, that supports the conclusion that the gridded CCI-2 CDR is stable over considered period. However Fig.6 shows that these trends might be caused not only by the intermission differences. The trends for thickness ranges 0 to 1 m and 1 to 2 m looks negative even separately for Envisat and CS2 data as well as for combined dataset. For thickness range 2 to 3 m two overlapping points in 2011 shows that Envisat rather overestimate sea ice draft as compared to CS2 as well as for thinner ice. Line 380: It can be noted here that not only snow depth, but specifically snow properties that influence the location of the main scattering horizon are a major source for uncertainty in the freeboard retrieval process.

Technical comments: Line 79: I suggest to replace ‘approaches’ by ‘instruments’, otherwise one may interpret it that both ADCP and ULS data are processed by two methods. Line 101: Abbreviation ‘BT’ is not needed here as it is not used further in the text. Figure 2: Colours of the first and second lines indicating trend values should be switched Line 231: I suggest to reformulate this sentence: ‘While individual stations deviate from this average the overall tendency indicates a dependency of the agreement between monthly mean gridded CCI-2 and VAL sea ice draft on sea ice thickness.’ Line 332: In the ‘newly formed FYI ice’ the word ‘ice’ is not needed. Line 383: The sentence ‘Furthermore . . .’ sounds not clear. Please, consider revising. Table 4: In the captions to the Table it is noted that the statistical parameters ‘were calculated for the four VAL data sets ’. However this table presents the results only for two stations with ULS measurements: Taymyr and 1893.

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