Comment on tc-2021-227
Anonymous Referee #1

Referee comment on "A comparison between Envisat and ICESat sea ice thickness in the Southern Ocean" by Jinfei Wang et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-227-RC1, 2021

Review of

A comparison between Envisat and ICESat sea ice thickness in the Antarctic

by

Wang, Jinfei, et al.

Summary:
Compared to the Arctic there has been less focus on sea-ice thickness retrieval and evaluation in the Southern Ocean - albeit a few data sets have been produced in the meantime. This paper reports on results of a straightforward evaluation study in which a satellite radar altimeter (Envisat-RA2) sea-ice thickness data set is compared with a respective satellite laser altimeter (ICESat GLAS) data set; in addition both satellite data sets are compared with moored upward looking sonar data in the Weddell Sea. The methodology used is robust, the results achieved plausible. The study contains some reasonable attempts to describe reasons for the observed discrepancies between i) the ULS and the satellite data sets and ii) the two satellite data sets. The study is a useful contribution to the existing literature.

I think the manuscript is written sufficiently well. While I list 5 general comments, only one (number 4) is really a severe one I’d say, which might even require some additional experiments. Otherwise, these general comments attempt to give an umbrella for the various topics touched in my specific comments.
General Comments (GC):
GC1: The paper would benefit from considerable more clarity in the writing - as is expressed in a comparably large number of specific comments mostly referring to issues I had with the way things were written up. Here the authors need to work to avoid misunderstandings.

GC2: The comparison of the two satellite data sets would benefit from an even clearer description of what has been done and also from a better presentation of those parts of the data that were actually used for the comparison. This refers in particular to the large differences in the spatial coverage of end-of-summer SIT maps between Envisat and ICESat which I found confusing.

GC3: While the authors worked on understanding the uncertainties in the data sets used better, there is still room for improvement. One issue I have is that the ICESat data set used potentially is not the sea-ice thickness but the total, i.e. snow + sea-ice thickness, an issue which should i) be mentioned in the paper carefully and which should ii) find its way in the interpretation of the results in such a way, that in the discussion you point out that the true sea-ice thickness values from that ICESat data set are possibly even smaller than those used.

GC4: I have to admit that I am disappointed by the way the authors included the freezing degree day model results into their discussion. Both the representation in the figures and the interpretation are rather poor in my eyes and need some more attention if these are to be kept in the paper. Indeed FDD model results, when adequately converted into net sea-ice thickness growth (in meters), do add valuable information. But these require more careful interpretation in comparison to the observed sea-ice thickness which calls for additional data to be included: precipitation and large-scale ice drift information.

GC5: At the end, I have a general suggestion with the style of your writing. It would read more fluently and more to the point if you would switch from passive to active mode. Example from line 17: Instead of writing "The inter-comparisons are conducted for the three seasons ..." write: "We compared results from three seasons ..."

Specific Comments: (I abbreviate Line with L)

Title: Since sea ice is an integral part of the Southern Ocean I suggest to use "Southern Ocean" instead of "Antarctic" ... perhaps even throughout the entire paper.

L50: At this point I suggest to provide a summary sentence which states that all these various data sets - despite covering limited regions and/or time periods - are extremely
useful for the evaluation of models and satellite retrieval methods. I suggest to also differentiate between data sets that provide sea-ice thickness information at one fixed location (ULS) and hence allow to check the consistency over time, and data sets which have a short duration but with high resolution cover comparably large regions (e.g. Operation ice bridge or AEM) and hence allow to check the spatial variability of the sea-ice thickness retrieved from satellite data.

L50-56: I suggest to reorganize this information a bit. First of all Kurtz and Markus 2012 and Li et al. 2018 utilize laster altimetry and hence fall into what you describe in the last sentence of the lines referred to here; this should somehow be merged. Secondly, Bernstein et al. is a paper about trying to estimate sea-ice thickness in the Ross/Amundsen Sea only from a very limited set of sea-ice charts. This data does not have the same value as the data sets of the other two papers cited in the same sentence.

L63/64: While I am totally fine with the sentence that snow affects radar altimetry SIT retrievals in two ways, you should first tell the reader the two ways before you come up with details of the shortcoming. First i) snow depth is required to a) correct the radar wave speed in snow and hence to appropriately convert the radar freeboard into the sea-ice freeboard and to b) convert sea-ice freeboard into sea-ice thickness. In both cases, but mostly in b) also the snow density plays a role. Secondly ii) the presence of snow simply modifies how the radar signal is reflected in / by the ice-snow system; the assumption of Beaven et al. is for DRY snow only. Hence, in addition to the more physical/mathematical influence of snow depth, there is this potential violation of the full-penetration assumption made by Beaven et al as is demonstrated by Willatt et al. These issues need to be specified first before you can come up with the details in Lines 65+

L81/82: Here you please need to check recent literature because Kwok and Kacimi or Kacimi and Kwok came up with more VERY useful work based on ICESat-2 data. You should include these references here as well - and ideally already point to the fact that the coverage with ICESat-2 is much better than with ICESat.

L106/107: If I am not mistaken, then the Paul et al reference point to some data analysis and algorithm development but is not specifically the reference to cite the sensor properties of Envisat RA-2. Please find a more appropriate reference which also details the footprint issue. I doubt that also Connor et al. 2009 is the adequate reference here. I am sure that are papers from the early 2000s when the altimeter was just up or about to be launched in which the system specifications are laid out well.

L115: It might make sense to add that Laxon et al. applied this method to ERS altimeter data, i.e. the predecessor of the Envisat RA-2 instrument.

L120/121: “revised version ... Cavalieri et al (2014)” I recommend to not refer to a data set description here but refer to the main core paper of the approach used which is the one by Markus and Cavalieri, 1998, and then it is the Comiso et al (2003) reference which
points to the AMSRE sea ice processing.

I suggest to make clear what the "revision" is (different tie point retrieval plus addition of retrieval errors). It would also be good if you could tell the reader on data of which years the snow depth climatology is based - because it extends well into the AMSR2 period. Finally, you may please change the URL into https://www.cen.uni-hamburg.de/icdc.

L122/123: "the actual SIT (... mean thickness ... of the grid cell area)" --> this does not go together well. The actual SIT would be the thickness of the ice floes as they float around in the grid cell. The mean SIT takes into account that the grid cell might not be fully covered by sea ice. Hence the actual SIT is always larger or equal than the mean SIT and it is important that you write this down in a clear way.

L138-140 / Eq. 3: I guess it is important to check this equation and the wording. If I am not mistaken, then the authors of these data claim on the respective web page that it is actually not the sea-ice thickness that is retrieved with this equation but it is the total (sea ice plus snow) thickness. Hence it is in a way the same type of thickness as is observed by that famous airborne EM sensor (see your introduction). In order to obtain the sea ice thickness from I retrieved using (3) one should possibly subtract the snow depth and/or reformulate equation (3) such that this effect is somehow included.

L141: Please check whether this product contains the mean gridded sea-ice freeboard or whether this is perhaps in fact the total (sea ice + snow) freeboard.

L147: "at more than 900 m underwater" --> I don't think that this is a relevant information because the actual sensor is mounted further up anyways - otherwise the comparably small footprint would not be possible to achieve and the footprint would possibly also change between ULS sensor locations.

L166: When I look at Fig. 8 I have difficulties to fully understand what you did. First of all, the annotation in the Figure is opposite to what you write here. Secondly, what are the start and end days for the FDD computation using, e.g. the period from FM to MJ? The same question for MJ to ON. I find it strange and not easy to understand that you kept the FDD in degrees C and did not attempt to translate this into a net ice thickness growth. With that it remains a very qualitative comparison.

L167: "neglects ice growth from snowfall, freezing rain or ridging" --> I suggest to be more specific with your formulation. "snowfall" per se does not lead to ice growth. It requires the process of flooding. "freezing rain" does not trigger ice growth - at least not to my knowledge. While melting of ice crystals requires energy, formation of ice from undercooled water releases energy; hence freezing rain, although contributing millimeters of ice - mostly on top of snow - warms the snow / ice. Finally ridging is no form of ice
growth. It causes dynamic thickening of the ice using ice which is already there.

L174-177: While this is possibly a good approach it leads to the observed partly considerably larger coverage with Envisat SIT data in Figs. 4 to 6, particularly Fig. 5, which at first glance is a bit puzzling. It is of course not relevant for the comparison as long as you only consider grid cells where both, Envisat and ICESat provide values. But as shown it implies that Envisat, e.g., has much more ice in summer 2005 (Ross Sea) or 2007 (several regions) but this is just because your Envisat SIT map shows data of the entire month, e.g. April, into which an ICESat period overlaps. You could include a comment about this into your text or, alternatively, only show Envisat SIT values where both satellites provide a SIT estimate.

L186-189: What is the motivation to use these sea-ice concentration data which I assume are based on the ASI algorithm? If you keep this product please make sure that you refer to the algorithm name and to also provide information about the native spatial resolution of this product (which is much finer than 100 km). It might also make sense to provide the URL to the data set web page at ICDC if there is any.

L207: The statement about the SIT uncertainties in the Worby 1-layer SIT data set is potentially not correct. I checked the data set and found uncertainties for both freeboard and thickness. Reading the paper Kern et al. 2016 it seems relatively clear that their computation of the SIT uncertainty included in the product is similar to their SICCI-2 SIT product from ICESat and hence based on uncertainties in densities and freeboard; only - and here you are correct - snow depth uncertainty is not included. You might want to rephrase your text accordingly. Also, if I am not mistaken, then the uncertainty estimates provided with the Envisat SIT data set are possibly too large because the data set producers those days did not adequately take potential correlations between the error contribution into account. I am quite sure that, for instance, for the currently available (from AWI) CS-2 sea-ice thickness data the uncertainty is considerably smaller than for the SICCI-2 project data set and I am sure the same applies to the Envisat RA-2 data set. But you have the producers among your co-authors. So you simply need to ask!

L215/216: I suggest to differentiate a bit better here between ICESat and Envisat - because Envisat provides a larger data set and hence your comparison is based on more data pairs.

While not possible for ICESat it would be possible for Envisat SIT to come up with a statement about the agreement of the seasonal cycle. Do ULS and satellite data sets provide the same seasonal cycle qualitatively?

L221/222: "one satellite SIT grid cell is scanned only one of twice through a month" --> Please make sure to be more specific here. Not all these grid cells are covered only one / twice a month. Also this is valid for ICESat but possibly not for Envisat.
L225-227: "However ... fixed ULS positions." --> While I agree that thanks to the ice motion and the integration period used the ULS point measurement kind of gains a larger representativity, it might be worthwhile to check i) how large the ice drift actually was and what their average direction was. You could use the NSIDC V4.1 sea-ice motion data set to figure this out.

L237: Not clear what you mean by "The same feature is found ..." --> Are you referring to the existence of a polynya? Or are you referring to the fact that for both polynya regions, Ross Sea and Weddell Sea Envisat SIT is much higher than ICESat SIT? Please be more specific.

L239: "possibly fails ..." --> This is not a specific enough wording. There are two things involved with that. A) using a 100 km grid naturally results in a land mask at the same grid resolution. Hence it is very likely that the land mask used in the ICESat product extends further into the open ocean than the landmask which is used in the Envisat product. B) As stated in Kern and Spreen, it is not overly bad to not take ICESat freeboard estimates close to the coast not into account because there the freeboard often is less accurate here compared to the open ocean due to various issues, mostly because of a lack of enough open leads detected by ICESat and hence a less accurate approximation of the local sea surface height and with that less accurate total freeboard.

L247/248: This apparent discrepancy could be mitigated by showing Envisat SIT only for those grid cells where ICESat has SIT values - as I mentioned earlier already. Otherwise it might be difficult to understand why the small difference between the sea-ice concentration thresholds used (60% vs. 70% ?) has such a large impact on the spatial coverage with SIT data.

L253-254: "probably ... resolve thick ice" --> while the statement made is correct for along-track data you need - in my eyes - to consider two issues here. The first one is that the ICESat product is gridded on a 100 km grid. Given the sparseness of ICESat overpasses with valid data such a 100 km grid SIT estimate in that region might be biased by the presence of thick landfast ice. The second one is that thanks to its finer along-track resolution ICESat can expected to be more sensitive to ocean swell. Ocean swell can result in anomalously high freeboard values which then convert into too high sea-ice thickness values. While this is a local phenomenon again the sparseness of ICESat overpasses with valid data can results in a similar effect as described above for landfast ice.

Fig. 8: I am wondering whether you could perhaps change the color table used for the FDD. It is not intuitive. A high number of FDD denotes cold conditions while a low number comparably warm conditions. I suggest you use a color table which goes from white (0 FDD) to blue (3000 deg C FDD). Please check whether it is common to express FDD this way. I find it strange to read about temperatures of 1500 and 3000 deg C. Also switching to the unit Kelvin does not solve the problem; ideally, as mentioned earlier, you would translate this to a net growth of sea ice (in meters).
Did you check that the FDD shown for MJ-ON is in fact for that period and not for the full FM to ON period? Please note that the notation MJ-FM and ON-MJ is opposite to what you write in the text. Since you aim is to express that the maps in the right two columns show a SIT difference of, e.g. ON minus MJ you might need to invest more annotation elements to not confuse the reader.

L273/274: "This pattern ..." --> I suggest to add the fact that the thick ice found in the southwestern Weddell Sea at the end of summer is advected northward. If you look at the SIT distributions it is both the tail at large SIT which is decreasing and the tail at small SIT which is increasing. In the particular case you mention here, the thick old ice is replaced by the thin younger ice formed in the polynya (plus other comparably thin ice that is recirculated from the Eastern Weddell Sea in winter.

L274-276: "The adverse ... reveal them" --> I would have wished for a more detailed discussion here because one can interpret a lot from these maps - provided one takes into account knowledge about typical snow fall patterns and ice motion. Here you could substantially add some more interesting information and interpretation to your paper.

Fig. 9: Please add to the caption what the black line and the dashed colored lines stand for.
You might also give the information whether you took data from all seasons available or whether we only look at data of years 2004, 2005 and 2006 as only from these years data from all three seasons are available from ICESat.

L322-326: Please note that the "nominal adjustment" suggested by Nandan et al. is derived for cases in the Arctic which might be special and not necessarily transferrable to the Southern Ocean. You could mitigate focussing too much on this exact value of 7 cm by providing a table into which you put sea-ice thickness changes in response to freeboard biases between 2 amd 10 cm in steps of 2 cm.

You choose typical first-year sea-ice density. Did you expereimt with other density values to see how dominant the freeboard change is compared to a density change? You could use densities between 880 and 940 kg/m3 in steps of 20 kg/m3 to illustrate this.

Why can the differences found here not also account for the differences between Envisat and ICESat in spring? And why do you consider the end of summer a season when this difference might apply?

L356-359: Please state that you took the same values for water and sea-ice density as in Eq. 5.
While your computation is of course correct, I am wondering whether the 2 cm bias assumed isn't a strong under-estimation. Yes, the analysis is based on monthly data, I agree. But the recommendation of Nandan et al you used in Eq. 5 is not tied to monthly data, is it? The monthly mean retrieval uncertainty you used should be considered the precision and not the potential bias which can be much larger - as you learned from Worby et al., Ozsoy-Cicek et al and as you could also see in the Kern and Ozsoy-Cicek paper in Remote Sensing from 2016; there we easily talk about 20 cm bias. Also te work of Kwok and Maksym from 2014 supports the notion that biases can be much higher over large regions. Hence, considering that also on a monthly scale the bias is an order of magnitude larger does not hurt and I invite you to, as suggested for Eq 5 provide a table into which you put sea-ice thickness changes in response to snow biases between 5 and 30 cm in steps of 5 cm; that would provide a much more realistic view of the potential bias due to using a snow depth data set that does not reflect the actual conditions.

L365-367: "While a snow ..." -- I agree to this and suggest to also stress one more time that sea-ice thickness differences you observe in your paper between different summer seasons (e.g. between Feb/Mar 2004, 2005, ... 2008) might, to a large extent, also simply be the result that the climatology does not match the actual conditions.

L374-386: You might want to mention here that possibly the approach by Kern et al. (2016) is providing the total (sea ice plus snow depth) thickness. Taking this into account, the actual 1-layer sea-ice thickness values shown in this paper would possibly even be a bit smaller - with the respective consequences for your results. See also my comments in the context of Eq. 3.

L402-404: "Compared to the FDD ..." -- In order to make this quite general statement you should investigate these maps in more detail and write more text in the respective section. See also my comments about your usage of FDD.

Editoral remarks / Typos:

L31: Actually, to obtain the sea-ice volume you need to combine the sea-ice thickness with the sea-ice area. I strongly recommend to change the working accordingly.

L57++: Please check the paper for the typo: CyroSat-2. It needs to read "CryoSat-2"

L104: "aboard on" -- either "aboard" or "on"
L112/113: “The delay correction ... " I suggest to delete this sentence here and instead add it in the discussion section when you discuss error sources / the uncertainties of the Envisat data.

L129: As ICESat is not operating anymore it is grammatically possibly more correct to write "lasted" instead of "lasts".

L153/154: "The uncertainty ... height calibration" --> I suggest to rewrite this: "The uncertainty in summer is smaller than in other seasons because open water occurs more frequently in the ULS footprint and with that the estimate of the sea surface height is more accurate.

L241/242: "However, ... near zero." -->perhaps better: "However, these differences have to be seen in the light of the standard deviations of ~0.6 m."

L258/259: "According to Table 5 ..." --> you could point out better that DESPITE the large difference and RMSD the correlation is actually the highest of the three seasons investigated.

L281: What are "splashes"?

L294/295: "though it is known ..." --> please support this knowledge with respective references.

L296: "footprint of" --> "footprint of the radar altimeter of"

L372: The perfect place for the Kwok and Maksym paper from 2014 (JGR-Oceans I think) and possibly for one of his more recent papers where he looked into ICESat-2.

L389/390: --> This sentence reads a bit strange in the context of what follows. My suggestion: "In this study, we compare estimates of the sea-ice thickness obtained from satellite altimeter observations by Envisat RA-2 (radar) and ICESat GLAS (laser) in the Southern Ocean."

L391: "Envisat-ULS" --> please make sure the reader understands the "-" as a minus so that it is clear that ULS sea-ice thickness values are smaller than Envisat (and ICESat)
values. Currently, this is not clear from the text.

L392: "The results ..." --> I don't understand this sentence in the context of the previous one. Consider to remove.


L395/396: "difference of ... between Envisat SIT minus ICESat SIT" reads strange. Please consider re-phrasing.
I note: In contrast to L391 here you spell out the ".-".

L406-408: You might want to rephrase this sentence after you have considered by comments in the context of Eq. 5 and 6.

Figure 3: I suggest that you avoid to write "ENV-ULS" and the like because it is easily misinterpreted as a difference Envisat SIT minus ULS SIT which I doubt is the quantity you are showing here.