Dear Reviewer:

We would like to express our gratitude to you for the helpful comments to improve this manuscript. For GC1 and GC2, we modify the descriptions carefully to avoid misunderstandings following your specific comments. For GC3, we add this "total thickness" information in the data description and discussion of ICESat SIT uncertainty. For GC4, we conduct further analyses towards the FDD results combining sea ice drift information. For GC5, we switch the descriptions to active mode as you suggest.
The specific responses and revisions are shown below. They are in blue font for clarity.

Qinghua Yang, Qian Shi, Robert Ricker, Stefan Hendricks
On behalf of all the authors

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.
Thanks for your comments. We replaced some of the "Antarctic" with "Southern Ocean", especially in the cases like "in the Antarctic".

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.

(1) We added a summary sentence: "Despite covering limited regions and/or time periods, all these various observational data sets are extremely useful for the evaluation of models and satellite retrieval methods."
(2) We changed the expression to differentiate the data sets: "**One type of observation data** is in situ measurements providing sea ice thickness information at one fixed location and some allow to check the consistency over time. For example, drilling data (e.g., Meiners et al., 2012) are accurate but extremely limited in temporal and spatial coverage, and hence they cannot be used to obtain an understanding of large-scale Antarctic sea ice thickness processes. Upward-looking sonars (ULS), located at 13 different sites in the Weddell Sea, provide valuable temporal evolution of sea ice draft (Harms et al., 2001; Behrendt et al., 2013a; Behrendt et al., 2013b), but a basin-wide spatial distribution cannot be derived. **The other type of data sets** has a short duration but with high resolution cover comparably large regions 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.

Thanks for your comments. We removed the references for the first sentence: "More recently, satellite remote sensing has been widely applied to investigate the spatial coverage and long-term trend of sea ice thickness in the whole Southern Ocean." Meanwhile, we cited "Kurtz and Markus, 2012" after the sentence "Satellite altimetry, including radar and laser altimetry, have been used in the Southern Ocean to retrieve sea ice thickness".

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 seaice 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+

Thanks for your comments. We modified the description as you suggested: "Firstly, snow depth is required to correct the radar wave speed in snow and hence to appropriately convert the radar freeboard into the sea ice freeboard, as well as to convert sea ice freeboard into sea ice thickness. Secondly, the presence of snow modifies how the radar signal is reflected by the ice-snow system. Specifically, over Antarctic sea ice, the complex snow stratigraphy and frequent snow flooding associated with the formation of snow ice and superimposed ice affect radar altimetry measurements (Willatt et al., 2010), i.e. the assumption of Beaven et al. (1995) is for DRY snow only. Besides, the snow depth climatology used in the retrieval of Envisat and CryoSat-2 SIT can cause additional uncertainties due to neglecting inter-annual variability in snow depth (Bunzel et al., 2018)."

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.

We modified this sentence: "However, ICESat-2, which has been in orbit since 2018, provides a new source of year-round observations of total freeboard and thus better coverage than ICESat (Kwok et al., 2019; Kacimi and Kwok, 2020)."

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.

We removed the citation of "Paul et al., 2017" and added the citation of "Peacock and Laxon, 2004".

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.

We added the information: "Sea ice thickness is retrieved from ice freeboard based on the hydrostatic equilibrium approach as first used by Laxon et al. (2003), which applying 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 apporach 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 .
We checked the snow depth climatology data and found it only covered AMSR-E period. Therefore, we removed the AMSR2 information and modified this part as you suggested: "A snow depth climatology **(based on 2002-2011)** is employed to retrieve sea ice thickness from sea ice freeboard here (**Markus and Cavalieri, 1998; Comiso et al., 2003**). This snow-depth climatology is derived from the passive microwave sensor Advanced Microwave Scanning Radiometer-EOS (AMSR-E) for the Antarctic and is based on a revised version of the approach **with different tie point retrieval plus addition of retrieval errors** and provided by the Integrated Climate Data Center (ICDC, 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.
We corrected the explanation of actual SIT: "ice thickness of the ice-covered fraction of the grid cell area".

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 substract the snow depth and/or reformulate equation (3) such that this effect is somehow included.
We checked the equation and corrected the mistake: "And total thickness (sea ice thickness plus snow depth) can be determined from it."

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.
We corrected the mistake by changing "sea ice freeboard" to "total 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.
Thanks for your comments. We agreed and removed this information.

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.

(1) We added the explanation for the annotation: "We compare FDD with the SIT variations from summer to autumn (FMMJ) and from autumn to spring (MJON) represented by Envisat and ICESat SIT. Specifically, FMMJ represents the differences between mean FDD in MJ and mean FDD in FM following ICESat operating periods and so does MJON." For example, during the period from FM to MJ in 2004, the FDD computation is conducted by subtracting mean summer SIT (from Feb 17 to Mar 20) from mean autumn SIT (from May 18 to Jun 20).

(2) We don't translate the FDD into thickness growth because we think FDD is a robust measure for potential thermodynamic ice growth (or melt). Converting FDD into thickness would require a model and additional assumptions with uncertainties that we cannot constrain sufficiently. Therefore, we think it is reasonable to only consider FDD.

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.

Thanks for your comments. We changed "ice growth" to "SIT variations" to avoid the misunderstanding.

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.

We considered it necessary to show more Envisat SIT data and we chose to add a discussion sentence to point out the problem: "It is noted that this approach can lead to considerably larger coverage with Envisat SIT data than ICESat when Envisat SIT maps show data of the entire month into which an ICESat period overlaps, e.g., summer 2005 (Ross Sea) or 2007 (several regions) in Fig. 5."

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.

Since this product is contained in the ICESat SIT data, we use it to convert the mean gridded SIT to actual SIT. We modified the introduction as you suggested: "The sea ice concentration data are derived from Special Sensor Microwave/Imager (SSM/I) and Special Sensor Microwave/Imager Sounder (SSM/IS) **based on ASI algorithm** provided by ICDC (Kaleschke et al., 2001; https://www.cen.uni-hamburg.de/en/icdc/data/cryosphere/seaiceconcentration-asi-ssmi.html) **with 12.5 km spatial resolution**, interpolated to 100 km grid NSIDC polar-stereographic grid and averaged over respective ICESat measurement periods."

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 you 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!

(1) We amended the sentence about the SIT uncertainties: "ICESat SIT uncertainties are also calculated based on the uncertainties of densities and freeboard (Kern et al., 2016)."
(2) We rewrote the potential reasons: "The large differences in the error bars between Envisat and ICESat mainly result from the inclusion of snow depth uncertainty in Envisat SIT, and without adequate regard for potential correlations between the error contribution."

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 agreemen of the seasonal cycle. Do ULS and satellite data sets provide the same seasonal cycle qualitatively?

We modified the summary sentence of the comparisons with ULS: "However, the numbers of valid data are too small to derive a reliable conclusion on the accuracy of ICESat. The comparison is based on more data pairs for Envisat, but the agreement of the seasonal cycle between ULS and satellite data sets is bad qualitatively (Fig. 3)."

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.

We modified this sentence: "one ICESat SIT grid cell is scanned once or twice on average through a measurement period."

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.

As we know, ULS indeed measures the continuous ice draft in a fixed location with diameter of several meters. Considering the ice motion, ULS actually acquired dozens to hundreds of kilometers records along the trajectory of sea ice motion on monthly basis, which have enough spatial representativeness compared with ICESat-1/Envisat. Here, we track the source of sea ice that flowing over the ULS on specified month by backward tracking method based on NSIDC Pathfinder data sets. We find the ice draft records included in ULS monthly mean calculation come from a wide range area (Fig. 1). Therefore, we think this is enough to prove that the spatial representativeness of the monthly average ULS data can be compared with that of ICESat-1/Envisat.

Besides, ULS data is one of the main reference dater sources used for assessing ice thickness from remote sensing in the past decades. ULS are used for comparison with the ice thickness derived from AVHRR (Yu and Rothrock, 1996; Drucker et al., 2003). It was also used to compare with ICESat-1 ice thickness in the Fram Strait (Spreen et al., 2009). In addition, the ULS data sets have been also used for comparison with reanalyses data in the polar region (Mu et al., 2018; Shi et al., 2021). In addition, the comparison with ULS data sets is also a convention for assessing the quality of ice thickness derived from altimeters in the European Space Agency (Kern et al., 2018).

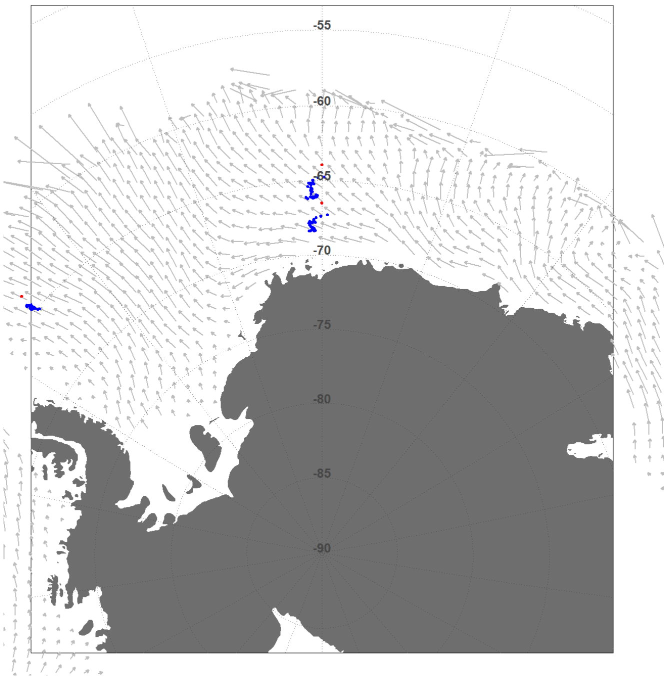

Fig. 1 The 30-day origins of the sea ice passing the three ULS sites in July 2011. The red dots stand for the ULS locations and the blue dots stand for the original locations of the sea ice using backward tracking method.

L237: Not clear what you mean by "The same feature is found ..." --> Are you referring to the existance 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.

We clarified this sentence: “The same feature is found for the Ronne Ice Shelf polynya in the Weddell Sea in 2007, with the polynya appearing only in the ICESat map.”

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 acount 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.

We added the discussion here as you suggested: "Using a 100 km grid naturally results in a land mask at the same grid resolution and 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. Besides, as stated in Kern and Spreen (2015), it is not overly bad to not take ICESat freeboard estimates close to the coast not into acount 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.

Thanks for your advice. However, we think that showing larger coverage of Envisat can help achieving improved understanding of Envisat product itself. Besides, the sea ice concentration threshold for Envisat is 70 % while 60 % for ICESat. Therefore, we don't think the different thresholds are the cause of the different spatial coverage in Ross Ice Shelf Polynya.

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 freeeboard 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.

Thank you for your comments. We considered the two issues you suggested here carefully and decided to remove this statement.

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 (3000degC 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.

We replotted the FDD figures by changing the color table, modifying the expression of the notations and removing the unit of FDD based on the explanation of FDD. We kept analyzing FDD instead of the converted SIT because we think FDD is a robust measure for potential thermodynamic ice growth (or melt).

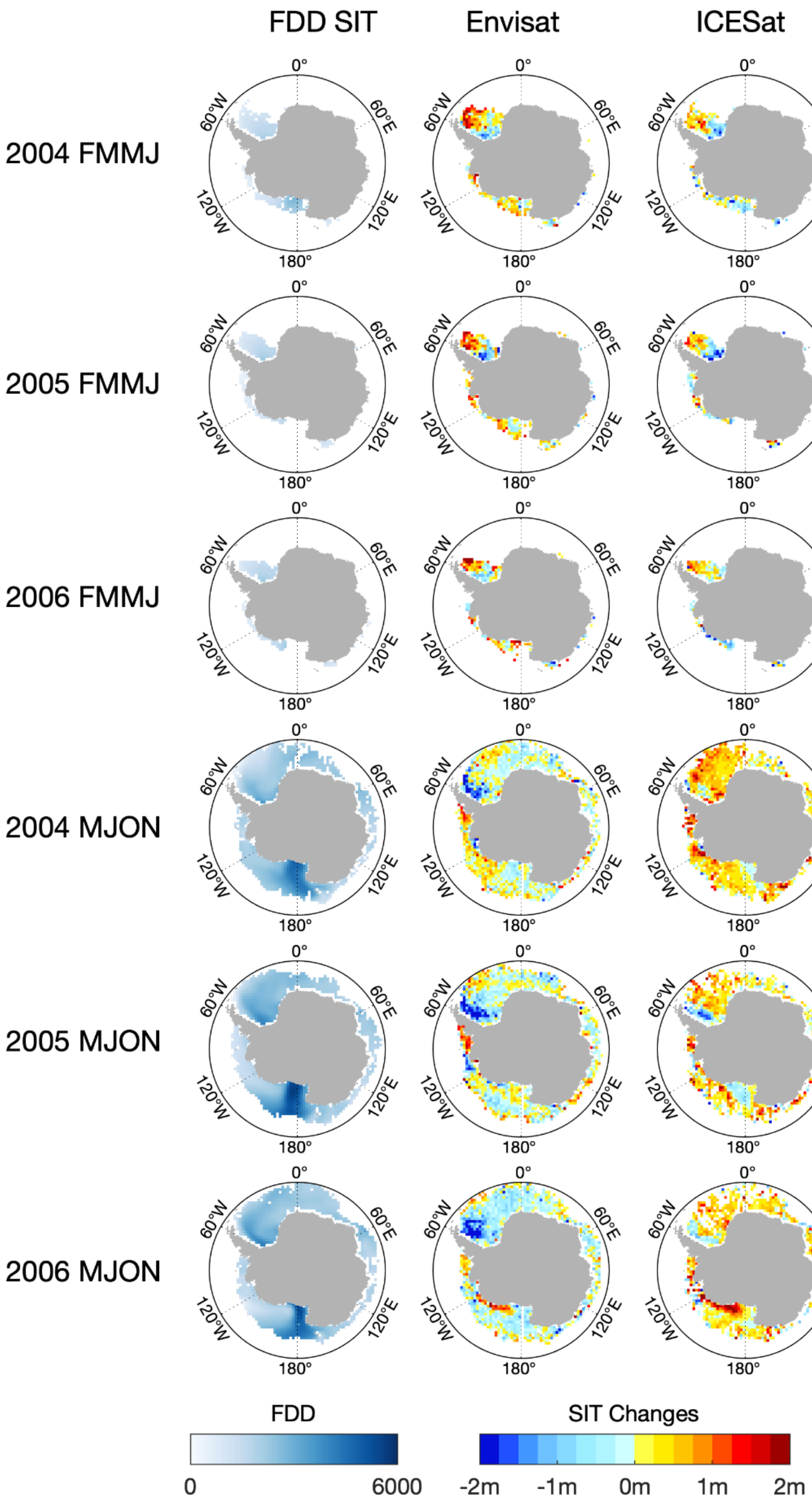

Fig. 2 The FDD differences and sea ice thickness differences from summer to autumn (FMMJ) and from autumn to spring (MJON) derived from Envisat and ICESat in 2004, 2005 and 2006. The FDD patterns are derived by forward tracking daily FDD with sea ice motion data.

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.

We added this information: “The thick ice found in the southwestern Weddell Sea at the end of summer is advected northward and the thick old ice is replaced by the thin younger ice formed in the polynya and 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.

We conduct the forward tracking on daily FDD with the NSIDC sea ice motion data to add the dynamic effects on the purely thermodynamic growth pattern (Fig. 2). We find that with the aid of sea ice motion, thick ice in the Weddell Sea and Ross Sea can be moved northward. However, the Envisat SIT decrease during MJON still cannot be explained considering the dynamic processes. Therefore, we assume the main reason of the SIT decrease is the overestimation of Envisat SIT in autumn. As for the snow fall patterns, we think it difficult to quantify the impact of snow, i.e., in which way the snow fall would lead to the sea ice thickness growth.

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.

We added the information you mentioned: “The data are taken from all seasons available.” “The black line is the 1-to-1 fit line and the dashed colored lines stand for linear regression lines.”

L322-326: Please note that the "nominal adjustement" 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 expereiment 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?

We experimented with different density values following your suggestions and changed the way to present the sensitivity of the SIT changes to freeboard biases, snow depth biases and sea ice density. From Fig. 3, we can see that the SIT changes are more sensitive to sea ice freeboard biases than to snow depth biases. Besides, with the increase of sea ice density, the SIT changes rise.

Considering the average freeboard biases, the corresponding SIT changes can be up to 0.5 m, which matches with the consistent positive differences in summer and autumn but not in spring.

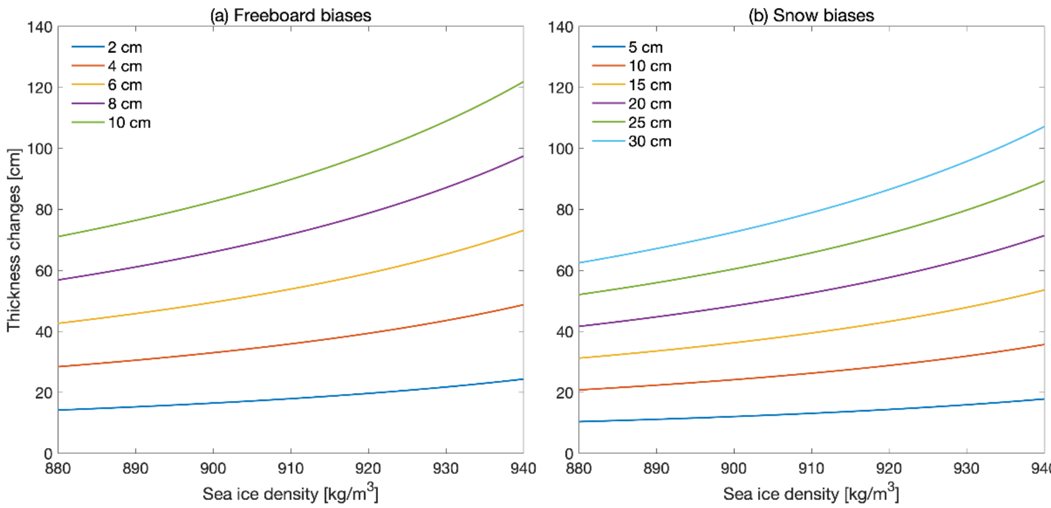

Fig. 3 Sensitivity of sea ice thickness changes to sea ice freeboard biases and snow depth biases as function of sea ice density. (a) SIT changes computed with Eq. (1) for different sea ice freeboard biases (2 cm to 10 cm). (b) Similar to (a) but computed for different snow depth biases (5 cm to 30 cm).

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.
We modified the analyses following your comments and the revision is shown above.

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.
We stressed this point as you suggested: “The SIT differences between different summer seasons might, to a large extent, also simply be the result that the climatology does not match the actual conditions. In addition, snow-depth dependent radar signal delay is applied to convert the radar freeboard into the sea-ice freeboard but the delay correction is based on a conventional assumption that has been revised (Mallett et al. 2019) since the generation of the SICCI data.”

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.

We added this discussion: “In addition, the approach by Kern et al. (2016) is actually 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.”

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.

We modified the statement based on the new results we achieve: “Compared to the FDD results in 2004, 2005 and 2006, we find that the Envisat SIT decrease during MJON cannot be explained considering the dynamic processes and we assume the main reason of the SIT decrease is the overestimation of Envisat SIT in autumn.”

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.

We changed “sea ice extent” to “sea ice area” since the slight increase and the abrupt decrease occur on sea ice area (Maksym, 2019).

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

We corrected these mistakes.

L104: "aboard on" --> either "aboard" or "on".

We deleted “on” here.

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.

We moved this sentence to the discussion section as you suggested.

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

We corrected these mistakes.

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.

We modified this sentence as you suggested.

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."

We modified this sentence as you suggested.

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.

We modified this sentence: “According to Table 5, despite the largest mean difference in autumn of 0.57 m and large RMSD of 0.47 m, the correlation is actually the highest of the three seasons investigated of 0.71.”

L281: What are "splashes"?

We replaced “splashes” with “cloud” here.

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

We added the reference here: Willatt et al., 2010.

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

We modified this expression.

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.

We added the references here: Kwok and Maksym, 2014; Kacimi and Kwok, 2020.

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."

We modified this sentence as you suggested.

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.

We spelled out the "-" here to make it clearer.

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

We removed this sentence.

L394/395: "According ..." --> three time usage of difference / different. Consider to rephrase.

We rephrased this sentence as: "According to the results, the differences between Envisat and ICESat sea ice thickness vary in each season, year and region".

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 "-".

We modified this sentence as: "More specifically, the smallest monthly average difference (SD in brackets) for Envisat SIT minus ICESat SIT exists in spring of 0.00 m (0.39 m), while larger differences (SD) exist in summer and autumn by 0.52 m (0.68 m) and 0.57 m (0.45 m), respectively."

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

We modified the sentence as: "Through the sensitivity experiments, we find that Envisat SIT changes are more sensitive to sea ice freeboard biases than to snow depth biases. Besides, with the increase of sea ice density, the SIT changes rise."

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.

Thanks for the comments but here "ENV-ULS" does represent the difference between Envisat SIT and ULS SIT.