The manuscript "On the retrieval of sea ice thickness and snow depth using concurrent laser altimetry and L-Band remote sensing data" deals with the possibility to retrieve snow and ice thickness simultaneously from two different satellite missions. The goal is to combine brightness temperatures (TB) as measured for example by the SMOS L-band satellite and freeboard (FB) heights as measured for example by laser altimeter ICESat(-2). While the idea sounds very interesting and would be extremely beneficial to the polar science community, the presented approach is not convincing: The benefit of using a combination of L-band and altimeter measurements is not shown at all, and I doubt that the results from this work are representative for a potential "actual" retrieval from satellite data (see comments A). Furthermore, the authors have sometimes not been very careful with their citations (see comments B). The manuscript would also benefit from improving the readability and the language (see comments D, which is not a complete list). Further comments are given in C.

**Comments A:**
The major concerns I have about the presented study are stated here:

a) The authors do not discuss or show any results at all on the advantage of their suggested approach. If the new contribution here is to combine L-band and altimeter measurements, why do you not show the difference between using this combination of TB and freeboard measurements as compared to just using freeboard measurements and the relationship that you found between snow depth (hs) and FB? This could be done by 1) finding global alpha and beta values (instead of a global s=alpha*beta) and then using these alpha and beta values and Eq. 3 to convert FB to hs and then ice thickness (hi) (using Eq. 1 afterwards) or 2) finding a good fitting formulation that relates hs and hi and then converting FB to hs and hi using Eq. 1. The correlation between FB and hs (R^2=0.67) is very similar to the correlation found between the retrieved and the OIB-observed hs for using the global s values and SMOS TBs (R^2=0.64) / simulated TBs (R^2=0.65). Maybe it is essentially the correlation between FB and hs that is behind this agreement of retrieved and observed hs values? From the presented results, I cannot see whether there is any advantage in using L-band additionally...

b) The retrieval as performed in this study is not very representative for an "actual" retrieval:
1. Only 50% of the available SMOS grid cells are used for the analysis, based on the criterion that "the error" (do you mean RMSE here?) between simulated and SMOS-observed TB is < 1.5 K (as compared to 3.1 K for all SMOS cells). In a "real" retrieval situation where we do not have the information from OIB (i.e. hi, hs, and surface temperature) to simulate TBs, how can we identify these cases where simulated and SMOS-observed TBs differ more?
2. In the retrieval here, TBs are simulated taking the surface temperature from OIB measurements. In a "real" retrieval situation, one would have to use other sources for surface temperature, which have different spatial and temporal resolutions; they could be (and most likely are) measured with many hours time shift! And as surface temperature has been shown to have a huge influence on L-band TB (see e.g. Maaß et al., 2013) and surface temperature can vary on short time scales, the results can be very different when using other temperature information. This is shortly mentioned at the very end of the manuscript, but it is not stressed how much this can influence the retrieval performance.
3. The retrieval is done for freeboard measurements from the OIB campaign's laser altimeter. It is not discussed how the freeboard data from this altimeter compare with satellite based laser altimeters, which are given as the target for an "actual" retrieval.

c) With the current presentation of results I am not convinced that this is more than "playing around" with the relationship between ice thickness and snow depth (as observed during the OIB flights), which is taken advantage of. One would expect a relationship in that older ice is on average thicker and has had more time to accumulate snow on top, which leads to on average thicker snow on thicker ice (considering larger scales). And of course snow depth and freeboard are correlated as snow depth and ice thickness (together with snow and ice densities) determine the freeboard. Thus, I would not consider it as an "result" that you found a correlation between them. One interpretation (assuming that the densities of ice and water are relatively constant) is that the obtained R^2 value shows the fraction of the FB that is actually snow (R^2=0.53 would then mean that about 53% of the FB part consists of snow, while the rest is ice). This is also pointed out in the paper by Kwok et al. (2011), which is cited by the authors.

**Comments B:**
- Acknowledgements: OIB and SMOS data are not cited correctly here, see e.g. the NSIDC's and ICDC's (University of Hamburg) conditions/suggestions of how to cite the data usage

- p. 3, L7f: "Several recent studies focus on the retrieval of snow depth over thick sea ice, based on L-band passive microwave remote sensing data from SMOS (Tian-Kunze et al., 2012)." -> I am not aware of "several" studies, they are also not given here, and the given reference (Tian-Kunze et al., 2012) is not about retrieving snow depth but (thin) ice thickness.

- p. 4, L17: As far as I know, "the OIB Level-4 product IDCSI4" that is claimed to have been used for 2012 to 2015 in this study is only available for the years 2009 to 2013... (While the IDCSI2 Quicklook data is indeed available for 2012 to 2015)

- p. 4, L11-13: "...based on Burke et al. (1979). ... An adapted version of the model was adopted by Tian-Kunze et al. (2014) and ..." -> This is not correct. Tian-Kunze et al. (2014) use another (simpler) approach, which is originally based on a paper by Menashi et al. (1993). (also: "adapted version... was adopted" sounds strange)

**Comments C:**
- p. 2, L17: "schematic view of remote sensing of sea ice" -> this seems a bit exaggerated to me, the figure mainly shows the definitions of snow and ice thickness and freeboard...

- p. 2, L28-29: Not everyone knows what "its adapted version for ... FYI" is

- p. 3, L9: Here, "near realtime" observations are compared with altimetry "which can only achieve basin coverage on the scale of about one month" -> It would be helpful to add that SMOS provides not only "near realtime" data but also "an almost daily coverage of the polar regions".

- p. 3, L13: "Despite the limited coverage..." -> strange wording/argumentation

- p. 4, L7: "due to the limitation of satellite’s orbital parameters, the inherent resolution is about 40 km" -> The resolution is determined by the antenna size, the frequency and the interferometry ("aperture synthesis") principle, not "orbital parameters".

- p. 4, L25-26: "we consider OIB measurements in the adjacent 3x3 cells ... of equal contribution" -> Maybe mention at least that this is an approximation (the contributions are actually not equal, see the SMOS "antenna gain function")

- p. 4, L30-31: "However, for certain segments of the OIB tracks, there exists extensive scanning which corresponds to a much larger value of M." -> I think it would be better and more precise to state the range of encountered M values (e.g. giving minimum, maximum and mean).

- p. 5, L3-5: "The purpose of these treatments is to rule out the factors that may compromise the quality of the OIB samples and allow focus on the discussion of the retrieval algorithm." -> However, these conditions that were excluded here do not only influence the OIB data quality but will probably also make a potential retrieval with SMOS data more difficult and should be discussed somewhere.

- p. 5, L16: Reference to used radiation model should be given already here.
From the given reference it is not clear how the authors of that paper have "reformulated the model to include multiple layers for sea ice and snow" (Zhou et al, 2017). If several ice layers are used in the model, the higher order reflection terms should be considered. I did not find a statement on this...

- p. 7, Eq. 3: Maybe better to use "arctan(...)" because tan^-1(...) could also be interpreted as 1/tan(...)

- p. 8, L5 - p. 9, L19: First you write about "scanning of alpha" without explaining it at all, then you present the results shown in Fig. 5. Then you explain the "scanning of alpha" procedure and finally refer to Tab. 2. This is very confusing. The scanning of alpha should be explained first. It would also be very interesting to see what values alpha takes in this procedure. Are they similar for the individual retrievals? Are they spread over a large range? Do the results shown in Fig. 5 and Tab. 2 belong to the same analysis?

- p. 10, L3-6: "There is minor increase in quality (0.91 versus 0.89 and 0.65 versus 0.637) and a relatively large gap to the “ideal" case. This indicates that the uncertainty (or error) in TB and radiation models plays an important role in affecting the quality of the retrieval. The uncertainty of TB may arise from that of the radiation model, as well as the mismatch between the altimetry and passive microwave remote sensing"
I think the very similar results for simulated and SMOS TBs cannot lead to these conclusions about the radiation model or the TB measurements! Even if (theoretically) the radiation model gave completely unrealistic TB values: If you do the hs/hi-retrieval by comparing this radiation model's TBs (for different hs,hi values) with this same radiation model's "true" TB (for the "true" hs,hi values), the difference in retrieved and "true" hs,hi will originate from other assumptions used in the retrieval (here: assumption of Eq. 3, choice of s-value, choice of thresholds, ...) or from the ill-posedness of the problem (TB ambiguities in the model, which can also exist in reality) but not the quality of the model to represent the "real world"/SMOS (because you are comparing it with its own output! you are within its "ideal model world"). In contrast, an existing difference between using SMOS and simulated TBs may contain information on the radiation model's performance to simulate SMOS TBs (and also on the effect of the spatial mismatch between altimeter and satellite measurements).
As far as I can see, the difference between using global and local s values tells you something about how good the global s value approach is. Here (with the results for

simulated and SMOS TBs being very similar), THIS (=using different s values) is where the "relatively large gap to the 'ideal' case" seems to come from!

- Fig. 5 would benefit from a legend and/or annotations of some of the lines, it is hard to remember from the figure caption what each of the 7 lines represents...

**Comments D (not a complete list!):**
- Usage of "etc" is not very precise, for example on p. 2, L5 & p. 2, L20 & p. 4, L11 & p. 5, L34

- p. 2, L5-6: "there is rapid" -> "there has been rapid"

- p. 2, L24: "hence limited spatial coverage" -> not a complete sentence

- p. 3, L6: "researches...obtain snow depth" -> strange wording

- p. 3, L10: "requires the prerequisite" -> either "requires" or "prerequisite"

- p. 3, L25: "achieves successfully retrieval" is not a correct expression

- p. 3, L26: "correspond" -> "corresponds"

- p. 4, L20: "Temporally, the date of each OIB campaign is located, and the SMOS TB data from the specific date is attained for the combined retrieval." -> An example for a case where the readability could be improved. This sounds like a complicated way of saying something like: "OIB and SMOS data from the same day are taken."

- p. 4, L23-24: "due to the inherent resolution of SMOS data is about 40km, therefore..." is not a correct expression

- p. 4, L26: "the 9 cells covers" -> "the 9 cells cover"

- p. 4, L28: "the total area the contributes" -> "the total area that contributes"

- p. 6, L28 - p. 7, L7: This part is hard to understand...

- p. 8, L1-2: "For hs > 0, there will be inundation due to: FB< hs." -> Do you mean: "For hs > 0, there will be inundation for values hs>FB."?