Dear Editor,
Thanks a lot for giving us the opportunity to revise the manuscript for another round.
Please find below our responses for Referee #2. We are thankful for the positive feedback and the valuable comments.
We revised our manuscript accordingly and tried to address the comments of the referee carefully.
Please find our responses to his comments in blue below.

Thank you again and best regards,
Anja Rösel on behalf of the co-authors.

Response for Referee #2:

Dear Stefan,
Thanks a lot for your very valuable comments and thoughts throughout the review process. Your input is as always highly appreciated.

Please find below our responses to your comments in blue.
Thanks again and best regards,
Anja and all co-authors

Review of the revised version of

Implications of surface flooding on airborne estimates of snow depth on sea ice

by Rösel, A., et al.

I really like this paper and appreciate that the authors invested the effort to revise their manuscript successfully.

The editor was so kind to invite me to have a second look, which I was delighted to do. Please find below my comments which you might consider to take into account before finally submitting this interesting contribution.

The majority of my comments point to minor edits or technical issues. I warmly recommend to once again cross-check usage of acronyms and subscripts, for instance.

There are two things I have more problems with.

The first one might be based on myself having misunderstood parts of the theory applied here. It deals with Equations 2 and 3, i.e. how you went from one to the next. I detailed this in my comment further below. Either there was just a typo. Or I misunderstood something. Or you forgot to take things into account adequately (in which case I don't know whether you might need to carry out some re-calculations.

Very good point and thanks for spotting it. It is indeed a typo in the equation and in line 215 (hfb2_IS instead of hfbs_EM,SP), resulting from the replacement of the variable renaming. This is corrected now.

The second one is perhaps a matter of taste. It deals with (my observation) the issue that Figures 5, 6, and 8 show so much more than you are actually commenting and discussing about and I am wondering whether you left out a few (those, see my comments) issues on purpose because you wanted to keep the paper short. In other words, I believe that the discussion section, which focuses a lot on the impact of a basal saline (and slushy) snow layer on radar observations - be it the OIB snow radar or be it an altimeter, could be tied much more to the very obvious discrepancies between in-situ snow depth observations and radar-derived snow depths - including the impact this has on freeboard estimation and freeboard-to-thickness conversion.

Finally, because of the substantial impact a basal saline (and slushy) snow layer apparently exerts on the radar observations, I felt that the notion given in the abstract about the overall differences between OIB snow depth and in-situ observations at both spatial scales considered (0.12 m and 0.06 m) could easily be misinterpreted. You stated on your own somewhere in the paper that the regional difference of 0.06 m, based on a comparison of much more diverse in-situ observations taken from 5 days within a several weeks timeframe with OIB measurements obtained within minutes on one particular day, is possibly not as trustworthy as the results obtained for the 2D field survey. In addition, the effect of the obvious snow depth under-estimation by the OIB radar becomes particulary striking in Fig. 6 and 8. Therefore, wouldn't it be a reasonable idea to stress that the observed (comparably small) snow depth differences and their downstream effects are a result of the combination of one part of the data having excellent agreement and one part of the data where the differences are substantial? This paper has the potential to clearly communicate this issue to the community. While this might be a minor issue for the Arctic, it has widespread consequences for Antarctic sea-ice thickness retrieval attempting to use satellite radar altimetry.
Thanks for the two comments above. We included the topic of the different scales and the different temporal resolution again in the discussion section (L.424-427 and 463-466)

Line 74: I am a bit confused with the year used for the citation Willatt et al.; I agree that the publication date is October 9, 2009, but the paper appeared in the first issue of the TGRS volume of the year 2010: 48(1). You might want to check which is more appropriate. Personally, I would go for the printed version in this case but perhaps policies have changed?
You are right: official citation is Willatt, 2010; we updated it accordingly.

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Table 1: Would it make sense to replace "combined snow depth + sea ice thickness" by "total (snow + ice) thickness" as is also used for the freeboard?
yes, updated

I suggest to replace the "/" by an "or" to avoid confusion with the mathematical symbol. Alternatively you could also write "(also: snow freeboard)" instead of "/ snow freeboard"
Yes, thanks. We go for the 2nd option.

You could add "(IS for in situ)" behind "from drill holes"
yes, updated

The "(the total component)" seems not to be required? Please check whether this supplementary information adds clarity.
It's deleted. It does not add further information.

"from radar" could be expanded into "from snow radar" so that the subscript "SR" matches better.
yes, updated

The "(no snow)" could also be considered superfluous because you described the term "total freeboard" above already. But this is clearly up to you.
It's deleted. It was more a note to ourselves which categories we have

In the last row you might want to replace "lidar" by "ATM" for consistency.
Yes, right.

############

Line 190: I suggest to add that you used the same sea-ice density for SYI as you used for FYI (it that applies).
I changed FYI to sea ice.

Should hi_IS in Eq. (1) be replaced by hi_EM,SP? [note that I use "_" to denote a subscript]. Otherwise I don't understand the sentence before. hi_IS denote the sea ice thickness from in situ measurements, doesn't it?
Sure, thanks for noticing it.

Equations (2) and (3): I have a problem to understand how Eq. (2), which contains sea ice thickness (from EM+SP i.e. possibly the one just computed in Eq. 1), total freeboard (to be derived) and snow depth (from SP), you compute the ice freeboard hfb_EM,SP. To me Eq. (3) would make sense if you'd write hfbs_EM,SP
Thanks – it was 'just' a typo as stated above

If you insist that Eq. (3) refers to ice freeboard ... then something went wrong from Eq. (2) to Eq. (3) because:

*hi_EM,SP = hfbs_EM,SP * rho_w/(rho_w-rho_i) - hs_SP * (rho_w-rho_s/rho_w-rho_i) -->*

*hi_EM,SP * (rho_w-rho_i)/rho_w = hfbs_EM,SP - hs_SP * (rho_w-rho_s)/rho_w -->*

*hi_EM,SP * (rho_w-rho_i)/rho_w = hfb_EM,SP + hs_SP - hs_SP * (rho_w-rho_s)/rho_w -->*

*hi_EM,SP * (rho_w-rho_i)/rho_w = hfb_EM,SP + hs_SP * (1 - (rho_w-rho_s)/rho_w) -->*

*hi_EM,SP * (rho_w-rho_i)/rho_w = hfb_EM,SP + hs_SP * rho_s/rho_w -->*

*hi_EM,SP * (rho_w-rho_i)/rho_w - hs_SP * rho_s/rho_w = hfb_EM,SP -->*

*(hi_EM,SP * (rho_w-rho_i) - hs_SP * rho_s) / rho_w = hfb_EM,SP*

... which is not your Eq. (3). I am happy to learn, however, that the step hfbs_EM,SP = hfb_EM,SP + hs_SP is not correct.

Lines 220/221: I guess the years of the references need to be in parentheses here.
Corrected

Line 227-229: Should, in line 227, hfb_EM,SP be hfbs_EM,SP? I am confused to see the subscript "IS" in line 229, where you in fact write about drill hole measurements which, according to your table 1 get a subscript "IS" (= correct), while in line 227 where you write "in situ freeboards" you appear to mix the general notation (= no subscript) with the one for the combined EM / snow probe data (_EM,SP). I am still not there, sorry.
The two sentences in line 227-229 are about uncertainties of the different freeboards; We revised it to make it understandable: "As described in Rösel et al., 2018 the uncertainty of the ice freeboard $hfb_{EM,SP}$ and the total freeboard *hfbs,* resulting from the propagation of uncertainties in the snow and ice densities and the sampling uncertainty, is estimated to be on average ±0.06 m. The accuracy of freeboards hfb_{IS} and $hfbs_{IS}$ from the in situ drill-hole measurements is ±0.01 m (Rösel et al., 2018)."

*Line 244: It might make sense to motivate here why you compute hi_ATM,SP using the Eqs. 5 and 6 and not simply using the "classical" Eq. 2, aka: hi_ATM,SP = hfbs_ATM * rho_w/(rho_w-rho_i) - hs_SP * (rho_w-rho_s/rho_w-rho_i)*
I guess the reason is kind clear ... because you want to stress the bias one gets using a radar altimeter in case of (refrozen) slush ... but it is not well motivated here.
Yes, we included it in L .245 "Ice freeboard ($hfb_{ATM,SP}$) and sea ice thickness ($hi_{ATM,SP}$), including a potentially refrozen slush layer, can be derived from a combination of the airborne data measurements acquired over the 2-D survey field site with the in-situ snow-probe data…"

Line 274: "of 0.55 m" could be deleted as both values, mean and modal, are given in Line 267.
ok

Line 277: "larger" --> The mean snow radar is 0.42 m and hence 0.08 m SMALLER than the snow depth at the drill-hole location of 0.50 m.
right, corrected

Lines 282-284: These 5 surveys were carried out during different days, nevertheless you average the snow depths and sea ice thickness values to one common value. This kind of precludes that there were not major snowfall events inbetween. Is this correct? Would it make sense to mention this in your sentence?
This is unfortunately not correct – in deed there were snowfalls in between. Adding the early transect from February and Early March to the average snow thickness might reduce the overall average a bit, but considering the amount of precipitation (relatively small), the drift and relocation we could not see a general increase in snow depth during the sampling period, therefore we neglect the snowfall in this period. This is also stated in Rösel et al (2018): "On Floe 2, the conditions were quite stable; the average thickness of ice and snow did not change within 3 weeks of the drift on this floe." We added a sentence in the manuscript to clarify this.

Line 287: This "snow radar" is the one on OIB, right? And the data are from the same measurement flight, i.e. March 19?

Yes, correct; we now write: “we compared observations from the OIB snow radar measurements from the same flight within a 10 km radius around the position of *R/V Lance…*”

Line 294: Looking back to what you just wrote: Does this mean that you consider the comparison between the OIB snow radar and the in-situ observations on the survey field more trustworthy? Would it make sense to state this?
I would prefer to leave it open, in a sense that the reader can make up his mind him/herself: I mean it is obvious: the comparison over the survey field was done on the same day, the comparison over the larger area was done over a longer time scale. And although I state earlier that the general snow/ice conditions did not change, we could observe local changes and different behavior of snow and ice thickness; i.e. from the buoy data. And of course, we will not know if 5 km away, one day before the overflight, there was a ridge build up and is messing up our averages. This is why we mentation here the possibility of such or other events.

Figure 5: b) Isn't it interesting to see - despite the differences in the spatio-temporal sampling - that the hs_SP distribution is much narrower than the hs_SR distribution and that the latter shows a substantially smaller count of small snow depth values (< 0.3 m) than hs_SP, while the tails towards thicker snow depth values (> 0.7 m) are very similar? Will you comment on that in your discussion? --> No ... having read the discussion you do leave this uncommented. Wouldn't it be straightfoward to refer back from and forth to the discussion section when you are dealing with the upward migration of the main scattering horizon in case of a saline / slushy basal snow cover to discuss / explain the above-mentioned differences in the snow depth distributions?
In the same context I note for panel a) that the count of hs_SR values is larger than that of hs_SP for low snow depths while it is the other way round for large snow depths. Comments?

Line 305: "hfb_EM,MP"?
Sorry this is a typo/remaining from ancient time when we still named the snow probe ‘Magnaprobe’ – I changed it to hfb_EM,SP.

Line 306: "eqn. 3" --> Eq. 3. Please also see my comments with respect to that equation.

Table 2: I note that the value in the toprightmost cell of the table does not correspond to the respective values given in the text in Line 305 (if I am not mistaken and these two values should be the same).
The row denoted "OIB snow radar inside 2D field" should, according to what is written in Line 273, have the values 0.42 in its first cell.
I have difficulties to interpret what the row just below that is representing. Is this the regional OIB estimate? In that case the number might need to be 0.49 m (see Line 290)? I am confused.

Line 322: I see this "hi_ATM,SR(all)" for the first time here? Perhaps either introduce the "all" earlier or don't use it? I note that in Fig. 8, caption, you assign this quantity with "Pass 2"; I am confused.

Line 325-326: You could potentially explain this thinner regional mean sea-ice thickness and larger variability with the fact that the longer surveys across Floe 2 included a considerably larger thin ice fraction than the 2D survey field, am I correct?

Line 326: "average sea ice thickness" --> add "of the 2D survey cite"
If you would have computed hi_ATM,SP using the classical freeboard-to-thickness equation which employs the total freeboard (instead of the artificially introduced sea ice freeboard), what would have been the result? Since the snow probe doesn't "feel" whether the snow at the bottom is slushy or not

you should obtain fairly accurate sea-ice thickness values, am I correct?
I have difficultis to find this value of 1.90 m for hi_ATM,SP in Fig. 8. If I am not mistaken then it should be the green dashed line which denotes hi_ATM,SP (according to the legend of Fig. 8) and associated with that line I find a value of 1.52 m. What is correct?

Line 329: "a bias of approximately 0.03 m in hfb_EM,SP" --> I am confused. When I look at Fig. 6, then I see a very convincing agreement betwen hfb_EM,SP and hfb_ ATM,SP ... where is the bias? Both values match the in-situ ice freeboard measurements by 0.01 m. Perfect. The only really striking thing I see in Fig. 6 is the bimodal distribution of hfb_ATM,SR. To my opinion both, the 1st mode at -0.05 m and the 2nd mode at 0.25 m require more dedication already in the description of this figure. It might be good to find an argument / explanation for the negative 1st mode and it might be good to state that the 2nd mode is potentially indeed caused by wet / saline snow pushing the main reflecting horizon for the snow radar upwards - and refering to the discussion section where you discuss this issue. I suggest to, also in the discussion section at the respective location, refer back to Fig. 6 (and also Fig. 8) so that the reader can again check how devastating the impact of a saline / slushy basal snow layer can be in detail.

Line 329/330: While this statement is possibly correct it is perhaps more complicated than this. If we take Fig. 3, 2nd panel from left and 4th panel from left as models, then the 2nd panel would provide an in-accurate sea-ice thickness values solely because of the densities not matching (i.e. the slushly snow at the bottom having a considerably larger density than the dry snow above). While hfb is zero, hs_SP equals hfbs. Fine. In that case usage of Eq. 2 would still be ok. The 4th panel is more tricky and the inaccuracy of the sea ice thickness obtained as several reasons. The actual hfb is < 0 but the measured (by radar) hfb is zero, the measured hfbs does not equal hs_SP because the snow probe just penetrates down to the actual ice surface and hence the snow probe measures a value for hs which equals hfbs + hsil. In addition to that: the snow density for the part above the water line is fine but the snow density for the slush part is considerably larger, possibly close to the one of sea ice if not even higher than that and for sure higher than for situation described above for the 2nd panel of Fig. 3. Usage of Eq. 2 would be ... dubious.

Figure 7: What is "MP" denoting?
Sorry this is a typo/remaining from ancient time when we still named the snow probe ‘Magnaprobe’ – I changed it to SP.

Is there any reason why you color potentially flooded areas (negative ice freeboard) in red while elevated positive freeboards appears in blue? Given the fact that flooding is associated with water I would find it more straightforward to use a reversed coloring in panels a) and b).
I know. We had the same discussion and I guess it’s a matter of taste.

Figure 8: It is amazing how far off hi_ATM,SR is for the 2D field survey site. While hi_ATM,SR(all) at least shows a hint of the thicknes distribution you also obtained for hi_ATM,SP there is no distribution at all for hi_ATM,SR(all) ... Comments? I could not find a note in your discussion section which would get back to this issue.
Thanks, I added a sentence into the discussion part (“However, ambiguous radar signal penetration through slushy layers (caused by sea ice flooding) and saline snow covers (caused by brine wicking from sea ice surface) may introduce a potential bias in accurate estimates of snow depth, and subsequently the resulting calculations on sea ice thickness as shown in Figure 8. In our field experiment we can clearly see an overestimation of the sea thickness, calculated from ATM surface elevation and snow radar data.“)

Line 371: I would say this is a good location to again refer to the work of Rosie Willatt et al. in both the Arctic and Antarctic (i.e. the paper you cited already plus one in the Annals of Glaciology 44 from 2011).
Thanks, added

Line 374: "deep snow pushes ... induces flooding" --> I would feel more comfortable with a formulation like "could" or "might induce flooding" because deep snow on thick ice is fine, for instance, and because impermeable cold ice or ice where sea water cannot enter laterally remain dry despite a negative ice freeboard.
Right, changed.

Line 377: "underestimate" --> "underestimation"
Changed

Line 378-383: These lines are written in a way that leads me to believe this is the typical situation. Is this the case?
Yes, from my point of of view this is clearly stated by the word 'typical C-shape profile'.

Line 383: " ." can be deleted.
done

Line 388: Wouldn't this be a good location to state that, strictly speaking, only remote sensing measurements involving radar are influenced by this? Neither the ATM nor the EM notice whether there is slush at the basal snow layer or whether the salinity profile in the snow has a C-shape. The same would apply to ICESat-2 by the way. Also the snow probe measurements are not influenced. Hence, only results that involve usage of hs_SR should be influenced. Is this the case?
Yes, thanks: "…using remote sensing techniques, which involve snow radar measurements."

Line 409: It is the other way, round, isn't it? The 0.12 m under-estimation were for the 2-D field site.
Thanks, corrected

Line 425/426: Please make sure the reader gets that this is a monthly average product.
Ok, extended it to "the monthly averaged CryoSat-2 sea ice products"

Line 427: A blank is missing before "Noticeably"
Done