In their paper "Retrieval of snow freeboard of Antarctic sea ice using waveform fitting of CryoSat-2 returns", the authors develop and apply an algorithm to obtain snow freeboard of Antarctic sea ice using waveform fitting of CryoSat-2 data. The waveform fitting is based on a forward model from earlier work of one of the authors and the main work here has been the to include backscatter from multiple interfaces (air/snow and snow/ice) in combination with snow volume backscatter for the application of sea ice in the southern hemisphere with its higher and more complex snow load. The authors compare the results with airborne validation data and earlier ICESat laser altimeter reC1 sults and conclude that their algorithm can be used to obtain snow freeboard with the CryoSat-2 during the maximum austral sea ice extent in October. They also investigate the potential to retrieve snow depth from CryoSat-2 waveforms, but do not find realistic results except for an area in the East Antarctic sector.

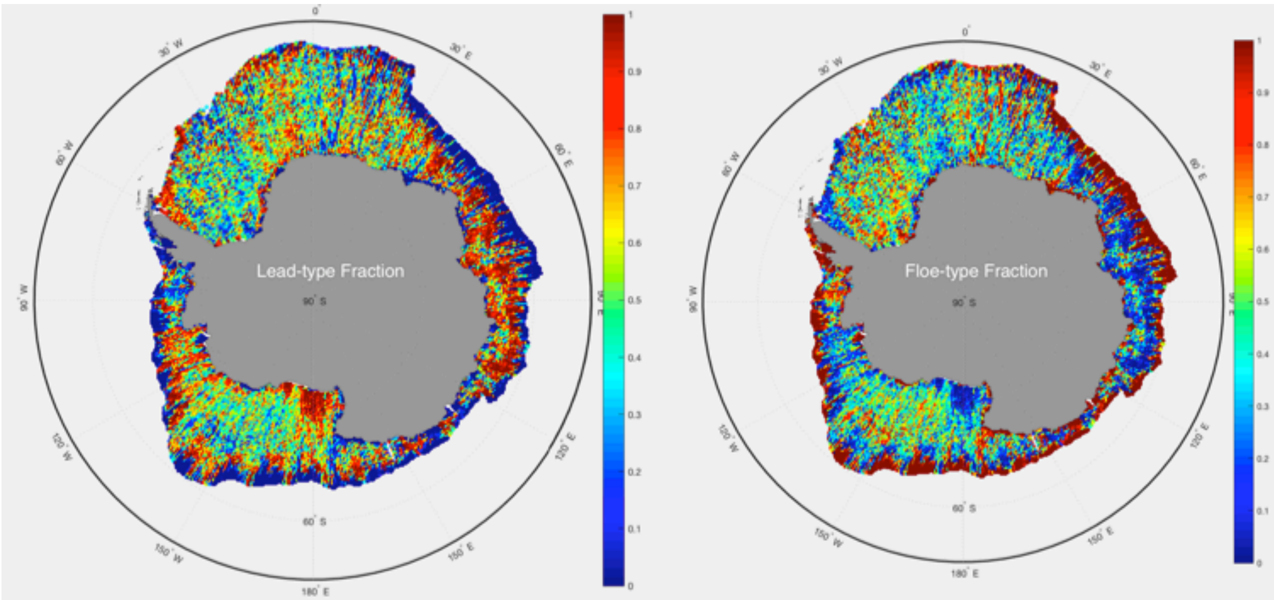
I have been part of the team that produced freeboard maps of Antarctic sea ice from Envisat & CryoSat-2 data in the Climate Change Initiative. We used an empirical retracking scheme, which made it difficult to include the contribution of snow backscatter in the freeboard algorithm without prior knowledge of its impact on waveform shape. We are acutely aware of this deficiency in the CCI sea ice thicknesses for the southern hemisphere, and it is commendable that the authors attempt to overcome this issue. Therefore this study is for me a very welcome and novel contribution to the field of remote sensing of Antarctic sea ice thickness. The concept is generally sound, but there are a number points that need to be addressed before publication. I have detailed my concerns in the general and specific comments below:
We would like to sincerely thank the reviewer, Stefan, for his important and insightful comments on this manuscript. The insight he shared from his own experience with CCI Antarctic sea ice freeboard retrievals certainly helped to improve the quality of this retrieval method. Our responses (in blue) to each of the comments (in black) can be found below.

General Comments:
1) It is understandable that maps of snow freeboard is the main objective of this work, but the authors show very little in terms verification of the different algorithm steps. The waveform fitting is based on nine free parameters, but it is not clear to me what the sensitivities are for resolving the snow backscatter properties. E.g. do snow backscatter/depth and surface roughness have an ambiguous impact on waveform shape and thus range? In general, the potential impact of surface roughness changes receives very little attention in the evaluation of the results. The fact that the snow depth resulting from the waveform fitting is unrealistic in wide parts of the study regions shows that there are issues with fully resolving the backscatter processes. A sensitivity study of the waveform forward model would help greatly to assess the skill of the algorithm for different snow conditions. Regional information on the average waveform fit quality would also be of interest to the reader as it might help to identify issues of the algorithm. I find this especially important as the direct validation with the Operation IceBridge ATM and snow radar data is very limited compared to the spatial and temporal extent of the CryoSat-2 data and the otherwise indirect comparison to ICESat.

We realize the original manuscript was indeed lacking in the verification of the sub-steps of this retrieval method, and will add additional figures and information throughout the revised manuscript.

The impact of surface roughness on the results was not discussed because we felt the impact would be small for our objective, which is simply to show the possibility of this technique to derive snow freeboard. Additionally, surface roughness is not explicitly handled by the model, but instead, backscatter values are taken from an initial guess and derived through the waveform fitting process. We acknowledge that surface roughness does play an important role on the backscatter properties, but it is one that will be explored in future work aiming to improve the accuracy of this algorithm. The revised manuscript will mention this fact. Waveform fit quality will also be made apparent in the revised manuscript, focusing on goodness of fit distributions across the sea ice pack as well as a discussion of the percentages of waveforms that are filtered out in this process. Surface type classification fractions will also be included to better assess the performance of the algorithm. See specific comments below for more details.

2) The conversion of surface elevations into snow freeboard is not state-of-the-art, in parts problematic and the authors risk to undermine the value of their retracker development work. For example, the authors use a surface type classification scheme that was originally designed for Arctic sea ice and an earlier version (baseline-b) of the CryoSat-2 Level-1 data. For the ESA CCI data set we had to define separate waveform parameter threshold for Arctic and Antarctic sea ice. The authors need to show that there is no preferential sampling of surfaces that may introduce a freeboard bias, which can be easily done by providing information on detection rates for lead and floe surfaces and compare those the surface type fractions of the ESA CCI dataset (Paul et al. TC 2018). I would also strongly suggest to compute freeboard per orbit and not by subtracting monthly mean elevations and sea surface height. In our experience the geophysical range corrections in the CryoSat-2 Level-1 product files are not good enough for this approach. Using the instantaneous sea surface height during the orbit will be more reliable and also yield better options for filtering incorrect retrievals.
It is acknowledged that the lead/floe classification scheme used here was originally developed for the Arctic which deserves further analysis, however since our methodology maintains the same 128 bin sampling as the Baseline B data there should be no difference with regard to product version. In the added verification steps, we will show detection rates for the lead/floe classification scheme. An exemplary figure from October 2016 is given here:



There are differences from Paul et al. (2018); here, there appears to be a smaller sea-ice-type waveform fraction overall and specifically a greater concentration of lead points in the Ross Sea as compared to Paul et al. (2018). It is likely that this method is classifying smooth, new ice in the Ross sea as lead points – a fact that needs to (and will) be discussed in the revised manuscript. The maps above are from October 2016 while Paul et al. (2018) has maps from September 2011, which could be a potential source of the differences observed. A more detailed discussion will be included in the revised manuscript.
In addition, we are updating our calculations of freeboard to be done on-orbit. Each of the figures will be revised to reflect the new calculation method.

3) Several earlier studies and datasets that are highly relevant for this topic are not mentioned and the manuscript gives the wrong impression that the work of the authors is the first application of CryoSat-2 for Antarctic sea ice. The authors also state that the impact of snow backscatter on ranging with Ku-Band frequencies is "often ignored" which I certainly do not agree with. It might be a matter of wording, since most operational CryoSat-2 products use the assumption that the freeboard is the average ice freeboard within the footprint. But this is due to the challenges of parametrizing snow backscatter and its temporal and regional variability, not the lack of awareness in the scientific literature. I have suggested references in the specific comments below. The lack of reference to existing publications that specifically deal with CryoSat-2 freeboard retrieval in the southern hemisphere (Schwegmann et al, 2016, Paul et al, 2018) is also unfortunate, as these are a good motivation for this study. There would be added value if the authors compare their snow freeboards to the freeboard information in the ESA CCI CryoSat-2 data set (see data availability in Paul et al. TC 2018) and demonstrate the improvements and limitations of using waveform fitting.
We agree that the original manuscript was lacking in terms of references to published literature on the topic. The introduction has been completely revised to include recent applications of

satellite altimetry to Antarctic sea ice, referencing Giles et al. (2008), Schwegmann et al (2016), Paul et al. (2018), and others. We have also revised other parts of the manuscript to make it more clear that the state of the science is well aware of the impact of snow backscatter on ranging from Ku-band and is not simply ignoring this fact. A comparison of the retrieved freeboard to that from Paul et al. (2018) would indeed be useful and will be included in the revised manuscript.

Specific Comments:

P2L7: I guess you mean "active" in the sense of active microwave sensors respective altimeters in general? I would recommend to use the term "satellite altimeters" instead of "active platforms" throughout the document. (Typo: remove -> remote)

The “active” was intended to be in contrast to the “passive instruments” mentioned in the previous paragraph, but admittedly added some confusion. All mentions of “Active” instruments and platforms has been changed to “satellite altimeters” (here and P2L15).

P2L27: Typo: "of off"

Corrected to just “off”.

P2L28: Beaven et al. 1995 states that the snow/ice interface is the dominated backscatter source. To my knowledge Beaven itself does not imply that cold snow under laboratory conditions is completely transparent for Ku-Band. This is a fine distinction but relevant for this paper.

Agreed, this is a necessary distinction to make for this work. The intro has been revised, and the new sentence reads “Most radar altimeters operate in the Ku band at around 13.6 GHz, a frequency that has been shown to produce a dominant backscatter from the snow-ice interface (Beaven et al., 1995).”

P2L39: Please rephrase the term "often ignored" as it does not properly reflect the state of the scientific literature. The issue is not lacking awareness of the importance of snow interface or volume backscatter for Ku-Band freeboard retrieval (e.g. Armitage and Ridout GRL 2015, Ricker et al. GRL 2015; Nandan et al. GRL 2017), it is the challenge of getting the temporal and spatial variability from the available waveforms. A better description would be "often not included freeboard retrieval algorithms, especially those depending on an empirical waveform evaluation".

That is better, thank you. We have revised this line to match the suggestion and included the appropriate references.

P3L30: The inverse barometric correction is included in the dynamic atmosphere correction. Both corrections should not be applied in combination.

Thanks for pointing this out, it was an error in the text as only the dynamic atmospheric correction is applied. For accuracy/clarity, the sentence has been changed to read “Geophysical corrections are applied by using the CryoSat-2 data products, which include the ionospheric delay, dry and wet tropospheric delay, oscillator drift, dynamic atmosphere correction (which includes the inverse barometer effect), pole tide, load tide, solid Earth tide, ocean equilibrium tide, and long period ocean tide."

P4L18: See comment above. There are several publications that investigate the impact of snow on CryoSat-2 freeboards and are not cited here.
We have added citations to a number of works that were missing from the original manuscript.

P7L7ff: At this point it would have been good if the authors had included a sensitivity study using their forward model. It is difficult to assess the skill of the waveform fitting if e.g. the impact surface roughness changes and snow backscatter on the waveform shape could be ambiguous.
Agreed – more sensitivity studies should be included in the text. We will include this in a revised manuscript. It is important to note that surface roughness is not explicitly included in the current form of the model, as we feel the impact of surface roughness will not affect our goal to show that snow freeboard retrievals are possible with CS-2. Surface roughness changes are important and will surely need to be taken into account in order improve the accuracy of the retrieval method, but this is something that will be done in future work.

P7L22ff: The authors should provide information on the detection rates for lead and ice surfaces. In Paul et al 2018 (TC) we needed to introduce different waveform parameter thresholds for Arctic and Antarctic sea ice. There is a risk of introducing freeboard biases if the surface type classification is not performing well (Schwegmann et al. TC 2016).
We will include some more verification/validation steps to the manuscript to track the performance of the algorithm at various parts. Specifically, maps of the lead/ice classification (similar to what was done in Paul et al. (2018)) will be added. Preliminarily, the detection rates can be found in the figure above (general comment #2 response.)

P7L30: The values for PP in Laxon et al. 2013 are: PP < 18 (not 0.18) and SSD < 4. I assume that the value of 0.18 is only a typo in the manuscript. But the issue that these thresholds are valid for an older version (algorithm baseline-b) of the CryoSat-2 Level-1 waveform. The authors must use baseline-c data for the later years of their data record and waveform oversampling introduced in this version changes pulse peakiness values. The authors should therefore verify their lead and ice detection rates (see above).
The PP and SSD thresholds are taken from Laxon et al. (2013), though there is a scaling factor of 100 difference present in the PP. As the PP was not explicitly deifned in Laxon et al. (2013), the values are calculated following Armitage and Davidson (2014). (This citation was included on P7L27 but not on P7L30 nor P8L8 – this has been corrected in the manuscript). Following Kurtz et al. (2014), we have resampled the baseline-c data so that each data type (SAR and SARin) are consistent with 128 range bins per shot. See above for lead/ice detection rates.

P8L8: Same PP threshold magnitude inconsistency. Laxon et al. 2013 reports PP < 9 and SSD > 4 as criteria for sea ice surface returns.

See answer above.

P8L33ff: Does this mean that the authors have removed the DAC and tides from the CryoSat-2 derived elevations and then applied a consistent DAC, tide and mss correction for both ATM and CryoSat-2? What about the inverse barometric correction (see comment above)? This approach however still leaves the ionospheric and tropospheric corrections as an uncertainty factor, which may a dynamic range of 10 cm or more (Ricker et al Remote Sensing, 2016) thus a non-negligible magnitude. A second validation in form of along-track freeboard might help to improve the initial validation.
To make the absolute elevation comparison, we are following the method put forth by Yi et al. (2018), which "…replaced the ocean tide, ocean loading tide, and the inverse barometer correction used in the CryoSat-2 Baseline C data product with ocean tide, ocean loading tide, and dynamic atmospheric correction…". Yi et al. (2018) was not published at the time of submission, but has since been cited in this manuscript. This was done to have consistent geophysical corrections between the CryoSat-2 and ATM data. The ionospheric and tropospheric corrections from the CryoSat-2 data product were applied.
An along-track freeboard comparison would help to improve the initial validation, however, IceBridge freeboard from these campaigns has not yet been processed. The revised manuscript will include a more thorough comparison.

P9L19ff: Filtering waveform is standard practice. The question is how many are filtered out?
This statistic was missing from the original manuscript. It will be added to the manuscript as part of the algorithm verification steps.

P9L24f: The practice of computing freeboard by subtracting monthly means of sea surface heights and surface elevation is definitely not state-of-the-art. It puts a lot of trust in the range and tidal corrections that to my experience is not justified. I strongly suggest to estimate freeboard orbit-wise, which also gives a better handle to identify sea surface height estimation issues.
Thank you for the suggestion. We are working on revising the freeboard calculation to be done on orbit. Once completed, this paragraph will be revised to include a new description of the process. All subsequent results and figures will be remade using this technique.

P9L29: The description is a bit confusing, as the earlier sentences reads as snow freeboard (per grid cell) = mean elevation (per grid cell) - mean ssh (per grid cell). From this sentence I get the impression that the authors compute a mean sea surface height, remove that from the all elevations, filter anomalous elevations and then compute the snow freeboard from the remaining values. Please revise.
Agreed. This paragraph will be revised to reflect the updated freeboard calculation technique.

P9L30: The authors seem to filter negative freeboards within a grid box. I assume this are then from individual waveforms? The filter should be lower than 0 meter or else the negative part of the range noise distribution will be filtered out for thinner ice and

thus cause the freeboard to be biased high. In the ESA CCI dataset, the filter range for CryoSat-2 along-track freeboard is -0.25 to 2.25 meters.
This will also be changed to reflect the new freeboard calculation technique. A more thorough description of the filtering will be found in the revised manuscript.

P9L34: Is the 125km filter applied to reduce noise, or to interpolate between gaps?
Mainly, the filter is applied to reduce noise, but it also interpolates between gaps caused by filtered/missing data. The sentence "Smoothing is applied to reduce noise in the CryoSat-2 data and also to fill gaps in the data" has been added to the manuscript.

P10L6: The impact of surfaces waves into the ice pack also needs to be considered for CryoSat-2 data.
Thank you for pointing this out – the effects of surface waves and ocean swells on CS-2 returns will be included in this section of the revised manuscript.

P10L17ff: For this comparison it would be helpful to have corresponding maps of surface type (lead/ice) detection rates as well as average resnorm values to look into these differences and verify that the sub steps of the CryoSat-2 snow freeboard algorithm are working as intended.
We will be adding some verification of the sub-steps of this algorithm throughout the manuscript. This will include maps of surface type detection rates (see figure above).

P10L16: Is the ICESat data also filtered to an effective resolution of 125 km?
That is correct – the ICESat data are filtered using the same method.

P10L26ff: It is good that the authors looked into the other output parameters of the waveform fitting. Unfortunately in this shorted form, more question are raised than answered. In essence, the retracking is based on a backscatter model of snow that when applied to CryoSat-2 waveforms, does not result in realistic snow conditions. Again, additional information such as regional differences in surface type detection rates or waveform fit quality parameters would greatly help to identify potential issues.
The verification that will be added throughout the manuscript should better explain why we are seeing the "snow depth" distributions found here.

P11L34: This is an overstatement given that all existing studies that used CryoSat-2 for Antarctic sea ice are not referenced in this paper.
This statement was included with intent to show the novelty of retrieving snow freeboard from CryoSat-2, though you are correct that more studies need to be referenced. We have included references to more studies throughout the paper, including in the revised introduction and here in the conclusion. This statement has been revised to better reflect the current state of the literature.

P19Fig6: axis annotations and ticks are difficult to read on a printed version
This figure has been remade – thank you.