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The Cryosphere Discussions

Interactive comment on "Lead detection in Arctic sea ice from CryoSat-2: quality assessment, lead area fraction and width distribution" *by* A. Wernecke and L. Kaleschke

Anonymous Referee #1

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General Comments

The authors present a statistical analysis of different CryoSat-2 waveform classification parameters and assess their effectiveness at detecting leads using MODIS imagery as ground truth. It is found that using a maximum waveform power threshold detects slightly more leads than the traditionally used pulse peakiness parameter. They also assert that using the maximum waveform power has less instances of ice classified as leads and reduces the number of off nadir leads however I am not entirely convinced by this. Radar altimeters are sensitive to leads even if they occupy only a small fraction of the footprint – smaller than the resolution of the MODIS imagery – so I don't think it is possible to say whether one parameter unambiguously has less ice detected as

leads. It seems possible that some areas of the MODIS imagery could contain small leads whilst the pixel still appears to be ice. Further, the authors present no evidence to suggest that one parameter is better than another at eliminating off nadir leads.

On the whole this is a good manuscript with thorough analysis and is well written and structured. It is a valuable contribution to the field however I believe that this contribution could be significantly enhanced if the authors included a comparison of the SSH retrieval using the optimised waveform classification parameters. This does not need to be a lengthy analysis, and should not be much work. It simply means comparing the SSH retrieved when using the different parameters, and perhaps seeing which optimised parameter minimises the SSH along track variance (for example). Apart from anything else, this would allow the authors to say for sure whether using the maximum power is better than pulse peakiness at getting rid of off nadir leads. Including the SSH as a performance indicator for the classification parameters would make this paper of much more interest to the altimetry community, and it could inform sea ice thickness retrievals going forward.

Specific comments

P2168L4: suggest "comparison" instead of "a combination".

P2168L16: suggest "strongly reduces" instead of "is strongly reducing". This statement isn't necessarily true for momentum – see Martin et al. (2014), "Seasonality and long-term trend of Arctic Ocean surface stress in a model", JGR Oceans, and what they found about the 'optimal sea ice concentration'. I would say something like: "Sea ice strong modifies air-ocean interaction etc. . ."

P2168L20-25: What is the resulting heat balance of leads? There is loss of latent heat as new ice is formed, but more absorption of solar radiation – what is the net effect?

P2169L16-17: Do you have a citation for this statement?

P2169L18: Suggest "dampens" rather than "is dampening".

P2169L28: Suggest "utilise" rather than "are utilising".

P2170L1: Are the authors aware of the paper by Renner et al. (2014), "Evidence of Arctic sea ice thinning from direct observations", GRL? It is not true that studies using airborne data are limited to just individual seasons.

P2170L16-18: Laxon et al. (2013) showed excellent agreement between CS2 and in situ data. I understand what you mean by this sentence but to say that altimeter estimates are not "satisfying" is unfair. I think it is more appropriate to say that knowledge of the snow loading and the radar interaction with the snow layer currently limits the accuracy of altimeter derived sea ice thickness estimates.

P2170L23: Reverse this sentence i.e. "SSH is crucial for altimeter based ice thickness retrievals"

P2170L26-28: What do you mean by thin ice? Ice $<\sim$10cm thick will have a fairly negligible effect on SSH retrieval as freeboard will be $\sim$1cm and speckle noise >10cm.

P2171L14-16: Do you have a citation for this statement? Not everyone in the altimeter sea ice community will know anything about MODIS interpretation so a citation would be useful.

P2171L18: "this" should be "these"

P2171L12-P2172L2: For the ground truth dataset, I worry about the effect of very small leads. It is known that even very small amounts (even $\sim$1% of the footprint area) of open water can cause specular reflections that will dominate the echo [Drinkwater, 1991], but would these small leads show up in MODIS data? Maybe using MODIS data means that the analysis is valid for leads over a certain size, but there might be some caveats for smaller leads?

P2172L3-P2173L7: You should provide some references/citations for this section.

P2173L4-7: This isn't a very clear explanation. This is just summing the power in each

beam and fitting a Gaussian. Maybe explain how the summed beam power relates to/varies with specular or rough surfaces.

P2173L16: I don't think that Laxon et al. (2013) give an explicit definition of pulse peakiness (PP). The PP of Peacock and Laxon (2004) is multiplied by 31.5 so I don't know where the factor of 100 comes from. I think you should be careful when you refer to "the Laxon et al. (2013) classification" throughout the paper as the classification is not explicitly stated in Laxon et al. (2013).

P2174L16-19: The trailing edge of CS2 waveforms follows a 1/sqrt(t) shape, not exponential [Wingham et al, 2006], so why the choice of an exponential fit?

P217420-P2175L3: Again, this isn't a particularly clear explanation of the beam behaviour parameters. Refer to the fact that a Gaussian function is used to approximate the summed power in each beam

P2175L4-10: Describe what the kurtosis represents intuitively i.e. the 'peakedness' of a distribution. Not all readers will be aware of this.

P2175L12-P2176L11: This seems like a good methodology, although I am unfamiliar with it myself. I also imagine that a lot of readers in the sea ice altimetry community are unfamiliar with this methodology as well. Please provide some references in this section for interested readers. I would also encourage you to provide a slightly longer and more detailed explanation here. For example, it is not clear how you derive the thresholds in the training subset. Perhaps it would be instructive to walk through the methodology in more detail for a given example (e.g. PP with w=0.05) and include one or two figures.

P2177L2-5: What do these waveforms look like? If they are specular what else (apart from leads) could have caused them? This links back to my comments about the resolution of the MODIS data, it's ability to detect very small leads and the sensitivity of altimeters to very small leads. Section 3.2: Would it be possible to quantify the classification performance in terms of the retrieved elevation? For altimetry, and in particular for estimating sea ice thickness, we are actually interested in the SSH determination. Perhaps for the best performing classification parameters and thresholds the authors could compare the elevation transects? Another way SSH precision is usually quantified is to calculate the along track variance of the SSH at 20Hz, 40H, 100Hz. This would be an excellent addition to the paper. Whilst the authors have shown that lead detection is of interest in itself (for lead distribution and lead width studies), this paper would be of a much broader interest to the altimetric community if the classification parameters were assessed based on their ability to measure SSH and not just detect leads in the first place.

P2179L3-14: Do the authors have an explanation for the apparent discrepancy between CS2 and AMSR-E? It seems that CS2 is detecting many more leads in the Marginal Ice Zone – perhaps CS2 is more sensitive to smaller leads that are not detected by AMSR-E? Section 3.3: Surely CS2 will consistently over estimate the lead width? The CS2 tracks will not always be orthogonal to the leads and when CS2 crosses leads at oblique angles the lead width as you have defined it will always by over estimated.

P2181L8-14: The 'false lead rate' again leads me to question whether the grounds truth you have presented are simply missing small leads, that could still dominate the CS2 return. If the waveforms are being identified as specular then there must be a very flat surface within the pulse-limited footprint (i.e. not sea ice), so what other explanations can the authors think of for these echoes? Also, it seems that these classifiers are performing very similarly in terms of the lead detection rates, but I would stress again that what really matters to CS2 users is the SSH determination. How do the MAX and PP classifiers perform when it comes to SSH determination? I would strongly encourage the authors to include this in the study.

P2182L20-P2183L3: This paragraph is very unclear and I do not really understand what you are saying. Suggest it is re-written more clearly.

P2183L5-6: This is not a 'possibility' as you say here - it has been shown to be true. Section 4.2: Whilst I think that you may be correct here, this section is all just vague speculation. Again, I encourage the authors to include some comparison of the elevation transects using the optimised classification threshold, and/or the along track SSH variance. This is an easy step to take and you would then be able to say for sure whether the MAX classifier is better than PP at getting rid of off-nadir leads. This is what is of interest to the altimetry community.

P2184L14-19: The ice edge from CS2 seems to extend right into the central Norwegian Sea, which isn't very realistic – so there seems to be some lead detection where there is no ice present?

P2185L5-7: I am not convinced by this explanation – it is known that leads can dominate the return if they cover a few percent of the footprint. Section 4.3 & 4.4: Again, surely the CS2 lead width and spatial distribution will be affected by the fact that CS2 will not necessarily cross leads orthogonally? Is it possible to account for this effect or correct for it in some way?

P2187L16-19: You haven't shown this to be true – it might be the case that using the different parameters and thresholds you have presented makes no difference to the SSH determination, or it might be that the MAX parameter is worse than PP. I don't think that this is necessarily the case, but you could easily show if it is true or not by including a comparison of SSH using the optimised lead classifications parameters.

Figure 2: It would be instructive to plot the across track extent of the CS2 pulse limited footprint if possible – this would give an indication of where off-nadir leads are dominating the return.

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