

General comments:

This manuscript introduces a new method for Arctic lead detection using a waveform mixture analysis. The method is evaluated by comparison of its performance with other lead detection methods over MODIS imagery. Maps of lead fraction are also presented, and compared with those from other methods. Whilst the application of waveform mixture analysis to CryoSat-2 waveforms is novel and I would like to see it published, I think major revisions are needed first. My key concerns are outlined below, followed by some specific and technical comments.

Methodology

The method for applying waveform mixture analysis to CryoSat-2 waveforms is not clearly explained, and a number of assumptions are not well justified. I suggest that Section 3 requires major revision. Some specific examples:

- On P5 L1-2 the authors state that proper selection of endmembers is essential. At this point they should explain what is meant by an endmember and endmember vectors, before the linear mixture model is defined. The brief description on P5 L9, which follows model definition, is insufficient.
- P5 L21: How/why were these particular CryoSat-2 files selected?
- P5 L25-28: I do not agree that the comparison of waveforms from March and April 2011-2014 to waveforms over the much broader timeframe (in terms of months and years) of January to May, and October to December 2011-2016 is sufficient to justify the use of DT for waveforms extraction. Ideally the comparison needs to be extended to the full timeframe. Furthermore, the authors should describe how such a comparison was done.
- P6 L10: How/why were these particular images selected?
- P6 L25-26: How were the observations permuted (by what increment), and why only 30% of the observations?
- P6 L29: The authors should justify why January to April 2011 was chosen as the timeframe for comparison of lead fractions from waveform mixture analysis and existing lead detection methods. This is especially important, as the selection of a different timeframe could alter the results outlined in Section 4.3.

Evaluation

The evaluation outlined in the manuscript is inconclusive, and I believe the results are oversold. Throughout the manuscript the authors state that evaluation of lead classification with MODIS imagery has shown “better performance” than previous methods (e.g. abstract, conclusions, P7 L19). However, I strongly disagree that this is proven by the results outlined in Section 4.1, Figure 2 or Figure 3. The accuracy of the selection should only be evaluated based on lead statistics (user’s and produce’s accuracy for leads), and there does not appear to be a significant statistical difference between all methods. Whilst I encourage the authors to discuss the subtle similarities and differences that they have found between the accuracies of each method I do not agree with their conclusion. It is stated in the discussion Section 5.1 (P13 L4-5) that “the overall accuracy metrics of the proposed waveform mixture analysis approach was comparable to those of the existing methods”, which is far more representative of the results shown.

Specific comments:

P1 L17: State which parameters (beam behaviour, pulse peakiness...)

P1 L23: Why only mention spring and winter? Leads are a common feature in all seasons, and the manuscript covers fall in addition to spring and winter (e.g. Figure 8). If the authors are defining seasons as certain months, then they need to be defined at this point in the text.

P1 L23 and L24: “large” and “huge” are meaningless words, without any quantification. This is an issue throughout the manuscript and I suggest the authors check for and remove all such adjectives.

P3 L7: Define SAR and SARIn

P7 L6: Are the authors basing this similarity on visual analysis only?

P7 L16: A description is needed for how the parameters were rescaled. This is crucial as the authors display these results as being representative of other methods (Rose, 2013; Laxon et al. 2013) after application to Baseline-C CryoSat-2 data, which may not be true. Related to this, it should be made clear that the ‘Rose (2013)’ and ‘Laxon et al. (2013)’ results are the authors own version, using the methodologies outlined in the related papers and therefore may differ from the actual results.

Figure 2 caption: State day and year of images, in addition to month

P10 L2: There are no letters on Figures 4 or 5

Figures 4 and 5: Whilst I appreciate the overview these figures provide, it is hard to see much detail, such as the higher lead fraction mentioned in spring 2013. There is also no logic in separating into two figures. The data would be better represented by maps for a single year to show seasonal progression of lead fraction, and a time series of mean lead fraction for each month to show inter-annual variability.

P12 L8: Again, more transparency needed that these are a reproduction of Laxon et al. (2013) results

P13 L14: From Figure 2k it appears that the Lee et al. (2016) DT method detects more than one lead in the region. This should be discussed here.

P13 L15: “typical” according to who? If some methods do detect a lead without surface elevation information how can the author be sure that a lead is not present?

P15 L13: Do not make a claim on trends, for only 6 years of data

P15 L14: The months corresponding to spring should be defined in the text, not just the figure caption. Do this at the point in the paper when seasons are first introduced.

Section 4.3. I would appreciate a comment on how these result may have been impacted by the timeframe selected by the authors. See also my concerns regarding methodology, final bullet.

Technical comments:

P1 L19: 2011-2016, rather than 2006-2011

P1 L22: “sea ices” to “sea ice”

P1 L23: “exchanges” to “exchange”

P1 L27: Change to “...could increase near surface temperature by 3.5 K...” or similar

P2 L4: “Recently, **the** Moderate...”

P2 L11: "...of **the** Advanced..."

P2 L13: Change to "Radar altimeters can detect leads as well"

P2 L24: "produced" to "produce"

P3 L3: "...carrying **the** Synthetic..." Check for missing "the" throughout the manuscript please. I won't correct any more.

P3 L3: "burst" to "bursts"

P3 L10: "...**so-called** Doppler beams..." (for clarity)

P11 L7: "...**statistical** uncertainties..."

P14 L1: "Figs. 8" to "Figs. 7"