Review of paper by Quartly et al.

General Comments:

The authors aim to provide a review of radar altimetry techniques over the Arctic Ocean for retrievals of sea surface height and sea ice freeboard/thickness. The scope is broad and includes a discussion of the available data sets, certain altimetry basics, altimeters, surface classification, retracking techniques, validation approaches, needed geophysical corrections for retrievals, and future opportunities. This is rather ambitious undertaking in attempting to address both the sea surface and ice surface derivations at the same time.

I find the manuscript to be somewhat disappointing; there are a number of technical inconsistencies and important omissions. I also expected a review rather than a survey. In a review, I should expect the authors to provide insights and critical/quantitative assessments of the current state of knowledge, what has been achieved, and what needs to happen to push forward. These essential elements are missing. Instead, I find a broad-brush survey that is not quite comprehensive enough for the non-expert (terms are insufficiently defined) and not that useful for the expert.

The manuscript is rather qualitative and offers few quantitative insights; the authors do not describe what is required (in terms of altimetric accuracy) to understand sea surface anomalies, and sea ice thickness, and the various error sources. The introduction should first detail the centimeter-level requirements for understanding changes in, for example, geostrophic circulation and sea ice thickness. The remainder of the manuscript should then answer questions relevant to what has been achieved (quantitatively, i.e., data quality) in recent altimetry work, geophysical understanding of trends or variability (in the published literature), and recommendations for future work.

The authors surveyed a number of techniques on re-tracking and approaches for validation of the results. However, the authors supplied very little conclusive evidence that there have been progress because the difficulty in assessing the variety of results and approaches. One of the few places I found a quantitative discussion of data quality was in the section on ice thickness.

Snow depth, one of the sources of uncertainty in ice thickness retrieval, received relatively little attention. Also, the need to understand snow properties, including salinity and stratigraphy, rather than just density and depth should be emphasized. Salinity, especially, affects freeboard estimates especially if a dual-band altimeter were to be used for snow depth retrieval.

The conclusions section should provide a summary of what has been accomplished in the several decades of radar altimetry over the Arctic. What was offered in the second paragraph of the conclusions section seems to suggest that retracking is the primary concern, and perhaps it is for altimetry experts. But, understanding the geophysical medium being sensed, which controls the returns and therefore the quality of geophysical retrievals, is as important if not more so. This aspect is missing and should be emphasized.

I do not recommend publication in its present form because a more focused manuscript with critical/quantitative assessments of procedures and current geophysical utility of radar altimetry would entail significant revisions to this paper. Any revisions to the current paper to address the above concerns should be considered a new submission and reviewed as such.

Detailed Comments:

Page.Line number

2.10 'ocean' temperature is perhaps better than 'sea'.

2.21: For navigational purposes, I think ice thickness would be more of an interest than ice volume.

2.25: Are ridging rates really a requirement? Please provide a citation here.

2.27: "Daily evolutions and high spatial resolution…" of what?

Figure 1. I question the usefulness of this figure, especially the bathymetry bit because it doesn't seem relevant to the review at hand. Also, it's difficult to see the purple circulation patterns.

4.4: '…typically every 300 m…' is not correct especially with the altimeters in Fig. 2. One needs synthetic aperture processing to achieve resolutions of 300-m along-track– showing the spot size of each of these altimeters in Table 1 would be useful. Since this is a review article, it may also be important to explain how these spot sizes, perhaps briefly, are defined.

4.5: The connection between along-track sampling and spatial coverage seems odd: "… tens of days of along-track sampling are required… to give good spatial sampling…' I suppose you mean across-track separation between the orbits.

4.15 I don't think the altimeters are not in the same orbit, only same orbit inclination.

Table 1. 'Non-circular antenna…" is not correct but why is this significance here?

5.1 '…superposition of multiple…" perhaps better to say aperture synthesis from multiple viewing geometries.

5.6: This may be a good place to provide a better definition of LRM.

5.9: What are "continuous ice floes"?

8.29: I think there needs to be a justification as to why these two techniques were reviewed in this manuscript. Do the authors think that these are more promising than others?
11.9: 'narrower' is better than 'smaller'.

11.10: please list the three techniques.

12.5: I guess the point to note is that the techniques are typically tuned to specific altimeters.

12.11: It would be useful, in a review article, to say something about actual lead widths in this context.

13.5: I think you mean ice drift and not currents. Also, 100 m hr-1 is not a good average number to use for the Arctic – more useful to provide a range.

13.10: what is water-leaving radiance?

13.35: this doesn't bode well for the use of SAR imagery. Dark areas in SAR imagery are not necessarily leads (meaning open leads); they could be quite thick first-year ice.

Section 2: Thus far, the use of other data sets to validate surface classification seems quite discouraging, wouldn't you say?

17.2: how does the thermal noise in the altimeter translate into ranging noise?

Figure 14. The area over which these anomalies are calculated should be shown.

26.1: The different geophysical processes that contribute to sea surface heights should be described and why they need to be removed for estimation of dynamic ocean topography and the calculation of geostrophic current should be clarified.

26.15 The distinction between DAC and inverted barometer should be described.

Section 4.1: discussion of a number of tides is missing, e.g., solid earth, pole, etc. Even though the authors chose to discuss two that are important, the others should be mentioned. I think the magnitude of these tides in the Arctic should be listed.

28.10: The length scale over which residual ocean tilt (due to eddies and other circulation patterns) may affect freeboard calculations is important and should be discussed.

28 – The current developments in estimation of snow depth should receive more attention.

32.30 The authors failed to mention the potential effect of salinity on returns – due to brine wicking- from the snow-ice boundary (reported in Nandan et al., 2018). This should be discussed.

Section 6.4 seems to be outside of scope of radar altimetry.

40.1: It should be snow properties, including salinity and stratigraphy, rather than just density and depth.

Section 7: The second paragraph suggests that retracking is the primary concern, and perhaps it is for altimetry experts. But, understanding the geophysical medium being sensed, which controls the returns and therefore the geophysical retrievals, is as important if not more so. This aspect should be emphasized.