Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication).

I would like to congratulate the authors on a very fine piece of work. This paper is a significant advancement in our collective ability to remotely sense the subsurface percolation facies of the Greenland ice sheet, It takes a significant leap above other L-band-based studies (including recent works by the authors) that calculated only the extent of firn aquifers across Greenland. The inclusion of ice slabs, using a single instrument, gives us the "other half" of the meltwater equation, and unlocks the future possibility of remotely sensing these facies annually to study the evolution of the ice sheet's percolation facies over time in a warming climate.

The authors thank the reviewer for the positive comments.

This manuscript has made significant improvements over the original manuscript, not the least of which is the clarity of its goals and conclusions. The simplification of the algorithm to detect "known" facies that have been previously mapped (even if only incompletely from airborne radar) represents a substantial advancement, without teetering into speculation of the unknown or unproven. This simplification does come with drawbacks, and the authors note that their algorithm as presented here does not have the ability to determine interannual changes in aquifer or ice-slab extent. But as they note, that possibility still exists with potential future work and further analyses.

Slight correction: we are definitely able to map interannual variability; however, with the uncertainty described in the paper.

I have some questions and a bit of a raised brow about some of the results outlined in Figure 7, with ice slabs & aquifers appearing in some regions where they have distinctly *not* been picked up using any other methods before. I note specifically the ice slab regions in far South Greenland (between 60-62.5 N) where no ice slabs have ever been found, as well as some isolated aquifers detected by this method in far North Greenland (above 78 N) where accumulation rates are almost certainly far too low to geophysically produce such perennial aquifers. Those areas raise a bit of concern about the reliability of the algorithm. But, I think the authors have taken care in this manuscript to identify this product as a "draft" that is subject to further revisions as time and effort allow, and they have deliberately noted the fact that even slight changes in the cutoff parameters can cause large shifts in the boundaries between these facies. Since I have no suggestions to improve their algorithm to fix these anomalies, and the authors note that errors likely do exist due to very-slight adjustments in the algorithm's parameters, I do not see a need to withhold the publication of this paper any further based on those uncertainties. They stand as they are and the authors note the uncertainties within. (And, perhaps, some future fieldwork or airborne campaign might discover these facies in those areas... I have to remember as a reviewer that the absence of evidence does not imply evidence of absence. The authors make a plausible case for why these facies may exist there despite never having been found there. Since I can't prove these facies *don't* exist in every spot they are identified here, it is sufficient to let the algorithm pass as is and save refinements for another day.)

The authors tried to be very specific about the limitations of the algorithm, and associated uncertainty. This is the limit of the four-parameter ice sheet-wide single frequency (L-band) mapping algorithm. It simply can't be tuned any closer to the airborne data, and that may not be appropriate because of the time shift between the two data sets. The authors are currently working on an active-passive multi-frequency approach which may reduce the uncertainty. But, it is a complicated retrieval.

This manuscript reads very well. Much of the methods are heavily technical in nature, but this is a methodology paper and the techniques used to process the data must be adequately described. (I disagree wholeheartedly with one of the other reviewers' comments on the original manuscript submission, who called the paper "too technical"... it would be incomplete if the authors left out most the equations and only vaguely described their methodologies using hand-waving descriptions. But I digress.)

Thank You. The authors are very much in agreement that the technical details are critical to this particular manuscript, and provide a more rigorous foundation to step from a

technical paper to a science paper – where the obvious objective will be to identify interannual variability and potential expansion.

The inclusion of "spatially-coherent melt layers" is perhaps unnecessary in this manuscript, given that they are not being detected or mapped at all by the SMAP algorithm, and seem a bit extraneous to the conclusions of the paper. However, their inclusion in Figure 7b as well as the zoomed figures 8 & 9 do help tell an interesting story, and as such I don't think they really need to be omitted for the manuscript to be approved. They are just a bit unnecessary to make the conclusions in the study, but don't need to be removed, in my opinion.

The authors agree that the "spatially-coherent melt layer" inclusion is a bit tangent to the main results of the mapping algorithm. The key reasons the authors decided to keep them in the manuscript were (1) they were a pronounced feature in the radargram in Fig. 3a, (2) there are distinct in L-band signatures throughout the upper percolation and dry snow facies (i.e., Figure 4d), (3) the authors believe in the future they will play a significant role in the evolution of both perennial firn aquifer and ice slabs (Section 4), and (4) there is clear potential to develop an L-band algorithm to map these features.

I see no significant reason (save one) for publication of this manuscript to wait any longer. (I outline the one caveat at the end of this review.) Let it be published. I have very slight line-item recommendations to make, outlined below. Not really "revisions", per se, just grammar checks, really, as well as some basic minutae. The authors can likely address most of these in 15 minutes or less.

Line 135: Include a period (".") at the end of this paragraph.

Corrected.

Line 156: "and an innovative extension of the science objectives." Perhaps for clarity, add the word "mission's": "and an innovative extension of the mission's science objectives." (Up to you, though.)

Corrected.

Line 275: "relatively thin (0.02 cm-2m)" ; Did the authors mean 0.2 cm (2 mm) rather than 0.02 cm? I don't know that anyone has been able to measure ice lenses just 0.02 cm (0.2 mm, just 200 microns) thick. Easy typo.

Are you sure we can’t measure ice lenses that thin? 😉 Corrected.

Line 361: I'm not sure that "runs-off" needs a hyphen there. Perhaps remove.

Corrected.

Line 636: "McFerrin" --> "MacFerrin"

Corrected

Line 672-673: "...meltwater run-off across ice slabs in the percolation facies was recently observed in visible satellite imagery collected by the NASA-USGS Landsat 7 mission during the 2012 melting season (MacFerrin et al., 2019)" ; Although MacFerrin, et al (2019) did prominently note this runoff over ice slabs in Figure 1 of their 2019 paper, the first use of this particular image was in Machguth, et al (2016), which displayed the same Landsat-7 image (with slightly different processing, used by an overlapping list of authors) to first illustrate runoff over impermeable ice layers. I would suggest changing this particular citation to "Macguth, et al (2016)" to note the first time this was observed in satellite imagery.

Thank you for noting the mistake. Corrected.

Lines 713-718: This paragraph (starting with "Future work will focus on...") reads more like a research proposal than a paper conclusion. This work may get done (I certainly have full confidence in the authors' abilities to do what they describe), but it seems unnecessary to list out future plans in the conclusion of a research paper. It's up to you and the editors, but I might consider omitting this paragraph as just being somewhat unnecessary. That isn't a "demand" though, if the authors & editor agree it's stronger with this paragraph in there, leave it be.

And then, the last point:

Line 750: "[data] are available from JZM upon request." I was excited about this manuscript right up until this line. 100% of this work was publicly funded, and the manuscript is being published in an open-access journal. "Data available [only] upon request" was a relict of the 20th century that needs to go away. In the entire paragraph preceding that statement starting on line 731, the authors did not need to personally 'beg' every lead author of the 10 different respective data products they used; it would have been ungainly and prohibitive to do so. The authors were able to publicly download every one of those datasets freely. This manuscript wouldn't have been possible without access to that data. Keeping the data presented in this paper behind a "locked door" is no longer an ethical use of resources, and in fact runs afoul of the stated policies of the NASA programs (see Acknowledgements) that funded this work. This one detail is the reason I had to change the conclusion from "Accepted as is" --> "Accepted subject to minor revisions." Publicly-funded work, built entirely using publicly-available data by scientists working in publicly-funded salaried positions, published to publicly-accessible open access journal, should be made public.

Whoah! Ease Back!

The authors are in complete agreement that publicly funded data should be publicly available. There was never any intention by the authors to keep the data behind a 'locked door' or make anyone 'beg' for it. Indeed, the data has already been very freely passed to other scientists for analysis prior to publication, with an open invitation for discussion, especially given the significant uncertainty noted in the above paragraphs.

The algorithm and the data are being actively developed as part of a SMAP Science Team project, which includes public distribution of the data (and metadata) through the NSIDC DAAC together with a full Algorithm Theoretical Basis Document (ATBD). Since this is an early version – a prototype - the author (JZM) was simply trying to make sure the data distributed to the public were the most up to date version at the time of the request.

That data has been uploaded here: https://www.scp.byu.edu/data/aquifer/.

Other than that, great paper. Excellent revisions. Let's see it get out!