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Discussions

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# Interactive comment on "Sensitivity of active layer freezing process to snow cover in Arctic Alaska" by Yonghong Yi et al.

**Anonymous Referee #2**

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General:

The paper by Yi et al. presents a study applying a remote sensing driven permafrost model, in situ observations and airborne P-band SAR retrievals to study the influence of snow cover characteristics on permafrost active layer dynamics in Alaska. The model, developed by the same group, is driven with diverse observational data including MODIS LST and SMAP L4 root soil moisture products. A part of the paper is devoted to downscaling MERRA-2 snow depth estimates to 1 km resolution using the MODIS SCE record and a digital elevation model, and assessing the validity of the downscaled product to in situ data. Along a limited study area, the authors report the autumn zero-curtain period predicted by the model to closely match observations, while both of these were found to be positively correlated with fractional snow cover.

Some supporting evidence is derived from the P-band SAR observations, although the analysis here is only perfunctory. In regional scale simulations, it is reported that the top active layer zero-curtain period is correlated with timing on snow cover onset, while deeper layers are more influenced by maximum thaw depth. The authors indicate that with climate induced deepening of the active layer this may result in a feedback of an extended unfrozen period in the lower active layer, resulting in increased carbon loss during winter. The theme of the paper is of high interest to the cryosphere community, as bridging the gap between what can be observed via remote sensing and what actually happens with permafrost is a long standing but still ongoing topic. Different proxies such as snow cover and surface freeze thaw driving physical models are required due to the difficulties of directly observing deep soil processes. This study presents a valuable step forward in modeling permafrost dynamics with aid of remotely sensed observations, which is why I feel the study merits to be published in the Cryosphere. Downscaling methods applied to MERRA-2 snow depth estimates may be of interest when applied to other coarse-scale products on snow depth and mass as well. The paper is well written, although parts of it are rather heavy reading (mainly section 2) and could benefit from cutting down some of the text, replaced by perhaps illustrations depicting the analysis process in the form of flowcharts etc. The authors may also consider the usefulness of some of the results, in particular the added value of the P-band SAR observations. Are these really needed, or do they provide only an unnecessary diversion? In any case I recommend publication of the work, following response to the following minor comments.

Minor comments:

1. Introduction, P3, lines 6-8: I think citing "high-frequency" instruments here is a bit misleading, as it refers mainly to the FT-ESDR dataset (where high-frequency Ka band data is indeed applied). This statement is not really applicable to scatterometry (e.g. C-band ASCAT) or L-band SMOS and SMAP F/T products. It is a good question how much "high frequencies" indeed tell about soil freezing due to very limited penetration

already at C-band in surface vegetation, and the influence of e.g. snow cover at frequencies above Ku band. Of course, the field of view of all of these instruments is very large resulting in coarse-scale products, so that part of the sentence applies (but what about C-band SAR, which are increasingly available from Sentinels and RCM?) I suggest to rewrite this sentence properly acknowledging the various benefits/caveats of different wavelengths. Also add a suitable reference to the scatterometry-based method as well as the SMOS soil F/T product.

2. Section 2 requires some effort from the reader. It is difficult to follow all the diverse steps required for first gap filling and downscaling MODIS and MERRA-2 snow products, steps required for other reference data, followed by the actual data analysis, and where all these data are finally applied. I understand this is due to the complexity of the analysis, but still it took me two to three read-throughs to finally get a grasp. A flow chart summarizing all of these data preparation steps and the consecutive analysis, indicating clearly where each bit of data is used, could greatly clarify the process. This could be supported by a brief introduction on what is to follow in the beginning of section 2 (following for example the one you have in the beginning of section 3).

3. P4 eq. 1. For clarity please define all variables in the equation, even though these may be self-evident ($z$, $t$, $z_s$, $z_b$ etc.). Furthermore it is not clear how volumetric heat capacity and thermal conductivity are calculated (it is only stated that these "vary" with F/T state, depth etc). Please clarify in the text.

4. P7 eq. 7. Again, please give all variables of the equation in the text.

5. P10 line 20. "Mostly focusing" is a bit unclear… can you elaborate? Where are other transects besides DHN used, could be stated here.

6. P11 line 21 "a cutoff threshold of 50%" is not introduced or explained anywhere from what I can see. A reader not familiar with the concept might be highly confused. Please elaborate.

C3

7. Section 3.1.2, p12 lines 18-26. Might parts of this paragraph be more suited in the methods section? Also, please explain further how thermal conductivity was assumed to "gradually increase". Was the increase linear? This might also give a partial answer to my comment 3, and the explanation might be also better suited for section 2.

8. P13 line 15: "in situ dielectric measurements in frozen soils have significant uncertainties". Why? This should be explained, and references given to support the claim. Also, avoid use of word "significant" unless you are discussing statistical significance.

9. Section 3.2.1. is the inclusion of the P-band airborne SAR observations really necessary? Although the results seem to corroborate your findings, I think these data might be of more interest as a separate paper by itself (provided more analysis can be provided; the data would seem useful for e.g. electromagnetic model exercises from frozen soil). The title of the section is at least strange, change that if nothing else.

10. P18 line 19 add some suitable references to passive microwave products already here (also e.g. works by Kelly et al.).

11. P18 line 22 "the use of radar is limited…" well this limitation applies to current operational systems as they typically have X-band as the highest frequency. However, QuikSCAT provided already some indication that snow volumetric properties could be captured using radar. Airborne data (e.g. Yueh et al., 2009; King et al., 2018) have provided similar indications (while also revealing limitations). What I am driving at: please be specific about the wavelength and that you are talking about radars currently in space when you make this claim.

12. P18 line 25 "no satellite lidar is currently available…" IceSAT2 just went up, and although no terrestrial snow products are immediately planned, it could be of use also for snow mapping (despite limited coverage). Any comment? Might be good to acknowledge the mission.

13. P19: "Radar backscattering measurements…" again I miss definition of the wave-

C4

length. A suitably long wavelength would be needed to get meaningful information from subsurface properties, while shorter wavelengths would provide limited information.

14. P19: line 9 "similar to all other inversion problems..." Replace 'all' with 'many' or 'typical'? Can't one can find unambiguous inversion problems?

15. Conclusions, p20. At the end you could cite upcoming or planned long-wavelength radar missions, which may be of use for the purpose you cite (NISAR, TanDEM-L, BIOMASS).

16. Figure 8: add time period for which trends were calculated to figure caption.

Editorial: 1. P14 line 8 "we then discussed" might sound better as "we then discuss" as the discussion is to follow. 2. Figure 9a: add unit (days) to y-axis

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