

Interactive comment on “Potential permafrost distribution and ground temperatures based on surface state obtained from microwave satellite data” by Christine Kroisleitner et al.

Anonymous Referee #3

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The authors further develop research done by Park et al. 2016 suggesting higher threshold of frozen surface state is needed to better estimate permafrost extent (as compared to IPA map) and use simple linear model to estimate MAGT (based on GTN-P). This is probably a reasonable approach when dealing with climatically driven permafrost which is in equilibrium with present climate, but unlikely to give reasonable estimates (or it is unclear in the text) in areas where permafrost temperature is controlled by ecosystem factors and decoupled from atmosphere and even ground surface conditions (snow, moss etc). I would therefore recommend to limit the comparison of the permafrost extent by the IPY map and that derived from satellite data to the continuous permafrost zone or treat continuous vs other permafrost zones comparison

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independently (there is a mention of separate classes but unclear in the text). This has a potential to show that the method used in the paper is actually useful and can be applicable on pair with others like modeling which will encourage permafrost scientist to consider using remote sensing in some areas/regions (decrease of standard error from 3 to 2.5 C is not very important in the overall argument for remote sensing as it spatially equates to 30% of temperature variability on permafrost due to bioclimatic gradient). Therefore paper needs to develop the idea of satellite products being useful further. Suggesting one-fit-for all thresholds as main outcome at a small geographical scale and reporting smaller errors probably is too little outcome of such an intensive work. Rather the explanation of where and why the methods worked best in relation to regions/permafrost type/maybe other variables like ground ice content will be very valuable but is hidden in bits and pieces all over the text. Structuring those pieces in overall argument may create broad interest from readership of cryosphere, which otherwise is unlikely not to read the paper past the abstract. I believe that the authors have all the component to restructure the paper in order to clearly define the permafrost regions where the satellite derived extent from the products that they used correspond to the IPA map and where (at least broadly) it agrees or disagrees with observational data (and if no observational data, maybe with modeled data as well). At the current form the manuscript is too technical for scientific publication, specifically results and discussion is very poorly written, unstructured and hard to follow.

This may be improved by merging results and discussion section and giving it geographic rather than technical focus. For example structure around general regions: Scandinavia/ European Russia/ West Siberia etc. Within each region present results/undertantices /errors and overall summary and discuss why it worked or it did not (and provide the factors that may contributed using references to support your findings). Overall findings from looking at all the regions may go next. What is different between the regions where the methods worked and what is the same (like better performance in continuous permafrost). Examination of regions (and their characteristics – continuum, temperature, ice contend, percent of lakes or zone mentioned in p2 I25-

35) where surface state derived from obtained from satellite data actually gives similar to observed by other method results and where (and why it does not).

I believe that providing readers with this region specific information may make this paper very valuable and useful for entire permafrost community, not just remote sensing community focused on cold regions.

Few specific comments: Interesting that frozen surface extent and MAGT have difference of only 6-8 days, this difference is physically very small, unless we think about surface of bedrock and MAGT of alpine permafrost. This small difference may result from assumption that snow melt days should be treated as unfrozen? It may be that the assumption of coldest sensor being representative of MAGT? The discussion of snow melt days that authors suggest to treat as unfrozen needs further clarification as it is not very straightforward for permafrost regions and does not make much sense. Develop the examples you cite in relation to what your results are rather than given references to support the argument. Short term record of ASCAT is concerning by itself, but if you use two years of data for validation and you are getting rid of half of your short dataset, so why not use cross-validation instead (also will improve your estimates)?

Figure 1 and discussion will greatly benefit from the map showing difference between ASCAT and SSM/I (at least for years with 2+), It is very hard to follow what authors are trying to say from p6 l 25 to p 7 l2. I suggest to expend 4.1 to have a better linkages between sentences which largely disconnected. There are typos and no mention of table 1. A2 needs different color scale (maybe cut off of two weeks to show variability, otherwise it all looks the same). What is grey – snow melt in <1 day? This is hardly the case for many regions showing in grey, so is it no data? P10 l 1 correct reference spelling P10 l5 Vorkuta region is not in West Siberia, it is in European Russia While the assumption of the coldest sensor may work in conditions of aggrading or equilibrium permafrost, in the conditions when permafrost is warming the coldest sensor is probably at some depth below the surface, but is it around ZAA, not convinced that this approach is reasonable, please explain further.

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