The Cryosphere Discuss.,
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The Cryosphere Discussions

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

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Many thanks for your constructive comments to the manuscript. We have included your comments in *italics* below, response in normal font.

I would 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 independently (there is a mention of separate classes but unclear in the text).

We agree that the direct comparison to MAGT should be made separately for different permafrost types. Results are shown in Figure 1 (top row). Differences to MAGT are larger in the transition zone (isolated and sporadic). They tend to be warmer than modelled. This effect can be reduced (especially in the sporadic zone) by exclusion of the snow melting period. In order to discuss other factors, we also suggest comparison to snow amount. As snow depth is usually not provided with the borehole data we suggest to use SWE from satellite derived GlobSnow records. Figure 1 (bottom row) separates four classes. Modelled results tend to be too cold in areas with high snow depth what exemplifies the limitations of the approach.

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. . . . 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. . . . 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.

Many thanks for this suggestion. We agree that reordering the discussion by region would be beneficial.

Also with respect to your comment below, we propose to structure the discussion into subsections:

- General issues (covering aspects such as ‘coldest sensor’, class comparability with Brown et al. (1997), scale issues)
- Regional pattern
- Performance differences between ASCAT and SSM/I (covering all technical aspects)

- The role of snowmelt (to address one of your further comments)

For rephrasing the results section, see our reply to your specific comment below.

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?

We assume that you refer to variations in linear fit (Figure 5): There is a difference in results between inclusion and exclusion of melting snow, but it is very small. It might therefore not be responsible for the similarity with in situ records in general. The coldest sensor appears to be representative for MAGT in most cases. See Figure in reply to reviewer one and also figure 3 (comparison with meta data in GTN-P database).

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.

We propose to replace the discussion with the following text (and add also separate all snowmelt discussion in an own subsection including remote sensing aspects): 'The number of snow melting days are in general highly variable in the Arctic (Bartsch,2010) but the melting period is comparably short (Zhang 2005). Snowmelt is expected to delay the soil surface warming due to latent heat and therefore cools the soils (Zhang 2005). Latent heat released due to refreeze of meltwater may have a warming effect after a few days (Dingman et al., 1980). Dingman et al. 1980 also report start of soil thaw before the end of snowmelt at Barrow. The overall impact of snowmelt is expected to be dependent on local conditions (Zhang 2005). Our results suggest that there is a warming effect with an impact on MAGT. Days with melting snow should be therefore treated as unfrozen. This leads to higher MAGT (on average 1°C for considered borehole locations) and better agreement with in situ measurements.'

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Dingman, S. L., R. G. Barry, G. Weller, C. Benson, E. F. LeDrew,and C. W. Goodwin (1980), Climate, snow cover, microclimate,vand hydrology, in An Arctic Ecosystem: The Coastal Tundra at Barrow, Alaska, edited by J. Brown et al., pp. 30–36, Van Nostrand Reinhold, Hoboken, N. J.

Zhang, T. (2005), Influence of the seasonal snow cover on the ground thermal regime: An overview, Rev. Geophys., 43, RG4002, doi:10.1029/2004RG000157.

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)?

We agree that it would be beneficial to include also results for the calibration years. We would suggest to use them specifically for discussion of regional patterns with respect to year to year variability as pointed out by your further comments below.

Figure 1 and discussion will greatly benefit from the map showing difference between ASCAT and SSM/I (at least for years with 2+),

Figure 2 shows a difference map of data in original figure 1. A difference of 4 years corresponds to areas where only SSM/I detects permafrost (and in all years), -4 years where only ASCAT detects permafrost. In agreement with reviewer one and for consistency we also propose to include difference maps instead of figure 7. These maps should then include the calibrations years, in agreement with your previous comment above.

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.

We suggest to separately discuss results of comparison to classes of Brown et al (1997) and comparisons to in situ measurement:

4.1 Threshold sensitivity analyses and permafrost extent

Mapped permafrost extent varies for each threshold step across the different zones. The area mapped as permafrost with a frozen day threshold of 180 days for the period 2007-2012 differs regionally between the ASCAT and the SSM/I data set (Fig. 1). The extent of areas where only ASCAT determines permafrost is about four times higher areas where this is the case for SSM/I. The latter largely occurs outside the expected permafrost extent. Deviations in the transition zone are lowest for Western Siberia. The classification using SSM/I data results in a smaller permafrost extent for the Canadian Arctic compared to Brown et al. (1997). The Canadian High Arctic is also largely not covered by the SMM/I dataset, The ASCAT map shows comparably good agreement in this region. On the other hand, ASCAT overestimates the permafrost extent in Scandinavia.

The largest deviation in area extent for continuous permafrost occurs in the SSM/I result with more than 2.5 Mio km2 lower values than in Brown et al. (1997) (more than 12 %, Fig. 2 [will be changed to table]). Less than 1 Mio km2 (less than 5% of total continuous permafrost area) is missed by ASCAT. This applies to thresholds between 180 and 200 frozen days (excluding snow melt days). Matching extent is lowest for 200 days in case of inclusion of snow melt days. ASCAT maps more permafrost outside the boundaries of Brown et al. (2007) than SSM/I in case of a 180 day threshold but agrees better than SSM/I for discontinuous and isolated permafrost area. This results in lower percentage agreement of the SSM/I product for the total permafrost extent (Fig. 3). The false permafrost detection by ASCAT is out-weighted by a significantly higher detection performance for the total extent. There is also more year to year variability in the results from the analysis using SSM/I data than from ASCAT.

4.2 [New subsection heading:]Determination of optimal threshold with Kendall's Tau Test and in situ measurements

The best Tau was found to coincide with 204 (FT) and 203 (FM) frozen DOY (Fig.1) for the ASCAT data sets, whereas for the SSM/I DOY of 190 showed the highest Tau with a steep gradient before and after the peak (Fig. 4). More borehole locations with positive

MAGT fall in areas with values below the original threshold of 180 days for both ASCAT results (with and without snowmelt days) than for SSM/I (Tab. 1). The agreement is highest for exclusion of snow melting days. In addition, the ASCAT results show nearly no negative MAGT below the threshold of 180. The thresholds delineated by the best correlation coefficient (Kendall's Tau) indicate a better performance regarding positive MAGT in areas below the threshold for all data sets. However, the higher thresholds lead also to more negative MAGT in these areas. For the SSM/I 77% of MAGT temperatures can be correctly allocated with both thresholds, the original 180 days and the 190 determined by the Kendall's Tau test. The ASCAT results show a higher accuracy with more than 80% correctly assigned values.

no mention of table 1.

Thanks for pointing out. The reference to figure 1 on page 7, line 6 needs to be replaced with 'Tab. 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?

Yes, grey corresponds to snow melt in <1 day. Thanks for pointing out the missing description. The higher numbers occur in some areas which are lake rich or in mountains. We therefore would suggest not to cut off the values but use a classification based on statistics (e.g. quantiles) for better visibility of differences between small numbers.

P10 l5 Vorkuta region is not in West Siberia, it is in European Russia

Thanks for pointing out. It should read 'Western Russia', in accordance with Figure A1.

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.

Figure 3 is a modified version of our reply to reviewer 1. It highlights the two locations from Vorkuta which have MAGT in their meta records. Difference between to the coldest sensor method and MAGT in GTN-P meta records is 0.17 and 0.04 °C respectively.

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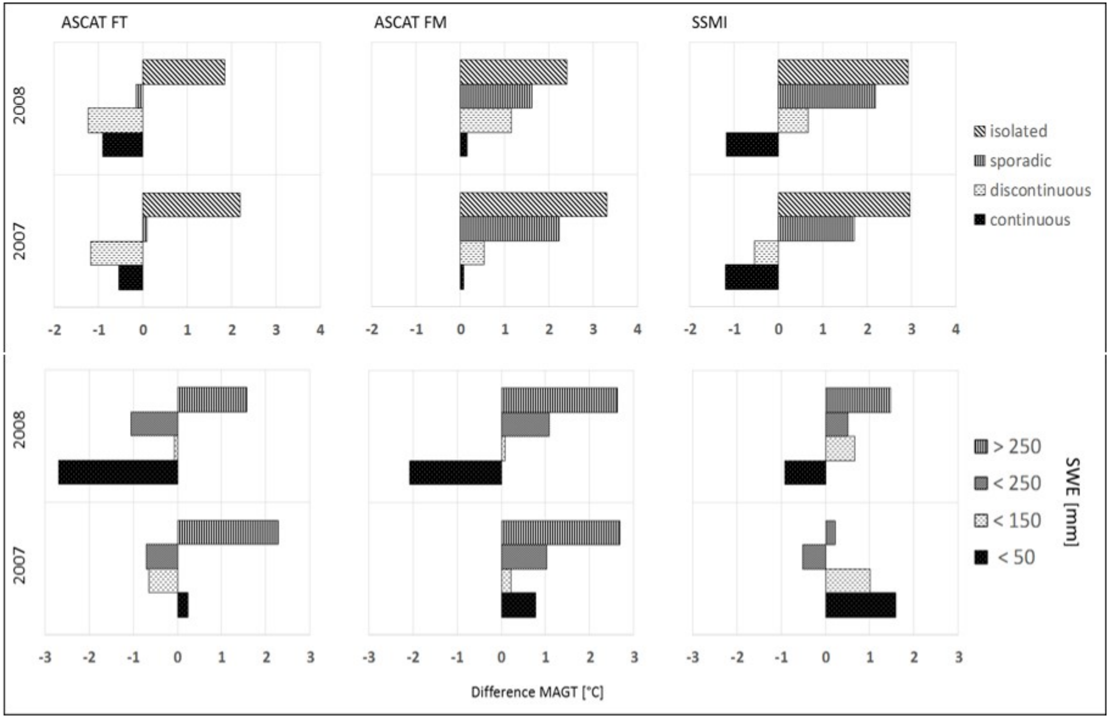

Fig. 1. Top: Difference between in situ MAGT (coldest sensor) and modelled MAGT by permafrost type (Brown et al. 1997) in 2007/8 and 2008/9. Bottom: Difference for SWE classes (source GlobSnow)

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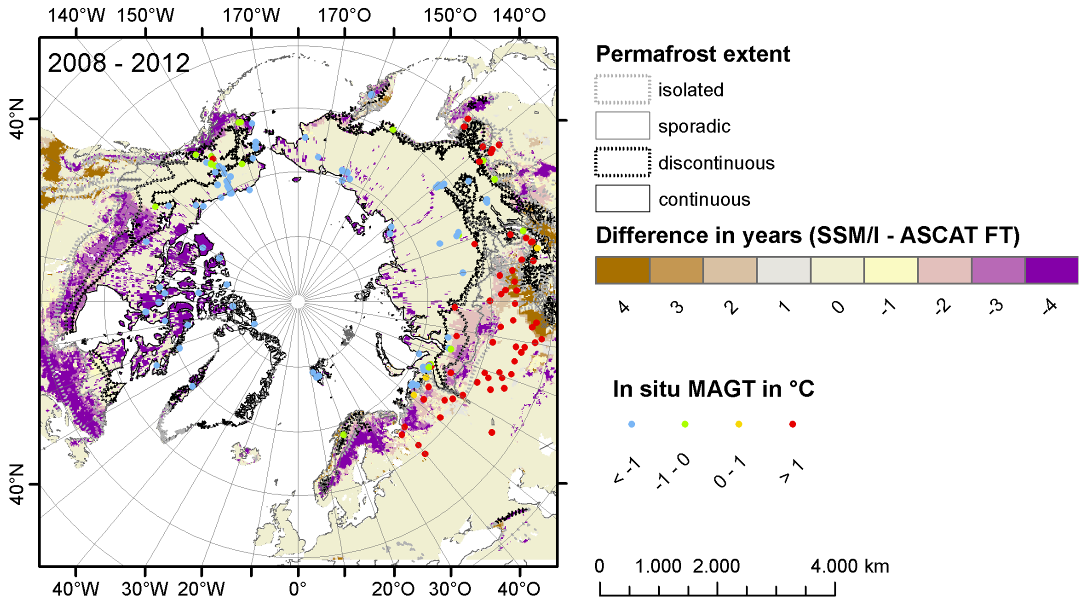

Fig. 2. Permafrost extent comparison of satellite data results from Metop ASCAT and SSM/I for 2008-2012 based on the original 180 day threshold method applied to all years

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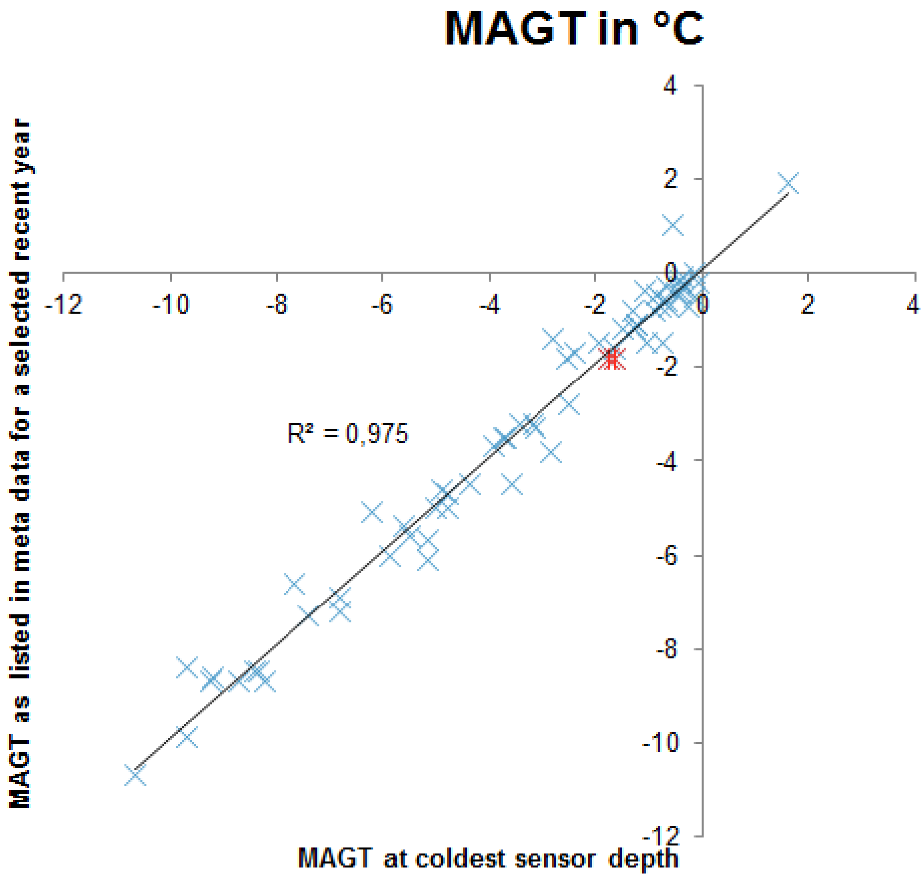

Fig. 3. Comparison of MAGT calculated at the coldest sensor (long-term) and MAGT provided in GTN-P meta records (where available). The latter represents single arbitrary years only. Red - Vorkuta sites