

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 #1

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This paper describes the determination of permafrost extent and mean annual ground temperature (MAGT) from microwave remote sensing data, using an approach that relates the number of frozen/unfrozen days to the existence of permafrost by Park et al. (2016). They discuss an adaptation of this method to their data and the data used in Park et al. (2016) and compare the results. They also try to find an empirical relationship between MAGT and frozen days to determine MAGT. They use borehole data from the GTN-P database as reference and validation. I have several major issues with this manuscript, regarding originality, description of methods and presentation of results and discussion.

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General comments

The paper presents the development of an “optimal” permafrost extend product from the ASCAT satellite dataset, using different references as the permafrost map of Brown et al. (1997) and observed ground temperatures. It relates the results to other derivations of permafrost extend from satellite data, reanalysis and observational air temperature data and numerical modelling. The paper focusses on the description of the technical derivation of the permafrost map from the ASCAD frozen/unfrozen day data. In this current form, the manuscript would be better suited to a remote sensing journal with an emphasis on such technical descriptions. However, I think this paper could be extended into a study of uncertainties in mapping of permafrost extend, providing an overview of the existing derivations from different methods. This could supply “users” like global climate modellers with a tool to evaluate permafrost extend in their models with some confidence. An across-approach evaluation of where permafrost is likely/unlikely to be, with an estimate of the uncertainty, would be something new and original (and very useful).

I have several major issues with the methods as they are described in this manuscript, see below. This covers the Methods section of the manuscript.

(1) Preparation of borehole data (sections 3.1 and 3.3)

You use data from the GTN-P database. You acknowledge that the depth of zero annual amplitude (ZAA) is not reached in all boreholes. You also write that you included shallow boreholes in your study, and boreholes not situated in permafrost, and that you used MAGT data. To decide if a borehole is in permafrost, you then look at MAGT at coldest sensor depth, because in a stable climate, the minimum MAGT would be the same as the MAGT at the ZAA. You argue the validity of this approach with a paper from Lachenbruch and Marshall (1986). You say that the minimum MAGT in a stable climate would be the same as the MAGT at the ZAA. However, they look at borehole temperatures in permafrost and only at data that was taken in depths well below the active layer

(surface in most of their plots actually refers to the permafrost table, not the ground surface). Basically, they look at deviations of measured soil temperature profiles from theoretical soil temperature profiles following the geothermal gradient. You need to find another reference that discusses the behaviour of the MAGT profile in the first 25m or so. In a sufficiently cold climate, MAGT might be lowest at shallow sensor depths well above the ZAA (see for example Figures 11 and 12 for Samyolov and Bayelva in Ekici, et al., 2015: Site-level model intercomparison of high latitude and high altitude soil thermal dynamics in tundra and barren landscapes <https://doi.org/10.5194/tc-9-1343-2015>). Did you check the MAGT profiles for the boreholes you used? You mention in your discussion(!) that you used a threshold of 1m depth. Why? Also, you state that GTN-P data is available in different temporal resolutions. Did you only consider data where MAGT was provided, or did you compute MAGT from higher temporal resolutions? If so, how did you treat missing values? You should address these issues carefully in you method description, to make sure that your comparison to observed ground temperatures/permafrost is valid!

(2) Model parameterization for ground temperature retrieval (chapter 3.4)

You state that only data in the range of 150 to 330 frozen days of year (DOY) were considered. Does that refer to observational data or satellite data? Why did you use those thresholds? From the phrasing, that is unclear. The issues of the determination of MAGT of the observational data described above also impact this paragraph. In addition, the scatter plots in Figures 5 and 6 show no indication that a linear model to fit frozen days to MAGT would be valid. I understand that the other models you tried to find an empirical relationship between frozen days and MAGT did not yield better results than the linear model, but that does not make the linear model valid!

Specific comments:

The figures need improving to be conclusive, and the Results and Discussion sections need to be more concise and refer to figures that actually help to understand them.

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Several of the figures are unnecessary. In the following, I give some very general suggestions on how parts of sections 4 and 5 and some figures could be improved. If the manuscript is rewritten to fit my suggestion in general comments section, they may not be of consequence.

Figure 1 Add the outlines of at least continuous and discontinuous permafrost from the map by Brown et al. (1997) as lines to both subplots as orientation.

Section 4.1

Figure 3 could very well be a table.

page 7, line 3 Why is there a reference to Figure 1 here? The description of Figure 1 states that it shows the permafrost extend based on the 180 days threshold.

Figure 4 This could easily be shown in just one panel, giving the different data sets different colors.

page 7, line 5 The paragraph on MAGT makes no sense.

Section 4.2 page 7, line 12 The first paragraph needs an introductory sentence.

Figure 6 should be included into Figure 5.

Figures 7 and 8. Your algorithm works well on both of you test years. That could be demonstrated in a simple table, it does not require those two figures.

Figure 9 should be a table.

page 8, lines 5 to 17 Those paragraphs discuss mapped permafrost extend. Why are they in the MAGT section?

Figure 10 The colors referring to Permafrost types are difficult to distinguish from each other. The same for the colors referring to threshold. Stations marked with red dots are not explained.

5. Discussion

I would suggest to follow the order in which you presented the results (first permafrost extend and then MAGT) also in the discussion. You mix up the discussion of both with no apparent reason.

page 10, line 17 You argue that in shallow boreholes in very cold climate like Central Russia and Central Siberia, boreholes with depths below five meters would yield positively biased MAGTs. That is not necessarily correct. Have you looked at the temperature profiles of those stations in detail, or are you just stating a hypothesis?

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