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Comment on esurf-2021-68

Anonymous Referee #2

Referee comment on "Probabilistic estimation of depth-resolved profiles of soil thermal diffusivity from temperature time series" by Carlotta Brunetti et al., *Earth Surf. Dynam. Discuss.*, <https://doi.org/10.5194/esurf-2021-68-RC2>, 2022

The paper « Estimation of depth-resolved profiles of soil thermal diffusivity from temperature time series and uncertainty quantification » presents a modeling study of near-surface thermal diffusivity of soil using the Bayesian method and validates its approach with synthetic experiments and field data collected at a site in Alaska.

General comments

The study is of high interest and the paper is very well written. Several points could be improved to help the paper gaining a broader impact and to make it clearer to a diverse audience such as one could expect with *Earth Surface Dynamics*. While the introduction shows efforts in clarifying the various modeling approaches, the authors are sometimes too straightforward on some aspects and may disconcert a part of the possible readers.

Field data and field site would deserve a more thorough description. What kind of ground it is? What is the period of measurement? Which sensors are used? What are the weather records during this period? etc. The connexion between modeling experiments and field data is quite unclear. The author thank Vladimir Romanovsky for providing data but do not explain why they chose this dataset. Details on the soil characteristics, lithology, climate characteristics, the data collection approach, the choice of the dataset, the data characteristics, etc. must be provided.

The broad significance of the study is also overlooked. Same words are repeated throughout the manuscript to point out the broad significance (e.g. carbon and water fluxes) but more details or discussion points against examples would have more impact.

The interest of this study is the reproducibility of the calculation and the tools and codes

for these calculations are not provided. Please, provide more details or data for reproducing the study on other ground and if relevant provide the code. Otherwise explain why it is not provided.

One crucial point remains unclear to me: the field data are collected in a permafrost ground but the modeling approach is not appropriate for permafrost modeling: the latent heat is ignored (see comment below) and freezing processes are not discussed. Furthermore results show positive temperature. These limitations raise a major concern about the modeling approach that encourages to reconsider the paper after major revisions if the authors can not clarify their approach.

Introduction : The introduction is very well written and provides an interesting overview of existing modeling approach.

L 89-90 : What does « numerous » means? What does « landscape » means when it is written later that all data are collected very close to each other in the same type of ground? The data are obviously not collected at a « landscape scale » but at a sub-hectometric or similar scale in homogeneous terrain. This remark points out the need to introduce the field site in more details.

Similarly, about questions (1) and (3), I do not understand how the study tackles the question of « different environmental conditions » or « different locations across the landscape » by focusing on a single site, even though several dataset are collected at this site.

Theory and method

The approach contains many steps. I would suggest that the authors add a diagram outlining the different steps and how they interact to make it clearer (section 2 and 3).

L 110-115: are the latent heat exchanges ignored in the modeling approach? It is a crucial process in permafrost ground and it has to be accounted for!

Section 2.4.2: how is the thermal diffusivity assumed? Based on which knowledge?

Results

I think that the results of the soil samples analysis outlined in section 2.4.4 should be clearly described such as the modeling results. This would also help to link modeling experiments and field data.

Discussion

It would be easier to follow with subsections. It seems that many paragraphs repeat each other and that the ideas are not well ordered.

Conclusion

It must be more specific with clear statements about the findings and broad significance of the results. In the current state it rather looks like paraphrasing of general statements provided in previous sections.

I am still not convinced about the use of wording such as « various locations » knowing that all data come from the same site.

Detailed comments

L 96: why is this 10% or 5% threshold relevant? Maybe providing examples of possible applications with such thresholds would be relevant.

L 137-138: on which basis these diffusivity values were determined?

L 142: some data are mentioned but they should be introduced earlier in the paper.

L 198: what reference/manufacturer are those sensors from?

L 207-208: on which basis are these values determined?

Fig. 1: what is the « true » thermal diffusivity ? The one calculated from field samples

(Sect. 2.4.4)?

L 333: what are these visual observations?