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The Cryosphere Discussions



# *Interactive comment on* "Transfer function models to quantify the delay between air and ground temperatures in thawed active layers" *by* E. Zenklusen Mutter et al.

**Anonymous Referee #1**

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Review for the manuscript: Transfer function models to quantify the delay between air and ground temperatures in thawed active layers by Zenklusen Mutter et al. (The Cryosphere Discussion)

Zenklusen et al. present a purely statistical approach to analyse the delay between air and ground temperatures in the active layer of several mountain permafrost boreholes in Switzerland. Even though the approach does seem interesting and potentially applicable to borehole sites in permafrost areas, it has to be shown what advantages this method has in comparison to simple regression and/or time-lag analysis as well as to well-published results from studies analysing the thermal properties of the different



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subsurface materials (diffusivity and the related conductivity/heat capacity).

Apart from detailed comments listed below I have the following general remarks on the paper:

1. What exactly are the aims of the study: to show the general applicability of a advanced statistical method or to analyse processes in the active layer of mountain permafrost ? In its present form, the paper adresses neither the former nor the latter.

2. why do you use a purely statistical approach and not an approach coupled to an analysis of the physical processes responsible for the different response times?

3. why do you analyse only one summer in detail, if data from other years are available ? If you use statistics without relation to physical processes, in my opinion the need for large data set is even higher, otherwise your results can be meaningless. Why did you for example not evaluate mean values for 2003-2009 ?

4. The Methods and Results chapter have to be split. Within the method section you have to subdivide between the theory, the validation of the method and then the steps for application to the real data. Do not mix these three parts! As far as I understood the method is not new, but has not been applied to permafrost time series before: this you have to point out, but then also validate the approach, especially if you apply it afterwards without any relation to the processes involved.

5. The method section is in many places unclear without explanation of abbreviations, the reason for applying specific features or showing specific figures and is lacking a discussion of its advantages/disadvantages to standard statistical and physically-based methods.

Detailed Comments:

Abstract

line 5: Why did you apply statistical transfer function models to the difference time



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series of air and ground temperature ? Why between air and 0.5m ground ? What is the aim of this ?

line 16-18: Here, the relation to the physical processes responsible for the different response times is missing - if it is a purely statistical result, nothing is learned

Introduction

p. 2936 - line 26: The reference (Burn) is not needed here and can be omitted, as the active layer is a standard expression in permafrost research

p.2937 - line 4: ...in mountainous terrain MAY lead to mass movements...?

p.2936 - line 5/line 18: use first 2004a then 2004b.

p.2936 - line 8: "Various approaches": if you cite them afterwards, you should at least mention the approaches themselves and not only write "various".

p.2938 - line 3-17: I do not understand the logical order of these two last paragraphs in the introduction: The first paragraph addresses different physically-based model approaches and the corresponding difficulties (but mixed with difficulties due to the scarcity of boreholes), the second leads over to the statistical approach of the study presented. Does that imply that the statistical approach is meant as alternative to the other approaches, which overcomes the difficulties mentioned above or is it just another way of approaching the subject ? It has to become clear from the introduction, why you did chose this method for the analysis of air-soil temperature relationships

p.2938 - line 5: they are not really "soils" in the standard meaning of organic soils.

p.2938 - line 13: Why only for one year ? for a statistical method this is surely not enough ?

p.2938 - line 17: I do not understand the reasoning for using only one summer: if the statistical relationship (or the differences between them for the individual sites) for 2006 is meaningful, then it must be similar for other years. So why not making the analysis



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for all available years ?

Statistical Methods and Results

I would propose to divide statistical methods and results in two separate sections! By this, it will be much easier to follow your method and assess the implications of your results.

p. 2939 - line 26: not clear: which processes ? and what is differenced ?

p. 2940 - line 1: where are the results of these tests (KPSS) shown or how can I see that ?

p.2940 - line 9: define your data sets more clearly: what is the air temperature difference series ? it becomes clear from the context, but it would be much easier to read if you define it in the beginning.

p.2941 - line 4: use a new paragraph and a sub-heading at this place

p.2941 - line 5: better: "Almon lag model (Almon 1965, see chapter 15 of Judge et al. (1985) for a review)."

p.2941 - line 15: how do you know it is stationary ? This is quite an assumption, especially if water flows are an issue after snow melt (snow patch in Figure 3). If you ASSUME it to be stationary, then you should say so.

p.2941 - line 17: the ARMA model (Autoregressive–moving-average model) is not known to all readers. This you have to introduce in a real methods section!

p.2941 - line 25: "standard techniques": for example ?

p.2942 - line 16: but are these different lags not due to the different surface/subsurface conditions ? Then you could also analyse and interpret them ? Would this analysis show already the same result than with your approach using the regression coefficients ?



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p.2942 - line 22/23: This I do not understand: these results you mentioned already in line 15-16 and there the maximum m2 was 6 ? Why using 8 now ?

p.2943 - line 6: "general" instead of "automatic"

p.2943 - line 11: not comprehensible for the general reader! Mention that R here is the name of a free software package and highlight "arima" somehow so that it is clear that it is a function. Better still: explain what it is all about!

p. 2943 - line 12: "It uses..." - WHAT uses ?

p. 2943 - line 14: explain AIC !

p. 2944 - line 1: "a so-called MA(1)" or explain !

p. 2944 - line 11/12 and rest of the section: up to here you explained the method and justified the approach. Can Figure 8 be seen as validation of the approach ? Then say so. Within this method section you have to subdivide between the theory, the validation of it and then the steps for application to the real data. Do not mix these three parts!

p.2944 - line 21: What is the difference or advantage of your approach to standard regression, as it is usually applied for data gaps ? Could you show a comparison of the two methods to show the improvement gained by your approach ?

p. 2944 - line 28: "The procedure described above...": Here begins finally your "Results" section!

p. 2945 - line 9/11: For a very coarse, blocky surface cover, and an analysed depth of 0.5m you have to specify exactly in which material the borehole was drilled. If you have blocks of the size of e.g. 1m and you drill a borehole in one of these blocks, then the sensor at 0.5m depth shows a thermal behaviour like in bedrock, i.e. high thermal conduction and fast propagation of the surface signal. If you analyse the sensors at larger depth, the thermally isolating effect of the air between the blocks will affect the measurements and the net thermnal conductivity will be low.



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p. 2945 - line 12: "delay is around two days": due to the bedrock again, but maybe higher porosity/weathering degree ?

p.2945 - line 16: see comment above: at 0.5m depth fine material contains a higher air content/larger porosity.

p. 2945 - line 21: up to now you have not written/discussed anything about diffusivity, conductivity and heat capacity. From that perspective "less diffuse" is quite unclear. On the other hand I strongly recommend to analyse your data also in terms of the above physical variables, especially as there are many papers on the subjects also for permafrost materials. Why do you use statistics for a topic which can be explained also by well-known physical relationships ?

p. 2945 - line 26: This cannot be seen from Figs 9 and 10 if you show only the envelopes. The shape of the distribution could be different in different years! You should show the distribution of the regression coefficients for each year separately using e.g. coloured lines within the same plot. Why did you not evaluate mean values for 2003-2009 ?

p. 2946 - line 11 and paragraph above: Again: what is the aim of your study ? Clearly not using the statistical approach to identify the periods of summer snow events - there are better ways of doing that! If you are interested in the differences in subsurface conditions you would have to find a way of eliminating the influence of snow events on your analysis!

Discussion and outlook

p. 2946 - line 21: It's not the blocky surface which leads to faster response time, it is the rock of the individual block itself where the borehole is drilled within! Blocky surfaces as a whole should rather insulate the ground much better than fine-grained surfaces in summer! (on the other hand cold temperatures propagate very quickly also through air-filled clefts and voids)



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p. 2946 - line 25/28: "The study confirms...": Is this confirmation not also possible with a simple regression and time lag analysis (auto-correlation with different time lags)?

p. 2947 - line 3/5: "Furthermore, the derivation of physical parameters...": definitely! But you have at least to address the comparison of your results with standard values of diffusivity for permafrost materials already in this study.

Figures and Tables:

p. 2955, Table 4: This table should be combined with information about the material composition of the surface and subsurface for easier comparison and interpretation

p. 2958: Figure 3: I would prefer to give the name of the station(s) in the caption and use the abbreviation A2 as header in the figure itself

p. 2959-60 Figure 4/Figure 5: you have to explain this figure (and Figure 5) in more detail in the text. What is the reason and relevance of showing both, autocorrelation and partial autocorrelation here ? What results do you draw from that figure, except that the two series are autocorrelated for some lags ? You should also point out the reason for the different autocorrelation result between air and ground temperatures

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Interactive comment on The Cryosphere Discuss., 5, 2935, 2011.