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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.

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Authors comments (AC) to referee #1:

AC: The authors would like to thank referee #1 for the constructive and useful comments on our article. We went through all comments carefully. The most important changes include the reorganization and extension of the introduction and the methods

section. In the introduction a stronger motivation for the application of the statistical model approach has been given. The rearrangement of the method section was executed as proposed by referee #1. Therefore the former section 3 entitled "Statistical methods and results" has been split into three separate sections presenting first the method (new section 3), then the model checking (new section 4) and finally the results (new section 5). The changes suggested by referee #1 have mostly been carried out and are discussed below. The authors hope that the answers meet the expectations.

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.

AC: The aim was to investigate the delay between air and ground temperatures in active layers of alpine permafrost. As there is only temperature data available, a statistical model demanding only these as input parameters was most suited for this analysis. The detailed goal is described on p. 2938 lines 7-11.

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?

AC: Physically-based process models demand input parameters giving detailed information on the ground (e.g. ice content, lithology). We only have minimal geological and geotechnical information and mainly temperature measurements (air and ground temperatures) at the analysed sites. This is why a statistical approach only demanding air and ground temperature measurements was so useful here. The following text passages commenting on this have been added: 1) to the abstract: "The peculiarity of our model is that it is purely statistical and does not require knowledge of the ground physics. It can thus be used as an alternative to process-based models if ground properties are unknown or processes are difficult to model." 2) to the introduction: "The application of such physically-, respectively process-based models, however, often requires information on the ground properties (e.g. knowledge about the ground lithology, its ice/water content or its thermophysical properties). If such information is not available, as in our case, assumptions have to be made, reducing the objectivity of the approach. Although statistical model approaches cannot replace process-based ones, they are a powerful complement (e.g. Blanchet and Davison, 2012). This is particularly the case when the physical processes are ill-understood or contain unknown input parameters."

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 ?

AC: We agree and therefore gave higher importance to the analysis of the other years. The grey area in figures 9 and 10 was replaced by the individual lines of the years 2006-2009 (we do not have data before for all sites). Furthermore coefficients estimated from mean temperatures of the period 2006-2009 were added. Also Table 3 was extended by the results for the other years.

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.

AC: We agree. Therefore the method section entitled “3) Statistical methods and results” has been split into three separate sections presenting first the method (new section 3), then the model checking (new section 4) and finally the results (new section 5). Furthermore a sentence commenting a previous study using transfer function mod-

elling to describe the relation between air and ground temperatures has been added to the introduction as follows: "The application of transfer function models to describe the relation between air and ground temperatures is not new (e.g. Lai, 1979), however, no previous study dealing with permafrost is known to the authors."

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.

AC: The method section has been split into three separate parts. In the new section 3, the transfer function model is introduced, the steps of its application to the data presented and the figures much more described in detail than before.

DETAILED COMMENTS:

*** ABSTRACT ***

line 5: Why did you apply statistical transfer function models to the difference time series of air and ground temperature ? Why between air and 0.5m ground ? What is the aim of this ?

AC: Point 1: We applied it to the difference series because the original time series were not stationary. A sentence commenting on this and a formula describing their calculation was introduced in the method section (new section 3): see answer to comment p. 2939 - line 26" in "Statistical Methods and Results" below. Point 2: To compare the results for different sites we had to choose a common depth which was in the thawed active layer over the whole summer, which was fulfilled by 0.5 m depth. The following sentence and a comment referring to site A3 where ground temperatures had to be interpolated as no thermistor at 0.5 m depth is available was introduced at the end of the data section (section 2): "As the active layer depth varies over time and space, a common depth of 0.5 m which was in the thawed active layer throughout the summer, was selected for all sites. For site A3 no thermistor is installed at 0.5 m, therefore ground temperatures had to be interpolated from the thermistor above (at 0.4 m) and below (0.6 m). This interpolation was done linearly and by considering the time lag between the measured temperatures above and below (Zenklusen Mutter and Phillips, in review). For the other six boreholes temperatures at 0.5 m are measured."

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

AC: The different response times are due to the different surface properties. The sentence in the abstract at line 16-18 was complemented as follows: "The fastest response times are found for a very coarse grained, blocky rock glacier whereas slower response times are found for blocky scree slopes with smaller grain sizes, confirming the distinct influence of different ground properties on the thermal regime of the active layer."

*** 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

AC: We agree, “Burn (1998)” has been deleted.

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

AC: We agree, “may” has been inserted.

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

AC: The ordering is according to the reference list, therefore not changed.

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

AC: Ok, the following passage describing the approaches has been introduced into the introduction: "Various approaches have been applied to investigate the relation be-

tween air and ground temperature. Lunardini (1978) calculated a so-called n-factor, the ratio between surface freezing (or thawing) index and air freezing or (thawing) index to describe the relation between air and surface temperature. The n-factor has commonly been used in engineering studies as well as in permafrost research. For example Harlan and Nixon (1978) used it to describe the influence of the accumulated thawing temperatures, soil properties and moisture content on active layer thickness. This approach was later applied in different studies, in both high latitude and high altitude permafrost regions (e.g. Brown et al., 2000; Hinkel and Nelson, 2003; Christiansen, 2004; Mazhitova et al., 2004; Åkerman and Johansson, 2008, Smith et al., 2009; Zenklusen Mutter and Phillips, in review). Another study by Beltrami (1996) used air-soil phase-space diagrams (so-called thermal orbits) to assess the relation between air and ground temperatures and deduced from their shape the kind of heat transfer occurring in the active layer. This approach was further developed and quantified by Smerdon et al. (2009)."

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

AC: The introduction has been extended to motivate the statistical approach better (see answer to comment 2 of the general remarks).

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

AC: The sentence has been dropped out due to the changes made in the introduction.

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

AC: The analysis has been done for the four summers from 2006-2009, however, first the discussion was mostly focused on summer 2006. This was changed and the focus enlarged on discussing all the summers of the analysed period.

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 for all available years ?

AC: Wee agree (see previous comment).

*** STATISITICAL 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.

AC: We agree. As already mentioned before, the former section 3 has been split into three separate sections presenting first the method (new section 3), then the model checking (new section 4) and finally the results (new section 5).

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

AC: “Processes” denotes the first difference time series here (see sentence line 26, p. 2939). However, for the reader’s benefit the passage has been extended and a formula describing the calculation of the difference series has been added: "As the original daily time series were not stationary, the first differences of both temperature series have been used for the modelling: $X(t)=X'(t) - X'(t-1)$, (1a) and $Y(t)=Y'(t) - Y'(t-1)$, (1b). In Equations (1a) and (1b), index t labels the time (i.e., days here) ranging from 1 to n. $X'(t)$ is the air temperature at day t and $Y'(t)$ the ground temperature at the same day. $X(t)$ and $Y(t)$ respectively represent the air temperature difference and ground temperature difference between day t and t-1. Thus we attempt to statistically model

how air temperature difference $X(t)$ influences ground temperature difference $Y(t)$. The idea behind this is to explain to what extent and with which delay, for example, a 10 $^{\circ}$C change in air temperature from one day to another is represented in the ground temperature in summer. The two processes resulting from differencing in (1) have zero mean (Fig. 4 and Fig. 5, top graphs) and are stationary."

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

AC: The results of the KPSS test as well as the Phillips Perron test were not visible before and have now been introduced as follows: "Indeed, the application of the Phillips-Perron test for A2 reveals p-values of 0.01 for both $X(t)$ and $Y(t)$, rejecting the null hypothesis that the tested time series have a unit root and keeping the alternative of being stationary (Phillips and Perron, 1988). The stationarity of the time series is confirmed by the Kwiatkowski-Phillips-Schmidt-Shin (KPSS) test (Kwiatkowski, et al., 1992) (p-values of 0.1 for both $X(t)$, and $Y(t)$, keeping the null hypothesis that the tested time series are stationary)."

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.

AC: Done as suggested. The whole passage has been extended and a formula describing the calculation of the difference series has been added (see previous comments).

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

AC: Ok, done as suggested.

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

AC: We agree, changed as suggested.

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.

AC: The stationarity of the first difference series has been checked with the Phillips Perron test and the KPSS test (see p. 2939, line 27 ff. and comment above).

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!

AC: It's not possible to describe the whole theory of the ARMA model here. However the authors introduced the following passage: "... These imply that serial correlation is also expected for the error term Z(t). To model this, we assumed in the regression model (3) that Z(t) follows some autoregressive moving average model, a so-called ARMA(p,q). By considering different values of p and q , this allows us to take into account different structures of serial correlation into the stationary time series Z(t) (see e.g. chapter 4.4 in Cryer and Chan, 2010)."

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

AC: The AIC criterion. An explanation and the definition has been introduced: "We used the penalized likelihood criterion AIC (Akaike, 1973): AIC = 2r - log(L), Eq.(4). Equation (4) shows the calculation of Akaike's Information Criterion (AIC) with r denoting the number of estimated parameters and L the maximum likelihood value for the ARMA model. The selected model is the one with the lowest AIC value, as it best explains the data (high L) with a minimum of free parameters (low r)."

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 ?

AC: No, not completely. These lags are only the significant cross correlations between

the transformed processes. They only give an idea about the amount of the lags responsible for the temporal relation between air and ground temperature. However, they cannot be used yet to quantify this relation.

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 ?

AC: We decided to use m2 = 8 to be sure to include all significant estimates. The sentence has been extended as follows: "Following the cross-correlation plots of the prewhitened data from all sites (not shown) and to be sure to include all significant coefficients, we decided to use m1 = 0 and m2 = 8."

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

AC: We don't agree, as different models are fitted and the one with the lowest AIC is selected automatically and not generally. The word "automatic" has been kept.

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!

AC: The sentence at line 11 has been linked to the consecutive one and the part dealing with the R function arima() has been skipped, as there are other software packages which could be used too. It now simply looks as follows: "All these models were fitted using the well-known Cochrane-Orcutt procedure (Cochrane and Orcutt, 1949) for fitting linear models with correlated errors." Additionally the matrix formulation (former Equation (3)) of the transfer function model has been deleted as this was only a trick to use the R-function arima().

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

AC: "It" referred to the model fitting procedure mentioned in the sentence before. It has been clarified as the sentence was joined to the one before (see previous comment).

p. 2943 - line 14: explain AIC !

AC: Yes, an explanation of AIC and the corresponding formula has been introduced (see answer to comment concerning p. 2941 - line 25).

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

AC: Extended as followed: "For A2 and summer 2006 an ARMA(0,1) (i.e. a moving average MA(1)) model has been selected for the residuals Zt (Table 3)."

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!

AC: We agree. Fig. 8 can be seen as a model checking plot and is now therefore also explained in the new section entitled with “Model checking”.

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 ?

AC: We compared the result from one-day ahead prediction with the one from simple linear interpolation. A revised figure 8 and the following sentences comparing the fitted values of the one-day ahead prediction and the linear interpolation to the measured ones has been added: "The comparison of these one-day ahead predicted with measured ground temperatures and fitted values resulting from simple linear interpolation usually applied to fill data gaps confirms the good fit of the transfer function model (Fig. 8, lower graph). Overall the performance of the linear interpolation and the one-day ahead prediction is similar for site A2 and summer 2006. However, the one-day-ahead prediction does not average short-term maximum and minimum peaks and therefore captures the extreme values and the rougher parts of the time series better. For smoother parts of the time series the difference between linearly interpolated and

one-day ahead predicted values is smaller."

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

AC: We started the results section a bit earlier, at p. 2944 line 23.

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.

AC: The sentence at p.2945, line 9/11 has been changed into: “This fast and efficient response at A3 can be explained by the large size of the blocks in which the sensors register similar temperature conditions as in a rock wall, with an efficient relation between air and ground temperature changes (Fig. 11c)."

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

AC: We have no information about the drilling for this site and therefore no statements on porosity and weathering degree are possible here.

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

AC: We completely agree and it’s also what is written (p.2945, line 14-16).

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 ?

AC: Point 1: “less diffuse” does not refer to diffusivity here, it’s related to the speed of reaction indicated by the coefficient distribution up to higher lags. Point 2: We know that the relation of the estimated model coefficients to physical parameters would be very important and we plan to work on this in future. At this stage, however, it makes no sense to compare the estimated model coefficients with any tabled standard values, most of all as we don’t have enough information on the ground properties at the sites.

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 ?

AC: Figures 9 and 10 have been replaced according to the suggestions.

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!

AC: It was never the goal to identify summer snow events. The detailed goal of the study has been presented in the introduction (see p. 2938, lines 7-11). However, for the reader’s benefit the goal has been picked up at the beginning of the conclusions section: "The goal was to quantitatively estimate and qualitatively describe and compare the relation between air and ground temperature at the different permafrost sites."

*** 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)

AC: We agree. The sentence has been extended as follows: "Very coarse-grained, blocky ground surfaces lead to faster response times in the near-surface regions of the blocks."

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)?

AC: The sentence has been rephrased as follows: "The study confirms the well-known influence of ground characteristics and snow cover on the thermal regime of the active layer and allows the quantitative and qualitative description of the determining lags between air and ground temperature changes in different types of terrain." Additionally the advantage of the transfer function model compared to the common lag-regression model has been introduced into the method section p.2940, line 19 as follows: "It is known variously as the transfer function model, the finite distributed lag model, or the dynamic regression model. This is a generalization of the lag-regression model taking into account several lags. Compared to the common lag-regression model which fits the output series as a function of the input shifted by one certain lag, a transfer function model uses the input series at different lags to model the output. By Thus, not only one estimated coefficient results (as in the case of the common lag-regression model) but all coefficients which characterize the relation between input and output enabling the interpretation of the strength, speed and duration of the relation."

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.

AC: We agree that to be able to derivate physical parameters from the estimated model coefficients would be very important and make this approach very useful and we plan to work on this in future. At this stage, however, it doesn't really make sense to compare the coefficients with any standard values as we don't have enough information on the ground properties at the sites.

*** 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

AC: We agree, done as recommended.

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

AC: Ok, done as suggested.

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

AC: We agree. The the text passage discussing these figures has been extended as follows: "The input X(t) and the output Y(t) are autocorrelated. To recognize this, the autocorrelation function (ACF) and partial autocorrelation function (PACF) of the two series (see e.g. chapter 6 in Cryer and Chan, 2010) are depicted on Figures 4 and 5 (bottom graphs). The dashed horizontal lines at $\pm 2/\mathrm{sqrt}(n)$ indicate the critical values for testing at the 95% level, whether or not the autocorrelation and partial autocorrelation coefficients are significantly different from those of a white noise series. Vertical bars going beyond the horizontal lines indicate a significant autocorrelation (or partial auocorrelation) of the series at the corresponding lag. For the difference series X(t)

and Y(t) at A2, significant autocorrelations and partial autocorrelations are visible at various lags, confirming the serial correlation of the underlying time series."

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