

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 #2:

AC: The authors would like to thank referee #2 for the valuable comments on our article. We looked at them carefully. The performed revisions and our comments are listed below. The authors hope that the answers meet the expectations. Note that the introduction and the methods section have been reorganized according to the suggestions of referee #1.

REFeree COMMENTS (RC) AND AUTHORS' ANSWERS (AC):

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RC: This paper provides a statistical analysis of a relatively small data set, and a qualitative interpretation of the results in terms of underlying physical processes. I find it difficult to evaluate this paper on its own terms, given that physically based approaches are usual for this type of study. In its current form, the bulk of the paper concentrates step by step on the application of the statistical method. I think the analysis of results needs to be strengthened before this paper can be accepted.

AC: Point 1: The paper presents the application of a purely statistical approach. The motivation behind this is the fact that physically-based process models require 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 requiring air and ground temperature measurements was so useful here. The following text passage commenting on this has been added 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." Point 2: According to the suggestions of referee #1 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). This and the fact that the interpretation now focuses more on all summers 2006-2009 and less on the single summer 2006, has strengthened the results.

RC: We know that this is a system that is controlled by seasonal, synoptic, and daily cycles, with diffusion mediated by spatially/temporally variable heat and mass transfer.

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Given that the authors suggest a number of analyses as a next step: (quantitative physical analysis, a study of the depth-dependence of time lags), I think it would be appropriate to include a preliminary physical analysis as part of this paper's discussion, to allow for comparison of physical and statistical parameters.

AC: We agree that the relation of the estimated model coefficients to physical parameters would be very important and valuable. 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.

RC: As a first step, the data have been reduced to daily means. (P 2939, lines 18-21: It is unclear that the authors mean by "these discrepancies have not been taken into consideration". Are the data included in the analysis?) As a result, a daily time step is the smallest increment for analysis, which is somewhat crude for a physical analysis at these time/distance scales. What was the frequency of measurement for the ground temperatures? Are the daily mean air temperatures based on hourly values or on daily maxima/minima? Only daily mean data are presented, so that it is difficult to judge whether any diurnal temperature signal remains in the ground temperature data. I would expect that a diurnal signal is apparent at sites with lags of 1-2 days. Analysis might also be possible with a running 24-hour-average time series based on the measurement frequency, allowing for a more precise estimate of the lag time in the ground.

AC: Point 1: Except for one site, where hourly data are available, the data resolution is daily (based on original hourly measurements, stored as daily means). Intra-site comparisons could therefore only be done on a daily basis. Point 2: Yes, the data from the 23 days when snow was present were included in the analysis. Unfortunately we don't have precipitation measurements at the sites and the ultra sonic measurements of snow depth do not work well for thin snow covers on rough ground surfaces. We therefore don't have detailed information on summer snow events. The time series at

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the other sites, however, do not show such effects. The following sentence commenting on this has been inserted at the beginning of the methods section (section 3): "The courses of air and ground temperature show clear similarities (Fig. 3). However, there is a period of 23 days in the middle of the summer 2006 during which discrepancies are visible. These are due to the presence of snow which attenuates the relation between both parameters. No such discrepancies are visible for the other sites in 2006 (Fig. 2). For simplicity, these 23 days with snow at A2 will not be treated differently than the remaining snow-free days."

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

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