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The Cryosphere
Discussions

Interactive comment on "Modeling the thermal dynamics of the active layer at two contrasting permafrost sites" *by* J. Weismüller et al.

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We gratefully acknowledge the thoughtful comments of both reviewers, which helped a lot to improve the manuscript. In the following, we will quote the reviewers comments in italic and give our answers in normal font.

Referee R. Daanen

1) *I would have liked to see more detail on why the model did not simulate the Tianshuihai site for vapor and convective flow, it seems to me that they could have forced the model boundary with a flux rather than the observed moisture content.*
Added a corresponding paragraph in section 5.3.

2) *Also there is no discussion point on the estimation of the freezing characteristic and its effects on the summer soil temperatures. It seems to me that too cold simulated conditions in summer would be a logical effect of having too much ice in the profile, or the temperature gradient while thawing is not steep enough.*
Added a new section 6.3.1 that addresses the influence of the freezing characteristic.

Because thawing is a much slower process than the equilibration of temperatures between the surface and the thawing front, the temperature gradient in spring is determined by the temperature difference between the surface and the $0°C$ isotherm at the thawing front. So if the thaw depth is too shallow in the simulation, we expect a temperature gradient that is too high rather than too low.

3) *P238 L4 The gas content of the medium should also depend on the ice content.*
We defined θ_w as the sum of liquid and frozen water content, but forgot to include this definition in the list of symbols. Has been added.

4) *P240 L16 Explain how the energy and mass balances where check using the rain and net radiation*
Added an explanation in paragraph 3.4.

5) *P243 L6-13 forcing the lower boundary also hides problems with parameterization in the upper profile.*
We agree that forcing the lower boundaries with Dirichlet type boundary conditions imposes a strong constraint that may force the computational domain from below, which is not in agreement with reality. Other common choices for the lower boundary involve adding an extra computational domain below the domain of interest or forcing with fluxes. We have experimented with both approaches, but the problem of missing information about the physical reality has turned out to allow for much larger ranges

of parameters than with our choice. In practical terms, the initialization of the deeper domain together with missing bore hole data as well as the rather arbitrary possible choices for the boundary fluxes have shown to introduce quite some arbitrariness. However, we are in the lucky position that we have high resolution measurements available at the lower boundary, which allows us to impose physically correct initial and boundary conditions. As mentioned in the paper, this approach gives the opportunity to adjust the parameterizations very carefully.

6) *P244 L23 and L26 twice the same information.*
Corrected.

7) *P245 L14 the 100% is not shown in figure 9 did you test this sepperately?*
Figure 9 shows the average energy flux over the whole profile. Heat flux in different depths was tested separately, with the very high peaks occurring only in the upper centimeters.

8) *P245 L18 why did you not simulate vapor if only convection is a problem? Vapor is likely very interesting in this dessert site.*
We agree that studying vapor transport would be very interesting at the Tianshuihai site. However, our formulation for vapor transport includes the migration of water through the soil, a process that is expressed by the modified Richards equation. As the inclusion of Richards' equation leads to a failure of the boundaries (see item (1)), vapor at this site cannot be modeled with our approach.

9) *P246 L1-14 Why did you not use the gradient in moisture content and temperature to simulate this with a boundary flux?*
This addresses the same issue as item (1). As described there, we added a corresponding paragraph in section 5.3.

10) *P246 L28 What is a reasonable value? It would be good to show a figure of the thermal conductivity properties in the model domain this is very easy in COMSOL. The values for Khsoil seem rather high.*
Merged the former section 5.3 into the discussion in section 6.2 and added some explanations.

We agree that some of the values of Khsoil are rather high. However, as described in the new section 6.2, we attribute this to the fact that the parameter is a conglomerate of geometry and the conductivities of the soil constituents.

The plot is shown in Fig. 1, where the influence of the three different values of Khsoil in the three layers on the resulting Kh can be seen very clearly. Also, we can see that in the unfrozen regime the values are somewhat lower than in the frozen soil, which is to be expected due to the higher conductivity of ice compared to water. However, for several reasons we would like to refrain from showing this figure in the paper: First, although the values seem obvious, the figure does not represent the results in a direct way, but a complex mixture between input parameters and actual results, which in our opinion is not intuitively clear. Basically, it is an image of the de Vries model, to which the parameter Khsoil in the different layers, the obtained water and ice contents and the geometry of the soil contribute. Also, Kh is not the parameter that dominates heat conduction, but the effective diffusivity Dh is, into which the heat capacity and the latent heat contribute in addition to Kh. However, a plot of Dh would be completely dominated by the latent heat contributions near the zero degree line.

11) *P248 L7-17 non-diffusive fluxes in the soil profile are included, just not in macro pores.*
We agree. Added some words for clarification. See also items (2) and (13).

12) *Freezing characteristic curve misfits are not discussed.*

See item (2)

13) *Snow melt infiltration in macro pores is not discussed.*
Added a new section 6.3.2

14) *P249 L6-7 evaporation does not make the soil warmer, but it does make the model cooler. You should be able to check on this flux in the model.*
This is correct, we should should have mentioned condensation instead of evaporation. As this process is not dominating water flow during rain events, we removed the corresponding parts of the text.

15) *P249 L15 Air convection does not transport heat down, only forced air convection can do this, like wind blowing over the surface causing a pressure gradient.*
This should have read "vapor convection" instead of "air convection". Corrected.

16) *P250 this is a rather long description for something that is not likely to be real*
Shortened this paragraph.

17) *P251 The mechanical aspect should deviate the temperature is winter not in summer, why discuss this?*
It is correct that the mechanical aspect should deviate the temperature in winter, and not in summer. However, as discussed in the manuscript, we adjust the parameters such that the temperatures in winter are represented accurately, such that we will underestimate the thermal diffusivity and thus also the heat conductivity of the soil $K_{\mathrm{h}_{\mathrm{soil}}}$ in summer, because the distances in the simulated soil are smaller than in reality.

18) *P253 L2 remove 'that could not be reproduced by the model' this is double, because you mentioned deviation.*

Done.

Anonymous referee

19) *In the definition of the hydraulic conductivity, it is normally used an impedance factor to account for the blocking effect of ice (i.e Hannson et al., 2004). If the impedance factor is not defined, the hydraulic conductivity may be overestimated. The authors should discuss this point.*
We agree that our model may overestimate hydraulic flow in the frozen regime, especially when temperatures are only slightly below freezing. As we describe in the manuscript in section 5, we identified one event at the Bayelva site in fall 1999, where too much rain infiltrated into the already frozen ground in the model compared to the data. However, including an impedance factor as used e.g. by Hannson et al. (2004) would introduce an additional free parameter. By calibrating this parameter, we could most likely remove this infiltration event. But as there are no other events where the parameter would have a significant influence, we could not verify it, so that we would give up some of the simplicity of the model without learning anything about the physical reality. We added section 6.1 which addresses this issue.

20) *I have some doubts if it is appropriate the assumptions that liquid water and ice are at the same pressure. In reality unfrozen water is at a negative pressure (this for example comes from the application of the generalized Clapeyron equation, see Christoffersen and Tulaczyk, JOURNAL OF GEOPHYSICAL RESEARCH, VOL. 108, NO. B4, 2222, doi:10.1029/2002JB001935, 2003)*
We agree that this is a rather crude assumption which might not be justifyable when one is interested in the hydraulics. However, to assess the thermal behaviour of the soil we believe that our simple model is sufficient.

The new section 6.1 also addresses this issue.

21) *I think that a few more explications on the numerical method used to solve the equations are needed. It is not sufficient to cite COMSOL. Numerical methods are an important part of these kind of models, and their importance cannot be left to a citation.*
Added some text to describe the solvers used for the solution in paragraph 3.4.

22) *Pg. 242 Line 15: the sentence "The heat capacity will only influence the values we obtain for Khsoil, but not..." is not clear to me. I think that the heat capacity does affect the thermal diffusivity and, therefore, the temperature profile.*
It is correct that the heat capacity does affect the thermal diffusivity. However, in our modeling process, we first assume some heat capacity, then estimate the thermal conductivity and therefore the diffusivity. Choosing a different heat capacity would change the values we obtain for the conductivity such that we obtain the same diffusivity and therefore also the temperature profile.

23) *In figures 7 and 8 I cannot see a blue curve, the charts should be redrawn. Probably it is better to avoid the use of colors and use differently dashed lines.*
We decided for colored instead of a black and white figures because the manual of the Copernicus Publications LaTeX Macro Package (http://www.the-cryosphere.net/Copernicus_LaTeX_Package.zip) explicitly encourages the use of colors. The blue curve is hidden behind the green; we have added some words to the figure description to clarify this.

24) *Figures from 5a to 6b are quite difficult to read. Clarity could be improved choosing a few short time intervals, and comparing the modeled and measured temperature and soil moisture in the same line chart.*
Added figures 7 and 8.

25) *Fig. 4 on the right: it is inappropriate choosing a soil freezing characteristic curve independently from total water content.*
As described in formula 4, we scale the freezing curve linearly with the total water content. In section 4, where we reference the figure, we describe how we fit the curve into the saturated branch. In the figure, only the branch for $\theta_w = \phi$ is shown.
The new section 6.3.1 also addresses the issue.

26) *Some more quantitative indices should be given (not only the absolute error) to assess the goodness of the fit (between model results and observations).*
Added figures 7 and 8 as well as some explanations in section 5.

27) *The discussion of various disagreements between model results and observations should be helped by line charts that clearly evidence the disagreements you are talking about. Otherwise it is very difficult to follow.*
Inserted figures 7 and 8.

28) *The authors should discuss the importance of three-dimensional effects that are not represented with a one-dimensional simulation. In particular, lateral fluxes of liquid water may be significant.*
Added section 6.4.

Interactive comment on The Cryosphere Discuss., 5, 229, 2011.

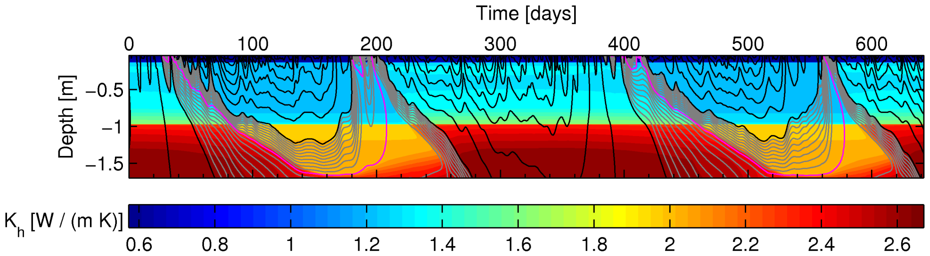

Fig. 1. Effective soil heat conductivity at the Tianshuihai site. For clarity the isotherms from the simulation as shown in Fig. 6 in the manuscript are repeated.