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Discussions

Interactive comment on "A surface model for water and energy balance in cold regions accounting for vapor diffusion" *by* Enkhbayar Dandar et al.

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As stated by the referee, we "propose a surface model to analyze the water and energy exchangein cold regions, and use it to analyze the moisture and thermal states and changes inthe upper Tuul River basin in Ulaanbaatar (Mongolia)".We thank him/her for his assessment that "the model and some in-situ datamay be helpful to the readers". He poses a number of questions:

1. There are many softwares, such as CoupModel, Hydrus and SHAW, for simulating moisture and heat changes in cold regions, and their validations were fully calibrated. Please clearly tell the reader what is the advantages of your model compared with current models?

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Discussion paper

We agree that there are many codes for simulating moisture and heat changes, although perhaps not so many in cold regions and few considering the full energy balance. All such codes, including ours, are similar. Therefore, all of them, including ours, can be considered as “partially” validated. The main difference stems from the fact that those codes only consider conductionas the heat transport mechanismwithin the soil. We include conduction, but we also take into account vapor diffusion, both in the water and energy balances. We find that the vapor diffusion flux turns out to be important in cold regions because large temperature differences should be expected within soil, especially in spring when the soil is partially frozen and vapor diffusion is the dominant mechanism to bring (latent) energy to melt the ice. An additional advantage of our modelis the use of the full energy balance, instead of the Temperature index method for calculating the soil temperature.

2. The proposed model was not fully calibrated by some in-situ data although there are lots of field data, so the reader cannot judge whether the model reflect the actual moisture and heat states and changes.

The referee is right in that we do not compare our model results to moisture or energy state in the soil. First, we do not have such data. But, second, and most important, our model is a lumped (in the vertical direction) phenomenological model. Therefore, such comparison would have been tricky (such comparison is more appropriate for distributed models). The goal of ourwork is to gain insight into the hydrological processes in cold regions. To this end, we analyze the sensitivity of the model to the values of the parameters, which implicitly allows us to assess the relative importance of the various processes in the soil. It is from this analysis that we conclude that vapor diffusion is a relevant mechanism that should not be ignored. We are preparing another work comparing model results with observation data. It must be acknowledged, though, that data are limited. Long records are only available for river discharge and snow. Thus we tested model validity by comparison to other studies in the region (Ma et al.,2003; Zhou et al., 2014)

3. There is plenty of air in the soil, however, it is neglected in the proposed model, why?

We are not quite sure we understand this comment. We do not perform an air mass balance, but we do implicitly acknowledge the presence of air. In fact, vapor diffusion occurs in the gas phase (mostly air), which we acknowledge through the term (ãĂŰ(mãĂŮ_sfˆâĹĚ-m_sf))/(m_sfˆâĹĚ). The air phase used to be considered in multiphase flow (distributed) models in the soil, but it has been shown that it was not really necessary because gas pressure is quite constant (e.g., Milly, 1984). We do not know of any lumped land surface model that incorporates air mass balance calculations.

4. There are some minerals in the soil, which makes the freezing point lower than 0 °C, but the freezing point is 0 ° in the manuscript.

Yes, the referee is right in that the freezing point for water with high salinity can be lower than 0°C. But water in our soils is extremely low salinity (below 100 mg/L). A trickier issue would be the effect of pressure differences between water and ice, which may allow the presence of liquid water well below 0°C. Again, this type of mechanism may be best acknowledged in (vertically) distributedmodels of permafrost, but it is not essential for lumped models such as ours.

5. Some parameters in the proposed model are changing with temperature and moisture content of soil, which makes the mathematical formulae nonlinear, while they are solved by the linear idea and method.

We do not agree. Our model is (highly) non-linear, as clearly shown in the sensitivity analysis. The fact that we solve it explicitly, for the most part, makes it easy to handle. But it is non-linear.

6. Although the reader can understand what the authors want to express, the English-grammar and vocabulary are needed to be polished before its publication.

We did subject our paper to edition by a native English speaking editor.

We will do it again in the final revised version of the paper, where we will also address the above comments in the spirit of this response.

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