

Interactive comment on “Simulations of water, heat, and solute transport in partially frozen soils” by Mousong Wu et al.

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REF1 comments General comments Wu et al. conduct a GLUE-type sensitivity analyses using a model of coupled heat, water, and solute transport. The study has some merit and potentially fills a gap. I will not deny that there are very few sensitivity studies of these phenomena; however, this paper is poorly presented both in terms of the technical information as well as the overall story.

Major comments 1. The English in this paper is not, in my opinion, even suitable for the first submission, let alone consideration for publication. It must be rewritten by a professional English editing company. I originally began to do this for the authors, but got exhausted by about L140.

Re: Thanks for the comments on English. We will take it seriously and ask for some

native speakers to help us in English expression. Meanwhile, we will send it to professional English editing company for further improvement.

2. Other places are grammatically correct, but incredibly vague. The authors must carefully reread through this study and make sure their sentences convey meaning. For example, 'Laboratory and field experiments on soil freezing/thawing have received more attention' (L54); what does this mean? More than what? Why is this needed? Similarly go to L36: "climate change in cold regions"? What about climate change? How does it relate to solute? Explain. This doesn't fit the sentence. There are many examples of such vague statements with little to no information (e.g., L286-287). I don't see this as an English problem, but rather as a contribution that needs to be carefully rewritten in general.

Re: We can understand this statement but we also strongly feel that the way of expressions from the reviewer is demonstrating and strange attitude to paper but using expressions such as "incredibly vague". However, we do admit that many expressions can be improved and make more precise and add references when needed to justify the statements. We will check them and make the expression much clearer for readers. For example, in L54, 'Laboratory and field experiments on soil freezing/thawing have received more and more attention', in L36, 'climate change induced nutrients loss in cold regions'. We will take this seriously and re-write vague sentences.

3. The introduction does not build a convincing story of why this contribution is needed. First of all it is too long (9 paragraphs). As an example of extraneous text, in Lines 37 to 95 the authors list a number of soil freeze-thaw field and modeling studies and where those studies were conducted. However, they really don't emphasize the contribution or key input of over half of these studies. These are described in far more detail in the review paper by Kurylyk and Watanabe (2013), and listing fewer of these and referring to this synthesis would be a more effective use of space. More importantly, the main objective (e.g. 'search for constrain' L133), makes no sense, and thus it is very hard to get excited about the rest of the paper.

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Re: Admittedly, the Introduction part is a little longer, because we wanted to give a good review of experiments and modeling work in soil freezing and thawing. However, some information was not properly conveyed here. We will re-write the Introduction part to make it more concise and related to our work. The main objective is reformulated to emphasize the important link between the solute concentrations and the explicit consideration of the dynamic impact on the freezing point depression.

4. The title does not have 'uncertainty' in it. In fact, the title could probably equally apply to about 12 of the other papers cited in this study. My point is that it should be rewritten somehow to reflect the distinct aspects of this study.

Re: In this paper, we mainly focused on the development of CoupModel by considering the influences of solutes on soil freezing point. Uncertainty analysis was used to calibrate the model and explain the model results. We use the title 'Simulations of water, heat, and solute transport in partially frozen soils' because we wanted to emphasize that in this study we modeled the transport of water, heat, as well as solute in partially frozen soils. This is the difference of this model with previous ones, because for previous models, the influences of solute on soil freezing and thawing were not taken into account carefully or neglected. We do understand that the uncertainty approach is an important aspect of the paper but not as a methodological contribution. The uncertainty is a standard tool to demonstrate to what extent the data could be used to justify the suggested new explicit of considering the dynamic simulated solutes impacts on the freezing point.

5. L168, why did the authors record TDR data at daily intervals? This seems like a low resolution given the frequency of the other data. Depending on the depth and spatiotemporal resolution, it can be very hard to calibrate or assess a model using only daily moisture data.

Re: The TDR data was only recorded in daily intervals in one study site, this is one source of uncertainty in the measurements. This was also discussed in the paper.

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That is also why we used Monte-Carlo method to calibrate the model. The model also detected the uncertainty in soil moisture modeling results. This could give us proof that in the next step of experimental design, to focus more on soil water content measurements in soil freezing/thawing period with higher spatial and temporal resolutions. However, the daily resolution is not a major problem when we consider sub-surface processes. The TDR has big difficulties to represent the very shallow soil horizons where the high temporal resolution is mostly pronounced.

6. There are issues with the only two equations I looked at very closely on a term by term basis. Equation 1: the terms in this equation do not have consistent units. Therefore something is clearly wrong. I think the second term on the right (vapor diffusion) has incorrect units and the vapor density should be expressed as a vol/vol. Equation 2: this equation is expressed incorrectly for the divergence of convection (second term on right hand side). The temperature has to be inside the derivative. It is changing with space. Also, if you leave it outside the derivative, you have the issue that it totally depends on what temperature scale you are using (Celsius vs. Kelvin) in terms of the magnitude of that term. All other terms are independent of the temperature scale, because it is only the change in temperature that matters (i.e. the other T terms are inside derivatives). Two errors in the two equation carefully considered does not give one confidence in the rest of the paper. I strongly recommend that the authors go through the equation appendix very carefully.

Re: Thanks for pointing out the units for vapor density, which should be m^3/m^3 . However, this does not mean the model has a mistake in numerical calculation. As to second term in heat transport equation, we agree that it should be inside of the derivative. We do invite all interested reader to check all equations. In addition the model is available as public domain to check additional details. Again, we will check all the equations and units to make them correctly illustrated.

7. The figures are poorly done in general. Is Figure 2 taken from the Coup manual, at least in revised form? If so, that should be stated. Figure 3 is unclear. What is energy?

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Is this sensible and latent heat? What do the different colors represent? Figure 4 is confusing. Why is the cross-hatching so similar? Are these cumulative? For example, for time period 2, the 0-100 section goes from 0 to -18 (I think), but the next one goes from -18 to about -38. What does this represent? Explain in the caption!

Re: Fig.2 was plotted by the author, according to the model theory in water, heat, and solute transport. It is original. Lines in Fig.3 will be changed in terms of color and line width. In Fig. 3, energy means total heat storage in soil. Different colors indicate different solute conditions. We will address them in detail in figure caption. In Fig.4, the cross-hatching represents cumulative value. It will be explained in the caption.

8. How does this paper differ from Wu et al. (2016) by the same authors. It is an uncertainty study using a similar sort of model it would seem. I'm not saying there are no differences, but it seems, at least superficially, to be similar. Wu, M., P.-E. Jansson, X. Tan, J. Wu, and J. Huang. 2016. Constraining parameter uncertainty in simulations of water and heat dynamics in seasonally frozen soil using limited observed data. *Water* 8(2):64, doi:10.3390/w8020064

Re: The paper published on <Water> focused on the uncertainties in parameters in calibration of a water and heat transport model. It is the first step of modeling work, which we investigated parameter uncertainties by using a static simple expression of freezing point depression that was not explicit to the dynamic variability of solutes in the soil. While this paper is focusing on the model performance in considering the dynamic variability of solute on the soil freezing processes. The current paper reflects a new theory compared to the previous paper. The similarity for these two papers is the data used are originating from the same experimental field study.

9. Oddly enough this is never cited. Also, the authors have a lazy reference list. For example, Wu et al. has been published in *Cold Reg. Sci. Technol.* For over a year now, but the authors list it as accepted. The cited Wu et al. 2016 study has no journal information, so it is not easy to look up. Again, those were the only two reference items

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that I looked at.

Re: We are sorry for this mistake. Actually, these papers were not published when writing this one, we did not check them when we submitted this paper. We will check the reference carefully to make sure they are correctly cited.

These are all very short general comments, but a more careful, rigorous review is really difficult given the present state of the manuscript. In summary, this is certainly not publishable in its current form, but I think it does have potential for publication in some journal some day given the content.

Re: Thanks for the constructive parts of your comments. We will revise this paper carefully according to the suggestions from the reviewer, and make it much easier to read without misunderstanding because of vague expressions or poor English.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-507, 2016.

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