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Interactive comment on "Three-phase numerical model for subsurface hydrology in permafrost-affected regions" *by* S. Karra et al.

Anonymous Referee #3

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General Comments

The paper deals with the use of a sophisticated model of the subsurface hydrology in cold conditions. The incipit of the paper is that large scale modelling is needed (and implicitly with high performance computing facilities) to model those large portion of Earth necessary to cope with, for instance, the problems raised by the global climate change. The paper, however, shows a one dimensional (indeed interesting) modelling, some comparison among two-dimensional models. It also shows three-dimensional modelling but, in a virtual experiment dealing with a simple slab of terrain of limited dimension. This progression of "modelling power", which could be impressive, does not bring answers to the problems that Introduction claims to address.

The main result of the paper, in my opinion, are those related to the transport of wa-

ter vapour and its role in forming growing lens of ice during the simulated seasons. However, this finding should be better described and investigated, at the light of the hypotheses made, including the possible bias introduced by the specific and arguable assumptions on the vapour-liquid-solid partition of water, made in equations 8. Another interesting result derives from the comparison of the two dimensional simulation in comparison with the results obtained in a previous work of the Authors on Mars. Unfortunately, this comparison is more mentioned that developed.

The paper itself builds on previous papers, and it is very difficult to understand what is its specific. This cannot be PFLOTRAN, cannot be the method of solution of equations (a Newton-Krylov one), cannot be initialisation strategies (which seem to me not particularly notable): all part that should be moved to an appendix without taking away novelty to the paper.

Moreover, if the equations of conservation of mass and energy are written correctly, and let understand that any process is included, it is not clear if soil deformation is also treated (see also below the comment on the vanGenucthen parameterisation): the type of writing itself, perfect English indeed, results too concise and "impervious" to any analysis the physics treated.

Therefore I believe that the paper needs at least a complete reshaping, with more focus on the real findings (it is unfocused, or wrongly focused), more discussion of the hypotheses, less clutter induced by addition of achievements obtained in previous papers and literature.

Detailed Comments

page 153 âĂŤline 27- "This paper addresses the details of process representation, and parallel implementation of . . . ": this does not seems to me to correspond to this paper content. Neither the first or the second where detailed, or possibly I and the Authors give a different meaning to the word "details". Actually, detailing the parallel implementation would not be so appropriate in TC, and better suited to a journal about numerics and/or informatics (maybe GMD).

page 154- line 2 - “Use of soil texture information in place of empirical soil freezing curve”: where ?

Equation 1a - The first divergence term contains velocities in different position. Given the commutativity of products, it does not matter. However, symmetry in writing could be better.

Equation 1b - Soil is explicitly included. However, its role is pretty passive. Understanding if the soil matrix is indeed rigid or not is crucial (for understanding if the transition to saturation is well designed).

Equation 8a-8b - They are crucial for the more relevant results obtained, and should be explained better. Besides, does neglecting non freezing-point depression have consequences on the results ?

page 158 - Equation 9 is the vanGenuchten parameterisation of the soil water retention curves. Its extension to negative suctions (positive pressures) with a constant could be source of flaws. In this case, in fact, the term containing the partial derivates with time of soil water is null, and water does not move. This term should be extended by using soil specific storage (e.g. Lu and Godt, 2012) to obtain the groundwater equation. Actually, the soil specific storage can be obtained directly from equation 1a. But if it is actually made is not clear in the paper.

page 163 -line 20 - “The simulations are designed to test whether gas . . . vapour flow”. This is the only clear research question I found in the paper related to physics.

page 165 - line 12 - I assume that all the parameters vary with water (ice, vapour) contents, according with equation 11. Therefore the parameters’ values refer to the small “k” in equation 11. Correct ? It should be clarified. Differently I would be very disappointed.

page 162 - Section 6.2 as written is non informative.

page 167 - line 9 - “However, those models have been limited to relatively small scales . . .”: In my opinion also this paper sticks with relatively small scale, so the statement is inappropriate, or requires further explanations.

page 167 -line 22 - “ However, Mars application . . . will generally require two component model.”: this is not an achievement of this paper, so it probably does not belongs to the conclusion of this paper. At least, the concept should be written differently for this context.

page 168 - line 11 - “However, the vapor diffusion model . . .. thus needed”. This statement belongs better to a “Discussion” than to a “Conclusion”.

page 168 - line 14- The statement that starts here can be eliminated. Nobody knows what “high parallel subsurface hydrology” is. This paper is not even on “parallel computing”. Maybe is “on the use of a parallel computing infrastructure for simulating subsurface hydrology”: but if this is the topic, I have to say it is not properly treated in the paper. In fact, then I would expect a comparison of performances for instance about how the computing performances by using 1,10,1000 processors, and more discussion about informatics than of freezing soil physics. I also remark, that, if it would be the case TC is not the appropriate outcome.

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