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Interactive comment on "Simulating the role of gravel on the dynamics of permafrost on the Qinghai-Tibetan Plateau" *by* S. Yi et al.

Anonymous Referee #2

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The manuscript "Simulating the role of gravel on the dynamics of permafrost on the Qinghai-Tibetan Plateau" by Yi et. al presents a sensitivity study of a modified version of CLM4 for climate forcing from Tibetan sites.

The manuscript is well written and the topic in itself is of high interest for permafrost modeling. However, I do not recommend the current manuscript for publication in The Cryosphere for the following reasons:

1. Although the new set of equations to a large part builds on previous peer-reviewed work, the employed mixing laws of the new model equations are highly unusual compared to physical laws and previous work. This concerns both the thermal and the water budget which suggests serious flaws in the model physics

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at least for some parameter combinations (see Major Comments).

2. The manuscript is a pure sensitivity study of the modified model equations. Without field validation data, it is not possible to judge whether the new model equations are an improvement beyond the state-of-the-art in describing the energy and water balance in gravelly soil (which is not impossible, despite of the potentially flawed model physics), although the results qualitatively appear sound. The authors, for instance, do not claim that the new scheme can better reproduce measured active layer thicknesses or borehole temperatures on the QTP, which would be a significant indication towards the soundness of the new model equations.

3. The ALT could in principle be compared to a measurement at the borehole. Here, the simulated ALTs of around 2m do not compare too favorably to a measured value of 3.4m? Only the case P3, S10 seems to fit (Fig. 7), which is at least in terms of the the soil stratigraphy (the slope at the borehole is not specified) consistent with the site description. But in that case, there is no permafrost (Fig. 8, page 4717,line 5) at the end of the simulations? How is the ALT defined in that case? The authors do not comment on this fact in the manuscript.

4. In summary, the manuscript presents a sensitivity study of a new model, for which the employed mixing laws are not in agreement with previous work and partly physical considerations, without benchmarking its performance against field measurements. From the manuscript, it is impossible to judge whether the modified model equations can quantitatively improve the representation of gravel in land-surface modeling.

5. I strongly agree with the concluding sentence of the abstract, that "robust relationships between soil thermal and hydraulic properties and gravel characteristics should be developed based on laboratory work" (and field data, e.g. borehole

measurements, in my opinion). The authors should do so, and evaluate the performance of their modeling scheme in such a way. I would be delighted to see the results of such thorough work published timely.

Major Comments:

- Eq. 10: In the cited previous work, the thermal conductivity of a medium with several constituents is calculated as the weighted geometric mean (compare Eq. 8), not the weighted arithmetic mean, of the single conductivities.

- Eq. 13: The same applies to the Kersten numbers. What is the physical reason for adding them as weighted arithmetic mean? As a result of Eqs. 10 and 13, there occur cross-terms, e.g. $\lambda_{dry,g} f_g K_{e,f} f_f$, in Eq. 14 which at least require explanation.

- Eq. 16: The saturated matric potentials are added as weighted geometric means. Considering the physics, potentials should be added as weighted arithmetic mean (compare Eq. 7.86, Technical notes CLM4, http://www.cesm.ucar.edu/models/cesm1.0/clm/CLM4_Tech_Note.pdf). Also, what is the factor A?

- In principle, the same applies to Eq. 15. The chosen mixing law must be at least motivated.

- The authors suggest that one of the shortcomings of the the widely employed Farouki model is that the thermal conductivity of frozen soil is always higher than for unfrozen soil. They claim that it is an improvement of the new scheme that frozen soils can have lower conductivities than unfrozen soils. This, however, is already possible in the scheme by Cote and Konrad (2005), since they propose different values of kappa (Eq. 12, this manuscript) for frozen and unfrozen soils based on field data, which leads to strongly different Kersten numbers and thus different contributions from saturated and dry conductivities for frozen and unfrozen conditions. On the other hand, their model is in many parts an empirical one, and they did not validate their scheme with field data in this regime, so that it is not entirely clear, whether their equations hold for the entire range of possible input soil properties.

The authors further claim that “several studies showed that thermal conductivities in winter were (...) smaller than those in summer on the QTP”. They specify one example (Feng et al. 2012) where the two conductivities differ only by 3%, so that they are within the uncertainty of measurement techniques for the thermal conductivity. The other study mentioned is published in chinese, and I was not able to entirely understand the methods and results, which is seriously unfortunate given the potentially important and little known results.

Considering the strong increase in the thermal conductivities from water to ice, it is in first place counterintuitive that the conductivity of a frozen soil is smaller than for unfrozen soil (under the constraint that the total water+ice content does not change). As far as I could tell, the authors do not provide a reference to a study where the decrease of thermal conductivities during freezing is documented by measurements for which accuracy margins for the measured thermal conductivities are given. Some methods employed to determine the thermal conductivity can easily feature uncertainties of 30% or more. In case of dry soils (as on the QTP), where the conductivity change during freezing is small, even seemingly significant differences between summer an winter can hence be in the range of uncertainty, thus giving a false impression of the temperature dependence. If the authors have convincing evidence that thermal conductivities decrease during soil freezing for some soils, they should present it and explicitely check that their model can reproduce this prominent feature for the soil conditions under which it occurs.

Interactive comment on The Cryosphere Discuss., 7, 4703, 2013.