The Cryosphere Discuss., 7, C2077–C2084, 2013
www.the-cryosphere-discuss.net/7/C2077/2013/

The Cryosphere Discussions

TCD

7, C2077–C2084, 2013

Interactive Comment

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

S. Yi et al.

yis@lzb.ac.cn

Received and published: 9 December 2013

Reply to Referee 2.

We would like to thank referee 2 for very helpful criticism and suggestions. We made point-by-point responses to the comments in the following part.

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.

Reply: The model we used in this study is a terrestrial ecosystem model (DOS-TEM), which has been tested over cold regions, e.g. Alaska and Arctic. The soil thermal and hydraulic properties in the "CLM" case are similar to those of CLM4.

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

Reply1: We followed Lawrence and Slater (2008) to use weighted arithmetic mean for most of the thermal and hydraulic variables and parameters, except for the saturated matric potential. The studies of Lawrence and Slater (2008) used CLM, which is a prestigious land surface model, to investigate the thermal and hydraulic properties of mixed mineral and organic soil. At present, these methods have been officially used in the 4th version of CLM, although no explanation has been provided on why the weighted arithmetic mean method should be used; and no field or laboratory data were used validate these methods. For our study, it is reasonable to use the similar method at the current stage to investigate the role of gravel, which is commonly neglected either in modeling or in laboratory studies. However, we agree with the referee that robust schemes should be developed for the mixed soil.

Lawrence, D. M. and A. G. Slater (2008), Incorporating organic soil into a global climate model , Clim. Dyn., 30, 145-160.

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.

Reply2: We did compare the simulated ALT and permafrost low boundaries with borehole measurements. And we did claim that the new scheme can perform better than the one with Farouki scheme on simulated permafrost low boundary. Jin et al. (2006) pointed out that the rate of upward degradation of permafrost is faster than downward degradation. It is impossible to simulate this phenomenon with Farouki scheme, which simulated much higher frozen thermal conductivity than unfrozen thermal conductivity.

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.

Reply3: We agree that the ALT simulated with gravel was smaller than measured. However, both the CLM (with Farouki thermal properties) and CKJ schemes simulated similar ALTs. The greatest differences among three schemes are the simulated permafrost low boundary. The scheme with gravel simulated shallower permafrost low boundary, which compared relatively well to borehole measurements. However, using the CLM scheme (with Farouki thermal properties), the simulated permafrost low boundary was much greater than measurement.

The slope of the place with borehole is gentle. We will add it into text. When permafrost disappeared, the ALT was assumed to be 4.1 m (the bottom of soil column; see Figure 7). We will add it into text.

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 physi-

cal 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.

Reply4: please see Reply1 and Reply2.

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.

Reply5: We agree with the referee that it is better to have robust schemes based on laboratory work and field data. However, it will take tremendous amount of work to fulfill the goal. It is beyond the work of a research group; it might require large scale cooperation. It is one of our main objectives to raise the awareness of the permafrost research community that gravel plays an important role in soil thermal and hydraulic properties. It should not be sieved and thrown away, like most of the current studies do, before measuring soil properties.

Major Comments: âĂć 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. âĂć 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. _dry;gfgKe;f ff , in Eq. 14 which at least require explanation.

Reply: We followed the work of Lawrence and Slater (2008) and CLM4, which use weighted arithmetic mean for all the thermal and hydraulic properties of mixed mineral and organic. This method was also used in Chen et al. (2011). We will modify manuscript the make this point clear. See also Reply1.

TCD
7, C2077–C2084, 2013

Interactive Comment

âĂć 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?

Reply: We used the weighted geometric mean for the following two reasons: 1) (psi/psi_sat)ˆ{-b}=(vwc/poro) is the equation to represent water retention curve, considering the form of (psi/psi_sat)ˆ{-b}, it is better to use weighted geometric mean; 2) we tried the weighted arithmetic mean. However, the gravel has very small effects on the curve (compare the Figure 4b in the text and attached Figure 1b), while real laboratory data shows that the effects of gravel are large (Wang et al., 2013). We will modify manuscript the make this point clear.

It is worth to note that the work of Lawrence and Slater (2008) does not provide any reason why the weighted arithmetic mean should be used.

Factor A does not exist in the original word manuscript. It is created by the conversion to pdf file. We will correct it.

âĂć In principle, the same applies to Eq. 15. The chosen mixing law must be at least motivated. Reply: please see Reply1.

âĂć 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.

Reply: First, the Cote and Konrad (2005) provides different parameters, which produce different Kersten numbers, for “pure” materials. However, in our study, we further considered mixture of soil (e.g. fine mineral and gravel), based on Cote and Konrad (2005).

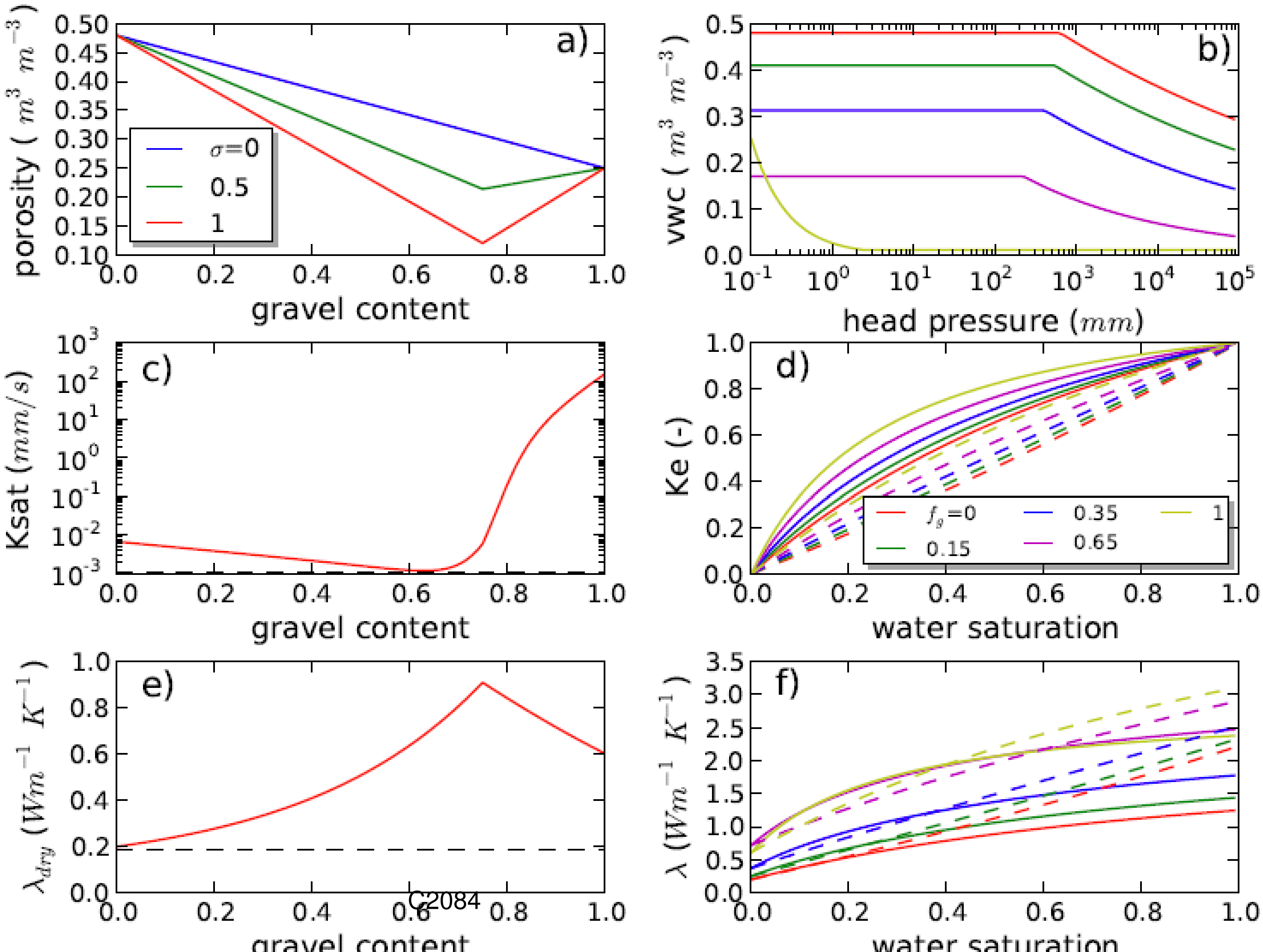
Second, we agree with the referee that the Cote and Konrad (2005) is empirical and most probably it cannot “hold for the entire range of possible input soil properties”. However, these shortcomings cannot prevent it from being used. And we don’t think there exits one scheme which can “hold for the entire 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.

Reply: We have to admit that we don't have any explicit measurements on the thermal conductivity of gravel soil with the same water content under different thermal condition (frozen or unfrozen); and we haven't read any report on it. The standard method of measuring soil thermal conductivity is to remove gravel (>2mm) and root, and then measure the rest (e.g. Chen et al., 2012). Previous studies, either modeling or laboratory, neglect the role of gravel. Therefore, it is very hard, if not impossible, to find references which do the above-mentioned study for soils with gravel. Again, we suggest that serious laboratory work should be done to investigate the role of gravel on soil properties under both frozen and unfrozen conditions.

As for the example of Feng et al. (2012), we didn't intend to use it as an example of smaller frozen thermal conductivity. We intended to demonstrate that Farouki scheme simulated much bigger frozen thermal conductivity than unfrozen thermal conductivity (see Table 3). The referee mentioned that "In case of dry soils (as on the QTP), where the conductivity change during freezing is small". However, in our test case with 10o slope, the soil is relatively dry, the simulated frozen thermal conductivity is about 2.07 times that of unfrozen thermal conductivity using the Farouki scheme (see Table 1).

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


a)
porosity ($m^3\ m^{-3}$)
gravel content
σ=0
0.5
1
b)
vwc ($m^3\ m^{-3}$)
head pressure (mm)
c)
Ksat (mm/s)
gravel content
d)
Ke (-)
water saturation
f_g=0
0.15
0.35
0.65
1
e)
λ_{dry} ($Wm^{-1}\ K^{-1}$)
gravel content
f)
λ ($Wm^{-1}\ K^{-1}$)
water saturation