

Review of “Analysis of Soil Hydraulic and Thermal Properties for Land Surface Modelling over the Tibetan Plateau”

By Hong Zhao, Yijian Zeng, Shaoning Lv, Zhongbo Su

This is an interesting paper that presents a wealth of soil physical data and related calculations with semi-empirical theoretical equations (with a focus on hydraulic and thermal soil properties) for the Tibetan plateau. In-situ hydraulic and thermal data are scarce, particularly for thermal properties, so this dataset is a welcome addition to what is available in the literature. As the authors point out, the Tibetan Plateau exerts a huge influence on the Earth’s climate system and is important from a land surface-atmosphere feedback point of view, so these data should in theory greatly improve future land surface and climate model outputs for this region and beyond.

Major comments:

- Although it is overall easy enough to understand what ‘went on’, the scientific and English language is imprecise in various places. I have given some examples below, but the authors should go throughout the entire manuscript carefully, and check that their description is as exact as possible.
- They also need to make a bigger effort to refer to the exact equation numbers and Appendix sections when describing their Material and Methods. At the moment this is rather hard to follow for a non-expert.
- How were the SWRCs determined exactly? Were the samples used in the pressure cell method undisturbed and still contained gravel? If this is the case, is it ‘fair’ to compare the SWRCs of such gravel-rich samples with SWRCs derived from the CH and VG equation with parameters derived from PTFs that were not developed for gravelly soils?
- At times, you have the heat capacity of your (average) samples going up and down with soil moisture content, instead of steadily increasing with SM. What has caused this? This must be an issue with your sensor?

Minor comments

Line 41-42: You say: “SHP and STP govern the partitioning of SM between infiltration and evaporation flux and water heat transport processes (Zeng et al., 2009a; Zeng et al., 2009b)”, but surely STP only has a significant role on SM during periods of freezing?

Line 44: Should be: ‘soil formation factors’ or “soil forming factors”?

Line 44: ‘Could be’ should be ‘can be’, also throughout manuscript.

Line 53: What exactly is meant by “soil physical consistency of LSMs
“?”

Line 69: ‘are accessible’

Lines 70-73. I am wondering whether this kind of info is more suitable for the Materials and methods?

Line 92: Could “**2. Experimental Designs and Parameterization Schemes**
“ not be “2. Materials and methods. Followed by : 2.1 Field experiments, 2.2 Laboratory Experiments, 2.3. Model equations.

Line 101 and throughout: please refer to this soil property as dry bulk density.

Line 120-123: I found this sentence hard to follow: “Based on fine minerals and the standard particle size classes of the United States Department of Agriculture (USDA), sand and clay percentages as well as the mean particle diameter of the fine component (FD) were determined using the Malvern Mastersizer 2000 particle size analyzer”. Did you not simply want to say that you determined sand and clay percentages and FD for the fine minerals with the Malvern..?

Line 134-136: Say something like : “only 30 out of 96 samples were used for the Naqu network as the structure of the other samples was so unconsolidated that the material did not remain enclosed within the rings”. The same occurred for a number of samples at Maqu and Ngari and therefore samples packed in standard rings were used for Naqu_north, SQ17, SQ18 and SQ21 sites”.

Line 136: Were the samples in the standard rings undisturbed? This needs to be made clear. To me ‘packed’ means that you inserted the soil yourself from bulk samples. Maybe say ‘contained in the rings’ for undisturbed samples and ‘repacked’ for disturbed?

Line 138-139: “The quality of the measured soil property dataset was evaluated based on quality indicators (e.g. observation date, level of trust, data quality rating and accuracy)”. This sounds somewhat vague? How is the date going to affect the quality, for example?

Line 141: “The entered data (level ‘A’) have been standardized (level ‘B’) and harmonized (level ‘C’).”

Although I appreciate the efforts to ensure data quality and consistency, I would like to know what procedures and criteria are behind the ‘standardising’ and ‘harmonising’. Have you actually changed raw data values??

Line 144-146: You say: “The collected basic soil property and SHP & STP datasets over Tibet-Obs **were further used to evaluate** the existing soil datasets of FAO-UNESCO (FAO/UNESCO, 2007), HWSO (FAO/IIASA/ISRIC/I 145 SSCAS/JR, 2012), BNU (Shangguan et al., 2012; Shangguan et al., 2013), SoilGrid1km (Hengl et al., 2014), SoilGrid250km and HPSS

(Montzka et al., 2017) over the TP. The detailed information of the datasets used is listed in Table S1 of the Supplement”.

Although Table S1 lists the details of these databases I can't say that I see an **evaluation** of these datasets using your own data. Did I misunderstand your sentence?

Line 151-152: You say “The Cosby et al. (1984) PTF that used sand percentage (hereafter the Cosby-S scheme) has been widely used”.

However, this refers to Cosby's uni-variate equation, but they also present a multi-variate PTF, that uses clay as well as sand. It would be good to mention this. In this Section you could also refer to the in-depth overview of PTFs by Van Looy et al. (2017).

Van Looy, K., Bouma, J., Herbst, M., Koestel, J., Minasny, B., Mishra, U., Montzka, C., Nemes, A., Pachepsky, Y., Padarian, J., Schaap, M., Tóth, B., Verhoef, A., Vanderborght, J., van der Ploeg, M., Weihermüller, L., Zacharias, S., Zhang, Y. and Vereecken, H. (2017) [Pedotransfer functions in Earth system science: challenges and perspectives](https://doi.org/10.1002/2017rg000581). Reviews of Geophysics, 55 (4). pp. 1199-1256. ISSN 1944-9208 doi: <https://doi.org/10.1002/2017rg000581>

Line 152-153: Porosity and bulk density are directly related via a standard soil physical relationship: $\text{Porosity} = 1.0 - \text{BD} / \text{density of solids}$. I am not sure I would call this a PTF 'scheme', although I can see why you want to use this word for consistency.

Line 154: I do not understand what is meant here: “However, with the existence of SOC, soil porosity and water retention capability both tend to increase”.

What is both referring to? And what exactly is increasing? If porosity goes up, BD should go down, they can't both increase at the same time? Or did you mean the performance of the schemes increase? But the ones mentioned so far do not use SOC etc.? Also, what is meant with 'existence'? You mean 'availability'. Do we need the word 'capability'? Could we just say 'data'?

Line 156: You mention the 'SocVg scheme'. I presume this refers to Van Genuchten, but not enough information is given here. This needs to be made clearer.

Can you not provide the reader with the exact equations in an appendix? You do in fact, but you are not referring to it here, as far as I can see.

Line 157: Replace 'depict' by 'calculate'.

Line 160: Start a new paragraph when introducing the SHPs. The previous sentence needs to sit with the porosity paragraph.

Lines 160-161: You need to refer the reader to Section A2 in your appendix for the exact CH and VG equations.

Line 161: Replace “suction pressure” with “matric pressure” as suction is always deemed positive.

Line 164-165: You say: “Furthermore, the selected PTFs (see Appendix, Table A1) combined with the optimal porosity scheme were used to estimate the SWRCs-CH and SWRCs-VG.”

I am not sure why you choose to make a separation between porosity and the other parameters in your water retention curves. The Cosby et al univariate equation for porosity is also a PTF function?!

Lines 165-166: You say “These two estimations (e.g. measured vs. PTFs) were compared, and suitable PTFs with the lowest bias were selected.”. You mean: “ the estimates of water retention curves (using Eqs. presented in Section A2) and measured WRCs were compared and the PTFs that yield parameters that resulted in the lowest bias between measured and modelled WRC were selected? Could that mean that a mix of PTFs was used for calculation of WRC? E.g. Cosby for porosity, Brakensiek for the b-parameter etc.? I find this very hard to follow.

Line 169-170:” To estimate the Ks of a mixture containing gravels, Peck and Watson (1979) used a heat-flow analogy correlating with the Ks of fine minerals and the volumetric gravel fraction”.

Please rephrase, I find this hard to follow. The fine minerals are in between the gravel. What exactly was correlated with what?

Also, where is the F coming from in the VGF scheme? I don’t see the logic here.

Line 178: You mean ‘standard’ PTFs or ‘PTFs from the literature’?

Line 179: You say: “Several (semi-) empirical models have been developed to estimate the STP”. There is not one single STP. There are 3 STPs: thermal conductivity, heat capacity and thermal diffusivity. In this case you meant lambda. Remind the reader again what this symbol stands for.

Line 185: Say: “Farouki (1985) proposed an alternative method and..

Line 186: say: “soil minerals”.

Line 188/190: Did you mean ‘factor’ instead of ‘feature’?

Line 193-194: You say: “For each λ scheme, a comparison was made using parameters (i.e. the λ of minerals) with and without gravel and SOC considerations”. This is a bit of a throw-away remark, but later on in your figures and discussion this consideration of gravel and SOC becomes quite important. How did you do this? You need to refer explicitly to Eqs. 21, 22 in your Appendix.

Line 195-196: You need to help the reader by referring to these separate A sections throughout the parameterization section. I found this quite hard to follow without the equations present.

Line 202-203: You say: “across the three climate zones over the TP. In the arid zone..” How do these zones fit with Naqu, Maqu etc.? Please refer consistently to the different locations. Also, Figure 1a does not show rainfall gradients or SM, so it is not obvious where the arid zone etc is, unless one knows the TP well.

Line 216: Is usually sub-humid used instead of semi-humid?

Line 229: A BD of 2.1 seems very high, this must have been nearly pure gravel? Yes, you mention that in line 215.

Line 234: ‘laying’? You mean ‘layering’? ‘stratification’, ‘variation’?

Line 239: the magnitude of mean Ks was **of the order of** 10^{-5} (m/s).

Line 240: ..exhibiting the **lowest values** of porosity’..

Line 241: ‘.. exhibited a variation of one order of magnitude..’

Line 241: Differed by 1 order of magnitude?

The English in this section really needs tidying up. I think you have got your orders of magnitude confused.

Line 245: “the negative correlation between saturated hydraulic conductivity and soil organic carbon in soil”. Are you sure this is the case? More SOC means a higher porosity so generally higher Ks, unless hydrophobicity plays a role, but then you need to state that.

Line 248: ... say: “Ks reached a minimum”.

Line 251-252: tend to change or decline with what? With depth? Be precise.

Line 251: “porosities for layers with a GFF....” See also line 255/257

Line 264: Replace “the heterogeneity issue may still exist”, by “heterogeneity may have had an effect on the values of soil properties and parameters throughout our sampling procedures, as with any soil field experimentation”

Line 268 and Figure 7: You refer to these and the TC plots as ‘profiles’, but in my opinion that is incorrect. A profile is a (plot of) a soil property with depth, but here you are showing the dependencies of Ch and lambda on SM, for different depths. That is a different thing..

Also: at times the Ch goes down even if SM has increased. Why is this? According to theory it increases linearly? Also the shape of the curve is more asymptotic than linear?

Line 270: You say: “ as soils dry out” but how do you know that it is not caused by the different Ch values of sand and silt/clay?

Line 271: “Figures 7d&e&f show how the relationship...”

Line 273: You say: “The mean λ ranged from 1.8-0.2 (W m⁻¹ K⁻¹) as the soils dried out”, but you mean to say “The mean λ ranged from 1.8 at saturation to 0.2 (W m⁻¹ K⁻¹) as the soil reached air-dry state”.

Line 279: .. “had lower lambda values” (not comprised)

Line 280 and below: You need to make it clear that you used in-situ values of sand content and BD to calculate porosity. Also, to me it seems strange to have a scheme based on BD to calculate porosity as they are so tightly related. If you have a correct estimate of particle density should they not give the same answer?

Line 291-295: Gravel is a solid thing, it does not have a porosity. Do you mean a soil packed with gravel ‘particles’ only?

Line 305: “Taking all these results together, it indicates that the BD scheme delivered the greatest predictive accuracy for estimating porosity in profiles across the three climate zones.” This comes at no surprise... (see my previous comment).

Line 309: remove ‘and’.

Line 310: SWRC do not ‘layer’, You mean ‘showed distinct variation with depth’.

Line 312 end: add: ‘compared to the measured WRCs’

Figure 11: you talk in the legend about Cosby et al. (1984) 1 & 2?? Are these the uni- and multi-variate estimates?

Line 314: Here it is ok to talk about suctions as you use positive values.

Line 316: which Table?

Line 317: PFTs cannot predict SM. You need to do a better job at describing exactly what was done and how the parameter estimates were obtained (in the Materials and Methods too).

Line 353-354: You say “In particular, the PTFs given by Cosby et al. (1984) (1&2) predicted K_s well for the CH model”. How is the CH model relevant? I thought you were comparing against measurements? Same goes for the VG model in line 354.

Line 360: “indicating that the estimated K_s for the VG model is less affected by gravels”. Again, I thought you were simply comparing K_s measured with K_s modelled (with BC & VG, with different PFTs).

Line 366: I think this section needs a more informative title to bring out the fact you are comparing measured data with theory?

Line 367-369:... bias.. compared to the measurements. You need to make that explicit?

Line 395: Basic soil properties

Line 415-416: But now these WRC equations (or rather the PTFs to find their parameters) are used with the independent (SoilGrids etc) datasets, right? You need to make that very clear..

Line 436-441: Make it very clear that it is only texture, BD and SOC that you use from these datasets to derive Ks via the PTFs.

Line 445: You are suddenly talking about a LSM? Also soil hydraulic diffusivity is rarely directly used in LSMs? It is directly related to the SWRC and the K curve, so once you have those two one would not need the diffusivity curve.

Line 450-451: "In the semi-arid zone, all datasets generated CH-D and CH-K similar to those" ...

The datasets themselves don't generate anything? Keep your language precise..?

Line 463: ... "slower heat transport"? Maybe say "reduced soil heat fluxes"?

Line 463-464: "and thereby subsequently the higher soil temperature derived from the land surface model, and vice-versa".

What is meant? The LSM will overestimate compared to soil temperature measurements?

Line 469: Did you mean 0.3 m³ m⁻³?

Line 491-494: "The Cosby et al. (1984) PTFs proved more applicable for SHP estimation by the Clapp and Hornberger (1978) (CH) model, and the continuous Wösten et al. (1999) PTFs for SHP estimation by the Van Genuchten (1980) - Mualem (1976) (VG) model"

This seems quite obvious to me. In fact, one could wonder whether one should even attempt to use VG PTFs to find CH parameters and vice versa...

Line 494: ..' deployed successfully'

Line 498: "This information provided indicative signs for soil parameterization in LSMs." This sentence tells us very little...

Line 499: ' are also used'

Line 506: '.. in LSMs'. There is not just one/a LSM?

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