Physical Pedotransfer Functions To Compute Saturated Hydraulic Conductivity From Bimodal Characteristic Curves For A Range Of New Zealand Soils

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RESPONSE TO REFEREE 1

Dear Reviewer 1,

We would like to express, our gratitude for your efforts for your review of our article: *Saturated hydraulic conductivity model computed from bimodal water retention characteristic curves for a range of New Zealand soils*. We really appreciate your positive evaluation. We also wish to acknowledge for the time and the efforts of your comprehensive review that helped us to significantly improve the manuscript.

MAJOR COMMENTS

However, the manuscript is fragmented in too many small parts and requires some minor improvement in its structure.

Thanks for bringing up this issue, to clarify the manuscript we simplified the subsections of the *Methods* section.

1) Subjective choice of hm_mac=3.16 cm (Eq. 15) in absence of measurements of data points near saturation. Maybe in this case, it would be recommended to optimize hm_mac in order to increase objectivity and add flexibility.

We agree that Eq. 15 needs further explanation and therefore we rewrote the section as follow:

$$h_{m\_mac} = \exp\left[\frac{\ln(H_{mac})}{P_{m\_mac}}\right] \qquad (15)$$

where P_{m_mac} is a fitting parameter greater than 1. We found the fitted value of P_{m_mac} was 2.0, however this fitted parameter was very broadly determined. The cause might be that we are optimizing $\sigma_{\_mac}$ and therefore h_{m_mac} and $\sigma_{\_mac}$ might be linked. Linked parameters (Pollacco et al., 2008a, 2008b, 2009) means that there is an infinite combination of sets of linked parameters h_{m_mac} and $\sigma_{\_mac}$ which produces values of objective function close to that obtained with the optimal parameter set and for which there exists a continuous relationship between h_{m_mac} and $\sigma_{\_mac}$. Further research needs to determine if having more data in the macropore domain would reduce the cause of non-uniqueness. To illustrate h_{m_mac}, the equivalent r_{m_mac} point is shown in Fig. 1, where r_{m_mac} is the inflection point of the macropore domain. Fig. 1 also shows that the matrix and the macropore domains meet at R_{mac} (H_{mac}).

2) The parameter W (“empirical” according to the authors) in the bimodal form of Romano et al. (2011) guarantees that the sum of the matrix and macropore domains gives Se=1 (same role as in Durner, 1994). The authors replace it with a new parameter (θs_mac). Indeed they state that this new parameter is “physically sound” and can be easily optimized with the other soil moisture parameters in the matrix range delimited by Hmac, that is empirically fixed at 10 cm. Isn’t it a contradiction? The authors should test this hypothesis on soil samples comprising measurements near saturation. This requires at least a few examples on soils taken from UNSODA or HYPRES for instance.

Thanks for your comments; nevertheless we do not fully understand why you believe there are contradictions. I improved the section and please inform us if it answers your concerns.

*The shape of $\theta_{bim}(h)$ is identical to that of $\theta_{bim\_rom}(h)$, but the advantage of $\theta_{bim}(h)$ is that it uses the physical parameter θ_{s_mac} instead of the empirical parameter W, and θ_{s_mac} ($\le\theta_s$) is **more easily** parameterized than W **particularly when there is no available data in the macropore domain**. **When we do not have data in the***

macropore domain, *θ_{s_mac} is determined by fitting the hydraulic parameters θ_{s_mac}, θ_r, h_m, σ of $\theta_{bim\_mat}(h)$ (Eq. (10b)) solely in the matrix range ($r < R_{mac}$ or $h > H_{mac}$) Fig. 1 shows that R_{mac} and θ_{s_mac} delimit the matrix and the macropore domains and that r_m of the Kosugi model is the inflection point of $\theta_{bim\_mat}(h)$ and r_{m_mac} is the inflection point of $\theta_{bim\_mac}(h)$.*

3) The RMSE-values obtained by this technique should be compared to the RMSE-values of existing methods (published in other articles) that estimate Ks from unimodal soil moisture parameters.

We agree that it will be best to compare our results with published data. We therefore compared our results to those of Pollacco et al., (2013):

The $RMSElog_{10}$ of K_{s_uni} for subsoil is 0.47 cm day^{-1} (Table 6) which is slightly worse compared to the $RMSElog_{10}$ of 0.420 cm day^{-1} by using UNSODA and HYPRES data sets (Pollacco et al., 2013).

4) Experimental design needs to be clear: The authors mention that the water content values were measured at the following matric potential points: 5, 10, 20, 40, 50, 100, 1500 kPa (Lines 296-297) please refer to the Book Methods of Soil Analysis, Part 4, Physical Methods" (J.H. Dane and G.C. Topp, eds.), pp. 692-698, SSSA Book Series N.5, Madison, WI, USA: which method was used to measure the moisture release curve? Hanging water column, suction tables, Pressure plate etc.

We agree that clarification of the experimental design is required and therefore we rewrote the section *4.1. Measurement of physical soil properties*

MINOR COMMENTS:

1) I doubt the term "pedotransfer function" is proper to identify the estimate of Ks from water retention parameters

We agree that the meaning of pedotransfer function is not well defined so therefore we are happy to change the title to:

Saturated hydraulic conductivity model computed from bimodal water retention characteristic curves for a range of New Zealand soils

We also made some minor corrections in the introduction to clarify that we are dealing with a model and not with a pedotransfer function.

2) Line 21 page 1: specify if you refer 100 mm to diameter or something else

Yes, we agree we need further specification.

The K_s data were collected using a small core size of 10 cm ***diameter.***

3) Line 27 page 1: I agree that there are uncertainties related to the core sizes, but eventual improvements should be tested on larger cores.

The manuscript purpose was to make the best out of the historical data stored in S-map https://smap.landcareresearch.co.nz/ which contains large uncertainties. Nevertheless, based on the recommendations made in section 6. *Recommended future work to improve the New Zealand soil database* we are now in the phase of collecting new data sets by using large core of size and by taking more measurement close to saturation and we plan to publish the results in due course.

4) Line 63 page 2: add references

We added the following historical reference since to our understanding there does not seem to be a specific paper written by Poiseuille which relates to the Hagen-Poiseuille and Darcy law.

Anon: The History of Poiseuille's Law, Annual Review of Fluid Mechanics, 25(1), 1–20, doi:10.1146/annurev.fl.25.010193.000245, 1993.

5) Line 144 and 168, page 5: why is it [0,1)?

This is a mathematical notation such that the feasible range is the internal interval including zero but not including one. This is because when τ_3 =1 then the K_s model returns NaN.

6) Lines 194-195, page 6: In Eqs 11b and 11c the two integral ranges are both Se=[0,1]. Shouldn't they be Se=[0 Se_mac] and [Se_mac,1]?

The reviewer's question is to determine if in the matrix domain the integral should go from [0, S_{e_mac}] and in the macropore domain the integral should go to [S_{e_mac}, 1] compared to the two integrals evaluated over the interval S_e=[0,1]. This was questioned during the development of the model, and we decided to use the simplified notation for which the two integrals go to S_e=[0,1] because numerically it makes little difference since the pore size distribution of the macropore and the matrix do not overlap since we constrained by σ_{mac} < 1.5.

7) Lines 250-254, page 8: The determination of saturated water content (namely θs) is rather easy, why do the authors use the artefact of Eq.6?

In the section 4*.1 Measurement of physical soil properties* we explained that the historical data used in this study had some issues in measuring θs, this is why we used Eq. 6:

"The total porosity, ϕ, described in Eq. (5) contains uncertainties from the measurement methods, where ϕ is derived from separate measurements of particle density and bulk density, rather than being directly measured".

8) Fig. 2 page 25: improve overall quality, enlarge fonts

We improved the quality of Figure 2.

9) Fig. 3 page 26: please add the 1:1 line. Fig. 3 and 4 should be the same size

We added the 1:1 line in the caption. It is not possible that Fig.3 and Fig.4 are the same size since they were designed by using different software: Fig. 3 was designed by using PyX (Python) and Fig 4 by using *R*.

10) I encourage the authors to investigate on possible relationships between tortuosity parameters and soil physical parameters (texture, porosity etc)

We are currently collecting a new set of data where we are specifically measuring near saturation collected by using large cores, and we will investigate relationship between tortuosity parameters and other soil physical parameters.