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 3

Dear Reviewer 3,

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.

I would recommend considering modifying the title, because the presented method is closer to a model describing the Ks than to a PTF.

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 functional model and not a pedotransfer function.

TEXT

Line 11: here and in the entire text instead of "moisture release" "moisture retention" is more frequently used in the literature, therefore it might be more preferable to use.

We agree and are happy to systematically replace *moisture retention* in the manuscript with *moisture release*.

Line 18: here and in the entire text please use "structured soil" instead of "structural soil" if soil having aggregates is referred.

We agree and are happy to systematically replace "*structured soil*" instead of "*structural soil*" in the manuscript where appropriate.

Line 42: please refer to more recent PTFs.

We added a recent PTF reference developed in New Zealand:

Cichota, R., Vogeler, I., Snow, V. O., and Webb, T. H.: Ensemble pedotransfer functions to derive hydraulic properties for New Zealand soils, Soil Research, 51, 94–111, doi:10.1071/sr12338, 2013.

Line 94-95: it would be helpful for the reader to highlight what r and r_m means. If r_m refers to the mean of soil-pore radius I would suggest writing r with overbar. If σ means the variance of the log transformed soil-pore radius, please make it clear in the notation.

To clarify the meaning of the Kosugi parameters we rephrased Eq. 1 as follow:

where θ_r and θ_s [$cm^3\ cm^{-3}$] are the residual and saturated water contents, r_m [cm] is the median pore radius and σ [-] denotes the standard deviation of ln(r).

Line 104: it might worth to give a number for the equation r=Y/h, than it is easier to refer it in 8b.

As in the recent paper *Using Bimodal Lognormal Functions to Describe Soil Hydraulic Properties* published by Romano et al., (2011) they did not include an equation number for the Young–Laplace capillary equation since it is understood that all soil scientist reading this specialized paper would be familiarised with the Young–Laplace capillary equation. Nevertheless, for clarity I added the following note in Eq. 8b:

I added

$r_m = Y/h_m$ ***(Young–Laplace capillary equation)***

Line 107: it might increase the readability/understanding if another notation would be used for the mean and standard deviation of ln(h_{m_mac}). If first ln of h_{m_mac} is calculated and then the mean and standard deviation of the transformed h_{m_mac}, than the present notation does not tell it. Please check it.

To clarify the meaning of the Kosugi parameters we rephrased Eq. 3 as follow:

where h_m [cm] is the median metric head

Lines 134-146: I hope that I didn't miss anything in Eq. 7-8b, if yes, sorry, just would like to clarify it. It seems that you have a small mistyping in the numbering of the equations, in line 146 you refer to Eq. 8 which is Eq. 7 in the text, Eq. 8 is missing. Please correct it in the entire manuscript.

Thanks for noting this issue in the manuscript. We did not find any further issues of the numbering.

If S_e equals to 1 in Eq. 7 as mentioned in line 146, why is it included after K_s which in theory tells already that it is a saturated state because you use the notation "s"? If it is needed to follow the mathematical logic, a possible solution might be to add S_e=1 under Eq. 7. If it is stated could Eq. 8a, 8b, 11a-14b, 19 be simplified?

Just to clarify we defined in *2.1 Kosugi unimodal characteristic water retention and unsaturated hydraulic conductivity curve* $S_\mathrm{e}(r) = (\theta - \theta_\mathrm{r})/(\theta_\mathrm{r} - \theta_s)$.

We took on board your simplifications by rewriting Eq. 8, 11, 14 and 19 by integrating between 0 and 1 instead of 0 and S_e. I also simplified the notation for e.g. by replacing $K_{s\_bim}(S_e)$ to K_{s_bim}. Nevertheless, we do not see how mathematically we can further simplify the equations.

Lines 154 and 174: I would suggest to use "bimodal water retention curve" instead of "bimodal characteristic curve" to make it completely clear for the readers that you have to deal with both water retention curve (Θ(h)) and hydraulic conductivity curve (K(Θ)).

Thanks for improving the manuscript, we agree that replacing "*bimodal characteristic curve*" throughout the manuscript with "*bimodal water retention curve*" clarifies the meaning.

Line 162: please give the terminology of H_{mac} too – as you did it for R_{mac}.

Thanks for helping us to clarify the manuscript, we made the modifications as suggested.

R_{mac} is the theoretical pore size r that delimits macropore and matrix flow ***and H_{mac} is the theoretical pressure that delimits macropore and matrix flow***

Line 167: same as in line 107. Please check it.

As suggested we made the following modifications.

*where θ_s, h_{m_mac} and σ_{mac} are, respectively, the saturated water content, t**he median pore radius and the standard deviation of ln(h)** of the macropore domain, θ_r, h_m and σ are parameters of the matrix domain, and W is a constant in the range [0,1).*

Line 226: maybe I miss something, for me it is not clear why 2 and why not 1.5. Can you please describe it?

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

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

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}).

Line 235: Please describe shortly or rephrase what do you mean by main horizon?

We removed the following sampling description since it is confusing and it does not add extra clarifications to the results:

~~Three horizons in each soil profile were sampled from deep soils (topsoil, horizon with slowest permeability, and the main horizon between these) and two from shallow soils (topsoil and the main horizon above gravels)~~.

Lines 236, 237: in case of undisturbed samples please provide the volume of the core.

We rewrote the *4.1. Measurement of physical soil properties section* and we provided extra details of the field and laboratory methods

Lines 247,248: please use cm also here.

For consistency, we changed mm to cm in all the manuscript.

Line 251: please refer which method was used to measure particle density.

This was already mentioned in the manuscript:

Laboratory analysis for particle size followed Gradwell (1972).

Line 259: please use cm also here.

You are right for consistency with the rest of the text we changed mm to cm in all the manuscript.

Line 262: point a) does not fit into the uncertainty due to measurement error. It increases the error of the model, therefore better to mention it later when the performance of the bimodal model is analysed.

The variability in both θ and Ks reflect variation within the stratum of a supposedly-uniform soil type. The effect is magnified by the small cores used, so in this sense it is an artefact of the measurement process and it is measurement error in the classical sense. We introduced this point in order to inform the reader concerning these historical datasets, which are considerably less accurate than modern datasets and that the reader should understand that if we had modern datasets the K_s model should be much better.

Line 279-280: "anthropogenic disturbance and biological activity" might cover better the disturbances influencing soil porosity.

This is much better and concise; we implemented the corrections in to the manuscript.

Line 287: Eq. 10c is called "modified Romano bimodal" curve, why is it called unimodal Kosugi here?

We believe that we did not make any typos since Eq. 10 does not use the empirical weighting so it is no longer $\theta_{bim\_rom}(h)$.

Line 290: please describe shortly how you optimized K_{s_uni} and K_{s_bim} models. Which measured parameters did you use?

If I understand properly your comments, you wanted us to provided further explanation on the objective function which is described below.

Optimization of the τ_1 , τ_2 , τ_3 of the K_{s_uni} model (Eq. (8)) and τ_{1_mac} , τ_{2_mac} , τ_{3_mac} , $\sigma_{\_mac}$ parameters of the K_{s_bim} models (Eq. (14)), where the physical feasible ranges of the tortuosity parameters are described in Table 3.

Line 302: could you provide reference or short explanation on why power was set to 6?

The computation of K_{s_bim} requires $\theta(h)$ to be accurate near saturation, when the drainage is mostly from large pores, and to achieve ***this balance we found by trial and error that best results are achieved*** *when P_{ower} = 6.*

Lines 307: instead of K(Θ) is not it more correct to write K_s? If yes, please rephrase sentence in lines 308-309.

The log transformation of OF_{ks} puts more emphasis on the lower ***K_s*** *and therefore reduces the bias towards larger conductivity*

Lines 319-322: it might worth to rephrase this section or include them separately under the subsections.

For clarity we provide at the beginning of the *Result and discussion* section the plan of the layout of the results.

Line 321: please include if the difference is significant between unimodal and bimodal K_s models.

We commented below in section 5.*2 Improvement made by using K_{s_bim} instead of K_{s_uni}*

Line 322-324: please include it in "materials and methods" section

Thanks for your recommendation we moved the equation of goodness of fit into the *material and methods.*

Lines 326-330 and 332-335 are not totally in line, please harmonize them.

If you are talking about the tabs than I lined them up. Thanks.

Lines 341-344: is the improvement significant – overall or only in case of subsoils? Please include it in the text.

The improvement is more significant in the topsoil than for the subsoil. We made a minor modification to the text to improve clarification:

As expected, the ***reasonable*** *improvement is greater for topsoil containing higher macroporosity (12% improvement) than for subsoil (4% improvement)*

Line 410: there is a mistyping, please delete "improved" before "Romano θ(h)". Please include the results of the modified bimodal model (10a) compared to Romano's model under "results" section too.

We agree that the wording was incorrect, we did not improve the model we just changed the form of parameterizing the model. Since the shape of the two models are identical we do not need to compare Romano θ(h) with θ(h) bimodal.

We report here on further adaptations to the saturated hydraulic conductivity model to suit it to dual-porosity structured soils (Eq. 10) by computing the soil water flux through a continuous function of a ***modified version*** *of Romano et al. (2011) θ(h) dual pore-size distribution (Eq. 18). The shape of the Romano θ(h) distribution is identical to the* ***modified*** *θ(h), but the advantage of the developed bimodal θ(h) is that it is more easily parameterized when no data are available in the macropore domain.*

Line 424: please include for what kind of soils you suggest to use the presented model and what are the limitations of its use.

This is indeed a valid question, but to answer this question correctly we would need to collect more soils samples in each subgroup (Table 4). Based on the section "*Recommended future work to improve New Zealand soil database*" we believe that the greatest challenge is to make predictions on slowly permeable soils as mentioned:

Therefore, this model's performance may be restricted in cases of non-Darcy flow, such as non-laminar and turbulent flow, which may occur in large macropores.

Make more accurate measurements on slowly permeable soils (< 1 cm day^{-1}), which are important for management purposes but are not well represented in the current databases.

TABLES

Line 540: please rephrase, possible solution: "Θ_5 which is". Why is Θ_5 the minimum value of Θ_s?

Due to uncertainties in measuring θs, we optimized θ_s. The feasible range of $0.6 > \theta > \theta_5$. Since as mentioned *The closest data point near saturation is $\theta(h = 50$ cm), which is in the matrix pore space.*

Lines 545-546: "When τ3 increases the connectivity of the soil increases", it seems to be in contradiction with lines 150-151 a 5th row of Table 3.

We total agree with you this is why in section *Optimal tortuosity parameters* I commented on this contradiction:

The optimal tortuosity parameters of K_{s_bim} and K_{s_uni} (Table 6) show that the optimal parameters are within the physically feasible limits, except for τ_{3_mac} of the subsoil, which are greater than τ_3. This is understandable because Pollacco et al. (2013) found τ_3 not to be a very sensitive parameter.

Lines 555-558: please rephrase title of the table and its content because it is not clear in present from without reading the main text of the manuscript.

We improved table 5 and the caption description.

FIGURES

Figure 3 and 4 has similar content, please consider including them under 1 figure caption maybe including a) and b) figures.

Thanks for suggesting merging figure 3 and figure 4. Since figure 3 relates to section *Improvement made by using Ks_bim instead of Ks_uni* and figure 4 relates to section *Uncertainty of the bimodal saturated hydraulic conductivity model predictions*, merging the 2 figures would give the wrong interpretation to the reader.

Technical corrections

Just a small suggestion, in Eq. 11a-11c and 12-13 maybe you can start with models regarding the macropore and then follow with the matrix similarly to Eq. 10a-10c, 14a-14b and 19, in this way you would have the same order in the equations in the entire manuscript.

Thanks for spotting this inconsistency, I will change the order of the equations mentioned starting from matrix than following for macropore. It is easier to start the explanations for matrix than for macropore.

Please check Eq. 11a, 11b and 11c, because they have different size that other equations.

Yes we corrected the quality of the equations and they now have the same size.

Line 322: please put log10 in subscript.

Thanks for spotting this typo: