Reviewer #2:

This study presents a meta-analysis of published hydraulic conductivity (K_h) data for saturated and near-saturated soils. The analysis is detailed and seemingly rigorous, the spatial scale of the study is global, and the dataset is large. The authors show that factors such as climate, land use, tillage and compaction are skilful predictors of hydraulic conductivity, likely because they serve as proxies for soil macroporosity. In particular, it is instructive to see that such factors are better predictors of soil hydraulic properties than texture and density used in traditional pedotransfer functions. The authors also show that unsaturated hydraulic conductivity at high tensions (low water contents) is not closely related to saturated hydraulic conductivity, as often assumed. The article is largely well written and is certainly within the scope of the journal. I believe it will be of interest to the readership. However, I have two main concerns about the presentation of the analysis that I recommend the authors should address before the article is published. Below, I also make some minor recommendations for presentation in the spirit of trying to help the authors improve their article.

We thank the reviewer for his positive feedback. Below we address all his remarks.

MAIN COMMENTS

1) The introduction identifies several previous studies that have performed (meta-) analyses of large databases of soil hydraulic properties, and in many cases indicates that these studies included predictive equations (pedotransfer functions) that allow readers to estimate hydraulic properties of interest, such as K_h. The methods then describe in detail the construction and statistical analysis of the present dataset. I was surprised, therefore, that I couldn't find anywhere in the current manuscript a new set of pedotransfer functions based on the authors' analysis. The authors have gone to all the trouble of building, scrutineering and analysing a new database, but they don't then provide the reader with a set of equations to implement the models. I couldn't see regression coefficients (for example) in any tables or supplementary materials. I apologise if I have missed these somewhere, but if that's the case then I would recommend better signposting. To me this is the main purpose of such a study – to allow readers to estimate hydraulic properties from simpler, cheaper measurements.

Within the framework of the CLIMASOMA project, in which this study was conducted, we aimed indeed at deriving new pedotransfer functions for saturated and near-saturated hydraulic conductivities. We undertook respective analyses that are published in the CLIMASOMA final report (chapter 6.5), which can be accessed here: https://cdn.curvenote.com/07ea3682-c7ce-4743-b274-dc105bd958f7/public/synthesis_report_v1.0.pdf

Since including the machine learning approach would have made the manuscript overly lengthy and unfocused, we decided to publish the presentation of the OTIM database together with exploratory statistics and meta-analysis as a standalone paper. We prefer to keep it this way and hope the reviewer agrees.

2) The figures caused me some issues, and will benefit from better labelling. Some figures I found to be illegible. I identify specific examples below, but taken as a whole the figures are currently confusing and do a poor job of illustrating some aspects of the results. This shouldn't be hard to fix.

We agree and will fix these issues in a revised version of this manuscript.

SPECIFIC COMMENTS

88-89: This may be true in mineral soils, but in peats we are starting to see pedotransfer functions for hydraulic conductivity with much greater explanatory power (r^2 up to 0.75). I'm sorry for citing my own work in a review, but the authors may wish to have a quick look at this, which like their work, also uses things like climate as a proxy for soil developmental processes: https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2022WR033181

We agree and will include a sentence making such a statement in the manuscript.

Line 160: …data were… (plural)

OK.

165: Superscript 2

OK.

Fig. 1: Better labelling required here. What is the broken, vertical black line? What is the difference between the orange and blue series? Please make sure the figures and their captions stand alone as readable entities without tiresome repeated reference to the main text.

OK.

199: is the double equality (==) a typo? Or is it intended to indicate some kind of equivalence relationship? This needs clarifying or respecifying.

OK.

212: …data were… (plural)

OK.

Fig. 3: Despite studying this figure carefully, I eventually drew a blank. Panel (a) is clear enough, a histogram showing the frequency distribution of K_h measurements at different tensions, split into two subgroups (focus and other). Panel (b) is unreadable – we have the same horizontal axis for tension, but the vertical axis is unlabelled. What are these black bars, that look like a Gantt chart? Panel (c) is also unreadable, again because the vertical axis is unlabelled. It's seemingly another frequency distribution, but we don't know what the categories are. All the focus measurements are in the top category, so this is seemingly important, but the reader (and this reviewer) can't tell what they are looking at. The caption doesn't shed any light. Please clarify what this is.

OK. We will modify the figure according to the reviewers' criticism in the revised version of this manuscript.

Fig 4. The panels for the categorical variables have been rotated through 90 degrees, so that the bars run horizontally, whereas all the continuous variables have vertically aligned histograms. Why? This makes it hard to compare between the variables, and doesn't

seem to serve any purpose. Suggesting rotating the four left-hand panels so they have frequency on the vertical axes, like the other variables.

OK.

Section 2.3: Use of harmonic mean here makes more sense to me than a geometric mean. Geometric mean is used to resolve the average of vectors with different directions (e.g., calculating the average *K* from measurements in vertical and horizontal directions), whereas here the measurements are simply directionless repetitions, so harmonic mean seems more appropriate. In the end it probably makes little difference to the result, but given that the authors have raised the issue they ought to justify their choice.

The geometric mean K corresponds to the n-th root of the product of all K. The use of the geometric mean is justified by the fact that saturated and near-saturated K are predominantly found to be log-normally distributed. We will justify our choice in a revised version of this manuscript.

319: “was” should be “were” (two items identified in the preceding list – the effect size and its error).

OK.

Fig. 5, 6, 7, 10: These all include insets without horizontal axis labels. The colours are the same as the scatterplot so the categories can be gained from that, but the reader is being made to work unnecessarily hard to make sense of what they’re looking at. A little labelling would help greatly. I think the histograms probably deserve their own dedicated panel with proper labels. Also, the tension ranges in the colour legends don’t have units.

OK.

Best wishes,

Paul Morris

We are thanking the reviewer once more for his constructive comments.

All the best,

John Koestel on behalf of the authors.