Editor

Dear Authors,
Your manuscript was evaluated by two reviewers who judged it with slightly different nuances, due in part to their different backgrounds. One reviewer gave very positive rates and basically put forward technical issues to be checked. The other reviewer, instead, was a bit more critical and raised a few questions that deserve in-depth discussions or may require changes.
From your responses during the discussion, I see room for improvement, especially for the benefit of a wider readership.
Therefore, I invite you to submit a revised version and, with a more global view of all comments and criticisms received during the discussion, a more detailed point-by-point reply to both reviewers. If you disagree with some comments, please explain why clearly.

We thank the editor for the favorable decision. We have added more detail to our replies to the reviewers where we found potential to do so.

Reviewer #1:

The authors presented a study evaluating the impact of soil management and climate on saturated and near saturated hydraulic conductivity measured by tension-disk infiltrometry. Therefore, the authors make use of an existing database and extended those by additional data published. In general, the topic well suits to HESS and has high relevance as the impact of climate and soil management on (mainly) saturated hydraulic conductivity has been discussed in recent papers but no such holistic analysis as those presented has been published yet. Even, as the authors pointed out, still climatic feedbacks on the (un) saturated hydraulic conductivity remain partly unresolved the results presented are a huge and important step forward. The manuscript is well written and structured and it was a please for me to read. I would like to get more articles in such an excellent shape on my desk to review. As the methodology is well described and the analysis is rigorous and detailed I would recommend minor revisions. Some minor points are listed below and some very minor ones can be found in the attached scan.

We thank the reviewer for the positive feedback. We have addressed carefully all the reviewers' remarks in a revised version of this manuscript.

Line 57: ..soil with larger near-saturated K tend to generate less water flow in macropore networks…Maybe I got it wrong, but shouldn't soil with lower near-saturated K generate less macropore flow. Or is this a question at which pressure head range you define macropore flow or near saturation K?

What we mean here is that at a specific irrigation rate (e.g. 5 mm/h), a soil with a higher near-saturated K will generate less preferential macropore flow than another soil with lower near-saturated K. This is because pores with larger diameters (and tensions closer to 0) are necessary to maintain the same water flow rate in the second case than in the first case. We have made this clearer in the revised version of the manuscript.

Line 65:….double ring infiltrometer methods….

Done.

Line 91:… and organic carbon as predictors for Ks.

Done.

Line 324: K100 should be introduced even if it should be K @ -100 cm I expect

We have now introduced K100 (and similar) at L196-198.

Line 332: …in the wet range… should be above 70 mm I believe as we are in the negative range.

Throughout the manuscript, we address the matrix potential in terms of tensions, i.e. negative pressure. The wet range then corresponds to tensions below 70 mm. We prefer to retain this convention. Nevertheless, as this clearly created confusion, we have now pointed this out explicitly at L195-196.

Table 4, 5 and 6: would be good to have the same colour coding for the Spearman rang correlation. Intuitively, I would use green as the best and red as the lowest but this is only a suggestion

We agree and have adapted the color coding of table 4 to the ones of tables 5 and 6 in the revised manuscript.

We also thank the reviewer for marking typos and errors in the sub- and superscripting in the annotated version of the manuscript. We have fixed them in the revised version of the manuscript.

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.

We intended this manuscript to present OTIM and illustrate its content. Including the machine learning approach would make 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.

Within the framework of the EJP Soil 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

While we are in general open to including the PTF part into the manuscript, we prefer to keep it out and rather publish a short paper featuring the results of the PTF attempts on this database as a companion paper, which we have now announced in the conclusion of this manuscript.

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.

Thanks for highlighting this. We have modified these figures and figure captions following the reviewer's suggestions.

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 have advised the readers of this fact.

Line 160: …data were… (plural)

Done.

165: Superscript 2

Done.

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.

Done.

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

Done.

212: …data were… (plural)

Done.

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.

See general reply to the figures above.

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.

Done.

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.

We do not agree. To our understanding, the harmonic mean would be correct to use to calculate average velocities from different velocities in series. E.g., the average K in a layered soil profile in which K differs with soil layer.

In this study, we want to express a representative value from a distribution of K. It may be imagined as averaging K in parallel. The geometric mean K is simply defined as the n-th root of the product of all K (Taagepera, 2008). It is equivalent to e to the power of the arithmetic mean of ln 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. The justification of using the geometric mean is given in L285-288.

Taagepera, R. 2008. 1209 Geometric Means and Lognormal Distributions. In: Making Social Sciences More Scientific: The Need for Predictive Models, p. 0. Oxford University Press.

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

Done.

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.

Done.

Best wishes,

Paul Morris

We thank the reviewer once more for his constructive comments.

All the best,

John Koestel on behalf of the authors.