Review report for: "Assessment of the Effect of Soil Amendments and A Three Phase Soil Water Retention Model"

In this paper the authors present a duel method retention curve evaluation procedure to evaluate the retention curve on a wide range of saturation degrees, and suggest a unified empirical model to evaluate the retention curve in the entire saturation spectrum. In addition, this paper presents an evaluation of adding a surface tension amendment to different soil mixtures with different fraction of clay to sand.

Here are my thoughts about this manuscript:

Readability: the paper is well organized and easy to follow. However, the paper includes some typos and many grammatical issues that are mostly annoying. In some cases, these errors reduce the intelligibility of the text. The reference list is not well organized (not uniform, duplicate entries, not all references are cited, etc.).

Response:

Thanks for the positive comment on the readability. We will be more careful next time when revise and edit the manuscript. Particularly, at the final proof-reading stage, we will more carefully double check all the highlighted issues thoroughly.

The **importance** of this study draws, according to the authors, from agricultural engineering in climatic unstable conditions. However, it eludes me how suction tension of more than 100MPa and saturation degrees below 10^{-3} are relevant for agricultural practices.

Response:

The evaluated water retainer product aims to prevent the soil pore water evaporation when exposed to atmospheric conditions. In practice, the water retainer is spread on the surface of cultivated field plots when doing irrigation. Cultivated plots are directly exposed to atmospheric humidity conditions in normal time. The Figure below shows the surveyed atmospheric Relative Humidity profile across Europe at a time in July. Under the RH situation, at the ground surface, soil pore water is subjected to a very high suction (calculated in term of the Kelvin equation, Eq. (1) in the paper, for the relation between the suction and RH) to evaporate fast. As the water retainer product can reduce the surface water evaporation under high suction, it can help to further reduce the water loss in deeper ground.

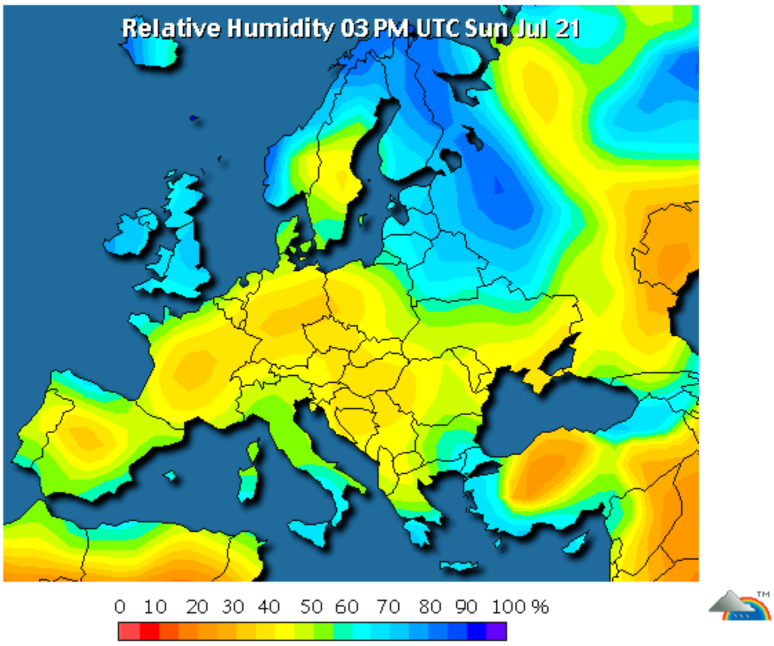

Figure: The atmospheric RH across Europe in July

Moreover, "improving" the retention curve according to this paper means more water in the soil, it remains unclear how unextractable high water content aids plants. The feasibility of adding clay and homogenization the soil mixture at any relevant agricultural scale is also not clear.

Response:

Regarding how soil water content aids plants and the effect of adding clay into soil on agricultural product are beyond this work and the expertise of the researchers/authors of this work. We Envisaged that the reported work could provide useful information to experts on farming practice.

The **scientific** quality and methods are inadequate. The *experiments* are conducted without repetitions. The different methods used to evaluate the retention curves at the different ranges directly affect the treatment (leaching vs evaporation affect the polymer concentration), but this is not discussed at all in the method section and partly mentioned in the results.

Response:

The test using HYPRO-2 for low suction range water retention curve (WRC) was repeated for couple of specific soil samples at start of the study. These starting tests justified that the HYPRO-2 was stable and produced repeatable results in these cases (we can provide the data in revision). As the HYPRO-2 test is time consuming, on the proved repeatability in these cases, not all the tests following-on were carried out with repetitions. However, for the Relative Humidity control tests for high suction range of WRC, each measurement was triplicated.

For the research, we tried but were unable to find a reliably effective consecutive approach through a single test to obtain the full range of soil water retention curve from fully saturated to nearly dry. This has been explained in the manuscript to justify the approach that we adopted. Both of the HYPRO-2 and RH control approach were to measure the drying process starting from initially fully saturated state, and the evaporation was controlled to be happening only at the top surface of the soil samples in containers. There is no leaching effect on the measurement. In the other words, for both approaches, the applied water retainer content remained no change, which justifies the consistence and compatibility of the two approaches for a certain soil water retention curve.

The type of clay mineral used is not mentioned. The porosity values of the different soil mixtures and the surface tension of the different polymer concentration are not reported.

Response:

The primary aim of the paper is to measure the effect of Water Retainer on WRC, so we didn't analyse the clay composition. On the other hand, we had no instrument to directly measure the surface tension of the Water Retainer solution, which need specific instrument and skills. So in this paper, we were only able to give qualitative discuss on the modelling results of the WRC measurement. It is on the point that we highlighted the advantage of the proposed 3 phase WRC model on the link to underlying physics mechanisms. This is a staged progressive research which opens new research topics that we will continue to study.

The *model*, is composed of a superposition of pressure elements of different process. It is based on a large number of strictly empirical constants. It is not mentioned how the parameters are fitted. The paper states that the model help understand the working mechanisms of the agents, this is not the case. The parameters are not discussed at all, and there is no attempt to correlate the parameters to the different soil properties while very week and non-monotonic effect of the amendment is presented. Despite the many parameters (9!), the model fails to satisfy the basic physical conditions at fully saturated and dry media.

Response:

The manuscript demonstrates the reliable performance of the proposed WRC model to represent the wide range of soil water retention curve from fully saturated to nearly fully dry. The proposed WRC model tries a novel approach to understand and represent the unsaturated soil water retention mechanisms based on classical interfacial physicochemical theory. On the underlying physics and mathematical derivation (there were more detailed report for the procedure in previous publication), all the parameters have their respective physical meaning. In this paper, we used the physicochemical WRC model to represent the effect of the water retainer effect in revised form by introducing an extra term with additional parameters. We acknowledge the reviewer's comment regarding the number of parameters involved. However, as a staged progressive research activity, we tried to provide a qualitative discussion to link the parametric values with the underlying physics. Certainly, more profound investigation and quantitative clarification are expected, but which need more elaborated experiments on wide soil types of varied composition nature, and particularly the study into material science, such as the interfacial energy characteristics of water on absorbents. Such kind of research is beyond the capacity and the range of this study. We really hope that the reported work is able to receive interest and open new research activities on the topic.

Finally, the **analysis** of the results is very limited. There are no recommendations on if/how to use the polymer. The usability of the model is unclear, how do the authors expect a practitioner to evaluate the different parameters?

Response:

The experimental and modelling results have suggested the optimised water retainer concentration, i.e. 3~5%, in the solution used for farming field watering.

We plan to add a numerical simulation in follow-on revision to demonstrate the implementation of the proposed model in hydrological modelling. We will conduct a 1D vertical ground water simulation when ground surface is exposed to varied atmospheric RH conditions (the boundary condition). In addition, we will provide additional experimental data of the water retainer effect on the soil water hydraulic conductivity and the surface evaporation rate.

There is no physical discussion on the parameters or the results. For example, what is the meaning of the alpha=beta observation? why all the fitting parameters have negative values? The fitted absolute large values of the parameters suggests very unstable and sensitive model which is unlikely to work for transient numerical schemes. Additional aspects that are not in the scope of this paper are not mentioned, such as the expected effects on the water flow and its distribution due to changes in the capillary forces.

Response:

These comments will be addressed by the planned additional work mentioned above.

Therefore, I suggest not to publish this work in HESS.

For future publication attempts, I attach some specific comments:

- Check typos and grammatical errors (for example):
    1. "They" instead of "The" in line 21
    2. "farce" in line 284
- Line 44 – Lemos et al. 2021 is not a good citation for this claim; I suggest you look for a relevant review.
- Line 47—Spitalaniak et al., 2019—also looked for the matric potential (i.e., a water availability indicator).
- Line 47 – Xerdiman et al., 2022 – investigated the construction of artificial soil on rocky slopes – not sure how this is related to this work.
- Lines 178-179 – 3 times increase compared to what?
- Line 180: "However, when the clay content is over 30%, the effect on soil water content increment becomes much less." It is not clear what you mean.
- Line 182: the difference in the saturation degrees is larger in high suction values, but the trend is more pronounced in the intermediate values (as expected).
- Line 183: The volume of surface film water is very small (what is the width of the film you are considering?). you don't need it. The reduction of pore sizes can explain this observation.
- What are the porosity values of the different soil mixtures?
- 2a – can you explain the non-monotonic behavior in clayey sand B for the high-suction? Using repetitions might help understand if this is an artifact or not.
- Line 190: I don't see this effect. In Fig.3a, clayey sand A, the saturation of the different treatments 3%>0%>5% for almost the entire suction spectrum, and in clayey sand B (Fig. 3b) 5%>0%>3% (up to the suction anomaly). Maybe if you use a linear axis, you will be more convincing. Also, repetitions and statistical evaluation may help.
- Line 216 – What do "inaccessible pores" mean? if they can be saturated, then why not drained?
- 4b is not mentioned in the main text. The proportionality of the residual "water" saturation to the initial WR concentration may suggest that the WR precipitated as a solid.
- 2: Remove one of the three equations (I think it is better to remove 2b).

- is S_s different than one? If not, it is redundant. Just put 1 in the equations.
- Line 259: is p_b different from p_v in Fig. 6? Please clarify
- Line 261: what are empty pore surfaces? The vapor-filled ones?
- 9, please clarify how the physical bounds at dry and saturated media are reached
- 7: the fitting procedure of the models is not explained. What are the values of the different constants, how do they relate to the clay and sand content and to the WR concentration?
- 8. It would be helpful if you kept the same colors as in previous figures for the different treatments. At the very least, keep the same colors for the different treatments in the plot (e.g., why do yellow markers confirm to the purple line and so on?)
- Line 352: The word "predicted" is used loosely. Have you forward estimated the fitting parameters?
- 10: What is the physical interpretation of the negative capillary suction values near saturation?
- 12: what about g?

- References: please revise the reference list. For example:
    1. You have two papers with the same authors and title that were written in the same year and published in the same journal on different pages (see Wang et al., 2008).
    2. Sometimes, you use a comma after the year and sometimes a dot.
    3. Not consistent italics for journal names.
    4. Not consistent "and" between authors (e.g., line 493)
    5. What is West Lebanon, NH (line 504)?
    6. Mikhail et al., 1968 not referenced.
    7. Monnier et al., 2010 not referenced.
    8. Also, in the text, you cite, for example, Wang et al., 2022 (in lines 55 and 57), but it is not clear to which entry in the reference list you are referring.

Response:

Thanks for the detailed breakdown list for the error, mistakes and inaccuracy in the manuscript. We will seriously address all these one by one in the revision. We would like to take this chance to appreciate the reviewer's insightful criticism and constructive comments.