# Author's Response to Reviewer 1

The authors would like to thank Dr. Vervoort for carefully reading our manuscript and providing constructive criticisms. In our response below we address the expressed concerns. The reviewer's comments are in **bold text**. Our responses are in plain text.

**This is an interesting paper which shows a theoretical mathematical development of a "weighting function" that can be used to describe and model the hysteresis as a result of soil hydraulic conductivity degradation and rehabilitation. The mathematical development is interesting and quite elegant, but the paper grossly overstates its significance when it comes to deriving the functions from real data.**

We thank Dr. Vervoort for his positive feedback regarding the model development. We respond to his concerns regarding the model's significance below.

**I believe that the paper requires a major review and needs to be stripped of most of the spurious claims about how this model can be used to model processes in real irrigation fields. The point I am raising is that the experimental data that is suggested to study the hysteresis of hydraulic conductivity suggests that the process is purely a physio-chemical instantaneous reaction that is governed simply by the concentration of the infiltrating fluid. I think this is true for a pure laboratory experiment with sieved soil under saturated conditions as described in the paper, but I don't think this is true in real soils, in real fields, under real climatic conditions. The impact of plant growth, drying and wetting cycles, carbon content and soil structure of the soil and many other factors that might impact the recovery of the soil after irrigation with saline/sodic irrigation water. In other words, the framework presented is an interesting theoretical framework, but I can't see the practical applications that are claimed in the paper. For example (line 300) the paper states: "This is a crucial step in improving our ability to assess the risk of soil degradation, because often degradation is triggered by seasonal patterns, which may only lead to significant declines in Ks after a number of years." Indeed, but your experiment takes about 3 hours (line 190). This means I also don't agree with the suggestion that the hysteresis curves should be measured more generally. As a result, I think the paper needs rewriting to better highlight the limitations to the research in a practical sense and to better highlight that this is a theoretical development. The major assumptions related to the development of the framework need to be better outlined in the methods (i.e. the fact that the application of the framework in the study is limited to laboratory measurements on packed columns). The difficulty to actually measure hysteresis in real field applications could also be reviewed. In fact, the paper could generally benefit from a much more critical discussion.**

(1)
We appreciate the concerns raised by Dr. Vervoort and agree that the paper can be improved by more carefully contextualizing the relationship between laboratory experiments and field conditions. In what follows, we suggest several changes so that the paper more clearly conveys the limitations of the presented framework. That said, we strongly believe that the laboratory experiments and model have real practical applications, as argued in the paper.

First, we would like to emphasize that the laboratory setup described in the paper extends upon the widely-accepted technique used to study soil degradation over the last several decades. Numerous papers, including the landmark Quirk and Schofield (1955) and McNeal et al. (1966, 1968) papers, and continuing to more recent work (Ezlit et al., 2013), have used column experiments to characterize a soil's susceptibility to degradation. The results from these experiments, and the empirical models derived from them, have likewise been used to study how saline and sodic irrigation waters may drive soil degradation in field environments. The well-known Hydrus package (Šimůnek, et al., 2013) implements the McNeal (1968) model for the feedback between soil water chemistry and hydraulic conductivity. Numerous studies have used the Hydrus framework to study soil degradation and rehabilitation (Šimůnek et al., 1997; Suarez, 2001; Reading et al., 2012; Mallants, et al., 2017; Shaygan et al., 2018). More recent papers (van der Zee et al., 2014) have integrated the Ezlit et al. (2013) model to study how saline and sodic conditions have the potential to drive soil degradation. The major difference between the model and experiments presented in this paper, as compared to those already published, is that we extend the experimental and modeling framework behind degradation to consider the question of rehabilitation, an important step forward in the study of how saline and sodic conditions affect soils.

Therefore, while we agree with the reviewer that there is a gap between laboratory and field studies, and that extrapolating results from laboratory studies to field conditions should always be done with caution, we also note that the presence of this gap is not new. McNeal (1974) addresses this very point, writing: "In most cases that can be visualized, changes in hydraulic conductivity with changes in soil salinity should be more pronounced in the laboratory than in the field, so that laboratory values include a safety factor when extrapolated to field conditions. This safety factor may partially offset any enhanced tendencies for soil dispersion during tillage operations." We further add, as the reviewer notes, that measuring hysteresis on a field level poses numerous difficulties. We believe this point further emphasizes the importance of models, which while surely not perfect, can help guide future field experiments, which are costly and difficult to undertake.

We suggest that the manuscript be modified to clearly present this nuance and the limitations of our modeling technique. Specifically, we propose the following changes:

The "Model Applications" section, on page 13, will start as follows:

> *In this section, we use the model to demonstrate how consideration of hysteresis is essential when analyzing degradation and rehabilitation in soils. While controlled soil*

*column experiments are expected to behave differently than field conditions, they still provide invaluable information regarding degradation and rehabilitation trends. More importantly, these experiments allow us to generate testable hypotheses on the interactions between soil and environmental conditions over the long run. However, the modeling formalism presented here is general, and does not depend on the idiosyncrasies of particular experiments. This is to say that, while we measure FORCs using soil columns, FORCs could also be measured with field experiments. The lab experiments described here are a first attempt to parametrize and constrain the model, and further lab and field experiments are necessary to ensure the model's applicability in real-life conditions.*

*At this stage, no experimental work has addressed the Ks hysteresis in detail. We incorporate results from the well-known McNeal and Coleman (1966) experiments to show how it is possible for soils with the same degradation patterns to exhibit very different levels of reversibility. [...]*

In the "Conclusions" section, we will modify the sentence that begins on line 312 as follows:

*[...] In this paper, we demonstrated how a soil's weight function can be determined from experimental data, namely the measurement of first-order reversal curves, or FORCs. We showcased how to parametrize the model with FORCs obtained from soil column experiments, but in the future field experiments could increase our understanding of rehabilitation mechanisms in a richer and more realistic environment, over longer time scales. Much more than the major loop curves [...]*

Finally, we would like to address what we believe were small misunderstandings in the experimental setup. The described experiment is not "**purely a physio-chemical instantaneous reaction"** and the experiment itself requires much more than three hours. The three hours noted by the reviewer (line 190) refers to the equilibration of the outflow for a single measurement point. As noted in lines 231-234, full characterization of a single soil's weight function would take approximately one month, while soils with higher clay contents may require significantly more time.

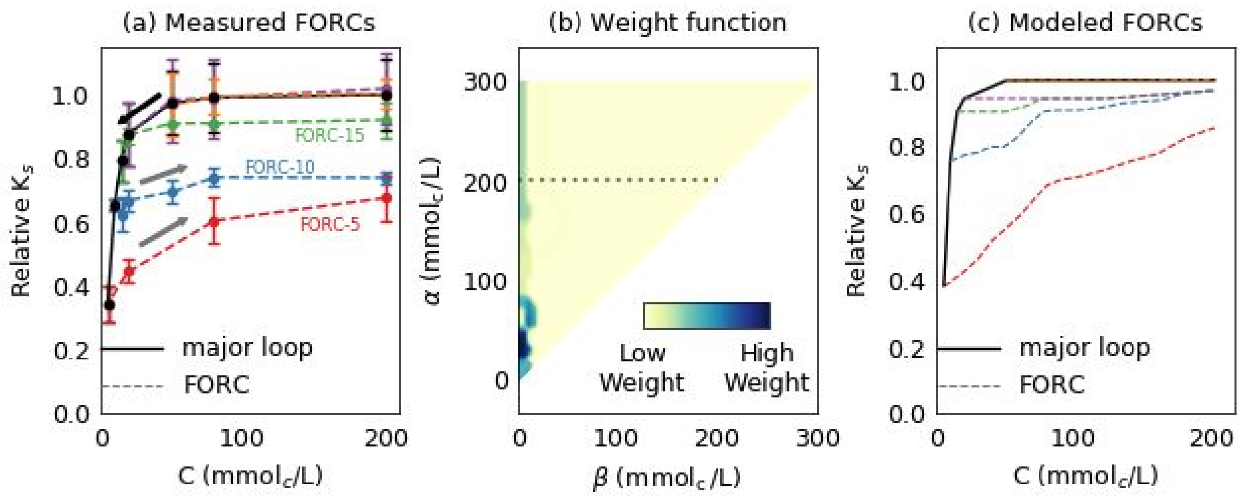
We will update the sentence that begins on line 190 to read, "The typical equilibration time for a single measurement point was three hours" to eliminate any confusion.

**There are further smaller issues: The last 2–3 paragraphs of the conclusions are really a discussion rather than a conclusion. These paragraphs need to be integrated in the discussion.**

(2)
We agree with the reviewer. We will change the name to this section to “Discussion and Conclusion” to better reflect its content.

**In appendix B you highlight that 3 replicates were measured for each hydraulic conductivity, but your hysteresis figures (Figure 4) show only single points. What was the spread in the measurements?**

(3)
An updated version of Figure 4 is included below. In this updated figure, we include error bars so that the spread of the measurements is clear.




**In my personal opinion, the paper is written too much as a promotion document and not really as a scientific study. For example, do you really need to state that you develop “the first model” (Abstract). For a grant application or a promotion, maybe yes, for a paper that seems overdone. And develop “novel” experimental results (Abstract)? Can you not leave that to the reader to decide? I also don’t understand the fashion to write everything as “our”.**

We believe that the concerns raised here are primarily stylistic. Particularly, mentioning that the model is the first for the effect of saline and sodic water on hydraulic conductivity that explicitly considers hysteresis is done in order to contextualize the presented model in relation to the literature. We believe this is an important point to emphasize, because several models have focused on how saline and sodic water affect hydraulic conductivity while ignoring potential irreversibility. We agree that the word “our” is used too often, and we will make modifications where we see fit.

**Please also note the supplement to this comment:**
**https://hess.copernicus.org/preprints/hess-2020-455/hess-2020-455-RC1- supplement.pdf**

Additional comments from PDF supplement:
*Note: The referee's comments in the PDF also pointed out some formatting and wording errors and made some suggestions. We sincerely thank the reviewer for catching these and will happily adopt his suggestions.*

**Page 2: Well, yes and no, what is the role of organic matter and the role of structure development through cracking and self-mulching? I assume you are excluding this**

Irreversible, in this sense, refers to the likely presence of hysteresis between the degradation and rehabilitation process. While we do not explicitly address developments such as cracking and self-mulching, we believe that even in their presence that changes in hydraulic conductivity, as driven by clay dispersion, are likely to be characterized by hysteresis.

**Page 3: Do you need this? seems like a conclusion for a future paper**

The highlighted sentence offers important contextualization and justification for the development of the presented model.

**Page 9: This assumes that the improvement of soil structure is a fast process, as I assume you have not run this over multiple years. In other words, your reversal is a very limited form, only changing the concentrations in the water.**
**Page 10: In other words, a very short time for a soil structure time scale**
**Page 13: I think the key conceptual issue here is that, while you mathematical development is interesting and elegant, I don't see a relevance for real irrigation problems, where interactions with organic matter and long resting times and plant growth between irrigation events. In addition, the switching between low C/SAR water and high C/SAR water would not be instantaneous (unless you have a heavy rainfall event just after an irrigation) and therefore there would be significant drying. This would affect the hysteresis pathways and this is not incorporated in your analysis and model. So this makes the development of the weight functions problematic.**
**Page 13: I agree it is essential, I am just not sure your model capture the process, it captures a very simple part of process that is really only happening in controlled circumstances**
**Page 15: But the big assumption is that real soil behaves like laboratory experiments, which is unlikely.**

We address these points in our response (1) above.

**Page 15: This section and everything below is discussion not conclusions**

We address these points in our response (2) above.

**Page 11: The data show single points, what was the spread in the replicates?**

**Page 20: If there were three replicates, why do your graphs only show single points? What was the spread in the results?**

We address these points in our response (3) above.

References

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