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Discussions

Interactive comment on "Hysteresis in soil hydraulic conductivity as driven by salinity and sodicity: a modeling framework" *by* Isaac Kramer et al.

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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. 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. 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. 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? 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”.

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

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