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Comment on hess-2021-298

Anonymous Referee #1

Referee comment on "Compaction effects on evaporation and salt precipitation in drying porous media" by Nurit Goldberg-Yehuda et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-298-RC1>, 2021

The paper reports experimental results of evaporation and salt precipitation from heterogeneous (layered) soil columns. The motivation of the study is to quantify effects of soil compaction on evaporation losses and salt precipitation. This is an important topic to understand water and solute fluxes in arid regions and for agricultural practice with irrigation. As analog of a compacted porous medium, the authors use a layered column with increasing particle size with depth. They motivate/justify this analogy with experimental results of an imaging study using micro-tomography that indicates smaller sand particle sizes (and pore distances) after compaction (increasing pore size from top to bottom as in layered column).

The authors reported a large effect of layering on evaporation losses in column experiments, with longer stage-1 duration for columns with finest layers at the surface. The authors reproduced these experimental findings qualitatively with simulations using Hydrus-1D simulations. The study contains many interesting elements (CT-imaging, lab-experiments and simulations) but these elements are – from my point of view – rather poorly connected and need some additional analyses as explained in more detail in the comments below.

MAJOR COMMENTS

1) The paper contains two studies that must be better connected; one study is focusing on imaging the effect of compaction on pore and particle size characteristics and a second one on evaporation and salt deposition from layered columns. The connection between the two parts is given by the hypothesis, that compaction results in smaller particle and pore sizes close to the surface with a profile of increasing pore size similar to the chosen layering. From my point of view, it would be important to connect these two studies by measuring evaporation from non-compacted and compacted sand columns (without layering). Could the authors conduct such experiments (not with the same sample used for measuring particle displacement, but packed with the same compaction method)? With

water and salt solution? These experiments could easily be conducted and connect the two parts of the study.

2) Layered columns as analog of compacted columns: my main concern is that conclusions on effect of compression (typically with sigmoidal or exponential profile of bulk density) are made using a layered system with a stepwise change in bulk density. In contrast to the macroscopic trend in bulk density reported in literature, the bulk density in the layered media is not decreasing with depth but the maximum bulk density (minimum porosity) was close to the bottom (see Table 1). Accordingly, the bulk density profile in the layered column is very different from the one expected for compaction. The expected trend of increasing pore size with depth may be partially represented by the chosen layering with increasing particle size with depth. However, the layering with sharp contrast of pore sizes will have other effects than a gradual change of bulk density (and pore sizes). For example, when the tip of the drying front crosses the boundary between fine and underlying coarse material, the capillary pressure jumps abruptly from a more negative to less negative value with rapid water redistribution from coarse to fine layer. Such abrupt changes are not expected for gradual changes in pore sizes. Accordingly, I don't know how representative a layered column is for a compacted column with gradual change of bulk density. Also from that point of view, the additional experiments proposed above would be important.

3) Numerical study: in Figure 5, the authors show that the addition of a salt layer on top of a homogeneous column stops stage-1 evaporation; but for the layered column, stage-1 evaporation can be sustained. It would be important to show the pressure evolution at the surface (or in profile) as a function of time to make clear why the capillary pumping will stop when adding a 'loamy sand layer' (as analog of the salt layer) in case of the homogeneous column but not the layered column.

To be consistent with the other lab experiments, the same simulations should be conducted with (i) the reverse layering and (ii) a gradual decrease of bulk density from top to bottom.

4) Effect of salt precipitation: based on the numerical experiments, the authors hypothesize that the deposition of salt affects the evaporation stage dynamics differently for homogeneous and layered columns. In the numerical experiments, evaporation stage transition occurred just after salt deposition for the homogeneous column; however, for the lab experiments, stage transition occurs much earlier (~15 hours based on Figure 7) than salt deposition (54 hours, page 23, line 508). But for the heterogeneous column, the stage transition occurred soon after salt precipitation (at 60 hours based on Figure 7, six hours after salt precipitation that occurred after 54 hours, page 24, line 517). Accordingly, the observations do not fully correspond to the findings of the numerical study. The authors should comment on that.

The discussion of the salt precipitation in the lab experiments is not consistent. With respect to salt precipitation in the homogeneous column, it is stated on page 23, line 508, that precipitation started after 54 hours. But on page 28, lines 611-612 it is concluded the

development of salt crust started after 10 hours. This is inconsistent. To agree with findings from the numerical studies, it is expected that a dry salt layer is built long before 54 hours (i.e. at end of stage-1). Do the authors have any experimental evidence (images) that salt was deposited after 10 hours?

For the heterogeneous column (FU), it is argued on page 29, lines 620-622, that the crust was hydraulically connected to the underlying medium. However, the stage transition (60 hours) occurred very soon after salt precipitation (54 hours, page 24, line 517) and it seems that liquid connections are not sustained very long through the salt crust. Accordingly, the conclusion that the hydraulic connection includes the salt crust could not be proven (page 30, lines 638-639). Or do the authors have experimental evidence (images) that there was salt deposition before 54 hours?

5) CT analysis: for the quantitative analysis of compaction, only five cross-sections per region were used to estimate grain number and area and mean pore distance. From my point of view, the change in porosity or bulk density is very important as well because bulk density is the property that is measured macroscopically (as described in the introduction). The authors should conduct an image analysis that shows the profile of porosity and bulk density (using all layers, not just five cross-sections per region) to reveal the effect of compaction with depth.

6) Grain breakage: in Figure 2 and text (page 16, line 357; page 18, lines 406-407) the term 'grain breakage' is used, indicating that sand particles break during compaction. For me it is not clear that sand particles break based on the presented analyses. For example, did the authors check in 3D if the particles shown in Figure 2a''' were really broken or if it is just a different arrangement of particles?

The quality of the applied particle counting method cannot be assessed based on figure 2 – an inset with higher resolution would be needed to show the performance of the grain counting method.

7) Conceptual model: The conceptual model presented in Figure 1 and described in the text needs some clarifications: (i) the authors should explicitly explain the difference between the drying front and evaporation front and the corresponding motion; (ii) the air entrance into the deeper layers must be explained (How does the deeper layers become unsaturated? I expect that there is air invasion in the non-compacted subsurface due to a large pore in the compacted region that brings air to the coarse layer.)

Based on Figure 1, it could be concluded that a different deposition pattern causes different evaporation rates for the two columns and that the dry top soil layer is limiting the evaporation rate. However, based on the numerical model, the drying of the salt crust and its hydraulic properties in contrast to the soil properties define end of stage-1. This could be clarified in the captions.

8) Evaporation stage transition: The point of evaporation stage transition is used in several analyses. How was this transition determined based on experimental data?

MINOR COMMENTS

Page 1, line 25: because the rates are the same in stage-1, I propose to write "evaporation losses", not "evaporation rates" that are higher in presence of small pores

Page 8, line 178ff: The authors should differentiate between motion of the evaporation front and drying front and discuss the displacement separately; otherwise, there may be some confusion. In this paragraph, the authors state that (i) in case of non-compacted soils the evaporation front moves downwards from the soil surface in transition to stage-2 and (ii) in case of compacted soils in stage-1 there is a reverse process with a continuous flow of water from the deeper layer to the surface. But both statements are true for both compacted and non-compacted layers: in stage-2, the evaporation front recedes from the surface for compacted and non-compacted soils and in stage-1 a continuous water flow from deeper layers to the surface is sustained for both columns. What is different, is the direction of the displacement of the drying front (interface between saturated and partially dry medium) that is downwards directed in non-compacted (and non-layered) soils but upwards directed in compacted/layered porous media.

Page 10, lines 225 and 234: Reducing the sample height of 30 mm by 2 mm does not correspond to an increase of 10% in bulk density (a decrease by 3 mm would correspond to 10%)

Page 11, line 260: Why did the authors choose different compaction values for CT (93%) and column study (95%)?

Page 11, line 264: How could it be ensured that the very same grains were found before and after compaction? Could the authors show an example how they could identify the same particle?

Page 12, line 282/283: The authors apply HYDRUS to solve water flow based on Richards equation that requires a continuous liquid phase with water content larger than 0.00 – the process leading to water content 0.00 is not simulated with HYDRUS. The justification of residual water content of 0.00 is not convincing.

Page 12, line 287: It is stated that n equal to 3 is the highest value permitted in HYDRUS. I made many simulations in HYDRUS with n larger than 3. Accordingly, I do not understand that statement. (by the way: in Assouline and Narkis (2019) HYDRUS simulations were conducted with much higher n values (Table 1)). Please explain.

Table 1: Please use greek symbols for water content; providing saturated conductivity values (and α and saturated water content) with so many digits could be misleading because the values are not known with such accuracy

Page 13, lines 300 ff: The authors state that HYDRUS is only valid during stage-1 evaporation with hydraulic continuity along the entire soil profile and limit the analysis to stage-1. Interestingly, in Assouline and Narkis, 2019, HYDRUS was applied for a similar set-up after stage 1 as well because "it has been assumed that daily averaged S_2 evaporation rates could be considered to be limited by liquid flow from deeper soil layers so that Richards' equation could still be applicable".

Page 14, line 306 ff: The authors should also specify the initial conditions of the added salt layer

Page 14, line 318: From a permeability in the order of 4 Darcy (one digit accuracy) it is concluded that the saturated conductivity is 376.32 cm/day (five digits accuracy) – providing a conductivity value with so many digits is misleading.

Figure 3: How is it possible that there are rectangles with 0 particles? Are there rectangles of 1.06 x 0.73 mm size without grains?

Page 20, lines 439 and 440: The statement that maximal compaction was measured in top parts and is in agreement with micro-scale study is not convincing: the maximum Δ_D was found in 30 mm depth, while for CT analysis the compaction was only measurable at top 7 mm.

Page 24, line 521: Did the authors observe salt deposition within the column for CU?

Page 24, lines 529-531: Based on Figure 7, the cumulative evaporation dropped from 14 to about 2 mm for saline conditions. This is much more than 50% as was stated in the text. Please clarify.

Page 25, line 551: Do you mean the drying front? When the evaporation front is receding,

the system is already in stage-2.

Table 2: What are the units?

Page 28, lines 594-595: S1 duration for DI set-up is about 65 hours (70 hours as stated seems a bit high) and this is quite shorter than 100 hours

Figure 8: The definition of the change is strange; should it not be 'evaporation_DI-evaporation_NaCl' divided by 'evaporation_DI' (multiplied by 100)?

Page 31, line 681: the drying front recedes from top to bottom in stage-1, not the evaporation front.