**Reply to reviewer #1:**

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.

We thank the reviewer for identifying the importance of the study. Following the reviewer comments we are editing and rewriting parts of the paper, in order to strengthen the connection between the two parts of the paper, as detailed below. Another column experiment will be done, upon the reviewer 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.

We agree with the reviewer that the two parts of the paper has to be better connected, and we liked the suggestion of the column experiments. A column experiment of "regular compaction" of sand column was conducted and results are being presented in Section 4.6. The results of this experiment are in agreement with the layered structure findings and indeed, this addition helps to correlate the layered structure experiments with realistic conditions of soil compaction in the field.
In addition, we changed the introduction section to include information about previous similar column experiments that were done in other studies. The differences between these works and ours are presented, but in general, the cited works supports our hypothesis and results.

For our own check - we conducted another simulation in HYDRUS for a domain with a gradual change of the soil hydraulic properties, and the results are in agreement with the experimental results and the simulation of the layered structure presented in the paper. In order to keep the paper simple, and since we consider the simulation only as another tool to prove the concept proposed by the hypothesis, we prefer not to add more simulations, under different setups, to the paper. We did mention in the text that a gradual change of the layers was tested by the model and that results were similar to the 5 layers structure (P16, L377-382)

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.

These are good points and we agree that the new column experiment will help to strengthen the notion that the layered structure is a good representative of a "normal compaction". The introduction, conceptual model and results and discussion sections where thoroughly changed in respect to these comments and others.

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.

The numerical model results section was rewritten (section 4.3, line 542-onward) and another image (Figure 7) was added. This Figure (Attached here below) describes changes of pressure, water content, and liquid flux along the soil profile at different times after the addition of the salt crust. This shows very nicely the moderate fall changes of these properties, for the compacted state, and explains the maintained hydraulic continuity between the moist soil and the salt crust, under compacted conditions.

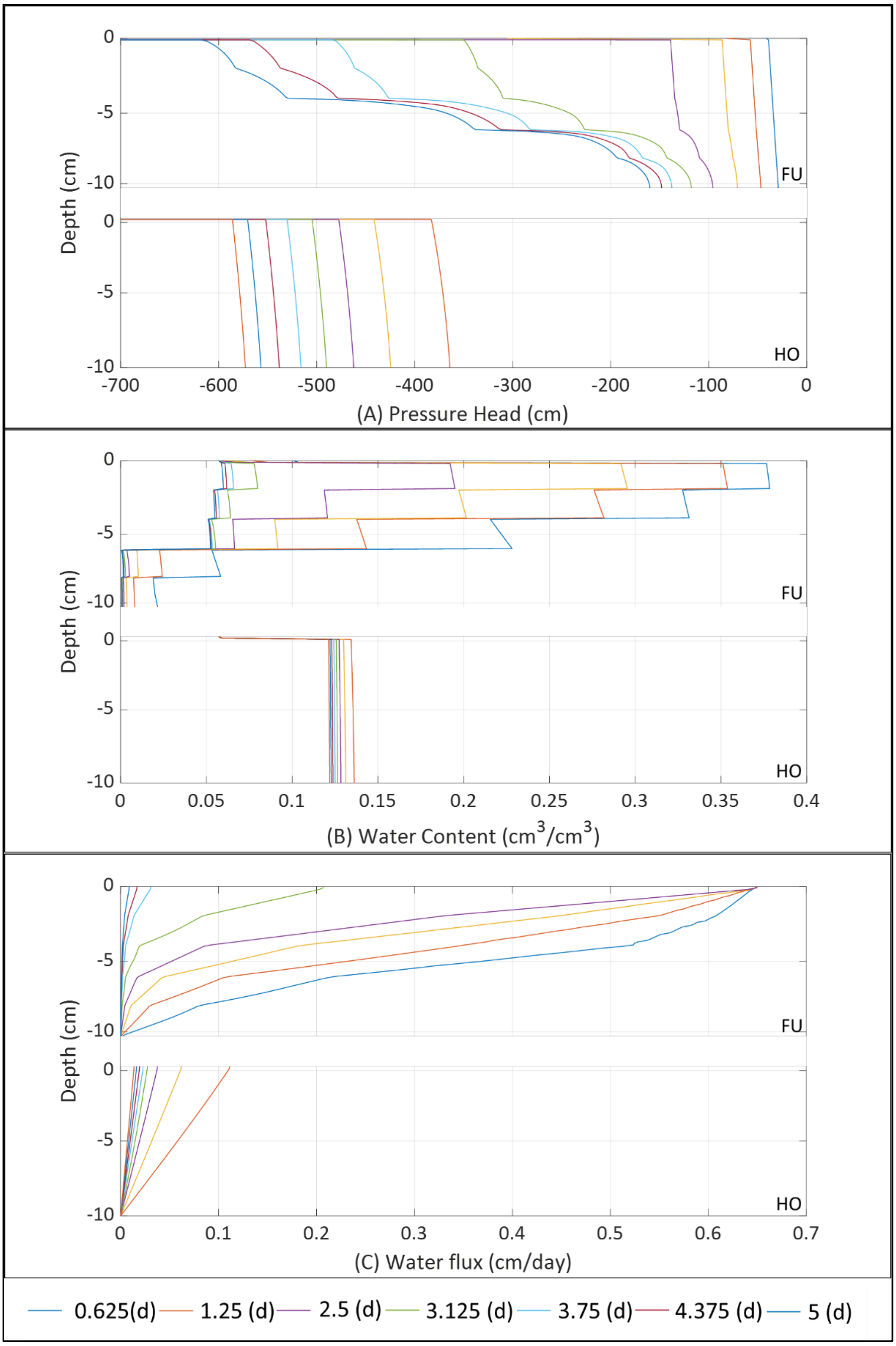



**Figure 7: Profiled simulation results of pressure head (A); water content (B); and upward water flux (C) for compacted (FU), and uncompacted (HO) scenarios. Results are for different time steps after the addition of the salt crust.**

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.

As detailed above, the numerical model is not the lion share of the work and is presented only as a proof of concept and used to strengthen the hypothesis. Therefore, and in order not to make the paper too long and cumbersome, we prefer not to present any additional simulation results. We believe that the added column experiments and the addition of more theoretical discussion to the paper, will contribute more to the understanding of the reader.

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?

We regret for this unnecessary confusion. The revised manuscript makes order in these disparities.

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.

The presented images are vertical transects and not horizontal cross sections (as understood by the reviewer). Therefore, we believe it gives a reliable information about the changes of the different properties, with depth. Apparently, at the length scale of the presented scans (~8 mm high) the changes are too small to be noticed. For this reason, we did the top scan (0-7 mm depth) and the deeper scan (9-18 mm depth), which allow to observe the differences in the soil physical properties between deep and a shallow samples.

In order to avoid similar misunderstood by the readers, this isbetter explained in the revised manuscript. See P18, L 424-428.

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.

A new Figure (Figure 2) was added to better explain and display visually the physical changes that the compacted sand went through, including reduction of pores size, and grains breakage.

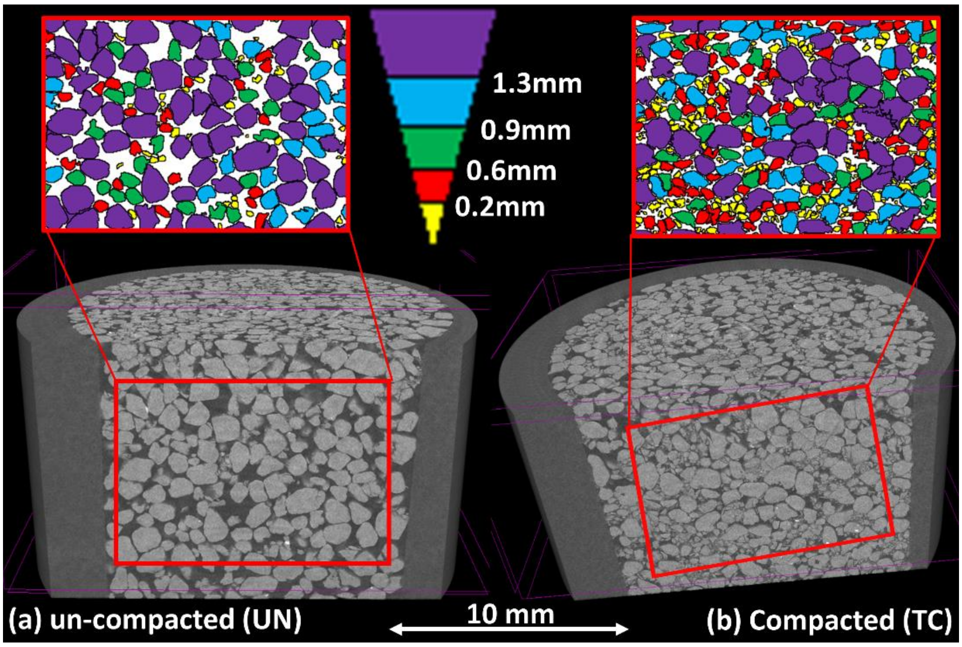



**Figure 2: 3D visualization of the top ~10 mm of uncompacted (a) and compacted (b) sand samples. Red rectangles exemplify the cropping of the 2D slices off the 3D structure. The 2D colored slices present, visually, the different grain size distribution between the compacted and uncopmacted samples. Grains sizes are in respect to the color-bar. In addition, it is seen in (b) that compaction reduced pores sizes, and increased grains angularity, due to breakage of the grains.**

As for Figure 2(C) (3C in revised manuscript) – the intention was not to allow the reader to count the grains. The purpose was to present the "color map" analysis that we used, which enables to observe visually the number of grain centers at each block of the generated grid. We rephrased and improved the text to make it more clear. In other words, the reader should be able to identify the different colors of the grid boxes, and not to count the number of grains within each box.

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;

The entire section was rewritten and the conceptual sketch was improved based of the comments of the two reviewers. See revised section 2. and revised image within:

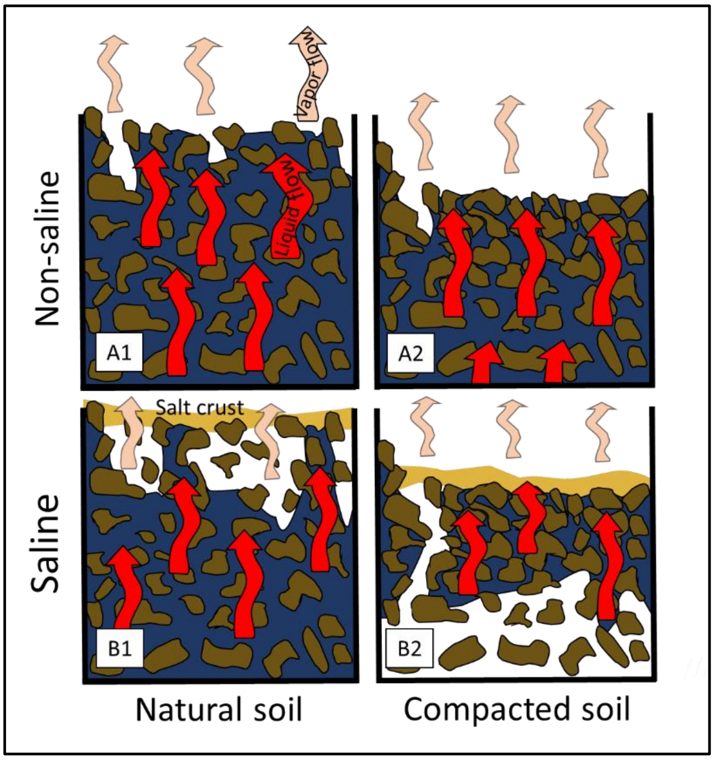



**Figure 1: Conceptual model of evaporation and salt precipitation under compacted and non-compacted conditions; (A) initial stages of S1 evaporation with the first air invasion into the matrix. (B) advanced stages of evaporation, where most of the soil is hydraulically disconnected from the salt crust for the non-compacted state (B1), whereas at the compacted state, most of the soil is hydraulically connected to the salt crust, thus evaporation front is at the salt crust upper surface (B2). Drying of the compacted media is from bottom to top, as liquid water is replaced by air that is flowing downward through the larger pores that act as air conduits.**

(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.

The reviewer is correct. The air has to come from some large pores at the surface. It is explained at the revised manuscript and seen in the revised conceptual sketch presented above.

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

See revised text at P 30, L 631-634, and revised Figure 9: "The transition times were determined by identifying the divergence of the curves from their tangents during initial stages of evaporation (see example for FU setup in Figure 9). The slope of each tangent line describes the initial (S1) evaporation rate of each curve."

MINOR COMMENTS

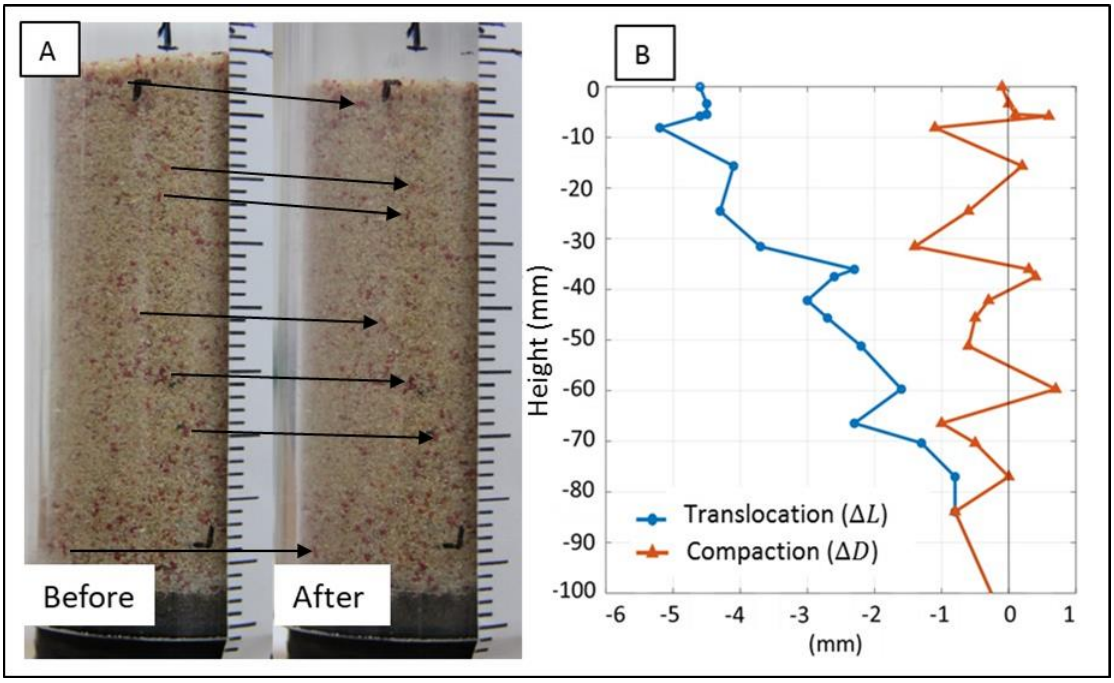
Page 1, line 25: because the rates are the same in stage-1, I propose to write “evaporation losses”, not “evaporation rates” that are higher in presence of small pores.
DONE

Page 8, line 178: 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.
We thank the reviewer for highlighting this point. It is important and this section was rewritten and clarified, as detailed above.

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%)
Changed to ~7%.

Page 11, line 260: Why did the authors choose different compaction values for CT (93%) and column study (95%)?
The two columns reacted differently to the compaction process.

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?
In fact, it was very easy to identify the grains by the naked eye. In the materials and method section we emphasized that it was a "visual analysis of the images". In the revised paper, at Figure 4 we added few arrows to demonstrate the movement of selected colored grains (see below).




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.

When evaporation is not involved, the residual water content of each soil is indeed the lowest water content that will be achieved by regular water flow processes. However, evaporation will take any soil (after sufficient time) to zero water content. Therefore, we used this value. In practice it is not a big difference from using the residual water content, as also for the residual state – there will be no flow of water when reaching the very low water content values. However, because we wanted to simulate evaporation with the ability to get to zero water content we used the value of 0. Similar approach was taken by Zhou, Šimůnek, & Braud (2021),we added a note about that at the M&M section (P14, L 324-328).

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.

This is true, but for the current combination of the soil physical properties, which many of them were defined experimentally and we couldn't "play with", higher n values gave relatively high errors, at the order of 6% (Relative error in the water mass balance of the entire flow domain).

We added a sentence saying: " …..n was taken as 3 for the uniform layers, as it was the highest n value permitted by HYDRUS, while keeping the relative error in the water mass balance of the entire flow domain, at low values at the order of 1% and below."

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

DONE

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 S2 evaporation rates could be considered to be limited by liquid flow from deeper soil layers so that Richards' equation could still be applicable".

Since HYDRUS is valid only for simulation of liquid water flow (and not vapor flow), it is impractical to use it when vapor flow plays a major role in the transport of water (vapor) from the evaporation front to ambient atmosphere. Assouline and Narkis stated this point exactly (and not as understood by the reviewer).

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

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.

True. Fixed.

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?

We understand the confusion. It is because grains are counted according to the location of their center mass of gravity. Therefore, it might be that a specific rectangle would be with no grains centers in it, therefore it would consider it self to be with 0 grains. In P19, L441 we change the sentence to say that it counts the grain centers.

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 delta_D was found in 30 mm depth, while for CT analysis the compaction was only measurable at top 7 mm.
We omitted the comparison with the micro CT analysis and changed the sentence to say: " This analysis further emphasizes the heterogeneous nature of soil compaction and the shear band effect, as different locations along the profile were more compacted compare to others. These differences are more notable at the macro scale, compare to the microscale observations, from the CT experiments. Nevertheless, it is evident that most of the profile underwent compaction, as most of the ΔD values are negative, and that maximal compaction was at the top ~30 mm of the sample.

Page 24, line 521: Did the authors observe salt deposition within the column for CU?
No salt crust was formed. Very minor crystals were observed in a very limited locations. It is now stated in the text.

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.
A mistake. Changed to say "more than 80%". The source of the mistake is that it was compared to the cumulative evaporation of the DI column, at the same time and not at the time of S1-S2 transition.

Page 25, line 551: Do you mean the drying front? When the evaporation front is receding, the system is already in stage-2.

Changed to 'drying'.

Table 2: What are the units?
As original data – [mm]

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
Changed. This change is part of a wider edit that we did for this results section.

Figure 8: The definition of the change is strange; should it not be ‘evaporation_DIevaporation_NaCl’ divided by ‘evaporation_DI’ (multiplied by 100)?
True – Changed

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

Changed

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**Reply to reviewer #2:**

The paper discusses the impact of soil compaction leading to a vertical gradient in hydraulic properties and grain size distribution on evaporation from soils, on the formation of salt crusts, and on the feedbacks between salt crust formation and evaporation dynamics. An interesting aspect is that the formation of a salt crust can have an additional impact on the dynamics of evaporation from saline soils, in addition to the impact of the osmotic potential on the vapor pressure. Depending on the hydraulic properties of the porous medium and of the salt crust, either the evaporation is reduced instantaneously when the salt crust is formed or the evaporation may be sustained at a potential rate for a certain time after the crust was formed (an aspect that clearly comes out of the numerical simulations which could be highlighted more). The timing of the reduction depends on the hydraulic properties of the salt crust and the underlying porous medium. The paper discusses in detail how the vertical variations (or gradients) in hydraulic properties of the underlying porous medium play a role in the evaporation dynamics and salt crust formation.

One part of the paper presents the impact of compaction on vertical gradients in grain and pore size of a coarse sand. Two types of methods are used: micro CT and macroscopic images of colored sand particles from which particle displacements and changes in bulk densities are derived. The CT images clearly show the effect of compaction on pore and grain sizes and that the compaction effects are larger near the surface but vanish deeper in the soil sample. Effects of compaction are also visible in the macroscopic images but unlike what the authors write, I do not see convincing vertical gradients in compaction in these images.

We wish to thank the reviewer for the detailed and constructive review.

We agree that at the macroscopic analysis, the compaction trend is not as neat as seen by the micro CT analysis. However, as detailed in the paper, there is a general trend of high compaction at the top of the column with a reduced effect downward. Highest compaction is observed at depth of 30mm. The text was changed to say that at the macroscale, the impact of the "stress chains" is more notable compare to the micro scale, therefore – the changes are not as monotonous as observed by the CT scans (P24, L 524-525).

A coarse sand is investigated and compaction of this coarse sand clearly led to a change in particle size due to a breaking up of sand particles. However, I am wondering whether compaction would have a similar effect on particle size in other texture classes (fine sand, silt, clay). Another aspect is that coarse sands are not known as soils where compaction has large effects on porosity. I would expect larger effects of compaction in silty and clayey soils. This could be addressed maybe in the discussion section.

This is a good point that deserves a note in the revised discussion section. Previous works have shown similar effect of compaction on evaporation, in fine texture soils. In these soils, it might be that grains breakage won't be seen, but reduction in pores size, will occur. Therefore – the overall effect would be similar. In the revised manuscript we noted about that at P38, L790-792, and P40, L 846-847.

In a second part of the paper, simulation and lab experiments are carried out to demonstrate the effect vertical gradients in soil properties on evaporation dynamics from saline soils (including salt crust formation). The impact of compaction on evaporation is demonstrated in these sections using a layered porous medium with layers with different grain size distributions with larger grain sizes deeper and smaller grain sizes closer to the soil surface (information about the bulk density is not given). The layered profile is then compared with a uniform profile that consists of a mix of grain sizes. This mix of grain sizes should represent the non-compacted soil. But, I wonder whether this is a consistent representation of the non-compacted soil. To be consistent, the authors should have used a uniform system that consists of the grains from the lowest layer in their layered setup. By using a mixture of grains, they generate a porous medium with a wider pore size distribution (as is reflected in the lower n value) and also a lower porosity (as is reflected in the lower saturated water content) which therefore has also a higher bulk density. The comparison between the mixed-uniform and layered systems is therefore not a suitable analogue for a comparison between compacted and non-compacted soil. The mixed uniform soil represents a medium with a wider grain size distribution and pore size distribution. According to the first part of the paper, this should be a characteristic of the top compacted layer but not of the non-compacted soil. Therefore, I think that authors should best include a new experiment with a uniform sample that represents the non-compacted lower soil layer.

We agree. Unfortunately, we do not have enough glass beads of the coarse texture to perform the suggested experiment. However, responding to the comments of reviewer #1, we added column experiment with homogeneous coarse texture sand (same sand used for the CT analysis), for compacted and non-compacted conditions. We believe this would be a reasonable solution for the point raised by the reviewer. See the new section 4.6 in the revised manuscript.

In general, the paper was well written. But, some parts should be described more clearly since readers might easily draw incorrect conclusions from certain sentences. For instance, the focus on the higher capillary suction that can be exerted by a fine top soil layer gives the impression that more water can be pulled up from coarser deeper soil layers than in case the fine textured layer is not present and that therefore more water can be evaporated when a fine textured layer is present at the soil surface. First, the authors should always make clear when they make comparisons with what they are comparing, i.e. what is the base case: the uniform fine texture layer or the uniform coarse texture layer. Often comparisons are made but it is not explicitly clear with what the comparison is made. In the list of detailed comments, examples of these comparisons are given.

The introduction, conceptual model, and discussion sections were rewritten, to improve clarity and better explain the complex processes discussed in the paper.

Second, the conclusion that more water can be pulled from underlying coarser layers when a fine layer is on top is incorrect (the authors do not write this but from what they write, one could easily draw this incorrect conclusion). The reason for larger evaporation losses when a fine layer is on top is that almost the same amount of water can be extracted from the underlying coarser layers and to this amount of water, the water that can evaporate from the fine layer can be added. The conceptual figure 1 does not really illustrate this but could be easily adapted to make this clear.

We are not sure we completely understand the reviewer comment. However, the entire paper was reedited and some parts were rewritten and we believe it would be more clear now. The conceptual model was better described and Figure #1 was changed to include the dynamics of drying front propagation (see comment #7, reviewer #1).

A second issue that should be explained better is the reason why the presence of the fine layer keeps the salt layer on top better connected to the deeper soil. The authors argue that it is related to the wetness of the fine layer that keeps the hydraulic connection. I think this should be rather the capillary pressure of the fine layer which is not too high (or pressure heads too low) so that the salt layer is not dried out too much and the water at the top of the salt crust stays hydraulically connected to the deeper soil. Therefore, the statement that the higher capillary pressure in the fine layer keeps the hydraulic connection between the salt crust and the deeper soil layers is to my opinion incorrect since it should rather be the opposite. In addition to showing the simulated wetness, I propose to include also simulated capillary pressure heads to make this clear.

This was raised by reviewer #1 also and it was improved through deeper discussion in the text. It is true that for the compacted conditions, the fine layer is more wet, with lower matric pressure, therefore water flow into the crust is possible. This is now better explained in the revised manuscript, by presenting simulation results that include information about pressure head changes at the media / salt crust (see reply #3 to reviewer #1).

Specific comments:

Ln 25: ‚comprised‘ The wording was not clear to me whether 'comprised' means an increased connectivity or decreased connectivity.

Changed to:"….due to the increase of hydraulic conductivity….."

Ln 53: Isn't the reason for compaction after tillage that soil aggregates slake after rain and the slaked particles create a crust?

This is true for the soil surface. However, in this sentence we are discussing the " the soil at the lower boundary of the tilled zone ".

Ln 54: Isn't that the plough pan?

Yes it is. We added this term to the sentence.

Ln 63: The uneven distribution of hydraulic properties does not necessarily lead to anisotropy. What determines the anisotropy is the shape, orientation of the heterogeneities.

Due to the lack of quantitative definition of the degree of samples anisotropy – we will omit it and use the term heterogeneity only.

Ln 111: manly à mainly

Done.

Ln 120: A precipitated salt layer may increase evaporation… : with respect to which conditions would the evaporation be increased? I am a bit sceptic that a salt layer can really 'increase' evaporation compared to the evaporation rates that would occur when there is no salt layer. Especially during phase I, when evaporation is mainly controlled by the available energy, I expect no big influence of the salt layer (even a reduction of evaporation since the albedo of the salt is higher than of the soil). I suppose the authors are referring here to the effect of the pore size distribution, which may be finer than in the underlying soil generating a higher capillary suction, on evaporation. The higher capillary suction in the top layer can lower the drying front in the underlying layer, but not more than by the thickness of the top layer. Thus, a thin top layer will lower the drying front only very little. The lowering of the drying front in the underlying layer by the thickness of the upper layer does not increase the amount of water that can be evaporated during phase I evaporation from the underlying layer. The increase in evaporation during phase I evaporation comes from the extra water that evaporates from the top layer. Thus, when this layer is very thin, there will be almost no effect on evaporation. This was also demonstrated by Li et al. (VZJ, 2020, DOI: 10.1002/vzj2.20049)

As explained in the text, this possible increase is a result of increase of the evaporating surface area, as the crystalized salt has a larger surface area compare to the underlying soil. For more information, please see Shokri-Kuehni et al., 2017.

However, we edited the sentence so it would be very clear that it might be correct in certain cases only and not always.

Ln 156: I agree that the higher capillary suction of the compacted top layer can pump up water from deeper in the underlying noncompated soil layer. But, this depth does not depend so much on the magnitude of the capillary suction that may be exerted (at least when it is above a certain threshold) by the compacted layer but rather on the thickness of the compacted layer. Furthermore, the depth from the top surface of the noncompated layer from which water is pumped up, does not increase. It is the depth from the soil surface that increases.

Dimensions of the compacted layer are important, but the fundamental property Is the ratio between the characteristic length of the porous medium and the thickness of the layer. In the revised manuscript we discuss the connection between the thickness of the compacted layer and the depth of the drying front, which are both affected by compaction. Please see the revised introduction and conceptual model sections.

Ln 181: 'where a continuous flow of water is sustained from the deeper layer of the soil profile to the soil surface, extending the duration of S1 and allowing more water to evaporate.' See comments above. First, it is important to explain with respect to which condition the duration of S1 is extended. When it is with respect to a fully compacted soil layer, i.e. without an noncompated layer below it, then I think it is not correct to state in general that an noncompated layer below a compacted soil layer leads to an extension of S1. When it is with respect to evaporation from an noncompated layer, then the formulation could be read as if the extension of S1 by the presence of the compacted layer on the surface is due to the fact that more water is extracted from the underlying noncompated layer during S1. But, I think this would be a wrong interpretation since the presence of the compacted layer on top of the compacted layer cannot increase the amount of water that can be extracted from the noncompated layer. The only reason why more water can be evaporated during S1 by the presence of a compacted layer (in comparison to an noncompated soil) is because additional water is lost from the compacted layer.

We hope we understood the comment correctly. The compacted layer results in that more water from the entire media are being pumped upward and participate in the evaporation process (S1). Without the combination of compacted layer over a loose layer, this effect wouldn't happen. The entire paper was revised to make sure this point is clear to the reader.

Ln 183: 'Consequently, the deeper soil layers dry out first, while the upper layers remain at relatively high levels of water content (Figure 1B(2)).' I agree. But, if the upper layers do not lose water during S1, then S1 will not be extended and there would not be more salt precipitating. So, I think there is a conceptual problem with figure 1 because the same amount of water was lost (and therefore the same amount of salt should be precipitated) in figures 1B1 and 1B2. I propose to include also some air in the compacted layer so that water is lost from figure 1Bmore is lost.

Figure was changes to include the process of air entry (see image above at the reply to reviewer #1). In addition, the text of the conceptual model section was improved also, in order to explain the important issue of air entry into the matrix.

Ln 187: 'In noncompacted conditions, the precipitated salt crust reduces evaporation as it acts as a barrier that reduces water vapor diffusivity from the evaporation front to the soil surface and to the atmosphere (Figure 1B(1)).' But this reduction occurs during S2 and would also occur during S2 in the compacted soil.

This section was rewritten, to better clarify that in homogeneous, non-compacted conditions, the salt crust is mostly with no hydraulic connection to the pore water, therefore it reduce vapor diffusivity. For compacted state, however, the liquid water flow through the salt crust is possible due to hydraulic continuity between soil pore water and the salt crust.

Ln 193: '…its impact on evaporation will be moderate compared to non-compacted conditions.' I think the impact is rather related to where evaporation is taking place, i.e. small impact when the evaporation takes place at the surface of the salt crusts but larger impact when the evaporation takes place deeper in the soil profile. Also in the compacted soil, the impact of the salt crust might be large during S2 whereas its impact might be small in the noncompacted soil during S1.

As stated above. This is better explained at the revised manuscript.

Ln 226: ‘with typical grain diameter of ~500 μm (sand characteristics can be found in Nachshon, 2016)’ What does the ‘typical grain diameter’ represent: the median, mode, ….? I propose to include also a uniformity index of the grain size distribution.

This is the mean. Text was changed to say that.

Ln 245: Which Matlab libraries or toolboxes are these functions from?

Image Processing Toolbox. Added to the text.

Ln 260: Shouldn’t it be ‘increased’ instead ‘reduced’?

True. Changed.

Ln 295 table 1: I would propose to include the hydraulic properties of the salt layer also in this table. The parameters are related to the particle size except for the saturated water content. Maybe a reason for this different behavior of the saturated water content with particle size could be given.

The parameters were added to the table.

Ln 324: I think that also fluid viscosity and surface tension are influenced by the salt concentration.

The sentence was changed to say that these properties may be affected also by the changes of the solution salt concentration.

Ln 360: grain sizes? Shouldn’t that be grain numbers?

True. Changed.

Ln 400: Figure 3: grain number, grain area and mean pore distance should have units. I do not understand the relation between grain number in Fig 3A and grain number in Figure 3B.

In Figure 3A, all parameters are normalized in respect to the non-compacted sample (as explained in the figure caption and the text). Therefore, it has no dimensions.

Figure 3B is not derived from Figure 3A. grains number in Figure 3A presents average of number of grains in a given surface area for the selected images. Then all averages were normalized in respect to the non-compacted state. In 3B however, the histograms present the number of the squares in the matrices (From Figure 2C).

This is explained in the text of the revised manuscript.

Ln 438: ‘Nevertheless, it is evident that most of the profile underwent compaction, as most of the Δð• • · values are negative, and that maximal compaction was measured in the top parts of the sample, in agreement with the results from the micro-scale study.’ I do not really see this in figure 4B. I would rather say that the compaction is heterogeneous but does on average not differ a lot with depth.

In accord with reviewer #1, the sentence was changed to say: This analysis further emphasizes the heterogeneous nature of soil compaction and the shear band effect, as different locations along the profile were more compacted compare to others. These differences are more notable at the macro scale, compare to the microscale observations, from the CT experiments. Nevertheless, it is evident that most of the profile underwent compaction, as most of the ΔD values are negative, and that maximal compaction was at the top ~30 mm of the sample. "

Ln 454: ‘…including the unique pattern of drying from top to bottom,’ Shouldn’t it be reverse: unique drying from bottom to top?

True. Changed.

Ln 474: ‘the presence of the salt crust resulted in hydraulic discontinuity between the saturated lower parts and the upper surface of the domain.’ I do not follow this statement. Looking at figure 5A2, the water content in the soil profile is quite uniform with depth so that the lower parts of the domain are not much more saturated than the upper parts of the soil column. It seems to me that the presence of the salt layer leads to a reduction of the effective hydraulic conductivity of the layered medium that consists of the salt layer and the underlying uniform soil.

Entire discussion of the model was rewritten

Ln 480: ‘The fine media at the top of the FU profile maintained wetness conditions that enabled liquid water flow from the soil into and through the salt layer, to replenish evaporation at its upper surface.’ I think the crucial point here is the capillary suction (and not the wetness) in the upper layer since that defines the suction and conductivity of the salt layer. I suppose that in the layered profile, the capillary suction at the top of the profile was lower (water pressure head was less negative) at the time of the initiation of the salt layer than in the uniform profile. As a consequence, the conductivity of the salt layer would be larger in the layered profile than in the uniform profile so that it could sustain S1 evaporation longer.

Entire discussion of the model was rewritten. Some more discussion about the pressure head profile was added.

Ln 565: please check the figure caption. The labels do not correspond with what is shown in the figure.

True. Changed.

Ln 575, table 2. The standard deviations should have units.

True. Added

Ln 639 'We suggest that, in the case of a homogeneous soil, the receding evaporation front breaks the hydraulic continuity to the salt crust.' I suppose that this depends on the hydraulic properties of the homogeneous soil and not on the fact whether the profile is layered or homogeneous. What would be the difference between DI and saline solution evaporation when the homogeneous soil would consist of the fine soil layer material?

From previous works it is known that under homogeneous soil conditions, at various textures, the salt crust usually act as a mulching layer that reduce evaporation. In most cases the salt crust is not hydraulically connected to the soil, as stated in this work, therefore it limits evaporation. The introduction, conceptual model, and discussion sections were thoroughly changed to better understand the findings of this work and the fact that was seen that compaction support a better hydraulic continuity between the soil and the salt crust.

Ln 690: 'This is attributed to the stronger capillary suction of the upper layers, at the FU structure, which pumps water from the underlying levels upwards, maintaining high saturation at the soil surface' I rather think it is the opposite (but I am not sure and it would be helpful to show simulated capillary pressures or pressure heads). The fine layers on top keep the capillary suction for a longer time at a relatively low level so that the salt layer does not dry out and connectivity between the evaporating surface at the top of the salt layer and deeper in the soil profile is not lost. In the uniform soil layer, the capillary pressures increase more (pressure heads become more negative) so that the salt layer dries out earlier and the hydraulic connection between the top surface of the salt layer and the underlying soil is lost earlier and evaporation reduced earlier.

Entire section was changed.