

1 Response to major comments

Considering the previous studies on this topic, it is difficult to determine where this manuscript is taking the readers? In other words, it is not clear to me what were the main development and advantages of this work compared to the numerous previous studies related to the liquid, vapor and salt transport in unsaturated zone? The authors must clearly clarify the steps they made in this manuscript.

We must stress that this comment is exactly the opposite of referee 1, who praised our courage for attempting to model such a complex system. We tend to agree with Referee 1. In fact, no one has modeled before the full suite of processes involved in this experiment. As we mentioned in the introduction section Wheeting 1925, Scotter 1974, Nassar&Horton 1989, and Scanlon 1992 studied this kind of problem using experimental data (without numerical simulations). Other authors (Jackson 1974, Scanlon&Milly 1994 and Boulet 1974) used numerical models to analyze a similar problem but without considering the role of salinity and the ones who did consider salt presence (Nassar&Horton 1989 and Nassar 1992) did it assuming diluted solutions, which is not sufficient to compute vapor and liquid pressures properly (Burns 2006). The work we are presenting here is the first to analyze all the processes taking place during evaporation from a salty soil under non-isothermal conditions simultaneously and comparing experimental data with numerical results. Our model is also the first to describe the dilution process due the condensation of a downward vapor flux, showing its evolution, magnitude and the mechanisms that account for it. Moreover, in this work the effect that salts precipitates and the heat boundary conditions have on the vapor fluxes are shown. We also present an accurate description of the evaporation front (width, depth, magnitude and evolution).

The point raised by referee 1 was that it is difficult to ensure that all our processes are properly represented. He/she may be right, in that we have made several simplifications. We contend that since our model reproduces the basic state variables (temperature, water content and salinity) we are allowed to investigate the processes. However, we agree that it can be disputed (and we have qualified our conclusions in the revised version of the paper). In short, we do not agree with referee 2.

Page 9, line 5-15: The described process is not physically correct, though by adjusting some fitting parameters, you might have obtained a reasonable agreement. . . Why do you set the enhancement factor 1.2 in the top 1.5 cm and 8 below? Is the top layer dry? If this is the case, then why just the Fick's law of diffusion is not enough to predict the diffusive flux (the enhancement factor was originally introduced to use in the case of partially wet conditions and it approaches 1 under dry condition)? On top of that, why do you consider two layers? It is well established (experimentally, analytically and numerically) that when there is liquid continuity between the receding drying front and the evaporation surface, liquid vaporization occurs at the surface resulting in preferential salt deposition toward the surface. You can describe salt distribution and deposition patterns by evaluating the competition between the diffusive and convective salt transport via Peclet number. I do not want to put words in your mouth, but there are some recent papers addressing this very exact problem without invoking any fitting parameters (see e.g. Huinink et al., 2002; Pel et al., 2002; Sghaier et al., 2007; Guglielmini et al., 2008 ; and Shorki et al., 2010).

As we answered to the referee 1, your questions come from an unclear explanation in the text. Like you said, we don't need this second material to simulate the processes (nor the vapor fluxes neither the salt precipitation) from the experiment. Indeed, these two materials are a single one. The only difference between them is the tortuosity factor (also called gas diffusion enhancement factor because it is larger than one). The reason for this distinction is to be able to modify some properties in the upper zone of the column. As the numerical model underestimates the amount of precipitates in the soil, we have changed the tortuosity coefficient (τ_0) value in the firsts 1.5cm to make up for this lack. In this way we reduce the vapor diffusion near the column surface, simulating more salt precipitates (salt crust formation) and reproducing more accurately the experimental data. We have modified the text to clarify this and we also included some of the suggested references in the revised version.

Page 10, line 22-27: All of these observations are expected and have been described in the references mentioned above. Besides, salt concentration does not "drop sharply". There is no discontinuity in salt distribution. If you had a lower Peclet number, the concentration profiles would decrease more gradually, because diffusion would be more comparable with convection...

The expected observations are the ones that take place above the evaporation front but

not the ones below. Values lower than the initial concentration during an evaporation process have been reported before only by Scanlon et al. 1992. Gran et al. 2011 also measured it experimentally and added an interpretation of this data presenting a water separation process conceptual model. In the present work, we modeled the experiment adequately to know where and when these processes occur and what the controlling factors are. We also quantify the fluxes and show their time evolution. Furthermore, we consider that a drop of two orders of magnitude in less than 1cm depth (see Figure 3) is a sharp drop. Of course we took into account both diffusion and advection, resulting the latter to be the dominant one (see reactive transport section for more detail).

Figure 1: It is well established that during stage 1, liquid vaporization occur at the surface (i.e. the vaporization plane is pinned at the surface). So, the illustrated conceptual picture is correct only if the authors refer to the stage 2 evaporation (which is not the case, since the columns were initially saturated).

Yes, the referee is right in that, the columns were initially saturated and the figure doesn't describe the firsts hours. During the rest of the experiment the columns were not saturated anymore which is the period that we analyzed and that is what the figure displays. You can see how in Figure 5 evaporation takes place below the surface from the first day. Therefore, Figure 1 represents the situation that occurs during most of the experiment. We have modified the caption of the Figure 1 to clarify that.

MINOR COMMENTS

Page 5, line 3-12: During evaporation from porous media, dissolved salt in water is transported by capillary liquid flow toward evaporation surface where it accumulates, whereas diffusion tends to homogenize concentrations in space. You must appreciate the salt diffusive transport and not just talk about advection.

As we said before when answering to comment *Page 10, line 22-27*, we already considered diffusion in our numerical model resulting to be less relevant than advection but still it has been modeled. In fact, one of the surprises in the model and in the experiments is the sharpness of the salinity profile. We have included in the text the missing data ($D = 10^{-9} \text{m}^2 \text{s}$).

Besides, does the evaporation front mean the interface between the saturated and unsaturated zone or do you mean the interface between unsaturated and dry zone?

The evaporation front is where water vaporizing occurs. As it turns out, it coincides with the area where saturation falls below residual saturation.

Where was it shown that the front is very narrow? What does “very narrow” mean quantitatively? The width of the front depends on the pore size distribution of the porous medium and can be quantified by the Bond number.

As can be seen in Figure 5, the width of the evaporation front is no more than 1 cm during all the experiment.

Page 5, line 18: How do you simulate the changes in water activity for high salt concentration?

To simulate high concentrations we use Pitzer (1973) equations and water activity is computed from the osmotic coefficient (ϕ , Felmy and Weare, 1986): $\ln a_w = -\phi MW$ where W is the molecular weight of the water and M is the total mol of solutes. We have included that in the revised version.

How did you obtain equation 7? (and by the way, what is $S_{0,min}$?)

Equation 7 is the relative permeability function obtained by the method proposed by Mualem (1976):

$$k_{rl} = k_{rl}(S_e, \lambda) = \sqrt{S_e} \left(1 - \left(1 - S_e^{1/\lambda} \right)^\lambda \right)^2$$

Note that no new parameter has been introduced, we simply renamed the parameter S_e to S_{ep} to represent effective saturation corresponding to the permeability curve.

S_{min}^0 is the degree of saturation from which it is assumed that the liquid permeability is zero (also called residual saturation). The text and the equations in the revised version haven been modified to make it clear.

In one hand you talk about pore scale processes (e.g. page 8, line 6-7: “water isolated in the meniscus that can not flow”), in the other hand you use a set of equations which are for macroscopic description...

We use a continuum mechanics porous medium approach. However, we acknowledge that detailed descriptions of some processes may require pore network, statistical

mechanics or even quantum mechanics approaches. We do not think there is a contradiction in acknowledging the complexity of some processes, yet seeking a simplified formulation (it is already complex as it is).

Page 15, line 10: What exactly are the new insights?...

We think we answered that question in the first response to the referee comments. Nevertheless, all the points mentioned in the conclusions section are new insights.

Figure 3, salinity profiles: what is the elapsed time for the experimental data?

As we said in our work (P532, L22-28) columns were dismantled at the end of the experiment after 12 days (note that the sampling was destructive). To obtain time evolution data two series of four identical columns were set up and dismantled at 4 different times during the experiment: after 2, 4, 5 and 12 days. More details can be found in Gran et al. 2011.

References

Felmy, A.R. and Weare, J.H. (1986), The prediction of borate mineral equilibria in natural waters: Application to Searles Lake, California. *Geochimica et Cosmochimica Acta*, 50(12): 2771-2783.

Mualem Y. (1976), A new model for predicting the hydraulic conductivity of unsaturated porous media. *Water Resour. Res.* 12(3): 513-522.