

## 1 Response to general comments

*First of all, I wish to express my sincere appreciation for the author's courage and competence to model such a complex experimental system. However, in the current state, the numerical simulations seem to be incompletely described which makes it quite hard to understand them in detail. Furthermore, the simultaneous occurrence of a multitude of processes makes the studied system so complex that the question arises whether it is really possible to discriminate the various processes and their influence on the simulated system response. Directly related to this is the need to improve the description of parameter estimation by inverse modeling. As noted by the authors (P540 L19.21) 'all parameters influence all processes'. I would therefore acknowledge if the issues of parameter identifiability / parameter interaction were studied and reported in more detail. In the current state, there is a great danger of ambiguity with respect to parameters and processes, i.e. parameters may not be identifiable and processes may not be distinguishable. Of particular relevance in this context is the parameterization of the soil hydraulic parameters which is based on strong assumptions and the treatment of the vapor diffusion enhancement factor. I will give more detailed explanations below.*

We wish to start this response by thanking the reviewer for his/her work. It is clear that he/she has gone into depth in reviewing our work. These criticisms have been helpful in improving the paper.

The general criticism is that our paper does not describe sufficiently the parameter estimation procedure, the distinction between processes and, in general, the description of the simulation. The reviewer contends, and we agree, that all parameters may not be identifiable because the interactions between processes are intricate, which makes hard to discriminate the effect of each process on the simulated system response. Actually our goal was just the opposite. We agree that the problem is complex from a phenomenological point of view and that all the processes (fluid flow, liquid and gas, heat transport and condensation-evaporation, precipitation, dissolution, etc) interact in subtle manners. However, the experimental set up is very simple and the materials are homogeneous and well characterized. Therefore, we modeled the experiments with virtually no calibration. Most parameters were evaluated independently (permeability, retention curve, porosity, heat conduction, etc) or were derived from literature. As a

result, the number of free “fudge” parameters was lower than usual. We only had to adjust the albedo and air transfer factor, the vapor diffusion enhancement factor and the energy loss through the walls. Since the model reproduces the basic state variables measured in the experiment (temperature, water content and salinity), we are allowed to investigate the system. We analyze simultaneously the processes that occur during evaporation from a salty soil under non-isothermal conditions and we compare experimental data with numerical results. Furthermore, we present a precise description of the evaporation and condensation processes (width, depth, magnitude and evolution) and we show the effect that salts precipitates and the heat boundary conditions have on vapor fluxes.

We agree that some information was missing and that some terms and explanations were unclear. During this revision process we have put our efforts in fixing the mentioned problems and in clarifying the doubts that you presented. We are of the opinion that our paper is now clear and precise, but to the point at the same time, and that this work is now a useful tool to help the interested reader to better understand the evaporation and water separation processes that occur in a salty soil under oven dry conditions.

## 2 Response to specific comments

Comments to the Author

*P531 L19-21: I suspect that the soil texture or more precisely the soil hydraulic properties exert an influence on the nature of the evaporation front. Am I right?*

Yes, you are right; the soil hydraulic properties affect the nature of the evaporation front, but not the sharpness of this front (as far as we have seen in our work). We have made the appropriate changes writing “*sharpness of the evaporation front*” instead of “*nature of the evaporation front*”.

*P532 L4: 'Second, under hot conditions...' – the adequacy of the concept of residual water content has been questioned before not only for high temperatures but for dry soils in general. I think the statement should be corrected.*

Our experiment was developed under hot conditions (promoted by an infrared lamp above the column) which lead also to dry conditions. We propose to modify the text to include both “*hot and/or dry conditions*”. We also said so in P535 L7.

*P532 L10: 'oven dry conditions near the surface must be acknowledged' – is it really certain that water potentials corresponding to oven-dryness are reached at the soil surface? How can we know it as long as we are unable to measure it?*

The point is well made. We clarify in the revised version of the paper that by “oven dry” we mean saturations below residual saturation. That is, we do not mean the level of dryness reached at an oven at 105°C for 24h. For one thing, that level of dryness needs not be a particular point, as it would be sensitive to soil salinity and moisture within the oven which are rarely ever controlled. Actually, we do know that the columns did not achieve the level of dryness during the experiment. When we dismantled the column, took the samples and put them in the oven, we observed that  $\text{MgSO}_4$  precipitates went from starkeyite ( $\text{MgSO}_4 \cdot 4\text{H}_2\text{O}$ ) to kieserite ( $\text{MgSO}_4 \cdot \text{H}_2\text{O}$ ) losing 3 molecules of water. So, for water fluxes purposes, we can say that our columns were absolutely dry. We have expanded this discussion in the revised version of the paper.

*P532 L20: please indicate the concentration of the epsomite, in the article by Gran et al. (2011) two different solutions are distinguished.*

We thank the referee by showing us this oversight, the text has been changed to include that missing information (14g/kg).

*P532 L22: the interested reader may want to know why the experiments were stopped after the overall saturation fell to 0.32.*

We fixed this value, after preliminary tests, to ensure a well developed crust and a deep evaporation. We have added an explanation in the revised version of the paper.

*P532 L 25: 'some identical columns' – please indicate the exact number of replicates*

We have included in the text the number of replicates made for the temporal evolution analysis: “Two groups of four identical columns were dismantled at different times to obtain the time evolution of those profiles.”

*P533 L3: 'Results displayed...' – this seems misplaced here. I suggest to either move it to the results section or to describe it in the introduction since these results have been published in your JH article. Alternatively, rename the section.*

We agree that the use of this sentence 'Results displayed' is not appropriate here. As we are not showing results but the conceptual model that was deduced from the preceding JH paper. We have changed the sentence in the text to correct this.

*P534 EQ (2): why is it necessary to consider air flow in the model? Why do you need a liquid air flux? Why do you need a source/sink term for air? State briefly which processes are responsible for the source/sink term of water (you mention it later on page 537, top). If the movement of air is accounted for, what causes the pressure gradient responsible for it? I think this part is not easy to understand for the reader. Note that the flux equations are not part of Table 1 which makes it even more difficult.*

Our goal in this section was to present the equations used in the thermohydraulic model in a general way just to give an idea about how the code works. We didn't want to enter too much inside this description because it is widely described in other works (Olivella et al. 1994). Nevertheless, we have made some changes in the text to clarify the source/sink terms of the equations and we included the cited reference to the interested reader.

We also want to answer the specific questions that you made:

*Why is it necessary to consider air flow in the model?*

Because the gas advective flux is small but not zero. At one point, we conjectured that advection might have been an important water flux mechanism. As it turns out, the model suggests otherwise, but this is a model result.

*Why do you need a liquid air flux?*

For completeness. Thinking on (dry) air as a component, since it dissolves in the liquid phase, we have to consider the dissolved air fluxes. As it turns out, they are very small and could be neglected.

*Why do you need a source/sink term for air?*

Air is conservative in this experiment, so there is no need of a source/sink term. Then in our case  $f^a = 0$ , but we left it in the text because we are showing the general code equations.

*If the movement of air is accounted for, what causes the pressure gradient responsible for it?*

The gas pressure is the sum of two terms, the air pressure and the vapor pressure. What we have is the air pressure fixed as the atmospheric on the boundary plus the boundary condition for vapor. The vapor pressure at the top of the column is controlled by the laboratory conditions. Inside the column, right at the evaporation front the vapor pressure increases. This rise generates a vapor pressure gradient between the front and the surface and also between the front and the bottom of the column. The overall result is also a small gas (air + vapor) gradient. We must acknowledge that, at this stage, we are pushing the model to the limit. This is why we did not want to discuss it in detail. Proper distinction between air and water gas fluxes would have required adopting the Dusty gas model, which would have added a significant degree of complexity into the model (recall response to general comment). We will mention it in the revised version of the paper.

*Page534 L14: please state explicitly what causes the internal/external supply of energy, i.e. mention phase changes or whatever is meant.*

As the evaporation-condensation is already taken into account on the left side of the equation, this term refers to the boundary conditions at the top and at the walls of the columns. Since we treat the columns as 1D, the latter becomes an “internal” sink of energy.

We have included in the text an explanation: *an internal/external supply ( $J m^{-3} s^{-1}$ ) that accounts for boundary conditions at the top (i.e. heat entry from the lamp) and at the laterals (i.e. heat exit through column walls).*

*P535 L8-16 and Table 1: It does not become clear how the parameters of the soil hydraulic functions  $\lambda$ ,  $\alpha$ ,  $P_0$ ,  $P_c^{dry}$ , and  $S_{min}^0$  were determined because it is described further down. The manuscript should be reorganized as pointed out below.*

We appreciate your comment as that was unclear in the text. We obtained the retention curve experimentally and then we adjusted the values of the parameters using a numerical model simulation. We have included this information in Table 1.

*P535 L17: note that  $S_i$  can become negative if  $P_c > P_c^{dry}$  if  $P_c^{dry}$  is set to a value smaller than 1000 MP. How do you prevent this in the numerical solution? Is the capillary pressure at the top of the soil limited to values smaller than  $P_c^{dry}$  ?*

Yes, that is correct, but this condition is not foreseen because in our experiment the suction was always below 1000MPa.

*P535 L21: ‘ $\alpha$  modifies somewhat’ – please be more precise about the way this parameter changes the retention function. In comparison to the Fayer and Simmons model  $\alpha$  is an additional parameter. Since it multiplies  $S_{min}^0$  it is not clear why it is needed at all. Note that  $\alpha$  is not used in the conductivity function which means that the two hydraulic functions are decoupled. Why is this necessary and on what information are these decisions based?*

Parameter  $\alpha$  scales the transition from the van Genuchten curve to the oven dry region of the retention curve. Its value may be chosen as  $\alpha = 1 / \ln (P_c^{dry}/P_c^0)$ , where  $P_c^0$  is the capillary pressure that coincides with the  $S_{min}^0$ . It should not be chosen smaller than  $P_0$  (or else,  $P_c$  will remain low for  $S_1$  much smaller than  $S_{min}^0$ ), nor very large (or else, the oven dry branch will separate from the van Genuchten branch for  $S_1$  much larger than

$S_{min}^0$ ). We adopted here  $P_c^0 = 0.025\text{MPa}$ , for which  $\alpha = 0.1$ , after some trial and error attempts. We have made the appropriate changes in the revised version to include this explanation.

*P536 L8: In Figure 2 the difference in the hydraulic conductivity functions does not become evident because the axes limits are not properly chosen. The most interesting part is for saturations smaller  $S_{min}^0$ . Please correct.*

We agree that the differences are not evident and we have changed the figure to include only the permeability curve that we are using (which is zero for saturations smaller than  $S_{min}^0$ ), which reflects that liquid fluxes are taken as zero under oven dry conditions.

*P536 L5-8: Setting the conductivity to zero for  $S < S_{min}^0$  is a very strong assumption. It seems to be chosen to guarantee the development of an evaporation front in the numerical model because with zero capillary conductivity, vapor flow is the only flux which can occur. Is there any experimental or physical justification for this Parameterization/assumption? The explanation given 'water isolated in the meniscus that cannot flow as a liquid phase but can still evaporate' is heuristic and does not seem to be supported by physical investigation. Note that theoretical models exist for the description of water flow in thin films (Tuller and Or, 2001; Lebeau and Konrad, 2010) which are in contradiction to the assumptions underlying the approach chosen by the authors.*

In fact this is not our assumption. Residual saturation is defined as the water content below which water does not flow as a liquid phase. Therefore water can be extracted only by putting the soil in an oven at  $105^\circ\text{C}$ . Furthermore, the material used in our experiments is silica sand which doesn't allow adsorption. That is the reason why we are not considering water flow in thin films. In our case the water present in the upper zone of the column, above the evaporation front, is the water isolated in the meniscus which can no flow anymore.

*P536 L9-11. Here you explain that the parameters were determined by calibration. But the description is insufficient, though. Note that these properties are very influential with respect to the results of the numerical simulation and must therefore be determined with high accuracy, in particular for small water contents occurring in the topsoil*

*above the evaporation front. Please provide more details. What kind of data were used for the calibration? Which objective function was used? How was it minimized? Did you use constraints? Was the resulting inverse problem stable and unique? Were the parameters identifiable or did they correlate significantly? I suggest that parameter values are reported as results instead of reporting them as fixed values in section 3. Standard errors of estimated parameters should be reported as well to illustrate how uncertain the estimates are. Some information about their cross-correlation should be added.*

We apologize for the misunderstanding. We already answered to this comment before in this document (see comment P535 L8-16).

*P536 L10: 'Based on these results...' – such results must be better documented to judge the correctness of the conclusion drawn.*

This modification was implemented to be able to model our experiments where processes take place under oven dry conditions. We have rewritten the text to modify this sentence as it is not something deduced from the results but a necessity. We have suppressed this sentence and we have added, at the beginning of the section, the following: “*To reproduce the experimental data of the degree of saturation curve (Gran2011) a modification of the retention curve and relative permeability functions is necessary.*”

*P536 L12-: the values for solute transport parameters (e.g. the dispersion length) are not reported, neither in the text nor in the tables. Maybe the equations can be simplified by omitting factors that are not accounted for in the simulations like kinetics?*

We have rewritten the text to include the missing data ( $D = 10^{-9} \text{m}^2 \text{s}$ ,  $\alpha = 0.0015 \text{m}$ ). We cannot simplify more factors because we are simulating kinetics.

*P537 L7: 240 elements were used for the numerical model. I am wondering whether this is a sufficient spatial resolution given the fact that in the work by Schneider-Zapp et al. (2010, cited by the authors) the outcome of a grid convergence study resulted in a very fine discretization, i.e. the upper finite element having a length of 1E-9 metre.*

We did a convergence analysis. The reason why this is a sufficient spatial resolution lies in the code formulation (Olivella et al. 1995): the element wise calculation of relative permeability as a function of element wise averaged degree of saturation implies that



convergence (as mesh is refined) takes place much faster than in the more traditional consistent formulation (element wise averaged permeability).

*P537 L6: Why did you use two materials and what does this really mean? It looks like the soil hydraulic properties were equal and only the enhancement factor was varied with depths? What is the physical justification for this? Note that this means that you calibrate the soil hydraulic functions and the enhancement factor simultaneously. Is this possible, i.e. is parameter correlation acceptable? Again, how was this calibration achieved? How do your values compare to values in the literature?*

We agree we need to explain this information better and sooner in the text and we added an explanation in the revised version.

These two materials are actually 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 ( $\tau_0$ ) 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. As mentioned earlier, the hydraulic functions were not calibrated. Had they been, which we do not recommend, it is unclear whether they would be correlated (they dominate different portions of the experiment).

*P537 L16: 'some loss of energy' – the amount is given in Table 2, good! But based on which information did you specify this flux?*

Although the columns were surrounded by a thermal insulator, this isolation was not 100% effective. Taking into account the thermal insulator properties, heat conductivity and width, we calculated the dissipated heat through the walls and we obtained a  $\gamma_e$  value. This value was later adjusted to fit the temperature profile measured in the column.

*P538 L 3: 'porosity change due to precipitation-dissolution' – how was this exactly simulated? Do you assume no influence of precipitation on the soil hydraulic properties? Please be more precise.*

The precipitated mass per unit volume is divided by the solid density of the salts and the volume variation obtained corresponds to the change in porosity. Referring to the hydraulic properties, porosity effects on the hydraulic and thermal transport are taken into account (Table 1 shows how several functions depend on porosity).

We have added an explanation in the revised version.

*P538 L11: 'Saturation [: :] reaches oven-dry conditions (vol. water content lower than the residual one)' – I don't think that this statement is in agreement with the definition of oven-dryness, i.e. zero water content. Does the residual water content correspond to the minimum saturation in Eq. (5) or the parameter  $S_{min}^0$  in Eq. (6)? Please be more precise.*

We thank the referee by this comment because we need to clarify our nomenclature. We define as oven dryness the situation that would be reached after 24h at 105°C. This situation was never reached (recall our earlier discussion about the MgSO<sub>4</sub> mineral states, comment P532 L10). We define as oven dry conditions those in which the oven dry branch of the retention curve needs to be activated. We have clarified our definitions in the revised version.

*Does the residual water content correspond to the minimum saturation in Eq. (5) or the parameter  $S_{min}^0$  in Eq. (6)? Please be more precise.*

$S_{min}^0$  is the residual saturation, for which liquid water becomes discontinuous so the liquid permeability is zero. We have clarified our definitions in the revised version.

*P538 L 14: 'profile similar to that of the sand retention curve' – this is ambiguous.*

*Does this imply that the vertical pressure head distribution is linear?*

Strictly speaking, it means that head is constant. It was used as qualitative comment that comes from the argument that when there is no flow we get the retention curve of the material. As the fluxes in the lower part of the column are very small, head variations in this zone are negligible.

*P538 L16-17: This statement is not supported by the results shown. It would be helpful to provide the results of the simulation without this correction to support this.*

We thought that this statement was obvious since without the modification of the retention curve we would not have been able to reproduce the step that experimental

data presents in the degree of saturation curve (see Fig. 3). We have clarified this in the revised version.

*P539 L3-5: What do you conclude from this mismatch? Please discuss possible reasons.*

The conclusion has been given at the end of the conclusions section.

*P541 L8-9: Correct conclusion, but this makes it even more important to inform the reader how the energy fluxes reported in Table 2 were derived.*

Yes, we agree and we think that has been answered in one of your previous comments (P537 L16).

*P541 L10: As far as I know, the Philip-de Vries theory uses the enhancement factor only for the thermal vapor flux. Increasing its value therefore should increase the downward vapor flux following the temperature gradient and not the upward flux following the pressure head gradient. Please explain why you applied the enhancement factor to both fluxes as it becomes obvious in the equations in Table 1.*

The function that has been used for  $\tau$  ( see Table 1) gives values lower than unity for wet conditions (tortuosity effect) and values larger than unity for very dry conditions (enhancement effect). This later is explained by the capacity of meniscus to act as bypass for evaporated water molecules (as explained by Philip and de Vries). In contrast, under wet conditions vapor diffusion is hindered by the poor connectivity of gas occupied pore space. This approach is nice because by means of a single function we reproduce both processes. We have clarified this in the revised version.

*P543 L2: I think it is stated for the first time that the reduction in the enhancement factor is used to model the influence of the salt crusts. However, this is not in agreement with the physical meaning of the enhancement factor discussed by Philip and de Vries. How can you be sure that \_ is not a mere fudge factor that is used to force the simulation results to the experimental data without a physical basis? Note that the formation of a salt crust must be expected to reduce the hydraulic conductivity thereby reducing capillary recharge.*

Diffusion enhancement is controversial. The explanation above (condensation-evaporation across meniscus) is conventional but cannot explain the big enhancement observed in many experiments. Our goal was not to investigate diffusion enhancement,

which is widely reported. Therefore, indeed, the diffusion enhancement factor must be considered a fudge factor. We acknowledge it in the revised version of the paper.

*P543 L3-5: 'This was needed: : :.' – please state this in the results section, too, it makes the article easier-to-understand. What is also missing is a visual display of the correspondence between observed and simulated evaporation rates.*

We agreed and we have modified the text in the revision version.

Since the evolution of the evaporation simulated by the model can be deduced from the vapor flux graph, in figure 4, we decided to don't include more graphics in the document. Furthermore, the experimental evolution of the evaporation was extensively presented in Gran et al. 2011.

*P546 Table 1: not all symbols occurring in the equations are defined. The flux laws are missing for the fluxes  $j$  occurring in Eq. (1), (2) and (3). While values for heat conductivities are given (without reference), values for heat capacities are not indicated.*

*P547 Table 2: not all symbols occurring in the equations are defined.*

We mostly agree with these comments and we have added the descriptions that were missed and the values that you asked for both in table 1 and 2.

The flux laws for fluxes  $j$  are writing as follows:

$$\begin{aligned}\mathbf{j}_g^w &= w_g^w \rho_g \mathbf{q}_g - \mathbf{D}_g^w \nabla w_g^w \\ \mathbf{j}_g^a &= w_g^a \rho_g \mathbf{q}_g - \mathbf{D}_g^a \nabla w_g^a \\ \mathbf{j}_l^w &= w_l^w \rho_l \mathbf{q}_l - \mathbf{D}_l^w \nabla w_l^w \\ \mathbf{j}_l^a &= w_l^a \rho_l \mathbf{q}_l - \mathbf{D}_l^a \nabla w_l^a \\ \mathbf{j}_{E_l} &= \mathbf{j}_l^w E_l^w + \mathbf{j}_l^a E_l^a \\ \mathbf{j}_{E_g} &= \mathbf{j}_g^w E_g^w + \mathbf{j}_g^a E_g^a\end{aligned}$$

With the aim to synthesize and since that these equations are already described in previous works (Olivella et al. 2000) we prefer to not include them in this paper.

### **3 Response to Technical details**

P536 L20: replace tensor by coefficient

P538 L16: 'confirms the validity' – I think this is too strong, I would prefer 'supports the validity'

P538 L27: but is still more dilute than

P539 L21: a decrease at the bottom of what?

We thank the referee for these comments. We have made the appropriate changes but not for the case of replacing the word tensor by coefficient. As we are presenting the general formulation the term should be a tensor, although you are right and for our model that would be a coefficient.

### **References**

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Olivella, S. et al. (2000): Vapour transport in low permeability unsaturated soils with capillary effects, *Transport in Porous Media* 40:219-241.