1 Response to general comments

First I think that the major reduction in salt concentration along the soil profile is related to the fact that most of the salt is being accumulated near the matrix surface. Naturally, for mass conservation reasons, the increasing of salt mass in this region (soil surface or more accurately: at or near the evaporation front location) should be compensate by reduction in salt mass in deeper parts of the soil profile. In other words, the fact that c/c0 (where c is the salt concentration) along the profile seen in Fig. 3 is likely to occur (per mass of soil!) regardless downward water vapor transport.

The concentration values that are shown in this work are not expressed per mass of soil, but as moles of salt per kg of water, and what is displayed in figure 3 is the salt concentration in the water. We are not talking about the salt mass in the soil but referring to the salt concentration in the liquid because it is the relevant variable for studying transport mechanisms. Liquid carries the salts upwards, the water evaporation causes the increase of this water concentration and that’s why the salt concentration (in liquid phase) increases at the top of the column. Besides, under the evaporation front there is no evaporation so the salt concentration in the water is expected to remain the same as the initial one. What is surprising here is that this concentration diminishes below the initial salt concentration of the column and, as water cannot flow upward without carrying the salts, the only way to decrease the concentration in the liquid is increasing the amount of liquid, that is to say by condensation of the water vapor. We emphasize the fact that we work with concentration per kg of water in the revised version.

In Figure 3 the authors presents water saturation and salinity along the soil column. Does salinity concentration is expressed for the solution or for the soil? To support the reduction in solution salinity due to vapors condensation I believe that the authors are referring to solution concentration. But for the low water content regions and especially, the regions considered as “oven dry”, the solution concentration is not relevant (practically speaking, there is no solution in the pores…). This is very confusing; both conceptually and practically in Figure 3.

As mentioned above, the concentration values that are shown in this work are expressed by moles of salt per kg of water. We are referring to the solution concentration. We have modified the axis to the figure to clarify this issue. The experimental procedure for the points in Figure 3 is described by Gran et al. (2011). As it turns out, the upper portion of the column was dry (almost all water was hydration water). Therefore, those concentrations are simply equal to the solubility of the salt.

Looking at Fig.1, it seems that there is vapor flux downward from the evaporation front. Maybe I miss something, but the water concentration below the evaporation front is supposed to be at saturation, isn't it? If so, why water vapor will move downward? Unless evaporation front here is not defined in the classical way. For example, if the "evaporation front line" is highly irregular, water vapor could move downward from "high capillary fingers" to dry area between these "fingers".

The evaporation front is where water vaporizing occurs. The soil immediately below the evaporation front doesn't need to be saturated. In fact, the saturation profile (figure 3) shows the evaporation front coinciding with a sharp decrease in saturation (at 4cm depth by the end of the experiment), and it is also clear that the soil is not saturated both below the front and in the rest of the column. That is why vapor flows can occur. For more details see the response to the minor comments of referee 2.

Another potential mechanism which the authors ignored is condensation of vapors on the precipitated salts crust. Subsequently, these condensed vapors can dissolve the precipitated salts and result in back diffusion of the dissolved ions downward.
The numerical model allows condensation to occur anywhere. The referee is right in expecting condensation to occur at the salt crust. In fact, it would occur if the column top cooled down, which did not happen in our experiment. As a side note, to emphasize this issue, when dismounting our columns, we had to hurry in weighting the crust. Once it cooled down, the weight increased slowly. We interpreted it as water from the laboratory air slowly condensing on the highly hygroscopic salts of the crust.

The conceptual model is very unclear, mainly in the beginning of the paper. It is clarify a bit when getting to the Results section. I suggest to describe the conceptual model earlier in the text and to better describe it.
We have made some changes in the revised version to extend the conceptual model description. Nevertheless, this conceptual model is described in detail in the experimental paper (Gran et al. 2011). However, the objective of the paper is to verify and quantify the processes. Therefore, it is natural that they become increasingly clear.

P531, L1-2. Authors mention (upon other references) that"… water flux in dry and salinized soils is controlled by salinity and temperature gradients…." without any consideration of the matrix properties. In many cases the matrix properties (e.g., permeability, porosity) may be the limiting factor for evaporation.
Yes, the referee is right and we have qualified this statement in the revised version. In our case, we are studying a sand column where permeability is so high, relative to the fluxes, that viscous energy losses are irrelevant. However, we also performed experiments with silt columns (we do not report them here, as everything occurred according to expectations, so that little was learned). Permeability was the limiting factor in those columns.

P531, L8.Authors are mentioning vapor downward migration upon the work of Gran et al., 2010. I would suggest elaborating a bit more about Gran's work as it is relatively new. It is not clear what the origin of the downward vapors is. Is it for a case of a receding evaporation front only?
Indeed, the conceptual model from the experiments is described in detail in Gran et al. (2011). We outline the summary of the conceptual model in section 2, which we have expanded in the revised version, but the truly rich description of processes comes from the numerical model. In this model, contrary to conventional modeling exercises that are mere quantifications, we unveil details that could be conjectured, but not supported a priori. In this sense, we thank the referee for his/her repeated comments that things become clear at the end. Indeed, they are the result of modeling.
The increase in vapor pressure in the evaporation front due the evaporation generates a vapor pressure gradient between the evaporation front and the bottom of the column (also a temperature gradient), which promotes the downward vapor flow.

P532, L2-3. Salt precipitation is mentioned in the text. Authors considered a salt precipitation pattern of "low permeability crust". While the salt crust formation is correct for some salts (NaCl in particular) it is not the case for many other salts that precipitate in the pores, many times below the evaporation front. An example for these differences can be found at Rodriguez-Navarro and Doehne, 1999 (Earth Surface Processes Landforms) and in Nachshon et al. 2011 (Water Resources Resources). I suggest at least mentioning that the salt crust precipitation pattern is not the only possible salt crystallization formation.

In our case there is no precipitation below the evaporation front. What we observed from the lab experiment was a salt crust at the surface, consisting of salt crystals mixed with sand (see figure 1 in Gran et al. 2011), and an accumulation of salt precipitates which gives consistence to the soil and decreases from the surface to the evaporation front. It is true that the location of precipitates is sensitive to solubility. Halite and epsomite are very soluble, so that they precipitate at the top. Less soluble salts would have precipitated further down, but we do not think they can precipitate below the evaporation front. We have added a comment on this issue in the revised version.

P532, L3. Authors may want to mention Fujimaki et al. work from 2006 (Vadose zone journal), where salt crust effect on evaporation was modeled and compared to experimental results.

Indeed they did, but considering isothermal conditions and that's why we didn't mention it in the original version. Nevertheless, we will mention them in the revised version of the paper.

P532, L4-6. Unclear. Saturation of what?

Water saturation in soil. To clarify it, we have modified the text in the revised version.

P532, L20-28. Why do you consider only the epsomite experiments of Gran, while ignoring the Halite experiments?

We had to focus on modeling one type of experiment and the selected was epsomite due its hygroscopic properties. This adds the possibility to simulate the hidratation and dehydratation of the minerals and the water exchange with soil that this implies.

P532, L20-28. I suggest mentioning the epsomite initial concentration, especially as at Gran et al. there are two concentrations for the epsomite experiments.

The text has been changed to include that missing information (14g/kg).

P532, L20-28. It is not clear from this work, nor from Gran et al.,2010 do evaporation rates (e.g., sand columns mass loss) were monitored continually during the evaporation experiments?

The columns mass loss was measured once or twice a day.

P532, L20-28. It is getting clear only at the results section why you are talking on oven dry conditions. Please try to clarify it here, as it looks strange to talk about oven dry conditions for natural, initially saturated conditions.

We clarify in the revised version of the paper that we model the processes in the soil from saturation to oven dry conditions (after some days under evaporation) and 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 105ºC for 24h.

Figure 1. I assume the "Min. Conc." Stands for minimal concentration but I suggest to point it out. In oppose to Min. Conc., where is the maximal concentration at the pore solution is expected to be?

Yes, Min. Conc. stands for the minimum value in salt concentration. We have included an explanation in the caption of figure 1.

The maximal concentration is expected (and was measured) at the upper zone of the column (as we said, P538 L22-25, and shown it, Fig. 3) going from the surface to the evaporation front.

P533, L8. Indicate if talking about water content saturation or salt saturation.

We are talking about salt saturation. We have modified the text to clarify it.

P533, L12. It would be very useful to show your (Gran) salt concentration measurements here with a graph. If you want to leave it for the results section, at least show it conceptually. The process you are talking about is not very clear conceptually; where is the maximal salt concentration expected to be?

We have added a description of the salt profile in this part of the revised version.

We think we already answered to the second question, both in the paper and two responses before (see comment on Figure 1).

In your conceptual model you are ignoring the ions diffusion within the solution which works against the ions advective flow with the solution capillary flow (Peclet number). As well, some works (e.g. Van Dam et al., 2009 GRL) talk about gravitational instabilities near the evaporation front which may drive convection flows of the pore solution which may postpone ion accumulation at the evaporation front.

The numerical model simulates ions diffusion but it results not to be dominant. Furthermore, as a previous referee has requested (Referee 2 comment on Page 10, line 22-27), we have included the value used for the diffusion coefficient ($D = 10^{-9} m^2/s$) in the revised version.

Nomenclature for equations variables is not good. Please go over the equations again and make sure all variables are described.

We have included some missing descriptions in the text to clarify some terms in the equations.

Table 1. Please make sure all variables in the equations are described.

Indeed, some were missing. We have added the necessary descriptions.

P537, L5-17. Please explain why you choose initial conditions to be of two different materials. Does it mean you start your simulation for an evaporation front 1.5 cm below surface? Why did you choose the 1.5cm length? What about the process prior to this state, where matrix is fully saturated? Naturally, in the beginning there is no precipitated salt.

Actually, initial conditions are constant throughout the matrix. The only difference for the top 1.5cm is the reduction of the vapor enhancement factor (indeed, increase in tortuosity for the thickness of the crust) which is only relevant when the crust has dried. (For more details see the answer to referee 1, comment P537 L6)

P537, L19. It could be that I miss here something, but if your initial conditions are of an evaporation front 1.5cm below the surface then it mean the evaporation process is 'on going'. Hence, it means that the radiation on the soil surface is already applied; therefore the isothermal assumption of 25OC for the entire column is somewhat problematic.

Our initial condition is saturated soil, so evaporation starts at the surface.

Figure 3. Does salinity concentration is expressed in mol/Kg of sand or Kg of solution? Obviously, this is an important issue.

The salt concentration is expressed in kg of solution. We have added that information both in the text and in figure 3.