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Hydrology and Earth System Sciences Discussions

Interactive comment on "Modeling evaporation processes in a saline soil from saturation to oven dry conditions" *by* M. Gran et al.

Anonymous Referee #1

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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.

Specific comments

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?

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.

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?

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

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

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

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.

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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.

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

P535 L8-16 and Table 1: It does not become clear how the parameters of the soil hydraulic functions λ, α, P_0, $P_\mathrm{c}^\mathrm{dry}$, and S^0_min were determined because it is described further down. The manuscript should be reorganized as pointed out below.

P535 L17: note that S_i can become negative if $P_c > P_c^\mathrm{dry}$ 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?

P535 L21: 'α modifies somewhat' – please be more precise about the way this parameter changes the retention function. In comparison to the Fayer and Simmons model α is an additional parameter. Since it multiplies S^0_min it is not clear why it is needed at all. Note that α 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?

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^0_min. Please correct.

P536 L5-8: Setting the conductivity to zero for $S < S^0_\mathrm{min}$ 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

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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.

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.

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

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?

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.

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?

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

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.

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^0_{min} in Eq. (6)? Please be more precise.

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?

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.

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

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.

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

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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.

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 τ 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.

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.

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.

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?

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P539 L29: 'because of depth' – with depth?

P540 L17: Sensitivity analysis

References

Lebeau, M. and J.-M. Konrad (2010): A new capillary and thin film flow model for predicting the hydraulic conductivity of unsaturated porous media, WRR 46, W12554.

Tuller, M. and D. Or (2001): Hydraulic conductivity of variably saturated porous media: film and corner flow in angular pore space, WRR 37, 1257-1276.

Philip, J.R. and de Vries, D. A. (1957). Moisture movement in porous materials under temperature gradients. Transactions of the American Geophysical Union, 38(2): 222-232.

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