Hydrology and Earth System Sciences Discussions

Interactive comment on "Infiltrative instability near topography with implication for the drainage of soluble rocks" *by* P. Genthon and A. Ormond

Anonymous Referee #1

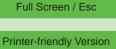Received and published: 25 April 2008

General comments:

The paper is very well written and well structured. Relevant questions, i.e., the understanding of the development of karst and its dependence on certain parameters, are addressed. The assumptions made for the numerical simulations are clearly outlined. Also the difference to models of other scientists, such as the assumption of homogenity of the interior of the formation for the initial state, are clearly outlined. Detailed conclusions from the numerical simulations are drawn, such as the dependence on parameters, and the potential relation of the numerical results to real world karst. The language is fluent.

However, some points I would like to discuss with the authors concerning the descrip-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

tion of their model:

p. 704: Are you sure about the factor "1/phi" (phi=porosity =fluid_volume/reference_volume) in eq.(2) in the accumulation term? I would expect the factor "phi" instead, if the concentration C refers to the fluid volume. (Though this has no influence on the following simulations, since a quasi-stationary state is assumed in the following.)

p. 706 l. 18: Probably 10^{-5} mol/kg instead of 10^5 mol/kg is meant?

p. 708: I fully agree with the equations (8) and (9) given there. But I think that the explanations for these equations given in the text are partially not correct or at least misleading, something is switched concerning where the high and the low values of C are. To me, the sentence in l. 6-8 should express the following: Inside the former (i.e., the non-dissolved volume) the concentration C_0 is low(!) (and will be assumed to be zero) since the high concentrations from the inflow have not yet reached that part of the domain. Inside the latter (i.e., the dissolved volume) the reaction, triggered by incoming H^+, is complete (i.e., the mineral is fully consumed); the concentration here is higher(!) than in the dissolved volume and governed by equation (8). In the fully dissolved part, the concentration of C should be higher than in the non-dissolved primordial part, as the authors show in Fig. 2a,b. One might indicate this once more also in Fig. 2d by writing "phi=1" and "C'>0" into the white (left) part and "phi<1" and "C'=0" into the grey (right) part. Hence, maybe the authors should reconsider carefully their lines 5-8 and the usage of "former"/"latter". (My conjectures are based on a reaction of type "Proton + Mineral <−> Product" with constant activity of the mineral, and equilibrium condition C(product)/C(proton)=solubility_product; equ. (9) and line 13 then could be justified by the assumption of small values for C(prod)/solubility_product.)

p. 708, last paragraph: What do the references with the prime, " (4)' " and " (5)' ", refer to? As far as I understand, in the case Da=infinity, equations (7) and (8) with constraint (9) have to be solved. Also, equation (5) is mentioned in this paragraph. But eq. (5) is

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

only solved in the kinetic case Da<infinity (together with eq. (7) on the non-dissolved and with (8) on the dissolved domain); right?

p. 712, first paragraph: In the equilibrium case there are only two media, non-dissolved and fully dissolved, and the permeability differs by a factor of R_k between the two. But what is the assumption for the dependence of the permeability on the porosity in the kinetic case, where the porosity varies smoothly?

p. 713: The authors discuss potential differences between simulations and "real" karst. Is it also an issue that the numerical models are 2-D? Is there any conjecture whether a 3-D version of their model would show a significantly different behaviour? Should this be an issue of future work? p. 704, second paragraph: The authors use Darcy's law also for the case where the whole soil matrix is dissolved and very large cavities develop. Probably Navier-Stokes flow would be a preciser description there. I think that it is an interesting result if it is true that the choice of flow in the dissolved areas should have no big influence on the evolving dissolution pattern...

typographical errors:

p. 702 l. 26: Aggregation

p. 703 l. 4: Probably "nondimensional" or "non-dimensional" is better than "non dimensional" (though I am not a native English speaker)

p. 705 l. 6: "in the equation" instead of "in equation"?

l. 10: "the steady state"

l. 11: There should be a dot between the gradient and the v (as in (6)).

p. 706 l. 23: "gradients are" or "gradient is" instead of "gradient are"

p. 709 l. 10: "an analog" instead of "a analog"

l. 20: "R_k" is written with "k" being a subscript on p. 705, 707, but here it is "Rk"

p. 713, l. 10: It seems that a word is missing, maybe a "by" after "introduced"

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 701, 2008.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper