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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

P. Genthon and A. Ormond

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I would like firstly to thank the referee for his work on the manuscript. His meaningful comments will be useful to improve the paper. The following comments are acknowledged, will be integrated in be the revised version of the paper, and do not seem to lead to discussion.

- Using 1/ phi instead of phi in eq. (2)

- Incorrect use of 'the former' and 'the latter' for explaining eq. (8) and eq. (9)

- Incorrect use of eq. (5)' and eq. (4)' that do not exist in the manuscript

Checking for typos is also acknowledged.

The following remarks deserve some discussion:

1) The porosity-permeability relationship in the case on non instantaneous dissolution: we use a phi**3/(1- phi)**2 (Kozeni-Karman type) relationship up to a porosity of 0.6. For larger porosity, the permeability tends asymptotically to 100. Interesting results might be gained with a permeability rising faster for low dissolution or if almost complete dissolution must be reached before a significant permeability increase is observed, but these effect are to be explored at the light of specific geologic examples.

2) Difference between 2D and 3D models: Indeed, the infiltration instability results from flow focusing in the high permeability zones created by dissolution, as well as competition between different dissolution zones (i.e. fingers). Flow focusing as well as finger competition will be also present in 3D models and probably the physic of the instability will not be deeply changed. However, I would expect that these two phenomena will be enhanced since they operate in the two directions perpendicular to a finger, instead of one direction only for 2D cases. On the other hand 1000**3 modeling grids are now handled with supercomputers, which implies that 3D runs are achievable. Thus once the interest for the karst community for infiltrative instability is proved by acceptance of the present paper, it would be interesting and feasible to explore 3D models. I propose to include one sentence on further 3D models in the discussion.

3) Darcy flow versus Navier Stokes in the dissolution zones: clearly, fluid flow in some voids of karstic networks do not obey to Darcy law. Moreover, several instabilities may develop in large voids of a fully developed karst, which deserve modeling. However, Ormond and Ortoleva (2000) have compared the fingering process modeled with a Darcy or a Brinckman equation (Darcy flow in the porous medium coupled with Navier Stokes in voids) and have shown that the fingering process was rather insensitive to the equation used in dissolved area. The reason is that flow focusing and finger competition are not sensitive to the kind of equation used in the finger, once flow is much easier in dissolution zones. Thus the infiltration instability may be modeled with Darcy flow as long as breakthrough is not reached. I propose to better explain the paper of

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Interactive Comment

Ormond and Ortoleva in section 2.1

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Interactive Comment