

Interactive comment on “Evolution of karst conduit networks in transition from pressurised flow to free surface flow” by M. Perne et al.

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Received and published: 18 July 2014

This paper is a new and highly significant contribution to the field of karst hydrology or more specifically to the modelling of cave evolution. Throughout the last two decades there have been numerous publications addressing cave evolution in settings with pressurized flow, i.e. phreatic conditions. To my knowledge this is (almost – see specific comment no. 3 below) the first that considers cave evolution under the transition from pressurized flow to open-channel flow and thus the paper is potentially groundbreaking within its field. That said I feel that the way the results are presented and discussed may obstruct the reader’s view of the novelty and significance of the contribution. The paper is well written and the scenarios are well explained. However, the explanations

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are focused on the results from the individual scenarios and the differences among them, but not so much on the differences between this new model and the earlier approaches that considered pressurized flow only. As the paper is now it is not at all obvious, which phenomena were similarly found to occur under purely phreatic conditions and which can be explained only by considering the transition to vadose flow. I think the paper would be strengthened if this were highlighted, perhaps by running some scenarios in a purely pressurized-flow mode for comparison; to keep the paper concise some other scenarios might be omitted.

Specific comments:

1) p. 6522, l. 3-4: While I understand that this general explanation of early conduit evolution has to be brief I feel that it is not sufficiently clear (how do the successful pathway diminish gradients along the competing pathways – perhaps this needs one or two sentences providing additional explanation).

2) p. 6522, l. 8: There is only one Palmer (2007) in the list of references; therefore “2007b” seems wrong or missing in the list of references. In addition to these two references there are a number of modelling studies (which of course the authors know) demonstrating the influence of the recharge distribution; the impact of the availability of recharge was probably less frequently addressed by modelling studies, but Hubinger and Birk (2011), which is published in HESS, has explicitly focused on this aspect.

3) p. 6522, l. 18-19: Basically, I agree with the authors that karst evolution modelling has been limited to pressurized flow so far. However, I would like to point to a perhaps not well known paper by Annable and Sudicky, Bulletin d’Hydrogeologie, no. 16 (1998) entitled “Simulation of karst genesis: hydrodynamic and geochemical rock-water interactions in partially-filled conduits”. It is relatively short and only addresses evolution of a single passage; to my knowledge there is no follow-up on this paper in one of the major journals. Nevertheless, this paper might deserve to be mentioned here.

4) Section “Dissolution and transport”: I think using this simplified dissolution kinetics

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is fine, as the focus is on the hydraulics here. However, it complicates the comparison with the existing work, which was strongly focused on limestone (though some of the earlier work was based on similar approaches). It is not clear to me why the authors focused on transport-controlled kinetics; it does not seem to be much more complicated to use surface-controlled kinetics. Perhaps this can be briefly explained. Similarly, when surface-controlled kinetics is used in one scenario, only linear kinetics is considered – why is this? I do not see any major problem in this, but it might be irritating for those who are familiar with the earlier literature in this field and thus it might deserve a brief comment.

5) p. 6527, l. 3-4: Delete this sentence. On the same page this is mentioned in l. 17-18 again, where it is more appropriate.

6) Section on “Low-dip networks”: In itself everything is fine here, but I miss a comparison to a scenario where flow is forced to remain pressurized – I think this is the key issue of the paper, the advancement from models that considered only phreatic conditions towards a model that includes the transition and further development under vadose conditions. Would it be possible to show a scenario where the hydraulic gradient imposed by the boundary conditions is identical but the conduits are sufficiently deep to remain pressurized? This strikes me as more important than e.g. the comparison of a network with uniform initial diameters (Fig. 6) and one with randomly diameters (Fig. 9) – similar issues were earlier considered for pressurized flow and the findings (concerning uniform vs. randomly distributed) seem to be similar; perhaps the scenario shown in Fig. 6 is not needed? Also, other findings such as the upstream propagation of the conduit evolution were earlier described for pressurized flow; thus, it is of course fine to describe and explain the scenarios as is done in the paper, but in addition there is a need to highlight the differences to the earlier models.

7) The scenario shown in Fig. 13 and its comparison with the one shown in Fig. 9 (but not Fig. 10 as indicated in the text on p. 6532, l.23) is very good and of great interest. Expanding on my previous point, I suggest that you add a corresponding pressurized

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flow scenario – I guess the tilting will not affect the result in that case, and thus this will clearly demonstrate the relevance of the transition to vadose flow.

8) p. 6529, l. 18: “The rock used is salt.” – Please refer to Tab. 1 here. Also, you may want to consider mentioning earlier in the paper that you consider salt, as this justifies the dissolution kinetics used in the model. As mentioned above: The decision to consider salt is unfortunate in that it complicates the comparison with earlier work.

9) Tab. 1: The units given for roughness coefficient and equilibrium concentration seem to be wrong.

10) p. 6529, l. 25: Delete one “the higher”

11) p. 6530, l. 9: “is flattens” – delete “is”

12) p. 6533, l. 15: “see discussion in Sect. 2.4” – to be honest I do not understand where this is discussed in section 2.4; perhaps it is more appropriate to add a brief explanation here.

13) p. 6536, l. 14: Delete one “is slow”

14) “Discussion and conclusion”: Although generally well written, here again the differences between conduit evolution under pressurized flow conditions and that under transition from pressurized to open-channel flow conditions should be further highlighted.

15) p. 6537, l. 22: Here again Palmer (2007b) is cited but there is only one Palmer (2007) in the list of references

16) p. 6538, l. 25: And here Palmer (2007a) is cited although there is only one Palmer (2007) in the list of references

17) p. 6538, l. 26: Using the terms “NW side” and “N face” seems to imply that you refer to the low dip scenario (see p. 6533, l. 26-28, where it is said that you use top, bottom, etc. for the sides of the network in the high-dip network), but I think these lines

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still address the high-dip network? Please clarify and correct.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 6519, 2014.

HESD

11, C2563–C2567, 2014

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