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Discussions

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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In my opinion this paper is very well written, each step in its modeling is explained clearly and it is very well illustrated with relevant figures that will help readers. To my knowledge the authors are exploring new territory in the computer modeling of solution cave genesis here. Their paper is enjoyable to read and think about, and worthy of publication. However, I have a number of comments that the authors may wish to consider in any revisions that they choose to make:-

(1) To aid review and historical comparisons in scholarly work it is desirable to adopt one given set of definitions. What is being discussed in this paper is the transition from what has been defined as Phreatic-to-Drawdown Vadose conditions (Ford & Williams

1989, p. 267 et seq): that review set out four different cave phreatic geometries (from Ford 1971; Ford & Ewers 1978) and differentiated Drawdown Vadose development from Primary Vadose and Invasion Vadose developments.

(2) on page 6522; in my reading, the concept of evolution of a solutional proto-conduit to its physical 'breakthrough' has its origin in the hardware modeling of Ralph Ewers (1972) that was then adapted for the chemical kinetics by White (1977) which were further explored by Dreybrodt (e.g. 1988).

(3) The basic 'Low dip' model of Figure 1 adopts the conceptual model geometry of Ford & Williams 1989, Fig. 7.3. It is a reduced version of the Multiple-input Multi-rank recharge situation in Ewers' hardware modeling (Ewers 1978, 1982).

(4) As the authors acknowledge, the structure of their low dip rectilinear network of phreatic conduits of uniform shape and size and with turbulent flow prevailing (i.e. post-breakthrough) is rather an extreme simplification. It is my personal opinion that use of rectilinear nets of conduits of uniform aperture is no longer 'state-of-the-art' in cave computer modeling, as noted in the Conclusions below.

Readers may wonder how such a network could be created as the initial condition for the drawdown modeling being undertaken in this paper? Some previous and quite different hydrodynamic setting would be needed to form it because such network could not be created by the surface recharge pattern set out in Figs. 1-4.

(5) Other modelers have used the SWMM package in recent computer modeling; see the summaries in Neven Kresić' recent books (2013, 2014).

(6) p. 6528 and Fig. 5– placing an outfall 'master' conduit 100 m below the low dip bedding plane that intercepts the karst flow initially is to be considered an extreme example of 'perched' phreatic drainage. Audra (1996) cites examples in his conceptual models.

What is being modeled here is a stack of penetrable bedding planes with a low dip that are crossed by a rectilinear joint system oriented normal to them. Solution shafts propagate down the intersections of the two joint sets in the system to reach the basal no-flow boundary.

(7) In Fig 6 and later, use of 'I1', 'I2', 'I3', etc. to denote the location of the input shafts may confuse readers who see the I as 1. I suggest In1, In2, etc. instead or, more simply, '1', '2', etc?

(8) The 'High Dip' modeling here is perhaps most appropriate for evaluating possible cave and karst developments along the edges of escarpments in bedded carbonate rocks; e.g. I see that it has some applications along the dolomitic Niagara Escarpment in Ontario.

(9) Congratulations to the authors for their insertion of a N→S tilt or warp into the modeling plane in Fig 13. It is a new idea in modeling, I believe, and recognizes what is a very common feature in caves. From my field experience such tilt or warping may favour the development of joint-controlled mazes on the down-tilt sides of trunk drawdown vadose passages; e.g. I have seen good examples in Mexico and Sweden. Similarly, use of a random distribution of apertures in the 'Inhomogeneous Case' (page 6535) is a significant addition to the reality of the model results.

(10) As they are modeling escarpments in salt, the authors may wish to invite the opinion of Professor Amos Frumkin, Hebrew University. He has studied real cases in the Mount Hermon salt dome in Israel that are broadly analogous.

(11). In their Conclusions and Discussion, page 6537 et seq, the authors make reference only to the writings of Art Palmer in his excellent 2007 volume on 'Cave Geology'. It may be arrogant but, in my opinion, scholarly publications should also mention earlier writing reporting the same conclusions. I find no results in this paper that would have surprised Ralph Ewers and me forty years ago (1974 - when Ewers had just completed his hardware modeling of the four different basic recharge scenarios for standard meteoric caves. For example, how does Figure 6 here advance the understanding from

Ewers' work that was summarized in Figs. 7.5, 7.7 and 7.11 in Ford & Williams (1989)?

(12) As noted, in my opinion cave genetic modeling using rectilinear networks only (with computation of the changes of conditions at their nodes) is no longer 'state-of-the-art'. The random aperture conventions of Hanna and Rajaram (1997) supplant them, at least in part. W.K.Annable (PhD thesis, University of Waterloo, Canada, 2002) combined a 3D rectilinear network with an H&R randomised aperture (bedding) plane in a computer modeling exercise that was also able to include joint transmission, matrix recharge and discharge, and interference from suspended load in the evolving solution conduits. Those are directions that I would recommend for future computer modeling.

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