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Interactive comment on "Early hypogene Carbonic Acid Speleogenesis in unconfined limestone aquifers by upwelling deep-seated waters with high CO_2 concentration: A first modelling approach" *by* Franci Gabrovšek and Wolfgang Dreybrodt

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This modelling study explores the evolution of caves in limestone settings where meteoric waters mix with upwelling deep-seated waters with high pCO2. Most of the scenarios shown assume that both the meteoric and the deep-seated waters are saturated with respect calcite such that the development of caves only results from mixing corrosion in the zone where the two flow components mix. In addition, scenarios are considered where one or both of the flow components are undersaturated with respect to calcite.

The scenarios shown in the paper give insight into the mechanisms of speleogenesis within this particular type of setting. Thus, the paper addresses relevant scientific questions within the scope of HESS. The scenarios shown are interesting and novel. However, it is difficult to assess how far the general concepts and ideas go beyond those that have been presented by similar modelling papers, as there is almost no comparison made to other modelling studies. It seems to me that this should be more clearly addressed in the introduction of the paper and in the discussion of the results by referring to the scientific literature. In particular, the discussion is very short and does not refer to other modelling studies, but it is also not very clear from the introduction how this contribution classifies into the existing (modelling) studies and the two different definitions of hypogene speleogenesis. I therefore recommend revision particularly of the introduction and discussion section.

Specific comments:

1) Abstract, l. 9: "Hypogene caves originate from upwelling deep-seated waters loaded with CO2 that mix with meteoric waters . . ." – in this general form this statement is not valid. Even if one applies the "geochemical view" of hypogene speleogenesis as outline in l. 39-41, it does not require mixing with meteoric waters, but any "aggressiveness [. . .] produced at depth" would be sufficient. Thus, this sentence needs to be revised to clarify that this paper considers one specific type of hypogene setting or at least indicate that this is only one type of hypogene speleogenesis (e.g. "Hypogene caves may originate from . . .").

2) Abstract, l. 18: Please correct "seepaall ge face" (seepage face?).

3) Introduction, l. 50/51: "In this work we take a first step to explore by digital modelling the processes governing the initial evolution of hypogene caves. " Again, this general statement is not valid. This appears to be related to the statement addressed by com-

ment 1, which suggests that the specific scenario considered in this paper generally represents hypogene speleogenesis. As pointed out in comment 1, however, even if the geochemical definition of hypogene caves is used there are other mechanisms that produce aggressiveness at depth, such as the temperature effect mentioned in l. 45-47. Therefore, at least some of the papers cited in those lines classify as models of hypogene speleogenesis. The cited paper by Chaudhuri et al. (2013) even is titled “Early-stage hypogene karstification . . .”, and the cited paper by Rajaram et al. (2009) includes a section titled “Hypogene karst simulations”. I think there are other, similar papers that are relevant, such as Andre and Rajaram (2005): Dissolution of limestone fractures by cooling waters: early development of hypogene karst systems. Water Resour. Res., 41, W01015. There are even more modelling studies addressing hypogene caves if one adopts the “hydrological” definition given in l. 41-44. In particular, the two cited papers by Birk et al. (2003) and Li et al. (2020), which consider the artesian hypogene speleogenesis as observed in the Western Ukraine, belong to this category, and again there are other papers addressing this type of setting such as Birk et al. (2005): Simulation of the development of gypsum maze caves. Environmental Geology 48 (3): 296-306; Rehrl et al. (2008): Conduit evolution in deep-seated settings: Conceptual and numerical models based on field observations. Water Resources Research 44, W11425; Rehrl et al. (2010): Influence of aperture variability on conduit evolution in hypogene settings. Zeitschrift für Geomorphologie, Suppl. 54 (2): 237-258.

4) In the light of the previous comment, l. 48-50 (citing Klimchouk et al. 2017) also appear to be inappropriate, in particular, as Klimchouk (2013), which is also cited in the paper, does refer to some of the above-mentioned modelling studies of artesian hypogene speleogenesis.

5) It is further noteworthy that with the “hydrological approach” (l. 41) the scenario considered here does not classify at all as hypogene speleogenesis, because the underlying mechanism clearly is not “independent of direct recharge from the overlying or immediately adjacent surface” (l. 43). This, of course, is not problematic, as the geo-

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chemical definition is fully acceptable. Nevertheless, I think that the authors should be clear about this, e.g. by explicitly saying that this work adopts the geochemical definition. Somehow the discussion of “agents active in hypogene speleogenesis” in l. 53-65 hints at this, but it could be introduced by a statement that explains this more clearly. I think that this would also make it easier to explain, e.g. at the end of the introduction, how this work goes beyond the existing research – as I understand, it focuses on hypogene settings in a geochemical view, and within those settings it addresses chemical mixing corrosion (as opposed to the temperature effect considered by others), etc.

6) Even if the focus (and thus the novelty) of the paper is more precisely defined as suggested in the previous comment, I wonder if there is other related work that should be addressed in the introduction and even more importantly in the discussion of the paper. One the one hand, this could be papers more generally addressing the role of mixing corrosion in cave evolution, particularly the paper by the same authors published in 2010 (“Karstification in unconfined limestone aquifers by mixing of phreatic water with surface”), which appears to address similar processes in a setting that differs in the source of one of the flow components but still seems sufficiently similar to warrant a comparison of the results and a discussion about the differences resulting from the different type of setting. On the other hand, there is a recent paper also addressing the role of mixing of meteoric water with “cross-formational warm water” (Gong et al. 2019: Modelling early karstification in future limestone geothermal reservoirs by mixing of meteoric water with cross-formational warm water. Geothermics 77:313-326), i.e. the paper appears to address a similar type of setting albeit with a focus on other (thermal) processes. It would be very interesting to learn about the similarities and differences between the results from simulations with this type of settings/processes and studies that looked at either at similar setting (but different processes) or similar processes (but different settings). For instance, it would be very interesting to see if there are features of the resulting cave patterns that are characteristic only for the setting/processes considered here. This and similar aspects should be addressed in the discussion section.

7) There seem to be some omissions in the list of references. Palmer (2017) is cited in l. 445, but I cannot find a corresponding publication in the reference list. The same applies to Palmer and Palmer (1989) (l. 454/455). Please check specifically these two references and also more generally the completeness of the reference list.

8) The paper includes a fairly high number of figures, obviously because several scenarios are described and discussed. I think all of the scenarios are generally of interest, but I wonder if all of the figures are needed. Perhaps not all of the figures showing the aperture widths along profiles are needed? I find it difficult to give clear recommendations in this regard, but I think the authors might want to think about this issue once they have more clearly worked out, which aspects of their models are most relevant when they compare with the findings from related modelling studies (as suggested above, particularly in comment 6). Yet, I want to emphasize that each of the figures is of interest and thus in my view deserves to be shown if the authors think they are needed to illustrate important aspects of their results.

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