

AUTHOR'S REPLY

We would like to thank Steffen Birk for the helpful review. We have considered his comments and critics. Below find our replies to general and specific comments. We also propose changes in the manuscript. Several issues have been addressed by both reviewers. To this extend part of replies to both reviewers overlap.

General comment: The main objective of both reviewers is that we generalize our special case of Carbonic Acid Speleogenesis. We must stress that we are aware of the other modelling efforts on the hypogene speleogenesis. Given the two quoted conceptual frameworks (hydrological and geochemical), speleogenetic settings are extremely diverse. All aspects are therefore beyond the scope of our work; however we agree that a better description of how our settings fit within the framework of the conceptual models, is needed.

To do this, we will reformulate the introduction and discussion to clarify that our model is a specific case in a wide variety of hypogene settings. Other modelling studies will be mentioned and properly cited.

The reviewer then specifies the comments in 8 points. Below we give a list of replies and intended changes in the manuscript:

Point 1: Abstract: We agree with the critics. To address it, the first sentence in the abstract "*Hypogene caves originate from upwelling deep-seated waters loaded with CO₂ that mix with meteoric waters in a limestone aquifer.*" will be replaced by "*Here we present first results on digital modelling of a specific setting of hypogene Carbonic Acid Speleogenesis (CAS).*" The last sentence will be changed to "*These findings give important insight into mechanisms of carbonic acid speleogenesis (CAS) in a special setting of unconfined aquifers. They also have implications to the understanding of corresponding sulphuric acid speleogenesis (SAS).*"

This clarifies that our paper considers a specific type of hypogene setting and speleogenesis. However, the mechanisms discovered with our model may be applied to other hypogene settings.

Point 2: Abstract: The typo will be corrected.

Point 3: Introduction:

We mostly agree with the comment. To this extend the introduction will be rewritten in order to put our settings within the framework of the two general concepts proposed by Klimchouk and Palmer. The hydrological concept of Klimchouk will be first introduced. We will refer to earlier modelling efforts that explore this concept in terms of "thermal hypgene speleogenesis" and cross-formational upwelling flow. The introduction to the geochemical concept will be extended. We will clarify that our work refers to an idealistic specific hypothetic setting of an unconfined aquifer for both SAS and CAS.

Point 4: Is considered in reply to P3.

Point 5: Thank you for pointing this out. The point can easily be addressed in the introduction. We agree that the settings presented in our work apply to the geochemical concept, which in principle needs mixing of waters with different origin to make the dissolution possible. To avoid confusion, this will be clearly stated in the introduction. By the way we have stated this already at the end of the conclusion: "*Our modelling results have some consequences to the definition of hypogene karst. Both definitions, that by Palmer (2000) from the geochemical view as well as that by Klimchouk (2007;2016) from a hydrological approach state independence of hypogene karstification on surface processes. In CAS as well as in SAS, however, surface processes can have a strong impact.*

Point 6: A direct comparison with other modelling studies and discussion of other related work as requested by both reviewers is not possible because such papers do not exist in the literature. The work of Gong *et al.* uses a modelling domain similar to ours. But it deals with thermal uprising water that gains renewed aggressiveness by cooling on its way up. As heat transport here plays a crucial role as driving mechanism a comparison to our work is not possible.

We suggest to present two new scenarios in the revised version: The first scenario is a response to Steffen Birk's comment referring to our work published in 2010 ("Karstification in unconfined limestone aquifers by mixing of phreatic water with surface"). In this scenario we assume that water from a distant input flows into the aquifer that is now confined by impermeable strata on top of it, and upwelling water enters into that aquifer from below. It turns out that here different processes act because a water table does not exist in this setting. From the results of these scenarios, we show that modeling must start from simple idealized settings to understand situations that are more complex.

Point 7: Will be corrected.

Point 8: We agree that the work is somehow graphical. We have already limited the number of cases to those that clearly present the basic findings. Although the aperture profiles can be estimated from the panels, they provide better "metric" information on the size of the conduits. We therefore in view of the on-line edition prefer to keep the figures.