RESPONSE TO THE REVIEW OF ALEXANDER KLIMCHOUK

We thank to Alexander Klimchouk for the helpful review. We have considered these comments and critics. In this reply we also describe planned changes in the revised manuscript.

The main concern of both reviewers is that our work addresses specific settings of hypogene speleogenesis and that this should be stated clearly. Several other issues have also been addressed by both reviewers. Therefore some parts of our 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 other modeling efforts on hypogene speleogenesis. The two quoted conceptual frameworks (hydrological and geochemical), speleogenetic settings are extremely diverse. All aspects therefore cannot be addressed. However, we agree that a better description is needed how our settings fit within the framework of general conceptual models.

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

We therefore suggest changes in the abstract, introduction and discussion.

The reviewer specifies his comment in three sections (1a-1c, 2, 3a-3c).

Comment 1a:

This has been raised by both reviewers and we agree with the critics. To address it, the first sentence in the abstract "*Hypogene caves originate from upwelling deep-seated waters loaded with CO_2 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.

Comment 1b and 1c:

To address the concerns raised, the introduction will be rewritten to place our setting within the framework of the two general concepts proposed by Klimchouk and Palmer.

The hydrological concept of Klimchouk, will be first introduced. The modelling works, which explore this concept in terms of “thermal hypgene speleogenesis” and cross-formational upwelling flow will be presented to give a broader overview. The introduction to the geochemical concept will be extended.

To conclude: we agree that our concept is not general but a specific case of hypgene speleogenesis. The Introduction will be changed to present it from the perspective of other more general concepts. We will clarify that our work refers to a specific hypothetic setting of an unconfined aquifer for both SAS and CAS.

Comment 1.c: Is considered in reply to 1b.

Comment 2: We are aware of earlier model of hypogene speleogenesis. However, a direct comparison with other modelling studies and discussion of other related work as requested by both reviewers is not possible because other works focus to different processes. 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 plays a crucial role as driving mechanism a comparison to our work is not possible. We will mention the work of Gong in the introduction to give a further example of multiple hypogene situations. We will take care that the reader is aware that out model is not a general one.

In summary: To meet the concerns raised in Comment 1 and 2, we will reformulate the introduction and discussion to clarify that we do not model general hypogene speleogenesis but a specific case. Other modelling studies will be mentioned by adding literature.

Comment 3a:

To study basic mechanisms we have chosen a simple scenario as most of the modelers do to reveal basic mechanisms. As an example, to apply multiple inputs from below adds complications and may hide the basics. Models cannot describe real caves, but in highly idealized settings they are a tool to understand processes acting in the early evolution of caves. Similarly, single fracture is a modeling artifact in epigene speleogenesis, but it is at the same time a basic building block for understanding epigene speleogenesis.

Comment 3b:

We agree that the unconfined setting with deep water inflow may be a special case, although relevant for situations where upwelling regional flows mix with local autogenic recharge. It is obvious that the imprint of past evolution (as for example in confined settings) makes a critical imprint to initial settings of our scenario. This, however, would mask the basic contributions to speleogenesis revealed by our idealised simple setting.

We do not agree that our unconfined setting is unrealistic. It may well be possible that permeable rocks cover the limestone strata thus allowing connection to the surface water. It may also be possible that the confined aquifer does not experience karstification that starts only when the confining cover is removed. Such situations cannot be excluded. It is, however, beyond the scope of our paper to discuss all these details raised in 3b. Actually these concerns question most modeling efforts so far published.

With respect to the geological evolution of an initially confined aquifer we have pointed out that the geological history of the cave evolution would add a complexity that masks the basic contributions to speleogenesis revealed by our idealised simple setting.

Furthermore we have added two new scenarios: The first is similar to our work published in 2010 (“Karstification in unconfined limestone aquifers by mixing of phreatic water with surface” as requested by S. B. Here we assume that water from a distant input flows into our aquifer that is now confined by impermeable strata on top of it and upwelling water enters into that aquifer from below. We have discussed the evolution of this setting by adding a new figure 20. It turns out that here different processes act because a water table does not exist in this setting.

We will add a second new scenario where a simple net of wide fractures is inserted into our net of relatively homogeneous fractures. Therefore we have to add another figure. At that point we want to state that we regard all figures in this work as useful and necessary. With respect of the online version of the paper this should not be a problem. From the results of this scenario we will show that modeling must start from simple idealized settings to understand more complex situations. We consider this as an example of the significance of idealistic simple models as building blocks to understand more complex situations.

Comment 3c:

Yes, it is likely that the temperature of the upwelling water differs from that of the infiltrating meteoric water. This adds complexity to the model as heat transport coupled to density driven flow needs to be considered as well as temperature dependence of calcite solubility. To include that into our model is not possible. One may question therefore the modelling efforts in general. We do not know any model in the literature that is complete with respect to other mechanisms that could act.

To conclude:

We are aware that this model addresses a limited set of situations possible in nature and that it is far from being general. On the other hand we would like to stress that aim of such modelling is to explore new mechanisms which could be important in natural settings. Such mechanisms are the basic building blocks of our understanding of more complex settings. By introducing all the natural complexity at once, these mechanisms cannot be seen. Of course, the complexity could be gradually added.

The model presented here demonstrates how the imprint of the early pathways may define the long-term evolution of conduits and therefore the structure of the resulting aquifer. It also demonstrates how flow focusing provokes mixing of contrasting solutions and sustains the high dissolution rates in the conduits. Focusing may be triggered by small irregularities or it can be a consequence of boundary settings.

Summary:

To summarize, we have revised the text to meet points 1 and 2 of A. K's summary, especially by rewriting the introduction and discussion to clarify that we do not model generally hypogene speleogenesis but a specific case. We have recognized other modeling studies by adding literature as suggested (point 2).

We have also introduced the scenarios suggested by A. K. Furthermore we have pointed the limitations of all attempts to model early karstification and explained why such models can reveal processes but cannot represent real karst systems (point 3). In other words: Models are a good tool to understand karstification but a bad tool to predict it.