Reply to the comments of Prof. Derek Ford:

We thank to Prof. Ford for thoughtful comments. Apart from several smaller comments that are mainly observations, terminological or citing suggestions, we see three main issues raised by Dr. Ford.

1. Comment 4 & 12: How relevant are the rectilinear network ?
Reply: We surely agree that the model structure and domain is an oversimplification of natural scenarios. As stated by Dr. Ford, speleogenesis has been modelled in a fracture with stochastic initial aperture field (Hanna & Rajaram, 1997), also embedded in 3D variably saturated porous matrix (Annable, 2002). These models have added complexity and pointed to some of the shortcomings of the models with simpler geometry. However, most of the processes revealed by such models, could also be even more clearly discussed in simpler rectilinear network (see the book of Dreybrodt et al., 2005). Additionally, these models are applied to study early protoconduit evolution with a laminar flow, which is not the case here.
Our model in principle allows any distribution of conduits, which might or might not be rectilinear. The idea followed in our work, is to keep the geometry simple and search for the basic mechanisms of the flow pathway selections. We have modelled some other configurations and came to the similar results. To some extent, the scenario in Fig. 13 could be considered as such, as the conduits are not parallel/normal to the dip. However, for the mechanisms described, changing the arrangement of conduits does not change the message of this paper.

Proposed action for the revised version: *We will add discussion related to this comment in the introductory and concluding part.*

2. Comment 4: How were our initial conditions formed ?
Reply: One of the shortcoming of our model is that that it starts with conduits that have passed the initial karstification phase, as it only works for the turbulent flow. In principle, it is feasible to combine this model with models that have been applied to calculate initial phases of conduit network evolution (see the book by Dreybrodt et al. (2005)). We have not done that yet, but it is one of the future tasks.
A lot is known about the selection mechanisms in fracture networks in the early stages of laminar flow regime and post breakthrough phase with expansion/integration of networks. Any of these networks could be an initial state in our model.

Proposed action for the revised version: As also suggested by the second reviewer (Dr. Birk) we intend to add a case demonstrating basic mechanisms of a conduit network evolution before the breakthrough and in the pressurised flow regime. This will make the paper more self contained and the pathway selection mechanisms in pressurised and free surface flow regimes will be clearly distinct. We will also extend the discussion by proper citations and remarks concerning this comment.

3. Comment 11: What new results do we get from the model compared to the existing results of physical and numerical modelling ? ***In particular:*** "I have found no results in this paper that would have surprised Ralph Ewers and me forty years ago ...".
Reply: To some extent we agree (as stressed by both reviewers) that we have failed (?) to clearly *present* the novelty of results with respect to the existing models. In fact, the results have not surprised us as well.
At this point one could discuss about the aims of such modelling: The speleogenetic models serve primarily to support/oppose the conceptual models and to point to the processes which are most probably present in the nature. So far, we have not seen many big surprises in any speleogenetic

models, however many mechanisms have been revealed which have been overlooked by simple empirical reasoning (see e.g. book by Dreybrodt *et al.* (2005)).
With the existing knowledge and computational power, it is possible to build very complex models; although these give impressive and realistic results, we often fail to recognise the basic governing mechanisms leading to them. That is why we advocate and use gradual building of the model complexity.
Some basic mechanisms shown here have been described and demonstrated in other existing models. Such is the integration of network due to redistribution of hydraulic head after breakthrough of the primary tubes.
Such integration can be observed in our low dip scenario and is a consequence of the hydraulic head drop along the pathway that first connects the inputs to the outputs. This attracts flow and growth from the other evolving pathways and results in integration into a branchwork pattern. This has been demonstrated many times in other physical/numerical models and is now a part of classic textbooks (e.g. Palmer, 2007; Ford & Williams, 2009).
Our model shows new mechanisms, which are intuitive, but have not been discussed so far:
- the selection of pathways is limited to a node (junction). Nodal geometry (i.e. initial conditions, incision in phreatic phase) at the transition to free surface flow regime and slope of outlet conduits are decisive for the final configuration of drainage network.
- High dip network has not been modelled in similar conditions. Similar models (Gabrovšek & Dreybrodt, 2001; Kaufmann, 2003) mainly discuss the drawdown of the water table and related formation of the water table caves. But the dissolution is limited to the phreatic zone. In this model, the dissolution of the vadose zone and formation of vertical patterns is modelled and discussed.

Proposed action for the revised version: *See also the answer to Comments 4 and 12. We will stress the novelty of results also by extending the review of the existing models and clearly pointing to new mechanisms which have not been discussed so far.*

Replies to other comments:

Comment 1:
Reply: We have kept our original terminology in the title. The term *Phreatic-to-Drawdown Vadose conditions* also describes scenarios discussed in this manuscript. However we believe that for somebody less acquainted with literature on speleogenesis, the term "*pressurised to free surface flow*" conditions is more understandable.
Proposed action for the revised version: We will additionally mention that we model *Phreatic-to-Drawdown Vadose conditions* and give proper citations.

Comment 2:
Reply: We agree with the reviewer.
Proposed action for the revised version: We will change text accordingly and gave proper citation in the Introduction.

Comment 3:
Reply: We agree with the reviewer.
Proposed action for the revised version: We will change text accordingly and gave proper citation in the Introduction.

Comment 5:
Reply: We have cited some works dealing with the use of SWMM in karst aquifers, but we are surely not aware of them all. We thank to the reviewer for pointing to new references.
Proposed action for the revised version: We will change text accordingly and gave proper citation in the Introduction.

Comment 6:
Reply: The comment probably refers to the Fig. 4c and not Fig 5.The "master" conduit is an artefact of the model, placed to enable free outflow from the modelling domain. They have no influence on the genesis and are not considered in our discussion
Proposed action for the revised version: We will change text to describe the role of the outfall conduit clearer.

Comment 7:
Reply: We agree with the reviewer.
Proposed action for the revised version: We will do as suggested and change it accordingly in Figures and in the text.

Comment 8:
Reply: We agree with the reviewer.
Proposed action for the revised version: We will add some discussion relating our models to some of the natural settings.

Comment 9 requires no respond.

Comment 10:
Reply: We are familiar with the work of Prof. Frumkin. The main reason for taking "salt" in our work was to stress the importance of hydrodynamics for dissolution and not to study caves in salt in particular.
Action: We will expand a discussion on dissolution kinetics (that concerns also the comments of Dr. Birk) and also refer to the work of Prof. Frumkin.