Response to comments by *W. Dreybrodt (comments by the referee are in italics)*:

The results are impressive, but their discussion and interpretation is extremely difficult to read because the physical processes causing them are not sufficiently revealed. It is possible, however, to interpret the results from a few well-known facts about the evolution of such simple standard karst aquifers.

We tried to address the physical processes that cause the observed behaviour briefly in our manuscript to keep it short. We now realize that our explanations may be too short and not sufficiently clear, and we are willing to rewrite these passages following the suggestions by W. Dreybrodt.

For the scenario with lognormal distribution with σ/μ0 = 0.1 one finds breakthrough times ranging between 2.5 ka and 5 ka whereas for distributions with σ/μ0 = 1 times range between 5 ka to 30 ka. The reason for this is not explained (page 5641, lines15-25). Inspecting the initial distributions for the high and low heterogeneity scenarios reveals that the high heterogeneity distribution, HHD, (σ/μ0 = 1) contains 50% of tubes with diameters below 0.3mm. Since their hydraulic resistance is related to the fourth power of the diameter, their resistance is higher at least by a factor of 10 compared to the remaining tubes with diameters above 0.5 mm. In contrast to the net with low heterogeneity distribution, LHD, (σ/μ0 = 0.1) where 50% of the tubes exhibit diameters above 0.5 mm the HHD contain high resistance pathways, which increase breakthrough times, BT, and cause also large variations.

The reason for the different variability of breakthrough times is briefly indicated in the lines following those mentioned above, namely in lines 25-27 on page 5641: "Thus, […] the initial bulk permeability tends to decrease with increasing heterogeneity since it is controlled by the smallest apertures." We assumed that it is obvious that the number of small apertures in the high heterogeneity distribution is higher than that in the low heterogeneity distribution. Another difference to the explanation provided by W. Dreybrodt is that our statement is very general, while he explicitly mentions the relationship between aperture and hydraulic resistance. We thus suggest that we extend our explanation similar to that provided by W. Dreybrodt, thereby referring to Fig. 3, which shows the initial aperture distribution and thus reveals the higher number of small apertures in the high-heterogeneity setting.

It may be interesting to plot the distribution of BT for both cases.

We are reluctant to add this plot, because we would like to keep the number of figures low and there are other comments below which suggest that at least two other figures should be added. In addition, the distribution of breakthrough times - even though generally of interest - does not seem to be highly relevant with respect to the topic of this paper, i.e. the evolving aperture distributions.

Animation 2 shows, how the distribution of diameters changes in time. For times below TB there is a slight widening and the distribution stays one-modal for all realisations. Immediately after breakthrough some pathways connecting the input and output boundaries attract all the water (animation 1) and widen quickly creating a second peak in the distribution of diameters, whereas almost all tubes outside these pathways stop widening.

We fully agree with this description. It is consistent with our description given in section 4.1.2 and it may enhance the readability of the paper if it is added to the paragraph describing animation 2. We thus suggest to add this or a similar sentence in line 16 on page 5641 (after "… in Anim. 2."), e.g.: "Before breakthrough there is a slight widening of all apertures and the distribution stays unimodal for all realisations. Immediately after breakthrough some pathways connecting the input and output boundaries attract almost all the water (Anim. 1), widen quickly and thus create a second peak in the aperture distribution, whereas the widening of nearly all tubes outside these pathways ceases."

As can be seen in animation 2 flow rates after breakthrough are higher than 10-7 m3/s. If flow is restricted to some value Qmax >10-7 m3/s breakthrough happens and the diameters of the tubes involved widen under first order kinetics linearly in time. The figure attached presents the flow rate through the aquifer as function of time. For all points A above a critical flow rate Qmax ≈ Q(TB) one finds a bimodal distribution of tube diameters.

Again, we fully agree and find this is consistent with our explanation given in section 4.2, in particular within the lines 13-24 on page 5643. Yet we obviously do not clearly enough mention the development of flow rates in time and the threshold marking breakthrough. We agree that this will help in understanding the processes that lead to the different type of karstification if flow is strongly limited. Thus, we suggest that we modify our explanation given in lines 13-24 on page 5643 accordingly and add a figure similar to that provided by W. Dreybrodt.

For points B with Qmax< Q(TB) the situation changes. Because the penetration lengths L1 for first order kinetics and that for higher order kinetics Ln are related linearly to the flow rate Q further penetration of breakthrough pathways is stopped once Qmax is reached, because the feedback of increasing L1 with increasing Q is switched off. For points B with Qmax well below TB only the entrance tubes will experience widening by first order kinetics, because L1 « L, the length of the tube. For all the other tubes higher order dissolution kinetics is active.

Our explanation in lines 1-7 on page 5646 is quite similar. However, we do not explicitly refer to the penetration lengths for first-order and higher-order kinetics. Here and elsewhere in the manuscript (lines 21-26 on page 5646, lines 14-17 on page 5647) we refer to the distance at which the water reaches the switch concentration (i.e. where the dissolution kinetics switches from the fast linear to the slow non-linear rate law), which is closely related to the penetration length for first-order kinetics (see equations in the appendix). We agree that mentioning the linear relationship between the flow rate and the distance where the switch concentration is reached (or the penetration length) will establish a better and more quantitative understanding of the processes as requested by the two reviewers. This can be easily done by modifying lines 1-7 on page 5646, e.g.: "The distance at which the switch to the slow higher-order kinetics is reached increases linearly with the flow rate. Yet, in the scenarios considered here, recharge is so strongly limited that the increase of flow rates is suppressed at a level that does not permit the calcium concentration at the discharge boundary to fall below the switch concentration. Hence, the positive feedback between conduit enlargement and increasing flow rates is switched off before breakthrough and thus breakthrough does not occur."

It is a different matter whether or not the explanation should be based on the penetration length for first-order kinetics or the distance at which the switch concentration is reached. As the dissolution rate laws are introduced in the paper, the meaning of the latter will be clear to the reader, whereas many readers will be unfamiliar with the concept of the penetration length. The above comment further mentions the penetration length for higher-order kinetics – we will come back to this below.

Because Ln increases with (1-c(x)/c)1-n after some distance xs in the net Ln will be much larger then the length of the aquifer and dissolutional widening will be slow and almost even in all the tubes beyond xs .Therefore one expects few wide tubes at the entrance causing something like surface denudation, whereas the diameters of the remaining tubes widen slowly from 5Â°u10-4 m to 5Â°u10-2 m in 3Ma.This is an entirely different mode of karstification without any preferred pathways of flow.

This comment on the "entirely different mode of karstification" refers to subsection 4.2.2 Suppressed flow. We agree with the general description given in the comment ("few wide tubes at the entrance", "remaining tubes widen slowly") and provide similar statements in our paper. However, as pointed out in the comment by F. Gabrovsek, the penetration length for higher-order kinetics will not be much larger than the length of the aquifer. We will come back to this in our response to the comment by F. Gabrovsek concerning lines 21-22 on page 5649.

The local distribution of the tube diameters in the net should be shown by a figure.

As the focus of the paper is on the evolution of the aperture distributions, we decided to avoid showing conduit patterns (i.e. the spatial distribution of tube diameters). In fact, most of the earlier publications on karst evolution models only showed conduit patterns but not aperture frequency distributions. Therefore, we agree that our paper will be easier to understand if we add this figure (and another one as suggested in a comment below). This is particularly true for the case of conduit development under suppressed flow conditions, because this was hardly addressed in publications before. We intend to add this figure and corresponding explanation within the text passage on the long-term simulations starting in line 5 on page 5647.

Animation 5 for Qmax< 10-8 m3/s shows nicely a one-modal distribution shifting to higher diameters in time and with increasing half width. The entrance tubes show up as a little peak at about 0.02 m, see Fig. 5.

We fully agree and provide a similar description in our paper within the lines 5-20 on page 5647, where this Figure (Fig. 7, not Fig. 5) and the animation are explained.

In between the two extreme cases of Qmax> Q(TB) with breakthrough and conduit evolution and Qmax< Q(TB) with even widening of all tubes without evolution of large conduits from the input to the output boundary intermediate cases must be considered. If Qmax< Q(TB) is very close to Q(TB), point B in the attached figure, some breakthrough channels have already invaded deeply into the aquifer but have not yet reached the output boundary, see animation 1 at 16.2 ky. Pathways originating at such a channel exhibit higher dissolution rates in comparison of most of the other tubes and will grow slowly towards the output. This may be *the case for the scenario with Qmax = 10-7 m3/s. After 3Ma it shows a bimodal distribution of tube diameters with a maximum at0.05 m. In contrast to the extreme breakthrough case the distribution is continuous. From animation 5 one finds that after 1.5 Ma dissolution is active only in tubes with diameters larger than 0.01 m. The authors should also address this intermediate case by presenting a figure of the spatial distribution of the tube diameters in the net.*

Again, we fully agree. Our corresponding description (from line 21, page 5647 to line 8, page 5648) is consistent with this comment, but focussed on aperture distributions rather than on the conduit patterns. Yet we agree that it will ease understanding if we add some remarks on the evolution of the conduit pattern similar to the comment above (e.g., "some channels have already invaded deeply into the aquifer but have not yet reached the output boundary") and if this is illustrated by one additional figure showing the conduit pattern at the end of the simulation period. We thus will modify the text passage from line 21, page 5647 to line 8, page 5648 accordingly.

If the authors revise the paper using the ideas outlined above as a frame to discuss and interpret their results the paper should be acceptable for publication. It has potential and presents an impressive set of data. The interpretation of the results, however, is entirely descriptive and incomplete.

As indicated in our responses above we are willing to incorporate the ideas provided by W. Dreybrodt. Nevertheless, we would like to point out that these ideas are, just as in most of the earlier papers on karst evolution modelling (see citations in the introduction), focused on the evolution of the (individual) conduit pattern, whereas the focus of our paper is on the evolution of aperture distributions in a number of statistical realisations. Several aspects related to this approach, such as the variability among realisations with identical statistical properties, are ignored in these comments but highly relevant with respect to our topic (see also conclusion section, lines 19-27 on page 5652). In our view, the paper therefore must not be simplified to the story of the evolution of conduit patterns with differing initial heterogeneity and varying maximum recharge.

Furthermore the paper is too long, especially the introduction. I had to read it several times until I found out, how to understand the results.

While we agree that this paper is not particularly short, it seems to us that the length of the paper is approximately equal to the average HESS paper and probably similar to many papers on similar topics in other journals. Nevertheless, we are willing to shorten the paper where possible. For instance, we suggest to delete the text passage from line 17, page 5633 to l. 8, p. 5634 and to include (most of) the citations in the (modified) sentence starting in line 9 on page 5634. We will also attempt to shorten the text passages following below in the introduction. Further, we intend to shorten the description of the dissolution process in section 2 as suggested by the referee F. Gabrovsek. In addition, it might be possible to delete the paragraph from line 26, page 5649 to line 9, page 5650 – we will come back to this in response to a comment on this text passage by F. Gabrovsek.
We are convinced that the incorporation of the ideas proposed by W. Dreybrodt, in particular the additional figures showing the development of flow rates in time and the

conduit pattern under suppressed flow conditions, will make it easier to understand our results.

Response to general comments by *F. Gabrovsek (comments by the referee are in italics)*:

The paper is lengthy and hard to read. The initial ideas are not clearly presented as they are hard to find within the text.

See our last response to the comments by W. Dreybrodt concerning the length and readability of the paper. The objectives of the paper are clearly stated at the end of the introduction and it will be even easier to find them if the introduction is shortened as suggested in our aforementioned response. In accordance with these objectives, the result section is structured in a subsection addressing the effect of heterogeneity and one addressing the effect of limited flow rates. The latter is further subdivided to account for the different types of conduit development, just as in the comments by W. Dreybrodt.

Authors basically vary two parameters, sigma/mju and Qmax and explore the resulting geometries by observing the aperture distributions. After reading the text a reader stays rather confused on what are the conclusions. These should be more clearly stated.

The conclusions are clearly stated in the conclusion section from line 5 on page 5652 to line 2 on page 5653. Unfortunately, none of the two referees provides any specific comment on this section. Therefore, it is unclear to us why the referee is "confused on what are the conclusions". The first paragraph of the conclusion section includes various aspects similar to those mentioned in the comments by W. Dreybrodt (but focused on resulting aperture frequency distributions rather than on conduit pattern), whereas the second paragraph is focused on statistical aspects, such as the variability among realisations with identical statistical properties, which is not addressed in the comments by the referees.

The weak point of the work is the interpretation of the modelling results. Authors often use expression "appears to be", where they should give clear physically based interpretation of results.

We agree that in many places "appears to be" can be replaced by "is" and intend to do so. We do provide physically-based interpretations, but as indicated in our responses to the comments by W. Dreybrodt we agree that they are sometimes too brief and not sufficiently clear and need to be supported by additional figures.

Some interpretations are also questionable.

After having examined the comments by F. Gabrovsek in the annotated manuscript, we agree that there is one interpretation in the discussion section that is somewhat misleading though not wrong (see the last two specific comments by F. Gabrovsek). Since the text passage is not essential to any of the conclusions and only intended to provide a comparison with earlier work, it can simply be deleted. Nevertheless, we will clarify the interpretation in our responses to this specific comment.

Some particular comments are given in the attached pdf file. Note that the comments given there are still to particular and a respond to these will not improve the paper sufficiently. Instead, a deep revision based on these comments and on the comments posted recently by W. Dreybrodt, is needed.

We will address the specific comments below. We appreciate the referee's specific suggestions regarding the readability and regarding the aforementioned misleading interpretation. These will be addressed in the revision of the paper. Yet there are some comments that suggest that parts of the work are misunderstood by the referee. Some of these comments refer to flow settings that are not considered here, others do not account for some findings from our model runs (which are also in accordance with results from the cited literature). It will hardly be possible to account for these comments in a deep revision but we will provide suggestions how we want to improve our explanations in the text to avoid the misunderstandings that we think are apparent from these comments.

Response to specific comments by *F. Gabrovsek (comments by the referee are in italics)*:

Comment on lines 19-22 on page 5633: *Siemeres and Dreybrodt (WRR, 1999) were looking for the pattern at the breakthrough: the result which they have got comes from the fact, that the difference between TB (= breakthrough times) of the competing pathways increases with increasing TB. However, they do not discuss complexity after the breakthrough. Furthermore, they give reasons why the net is more uniformly developed when you have higher occupation probability.*

In a response to the comment by W. Dreybrodt that the introduction is too long we suggest to delete this text passage such that this comment will be obsolete. Nevertheless, we want to point out that we cite Dreybrodt and Siemers (2000) (not Siemers and Dreybrodt 1999) who write: "[…] cave patterns depend on breakthrough times in such a way that simple mainly linear patterns arise for large breakthrough times. […] breakthrough time decreases, the patterns become branched and more complex." Therefore our statement is fully in accordance with the cited literature. Later in our paper our results show (and this is already known, e.g., from Birk et al., 2005, which is cited in our paper) that the pattern do not change after breakthrough if the flow rate is limited and thus the conclusion drawn by Dreybrodt and Siemers (2000) is relevant to us.
Here and in some of the following comments the referee seems to indicate that the results from the cited literature need to be explained in more detail (this is not clearly stated but e.g. the referee repeatedly comments "why"). This is obviously conflicting with the request to shorten the introduction (see comments by W. Dreybrodt).

Comment on line 26 on page 5633: *Why?*

As stated in our response above, any additional explanation is conflicting with the request to shorten the introduction and as we suggest that this passage is deleted the comment will be obsolete.

Comment on lines 4-6 on page 5634: *Why is enlargement more uniform after breakthrough. It is clear that the winning passage widens more uniformly, but tell why maze development is more active...*

Again, any additional explanation is conflicting with the request to shorten the introduction.

Comment on line 12 on page 5634: *end members*

Elsewhere in the manuscript (line 22 on page 5647) we use "end-members" and this is also the notation used in the cited literature. We thus propose the consistent use of "end-members".

Comment on line 15, page 5634: *Bloomfield et al. is cited several times and results are compared to their results. At some point you should tell few words about their modelling concepts,*

It is true that we only mention that they use a generic rate law instead of the (empirically established) rate laws considered here and in most of the earlier studies on karst evolution. This rate law is the main difference between the modelling approaches, and we are willing to provide more details on the rate law they used (e.g. a brief explanation such as "aperture growth is assumed to be a first-order polynomial function of the flow rate"); we will also mention that they only consider Darcian flow (while in our model flow can be turbulent). We already mention that the geometric setup of our model corresponds to that used by these authors (page 5637, lines 25-26).

Comment on lines 24-25, page 5634: *This has been done much before and also explained why (e.g. Gabrovsek, 2000; Dreybrodt et al., 2005)*

Yes, this is true as far as the conduit pattern is concerned and we mentioned this earlier in the introduction where we cite Dreybrodt et al., 2005. However, at least Dreybrodt et al., 2005 do not show aperture frequency distributions, which is the issue discussed in this sentence.

Comment on line 7, page 5636: *What about CO2 conversion; if you mention the rate limiting processes, mention them all. However, I would skip large part of this section and give appropriate citations!*

We agree that the text passage on the dissolution kinetics can be shortened and we are willing to do this in the revision of the manuscript.

Comment on lines 22-24, page 5636: *I do not like the expression" reaction control". There are many reactions in the bulk of the fluid as well. However, you want to stress "surface reaction". Commonly, surface control is used for what you talk about here. Furthermore, also surface controlled reaction show two regimes os dissolution...*

We will use the term "surface control" proposed by the referee when we revise the paragraph (see response to previous comment).

Comment on lines 2-3, page 5638: *Siemers and Dreybrodt do not unse Log-Normal distribution. If I remember right Hanna & Rajaram use spatially correlated distribution. Similar log-normal distribution was unsed in Gabrovsek et al. (Journal of Hydrology, 2004).*

The referee is partly right: Hanna and Rajaram (1998) indeed used a spatially correlated distribution and therefore we will replace the citation by the more appropriate one provided by the referee. However, Siemers and Dreybrodt (1998) write: “We have, however, also performed model calculations of breakthrough times with statistically distributed apertures, using a lognormal distribution with mean values of a_0 from 0.03 to 0.1 cm and standard deviations of 0.01 cm.” Contrary to the referee’s comment this suggests that they have used log-normal distributions.

Comment on line 3, page 5638: *Is this the real mean ? mju in log normal, means the mean of the logarithm of variable....*

The referee is correct, μ is the mean of the logarithm of the aperture. We will clarify this in the text when revising the manuscript.

Comment on lines 18-27, page 5638: *This paragraph is confusing, although crucial for further understanding. If I understand right you define qmax so that qmax*i=Qmax. In other words, you define maximal inflow into an input node. When recharge to the node is qmax, it stays there and this is valid for all input nodes. However it is not necessary that all input nodes reach qmax and consequently that total inflow reaches Qmax in your runs (depends also on how long you left your simulation running). If my understanding is correct, I suggest that you tell that you simply limit the inflow to each input node to qmax.*

We agree that this is crucial for understanding and can be worded much simpler than we did. We will therefore revise this text passage as proposed by the referee, which will also contribute to shorten the paper. However, it should be noted that, contrary to the statement by the referee, it turns out that Qmax is almost always reached immediately after breakthrough, i.e. the time when the total inflow is limited nearly coincides with the breakthrough time. This is shown by Fig. 6 (please note the corresponding remark in the figure caption) and mentioned in the corresponding text on page 5645, lines 14-16. Since some of the following comments seem to be based on the (wrong) assumption that there is a significant time period after breakthrough during which the total inflow continues to increase, we think that this has been overlooked by the referee. The observed behaviour is explained by the rearrangement of the hydraulic head distribution after the breakthrough (see e.g. Dreybrodt et al. 2005): The breakthrough and the subsequent flow limitation at the corresponding input node causes a head drop along the highly conductive pathway and thus steep hydraulic gradients between the other input nodes and that pathway evolve; as a consequence, there is a rapid development of tributaries that connect the other input nodes to the highly conductive pathways. It seems to us that the missing description of this behaviour is the main reason for the difficulties in understanding the paper. We will therefore try to explain this early in the result section, e.g., in section 4.1.1 where the development of the conduit pattern is described.

Comment on lines 23-25, page 5639: *A bit confusing: A reader might thing that there is no breakthrough is only non-linear kinetics is valid... Which is not the case.*

It is unclear to us what the referee is saying here. As mentioned in lines 16-17 on the same page, we define breakthrough as the situation where first-order kinetics is active along an entire pathway. Although there are also other ways to define breakthrough, this one is very frequently used. The explanation given in the lines commented by the referee is consistent with this definition and with the descriptions of the feedback mechanism found in the literature.

Comment on lines 6, page 5640: *This is a bit hard statement. First, other pathways do not develop independently (the gradients in the net are strongly influenced by the most efficient pathway);secondly*

We agree that the evolution of the "additional" conduits is strongly influenced by the most efficient pathway and therefore we will delete the term "independently" which seems to have caused confusion. Moreover, as pointed out in the response to the comment on the flow limitation, we will modify the text passage to address the redistribution of the hydraulic heads and its influence on the evolving conduit pattern as well as the consequences with respect to the flow limitation at the inflow nodes.

Comment on lines 10-11, page 5640: *Not necessarily true: After the breakthrough, one would expect the "inflation" of the evolved network (see Dreybrodt et al., 2005) if no flow limitation (or large qmax) is considered.*

The paragraph addressed by this comment provides a description of the evolution of the conduit pattern shown in Fig. 2 but is also valid for all other "competitive flow" scenarios considered in the paper (see also our above responses related to the flow limitation). We do not consider scenarios without flow limitation in our paper (although we have conducted these simulations too), firstly, because there is no environment with infinite flow, and secondly, because the result is quite obvious (all tubes develop if the simulation time is sufficiently long).

Comment on lines 22-25, page 5640: *Is there a simple explanation why ?*

This result is intuitively expected because in a heterogeneous network the flow along the regional hydraulic gradient will be more frequently hampered by small apertures than in a homogeneous net. As pointed out in our paper this finding is not new and therefore it does not seem to be appropriate to provide a discussion on this aspect (although we can do so if the Editor advises). The main intention of this paragraph is to show that our model results are consistent with similar earlier models and to provide the basic knowledge required for understanding the following sections, which represent the main novelty and innovation of the paper.

Comment on lines 9-11, page 5641: *One of the issues is that you stop at 30 ky for all cases. What if you would use the same T/Tb (e.g. 2TB), which in a way better defines the stage of evolution. Would the conclusions be the same.*

This is not an issue at all – see our above comments on the flow limitation. Generally, the total flow limitation is reached immediately after breakthrough and from then on the structure of the conduit system (and thus the number of large tubes) does not change anymore. This is explained within the text passage (lines 5-11 on page 5640) addressed in two of the comments above. Therefore, it does not matter whether we use the same T/Tb or

30 ka. This is also demonstrated by the animations showing that after breakthrough the frequency distribution of the small apertures is “frozen” (stays constant), thus showing that the number of small tubes and consequently also the number of large tubes stays constant after breakthrough (see lines 19-21, page 5644); also Fig. 8 shows that the number of large tubes stays constant after breakthrough. Again, this misunderstanding can be resolved by modifying the text in section 4.1.1 (as suggested in response to the comment on line 6 on page 5640), which provides the basic understanding of the processes.

Comment on lines 13-14, page 5641: *In what way and why ?*

The minimum number of largely widened tubes is defined by the number of tubes along a straight line from a node at the recharge side to a node at the discharge side. In fact, the number must be higher than this minimum because of the additional tubes that connect all the input nodes to the main pathway. In any case it seems obvious to us that the ratio of these large tubes to the total number of tubes is dependent on the geometric setup of the network. For instance, if the size of network is increased in both directions by the same factor the number of tubes connecting input and output nodes increases by this factor too but the total number of tubes in the network increases by the square of this factor. Our intention was to indicate that the percentages from the different scenarios should be considered relative to each other, whereas the absolute percentage has to be interpreted with great care. Since the sentence addressed by this comment seems to create confusion rather than it helps, we propose to the delete it when revising the manuscript.

Comment on lines 16-20, page 5642: *At this point I only partly agree. I would argue with following statements:*
1. If the recharge keeps the initial head at the input of the winning pathway, the region of enlarged (post breakthrough conduits) will grow and occupy the whole net. This has been demonstrated by other models and can be proven by simple arguments.
2. If the recharge is limited, the head drop at the entrance and along the winning pathway will attract flow from other pathways.

The referee refers to a scenario that is not considered in our paper. We consider only settings where flow is limited (i.e. 2.) but not the situation where the head is kept constant after breakthrough (i.e. 1). A constant head cannot be maintained after breakthrough in a real setting. As pointed out in our response to the comment on lines 10-11, page 5640, we have conducted these simulations too, and obtained the result mentioned by the reviewer. Inclusion of this scenario is pointless, as the result is obvious and well known (see comment by the reviewer) and does not contribute to any of our objectives and conclusions.

Comment on line 7, page 5644: *There must be simple argument for that...*

We agree. The first-order dissolution rate in the pathway that has broken through is proportional to exp(-1/Q) and the rate of aperture growth da/dt is proportional to the dissolution rate (see e.g. Dreybrodt et al. 2005). Thus, the rate of aperture growth increases with increasing flow rate. We will add this (or a similar) remark when revising the paper.

Comment on line 9, page 5644: *The number of evolved tubes increase with increasing maximum recharge. Why: the evolved network "inflates" until qmax is reached. The consequence is: higher qmax, more evolved fractures.*

This is exactly what we were saying. We used the number of tubes that did not evolve for the explanation because we think it is easier to compare them than the number of tubes that do evolve in Fig. 4.

Comment on lines 6-7, page 5649: *Which model of Kaufmann & Braun do you refer to ?*

We refer to the conclusion they draw in the cited paper. This conclusion is drawn from the scenarios with fixed recharge described in that paper. We will add "… in model scenarios with fixed recharge." at the end of the sentence to clarify this.

Comment on lines 21-22, page 5649: *Why ??? Comparison to high cin case is questionable ? There, a constant head is kept high. High cin makes the penetration lengths (lambda) long, so there is a small variation of dissolution rates along any chosen pathway and also between different pathways. That makes their growth rates comparable and the networks widens uniformly. However, I think that the case of limited flow cannot be interpreted the same way. The penetration length even drops in time after the qmax is reached. The only mechanism I can envisage there is an integration of other pathways to one which reaches the qmax first.*

We provide a discussion of the penetration length which has been addressed by both referees in the appendix attached to this response. However, we are reluctant to incorporate this in the main text of the paper, as we feel this will strongly impair the readability of the paper and is not required to understand the paper. Please note that there is no "comparison to high cin case" in the text passage addressed by this comment but only in the following.

Comment on lines 3-8, page 5650: *Same as in the previous comment..*

We agree that the "comparison to the high cin case" is questionable. In the manuscript we just say "the effects appear to be similar" but evidently this may suggest to the readers that the mechanisms are similar too, which is not the case (as rightly pointed out by the referee F. Gabrovsek). The comparison of the aperture distributions resulting under the two different conditions (flow limitation versus high input concentration) is hardly possible because aperture frequency distributions (to our knowledge) are not published for the high-input-concentration scenarios (apart from one single scenario addressing gypsum karst – Rehrl et al. 2008). Even if available it would be problematic to compare aperture frequency distributions from different model setups. Thus, it seems to us that this comparison should be done in future work using one consistent model setup for both conditions. In the light of these comments the discussion provided in our manuscript from line 26, page 5649 to line 9, page 5650 is rather speculative and perhaps misleading. As this text passage is not required for understanding the other parts of the paper and does not contribute to our objectives and conclusions we suggest that it will be deleted when revising the manuscript.

Comment on Fig. 8, page 5666: *I do not understand the red set of curves on the panel a. It means that in conditions of highly suppressed flow, the portion of evolved fractures grows. I* *see no reason to that. If you highly limit the flow: I expect that (particulary in case of tubes) the dissolution rates drop along pathways, inputs probably integrate, but no further expansion of the evolved net occurs. I would like to see an example o this.*

Flow and dissolution rates are very low throughout the network. Since the recharge is extremely limited, the competition of the different tubes for available water is switched off and preferential pathways do not develop as they do in the case of higher flow rates. As can be seen from the evolution of the aperture distribution shown in Fig. 7, indeed some tubes grow faster than others. However, because the total flow rate is limited at a very early stage there is no separation of a "winning" pathway (i.e. one that has broken through) and thus all tubes continue to be enlarged (albeit at different rates). Hence, the range of aperture sizes increases during the simulation but the aperture distributions do not show the gaps between small and large apertures which are typical for the competitive flow scenarios. This is also confirmed by test runs for much longer times than shown here (mentioned in lines 8-10, page 5647). We will add a figure showing the conduit patterns (as already mentioned in the response to a comment by the other referee), which will make it easier to understand the explanation given in the manuscript.

Appendix

In their comments both reviewers refer to the penetration length, which is not mentioned in our manuscript. To clarify their comments and our responses this appendix provides some equations taken from Dreybrodt et al. (2005). We will use these equations to derive an expression for the penetration length for the conditions considered in our models.

The first-order dissolution rate is given by (here and below the numbers of the equations refer to those in Dreybrodt et al. 2005; symbols that are not explained correspond to those in the manuscript):

$$F(x) = F(0)\exp\left(-\frac{x}{\lambda_1}\right) \quad x \le x_s \qquad (3.27)$$

where the distance x_s at which the rate law switches from first-order to higher-order dissolution kinetics is given by

$$x_s = \frac{Qc_{eq}}{Pk_1} ln\left(\frac{1-c_0/c_{eq}}{1-c_s/c_{eq}}\right) \qquad (3.18)$$

Note that the concentration in the inflow c_0 = 0 in our scenarios and the perimeter $P = \pi a$. The penetration length for first-order kinetics is given by

$$\lambda_1 = \frac{Qc_{eq}}{Pk_1} \qquad (3.23)$$

With the values used in our models (c_0 = 0 and c_s = 0.9 c_{eq}) the distance at which the switch to higher-order kinetics occurs x_s = 2.3 λ_1. We agree with the referee W. Dreybrodt that mentioning the proportionality between λ_1 (or equivalently x_s) and the flow rate Q will make it easier to understand the physical reasons for the behaviour observed in the model simulations.

If $x > x_s$ the dissolution rate is given by

$$F(x) = F(x_s)\left[1 + \frac{x - x_s}{\lambda_n(x_s)}\right]^{\frac{n}{1-n}} \quad (3.28)$$

and for $x_2 > x_1 > x_s$

$$F(x_2) = F(x_1)\left[1 + \frac{x_2 - x_1}{\lambda_n(x_1)}\right]^{\frac{n}{1-n}} \quad (3.29)$$

where the penetration length for higher-order kinetics is given by

$$\lambda_n(x) = \frac{Qc_{eq}}{Pk_n}\frac{\left(1 - c(x)/c_{eq}\right)^{1-n}}{(n-1)} \quad (3.25)$$

Inserting the concentration profile for $x > x_s$

$$\left(1 - c(x)/c_{eq}\right)^{1-n} = \left(1 - c_s/c_{eq}\right)^{1-n} + \frac{Pk_n(n-1)(x - x_s)}{Qc_{eq}} \quad (3.17)$$

yields

$$\lambda_n(x) = x - x_s + \frac{Qc_{eq}\left(1 - c_s/c_{eq}\right)^{1-n}}{Pk_n(n-1)} \quad (3.17/3.25)$$

In the case of the scenarios with strongly limited flow (termed "suppressed flow" within the manuscript), x_s is low, typically less than the length of one tube, i.e. first-order kinetics is operating only close to the inflow boundary. From then on the rates drop according to eq. (3.28). As pointed out by the referee F. Gabrovsek, the penetration length for higher-order kinetics is not very large either, i.e. the dissolution rates drop rapidly along the flow distance. For the lowest flow rates considered in our paper the higher-order penetration length will be in the order of metres and less (depending on the diameter of the tubes) at $x = x_s$. Since x_s and the third term on the right-hand side of eq. (3.17/3.25) are very small the higher-order penetration length approaches x (in fact it stays slightly below x) at distances $x >> x_s$ and thus λ_n is almost unaffected by the different flow rates and the different aperture sizes within the network. This suggests that after some distance the dissolution rates decrease in a similar way in all tubes independent of their aperture, which might provide an explanation for the continuing enlargement of all tubes observed in our simulation.

These considerations seem to be highly interesting and we are grateful for the related comments by the reviewers. The insight gained from these equations indeed might help to better understand the differences and similarities of the limited flow scenarios considered in our paper and the high inflow concentration scenarios considered by Dreybrodt et al. (2005). However, since the comparison of these two different types of scenarios is not essential for understanding our paper and does not contribute to the objectives and conclusions of the paper, we prefer to remove the corresponding text passage (which in its current state is incomplete and misleading) from the discussion section (thereby shortening the paper) rather than to include an extensive additional discussion.