Reply to the Interactive comments by A. Palmer

We sincerely thank to the referee for taking a time for a careful reading of the manuscript and pointing to many inconsistencies and poorly formulated statements. We are very glad that the reviewer has seen the importance and potential impact of this manuscript, which gives us additional confidence in our work.

We have considered **all** comments and suggestions and did appropriate changes as suggested, which is well visible in the tracked version of the revised manuscript, where all changes are visible and those corresponding to the comments of dr. Palmer are highlighted yellow.

However, there are some comments that need a bit more discussion. These comments and responses to them are listed below:

- **2/6: The measurements demonstrating this switch was that of Plummer and Wigley (1976) Geochimica et Cosmochimica Acta v. 40, p. 191-202. White (1977) based his suggestion on the data and interpretation in this paper. Wigley is (or was at that time) a karst scientist with specific interest in the kinetics of cave origin, although the 1976 paper did not pursue details on that topic.**

  Response: *We have changed the text on P 2, L 5-10: The experiments of Wigley and Plummer (Plummer and Wigley, 1976) demonstrated a switch in the dissolution kinetics to a non-linear regime close to the equilibrium concentration of calcium ions with respect to calcite . Based on these results White (1977) suggested that such a switch–reduces the dissolution rates and causes deep penetration of dissolution power into the rock.*

  Cited and added to the literature list: *Plummer, L. N., and Wigley, T. M. L.: The dissolution of calcite in CO2-saturated solutions at 25°C and 1 atmosphere total pressure, Geochimica et cosmochimica acta, 40, 191-202, https://doi.org/10.1016/0016-7037(76)90176-9, 1976.*

  6/18: This is an important point, because a tiny penetration of water at zero c concentration through a very narrow opening will appear to drive the fluid to supersaturation in the model. This has given some modelers the wrong impression that no further dissolution can take place in the fissure. The authors are aware of this problem, although readers and other modelers may come to incorrect conclusions (for example, that there is a minimum aperture below which no dissolution can take place). It may be appropriate to make brief mention of this point.

  Response: *We have mentioned this point and cited a reference to the topic.*
  We have added text (P 7, L 10-15) reads:
  Otherwise wrong conclusions can be the result as in the work of Groves and Howard (1994) who claimed that for a achieving breakthrough of a fracture a minimum aperture width is necessary.

  Cited and added to the literature list*: Groves, C. G., and A.D. Howard, (1994) Minimum hydrochemical conditions allowing limestone cave development, Water Resour. Res., 30, 607-615*

- **7/6: “Periodic” conditions = unclear. Would “variable” be more appropriate (i.e., varying with time)?**

  Response: *As Periodic Boundary Conditions* (PBC) *are less known within the broader research community, we gave some extra explanation there. Using PBC we excluded the influence of boundaries at the top and bottom of the domain. We introduced PCB by »stitching« these two boundaries. This way the »vertical« flow entering the lower boundary continues down from the upper boundary and vice versa. As said in the text, we somehow wrap the 2D plain domain around a cylinder.*

  At P7, L25 we have added: *The upper and lower boundaries have have periodic conditions. Topologically this means that a 2D domain is mapped onto a cylinder. This makes the evolution of fractures independent from their distance from the upper/lower boundaries, which is not the case if these are no-flow boundaries.*

- **12/14: … the vertical outflow increases and, consequently, the input flow rises.” The rate of inflow is the result of greater overall efficiency of the conduit, rather than the result of increasing vertical outflow.(Both depend on continuity of flow and are the result of greater efficiency.) So a minor change in wording is suggested.**
- **15/3: … emits transverse flow that increases its input flow.” Here also, it appears that the increased inflow (and outflow) is in response to increasing overall efficiency of the conduit, rather than the result of increasing outflow at the tip. On the other hand, if water in the growing tip is being attracted by the porous medium that it is invading, as when water enters a dry sponge, then my statement is less appropriate.**
  - **Response:** *We have made minor changes in the text accordingly to make the message clearer. However, the main mechanism of the wormhole growth is the increasing transverse flow, through which a wormhole »invades« the flow field of competitors. Offering small resistance to flow, the high head from the boundary penetrates deep into the network along the wormhole, making the wormhole a high head region injecting the flow upward and downward into the adjacent fractured »matrix«. This allows high flow and dissolution rates in the wormhole. It is of course true that the resistance of the wormhole itself is decreasing and that the gradient between its tip and the outflow boundary increases as well, making it more flow efficient. However, this less effective than the transverse flow, which actually makes the difference to a 1D scenario. The referee's concept of »dry sponge« is conceptually close to what happens here, although the surrounding matric is not dry, but at lower head.*

    At P13, L 15 the text is added: *With increasing time and length of the wormhole, the vertical outflow increases allowing rising input flow at the constant head boundary.*

    Also at P16, L15- the text now reads: We, therefore, postulate that the main mechanism causing progression of the wormhole is an increase of the input flow caused by ejection of transverse flow into the net. In conclusion, the following feedback mechanism seems to be plausible. *As soon as one wormhole, for whatever reasons, becomes longer than the neighbouring ones it emits transverse flow that increases its input flow. The resulting enhanced dissolution capacity increases the length from where transverse* flow can be emitted and, consequently, the amount of outflow increases (see Fig. 8) causing growing inflow. It is interesting to note that for a net of soluble fractures the advancing dissolution front retards breakthrough considerably.

- **22/2: If the net is insoluble, clarify to show how wormholes can develop in an insoluble net.**
    - ***Response:*** *This was formulated wrongly. Of course the line of fractures with the wormhole soluble while the rest of the net is insoluble.*
      At P23, L 0-5 *the text has been changed to make the situation clear. It now reads: If only one line of fractures connecting the input to the output boundary is soluble and all other fractures in the net are insoluble competition is excluded and the evolution of a dissolution front is thus not possible, so that the wormhole starts to grow immediately.*

- **26/10: Clarify "dimensions of 1 cm by 1 cm and a width of 1 cm." Should "width" refer to the largern block outlined in black, and therefore is greater than 1 cm? Or does it refer to thickness of the model?**
    - **Response:** We have revised the text at this point to make the concept clearer. The revised paragraph on ***P27, L10*** *now reads:*
      To verify this finding we have employed the following approach (Fig. 23). We consider a fracture, that has just been reached by the wormhole (Fig. 23a,b). It has experienced almost no dissolution so far. We discretize this fracture into a network of 100 by 200 fractures, each 1 cm long and 1 cm wide with aperture width of 0.02 cm. (Fig. 23c). Fig. 23a shows the 2D net with the wormhole and the even dissolution front. A square marks the region enlarged in Figure 23b, where the fracture of interest, at the tip of the wormhole, is marked by the blue arrow.