Response letter for HESS-2022-280 Round 3

Title: Improving understanding of groundwater flow in an alpine karst system by reconstructing its geologic history using conduit network model ensembles

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Editor's comment: The manuscript has been reviewed by the most critical Reviewer involved in the prior round of reviews. While I recommended a set of major revisions to be implemented by the Authors, it is clear from the reviews that some additional clarifications are required at this stage. I am strongly recommending the Authors to consider the issues raised by the reviewer and incorporate their answers to these in their revised work.

Response: Thank you for this decision. We have made the requested changes, described in our detailed responses to reviewer comments below.

Materials included in this submission

1. The revised manuscript with markup showing the changes made
2. A point-by-point-response list
3. A clean version of the revised manuscript

Point-by-point response to referee #2:

Comment 1: In this updated manuscript, the authors have made small changes. While some sentences have been added, I must admit I am somewhat disappointed. Despite some limitations I have identified, the current version is now more focused and allows future readers to grasp the overall work done. Moving forward, my remarks below, addressing the comments from the previous round, will be my final input. I trust the editor will consider them as they see fit.

Response: We thank the reviewer for taking the time to provide thoughtful feedback over multiple rounds of review for this paper. We made additional corrections to ensure that the most recent version of the manuscript addresses the remaining comments, and is more satisfactory than the previous version.

Comment 2: 1) yes, the author do not explicitly mention in the current manuscript that they were the first to employ anisotropic fast marching, BUT they acknowledged it as the second point in their online comments (visible online, in their first response of AC2, end of the paragraph…) and also in their response to the reviewers.

2) I already pointed out in my comments that the two approaches are indeed different, so it is disheartening to read an answer that implies I do not understand something that I have already myself explained to the authors. The same applies to the timeline. I was already aware of it.

3) They included a reference to Luo's work; however, they seem to downplay Luo's contribution in comparison to Fandel's. They emphasize that their own implementation is more general, but their arguments lack strength. The fact that Luo's paper does not explore the costs does not necessarily mean it is not feasible, as both works are based on Borghi's algorithm. Additionally, they overlooked mentioning that Luo's work is in 3D, while Fandel's current implementation remains in 2D. I want to clarify that I find Fandel's work interesting, but I am concerned about the authors' persistent attempts to diminish the significance of other researchers' contributions." => See the following comment, I propose a re-writing of lines 145-175.

Response: We did not intend to downplay the importance of Luo et al.'s contribution – it simply is not as applicable for the purposes of this particular paper, in which the most important attributes are quick computation times and ability to consider multiple different influences (beyond fractures) on travel cost for conduit formation (e.g. rock type or surface cover). The ability to model the system in 3D is less important (although the 3D version of pyKasso is now functional as well). We have re-written lines 145-175 to clarify these points (see answer to comment below).

Comment 3: The other references are also not well located to refer correctly to each one's contribution. More accurate citations would be (lines 145-175) (modified parts in capitals):

"To test these hypotheses, many possible network configurations were modelled using A STOCHASTIC SIMULATION METHOD, implemented in the Python karst modelling package pyKasso (Fandel et al., 2022). THIS PACKAGE IMPLEMENTS IN 2D, THE SKS APPROACH ORIGINALLY PROPOSED BY BORGHI ET AL. (2012), BUT USES AN ANISOTROPIC FAST MARCHING ALGORITHM (SETHIAN, 1999, MIREBEAU, 2014). SKS APPROACH CALCULATES the optimal path from one point to another through a medium, in which the ease of travel varies both spatially and directionally. Karst conduits can be simulated using this type of algorithm based on the assumption that a conduit represents the fastest path from an inlet (such as a doline or swallow hole) to an outlet (a spring). Luo et al. (2021) proposed also to use an anisotropic fast marching IN A 3D SKS IMPLEMENTATION, IN ORDER TO INCORPORATE THE EFFECT OF A FRACTURE NETWORK WITHOUT EXPLICITLY GENERATING A DISCRETE FRACTURE NETWORK. IN the pyKasso package, THE anisotropic fast marching ALLOWS TO RENDER THE EFFECT OF THE TOPOGRAPHY OF THE SURFACE ON WHICH THE KARST DEVELOPS, AS IT IS 2D. In pyKasso, the travel medium represents the geologic setting, in which some rock units are more soluble than others (i.e. easier for conduits to travel through). Conduits are also assumed to form preferentially in certain orientations: in the direction 155 of the maximum downward hydraulic gradient, and/or along the dip direction of bedding planes (Audra and Palmer, 2015; Dreybrodt et al., 2005; Palmer, 1991)."

Response: We thank the reviewer for their suggestions and have revised significantly this section to clarify that we value Luo et al.'s contributions. We tried to explain more precisely the differences between the two implementations and our choice of pyKasso to address the questions posed in this specific paper. We have added a paragraph describing circumstances in which Luo et al.'s implementation may be a better choice.

Comment 4: Once more, upon reviewing the response, I get the impression that the authors believe I did not comprehend the work. They turn around the point I raised: when I speak about "fixed costs" I speak about the fact that they are constant per media and per hypothesis. In their previous paper, they themselves demonstrated the impact of the relative cost variation (for example between the surrounding media and the fractures). Also, they state that their cost vary temporally: in the manuscript there are no mention of simulations in several steps with changing costs between the steps (unless the updating due to the iterative aspect of path after path generation when more than one source-sink couple is considered: in that case, the only cost which is updated (from what we can guess from previous papers) is the one of the cells crossed by a simulated conduit.

However, the added paragraph line 309-320 perfectly renders what I underlined, and I am OK with it.

Response: The lack of comprehension was on our end, not on the reviewer. We had some difficulty understanding what was being asked, and we apologize for our misunderstanding.

Indeed, the travel cost in each medium (rock type, fractures, conduits, subglacial zones) is fixed. This is because we wished to keep all variables other than those being tested constant (and because we do not expect things like the solubility of the limestone to change over time). The variables being tested are the locations of the active outlets, and the extent of glaciation. From this comment, it appears that the reviewer is satisfied with the explanation in lines 309-320, so we have not made any further changes.

Addition: reading again part 5.2, I realize that for the description of both hypothesis, the authors use fuzzy terms like "high travel cost" (line 229): the exact value should be given into brackets, for precision.

Response: Thank you for pointing this out. We have inserted a table listing the travel costs associated with each geologic setting.

Comment 5: Yes, the mapped portion of inactive conduits lies approximately along a line of equal elevation BUT the algorithm computes the SHORTEST path between an inlet and an outlet, not all the possible paths. (REMINDER: FMA is determinist, not stochastic). In the absence of fractures (which are said to merely affect the results) the shortest path is mainly driven by the "topographic gradient" of the bottom surface. A gradient goes to 1 point not 2 points. It does not make a detour to stay at the same "level". Thus, it is not controlled by the elevation of the outlet, but more by the orientation of the above isolines (with the mitigation of the others elements included in the cost which, here, are only the fractures).

Anyway, I do not see in the added paragraph in section 7 (lines 309-320) any answer to that point (even the one of the authors with which I disagree): the added part answers quite well comment 4, not comment 5.

Response: The reviewer is right: FMA is deterministic. But as shown and explained in the previous papers (Borghi et al. 2012, Fandel et al. 2021, Fandel et al. 2022) there are several level of stochasticity within SKS and pyKasso. These methods do not simply connect the points that are the closest. This is illustrated on figures 3 or 4, where many different geometries are shown. They are all compatible with the general principle and key assumptions. And for each one, the procedure is influenced differently by the interactions between the slope of the base topography, the presence of different fractures, and the order in which we activate the different springs as the reviewer clearly points out in the end of this comment.

We corrected the paragraph to remind the reader that stochasticity arises also because of the hierarchy, but we did not change the main content of this paragraph because we think that it already explains without ambiguity that results that our results depend from our assumptions and the way we implemented them. Therefore, they may not be fully correct and we discuss precisely that point.

Comment 6: Now with the fact that the chronology is uncertain, I am less convinced. But well.

Comment 7: What has provoked the release of QS? Sudden event or abrupt one? If the authors have no answers, this point should be added as an interesting point to mitigate the results in the discussion, not just ignored as it is now.

Response: We added a paragraph addressing these questions:

“One limitation of this study is that the order of events and the rate of change in the landscape is not precisely known. Field observations suggest a late glacial to postglacial age of the Sägebach spring and a relatively rapid exposure. The lack of precise dating limits our understanding of the system as a whole.”

Comment 8: [If H1 is not the explanation for the inactive Hölloch conduit, but N6 + n11 existed and were drained by Qo, how do you imagine today's network (combining active + inactive)? Should we need to "superpose" the conduits simulated by H1, to those by H2, and then to the active ones?]:

There is nothing in lines 275-281 answering really this question.

Response: We are sorry, but we do not understand this question. The reviewer asks what would happen if the hypothesis H1 were not correct, "but N6 and N11 were drained to QO". But the hypothesis H1 *is* that N6 and N11 drain to QO. The last paragraph of the discussion does address a similar question, and states that it's possible that the actual conduit network looks like a superposition of H1 and H2. Testing additional scenarios would be very interesting but is beyond the scope of the present paper.

Comment 9: Partly OK: the modifications (made) are not just the topography, but mainly the "formations" encountered at this elevation, and so the cost affected to the 2D grid on which the computation is performed. This is the main source of the different results that have been obtained in this revision round. It is not just a topographic gradient. For the reader's understanding, this should still be better explained.

Response: In revising this section to address this comment, we realized that in fact we had superfluous text that repeated the explanation of differences between the original version and the current version twice. We have therefore removed the repeated text and clarified and consolidated this explanation into lines 220-230.

Comment 10: the remarks concerning the paragraph about stochasticity. Indeed, the authors claim to clarify the point line 166, but they inserted a sentence line 170, which is not accurate and lost the reader. The concerned paragraph is between line 162-166, and I insisted on the fact that they should say clearly here that in the proposed manuscript the pairing of inlet and outlet is fixed, and thus, as, they were themselves saying in their previous works, HERE the amount of variability is quite reduced (said again differently, everything rest on the DFN generation which is only a quite limited source of variability). Although the term "fixed pairing" is mentioned at line 240, it appears deep within the manuscript's details and may not readily convey the implications concerning stochasticity to a more casual reader.

Response: We have now dedicated a specific block of text in Section 5.1 explicitly to describing the differences between the models presented in our 2022 paper and those presented in the current manuscript, including mention of the inlet/outlet pairings, so that it is easier for x reader to identify all the differences together (see response to previous comment). We have not added any text in section 5, because this section describes how pyKasso works *in general* and not which settings and modeling choices were applied to this *particular* case. Those details are in section 5.1.

Figure 1: So, the cross-section is still inconsistent (even with the map modifications, because the problems were not there), but the "schematic" adjective seems enough for the authors to justify it. As a geologist, I do not understand what forbid to draw a consistent cross-section as the map is quite simple. But well. The caption modification consists in precising that the cross-section is "with the structure generalized in the Schwarzwasser valley".

Response: The original idea of our schematic cross-section was to represent the highly complex 3D topographical and geological environment (e.g. axial culminations, bent fold axes, etc.) in the form of a conceptual hydrogeological model showing all relevant and characteristic features in one profile, e.g., the prominent rockfall mass in the valley was projected onto the section line, although it is just east of the section line. However, it seems that this approach has caused more confusion than clarity. So instead we created a conventional and realistic cross-section, albeit still slightly simplified and generalized, e.g., patchy and thin Quaternary deposits on the slopes are not shown in cross-section. In the new version, we also differentiate between "overlying (Cretaceous) sandstone and marl" and the "flysch", although both units have similar hydrogeological properties (low permeability, predominant surface flow).