Response letter for HESS-2022-280 Round 2

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

Authors: Chloé Fandel, Ty Ferré, François Miville, Philippe Renard, Nico Goldscheider*

* corresponding author

Editor's comment: The results of this second round of reviews enable me to suggest an additional round of very careful revisions. These should include an appraisal of the originality of the anisotropic fast marching, considering the context tackled, as well as a detailed assessment / re-evaluation of the interpretations offered for the modeling results. It is also not my intention to discount any of the constructive comments emerged during the revisions. The revised manuscript will then undergo a very last round of reviews.

Response: Thank you for accepting our revised manuscript. We have included information for the editor regarding the question of the originality of anisotropic fast marching, as well as a reassessment of the interpretation of the modeling results. Please see 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 for the reviewers
3. A clean version of the revised manuscript

Point-by-point response to referee comment 1:

Comment: I am satisfied by the answers provided by the authors. All the comments/suggestions are correctly addressed in the revised version of the manuscript.

Thank you for this clear and concise assessment of our work.

Point-by-point response to referee comment 2:

Comment 1: The authors have made substantial changes in this new version that are going in the right direction. The new paper is more concise, more focused on the contribution of this work, which is better explained. However, some modifications are still required before considering it for publication.

Response: Thank you for taking the time to review our revised paper. The requested modifications are discussed point-by-point below.

Comment 2: Concerning the contribution, the authors now better frame it. However, they still insist on the fact that they are the first to use anisotropic fast marching, which is not true: Luo et al. published in 2021 (in Journal of Hydrology) a paper proposing to use anisotropic fast marching (AFMA) also in the frame of the algorithm by Borghi et al. (2012) and also for stochastic karst network modeling. Luo et al. use the algorithm in 3D, and exploit the AFMA to render the effect of fracturation without explicitly simulating the fractures, which is very interesting (I specify that I am not in this team, nor related to it). This is a different exploitation of AFMA that the one proposed by Fandel et al., which uses AFMA as a way to render the effect of "bathymetry of the inception level" in a 2D model. As well as the previous *papers should have better presented the alternatives to Borghi et al., the authors should now present this paper by Luo et al. and explain honestly the differences. [complete ref : Luo, L., Liang, X., Ma, B., Zhou, H., 2021. A karst networks generation model based on the anisotropic Fast Marching algorithm. J. Hydrol. 600, 126507. https://doi.org/10.1016/j.jhydrol.2021.126507].*

Response: We have added a reference to Luo et al., as requested. However, we would like to note that this reviewer comment is not fully correct: nowhere in the previously revised version of our manuscript do we claim that we were the first to employ anisotropic fast marching. We only explain that we test an assumption with a given model and we describe that model (pyKasso with anisotropic fast marching). We have inserted a brief mention of Luo's work in the text and briefly describe the major differences between the two approaches in line 151. An in-depth comparison of the methods is beyond the scope of this paper.

In addition, we would like to highlight three facts:

1. The primary concern of our study is not the development of anisotropic fast marching methods for simulating karst conduit networks. It is the application of these methods to hypothesis testing in a real catchment, regardless of the timing and development of these methods. In our previous round of revisions in response to reviewer comments, we removed several sections of text to focus more specifically on hypothesis testing rather than re-explaining method development.
2. The two approaches are quite different from one another, but we are certainly interested in comparing and perhaps merging the two approaches for future applications! A brief table summarizing the differences is provided below for the interest of the reviewers and editors, but will not be incorporated into the manuscript. We particularly note that, because Luo's approach does not allow for quickly generating multiple conduit network realizations of the same system, it would not be possible to use it for the type of hypothesis testing presented in our paper.
3. After examining the publication timelines, we found that the points raised in this comment are likely a result of publication delays. Our paper initially introducing our use of anisotropic fast marching for modeling karst networks was submitted on May 1st, 2021, one month *before* the paper by Luo et al. was published on June 1st, 2021. The paper by Luo et al. went through an unusually rapid review process (< 2 months from submission to publication). Our paper instead went through an unusually slow review process (10 months from submission to publication, despite the paper being accepted with only minor revisions). When we initially wrote about the use of anisotropic fast marching for karst network generation, it was indeed the first demonstrated use of this method, as the paper by Luo et al. had not yet been published. Additionally, several public presentations of our work were given between October 2020 and April 2021, introducing the idea of using anisotropic fast marching to simulate karst networks, all prior to Luo's publication. However, now that another research team has begun exploring similar methods, we are delighted to have more examples of this approach to draw on.

Feature	**Luo et al.**	**Fandel et al.**
Conduit generation algorithm	Anisotropic Fast Marching	Anisotropic Fast Marching
Field data requirements	1. Spatial extent of major hydrogeologic units 2. Locations of system inlets and outlets 3. Descriptive statistics for fracture network	1. Spatial extent of major hydrogeologic units 2. Locations of system inlets and outlets 3. Descriptive statistics for fracture network
Number of dimensions	3D	2D (with 3D in development)
Discrete Fracture Network	Generated using MATLAB ADFNE1.5 toolbox (Algalandis, 2018)	Generated using built-in Python function in pyKasso, based on concepts in Baghbanan & Jing (2007) & Davy et al. (2013).
Travel cost	Not considered	Assigned based on hydrogeologic unit, and presence of fractures or existing conduits
Anisotropy field	Calculated based on hydraulic conductivity tensor of representative elementary volume voxel	Multiple options: 1. Orientation of geologic contacts between units 2. Land surface orientation 3. Hydraulic gradient orientation 4. Any other surface provided by the user
Stochasticity	Not considered	Multiple sources: 1. Discrete Fracture Network generation 2. Inlet/outlet locations & numbers 3. Inlet/outlet pairings
Influence of existing conduits	Not considered	Two options: 1. Known conduits can be included in the model from the beginning 2. Multiple iterations can be run for the same system, in which conduits generated by earlier iterations are considered in later iterations
Computational efficiency	Up to 56 hours for a single step (finding the equivalent hydraulic conductivity of a single REV subdomain in a single layer)	Under 2 minutes for 100 complete simulations (fracture network and conduits)
Language	MATLAB	Python
Availability	Closed-source (not public)	Open-source (source code and documentation of all components and libraries are publicly available)

Comment 3: As a consequence, the contribution should only focus on the idea to test 2 hypotheses of formation with a conduit network model ensemble, and put aside the "AFMA" part which was already the contribution of Fandel et al., 2022. Several proposals are made in that goal in the latter "On the flow" comments.

About the references, the authors should cite the founding works, and not just the papers of Fandel which re-implement existing methods:

• About AFMA (for example):

o Sethian (1999), implemented by Konukoglu et al. (2007); and Mirebeau et al. 2014. [Sethian, J.A., 1999. Level Set Methods and Fast Marching Methods Evolving Interfaces in Computational Geometry, Fluid Mechanics, Computer Vision, and Materials Science (second edition).

o Konukoglu, E., Sermesant, M., Clatz, O., Peyrat, J.M., Delingette, H., Ayache, N., 2007. A recursive anisotropic fast marching approach to reaction diffusion equation: application to tumor growth modeling. Inf. Process. Med. Imaging 4584, 687–699. https://doi.org/10.1007/978-3-540-73273-0_57

o Mirebeau, J.M., 2014. Anisotropic fast marching on Cartesian grids, using lattice basis reduction. SIAM J. Numer. Anal. 52 (4), 1573–1599. https://doi.org/10.1137/ 120861667.]

• For the modelling method:

o Cite Borghi et al. 2012.

Response: We have inserted references to most of the key works mentioned above in Section 5 of the manuscript. Because the previous round of reviewer comments emphasized *removing* the sections of the text describing the development of the anisotropic fast marching techniques used in the current paper, we point readers to our previous work for a full discussion of the development of the methods and for grateful acknowledgement of the work by other authors upon which we built our approach.

Comment 4: I still have reservations about the interpretations of the modelling results, but if the discussion is nuanced, it could be ok. In particular, the discussion should moderate the results of the model considering that:

o In pyKasso, and other similar approaches, the relative costs ("speeds" in AFMA) have been demonstrated to have an impact on the results. Here the costs are fixed (similar to those of Fandel 2022). Should other influencing factors have impacted the results? In particular, should other relative costs (less impact of the "distance to the source" relatively to "hydraulic gradient") have induced less attraction to QO in paths from N6 and N11 during glaciation?

Response:

We have inserted a block of text in Section 7 that addresses both this comment and Comment #5 below. Essentially, the question is whether using a different parameter set (travel cost distribution, importance of the anisotropy field, etc.) for pyKasso could have resulted in a different conclusion.

In this comment, the reviewer states that the travel costs are fixed. However, in fact, we vary the travel costs both spatially and temporally. Different locations within the study area have different travel costs (limestone has a lower cost than insoluble units, fractures have a lower cost than limestone, etc.), and the travel costs in the scenario representing Hypothesis 1 are differently distributed than the travel costs in the scenario representing Hypothesis 2. In Hypothesis 1, during glaciation, the travel cost through areas covered by the glacier is higher than in limestones not covered by the glacier, which does impact which paths conduits take, since the conduits will tend to avoid glaciated/high cost zones (see Figure 4 – some of the predicted paths under Hypothesis 2 would go through glaciated zones under Hypothesis 1, and indeed, these paths are not seen in the Hypothesis 1 scenario). Additionally, the fractures (not shown in Figure 4 because each simulation has a unique fracture network) also have a lower travel cost than the surrounding limestone, which influences the predicted conduit paths. In some cases, low-cost fractures lead to paths going more directly towards QO, and in other cases, the fractures direct paths away from QO. Because the manuscript already includes a discussion of travel cost distribution, we have focused our response more on addressing Comment #5, on the influence of the anisotropy field.

Comment 5: In their answers as well as in the text, the authors often refer to "hydraulic gradient". I completely agree with them that it is what controls the flows in saturated zones. But in their simulations, the hydraulic gradient is approximated by "gradient of the lower contact between karstifiable unit and [the impermeable layer below]" (lines 180-185). Thus, it is probably constant over times, and could not allow an "inversion" from QA to QO as the hypothesis 1 would require, and as the authors described in some particular circumstances.

Said differently, the modelling approach gives a high importance to the "shortest path" between an inlet and an outlet. It is only slightly mitigated by fractures, and the AFMA is here to mitigate it by the "inception horizon" gradient (lines 180-185). As it is not directed QA to QO in the H1 test, reproducing the inactive conduit with the proposed approach would have been very surprising. => The results remain interesting, but nuance and moderation are required in the discussion and conclusion.

Response: We have added text discussing this in Section 7. The gradient of the lower contact of the karstifiable unit is indeed constant in time in these simulations. However, the mapped portion of the inactive conduit network lies approximately along a line of equal elevation (Figure 1), which also roughly coincides with the edge of the glaciated extent as determined from geomorphological field evidence. It is therefore not at all immediately obvious to predict what path conduits might take in this zone, where the influence of the gradient and the influence of the travel cost overlap. It would have been entirely plausible for the conduits to follow the maximum downward gradient southeast until they encountered the glaciated high-cost zone, and then to follow along the contour (neither up nor down gradient) until they reached QO. Our initial expectation was in fact that the stochastic fracture distribution would slightly favor such a path in some simulations, while slightly favoring a more direct path ESE in other cases, resulting in a much broader spread of simulated conduit paths than what we actually saw.

Comment 6: In the discussion and/or earlier in the text, it lacks "time lines" and explanations:

o Now the text is clear that the "glaciation phase" as well as the "covering of QS" have both happened. I suppose that the "covering" is after "glaciation", but it is not explicitly said: could you provide an approximative datation of these events or at least provide an relative chronology of events?

Response: In line 140, we mention that the temporal order of events influences the conduit formation. We do not have enough clear field evidence to determine whether the glaciation preceded or followed the uncovering of QS. We have now stated more clearly in this paragraph that the chronology is uncertain.

Comment 7: What has provoked the release of QS ? Sudden event or abrupt one?

Response: Again, we do not have enough clear field evidence to answer this question.

Comment 8: If H1 is not the explanation for the inactive Holloch 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 ?

Response: This is discussed in the text in lines 275-281. It is indeed possible that there are conduits connecting N6 and N11 to QO, which have not yet been explored. The existence of such conduits and the existence of the mapped inactive conduits are not mutually exclusive. Since this is already discussed in the manuscript, no changes have been made to the text.

Figure 1 : The cross-section line on the map is not consistent with the cross-section. I already mentioned indirectly this by asking to position the projection of N1, N6, N11 and N16 on it. Here is the fact : if you look at the map, along the cross-section starting from A, non-karst area (violet) is touching limestone (green) in a valley before quaternary sediments (yellow) on the north flank. On the cross-section, violet touches yellow which is in the valley… It should be corrected.

Response: The cross-section is schematic to best show the general patterns of relationships between different units, and the map lumps all the different geologic units in the non-karst area together, such that the quaternary units are not shown. The cross-section also does not show the quaternary units outside of the main valley. The map has been updated to show more detail in the non-karst area, and the cross-section and caption have also been updated.

Figures 3 and 4: why the isolines are those of the topography and not the isolines for the bottom of the limestones ? I hope it is just for the figures and the simulations consider the correct isolines (the ones of the bottom of limestones, as said in the new text)?

Response: The contour lines on the figures show the land surface topography so that the reader can visualize the site. However, the surface used for simulation is the bottom of the limestone. The two surfaces are often roughly parallel, as can be seen in Figure 1c. We have updated the figure legends to clarify that the lines shown are topographic contours.

Comment 9: The changes made in the model to be consistent with the author's own hypothesis (a development along the bottom of the limestones => thus a 2d model which considers the right geological formation at this level) imply quite different results clearly visible in figure 3, that, fortunately, do not modify the contribution nor the main conclusions of their previous work => I think it could be good to clearly assume that these new results are more consistent than the previous one and should be considered instead. Everyone could make an error, it would be clearer for the reader to assume as it is (and even say the "error" word in their text).

Response: Thank you for pointing this out. We have added text in line 190 clarifying that the conduit networks simulated using the contact surface should be considered as more realistic representations of the system than those we previously published.

On the flow comments (lines refer to the diff version):

Line 18 (abstract): suppress "(built on anisotropic fast marching methods)" because it is not the core of the paper (see comment1)

Response: Because this paper does rely heavily on anisotropic fast marching methods, and because readers often rely on the abstract to identify papers of interest to read, we prefer to keep these keywords in the abstract so that readers interested in applications of anisotropic fast marching methods can more easily find this work.

Lines 54-55 : rephrase (because the method has already been presented in previous papers, and to ease better understanding the contribution) : "This study USES a model-based approach to identifying the geological processes WHICH COULD EXPLAIN A PARTICULAR cave formation. IN this real karst SYTEM, detailed cave maps (...)"

Response: We have revised this text based on these suggestions.

Lines 96-100 : OK, but could you precise in the text if you consider this "expected network" as an exact location or an "approximative equivalent network". For what I understand, you have several clues on the "global path", but still some uncertainties about its precise location (as the differences between the mapped Holloch cave and the "expected network" at the same place demonstrate).

Response: We have inserted text in line 97 to clarify that the expected network does not represent exact location, but rather the general configuration and approximate location of the conduits.

Line 107 : remove Fandel et al. 2022 : no flow modelling is performed in this paper.

Response: We have removed this paper from this list.

Lines 185-190 about the "stochasticity": In Fandel et al.2022, it was said (legend of fig 18. A) "when the inlet/outlet are kept fixed, the fractures add only a small amount of variability in the network structure". The authors should remind this important aspect of their approach, because here, pairing is fixed. Thus, fracturation is the only remaining source of stochasticity.

Response: We have inserted text in line 166 clarifying this point.

Figure 3 : what are the paths on figure 3.c : they seem different from the "expected net" ? precise it in the legend

Response: Figure 3c is intended to show the extent of the karstifiable limestone. One of many possible conduit network simulations is shown embedded in the limestone, but this is not the primary point of this panel. We have therefore removed the conduits from this panel.

Line 260 : "in" is missing : "prediction are more certain IN some areas(...)"

Response: Thank you for pointing this out. It has now been corrected in the manuscript.

Line 300 : "better under hypothesis 1", I suppose you mean "hypothesis 2".

Response: Thank you for pointing this out. It has now been corrected in the manuscript.

Line 349 : "two-phase kastification" : if evidence of sink in glaciation times exist, don't we have three-phase kastification ?

Response: Thank you for pointing this out. It has now been corrected in the manuscript.