Reply to the Comments by Reviewer 1:

Rebuttal of the manuscript entitled "On understanding mountainous carbonate basins of the Mediterranean using parsimonious modeling solutions"

We thank the Associate Editor and the reviewer for their remarkable and constructive comments. We have carefully read all the comments and provided point-by-point answers along with an indication of possible modifications to the manuscript. **Reviewers' comments** are in boldface.

The colour of the text in the revised manuscript is as follows:

1. blue is used to specify the changes related to reviewer 1's comments.
2. cyan is used to specify the changes related to reviewer 2's comments.
3. red is showing the changes which commonly refer to both reviewers' comments.

Reviewer's Comments:

I read this manuscript with great interest, knowing the complexity of modeling karst systems and the unique and important role they play in hydrology.

Reviewer's Comment 1: However, in the current reading of the discussion paper, the novelty of the work is not clear beyond the study area. The introduction states that the study demonstrated that "good results can still be obtained by using few experimental data and time series analysis"(L66) but the research experiment itself does not follow a systematic approach that shows how lesser and lesser data still results in similar results. Is the goal of the study to show that less data can still result in good modeling of a karst system or that it is not necessary to fully couple the surface-water and groundwater systems in a karst system (L65)? The study design does not seem to follow a systematic testing for either approach; rather it applies the GEOframe-Newage tools to the study area. It would be more compelling to compare these results to how the system could be modeling with other couplings or with more (or less) data.

Authors' Reply:
We thank the reviewer for the valuable comment that prompted us to clarify our primary objectives. The novelty of our study lies in developing an approach to model carbonate/karst catchments -under the circumstance of groundwater data shortage- taking advantage of flexible modeling approaches and rainfall-discharge time series analysis. Considering these kinds of catchments becomes more important since of their vulnerability and effectiveness to drought and climate change conditions.

The innovations in our study can be summarized as follows:

1. We have introduced a novel approach by taking advantage of a correlation analysis technique to extract groundwater (GW) hydrological response to precipitation. This technique has not been previ-

ously applied to carbonate systems for determining GW hydrological response. By incorporating this information, we have enhanced the reliability of the hydrological modeling of a complex carbonate rock catchment.

2. In addition to traditional goodness-of-fit indicators, we have proposed a new methodology called Empirical Conditional Probability (EPC). This approach provides valuable insights into the reliability of the model, offering a comprehensive assessment of its performance.

We tried to clarify the novelties and corresponding highlighted findings in the revised Introduction and in the Conclusions section.

The proposed methodological tools are not limited to the specific catchment studied in this research. They can be applied to other catchments, including non-carbonate ones. However, the Nera catchment itself is an intriguing study area and serves as a representative example of the behaviour of carbonate basins. Consequently, our experimental findings can be extended to other carbonate regions across the Mediterranean, providing valuable insights into their hydrological dynamics. We address these points in **Lines 57-62, Lines 201-232, Lines 370-406** of the revised text.

Comment 1b: 'Reviewer asked the authors to specify clearly if the study scope is Showing that it is not necessary to fully couple the surface-water and groundwater systems in a karst system'

A: Our contribution does not seek to prove that fully-physical distributed models are unnecessary in complex basins. Instead, we aim to demonstrate that alternative effective solutions can be implemented, particularly when data availability is limited. In essence, we acknowledge that fully-physical modeling requires extensive and costly long-term groundwater data for calibration and execution. However, our research illustrates that a viable solution can still be achieved through a more parsimonious and less data-intensive approach (refer to Fig. 1). To clarify this **Lines 159-162 and 388-390 have been added to the main text.**

Comment 1c: Reviewer asked the authors to specify clearly if the study scope is Showing that less data can still result in good modeling of a karst system.'

A: Understanding to what extent reduced data can still yield comparable results is not the scope of our study, since to satisfy this target an abundance of groundwater data is needed that is typically unavailable. Instead, our objective was to demonstrate that even in the absence of long-term groundwater data, it is still possible to reasonably represent the intricate interactions within a carbonate catchment by utilizing precipitation and discharge data, along with an estimation of the external carbonate area. To validate our hypothesis, we also incorporated fieldwork findings from hydrogeologists. **Lines 1-8, 58-60 (research questions 1 and 2), and 372-377** refer to this comment in the revised version of the manuscript.

rev.png

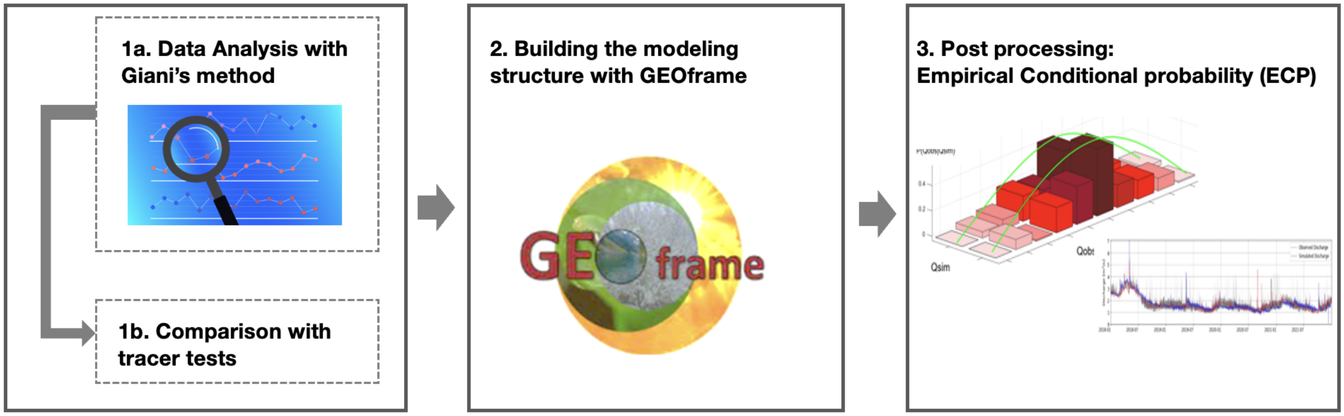

Figure 1. The proposed scheme in case of dealing with carbonate/karst systems without sufficient available groundwater data (the figure is for the reviewer and is not be added to the main text).

Reviewer's Comment 2: The contribution is made more difficult to understand because the experiment organization in L70-79 appears to read more as results than hypotheses about what the study will test. The lack of clear hypotheses makes it difficult to understand the broader contribution of this work.

Authors' Reply:

We thank the reviewer for raising this. We realized that the manuscript needs more effort to define better our research hypothesis. Thus we clearly mentioned the research questions at the end of the introduction, as follows (**see Lines 56-73 in the revised text**):

" This study aims to address the following five research questions (RQs):

1. Can the complex response of carbonate catchments to precipitation be modeled with HDSys relying only upon streamflow and precipitation time series, aided by cross-correlation analysis?
2. What type of modeling solution is suitable for this task, and is a parsimonious modeling approach appropriate?
3. Are the classic goodness-of-fit scores enough to evaluate the reliability of the models?
4. What is the impact of external contributing areas on streamflow in catchments with fractured carbonate rocks? To what extent does this contributing area affect the total streamflow from small headwater catchments to the main outlet?
5. What is the role of storage in sustaining streamflow during years with significant precipitation deficit in these types of catchment?

We have examined the water budget of the Nera River basin, which is a significant tributary of the Tiber River, the second-largest river in Italy. The Nera River basin contributes nearly 50% of the total discharge of the Tiber River and is characterized by a significant portion of fissured and fractured carbonate rocks feeding the river discharge by releasing a large amount of groundwater into the river bed from streambed springs. Thus, this catchment is a good representative of the carbonate catchments for answering the RQs."

Reviewer's Comment 3

There are also quite a few qualitative statements that are not for the authors to decide about the quality of the modeling results. For example, on L234, the text states "these values are more than acceptable." It is not possible for the authors to make this assessment because they do not know what applications the readers may deem are "acceptable" - this is a qualitative statement based only on the authors' subjective assessment. The results should simply be reported and allow the reader to decide if these results are acceptable for their application or need. Another example is in L254, where the sentence reads, "It is apparent that the model is very good at reproducing the lowest discharges…" This should be changed to read something like, "The model is able to reproduce flows at the lowest discharges…" and then report or reference the accuracy at which the flows can be reproduced.

Authors' Reply:

In the revised version of the manuscript, we have made significant improvements to address the concerns raised about ambiguous expressions and enhance the rigor of our writing. We have carefully reviewed and polished the manuscript to ensure that it accurately conveys our findings and methodology.

Reviewer's Comment 4:

The results, interpretations, and discussion all relate specifically to the study area and there appears to be no further attempt to generalize or broaden the findings to the wider audience of HESS. There are also few stations used in the analysis, further limiting the interpretation of the results more widely. It would be helpful to frame these sections with a broader audience in mind beyond the study area.

Authors' Reply:

We do agree in this context with the reviewer. In this regard, we highlighted the following points which make the manuscript more suitable for a wider range of journal readers:

1) In the modified Introduction, we describe different approaches to take account of external groundwater contributions to a basin together with the advantages and drawbacks of each approach. Furthermore, we explain the challenges of the lumped modeling approaches under the circumstance of data scarcity and then our strategies to address these challenges have been discussed. This allows the paper to reach a broader audience (**please see the Introduction of the revised manuscript**).

2) We consider the role of the storage for these types of catchments in sustaining streamflow during years of significant precipitation deficit. Nowadays, the European drought issue is at the core attention of many researchers, and investigating this issue for the long-memory hydrological catchments (like the Nera River basin) is interesting for the readers or the journal. Therefore, considering the drought point of view, **Section 5.2 (Line 320-368)** has been totally modified.

3) The study area (Nera River basin) is a good representative of the carbonate basin behavior and makes our research experiment scalable to other carbonate areas across the Mediterranean region. Besides the importance of knowing the discharge of the Upper Nera River as the resource of multipurpose water supplies, this area as a representative of carbonate basins with the problem of groundwater data shortage could be of interest to those dealing with the carbonate basins with similar challenges (**Lines 66-73 have been added to the text**).

The Introduction has been rewritten as follows:

"**Introduction**:

Carbonate/karst landscapes represent approximately 7-12 percent of the Earth's continental area and they provide a significant challenge for hydrologists (Hartmann et al. (2014)). Due to the capability of these landscapes to retain water for a longer period (i.e., long-term hydrological memory catchments), their storage plays an important role in the control of drought propagation and delayed hydrological recovery (Alvarez-Garreton et al. (2021)).

Generally, a carbonate/karst landscape forms when the percolated precipitation dissolves the subterranean carbonate bedrock and creates extensive fissures, open fractures, conduits, and caves. This can result in a complex network of groundwater flowpaths occurring within the same or adjacent aquifers (Kiraly et al., 1995). To model these types of systems one powerful solution is to use distributed, process-based models (PB) (e.g., Rooji, 2020; Hartmann et al., 2014), which are based on solvers for groundwater partial differential equation. Yet, the main challenge of this kind of distributed model is that they require a large amount of hydrogeological data and extensive field analysis to set appropriate physical parameter values and correct boundary conditions. On top of that, large computational power is needed to run these models (Li et al., 2022).

An alternative to PB is black-box models based on machine learning (MLM) in which all the details about the structure of the aquifer and the hydrodynamics parameters are not needed (e.g., Tapoglou et al. (2014) and Castilla-Rho et al. (2015)). Although the implementation of MLM is easy, their model parameters do not have a physical meaning and are only indirectly related to the characteristics of the carbonate system (Zhou et al., 2019). Furthermore, MLM does not explicitly solve the water budget and thus it is not possible to have information about the dynamics of all water budget components.

Hydrological Dynamical Systems (lumped models, HDSys) represent another type of model, based on a set of ordinary differential equations (ODEs) that conceptualize the entire carbonate system as a series of reservoirs (e.g., Bancheri et al., 2019; Hartmann et al., 2014; Rimmer and Hartmann, 2012; Butscher and Huggenberger, 2008; Tritz et al., 2011; Jukic and Denić-Jukić, 2009; Duboisl et al., 2020). Instead of explicitly considering spatial variables, HDSys specify the interconnection of fluxes between different reservoirs, which leads to reducing the computational complexity. However, HDSys still require the definition of model parameters, which typically rely on calibration and inverse modeling using monitored discharge data or other relevant data sources (Hartmann et al., 2014). Several studies have also explored modeling the fast and slow drainage from carbonate systems using tracer information (e.g., Rimmer and Hartmann, 2012; Dubois et al., 2020). This approach involves introducing an artificial tracer into a sinkhole and then tracking the tracer's movement in the surrounding areas at different times (Hartmann et al., 2014; Zhang et al., 2021; Nanni et al., 2020). While this technique can be useful, it is time-consuming and may not always be feasible due to accessibility issues.

These HDSys can be conjugated by techniques that rely on the correlation between precipitation and discharge can provide valuable insights about the behavior of carbonate systems, particularly in situations where field information about water circulation is limited. It could also provide useful information in the situation of missing tracer test analysis. For example, Fiorillo and Doglioni (2010) used cross-correlation analysis to estimate the time that water requires to flow through fissured aquifers. Another useful method, borrowed from applied economics (Kristoufek, 2014, 2015), was employed by Giani et al. (2021) to estimate the basin response time of hydrographs to precipitation, with successful results. However, according to the authors' best knowledge, to date, this data analysis technique has not been applied to complex carbonate systems to determine their hydrological response to precipitation.

This study aims to address the following five research questions (RQs):

1. Can the complex response of carbonate catchments to precipitation be modeled with HDSys relying only upon streamflow and precipitation time series, aided by cross-correlation analysis?

2. What type of modeling solution is suitable for this task, and is a parsimonious modeling approach appropriate?

3. Are the classic goodness of fit scores enough to evaluate the reliability of the models?

4. What is the impact of external contributing areas on streamflow in catchments with fractured carbonate rocks? To what extent does this contributing area affect the total streamflow from small headwater catchments to the main outlet?

5. What is the role of storage in sustaining streamflow during the years with significant precipitation deficit in these types of catchments?

We have examined the water budget of the Nera River basin, which is a significant tributary of the Tiber River, the second-largest river in Italy. The Nera River basin contributes nearly 50% of the total discharge of the Tiber River and is characterized by a significant portion of fissured and fractured carbonate rocks feeding the river discharge by releasing a large amount of groundwater into the river bed from streambed springs. Thus, this catchment is a good representative of the carbonate catchments for answering the RQs. Additionally, groundwater data shortage is a problem that is not unique to the Upper Nera River area, and the findings of this study could help inform water management and policy decisions in other carbonate basins as well. By providing a comprehensive analysis of the water cycle in this area, this study could also help identify potential sources of water stress and suggest strategies to mitigate them. "

Reviewer mentioned that "here are also few stations used in the analysis, further limiting the interpretation of the results more widely":

A: In fact, this is the main challenge that we want to solve (i.e., implementing the model over regions with data scarcity issues).

We added some comments on that in the revised manuscript to remark on this (**like Lines 1-4 and 70-72.**).

Reviewer's Comment 5

The conclusions make some interesting points, which actually do emphasize some of the potential novel aspects of the work but they are not emphasized in the manuscript elsewhere. For example, Conclusion #1 and the sentence on L356-357 discuss the insight that the classical approach for delineating basins is not appropriate and a preliminary check on the water balance is needed for karst system, especially if runoff coefficients are high. I am not sure of the novelty of this finding but this is a point that is noted in the title but then not mentioned again until the conclusions. The paper should be reframed with these contributions in mind. I will note again that I am not sure this will improve the novelty of the work but the conclusions are much more clearly stated as to the contribution of the work and it was unfortunate to wait until the end of the paper to understand the potential contributions of this work.

Authors' Reply:

Thanks for the comment leading us to reframe the manuscript and to make everything more clear.

Other Issues:

Minor comments have been implemented in the revised manuscript. Additional modifications that have been added to the revised manuscript are as follows:

1. Based on the reviewer's comments, the Abstract is changed to accomplish their suggestions.
2. We improved Fig. 7, 8 and 9 in the revised manuscript by highlighting the proposed evaluation method (based on empirical conditional probability) demonstrating that the general classical scores are not enough to evaluate the models.
3. Based on the reviewer's comments, the Conclusions is modified also in the manuscript.

References

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Duboisl, E., Doummar, J., Pistre, S., and Larocque1, M.: Calibration of a lumped karst system model and application to the Qachqouch karst spring (Lebanon) under climate change conditions., Hydrol. Earth Syst. Sci., 24, 4275–4290, 2020.

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Reply to the Comments of Reviewer 2:

Rebuttal of the manuscript entitled "On understanding mountainous carbonate basins of the Mediterranean using parsimonious modeling solutions"

We thank the Associate Editor and the reviewer for their valuable comments that certainly improve our final manuscript. We have carefully read all the comments and provided the answers along with possible modifications. The reviewer made several suggestions about the wording. They were all accepted and are not reported here. **Reviewers' comments** are in boldface.
The color of the text in the revised manuscript is as follows:

1. blue is used to specify the changes related to reviewer 1's comments.
2. cyan is used to specify the changes related to reviewer 2's comments.
3. red is showing the changes which commonly refer to both reviewers' comments.

Reviewer's Comment:

The manuscript from Shima et al. is dealing with a relevant topic, having potential interest for the readers of EGUsphere. The manuscript is well organized and containing useful information, sufficient data, good modeling efforts. Nevertheless, the goal of their research seems not well focused on novelties. In addition, the english language tremendously suffers for an misleading use of italian sentences which have been translated in english maintaining a classical italian structure.

Comment 1: The starting point is the non-correspondence of hydrographic basins and hydrogeological basins. This finding is a very basic one, everyone knows the difference and it is absolutely non a novelty for the scientific community. In their text, the Authors are presenting this issue as a novelty, instead of presenting the problem in the introduction chapter, using the relevant and abundant literature on this topic. So, the novelty of their manuscript has to be searched in the methods they applied to solve the problem. This is in my opinion the logical approach and I suggest them to completely rewrite the introduction focusing on the problem they want to analyse (how to take into account the overflow in river discharge due to external groundwater flow feeding your basin). By this way, they can easily highlight their findings, mainly related to the useful modeling and methodology they performed during the study.

Authors' Reply:

We appreciate the reviewer for the valuable comment. While the non-correspondence of hydrographic and hydrogeological basins is not considered as the novelty, our paper focused on addressing the challenges associated with data scarcity in these basins. These challenges become more highlighted if we are going to investigate the effect of drought and climate change on these basins. We have proposed a reliable approach that incorporates new methods and tools to accurately reproduce the water budget of such complex basins. We acknowledge that the original version of the manuscript did not effectively convey this message, and we have implemented significant revisions to the Introduction, Results, and Conclusions sections to clarify every thing. In the revised manuscript, these sections are highlighted in red to facilitate tracking and reviewing. We believe that these modifications will shed light on the significance of our work.

In particular, the introduction has been re-organized as follows:

1. We explained the general characteristics of the carbonate system.
2. We mentioned different approaches of taking account of external groundwater flux contributions to a basin together with the advantages and drawbacks of each approach. Furthermore, we described the challenges of the lumped modeling approaches versus the fully distributed ones. This allows the paper to be interesting for a broader audience.
3. We clarified the study objective in five main research questions (RQs) which is answered in different sections of the manuscript.

The research questions are as follows:

1. Can the complex response of carbonate catchments to precipitation be modeled with HDSys relying only upon streamflow and precipitation time series, aided by cross-correlation analysis?
2. What type of modeling solution is suitable for this task, and is a parsimonious modeling approach appropriate?
3. Are the classic goodness of fit scores enough to evaluate the reliability of the models?
4. What is the impact of external contributing areas on streamflow in catchments with fractured carbonate rocks? To what extent does this contributing area affect the total streamflow from small headwater catchments to the main outlet?
5. What is the role of storage in sustaining streamflow during the years with significant precipitation deficit in these types of catchments?

A proposal for the new Introduction can be found in the answer to the reviewer's #1 comments.

Reviewer's Comment 2

The second concern is related to the English language. Too many sentences are too long, with secondary sentences included. The uses of commas is limited and this approach cannot be approved by international readers. Please rewrite the entire document using shorter and clear sentences: one concept, one phrase. I strongly suggest the support of a mother tongue for providing a successful review.

Authors' Reply:

The manuscript has been reviewed by a native English speaker.
Herein below we try to answer the "Detailed comments". The other ones related to the typos and incorrect use of English words were all accepted and are not reported here.

Detailed Comments

1. **line 92: what do you intend with "linear" springs? Perhaps "streambed" springs?**

   Authors: Yes, thanks for the suggestion. We replaced the term and you can find it in cyan color.

2. **120: My?**
   Authors: This is a typo. It should be MU which is the acronym for Madonna dell'Uccelletto. It has been modified.

3. **line 160: you are in a karst domain, so a response in 3 days would be due to karst circuits. Please evaluate this possibility and if you exclude this possibility please explain why**
   Authors: According to Petitta et al. (2022), the continental deposits preserve this carbonate aquifer from the direct dissolution processes limiting the mature karst development in the saturated zones. Additionally, they demonstrated that the fast flow contributes to only a minor percentage of the discharge in this area and the groundwater circulation is mainly driven by fractures and fissures. That is why we have considered just 30 days as the response time of the carbonate catchment (as highlighted also in Nanni et al. (2020)). However, we have modified the manuscript to clarify that the study area is not considered as a full karst system (please see **Lines 106-108** in the revised text in cyan).

4. **caption of figure 3: " is still high" is qualitative evaluation, please specify the number (it seems that in this case is lower than 1, so why you think is high?)**

   Authors: The caption of Fig. 3 has been modified in the revised text and is highlighted in cyan: "(a1) Cumulative observed discharge at CSA versus cumulative precipitation recorded at the closest station to CSA; (a2) Coefficient time series computed by dividing the discharge at CSA by the precipitation time series recorded at different stations. (b1) Cumulative observed discharge at Visso versus cumulative precipitation related to a station close to Visso; (b2) Coefficient time series computed by dividing the discharge at Visso by the precipitation observed at several stations. (c1) Cumulative observed discharge at Ussita versus cumulative precipitation related to a station close to Ussita; (c2) Coefficient time series computed by dividing the discharge at Ussita by the precipitation observed at several stations. The 1:1 line is shown in green. For CSA and Ussita the runoff coefficient is about 4 and 1.5, respectively, and this value is around 1 at the outlet of the basin (Visso)"

5. **line 205: using the period 2017-2018, do you not have problems with the reaction to the earthquake? I read some papers indicating a long reaction in discharge in this zone**

   Authors: We have not considered the discharge records affected by the seismic sequences during Nov. 2016-Nov. 2018 (see Fig. 2 in the revised manuscript). Additionally, in the revised manuscript,

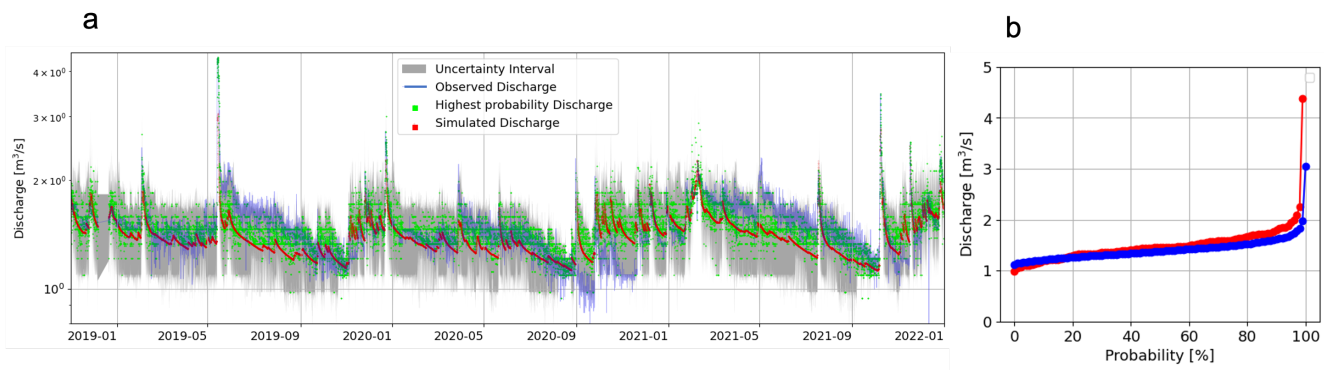

Figure 1. This figure is Fig. 9 in the current version of manuscript: (a) Simulated discharges at the Ussita outlet for the calibration period (2019-2021). The Uncertainty analysis results obtained by the ECP method for the simulated discharge are shown with the grey area (b) The flow duration curve for observed and modeled discharge are in red and blue, respectively.

we have modified the period of calibration for MU to Nov. 2018-2021. The model has been then validated at the outlet of the basin (Visso) during 2019-2021. In this regard, Fig. 1 in this rebuttal is the modified version of Fig. 9 in the manuscript and it has been replaced. **All the changes in section 5.1 (Lines 241-283) are specified in red.**

6. **line 297: this is the real core of your manuscript and this has to be highlighted both in the discussion and in the conclusion!**

   Authors: Thanks for the useful comment. The content of section 5.2 and also the figures have been reanalyzed. Additionally, we highlighted the important role of storage in supporting the river discharge in the years with precipitation deficit. Furthermore, the recovery from dry years in these kinds of basins with long hydrological memory has been discussed. Section 5.2 has been totally modified, **see Lines 320-368**.

7. **line 321: where is Pescara spring? Out of your study area? So why you includes this spring in your comments here? I suggest to cancel this reference**

   Authors: We removed it from the text to avoid confusion.

8. **line 324: I did not find a "lack of clear recharge signal" in the reference you cited here. I suggest to cancel this part, not necessary and not included in your study area**
   Authors: We removed it from the text accordingly.

9. **line 326: the sentence is not clear, please rephrase the concept. I know that aquifer recharge is EVER going to springs/river, producing discharge (not runoff)**
   Authors: We removed the sentence to avoid any ambiguity.

10. **line 328: you have not discussed the role of Karst, so I suggest to not include karst in the conclusion**
    Authors: Based on point 3 the basin cannot be considered fully karst so the text is modified accordingly.

11. **line 332: if you have karst, please discuss in the text, not in the conclusion**
    Authors: We reorganised the text to avoid ambiguity (see points 3 and 10). Thanks for mentioning this point.

12. **line 357: your findings are not based on isotopes neither in tracer tests, so why you added in the conclusion?**

    Authors: We have modified the Conclusions accordingly (**Lines 370-421**).

Authors:

ADDITIONAL modifications that have been added to the manuscript are as follows:

1. Based on the reviewer's comments, the abstract has been changed.
2. The Introduction has been rewritten.
3. We improved Fig. 7, 8, and 9 in the revised manuscript by highlighting the proposed evaluation method (based on empirical conditional probability) demonstrating that the general classical scores are not enough to evaluate the models.

References

Nanni, T., Vivalda, P. M., Palpacelli, S., Marcellini, M., and Tazioli, A.: Groundwater circulation and earthquake-related changes in hydrogeological karst environments: a case study of the Sibillini Mountains (central Italy) involving artificial tracers., Hydrogeology Journal, 28, 2409–2428, https://doi.org/https://doi.org/10.1007/s10040-020-02207-w, 2020.

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