Reply to the Comments of Reviewer 2:

Rebuttal of the manuscript entitled "Resolving the water budget of a complex carbonate basin in Central Italy with parsimonious modelling solutions"

We thank the Associate Editor and the reviewer for their valuable comments that certainly will 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. Underlined parts refer to changes in the revised manuscript.

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-correspondance 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 relevand 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 thank the reviewer for the valuable comment. Our study is dealing with the simulation of the complex hydrological behavior of basins where there is not correspondence between hydrological and hydrogeological contributing areas. The existence of these kinds of basins is not certainly a novelty, but what we have proposed with this paper is how to deal with them in cases of data scarcity. Besides, we introduce a couple of new tools and the use of data from tracers collected by some of the Authors. From the reviewer's

comments, we realized that this message was not clear in the original version of the manuscript, and in the revised manuscript we will change the Introduction, Results, and Conclusions sections.

In particular, the introduction will be re-organized as follows:

1. We will explain the general characteristics of the karst system.
2. We will mention different approaches of taking account of external groundwater flux contributions to a basin together with the advantages and drawbacks of each approach. Furthermore, we will describe the challenges of the lumped modeling approaches versus the fully distributed ones. This will allow the paper to be interesting for a broader audience.
3. We will clarify the study objective in three main research questions (RQs) which will be answered in different sections of the manuscript.

Three research questions are as follows:

1. Is it possible to model the complex carbonate catchment response to precipitation by relying only upon streamflow and precipitation time series? What kind of modelling solution is suitable to do that? And, is the parsimonious modeling solution suitable for that?
2. What is the impact of the external contributing area on streamflow in catchments characterized by fractured carbonate rocks behaviour? And, to what extent does this contributing area impact the total streamflow from small headwater catchments to the main outlet?
3. What is the role of the storage for these types of catchments in sustaining streamflow during years of significant precipitation deficit?

A proposal for the new Introduction can be found in the answer to 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 mothertongue for providing a successful review.

Authors' Reply:

The manuscript will be reviewed by a native English speaker.
Herein below we will 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 will replace the term.

2. **120: My?**
   Authors: This is a typo. It should be MU which is the acronym for Madonna dell'Uccelletto. It will be 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. The three days delay is already well trated by a groundwater linear reservoir. That is why we have considered just the 30 days as the response time od the karst catchment (as highlighted also in Nanni et al. (2020)). However, we will modify the manuscript to clarify that the study area is not considered as a fully karst system.

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: We will modify the caption of Fig. 3: "(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 2016-2017 (see Fig. 7 and 9 in the manuscript). In the revised manuscript, we will modify the period of calibration for MU to 2018-2021. The model will be then cross-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 will be replaced.

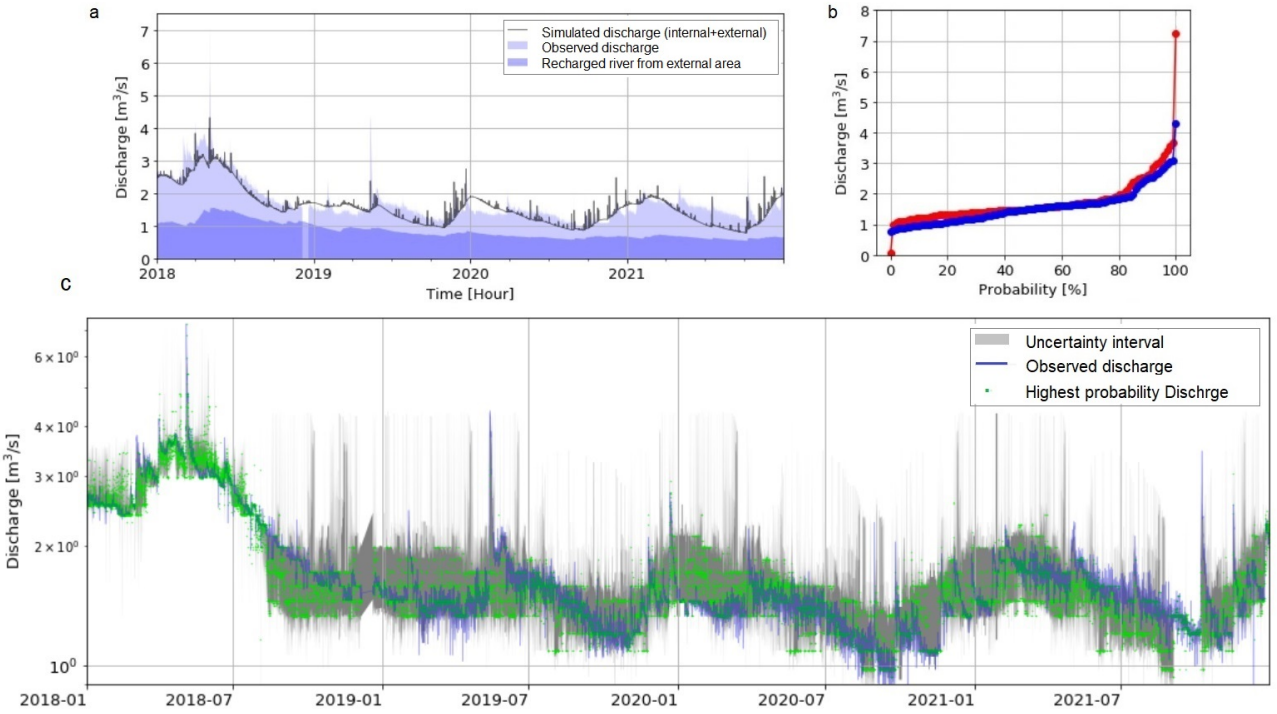

Figure 1. This figure will be Fig. 9 in the manuscript: (a) Simulated discharges at the Ussita outlet for the calibration period (2018-2021). The dark and light blue-shaded parts are related to the carbonated and total discharge drained at Ussita station, respectively. (b) The flow duration curve for observed and modeled discharge are in red and blue, respectively. (c) The Uncertainty analysis results for the simulated discharge obtained by the ECP method.

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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!**

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Authors: Thanks for the useful comment. The content of this section is related to the water budget analysis and also the figures will be reorganized. Additionally, we will highlight 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 will be discussed. Section 5.2 will be totally modified.

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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 will remove 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 will remove 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 will remove 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 will be modified accordingly.

11. **line 332: if you have karst, please discuss in the text, not in the conclusion**
    Authors: We will re-organize 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: The reviewer is right. We will modify the Conclusions accordingly.

Authors:

ADDITIONAL modifications that will be added to the manuscript are as follows:

- Based on the reviewer's comments, the abstract will be changed.
- The Introduction will be rewritten.
- We will improve Fig. 7, 8, and 9 in the original 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 hydro-
165 geological 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.

Petitta, M., Banzato, F., Lorenzi, V., Matani, E., and Sbarbati, C.: Determining recharge distribution in fractured carbonate aquifers in central Italy using environmental isotopes: snowpack cover as an indicator for future availability of groundwater resources., Hydrogeology Journal, 10, 1619—-1636, 2022.