Title: Effects of passive storage conceptualization on modelling hydrological function and isotope dynamics in the flow system of cockpit karst landscape
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Responses to the Editor:

Thank you for your letter and the reviewers' comments concerning our manuscript entitled "Effects of passive storage conceptualization on modelling hydrological function and isotope dynamics in the flow system of cockpit karst landscape". We have revised the manuscript according to the reviewers' comments. The revised sentences and sections in the revised manuscript are highlighted in blue color.

Thank you for your editorial work.

Sincerely,

Xi Chen
On behalf of all co-authors

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Responses to the Reviewer #1:

I thank the authors for their comprehensive responses and their incorporation into an expanded version of the paper. The elements provided fill the main gaps in the description of the physical context and the justification of the model structure.

Q(1) For the description of the physical environment, the improvements made by the authors allow a much better appreciation of the hydrogeological context. In particular, the geological features of the depression are given and the flow conditions in this area are more clearly described. Nevertheless, referring to the work of Chen et al (2018), could you be more specific on the issue of high or low permeability zones in the respective areas of W1 and W4? You mention an aquifer confinement but I rather understood that the permeability reduction was due to a different nature of fracturation (less open fractures? less connected? partially filled?). Please add details in line 189.

Reply:

The sentences were revised as “W1 is located in a local confined aquifer, consisting of extensively fractured carbonate rock surrounded by rock with low secondary porosity. W4 is located in unconfined aquifer with the vertical permeability reduction from large rock fractures and high secondary porosity to low secondary porosity (Chen et al., 2018)”.

Please refer to pages 13-14 lines 188-192.

Q(2) I am still not totally convinced by the proposed adjustment lines in figure 3. For example, the correlation relative to point W1 may be statistically significant but it is meaningless when looking at the structure of the scatter plot (a large cluster and some points that pull the relationship). I don't understand why the measurement points in the groundwater and surface water (all points together) do not lie on the local meteoric water line (which by the way is very similar to the global meteoric water line). Even though there is a large scatter, all the points line up correctly on a meteoric water line with a slight deuterium excess. In the context of the study, all waters should be aligned with the LMWL unless there is an evaporation effect or re-condensation of vapour from evaporated local surface water in a low humidity context. I do not think that this second effect is dominant here but if it is the case it could explain the higher deuterium excess observed.

Reply:

In terms of the plot of $\delta^{18}O$-δD for W1, W4, and LMWL (Fig. S1), all waters at W4 and W1 come from rainfall recharge since the line of $\delta^{18}O$-δD for W4 is closely aligned with the LMWL and the scattered points of $\delta^{18}O$-δD for W1 are concentrated around the LMWL. The agreement of the line of $\delta^{18}O$-δD for W4 with the LMWL corresponds to more rainfall recharge into the unconfined aquifer at W4 and fast groundwater flow response to rainfall (refer to Fig. 4 by Chen et al.(2018)). By contrast, the scattered points of $\delta^{18}O$-δD for W1 correspond to the confined aquifer at W1 and slow groundwater flow response to rainfall. We revised the associated descriptions.

We corrected our previous reply about re-condensation. The LMWL does not show re-condensation of vapour from local evapotranspiration (see the line of $\delta^{18}O$-δD for precipitation in Fig. 3 in the revised manuscript).

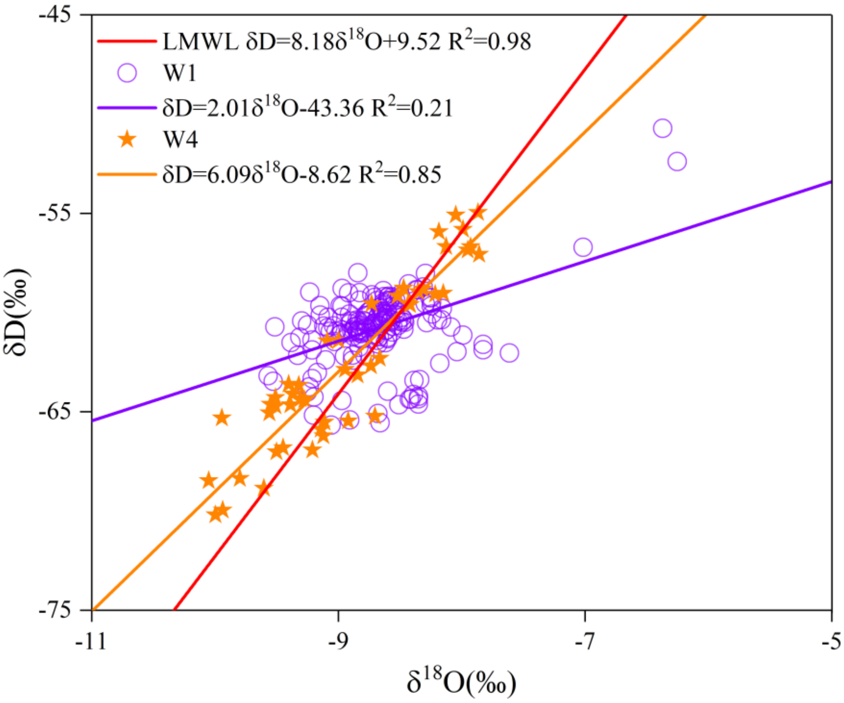

Figure S1. Plot of $\delta^{18}O$-δD for W1 and W4

Please refer to pages 15-16 lines 222-233.

Q(3) I still think that there is no really interpretable difference between W1 and W4. Of course, the average difference between the 2 points is slightly above the uncertainty but the number of measurement points is very different. I think the important thing is that the dispersion of the points is greater upstream (W4, point cloud more elongated on the relation in figure 3) than downstream (W1). This probably reflects the effects of a progressive damping of the signal downstream from the mixing of the different water fractions. Nevertheless, this descriptive part is not crucial and does not call into question the core of the work. One possibility would be to drastically reduce this paragraph by simply mentioning the average differences observed (especially between catchment outlet and hillslope spring) and by pointing out the few alterations due to evaporation in surface waters.

Reply:

We have deleted the dispersion of the isotope values (CV in Table 3) since the number of measurement points is very different at sites and the range of isotope values can reflect the variability.

We revised the descriptions in this paragraph. We believe that the use of isotopic signatures to indicate the hydrological functions of flow damping, water evaporation and connection from hillslope spring to catchment outlet has been sufficiently described in the revised manuscript.

Please refer to pages 15-16 lines 222-233 and page 18 lines 254-256 (Table 3).

Q(4) The structure of the model is related to the geomorphological context. Once the conceptual model is established, it is easier to understand the choices for the interactions between UZ and SZ and between hillslope and depression. It is interesting to have introduced a parameter allowing a proportion of the water from the hillslope slow reservoir to join the depression slow reservoir. From the new optimization procedure, the optimized ratio rhd was 0,39 for model f. For what I understand, this mean that 61% of the slow flow from the hillslope reaches the depression conduits. If so, the overall concept is therefore significantly different. Is the increase of the fast flows proportion in the depression underground channel still consistent with the observations of the flow dynamics at the underground channel outlet? (not only the proportions of total flow)

Reply:

The hillslope flow is composed of the most fast flow component (over 80%) and thus the slow flow amount is small (less than 20%). So even though 61% of the slow flow on the hillslope enters the conduit of the depression, the hillslope slow flow only accounts for 13% of the total flow allocated from the hillslope to the depression as shown in Table 8 (the revised manuscript). So, it is still consistent with the observations of the flow dynamics at the underground channel outlet that fast flow is the dominant component in the catchment.

Q(5) technical points :

1)- Line 178 and line 181 : only one spring I suppose? If so, please leave "spring" in the singular

2)- Line 205 : hillslope flow : do you mean hillslope spring discharge ?

3)- Line 372 : Would it be appropriate to complete figure 5 by mentioning the parameter Vm? As the surface flow represents almost half of the total discharge, Vm is a major parameter.

Reply:

We have modified those points according to the comments of the reviewer.

Please refer to page 13 lines 178-181, page 14 lines 207-208 and page 20 lines 278-281 (Fig.5).

Responses to Editor Polina Shvedko:

Notification to the authors:
For your next revision: Please add the country to the second affiliation. Please make sure that all text including citations are in black font. Please ensure that the colour schemes used in your maps and charts allow readers with colour vision deficiencies to correctly interpret your findings. Please check your figures using the Coblis – Color Blindness Simulator (https://www.color-blindness.com/coblis-color-blindness-simulator/) and revise the colour schemes accordingly.

Reply:

We have revised our manuscript according to your requirements.

1) We have added the country to the second affiliation. Please refer to page 1 line 12.
2) We have revised our manuscript to make sure that all text including citations are in black font. Please refer to the revised manuscript and the authors' tracked changes.
3) After checking all the figures with the Coblis - Color Blindness Simulator, we found that there was a problem with the color schemes in Fig.4, so we redraw it. Please refer to page 18 lines 250-252 (Fig.4).