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Comment on hess-2021-492

Anonymous Referee #1

Referee comment on "Effects of passive-storage conceptualization on modeling hydrological function and isotope dynamics in the flow system of a cockpit karst landscape" by Guangxuan Li et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-492-RC1, 2022

General comments:

The paper presented by Li et al. deals with the internal organisation of hydrological systems in terms of the number of reservoirs involved, interactions between these reservoirs and their relative contributions. It is a classical conceptual approach comparable to that of global hydrological models but enriched here by the contribution of tracer data. The article is well written overall, well structured and the illustrations are of good quality (except for figures 6 and 7 which are difficult to read because of the chosen scales). The objectives are clearly stated and the methods used are appropriate and sound. This approach is not, however, original and is a contribution to the series of studies that have been carried out for several years on the contribution of isotopic data to improving the structure of hydrological models (see Uhlenbrook S, Leibundgut C. 1999 for one of the first studies in recent advances). The list of references appears well balanced at first glance with about one third of the references cited being less than 5 years old and half being less than 10 years old. There is little very recent literature on the understanding and modelling of karst systems or on coupled flow-isotope modelling involving questions on mixing processes, residence time distribution or the relationship between velocity and celerity. On the other hand, one third of the articles cited that are less than 5 years old already concern the basin studied (+ 2 other older articles), one of which mentions a coupled hydrology-isotope model. The topic is therefore promising, but we must ask ourselves how this new study improves our knowledge of the system and whether we have made any progress in terms of conceptualisation. Apart in the introduction, this question is never addressed and as it stands it does not seem that a totally convincing conceptual scheme has been proposed. In particular, there is too great a disconnection with the field. Beyond the relative adequacy with the flow and isotope data, how does the structure of the model match the morphology of the catchment, underground and on the surface? A broader discourse is also missing. The authors partly answer the initial question (line 111) but this only concerns the micro site studied. Can the proposed structure be generalised to larger areas (in comparable cockpit karst contexts) ?

Specific comments :

Lines 60-65: the list of references could be extended by some more recent articles (<10 years)

Just as examples :

Hydraulics in karst : Ding, H., Zhang, X., Chu, X., Wu, Q., 2020. Simulation of groundwater dynamic response to hydrological factors in karst aquifer system. Journal of Hydrology 587, 124995. https://doi.org/10.1016/j.jhydrol.2020.124995

Modeling in karst : Ollivier, C., Mazzilli, N., Olioso, A., Chalikakis, K., Carrière, S.D., Danquigny, C., Emblanch, C., 2020. Karst recharge-discharge semi distributed model to assess spatial variability of flows. Science of The Total Environment 703, 134368. https://doi.org/10.1016/j.scitotenv.2019.134368

Lines 110-111: the question here is answered in the specific case of the study site. In what way is the structure of the model finally proposed transposable elsewhere and at a different scale? Are all cockpit systems of the same nature in terms of their hydrological functioning?

Study area section: The description of the site, especially the depression area, is very small. As expected, the average soil thickness is lower in the hillslope unit than in the depression unit. It is also expected that the nature of the soils is different and therefore also the field capacity. Can the authors provide details on these field characteristics ? Also, please explain the phrase "perennially flowing underground conduit connecting the hillslopes to the catchment outlet" (see also line 337-338). Do you mean that there is a main karst conduit within the depression that transmits water to the outlet? What is there between the 2 m depth at the base of the soil and the water level reached at 13-30 m (see line 187)? How deep is the bedrock? What is its nature? What is the nature of the water table in the depression? It is doubtful that we are still in a karst system of the same nature as the hillslope.

Fig 1: there are 2 points for the outlet. Please define a single outlet for the catchment area. The location of the springs is not indicated on the map (only one spring? there is only one point on figure 2. Please specify)
Lines 164-166: Do the hydrographs mentioned refer to those observed at the outlet? There is ambiguity because the following sentence refers to epikarst springs.

Observationnal dataset:

The interpretation of figure 2 is very questionable, especially the regression line of the W1 points. There is a very large dispersion but the points W1, W4 and hillslope spring are to

be included in the same O/D relationship which is also that of the local rainfall. The purple, green and black lines are disturbed by the few points indicating evaporation. The grey points (outlet) under the rainfall line are divided into two groups, probably indicating an evaporation process under different relative humidity conditions. In my opinion, it is not possible to argue about the age of the water from isotopic enrichment or depletion information alone:
1) The differences between the means for each set are modest and the number of measurement points is different each time. The difference in the mean between W1 and W4 is of the order of the measurement error. These differences are therefore not statistically interpretable.
2) Can't the apparent enrichment of W1 and outlet (vs W4) come from the inclusion of evaporated water?
3) Why is the dispersion on W1 the lowest? Could there be a different origin of water in W1 and W4? (linked to the organisation of the fracture network in hillslope).
Also specify the number of points and the origin of the data for the LWML.

Conceptual model structure :

In connection with the study area section, one level of explanation is again missing for a satisfactory understanding of the system. The structure of the model logically foresees a dual flow system in the 2 units and in the 2 compartments ZNS-ZS. But it seems to me that the authors make two very strong assumptions that need to be justified:
1) The fast and slow reservoirs are in perfect connection between the unsaturated zone and the saturated zone. This can be understood in the karstic part of the hillslope system but the continuity does not seem so obvious in the depression part where the nature of the slow flow/fast flow partition can be quite different between the soil and the water table.
2) The authors suppose a hydrological continuity between the slow and fast flowing reservoirs of the 2 units (see also lines 330-331). Are there any tangible arguments to assume that slow flows from hillslope will retain this slow flow property in the depression (same for fast flows)?

Lines 235-241: these sentences are written as if to compare the nature of the slow and fast flows in the 2 units (hillslope vs depression epikarst vs upper soil). In this context, the sentence referring to fast flows speaks of large fractures vs. swallow holes, which suggests that the latter formations are in the depression part. Can you confirm this impression? If so, should this be linked to the "perennially flowing underground conduit" mentioned in the study area section and the "underground channel in depression" in line 337-338?

Overall, the authors should make an effort to describe the hydrogeomorphological context of the system and better relate this information to the structure of the proposed model.

3.1.2 isotopic concentration routing: you do not take into account isotopic fractionation, whereas Figure 2 shows that there is evaporation. This fractionation is however integrated in the model proposed in Zhang et al (2019). Can you explain why you chose to ignore this process?

3.2 Model calibration and validation: this section is again too far from the reality of the field. The choice of parameter values to be set and calibrated must depend on the characteristics expected in the hillslope unit and in the depression unit. It is not obvious *a priori* to admit that the proportion of the matrix volume is the same in the two units. Similarly, the parameter b should be dependent on the nature of the matrix and that of the fast flow paths. Are these natures the same in the two units? Finally, how do you justify the same value of Wpas for both units and the fact that Wpas=Vpas?

Table 4: For the calibrated parameters, which model is presented on the 14 scenarios?
I may have missed something but I don't understand the NA for φsd, φfd and for Vth-pas and Vfd-pas

Line 383: prefer μ to σ to express an average (or a ratio of averages)

Line 397: I am not very familiar with multi-objective optimisation algorithms but the number of iterations seems low. In fenicia et al (2007), the number of iterations is rather in the order of a few thousand.

4.1. Performance of models:
Lines 428-429 (and Figures 6-7): The authors should be more critical about their results. In particular, the model does not really succeed in capturing isotopic variations. In many cases, it overestimates or underestimates the observed values, especially in the calibration period. In validation, the results are better because there is less variability. Finally, the performances are not better (or even worse) than those obtained with the model of Zhang et al. How can these shortcomings be explained in terms of the structure (defaults) of the model? The announced contributions of the fast flow reservoir are not inconsistent, but it is not reasonable to justify these results by those of another model. Once again, there is a lack of arguments from the field.
Table 5: How can it be explained that the parameters for the validation are better than those for the calibration? Did the authors consider switching the 2 periods?

4.2 The effect of number of passive storage
It seems difficult to me to isolate the number of passive storage from their positions in the model. The comparison of models with different numbers of passive storage combines very different situations that are not very compatible (multiple combinations between slow flow vs fast flow and hillslope vs depression). The authors could consider combining parts 4.2 and 4.3 while trying to be more concise (e.g. be more synthetic on the damping effect of passive storage on isotope simulations)
Line 493: "observed values at the underground channel": what is this about? How were these observed underground flux values obtained? And why were these results not presented in part 2.2?

4.4. The dominant transport processes
Line 619: do you mean "hillslope unit"?
Line 622-625: peclet number = vL/D where D is the dispersion coefficient (since molecular diffusion is negligible in the context presented). If Pe hillslope > Pe depression, this means that advection (not dispersion) processes are more important in hillslope unit than in depression unit. This is consistent with the idea we have of the truly karst part of the catchment. But it does not seem to be consistent with a large exchange flow between active and passive storage (EGM) which effectively leads to a larger dispersion effect. Please clarify this point.

Technical corrections :

The article is generally well written. As I am not a native English speaker, there may be some improper sentence structures, but I did not have any major difficulties in following the development of the ideas. The general structure also seems to me fine.

Lines 292, 293, 443 and table 8: the "multiply" sign can be confused with the "minus" sign.

Lines 582-585: Not clear. Please review this sentence structure.