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Comment on hess-2022-75

Anonymous Referee #2

Referee comment on "Evaporation loss estimation of the river-lake continuum of arid inland river: Evidence from stable isotopes" by Guofeng Zhu et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2022-75-RC2>, 2022

Review comments on Zhu et al. "Evaporation loss estimation of river-lake continuum of arid inland river: Evidence from stable isotopes" submitted to Hydrology and Earth System Science

The manuscript by Zhu et al., presents monthly d18O and d2H observations in river/lake water between April 2017 and Sep 2019 in the Shiyang river basin. The Hydrocalculator is used to derive E/Is (evaporation over input ratio, f noted in the manuscript) from isotopic signatures and factors impacting the quantitative evaluations are discussed. They conclude that the evaporation loss in the Shiyang River basin progressively increases from mountainous river reach (1.3%) to the oasis section (2.87%); an oasis reservoir (7.97%); and finally to the terminal lake (41.37%).

The data in the paper has been reported in Sun et al., 2021, which was also focusing on evaluating evaporation loss with isotopic approaches. Therefore, thorough data analysis with a sophisticated approach is expected for this manuscript. Unfortunately, the setup of the isotopic mass balance in this manuscript is questionable; Compared to the previous study(Sun et al, 2021), the results do not provide more insights into the isotopic system or hydrology in an arid/semi-arid region; and the discussion on the impacting factors on their isotopic evaluation stay superficial. I would not recommend the publication of the manuscript. Here, I provide my general comments on the methods, followed by line-specific comments.

- A simple application of the Hydrocalculator without details on parameter setups and assumptions on hydrology is not acceptable here. Hydrocalculator was developed for the purpose of easing isotopic mass balance calculation, but it still requires prior

knowledge of hydroclimatology and regional isotopic signatures in the study site to be able to choose isotopic inputs with reasonable assumptions. To help readers understand and evaluate the results, details on parameters, (especially dI and dA) must be provided.

According to this manuscript, the water balance in the region is complicated. In addition to common natural components such as precipitation, mountainous runoff, evaporation, and river discharges, some unusual components such as water abstraction, irrigation returns, surface water transfer, and possible groundwater are also important elements in the water mass balance of the watershed. These elements could be reach-specific. As a result, assumptions and discussion on dI are essential. This manuscript appears to characterize dI (isotopic composition of input water) only with dP (isotopic composition of precipitation), (no details provided in the method though). This makes the following discussion on impacting factors extremely weak and consequently qualitative.

On the other hand, the isotope system in the study region is extremely interesting as revealed in Figure 5. Isotopic signatures in mountainous reach (Figure 5a-5c) are characterized by a high slope (close to 8) and high d -excess (above the GMWL/LMWL), which is rare (I only see a similar pattern in coastal lakes in the North America). I suspect that the ambient moistures are decoupled from dP - dA equilibrium. Isotopic signatures in the oasis reach (Figure 5 d-e) are generally characterized by values depleted than the intersection of GMWL and SWL, which indicates sources other than mean annual precipitation sustain the streamflow. Isotopic signatures in an oasis reservoir are characterized by unusual spread and possible clusters. I am not sure what lead to this. (different source mixing in different seasons?). While, Figure 5f (terminal lakes) show sign of evaporation of precipitation before the precipitation reach the ground, which may contribute to the enriched isotopic signature in lake water.

So, the parameter setup for the Shiyang River basin could be reach-specific. Details and assumptions need to be provided for readers for a better understanding of the paper and the system.

- dA (isotopic composition of ambient moisture) is an important, but the most uncertain parameter for the Hydrocalculator, as suggested by Skrzypek et al (2015). This manuscript did not provide any info on how was dA derived, other than a brief discussion on dA (lines 341-348). It is very interesting to note that, the leading author recently published a paper on the contribution of recycled moisture to precipitation in the Shiyang river watershed (Zhu et al., 2019). Providing systematic sampling of precipitation and surface water; numerous weather stations and previous knowledge on moisture recycling in the region, I think the authors are in a good position to develop detailed isotopic mass balance models with consideration of various scenarios of dA. This could also lead to reconciling E/I discrepancies based on d18O and d2H separately. It is disappointed that this manuscript did not make any effort to investigate and discuss several possible dA scenarios.

- The manuscript uses equation 2 to scale-up station-based estimation of f to catchment-scale f . However, their weighting approach is not justified. First of all, it is not clear what is the unit of V used here (mm or m³)? I do not know if the contribution area plays a role. Secondly, the manuscript does not provide any justification for using V as weighing metrics? Why not use potential evaporation or precipitation as weighting metrics?

- The sensitivity/uncertainty analysis (section 5.2.3) could be significantly improved. First of all, the author missed dA in the sensitivity analysis, which is an important source of uncertainty as I aforementioned. Secondly, uncertainties are not only sensitivities as shown in Figure 7, it is the product of sensitivity and variability of input parameter. I agree with the author that E/I is sensitive to h , however, if the overall variability of individual parameters is considered, the impact on uncertainties may be different for input parameters. Providing systematic sampling on dP, and numerous weather stations, the combination of sensitivities and parameter variabilities shall be evaluated together to assess uncertainties.

Line specific comments:

- P4, lines 71-73 what is "constant climate change"?
- P5, line 79, what is "mobile water system"?
- P5, lines 80-87 I disagree with this statement! What do the authors mean by "instability factors"? These listed factors also impact isotopic application in lakes, wetlands, soils and reservoirs....
- P5 lines 89-90 What do the authors mean by "surface river system is the main contributor to the surface evaporation"?
- P5 line 92, "production and life of local residents" is not the right expression
- P6 line 113, "a peak of 1300-2000 m" is not the right expression. May be "with the elevation in the range of 1300-2000 m"?
- P6 lines 118-119, Change "Precipitation" to "Precipitation amount"; "greater" to "great"
- P6, lines 121-125 running sentence, please rephrase
- P8, line 146, correct "Shyang" to "Shiyang"
- P8 line 150 remove "more"
- P8 line 152, wonder why "natural river" is emphasized here?
- P9, line 168, no precipitation amount measured at weather stations?
- P11 line 209, unit for f and V, please
- P14, line 246, "a trend of gradual enrichment" is not an accurate way to describe the observed pattern in Figure 3
- Figure 3, I would suggest adding standard deviation bars in the Figure.
- P15, lines 265-268 was the water transfer conducted seasonally or year-round?
- P16 lines 294-297 how does the annual average runoff (in Mm³) come?
- P19 Lines 331-334, I do not agree with the statement
- P20, line 359-361 Water conservation projects deserve a section in the study area
- P21 line 371-372, I do not exactly follow the argument! The statement is different from the illustration (Figure 6)
- Line 413, figure 8?
- Lines 420-422 I do not follow the statement. Stable state vs flowing state? Any reference for it?
- Line 425, shall "Li et al., 2021" be "Li et al., 2016"?
- Line 433, "the interface between the surface water and vapor"? Would be "between surface water and air"?
- Lines 434-436, I do not agree with this statement. The sensitivity exercise (Figure 7) only demonstrates that E/I results are more sensitive to h, than T, but it does not support the statement that the main source of uncertainties is h, because we did not know the overall variability of h, T, dA, while numerous weather stations are available to the study.
- Line 448 - "initially strong"?
- Line 467, Encryption sampling? what is it?