

Dear Mrs Ali,

Thank you for considering our manuscript entitled “Triple oxygen and isotope systematics of evaporation and mixing processes in a dynamic desert lake system” for publication in *Hydrology and Earth System Sciences*. We highly appreciate the constructive comments and further suggestions of the two reviewers. We followed almost all suggestions of both reviewers. In our opinion, the proposed splitting of the section on the “determination of the turbulence coefficient” does not lead to an improvement of the readability of the manuscript, and is better presented in context. Therefore, we have not implemented this. Please find attached a revised version of our contribution and a detailed point-by-point response to the reviewer’s comments below.

Reviewer 1:

I congratulate the authors for a thorough revision of their paper, which now demonstrates the utility and benefits of triple isotope studies in arid environments. The methods improved and the paper clearly conveys important key messages. The only very minor suggestion I still have is that I would urge the authors to include the three specific objectives stated in the response to Reviewers also at the end of the introduction in the paper.

We highly appreciate the comments of Reviewer #1 and list now the three previously stated objectives in the introduction.

Reviewer 2:

General Comments

The authors have done a good job revising the manuscript, which has greatly improved the readability and replicability of the results. Nevertheless, there were a few instances of odd structuring (e.g. methods in results, results in discussion) that should be revised. The majority of this revision is minor clarification and rearranging text already within the manuscript. I have highlighted the suggested changes in the specific comments below.

We again appreciate the constructive comments and suggestions of Reviewer #2, which helped to finalize the manuscript for publication. We followed these suggestions in the revised version of the manuscript. Please, find below in black the comments of the reviewer, in blue our responses to the comments and how these comments are addressed in the revised manuscript.

Specific Comments

General comments throughout the manuscript: Throughout, suggest changing “about” to “approximately” for more formality. Check the presented precision of the data. Significance levels change throughout the manuscript (suggest keep to 1 decimal)

We changed “about” to “approximately”, where we found it to be appropriate. We checked the precision. Presented significant levels are 1 digit for primary isotope parameters ($\delta^{17}\text{O}$, $\delta^{18}\text{O}$, $\delta^2\text{H}$) and no digits for secondary isotope parameters (d-excess, ^{17}O -excess).

P1L28: “The GMWL describes the equilibrium...” There needs to be some context here. The GMWL described the equilibrium of deuterium and oxygen-18 (these isotopes are not mentioned above).

Sentence rephrased.

P2L31: suggest changing “from” to “using”

Done.

P2L33: I would take out groundwater recharge from here. The C-G model can be applied to pan evaporation (i.e. not continuous groundwater recharge).

We agree, the C-G model can also be applied to pan evaporation. However, in this case the ‘groundwater recharge’ is still a variable assuming a value of zero.

P2L37: Suggest adding a reference here.

Reference added.

P3L73: Are these annual average fluctuations? Or seasonal averages

These are ‘mean seasonal values’, i.e. average values of the respective season (Dec-Mar for austral summer, Jun-Sept for austral winter) averaged over the period since the installation of the meteorological station in 2015.

P3L74: Can you provide a value for “calm” as you did for “very windy”?

The wind speed is usually < 1 m/s in the morning. Information added.

P3L83: Suggest changing “higher” to “more enriched”

The water is more enriched in ^{18}O and thus comprises a higher $\delta^{18}\text{O}$ value. Thus, it’s more correct using “higher”.

P3L91: Suggest changing “rainy season” to “rainy season (summer)”

Done.

P5L133: Suggest revision to “The 600 ml pan dried up...”

Done.

P5L136: Swap the numbers to for the order listed (RH then temp)

Done.

P5L148: Why is T2 presented before T1? Suggest changing the order.

To keep the logical structure in the supplement (Terminology before showing results), we decided not to change the order.

P6L174: “relative humidity, h, normalized ...”

Done.

P6L174: Do you mean that the RH at 1.5m is adjusted to the surface based on water surface temperature?

Yes.

P6L182: I would suggest having the definitions of both α ’s earlier with the equations. The causation is fine to keep here.

Done.

P6L185: Specify that “n” is the fractionation factor. Again, this would be better to be introduced with the equations above. Please change the location of the n superscript for consistency with the equation.

We removed n from the equations and introduce it now separately below.

P7L201: It is odd to switch from using R to denote isotopic composition and δ notation here. If R is different from the isotopic composition δ (i.e. not standardized, in concentration, etc) then it should be specified.

We replaced “composition” by “ratio” in the definition of R and added the formula relating R and δ .

P7L201: If I understand correctly for the later mixing models, you use this equation (δX_{mix}) for mixing the incoming water. If not, where is δX_{mix} used? It might be better to use the RWS equation from T1 here if that is used for Figure 11.

This equation defines the mixing line shown in Figure 3, 4, and 10. The R_{WS} equation defines recharge evaporation trajectories shown in Figure 3, 4, and 11.

P7L202: Remove the underline from X_{mix}

Done.

P7L205: Suggest changing to “..mixing processes are likely transient...”

Done.

P8L247-248: Increasing and decreasing with relative to what? Time periods?

Values of $\delta^{18}O$ increase with decreasing ^{17}O -excess and d-excess, which is characteristic for evaporation. Sentence rephrased.

P9L262-268: Parts of this section seem more suited for the discussion than results. Additionally, the statement of similarities between vapour estimated by OIPC and measured comes before the values of OIPC are given.

The isotopic composition of the atmospheric vapor is a prerequisite for modelling the C-G evaporation trajectories, and not the focus of the manuscript. Therefore, we think it improves readability when the determination of individual parameters of the C-G equation is presented in individual subsections rather than distributing the information over the whole manuscript.

P9L272-273: Initial composition is better suited to the methods section rather than results.

This is the results of the measurement and thus belongs in the results section.

P9L276: No need to describe what the n value is again here.

We agree.

P9L277-281: This should be in the methods section to describe how you get the n value. The reader should know how you will get the n value before the results.

How to derive the turbulence coefficient is a method that is not established but rather developed within the manuscript. Thus, from our view it belongs in the results / discussion section.

P10L283: I think it would be more correct to say that the fit produces the X n value, suggesting higher than average turbulence (i.e. $n < 0.5$).

Sentence rephrased.

P10L283-286: The comparison to literature and explanation of differences is better given in the discussion than here.

We agree that this section includes results as well as discussion of the pan evaporation experiment. However, this subsection aims to determine the turbulence coefficient used for the C-G model, which is a prerequisite for applying the C-G model presented in the discussion. From our point of view, dividing in several subsection would reduce the readability of the manuscript.

P10L287: As the comparison of $\delta^{2}\text{H}$, $\delta^{18}\text{O}$, and d-excess v is important to understand here, the rationale for examining all three should be moved the methods section alongside the methods of obtaining n. Right now, the examination of $\delta^{2}\text{H}$, $\delta^{18}\text{O}$ seem separate and as an afterthought to d-excess. It would help the flow of this section to present the fitting of $\delta^{2}\text{H}$, $\delta^{18}\text{O}$, and d-excess v, then present the sensitivity of d-excess fit n as it gives the most reasonable value (and by reasons described by Gonfiantini et al., 2020).

In fact, we used two methods to derive the turbulence coefficient. 1) Fitting the evaporation trajectory individually to $\delta^{18}\text{O}$ and $\delta^{2}\text{H}$ vs the fraction of remaining water. 2) Fitting the evaporation trajectory directly to d-excess vs the fraction of remaining water. The latter is advantageous due to its insensitivity to other model input parameters, especially $\delta^{18}\text{O}_v$. We rephrased these two paragraphs to clarify this.

P10L290: Figure 8 caption is in the incorrect order (should be d-excess, $\delta^{18}\text{O}$, the $\delta^{2}\text{H}$)

Thanks, changed.

P10L309-320: This discussion of where the C-G model is still deviating from the samples is better served in the discussion section 6.4, on the C-G- applicability.

We agree that this section combines results and discussion. However, for the reasons mentioned above, we decided to divide the sections by topic.

P11L327: Please provide the most relevant papers from within the study.

Done.

P12L355: Which figure is the envelope mixing referring to?

Fig. 4a. Added.

P12: Section 6.3. This whole section appears to be more methods for how to average the temperature and relative humidity of the mixing models than a discussion of the above-presented results. Most of this section should be moved to the methods, with the remaining going to the results (e.g. Section 5.1. presenting the conditions of the salar). This way the effect/change of the residence times can adequately be discussed in Section 6.4.

We agree and shifted this section in the Methods.

P13: Section 6.4. There is a great deal of this section that should be in the results as an evaluation of the C-G model robustness rather than in the discussion. The authors have improved on the presentation of mixing, though it restricted primarily to the supplementary material and is not referred to much. Additionally, the references to Fig 9 should be Fig 11. Further discussion of the differences/sensitivity of the C-G model would additionally help this section.

It is unclear what the reviewer is missing in our discussion. We have added two sentences on the relationship between $\delta^{18}\text{O}$ and salinity.

P14L423: I would suggest starting with a conclusion of the system before concluding findings of the C-G model, for consistency with the objectives.

We revised the conclusion and summarize now first the conclusions drawn from the triple oxygen isotope system, before evaluating the impact of hydrological dynamics on the C-G model.

P14L425: A requirement for what? A good estimation using the C-G model?

A requirement for the application of the C-G model. Added.

P14L434: Suggest changing “fall on top of each other” to “overlap”

Done.