

# ***Interactive comment on “Triple oxygen isotope systematics of evaporation and mixing processes in a dynamic desert lake system” by Claudia Voigt et al.***

## **Anonymous Referee #2**

Received and published: 8 September 2020

The paper “Triple oxygen isotope systematics of evaporation and mixing processes in a dynamic desert lake system” by Voigt et al submitted for potential publication in HESS presents a field evaporation experiment with additional monitoring and sampling from the Huasco salt lake in the Chilean Altiplano desert for a triple oxygen isotope study and modelling with the Craig and Gordon model. The data shown is novel and there are not many publications in hydrology available that use oxygen-17 additionally to the much more common oxygen-18 and deuterium based stable water isotope studies. The study was also carried out in an extreme hydroclimatic environment providing new insights into the recharge and mixing processes of the salar. The novelty in hydrological process understanding could indeed be inferred from the oxygen-17 tracer additionally

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to the other measured tracers and be supported by the modelling. I, therefore, see potential for this paper to contribute to the body of literature on isotope hydrology.

Having said that, I think that the paper could benefit from a thorough revision of structural aspects to clarify the key messages and conclusions. The paper does mix methods and results in many parts which I think rather confuses the reader. For example, already in the introduction you use a lot of detailed methods including equations 1 and 2 followed by your own results without stating clear research objectives. This might be a bit of a style questions and I see merit in this approach for a theoretical paper, but your paper is based on experimental work in a specific environment and falls out of the former category of scientific works. Therefore, I suggest to more generally introduce the potential utility and challenges of the oxygen-17 tracer in hydrology as this is still not widely used. You could also point towards the fact that you used IRMS and not a laser instrument. I would also urge the authors to present two or three specific objectives for clarity that can be used to guide the reader through the paper. In the methods, I found that the HYSPLIT analysis, the OIPC and the E/I modelling is not explained. I would also suggest to present the Craig and Gordon model with equation and in more detail in the methods clearly stating which parameters you varied to assess potential model uncertainty, how exactly you derived the wind turbulence parameter (this appears in the results) and the model experiments you are undertaking to assess the influence of measured atmospheric vapour isotope composition in the model. This leads me to suggest separating the results from the discussion and to only use two to three sub-headers that refer back to your specific results rather than at the moment 6 results sub-headers for clarity. These could be grouped according to field experiments, hydrological processes and model experiments as an example.

For the above reasons, I feel that this paper has potential but is not quite ready for publication in HESS and I invite the authors to consider my comments before the manuscript can be published with a clear message of the novel contributions.

Specific comments:

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Title: I would suggest to substitute the term “systematics” with e.g. “dynamics” as systematics implies a general classification scheme of processes and their inter-relation, which I think is an overstatement for a case study.

Abstract: - I am not sure what you are referring to with the fundamental hydrologic process of recharge evaporation. If this is a new term you are defining it needs a proper definition and comes a bit early in the abstract. - I don't think you resolve the hydrologic balance in terms of a water balance of the lake as you don't calculate any fluxes.

Keywords are missing?

Table 2: Atmospheric vapour isotope composition was measured and not estimated.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2020-255>, 2020.

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