Reply to Referee #2

We appreciate the helpful comments and suggestions of Reviewer #2. The provided comments mainly concern structural aspects, which were also criticised by Reviewer #1. We will follow these suggestions in the revised version of the manuscript as we have realised that it significantly improves the readability of the text and makes key messages clearer. Please, find below in black the comments of the reviewer, in blue our responses to the comments and how the comments will be addressed in the revised manuscript.

General Comments:

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 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.

This is a point that was also considered by reviewer #1. We thoroughly revised the introduction and focused on the larger implication on the 17O-excess parameter with a minimum of equations that are necessary to understand how 17O-excess and d-excess are derived. A detailed section regarding the terminology will be provided in the supplement. Furthermore, we now better point out the main objectives of the manuscript to: 1) test the potential of triple oxygen isotope analyses to resolve fundamental hydrologic processes of evaporation and mixing of sources that cannot be resolved by the classical δ^2H-$\delta^{18}O$ analyses; 2) test the robustness of the Craig-Gordon model in a highly dynamic environment with considerable seasonal variability in all the model input parameters; and 3) demonstrate the potential of triple oxygen isotope analyses to derive the hydrological balance of lakes from water isotope and climate monitoring. The site of the Salar del Huasco was chosen because of its known extreme seasonality in order to obtain a maximum range of isotopic variability as a result of the above processes. The study's purpose was not to primarily investigate the seasonal dynamics of the Salar del Huasco in detail.

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.

In the revised manuscript, we have added subsections on the determination of the turbulence coefficient, atmospheric vapour and the model sensitivity tests in the methods sections. We have also added a paragraph describing the Craig-Gordon model with the major equations. Details on terminology and the Craig-Gordon model will be provided in the supplement.

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.

We revised the structure of the results and discussion section but we found that separating results and discussion rigidly would in this specific case hinder the flow of reading.

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.

We have now updated the manuscript considerably, following the reviewer's suggestions. It is now better suitable for a broader audience with clear messages on the novelty of the ^{17}O-excess parameter to investigate evaporation and mixing.

Specific Comments:

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.

The term dynamics is already included in the title. It is unclear why the triple oxygen isotope systematics for our case study should not apply globally. Mixing and evaporation occur in all such environments.

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.

Recharge evaporation is now explained. It refers to a pond that is constantly recharged while water is lost via evaporation and outflow.

The C-G model is designed to estimate steady-state (i.e. the water balance is constant). However, building a proper flux model requires a solid understanding of all relevant fluxes. The C-G model is a fundamental step in this direction. In fact, we already discuss fluxes that are important e.g. to understand how fast a pond adapts to steady state.

Keywords are missing?

Keywords are not requested at this point of submission, but will be provided.

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

We will change this accordingly.