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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 #1

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General Comments

The manuscript "Triple oxygen isotope systematics of evaporation and mixing processes in a dynamic desert lake system" explores the isotopic dynamics of a terminal lake system in Chile using evaporation pan experiments and Craig-Gordon evaporation modelling. The authors collected samples from numerous small lakes and ponds, groundwater springs, and atmospheric vapour to evaluate the effect of evaporation, sensitivity of input variables for the Craig-Gordon model, and assess the mixing of ponds. The study shows the highly sensitive nature of O17 in ponded water during fractionation and is an assess to partitioning water into mixed and evaporated water

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Discussion paper

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pools.

I would recommend major revisions to improve the presentation of the manuscript. Three key issues need to be resolved as follows.

Firstly, the objectives and significance of the study are not clearly presented in the introduction. There is a limited introduction to the implication of using oxygen-17 other than “a potentially powerful tool” with much of the remaining introduction on oxygen-17 more suited to a methods section than an introduction. The importance of desert lake systems is central to this manuscript but is has limited emphasis only to oxygen-17. The objectives of the manuscript appeared to be only a sensitivity test of input variables in the Craig-Gordon model rather than assessing the dynamics of the salar system as a whole and using the Craig-Gordon model as a tool. The last part of the introduction seemed to be more of an abstract than an introduction and needs revision.

Secondly, the issues with the presentation of the methods and sampling are closely related to the third issue (results and discussion). Some of the information in “Sampling” belongs in “Study Site” (e.g. connectivity of ponds) and the section would benefit from more emphasis on the different conditions of each area during the sampling periods. Through the “Sampling”, “Methods”, and “Craig-Gordon” sections (as well as some introduction parts) there are terms that are not introduced properly or defined (e.g. d-excess, E/I). The “Sampling” section does not include the measurement height of the atmospheric data that was collected (temperature, relative humidity, δv), which may be significant for use in the Craig-Gordon model. The section on Craig-Gordon modelling lacked sufficient detail to allow for the replicability of the results. The formulation of the Craig-Gordon model used for oxygen-17 was not provided (I assume it is a similar form to Surma et al., 2018) which would be useful for the readers to understand the sensitivity assessed by the authors. It would also be useful if the authors would provide the other values used in the Craig-Gordon model (e.g 17αl-v_evap, 17αl-v_diff). Additionally, there is no information on how the authors accounted for mixing. Is it changes to the input end-member? Is it changes to the E/I ratio?

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Thirdly, there are three main issues with the results and discussion section, the number of new methods introduced in the results, the amount of significance placed on few data points (vapour compositions), and the limited discussion of the results. Methods introduced in the results section include the HYSPLIT model (results shown without any previous mention of the model), translation of δ18Op (from OIPC) to δ18Ov, Monte Carlo simulations and fitting of Craig-Gordon to evaporation pan data, and the set-up of sensitivity testing and the evaluation of the sensitivity. These components should all be introduced and described in the methods section. Through the results and discussion section, a lot of weight was placed on the atmospheric vapour compositions which were sampled over two days. While these samples are very important to constrain the Craig-Gordon model and an uncertrainty approach has been taken to assess some of the variability, the likelihood of large annual variability and impact should be discussed in more detail rather than discrediting the OPIC on two sample days. The discussion of the results is limited, particularly with the model uncertainty and the explanation of the dynamics of the salar, in context to the literature. Some ideas that may help the discussion could include (1) the impact of ice and high temperatures on evaporation pans and isotopic fractionation (2) the larger implications of model uncertainty, and (3) discussion on the causes of intra-annual changes of specific ponds (e.g causes of shifts in d-excess- or 17O-excess-δ18O space in Figs 9 & 10).

If the authors can make substantial improvements to clarifying the objectives and larger significance, describing methods, and expanding the discussion, the results could be a significant contribution for publication. Many of the issues above are described further in the Specific Comments section.

Specific Comments

P1L14: What is “recharge evaporation”

P1L19: Is it really a main finding to give a specific value of wind turbulence?

P1L21-26: What are the results that this tools give us? No need for all of the mineral

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examples.

P1P26: Change the word “predicts” to a more relevant term

P1L27: Define α17 and α18

P1L27-29: Sentence not clear

P2L31: Define δ17O and δ18O

P2L33: Define what “x” is

P2L36: Describe what 17O-excess shows (i.e. more negative is more fractionation/evaporation)

P2L47-58: The section is more of an abstract than an introduction/objectives

P3L72: How many years were used for long-term averages?

P3L81: Is there water loss from the lakes back to the groundwater system during low groundwater levels?

P3L86: Change the word “probably”

P4L98: Suggested change to “. . .18:00, but on the third day at 13:00”.

P4L100: Where are the weather station and evaporation pan? What are the ‘+’ markers in Figure S1? Is that water temperature?

P4L100: take out “at the experiment”

P4L104: Is the evaporation pan completely thawed at 9:30?

P4L114: Remove ”i.e. the general direction of the Pacific coast”.

P5L135: There was no previous mention of chemistry data. This data would be a useful discussion point in the manuscript for water sources and would help justify input sources for the Craig-Gordon model and the overall mixing of salar

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P5L141: d-excess is not defined.

P5L141: Why is d-excess reported here when d-excess is dependent on δ18O and δ2H

P6L154: Change the wording of “classic evaporation theory”.

P6L154: There are more than two scenarios; this manuscript is only examining two.

P6L156: “e.g. as a result of flooding or snowmelt”. Meaning the addition of other source waters?

P6L157: What trajectories?

P6L158: What was used to create Figure 3? There isn’t much discussion of what is on Figure 3.

P6L164: “initial or inflowing water”. Should this be “initial and inflowing water”? In general, these two are different components. An assumption needs to be stated here that they are the same.

P6L167: Oceans have a value of $\approx$ 0.5, stating a theoretical value doesn’t make sense here under natural conditions.

P6L170: Remove “classic”

P6L175: This is more results using data from the region rather than a method. Each subplot needs to be described or this should be in the supplementary materials.

P6L180: What about the diffusive properties? They could have a large effect on the results.

P6L182: Give the method used in Section 6.2 for determining n here

P6L182: What was the height of the measurement? Is it still used if it may not be representative? Not clear what value was used in the end.

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P7L186-191: This should be in the study site section.

P7L194: This should be in methods. How many samples were taken? What was the temporal resolution?

P7L195-199: This should be introduced in the methods section if it is significant enough for a figure in the manuscript. Otherwise, the discussion can refer to it in the supplementary material (with a description of the model in the supplementary material).

P7L200: There needs to be a definition for the OIPC.

P7L200: How did you get the δ18Ov from the precipitation? What values did you use for the correlation of precipitation to vapour? Also, the Bowen et al., 2005 reference should be for monthly values not annual values. Bowen et al., 2003 should be for annual

P7L204: "rainfall data", suggest a change to "rainfall isotopic estimates"

P7L205-208: Provide references that would suggest that the OIPC would not provide a reasonable annual or seasonal value of precipitation. The OIPC isn't intended for use on temporal scales less than one month (in an average year), so it would not be surprising if two samples may deviate from the average of a month.

P7L213: change the wording of "well constrainable"

P7L214: Suggest changing ". . .derived empirically from a plot. . ." to ". . .estimated from a best fit curve. . ."

P7L215: Fig 6a

P7L216: What is meant by "barely sensitive"?

P8L217: "this approach" suggest a change to "the C-G model"

P8L218: Why a monte carlo approach? There is only one unknown. There is no previous description of monte carlo simulation approach to the C-G model

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P8L218: With the monte carlo approach did you take the best value? Is there no uncertainty with the Monte Carlo approach here?

P8L220-221: What about changes to evaporation due to the overheating of the evaporation pans?

P8L225: What about the effect of fractionation due to ice-freezing and thawing or through sublimation?

P8L230: Is the fitting done via a step-wise approach? Needs to be clarified.

P8L230: How much is “considerable sensitivity”?

P8L234: Is stating the δ18Ov necessary?

P8L235: The description of obtaining δ18Ov from the OIPC needs to be earlier

P8L239: Re-word the sentence. Why would it be tentative?

P9L251: Re-word the sentence. “fortunately”? The abstract and methods suggest that this value is well constrained. If it is not sufficiently well constrained then there should be a suggestion for further analysis and measurements.

P9L262: What about the LMWL?

P9L263: It could fall below the GMWL due to precipitation sources. It would be more relevant if this was compared to the LMWL

P9L265: Fig 8b not 7b

P9L266: Again Fig 8 not 7

P9L267: Again Fig 8 not 7

P9L271: Show the sample location on the site plot

P9L276-278: Where is the data to support the evaporation theory? What are the tributary values? How much enrichment is observed from the tributaries to the Collacagua

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River?

P10L283: Evaporation and groundwater recharge are the only two aspects tested here, so should be the dominant factors. There needs to be a statement on how equal these factors are.

P10L290: “following”. Following what?

P10L293-294: “two-spot measurement”. Two measurements? Two-days of measurements? It is not clear how many measurements there are from the methods section.

P10L295: Suggest “estimated” rather than “derived”

P10L295: “Previously shown” shown in this study? Or which studies also show this?

P10L305: E/I was never introduced. Most figures include this as E/I = 0. Is figure 10 not E/I = 1? Where is the trajectory where E/I $\neq$ 1?

P10 L309: where does the assumption of $\pm$ 5 ‰ come from? How was this value determined? Is it from the uncertainty of the OIPC? Is it the range of monthly precipitation isotopes?

P11L317: Should be “oC” rather than “%”

P11L328: Which ponds are E/I > 0.5?

Section 6.4.2: Suggested that the causes of intra-annual changes (e.g. Figure 10) are discussed for different ponds. E.g. changes in E/I for a given year?

P12L349: As with the abstract, I would suggest clarifying “recharge evaporation” here. It is defined in the manuscript as an evaporation trajectory of a pond sourced by recharge that has evaporation, but it is not clear unless one line in the manuscript is read.

Figure 1: I would suggest adding in the measurement location of the Collacagua River here

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Figure 2: For this figure and other figures, while the color scheme is good, I would suggest that the symbols be unified for the ponds (e.g. using square for all ponds/lakes and triangles for springs). The upside-down triangle (Laguna Grande) is difficult to identify on some plots).

Figure 3: What values were used to create the conceptual figure?

Figure 4: Is there an expectation of significant evaporation when the temperature is 0oC?

Figure 5: Is this figure necessary for the manuscript. There is a similar figure in the supplementary materials that would suffice. What are the thin lines on the figure?

Figure 6: What is the starting value of each interval?

Figure 9: Relabel the figure to make clearer. It is not clear that the left hand side shows the 17O-excess v. δ18O while the right hand side it d-excess v. δ18O

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