Editor

Many thanks for submitting a revised version of your paper about the fascinating L-WAIVE experiment. As you can see, the reviewer who asked for major revisions in the first round, mentions that the manuscript has improved, but fails in providing a consolidating vision, which is one of your main objectives. I also read your revised version and I agree with the reviewer that a further focusing and consolidation would greatly increase the value of this paper. My overall impression is that the paper tries to cover too many things and that leaving away certain aspects - those that are not essential for the water vapor isotope profiles - and discussing a bit more the key results could help a lot. Here a few more specific impressions I had when reading the paper:

1) Since I saw the paper for the first time, I found the title a bit cumbersome: to me, a simpler title like "A field experiment to investigate the vertical water vapor isotope profile above an Alpine lake" would be more appealing and informative.

We changed the title to go in the direction of the editor. However, we also wanted to highlight that there are not only vertical profiles. Thank you for this suggestion which helps to better understand the scientific objectives of the article.

2) I think your abstract has a rather weak ending. Lines 25-30, where you describe the first results from L-WAIVE contain very vague statements. In essence you write that there is a lot of variability depending on the synoptic circulation. To play the devil's advocate, you don't need to do a field experiment to come to this conclusion (this is obvious) ... your measurements and results contain much more precise and interesting information, and I would like to invite you to document some of them also in the abstract.

The abstract has been revised to reflect the evolution of the article and to better highlight the scientific aspects that are discussed and the main results.

3) I agree with the reviewer that the paper has a strange starting point (ice melt, biodiversity). These phenomena and concepts never appear again in the paper; I think you can directly start with humidity profiles etc.

We fully agree and the 1st paragraph has been deleted.

4) The title of the paper and the main objectives clearly focus on water vapor profiles and the stable isotope signals (which I think is perfectly fine!). However, the paper then also presents results about lake temperature profiles and aerosols, but I couldn't find out how these results influence your analysis of the vapor/isotope profiles. I rather felt distracted by the lake temperatures and aerosol results and had the impression that I was loosing track of the storyline. My suggestion is that you consider omitting these aspects of the paper and really focus on the instruments and data you need for the vapor profile analysis. This would also shorten the instrument description part.

These elements were used to help interpret the water vapour data. The position of the thermocline is important to know the thickness of the layer where homogeneous mixing can be expected. The lidar atmospheric backscatter data are used to clearly position the rainfall periods in time, but also as a tracer of the vertical dynamics of the atmosphere.

We have removed Fig. 4 and used a reference to give the position of the thermocline of Lake Annecy in summer. Fig. 10 has been revised to make it clearer and to highlight the transition between the valley boundary layer and the free troposphere. This figure is important because it gives information on the temporal evolution of this transition zone which helps to explain the vertical water vapour profiles.

In addition, we have grouped all the profile-time evolution figures (Figs 12 and 13) in the same subsection because it is preferable to discuss them together in order to describe the observed atmospheric structures. The comparison to ERA5 has been removed as it did not add much to the discussion, which is mainly about local effects, the synoptic view having been given before, in subsection 4.1.

The text of the article has been amended accordingly and paragraphs have been moved.

5) On a more detailed level, I think the paper could be shortened in several places. It sometimes reads a bit too much like a campaign report. For instance, in line 157 you mention "golden days", but then I could not find further analysis of these golden days. So why should the reader know about golden vs. silver days?

We agree that these terms are not very appropriate, they have been removed. This is all the more important if it gives the impression that this article is a report. This paragraph has been removed as the information is already present in Appendix A.

6) I also suggest to show less synoptic charts. They fill 3 pages but are not really discussed in great detail. Also, the link between a few selected charts and the analysis of the profiles could be strengthened.

We agree with the editor and the reviewer. The multiple charts had been added at the request of one of the previous reviewers. Now, these 3 sets of figures have been reduced to one figure (Fig. 3) as there is not much variability in synoptic flows.

7) I am not fully sure what I should think of the artwork in Fig. 16. On the one hand, I like schematics, but here too many things are unclear to me. For instance, what is the 0.46 km label refer to (altitude of the lake?). Is the lake boundary layer then extending to 1 km above the lake? The abstract emphasises an altitude of 1.5 km above ground level, is this 0.46+1 km? I am confused. And maybe more importantly, how can you show a typical profile of RH if conditions were so variable? And why is the cloud located where your RH profile has a minimum? I tend to suggest that you better omit this schematic or establish a (much) stronger link to the actual measurements.

Fig. 16 was made to summarize the vertical structures encountered. It is true that they are quite fluctuating and that the figure is not representative of all situations. We have therefore removed this figure, which is not fundamental to the scientific objectives of the paper.

Despite my critical remarks, let me conclude by emphasising that I congratulate the team for collecting an impressive and highly valuable data set (with a very interesting measurement strategy), and that I hope that the comments from the reviewer and myself help you to further improve the manuscript.

Your criticism is welcome and has helped us to refocus the article. Our vision was to give a broad view of L-WAIVE, but as you pointed out, this dispersed the objectives. This measurement campaign brought original data on different aspects and not all of them can be described in one article. Therefore, we have focused on the temporal and vertical evolutions in this article.

We have hence removed all the measurement tools that were not directly related to this new version or that were too general. Figures 3, 4, 5 and 11 have also been removed, along with the associated texts.

All this has led to the restructuring of some paragraphs, moving some parts, and completing others. Our objective has been refocused largely on the characterization of vertical profiles. We have therefore revisited paragraph 6 and added Fig. 8. This figure is important in this context because it shows that the vertical structures observed via isotopic measurements exist. It also allows a discussion on their evolution during the day and according to two contrasting local meteorological situations. We therefore thought that this would be a real plus for the scientific contribution of the article.

Reviewer

General comments:

The manuscript revision is improved and no longer over-sells in the intro and under-delivers in the interpretation. The editor's instructions were to:

"… perform major revisions of your manuscript, considering all the reviewers' comments and with a special focus on better framing the paper as (i) a campaign overview paper but (ii) also a paper that provides new insight into the many questions posed in the introduction, based on the interpretation of the new measurements."

The research objectives that the authors identify now are:
"The main objective of this paper is to present a novel experimental approach to measure stable isotopes of water from the interior of steep valleys to the free troposphere in order to help identifying the origin of the air masses that contribute to the observed isotope ratios. " and "… proposes a consolidated vision of water isotopologues across the air/water compartments in a lake area."

The main results pertaining to these research objectives are in Fig 17 and there is a overview discussion of how the observations are different in the different layers, but this is far from providing new insight or a consolidated vision. Figure 16's atm structure diagram is very conceptual (at least the data that it's based on is not transparent). There is an attempt at some new interpretation, but this falls solidly in a campaign overview paper. Again, I leave the suitability of a campaign overview paper for ACP up to the editor.

We searched the scientific literature for studies on the vertical distribution of water vapour in lake valleys. We did not find anything similar to the results of L-WAIVE. So there must be some originality in this work. We have refocused the paper and removed all elements that were not directly related to the vertical structure of the atmosphere and the evolution of stable water isotopes. We do not agree that this article is a campaign report. There is an initial description of the campaign and the observations because it is necessary to explain the basis on which the data were acquired before using them to interpret processes. The text has been reworked in some places and information moved to make the conclusions more prominent.

Fig. 16 has been removed as it is confusing, and we agree that it is not representative of the generality of encountered atmospheric situations.

Specific comments:

The intro spin as a biodiversity concern feels weak. There is a nice discussion of the knowledge gap of the vertical structure of the water vapor field in the lower troposphere above Alpine lakes. It would feel more natural to tie the lake influence to the surface energy budget and atm-surface interactions in complex terrain, e.g. Wang, Wei, Xuhui Lee, Wei Xiao, Shoudong Liu, Natalie Schultz, Yongwei Wang, Mi Zhang, and Lei Zhao. "Global Lake Evaporation Accelerated by Changes in Surface Energy Allocation in a Warmer Climate." Nature Geoscience, 2018, 1–7. https://doi.org/10.1038/s41561-018-0114-8.

The first paragraph has been removed to go directly to the moisture profiles as suggested by the editor.

Line 340 – Switching interval between intakes? Or physically move the sampling inlet? Length of tubing, tubing type, flow rate?

We used 2.5 m of 1/4" PTFE tubing and a 40cm 1/4" stainless steel tip on the first inlet, and about 1.5m of tubing with a 50 cm stainless steel tip. A flow rate of about 10 lpm was provided by an external manifold pump (N022AN, KNF, Germany) to either of the selected inlet lines. These details have been added to the revised manuscript.

Are figures 7-9 all necessary?

No, it was reduced to 1, but this was in response to the request of the other reviewer.

Fig 14b: any way to make flight 10 (green) data visible below 3,000m. Discussed on line 490.

To make the flights more visible, the figure has been divided in two. Flights with a gradient have been separated (Fig. 6b-c in the new version).

Fig 14a: what are the values of the 2 mixing sources?

The values of the end point have been added in the figure caption of Fig. 6.

Fig 15a: are there no vapor observations on 20 Jun AM and 21 Jun?

In the table of Appendix A there is data on 20 June, but not on 21 June.
In Table B2, there is data on 20 June with flights 14 and 15, but not on 21 June.
On 20 June AM there is data for flight 14 (late morning/midday).
All information is present.

Fig 15b: I don't see an orange dot.

The orange dot (sampling) is not available on 22 June. The correction has been done in the text.

Fig 16: The artwork is lovely, but why not show actual RH profile data? Why does RH increase from 1 km to 2.5 km? But 10 km lake regional influence is within the lower 2.5 km?

This figure has been removed.

Line 531-547: I think the authors are referring to the lake-atm interface layer as the lake water surface layer. However, when discussing equilibrium vapor throughout, it's hard for the reader keep the liquid frame of reference.

This section has been reorganized to make it clearer.

536-547 could be cut or moved after the next paragraph perhaps?

The correction has been made.

Fig 15b: Lake at 2m depth -9.5, the values with very low dex at 2m are very curious. I would not have expected that other than the surface water samples.

We don't understand the reviewer comment. There is no data point with a dex of -9.5 for the 2m samples. Maybe the reviewer looked at the circle points, which are microlayer? The only point with an unusually low dex at 2m is from 16 June (blue rhomb), with about -14 permil. This data point stands out with a low d-excess, but that we have no indication for evaporation loss after sampling. More systematic measurements would be needed to confirm if this is a realistic measurement value.

579: confidence interval looks like an equation where the terms are multiplied, but that is not the intent. Maybe place a comma between them?

The correction has been made.

Fig 17: Add the statistical description of the box-whisker plot to the figure caption. The confidence intervals are so small compared to the 25th and 75th percentiles. Is that because n for the vapor measurements so large? What if you did 1-min averaging instead of 10 sec averaging of the data? That would decrease n by a factor of 6 and widen the confidence intervals without influencing the mean values. What is an accurate representation of statistically significant differences?

What was once in the text has been added to the legend but removed from the text. Averaging does not change the median value. On the other hand, it will decrease the dispersion. In terms of the presentation of the calibrated data, we have chosen to keep the temporal resolution and therefore the vertical resolution. This is important because we make airborne measurements, and we need to sample quickly enough to observe atmospheric variability. Subsequently, the vertical profiles are degraded in vertical resolution in order to improve the signal-to-noise ratio of the data (new subsection 6.1) while keeping the main vertical structures.

Line 595-597: this sentence is vague. Can you be more specific about what values are estimated for ET?

This sentence may be too speculative and has been removed. Nevertheless, using the nearby IAEA measurements in precipitation, one can take the summer precipitation values as an upper bound of what would be contributed by vegetation (Based on data available until 2018, typical summer $\delta 18O$ (δ $2H$) values fall within the interval [-16, -1] ‰ ([-120, -20] ‰). In addition, there would be a contribution from other seasons to groundwater, drawing the vegetation signal to more negative values.

Line 649-650: “Moreover, the $\delta O18$ in equilibrium condensate above the lake is generally substantially more depleted, confirming the existence of non-equilibrium fractionation during lake evaporation.” This could just mean relatively little lake influence compared to advection.

Indeed, the correction has been made.