Response to the Reviewers' comments on the manuscript *Exploring the daytime boundary layer evolution based on Doppler spectrum width from multiple coplanar wind lidars during CROSSINN* [egusphere-2023-1977] **ROUND 2**

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We thank the anonymous Reviewer for their constructive comments, critiques and suggestions, which have helped us further advance the quality of this manuscript. Individual reviewer comments below are in bold font, while our responses are in regular font. Please note that, following the Copernicus guidelines for the write-up of final responses, we applied the *latexdiff* tool to the 2nd and 3rd versions of the manuscript. The resulting PDF has been attached with this document. The lines we refer to throughout this response correspond to lines in this *latexdiff*-generated document.

I thank the authors for their detailed answers to my questions and I appreciate the first author's dedication to the manuscript despite leaving academia. However, the author's current professional situation should not alter the manuscript's quality, and furthermore, there are four more co-authors on the list who could also contribute to the improvement of the manuscript.

We thank the Reviewer for these remarks and for giving us an additional opportunity to further enhance the manuscript's storyline.

The revised manuscript was shortened (by moving most of the methods section into a supplement) and a new chapter on MoBL evolution the valley was added (this is much appreciated), but asides from these efforts, not much changed in the revised version, despite multiple questions and concerns by both referees. By this, I mean that many questions and concerns from me (and also from the other referee) were answered in a detailed way in the response to referees document (thank you!), but were not included in the manuscript. Since the authors already made the effort in answering and creating additional figures, why do they not include the answers in some way in the manuscript? Please be aware that referee's questions are always also potential questions raised by future readers of the manuscript.

We agree with the Reviewer regarding the reader, at one point, posing the same and/or similar questions. Ultimately, when finishing up the revised version of the manuscript during the first round, we decided to err on the side of caution, given that both the reviewers raised very valid

concerns regarding the manuscript's initial excessive length.

I would really encourage the authors to include a concise discussion section to put their research into broader context, because right now, this study is still very localized and limited to a few hours (!) of few days (three, if counted correctly) within a small section of an Alpine valley (peak-to-peak distance of 10km). I would like to remind the authors on WCD's guidelines on submitting research articles (`https://www.weather-climate-dynamics.net/about/manuscript_types.html`): "Research articles report substantial new results and conclusions from scientific investigations of dynamical processes in the atmosphere within the scope of the journal. Please note that the journal scope is focused on studies with general implications for atmospheric science rather than investigations which are primarily of local interest." Therefore, I would still suggest a second round of major revisions to give the authors the chance to improve the manuscript and add important discussion points to the manuscript.

Once again we thank the Reviewer for granting us a chance to better our manuscript even more. As per the Reviewer's suggestion, we have introduced a Discussion section into the revised manuscript to put our research into a broader context, given the scale disparity and generalization of our results to other valleys worldwide. We would like to point out that, by such a line of reasoning, **every** study focusing on complex mountainous terrain (valleys being the most studied setting within such terrain) suffers from limited generality. Indeed, there are no two identical valleys in the world, at least in the sense of land use characteristics, horizontal and vertical dimensions, sidewall angle, etc. This problem is well known in the mountain meteorological community. However, we do not necessarily see this as a critically hindering drawback prohibiting peer review publication, but rather as each such study contributing a *data point* to a hypothetical parameter space bounded by the variables listed in the previous sentence. Our study contributes with an additional point to this parameter space. With each new measurement campaign (e.g. TEAMx Observational Campaign being the next major one, `http://www.teamx-programme.org/observational-campaign/`), this parameter space is continually being populated with more and more data points, which ultimately advances our knowledge of mountain meteorological processes.

The new Discussion section has been added starting on line 550 (for brevity of this document we decided not to copy/paste all the text here).

I can understand that not all of referee's remarks are equally useful, but I would like to mention examples from the response to reviewers document which would improve the manuscript:

***) page 6, R1's inquiry about the choice of the CBL threshold of 1100m. Wouldn't it make sense to add the reasoning behind it in the manuscript discussion?**

We have added the response to Reviewer R1's question regarding this threshold into the Supple-

ment starting from line 162 (via Fig. S6), rather than into the discussion in the main text, since the threshold in question is defined in the Supplement.

***) page 10, R1's question on the presence of a LLJ;**

We have added the response to Reviewer R1's question regarding this LLJ/high spectrum width near the surface into the main text on line 420:

Extremely high values of spectrum width are related to significant turbulence shear production at this time of day, which results from large vertical wind shear dU/dz in the layer below the jet maximum, and possibly also from significant momentum covariance $\overline{u'w'}$ (Stull, 1988).

***) page 16, R2: Does the new CBL height determination method help us [...]? - The author's answer to this question would perfectly fit in a discussion section.**

This question by the Reviewer from the first round of revisions has now been addressed via the newly added Discussion section.

***) page 19, R2: Rivera Valley: Thank you for this detailed answer - the manuscript would benefit if you added this to the manuscript. I don't think that a direct quantitative comparison is necessary - a qualitative one is useful as well, especially these sentences: "However we argue that drawing conclusions based on just a few i-Box stations, regarding slope influences (similar to the Rivera Valley), though potentially valid, would be incomplete without more spatially-focused measurements (e.g. scintilometer-based H measurements) or numerical modelling with appropriately fine horizontal resolutions."**

We thank the Reviewer for this suggestion. With the exception of the numerical modelling remark (which is already covered at various parts of the Conclusion section), the proposition Reviewer suggests is already (though not word-for-word) present in the last three sentences of the Conclusion section found on line 648:

On the other hand, although we have been able to provide highly resolved cross-valley transects of z_i and w_L, our conclusions were nonetheless restricted to having just three discrete sites offering H. To truly explore the CBL growth framework, similar highly resolved transects of H are necessary, a demand potentially fulfilled with an array of carefully sited scintilometers (Ward et al, 2017). We are confident that the upcoming Multi-scale Transport and Exchange Processes in the Atmosphere over Mountains (TEAMx) programme and experiment (Serafin et al, 2020; Rotach et al, 2022) will offer the necessary means and resources to address these remaining challenges.

***) R2: "well-mixed" ABL: I am sorry to say this, but it is problematic to call a bo-**

undary layer with a negative heat flux "well-mixed", because it is usually associated with CBLs or forced convection. (AMS Glossary: "The terms mixed layer, convective mixed layer, and convective boundary layer commonly imply only the buoyantly stirred layer." `https://glossary.ametsoc.org/wiki/Mixed_layer`). At 16UTC, buoyancy is likely negative in the Inn Valley (cf. Goger et al, 2018, their Figs 5 and 7). I would strongly suggest either "turbulent layer" or "shear-driven".

Following the Reviewer's suggestion, we replaced all *well-mixed* labels with *turbulent* throughout the revised manuscript.

***) page 24, line 509: Thank you for these interesting figures! I have to disagree with the authors, I think adding all the IOPs would benefit the manuscript - especially because it shows that the three IOPs exhibit similar conditions, which might allow for some more general conclusions? I would like to remind the authors that the extreme localization and restriction a few days is still a weakness of the manuscript.**

Following the Reviewer's recommendation, we have moved the three figures in question from the Supplement into the main text, while also adding specific descriptions for the conditions observed for each of the three IOPs (IOP 2b, IOP 3, IOP 4) into a new paragraph. The initial description for IOP 2a is still the main one, and we envisioned the new additions to simply describe the differences found on those other three IOPs compared to IOP 2a. To summarize, the new paragraph has been added starting from line 533:

Concerning the behavior of the other IOPs, the overall similarity between IOPs 2a and 4 is evident, i.e. somewhat higher z_i values over the valley floor and the southern slopes between 12:00 and 14:00 UTC (Figs. 8a, 11a). As already mentioned in Sec. 4.1, the deeper CBL on these two IOPs goes along with less resistance during their deepening, as their Γ values decreased in time from roughly 4 to 1 K km^{-1} (Fig. 8c, 11c). The pronounced cross-valley asymmetry in the w field on IOPs 2a and 4 due to the CVV after 14:00 UTC, although similar, differs on IOP 4 owing to relatively lower CBL still detectable by the bottom-up method. Furthermore, during the CVV phase (Fig. 11a), i.e. during the third regime (Fig. 7l), the deeper detected CVV over the plateau compared to the southern part of the valley is another significant difference compared to IOP 2a. Figure 9a indicates that IOP 2b had evidently the shallowest CBL of all four IOPs. This behavior holds for the whole N-S cross section, though z_i reached somewhat higher over the southern slope and plateau than over the valley floor until 14:00 UTC. As mentioned before, on this day a CVV did not develop in the afternoon (indicated by a nearly symmetric w field, Fig. 9b) while Γ was quite high in the morning (Fig. 9c). On IOP 3, despite the developing foehn, the CBL was nonetheless able to develop in a manner similar to the other IOPs until about 11:00 UTC (Fig. 10a). Afterwards, the continually descending foehn layer (Fig. 6g,k) reached the up to then undisturbed CBL, resulting in the inability of the bottom-up method to yield meaningful z_i anymore.

Figure edits

- As per the request by the editorial support staff, we had to redo the color schemes on Figs. 1, 5 and 6 to conform to readers with color vision impairments. Therefore these figures have been modified in the revised version (just the visuals, the content remained the same as in the previous version of the manuscript).