Authors' replies to Referee #2

We thank Referee #2 for their helpful suggestions and comments. Below, we provide our replies. The line numbers in our replies refer to the revised version.

We make it clear that the paper contains two independent analyses, one is the statistical analysis over the century of the vertical temperature gradient inside the valley from MAR data, the other one aims at analysing the vertical structure of the temperature and horizontal velocity fields inside the valley from two episodes, one in the past and one in the future. These analyses are not connected and, therefore, they cannot be in contrast. The fact that the two episodes (selected from common criteria so as to perform a meaningful comparison) do not reflect the statistical trend is not surprising since only two episodes are selected.

Minor points:

1. *Lines 23-38: The first paragraph of the introduction could be improved. It reads a bit bumpy. Moreover, I think that you should describe the implications of PCAPs on society in more detail and introduce the relevance of your study for society, i.e. why it is important to study this phenomenon.*

   We improved and updated the Introduction following the Referee's suggestions (L 35-41).

2. *Line 36: "The qualificative ground-based,..." - can you please rewrite the sentence or put the terms ground-based, surface-base etc in italics? This would be easier to understand.*

   We used the italic style for these terms (L 50-51).

3. *Line 117: "(CMIP6, Eyring et al. (2016))" - should be: (CMIP6, Eyring et al., 2016).*

   We changed it in the revised version of manuscript (L 123).

4. *Line 189: Is "Noah Land surface model" correct? Moreover, I cannot find the reference Chen and Dudhia (2001) in your reference list!*

   We confirm that the land surface model used in WRF simulations is the Noah LSM (this is set in the namelist with *sf_surface_physics = 2*; all option of the namelist are visible in this link).
   Thanks for noticing that the reference is missing; we added it.

5. *Figure 6: Can you try to plot a violin plot or a box plot. This could improve your plots. It is not easy to grab information from these pure scattered points.*

   Plots in Fig. 6 are not scatter plots but temporal series of daily winter temperature gradient, plotted with a dot mark for each day (instead of using a line). The objective of these plots is showing the "raw" data and the trend over the century. Moreover, they are useful for Fig. 7, as they help visualizing the windows of variable length considered in this figure. A box plot (which "collects" data) would obscure the key point of the Fig. 6, which is the decaying trend, and, therefore, would be not appropriate.

6. *Figure 7: Can you please again explain in more detail: why do you see the significance of trends for larger periods, but not for the smaller ones?*

   The color of each "pixel" in Fig. 7 indicates the slope of the trend computed within a window of length equal to the value on the y-axis, centered in the year equal to the value on the

x-axis, moving along the temporal series of Fig. 6. For each window, the statistical significance has been computed using the p-value of the null hypothesis test. When the result is statistically significant at 95%, i.e. p-value < 0.05, we marked the pixel with a black cross. For the worst-case scenario, we can see that all pixels for window length = 80 up to the vertex of the triangle are marked; this is the reason of our sentence "the trends are [...] and always statistically significant with windows longer than 70 years". Only longer periods present statistically significant trends because of the high variability of the temporal series (visible in Fig. 6).

7. *Figure 8: Do your trends stay significant if you choose different periods, for example 2020-2080? The variability seems so high, that I wonder what will happen, if you change periods (for example by rolling over 20 years).*

   Like in Fig. 7, where we see the variability of the statistical significance, also in this case the statistical significance of the yearly PCAPs characteristics is variable, as the Referee correctly imagined. We made some tests for different periods with windows of 100, 80 and 60 years (while the "window" considered in Fig. 7 corresponds to the entire time series, i.e. 120 years):
   - 2000-2100 (window of 100 years): we obtain a statistically significant negative trend only for the yearly mean of PCAP dT/dz for SSP5-8.5;
   - 2000-2080 (window of 80 years): no significant trends;
   - 2020-2100 (window of 80 years): we obtain a statistically significant negative trend only for the yearly mean of PCAP dT/dz for SSP5-8.5;
   - 2020-2080 (window of 60 years): this time, we obtain a statistically significant negative trend for the yearly number of PCAP episodes for SSP5-8.5.

8. *Figure 9: It is really difficult to see the warm temperature values on the left figure. I understand, that your goal is to show that it is much warmer in the future. However, it would be great if you can additionally plot the figure by normalized values (for example between minimum (=0) and maximum (=1)? This figure could also be added to the supplementary material if you do not want to add another figure to your main paper.*

   We thank the Referee for their suggestion. We produced Figure **??** displaying the temperature rescaled between minimum and maximum values over the three areas of the Grenoble valley, for each episode. This figure clearly shows similarities and differences between the two episodes. However, following the Reviewer's comment, we think it is more useful to display the temperature profile for each episode (new Figure S9 upper row). For this reason we did not include Figure 1 in the paper.

9. *Line 391, equation (4) and line 425-427: Can you please explain why you do not expect much difference if you set z_t to 1500 m or to the inversion height (if I understood it correctly?). Maybe you can give some simple, idealized examples (for different vertical profiles) to explain your assumption?*

   The VHD is a bulk measure of stability and should therefore reflect the fact that the future episode has a stronger stability than the past one. The value of the VHD actually depends upon the upper bound of the integral defining it. It is therefore important this upper bound to coincide with the height of the cold-air pool. In the submitted version, this upper bound was well above the top of the cold-air pool and, as a result, the values of the VHD were similar for the two episodes. We now adjusted this upper bound so that it coincides with the height of the cold-air pool (see L 415-416). We found that VHD is larger for the future episode than for the past one, as expected (see new values in Table 3 and L 435-436). There is no contradiction with our statement in the submitted version where we wrote that "values of VHD are of the order of 7-8 MJ/m^2": in overall indeed VHD values for Ep1988 are equal to 7.3 MJ/m^2 and for Ep2043 equal to 8.2 MJ/m^2, namely VHD is all the higher the stability is stronger. This is also consistent with

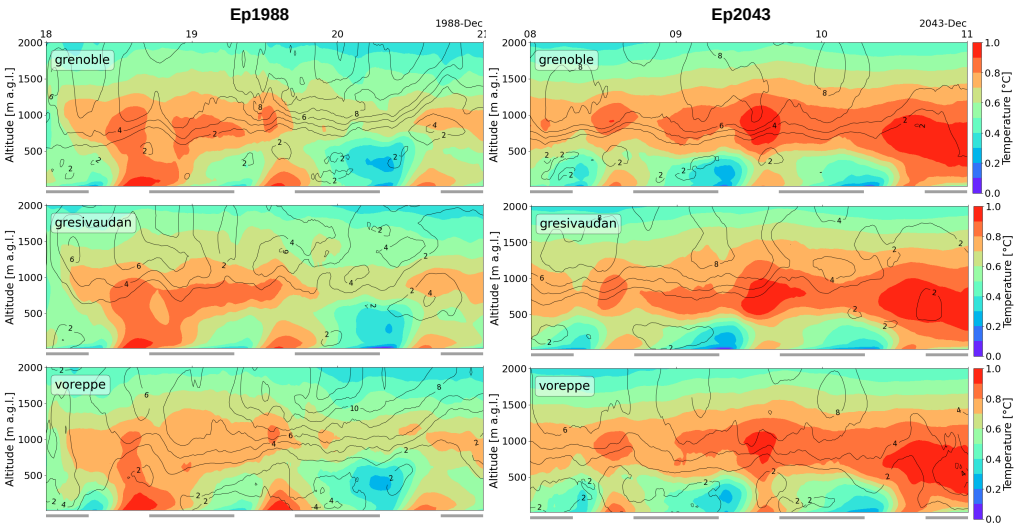

Figure 1: Normalized temperature field computed as $(T - T_{min})/(T_{max} - T_{min})$ where T_{min} and T_{max} are the minimum and maximum air temperature, respectively, among the three valleys (each episode has one T_{min} and one T_{max}).

the fact that, when the potential temperature profile is linear, the VHD is related to height of the cold-air pool by the relation $VHD = 0.5\rho c_p H_{inv} \Delta\theta$ where $\Delta\theta$ is the temperature difference across the inversion (see L 441-443).

10. *Line 409 (see also Table 3): You say that the inversion height is on average 100m lower in the future, however, this is not significant! Please clarify!*

    Yes, we agree, we now write that the two episodes have similar inversion heights (L 431-433).

11. *Line 410: "Consistent with these observations ..." - Why is this congruent? In the preceding sentence, you noted a lower inversion height, while here, you mention a similar Vertical Heat Diffusivity (VHD). Can you please provide clarification?*

    As noted above, we rewrote this part of the section dedicated to the analysis of VHD and the PCAP height.

12. *Lines 425-427: I do not understand the sentence starting with "Figure S8 [..]". Please explain.*

    Following also the comment by Referee #1, we removed these two sentences.

13. *Line 460: I think that you cannot draw the conclusion that the inversion height in the future is generally lower. Please clarify that you just looked at two example periods. Since the variability between episodes is large, in my opinion it is not possible to draw such conclusions.*

    We clarified the behavior of inversion height on L 428-433.

14. *Line 486: There is a word missing in the sentence.*

    Thanks a lot for noticing this.