

# Referee #1 comments

The authors would like to thank Referee #1 for their detailed and insightful comments, which have been helpful to improve the manuscript of the article.

Please find below their comments (in bold) with the authors' responses (AC for Author Comment).

## General comment

**The paper by Castellanos et al., entitled “Projected elevation-dependent warming in the Alps depicted with surface energy balance trends”, is a well-executed study that addresses a research topic of growing interest using robust and appropriate methodological approaches. The study investigates projected EDW in the European Alps using the MAR model, highlighting different warming patterns as a function of elevation at the surface and altitude in the free atmosphere. The results show an enhanced warming signal that progressively shifts toward higher elevations throughout the seasons, in connection with the seasonal migration of the snowline. The authors further investigate the physical processes underlying this EDW pattern, showing that changes in net shortwave radiation and in the energy available for snowmelt play a key role. One notable novelty of this work is the inclusion of an analysis of the free atmosphere in addition to surface processes. I therefore suggest that the authors consider explicitly reflecting this aspect in the title of the manuscript.**

**Overall, the paper is sound and suitable for publication; however, some major points need to be reconsidered. In particular, some sections would benefit from a more technical and specific treatment in order to strengthen the interpretation of the results and the robustness of the conclusions.**

AC : Thank you for your feedback. The new working title of the manuscript is the following : “Projected elevation-dependent warming in the Alps : contrasting free-atmosphere and surface trends with surface energy balance drivers”.

## Major comments

**- The abstract is not fully clear to me. In particular, sentences from 25 to 28: when reading the abstract on its own, it is not clear what is meant by maximum. Moreover, the definition of the EDW signal is not explicitly stated. Clarifying these aspects in the abstract would substantially improve its clarity and allow readers to better grasp the main results at first glance.**

AC : Thank you for your feedback on this issue. We are preparing a version to clarify these aspects in the abstract.

This version will replace the phrase :

“EDW signals are found to be different near the surface than in the free atmosphere, with a maximum signal in the former that is migrating to higher elevations through the seasons, linked to the snowline migration.”

by :

“EDW profiles are found to be different near the surface than in the free atmosphere. Near the surface, a maximum warming is found in spring at mid-elevations that is migrating to higher elevations in summer and autumn, a process that is not found in the free atmosphere. The elevation of the maximum warming is closely linked to the snowline migration.”

**- Lines 165–169: it would be interesting to better explain why only these three variables were selected to evaluate GCM skill. Did you analyse both means and trends? Additional information would be valuable. I do not think this analysis needs to be included in the main manuscript, but it could be very interesting in the supplementary material or Appendix, with a brief mention in the main text.**

AC : We did not consider a trend analysis in the GCM evaluation, as we deemed that trends can be right for the wrong reasons. Indeed, if we consider the nonlinearity of the climate response to greenhouse gas emissions superimposed to the internal variability of the climate, there is a chance that a model with low skill over the region can show a good trend, and vice-versa. Instead, we focus on finding GCMs with appropriate skill for reproducing the mean climatology. The assumption is that if they are able to represent processes correctly in the historic period, then they will produce more realistic future projections, an assumption made by Sobolowski et al., 2025 as well.

The choice of these three variables was made to broadly take into account relevant large-scale climate indices that finally impact the atmospheric conditions over the Alps. That is, to consider the Sea-Surface Temperature to make sure the oceanic forcing is well represented ; the temperature at 700 hPa to verify that the model simulates correctly the warming in the mid-troposphere (as a GCM can simulate surface temperature adequately while having a significant bias in the atmosphere) ; and the geopotential height at 500 hPa to make sure that the atmospheric circulation affecting the Alps is well represented.

From the temperature at 700 hPa, we further made a selection on climate sensitivity. The first criterion being that it is realistic and doesn't depart too much from the multimodel mean, the second being to select a model with a large climate sensitivity (EC-Earth3) and one with a lower climate sensitivity (MPI-ESM1-2-HR), which is in accordance with Meehl et al., 2020 (see table 2 within).

With your suggestion in mind, we are adding an Appendix section to include this explanation and a table with the GCM scores from this evaluation. Thank you for your comment !

Meehl, G. A., Senior, C. A., Eyring, V., Flato, G., Lamarque, J.-F., Stouffer, R. J., Taylor, K. E., and Schlund, M.: Context for interpreting equilibrium climate sensitivity and transient

climate response from the CMIP6 Earth system models, Science Advances, 6, eaba1981, <https://doi.org/10.1126/sciadv.aba1981>, 2020.

**- Lines 175–182: Why was this specific ensemble selected instead of considering all possible combinations of GCMs, model versions, and realizations? Including a larger number of members could provide additional insight into model variability. Or perhaps I missed some important information?**

AC : Resource and time limitations prevented us from exploring a larger ensemble or even all possible realizations of the ensemble that is used. In particular running three centennial future projections with our MAR RCM model was accessible whereas it would have been impossible for us to run a full ensemble using more GCMs as lateral conditions for our model. As mentioned in lines 590-596, we believe also that studying single experiments provides a clear story-line of the physical processes at play, which might be tricky when considering ensemble average and different models with contrasted physical processes. Anyway, further investigations based on multiple GCM-RCMs should be considered also to further dig into this research.

We are adding these details in the discussion part of the article.

**- Lines 202–217: in addition to analysing spatial biases, it would be very useful to examine the annual cycle of the three analysed variables. As stated by the authors, biases arise not only from models but also from observational limitations at high elevations. Since the main goal is to assess whether models capture the dominant climate features, including the annual cycle would strengthen the analysis.**

AC : We will include in the appendices the annual cycle of the analysed variables. Here is the first one, for temperatures :

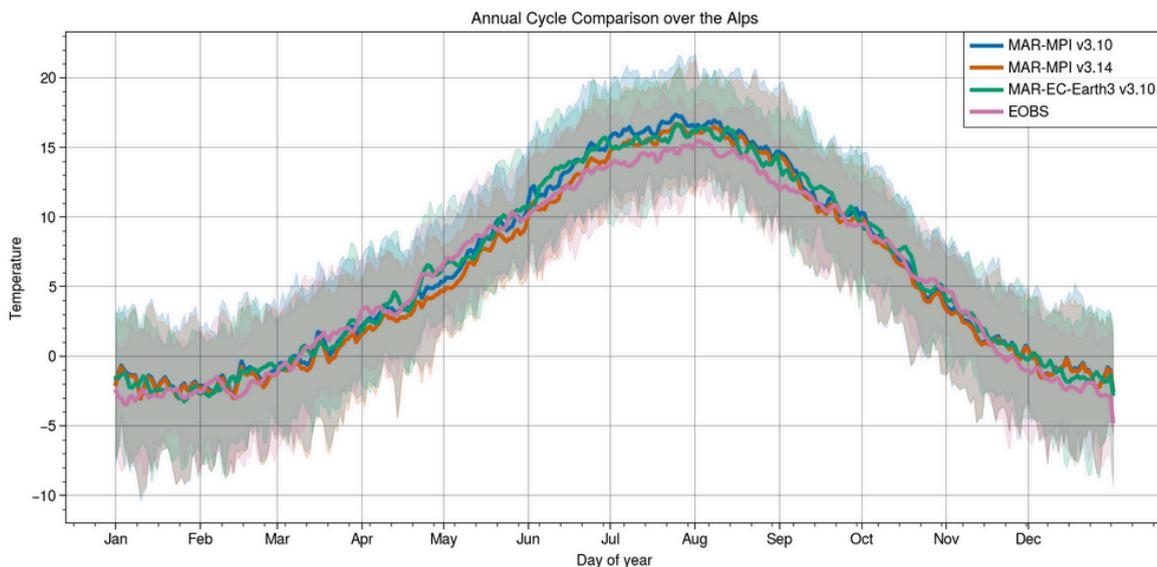


Figure C2 : Mean daily temperature at 2 m (T2m), averaged over 1961-2014 and over the Alps (coloured lines). The shaded bands represent the 10th and 90th percentile over 1961-2014.

- Lines 252–255: did the authors perform a sensitivity analysis before selecting these threshold values? Why did you adopt such a strict rule for pixel selection? I suggest including in the Appendix or in the Supplementary:

1) a map showing all selected pixels;

2) a table with the number of pixels per elevation band.

In my opinion, these elements would be very helpful for understanding the results.

AC : We tried out several threshold values to select the gridpoints belonging to the Alps in the MAR simulations. The goal was to accurately trace the contour of the Alps while excluding other mountain areas present in the simulated domain. The values presented and used in the paper were the most adequate in that regard.

Thank you for your suggestion, we agree that this would be helpful for understanding the results. The contours in Figs. 1 and 2 show all selected gridpoints. We decided to add a histogram to figure 1 showing the number of gridpoints per elevation band. Here is the updated figure 1 :

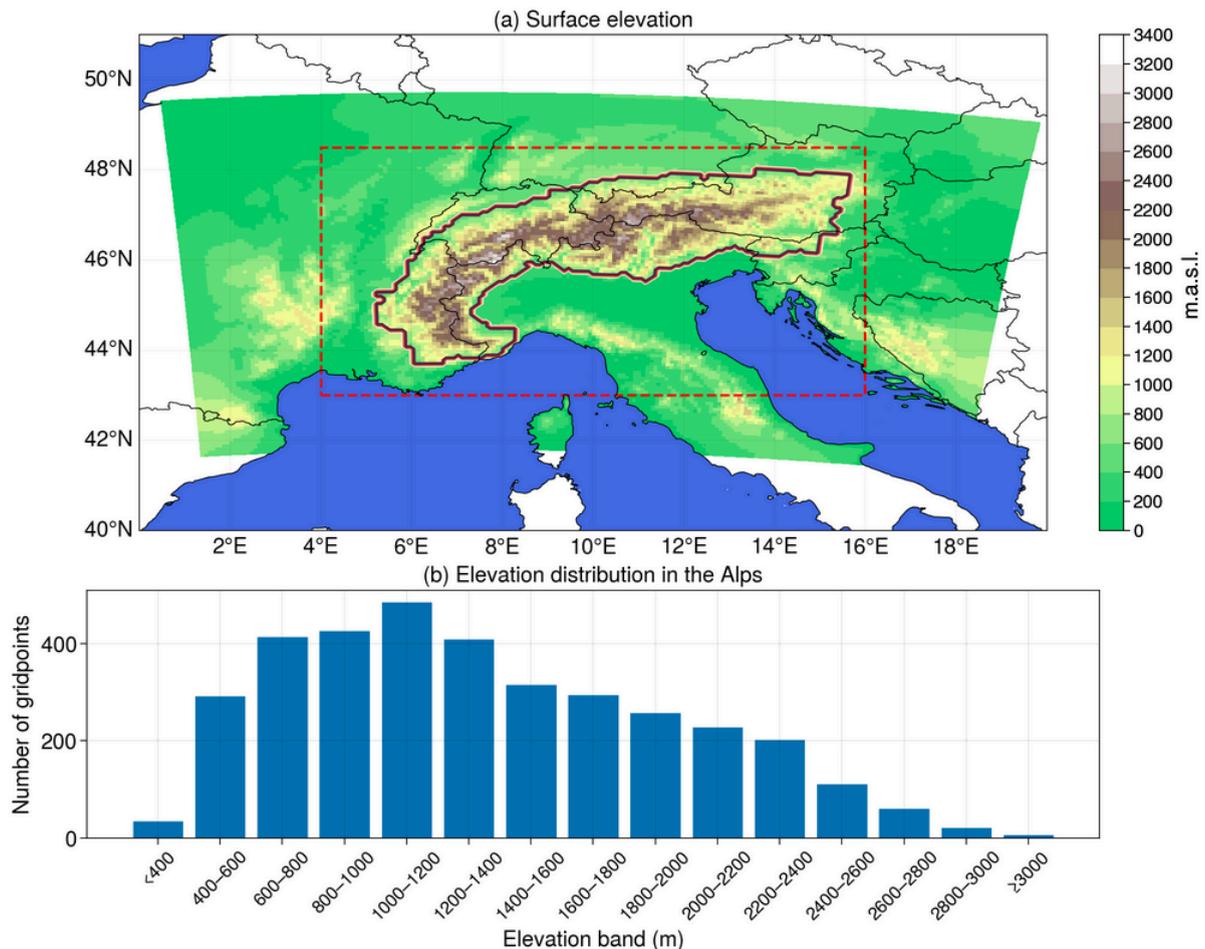


Figure 1 : (a) Domain and topography of MAR simulations. The contour defines the Alps (see Sect. II.2.2) and is used as a mask further on in this paper (e.g. whenever there is a value averaged over the Alps). The red box is the area shown in Fig. 2, corresponding to the domain considered after excluding the lateral parts affected by boundary conditions in the MAR experiments. (b) Elevation distribution in the contour, by elevation band.

There are six gridpoints above 3000 m with this topography.

**- Lines 301–304 and Figure 2: I suggest using elevation isolines instead of national borders. Since the analysis focuses on elevation, the current representation may be difficult to interpret without detailed regional knowledge.**

AC : Thank you for this suggestion ! We believed that national borders offered relevant and easy to identify landmarks. We have tried a version with isolines, and find that it complements the spatial warming patterns well and helps with the interpretation. We will rely on Figure 1 for context regarding national borders (as having the national borders in addition to the isolines made the figure 2 too crowded). Here is the new version :

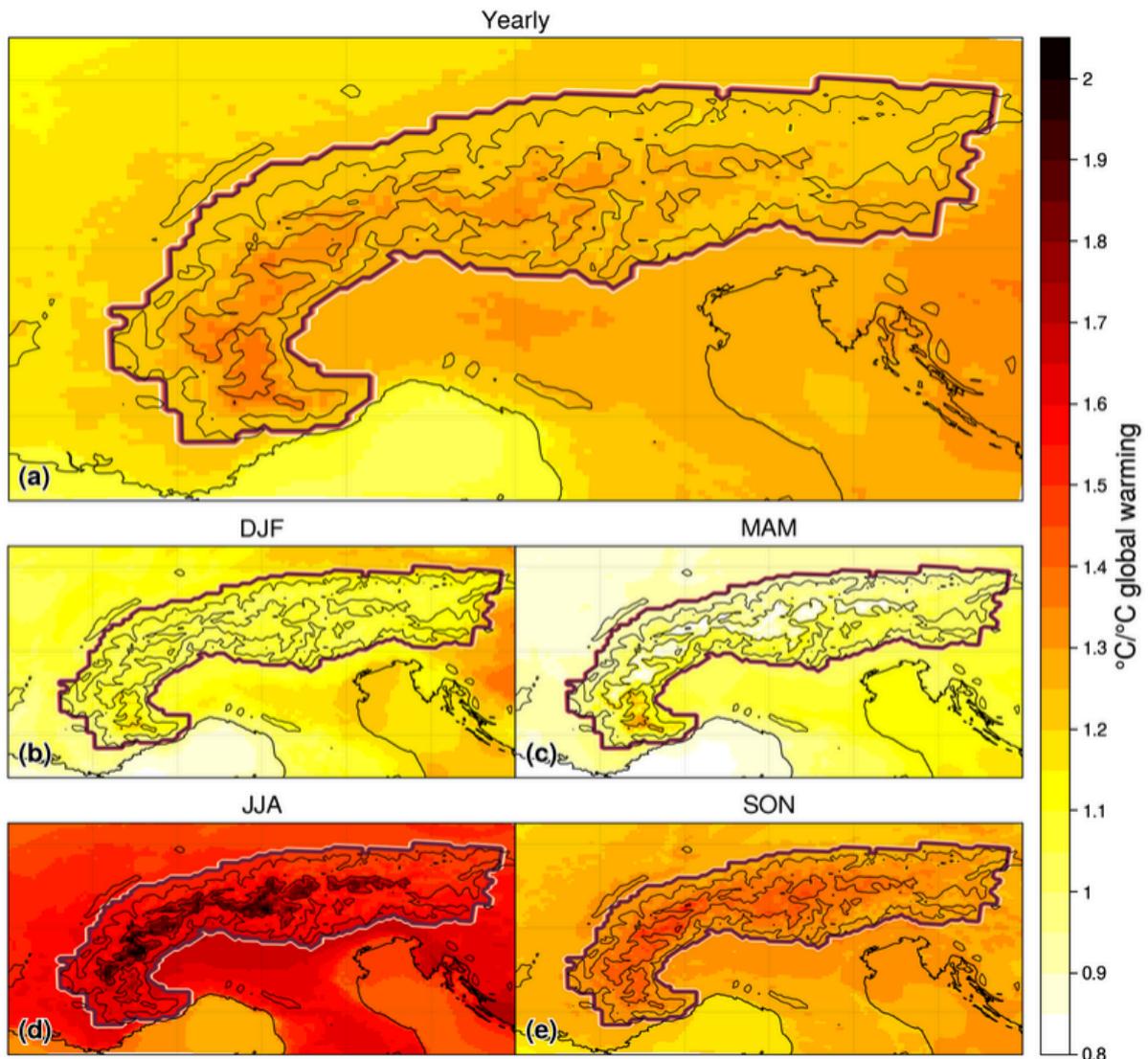


Figure 2 : (a) Yearly warming in the three v3.10 simulations scaled with respect to global warming level in the forcing GCM. (b), (c), (d), (e) : same as (a) at the seasonal timescale. See Figure 1 for the definition of the contour line. Isolines every 1000 m are shown.

**- Figure 3: it is not clear which brown line corresponds to the dashed and which to the solid representation. I suggest considering an alternative visualization (e.g., shaded**

bands instead of error bars?), which could improve readability. In addition, it may be worth evaluating whether to include daily surface temperature in this figure to provide a more complete picture, even though it is already shown in Figure 4.

AC : Thank you for your feedback on this matter. We have implemented shaded bands to improve the visualization of this figure. However, we believe that adding daily surface temperature overburdens the figure and makes it more difficult to read. Below is the new version.

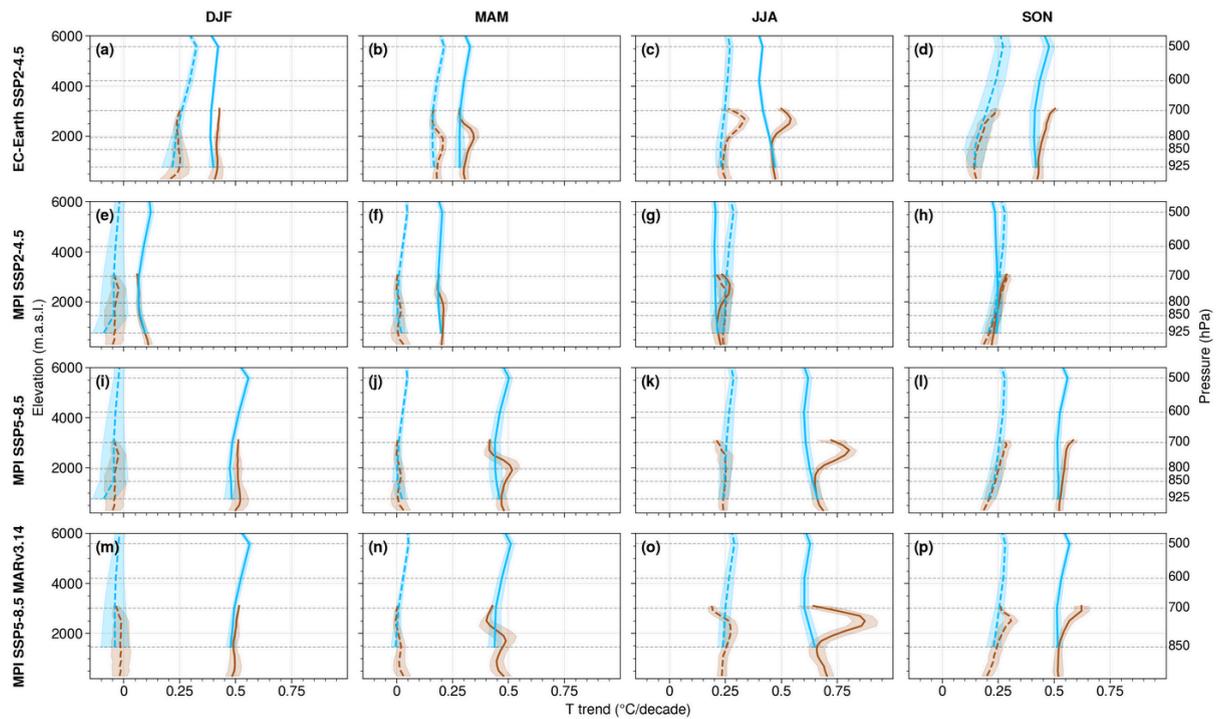


Figure 3 : Alpine temperature trends near the surface (along the slopes) averaged over 200 m-altitude bins (T2m, brown) and in the free atmosphere averaged over the available pressure levels (Tp, blue). The historical period (1961-2014) is in dashed lines, projection (2015-2100) is in full lines. Shaded bands represent the spatial standard deviation (1 sigma). The first three rows are the MAR v3.10 simulations. Columns are for each season.

- Lines 478-491: This paragraph, in my opinion, needs to be reconsidered and clarified for several reasons. First, the description of the results related to the “spatially averaged” trends is unclear to me. The manuscript refers to spatially averaged trends of the snowmelt energy flux and net shortwave radiation, but then reports maximum and minimum values without clearly specifying the dimension over which these extrema are computed. This ambiguity makes it difficult to properly interpret the magnitude and meaning of the reported trends, as well as the subsequent comparison between net shortwave radiation and snowmelt energy flux. The second comment mainly concerns the method used to infer the causes of the EDW pattern. The approach adopted to isolate the effect of the snowline by subtracting low-elevation trends from the elevation-dependent signals is conceptually reasonable and provides useful insight into the role of snow-related processes. However, this method relies on the assumption that trends at low elevations represent a warming signal that is independent of elevation and unaffected by snow-related feedbacks. As a

consequence, subtracting low-elevation trends may not fully isolate the snowline effect, but rather provide a first-order proxy of snow-related contributions. The authors are therefore encouraged to clarify the assumptions underlying this approach and to explicitly discuss its limitations. Moreover, I would be very cautious in stating that these variables are the causes of the observed changes, as many other factors are not considered (as the authors describe in the discussion). Throughout the manuscript, it would be more appropriate to describe these variables as one of the possible contributors to the observed changes.

AC : Thank you very much for this feedback. We are reworking the text to make it more explicit and clear.

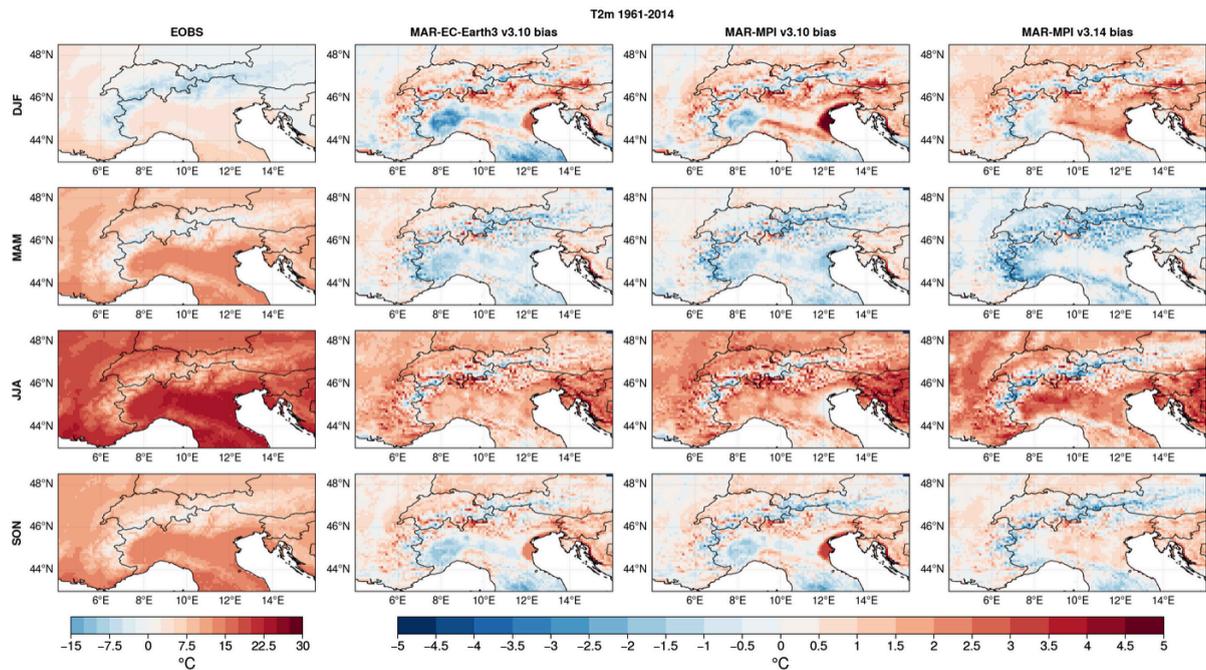
Indeed, it is more correct to say that we compare the trends at low altitudes and at the altitude of the snowline, and interpret the difference to be mainly due to snowline-related effects, and not to any snow related effect. This is slightly different to actually “isolating” the snowline, and we will change the text to reflect that.

**- Lines 598-606: In my opinion, these aspects should not be considered true limitations of the study, as the authors intentionally adopt a sort of idealized experimental framework that does not account for several additional environmental changes. These simulations appear to adequately represent the climate of the domain while deliberately focusing on a limited set of processes and changes. Such changes do have an impact; however, as the authors themselves note, further work would be required to explicitly include these processes and assess their influence. I recommend that the authors reconsider and reformulate this paragraph.**

AC : Thank you for the thoughtful analysis on what we presented as limitations of our work. We are rewriting this paragraph to present these aspects as further investigation prospects rather than limitations.

**- Figure B1: I suggest using white for the sea instead of the current color, as it may otherwise be confused with the colorbar.**

AC : This is indeed a risk, we will modify the figures to correct that. Here is one example of an updated figure, for T2m :



## Minor comments

- From the beginning of the manuscript, whenever referring to topography, it would be preferable to use “elevation” rather than “altitude”, which is more commonly used in atmospheric contexts (e.g., lines 409–410).

- Line 42: climate changes → climate change

- Line 49: Elevation-dependent warming → Elevation-Dependent Warming

- Line 98: please define the acronym ERA-20C and cite the reference  
<https://doi.org/10.1175/JCLI-D-15-0556.1>

- Line 103: instead of stronger, I would suggest strong, unless a direct comparison is being made.

- Lines 142: I would avoid breaking the sentence into a new line.

- Lines 272–276: if I understand correctly, the lowest 10% and the highest 0.3% of elevations are excluded in order to focus on maximum warming at intermediate elevations, consistent with the definition of EDW. However, this choice excludes possible monotonic elevation trends. To be precise, it should be clearly stated that this study only considers elevation-dependent trends that exhibit intermediate-elevation maxima.

- Line 282: remove the extra space before “.”.

- Line 318: remove the extra space before “.”.

- **Line 335: the temperature trend 2 meters → the temperature trend at 2 meters**
- **Line 579: remove the extra space before “:”.**
- **Line 616: remove the extra space before “:”.**
- **Figure A1: this figure needs to be cited in the manuscript.**
- **Figure B2: please specify clearly in the caption that one panel shows the relative change, while the other shows the absolute change.**

AC : Thank you for these detailed comments ! We are preparing a revised version of the manuscript which takes them into account.