Final Response to Referee #2

We appreciate the time the reviewer has invested in reading the manuscript in such a careful and thorough manner. The comments have been carefully considered and responded. Please find below our response to each comment.

General comments

1. ***An interesting study investigating the sensitivity of the glacial Alpine hydro-climate to northern hemispheric and local ice-sheet changes, using the chain of GCM-RCM simulations to perform comparison and sensitivity analysis for two glacial periods, the LGM and MIS4, with the different ice-sheet thickness (in different glacial regions) effects on the Alpine hydro-climate conditions. The results are analyzed in very much detail, although in some cases the effects are rather small and with respect to the simulation concept, I would not be so sure that the conclusions are so firm (see some further more detailed comments).***

RESPONSE:

We thank you for your detailed comments. We will take care of the concerns related to the conclusions in the following responses and in the revised manuscript.

Major comments

2. ***First, to make these more strong at least some small ensemble (a few models) should be employed to see the robustness of the results.***

RESPONSE:

We agree that an ensemble can increase the robustness of the results, especially when analysing the climate simulations separately. However, we think that this suggestion is beyond the scope of our manuscript as this study focuses on topography-related sensitive experiments rather than assessing the uncertainties of the climate simulations. Moreover, we would like to mention that each regional climate simulation implies very high computational costs, e.g. calculation time and storage and our current systems at use do not allow for further simulations. Note further that we would also need to perform global model simulations. If the reviewer suggests to use different model chains, i.e., another GCM and another RCM, then this is certainly beyond the capacity of the group as this would imply gaining knowledge and experience with these models. Even modelling centres only use and maintain one model. Our guess is that the reviewer has a community effort in mind, so that a small ensemble would rather orientate within the modelling community, e.g. CORDEX (CORDEX, 2019). Clearly, we agree that this would be beneficial, but to our knowledge regional modelling in the paleo perspective is rather new. We know only two other groups working on paleo issues and none has used convection permitting scales (except our group). Therefore, we see this study as a starting point and the robustness is given by statistical tests. We will try to be more specific that the results obtained and discussed may depend on the model chain used.

3. ***Second, the chain of the model domains is a bit strange. In my opinion, the innermost domain is too small to develop properly the circulation in the vicinity of the Alpine region and due to its location at the edge of further domain boundaries from all the three sides, the discussion of the results of simulation on the north-west and south-east sites are not equally valid. What comes from the south is more or less based on the 18 km resolution domain as with respect to the proximity to the other domains edges there is no enough room and time to properly develop in CP resolution. This might be of importance as there is a significant change of land-use in Adriatic till this border, as well as with respect to the shift of polar front under glacial conditions. I understand the limitations of these extensive and demanding simulations, however, these aspects should appear some way in the presentation and discussion of the results, with the limitations clearly declared and possible uncertainties pointed out.***

RESPONSE:

We appreciate that the reviewer brought to our attention that the model setup is still a bit unclear. We would like to mention that there is a relaxation zone that refers to the lateral areas of the domain where the WRF model is nudged or relaxed towards the larger-scale input data, i.e. the lateral boundary conditions. We used a relaxation zone of 5 grid points (10 km in the innermost domain D4) at each edge which is deleted before the analysis, i.e. the figures in manuscript do not show the relaxation zone. Without this relaxation zone in the innermost domain, there are still around 60, 80, 120 and 80 km distance (30, 40, 60 40 grid points, respectively) from the first elevated areas (e.g. above 1500 m a.s.l.) to the west, north, east and south edges of the domain, respectively. Furthermore, the domain D3 in 6 km resolution also uses a 5 grid point relaxation zone and is convection permitting. Checking the simulations we find convection-type structures emerging also from the south, which where also found in simulations with the same setup but driven by reanalysis data (Gómez-Navarro et al., 2015, 2018). Still, we agree that a wider region would be beneficial. In the revised manuscript, we will clarify this in the method section and we will also mention it in the conclusion part.

4. ***Further, concerning the discussion of the relative humidity, I would like to stress the dependence of it on the actual temperature itself as well, which can be quite significant when comparing the PD and LGM, see below in specific comments, but please check throughout the paper. The same distance between the dew point and actual temperatures under these different conditions of PD and LGM will not mean the same relative humidity.***

RESPONSE:

We fully agree that the relative humidity also depends on the air temperature which is strongly different between PD and LGM. Also, we think that the distance between the dew point and air temperature would poorly represent the relative humidity on a linear-scale diagram, not even a qualitatively representation. However, we believe that the logarithmic scale on the SkewT diagram allows to interpret the distance between the dew-point temperature and air temperature as a qualitative representation of the relative humidity. We will take care of this concern in the following responses and in the revised version the manuscript.

5. ***Moreover, concerning the mixing ratios in the diagram, that means saturated mixing ratio under the actual atmospheric conditions, which is not saying too much about the precipitable water, it depends on the relative humidity. Connection to Clausius-Clapeyron equation makes more sense in the discussion of extreme precipitation, where really different temperature of the atmosphere with different maxima of potential mixing ratios results in different amount of precipitable water. However, in the results presented the relative humidity looks to be rather lower.***

RESPONSE:

We will change the name of the mixing ratio lines (page 6 lines 178) to saturated mixing ratio lines in the revised manuscript. Additionally, we will modify the following lines (page 6 lines 185-186):

"...dew-point temperatures (dashed lines). The latter simply indicate temperatures at which the air becomes saturated and is used to deduce the mixing ratio with height, i.e., the amount of water vapour in the air where the dew point temperature line crosses the mixing ratio line."

to

"...dew-point temperatures (dashed lines). The temperatures are used to deduce the mixing ratios with height, i.e., the amount of water vapour in the air with height, whose values are obtained from the saturated mixing ratio lines when they are crossed by the temperature vertical profiles. The dew-point temperatures indicates the temperatures at which the air becomes saturated."

Regarding the precipitable water (PW), also called integrated water vapour (IWV), we agree that the PW is not directly represented by the mixing ratio at one pressure level (value of mixing ratio is obtained from the saturated mixing ratio line when it is crossed by the air temperature vertical profile). The PW represents the amount of water vapour of an atmospheric column expressed as the depth of water if that vapour were condensed, i.e. water vapour available for precipitation. The PW value is calculated by vertically integrating the water vapour mixing ratios across height and it does not depend on the temperature (Eq. 1; e.g. Rozsa, 2012; González-Rojí et al., 2018). These values are shown at the top of each panel of figure 4 and 8 in the manuscript.

$$PW = IWV = \frac{1}{g} \int_{sfc}^{top} q \, dp, \tag{1}$$

where g is the gravitational acceleration, q the vapour mixing ratios, top and sfc the pressure at the uppermost and lowermost model level, respectively. We will clarify this in the method's and result's section of the revised version of manuscript.

6. ***A formal comment concerns the rotation of the wind (appearing throughout the paper). I would recommend using the term turning of the wind, which is commonly used when describing the changes of wind direction with height, i.e. wind turns clockwise or anticlockwise. Similarly, something can cause to turn the wind to some direction, e.g. changes of ice-sheet heights, LGM conditions etc.***

RESPONSE:

We appreciate this suggestion. We have changed the term "rotation" to "turning" in the revised manuscript.

Specific comments

A1. ***Page 1, line 7: explained***

RESPONSE:

It will be changed.

A2. ***Page 4, line 126-9: Actually, this simulation strategy is not too rigorous comparing what is commonly required from RCM simulations***

RESPONSE:

We partly agree with the reviewer. A better simulation strategy would generally be when a RCM runs for 30 years in a single piece with a spin-up period longer than 2 months. However, we think that this better strategy would hardly influences our conclusions, especially when our analyses are completely based on mean values, i.e. the climatology. We would like to highlight the reason of choosing our strategy in the following.

Splitting up the simulations is explained by the time-consuming setup to run a simulation over the Alps at 2 km resolution. Namely, 3 model years are equivalent to 1 month in real time, which means that a 21-years simulation in a single piece would have taken at least 7 months in real time without any interruption. Each regional climate simulation is forced under perpetual conditions, i.e. constant climate conditions, which allows to split it into 3-years simulations that represent the same climate state. For example, a 21-years simulation is performed under perpetual 1990 conditions. To that end, the 21-years simulation is split into seven individual 3-years simulations which all represent the same 1990 climate conditions. We will briefly mention the common strategy and reformulate these lines in the revised version of the manuscript as follows:

"...simulation segments, respectively. Note that the reason of splitting is to efficiently use the available computer facilities (similar to accompanying studies such as Velasquez et al., 2020, 2021), even though regional climate simulations would commonly be performed in one single piece. For each segment, a 2-month spin-up ..." .

A3. ***Page 6, line 171: not clear how 30 annual mean samples can be selected from 21 or even 12 years simulations?***

RESPONSE:

We thank the reviewer for this comment. The number "30" is a mistake. It should not have been written and it will be deleted in the revised version of the manuscript.

A4. ***Page 6, line 188-9 the relative humidity still depends on the actual temperature as well, the same difference between the actual and dew point temperature does not imply the same relative humidity, especially under quite different temperatures like for PD and LGM***

RESPONSE:

We fully agree that relative humidity (RH) depends on the actual air temperature as well. This dependency is implicitly included in the SkewT diagram and therefore the values of RH can be qualitatively evaluated by the distance between the temperatures profiles, i.e. the distance between the dew-point temperature and air temperature. We will clarify this in the revised version of the manuscript as follows:

Lines 186-189:

"...Both temperatures are used to investigate the relative humidity, i.e., the level of saturation at a certain pressure and temperature. Note the SkewT diagram implicitly includes the dependency of the relative humidity on temperature by the logarithmic scale of the saturated mixing ratio lines. Therefore, the relative humidity can be obtained by qualitatively estimating the distance between both temperatures profiles. A short distance indicates a high relative humidity and, inversely, a large distance a low relative humidity."

A5. ***Page 8, line 230: In fact, mixing ratios are not shown, in the diagram, there are saturated mixing ratios for the given conditions provided, precipitable water depends on actual relative humidity***

RESPONSE:

We will change the name of these lines in the revised manuscript. Also, we have clarified the concern about the relative humidity in a previous answer. Please refer to response to the fifth major.

A6. ***Page 8, line 231-2: relative humidity is not the same with the same difference between the dew point temperature and actual temperature, it depends on temperature itself as well (see above)***

RESPONSE:

We have clarified this concern in a previous answer. Please refer to response to the fifth major.

A7. ***Page 8, line 244: I do see boundary layer in PD as well, of course, clearly, with less stable lapse rate due to surface warming in summer***

RESPONSE:

We agree that there is a visible boundary layer in PD as well with less stable lapse rate due to surface warming in summer. We will modify this line as follows:

"...layer under PD and LGM conditions, particularly clear during the LGM."

A8. ***Page 8, line 249: Actually, for the wind field (circulation) analysis rather larger scale (domain) should be shown with pressure fields changes, which will be probably a stronger driver of the circulation. The alpine effect can be seen just in the close proximity, where even in 700 hPa level Alps can create barrier with different height during PD and LGM (MIS4) inducing either overflow or flow around some parts, which can be well resolved in the highest resolution (in connection to stability as well)***

RESPONSE:

We would like to mention that the PD and LGM CCSM4 simulations have been analysed over Europe in a variety of studies, including additional simulations for other glacial and interglacial states (e.g. Hofer et al., 2012a,b; Merz et al., 2013, 2014b,a, 2015, 2016; Landais et al., 2016). These study particularly focused on changes in the atmospheric circulation during glacial times. Compared to PD conditions, the LGM simulation reveals a clear southward shift, a more zonal orientation of the storm track over the North Atlantic and substantial changes in the weather patterns (Hofer et al., 2012b,a). These changes are able to explain precipitation anomalies over Europe, especially over the Iberian Peninsula and the western part of the Mediterranean Sea.

Nevertheless, we will include a brief analysis of the larger-scale sea level pressure and wind fields at 700 hPa in the revised version of the manuscript to further examine the drivers of the changes in the Alpine Hydro-climate.

A9. ***Page 9, line 279: Actually, I do not see so much significant difference (despite formal statistical significance) to discuss here except the cases of the proximity of Alpine ridge, where the differences can be due to different heights of the terrain (glaciers), as mentioned above***

RESPONSE:

The reviewer refers to Fig 5. There, the Alpine ice sheet is not changed so the changes are not due to topographic differences. We agree that the wind vectors are not suggesting a similar strong changes in Fig 5d than e.g. Fig 5a. Still, the shading shows the statistical significance of the changes. Maybe a problem is the colours of the wind vectors so we will try to redesign the figure.

A10. ***Page 10. Line 319: it is rather tiny***

RESPONSE:

As answered in previous response, we will face this concern in the analysis of the larger-scale of wind fields at 700 hPa in the revised version of the manuscript.

A11. ***Page 10, line 321: This will be really negligible, especially with respect to the relative humidity, which will be rather low. By the way, again, the green dashed lines represent saturated mixing ratios, not actual mixing ratios.***

RESPONSE:

As in response to fifth major comment, we would like to mention that a mixing ratio is obtained from the saturated mixing ratio line when it is crossed by the temperature vertical profile. Regarding the definition of precipitable water, please refer to the same response (fifth major comment). We will reformulate this line in the revised version of the manuscript as follows:

"...atmosphere. The latter results in a small increase of moisture availability in the middle-to-low atmosphere (dashed green lines crossed by the air temperature profile), especially in the central-southern region (site B; Fig. 8c) where there is more precipitable water (PW values at the top of Fig. 8a and c).

A12. ***Page 10, line 325: As above, saturated mixing ratios***

RESPONSE:

The name will be changed in the revised manuscript.

A13. ***Page 10, line 328: glacial climate conditions in Alpine region***

RESPONSE:

We thank the reviewer. This will be changed in the revised version of the manuscript.

A14. ***Page 11, line 335-6: Again, the changes are really very tiny, despite statistical significance***

RESPONSE:

We agree that the differences are visually tiny. As mentioned in the response to the minor comment A9, we will face this concern in the analysis of the larger-scale of wind fields at 700 hPa in the revised version of the manuscript.

A15. ***Page 11, line 354: Actually, it is difficult to see the significance in such a small region***

RESPONSE:

In the revised version of the manuscript, we will change the visualisation of the significance to another drawing pattern.

A16. ***Page 11, line 355-7: Actually, direct westerly inflow is more or less perpendicular to the barrier of the Alpine ridge in the western part, while in the eastern part it is rather parallel.***

RESPONSE:

We agree with the reviewer. In these lines, we referred the precipitation changes to the modification of ice-sheet thickness rather than the wind changes. To clarify this, we will add more information to these lines in the revised version of the manuscript as follows:

"...Interestingly, the reduction in precipitation is higher in the western part than in the central to eastern part of the Alps, although the Alpine ice-sheet thickness is reduced more strongly in the east than in the west (Fig. 2c). This cannot directly be explained by the changes in the wind field around the Alps at 700 hPa, which are caused by the reduction of the Alpine ice-sheet thickness. The reason is that we would expected wetter conditions in the western part since the winds slightly turn clockwise (more perpendicular to the Alps) and slightly stronger."

A17. ***Page 12, line 371-2: Actually, despite the formal statistical significance I do not see so big changes except in the close proximity of the mountain ridge (as noticed above), where the direct interaction of the changed top of the barrier is evident and causing direct changes in flow patterns on that level.***

RESPONSE:

We agree with the reviewer. We will reformulate these lines in the revised manuscript as follows:

"...The $LGM_{ALPSLESS}$ winds are a bit stronger and slightly turn clockwise compared to LGM_{LGM}. The slight turning is associated with significant changes in V at the northern face of the Alps, in both wind components over the Alpine axis and in U in the south of the Alps (Fig. 11a)."

A18. ***Page 12, line 374-7: However, a strong issue is how the model represents the transfer of precipitation from the place of creation downstream with the flow to the place it is considered as reaching the surface.***

RESPONSE:

Our guess is that the reviewer suggests to perform a back trajectory analysis to identify where the precipitation is coming from, e.g, North Atlantic, land surface over Europe (water recycling) and the Mediterranean. Clearly, this would be a interesting question, but we think that this is beyond the scope of this study but certainly a next step to be mentioned at the end of this manuscript.

A19. ***Page 13, line 405-6: This would be nice shown on the analysis of convective precipitation***

RESPONSE:

We would like to mention that this analysis is not possible as our simulations are performed using convection-permitting in the two innermost domains (6 and 2 km). This does not allow to separate the convective precipitation from the total precipitation since it is directly resolved in the model. CAPE is another quantitative measure that refers to convection activity. CAPE values are shown at the top of each panel on figure 4 and 8, when they are greater than zero.

A20. ***Page 13, line 413-5: The differences are again hardly visible, difficult to expect any effect on foehn, and thus to resolve if pure dynamical or thermodynamical influence***

RESPONSE:

We partly agree that the interpretation might be a bit superficial. We will weaken the statements a bit and formulate the foehn connection as a speculation in the revised version. Still, we would like to emphasise that the wind changes are statistical significant (Please refer to the minor comment A9). To clarify this statement, we will reformulate these lines in the revised version of the manuscript as follows:

"...The drier conditions at the southern face of the Alps may be explained by an enhanced Foehn effect due to the slightly clockwise turned winds (statistical significant). Thus, dynamical processes could also play a role to explain the summer precipitation changes between MIS4 and LGM."

A21. ***Page 27, Fig. 4 and further: order of the periods in legend might be the same as for winds columns. In the caption missing explanation of the profile lines (temperature solid, dew point dashed) and correctly it should be ... saturated mixing ratio increases ...***

RESPONSE:

In the revised version of the manuscript, we will change the order of the periods according to the wind profiles. Also, we will add the explanation of the profile lines and change "mixing ratio" to "saturated mixing ratio".

Once again, we would like to thank the referee for the time invested in reviewing our manuscript so carefully. We look forward to meeting her/his expectations.

Best regards,

Patricio Velasquez (on behalf of the author team)

References

CORDEX: CORDEX – Coordinated Regional Climate Downscaling Experiment, URL https://www.cordex.org/, 2019.

Gómez-Navarro, J. J., Bothe, O., Wagner, S., Zorita, E., Werner, J. P., Luterbacher, J., Raible, C. C., and Montávez, J. P.: A regional climate palaeosimulation for Europe in the period 1500–1990 – Part 2: shortcomings and strengths of models and reconstructions, Climate of the Past, 11, 1077–1095, https://doi.org/10.5194/cp-11-1077-2015, 2015.

Gómez-Navarro, J. J., Raible, C. C., Bozhinova, D., Martius, O., García Valero, J. A., and Montávez, J. P.: A new region-aware bias-correction method for simulated precipitation in areas of complex orography, Geoscientific Model Development, 11, 2231–2247, https://doi.org/10.5194/gmd-11-2231-2018, 2018.

González-Rojí, S. J., Sáenz, J., Ibarra-Berastegi, G., and Díaz de Argandoña, J.: Moisture balance over the Iberian Peninsula according to a regional climate model: the impact of 3DVAR data assimilation, Journal of Geophysical Research: Atmospheres, 123, 708–729, https://doi.org/10.1002/2017JD027511, 2018.

Hofer, D., Raible, C. C., Dehnert, A., and Kuhlemann, J.: The impact of different glacial boundary conditions on atmospheric dynamics and precipitation in the North Atlantic region, Climate of the Past, 8, 935–949, https://doi.org/10.5194/cp-8-935-2012, 2012a.

Hofer, D., Raible, C. C., Merz, N., Dehnert, A., and Kuhlemann, J.: Simulated winter circulation types in the North Atlantic and European region for preindustrial and glacial conditions: glacial circulation types, Geophysical Research Letters, 39, L15 805, https://doi.org/10.1029/2012GL052296, 2012b.

Landais, A., Masson-Delmotte, V., Capron, E., Langebroek, P. M., Bakker, P., Stone, E. J., Merz, N., Raible, C. C., Fischer, H., Orsi, A., Prié, F., Vinther, B., and Dahl-Jensen, D.: How warm was Greenland during the last interglacial period?, Climate of the Past, 12, 1933–1948, https://doi.org/10.5194/cp-12-1933-2016, 2016.

Merz, N., Raible, C. C., Fischer, H., Varma, V., Prange, M., and Stocker, T. F.: Greenland accumulation and its connection to the large-scale atmospheric circulation in ERA-Interim and paleoclimate simulations, Climate of the Past, 9, 2433–2450, https://doi.org/10.5194/cp-9-2433-2013, 2013.

Merz, N., Born, A., Raible, C. C., Fischer, H., and Stocker, T. F.: Dependence of Eemian Greenland temperature reconstructions on the ice sheet topography, Climate of the Past, 10, 1221–1238, https://doi.org/10.5194/cp-10-1221-2014, 2014a.

Merz, N., Gfeller, G., Born, A., Raible, C. C., Stocker, T. F., and Fischer, H.: Influence of ice sheet topography on Greenland precipitation during the Eemian interglacial, Journal of Geophysical Research: Atmospheres, 119, 10,749–10,768, https://doi.org/10.1002/2014JD021940, 2014b.

Merz, N., Raible, C. C., and Woollings, T.: North Atlantic eddy-driven jet in interglacial and glacial winter climates, Journal of Climate, 28, 3977–3997, https://doi.org/10.1175/JCLI-D-14-00525.1, 2015.

Merz, N., Born, A., Raible, C. C., and Stocker, T. F.: Warm Greenland during the last interglacial: the role of regional changes in sea ice cover, Climate of the Past, 12, 2011–2031, https://doi.org/10.5194/cp-12-2011-2016, 2016.

Rozsa, S.: Estimation of integrated water vapour from GPS observations using local models in Hungary, 136, 823, https://doi.org/10.1007/978-3-642-20338-1$_1$03, 2012.

Velasquez, P., Messmer, M., and Raible, C. C.: A new bias-correction method for precipitation over complex terrain suitable for different climate states: a case study using WRF (version 3.8.1), Geoscientific Model Development, 13, 5007–5027, https://doi.org/10.5194/gmd-13-5007-2020, 2020.

Velasquez, P., Kaplan, J. O., Messmer, M., Ludwig, P., and Raible, C. C.: The role of land cover in the climate of glacial Europe, Climate of the Past, 17, 1161–1180, https://doi.org/10.5194/cp-17-1161-2021, 2021.