Final Response to Referee #1

We thank the referee Maria Fernanda Sanchez Goñi for the careful and thorough reading of our manuscript. The comments have been carefully considered and responded. Please find below our response to each comment.

General comments

1. ***The manuscript submitted by Velasquez et al. to the Climate of the Past presents a combination of global and regional model simulations to test the sensitivity of the glacial Alpine hydro-climate to northern hemisphere, Laurentidae and Fennoscandian, and local ice-sheet changes during the Last Glacial Maximum (LGM) and Marine Isotope Stage (MIS) 4. For the LGM, they find that thickening of the northern hemisphere ice-sheets, mainly the Laurentidae ice caps, and local ice-sheet topography generally lead to increase in winter precipitation and decrease in summer rainfall, both enhancing glacial conditions. In winter, dynamics processes related to the intensity and position of the Alpine winds explain the moistening in the southern part of the Alps while the simulated summer drying all over the Alps is related to thermodynamic processes, i.e. colder temperatures. In contrast, Fennoscandian ice-sheet changes have a negligible impact on the Alpine hydro-climate. For MIS 4, marked by lower global ice volume than the LGM, Velasquez et al. find wetter climate in the Alps attributed to thermodynamic processes, i.e. warmer temperatures. This manuscript is clearly written and convincing for the LGM. In contrast, I have several caveats related to the MIS 4 model results and comparison with the regional (western European) climate at that time when compared to that of the LGM (see below). Overall, this work deserves publication in CP after the authors address the comments that I have listed below. I am not a modelling expert, and I will only comment on the data discussed in this work.***

RESPONSE:

We thank you for your detailed comments. We have taken care of the concerns related to analysis of the MIS4 climate and the comparison with the LGM in the following responses and in the revised manuscript.

Major comments

2. ***Lines 40-50: It would be relevant to cite the paper by Harrison and Digerfeldt (1993, Quaternary Science Reviews), one of the first paper showing that southern Europe was wet during the LGM (centered at 21 ka) based on the high water levels recorded in several lakes around the Mediterranean region. Iberian margin pollen records also provide evidence that the LGM in southern Europe was wetter than the Heinrich Stadial (HS) 2 and HS 1 bracketing it (Naughton et al., 2007, Marine Micropaleontology ; Turon et al., 2003, Quaternary Research).***

RESPONSE:

We agree that some pollen-based reconstructions show a wetter southern Europe compared to present day. Therefore, we reformulated these lines (lines 40-50) and added this information in the revised version of the manuscript. Additionally, we included other studies that highlight the wetter conditions in southern Europe compared to other periods, e.g. Heinrich Stadials and Marine Isotope Stages. The changes have been done as follows:

"...The same data are also used to reconstruct the hydro-climatic response over Europe at the LGM mainly showing drier conditions over Europe (reduction in precipitation of around 200 mm year^{-1}) compared to PD (Wu et al., 2007; Bartlein et al., 2011). Roucoux et al. (2005) indicated that the LGM was not necessarily a dry period everywhere in Europe. For instance, Turon et al. (2003); Roucoux et al. (2005); Naughton et al. (2007) and Ludwig et al. (2018) suggested that southern Europe was wetter compared to the rest of Europe and also to adjacent periods, i.e. the Marine Isotope Stage 3 (Voelker et al., 1997; van Kreveld et al., 2000) and the Heinrich events 1 and 2 (Sanchez Goñi and Harrison, 2010; Álvarez-Solas et al., 2011; Stanford et al., 2011). Compared to PD, many studies suggest that the wetter conditions in southern Europe can be explained by a southward shift in the North Atlantic storm track during the LGM (e.g. Hofer et al., 2012a; Luetscher et al., 2015; Merz et al., 2015; Ludwig et al., 2016; Wang et al., 2018; Raible et al., 2020; Lofverstrom, 2020). This southward shift is in line with other climate reconstructions that suggest circulation-induced changes in the moisture transport. For instance, Harrison and Digerfeldt (1993) found very different patterns of lake-level changes across Europe suggesting major changes in European atmospheric circulation patterns. Other climate reconstructions also suggest circulation-induced changes in the moisture transport (Florineth and Schlüchter, 2000). In this case, the atmospheric circulation is..."

3. ***Lines 50-54 and lines 407-415: To support the idea that MIS 4 was warmer and wetter than the LGM, Velasquez et al. only refer to global studies (Eggleston et al., 2016), Australasian records (De Deckker et al., 2019 ; Newham et al., 2017) and model simulations for the North Atlantic and Greenland climate (Hofer et al., 2012; Merz et al. papers) that cannot be used to account for the climate in Europe at that time and, particularly, at 65 ka, the date chosen for their simulations. This date is concomitant with the maximum of global ice volume during MIS 4 (Waelbroeck et al., 2002, Quaternary Science Reviews), coincides, within the chronological uncertainties, with Greenland Interstadial 18 and precedes the massive iceberg discharges in the North Atlantic leading to the HS 6, 64-60 ka (Sanchez Goñi et al., 2013, Nature Geoscience, Figure S3 of the supplementary information).***

To realistically compared the recorded and simulated climate in Europe at 65 ka, the authors should discussed their wind field and climate reconstructions in the context of the climate prevailing in the western European margin and the adjacent landmasses during this period, climate that is mainly controled by the westerlies during winter. The work by Sanchez Goñi et al. (2013, Nature Geoscience, Figure 2 and Figure S3 of the supplementary information) zooms in on MIS 4, and shows relatively wet and warm atmospheric conditions at 65 ka, based on the increase of heathlands and pine forest, contemporaneous with foraminifera-based warm summer sea surface temperatures in the western European margin, reaching 15°C in the Bay of Biscay and the SW Iberian margin and 10°C in the NW Iberian margin. However and in contrast with the authors' idea that the LGM was colder than MIS 4 in the European margin, higher sea surface temperatures in the Bay of Biscay (Sanchez Goñi, 2020, Evolutionary Human Sciences, Figure 2) and in NW and SW Iberia (Sanchez Goñi et al., 2008, Quaternary Science Reviews, Figures 3 and 4) are recorded during the LGM compared to MIS 4. Both periods are characterised by low and similar temperate forest abundance and similar heathlands development suggesting that MIS 4 was not warmer and wetter compared to the LGM.

RESPONSE:

We agree that the description of MIS4 is too short and the citations do not focus on the European climate. We thank the referee for highlighting these studies. In the revised manuscript, we further introduced the MIS4 climate and briefly discussed its uncertainties. Therefore, we included the studies of Sánchez Goñi et al. (2008) and Sánchez Goñi (2020) and we have also reformulated these lines and added more information as follows:

Lines 50-54:

"... The MIS4 climate is less understood compared to the LGM as proxy data availability is further reduced compared to the LGM. Available paleoclimate reconstructions characterise MIS4 to be warmer than the LGM on a global scale (e.g. Eggleston et al., 2016; Newnham et al., 2017; De Deckker et al., 2019) with a global sea level drop of roughly 80 m compared to PD (e.g. Cutler et al., 2003; Siddall et al., 2008, 2010; De Deckker et al., 2019). Focusing on Europe, MIS4 shows relatively wet and warmer conditions at 65 ka compared to the period 85-50 ka (Sánchez Goñi et al., 2013). Still, Sánchez Goñi et al. (2008) and Sánchez Goñi (2020) found drier and colder conditions around the Iberian Peninsula during MIS4 compared to the LGM. Their findings suggest, similar to the LGM period, that the MIS4 climate was not necessarily homogeneously wetter and warmer across Europe."

Lines 407-415:

"...The $MIS4_{LGM}$ climate shows enhanced winter precipitation compared to the LGM. The reason is that the MIS4 climate state is generally warmer (Hofer et al., 2012a,b; Merz et al., 2013, 2014a,b, 2015, 2016) and thus more moisture is globally available. Wind changes do not contribute to these wetter conditions in the Alpine region as they become weaker and therefore reduce the orographically forced uplifts, which also suggests an overall reduction of the moisture transport to the Alps. Thus, we interpret the winter changes between MIS4 and LGM to be purely thermodynamically driven (Clausius–Clapeyron equation). In summer, $MIS4_{LGM}$ shows slightly wetter conditions at the northern face and drier conditions at the southern side of the Alps. The northern wetter conditions are induced by an increase in the tropospheric vertical wind shear enhancing convection processes. 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 (statistically significant). Thus, dynamical processes could also play a role to explain the summer precipitation changes between MIS4 and LGM."

4. ***Line 395-406: The authors should add in the revised version of the manuscript the new evidence from a cryogenic carbonate record in the Alps (Spötl et al., 2021, Nature Comm.) showing heavy snowfall during autumn and early winter during the LGM. These results combined with thermal modelling, provide compelling evidence that the LGM glacier advance in the Alps was fuelled by intensive snowfall late in the year, likely sourced from the Mediterranean Sea.***

RESPONSE:

We appreciate the referee to bring to our attention this study. We have included this new evidence in the conclusions of the revised version.

5. ***Lines 436-440: The authors should delete the references of Finlayson et al., 2004, 2006 and 2008 and that of Burke et al., 2014 and Baena Preysler et al., 2019. These works do not refer to the Alpine***

regions and, therefore, they are not relevant for this study.

RESPONSE:

We have deleted these references in the revised version of the manuscript.

Minor comments

m1. ***Line 72: Please add Regional Climate Models to explain RCM.***

RESPONSE:

We have added it in the revised version of the manuscript.

m2. ***Line 158: Please replace « «eighth experiment » with « eighth experiments ».***

RESPONSE:

We cannot understand your suggestion as we refer to a single experiment in this sentence, i.e. the experiment number eight. To clarify any misunderstanding, we have reformulated this line in the revised manuscript as follows:

"...In the experiment number eight, we investigate..."

m3. ***Line 255: Please replace « associated to » with « associated with ».***

RESPONSE:

We have replaced it in the revised version of the manuscript.

We would like to thank the referee Maria Fernanda Sanchez Goñi for the time invested in reviewing the manuscript so carefully. We are looking forward to meeting the referee's expectations.

Best regards,

Patricio Velasquez (on behalf of the author team)

Final Response to Referee #2

We appreciate the time the referee 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 have taken 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 causes 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 referee 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 referee 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 paleoclimate perspective is rather new. We know only two other groups working on paleoclimate 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 have been more specific in the conclusions that the results may depend on the model chain used. Thus, we added the following line at the end of the conclusions:

"Additionally, since the results of this study may depend on the chosen global and regional climate model, future modelling efforts are needed to perform more regional paleoclimate simulations, especially using different models to develop a model ensemble. This ensemble would allow to better assess the uncertainties of the simulated glacial climates."

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 referee 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 discarded from 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 and 40 grid points, respectively) from the first elevated areas (e.g. above 1500 m a.s.l.) to the western, northern, eastern and southern edges of the domain, respectively. Furthermore, the domain D3 in 6 km resolution also uses a 5 grid point relaxation zone and is already 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 have clarified this in the method section and we will also mention it in the conclusion part as follows:

Model section: Page 4 , line 124

"...two domains. Also, we use a relaxation zone of five grid points (e.g. this means 10 km in the innermost domain) at each edge in each domain, which is not included in the analysis. WRF uses..."

Conclusion: Last paragraph

"Still, using a larger innermost domain would be beneficial in a future work to enhance the model-related development of the atmospheric circulation around the Alps."

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 linearly scaled diagram, not even as a qualitative representation. However, we believe that the logarithmic scale of the Skew–T diagram allows to interpret the distance between the dew-point temperature and air temperature as a qualitative representation of the relative humidity. We have taken care of this concern in the following responses and in the revised version of 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 have changed the name of the mixing ratio lines (page 6 lines 178) to saturated mixing ratio lines in the revised manuscript. Additionally, we have modified 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 air 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 indicate 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. Note that the value of the 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.

$$PW = IWV = \frac{1}{g} \int_{p_{sfc}}^{p_{top}} q \, dp, \qquad (1)$$

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

To clarify the interpretation of PW, we have added more information on page 6 in line 193 as follows:

"...energy (CAPE). 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 (e.g. Rozsa, 2012; González-Rojí et al., 2018). Cape quantitatively..."

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 has been 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 referee. A better simulation strategy would generally be when an RCM runs for 30 years in a single piece with a spin-up period longer than 2 months. However, we think that this single simulation strategy hardly influences our conclusions, especially as our analyses are completely based on mean values, i.e. the climatology. We would like to highlight the reason for 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, three model years are equivalent to one month in real time, which means that a 21-years simulation in a single piece would have taken at least seven 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 have briefly mentioned the common strategy and reformulated these lines in the revised version of the manuscript as follows:

"...simulation segments, respectively. Note that we split the simulations 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 simulation. For each segment, a 2-month..."

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 referee for this comment. The number "30" is a mistake. It has been 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 Skew–T diagram and therefore the values of RH can be qualitatively evaluated by the distance between the temperature profiles, i.e. the distance between the dew-point temperature and air temperature. We have clarified 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 Skew–T 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 temperature 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 have changed the name of these lines in the revised manuscript. Also, we clarified the concern about the relative humidity and precipitable water in a previous answer. Please refer to our response to the fifth major comment.

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 clarified this concern in a previous answer. Please refer to our response to the fifth major point.

A7. ***Page 8, line 244: I do see boundary layer in PD as well, of course, clearly, with a 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 have modified this line as follows:

"...layer under PD and LGM conditions, particularly distinct 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 studies particularly focused on changes in the atmospheric circulation during glacial times. Compared to PD conditions, the LGM simulation reveals a clear southward shift and 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.

We have analysed the larger-scale atmospheric circulation in our simulations using wind vectors at 700 hPa for the second domain (over Europe at 18 km resolution). Compared to PD, winter winds are turned anticlockwise (more zonal) and generally intensified during the LGM, which is mainly attributed to significant changes in both wind components (Fig. 1a). We also observe that the winter jet stream is shifted southward during the LGM. In summer, winds overall show a north-south turning pattern with the axis at around 48 °N during the LGM (Fig. 1c): anticlockwise and clockwise turning in the northern and southern part of the domain, respectively. This north-south turning pattern is mostly associated to either significant changes in the zonal component (U) only or both wind components (i.e. areas in blue or red, respectively; Fig. 1c). Compared to the LGM, $MIS4_{LGM}$ winds at 700 hPa become weaker with an almost absent turning in winter (Fig. 1b), whereas they are slightly stronger with a minor clockwise turning in summer (Fig.

1d). Furthermore, we observe that winds at 700 hPa are generally intensified and turned anticlockwise with increasing the ice-sheet thickness (Fig. 2). Namely, increasing the thickness of the northern-hemispheric ice sheet leads to generally more zonal winter winds in the western part of the domain, which suggests that the flow has a warmer source and therefore more water vapour available. This would confirm the wetter conditions in winter (comparing Fig. 5b and c of the revised manuscript). Note that winds mainly show very weak differences for the sensitivity of the FIS over Europe (therefore not shown). Same results were already found in the previous studies over Europe (above mentioned). This indicates that there is a strong correlation of the larger-scale atmospheric circulation between the driving CCSM4 and the WRF simulations. Therefore, we believe that including a in-detail analysis would be a repetition of these previous studies.

Nevertheless, we have included a brief analysis of the larger-scale atmospheric circulation in section 4.1 (sixth paragraph) of the revised version of the manuscript as follows:

"...To gain further insights in the advection of moisture, we first focus on the larger-scale mid-atmospheric circulation for the second domain (over Europe at 18 km resolution). The comparison to PD conditions shows that the climatological mid-atmospheric mean flow turns anticlockwise with stronger wind speeds, which indicates a more zonal, intensified and southward shifted winter jet stream during the LGM. Same results were already found in previous studies over Europe, which analysed the CCSM4 simulations that drive our WRF simulations (Hofer et al., 2012a,b). This indicates that there is a strong correlation of the larger-atmospheric circulation between the driving CCSM4 and the WRF simulations. Note that these CCSM4 simulations and their underlying atmospheric circulation have been already analysed over Europe in a variety of studies (e.g. Hofer et al., 2012a,b; Merz et al., 2013, 2014b,a, 2015, 2016; Landais et al., 2016); therefore please refer to these studies for a in-detail analysis of the European atmospheric circulation. Secondly, we exhibit the wind vectors at 700..."

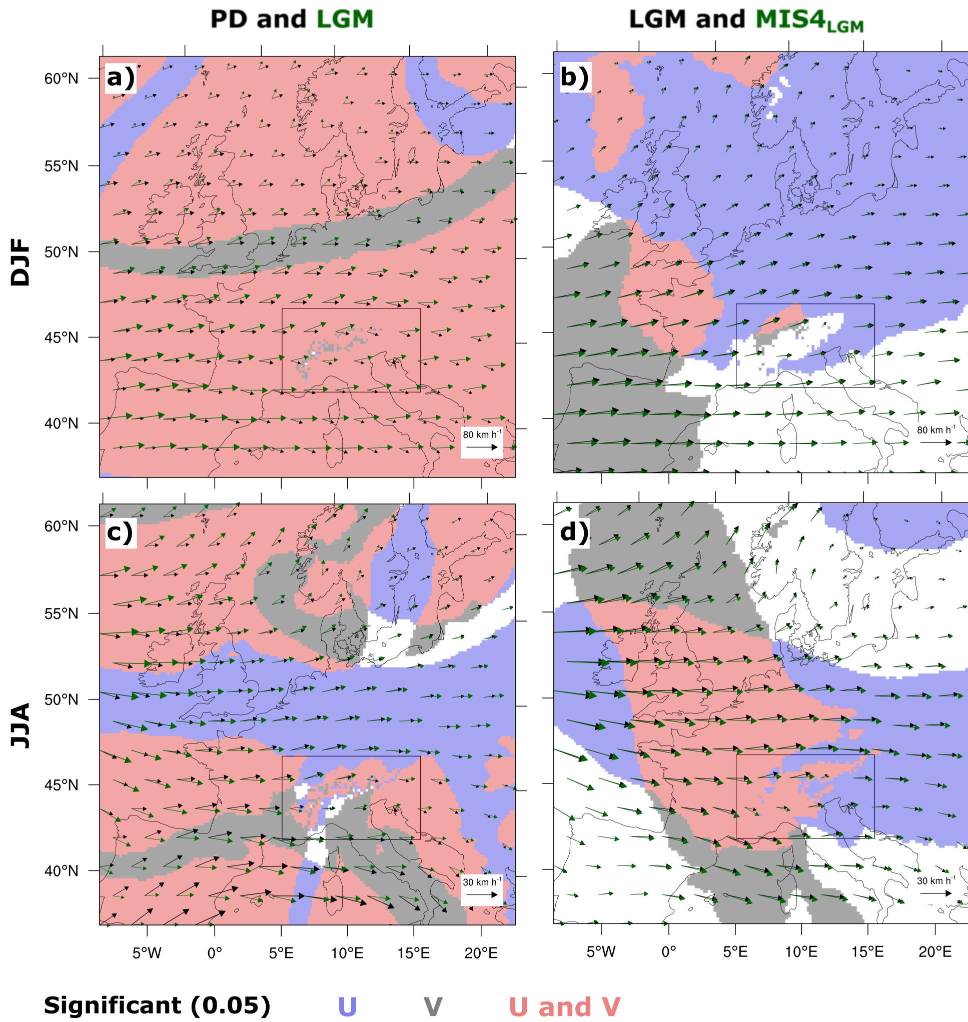

Figure 1: Climatological mean wind vectors over Europe for (a and b) DJF and (c and d) JJA: (a and c) black and green wind vectors correspond to PD and LGM, respectively, (b and d) black and green wind vectors correspond to LGM and MIS4, respectively. Red shading illustrates statistically significant differences in zonal (U) and meridional (V) wind components with a significance level of 0.05 (two-tailed bootstrapping technique), blue and grey shading indicate significance either in the U or V wind component, respectively. Inner box represents the innermost domain (at 2 km), i.e. the Alpine region. Please note that the reference wind vectors differ for DJF and JJA.

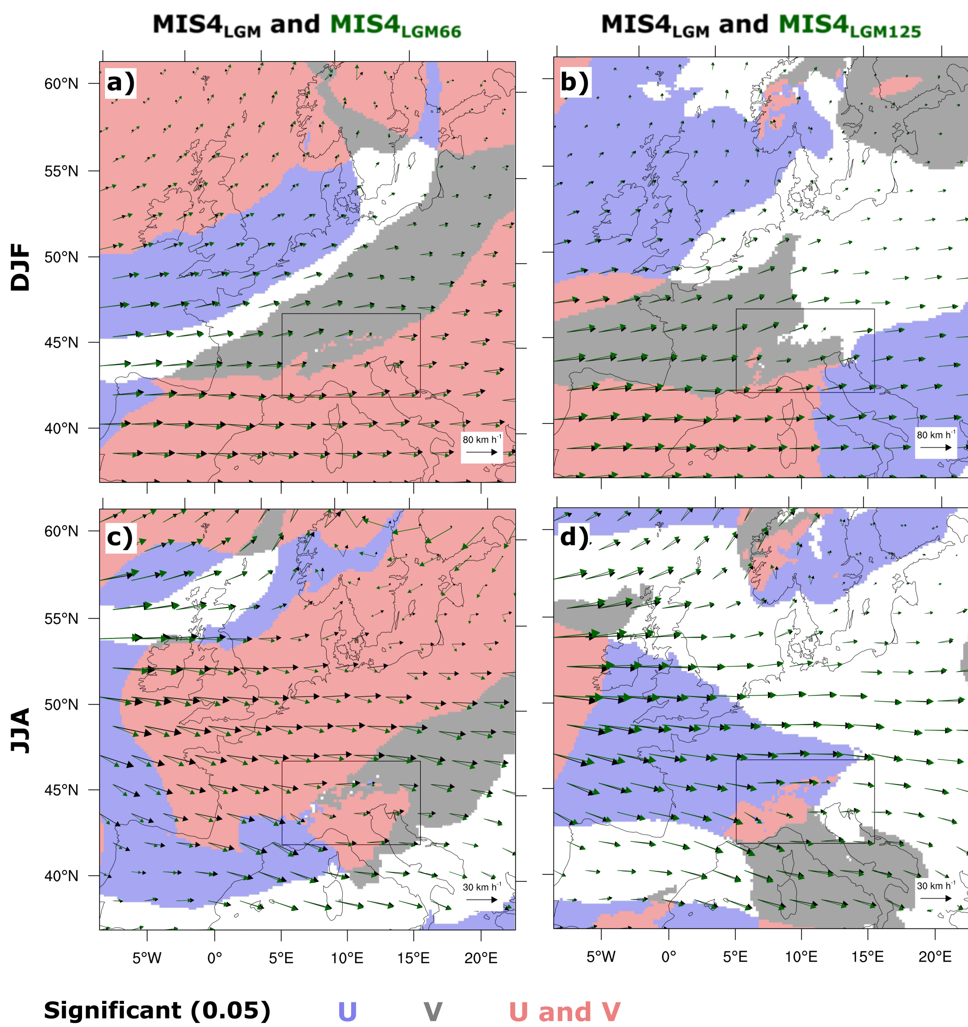

Figure 2: Climatological mean wind vectors over Europe. (a) represents wind vectors for DJF, black and green vectors correspond to $MIS4_{LGM}$ and $MIS4_{LGM66}$, respectively, (b) as (a) but green vectors correspond to $MIS4_{LGM125}$. (c) and (d) as (a) and (b) but for JJA. Red shading indicates statistically significant differences in zonal (U) and meridional (V) wind components at a significance level of 0.05 (two-tailed bootstrapping technique), blue and grey shading as the red one but only in U and V wind components, respectively. The box represents the innermost domain (with 2 km horizontal resolution), i.e. the Alpine region. Please note that the reference wind vectors differ for DJF and JJA.

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 referee refers to Fig 5d. There, the Alpine ice sheet is not changed so the changes are not due to topographic differences. We agree that the wind vectors do not suggest the similarly strong changes in Fig 5d as in Fig 5a. Still, the shading shows the statistical significance of the changes. We have added few information in the previous lines to better clarify the topographic differences as follows:

"...Therefore, we assess the wind vectors at 700 hPa for the innermost domain (over the Alpine region at 2 km resolution). Note that the LGM and $MIS4_{LGM}$ simulations use the same topography, i.e. the LGM topography. Figure 5d..."

Maybe the visualisation problem has been due to the chosen features of the wind vectors; therefore, we have redesigned the figure to better represent the wind vectors.

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

RESPONSE:

As answered in previous response, we have faced this concern in the analysis of the larger-scale 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 the 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 vertical profile of the temperature. Regarding the definition of precipitable water, please refer to the same response (fifth major comment). We have reformulated this line and added additional information in the revised version of the manuscript as follows:

"...warmer atmosphere. Even though the Skew–T diagram indicates that the relative humidity is rather low, the warmer atmosphere results in a small increase of moisture availability in the middle-to-low atmosphere (water vapour). Note that the higher moisture availability is illustrated by the increase of the values of the mixing ratio, which are obtained from crossing the saturated mixing ratio lines with the vertical profile of the air temperature. This moisture increase is especially true for 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***

<u>RESPONSE:</u>

The name has been changed in the revised manuscript accordingly.

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

<u>RESPONSE:</u>

We thank the referee. This has been changed in the revised version of the manuscript.

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

<u>RESPONSE:</u>

We agree that the differences are visually very small. As mentioned in the response to the minor comment A9, we have faced this concern in the analysis of the larger-scale 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***

<u>RESPONSE:</u>

In the revised version of the manuscript, we have changed 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.***

<u>RESPONSE:</u>

We agree with the referee. In these lines, we referred the precipitation changes to the modification of ice-sheet thickness rather than to wind changes. To clarify this, we have added more information to these lines at the end of the following paragraph of the revised manuscript as follows:

"...Note that wind changes in winter cannot directly explain the stronger reduction of winter precipitation in the western part (see previous paragraph), which are caused by the reduction of the Alpine ice-sheet thickness. The reason is that we would expect wetter conditions in the western part since the winds slightly turn clockwise (more perpendicular to the Alps) and slightly stronger. Additionally, the further analysis of the larger-scale shows no differences (therefore not shown)."

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 referee. We have reformulated these lines in the revised manuscript as follows:

"...The $LGM_{ALPSLESS}$ winds are a bit stronger and slightly turned 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 referee 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 an interesting question, but we think that this is beyond the scope of this study but certainly provides a next step, which has been pointed out at the end of the 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 convection-permitting in the two innermost domains (6 and 2 km). This does not allow to separate the convective precipitation from the large-scale precipitation since the total precipitation is directly resolved in the model. CAPE is another quantitative measure that refers to convective 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 have weakened the statements a bit and formulated 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 have reformulated 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 (statistically 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 have changed the order of the periods according to the wind profiles. Also, we have added the explanation of the profile lines and changed "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 the referee's expectations.

Best regards,

Patricio Velasquez (on behalf of the author team)

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