Climate of the Past Discussions

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Comment on cp-2021-67

Anonymous Referee #2

Referee comment on "The role of ice-sheet topography in the Alpine hydro-climate at glacial times" by Patricio Velasquez et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-67-RC2, 2021

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

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. 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.

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.

Specific comments:

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l. 7 explained

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l. 126-9 Actually, this simulation strategy is not too rigorous comparing what is commonly required from RCM simulations

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l. 171 not clear how 30 annual mean samples can be selected from 21 or even 12 years simulations ???

l. 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

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l. 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

l. 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)

l. 244 I do see boundary layer in PD as well, of course, clearly, with less stable lapse rate due to surface warming in summer

l. 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)

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l. 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

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l. 319 it is rather tiny

l. 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.

l. 325 As above, saturated mixing ratios

l. 328 … glacial climate conditions in Alpine region

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L 335-6 Again, the changes are really very tiny, despite statistical significance,

l. 354 Actually, it is difficult to see the significance in such a small region

l. 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.

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l. 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.

l. 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.

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l. 405-6 This would be nice shown on the analysis of convective precipitation

l. 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

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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 …