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Geoscientific Model Development
Discussions
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Interactive comment on "Comparison of spatial downscaling methods of general circulation models to study climate variability during the Last Glacial Maximum" *by* Guillaume Latombe et al.

Anonymous Referee #2

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This manuscript deals with the application of a downscaling technique combining interpolation (through three techniques) and General Additive Models (GAMs), over Western Europe during the Last Glacial maximum (LGM). Results are compared to site-specific climate proxys from pollen and vertebrate remains data. Its seems well within the scope of Geoscientific Model Development, and deals with the relevant topic of developing statistical downscaling tools that may be used in very different climates like the LGM. The manuscript needs in my opinion some tightening of the objectives, some work on the clarity of the text and take-home messages, as well as some additional simulation analysis. I detail below these few main comments, together with many specific ones. I can therefore recommend publication of the manuscript only once all these

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Discussion paper

comments are addressed.

Main comments

1. It is not clear from the start (and down to the choice of figures)what are the objectives of this manuscript. Is it the comparison of downscaling methods (i.e. through different interpolation techniques)? Is it the adequate simulation of reconstructed climate proxy data? Is the target location the whole Europe or only the proxy specific sites? All these questions should be answered from the beginning of the manuscript. As they are currently not answered, the organization of the manuscript and the choice of figures are indecisive (see specific comments below).

2. The simulation set-up clearly lacks some present-day validation, as already pointed out by reviewer #1. This would hopefully help disentangling errors/biases from the interpolation, GAM models, and the driving GCM (see specific comments below).

3. Another consequence of the first main comment above is that a large number of supplementary figures are commented in the main text, which is quite frustrating for the reader. The organization of figures (and associated text) should definitely be redesigned (see specific comments below).

4. In relation to the second main comment above, there is little uncertainty discussed in the manuscript, be it a result of the short calibration period for GAMs or from another source like using a single GCM. This should definitely be taken on by the authors for the manuscript.

Specific comments

1. P3L1: Please define “taphonomic”
2. P3L23-25: The length of the two GCM simulations is not clear here
3. P4L29: The reference used here for the mgcv R package is not one of those recommended in the citation info of the package. Please correct this.
4. P5L1-2: Is there actually a theoretical reason for the requirement of the same scale? I fully understand the advantage of using e.g. downscaled precipitation as a predictor for local precipitation, but when considering other predictors like SLP, the most informative scale for local precipitation my clearly not be the local scale, but a larger domain shifted in the direction of the prevailing winds (at least in western Europe). This would open quite different approaches for performing this kind of studies that would not require the interpolation step. But this may lead to difficulties given the change in land/sea mask and the presence of ice caps when considering LGM simulations. I would appreciate a comment on that.
5. P5L13-15: This starts to be confusing in terms of data. I believe that (for any location) not only the monthly regime (i.e. 12 values only) is used, but the whole 31-year monthly time series. Please be more specific.
6. FigureS1: This figure is not readable at all. Same for Figures S2 to S7. I would strongly recommend finding a way to make them actually useful.
7. P528-P6L3: “Aco” should be defined mathematically in the text without having to look into Vrac et al. (2007). There is no need to define “Dco” if not used, apart maybe from writing that it is highly correlated to “Aco”.
8. P6L16: There should be a reference here to Table S1.

9. Section 2.5: There should be two additional subsections on the interpolation/downscaling for the present-day reference period (1960-1990) and for a present-day validation period (see Main comment above and comments from reviewer 1).

10. P6L24: “Extremes” is a much too strong word here. This set-up (length of time series and temporal resolution) prevents assessing extremes.

11. Figure S8: This map should definitely be included in the main text, because this critically shows where to look in European map results. It might also be relevant to systematically indicate these locations in the results maps (depending on their size).

12. P7L18-25: Results on the SPI and STI are not used at all in the manuscript (only in the Supplementary material), so please remove their description (and possible comments) from the main text.

13. P7L18-25: The description of SPI computation lacks many important details: (1) what is the climatic norm, i.e. the reference period over which the standardization is based (present-day, LGM) and why? (2) what is the chosen distribution function for monthly precipitation? (3) Is it the same everywhere in Europe? Results are quite sensitive to these issues, as clearly shown in the literature (see e.g. Wu et al., 2005; Stagge et al., 2015)

14. P7L18-25: The choice of a variability index as the number of months with SPI between -1 and 1 is actually very strange (and indeed quite irrelevant). The SPI is by definition normally distributed, so the probability of having a SPI between -1 and 1 is 68.27%, which amounts to around 410 months in 50 years (95% confidence interval: 387-432), if the reference period for fitting the precipitation distribution is the same as the computation period (which I believe is the case here, see P7L20). So the spatial pattern observed in Figure S19 is a complete

artifact due to (1) the limited length of the period used for fitting the distribution, and (2) the relevance of the specific theoretical distribution used for fitting. Based on the 3 above comments, I strongly suggest removing all the analysis done with SPI/STI.

15. Figure 1: It would be great to see the range of present-day and LGM predictors in these figures in order to directly check statements made in the text P8L12-18.

16. Figure 2: Like reviewer #1, I believe that dotted lines are for the present-day period. Please remove the wrong legend definition from the caption.

17. Section 3.2: As mentioned above for section 2.5, there should be an additional result section for validation the interpolation/downscaling process in a present-day period distinct from the calibration period.

18. Most of results are presented at the annual time scale of for two 3-month seasons. What is then the advantage of fitting GAMs for individual months? I would expect a larger explained variance for annual or seasonal averages. I would appreciate some comments on this issue in the manuscript.

19. Figure 4: Possible differences between the three interpolation techniques cannot be appreciated from these maps with a common colour scale, because of (1) the large spatial range, and (2) the large seasonal range. Figure 5 looks into all possible differences between the 3 techniques, making both figures relatively redundant. I would therefore recommend choosing one interpolation technique as reference (ideally the one that should be recommended in the conclusion of the manuscript) and plot (1) maps as in Figure 4 for this technique, and (2) differences from this reference with a specific colour scale, as in Figure 5. This would hopefully reduce the number of figures and make the message clearer ("we choose this technique and results with the others are not that different.")

20. Figures S9 to S12. This is a much too high number of figures which shows that work on synthesizing results is clearly lacking. The reader should be presented two things: first, how temperature (and precipitation in a second step) is transformed by the whole downscaling process, through maps of raw, interpolated, interpolated +downscaled, and observations (CRU) in the present-day period. A similar presentation should be made for the validation period, and for the LGM period (for which CRU observations may be replaced by the pollen and vertebrate proxys). This could be made only for the reference interpolation technique. Second, additional maps should show the differences with the two other techniques, possibly through the whole downscaling process. This would require reorganizing figures and text (P9L6-28), but for a much better clarity of the manuscript!

21. P9L25-28, and Figure6: I am not convinced by results as presented here, as these plots are not very appropriate for identifying agreement for each site independently. I would therefore recommend trying scatterplots (with uncertainty bars as here or better uncertainty squares), with reconstructions (BCI, Wu et al. data) on the x-axis and simulations from this paper on the y-axis. The overlap of uncertainty ranges with the diagonal might better inform on the agreement of simulations with reconstructions.

22. P9L30-P10L16: cf. comments on temperature for an additional validation period, revised figure organization, etc.

23. P10L9-11, "This is due... such as precipitation (Wood et al., 2004)": I don't understand why this should lead to the European-scale discrepancies noted in the previous sentence. Please make it clearer.

24. P10L23-31: I find this paragraph a bit long, compared to other issues elsewhere that would also deserve some explanations.

25. P11L5-9: As mentioned above, please remove the SPI/STI analysis and results.

26. P11L25-26: So should we use kriging? Please be more specific on your conclusions about the preferred interpolation method.
27. P12L14-15, “more reliable variability”: I don’t understand. Please make it clearer.
28. P12L19-29: Well, this clearly poses the question on whether one should put confidence in GCM outputs at high latitudes (at least…). And for this study, this raises the following issue: should the interpolation/downscaling take place over the whole of Europe for reconstructing only a few sites located in the south of the continent. This issue should be seriously taken into account by the authors for the manuscript. Indeed, there may some biases in LGM results in the south due to present-day biases in the north via the continent-wide GAM modeling… I am definitely expecting comments on this potential issue.
29. P13L3, “larger-scale patterning”: Could you explain and make it clearer?
30. P13L4-6: I am not sure this sentence is relevant here.
31. Table 1, “AIC weights”: this should be defined and commented in the text.

Technical corrections

1. P5L25: Remove “interpolated variable”
2. P6L29-30: Redundancy of “downscaled, simulated”
3. P7L3: Please specify that “bio-climatic indices” is abbreviated as BCI(s) in the following.
4. P8L10: font size of “predictor”
5. P11L31: “than for the temperature”

References

Stagge, J. H., Tallaksen, L. M., Gudmundsson, L., Van Loon, A. F. Stahl, K. (2015) Candidate distributions for climatological Drought Indices (SPI and SPEI). International Journal of Climatology, 35, 4027-4040. doi: 10.1002/joc.4267

Wu, H., Hayes, M. J., Wilhite, D. A. Svoboda, M. D. (2005) The effect of the length of record on the standardized precipitation index calculation. International Journal of Climatology, 25, 505-520. doi: 10.1002/joc.1142

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