

Interactive comment on “Impact of precipitation intermittency on NAO-temperature signals in proxy records” by M. Casado et al.

Anonymous Referee #3

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This manuscript by M. Casado and colleagues examines the potential bias of precipitation intermittency on temperature reconstructions based on stable water isotope measurements. The authors use NCEP and ERA-interim reanalysis data as well as the output of the isotope-enabled LMDZiso model to determine precipitation-weighted summer and winter temperatures and delta O-18 signals for the Northern Hemisphere. Compared to the standard non-weighting calculation, strong local temperature biases and large inter-annual variability are found. In a further step, the impact of precipitation intermittency on the correlation between NAO and surface temperatures is investigated. According to the authors' findings, precipitation-weighting reduces the correlation between temperature and NAO in many regions but does not alter the general spatial correlation patterns.

With the chosen topic the authors address a very important problem of paleoclimate research, which is often overlooked in temperature reconstructions from the various archives. Thus, I rate the manuscript for highly valuable and support a publication in *Climate of the Past*, in general. However, in my opinion major revisions focusing on three different aspects are required before publication.

(1) Restructuring of manuscript and supplement:

The whole paper consists of a rather short manuscript, including 4 figures, and a rather lengthy supplement, including 9 more figures. From my understanding, any scientific paper should be fully understandable by its own, and a supplement should only add further information for part of the readership, interested to go into more detail. This is not the case with this manuscript. For example, the whole paragraph 3.2 (Impact of precipitation intermittency on JJAS NAO-temperature relationships) refers to figures (S5, S8, S9), which are just included in the supplement. Or, as a second example, a comparison of the different used climatologies is given in an inadequate extremely brief form in Supplement A, only. But such a comparison is highly critical for the paper, as all following analyses are most likely also affected by the shown differences between the chosen NCEP, ERA and LMDZiso data sets. Thus, such comparison should be moved to the main text and explain in much more detail common basic features and key differences among the three data sets.

(2) Rewording of presented results:

For many statements, I rate the authors' description of the presented results as too short and sometimes also too positive. Again, I give two examples: (i) In paragraph 3.1, the authors state in the last sentence: "The inter-annual standard deviation of the summer bias (Fig. 2b) remains smaller than for winter, with the exception of LMDZiso which produces both a large and variable bias around the Mediterranean area and Central Russia." But looking at Fig. 2b, larger inter-annual standard deviation can also be detected for Western Europe and large parts of the United States. In fact, from Fig.

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2b it is not clear to me, if not the total area in LMDZiso showing a high inter-annual bias is larger for summer than for winter. (ii) In paragraph 3.5, second text block, the authors write: “First, the LMDZiso model results are described (Fig. 3). The model results clearly show the same pattern of winter NAO correlation with Tp and d18O, with negative correlations over parts of Greenland and NW America (Québec area), and positive correlations in Northern Eurasia.” Here, the authors omit that the negative correlations over Greenland clearly differ in region between Tp and d18O. The extent of positive correlation over Northern Eurasia between Tp and d18O is also substantially different in Fig. 3. I could give several more examples of such inaccurate and selective description of the presented figures and strongly suggest that the authors are more precise and exhaustive with their result descriptions in a revised manuscript.

(3) More detailed explanations:

At several occasions, the authors simply describe the detected differences in their results but do not propose any scientific sound explanation for their findings. This leaves the reader with uncertainty about the relevance of the presented results. Once again, two examples: (i) In paragraph 3.3 it is written “Surprisingly, LMDZiso produces a precipitation intermittency bias that is more similar to the magnitude derived from NCEP data, while its mean climate is closer to that of ERA-interim.” This is a very interesting finding, but how can this be explained, given the fact that LMDZiso was nudged to ERA-fields. Is the nudging not strong enough for influencing precipitation timing in LMDZiso? And/or are the precipitation schemes in NCEP and LMDZiso more alike that the one used for ERA reanalyses? (ii) In the conclusions, the authors state “From existing isotopic datasets, LMDZiso seems to overestimate the strength of the summer NAO imprint on Greenland water stable isotopes, possibly due to the isotopic composition changes associated with different air mass origins.” Why do the authors rate a mixing of different air masses as the most likely cause of the overestimated NAO signal on Greenland in LMDZiso? Isn’t such mixing of air masses included in LMDZiso, and should the model not yield most realistic results if run in nudged mode? Could any

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other reason cause this bias, too? And could one perform further analyses (if so, which ones?) to determine the reason for this model deficit? Once again, I could give several more examples of such limited explanations and advise the authors to go one step further in their scientific analyses.

As I ask for a major text revision, I omit any list of minor corrections at this stage of the review process.

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