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Climate of the Past Discussions
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Interactive comment on "A new multi-variable benchmark for Last Glacial Maximum climate simulations" *by* Sean F. Cleator et al.

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The contribution provides up-to-date global maps of seasonal climatic indicators of the LGM, intended to be used as a reference ("benchmark") for PMIP4/CMIP6 entry-card LGM simulations. The reconstructions are obtained by a variational method using a prior based on the CMIP3/PMIP5 ensemble, and updated on pollen-based reconstructions (CO2-corrected) of various indices.

The purpose of the study is clear, and the contribution is justified in the framework of the PMIP4 effort. However, before publication, it is advisable to revise the description of the methods and improve the wording accuracy.

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Discussion paper

1 On the variational method

I will focus here mainly on the variational technique (section 2.4). Mathematical details of the technique applied in this study are available in Cleator et al., 2019a. This is an arXiv preprint. It is not clear whether the latter is intended for a peer-reviewed journal or whether it was part of a thesis examination. I understand from that arXiv contribution that the different indices (α, MAT, etc.) are first computed in the different simulations; precipitation is log-transformed to avoid negative predictions, and a matrix B encodes the covariances between the indices simulated in the ensemble (this matrix is then called the covariance of the "uncertainties in the background"). The matrix B is assumed to be the prior covariance.The variational approach further assumes Gaussian distributions and a fixed spatiotemporal covariance (with length scales of 1 month and 400 km, respectively). It is mathematically equivalent to Bayesian updating of a prior (the PMIP3/CMIP5 ensemble) by observations, which have their own error variances.

There are a number of points in this approach which deserve discussion. For this reason it would have been better to see these method details in the *Climate of the Past* paper, so that the paper, the review, and possible responses constitute a self-contained contribution.

1. Even though this is a reasonable and convenient choice, the PMIP3/CMIP5 ensemble is not a fully legitimate prior. For two reasons.

    1. Unlike what (roughly) obtains when using time series of a numerical weather prediction system, there is a priori no guarantee that the covariance matrix of a multi-model ensemble produces modes which satisfy "physical consistency". Why would we expect that the inter-model differences provide knowledge about how different variables should co-vary?

2. In principle, a “prior” encodes what we a priori believe the climate could be. The authors have then chosen to mask regions with little update by observations, and leave visible the grid points where the observations have seriously shifted the prior. This seems at first sight reasonable because the idea is to focus on the pollen reconstructions and not on the PMIP3 output. Yet, at face value, this approach is inconsistent with a Bayesian interpretation. Grid points of strong update are associated, in the Bayesian interpretation, with a very small marginal likelihood (a wrong prior means a wrong model). Hence, this leads me to two sub-questions:
    - To what extent should we be concerned that the posterior variance remains influenced by the prior variance? Indeed, mathematically, the posterior variance is bounded by the prior variance, which — if we admit the models are really off — is meaningless.
    - To what extent the prior covariance (link between different variables) may still be trusted at all if the models are so wrong? This remark strengthens the original concern about the physical meaning of the covariance matrix, even when the prior is only mildly updated. What is the advantage of this approach over a mere Gaussian interpolation (flat climate prior), which in this case might turn out to be more reliable and free of the dubious claim of “physical consistency”?

2. Were the length scales tested by some form of cross-validation (e.g. leave-one-out), or were they merely chosen because they are a priori reasonable?

3. The arXiv paper provides the definition of the moisture index. It should be repeated here (moisture index is currently introduced l. 297 without definition)

4. The authors should consider providing a link to supporting code. The maps are currently provided as University of Reading dataset (with a doi) but its lifecycle is detached from the present contribution. A dataset consistent with the current

Climate of the Past submission, reflecting a possible response to concerns of the reviewers, might best be included as supplementary information.

2 Uncertainty (Uncertainties) vs variance

- It is important to distinguish the notion of variance from the notion of uncertainty. They are not synonymous. Variance describes the second momentum of a distribution; uncertainty is a reference to an identified lack of knowledge. Only when the distribution is assumed reflects our knowledge of a given quantity is it legitimate to identify both. Multi-model ensembles, in general, cannot be said to capture our knowledge of the state of climate at a given time. For this reason, I would argue not to call the PMIP3 covariance a "background uncertainty". The legend of Figure 2 clearly identifies "uncertainties" with "standard deviation of the non-dimensionalised multi-model ensemble" but this seems inadequate to me. Adding to the confusion, different qualifiers occur throughout the text: "explicit uncertainty" (l. 97), "analytical uncertainty" (l. 406), and, on Figure 3, "grid-based errors in the prior" and "global uncertainty".
- As the uncertainty quantification seems to be a selling point of the present article, the assessment should be more open and transparent about sources of uncertainty, and discuss which of theses sources can be quantified and how. For example, little is said about uncertainties introduced by the CO2 physiological correction. Is it guaranteed to be accurate?
- The strategy for identifying grid points with little posterior update explained l. 406 is not quite clear. Why not consider a Kullback-Leibler divergence? At the risk of repeating myself, I am concerned about the (meaningless) residual influence of the prior variance and covariance in cases where the prior is effectively discarded by the observations.

3 About the discussion section

- This is a minor comment, but the comparison with Goosse et al. 2006 is perhaps slightly misleading. The Goosse et al. purpose was dynamic reconstruction, while the purpose of the present contribution is to provide a diagnostic reconstruction. In passing, Goosse (2006) did not use a "Kalman particle filter" (whatever it means). Goosse et al. used what they called an "optimal realisation" iteration, which can be interpretated as a highly degenerate form of particle filter. Dubinkina et al. 2011, doi 10.1142/S0218127411030763, adopted a more standard particle filter.
- This said, the argument that the variational approach produces maps outside the realm of climate simulations is a double-edged sword. The variational approach assumes Gaussian distributions, and is mathematically equivalent to a Laplace approximation of arbitrary distributions. This is this approximation which allows generating posterior distributions far from the prior. But, in this case, sound Bayesian interpretation should lead us to treat such posterior as utterly suspicious.
- line 384 : It is said that it "would be worth taking [changes in length scales] into account." I would advise either deleting this sentence, or giving more substance to the claim. For example, have you already performed some sensitivity experiments.

4 Other editorial comments

- Is the very first paragraph really necessary?
- There is room for improving wording accuracy. In what sense is the benchmark

"robust" (l. 37) ? l. 97: You write: "explicit uncertainties attached to it". Did you mean "uncertainties explicitly attached" ? Avoid, where possible, the phrase "in terms of" or "means that" (ll. 321 - 326, in particular, need rewording). What is meant by a "statistical reconstruction method" l. 370 (the present exercise is a statistical reconstruction isnt'it ?).

- Figure 5: Shouldn't "pre-industrial reference" be preferred over the vague wording "original" as x-axis label?

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