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

Sean F. Cleator et al.

s.p.harrison@reading.ac.uk

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Response to Anonymous Referee #3 Review

The paper is quite short and lacks any detailed evaluation of the resultant product. The community's use of this new data product would in my opinion be aided by a more in-depth evaluation of the properties of the reconstruction. It's not clear how important the choices around the assimilation formulation are for the final reconstruction. Specifically the section around lines 268-278 should in my opinion be spelled out and the sensitivity to these choices evaluated.

Response: It is unclear what kind of evaluation of the product the reviewer envisages, given that there is no global ground-truth data set other than the pollen-based recon-

structions themselves. We have already pointed out (lines 317-321) that the analytical reconstructions of temperature are close to the Bartlein et al. (2011) data set, both in terms of magnitudes and spatial patterns. The differences between the analytical reconstructions and the Bartlein et al. (2011) reconstructions of moisture variables are a consequence of the fact that statistical techniques based on modern pollen-climate relationships cannot account for CO2-induced changes in water-use efficiency. In terms of the impact of methodological choices, the major issue here is the choice of length scales. We have made sensitivity analyses to examine the implications of the choice of length scales, and this was discussed in the arXiv preprint. In expanding the description of the application of variational techniques here (see text in response to Michel Crucifix's review) we have commented further on this.

The statistical methodology that forms the basis of this study is also not described here but in a arXiv article. I'd like to see more of this brought into the present manuscript to make it self-contained.

Response: This is a point raised by Michel Crucifix in his review. We have now modified the text describing the application of the variational method to include a fuller description of our approach. As we point out in the response to Michel Crucifix's review, the full details of the method are now in review for JAMES and we have made the post-review version of this paper available on arXiv.

Line 127: define MI here.

Response: The reference to MI is inappropriate in the present context because the text refers to a generic control by moisture availability rather than a specific index. We also note there was a crucial comma missing in this sentence! In response to Michel Crucifix's review, we have modified this text to read:

which is generally taken into account by process-based ecosystem models, but not by statistical models, using projected changes in vapour pressure deficit or some measure of plant-available water

Line 209: modelsfor -> models for

Response: We have corrected this typo.

Line 252-253: I think it might be appropriate to bring some/all of this methodology into the present text, as discussed above. Response: We have modified the text here to provide more detail about the method. Please see proposed revised text given in the response to Michel Crucifix's review.

One question that arises from briefly reading the methodology paper, relates to figure 1 in the arXiv article. Here the assimilation appears not satisfy the pollen-inferred MTCO. Is this because the prior (from the models) is relatively consistent, and so doesn't allow the assimilation to get that cold? Does this happen when applied to the pollen data here?

Response: Figure 1 in the arXiv pre-print does not show a real situation but was designed (as explained in that paper) to illustrate the procedure. In general, the pollen-based reconstructions of MTCO are further away from the model-based prior then summer temperature measures. If the variance in the pollen-based MTCO reconstructions is small, then the analytical reconstructions will be close to the pollen-inferred MTCO. If there is high uncertainty in the pollen-based reconstructions, then the analytical reconstructions are not strongly constrained by these reconstructions and will be further away. This makes intuitive sense because we do not want to rely on pollen-based reconstructions if there is large uncertainty. Thus, it is possible for the assimilation to produce cold results but only if there is tight agreement between the observations about the magnitude of the cooling.

How do we interpret these choices, given that the climate models themselves could feasibly be systematically biased, e.g. through not including aerosols, or using modern vegetation distributions? How have you addressed the possible systematic bias in the models and hence in your prior?

Response: It is possible that the models show a systematic bias because they do not include all of the appropriate forcings for the LGM climate. We assume that such a systematic bias would primarily influence the magnitude of changes rather than the physical relationship between variables or across space. The presence of a systematic bias is therefore not important because the pollen-based reconstructions effectively correct for any systematic biases in the model-based prior, providing the pollen-based reconstructions have low uncertainty. One of the reasons that we discuss in the paper for adopting a variational technique, rather than some kind of filtering, is that this approach means that the analytical reconstructions can go beyond the range of the simulated climate.

Line 268-276: This section seems crucial to me, but is not clearly described. Please include the mathematical formulation used and a justification for choices made.

Response: We have expanded the text describing the application of the variational method, including a description of the composite errors. Please see revised text included in the response to Michel Crucifix's review.

Lines 276-278: Do you mean that if the data is too uncertain you mask it based on a 5% criteria? Please could you re-phrase to clarify.

Response: When the change in the variance between the analytical reconstruction and the prior is less than 5%, it does indeed mean that the climate is not well constrained by observations (i.e. that there is high uncertainty in the observations). We have modified this text (and the discussion of the choice of cutoff in the Discussion) to clarify this point. Please see revised text in response to Michel Crucifix's review.

Lines 288: How does your product compare with the original Bartlein et al 2011, and the GCM-based prior? Could you show this?

Response: The GCM-based prior is shown in SI Figure 1 and the original pollen-based reconstructions (from Bartlein et al, 2011 and from Prentice et al., 2017) are shown in

SI Figure 3. Comparison of these figures with the analytical reconstructions shown in the paper in Figure 4 shows the difference with our product. We could add a new set of figures to the Supplementary showing difference maps, if necessary.

How well is the seasonality captured and how does it differ from the simulated seasonality in the GCM prior?

Response: It is not clear what the reviewer is asking here. We have no independent measure of seasonality that can be used to assess the analytical reconstructions. The analytical reconstructions of MTCO and MTWA, the difference between which is the measure of the strength of temperature seasonality, are only shown when the pollen-based reconstructions contain sufficient information to modify the model-based prior and thus when the uncertainty in the pollen-based reconstructions is small. We could produce maps showing the temperature seasonality from the analytical reconstructions and the model ensemble (and indeed the difference between them) but it is not clear what these would add to the manuscript.

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