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Climate of the Past Discussions

Interactive comment on "Using data assimilation to study extratropical Northern Hemisphere climate over the last millennium" *by* M. Widmann et al.

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I understand that this manuscript presents a review of the state-of-of the art in the field of paleodata assimilation in climate model simulations. After some theoretical consideration about data assimilation and the particularities that arise in the assimilation of paleodata, the authors describe three methods that so far has been used in paleo simulations with climate models: ensemble member selection, Forcing Singular Vectors and Pattern Nudging. The advantages, caveats and ranges of applicability of each method are discussed, and some results are also presented.

In my opinion, the manuscript is generally well written and informative. This area of

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research is in its infancy and the authors present a candid view of the hurdles that still need to be resolved. In this sense I am happy to recommend the manuscript for publication.

However, there are some aspects of the manuscript that, in my view, could be improved. I also found a bit irritating that some of the results discussed in extent here are taken from manuscripts in preparation (perhap submitted?), e.g. Widmann et al. (2009) and Palastanga et al. (2009). These citations do not occur in passing but sometimes constitute the basis for whole paragraphs end even sections. This can be a matter of taste, but I do not think this is a good idea. The interested reader will not be able to locate the papers in question, as even the contents, journal, title and publication date are not precisely known. If these results are deemed important enough, they should be presented here in more detail.

The introductory sections 1 and 2 may be a bit too long. Some messages are found repeatedly, for instance, that climate models cannot reproduce the observed or reconstructed evolution of the internal variability. On the other hand, the reader will perhaps grapple with some technical details that receive too little space, for instance section 3.3.1, in particular equation 4 and 5.

The first paragraph in page 2127, which is included in the section about ensemble member selection, actually is devoted to more general questions about data assimilation. One could consider to move it to the introduction section.

Page 2118, line 3: Total climate variability may perhaps not be decomposed as a 'sum' of internal variability and externally forced variability, as both may interact non-linearly. Perhaps "combination' is a better word here?

Page 2120, line 8: 'examples of the third case..' The previous paragraph refers to just two ways for data assimilation. which is the third case?

Page 2123, line 8 :'meteorological'

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Page 2126,line 6: 'the average of 11 simulations appears to underestimate multi-decadal variations'. Perhaps part of the multi-decadal variability is also internally generated. In this case, an average of simulations would display less variability than a reconstruction, also at these time multi-decadal scales.

Page 2127, beginning of section 3.3. As far as I understood, the ensemble -member-selection method is not limited to assimilation of temperature. It could be in principle used to assimilate circulation, also to assimilate up-scaled circulation patterns, if needed. I do not see here what substantial difference to FSV and PN could be. The discussion in these rather long two paragraphs as to why the assimilation of atmospheric circulation, as opposed to assimilation of temperature, seems to require special methods is not clear to me. I agree with the authors that the variability of the atmospheric circulation is probably less tightly controlled by the external forcing, and that the atmospheric circulation may be responsible in the extra-tropics for variability of regional temperature. But the ensemble-member-selection method as presented in the previous section assimilates a set of local temperatures, and not a reconstructed large-scale temperature pattern. According to this reasoning, the ensemble-member selection method would not be adequate for temperature either. All in all, I found these introductory paragraphs in this section confusing. Perhaps the authors may consider if they are really needed.

Page 2129, line 16. The FSV and PN are presented again as advantageous because they can assimilate large-scale patterns. I do not see why the ensemble-member-selection cannot be used for this as well.

Page 2130, line 3:'seriously'

Page 2130, line 8:'a clear principal advantage'

page 2130, line 10: I think this section is too tightly described. The first sentence is not really encouraging for the reader. A more clear description of what the concepts of perturbation growth and tendency perturbations are would be really helpful.

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Page 2132, line 5. This sentence seems to suggest that it is impossible to construct an adjoint model of a General Circulation Model. However, computer methods already exist for quite a long time that automatically generate the code of the adjoint model from the code of the climate model. The existing literature is not small, I just cite here one paper: Construction of the adjoint MIT ocean general circulation model and application to Atlantic heat transport sensitivity, Marotke et al JGR 1999. The authors may refer to a series of papers by R. Giering, T. Kamisnki and P. Heimbach. Even internet tools exist to generate the adjoint code http://autodiff.com/tamc/

Page 2132, line 15: 'Atmospheric physics..' this expression can be confusing. Most modellers will understand that the physical parameterizations are meant here, whereas other readers will understand the set of all physical processes in the atmosphere.

Page 2132: 'The target pattern is assimilated only in winter'

Page 2133, middle: in discussion about the temperature anomalies in the Dalton Minimum, the text seems to suggest that these anomalies are completely controlled by the atmospheric circulation in winter and by the ocean in summer. Is there no contribution of the external forcing in the Dalton Minimum? Is the Dalton Minimum attributable completely to internal variability? this conclusion seems to me very bold.

Page 2134, line 1 '...described above, puts some..' I think the comma should be deleted

Page 2134, line 9: '..the JFM-averaged stream function is determined as deviation from the mean of the control climate'

Page 2138,last paragraph: this paragraph is an example of the problem I mentioned at the beginning. The present manuscript contains no details about the simulation with HadCM3 used in Pastalanga et al. and the reader cannot access that information either (the references list is incomplete here and I could not locate this paper). Is this simulation a control run, a forced run, with which forcings, etc , etc? These aspects are important for the discussion of the inability of the data assimilation scheme to repro-

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duce the reconstructed temperature pattern in the Maunder Minimum. i.e. cold temperatures throughout Europe.. The authors attribute this inability to erroneous mean westerly winds in the model, but if this simulation is a control run it could be very well attributed simply to the lack of external forcing. Even if it is a forced run, it could be interpreted as a too weak prescribed external forcing.

Page 2139. line 14:' data assimilation methods have a tendency to produce anomalies that are with in the model's range internal variability' I would not agree completely with this sentence. I do not think it has been shown in the manuscript either. For instance, the ensemble-member-selection method could in principle very well simulate anomalies outside the range of internal variability if members in the ensemble have been created with different external forcings.

Interactive comment on Clim. Past Discuss., 5, 2115, 2009.