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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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We would like to thank Eduardo Zorita, a second, anonymous reviewer, and James Annan for their very careful, constructive and helpful comments, which will lead to substantial improvements in the revised version. Our responses to the individual comments are listed below.

Reviewer 1 (Eduardo Zorita).

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.

We understand these concerns. We would have preferred to submit this manuscript after the Widmann et al. and the Palastanga et al. (2009) manuscripts had been published. However, the external deadlines associated with this CoP special issue led to this non-optimal timing.

Palastanga et al. (2009) is now accepted, and we will thus keep the reference.

Widmann et al. (2009) will be submitted in spring 2010 around the time when the revised version of our manuscript would be published. As it contains relevant validation of the pattern nudging method we suggest to keep the reference but we will make the present manuscript more self-contained. In accordance with the reviewer's suggestion we will include additional information on pattern nudging. The key aspect for this paper is that the pattern nudging produces NAM-type winter target anomalies well. We will demonstrate this by including the SLP target anomaly as an additional panel in Fig. 7, such that the target pattern and the response produced by pattern nudging are shown. The discussion of the performance of pattern nudging in other season will be deleted. Some of the technical comments on p23/24 on the choice of the nudging constant G and on the nature of variability in the nudged simulation are self-contained and do not need the citation of Widmann et al. (2009), thus in these paragraphs the reference will be deleted.

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.*

We will check carefully for and remove redundant text and add more explanation for eqns. 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.

We agree that this paragraph should be in the introduction and will move it.

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?

We agree and will change the wording.

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?

It should be 'second case'.

Page 2123, line 8 :'meteorological'

Will be corrected.

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.

We were not clear enough here in the submitted version. If we were taking 11 sim-

ulations without data assimilation, we would completely agree with the reviewer that it would tend to reduce the natural variability. As a consequence, the average would underestimate the variability present in the observations. On the other hand, if the simulations with data assimilation were perfect, they will all include both the forced response and the real, observed natural variability. Averaging over 11 simulations of this type would thus not smooth out the natural variability as the same signal would be present in the 11 simulations. Actually, the 11 simulations have a variability at decadal scale-scale which as an amplitude similar to the observations in Scandinavia. However, their average has a lower amplitude because this simulated natural variability is not consistent between the 11 experiments. This indicates that the data assimilation procedure was not able to constraint sufficiently the variability in that time-scale. The causes of this discrepancy are not clear. However, it is interesting to note that in another group of experiments, using thermometer data to constraint model result such an underestimation of decadal variability did not occur. This point will be clarified in the revised version.

Page 2127, beginning of section 3.3. As far as I understood, the ensemble –members election 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.*

We agree that ensemble member selection can be used with any data, including large-scale circulation patterns. We also agree that the regional temperatures that are assimilated in the current version of ensemble member selection are likely to be partly caused by circulation anomaly. The manuscript has not been clear on this and will be shortened and corrected. We will consider moving this discussion to the introduction and point out that the due to the link between circulation and temperature assimilation of either of the two variables seems justified. It

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.

As stated in the previous point, the ensemble-member selection can be used for large-scale patterns but it was not really designed for that as it is much more expensive than FSV and PN. It is also worth pointing out that ensemble member selection can assimilate sparse local information, while FSV and PN can not. The sentence will be modified in the revised version to make these points clearer.

Page 2130, line 3:'seriously'

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

Will be corrected.

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.

We will add further explanations to the text.

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/

It is indeed possible to construct adjoint models for GCMs, but for most GCMs adjoint code does not yet exist.

A second, more fundamental problem is that the linearization needs to be applicable on the timescales on which assimilation is performed. In weather forecasting this is on the order of a day, whereas for applications in palaeoclimatology the relevant timescales are much longer. The linearity assumption is valid if the "forecast" is short enough. If one wanted to use these techniques for palaeodata, then one would need a tangent linear model which remains valid for time periods over which the single proxy measurement is valid, typically a season or a year. On these time scales, the linearity assumption breaks down.

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.

Will be clarified.

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

Formulation will be changed.

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.

Although the DA experiment shows that much of the European temperature anomalies during the Dalton Minimum can indeed be explained by circulation anomalies in winter and ocean temperatures in summer, it is important to note that the circulation anomaly itself can be either forced or internally generated, and that hemispheric anomalies may be more strongly linked to solar and volcanic forcing. We will include a more balanced discussion in the revised version.

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

Will be corrected

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

the mean of the control climate'

Will be changed.

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 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.

Palastanga et al. is now accepted. We will also provide some more detail on the simulation and on the issues raised by the reviewer in our manuscript.

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.

We agree with the point of the reviewer. It is the reason why we used the wording "*... data assimilation methods have a tendency...",* meaning that it is possible to force the model to simulate states outside its range of uncertainty but a special treatment/procedure is required. Up to now, no treatment/procedure of this type has been applied successfully to our knowledge to study the climate of the past centuries. This will be explained more clearly in the revised version of the manuscript.

Reviewer 2

2nd paragraph: I think it would be usefull to briefly describe the main differences between EMICS and GCMs related to internal climate variability by a few words

We will include some text on this.

3rd paragraph: Could you say sth about the errors included in the reconstruction of climate indices based on proxy data – are the numerical simulations carried out with GCMs and EMICS forced with changes in external forcings are really independent [i.e. thinking about the reconstruction of the solar and volcanic activity ?]

As far as we know the proxy data used for forcing reconstructions and those used for circulation or temperature reconstructions are independent. We will clarify this point.

4th paragraph: You state that the NAO/NAM are dominant modes of natural variability. Does this imply that under externally forced factors these modes are not dominant anymore ? Maybe it would be more appropriate to formulate dominant modes of atmospheric variability

The point we wanted to make is that they are dominant without anthropogenic forcin, but indeed they are also dominant with anthropogenic forcing. To avoid confusion we will rephrase as suggested.

Compared to the total length of the paper the introduction is quite short. Maybe the authors could include at least one paragraph about the main climatological characteristics of candinavia and related work [i.e. climate reconstructions already carried out, the impact of the north Atlantic drift in that region]

We will add a short discussion of the climatological characteristics of Scandinavia, and will also move several paragraphs from the main text to the introduction.

3.1 Differences between weather and forecasting and palaeoproblems

4th paragraph: The authors should mention that not only the spatial and temporal resolution is limited in proxy data, but they also contain large errors related to the

reduction of variability induced by many reconstruction methods, i.e. linear methods such as regression or canonical correlation.

Good point, will be included.

What do the authors conclude about the variational techniques ? Is the 'standard approach' as for example used in NWP appropriate to address paleoclimatic issues or does – based on a pragmatic point of view – the [proper] estimation of the model and observational errors preclude the usage of the established terminology of data assimilation when used on paleoclimatic problems.

I also would suggest to shorten the whole section, for example the discussion of the NCEP/NCAR reanalysis.

It seems unlikely that the standard variational approach can be used for palaeo problems, because of the problems related to error estimation and to minimizing the cost functions over seasonal to decadal timescales. The problem with variational techniques is that for minimizing the cost function one uses the tangent linear model of the NWP and its adjoint to accommodate the measurements. As mentioned above the linearity assumption is valid if the "forecast" is short enough. If one wanted to use these techniques for paleodata, then one would need a tangent linear model which remains valid for time periods over which the single proxy measurement is valid, typically a season or a year. On these time scales, the linearity assumption breaks down.

The NCEP/NCAR text will be shortened.

3.2 Ensemble member selection

Proxy Data over Scandinavia: Could you just mention that the number of degrees of freedom is strongly reduced when decadal filters are applied – this then also would explain part of the high correlations, especially in Fig. 1c. Moreover the model is constrained to the proxy data and therefore a-priori knowledge about the state of the system is included in the modeled time series [as you mention in the following para-

graph]. Therefore it should not be a surprise to attain the high correlations. I think more important in this respect would be what we learn about the large-scale state of the system, i.e. which states of the atmosphere and/or ocean control the evolution of the local proxy time series [as you state in the last paragraph in this section].

We completely agree with the Reviewer. This will be mentioned explicitly in the revised version

I think for the reader that is not so deeply involved in the topic it would be very helpful to make this points really clear: High correlations between modeled and proxy time series should not be regarded as specific virtue of DA – the real added value of DA is instead to get information about local climatic conditions where no proxies are available and the large scale climatic state in terms of atmospheric and also oceanic circulation – the latter might be especially important for Scandinavia because of the pronounced influence of the North Atlantic drift. One could therefore also hypothesis on the stability of the atmospheric circulation-climate connection under changed oceanic lower boundary conditions.

We agree that it is an important point. As it is valid for all the methods, it will be discussed in the introduction of the revised version.

3.3 Upscaling and control of large-scale circulation

I would suggest to shorten the introduction to the main ideas because a lot of information contained in this section will be discussed in the following subsection and therefore

this section includes somewhat redundant information.

As mentioned in the reply to reviewer1, some of the introduction to 3.3 will be moved to the general introduction. We will also carefully check that the text is concise and non-redundant.

3.3.1 Forcing singular vectors

Figure 3: You state that there is a 'strong qualitative similarity' between the target

pattern and the difference pattern. I would suggest to leave out 'strong' – simply because the positive geopotential height anomaly is deflected to the east in the difference

pattern and therefore the well established and enclosed negative geopotential height

anomaly over central eastern Europe is not fully established – this should also lead

to changes in the resulting air flow being would be more zonal in the data assimilated

simulation. This could potentially also be seen in the temperature patterns in DJF because temperature anomalies over central eastern Europe do not show the significantly

reduced temperatures as indicated in the reconstructed Luterbacher data.

We will formulate this more carefully

3.3.2 Pattern nudging

2nd paragraph: the application of the ECHO-G model should could be shortened because it does not present direct information for data assimilation.

We agree and will shorten this part.

The paper Widmann et al. 2009 (in preparation) is cited a few times. I am not quite sure how to deal with this issue because the interested reader will not be able to consult the paper.

See comments to reviewer 1

4 Summary and conclusions:

I think a short paragraph emphasizing the real added value of data assimilation specifically it's main skill should be included. For example, DA is not a tool simply increasing the correlation between results based on observations/reconstructions and model *simulations. This can be seen in most cases, because a-priory knowledge about the evolution of the local scale or large scale climate is included in the model simulation via DA. I think the point where DA can really be used is to understand potential dynamical processes controlling the local scale variables. This information about dynamical considerations should be highlighted as a virtue of Data assimilation into paleocliamtological simulations*

As suggested by the reviewer, this important point will be discussed both in the introduction and in the conclusions.

James Annan

I would be interested to see more discussion of the treatment of uncertainty in this field. One powerful aspect of assimilation methodology is the explicit generation of uncertainty estimates associated with the optimal analysis. For example, one might reasonably hope for reconstructions of past climate variability that have lower, and better defined, uncertainties, than those existing reconstructions which are based on a purely statistical treatment of proxy data. However, this hasn't really been addressed in the manuscript (and perhaps in the existing work upon which it is based, in which case maybe it could be discussed most appropriately in terms of a goal for future work). Anyway, I would appreciate the thoughts of the authors on this matter.

We agree that the generation of improved uncertainty estimates is a potential advantage of data assimilation. However, as surmised by James Annan, the current work has not yet explicitly addressed this issue. The problem is that the pragmatic data assimilation approaches discussed in our manuscript are not formulated within the standard conceptual framework, thus it is not immediately clear how the uncertainty estimates could be produced. Necessary components for generating uncertainty estimates seem to be realistic estimates on the error of the proxy data (when using forward models/observation operators), as well as model errors. The latter may be estimated from the ensemble spread for the ensemble member selection method, for PN and FSV

the way how to estimate the model errors is less clear.

We will continue the discussion of this topic and will include comments as a goal for further work, as suggested by James Annan.

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