Clim. Past Discuss., 9, C2442–C2445, 2013
www.clim-past-discuss.net/9/C2442/2013/

Climate of the Past Discussions

Interactive comment on "Changing correlation structures of the Northern Hemisphere atmospheric circulation from 1000 to 2100 AD" *by* C. C. Raible et al.

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Received and published: 30 October 2013

General Comments This manuscript concerns a very interesting subject, namely the temporal stability of atmospheric teleconnections. With respect to out knowledge of past climates, this paper addresses the important question: Can we use the structure of atmospheric teleconnections that are derived from observations during the instrumental period to interpret paleoclimate records? The paper has the potential to be an important contribution to this subject. However, I believe that there are some significant issues with respect to the data used, its interpretation and the presentation of the results that need to be addressed before publication.

Major Comments 1) The paper makes extensive use to the 20th Century Reanalysis (20CR). I believe that readers of the paper need to be given more information on this novel reanalysis so as to place into proper context the results that are presented. Most importantly, the 20CR only assimilates surface pressure data and so there is some concern about its ability to represent tropospheric climate variability as opposed to just the mean tropospheric climate. I'm concerned that the authors use the 500mb geopotential height field from the 20CR and present no information on the ability of the 20CR to capture the variability in this field. At a minimum, the authors should confirm that 20CR is able to represent the variability in the 500mb height field over the period for which upper-air data is available (i.e. the late 1940s onwards).

This could be accomplished by comparing the teleconnectivity structure over this period with that from a more traditional reanalysis (such as the NCEP Reanalysis or ERA40) that assimilates this upper-air data. Alternatively, the authors could look at the teleconnectivity structure in the sea-level pressure field. Given the 20CR's reliance on surface pressure data, there is probably greater confidence in its ability to represent this field. Moore et al (2013, "Multidecadal Mobility of the North Atlantic Oscillation", J. Climate) present such a comparison with respect to the ability of the 20CR to represent the surface climate variability in the North Atlantic region.

The 20CR also has an issue at high northern latitudes that is related to the representation of sea ice in coastal regions that is discussed in the Compo et al (2011) paper. Given this documented problem with the 20CR, I'd be careful to show teleconnectivity patterns in the Arctic.

In addition, the ECMWF has just released its ERA-CM reanalysis product that uses a similar approach to provide a representation of the state of the atmosphere since 1900. Unlike the 20CR, they provide the actual ensemble members and so it would be possible with the ERA-CM to validate the results obtained with the 20CR as well as providing some measure of the spread in the ensemble members, something that is not possible with the 20CR. It also presumably doesn't suffer from the same issue at

high northern latitudes.

2) On a related note, the authors use the 500mb height field to define the NAO. Most of the research on the NAO focuses on its expression in the sea-level pressure field and so for completeness and to provide a bridge to the large body of work on the NAO, I recommend that the authors include the teleconnectivity structure of the sea-level pressure field.

3) The authors present two new teleconnection patterns (WADP and AWAVE) to describe their results. I'm concerned that this approach may be suboptimal and doesn't address other teleconnection patterns from the current climate (such as the East Atlantic pattern and the Scandinavian pattern) that may be invoked to describe the variability in the North Atlantic and North Pacific regions. The authors should also refer to Shabbar et al (1997, "The association between the BWA index and winter surface temperature variability over eastern Canada and west Greenland" Int. J. Climatol.) who describe an upper-tropospheric dipole-like pattern in the western Atlantic that may be related to the WADP. The impact that this dipole has on the NAO has been discussed in Moore et al (2011, "Complexities in the Climate of the Subpolar North Atlantic", Q. J. Roy. Met. Soc.). I am also curious why the authors did not use a more traditional EOF analysis to capture other modes of climate variability. The approach that the authors use appears to be somewhat ad-hoc.

4) The issue of multi-decadal variability in the structure of the NAO that the authors discuss is a very interesting one that has been identified previously using the sea-level pressure field from the 20CR. For example, please refer to Moore et al (2013, J. Clim.). A discussion of how the variability that is observed at 500mb maps into the surface variability would be a useful addition to the paper.

5) I found the quality of the figures to be very low. The colour map used to describe the teleconnectivity field does not have enough contrast to pick up the important features of this field. It should be revisted so as to allow readers to identify these features.

The size of the individual fields in Figures 2,3 and 4 should also be increased so that readers can see the important fine-scale features in these figures.

6) As commented on by another reviewer, the paper has a number of typos and awkward sentence structures that should be corrected prior to publication.

Interactive comment on Clim. Past Discuss., 9, 4987, 2013.