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Climate of the Past Discussions

Interactive comment on "Recent warming inconsistent with natural association between temperature and atmospheric circulation over the last 2000 years" *by* P. A. Mayewski and K. A. Maasch

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This paper uses ice core chemical measurements covering the last 9000 years from Greenland and Antarctica, and treats them as measures of different aspects of the atmospheric circulation. It then mainly concentrates on the last 2000 years, comparing these chemical data with published temperature reconstructions. The major conclusion of the work is that the recent warming is not accompanied by the expected (preceding) changes in atmospheric circulation seen in "natural" events, and that this is further evidence that recent warming is of another origin.

This is clearly an interesting approach to understanding the twentieth century in the longer context, and well worth exploring. Simple temperature trends can only be interpreted so far, and more detailed looks at the spatial patterns of change, and at the pattern of different aspects of the climate system (as used here), offers an additional way to diagnose the cause of change, especially in comparison to the predictions of climate models. I therefore support the thinking behind this paper, and within the material here, there is a paper that could be published in CP. However, I found the paper incautious: that is to say, it makes a number of assertions that are difficult to recognise in the data, and by stringing them together, reaches conclusions that seem unsupported. If the authors wish to proceed, they will need either to support their assertions by a more rigorous treatment, or else present much more cautious and limited conclusions. In particular, I do not believe that this paper establishes what the natural sequence of changes (atmospheric circulation, SH and NH temperature) is, and therefore it cannot be stated that the recent warming has an inconsistent pattern.

The first stage in the argument is establishing the chemical proxies as representing particular aspects of atmospheric circulation. As the authors state, this has been discussed in a series of earlier papers. These links are in some cases probably not as well-established as the authors suggest: the calibration period is often short compared to the period dominating the variability, especially in Antarctica where the reanalysis data can only be considered reliable back to sometime in the seventies; in some cases the mechanism for the association is not very obvious (I am not aware of any geochemical evidence that makes us expect nss K+ to be a marker for any particular source); and we do not know if the links found in the instrumental period are persistent. For this reason, I am not comfortable with the chemical markers being called out as the atmospheric circulation anomaly in the rest of the paper, as this seems to turn a hypothesised proxy into something much more definite. In fact, for the comparisons made in the rest of the paper, it is not necessary to convert the chemical signal into a circulation anomaly at all: it would be just as interesting to compare the pattern of change in SD Na and GISP2 Ca vs NH temperature at different time periods. So, for this part of

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the paper, I would just urge the authors to be cautious in making their translation from chemical to atmospheric feature through the rest of the paper. It then becomes possible to look at the relationships and phasing between the different measures without worrying too much what all of them mean.

The section about the 9000 year perspective (section 3.1) is mainly a description of Figure 2. I have a couple of small comments here:

* Page 334, line 16 onwards ("the most notable change...") is not very clearly expressed. I think the authors are simply trying to say that the change at 1400 AD (Greenland) is the most notable one, but it took me several readings to understand that it was not the change at 2400 years ago or 5000 years ago that I should be looking at. This should be clarified.

* The use of the word "mild" to describe atmospheric circulation is also a bit confusing (maybe "calm"?). Mild normally refers to temperature.

* The authors have adopted the b2K usage in this figure. I think this should be spelt out, since it is probably not yet widely understood or recognised.

The major problem I have with the paper is in sections 3.2.1 and 3.2.2. I take each in turn. For the "warming analog", firstly I struggled to see some of the changes described. While the NH warming at ˜800AD can be seen, the small blip at 500AD in the SH seems of no particular significance: it is followed by an almost equal cooling in the following century, and by a larger warming around 900 AD. I cannot see any reason to pick 500 AD as anything special. Equally, I cannot see any support for the statement that GISP2 nssK or Na weaken after 200 AD: all we can see is that there is a little extra variability around 150 AD (same with the GISP nssCa, where what we see is a peak in the 4th century, not a weakening from a higher background). To establish a change of multicentennial significance (which seems to be the intention here), a change in level must be found, and that does not seem to be the case. Finally, even if these were level changes, we have no way of knowing that they are in any way causally connected,

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so it is much too strong to state that atmospheric chemistry (or circulation) precedes SH temperature precedes NH temperature. If there was a pattern where several NH warmings all showed this pattern then a case might be made but that is not the case here.

The same applies for the “cooling event”. There is apparently a SH temperature peak in the 10th century (if we believe the SH reconstructions this far back to this level of detail), but it is an exaggeration to describe the end of this as a “cooling at 1000 AD”, and to link it with a supposed NH event at 1400 AD. One could make at least as good a case for linking the small SH warming at 1300 AD with the 1400 AD NH cooling (on theoretical grounds an antiphase response can be justified as easily as an in phase response).

My issue is that, from complex timeseries, which have ups and downs as well as some jumps and trends, the authors seem in several cases to have picked points which don’t seem obvious; from that they have built a sequence of events and phasings that I am not convinced is real. Perhaps they can give a statistical justification for their choices, but otherwise a much weaker interpretation is all that is possible.

Finally, the point is made that the sequence of events for the recent NH temperature change is different from that of the last natural warming, and it is suggested that this is additional evidence that the recent warming is not natural. Based on the discussion of the natural “warming event”, I am not convinced the authors have described a real sequence of linked changes in the different proxies for the natural event. However, even if they have, there is only one natural warming described: we would have no way of knowing whether this was the rule, so we cannot read anything significant into the fact that the recent warming shows a different pattern.

Interactive comment on Clim. Past Discuss., 2, 327, 2006.