

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

P. A. Mayewski and K. A. Maasch

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We thank the reviewer for her time.

Perhaps the over-arching issue raised by the reviewer is the fact that we used the Mann and Jones hemispheric reconstruction of temperature rather than the stable isotope - temperature reconstructions developed from ice cores. We chose to use the hemispheric reconstructions as noted in the paper because: (1) many previous papers (including some authored by us) have already addressed the association between ice core derived temperature and atmospheric circulation for regional scale changes; (2) ice core derived atmospheric circulation reconstructions for features such as the west-

erlies represent significantly larger geographic areas than ice core derived temperature reconstructions; (3) changes in atmospheric circulation are strongly associated with change in temperature (eg., monsoonal systems, shifts in the path of the westerlies) hence the examination; and (4) this paper is intended to provide an investigation of hemispheric scale temperature change as occurring with modern greenhouse gas rise compared to pre-anthropogenic analogs for warm and cold climate

Referring to the details of the review we offer the following responses:

The reviewer suggests that the paper is too long and sometimes speculative. Oddly a previous version of this paper was submitted to CPD and the editor requested that we add more material before the paper could be published in CPD. Further, as evident from the following, to address the reviewer's comments would require a considerably longer paper? As to sometimes being speculative we have provided all of the cited references and summarized the statistics from these papers for the atmospheric circulation reconstructions utilized in our paper and offer all of the temperature - circulation data and statistical comparisons. Our conclusions are drawn from direct examination of the data. We do not necessarily attempt to explain the controls on the changes since that is not in the scope of this paper.

We are cautioned by the reviewer concerning the reliability of the hemispheric temperature reconstructions from Mann and Jones and the temporal persistence of teleconnections. We utilize the heavily scrutinized results from Mann and Jones (2003) because they are the best composite approximation of hemispheric temperature available. Attributes and deficiencies in this reconstruction are addressed in the Mann and Jones (2003) paper very elegantly and in many following papers. As to the temporal persistence of the teleconnections we choose only the last 2000 years for investigation to avoid stretching the limits of modern boundary conditions (save the forcing in question - anthropogenic impacts because that is a primary test in the paper). Further atmospheric circulation features such as an Icelandic Low, westerlies, etc may change in strength and location over time but they are part of the basics physics of the climate

system. Movement of heat and moisture through changes in atmospheric circulation are also part of the basics physics of the system.

It is not clear how we can “make it shorter but add clear statements about the uncertainties”. The primary uncertainties we can present are related to dating of the records utilized. These are all covered in our paper or cited papers. As to other uncertainties we can only guess that the reviewer might mean the resultant calibrated values for temperature (addressed in Mann and Jones (2003) or atmospheric circulation (addressed in cited references and summary statistical correlations included in the paper)? Uncertainties related to the presence of other climate factors such as sea ice extent, cloudiness, etc would be wonderful to include but we do not have 2000 year long reconstructions for these parameters and neither does anyone else to our knowledge.

“The authors must introduce what they call as “natural variability”, “forced variability”, and “abrupt changes” regarding their proxy records.” We do not understand this request since our paper builds upon previously published papers that define Holocene abrupt change events (eg., synthesis by Mayewski et al., 2004, QR summarizing >50 paleoclimate records). Re natural vs forced variability does the reviewer mean naturally vs anthropogenically forced as we suggest in the paper or does the reviewer mean that some variability has no apparent forcing as in the case of noise? Our paper does not address the issue of forcing directly rather it demonstrates associations between temperature and circulation during three separate times: naturally cold and warm analogs compared to present.

“There should be a section on the expected relationships between hemispheric temperature and changes in circulation.” There is such a section in the introduction to our paper where we describe referenced past associations (see refs in our intro to: Mayewski et al., 1997; Thompson and Wallace, 2000; Bertler et al., 2004; Masson-Delmotte et al., 2005; Schneider et al., in review (now 2006)).

“I (the reviewer) would not expect Ę. changes in patterns of NAO to induce large temp fluctuations at the hemispheric scale”. Neither would we nor did we say this. We said that changes in the intensity of features such as the Icelandic Low portion of the NAO, the westerlies etc all of which deliver heat and moisture come before or during hemispheric changes in temperature. They no doubt come very close to the actual timing of changes in temperature on a regional scale (eg., Icelandic Low and North Atlantic temperature change). There are most certainly amplifiers at play but we do not have proxy reconstructions for cloudiness or sea ice and to invoke their involvement would make our paper more speculative.

The reviewer suggests changes to the abstract but offers no reason for the suggested changes. If a rationale for this change were to be provided we would be happy to consider changes.

“Luterbacher et al. have published several papers on the use of historical records to reconstruct European atmospheric circulation and temperature ĘĘ” We are familiar with these papers and agree they are very good contributions but our paper is not intended to be a summary of such findings (making the paper yet longer) rather we are addressing large scale atmospheric circulation vs hemispheric temperature in light of modern warming and we choose to use for the atmospheric circulation reconstructions our ice core records and in the process present new data that we feel will be of interest and value to the scientific community.

“The word “naturally forced” suggests a good understanding of the forcings and response. To my (the reviewer) knowledge this is not the case for any rapid change of the Holocene apart from the 8.2 event.” We are not sure why the use of the term “naturally forced” would invoke anything more than the idea that something is naturally vs non-naturally forced where natural forcings include insolation, solar variability, aerosols, natural oscillations. These natural forcings appear in numerous papers (eg., Hansen and many others). With respect to the 8.2 event being the only one for which the actual forcing is known or the only rapid (abrupt) climate change event in the Holocene there

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is certainly a well-respected contrary view. The causes of the 8.2 event are not fully known, although there may be intriguing possibilities related to fresh water discharge, and this is certainly not the only abrupt change event of the Holocene (see summary of Holocene events and forcings cited in our paper under Mayewski et al., 2004 - this paper includes many other papers referring to several abrupt events in the Holocene (eg., work by Denton and Karlen, Bond, O'Brien, Mayewski) plus various summaries done by numerous authors in research projects such as the NSF ESH project..

Although the reviewer suggests that we do not give the timing of the abrupt change events it is one of the primary results of the paper. We offer the onset timing for circulation change and temperature change for recent analogs of warm and cold climate.

The reviewer suggests that we cite the US Academy of Sciences Report analyzing the Mann and Jones paper that was published in 2006. We would have been happy to do so but our paper has been in the CPD process for almost a year and precedes the NAS report publication date so it could not have been included.

“There is a general ambiguity in the text because there is a potential to discuss polar temperature changes as indicated by stable isotopes of water from ice cores, which is never used.” We understand that the reviewer specializes in temperature reconstructions from ice core isotope series and she has produced along with others several fine papers. As explained in our paper and in the beginning of this response we are addressing a different issue, notably hemispheric temperature.

“Ë. Mention representativeness of GISP2 and SD records compared to other Greenland and Antarctic sites spanning the Holocene.” We do discuss the representativeness of the GISP2 and SD records by presenting the atmospheric circulation calibrations. This is exactly the point of the instrument-based calibrations we utilize in our reconstructions. The GISP2 record and its GRIP counterpart are still the highest resolution, best preserved reconstructions of the Holocene available from Northern Hemisphere ice cores. We do not have access to high resolution Antarctic ice core records cov-

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ering the Holocene other than SD and Taylor Dome. If this data exists, notably with calibrations, we would be more than glad to include it in our future work.

“Sources of uncertainties in the relationship between isotopes and temperature in polar cores could be mentioned.” We are not sure why since we do not use use polar ice core temperature reconstructions in our paper?

“There should be discussion of source, transport and deposition impacts on glacio-chemical records.” We do discuss source (eg., seasalt) , transport (circulation calibration), and deposition (see flux vs concentration discussion in our paper) and all with cited references.

“Please discuss the impact of the resolution of the records on the detection of the natural variability for the Holocene vs the high resolution records of past millennia.” We do discuss the sample and data resolution of our ice core records. It is higher resolution than that for pre-Holocene (for example GISP2 and SD are sampled sub-annually for the calibration era, at ~2.5 years per sample for the Holocene), and although not discussed in our paper the deglacial and younger end of the ice age are sampled at 2.5-15 years (see citations for details unrelated to the purpose of our paper). Does the reviewer mean by this question that the resolution difference impacts the temperature - circulation association? If so the best test would be to look at other recent high resolution examinations of circulation vs temperature during the last few centuries (see references in our paper to for example: Schneider et al (2006) and Bertler et al.(2004)).

Reviewer’s comments 2.2 - We (authors) are not sure what is meant by coherency between chemical and water isotopic records since we do not use water isotopic records in our discussion? All other comments re origin of dusts etc are dealt with in cited references and would only lengthen the paper in opposition to the reviewer’s request that we shorten the paper?

“The introduction paragraph is very unprecise, and mixes different time ranges.” The intent of and order within the paragraph is clearly stated as follows: (“We focus here ini-

tially on the perspective gained from examination of the full Holocene record, followed by more detailed examination of the last 2000 years of the climate change record.”).

“Discuss the detection of the events in local temperature (stable isotope records).” This is not the intent of the paper since we are comparing hemispheric temperature. Other papers cited provide some of this information (eg., Bertler et al., 2004; Schneider et al., 2006).

“Discuss regional representativeness of selected sites.” The reviewer asked this in an earlier portion of the review. See response above.

Give the timing of the events (asked above and responded above). “. accumulation impact” (asked above and responded above - see our discussion of flux vs concentration in the paper).

“What about anthropogenic effects related to NH land use changes?” What about them - not in the scope of this paper??

“Place clearly the fluctuations of the past 2000 years in a Holocene perspective.” - Please see Figure 1 and related discussion that places the last 2000 years of our atmospheric circulation reconstruction in the perspective of the last 9000 years?

“3.2 should be termed “atmospheric circulation and hemispheric temp change over the past 2000 years.” as opposed to the 2000 year perspective? Sure why not?

“Sections 3.2.1, 3.2.2, 3.2.3 should be combined.” In the opinion of the authors this would make reading less clear and no rationale is given.

The reviewer’s comments re 3.2 and 4 are very sketchy and jump all over in time, space, and process. The reviewer mixes comments re stadial events, gas fractionation issues, chemistry records. In some cases the reviewer revisits for the second and in some cases the third time issues raised earlier for which responses are provided above. We do not understand why the reviewer requests that the methodology used to obtain figs 5-8 be described - it is in the accompanying text along with the results.

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The plots are straightforward correlations between hemispheric temperature and atmospheric circulation for pre-anthropogenic era warm and cold intervals and modern era. The reviewer suggests that “the hemispheric temperature increase over the past few decades is not accompanied by an unusual signature in terms of circulation”. This is an oversimplification of the primary results of the paper.

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

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2, S405–S412, 2006

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