



Interactive comment on “Inter-hemispheric linkages in climate change: paleo-perspectives for future climate change” by J. Shulmeister et al.

Anonymous Referee #2

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GENERAL COMMENTS:

This paper presents (mostly) previously published work to illustrate some of the research done in the context of the PEP projects of the PANASH program. It focuses on three topics, namely: patterns and timing of deglaciation from the LGM; Holocene evolution of the El Niño Southern Oscillation; and a brief section on the Antarctic Cold Reversal. In addition to presenting some of the results of these pieces of works, the authors provide some of their own interpretations. Although each of the topics highlighted is of broad interest, and the paper presents some interesting ideas, I found it to fall somewhat uncomfortably between stools. More specifically, I felt that the work fell short of the comprehensive and objective approach you would look for in a rigorous review of a subject area, and that, on the other hand, some of the authors' own ideas were presented somewhat dogmatically without adequate supporting evidence.

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For those who are already well versed in the debates surrounding the selected topics, this does not present a problem; the readers will be able to spot the ‘opinion’ as opposed to the ‘fact’. However, for the broader audience I suspect that the work could be misleading, giving the reader the impression that some of the ideas presented rather speculatively have more evidence behind them than they at present do. Therefore, the paper should be read with a ‘health warning’: there are some interesting ideas here, but not all scientists working in these areas would endorse some of the interpretations.

SPECIFIC COMMENTS:

SECTION 2: Timing of the LGM and deglaciation in the Southern Hemisphere tropics and mid-latitudes: In this section, the paper focuses mostly on terrestrial evidence for the timing of glacial advances and retreats and for temperature changes through the LGM-deglacial transition. This is based on previous work, and on some unpublished data. I have a number of general and specific points:

a) On page 83, paragraph 2, (i.e., before the section begins) the stage is set by saying that the LGM-modern transition was as large, or larger than climate change predicted under future greenhouse warming. To be balanced, I suggest that the authors note that the difference is at least an order of magnitude in terms of rate, i.e., future change of a few °C will take place over the next century, whereas the glacial-interglacial warming of similar magnitude took thousands of years. This is an example of where the text could be misleading to the casual reader; the LGM-Holocene transition is not a good direct analogue for future warming. Following in the same paragraph, the authors tell us the timing of the initiation of deglaciation in Antarctica and Greenland without any discussion or references. Again, I think this could be misleading. As has been discussed by plenty other authors, it is hard to be precise about a ‘simple’ onset of deglaciation, particularly in Greenland, due to the presence of the substantial millennial timescale variability. In this manuscript, the onset of Greenland deglaciation is given as about 14,700 years ago, at the onset of the B/A warming, but without reference to the fact that the cool temperatures centred at around 16-17,000 are probably related

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to Heinrich event 1 (H1) and that an alternative scenario is that deglaciation may have initiated earlier and was briefly punctuated by a reversal during H1.

b) Page 88, lines 2-19: The presentation of the Williams et al record needs at least some mention of how to go about interpreting it. As the original authors went to pains to explain, this speleothem record is difficult to interpret in an unambiguous way due to the variety of factors that can influence the $\delta^{18}O$ of the calcite. This makes it hard for the reader of this review paper to make sense of the graphs in Figures 3 and 7.

SECTION 3: Changes in ENSO through the Holocene: Here, largely published data is used to support the concept that ENSO has changed its frequency through the Holocene, and to suggest that it is the precipitation part of the system that change, not the SST part. Here, I have some serious misgivings about the way the terms “ENSO” and “frequency” are used. Several of the records used cannot distinguish the underlying frequency of interannual ENSO dynamics, and are just as likely to be telling us something about changes in mean background conditions and in the occurrence of events over some threshold in size. Since this seems to be a common area of confusion, I expand on it below.

a) Frequency of ENSO through the Holocene: The authors suggest that there is strong evidence to support the idea that the underlying periodicity of ENSO has shortened through the Holocene. The authors need to much more carefully define what it is that they are seeing in the data, and to carefully distinguish between true changes in the interannual frequency of the system, as opposed to changes in the amplitude of events. More specifically: the important Pallcacocha lake records do not have annual laminations; the laminae relate to intermittent influx of terrigenous sediment (probably during rainfall and run-off events), and their frequency is determined by interpolation between radiocarbon-dated samples. In addition, the ‘El Niño’ signal in them is dependant upon local conditions crossing some threshold in order to be recorded. This makes it very hard to use the record to separate out the different effects of changes in interannual frequency, changes in amplitude and changes in background conditions (e.g., to more

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El Niño-like or more La Niña-like). For example, take a scenario where a (hypothetical) climate oscillation has a fixed periodicity of 4 years, but where cycles randomly vary in amplitude one from another within a reasonable range (i.e., a system like a very regular ENSO). In this scenario, during an interval of generally high amplitude variability, it is possible that all cycles will cross some threshold, and therefore be preserved in an event-driven sediment archive. Subsequent analysis of the archive in the frequency domain will produce a simple concentration of variance at 4-year period. If we keep the threshold level the same, and keep the same underlying 4-year period, but reduce the amplitude of the signal, only the larger cycles will cross the threshold and be recorded. Frequency analysis of the resulting sediment record will now suggest a lower frequency occurrence of events. But notice that, in reality, the underlying frequency has remained the same, it is just the amplitude that has changed. This is clearly a crucial distinction to make. In my view, the original papers of Rodbell et al, and Moy et al. were careful not to talk about changes in inherent frequency of the interannual system. The current authors need to do the same.

b) The Rein et al marine record has an average temporal resolution of about 1 sample/4 years over the past 20,000 years, and has a period of non-deposition/erosion during the crucial early-mid-Holocene period. Furthermore, for MIS2, the resolution is about 1 sample/decade. Therefore, it is misleading to talk about change in “ENSO frequency” in MIS2 (page 94, line 15). Rather, the record may indicate that it was wetter during part of MIS2, which may mean more ENSO events crossing some threshold, but could also be interpreted as more El Niño-like average conditions.

c) Temperature vs. precipitation signals and the strength of ENSO through the Holocene: On page 95, then in lines 2-5 of page 99, the authors explore the idea that the SST component of ENSO may still have been relatively strong during the early-mid-Holocene and that is was just the precipitation anomalies that were reduced. Although this is an interesting idea, I cannot find the data to support this assertion. For example:

- The presence of significant ENSO-related SST anomalies in the WPWP in the early-

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mid-Holocene appears to be largely based on unpublished data which are not shown here (page 95, line 15), and on short coral records that cover only a few decades at most. The SST anomalies associated with modern El Niño events in the core WPWP region are small (about 0.5 °C), and quite variable in amplitude from event to event, and on interdecadal timescales. Therefore, to demonstrate that the SST anomalies associated with ENSO in the early-mid-Holocene were similar to what they are at the present-day would require long coral SST records (>100 years?) from both modern and early-mid-Holocene corals at the same site. At present, Sr/Ca is the best tool for reconstructing such temperature changes, and I am not aware of any such long records that can be used to address the issue. I suspect that the signal to noise ratio may be so small as to make this a challenge. Therefore, although this is an interesting idea and worth pursuing, at present I think that it is just as likely that the SST-part of the interannual ENSO system was also significantly subdued during the early-mid-Holocene.

d) Page 92, lines 2-18: description of the ENSO phenomenon. I doubt that most dynamicists concerned with ENSO would be so confident about the simple split of ENSO into two distinct modes and of the role of solar forcing. The referenced papers (especially the Nuzhdina and Tomita and Yasunari ones) are not particularly highly cited compared to many others discussing the controls on ENSO frequency and strength. Therefore, once again, since the paper has the air of a review, I am concerned that the casual reader may pick up a rather false impression on the current state of knowledge in this field.

e) ENSO and forcing: On page 99, lines 16-18, the authors state that “The available records indicate that ENSO is riding on the back of longer-term oscillations in the tropical general circulation (e.g., the Interdecadal Pacific Oscillation) driven by external forcings (e.g., solar forcing)”. I think this statement is sufficiently vague and open to misinterpretation that it is unhelpful.

f) Conclusions: Implications of PANASH work for future management: This section does not contain any specific details on how to use paleoclimate research for manage-

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ment purposes beyond the obvious statement that the past can provide useful insights into the operation of the climate system and provides a test for models used in future climate prediction. Therefore, I would scrap it.

TECHNICAL CORRECTIONS: Page 86, line 27: remove second word (“the”) Sections 2.1.1 and 2.1.4: why not combine these? Give Latitudes and longitudes of all sites. Page 88, line 5: explain what is meant by “nadir” and how to interpret this (complex) speleothem record Figure 6: In black and white so I cannot distinguish the sites. Also, you need to be more specific about how the reduction in ENSO strength has been estimated from the records. Page 97, lines 19-20: How do you know that ENSO activity stepped up from these records? Page 98, lines 1 and 2: Where is this “earliest flickering” of a signal in the New Zealand record in Figure 7? Ę nothing stands out particularly at 6,000 years ago. Figure 7: what are the grey shaded bars? Page 104, lines 1-2: In the data presented and papers cited, I do not see the support for this bold statement: “.. the core ENSO system probably operates continuously through the Holocene Ę”. Figure 2: Fig 2a is based on unpublished data so it is hard to assess the confidence that should be placed on the interpreta

Interactive comment on Climate of the Past Discussions, 2, 79, 2006.

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