

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

**J. Shulmeister et al.**

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Overall comments:

The biggest single difficulty in writing this paper was how to approach it. We were tasked with giving an overview on PEP science and highlight how it contributed to future management. It rapidly became apparent that a comprehensive overview was virtually impossible, because of the geographic scale of the work; the disparate subject matter and the difficulty of getting the community buy in to such a review. Instead we chose to focus on the overlapping interests of the authorship group. We are very explicit that this is what we have done and that we have exemplified rather than completely surveyed the work done in the PEPs. Because of the topics we chose (ENSO and the LGM and deglaciation) we have actually covered about ½ the planet in our geographic coverage and we have genuinely covered inter-hemispheric linkages.

Because we had the ongoing research results of three separate groups in hand, there is an appearance that a significant amount of new material is presented (which both referees commented on and referee 3 thought inappropriate). In fact, there is almost no unrefereed material presented in the paper. The few instances where unpublished materials are referred to are very clearly so labeled. What there is, and what we unashamedly defend, is our synthesis on how these data might be interpreted. A simple review, without such a synthesis, would be uninformative. We are happy to highlight where the views expressed in this synthesis are either new perspectives or not universally held and have amended text throughout the paper to achieve this.

## ANONYMOUS REFEREE #2

Specific Comments:

### SECTION 2

a) It is a fair point that the predicted rate of change under greenhouse warming is lower than during the deglaciation. We have reworded to acknowledge this.

We have also added a reference to support the timing of deglaciation in Greenland (Rasmussen et al., 2006). There may well be structure in the Greenland record and the cooling at 17-16 ka may be associated with the H1 event but there is widespread agreement that the glacial termination in Greenland can be placed at about 14.7 ka, whereas the glacial termination in Antarctica is earlier. We have altered the text to identify the 14.7 ka event clearly as the glacial termination. The first section in our paper is set up as a case where we can make an alternative interpretation to the widely accepted norm, so we have explicitly acknowledged that other interpretations are possible. We quote from the conclusion of this section "Our purpose, however, is not to directly dispute the understanding of patterns of global change. It is to emphasise the extent of the uncertainty, given the limitations of the available paleoclimate records."

b) Several sentences explaining the complexity of interpretation of the speleothem

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record have been added to the text at first mention of the Williams et al., 2005 paper.

### Section 3

(a) We have acknowledged the issue of thresholds and changes in amplitude and have added the following paragraph.

A limitation of the Lake Pallcacocha record is that the banding is non-annual and the bands reflect individual sedimentation (precipitation) events. Consequently, there is a minimum threshold that needs to be exceeded before a band will be deposited. Changes in the amplitude of ENSO events could mimic the effect of changes in periodicity, if the changes in amplitude fluctuate around the threshold value. Changes in amplitude are interpreted to be present in the record.

(b) We don't actually talk about changes in ENSO frequency in this paragraph. We simply note strong ENSO frequencies during MIS2. We see no change required.

(c) This is one instance where we stand by as yet unpublished results. The published records may only cover windows of decades at different times, but we reject the claim that continuous centennial scale ENSO records from corals are required to validate statements of similarity of event styles.

(d) In retrospect, given the likely use of this paper by non-specialists we agree that the inference of insolation control is too speculative and we have dropped the reference to Nuzhdina and consequentially to Enfield and Cid and tidied up this section. The key paragraph now reads;

The bandwidth for modern ENSO oscillations is 2-7 years. Changes in the dominant period of ENSO variability can be identified from the analyses of instrumental records (e.g. Setoh et al., 1999; An and Wang, 2000) as well as predicted conceptually from modelling (Lau and Sheu, 1988). It should be noted that some climatologists and oceanographers (e.g. Federov and Philander, 2000) sound a warning on the existence

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of multiple modes of ENSO periodicities, pointing out that they may indicate changes in longer term background climatology, modifying the apparent expression of ENSO rather than the phenomenon itself.

The reason for the citation of the relatively little known Tomita and Yashunari paper is because they are about the only authors we can find to actually follow through and describe different climatological impacts associated with different ENSO modes rather than simply describing the different modes. Because of this, we still regard this paper as valuable but have added a cautionary rider to the text. “This bi-modal ENSO of Tomita and Yasunari has not been widely recognised and the example is provided as a case study of how variable ENSO periodicities could impact regional climates”.

(e) We have simply deleted this sentence.

(f) We have deleted the title to the section rather than the section itself. It is now simply Conclusions. We agree that specific details on how to use PANASH work for future management are missing but we disagree that this section is valueless, in fact we consider that we have made some useful points, some obvious and some less so. The referee correctly pointed out to us in various places that we are dealing with a more general audience for this paper. Some of the conclusions are apparent for paleoclimatologists but other readers are likely to need to have them spelled out.

### ANONYMOUS REFEREE #3

See the intro paragraph to this response for our response to the general comments on the approach to the paper. We are unwilling to recast the entire paper.

The claim by the referee that new work is presented on the Antarctic Cold Reversal (ACR) and ENSO variability is inaccurate. All the basic information in this story is published in Pepper et al., 2004 which is cited extensively in section 4.1. Specifically the link between the ACR and ENSO is highlighted in that paper. We have elaborated a little on the story here but there is no new information, though some different insights.

We struggled with contribution of PANASH/PEP science to future management and accept the criticism that in trying to make it appear more relevant that we may have over-stepped the mark. We believe that paleo-science has a big contribution to make to future management issues. Most paleo-science is still in too early a phase, however, to so contribute. We have amended the conclusions accordingly.

Abstract:

Acronyms: The paper is a review of PEP/PANASH science. We can hardly write such a review without mentioning the basis for the review and unfortunately that means acronyms. We have explained the acronyms used.

We disagree with this referee on whether the ACR is an abrupt event. As the referee notes, it is widely identified as such in the literature. It may be that for NH workers they are calling it abrupt because of the idea that it reflects an Allerod warming, that is for NH based scientists to decide. We used the term abrupt because it manifests as an abrupt change in the SH records we are familiar with, especially away from Antarctica.

P84/p85 - The statement that “with the dramatic reduction in the revised extent of the LGM comes a new calculation of the ELA depression” actually explains the basis of any major change. The moraines that were previously identified as LGM are older and therefore the LGM glaciers extent was considerably smaller. Hence lower ELAs.

P90, line 10 - The referee is in a minority position challenging the 14.7 ka deglaciation, though the referee may well be right. We cite 15.5 ka because that is the low point in Figure 3, the switch to deglaciation is at 14.7 ka which is why we state after 15.5 ka. We have clarified this point in the text.

P90, 13-14. We fundamentally disagree with the referee on this point. One of the major debates in paleoclimatology has been on the nature of transfer of Northern Hemisphere signals to the Southern Hemisphere based on either atmospheric or thermohaline transmission mechanisms. An alternative view is that the SH leads the NH. Here

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we suggest that the polar regions may not be driving the system at all. We would call this a challenge to basic assumptions.

Page 100. The GCM acronym means both Global Climate Model (Lamont-Doherty have one of these for example- EdGCM) and General Circulation Model. General Circulation Model is a better usage here and has been changed.

Page 89 last line through page 90. The referee raises a whole series of interesting points. The points are excellent and we agree with most of them. However, the request for an in depth discussion of the physics of how long it takes glaciers to expand in the various regions and how temperature and precipitation interact is beyond this paper. These are not review data but the foci of the ongoing research. Of course our findings don't challenge the NH Milankovitch metronome and obviously SH climate change cannot be explained by greenhouse gas changes on their own.

Page 98+

We disagree that the linkage between the lake record and ENSO is tenuous. The role of ENSO in the modern New Zealand climate may not be well recognised outside New Zealand but its role in the NZ climate is very well established (e.g. Gordon, 1985, 1986 and numerous references thereafter (especially by Kidson and by Renwick)). ENSO is the principle EOF in most analyses of the New Zealand inter-annual climate. Auckland and the surrounding regions have significant SO correlations ( $p < 0.05$ ) for temperature in Spring, Summer and Winter and for rainfall in Spring and Summer (Gordon, 1986). It just misses significance in Winter. The regional climate of Auckland is dominated by SW flows and the pressure anomaly maps (fig 11) in Gordon 1986 highlight the susceptibility of this flow to SO variations. We have now added this detail to the paper. Consequently, the main things that we need to show for the maar lake are that 1) the system is such (i.e. a closed, biological productivity based, system) that it should respond to a climate forcing and 2) that the only other likely candidate for such a forcing (insolation) is not viable. We have done both.

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The spectral amplitude of the inset in Figure 7 categorically does not rise above noise level. That is the whole point of the inset and obviously it does not come across. We have revamped the caption to try and more clearly explain what it means. There are a large number of proxy records which suggest no/very weak ENSO signal in New Zealand (and South America) in the Early Holocene. This figure supports this hypothesis.

We have explained the use of shading in the Figure 7 caption. The referee points out that “If the coherence between the Ecuador and NZ records is correct, then these data do add to the evidence for ENSO variability through the Holocene”. This seems inconsistent with the next statement that these same data do not “tell us anything fundamentally new.” The referee follows with “The idea that seasonality plays a role is believable – but not fully developed enough – to be particularly convincing”.

We are presenting ideas backed up by the available data. We agree that the paper hardly represents a final view on these issues but surely this is what synthesising is about – drawing new conclusions from the data we have reviewed. We are happy to highlight that these are our views and ideas rather than facts.

See P98+ point for ENSO influences on NZ

Because of the use of a rolling window, the application of a single confidence level to the whole of the PSD diagrams in Figure 9 is inappropriate. Instead each window should be analysed separately. This is difficult to represent on a single revised figure. Instead we have added a figure showing the significance results for the rolling window including the Antarctic Cold Reversal to the revised manuscript. It demonstrates that the 5-7 year period PSD peak in this window is significant at 3 sigma. We have also examined the other rolling windows and the obvious spectral peaks in ENSO bandwidths are significant at 2 sigma with the highest peaks significant at 3 sigma. In contrast, and unsurprisingly, there are no significant peaks in the Early Holocene diagram, or away from the two distinct events in the deglaciation part of the figure. If required we

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could supply a series of figures to demonstrate statistical significance for spectra in each rolling window through the figure.

Given that the referee will now (hopefully) accept that the change in amplitude in the 7 year band variability is meaningful, then the linkage to the westerlies is acceptable to the referee. We agree that there is certainly an Antarctic-Oscillation (AAO) like signal in the data. The AAO and ENSO phenomena are already widely recognised as linked in the modern climatological literature (e.g. Fogt and Bromwich, 2006; L'Heureux and Thompson, 2006 as well as the Bertler et al., paper already cited and many others). It is not a question of one or the other. To highlight this point we have added citations to the modern AAO/ENSO linkage [the AAO appears in many of these papers under one of its other guises - the Southern Annular Mode (SAM) and the High Latitude Mode (HLM)].

#### RESPONSE TO COMMENT BY R. GANJOO

We recognise that the Himalaya region contain excellent and exciting records of climate change, including records covering the deglaciation. We simply focussed on South America and Australasia for our discussion as it is where the authors have expertise. The lack of mention of the Himalaya was in no way intended to imply that records from that region are unimportant.

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Interactive comment on Clim. Past Discuss., 2, 79, 2006.