

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

### **Anonymous Referee #3**

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This paper is an attempt to provide a comprehensive review results from the Pole-Equator-Pole (PEP) program. Unfortunately, the paper suffers from trying to several things at once, and I find that it does none of them well. On the one hand it tries to review a large body of work, yet the work reviewed is defined largely by being part of the “PEP” program; a review that simply considered the best science on the subject, whether formally part of PEP or PANASH or not, would seem a better strategy to me. The result is that many important references are left out, giving the reader a rather uneven view of the state of knowledge on the latitudinal distribution of climate change. On the other than, there is an attempt to present new research – notably the claim that there is a link between the timing or character of the Antarctic Cold Reversal and ENSO variability. Such new work is out of place in a review paper. It is also quite unconvincing.

Finally, there is an attempt make definitive statements to illustrate the success of the PEP program in making “key contributions” to the science of climate change. Yet many of the claims made are in fact not definitive, and some are simply incorrect. Several other ideas are presented that are neither new nor have been particularly enhanced by the PEP program, yet are presented as such.

Overall, I think that the authors need to entirely re-cast the paper so that it better serves its stated purpose, which the title suggests is to review the evidence for the interhemispheric pattern of climate change in the past, and to show how these results inform our understanding of the climate system. I would strongly encourage them to try to better partition well established fact from as-yet unproven hypotheses, and to take care not to overstate the relevance of the results (on this latter note, I find the claim in the abstract that “PANASH science ... contribute[s] to the management and mitigation of” future climate change to be a great overstatement. It would be nice to believe that paleoclimate science of the type reviewed in here has a major contribution to make in our predictions of future climate change, and perhaps that case can be made. But simply stating it to be true does not make is so.

More specifically, I have outlined below concerns that I have with individual sections and statements within the paper.

### **Abstract.**

Avoidance of so many acronyms would be helpful here. The fact that the work reviewed was done under the auspices of PEP or PANASH is ultimately irrelevant to the advance of the science.

The Antarctic cold reversal (ACR) is NOT an abrupt event by meaningful definition. The use of this language is common in the literature, and reflects the idea that the ACR may be a response to the Allerod warming and Younger Dryas coolings (which ARE abrupt) in the Northern Hemisphere. As this paper demonstrates, the link between these events is by no means fully understood, and in any case the changes in the

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Comment

Antarctic are clearly much more gradual. As I will discuss further below, I am not at all convinced that the ACR somehow “modulates ENSO. This is an interesting idea but it is not constrained by the data, nor supported by theory.

Page 83, Line 11. It is stated that “Antarctica started to deglaciare at 17 000 calendar years ago whereas Greenland did not deglaciare until 14 700 years ago.” This statement is frequently made in the literature, but it is based on a visual assessment of when the coldest temperature (as recorded in ice cores) occur. I’ve always found this claim strange, because in fact the lowest  $\delta^{18}O$  values in GISP2 are around 22,000 years, somewhat earlier than in Antarctica. It warms up and cools down several times between 22 ka and 14.7 ka. On what basis does one define the most recent warming as the “start of deglaciation”? Alley et al. (QSR, 2003) looked at this more carefully a few years ago and concluded that the North actually leads the South on these timescales. At the very least this diametrically opposing view of the same data ought to be cited.

Page 83, Line 16-19. More consistent use of terminology to refer to the “thermohaline mechanism”, “deep ocean overturn mechanism”, etc. would make this paper more accessible to readers not already familiar with the confusing array of literature on this subject.

Page 84, Line 15. “The general pattern of the ages is compelling.” This is an exceedingly vague statement, that doesn’t tell the reader anything.

Pages 84 – 85. I find the discussion of the revised ELA results intriguing, yet this review tells the reader very little about why we should believe these results. Rather than expecting readers to go back to the primary literature that it cited, it would be very useful to detail the key findings that lead to a revised ELA estimate. This seems to me one of the more important results from the PEP project, and deserves greater attention in this review.

Page 90, Line 10. The same problem applies here as in the abstract. The evidence that “Greenland didn’t warm” until about 15 kais a highly subjective interpretation of the

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data. It is stated here that the relevance data is 15.5 ka, yet in the Abstract it is 14.7 ka! I am skeptical in any case that either statement is very meaningful.

Page 90, 13-14. “The significance of these observations is that they challenge the basic assumptions of climate change on which we base our predictions of global climate response.” This is a major overstatement. I very much doubt that these results will have any significant bearing on what we predict in the future. Modern general circulation models are quite independent of any assumptions about the relative timing of deglaciation around the globe.

Page 100, line 18-19. GCM means “general circulation model”, not “global climate model”.

Page 89 (last line) through Page 90.

It is stated that “A reasonable case can be made that the first step towards an interglacial climate occurred at about 18 500 cal yr BP in the southern midlatitudes.” I agree, yet I think other, perhaps more reasonable interpretations can be made. For example, why can't the data be explained by the relatively small size of the glaciers and ice caps in the midlatitudes, compared with Antarctica and the Laurentide? Surely one would expect deglaciation to occur much more quickly on small ice caps, so it is not at all clear that the data somehow require that climate change “begins” anywhere in particular. I am by no means suggesting that the old idea of globally synchronous glaciation is correct; certainly the data discussed in this paper show this to be wrong. On the other hand, the idea of “globally synchronous glaciation” in the literature reflects a rather careless conflation of *climate* change with *glacier extent*. Unfortunately, this paper makes the same mistake. I would really like to see a more complete discussion of the data, with a bit of physics involved. How long does it take glaciers to expand in different regions? What are the relative roles of precipitation and temperature? Do the results really compel us to abandon the fundamental notion that Milankovich forcing at high Northern latitudes is the primary driver of the ice ages? Can the Southern Hemi-

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sphere results be explained by greenhouse gas changes, as is conventionally done?

Page 98+ The link between the NZ lake records and ENSO is very tenuous: “we interpret these records to show variability in the southwest wind which, in the Auckland region, is strongly related to ENSO variability.” Data are shown that purportedly demonstrate strong coherence between these records and the data from Ecuador, but the statistics are not shown. (It is unclear what the unlabeled spectral analyses in the inset to Figure 7 are supposed to show; presumably this is to demonstrate the purported coherence, but the spectral amplitude is not demonstrably above the noise level). The visual comparison – Figure 7 – is intriguing, but the use of horizontal shading to connect different time periods in the records tends to be misleading. Only in the most recent (uppermost) shaded region is there any hint (to my eye) of a connection.

If the interpretation of coherence between the Ecuador and NZ records is correct, then these data do add to the evidence for ENSO variability through the Holocene. However, it is difficult to see – from this paper – how these data tell us anything fundamentally new. The idea that seasonality plays an important role is believable, but not fully developed enough in this paper to be particularly convincing.

Page 101+ The claim that there is an increase in ENSO influences on NZ during the ACR is simply not convincing. I agree that there is an increase in variability in the 7-year band, but it is not demonstrated that this is statistically significant. If one accepts that the change in amplitude is meaningful, then the idea that it is due to an increased westerlies is believable, but this is then an interpretation of the ACR as an “Antarctic Oscillation-like” change in atmospheric circulation, rather than an ENSO-related change. It is not clear how these changes would “amplify” the ENSO signal in New Zealand. Again, there are some interesting ideas here, but they are not very fully developed and therefore are not particularly convincing.

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