

# ***Interactive comment on “Hydroclimate of the Last Glacial Maximum and deglaciation in southern Australia’s arid margin interpreted from speleothem records (23–15 ka)” by Pauline C. Treble et al.***

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We thank Reviewer 4 for their thorough review, in particular, their helpful suggestions for strengthening the Introductions and Conclusions. Our responses are embedded below.

Anonymous Referee #4 Received and published: 22 February 2017

Treble et al. present new and invaluable speleothem data from the southern central arid region of Australia. The new data alone mean that this paper will represent a significant contribution to our understanding of late glacial climate variability in a chronically

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undersampled part of the Southern Hemisphere. The other reviewers have highlighted several questions/issues arising from this manuscript that warrant further consideration. I concur with these suggestions, whilst making some additional comments. The authors raise the conundrum of apparently wetter conditions in the Flinders Ranges coincident with a southwards migration of the subtropical front. This apparent contradiction leads to the interpretation that tropical sources may have led to the apparent increase in extreme precipitation, a central conclusion of the manuscript. I suggest at least two alternative explanations warrant consideration before this conclusion is made: Firstly, several papers (e.g. Kohfeld et al. 2013; Liu et al. 2015) have highlighted the disparity (a) between marine, dust and terrestrial moisture records and the perspective they place on the position and strength of the Southern Westerly Winds (SWW); (b) that in models that best represent SH LGM conditions, behaviour of the SWW and subtropical front (STF) appear decoupled or even inversely related, due to the shielding effect of sea ice on oceanic wind stress. Thus, although evidence from the Murray Canyons are interpreted to reflect changes in the position of the STF (De Deckker et al. 2012), we can be less certain that these changes track the position of the SWW. If true that the SWW and STF do not necessarily migrate in concert, then the apparent disconnect between a southwards migrating STF and increased precipitation in the Flinders Ranges becomes less controversial.

Response: We thank Reviewer 4 for this insight. Lines 107-109 point out the potential shortcoming of the marine data as a record for the westerly winds in the Introduction. We could add to the three hypotheses at line 804 a fourth possibility, that the marine record does not reflect changes in the westerly winds. However, we argue that it is not necessary to do so, as in laying out the evidence for our third hypothesis, there is firm supporting evidence from the comparison with tropical speleothem records and the Mohtadi marine record that the period of enhanced recharge in the Mairs Cave record between 18.9 – 15.8 ka is due to the enhanced availability of tropical moisture.

Secondly, whilst detailed discussion is provided re. the latitudinal origin of precipitation

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in the Flinders Ranges, little attention is given to seasonality. Could it be that the wetter phases observed during the LGM actually relate to an increase in summer precipitation, or summer humidity (lowering summer evaporation rates)? It's appreciated this is difficult to constrain, but it should be considered.

Response: The potential role of reduced evapotranspiration is considered in Section 5.2.3. An increase in summer precipitation would imply moisture from lower latitudes, consistent with our arguments.

Specific comments Line 67 'The nature and timing of climatic episodes'. This sentence doesn't really make sense. I don't think we can seek to redefine the timing of HS1 from Australian records – what the paper really aims to do is to investigate the nature of SH climate variability during episodes such as HS1.

Response: We will rephrase to 'Paleoclimate data from the Southern Hemisphere mid-latitudes during key episodes such as Heinrich Stadial 1 (HS1) is of high interest owing to the potential role of the westerly winds in driving CO<sub>2</sub> ventilation from the Southern Ocean and the impact of its release on global warming'

Overall, while the first two paragraphs introduce post LGM climate variability in the SH, and role of the ITCZ, they fail to highlight the important point here – that is that during events such as HS1, we might observe both oceanic and atmospheric teleconnections with the Northern Hemisphere, and they might impart different effects upon SH or Australian climate. Whether we agree with it or not, the bipolar seesaw hypothesis is widely cited, and if correct describes an oceanic teleconnection between the Southern Ocean and the North Atlantic which leads to a lagged period of SH warming during episodes of North Atlantic cooling and ice rafting. This in turn may effect the position and strength of the SWW. By contrast, H events may also lead to atmospheric teleconnections, e.g. changes in the position of the ITCZ in response to cooling in the North Atlantic. Those atmospheric shifts may/may not impact upon Australian climate, but if they do, presumably their effects would be different in terms of climate response

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and time lag. It's therefore important to consider at the outset how a mixture of these two effects would lead to changes in Australian rainfall or evaporation.

Response: We will re-organise the first two paragraphs; the first will introduce the LGM in the Australian environment and the second will focus on the opportunity to understand atmospheric and oceanic teleconnections between the hemispheres when global climate is provoked by cooling in the North Atlantic, by examining the climatic response in Australia over HS1. This will largely consist of reorganising the existing text in the first two paragraphs, with an additional sentence on potential oceanic teleconnections and a final sentence 'Evidence in the paleoclimate record for the nature and timing of changes in the Australian environment, can assist in the critical assessment of how the climate system reacts to complex feedbacks in the ocean/atmosphere system'. Several additional references will be given.

Line 93, up until now the discussion has centred around HS1, but the Kohfeld paper relates to conditions at the LGM, which is a bit of an unnatural jump. It would be helpful to specify which window of time we're interested in, or at least note that the Kohfeld paper is interesting as a potential analogy for HS1.

Response: Lines 92-96 will be moved up to the end of the first paragraph, consistent with focus on LGM in first paragraph. Paragraph 2 will now focus on HS1, as in response to the above comment.

Line 100. According to the International Hydrographic Organisation, the coring location of MD03-2611 is indeed within the Great Australian Bight. However, the Australian Hydrographic Service would disagree, defining the eastern boundary of the GAB as the tip of the Eyre Peninsula. This latter definition is certainly the one I am most accustomed to, and had always thought of the location of MD03-2611 as being in the Murray Canyon(s), which is how it's referred to in other papers on the same core (e.g. Moros et al. 2009). I would suggest that referring to this record as being from the GAB is slightly misleading to some readers.

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Response: We apologise for this oversight. We have checked De Deckker et al. (2012) and they use the term 'south of Australia' plus map and co-ordinates. To avoid confusion, we will remove 'Great Australian Bight' or 'GAB' throughout and replace with core name 'MD03-2611'. Line 102. In reference to the De Deckker et al (2012) paper (note De Deckker, not DeDeckker), be more specific as to which foraminifera you're referring to as proxies for the position of the STF. Response: We will add foraminifera species and fix spelling of De Deckker. Line 109-110. There is a circular argument arising here. Is the objective of this paper to examine the terrestrial hydrological response of changes in the westerlies, or is it to use an assumed teleconnection between the westerlies and terrestrial hydrological change to infer past changes in the westerlies? I don't think it's possible to do the latter if you aren't confident about the former. Response: We believe that our wording has created some unintended confusion here. We will replace 'A key assumption of this study ...' with 'A key assumption of the study by De Deckker et al. ...'.

Line 131. Spelling: Callabonna, not Callabonne. Response: We will make this correction.

Line 133. On the subject of mega Lake Frome (and indeed Torrens, Eyre), have you considered the effect of these lakes being filled and the  $\delta^{18}\text{O}$  of precipitation that would result from their evaporation? Response: This is a really interesting point but unfortunately something that could only be adequately investigated with a modelling study. Our speleothem  $\delta^{13}\text{C}$  and Mg/Ca data support a period of enhanced recharge between 18.9 and 15.8 ka, independent of  $\delta^{18}\text{O}$ .

Line 152. 'climate-speleothem  $\delta^{18}\text{O}$  signal' – here, I think you mean 'climate-speleothem  $\delta^{18}\text{O}$  relationship/response' or similar. Response: We will rephrase to 'the complexity of the response of the speleothem  $\delta^{18}\text{O}$  signal to climate'

Line 236. Missing full stop after 'years' Response: We will insert period.

Line 234. I suggest: 'is one of the most significant hydrological episodes in..' Response:

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(L243) We will insert 'hydrological' as suggested.

Line 268. Capital 'F' in 'For example, 3 m of water..' Response: We will capitalize.

Line 286. 'Rapid connectivity'. I'm not sure this is a phrase. By this, do you mean 'rapid throughflow' or 'greater connectivity'? Response: We will replace 'rapid' with 'greater'.

Lines 308-314. This paragraph largely repeats what was said in lines 264-276. Response: We will integrate with earlier text to remove repetition.

Line 347. Insert 'a' before slit? Response: We will insert 'a'.

Lines 353-354. ': : standardised to using NIST 612: : :'. Delete either 'to' or 'using' Response: We will make this correction to read 'using'.

Line 362. Delete 'The' from 'The petrographic..' Response: We will make this correction.

Line 374. '.. examined in thin section and fabric and possible dislocations: : :'. Clunky repeated use of 'and' could be improved. Response: We will insert full-stop after 'thin section' and delete first 'and'.

Line 383. ': : : age models using Monte Carlo simulation of equation 1 of Hellstrom: : :'. This sentence could be clarified slightly. Response: We will rephrase 'The uncertainty was fully propagated for both stalagmite age models using Monte Carlo simulations of equation 1 in Hellstrom (2006).'

Line 407. 'Thus the short lived..' Starting sentence with 'Thus'. Would read better as 'On these grounds, the short lived..' or similar. Response: We will replace text with suggested.

Line 525. '2s' – you mean 2 sigma? Response: Yes, we will replace with 'σ<sub>i</sub>Äğ.

Line 529. Both 'a' and 'years' are used to describe time in this manuscript. Be consistent. Also, although 'a' is correct, it is sometimes hard misleading. Response: We will

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make consistent.

Line 531. Spectral analysis. Did you do a wavelet analysis too? It would probably offer a useful comparison. Response: We have now performed a wavelet analysis, which has the advantage of naturally showing changes in periodicity over time but with the disadvantage that the original time series must first be interpolated onto a regular grid. It gives similar results to the Lomb-Scargle method and supports the ~180 year period that appears in both MC-S1 and MC-S2. We have provided the wavelet analysis in the supplement to this document.

Line 559. As far as I can tell, the frequently used acronym 'PCP' is not defined in this manuscript, which is very confusing to the non speleologist. Response: We will spell out acronym and insert a short definition at lines 558-559.

Line 673. Delete 'natural'? Response: See response to Reviewer 1 L673.

Line 691. 'it has to be' – reword using a more circumspect tone. Response: Text will be reworded as suggested.

Line 696. This is not a clear sentence. I think you mean 'This modelling suggested that the effects of global ice volume (and temp? how did you do that?) could account for a 2‰ change.' Response: We will reword as suggested. The method is given in Supplementary section S3.

Line 727. This reference to the 'Flinders silts' is very vague – I'm not sure that 'Flinders silts' is even the best way to name that archive. What are they, where are they, what types of process do they record. This archive warrants a more detailed/rigorous description. Response: Various names have been used in reference to this archive in the past, we have used 'Flinders silts' as this was the term used in the most recent paper on this archive (Haberlah et al., 20110, cited in the text) which is the most comprehensive and up to date examination of this archive. Ten lines (726-737) in the text already appear describing this archive and its interpretation. References are also given. We

will make minor changes to this existing text to improve its readability.

Line 739. 'remarkably well' is something of an exaggeration. Response: It is remarkable. The timing of rapid aggradation of the silts begins at the same time (24 ka) as speleothem growth switches on and ends (16 ka) when the speleothem growth ceases. The mid-transition at 19 ka to reduced storm magnitude/frequency also coincides with the transition in the speleothem record to wetter conditions.

Line 849. 'and the Liang Luar records' Delete 'the' and 'records' Response: We will make this correction. as suggested.

Line 860. 'are experiencing recharge' – use past tense here, consistent with the rest of the paper. Response: We will change to past tense.

Line 866. Ditto above. Response: We will change to past tense.

Line 924. Change the wording of this first sentence to something like. 'Two stalagmites from Mairs Cave, Flinders Ranges, are interpreted to record: : : ' Response: We will replace with suggested.

Line 964. 'Cohen, (year?); Haberlah et al. (2010): : : ' Response: We will insert year.

Figure 2b. Is there an age reversal (18.9, 18.2, 18.9 ka) at this key transition? I don't recall reading about that in the manuscript. Response: Thanks for picking this up. The age  $18.2 \pm 0.4$  was rejected from the age model and will be deleted from Fig 2b.

Figure 3. Fabric log. Please remind us in the caption what the fabric log means. Response: This will be described in the Methods, as outlined in response to Reviewer 2.

Figure 5 a-b. Please plot spectral analyses using the same x- axis. Response: The spectral analysis plots will be re-drawn as suggested.

Please also note the supplement to this comment:



<http://www.clim-past-discuss.net/cp-2016-135/cp-2016-135-AC3-supplement.pdf>

Interactive comment on Clim. Past Discuss., doi:10.5194/cp-2016-135, 2016.

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