

Clim. Past Discuss., author comment AC3  
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## Reply on RC3

Briony Kate Chamberlayne et al.

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Author comment on "1750 years of hydrological change in southern Australia: a bivalve oxygen isotope record from the Coorong Lagoon" by Briony Kate Chamberlayne et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2022-39-AC3>, 2022

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The two primary concerns I have are 1) if this is a temperature or hydroclimate record and 2) in relation to the age model.

Firstly – the authors provide fairly weak evidence that despite establishing a d18O and temperature relationship for this site, they believe their d18O record better represents rainfall/flow. The authors need to provide further evidence/discussion of this through analysis between contemporary flow/rainfall and d18O in the top layers. This is crucial as an alternative interpretation of the results (based on the stated temperature relationships) is that that 500-1100 was cool rather than wet and 1100-1750 was warm rather than dry.

R: The authors thank the reviewer for this valid comment, but we do not believe it would be possible to add to this study due to the lack of samples in the top centimetres of the core from the South Lagoon. Furthermore, while temperature may contribute to changes in d18O, the change required to result in the range of d18O values measured in *A. helmsi* valves is unrealistic for the region (~10 °C). This is discussed in section 4.1 of the manuscript. The authors can edit this section to make a stronger argument.

Secondly – The top 500 years of the age model doesn't appear to be well constrained and the authors exclude some dates based on being outliers. There is a sedimentary horizon at 40cm with a date below of 1444 and a date above (20cm) of 1783. This is a big gap. Could there be a hiatus in between these dates or a change in deposition rate? Another question is if the shells were in dead or live position as I am aware they can burrow into sediments and therefore may 'move into older layers'. Also the marine reservoir effect may vary over time. For example, a bushfire could result in a high influx of young carbon, so subtracting 800 years in this instance would be erroneous. If the lake has a lot of vegetation and organic matter surrounding it, this may act to offset the carbon - groundwater age. While this may not be able to be completely addressed, the authors should discuss this in their discussion and conclusions as potential sources of uncertainty.

R: The points made by the reviewer in this comment are valid and we agree that a more detailed discussion of these potential uncertainties should be included in the text. Resolving these uncertainties is outside the scope of this study, but the authors will highlight the potential for further research to better constrain the timing of changes in the d18O record. We will add detail to the discussion and conclusions of the manuscript highlighting this source of uncertainty.

Other comments:

In the first line of the abstract, the authors mention the resilience of aquatic ecosystems, however, palaeoclimate data is relevant to all ecosystems including terrestrial.

R: We agree and will remove the word aquatic

Line 35 – highlights the lack of decadal scale records. However this statement is true for both high and low frequency reconstructions Reference to Dixon et al 2007 as 'recent' is probably not quite right. There have been advancements in the last 5 years. Particularly for Tasmania for example but also WA.

R: Dixon et al was published in 2017. We will add a sentence highlighting some recent papers from TAS and WA.

Of the 9 records in Dixon, how many are on the mainland in SE Australia – it is worth noting for context for this study.

R: We will add a map to Figure 1 showing the locations of the study sites from these studies in relation to the site in the current study.

Line 40 – I don't think it is appropriate to lump the ANZDA in with reconstructions based on a single remote proxy. The ANZDA is based 176 tree-ring chronologies and one coral series from both Australia and wider Pacific.

R: The authors will highlight the number of studies included in this reconstruction in the text and clarify any comparisons with single proxy data studies.

Line 47 – A flood is not a decadal phenomenon. Floods tend to build rapidly, peak and subside within a week or so. Droughts can last seasons to years. The term pluvial or flood dominated epoch would be more appropriate.

R: Agree, we will instead refer to drought or flood dominated epochs.

It would be good if the authors could provide a stronger case as to why low frequency reconstructions are useful. In the first line of the introduction the authors state that "Multi-decadal to centennial records of past hydroclimate variability are crucial for understanding long term climate drivers, for calibrating and validating climate models, for assessing hydroclimate sensitivity to external drivers and for estimating the probability of multi-decadal climate extremes", however much of this requires annual or sub annual data. I am not suggesting low frequency reconstructions that cover longer periods are not useful but please spell out why they are an important piece of the puzzle in the introduction.

R: The authors will add context to this statement to build a case as to why both low and high frequency records are valuable.

Line 70 – 'Moreover' should be 'however'

R: Amended

Line 91 – Remove "formally speaking"

R: Amended

The authors mention the use of Pinus Pollen in identifying when modern section of the record. How is the pollen actually identified? Please include details

R: The authors will clarify in the text that the pollen collection and identification method is outlined in another paper (Krull et al. 2009)

Line 135 – missing “the”

R: Added

The authors mention some samples with erroneous dates, where they were identified as older than the sequence in which they were in. is this evidence of the aged carbon offset not being constant? If not, why might then be in error?

R: The erroneous dates may have been a consequence of mixing in the top sediments during storm activity or similar. Another possibility is that old shells were washed into the lagoon. Along the banks of the lagoons are past shorelines which contain abundant amounts of *A. helmsi* shells. It is possible that these were transported by wind, water or wildlife. We will add this information to the text.

Section 4.1 – Perhaps the authors could compare to Tasmanian temperature reconstructions by Allen et al? Or the Lake Tay reconstruction by Cullen and Grierson 2009

R: The authors will include references to these papers in section 4.1

Figure 1 – Add a box around Australia as it is a different scale to the rest of the map

R: The authors will add a box around Australia in Figure 1.