

# Last interglacial (MIS 5e) sea-level proxies in southeastern South America – response to comments

Evan J. Gowan<sup>1,2</sup>, Alessio Rovere<sup>2</sup>, Deirdre D. Ryan<sup>2</sup>, Sebastian Richiano<sup>3</sup>,  
Alejandro Montes<sup>4</sup>, Marta Pappalardo<sup>5</sup>, Marina L. Aguirre<sup>6,7</sup>

December 9, 2020

1. Alfred Wegener Institute, Helmholtz Center for Polar and Marine Research, Bremerhaven, Germany
2. MARUM, University of Bremen, Bremen, Germany
3. Instituto Patagónico de Geología y Paleontología, IPGP-CENPAT-CONICET. Puerto Madryn, Argentina
4. Universidad Nacional de Tierra del Fuego, Ushuaia, Argentina
5. University of Pisa, Pisa, Italy
6. CONICET, Consejo Nacional de Investigaciones Científicas y Técnicas, Argentina
7. Facultad de Ciencias Naturales y Museo (FCNyM), Universidad Nacional de La Plata (UNLP), 1900 La Plata, Argentina

## 1 Overview

First off, we would like to thank Dr. Rojas for reviewing our paper again, and Dr. Simms for handling the editing of the paper. Here, we summarize the main steps we have taken to address the comments. The original comments from the reviewers are in italics, our response is in standard font, and changes to the text in the manuscript are shown in blue.

## 2 Comments by Dr. Simms

*Thank you very much for taking the time to address the comments of the reviewers. As you can see, I sent it back to one of the original reviewers and they largely seem pleased with the corrections, although they do provide a few minor comments. Please consider these remaining comments - which likely only involve a subtle rewriting/rewording of a few lines of text. I have also read through your manuscript and have provided some minor (mostly editorial) comments. Once these are addressed I think it should be ready for publication. Thank you again for contributing to our special issue as I think your paper will make a valuable contribution to the project.*

**Response:** We thank Dr. Simms for editing our paper. We have made the editorial corrections that have been suggested.

## 3 Comments by Dr. Rojas

### 3.1 Main comments

*As expressed in my first review, this manuscript represents a very valuable summary of the data on the sea-level proxies available for southeastern South America during the Last Interglacial.*

*The present version of the manuscript took into account the majority of the suggestions made and I find the outcome is a more complete and accurate text with uniformised data and information for the different sections. Moreover, the new tables and modified figures improve the preprint. Also, the authors provided explicit explanation and relevance for the inclusion of the Holocene curve that I found it was not sufficiently explained in the first version. I am in general satisfied with the author's response to my main concerns as well.*

*Despite this, I have some comments regarding some author's responses and some minor changes to be fixed.*

**Response:** We thank Dr. Rojas for taking the time to review our paper again.

*Regarding the author's responses: In page 2: - When they refer to the deposits from northern Argentina and Uruguay and that ages will never have the precision of deep ocean sediments, this is true for all the deposits considered in the present paper not only for that particular area.*

**Response:** We completely agree. We have modified the last sentence of paragraph 2 in the Introduction to read:

We acknowledge that due to the imprecision of the dating methods applied to the deposits in the entire study area that they may represent a MIS 5a or MIS 5c highstand, or even Holocene or pre-MIS 5 highstands.

*- When the authors refer to the taphonomy issue, I never meant that taphonomy can substitute an age assignment. However, a taphonomic analysis along with the information derived from the molluscan assemblage (both!) can provide relevant evidence, especially in those areas more prone to show biogeographic changes of thermally sensitive species. That is, for northern Argentina and Uruguayan assemblages, as shown in the specific references. Moreover, numerical dating techniques are not the revealed truth, and fortunately discussed in this manuscript's version, especially those ages derived from dating methods advised against to be performed on molluscs. Instead, numerical ages should be interpreted considering other lines of evidence, such as palaeontological data, global or local temperature scenario, deposit elevation, and global and local sea level position. This is often a complex discussion and some of these evidences may yield contradictory information.*

**Response:** We agree that using the taphonomy over a wide area can be used to compliment the age assignment of a deposit, especially since in many places they have not be securely dated. However, due to the ambiguity of age, for instance if a deposit is MIS 5e or an earlier warm interglacial (*e.g.* MIS 11 or 31), we still believe that it is necessary to provide numerical age control to confidently assign a Last Interglacial age to a deposit (which is the purpose of this review). This is why we have assigned low quality scores to most of the sites. We have added a sentence to the end of section 6.1 to address this.

Considering the wealth of faunal data, we foresee the combination of these datasets, along with more securely dated and differential GPS measured shoreline deposits, to provide a way to provide a detailed comparison environmental changes and links to sea level change.

*Also, in favour of taphonomy and related data, the elevation of a given fossiliferous deposit cannot be simply taken as a proxy of sea level position. A taphonomic and sedimentological analysis can provide the original depth of deposition, an information of paramount importance to determine past sea level position (not only the elevation alone).*

**Response:** This is true if it can be confirmed the fossil is in-situ, and it is well established what depth the organisms live at. Most of the deposits included our database are beach and beach ridge deposits, and the shells will most likely be redeposited. For these kind of deposits, we have defined the sea level position based on the IMCalc program (see section 2) For near-shore and estuary deposits this could potentially be used to determine an absolute sea level position if there are enough species available to make such a determination. We have added a sentence to section 6.1 to address this.

If the fauna are in-situ, they also provide the possibility to make a link to sea level position in the estuary and near-shore deposits we have considered to be marine limiting.

*I do not want to extend on the the Río de La Plata and salinity issue, but the presence of extralimital warm water taxa, especially at the northern area considered in this study, certainly respond to palaeotemperature changes when the above barrier was not sufficiently strong as today (as show in the specific papers). Finally, when the authors say in the first paragraph of page 3 that differences in taxonomic molluscan composition is not a valid tool to discriminate between substages, only for example between Last Interglacial and Holocene, well, it depends on the specific study area. The area reviewed in this study is very broad and as said before, there are more sensitive sectors than others in terms of palaeoenvironmental change to be detected. Also, to discriminate Holocene assemblages, radiocarbon ages do constitute here a confident tool.*

**Response:** In terms of the environmental changes, we agree that this is possible, but it beyond the scope of this review, which is purely to evaluate sea level position during MIS 5. We agree that Holocene shorelines can be confidently dated with radiocarbon methods, and have added a sentence to the end of section 4.5.

Radiocarbon dating remains the most widely applied method to date Holocene shorelines, and has been successfully applied to many of the same regions that have Pleistocene deposits (see Section 6.4).

### 3.2 Comments from the paper

Line 18: *interstadial*

**Response:** Fixed.

Line 253: *was not necessarily warmer as the La Coronilla assemblage suggests.*

**Response:** This has been added to the sentence here.

Line 253: *See also Mart<sup>~</sup>Áñez and Rojas (2013): Relative sea level during the Holocene in Uruguay*

**Response:** We have added the reference here.

## 4 Other changes

Since the submission of the last revision of the paper, a new paper by Björck et al. (2021) has come out, that provides new infrared stimulated luminescence (IRSL) dates for the Puerto Williams site. We have added this information to the database and updated the paper.

We have added a new subsection in section 4 on IRSL:

IRSL ages from K-feldspar grains were collected from the Puerto Williams site in Chile (Björck et al., 2021). K-feldspar was chosen over quartz as the luminescence signal was too weak in the quartz. The date derived from the pIRIR signal at 290°C, with the assumption of no fading, was chosen represent the age. Analysis was completed at Lund University.

And added the following sentences to the Puerto Williams section:

Björck et al. (2021) revisited the site, or perhaps another closeby since they were unable to find the till unit. They collected IRSL dates that are consistent with an MIS 5 assignment. They remeasured the elevation with a clinometer, but since the benchmark was not stated, it does not improve the uncertainty of the elevation from the previous descriptions.

Best Regards,  
Evan J. Gowan (on behalf of the authors)

## References

Björck, S., Lambeck, K., Möller, P., Waldmann, N., Bennike, O., Jiang, H., Li, D., Sandgren, P., Nielsen, A. B., and Porter, C. T.: Relative sea level changes and glacio-isostatic modelling in the Beagle Channel, Tierra del Fuego, Chile: Glacial and tectonic implications, *Quaternary Science Reviews*, 251, 106 657, <https://doi.org/10.1016/j.quascirev.2020.106657>, 2021.