

## ***Interactive comment on “Marine terraces of the last interglacial period along the Pacific coast of South America (1° N–40° S)” by Roland Freisleben et al.***

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The manuscript entitled “Marine terraces of the last interglacial period along the Pacific coast of South America (1°N-40°S)” presents a supposedly exhaustive dataset of the marine terraces of the last interglacial all along the Pacific margin of the Andes (between 1°N and 40°S). The dataset is interesting and deserves publication. Nevertheless, the manuscript deserves more attention as a number of points could be strengthened to make it more comprehensive.

I begin with a very general remark. Like the authors, I have often tried to make studies that are as objective as possible. But when dealing with data from Nature, we are often

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forced to make subjective choices. Here, it concerns the location of the profiles, or the parameters of the QR equation. I would be grateful if the authors could further explain how they choose the location of the profiles and possibly show a less "obvious" site than Chala. For QR, one understands, one accepts, the weight put on each of the terms, but the exponent  $e$  seems a bit magical. Couldn't the authors get rid of  $e$ ?

Another general remark: the paper is difficult to follow because many points are badly presented or discussed. Firstly, how can the authors be sure of the age of the terraces? Is the signal continuous (which I doubt since there are spaces of more than 100 km between some terraced areas in Figure 4)? For example, I was not aware of any terraces in north-central Peru, which I thought were subsiding (see le Roux et al. 2000): are the terraces really MIS 5? Secondly, the authors use terminology that I understand established by their own group. More explanations/details would be necessary about what the indicative meaning is, about what a referencing point is, about the location and nomenclature of the measuring points (figure 4 should show the names of the points, Pe2, Ch1.... As well as some of the names in figures 5 - 8).

The TerraceM data is clean. In order to interpret them in terms of uplift rate or ancient sea level, the authors try to precisely quantify the uplift. This is not trivial: the authors do a good job on sea level but ignore the fact that the current shoreline angle is not at an altitude of 0, it is often higher. Even if the authors do not account for this offset, it would be good if they mentioned it and possibly the uncertainty it introduces.

Nevertheless, the systematic use of TerraceM is a good initiative, and I support the publication of this data with the paper that goes with it.

Specific comments

Lines 104-105. Steep vs flat slab not really introduced.

Paragraph 2.1.2. I understand the interest of presenting active tectonics, but the paragraph is neither concise nor exhaustive, so I doubt its usefulness. Perhaps it would

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be better to quote the two recent compilations by Melnick et al (2019) and Costa et al (2020).

Lines 325-335. Explanation difficult to follow. Why not starting with the QR equation?

Lines 575-581. It is indeed interesting; generations of researchers have not waited for the authors to get interested in using marine terraces to study the uplift of key areas such as Arauco or Mejillones.

Lines 582-593. I am much more interested in less studied areas such as north-central Peru. The authors could expand a little more on this point.

Technical corrections

Line 40: Reference to Siddall OK, but references to marine terraces would be welcome.

Line 55. I would move the reference to Regard et al. to line 50 as it represents a fairly continuous signal.

Line 80 and in other places, as line 288. The reference may be Pedoja et al. 2011, more focused on the last interglacial than Pedoja et al. 2014.

Line 184: "a slight increase in distance". Which distance?

Line 187: "Wave erosion forms wave-cut terrace levels" This is what the community think, but it is not certain (see Premeillon et al. 2018).

Lines 281-282. I think I understand, but the sentence "The DEMs were converted to orthometric heights using the ellipsoid projection of the World Geodetic System (WGS1984) and the EGM2008 (EEGM08) geoid" is misspelled. Does this mean that the authors used a grid of EGM heights above WGS84?

Legend of Figure 3: There was a switch between x-axis and y-axis.

QR. Indicate that it varies between 1 and 5; there is an error on the 3rd coefficient: 0.4 rather than  $0.4 * 1.2$ .

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Line 555. Is there a reason why error and dispersion are correlated (not clear to me)?

Line 617. The authors must quote Macharé and Ortlieb, a key paper.

Line 651-652. A ref to support this assertion is missing.

Line 667-671. It is possible that wave power is not the main driver for coastal erosion...

Additional references

Costa, C. et al., 2020, Hazardous faults of South America; compilation and overview: *Journal of South American Earth Sciences*, v. 104, p. 102837, doi:10.1016/j.jsames.2020.102837.

le Roux, J.P., Correa, C.T., and Alayza, F., 2000, Sedimentology of the Rimac-Chillon alluvial fan at Lima, Peru, as related to Plio-Pleistocene sea-level changes, glacial cycles and tectonics: *Journal Of South American Earth Sciences*, v. 13, p. 499–510.

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